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

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We analyze a policy that substantially expanded the supply of primary care physicians in Brazil. The program increased doctor visits across all age groups and led to greater utilization of doctors for prenatal care. However, these physicians replaced nurse visits for prenatal care without increasing the overall number of visits women receive. We find no evidence of gains in widely used metrics of infant health, including birth weight, gestation, and infant mortality. Together, these findings provide suggestive evidence that physicians and nurses may be good substitutes in the production function of infant health. (*JEL* I11, I12, I18, J13, J16, J44, O15)

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 (IHS 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

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[1] Estimates by the World Health Organization (WHO) 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 9, 2018).
[2] For example, several reports of the World Health Organization (2014, 2010b) stress the importance of having an adequate supply of primary care physicians and other health workers to reduce inequalities in the access to basic medical care.

of compulsory services, financial incentives, and expansion of medical schools. Yet despite the widespread interest in increasing physician numbers, 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.

The focus on infants is compelling to understand the short-run health effects of the increased supply of physicians for at least two reasons. First, the months in utero are a very well-defined period and infants are particularly vulnerable to short-run changes in circumstances and events during that period. Thus, the relation between cause and effect is likely to be immediate, relative to measures of adult health that may reflect behaviors that occurred many years ago. Second, there is a widespread belief that increasing the number of physicians can be particularly effective in improving infant health and welfare. For example, an influential report of the US Institute of Medicine identified the limited access to primary care physicians in poor regions as one of the obstacles for developing policies to reduce disparities in birth weight (Institute of Medicine 1985). Relatedly, some have suggested that the shortfall in the supply of physicians in disadvantaged areas is an important reason why the "substantial expansions in Medicaid coverage of pregnant women during the 1980s resulted in relatively small reductions in infant mortality" (Gray 2001, 572). More recently in the context of the MPP, Minister of Health Arthur Chioro heralded the program as a success and concluded that after its introduction "there was an increase in the number of prenatal care visits, implying a decline in infant mortality rates and better health for mothers and babies" (quoted in Núcleo de Educação em Saúde Coletiva 2014). Similarly, Dr. David Oliveira de Souza, a director of Doctors Without Borders in Brazil from 2007 to 2010, stated in a famous letter:

> *It will be good to see you* [*MPP doctors*] *preventing congenital syphilis, a cause of severe sequelae in so many Brazilian babies, only because their mothers did not have access to a physician … It will be good to see the relief that poor mothers will feel when you prescribe antibiotics to their children after diagnosing pneumonia* [,..,] *gastroenteritis, asthma attacks and so many diagnoses for which the doctor and her stethoscope suffice.*
>
> —Folha de São Paulo (2013)

These claims are based in part on academic research showing a positive cross-sectional relationship between the number of primary care physicians and measures of infant health (Shi 1994; Vogel and Ackermann 1998; Shi et al. 2004; Macinko, Starfield, and Shi 2007). Given the important role that neonatal health plays in subsequent health conditions (Almond, Chay, and Lee 2005; Black, Devereux, and Salvanes 2007), cognitive ability (Figlio et al. 2014; Bharadwaj, Løken, and Neilson 2013), and labor market outcomes (Black, Devereux, and Salvanes 2007; Almond 2006), understanding the effects of physicians on infant health may have significant implications for the debate on the costs and benefits of physician distribution programs.

MPP represents an exceptional opportunity to study the effects of increased supply of physicians. To attract newly trained physicians to underserved areas, the program provided a considerable remuneration and an increase in the scoring of medical residency exams. A prominent feature of the program has been the collaboration of international agencies. In particular, the governments of Brazil and Cuba signed a cooperation agreement with the Pan American Health Organization (PAHO) to facilitate the large-scale participation of Cuban doctors. The PAHO assisted in the selection, transportation, and integration of Cuban doctors into healthcare teams in Brazil. Both the government of Cuba and PAHO received financial compensation as an amount paid by each Cuban physician enrolled in the program. By all accounts, the program was extremely successful in recruiting physicians. Between 2013 and 2016, approximately 18,000 primary care physicians were enrolled in the program, out of which 11,000 were the result of the cooperation agreement with Cuba and PAHO. As a whole, the number of recruited physicians represents as much as 60 percent of all physicians in BHUs in 2012. This makes MPP one of the fastest physician distribution program ever undertaken in the world (United Nations 2016).

