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Abstract

Can political turnover and government transitions, which are transitory by nature, have adverse impacts on development outcomes? We conjecture that this can be the case should transitions coincide with critical periods for human development. To test for this, we examine transition effects on health services and infant health outcomes in Brazilian municipalities by using municipality-by-month level data, and close elections for identification. We find negative effects on health services delivery, starting immediately after elections and continuing throughout the transition period and the beginning of the newly elected government. These fluctuations have irreversible impacts when they overlap with critical gestational periods. While the most critical period of exposure lasts for a few months, by the turn of governments, adverse effects are non-trivial. Children exposed to transitions in utero have worse outcomes at birth and experience an increase in mortality by 0.96 deaths per 1,000 live births, or 6.7% of the mean. Given the timing of the gestational period and child development, these effects manifest solely during the new administration, throughout the first two years after the change in government has occurred. Fiscal incentives and health care personnel dismissals, used to reduce spending before the end of the electoral term, are relevant mechanisms.

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

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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 lead to decisions that harm the quality of public services (Toral, 2023b). 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 have more far-reaching effects on development outcomes at the micro level, beyond the potential transitory effects on the provision of public services. More generally, empirical evidence on the social costs of transition processes is still scant.

In principle, transitions may bear only transitory costs, diluted over time and without irreversible consequences. That might be the case through the replacement of professionals of the same quality or temporary operational disruptions without lasting impacts. More generally, that might be the case should government organizations remain immune to transitory changes in personnel and political incentives. In fact, while existing studies document negative effects of transitions on service delivery, there is not any conclusive evidence on whether political turnover and government transitions have more consequential impacts on development outcomes at the micro level. 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 employees. Adverse effects on educational outcomes, however, seem to fade away over time. Here we conjecture that while government transitions are transitory by nature, their effects can be particularly pervasive should they coincide with critical periods for investments in inputs conducive to human and economic development. In that case, variations in service provision may lead to more costly consequences.

In this paper, we use data from Brazilian municipal elections to assess the effects of political turnover and government transitions on health services and infant health outcomes. Estimating effects on health services and outcomes provides us with a direct assessment of our conjecture, as health care seekers often require interventions at specific points in time and timely access to services when needed. We first investigate and fully characterize whether and how political turnover and transitions lead 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 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 irreversible birth outcomes of those exposed in utero, in particular infant mortality rates throughout the first year of life.

We focus both on the transition period and on the first year of the newly elected government. 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 in 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 turnover and transitions on health services and outcomes, we rely on a event study design with close elections. 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. 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 turnover should be as good as random in close elections. We provide evidence in this regard.

We first document that political turnover is associated with temporary but sharp reductions in health care service production. Right after elections happen, in October of the electoral year, we observe a reduction of prenatal consultations 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 in December, the last month of the defeated government, and corresponds to 10.3% of the mean number of consultations. We also observe that the transitory impact on service delivery translates into persistent effects on access to services, and less consultations as measured at the end of pregnancies. We find a reduced share of pregnancies that ended up with the adequate number of prenatal consultations (decrease of 5 percentage points) and that tested for syphilis and HIV (reduction of 4 percentage points).

We then investigate health outcomes at birth. 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, 2019; Rocha and Soares, 2015; Schwandt, 2019). 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 an increase in the share of low and 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 development in the long-run (Mara, 2003; Shenkin, Starr, and Deary, 2004; Linnet, Wisborg, Agerbo, Secher, Thomsen, and Henriksen, 2006; Currie and Almond, 2011). Even those children that survive may therefore face adverse conditions and future impairment.

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 6.7% of the mean. When considering deaths caused by preventable causes (amenable to health care) the effect is even larger, an increase of 1.0 deaths per 1,000 live births (11.7% of the mean). We

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

compare this result to that found by Bhalotra, Rocha, and Soares (2019). 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. In that sense, a large scale and successful restructure of the Brazilian health system had a similar magnitude of impact on infant mortality rates after two years as did a short-lived government transition period. While the most critical period of exposure lasts for a few months, localized at the turn of the year, effects are therefore non-trivial. Moreover, although exposure takes place during the transition process and by the turn of governments, 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. 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 politicians' behavior and 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. Choosing close elections therefore provides us with an quasi-experimental setting to overcome identification concerns. Yet, we present additional evidence on statistically similar post-electoral impacts in both close and general 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 health care sector. 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 health care personnel under civil servant contracts during the same time. To further understand these effects, we divide the sample of civil servant contracts into three common occupations in primary health care: physicians, nurses, and community health workers. We find that effects are present in all occupations, but seem to last longer among higher paid ones, such as for physicians. The effects are also present only in contact terminations initiated by the government. Terminations initiated by the employee or because of retirement are not affected by the transition process.

What could explain the patterns in dismissals, particularly among physicians? As theoretically suggested by Toral (2023b), 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. During this time politicians may become increasingly concerned about their future, fearing prosecution for wrongdoings while in office or may 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] We perform additional heterogeneity analyses to test for incumbent incentives and career concerns. 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 incumbents were close to the cap during the election year, they would have incentives to clear the accounts before stepping out of office. We split the sample between municipalities below and above the median share of

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

revenue spent with personnel. Results show that municipalities close to the cap dismiss more workers under civil servant contracts – especially those who do not have contractual stability or tenure. This can explain why dismissals are particularly high 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) document the presence of upheavals in municipal bureaucracy associated with turnovers at municipal elections. Moreover, the authors extend their analysis to test for specific effects on public service provision and outcomes in education. They find that increases in the replacement of personnel in schools were associated with lower test scores in the following years. This result is relevant as it links what happens to the bureaucracy with outcomes that most directly affect citizens. Toral (2023b) also studied political turnover in Brazilian municipal elections and was the first to look at transition periods, documenting impacts on the composition of the bureaucracy and on healthcare service delivery.

We contribute to the literature by providing novel evidence on the social costs of transition processes. We document far-reaching effects on birth and health outcomes, beyond the transitory effects in the provision of public services. While transitions may lead to temporary service disruption, as documented by Toral (2023b), 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 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 map critical periods and reveal irreversible effects on child outcomes, but also enables us to provide a thorough characterization of cycles triggered by transition processes. In that sense, we complement Akhmedov and Zhuravskaya (2004) by documenting political cycles not only in service provision, as a response to fiscal incentives within the electoral period, but also in health outcomes, as effects manifest only in the following government. 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 also complement both Akhtari et al. (2022) and Toral (2023b). 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 (2023b).

Finally, we examine specific mechanisms, thus leading to 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 can have at the macro level and, more generally, from democratic elections.

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., 2019). 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 health care production and the largest spending category in the municipal health budget (Medeiros, Albuquerque, Tavares, and Souza, 2017). Yet, there are spending limits on personnel. 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 spend 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 consultations, which should begin as soon as the mother starts the gestational period. The Ministry of Health recommends at least six prenatal consultations, the first being in the first trimester of gestation, for an adequate following of the pregnancy. Prenatal consultations are crucial for, among others, identifying diseases the mother might have that are detrimental to the baby's health if not diagnosed and treated. 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, Camargo, and Balhotra, 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 consultations 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 consultations 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., 2019).

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.

3.1 Electoral Data

Our study covers the 2008, 2012, 2016, and 2020 electoral cycles. We obtain electoral data from the Superior Electoral Court's (TSE) repository. We focus on mayoral races, and use candidates' data to

[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 be not as insulated from political influence as commonly assumed (Toral, 2023b).

find the identity of the incumbent mayor and who the candidates that ran in each electoral period were. 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 number of pregnant women who underwent a prenatal consultation with a doctor or nurse in a health facility in a given month. 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 and who completed at least six prenatal consultations, and the share of women that gave birth in a given quarter that underwent tests for syphilis and HIV.

We use vital statistics to construct birth outcomes. The National System of Information on Birth Records (SINASC, Ministry of Health) provides detailed information on all registered births in Brazil.[4] 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 consultations the mother had during the gestational period. We also construct 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.

The National System of Mortality Records (SIM, Ministry of Health) provides detailed information on every officially registered death in Brazil. We select all deaths of children up to one year of age (infant deaths). We gather information on the municipality of residence, 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 health care.[5]

We merge information from SINASC and SIM to build a municipality-by-month of birth panel 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 connecting municipality of residence and month of birth in SINASC and SIM following Rocha and Soares (2015).

[4]Different to SIAB and SISAB, which only include health care production and gestations covered by SUS.

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

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 in Brazil.[6] 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 in our main specifications. We restrict our analysis to health care workers (physicians, nurses and community health workers) to build a municipality-by-month panel of hirings and dismissals for the 2011-2018 period. Here we consider the municipality where the individuals work.

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 and splitting the sample between those closer or not the cap.

3.5 Sociodemographics

Most of the sociodemographic information at the municipal level comes from IEPS Data, a portal that provides harmonized 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.

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

[6] 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.

[7] Primary health care 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).

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. In our main specifications, on outcomes constructed from SIM and SINASC, the panel runs from 2007 up to 2018, covering three electoral cycles (2008, 2012, and 2016). It was not possible to include the 2020 elections since data for 2021 were not available at the time our analysis was completed. Additionally, elections in 2020 were atypical as they happened in November due to Covid-19 restrictions. The 2020 electoral cycle is therefore used only for auxiliary exercises on SISAB data. Analyses on hirings and dismissals cover only two electoral cycles (2012 and 2016). We excluded 2008 due to a change in the classification of occupation codes in that year. Table A.1 lists the period of analysis and the electoral cycles covered by each source of data.

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 G, 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).

In sum, we stack electoral cycles, each of them divided into blocks of four years and representing forty eight months. Panel A of Table 1 shows the total number of municipalities that remain per election, distinguishing between those that experienced turnover and those where the incumbent was reelected. The 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. Panels B to E 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. We see no evidence of statistically significant differences. Finally, Column 5 displays the data source of each variable.

Table (1) Descriptive Statistics and Test for Discontinuity at Baseline

	(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: Municipalities with Close Elections (N)					
2008	676	928			
2012	654	650			
2016	729	613			
2020	676	817			
Panel C: Municipal Characteristics					
Population	21257.53	19341.49	57.80	0.68	IBGE
MHDI	0.65	0.65	0.00	1.00	IEPS DATA
IDEB, 9th year	4.17	4.22	-0.01	0.88	IEPS DATA
IDEB, 5th 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 D: Health Outcomes					
Pregnancies with Prenatal consultations in Month, (%)	91.72	91.83	1.55	0.28	SIAB
Pregnant women undergoing tests for Syphilis and HIV (%)	28.22	30.15	0.99	0.67	SISAB
Pregnancies with adequate n° of Prenatal consultations (%)	27.44	26.98	1.49	0.49	SISAB
IMR	14.81	15.47	1.78	0.30	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 Consultations (7+), (%)	60.27	59.02	-0.21	0.89	SINASC
Prenatal Consultations (1-6), (%)	35.58	36.25	0.89	0.54	SINASC
Prenatal Consultations (0), (%)	1.80	1.72	0.19	0.45	SINASC
Panel E: 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 F: 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 = 0.14) 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

We estimate the effects of political turnover and government transitions on health outcomes by using an event-study in close elections approach. 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 non-observable confounders. A simple comparison of outcomes in municipalities where the incumbent won versus others where they 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. 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, i.e., only for the sample of municipalities around the discontinuity. We present the results without the restriction on close elections in the Appendix Section C.

The validity of our empirical strategy relies on two identifying assumptions. First, that potential outcomes are continuous around the threshold where the vote margin is equal to zero, therefore, making treatment and control municipalities similar. We perform a 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, that 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 periods distant or just 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. $Turnover_{mc}$ 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 = 3. 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.

Estimating the effects on health services enables us to identify a critical period of exposure to turnover –the identification of the exposure period is detailed in the next section. We then estimate a second model to assess whether exposure to turnover during the time in utero affects birth outcomes. More specifically, 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.

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. 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 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. Again, we add municipality fixed effects λ_m and time fixed effects γ_{ct}, while standard errors are clustered at the municipality level.

5 Results

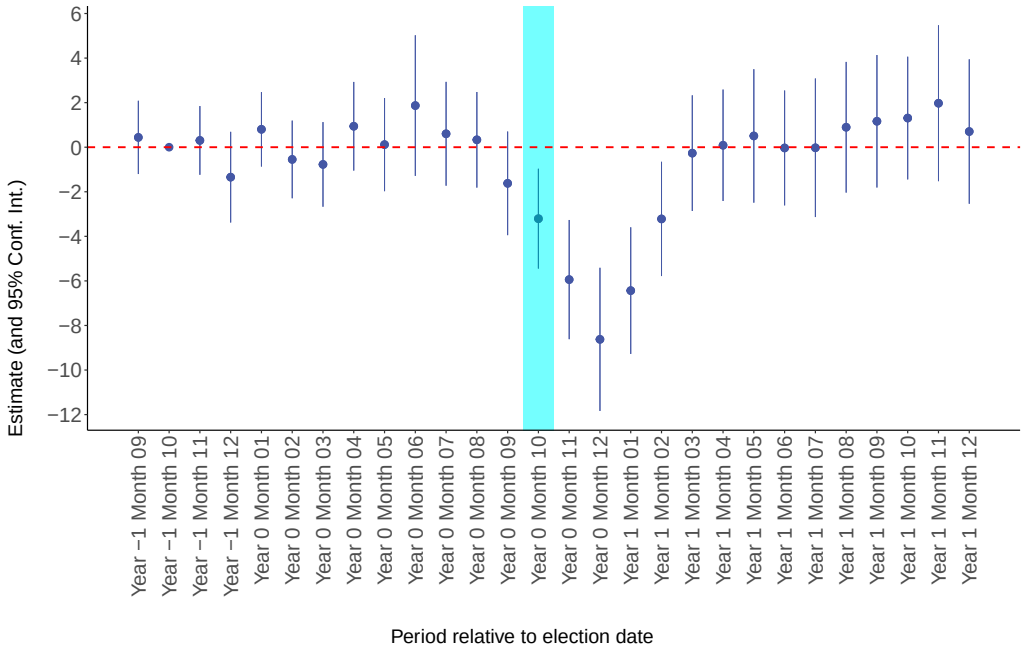
We first present short-term effects of political turnover on health care production for the 2008 and 2012 electoral periods, and separately for 2020. We then present results on birth outcomes and mortality for the 2008, 2012, and 2016 electoral periods. In all regression plots, the election year is indicated 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.

5.1 Political Turnover Effects on Health Care Services

Figure 1 shows effects on the number of pregnant women who had prenatal consultations in a given month. Right after elections happen, in October of the electoral year, we observe a reduction of prenatal consultations 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.2 shows the corresponding regression results. The difference reaches the

lowest in December, the last month of the defeated government (-8.6 consultations per month, which corresponds to 10.3% of the municipal mean number of monthly consultations). This effect is very localized and appears roughly symmetric around December, with November and January as months of accelerating decay and recovery, respectively.

