Provider Supply, Utilization, and Infant Health: Evidence from a Physician Distribution Policy*

Bladimir Carrillo Jose Feres

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Abstract

We analyze a policy that substantially expanded the supply of primary care physicians in Brazil. The program is associated with a significant increase in doctor visits across all age groups, and greater utilization of doctors as source of prenatal care. However, this increased used of doctors was accompanied by significant reductions in prenatal care from nurses. As a result of this shift in the provider of care, there were no gains in widely-used metrics of infant health, including birth weight, gestation and infant mortality. These findings suggest that physicians and nurses may be good substitutes in terms of neonatal health.

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

JEL Codes: I12, I18, I38

Resumo:

Este estudo analisa uma política que ampliou substancialmente a oferta de médicos de atenção primária no Brasil. O programa está associado a um aumento significativo de consultas médicas em todas as faixas etárias e à maior utilização de mé dicos como fonte de assistência pré-natal. No entanto, esse aumento no uso de mé dicos foi acompanhado por reduções significativas no número de consultas pré-natais fornecidas por enfermeiras. Como resultado dessa mudana no provedor da atençâ o, não houve ganhos em métricas amplamente utilizadas de saúde infantil, incluindo peso ao nascer, gestação e mortalidade infantil. Esses resultados sugerem que médicos e enfermeiras podem ser bons substitutos em termos de saúde neonatal.

Palavras chaves: médicos de atenção primária; uso de médicos; saúde infantil; avaliação de impacto

Área ANPEC-Regional: Economia do Trabalho, Economia Social e Demografia

*Contact: bladimir.bermudez@ufv.br, address: DER, UFV; jose.feres@ipea.gov.br, address: IPEA, Rio de Janeiro.

1 Introduction

Providing efficient, basic health care has been an important objective of many governments, but even today several hundred million people do not receive primary and preventive health services.[1] It is often emphasized that these disparities are the result of the limited access to qualified physicians in some regions.[2] The World Health Organization (WHO, 2006) estimates that 57 developing countries face a severe shortage of physicians, and recent reports suggest that even affluent countries such as the United States will suffer from this phenomenon over the next decade (Association of American Medical Colleges, 2017). As a result, some nations have implemented a number of initiatives to improve the recruitment and retention of physicians in underserved areas, including the use of compulsory services, financial incentives, and expansion of medical schools. Yet despite the widespread interest in increasing physician numbers to improve care, there is little rigorous research measuring the extent to which increasing the supply of physicians promotes greater utilization, and even less evidence on whether it ultimately translates into improved public health.

This paper studies this question by examining the effects of a policy that substantially expanded the supply of primary care physicians in Brazil. In 2013, the Brazilian government launched a major program, the More Physicians Program (MPP), aimed at alleviating the shortage of primary care physicians in some regions. Placed in community health clinics - called Basic Health Units (BHU) - MPP physicians provide a number of primary health services free-of-charge to all citizens, including prenatal care, treatment of minor illnesses, and health counselling to prevent and treat diseases. We study the effects of MPP, and thus the increased number of physicians, on the utilization of medical care. As an initial evaluation of the health effects of the program, we also examine policy impacts on the health of infants born to mothers living in treated areas.

Our identification strategy compares the outcomes of treated and untreated municipalities before and after the implementation of MPP in a differences-in-differences framework. We begin our analysis by measuring the relationship between MPP implementation and physicians. This analysis is important in view that some local administrations may be "taking advantage of the More Doctors program to dismiss other doctors who already were working for the municipality to cut spending" (see Jornal Nacional, March 4, 2017).[3] If there is indeed a systematic substitution of physicians, then MPP could fail to increase the availability of physicians in treated areas. We find that program adoption led to an immediate and statistically significant increase of 0.11 in the total number of physicians per 1000 residents. Compared with the baseline mean of 0.67, this represents an increase of 18 percent.