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 given the 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 1,000 residents. Compared with the baseline mean of 0.67, this represents an increase of 18 percent. The size effect is much larger when we consider only primary care physicians. Indeed, we find that the program increased the supply of family physicians by as much as 40 percent. Overall, our calculations suggest large and equalization effects of the policy on the supply of physicians.

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

[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.

increases 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 gestation. Given the systematic shift in the healthcare provider from nurses to physicians, 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 0, 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 baseline characteristics of the municipality. Finally, there are no effects of the policy on infant mortality even when we examine different causes of death.

Several pieces of evidence support the identifying assumption that there were no major differential trends in unobservables correlated with MPP's implementation. Most importantly, we estimate event-study specifications and show that treated and untreated areas have similar trends in all the outcomes of interest before policy adoption. We also show that our estimates are in general insensitive to controlling for region-specific time trends and for linear trends interacted with a wide range of pretreatment characteristics. Moreover, there is no evidence that MPP coincides with differential changes across municipalities in other health resources. Finally, we find that policy adoption is not correlated with maternal characteristics, fetal deaths, birth rates, or sex ratios, casting doubt on the possibility that changes in the composition of women giving births influence our results.

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 prenatal/postnatal 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, Carr-Hill, and Dixon 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, Guanais, and de Souza 2006; Bhalotra, Rocha, and Soares 2016). Other studies also provide suggestive evidence that nurse/midwife care can be beneficial for infant health (Pettersson-Lidbom 2015; Hjort, Sølvsten, Wüst 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 or midwife care, then that might have limited effects on infant health relative to when they replace "nothing."

This paper is also related to a large literature linking provider supply and healthcare utilization. Previous research has found mixed evidence on this relationship. Early studies have found positive effects of physician on utilization rates, interpreting it as evidence of physician-induced demand or improved availability (Fuchs 1978, Wilensky and Rossiter 1983, Cromwell and Mitchell 1986, Busato and Künzi 2008). Other bodies of work, however, do not always support a positive relationship between physician supply and utilization outcomes (Carlsen and Grytten 1998, Grytten and Sørensen 2001). For example, Grytten and Sørensen (2001) find no evidence that greater physician supply is associated with more primary care visits even for physicians who have explicit financial incentives to provide extra medical care. However, many of the existing studies have focused on cross-sectional comparisons, which might be subject to omitted variable bias from unobserved shocks that can affect both provider supply and healthcare utilization. Studies that aim to have a more causal interpretation are rare and often focus on the role of nurses and physician assistants (Stange 2014, Traczynski and Udalova 2018). To the best of our knowledge, the present study is the first to exploit a quasi-experimental design to investigate the relationship between primary care physicians and healthcare utilization.

Finally, our paper adds to a broad literature on the effects of interventions aimed at improving access to prenatal/postnatal care services and reducing health disparities. This literature has typically focused on demand-side reforms, including free or subsidized health insurance (Currie and Gruber 1996; Miller, Pinto, and Vera-Hernández 2013; Camacho and Conover 2013; Conti and Ginja 2016; Goodman-Bacon 2018), conditional cash transfer programs (Barham 2011, Aizer et al. 2016), subsidized food programs (Almond, Hoynes, and Schanzenbach 2011; Aizer et al. 2016), and reduced out-of-pocket medical payments (Gruber, Hendren, and Townsend 2014; Limwattananon et al. 2015). Much less evidence is available on the effects of policies affecting primarily the supply side of the market. Some exceptions have focused on the establishment of mother and child healthcare centers (Bütikofer, Løken, and Salvanes 2017) and community-based health interventions (Rocha and Soares 2010, Cesur et al. 2017). Most closely related to our study are the contributions of Currie, Gruber, and Fischer (1995) and Iizuka and Watanabe (2016). Currie, Gruber, and Fischer (1995) finds that increased fees paid to physicians are associated with lower infant mortality rates in the United States. Iizuka and

Watanabe (2016) documents that a policy, which reduced the number of hospital physicians, is associated with poorer health outcomes in Japan. However, this result is somewhat difficult to interpret given that, as they show, the policy also led to a reduced availability of other important health resources, such as hospitals.

