More Doctors, Better Health? Evidence from a Physician Distribution Policy*

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Abstract

In 2013, the Brazilian government implemented one of the largest physician distribution programs on record. Using a difference-in-difference framework, we document that the number of physicians increased by 17 percent in treated areas, with effects that are substantially larger in magnitude for family doctors. This expansion increased doctor visits by 4.3 percent and prenatal care by physicians by 10 percent. Yet despite these improvements in physician supply and utilization of doctors, we find little evidence that the program led to better infant health, measured by low birth weight, prematurity and infant mortality.

Keywords: primary care physicians; doctor utilization; infant health; policy evaluation

JEL Codes: I12, I18, I38

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1 Introduction

Improving infant health in a cost-effective way is a basic concern for policy makers. Much of the initiatives promoting access to medical care, including insurance expansions among children, are centered around the notion that greater utilization of medical care will improve the health of children.[1] Several scholars and international agencies have emphasized that the limited access to primary care physicians is a major threat to the provision of basic health care and the health of children in several regions. The World Bank has claimed that policy-makers in Africa should address physician shortages if the objective is to prevent morbidity and mortality in infants (Taylor and Dhillon 2011), while the World Health Organization states that maldistribution of these health professionals undermines the ability of many developing countries to achieve the Millennium Development Goals (WHO 2013; Taylor and Dhillon 2011). As a result, some governments have launched ambitious programs to address physician shortages across regions. The costs of these initiatives are high. In Brazil alone, the government has spent at least USD $2 billion in a physician distribution program between 2013 and 2016. Yet despite these policy efforts and assertions on the role of primary care physicians for infant health, we lack a rigorous empirical understanding of the extent to which increasing the supply of these health professionals can affect utilization of medical care and infant health. This paper is among the first to employ a quasi-experimental design to provide insights on this important question.

There are at least two reasons why simply increasing the number of physicians could fail to improve infant health. First, increases in physician supply do not guarantee more health care utilization or greater care efficiency. Individuals who have not traditionally seen a doctor or those less well informed about public programs may not automatically increase visits to doctor's offices, and these individuals may be those with poorer health outcomes and thereby who would benefit from better access to physicians. Furthermore, the utilization of medical care is not just a patient decision, but it depends also on physician outreach efforts. Hence, even if individuals seek more medical care, physicians may exert low effort and not treat all patients if the incentives to provide extra medical care are limited. In the United States, for example, it has been very well documented that physicians are less likely to treat publicly insured patients when reimbursement rates are low relative to private fees (Currie et al. 1995; Decker 1993). Second, even if children receive more care, the health returns to physicians may be limited, contrary to what is argued by many observers. For instance,

[1] In the United States, for example, the expansion of Medicaid coverage to pregnant women was motivated by the belief that it would lead to increases in birth weight and lower infant mortality rates (Currie et al. 1995)

some neonatologists are skeptical that prenatal care, either delivered by physicians or nurses, causally affects birth outcomes (Fiscella 1995; Sikorski et al. 1996), and convincing work in economics tends not to find strong evidence.[2] Moreover, many children already receive health care from nurses or other alternative sources, so increased supply of physicians will not lead to better infant health unless the quality of care is significantly higher in physician's offices.

In this paper, we exploit a policy that caused a sharp variation across sites in physician supply to estimate its effects on utilization of doctors and infant health. In 2013, the Brazilian government launched a major physician distribution program, the More Physicians Program (MPP), aimed at alleviating the shortage of primary care physicians. To attract newly trained physicians to remote and needy areas, the program provided a considerable remuneration and an increase in the scoring of medical residency exams. The program offered exceptional conditions for participation. For instance, a foreign physician can enroll in the program without proof of Portuguese proficiency. Participant physicians were placed in Basic Health Units (BHU), where families have free access to primary health care services such as prenatal care, vaccines, dressings, and medical consultations. Between 2013 and 2016, more than 18,000 physicians were enrolled in the program, a figure as large as 60 percent of all physicians in BHUs in 2012.

Our empirical strategy relies on the fact that the program was implemented in a limited set of municipalities. We use a difference-in-difference framework that compares the outcomes of treated and untreated municipalities before and after the implementation of MPP. The identifying assumption underlying this statistical approach is that the outcomes of treated and untreated areas would experience similar trends over time in the absence of MPP's implementation. Although MPP status is somewhat correlated with baseline characteristics, we show that the outcomes of interest of treated and untreated municipalities were similar before the MPP. We also provide other pieces of evidence supporting the identifying assumption.

While the program was successful in "recruiting" physicians, it is uncertain whether the program was effective in increasing the number of physicians serving in BHUs. Since Brazil operates under a decentralized scheme, governments at the municipality level have considerable autonomy to make decisions in the hiring and firing of public workers. A Federal law prohibits local governments from terminating the contracts of physicians enrolled in the program, but they retain discretion over physicians not linked to the program. If local administrations have incentives to substitute current physicians for MPP physicians, the

[2] For example, Evans and Lien (2005) find weak impacts for mothers whose prenatal care was disrupted by a bus strike in Pittsburgh, Goodman-Bacon (forthcoming) finds that Medicaid in the US does not increase birth weight (but reduces infant mortality via hospital care), and Hanratty (1996) finds something similar after the introduction of national health insurance in Canada.

program may be unable to increase the availability of primary care physicians. The popular press suggests that this is the case (see Jornal Nacional, March 4, 2017), but no study has rigorously investigated the extent to which the program affected physician supply.

Our first contribution, therefore, is to measure the relationship between MPP implementation and physicians. To do so, we compile detailed administrative records on the universe of physicians and construct panel data files at the municipality level before and after policy adoption. Using the difference-in-difference estimator, we document that treated municipalities experienced an unprecedented increase in the supply of physicians. The results indicate that program adoption led to an immediate and statistically significant increase of 0.11 in the number of physicians per 1,000 residents. Compared with the baseline rate of physicians of 0.67, this implies an increase of 17 percent. We show that this increase is largely driven by family doctors, which is consistent with the target of policy. Our estimates imply a 40 percent increase in the supply of family doctors in treated areas. To further place these results in perspective, it is worth noting that the introduction of the policy was able to eliminate the remarkable gap in primary care physicians between treated and untreated areas.

Having documented a strong and robust "first stage", we then study MPP's overall impacts on the utilization of medical care. We first show that MPP is robustly associated with doctor visits. Our estimates imply that MPP implementation increased doctor visits by 4 percent. When we explore heterogeneity in treatment effects with regard to patient's age, we find significant impacts across all age groups considered but the effects are of the largest magnitude for infants under one year of age. The implementation of MPP also raises the quantity of prenatal care received by physicians by 10 percent in treated areas. We also find some evidence that MPP implementation was accompanied by reductions in the rates of prenatal care delivered by nurses, suggesting that MPP led a shift in the provider of prenatal care delivery. Given the high fraction of prenatal care visits delivered by nurses (about 52 percent in our setting), it is perhaps unsurprising that the overall effect of the program on prenatal care visits tends to be statistically indistinguishable from zero and less robust across different specifications.

We then estimate the effects of the program on infant health, measured by infant mortality, low birth weight and prematurity. The focus on infants is compelling to study the health effects of MPP because the relation between cause and effect is likely to be immediate, relative to measures of adult health that may reflect health behaviors and care that occurred many years ago. Yet our analysis reveals very little evidence that the program led to gains in infant health. The effects on these outcomes can usually be bounded to a tight interval around zero. For instance, we can rule out effects of MPP on low birth weight smaller than 1 percent of a standard deviation. We continue to find virtually zero policy effects when stratifying the sample according to baby's sex, maternal characteristics and observable municipality characteristics. We also find no effects on infant mortality even when we examine different causes of deaths. Taken in their entirety, the findings of this paper provide little evidence that increasing the number of primary care physicians leads to improvements in infant health.

We argue that these results cannot be explained by specific features of the empirical strategy. A natural concern regarding the infant health analysis is selective mortality. If MPP implementation led to significant reductions in miscarriages and stillbirths, "saving" in part marginal babies that are more likely to have poor health outcomes, this will bias our estimates toward zero. However, we show that policy adoption is not associated with changes in fetal deaths, birth rates or sex ratios, casting doubt on the hypothesis of selective mortality.[3] We also show that MPP implementation is not associated with maternal characteristics, suggesting that changes in the composition of women giving births do not drive our results. We can also rule out changes in other health resources, as proxied by local hospital capacity. Our results are also robust to using a difference-in-difference strategy across matched pairs of municipalities or when the observations are reweighted either by weights that depend directly on the propensity score or distances to treatment observations. If anything, these different estimation techniques make our estimates less precise and thus the qualitative nature of our findings remains essentially the same.

Our analysis improves upon much of the literature linking physician supply and infant health. The vast majority of previous studies examining this question has relied on cross-sectional comparisons between region-specific physician numbers while controlling for observable socioeconomic and regional characteristics.[4] These studies find that a greater supply of primary care physicians, especially of family doctors, is significantly associated with lower infant mortality and reduced rates of low birth weight.[5] While often the best evidence available, these cross-sectional comparisons do not necessarily imply causation. If health professionals have strong preferences for working in more developed regions, where there are often higher quality parents and more health resources, then standard techniques that fail to account for this sorting may substantially exaggerate the health benefits of physicians.

Our work is closely related to two important contributions, Currie et al. (1995) and Iizuka

[3]A literature has documented that male fetuses are more vulnerable to in utero shocks than female fetuses (Almond and Mazumder 2011; Eriksson et al. 2010; Kraemer 2000). Hence, if there were substantial reductions in miscarriages and stillbirths associated to the MPP introduction, then we should observe a higher rate of male births during the post-intervention period in treated areas relative to the comparison group. We do not find any evidence towards this.

[4]For a review, see Starfield et al. (2005).

[5]For example, using data at the State level from the United States, Shi et al. (2004) show that this relationship holds after controlling for income and various socioeconomic characteristics.

and Watanabe (2016). Currie et al. (1995) explore the impacts of fees paid to physicians on infant mortality in the United States. They find that this policy is associated with lower infant mortality rates. Differently from the MPP, these authors focused on a policy that directly affected physician's incentives to provide extra medical care, which might have different implications than simply increasing the number of physicians in a given area. Iizuka and Watanabe (2016) provide estimates of the effect of a change in physician policy on infant health in Japan. They find that a large decline in the supply of hospital physicians is associated with poorer health outcomes. However, this result is somewhat difficult to interpret given that, as they show, the policy also led to significant reductions in other important health resources, including the number of hospitals open.