Having documented a strong and robust "first stage", we then study MPP's impacts on the utilization of medical care. The results indicate that MPP significantly increases

[1]Estimates by the World Health Organization indicate that about 1.3 billion people lack access to basic medical care (see `http://www.who.int/bulletin/volumes/86/11/07-049387/en/`, last accessed on April 9th, 2018).

[2]For example, several reports of the World Health Organization (WHO, 2013, 2010) stress the importance of having an adequate supply of primary care physicians and other health workers to reduce inequalities in the acces to basic medical care.

[3]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 MPP, but they retain discretion over physicians not linked to the program.

doctor visits by 5 to 8 percent. We observe this relationship for infants, children, adults and the elderly. Combined with the physician results, our calculations suggest that a 1-percent increase in the number of physicians as a result of MPP would increase doctor visits by 0.33 percent. We also find that MPP led to an increase of 10 percent in the quantity of prenatal care provided by physicians. However, the data also reveal that the policy is associated with a significant reduction in the quantity of prenatal care by nurses. As result of this systematic substitution of nurse for physician care, the overall effect of MPP on the number of prenatal care visits women receive is not statistically distinguishable from zero.

We next evaluate whether the program is associated with changes in infant health outcomes, measured by infant mortality, birth weight and prematurity. Given the evidence that MPP caused a shift in the providers of care from nurses to physicians, without an increase in the number of prenatal care visits women receive, one would expect positive effects on infant health if the quality of care provided by physicians is significantly higher relative to that provided by nurses. The data reveal very little evidence that MPP led to gains in infant health. We find estimates that can usually be bounded to a tight interval around zero, allowing us to rule out effects larger than 0.01 of a standard deviation. We continue to find virtually zero policy effects when stratifying the sample according to baby's sex, maternal characteristics, and pretreatment characteristics of the municipality. Finally, there are no effects of the policy on infant mortality even when we examine different causes of death.

Our findings contribute to an ongoing debate on laws encouraging substitution of doctors for nurses (Laurant et al., 2005, Stange, 2014, Traczynski and Udalova, 2018), and to a growing literature relating pre-natal/post-natal care by nurses and infant health. Given the difficulty of retaining physicians in some regions and increasing pressure to contain costs, several governments have introduced reforms to expand nurses' role in the provision of primary care services (Jenkins-Clarke et al., 1998, Laurant et al., 2005). However, research on whether trained nurses can produce comparable quality of care as primary care doctors has been limited (Laurant et al., 2005). Wüst (2012) documents that greater nurse care through home visits is associated with reduced risk of infant death in Denmark. Moehling and Thomasson (2014) show that activities conducted under the Sheppard-Towner Act, which include home visits by nurses, is associated with significant reductions in infant mortality rates in the United States. In Brazil, some studies document that the expansion of care by nurses and physician assistants during the mid-1990s was associated with improved birth outcomes and reduced risk of infant death (Macinko et al., 2006, Bhalotra et al., 2016). Other studies also provide suggestive evidence that nurse/midwife care can be beneficial for infant health (Pettersson-Lidbom, 2015, Hjort et al., 2017). By evaluating the effects of a physician program in a context where nurses previously substituted doctors in primary care, our study provides suggestive evidence on the relative efficacy of physicians versus nurses for infant health. Taken together with the evidence from previous studies, our findings suggest that when it comes to neonatal health outcomes, nurses and primary care doctors may be good substitutes. This implies that the infant health returns of physician distribution interventions may depend on what doctor visits "replace": If they replace nurse visit, or midwife care, then that might have limited effects on infant health relative to when they replace "nothing".

