Political Turnover and Fatal Government Transitions*

Helena Arruda† Rudi Rocha‡

July 31, 2023

Abstract

This paper investigates the effects of political turnover and government transitions on primary health services and infant health outcomes at the municipal level in Brazil. We employ an event-study in discontinuities approach exploring dynamic effects of variations in the margin of victory of candidates in close elections, focusing on elections from 2008 up to 2020 to identify causal political turnover effects. We document that political turnover leads to a temporary reduction in health service production. This effect is concentrated in the transition period and through the first months of the new government. We then assess whether political turnover and government transitions do have more far-reaching and persistent effects on health outcomes, beyond the transitory impacts on the provision of public services. We find that those exposed to government transitions in utero have worse outcomes at birth and experience an increase in the likelihood of mortality by 0.96 deaths per 1,000 live births, which corresponds to 6.6% of the mean infant mortality rate. We provide evidence that a reduction in the supply of healthcare workers is a relevant mechanism driving our results, especially in municipalities under fiscal incentives to reduce spending before the end of the electoral term.

JEL Codes: D72, H75, I12 I15.
Keywords: political turnover, government transitions, birth outcomes, infant mortality.

*We thank Claudio Ferraz, Cristine Pinto, Sergio Firpo and seminar participants at IEPS, FGV EAESP, RIDGE/LACEA Health Economics Network Workshop and PUC-Rio for their comments and suggestions.

†Institute for Health Policy Studies (IEPS), helena.arruda@ieps.org.br.

‡São Paulo School of Business Administration (FGV EAESP), rudi.rocha@fgv.br, and Institute for Health Policy Studies (IEPS).

1 Introduction

Political turnover and government transitions are intrinsic to democracies and can be instrumental to economic development. At the macro level, recent evidence indicates that national transitions may lead to policy change, improved governance, and reduced perception of corruption, consistent with new leaders exerting more effort due to stronger reputation concerns (Marx, Pons, and Rollet, 2022). Recent evidence at the micro level, however, indicates that political turnover and government transitions may also come along with adverse consequences on public service provision, for instance, through politically motivated replacement of public employees and disruption of service delivery (Akhtari, Moreira, and Trucco, 2022; Iyer and Mani, 2012). Transition effects may start even before the new administration takes office, during a period when, after defeat, the incumbent's political incentives may harm the quality of public services (Toral, 2023). Yet, the time span between elections and the inauguration of new governments is typically short, and very little is known about whether political turnover and government transitions do have more far-reaching and persistent, rather than transitory effects in the provision of public services and in development outcomes at the micro level. More generally, empirical evidence on the social costs of transition processes is still scant.

In this paper, we assess the effects of government transitions on health services and health outcomes by using data from Brazilian municipal elections. We focus both on the transition period and on the first year of the newly elected government. We first investigate and fully characterize whether and how political turnover leads to detrimental short-term impacts on primary healthcare production, particularly inspecting the timing of the effects on services and outputs related to maternal and infant health. This allows us to precisely map service provision fluctuations during the gestational period, and identify a critical period of exposure to transitions. We are then able to assess whether transitions affect birth outcomes of those exposed in utero, potentially leading to irreversible effects on infant mortality throughout the first year of life.

Brazilian local elections provide a unique context for this analysis. First, the Brazilian health system is decentralized, giving considerable discretion to mayors to influence the provision and the quality of healthcare services at the local level. Second, the transition period between electoral results and when the newly elected mayor takes office in Brazil consists of around 65 days on average (on occasions when only one round is needed), which is similar to the average length of transition periods from other countries.[1]

We build a comprehensive panel at the municipality-by-month level by using administrative microdata from multiple sources. To estimate the causal effects of political turnovers on health services and outcomes, we rely on a generalization of the difference-in-discontinuities design from Grembi, Nannicini, and Troiano (2016), and similar to Szerman (2022). In this way we combine the sharp cross-sectional variation in turnover from the discontinuity around the margin of victory, with municipality and time fixed-effects plus additional controls, thus absorbing differences in levels and common time trends. Further restricting the analysis to close elections means that we only consider elections where the incumbent barely won or lost to the strongest challenger candidate. This strategy avoids the bias that would arise from simply comparing municipalities that experienced turnover with those that did not – as they could differ in important observable or omitted ways that could, in turn, affect the likelihood of turnover. The main assumption underlying this strategy is that political

[1] In a sample of 20 democracies, Toral (2023) documents an average length of 49,5 days between the day elections are held and when the winner takes office.

turnover should be as good as random in close elections. We provide evidence in this regard.

We find that political turnover is associated with temporary but sharp reductions in healthcare service production. Right after elections happen, in October of the electoral year, we observe a reduction of prenatal consults per month in municipalities that experienced turnover relative to those that did not. This negative shock is transitory, dissipating after a few months by the beginning of the new government. The difference reaches the lowest at -2.9 percentage points in December, which corresponds to 3.2% of the mean. We observe a similar pattern regarding the share of pregnancies that end up with the adequate total number of prenatal consults (decrease of 4.7 percentage points) and the share of pregnant women undergoing tests for syphilis and HIV (reduction of 4 percentage points) in the quarter following elections.

In principle, transitions may bear only transitory costs through an adaptation process, diluted over time. This might be the case through the replacement of professionals of the same quality or temporary operational disruptions without lasting impacts. More generally, this might be the case should government institutions and routine public service operations remain immune to transitory changes in personnel and their incentives. Yet, existing evidence documents detrimental effects on service delivery. Ronfeldt, Loeb, and Wyckoff (2013) and Akhtari et al. (2022), for instance, provide evidence of disruption in the provision of schooling and adverse effects on educational outcomes associated with the replacement of bureaucrats of different quality, as well as by losses due to the lack of on-the-job knowledge by the newly hired employee. Adverse effects on educational outcomes, however, seem to fade away over time (e.g. as in Akhtari et al., 2022). Still, although government transitions are transitory by nature, their effects may be particularly pervasive should they coincide with critical periods for human capital and economic development. In that case, variations in service provision – either transitory because of personnel turnover or due to specific disruptions – may lead to far-reaching detrimental consequences. We conjecture that this should be especially the case for health services.

In order to investigate that conjecture, we start by defining a critical period, when exposure to turnover effects can be potentially harmful to birth outcomes and child development. To do so, we overlap the timing in the gestational stages with the observed timing in service reduction, thus comparing gestations exposed versus not exposed to transitions. In particular, existing evidence suggests that the second trimester of gestation is especially sensitive to shocks, which may lead to long-term developmental consequences (e.g. as in Black, Bütikofer, Devereux, and Salvanes, 2013; Rocha and Soares, 2015; Schwandt, 2018). Given the richness of our data, we are able to exploit fine-grained variation in exposure to transitions at the monthly level, thus assessing impacts within the gestational period.

We find a reduction in the average weight at birth and an increase in the share of very low-weight births for those exposed during the second trimester of gestation. Importantly, previous studies have shown that low birth weight may lead to impaired cognitive development in the long-run (Linnet, Wisborg, Agerbo, Secher, Thomsen, and Henriksen, 2006; Mara, 2003; Shenkin, Starr, and Deary, 2004) and also have long-term effects on education (Almond and Currie, 2010). Therefore, even those children that survive may face adverse conditions and future impairment as indicated by other birth outcomes.

We complete our analysis by examining a critical and irreversible outcome, infant mortality rates. We find an increase of 0.96 deaths per 1,000 live births in infant mortality rates (up to one year of age)

for those exposed in utero during the second trimester of gestation. This effect is sizable, representing 6.6% of the mean. Moreover, when considering deaths caused by preventable causes (amenable to healthcare) the effect is even larger, an increase of 1.0 deaths per 1,000 live births, corresponding to 11.8% of the mean. We compare this result to that found by Bhalotra, Rocha, and Soares (2016). The authors find that universalizing access to health in Brazil led to a drop of 1.72 deaths per 1,000 live births after two years of intervention, representing 9.4% of the average infant mortality rate at baseline. Therefore, a large scale and successful restructure of the entire Brazilian health system had a similar magnitude of impact on infant mortality rates after two years as did a short-lived government transition period. Given the timing of the gestational period and child development, the effects on mortality rates manifest solely during the new administration, throughout the first two years after the change in government has occurred. We assess robustness in a series of additional exercises, in particular by inspecting pre-trends just before elections and in non-electoral years, as falsification tests. To assess the external validity of our findings, we ask whether the effects of turnover hold only locally for close elections or, more broadly, for others that were less close (e.g. as in Marx et al., 2022). Differences in the margin of victory between incumbent and challenger candidates could be related to factors such as candidate quality. For instance, weak incumbents already expecting to be defeated could engage in different strategic behavior in the months preceding and after the elections (Marx et al., 2022). Even if we do not observe the quality of the candidates, if it does not change discontinuously around the cutoff, it should not be a concern in terms of identification. However, it could be the case that the negative results only hold when elections are close. We present robustness exercises in the appendix that show evidence of similar effects in both close and not close elections.

Next, we explore the underlying mechanisms behind our results. In particular, we exploit the fact that the provision of primary health services relies to a great extent on the supply of human resources. We analyze the flow of hirings and dismissals in the healthcare sector, distinguishing between civil servants and temporary workers. We find that during the electoral month, there is a 3% decrease in hirings for temporary workers. On the other hand, there is a 9% increase in dismissals among healthcare personnel under civil servant contracts during the same time, including those with and without contractual stability. To further understand these effects, we divide the sample of civil servant contracts into three common occupations in primary health: physicians, nurses, and community health workers. We find that the effects are present in all occupations, but seem to last the longest among physicians.

What could explain the patterns in dismissals, particularly among physicians? As theoretically conjectured by Toral (2023), who also investigates personnel turnover, incumbent losers have fewer incentives to ensure the delivery of services to the population, which is especially salient in the transition period. In particular, during this time politicians may become increasingly concerned about their future, fearing prosecution for wrongdoings while in office or simply start slacking on the job. Indeed, recent evidence has shown that losing office increases politicians' odds of being prosecuted and convicted (Lambais and Sigstad, 2023).[2]

In an effort to shed light on the incumbent incentives, we perform an additional heterogeneity analysis. According to the Fiscal Responsibility Law (Brasil, 2000), mayors have to observe a cap of 60% of spending on personnel relative to total revenue. Consequently, if the incumbent were close to

[2]Looking into court cases against official misconduct involving local politicians in the trial courts of the Brazilian state and federal judiciaries, the authors find that candidates that barely lose an election have a 17 percent conviction rate, compared to 6 percent for those that barely win. This could be due to favor exchanges and career concerns.

the cap during the election year, she would have incentives to clear the accounts before stepping out of office. We then split the sample between municipalities below and above the median share of revenue spent with personnel. Results show that municipalities close to the cap dismiss more workers under civil servant contracts – especially those that do not have contractual stability or tenure. This could explain why dismissals are particularly higher among physicians, as their wages represent a relatively higher cost within personnel spending.

This paper relates to a scant stream of research on political turnover and government transitions. Most of the existing literature focuses on how political turnover impacts bureaucratic turnover. Iyer and Mani (2012) were among the first to do so empirically, finding that politicians in India affect the process of bureaucrat assignment and that political turnovers are associated with significant increases in the probability of bureaucrats being reassigned. With a similar design but focusing on the Brazilian case, Akhtari et al. (2022) documents the presence of upheavals in municipal bureaucracy associated with party turnover at municipal elections. Moreover, the authors extend their analysis to disentangle the effects of public service provision in education. They find that increases in the replacement of personnel in schools were associated with a reduction in the quality of public education, measured by lower test scores in the following years. This result is relevant insofar as it helps to link what happens to the bureaucracy with outcomes that most directly affect citizens. Toral (2023) also studied political turnover in Brazilian municipal elections and was the first to look at transition periods, focusing on the composition of the bureaucracy and healthcare service delivery. The author finds that the incumbent's electoral defeat causes bureaucracy rearrangements and reductions in healthcare service delivery during the transition period.

We contribute to the literature by providing novel micro evidence on the social costs of transition processes. We document far-reaching and persistent, rather than transitory effects in the provision of public services and in development outcomes. While transitions may lead to temporary service disruption, as documented by Toral (2023), or may have transitory impacts on reversible outcomes, as documented by Akhtari et al. (2022), we show that adverse impacts may arise and be irreversible should the transition timing affect specific services and coincide with critical periods for human capital and economic development. In this way we complement Marx et al. (2022) by revealing, at the micro level, adverse social costs of political turnovers.

We also advance the literature on the impacts of political turnover and government transitions by providing a comprehensive analysis and detailed timing of the effects, covering both pre- and post-electoral periods as well as the first years of the new government. This not only allows us to precisely map critical periods and reveal persistent effects on child outcomes, but also enables us to provide a thorough characterization of transition processes at a very micro level. We show that much of the variation in the bureaucratic turnover starts with dismissals before the new government takes office and responds to spending incentives within the political cycle. In this way, we complement both Akhtari et al. (2022) and Toral (2023). We show that the observed bureaucratic turnover and hirings in the new government, as documented by Akhtari et al. (2022), may be actually responding to vacant positions left by the previous administration, and to fiscal incentives faced by the defeated incumbent, as conjectured by Toral (2023).

Finally, we examine specific mechanisms, thus leading to relevant policy implications. We first show that personnel dismissals are a relevant lever – the timing of the decline and recovery in service production and in birth outcomes is associated with the timing of personnel turnover, especially of

physicians. We then show that this lever is used as a response to fiscal incentives. Importantly, both the lever and the incumbent's incentives are subject to change through regulation aiming at smoothing bureaucratic turnover and protecting service delivery. In this way, by regulating government transitions at the micro level, societies may benefit the most from the positive impacts that political turnover may have at the macro level, as documented by Marx et al. (2022).

The remainder of the paper is organized as follows. Section 2 describes the institutional context. Section 3 presents the data and shows descriptive statistics. Section 4 details our empirical strategy. Section 5 presents the main results, while Section 6 discusses mechanisms. Section 7 concludes.

2 Institutional Background

2.1 Elections in Brazilian Municipalities

Municipalities are the smallest administrative units in Brazil, and are governed by mayors, who are elected in municipal elections held every four years on the same day (first Sunday of October) for the entire country. For most municipalities, these elections only last one day. For a small fraction, however, runoff elections can be held on the last Sunday of the month if no candidate obtains an absolute majority in the first round. Only municipalities with over 200,000 inhabitants need to obey this rule, corresponding to a small share of the total (in 2016, fewer than 2% of the total number of municipalities).

Incumbent mayors are eligible for reelection only once. Even though it is possible for a mayor already elected twice to run again after an electoral cycle out of office, very few return after their second term (Ferraz and Finan, 2011). Elections happen in October in the election year, and the winner takes office in January of the following year (Figure A.1 illustrates elections timeline). There is therefore a period of roughly three months of transition during which the defeated incumbent is still governing.

2.2 Delivery of Health Care Services

In 1988 the Brazilian Constitution created the Unified Health System (SUS), which was implemented in the following years and reorganized the Brazilian public health sector. SUS follows a national health system model (similar to the NHS in the UK), and a decentralized organization of service delivery.

Municipalities, in particular, are responsible for the provision of primary care services. SUS shifted healthcare provision from a centralized model based in public hospitals in urban centers to a decentralized one, where the first point of contact between citizens and the public health system takes place in local communities, within municipalities. Services are mainly delivered through the Family Health Program (FHP), the leading model of primary healthcare delivery and which covers around 65% of the Brazilian population (Mrejen, Rocha, Millett, and Hone, 2021; Bhalotra et al., 2016). Under this arrangement, municipalities have autonomy to provide primary care, and typically do so either in small clinics called Basic Health Units (UBS) or through household visits within their catchment areas. Health care provided in a typical UBS covers a range of services, from preventative health to minor procedures and exams. The main frontline workers at this level of care are physicians, nurses, and community health agents (Mrejen et al., 2021). These workers can be employed under civil service contracts – most of which give them employment protections and career stability – or other types of

contracts, such as temporary and formal work contracts (CLT). Despite some restricting rules, mayors and high-level bureaucrats have large discretion over hirings and dismissals.[3]

Human resources are the main input to primary healthcare production and the largest spending category in the municipal health budget (Medeiros, Albuquerque, Tavares, and Souza, 2017). Yet, there are limits mayors must obey regarding the financing of personnel in order to meet fiscal compliance. In particular, the Fiscal Responsibility Law (LRF) determines that municipalities must spend less than 60% of their revenue on personnel (Brasil, 2000). Those not complying, or close to the cap, might have different incentives regarding their spending relative to those that have spent less, especially in electoral years (Sakurai and Menezes-Filho, 2011; Szklo, 2022).

2.3 Primary Care and Maternal Health

One of the main focus of municipal primary care services is the delivery of maternal and infant health care, in particular prenatal consults, which should begin as soon as the mother starts the gestational period. The Ministry of Health recommends at least six prenatal consults, the first being in the first trimester of gestation, for an adequate following of the pregnancy. Prenatal consults are crucial for, among others, identifying diseases the mother might have that are detrimental to the baby's health if undiagnosed and untreated. Syphilis, a sexually transmitted disease that may lead to morbidity and mortality, is one example. Infected mothers can transmit the disease to their fetuses, with vertical transmission depending mainly on factors such as the maternal stage of syphilis and the duration of exposure in utero (Rocha, 2020). If detected in time, inexpensive medication can avoid vertical transmission with a success rate estimated at 98% (CDC, 2017).

A nationwide study has found that most mothers in Brazil (89.6%) had their prenatal care consults in the public system, mainly at municipal UBS (Viellas, Domingues, Dias, Gama, Theme Filha, Costa, Bastos, and Leal, 2014). The survey shows that almost 90% of them report having had their prenatal consults with the same health professional throughout the entire gestational period. The evidence also indicates that forming an emotional bond and a relationship of trust are important determinants of mothers' adherence to adequate prenatal care. Municipal primary care services and their health teams therefore play a crucial role in the delivery of maternal and infant health care. Overall, these services have been responsible for sizable reductions in infant mortality rates over the last decades in Brazil (Mrejen et al., 2021; Bhalotra et al., 2016).

3 Data

We gather data from multiple sources, divided into five broad sections: electoral, health, human resources, finance and public spending, and sociodemographics. All data described here are publicly available.

[3]For instance, to be hired, civil servants must take public exams. They can be hired if ranked high. However, even though the mayor cannot change their ranking, they can choose both the timing and the number of individuals to be hired. Recent evidence indeed suggests that, even for civil servants, the hiring process might not be as insulated from political influence as commonly assumed (Toral et al., 2020).

3.1 Electoral Data

Our study covers the 2008, 2012, 2016, and 2020 electoral cycles. We obtain all the information regarding elections from the Superior Electoral Court's (TSE) repository. We focus on mayoral races, and use candidates' data to find the identity of the incumbent mayor and also who were the candidates that ran in each electoral period. We analyze candidates rather than party turnover since Brazilian local politicians have weak partisan attachments (Boas, Hidalgo, and Melo, 2019). We also use data on election results, particularly votes, to build the share of votes received by each candidate in each municipality as a fraction of all votes cast. This information, coupled with the identity of the incumbent in each municipality, is used to build the margin of victory of the most voted challenger candidate, and identify close elections and electoral turnover.

3.2 Health Data

We build health production indicators from the Primary Health Care Information System (SIAB, Ministry of Health), which covers all primary health care production delivered by family health teams and community health agents. This data set is available monthly from January 1998 to December 2015. We use information from 2007 to 2014. The variable used from SIAB is the share of pregnant women who underwent a prenatal consult with a doctor or nurse in a health facility in a given month. This is obtained by dividing the number of pregnant women that had prenatal consultations in the month by the total number of pregnant women recorded in the system, by municipality of residence.

After the discontinuation of SIAB in December 2015, the federal government launched the new System of Primary Health Care Services (SISAB), implemented in 2018. The system contains data on primary health care service production at the quarterly level. From this source we use the share of women that gave birth in a given quarter with at least six prenatal consults, the first one having occurred up to the 12th week of the gestational period, and the share of women that gave birth in a given quarter that underwent tests for syphilis and HIV. Further, we add a monthly variable on the number of gestations that ended up with six or more consults by month of the first consult.