I. Policy Background

A. *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.[4] Two years later, a series of laws created the current health system, which provides healthcare through private and public, government-dependent institutions. Consumers can use services in both public and private sectors, depending on their ability to pay. On the side of the public sector, the government introduced the Unified Health System (SUS, by its acronym in Portuguese), which provides access to preventive and curative care fully financed and administrated by the central government. These services are provided free-of-charge (no co-payments, coinsurance, deductibles, or other fees) to all citizens through a network of healthcare centers and hospitals. An important feature of the SUS is the decentralization of 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 are a responsibility at the federal level. In turn, municipalities are responsible for managing and providing primary healthcare 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 care through the Ministry of Health, while the other segments of the population depended largely on philanthropic institutions and out-of-pocket expenses.

Individuals can receive services in any public health facility by presenting only an identification card. The SUS provides primary and preventive health services through community health centers or Basic Health Units (BHU).[5] The goal of these public centers is to provide preventive and curative care services without the need for referral to other services, such as hospitals. The services provided in BHUs include monitoring of pregnant women, diagnostic services, immunization, health education services, treatment of simple diseases, and screening for chronic and noncommunicable diseases. The cases for which the health practitioner could not solve a problem with the resources available are referred to secondary care centers or hospitals. The BHUs are easily accessible as they are located within neighborhoods. In 2012, there was 1 BHU for every 5,000 individuals.

[4] This section draws primarily on Paim et al. (2017) and Victora et al. (2011), who provide a detailed description of the Brazilian health system.

[5] In practice, while BHUs are specialized in providing primary and preventive health services, individuals can also opt for hospitals to receive the same services.

Physicians, nurses, and assistant nurses are responsible for the provision of healthcare in BHUs. In 2012, physicians represented 20 percent of all health workforce in BHUs, with a high fraction being clinician/general practitioners (27 percent) and family doctors (40 percent), followed by gynecologists (15 percent), and pediatricians (6 percent).[6] Nurses and assistant nurses play a prominent role in the provision of basic healthcare, representing 25 and 55 percent of the health workforce in the same year, respectively. In fact, to meet the growing demand for care as a result of the new health system, the government promoted primary care mainly through nurses and assistant nurses. Since 2005, these health workers are present in virtually all municipalities and communities (Rocha and Soares 2010), and thus they have been important to provide basic care in areas underserved by doctors.

Consumers who opt for private care can receive ambulatory and hospital services either through out-of-pocket payments or private health insurance plans. The private health market operates via contractual arrangements that can include a variety of healthcare services. The plans generally include at least the same primary and preventive services offered in BHUs. Insured individuals received medical care in private doctors' offices and hospitals. The demand for health insurances is mostly from some public and private companies that offer insurance coverage to their employees. The insurance market has played an important role in the Brazilian health system. In 2008, about 26 percent of the population had private health insurance (Paim et al. 2017). The private and public sectors are partially interconnected, since some services of the SUS are provided by outsourcing to private providers. These services typically include the treatment of complex and rare diseases in secondary care centers or hospitals.

B. *The More Physicians 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 20 percent of municipalities have a physician rate over 1, and 5 percent present a 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. 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 US$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

[6] The remaining 12 percent corresponds to physicians who have some other medical specialty and practice primary care medicine in BHUs.

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).[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. 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.[8] 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 a 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, Brazilian doctors receive priority.[9]

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

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

[9] 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.

