Left for Dead:

The Electoral Cost of Work-Related Fatalities in Brazil

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Worldwide, more than two million workers are killed each year in work-related accidents. In this paper I investigate the electoral effects of workplace accidents in the Brazilian labor market—one of the most dangerous in the world. Do voters punish incumbent parties for preventable work-related accidents, such as dust explosions and factory fires? Are leftist incumbents punished more than other parties? Or, on the contrary, do voters give more support to the left in hopes of greater intervention in labor relations? I develop a model where voters evaluate government officials' success in regulating labor markets (both lowering unemployment and improving working conditions) according to the expectations that leaders and party labels set in advance. Using monthly data from death certificates issued between 1996-2012, I show that workplace fatality shocks substantially decrease the local incumbent party's vote share, vote change (from one election to the other), and re-election probability—but only for the party of the left.

Better working conditions and workplace safety have often come at the great cost of human lives, with large industrial disasters being followed by governments improving labor standards. A keystone moment is the fire at the Triangle Shirtwaist Factory in New York City. In the evening of March 25th, 1911, 146 garment workers—mostly poor immigrant women, working nine hours a day, six days a week—died from the fire, smoke inhalation, or jumping to their deaths. Nine days before the Triangle Fire, the *New York Call*—a socialist daily newspaper—had published excerpts from a report of the New York Joint Board of Sanitary Control describing the conditions at the garment industry: 99% of the shops lacked fire escapes, drop ladders, emergency exits, and fire drills.[1]

Before the fire, the Democratic Party had supported the real-estate and manufacturing industries' resistance to improvements in the workplace. The fire generated scuffles within the party over responsibility for the accident and widespread public outrage with local and state officials, fueled by the Women's Trade Union League (a middle-class organization) and the International Ladies' Garment Workers' Union (a working-class organization of immigrant female workers).[2] After the fire, the Tammany Hall machine supported laws mandating occupational safety conditions, minimum wages, maximum hours, and the end of child labor.[3] Key in this about-face of the Democratic Party was the fear of electoral defeat. Should widespread outrage accelerate, working-class voters could defect to the Socialists, and middle-class voters to Roosevelt's Progressives.[4]

Current labor conditions in developing countries do not differ much from those in New York City at the turn of the 20th century, as evidenced by the 2013 Savar building collapse, in Bangladesh. Worldwide, 270 million workplace accidents occur annually, according to estimates from the International Labour Organization (ILO). Roughly two million of those result in death.

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[1] Leon Stein, The Triangle Fire (New York: A Carroll & Graf/Quicksilver Book, 1962), pp. 26-28.

[2] See *New York Times*, "Blame Shifted On All Sides For Fire Horror," 1911.

[3] Governor John Alden Dix established the New York State Factory Commission and promulgated 32 worker safety bills.

[4] See Von Drehle (2003) and Combing the Ashes of Another New York Disaster.

Identifying the conditions under which politicians are responsive to voters is a central concern of political scientists, particularly in developing countries, where voters "rely in large measure on state action for their survival" (Besley and Burgess 2002). A vast number of scholarly studies show that elections provide strong incentives for better governmental performance, and that voters punish incumbents following negative events, sometimes even irrationally. The Triangle Fire is just one example of how fear of electoral punishment can promote deep transformations in public goods provision.

This paper asks the following questions: Do voters punish incumbent parties when workers die in dust and methane explosions, factory fires, mine disasters, and the like? Are leftist incumbents punished more for failing to prevent these events or, on the contrary, do voters give more support to left parties, in search of more interventionism in labor relations? Finally, is electoral punishment sufficient for democratic accountability? I examine these questions in Brazil, the country with the third highest number of work-related fatalities in the world (ILO 2010).

It is possible that voters punish left parties more because they judge parties according to the expectations that parties set in advance. Politicians set expectations (e.g., Republicans will lower inflation, Democrats will lower unemployment). Voters observe a party in office (say Republican), compare its performance with the expectations they had about the party (it will lower inflation), and then punish or reward the party based on whether it delivered on its promises. This is the framework suggested by Powell Jr and Whitten (1993):

> In general, observers—and voters, we think—expect left-wing governments to deal better with unemployment and short-term economic stimulation, while they expect right-wing governments to deal better with inflation. If voters are assessing governments retrospectively, judging their performance in office, they might well hold right-wing governments to a higher standard on inflation and be less concerned about unemployment. Vice versa, for left-wing governments.

> Moreover, the core constituencies of right-wing governments are more likely to be those who care more about inflation, while the core constituencies of left-wing governments are more likely to be those who care about unemployment (p. 404)

It is also possible that, after occupational risks become evident for the general public, voters will favor candidates that run on a pro-workers platform, on the expectation that these candidates will improve working conditions, regardless of whom the incumbent is. Some parties "own" issues; thus, when those issues become more salient, voters will turn in their support for these parties (Petrocik 1996). For instance, Wright (2012) shows that Democrats have a marketing advantage as the party best able to handle unemployment. Thus, Democratic electoral fortunes rise and fall with rising and falling unemployment, regardless of whether they are in power or not.

The role of expectations is crucial, albeit rarely studied systematically (for a recent exception, see Malhotra and Margalit 2014). Campaign platforms and party labels convey information to voters about candidates' likely behavior in office and voters can use this information to evaluate the performance of incumbents retrospectively or prospectively.

To the best of my knowledge, the question on whether an electoral punishment is sufficient to hold politicians accountable has not been systematically studied. As mentioned, large industrial disasters have triggered government action to improve conditions and workplace safety in many regions of the world. Yet weak labor standards remain the norm in most parts of the developing world. What motivates governments to improve labor conditions? In my dissertation, I claim that leftist incumbents reduce enforcement to garner support from two constituencies with opposing interests in the labor market. These two groups are sometimes called incumbent and new entrant workers, formal and informal, or insider and outsider workers. In the following months, I plan to study this "electoral disconnection."