We use vital statistics to construct birth outcomes. The National System of Mortality Records (SIM, Ministry of Health) provides detailed information on every officially registered death in Brazil. It also covers fetal deaths (those before the fetus was expelled or extracted from the mother's body). We select all deaths of children up to one year of age (infant deaths). We gather information on the municipality of residency, the dates of birth and death, and the cause of death using the ICD-10 classification (International Classification of Diseases 10th revision). Regarding the cause of death, we separate deaths into preventable and non-preventable causes. Deaths by preventable causes are those that could be avoided by timely and adequate access to healthcare.[4]

The National System of Information on Birth Records (SINASC) provides detailed information on all registered births in Brazil. We select data on the municipality of residence of the mother, the exact date of birth, the weight of the child at birth, and the number of prenatal consults the mother had during the gestational period. We also construct a measure of premature births defined as those that happen before 37 weeks of gestation, a measure of low birth weight defined as those below 2,500 grams, and a measure of very low birth weight defined as those below 1,500 grams.

We merge information from SIM and SINASC to build a municipality-by-month of birth panel

[4]We use Nolte and McKee (2004)' classification of preventable causes as reference.

for the 2007-2018 period. In this period, the microdata make a total of 35,015,775 registered births and 475,010 infant deaths. We calculate infant mortality rates up to 1 year of life by municipality of residence and month of birth following Rocha and Soares (2015).

3.3 Human Resources

We gather information on hirings and dismissals from the Annual List of Social Information (Rais), which contains data on all formal employees.[5] We use the microdata to select contract start and end dates, type of contract, and the reason of dismissal. We distinguish between civil servants and temporary employees, and consider only terminations conducted by the employer. We restrict our analysis to healthcare workers with selected occupations (physicians, nurses and community health workers) to build a municipality-by-month panel of hirings and dismissals for the 2011-2018 period. We consider not the municipality where the individuals live, but the municipality where they work.[6]

3.4 Finance and Public Spending

We use annual public spending data at the municipality level from the Brazilian Finance System (FINBRA), covering the period of 2008 up to 2019. FINBRA provides data on total public spending, and spending by aggregated categories, such as health and sanitation, education and culture, among others, as well as data on taxes and revenues. We create indicators for LRF compliance by assessing the distance of municipalities' spending on personnel to the 60% cap.

3.5 Sociodemographics

Most of the sociodemographic information at the municipal level comes from IEPS Data, a portal that covers official data from multiple sources. We select variables such as population, the share of people with sewage systems in their homes, and the Human Development Index. Information on Covid-19 cases and deaths were extracted from Cota (2020)'s repository.

3.6 Sample Selection and Summary Statistics

In order to select the municipalities in each electoral cycle, we follow three main steps (as in Akhtari et al., 2022). The first step consists of dropping municipalities with irregular (supplementary) elections. This means dropping 118, 111, 172, and 58 municipalities in 2008, 2012, 2016, and 2020, respectively. Second, we drop municipalities eligible for elections in two rounds (those above the threshold of 200,000 inhabitants). This led to the exclusion of 78, 83, 92, and 95 municipalities, respectively.[7] Third, we restricted the sample to municipalities where the incumbent ran for reelection and had at least one challenger (so the incumbent was in office for only one term and chose to run for reelection). Since most incumbents try to reelect themselves, this leaves us with approximately half of the remaining municipalities in the sample for each electoral period.

[5]Rais is a comprehensive dataset that is periodically updated. All formal employers are legally required to report their contracts annually to the Ministry of the Economy.

[6]We choose to start collecting data in 2010 (thus excluding the 2008 elections) because in 2008 there was a change in the Classification of Occupations (CBO) in the Rais data.

[7]Primary healthcare is especially important in smaller municipalities. The average coverage for PSF teams in 2020 was around 50% for municipalities above the threshold of 200,000, and 87% for those below that (own calculations made with official microdata available at IEPS Data).

We create a variable t that enumerates the months within each electoral cycle. We consider that each electoral cycle starts one year before the election year and lasts for four years. As an example, for the 2008 elections we begin with t = 1 in January 2007, and end with t = 48, four years later, in December 2010. For the analysis using quarterly SISAB data the variable enumerates quarters instead of months, going from t = 1 up to t = 12. Our final data set consists of a panel at the municipality-by-month or quarter level covering all electoral cycles between 2008-2020. For our most complete specification, with outcomes constructed from SIM and SINASC, the panel runs from 2007 up to 2018, covering three electoral cycles (2008, 2012, and 2016).[8]

Finally, our main specifications consider close elections only. In this case, in order to further restrict the sample, we first stack the data across all electoral periods, after selecting the sample through the three steps discussed above. We then estimated the optimal bandwidth by using the method proposed by Calonico, Cattaneo, and Titiunik (2014) and triangular kernels for each outcome of interest. To ensure consistency, we apply a fixed bandwidth of 0.14 to the entire sample, taking into account the optimal bandwidth of 0.142 for infant mortality rates as a reference. Importantly, the average of all optimal bandwidths, across all outcome variables used in our analysis, is 0.135, which lends further support to our chosen bandwidth of reference. We provide additional robustness checks in Appendix F, where we replicate our analysis for all outcomes using their respective optimal bandwidths, vary the kernel function (including triangular, uniform, and Epanechnikov kernels), and also replicate the results without restricting to close elections (reported in Appendix C). Panel A of Table 1 shows the number of municipalities that remain per election, in close elections, distinguishing between those that experienced turnover and those where the incumbent was reelected. Our main sample ends up with 2,900 municipalities in 2008, 2,200 in 2012, 2,395 in 2016, and 3,040 in 2020. It is possible that a municipality appears once, twice, or three times in the panel. In practice, we have 2,606 municipalities that appear once, 2,155 that appear twice, and 193 that appear in all three electoral cycles.

In sum, we stack electoral cycles, each of them divided into blocks of four years and representing forty eight months. Panels B to E of Table 1 present summary statistics at baseline (defined the year before elections) for municipalities that experienced turnover and municipalities that did not, in close elections. Column 3 displays the estimate corresponding to the coefficient on turnover using variables at baseline as dependent variables, and Column 4 shows the correspondent p-values. Overall, we see no evidence of statistically significant differences. Finally, Column 5 displays the data source of each variable.

[8]It was not possible to include the 2020 elections since we would need data for 2021, not available from SIM and SINASC at the time our analysis was completed. Additionally, elections in 2020 took place in November due to Covid-19 restrictions. Analysis on hirings and dismissals cover only two electoral cycles (2012 and 2016). We had to exclude 2008 due to a change in the classification of occupation codes in that year.

Table (1) Descriptive Statistics and Test for Discontinuity in Baseline Characteristics

	(1) Turnover	(2) No Turnover	(3) Estimate	(4) p.value	(5) Source
Panel A: Municipalities (N)					
2008	978	1,922			
2012	964	1,236			
2016	1,274	1,121			
2020	1,067	1,973			
Panel B: Municipal Characteristics					
Population	21257.53	19341.49	57.80	0.68	IBGE
MHDI	0.65	0.65	0.00	1.00	IEPS DATA
IDEB, 9° year	4.17	4.22	-0.01	0.88	IEPS DATA
IDEB, 5° year	5.20	5.26	-0.01	0.86	IEPS DATA
Rural Pop. (%)	37.81	38.53	0.00	1.00	IEPS DATA
Sewerage (%)	36.65	37.38	0.00	1.00	IEPS DATA
Life Exp (%)	72.77	72.81	0.00	1.00	IEPS DATA
PIB per capita	20.95	23.78	-0.30	0.69	IEPS DATA
Income per capita	490.59	495.70	0.00	1.00	IEPS DATA
Panel C: Health Outcomes					
Pregnancies with Prenatal Consults in Month, (%)	91.72	91.83	1.55	0.28	SIAB
Gestations ending with 6 plus consults by month of first prenatal consult, (N)	4.53	4.33	-0.12	0.90	SISAB
Pregnant women undergoing tests for Syphilis and HIV (%)	28.22	30.15	0.99	0.67	SISAB
Pregnancies with adequate n° of Prenatal Consults (%)	27.44	26.98	1.49	0.49	SISAB
IMR	14.81	15.47	1.78	0.30	SIM/SINASC
IMR preventable	9.05	9.19	1.55	0.27	SIM/SINASC
IMR non-preventable	5.77	6.29	0.23	0.82	SIM/SINASC
Low Weight, (%)	7.44	7.31	0.32	0.39	SINASC
Very Low Weight, (%)	1.05	1.02	0.04	0.74	SINASC
Prenatal Consults (7+), (%)	60.27	59.02	-0.21	0.89	SINASC
Prenatal Consults (1-6), (%)	35.58	36.25	0.89	0.54	SINASC
Prenatal Consults (0), (%)	1.80	1.72	0.19	0.45	SINASC
Panel D: Human Resources					
Dismissals of Healthworkers	1.06	1.19	-0.86	0.06	RAIS
Hiring of Healthworkers	1.29	1.48	-0.33	0.46	RAIS
Panel E: Covid Outcomes, 2020					
Total Cases (per 100k inhab.)	2147.43	2131.02	220.70	0.20	CODA
Total Deaths (per 100k inhab.)	42.16	42.50	-3.27	0.39	CODA

Notes: This table displays descriptive statistics for municipalities that experienced turnover and municipalities that did not in close elections (bandwidth = 14) in Columns 1 and 2. Column 3 displays the estimated coefficient in regressions testing for discontinuities at baseline (defined as one year prior to elections) with each variable measured at baseline used as dependent variable. Column 4 shows the corresponding p-values, and Column 5 shows the data source.

4 Empirical Strategy

4.1 Identification

In order to estimate the effects of political turnover and government transitions on health outcomes, we employ an event-study in discontinuities approach – as a generalization of the difference-in-discontinuities design from Grembi et al. (2016), and similar to Szerman (2022). In this way we combine the sharp cross-sectional variation in turnover from the discontinuity around the margin of victory, with municipality and time fixed-effects plus additional controls, thus absorbing differences in levels and common time trends. Further restricting the analysis to close elections means that we only consider elections where the incumbent barely won or lost to the strongest challenger candidate. This not only provides balance between groups in observables (as seen in Table 1), but also helps us overcome the influence of relevant non-observable confounders. A simple comparison of outcomes in

municipalities where the incumbent won versus others where she lost could be biased. For instance, in municipalities where the incumbent loses by a significant margin, local actors may act in advance, anticipating good or bad electoral results that have not yet been realized. In those cases, worse prenatal outcomes could be more of a cause than a consequence of the electoral result. Our empirical design provides a quasi-random assignment of the candidate in power just after elections. Yet, due to the restriction imposed by close elections, our main specifications estimate local average treatment effects (LATE), i.e., only for the sample of municipalities around the discontinuity.[9]

The validity of our empirical strategy relies on two key identifying assumptions. First, that potential outcomes are continuous around the threshold where the vote margin is equal to zero. We perform a formal test to spot manipulation on the running variable as proposed by McCrary (2008). In Figure A.2, we see no visual sign of manipulation. A formal test fails to reject the null hypothesis that the running variable is continuous around the zero threshold. Second, there are no time-varying confounders driving differences in treated and control municipalities after elections. We test and reject the existence of pre-treatment trends by running our analysis in several periods before the event (elections) takes place, which lends further support to our identification strategy.

We use local linear regressions with triangular weights for estimation, with treatment effects estimated using only the observations that fall within the optimal bandwidth on the margin of victory. As mentioned before, in an effort to keep the estimation sample fixed, in our main specifications we calculate this margin based on Calonico et al. (2014) by using as outcome variable the infant mortality rate.

4.2 Estimation

We begin by estimating differences in health service outcomes between municipalities that experienced political turnover and municipalities that did not. To do so, we analyze the electoral periods of 2008 and 2012 by using SIAB monthly data, and the 2020 electoral period by using SISAB quarterly data. In both cases, for municipality m, electoral cycle c and period t we estimate:

$$Y_{mct} = \beta_1 Turnover_{mc} + \beta_2 MV_{m,c} + \beta_3 Turnover_{m,c} * MV_{mc} + \gamma_{ct} + \lambda_m + \sum_{e=1}^{48} \alpha_e Turnover_{mc} * Period_t^e + \varepsilon_{mct} \quad (1)$$

Where Y_{mct} represents one of the health service outputs of interest. $Turnover_{mc}$ is the treatment variable that indicates if there was political turnover in municipality m and electoral cycle c. The running variable is MV_{mc}, which shows the difference between the vote shares of the strongest challenger candidate and that of the incumbent. We include λ_m as municipality fixed effects, and γ_{ct} as time fixed effects obtained by combining period t with electoral cycle c. Finally, we add interactions of turnover with $Period_{mt}^e$, dummy variables enumerating months (or quarters) within each electoral cycle. When working with months, this variable goes from t = 1 up to t = 48, with the baseline set at t = 10 (one year before elections). For quarters, it goes from t = 1 up to t = 12, baseline at t = 4 The term ε is an idiosyncratic error. Standard errors are clustered at the municipality level. We are interested in the α_e parameters, which represent the differential effect of turnover in a given period relative to baseline.

The analysis of the turnover effects on health services enables us to identify a critical period of

[9]For the sake of completeness, in Appendix C we present main results without the restriction on close elections.

exposure to the turnover shock. We then estimate a second model to assess whether exposure to turnover during the time in utero affects birth outcomes. In particular, we investigate if children whose gestation overlapped with the exposure period experienced differences in outcomes at birth and infant mortality rates over the first year of life. In this analysis we distinguish the effects by trimester of gestation to assess impacts by the timing of exposure.[10]. Moreover, we also include additional trimesters: one preceding conception (which we call trimester minus one) and two trimesters after birth (the fourth and fifth) as placebos. The control group for this specification consists of all children born in the sample that had gestations overlapping with the exposure period prior to trimester minus one or after trimester five – hence children not exposed.

For this part of the analysis we use a municipality-by-month panel based on the child's month of birth covering the period of 2007 to 2018. Since mortality and other birth-related variables are measured with less precision when there are fewer births, we weight regressions by the average number of births per year in the municipality. Our specification is:

$$Y_{mct} = \alpha_1 * Turnover_{mc} + \alpha_2 * MV_{mc} + \alpha_3 Turnover_{mc} * MV_{mc} + \gamma_{ct} + \lambda_m + \sum_{e=-2}^{5} \delta_e Turnover_{mc} * ExposureTri^{e}_{mt} + \varepsilon_{mct} \quad (2)$$

Where Y_{mct} represents one of the health outcomes of interest in municipality m, electoral cycle c and time of birth t. Most regressors follow definitions as in equation (1). However, here we add interactions of turnover with *ExposureTri*, dummy variables equal to one if a child born in municipality m, electoral cycle c and time t had her gestational period coinciding with the exposure period in gestational trimester e, and equal to zero otherwise. A child would not have been exposed in any trimesters if the pregnancy started (ended) nine (one) months after (before) the exposure period. In this specification, it does not matter how many months (one, two, or three) within each trimester coincided with the exposure period. In additional exercises we complement the analysis by accounting for potential differential effects depending on the intensity of exposure in each trimester, turning the variable *ExposureTri* from dummies into continuous variables ranging from zero to three.[11] Again, we add municipality fixed effects γ_{ct} and time fixed effects λ_m, while standard errors are clustered at the municipality level. We are interested in the δ_e parameters, which measure to what extent exposure in utero to turnover and government transitions affects birth outcomes.

5 Results

We first present short-term effects of political turnover on health care production for the 2008 and 2012 electoral periods, and separately for 2020. We then present results on birth outcomes and mortality for the 2008, 2012, and 2016 electoral periods. In all regression plots, the election year is indicated as "Year 0", while "Year 1" represents the following year, when the new government takes office. We consider as reference category one year before the elections, therefore October of the previous year.

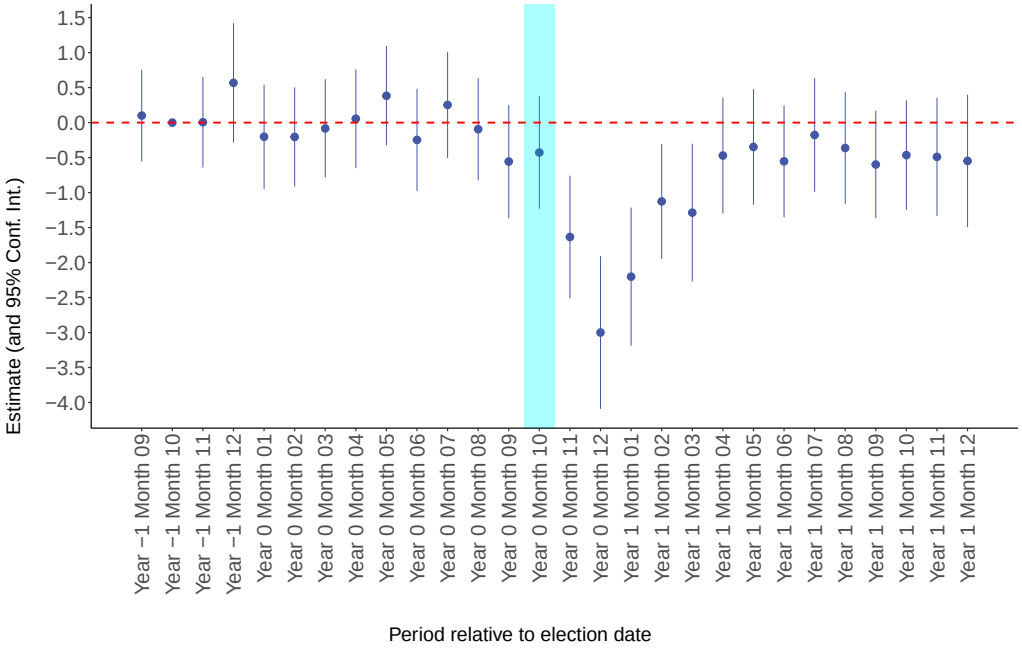
[10]We consider that all births happen in the ninth month of gestation.

[11]Results reported in Appendix D.

5.1 Political Turnover Effects on Health Care Services

Figure 1 shows effects on the share of mothers who had prenatal consults in a given month. Right after elections happen, in October of the electoral year, we observe a reduction of prenatal consults per month in municipalities that experienced turnover relative to those that did not. This negative shock is temporary, dissipating after a few months by the beginning of the new government. Table A.1 shows the corresponding regression results. The difference reaches the lowest at -2.9 percentage points in December, which corresponds to 3.2% of the mean.

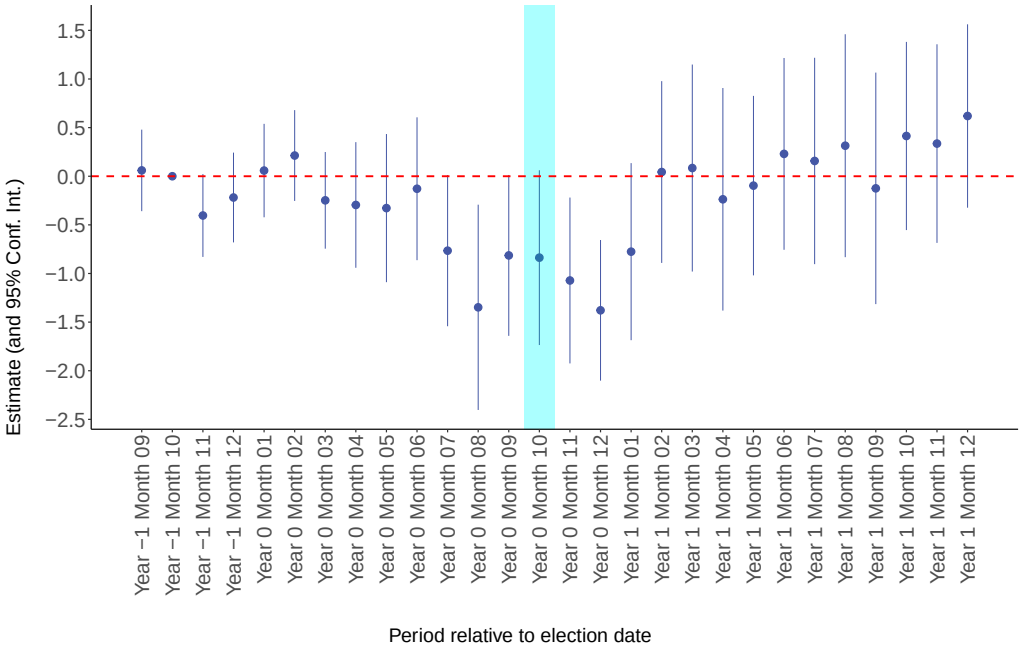
Figure (1) Pregnancies with Prenatal Consults in Month, (%)



Note: This figures shows graphically results using a municipality by month panel for the 2008 and 2012 electoral periods. The dependent variable is the share of pregnancies with prenatal consults in month. We examine results from September in the year preceding elections (Year -1) up to December of the year following elections (Year 1). In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors. The blue shaded area represents the time elections are held. Regression results appear at Table A.1

Yet, it is possible that even if mothers are not having prenatal consults in a given month, they are still ending up with an adequate number of consultations by having them shortly afterwards, in the following months. Figure 2 complements the analysis by showing the number of pregnancies that, by the time of birth, had six or more prenatal consults, considering as reference the month when the first consult happened – regardless of the gestational month in which that occurred. We find that the total number of pregnancies with 6 or more completed consults at the time of birth is reduced, so many consultations are indeed missed.

