

The Political Economy of Democracies

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The Political Economy of Democracies
Three Empirical Essays

Sliwa Ruiz, Stefan

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university of
 groningen

The Political Economy of Democracies

Three Empirical Essays

PhD thesis

to obtain the degree of PhD at the
University of Groningen
on the authority of the
Rector Magnificus Prof. J.M.A. Scherpen
and in accordance with the decision by the College of Deans.

This thesis will be defended in public on

Monday 27 January 2024 at 16:15

by

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Why did I start a PhD? I would love to say it was driven purely by my curiosity, thirst for knowledge, and ambition to contribute meaningfully to society—but the reality is that needing a job during COVID probably played an equally crucial role. However, when it comes to how I managed to finish the PhD, the answer is much clearer: I couldn't have done it alone. These acknowledgments are dedicated to some of the incredible people who kept me afloat throughout these years

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Stefan Sliwa Ruiz,
La Paz, November 2024

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Chapter 1

Introduction

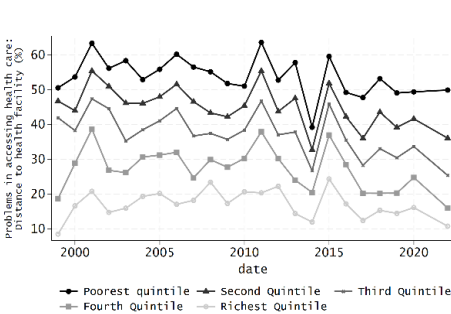
*Medicine is a social science, and politics
is nothing else but medicine on a large
scale.*

Rudolf Virchow

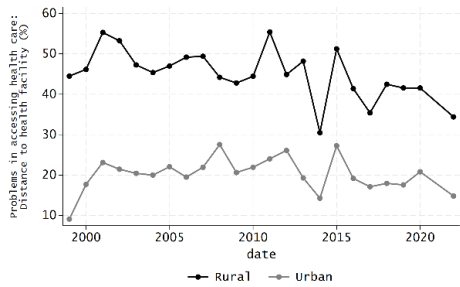
*Deliberation and debate is the way you
stir the soul of our democracy.*

Jesse Jackson

Inequalities, whether between social groups or regions, are rising in most countries (OECD, 2015; Ortiz & Cummins, 2011). Economic inequalities exert a defining influence on every social aspect, strongly affecting inter alia health and healthcare (Graham & Felton, 2006; Pickett & Wilkinson, 2015), education (Busso & Messina, 2020; Cruces et al., 2014), as well as job opportunities and fundamental life prospects (Bourguignon et al., 2007; Moretti, 2012; Roemer & Trannoy, 2015). One important example of how the relative social rank of a person or region determines everyday welfare and well-being is access to healthcare: Figures 1.1a and 1.1b illustrate the large and persistent gaps in being prevented from accessing a healthcare provider due to travel time between populations of different economic ranks and places of residence in developing countries; living in relative disadvantage, whether socioeconomically or geographically, much more often prevents individuals from accessing basic services, such as healthcare.



(a) Percent of the the population that reports having problems in accessing health care due to the distance to health facilities, stratified by wealth quintiles.



(b) Percent of the the population that reports having problems in accessing health care due to the distance to health facilities, stratified by either living in a rural or urban area.

Figure 1.1. Data from Healthcare access (DHS Program), which is part of the WHO Health Inequality Data Repository (Kirkby et al., 2023). Countries included in the DHS Program dataset are Afghanistan, Albania, Angola, Armenia, Azerbaijan, Bangladesh, Benin, Bolivia, Burkina Faso, Burundi, Cambodia, Cameroon, Chad, Comoros, Congo, Côte d'Ivoire, Democratic Republic of the Congo, Dominican Republic, Egypt, Eritrea, Eswatini, Ethiopia, Gabon, Gambia, Ghana, Guatemala, Guinea, Guyana, Haiti, Honduras, India, Indonesia, Jordan, Kenya, Kyrgyzstan, Lesotho, Liberia, Madagascar, Malawi, Maldives, Mali, Mauritania, Morocco, Mozambique, Myanmar, Namibia, Nepal, Nicaragua, Niger, Nigeria, Pakistan, Papua New Guinea, Peru, Philippines, Republic of Moldova, Rwanda, São Tomé and Príncipe, Senegal, Sierra Leone, South Africa, Tajikistan, Timor-Leste, Togo, Uganda, United Republic of Tanzania, Yemen, Zambia, and Zimbabwe.

Concomitantly, political dissatisfaction and distrust are widespread and increasing: In the OECD, roughly two-thirds of citizens now report low or no trust in their government, and an even higher number does not believe that they have a

say in government affairs (OECD, 2024). High levels of national inequality, along with individuals' and regions' relative positions within these inequalities, heighten feelings of dissatisfaction and disillusionment (Graham & Felton, 2006; Lenzi & Perucca, 2021; OECD, 2024).

Consequently, institutional distrust is especially pronounced in highly unequal societies, such as the ones in Latin America, which are often considered to be the most unequal in both, economic and social dimensions (Busso & Messina, 2020). Here, 70% lacks trust in their institutions, and nearly half of the citizens do not believe that democracy is the best form of government (Latinobarómetro, 2023).

Cleavages between social groups and regions have started to manifest in increased political polarization, often centered around notions of globalization. As a process that creates both winners and losers, globalization is frequently blamed for exacerbating regional inequalities (Hoen, 2011; Rodrik, 2021). Its negative impacts are often geographically concentrated, as globalization can destroy local industries, increase competitive pressures on (unskilled) workers, destabilize and undermine the effectiveness of national welfare systems, and erode local social and cultural cohesion (Antràs et al., 2017; Iammarino et al., 2019; Inglehart & Norris, 2016; Stiglitz, 2017). These "places of discontent" often feel neglected by the government and "political elites" more general, leading to a reinvigorated appetite for nationalistic policies. In contrast, populations in relatively prosperous urban centers frequently embrace stronger economic, political, social, and cultural internationalization (Becker et al., 2017; Dellmuth et al., 2022; Katz & Mair, 2009; Rodríguez-Pose, 2018).

To address these growing regional disparities and mitigate social and political polarization, scholars and policymakers have advocated for political reforms and "place-sensitive" policies tailored to local needs (Austin et al., 2018; Bartik, 2020; Ehrlich & Overman, 2020; Iammarino et al., 2019). The European Social Cohesion, for example, aims to spur growth and build strong local institutions through tailored interventions in underdeveloped regions (Farole et al., 2011). Such strategies can be effective because they foster a reciprocal relationship between policymaking and public trust. When citizens perceive policymaking as responsive to their needs, it strengthens institutional trust (Grossman & Slough, 2022). High levels of trust, in turn, facilitate long-term planning, reduce enforcement costs, and promote eco-

conomic stability and growth (Dasgupta, 2011). Conversely, policy which is perceived as lacking or unresponsive by local citizens can erode trust, diminishing policy and government effectiveness by decreasing public compliance and increasing transactional costs within and between various public and private sectors (Fukuyama, 1996).

Thus, policies have the potential to increase institutional trust and help mend social and political divisions, provided that citizens perceive them as fair and responsive to local needs (Bartik, 2020; Grossman & Slough, 2022). Currently, there exist widespread perceptions that government policy is biased towards prosperous, urban areas (McKay et al., 2024). There is therefore a need to broadly and interdisciplinarily analyze societal changes following specific government actions and policies, contributing to our evolving understanding of what constitutes, and is perceived as, responsive governance.

This interdisciplinary dissertation combines methods and theories from economics and political science to examine instances of government (un)responsiveness in Brazil and France. These countries represent starkly different contexts: France is a relatively mature democracy with moderate levels of inequality of the Global North, while Brazil is a young and vulnerable democracy with very high poverty and inequality levels situated in the Global South. Nevertheless, in both countries, the national government, and especially the president, wields strong executive powers to which citizens are highly responsive (de Castro et al., 2021; Lewis-Beck & Nadeau, 2000; Santos, 2008). The examined case studies illustrate how government responses and policies affect the social and political domains in democracies with different socio-economic backgrounds.

1.1 Thesis Structure

The thesis is composed of three empirical studies, each presented as a separate chapter that can be read independently. They are connected through the theme of government responsiveness: While Chapter one reflects on the repercussions of the French government's decision to disregard the public's rejection of the proposed EU Constitution, Chapters two and three examine key events of a public policy designed to respond to citizens' healthcare needs in Brazil. In terms of outcomes and unit of analysis, the studies explore the impact of *national* government decisions on

municipal variables, allowing for an informed, fine-grained, study of averages and distributions of local treatment impacts. More concretely, I study impacts on municipal voting (Chapters two and three) and municipal healthcare outcomes (Chapter four). To analyze these relationships, I draw from interdisciplinary theoretical foundations that include insights from economics, political science, and public health, employing econometric methods commonly used in economics, such as difference-in-differences models. I argue that such a interdisciplinary approach is not only warranted but needed, given the complexity of analyzing government policies and its obvious connections to multiple dimensions of society. Hence, this thesis is a rare attempt to use theories and insights from various fields concurrently and on an equal footing (Lynch, 2023). Table 1.1 summarizes the chapters' contents on a glance, and below I provide a brief summary of them.

	Chapter Two	Chapter Three	Chapter Four
Topic	Impacts of Party Cartelization and Ignoring a Referendum	Impacts of introducing a salient, large-scale healthcare access program	Impacts of large-scale exit and turnover of doctors
Outcomes	Voter turnout, protest voting, voting shares of mainstream & anti-system parties	Voting share for the incumbent	healthcare utilization, health outcomes, healthcare market outcomes
Treatment	Share of no-vote in referendum 2005 (continuous)	Participation in program (binary), 2013	Exposure to the exit shock of Cuban doctors (categorical), November 2018
Main Method ∞	OLS, cross-sectional analysis	dynamic (panel) event-study, matched Two-way Fixed Effects DiD model	dynamic (panel) event-study, Two-way Fixed Effects DiD model
Robustness	Beta regression model, Oster test, falsification tests	DiD estimators for heterogeneous treatment effects & staggered treatment adoption, different weighting schemes and more	Poisson pseudo-likelihood regression model, alternative treatment definition, alternative comparison groups, inclusion of time-varying controls, leave-one-out analysis and more
Sample	$\approx 33,000$ French municipalities	4,399 Brazilian municipalities	4,162 Brazilian municipalities
Level of analysis	Municipality-election level	Municipality-election level	Municipality-month level
Time Period	2007, 2009, 2012, 2014	2002, 2006, 2010, 2014, 2018, 2022	October 2017 - December 2019

Chapter Two: Voter Disenchantment in the Aftermath of the 2005 EU Constitutional Referendum in France

What happens when a government turns a deaf ear to a clear mandate of its citizens? Chapter two analyzes the electoral impacts of ignoring and bypassing a popular referendum vote. In the context of increasing European Union integration, member states were expected to ratify a common Constitution in 2005. The French government called for a referendum on this issue but, faced with defeat at the ballot box, effectively ignored the popular vote in favor of political expediency. Instead, the government, together with mainstream opposition parties, opted for parliamentary ratification of a modified version of the Constitution in 2008. Meaningful political objections to the ignorance of the *vox populi* were only voiced by civil society organization, such as labor unions, and anti-system far-left and far-right political parties.

Chapter two argues that this case serves as a paradigmatic example of the theory of party cartelization, one of the main theoretical underpinnings explaining rising disenchantment with traditional democratic forces (Katz & Mair, 1995). The theory posits that large segments of the population no longer perceive traditional political parties as legitimate representatives of specific groups. Instead, these parties are seen as top-down technocratic administrators of governance, displaying minimal ideological differences (Katz & Mair, 2009; Mair, 2013). The French case of bypassing the popular vote in 2005 exemplifies this theory, highlighting how politicians might be perceived as professional managers who subordinate and sidestep popular demands in favor of a set policy agenda. In the French case, this agenda implied increasing globalization, favoring the highly-educated professionals and urban areas, something the referendum's no-vote, carried to victory by the relatively poorer and rural regions, explicitly rejected (Startin & Krouwel, 2013). In that sense, the study represents an early example of a polarized vote on matters of globalization.

These events are considered an important moment in the observed growth in voter apathy and Euroscepticism in France. Many disillusioned voters are said to have been mobilized by anti-system parties, especially of the far right, in more recent elections (Gabriel, 2015; Koch et al., 2023). To empirically analyze the relevance of the mainstream parties' disregard of the referendum results, Chapter two inves-

tigates how the strength of a municipality's rejection of the referendum relates to municipal voting behaviour in subsequent elections. The article focuses on three outcomes: turnout, to evaluate voting apathy; blank and invalid votes, to evaluate protest voting; and vote shares for mainstream and anti-system parties, i.e. far right and far left parties explicitly opposing the political establishment and status quo. The main hypothesis is that ignoring the local voting outcome of the referendum, i.e. the strength of the municipal no-vote, is associated with lower levels of turnout, increased protest voting, and increased support for anti-system parties at the expense of mainstream parties.

Using data on national elections held between 2005 and 2015 in France, the chapter employs cross-sectional ordinary least squares (OLS) analyses to regress municipal elections outcomes on the municipal no-vote share, controlling for past municipal election outcomes and an array of control variables. The chapter finds evidence that a higher no-vote in the referendum is associated with a lower turnout and higher protest voting in subsequent elections, illustrated in Figure 1.2, as well as a stronger support for anti-system parties in subsequent elections. The magnitudes of the coefficients are meaningful, particularly for voter turnout. For example, the impact of the referendum vote on turnout is more than three times greater than that of municipal unemployment levels in the 2009 European Parliament elections. Furthermore, examining survey data before and after the referendum was held in 2005, the chapter shows that French citizen reported less trust in the way democracy works in France in the aftermath of the referendum.

The chapter contributes to the literature by showing that disregarding a popular vote can entail detrimental effects for democratic participation and help anti-system parties. It suggests that "Brexit" is not the only case where using a referendum to mend fissures around international integration backfired; in fact, governments have been increasingly using referendums as a way to domestically legitimize increasing international integration (Walter, 2020). However, decision-makers should be careful to reduce complex questions into binary ones, especially if they are not willing to neither meaningfully engage nor follow through with undesired outcomes. Instead, this research suggests that they need to increase efforts to convince voters of the benefits and be transparent on the drawbacks of their preferred policies. Exhibiting democratic responsiveness and honestly illuminating

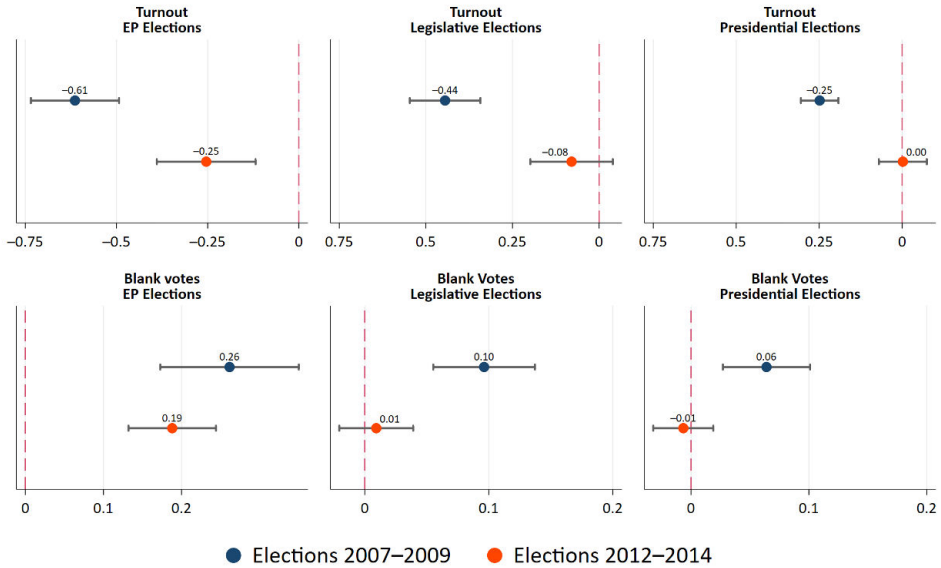


Figure 1.2. Relationship between municipal no-vote and subsequent election outcomes. A one standard deviation increase in municipal no-vote, corresponding to roughly ten percentage points, is, for example, associated with a significant 0.61 percentage point decrease in the municipal turnout for the European Parliament (EP) elections held in 2009.

both sides of a political issue is one way of how to tackle “the age of disinformation”. While increasing media literacy and pertinent regulation are, among others, facets of how to improve the current arena of debate between politicians and the public, transparent communication is another crucial factor. This could, *inter alia*, include providing accessible data and regularly updated information on the factors underlying any decision, create areas of open discussions, and engage with local communities (Shu et al., 2020). Otherwise, continuing with opaque top-down decision-making is likely to add to the breakdown in political trust and social cohesion.

Chapter Three: Policy Targeting and Voting Outcomes: Evidence from a Health Care Program in Brazil

While Chapter two discusses how democratic unresponsiveness to concerns about globalization and inequality might have entailed adverse consequences for social cohesion and the political system in France, Chapter three, in turn, examines the electoral impact of a social policy aimed at responding to and mitigating regional inequalities in Brazil. Public policies are the main government tool to distribute

globalization gains among the population and to insulate disadvantaged groups and regions from globalization's adverse impacts (Antràs et al., 2017).

Despite seeing an uptick in inequality in recent years, Brazil has experienced a broad decline in economic disparity during the 21st century (Firpo & Portella, 2019). Successive governments have used social policies, primarily the conditional cash transfer program *bolsa família*, to mitigate the adverse impacts of globalization for disadvantaged populations. These efforts have enabled Brazil to reap the benefits of globalization while concurrently reducing poverty and economic disparities between social groups and regions (Nissanke & Thorbecke, 2010; Sánchez-Ancochea & Mattei, 2011). Nevertheless, researchers and activists have argued that economic assistance programs alone do not suffice to narrow the persistent regional disparities in social dimensions, such as healthcare. The Brazilian government would need to ease supply-side constraints, particularly in terms of the availability of facilities and personnel (Sánchez-Ancochea & Mattei, 2011; Soares et al., 2010).

Heeding this call, Brazil introduced the "Programa Mais Médicos" (PMM) in 2013. The PMM was a large-scale healthcare access program which at its height employed around 15,000 primary care doctors to practically connect mainly rural and poor municipalities to Brazil's universal healthcare system. While previous literature assessed the healthcare impacts of the program as positive, for example increasing primary healthcare access and decreasing hospitalization rates (Fontes et al., 2018; Hone et al., 2020), Chapter three examines whether the program's introduction also entailed electoral effects.

The scholarly literature connecting social policies with voting outcomes has focused so far on conditional cash transfers, typically finding substantial positive effects for the incumbent (Linos, 2013; Manacorda et al., 2011). For example, the *bolsa família* program has been found to substantially boost the electoral fortunes of the political party implementing it, the *Partido dos Trabalhadores* (PT; Workers' Party) and Brazil's then (and current) leader Lula (Zucco Jr, 2013). However, evidence on the electoral impacts in non-financial social policies remain scarce (Golden & Min, 2013; Sandholtz, 2023). Healthcare policies in particular are becoming increasingly politicised, but the evidence so far paints a mixed picture on whether they yield electoral gains for their political architects (Imai et al., 2020; Konisky &

Richardson Jr, 2012).

While the PMM was hailed by the PT and social activists as a necessary step to combat inequalities in Brazil, the conservative political opposition and local doctors' associations opposed the program (Silva et al., 2018). Among their critiques featured the program's high costs, the large role it gave to the government on the medical labor market, and, most saliently, that a majority of the PMM doctors were from Cuba: To fill vacancies for which no Brazilian doctor applied, the Brazilian government signed an international contract with the Cuban government which stipulated that thousands of Cuban doctors would work for the PMM, with part of their salary paid to the Cuban state. The program became a contentious political topic. As a federal program under the direct control of the Ministry of Health, the program became a focal point of debate for every subsequent presidential election, with the PT candidate defending it against the main conservative opposition candidate, who sought to abolish it.

Policy analysts also noted that policy targeting was not always efficient (Oliveira et al., 2016). While some of the poorest municipalities did not participate in the program, relatively wealthy municipalities did participate. The chapter examines whether the Brazilian government, by targeting these two distinct groups of municipalities, pursued distinct policy motivations. More specifically, the chapter argues that the government pursued equity consideration for the needy municipalities which were considered priority according to the PMM's own requirements, but pursued electoral considerations for the municipalities which participated in the PMM but did not fulfill the official requirements. Thus, this project examines i) whether the introduction of the PMM led to an increase in the PT vote share in participating municipalities in subsequent presidential elections and ii) whether the PMM was electorally more important in the needy municipalities relative to the better-off municipalities.

Voter and election data comes from the Brazilian Superior Electoral Court. Data on healthcare is sourced from different datasets of Datasus, the data dissemination tool of the Brazilian Ministry of Health. The Brazilian Institute of Geography and Statistics provides me with detailed data on municipality characteristics. The chapter connects the three data sources to analyze the PT vote share of roughly 4,400 municipalities in the presidential elections held between 2002 and 2022. The

chapter employs a matched difference-in-difference technique to regress the PT's municipality vote share on PMM participation, allowing to treat PMM participation as a quasi-random treatment and make causal claims about its policy impacts (Imai & Ratkovic, 2014).

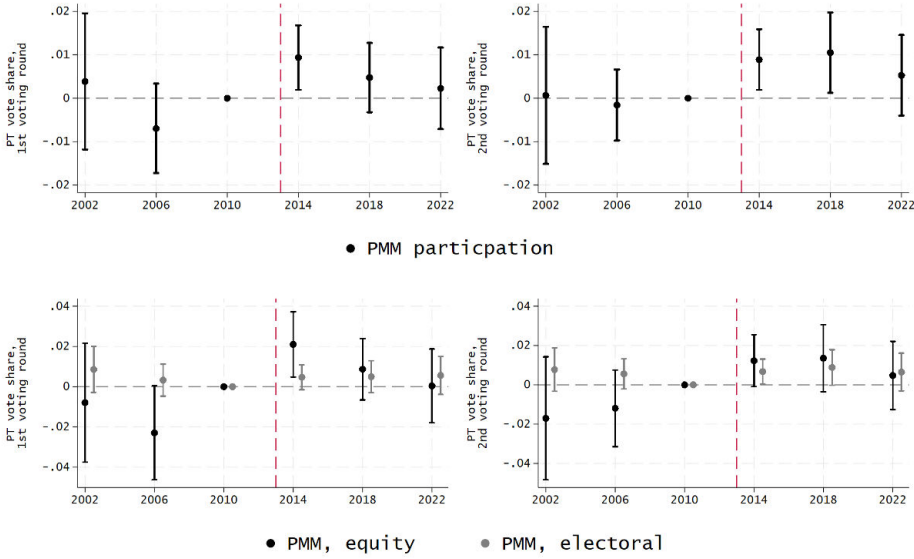


Figure 1.3. PMM introduction and PT vote share on the municipality level, presidential election 2002-2022

My results show that the introduction of the PMM increased the PT vote share up to the elections 2018, see Figure 1.3. For the elections in 2014, its impact is especially pronounced for municipalities targeted due to equity considerations, i.e. poor municipalities. This study is the first to show that the PMM had any political effects, and it is one of the first showing that healthcare policies can be electorally rewarding in nationwide elections. It also shows that local needs and political gains can be complementary.

Nevertheless, the PMM managed to only brake falling distrust levels in the PT party: between 2010 and 2022, the party lost confidence across all municipalities, but the drop was less steep for municipalities benefiting from the PMM. This mirrors falling levels of trust in the mainstream political forces globally; Figure 1.4 shows, as an example, reported trust in political parties in Brazil and France and indicates falling trust levels in both countries from 2010 onwards. Both countries'

reported levels received a boost when relative outsiders challenged the political mainstream. On the one hand, Emmanuel Macron managed to reinvigorate French politics in 2017 by offering to reconcile traditional liberal policy with social cohesion and increased democratic representation. On the other hand, Jair Bolsonaro upset the Brazilian status-quo in 2018 when his nationalistic ideas mobilized many voters fed up with the political establishment. Scholars emphasized that both countries experienced backlashes against the then political mainstream which stood reflective of broader global patterns of political frustration (Hunter & Power, 2019; Palombarin & Amable, 2021).

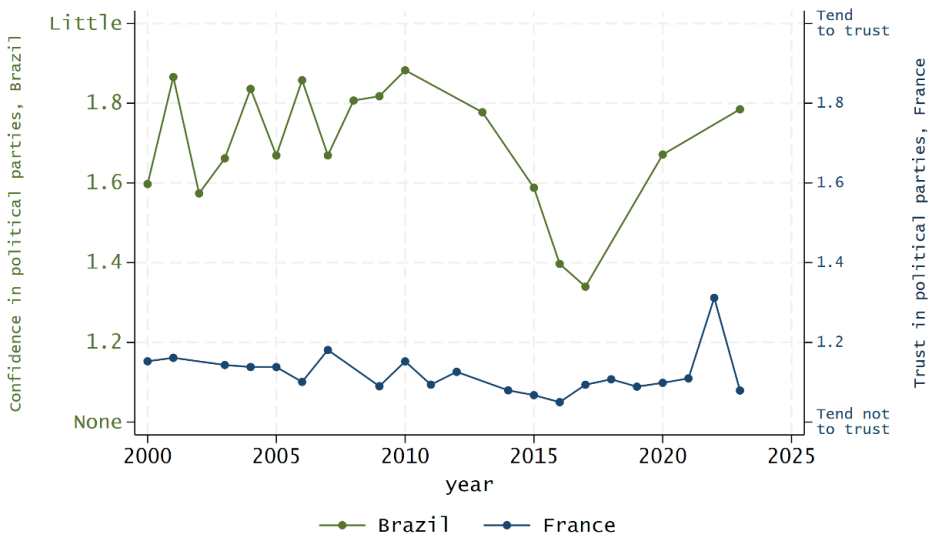


Figure 1.4. Original data on France sourced from Eurobarometer, data on Brazil from Latinobarometro. Re-scaled answers to variations of the question: "How much trust you have in political parties. Would you say you have a lot (4), some (3), a little (2) or no trust (1)" for the Latinobarometro data and "please tell me if you tend to trust political parties (2) or tend not to trust political parties (1)" for Eurobarometer data. Other answers, such as "do not know", have been excluded.

Chapter Four: Doctor Turnover and Health Outcomes: Evidence from the Exit of Cuban Doctors in Brazil

The PMM was implemented in the context of strongly decreasing trust and satisfaction with Brazilian institutions by parts of the population. Due to economic decline, mismanagement, and corruption scandals, the PT party lost much support, and social policies could only stem the tide so much. In fact, many Brazilians started to reject social policies merely due to the association with the PT and the Brazilian

political left, irrespective of the underlying respective policy merits (Gallego et al., 2017; Lopes et al., 2020; Milhorange, 2022).

Ultimately, Brazil ousted the PT party and elected the far-right, self-declared anti-communist politician Jair Bolsonaro as president-elect in 2018. Driven by fervent resentment and anti-system sentiments, the new government dismantled many of the PT's social programs. While such populist decisions pleased part of his supporters, it is said to have had detrimental effects on the economy (Rennó, 2020). Especially marginalized communities were hurt, but many of those communities supported Bolsonaro based on feelings of political resentment.

One of the first consequences of Bolsonaro's electoral victory was that the Cuban government pre-emptively withdrew from its international agreement with Brazil in November 2018, calling home the 8,316 Cuban primary care doctors working across more than 2,800, mostly rural and poor, municipalities at that time. This represented an unexpected shock to the Brazilian healthcare system, as neither the doctors nor any involved institution were previously informed of, let alone included in, the decision; the Cuban PMM doctors had got their contracts extended only a year before (Sobrinho, 2018). Facing thousands of vacancies to fill, the Brazilian government exerted great efforts to keep the PMM running, and managed to fill most positions after just a few months (Maffioli et al., 2019). The question that this chapter answers is how the sudden exit of Cuban doctors and the subsequent large-scale doctor turnover affected municipal healthcare utilization, health outcomes, and general healthcare systems.

Based on the existing literature on doctor turnover, Chapter four hypothesizes negative effects on primary care utilization (Staiger, 2022), especially for services primarily delivered by doctors, and for conditions that are susceptible to disruptions in ongoing care, such as treatment for chronic conditions (Bischof & Kaiser, 2021; Sabety et al., 2021). As a result of primary care service disruption, we might see an increase in hospitalizations and mortality for conditions amenable to primary care (Sabety, 2023). We also expect the disruption to have led to increased healthcare costs. These effects would be especially pronounced for rural, remote municipalities, as their primary care provision heavily relied on the Cuban doctors and their local medical labor markets might not be able to absorb the large departure of an experienced labor force. We thus distinguish between these most affected

municipalities and partially affected municipalities, consisting of averagely larger urban peripheries.

Chapter four combines various data sources on the number of doctors, health-care utilization and inputs, sales of medicine, health outcomes, and healthcare costs at the municipality-by-month level, covering the period from October 2017 to December 2019. The project employs a difference-in-difference estimation technique, again making use of a two-way fixed effects model to compare outcome changes in affected and unaffected PMM municipalities before and after the Cuban exit. This comparison is possible, as the Cuban presence differed between municipalities participating in the program, with the comparison group having no Cuban doctors. Besides controlling for baseline heterogeneity in levels of the healthcare outcome variables and common time trends, my design also include interactions between month and state fixed effects. Despite relying on high-frequency data, the chapter additionally controls for time-varying municipality characteristics in robustness checks to reinforce claim that confounders are not relevant in driving any effects.

Chapter four reveals that following the Cuban doctors' exit, there was a sharp decrease in all primary care service consultations performed by doctors. As vacancies are being re-filled, care provision on acute conditions, such as infections, quickly return to pre-shock levels. However, the same is not true for consultations on chronic conditions which remained strongly below pre-shock levels even after the Cuban doctors have been replaced around May 2019. Figure 1.5 illustrates this, showing trends in the number of consultations on different conditions. We do not find evidence that these decreases in primary care utilization translate into increased hospitalization or mortality rates during our sample period.

We explain our results with illustrating how local healthcare systems adapted to the shock. Within primary care, the doctors replacing the Cubans were often inexperienced. The primary care system reacted to doctor supply bottlenecks by systematically prioritizing acute care over ongoing care. At the same time, patients' access to prescription drugs only suffered from a temporary disruption, ensuring self-medication of diagnosed patients. Beyond primary care, we document an increase in the use of emergency rooms for basic healthcare issues. With respect to market adaptations, we do not find an increase in the use of private health care,

but we do find that public primary care doctors not affiliated with the PMM experienced a strong increase in their wages.

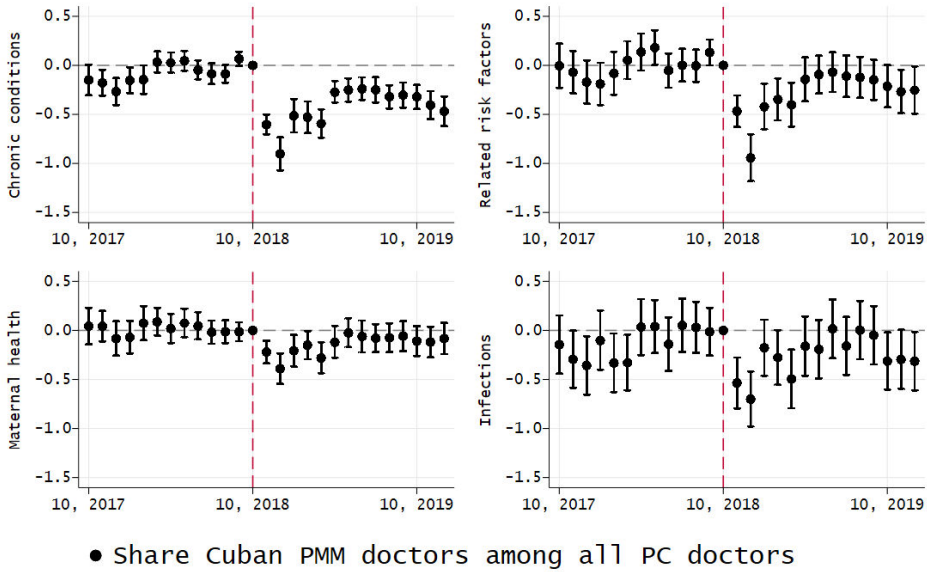


Figure 1.5. Cuban exit and primary care consultations on disease families, October 2017 - December 2019

Existing evidence on turnover in the healthcare sector is limited to small-scale, high income setting (Sabety et al., 2021; Simonsen et al., 2021; Zhang, 2022). In terms of healthcare, this chapter shows how the Brazilian primary care system adapted to a large-scale turnover shock, with important implications for policy-makers: faced with a widespread loss in human capital and place-specific expertise, the primary care sector focused on providing urgent, acute care, systematically shifting away from providing time-intensive ongoing care. While this prevented detrimental health outcomes in the short-term, a lack of professional care and detection in non-communicable disease might entail increasing burdens and costs on the long-term. To prevent this, policy-makers should find incentives for patients and practitioners to ensure a smooth transition of ongoing care provision when faced with turnover disruptions. This is especially relevant for relatively poor and remote regions, which disproportionately often suffer from such disruptions (WHO, 2010; Zhang, 2022). In terms of the wider political implications, it shows that political division and resentment is able to upset not just politics, but

also social dimensions, such as basic healthcare provision. In Brazil, especially the long-term human health costs of the temporary populist strife between Cuba and Brazil might be significant.

Public policies and economic decisions are inextricably linked with politics, and are often contingent on continued government support. For scholars, this underscores the importance of adopting a holistic and interdisciplinary approach when analyzing policy issues. For policymakers, it means that provision of public services, be it in education or healthcare, needs to be insulated as best as possible from political turmoil. This is especially true for programs and policies dependent on international cooperation agreements, such as the various medical cooperation that various countries have with Cuba, as well as the ones enabled by the European Regional Development Fund. Otherwise, it might be the relatively disadvantaged regions and populations, who incidentally are also often the ones upset with the status-quo, who suffer the strongest consequences for their well-being.

The three chapters examined how inequality and social divisions called for government responsiveness, and how such (lack of) responsiveness affected subsequent voting and health outcomes. All in all, the chapters of this thesis show that politicians and policy-makers need to visibly and transparently respond to the needs and calls of needy populations and regions in order to reduce resentment and perceptions of unfairness.

1.2 Future Research Avenues and Policy Implications

The three chapters open up fruitful avenues for future scholarly investigation. I first propose some of them specific to the examined cases, before suggesting a broader policy implication and a potential future research agenda.

Future Research on presented Case Studies

Considering Chapter two, referendums have become a popular tool to gain legitimacy for international agreements. Voters in, inter alia, Iceland, Ireland, Greece, the Netherlands, and Switzerland have made use of referendum to reject the increasing internationalization of their countries (Walter, 2020). Most closely aligned with the French case is the Dutch rejection of the same European Constitution via a referendum in 2005. The reasons for the no-vote in France and the Netherlands are related, but French voters were more strongly driven by concern around an

undermining of the French social model while Dutch voters emphasized cultural concerns more strongly (Startin & Krouwel, 2013). Nevertheless, the Dutch government similarly opted to bypass the popular vote and to ratify the slightly revised version of the constitution through its parliament, creating a similar uproar as in France (Voermans, 2010). Recent national elections hurled these topics in the focus of national politics, with a far-right Dutch government rallying to victory on concerns about immigration and a loss of Dutch sovereignty and the French Left becoming the strongest parliamentary force based on issues of social inequality, deprivation, and lack of democratic participation in the political decision-making (Cebulak & Lippert, 2024; Fieschi, 2024). Examining whether bypassing the Dutch referendum, but also the cases in other countries, similarly fueled political apathy and resentment with mainstream parties, and how it shaped the form and direction of anti-system movements, would confirm and accentuate the relevancy of the results presented by chapter two.

Considering Chapter three, the chapter frequently invokes another large social program introduced by the PT in Brazil, the *Bolsa Família* program. The *Bolsa Família* is a conditional cash-transfer program of utmost popularity which is said to have strongly expanded the electoral success of the PT party on all levels of government (Souza, 2015; Zucco Jr, 2013). The PMM was presented as a complementary pro-poor policy, tackling local supply constraints instead of individual demand, but nevertheless geared towards alleviating local poverty and deprivation (Castro et al., 2019; Soares et al., 2010). Chapter three is the first project to document that the PMM entailed electoral gains. This is surprising, as previous analyses often find non-financial social policy benefits to be too diffused and abstract to effectively translate into electoral gains (Imai et al., 2020). The PMM's introduction, however, was more salient than other healthcare policies due to the strong debate around it and the involvement of the Cuban doctors. Voters also clearly associated the program with the PT party and knew that its realization hinged upon the PT staying in power. It might thus be that the program had political spill-over effects, by association benefiting other political offices, such as municipal mayors and state senators. Establishing comprehensive insights into the political rewards and complementarity of social policies is crucial for making large-scale policies more politically viable. The more politically rewarding and synergistic a policy is, the fewer

constraints there are in achieving its policy goals (Golden & Min, 2013; Mayhew, 2004). Moreover, while my work focuses on the introduction of the PMM as a significant healthcare policy, it also provides an opportunity to explore the connection between service provision and immigrant workers within the context of extreme political fragmentation regarding migration and preferred policies.

Chapter four, in turn, analyzed the health impact of the breakdown of the international agreement which formed the basis of employment for the thousands of Cuban PMM doctors in Brazil. Importing healthcare professionals is becoming increasingly popular, with even high-income countries such as Italy and Portugal having closed an international contract with Cuba to partly satisfy their great needs for more medical professionals (Lusa, 2023; Pata et al., 2024). Globalization accelerates the strong competition for healthcare workers, leaving less well-off countries, but also regions within countries, struggling to provide essential services to their population (Clark et al., 2006). Thus, targeted public policies and a more effective role of the state in ensuring essential and equal service provision might become more important in various settings. The PMM was a policy designed to remediate the large inequalities in primary care provision within Brazil. However, its position was tenuous and controversial, being fiercely criticized and opposed by parts of the political and medical professional class (Scheffer, 2016). As healthcare in general is becoming an ever-increasing political and partisan issue (Konisky & Richardson Jr, 2012), future research could elucidate whether and how the forced Cuban departure affected political outcomes and institutional trust, and how to better balance political and social aspirations of different stakeholder in order to make policies more resilient and enduring (Sandholtz, 2023).

Research and Policy Outlook: Deliberative Policymaking

The case studies in France and Brazil illustrate the widespread need for increasing the responsiveness of governments faced with regional inequalities. This dissertation agrees with the findings of the previous literature on the need for policies which are tailored to tackle exigent local issues (Iammarino et al., 2019). Even when such policies are in place, they often do not dispel the notion that governments are displaying a geographic bias towards the places already benefiting from globalization, i.e. agglomerations housing highly-skilled professionals (McKay et al., 2024).

Besides explicitly integrating regional and social cohesion metrics into the de-

sign, implementation, and evaluation of national and local policies (Oliveira Martins, 2021), innovative forms of policymaking could be of help to disperse perceived unfairness. An emerging literature is picking up again notions of integrating forms of deliberative democracy into the policymaking process. Deliberative democracy argues that at least part of the political decision-making process should be based on reasoned discussion and debate among citizens. This can take various forms, but often it involves setting up a forum of local citizens, sometimes already including politicians, policymakers, and bureaucrats, to enable meaningful deliberations on preferred policies by a diverse set of local voices. The underlying aspiration is to act according to the social justice imperative to, ultimately, increase democratic legitimacy and quality (Button & Mattson, 1999; Kahane et al., 2010).

France, for example, introduced citizens' assemblies in 2019, bringing together randomly selected citizens to discuss and propose policies, which were then passed on to the national parliament for debate and ratification. This comes as a reaction to widespread social protests who deplored the perceived unfairness of government policies (Giraudet et al., 2022). In Brazil as well, a few cities practice deliberative democracy through regional and thematic assemblies, where citizens are involved in deciding how public funds are allocated in their communities. This participatory budgeting approach allows ordinary citizens to engage in meaningful discussions about local priorities and resource allocation, leading to more transparent and inclusive decision-making (Avritzer, 2006).

Clearly, current structures of liberal, representative democracy are often struggling to give deprived areas the feeling of adequate representation. While giving local voices a say in the policy-making process is no panacea, citizens' assemblies and other innovative institutional designs might be able to mitigate rising feelings of unfairness and alienation (Button & Mattson, 1999). Research should use interdisciplinary tools and theories to design and evaluate such new approaches. The academic body should aid such democratization efforts by conceptualizing fruitful and effective ways of increasing citizen participation in policymaking and evaluate their merits. Given the broad set of involved dimensions, synergistic and interdisciplinary research is best suited to find holistic suggestions for reforms of the policymaking process in representative democracies. Given the urgency to tackle large, international challenges such as climate change, Artificial Intelligence, and

Automation, enabling and facilitating cooperation by fostering social and institutional trust is becoming an ever-more important endeavor.

Chapter 2

Voter Disenchantment in the Aftermath of the 2005 EU Constitutional Referendum in France: An Empirical Examination^{*}

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Abstract

In a popular referendum in 2005, French voters rejected their country's adoption of a proposed EU constitution. Yet, in seeming defiance of the popular vote, the government subsequently proceeded to implement the core of the legislation without again consulting the public. This article empirically examines the electoral impacts of these events. We build a comprehensive fine-grained dataset of nation-wide election results for more than 36,000 metropolitan French municipalities. Employing cross-sectional analysis for all national elections held in the decade after the referendum vote, we find that the strength of a municipality's rejection of the EU constitution in 2005 is associated with (i) lower voter turnout, (ii) higher shares of blank votes, and (iii) larger gains for anti-system parties in subsequent elections. The findings are robust to various modelling choices and the inclusion of a large array of controls. The results indicate that bypassing a popular vote could entail protracted adverse effects for the quality of democratic participation and deliberation.

2.1 Introduction

On 29 May 2005, France voted on the adoption of the Treaty establishing a Constitution for Europe (TCE) in a popular referendum. A surprisingly clear majority of 55 percent of voters rejected the Constitution. Yet, despite the popular rejection, the government continued working on implementing the international agreement. In 2008, the revised version of the TCE, the Lisbon Treaty, was ratified without consulting French voters anew.

The purpose of this article is to examine the electoral impact of this bypassing of the popular vote on subsequent national elections. To do so, we compile a municipality-level dataset containing more than 36,000 French communes, linking the voting outcomes of the 2005 TCE referendum to two cycles of European Parliament (EP), legislative, and presidential elections throughout the decade following the referendum vote. Applying cross-sectional analysis, we evaluate the impact of the bypassing of the referendum on three expressions of voter disillusion with the political process: turnout (passive disengagement), blank votes (active disengagement), and vote shares of anti-system parties on the political fringes.

Ceteris paribus, communes with higher No-vote shares at the referendum in 2005 registered significantly lower voter turnout and higher shares of blank votes in subsequent elections (with the size of effects gradually waning over time), while vote shares of fringe parties increased. Results are robust across various model specifications.

The findings uncover new empirical evidence that the bypassing of popular votes is detrimental to the quality of democratic deliberations. While our empirical analysis delves into one particular case, the findings are of broader relevance. For one, the referendum was an early manifestation of the strengthening political cleavages around globalization and international integration that continue to reshape electoral politics in early 21st-century liberal democracies (Bornschier, 2008; Ivaldi, 2018). Additionally, the bypassing of the French referendum vote represents a paradigmatic example of a more regular feature of contemporary democratic politics, emphasized by accounts of party cartelization (Katz & Mair, 2009), in which governing apparatuses (are perceived to) sidestep popular demands to push through a policy agenda favored by political elites. Our analysis illustrates the potential electoral implications of such behavior in fine-grained empirical de-

tail. In this sense, the article speaks to deeper drivers of the causes and modes of EU disintegration that pre-date the Brexit vote. At the same time, looking forward, popular referendums on questions regarding international integration are becoming increasingly common as the emerging literature on the new “mass politics of international disintegration” has highlighted (Walter, 2020). It is thus likely that more future governments will be facing similar dilemmas as the one confronted by French governing elites in the 2000s, forced to make a choice between respecting international obligations or the popular will. Revisiting the case can help us better understand what is at stake under such circumstances.

2.2 Voter Alienation and the Rise of Anti-system Politics

Established party systems in rich democracies on both sides of the Atlantic have experienced dramatic disruptions over the past decade. Turnout has fallen to record lows (Mair, 2013) and electoral support for centrist mainstream parties has declined rapidly (Trubowitz & Burgoon, 2022). Anti-system protest parties have thrived in many countries (Hopkin, 2020). In response to these developments, a rapidly growing field of literature in political science and economics has set out to examine the roots of this apparent electoral backlash against the political mainstream. Much of that literature has debated the deeper cleavages underlying the politicization of globalization and the extent to which these voter sentiments were driven by cultural or economic factors (Kriesi et al., 2006; Walter, 2021).

Theories of party “cartelization” (Hopkin, 2020; Katz & Mair, 1995, 2009) in particular have suggested that in addition to economic grievances and cultural anxieties, the backlash against international integration is also fuelled by voters’ growing disenchantment with mainstream parties and a loss in trust towards political elites. Patterns in the organization of political parties in advanced democracies emerging in the 1980s and 1990s and firmly established in the 2000s considerably widened the gap between large parts of the population and the political elite. Changes in party financing (e.g. a reduced reliance on member contributions) and an associated professionalization of political elites (e.g. the rise of “career politicians” and political managerialism) coupled with an emerging consensus about the primacy of free markets and the desirability of deep international economic

integration gradually alienated mainstream parties from their voter bases (Blyth & Katz, 2005; Scarrow, 2006). Mainstream parties' discourses increasingly aligned with the views of socio-economic insiders (i.e. the highly educated in urban areas with relatively secure professions) which left little room for meaningful ideological competition (Blyth & Katz, 2005; Jacobs et al., 2021; Keman, 2014; Linsi et al., 2022). As a result, large segments from society, and in particular underprivileged groups, felt less and less represented in national politics and gradually lost trust in political institutions, generating political apathy and laying the ground for the rise of anti-system parties keen to exploit existing grievances (Hopkin, 2020).