Case Study

Brazil has 5,564 municipalities. Municipal governments play a central role in governance: they are in charge of collecting taxes and providing several public goods and services, including education and health services. Although the federal government is formally responsible for enforcing labor law, several studies show that municipal governments play a key role in shaping labor standards.[5] In addition, all businesses and firms must register with the municipal tax agency and obtain an operation permit. Municipal governments also supervise the operation of particular sectors. For instance, they supervise construction projects: they examine practical and installation aspects, issue equipment-operating permits, and oversee that the construction conforms to municipal legislation, zoning laws, and the municipal building code.

Mayors are elected by popular vote. Elections typically take place the first or second week of October. If the district has more than 200,000 voters, then the election is held under a runoff system (this also applies to state and national executive elections). Otherwise, mayors are elected through plurality rule. All executive offices are elected every four years. Elections for the president, governors, and national legislators take place simultaneously, while municipal elections are staggered by two years.

In this paper, I analyze results for all incumbent parties and then focus on two parties, the Workers' Party (PT) Brazilian Party for Social Democracy (PSDB), which have a broad participation in local elections and have relatively consistent ideological stances, with the PT tilting to a pro-workers left and the PSDB tilting to a technocratic right.[6] In Brazil, parties at the local level are typically portrayed as weak, patronage-oriented, and personalistic organizations, with the Workers' Party (PT) being the exception. By all accounts, the PT is the archetypal strong organization—a party with a coherent program for social change, a disciplined membership, and deep roots in civil

[5] (Almeida; Coslovsky; Feierherd; Pires; Tendler)

[6] For presentation purposes, most of the empirical analysis of the PSDB is relegated to the appendix.

society. The PSDB, in turn, is best described as a "loosely organized federation of regional leaders" that relies on these leaders' personal popularity (Samuels and Zucco 2013, p. 4).

Data

I use two sources of data to measure how many workers die from work-related accidents in Brazil. The first data comes from the Brazilian Social Security Agency (INSS). This data is available for every year at the municipal level since 2002 and covers accidents suffered by *insured* workers, in the formal sector, both private and public, under de *Consolidação das Leis do Trabalho* (CLT) system. This tally also includes commuting deaths, which have less to do with worksite conditions and more with the overall quality of infrastructure in the municipality (although workers rushing to work is frequently considered a labor market problem). Epidemiologists, public health specialists, and economist all agree that this dataset is the most reliable for measuring labor accidents in the *formal* sector.

This data, however, has two problems. The first problem is that the data is aggregated at the year level and, as mentioned, elections typically occur in October; therefore, measuring workplace accidents for the whole year means that the independent variable may be partly endogenous (i.e., it will include accidents taking place after the election—but before the winner of the election takes office). Similarly, because it includes accidents happening early in the year, the indicator may also have an impact on the decision of incumbents to run for re-election. More importantly, about half of the workers in Brazil are informal (either self-employed or in informal jobs) and thus are not counted for social security benefits. Therefore, the number of fatal accidents is clearly underestimated in this dataset.

To deal with these problems, I collected monthly death tallies at the municipal level since 1996 from death certificates (SIM). The data is recorded by doctors, who must report health agencies about the death causes. The SIM uses the ICD-10 medical classification list

by the World Health Organization (WHO), which includes 14,400 different codes for diseases, social circumstances, and external causes of injury or death. Doctors are supposed to report whether the injury or death was work-related but they rarely do so. According to a RAND report, "SIM dramatically undercounted fatalities because [doctors] usually omitted information pertaining to work-relatedness" (2014, p. 66).

Instead, I coded a death as work-related based on both the classification code and the information provided by doctors on work-relatedness.[7] I coded the 373 accident categories as either more likely or less likely to be work-related; for those coded as more likely to be work-related (e.g., "contact with agricultural machinery," "explosion and rupture of gas cylinder"), I included all death tallies where doctors either declared the death as work-related or did not supply any information, and excluded those cases where doctors said it was not work related. For those coded as less-likely to be work-related (e.g., "discharge of firework," "contact with venomous snakes and lizards"), I only included death tallies where doctors said the death was work-related. The municipal-level correlation of the two datasets (INNS and SIM) for work-related deaths is .85. The total number of deaths differs tremendously, with data from SIM approaching the estimation made by Giufridda et al. (2003).[8]

Identification Strategy

Obviously, workplace fatalities do not happen as-if randomly: some municipalities have unions that work along workers and firm managers to improve health and safety

[7] For more details, see "ICD-10 Chapter XX: External causes of morbidity and mortality."

[8] Giufridda et al. (2003) make two calculations for 1998, when 3,800 deaths at the workplace were formally recorded. Their conservative estimate assumes that a) this number constituted 100% of the deaths in the formal sector; and b) the formal sector accounted for a third of total exposures; and c) the average riskiness of the two sectors were equal. Then, it would be reasonable to triple the number of deaths reported, to 11,400. Their "less conservative" estimate, in turn, assumes that a) only 50% of formal sector deaths were reported and b) that the true risk in the informal sector was 50% higher than in the formal sector. Those assumptions would generate an estimated total for Brazil in 1998 of 7,600 in the formal sector. Then the informal sector would be assumed to have two times as many workers and a rate 50% higher, for a total of 3 times as many deaths, or 22,800. Thus the high estimate for Brazil as a whole in 1998 would be 30,400.

conditions; some municipalities are more wealthy and have better enforcement agencies, healthcare systems, and the like. To identify the electoral effects of workplace fatalities, I exploit both year and month variation in the number of dead workers around local elections. The strategy presented here uses a difference-in-differences design, using fixed effects for year/municipality, combined with a plausibly exogenous "shock"—a spike in the number of accidents in the run-up to the election, relative to the number of accidents during the same period, one year before the election. As one inspector noted during my fieldwork in Brazil,

> Every accident has multiple causes. One day, something changes, the wind, the rain, the operator who is a little tired, the machine with a certain problem. It is not one single cause, but a succession of problems and mistakes.