This paper also contributes to the debate on whether the government should be focusing on supply-side changes or intervening directly on the demand-side to improve health outcomes. Several global health policy reports encourage increasing the supply of physicians as a way to reduce inequalities in health, and the final goal of the MPP was similarly to increase the utilization of medical care and reduce the risk of poor health outcomes. Our findings indicate that although the program did increase substantially the supply of physicians and the utilization of doctors, there were no health improvements. In contrast, many previous studies conducted in a variety of countries convincingly show that demand-side changes, such as expanding health insurance, lead to gains in a number of infant health outcomes, including birth weight and infant mortality (Aizer 2007; Camacho and Conover 2013; Chou et al. 2014). Our results therefore suggest that interventions focusing on increasing only physician supply, at least as implemented, may not be the most cost-effective policies to improve infant health.

We should emphasize at this point that our findings do not imply that physicians are not important in determining the performance of a healthcare system. Access to physicians may have positive effects on measures of adult health - such as chronic diseases - that could require many years of regular visits to doctor's offices. In addition, this paper focuses on an intervention that affected the supply of primary care physicians and does not address the question of whether access to more specialized physicians are beneficial for infant health. For example, Currie et al. (1995) suggest that the effects of changes in Medicaid fees on infant mortality are driven by an increased participation of pediatricians and obstetrician/gynecologists in the Medicaid program.

The rest of the paper is organized as follows. Section 2 provides more information on MPP, while Section 3 introduces the data and our empirical strategy. Section 4 presents the main results and robustness tests. Finally, section 5 concludes.

2 Policy Background

2.1 Brazilian Health System

The creation of the current health system was gradual, beginning with recognition of healthcare as a citizen's right in the 1988 Brazilian constitution. Two years later, a series of laws created the Unified Health System (SUS, by its acronym in Portuguese), which provides free, universal access to preventive and curative care. A major innovation of the SUS was to decentralize health policy, where the different spheres of government (i.e., at the federal, state, and municipality level) have specific responsibilities in the provision of health services. The development and financing of national health policies is a responsibility at the federal level. In turn, municipalities are responsible for managing and providing primary health care services, while states provide technical and financial assistance. The creation of the SUS represented an unprecedented change in health policy. Prior to the SUS, only formal workers received health care provided by the Ministry of Health, while the other segments of population depended largely on philanthropic institutions and out-of-pocket expenses.[6]

The Basic Health Units (BHU) are public health centers through which the SUS provides accessible, affordable and primary health care. The goal of these centers is to provide health care services to individuals, without the need for referral to other services, such as hospitals. The main services offered are prenatal care, general medical consultations, inhalations, injections, curatives, vaccines, collection of laboratory exams, dental treatment, and provision of basic medication. In 2012, there was one BHU for every 5,000 individuals.

Physicians, nurses and assistant nurses are responsible for the provision of healthcare in the BHUs. A high fraction of physicians are clinician/general practitioners and family doctors. In 2012, clinicians and family doctors accounted for 27 and 40 percent of all physicians in BHUs, respectively. Gynecologists, pediatricians, and all other specialists represent respectively 6, 15 and 12 percent of BHU physicians.

2.2 The More Physician Program

Brazil is a developing country characterized by a highly unequal distribution of physicians. In 2012, the number of physicians per 1,000 residents was 1.6, but that figure was below 0.46 in 50 percent of municipalities, and 15 percent have a physician rate lower than 0.20. Only twenty percent of municipalities have a physician rate over 1, and five percent present a

[6]See Paim et al. (2017) for a detailed review of the history of the Brazilian health system.

physician rate over 2. The number of physicians per capita was the lowest in the poorer, less populous and more remote municipalities. To place these figures in perspective, the average physician rate across the OECD countries is 3.7.

To alleviate this imbalance in the distribution of physicians, the government implemented the More Physicians Program (MPP) in September 2013. Under this program, enrolled physicians receive a monthly salary of about USD 3,000 and an increase in the scoring of medical residency exams to provide primary health care services in needy areas for a period of three or more years. In addition, these health professionals receive housing and food benefits financed by the local governments. The BHUs function as the workplace of the recruited physicians. Typically, recent graduates are the physicians expected to enroll in the program, as long as they are able to exercise the medical profession as a general practitioner, as well as those who specialize in Family Health. Those physicians who do not have a family health degree are asked to complete such a specialization course financed by the program. The enrolled physicians must meet a weekly workload of 40 hours, with 32 hours reserved for activities in the BHUs of the municipality and 8 hours for completing the specialization course. A senior doctor is responsible for monitoring and supporting the program's physicians in a given region.

The MPP was implemented only in a set of municipalities. Although the pretreatment number of physicians in BHUs was a major criterion for eligibility, the Ministry of Health defined further target areas according to demographic and socioeconomic characteristics. Specifically, a municipality is considered priority if at least one of the following criteria is satisfied:

i) Extreme poverty rate over 20 percent;

ii) Being among the 100 municipalities with more than 80,000 inhabitants;

iii) Being located in the area of action of the Indigenous Special Sanitary District (ISSD);[7]

The Federal law 12,871/2013 allowed eligible municipalities to voluntarily join the program. The remuneration of the program's physicians is a responsibility at the Federal level, but local governments that choose to join the program are responsible for running the housing and food benefits for physicians. Program take-up was high, with the vast majority of eligible municipalities choosing to enroll. Out of all 5,570 Brazilian municipalities, the program was implemented in approximately 4,132. Appendix Table A.1 shows the fraction of eligible municipalities that joined the program according the priority criteria.

[7]The ISSD are federal sanitary units corresponding to one or more indigenous lands.

Having defined target areas, it establishes the maximum number of physicians that will be allowed to participate in the program based on the capacity of each municipality. A Law establishes an order of priority to select the physicians who will participate in the program. The participation is first offered to Brazilian and foreign physicians registered with the Regional Medical Council (CRM).[8] If vacancies remain after the choice of this first group, they will be offered to a second group, composed of Brazilian doctors trained abroad. The remaining vacancies are offered to a third group of foreign doctors trained outside the country.[9] If vacancies persist even after they are offered to these three groups, the Ministry of Health is allowed to make cooperation agreements with international organizations to fill the remaining positions.

Physicians registered with the CRM had priority in choosing the municipality in which they would carry out their responsibilities. In the case of foreign physicians who were trained abroad, the Ministry of Health chooses the municipality to which they would be allocated. Participation conditions were flexible for foreigners. They were not asked to take Portuguese proficiency examinations for participation.

Some physicians who were already working in a BHU in treated municipalities prior to policy may want to enroll in the program. However, since the goal of the program was to increase the number of physicians in the municipalities, those physicians were allowed to participate in the program only if they were willing to be allocated to a municipality with greater shortage of physicians. In any other case, these physicians are unable to participate in the program.

To deal with the misallocation of physicians in the long-run, the MPP aims to make investments for improving the infrastructure of the healthcare network. For that, the MPP seeks to modernize, expand and build new BHUs, with an estimated total cost of USD $1.3 billion. In the same vein, an additional strategy of the MPP is to create new undergraduate medical schools and new medical residency positions. With these strategies, the government seeks to guarantee an adequate annual number of newly graduated physicians for satisfying the demand for these health professionals.

The program was extremely successful in recruiting physicians. Between 2013 and 2016, more than 18,000 physicians were enrolled in the program, a figure as large as 60 percent of all physicians in BHUs during 2012. While this figure is suggestive, it is still unclear whether the goal of increasing the availability of BHU physicians was met. As hinted in the

[8]Every student who graduates from a Brazil school of medicine with the title of physician is allowed to register its diploma with the CRM to exercise the medical profession in the country. Students who graduate from foreign institutions are asked them to revalidate their diplomas in order to register with the CRM.

[9]In addition, physicians in the second and third order of priority cannot have been graduated or practiced medicine in countries with a number of physicians below 1.8 per 1,000 residents.

Introduction, the autonomy of local governments may jeopardize such a goal. In particular, some municipalities may have incentives to substitute current physicians with MPP physicians to increase the availability of resources for other purposes. The popular press suggests that this has been the case, arguing that the increase in the number of BHU physicians was much lower than expected. Indeed, between 2013 and 2016, the number of BHU physicians increased by 11,000, a number 38 percent lower than expected. Although suggestive, this aggregated figure may simply reflect the fact that some physicians who already were in a BHU prior to policy implementation decided to join the program. In addition, that figure may be not be very informative on the impacts of the program because the supply of physicians may have increased during this period independently of MPP.

2.3 Factors Associated with Program Adoption

As discussed above, the Ministry of Health defined eligible areas based on demographic and socioeconomic characteristics. If variation in adoption is systematically related to municipality characteristics that are associated with differential trends in the outcomes of interest, then this could lead to spurious estimates of the effects of the program. To explore this issue, we compiled a set of geographic and pretreatment socioeconomic characteristics of municipalities, which are described in more detail in Appendix Table A.2. We then use these predetermined characteristics to predict the probability that the municipality adopted the program by using probit and OLS regression models.

We present the results in Appendix Tables A.4-A.5. We find that MPP adoption is significantly associated with the number of physicians. On average, municipalities with lower pre-MPP physician rate are more likely to implement the program, which is consistent with the target of the policy. Program adoption was also more likely in the poorer and more populous municipalities. In addition, those municipalities that are part of legal Amazon region and municipalities with higher rural population share are also more likely to participate in the program. There is also a statistically significant positive association between local spending on Bolsa Familia and program implementation.[10] We also find that a large set of characteristics do not have a statistically significant effect on treatment probability, including for example indigenous population rate, unemployment rate, Gini index and all geographic characteristics.

Although these results suggest some significant effects of predetermined characteristics

[10]In particular, the Bolsa Familia program is a major social policy in which poor families receive a monthly cash transfer conditional on school attendance and health center visits. The monthly cash transfer from Bolsa Familia is equivalent to 40 percent of the monthly minimum wage. The Bolsa Familia program was launched in 2004, almost 10 years before MPP implementation.

on MPP adoption, the quantitative importance of each variable is small. For instance, a 20-percent increase in per capita GDP is associated with a decrease of 0.6 percentage points in the MPP adoption probability, which is very small relative to the mean adoption of 72 percent. Similar magnitudes are found for the other variables. More remarkably, these characteristics explain only 15 percent of the total variation in program adoption, leaving a substantial portion of variation unexplained. In addition, more than half of the explained variation is attributable to the pre-MPP supply of BHU physicians. This suggests that, conditional on this variable, much of the variation in MPP adoption appears to be idiosyncratic in practice, given the large set of characteristics we evaluated. While this is a strength for identification, we conservatively include pretreatment characteristics interacted with time trends in our difference-in-difference regressions to control for possible differential trends across municipalities that may be correlated with MPP effects.

3 Data and Estimation

3.1 Data

To investigate the effects of the program on the supply of physicians and patient care, we use administrative records from the Ministry of Health covering the period from 2008 to 2016.[11] We supplement these data files with information from Vital Statistics of Brazil, available for the 2008-2015 period, to analyze MPP's overall impacts on infant health.[12] The Ministry of Health managed all these data across different information systems with support of local and regional public health agencies. We make use of the municipality identifiers that are available in these data to construct panel data files of municipalities, the geographic level at which the policy was implemented.[13] We use bimonthly variation in our analysis because monthly data are noisy, particularly for infant health measures.[14,15] For each panel dataset of the outcome variable of interest, we exclude those municipalities with zero observations

[11]We do have information prior to 2008, but there is a series of issues that limits the use of these data. For example, patient care data often duplicate visits or aggregate multiple visits into an single one. Data on physicians are available from 2005, but they cover the entire country only from 2008 and onwards.