2.3 The EU Constitutional Referendum in France

2.3.1 Economic and Political Background

The politicization of globalization and the cleavages underlying them started to become salient in France earlier than in many other Western democracies. Having vocally resisted the full embrace of economic internationalization and visions of a U.S.-led liberal order during its Gaullist period, the government irrevocably shifted towards market-based strategies in the 1980s. The liberalization strategy gradually transformed economy and society by curtailing the government's power to intervene in markets and provide sustenance to its citizens while expanding the free flow of people, capital, and goods (Schmidt, 1996). The imposition of these liberalizing reforms was accompanied by structural changes in the political landscape, not least because they were adopted by a nominally left-wing president, François Mitterrand. Political competition started to move away from traditional "left-right" socio-economic and religious lines towards divisions focused on national sovereignty, culture, and values. European integration, in particular, became one of the most politically salient issues and contributed to an increasingly fragmented party system (Bornschieer, 2008; Grunberg & Schweisguth, 2003). While established centrist parties converged in their approval of an ever-deeper integration into the European and international free-market framework, fringe parties became successful by lumping "the system" together and offering a contrasting alternative (something that became apparent already in the presidential elections of 2002 in which a far-right candidate reached the second round). It was in the context of this shifting political landscape that the referendum was held in 2005 (Bornschieer, 2008).

2.3.2 Background of the TCE

After several years of consultation, the Treaty establishing a Constitution for Europe (TCE) was signed by representatives of all the 25 then-members of the EU on 29 October 2004. The goal of the treaty was to integrate the various EU treaties into one single text and to somewhat strengthen political integration through some modifications of existing rules. After its signing, the treaty had to move through national ratification procedures in all member countries. Whereas 15 member states opted for parliamentary ratification, ten—including France—decided to hold a popular referendum on the adoption of the TCE (Crum, 2007).

2.3.3 The Position of French Parties Before the Vote

The French decision to hold a referendum was taken by then President Jacques Chirac. On the one hand, since France had a history of putting important matters before a popular vote (Morel, 2007), Chirac faced outside pressure to act democratically. On the other hand, Chirac reportedly was confident that the Treaty would easily pass the referendum, boost his image as a capable statesman, and weaken the opposition Socialist Party, which was internally divided over the issue (Hobolt, 2006; Shields, 2006). Early polls indicated a comfortable margin of up to 20 percent in favor of the TCE. Chirac's own party, Union pour un Mouvement Populaire (UMP), was clearly in favor of ratification. The main opposition party, the center-left Parti Socialiste (PS), was split: while most of the leadership advocated supporting the Treaty, a large part of the base opposed it, perceiving it as promoting a "neoliberal" version of the EU (Crespy, 2008; Wagner, 2008). An internal vote finally led the PS to officially endorse the Yes campaign, but the margins were close. The parties on the far Left (most notably the Lutte ouvrière but also the larger and better-known Parti communiste français) and far Right (most notably the Front National) opposed ratification of the TCE, which the Left portrayed as an expression of (Anglo-Saxon) free-market capitalism while the Right framed the treaty as a threat to national sovereignty and the cultural heritage of France (Richard & Pabst, 2005).

2.3.4 The Referendum Vote in France

The first popular referendum on the TCE was held on 20 February 2005 in Spain. A large majority of more than 80 percent of Spanish voters approved ratification. On

29 May 2005, the second popular referendum was held in France. A clear majority of 55 percent of French voters ended up rejecting the Treaty (voter turnout was around 70%, the largest ever for a referendum in modern France (Shields, 2006). The French rejection, together with a similarly clear No-vote in the Netherlands shortly after, derailed the ratification process and the constitutional project was postponed (Crum, 2007; Malet, 2022).

Why did French voters reject the referendum? Political analysts have highlighted both attitudinal and second-order forces (Brouard & Tiberj, 2006; Startin & Krouwel, 2013): the combination of different kinds of Euroscepticisms on the Left (socio-economic) and Right (cultural) (van Elsas, 2017) and the referendum as an opportunity for voters to express discontentment with President Chirac or political elites more generally (Shields, 2006). Exit polls confirm the role of these factors and in particular the salience of socio-economic concerns (Startin & Krouwel, 2013).

2.3.5 The Bypassing of the Referendum Vote

Although the popular rejection of the TCE in France severely interrupted the constitutional project, it did not ultimately halt it. After a “period of reflection” during which the French government made little attempt to address the referendum outcome (De Beer, 2006), the Berlin Declaration signed on 25 March 2007 set out a roadmap for the elaboration of a new version of the TCE. The new treaty simplified the structure of the TCE and omitted some of the dispositions about symbolical issues that proved particularly controversial (e.g. the EU flag and anthem) but in substance took over most of the content that had been rejected by French voters in 2005 (Cleppe, 2009).

The revival of the constitutional project had been boosted in no small part by the French presidential elections in May 2007, which were won by Nicolas Sarkozy who, in contrast to his challenger Ségolène Royal, had painted the acceptance of the treaty as inevitable. Despite strong protests by far Left and far Right parties, as well as trade unions which called the government to respect the people’s will, Sarkozy rushed ratification through parliament. While some abstained in protest, a majority of members of the Socialist Party ended up backing Sarkozy’s strategy. On 7 February 2008, the Lower House and Senate authorized ratification, paving the way to make France the second country (after Hungary) to formally ratify the

Lisbon Treaty.

This bypassing of the popular referendum vote has been described as an important moment in the observed growth in voter apathy and Euroscepticism in France. Liberal activists decried politicians' concerted negligence of the popular vote as a "betrayal" of democratic principles (Cleppe, 2009) which exposed underlying "democratic deficits" (De Beer, 2006). Jean-Luc Mélenchon, one of today's most popular leftist politicians in France, accused elites "to despise" the people (Le Monde, 2005). The Communist Party leader Marie-Georges Buffet remarked that "[t]he referendum marked a break. People voted and there has been a refusal to accept the decision of the people. Disillusionment settled in" (Local, 2016). Or as the president of a progressive pro-EU movement (Sauvons l'Europe) phrased it: "It's not surprising that people don't care anymore" (Local, 2016).

2.4 Research Design, Data, and Methods

2.4.1 Research Design

In our empirical analysis, we test claims of voter frustration econometrically. Zooming in on French municipalities ("communes"), we evaluate the relationship between the strength of rejection of the TCE in a commune and its political behavior in the aftermath of the referendum vote and its bypassing. We focus on three outcome variables: First, turnout at elections as an indicator of the degree of political participation and civic engagement (Mair, 2013). Second, the number of blank or invalid votes cast at elections as a proxy of protest voting and expression of frustration with the political system (Driscoll & Nelson, 2014; Ugglå, 2008). Third, vote shares of mainstream vis-à-vis anti-system parties on the far Right or Left (Hopkin, 2020; Mair, 2013; Trubowitz & Burgoon, 2022).

Our prior expectation is that as it became increasingly clear that the government would bypass the referendum outcome, political participation decreased, and support for anti-system parties increased in municipalities in which the referendum was rejected by a larger margin. We thus expect a negative relationship between the municipality-level No-vote share at the referendum in 2005 and turnout levels in subsequent national elections; a positive relationship with the number of blank votes; a negative relationship with vote shares of traditional centrist parties (PS and UMP) and a positive one with anti-system parties on the fringes. Since party vote

shares can be strongly contingent on idiosyncratic election-specific factors (such as candidates' popularity or evolving party coalitions) and because France's political party landscape is known to be particularly volatile (Bornschier, 2008), we expect these relationships to be less clear-cut than those for turnout and blank votes. Furthermore, in line with previous literature on voter "memory" (Bechtel & Hainmueller, 2011), we expect effects to be strongest in elections taking place during and soon after the bypassing process and fading out after a few years.

Having no detailed and comprehensive data on individual voters' opinions, our unit of analysis is the municipality and we focus on variation in electoral politics between municipalities. Although it is individuals, not municipalities as such, who vote, a substantial body of evidence indicates that local environments and cultural milieus influence vote choices of individual voters (Ansolabehere et al., 2014; Harteveld et al., 2021; Rodríguez-Pose, 2018; Walsh, 2012). According to this literature, local conditions and interactions can serve as filters of perception that play a role in shaping political attitudes and voting behavior. David et al. (2018) argue that the level of municipalities is optimal in identifying such effects statistically. Applied to our case, we assume that individual voters take cues from the (municipality-level) communities they are living in.

2.4.2 Data

Using election results published by the French Ministry of Interior and population census data from the French National Statistical Office (Insee), for our main analysis we build a municipality-level dataset containing the two European Parliament (EP) elections in 2009 and 2014 and the two first rounds of the presidential and the legislative elections held in 2007 and 2012. Municipality-level outcomes from these six national elections are linked to the local outcomes of the 2005 referendum.

We limit our sample to mainland municipalities and to municipalities which are available throughout the sample period (including the pre-referendum years 2002 and 2004). Elections in Corsica and in the overseas territories tend to follow somewhat different dynamics from the mainland and are more strongly influenced by local issues, such as the independence movement in Corsica or the legacy of colonialism in the overseas territories (De la Calle & Fazi, 2010; Hintjens et al., 1994). Additionally, for many overseas departments, election and referendum re-

sults are not reported at the municipality-level. We therefore exclude Corsica and overseas territories from our main analyses. (Additional analyses available in the supplementary files show that results are not sensitive to the inclusion of the available data from Corsica and overseas territories).

To control for various socio-economic factors in our regression analyses, we match the data on election and referendum outcomes with the respective annual municipality-level information provided by Insee. This data-collection effort yields a database with complete information on about 33,000 French communes (out of a total of around 36,200 mainland communities in 2005 according to Insee) over the referendum vote and two subsequent national election cycles.

2.4.3 Variables

Our main dependent variables are voter turnout and blank votes as indicators of voter (dis-)engagement. In further analyses, we also examine the impact of the referendum vote on party vote shares. Our primary independent variable is the municipality-level No-vote share at the constitutional referendum of 2005, which is depicted geographically in Figure 2.1. All electoral information is from the Ministry of the Interior.

To control for relevant political, socio-economic, and demographic characteristics of communes, we include a large array of relevant municipality control variables which we source from Insee (for a full list, see the notes beneath Table 1. We provide detailed descriptive statistics in the supplementary files to this article.)

2.4.4 Empirical Strategy

We estimate our main results with standard linear regression analysis. In additional analyses, we check the robustness of our findings in a beta regression model. Our modeling choices are justified in greater detail in the supplementary material.

To minimize the threat that unobserved variables are driving our results, we (i) include a large array of socioeconomic, demographic, and political control variables; (ii) conduct Oster tests (OT) (Oster, 2019) which evaluate treatment selection on unobservable variables and assess coefficient stability (test results above the value of 1 indicate that omitted variables would need to be at least as relevant as all included variables together to nullify the effect of the tested coefficient); (iii) check that there is no effect on most pre-referendum outcomes and (iv) check our

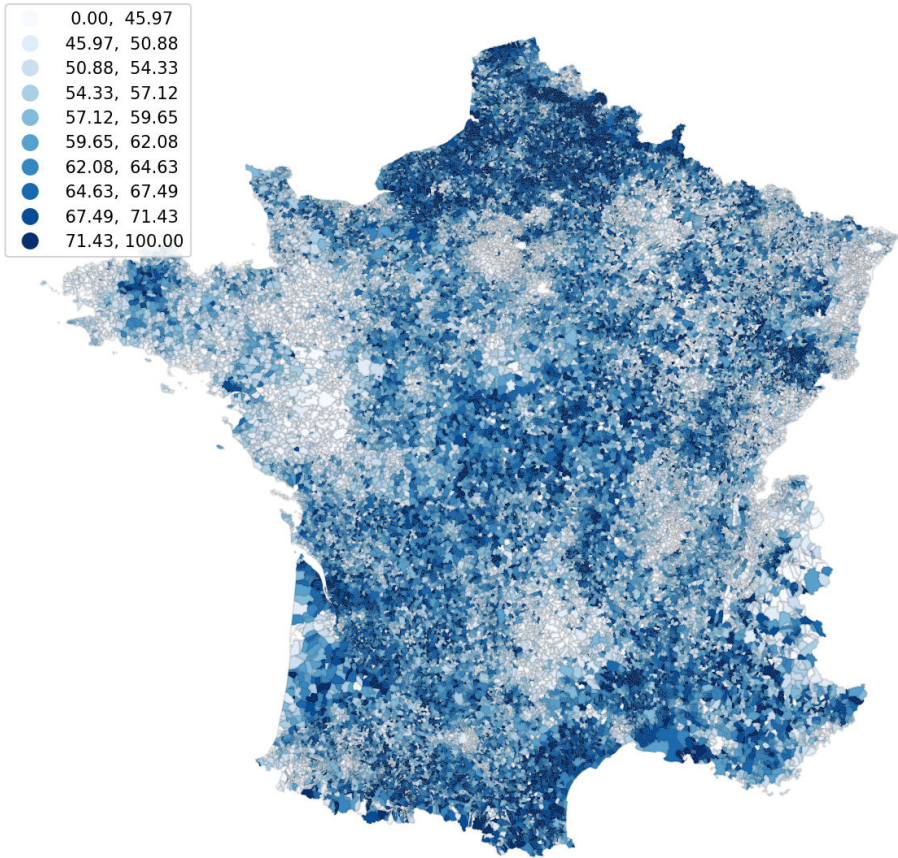


Figure 2.1. Municipal Voting in the French Referendum 2005. Note: Municipalities are allocated into deciles according to the strength of their No-vote; the darker the shade of blue, the higher the municipal No-vote.

results with an alternative estimation technique (all tests are provided in the supplementary files).

We estimate cross-sectional regression analyses covering presidential, legislative, and European parliament elections from 2007 to 2014. For a randomly drawn commune i at election year t , we specify our full model as follows:

$$y_{it} = \beta_0 + \beta_1 \text{No-vote}_{i,2005} + X'_{it}\beta_3 + \beta_4 y_{ipre-ref} + \theta_i + u_{it}$$

The dependent variable y is either turnout, blank vote share, or vote shares of specific parties. The explanatory variable of interest is the intensity of the municipal rejection of the EU constitution, measured by the percentage share of No-votes among the valid votes in the referendum in France in 2005. The control matrix X represents a battery of political, economic, and sociodemographic municipality controls. To account for political cleavages around globalization and international integration that predate the referendum vote and which might have also mattered for the referendum and subsequent elections, we include the variable $y_{ipre-ref}$, which represents the respective outcome of the most recent election of the same kind (i.e. presidential, legislative, or European election) held before the referendum in 2005. The inclusion of this “lagged” outcome variable ensures that the coefficient of the No-vote variable measures the referendum’s effect on current differences in turnout or party support, all else being equal (including distinct outcome levels in the election predating the referendum). It furthermore diminishes the threat of omitted variable bias since many unobserved factors that affect both municipal election outcomes after the referendum and its referendum No-vote are likely to be absorbed by previous election outcomes (Wooldridge, 2013, p.313).

To facilitate interpretation, all continuous variables are standardized. Finally, the regional fixed effects θ_i capture broad unobserved characteristics of 21 NUTS2 regions. We cluster our standard error over 95 departments to allow for potential correlation in the error term of municipalities located in the same department.

2.5 Main Results

We first show how the referendum vote affected electoral participation in the form of voter turnout and blank votes in subsequent elections. Then we turn to investigate the effects of vote shares of principal mainstream and anti-system parties. Our

presentation prioritizes the outcomes of EP elections which are of lower salience and thus arguably less influenced by idiosyncratic factors (such as strategic voting, individual candidates, changes in party landscapes, or particular local issues) than either presidential or legislative election which can make them more valid to assess broader underlying trends and revealed voter sentiments (Van der Brug et al., 2007, p. 5; Van der Eijk et al., 1996). Moreover, and specifically with respect to party vote shares, the European elections might also be the type of elections most likely to capture rejection of further European integration. This being said, we conduct all main analyses at the level of legislative and presidential elections as well. Findings that are consistent across the different types of elections are particularly strong because they are robust to election-type-related idiosyncrasies.

2.5.1 Electoral Participation

Table 1 presents the regression results of the No-vote on EP elections turnout in 2009 and 2014. The first three columns show the impact of the No-vote on municipal turnout rates in 2009, gradually adding control variables to the model until arriving at the above-defined full model in column (3). Columns (4)-(6) repeat this exercise for the election in 2014.

Throughout all model specifications in both elections, the No-vote is consistently negatively associated with municipal turnout. The first specification includes past political variables (namely the municipality turnout in the EP election in 2004 and participation in the referendum in 2005) as well as fixed effects for the 21 French NUTS2 regions. In the second column, municipality-level control variables are added to the equation. Finally, in column (3), municipality-level median income is included, restricting our sample size by excluding very small municipalities due to data availability).

Looking at the fully specified model results in column (3) and (6), the control variables attenuate the impact of the No-vote, which indicates that disregarding socio-economic characteristics entails an upward bias in the coefficient of the No-votes. OT deltas exceed unity, making it highly unlikely that the correlation between the No-vote and the EP election turnout in 2009 and 2014 is spurious. A one standard deviation higher No-vote (about 10.1 pp, see appendix Table A2) is associated with a lower turnout of around 0.61 percentage points (pp) in 2009 and

with 0.25 pp in 2014, *ceteris paribus*. To put the magnitude of these coefficients into perspective, in 2009, a one standard deviation higher unemployment rate (18.9 pp) decreases turnout by 0.17 pp (0.15 pp in 2014), and in a municipality with a one standard deviation higher share of university graduates (31.6 pp), turnout increases by 0.81 pp (0.80 in 2014), *ceteris paribus* (full regression results of every equation are available upon request). The effect of the No-vote for turnout is hence substantive, at least for the EP elections in 2009.

In the 2014 EP election, the impact is smaller in line with our expectation of the referendum vote's waning importance over time, but its impact is still larger than a one standard deviation difference in unemployment. In additional analyses, we further extend the timeframe to the EP elections held in 2019 where the association between the municipality-level No-vote and turnout disappears. This suggests that the referendum vote by itself ceases to be an important factor for electoral outcomes in France beyond the first decade after the referendum was held. This is in line with the literature on electoral myopia which argues that voter memory on policy rarely extends two election cycles (Bechtel & Hainmueller, 2011). Another consideration is important changes over that period in the French political landscape, where former chief actors in the referendum process, such as the PS and UMP, had lost much of their electoral relevance by 2019.

Estimation results on legislative and presidential elections corroborate this picture. Figure 2.2 graphically illustrates the results of applying the full model on all three election types (underlying regression tables available in the supplementary files).

There is a significant and substantive effect for all immediate post-referendum elections, which is strongest for EP elections (which is the “most likely case” for the reasons discussed above) followed by legislative and presidential elections. Overall, the results suggest that the more strongly a municipality voted against the TCE, the more people abstained from voting in major national elections following the referendum's bypassing. At the same time, the participation-depressing effects appear to have largely dissipated in the second election cycle: there is no longer any statistically significant association for the legislative and presidential elections of 2012, and the size of the coefficient for the EP election in 2014 more than halves (and finally dissipates for the election in 2019).

Table 2.1. No-vote Share and EP Election Turnout; Stepwise Inclusion of Control Variables

	EP elections 2009			EP elections 2014		
	(1)	(2)	(3)	(4)	(5)	(6)
No-votes	-0.954*** (0.0663)	-0.525*** (0.0602)	-0.614*** (0.0609)	-0.634*** (0.0893)	-0.211*** (0.0778)	-0.254*** (0.0682)
Municip. Controls	No	Yes	Yes	No	Yes	Yes
Median Income	No	No	Yes	No	No	Yes
Election controls	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
N	36166	36164	33047	36166	36164	33047
Adj. R ²	0.60	0.64	0.65	0.50	0.57	0.58
OT delta			1.74			1.12

Notes: Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older (4) the share of residences that lay vacant (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate) (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor (9) agriculture (10) education and science and (11) artisans (such as craftsmen, tradesmen, and small business owners) (12) the population's immigration share and (13) the unemployment rate. Median income is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * p < 0.10 ** p < 0.05 *** p < 0.01

2.5.2 Blank Votes

The share of blank votes is our second measure of voter disengagement. To the extent that casting a blank vote requires a greater effort than not voting, it can be considered a more “active” form of expressing voters’ discontent with the political system (Driscoll & Nelson, 2014; Uggl, 2008).

The results for blank votes are presented in Figure 2.3. The findings mirror the turnout results: all else equal, the share of blank votes was significantly higher in municipalities that had a higher No-vote share immediately following the referendum’s bypassing. As with turnout, municipality differences become null for the legislative and presidential elections in 2012 but remain statistically significant for the EP election in 2014 (before again dissipating in 2019).

The results presented so far thus strongly suggest that the bypassing of the referendum outcome further fueled voter disengagement in municipalities that had

rejected the EU constitution.

2.5.3 Party Vote Shares

To evaluate the impact of the referendum's bypassing on municipality-level party support, we focus on the combined vote share of the two centrist mainstream parties at the time of the referendum, PS and UMP, and anti-system forces on the fringes, the Front National on the far Right, and a group of parties associated with the far Left (see Appendix for detailed listing).

Looking at party vote shares at all six elections (figure 2.4), we observe some volatilities as is to be expected given the different types of elections and the evolution of the French party system over the sample period. Yet the general pattern is in line with our expectations: while the municipal vote share for anti-system parties correlates positively and strongly with the referendum No-vote, the established centrist mainstream parties lost support in municipalities that had more strongly rejected the TCE. The decline for the traditional mainstream parties is driven by vote share losses of the UMP while both the far Left and the far Right experience

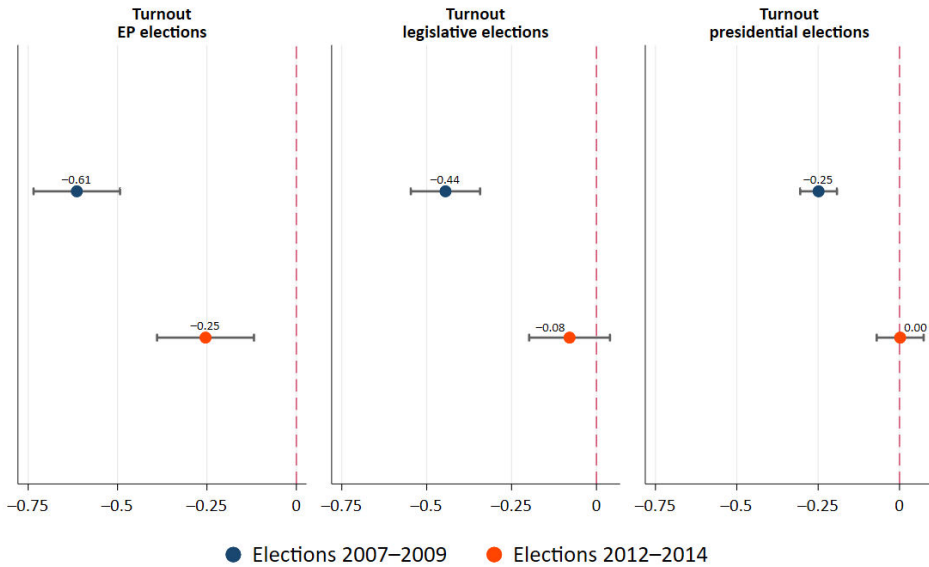


Figure 2.2. Association Between Municipal No-vote and Turnout in National French Elections 2007-2014. Notes: Results are from the fully specified model with all covariates included (as in column (3) and (6) in table 1). Dots represent the estimated coefficients; whiskers show the 95% confidence interval around them.

gains in the aftermath of the referendum.

An important nuance of the party vote share analyses also is, however, that results are getting stronger over time (also beyond our sample period) which stands in contrast to turnout and blank voting results. We interpret this as an indication that in the aftermath of the referendum and its bypassing, populist anti-system parties were successful in mobilizing apathetic disaffected voters, particularly with Eurosceptic and anti-establishment platforms (Ivaldi, 2018; Krouwel & Abts, 2007). At the same time, it is also plausible that over time, factors other than the referendum itself started to play a greater role in the mobilization of disaffected voters by anti-system parties. In that sense, the referendum may have fed into—and further amplified—pre-existing cleavages.

In sum, our analyses uncover evidence for all three types of voter disengagement in response to the bypassing of the referendum. Municipalities that had more strongly rejected the referendum experienced lower turnout, more blank votes, and higher vote shares for anti-system parties in subsequent elections. Underlying re-

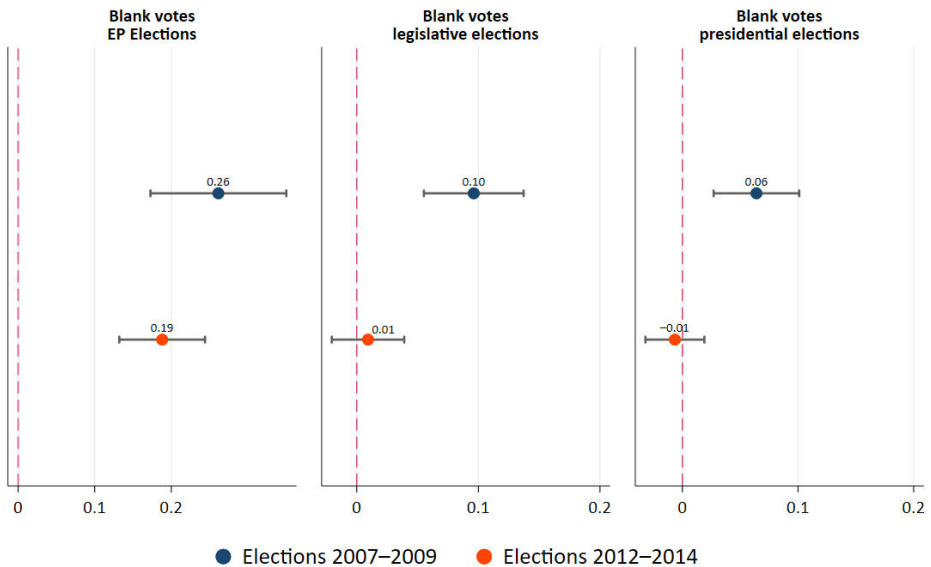


Figure 2.3. Association Between Municipal No-vote and Blank Vote Share in National French Elections 2007-2014. Notes: Results are from the fully specified model with all covariates included (as in column (3) and (6) in table 1). Dots represent the estimated coefficients; whiskers show the 95% confidence interval around them.

gression tables for these results are provided in the supplementary files.

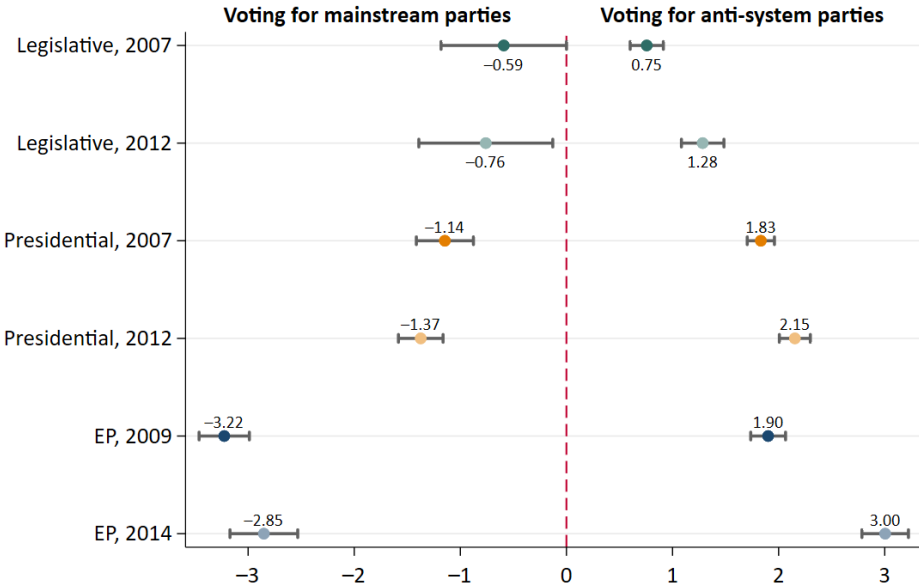


Figure 2.4. Association Between Municipal No-vote and aggregate vote shares in National French Elections 2007-2014 (aggregate share of the established traditional parties on the left side and for the Far Right and Left on the right side). Notes: Results are from the fully specified model with all covariates included (as in column (3) and (6) in table 1). Dots represent the estimated coefficients; whiskers show the 95% confidence interval around them.

2.5.4 Survey Data Analysis

In a final step, we assess whether the referendum and its bypassing can be linked to changes in individual attitudes. We do so by looking at trends in satisfaction with French and European democracy from 2000 to 2015 as expressed by French respondents in Eurobarometer surveys.

Given the central role played by the French government in the bypassing of the referendum, we would expect that French voters were more pessimistic about the democratic functioning of their state, especially so in regions which voted more strongly against the TCE. At the same time, to the extent that the electorate perceived the EU as an accomplice in or even reason for ignoring the popular demands voiced by the No-vote, there might have been a backlash against the EU as well.

The two Eurobarometer survey questions that we investigate in more depth are:

- “On the whole, are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy works in France?”
- “And how about the way democracy works in the European Union?”

We split the sample into three groups according to the intensity of the No-vote of the respondents’ residence (the Eurobarometer provides information only on the NUTS2-level) and recode the dependent variable to go from 0 (not at all satisfied) to 1 (very satisfied). We then perform event studies for each group regressing the Eurobarometer answers on region and year fixed effects using as a reference point the year 2000.

Figure 2.5 depicts the coefficients for the yearly fixed effects for both outcomes. Dots represent point estimates, i.e., relative differences to the assessment in the base year 2000, and the whiskers indicate statistical significance of these differences on the 95% confidence intervals. We highlight the referendum in May 2005 and the official ratification of the Lisbon treaty by the French Parliament in February 2008 with dashed vertical grey lines. We interpret divergences between the three groups after the referendum and its bypassing as an indication that the regional No-vote mattered for individual sentiments towards the EU.

While results are necessarily cruder due to the higher level of geographical aggregation (NUTS2 level instead of municipalities), we observe some interesting patterns. The top panel shows that satisfaction with French democracy is relatively stable until the referendum in 2005. Beginning in 2005, the average assessment drops and recovers only for short “honeymoon period” stints in the election years of 2007 and 2012. For every other year, the average assessment is relatively more pessimistic across all regional clusters. For example, for the years 2013–2015, the assessment is around 0.08 points lower than for the base year 2000. While the entirety of the country expressed greater dissatisfaction during this period of François Hollande’s presidency, there was notably more regional disparity in the years immediately following the referendum. In 2005 and 2006, years in which the media covered the referendum and its bypassing extensively, satisfaction with French democracy hit a low and especially so in regions which voted more strongly against the TCE. However, Eurobarometer respondents expressed these negative perceptions only for a confined time.

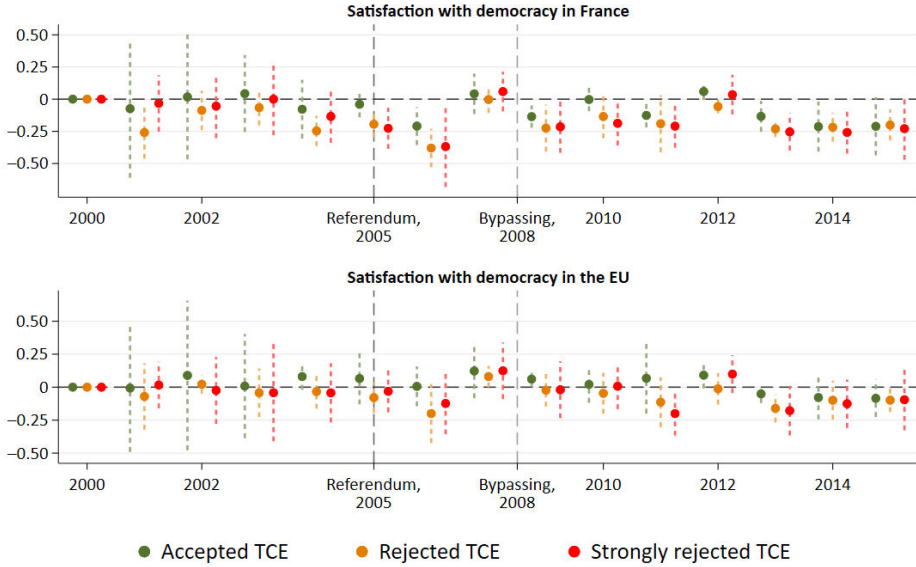


Figure 2.5. Eurobarometer trend assessing average satisfaction with French and EU democracy in the years 2000 to 2015. Notes: The markers show decreases and increases relative to the base year 2000. Differences are statistically different from 0 when the dashed whiskers do not cross the grey horizontal line.

The bottom panel shows the same trend but with respect to democracy in the EU. Dissatisfaction with the EU seems more muted and fewer obvious trends are discernible. In this sense, the analyses suggest that the French electorate does not seem to have blamed the bypassing of the referendum on European institutions in particular. The backlash from the bypassing thus appears to have more strongly affected views on domestic democratic processes.

2.6 Conclusions

Our analyses leverage the bypassing of the popular referendum on the TCE in France as a case study to empirically examine the electoral consequences of technocratic political engineering and the cartelization of mainstream political parties more broadly. Our analyses uncover evidence that turnout decreased, share of blank votes increased, and support for anti-system fringe parties grew in municipalities that had more strongly rejected the referendum. The findings suggest that the decision of the French government to ratify the Lisbon Treaty led to both voice

and exit behavior among French voters, with adverse consequences for the quality of democratic participation and deliberation in France. In particular, it appears to have further fueled the sustained decline of electoral participation and support for traditional centrist parties in France. Survey assessments corroborate that the referendum process seems to have contributed to decreased satisfaction with French democracy and its institutions. The behavior and structure of political parties might thus have contributed to increasingly widespread perceptions among the electorate to be living in a deficient democracy (Keman, 2014).

In an effort to strengthen the democratic legitimacy of international institutions, popular referendums on inter-governmental agreements have become more frequent in Europe in recent years (Walter, 2020). However, perceptions about the benefits of international cooperation often diverge between a nation's decision-makers and its population (Dellmuth et al., 2022). When voters reject an international treaty in such a vote—as they did in the 1992 Danish Maastricht Treaty referendum, the French TCE referendum in 2005, the Irish vote on the Lisbon treaty 2008, or the Brexit vote in 2016—they can create difficult dilemmas for governments who can be left to choose between breaching its multilateral obligations and relations with other states (as in the case of Brexit), forcing a re-negotiation with their partners (as in the case of Denmark 1992 or Ireland 2008), or ignoring a democratically taken decision at the risk of fostering distrust towards elites and eroding electoral participation (as in the case of France 2005) (Morel, 2007). Either way, the stakes are high. Understanding the causes and modes of EU disintegration thus make it imperative to improve our understanding of the tensions between collective decisions taken at the international and the domestic level and how they affect the quality of governance and the viability of international cooperation in the longer run.

Chapter 3

Policy Targeting and Voting Outcomes: Evidence from a Health Care Program in Brazil^{*}

^{*}The author is thankful for comments and suggestions from Amma Panin, Flávio Eiró, Gabriel Lyro, Jan Priebe, Lukas Linsi, Maximilian Guigas, Milena Nikolova, Rob Alessie, and Viola Angelini.

Abstract

This paper investigates whether the “Programa Mais Médicos”, a large-scale health-care access program initiated by the Brazilian government in 2013, yielded electoral benefits for the party that implemented it. By the 2014 presidential elections, the program had brought over 14,000 primary care doctors to mostly remote and poor municipalities. Employing a matched difference-in-difference estimation, I find that in participating municipalities, the program increased the incumbent’s vote share by 0.9 percentage points in the first voting round of the 2014 presidential elections, up from an average vote share of 50 points in the reference elections of 2010. The impact on the second voting round is similar but extends to the 2018 elections. In the competitive Brazilian presidential elections, this represents a meaningful impact. However, the political impact varied across municipalities. I argue that the government targeted some municipalities due to equity concerns, aiming to improve health care access, but also targeted others with the strategic goal of improving re-election chances. The latter group consists of electorally competitive constituencies with little to no need for federal health care support. I find that municipalities targeted due to equity concerns increased their vote share by 2.1 and 1.2 points in the first and second round of the elections 2014, respectively. Electorally targeted municipalities saw no first-round gains but saw increases of 0.7 and 0.9 points in the second round of 2014 and 2018, respectively. The results suggest that improving healthcare access can lead to electoral rewards, especially where there is local need for the policy.

3.1 Introduction

One of the main responsibilities of governments is to provide public goods and services, intended to increase welfare and equity among the population (Batina & Ihori, 2005, p.1, p.35). In democracies, incumbent governments often employ such policies strategically to increase their re-election chances. These two objectives often imply a trade-off between efficiently achieving socially desired policy outcomes and electoral returns (Golden & Min, 2013; Luna & Mardones, 2016).

Health care often accounts for the largest component of public expenditure, and is increasing steeply worldwide. Nevertheless, health care quality and access remain highly unequal on a global scale but also within countries (OECD, 2021). The latter is especially true for developing and emerging economies, in which the government often needs to assume an active role in ensuring a more equal and equitable health care access. Such government interventions tend to be large, costly, and re-distributive in nature, as they disproportionately benefit lower-income individuals who rely on the public provision of health care. Related policies can therefore also politically be consequential because they imply public budget trade-offs and/or a higher tax burden (Mobarak et al., 2011).

While extensive literature exists on the electoral returns of economic policies (Diaz-Cayeros et al., 2016; Linos, 2013; Manacorda et al., 2011; Zucco Jr & Power, 2013) and there is an emerging interest in social policies in domains such as education (Acemoglu et al., 2021; Sandholtz, 2023), the potential returns on health care policies remain relatively understudied. This is surprising given the political salience of the healthcare sector. A burgeoning body of the political science literature studies the reverse, i.e how political conditions foster certain public health care approaches and policies (Lynch, 2023; Mobarak et al., 2011; Navarro et al., 2006). There is ample evidence that healthier people are better able to participate in politics (Mattila et al., 2013; Pacheco & Fletcher, 2015). There is also some evidence on voters' preferences in terms of healthcare systems, such as mainly relying on the private or public provision of health care (Konisky & Richardson Jr, 2012). However, there is not much rigorous evidence on how health care policies, and their potentially strategic employment, affect elections.

This article contributes to filling this knowledge gap by analyzing a large-scale Brazilian health care policy. Brazil is not only one of the world's largest democracies

but also home to the largest universal healthcare system globally (Castro et al., 2019). Approximately 75% of the country's 215 million inhabitants rely exclusively on publicly provided health care, with only the more affluent primarily benefiting from private health care (Rocha et al., 2021). Despite the universal nature of health care provision, access to and quality of it is highly unequal across regions (Paim et al., 2011). In 2011, large cities, such as São Paulo and Brasília, hosted more than 2 doctors per 1,000 inhabitants, a ratio comparable to European or U.S. standards. However, many rural regions had fewer than 0.45 doctors per 1,000 inhabitants, a value similar to much of the developing world (Oliveira et al., 2016). Analysts argued that only sustained public investments will be able to reduce the entrenched disparities (Castro et al., 2019; Massuda et al., 2018; Mobarak et al., 2011; Sánchez-Ancochea & Mattei, 2011).

Responding to this need, the Brazilian government introduced the *Programa Mais Médicos* (PMM), which in English stands for the "More Doctors Program", in 2013. The main goal of the PMM was to provide economically and socially vulnerable municipalities with thousands of primary care doctors, thereby addressing the large regional inequalities in health care access and quality. Previous evidence found that the program indeed considerably expanded access to primary health care and improved health outcomes (Carrillo & Feres, 2019; Fontes et al., 2018; Hone et al., 2020).

However, going beyond the program's previously studied impact on health care, this paper analyzes whether the program also mattered politically. The PMM came to be intricately linked with its political architects, the then-incumbent *PT* (acronym of the Portuguese *Partido dos Trabalhadores*), the *Workers' Party*, Brazil's biggest leftist party. The PMM represented an ambitious government intervention in the local health care market, in line with the PT's progressive stances of using public funds to combat inequality. At the same time, conservative opposition parties were strongly opposed to the program. Already for the presidential elections in 2014, one year after the PMM's introduction, the program was coined a "trump card" for the PT's re-election bid (Bergamasco, 2013). The PMM also featured heavily in the presidential debates and campaigns of the elections in 2018 and 2022 (Quintella, 2022; Silva et al., 2018).

This paper examines whether the introduction of the PMM was electorally con-

sequential for Brazilian presidential elections. For this, I am comparing trends in the PT vote share in municipalities which participated in the program with those that did not. I employ a matched Difference-in-Difference (DiD) event-study regression on a dataset containing roughly 4,300 Brazilian municipalities that spans six presidential elections held between 2002 to 2022, with each having two voting rounds.

I distinguish the PT vote share in first and second round of the presidential elections in Brazil. In first round elections, multiple candidates vie for obtaining the absolute majority of votes, i.e. over 50% of all cast votes. Usually, no candidate achieves this, and voters then decide between the two leading candidates in the second voting round. Thus, first round voting better reflects ideological and polity support for the party and candidate, as the chosen candidate is the voter's first preference. Second round voting, in turn, can also be driven by opposition to the other remaining candidate (Plutowski et al., 2021; Zucco Jr & Power, 2013). In my sample period, the second voting rounds always cast a PT candidate against a conservative rival who opposed the PMM program. Thus, while first voting rounds could measure increased PT support, second voting rounds could additionally include strategic considerations, such as preventing an abandonment of the PMM.

I find that the PMM had a significant but short-lived electoral impact. From an overall sample baseline of roughly 50% in the first voting round and 56% in the second voting round, the PT's vote share increased by 0.9 percentage points (pp) in participating municipalities in both rounds of the presidential elections of 2014. This increase represents more than a fifth of the difference between treated and non-treated municipalities at baseline and is hence considerable. It is also roughly two thirds of the general electoral impact of *Bolsa Família*, a Brazilian conditional cash transfer program which has been found to have produced strong electoral gains for the PT (Zucco Jr & Power, 2013).

However, I argue that these results, comparing PMM participation with non-participation, mask considerable heterogeneity. Previous evidence noted that it is not clear why some municipalities were allowed to participate, as they were neither vulnerable nor needy (Oliveira et al., 2016). Thus, after investigating the aggregate participation trends, I distinguish PMM municipalities based on policy need, which the program assessed mainly based on poverty indicators. I argue that a part of the

PMM municipalities was able to participate due to the underlying policy goal of improving *equity* in health care access, and the other part, at least partly, due to *electoral* concerns. Using information on a municipality's government classification, need for federal healthcare funding, past voting results, and timing of PMM accession, I identify municipalities targeted for electoral versus equity reasons. I find that the PMM policy was electorally most rewarding in municipalities which were targeted due to equity concerns. In these municipalities, the PT's vote share increased by 2.1 pp in the first voting round and 1.2 pp in the second voting round in 2014. In contrast, electorally targeted municipalities did not show increased PT support in any first rounds, but saw an increase of 0.7 and 0.9 pp in the second round of voting in the election of 2014 and 2018 respectively, potentially indicating increased political polarization concerning social programs in Brazil.

This paper contributes to our understanding of how and why broad health care policies can be electorally rewarding. It is one of the few studies that causally links governmental health care interventions with election results (Braga, 2020; Camargo, 2021; Imai et al., 2020; Konisky & Richardson Jr, 2012). It is also the first study that stratifies the effects of a health care policy based on policy goals, distinguish between targeting based on equity and electoral considerations and evaluating the respective political returns. Previous evidence suggested that policies accrue electoral benefits through strategic targeting or "pork-barrel" politics and that there is a fundamental trade-off between socially desired policy outcomes, such as increased equity and welfare, and politically desired outcomes in the form of electoral rewards (Imai et al., 2020; Lizzeri & Persico, 2001). In my setting, electoral rewards are greater when there is greater local need for the policy. This suggests that the political logic of healthcare access policies can in fact be complementary to the local social needs. These insights are especially relevant for broad public policies in areas such as health care and education, which usually target groups or places rather than individuals, and which require considerable investments and are often deemed politically not viable due to the lack of electoral returns (Imai et al., 2020; Sandholtz, 2023).

The paper proceeds as follows: Section 2 introduces the context and shows that PMM municipalities are very heterogeneous. Section 3 draws up a conceptual framework from the existing literature and formulates research questions mo-

tivated by suggestive evidence based on the underlying data. Section 4 describes my data sources. In section 5, I introduce the methods used to obtain the empirical results of section 6. Section 7 addresses limitations and future avenues of research, and Section 8 concludes.

3.2 Context

3.2.1 Political Context

Brazil has a bicameral, presidential democratic system. Voting is compulsory, with non-adherence being moderately fined, making turnout a less salient voter response to policies than in non-compulsory voting settings (Power, 2009). A presidential candidate needs the absolute majority of votes, which is usually achieved in two rounds of voting, with the second round of voting pitching only the two strongest candidates against each other.¹

Brazil's political landscape is very fragmented and most political parties are weakly institutionalized. This means that many voters cast their vote for candidates, not necessarily for a party. However, the PT is often described as an exception to this rule, as the party displays a consistent and coherent ideology which emphasizes social inclusion and equitable growth as the tenets of its political program. Effectively, this means that voters associate PT policies with the party, and not just with their candidate (Ribeiro, 2013; Samuels, 1999). The party has been the preponderant political force in Brazil in the current century. Figure 3.1 shows the vote share of the PT candidate and its main opponent in the first round of Brazilian presidential elections 2002-2022.² Primarily carried by Brazil's middle class to their first presidential victory in 2002, the party successfully expanded their voting base to the poorer strata of society in 2006, mainly due to the introduction of,

¹ While there are multiple elections next to the presidential elections, such as municipal, legislative, and parliamentary elections, this paper focuses on presidential elections. This is because the PMM represent a flagship *federal* policy of the PT. The program's implementation stands under direct control of the Ministry of Health and the federal government. While implementation needed to rely on a certain level of municipal co-administration, previous federal programs were able to reach target groups despite differences in the local administrative capacity and without large local political interference (Sugiyama & Hunter, 2013). The public would thus primarily associate the program with the president of the party, and it was also discussed as such by the Brazilian media (ABM, 2013; Bergamasco, 2013). Nevertheless, it is possible that the PMM might have also entailed complementary benefits for other elections, such as the one deciding on the municipality's mayor (Zucco Jr, 2015).