Figure (1) Number of Prenatal Consultations per Month



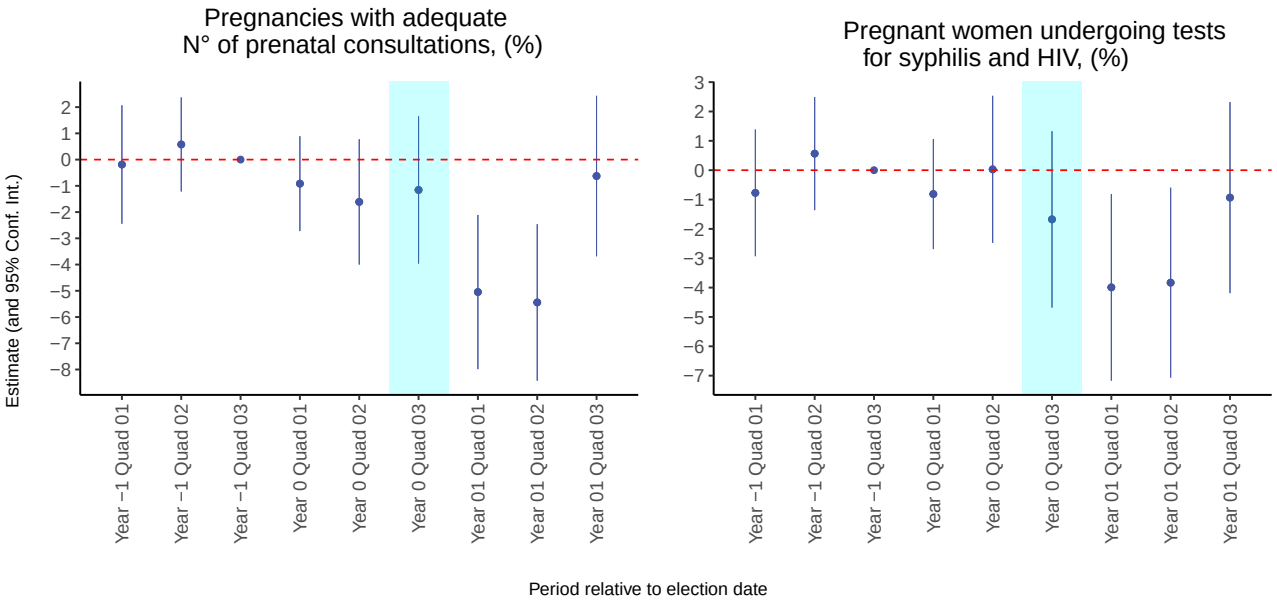
Note: This figure shows graphically results from equation 1, which is based on a municipality-by-month panel for the 2008 and 2012 electoral periods. The dependent variable is the number of prenatal consultations in the respective month. We examine results from September in the year preceding elections (Year -1) up to December of the year following elections (Year 1). The blue shaded area represents the time elections are held. We set a bandwidth equal to .14 to select the sample of municipalities. Regression results, additional descriptive statistics and specification details are reported in Table A.2.

Yet, it is possible that even if mothers are not having prenatal consultations 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 further characterizes turnover effects on health services. The first panel shows the share of pregnancies ending up with an adequate number of prenatal consultations (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). The results indicate that the total number of consultations is affected, and the transitory impact on service delivery translates into persistent effects on access to services at the end of pregnancies. Table A.3 shows the corresponding regression results. On average, turnover leads to a decrease of approximately 5 percentage points in the share of pregnancies that end up with an adequate number of prenatal consultations in the two quarters following elections, and of 4 percentage points in the share of pregnant women undergoing tests for Syphilis and HIV.

Figures B.1, and B.2 in Appendix B expand the plotted coefficients and show that there are not any similar effects in periods far from the elections and the transition period. Furthermore, Figures C.1, and C.2 in Appendix C present the previous analyses with the full sample of municipalities, without

restricting to close races. We observe a small anticipation effect in September, but results remain quantitatively similar. Finally, we repeat all exercises using the optimal bandwidth and different weights for each variable. Results appear in Figures G.1 and G.2 in Appendix G. We do not observe any substantial changes.

Figure (2) Indicators of Access to Prenatal Services Measured at the Time of Delivery, Quarterly Outcomes



Note: These figures show graphically results from equation 1, which is based on a municipality-by-quarter panel for the 2020 electoral period. The dependent variables are the share of pregnancies that ended up with an adequate number of prenatal consultations, and the share of pregnant women that underwent tests for syphilis and HIV as measured at the time of delivery. We examine results from the first quarter in 2019 (Year -1) up to the last quarter 2021 (Year 1). In 2020 elections were exceptionally held in November (but within the last quarter of 2020). The blue shaded area represents the time elections are held. We set a bandwidth equal to .14 to select the sample of municipalities. Regression results, additional descriptive statistics and specification details are reported in Table A.3.

5.2 Mapping the Critical Period

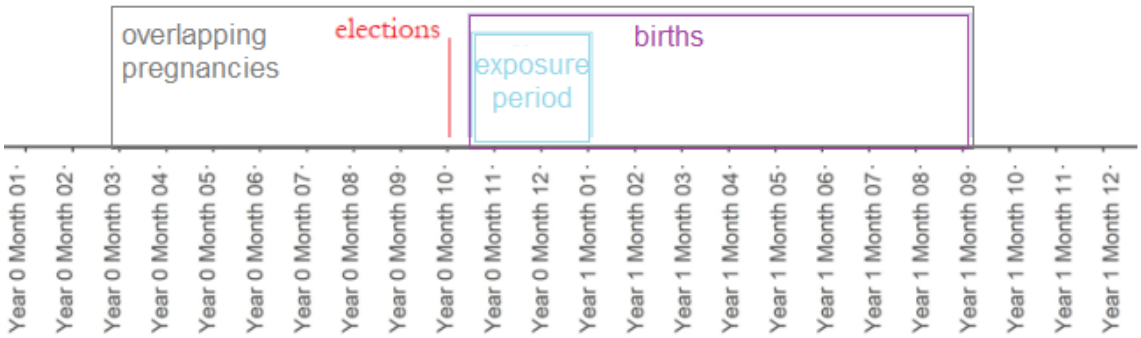
In this paper we conjecture that the short-lived but well-marked effects of turnover on service provision, starting right after elections, may have far-reaching detrimental effects should they overlap with critical gestational stages. To test for this, we first define a critical period, when exposure to turnover effects can be potentially harmful to birth outcomes and child development. More specifically, 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, 2019). In particular, this is the time when critical neural brain development occurs (Black et al., 2019). An adequate number of prenatal consultations, on the other hand, can be protective of both infant and maternal health.[8]

[8]For instance, 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, Schneider, Bubach, Silveira, Bertoldi, Duarte, Menezes, Domingues, and Bassani (2019) find strong associations between mothers having had less than six prenatal care consultations and infant mortality rates.

We define the exposure period and the pool of potentially affected pregnancies by combining the results from Figures 1 and 2. From Figure 1, we observe that the negative effect on health care production lasts from October to February of the following year. However, it reaches its peak in December, with a coefficient two times as large as the ones reported in the more distant months of October and February. Also, for those women ending their pregnancy in October or starting in February, most of their needs might have been covered or will still be covered in the following months, respectively. Alternatively, those in need of care in December, in particular those halfway within their gestational periods, may face constrained access to services not only in December but also in the vicinity months of November and January. Figure 2 is consistent with these women being the most restricted in services during their pregnancy. The turn of December to January also marks the inauguration of the new government, when turnover effects on government organizations and functioning are likely enhanced. We therefore define as the main period of exposure the months of November throughout January. We also test multiple periods of exposure in auxiliary exercises in order to empirically assess how localized the effects are, ranging from (i) only December; (ii) November and December; (iii) December and January; and (iv) October to February.

To summarize, assuming a nine-month gestational period for all pregnancies, children born from mothers ending their pregnancies by the time of elections as well as those born from pregnancies starting in March of the following year were not exposed in utero. Those pregnancies that overlapped the period from October until February may have been affected, in particular if exposed in December and its vicinity months. Figure 3 illustrates that timing.

Figure (3) Overlapping Pregnancies



Note: This figure illustrates the timing of pregnancies, elections and government transitions.

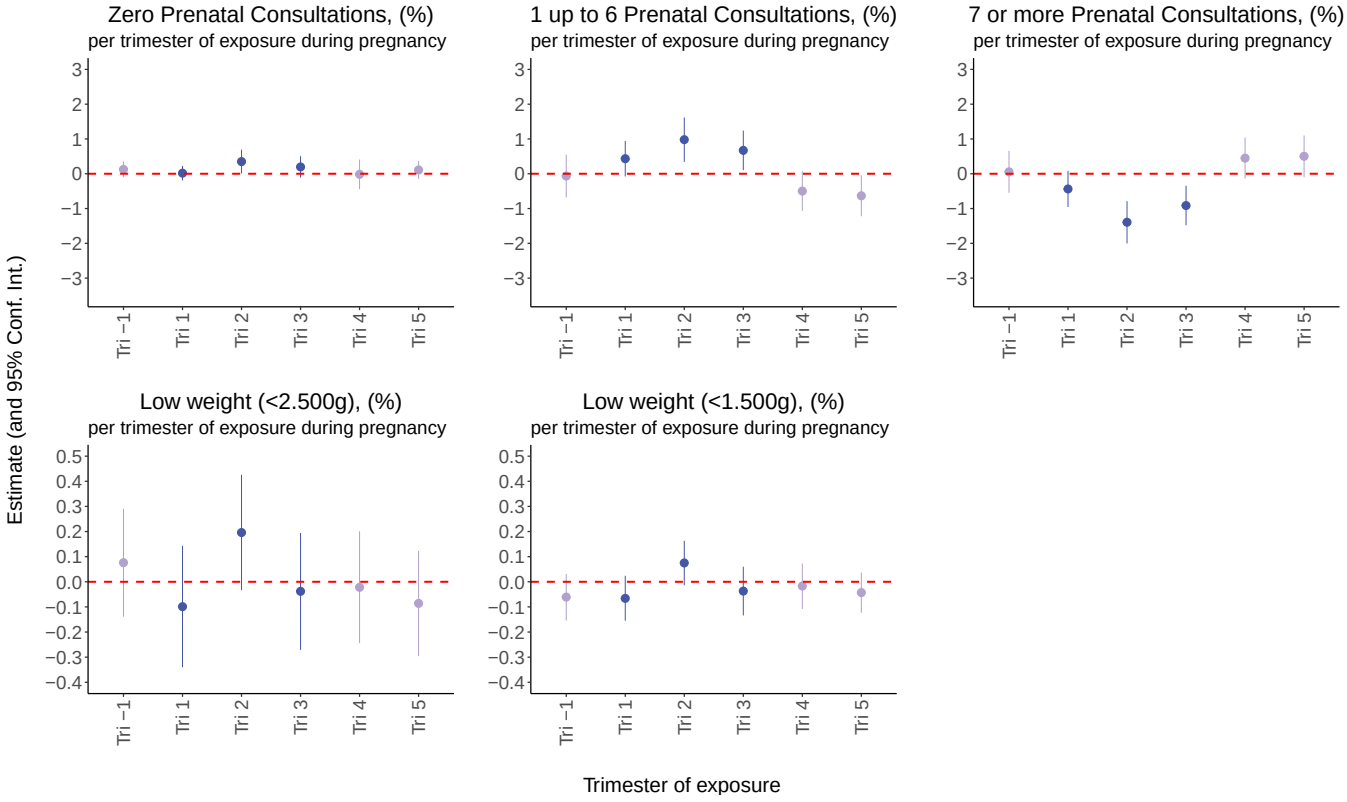
5.3 Effects on Birth Outcomes and Mortality

We rely on equation (2) to assess the effects on birth outcomes and mortality. In our benchmark specifications, exposure by trimester is defined by dummy variables. The upper plots of Figure 4 show, now using data from SINASC, that exposure to turnover and government transitions is associated with fewer number of prenatal consultations as measured at the time of delivery. In particular, pregnancies exposed during the second and third trimesters had, on average, fewer prenatal consultations 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 consultations 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. (2019) find that granting access to the Family Health Program, which induced the universalization of primary health care in Brazil, led to an increase of 1 percentage point (2.2% of the mean) in the number of prenatal consultations upon the introduction of the services.

Moving to outcomes at birth, the second row in Figure 4 suggests increases in the share of children with low birth weight ($<$ 2.500 grams) and very low birth weight ($<$1.500 grams) 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. Table A.4 shows these results. We see an increase of 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 (7% of the mean). Putting these results into perspective, Schwandt (2019) finds that infants exposed to maternal influenza infections in utero were more likely to have low birth weight in Denmark. 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.

Figure (4) Birth Outcomes

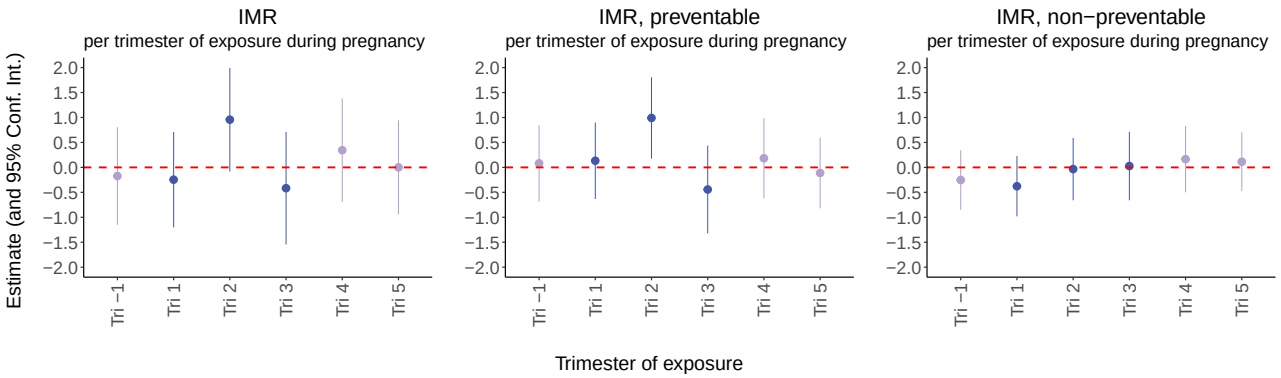


Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations on the plots of the first row. In the second row they represent the share of newborns with low weight at birth and very low weight. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. Regression results, descriptive statistics and additional specification details appear on Table A.4.

The first plot in Figure 5 shows higher infant mortality rates for children exposed during the second trimester of pregnancy. We document an increase of 0.96 in infant mortality rates (representing 6.7% of the mean). Effects are greater for mortality from preventable causes (amenable to health care), with a point estimate for the second trimester of 1.0, which corresponds to 11.7% of the mean. Table A.5 documents these results. These effects are non-trivial. Bhalotra et al. (2019), for instance, find a reduction of 9% in infant mortality rates in the second year after the introduction of the Family Health Program in Brazil. For the Mexican program Seguro Popular, Conti and Ginja (2023) detect significant reductions in mortality only when considering infant mortality rates in poor municipalities. The political transition effects documented here, therefore, although short-lived in time, stand out by paralleling in similar effect sizes those from large-scale national programs.[9]

[9]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 selected (survivor bias). In this case, bias from fetal selection would most likely contribute to underestimation of effects.

Figure (5) Infant Mortality



Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are Infant Mortality Rates (IMR), IMR for preventable causes, and IMR for non-preventable causes. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. Regression results, descriptive statistics and additional specification details appear on Table A.5.

While the magnitude of estimates is relevant, how localized or specific in time transition effects are? In order to test for this, in Appendix D we report analogous specifications in which we vary the period of exposure. Overall, restricting the period for December or December-January allows slightly greater and more precise point estimates in comparison to November-December. This suggests that the period around the turn of the year, by the very end of the defeated government and the beginning of the new administration is particularly critical. When we increase the period to include October until February, although we still observe suggestive effects on access to services, impacts on birth outcomes and mortality become close to zero. The results combined indicate that turnover effects are non-trivial but localized in time.

Finally, we perform additional robustness exercises. In Appendix B, Figures B.3 and B.4, we create a falsified period of exposure one year after elections happen. We do not observe any significant effects in periods far from elections. In Appendix C, Figures C.3 and C.4 repeat the analysis without the selection on close races, using instead the full sample of municipalities. Results remain qualitatively similar. In Appendix E, we change the definition of exposure to consider the intensity of exposure per month (i.e. the number of months exposed per trimester of gestation) rather than dummies for exposure per trimester. Results in Figures E.1 and E.2 remain qualitatively similar to our benchmark specification. In Appendix G, 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 G.3 and G.4. 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, 2023a) 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 of civil servants in tenured 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 health care 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. When working with dismissals, we first restrict to dismissals initiated by the employer, with or without fair cause. 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 periods.[10] The remaining details of the specification follow those previously described in equation 1.

