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Evaluating the impact of Cuban physician cooperation: Evidence from Brazil's Mais Médicos Program

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Abstract

Cuba is a world leader in medical diplomacy, supplying health care workers to underserved and developing countries on a global scale. One of Cuba's largest cooperations, its participation in Brazil's Mais Médicos Program (More Doctors Program, MMP), ended in the abrupt withdrawal of cooperative Cuban physicians in 2018 following political disputes. This thesis aims to quantify heterogeneity in treatment effects between domestic physicians and Cuban cooperative physicians participating in MMP. Using a differences-in-differences method with municipal panel data between 2008 and 2018, I examine the effect of receiving Cuban cooperative physicians as compared to Brazilian physicians on infant health outcomes, as measured by infant mortality, low birth weight, and preterm rate. I find that overall MMP implementation led to reductions in preterm rate in participating municipalities by an average of 8.5 percent. Considering heterogeneity across physician nationality, I find that reductions in preterm rate were over 9 percentage points lower among municipalities receiving Cuban cooperative physicians as compared to municipalities receiving Brazilian physicians. I find no treatment effect across either physician nationality on infant mortality or low birth weight rate.

Keywords: Mais Médicos Program; medical diplomacy; infant health; differences-in-differences
JEL: I14, I18, I38, O19

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1 Introduction

One of the most prominent health challenges faced by developing nations is the recruitment of adequate health personnel to serve rural areas. The World Health Organization (WHO) has identified strengthening international cooperation as one of the key policy responses to addressing health worker shortages in developing countries (Taylor and Dhillon, 2011). On the forefront of global medical cooperation is Cuba, an international leader in supplying medical aid to conflict-ridden and developing countries. In the 60 years since the end of the Cuban Revolution, the country has supplied hundreds of thousands of health care workers to over 100 different countries. Yet, critics point to Cuba's medical diplomacy as a political tactic to gain "soft power", using its global health policy to shape international approval. One of the most prominent examples of controversy surrounding Cuban medical diplomacy is exemplified by one of its largest medical mobilizations to date: the participation of Cuba in the Mais Médicos ('More Doctors') Program in Brazil.

Brazil's Mais Médicos Program (MMP) is a large-scale physician distribution program aimed at universalizing medical care across the most rural and underserved areas of the country and create long-term improvements in health care. MMP was first launched in 2013 by former president Dilma Roussef, and is still active today. The program's priority was the immediate and emergency provision of primary care physicians (PCPs) to rural and disadvantaged municipalities. MMP was originally intended to only recruit domestic Brazilian physicians, but after drastically failing to meet initial enrollment targets, the Brazilian Ministry of Health contracted Cuban health workers to fill the remaining vacancies. From 2013 to 2018, Cuba provided over 11,000 doctors, referred to as *cooperados* ('cooperatives'), to participate in the program. It is estimated that the percentage of Brazilians receiving primary care rose from 59.6 to 70.0 percent in the first four years of MMP (Organização Pan-Americana da Saúde, 2018). Several municipalities received their only doctor as part of MMP. However, the future of the program was threatened by the election of Jair Bolsonaro in 2018, a long-time critic of the Cuban cooperation. Following a series of political disputes and disadvantageous changes to the agreement, Cuba ultimately announced its withdrawal from the program in November 2018. The withdrawal left Brazil scrambling to fill the over 8,000 vacancies left by the *cooperados*, putting into question how the health system would cope without foreign assistance and how this would affect health outcomes.

At the same time, there is a lack of literature examining both the overall health impacts of the Mais Médicos Program as well as the specific contributions of *cooperados*. Previous literature has established a causal relationship between implementation of MMP and increased health care access, in the form of increased number of primary care physicians (PCPs) per capita and number of health care visits attended in participating municipalities (Carrillo and Feres, 2017; Fontes et al., 2018; Mattos and Mazetto, 2019). Yet, there has been little evidence supporting that this increase in health care access has translated into improved health outcomes. There is an even more limited literature examining potential heterogeneities across *cooperado* versus Brazilian physicians participating in the program. The differences in the medical training models between Brazil and Cuba may lead to observable differences in the provision of care, especially in the challenging working conditions of remote and under-resourced areas. The Cuban health care model focuses heavily on preventative medicine, oriented towards the treatment of parasites, malaria, diarrhea, dehydration, and other illnesses common to rural regions (Betto, 2013). In contrast, the Brazilian health care model is highly centralized, providing health personnel primarily in urban areas and city centers and a medical training structure that focuses on sophisticated technological medicine, often impractical in rural and underserved areas (Kirk et al., 2016). Moreover, many of the *cooperados* had previously completed at least one foreign mission prior to participating in MMP (Kirk et al., 2016). The Cuban medical training model coupled with previous experience serving in similar conditions may lead to improved health outcomes in municipalities receiving *cooperados* as compared to Brazilian physicians.

The withdrawal of the *cooperados* from Brazil demonstrates the political fragility of Cuban medical diplomacy and highlights the importance of evaluating its efficacy in order to qualm misinformation and discrimination. Motivated by the current political climate and recent withdrawal of *cooperados*, this thesis seeks to bridge the gap in economic literature regarding Cuba's medical diplomacy and quantify the contributions of the *cooperado* physicians. The Mais Médicos Program in Brazil provides a unique setting of a large-scale foreign PCP mobilization that complemented the existing domestic health labour force, allowing for comparison across physician nationality. This thesis applies a generalized difference-in-difference (DD) framework, exploiting the regional and temporal variation in program implementation across Brazilian municipalities from 2013 to 2017, to evaluate the effect of the Mais Médicos Program on infant health as measured by infant mortality rate, low birth weight rate, and preterm birth rate. In a first step, I evaluate the overall treatment effect of MMP on infant health. In a second step, I explore the existence of heterogeneous treatment effects across physician nationality.

The thesis proceeds as follows. The remainder of Section 1 provides a brief discussion on the health context in Brazil and policy background of the Mais Médicos Program. Section 2 reviews the previous literature. Section 3 describes the data and summary statistics. Section 4 discusses the empirical strategy. Section 5 presents the results and Section 6 provides robustness checks. Section 7 offers a discussion of validity and potential mechanisms. Section 8 concludes.

1.1 Background on Health in Brazil

Brazil is a large and populous country, with over 211 million inhabitants and 8.5 million square kilometers of land. Providing universal health care across its vast scope has been one of the main challenges faced by the Brazilian Ministry of Health. The country is divided into five regions: North, Northeast, Central-West, Southeast, and South. Together, the regions are composed of 27 states, referred to as 'unidades federativas' (UFs), further divided into a total of 5,570 municipalities. In addition to the 5,570 municipalities, there are also 34 Special Indigenious Health Districts, or 'Distrito Sanitário Especial Indígeno' (DSEI). DSEIs are regions defined by ethno-cultural diversity that are autonomous from the publicly funded universal health care system. A map of the DSEIs is shown in the Appendix, Figure A.2.

Brazil has historically struggled with low physician rates and a widespread inaccessibility to health services. Medical attention is especially lacking in Brazil's most rural regions, the North and Northeast. In 2013, the year of MMP implementation, Brazil had an average of 1.8 physicians per 1,000 inhabitants, whereas neighboring countries Argentina and Uruguay had rates of 3.2 and 3.7, respectively. Of the 27 Brazilian states, 5 had an average physician rate below 1.0 per 1,000 inhabitants (Acre, Amapá, Maranhão, Pará, and Piauí). Of the 5,570 municipalities, over 700 had less than 1 physician per 1,000 inhabitants (Ministério da Saúde, 2013, 2015). In addition to the geographical challenges of providing health care across a vast country, Brazil also struggled with training and recruiting adequate health care personnel. In 2013, Brazil only had 93,000 medical graduates to fill 146,000 positions. From 2002 to 2012, the doctors educated in Brazilian medical schools constituted only 65% of the total market demand, a shortage of 53,000 doctors (Ministério da Saúde, 2015).

One of the primary objectives of the Mais Médicos Program in creating long-term and cost-effective health improvements was to improve infant health outcomes. Reducing child mortality is the fourth of the Millennium Development Goals (MDG) set by the United Nations, aiming to end preventable deaths of newborns and children under 5 years of age by 2030 and reduce neonatal and under-5 mortality rates to at least as low as 12 and 25 per 1,000 live births, respectively. Brazil has historically faced high rates of infant mortality, low birth weight, and preterm birth, especially in its most rural areas. In 1990, Brazil's infant mortality rate was 52.5 infant deaths per 1,000 live births. By 2010, Brazil had decreased its infant mortality rate to 16.7. Despite immense progress, Brazil's infant mortality rate was still far higher than that of most developed nations[2] and there were wide disparities in infant mortality across the five regions. In the South region of Brazil, the infant mortality rate in 2002 was 17.9, while it was more than double that in the Northeast region in the

[2]The EU had an average of 4.0 infant deaths per 1,000 live births in 2010. The US infant mortality rate in the same year was 6.2 per 1,000 live births (UNICEF, 2005).

same year, at 41.4 deaths per 1,000 live births (UNICEF, 2005).

Similarly, Brazil has also faced high low birth weight and prematurity rates, both identified as primary predictors of neonatal and under-5 mortality (Liu et al., 2016). Preterm births account for almost half of all newborn deaths worldwide (Jawaid, 2012). According to the WHO, Brazil also has one of the highest numbers of preterm births in the world, with nearly 280,000 preterm births in 2010 (Jawaid, 2012). Birth weight has also been found to be inversely associated with adult mortality, especially among cardiovascular mortality (Risnes et al., 2011). Brazil's low birth weight rate has remained steady around 8.5% of live births in the past 15 years, one of the highest rates among developing nations.

Considering Brazil's objective to create long-term health improvements through the introduction of MMP, this thesis focuses on MMP effects on infant health outcomes. Three infant health outcomes are considered: infant mortality rate, low birth weight rate, and preterm birth rate.

1.2 Background on Mais Médicos Program

President Dilma Rousseff launched the Mais Médicos Program in July 2013 to address the faults in the Brazilian health care system and reduce the inequalities in health care access across Brazil. The program was enacted by Law 12,871 in October 2013 and is still in practice today.

The Mais Médicos Program included three strategic axes of intervention: (1) investment in health care facilities, (2) increasing medical school enrollment and improving medical training, and (3) addressing the emergency provision of doctors for primary care. As part of the first axis of the program, the Brazilian government invested R$7.5 billion, roughly 1.35 billion USD, in the construction of 818 hospitals, 877 emergency health units, and 16,000 basic health units (BHUs) across the country (Ministério da Saúde, 2013). In order to address the shortage in health care professionals, the Ministry of Health revamped its medical training program by both increasing vacancies and restructuring curriculums to encourage specialization. With these changes, the Ministry of Health aims to increase the number of domestically trained physicians from 374,000 to 600,000 by 2026 (Ministério da Saúde, 2013). The most urgent focus, and main difficulty, of the Mais Médicos Program was the third axis: recruitment of physicians. In order to tackle the regional inequality in health care access, the Ministry of Health called for an emergency recruitment of Brazilian doctors to fill over 15,000 vacancies.

This paper will focus on the third axis of the Mais Médicos Program, the emergency provision of physicians. Physicians and municipal leaders applied to join MMP via the Ministry of Health website. The first enrollment call took place in July 2013, where 3,511 municipalities applied, requesting a total of 15,640 doctors. Although 18,450 Brazilian doctors began the application process, only 1,096 completed their application and accepted their placement. To fill the remaining vacancies, the Ministry of Health opened up the application to foreign doctors and foreign-educated Brazilian doctors before forming an agreement with the Pan-American Health Organization (PAHO), recruiting Cuban medical professionals to fill the remaining shortage. The selection of doctors for the program was made with the following order of preference:

(i) Doctors registered in Brazil - either trained in Brazil or trained abroad and revalidated their diploma in Brazil.

(ii) Brazilian doctors trained abroad[3].

(iii) Foreign doctors trained abroad[4].

(iv) Cuban doctors through cooperation with PAHO.

Doctors are contracted for three years, with the ability to renew at the end of the third year. Recruitment criteria for foreign physicians include (i) qualification to practice medicine from country of origin, (ii) proficiency in Portuguese, and (iii) medical training from a country with a higher physician rate per 1,000 inhabitants than that of Brazil (1.8). Upon inscription

[3]There is a lack of data on which municipalities received physicians of Brazilian nationality but with foreign training. For the purpose of this paper, these doctors will be considered as Brazilian doctors.

[4]For the purposes of this thesis, foreign non-Cuban physicians participating in MMP are grouped in the Brazilian nationality reference group. This classification is discussed in greater detail in Section 3.

into the program, physicians select between six categories of municipalities where they wish to practice: (i) UF capital, (ii) metropolitan region, (iii) G100 (group of 100 most vulnerable municipalities, defined as those with lowest physician rates), (iv) municipalities with over 20% extreme poverty, (v) DSEIs, or (vi) other. The Ministry of Health then allocates the physicians across participating municipalities, giving priority to municipalities fulfilling at least one of the following criteria[5]: (i) 20% or more of the population living in extreme poverty, (ii) more than 80,000 inhabitants and lowest levels of public revenue per capita, (iii) indigenous health districts (DSEIs). Physicians must accept their allocation before they are confirmed into the program. Brazilian degree-holders travel directly to the municipality and begin practicing. Foreign physicians first attend a 3-week intensive training course which addresses clinical language, health care protocols, and the national health system. Participating physicians practice in BHUs, small medical clinics offering basic services. BHUs are the dominant form of health services in many rural municipalities. Participating doctors are primarily practicing family medicine and preventative medicine.

In total, 1,557 Brazilian and foreign (non-Cuban) physicians accepted enrollment in the first round of the program, representing only 10% of the demand solicited by municipalities. The physicians were allocated amongst 579 municipalities and 18 DSEIs, representing only 16% of the participating municipalities.

1.3 Cuba's Involvement and Withdrawal

With the first round of MMP recruitment failing to attract enough Brazilian physicians to meet the solicited demand, the Ministry of Health formed an agreement with PAHO to supply Cuban *cooperado* physicians. Of the 4,820 physicians practicing in the first year of MMP, 3,263 were Cuban. Of the over 100,000 doctors that participated in MMP from 2013 to 2018, over 60 percent were Cuban *cooperados*. The number of physicians participating in MMP per year and by nationality is shown in Appendix Figure A.1.

[5] Assumptions on the allocation of physicians across municipalities are further discussed in Section 4 below.

Cuba's deployment to Brazil is far from the first of its medical missions. Medical internationalism has been at the forefront of Cuba's foreign policy and export economy since the onset of the Cuban revolution over sixty years ago. Since 1960, Cuba has supplied over 185,000 medical professionals to over 100 countries (Ojito, 2009). Despite its wide-ranging provision of medical missions, Cuba's medical diplomacy has been met with heavy criticism on a global scale, specifically surrounding its pay structure. Cuban *cooperados* received approximately USD $1,000 (split between receiving $400 immediately and $600 deposited to a Cuban account) of the $4,200 total the Brazilian government pays per worker per month, despite the challenging working conditions (Kirk et al., 2016). In November 2018, right-wing president-elect Jair Bolsonaro appealed to the criticism over *cooperados* in his campaign, stating that Cuban presence in Brazil was a threat to democracy and domestic health jobs and threatened to end the PAHO agreement (Darlington, 2018; Dyer, 2018). Upon his election, Bolsonaro announced that Cuban physicians would no longer be contracted through PAHO and must instead be contracted individually with the Ministry of Health. Under the new legislation, the *cooperados* were required to take Brazilian medical exams and register in the Brazilian medical system, a complicated and timely process, rather than be hired through a group agreement through PAHO. As a response, the Cuban government withdrew over 8,000 physicians practicing in Brazil in November 2018 (Alves, 2018).