² The first round of voting includes a variety of options, and thus better shows ideological and polity support for the party and candidate, as opposed to rejection of the single opposition candidate in the second round of presidential elections in Brazil (Plutowski et al., 2021; Zucco Jr & Power, 2013).

and successful party association with, the globally largest conditional cash transfer program *Bolsa Família* (Zucco Jr & Power, 2013). After 2006, the PT's vote share gradually decreased, bottoming-out in the elections 2018, the only popular presidential vote the party lost in the last 20 years. This decrease was mainly driven by adverse economic conditions and corruption scandals, but the PMM might have cushioned the downturn in participating municipalities (Zucco Jr, 2015).

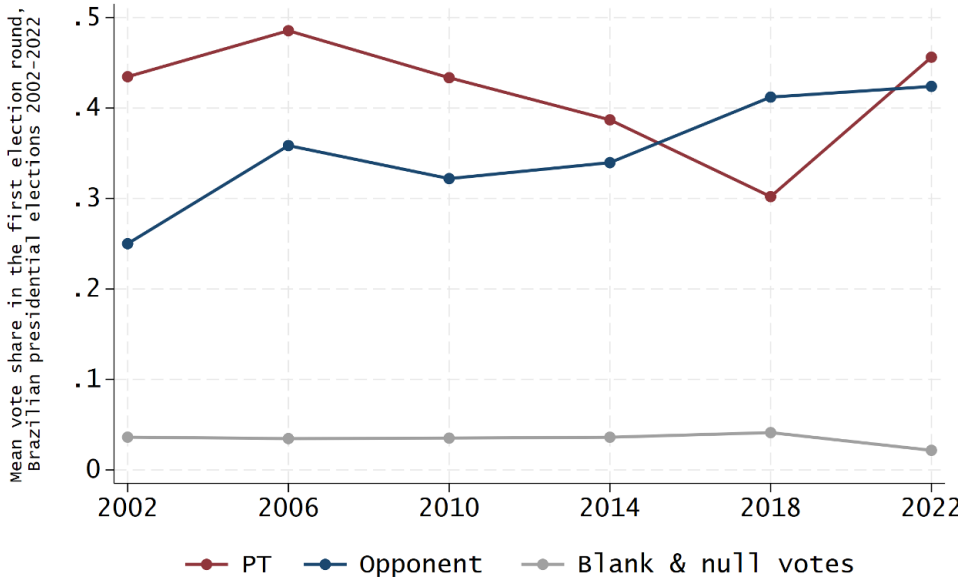


Figure 3.1. Mean first round vote share of the two main candidates, Brazilian presidential elections, 2002-2022.

3.2.2 Programa Mais Médicos: Municipality Classifications

The PMM was devised to tackle some of the key equity challenges faced by the health care sector. The program's most salient feature was the strong and immediate expansion in the number of primary health care doctors in targeted municipalities.

Figure 3.2 shows the total number of doctor contracts in public primary care centers in Brazil from 2012 to 2022, as well as its fraction occupied by PMM doctors. It shows that since its inception the PMM is a meaningful component of Brazilian primary health care provision.

Participation in the program was mostly conditioned on poverty indicators

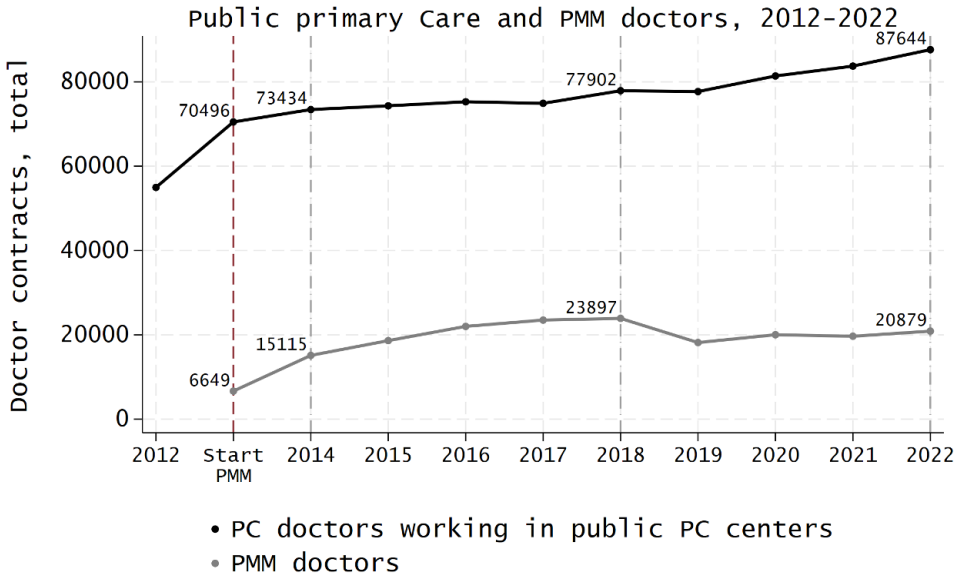


Figure 3.2. The graph shows the annual, country-wide mean in the numbers of public primary care doctor contracts (black line) and PMM doctor contracts (gray line) from 2012 to 2022. Original data from the Ministry of Health and CNES.

and policy outreach, i.e. population size, rather than on local health care needs (Hone et al., 2020). The Ministry of Health classified all Brazilian municipalities³ to fall in one of the following categories: having at least 20% of its population being considered extremely poor; belonging to the 100 municipalities with the lowest income per capita and with a population of at least 80,000; state capitals and their metropolitan regions. All municipalities fulfilling these criteria were considered priority to participate in the PMM and most did. However, many PMM municipalities lacked a clear categorization. Table 3.1 shows the distribution of municipalities according to the Ministry of Health PMM eligibility criteria in 2014. It shows that close to half of the PMM municipalities, 1,646 out of 3,334, did not fall under a clear categorization.

The Ministry of Health sporadically further sub-classified the non-priority municipalities in the “other” category (see e.g. the list of municipalities eligible for PMM in 2017 (Brazilian Ministry of Health (2017))), which replaces the “other”

³ The municipality is the smallest administrative unit in Brazil and the level on which PMM is implemented

Profile	min. 20% poverty	Capitals	G100	Metropolitan region	Other	Total
N	1,708	27	98	509	3,228	5,570
in PMM by 2014	1,212	26	94	356	1,646	3,334

Table 3.1. Distribution of all Brazilian municipalities, according to the eligibility criteria of the Ministry of Health. Categories are: (1) having at least 20% of the population living in extreme poverty, (2) being one of the 27 federal capitals, (3) belonging to the 100 poorest municipalities with more than 80,000 inhabitants, (4) being located in a state capital metropolitan region, and (5) "other". The PMM group only encompasses my later sample, which comprises PMM municipalities which joined the program by the presidential elections in October 2014 and never ceased to be a part of it for a prolonged period of time until 2022.

group with four subcategories. The first sub-category consists of municipalities which are considered historically or climatically vulnerable, e.g. because they are indigenous communities or are located in semi-arid regions. The three remaining sub-profiles are classified according to the the "*Piso de Atenção Básica*" (PAB), the "Primary health care floor". The PAB classifies all Brazilian municipalities according to the need for federal funding in local primary care services. Each Brazilian municipality is allocated to a PAB, so it is not clear why this classification is used for these municipalities in particular in the context of the PMM. Indeed, previous papers assessing the PMM noted that many of the "other" PMM municipalities were granted access due to to opaque reasons, and some of them were in fact quite wealthy (Hone et al., 2020; Oliveira et al., 2016).

Table 3.2 shows the sub-division of the "other" PMM municipalities. The higher the PAB level, the less the relative need for federal support for local primary health care provision.

Sub-profile "other" municipalities	Vulnerable areas	PAB I	PAB II	PAB III and IV	Total
N	554	1,622	775	277	3,228
in PMM by 2014	359	751	383	153	1,646

Table 3.2. Sub-distribution of "other" PMM municipalities in 2014. The PAB categories indicate relatively higher (I) and lower (IV) federal funding needs for local primary health care services. Some municipalities are instead designated as vulnerable because of climatic or historical reasons.

Programa Mais Médicos: Political Elements

Until the implementation of the PMM, the main determinants of the presence of sufficient public primary care doctors were market-based, with the local quality of life and proximity to urban centers as the most important pull and retain fac-

tors. Primary care provision was largely in the hands of municipalities, with more wealthy municipalities being able to offer better conditions and medical infrastructure and being more successful in attracting and retaining doctors in general (Massuda et al., 2018). Hence it is not surprising that, despite offering competitive remuneration and extensive benefits, few Brazilian doctors initially applied to be part of PMM, as it would have required them to stay in poor and remote municipalities, at least according to policy targeting criteria. Thus, the government opened applications to the program to doctors trained abroad and not certified by Brazil's doctor association. Through an international agreement between Cuba and Brazil, and managed by the Pan-American Health Organization, the Cuban government agreed to send thousands of Cuban doctors to fill PMM vacancies for which no other professional could be found (Oliveira et al., 2016). Cuba has a history of "medical internationalism" in which the country sends doctors abroad for humanitarian and development reasons, but also to improve diplomatic ties and state revenue (Huish & Kirk, 2007).⁴ Table 3.3 shows that by 2014 the vast majority of PMM municipalities hosted at least one Cuban doctor.

PMM participation	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
No	3,523	1,891	1,627	1,587	1,615	1,694	1,710	1,696	1,703	1,570
Yes	2,047	3,679	3,943	3,983	3,955	3,876	3,860	3,874	3,867	4,000
Yes, hosted Cubans (%)	87.20	92.17	86.51	84.99	82.96	74.33	0.00	32.21	35.30	35.10
N	5,570	5,570	5,570	5,570	5,570	5,570	5,570	5,570	5,570	5,570

Table 3.3. Number of municipalities which hosted PMM doctors throughout the years, and the percent of participating municipalities which hosted at least one Cuban doctor. Original data from the Ministry of Health.

Municipalities could request PMM participation and how many doctors they would like to receive. The Ministry of Health then validated these requests and decided on the number of approved vacancies per municipality. First Brazilian and then foreign doctors could enlist in a vacancy of their choice, but the Cuban doctors were then deployed to fill the remaining vacancies.

In terms of health care services and outcomes, most analysts positively assessed the PMM. The program was successful in achieving a meaningful expansion of primary care doctors. At the height of the program, more than 18,000 doctors

⁴ In the case of the PMM, Cuba is reported to have received more than half of the doctor's salary (Madeiro, 2018).

worked in a majority of Brazilian municipalities and expanded health care access to around 20 million Brazilians, an additional 15% of the total population. Overall quality of care and patient satisfaction rose (Castro et al., 2019).

In terms of politics, however PMM was controversial since its inception. While cheered on by progressive politicians as a necessary step towards improved equity and equality in Brazilian healthcare, conservative politicians, but also national and regional medical associations, largely opposed the program (Scheffer, 2016; Silva et al., 2018).

Since its introduction by the PT government of Dilma Rousseff in July 2013, the PMM featured strongly in all presidential elections. In 2014, the PT touted the program as a massive success and the party candidate Dilma Rousseff made it one of the main pillars of her re-election bid (Bergamasco, 2013). Opponents criticized the excessive state involvement in health care management, the program's large costs, and Cuba's participation in it. Especially the latter critique became one of the focal points of contention in the elections in 2018. The candidate rivaling the PT, Jair Bolsonaro, accused the Cuban doctors to be subversive communist agents (Terra, 2019), and his election victory resulted in all Cuban doctors leaving the program at the end of 2018. The PMM itself was to be gradually phased out, to be replaced by Bolsonaro's own doctors program "Médicos pelo Brasil", and the PMM's salience for Brazilian healthcare gradually waned over the coming years (Pires, 2023). The phasing out of the program was temporally halted due to the Covid health crisis, which also triggered that some Cuban doctors were allowed to return. In the end, the PT's renewed election victory in 2022 reinvigorated the PMM as the flagship government healthcare program.

In short, the PMM's political salience was very high for all presidential elections since its introduction, always closely associated with the PT while being opposed by its main political rivals.

3.3 The Political Economy of Public Policies

This analysis builds upon a rich body of literature which connects public good provision to political outcomes in democracies. The point of theoretical and empirical departure is usually the theory of retrospective voting which assumes voters to be rational agents that will reward politicians for improvements made during their

tenure. Both, personal ('egocentric') and social ('sociotropic') improvements are bases for such assessments (Kinder & Kiewiet, 1981; Kramer, 1971). If public goods increase the utility of voters, politicians in turn have an incentive to provide them to their electorate. This should hold especially in developing economies with high levels of income inequality, where a large share of the population benefits strongly from the public provision of goods (Cerdeira & Vergara, 2008).

Most empirical studies on public policies stop short of analyzing their electoral returns, and instead focus on their implementation and the choices around it. In other words, they investigate why, how, and to whom resources are being allocated, by, for example, studying the influence that patronage or elite capture has on policy resource distribution (see Golden and Min (2013) discussing many examples).

For this paper, the most relevant insights from the literature concern how governments employ public policies strategically to garner electoral gains. In order to be electorally salient, a policy's investments need to be visible (Mobarak et al., 2011) and clearly associated with one political faction (Manacorda et al., 2011; Zucco Jr & Power, 2013). Beyond that, however, the literature notes the importance of targeting marginal constituencies and the timing of policy implementation. Kang (2015) finds that core constituencies, i.e. voters who reliably vote for the incumbent, receive more government support when elections are further away, but marginal constituencies, i.e. those which are electorally competitive and where the majority of votes tends to "swing" between incumbent and opposition, receive increased support when elections are looming. This is in line with the notion of "political cycles", which implies that investments made closer to an election are often strategically employed to sway the votes of marginal constituencies in favor of the incumbent (Nordhaus, 1975; Shi & Svensson, 2006). However, electoral returns to public policies are often only short-term and do not cement elevated support in the long-term (Achen & Bartels, 2008; Bechtel & Hainmueller, 2011; Zimmermann, 2021).

Nevertheless, besides the rational motivation of increasing their re-election chances, governments usually and naturally also follow social considerations, such as improving development or achieving greater distributional fairness. In fact, most distributive policies, i.e. policies that involve distributing public resources to identifiable groups or locations, are guided by both, equity *and* electoral considerations (Golden & Min, 2013; Imai et al., 2020; Luna & Mardones, 2016; Sugiyama &

Hunter, 2013). Weitz-Shapiro (2012), for example, investigates the Argentinian context and argue that it is a combination of political competition and poverty levels which shapes the strategic political decisions of how to distribute policy resources. The fact that politicians often use distributive politics as a tool to secure electoral gains is part of democracy, but it might come at the cost of policy efficiency and fairness (Imai et al., 2020; Luna & Mardones, 2016).

Of the comparatively few papers empirically studying and quantifying political rewards, the majority discusses the usually large electoral returns of economic policies, such as conditional cash transfers (Conover et al., 2020; Diaz-Cayeros et al., 2016; Linos, 2013; Manacorda et al., 2011; Zucco Jr & Power, 2013). More recent analyses discuss the electoral impacts of policies in a diverse range of sectors, such as education (Acemoglu et al., 2021; Bursztyn, 2016; Sandholtz, 2023) and infrastructure (Burnett & Kogan, 2017; da Rocha Loures Bueno et al., 2024), and paint a mixed picture on whether they produced significant and meaningful electoral returns.

3.3.1 The Political Economy of Health Care Policies

Economic theory argues that health care services are merit goods, i.e. goods whose consumption is socially beneficial and desirable but insufficient if left solely to market forces. In practice, this calls for the government to be an active, and usually redistributing, agent in supplying and regulating health care (Tuohy & Glied, 2011). Also politically, theoretical models would predict that in a highly unequal country with widespread poverty, such as Brazil, the majority of the electorate would support resource distribution and a greater provision of public goods (Cerdeira & Vergara, 2008; Meltzer & Richard, 1981).

Empirically, there is substantial evidence that effective health care policies improve health outcomes, especially for lower-income populations (see e.g. Gwatkin (2000), Macinko et al. (2009)). However, evidence of these policies increasing political support is rare, even in developing economies (Imai et al., 2020; Luna & Mardones, 2010). So far, only few papers have directly connected health care policies with election results, and none have analyzed differential targeting within the same policy.

Most of the evidence on the electoral returns of health care politics stems from

the US and concerns whether changes in Medicaid affected voting. This literature finds that Medicaid expansion increased turnout, at least temporarily (Baicker & Finkelstein, 2018; Clinton & Sances, 2018). In terms of vote shares, there is evidence that voters punished the "Affordable Care Act" health care reform in the 2010 midterm elections (Konisky & Richardson Jr, 2012). This shows the importance of health care as a political issue, especially when a policy is clearly associated with a political faction. However, Medicaid reforms represent broad policy changes affecting the entire country, without targeting regional constituencies or featuring salient local aspects.

Outside of the US, unpublished evidence from Brazil finds that an expansion of the municipal's primary health care supply helped incumbent mayors to retain their offices in municipal elections in 2008 (Braga, 2020) and 2012 (Camargo, 2021). Before the introduction of the PMM, primary health care provision was largely in the hands of municipal administration, and these findings show that regular health care investments and expansion can be electorally rewarding on the local level in Brazil. Thus, while these papers show that a general expansion of health care services, e.g. by building new clinics, can be electorally rewarding for mayors, they neither discuss health care policies as such nor do they answer whether health care and health care policies can affect national elections in Brazil. Investigating the national level, Imai et al. (2020) found no electoral benefits from a Mexican program aimed at cushioning drastic health expenditures among the poor, despite the policy's effectiveness in achieving its equity goals. The paper argues that the program was not employed strategically enough, neither in its timing nor in its targeting, and its weak association with a specific party limited its political impact. Another reason for why strengthening local health care structures might not yield electoral returns for national elections is because the produced benefits of social policies are often indirect and diffused, which contrasts the more widely studied case of direct and individualized government cash transfers (Bursztyn, 2016). While the latter is also true in the case of the PMM, the PMM is a government program which is clearly associated with the PT, and which was, due to its size and the Cuban doctors, very salient and visible for Brazilian society.

Given the high costs of conducting health care policies and the uncertainty about their political returns, why do incumbents pursue them? While the social

benefits of health care policies are mostly evident, the lack of corresponding electoral support is puzzling. My paper addresses this puzzle partly, by being the first to connect a large-scale, politically salient, and place-based primary health care policy with political returns in national elections. It is the first attempt to disentangle different targeting strategies within the same health care policy and evaluate whether they elicited distinct electoral responses. In the next section I will analyze municipality characteristics and past voting behavior, producing suggestive evidence that supports this paper in arguing why certain municipalities were targeted and in hypothesizing on what the electoral response in the different groups were.

3.4 Conceptual Framework – The Political Economy of the PMM

Internationally, the PT is probably best known for their large-scale conditional cash transfer “Bolsa Família”, introduced by the party in 2003, and widely hailed as a successful policy in remedying poverty. The program also seems to have consistently helped the party electorally, especially for the elections in 2006 (Zucco Jr & Power, 2013). However, also for the elections in 2014, Zucco describes a notable increase in the voting trends of municipalities with high *Bolsa Família* coverage, which he hypothesizes could be due to “additional social programs targeted at similar population, government expenditures that flow to the same municipalities covered by Bolsa Família” (2015, p.146). This uptick could be, at least partly due to the the PMM, which was described to act complementary to the more direct financial support of *Bolsa Família*: by expanding primary health care access for needy communities and regions, the PMM was tackling inequalities indirectly, through the supply side of health care, rather than directly, through stimulating increased demand with cash transfers (Sánchez-Ancochea & Mattei, 2011; Soares et al., 2010). However, while previous literature on the PMM found it to improve health care access and health outcomes (Fontes et al., 2018; Hone et al., 2020; Santos et al., 2017), whether these social gains translated into political gains for the PT remains to be investigated, especially as the PMM represents an indirect and less visible pro-poor policy than *Bolsa Família*.⁵

⁵ Albeit health care policies are not as visible as direct cash transfers, the PMM is relatively visible and salient, not least because most PMM doctors came from Cuba.

Thus, the question this paper answers is whether the PMM produced electoral gains for its political architect, the PT. Even though empirical evidence on political returns to specific health care policies is sparse, the theoretical considerations of retrospective voting would predict that municipalities which benefited from the PMM should reward its implementation:

Research question 1: *Did the introduction of the PMM lead to an increase in the PT vote share in participating municipalities in elections following the program's introduction?*

Furthermore, I argue that the government had two main concerns with the PMM: on the one hand, the government wanted to tackle regional inequalities in primary health care access. Given this *equity concern*, the PT targeted poor municipalities with a need for government intervention in health care. On the other hand, the government employed PMM strategically and targeted some municipalities mainly, or at least partly, for *electoral concerns*. I further argue that these concerns were consequential for how program participation succeeded and its potential political effects.

How can I distinguish those two concerns for targeting municipalities? My underlying data provides me with some suggestive evidence on the principal motivation behind the PMM targeting of specific subgroups.

Papers evaluating the efficiency and impact of the PMM in terms of health outcomes noted that PMM municipalities consisted broadly of two groups: for the first one, PMM participation is clearly justified by set criteria.⁶ Municipalities in the second group, though, did not fall under any specified category and were simply coined as "other" by the Brazilian Ministry of Health, as shown in Section 3.2.2. Which concerns directed the government to target these 'other' PMM municipalities is unclear (Hone et al., 2020; Oliveira et al., 2016). Could political considerations have played a role?

Table 3.4 shows pre-PMM election results and the share of municipalities which were granted access only in the election year of 2014, picking up again the government classification of municipalities introduced in Section 3.2.2. With respect

⁶ It is less clear why some municipalities in this group did not participate in the PMM. Municipalities were responsible to apply and request a specific number of doctors, which was then evaluated and potentially approved by the federal government (Oliveira et al., 2016). It might be that some of these municipalities either did not apply or applied faultily, e.g. because of a lack of administrative capacity.

to the “clear criteria” municipalities, the table reveals that they were mostly PT strongholds, and most of them gained PMM access already at the program’s introduction in 2013. Considering the non-priority, “other”, municipalities, the table shows that, with the exception of the “vulnerable areas” group, the pre-PMM presidential elections were on average much more competitive in these municipalities than in the ones targeted on grounds of clear criteria. Furthermore, most of them joined the program only in 2014, shortly before the presidential elections of 2014.

PMM municipalities	N	Average PT share, 2002-2010	Vote	Share which gained PMM access in 2014
Clear criteria:				
Total ‘clear criteria’	1688	59.78%		11%
20% poverty	1,172	61.7%		12.7%
G100	89	62.5%		4.5%
Capitals+metrop. area	366	53.2%		8.2%
Other:				
Total ‘other’	1646	51.47%		73.6%
Vulnerable areas	350	59.6%		19.5%
PAB I	723	50.4%		89%
PAB II	367	48.6%		89.3%
PAB III and IV	144	48.4%		86.3%

Table 3.4. Sample PMM municipalities grouped according to classification of the Ministry of Health Oliveira et al., 2016. The average PT vote share column shows the average results of the second round voting in the pre-PMM presidential elections 2002, 2006, and 2010. The last column shows what percentage of the municipalities joined PMM in 2014 as opposed to 2013.

These two features, targeting marginal constituencies and the timing of policy introduction, are prominently discussed as mechanisms through which politicians aim to secure electoral gains via their policies. Considering the timing aspect, voters often exhibit myopic voting behaviors; therefore, introducing the PMM in electorally competitive municipalities shortly before elections might be more effective in generating electoral returns (Nordhaus, 1975; Shi & Svensson, 2006; Zimmermann, 2021). While the Brazilian president is elected by a majority of the country-wide popular vote, and achieving a local majority is not necessarily decisive for the overall vote, I argue that the logic of directing resources to swing or marginal municipalities to maximize the PMM’s electoral success remains valid. First, these

municipalities generally have the lowest average PT voting share, indicating that there is relatively more to gain for the party (see Appendix Figure B.2 for voting trends of equity and electorally targeted municipalities). Given that these municipalities are relatively less poor, the PMM, as a municipal social policy, provides the PT an opportunity to reach voters who are not eligible for other, more direct, social policies (such as Bolsa Família). This enables the party to demonstrate government presence and the benefits of their progressive agenda to the general local population. Second, there is a psychological effect to consider, especially given that initial voting results can create momentum for the second voting round (Plutowski et al., 2021). The media also tends to declare municipalities in favor of a party if its candidate achieved the majority of the local vote, even already after the first round. Third, these municipalities are competitive for other elections too and a successful federal program could benefit PT politicians more broadly. For example, alongside the president, voters also decide on state and federal members of parliament, both of which are elected by a proportional system, at the federal elections. Municipal elections on the mayor office are always held two years after the federal elections. These and other positions, in which the local majority is more decisive, could also see advantages from the PMM program (Sugiyama & Hunter, 2013).

While Table 3.4 is only suggestive, the patterns are in line with Kang (2015) who found that core constituencies benefit from policies in non-election years, while more competitive constituencies start benefiting when elections loom. Hence, the evidence gives some indication that political considerations might have played a role in why most of the “other” municipalities participated in PMM by the elections of 2014. More concretely, while “vulnerable areas” were plausibly targeted mainly due to equity concerns, the remaining “other” municipalities are not. These municipalities were categorized according to the PAB indicator, which additionally allows me to rank these municipalities according to local health care needs: PAB I represents the municipalities with the highest relative needs, and PAB IV represents the most affluent and least needy municipalities.

Next to evaluating whether the PMM had any electoral effect, the aim of this paper is to juxtapose equity with electoral considerations within the PMM, investigating whether this distinction mattered electorally. The strongest case for electoral concerns are fulfilled by the PAB municipalities. To better assess the equity case,

Appendix Table B.2 looks at the means of demographic and socio-economic indicators of all PMM subgroups. Unsurprisingly, the 20% poverty group, for which at least 20% of the municipality population is considered to be extremely poor, is the most destitute subgroup, considering, e.g., GDP pc and number of families which receive the *Bolsa Família*. Previous analyses of the PMM emphasized that these municipalities should have been in principle the PMM's main target group (Oliveira et al., 2016; Santos et al., 2017). The table also substantiates the claim that vulnerable areas are targeted due to equity concerns, as they constitute the second poorest group, on average. The smallest group is the G100, consisting of the poorest among Brazil's 100 most populous municipalities, thereby combining equity concerns with outreach concerns. The outreach concern, i.e. reaching the highest number of people possible with a place-based policy investment, seems to be wholly dominant for the state capitals and their metropolitan area, as this group is much closer to the PAB groups in terms of local prosperity and local health care needs.

How does this information guide me in assessing which targeting strategy, equity or electoral considerations, mattered for which subgroup?

In terms of equity, it is clear that the 20% poverty group, the vulnerable areas, and also the G100 are relatively destitute. Furthermore, these groups are not electorally competitive and became part of PMM in 2013, not in the election year 2014. I thus assess these municipalities to be the clearest example of equity targeting. In terms of political considerations, it is the "other" municipalities with the exception of the "vulnerable areas". Thus, I assess all "PAB" subgroups as the most likely candidates of electoral targeting. They consist of relatively wealthy communities, which are electorally competitive and which have mostly been admitted to the PMM in the election year 2014. State capitals and surrounding areas do not fit clearly into either of these categories and hence do not form part of this analysis.

Table 3.5 depicts the new categorization of PMM municipalities into equity and electorally targeted municipalities and Appendix Table B.3 shows the distribution of all sample municipalities, PMM and non-PMM, into these newly created groups.

Could I expect a distinct electoral reaction to these two strategies? Imai et al. (2020) found no evidence for a pro-poor health care policy in Mexico and argued that it was because it was not politically targeted. On the other hand, policy meaningfulness, i.e. the local need and impact of a policy, has been found to matter for

PMM municipalities	N	N, %	Average PT Vote share, 2002-2010	Share gained PMM access in 2014	which PMM ac-
Equity	1,611	56.63%	60.96%	13.73%	
Electoral	1,234	43.37%	49.64%	88.9%	

Table 3.5. Sample PMM municipalities grouped by whether they were targeted by equity or electoral considerations. The equity group comprises municipalities classified as "20% poverty", "G100", and "vulnerable areas" in Table 3.4. The electoral group are all municipalities classified by their PAB level. Excluded are municipalities merely targeted due to outreach, i.e. state capital and surrounding metropolitan areas. The average PT vote share column shows the average results of the second round voting in the pre-PMM presidential elections 2002, 2006, and 2010. The last column shows how many of the municipalities joined PMM in 2014 as opposed to 2013.

the electoral impact of social policies outside of health care (Golden & Min, 2013; Sandholtz, 2023). A priori, it is thus difficult to hypothesize the outcome. From a policy perspective, it would be advantageous if the PMM produced electoral gains based on local needs for health care support, i.e. in the group targeted due to equity considerations.

Research question 2: *Was the PMM policy electorally more successful in municipalities targeted due to equity considerations?*

The following sections of the paper set out to answer these research questions.

3.5 Data

3.5.1 Data Sources and Main Variables

I combine data sources on Brazilian health care, election results and socioeconomic variables on the municipality-year level of every election year, starting in 2002 every four years until 2022. Below I describe the main variables of my analyses and the respective sources of data.

Electoral Data

My outcome variable is the PT vote share in all presidential elections held between 2002 and 2022. I investigate both, the first and second round of voting. The first round results arguably better reflect voters' preferred candidate and party, as they are less tainted by strategic considerations (Plutowski et al., 2021; Zucco Jr & Power, 2013). I also assess the second round of voting because in my setting the rival

candidate always largely or fully opposed the PMM, which could make a vote for the PT a strategic vote for the PMM. If the policy garnered support for the PT as such, it should be reflected in the first voting round results. If the policy, but not necessarily the PT itself, are perceived favorably, second round voting results might arguably better capture this effect. A large difference between those two would indicate whether the PMM improved the PT's appraisal or whether voters mainly favored the policy.

As a covariate, I also include the party of the ruling mayor, to account for potential political favors concentrated in parties aligned with the federal government (Sugiyama & Hunter, 2013). I source all electoral information from the Brazilian Superior Electoral Court (Tribunal Superior Eleitoral; TSE). I do not analyze the policy's effect on turnout, as Brazil's elections are based on compulsory voting and I thus do not expect great changes.

Health Care Data

I obtained information on the PMM through the government portal "Lei Acesso Informacao", a platform on which every "netizen" can request the Brazilian government to provide what is or should be publicly available information. This way, I obtained information on municipal PMM participation, categorization, and the number of doctors throughout my sample period. I obtain the number of privately insured individuals from Brazil's National Agency for Supplementary Health (ANS). All other information on health care, such as the primary care coverage per municipality and its number of public primary care doctors, I obtain from *Datasus*. *Datasus* is the Ministry of Health's department tasked with collecting and disseminating information and data on health care in Brazil.

Socioeconomic Municipality Characteristics

I use time-varying municipal socioeconomic characteristics, which influence both, electoral results and PMM targeting, as control variables. I use a municipality's GDP per capita, population size, population density, share of residents over the age of 60, number of families receiving *Bolsa Família* per 100,000 residents, total value added (VA) in 2021 prices per 100,000 residents, the public sector's share of total VA, and the agricultural sector's share to total VA to control for socio-economic

	N	never-PMM	always-PMM	late-joiner	dropped	covariates missing
Sample	4,399	1,188	3,211			
Excluded	1,171			593	230	348
	5,570					

Table 3.6. Included and excluded municipalities of my sample.

and demographic changes within the sample period. Information on the annual level stems from the Brazilian Institute of Geography and Statistics.

Sample Composition

Section 3.2.2 explains that most, but not all, municipalities either joined the PMM program by the presidential elections of 2014 or did not join ever during the period of analysis.

My main objective is to analyze whether the PMM's *introduction* resulted in electoral effects. Due to this, and for conceptual and econometric simplicity, I restrict my sample to the always and never treated municipalities. That means dropping 230 municipalities who ceased PMM participation at one point between 2014 and 2022, and excluding 593 municipalities which entered the program after 2014. Additionally, I excluded 348 municipalities for which there is a missing outcome (156) or covariate (192) observation in any sample year.⁷ The result is a final sample of 4,399 municipalities out of a total of 5,570 Brazilian municipalities.

3.6 Empirical strategy

PMM participation is not a treatment which was randomly assigned to municipalities. On the contrary, we would expect that PMM municipalities are on average poorer than non-PMM municipalities. Hence, I employ a matched difference-in-difference (DiD) strategy. This approach allows to control for selection into treatment based on observable municipality characteristics via matching, while controlling for unobservable, fixed municipality characteristic through the DiD. The first step involves pre-processing my sample to obtain weights that balance the moments of the treatment group covariates with the ones from the comparison group.

⁷Dropping these municipalities ensures that I maintain the same sample composition throughout different regression analyses. Including the 192 municipalities with a missing covariate does not, however, change the later results in a meaningful way. Results available upon request.

In the second step, I then employ the canonical two-way fixed effects estimator, comparing the trends of the weighted treatment and control units. This way, I am able to make unconfounded comparisons between the voting trends of PMM and non-PMM municipalities (Rosenbaum & Rubin, 1983).

3.6.1 Covariate Balancing and Matching

Table 3.7 confirms that PMM municipalities have on average a lower GDP per capita, are less productive, the government provides less basic primary healthcare coverage, and fewer individuals are affording private insurance than in non-PMM municipalities. However, it also shows that disadvantaged communities can take many forms in Brazil - while some of them are remote, rural municipalities, the many urban peripheries in Brazil make the PMM municipalities have on average a larger population size and be more densely populated.

	No-PMM	PMM	$p > t $
Population size	11,449	49,052	0.000
Population density	51	148	0.000
Share of elderly (60+)	0.13	0.12	0.000
Share nonwhite	0.46	0.54	0.000
GDP per capita (R\$ 1,000)	14,230	11,886	0.000
No. of bolsa família families (per 100k)	8,133	9,604	0.000
Bolsa família expenses per resident (in 2021 prices)	171	214	0.000
Value added, total (per 100k)	1,290,000	1,060,000	0.000
Value added, agronomy share	0.24	0.20	0.000
Value added, public share	0.33	0.34	0.017
Primary healthcare coverage (%)	92	82	0.000
Number of privately insured (per 100k)	8,331	7,320	0.018
HDI 2010	0.68	0.65	0.000
HDI change 2000 - 2010	0.13	0.14	0.000
N	1,188	3,211	

Table 3.7. Municipal averages from 2010, the last presidential election year before the PMM's introduction in 2013. All variables obtained through IBGE. The column $p > |t|$ shows the p-value of the pairwise t-test of equality of means for the PMM and non-PMM municipalities.

In order to claim that the non-PMM municipalities are a credible comparison group, I will apply appropriate weighting to make non-PMM and PMM municipalities as similar as possible considering relevant municipality characteristics, thereby emulating random assignment to treatment (based on observable munici-

pality characteristics).

Traditionally, this has been done through considering the propensity score of a unit receiving treatment, usually weighting the observation with the inverse probability weighting of its propensity score (Rosenbaum & Rubin, 1983). More recent techniques, however, are often more efficient and robust to model misspecification and imbalance in covariate distributions. These techniques usually focus on weighting treatment and control groups in such ways that the distribution of covariates is directly balanced. One prominent example for this is the "Covariate Balancing Propensity Score" (CBPS) method (Imai & Ratkovic, 2014). CBPS builds on the traditional approach of estimating a unit's propensity score through a logit model, but additionally integrates covariate balancing directly into the estimation of the propensity score. CBPS uses techniques from the generalized method of moments (GMM) or empirical likelihood models, selecting multiple conditions, or "moments", into the estimation process to ensure that the distribution of covariates is similar across groups. As CBPS strongly improves the robustness of propensity score matching, it has become a widely used technique for causal inference in observational studies.

Three features make the CBPS particularly suited for my case: the first is that it is suited to adjust for many covariates, allowing me to incorporate a multitude of demographic, economic, and healthcare characteristics of the municipality before the PMM (Athey & Imbens, 2017). The second one is that CBPS is very flexible and can be applied across various types of treatments, which is crucial in my case when investigating heterogeneity within the PMM municipalities, as requested by research question 2. To answer whether "equity" PMM municipalities reacted differently from PMM municipalities targeted due to electoral concerns, I will subdivide my sample further, by e.g. balancing the covariates of "equity" PMM municipalities to "equity" comparison municipalities. The third reason is that it is computationally efficient (Imai & Ratkovic, 2014).

To make PMM and non-PMM municipalities as comparable as possible, I use all covariates present in Table 3.7, together with state dummies, based on information of the last pre-PMM election year 2010. This year is the reference year for the later DiD estimations, and it is also a census year, allowing the use of variables not available for the other sample years, such as the municipal human development

index (HDI) and the local racial composition.⁸

Table 3.8 mirrors the municipality characteristics of Table 3.7 after balancing and shows that the covariate balancing process achieved to balance treatment and comparison group to a great extent.

	No-PMM	PMM	$p > t $
Population size	37,143	49,052	0.318
Population density	135	148	0.707
Share of elderly	0.116	0.115	0.664
Share nonwhite	0.537	0.541	0.831
GDP per capita	12,031	11,886	0.871
No. of bolsa família families (per 100k)	9,625	9,604	0.959
Bolsa família expenses per capita	215	214	0.982
Value added, total (per 100k)	1,070,000	1,060,000	0.884
VA agro share	0.202	0.202	0.990
VA public share	0.339	0.342	0.812
Primary healthcare coverage	82.7	82.5	0.926
Number of privately insured (per 100k)	7349	7320	0.969
HDI 2010	0.653	0.653	0.988
HDI change 2000 - 2010	0.138	0.138	0.929
N	1188	3211	

Table 3.8. Municipal averages in 2010, after matching early PMM adopters with never adopters through CBPS (Imai & Ratkovic, 2014). All variables plus state fixed effects are part of the balancing process. The column $p > |t|$ shows the p-value of the pairwise t-test of equality of means for the PMM and non-PMM municipalities.

To compare municipalities based on their classification under either equity or electoral targeting, I reweigh my sample to balance covariates within the two sub-categories while maintaining the overall balance between treated and non-treated municipalities. This approach ensures that the characteristics of PMM-participating “equity” municipalities are balanced with those of non-participating “equity” municipalities, meaning that municipalities are as similar as possible except for their participation in the PMM. Also for this intricate weighting scheme CBPS performs well, balancing covariates across sub-groups and the aggregated treatment and comparison group. Appendix Table B.4 shows the balance between PMM and non-PMM municipalities with the new sub-group weighting and Appendix Table B.5

⁸ Instead using a balancing process of all pre-PMM election year observations, i.e. from 2002, 2006, and 2010, is largely inconsequential for the estimations, as shown in Section 3.7.3.

shows the balance across the equity subgroups and the non-equity, or electoral, subgroup.

3.6.2 Difference-in-Difference

In order to investigate whether PMM participation and expanded primary care access had some electoral effects, I use a Difference-in-Difference (DiD) event-study-style regression, comparing municipal voting outcomes between 2002 and 2022.

I employ a dynamic DiD event-study design with an absorbing, non-staggered binary treatment adoption. Thus, the treatment variable distinguishes between “clean” comparison units which are not treated throughout the sample period, and units in the treatment group which all received treatment at the same time and stayed treated throughout the sample period. This set-up allows the use of the canonical two-way fixed effects (TWFE) estimator to assess the average treatment effect on the treated (ATT) in a DiD setting (Roth et al., 2023).

Focusing on early- and never adopters, on the one, hand facilitates interpretation of the estimated coefficients, relaxes the parallel trends assumption required in staggered adoption settings, and avoids complications from dynamic and heterogeneous treatment effects, which are challenging to model and validate in such designs. On the other hand, it reduces the sample size: 593 municipalities joined the PMM only after the presidential elections of 2014 and were thus dropped from my sample relying on stable treatment group composition. While diagnostic tools do not indicate that aggregation issues under staggered treatment timing and heterogeneous treatment effects are large issues for my sample, and it is thus, in principle, justified to use the TWFE on the whole sample (De Chaisemartin & d’Haultfoeuille, 2020), I nevertheless check all my results with more recent DiD estimators (De Chaisemartin & d’Haultfoeuille, 2020; Sun & Abraham, 2021).

The equation for the canonical TWFE model for municipality i in election year t is:

$$Y_{it} = \phi_i + \gamma_t + \gamma_t^* \times \phi^{*s} + X_{it}\beta_i + \sum_{t=-T}^{t=-2} \beta_t(t \times PMM_i) + \sum_{t=0}^{t=T} \beta_t(t \times PMM_i) + \epsilon_{it}$$

Y_{it} is the outcome of interest: the vote share of the PT presidential candidate either in the first or the second voting round in municipality i at election year t . PMM_i represents whether a municipality participated in PMM by the elections in

2014 (=1), or whether it never participated in PMM (= 0). The first summation of the equation sums up the coefficients of pertaining to the group of PMM municipalities on the elections prior to the actual introduction of the program in 2013. The second summation summarizes all such coefficients for elections after the PMM's introduction, i.e. from 2014 to 2022. Municipality fixed effects ϕ_i are meant to absorb time-invariant factors that could affect both, PMM program participation and election results on the municipality level. Examples are a municipality's seasonal weather conditions or the fact that voters in Latin America frequently exhibit sticky political partisanship (Linos, 2013). Election fixed effects absorb annual trends common to all municipalities, such as the "lava jato" corruption scandal which started in 2014 and badly damaged Brazil's political establishment. Additionally, I add flexible time trends for all 27 Brazilian states $\gamma_t^* \times \phi^{*s}$, absorbing variation that is particular to any of the 27 Brazilian states over time. Finally, I control for the municipal time-varying variables X_i such as whether the municipality's mayor stems from a party aligned with the government coalition and all variables listed in Table 3.7, with the exception of any varying health care variables due to the concern of them being bad controls (Angrist & Pischke, 2009, p.47-51).⁹

3.7 Results

3.7.1 PMM Participation

I first investigate whether the PMM's introduction increased the PT's vote share in participating municipalities in the presidential elections held between 2002 and 2022. If the PMM mattered, it should affect the elections after the program's introduction in 2013.

Section 3.6 explained why measuring the PMM's electoral effect requires careful reflections on how to compare treated to non-treated municipalities. Appendix Table B.6 shows naive regression results without including any weighting and/or fixed effects. The obtained coefficients indicate that that PMM municipalities generally had a higher PT vote share. Adding fixed effects, nullifies most of the statisti-

⁹ There are also possible concerns about including political covariates, such as the mayor's party. While the latter covariate accounts for the traditionally strong influence that local administrations, and their connections to higher echelons of government, play on social service provision in Brazil (Souza, 2015; Sugiyama & Hunter, 2013), the variable itself could be influenced by PMM participation and could be a "bad control". Omitting it from my equations does not change results in any meaningful way. Results available upon request.

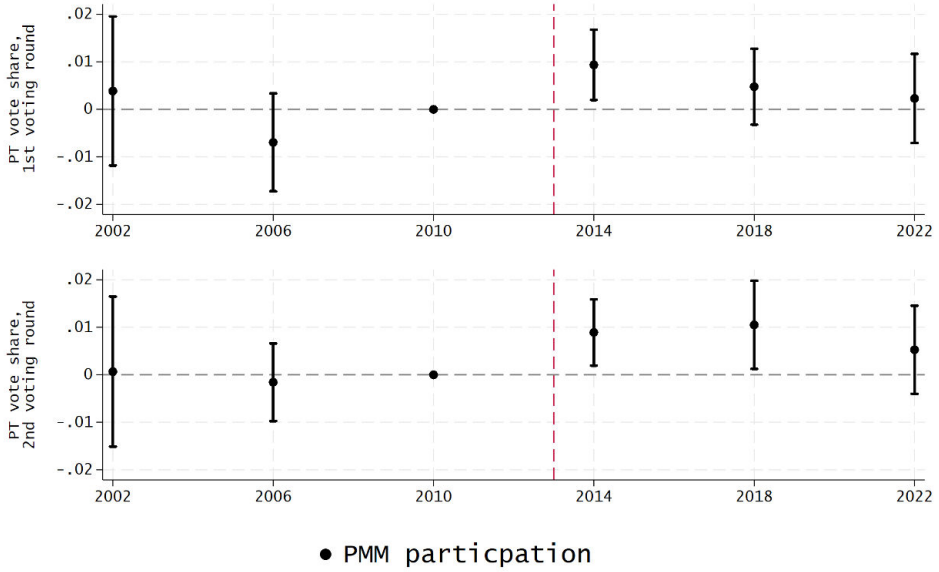


Figure 3.3. PMM Participation and PT Vote Share

cally significant differences, indicating that PMM municipalities only had a significantly higher PT vote share in the pre-PMM presidential elections of 2006, which might reflect the strong popularity of the PT in poorer municipalities in that election in particular (Zucco Jr & Power, 2013). However, PMM and non-PMM municipalities lack common support, see Table 3.7. In order to identify the effect of the PMM, I proceed by balancing the distribution of covariates between the treatment and control groups. The constructed weights ensure that PMM and non-PMM municipalities are comparable, also for relevant variables which cannot be included as time-varying controls because they are only available in the census year 2010, such as the municipal HDI and its racial composition. As explained in Section 3.6, this approach emulates random treatment assignment, mitigating potential biases from both observable and unobservable factors and allowing me to derive more reliable and interpretable estimates of the PMM's electoral impact.