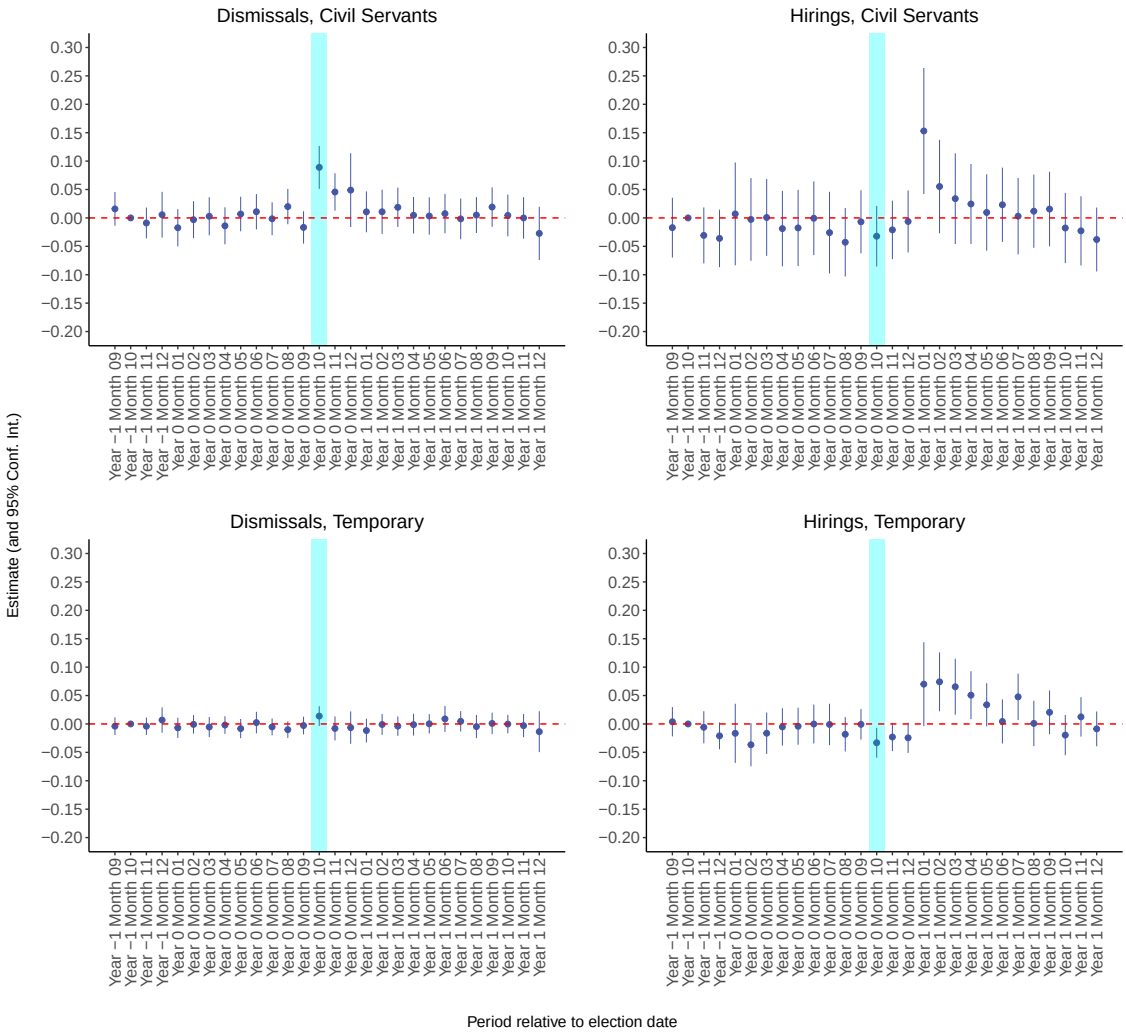
In Figure 6 we observe a substantial increase in workers' dismissals under civil servant contracts right after elections occur, as well as a smaller but still positive effect on dismissals of temporary workers. Point estimates reported on Table A.6 show, for instance, that political turnover is associated with an increase of 9% in the dismissal of health care personnel under civil servant contracts in the month of elections.[11] These effects are reverted throughout the first months of the new government. In particular, we see a peak in hirings in January, which helps explain the quick recovery in access to services shown in Figure 1. These results are consistent with Akhmedov and Zhuravskaya (2004), who document opportunistic political cycles in budget spending in Russia. In particular, they show that spending in education, culture, and healthcare decreases 18% percent in the two months following elections.[12] To better assess if the effect in dismissals reflects a broader impact over contract terminations, we add in Appendix F, Figure F.2 a similar analysis for civil servants but considering only contract terminations initiated by the employee (left panel) and cases of retirements (right panel). In both cases the estimated coefficients are not statistically different than zero.

[10]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.

[11]As mentioned before, elections occur in the first Sunday of October. Hiring or dismissing civil servants who generally hold stable contracts should be relatively more difficult in electoral years. In Appendix F, Figure F.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 6 mostly come from workers who, albeit civil servants, have less stability, making hirings and dismissals relatively more likely.

[12]These results are also consistent with other studies that assessed bureaucratic turnover in Brazil (e.g. Toral, 2023a; Ramos, 2022).

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



Note: These figures show graphically results from equation 1, using a municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of dismissals and hirings of healthcare workers, separated by type of contract (civil servants and temporaries). The blue shaded area represents the time elections are held. We set a bandwidth equal to .14 to select the sample of municipalities. Regression results, descriptive statistics and additional specification details appear on Table A.6.

In Figure F.3, we focus on civil servant contracts and separate the analysis in three categories of health care 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 month. 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. We investigate this conjecture in the next section.

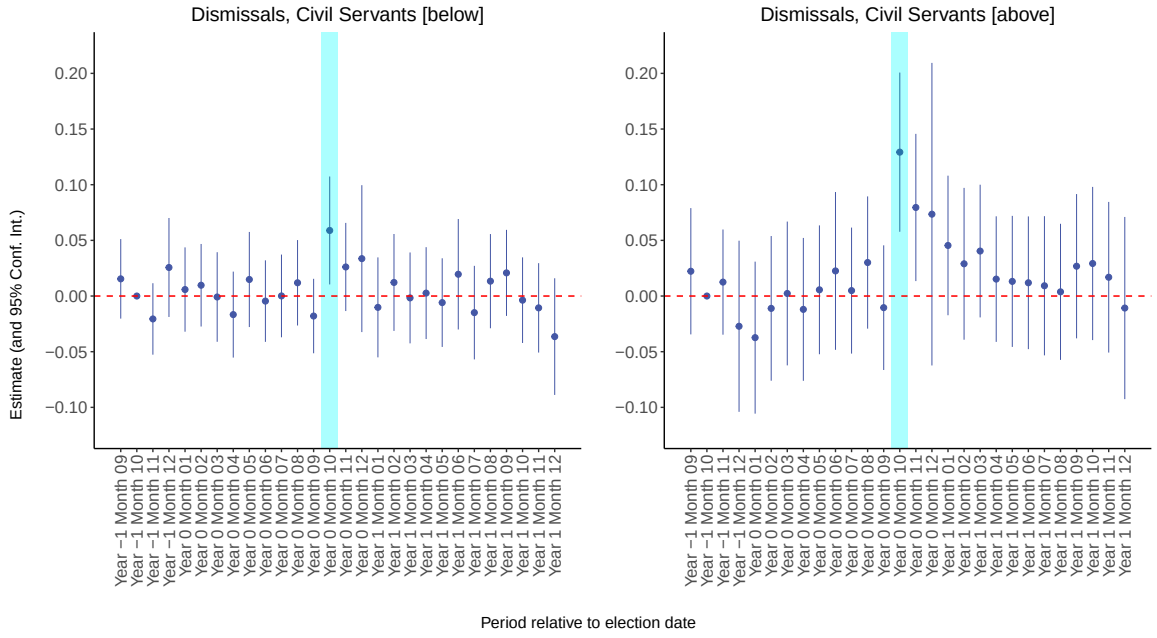
6.2 Fiscal Incentives

In 2000, the Fiscal Responsibility Law (LRF) defined that municipalities could not 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. 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 of analysis.

Figure 7 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%). Taken together, these results suggest that municipalities where the incumbents dismissed more health care workers after the elections were those closer to reaching or above the LRF cap. This is consistent with incumbents reacting to incentives and dismissing workers to avoid surpassing the cap, and therefore to comply with the Fiscal Responsibility Law.

Figure (7) Human Resources Turnover: Dismissals According to Compliance with the Fiscal Responsibility Law



Note: These figures show graphically results from equation 1, using a municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of dismissals of healthcare workers (civil servants), for the sub-samples of municipalities with the share of revenues spent with personnel below versus above the median. The blue shaded area represents the time elections are held. We set a bandwidth equal to .14 to select the sample of municipalities. Regression results, descriptive statistics and additional specification details appear on Table A.7.

7 Final Comments

In this paper we assessed whether political turnover and government transitions, which are transitory by nature, can have more consequential impacts on development outcomes. We conjectured that this might be the case should transitions coincide with critical periods for investments in inputs conducive to human and economic development. To test for this, we empirically examined impacts on health services and infant health outcomes in Brazilian municipalities by using municipality-by-month level data that aggregates different election cycles as well as close elections for identification. We found significant reductions in health services 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. We then found that the deterioration in health services translates into worse birth outcomes and higher infant mortality rates for those exposed to transitions during the time in utero. Impacts are non-trivial. Although transitory and short-lived, transition processes impacts parallel in similar magnitude to those from large-scale primary health care programs implemented in different countries.

It is possible that transition processes have even more pervasive impacts should they coincide with critical periods for investments in other relevant determinants of development, such as incentives for accumulation of physical capital as well as for innovation. Yet, incumbent's incentives can be subject to change through regulation aiming at smoothing bureaucratic turnover, protecting service delivery, and keeping politicians' actions in a way they remain conducive to human and economic development. In our context, we observed that the timing of the decline and recovery in service production and in birth outcomes is associated with the timing of personnel turnover. We also observed that this movement responds to fiscal incentives.

In that case, restricting personnel turnover and increasing the monitoring of incumbent's decisions during the transition process can mitigate detrimental impacts on service delivery and health outcomes. On the one hand, fiscal rules can be designed to limit the incumbents's room for sharp fiscal maneuvering around elections. On the other hand, municipality legislative chambers and officially appointed transition committees could play important roles in the monitoring of the executive's maneuverings. By regulating and monitoring government transitions, societies may benefit the most from political turnovers and, more generally, from democratic elections.

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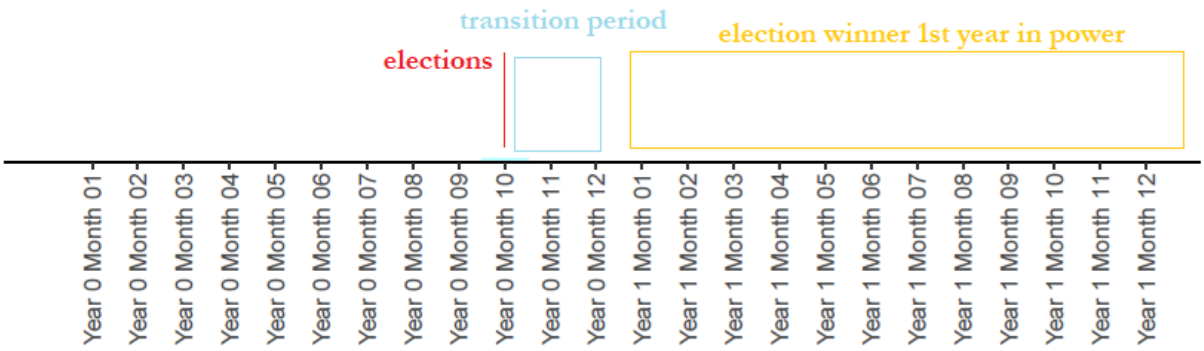
A Additional Tables and Figures

Figure (A.1) Timeline of Elections



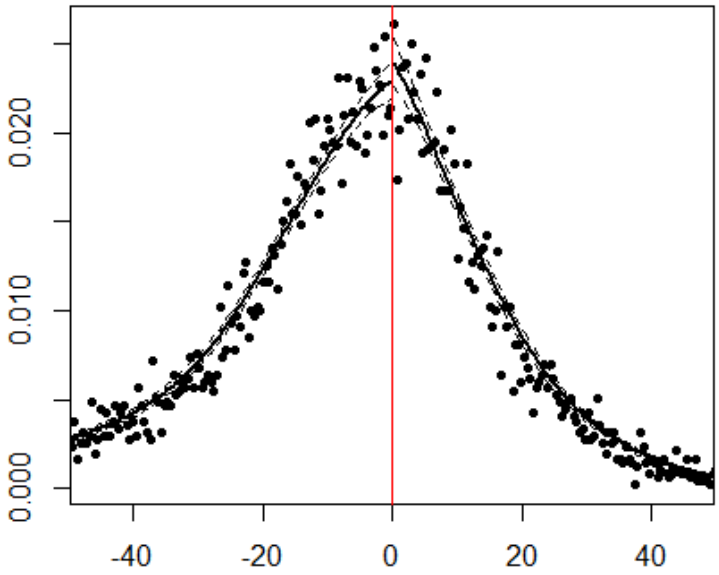
Note: This figure illustrates the timeline of elections in Brazil. We call the electoral year by Year 0. Elections happen in October (month 10) of electoral years, except for 2020, when elections were held in November. The new government starts on the first day of January.

Table (A.1) Data Sources, Period of Analysis and Electoral Cycles

Outcome	**Database**	**Period**	**Electoral Cycles**
Outcomes at Birth	SINASC	2007-2018	2008 - 2012 - 2016
Infant Mortality Rate	SIM/SINASC	2007-2018	2008 - 2012 - 2016
Primary Health Care	SISAB	2019-2022	2020
Primary Health Care	SIAB	2007-2014	2008 - 2012
Human Resources	RAIS	2011-2018	2012 - 2016

Notes: This table details the data used from each source and for each electoral cycle.

Figure (A.2) McCrary Density Test



Notes: This figure shows the McCrary test for manipulation of the running variable in a RDD, the margin of victory (MV) of the challenger candidate for the 2008-2020 electoral periods. 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.2) Monthly Number of Prenatal Consultations, SIAB Data

	Number of Prenatal Consultations
Turnover	-3.344
	(2.990)
MV	1.047**
	(0.5315)
Turnover × MV	-1.467**
	(0.6406)
Turnover ×D(Year 0 Month 1)	0.8029
	(0.8513)
Turnover ×D(Year 0 Month 2)	-0.5482
	(0.8887)
Turnover ×D(Year 0 Month 3)	-0.7721
	(0.9685)
Turnover ×D(Year 0 Month 4)	0.9404
	(1.014)
Turnover ×D(Year 0 Month 5)	0.1174
	(1.066)
Turnover ×D(Year 0 Month 6)	1.869
	(1.609)
Turnover ×D(Year 0 Month 7)	0.6028
	(1.188)
Turnover ×D(Year 0 Month 8)	0.3329
	(1.094)
Turnover ×D(Year 0 Month 9)	-1.622
	(1.186)
Turnover ×D(Year 0 Month 10)	-3.208***
	(1.141)
Turnover ×D(Year 0 Month 11)	-5.941***
	(1.364)
Turnover ×D(Year 0 Month 12)	-8.623***
	(1.638)
Turnover ×D(Year 1 Month 1)	-6.436***
	(1.449)
Turnover ×D(Year 1 Month 2)	-3.218**
	(1.307)
Turnover ×D(Year 1 Month 3)	-0.2669
	(1.324)
Turnover ×D(Year 1 Month 4)	0.0873
	(1.275)
Turnover ×D(Year 1 Month 5)	0.5064
	(1.527)
Turnover ×D(Year 1 Month 6)	-0.0329
	(1.314)
Time Fixed Effects	Yes
Municipality Fixed Effects	Yes
Mean Dep. Var	83.49
Using Bandwidth	.14
Municipalities	2,607
Observations	139,584
R^2	0.94151

Notes: Standard errors clustered at the municipality level reported in parentheses. Significance: ***p< 0.01, **p< 0.05, *p< 0.1. The dep. var. is the monthly number of prenatal consultations. Mean dep. var. is the average dependent variable one year before elections for the treated group. The regression follows equation (1) and includes time and municipality fixed effects. We report only a subset of the estimated coefficients on Turnover*D(Year,Month) interactions, ranging from January of the electoral year (Year 0) to June of the following year (Year 1). The regression also includes all interactions but the omitted category as described in equation (1). This specification uses the 2008 and 2012 electoral cycles.