Figure (2) Gestations that ended up with 6+ consults, by month of first prenatal consult (N)

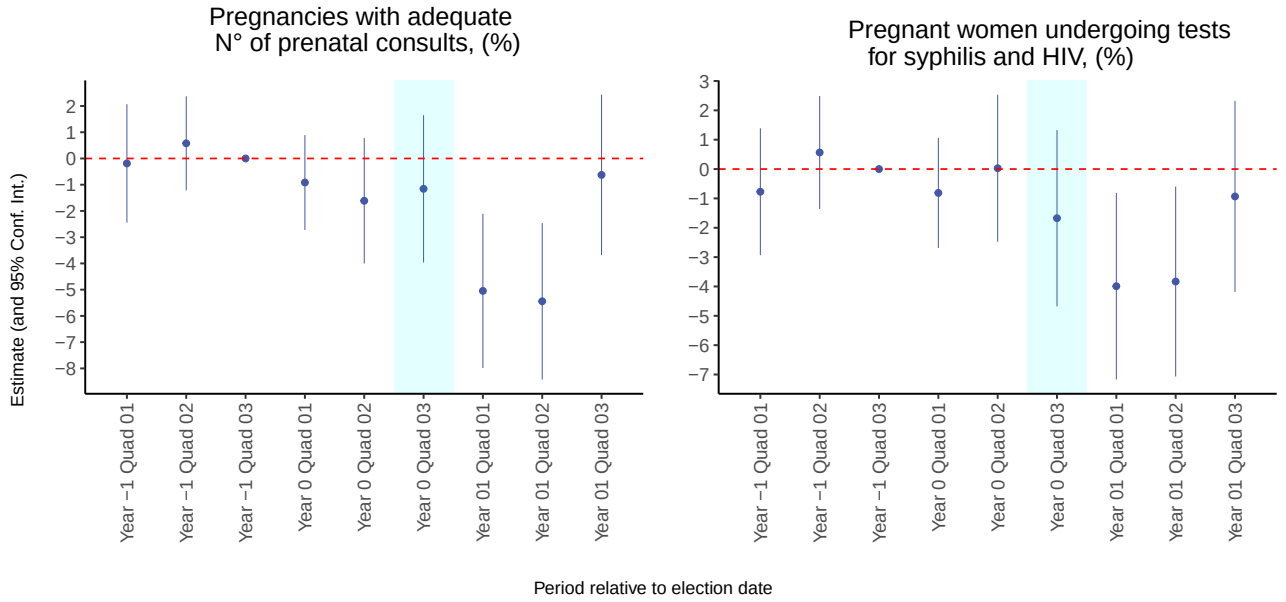


Note: This figures shows graphically results using a municipality by month panel for the 2020 electoral period. The dependent variable is total of pregnancies that ended up with 6 or more prenatal consultations by month of the first consult. We examine results from September in the year preceding elections (Year -1) up to December of the year following elections (Year 1). In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors. The blue shaded area represents the time elections are held. Regression results appear at Table A.2.

Figure 3 further characterizes turnover effects on health services by using SISAB quarterly data. The first panel shows the share of pregnancies ending up with an adequate number of prenatal consults (at least six, with the first one in the first trimester), and the second reports the share of pregnant women undergoing tests for syphilis and HIV. We consider as reference one year before the elections, omitting the third quarter of the previous year (3rd quarter of Year -1). We observe again transitory adverse effects in both outcomes. Table A.3 shows the corresponding regression results. On average, political turnover leads to a decrease of 5 percentage points in the share of pregnancies that end up with an adequate number of prenatal consults one quarter after elections, and of 4 percentage points in the share of pregnant women undergoing tests for Syphilis and HIV.

Figure B.1 in Appendix B expands the plotted coefficients for SIAB monthly data and shows that there are not any similar effects in periods far from the elections and the transition period. Furthermore, Figures C.1, C.2, and C.3 in Appendix C present the previous analysis with the full sample of municipalities without restricting to close races. Results are qualitatively similar, thus suggesting external validity. Finally, we repeat all exercises using the optimal bandwidth and different weights for each variable. Results appear in Figures F.1, F.2, and F.3 in Appendix F. We do not observe any substantial changes to the main results.

Figure (3) Prenatal Outcomes SISAB, quarter



Note: This figures shows graphically results using a municipality by quarter panel for the 2020 electoral period. The dependent variables are the share of pregnancies ending with the correct number of prenatal consults and the share of pregnant women undergoing tests for syphilis and HIV. We examine results from the first quarter in 2019 (Year -1) up to the last quarter 2021 (Year 1). The blue shaded area represents the time elections are held. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors. Regression results appear at Table A.3.

5.2 Mapping the Critical Period

While most evidence on political turnover focuses on what happens *after* the new incumbent takes office, the effects documented here begins *before* this event. This pattern aligns with Toral (2023), who argues that political turnover leads to incentives that may have detrimental effects on service delivery during the transition period. Nonetheless, we show that effects last longer than that, as they are still active throughout the first months of the new government. We now investigate whether these transitory patterns lead to persistent health effects.

We start by defining a critical period, when exposure to turnover effects can be potentially harmful to birth outcomes and child development. To do so, we overlap the timing in the gestational stages with the observed timing in service reduction. Existing evidence suggests that the second trimester of gestation is especially sensitive to shocks, which may lead to long-term developmental consequences (e.g. Rocha and Soares, 2015; Schwandt, 2018). In particular this is when critical neural brain development occurs (Black et al., 2013).

We conjecture that the short-lived, but well-marked effects of turnover on service provision, starting right after elections, may have far-reaching detrimental effects should they overlap with critical gestational stages. Indeed, recent evidence indicates that higher frequency of prenatal consultations can reduce the probability of both infant and maternal death (Varela, Schneider, Bubach, Silveira, Bertoldi, Duarte, Menezes, Domingues, and Bassani, 2019). They can help screen for warning signs of pregnancy complications and provide proper care to improve the survival and health of babies by reducing stillbirths and infant deaths (Wondemagegn, Alebel, Tesema, and Abie, 2018). In a cohort study conducted in Brazil in 2015, Varela et al. (2019) find strong associations between infant mortality

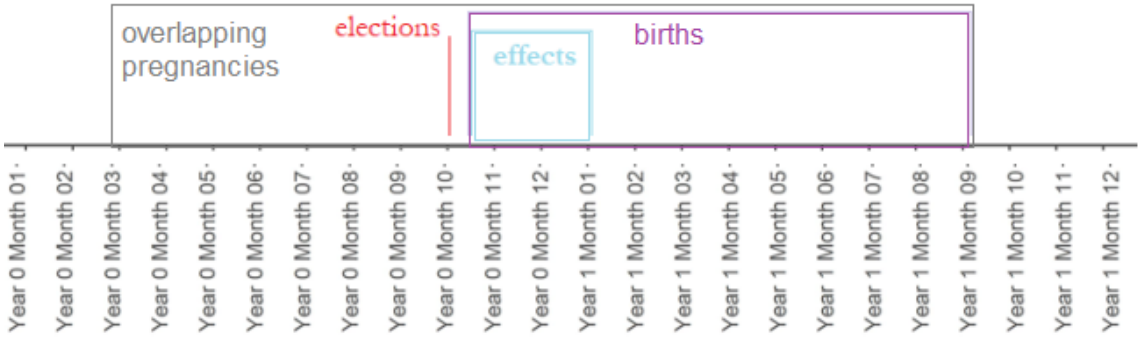
rates and having had less than six prenatal care consultations in utero.

We more precisely define the exposure period and the pool of potentially affected pregnancies by combining the results presented in Figures 1, 2, and 3. From Figure 1, we observe that the negative effect on health care production lasts from November (the first month after elections) to March of the following year. However, Figure 1 does not provide a clear distinction between missed and delayed prenatal consultations, as some mothers may still meet the adequate number of prenatal consults by the end of their pregnancy, without any harmful effects due to the timing in the access to services. To resolve this issue, we use the combined results from Figures 2 and 3 to more accurately determine a critical timing of exposure. Figure 2 allows us to define critical pregnancies as those ending by September of the year following elections. There is no evidence of fewer prenatal consults after this point. This is supported by results in Figure 3. Based on a 9-month gestational period, if children born in October in the year following elections are not affected, we may conclude that the exposure period ended by January of that year. Finally, using the results in Figure 1 we consider November of the electoral year as the beginning of the exposure period, which lasts for three months until January of the following year.

Figure 4 illustrates this timing, and identifies the pregnancies that are expected to overlap with turnover effects. In sum, Assuming a nine-month gestational period for all pregnancies, mothers starting their consults in January of the year following elections would most likely have their due-dates up to September of the same year.[12] Since pregnancies with the first consult happening in February are not affected, we assume that all births from November of the electoral year until September of the following year were affected by the transition process, having been exposed to reduced access to health services for at least a month during the gestational period. Every child born during the first month after the elections, and up to the ninth month of the new government thus has at least part of their gestational period overlapping with the exposure period. These children are those likely to be the most directly affected.

[12]We consider January of the year following elections as critical as we still observe in Figure 2 fewer gestations with the first consult in that month and that ended up with the adequate number of prenatal consults – result significant at 90%. We consider the possibility that the first consult occurs after the first month of pregnancy. If this first consult happens in the second or third months, then due-dates would happen before, in July or August.

Figure (4) Overlapping Pregnancies



Note: This figure is a graphic representation of potentially affected children. Considering that the negative results from political turnover last three months, from November to January of the following year, all children born in November of the election year (Year 0) up to September of the following year (Year 1) had their gestational period overlapping with at least one month of the negative shock. The gray shaded area represents a comprehensive measure of affected pregnancies, from conception to birth of all potentially exposed.

5.3 Effects on Birth Outcomes and Mortality

We rely on equation (2) to assess effects on birth outcomes and mortality. In our benchmark specifications, exposure by trimester is defined by dummy variables. The upper plots of Figure 5 show, now using data from SINASC, that exposure to turnover and government transition is associated with fewer prenatal consults by the end of pregnancies. In particular, pregnancies exposed during the second and third trimesters had, on average, fewer prenatal consults compared to those in municipalities without turnover. Table A.4 shows the corresponding regression results. Column 3 shows a decline of -1.5 percentage points in the share of children born from pregnancies with 7 or more prenatal consults when exposed during the second trimester of gestation (2.3% of the mean). In order to put the magnitude of this effect into perspective, Bhalotra et al. (2016) find that granting access to PSF in Brazil led to an increase of 1.0 percentage point (2.2% of the mean) in the number of prenatal consults upon the arrival of the program.

Moving to outcomes at birth, the second row in Figure 5 suggests increases in the share of children with low birth weight (< 2.500 grams) and very low-weight (<1.500 grams) births for those exposed in the second trimester of gestation. Coefficients are almost significant at the 90% level, with p-values of .109 and .106, respectively. Finally, we also observe a decrease in the average weight (in grams) at birth. All these effects appear when the child was exposed during the second trimester of gestation. Table A.5 shows these results in columns 4, 5, and 6. We see an increase of, on average, 0.19 in the share of low birth weights (2.5% of the mean) when the exposure happened in the second trimester; and an increase of 0.07 in the share of very low birth weights (representing 7% of the mean). Putting these results into perspective, Schwandt (2018) finds that infants exposed to maternal influenza infections in utero were more likely to have low birth weight. Although the author finds greater effects, point estimates are roughly of the same magnitude in terms of the sample mean. Furthermore, our results are very similar in magnitude to those of Reader (2023), who finds that providing a lump sum cash

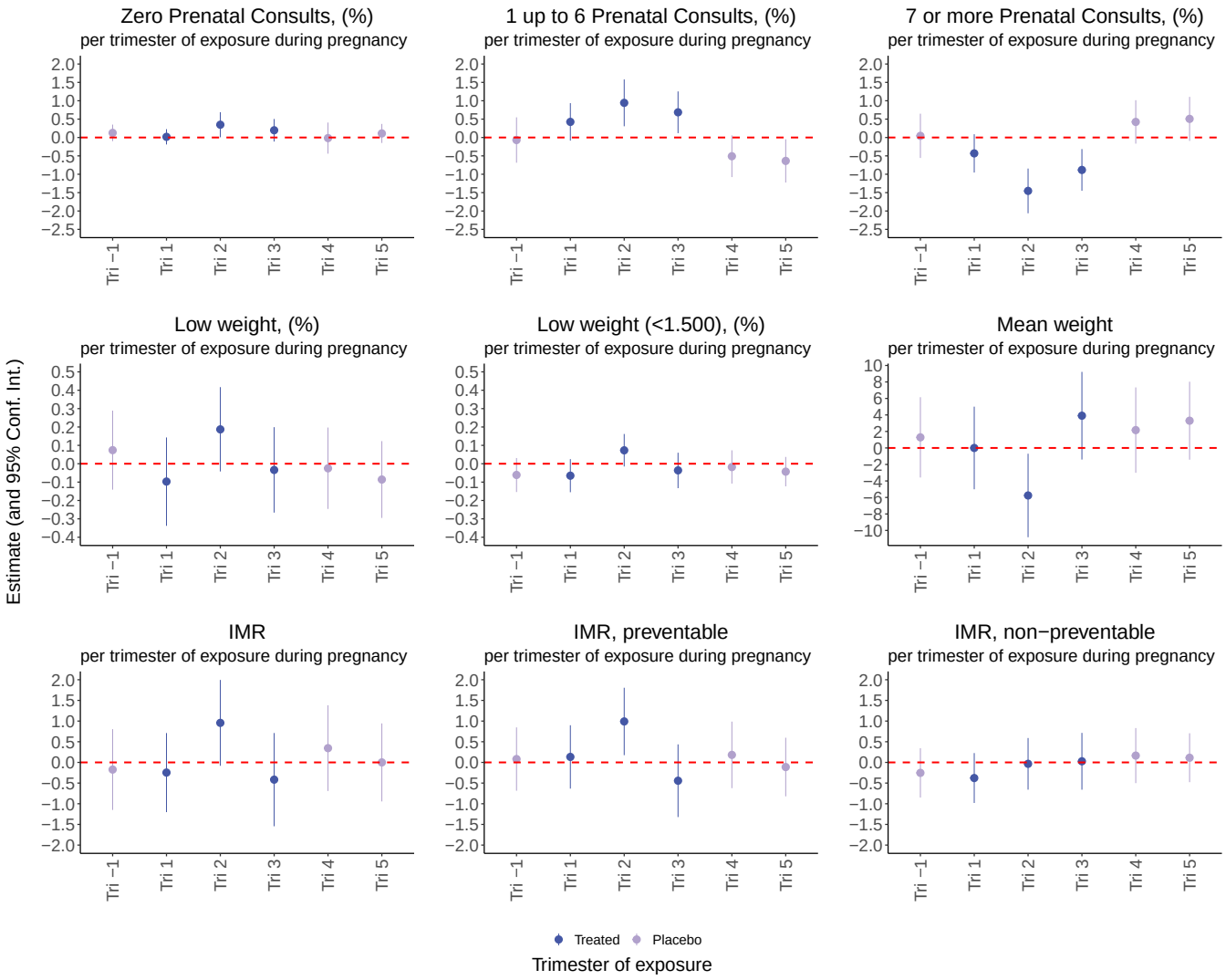
benefit to pregnant mothers in the UK increased birth weight by 8-12 grams on average and reduced low birth weight by 3-6 percent.

Finally, the third row of Figure 5 shows higher infant mortality rates for children exposed during the second trimester of pregnancy. This effect is significant when considering all causes of death, but is more prominent when we look at preventable causes. Columns 1 to 3 document this effect in Table A.5. For all causes, we see an increase of 0.96 in infant mortality rates (representing a sizable impact of 6.6% of the mean). For preventable causes, the point estimate of 1.0 corresponds to 11.8% of the mean. [13]

For better contextual understanding our findings, we note that (Bhalotra et al., 2016) find a drop of 9% in infant mortality rates in the second year after the restructuring of the public health system in Brazil. Moreover, the authors also note that a Mexican program (Seguro Popular) was responsible for a 7% reduction in child mortality only after three or more years of exposure. In addition, Gertler and Giovagnoli (2014) provide evidence that the program Plan Nacer, implemented in Argentina, was effective in improving birth outcomes and reducing infant mortality rates. Importantly, the program was associated with significant increases in the number of prenatal care visits. Therefore, our results stand out by demonstrating effects of similar magnitudes, but with an opposing direction, resulting from a brief and transitory event (transitions) as opposed to large-scale national programs.

[13] Given that we only observe outcomes at birth for surviving fetuses, if political turnover is correlated with increases in fetal deaths, surviving newborns may be different from what it would otherwise have been (survivor bias). In this case, however, fetal selection would most likely understate adverse effects.

Figure (5) Birth Outcomes and Mortality



Note: This figures shows graphically results using a municipality-by-month panel covering the 2007-2018 period. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consults. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters, -1, 4 and 5 are placebos. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors. Regression results appear at Table A.4

We perform additional robustness exercises. In Appendix B, Figures B.2, B.3, and B.4, we add 12 months to the actual period of effects in each trimester of gestation to create a falsified period of exposure one year after elections happen. Results show that we do not observe similar effects in periods far from the election. In Appendix C, Figures C.4, C.5 and C.6, we repeat the analysis without the selection on close races using instead the whole sample. Results remain qualitatively similar. In Appendix D, we change the definition of exposure variables to consider the intensity of exposure per month (i.e. the number of months exposed by trimester of gestation) rather than dummies for exposed/not exposed per trimester. Results in Figures D.1, D.2, and D.3 remain qualitatively similar to our benchmark specification, indicating that not only exposure in a given trimester is relevant, but also that being exposed for more months per trimester leverages this effect. Finally, we repeat all exercises using the optimal bandwidth with different weighting methods for each variable as they

appear in regression tables. Results appear in Figures F.4, F.5, and F.6 in Appendix F. We do not observe any relevant changes to the results.

6 Discussion on Mechanisms

6.1 Bureaucratic Turnover

We now assess potential mechanisms that could explain the connection between political turnover, government transitions, deteriorated services and worse health outcomes. We focus on the turnover of healthcare workers and on fiscal incentives. Most maternal health services occur at public primary health care facilities (mainly UBS), which are heavily dependent on human resources. The deterioration in maternal health services may therefore stem from a shortage of professionals.

Previous findings indicate that during both the transition period (Toral, 2022) and the phase-in of the new elected government (Akhtari et al., 2022), civil service hirings and dismissals might not be as insulated from political influence as commonly assumed. Although the Brazilian Electoral Law (Law 9,504, 1997) constrains the political use of hirings and dismissals of public sector employees during the electoral period (through a six-month freezing period, three months before and three months after elections), there are some exceptions, including hirings for positions of trust. Additionally, while mayors have less discretionary power over the hiring and firing off civil servants in tenure-track positions, they can choose the timing and the number of people hired within an ordered list of candidates.

We examine the monthly evolution of hirings and dismissals of healthcare personnel, looking separately for civil servants and temporary workers, throughout the period covering the 12 months before elections, the transition period, and the first year of the new government. We employ a model similar to equation 1. Now, Y_{mct} represents one of the human resources outcomes of interest, and the variable t represents months for the 2011-2018 period, encompassing the 2012 and 2016 electoral cycles.[14] The remaining details of the specification follows those previously described in equation 1.