[12]The collection and preparation of vital statistics takes about two years, so we did not have any information regarding 2016 at the time of preparation of this manuscript.

[13]For the infant health analysis, we use the municipality in which the mother lives as reference for constructing the panel datasets.

[14]We use "bimonth" to refer to a two-month period.

[15]In addition, the use of bimonthly variation considerably reduces the computational burden.

during the complete study period.[16] We also obtained individual records on all physicians enrolled in the MPP, which contain information on the municipality in which each physician was placed and thus allows us to identify treated areas.

The data on physicians are obtained from the National System of Health Facility (CNES). The CNES records are a very rich source of data collected monthly that cover all health facilities in Brazil. They provide detailed information about physicians linked to some healthcare facility, including practice and levels of specialization. If a new health professional is incorporated into or leaves the workforce, the health facility is required to report this information in the following month in the system. Moreover, the system also compiles background information about the health facility, including type of health facility (e.g., BHU versus hospital), source of funding (public or private), and description of medical equipments. A major strength of these data is their universal nature and high frequency observations. For the purposes of this study, these features provide us with the ability to generate counts of the number of doctors for each municipality at given moments of time and identify the exact timing of any effect associated with the introduction of the MPP. Our main outcome of interest is the total number of physicians. Since the MPP focused on primary care physicians, one can interpret changes in the total number of physicians following the MPP implementation as being largely driven by changes in physicians serving in BHUs. It may be possible to observe changes in the supply of physicians in other public and private health facilities after MPP implementation as a result of job reallocation by local administrations. We take advantage of the large sample size (several hundred thousand physicians each month) to generate counts of doctors for precise physician groups, including classifications according to medical specialty and characteristics of the health facility where the physicians deliver their services. We consider five categories: gynecologists, clinicians, family physicians, pediatricians, and "other specialties".

To estimate the changes in patient care, we have obtained data on ambulatory visits for all patients from the National System of Information on Ambulatory Care (SIA) - approximately 200 million records. These files contain details on the date of the visit, patient's age, the medical care facility and health professional involved. All health facilities that provide low-complexity primary health services are required to provide this set of information to the Ministry of Health each month through the SIA. Our key outcomes are prenatal visits and doctor visits. Using information on the health professional involved, we analyze separately the effects of the program on prenatal care obtained from trained midwives/nurses (or simply

[16]In the vast majority of cases, this results in excluding less than 3 percent of municipalities. The only exception is the panel of physicians in private facilities, where 60 percent of municipalities has zero observations during the entire study period.

nurses) and physicians. Trained nurses play a major role in providing prenatal care - in our sample approximately 52 percent of this care is delivered by nurses. For the analysis of doctor visits, we also exploit information on patient's age to create counts of doctor visits for very precise age groups, including infants and older patients.[17] We use population counts from Brazil's census bureau to construct age-specific doctor visit rates.

Vital statistics records provide details on the universe of births and deaths occurring each year in Brazil as reported on birth and death certificates. The birth records have information on exact date of birth, weeks of gestation, baby's sex, birth weight and background information about the mother, including municipality of residence, age, education and marital status. The worksheet containing this information set is completed by the medical facility where the birth takes place using medical records. For home births, the information is collected in a notary's office at birth registration.[18] The death certificate microdata provide comprehensive information on date of death, cause of death, birth date, race and gender. For infants who die before age 1, some demographic characteristics of the mother are also provided (municipality of residence, education and age). The coding for cause of death follows the International Statistical Classification of Diseases and Related Health Problems 10th Revsion (ICD-10), created by the World Health Organization (WHO) (2010). The laws governing the collection of the death certificates are national and no burial can be performed without a death certificate. This dataset covers over 96 percent of all annual deaths inferred from demographic census.[19] Taken together, these data contain information on approximately 300,000 infant deaths and 24 million births.

We use three outcome variables to characterize the health effects of increased supply of primary care physicians. First, we consider infant mortality within one year of birth, an appealing measure of severe health problems and an outcome of direct interest for policy makers. To explore the relationship between MPP and infant death by cause, we group our sample into broad, mutually exclusive categories: infectious and parasitic diseases (4.7 percent), respiratory system diseases (5.2 percent), perinatal conditions (58 percent), congenital abnormalities (20 percent), and other diagnoses (12.1 percent). Second, like the most previous studies of infant health, we also explore the effects of the program on low birth weight (defined as birth weight less than 2500 grams) and prematurity (defined as gestation less than 38 weeks). These birth outcomes have been linked to infant mortality and a number of

[17]A natural extension would be analysing the effects of the program on doctor visits separately by gender, but unfortunately this information is not collected in the SIA.

[18]Although vital records for home births are likely to be noisy, it is unlikely to be a major issue given the low fraction of such births. In our period study, only 0.8 percent of babies were born at home.

[19]Information on death coverage from SIM are available at `http://tabnet.datasus.gov.br/cgi/sim/dados/cid10_indice.htm`.

health and developmental difficulties among babies who survive the infancy.[20]

Additionally, we have a rich set of municipality-specific characteristics. These include GDP, percentage of indigenous population, Gini index, unemployment rate, illiteracy rate, share of rural population, number of inhabitants, social spending, and a set of geographical controls. We include interactions between this set of characteristics and time trends in our regressions to control for possible differences in trends across municipalities that are correlated with the municipality treatment effect.[21] Appendix Table A.2 describes the sources of these variables.

3.2 Summary Statistics

The sample means of key variables in each of the datasets used in this study are shown in Appendix Table A.3. Physicians are measured per 1,000 residents. The average ratio of physicians is 0.67, with a standard deviation of 0.63. Remarkably, there is a striking difference in this outcome pre- versus post-intervention period. The average during the pre-MPP period is 0.62, while the average during the post-MPP period is 0.76. This relatively large increase seems to be driven largely by those municipalities implementing the program. Indeed, the pre-MPP and post-MPP difference in this ratio among untreated municipalities is approximately 0.05, while among treated areas the analogous figure is about 0.17 - a difference of 0.12. This certainly crude difference-in-difference is almost identical to the MPP effects that we below estimate more formally.

Appendix Figure A.1 illustrates graphically how the supply of physicians evolved over time in treated and control areas. Aside from the total physician rate, we also examine the trends separately for public and private physicians and for those serving in BHUs. The figures demonstrate two important features. First, prior to the implementation of the MPP, the number of public physicians was extremely similar between both groups, but there is a remarkable gap in favor of untreated municipalities when one considers only physicians in BHUs. Second, there is a dramatic and immediate increase in the total number of physicians after the introduction of the MPP in treated areas, implying that now the physician rate is higher in these areas relative to the control group. This relative increase is largely driven by a sharp increase in the number of physicians serving in BHUs, so that the aforementioned gap in the supply of primary care physicians has been virtually eliminated.

[20]Previous studies have shown, for example, that low birth weight is associated with health problems such as cerebral palsy, deafness, epilepsy, blindness, asthma, and lung disease (Brooks et al. 2001; Kaelber and Pugh 1969; Lucas et al. 1998; Matte et al. 2001). See Currie (2009) for a very comprehensive review of this literature.

[21]This strategy is essentially the same as that of Acemoglu et al. (2004).

The data also indicates differences in medical care utilization before and after the implementation of the program. Prior to the MPP, the number of visits to physician offices was 154 and 217 per 1,000 residents in treated and untreated areas, respectively - a difference of 63 or 30 percent. During the post-intervention period, the rate of doctor visits increased in treated and untreated areas, but the former group experienced a greater increase and thus the difference relative to control municipalities falls to 55. This suggests a difference-in-difference of 8 or 4.6 percent increase relative to the pre-MPP mean. The analogous figure for prenatal care utilization indicates a statistically insignificant increase of 2 percent. When we distinguish between the health professional who deliverers the service, we find a increase of about 7 percent in the utilization of prenatal care provided by doctors and a statistically significant reduction of 1.8 percent for prenatal care obtained from nurses. Aside from confirming these patterns, Appendix Figure A.2 suggests that the trends in treated and untreated municipalities were very similar prior to the MPP, providing some informal evidence that both groups would have experienced similar trends in the absence of the MPP. We below provide more formal evidence that the increase in doctor visits is unlikely to be the result of pre-existing differential trends.

Appendix Figure A.3 provides pictures of the relationship between MPP and infant health. As one can see from the figures, there is an overall downward trend in the rates of infant mortality. There is no a clear evidence that treated municipalities experienced a greater reductions in infant mortality compared to control areas. Indeed, infant mortality rates are very similar in both groups before and after the MPP. Figures also show a lack of clear patterns in the rates of low birth weight and preterm births over time. In particular, we do not see a trend break in treated municipalities relative to the control group after the implementation of the MPP. What is clear from these pictures is that the evolution of these outcomes was rather similar between both groups during the entire study period. In principle, there is no evidence that the MPP's implementation is associated with gains in infant health.

3.3 Estimation Strategy

To identify the relationship between MPP, physicians, patient care and infant health, we use the following specification:

$$y_{ibt} = \alpha + \beta Post_{ibt} \times Treatment_i + \gamma time \times Z_i + \eta_i + \mu_{bt} + \xi_{ibt} \tag{1}$$

where y is the dependent variable of interest for municipality i in bimonth b and year t. The independent variable of interest is the interaction of $Treatment_i$, which is an indicator variable for whether the municipality i adopted the program, and "Post", which denotes post-intervention observations starting September/October 2013. The covariates Z_i interacted with a linear time trend include a set of pre-intervention municipality characteristics (per capita GDP, log of population, illiteracy rate, share of indigenous population, Gini index, unemployment rate, share of rural population, municipality area, altitude, distance to capital, temperature, rainfall, an indicator for whether municipality is part of the legal Amazon region, an indicator for whether the municipality is part of the semiarid region, per capita spending on education, per capita spending on health, per capita spending on Bolsa Familia, and per capita physicians in BHUs). Some specifications include state linear time trends. When the dependent variable is an infant health outcome, we also control for maternal characteristics, including average age, the proportion of births by mothers with less than 4 years of education, and the proportion of births by unmarried mothers.

The models include municipality fixed effects (η_i), which absorb any unobservable time-invariant factors, including initial conditions and persistent municipality characteristics such as geography, transport infrastructure and area-specific risks of diseases. Year $\times$ bimonth fixed effects (μ_{bt}) control for common time trends such as seasonal fluctuations in infant outcomes (as documented by Buckles and Hungerman (2013)), macroeconomic conditions, and common national policies.[22] All our models use robust standard errors adjusted for clustering at the municipality level to account for serial correlation (Bertrand et al. 2004).