Table (A.3) Access to Prenatal Services Measured at the Time of Delivery, Quarterly Data from SISAB

	Pregnancies with adequate prenatal consultations (%)	Pregnant women undergoing tests for Syphilis and HIV (%)
	(1)	(2)
Turnover ×D(2019Q01)	-0.1878	-0.7724
	(1.150)	(1.101)
Turnover ×D(2019Q02)	0.5778	0.5643
	(0.9139)	(0.9818)
Turnover ×D(2020Q01)	-0.9155	-0.8131
	(0.9217)	(0.9557)
Turnover ×D(2020Q02)	-1.613	0.0284
	(1.217)	(1.277)
Turnover ×D(2020Q03)	-1.157	-1.675
	(1.430)	(1.531)
Turnover ×D(2021Q01)	-5.047***	-3.992**
	(1.498)	(1.620)
Turnover ×D(2021Q02)	-5.442***	-3.832**
	(1.520)	(1.649)
Turnover ×D(2021Q03)	-0.6264	-0.9331
	(1.559)	(1.659)
Turnover ×D(2022Q01)	0.8147	-2.956*
	(1.817)	(1.678)
Turnover ×D(2022Q02)	0.5645	-2.368
	(1.791)	(1.727)
Turnover ×D(2022Q03)	-0.2943	-0.8941
	(1.716)	(1.718)
Time Fixed Effects	Yes	Yes
Municipality Fixed Effects	Yes	Yes
Mean Dep. Var	29.27	31.1
Using Bandwidth	.14	.14
Municipalities	1,493	1,493
Observations	17,916	17,916
R^2	0.64478	0.56466

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 number of prenatal consultations and the share of pregnant women that underwent tests for syphilis and HIV as measured at the time of delivery. Mean dep. var. is the average dependent variable one year before elections for the treated group. All regressions follow equation 1 and include time and municipality fixed effects. We report only a subset of the estimated coefficients ranging from the first quarter of the electoral year (Year 0) to the third quarter of the following year (Year 1). The regressions also include all interactions but the omitted category as described in equation (1). This specification uses only the 2020 electoral cycle.

Table (A.4) Prenatal Consultations Measured at the Time of Delivery, Monthly Data from SINASC

	0 Prenatal consultations	1 up to 6 Prenatal consultations	7 or more Prenatal consultations
	(%)	(%)	(%)
	(1)	(2)	(3)
Turnover	0.6678*	0.9150	-1.368
	(0.3471)	(2.011)	(2.010)
MV	-0.1154**	0.0607	0.0201
	(0.0476)	(0.2199)	(0.2211)
Turnover × D(Trimester -1)	0.1260	-0.0652	0.0558
	(0.1148)	(0.3155)	(0.3073)
Turnover × D(Trimester 1)	0.0173	0.4326*	-0.4367*
	(0.1062)	(0.2601)	(0.2649)
Turnover × D(Trimester 2)	0.3501**	0.9797***	-1.394***
	(0.1739)	(0.3255)	(0.3098)
Turnover × D(Trimester 3)	0.1965	0.6725**	-0.9140***
	(0.1567)	(0.2903)	(0.2890)
Turnover × D(Trimester 4)	-0.0159	-0.4985*	0.4490
	(0.2164)	(0.2897)	(0.3014)
Turnover × D(Trimester 5)	0.1108	-0.6330**	0.5021
	(0.1320)	(0.3000)	(0.3057)
Turnover X MV	0.1585**	-0.1672	0.0711
	(0.0658)	(0.2747)	(0.2720)
Time FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Weighted (avg number of newborns)	Yes	Yes	Yes
Mean Dep. Var	2.0	34.17	63.16
Using Bandwidth	.14	.14	.14
Municipalities	3,296	3,296	3,296
Observations	200,085	200,085	200,085
R^2	0.39200	0.71927	0.74741

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 consultations, 1 up to 6 prenatal consultations, and 7 or more prenatal consultations as measured at the time of delivery. Mean dep. var. is the average dependent variable one year before elections for the treated group. All regressions follow equation (2) and include time and municipality fixed effects. Trimesters are dummy variables that refer to the trimester of the pregnancy when the child was exposed in-utero to political turnover. The benchmark period of exposure is defined at November-January. Trimesters 1, 2 and 3 refer to the first, second and third trimesters of pregnancy, respectively. This specification uses 3 electoral cycles (2008, 2012, 2016).

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

	IMR	IMR (preventable)	IMR (non-preventable)	Low Birth Weight (< 2500g, %)	Very Low Birth Weight (< 1500g, %)
	(1)	(2)	(3)	(4)	(5)
Turnover X MV	-0.0104	0.0632	-0.0736	0.0252	0.0069
	(0.1371)	(0.1016)	(0.0647)	(0.0220)	(0.0082)
MV	-0.0062	-0.0895	0.0833*	-0.0160	-0.0021
	(0.1052)	(0.0791)	(0.0494)	(0.0186)	(0.0057)
Turnover × D(Trimester -1)	-0.1727	0.0811	-0.2538	0.0763	-0.0613
	(0.4983)	(0.3906)	(0.3043)	(0.1097)	(0.0472)
Turnover × D(Trimester 1)	-0.2464	0.1313	-0.3777	-0.0986	-0.0655
	(0.4878)	(0.3909)	(0.3077)	(0.1230)	(0.0459)
Turnover × D(Trimester 2)	0.9565*	0.9913**	-0.0348	0.1961*	0.0747*
	(0.5295)	(0.4159)	(0.3186)	(0.1170)	(0.0452)
Turnover × D(Trimester 3)	-0.4170	-0.4445	0.0275	-0.0384	-0.0369
	(0.5754)	(0.4485)	(0.3505)	(0.1186)	(0.0494)
Turnover × D(Trimester 4)	0.3452	0.1824	0.1628	-0.0217	-0.0170
	(0.5290)	(0.4104)	(0.3397)	(0.1133)	(0.0462)
Turnover × D(Trimester 5)	-0.0004	-0.1123	0.1119	-0.0863	-0.0434
	(0.4814)	(0.3617)	(0.3016)	(0.1067)	(0.0408)
Turnover X MV	-0.0104	0.0632	-0.0736	0.0252	0.0069
	(0.1371)	(0.1016)	(0.0647)	(0.0220)	(0.0082)
Time FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Weighted (avg number of newborns)	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	14.32	8.44	5.87	7.47	1.064
Using Bandwidth	14	14	14	14	14
Municipalities	3,296	3,296	3,296	3,296	3,296
Observations	200,160	200,160	200,160	200,107	200,107
R^2	0.03560	0.02808	0.03032	0.06190	0.03281

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 1 year old), infant mortality rates by cause of death, and the shares of newborns with low and very low birth weight. Mean dep. var. is the average dependent variable one year before elections for the treated group. All regressions include time and municipality fixed effects and are weighted by municipality average number of births per year. Trimesters are dummy variables that refer to the trimester of the pregnancy when the child was exposed in-utero to political turnover. The benchmark period of exposure is defined at November-January. Trimesters 1, 2 and 3 refer to the first, second and third trimesters of pregnancy, respectively. This specification uses 3 electoral cycles (2008, 2012, 2016).

Table (A.6) Human Resources Turnover: Hirings and Dismissals by Type of Contract

	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
	(0.0027)	(0.0081)	(0.0023)	(0.0069)
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)
Time FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Mean Dep. Var	0.095	0.227	0.016	0.073
Using Bandwidth	.14	.14	.14	14.
Municipalities	2,376	2,376	2,376	2,376
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 inverse hyperbolic sine transformations of the number of dismissals and hirings of health workers, by type of contract (civil servants, and temporaries). Mean dep. var. is the (non transformed) average dependent variable one year before elections for the treated group. All regressions follow equation (1) and include time and municipality fixed effects. We 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). The regressions also include all Turnover*D(Year,Month) interactions but the omitted category, as described in equation (1). These specifications use the 2012 and the 2016electoral cycles.

Table (A.7) Human Resources Turnover According to Compliance with the Fiscal Responsibility Law

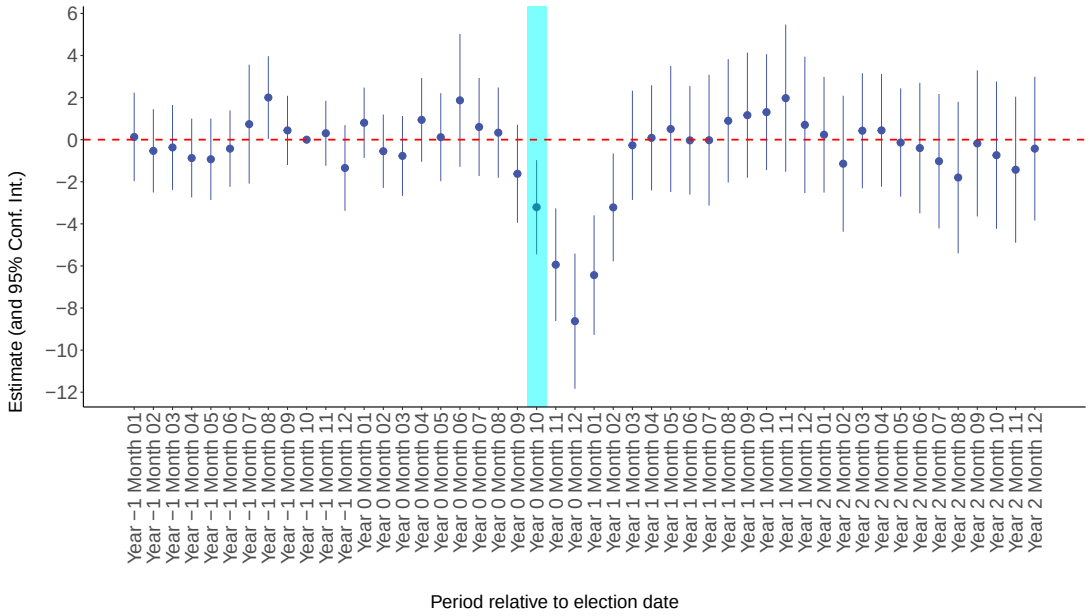
	Civil Servants, Dismissals	
	Below Md. LRF (1)	Above Md. LRF (2)
Turnover	-0.0085	-0.1199*
	(0.0188)	(0.0690)
MV	0.0018	0.0024
	(0.0014)	(0.0059)
Turnover X MV	-0.0020	0.0093
	(0.0020)	(0.0078)
Turnover ×D(Year 0 Month 1)	0.0046	-0.0378
	(0.0193)	(0.0348)
Turnover ×D(Year 0 Month 2)	0.0085	-0.0136
	(0.0189)	(0.0332)
Turnover ×D(Year 0 Month 3)	-0.0021	0.0017
	(0.0205)	(0.0329)
Turnover ×D(Year 0 Month 4)	-0.0158	-0.0092
	(0.0197)	(0.0328)
Turnover ×D(Year 0 Month 5)	0.0136	0.0051
	(0.0218)	(0.0295)
Turnover ×D(Year 0 Month 6)	-0.0057	0.0236
	(0.0187)	(0.0361)
Turnover ×D(Year 0 Month 7)	0.0009	0.0024
	(0.0190)	(0.0289)
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)
Turnover ×D(Year 1 Month 2)	0.0122	0.0251
	(0.0222)	(0.0347)
Turnover ×D(Year 1 Month 3)	-0.0017	0.0381
	(0.0208)	(0.0303)
Turnover ×D(Year 1 Month 4)	0.0026	0.0163
	(0.0210)	(0.0287)
Turnover ×D(Year 1 Month 5)	-0.0059	0.0143
	(0.0203)	(0.0299)
Turnover ×D(Year 1 Month 6)	0.0195	0.0130
	(0.0253)	(0.0303)
Time FE	Yes	Yes
Municipality FE	Yes	Yes
Mean Dep. Var	0.026	0.149
Bandwidth	.14	.14
Municipalities	1,681	1,706
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. Columns 1 and 2 refer to sub-samples of municipalities below and above the median share of municipal spending with personnel (48%), respectively. The remaining details on the specification and electoral cycles used are analogous to those described in Table A.6.

B Extended Analysis on Pre-Trends

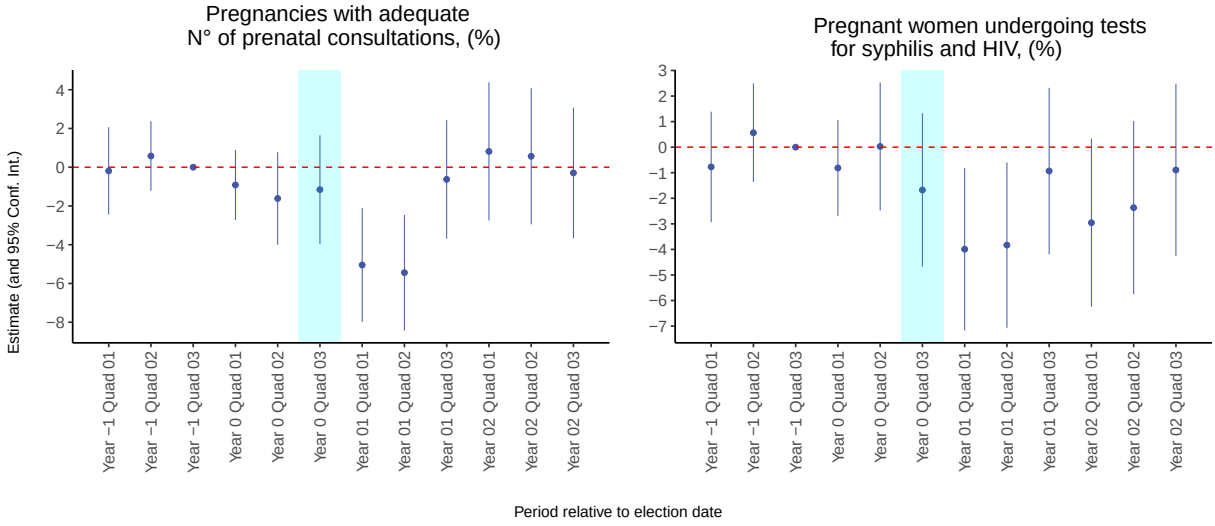
In this appendix section we provide additional exercises on the timing of the effects. We test whether effects appear in periods far from the elections and the transition period. For birth and mortality outcomes, we do this by creating falsified elections in Year 2, and thus repeating our main analysis analogously. For the remaining variables we merely expand the plotted coefficients to cover a longer period.