In Figure 6 we observe a substantial increase in workers' dismissals under civil servant contracts right after elections occur, as well as a smaller but still positive effect on dismissals of temporary workers. Point estimates reported on Table A.6 show, for instance, that political turnover is associated with an increase of 9% in the dismissal of healthcare personnel under civil servant contracts in the month of elections.[15] These effects are reverted throughout the first months of the new government.[16] In Figure 7, we focus on civil servant contracts and separate the analysis in three categories of healthcare workers (community health workers, nurses, and physicians). We observe that the results follow a similar pattern across all categories. However, despite similar point estimates for the three categories in the month of elections, the effects on physicians and nurses also appear positive in the following months. Although coefficients are statistically similar, they suggest that dismissals are more likely for higher-paid workers, eventually resulting from an effort to reduce spending with personnel.[17] We

[14]To account for the fact that the employment indicators are mostly right-skewed, we transform the dependent variables by using the inverse hyperbolic sine transformation.

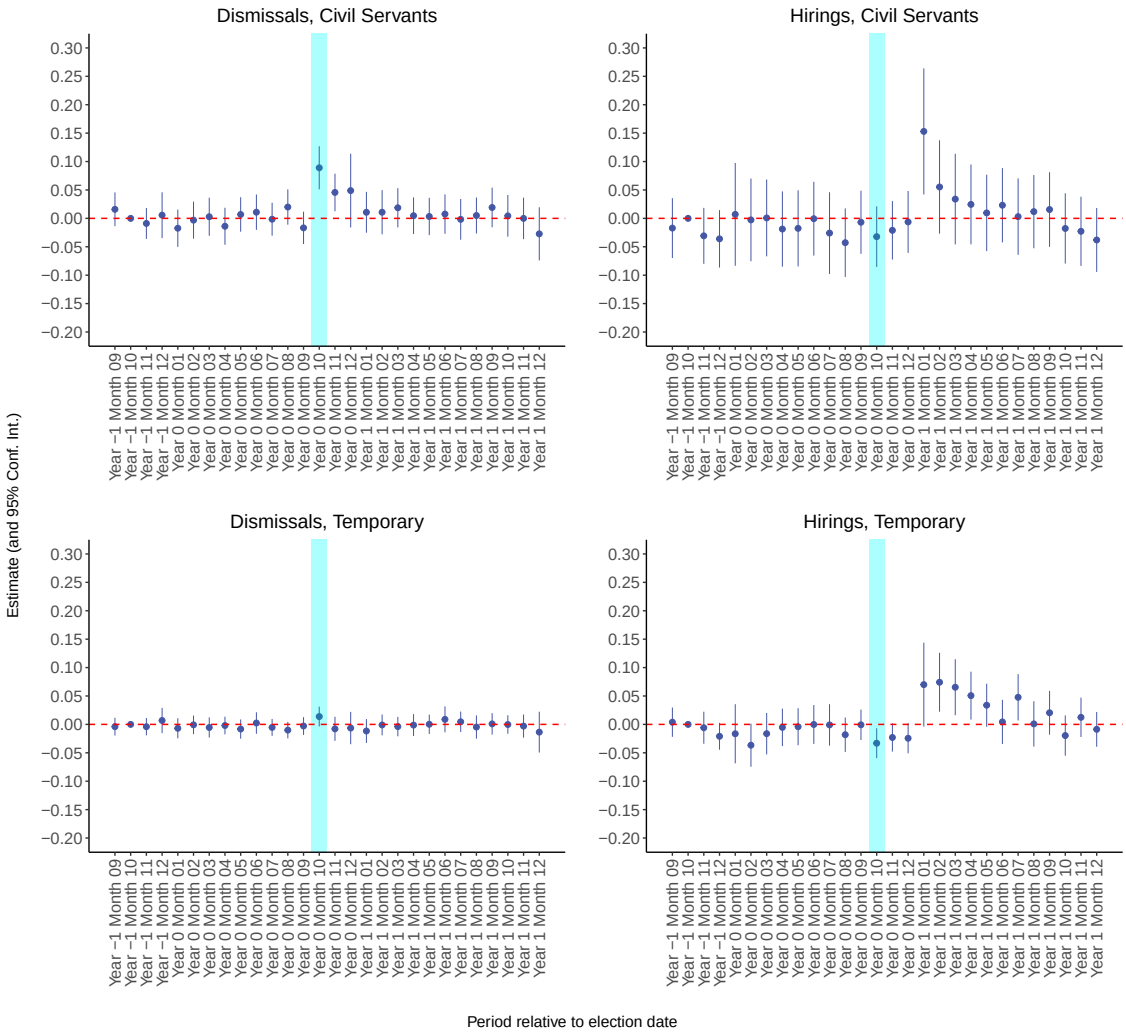
[15]Hiring or dismissing civil servants who generally hold stable contracts should be relatively more difficult in electoral years. In Appendix E, Figure E.1 repeats the analysis separating between those civil servants with contracts that have more stability (tenure) and those who do not. Results suggest that the effects on dismissals reported in Figure 8 mostly come from workers who, albeit civil servants, have less stability, making hirings and dismissals relatively more likely.

[16]These results are consistent with other studies that assessed bureaucratic turnover in Brazil (e.g. Toral, 2022; Ramos, 2022).

[17]These results are consistent with Akhmedov and Zhuravskaya (2004), who document opportunistic political cycles in

investigate this conjecture in the next section.

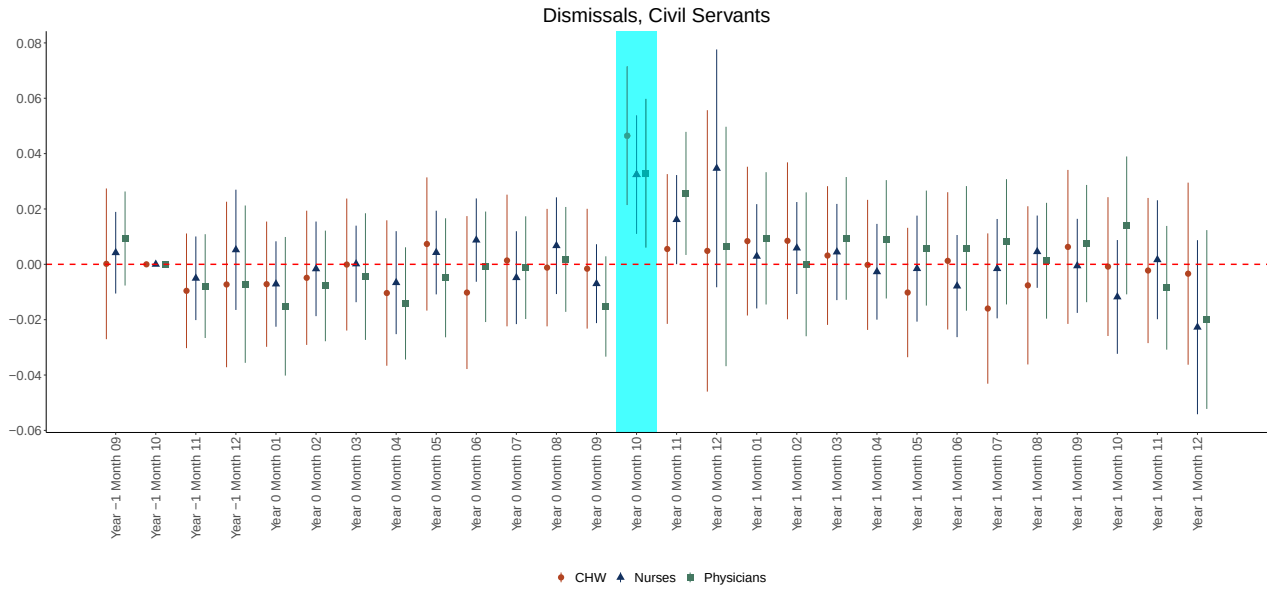
Figure (6) Human Resources Turnover: Hirings and Dismissals by Type of Contract



Note: This figure shows graphically RD results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the counts of dismissals and hirings of healthcare workers, separated by type of contract (civil servants and temporaries). The variables are normalized by the inverse hyperbolic sine transformation. In an effort to keep the estimation sample fixed we select the optimal bandwidth using the infant mortality rate variable. Nevertheless, the results are not sensitive to this procedure. Regression results appear at Table A.6.

budget spending in Russia. In particular, they show that spending in education, culture, and healthcare decrease 18% percent in the two months after elections.

Figure (7) Human Resources Turnover: Dismissals of Civil Servants by Occupation



Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the counts of dismissals and hirings of healthcare workers with civil servant contracts. The variables are normalized by the inverse hyperbolic sine transformation. In an effort to keep the estimation sample fixed we set a bandwidth equal to .14 for all regressors.

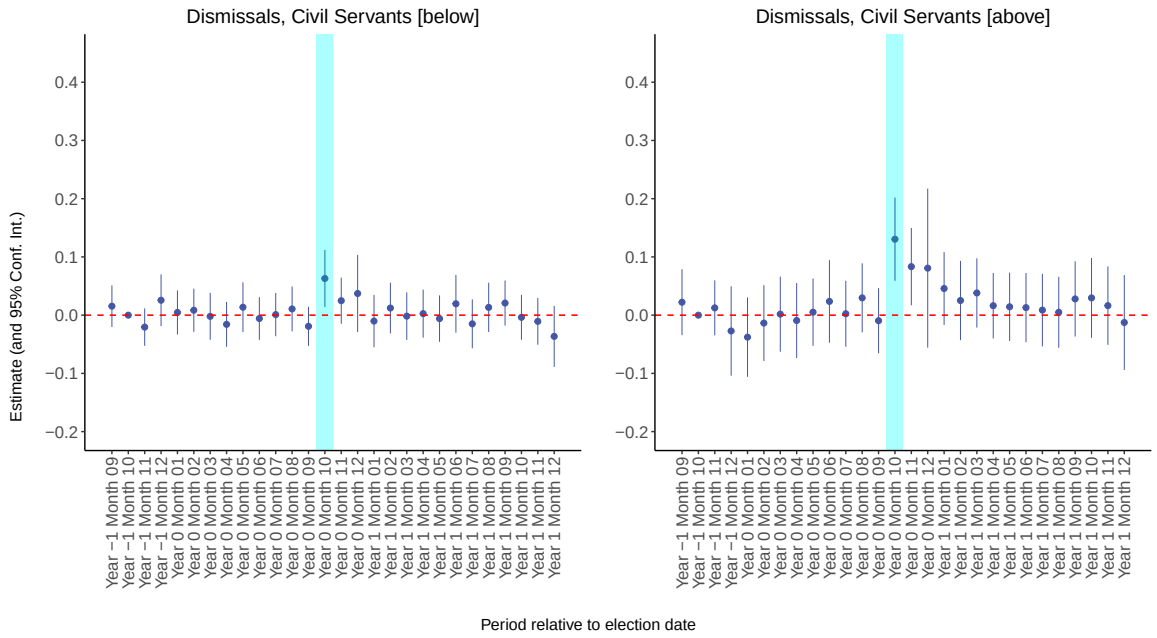
6.2 Fiscal Incentives

In 2000, the Fiscal Responsibility Law (LRF) defined that municipalities cannot spend more than 60% of their revenue on personnel (Brasil, 2000). Aiming at "clearing" the accounts before leaving office, mayors not complying or close to the cap might have different incentives relative to others spending relatively less with personnel.[18] To investigate this, we split the sample into municipalities close to the cap and those far from it by using the median (48%) of the share of revenues spent with personnel in the period under analysis.

Figure 8 shows greater effects in dismissals among municipalities operating close to or above the threshold. The effect also lasts longer, for two months, as opposed to only a month for those below the median. Table A.7 reports the corresponding regression results. We observe an increase of 6,3% in dismissals of health workers in municipalities below the median in the electoral month, while among those above the median the effect is nearly twofold (13,04%). Taken together, these results suggest that municipalities where the incumbents dismissed more healthcare workers after the elections were those closer to reaching the LRF cap. Given the timing of these events, this could be considered suggestive evidence that, after losing, incumbents would dismiss workers to avoid surpassing the cap, and therefore to comply with the Fiscal Responsibility Law.

[18]This was first hypothesized by Toral (2023).

Figure (8) Human resources turnover: heterogeneity in dismissals using Fiscal Responsibility Law median proportion



Note: This figure shows graphically RD results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the counts of dismissals and hirings of healthcare workers under civil servant contracts, below and above the Fiscal Responsibility Law median proportion. The variables are normalized by the inverse hyperbolic sine transformation. In an effort to keep the estimation sample fixed we select the optimal bandwidth using the infant mortality rate variable. Nevertheless, the results are not sensitive to this procedure. This figure shows results regarding compliance with the LRF in electoral years (this median is close to 45% of municipal spending with personnel).Regression results appear at Table A.7.

7 Conclusion

Whereas political turnover is usually considered beneficial for citizens and an intrinsic phenomenon to the well-functioning of democracies, this paper studies the possibility that government transitions could lead to significant long term and irreversible detrimental effects. In particular, we empirically test how political turnover impacts primary healthcare outcomes in Brazilian municipalities. We focus on municipalities that experienced close elections using multiple sets of administrative data to build panels at the municipality-by-month level and use them in an event-study in discontinuities framework.

We find significant declines in public health service delivery in municipalities that experienced turnover compared to those that did not. The negative impact starts immediately after the election and continues during the transition period until the early months of the new government. The new administration eventually makes up for the deficit.

Since prenatal healthcare is crucial for the health of an unborn child, we focus on children who were in utero during the time of the transition negative impact. We examine if they experience worse birth outcomes, such as low birth weight, and increased infant mortality rates. Our findings show a higher prevalence of low-weight births and increased infant mortality rates in municipalities that experienced political turnover compared to those that did not.

To understand the causes behind these results we investigate the availability of human resources, specifically healthcare personnel. We provide evidence that municipalities that experienced turnover

had on average an increase in dismissals of health professionals on civil servant contracts relative to those that did not experience turnover. We split our sample between municipalities below and above the median on the share of spending with personnel to test if intents to comply at the last minute with the Fiscal Responsibility Law (LRF) could help explain the results. Indeed, those municipalities operating closer to the cap seemed to have higher effects on dismissals, especially for physicians – the occupation typically associated with higher wages and costs.

In sum, we find that children exposed in utero to political turnover and government transitions had worse outcomes related to maternal health and later had worse birth outcomes measured as more low-weight births and increased mortality up to the second year of the new government. Our findings suggest that these effects are correlated to a shortage of professionals available to provide primary care in the months following elections.

Our study has highlighted the effects of government transitions over specific healthcare-related aspects. Still, it is important to note that turnover may lead to other unfavorable impacts on public health. Therefore, the implications of our findings are far-reaching, with a significant impact on health policy. To mitigate the adverse effects and considering a well-functioning democracy that adheres to the law, regulations could be implemented to incentivize incumbents to prioritize smooth bureaucratic turnover, ultimately safeguarding service delivery. In this way, we can ensure that the health of the population is not compromised during times of government transition.

References

Akhmedov, A. and E. Zhuravskaya (2004). Opportunistic political cycles: test in a young democracy setting. *The quarterly journal of economics 119*(4), 1301–1338.

Akhtari, M., D. Moreira, and L. Trucco (2022). Political turnover, bureaucratic turnover, and the quality of public services. *American Economic Review 112*(2), 442–93.

Almond, D. and J. Currie (2010). Human capital development before age five. nber working paper no. 15827. *National Bureau of Economic Research*.

Bhalotra, S. R., R. Rocha, and R. R. Soares (2016). Does universalization of health work? evidence from health systems restructuring and maternal and child health in brazil. Technical report, ISER Working Paper Series.

Black, S. E., A. Bütikofer, P. J. Devereux, and K. G. Salvanes (2013). This is only a test? long-run impacts of prenatal exposure to radioactive fallout. Technical report, National Bureau of Economic Research.

Boas, T. C., F. D. Hidalgo, and M. A. Melo (2019). Norms versus action: Why voters fail to sanction malfeasance in brazil. *American Journal of Political Science 63*(2), 385–400.

Brasil (2000). Lei complementar no 101, de 4 de maio de 2000 - lrf. Technical report.

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica 82*(6), 2295–2326.

CDC (2017). Syphilis - cdc fact sheet (detailed). *https://www.cdc.gov/std/syphilis/stdfact-syphilis-detailed.htm (last accessed on August 07, 2022) 19*(S1).

Cota, W. (2020, May). Monitoring the number of COVID-19 cases and deaths in brazil at municipal and federative units level. *SciELOPreprints:362*.

Ferraz, C. and F. Finan (2011). Electoral accountability and corruption: Evidence from the audits of local governments. *American Economic Review 101*(4), 1274–1311.

Gertler, P. J. and P. I. Giovagnoli (2014). Rewarding provider performance to enable a healthy start to life: evidence from argentina's plan nacer. *World Bank Policy Research Working Paper* (6884).

Grembi, V., T. Nannicini, and U. Troiano (2016). Do fiscal rules matter? *American Economic Journal: Applied Economics*, 1–30.

Iyer, L. and A. Mani (2012). Traveling agents: political change and bureaucratic turnover in india. *Review of Economics and Statistics 94*(3), 723–739.

Lambais, G. and H. Sigstad (2023). Judicial subversion: The effects of political power on court outcomes. *Journal of Public Economics 217*, 104788.

Linnet, K. M., K. Wisborg, E. Agerbo, N.-J. Secher, P. H. Thomsen, and T. B. Henriksen (2006). Gestational age, birth weight, and the risk of hyperkinetic disorder. *Archives of disease in childhood 91*(8), 655–660.

Mara, D. D. (2003). Water, sanitation and hygiene for the health of developing nations. *Public health 117*(6), 452–456.

Marx, B., V. Pons, and V. Rollet (2022). Electoral turnovers. Technical report, National Bureau of Economic Research.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics 142*(2), 698–714.

Medeiros, K. R. d., P. C. d. Albuquerque, R. A. W. Tavares, and W. V. d. Souza (2017). Lei de responsabilidade fiscal e as despesas com pessoal da saúde: uma análise da condição dos municípios brasileiros no período de 2004 a 2009. *Ciência & Saúde Coletiva 22*, 1759–1769.

Mrejen, M., R. Rocha, C. Millett, and T. Hone (2021). The quality of alternative models of primary health care and morbidity and mortality in brazil: a national longitudinal analysis. *The Lancet Regional Health-Americas 4*, 100034.

Nolte, E. and M. McKee (2004). Measuring the health of nations: analysis of mortality amenable to health care. *Journal of Epidemiology & Community Health 58*(4), 326–326.

Ramos, M. (2022). *Essays on Development Economics and Political Economy*. Ph. D. thesis, University of California, Berkeley.

Reader, M. (2023). The infant health effects of starting universal child benefits in pregnancy: Evidence from england and wales. *Journal of Health Economics 89*, 102751.

Rocha, Rudi; Camargo, M. B. S. (2020). Exposure to infectious diseases during pregnancy, birth outcomes and infant health. Technical report, Tech report, unpublished.

Rocha, R. and R. R. Soares (2015). Water scarcity and birth outcomes in the brazilian semiarid. *Journal of Development Economics 112*, 72–91.

Ronfeldt, M., S. Loeb, and J. Wyckoff (2013). How teacher turnover harms student achievement. *American educational research journal 50*(1), 4–36.

Sakurai, S. N. and N. Menezes-Filho (2011). Opportunistic and partisan election cycles in brazil: New evidence at the municipal level. *Public Choice 148*, 233–247.

Schwandt, H. (2018). The lasting legacy of seasonal influenza: In-utero exposure and labor market outcomes. *Available at SSRN 3098159*.

Shenkin, S. D., J. M. Starr, and I. J. Deary (2004). Birth weight and cognitive ability in childhood: a systematic review. *Psychological bulletin 130*(6), 989.

Szerman, C. (2022). The labor market effects of disability hiring quotas. *Available at SSRN 4267622*.

Szklo, M. (2022). *Essays on demographic and health economics*. Ph. D. thesis.

Toral, G. (2022). Political bureaucratic cycles: Public employment and service delivery around elections in brazil. Technical report, Working paper.

Toral, G. (2023). Turnover: How lame-duck governments disrupt the bureaucracy and service delivery before leaving office. Technical report, Working paper available at https://www. guillermotoral. com.

Toral, G. et al. (2020). *The political logics of patronage: uses and abuses of government jobs in Brazil*. Ph. D. thesis, Massachusetts Institute of Technology.