With an estimated 24 million Brazilians receiving care from Cuban doctors, the withdrawal of Cuban doctors left a massive dent in medical coverage (Alves, 2018). To fill the resulting vacancies, president Bolsonaro issued a new call for domestic doctors with initially optimistic results: 36,490 applicants for 8,517 positions. However, as of July 2019, over 3,800 vacancies across 3,000 municipalities remained unfilled (Darlington and Casado, 2019). Gabriel Vivas et al. (2020) forecast that the failure to replace MMP physicians would lead to over 10,000 avoidable under-5 deaths in 2030 and a cumulative total of over 100,000 avoidable child and adult deaths.

2 Literature Review

This paper relates primarily to two main bodies of literature: (1) literature on Cuban medical internationalism and its effect on health outcomes in recipient nations, and (2) literature on the Mais Médicos Program and similar PCP programs both in the Brazilian and worldwide context.

2.1 Literature on Cuban Medical Internationalism

There exists an extensive qualitative literature on the socio-political perceptions of Cuba's medical missions across the world (Blue, 2010; Feinsilver, 2010; Kirk, 2015), but there is a large gap in economic literature evaluating its impacts on health outcomes in recipient countries. Hone et al. (2020) examine the effect of the Mais Médicos Program on amenable mortality, evaluating heterogeneous effects across physician nationality. They find a significant reduction in amenable mortality of 1.50 deaths per 100,000 per year among municipalities receiving less than 20 percent Brazilian physicians. Rech et al. (2018) conducted a national cross-sectional survey assessing user perception of primary health care performance as part of MMP, looking at three physician categories: Brazilian physicians participating in MMP, Cuban physicians participating in MMP, and Brazilian physicians not linked to MMP. They find no significant variation in primary care performance score across physician categories. To the extent of my knowledge, Hone et al. (2020) and Rech et al. (2018) are the only other papers to directly compare health outcomes between Cuban and domestic physicians.

Cuba's medical internationalism has mainly targeted impoverished and disaster-stricken nations lacking a strong domestic health work force, beginning in Africa and later expanding to Latin America. By the late 1980s, Cuba had sent doctors to over 30 countries in Africa, making up a significant portion of the health labour force (Feinsilver, 1996). Infant mortality rates in several African countries decreased substantially during the period when Cuban health workers were active. For example, infant mortality decreased from 59.0 to 7.8 per 1,000 live births in Ghana, 48.0 to 10.6 in Eritrea, and 131.0 to 35.5 in Equatorial Guinea while Cuban health workers were practicing (Kirk, 2015). Similar trends were evident in Latin America. The infant mortality rate in Guatemala fell from 41.0 deaths per 1,000 live births in 1998, when Cuban doctors arrived following the devastation of Hurricane Mitch, to 35.0 in 2005 (Gorry, 2009). While it is likely that the arrival of Cuban doctors helped save lives and improve health outcomes in these impoverished areas, there is a lack of economic literature exploring potential mechanisms and determining a causal relationship.

2.2 Literature on MMP and Similar Programs

There has been a growing body of economic literature on the Mais Médicos Progam in the years since its launch in 2013. Previous literature has found that MMP implementation led to significant increases in physician supply and health care utilization while decreasing hospitalizations (Carrillo and Feres, 2017; Fontes et al., 2018; Mattos and Mazetto, 2019). Carrillo and Feres (2017) find that while MMP increased physician supply by 17 percent in treated areas, there were no significant effects on infant health outcomes, including infant mortality rate, proportion of low birth weight births, and proportion of preterm births. Fontes et al. (2018) find that MMP led to a decrease in hospitalizations for ambulatory care sensitive conditions, with the size of effect increasing with years of MMP exposure. Mattos and Mazetto (2019) use a sample of 2,940 municipalities for the period 2010 to 2015 to examine MMP impact on health care visits and various mortality rates. They find that MMP led to increases in health care utilization, including appointments, consultations, and home visits, but no statistical impacts on general, elderly, maternal, or under-5 mortality rates. As opposed to the literature on MMP, studies have found evidence that the preceding program in Brazil, the Family Health Strategy, led to both increases in primary care physicians and reductions in infant mortality (Aquino et al., 2009; Rocha and Soares, 2010; Russo et al., 2019).

This paper also relates to the greater body of literature on PCP-oriented health policies on a global scale. Literature from a range of countries with similar contexts of socioeconomic disparity and inequalities in health care access have found evidence of similar PCP-oriented programs leading to reductions in infant mortality. Frankenberg (1995) uses a maximum likelihood model to estimate the effect of health care access on infant mortality rates in Indonesian villages, finding that the presence of an additional doctor decreased the odds of infant mortality by 1.7 percent. Using data from US states from 1985 to 1995, Shi et al. (2004) find that the number of primary care physicians is negatively associated with infant mortality and low birth weight, especially in areas of high socioeconomic inequality. An increase of one PCP was associated with a reduction in infant mortality by 2.5 percent and a 3.2 percent reduction in low birth weight births. Naderimagham et al. (2017) find that a similar rural family physician program implemented in Iran in 2005 led to decreases in neonatal and infant mortality rates by 0.341 and 0.016, respectively.

2.3 Contributions to Previous Literature

This paper contributes to previous literature by utilizing the geographic, temporal, and physician nationality variation in MMP implementation across municipalities to compare health outcomes in municipalities receiving Brazilian and Cuban physicians. While previous literature has not found consistent evidence for MMP improving infant health outcomes, these effects may be evident when looking across physician nationality. Furthermore, I expand on the previous literature by including (at least) two additional years of data, expanding the study period to 2018 and including municipalities joining MMP up until 2017. To my knowledge, this is the first paper aside from Hone et al. (2020) to quantitatively evaluate *cooperado* contributions to health outcomes in the MMP context and the first to evaluate *cooperado* contributions to infant health outcomes, specifically.

3 Data

The data used for this thesis was collated across various databases publicly available via the Brazilian government website[6] in order to construct yearly municipal panel data. The

[6] https://www.gov.br/pt-br/orgaos-do-governo

data covers the period from 2008 to 2018, including the first five years of MMP implementation from 2013 to 2017, and is comprised of 4,920 municipalities.

3.1 Dependent Variables

Live birth and infant death data were obtained from Brazil's publicly funded health care database, DataSUS. The Information System of Live Births (SINASC) and Information System of Mortality (SIM) contain records of over 32 million live births and 400,000 infant deaths during the study period, respectively[7]. An infant death is defined as death in the first year of life. Infant mortality rates (IMR) were calculated by aggregating infant death counts per municipality-year and dividing by the total number of live births in the same municipality-year, scaled to 1,000 live births. While SIM is estimated to cover over 96% of deaths[8], there is a possibility that the absence of a birth or death in a given municipality-year is due to lack of recording. This uncertainty could be especially problematic in smaller municipalities with fewer observations. The absence of death or birth records would bias the infant mortality estimates (depending on whether death or birth records are absent, mortality estimates could be either under- or over-stated). In order to control for this as much as possible, I restrict the data sample to include only municipalities with both death and birth data for at least two years in both the pre- and post-treatment periods. Municipalities joining MMP in 2017 were restricted to those with two periods of both death and birth data in the pre-treatment period and one period of death and birth data in the post-treatment period, corresponding to 2018, the last period of the study period.

The SINASC and SIM databases also include data on birth weight, gestational age, and number of prenatal consultations attended by the mother. The birth weight and gestational

[7]The records are completely anonymous and contain no names or other identifying information.

[8]More information on SIM coverage can be found at:
http://tabnet.datasus.gov.br/cgi/sim/Consolida_Sim_2011.pdf. Data is not available on the distribution of SIM coverage across municipalities. I assume that the rate of coverage is uniform across municipalities.

age data was used to calculate the proportion of low birth-weight births[9] and preterm births per 1,000 live births per municipality-year. I follow WHO definitions and define low birth weight as birth weight under 2,500 grams (irrespective of gestational age) and preterm birth as birth under 37 weeks gestational age. Low birth weight and preterm birth rates were calculated by dividing the number of low birth weight and preterm births by the total number of live births with non-missing birth weight or gestation information in the same municipality-year. Prenatal consultation data was averaged per municipality-year and used as the dependent variable in a first-stage regression.

3.2 Independent Variables

The SINASC and SIM databases also include information on maternal characteristics, including mother age and years of schooling completed by the mother. Maternal characteristic covariates were created by averaging this data across the SINASC and SIM databases per municipality-year. The remaining covariate data was obtained across several publicly available databases. Annual population estimates and GDP per capita data were obtained at the municipal-level from the Brazilian Institute of Geography and Statistics (IBGE) website. Yearly municipal-level data on water treatment coverage (as percentage of population) was available from the National System of Information on Sanitation (SNIS). Yearly municipal-level vaccination coverage data was obtained from the National Immunizations Program (PNI). Data on number of BHUs in the municipality was obtained from the National Registry of Health Facilities (CNES) and scaled per 10,000 population. Data on annual Bolsa Família Program[10] expenditure per capita was obtained from the Brazilian Open Data Portal ('Portal Brasileiro de Dados Abertos') and scaled per 1,000 population. Municipalities with missing covariate data were dropped from the sample.

[9]The live birth dataset included 20,499 single (as opposed to twin or more) birth observations with a recorded birth weight under 1,000 grams (classified by the WHO as 'extremely low birth weight'), gestational age above 32 weeks, and combined apgar-1 and apgar-5 score above 15. These observations were excluded from the dataset due to suspicion of data recording error (0.06% of the entire sample).

[10]The Bolsa Família Program is discussed in Section 4.

3.3 Data on Physician Nationality and Allocation

Data on allocation of Brazilian physicians across municipalities was obtained from the Mais Médicos Program Management System ('Sistema de Gerenciamento de Programas') through accessing ordinance archives on their website and manually matching physician records to municipalities. Supplemental nationality data was obtained by request from researcher Thomas Hone of the Imperial College London[11] and by request directly from the Mais Médicos Program administration[12].

This thesis considers the study period from 2008 to 2018, including the first five years of MMP implementation between 2013 and 2017. 27 municipalities joining the program in 2018 are omitted from the sample. The treatment group is defined as municipalities receiving at least one physician through the Mais Médicos Program. Participating municipalities received either Brazilian physicians, Cuban *cooperado* physicians, foreign non-Cuban physicians, or a combination. The sample size of municipalities receiving solely foreign non-Cuban physicians is not sufficiently large ($N = 10$) to obtain reliable estimates when included as a separate treatment subgroup. Treated municipalities received an average of 7.5 percent foreign non-Cuban physicians as proportion of total MMP physicians. There were no municipalities in the sample that received both *cooperado* and foreign non-Cuban physicians but not Brazilian physicians. As such, for the purposes of this thesis, foreign non-Cuban physicians are considered as part of the Brazilian treatment group. This thesis thus considers two nationality subgroups: Brazilian (including foreign non-Cuban physicians)[13] and *cooperado* physicians. The overall treatment group is comprised of 4,202 municipalities, of which 835 received solely Brazilian MMP physicians throughout the study period, 1,049 municipalities received solely *cooperado* physicians, and an additional 2,318 municipalities received both

[11] Hone et al. (2020) manually match physician records obtained from the Ministry of Health and the National Registry of Health Facilities (CNES) according to municipality of allocation and physician nationality. Using this data, they examine the existence of heterogeneous treatment effects of MMP exposure across physician nationality on amenable mortality.

[12] All physician data used is anonymous.

[13] For the sake of simplicity, the Brazilian and foreign non-Cuban subgroup is merely referred to as the 'Brazilian' subgroup going forward.

Brazilian and *cooperado* physicians throughout the study period. The control group is comprised of 718 municipalities that did not receive any MMP physicians throughout the study period. Although *cooperado* physicians were recruited to fill vacancies all across the country, a potential concern is that they were disproportionately placed in more disadvantaged areas with a higher likelihood for poor birth outcomes, as compared to the placements of Brazilian physicians. I find no indication that the municipalities receiving Brazilian versus *cooperado* physicians differ drastically in values of covariates that may indicate health outcomes (see Appendix Table B.2).

Table 1 displays the pre- and post-treatment means for the dependent and independent variables. On average, infant mortality low birth weight, and preterm rates were all lower in the treatment group as compared to control group prior to policy implementation, differing by -3.10, -2.25, and -1.17, respectively. Table 2 displays the summary statistics for the distribution of MMP physicians across the post-treatment periods for treated municipalities, by nationality. There is a large variation in total MMP physicians received across treated municipalities, ranging from one to 3,413 with a mean of 47.73 and standard deviation of 109.56. It should be noted that this maximum value is an outlier: only 11 municipalities received more than 1,000 total MMP physicians in a given year and only 38 received over 500. These municipalities were either the country capital, Brasilia, or state capitals. The average municipality received 61.78 percent *cooperado* as proportion of total MMP physicians, with a standard deviation of 39.69. Appendix Table B.1 shows distribution of MMP physicians per state (UF) and region.

Table 1 Summary statistics: Comparison of pre- and post-treatment means

	Pre			Post		Δ	
	Control	Treat	Δ	Control	Treat	Control	Treat
Panel A: Dependent							
Infant Mortality	19.19	16.09	-3.10***	16.65	14.30	-2.54	-1.79
Low Birth Weight	80.44	78.19	-2.25***	81.91	79.66	1.47	1.47
Preterm Rate	88.58	87.41	-1.17	111.10	113.27	22.52	25.86
Consultations	6.10	5.88	-0.22***	6.32	6.10	0.22	0.22
Panel B: Covariates							
Ln Pop	8.94	9.87	0.93***	8.95	9.96	0.01	0.09
Ln GDP (per capita)	9.26	9.20	-0.06***	9.77	9.62	0.51	0.42
BHUs	3.24	2.59	-0.65***	3.55	2.90	0.31	0.31
BFP Exp	90.47	96.15	5.68***	153.16	180.02	62.69	83.87
Water Treat	68.79	66.66	-2.13***	71.58	68.39	2.79	1.73
Vaccination	81.56	80.16	-1.40***	87.44	80.09	5.88	-0.07
Mother Age	25.38	25.47	0.09	26.23	26.07	0.85	0.60
School	7.37	7.17	-0.20***	8.27	7.99	0.90	0.82

Notes: The table displays pre- and post-treatment means for the dependent variables and covariates used in the analysis. Covariates include the log of population, log of GDP per capita (in Brazilian Real), number of basic health units (BHUs) per 10,000 population, Bolsa Familia expenditure per 1,000 population, percentage of population covered by water treatment, vaccination coverage, average mother age, and average years of schooling completed by expecting mothers in the dataset. Pre- and post-treatment periods for the control group are defined as pre- and post-2013. The pre-period for the treatment group is defined as the years before MMP implementation. The post-period is defined as the years including and after MMP implementation. Column 'Δ' under 'Pre' displays the difference in pre-treatment means between the treatment and control groups. * $p < .10$, ** $p < .05$, *** $p < .01$ for a t-test on the unconditional difference in pre-treatment means between the treatment and control groups.

Table 2 Summary statistics: MMP physicians

	Mean	Std. Dev.	Min.	Max.
Total MMP Physicians	47.73	109.56	1	3,413
Brazilian Physicians	17.24	59.18	0	2,586
Cooperado Physicians	26.39	59.07	0	2,481
Foreign (non-Cuban) Physicians	4.11	20.96	0	859
Proportion Cooperado	61.78	39.69	0.00	100.00

Notes: The table displays summary statistics for the distribution of MMP physicians in treated municipalities in active treatment periods. Variables include the total number of MMP physicians present in a given active municipality-year, number of Brazilian, *cooperado*, and foreign (non-Cuban) MMP physicians present in a given active municipality-year, and the proportion of *cooperado* physicians as percentage of total MMP physicians in a given active municipality-year. It should be noted that the maximum values for number of MMP physicians are outliers.