Figure (B.1) Number of Prenatal Consultations per Month, Extended Plot



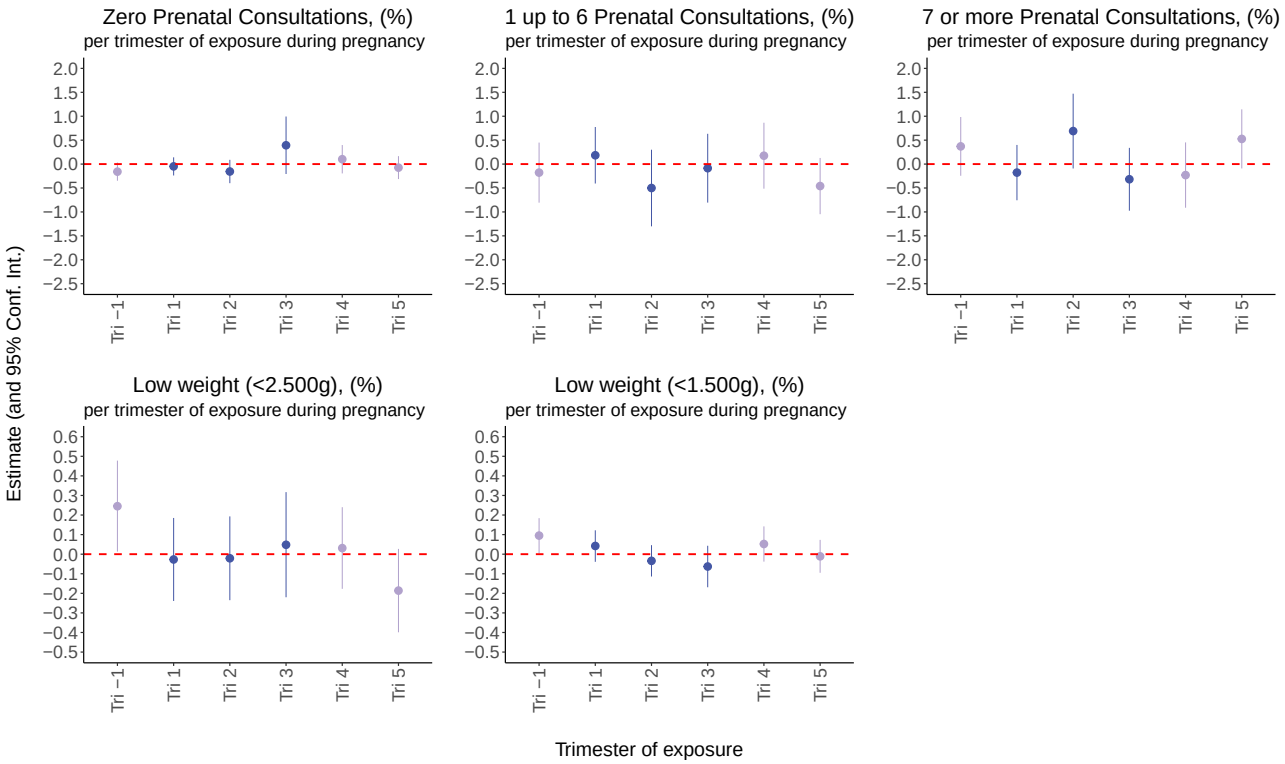
Note: This figure shows graphically results from equation 1, which is based on a municipality by month panel for the 2008 and 2012 electoral periods. The dependent variable is the number of pregnancies with prenatal consultations in the respective month. We set a bandwidth equal to .14 to select the sample of municipalities. We show the estimated coefficients on Turnover*D(Year,Month) interactions, ranging from January of the year preceding elections (Year -1) up to December of the second year following elections (Year 2). The regression includes all interactions but the omitted category as described in equation (1). For this exercise we have 139,584 observations in a sample of 2,607 municipalities.

Figure (B.2) Indicators of Access to Prenatal Services Measured at the Time of Delivery, Quarterly Outcomes, Extended Plots



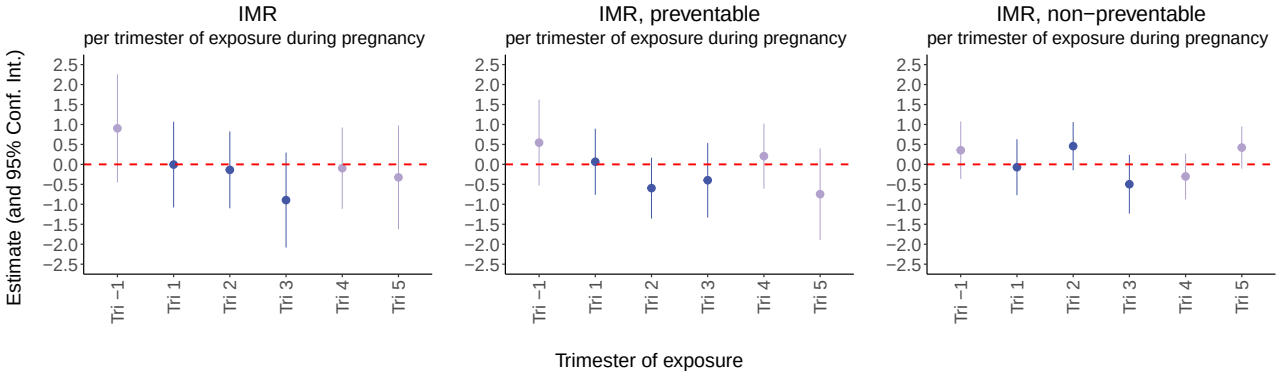
Note: These figures show graphically results from equation 1, which is based on a municipality by quarter panel for the 2020 electoral cycle. The dependent variables are the share of pregnancies that ended up with an adequate number of prenatal consultations, and the share of pregnant women that underwent tests for syphilis and HIV as measured at the time of delivery. We set a bandwidth equal to .14 to select the sample of municipalities. All regressions follow equation 1 and include time and municipality fixed effects. We show the estimated coefficients ranging from the first quarter in the year preceding elections (Year -1) to the third quarter of the second year following elections (Year 2). The regressions also include all interactions but the omitted category as described in equation (1). For this exercise we have 17,916 observations in a sample of 1,493 municipalities.

Figure (B.3) Birth Outcomes, Falsified Elections in Year 2



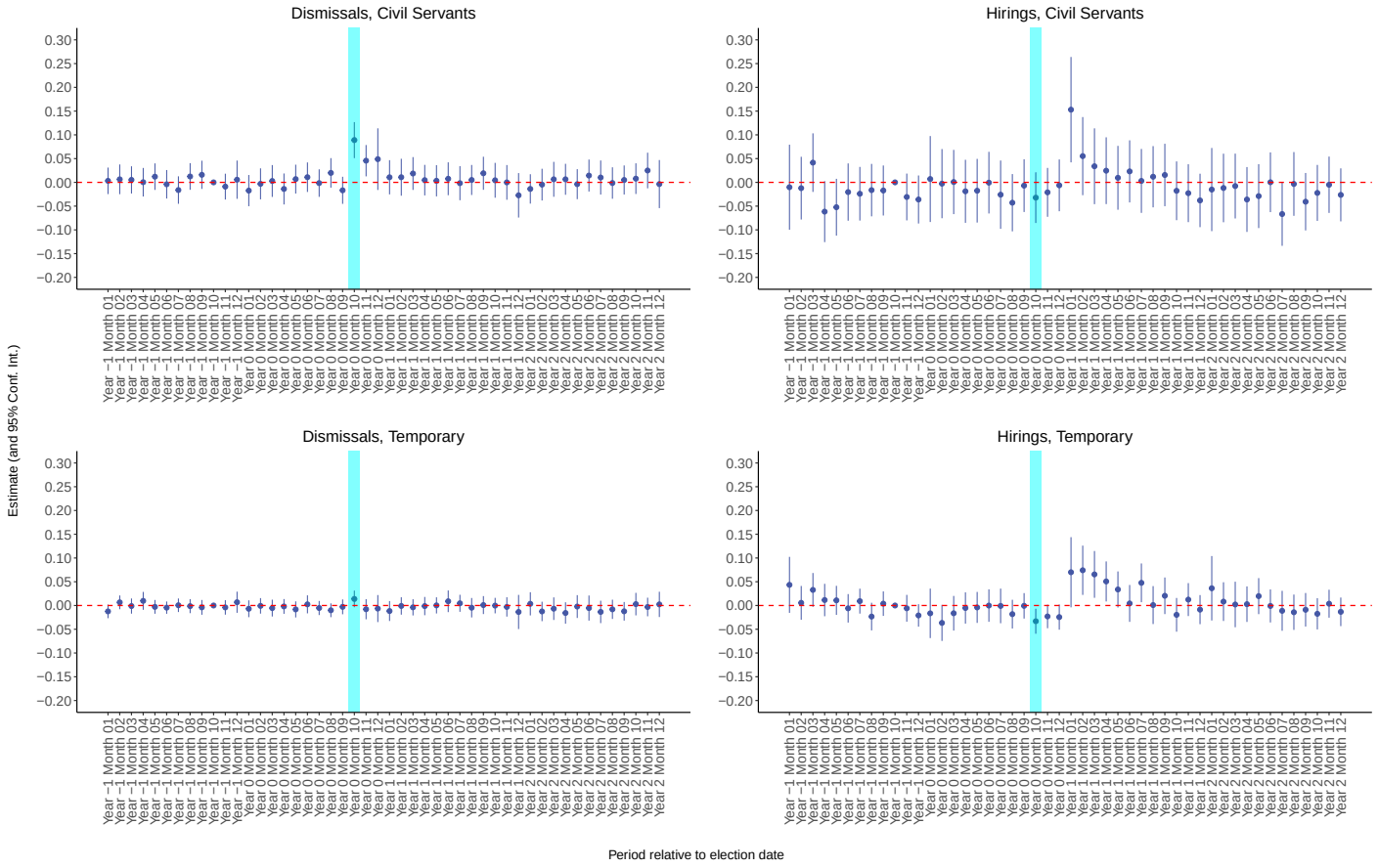
Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2007-2018 period. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations in the plots of the first row. In the second row they represent the share of newborns with low weight at birth and very low weight. To provide evidence that the effects do not appear in periods far from the elections we create falsified elections in the second year of the new government (Year 2) and repeat our benchmark exercise. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,085 observations for prenatal variables and 200,107 for low weight variables in a sample of 3,296 municipalities.

Figure (B.4) Infant Mortality, Falsified Elections in Year 2



Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2007-2018 period. The dependent variables are Infant Mortality Rates (IMR), IMR for preventable causes, and IMR for non-preventable causes of death. To provide evidence that the effects do not appear in periods far from the elections we create falsified elections in the second year of the new government (Year 2) and repeat our benchmark exercise. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,160 observations in a sample of 3,296 municipalities.

Figure (B.5) Human Resources Turnover: Hirings and Dismissals by Type of Contract, Extended Plot

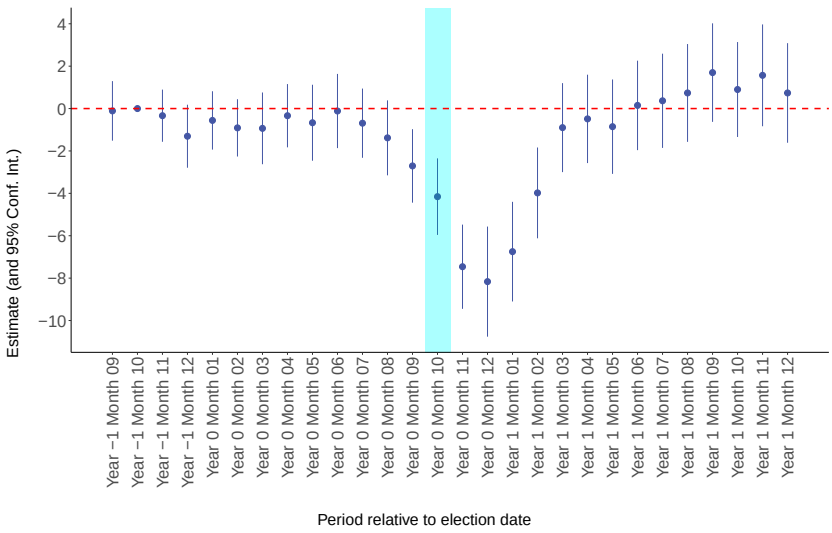


Note: These figures show graphically results from equation 1, which is based on a municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of dismissals and hirings of healthcare workers, separated by type of contract (civil servants and temporaries). We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 113,292 observations in a sample of 2,376 municipalities.

C Full Sample

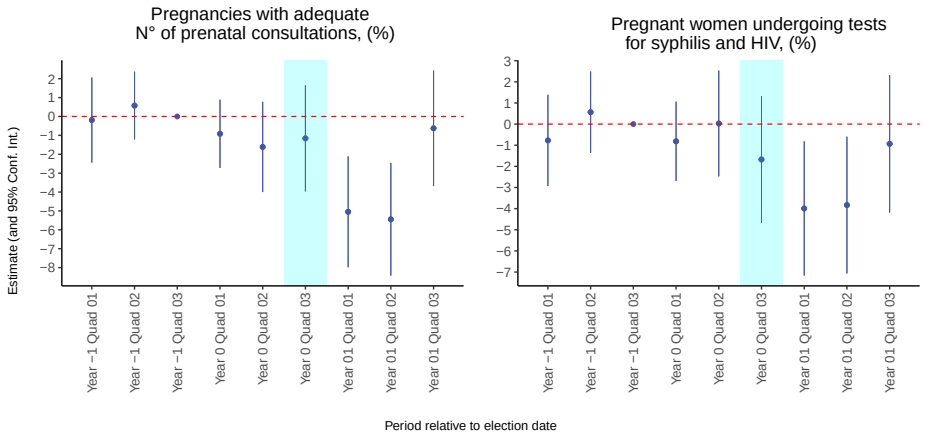
This appendix performs additional exercises removing the restriction on close elections, thus analyzing results for all elections where an incumbent ran and had at least one opponent. Without the restriction, the number of municipalities per electoral cycle increases in each cycle reflecting what is reported in Panel A of Table 1. We perform this exercise to assess the external validity of our findings.

Figure (C.1) Number of Prenatal Consultations per Month, Full Sample



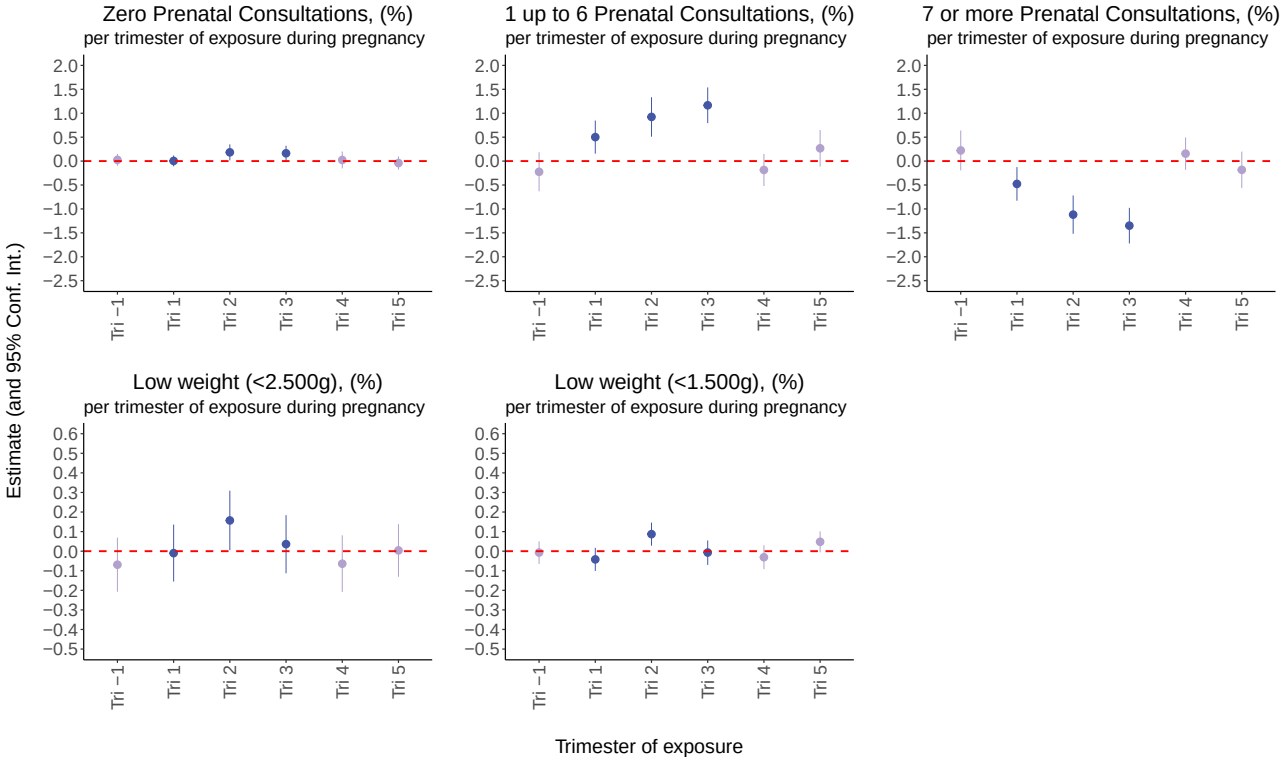
Note: This figure shows graphically results using a municipality by month panel for the 2008 and 2012 electoral periods. The dependent variable is the number of prenatal consultations in the respective month. We do not restrict the analysis to any bandwidth. We show the estimated coefficients on Turnover*D(Year,Month) interactions, ranging from September of year preceding elections (Year -1) up to December of the year following elections (Year 1). The regression also includes all interactions but the omitted category as described in equation (1). For this exercise we have 244,752 observations in a sample of 4,377 municipalities.