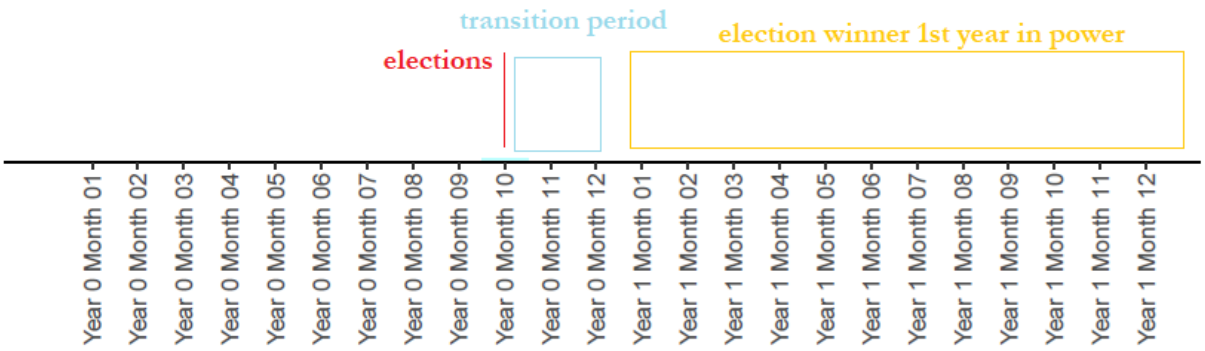
Varela, A. R., B. C. Schneider, S. Bubach, M. F. Silveira, A. D. Bertoldi, L. S. M. Duarte, A. M. B. Menezes, M. R. Domingues, and D. G. Bassani (2019). Fetal, neonatal, and post-neonatal mortality in the 2015 pelotas (brazil) birth cohort and associated factors. *Cadernos de Saúde Pública 35*.

Viellas, E. F., R. M. S. M. Domingues, M. A. B. Dias, S. G. N. d. Gama, M. M. Theme Filha, J. V. d. Costa, M. H. Bastos, and M. d. C. Leal (2014). Prenatal care in brazil. *Cadernos de saude publica 30*, S85–S100.

Wondemagegn, A. T., A. Alebel, C. Tesema, and W. Abie (2018). The effect of antenatal care follow-up on neonatal health outcomes: a systematic review and meta-analysis. *Public health reviews 39*(1), 1–11.

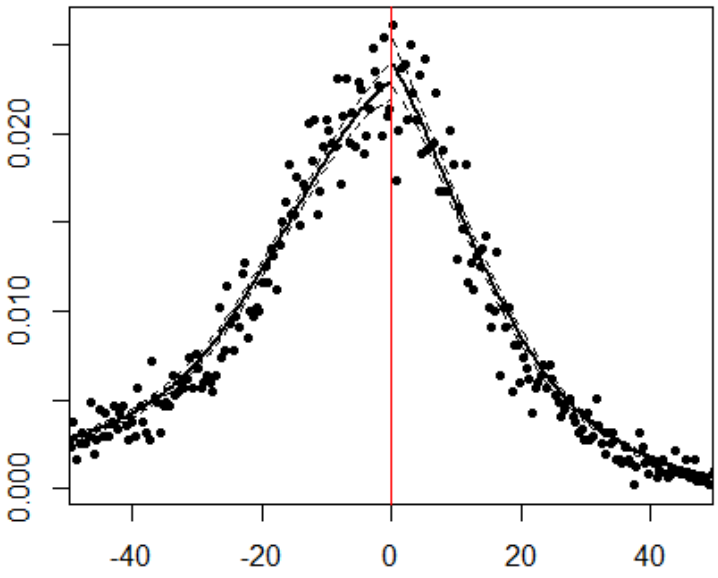
A Adittional Figures and Tables

Figure (A.1) Timeline of Election



Note: This figure illustrates the timeline of elections. We call the electoral year by Year 0. Elections happen in October (month 10) and, after approximately three months of a transition period, the new government starts in January.

Figure (A.2) McCrary Density Test



Notes: This figure shows the McCrary Test for manipulation of the running variable in the RDD, Margin of Victory of the Challenger Candidate for the 2008-2020 electoral period. The test fails to reject the null hypothesis that MV is continuous at the zero threshold. The estimated discontinuity has a p value of 0.316

Table (A.1) SIAB Prenatal Outcomes, monthly

Dependent Variables:	Pregnancies with Prenatal Consults in Month, (%)
	(1)
Turnover	-0.2680
	(0.9183)
MV	0.0375
	(0.1054)
Turnover X MV	0.0158
	(0.1209)
Turnover ×D(Year 0 Month 1)	-0.2020
	(0.3796)
Turnover ×D(Year 0 Month 2)	-0.2047
	(0.3611)
Turnover ×D(Year 0 Month 3)	-0.0838
	(0.3589)
Turnover ×D(Year 0 Month 4)	0.0558
	(0.3597)
Turnover ×D(Year 0 Month 5)	0.3835
	(0.3624)
Turnover ×D(Year 0 Month 6)	-0.2469
	(0.3727)
Turnover ×D(Year 0 Month 7)	0.2528
	(0.3861)
Turnover ×D(Year 0 Month 8)	-0.0942
	(0.3722)
Turnover ×D(Year 0 Month 9)	-0.5565
	(0.4134)
Turnover ×D(Year 0 Month 10)	-0.4274
	(0.4094)
Turnover ×D(Year 0 Month 11)	-1.634***
	(0.4465)
Turnover ×D(Year 0 Month 12)	-2.999***
	(0.5563)
Turnover ×D(Year 1 Month 1)	-2.202***
	(0.5035)
Turnover ×D(Year 1 Month 2)	-1.126***
	(0.4187)
Turnover ×D(Year 1 Month 3)	-1.287**
	(0.5020)
Turnover ×D(Year 1 Month 4)	-0.4708
	(0.4222)
Turnover ×D(Year 1 Month 5)	-0.3479
	(0.4214)
Turnover ×D(Year 1 Month 6)	-0.5530
	(0.4074)
Fit statistics	
Period Fixed Effects	Yes
Municipality Fixed Effects	Yes
Mean Dep. Var	92.84
Using Bandwidth	14.
Municipalities	2,607
Observations	130,378
R^2	0.49448

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. The Dependent variable is the share pregnancies with prenatal consults in that month. Mean Dep. Var. is the average dependent variable one year before elections for the treated group. All regressions include time fixed effects. We include period dummies as regressors and report only a subset of the estimated coefficients ranging from January of the electoral year (Year 0) to June of the following year (Year 1).

Table (A.2) SISAB Prenatal Outcomes, monthly

Dependent Variables:	Gestations ending with 6+ consults by month of first prenatal consult, (N)
	(1)
Turnover ×D(Year 0 Month 1)	0.0584
	(0.2449)
Turnover ×D(Year 0 Month 2)	0.2130
	(0.2380)
Turnover ×D(Year 0 Month 3)	-0.2478
	(0.2533)
Turnover ×D(Year 0 Month 4)	-0.2953
	(0.3293)
Turnover ×D(Year 0 Month 5)	-0.3276
	(0.3883)
Turnover ×D(Year 0 Month 6)	-0.1285
	(0.3744)
Turnover ×D(Year 0 Month 7)	-0.7653*
	(0.3961)
Turnover ×D(Year 0 Month 8)	-1.348**
	(0.5381)
Turnover ×D(Year 0 Month 9)	-0.8141*
	(0.4217)
Turnover ×D(Year 0 Month 10)	-0.8371*
	(0.4581)
Turnover ×D(Year 0 Month 11)	-1.072**
	(0.4348)
Turnover ×D(Year 0 Month 12)	-1.379***
	(0.3686)
Turnover ×D(Year 1 Month 1)	-0.7755*
	(0.4641)
Turnover ×D(Year 1 Month 2)	0.0433
	(0.4765)
Turnover ×D(Year 1 Month 3)	0.0844
	(0.5426)
Turnover ×D(Year 1 Month 4)	-0.2371
	(0.5834)
Turnover ×D(Year 1 Month 5)	-0.0965
	(0.4704)
Turnover ×D(Year 1 Month 6)	0.2299
	(0.5027)
Fit statistics	
Period Fixed Effects	Yes
Municipality Fixed Effects	Yes
Mean Dep. Var	3.77
Using Bandwidth	14.
Municipalities	1,493
Observations	60,295
R^2	0.66016

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. The dependent variable is total of pregnancies that ended up with 6 or more prenatal consultations by month of the first consult. Mean Dep. Var. is the average dependent variable one year before elections for the treated group. All regressions include time fixed effects. We include period dummies as regressors and report only a subset of the estimated coefficients ranging from January of the electoral year (Year 0) to June of the following year (Year 1).

Table (A.3) Prenatal outcomes from SISAB, quadrimester

Dependent Variables:	Pregnancies with adequate Prenatal Consults (%)	Pregnant women undergoing tests for Syphilis and HIV (%)
	(1)	(2)
Turnover ×D(201801)	0.2869	-1.319
	(1.355)	(1.408)
Turnover ×D(201802)	1.702	0.3906
	(1.293)	(1.340)
Turnover ×D(201803)	1.517	-0.2151
	(1.273)	(1.314)
Turnover ×D(201901)	-0.1878	-0.7724
	(1.150)	(1.101)
Turnover ×D(201902)	0.5778	0.5643
	(0.9139)	(0.9818)
Turnover ×D(202001)	-0.9155	-0.8131
	(0.9217)	(0.9557)
Turnover ×D(202002)	-1.613	0.0284
	(1.217)	(1.277)
Turnover ×D(202003)	-1.157	-1.675
	(1.430)	(1.531)
Turnover ×D(202101)	-5.047***	-3.992**
	(1.498)	(1.620)
Turnover ×D(202102)	-5.442***	-3.832**
	(1.520)	(1.649)
Turnover ×D(202103)	-0.6264	-0.9331
	(1.559)	(1.659)
Fit statistics		
Period Fixed Effects	Yes	Yes
Municipality Fixed Effects	Yes	Yes
Mean Dep. Var	34.20	38.46
Using Bandwidth	14.	14.
Municipalities	1,493	1,493
Observations	17,916	17,916
R^2	0.62465	0.63328

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. Dependent variables are the share of pregnancies with adequate amount of prenatal consults and share of pregnant women undergoing tests for syphilis and HIV. Mean Dep. Var. is the average dependent variable one year before elections for the treated group. All regressions include time fixed effects. We include dummy variables representing quarters from 2018 to 2021 and interact them with our treatment, the turnover variable.

Table (A.4) Prenatal Coverage

Dependent Variables:	0 Prenatal Consults	1 up to 6 Prenatal Consults	7 or more Prenatal Consults
	(%)	(%)	(%)
	(1)	(2)	(3)
Turnover	0.6652*	0.9308	-1.363
	(0.3462)	(2.007)	(2.008)
MV	-0.1150**	0.0609	0.0245
	(0.0474)	(0.2195)	(0.2208)
Turnover × D(Trimester -1)	0.1258	-0.0685	0.0455
	(0.1146)	(0.3151)	(0.3072)
Turnover × D(Trimester 1)	0.0185	0.4253	-0.4321
	(0.1061)	(0.2598)	(0.2655)
Turnover × D(Trimester 2)	0.3477**	0.9432***	-1.453***
	(0.1736)	(0.3249)	(0.3104)
Turnover × D(Trimester 3)	0.1970	0.6860**	-0.8827***
	(0.1565)	(0.2898)	(0.2895)
Turnover × D(Trimester 4)	-0.0163	-0.5097*	0.4241
	(0.2159)	(0.2891)	(0.3016)
Turnover × D(Trimester 5)	0.1105	-0.6372**	0.5072*
	(0.1318)	(0.2998)	(0.3056)
Turnover X MV	0.1580**	-0.1691	0.0667
	(0.0657)	(0.2740)	(0.2713)
Fit statistics			
Time FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Weighted (avg number of newborns)	Yes	Yes	Yes
Mean Dep. Var	1.96	33.54	62.00
Using Bandwidth	14.	14.	14.
Municipalities	3,296	3,296	3,296
Observations	204,000	204,000	204,000
R^2	0.39161	0.71769	0.73530

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. Dependent variables are the shares of newborns with 0 prenatal consults, 1 up to 6 prenatal consults, and 7 or more prenatal consults. Mean Dep. Var. is the average dependent variable one year before elections for the treated group. All regressions include time fixed effects and are weighted by municipality average number of births per year. Trimester are dummy variables that refer to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos.

Table (A.5) Outcomes at Birth and Infant Mortality

Dependent Variables:	IMR	IMR (preventable)	IMR (non-preventable)	Low Birth Weight (%)	Low Birth Weight (< 1500g, %)	Average Weight (g) (%)
	(1)	(2)	(3)	(4)	(5)	
Turnover	-0.0922	0.7996	-0.8932*	0.1474	0.0237	-5.469
	(0.7634)	(0.5238)	(0.4585)	(0.1713)	(0.0503)	(5.975)
MV	-0.0062	-0.0895	0.0833*	-0.0155	-0.0020	0.6783
	(0.1052)	(0.0791)	(0.0494)	(0.0186)	(0.0057)	(0.6749)
Turnover × D(Trimester -1)	-0.1727	0.0810	-0.2534	0.0738	-0.0612	1.286
	(0.4983)	(0.3906)	(0.3043)	(0.1095)	(0.0471)	(2.483)
Turnover × D(Trimester 1)	-0.2464	0.1326	-0.3775	-0.0974	-0.0651	-0.0019
	(0.4878)	(0.3909)	(0.3077)	(0.1228)	(0.0458)	(2.553)
Turnover × D(Trimester 2)	0.9565*	0.9906**	-0.0343	0.1875	0.0731	-5.761**
	(0.5295)	(0.4159)	(0.3185)	(0.1168)	(0.0452)	(2.581)
Turnover × D(Trimester 3)	-0.4170	-0.4431	0.0271	-0.0337	-0.0362	3.909
	(0.5754)	(0.4484)	(0.3505)	(0.1184)	(0.0493)	(2.710)
Turnover × D(Trimester 4)	0.3452	0.1819	0.1650	-0.0245	-0.0175	2.164
	(0.5290)	(0.4104)	(0.3398)	(0.1131)	(0.0461)	(2.639)
Turnover × D(Trimester 5)	-0.0004	-0.1117	0.1134	-0.0859	-0.0430	3.303
	(0.4814)	(0.3617)	(0.3017)	(0.1066)	(0.0407)	(2.409)
Turnover X MV	-0.0104	0.0632	-0.0734	0.0246	0.0068	-1.271
	(0.1371)	(0.1016)	(0.0648)	(0.0219)	(0.0082)	(0.8169)
Fit statistics						
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Weighted (avg number of newborns)	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	14.32	8.44	5.87	7.34	1.04	3,150.5
Using Bandwidth	14	14	14	14	14	14
Municipalities	3,296	3,296	3,296	3,296	3,296	3,296
Observations	200,160	200,152	200,142	204,000	204,000	204,000
R^2	0.03563	0.02811	0.03034	0.06171	0.03261	0.16330

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. Dependent variables are total infant mortality rates (up to age 1) and by cause and other health outcomes. Mean Dep. Var. is the average dependent variable one year before elections for the treated group. All regressions include time fixed effects and are weighted by municipality average number of births per year. Trimester are dummy variables that refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos.

Table (A.6) Human resources turnover

Dependent Variables:	Civil Servants		Temporaries	
	Dismissals	Hirings	Dismissals	Hirings
	(1)	(2)	(3)	(4)
Turnover	-0.0531	0.0064	-0.0054	0.0476
	(0.0338)	(0.0565)	(0.0201)	(0.0538)
MV	0.0036	0.0046	0.0009	-0.0070
Turnover x MV	0.0004	0.0026	-0.0050	-0.0016
	(0.0034)	(0.0097)	(0.0044)	(0.0085)
Turnover ×D(Year 0 Month 1)	-0.0173	0.0070	-0.0069	-0.0165
	(0.0168)	(0.0461)	(0.0090)	(0.0266)
Turnover ×D(Year 0 Month 2)	-0.0031	-0.0027	-0.0008	-0.0366*
	(0.0166)	(0.0371)	(0.0083)	(0.0193)
Turnover ×D(Year 0 Month 3)	0.0029	0.0008	-0.0054	-0.0163
	(0.0171)	(0.0345)	(0.0090)	(0.0186)
Turnover ×D(Year 0 Month 4)	-0.0139	-0.0188	-0.0020	-0.0052
	(0.0166)	(0.0338)	(0.0079)	(0.0168)
Turnover ×D(Year 0 Month 5)	0.0069	-0.0176	-0.0082	-0.0040
	(0.0155)	(0.0342)	(0.0086)	(0.0166)
Turnover ×D(Year 0 Month 6)	0.0109	-0.0006	0.0024	-0.0001
	(0.0158)	(0.0331)	(0.0097)	(0.0174)
Turnover ×D(Year 0 Month 7)	-0.0015	-0.0259	-0.0053	-0.0009
	(0.0148)	(0.0367)	(0.0076)	(0.0186)
Turnover ×D(Year 0 Month 8)	0.0199	-0.0428	-0.0100	-0.0182
	(0.0158)	(0.0307)	(0.0074)	(0.0154)
Turnover ×D(Year 0 Month 9)	-0.0167	-0.0068	-0.0028	-0.0007
	(0.0145)	(0.0283)	(0.0080)	(0.0137)
Turnover ×D(Year 0 Month 10)	0.0890***	-0.0322	0.0138	-0.0331**
	(0.0193)	(0.0272)	(0.0088)	(0.0134)
Turnover ×D(Year 0 Month 11)	0.0456***	-0.0210	-0.0079	-0.0230*
	(0.0168)	(0.0262)	(0.0108)	(0.0127)
Turnover ×D(Year 0 Month 12)	0.0489	-0.0063	-0.0064	-0.0244*
	(0.0331)	(0.0278)	(0.0145)	(0.0135)
Turnover ×D(Year 1 Month 1)	0.0107	0.1530***	-0.0117	0.0700*
	(0.0184)	(0.0566)	(0.0107)	(0.0377)
Turnover ×D(Year 1 Month 2)	0.0108	0.0552	-0.0010	0.0742***
	(0.0198)	(0.0419)	(0.0093)	(0.0264)
Fit statistics				
Time FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Mean Dep. Var	0.09585377	0.2278199	0.0169416	0.0736
Using Bandwidth	14.	14.	14.	14.
Municipalities	3,943	3,943	3,943	3,943
Observations	113,292	113,292	113,292	113,292
R^2	0.28448	0.30795	0.27909	0.35540

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. The dependent variables are the counts of dismissals and hirings of health workers, separated by type of contract (civil servants, and temporaries). The variables are normalized by the inverse hyperbolic sine transformation. Mean Dep. Var. is the (untransformed) average dependent variable one year before elections for the treated group. All regressions include time fixed effects.

Table (A.7) Human resources turnover, heterogeneity in dismissals using Fiscal Responsability Law proportion

Dependent Variables:	Civil Servants, Dismissals	
	Below Md. LRF (1)	Above Md. LRF (2)
Turnover	-0.0085	-0.1199*
	(0.0188)	(0.0690)
MV	0.0018	0.0024
	(0.0014)	(0.0059)
Turnover X MV	-0.0020	0.0093
	(0.0020)	(0.0078)
Turnover ×D(Year 0 Month 8)	0.0107	0.0297
	(0.0196)	(0.0302)
Turnover ×D(Year 0 Month 9)	-0.0191	-0.0095
	(0.0171)	(0.0285)
Turnover ×D(Year 0 Month 10)	0.0630**	0.1304***
	(0.0250)	(0.0364)
Turnover ×D(Year 0 Month 11)	0.0248	0.0833**
	(0.0202)	(0.0338)
Turnover ×D(Year 0 Month 12)	0.0372	0.0807
	(0.0338)	(0.0696)
Turnover ×D(Year 1 Month 1)	-0.0102	0.0457
	(0.0229)	(0.0319)
Fit statistics		
Time FE	Yes	Yes
Municipality FE	Yes	Yes
Mean Dep. Var	0.02690583	0.1496865
Bandwidth	14.	14.
Municipalities	1,540	2,406
Observations	53,556	56,712
R^2	0.27059	0.31382

Notes: Standard Errors Clustered at the municipality level. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. The dependent variables are the counts of dismissals and hirings of health workers under civil servant contracts, below and above the Fiscal Responsibility Law median proportion (45% of municipal spending with personnel). The variables are normalized by the inverse hyperbolic sine transformation. Mean Dep. Var. is the (untransformed) average dependent variable one year before elections for the treated group. All regressions include time fixed effects.

