



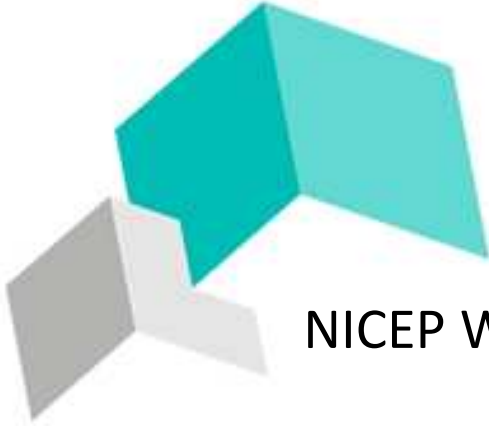
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State capacity, informality and clientelism

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State Capacity, Informality, and Clientelism*

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Abstract

Understanding how to tackle clientelism is crucial for enhancing political accountability. While its negative impacts on governance are well-documented, less is known about strategies to disrupt these networks. This paper argues that stricter enforcement of labor regulations can weaken clientelistic practices. Using an instrumental variable approach based on within-municipality changes in proximity to labor offices and state-level variations in labor inspectors in Brazil, we find that enhanced enforcement significantly reduces the electoral success of clientelistic parties and curtails patronage and vote-buying. These outcomes are largely driven by a decline in informal workers, who are more likely to support 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 mutually reinforcing and detrimental cycle. Ineffective state institutions, unable to adequately meet citizens’ needs, create fertile ground for clientelistic relationships. In turn, clientelistic practices incentivize political actors to actively maintain or even exacerbate institutional weakness, as fragile governance structures facilitate the consolidation of their power through patronage networks. This cycle erodes public trust in government, enables corruption, and undermines public accountability, resulting in poor governance and inadequate public goods provision.¹ While the detrimental consequences of this dynamic are well-documented, the ways in which state capacity can be strengthened to break this cycle remain understudied. This paper addresses this gap by investigating whether targeted investments in a specific dimension of state capacity—the enforcement of labor laws—can disrupt entrenched clientelistic networks and reshape local political dynamics.

A growing body of literature demonstrates how clientelism can hinder efforts to strengthen local state institutions (e.g., Fergusson et al. 2022b; De La O 2024). While scholars recognize the self-reinforcing nature of this dynamic (Fergusson et al. 2022a), far less attention has been devoted to the inverse causal relationship: can enhancing *local* state capacity effectively weaken clientelistic practices? This is a crucial, yet understudied, question. One reason for this lacuna is the inherent endogeneity of state capacity. Institution-building efforts often threaten established political rents (Martin 2023), diminish incumbents’ electoral prospects (Prichard 2018; Christensen and Garfias 2021), and reduce the effectiveness of particularistic strategies (Fergusson et al. 2022b; Gottlieb 2021). Consequently, meaningful reforms are often deterred in clientelistic strongholds. Furthermore, once established, clientelistic networks can politicize bureaucracies, cement patron-client relationships, and actively obstruct institutional development (Calvo and Murillo 2004; Hicken 2011; Cruz and Keefer 2015; Oliveros 2021; Brierley 2021; De La O 2024). This creates a challenging environment for state-building, where investments in local capacity are difficult to initiate and sustain. The critical question, therefore, is whether and how such efforts can successfully disrupt deeply rooted clientelism.

This paper addresses this question by examining whether state-building initiatives originating from higher levels of government can meaningfully disrupt and displace clientelistic

¹For 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).

practices at the local level. Specifically, we study how the centrally driven expansion of labor law enforcement influences the scope of clientelism, ultimately leading to improved governance outcomes. Labor law enforcement represents a strategic form of state capacity with broad social and economic repercussions: when stronger enforcement reduces informality, it also diminishes the economic vulnerabilities that render citizens likely to rely on clientelist exchanges as an informal insurance mechanism (Bobonis et al. 2022; Rains and Wibbels 2023). Furthermore, unlike initiatives led by local politicians, enforcement by central authorities is generally better insulated from local capture. Together, these two factors suggest that centrally-led investments in enforcement capacity can weaken the electoral appeal of clientelistic parties and foster more programmatic policies.

We examine this question in the context of Brazil, where the Ministry of Labor oversees labor law enforcement, including the establishment of labor offices and the geographic deployment of inspectors. Using spatial and temporal variations in enforcement intensity—measured as the number of inspections per 100 formal firms—we assess its impact on clientelism. To capture outcomes such as electoral support for clientelistic parties, vote-buying, and patronage, we draw on diverse data sources, including three expert surveys at the party level (Kitschelt et al. 2013; Norris 2020; Lindberg 2022), microdata on the entire universe of Brazilian bureaucrats (RAIS), and individual-level data from five waves of the *Latino-barómetro* survey.

To mitigate endogeneity concerns, we employ an instrumental variable approach that leverages two sources of exogenous variation: the distance to the nearest labor office and the number of labor inspectors allocated to each state. These instruments reflect central decision-making processes and enable us to isolate the causal impact of enforcement intensity on the electoral success of clientelistic parties. Our approach builds on methodologies developed in previous studies (Almeida and Carneiro 2012; Ponczek and Ulyssea 2022), but advances them by exploiting temporal variations within municipalities and redirecting the focus from labor market outcomes to political dynamics.

Our findings demonstrate that enhanced labor law enforcement significantly lowers the vote share of clientelistic parties and reduces their chances of winning local elections. These effects stem from both the supply-side, with a decrease in the number of clientelistic candidates running for office, and from the demand-side, where voter support for candidates from clientelistic parties diminishes—even after accounting for the proportion of candidates representing these parties. As for the mechanism, we show that municipalities with greater enforcement capacity experience a significant increase in the share of formal workers and a notable decline in informality. Using survey data, we further explore the relationship between informality and clientelism, finding that informal workers are disproportionately

more likely to support clientelistic candidates than formal workers with comparable income levels. Additionally, we observe that stricter enforcement of labor regulations reduces the hiring of bureaucrats on temporary contracts—which are commonly associated with patronage appointments (Kemahlioglu 2013; Brollo et al. 2017; Colonnelli et al. 2020; Toral 2023)—decreases reports of vote-buying, and improves municipal outcomes in key areas such as education and health.