This means that, although some places may have better labor conditions than others, the timing of accidents, after controlling for overall working conditions and time trends, may be exogenous to both the overall level of working standards and political conditions.

To evaluate the effect of workplace fatal accidents, I construct two "shock" indicators. The first measure compares the number of workplace fatalities in the election year with the number of fatalities in the year before the election, using the INSS (formal workers) and SIM (formal and informal workers) data. For the indicator constructed with the INSS data, I use the variation across years. For the SIM data, I sum the number of casualties in the three months before the election (July, August, September), and compare that number with the number of casualties during those same months, one year before the election. Municipalities that double the number of work-related deaths during an election year are "treated"; those with less than double are coded as the "control" group; all the rest are coded as missing.

$$\text{Shock1}_{i,t4} = \begin{cases} 1 & \text{if } Fatalities_{i,t4} > 2 \cdot Fatalities_{i,t3} \\ 0 & \text{if } Fatalities_{i,t4} < 2 \cdot Fatalities_{i,t3} \\ . & \text{if } Fatalities_{i,t4} = Fatalities_{i,t3} \end{cases}$$

Circumscribing the period to the run-up to the election is relevant for at least two factors: an incumbent party may decide not to run in the first place if it perceives that its chances are diminished by events that occur before the official registration of candidates. Also, voters will weight events occurring just before the election day more heavily than those than happen at the beginning of an electoral year.

The second indicator mimics the first one, with one variations: I define a municipality as "treated" if, conditional on experiencing at least one fatal accident, either over the year (INSS) or during the three months (SIM) before an election, and the year or previous year months, a municipality experienced a double of casualties.[9]

$$\text{Shock2}_{i,t4} = \begin{cases} 1 & \text{if } Fatalities_{i,t4} > 2 \cdot Fatalities_{i,t3} \\ & \text{and } Fatalities_{i,t4} > 0 \text{ and } Fatalities_{i,t3} > 0 \\ 0 & \text{if } Fatalities_{i,t4} < 2 \cdot Fatalities_{i,t3} \\ & \text{and } Fatalities_{i,t4} > 0 \text{ and } Fatalities_{i,t3} > 0 \\ . & \text{if } Fatalities_{i,t4} = Fatalities_{i,t3} \end{cases}$$

As an additional test, I also estimate the effect of a percentage change ($Change_i$) in the number of workplace fatalities from one period to the other, using both datasets:

[9] This procedure is similar to the one used by Marshall (2016), with two differences: first, I compare the number of deaths using the same time-period, during the election year and one year before the election, because workplace fatalities may exhibit seasonal trends. Second, I define a municipality as receiving a shock if it *doubles* the fatality rate. Marshall, instead, codes any increase as a shock.

$$\text{Change}_{i,t4-t3} = \begin{cases} (Fatalities_{i,t4} - Fatalities_{i,t3})/Fatalities_{i,t3} & \text{if } Fatalities_{i,t3} \neq 0 \\ . & \text{if if } Fatalities_{i,t3} = 0 \end{cases}$$

For the party-specific samples (i.e., when the dependent variables refer to the performance of the PT or PSDB), I include a variable indicating whether the incumbent mayor belongs to any of these parties and its interaction with the $Shock_i$ or $Change_i$ variables.

Exogenous shocks?

The key identifying assumption is that, within a municipality, a sudden increase in work-related fatalities is effectively random. The difference-in-differences approach controls for within-municipality variation for all time-invariant municipal-level characteristics and also controls for factors that change over time but remain constant within municipalities. Although the timing of work-related accidents seems highly idiosyncratic (as suggested by the inspector's quote above), this does not imply that fatality shocks happen by chance.

Figures 1 to 4 show balance tests for the four "shock" indicators—two for each measure ($Shock1_{i,t4}$ and $Shock2_{i,t4}$) and dataset (INSS and SIM). The first two figures show balance tests for 17 pre-treatment covariates for $Shock1_{i,t4}$ and $Shock2_{i,t4}$ using the INSS data. Both shock measures are uncorrelated with a wide variety of pre-treatment covariates capturing demographic, socioeconomic, and political outcomes. Some of these variables capture levels (e.g., the number of workers that died the year the incumbent mayor took office) while other capture variation from the year before the election to the year of the election (e.g., local GDP growth). The shocks seem to be unrelated to the number of inspections received by a municipality in the year the mayor took office, the number (and yearly change) of labor fines received by firms, the municipal-level GDP, GDP growth, population, and size of the service, agricultural, and industrial sector. For the

Shock1$_{i,t4}$ measure, only one covariate is statistically significant at the .05 level, consistent with chance. The variable indicating that the municipality is governed by a PT mayor also shows a slight imbalance at the .1 significance level, with municipalities governed by the PT being more likely to experience a shock.[10] For the Shock2$_{i,t4}$ measure, none of the variables are statistically significant.

Figures 3 to 4 show balance tests for the SIM dataset. Again, Shock2$_{i,t4}$ shows a better performance in terms of balance, with only one variable (the number of parties competing the election won by the incumbent) showing a small imbalance. Shock1$_{i,t4}$ does show some imbalance in terms of the size of the agricultural sector, GDP growth, and PT mayor, but, again, these could be due to chance. Overall, treated and control municipalities appear, on average, very similar to one another.

[10] Although this may introduce some bias when analyzing the incumbent sample, it should not bias the results when comparing the effect of the shock only across PT-governed municipalities.