Table (A.8) Human resources turnover, heterogeneity in dismissals using Fiscal Responsability Law proportion interaction

Dependent Variable:	Dismissals
Model:	(1)
Turnover	-0.0572
	(0.0355)
Above Md LRF	-0.0078
	(0.0181)
MV	0.0035
	(0.0028)
Turnover × Above Md LRF	-0.0054
	(0.0288)
Turnover X MV	0.0014
	(0.0037)
Turnover ×D(Year 0 Month 10)	0.0675***
	(0.0252)
Turnover ×D(Year 0 Month 11)	0.0292
	(0.0204)
Turnover ×D(Year 0 Month 12)	0.0419
	(0.0342)
Above Md LRF×D(Year 0 Month 10)	0.0068
	(0.0243)
Above Md LRF xD(Year 0 Month 11)	-0.0249
	(0.0206)
Above Md LRF ×D(Year 0 Month 12)	0.1174**
	(0.0537)
Turnover × Above Md LRF ×D(Year 0 Month 10)	0.0621
	(0.0450)
Turnover × Above Md LRF ×D(Year 0 Month 11)	0.0495
	(0.0394)
Turnover × Above Md LRF ×D(Year 0 Month 12)	0.0427
	(0.0771)
Fit statistics	
Time FE	Yes
Municipality FE	Yes
Mean Dep. Var	0.02690583
Bandwidth	14.
Municipalities	3,943
Observations	110,268
R^2	0.28608

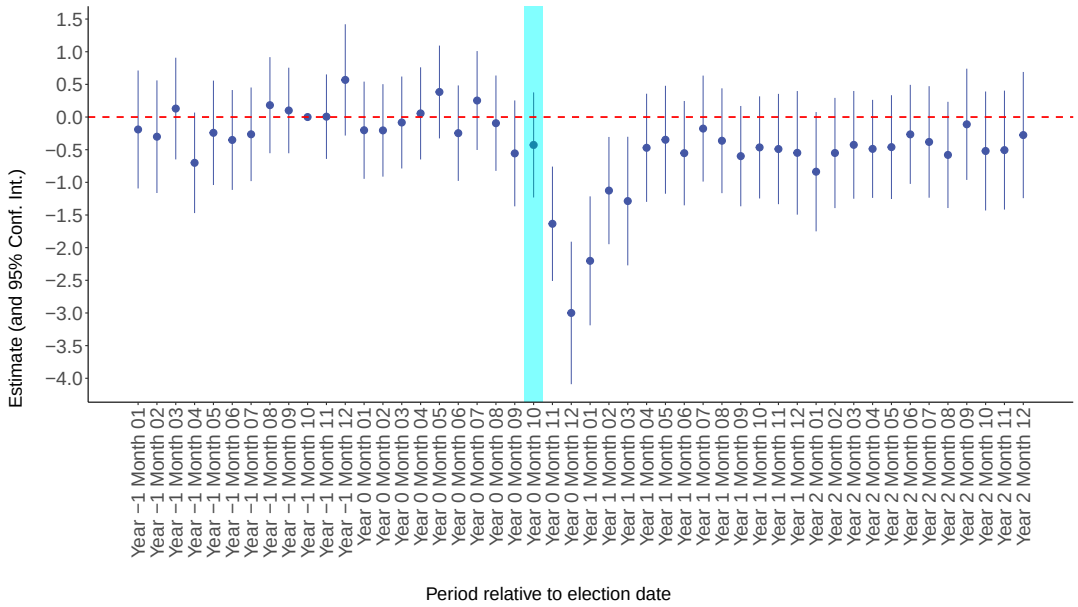
Clustered (codmun) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B Extended Analysis

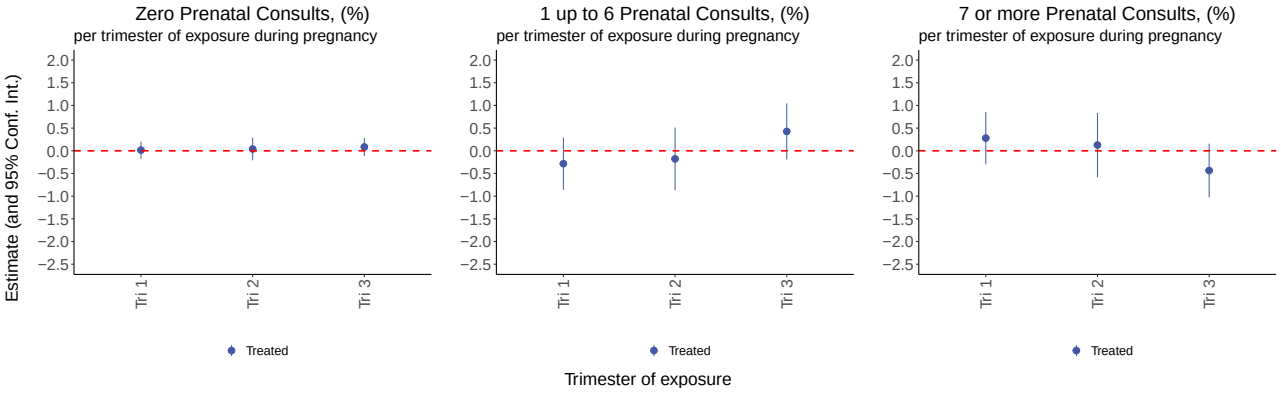
This appendix provides a robustness exercise to show that the documented effects do not appear in periods far from the elections and the transition period. Results shown here are merely extending the coefficients estimated previously in the analysis.

Figure (B.1) SIAB, Month



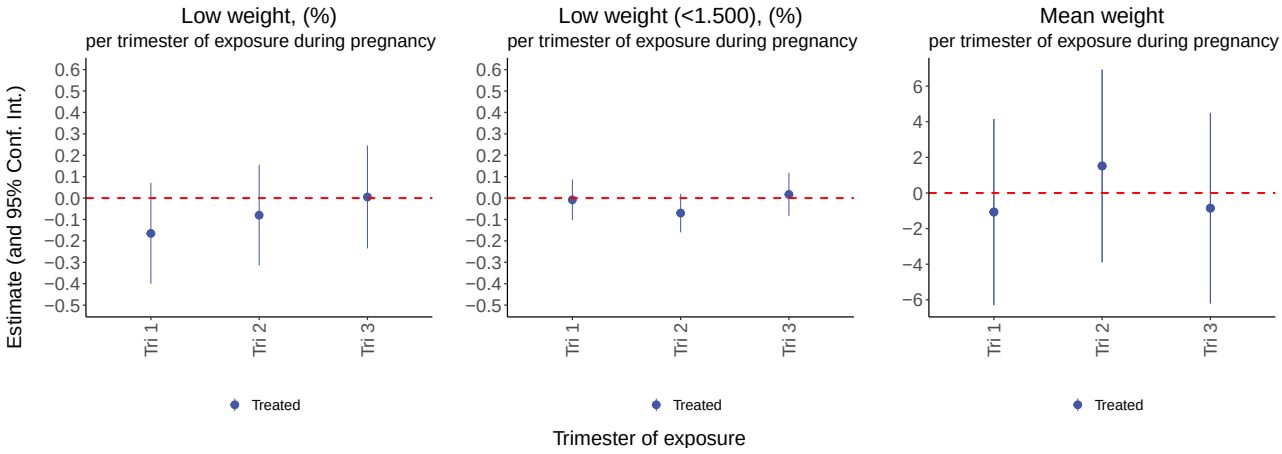
Note: This figures shows graphically results using a municipality by month panel for the 2008 and 2012 electoral periods. The dependent variable is the share of pregnancies with prenatal consults in month. We examine results from January in the year preceding elections (Year -1) up to December of second year following elections (Year 2). In an effort to keep the estimation sample fixed we select the optimal bandwidth using the infant mortality rate variable. Nevertheless, the results are not sensitive to this procedure.The blue shaded area represents the time elections are held.

Figure (B.2) Prenatal Coverage



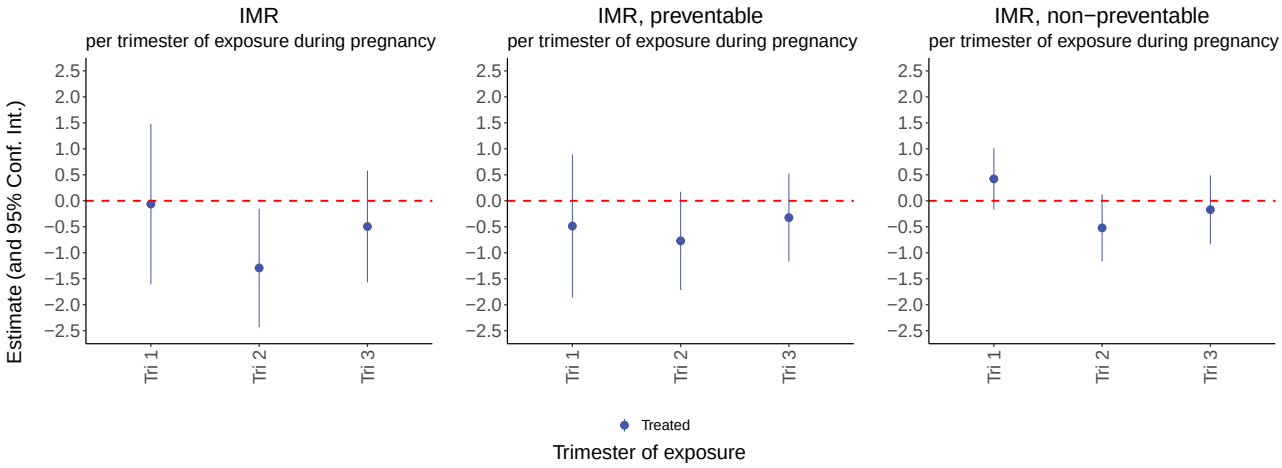
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consults. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. We add 12 months to the actual period of effects, so that this represents a robustness exercise focused on the year after elections. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

Figure (B.3) Infant Mortality Rates



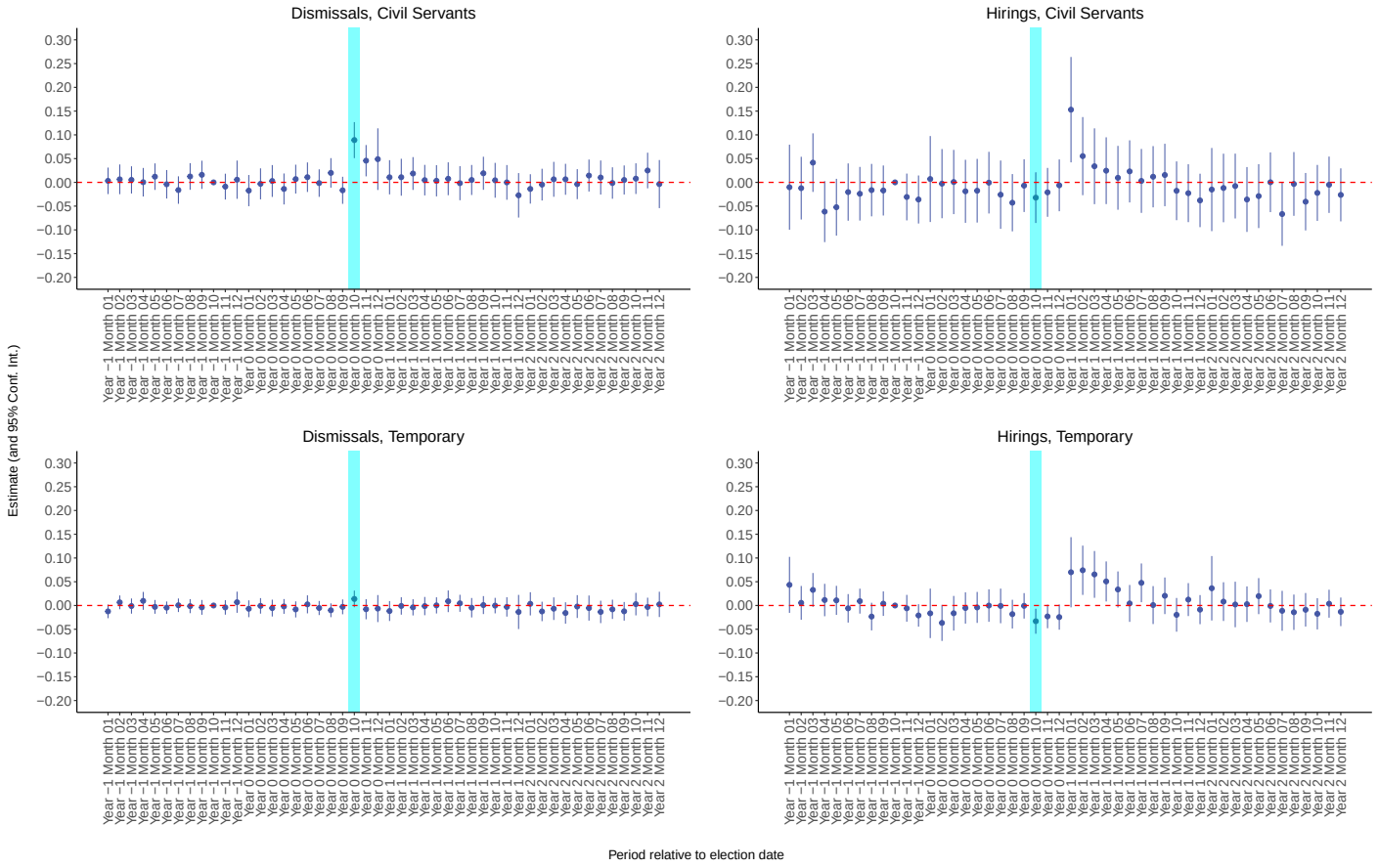
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the share of premature (less than 37 weeks of gestation) births and the share of low weight (less than 2.500 grams). 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. We add 12 months to the actual period of effects, so that this represents a robustness exercise focused on the year after elections. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

Figure (B.4) Infant Mortality Rates



Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are Infant Mortality Rates (total, perinatal, and others). 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. We add 12 months to the actual period of effects, so that this represents a robustness exercise focused on the year after elections. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

Figure (B.5) Human resources turnover, civil servants

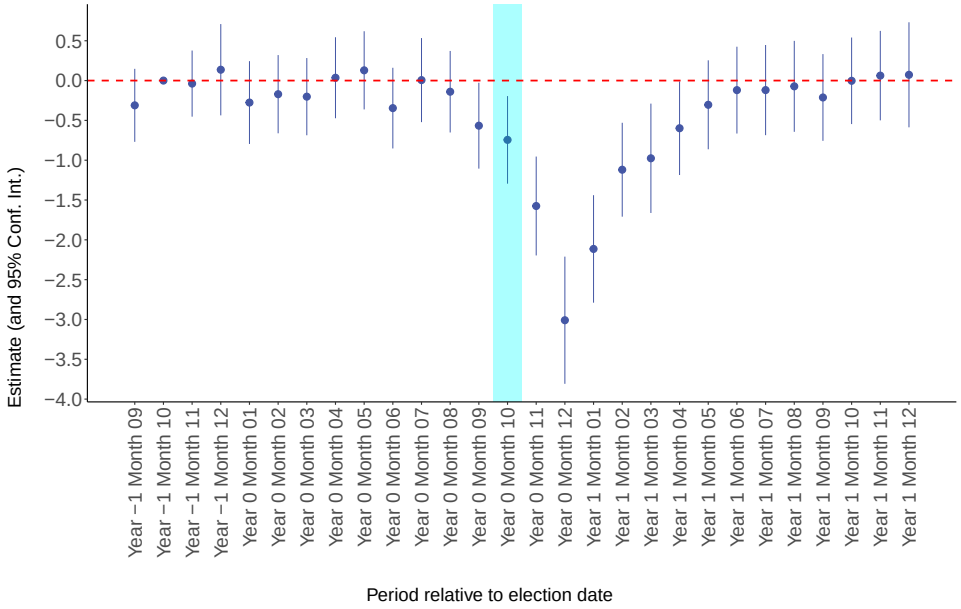


Note: This figures shows graphically results using a municipality-by-month panel covering the 2008- 2019 period. The dependent variables are the counts of dismissals and hirings of doctors, separared by type of contract (civil servants, and temporaries). The variables are normalised by the inverse hyperbolic sine transformation. We examine results from January in the year preceding elections (Year -1) up to December of the second year of the next government (Year 2). In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

C Whole Sample

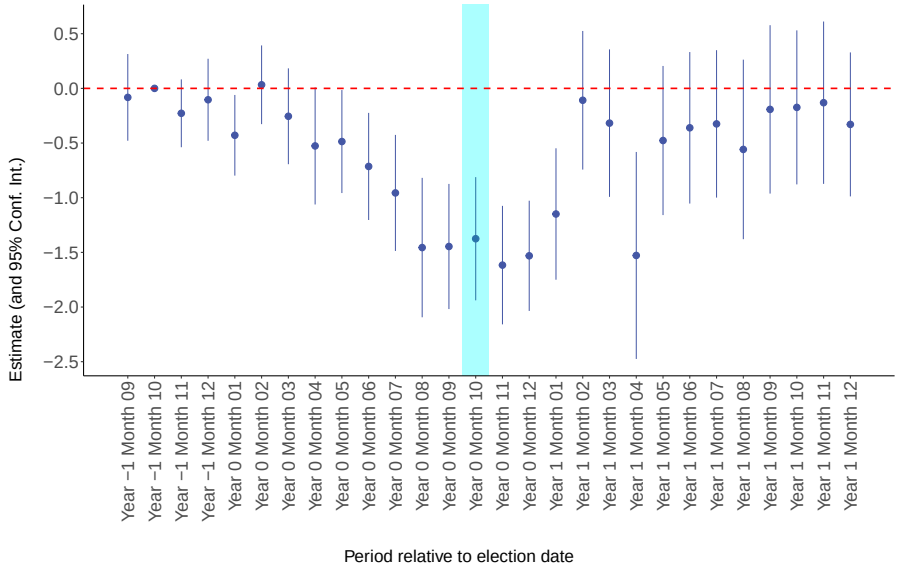
This appendix we perform a robustness exercise where we remove the restriction on close elections and analyze results for all elections where an incumbent tried to run for reelection and had at least one opponent.

Figure (C.1) SIAB, Month



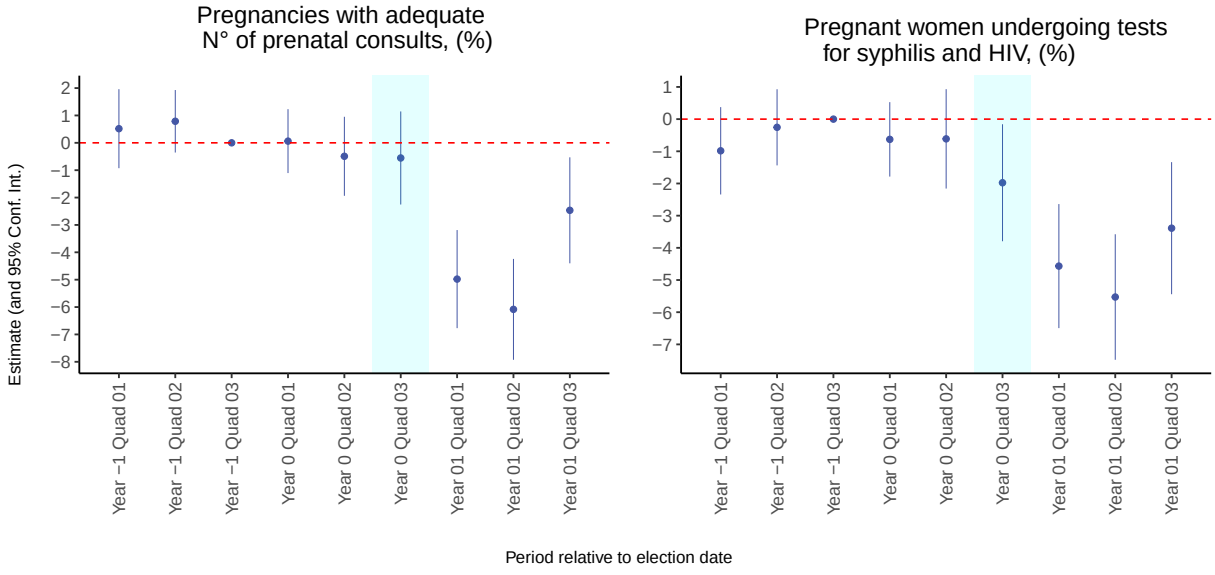
Note: This figures shows graphically results using a municipality by month panel for the 2008 and 2012 electoral periods. The dependent variable is the share of pregnancies with prenatal consults in month. The blue shaded area represents the time elections are held. We do not restrict the analysis to any bandwidth.