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 The More Physician Program

To alleviate physician shortages, the Brazilian government implemented the More Physicians Program (MPP) in September 2013. The program operates by recruiting physicians to work in underserved areas for a period of three or more years. Enrolled physicians are public employees and receive a fixed salary of about USD 3,000. This salary is untaxed, and five times larger than the federal minimum wage for physicians in 2013 (established by the Law Decree no. 3.999/61). In addition, MPP physicians receive housing and food benefits financed by the local governments. Physicians interested in joining the MPP are required to complete a training program in family health medicine, which includes a distance-learning orientation administrated while working. The BHUs function as the workplace of the recruited physicians, where they provide a number of free-of-charge primary care services. 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 training program. A senior doctor is responsible for monitoring and supporting the program's physicians in a given region. Failure to meet the activities could result in contract termination.

The program was implemented only in a set of municipalities. While 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);[4]

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. As shown in Table 1, about 90 percent of eligible municipalities joined the program. As a whole, out of all 5,570 Brazilian municipalities, the program was finally implemented in approximately 4,132.

Participation is open to the set of existing physicians within both the private and public sectors, and recent graduates of medical schools.[5] Physicians who were already working in a BHU in treated municipalities prior to policy can enroll in the program only if they are willing to be reallocated to a municipality with greater shortage of physicians. There are several rounds of selection where physicians could voluntarily enroll in the program. To increase the chances of recruitment, physicians who practice medicine in countries with a number of physicians above 1.8 per 1,000 residents are allowed to join the MPP. Foreign doctors have undergone training, which includes Portuguese classes and orientation on the functioning of the SUS. While participation is open to foreigners,

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

[5]Physicians working in the public sector could participate in MPP by taking a leave of absence from their current position.

Brazilian doctors receive priority.[6] In practice, only 10 percent of vacancies were filled in the first round of selection. As a response, the Brazilian government immediately put in place a cooperation agreement with PAHO to facilitate the large-scale participation of Cuban doctors in the program. The agreement had been studied and signed several months before the MPP was officially announced, and the intention was to eventually use it in case of low enrollment rates of Brazilian doctors.

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.

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.[7] 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.[8] 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.[9] We use bimonthly variation in our analysis because monthly data are noisy, particularly for infant health measures.[10,11] For each panel dataset of the outcome variable of interest, we exclude those municipalities with zero observations during the complete study period.[12] 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 private and public health facilities in Brazil. They provide detailed information about

[6] Specifically, the order of priority establishes that participation is first offered to Brazilian and foreign physicians registered with the Regional Medical Council (CRM). If vacancies remain, they are offered to Brazilian doctors trained abroad. The remaining vacancies are then offered to a third group of foreign doctors trained outside the country.

[7] 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 a single one. Data on physicians are available from 2005, but they cover the entire country only from 2008 and onwards.

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

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

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

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

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

physicians linked to some healthcare facility, including practice and levels of specialization. Our main outcome of interest is the total number of physicians both in the private and public sectors. 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.

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. Our key outcomes are prenatal care 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 nurses) and physicians.

Vital statistics records provide details on the universe of births and deaths occurring each year in Brazil as reported on birth and death certificates. We use three outcome variables to characterize the health effects of increased supply of primary care physicians. First, 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 health and developmental difficulties among babies who survive the infancy.[13]

Second, we consider mortality within one year of birth, an appealing measure of severe health problems and an outcome of direct interest for policy makers. We also examine different cause-specific mortality rates. Specifically, we group our sample into five 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).

Additionally, we have a rich set of time-invariant 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 characteristics. The source of the socioeconomic and demographic characteristics is the 2010 Census, which is the most recent full population census available. We use these data to control for differential trends in these characteristics in our estimates of the effects of MPP on the outcomes of interest.

3.2 Estimation Strategy

We employ a differences-in-differences design to estimate the effects of MPP on physicians, patient care and infant health outcomes:

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

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 coefficient

[13] 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.

β measures then the effect of MPP on the outcomes of interest. The covariates Z_i, interacted with a linear time trend, include a set of pre-intervention municipality characteristics (measured only at one point in time before MPP adoption). We also control for state-specific linear time trends. When the dependent variable is an infant health outcome, we also control for maternal characteristics. These include 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 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. All our models use robust standard errors adjusted for clustering at the municipality level to account for serial correlation (Bertrand et al., 2004).