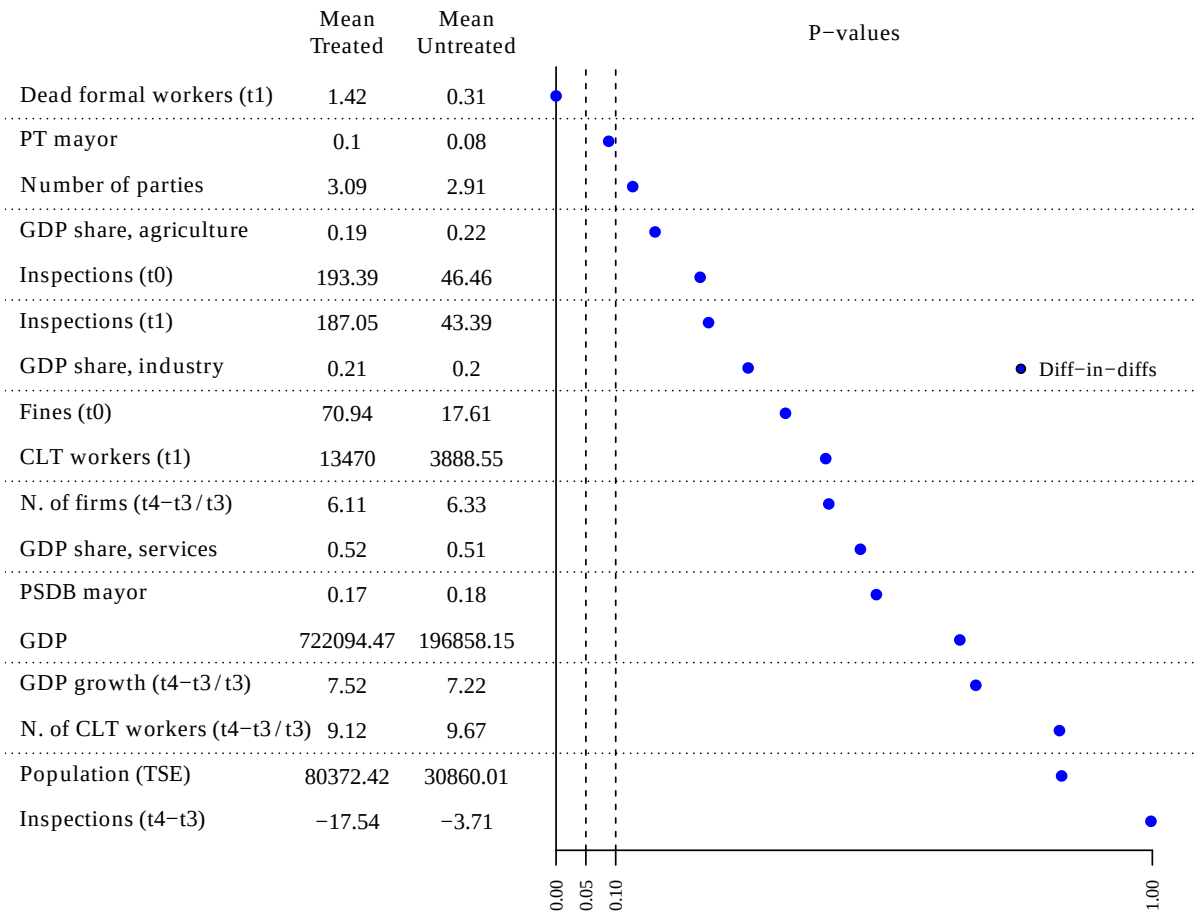

Figure 1: INSS data: p-values for pre-treatments variables on $\text{Shock1}_{i,t4}$. All specifications include municipality and year fixed effects, and are estimated using OLS.