4 Empirical Strategy

4.1 Empirical Strategy

Making use of the staggered geographic and temporal implementation of the Mais Médicos Program across Brazilian municipalities, I use a difference-in-difference framework with two-way fixed effects to examine the treatment effect of receiving MMP physicians on infant mortality rate, low birth weight rate, and preterm birth rate. Furthermore, making use of the variation in physician nationality across treated municipalities, I examine whether treatment effects differ in municipalities receiving Brazilian versus *cooperado* physicians. Treated municipalities are defined as municipalities that received at least one physician through the MMP program. Control municipalities are those that did not receive any MMP physicians during the study period.

A treatment dummy, $Active_{it}$, is created, taking the value of 1 for each year t when MMP physicians are present in municipality i and 0 otherwise. $Active_{it}$ is equal to 0 in all time periods for control municipalities. In the absence of simultaneous treatment implementation across all treated observations, $Active_{it}$ serves as the interaction between being in the treatment group and being in the post-period, allowing for dynamic entry into and exit out of the treatment group over the course of the study period. All models are estimated using municipality and year fixed effects, which control for any time-invariant municipal-level factors and time-varying factors that were common across municipalities.

In order to estimate the overall treatment effect of MMP, I first estimate the following model:

$$y_{it} = \gamma_i + \mu_t + \delta Active_{it} + \mathbf{Z}_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is the outcome variable of interest (infant mortality rate, low birth weight rate, or preterm rate) in municipality i at time t. $Active_{it}$ is a dummy variable indicating whether MMP physicians were present in municipality i in year t. γ_i and μ_t are municipality and year fixed effects, capturing any time-invariant and municipal-specific or time-varying and municipal-invariant factors, respectively. $\mathbf{Z}_{it}$ is a vector of time-variant municipal characteristics, including a standardized variable for total MMP physicians received, log of GDP per capita (in Brazilian Real), log of population, number of BHUs per 10,000 population, Bolsa Família Program expenditure per 1,000 population, water treatment coverage, vaccination coverage, and maternal characteristics, including average years of schooling and average mother age. All errors are clustered at the municipal level to account for serial correlation. δ is the parameter of interest.

In the second step of the estimation, I examine the existence of heterogeneous treatment effects across physician nationality. A dummy variable, $Cooperado_i$, is created, taking the value of 1 for all periods if municipality i received at least one *cooperado* physician during the study period, and 0 for all periods otherwise. The model in (1) is extended to include an interaction between $Active_{it}$ and $Cooperado_i$, allowing for the variation in slope of treatment effects in municipalities receiving *cooperado* physicians compared to municipalities receiving Brazilian physicians. To allow for variation in levels, a second variable is created indicating the proportion of *cooperado* physicians as percentage of total MMP physicians in municipality i in year t and included in the vector of covariates $\mathbf{Z}_{it}$. The following DD model is estimated[14]:

$$y_{it} = \gamma_i + \mu_t + \beta_1 Active_{it} + \beta_2 (Active_{it} \times Cooperado_i) + \mathbf{Z}_{it} + \epsilon_{it} \tag{2}$$

where $Cooperado_i$ is an indicator variable taking the value of 1 if municipality i received *cooperado* physicians. A variable indicating the proportion of *cooperados* as percentage of total MMP physicians is included among the regressors in $\mathbf{Z}_{it}$. The remaining variables are as described in equation (1). The coefficient β_1 on $Active_{it}$ estimates the average treatment effect of MMP in municipalities receiving solely Brazilian physicians, conditional on covariates. The coefficient β_2 on the interaction term between $Active_{it}$ and $Cooperado_i$ measures

[14]Note the term $Cooperado_i$ is omitted from the regression as it is constant within groups and thus differenced out by the fixed effects design.

the additional treatment effect in municipalities receiving *cooperado* as compared to Brazilian physicians (the total treatment effect in municipalities receiving *cooperado* physicians is given by the sum of β_1 and β_2), conditional on covariates. I first estimate the basic regression equations (1) and (2) for each outcome variable. The results are displayed in Table 6.

In a third step, I expand the estimations in (1) and (2) in two ways. Firstly, a main econometric concern is that secular trends in infant health outcomes vary differently across municipalities. Given Brazil's vast geographic area and high level of income inequality, it is likely that trends in infant health outcomes differ across regions. To control for this, I include state-specific linear time trends among the regressors in $\mathbf{Z}_{it}$ to allow for differential trends in the outcome variable[15] (states in Brazil are referred to as 'Unidades Federativas' and will henceforth be denoted 'UF'). Estimated treatment effects are then interpreted as deviations from pre-existing UF-specific trends. Secondly, in the MMP context it can also be expected for the treatment effect to vary with periods of exposure due to effects of improved maternal health on future pregnancy outcomes. In order to examine how treatment effects vary with time of exposure, I extend the model in (2) to allow for dynamic treatment effects across post-treatment periods:

$$\begin{aligned} y_{it} = \gamma_i + \mu_t + \beta_1 Active_{it} + \beta_2 (Active_{it} \times Cooperado_i) \\ + \sum_{m=1}^{M} Active_{i,t-m} \lambda_m + \mathbf{Z}_{it} + \epsilon_{it} \end{aligned} \tag{3}$$

where λ_m measures additional effects of active MMP occurring m periods after most recent MMP implementation[16], and all other variables are defined as in equation (2) with UF-specific linear time trends added among the regressors in $\mathbf{Z}_{it}$. The time period of reference is the period of implementation, and $\mathtt{m} \in \mathtt{[1,5]}$.

[15] I choose to include state-specific time trends rather than municipality-specific time trends to avoid exhausting degrees of freedom, over-fitting the model, and to ease computational capacity. The dataset is comprised of 4,920 municipalities spanning all 27 states and five major geographic regions.

[16] If municipalities move out of the treatment group in one period (i.e. received no MMP physicians) and re-enter at a later period, the indexing resets relative to the most recent treatment period.

4.2 Common Trends

In a randomized control trial (RCT), treatment exposure is independent of any factor that might affect the outcome of interest, allowing for a causal effect to be estimated. The Mais Médicos Program, however, was implemented over the span of five years in the dataset (from 2013 to 2017), with priority given to at-risk municipalities (as discussed in Section 1). In the absence of random assignment of treatment, the underlying assumption of the DD framework is then that variations from unmeasured confounders are either group-specific and time-invariant, or time-varying and group-invariant, which are differenced out by the two-way fixed effects design. Taken together, these assumptions imply that the trends in outcome variables differ by a fixed amount in each pre-treatment period. In other words, the underlying assumption of the DD framework is that treatment and control groups must exhibit common trends in the outcome variable in the pre-treatment period. Before formally testing the common trends assumption, I first visually inspect the trends in outcomes by plotting the outcome variables based on time period relative to program implementation[17], shown in Figure 1. A preliminary visual inspection suggests that the common trends assumption holds for at least infant mortality and preterm rate, while the trends in low birth weight rate appear more volatile.

One way to formally test the common trends assumption is to allow for leads and lags of the treatment variable to examine deviations in levels of the outcome variable in the treatment group compared to control group prior to policy implementation (Angrist and Pischke, 2009; Wing et al., 2018). Let $Treat_i$ be a dummy variable taking the value of 1 for all periods if municipality i received MMP physicians at any point during the study period, and 0 otherwise. Let s be a lead variable, indicating the number of periods prior to policy implementation, and m be a lag variable indicating the number of periods after implemen-

[17] I define the pre-treatment period for the control group as the years where no municipalities implemented treatment, i.e. before 2013, and the post-treatment period as the years after 2013. The treatment groups are indexed relative to year of program implementation. I present the relative time index within the `[-5,5]` interval.

tation. Interacting $Treat_i$ with each lead and lag variable allows for the examination of time-varying treatment effects relative to the effect in the same period on the control group. To examine the common trends assumption, the following equation is estimated for each outcome variable:

$$y_{it} = \gamma_i + \mu_t + \sum_{\tau=0}^{m} \beta_{-\tau} Treat_{i,t-\tau} + \sum_{\tau=1}^{s} \beta_{+\tau} Treat_{i,t+\tau} + \mathbf{Z}_{it} + \epsilon_{it} \tag{4}$$

where β_{-1}, β_{-2},..., β_{-m} allow for m lag periods (post-treatment effects) and β_{+1}, β_{+2} ,..., β_{+s} allow for s lead periods (anticipatory effects). The omitted period is the period of MMP implementation, $\tau = 0$. Figure 2 plots the $\beta_{-\tau}, \beta_{+\tau}$ coefficients and corresponding 95% confidence intervals from estimating equation (4) on each outcome variable. The full results of the estimations including $\beta_{-\tau}, \beta_{+\tau}$ coefficients, standard errors, and 95% confidence intervals are listed in Appendix Tables C.3 through C.5.

The common trends assumption requires that the coefficients on all leads be statistically indistinguishable from zero; $\beta_{+\tau} = 0 \; \forall \; \tau > 0$. In the estimations of infant mortality rate and low birth weight rate, the coefficients on the lead variables are not statistically different from zero. For preterm rate, the lower bound of the coefficient on the second lead period, β_{+2}, lies significantly above zero. The upper and lower bound estimates for the first and third lead periods, β_{+1} and β_{+3}, respectively, lie very close to zero. For infant mortality rate and low birth weight rate, the results of the event study support the common trends assumption. The event study results for preterm rate give noisier estimates and suggest deviations from common trends in at least the second pre-treatment period.

Given that some of the estimate bounds from the event study coefficients lie close to zero, I employ a second strategy to further probe the robustness of the common trends assumption. An alternative way to testing the common trends assumption is to examine the effect of including group-specific linear time trends in the estimation equation (Autor, 2003; Angrist and Pischke, 2009; Wing et al., 2018). To employ this approach, I include linear time trends at the state (UF) level in the estimation equation in (2). If the inclusion of UF-specific time trends substantially changes the coefficient estimates, this suggests that the introduction of MMP is correlated with other unobserved UF-level trends in the outcome variable, violating the common trends assumption. If the estimates remain unchanged, the policy treatment effect can be interpreted as independent of underlying UF-level trends. To assess this, I include a UF-specific linear time trend to the equation in (1):

$$y_{it} = \gamma_i + \mu_t + \delta Active_{it} + \beta_i(UF_i \times year) + \mathbf{Z}_{it} + \epsilon_{it} \tag{5}$$

and compare the treatment effect estimates to those from the unrestricted model in equation (1). The results of the estimations of equation (1) are presented in column (2) for each outcome variable in Table 5 and the results from the estimation of equation (5) are presented in column (1) of Table 6 for each outcome variable. The point estimates for all three outcome variables are robust to the addition of UF-trends in regards to both magnitude and significance, in support of the common trends assumption. The results are discussed in further detail in Section 5.

4.3 Assumptions

The common trends assumption is discussed at length above. Evidence presented in the event study of the outcome variables coupled with the robustness probing by including UF-specific time trends is supportive of the common trends assumption.

A second assumption of the DD framework is that of strict exogeneity. In order to determine a causal policy effect, implementation of MMP must be statistically independent from the outcome of interest. In other words, policy adoption in future periods must not be anticipated by outcomes in earlier periods, conditional on municipality and year fixed effects. Formally, the strict exogeneity assumption requires that

$$E[Y(j)_{it}|\gamma_i, \mu_t, Active_{i1}...Active_{iT}] = E[Y(j)_{it}|\gamma_i, \mu_t]$$

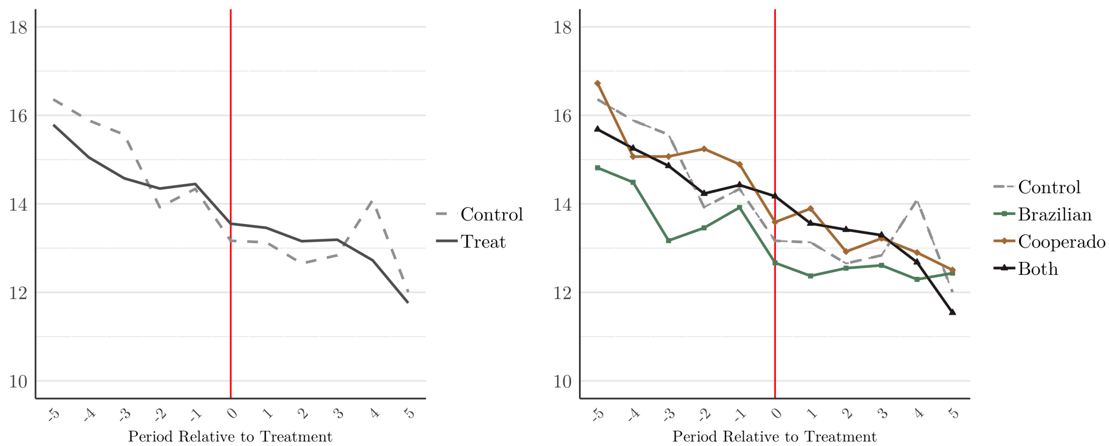
for $j = 0, 1$. If the adoption of MMP was influenced by the outcome variable, error terms of the model estimates of the treatment effect would be correlated and could lead to spurious estimates. However, this is only a concern if the correlation is due to something other than municipality or year fixed effects or the UF-specific time trends. As discussed in Section 1, eligibility of municipalities to receive physicians was determined by a set of demographic and socioeconomic criteria defined by the Brazilian Ministry of Health. Because adoption is correlated with secular trends in pre-existing conditions, this is taken care of by the municipality fixed-effects present in the difference-in-difference framework[18].

A third assumption for DD validity is that MMP implementation did not lead to composition changes among mothers in treatment and control municipalities. Covariate distribution across treatment and control groups can vary in levels but should remain stable over the study period (Wing et al., 2018). In order to examine the existence of composition changes, I estimate the equations (1) and (2) with the maternal characteristic covariate as the dependent

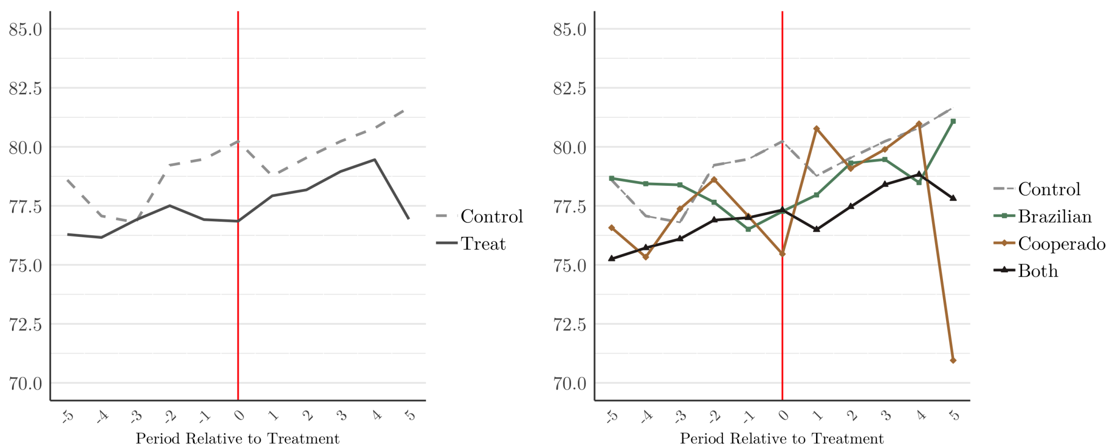
[18]Furthermore, Carrillo and Feres (2017), Fontes et al. (2018), and Mattos and Mazetto (2019) find that, as opposed to the intention of Ministry or Health, there is little correlation between policy adoption and underlying municipal characteristics aside from pre-MMP number of physicians. This assumption is further probed in Section 6.