Table 1—Program Take-Up

Priority criteria	Eligible municipalities	Treated municipalities	Percent
Extreme poverty rate over 20 percent	1,708	1,393	81.56
100 municipalities with more than 80,000 inhabitants	98	93	94.90
Indigenous special sanitary district	34	34	100
Other	2,680	2,612	97.46
Total	4,520	4,132	91.42

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.

Under the cooperation agreement, PAHO acts as an intermediary and assists in the selection, transportation, and integration of Cuban doctors into healthcare teams in Brazil. The salary of doctors is directly passed on to the government of Cuba (through the PAHO), which in turn passes between 40 and 50 percent of this money to the doctors. PAHO receives a commission of 5 percent of the total value of salaries. This cooperation agreement is known as the "package" of doctors sold by the Cuban government to Brazil (BBC Brazil, September, 2013). So, the program was a "win-win" for both countries. Cuba received about US$270 million a year from the Brazilian government.[10] The cooperation agreement was key to the success of the program in the recruitment of doctors. Between 2013 and 2016, approximately 18,000 primary care physicians were enrolled in the program, out of which 11,000 were the result of the cooperation agreement with Cuba and PAHO. As a whole, the number of recruited physicians represents as much as 60 percent of all physicians in BHUs in 2012. The participation of Cuban doctors is more remarkable in the first year of the program, when more than 80 percent of the recruited physicians were the resulted of the cooperation agreement with PAHO. This makes MPP one of the fastest physician distribution programs ever undertaken in the world (United Nations 2016). The United Nations has called the program a success story and argued that it is "replicable and would potentially be beneficial in any country that decides to adopt it" (United Nations 2016, 40).[11]

[10] The good diplomatic relations between Cuba and Brazil could also have contributed to the rapid implementation of the agreement. Specifically, Cuban president Raul Castro has in recent years implemented gradual reforms, and Brazil has played a major role in the country's redevelopment. For example, Brazilian exports to Cuba have quadrupled over the past decade and the country has become a top food supplier to Cuba (Zahniser and Cooke 2015).

[11] Political incentives of the Brazilian government may also have contributed to the rapid implementation of the program. A year after the introduction of the program, President Dilma Rousseff faced a reelection year. Her administration was associated with a variety of problems, including corruption, inflation, and government mismanagement of the economy. Thus, by sending a massive number of doctors to needy areas through the MPP, perhaps Rousseff hoped to improve her popularity indicators and increase her chances of reelection in 2014. Indeed, the popular press described the MPP as one of Rousseff's best hopes to gain support in the 2014 reelection campaign (e.g., Pereira 2013).

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 US$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.

C. *Expected Effects of More Physicians Program*

MPP could affect utilization through several channels. Increased availability of primary care physicians may increase utilization among individuals who were previously underserved because they were not able to find a doctor. Furthermore, increases in physician supply may lead to greater utilization among individuals previously served if there are reductions in waiting times, and thereby patients' opportunity costs of doctor visits. Prior to the MPP, prolonged waiting times and overcrowding in BHUs were the most frequently noted cause of problems to receive primary care, with some patients leaving without seeing a doctor and others being induced to pay for private services to receive timely ambulatory care (Pereira de Figueiredo and Rossoni 2008; Spedo, Rodrigues da Silva Pinto, and Yoshimi Tanaka 2010). Thus, if MPP improved waiting times and overcrowding in BHUs, then it would be plausible to expect gains in utilization. However, healthcare utilization is not just a patient decision, it depends also on physician outreach efforts. Since MPP physicians are public employees with limited incentives to provide extra medical care or induce demand for their services, they may exert low effort and not see all patients. Consequently, increases in the supply of physicians as result of MPP might not necessarily lead to greater ambulatory visits.