A disadvantage of the specification based on equation (1) is that it does not provide any insight into the timing of the program's 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 equation (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.

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. The identifying assumption would be violated only if there were differential trends in time-varying determinants of outcomes across treated and untreated areas. In particular, disadvantaged areas may be more likely to adopt MPP than advantaged areas, so one may be concerned if there are differences in trends in these characteristics spuriously correlated with the treatment effect. To account for this possible threat to internal validity, we control for interactions of a wide range of pre-treatment municipality characteristics with a linear time trend, and for state-specific linear time trends in our baseline specification (as in Hoynes and Schanzenbach (2009) and Bailey and Goodman-Bacon (2015)). Reassuringly, point estimates are largely unaffected by the inclusion of these trends in most cases, suggesting that our results are unlikely to be driven by other differential trends across treated and control municipalities.

Another potential concern with our difference-in-difference empirical design is whether there was a shift of resources from untreated to treated areas because it is more lucrative for physicians to join the treated municipalities due to the program. This would mean that control municipalities might also be affected by attracting fewer physicians, and consequently we could overestimate the effects of the policy on medical utilization and infant health. However, the data suggest that this issue is unlikely to be important in practice. However, the changes in physician rate during the post-intervention period are largely driven by increases in the number of physicians in treated areas and not also by decreases in control areas. This is unsurprisingly given that most physicians enrolled in MPP are from abroad.

More generally, we can use the event-study specification to check for differential pre-trends in the outcomes of interest and judge directly the plausibility of the identifying assumption. 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. As shown in detail below, the results from estimating the event-study specification are largely consistent with the identifying assumption. After presenting the main results, we provide further tests for specific threats to the internal validity of the empirical approach.

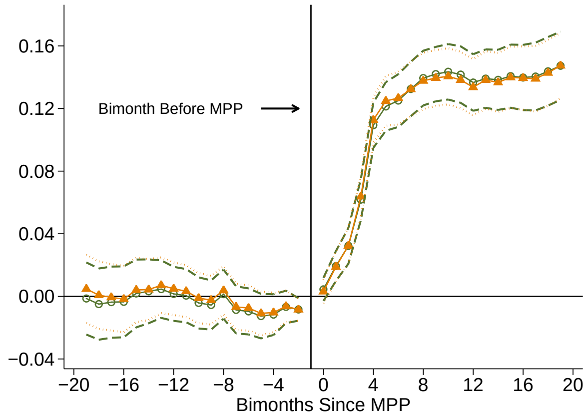
4 Results

4.1 Effects of MPP on Physicians

We begin by examining the relationship between policy adoption and the supply of physicians. Figure 1 shows the results from estimating event-studies for the number of physician per 1,000 residents. The series plotted with triangles presents the results from a specification that includes only controls for municipality and bimonth-by-year fixed effects. The series with open circles correspond to a specification that adjusts in addition for state-specific linear time trends and interactions of pre-MPP characteristics with a linear time trend. The respective 95 confidence intervals for both series are shown in the dashed lines. The results are extremely similar across both specifications and show a remarkable increase in the supply of physicians immediately after policy implementation in treated areas relative to control municipalities. This increase peaks at the bimonth 10 and persists for the rest of the post-intervention period. Importantly, there no statistically significant differential trends in physician rates before the introduction of the program. This provides strong support for the identifying assumption that treatment and control municipalities would have experienced similar changes in physician rates in the absence of MPP.

Table 2 reports regression results of the average effect of MPP on physician rates. It also shows in detail how the estimated treatment effect varies across different specifications. Column (1) is based on a specification that adjusts only for municipality and time fixed effects. 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 very 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 interactions between linear time trends and a set of pre-treatment characteristics (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). The

Figure 1: Effects of MPP on physicians



Notes. These are event studies for the number of physicians per 1000 residents. The series plotted with triangles presents the results from a specification that includes only controls for municipality fixed effects and bimonth-by-year fixed effects. The series with open circles correspond to a specification that adjusts in addition for state-specific linear time trends and interactions of pre-MPP characteristics with a linear time trend. The dotted and dashed lines represent the respective 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.

estimated coefficient now stands at 0.106. 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 provides reassuring evidence that the results are unlikely to be driven by differential trends across treated and comparison municipalities.