The coefficient β measures the effect of MPP on the outcomes of interest. The primary identifying assumption of our statistical approach is that in the absence of the MPP, municipalities in the treatment and control groups would have experienced the same trends in the outcome of interest. Although MPP adoption appears to be idiosyncratic in practice, treated and untreated areas may still be different in some dimension that could influence the outcomes of interest. Note that the inclusion of municipality and time fixed effects will strip out any time-invariant municipality-level factors and overall trends that might affect the outcomes. The identifying assumption would be violated only if there were differential trends in time-varying determinants of outcomes across treated and untreated areas. By including differential trends parameterized as functions of a number of municipality-specific baseline characteristics, we control for observable determinants of MPP adoption that might be associated with differential trends in the outcomes of interest. Further, the inclusion of state-specific linear time trends allows to account for differential changes across states in

[22]We also estimate models that include municipality-by-bimonth fixed effects and find very similar results. For the interested reader, these results are presented in Appendix Table A.20.

the factors affecting the outcome variables over the time period of analysis (some robustness specifications control rather for state × bimonth × year fixed effects). However, as we show below, our results are not sensitive to the inclusion of these controls. More importantly, we provide several pieces of evidence supporting the identifying assumption that there were no major differential trends across municipalities that are spuriously correlated with the treatment effect.

A disadvantage of the specification based on equation (1) is that it does not provide any insight into the timing of the program effects. To evaluate how the outcomes of interest evolved over the bimonths surrounding the introduction of the MPP and thus examine the timing of the effects, we also employ a flexible event-study design. To do so, we modify the regression equation above to include indicators for k bimonths before and after MPP adoption, interacted with the treatment group dummy. Our event-study specification is therefore:

$$\begin{aligned} y_{ibt} = \alpha + \sum_{k=-2}^{k=-K} \beta^k_{pre} \mathbf{1}[D_{bt} = -k] \times Treatment_i + \sum_{k=0}^{k=K} \beta^k_{post} \mathbf{1}[D_{bt} = k] \times Treatment_i \\ + \gamma time \times Z_i + \eta_i + \mu_{bt} + \xi_{ibt} \end{aligned} \tag{2}$$

where $\mathbf{1}[D_{bt} = .]$ is an indicator for k bimonths between MPP implementation and bimonth bt. The omitted category is -1. The bimonth zero is September/October 2013, when the policy was implemented. We estimate (2) for K bimonths before and after the initiation of the MPP. The rest of the variables are the same as in equation (1). Now the parameters of interest are β^k_{pre} and β^k_{post}, which represent the effects of the program relative to 1 bimonth prior to MPP before and after policy adoption. Thus, this specification allows us to test for differences in effects by length of time of exposure, providing a more detailed picture of the relationship between MPP and outcome variables. Moreover, it provides us with an opportunity to directly judge the validity of the difference-in-difference empirical design. If treated and untreated municipalities have similar trends before policy adoption, and diverge only after policy, it provides strong evidence that such changes were caused by the program rather than an unobservable factor. In this case, one would expect the coefficients β^k_{pre} to be statistically indistinguishable from zero.

4 Results

4.1 Effects of MPP on Physicians

We begin by examining graphically the relationship between policy adoption and the supply of physicians. Figure 1 shows the results from estimating the event-study specification of equation (2) for physician rates, plotting the respective coefficients and 95 percent confidence intervals. It reveals that, before the introduction of the program, there are no statically significant differential trends in physician rates. In the post-intervention period, there is a clear pattern indicating that the number of physicians increased much more rapidly in treated municipalities than in the comparison group. Moreover, the event-study shows that such differential increase occurred immediately after implementation, peaking at the bimonth 10, and persisting for the rest of the post-intervention period. The lack of a statistically significant pre-trend is consistent with the informal analysis in Section 3.2 and yields further support for the identifying assumption that the treatment and control municipalities would have experience similar changes in physician rates in the absence of MPP adoption.

Table 1 reports the estimates from equation (1), which confirms the graphical evidence. Column (1) is based on a specification that adjusts only for municipality and time fixed effects. The effect of MPP on the overall rate of physicians is positive and significant. The estimated coefficient implies that policy adoption resulted in a statically significant increase of 0.12 physicians per 1,000 residents. In general, the estimated relationship is qualitatively similar across different specifications, and always significant at less than 1 percent. The estimated coefficient is quite similar and somewhat smaller when we account for a set of interactions between linear time trends and basic pre-determined municipality characteristics, which now stands at 0.106 (standard error = 0.009). The inclusion of other differential trends, parameterized as functions of various observable baseline characteristics, and specific state linear time trends leaves the estimated coefficient of interest virtually identical. This remarkable stability across specifications further points to the robustness of the finding and provides very reassuring evidence that the results are unlikely to be driven by differential trends across treated and comparison municipalities.

The estimated coefficient from the specification that adjusts for the full set of controls is 0.116. Relative to the pre-MPP mean physician rate of 0.63, the effect is somewhat large at 18 percent. The rate of physicians in the treatment group increased by 0.14 per 1,000 over this period, so MPP is responsible for about 78 percent of this increase. There seem to have been other factors causing increases in the rates of physicians, but the bulk of the increases are the ones associated with the program.

In Table 2 and Figures 2 and 3, we examine the sources of the increase in physician rate. Columns (1)-(2) show the results from estimating the difference-in-difference specification for public and private physicians, respectively. Column (1) reports that the estimated coefficient for public physicians is strikingly identical to that of the overall rate of physicians. Now, relative to the pre-MPP mean of 0.58 public physicians per 1000, the magnitude of the effect increases to 20 percent. Column (2) shows that the policy had no discernible effects on the rates of private physicians, which is consistent with the corresponding event-study result shown in Figure 2. Collectively, these results suggest that there was no large spillovers in the physician workforce across the public and private sectors of the health system.

We also distinguish between BHU and non-BHU physicians. One could expect significant effects on physicians in health facilities other than non-BHUs if physicians who were already working in a BHU were reallocated to other health facilities after MPP implementation. Column (3) reveals a strong and statistically significant effect of the program on physicians in BHUs, with a estimated coefficient of 0.105 and standard error of 0.006. This estimate is only somewhat smaller compared to that for the overall physician rate, but given the high precision with the which the parameter is estimated, we reject the null hypothesis that both coefficients are the same. Figure 2, panel (c) also suggests that the overall effects in physician supply are largely driven by physicians in BHUs. Both the patterns and point estimates for this group of physicians are extremely similar to that of the overall physician rates. Indeed, the figure shows a marked divergence between treated and control municipalities after the introduction of the program indicating that the number of BHU physicians increased rapidly in municipalities implementing the MPP. The coefficients are very precisely estimated and thus in some cases we observe differences in pre-trends that are marginally significant. But these differences are very small and thus are not of the right order of magnitude to account for the increase observed just after the introduction of the program. In examining physicians in health facilities other than BHUs, we find that policy adoption is associated with a small increase in the number of these physicians (Figure 2, panel(d)), which may indicate some evidence for reallocation of physicians who were not linked to the program.

When considering the group of physicians in BHUs, we find effects that are substantial in magnitude. The estimated coefficient of 0.105 implies an increase of 50 percent in the number of physicians serving in BHUs relative to a pre-MPP mean of 0.21. To put this result in perspective, the mean difference in BHU physicians during the pre-intervention period between treated and comparison areas is 0.09 per 1,000 residents, suggesting that MPP introduction virtually closed the gap between both groups. As highlighted earlier, Appendix Figure A.1 provides visual evidence for this convergence process, showing that the rate of BHU physicians between treated and untreated becomes equal in the post-intervention period.

To uncover more detail about the relationship between MPP and the supply of physicians, we estimate equation (1) separately for physicians with different medical specialties (results shown in columns (5)-(9)). The results suggest that MPP led to increases in the rate of family physicians by about 0.075 (standard error = 0.004). The estimated effect is large in magnitude. Relative to the pre-MPP mean of 0.18, the results imply a 40 percent increase. The event-study graph in Figure 3, panel (a) shows substantial heterogeneities with respect to length of exposure. It reveals that the effect of the program on family doctors is increasing until the eighth bimonth and then remains about 0.11 per 1,000 (or 60 percent of the pre-MPP mean). We also find significant effects on clinicians. The significant positive coefficient of the interaction term implies that policy implementation led to an increase in the rate of clinicians of 0.022 per 1,000 (standard error=0.006). This represents an increase of 10 percent at the pre-MPP mean and thus the effect is of a magnitude much smaller than that on family doctors. Finally, columns (7)-(9) and the corresponding event-study graphs show a lack of correlation between MPP and pediatricians, gynecologist and all other types of physicians.[23]

In summary, the findings in this section suggest that the policy implementation led to a large and robust increase in the overall rate of physicians. These effects are largely driven by family doctors and clinicians in BHUs, with no evidence of any effect for other medical specialties or private physicians. We take these results as evidence of a strong "first stage" that MPP increased the supply of primary care physicians in treated areas.

4.2 Effects of MPP on Utilization of Care

After confirming that MPP led to a substantial increase in the supply of primary care physicians, we turn to the analysis of patient care. Figure 4 plots the coefficients and 95 percent confidence intervals from estimating the event-study specification of equation (2)

[23]There are a few coefficients that are statistically significant during the pre-intervention period for pediatricians. The significant pre-differences coincide with the changes in the Brazilian Classification of Occupations (CBO) in August 2011, which made data on pediatricians not completely comparable before and after this date (unlike other medical specialties). In particular, even when one accounts for these changes following the Brazilian Ministry of Labor's guidelines, some pediatricians are systematically classified into another medical specialty during the period before the implementation of this CBO. Indeed, the rates of pediatricians change discontinuously at the bimonth of implementation of the new CBO. However, when we check for pre-trends separately before and after the changes in CBO, we do not observe statistically significant differential trends in treated versus untreated areas in either of these periods. Moreover, our results are very similar when we exclude the period prior to August 2011 from the estimation sample. Specifically, we find a coefficient on the interaction term of -0.0032 (standard error=0.034) in this limited sample, which is virtually identical to the one reported in Column (8) of Table 2.

for the number of doctor visits per 1000 residents. In the pre-MPP period, it provides no evidence of a differential trend across treated and untreated areas. This suggests that our econometric approach accounts for the overall increase in the rates of doctor visits prior to MPP implementation (see Appendix Figure A.2). The estimates of the pre-MPP effects fluctuate randomly around zero and are never statistically significant. After the introduction of the MPP, doctor visit rates increased gradually. In the first four bimonths after the MPP was introduced, doctor visit rates was 9 visits per 1,000 higher for treated areas, or a 5-percent increase relative to the pre-MPP mean of 176 visits per 1,000. In the subsequent 4 bimonths, that increase was 4 per 1,000 or 8 percent relative to the pre-MPP mean, an effect that persisted for the rest of the post-intervention period. The estimates of the effects of the program are in general statistically different from zero during the entire post-MPP period. We also estimate the event-study specification for different age groups: 0-1, 1-5, 5-15, 15-40, 40-60, and 60+ years. Qualitatively, the results for each age group separately indicate a statistically significant increase in doctor visits in treated ares durgin the post-MPP period, replicating the pattern found for the overall rate of doctor visits (Figure 5).