Figure (C.2) SISAB, Month



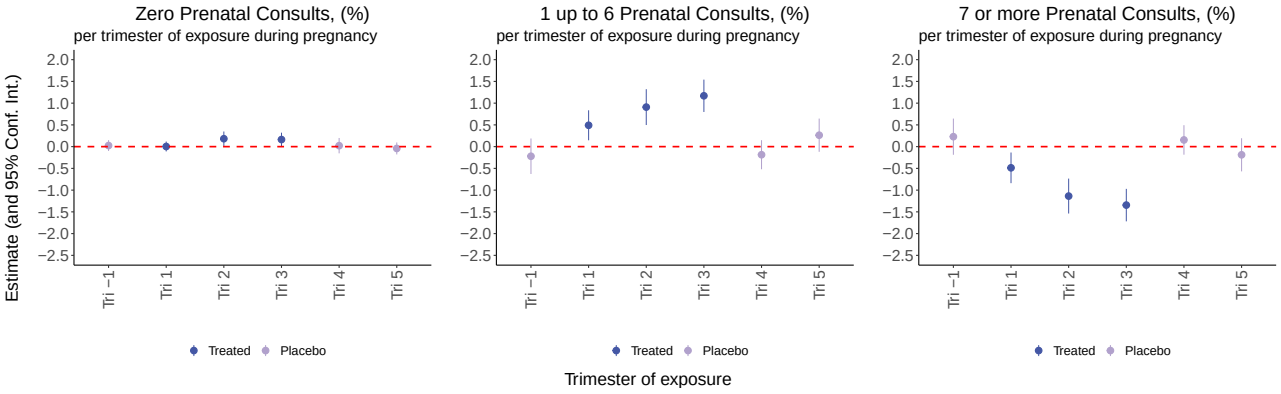
Note: This figures shows graphically results using a municipality by month panel for the 2020 electoral period. The dependent variable is total of pregnancies that ended up with 6 or more prenatal consultations by month of the first consult. The blue shaded area represents the time elections are held. We do not restrict the analysis to any bandwidth.

Figure (C.3) SISAB, Quarter



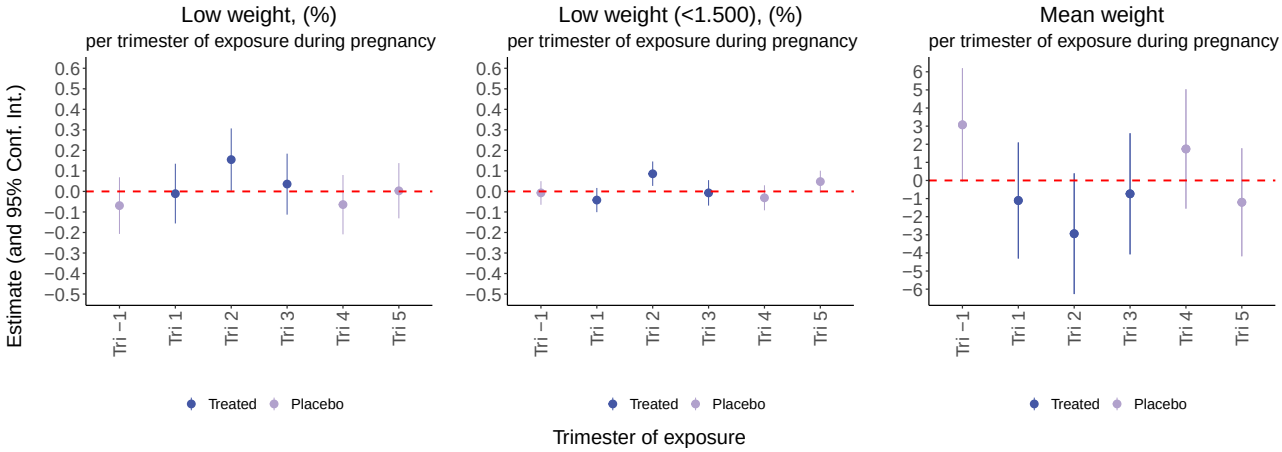
Note: This figures shows graphically results using a municipality by quarter panel for the 2020 electoral period. The dependent variables are the share of pregnancies correct number of prenatal consults and the share of pregnant women undergoing tests for syphillis and HIV. The blue shaded area represents the time elections are held. We do not restrict our sample to any bandwidth.

Figure (C.4) Prenatal Coverage



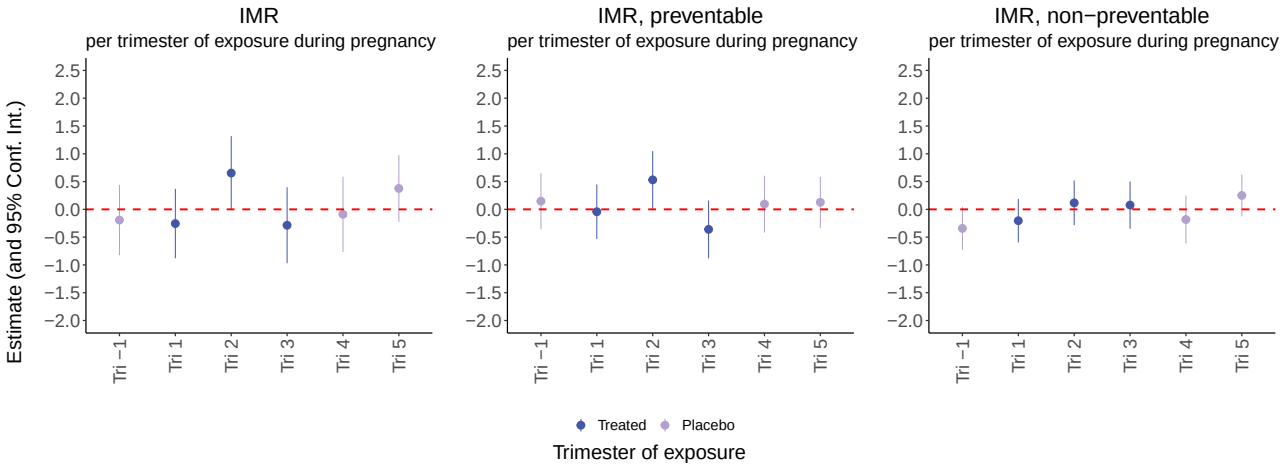
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consults. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos. We do not restrict our sample to any bandwidth.

Figure (C.5) Outcomes at Birth



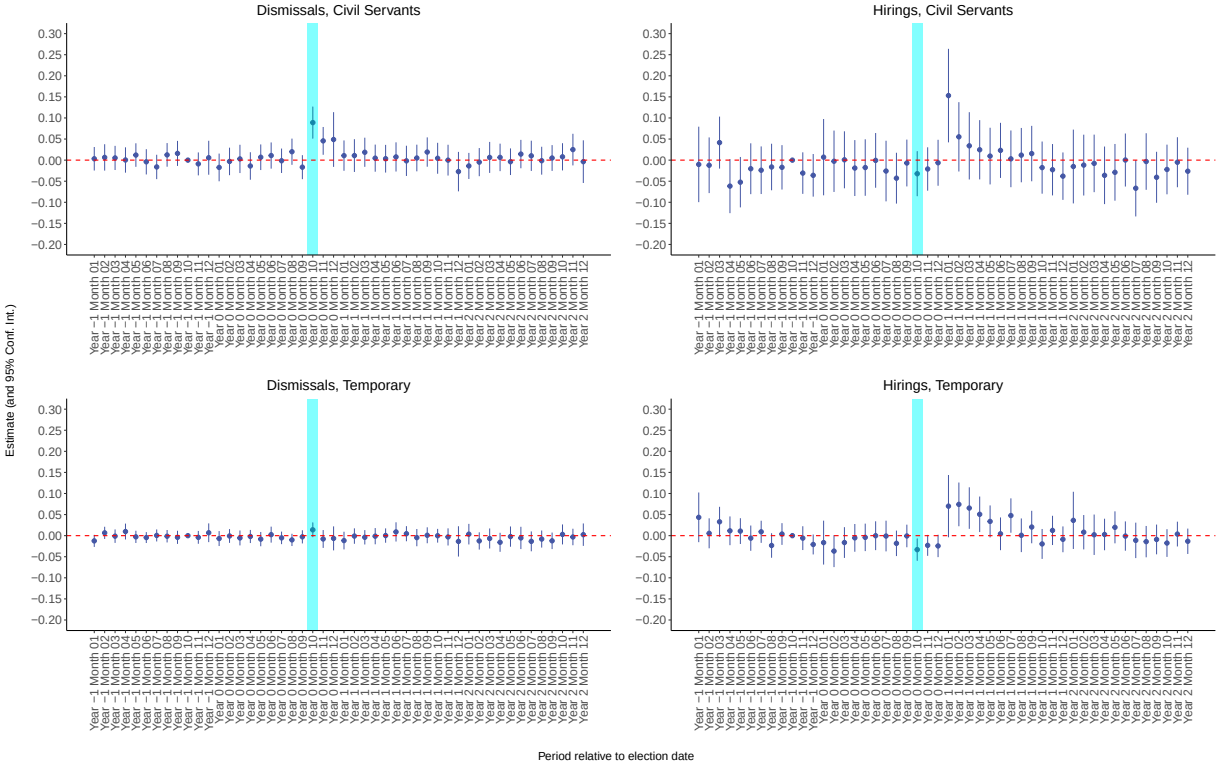
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the share of premature (less than 37 weeks of gestation) births and the share of low weight (less than 2.500 grams). 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos. We do not restrict our sample to any bandwidth.

Figure (C.6) Infant Mortality Rates



Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are Infant Mortality Rates (total, perinatal, and others). 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos. We do not restrict our sample to any bandwidth.

Figure (C.7) Human resources turnover

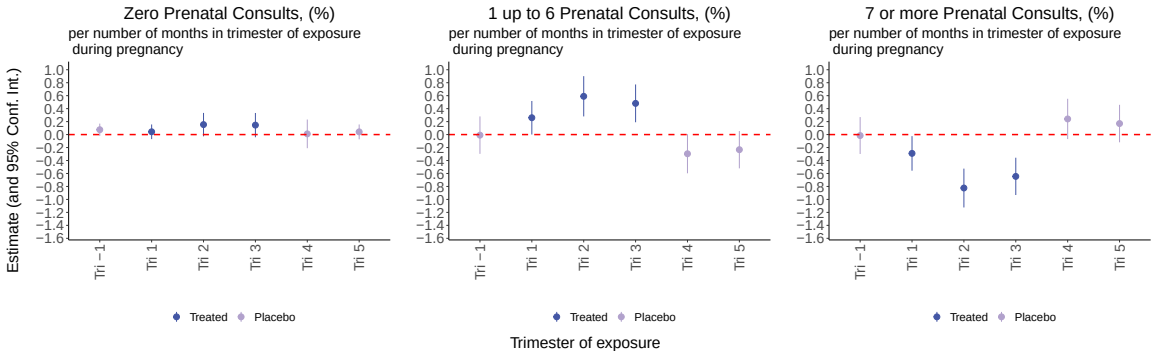


Note: This figures shows graphically results using a municipality-by-month panel covering the 2011-2018 period. The dependent variables are the counts of physician contracts working inside/outside of primary health care faclities, normalised by the inverse hyperbolic sine transformation. We do not restrict our sample to any bandwidth.

D Birth Outcomes and Mortality alternative exercise

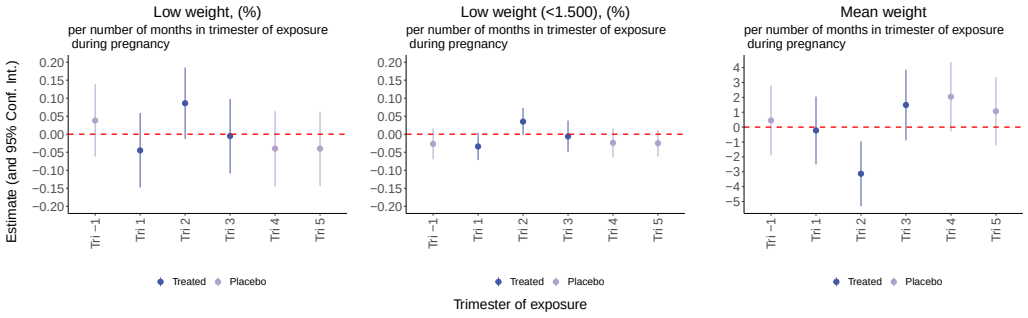
This appendix presents additional robustness exercises to the section of results on Birth Outcomes and Mortality.

Figure (D.1) Prenatal Coverage



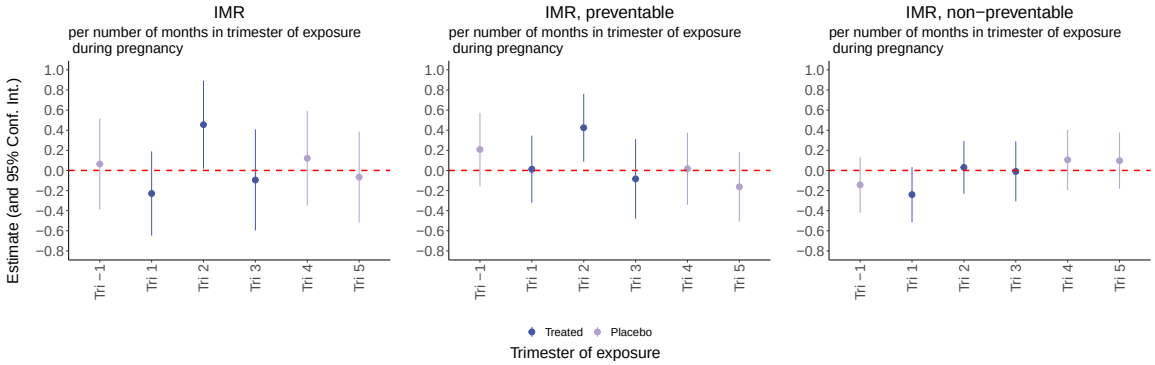
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consults per number of months of exposure in each trimester during pregnancy. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

Figure (D.2) Outcomes at Birth



Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the share of premature (less than 37 weeks of gestation) births and the share of low weight (less than 2.500 grams), per number of months of exposure in each trimester during pregnancy. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

Figure (D.3) Infant Mortality Rates

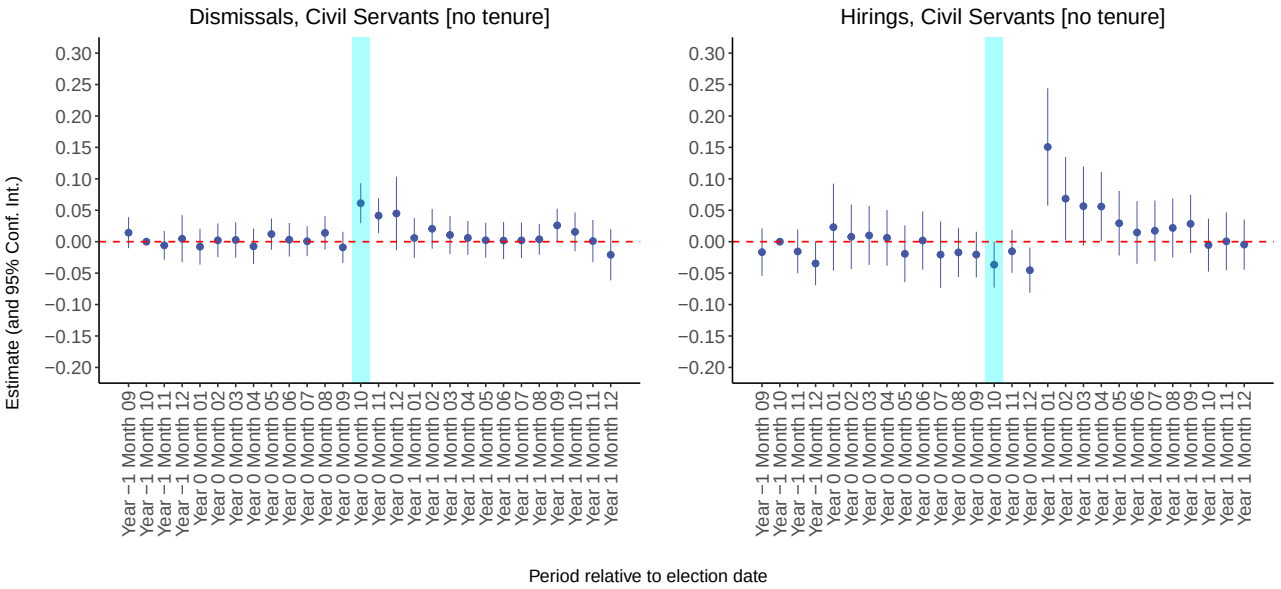


Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are Infant Mortality Rates (total, perinatal, and others) per number of months of exposure in each trimester during pregnancy. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -1, 4 and 5 are placebos. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

E Human Resources alternative exercise

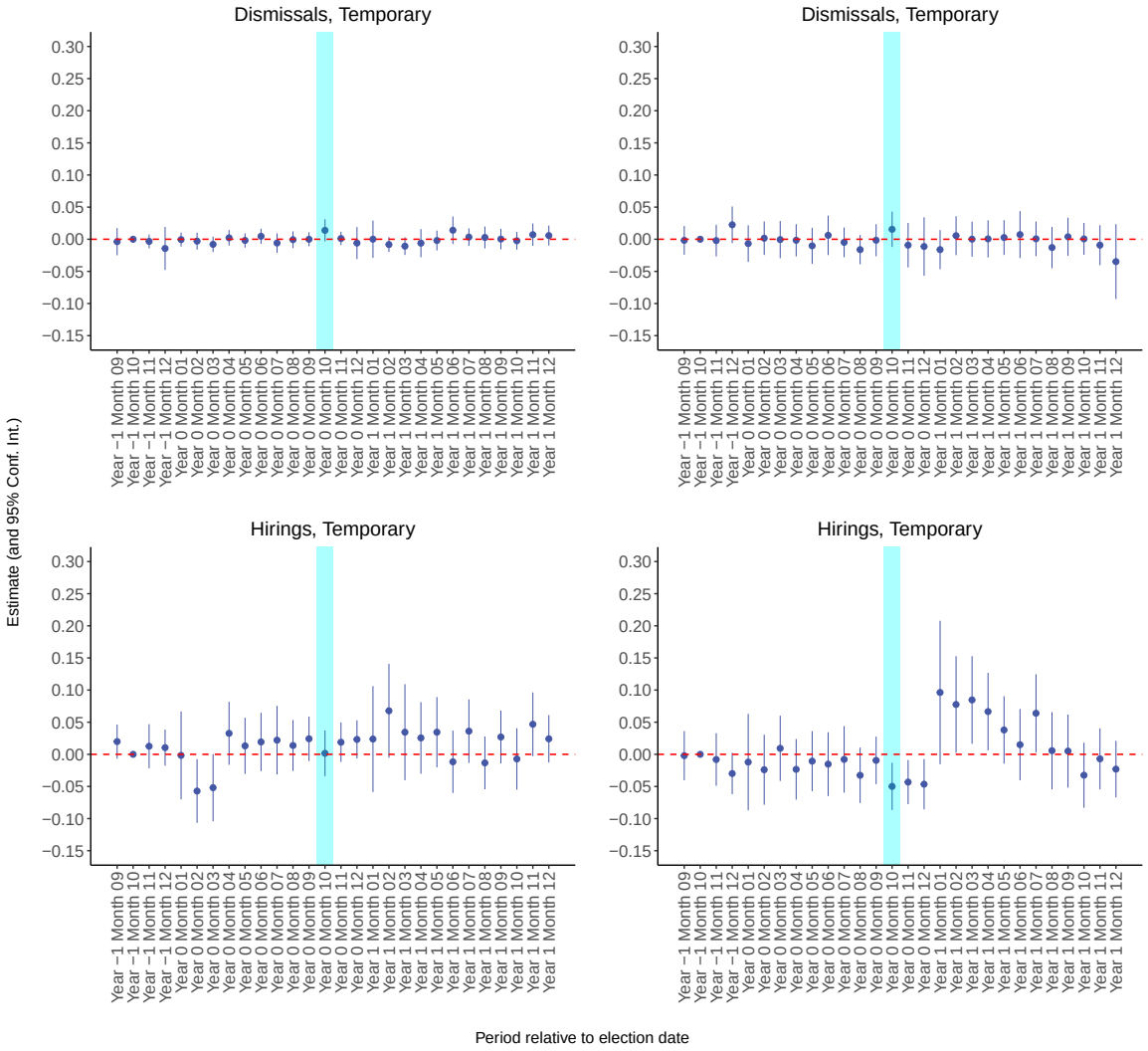
This appendix presents additional robustness exercises to the section of results on Human Resources

Figure (E.1) Human resources turnover, Non-tenured civil servants



Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the counts of dismissals and hirings of healthcare workers in civil servant contracts without tenure. The variables are normalized by the inverse hyperbolic sine transformation. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors.