We address potential threats to identification and ensure the robustness of our results through several approaches. For instance, to mitigate concerns about the non-random placement of labor offices and avoid bias related to the selection of host municipalities, we exclude these municipalities from our analysis. Instead, we concentrate on examining how changes in enforcement capacity affect clientelism in neighboring areas. Crucially, we demonstrate that our instrumental variables exhibit no correlation with neighboring municipalities’ population size, GDP, initial levels of fiscal capacity, and other local outcomes. Additionally, we incorporate time-varying controls and implement an event-study approach based on 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 robustness checks further incorporate alternative measures of clientelism, extend the analysis to city council elections, and account for mayor-coalition clientelism. We also adopt multiple definitions of programmatic parties, rule out alternative explanations tied to personalism, populism, or party ideology, and verify that state or national leadership changes do not drive our findings. Lastly, we test various first-stage specifications, explore different measures of distance to labor offices, and consider multiple approaches to missing data. Collectively, these steps strengthen the credibility of our interpretation of results.

This paper primarily contributes to the literature on clientelism and state capacity. Prior research has largely focused on how clientelism undermines state capacity—affecting the size of the public sector, promoting bureaucratic politicization (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 perpetuating poor governance outcomes (Fergusson et al. 2022b; De La O 2024). For example, Gottlieb and Hollenbach (2021) shows that in Brazilian municipalities with weak tax institutions, politicians face higher electoral costs when attempting fiscal reforms, thereby discouraging improvements in fiscal capacity. Similarly, Fergusson et al. (2022b) finds that efforts to strengthen local bureaucratic capacity create electoral disadvantages for incumbents relying on clientelistic rather than programmatic political support. Yet, as Fergusson et al. (2022a) observes, causality between clientelism and state capacity can run in both directions, and systematic

evidence on the causal pathway running from state capacity to clientelism remains scarce.² We address this gap by leveraging exogenous variation in labor law enforcement—a critical yet underexamined aspect of state capacity—and demonstrate that enhanced enforcement can weaken local clientelistic networks.

We also add to a growing body of evidence that underscores the ability of national or supra-local programs to erode clientelistic networks at the local level. Such top-down initiatives offer two main advantages. First, they curb self-selection bias: without an external mandate, the only municipalities that would voluntarily strengthen state capacity are those already less vulnerable to clientelism. Second, they limit attrition bias by reducing opportunities for local elites to stall, subvert, or co-opt institutional reforms. Empirical evidence underscores these gains. In Mexico, Dower and Pfutze (2015) show that a federally imposed land-certification program weakened the dominant party’s grip by safeguarding property rights. Likewise, Frey’s (2019) study of Brazil’s *Bolsa Família* demonstrates that redistribution can erode clientelistic ties by lifting households permanently out of poverty. Complementing these findings, Bobonis et al. (2019) document that anti-corruption audits in Brazil’s Northeast reduced vote-buying. Our study builds on this literature by focusing on centrally implemented investments in state capacity—specifically, labor law enforcement—and demonstrating that such interventions can also be effective in disrupting local clientelistic networks.

Additionally, this paper advances the literature on labor-law enforcement and labor-market structure by combining a novel empirical design with new subnational panel data from Brazil. Although earlier studies establish that stricter enforcement raises formal employment (e.g., Almeida and Carneiro 2012; de Andrade et al. 2016; Brotherhood et al. 2023), most rely on firm-level analyses or cross-sectional analyses.³ Shifting the focus to municipality-level outcomes over time, we find that stronger enforcement not only increases formality, but also curbs informality, self-employment and even non-employment—echoing recent evidence that tighter labor regulations do not necessarily drive up unemployment (Meghir et al. 2015; Ulyssea 2020; Haanwinckel and Soares 2021; Dix-Carneiro et al. 2021).

Finally, we contribute to the literature on the individual-level determinants of demands for clientelism. Existing research has shown that poverty increases individuals’ susceptibility to clientelistic appeals (Calvo and Murillo 2004; Acemoglu 2005; Kitschelt and Wilkinson

²In a cross-national study, Bustikova and Corduneanu-Huci (2017) have shown that historical state capacity is strongly associated with contemporary clientelism patterns, as past experiences with public bureaucracies influence voters’ and parties’ expectations about the state’s ability to deliver public goods. Nonetheless, the causal effects of short-term, local-level variations in state capacity on the electoral success of clientelistic parties remain understudied.

³See Ulyssea (2020) for a review of this literature.

2007; Stokes 2005; Robinson and Verdier 2013; Bobonis et al. 2022), and that as citizens’ financial standing improves, they tend to abandon such relationships in favor of what Magaloni (2006) terms “ideological investments.” Nevertheless, poverty alone does not fully capture the risks and vulnerabilities that fuel clientelism. Recent studies highlight that even among individuals with similar incomes, those facing irregular employment or unstable earnings are more likely to rely on patron-client relationships. For example, Bobonis et al. (2022) shows that installing residential water cisterns in drought-prone areas of Brazil reduced residents’ reliance on private benefits from local politicians. Within this broader framework, labor market informality emerges as a critical source of economic insecurity. Informal workers, lacking access to social insurance mechanisms, are more exposed to life shocks such as illness, unemployment, or disability. Confronted with these risks, they often view universal, programmatic policies as either inadequate or unlikely to reach them (Auerbach et al. 2018; Rains and Wibbels 2023). As a result, they gravitate toward particularistic arrangements that deliver immediate, concrete benefits, preferring these to impersonal, standardized programs (Altamirano 2015; Rains and Wibbels 2023; Gottlieb 2024).⁴ While existing research based on survey data has established a link between labor informality and clientelism, we still lack empirical evidence demonstrating that reducing informality effectively curbs clientelistic politics. This paper addresses this gap by analyzing a policy intervention that directly affects labor market conditions. We show that strengthening labor law enforcement—and thereby expanding formal employment—significantly reduces the electoral success of clientelistic parties.

The remainder of this article is organized as follows. Section 2 presents the conceptual framework and hypotheses. Section 3 provides background on labor law enforcement in Brazil. Section 4 details our data and empirical strategy, and Section 5 presents our findings and explores mechanisms. Section 6 discusses threats to identification, performs robustness checks and additional analyses, and Section 7 concludes.