The estimated coefficient from our preferred specification that adjusts for all baseline 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 summary, the findings in this section suggest that the policy implementation led to a large and robust increase in the overall rate of physicians.

4.2 Effects of MPP on Utilization of Care

Doctor visits. 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 2 plots the coefficients and 95 percent confidence intervals from estimating the event-studies 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. The estimates of the pre-MPP effects fluctuate randomly around zero and are never statistically significant. In the first three bimonths after the MPP was introduced, doctor visit rate increased by 9 points in treated areas compared to control municipalities, or by 5-percent from the baseline mean. In the subsequent two bimonths, that increase was 14 points or 8 percent, an effect that persisted for the rest of the post-intervention period. The estimated effects are in general statistically different from zero during the entire post-MPP period, and remarkably stable across different specifications (Table 4). Using our baseline specification of equation (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.

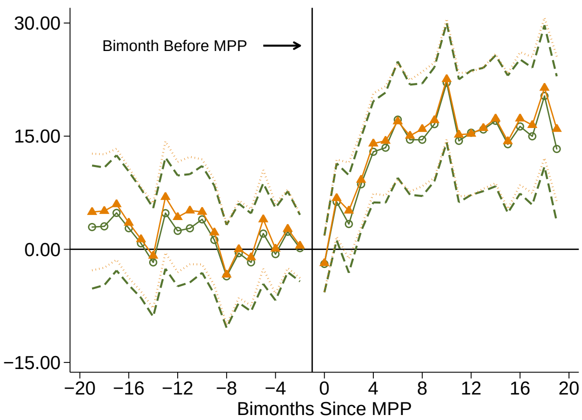
Table 2: The effect of MPP on doctor visits

	(1)	(2)	(3)	(4)	(5)
Post $\times$ 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.

we find that MPP led to an average increase of 11 ambulatory visits to physicians per 1000 residents (Table 4, column 5).

Figure 2: Effects of MPP on doctor visits

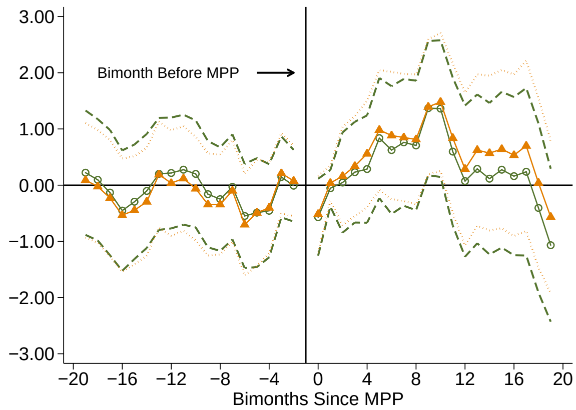


Notes. These are event studies for the number of doctor visits (per 1000 residents). The series plotted with triangles presents the results from a specification that includes only controls for municipality fixed effects and bimonth-by-year fixed effects. The series with open circles correspond to a specification that adjusts in addition for state-specific linear time trends and interactions of pre-MPP characteristics with a linear time trend. The dotted and dashed lines represent the respective 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.