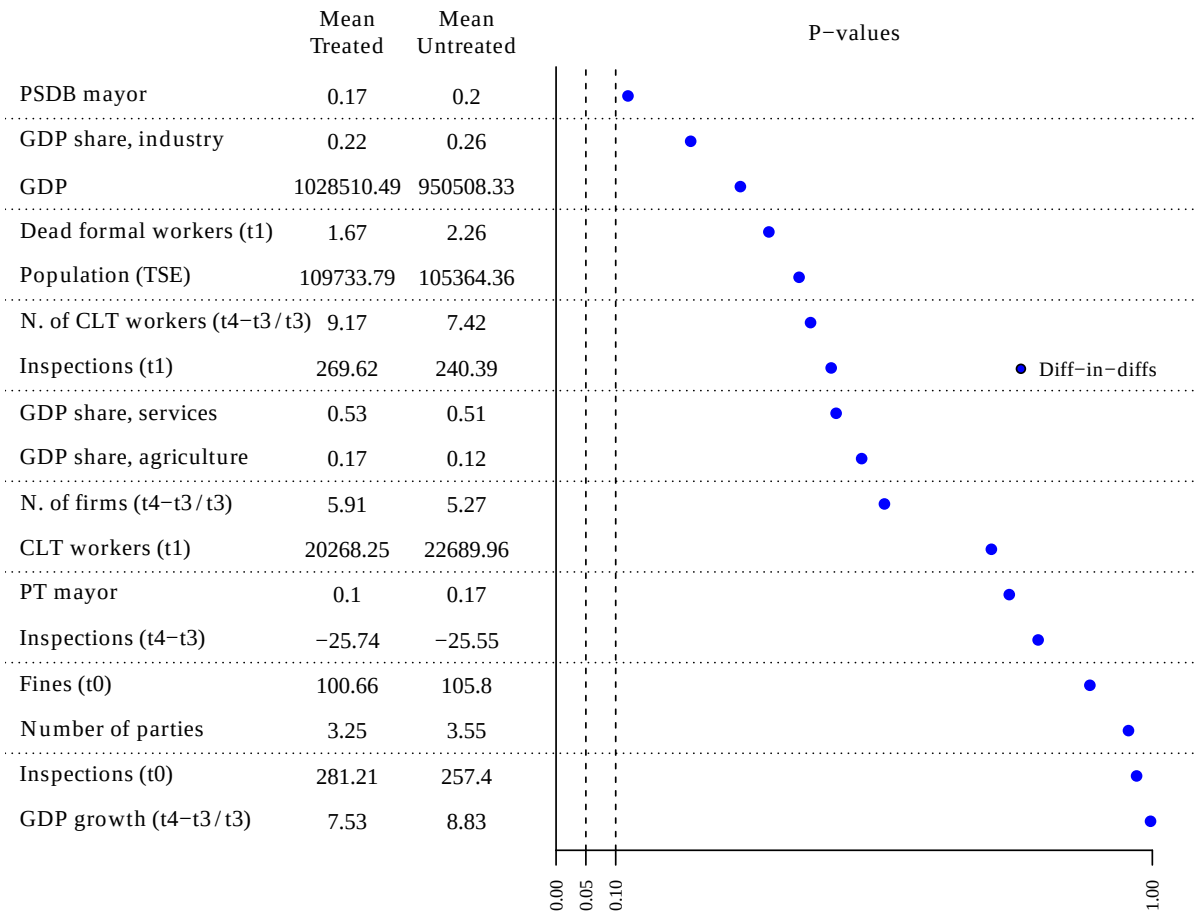

Figure 2: INSS data: p-values for pre-treatments variables on $\text{Shock2}_{i,t4}$. All specifications include municipality and year fixed effects, and are estimated using OLS.

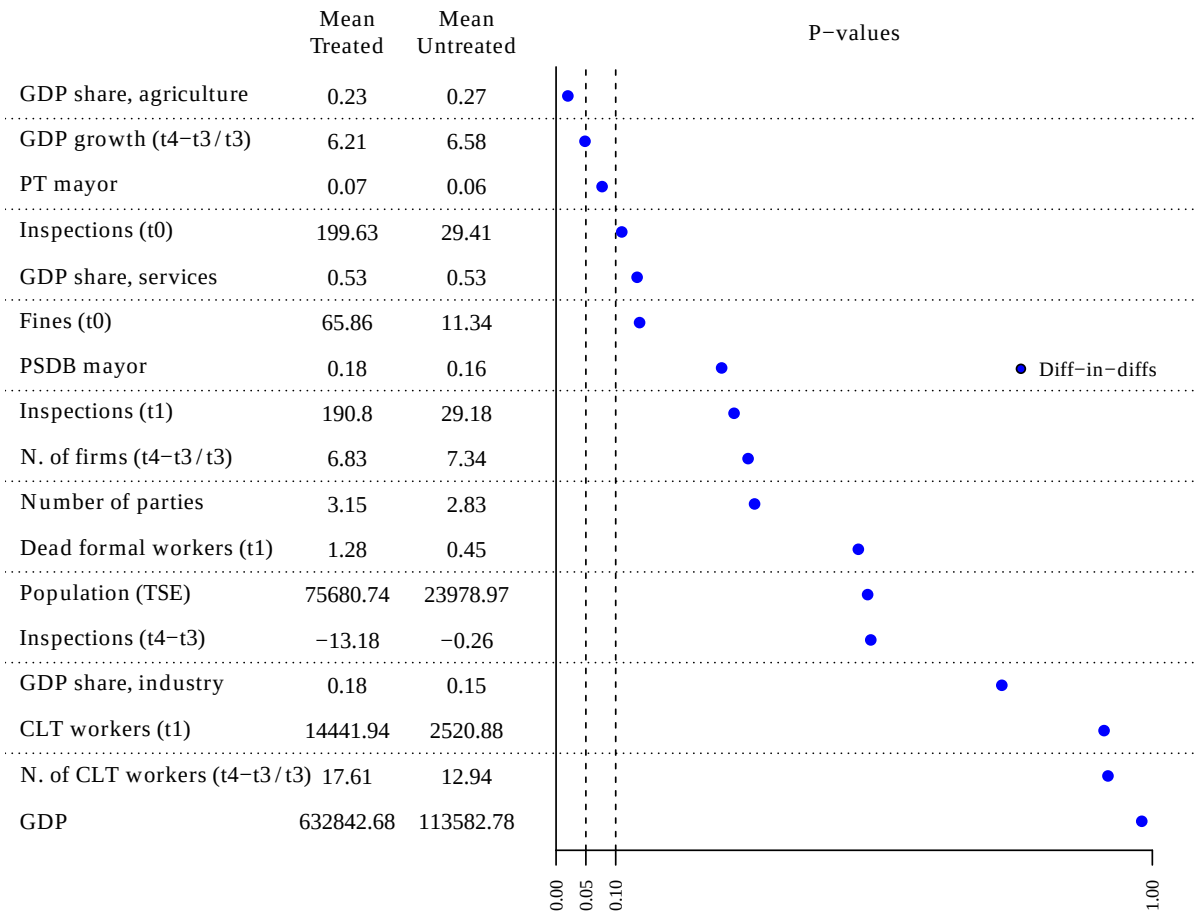

Figure 3: SIM data: p-values for pre-treatments variables on Shock1$_{i,t4}$. All specifications include municipality and year fixed effects, and are estimated using OLS.