2 Conceptual Framework

We propose a conceptual framework in which stronger enforcement of labor laws reduces clientelism by altering both voter behavior (demand side) and politicians’ strategies (supply side). By lowering labor informality and diminishing citizens’ reliance on particularistic

⁴Indeed, scholars have argued that clientelistic networks often function as informal insurance mechanisms (Schmidt et al. 1977; Scott 1972), providing access to essential resources, such as financial aid, healthcare and employment, which makes them particularly appealing to vulnerable populations (Murillo et al. 2021).

exchanges, better enforcement can shift voter preferences toward more policy-oriented candidates. Consequently, clientelistic politicians face a new electoral environment that forces them to modify their tactics. In the following section, we unpack these dynamics in greater detail and present a set of testable hypotheses to guide our empirical analysis.

On the demand side, increased enforcement of labor regulations is expected to reduce citizens' support for clientelistic parties: as individuals transition to the formal sector and gain access to mechanisms—such as social security, health insurance, or stable contracts—that mitigate economic risks, their reliance on patron-client networks as a form of insurance declines. This is crucial because voters play an active role in sustaining these exchanges and often act as key instigators, by approaching brokers and politicians to request assistance and negotiate agreements (Auyero 2000; Lindberg 2010; Nichter and Peress 2017; Nichter 2018; Hicken et al. 2019; Nathan 2019). More vulnerable groups are particularly inclined to do so, given their precarious circumstances and pressing need for immediate assistance. However, as labor informality declines under stricter enforcement, voter-initiated demands for particularistic exchanges should decrease, weakening one of the key foundations of clientelism.

These changes in voter behavior create a more challenging context for clientelistic politicians. First, with fewer demands being made, transaction costs rise because politicians must invest more effort in locating and negotiating with potential targets in an environment where voter intentions are less transparent. Second, as voters become less economically vulnerable, politicians must offer larger material incentives to secure their support, driving up the costs of clientelistic tactics. Third, with fewer signals from voters, politicians are more likely to misallocate resources by directing benefits to individuals who may not reciprocate with electoral support. Collectively, these factors erode the efficiency and undermine the returns of clientelistic strategies, reducing their overall appeal in contexts marked by lower vulnerability.

On the supply side, clientelistic politicians who find themselves in this shifting electoral landscape can opt for one of three responses: they may pivot toward more programmatic platforms, continue to rely on clientelistic strategies despite their diminishing returns, or withdraw from electoral contests entirely. Each choice influences outcomes such as the prevalence of particularistic exchanges, the pool of clientelistic candidates, and the vote share these parties secure. A turn toward programmatic appeals—substituting targeted transfers with promises of broad public goods—typically reduces reliance on clientelist tactics, such as vote buying, turnout buying, abstention buying, and double persuasion (Gans-Morse et al. 2014). However, it also weakens electoral support for clientelistic parties. Because candidates from these parties lack credibility in delivering public goods, they are less likely to

attract as many votes through programmatic pledges as they do through targeted exchanges. A second possible response is to persist with traditional clientelistic strategies. However, as voter vulnerability declines and the cost of securing individual votes rises, candidates with fixed budgets can buy only a fraction of the votes they could previously afford. This approach thus leads to a decline in vote buying and a corresponding drop in electoral support. The third, and most drastic, supply-side response is candidate withdrawal: faced with higher costs associated with vote-buying and low credibility when making programmatic appeals, some clientelistic candidates may determine that winning is untenable under the new conditions. In turn, fewer such candidates enter races, further reducing the prevalence of particularistic exchanges and diminishing the vote share of clientelistic parties. These dynamics lead to the following testable hypotheses:

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

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

The supply-side mechanisms discussed above suggest that increased enforcement should lead to a reduction in the vote share of clientelistic parties, as the number of clientelistic candidates decreases. On the demand side, as voter vulnerability declines, the appeal of clientelistic candidates weakens. Together, these dynamics indicate that enhanced enforcement reduces support for clientelistic parties by affecting both the supply of their candidates and voter demand. This leads to our next hypothesis:

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

These combined supply- and demand-side pressures also point to a broader outcome: strengthening labor law enforcement should reduce the likelihood of clientelistic politicians winning local elections. As clientelism becomes less effective and more voters shift toward programmatic alternatives, the electoral prospects of clientelistic candidates should decline. This logic underpins our broader hypothesis:

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

These hypotheses establish a framework for empirically examining how enhanced labor law enforcement and declining informality reshape clientelist politics. Figure A1 synthesizes these processes, illustrating how enforcement intensity impacts voters' behavior, politicians' strategies, and electoral outcomes. Section 4 details the empirical strategy and data employed

to test these hypotheses, building on the contextual foundation established in Section 3.

3 Context

3.1 Labor Law Enforcement in Brazil

Brazil’s labor regulations originate from the *Consolidação das Leis Trabalhistas* (CLT), established in 1943 under a civil law framework, governing permissible contract types as well as hiring and firing conditions. The 1988 Federal Constitution expanded worker benefits and protections, further amplifying both the complexity and cost of hiring (de Barros and Corseuil 2004). Labor taxes in Brazil 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. These significant administrative and financial burdens frequently prompt firms and workers to operate informally, highlighting the critical importance of enforcement in promoting formal employment in the country.

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. Yet nearly 32% of workers in 2010 were self-employed or in informal jobs, leaving them ineligible for these protections. Because the system delivers substantial benefits only to those registered as formal employees, a large segment of Brazil’s workforce remains economically vulnerable.

Labor law enforcement in Brazil is primarily the responsibility of the Ministry of Labor, which makes centralized decisions but 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. Inspectors, stationed at specific *gerências* and rotated periodically to deter corruption (Cardoso and Lage 2007), evaluate firms for compliance with labor regulations, including formal worker registration, minimum wage adherence, and lawful working hours. Their compensation includes a performance-based component, which can account for up to 45% of their salary (Almeida and Carneiro 2012). Local offices operate under general directives from the national headquarters of the agency, but ground-level inspectors retain some discretion over which establishments to visit and whether to address violations informally or proceed with formal citations and adjudication. Firm inspections are typically triggered by random audits or reports of noncompliance,

which may originate from workers, unions, the public prosecutor’s office, or the police. However, most inspections are initiated through anonymous reports. These operational features highlight two important points. First, the primary goal of inspections is to promote formalization, as evidenced by studies showing that firm visits can increase formal employment by up to 27 percentage points (de Andrade et al. 2016). Second, centralized decision-making structure, as well as the rotation and performance-based pay structures of inspectors reduce the likelihood of local political interference in enforcement activities.