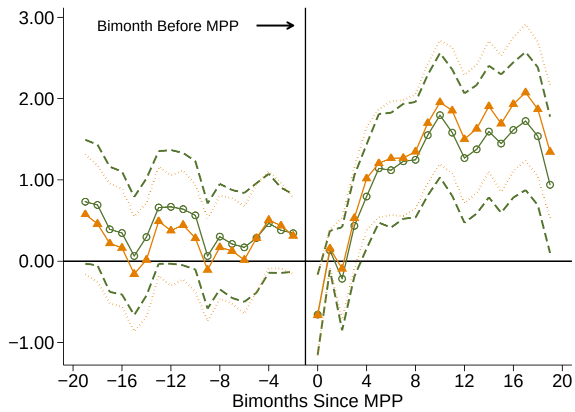
Prenatal care. Figure 3 presents the results from estimating event-studies for prenatal care visits. Panel (a) reveals no visual evidence of an increase in prenatal care visits associated with MPP. Indeed, this outcome evolved similarly in treated and untreated areas both before and after policy implementation. Consistent with the graphical evidence, the results in Table 6, panel (a), show no evidence that MPP is associated with higher use of prenatal care, irrespective of the set of controls included in the regressions. With all baseline controls, the coefficient of interest is estimated as 0.114 (with a standard error of 0.443), which is small relative to the baseline mean (at less than 0.5 percent).

These results, however, do not distinguish between prenatal cared visits by physicians and nurses. As discussed before, nurses play a prominent role in the provision of care in areas underserved by physicians. In the sample as a whole, nurses account for more than 50 percent of all prenatal visits women receive. Thus, a possibility is that MPP caused a shift in the providers of care from nurses to physicians, and consequently the average effect on the total number of prenatal visits is zero. To investigate this question, we examine separately the effects of MPP on prenatal care provided by physicians and nurses. The results in Figure 3, panel (b), reveal a statistically significant increase in the number of prenatal care visits by doctors. By the fifth bimonth since the introduction of the program, the average increase is estimated at 0.43 per 1000 or 4 percent relative to the pre-MPP mean. This increase becomes 12 percent by the seventh bimonth, and stands at around 16 percent in the following bimonths. Table 6, panel (b) documents that the average increase in prenatal care visits by physicians as result of MPP is about 0.63 per 1000. Relative to the baseline mean, this represents an increase of approximately 6 percent. Combined with our physician results, our calculations suggest that a 1-percent increase in the supply of physicians as result of MPP would lead to a 0.35-percent increase in the number of prenatal care visits by physicians.

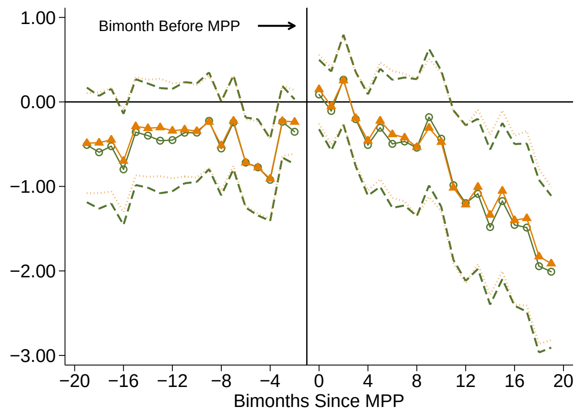
Figure 3: Effects of MPP on prenatal care



(a) prenatal visits



(b) prenatal visits by physicians



(c) prenatal visits by nurses

Notes. These are event studies for prenatal care visits (measured per 1000 residents). The series plotted with triangles presents the results from a specification that includes only controls for municipality fixed effects and bimonth-by-year fixed effects. The series with open circles correspond to a specification that adjusts in addition for state-specific linear time trends and interactions of pre-MPP characteristics with a linear time trend. The dotted and dashed lines represent the respective 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 3: 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.