Figure 6 shows the results from estimating the event-study specification for prenatal care. Panel (a) reveals no visual evidence of an increase in prenatal visits associated with MPP. Indeed, this outcome evolved similarly in treated and untreated areas both before and after policy implementation. However, when we distinguish between prenatal care delivered by nurses and physicians, we find substantial heterogeneities. Panel (b) documents that, while trends in prenatal care visits delivered by physicians prior to MPP are statistically the same across treated and untreated municipalities, they diverge during the post-intervention period. By the fifth bimonth from policy adoption, the number of prenatal visits delivered by doctors significantly increased in treated areas relative to the comparison group, an effect that persisted in general for the rest of the post-intervention period. Panel (c) explores prenatal care delivered by nurses and shows evidence for a decline that is significant at twelfth bimonth and onwards from policy adoption. This relative fall may explain why the effects of the program on the overall rate of prenatal visits is statistically indistinguishable from zero. There appears to be a shift in the provider of prenatal care delivery from nurses to physicians.

Table 3 presents regression results. They indicate that MPP implementation increased doctor visits by 7.6 per 1,000 in treated areas. Compared with the baseline mean, this suggests an increase of 4.4 percent. The results are qualitatively similar across different specifications and always statistically significant at less than 1 percent level of confidence. Remarkably, once one controls for municipality and time fixed effects and some basic controls, the inclusion of additional interactions between baseline municipality characteristics and linear time trends and state-specific time trends has very little effect on the estimated coefficient of interest, providing additional support for a causal interpretation of these results.

We next estimate the changes in doctor visits by patient's age. For ease of comparison, we report the pre-MPP mean for each age group. These results are shown in Table 4. Qualitatively, the results separately for each age group replicate the patterns found before, with estimates that are very precisely estimated and thus statistically significant at conventional levels of significance. The estimated coefficient is larger for individuals over 60 years than for any other age groups, but the baseline pre-MPP mean is high and thus the magnitude of the effect is relatively smaller. In contrast, we find the effects of the largest magnitude for infants under one year of age (10 percent), followed by children who are between 1 and 5 years of age (7 percent), and by individuals aged 15-40 (6.8 percent). Overall, results indicate that visits of children were more responsive to the changes in physician supply than older age groups.

Table 5 presents the results for prenatal care visits. Consistent with the graphical evidence, panel (a) shows no robust evidence that MPP is associated with higher use of prenatal care, irrespective of the specification considered. In the specification that incorporates the full set of controls, the coefficient of interest is estimated as 0.114 (with a standard error of 0.443), which is very small relative to the baseline mean (at less than 0.5 percent). By contrast, panel (b) reveals an statistically significant increase in the number of prenatal care visits by physicians. The results from the baseline specification imply that, on average, the number of prenatal care visits by these health professionals increased by 0.75 per 1,000 (with a standard error of 0.23) and the inclusion of other controls hardly affects this estimate. Compared with the pre-MPP mean, this indicates that MPP raised prenatal care obtained from doctors by approximately 8 percent. Conversely, panel (c) shows that the number of prenatal visits by nurses declined in treated areas during the post-intervention period, although this effect is only marginally significant in the complete specification.

Overall, these patterns indicate that the increase in the supply of physicians was accompanied by an increase in doctor visits and prenatal care by physicians. One means of interpreting the size of the estimated effects is to combine them with the results on physician supply to calculate the implied elasticity. The results from the specification that incorporates the full set of controls indicate that MPP is associated with a 0.115 increase in physician rate, a 7.6 increase in doctor visit rates, and a 0.75 increase in prenatal care visits delivered by physicians. This implies that a 1-percent increase in physician supply as a result of MPP is associated with 0.26 and 0.48 percent increases in doctor visits and prenatal care visits from physicians, respectively. We should emphasize that, while the identifying assumption of the difference-in-difference is sufficient to estimate the impact of the program, a causal interpretation of these elasticities requires that the program had no effects on the measures of health care utilization other than by increasing the supply of physicians. Perhaps the most serious concern for this interpretation is that MPP might have affected both quality and the quantity of physicians willing to provide primary health care and that changes in visits capture both effects. While physicians enrolled in the program are younger and have less experience than the average physician in general, they are more similar in these dimensions to those family doctors who typically provide primary care services in BHUs. Although this is suggestive of limited changes in physician quality, we cannot completely rule out the possibility that physician quality played a role in driving these elasticities.

4.3 Effects of MPP on Infant Health

We now examine the effects of policy on infant health, namely low birth weight, prematurity and infant mortality. Figures 7 plots the coefficients and 95 percent confidence intervals from event-studies that include indicators for 13 bimonths before and after of policy adoption, interacted with a indicator for "treatment". The omitted category is the bimonth 1 prior to policy. The figures reveal that during the pretreatment period, the trends in all infant health outcomes we considered were in general similar between treated and untreated areas. Moreover, the vast majority of estimated coefficients are small in magnitude. Yet the event-studies show no evidence for a change in the trends between treated and untreated municipalities during the post-intervention period, suggesting that policy adoption did not affect infant health outcomes.

Table 6 presents the corresponding difference-in-difference results from estimating equation (1). We find no statistically significant effects of the program on any of these infant health measures, confirming the graphical evidence. In addition, the estimated coefficients are very small in magnitude. For instance, the estimated coefficient of interest for prematurity is 0.0004, relative to a pre-MPP of 0.11. Importantly, note that these results are not driven by large standard errors. Indeed, our estimates suggest policy effects on these outcomes that can be bounded to a tight interval around zero. For example, we can rule out effects of MPP on low birth weight smaller than 1 percent of a standard deviation.

One might argue that these null effects mask important forms of heterogeneities. To explore this issue, Table 7 shows the results from running the regressions separately for different subgroups based on baby's sex and maternal characteristics. The results separately by gender do not reveal any evidence for significant effects of policy on infant health. We also stratify the sample by mother's education (low and high education), mother's age (< 20 yrs.) and marital status (unmarried and married). Across all these subsamples, we continue to find extremely small estimates tightly bound around zero.

Table 8 examines mortality results by cause of death. When we group causes of death into broad categories, we find only a marginally statistically significant effect of policy for infectious and parasitic diseases. The difference-in-difference estimate implies that MPP introduction reduced infant mortality rates in this category by 0.09 deaths per 1,000 births. However, this result appears to be driven by an observation during the pre-MPP period in which this cause of death was atypically high in untreated areas. Once this noisy observation is removed from the data, the estimated coefficient of interest falls substantially such that it is now -0.03 (standard error=0.056) and thus far from being statistically significant. The corresponding event-studies in Appendix Figure A.4 make clear the lack of a statistically significant correlation between MPP adoption and infant mortality by cause of death.

Appendix Table A.6 assumes that MPP was implemented in 2014 and allows the effects to vary over time. Again, there is no evidence that policy leads to better infant health. Point estimates are small and not statistically significant at the conventional levels of significance. One could argue that noise in bimonthly observations affects the precision with which the parameters of interest are estimated and thus the ability to detect significant effects. To examine this possibility, Appendix Table A.7 presents the results of estimating the effects of policy using data aggregated at the municipality-by-year level. This exercise assumes that the program was implemented in 2014 and allows the effects to vary over time. All estimates continue to be indistinguishable from zero, suggesting no evidence for an effect of MPP on infant health.

Overall, the results suggest that the health returns to the program are, at best, small. Next, we assess whether our results may be driven by selective mortality, an issue that emerges in any infant health analysis.[24] This could arise in our setting if policy adoption led to significant reductions in miscarriages and stillbirths, "saving" in part marginal babies that are more likely to have poor health outcomes. Ignoring this will likely lead to an underestimate of the true effect of policy on infant health. We examine this issue directly in Appendix Table A.8. Column (1) explores policy effects on fetal death rate, which is calculated dividing fetal deaths by the number of potential births (births plus fetal deaths). Column (2) considers the number of fetal deaths per 1,000 residents. Irrespective of how the dependent variable is measured, we find no evidence that policy led to reduced fetal deaths. Given this result, it is unsurprising that we find a statistically insignificant effect of policy on an expanded measure of infant mortality that considers fetal deaths (column (3)).

While very informative, this exercise is imperfect because official data on fetal deaths do not adequately capture spontaneous abortions that occur during the first weeks after

[24]See, for example, Currie (2009).

conception (Casterline 1989; Nepomnaschy et al. 2006). Column (4) looks at the number of live births per 1,000 residents. If the introduction of the program led to substantial reductions in miscarriages and stillbirths, then we may observe a higher number of live births during the post-intervention period in treated areas relative to the comparison group. We do not find any evidence that MPP is significantly associated with changes in birth rates. A caveat to the analysis of both fetal death and birth rates is that at least in theory greater access to primary care services may have affected conceptions through changing the distribution of family planning technologies. In this case, our analysis might provide limited information on the presence of selective mortality. To further check for fetal selection, we examine whether policy adoption had significant effects on the sex ratio at birth. Consistent with the literature on fragile males, if policy leads to lower fetal deaths, then we would expect to see increases in the relative number of male births (Almond and Mazumder 2011; Eriksson et al. 2010; Kraemer 2000). Columns (5)-(6) indicate no effect of policy on sex ratio at birth or the percentage of male births. Consistent with regression results, the corresponding event-study graphs in Appendix Figure A.5 shows that our results are unlikely to be driven by substantial reductions in miscarriages and stillbirths.

Our empirical analysis relies on the assumption that the demographic characteristics of mothers in treated municipalities changed in a way that is similar to those of mothers residing in comparison municipalities in the aftermath of policy adoption. Appendix Table A.9 tests this assumption by examining whether observable maternal characteristics changed after policy implementation in treated areas relative to comparison municipalities. Specifically, we run difference-in-difference regressions where maternal characteristics are dependent variables. If there were no compositional changes, point estimates on these regressions should be statistically insignificant and close to zero. This is exactly what we find. Aside from helping us rule out changes in the composition of women giving births, this result provides further indirect evidence that fetal selection is not a major issue in our setting.

4.4 Further Results and Robustness

We perform a number of other specification checks to test the robustness of our main results. In Appendix Tables A.10-A.12, we use different matching techniques to create similar treated and control municipalities, and estimate difference-in-difference regressions across this matched sample. To identify similar pairs, we use either propensity score or Mahalanobis-Metric matching. We also implement difference-in-difference regressions that reweight the observations either by entropy-weights (Hainmueller 2012), or by weights that depend on the propensity score or distances to treatment observations (DiNardo et al. 1996;

Heckman et al. 1998).[25] In general, the results are broadly similar to the baseline across these different estimation strategies.