The density, infrastructure, and staffing of labor offices vary significantly according to a state’s geographic size and economic profile, producing substantial subnational diversity in the Ministry’s enforcement apparatus. Previous research underscores the importance of the number of inspectors per office, as well as the distances they must travel to reach firms, in shaping overall frequency of inspections (Almeida and Carneiro 2012; Ponczek and Ulyssea 2022). Moreover, enforcement intensity also fluctuates over time, as the staggered establishment of new labor offices increases the likelihood that firms in previously unmonitored regions will be audited. Overall, these spatial and temporal variations in enforcement capacity offer a natural testing ground for examining how stronger labor-law oversight affects informality, reduces worker vulnerability, and influences the viability of clientelistic strategies.

3.2 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, elected every four years via a majority rule system, face a two-term consecutive limit and a potential runoff round if no candidate wins more than 50% of the vote.⁵ They wield substantial authority over local budgets and resource allocation, with one critical facet of this power being the patronage they can distribute through temporary contract jobs and commissioned positions. While Brazil’s civil service is largely meritocratic, these discretionary appointments enable mayors to reward political supporters and reinforce 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 tenuous (Limongi and Cortez 2010). Indeed, Samuels and Zucco (2014) show that party iden-

⁵Municipalities 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.

tification 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 platforms (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).

Although non-programmatic linkages are predominant, some Brazilian parties demonstrate relatively higher levels of institutionalization and an emphasis on policy-centered appeals. Two such examples are 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). Overall, this coexistence of programmatic and clientelistic parties raises critical questions about why certain parties succeed in some localities but not others, further motivating an investigation into whether and how local state capacity influences the prevalence and persistence of patron-client linkages.

4 Empirical Framework

4.1 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 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.2 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

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

⁷Only seven offices closed during the studied period.

⁸See <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 (Latinomarmetro) (%)	26.90	19.05	23.08
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 (%)	17.98	8.76	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 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.2.

measures. Since the scores are assigned at the party level, we apply the corresponding 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 primary analysis uses a binary measure of clientelism based on the party affiliation of the elected mayor, Section 6 provides additional analyses that employ (i) a continuous measure of clientelism, and (ii) the average clientelism score across all parties in the mayor’s coalition.

While the expert surveys provide a broad assessment of the extent to which parties engage

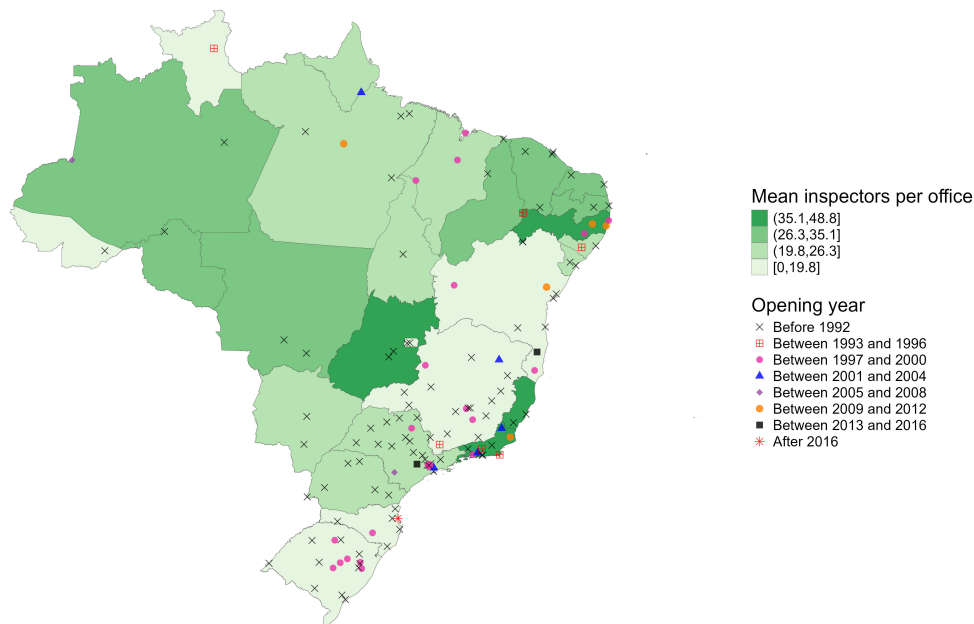
⁹Following Frey (2019), parties with missing scores are coded as non-clientelistic, as these are typically small parties with minimal clientelistic practices. However, Section 6 demonstrates the robustness of our results when excluding these parties or treating them as clientelistic.

in clientelistic practices—such as vote-buying, selective provision of public goods, allocation 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.

Figures 1 and A2 illustrate the main sources of variation that anchor our empirical strategy. Figure 1 plots enforcement capacity—measured by the number of labor inspectors per office—and traces the staggered establishment of local labor offices. The map shows pronounced variation both across municipalities and over time. Figure A2 displays the electoral fortunes of clientelistic parties, revealing sharp municipal contrasts in their vote shares. Viewed together, the two maps underscore the substantial heterogeneity in state enforcement strength and clientelistic influence that our analysis exploits.

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 1: Enforcement Capacity: Labor Offices and Inspectors



Notes: The map displays the distribution of the average number of inspectors per office from 1995 to 2020, and labor offices categorized by their opening year.

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 constitute 17% of the economically active population, a proportion comparable to both formal workers and the self-employed. Moreover, as illustrated in Figure A3, there is a strong negative correlation between the size of the formal sector and the probability that a clientelistic party will win local elections.