Figure 1 Assumptions: Visual inspection of parallel trends

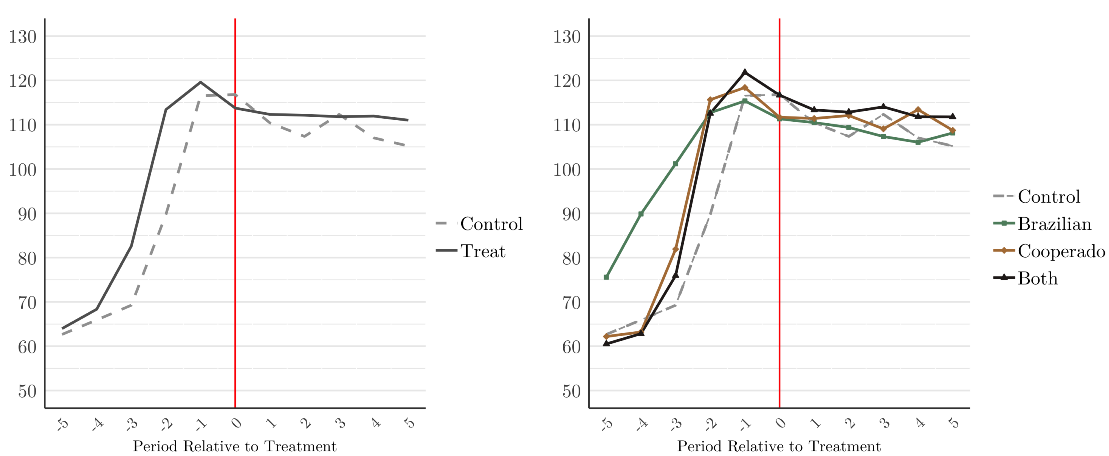
(a) Infant Mortality Rate



(b) Low Birth Weight Rate

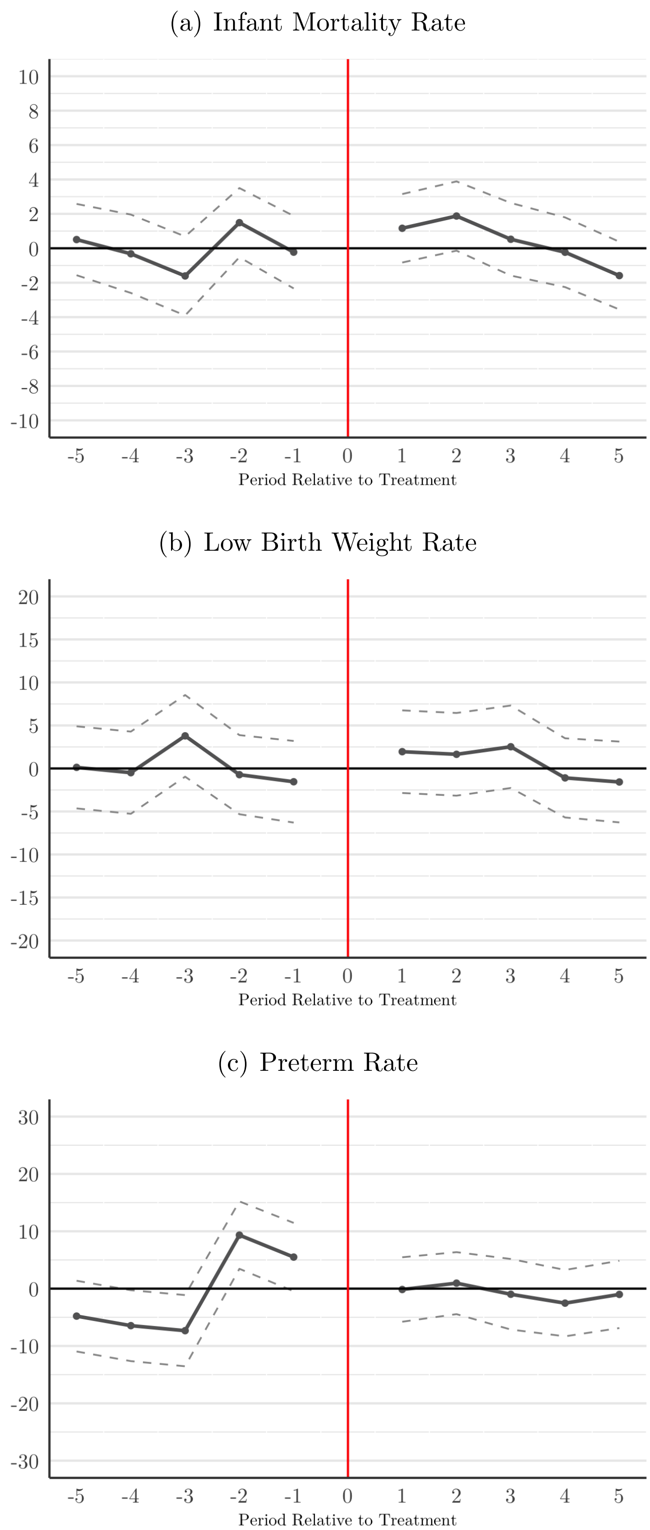


(c) Preterm Rate



Notes: Infant mortality, low birth weight, and preterm rates are calculated per 1,000 live births. X-axis displays period relative to treatment. Periods for the control group are indexed relative to 2013. The sample is restricted to the `[-5,5]` interval.

Figure 2 Assumptions: Event study of dependent variables



Notes: Y-axis displays $\beta_{-\tau}$, $\beta_{+\tau}$ coefficients when estimating equation (4) on the outcome variables of interest. Infant mortality, low birth weight, and preterm birth rates are calculated per 1,000 live births. X-axis displays periods relative to treatment. Periods for the control group are indexed relative to 2013. The sample is restricted to the `[-5,5]` interval. Dashed lines indicate the 95% confidence interval, using robust standard errors clustered at the municipal level.

variable regressed on the active treatment variable, $Active_{it}$, controlling for municipality and year fixed effects, UF-specific linear time trends, and the remainder of covariates included in Z_{it}. The results are presented in Table 3. If treatment exposure did not lead to changes in the distribution of covariates, it is expected that $\delta' = 0$. This is what I find. I find no significant effect of MMP on average mother age in either municipalities receiving Brazilian or *cooperado* physicians. While active MMP is estimated to increase years of schooling by an average of 0.040 years in the basic model, significant at the 5% level, this is negligible in magnitude. When estimating the interacted model for schooling, I find no significant treatment effect in either municipalities receiving Brazilian or *cooperado* physicians. Schooling is also estimated to decrease by 0.017 years for each standard deviation increase in total MMP physicians, but this is again negligible in magnitude (0.2 percent of the pre-treatment mean).

Another concern is the existence of omitted variables. The two-way fixed effects present within the DD framework will difference out any characteristics that are constant across municipalities or across time periods. The existence of factors that vary across both municipality and time dimensions and impact the outcome variables outside of the mechanism through MMP would lead to omitted variable bias of the estimates. It is possible that governments that adopt MMP also enact other health-oriented policies or spending which may bias the results. To control for this, I account for a range of time-varying municipal characteristics related to health policy, including log of population, log of per capita income, water treatment coverage, vaccination coverage, number of basic health units, and expenditures of the Bolsa Família Program per 1,000 population. The Bolsa Família Program is a large-scale conditional cash transfer (CCT) program, providing mothers with a monthly allowance conditional on children attending regular health screenings and meeting vaccination and school attendance targets. Through including Bolsa Família expenditure per 1,000 population in the vector of covariates, I assume this variable can also capture changing attitudes in municipal governments towards public health spending.

Lastly, a final concern in using the DD framework is serial correlation of the error terms.

Table 3 Assumptions: Difference-in-Differences on maternal characteristics

	Mother Age		**Schooling**	
	(1) Basic	(2) Interacted	(1) Basic	(2) Interacted
Active (β_1)	-0.049	-0.019	0.040**	-0.017
	(0.037)	(0.064)	(0.018)	(0.031)
Active × Cooperado (β_2)		0.054		0.032
		(0.075)		(0.038)
Proportion Cooperado		-0.001*		0.001
		(0.001)		(0.000)
Total MMP Physicians	0.007	0.005	-0.027***	-0.028***
	(0.008)	(0.008)	(0.005)	(0.006)
Covariates	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y
UF-Specific Time Trends	Y	Y	Y	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	0.345	-	0.243
P-Value F-Stat	-	0.557	-	0.622
Pre-Treatment Mean	25.504	25.504	7.235	7.235
Observations	43,257	43,257	43,257	43,257

Notes: The table displays the results of the estimations of equations (1) and (2) on maternal characteristics. All regressions include covariates (excluding the variable being estimated), year and municipality fixed effects, and UF-specific linear time trends. Robust standard errors, in parentheses, are clustered at municipal level. 'P-Value F-Stat' displays the p-value of an F-test that the sum of coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero.

* $p < .10$, ** $p < .05$, *** $p < .01$

Bertrand et al. (2004) find that model DD standard errors are severely understated when focusing on serially correlated outcomes. Infant health outcomes are suspected to be highly serially correlated across years, and model DD standard errors would fail to capture this correlation within each group. Failure to correct for this leads to an underestimation of standard errors and misleadingly narrow confidence intervals. In order to correct for this, I use robust standard errors clustered at the municipal level which account for heteroskedasticity across municipal clusters (Arellano, 1987; Bertrand et al., 2004)[19].

[19] A caveat of using cluster-robust standard errors is that the number of cluster groups must be sufficiently large. Bertrand et al. (2004) warn that statistical power of cluster-robust standard errors decreases as the number of cluster groups decreases. At N = 4,920 clusters, well above the encouraged sample size of 50, this is not expected to be an issue.

5 Results

The empirical results for the outcome variables of interest are presented in tables 4, 5, and 6 below. Table 4 presents the result of the estimations of the baseline and augmented models on prenatal consultations. Tables 5 and 6 present the results for the estimations for infant mortality rate, low birth weight rate, and preterm rate. Table 5 presents the results of the baseline estimations as specified in equation (1), without and with controls (columns (1) and (2) of each outcome variable), and the interacted model as specified in (2) without the inclusion of UF-specific linear time trends (column (3)). Table 6 presents the results of the augmented estimations: the basic model with inclusion of UF-specific time trends (column (1)), interacted model with UF-specific time trends (column (2)), and dynamic treatment effects model (column (3)). All models are estimated including year and municipality fixed effects and robust standard errors clustered at the municipal level. In the estimations of the interacted models, tables also present the F-statistic and p-value of an F-test that the sum of the treatment coefficient on *Active* and interaction term between *Active* and *Cooperado* is equal to zero. In an additional step, I examine the existence of potential 'selective mortality' effects that may bias the estimates, discussed below. The results for the selective mortality estimations are presented in Table 7. For simplicity, municipalities receiving Brazilian physicians will be abbreviated as 'MMPB' and municipalities receiving *cooperado* physicians will be abbreviated as 'MMPC'. The results presented in this section are discussed in further detail in Section 7.

5.1 Main Results

As a first stage, I begin by estimating the effect of MMP implementation on the average number of prenatal consultations attended by expecting mothers. The results are shown in Table 4. In the basic model (column (1)), active treatment is estimated to increase the number of prenatal consultations attended by expecting mothers by an average of 0.0265, significant at 1%, but negligible in magnitude. The coefficient on the active treatment variable is no longer significant when estimating the interacted models (columns (2) and (3)). In the dynamic effects model, I reject the null hypothesis that the overall average treatment effect in *cooperado*-receiving municipalities (the sum of β_1 and β_2) is equal to zero (at the 1% level). At a magnitude of 0.029, and increasing by 0.0003 for each additional percentage proportion of *cooperados*, this is negligible in magnitude. The coefficients on post-treatment periods in the dynamic effects estimation are all significant at at least the 5% level, but small in magnitude, with a maximum increase in consultations by 0.0880 in the fifth post-treatment period as compared to period of implementation (1.5 percent of the pre-treatment mean). Overall, the results suggest that any potential increases in prenatal consultations among the MMPB or MMPC subgroups are minimal in magnitude.

Secondly, I examine the impact of MMP implementation on infant mortality rate, low birth weight birth rate, and preterm birth rate. Table 5 shows the results of the estimations of the basic model (1) without controls in column (1) of each outcome variable and with controls in column (2). Column (3) for each outcome variable presents the results from the estimation of the interacted model (2), allowing for slope to differ across municipalities receiving *cooperado* physicians as compared to municipalities receiving Brazilian physicians. All models in Table 5 are estimated without the inclusion of UF-specific linear time trends. In the estimations of infant mortality rate, the point estimate on the active treatment variable is of the opposite sign than expected in all three specifications, though only significant (at 5%) in the basic specification with controls at a magnitude of 0.385. In the interacted specification of column (3), I find no significant treatment effect among either municipalities receiving Brazilian or *cooperado* physicians. Similarly, I find no significant estimate of active treatment on low birth weight rate in either MMPB or MMPC municipalities across the the three specifications. Low birth weight rate is estimated to decrease for each standard deviation increase in total MMP physicians, significant at 1% in all models but not jointly significant with the coefficient on *Active*. I find no significant difference in treatment effect on low birth weight between municipalities receiving Brazilian and *cooperado* physicians. In contrast, I find large and significant treatment effects on preterm rate across all three specifications. In the basic model with controls, active treatment is estimated to reduce preterm rate by 7.188 per 1,000 live births, on average, decreasing by an additional 2.014 per standard deviation increase in total MMP physicians. When allowing for heterogeneous treatment effects across physician nationality subgroups in the interacted model, active treatment is estimated to reduce preterm births by an average of 11.130 per 1,000 live births in MMPB municipalities, significant at 1%. The coefficient on the interaction term is insignificant, and I cannot conclude the existence of an additional treatment effect among MMPC municipalities compared to MMPB municipalities. Conversely, preterm rate is estimated to increase by an additional 0.044 for each one-percent increase in proportion of *cooperados* while decreasing by 2.017 for each standard deviation increase in total MMP physicians. These results suggest that municipalities receiving predominantly Brazilian physicians through MMP experienced greater reductions in preterm rate.

Table 6 presents the results of estimating the basic model (1) including UF-specific linear time trends (column (1) for each outcome variable), the interacted model (2) including UF-specific linear time trends (column (2)), and the dynamic effects model (3) (column (3)). The conclusions from Table 5 are unchanged. In the estimations of the basic model with UF-trends for infant mortality rate, the coefficient on *Active* remains robust in magnitude and significance as compared to the estimations from Table 5. The estimated coefficient on *Active* is no longer significant when adding treatment subgroup interactions and I find no significant treatment effect in either MMPB or MMPC municipalities. I find no significant treatment effects of MMP implementation on low birth weight rate among either MMPB or MMPC municipalities. For preterm rate, the point estimate on *Active* in the basic and interacted models are robust in significance and magnitude when including UF-trends in Table 6. Active MMP treatment is estimated to decrease preterm rate by 7.650 per 1,000 live births (8.5 percent of the pre-treatment mean) significant at 1%. When considering treatment heterogeneity, I find no significant additional treatment effect among municipalities receiving *cooperado* as compared to Brazilian physicians in the interacted model of column (2). When estimating the dynamic effects model in column (3), however, I find large and significant differences between treatment effects in MMPB and MMPC municipalities. The coefficient on *Active* increases in magnitude to -11.163 (12.4 percent of the pre-treatment mean) and the coefficient on the interaction term between *Active* and *Cooperado* changes sign and increases starkly in both magnitude and significance, from -1.080 to 8.368, now significant at 1%. The average treatment effect among MMPC municipalities in the period of MMP implementation is estimated at -2.795 (3.1 percent of the pre-treatment mean), significant at 5%. Preterm rate is estimated to decrease by an additional 0.508 for each standard deviation increase in total number of MMP physicians, significant at 5%. I also find large and significant estimates on all but the second post-treatment period, with preterm rate decreasing by between an additional 4.120 to 5.769 as compared to being in the period of MMP implementation. The results from the estimations presented in this section are discussed in further detail in Section 7 below.