The effects of MPP on infant health outcomes depend on whether it led to greater utilization of doctors, and on the effectiveness of physician care relative to alternative sources of care. Prenatal care is a major channel through which MPP physicians could have affected birth outcomes and infant mortality. Prenatal care services provided by MPP physicians and other healthcare practitioners follow specific guidelines established by the Ministry of Health (Ministério da Saúde 2006). It recommends pregnant women to have at least six prenatal care visits, one in the first trimester, two in the second trimester, and three in the third trimester of pregnancy. At the first prenatal care visit, the health provider performs a number of diagnostic tests to determine sexually transmitted diseases, asymptomatic bacteriuria, iron levels, and Rh compatibility. This initial assessment allows physicians to identify potential risk factors for the pregnancy, and monitor changes in the health status of the mother. In the second trimester, uterine height is measured to assess whether fetal growth is compatible with the time of gestation, and blood pressure and glucose levels are measured to check for hypertension and gestational diabetes. Screening tests are also performed to find any health problems that could affect the health of the fetus, such as infectious diseases and physical abnormalities. In the third trimester, the healthcare practitioner continues to monitor blood pressure and weight gain, as well as baby's heartbeat and movements. Mothers are also tested for the presence of group-B streptococcus, a common bacterium in adults that can infect babies during vaginal delivery and become a serious illness. During every antenatal visit, the medical provider emphasizes the importance of certain iron and folic acid supplements to prevent maternal anemia and reduce the risk of low birth weight and preterm neonates. Moreover, the healthcare practitioner helps mothers to understand the risk of alcohol, tobacco, and illegal substances to the health of the fetus.

Changes in postnatal care visits could also affect infant survival. Postnatal visits provide information about the benefits of adopting good food and water hygiene, encourage the use of inexpensive methods to timely treat infectious diseases, and help mothers understand the risk of not breastfeeding. If physicians influence existing knowledge or practices to prevent and treat infectious diseases such as diarrhea, then it may result in reduced mortality rates among infants (Prüss-Ustün et al. 2006, Hisar and Hisar 2012, Spears 2012). In addition, physicians may increase survival among infants by helping parents get earlier diagnosis and timely treatment of lethal diseases.

If increases in the supply of physicians lead to greater utilization of prenatal/postnatal care visits, and that care is effective in improving neonatal health, then MPP could result in better birth outcomes and reduced infant mortality rate. However, the Law Decree no. 94406/1987 establishes that prenatal care can also be fully provided by nurses and midwives, and, similarly, postnatal care could also be provided by these healthcare practitioners when a doctor is not available. So, many pregnant women already received care from these healthcare practitioners prior to the MPP. Consequently, increases in the supply of physicians may simply cause women to shift from nurse to physician care without an increase in the overall number of visits they receive. A series of studies conducted in Brazil have shown that the reforms introduced in the mid-1990s, which improved the access to prenatal care services provided mainly by nurses, led to gains in birth outcomes and infant mortality (Macinko, Guanais, and de Souza 2006; Bhalotra, Rocha, and Soares 2016).[12] Thus, shifts in the providers of care from nurses to physicians will have positive effects on newborn health outcomes only if the quality of care provided by physicians is significantly higher to that provided by nurses. This could occur if physicians are better able to diagnose and identify risk factors for the mother or infants, or if physicians are more effective in promoting good health behaviors. Consistant with this idea, qualitative evidence suggests that patients place greater value on health-related advice when they consult physicians rather than nurses (Caldow et al. 2007, Redsell et al. 2007). Alternatively, if the quality of care provided by physicians is lower relative to that provided by nurses, then MPP could even lead to worse neonatal health outcomes.

[12] The findings of these studies suggest that increased access to prenatal care leads to improved birth outcomes, which is consistent with various medical studies (Blondel and Marshall 1998; Raatikainen, Heiskanen, and Heinonen 2007; Cox et al. 2011). However, a series of studies conducted in the economics literature do not always find that prenatal care is associated with better neonatal outcomes in the United States or Canada (Evans and Lien 2005, Goodman-Bacon 2018).