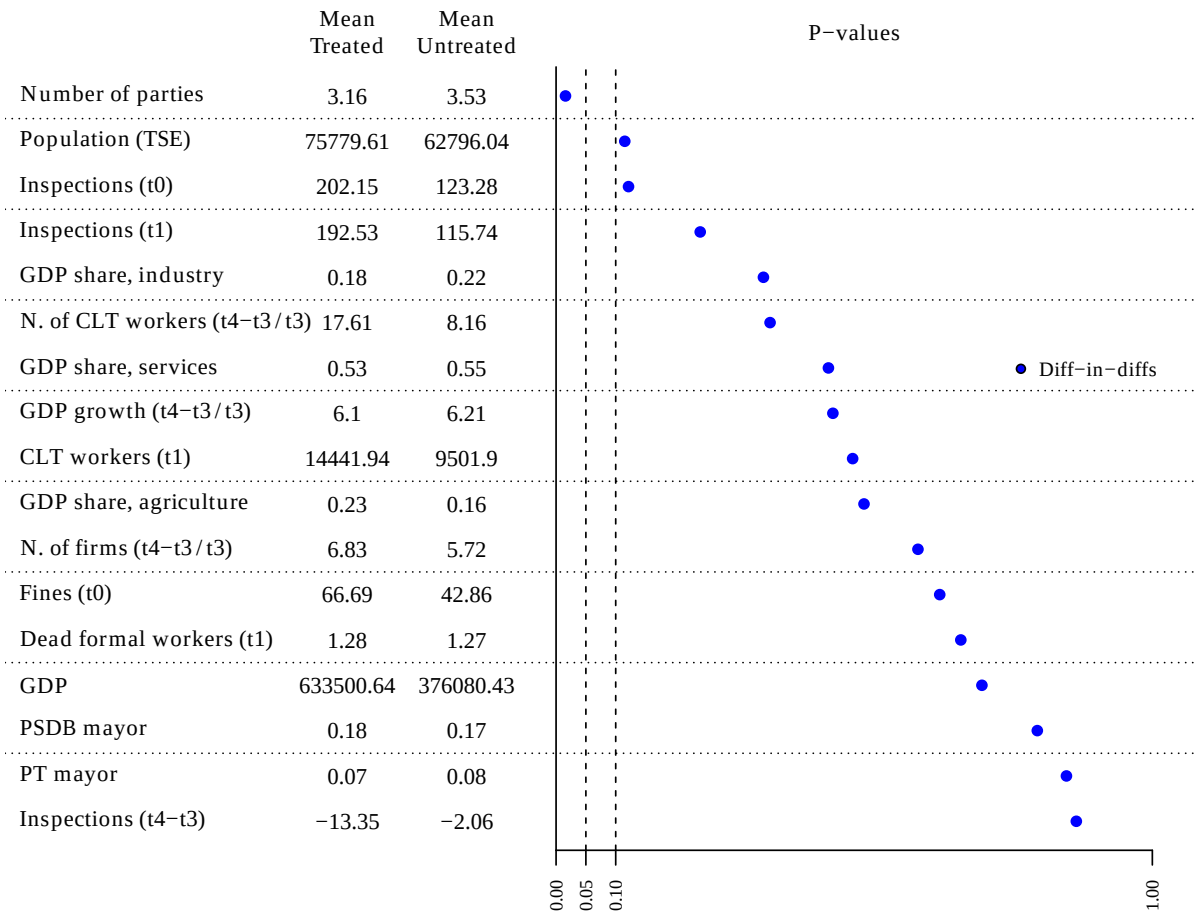

Figure 4: SIM data: p-values for pre-treatments variables on $\text{Shock1}_{i,t4}$. All specifications include municipality and year fixed effects, and are estimated using OLS.

Time trends

Forthcoming.

Dependent variables

I construct three dependent variables: the vote share of the incumbent party in the municipal election (for municipalities where the incumbent does not run, I code the electoral outcome as zero); whether the incumbent party wins the next election (again, when the party does not run, I code the variable as zero—i.e., the party looses); and the change in the vote share of the incumbent party in the current election, with respect to the following election. I replicate this analysis for the PT and the PSDB to evaluate whether these parties do better or worse in places where they are the incumbent party. Thus, for each party, I construct three dependent variables: the vote share for the PT (PSDB) in the municipal election; whether the PT (PSDB) wins the next election; and the change in the vote share of the PT (PSDB) in the current election, with respect to the previous election. (For the party-specific samples, if the party does not run in either election, the variable takes a value of zero; if the party wins 50 per cent of the vote in each election, the variable also takes the value of zero).

Results

Does an increase in fatal workplace accidents affect the reelection chances of incumbents? Do voters judge incumbent parties differently, according to the expectations that parties set in advance? This section presents results for the incumbent sample and the PT sample. Results for the PSDB largely resemble those of the incumbents sample and thus are relegated to the appendix.

INSS data (formal workers)

Table 1 shows difference-in-differences results for the incumbent party sample. Models 1 to 3 estimate the effect of the $\text{Shock1}_{i,t4}$ indicator; models 4 to 6 use the $\text{Shock2}_{i,t4}$ variable; models 7 to 9 use the $\text{Change}_{i,t4}$ variable. The main conclusion is that an increase in the number of fatal work accidents (in the formal labor market) does not hurt the electoral chances of incumbent parties.

Table 2 shows that the PT fared worse in municipalities governed by a PT mayor that experienced an increase in the number of workplace fatalities during the electoral year (compared to the year before the election): for the $\text{Shock1}_{i,t4}$ variable, the PT vote share decreased in almost 10% points (when compared to municipalities that did not experience a shock), its probability of being reelected decreased in 13%, and the party got 14% less votes compared to the previous election. Results are even more pronounced for the $\text{Shock2}_{i,t4}$ measure. The PT suffered a penalty of around 14% points in municipalities that suffered a shock, and reelection changes dropped in almost 40%. Compared to the previous election, the PT incumbent suffered a penalty of around 11% points, but the coefficient is not statistically significant at conventional levels. Similar results are obtained using the $Change_{i,t4}$ measure (negative and statistically significant—with the exception of Model 7, where the coefficient is negative but fails to achieve statistical significance). An increase in 10% in the number of occupational fatalities decreases reelection chances for the PT in almost 5% points. The unconditional effect of a PT mayor on PT's electoral performance in the following election are negative and statistically significant for 6 out of 9 models, consistent with the literature on incumbency disadvantage in Brazil.

The PSDB did not experience similar effects: coefficients are negative but close to zero and statistically insignificant (despite lower standard errors in models 2 and 3; see the appendix).