4.2 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

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

over time affect electoral outcomes. We start by considering the following reduced form model:

$$Y_{m,t} = \beta \textit{Enforcement}_{m,c(t)} + \mu_m + \lambda_t + \epsilon_{m,t} \quad (1)$$

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, $\textit{Enforcement}_{m,c(t)}$ 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 $\textit{Enforcement}_{m,t-1}$ 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, economic conditions, or broader development trends. Alternatively, non-enforcement may result from a political decision by bureaucrats or politicians (Holland 2017; Feierherd 2020; Amengual and Dargent 2020; Brollo et al. 2020). 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 ($\textit{Inspectors}_{r,c(t)}$) and the travel time by car to the nearest labor office ($\textit{Distance}_{m,c(t)}$). 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 inspec-

¹¹In 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.

tions, 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 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:

$$\begin{aligned} \text{Enforcement}_{m,c(t)} = & \alpha_1 \text{Inspectors}_{r,c(t)} + \alpha_2 \text{Distance}_{m,c(t)} + \alpha_3 \text{Inspectors}_{r,c(t)}^2 + \\ & \alpha_4 \text{Distance}_{m,c(t)}^2 + \alpha_5 \text{Inspectors}_{r,c(t)} \times \text{Distance}_{m,c(t)} + \quad (2) \\ & \mu_m + \lambda_t + \epsilon_{m,t} \end{aligned}$$

where, when using election-year data, $\text{Distance}_{m,c(t)}$ 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 $\text{Inspectors}_{r,c(t)}$ is the average number of inspectors per office in the corresponding state during the same period. For year-level regressions, we use $\text{Enforcement}_{m,t-1}$, which is defined as the logged number of inspections per 100 firms in municipality m during the previous year ($t-1$), $\text{Distance}_{m,t-1}$, which denotes the car travel distance between the municipality and the nearest labor office (per 100 kms) in year $t-1$, and $\text{Inspectors}_{r,t-1}$, 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 6. 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 $\widehat{\text{Enforcement}}_{m,c(t)}$:

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

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.3). 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. In Section 6.1, we discuss other important threats to identification, including potential violations of the exclusion restriction and the role of political manipulation.

5 Results

5.1 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.¹² 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 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 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.

¹²These variables are derived from the three expert surveys discussed earlier: DALP, GPS, and V-Party (see Section 3 and Appendix OA.2).

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)
Enforcement $_{m,c(t)}$	−0.383*** (0.083)	−0.239*** (0.074)	−0.145** (0.072)	−0.205*** (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. 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.2.

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, are both larger in magnitude and more statistically significant than the ordinary least squares (OLS) estimates. 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.¹³

We next examine the overall electoral support for clientelist parties in mayoral elections. The results, shown in Table A.4, 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

¹³One potential concern is that our findings may not generalize to larger municipalities, as we exclude host municipalities from our analysis (see Section 4). However, these exclusions represent only 139 out of 5,596 municipalities, and our results remain robust even when they remain in the sample, as shown in Table A.3.

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)
Enforcement $_{m,c(t)}$	-0.337*** (0.045)	-0.385*** (0.068)	-0.091 (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. 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.2.

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 is likely to weaken their mandate, to reduce their legitimacy, and to limit 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).

Isolating demand-side dynamics from supply-side responses is inherently difficult. Nonetheless, to explore the demand channel, we re-estimate the models in Table A.4, this time adding the share of candidates from clientelistic parties as a control. As reported in Table 4, the negative effect of stricter enforcement on those parties' vote share remains statistically significant for two of the three clientelism measures, providing suggestive support for H3. Table A.5 replicates the exercise with the probability of winning as the outcome, and enforcement continues to exhibit significant negative effects across all clientelism indicators. However, as the candidate-share variable is measured after treatment, we caution against drawing causal conclusions from these results.

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)
Enforcement _{<i>m,c(t)</i>}	−0.148*** (0.042)	−0.064 (0.040)	−0.151*** (0.039)
Num of clientelistic candidates (log) _{<i>m,t</i>}	0.700*** (0.005)	0.659*** (0.005)	0.556*** (0.005)
Num of candidates (log) _{<i>m,t</i>}	−0.514*** (0.010)	−0.431*** (0.009)	−0.340*** (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. 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.2.

In Table A.6 we examine the impact of enforcement on the likelihood 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).

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

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

	(1) Reported vote-buying	(2) Reported vote-buying
Enforcement $_{m,t-1}$	-0.439** (0.203)	-0.463** (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 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.2.

the perspective of the electorate.¹⁴ The results presented in Table 5 indicate that increased 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.

5.2 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.

¹⁴The wording of the questions varies slightly across different waves. For detailed descriptions of the relevant questions, see Appendix OA.2.

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.¹⁵ 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.

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. We would expect a shift toward more educated bureaucracies and fewer temporary contracts. As noted in Section 3, 85% of Brazilian bureaucrats are selected via merit-based exams, while the remaining 15% hold temporary, non-civil servant contracts often linked to patronage (Brollo et al. 2017; Colonnelli et al. 2020; Toral 2023; Ferrali et al. 2024). Political discretion in hiring has also been shown to lower bureaucratic competence, typically measured by education levels (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 their composition.¹⁶ Specifically, a 10% increase in enforcement reduces the share of temporary bureaucrat hires by 1.67 percentage points. Additionally, we observe a positive effect

¹⁵For 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).

¹⁶We 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.

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)
Enforcement _{$m,c(t)$}	-0.368 (1.499)	-0.166*** (0.062)	0.041 (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. 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 4.1 and Appendix OA.2.

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.

Our public-service input measures focus on the tangible resources available in each municipality. For education, we draw on Brazil’s School Census (1997–2020) to record whether local schools possess key infrastructure—Internet access, libraries, and science laboratories.¹⁷ Similarly, our healthcare input indicators measure the average number of hospital beds and

¹⁷We use the same set of educational indicators 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
Enforcement $_{m,c(t)}$	0.107*** (0.042)	0.047*** (0.014)	0.591*** (0.080)	0.020* (0.011)	0.528*** (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. 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.

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. We favor these direct inputs over budget figures because, in clientelistic settings, reported spending often bears little relation to the quantity or quality of services actually delivered, as funds are easily diverted or misreported.¹⁸ Outcome variables such as test scores, mean years of schooling, infant mortality, or life expectancy are likewise avoided, as they confound a mayor’s effort with deep-seated structural differences across municipalities. Our results, presented in Table 7, indicate that stronger enforcement significantly enhances the quality of health and educational outcomes. 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.