Figure 3.3 and Appendix Table B.7 show the DiD coefficient of trends in the vote share of the PT on the CBPT weighted sample in the first and second voting rounds of presidential elections in Brazil, 2002 to 2022.

The estimations show that there are no statistically significant differences prior

to the PMM implementation in 2013, but the program affected the elections in 2014, the elections most immediately following the program's introduction, on a statistically significant level. The PT's vote share in participating municipalities increased by 0.9 percentage points (pp) in both, the first and the second round of voting of this election. This corresponds to roughly 615,000 additional PT votes in both rounds. In 2018, the PT achieved a 1 pp higher vote share, or 683,000 additional votes, only in the second round of voting. This could show that opposition to Bolsonaro, whose opposition to the PMM was especially fierce, could have mobilized voters in participating municipalities to cast their vote for the PT (Terra, 2019). Voters possibly perceived the program to be beneficial for their local community or generally agreed with the program's aims in a sociotropic way, rendering the PT the best available alternative out of the two candidates.

The observed effects are sizeable, especially because Brazilian presidential elections tend to be close: For the reference election in 2010, the first round PT vote share across all municipalities was 50.01%, and the difference between PMM and non-PMM voting results was 4.12 percentage points. In 2014, the PMM thus increased the vote share by roughly 1.8% relative to the base average, which represents approximately 22% of the difference between the groups' vote share at base. The margin and policy meaningfulness were roughly the same for the second voting round in 2014.

To put this in perspective, a one standard deviation increase in GDP per capita is associated with an average decrease in the PT vote share of 3.5 pp in my sample. Local economic conditions are often described as one of the most important factor in not just the Brazilian elections (Duch & Stevenson, 2006; Zucco Jr, 2013). Perhaps more pertinent is comparing the PMM impact to the one exerted by *Bolsa Família*: a one standard deviation increase in the number of families receiving *Bolsa Família*, corresponding to roughly 5,580 families per 100,000 residents in the municipality, is associated with an average increase of the PT's vote share by 1.4 pp and 1.2 pp in the first and second voting rounds, respectively, for all elections in my sample. However, previous evidence showed that its impacts varied throughout the elections; *Bolsa Família*'s introductory effect in 2006 is said to have swayed around 2.6 million voters, or 2.7 percentage points of the country-wide vote (Zucco Jr, 2015). Thus, the impact of the direct cash transfer program was, expectedly, larger than

the introductory effect of the PMM. Curiously, however, there is evidence that, on the municipality level, *Bolsa Família*'s impact was especially large for the elections of 2014 where it is hypothesized that "non-beneficiaries voted for Dilma at much higher rates in places with high BFP [Bolsa Família Program] coverage than in places with low BFP coverage" (Zucco Jr, 2015, p.143). Given that the PMM was assessed to be complementary to *Bolsa Família* (Soares et al., 2010), and Table 3.7 confirms that PMM municipalities indeed hosted a considerable higher number of *Bolsa Família* beneficiaries, it might be that the PMM is at least partly explaining Zucco's results - making the two PT flagship policies, *Bolsa Família* and the PMM, mutually reinforcing and potentially even electorally decisive for the elections of 2014.

The inquiry of the first research question 1 hence yields that the PMM on the whole had an electoral effect. The magnitude, even though considerable, standing alone was not decisive for country-wide election results.

3.7.2 Equity and Electoral Targeting

Did the differences in targeting, described in Section 3.4, matter in how municipalities reacted to the PMM's introduction?

Figure 3.4 and Appendix Table B.8 show the results when splitting the treated municipalities into those targeted either due to equity or to electoral considerations, comparing them to the respective non-treated comparison group. On the one hand, it reveals that the *equity* group exhibits a significant and positive effects for first round voting: on average, these relatively destitute and poor municipalities increased their vote share by 2.1 pp relative to comparable municipalities which did not participate in the PMM. This positive electoral impact is even contrasting a previous disadvantage in the PT vote share of roughly the same magnitude for the elections in 2006. On the other hand, the program seems wholly inconsequential for first round voting results in the electorally targeted municipalities.¹⁰

¹⁰ For easier interpretation, I present the results for the equity and electoral groups separately. Using instead an equation involving a triple interaction effect between the respective election year, targeting group, and treatment status is mathematically equivalent but allows for testing the equivalence of means between the coefficients of the treatment groups and for structural breaks between said groups. A chow test is used to analyze the latter, and it reveals that the 'equity' and 'electoral' groups are on the whole different (p-value of 4.7%). A Wald test analyzing the hypothesis of equivalence of the treatment effects between the two groups for each of the two election years 2014 and 2018, however, cannot be rejected at conventional levels of statistical significance.

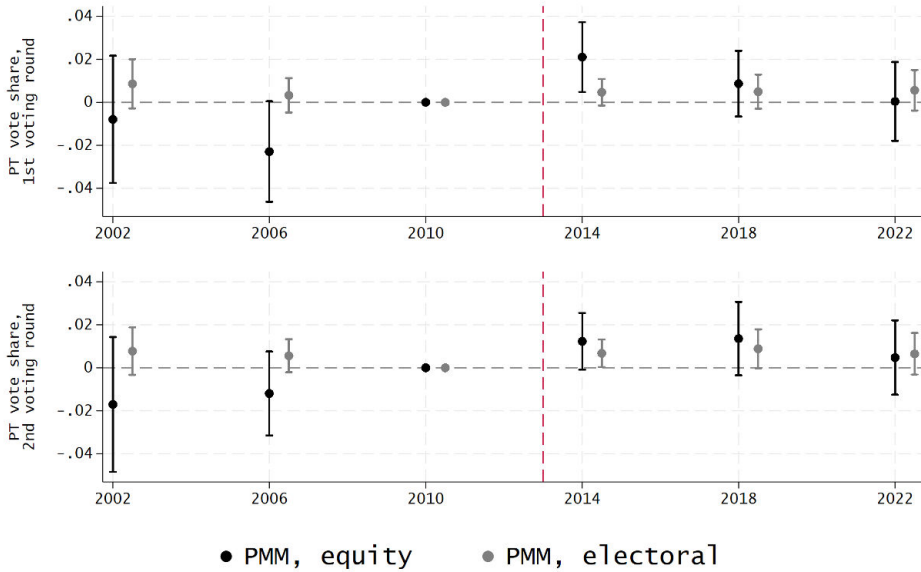


Figure 3.4. PMM Groups and PT Vote Share

Repeating the regressions for the second round voting results shows that equity PMM municipalities had a 1.2 pp higher PT vote share than their non-participating counterparts. In 2018, the coefficient of 1.4 pp is marginally not significant on the ten percent significance level (p-value of 11.8%). Also electorally targeted municipalities now see a significantly higher PT vote share of 0.7 pp in 2014 and 0.9 pp in 2018. Again, one explanation might be that voters in electorally targeted municipalities appreciated the program, be it due to personal or societal benefits, and did not want to see it abolished. Given the strong contrast between the two remaining candidates with respect to the PMM, the program became an even more salient point of discussion in the second rounds of the presidential elections (Fellet, 2014; PODER360, 2018; Silva et al., 2018). Faced with two candidates holding opposing views on the PMM, they voted for the PT candidate, even if it was not their preferred candidate in the first voting round.

All in all, the results indicate that the PMM's impact was electorally meaningful, but less important than economic conditions and economic policy. This is unsurprising, given that social policies are generally less salient and electorally relevant than direct government support through, e.g., the *Bolsa Família* program,

or economic indicators (Alvarez et al., 2000). Nevertheless, the program yielded electoral returns, especially when voters were facing starkly opposing views on its continuation in the second rounds of voting. Furthermore, the PMM generally had a stronger impact in municipalities with a stronger need for the program. For policy-makers, this implies that design and targeting of health care programs can influence their effectiveness in garnering electoral support, and that such political benefits can add to a policy's positive impacts in terms of, e.g., improved health care access and reduced mortality.

3.7.3 Robustness check

Alternative Sample Composition

Results are robust to further relevant splitting of the treatment groups. Table B.2 shows, for example, that the "PAB I" group is numerically dominating the electorally targeted group, while also being on average poorer than their PAB II-PAB IV peers. Isolating its effect, I can treat it as a "hybrid" group, i.e. targeted due to electoral considerations but with relatively more need for the PMM policy and its health care access impacts than the other PAB groups.

Appendix Table B.9 shows that there remains no increase in support for the PT in any first round of the elections subsequent to the PMM introduction in any PAB group, but it indicates that it is the "hybrid" group, which saw an increase of the PT vote share by one pp, that drove the increase in the PT vote share in the second round of the elections 2018. Again, it is possible that the PMM became a decisive factor for parts of the participating PAB I population when faced between the contrasting options of the PT candidate or Bolsonaro, who made closing down the PMM part of his political platform. In the remaining PAB II - IV municipalities, in which large shares of the population are privately insured (Appendix Table B.2), PMM participation did not matter for any election results.

Alternative Weighting Schemes

Results hold, when applying alternative weighting schemes, such as applying traditional IPW of the propensity scores (Rosenbaum & Rubin, 1983) or entropy balancing (Hainmueller, 2012). While these weighting schemes do not perform as well as the CBPT, often failing to converge the means of the groups on conventional

statistical levels, they provide an indication that the results are not contingent on the specific mechanisms underpinning the CBPT. See Appendix Tables [B.10](#) for PMM participation treatment effects and [B.11](#) for treatment effects on the equity and electoral groups when applying a IPW propensity score weighting scheme, and Appendix Tables [B.12](#) and [B.13](#) for the estimations when applying entropy balancing.

Balancing covariates based on the observations in every pre-PMM election year, i.e. 2002, 2006, and 2010, instead of just in the reference year 2010 also largely does not change regression results, see Appendix Tables [B.14](#) and [B.15](#). However, using all pre-election years to construct the CBPS weights leaves me with fewer variables to construct the common support, as some variables are only available through the census in 2010. This potentially explains some of the results that show that the PMM's electoral effects are amplified and extended under this weighting scheme.

All in all, applying these alternative weighting schemes does not alter the broad picture conveyed by Section [3.7](#): PMM municipalities targeted due to equity concerns are the ones to consistently display a statistically significant electoral effect of the PMM on the first round of election in 2014. Electorally targeted municipalities show an effect in the second rounds of voting, especially in the elections of 2018. These results hold independently of the weights used for balancing covariates between PMM and non-PMM municipalities.

Alternative estimators

For simplicity and econometric clarity, this paper relied on the canonical TWFE estimator to estimate the DiD. More recently developed estimators allow the staggered adoption of treatment and hence the inclusion of the PMM late-joiners, see Table [3.6](#) (see, for example, Roth et al. (2023) for an in-depth discussion about the recent DiD estimators and how they relate to the canonical TWFE estimator).

I check the obtained TWFE results with two of the most commonly applied recent DiD estimators. The first one, developed by Sun and Abraham (2021), estimates cohort-specific treatment effects, defining the cohorts relative to their time of treatment adoption and aggregating their weight on the estimated ATT relative to their cohort size. Its interpretations thus slightly differs from the period-specific

ATT estimated by the TWFE estimator. Appendix Table B.16 shows that PMM participation had an aggregate 0.05 pp positive impact on the PT vote share of the first and second round of the first presidential election following the local PMM adoption. Appendix Tables B.17 and B.18 explain the composition of the aggregate treatment effects. Table B.17 shows that late-joiners did not display the same positive effect that the always-treated group did: For example, for $T = 1$, i.e. the first election following the municipality's PMM access, municipalities which adopted the PMM by 2014 saw a 0.00766 increase in the PT vote share but the ones which adopted the PMM by 2018 and 2022 saw a decrease of 0.0058 and 0.013, respectively. Multiplying these values with the respective weight of the cohort leaves us with an aggregate effect of $0.00766 \times 0.855 - 0.0058 \times 0.089 - 0.013 \times 0.055 \approx 0.5$ percentage points.

The second one, developed by De Chaisemartin and d'Haultfoeuille (2020), is, by now, another widely used estimator that addresses the econometric problems of treatment effect heterogeneity and non-parallel trends in DiD setups with multiple periods and staggered treatment adoption. Whereas Sun & Abraham focus on cohort-specific treatment effects and aggregate them relative to cohort size, de Chaisemartin & D'Haultfoeuille use a weighted average of DiD estimators that compare outcome changes between groups that change their treatment status and those that do not. The control group thus consists of municipalities which have not yet "switched" treatment status (e.g. from non-participation to participation). By construction, all the underlying estimators have positive weights only, ensuring no cohort receives a distorting negative weight, potentially counteracting the treatment effect of other cohorts and biasing the overall composite effect.¹¹ Results again are broadly in line, see Appendix Table B.19.

3.8 Limitations & Future Research

Two large conceptual dimensions that future research on the topic of the political rewards of the PMM should address are (1) the local **intensity** of the PMM policy,

¹¹ Additional advantages of this estimator are that the estimator allows for non-binary treatments and the inclusion of units which switch their treatment on and off. I do not exploit both of these features, as I am restricting my analysis to the binary choice of whether a municipality participates in the PMM or not, and while the estimator would allow for the inclusion of the 230 municipalities that at one point stopped PMM participation (see Table 3.6), I stick to my sample to ensure cohesiveness between the models.

and the (2) **Cuban presence** within the PMM. Both dimensions could be crucial for how the policy translated into political effects. Also for the empirical estimation, future research would benefit from further exploring how potential heterogeneous treatment effects and the staggered treatment adoption influences how the PMM translated into electoral effects.

The first conceptual dimension recognizes that the PMM does not affect all participating municipalities equally. Table 3.9 shows the relative importance of PMM in different municipality sub-groups. It shows the average number of PMM doctors per 100,000 inhabitants, and the ratio of PMM doctors relative to public primary care doctors working in the municipality in 2012, i.e. one year before the general PMM introduction (Fontes et al., 2018). The table reveals some heterogeneity in terms of PMM intensity. The ratio shows that the PMM strongly expanded the number of available doctors in all participating subgroups, but especially so in the "equity" sub-groups. Comparing "equity" PMM municipalities with non-participating "equity" municipalities mitigates the concern that it is the actual number of doctors, rather than just policy participation, which is driving the electoral effects, but future research should address this intensity dimension more systematically.

	Municipality profiles						
	Capit.+area	G100	20% poor	Vuln. areas	PAB I	PAB II	PAB II+
PMM doctors (per 100k)	12.2	8.8	17.8	14.3	12.9	9.6	6.9
PMM-PC doctors ratio	1.1	1.4	1.3	1.1	0.8	0.8	0.9
N	448	91	1365	439	1241	615	200

Table 3.9. PMM municipality profiles, their respective mean number of PMM doctors per 100,000 residents, and their ratio of PMM doctors relative to pre-PMM (2012) public primary care doctors in 2014.

The second one highlights the disproportional importance that Cuban doctors played not just in terms of filling PMM vacancies but also in terms of politicising the program. Table 3.3 indicates that 92% of all PMM municipalities hosted at least one Cuban PMM doctor in 2014. Nevertheless, there is some heterogeneity. Table 3.10 shows that the Cubans were less dominant in the subgroups composed of larger municipalities, i.e. in the regional Capitals and their surrounding metropolitan area and in the G100, but they constituted the overwhelming majority of the PMM doctors in all remaining, smaller, municipalities, even the relatively developed PAB

ones.

	Municipality profiles						
	Capit.+area	G100	20% poor	Vuln. areas	PAB I	PAB II	PAB II+
PMM Cuban doctors (per 100k)	8.35	5.05	14.55	13.25	11.71	8.97	6.33
Cuban doctors share, PMM	63.79%	55.63%	82.16%	90.90%	91.16%	90.47%	83.09%
N	448	91	1365	439	1241	615	200

Table 3.10. PMM municipality profiles, their respective mean number of Cuban PMM doctors per 100,000 residents, and the share of PMM doctors which were Cuban in 2014.

The Cuban presence sharpens the visibility of the policy. This could have a positive electoral influence, for example if patients and voters appreciate the Cuban doctors and acknowledge the governmental effort to have imported them to Brazil, but it could also yield negative effects relative to PMM municipalities without Cuban doctors or even to non-PMM municipalities, for example if they are perceived as political actors rather than as medical professionals. The PMM offers an excellent setting to study the interplay between social policies, foreign service agents, and a very salient political left-right divide.

Empirically, my analysis shows trends in the two largest groups of Brazilian municipalities - the ones that adopted the policy early on and never dropped it and those that never adopted it within the sample period. In complementary analyses, I employed DiD estimators which are able to consider staggered treatment adoption, allowing me to also include the 593 "late-joiner" PMM municipalities, and results largely align with the canonical main model. Nevertheless, future analyses would benefit from a more careful discussion on the assumptions needed for each estimator. For example, the estimators discussed in Section 3.7.3 also account for treatment effect heterogeneity, i.e. that different cohorts could be affected differently by the treatment. The PMM was always perceived to be political, but discussions around it heated up especially during the time of the elections in 2018. Treatment effect heterogeneity based on the timing of policy access is thus plausible and merits a deeper and more careful discussion, also in order to motivate and identify the use of the most adequate estimator.

3.9 Conclusion

This paper investigates whether the implementation of the large-scale health care access policy *Programa Mais Médicos* (PMM) resulted in electoral gains for its po-

litical architects, the PT party, in the Brazilian presidential elections of 2014, 2018, and 2022. The program, which brought over 14,000 primary care doctors to underserved areas by 2014, was described as the “trump card” for the incumbent’s re-election campaign in that year (Bergamasco, 2013; Pires, 2023). My findings indicate that the PMM was electorally important for the elections in 2014 and 2018: program participation increased the PT’s vote share by roughly 0.9 percentage points in the first and second round of the presidential elections of 2014. For the presidential elections of 2018, I find a significant increase of 1 pp only in the second round of voting. The second round, featuring a final vote between the two strongest candidates from the first round, always included a confrontation between the PT candidate and a conservative opponent in my sample. While the PT candidate was associated with the PMM, the opponent was linked to opposition against the healthcare program. The dynamics observed in the second round likely reflect voter approval of the PMM and distributive social policies more broadly, rather than support for the PT itself (Zucco Jr, 2015). The magnitude of these effects amounts to over one-fifth of the difference between non-participating and participating municipalities in the 2010 base election and up to two-thirds of the average impact that the Bolsa Família program, known for produce large and consistent electoral returns for the PT (Zucco Jr, 2015; Zucco Jr & Power, 2013).

Furthermore, the PMM’s electoral impact depended on the actual need for the policy. Poor municipalities, targeted due to equity concerns, increased their PT vote share by 2.1 percentage points in the first round of voting in the 2014 elections. Places targeted due to electoral concerns but with less need for equity-improving healthcare measures did not change their PT vote share in the first round of elections, but increased it by 0.7 percentage points in the second round of the 2014 elections and by 0.9 percentage points in the second round of the 2018 elections. Further analyses indicate that most of this increase seems to stem from the relatively more needy municipalities within the electorally targeted groups. Again, voting patterns in the strategic second round of the elections could partly reflect disapproval of the opposition candidate’s plans to cancel the PMM.

This paper is among the few that causally link a health care policy to election outcomes and is the first to distinguish the electoral returns between target groups – municipalities more and less in need for the policy’s health care impacts. More-

over, my results have important policy implications: Efficient policy-making often meets its limitations facing a policy's political feasibility and viability. If a policy is unlikely to produce electoral advantages, politicians might be less willing to undertake it. In fact, a policy's effectiveness in terms of improving social outcomes and political chances is often at odds (Imai et al., 2020). Especially in developing economies, governments are said to frequently employ public funds to maximize political rather than societal gains (Golden & Min, 2013; Sugiyama & Hunter, 2013; Yoong & Closser, 2011). This paper paints a more nuanced picture: Municipalities in need of the PMM experience strong electoral returns, albeit only for one election cycle. The impact on electorally targeted municipalities is smaller and likely driven by support for the policy rather than its political architects.

Considering the evidence on electoral returns to distributive public policies, discussions on the effects of economic policies, such as conditional cash transfers, so far dominate the literature. The evidence on social policies, especially health care policies, is mixed or paints a pessimistic picture (Imai et al., 2020; Konisky & Richardson Jr, 2012; Sandholtz, 2023). This paper found a positive electoral impact of the PMM program in Brazil, and these political returns add to the benefits accrued to the ones in health care access and health outcomes. By showing that the places most in need for the health care policy also exhibit the highest electoral return, this paper shows that social policy targeting based on need can be electorally rewarding, helping to reconcile the at times conflicting societal and political aspirations on health care and other social policies.

Chapter 4

Doctor Turnover and Health Outcomes: Evidence from the Exit of Cuban Doctors in Brazil^{*}

^{*} This chapter is co-authored by Malte Becker, Rudi Rocha, and Thomas Hone. The authors thank Viola Angelini, Sebastian Jävervall, Lukas Linsi, Matías Mrejen, Milena Nikolova, Marcello Perez, Leonardo Rosa, Daiki Saka, and Renato Tasca for their valuable feedback.

Abstract

This paper studies the effects of a large-scale doctor turnover on health care utilization, health outcomes, and health system inputs in Brazil. Identification relies on an unexpected massive exit and replacement of doctors in affected municipalities. We find a strong and persistent decrease in the care of chronic diseases, while service utilization for conditions requiring immediate care, such as infections, recovered quickly throughout the turnover period. The reduction in service utilization did not translate into any changes in health outcomes. Adaptation of local health systems and demand diversion helped mitigate turnover effects without major immediate adverse repercussions for population health.

4.1 Introduction

Developed and developing countries alike have large regional disparities in the provision of medical doctors, with remote and under-served areas often facing more limited supply of personnel. Additionally, retaining doctors in these areas remains a major challenge (WHO, 2010). Low retention rates and the resulting turnover of doctors can negatively affect health care and health outcomes due to understaffed services, discontinuity of care and disruptions in the patient-doctor relationship (WHO, 2018). Yet, as the global demand for health care professionals continues to grow, doctor turnover is projected to increase, especially in remote areas (Esu et al., 2021; Liu et al., 2017). Evidence on the effects of doctor turnover on health services and health outcomes is nevertheless sparse and has come from specific contexts in developed countries —e.g. from episodes of physician retirement or of changes in the network of providers within private insurance schemes— where relatively fast replacement of professionals, wider access to services and patient choice can, in principle, potentially mitigate any adverse effects on health outcomes. Very little is known, however, about how doctor turnover affects health services and patient outcomes in resource-scarce contexts, and how other health care system inputs respond to the exit and replacement of doctors in general.

This paper studies the effects of a large-scale doctor turnover event on health care utilization and health outcomes, exploiting a unique shock in Brazil. To overcome a chronic lack of doctors in under-served areas, in 2013 Brazil introduced the More Doctors Program (*Programa Mais Médicos* – PMM). Over 18,000 doctors served in primary care services in eligible municipalities, roughly half of them being from Cuba. Five years after the introduction of PMM, following the election of Brazil’s former right-wing president Jair Bolsonaro in October 2018, the Cuban government unilaterally withdrew all of the Cuban doctors from the program. The sudden exit led to a major turnover of primary health care doctors in municipalities where Cuban doctors had been working. Some municipalities were left without PMM doctors for a few months, until Cuban doctors were replaced by new doctors. Other municipalities, where no Cuban doctor had been working, remained unaffected.

We exploit the sharp and unexpected exogenous timing of the Cuban exit from PMM in municipalities that relied more *versus* less on Cuban PMM doctors at the

time of the exit to estimate the effects of doctor turnover on health care utilization, health outcomes and system adaptation. More specifically, we use an event-study approach to compare outcome changes in affected and unaffected PMM municipalities before and after the Cuban exit, conditional upon time and municipality fixed effects, therefore controlling for baseline heterogeneity in levels and common time trends. We combine different fine-grained administrative microdata sets on health care utilization, hospitalizations and mortality, as well as on the supply of healthcare facilities, personnel, salaries, prescription drug sales and private health insurance into a municipality-by-month panel of data between October 2017 and December 2019. The overall evidence indicates that pre-trends in outcome variables remain irrelevant, while alternative specifications, alternative treatment definitions and robustness checks help support our identification strategy.

We find an immediate drop in the number of primary care doctors in affected municipalities from November 2018 on. That reduction is particularly pronounced for municipalities where all PMM doctors were Cuban. From January 2019 on, affected municipalities started to fill PMM vacancies, and by May 2019 doctor supply stabilized, almost reaching pre-treatment levels. We call the period in which vacancies were filled as the turnover period. Next, we document meaningful reductions in primary care utilization. Effects are limited to physician duties, and most persist after the turnover period. Persistent reduction in service utilization is primarily driven by conditions requiring prevention and ongoing care, such as chronic conditions and related risk factors. For example, hypertension consultations decline on average by 41% in the most affected municipalities immediately after the Cuban exit, and remain 25% lower than base levels throughout and after the turnover period. Importantly, while there was a strong and persistent decrease in the care of chronic diseases, service utilization for conditions requiring immediate care, such as infections, recovered quickly in tandem with the filling of vacancies during the turnover period. Finally, the reduction in health care utilization surprisingly did not translate into any systematic changes in health outcomes even throughout the end of the following year after the Cuban exit. We do not observe any effects on hospital admissions nor on mortality, neither on aggregate levels nor from specific causes.

Adaptation of local health systems and demand diversion seem to have helped

mitigate turnover effects without major adverse repercussions for the affected population. We explore different intermediate outcomes that could explain the overall pattern. We first focus on adaptation within primary care services. We do not find any changes in the supply of other health professionals nor in task substitution across different professionals. Moreover, we do not observe an improvement in the experience profile of the new doctors that replaced the Cuban professionals. Yet, patients in need of prescription medication for chronic diseases were able to secure their access despite the reduction in access to other related health care services, such as consultations. Together with the rapid recovery of consultations for conditions requiring immediate care, the overall evidence suggests a prioritization of primary care services towards urgent and curative needs as well as other protective services.

Second, we investigate system adaptation and demand diversion beyond primary care services. While there was not any significant changes in the use of hospitals and specialists, we observe an increase in ER visits for low-complexity conditions as well as in the number of ERs offering basic ambulatory care. Patients therefore seem to partly substitute treatment at primary care centers with treatment at ER facilities, while municipalities start providing more low-complexity services in these facilities. Importantly, this diversion persists even after the turnover ends. We also provide suggestive evidence for broader labor market effects: We document a large, but imprecisely estimated, increase in non-PMM doctors' salaries in most affected municipalities. As outside of PMM municipalities are responsible for paying out for the wages of public primary care doctors, this increase implies rising direct costs for the affected municipalities.

Finally, we do not observe any changes in private insurance coverage in affected municipalities nor detect any spillover effects across municipalities. Adaptation therefore comes at the cost of a reduction in preventive care in primary care services, an increase in utilization of ER facilities and an increase in health care expenditure. This could entail fragmentation and decreased efficiency in health care, with potential negative long-term consequences for tackling the increasing burden of chronic diseases.

This paper contributes novel evidence to the emergent literature on the impacts of doctor supply, more generally, and of doctor turnover, more specifically, on health care utilization and population health outcomes. Existing studies docu-

ment positive associations between the supply of health professionals and health outcomes, but often struggle to establish causality (Anand & Bärnighausen, 2004; Basu et al., 2019). Causally identified studies are scarce and document a nuanced picture, especially in resource-scarce contexts. Experimental evidence has recently come from Okeke (2023), who shows that the marginal addition of doctors in primary care services in Nigeria significantly improves health outcomes, while lower-qualified health workers do not have any significant effects. Assessments of the introduction of PMM in Brazil provide quasi-experimental evidence and have gained academic attention both in economics and public health. Similarly to other contexts, the country has long faced an unequal distribution of health care professionals, both in terms of quantity and quality (Costa et al., 2019). Differently from Nigeria and other low-income countries, however, where most medical care is paid for out-of-pocket, Brazil has a national health system model where access to services is free at the point-of-use. Despite increasingly dense, the literature on the effects of the introduction of PMM has yielded mixed results. For instance, Carrillo and Feres (2019) document that PMM led to the replacement of nurses by higher-qualified primary care doctors, without any effects on infant health. More aligned with Okeke (2023)'s results, Fontes et al. (2018) and Mattos and Mazetto (2019) find that the introduction of PMM decreased hospitalizations, while Hone et al. (2020) document a reduction in amenable mortality.

Existing research on doctor turnover, more specifically, has made progress at the micro level by examining the effects of specific disruptions in the patient-physician relationship —e.g., by exploiting the exit of physicians from the labor force or the re-matching of patients to new physicians due to physician retirement, relocation or death (Fadlon & Van Parys, 2020; Sabety, 2023; Sabety et al., 2021; Schwab, 2023; Zhang, 2022), practice closures (Bischof & Kaiser, 2021; Simonsen et al., 2021) and changes in insurance managed care networks (Staiger, 2022). Studies come to mixed results. For instance, while Simonsen et al. (2021) do not find major changes in health care utilization in Denmark, Sabety et al. (2021) and Zhang (2022) find a decrease in primary care utilization which is often substituted by an increase in specialized or emergency care in the US. Effects on hospitalization are often limited, and mortality remains generally unaffected. Yet, studies have focused on managed care and private insurance settings from developed countries,

where turnover events may be anticipated, patients can often choose among alternative providers, and may even benefit from changes. As Simonsen et al. (2021) document, in Denmark transition between providers is “smooth”, while Simonsen et al. (2021) and Zhang (2022) find an increase in the diagnoses of chronic diseases after patients visit a new primary care doctor, potentially due to changes in practice styles. In that case, the re-matching of doctors can be beneficial for patients.

We contribute novel evidence to the literature by exploiting a unique setting where an exogenous political shock led to a massive exit and turnover of doctors across local health systems in Brazil. We focus at a temporary decrease and replacement rather than an increase in the number of doctors, and characterize the periods of reduction in doctor supply as well as of turnover and supply stabilization. This is relevant as long as hirings and increases in the supply of doctors are generally followed by challenges in workforce retention and turnover, and positive and negative variations in doctor supply do not necessarily lead to symmetric effects on outcomes. Yet, while doctor turnover is a widespread policy challenge in both developed and developing countries, existing evidence comes from specific disruptions in the patient-provider relationship within developed countries, and generally overlooks response beyond the patient level. In this paper, we not only assess effects on health utilization and population health outcomes in under-served Brazilian localities, but also document how other health care system inputs respond to the exit and replacement of doctors. Results are therefore generally informative for resource-scarce contexts, where health systems are typically less resilient, and access to services and patient choice among providers are often constrained.

This paper is organized as follows. In Section 4.2 we describe the institutional background. In Section 4.3 we detail the data while in Section 4.4 we describe our empirical model. Results are presented in Section 4.5. In Section 4.6 we discuss potential explanations behind the results. Section 4.7 concludes.

4.2 Background

4.2.1 The Brazilian Health System and Primary Care Services

The Brazilian Unified Health System (*Sistema Único de Saúde*, SUS) is a tax-funded public health care system that provides health care services that are free-of-charge at the point of care at all levels of care. Although there exists private provision of

health insurance and health services in Brazil, approximately 75% of the Brazilian population rely exclusively on SUS for health care (Rocha et al., 2021).

SUS has successfully expanded access to health care and improved health outcomes in Brazil (Castro et al., 2019). The Family Health Program (FHP), implemented by municipal governments and now covering over 60% of the population, has been key to that success (Bhalotra et al., 2019; Mrejen et al., 2021). The program provides preventive and curative primary care services through Family Health Teams (FHTs). Each team is based in a primary health care facility, called Basic Health Unit (*Unidade Básica de Saúde*, UBS), and typically consists of at least one doctor, nurses, community health workers and technicians. Each FHT covers around 3,500 individuals from a specific catchment area, and should respond to most of the population's needs by providing curative care, health education, health promotion, disease prevention, less complex medical procedures, and referencing the more severe cases to specialized secondary or tertiary health care services when needed.

4.2.2 The More Doctors Program

Despite increasing numbers of doctors over the last decades, the supply of doctors and access to medical services and specialist care remain major bottlenecks for SUS in many regions, resulting in unmet demand, long waiting times, and delays in diagnoses (Castro et al., 2019). Brazil has long suffered from a highly unequal provision of doctors across municipalities, with urban peripheries and remote areas severely under-served. Making an effort to increase the supply of doctors in those regions, in 2013 the Brazilian government introduced the More Doctors Program (*Programa Mais Médicos*, PMM). The program was designed to pay competitive wages and provide additional benefits to doctors who agreed to work in eligible municipalities. Eligibility criteria are based on municipalities' socioeconomic characteristics such as population size, income per capita, and poverty rates (Hone et al., 2020).

At the program's inception, the government offered PMM positions to all Brazilian doctors. As low numbers of Brazilian doctors applied to fill the vacancies, the program was opened to doctors trained abroad. Through an international agreement between the governments of Cuba and Brazil, facilitated and managed by the

Pan American Health Organization, a substantial number of Cuban doctors were sent to Brazil (Santos et al., 2017). Out of around 18,000 doctors that enrolled in the program between 2013 and 2018, roughly half came from Cuba. Whereas Brazilian doctors had priority and were able to freely choose their destination, Cubans were assigned to vacant positions and were seen as the last resort to fill PMM vacancies. Moreover, Brazilian doctors could work at multiple municipalities at the same time, whereas Cuban doctors were only allowed to work in one municipality. Given that Brazilian PMM doctors had a strong preference for urban centers, the Cuban doctors were essential for filling vacancies in poorer municipalities and remote areas (Santos et al., 2018).

In some participating municipalities, PMM doctors work alongside non-PMM primary care doctors. But the work schedules of PMM and non-PMM doctors differ in important ways. First, PMM doctors are only allowed to work in one primary health care unit, while non-PMM doctors are allowed to work in multiple units at the same time, also across municipalities (Santos et al., 2017). Second, PMM doctors are closely monitored and work a set of fixed 40 weekly hours, which make them more accessible and reliable than non-PMM doctors (Comes et al., 2016b). Finally, qualitative evidence suggests that PMM doctors play a crucial role in delivering primary care to Brazil's poorest population. Cuban doctors in particular were often integrated into the local community, and most of them decided to apply for an extension of their stay in 2017 (Comes et al., 2016a; Rech et al., 2018).

Cuba's participation in PMM was highly politicized. One fierce opponent was Brazil's former president, Jair Bolsonaro, who claimed the true goal of PMM was the installation of a foreign communist guerilla in Brazil (Terra, 2019). Following his election in October 2018, the Cuban government unilaterally and unexpectedly decided to withdraw all of its 8,316 primary care doctors from PMM. As a consequence, Cuban doctors had merely a month to leave the country, and all consequently ceased to work before Bolsonaro took office in January 2019 (Santos et al., 2018). According to the media and policy makers, the decision came as a shock for PMM doctors and the involved institutions, given that the international contract had been extended only a year before (Sobrinho, 2018). Given the importance of the Cuban doctors for health care delivery, the media and academic community raised strong concerns about the potential adverse effects on health outcomes

(Alves, 2018). The Brazilian government put great effort and expenses into trying to replace the missing primary care doctors, managing to fill almost all resulting PMM vacancies after a few months (Maffioli et al., 2019).

4.2.3 Expected Effects

The Cuban exit from PMM triggered a large-scale turnover of more than 8,000 primary care doctors, affecting over 2,800 municipalities. Building on the existing literature on doctor turnover, we explore various outcomes and mechanisms. Specifically, we examine potential impacts on (i) health care utilization and patient health outcomes; (ii) the organization of work and services within affected primary care centers; (iii) the demand for health care services beyond affected primary care centers; and (iv) physician labor markets and municipality-level spending.

A priori, we expect negative effects on primary care utilization (Staiger, 2022). Reductions may be strongest for services that are primarily delivered by doctors, and for conditions that are most susceptible to disruptions in the medical workforce such as chronic conditions (Bischof & Kaiser, 2021; Sabety et al., 2021). The potential decrease in primary care may increase hospitalizations and mortality, especially for conditions most affected by the turnover (Sabety, 2023).

The extent to which these potential effects on health care utilization and health outcomes materialize depends on adaptation within and beyond the affected primary care centers. Within primary care centers, task-shifting, re-staffing, forms of service prioritization, and other adaption mechanisms can all lead to changes in the profile of treatment and service delivery (Fadlon & Van Parys, 2020; Garthwaite, 2012; Reddy et al., 2015). Task-shifting could, for example, entail that nurses take over tasks and responsibilities previously assumed by doctors (Carrillo & Feres, 2019). In terms of services, primary care doctors might need to prioritize the provision of acute care, at the expense of preventive care, to cushion the most adverse effects in the short term (Cunningham et al., 2008).

Beyond affected primary care centers, both demand and supply-side factors can mediate the impact that turnover has on service utilization and health outcomes. Variation in the network or the quality of health care providers can lead to demand diversion towards alternative health care facilities — i.e., patients substituting public primary care with emergency, hospital, or fee-for-service care (Bhalo-

tra et al., 2020; Sabety et al., 2021; Simonsen et al., 2021).

Finally, the broad turnover shock may affect labor markets and incurred costs (Costa et al., 2019). Specifically, the sudden increase in demand for doctors could drive up doctors' salaries.

4.3 Data

4.3.1 Data Sources and Main Variables

We use different data sources to combine information on the number of doctors, health care utilization and inputs, subsidized prescription drug sales, and health outcomes at the municipality-by-month level. We cover the period from October 2017 to the end of December 2019. We end our analysis in December 2019, a year after the Cuban exit, just before the Covid-19 crisis. We detail below the main variables used in our analysis and their respective sources of data.

Data on doctors and PMM. We use data on municipalities' PMM participation from the Brazilian Ministry of Health. The data set contains PMM eligibility and the number of PMM doctors (Cuban and others) registered in each municipality, for all Brazilian municipalities in a given month. We limit our sample to municipalities that are eligible to participate in PMM. Excluding non-eligible municipalities, which are typically Brazil's most developed ones, makes our main sample more homogeneous.¹ Out of all 5,570 municipalities in Brazil, 4,162 participated in PMM.² Given this sample, we divide PMM municipalities into three groups, based on the relevance that Cubans played for the local PMM in October 2018, the month just before the Cuban withdrawal. Municipalities with no Cuban PMM doctors in October 2018 form our control group ($N = 1,320$). Municipalities with Cuban PMM doctors, but also non-Cuban PMM doctors form our first treatment group ($N = 1,260$). This group lost on average around 50% of their PMM doctors with the Cuban exit. Municipalities with only Cuban PMM doctors form our second treatment group ($N = 1,583$). Having temporarily lost all their PMM doctors with the exit, we expect this treatment group to be most strongly affected by the shock.

¹ Robustness checks show that results remain stable upon including these municipalities into our control group.

² We exclude the 34 special indigenous health districts from our analysis, as they can not be adequately captured by municipal borders.

We complement the PMM data with information on the total monthly number of primary health care doctors per municipality (PMM and non-PMM doctors) from the National Registry of Health Facilities (*Cadastro Nacional de Estabelecimentos de Saúde*, CNES). CNES contains information on all Brazilian healthcare facilities, healthcare workers enrolled in each facility, and their weekly working hours. A limitation of the CNES data is that it is not well suited to capture high frequency changes. Even though data is available on a monthly level, information is updated irregularly (facilities need to update their employment numbers only at least every 6-months). Hence, CNES may not fully capture monthly changes and will be used as an auxiliary source of information. Moreover, the CNES data set does not indicate whether primary care doctors are enrolled in PMM or not. To distinguish between PMM doctors and non-PMM doctors in the CNES dataset, we merge the PMM dataset with CNES through the names and municipality work location of the doctors.³

Primary healthcare production. We obtain data on primary health care production and utilization from the National Information System on Primary Health Care (*Sistema de Informação Atenção Básica*, SISAB). The system registers all primary care services provided in a given municipality in a given month and funded by SUS, including information on the nature of the service and the responsible health care professional. The system allocates health care services into four overarching categories: consultations, procedures, dental care, and home visits. Out of these four, consultations are predominantly done by doctors, while the others are mostly delivered by other health care workers. Primary care consultations, i.e. doctors' main duties, are further divided into the type of appointment (spontaneous, scheduled one time, scheduled on regular basis etc.) and into specific conditions (diabetes, hypertension, tuberculosis, obesity etc.). Tables C..1 and C..2 provide the details.

We collect data on primary care utilization related to asthma, diabetes, hypertension, cardiovascular diseases, cancers, chronic obstructive pulmonary disease (CPOD) and mental health as chronic conditions. Related risk factors are malnutrition, obesity, rehabilitation, and consultations concerning tobacco, alcohol, and drug use. Maternal health is concerned with the full cycle of pre- and postpartum. Dengue, leprosy, tuberculosis, and sexually transmitted diseases cover outcomes

³ We were able to match around 81% of PMM doctors to their corresponding CNES information.

related to infectious conditions in the data.

We also use data on ambulatory care production from the National Information System of Ambulatory Care (*Sistema de Informação Ambulatorial*, SIA). This data set contains information on ambulatory care production by number and type of procedures which are funded by SUS and delivered at different providers, per municipality and month. These data allow us to distinguish between outpatient care provided at emergency rooms, hospitals or specialty centers.

Prescription drugs and private insurance coverage. We obtain data on prescription drug sales from Brazil's large-scale subsidizing program of prescription drugs (*Aqui Tem Farmácia Popular*, ATFP). The program covers sales on medication for different conditions such as diabetes and hypertension, which are handed out to patients at strongly subsidized prices or for free.⁴ The data cover the amount and value of prescription drugs handed out by the 24,336 pharmacies enrolled in ATFP, for different conditions, by month and year from Brazil's Ministry of Health. To get access to the subsidized medication, patients need to have a medical prescription, which is valid for up to 180 days (Américo & Rocha, 2020). Both the PMM and the ATFP program aim at ensuring accessible and affordable health care for everyone. In 2019, the ATFP program reached a substantial share of patients suffering from conditions that are covered by the program—around 50% of Brazilian patients who recently took medication for diabetes or hypertension obtained at least some of them via ATFP (Américo & Rocha, 2020). We use data on private insurance coverage from Brazil's National Agency for Supplementary Health (ANS). The data include information on the number of individuals with a private health insurance per municipality for every three months.

Hospitalization and mortality. We obtain microdata from the National System of Information on Hospitalizations (*Sistema de Informação Hospitalar*, SIH), which includes all hospitalizations publicly funded by SUS disaggregated by primary cause of admission (based on ICD-10 code; International Classification of Diseases 10th Revision) and the patients' municipality of residence. We also obtain microdata from the National System of Information on Mortality (*Sistema de Informação de Mortalidade*, SIM), which includes official registries of all deaths recorded in Brazil. The

⁴ Drugs for diabetes and hypertension are fully subsidized and free for patients, while prices for other medications did not change during our sample period.

microdata include the municipality of residence and the ICD-10 code for the main cause of death.

Doctors' Wages Data on doctors' monthly wages comes from RAIS, a database that covers all formal employees in Brazil including their occupation code. PMM doctors are not included in the data set, because their salaries are considered *stipends* and thus do not enter RAIS. While the data set does not allow us to directly identify primary care doctors and their corresponding primary care centers, we use information on specific doctor classifications, for example community doctors, in combination with information on whether their employer is a public or a private entity. RAIS data is not available for all Brazilian municipalities. From the 5,561 municipalities included in all other analyses, we can only work with RAIS data from 4,228 municipalities (1,034 in the control group, 1,034 and 1,115 in the first and second treatment group, respectively, and 1,045 from non-PMM municipalities).

Socioeconomic characteristics. We use annual municipal socioeconomic characteristics as control variables in auxiliary exercises. We include the number of families receiving the Bolsa Familia social welfare transfers per 100,000 residents, and the municipal costs of Bolsa Familia per resident (data from the Ministry of Social Development, MDS); the municipal GDP per capita, the value added by public sector employment to the local economy, the share of the population above the age of 65, the sex ratio, the size of the municipal population, and the municipality's population density (data from IBGE).

4.3.2 Descriptive Statistics

Figure C..1 illustrates the spatial distribution of municipalities according to their treatment status. White municipalities do not participate in PMM and are thus not a good comparison group for the treated municipalities and excluded from our main analysis. Light grey municipalities are our control group, dark grey municipalities represent treatment group 1, and black municipalities form treatment group 2. There is no discernible geographical pattern with respect to the allocation of municipalities into the different groups.

Table C..3 summarizes all outcome variables investigated in our analyses, per 100,000 residents in October 2018 (the baseline month), including doctor supply, health care utilization and health outcome variables. The average control muni-

pality employed 11 PMM doctors per 100,000 residents, as compared to 18 and 20 in our two treatment groups. In our first treatment group, 52% of PMM doctors were Cuban, which is equivalent to about 29% of all primary care doctors working in public primary care facilities.⁵ In municipalities in treatment group 2 the Cubans, and, by extension, PMM doctors, represented approximately 45% of all primary care doctors working in primary care facilities. Primary care doctors were responsible for 64% of the primary care consultations during the pre-treatment period, but only for 4% of the ambulatory procedures and for 0% of home visits and dental care services. Yet, while consultations in general are doctors' main duties, some of them are carried out by other health workers, depending on the specific condition.⁶ For descriptive statistics on the auxiliary socioeconomic variables see Table C.4.

4.3.3 Trends in PMM Coverage

How did the Cuban exit affect the supply of primary health care doctors? Figure C.2 shows the monthly unconditional means of the number of PMM doctors (upper plot), of the number of primary healthcare doctors not affiliated with PMM (middle plot), and of the number of all primary health care doctors (PMM and non-PMM, lower plot) per 100,000 residents by treatment group. In general, the unconditional means move similarly throughout the groups before the treatment. The Cuban exit and resulting turnover clearly marks a recess in the trends for affected municipalities. When examining PMM doctors, we observe a sharp drop in coverage for treatment groups 1 (dark gray) and 2 (black), with treatment group 2 being more strongly affected. While in treatment group 1 the number of PMM doctors is halved, the second treatment group briefly loses all PMM doctors. Importantly, there is not any clear inflection in the control group (light gray). Within a few months after the shock, PMM coverage in treatment municipalities recovers and almost returns to pre-treatment levels.