Table 1: The effect of workplace deaths on municipal electoral outcomes (incumbent sample, INSS data)

	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**
$\text{Shock1}_{i,t4}$	−0.087	−0.020	0.555						
	(1.055)	(0.020)	(1.198)						
$\text{Shock2}_{i,t4}$				−1.390	−0.038	−1.932			
				(2.900)	(0.054)	(3.304)			
$\text{Change}_{i,t4}$							0.032	−0.0003	0.168
							(0.309)	(0.004)	(0.352)
N	6300	6038	6248	3100	3006	3083	10657	10244	10460
R-squared	0.005	0.008	0.011	0.008	0.016	0.017	0.007	0.007	0.012

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table 2: The effect of workplace deaths on municipal electoral outcomes (PT sample, INSS data)

	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**
PT mayor	0.576	−0.228***	−33.010***	3.957*	−0.155***	−27.740***	5.263***	−0.060***	−30.760***
	(1.786)	(0.035)	(2.372)	(2.392)	(0.048)	(3.168)	(1.139)	(0.023)	(1.471)
$\text{Shock1}_{i,t4}$	−0.179	−0.001	−0.493						
	(0.633)	(0.011)	(0.828)						
$\text{Shock2}_{i,t4}$				2.133	0.022	1.096			
				(1.610)	(0.032)	(2.061)			
$\text{Change}_{i,t4}$							0.010	−0.002	0.147
							(0.130)	(0.002)	(0.149)
$\text{PT} \cdot \text{Shock1}_{i,t4}$	−9.847***	−0.132*	−14.170***						
	(3.623)	(0.071)	(4.852)						
$\text{PT} \cdot \text{Shock2}_{i,t4}$				−14.810**	−0.394***	−11.950			
				(6.809)	(0.145)	(7.809)			
$\text{PT} \cdot \text{Change}_{i,t4}$							−1.376	−0.046**	−2.544***
							(0.872)	(0.021)	(0.985)
N	6237	6038	6237	3083	3006	3083	10463	10244	10463
R-squared	0.010	0.060	0.193	0.011	0.053	0.141	0.044	0.028	0.148

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

SIM data

As mentioned, the SIM data includes death tallies for both formal and informal workers and is desegregated at the month level since 1996. Also, I can estimate the effect of the accidents occurring in the run-up to the election and compare them to the number of accidents happening during the same time-period, one year before. Tables 3 and 4 replicate the findings of the previous section.

Again, an increase in the number of accidents has no effect on the electoral performance of the incumbent in the following election. There is no evidence that an increase (or a change) in the number of fatal workplace accidents—accounting for both formal and informal sectors of the economy—has an effect on the electoral standing of incumbent parties. Table 4 shows that the PT did suffer a large electoral penalty in municipalities that were "treated" with a shock in the number of fatal accidents. For the $\text{Shock2}_{i,t4}$ variable—that has better balance in terms of treatment and control municipalities—the PT vote share decreased in almost 9% points (when compared to municipalities that did not experience a shock), its probability of being reelected decreased in 27%, and the party got 10% less votes compared to the previous election. Results are similar for the other two measures, but not always statistically significant. The PSDB did not receive an electoral penalty from an increase in the number of accidents (see the appendix).

Table 3: The effect of workplace deaths on municipal electoral outcomes (incumbent sample, SIM data)

	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**
$\text{Shock1}_{i,t4}$	−0.379	−0.019	0.063						
	(0.721)	(0.013)	(0.811)						
$\text{Shock2}_{i,t4}$				−1.390	−0.038	−1.932			
				(2.900)	(0.054)	(3.304)			
$\text{Change}_{i,t4}$							0.032	−0.0003	0.168
							(0.309)	(0.004)	(0.352)
N	10251	9774	10002	3100	3006	3083	10657	10244	10460
R-squared	0.006	0.008	0.012	0.008	0.016	0.017	0.007	0.007	0.012

***p < .01; **p < .05; *p < .1

Table 4: The effect of workplace deaths on municipal electoral outcomes (PT sample, SIM data)

	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**
PT mayor	7.152***	0.010	−28.480***	7.180***	0.022	−27.980***	5.263***	−0.060***	−30.760***
	(1.348)	(0.028)	(1.679)	(1.453)	(0.029)	(1.816)	(1.139)	(0.023)	(1.471)
$\text{Shock1}_{i,t4}$	−0.157	−0.007	0.292						
	(0.405)	(0.007)	(0.474)						
$\text{Shock2}_{i,t4}$				−0.966	−0.023	−1.116			
				(0.811)	(0.014)	(0.946)			
$\text{Change}_{i,t4}$							0.010	−0.002	0.147
							(0.130)	(0.002)	(0.149)
$\text{PT} \cdot \text{Shock1}_{i,t4}$	−2.699	−0.098*	−4.884						
	(2.541)	(0.053)	(3.186)						
$\text{PT} \cdot \text{Shock2}_{i,t4}$				−8.970*	−0.265***	−10.120*			
				(4.673)	(0.100)	(5.586)			
$\text{PT} \cdot \text{Change}_{i,t4}$							−1.376	−0.046**	−2.544***
							(0.872)	(0.021)	(0.985)
N	9899	9680	9899	6882	6741	6882	10463	10244	10463
R-squared	0.045	0.023	0.136	0.044	0.025	0.143	0.044	0.028	0.148

***p < .01; **p < .05; *p < .1

Controlling for time-varying covariates

As a robustness check, this section shows results using four time-varying covariates that may correlate both with the number of labor accidents and the electoral fortune of incumbent parties. First, I control for GDP growth, comparing the size of the local economy during the electoral year with the year before that. In principle, both high and low economic growth could be associated with an increase in the number of workers killed. During a recession, workers may accept riskier jobs, ignoring health and safety concerns. Alternatively, during economic booms, more workers may be employed, thus potentially increasing the number of people who can be injured on the job. Second, I control for the percentage change in the number of formal workers, again, comparing the electoral year with the year before the election (unfortunately, there is no data on the number of informal workers at the year/municipal level). The third control variable is the change in the number of labor inspections conducted by the federal government. More inspections can be evidence of labor market problems in the municipality; also, voters may be responding to an increase in inspections following labor accidents rather than to the accidents per se. Finally, I control for the percentage change in the number of *formal* firms. Tables 5 and 6 show results for analogous difference-in-differences regressions. Results remain largely unaffected by the introduction of controls (i.e., similar in magnitude and statistical significance to those shown in the previous section).