Table 4 Results: Difference-in-differences on prenatal consultations

	(1) Basic	(2) Interacted	(3) Dynamic Effects
Active (β_1)	0.0265***	0.0035	-0.0153
	(0.0055)	(0.0093)	(0.0094)
Active × Cooperado (β_2)		0.0108	-0.0137
		(0.0125)	(0.0127)
Proportion Cooperado		0.0002**	0.0003***
		(0.0001)	(0.0001)
Total MMP Physicians	0.0001	-0.0002	-0.0081*
(standardized)	(0.0031)	(0.0031)	(0.0043)
Post-Treatment Period:			
First			0.0360***
			(0.0138)
Second			0.0573***
			(0.0141)
Third			0.0706***
			(0.0148)
Fourth			0.0711***
			(0.0154)
Fifth			0.0880***
			(0.0177)
Covariates	Y	Y	Y
Year and Municipality FE	Y	Y	Y
UF-Specific Trends	Y	Y	Y
Dynamic Treat Effects	N	N	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	2.091	8.921
P-Value F-Stat	-	0.148	0.003
Pre-Treatment Mean	5.918	5.918	5.918
Observations	43,451	43,451	43,451

Notes: Robust standard errors, in parentheses, are clustered at the municipal level. All models include a full set of covariates and year and municipality fixed effects. Model (1) is the basic model without interactions, (2) includes treatment type interactions, and (3) adds dynamic treatment effects. 'P-Value F-Stat' displays the p-value of an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 5 Results: Difference-in-differences on outcome variables (basic models)

	Infant Mortality Rate			**Low Birth Weight**			**Preterm Rate**		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	Basic, No Controls	Basic, Controls	Interacted	Basic, No Controls	Basic, Controls	Interacted	Basic, No Controls	Basic, Controls	Interacted
Active (β_1)	0.166	0.385**	0.389	-0.111	-0.218	-0.121	-5.432***	-7.188***	-11.130***
	(0.187)	(0.192)	(0.290)	(0.432)	(0.435)	(0.704)	(0.632)	(0.690)	(1.147)
Active × Cooperado (β_2)			-0.353			0.467			1.482
			(0.345)			(0.817)			(1.423)
Proportion Cooperado			0.005			-0.008			0.044***
			(0.003)			(0.007)			(0.012)
Total MMP Physicians	-0.505***	0.034	0.051	-0.801***	-0.395***	-0.419***	-2.995***	-2.014***	-2.017***
(standardized)	(0.142)	(0.036)	(0.036)	(0.188)	(0.114)	(0.118)	(0.642)	(0.485)	(0.494)
Covariates	N	Y	Y	N	Y	Y	N	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
UF-Specific Trends	N	N	N	N	N	N	N	N	N
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	-	0.016	-	-	0.277	-	-	73.262
P-Value F-Stat	-	-	0.900	-	-	0.599	-	-	0.000
Pre-Treatment Mean	16.267	16.267	16.267	78.551	78.551	78.551	91.508	91.508	91.508
Observations	55,780	43,451	43,451	55,780	43,451	43,451	55,780	43,451	43,451

Notes: Robust standard errors, in parentheses, are clustered at the municipal level. All models are estimated including year and municipality fixed effects. Estimation (1) of each outcome variable represents the basic model without interactions nor covariates. Estimation (2) includes covariates. Estimation (3) includes both covariates and treatment type interactions. 'P-Value F-Stat' displays the p-value of an F-test that the sum of coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 6 Results: Difference-in-differences on outcome variables (interacted models)

	Infant Mortality Rate			**Low Birth Weight Rate**			**Preterm Rate**		
	(1) Basic, UF Trends	(2) Interacted, UF Trends	(3) Dynamic Effects	(1) Basic, UF Trends	(2) Interacted, UF Trends	(3) Dynamic Effects	(1) Basic, UF Trends	(2) Interacted, UF Trends	(3) Dynamic Effects
Active (β_1)	0.387**	0.381	0.333	-0.272	-0.127	0.027	-7.650***	-9.933***	-11.163***
	(0.192)	(0.294)	(0.311)	(0.433)	(0.706)	(0.753)	(0.683)	(1.086)	(1.189)
Active $\times$ Cooperado (β_2)		-0.305	-0.243		0.142	0.121		-1.080	8.368***
		(0.348)	(0.349)		(0.825)	(0.835)		(1.365)	(1.377)
Proportion Cooperado		0.004	0.003		-0.004	-0.004		0.051***	-0.004
		(0.003)	(0.003)		(0.007)	(0.007)		(0.012)	(0.012)
Total MMP Physicians	0.033	0.045	0.044	-0.521***	-0.528***	-0.483***	-2.583***	-2.525***	-0.508**
(standardized)	(0.037)	(0.038)	(0.040)	(0.136)	(0.139)	(0.137)	(0.567)	(0.563)	(0.224)
Post-Treatment Period:									
First			0.764			2.529			-4.120*
			(0.768)			(1.782)			(2.259)
Second			1.636**			1.624			-0.943
			(0.798)			(1.864)			(2.253)
Third			0.099			2.635			-5.353**
			(0.843)			(1.931)			(2.544)
Fourth			-0.627			-1.630			-5.769**
			(0.833)			(1.821)			(2.353)
Fifth			-1.761**			-2.441			-5.353**
			(0.818)			(1.830)			(2.387)
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
UF-Specific Trends	Y	Y	Y	Y	Y	Y	Y	Y	Y
Dynamic Treat Effects	N	N	Y	N	N	Y	N	N	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	0.068	0.089	-	0.000	0.046	-	97.079	5.998
P-Value F-Stat	-	0.795	0.766	-	0.982	0.831	-	0.000	0.014
Pre-Treatment Mean	14.787	14.787	14.787	77.529	77.529	77.529	89.888	89.888	89.888
Observations	43,451	43,451	43,451	43,451	43,451	43,451	43,451	43,451	43,451

Notes: Robust standard errors, in parentheses, are clustered at the municipal level. All models include a full set of controls and year and municipality fixed effects. Estimation (1) of each outcome variable estimates the basic model including UF-specific linear time trends. Estimation (2) adds treatment nationality interactions. Estimation (3) allows for dynamic treatment effects. 'P-value F-Stat' displays the p-value of an F-test that the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' are jointly equal to zero. * $p < .10$, ** $p < .05$, *** $p < .01$

5.2 Selective Mortality

As noted by Carrillo and Feres (2017), the threat of selective mortality may bias the estimates of MMP treatment on infant health outcomes. Selective mortality would occur if MMP implementation led to a significant reduction in miscarriages and stillbirths, saving 'marginal' babies that are more likely to have poor health outcomes such as prematurity and low birth weight. The occurrence of selective mortality would impose a downward bias on infant mortality and an upward bias on proportion of low birth weight births and preterm births. If the introduction of MMP led to decreases in mortality of marginal infants, we would expect to see an increase in the number of live births and a decrease in the number of fetal deaths in treated municipalities. To check for selective mortality, I run the estimations of equations (1), (2), and (3) with fetal mortality rate (FMR) and live birth rate as the dependent variables. Fetal mortality rate is defined as the number of fetal deaths[20] per 1,000 potential births (fetal deaths plus live births). Live birth rate is defined as the number of live births per 10,000 population. The results are shown in Table 7.

I find a significant treatment effect of MMP on fetal mortality rate in the basic and interacted models (columns (1) and (2)), but in the opposite direction of what is expected under the hypothesis of selective mortality. In the basic model, MMP is estimated to increase FMR in treated municipalities by 0.357 fetal deaths per 1,000 potential births, significant at 5%. In the interacted model, the point estimate on *Active* increases slightly in magnitude to 0.480, significant at 10%. This estimate falls in magnitude and is no longer significant when adding dynamic treatment effects in column (3). I find no significant additional treatment effect among MMPC municipalities in either the interacted or dynamic effects specifications and fail to reject the null hypothesis of zero treatment effect among MMPC municipalities. Conversely, I find significant treatment effects on live birth rate in MMPB municipalities across all three specifications, but fluctuating in direction. In the interacted model, MMP implementation is estimated to decrease live birth rate by 1.210 births per 10,000 popula-

[20]Fetal death refers to the spontaneous intrauterine death of a fetus at any time during pregnancy.

tion in MMPB municipalities, significant at 10%. In the dynamic effects estimation, MMP implementation is estimated to increase birth rate by an average of 2.138, significant at 1%. I find significant estimates on the coefficients of the interaction term in the interacted specifications, but fail to reject the null hypothesis that the average treatment effect in MMPC municipalities is zero in both models. Interestingly, there is a large jump in birth rate in the fourth and fifth post-treatment periods, increasing by 4.368 and 25.270, respectively, as compared to being in the period of MMP implementation. Coupled with the estimated decrease in FMR in the fourth post-treatment, the increase in birth rate in the fourth and fifth post-treatment periods may suggest some degree of selective mortality effects, but considering the relatively large magnitude of the birth rate increase there are likely other predominating mechanisms.

Overall, the results of the selective mortality regressions show that while more babies are being born, there is little evidence to suggest it is due to marginal babies being 'saved'. On the contrary, the results present suggestive evidence for increases in fetal mortality rate among MMPB municipalities in the basic and interacted models. In interpreting these results, it is also important to consider self-reporting norms for fetal deaths. Women who experience early miscarriages may not report this to their local health clinic if it does not require a medical intervention. The increased presence of MMP physicians in underserved municipalities may have encouraged women to report miscarriages that would otherwise not have been recorded in the SIM database, and thus introduce an upward bias on FMR estimates. Further investigation into the mechanisms behind selective mortality is beyond the scope of this thesis.

Table 7 Results: Difference-in-difference on selective mortality

	Fetal Mortality Rate			**Live Births per 10,000 Pop**		
	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects
Active (β_1)	0.357**	0.480*	0.308	-3.533***	-1.210*	2.138***
	(0.173)	(0.286)	(0.304)	(0.413)	(0.710)	(0.711)
Active $\times$ Cooperado (β_2)		-0.384	0.043		2.179**	-2.629***
		(0.319)	(0.320)		(0.953)	(0.970)
Proportion Cooperado		0.003	-0.000		-0.066***	-0.028***
		(0.003)	(0.003)		(0.008)	(0.008)
Total MMP Physicians	-0.031	-0.016	0.012	0.510**	0.408*	0.207
(standardized)	(0.032)	(0.032)	(0.035)	(0.245)	(0.237)	(0.208)
Post-Treament Period:						
First			-0.653			-0.862
			(0.690)			(1.007)
Second			-0.996			-0.191
			(0.741)			(1.010)
Third			-0.100			0.771
			(0.734)			(1.140)
Fourth			-1.377*			4.368***
			(0.739)			(1.212)
Fifth			-0.907			25.270***
			(0.687)			(2.251)
Covariates	Y	Y	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y
UF-Specific Trends	Y	Y	Y	Y	Y	Y
Dynamic Treat Effects	N	N	Y	N	N	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	0.142	1.797	-	1.574	0.417
P-Value F-Stat	-	0.706	0.180	-	0.210	0.518
Pre-Treatment Mean	12.716	12.716	12.716	142.862	142.862	142.862
Observations	43,451	43,451	43,451	43,451	43,451	43,451

Notes: Robust standard errors, in parentheses, are clustered at the municipal level. All models are estimated using a full set of controls, year and municipality fixed effects, and UF-specific linear time trends. Fetal mortality rate is calculated as fetal deaths divided by possible births (fetal+live births). 'P-Value F-Stat' displays the p-value of an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero.
* $p < .10$, ** $p < .05$, *** $p < .01$

6 Robustness

In this section, a series of robustness checks are conducted relating to the assumptions of the empirical specification, as discussed in Section 4. I consider the matching of birth and death records to municipalities to mitigate migratory effects, weighting estimations by propensity scores from various matching methods, and controlling for the Zika epidemic that began in 2015. The results of the robustness estimations are shown in Appendix Tables D.6 through D.9.

6.1 Classification of Municipalities

As a first robustness check, I examine the matching of death and birth observations to corresponding municipalities to mitigate potential migratory effects. One of the underlying assumptions of the DD model is that there are no compositional changes in treated and control municipalities. This assumption would be violated if the arrival of a MMP physician to a nearby municipality encouraged mothers to travel outside of their municipality of residence for general health care, prenatal care, or delivery. Any improvements in birth outcomes in control municipalities may then be a reflection of the prenatal care received outside of the municipality of residence, which would lead to an underestimation of the treatment effect on the outcome variables. While the SINASC and SIM databases do not contain information on the location of prenatal visits, they do have information on the municipality of residence of the mother and municipality of the birth or death occurrence. To examine this assumption, I re-run the estimations restricting the sample to only those observations for which the municipality of residence and municipality of occurrence were the same, attempting to mitigate the possibility of migratory effects and compositional changes between the samples. The sample size decreases to 56% of all infant death observations and 71% of all live birth observations. The panel dataset decreases from 43,451 to 20,160 observations and from 4,920 to 2,414 municipalities. A visual comparison of the full sample and municipality-restricted sample is shown in Figure A.3, where values for the restricted sample are shown in blue. The municipality-restricted sample has lower levels and more volatility in the outcome variables (except infant mortality rate). The results of the robustness check are presented in Table D.6.

The results from Table 6 are robust to the stricter definition of treated municipalities. The point estimates from the infant mortality and low birth weight estimations increase in magnitude, but taking into account the increased standard errors, I cannot conclude they differ significantly from zero and the overall conclusions are unchanged. The coefficients from the estimations of preterm rate increase in magnitude compared to the unrestricted estimations, and the overall conclusions are unchanged. The point estimates on *Active* increases in magnitude from -11.163 to -14.383, significant at 1%, while the coefficient on the interaction between *Active* and *Cooperado* increases in magnitude from 8.368 to 9.003, significant at 5%. Moreover, preterm rate is estimated to decrease by an additional 1.355 for each one standard deviation increase in total MMP physicians, significant at 5%, as compared to a magnitude of 0.508 in the unrestricted model. The largest difference between the two samples is the increase in estimated treatment effects in post-treatment periods. The estimates on the third and fourth post-treatment periods increase by more than three times in magnitude, from -5.353 and -5.769 to -16.879 and -19.823, respectively. Together, the results from the municipality-restricted regressions indicate that mothers who did not travel to give birth experienced greater decreases in preterm rate as compared to mothers who gave birth outside of their municipality of residence. The results from the main regressions of Table 6 should thus be considered as lower bounds of the treatment effect estimates.