Figure (E.2) Human resources turnover, heterogeneity in hirings and dismissals using Fiscal Responsability Law proportion for Temporaries

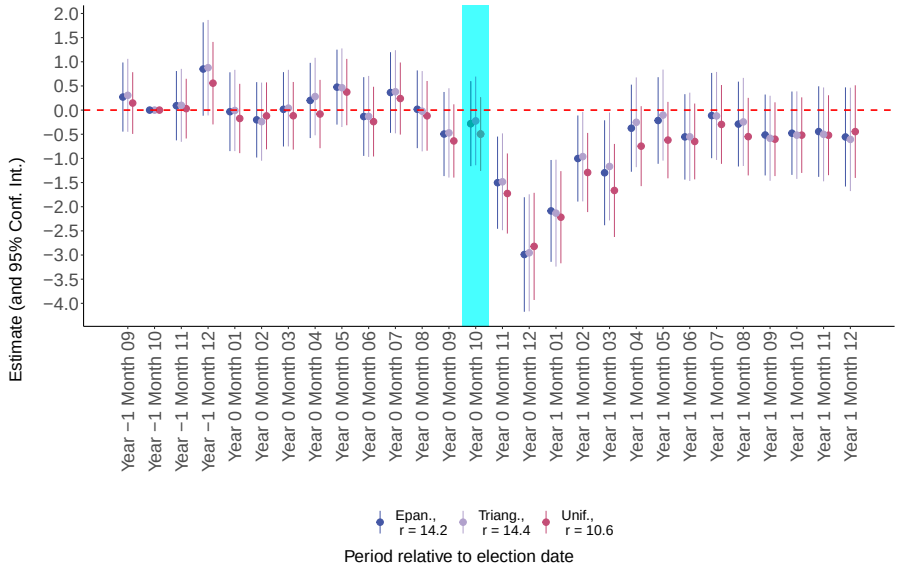


Note: This figure shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the counts of dismissals and hirings of healthcare workers under temporary contracts, below and above the Fiscal Responsibility Law median proportion. The variables are normalized by the inverse hyperbolic sine transformation. In an effort to keep the estimation sample fixed we set a bandwidth equal to 14 for all regressors. This figure shows results regarding compliance with the LRF in electoral years (this median is close to 45% of municipal spending with personnel).

F Bandwidth Robustness

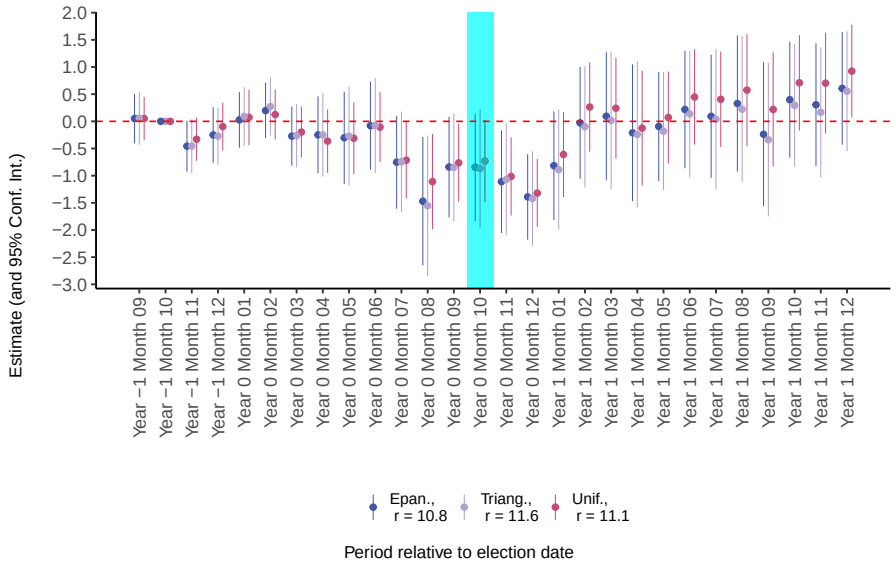
This appendix provides evidence that the choice to perform our main analysis with the same bandwidth to keep the estimation sample fixed does not overall change our results vis-a-vis to choosing the optimal bandwidth for each variable.

Figure (F.1) SIAB, Month



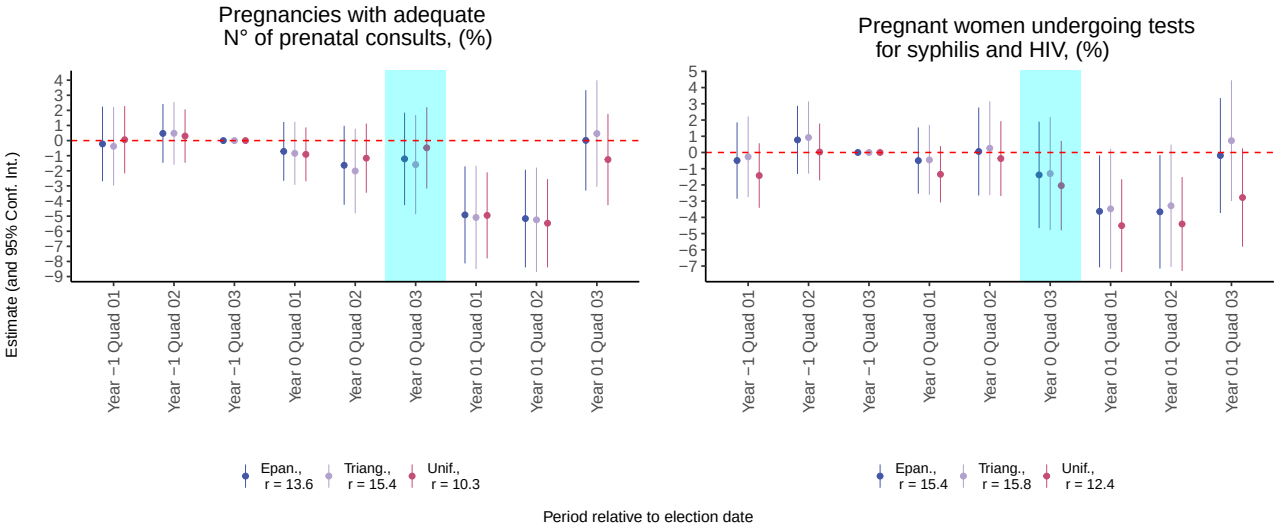
Note: This figures shows graphically results using a municipality by month panel for the 2008 and 2012 electoral periods. The dependent variable is the share of pregnancies with prenatal consults in month. We examine results from January in the year preceding elections (Year -1) up to December of second year following elections (Year 2). Here we test different weights with the respective optimal bandwidth.

Figure (F.2) Prenatal Outcomes SISAB, monthly



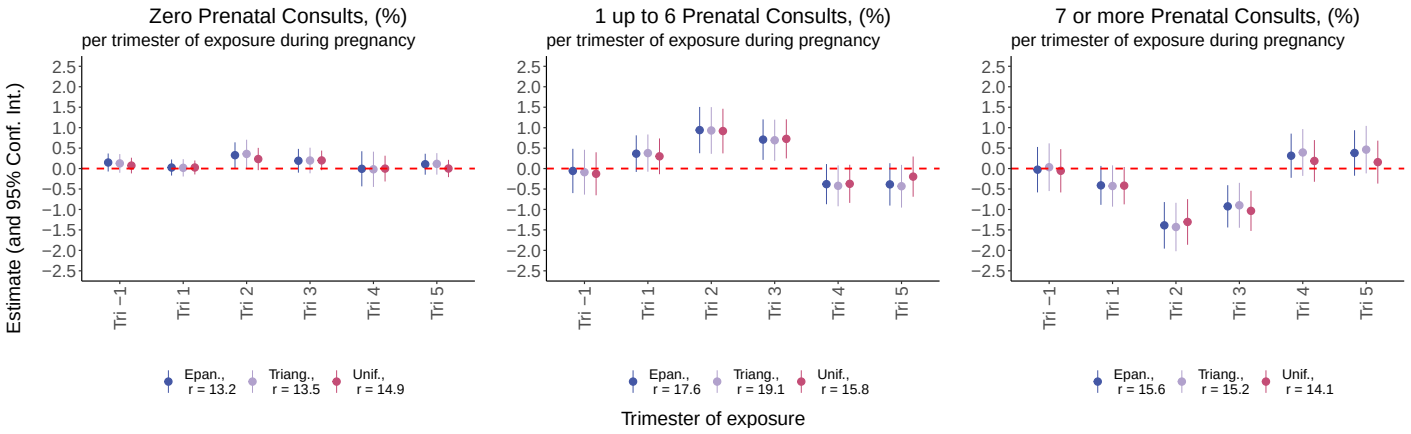
Note: This figures shows graphically results using a municipality by month panel for the 2020 electoral period. The dependent variable is total of pregnancies that ended up with 6 or more prenatal consultations by month of the first consult. We examine results from September in the year preceding elections (Year -1) up to December of the year following elections (Year 1). Here we test different weights with the respective optimal bandwidth.

Figure (F.3) Prenatal Outcomes SISAB, quadrimester



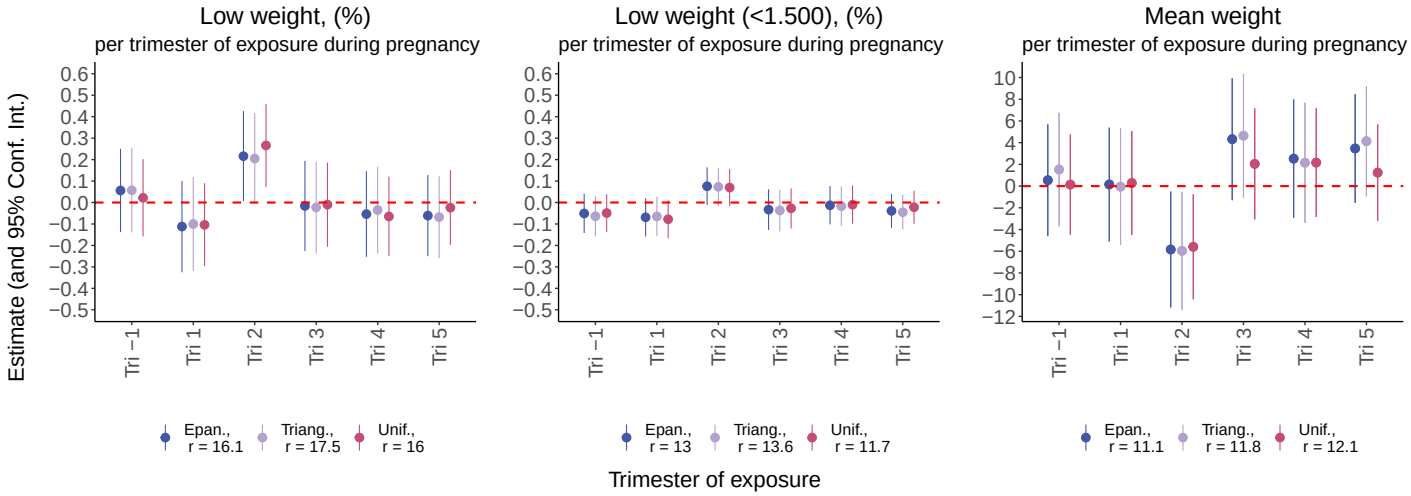
Note: This figures shows graphically results using a municipality by quadrimester panel for the 2020 electoral period. The dependent variables are the share of pregnancies ending with the correct number of prenatal consults and the share of pregnant women undergoing tests for syphilis and HIV. We examine results from the first quadrimester in 2019 (Year -1) up to the last quadrimester 2021 (Year 1). The blue shaded area represents the time elections are held. Here we test different weights with the respective optimal bandwidth.

Figure (F.4) Prenatal Coverage



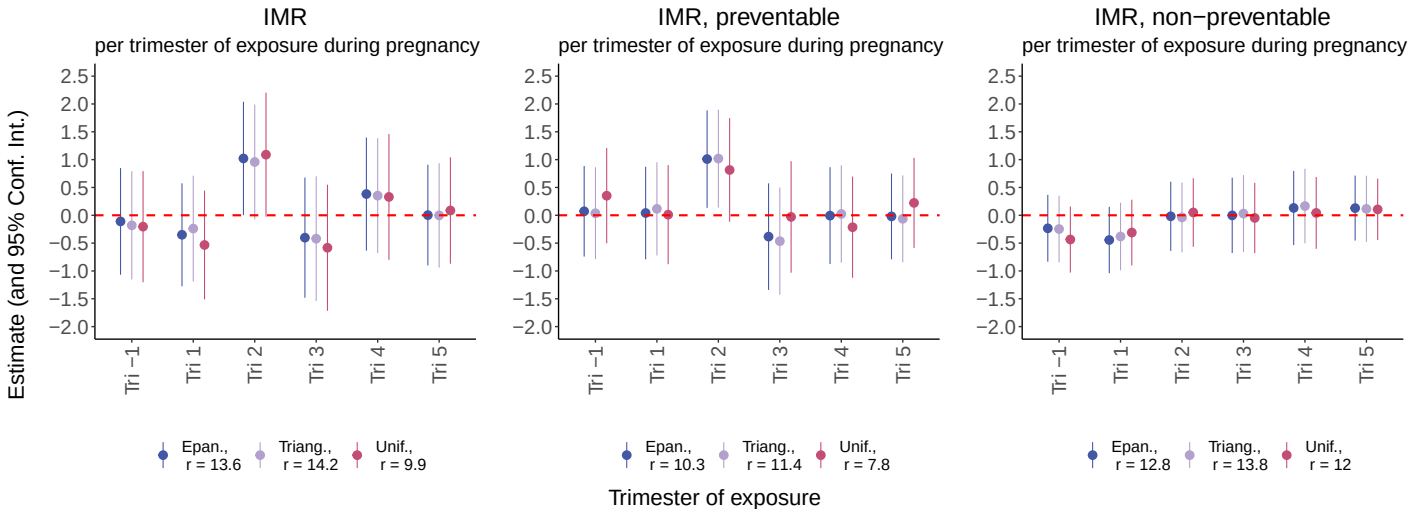
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consults per number of months of exposure in each trimester during pregnancy. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -2, -1, 4 and 5 are placebos. Here we test different weights with the respective optimal bandwidth.

Figure (F.5) Outcomes at Birth



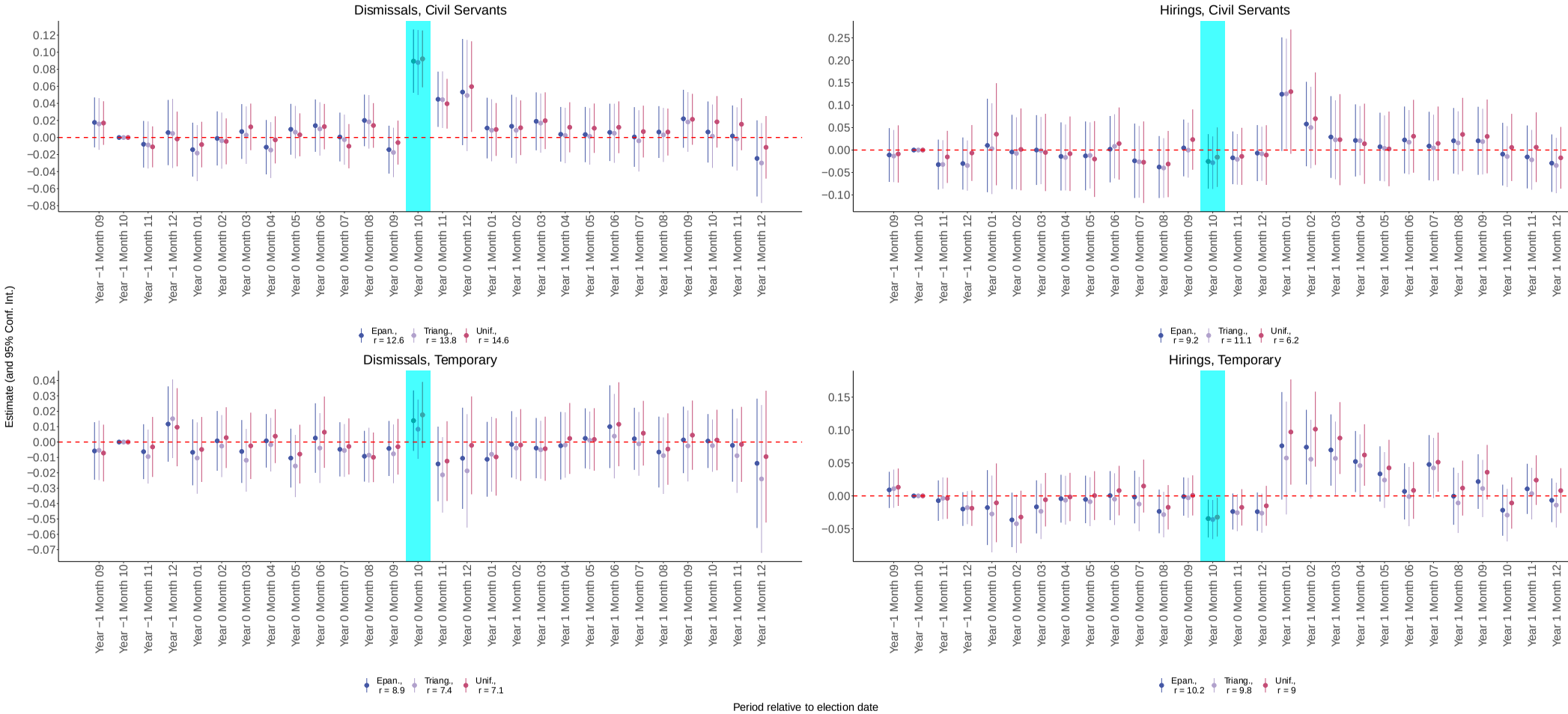
Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the share of premature (less than 37 weeks of gestation) births and the share of low weight (less than 2.500 grams), per number of months of exposure in each trimester during pregnancy. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -2, -1, 4 and 5 are placebos. Here we test different weights with the respective optimal bandwidth.

Figure (F.6) Infant Mortality Rates



Note: This figures shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are Infant Mortality Rates (total, perinatal, and others) per number of months of exposure in each trimester during pregnancy. 'Tri' refers to the trimester of the pregnancy when the child was in-utero exposed to the negative shock caused by political turnover. Trimesters -2, -1, 4 and 5 are placebos. Here we test different weights with the respective optimal bandwidth.

Figure (F.7) Human resources turnover



Note: This figure shows graphically results using a municipality-by-month panel covering the 2008-2019 period. The dependent variables are the counts of dismissals and hirings of healthcare workers, separated by type of contract (civil servants and temporaries). The variables are normalized by the inverse hyperbolic sine transformation. Here we test different weights with the respective optimal bandwidth.