¹⁸See Fergusson et al. (2023) for evidence that, under clientelism, education spending is a poor proxy for actual service delivery.

5.3 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.

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.¹⁹ 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.²⁰

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 the share of non-employed individuals.²¹ 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).²² Taken together, these findings provide compelling evidence that

¹⁹As in Almeida and Carneiro (2012), we focus on non-employment rather than unemployment due to the lack of census questions that reliably distinguish between individuals who are not working and those unemployed but actively seeking employment.

²⁰A 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.4.

²¹This 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).

²²Unlike 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,

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)
Enforcement _{<i>m,t-1</i>}	0.069*** (0.025)	-0.045** (0.020)	-0.092*** (0.030)	-0.117** (0.048)	0.637*** (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. 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.2.

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. Although we lack individual-level panel data to track these changes over time, we can still provide suggestive evidence that informal workers are, on average, 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 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 A.27 and further discussed in Section 6.

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

as demonstrated in Table A.7, 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) Saliency of clientelism of voted party (V-Party)
Informal Worker	0.315*** (0.107)	0.278*** (0.098)	0.171* (0.092)	0.203** (0.092)
Observations	829	829	829	829
Individual-level controls	Yes	Yes	Yes	Yes
R-squared	0.044	0.036	0.033	0.025

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. 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.2.

considered informal (Gasparini and Tornarolli 2009).²³ 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. Importantly, we control for income levels, allowing us to distinguish the vulnerability stemming from the uncertainty of informality from that driven by poverty. 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).

6 Threats to Identification and Robustness Checks

6.1 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 still raises concerns about endogeneity—both through potential reverse causality and possible breaches of the

²³The 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).

exclusion restriction. 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.3). 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. We further probe potential biases in three ways. First, we estimate event-study models to identify potential pre-trends and mitigate concerns regarding reverse causality. Second, we examine whether our instruments correlate with baseline economic and political conditions; the absence of such correlations suggests that office placement was not guided by developmental or partisan criteria, supporting the exclusion restriction. Third, we demonstrate that the main results are robust to adding time-varying municipal controls and state-by-year fixed effects, which absorb a wide array of unobserved shocks.

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.

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.²⁴

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. Specifically, we estimate the following Equation (4):

²⁴Unlike 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.

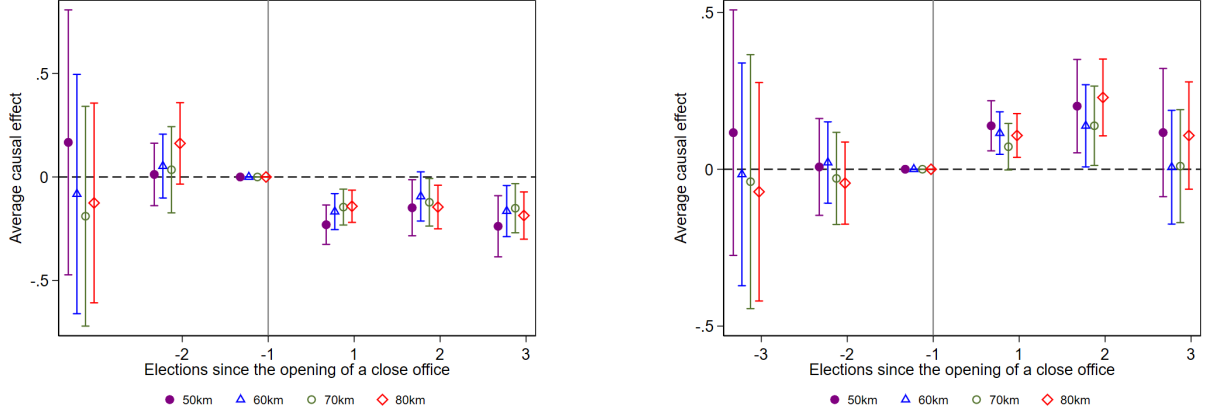
$$y_{m,t} = \sum_{k \in \{-x, \dots, z\} \setminus \{-1\}} \beta_k t_{m,t}^k + \alpha_{m,t} \mathbf{X}_{\mathbf{m},t} + \mu_m + \lambda_t + \epsilon_{jt} \quad (4)$$

with $t_{m,t}^k$ 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 $\mathbf{X}_{\mathbf{m},t}$ 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 2 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 A4 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 three 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.

Local Political Manipulation. Another concern is the potential influence of mayors 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.8 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.9, there is no evidence of a correlation between the number of inspectors per office and the proportion of clientelistic incumbents within each

Figure 2: Event study



(a) Panel A: Winning Party's Clientelism Score

(b) Panel B: Number of Formal Workers

Estimates of Equation (4) controlling for the number of inspectors and the number of pre-existing 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.2.

state.

Exclusion restriction. Although political manipulation appears to play little role in the siting of labor-enforcement offices, two endogeneity risks remain: expansion of the enforcement network might correlate with broader trends in local economic or political conditions, and it could respond to earlier gains in local fiscal capacity. We confront these possibilities with three complementary tests.

First, we verify that the instruments are orthogonal to baseline conditions across municipalities. Table 10 shows that distance to the nearest labor office is uncorrelated with lagged local GDP, population, or the educational and health indicators described in Section 5.2. Nor is it related to initial fiscal capacity, whether measured by tax revenues per capita, tax revenues net of transfers per capita, or the share of property taxes in total tax revenues. Table A.10 reaches the same conclusion for the regional stock of inspectors.