Although less pronounced, a similar pattern can be observed when looking at overall primary health care doctors (lower plot). The treatment groups experience a drop in coverage, followed by a recovery a few months after the shock. The drop, however, is substantially smaller than the drop in PMM doctors, which can

⁵ Doctors working in multiple healthcare facilities in the same municipality are not double-counted.

⁶ Disaggregating consultations by conditions, however, comes at the cost of decreasing data quality due to missing values. On average, around 40% of all primary care consultations have no information about the related condition and the responsible professional (Appendix Table C.1).

be explained by two factors. First, there was an expansion in primary health care doctors not affiliated with PMM after the Cuban exit which partially cushioned the Cuban exit (middle plot). Second, as mentioned in Section 4.3.1, even though CNES data is available at the monthly level, records are not immediately updated as facilities need to update their employment numbers only at least every 6 months, therefore smoothing aggregated trends.

4.4 Empirical Model

We exploit the sharp and exogenous timing of the Cuban exit from PMM in municipalities that relied more *versus* less on Cuban PMM doctors at the time of the exit to estimate the effects of doctor turnover on health care utilization, system adaptation, and health outcomes. More specifically, we use a two-way fixed effects model to compare outcome changes in affected and unaffected PMM municipalities before and after the Cuban exit, conditional upon time and municipality fixed-effects, therefore controlling for baseline heterogeneity in levels and common time trends. For a PMM municipality i at year-month t , the equation takes the following form:

$$Y_{it} = \alpha_i + \gamma_t \times \phi^s + \sum_g \sum_{t=-13}^{t=-2} \beta_t(T_t \times D_i^g) + \sum_g \sum_{t=0}^{t=14} \beta_t(T_t \times D_i^g) + \epsilon_{it} \quad (4.1)$$

Where Y_{it} refers to outcomes of interest.⁷ The term D_i^g is our categorical treatment variable and takes $D_i^{g=1} = 1$ if the municipality had some Cuban doctors, and $D_i^{g=2} = 1$ if it had only Cuban doctors in PMM in October 2018, the month before the Cuban exit. Municipalities are assigned to the control group otherwise.

The first summation set captures the coefficients of the interactions between treatment status and year-month dummies T_t before October 2018, while the second refers to coefficients after the shock. We include municipality fixed effects α_i to control for fixed characteristics and baseline heterogeneity across municipalities. As we focus on a relatively high frequency data just around the shock – i.e. monthly variation within approximately a 2-year period – the term α_i is expected to absorb relevant slow-moving determinants of health, such as the presence of hospitals and

⁷ Most outcomes are measured as per 100,000 residents, and we transform them with the hyperbolic sine transformation (HST). For outcomes which display a reasonable amount of zero values (more than 10%), we also repeated all estimation with outcomes as rates per 100,000 residents weighted by the population size of the municipality. Results are robust throughout all forms of outcome specification.

transport connections. Year-month fixed effects γ_t control for common time trends, particularly the common timing of the Cuban exit and the influence of the political cycle. Although we focus on a short period of time, we further consider interactions between time and state fixed effects ϕ^s , therefore allowing for flexible time trends per state. This is expected to absorb the influence of state policies, particularly relevant in health care as states are typically responsible for secondary and tertiary health services. We cluster standard errors at the municipality level to allow for correlation of the error term within the same municipality throughout time.

We assume that the variation in the interaction between the sharp and unexpected exit of Cuban doctors (time-series variation) and the baseline variation in the presence of Cuban doctors (across municipalities) is exogenous to municipalities, conditional on fixed effects. Identification relies on the assumption that trends in outcomes in both treatment groups would not have differed from the observed trend in the control group should Cuban doctors had not left PMM. We investigate the parallel trends assumption by looking at monthly differences in outcomes between control and treatment groups throughout the year before the Cuban exit. More specifically, we visually inspect and formally test whether the coefficients β_{-13} (October 2017) to β_{-2} (September 2018) portray any systematic pattern. The overall evidence, across outcome variables, indicates that pre-trends remain irrelevant.

We also present results from alternative specifications and treatment definitions. First, instead of considering treatment as categorical groups interacted with time fixed effects, we use the share of Cuban PMM doctors among all local primary care doctors at baseline interacted with time fixed effects, as follows:

$$Y_{it} = \alpha_i^* + \gamma_t^* \times \phi^{*s} + \sum_{t=-13}^{t=-2} \beta_t^* (T_t \times S_i) + \sum_{t=0}^{t=14} \beta_t^* (T_t \times S_i) + \epsilon_{it}^* \quad (4.2)$$

Where S_i refer to the share of Cuban PMM doctors in municipality i in September 2018 among all local primary care doctors (PMM and non-PMM). In this case we must rely on a more stringent assumption of parallel trends along the distribution of S_i . This specification nevertheless comes with the benefit of estimating average effects across all PMM municipalities rather than splitting the estimates for different treatment groups. Finally, although we rely on a high frequency panel

of data, in additional robustness checks we include time-varying controls to inspect the stability of our estimates. We show that results remain stable to adjusting for municipality annual GDP per capita, population sex and age structure, and social welfare transfers.

4.5 Turnover, Health Utilization and Outcomes

4.5.1 Turnover and Supply of Doctors

We start our empirical analysis by investigating the effect of the Cuban exit on the supply of primary care doctors in affected municipalities. Figure 4.1 shows event studies for the number of PMM doctors, the number non-PMM doctors (i.e. primary health care doctors not affiliated with PMM) and the number of all primary care doctors (PMM and non-PMM) per 100,000 residents.

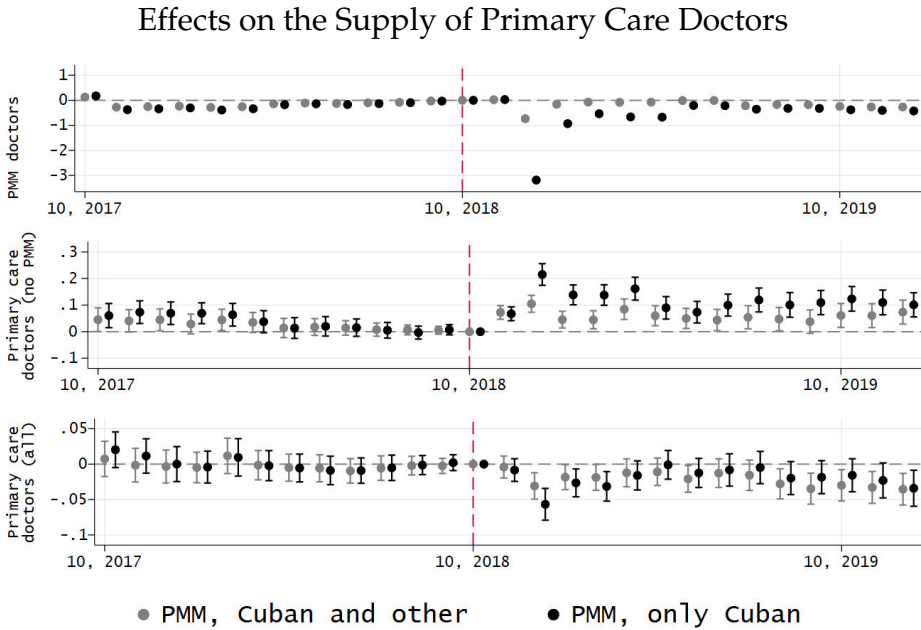


Figure 4.1. The graph shows event studies for the number of PMM doctors (upper plot), non-PMM primary care doctors (medium plot), and all primary care doctors (lower plot), following equation 4.1. Outcomes per 100,000 residents and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health and CNES.

We observe a steep reduction in the supply of PMM doctors, especially for treatment group 2. In December 2018, at the height of the impact, PMM coverage decreased, on average, by 52% in municipalities of treatment group 1, and by 96% in municipalities of treatment group 2.⁸ This corresponds to 9 fewer PMM doctors per 100,000 residents in treatment group 1 (relative to a group average of 18 PMM doctors per 100,000 residents in October 2018) and 19 fewer PMM doctors per 100,000 residents in treatment group 2 (relative to a group average of 20 PMM doctors per 100,000 residents in October 2018)⁹. The reduction is meaningful in absolute terms, and closely mirrors the increases in primary care doctors following the PMM introduction (Carrillo & Feres, 2019; Hone et al., 2020). The supply of PMM doctors recovered for both treatment groups a few months after the Cuban exit, with treatment group 1 filling vacancies faster than treatment group 2. While the reduction in PMM coverage remains significant beyond the turnover period, the difference is small in absolute terms and in comparison to pre-treatment levels.

Examining non-PMM primary health care doctors shows there is an expansion in their supply, seemingly as an immediate reaction to the Cuban exit. For treatment groups 1 and 2, there is an increase of 9.4% and 18.9% of non-PMM primary health care doctors in December 2018, corresponding on average to 2 and 5 doctors per 100,000 residents, respectively. These numbers halve in 2019 as PMM vacancies are filled, but remain elevated throughout our sample period. We also observe a decaying tendency in the number of non-PMM primary health care doctors by the turn of the 2017-2018 years. This trend is nevertheless smooth and point estimates are relatively small in comparison to the period after the shock. Effects stabilize for most of 2018, remaining systematically around zero in the months before the Cuban exit.

Finally, looking at all municipal primary care doctors (PMM and non-PMM), we see a small but significant drop during the supply shock period. Despite the expansion of non-PMM doctors, the number of all primary care doctors decreased, on average, by 3% in treatment group 1 and 5.5% in treatment group 2 in December

⁸ Cuban doctors left Brazil from mid-November 2018 onward. Thus, December 2018 was the first month in which vacancies were reported. One reason why the estimated decline in treatment group 2 is slightly short of 100% is that the Brazilian government might have managed to fill few of the resulting vacancies in the second half of November 2018.

⁹ Throughout the analysis, we use group-specific means from Table C.3 to interpret effect sizes in absolute numbers.

2018, corresponding to roughly one and two primary care doctors per 100,000 residents. These effects can be interpreted as a lower bound as CNES records do not accurately respond to monthly changes in actual figures.

Overall, we document a sharp and short-lived drop in the number of PMM doctors in municipalities where PMM enrolled Cuban doctors. This drop resulted in a large-scale doctor turnover, where non-PMM doctors partly compensated for the exiting Cubans, but still resulting in a transitory decrease in doctor supply.

4.5.2 Primary Care Utilization

Given the turnover and the short-term drop in the supply of primary care doctors, we expect a concurrent short-term reduction in primary care utilization related to consultations, which are doctors' main duties. Figure 4.2 shows a 12% reduction in consultations in treatment group 1, and a 28% drop in treatment group 2 in December 2018. Effect sizes are broadly in line with patterns observed just after the introduction of PMM (Carrillo & Feres, 2019; Hone et al., 2020; Mattos & Mazetto, 2019). The decline is meaningful: on average, the reduction corresponds to 1,401 and 4,533 fewer monthly consultations per 100,000 residents in treatment group 1 and 2, respectively. The negative effect largely recovers as vacancies are filled with new PMM doctors. Recovery is quicker in treatment group 1, but a negative effect of around 10% persists beyond the turnover period in both treatment groups until the end of 2019, corresponding to roughly 1,170 and 1,620 fewer consultations per 100,000 residents. Figure 4.2 also shows that effects are limited to primary care consultations, which are doctors' main duties. There are no significant effects for tasks that are primarily performed by nurses and other healthcare workers in primary health care centers, such as minor procedures. Those patterns reassure us that effects are in fact driven by the doctor turnover and not by other potentially correlated shocks.

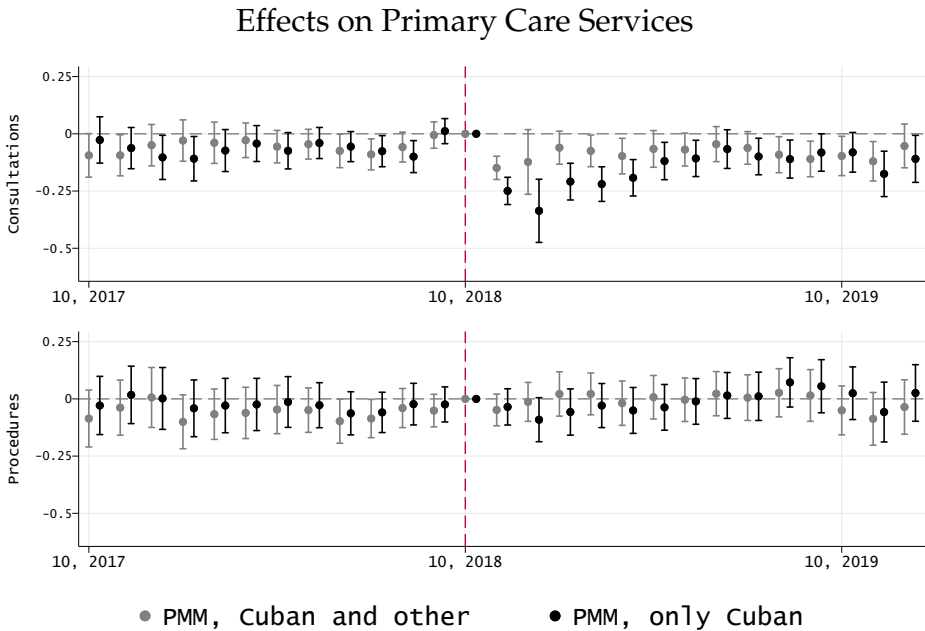


Figure 4.2. The graph shows event studies for the number of consultations (upper plot) and procedures (lower plot) performed in primary health care facilities, following equation 4.1. Outcomes per 100,000 residents and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB.

When unpacking consultations further, we see that not all primary care needs are affected in the same way. In Figure 4.3, we distinguish between primary care consultations on chronic conditions, related risk factors to chronic conditions, maternal health, and infections. The effects on chronic conditions and related risk factors are strong and persistent, especially for treatment group 2. This group experiences a drop of roughly 30% for chronic care (around 1,500 per 100,000) and for related risk factors (around 600 per 100,000) in December 2018, and this decrease persists in magnitude until the end of our sample period. Consultations on maternal health are only temporarily affected and fully recover after the turnover period. For example, there were on average around 12% (around 350 per 100,000) fewer maternal health consultations for treatment group 2 in December 2018, but the trends were indistinguishable from zero for most of 2019. Consultations on infectious diseases experience a short-term reduction, followed by recovery, with some significant effects towards the end of our study period.

Effects on Primary Care Consultations

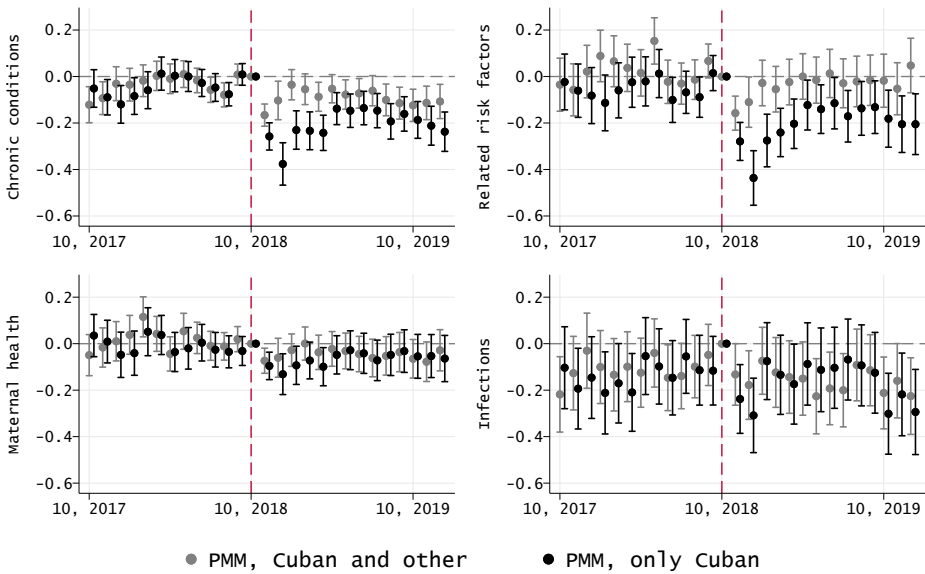


Figure 4.3. The graph shows event studies for the number of consultations performed in primary health care facilities in four different primary care categories, following equation 4.1. Outcomes are per 100,000 residents and transformed using the inverse hyperbolic sine. Chronic conditions include asthma, diabetes, hypertension, cardiovascular diseases, cancers, COPD and mental health. Related risk factors include malnutrition, obesity, rehabilitation and the use of drugs (tobacco, alcohol and other drugs). Maternal health includes the full cycle of pre- and postpartum. Infectious diseases include dengue, leprosy, tuberculosis and STDs. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB.

Figures C.1, C.2 and C.3 show results on specific diseases and confirms the overall picture. We observe in Figures C.1 and C.2 that consultations on chronic conditions mostly experience a strong and persistent decrease. On the other hand, Figure C.3 shows that infectious diseases are generally less and only temporarily affected, even when doctors are more involved, such as for consultations on STDs.

In sum, there was a sharp decrease in primary care utilization immediately after the Cuban doctor exit. However, while primary care conditions requiring immediate care, such as infections, recovered quickly, there was a strong and persistent decrease in the management and care of chronic conditions, especially in the most affected municipalities, where PMM relied entirely on Cuban doctors.

4.5.3 Hospitalizations and Mortality

We now investigate whether the exit of Cuban doctors and the resulting turnover of doctors affected health outcomes. Figure 4.4 presents effects on total hospital admissions as well as by specific causes of admission. Figure 4.5 reports analogous results for mortality. We distinguish between total outcomes, outcomes amenable to primary care, and those specifically related to infections, infant health, and chronic conditions among the population aged 60 and above. We observe that estimates are close to zero and mostly insignificant, even for outcomes that are expected to be more sensitive to short-term changes in access to health care, such as infant hospitalizations or hospital admissions of causes amenable to primary care¹⁰ (Carrillo & Feres, 2019).

While effects on health outcomes may take a period of time to materialize (Hone et al., 2020; Özçelik et al., 2020), we observe that a sharp and non-trivial reduction in primary health care utilization does not translate into any systematic changes in health outcomes even throughout the end of the following year after the shock. We further discuss this result in Section 4.6. In particular we examine whether and how demand and local health systems responded to the shock, potentially protecting and leaving health outcomes unaffected.

¹⁰ Causes amenable to primary care are defined by the Brazilian Ministry of health. These causes include, for example, conditions which are preventable through immunization, pre-natal and birth-related conditions, and different chronic conditions such as asthma, hypertension and diabetes.

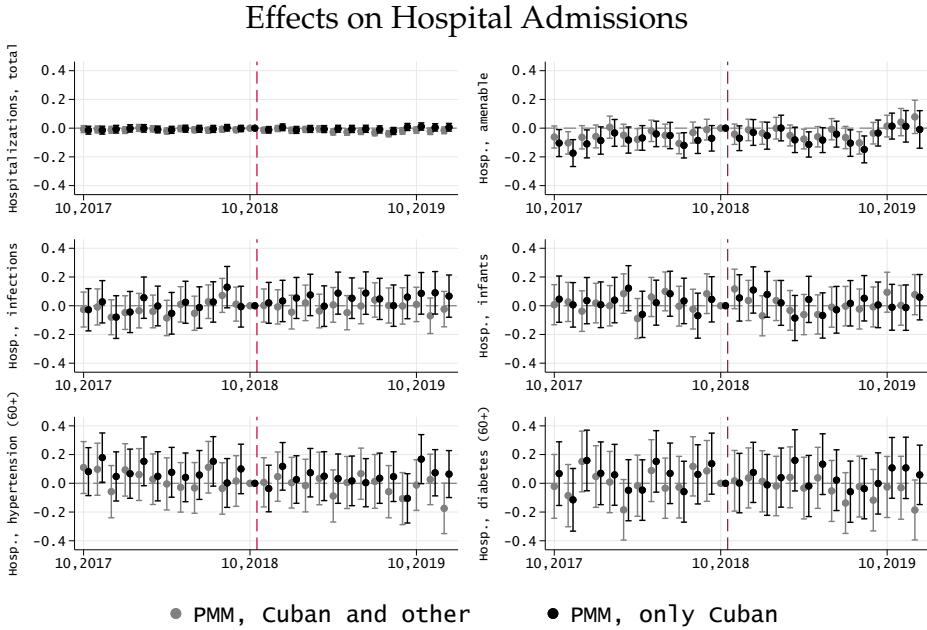


Figure 4.4. The graph shows event studies for the number of hospitalizations in six different categories, following equation 4.1. Outcomes per 100,000 residents and transformed using the inverse hyperbolic sine. Where indicated, the number of hospitalizations only includes individuals aged 60 or more. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the SIH/Datusus.

Effects on Mortality

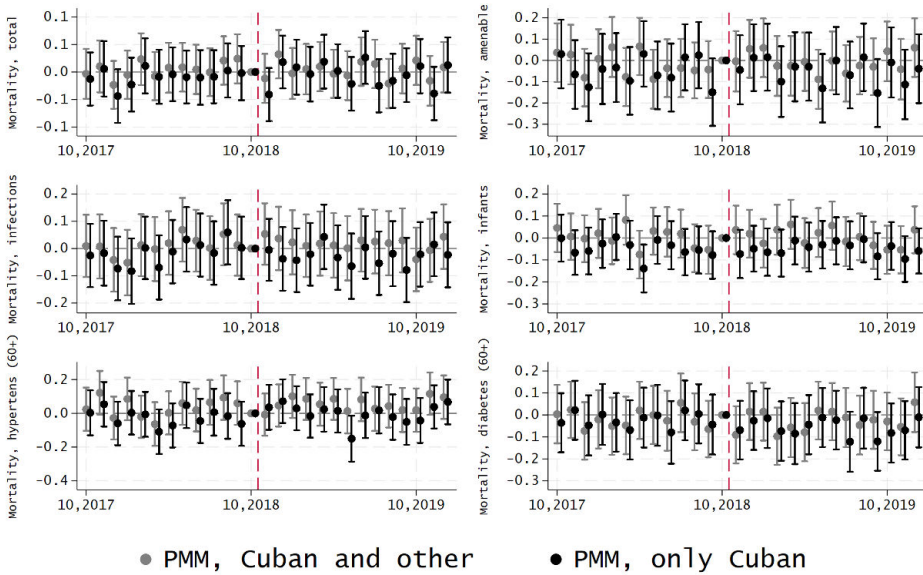


Figure 4.5. The graph shows event studies for the number of hospital mortality in six different categories, following equation 4.1. Outcomes per 100,000 residents and transformed using the inverse hyperbolic sine. Where indicated, the number of hospitalizations only includes individuals aged 60 or more. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the SIM/Datusus.

4.5.4 Alternative Specifications and Robustness Checks

In this section we further discuss the robustness and the interpretations of our main results. We present robustness checks for our main results on primary care utilization (consultations, procedures and consultations by condition type) and on health outcomes (hospitalizations and mortality). We first test the stability of our estimates to the inclusion of time-varying controls. In our main specifications, we employ municipality and state-time fixed effects. Given the high frequency data within a relatively short period of analysis, and conditional upon municipality fixed effects, we expect the observed variation in the supply of doctors to be exogenous to municipality fixed characteristics as well as to slow-moving municipality trends. In order to test for this, in Appendix Section C we replicate all our results now from specifications that include annually available socioeconomic control variables at the municipality level, such as the population share of Bolsa Familia coverage (the main social welfare program in Brazil), Bolsa Familia expenses per capita, GDP per capita, value added by public sector employment (in R\$ 1,000), the share of the population above 65, and the sex ratio (Appendix Figures C.1 to C.4). Results remain stable in comparison to our main results in Figures 4.2 to 4.5.

First, we test the stability of our estimates to the inclusion of time-varying controls. In our main specifications, we employ municipality and state-time fixed effects. Given the high frequency data within a relatively short period of analysis, and conditional upon municipality fixed effects, we expect the observed variation in the supply of doctors to be exogenous to municipality fixed characteristics as well as to slow-moving municipality trends. In order to test this, we repeat all our estimations but include various municipality characteristics as additional control variable, such as the population share of Bolsa Familia coverage (the main social welfare program in Brazil), Bolsa Familia expenses per capita, GDP per capita, the share of value added by public sector employment, the share of the population above 65, and the sex ratio. Results are available in Appendix Section C and shows that estimations on primary care services (Appendix Figure C.1) to mortality C.4) remain robust and largely unchanged.

Second, in Appendix Section C we test whether our results are robust to alternative definitions of the control group. We do not observe any substantial changes when (i) adding non-PMM municipalities to our control group (Appendix Figures

C..5 to C..8) or when (ii) excluding PMM municipalities that employed no PMM doctors in October 2018 from our control group (Appendix Figures C..9 to C..12).

Third, our main results rely on equation (1), where treatment is defined as categorical variables indicating different groups of municipalities, more *versus* less exposed to the Cuban exit. We now present results from an alternative specification, based on equation (2), where exposure is defined as the share of Cuban PMM doctors among all local primary care doctors at baseline interacted with time fixed effects. Appendix Section C replicates our main results. We observe similar patterns. Overall, pre-trends oscillate around zero up to October 2018, when we observe a sharp decline in service utilization (Appendix Figures C..13 to C..14). Again, we do not observe any systematic effects on hospital admissions and mortality (Figures C..15-C..16). In general, results remain qualitatively similar in comparison to Figures 4.2 to 4.5.

Fourth, as an alternative estimation approach to deal with potential zeros some of our outcome variables, we re-estimate our main results using a Poisson pseudo-likelihood regression instead of the inverse hyperbolic sine transformation (Chen & Roth, 2024; Correia et al., 2019). Again, results remain virtually unchanged, see Appendix Section C.

Finally, as mentioned in Section 4.3, not all primary care consultations contain information on specific conditions. As a result, Figure 4.3 plots coefficients only for a subset of all consultations that occurred during our sample period. To ensure that these results are not driven by outliers with more/less information on specific conditions, we re-estimate effects on consultation categories, each time omitting one municipality from the estimation. For simplicity, we re-code the categorical treatment variable into a dummy variable that takes the value of 1 if a municipality has been affected by the Cuban exit and zero otherwise. Appendix Figure C..21 plots the differences between the estimated coefficients from the leave-one-out analysis and the coefficients from the full model for all 4,163 iterations. The closer to the horizontal line at $y = 0$, the less do the point estimates from the leave-one-out analysis diverge from the point estimates in the full model. Gradually omitting municipalities from the analysis leaves all estimated coefficients from Figure 4.3 virtually unchanged. The largest deviation between coefficients from the full model and coefficients from the leave-one-out analysis is smaller than 0.01.

4.6 Health System Inputs and Mechanisms

4.6.1 What Changed within Primary Care Services?

Doctors' Profile, Workload and Practices

We start by investigating the profile of doctors that replaced the Cubans.¹¹ We first investigate how affected municipalities managed to attract new primary healthcare doctors and to fill vacancies in PMM. Qualitative evidence suggests that the newly recruited doctors, both PMM and non-PMM, had often little work experience and stayed only for a short time in their new jobs (Poder360, 2019). In contrast, the vast majority of Cuban doctors were experienced general practitioners, specialized in primary care and having accumulated at least 5 years of experience. Furthermore, the Cubans were perceived as being more accessible than their Brazilian PMM colleagues, they had a good relationship with their patients and the local community, and typically stayed in the same municipality for several years (Comes et al., 2016b; Rech et al., 2018). These factors likely contributed to accumulating specific knowledge and facilitated ongoing care and a close patient-doctor relationship (Sobrinho, 2018). Descriptive statistics confirm this picture. Appendix Figure C.1 shows the trend in the share of doctors with no prior work experience, i.e., it captures how many of the active doctors started working only in that month. In the upper panel, we see that the share of PMM novices increased to 15% in treatment group 1 and 41% in treatment group 2. This shows that PMM vacancies were largely filled by doctors without any prior work experience. Trends in the experience profile of non-PMM primary health care doctors paint a similar picture for treatment group 2, in which the share of inexperienced non-PMM doctors peaked at 21% in December 2018. Replacing experienced with inexperienced primary care doctors might partly explain the persistent decrease in health care usage.

Next, we examine the participation and tasks performed by healthcare workers other than doctors in public primary care centers. During the turnover period, when vacancies had not been filled yet, non-doctors, such as nurses or community

¹¹ Doctors' profiles and related practice styles can impact health care utilization and health outcomes for multiple reasons. First, doctors' individual and shared work experience is important for the quality of services that they deliver (Chen, 2021). Second, health care professionals' performance and practice styles have a strong influence on their patients' health care utilization (Fadlon & Van Parys, 2020; Simonsen et al., 2021). And third, it implies a loss of team and unit-specific human capital built up over various years (Bartel et al., 2014).

health workers, might have taken over doctoral duties. Carrillo and Feres (2019) show that the introduction of PMM led to doctors taking over tasks from nurses, resulting in null-effects on infant health outcomes. The opposite might have occurred as PMM doctors left the program. Our data allow us to examine the extensive and intensive margins of adjustment of the labor supply of these professionals, and to identify which task was carried out by which health care professional. Overall, we do not find evidence for a substitution of doctors by non-doctors. Appendix Figure C.2 shows that there was neither meaningful increases in the number of non-doctor primary healthcare workers in public primary healthcare facilities (upper panel), nor in the average weekly hours worked by non-doctors (lower panel). Further, we do not observe changes in the number of consultations, procedures, and home visits carried out by non-doctors (Appendix Figure C.3). The results therefore do not indicate a substitution effect or an increased workload for these workers. If anything, the exit of Cuban doctors might have led to a complementary decrease in procedures performed by non-doctors during the turnover period.

In sum, we do not observe an improvement in the experience profile of the new doctors that replaced the Cuban professionals. We neither observe an increase in the supply of other health care professionals, nor task substitution across different healthcare professionals. In that sense, while some of these factors may help explain changes in service utilization, the replacement of professionals of different quality, the reallocation of practices among professionals, and changes in the supply of other health professionals are unlikely explanations for the lack of turnover effects on health outcomes.

Health Service Delivery

We now assess the profile of the services delivered at primary care centers by examining how the turnover affected different types of consultations and access to medication. First, SISAB data broadly distinguishes between two types of consultation: scheduled and spontaneous ones. Scheduled consultations are further subdivided into one-time consultations and consultations which are part of ongoing care. Spontaneous consultations are divided into walk-ins and urgent care. We estimate effects for each type of consultation.

Appendix Figure C.4 shows the results. Urgent care remains completely un-

affected by the turnover, even though it is the consultation type with the highest doctoral share (70% in October 2018). However, urgent care requires prioritization by the health team even when severely understaffed and cannot be postponed. For spontaneous walk-ins and scheduled one-time care, we see a strong short-term drop of around 10.5% and 10% (around 470 and 430 per 100,000) in treatment group 1, and 18% and 21.5% (around 1,390 and 1,130 per 100,000) in treatment group 2 during the turnover period, respectively, but both types of treatment mostly stabilize back to base levels as soon as missing doctors are replaced. For consultations focused on health conditions requiring scheduled ongoing care, we observe a decrease that seems to persist until the end of the period of analysis in the most affected municipalities, even though point estimates are not always significant. Initial decreases are 10% (around 230 consultations per 100,000) in treatment group 1 and 17% (around 470 per 100,000) in treatment group 2, and stabilize at around 5% (around 110 per 100,000) and 11% (around 300 per 100,000) fewer scheduled ongoing consultations throughout 2019. These results suggest a restructuring of service delivery towards a prioritization of urgent and curative care over ongoing care for chronic diseases. This is consistent with evidence indicating that the latter can be delayed without immediate adverse health effects (Shurtz et al., 2022).

If primary care centers accommodate the turnover shock by de-prioritising ongoing care and care for chronic conditions, how does this affect patients that require related essential services, such as access to prescription drugs? Many essential prescription drugs in Brazil can be purchased at highly subsidized prices (Américo & Rocha, 2020). As access to subsidized drugs in Brazil is conditional on a medical prescription, the decrease in health care utilization for chronic diseases might translate into a decrease in related medication utilization. When examining trends in medication for diabetes and hypertension, we see a short but pronounced decrease in medication purchased. However, this is limited to shortly after the exit of the Cuban PMM doctors. In December 2018, the value of medication sold for hypertension and for diabetes (Appendix Figure C.5) decreased both by roughly 7% (around R\$4,860 and R\$2,130, respectively) in treatment group 2. The reduction in medication sales quickly disappears and does not persist beyond the turnover period. Plausibly, patients in need of prescription medication for chronic diseases were therefore able to secure access to medications despite the reductions in pri-

mary care consultations. While patients are supposed to renew their prescription every month, doctors can write prescriptions covering up to 180 days (Américo & Rocha, 2020). Patients and/or doctors may have responded to the shock by guaranteeing access to medication even in a setting where access to consultations was restricted. Such adaptation would be in line with a prioritization of services concerning the most immediate needs, thus consistent with a pronounced but only short-term drop in drug sales.

In sum, the results suggest the possibility of demand adaptation and of restructuring of health service delivery towards urgent and curative needs as well as other protective services, buffering patients against more extreme adverse outcomes such as hospitalizations and deaths.

4.6.2 What Changed Beyond Primary Care Services?

We now examine whether the turnover led by the Cuban exit had impacts beyond the changes in the profile of professionals and services delivered at primary care centers. We first assess impacts on service use from other health care providers. We then examine private insurance markets and spillover effects on neighboring municipalities.

Other Health Care Services and Providers

With a reduction in the availability of primary care consultations, patients might seek for care in other sources of health services. The existing studies on small-scale turnover events provide evidence that doctor turnover can lead to patients substituting primary care by directly going to specialists, hospitals or emergency rooms (Agha et al., 2019; Sabety et al., 2021). Patients could also opt to increasingly use private health care by resorting to out-of-pocket spending and the purchase of private insurance plans (Silva et al., 2022). To investigate whether patients bypass the primary care sector to seek ambulatory care at other facilities, we turn to data on ambulatory care visits beyond primary care centers, distinguishing between specialist, hospital, and emergency room (ER) visits, as well as by complexity of treatment (basic, medium, high). Primary care needs mostly fall under the basic complexity category.

While we do not find significant changes for the use of hospitals and special-

ists for basic procedures (see Appendix Figure C.6), we do find a significant increase for such procedures performed at ERs. Figure 4.6 shows that the number of basic complexity treatments performed in ERs gradually increases following the turnover. The upper panel shows the number of basic complexity procedures at ERs, and the lower panel indicates how many ER facilities reported having supplied basic complexity treatments. By the end of 2019, ERs reported around 18% more treatments of basic complexity in both treatment groups, while treatments of higher complexity remain unaffected. Parallel to this, we also find that more ER facilities start delivering basic ambulatory care services (around 2% in treatment group 1 and 3.5%-4% in treatment group 2 from April 2019 onward). Patients therefore seem to partly substitute treatment at primary care centers with treatment at ER facilities, while municipalities start providing more low-complexity services in these facilities. It is important to note that the diversion persists even after the turnover ends. Although ERs are very accessible to the general population and cannot deny services to patients, they are explicitly not set up to take care of ongoing care or deferrable treatments (Carret et al., 2009). These upward trends in the use of ER facilities thus suggest increased fragmentation and misallocation of resources within municipalities, with preventive care being reduced and basic services being diverted towards facilities that should handle more urgent cases and emergencies.¹²

Private Insurance and Private Services

Patients might substitute health care delivered by the public sector with private health care. Yet, we do not observe any changes in private insurance coverage in affected municipalities (see Figure Appendix C.7). While we are not able to measure out-of-pocket expenditure, we conjecture that a similar pattern would emerge. The PMM's target population are of low socioeconomic status. Given that SUS provides access to services for free at the point of use, low-SES patients have long resorted to public services when seeking care. In that sense, we expect patients to mainly bypass the primary care services in order to seek care at other public facilities. Offsetting effects through private care utilization are therefore unlikely.

¹² A substitution of primary care by emergency care following disruptions in primary care services is also documented for the US context —see, for example, Sabety (2023) and Zhang (2022).

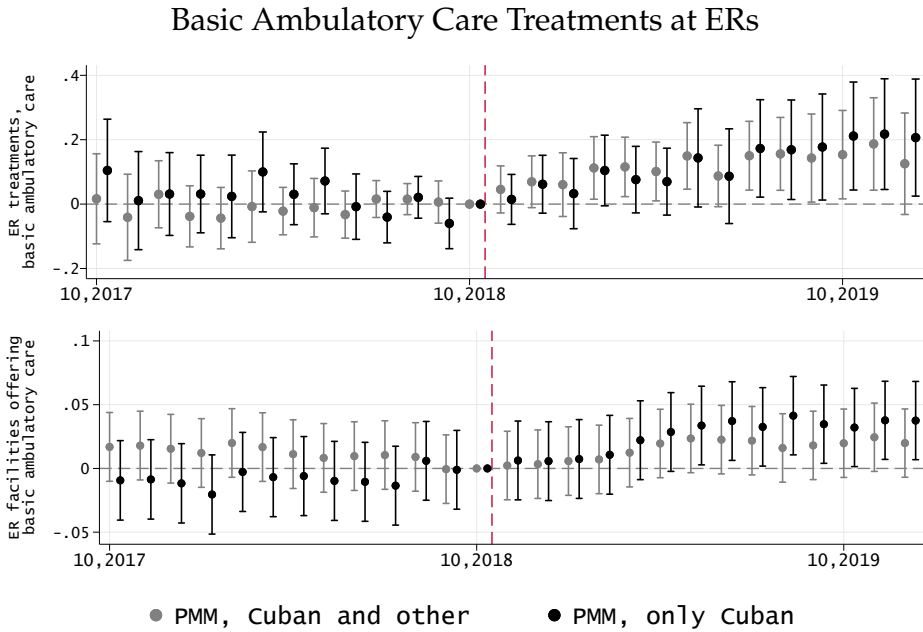


Figure 4.6. The graph shows event studies for the number of emergency room treatments of basic ambulatory care (upper panel) and the number of ER facilities reported to have offered basic ambulatory care treatments (lower panel), following equation 4.1. Outcomes per 100,000 residents and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health, SIA/Datasus and CNES.

Wages and Public Spending

The exit of the Cubans decreased the supply of primary care doctors in the Brazilian health care market, potentially affecting salaries due to increased demand. We investigate potential market adaptations by examining non-PMM doctor salaries with data from RAIS.

Figure 4.7 shows event studies for the total municipality-level expenses for public primary care doctors' salaries in the upper panel, and remuneration by doctor in the lower panel for the two treatment groups.¹³

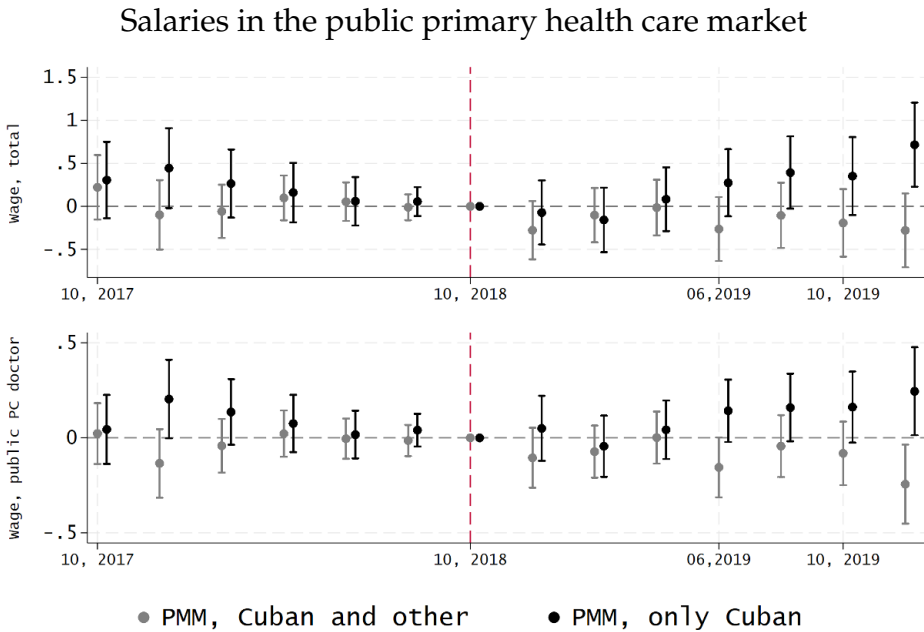


Figure 4.7. The graph shows bi-monthly event studies for the total amount of wages paid out to doctors working in public primary health care per 100,000 residents (upper panel) and the average wage received per doctors (lower panel) on the municipality level. Estimation results obtained by regressions following equation 4.1 with the outcomes transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from RAIS.

While we do not see diverging trends for the first treatment group (which on average consists of large, densely populated municipalities), we see a gradual in-

¹³ We are using bi-monthly data to drown out outliers and noise prevalent at the monthly level of the RAIS data.

crease in the wages paid out to doctors in the second treatment group. This treatment group mostly consists of small, rural municipalities, for which substitution of doctors is difficult. Consequently, we see a strong increase in the total remuneration of the doctors, starting on a statistically significant scale in the second half of 2019, increasing by 71.5% in the last two months of that year. Wages per doctor also increase, with the increase reaching 24.4% for the months of November and December 2019. These increases are not driven by a change in the number of doctors or hours worked in the dataset, but solely from an increase in the remuneration. These results underscore that in resource-limited environments, such as the municipalities in treatment group 2, doctor exit and turnover will require profound market adaptation to maintain health care service provision as best as possible while limiting the adverse population health consequences. Nevertheless, coefficients are often imprecisely estimated and RAIS does not cover the wages of PMM doctors.

Examining potential spillovers, Appendix Figure C.8 contrasts aggregate trends in the salaries paid to all primary care doctors (left side) and non-primary doctors (right side). Appendix Figure C.9 contrasts wages paid out in the public health care sector vs the ones in the private sector. Both figures confirm that it is primarily the public primary sector which is affected by the Cuban exit, as wages in non-primary care and in privately provided care remain unaffected.

Geographical Spillovers

Finally, patients from affected municipalities might seek care in neighboring municipalities that have been unaffected or less affected by the turnover. This type of spillover is nevertheless unlikely in our setting. Primary health care units are explicitly set up to cover a specific catchment area and its resident population. Still, spillovers might be active to some extent.

We test for spillover effects between neighboring municipalities by accounting for neighbors' average treatment status. For each municipality i , we calculate the average share of Cuban PMM doctors among all primary care doctors of i 's neighboring municipalities in October 2018.¹⁴ We then follow equation 4.2, where we include both the interactions of municipality i 's baseline share of Cuban doctors with

¹⁴ To calculate municipality i 's neighbors' average treatment status, we use a row-standardized contiguity matrix. We use all municipalities (PMM and non-PMM) to calculate neighbors' average treatment. For the estimation, we only use PMM participating municipalities.

time dummies, and the interaction of neighbors' average baseline share of Cuban doctors with time dummies. If patients from affected municipalities seek primary care in neighboring municipalities, we would expect a positive relationship between the average treatment intensity of municipality i 's neighbors and municipality i 's primary care consultations or procedures after the Cuban exit. Appendix Figure C.10 plots the coefficients from the interaction between neighbors' average share of Cuban doctors and the time dummies, for consultations per 100,000 residents (upper plot) and procedures per 100,000 residents (lower plot) as outcome variables. We do not find any evidence of spillover effects.

4.7 Final Comments

In this paper we investigated the unique case of a large-scale turnover of doctors in Brazil, where over 8,000 doctors from more than 2,800 municipalities left the health system. We found a significant and meaningful decrease in health care utilization, but no effect on hospitalizations and mortality during our sample period. These results are robust to multiple checks and sensitivity analyses.

The results suggest that patients and local health care systems were able to adapt within a few months, and generally proved flexible when under distress. Adaptation nevertheless came at the cost of de-prioritizing ongoing care and care for chronic conditions. This may be an issue over the longer-run as chronic conditions require prevention, early detection and ongoing care. Existing challenges, such as under-diagnosis and insufficient treatment of chronic conditions, may be strongly exacerbated by doctor turnover as it implies a loss of location- and position-specific knowledge (Manne-Goehler et al., 2019; Mauer et al., 2022). We also identified an increase in the use of ER facilities for low-complexity treatments. Besides being inefficient, seeking treatment for non-urgent conditions at ERs does not promote the rebuilding of long-term patient-doctor relationships and concomitant patient education, two factors highly desired within health care (Carret et al., 2009; Gill, 1999). Finally, we found suggestive evidence for an increase in municipality-level spending through increases in the wages of public primary care doctors' salaries.

Appendix A

Appendix of Chapter Two

A: Descriptive Statistics

A.1 Outcome Variables

Table [A..1](#) shows summary statistics for the outcome variables for the analysis of the EP elections in 2009.

Table A..1. Descriptive Statistics, EP Election 2009

Variable	N	Mean	SD	Min	Max
Turnout	36194	46.33	8.46	16.68	100
Blank votes share	36194	6.11	3.78	0	62.87
Trad. mainstream parties vote share	36194	43.58	8.82	0	100
Extreme parties vote share	36194	15.25	7.89	0	74.29
PS vote share	36194	15.50	7.17	0	88.24
UMP vote share	36194	28.09	9.31	0	90.48
Far Left vote share	36194	7.86	5.94	0	74.29
FN vote share	36194	7.39	5.38	0	64.71

As explained, we pay special attention to EP elections. Plotting their turnout rates and coarsely distinguishing municipalities in terms of their No-vote provides preliminary motivation to investigate this relationship more deeply. While turnout rates were falling in all French municipalities, there was a reversal for the municipalities that accepted the TCE for the elections in 2009, as seen in Figure [A..1](#). This

stands in contrast to the remaining municipalities, especially to the ones that rejected the TCE by a larger margin.

The “Far Left” is defined as Lutte ouvriere (LO), Ligue communiste révolutionnaire (LCR) and Parti communiste français (PCF) in the elections in 2007, and as LO, Nouveau Parti anticapitaliste (NPA, formerly LCR) and Liste Front de Gauche (PCF, Parti de Gauche and Gauche unitaire) in all remaining elections.