Appendix Tables A.13-A.15 present the results from specifications that include mesoregion (137) and microregion (586) linear time trends instead of state linear trends.[26] For ease of comparison, column (1) in each table replicates the baseline specification. The findings continue to be essentially the same compared to the baseline. Column (4) uses a specification that includes rather state $\times$ bimonth $\times$ year fixed effects (27 states, 6 bimonths, 8/9 years). Our results are in general robust even using this more demanding specification.

In Appendix Tables A.16-A.18, we estimate the effects of MPP stratifying the sample according to the set of socioeconomic characteristics of municipalities. Looking at the number of physicians, we find substantial heterogeneities according to population size and local social spending, with effects of the largest magnitude for less populous areas and municipalities with greater local social spending. When we explore utilization of care, we find substantial heterogeneities. For doctor visits, we find larger effects in areas with a larger share of population that is rural and less populous. The results also indicate that the effects of the program on prenatal care delivered by doctors are notably larger in municipalities with increased illiteracy rates, higher spending on social programs, and lower per capita GDP - in these areas point estimates more than double the baseline. The patterns are less clear for prenatal care delivered by nurses, but in some cases they suggest declines statistically significant for more developed areas.

When we explore infant health outcomes, we do not find a consistent pattern. For each infant health outcome, we find evidence of a significant effect of policy in one subsample (out of 20). We find significant reductions associated to policy for prematurity in low unemployment rate areas. For low birth weight, there is only evidence of a significant decline in areas with high local social spending on education. Finally, we find a marginally statistically significant coefficient of interest, with the wrong expected sign, for infant mortality. This lack of a consistent pattern suggests that the few statistically significant estimated coefficients are most likely due to sampling error.

In Appendix Table A.19, we investigate whether the implementation of MPP coincided with changes in other health resources. This could be the case if, for instance, MPP implementation encouraged municipality governments to increase local hospital size, for instance. Alternatively, local administrations might reduce health resources to increase the availability

[25]See Appendix Figure A.8 for descriptive statistics on the distribution of propensity scores in treated and untreated municipalities.

[26]Mesoregion and microregion are subdivisions that aggregates several municipalities of a given geographic area with similar economic and social characteristics. The Brazilian Bureau of Statistics (IBGE) created these subdivisions for statistical purposes.

of public resources for other purposes. Using number of hospitals, number of hospital beds, number of ultrasound machines, number of complete dental equipment, and number of X-ray machines as dependent variables (all measured per 1,000 inhabitants), we find insignificant policy effects on these outcomes, with estimated coefficients extremely small in magnitude relative to the pre-MPP mean. The corresponding event-study graphs in Appendix Figure A.7 also confirm these results.

5 Conclusion

This paper has offered new evidence on the extent to which increases in physician supply affect infant health. This question is particularly important in countries with limited access to physicians where arguments are often made that the health returns to increasing the supply of physicians are large. Despite these claims, there is little careful empirical research on whether policies promoting increased access to primary care physicians in fact translates into infant health improvements in practice when implemented. Rather, policy prescriptions have been made without a careful empirical understanding on their potential effectiveness.

To advance our understanding of this important question, this paper exploits an intervention that caused a substantial increase in the supply of physicians in Brazil. The intervention that we examine "recruited" a substantial number of primary care physicians by offering considerable remuneration and an increase in the scoring of medical residency exams. Using a difference-in-difference empirical strategy, we document that municipalities implementing the program experience an abrupt increase in the number of physicians serving in basic health units, which is largely driven by family doctors. We then show that this increase in physician supply was accompanied by improvements in doctor visits and prenatal care delivered by physicians. Interestingly, the magnitude of the effects on doctor visits are the largest for infants under one year of age, compared with that of older patients. Yet despite these improvements in the utilization of medical care, we find very little evidence that the program led to improved health outcomes for infants. Indeed, the effects of program on prematurity, low birth weight and infant mortality can be bounded to a tight interval around zero. Remarkably, these findings are essentially the same across subgroups from a wide range of municipality and maternal characteristics. The paper is able to show that selective mortality or other specific features of the empirical setting cannot explain these results.

This overall absence of health benefits is surprising given that improving infant health was a major motivation and final goal of the program. It suggests that policymakers may be overstating the health returns of policies promoting access to primary care physicians. Thus, policymakers hoping to improve infant health should consider the potential benefits of interventions against the costs of increases in the supply of primary care physicians that may not have any impact on health. While the context is particular to Brazil, our findings may be of interest to other countries using similar strategies to increase the availability of physicians and provide inputs for future cost-benefit assessments of policy. For example, the Governments of Australia, Canada, Mexico, and Venezuela have created programs encouraging newly trained physicians to provide basic health care services in remote and needy communities by offering remuneration and medical residency exam incentives.

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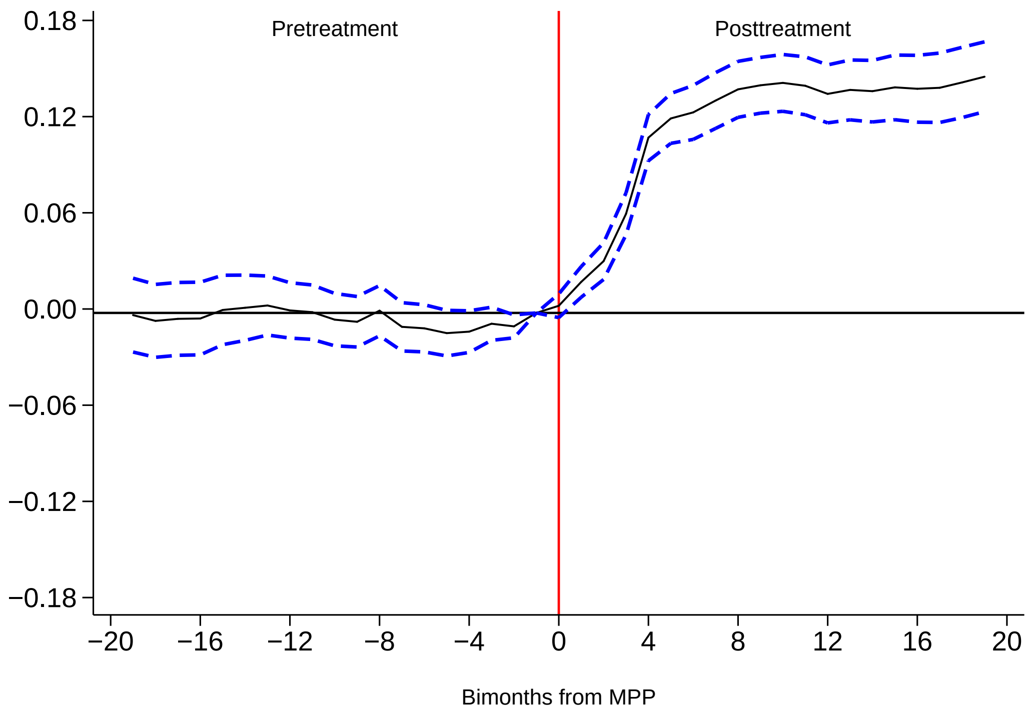
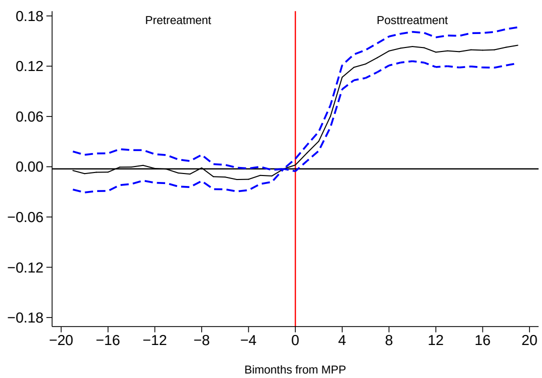
Figure 1: Effects of MPP on physicians

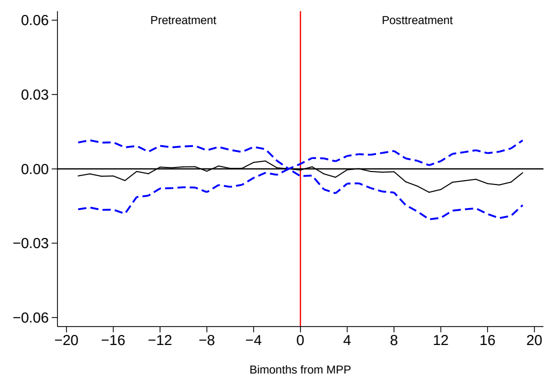


Notes. This is an event-study created by regressing physicians (per 1,000 residents) for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of physicians in treated versus untreated areas before and after of policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

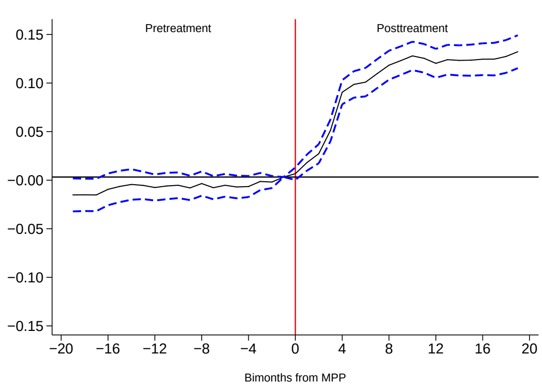
Figure 2: Effects of MPP on physician groups



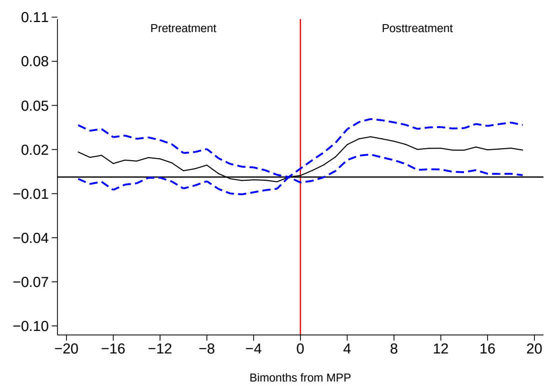
(a) Public physicians



(b) Private physicians



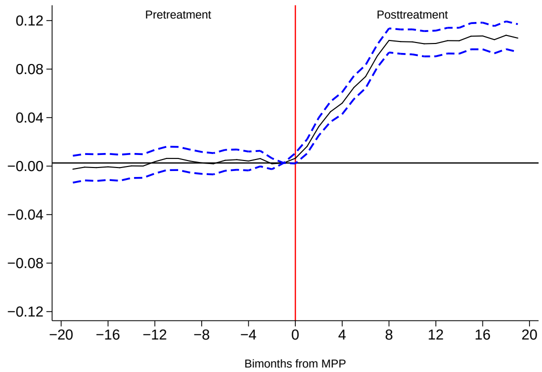
(c) BHU physicians



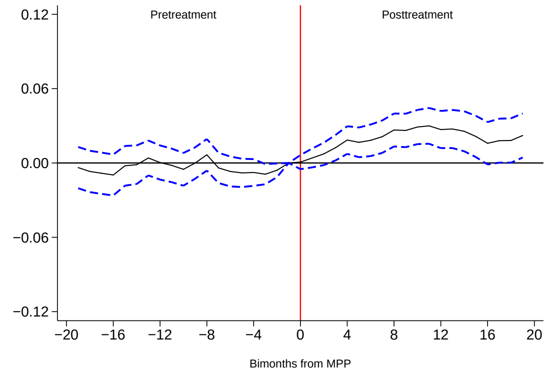
(d) non-BHU physicians

Notes. These are event-studies created by regressing physician outcomes (measured per 1,000 residents) for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of physicians in treated versus untreated areas before and after policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