Figure (C.2) Indicators of Access to Prenatal Services Measured at the Time of Delivery, Quarterly Outcomes, Full Sample



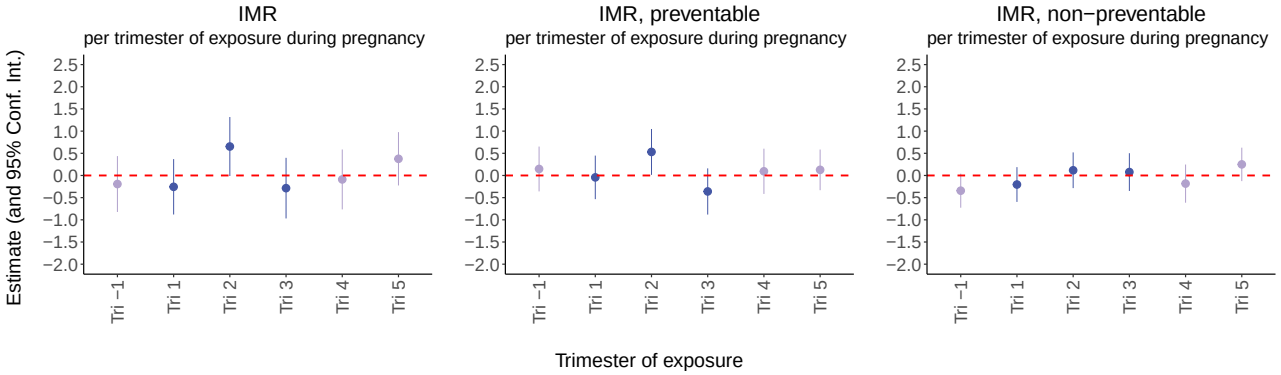
Note: This figure shows graphically results using a municipality by quarter panel for the 2020 electoral period. The dependent variables are the share of pregnancies ending with the adequate number of prenatal consultations and the share of pregnant women undergoing tests for syphilis and HIV. We do not restrict our sample to any bandwidth. All regressions follow equation 1 and include time and municipality fixed effects. We show the estimated coefficients ranging from the first quarter in the year preceding elections (Year -1) to the third quarter of the year following elections (Year 1). The regressions also include all interactions but the omitted category as described in equation (1). For this exercise we have 36,429 observations in a sample of 3,040 municipalities.

Figure (C.3) Birth Outcomes, Full Sample



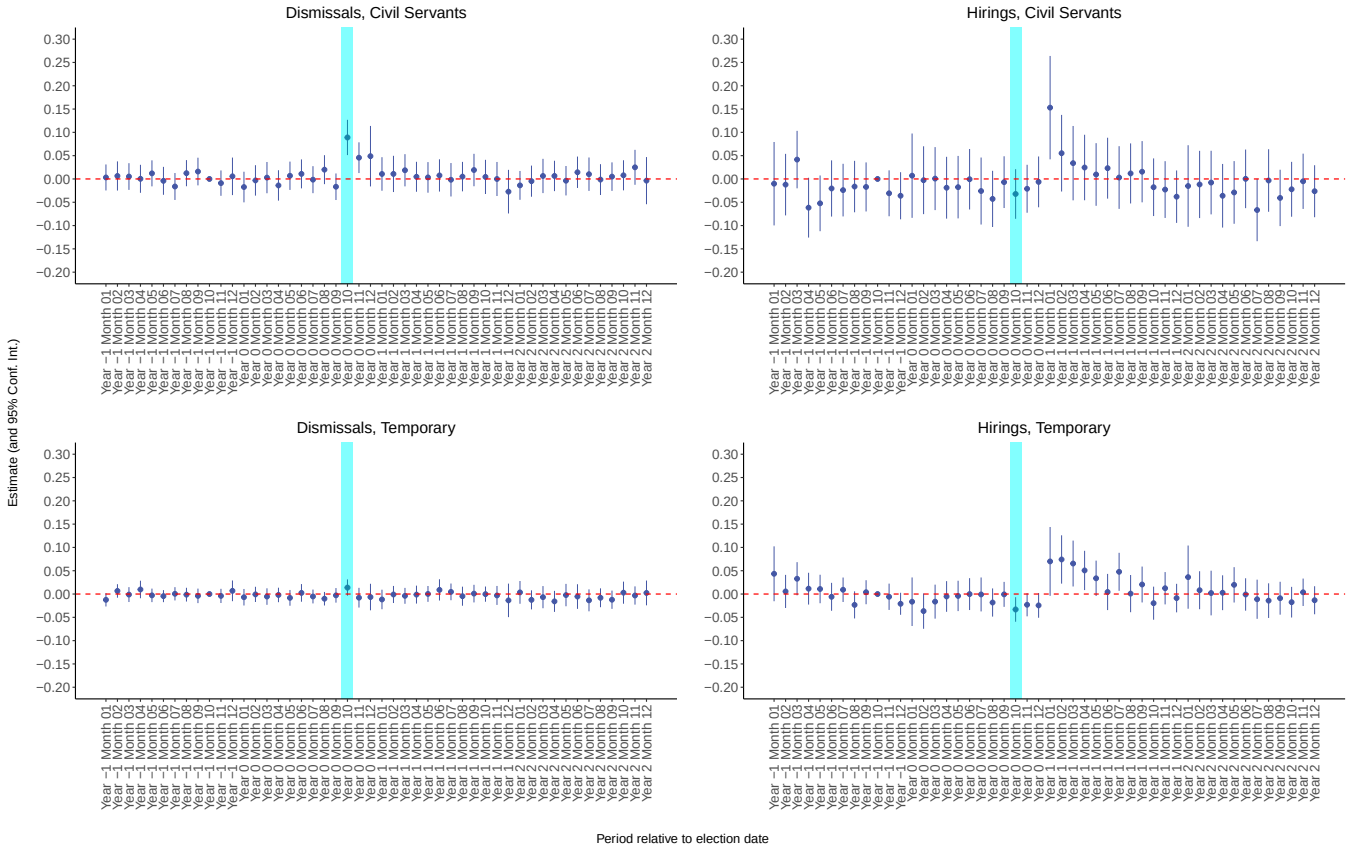
Note: These figures show graphically results using a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations on the plots of the first row. In the second row they represent the share of newborns with low weight at birth and very low weight. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We do not restrict our sample to any bandwidth. For this exercise we have 353,245 observations for prenatal variables and 353,283 for low weight variables in a sample of 4,954 municipalities.

Figure (C.4) Infant Mortality, Full Sample



Note: These figures show graphically results using a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are Infant Mortality Rates (IMR), IMR for preventable causes, and IMR for non-preventable causes of death. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We do not restrict our sample to any bandwidth. For this exercise we have 353,385 observations in a sample of 4,954 municipalities.

Figure (C.5) Human Resources Turnover: Hirings and Dismissals by Type of Contract, Full Sample

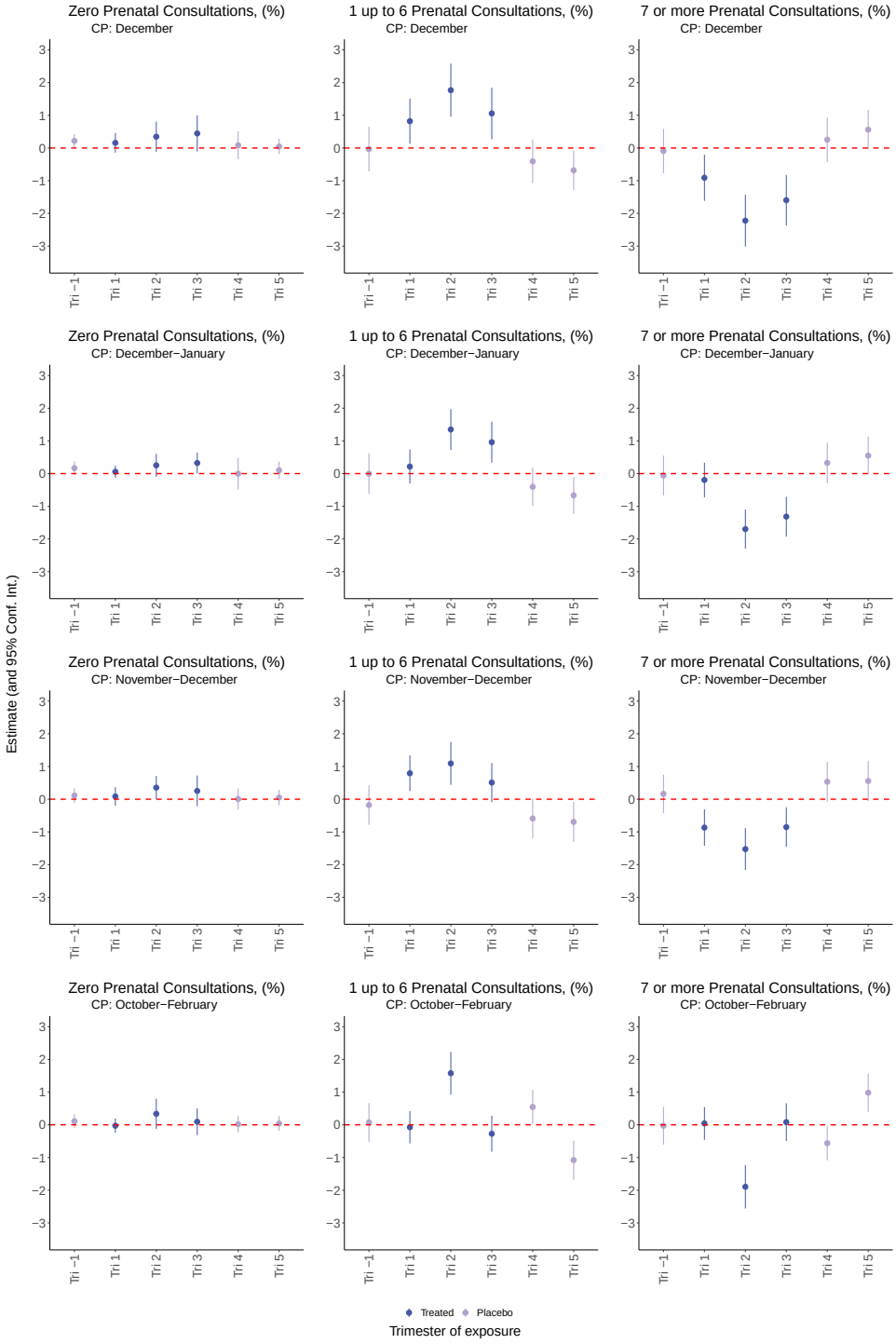


Note: These figures show graphically results using a municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of dismissals and hirings of healthcare workers, separated by type of contract (civil servants and temporaries). We do not restrict our sample to any bandwidth. For this exercise we have 199,248 observations in a sample of 3,952 municipalities.

D Birth Outcomes and Mortality: Varying the Exposure Period

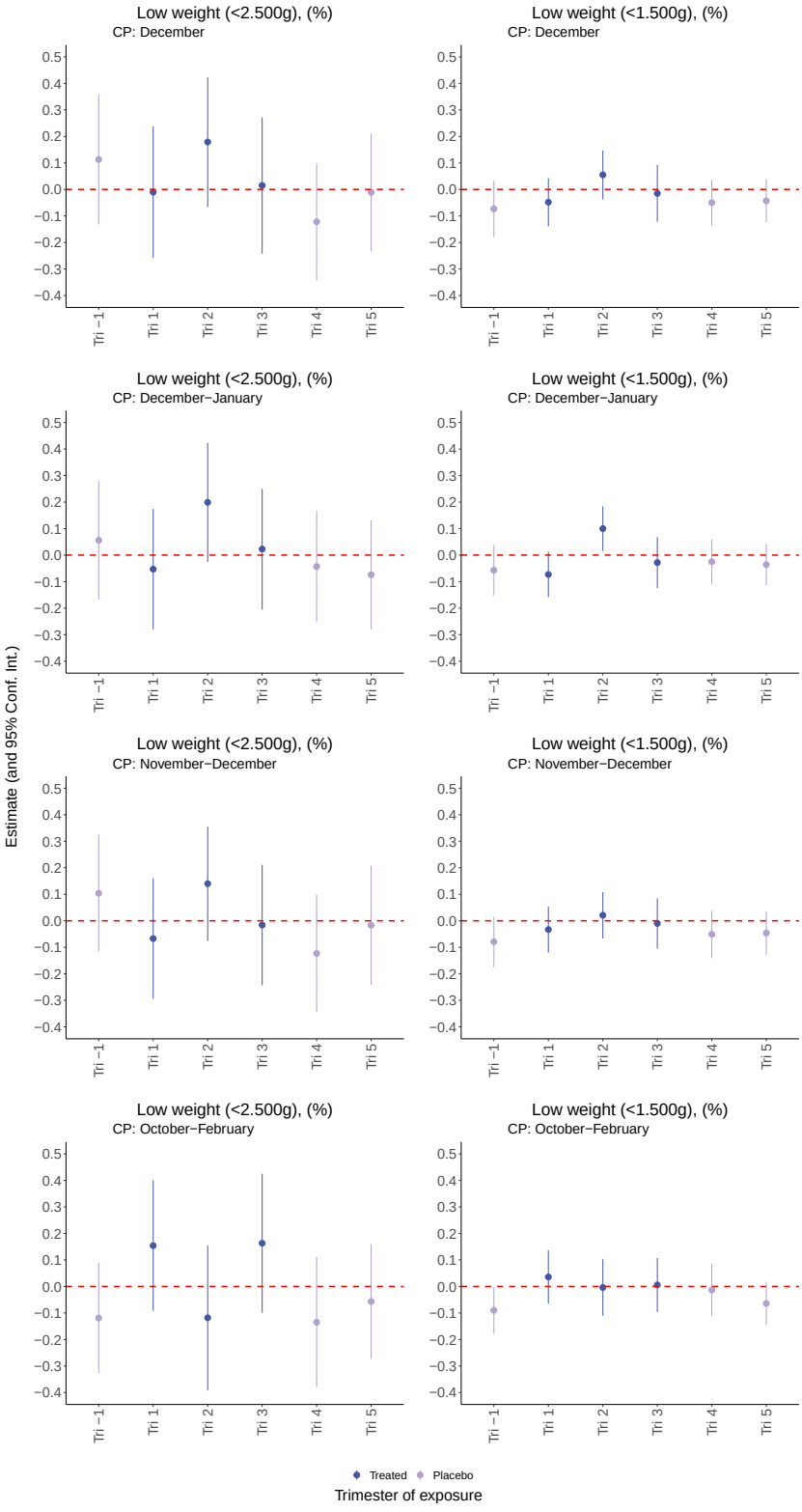
This appendix presents the main results for birth and mortality outcomes using alternative definitions for the critical period of exposure. While for the main definition we consider the exposure period as ranging from November of the electoral year up to January of the following one, here we show our main results for: i) December, ii) December - January; iii) November - December; iv) October-February.