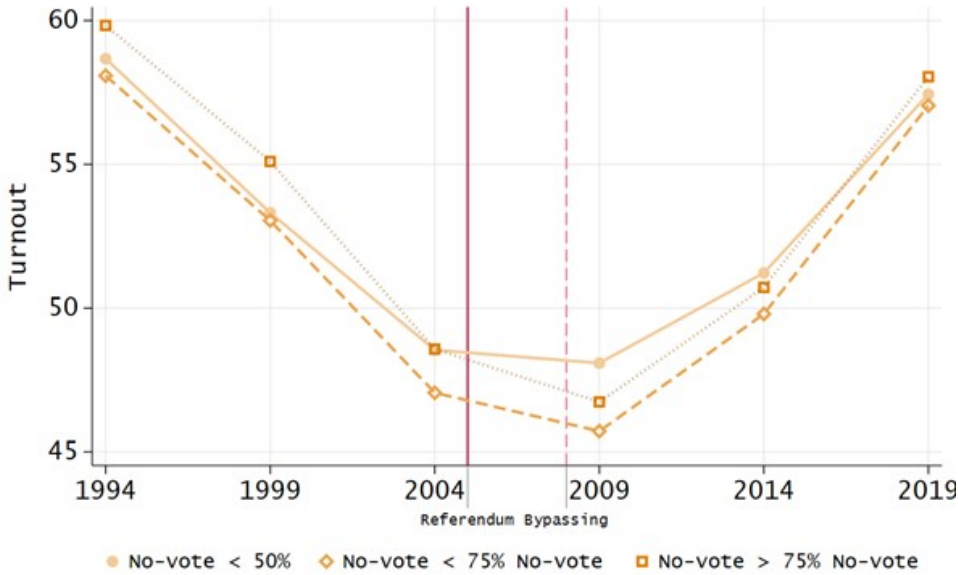


Figure A.1. Turnout in EP Elections France 1994 – 2019. Notes: The vertical axis represents percentage points; the horizontal axis depicts election years. Municipalities are allocated to coarse groups based on their No-vote share in the referendum on the TCE in 2005.

A.2 Explanatory Variables

In order to mitigate the threat of omitted variable bias, we employ a large array of control variables which jointly influence our key explanatory variable and our political outcome variables.

Table A..2. Descriptive Statistics for Covariates

Variable	N	Mean	SD	Min	Max
Share of No-votes Referendum 2005	36191	58.95	10.15	0	100
Turnout Referendum 2005	36191	74.97	5.8	32.25	100
Eligible voting population	36202	1172	8233	8	1217372
Share of population aged 60+	36202	24.75	8.05	0	91.67
Share of buildings laying vacant	36202	7.1	4.12	0	53.33
Avg household size	36202	2.42	0.27	1	7
Share of population low education	36202	31.66	8.86	0	100
Share of population high education	36202	18.85	7.64	0	100
Unemployment	36202	7.9	3.55	0	40
Share of labour force (lf) agriculture	36202	7.14	10.44	0	100
Share of lf blue-collar jobs	36202	27.96	12.07	0	100
Share of lf education & science	36202	9.06	7.78	0	100
Share of lf self-employed & small business owners	36202	7.02	6.2	0	100
Share of immigrant population	36202	4.49	4.54	0	67.47
Median income	33135	17986.59	3217.40	7884	53591
Density category: Very sparsely populated	12487	15	12	0.65	450
Density category: Sparsely populated	19573	57	60	0.83	1300
Density category: High density	3378	303	296	24	3154
Density category: Very high density	776	1888	2030	102	16770

B: Justification for Used Methods

We argue that cross-sectional analysis is the most adequate estimation technique to explore how the French “No” affected the country’s political outcomes. We motivate this by the following reasons:

First, we acknowledge that our key explanatory variable, the share of the No-votes in the French TCE referendum in 2005, is not exogenous; as its intensity can stem from municipality-specific (unobserved) characteristics that could also influence election outcome variables (for example the local infrastructure, popularity of local politicians, and traditional voting patterns). Municipalities self-select into their “treatment” intensity (i.e. the fraction of No-votes in 2005) and precursors of the No-vote might be discernible in outcomes of pre-treatment periods. Moreover, there is no “threshold” which credibly divides a treated from an untreated group, since the referendum vote was wholly inconsequential for municipalities on a practical level. Thus, causal inferential estimation techniques requiring strict exogeneity, such as the difference-in-difference technique or regression discontinuity design, are not suitable for our research. Furthermore, since our key explanatory variable is a one-time observation (the referendum vote in 2005), within-variation in the key variable cannot be exploited and we also cannot employ proper panel data techniques.

For these reasons, we deem investigating elections independently and focusing on differences between all communes to be the most appropriate approach in our context. A different challenge for the assumptions of standard linear regression arises from the bounded nature of proportional dependent variables. Electoral outcomes are inherently bounded between 0 and 100 percent, and if strongly skewed towards one of the bounds it could lead to non-normality and heteroskedasticity in the error term.

C: Main Results

This section shows further regression results with respect to our key explanatory variable, the municipality No-vote in the referendum 2005 on different election outcomes.

C.1 Turnout and Blank Votes

Table A..3. Linear Regression Estimation Results: Impact of No-vote Share in 2005 on Turnout and Blank Voting in National Elections in 2007-2014

	Effect of No-vote on Turnout	Effect of No-vote on Blank Vote
Leg., 2007	-0.446*** (0.0513)	0.0962*** (0.0206)
OT delta	3.61	1.37
Leg., 2012	-0.079 (0.0599)	0.0093 (0.0150)
OT delta	0.29	0.60
Pres., 2007	-0.248*** (0.0284)	0.0640*** (0.0186)
OT delta	1.23	1.76
Pres., 2012	0.0021 (0.0363)	-0.0065 (0.0128)
OT delta	-0.20	0.48
EP., 2009	-0.614*** (0.0609)	0.261*** (0.0447)
OT delta	1.72	1.60
EP, 2014	-0.254*** (0.0682)	0.188*** (0.0282)
OT delta	1.12	1.66
N	33,047	

Notes: Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the population's immigration share, and (13) the unemployment rate. Median income (14) is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4. No-vote Share, Pre-Referendum Elections

	EP, 1999	EP, 2004	Legislative, 2002	Presidential, 2002
Turnout	0.109 (0.103)	-0.063 (0.0728)	-0.398*** (0.0745)	-0.003 (0.0573)
adj. R^2	0.558	0.574	0.525	0.440
OT delta	0.14	0.34	0.49	-0.01
Blank Votes	0.093 (0.0574)	0.101** (0.0330)	0.153*** (0.0213)	0.0431 (0.0312)
adj. R^2	0.239	0.195	0.085	0.150
OT delta	0.26	0.41	1.21	0.21
N	33,047			

Notes: Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of buildings that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the unemployment rate, and (13) median income. The share of immigrant population is not available for elections prior to 2007. All continuous explanatory variables are standardized. Also included in all regressions are Nuts2 fixed effects. Robust standard errors clustered over 95 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.2 Party Vote Shares

Table A..5. Effect of No-vote on Party Vote Shares, EP Election 2009

	Mainstream Parties	Anti-system Parties	Far Left	FN
No-vote 2005	-3.23*** (0.12)	1.90*** (0.08)	1.01*** (0.06)	0.386*** (0.05)
Election Controls	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes
Median Income	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
N	33047	33047	33047	33047
Adj. R^2	0.37	0.61	0.37	0.62
OT delta	1.21	2.54	0.65	0.41

Notes: The two traditional parties are the Parti socialiste (PS) and the Union pour un mouvement Populaire (UMP) (column 1). As anti-system parties (column 2) we count the far Right Front National (FN) (column 3), and the far Left parties Nouveau Parti anticapitaliste (NPA), Lutte ouvriere (LO), as well as the far Left coalition Front de gauche (LFG) consisting of the Parti communiste français (PCF), the Parti de gauche (PG), and the Gauche unitaire (GU) (column 4). Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the population's immigration share, and (13) the unemployment rate. Median income (14) is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table A..6. No-vote Share and Party Vote Shares, EP Election 2014

	Mainstream Parties	Anti-system Parties	Far Left	FN
No-votes	-2.823*** (0.156)	3.004*** (0.112)	0.871*** (0.053)	1.733*** (0.128)
Election Controls	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes
Median Income	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
N		33,047		
adj. R^2	0.367	0.638	0.59	0.6
OT delta	0.77	2.13	0.68	0.85

Notes: The two traditional parties are the Parti socialiste (PS) and the Union pour un mouvement Populaire (UMP) (column 1). As anti-system parties (column 2) we count the far Right Front National (FN) (column 3), and the far Left parties Nouveau Parti anticapitaliste (NPA), Lutte ouvrière (LO), as well as the far Left coalition Front de gauche (LFG) consisting of the Parti communiste français (PCF), the Parti de gauche (PG), and the Gauche unitaire (GU) (column 4). Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the population's immigration share, and (13) the unemployment rate. Median income (14) is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A..7. No-vote Shares and Party Vote Shares, Legislative Elections 2007 and 2012

	Mainstr. Parties		Anti-syst. Parties		Far Left		FN	
	2007	2012	2007	2012	2007	2012	2007	2012
No-vote 2005	-0.592* (0.30)	-0.771** (0.32)	0.758*** (0.08)	1.284*** (0.10)	0.582*** (0.07)	0.792*** (0.08)	0.159*** (0.02)	0.418*** (0.05)
N	33,058		33,058		33,058		33,058	
adj. R ²	0.37	0.26	0.50	0.46	0.56	0.44	0.452	0.498
OT delta	0.77	1.01	1.02	1.60	0.88	1.35	0.72	0.84

Notes: The two traditional parties are the Parti socialiste (PS) and the Union pour un mouvement Populaire (UMP) (column 1). As anti-system parties (column 2) we count the far Right Front National (FN) (column 3), and the far Left parties Nouveau Parti anticapitaliste (NPA), Lutte ouvriere (LO), as well as the far Left coalition Front de gauche (LFG) consisting of the Parti communiste français (PCF), the Parti de gauche (PG), and the Gauche unitaire (GU) (column 4). Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the population's immigration share, and (13) the unemployment rate. Median income (14) is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table A..8. No-vote Shares and Party Vote Shares, Presidential Elections 2007 and 2012

	Mainstr. Parties		Anti-syst. Parties		Far Left		FN	
	2007	2012	2007	2012	2007	2012	2007	2012
No-vote 2005	-1.140*** (0.14)	-1.373*** (0.10)	1.832*** (0.07)	2.149*** (0.07)	0.741*** (0.04)	0.719*** (0.05)	0.804*** (0.05)	0.949*** (0.07)
N	33,037		33,037		33,037		33,037	
adj. R ²	0.40	0.43	0.62	0.57	0.50	0.50	0.63	0.61
OT delta	0.81	0.83	1.05	1.11	0.75	0.87	1.09	1.00

Notes: The two traditional parties are the Parti socialiste (PS) and the Union pour un mouvement Populaire (UMP) (column 1). As anti-system parties (column 2) we count the far Right Front National (FN) (column 3), and the far Left parties Nouveau Parti anticapitaliste (NPA), Lutte ouvrière (LO), as well as the far Left coalition Front de gauche (LFG) consisting of the Parti communiste français (PCF), the Parti de gauche (PG), and the Gauche unitaire (GU) (column 4). Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the population's immigration share, and (13) the unemployment rate. Median income (14) is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A..9. No-vote Share on Party Vote Share, Liste Extreme Gauche (LO + RCL/NPA)

	Far Left
EP 2009	0.460*** (0.051)
EP 2014	0.094*** (0.0111)
Leg., 2007	0.367*** (0.0302)
Leg., 2012	0.09*** (0.0113)
Pres., 2007	0.519*** (0.0239)
Pres., 2012	0.0521 (0.0135)
N	33,037

Notes: This table presents results for a narrower definition of the French Far left, namely only the coalition between Lutte ouvriere (LO) and Ligue communiste révolutionnaire (LCR - until 2009)/ Nouveau Parti anticapitaliste (NPA – from 2009 on). While ideologically strongly overlapping with the other far Left parties, the Liste Extreme Gauche parties explicitly rule out any coalition with the traditional Parti Socialiste and could therefore be regarded as more consequentially illiberal. Results show that significance of the estimated positive coefficients for the far Left does not depend on any specific denomination within it.

Election controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base (2) the population density in four categories (from very densely to very sparsely populated) (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen and small business owners), (12) the population's immigration share, and (13) the unemployment rate. Median income (14) is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

D: Beta Regression

We check our results with a GLM framework employing a Maximum Likelihood estimation and pre-specifying a beta distribution. This approach aims to tackle issues inherent to dependent variables (DV) that are proportions (i.e. strictly bounded between 0 and 100 percent). This “beta regression” is thus, in theory, a more adequate estimation technique in our electoral outcome setting. If not indicated otherwise, the following delineation of the approach relies on the works of Paolino (2001) and Smithson and Verkuilen (2006), sources that the reader should consult for a more in-depth explanation. The following tables show that GLM confirms most of our standard linear regression results. If anything, standard linear regression underestimates the effect of the No-vote on the predictions on electoral participation. Party vote shares are very similar as well, with the exception of the UMP vote share in the legislative election 2007. Wald test χ^2 values (omitted in the tables) show the joint significance of our independent variables in all model specifications.

Table A..10. Beta Regression Results: No-vote Share on Turnout and Blank Voting

	Effect of No-vote on Turnout	Effect of No-vote on Blank Vote
EP 2009	-0.614*** (0.059)	0.253*** (0.040)
EP 2014	-0.253*** (0.069)	0.157*** (0.0223)
Leg., 2007	-0.54*** (0.056)	0.074*** (0.0137)
Leg., 2012	-0.131* (0.0676)	0.00233 (0.0098)
Pres., 2007	-0.297*** (0.0333)	0.0523*** (0.0077)
Pres., 2012	-0.0377 (0.0388)	-0.0141 (0.0115)
N	33,042	33,037

Notes: Election controls in all model specifications include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the share of the population 60 years and older, (2) the share of buildings that lay vacant, (3) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (4) the share of the population with a university degree, the share of workers engaged in (5) blue-collar labor, (6) agriculture, (7) education and science, and (8) artisans (craftsmen, tradesmen, and small business owners) (9) the unemployment rate, (10) the average household size, and (11) the size of the eligible voter base. Median income and population density categories are listed separately as it excludes municipalities with less than 50 households from the sample. All continuous explanatory variables are standardized. Also included in all regressions are fixed effects for 21 NUTS2 regions. Robust standard errors clustered over 95 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A..11. Beta Regression Results: No-vote Share on Party Vote Shares

	Leg. 2007	Leg. 2012	Pres. 2007	Pres. 2012	EP 2009	EP 2014
PS	0.685*** (0.2066)	0.542* (0.3173)	1.312*** (0.0581)	1.151*** (0.0695)	0.315*** (0.068)	-0.2154*** (0.0520)
UMP	-0.298 (0.5017)	-1.045*** (0.406)	-2.16*** (0.1423)	-2.40*** (0.112)	-2.943*** (-0.154)	-2.267*** (0.1843)
Far Left	0.319*** (0.0269)	0.079*** (0.0109)	0.508*** (0.0244)	0.038 (0.147)	0.316*** (0.034)	0.0712*** (0.0076)
FN	0.175*** (0.0228)	0.474*** (0.0673)	0.822*** (0.0517)	0.956*** (0.0814)	0.382*** (0.0395)	1.772*** (0.1323)
N	33,058	33,058	33,037	33,037	33,047	33,047

Notes: Election controls in all model specifications include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the share of the population 60 years and older, (2) the share of buildings that lay vacant, (3) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (4) the share of the population with a university degree, the share of workers engaged in (5) blue-collar labor, (6) agriculture, (7) education and science, and (8) artisans (craftsmen, tradesmen, and small business owners) (9) the unemployment rate, (10) the average household size, and (11) the size of the eligible voter base. Median income and population density categories are listed separately as it excludes municipalities with less than 50 households from the sample. All continuous explanatory variables are standardized. Also included in all regressions are fixed effects for 21 NUTS2 regions. Robust standard errors clustered over 95 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E: Supplementary Analysis: Corsica and Overseas Territories

We excluded Corsica and French overseas departments from our main analysis as elections in both regions tend to be marked by idiosyncratic issues. For overseas departments, there is mostly only aggregate, i.e., departmental election and referendum results available. We are thus able to add 472 additional units in this supplementary analysis—360 from Corsica and 112 from the overseas departments. Results largely stay the same, as seen in Figures A.2 and A.1.

Figure A.1. Association Between Municipal No-vote and Turnout in National French Elections 2007-2014.

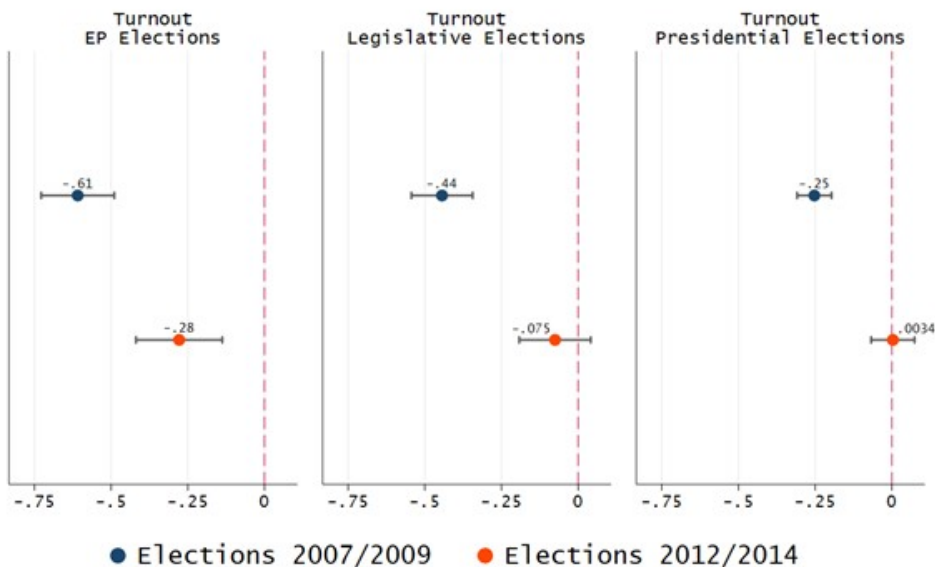
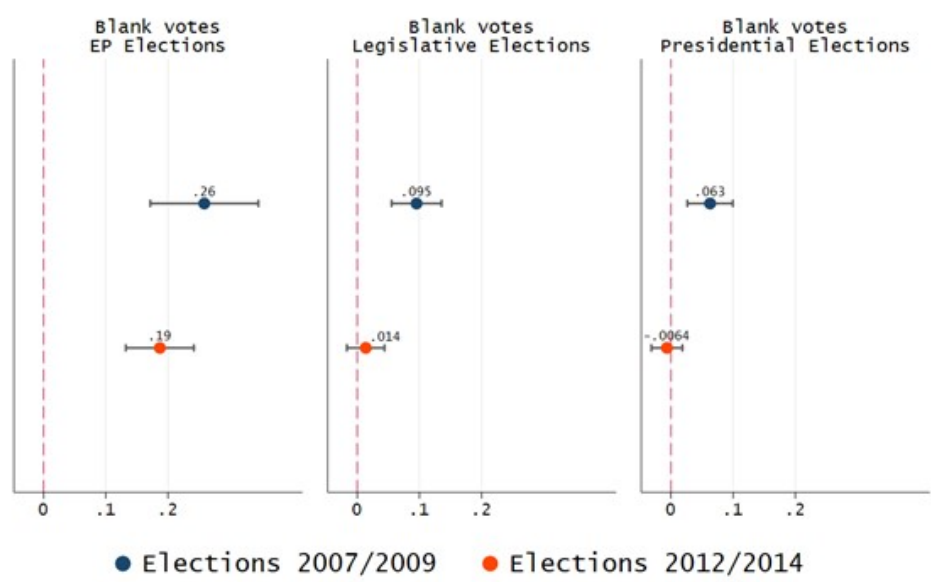


Figure A.2. Notes: Results are from the fully specified model with all covariates included (as in column (3) and (6) in Table 1). Dots represent the estimated coefficients; whiskers show the 95% confidence interval around them.

F: Supplementary Analysis: EP Elections 2019

Table A.1 shows the coefficients of the municipal No-vote in the referendum 2005 and EP election turnout and blank voting in 2019. Estimation results turn insignificant.

Figure A..3. Association Between Municipal No-vote and Blank Voting in National French Elections 2007-2014.



Notes: Results are from the fully specified model with all covariates included (as in column (3) and (6) in Table 1). Dots represent the estimated coefficients; whiskers show the 95% confidence interval around them.

Table A..1. No-vote Share and Political Participation, EP Election 2019

	Turnout	Blank Votes
No-votes	0.0896 (0.059)	0.0441 (0.0427)
Political Controls	Yes	Yes
Municipality Controls	Yes	Yes
Median Income	Yes	Yes
Region FE	Yes	Yes
N	33,047	31,507
Adj. R^2	0.487	0.257
OT delta	-0.84	0.22

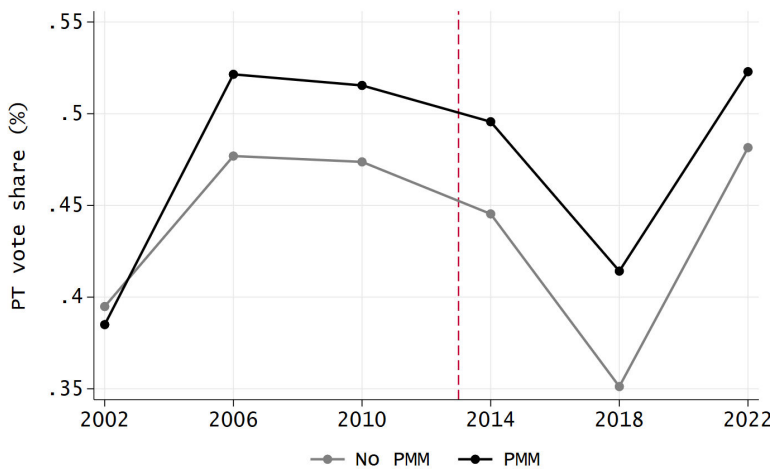
Notes: Political controls include turnout in the previous EP election and turnout for the referendum 2005. Municipality controls contain (1) the size of the eligible voter base, (2) the population density in four categories (from very densely to very sparsely populated), (3) the share of the population 60 years and older, (4) the share of residences that lay vacant, (5) the average household size, (6) the share of relatively lowly educated graduates (highest degree vocational studies or aptitude certificate), (7) the share of the population with a university degree, the share of workers engaged in (8) blue-collar labor, (9) agriculture, (10) education and science, and (11) artisans (such as craftsmen, tradesmen, and small business owners), (12) the immigrant population share, and (13) the unemployment rate. Median income is listed separately as it excludes municipalities with less than 50 households from the sample. All independent variables are standardized. Also included in all regressions are regional fixed effects. Robust standard errors clustered over 94 departments are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Appendix of Chapter Three

Context

Figure B..1. PT vote share , 2002-2022



The graph shows the aggregated average vote share received by the PT presidential candidate in non-PMM and PMM municipalities in Brazilian federal elections from 2002 - 2022. Original data from the TSE.

Conceptual Framework

Description	N	PT vote share, 2002-2010	Share which gained PMM access in 2014
PAB III and IV	56	48.3%	-
PAB II	248	47.3%	-
PAB I	518	51.7%	-
Vulnerable areas	89	58.5%	-

Table B..1. Sub-classification of the never-PMM “other” municipalities in 2014. PAB I represents the category that receives relatively higher federal funding for health care than the other PAB groups. “Vulnerable areas” represent municipalities which the Ministry classifies as vulnerable due to climate or historical reasons.

	Municipality profiles						
	Capit.+area	G100	20% poverty	Vuln. areas	PAB I	PAB II	PAB III & IV
Population size	161,005	196,810	18,181	23,785	9,170	20,832	96,401
Population density	569	1,287	33	36	25	56	192
Share elderly	.11	.088	.11	.11	.13	.14	.12
Share nonwhite	.38	.62	.71	.67	.44	.3	.32
GDP per capita (R\$ 1,000)	20,303	9,993	5,615	8,418	13,520	17,248	31,503
VA, agronomy share	.12	.043	.19	.18	.31	.21	.093
VA, public share	.21	.28	.5	.41	.28	.2	.14
VA, total (per 100k)	1,714,506	864,940	523,247	768,277	1,250,373	1,557,219	2,686,734
No. of bf families (per 100k)	5,459	7,770	14,121	12,048	7,751	4,193	2,906
Bf expenses, pc	112	172	331	269	157	79	55
Public primary care coverage	80	63	89	89	89	80	65
No. of priv. insured (per 100k)	16,913	12,758	1,493	3,340	5,166	13,562	32,076
HDI 2010	.72	.7	.58	.63	.68	.73	.76
HDI change 2000 - 2010	.11	.12	.17	.15	.12	.1	.092
N	446	91	1,366	439	1,241	615	200

Table B..2. Means of municipality characteristics by category, 2010. “Other” municipalities separated into PAB I - IV and vulnerable areas

PMM	Equity	Electoral	Total
0	285	822	1,107
1	1,611	1,234	2,845
Total	1,896	2,056	3,952

Table B..3. Distribution of Brazilian sample municipalities according to whether they are, or would be, targeted due to equity or electoral considerations by the PMM see Section 3.4.

Balance tables, with weighting			
Variable	no-PMM	PMM	P-value
Population size	26350.178	49052.413	0.003***
Population density	120.600	147.730	0.504
Share of elderly	0.117	0.115	0.562
Share nonwhite	0.536	0.541	0.793
GDP per capita	11923.590	11886.211	0.962
Value added, total (per 100k)	1.07e+06	1.06e+06	0.969
VA agro share	0.204	0.202	0.884
VA public share	0.339	0.342	0.830
No. of bolsa família families (per 100k)	9660.649	9604.272	0.891
Bolsa família expenses per capita	215.355	214.465	0.927
Primary healthcare coverage	83.148	82.495	0.698
Number of privately insured (per 100k)	7244.663	7320.175	0.914
HDI 2010	0.653	0.653	0.937
HDI change 2000 - 2010	0.138	0.138	0.981
N	1188	3211	

Table B.4. Covariate means by PMM participation. CPBS weights constructed to balance the covariate moments between PMM and non-PMM municipalities and the subgroups that are either classified as targeted due to equity or due to electoral considerations.

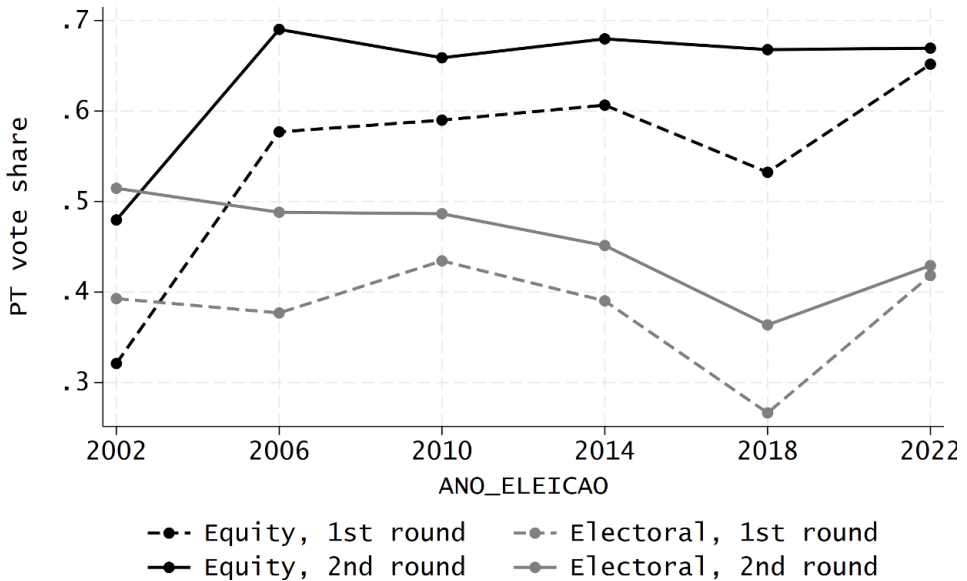


Figure B.2. PT vote share in equity and electoral PMM municipalities

Balance tables, with weighting

Variable	no-PMM, equity	PMM, equity	P-value
Population size	23277.641	31360.652	0.314
Population density	83.384	102.761	0.608
Share of elderly	0.108	0.105	0.440
Share nonwhite	0.706	0.705	0.957
GDP per capita	6044.038	6548.589	0.379
Value added, total (per 100k)	559,000	602,000	0.386
VA aggro share	0.188	0.186	0.922
VA public share	0.465	0.456	0.538
No. of bolsa família families (per 100k)	13647.812	13182.678	0.277
Bolsa família expenses per capita	317.743	307.026	0.256
Primary healthcare coverage	88.196	85.705	0.342
Number of privately insured (per 100k)	2407.358	2547.903	0.836
HDI 2010	0.594	0.599	0.521
HDI change 2000 - 2010	0.164	0.162	0.814
N	285	1611	

Variable	no-PMM, electoral	PMM, electoral	P-value
Population size	27726.826	27944.181	0.983
Population density	56.265	54.542	0.877
Share of elderly	0.126	0.125	0.593
Share nonwhite	0.438	0.443	0.732
GDP per capita	15046.583	14669.275	0.683
Value added, total (per 100k)	1,350,000	1,330,000	0.720
VA aggro share	0.236	0.237	0.929
VA public share	0.264	0.268	0.645
No. of bolsa família families (per 100k)	7396.713	7446.613	0.864
Bolsa família expenses per capita	154.503	155.919	0.838
Primary healthcare coverage	81.444	81.184	0.902
Number of privately insured (per 100k)	9187.825	8966.604	0.712
HDI 2010	0.689	0.688	0.767
HDI change 2000 - 2010	0.121	0.123	0.546
N	911	1,584	

Table B..5. Covariate means by PMM participation and subgroups. CPBS weights constructed to balance the covariate moments between PMM and non-PMM municipalities and the subgroups that are either classified as targeted due to equity or due to electoral considerations.

Results

	First round	Second round	First round	Second round
PMM participation - 2002	-0.036*** (0.006)	-0.024*** (0.005)	0.002 (0.004)	-0.002 (0.004)
PMM participation - 2006	0.018*** (0.003)	0.013*** (0.003)	0.007*** (0.003)	0.006** (0.002)
PMM participation - 2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
PMM participation - 2014	0.010*** (0.003)	0.009*** (0.003)	0.004 (0.002)	0.003 (0.002)
PMM participation - 2018	0.021*** (0.004)	0.027*** (0.004)	0.004 (0.003)	0.001 (0.003)
PMM participation - 2022	0.001 (0.004)	-0.002 (0.004)	0.005* (0.003)	0.002 (0.003)
Weighting	No	No	No	No
Fixed Effects	No	No	Yes	Yes
adj. R^2	0.485	0.468	0.844	0.840
N	26361	26361	26355	26355

Table B..6. Naive event study coefficients estimated with **pooled OLS**, depicting the marginal impact that participating in PMM had on the PT vote share in the first and second round of presidential elections in Brazil, from 2002 to 2022. The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. Regressions only include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,399 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	First round	Second round
PMM participation - 2002	0.004 (0.008)	0.001 (0.008)
PMM participation - 2006	-0.007 (0.005)	-0.002 (0.004)
PMM participation - 2010	0.000 (.)	0.000 (.)
-----	-----	-----
PMM participation - 2014	0.009** (0.004)	0.009** (0.004)
PMM participation - 2018	0.005 (0.004)	0.010** (0.005)
PMM participation - 2022	0.002 (0.005)	0.005 (0.005)
Adjusted R^2	0.862	0.858
N	26018	26018

Table B..7. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the **first and second round** of presidential elections in Brazil, from 2002 to 2022. Groups balanced and matched through CBPT. The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	First round		Second round	
	Equity	Electoral	Equity	Electoral
PMM participation - 2002	-0.008 (0.015)	0.009 (0.006)	-0.017 (0.016)	0.008 (0.006)
PMM participation - 2006	-0.023* (0.012)	0.003 (0.004)	-0.012 (0.010)	0.006 (0.004)
PMM participation - 2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
PMM participation - 2014	0.021** (0.008)	0.005 (0.003)	0.012* (0.007)	0.007** (0.003)
PMM participation - 2018	0.009 (0.008)	0.005 (0.004)	0.014 (0.009)	0.009* (0.005)
PMM participation - 2022	0.000 (0.009)	0.006 (0.005)	0.005 (0.009)	0.006 (0.005)
Adjusted R^2	0.857	0.811	0.807	0.811
N	11217	12131	11217	12131

Table B..8. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the **first and second round** of presidential elections in Brazil, from 2002 to 2022. Treatment municipalities are distinguished based on whether they were targeted primarily due to equity or electoral considerations. Groups balanced and matched through CBPT on the covariates of the base election year 2010. The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Covariate balancing propensity score weighting, all pre-PMM election years						
	First Round			Second Round		
	(1) Equity	(2) Electoral	(3) Hybrid	(1) Equity	(2) Electoral	(3) Hybrid
PMM participation - 2002	-0.008 (0.015)	0.005 (0.011)	0.006 (0.007)	-0.017 (0.016)	-0.001 (0.012)	0.007 (0.007)
PMM participation - 2006	-0.023* (0.012)	0.013** (0.006)	-0.005 (0.006)	-0.012 (0.010)	0.012** (0.006)	-0.001 (0.006)
PMM participation - 2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
PMM participation - 2014	0.021** (0.008)	0.004 (0.005)	0.001 (0.005)	0.012* (0.007)	0.006 (0.006)	0.002 (0.004)
PMM participation - 2018	0.009 (0.008)	-0.003 (0.006)	0.007 (0.006)	0.014 (0.009)	-0.003 (0.008)	0.010* (0.006)
PMM participation - 2022	0.000 (0.009)	0.001 (0.009)	0.007 (0.007)	0.005 (0.009)	-0.003 (0.010)	0.009 (0.006)
adj R ²	0.857	0.859	0.807	0.807	0.868	0.798
N	11217	4800	7330	11217	4800	7330

Table B..9. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the **first round** and **second round** of presidential elections in Brazil, from 2002 to 2022. Treatment municipalities are distinguished based on whether they were targeted primarily due to equity, electoral considerations, or a hybrid between those two, i.e. electorally targeted but with relatively high health care needs. Groups balanced and matched through CBPT on the covariates observed in the base election year 2010 (Table 3.7). The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the “Bolsa Família” program per 100,000 inhabitants. Political covariates are the current mayor’s political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Inverse probability weighting propensity score		
Variable	First round	Second round
PMM participation - 2002	-0.002 (0.008)	-0.005 (0.008)
PMM participation - 2006	-0.010** (0.005)	-0.004 (0.004)
PMM participation - 2010	0.000 (.)	0.000 (.)
-----	-----	-----
PMM participation - 2014	0.010* (0.003)	0.011*** (0.003)
PMM participation - 2018	0.006 (0.004)	0.013*** (0.004)
PMM participation - 2022	0.001 (0.004)	0.005 (0.004)
Adjusted R^2	0.916	0.921
N	26018	26018

Table B..10. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the first and second round of presidential elections in Brazil, from 2002 to 2022. **Groups balanced and matched through inverse propensity weighting of the propensity on receiving treatment** based on the covariates of the base election year 2010 (Table 3.7). The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.1$, *** $p < 0.01$

Inverse probability weighting propensity score				
	First round		Second round	
	Equity	Electoral	Equity	Electoral
PMM participation - 2002	-0.026** (0.011)	0.005 (0.005)	-0.019 (0.013)	0.008 (0.006)
PMM participation - 2006	-0.024** (0.010)	0.005 (0.004)	-0.010 (0.008)	0.006 (0.004)
PMM participation - 2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
PMM participation - 2014	0.021** (0.007)	0.008** (0.003)	0.012*** (0.003)	0.007 (0.006)
PMM participation - 2018	0.002 (0.006)	0.007 (0.004)	0.012 (0.007)	0.014*** (0.004)
PMM participation - 2022	-0.015* (0.008)	0.006 (0.005)	0.002 (0.007)	0.007 (0.005)
N	11217	12131	11217	12131

Table B.11. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the first and second round of presidential elections in Brazil, from 2002 to 2022. Treatment municipalities are distinguished based on whether they were targeted primarily due to equity or electoral considerations. **Groups balanced and matched through inverse probability weighting of the propensity on receiving treatment** based on the covariates of the base election year 2010 (Table 3.7). The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Entropy balancing		
	First round	Second round
PMM participation - 2002	-0.009** (0.005)	-0.011** (0.005)
PMM participation - 2006	-0.003 (0.003)	-0.002 (0.003)
PMM participation - 2010	0.000 (.)	0.000 (.)
-----	-----	-----
PMM participation - 2014	0.006** (0.003)	0.006** (0.003)
PMM participation - 2018	0.002 (0.003)	0.003 (0.003)
PMM participation - 2022	-0.001 (0.003)	-0.001 (0.003)
Adjusted R²	0.849	0.848
N	26006	26006

Table B..12. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the first round of presidential elections in Brazil, from 2002 to 2022. Groups balanced and matched through **entropy balancing on the covariates** of the reference year 2010 (Table 3.7). The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Entropy balancing				
	First round		Second round	
	Equity	Electoral	Equity	Electoral
PMM participation - 2002	-0.017*	- 0.002	-0.020**	0.008
	(0.009)	(0.005)	(0.010)	(0.006)
PMM participation - 2006	-0.01*	0.001	-0.012	-0.009
	(0.005)	(0.004)	(0.010)	(0.005)
PMM participation - 2010	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
PMM participation - 2014	0.017**	0.004	0.010**	0.005*
	(0.008)	(0.003)	(0.005)	(0.003)
PMM participation - 2018	-0.001	0.003	0.014	0.009*
	(0.006)	(0.004)	(0.009)	(0.005)
PMM participation - 2022	-0.004	0.003	0.005	0.006
	(0.005)	(0.004)	(0.009)	(0.005)
Adjusted R²	0.857	0.811	0.807	0.811
N	11217	12131	11217	12131

Table B..13. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the first and second round of presidential elections in Brazil, from 2002 to 2022. Treatment municipalities are distinguished based on whether they were targeted primarily due to equity or electoral considerations. **Groups balanced and matched through entropy balancing on the covariates** of the base election year 2010 (Table 3.7). The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Covariate balancing propensity score weighting, all pre-PMM election years		
Variable	First round	Second round
PMM participation - 2002	0.004 (0.008)	0.003 (0.008)
PMM participation - 2006	-0.007 (0.005)	-0.002 (0.004)
PMM participation - 2010	0.000 (.)	0.000 (.)
-----	-----	-----
PMM participation - 2014	0.009** (0.004)	0.009*** (0.003)
PMM participation - 2018	0.005 (0.004)	0.009** (0.004)
PMM participation - 2022	0.002 (0.005)	0.004 (0.004)
Adjusted R^2	0.862	0.855
N	26018	26018

Table B..14. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the first round of presidential elections in Brazil, from 2002 to 2022. **Groups balanced** and matched through CBPT **on the covariates (Table 3.7) observed in all the pre-treatment election years 2002, 2006, and 2010.** The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the “Bolsa Familia” program per 100,000 inhabitants. Political covariates are the current mayor’s political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Covariate balancing propensity score weighting,
all pre-PMM election years**

	First round		Second round	
	Equity	Electoral	Equity	Electoral
PMM participation - 2002	-0.006 (0.014)	0.009* (0.005)	-0.001 (0.014)	0.010* (0.005)
PMM participation - 2006	-0.020* (0.010)	0.006 (0.004)	-0.010 (0.008)	0.007** (0.003)
PMM participation - 2010	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
-----	-----	-----	-----	-----
PMM participation - 2014	0.012* (0.007)	0.007** (0.003)	0.012** (0.006)	0.009*** (0.003)
PMM participation - 2018	-0.000 (0.006)	0.006 (0.004)	0.006 (0.008)	0.009** (0.004)
PMM participation - 2022	-0.010 (0.008)	0.008* (0.004)	-0.002 (0.008)	0.008* (0.004)
Adjusted R^2	0.852	0.804	0.806	0.806
N	11217	12131	11217	12131

Table B..15. Event study coefficients estimated with a TWFE estimator, depicting the marginal impact that participating in PMM had on the PT vote share in the first and second rounds of presidential elections in Brazil, from 2002 to 2022. Treatment municipalities are distinguished based on whether they were targeted primarily due to equity or electoral considerations. **Groups balanced and matched through CBPT on the covariates (Table 3.7) observed in the pre-treatment election years 2002, 2006, and 2010.** The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the “Bolsa Familia” program per 100,000 inhabitants. Political covariates are the current mayor’s political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Sun & Abraham (2021)		
	First round	Second round
PMM participation - 2002	-0.003 (0.004)	-0.0009 (0.003)
PMM participation - 2006	-0.001 (0.005)	-0.003 (0.006)
PMM participation - 2010	0.000 (.)	0.000 (.)
PMM participation - 2014	0.005* (0.003)	0.005* (0.003)
PMM participation - 2018	0.003 (0.003)	0.007* (0.004)
PMM participation - 2022	0.003 (0.004)	0.004 (0.004)
Adjusted R-squared	0.860	0.860
N	31,391	31,391

Table B..16. Event study coefficients estimated with the **estimator defined by Sun and Abraham (2021)**, depicting the marginal impact that participating in PMM had on the PT vote share in the first round and second round of presidential elections in Brazil, from 2002 to 2022. The comparison group are municipalities that have never been treated. Groups balanced and matched through CBPT. The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, state fixed effects, and dynamic linear time trends for every state times election year. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the "Bolsa Familia" program per 100,000 inhabitants. Political covariates are the current mayor's political party. Standard errors are clustered over 4,311 municipalities, and in parentheses. * p<0.1, ** p<0.05, *** p<0.01

	T = -2	T = -1	T = 1	T = 2	T = 3
Adopted by 2014	-.0033487	.00474765	.00766004	.00395903	.00261965
Adopted by 2018	-.00299609	-.0028622	-.00583633	-.00498995	0
Adopted by 2022	-.00201033	-.0243932	-.01299989	0	0

Table B..17. Cohort-specific treatment effect estimates for each relative time, first voting rounds, Sun & Abraham 2021 estimator.

	$T = -2$	$T = -1$	$T = 1$	$T = 2$	$T = 3$
Adopted by 2014	.85340314	.70578122	.85562159	.90785619	1
Adopted by 2018	.09050112	.15194498	.08915305	.09214381	0
Adopted by 2022	.05609574	.1422738	.05522536	0	0

Table B..18. Cohort-specific weights for each relative time, Sun & Abraham 2021.

de Chaisemartin & D’Haultfoeuille 2020		
	First round	Second round
PMM participation - 2002	0.005 (0.006)	-.001 (0.011)
PMM participation - 2006	-0.003 (0.003)	-0.009 (0.005)
PMM participation - 2010	0.000 (.)	0.000 (.)
PMM participation - 2014	0.004* (0.003)	0.010*** (0.004)
PMM participation - 2018	0.002 (0.003)	0.010** (0.005)
PMM participation - 2022	0.002 (0.004)	0.004 (0.005)
N	31,391	31,391

Table B..19. Event study coefficients estimated with the **estimator of De Chaisemartin and d’Haultfoeuille (2020)**, depicting the marginal impact that participating in PMM had on the PT vote share in the first round and second round of presidential elections in Brazil, from 2002 to 2022. The comparison group are municipalities that have not yet been treated. Groups balanced and matched through CBPT. The dashed line divides elections before and after the PMM introduction, i.e. placebo from treatment periods. All coefficients are relative to the base election year 2010, the last presidential election before the PMM introduction. All regressions include municipality fixed effects, election fixed effects, municipality profile fixed effects, and state fixed effects. All regressions also include a number of standardized time-varying socioeconomic characteristics on the municipality level, namely GDP per capita, total value added (VA) in R\$ per 100,000 inhabitants, the share of VA produced by the agribusiness sector, the share of VA produced by the public sector, the share of the population equal or older than 60, population density, total population, and the number of households receiving social welfare through the “Bolsa Familia” program per 100,000 inhabitants. Political covariates are the current mayor’s political party. Standard errors are clustered over 5,513 municipalities, and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

Appendix of Chapter Four

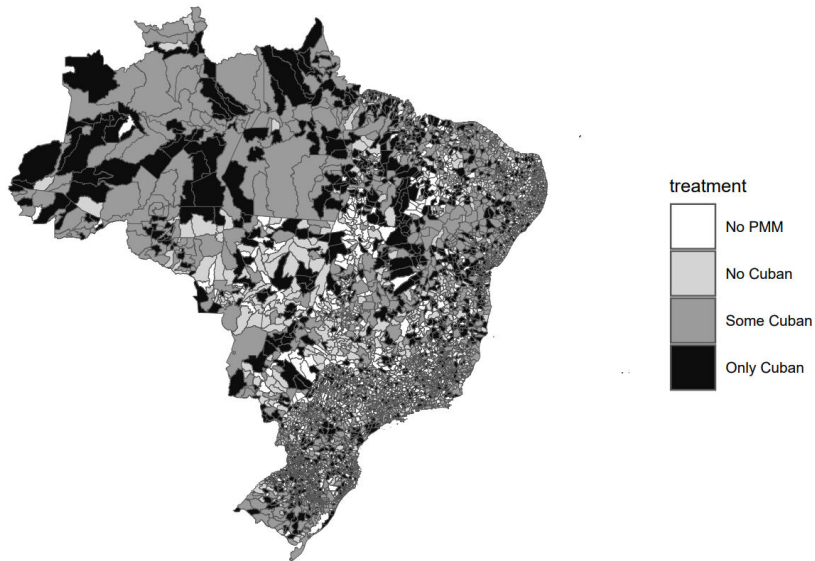


Figure C..1. PMM allocation of Brazilian municipalities in October 2018 (last month before the Cuban exit). White municipalities did not participate in PMM and are excluded from the analysis. Light grey municipalities had no Cuban PMM doctor in October 2018, grey municipalities had some Cuban PMM doctors and black municipalities had only Cuban PMM doctors. Original data from the Brazilian Ministry of Health.