6.2 Propensity Score Matching

As a second robustness test, I use propensity score matching to account for systematic differences in baseline characteristics between treated and control municipalities. A second intention in employing propensity score matching is to control for the size imbalance between the treatment group (N=4,202) and control group (N=718). The ideal research environment for establishing causal relationships of a treatment intervention is a randomized control trial (RCT) setting. However, this is not the case in observational data, such as the MMP setting, where the likelihood of receiving treatment is associated with existing measured and unmeasured characteristics. As opposed to a RCT, this introduces the possibility of selection bias. A useful tool to control for this is propensity score matching (PSM). PSM predicts the probability of a unit receiving treatment, conditional on baseline characteristics, and matches treated units to similar untreated units. The use of PSM minimizes the possible biases arising from the distribution of observable characteristics that influence treatment assignment (Rosenbaum and Rubin, 1983). I use a logit model to estimate the propensity scores of municipalities participating in MMP conditional on the pre-2013 mean values of municipal characteristics. I then employ three matching methods to create matched pairs of treated and control municipalities and estimate equation (3) using propensity weights from the matched sample.

Treated and control municpalities are matched on pre-2013 mean values of the following municipal characteristics: log of population, log of GDP per capita, water treatment coverage, vaccination coverage, and Bolsa Família expenditure per 1,000 population. For robustness, I employ three matching techniques: nearest neighbor, kernel, and Mahalanobis distance matching[21]. 242 municipalities were dropped from the sample due to not having a control group match. Covariate bias distribution plots for each matching method are displayed in Figure A.4. The results from the weighted regressions are presented in Table D.7. The results from the main regressions are robust to all three methods of propensity score matching.

[21] Nearest neighbor matching selects the control observation with the smallest distance from the treatment group. Kernel matching uses weighted averages of all control groups to create matched pairs. Mahalanobis distance matching uses a distance metric based on multivariate distances. For an extensive discussion of different matching methods, see Caliendo and Kopeinig (2008).

6.3 Zika Epidemic

Lastly, an additional assumption of the estimation strategy is that the implementation of MMP did not coincide with changes in other health resources or exogenous shocks to health outcomes that affected treatment and control groups differently[22]. This is commonly referred to as the 'common shocks' assumption of multi-period DD analysis. A threat to this assumption is the onset of the Zika virus (ZIKV) epidemic in 2015, which spread rapidly across Brazil and infected over 200,000 people by 2016. ZIKV has been linked to severe birth defects among infants born to infected mothers, most notably microcephaly, a congenital abnormality resulting in defective brain development and an abnormally small head circumference (Teixeira et al., 2016). The Zika epidemic was concentrated in the Northeast region of Brazil, having over 40 percent of confirmed ZIKV cases and over 75 percent of microcephaly cases (Faria et al., 2017). The Northeast also received over 30 percent of all MMP physicians, more than any other region (see Appendix Table B.1). Because of the unequal spread of the virus across the country, any ZIKV-related effects that affect the outcome variables through various mechanisms would bias the estimates. If a majority of the ZIKV-affected municipalities were part of the treatment group, the MMP treatment estimates may be picking up ZIKV-related effects on the outcome variables rather than treatment effects.

While a strong link has been found between ZIKV-infected mothers and infants born with microcephaly, it is unclear how many infant deaths, low birthweight births, or preterm births the virus has caused. ZIKV-related fetus and infant deaths are believed to be underreported, as congenital anomaly deaths were generally not thoroughly investigated, and the exact mechanisms of ZIKV effects on fetal development are unknown (Oliveira et al., 2016; Teixeira et al., 2016). For example, Coelho et al. (2017) find suggestive evidence of ZIKV causing early miscarriages, which are unlikely to be reported in the SIM database. Moreover, it is also believed that ZIKV affected infant mortality rates through other social and economic

[22]The inclusion of UF-specific time trends partially controls for this by allowing variations in trends of the outcome variable across UFs. These trends are however not robust to exogenous shocks.

mechanisms, such as delaying family planning decisions (Lowe et al., 2018). Because the exact mechanisms of Zika's effect on birth outcomes is unknown and likely to be multi-faceted, I control for possible ZIKV-related effects that influence the outcome variables in two ways. First, I exclude all infant death observations with microcephaly as the recorded death cause. There were 649 recorded incidences of microcephaly during the study period (0.15 percent of total infant deaths in the dataset), of which 340 occurred in 2015 or later (0.23 percent). The results are presented in Table D.8. The results are virtually unchanged from those of the unrestricted model in Table 6.

Yet, excluding microcephaly deaths from the sample is unlikely to capture the full effect of Zika on infant health outcomes. To control for other potential mechanisms, I conduct a second robustness check, re-running the estimations excluding the nine states in the Northeast region (NE), which was disproportionately affected by ZIKV (Alagoas, Bahia, Ceará, Maranhão, Paraíba, Pernambuco, Piauí, Rio Grande do Norte, and Sergipe). The sample size decreases from 43,257 to 27,491 observations and from 4,920 to 3,245 municipalities. The results are presented in Table D.9. The results from the estimations of low birth weight and preterm rate are robust in magnitude and significance to those from the unrestricted sample presented in Table 6. The estimations for infant mortality rate differ slightly: in the NE-excluded sample, the estimated treatment effect of being in an active MMPB municipality increases in magnitude and significance. The estimated coefficients on *Active* in the interacted and dynamic effects models increase from 0.381 and 0.333 to 0.884 (significant at 5%) and 0.750 (significant at 1%), respectively. This suggests that including NE in the sample puts downward influence on IMR estimates, indicating that while Zika did not have an affect on increasing IMR disproportionately in treated NE municipalities, it may have affected infant mortality rates through other mechanisms, such as delaying family planning decisions. Further investigation into these mechanisms lies beyond the scope of this thesis and I conclude that the main results of the preterm rate estimations are robust to the exclusion of the Northeast region, while the results of the infant mortality estimations are inconclusive.

7 Discussion

The empirical results presented in this thesis find consistent evidence of large and significant effects of MMP implementation on reducing preterm birth rate. Overall MMP implementation is estimated to decrease preterm rate by at least 8.5 percent in participating municipalities. When considering heterogeneity across the treatment subgroups, MMP is estimated to decrease preterm rate by at least 12.4 percent in municipalities receiving Brazilian physicians and by at least 3.1 percent in municipalities receiving *cooperado* physicians, decreasing further with time of exposure. On the contrary, the analysis finds no evidence of MMP implementation having an effect on infant mortality rate or low birth weight rate. The findings of no significant MMP effect on infant mortality rate and low birth weight are in line with the findings of previous literature (Carrillo and Feres, 2017; Mattos and Mazetto, 2019). Conversely, the findings of a large and significant reduction in preterm births differs markedly. While the analysis in this thesis differs from Carrillo and Feres (2017) in that preterm birth is defined as gestational age under 37, as opposed to 38, weeks, this definition difference is not expected to account for the large observed difference in results. A potential explanation for the deviation from their findings is that the treatment effect identified in this analysis is driven by effects in later post-treatment periods which were not included in their study period, which ranged from 2008 to 2015. In the dynamic effects model, large and significant coefficients are estimated for the third, fourth, and fifth post-treatment period (`m=3`, `m=4`, and `m=5`). These periods correspond to years 2016, 2017, and 2018 for the majority of municipalities that joined MMP in 2013, suggesting that increased MMP exposure drives the results. Alternatively, there were an additional 173 municipalities that joined the program between 2016 and 2017 which would not have been included in the analysis of Carrillo and Feres (2017) and could be driving the observed treatment effect. To examine this possibility, I expand the model of equation (3) by allowing the slope to differ across late-treated municipalities. The estimation strategy is discussed further in Appendix E and results are presented in Table E.10. I find that later-joining MMPB municipalities experienced an additional decrease in preterm rate by an average of -5.829 as compared to earlier-joining MMPB municipalities. This suggests that the observed treatment effect in the initial results of Table 2 is at least in part driven by these later-joining municipalities that were not included in previous literature, including Carrillo and Feres (2017) and Mattos and Mazetto (2019).

The proposed mechanism through which MMP affects preterm rate is by improving maternal health in aspects related to preventative care before and during pregnancy. MMP physicians are primary care physicians, predominantly practicing preventative medicine and generally not practicing gynaecological or obstetric medicine. Given that the estimated effect of MMP on increasing prenatal consultation attendance is minimal, it can be ruled out that MMP affected preterm rate through encouraging women to seek more prenatal care. Thus, the mechanisms through which MMP physicians influence infant health outcomes are limited to mutable factors such as maternal behaviors and maternal and population health. There are numerous risk factors associated with preterm birth, including immutable factors such as genetic predispositions and cervical or uterine abnormalities (Dekker et al., 2012), as well as demographic, environmental, and socioeconomic factors and behavioral choices before and during pregnancy (Kramer, 1987; Goldenberg et al., 2008). Institute of Medicine (US) (2007) identifies a number of maternal behavioral risk factors associated with preterm births that are potentially mutable, including prenatal care usage, cigarette smoking, drug use, alcohol and caffeine consumption, and dietary intake. These factors are in line with what PCPs can be expected to influence. By providing guidance on healthier habits in remote areas with historically low dissemination of health care attendance and information, MMP doctors may be helping to improve not only general population health, but also reduce the risk of preterm births. This proposed mechanism is supported by the results in this thesis, indicating that increased time of exposure to the program led to further reductions in preterm births. Furthermore, Dole et al. (2003) find a positive association between pregnancy-related anxiety and preterm birth risk. The presence of MMP doctors may have helped to reduce anxiety regarding health care and thereby reduce anxiety-related spontaneous preterm labour. Additionally, Dole et al. (2003) find that history of previous preterm birth is the most important risk factor of preterm birth. The large observed decreases in preterm birth rate in the later years of MMP adoption could be due to amplified effects of later pregnancies. Finally, although most MMP physicians are not formally trained in gynaecological or obstetric medicine, they are likely able to identify and address common risk factors and early pregnancy complications, such as preeclampsia (high blood pressure in pregnancy), which has been linked to preterm births (Sibai, 2006).

While the results presented in this thesis show that MMP reduced preterm rate in participating municipalities, it is unclear whether these babies were healthier or at higher propensity to survive past the first year of life. While preterm birth is itself a risk factor for infant mortality, this thesis finds no subsequent decreases in infant mortality (or low birth weight rate) as a result of the decrease in preterm birth rate. An initial hypothesis is that the presence of MMP physicians was able to affect mutable factors that improved the quality of pregnancy, leading to decreased rates of preterm birth, but once born, environmental factors, unchanged by MMP, led to the same rates of infant mortality. Overall, the existing literature on preterm birth suggests that many of the well-established risk factors of preterm birth are immutable, and those that are not are multi-faceted. This thesis presents a starting point for further research to examine MMP physician impact on population health habits linked to pregnancy outcomes (such as smoking, alcohol and drug consumption) and anxiety levels, subject to data availability.

Secondly, this paper examines the existence of heterogeneous treatment effects on infant health outcomes across physician nationalities. An initial hypothesis is that municipalities receiving *cooperado* physicians experience greater improvements in infant health outcomes as compared to Brazilian physicians. There are a few reasons to expect this. First, as indicated by the failure of the initial hiring round of the program to attract adequate Brazilian physicians, many are unwilling to work in remote areas. On the other hand, *cooperados* voluntarily inscribe into the program. Moreover, many of the participating *cooperado* physicians had completed a medical mission before MMP and are more familiar with the working conditions. Second, the Brazilian health care model is focused on advanced medicine, whereas the Cuban health diplomacy model focuses on preventative care, which is better suited to address the health needs of rural populations. Instead, the results suggest that municipalities receiving *cooperado* physicians experienced smaller decreases in preterm rate as compared to municipalities receiving Brazilian physicians, differing by at least 9 percentage points. Given the political controversy surrounding Cuban involvement in MMP, a natural hypothesis may be that this is due to difference in perception of or trust in Cuban doctors, leading to underutilization of health care in MMPC municipalities. The same reasoning follows for concerns regarding cultural differences or language barriers. On the contrary, studies have found high patient perception ratings of *cooperados* physicians, especially among the more rural municipalities (Silva et al., 2017; Santos et al., 2019). A differing hypothesis is that the observed difference in magnitude of treatment effect may actually have been driven by reporting norms due to increased health care utilization. For example, *cooperado* presence in municipalities which previously experienced a lack of health care workers may encourage women to seek medical care for pregnancy complications, such as preterm labour, whereas they may not have previously. As MMPC municipalities had lower pre-MMP physician rates, on average, these effects are expected to be greater in MMPC municipalities. Although MMP is still estimated to decrease preterm rate in municipalities receiving *cooperados*, the aforementioned reporting effects may lead to these estimates being understated.

The analysis presented in this thesis may be subject to a number of limitations. First, the analysis in this thesis does not consider the possibility of substitution effects where the arrival of MMP physicians led to declines in the number of non-MMP PCPs. The literature on the extent of substition effects is mixed: Carrillo and Feres (2017) find an immediate and statistically significant increase in PCP supply by 17 percent in MMP municipalities whereas Hone et al. (2020) find that while MMP was associated with an overall increase of

5.7 PCPs per 100,000 population, this increase constituted a 15.1 percent increase in MMP physicians and a reduction of 9.4 percent in non-PMM physicians. Secondly, this thesis does not consider how MMP implementation affected reporting norms surrounding miscarriages or births. Subject to data availability, future research should examine the mechanisms of MMP effects on fetal and infant death reporting in the unified health system. Thirdly, detailed data on *cooperado* placements was received relatively late in the process. Due to time constraints, this thesis does not consider further variations along the intensive margin of *cooperado* physician proportion where stronger treatment effects may be observed. Lastly, heterogeneity testing may increase the risk of identifying false positives due to multiple inference problems. In order to limit these concerns, I restrict the analysis to consider only three infant health outcomes commonly addressed in health literature and only two subgroups, Brazilian and Cuban *cooperado* physicians.

8 Conclusion

This thesis estimates the effect of receiving physicians through the Mais Médicos Program on infant health outcomes and evaluates the existence of heterogeneous treatment effects across physician nationality. I consistently find a large and statistically significant effect of receiving MMP physicians on reducing preterm birth rate. This effect is estimated to be lower in municipalities receiving Cuban physicians than in municipalities receiving Brazilian physicians. In particular, the stark reductions in preterm rate appear to be driven by municipalities joining the program in later years, which were not encompassed by the study periods of previous literature. However, this thesis finds no statistically significant effect of MMP implementation on infant mortality rate and low birth weight rate. These results persist when examining the possibility of confounding effects due to selective mortality and are robust to a number of robustness and sensitivity tests, including a more restrictive classification of treated municipalities, weighting from various propensity score matching methods, and controlling for potential confounding effects of the Zika epidemic in 2015.

This study's findings point to a substantial success of the Mais Médicos Program in reducing preterm rate, which is of considerable relevance to policymakers evaluating PCP-oriented policies to overcome physician shortages and improve infant health. Although this thesis finds lesser reductions in preterm rate among municipalities receiving *cooperado* as compared to Brazilian physicians, the findings indicate a success of the program in achieving overall reductions in preterm rate by addressing PCP shortages through international cooperation. In the midst of political tensions between Cuba and Brazil, it is important to underscore that the evidence for differential treatment effects from receiving *cooperados* as part of Mais Médicos does not imply that the *cooperado* physicians, PAHO agreement, or Cuban medical diplomacy in its entirety are not valuable to improving other aspects of infant or population health not examined in this thesis. Further research is needed to investigate the existence of heterogeneous treatment effects across other dimensions of infant health, such as death causes, baby's sex, and other maternal characteristics.