Second, we present two specifications incorporating controls for municipal demographic and economic characteristics that may influence labor market outcomes. Panel A of Table A.11 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

Table 10: Initial Conditions and Distance to Nearest Labor Office

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Local GDP _{<i>m,t-1</i>}	-5.549 (4.394)									
Population _{<i>m,t-1</i>}		-69.054 (57.624)								
Medical equipments per health establishment _{<i>m,t-1</i>}			-0.048 (0.041)							
Hospital beds per health establishment _{<i>m,t-1</i>}				-0.001 (0.614)						
% of schools with library _{<i>m,t-1</i>}					7.679 (7.865)					
% of schools with science lab _{<i>m,t-1</i>}						5.452 (6.322)				
% of schools with internet _{<i>m,t-1</i>}							36.768 (32.468)			
Tax revenues per capita _{<i>m,t-1</i>}								-28.876 (26.190)		
Tax revenues net of transfers per capita _{<i>m,t-1</i>}									-8.717 (8.319)	
% of property tax _{<i>m,t-1</i>}										-0.049 (0.052)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep.var	1.554	1.649	1.962	1.531	1.630	1.630	1.607	1.576	1.576	1.544
Observations	89524	146821	42449	88031	130576	130576	120370	130369	130337	129806
R-squared	0.932	0.829	1.000	1.000	0.832	0.832	0.843	0.844	0.843	0.848

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates. Dependent variable is the the mean distance to the nearest labor office in year *t*. Local GDP, population, hospital beds and equipments, and taxes variables are in log levels. 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). Taxes variables are sourced from Instituto de Pesquisa Econômica Aplicada (IPEA). Our analysis is limited to schools and health establishments that are managed at the municipal level. Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects.

data limitations lead to a reduction in the number of observations, the results remain robust and, if anything, exhibit even stronger magnitudes.

Third, we explore whether 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 determine whether our results are driven by coattail effects or political pressure from state-level politicians, we first exclude candidates from the governor's party and co-partisans of the president from our sample. As shown in Tables A.12 and A.13, the results remain robust, suggesting that these factors do not explain our findings. Additionally, in Table A.14, we control for whether the incumbent governor at the time of each mayoral election is clientelistic. The results remain

Table 11: 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)
Enforcement _{$m,c(t)$}	-0.391*** (0.075)	-0.356*** (0.069)	-0.300*** (0.070)	-0.223*** (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. 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.2.

virtually unchanged, further strengthening our conclusions.²⁵

6.2 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 analyses reinforce the evidence that our results capture a genuine reduction in clientelism and a broader shift toward programmatic governance.

Alternative Dependent Variables. Our baseline specifications measure clientelism as a dichotomous indicator derived from expert-survey scores. As an initial robustness check, Table A.15 replaces this indicator with the original continuous clientelism scores. The point estimates and levels of statistical significance remain virtually unchanged, reinforcing the credibility of our core findings.²⁶

We next turn to municipal-council races. Prior work shows that council candidates frequently serve as brokers for their party's mayoral ticket (Novaes 2018). If stronger labor-law enforcement blunts the effectiveness of clientelism, it should therefore erode electoral support for council candidates fielded by clientelist parties as well. Table A.16 confirms this expectation: tighter enforcement leads to a significant drop in those parties' council vote

²⁵Election year fixed effects account for the level of clientelism of the sitting president.

²⁶Variable definitions are provided in Appendix OA.2. Consistent with the treatment of the binary measures, missing values are imputed with the minimum observed score.

shares. The result suggests that enforcement weakens clientelistic networks not only at the executive level but across the wider arena of local governance.

We also account for electoral coalitions. Under Brazilian electoral rules, each candidate must be formally nominated by a single party, but parties can—and routinely do—form coalitions in mayoral contests. 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 11, our results remain robust under this specification.

Finally, we investigate whether our estimates might be driven by lower participation rather than by shifts in voter choice. If tougher enforcement merely discouraged clientelist supporters from turning out, overall turnout should fall. We observe the opposite: municipalities exposed to stronger enforcement register higher turnout (Column 1, Table A.17), a pattern consistent with a decline in “abstention-buying”—the practice of paying supporters to stay home (e.g., Gans-Morse et al. 2014). Higher participation, however, could itself explain the erosion of clientelist parties’ vote share. To rule this out, we re-estimate our baseline specifications while conditioning on turnout (Columns 2–5, Table A.17). The enforcement coefficient remains large and statistically significant, indicating that the observed reduction in clientelism reflects genuine changes in voter preferences rather than mere shifts in the size of the electorate.

Alternative Definition of Enforcement. Our baseline analysis quantifies enforcement as the number of inspections per 100 firms, consistent with Almeida and Carneiro 2012 and Ponczek and Ulyssea 2022. Recognizing that the number of firms could be influenced by labor law enforcement over time, we demonstrate the robustness of our findings by presenting results using inspections per capita in Table A.18.

Addressing Missing Clientelism Scores. As detailed in Appendix OA.2, the expert surveys used to classify parties by their level of clientelism cover approximately 92% of mayoral candidates.²⁷ 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.19, 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

²⁷Refer to Table A.27 for a complete list of parties and their respective clientelism scores, including those with missing values.

as clientelist (Panel B). These findings confirm that our conclusions do not hinge on how missing clientelism scores are handled.

Alternative Definitions of Programmatic Party. Our hypothesis suggests that stronger enforcement reduces clientelism and boosts programmatic parties. To further validate this proposition, we explore restrictive 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.²⁸ 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.²⁹

A further concern is that our coding scheme—based on national party labels—could misrepresent the tactics of individual candidates who deviate from their party’s country-wide stance. To guard against this, we tighten the definition of “programmatic” parties, limiting the category to organizations whose national platforms and reputations leave little room for local candidates to stray from programmatic norms. Recent research emphasizes that understanding party strategies requires looking beyond voter-party linkages to also consider critical dimensions of party organization and resource structures (Aspinall et al. 2022; Berenschot and Aspinall 2022; Hicken and Nathan 2020): (1) the degree of party centralization, (2) organizational capacity, and (3) sources of funding.

First, centralization shapes candidates’ ability to use clientelism. Decentralized parties grant local elites autonomy (Mainwaring 1999), while centralized ones enforce programmatic discipline (Morgenstern 2003; Sells 2020). We measure centralization by the extent to which national leaders control candidate selection and strategy. Second, organizational strength also limits candidate autonomy. Strong parties maintain permanent local offices and deploy staff active year-round. Third, funding sources further constrain behavior. For instance, parties that rely on candidates to fund campaigns allow them to bypass leadership controls (Yıldırım and Kitschelt 2022; Hale 2005). We use V-Party data to capture whether candidates are the main financial contributors.