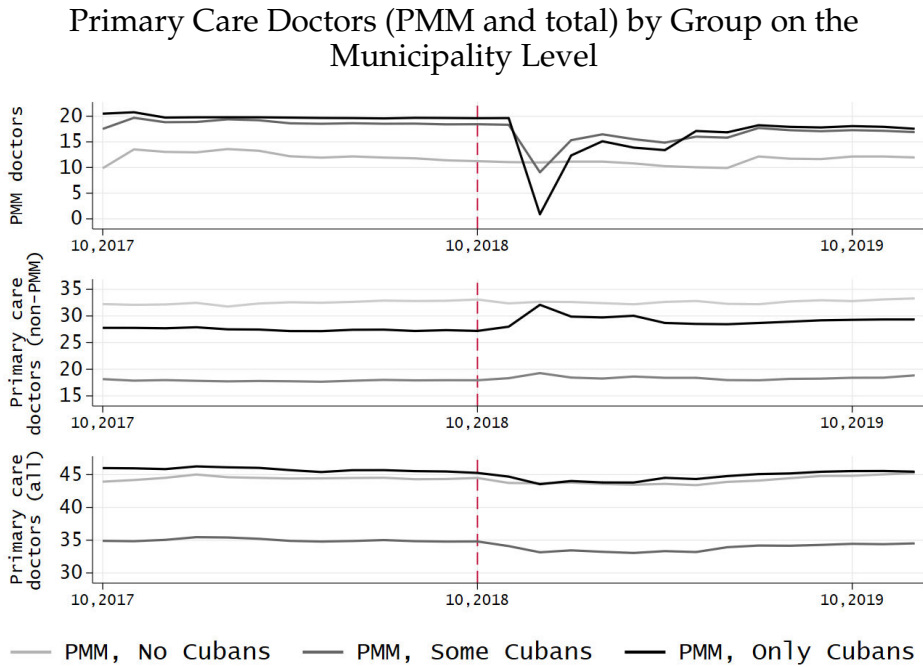


Figure C..2. The graphs shows trends in the group means of the number of PMM doctors (upper plot), non-PMM primary care doctors (middle plot), and all primary care doctors (lower plot) per 100,000 residents. The series in dark grey presents treatment group 1, the series in black presents treatment group 2. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Brazilian Ministry of Health and CNES.

Table C..1. Primary Care Consultations: Conditions and Consultation Types by Responsible Professionals, PMM Eligible Municipalities

	Mean per 100k inhab.	Share doctors	Share non-doctors	Share no information
Consultations	14,527	.64	.36	0
<i>By condition</i>				
Related to chron. cond.	4,196	.61	.34	.06
Asthma	68	.87	.13	0
COPD	43	.8	.2	0
Diabetes	601	.7	.29	.01
Hypertension	1,738	.69	.25	.06

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Table C..1 – Continued from previous page

	Mean per 100k inhab.	Share doctors	Share non-doctors	Share no information
Mental health	1,149	.66	.32	.02
Breast cancer screening	205	.45	.54	.01
Cervical cancer screening	333	.32	.67	.01
Cardiovascular screening	61	.68	.32	0
Related to risk factors for chron. cond.	1,547	.48	.5	.02
Malnutrition	74	.74	.26	0
Obesity	224	.57	.43	0
Smoking	65	.68	.32	0
Alcohol	32	.73	.27	0
Other drugs	16	.64	.36	0
Rehabilitation	1,136	.31	.67	.02
Related to maternal health	2,817	.45	.51	.04
Prenatal care	638	.51	.48	.01
Pediatrics	1,215	.45	.52	.03
Postpartum care (first 42 days)	48	.37	.63	0
Sexual and reproductive health	915	.39	.58	.03
Related to infectious diseases	60	.57	.43	0
STDs	34	.61	.39	0
Dengue	5	.77	.23	0
Hansen's disease	12	.53	.47	0
Tuberculosis	10	.48	.52	0
Without classified conditions	5,969			
<i>By type of consultation</i>				
Scheduled singular care	4,962	.65	.35	0
Scheduled ongoing care	2,606	.41	.59	0

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Table C..1 – Continued from previous page

	Mean per 100k inhab.	Share doctors	Share non-doctors	Share no information
Non-scheduled walk-ins	6,483	.67	.33	0
Non-scheduled initial hearing	1,053	.15	.85	0
Non-scheduled urgent care	259	.7	.3	0
Without classified type	0			

Notes: Municipality averages in rates per 100,000 inhabitants for October 2018 (the pre-treatment month). Data on private insurance coverage for September 2018 (the last available pre-treatment period). Where specified, variables only include observations for individuals aged 60 years or more. Column 1 includes all Brazilian municipalities (excluding special indigenous districts). Column 2 includes all municipalities that participated in PMM. Columns 3 to 5 distinguish PMM participating municipalities based on the presence of Cuban PMM doctors in October 2018. Control municipalities had no Cuban PMM doctor in October 2018. Treatment group 1 (T1) had some Cuban and some non-Cuban PMM doctors in October 2018. Treatment group 2 (T2) had only Cuban PMM doctors in October 2018. Numbers within columns do not always sum up as data becomes less precise with disaggregation. Data on doctor supply originally from the Ministry of Health. Data on primary care services originally from SISAB. Data on mortality and hospitalizations originally from SIM/Datasus and SIH/Datasus. Data on medication originally from the Ministry of Health. Data on private insurance coverage originally from ANS. Data on ambulatory care in secondary and tertiary care centers originally from SIA.

Table C..2. Definitions of the Type of Consultations according to SISAB

Type of consultation	Definition according to SISAB
Scheduled continuous consultations	These are consultations that constitute individual programmatic actions, directed to life cycles, diseases and priority diseases, which require continuous monitoring. As an example, the care given to pregnant women, children, the elderly, people with chronic diseases (hypertension, diabetes) etc.
Scheduled consultations	It comes from spontaneous demand or by direct appointment at the reception, of a non-urgent nature, not attended on the same day of the demand, but scheduled for another day. For example, cases of skin lesions, without signs of inflammation or infection; nonspecific complaints of fatigue; tiredness; chronic headache; change or beginning of contraceptive contraceptive medication, etc.
Initial reception/orientation	Refers to reception done by a senior professional at the moment the user arrives at the health service, reporting complaints or signs of symptoms. When the patient arrives at the health service, reporting complaints or signs and perceived symptoms It does not include flow orientations within the UBS.
Consultation on the same day	A consultation that takes place on the same day that the user seeks service, of a non-urgent nature. As an example, cases with symptoms of great pain or those that cannot wait for an appointment on another day, such as low back pain, sore throat, urinary symptoms, etc. Another characteristic for same-day care may be related to the social or psychological vulnerability of the patient.
Urgent consultations	Care provided to the patient when there is a possibility of worsening of the condition or risk of life and that determines the need for immediate relief of physical and/or psychological suffering, recovery of the health condition, stabilization/life support and/or referral to another part of the network when necessary. For example, cases of chest pain, neurological symptoms and/or symptoms and/or neurological signs, hypertensive emergency, etc.

Table C.3. Municipality Averages of Main variables in October 2018

	Pooled		By Treatment groups		
	All	PMM	Control	Treat 1	Treat 2
Primary health care professionals					
Primary care doctors (per 100k)	44	42	44	35	45
Non-doctors (per 100k)	257	318	172	761	87
PMM doctors (per 100k)	12	17	11	18	20
Cuban PMM doctors (per 100k)	8	10	0	10	20
Share of Cubans among PMM doctors	.59	.59	0	.52	1
Share of Cubans among all PC doctors	.19	.26	0	.29	.45
Total hours, non-doctors (per 100k)	22,947	22,704	22,775	22,111	23,118
Av. weekly hrs., primary care doctors	36	36	36	36	37
Avg. weekly hrs., non-doctors	38	38	38	37	38
Primary care services					
Consultations (per 100k)	15,734	14,527	15,260	11,676	16,190
<i>by condition group</i>					
Chronic conditions	4,331	4,196	4,242	3,835	4,448
Related risk factors	1,703	1,547	1,747	1,112	1,731
Maternal health	2,864	2,817	2,765	2,740	2,920
Infections	58	60	50	67	62
No classification	6,884	5,969	6,603	3,927	7,069
<i>by consultation type</i>					
Scheduled singular care	5,244	4,960	5,200	4,341	5,271
Scheduled ongoing care	2,786	2,607	2,806	2,255	2,745
Non-scheduled walk-ins	7,243	6,484	6,992	4,481	7,698
Non-scheduled initial hearings	1,286	1,053	1,183	680	1,274
Non-scheduled urgent care	312	259	246	212	320
Share consultations by doctors	0.64	0.64	0.64	0.63	0.65
Procedures (per 100k)	16,231	14,307	15,052	10,258	16,915
Share procedures by doctors	0.04	0.04	0.03	0.03	0.04
Home visits (per 100k)	26,295	25,235	26,500	21,132	27,455
Share home visits by doctors	0	0	0	0	0
Hospitalizations					
Total (per 100k)	563	557	562	537	570
Amenable to pc (per 100k)	119	120	115	110	132
Non-amenable to pc (per 100k)	444	437	447	426	438

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Table C..3, continued from previous page

	Pooled		By Treatment groups		
	All	PMM	Control	Treat 1	Treat 2
Chronic conditions (per 100k)	186	181	188	163	190
Infections (per 100k)	40	41	36	39	46.81
Infants (per 100k)	16	17	16	19	17
Diabetes (60+, per 100k)	33	35	32	35	37
Hypertension (60+, per 100k)	21	22	18	21	26
Mortality					
Total (per 100k)	57	55	58	51	56
Amenable to pc (per 100k)	17	16	17	15	16
Non-amenable to pc (per 100k)	40	39	41	36	39
Chronic conditions (per 100k)	37	36	38	34	37
Infections (per 100k)	2	1.9	1.93	1.87	1.86
Infants (per 100k)	1.5	1.6	1.35	1.62	1.67
Diabetes (60+, per 100k)	22	22	21	22.5	22
Hypertension (60+, per 100k)	19	19	21	18	19
Medication					
Subs. hypertension med. (R\$, per 100k)	65,239	62,386	69,793	47,368	69,425
Subs. diabetes med. (R\$, per 100k)	33,270	31,411	34,982	28,819	30,461
Private health insurance					
Beneficiaries (per 100k)	8,038	8,234	8,254	10,438	6,462
Ambulatory care outside of UBS					
Basic complexity treatments					
Emergency rooms (per 100k)	19	19	16	26	10
Hospitals (per 100k)	158	147	69	108	179
Specialists (per 100k)	138	138	153	125	143
Facilities offering basic complexity treatments					
Emergency rooms (per 100k)	1	1.5	2	5	6
Hospitals (per 100k)	4	4.5	6	2.5	6
Specialists (per 100k)	4	5	5	5	6
N	5,561	4,163	1,320	1,260	1,583
RAIS data (non-PMM)					
Total wages					
Primary care (per 100k)	327419	295649	318931	291969	276131

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Table C..3, continued from previous page

	Pooled		By Treatment groups		
	All	PMM	Control	Treat 1	Treat 2
Non-primary care (per 100k)	96071	90330	90992	85826	97968
Public sector (per 100k)	288564	259330	278617	250737	249750
Private sector (per 100k)	110592	105048	107652	111687	84793
Wages per doctor					
Primary care	4717	4803	4702	4238	5090
Non-primary care	3605	2806	3380	3463	3923
Per doctor, public sector	4085	4009	4018	3669	4388
Per doctor, private sector	4026	3949	4022	3730	4397
N, RAIS	4,228	3,183	1,034	1,034	1,115

Notes: Municipality averages in rates per 100,000 inhabitants for October 2018 (the pre-treatment month). Data on private insurance coverage for September 2018 (the last available pre-treatment period). Where specified, variables only include observations for individuals aged 60 years or more. Column 1 includes all Brazilian municipalities (excluding special indigenous districts). Column 2 includes all municipalities that participated in PMM. Columns 3 to 5 distinguish PMM participating municipalities based on the presence of Cuban PMM doctors in October 2018. Control municipalities had no Cuban PMM doctor in October 2018. Treatment group 1 (T1) had some Cuban and some non-Cuban PMM doctors in October 2018. Treatment group 2 (T2) had only Cuban PMM doctors in October 2018. Numbers within columns do not always sum up as data becomes less precise with disaggregation. Data on doctor supply originally from the Ministry of Health. Data on primary care services originally from SISAB. Data on mortality and hospitalizations originally from SIM/Datasus and SIH/Datasus. Data on medication originally from the Ministry of Health. Data on private insurance coverage originally from ANS. Data on ambulatory care in secondary and tertiary care centers originally from SIA. Data on salaries (non-PMM) from RAIS, with differing number of available municipalities.

Table C..4. Socio-demographic Control Variables on the Municipality Level

	Control	Treat 1	Treat 2	Pairwise t-test		
				(1)-(2)	(1)-(3)	(2)-(3)
Number of families receiving Bolsa Familia (per 100k)	9,096.367 (98.263)	9,731.207 (105.103)	10,321.890 (97.327)	-634.840***	-1225.523***	-590.683***
Bolsa Familia expenses per resident (R\$)	247.113 (3.135)	275.399 (3.564)	301.820 (3.371)	-28.287***	-54.707***	-26.421***
GDP per capita (R\$)	23,565.167 (348.395)	22,988.361 (359.845)	21,469.380 (302.367)	576.806	2,095.787***	1,518.981***
Share of public sector value added	0.329 (0.003)	0.317 (0.003)	0.361 (0.003)	0.012***	-0.032***	-0.044***
Share of population over 65 (%)	10.545 (0.047)	9.032 (0.045)	10.320 (0.050)	1.512***	0.224***	-1.288***
Sex ratio in the municipality	102.539 (0.546)	99.916 (0.185)	102.760 (0.101)	2.623***	-0.221	-2.844***
Number of observations	1,316	1,259	1,578	2,575	2,894	2,837

Notes: Municipality averages in rates per 100,000 inhabitants by treatment group, pooled over the years 2017 to 2019. Original data from the Brazilian Institute of Geography and Statistics (IBGE).

Additional Results

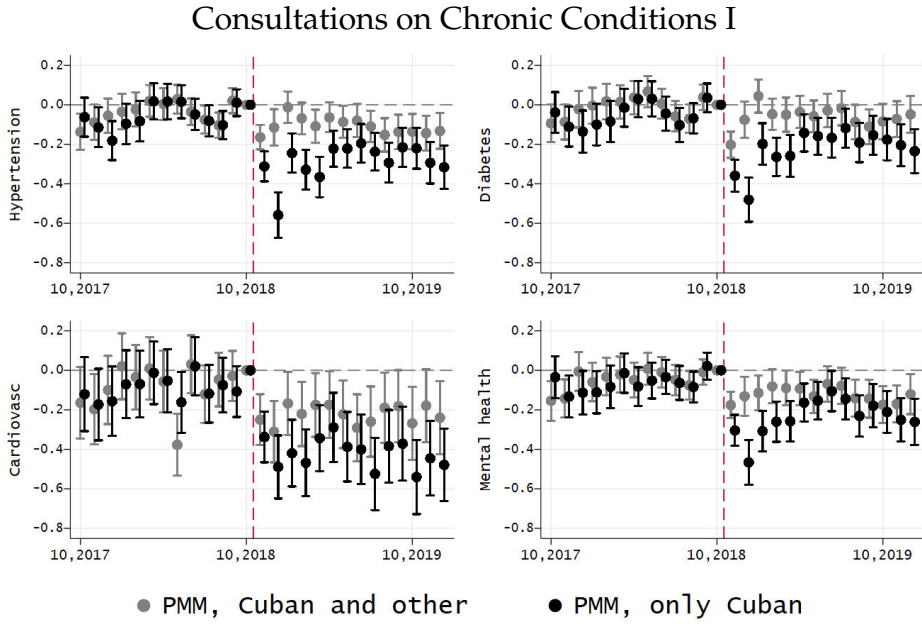


Figure C.1. The graph shows event studies for the number of primary care consultations on chronic conditions, (hypertension, diabetes, cardiovascular diseases, and mental health), following equation 4.1. Outcomes are per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health and CNES.

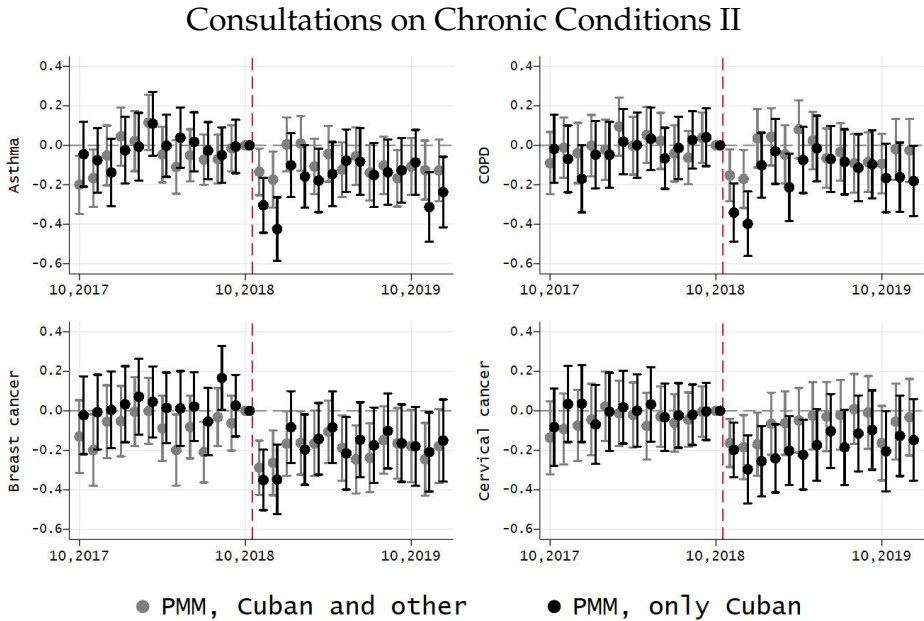


Figure C..2. The graph shows event studies for the number of primary care consultations on asthma, COPD, and breast and cervical cancer screenings, following equation 4.1. Outcomes are per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health and SISAB.

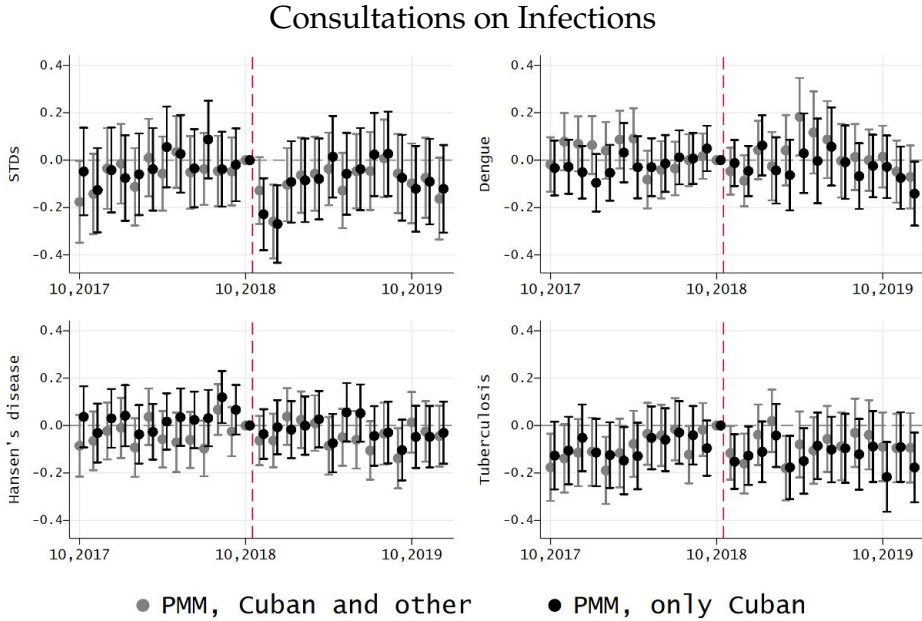


Figure C.3. The graph shows event studies for the number of primary care consultations on sexually transmitted diseases, dengue, Hansen's disease, and tuberculosis, following equation 4.1. Outcomes are per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health and SISAB.

Robustness

Time-Varying Controls

Effects on Primary Care Services with Time-Varying Controls

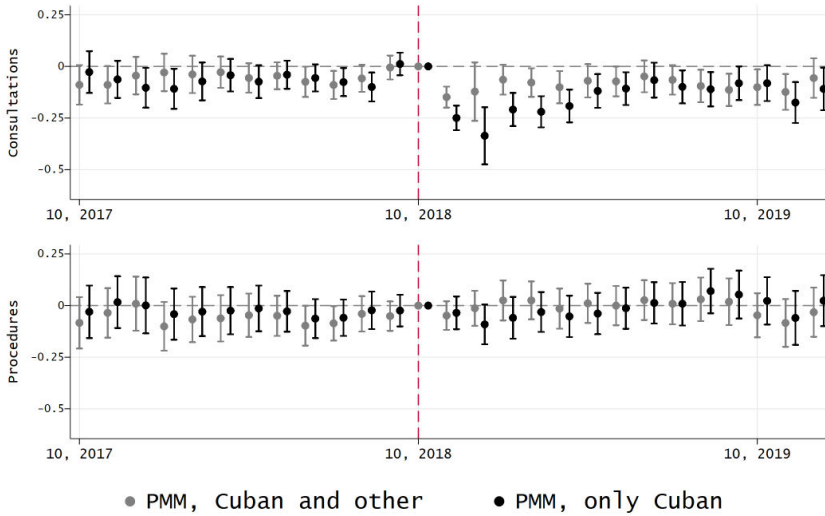


Figure C.1. The graph shows event studies for the number of consultations (upper panel) and procedures (lower panel) performed in primary health care facilities, following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects, together with annual municipality-level controls (population share of Bolsa Familia coverage, Bolsa Familia expenses per capita, GDP per capita, value added by public sector employment (in R\$ 1,000), the share of the population above 65, and the sex ratio). Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and IBGE.

Effects on Primary Care Consultations with Time-Varying Controls

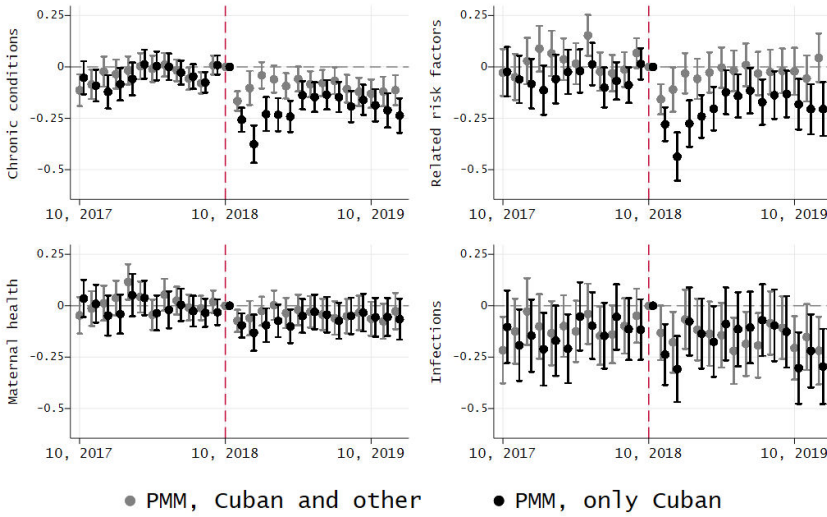


Figure C..2. The graph shows event studies for the number of consultations in four different categories, performed in primary health care facilities, following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects, together with annual municipality-level controls (population share of Bolsa Familia coverage, Bolsa Familia expenses per capita, GDP per capita, value added by public sector employment (in R\$ 1,000), the share of the population above 65, and the sex ratio). Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and IBGE.

Effects on Hospitalizations with Time-Varying Controls

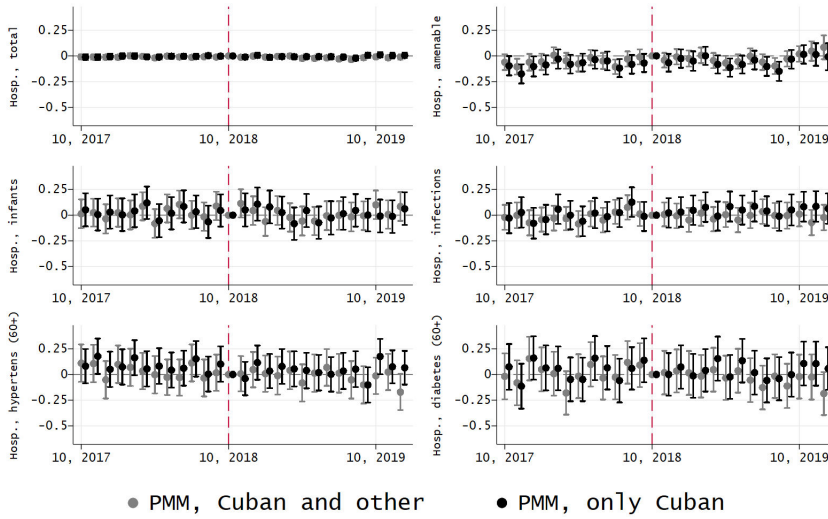


Figure C.3. The graph shows event studies for the number of hospitalizations in six different categories, following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects, together with annual municipality-level controls (population share of Bolsa Familia coverage, Bolsa Familia expenses per capita, GDP per capita, value added by public sector employment (in R\$ 1,000), the share of the population above 65, and the sex ratio). Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SIH/Datasus, the Ministry of Health and IBGE.

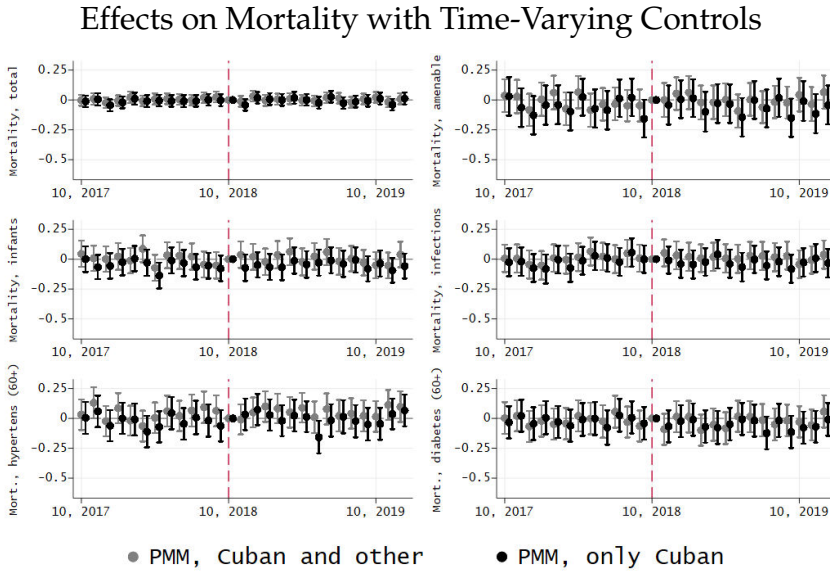


Figure C.4. The graph shows event studies for the number of hospital mortality in six different categories, following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects, together with annual municipality-level controls (population share of Bolsa Familia coverage, Bolsa Familia expenses per capita, GDP per capita, value added by public sector employment (in R\$ 1,000), the share of the population above 65, and the sex ratio). Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SIM/Datasus, the Ministry of Health and IBGE.

Alternative Definition of the Control Group

Effects on Primary Care Services – Extended Control Group

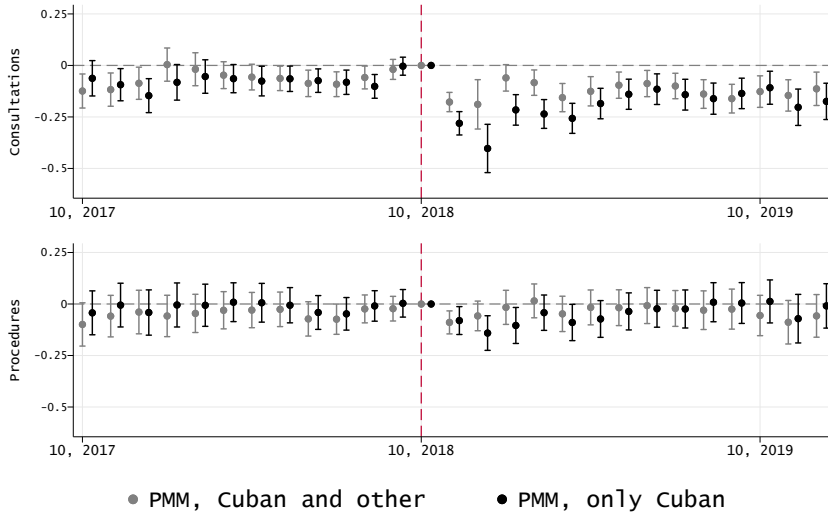


Figure C..5. The graph shows event studies for the number of consultations (upper panel) and procedures (lower panel) performed in primary health care facilities, following equation 4.1 but including non-PMM municipalities in the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Effects on Primary Care Consultations – Extended Control Group

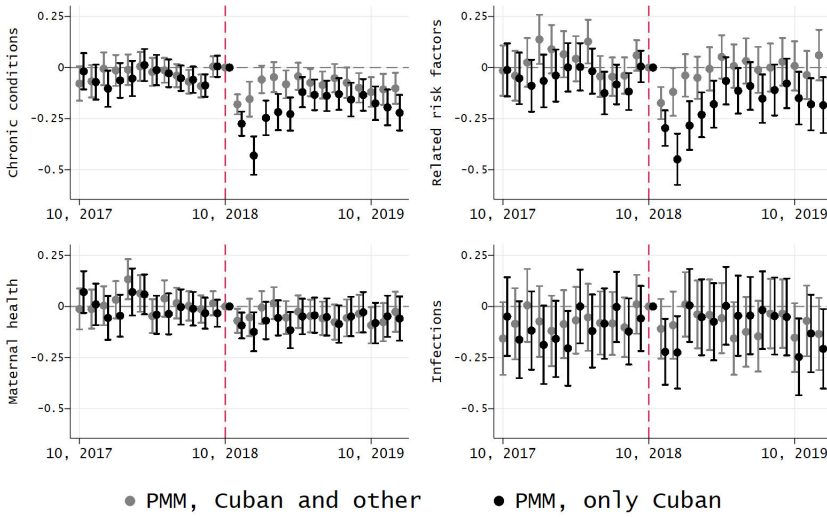


Figure C..6. The graph shows event studies for the number of consultations in four different categories, performed in primary health care facilities, following equation 4.1 but including non-PMM municipalities in the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

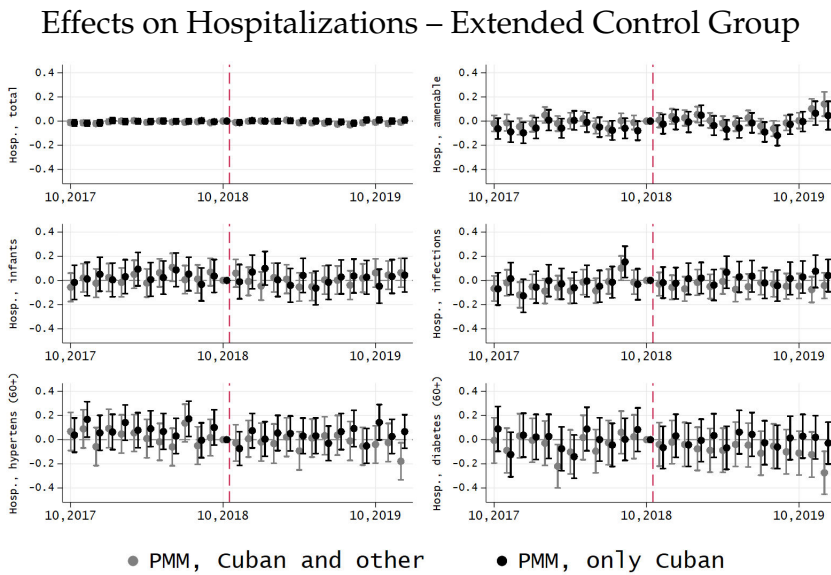


Figure C..7. The graph shows event studies for the number of hospitalizations in six different categories, following equation 4.1 but including non-PMM municipalities in the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SIH/Datasus and the Ministry of Health.

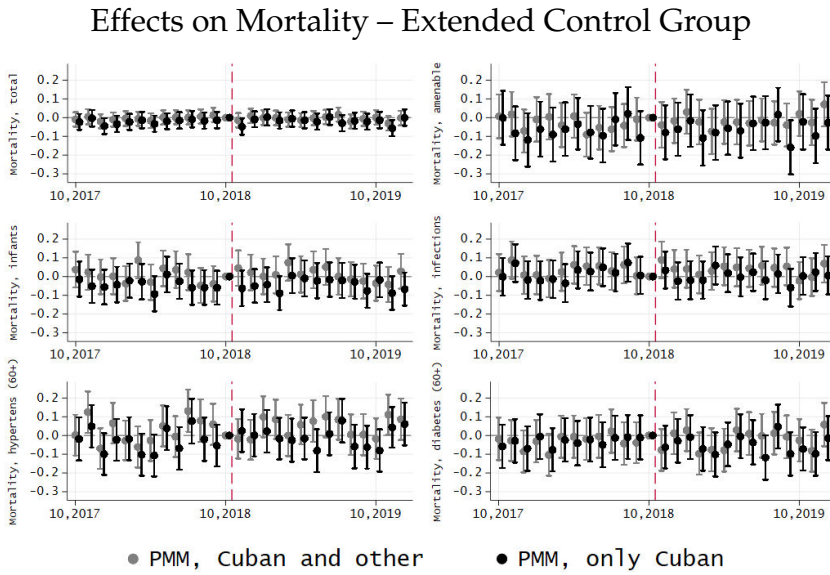


Figure C..8. The graph shows event studies for the number of hospital mortality in six different categories, following equation 4.1 but including non-PMM municipalities in the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SIM/Datasus and the Ministry of Health.

Effects on Primary Care Services – Restricted Control Group

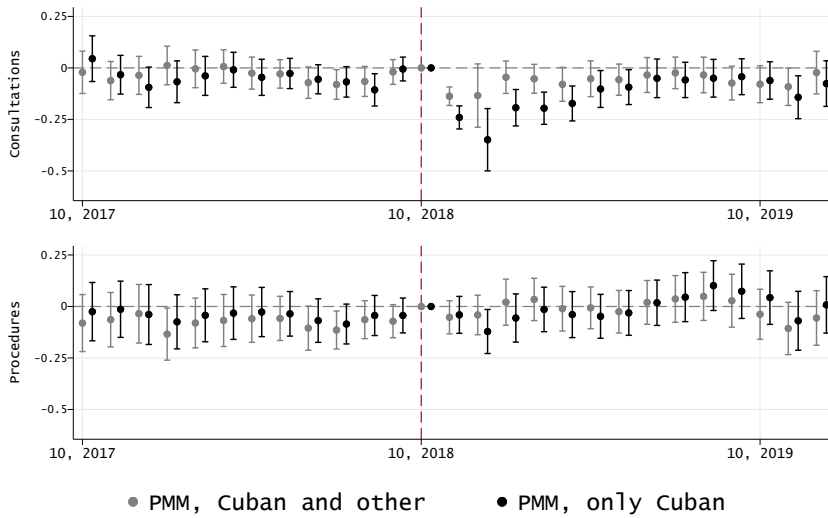


Figure C..9. The graph shows event studies for the number of consultations (upper panel) and procedures (lower panel) performed in primary health care facilities, following equation 4.1 but excluding municipalities which were formally part of PMM but did not employ any PMM doctor in October 2018 from the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Effects on Primary Care Consultations – Restricted Control Group

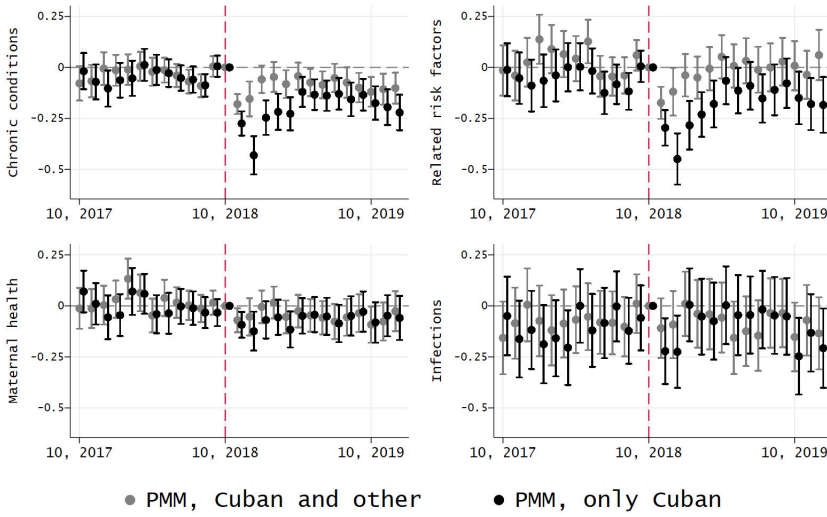


Figure C..10. The graph shows event studies for the number of consultations in four different categories, performed in primary health care facilities, following equation 4.1 but excluding municipalities which were formally part of PMM but did not employ any PMM doctor in October 2018 from the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Effects on Hospitalizations – Restricted Control Group

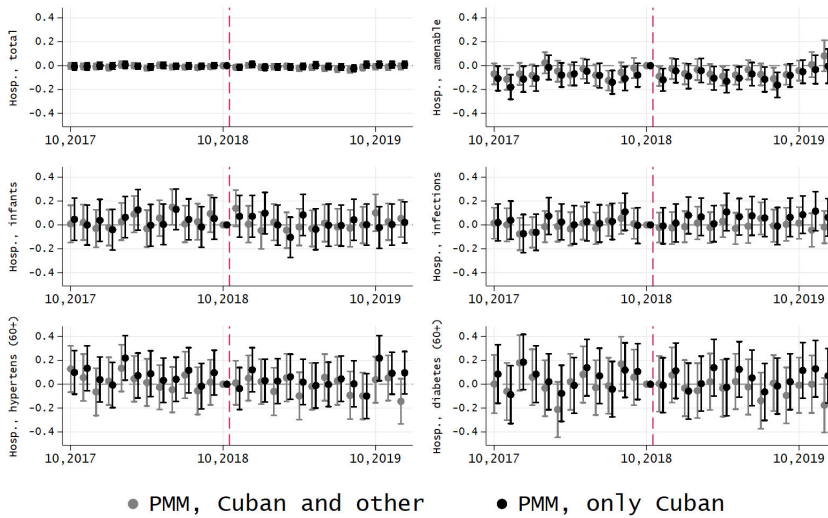


Figure C.11. The graph shows event studies for the number of hospitalizations in six different categories, following equation 4.1 but excluding municipalities which were formally part of PMM but did not employ any PMM doctor in October 2018 from the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SIH/Datasus and the Ministry of Health.

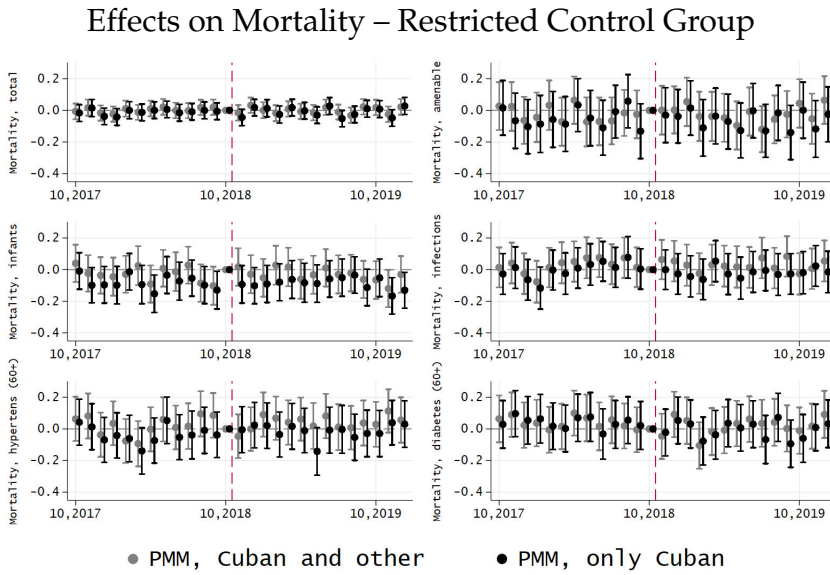


Figure C.12. The graph shows event studies for the number of hospital mortality in six different categories, following equation 4.1 but excluding municipalities which were formally part of PMM but did not employ any PMM doctor in October 2018 from the control group. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SIM/Datasus and the Ministry of Health.

Alternative Definitions of Treatment Variables and Specifications

Effects on Primary Care Services – Continuous Treatment

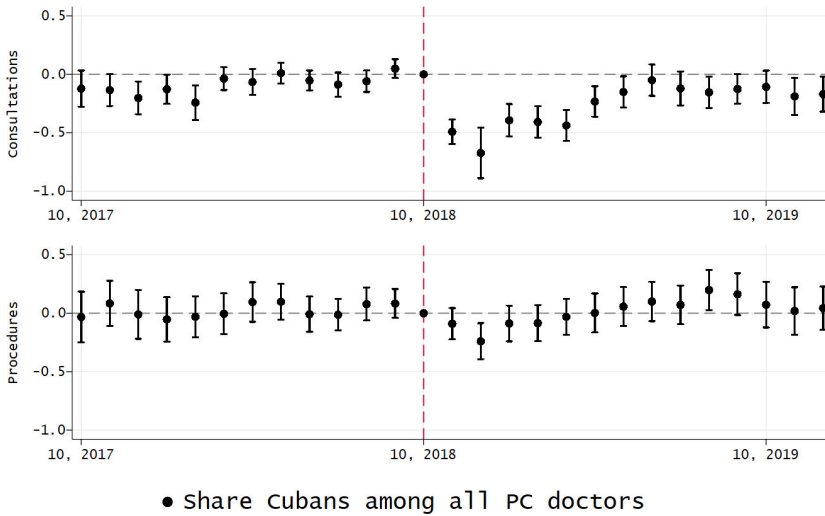
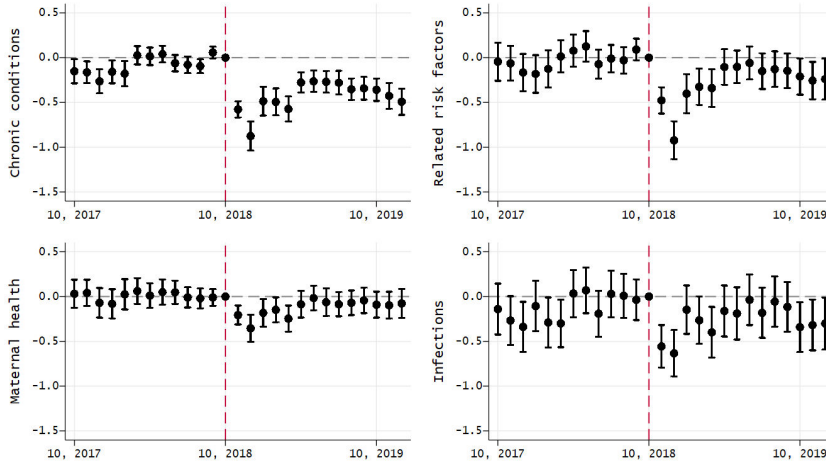


Figure C.13. The graph shows event studies for the number of consultations (upper panel) and procedures (lower panel) performed in primary health care facilities, following equation 4.2, where the treatment variable is defined as the share of Cubans among all municipal primary care doctors in October 2018 (PMM and non-PMM). Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and CNES.

Effects on Primary Care Consultations – Continuous Treatment



● Share Cuban PMM among all PC doctors

Figure C..14. The graph shows event studies for the number of consultations in four different primary care categories performed in primary health care facilities, following equation 4.2, where the treatment variable is defined as the share of Cubans among all municipal primary care doctors in October 2018 (PMM and non-PMM). Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and CNES.

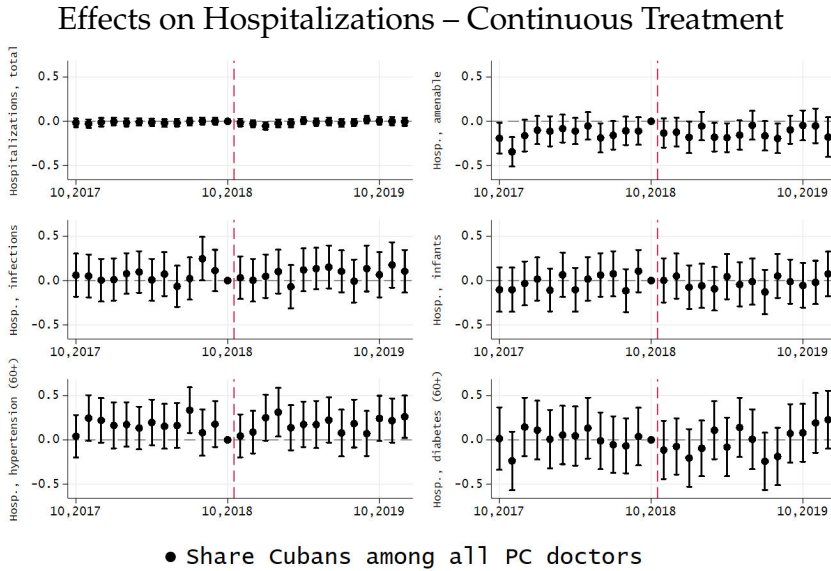


Figure C.15. The graph shows event studies for the number of hospitalizations in six different categories, following equation 4.2, where the treatment variable is defined as the share of Cubans among all municipal primary care doctors in October 2018 (PMM and non-PMM). Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Where indicated, the number of hospitalizations only includes individuals aged 60 or more. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the SIH/Datasus, CNES and the Ministry of Health.