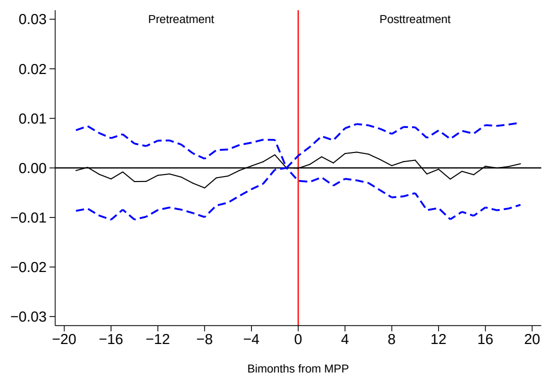
Figure 3: Effects of MPP on physician by specialty



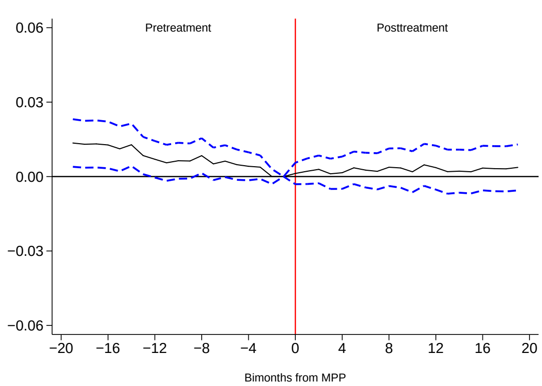
(a) Family doctors



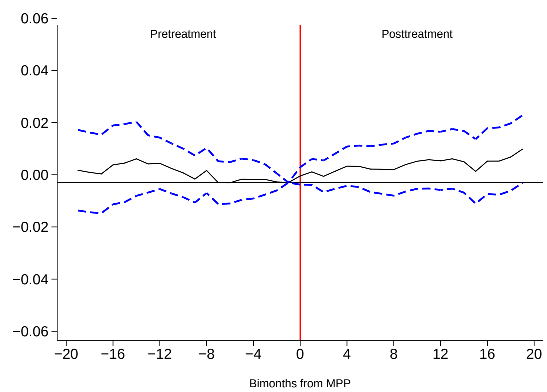
(b) Clinicians



(c) Gynecologist



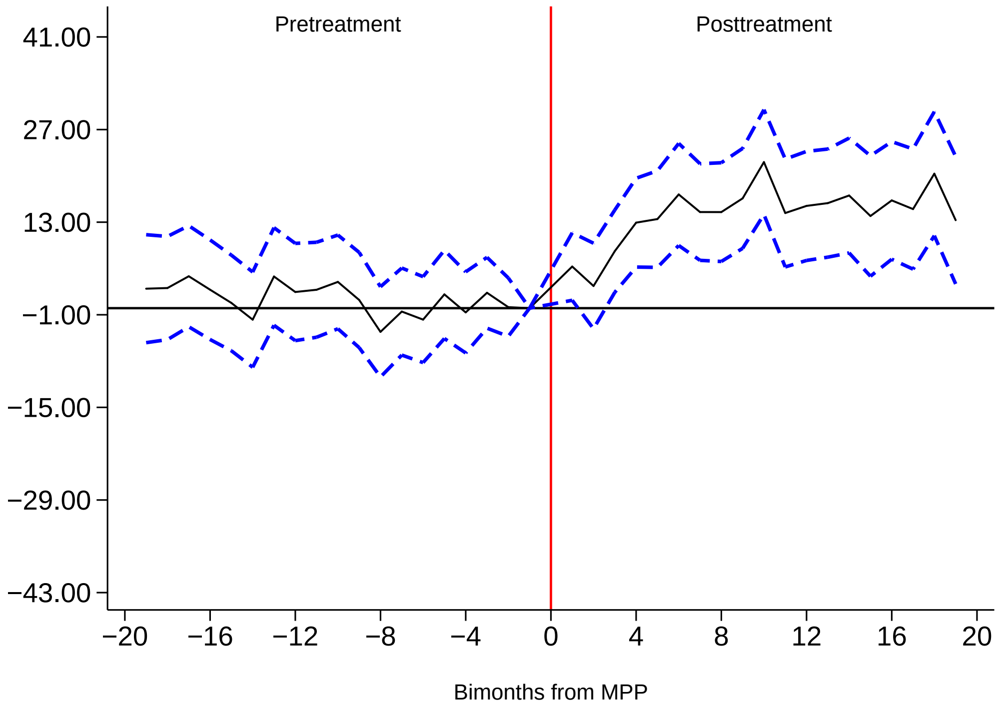
(d) Pediatricians



(e) Others

Notes. These are event-studies created by regressing physician outcomes (measured per 1,000 residents) for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of physicians in treated versus untreated areas before and after policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

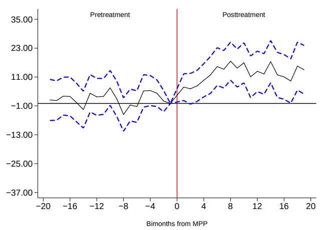
Figure 4: Effects of MPP on doctor visits



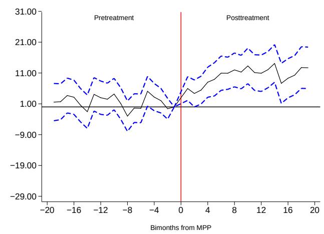
Notes. This is an event-study created by regressing doctor visits (per 1,000 residents) for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of doctor visits in treated versus untreated areas before and after policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

Figure 5: Effects of MPP on doctor visits by age

(a) 0-1

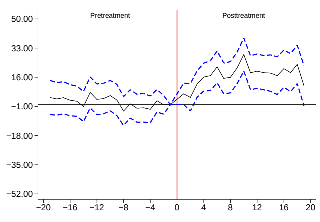


(b) 1-5



(c) 5-15

(d) 15-40

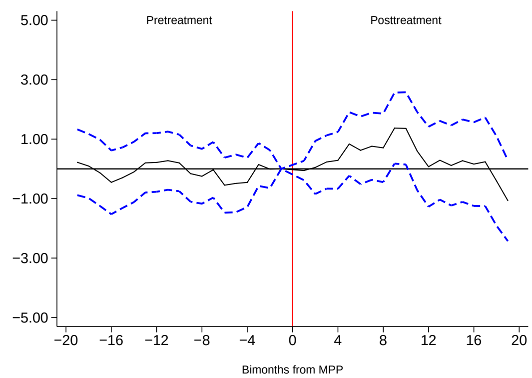


(e) 40-60

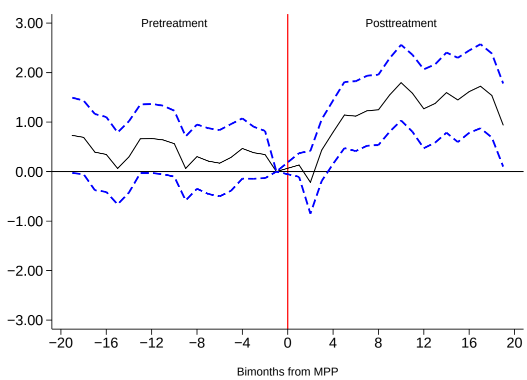
(f) 60+

Notes. These are event-studies created by regressing each outcome of interest (measured per 1,000 residents) for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of doctor visits by age in treated versus untreated areas before and after policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

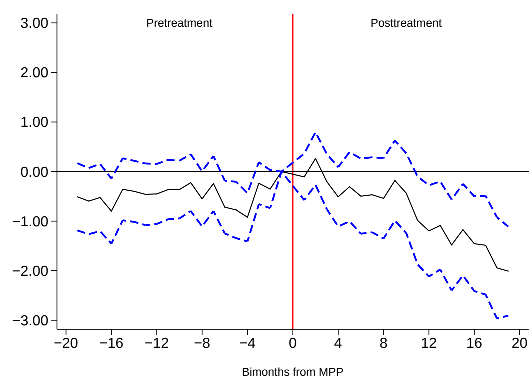
Figure 6: Effects of MPP on prenatal care



(a) prenatal visits



(b) prenatal visits by physicians

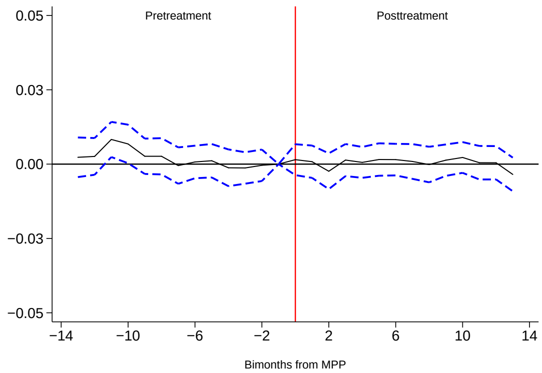


(c) prenatal visits by nurses

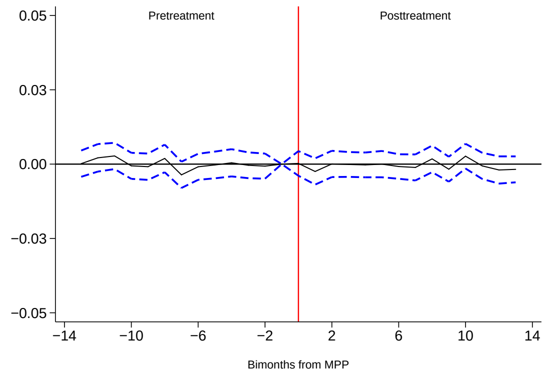
Notes. These are event-studies created by regressing each outcome of interest (measured per 1,000 residents) for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of prenatal visits in treated versus untreated areas before and after policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

Figure 7: Effects of MPP on infant health

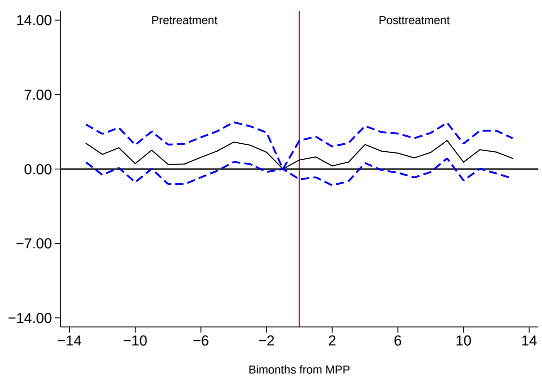
(a) Fraction preterm births



(b) Fraction low birth weight



(c) Infant mortality rate



Notes. These are event-studies created by regressing each outcome of interest for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, maternal characteristics, state linear time trends and the full set of municipality characteristics interacted with linear trends. The observations are weighted by the number of births. The figure reports the coefficients for event-time, which plot the time path of the outcome of interest in treated versus untreated areas before and after policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -1.