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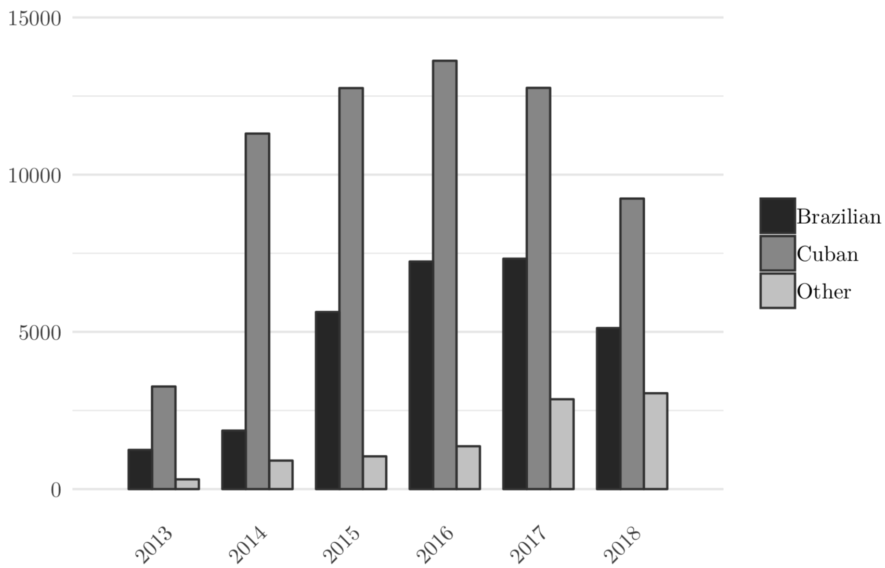
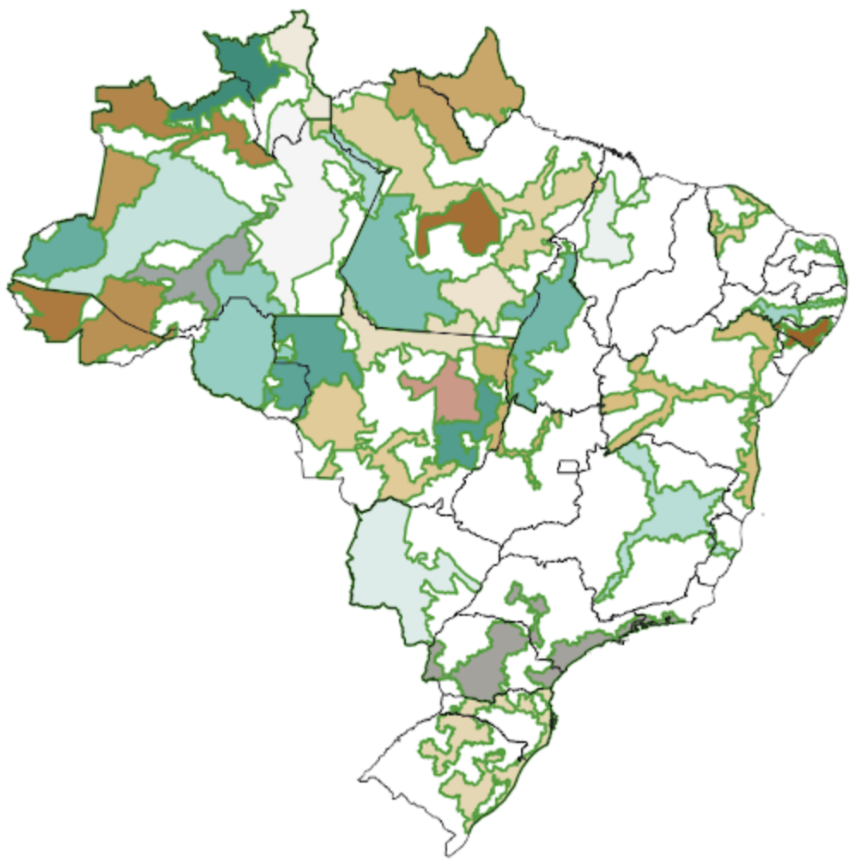
A Figures

Figure A.1 Summary statistics: MMP Physicians per year



Notes: Number of MMP physicians practicing per year, by nationality.

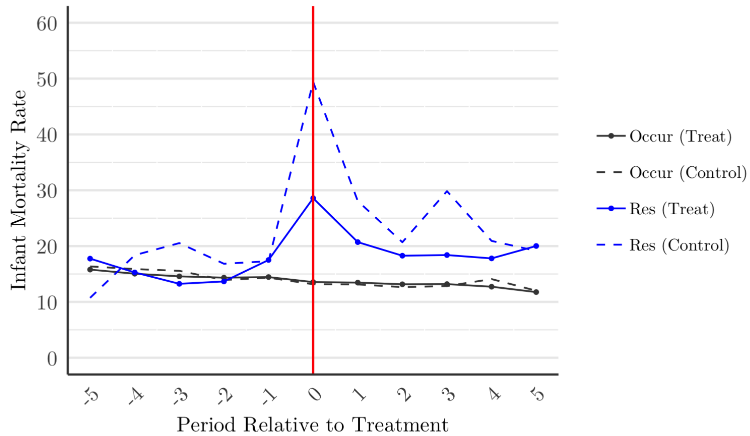
Figure A.2 Special Indigenous Health Districts (DSEIs)



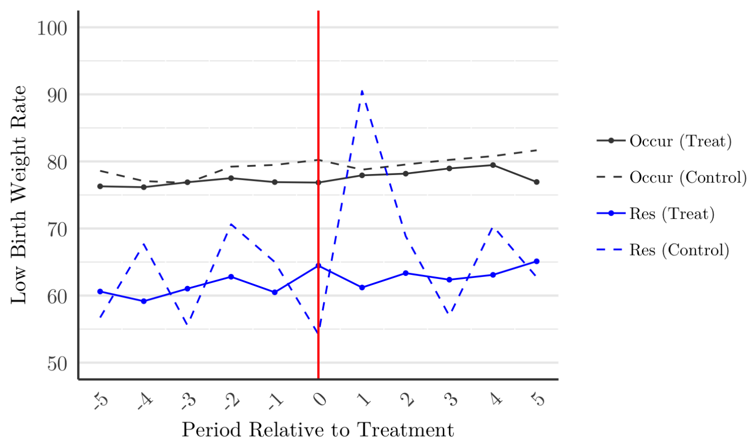
Source: Ministério da Saúde

Figure A.3 Robustness: Comparison between full and municipality-restricted sample

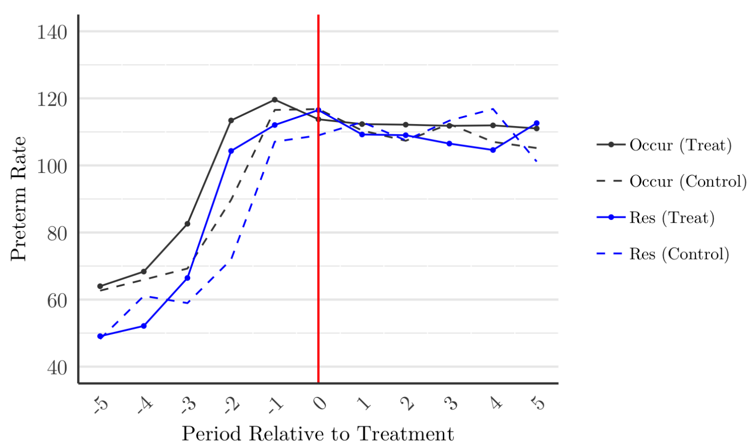
(a) Infant Mortality Rate



(b) Low Birth Weight Rate



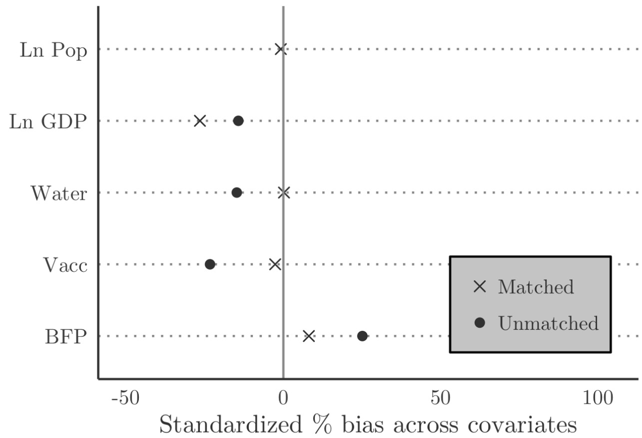
(c) Preterm Rate



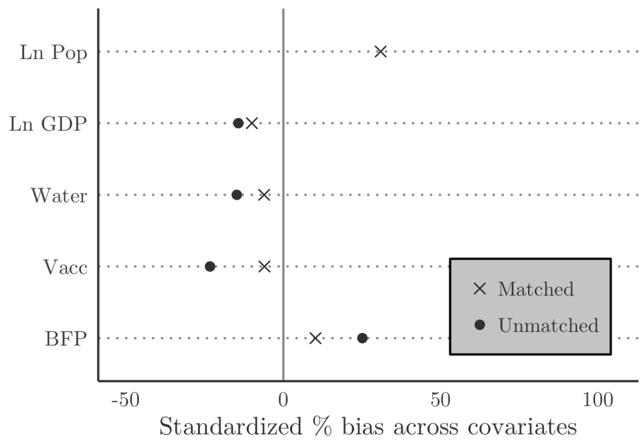
Notes: The plots present a comparison between the full sample and the restricted subsample consisting only of observations where the same municipality of residence of the mother and occurrence of the birth or death were the same. Rates are calculated per 1,000 live births. X-axis displays periods relative to treatment. Periods for the control group are indexed relative to 2013. The sample is restricted to the [-5,5] interval.

Figure A.4 Robustness: Propensity Score Matching covariate distributions

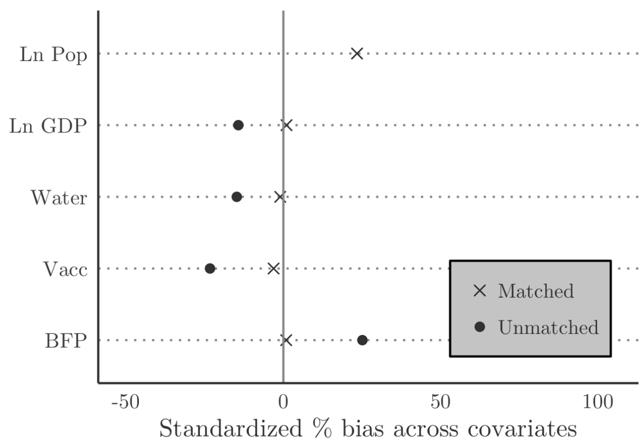
(a) Nearest Neighbor Matching



(b) Kernel Matching



(c) Mahalanobis Matching



Notes: Figures display percentage bias across covariates of various propensity score matching techniques. Figure (a) displays covariate distributions from nearest neighbor matching, figure (b) kernel matching, and figure (c) Mahalanobis matching.

B Additional Tables

Table B.1 Summary Statistics: MMP Physicians per UF

Region	Unidade Federal (UF)	MMP Physicians
North	Rondônia	664
	Acre	332
	Amazonas	1,089
	Roraima	293
	Pará	1,713
	Amapá	261
	Tocantins	365
	Total	4,717 (12.4%)
Northeast	Maranhão	1,662
	Piauí	819
	Ceará	2,997
	Rio Grande do Norte	726
	Paraíba	915
	Pernambuco	2,090
	Alagoas	555
	Sergipe	459
	Bahia	3,555
	Total	13,778 (36.3%)
Southeast	Minas Gerais	3,159
	Espírito Santo	958
	Rio de Janeiro	1,563
	São Paulo	5,121
	Total	10,801 (28.4%)
South	Paraná	2,076
	Santa Catarina	1,157
	Rio Grande do Sul	2,655
	Total	5,888 (15.5%)
Central-West	Mato Grosso do Sul	471
	Mato Grosso	525
	Goiás	1,479
	Distrito Federal	347
	Total	2,822 (7.4%)

Notes: The table displays the number of MMP physicians practicing across Brazil's five regions and 27 UFs from 2013 to 2018. Percentages display percentage of total MMP physicians practicing in Brazil.

Table B.2 Summary statistics: Comparison of pre-treatment means in MMPB and MMPC

	Only Brazilian	Only Cooperado	Δ
Panel A: Dependent Variables			
Infant Mortality Rate	15.46	17.69	2.23
Low Birth Weight	79.11	78.19	-0.92
Preterm Rate	94.60	91.46	-3.14
Consultations	6.00	5.94	-0.06
Panel B: Covariates			
Ln Pop	9.73	9.19	-0.54
Ln GDP (per capita)	9.31	9.27	-0.04
BHUs	2.98	2.71	-0.54
BFP Exp	114.10	102.02	-12.08
Water Treat	68.96	62.18	-6.78
Vaccination	81.77	82.58	0.81
Mother Age	25.68	25.44	-0.24
Schooling	7.36	7.12	-0.24
Observations	5,180	4,952	

Notes: The table compares the pre-treatment means of the dependent and covariates variables for municipalities receiving only Brazilian physicians and only *cooperado* physicians. Column 'Δ' displays the difference in means.

C Event Study

Table C.3 Event study estimates: Infant mortality rate

t	Coefficient	SE	CI Lower	CI Upper
-5	0.507	-1.057	-1.565	2.579
-4	-0.319	-1.162	-2.596	1.959
-3	-1.607	-1.171	-3.902	0.689
-2	1.490	-1.025	-0.519	3.500
-1	-0.225	-1.075	-2.333	1.882
0	-	-	-	-
1	1.163	-1.015	-0.827	3.152
2	1.875	-1.026	-0.137	3.887
3	0.525	-1.077	-1.586	2.636
4	-0.230	-1.033	-2.256	1.795
5	-1.588	-1.003	-3.555	0.379
N	43,451			

Notes: This table displays the coefficients from the estimation of equation (4) for the dependent variable infant mortality rate. t indexes the time periods relative to MMP adoption. Regressions are run including the full set of covariates. Robust standard errors are clustered at the municipal level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table C.4 Event study estimates: Low birth weight rate

t	Coefficient	SE	CI Lower	CI Upper
-5	0.127	-2.434	-4.644	4.899
-4	-0.492	-2.437	-5.271	4.286
-3	3.790	-2.420	-0.955	8.535
-2	-0.722	-2.345	-5.320	3.876
-1	-1.546	-2.420	-6.291	3.198
0	-	-	-	-
1	1.950	-2.450	-2.853	6.752
2	1.644	-2.451	-3.162	6.449
3	2.523	-2.445	-2.270	7.316
4	-1.095	-2.347	-5.697	3.506
5	-1.572	-2.396	-6.270	3.126
N	43,451			

Notes: This table displays the coefficients from the estimation of equation (4) for the dependent variable low birth weight rate. t indexes the time periods relative to MMP adoption. Regressions are run including the full set of covariates. Robust standard errors are clustered at the municipal level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table C.5 Event study estimates: Preterm rate

t	Coefficient	SE	CI Lower	CI Upper
-5	-4.793	-3.145	-10.960	1.374
-4	-6.456*	-3.152	-12.640	-0.276
-3	-7.331*	-3.166	-13.540	-1.124
-2	9.333**	-2.998	3.455	15.210
-1	5.498	-3.039	-0.460	11.460
0	-	-	-	-
1	-0.152	-2.867	-5.774	5.469
2	0.956	-2.758	-4.450	6.362
3	-0.982	-3.138	-7.135	5.171
4	-2.531	-2.952	-8.317	3.256
5	-1.009	-2.993	-6.876	4.858
N	43,451			

Notes: This table displays the coefficients from the estimation of equation (4) for the dependent variable preterm rate. t indexes the time periods relative to MMP adoption. Regressions are run including the full set of covariates. Robust standard errors are clustered at the municipal level.
* $p < .10$, ** $p < .05$, *** $p < .01$