Figure (D.1) Prenatal Outcomes, Alternative Definitions for the Critical Period



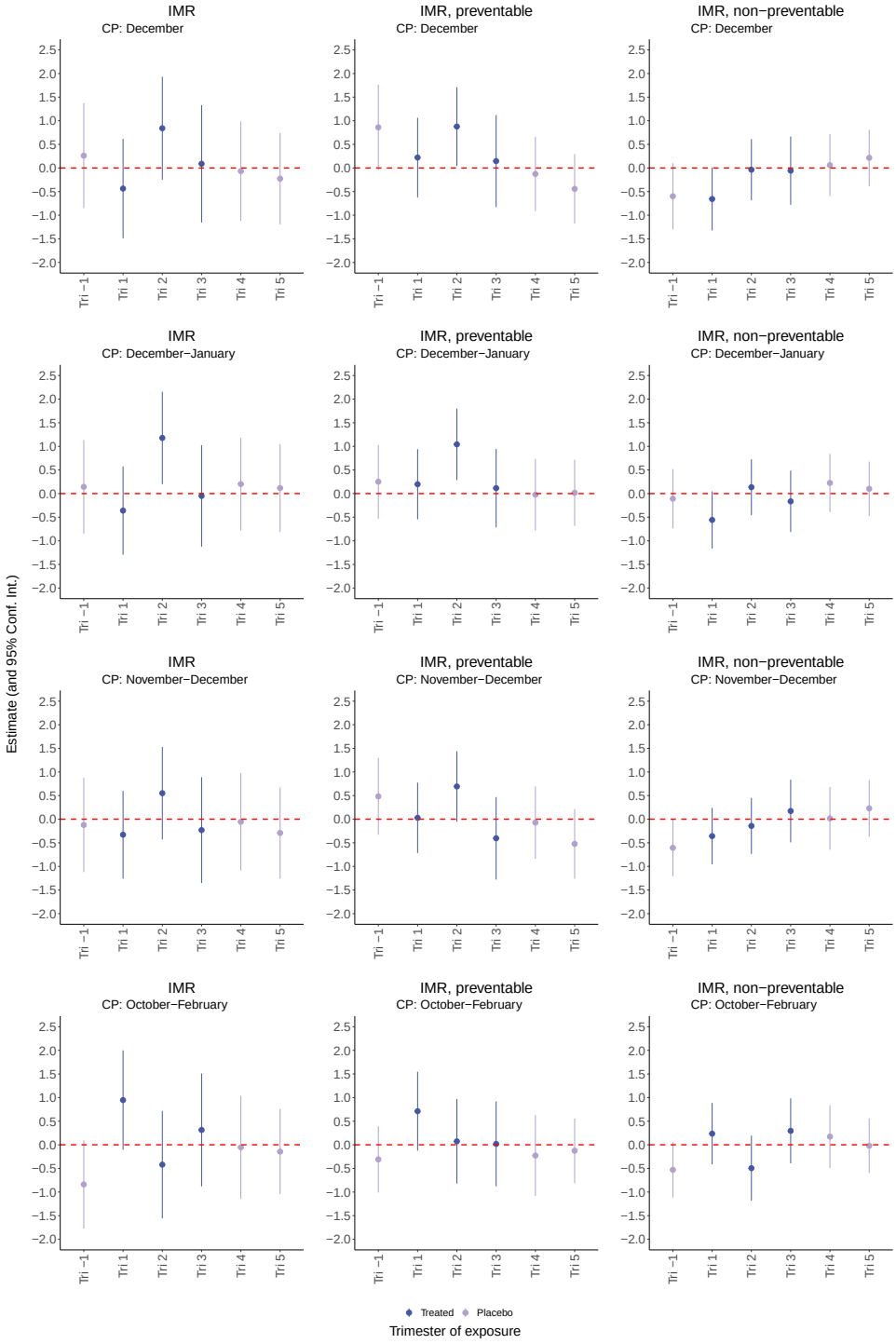
Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations on the plots of the first row. In the second row they represent the share of newborns with low weight at birth and very low weight. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,085 observations in a sample of 3,296 municipalities.

Figure (D.2) Birth Weight, Alternative Definitions for the Critical Period



Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations on the plots of the first row. In the second row they represent the share of newborns with low weight at birth and very low weight. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,107 observations in a sample of 3,296 municipalities.

Figure (D.3) IMR, Alternative Definitions for the Critical Period

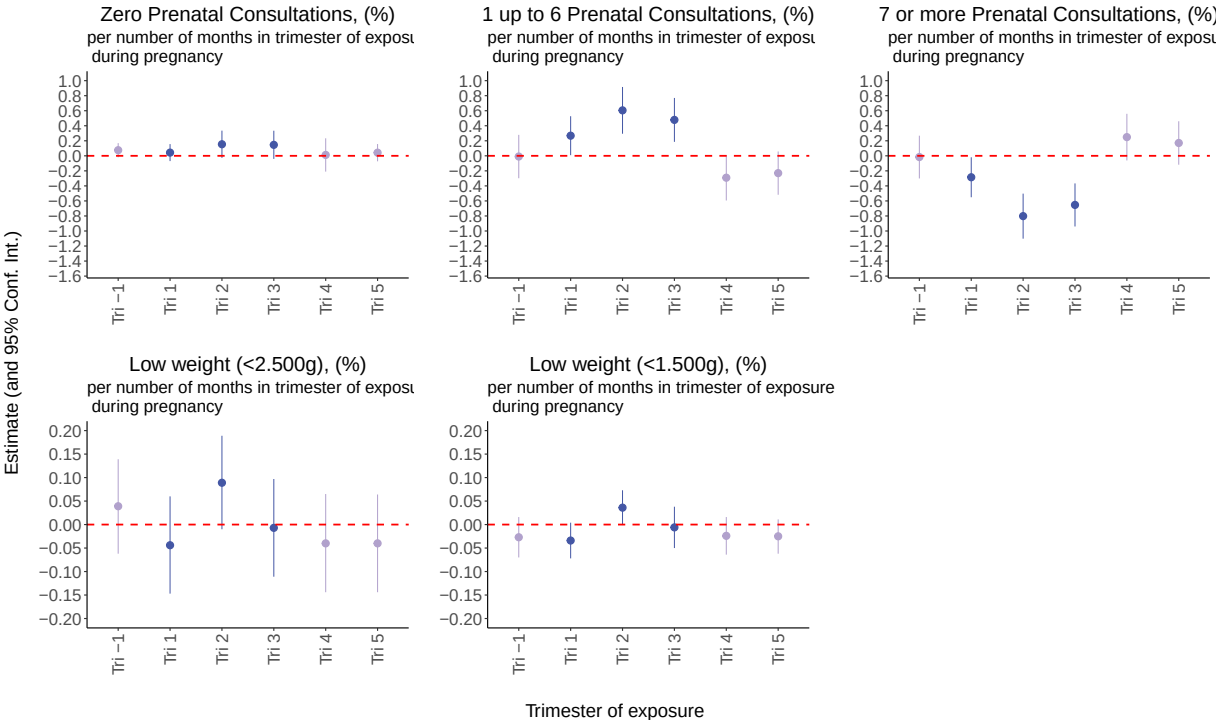


Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are Infant Mortality Rates (IMR), IMR for preventable causes, and IMR for non-preventable causes of death. To provide evidence that the effects do not appear in periods far from the elections we create fake elections in the second year of the new government (Year 2) and repeat our benchmark exercise. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,160 observations in a sample of 3,296 municipalities.

E Birth Outcomes and Mortality: Intensity of Exposure to Transitions

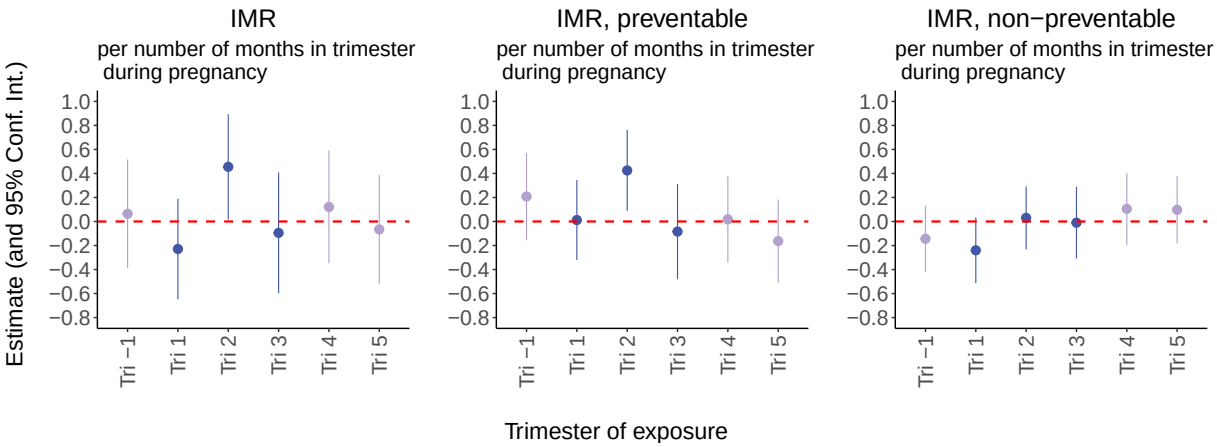
This appendix section presents additional robustness exercises on birth outcomes and mortality. Here we change the definition of the exposure variables to consider the intensity of exposure per month (i.e. the number of months exposed by trimester of gestation, ranging from 0 to 3) rather than dummies for exposed/not exposed in-utero per trimester. Considering the exposure period lasting from November to January, it is possible to be exposed up to three months in one trimester of gestation.

Figure (E.1) Birth Outcomes, Intensity of Exposure



Note: These figures show graphically results from equation 2 changing the definition of the exposure variables to consider the intensity of exposure per month (i.e. the number of months exposed by trimester of gestation). All specifications are based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations on the plots of the first row. In the second row they represent the share of newborns with low and very low birth weight. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,085 observations for prenatal variables and 200,107 for low birth weight variables in a sample of 3,296 municipalities.

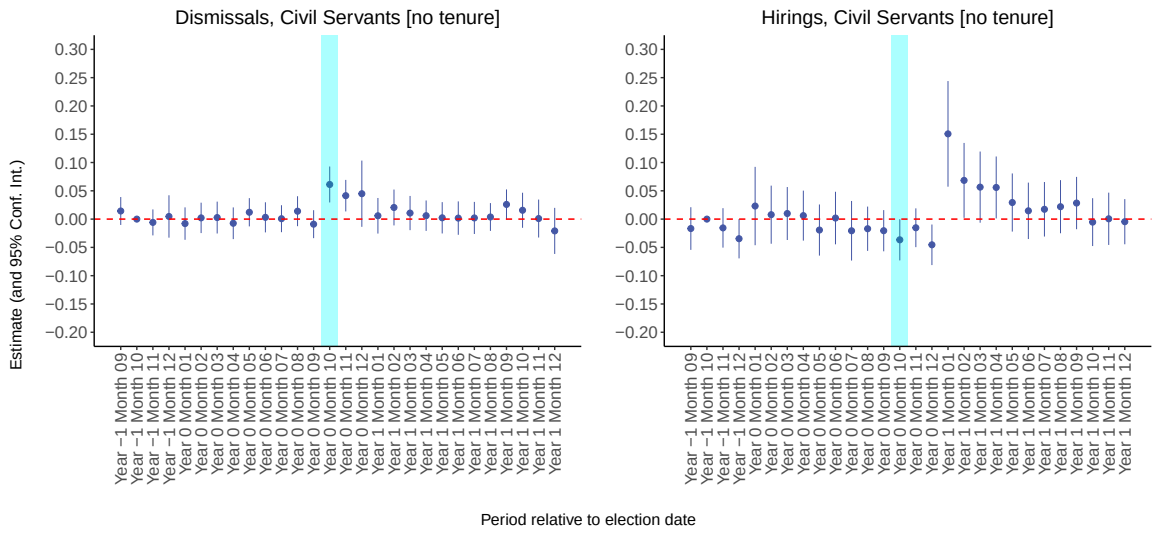
Figure (E.2) Infant Mortality, Intensity of Exposure



Note: These figures show graphically results from equation 2 changing the definition of the exposure variables to consider the intensity of exposure per month (i.e. the number of months exposed by trimester of gestation). All specifications are based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are Infant Mortality Rates (IMR), IMR for preventable causes, and IMR for non-preventable causes of death. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 200,160 observations in a sample of 3,296 municipalities.

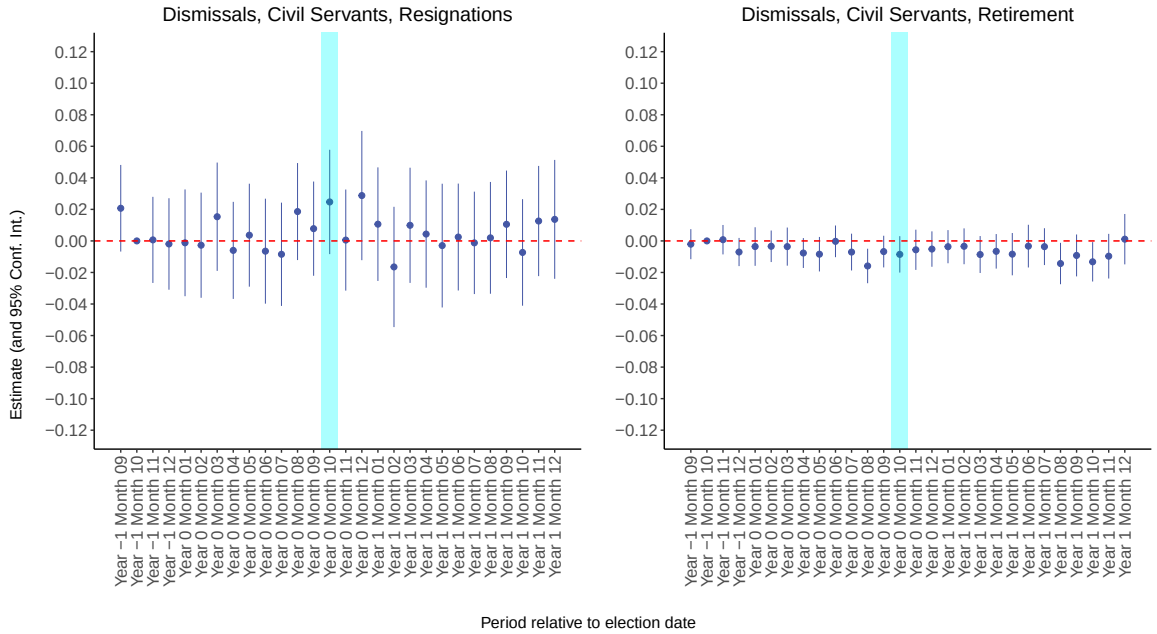
F Human Resources: Additional Results

Figure (F.1) Human Resources Turnover: Non-Tenured Civil Servants



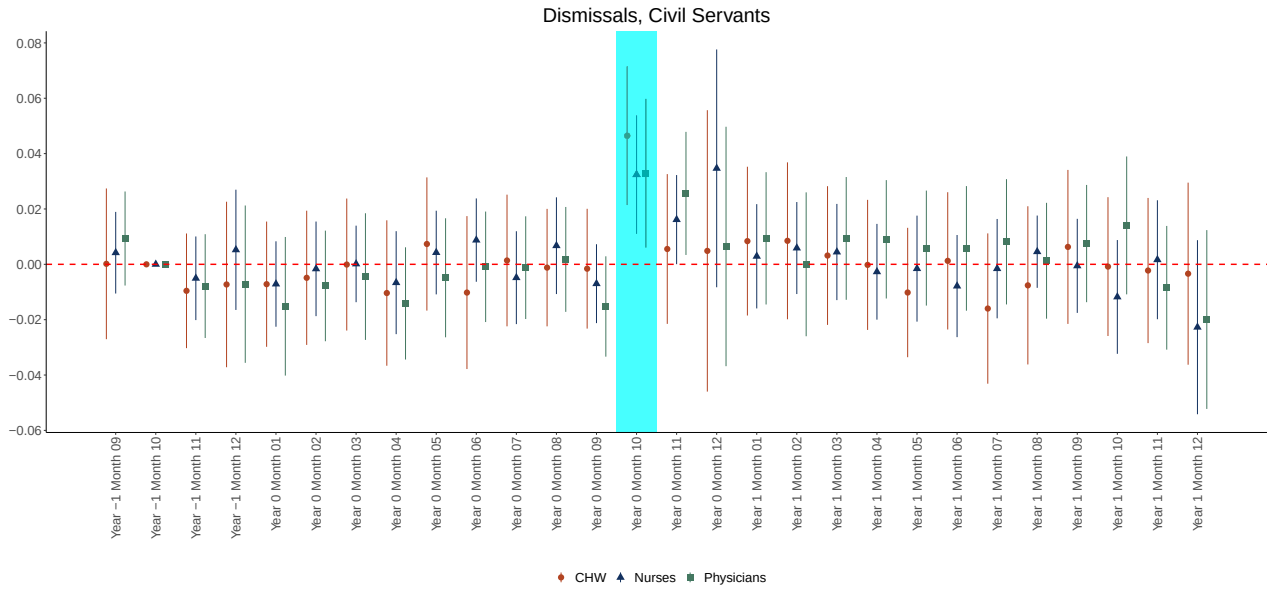
Note: These figures show graphically results using a municipality-by-month municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of dismissals and hirings of healthcare workers under civil servant contracts without tenure (stability). We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 113,292 observations in a sample of 2,376 municipalities.