Table 1: The effect of MPP on physicians

	(1)	(2)	(3)	(4)	(5)
Post × Treatment	0.120	0.106	0.106	0.111	0.116
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)
Pre-MPP mean	0.63	0.63	0.63	0.63	0.63
R^2	0.88	0.87	0.87	0.87	0.88
N	300024	290304	286416	285012	285012
Time trends interacted with:					
Basic characteristics	No	Yes	Yes	Yes	Yes
pre-MPP BHU physician rate	No	No	Yes	Yes	Yes
Social spending	No	No	No	Yes	Yes
State indicators	No	No	No	No	Yes

Notes. Dependent variable is the total number of physicians per 1,000 residents. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects. Basic characteristics are time-invariant variables that include per capita GDP, log of population, illiteracy rate, indigenous population rate, Gini Index, unemployment rate, rural population rate, municipality area, altitude, distance to capital, temperature, rainfall, legal Amazon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education, health and *Bolsa Familia*. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 2: The effect of MPP on physicians - heterogeneities

	Funding		Site		Specialty				
	Public	Private	BHU	Other	Family	Clinicians	Gynecologist	Pediatricians	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treatment	0.116	-0.000	0.105	0.011	0.075	0.022	0.001	-0.003	0.004
	(0.008)	(0.006)	(0.006)	(0.008)	(0.004)	(0.006)	(0.003)	(0.003)	(0.005)
Pre-MPP mean	0.58	0.12	0.21	0.42	0.18	0.23	0.04	0.10	0.23
R^2	0.84	0.88	0.70	0.90	0.65	0.73	0.58	0.81	0.95
N	285012	115236	285012	285012	280260	266814	140292	100161	125705

Notes. Dependent variable in each column is measured per 1,000 residents. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects, state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. The number of observations differ across outcomes because municipalities with zero values during the entire period are excluded from the regression estimation. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 3: The effect of MPP on doctor visits

	(1)	(2)	(3)	(4)	(5)
Post × Treatment	7.603	13.343	12.278	12.316	11.280
	(2.825)	(2.833)	(2.828)	(2.832)	(2.825)
Pre-MPP mean	171.25	171.25	171.25	171.25	171.25
R^2	0.66	0.66	0.66	0.66	0.67
N	300510	290790	286416	285012	285012
Time trends interacted with:					
Basic characteristics	No	Yes	Yes	Yes	Yes
pre-MPP BHU physician rate	No	No	Yes	Yes	Yes
Social spending	No	No	No	Yes	Yes
State indicators	No	No	No	No	Yes

Notes. Dependent variable is the total number of doctor visits per 1,000 residents. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects. Basic characteristics are time-invariant variables that include pre-MPP per capita GDP, log of population, illiteracy rate, indigenous population rate, Gini Index, unemployment rate, rural population rate, municipality area, altitude, distance to capital, temperature, rainfall, legal Amazon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education, health and *Bolsa Familia*. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 4: The effect of MPP on doctor visits by age groups

	Age group:					
	0-1	1-5	5-15	15-40	40-60	60+
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treatment	16.347	10.510	7.911	9.487	13.281	21.052
	(3.757)	(2.783)	(2.002)	(2.395)	(3.416)	(6.453)
Pre-MPP mean	162.52	149.92	119.03	141.80	205.24	371.92
R^2	0.48	0.69	0.71	0.67	0.61	0.61
N	285012	285012	285012	285012	285012	285012

Notes. Dependent variable in each column is measured per 1,000 residents. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects, state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 5: The effect of MPP on prenatal care

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Prenatal visits				
Post × Treatment	0.562	0.743	0.675	0.591	0.114
	(0.450)	(0.458)	(0.463)	(0.465)	(0.443)
Pre-MPP mean	20.36	20.36	20.36	20.36	20.36
R^2	0.55	0.56	0.56	0.56	0.57
N	300510	290790	286416	285012	285012
	Panel (b): Prenatal visits by physicians				
Post × Treatment	0.752	0.836	0.795	0.738	0.625
	(0.237)	(0.245)	(0.248)	(0.248)	(0.246)
Pre-MPP mean	10.11	10.11	10.11	10.11	10.11
R^2	0.52	0.53	0.53	0.53	0.53
N	300510	290790	286416	285012	285012
	Panel (c): Prenatal visits by nurses				
Post × Treatment	-0.188	-0.093	-0.121	-0.149	-0.514
	(0.313)	(0.318)	(0.320)	(0.322)	(0.303)
Pre-MPP mean	10.25	10.25	10.25	10.25	10.25
R^2	0.63	0.63	0.64	0.64	0.65
N	300510	290790	286416	285012	285012
Time trends interacted with:					
Basic characteristics	No	Yes	Yes	Yes	Yes
pre-MPP BHU physician rate	No	No	Yes	Yes	Yes
Social spending	No	No	No	Yes	Yes
State indicators	No	No	No	No	Yes

Notes. Dependent variable in each column is measured per 1,000 residents. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects. Basic characteristics are time-invariant variables that include pre-MPP per capita GDP, log of population, illiteracy rate, indigenous population rate, Gini Index, unemployment rate, rural population rate, municipality area, altitude, distance to capital, temperature, rainfall, legal Amazon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education, health and *Bolsa Familia*. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 6: The effect of MPP on infant health

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (a): Prematurity					
Post × Treatment	-0.003	0.001	0.000	0.000	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pre-MPP mean	0.08	0.08	0.08	0.08	0.08	0.08
R^2	0.32	0.33	0.33	0.33	0.33	0.34
N	266720	257733	253904	252665	252665	252665
	Panel (b): Low birth weight					
Post × Treatment	-0.001	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pre-MPP mean	0.08	0.08	0.08	0.08	0.08	0.08
R^2	0.13	0.12	0.12	0.12	0.12	0.12
N	266720	257733	253904	252665	252665	252665
	Panel (c): Infant mortality					
Post × Treatment	0.387	0.035	0.054	0.046	0.042	0.002
	(0.277)	(0.234)	(0.236)	(0.236)	(0.236)	(0.237)
Pre-MPP mean	15.98	15.98	15.98	15.98	15.98	15.98
R^2	0.17	0.06	0.06	0.06	0.06	0.06
N	268560	258480	254592	253344	252665	252665
Time trends interacted with:						
Basic characteristics	No	Yes	Yes	Yes	Yes	Yes
pre-MPP BHU physician rate	No	No	Yes	Yes	Yes	Yes
Social spending	No	No	No	Yes	Yes	Yes
Maternal characteristics	No	No	No	No	Yes	Yes
State linear time trends	No	No	No	No	No	Yes

Notes. Dependent variables in panel (a) and (b) are proportion of preterm births and proportion of low birth weight babies, respectively. Dependent variable in panel (c) is the number of infant deaths per 1,000 live births. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects. Basic characteristics are time-invariant variables that include pre-MPP per capita GDP, log of population, illiteracy rate, indigenous population rate, Gini Index, unemployment rate, rural population rate, municipality area, altitude, distance to capital, temperature, rainfall, legal Amazon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education, health and *Bolsa Familia*. Maternal characteristics include average age, proportion of births by mothers with less than 4 years of schooling, and proportion of births by unmarried mothers. The observations are weighted by the number of births. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 7: The of MPP on infant health according to baby's sex and maternal characteristics

	Male	Female	Mother's education < 4 years	Mother's education > 4 years	Unmarried	Married	Mother's age < 20	Mother's age > 20
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel (a): Prematurity				
Post × Treatment	0.0013	-0.0004	0.0031	0.0000	-0.001	0.0007	0.0027	-0.0001
	(0.0014)	(0.0015)	(0.0046)	(0.0013)	(0.0017)	(0.0015)	(0.0020)	(0.0013)
R^2	0.248	0.227	0.115	0.325	0.235	0.224	0.172	0.3
N	248825	248292	175136	251872	242246	248638	230857	251424
				Panel (b): Low birth weight				
Post × Treatment	0.0009	-0.001	-0.0002	-0.0003	-0.0005	-0.0007	0.0006	-0.0004
	(0.0008)	(0.0009)	(0.0031)	(0.0007)	(0.0011)	(0.0009)	(0.0014)	(0.0007)
R^2	0.073	0.078	0.051	0.115	0.084	0.076	0.049	0.116
N	248825	248292	175136	251872	242246	248638	230857	251424
				Panel (c): Infant mortality rate				
Post × Treatment	0.1418	-0.0998	-0.1126	-0.0897	-	-	-0.4316	0.0198
	(0.3407)	(0.3224)	(0.2259)	(0.2306)			(0.5402)	(0.2687)
R^2	0.043	0.038	0.066	0.05			0.032	0.055
N	252001	252001	251565	252001			252001	252001

Notes. Each coefficient is from a different regression. Municipality and bimonth-by-year fixed effects are included in all specifications. Regressions include also maternal characteristics, state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. When the sample is stratified by the maternal characteristic X, then the variable X is excluded from the regression. Mother's marital status is not available for death records. Observations are weighted by the number of births. Robust standard errors (reported in parenthesis) are clustered at the municipality level.

Table 8: The effect of MPP on infant mortality rate by cause

	Infectious and parasitic diseases	Respiratory diseases	Perinatal conditions	Congenital abnormalities	Other diagnoses
	(1)	(2)	(3)	(4)	(5)
Post × Treatment	-0.0974	0.0214	-0.0376	0.1297	-0.0144
	(0.0509)	(0.0464)	(0.1762)	(0.0919)	(0.0868)
Pre-MPP mean	0.645	0.705	8.007	2.71	1.546
R^2	0.04	0.044	0.048	0.026	0.038
N	252001	252001	252001	252001	252001

Notes. Each coefficient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects. Regressions include also maternal characteristics, state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. The observations are weighted by the number of births. The coding for cause of death follows the International Statistical Classification of Diseases and Related Health Problems 10th Revsion (ICD-10). Robust standard errors (reported in brackets) are clustered at the municipality level.