The results from estimating event-studies for the number of prenatal care visits by nurses are shown in Figure 3, panel (c). As can be seen from the figure, the number of women receiving prenatal care from nurses declined systematically after the introduction of MPP. At the twelfth bimonth since MPP adoption, prenatal care visits by nurses declined by nearly 0.55 per 1000, or 5 percent compared to the baseline mean. This reduction is much more pronounced in the subsequent bimonths: the reduction is estimated at 10 percent around the eleventh bimonth and at 15 percent around the seventeenth bimonth since MPP. Table 6, panel (c) summarizes the average effect of the program on this outcome. Using our preferred specification of equation (1), the results indicate that on average MPP is associated with a 5-percent reduction in the quantity of prenatal care provided by nurses. These magnitudes are strikingly similar (in absolute value) to that observed for prenatal care by physicians, and thus there were a nearly perfect substitution in the provision of prenatal care between nurses and physicians. As a result, the effect of MPP on the total number of visits women is indistinguishable from zero.

4.3 Effects of MPP on Infant Health

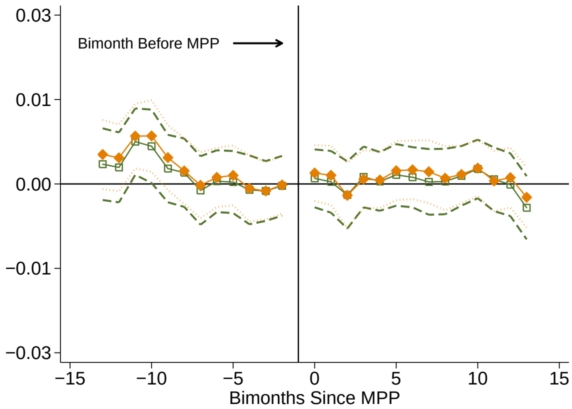
We now examine the effects of policy on infant health, namely low birth weight, prematurity and infant mortality. Given that MPP caused a systematic substitution of prenatal care from nurses to physicians, without an increase in the number of visits women receive, one could plausibly expect positive effects on infant health if the quality of care provided by physicians is significantly higher relative to that provided by nurses. In addition, the results documented above that MPP led to greater utilization of doctors among infants imply that the program could also affect infant mortality through this change in postnatal care if the effectiveness of care provided by physicians is high relative to alternative sources.

The results are shown in Figure 4 and Table 7. As for physician and utilization results, we show event-study figures based on a specification that adjust only for municipality and time fixed effects, and other that includes the complete set of controls. 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. While point estimates tend to be very similar across both specifications, the inclusion of the full set of controls is helpful in eliminating some pre-differential trends in preterm births. Yet there is no evidence that MPP led to changes in any of the infant health measures. In addition, the estimated coefficients are very small in magnitude. For instance, the estimated coefficient of interest for prematurity is smaller than 0.0001 (Table 7, column 6), relative to a pre-MPP mean 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.

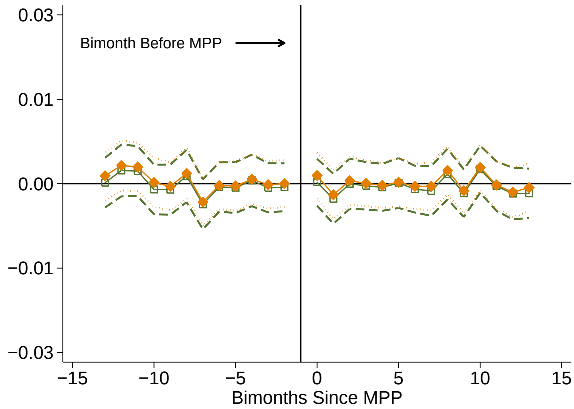
One might argue that these null effects mask important forms of heterogeneities. To explore this issue, Table 8 shows the results from running the regressions separately for different subgroups based on baby's sex and maternal characteristics. The results separately by gender do not reveal any evidence that the policy affected infant health outcomes. 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 estimates that are not statistically distinguishable from zero and precise enough to rule out even very modest effects.