D Robustness

Table D.6 Robustness: Municipality-restricted sample

	Infant Mortality Rate			Low Birth Weight Rate			Preterm Rate		
	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects
Active (β_1)	1.341	2.558	0.335	-1.729	-2.897	-2.716	-9.652***	-11.753***	-14.383***
	(1.621)	(3.145)	(3.096)	(1.246)	(2.229)	(2.321)	(1.732)	(3.372)	(3.478)
Active × Cooperado (β_2)		-3.264	-2.308		2.979	3.443		-2.620	9.003**
		(3.569)	(3.679)		(2.532)	(2.600)		(3.746)	(3.804)
Proportion Cooperado		0.025	0.012		-0.022	-0.023		0.070**	0.012
		(0.026)	(0.027)		(0.021)	(0.021)		(0.030)	(0.030)
Total MMP Physicians	-0.902*	-0.788	-1.339**	-1.014***	-1.119***	-0.919**	-4.564***	-4.477***	-1.355**
(standardized)	(0.498)	(0.482)	(0.533)	(0.375)	(0.397)	(0.382)	(1.162)	(1.163)	(0.632)
Post-Treatment Period:									
First			-7.393			-14.259			-18.625
			(10.906)			(11.989)			(11.644)
Second			-1.257			5.330			-3.725
			(7.500)			(6.025)			(8.143)
Third			-7.862			6.479			-16.879**
			(13.062)			(6.135)			(6.729)
Fourth			-9.719			-1.660			-19.823*
			(8.820)			(8.804)			(10.198)
Fifth			-0.878			-0.062			4.543
			(7.414)			(6.954)			(6.521)
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
UF-Specific Trends	Y	Y	Y	Y	Y	Y	Y	Y	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	0.074	0.667	-	0.002	0.134	-	27.261	3.571
P-Value F-Stat	-	0.785	0.414	-	0.967	0.714	-	0.000	0.059
Pre-Treatment Mean	16.402	16.402	16.402	62.084	62.084	62.084	72.839	72.839	72.839
Observations	20,160	20,160	20,160	20,160	20,160	20,160	20,160	20,160	20,160

Notes: This table presents the results from estimating equations (2) and (3) on the municipality-restricted sample where the municipality of residence of mother and occurrence of birth or death record were the same. Robust standard errors, in parentheses, are clustered at the municipal level. All models include a full set of controls, year and municipality fixed effects, and UF-specific linear time trends. Estimation (1) estimates the basic model. Estimation (2) adds treatment nationality interactions. Estimation (3) allows for dynamic treatment effects. 'P-Value F-Stat' displays the p-value of an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table D.7 Robustness: Propensity Score Matching

	Infant Mortality Rate			**Low Birth Weight Rate**			**Preterm Rate**		
	(1) Nearest Neighbor	(2) Kernel Matching	(3) Mahalanobis Matching	(1) Nearest Neighbor	(2) Kernel Matching	(3) Mahalanobis Matching	(1) Nearest Neighbor	(2) Kernel Matching	(3) Mahalanobis Matching
Active (β_1)	0.292	0.198	0.151	-0.251	-0.776	-0.584	-14.762***	-15.032***	-14.423***
	(0.394)	(0.376)	(0.398)	(0.982)	(0.911)	(0.979)	(1.550)	(1.523)	(1.590)
Active × Cooperado (β_2)	-0.285	-0.141	-0.075	0.310	0.425	0.379	9.532***	9.025***	8.871***
	(0.392)	(0.393)	(0.397)	(0.939)	(0.929)	(0.938)	(1.531)	(1.553)	(1.556)
Proportion Cooperado	0.005	0.005	0.004	-0.006	-0.005	-0.005	-0.003	0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.013)	(0.014)	(0.013)
Total MMP Physicians	0.436*	0.399	0.664***	-1.881***	-1.651***	-1.818**	0.659	0.711	0.973
(standardized)	(0.253)	(0.248)	(0.255)	(0.682)	(0.616)	(0.708)	(1.142)	(1.145)	(1.127)
Post-Treatment Period:									
First	0.212	0.480	0.810	2.169	4.763***	3.897*	-3.265	-2.882	-4.236
	(0.880)	(0.681)	(0.725)	(1.968)	(1.719)	(2.006)	(2.403)	(2.389)	(2.804)
Second	0.379	0.854	1.193	1.090	1.529	0.408	1.105	0.055	-1.037
	(0.894)	(0.730)	(0.903)	(1.901)	(1.663)	(1.963)	(2.101)	(2.213)	(2.191)
Third	0.131	0.511	0.256	2.158	2.705	4.132**	0.487	-2.020	-2.039
	(0.763)	(0.670)	(0.827)	(1.929)	(1.699)	(2.083)	(2.838)	(2.479)	(2.984)
Fourth	-1.128	-0.757	-0.689	-2.143	0.223	-1.604	-4.830*	-1.764	-4.804*
	(0.979)	(0.729)	(0.915)	(2.105)	(1.755)	(2.024)	(2.894)	(2.332)	(2.695)
Fifth	-1.076	-0.692	-0.493	-1.500	-0.375	-2.499	-1.968	-0.273	-2.139
	(0.777)	(0.699)	(0.796)	(1.754)	(1.705)	(1.780)	(3.425)	(2.677)	(3.001)
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
UF-Specific Trends	Y	Y	Y	Y	Y	Y	Y	Y	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	0.000	0.024	0.036	0.004	0.158	0.045	12.287	17.285	13.882
P-Value F-Stat	0.986	0.877	0.849	0.950	0.691	0.832	0.000	0.000	0.000
Pre-Treatment Mean	16.380	16.380	16.380	78.080	78.080	78.080	91.259	91.259	91.259
Observations	39,813	39,813	40,162	39,813	39,813	40,162	39,813	39,813	40,162

Notes: The table presents results from estimations of the interacted model of equation (3) using weights from propensity score matching with various matching methods. Robust standard errors, in parentheses, are clustered at the municipal level. All models include a full set of controls, year and municipality fixed effects, and UF-specific linear time trends. Estimation (1) for each outcome variable uses weights from nearest neighbor matching. Estimation (2) weights using kernel matching. Estimation (3) weights using Mahalanobis distance matching. 'P-value F-Stat' displays the p-value for an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table D.8 Robustness: Microcephaly-restricted sample

	Infant Mortality Rate			Low Birth Weight Rate			Preterm Rate		
	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects
Active (β_1)	0.388**	0.384	0.337	-0.272	-0.128	0.026	-7.652***	-9.937***	-11.163***
	(0.192)	(0.294)	(0.311)	(0.433)	(0.706)	(0.753)	(0.683)	(1.086)	(1.189)
Active $\times$ Cooperado (β_2)		-0.309	-0.246		0.143	0.121		-1.077	8.368***
		(0.348)	(0.349)		(0.825)	(0.835)		(1.365)	(1.378)
Proportion Cooperado		0.004	0.003		-0.004	-0.004		0.051***	-0.004
		(0.003)	(0.003)		(0.007)	(0.007)		(0.012)	(0.012)
Total MMP Physicians	0.039	0.052	0.051	-0.522***	-0.529***	-0.484***	-2.584***	-2.527***	-0.507**
(standardized)	(0.037)	(0.038)	(0.040)	(0.136)	(0.139)	(0.137)	(0.568)	(0.563)	(0.223)
Post-Treatment Period:									
First			0.763			2.530			-4.120*
			(0.768)			(1.782)			(2.259)
Second			1.634**			1.625			-0.943
			(0.798)			(1.864)			(2.253)
Third			0.097			2.636			-5.353**
			(0.843)			(1.931)			(2.544)
Fourth			-0.629			-1.629			-5.769**
			(0.833)			(1.821)			(2.353)
Fifth			-1.763**			-2.440			-5.353**
			(0.818)			(1.830)			(2.387)
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
UF-Specific Trends	Y	Y	Y	Y	Y	Y	Y	Y	Y
Dynamic Treat Effects	N	N	Y	N	N	Y	N	N	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	0.066	0.090	-	0.000	0.045	-	97.080	5.999
P-Value F-Stat	-	0.797	0.764	-	0.982	0.831	-	0.000	0.014
Pre-Treatment Mean	16.252	16.252	16.252	78.547	78.547	78.547	91.492	91.492	91.492
Observations	43,451	43,451	43,451	43,451	43,451	43,451	43,451	43,451	43,451

Notes: This table displays results from estimations on the sample excluding infant deaths due to microcephaly. Robust standard errors, in parentheses, are clustered at the municipal level. All models include a full set of controls, year and municipality fixed effects, and UF-specific linear time trends. Estimation (1) for each outcome variable estimates the basic model. Estimation (2) adds treatment nationality interactions. Estimation (3) allows for dynamic treatment effects. 'P-val F-test' is the p-value on a test that the sum of the coefficients on 'Active' and the interaction term with 'Active' and 'Cooperado' is equal to zero.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table D.9 Robustness: Excluding Northeast region

	Infant Mortality Rate			Low Birth Weight Rate			Preterm Rate		
	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects	(1) Basic	(2) Interacted	(3) Dynamic Effects
Active (β_1)	0.384	0.884**	0.750*	-0.700	-0.391	-0.404	-8.069***	-9.443***	-10.692***
	(0.248)	(0.392)	(0.415)	(0.556)	(0.920)	(0.971)	(0.854)	(1.335)	(1.476)
Active × Cooperado (β_2)		-0.494	-0.501		0.246	0.221		-0.396	7.584***
		(0.464)	(0.468)		(1.095)	(1.107)		(1.706)	(1.743)
Proportion Cooperado		-0.001	-0.002		-0.008	-0.007		0.026*	-0.017
		(0.004)	(0.004)		(0.009)	(0.009)		(0.015)	(0.015)
Total MMP Physicians	0.039	0.055	0.052	-0.523***	-0.530***	-0.459***	-1.841***	-1.832***	-0.286
(standardized)	(0.045)	(0.047)	(0.049)	(0.177)	(0.181)	(0.174)	(0.472)	(0.473)	(0.278)
Post-Treatment Period:									
First			1.349			3.675*			-5.833**
			(0.862)			(2.092)			(2.611)
Second			1.554*			2.336			0.133
			(0.909)			(2.177)			(2.586)
Third			0.258			4.248*			-5.555*
			(0.973)			(2.264)			(2.913)
Fourth			-0.037			-1.254			-4.450
			(0.906)			(2.113)			(2.708)
Fifth			-0.019			-0.796			-0.379
			(0.950)			(2.165)			(2.751)
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year and Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
UF-Specific Trends	Y	Y	Y	Y	Y	Y	Y	Y	Y
Dynamic Treat Effects	N	N	Y	N	N	Y	N	N	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	-	1.042	0.391	-	0.028	0.041	-	48.100	4.584
P-Value F-Stat	-	0.307	0.532	-	0.868	0.840	-	0.000	0.032
Pre-Treatment Mean	14.070	14.070	14.070	81.137	81.137	81.137	91.266	91.266	91.266
Observations	27,491	27,491	27,491	27,491	27,491	27,491	27,491	27,491	27,491

Notes: The table displays results from running estimations on a restricted subsample excluding observations from nine states in the Northeast region. Robust standard errors, in parentheses, are clustered at the municipal level. All models include a full set of controls, year and municipality fixed effects, and UF-specific linear time trends. Estimation (1) of each outcome variable estimates the basic model. Estimation (2) adds treatment nationality interactions. Estimation (3) adds dynamic treatment effects. 'P-Value F-test' is the p-value on an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero.
* $p < .10$, ** $p < .05$, *** $p < .01$

E Discussion

In order to examine the mechanism of the large observed treatment effect on preterm rate, I expand the estimation equation from (3) to allow the slope to differ across municipalities who joined MMP in later periods. I create a new dummy variable, $LateTreat_i$, taking the value of 1 if municipality i implemented MMP in either 2016 or 2017. I expand the equation by adding an an additional interaction between $Active_{it}$ and $LateTreat_i$ and a triple-interaction between $Active_{it}$, $LateTreat_i$, and $Cooperado_i$ to allow the slope to differ across late-joining municipalities and further across late-joining and *cooperado*-receiving municipalities. The estimation equation becomes[23]:

$$\begin{aligned} y_{it} = \gamma_i + \mu_t + \beta_1 Active_{it} + \beta_2(Active_{it} \times Cooperado_i) + \beta_3(Active_{it} \times LateTreat_i) \\ + \beta_4(Active_{it} \times Cooperado_i \times LateTreat_i) + \sum_{m=1}^{M} Active_{i,t-m}\lambda_m + \mathbf{Z}_{it} + \epsilon_{it} \end{aligned} \quad (6)$$

The overall average treatment effect of MMP among later-joining municipalities receiving Brazilian physicians is the sum of coefficients on *Active* and interaction between *Active* and *Late Treat* ($\beta_1 + \beta_3$). The overall average treatment effect of MMP on later-joining municipalities receiving *cooperado* physicians is the sum of coefficients on *Active*, *Active* $\times$ *Cooperado*, *Active* $\times$ *Late Treat*, and *Active* $\times$ *Cooperado* $\times$ *Late Treat* ($\beta_1 + \beta_2 + \beta_3 + \beta_4$). I conduct three F-tests testing the joint significance of the overall treatment effects on the aforementioned subgroups. The results are presented in Table E.10.

[23] Note the terms $Cooperado_i$, $Late_i$, and $Late_i \times Cooperado_i$ are omitted from the regression as they are constant within groups and thus differenced out by the fixed effects design.

Table E.10 Discussion: Municipalities treated between 2016 - 2017

	IMR (1)	**Low BW** (2)	**Preterm** (3)
Active (β_1)	0.449	0.286	-8.164***
	(0.305)	(0.736)	(1.131)
Active × Cooperado (β_2)	-0.345	0.056	-1.687
	(0.358)	(0.849)	(1.410)
Active × Late Treat (β_3)	-0.981	-1.855	-5.829**
	(0.851)	(1.963)	(2.824)
Active × Cooperado × Late Treat (β_4)	-1.159	-0.309	-0.567
	(1.411)	(3.493)	(6.055)
Proportion Cooperado	0.004	-0.005	0.051***
	(0.003)	(0.007)	(0.012)
Total MMP Physicians	0.028	-0.320***	-1.526***
(standardized)	(0.023)	(0.084)	(0.340)
Covariates	Y	Y	Y
Year and Municipality FE	Y	Y	Y
UF-Specific Trends	Y	Y	Y
F-Statistic ($\beta_1 + \beta_2 = 0$)	0.126	0.258	73.683
P-Value F-Stat (Cooperado)	0.723	0.611	0.000
F-Statistic ($\beta_1 + \beta_3 = 0$)	0.428	0.709	27.912
P-Value F-Stat (Late Treat)	0.513	0.400	0.000
F-Statistic ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$)	3.149	0.382	9.025
P-Value F-Stat (Coop & Late Treat)	0.076	0.537	0.003
Pre-Treatment Mean	14.962	79.496	89.739
Observations	43,257	43,257	43,257

Notes: Robust standard errors, in parentheses, are clustered at the municipal level. All models are estimated using a full set of controls and year and municipality fixed effects. 'P-Value F-Stat (Cooperado)' is the p-value on an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Cooperado' is equal to zero. 'P-Value F-Stat (Late Treat)' is the p-value on an F-test that the sum of the coefficients on 'Active' and interaction between 'Active' and 'Late Treat' is equal to zero. 'P-Value F-Stat (Coop & Late Treat)' is the p-value on an F-test that the sum of the coefficients on 'Active' and interactions between 'Active' and 'Cooperado', 'Active' and 'Late Treat', and 'Active' and 'Cooperado' and 'Late Treat' is equal to zero.
* $p < .10$, ** $p < .05$, *** $p < .01$