Using data from the DALP and V-Party datasets (Kitschelt et al. 2013; Lindberg 2022), we classify a party as programmatic if it is non-clientelistic and meets at least one of three criteria: centralized, organizationally strong, or party-funded.³⁰ Each variable is normalized to a [0,1] scale and weighted by a binary indicator of programmatic status.³¹ The results

²⁸See Appendix OA.2 for the precise definition of this variable.

²⁹The p-value for the estimated coefficient is 0.105.

³⁰For detailed descriptions of the corresponding survey questions, see Appendix OA.2.

³¹Programmatic parties are identified using a binary DALP-based measure of non-clientelism.

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

	(1) Saliency of economic issues	(2) Programmatic and centralized candidate 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
Enforcement _{<i>m,c(t)</i>}	0.022 (0.013)	0.141** (0.066)	0.111** (0.050)	0.125*** (0.047)	0.035 (0.045)	0.065** (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. 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.2.

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 identified 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 stronger enforcement merely boosts programmatic parties' vote shares because clientelist politicians jump ship and run under programmatic labels. Two considerations make this unlikely. This is unlikely to pose a concern for two reasons. First, party switching in Brazil tends to occur among ideologically fungible, clientelist parties that impose few exit costs. By contrast, programmatic parties—exemplified by the Workers' Party (PT)—maintain distinctive ideological platforms, cohesive organizations, and well-established brands, which together discourage opportunistic defections (Sells 2020; Ames 2005; Mainwaring 1999; Samuels 2006; Desposato 2006). Second, even if clientelist politicians did migrate to programmatic parties, such moves would

themselves signal shifting electoral incentives—namely, the growing appeal of programmatic labels and the diminishing effectiveness of clientelistic strategies under heightened enforcement. We put this possibility to a tougher test by re-classifying programmatic parties using a stricter criterion—restricting the category to organizations with highly centralized structures and tight control over local nominations (Table 12). The core results are unchanged. Moreover, municipalities exposed to greater enforcement show clear declines in vote-buying and patronage, alongside increases in public-goods provision, further indicating that the observed electoral shifts reflect genuine programmatic realignment rather than cosmetic rebranding by clientelist politicians.

6.3 First-Stage Robustness

Linear specification. 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.20 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.

Year-level specification. 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.21.

Alternative Measures of Distance. 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 A.22. The robustness of our findings is further confirmed by the use of Euclidean distances, as shown in Table A.23. 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 A.24.

6.4 Alternative Explanations

In this section, we address several potential confounders that could influence the interpretation of our findings.

State-Level Dynamics. A remaining worry is that clientelistic tactics may depend more on state-level political dynamics than on 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.11, 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.

The Role of the PT. A further threat to identification arises from the policy leverage of the *Partido dos Trabalhadores* (PT), a highly disciplined national organization with deep local roots. 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 influence. 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.25, 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. Yet, another possibility 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.25, 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.

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
Enforcement _{$m, c(t)$}	0.045 (0.302)	0.265 (0.171)	−0.011 (0.021)	−0.023 (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. 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.2.

Differentiating Clientelism from Ideology, Personalism, and Populism. A final validity check asks whether our clientelism measure inadvertently captures other partisan attributes—specifically left-right ideology, personalism, or populism. Table 13 addresses each possibility. Columns 1–2 use DALP and V-Party ideological scores to predict the ideological position of winning parties. Enforcement has no discernible effect, consistent with Brazil’s pattern of ideologically similar parties adopting very different linkage strategies.³² Column 3 shows that stronger enforcement does not reduce support for candidates from personalist parties, reinforcing the idea that personalism relies on charisma and social ties rather than on material inducements likely to be affected by labor-law oversight (e.g., Sells 2020; Lyne 2005; Kitschelt 2000). Finally, Column 4 links enforcement to populism scores from the V-Party dataset and again finds no systematic relationship, underscoring the conceptual distinction between clientelism and populism.³³

Labor Market Dynamics. We argue that enforcement reduces clientelism mainly by decreasing informal labor. However, our results also show that it is linked to a decline in non-workers. If non-workers disproportionately supported clientelistic parties, their declining share could explain the observed effects instead of changes in informality. However, we

³²For example, both PDT and PSB are left-of-center (scores 3.5 and 3.1, respectively), yet only PDT is coded as clientelistic. On the right, PTB and PSDB register nearly identical scores (6.0 and 6.1) but differ on clientelism.

³³Variable definitions are provided in Appendix OA.2.

argue this is unlikely, as non-employment includes job seekers, retirees, students, and benefit recipients—many of whom are not economically vulnerable. We test this with the Brazilian Electoral Panel Study (BEPS), replicating the analysis from Table 9 while adding both non-workers and the unemployed. As Table A.26 shows, these groups are no more likely than formal workers to back clientelist parties. A smaller non-working population therefore cannot explain the observed decline in clientelism.

7 Final remarks

Many developing countries face a weak state trap, where low state capacity and entrenched clientelism reinforce each other, undermining accountability, political competition, and public goods provision (Fergusson et al. 2022a). While prior research has shown how clientelism obstructs state-building (De La O 2024; Fergusson et al. 2022b), we know far less about whether investments in state capacity can, in turn, reduce clientelism.

We examine this question by exploiting the centrally led rollout of Brazil’s labor-inspection system—an initiative largely shielded from local capture. Using geographic and temporal variation in office openings, instrumented by distance to the nearest labor office and inspector allocations, we find that tougher enforcement sharply reduces support for clientelistic parties, curbs vote-buying and patronage, and improves public goods provision.

Labor-market formalization is the key mechanism. Enhanced enforcement leads to higher formal employment and lower informality—shifts that matter politically, as informal workers are more likely to support clientelistic candidates (Bobonis et al. 2022; Rains and Wibbels 2023; Gottlieb 2024). These results highlight how centralized state interventions can promote programmatic politics and strengthen democratic responsiveness.

Whether these gains persist is less certain. Enhanced enforcement could spark a virtuous cycle, whereby greater capacity and formalization builds trust in institutions, which further discourages clientelism (Bustikova and Corduneanu-Huci 2017). Yet clientelist networks may adapt, devising new inducements for a changing electorate. Thus, while early effects are promising, their durability depends on whether the state can sustain its lead over such political innovation.

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