Table 5: The effect of workplace deaths on municipal electoral outcomes (PT sample, INSS data; time-varying controls)

	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**
PT mayor	0.650	−0.227***	−32.950***	3.983*	−0.154***	−27.700***	4.661**	−0.137***	−24.110***
	(1.783)	(0.035)	(2.373)	(2.378)	(0.047)	(3.157)	(2.259)	(0.044)	(3.061)
$\text{Shock1}_{i,t4}$	−0.141	0.0001	−0.457						
	(0.633)	(0.011)	(0.828)						
$\text{Shock2}_{i,t4}$				2.099	0.022	1.145			
				(1.600)	(0.032)	(2.051)			
$\text{Change}_{i,t4}$							0.885	0.010	0.609
							(0.540)	(0.011)	(0.688)
$\text{Workers}_{i,t4-t3}$	0.011	0.0001	0.011	−0.007	−0.00002	−0.022	0.003	−0.00001	−0.007
	(0.010)	(0.0001)	(0.013)	(0.017)	(0.0003)	(0.025)	(0.012)	(0.0002)	(0.021)
$\text{Firms}_{i,t4-t3}$	−0.058	−0.001	−0.071	−0.077	−0.001	0.087	−0.178	−0.0001	−0.133
	(0.045)	(0.001)	(0.059)	(0.086)	(0.001)	(0.100)	(0.137)	(0.002)	(0.164)
$\text{GDP}_{i,t4-t3}$	0.010	0.0002	−0.004	0.038	0.0004	0.018	0.011	−0.00004	0.035
	(0.018)	(0.0003)	(0.022)	(0.032)	(0.001)	(0.040)	(0.042)	(0.001)	(0.057)
$\text{Inspections}_{i,t4-t3}$	0.001	0.0001*	−0.0002	0.0004	0.0001**	−0.0003	0.0001	0.0001*	−0.001
	(0.002)	(0.00003)	(0.004)	(0.003)	(0.00003)	(0.004)	(0.003)	(0.00004)	(0.004)
$\text{PT} \cdot \text{Shock1}_{i,t4}$	−10.070***	−0.136*	−14.370***						
	(3.634)	(0.071)	(4.862)						
$\text{PT} \cdot \text{Shock2}_{i,t4}$				−14.590**	−0.394***	−12.000			
				(6.824)	(0.146)	(7.809)			
$\text{PT} \cdot \text{Change}_{i,t4}$							−1.018	−0.059	−1.168
							(2.215)	(0.052)	(2.418)
N	6231	6032	6231	3079	3002	3079	1740	1712	1740
R-squared	0.011	0.062	0.194	0.013	0.056	0.142	0.014	0.036	0.120

***p < .01; **p < .05; *p < .1

Table 6: The effect of workplace deaths on municipal electoral outcomes (PT sample, SIM data; time-varying controls)

	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$	$Share_{t+1}$	$Reelect_{t+1}$	$VoteChange_{t+1}$
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**
PT mayor	0.844	−0.157***	−33.730***	1.251	−0.144***	−32.680***	−1.575	−0.226***	−36.540***
	(1.646)	(0.031)	(2.176)	(1.741)	(0.033)	(2.342)	(1.414)	(0.026)	(1.936)
$\text{Shock2}_{i,t4}$	−0.347	−0.012	0.065						
	(0.514)	(0.008)	(0.652)						
$\text{Shock2}_{i,t4}$				−1.388	−0.025	−1.271			
				(1.064)	(0.017)	(1.302)			
$\text{Change}_{i,t4}$							0.064	−0.002	0.164
							(0.162)	(0.003)	(0.198)
$\text{Workers}_{i,t4-t3}$	−0.001	0.00001	−0.002	0.0005	0.00002	−0.0005	0.001	0.00005	−0.002
	(0.003)	(0.00004)	(0.003)	(0.002)	(0.00004)	(0.003)	(0.004)	(0.0001)	(0.003)
$\text{Firms}_{i,t4-t3}$	0.001	0.0002	0.003	0.001	0.00003	−0.010	−0.009	−0.00005	0.0002
	(0.031)	(0.001)	(0.040)	(0.051)	(0.001)	(0.072)	(0.028)	(0.001)	(0.039)
$\text{GDP}_{i,t4-t3}$	0.012	0.0004	0.003	−0.015	0.0001	−0.028	0.013	0.0005	0.001
	(0.017)	(0.0003)	(0.020)	(0.025)	(0.001)	(0.031)	(0.015)	(0.0003)	(0.019)
$\text{Inspections}_{i,t4-t3}$	0.001	0.0001*	0.0001	0.001	0.0001*	0.0002	0.001	0.0001**	−0.00003
	(0.002)	(0.00003)	(0.004)	(0.002)	(0.00003)	(0.004)	(0.002)	(0.00003)	(0.004)
$\text{PT} \cdot \text{Shock1}_{i,t4}$	−3.477	−0.107*	−5.996						
	(3.058)	(0.061)	(3.879)						
$\text{PT} \cdot \text{Shock2}_{i,t4}$				−10.120*	−0.320***	−14.630**			
				(5.608)	(0.115)	(6.593)			
$\text{PT} \cdot \text{Change}_{i,t4}$							−1.531	−0.048**	−2.343*
							(1.084)	(0.024)	(1.261)
N	7359	7141	7359	4993	4853	4993	7784	7565	7784
R-squared	0.007	0.040	0.169	0.008	0.048	0.178	0.008	0.057	0.183

***p < .01; **p < .05; *p < .1

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