Figure (F.2) Human Resources Turnover by Type of Contract Termination



Note: These figures show graphically results using a municipality-by-month municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of contractual terminations initiated by the employee (resignations, and retirement) of healthcare workers under civil servant contracts. We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 113,292 observations in a sample of 2,376 municipalities.

Figure (F.3) Human Resources Turnover: Dismissals of Civil Servants by Occupation

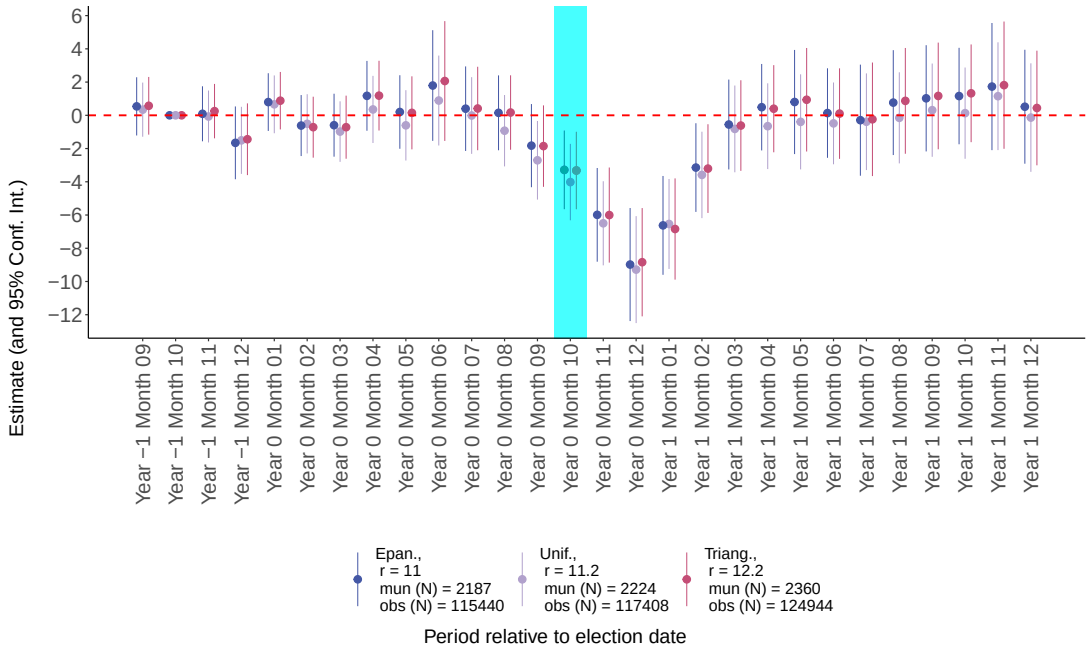


Note: This figure shows graphically results using a municipality-by-month municipality-by-month panel for the 2012 and 2016 electoral periods. The dependent variables are the IHS transformations of the number of dismissals of healthcare workers under civil servant contracts by type of occupation (Community Health Workers, Nurses, Physicians). We set a bandwidth equal to .14 to select the sample of municipalities. For this exercise we have 113,292 observations in a sample of 2,376 municipalities.

G Alternative Bandwidths

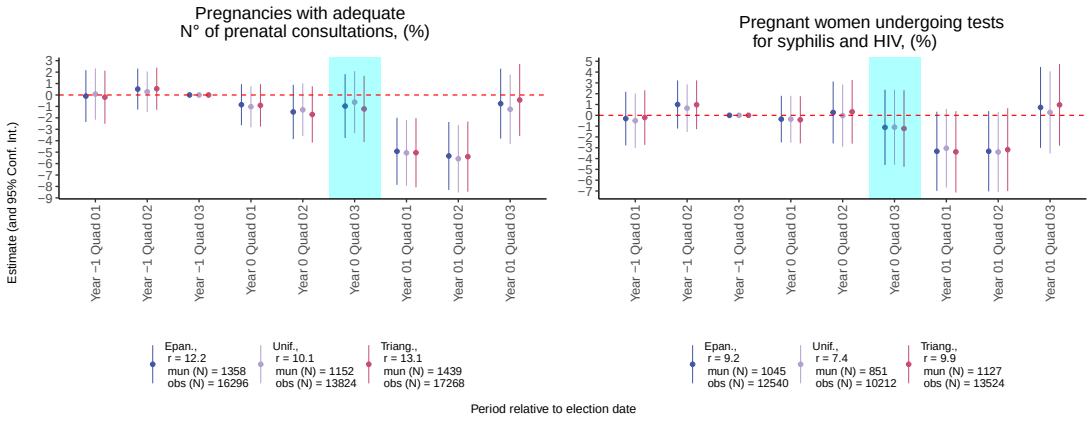
This appendix section reports results from alternative specifications and samples defined from different bandwidths by (i) choosing the optimal bandwidth for each variable to select the close elections; and (ii) varying the weighting method over Epanechnikov, Uniform, and Triangular weights.

Figure (G.1) Number of Prenatal Consultations per Month, Alternative BD and Weighting Definitions



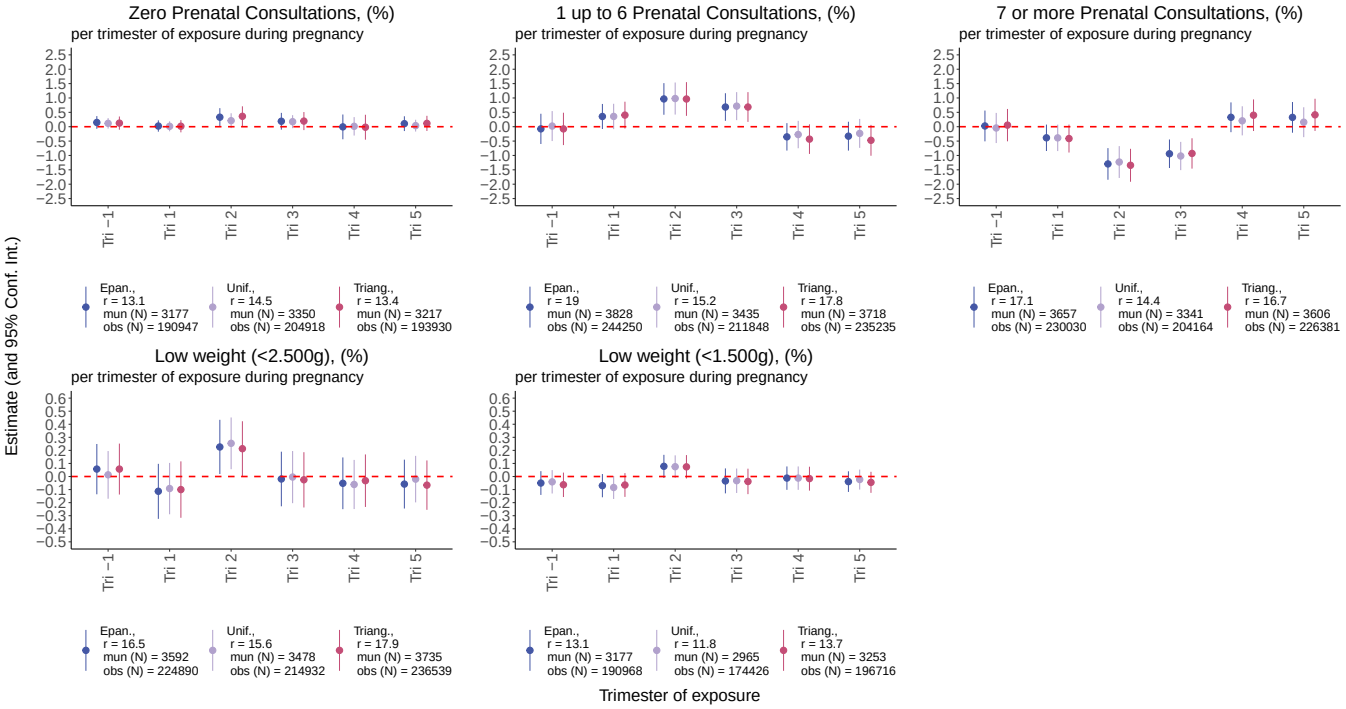
Note: This figure shows graphically results using a municipality by month panel for the 2008 and 2012 electoral cycles. The dependent variable is the number of pregnancies with prenatal consultations in the respective month. Here we test different weighting methods (epanechnikov, uniform, triangular) with the respective optimal bandwidth. We show the estimated coefficients on Turnover*D(Year,Month) interactions, ranging from September of year preceding elections (Year -1) up to December of the year following elections (Year 1). The regression also includes all interactions but the omitted category as described in equation (1). The optimal bandwidth (r), the number of municipalities, and the number of observations in each exercise are reported in the plot.

Figure (G.2) Indicators of Access to Prenatal Services Measured at the Time of Delivery, Quarterly Outcomes, Alternative BD and Weighting Definitions



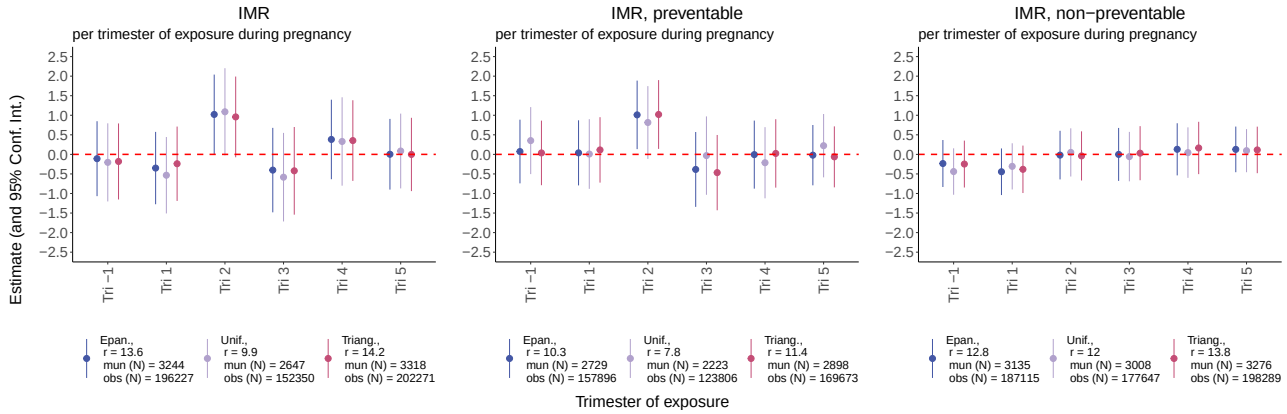
Note: These figures show graphically results using a municipality by quarter panel for the 2020 electoral cycle. The dependent variables are the share of pregnancies that ended up with an adequate number of prenatal consultations, and the share of pregnant women that underwent tests for syphilis and HIV as measured at the time of delivery. Here we test different weighting methods (epanechnikov, uniform, triangular) with the respective optimal bandwidth. All regressions follow equation 1 and include time and municipality fixed effects. We show the estimated coefficients ranging from the first quarter in the year preceding elections (Year -1) to the third quarter of the second year following elections (Year 2). The regressions also include all interactions but the omitted category as described in equation (1). The optimal bandwidth (r), the number of municipalities, and the number of observations in each exercise are reported in the plot.

Figure (G.3) Birth Outcomes and Infant Mortality, Alternative BD and Weighting Definitions



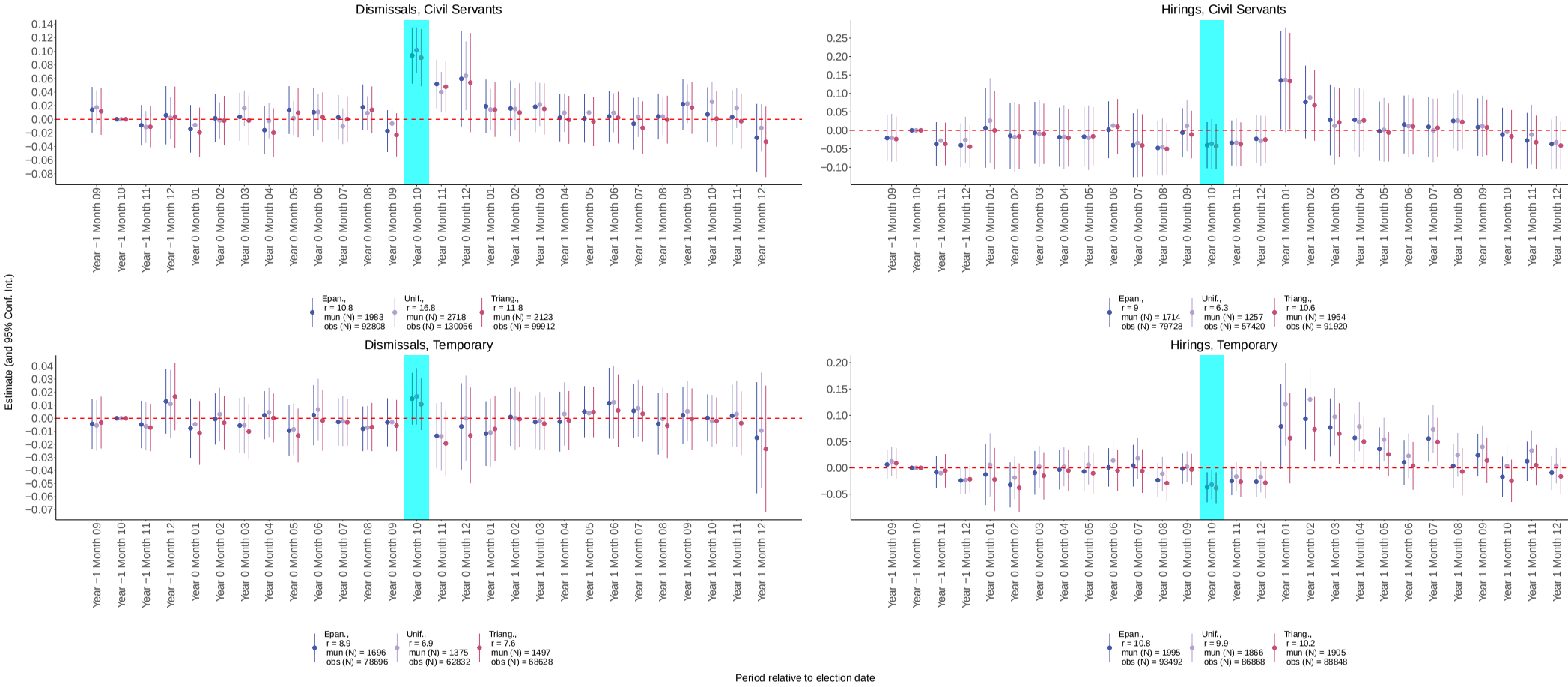
Note: These figures show graphically results using a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are the shares of newborns with zero, 1 up to 6 and 7 or more prenatal consultations on the plots of the first row. In the second row they represent the share of newborns with low weight at birth and very low weight. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. Here we test different weighting methods (epanechnikov, uniform, triangular) with the respective optimal bandwidth. The optimal bandwidth (r), the number of municipalities, and the number of observations in each exercise are reported in the plot.

Figure (G.4) Infant Morality by cause of death, Alternative BD and Weighting Definitions



Note: These figures show graphically results from equation 2, which is based on a municipality-by-month panel for the 2008, 2012, and 2016 electoral periods. The dependent variables are Infant Mortality Rates (IMR), IMR for preventable causes, and IMR for non-preventable causes. "Tri" refers to the trimester of the pregnancy when the child was exposed in-utero. Trimesters -1, 4 and 5 refer to periods before/after gestation, considering gestations of 9 months as benchmark. Here we test different weighting methods (epanechnikov, uniform, triangular) with the respective optimal bandwidth. The optimal bandwidth (r), the number of municipalities, and the number of observations in each exercise are reported in the plot.

Figure (G.5) Human Resources Turnover: Hirings and Dismissals by Type of Contract, Alternative BD and Weighting Definitions



Note: These figures show graphically results using a municipality-by-month panel for the 2012 and 2016 electoral periods. 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. The optimal bandwidth (r), the number of municipalities, and the number of observations in each exercise are reported in the plot.