Effects on Mortality – Continuous Treatment

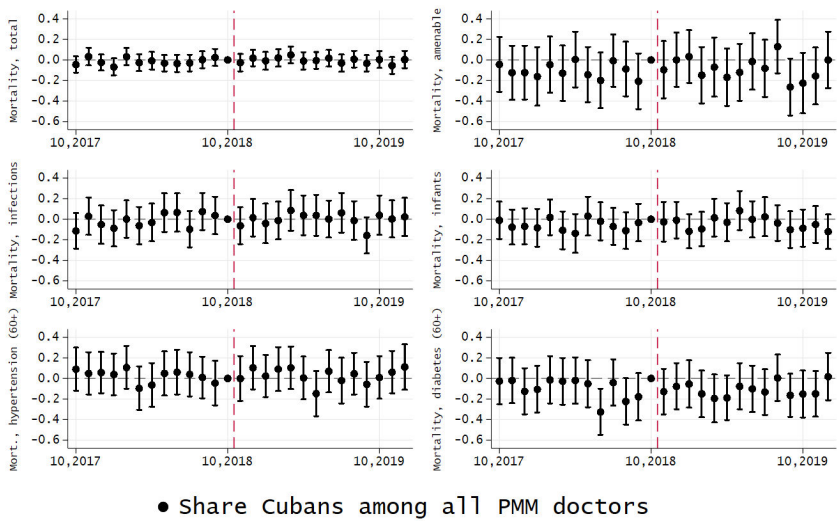


Figure C..16. The graph shows event studies for the number of hospital mortality in six different categories, following equation 4.2, where the treatment variable is defined as the share of Cubans among all municipal primary care doctors in October 2018 (PMM and non-PMM). Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Where indicated, the number of hospitalizations only includes individuals aged 60 or more. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the SIM/Datasus, CNES and the Ministry of Health.

Poisson pseudo-likelihood regression

Effects on Primary Care Services – Poisson pseudo-likelihood regression

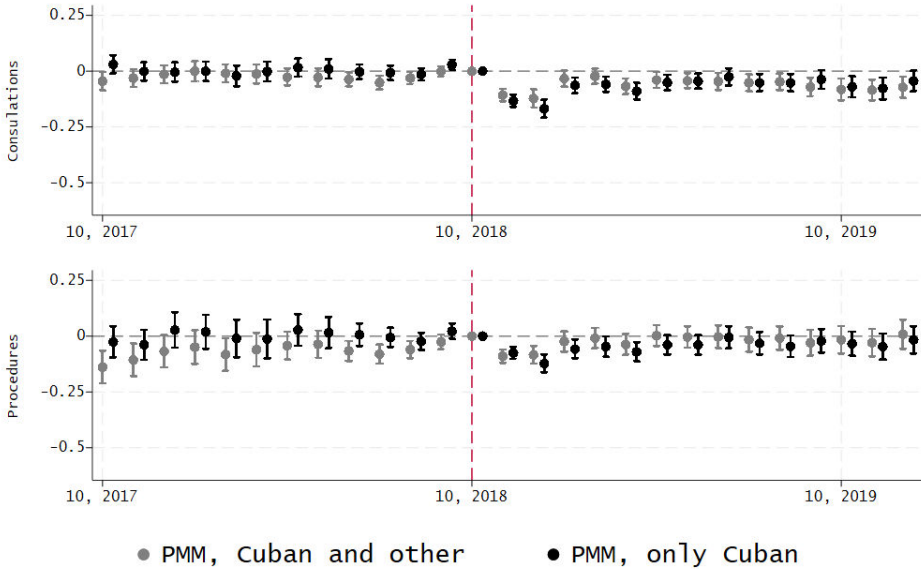


Figure C..17. The graph shows event studies for the number of consultations (upper panel) and procedures (lower panel) performed in primary health care facilities, following a poisson pseudo-likelihood regression, which is geared towards handling count and ratio outcomes including potentially multiple zero values. Outcomes per 100,000 inhabitants. Regressions include municipality, month-year and state times month-year fixed effects. Standard errors are clustered at the municipality level. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Effects on Primary Care Consultations Categories – Poisson pseudo-likelihood regression

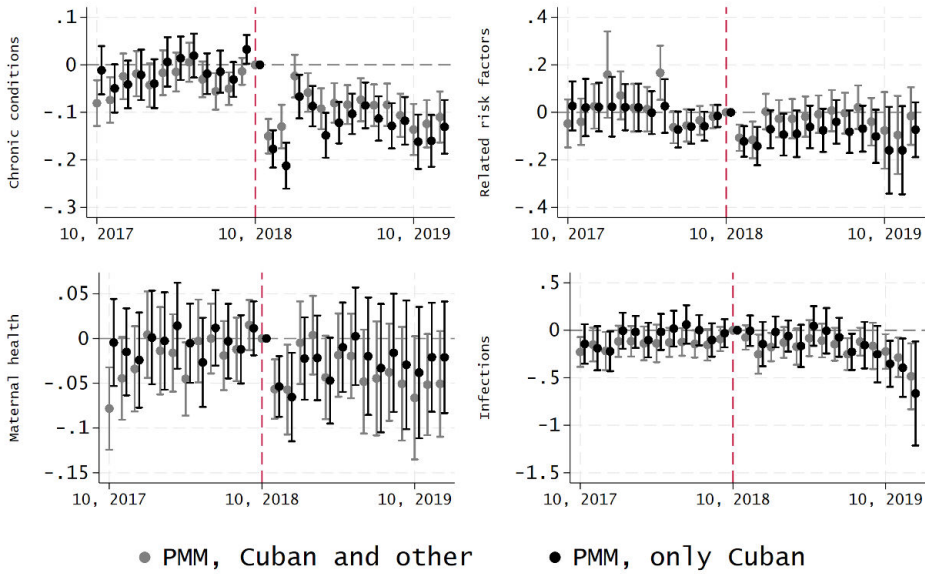


Figure C..18. The graph shows event studies for the number of consultations in four different primary care categories performed in primary health care facilities, following a poisson pseudo-likelihood regression, which is geared towards handling count and ratio outcomes including potentially multiple zero values. Outcomes per 100,000 inhabitants. Regressions include municipality, month-year and state times month-year fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and CNES.

Effects on Hospitalizations – Poisson pseudo-likelihood regression

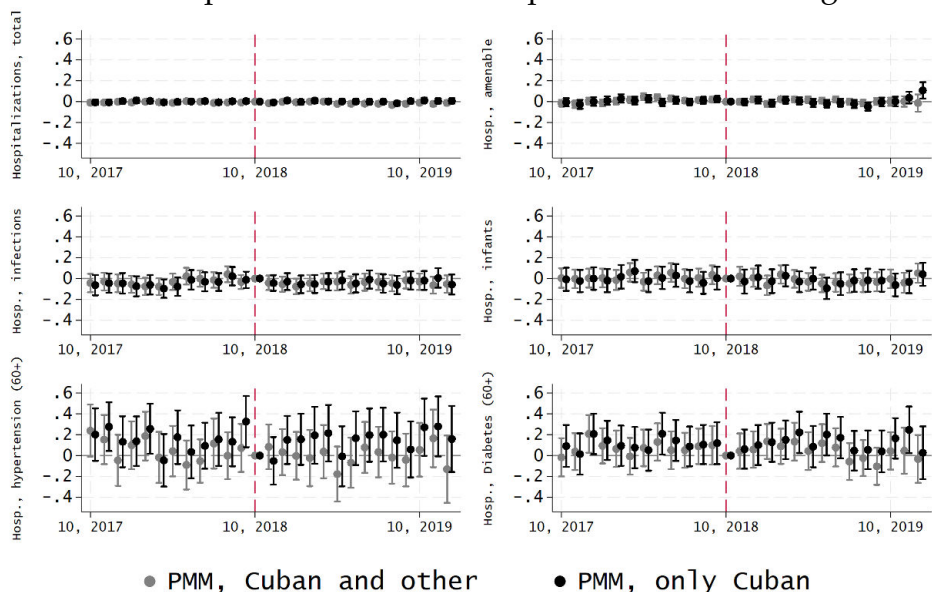


Figure C.19. The graph shows event studies for the number of hospitalizations in six different categories, following a poisson pseudo-likelihood regression, which is geared towards handling count and ratio outcomes including potentially multiple zero values. Outcomes per 100,000 inhabitants. Regressions include municipality, month-year and state times month-year fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and CNES.

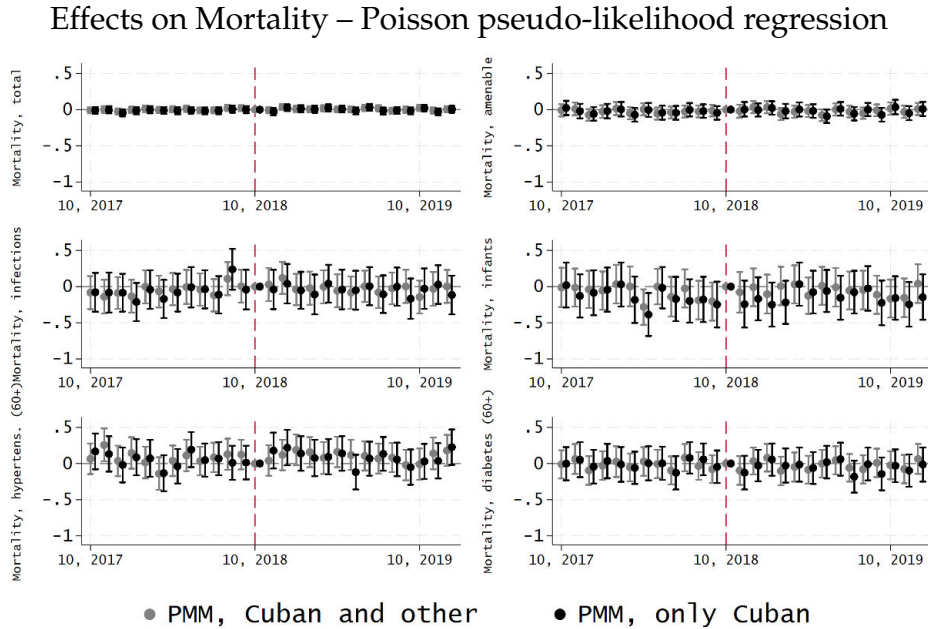


Figure C..20. The graph shows event studies for the number of deaths in six different categories, following a poisson pseudo-likelihood regression, which is geared towards handling count and ratio outcomes including potentially multiple zero values. Outcomes per 100,000 inhabitants. Regressions include municipality, month-year and state times month-year fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, the Ministry of Health and CNES.

Leave-One-Out Analysis

Effects on Primary Care Consultations Categories – Leave-One-Out-Analysis

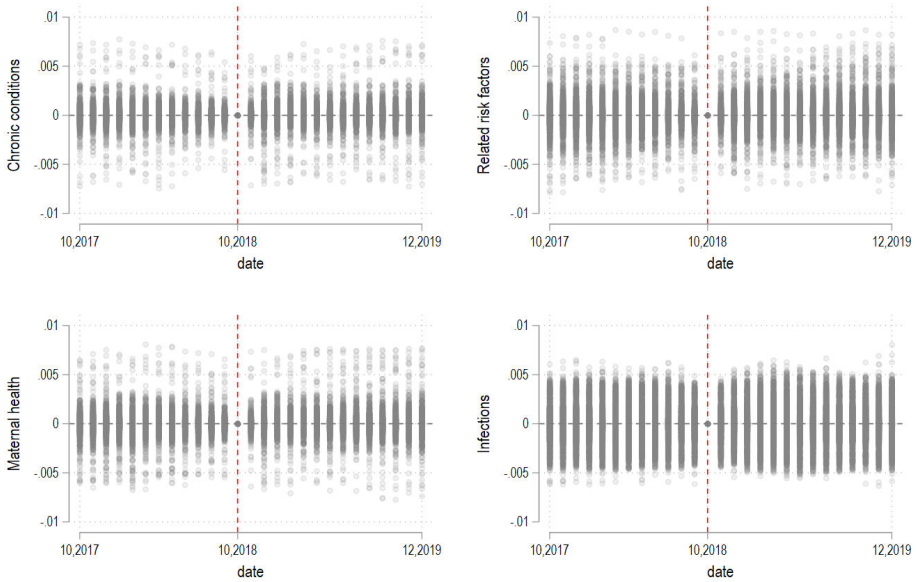


Figure C.21. The graph shows the differences between the estimated coefficients from the full event study and the leave-one-out analysis for the number of consultations performed in primary health care facilities in four different primary care categories. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. We follow equation 4.1, but use a binary treatment variable instead of a categorical one. Regressions include municipality, month-year and state times month-year fixed effects. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Discussion on Mechanisms and Health Systems Response: Additional Results

What Changed Within Primary Care Centers?

Share of Doctor Novices among PMM and no PMM Doctors

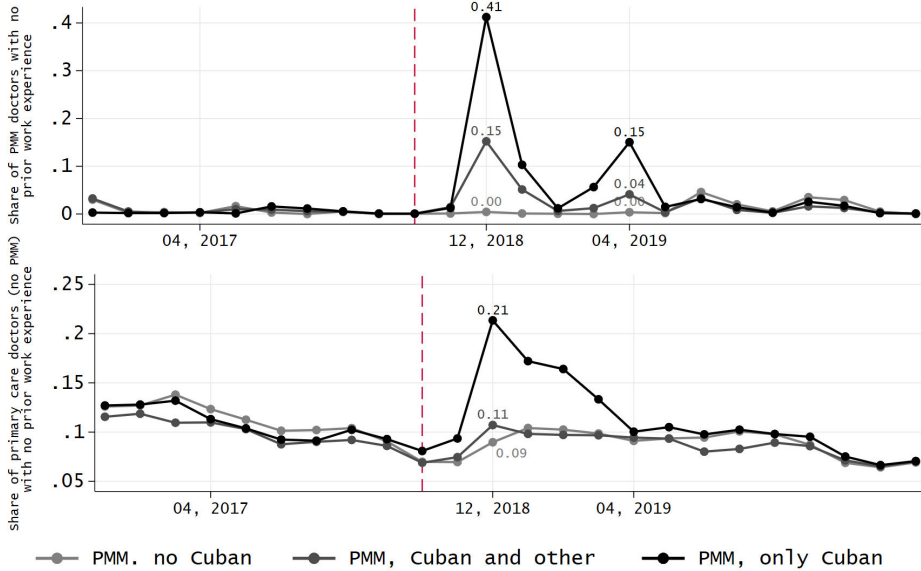


Figure C..1. The graph shows the share of PMM doctors with no prior work experience among all PMM doctors, calculated on the municipality-month level for all PMM-participating municipalities. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health and CNES.

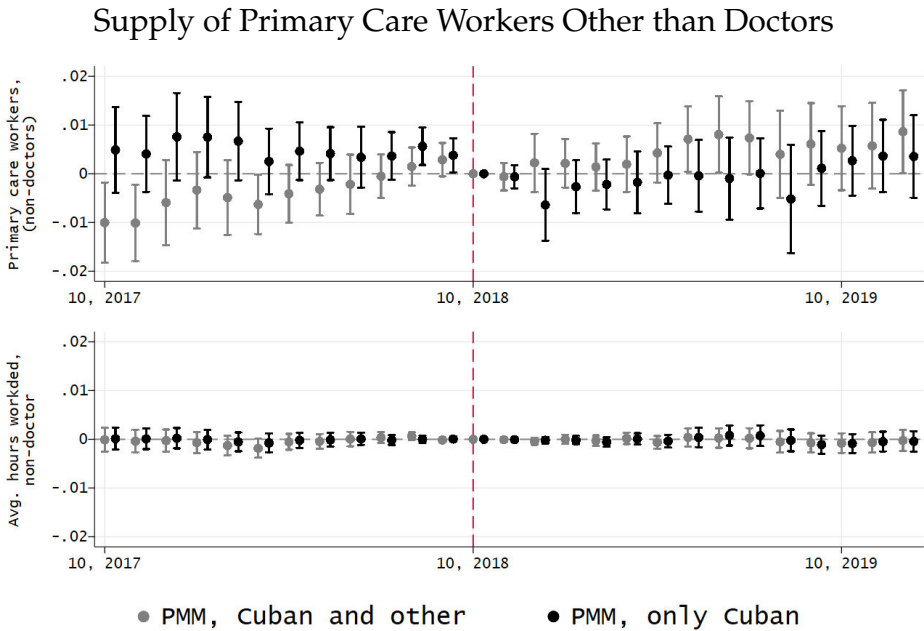


Figure C..2. The graph shows event studies for the the number and average hours worked of non-doctors (nurses and community health workers) following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health and CNES.

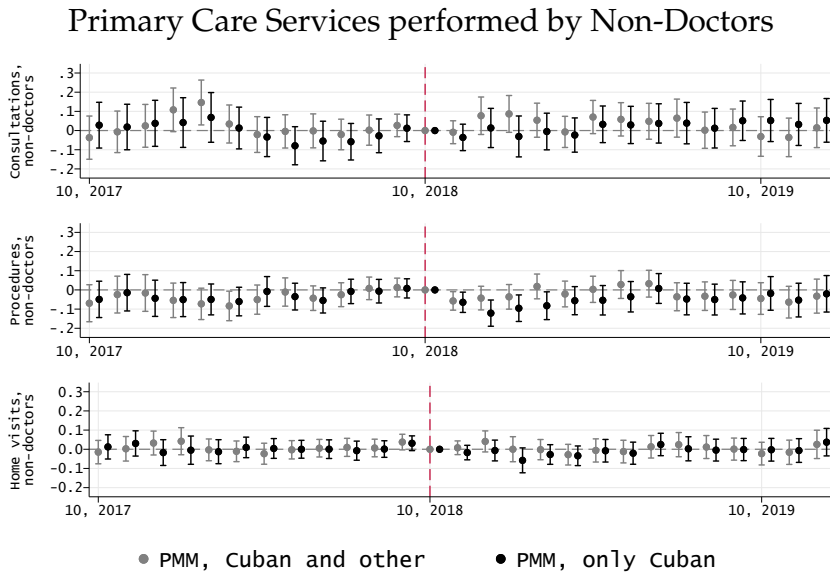


Figure C..3. The graph shows event studies for the number of consultations (upper panel), procedures (middle panel) and home visits (lower panel) performed in primary health care facilities by non-doctors, following equation 4.1 Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Health Service Delivery

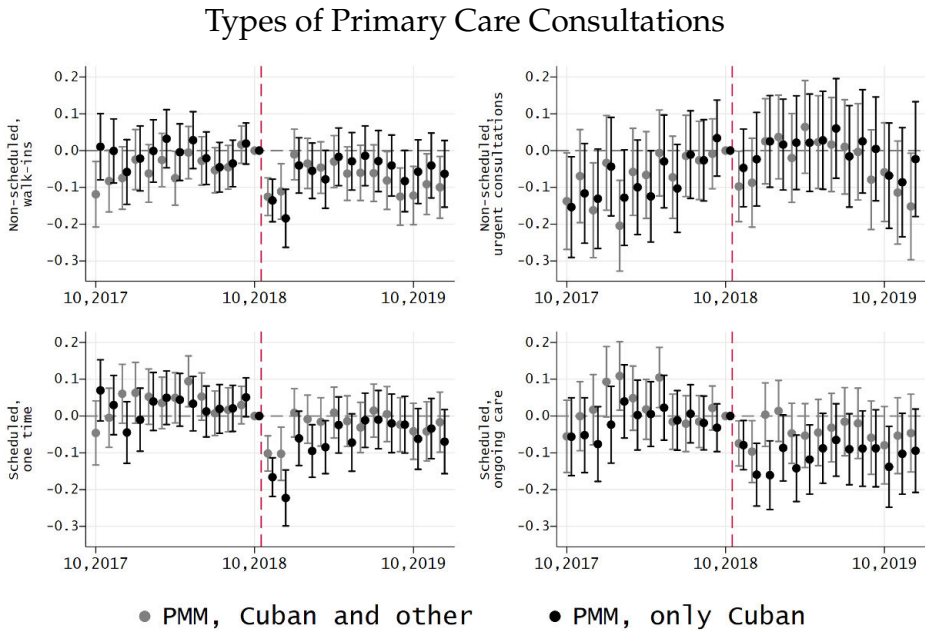


Figure C.4. The graph shows event studies for the number of consultations performed in primary health care facilities by the type of consultation, following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB and the Ministry of Health.

Value of Subsidized Medication Sales on the Municipality Level

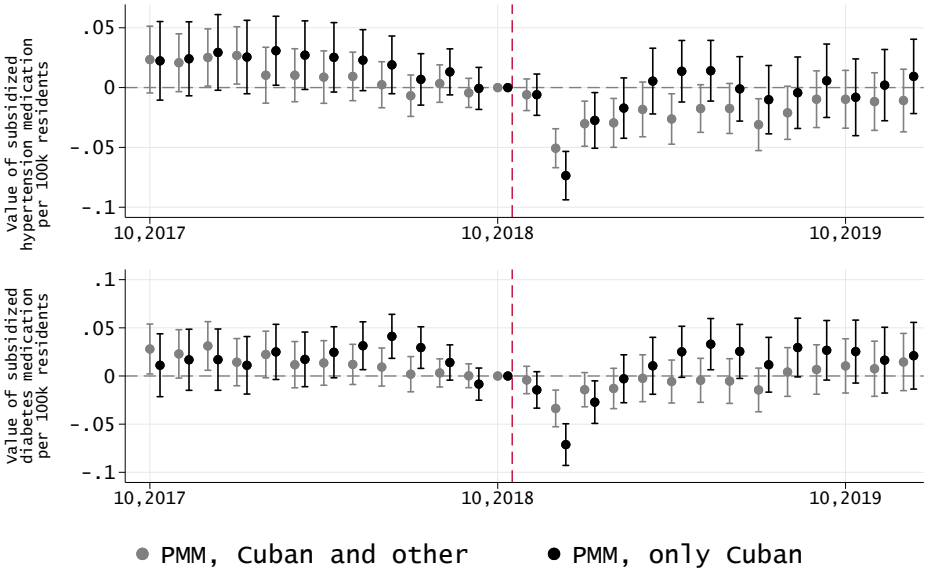


Figure C..5. The graph shows event studies for the value of prescription drugs for hypertension (upper panel) and diabetes (lower panel), distributed through *Aqui Tem Farmácia Popular*, following equation 4.1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the Ministry of Health.

What Changed Beyond Primary Care Centers?

Basic Ambulatory Care Treatments Outside of Primary Care Centers

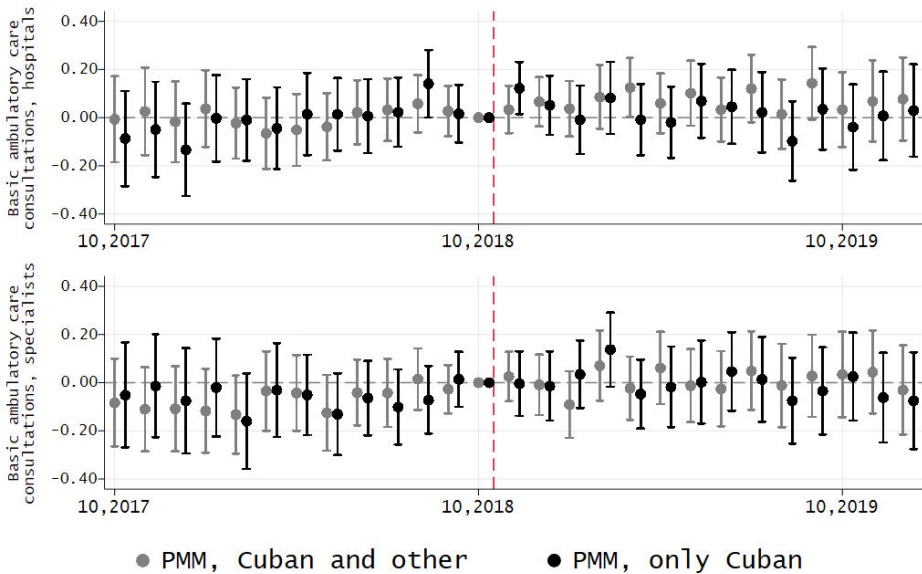


Figure C..6. The graph shows event studies for the number of basic ambulatory treatment at hospitals (upper panel) and specialists (bottom panel), following equation 1. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from the SIA/Datasus and the Ministry of Health.

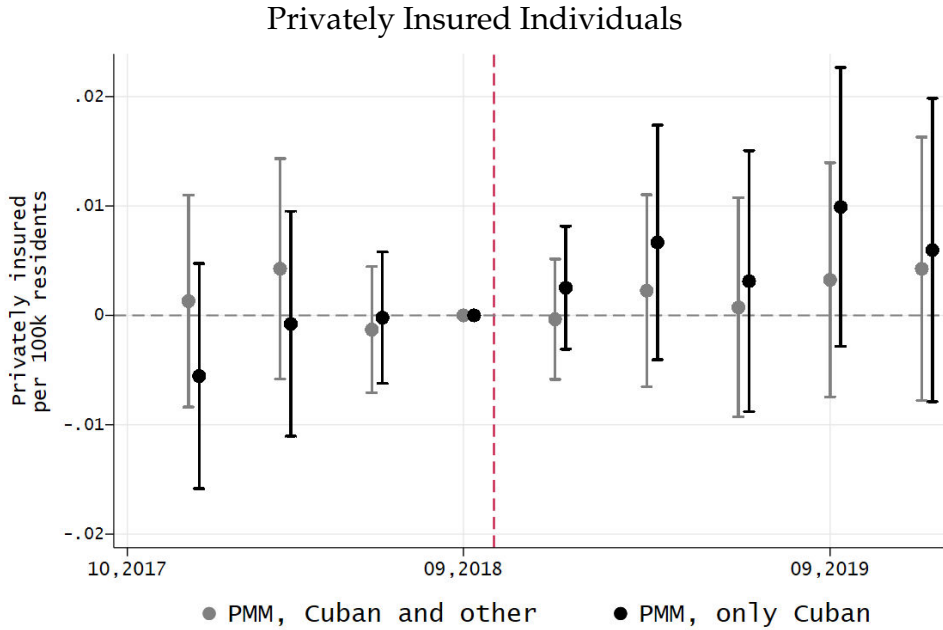


Figure C..7. The graph shows event studies for the number of privately insured people, following equation 4.1. The data is available every three months. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present September 2018, the last month with data availability before the treatment. Original data from ANS and the Ministry of Health.

Salaries of primary care and non-primary care doctors

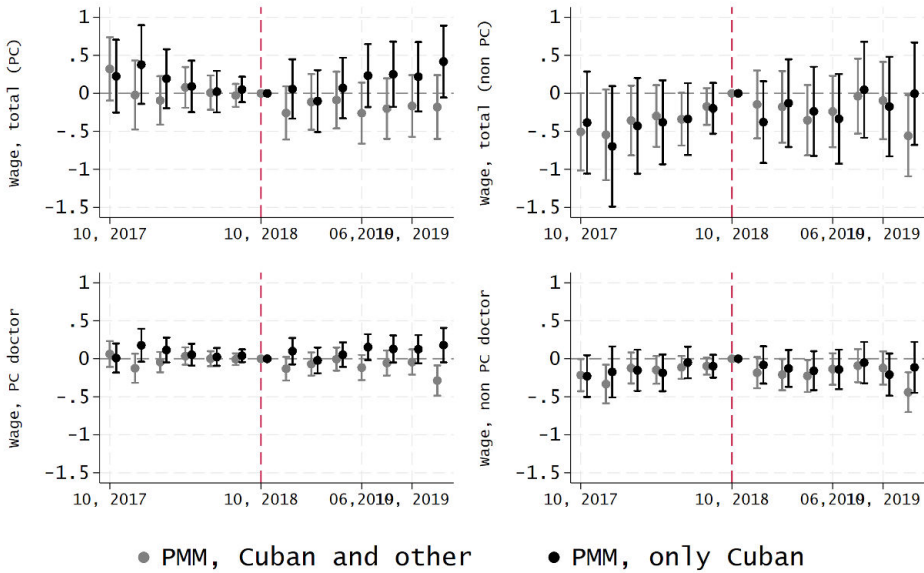


Figure C..8. The two left figures of the graph show bi-monthly event studies for the total amount of wages paid out to all primary care doctors (private and public sector) working per 100,000 residents (left upper panel) and the average wage received per primary care doctor (left lower panel) on the municipality level. The two right figures mirror the estimations for outcomes of non primary care doctors. Estimation results obtained by regressions following equation 4.1 with the outcomes transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from RAIS.

Salaries in the private and in the public health care sector

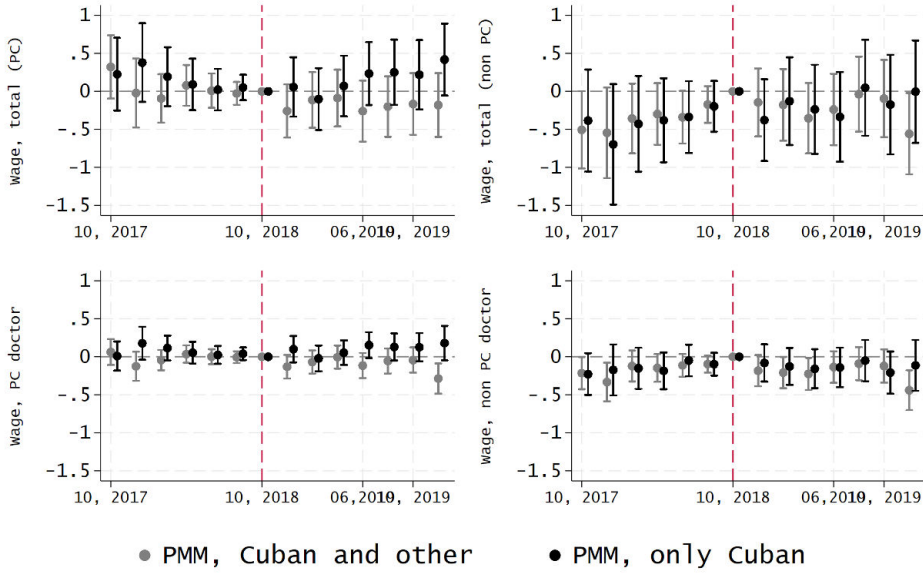


Figure C.9. The two left figures of the graph show bi-monthly event studies for the total amount of wages paid out doctors working in the public health care per 100,000 residents (left upper panel) and the average wage received per doctor in the public sector (left lower panel) on the municipality level. The two right figures mirror the estimations for outcomes of the private health care sector. Estimation results obtained by regressions following equation 4.1 with the outcomes transformed using the inverse hyperbolic sine. Regressions include municipality, month-year and state times month-year fixed effects. The series in grey presents treatment group 1, the series in black presents treatment group 2. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from RAIS.

Effect of Neighbors' Treatment on Primary Care Consultations on the Municipality Level

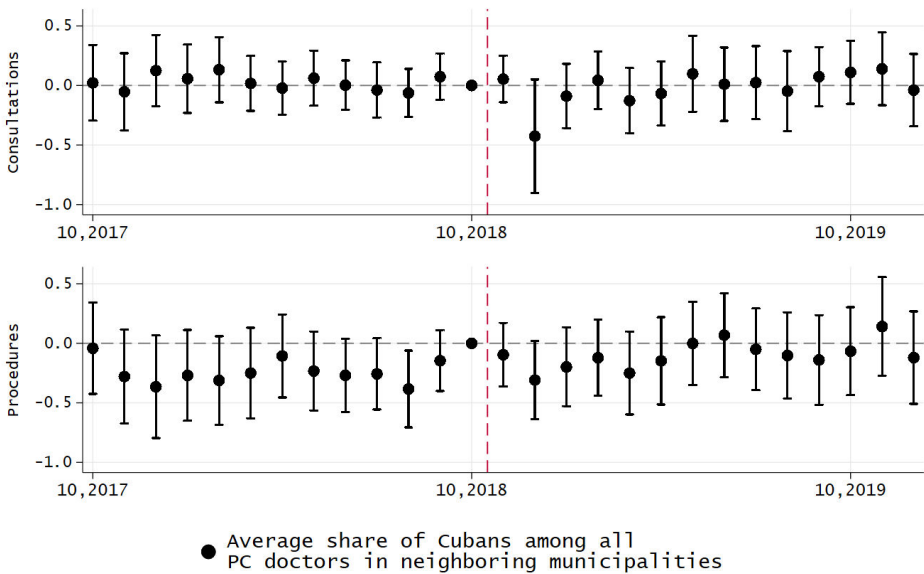


Figure C..10. The graph shows event studies for the effect of municipality i 's neighbors' average share of Cuban PMM doctors among all primary care doctors in October 2018 on municipality i 's primary care consultations (upper panel) and procedures (lower panel). Neighbors' average treatment status is calculated using a row-standardized contiguity matrix. Outcomes per 100,000 inhabitants and transformed using the inverse hyperbolic sine. Regressions include municipality, month and state times month fixed effects and control for municipality i 's own treatment. Whiskers around the point estimates present 95 percent confidence intervals, where standard errors are clustered at the municipality level. The dashed vertical lines present the pre-treatment month October 2018. Original data from SISAB, municipality shape files from IBGE.

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English summary

In an era of political polarization, democratic governments face increasing challenges to maintain public trust and legitimacy. These challenges are intensified in societies where economic disparities are stark, and where the perceived responsiveness of central governments to local needs differs across regions. The Political Economy of Democracies: Three Empirical Essays explores these dynamics by examining how government actions impact voting outcomes, health, and public trust across two different democratic contexts. Focusing on the cases of France and Brazil, this dissertation investigates the effects of specific government policies on voter behavior and public health outcomes, emphasizing the importance of policy responsiveness in providing adequate public services and sustaining institutional trust.

The dissertation comprises three empirical studies, each centered on the theme of government responsiveness within different social dimensions. Chapter 1 introduces the broader framework of the dissertation, situating it in the current literature on economics, political economy, public policy and government responsiveness. It outlines how political polarization has surged in recent years, especially in countries where inequalities are salient and persist – not only in terms of income but also in access to essential public services like healthcare and education – and increasing parts of the population feel disconnected from the political process and left behind by the economic one. The chapter argues that democratic governments need to address the growing divides through implementing policies that are not only effective but also perceived as equitable and locally relevant.

Chapter 2 explores voter disenchantment in France following the 2005 EU Constitutional Referendum. The study investigates how government actions—or inac-

tions—regarding European integration can significantly affect voting outcomes and public trust in democratic institutions. By focusing on the political fallout from the French government’s response to the referendum results – a majority of French voters rejected the proposed Constitution, but the French government disregarded the popular vote – this chapter highlights how issues of international integration can polarize public opinion and erode trust in political elites. The chapter shows that a municipality’s no-vote in the referendum correlates with decreased turnout in subsequent elections as well as an increase in the vote shares of far-left and far-right parties. These findings indicate that when democratic governments ignore popular sentiments on key issues, particularly those concerning economic inequality, national sovereignty and cultural identity, they risk deepening political disillusionment and fueling longer-term skepticism toward democratic institutions.

Chapter 3 shifts the focus to Brazil and investigates how policies aimed at addressing specific local needs impact voting outcomes. Specifically, it examines a healthcare program introduced by the Brazilian government to improve healthcare access in underserved municipalities. This chapter analyzes how such targeted interventions affect local voting outcomes. It is one of only few studies to causally link governmental healthcare interventions with national election results, revealing that improvements in healthcare services can boost political support for the implementing party. Municipalities participating in the policy increased their vote share for the policy’s political architects for the following two presidential election rounds. Political benefits were especially large in more disadvantaged municipalities and in the first out of two voting rounds typical for Brazilian elections, indicating that the party made ideological gains in these municipalities. In less disadvantaged municipalities, the impact was only significant for second voting rounds of the presidential election, potentially indicating support for the policy but not for the implementing party. The findings underscore the political benefits of addressing local needs, as citizens respond positively to policies that visibly enhance their quality of life and address their specific socio-economic challenges.

Chapter 4 examines the ramifications of doctor exit and turnover on local healthcare utilization, healthcare systems, and public health outcomes in Brazil, particularly examining the consequences of the exit of Cuban doctors who had been deployed in rural and underserved areas. This chapter provides a comprehensive

analysis of how sharp reductions in the supply and the subsequent turnover of primary care doctors impact healthcare utilization and public health outcomes. While local healthcare systems proved relatively flexible in terms of public health outcomes, primary care utilization suffered from major disruptions following the exit of the Cuban doctors. Although consultations on urgent conditions, e.g. infections and maternal health issues, recovered as soon as the Cuban doctors were replaced, consultation numbers on conditions requiring ongoing care, e.g. chronic diseases, remained strongly below the levels before the Cuban exit. This indicates that the large-scale exit of doctors required an adaptation of the healthcare system, prioritizing urgent and curative needs over ongoing care, with possible long-term consequences in terms of detection and adequate care for chronic conditions. Furthermore, the sudden exit disrupted Brazilian healthcare, leading to fragmented services and a misallocation of resources, as more people began using emergency rooms for primary care needs. The study thus demonstrates that government responsiveness—or lack thereof—in managing healthcare resources significantly influences the provision and utilization of essential public resources, with potentially long-term effects on local healthcare systems and wellbeing.

Taken together, these studies illustrate the complex interplay between government policy, public services, and institutional trust in democracies. The dissertation argues that democratic governments must prioritize locally responsive policies to foster trust and mitigate the impacts of inequality. It suggests that interdisciplinary approaches, combining insights from economics, political science, and public health, are essential to understanding the full scope of how (healthcare) policy interventions shape democratic societies. Through its focus on France and Brazil as case studies, this research contributes to the broader discourse on the sustainability of democratic institutions in the face of rising inequality and regional disparities, offering insights that could guide future policy designs in diverse democratic contexts.

Nederlandse samenvatting

In een tijdperk van politieke polarisatie staan democratische regeringen voor toenemende uitdagingen om hun legitimiteit bij het volk en het publiek vertrouwen te behouden. Deze uitdagingen worden versterkt in samenlevingen waarin economische ongelijkheden aanzienlijk zijn en waarin de waargenomen responsiviteit van centrale overheden op lokale behoeften verschilt tussen regio's. *The Political Economy of Democracies: Three Empirical Essays* onderzoekt deze dynamiek door te analyseren hoe overheidsoptreden stemuitkomsten, gezondheid en publiek vertrouwen beïnvloedt in twee verschillende democratische contexten. Door de casussen van Frankrijk en Brazilië te bestuderen, onderzoekt dit proefschrift de effecten van specifieke overheidsmaatregelen op het stemgedrag en de gezondheid van de bevolking, waarbij het belang van beleidsresponsiviteit in het leveren van adequate publieke diensten en het behouden van institutioneel vertrouwen wordt benadrukt.

Het proefschrift bestaat uit drie empirische studies, elk gericht op het thema van overheidsresponsiviteit binnen verschillende sociale dimensies. Hoofdstuk 1 introduceert het bredere kader van het proefschrift en plaatst het binnen de huidige literatuur over economie, politieke economie, overheidsbeleid en overheidsresponsiviteit. Het schetst hoe politieke polarisatie de laatste jaren sterk is toegenomen, vooral in landen waar ongelijkheden opvallend en persistent zijn – niet alleen in termen van inkomen, maar ook in toegang tot essentiële openbare diensten zoals gezondheidszorg en onderwijs – en steeds grotere delen van de bevolking zich losgekoppeld voelen van het politieke proces en zich achtergesteld voelen door het economische proces. Het hoofdstuk stelt dat democratische regeringen de groeiende kloven moeten aanpakken door beleid te voeren dat niet alleen effectief is,

maar ook als rechtvaardig en lokaal relevant wordt ervaren.

Hoofdstuk 2 onderzoekt de teleurstelling van Franse kiezers na het EU Grondwettelijk Referendum van 2005. Deze studie onderzoekt hoe overheidsoptredens – of het ontbreken daarvan – met betrekking tot Europese integratie de verkiezingsuitkomsten en het publiek vertrouwen in democratische instellingen aanzienlijk kunnen beïnvloeden. Door te focussen op de politieke nasleep van de reactie van de Franse regering op de resultaten van het referendum – waarbij een meerderheid van de Franse kiezers de voorgestelde grondwet verwierp, maar de Franse regering de volksstemming negeerde – benadrukt dit hoofdstuk hoe kwesties van internationale integratie de publieke opinie kunnen polariseren en het vertrouwen in politieke elites kunnen ondermijnen. Het hoofdstuk laat zien dat een ‘nee’-stem in het referendum in een gemeente correleert met een lagere opkomst bij latere verkiezingen en een stijging van het aantal stemmen voor extreemlinkse en extreemrechtse partijen. Deze bevindingen duiden erop dat wanneer democratische regeringen populaire opvattingen over belangrijke kwesties negeren, met name deze die betrekking hebben tot economische ongelijkheid, nationale soevereiniteit en culturele identiteit, zij het risico lopen politieke teleurstelling te vergroten en op de lange termijn scepsis ten opzichte van democratische instellingen te voeden.

Hoofdstuk 3 verplaatst de focus naar Brazilië en onderzoekt de invloed van beleidsmaatregelen gericht op specifieke lokale behoeften op de nationale verkiezingsuitkomsten. Specifiek wordt een gezondheidszorgprogramma onderzocht dat door de Braziliaanse overheid werd geïntroduceerd om de toegang tot gezondheidszorg in achtergestelde gemeenten te verbeteren. Dit hoofdstuk analyseert hoe dergelijke gerichte interventies lokale verkiezingsuitkomsten beïnvloeden. Het is een van de weinige studies die een causaal verband legt tussen overheidsinterventies in de gezondheidszorg en nationale verkiezingsresultaten en hierbij aantoonst dat verbeteringen in gezondheidszorgdiensten de politieke steun voor de uitvoerende partij kunnen versterken. Gemeenten die deelnamen aan het beleid verhoogden hun stemaandeel voor de politieke architecten van het beleid in de twee daaropvolgende presidentiële verkiezingsrondes. De politieke voordelen waren voornamelijk groot in meer achtergestelde gemeenten en tijdens de eerste van de twee stemrondes die typerend zijn voor Braziliaanse verkiezingen, wat aangeeft dat de partij ideologische winst boekte in deze gemeenten. In minder achtergestelde ge-

meenten was de impact alleen significant tijdens de tweede stemronde van de presidentsverkiezing, wat mogelijk steun voor het beleid zelf maar niet voor de uitvoerende partij aangeeft. De bevindingen onderstrepen de politieke voordelen van het aanpakken van lokale behoeften, aangezien burgers positief reageren op beleid dat zichtbaar hun levenskwaliteit verbetert en hun specifieke sociaaleconomische uitdagingen aanpakt.

Hoofdstuk 4 onderzoekt de gevolgen van het vertrek en verloop van artsen voor het lokale gebruik van gezondheidszorg, zorgsystemen en volksgezondheidsresultaten in Brazilië, met name door de gevolgen van het vertrek van Cubaanse artsen die in rurale en achtergestelde gebieden waren ingezet te analyseren. Dit hoofdstuk biedt een uitgebreide analyse van hoe sterke verminderingen in de beschikbaarheid van artsen en het daaropvolgende verloop van huisartsen het gebruik van gezondheidszorg en de volksgezondheidsresultaten beïnvloeden. Hoewel lokale zorgsystemen relatief flexibel bleken in termen van volksgezondheidsresultaten, leed het gebruik van eerstelijnszorg onder aanzienlijke verstoringen na het vertrek van de Cubaanse artsen. Consultaties voor urgente aandoeningen, zoals infecties en moeder- en kindzorg, herstelden snel zodra de Cubaanse artsen werden vervangen, maar het aantal consultaties voor aandoeningen die voortdurende zorg vereisen, zoals chronische ziekten, bleef aanzienlijk lager dan vóór het vertrek van de Cubanen. Dit wijst erop dat het grootschalige vertrek van artsen een aanpassing van het zorgsysteem vereiste, waarbij urgente en curatieve behoeften voorrang kregen boven voortdurende zorg, met mogelijke langetermijngevolgen voor de detectie van en adequate zorg voor chronische aandoeningen. Bovendien leidde het plotselinge vertrek tot fragmentatie van de Braziliaanse gezondheidszorg en een onjuiste allocatie van middelen, aangezien meer mensen de eerstehulpdiensten begonnen te gebruiken voor basiszorgbehoeften. De studie toont dus aan dat overheidsresponsiviteit – of het gebrek daaraan – bij het beheer van zorgmiddelen een aanzienlijke invloed heeft op de beschikbaarheid en het gebruik van essentiële openbare voorzieningen, met mogelijk langdurige effecten op lokale gezondheidssystemen en welzijn.

Gezamenlijk illustreren deze studies de complexe wisselwerking tussen overheidsbeleid, openbare diensten en institutioneel vertrouwen in democratieën. Het proefschrift betoogt dat democratische regeringen prioriteit moeten geven aan lo-

kaal responsief beleid om vertrouwen te bevorderen en de gevolgen van ongelijkheid te verminderen. Het suggereert dat interdisciplinaire benaderingen, die inzichten uit de economie, politicologie en volksgezondheid combineren, essentieel zijn om de volledige reikwijdte te begrijpen van hoe (gezondheidszorg)beleidsinterventies democratische samenlevingen vormen. Met de focus op Frankrijk en Brazilië als casestudy's, levert dit onderzoek een bijdrage aan het bredere debat over de houdbaarheid van democratische instellingen in het licht van toenemende ongelijkheid en regionale ongelijkheden, en biedt het inzichten die toekomstige beleidsvorming in diverse democratische contexten kunnen begeleiden.