Table 9 examines mortality results by cause of death. When we group causes of death

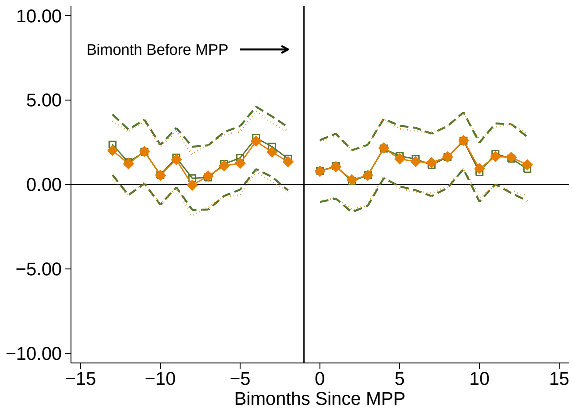
Figure 4: Effects of MPP on infant health



(a) Fraction preterm births



(b) Fraction low birth weight



(c) Infant mortality rate

Notes. These are event studies for infant health outcomes. The series plotted with triangles presents the results from a specification that includes only controls for municipality fixed effects and bimonth-by-year fixed effects. The series with open circles correspond to a specification that adjusts in addition for maternal characteristics, state-specific linear time trends and interactions of pre-MPP characteristics with a linear time trend. The observations are weighted by the number of births. The dotted and dashed lines represent the respective 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 4: 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	0.383	0.033	0.051	0.044	0.040	-0.000
	(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	268608	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 5: 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.

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.

5 Conclusion

This paper has offered new evidence on the extent to which increases in physician supply affect health care utilization and infant health. This question is particularly important in countries with limited access to physicians where arguments are often made that the 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 greater utilization and ultimately improvements in infant health. 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. Using a difference-in-difference empirical strategy, we document that municipalities implementing the program experienced an abrupt increase in the number of physicians serving in basic health units, which is largely driven by family doctors. We then show that the program is associated with a significant increase in doctor visits across all age groups, and greater utilization of doctors as source of prenatal care. However, this increased used of doctors was accompanied by reduced prenatal care from nurses. As a result of this systematic substitution of nurse for physician care, there were no gains in widely-used metrics of infant health, including birth weight, gestation and infant mortality.

An important lesson from our analysis is that the infant health returns of physician distribution interventions may depend on what doctor visits replace. If they replace nurse visit, or midwife care, then that might have limited effects on infant health relative to when they replace "nothing". This has significant implications for the debate on the costs and benefits of policies encouraging substitutions of doctors for nurses. The motivation of shifting care from doctors to nurses is to reduce the direct costs of services (because nurses are cheaper to hire than physicians) and improve access to care in underserved areas (Jenkins-Clarke et al., 1998, Whitecross, 1999). Some critics of nurse-physician substitution allege that nurses have limited ability to detect some illnesses, and it would adversely affect health outcomes (Breen et al., 2004, Offredy et al., 2008). Our findings suggest that physicians and nurses may be good substitutes, at least in terms of newborn health outcomes.

There are important caveats that we wish to stress. First, this paper focuses on an intervention that affected access to primary care physicians, and does not address the question of whether access to more specialized physicians are effective in improving infant health outcomes. For example, Currie et al. (1995) find suggestive evidence that increased access to obstetrician/gynecologists as a result of increases in Medicaid fees paid to these physicians is associated with reductions in infant mortality rates. Second, our results do not imply that the program would have no impact on other demographic groups. This type of program could benefit adult health conditions in the long-term, where substitutions from nurses to doctors could be relatively less important compared to “true” expansions in access. A recent contribution by Bailey and Goodman-Bacon (2015) shows that Community Health Centers, which deliver primary care services through physicians, nurses and social workers, are associated with large reductions in mortality rates among individuals 65 and older. Thus, caution is warranted in extrapolating our results to the medium-to-long-run health effects of MPP. We believe that future work using longer series of data (when available) could shed light on this question. Finally, the policy could have affected other important dimensions of well-being, including patient satisfaction, hospital care use, local health spending, and physician labor market, which are out of the scope of this study and may be important in evaluating the cost effectiveness of physician distribution interventions.

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