D. *Factors Associated with Program Adoption*

As discussed above, the Ministry of Health defined eligible areas based on demographic and socioeconomic characteristics. If treated and untreated areas differ substantially in important factors, then it may increase the risk of differential trends in the outcomes of interest driven by other factors. To explore this issue, we compiled a set of geographic and pretreatment socioeconomic characteristics of municipalities, which are described in more detail in online Appendix Table A.1. 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 online Appendix Tables A.3–A.4. We find that MPP adoption is significantly associated with the number of physicians. On average, municipalities with lower pre-MPP physician rates 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.[13]

Although these results suggest some significant effects of predetermined characteristics 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. Moreover, 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. Furthermore, all the independent variables included in the regressions explain only 15 percent of the total variation in program adoption, leaving a substantial portion of variation unexplained.

II. Data and Estimation

A. *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.[14] We supplement these data files with information from Vital Statistics of Brazil, available for the 2008–2015 period, to analyze MPP's overall

[13] In particular, the *Bolsa Familia* program is a major social policy under 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 ten years before MPP implementation.

[14] We do have information prior to 2008, but there is a series of issues that limit 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 onward.

impacts on infant health.[15] 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.[16] We use bimonthly variation in our analysis because monthly data are noisy, particularly for infant health measures.[17] For each panel dataset of the outcome variable of interest, we exclude those municipalities with zero observations during the complete study period.[18] 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 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 the type of health facility (e.g., BHU versus hospital), the source of funding (public or private), and description of medical equipment. 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 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. 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. In terms of medical specialty, 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

[15] 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.

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

[17] We use "bimonth" to refer to a two-month period. In addition, the use of bi-monthly variation considerably reduces the computational burden.

[18] 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 have 0 observations during the entire study period.

of the visit, patient's age, the medical care facility, and health professional involved. All private and public 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 care and doctor visits. Using the 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. 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. 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.[19] 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 one, 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 Revision (ICD-10), created by the World Health Organization (2010a). 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.[20] 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, 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 2,500 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.[21]

Second, we consider mortality within one year of birth, an appealing measure of severe health problems and an outcome of direct interest for policymakers. We also examine different cause-specific mortality rates. If physicians are particularly effective in promoting good food and water hygiene practices, then one would expect

[19] 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.

[20] Information on death coverage from SIM are available at http://tabnet.datasus.gov.br/cgi/sim/ dados/cid10_indice.htm (last accessed on June 22, 2017).

[21] 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, Morley, and Cole 1998; Matte et al. 2001). See Currie (2009) for a very comprehensive review of this literature.

to see significant reductions in infant mortality due to infectious and parasitic diseases, and respiratory conditions. Similarly, if prenatal care provided by physicians result in better neonatal health, then MPP could have reduced infant mortality due to abnormalities and conditions originating in the perinatal period.[22] Thus, to explore the relationship between MPP and infant death by cause, we group our sample into 5 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. Online Appendix Table A.1 describes in more detail the sources of all these variables.

B. *Summary Statistics*

The sample means of key variables in each of the datasets used in this study are shown in online Appendix Table A.2. 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 largely driven 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 we estimate more formally below.

Online 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.

[22] In particular, perinatal conditions include disorders relating to low birth weight and short gestation, and hematological disorders of fetus and newborn, which are sensitive to prenatal environmental conditions (Almond, Chay, and Lee 2005; Oreopoulos et al. 2008).

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. Aside from confirming these patterns, online 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 provide below more formal evidence that the increase in doctor visits is unlikely to be the result of pre-existing differential trends.

Online 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 clear evidence that treated municipalities experienced a greater reduction in infant mortality rates 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.

C. *Estimation Strategy*

We employ a differences-in-differences design to estimate the effects of MPP on physicians, patient care, and infant health outcomes. The high take-up of the program means that an "intention-to-treat" estimation strategy should yield estimates nearly equal to an approach that compares directly treated and untreated areas over time. Thus, we follow the latter strategy to produce more precise estimates of the treatment effect:

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

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 β measures 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 four 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.[23] All our models use robust standard errors adjusted for clustering at the municipality level to account for serial correlation (Bertrand, Duflo, and Mullainathan 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

$$(2) \qquad y_{ibt} = \alpha + \sum_{k=-2}^{-K} \beta_{pre}^{k} \mathbf{1}[D_{bt} = -k] \times Treatment_i + \sum_{k=0}^{K} \beta_{post}^{k} \mathbf{1}[D_{bt} = k] \times Treatment_i + \gamma time \times Z_i + \eta_i + \mu_{bt} + \xi_{ibt},$$

where $\mathbf{1}[D_{bt} = \cdot]$ 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_{pre}^{k} and β_{post}^{k}, which represent the effects of the program relative to one bimonth prior to MPP before and after policy adoption. Thus, this specification allows us to test for differences in effects by the 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. As shown above, disadvantaged areas were more likely to adopt MPP than advantaged areas. Although these differences tend to be small in magnitude, one may still be concerned if there are differences in trends in these characteristics spuriously correlated with the treatment effect. This could occur if disadvantaged and advantaged areas were already being on different paths in the outcomes of interest prior to MPP. For example, if poorer municipalities were converging to richer ones in terms of income or any other potential determinant of infant health, then it may lead to overestimates of the program's

[23] 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 online Appendix Table A.19.

impacts on infant health. To account for this possible threat to internal validity, we control for interactions of a wide range of pretreatment 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).[24] 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. As is evident from online Appendix Figure A.1, 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 unsurprising 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.

III. Relationship between MPP and Physicians

A. *Results for Overall Physician Rate*

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 corresponds 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 percent 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 ten and persists

[24] We also estimate models that include state-specific linear time trends based on the pretreatment data only, and the results are, in general, extremely similar to our baseline.

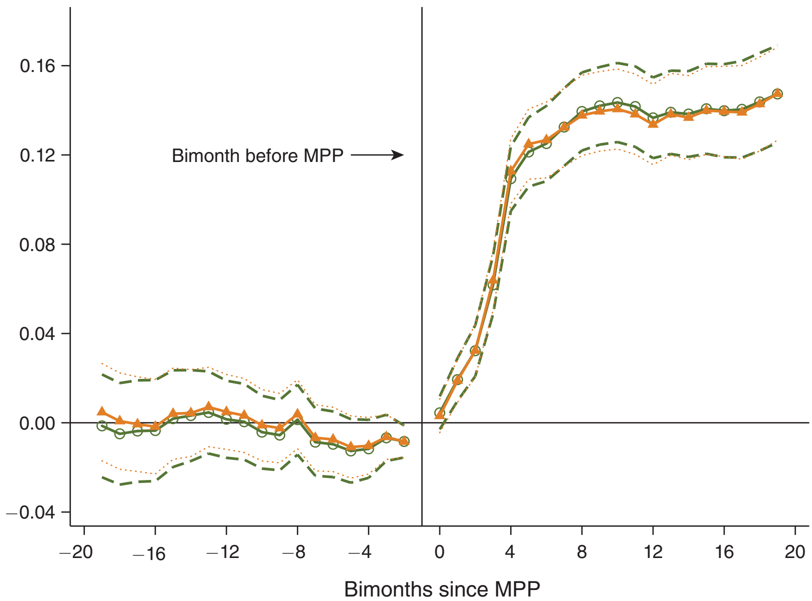

Figure 1. Effects of MPP on Physicians

Notes: These are event studies for the number of physicians per 1,000 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.

for the rest of the post-intervention period. Importantly, there were 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 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

Table 2—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
Observations	300,024	290,304	286,416	285,012	285,012
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 parentheses) are clustered at the municipality level.

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.

B. *Relationship between MPP and Physicians by Source of Funding and Specialty*

In Table 3 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 1,000, 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 were 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. Recall that MPP physicians provide their services in BHUs, so size effects should be larger when examining only the supply of physicians in these health facilities. One could also 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

Table 3—The Effect of MPP on Physicians—Heterogeneities

	Funding		Site		Specialty				
	Public (1)	Private (2)	BHU (3)	Other (4)	Family (5)	Clinicians (6)	Gynecologist (7)	Pediatricians (8)	Other (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
Observations	285,012	115,236	285,012	285,012	280,260	266,814	140,292	100,161	125,705

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 parentheses) are clustered at the municipality level.

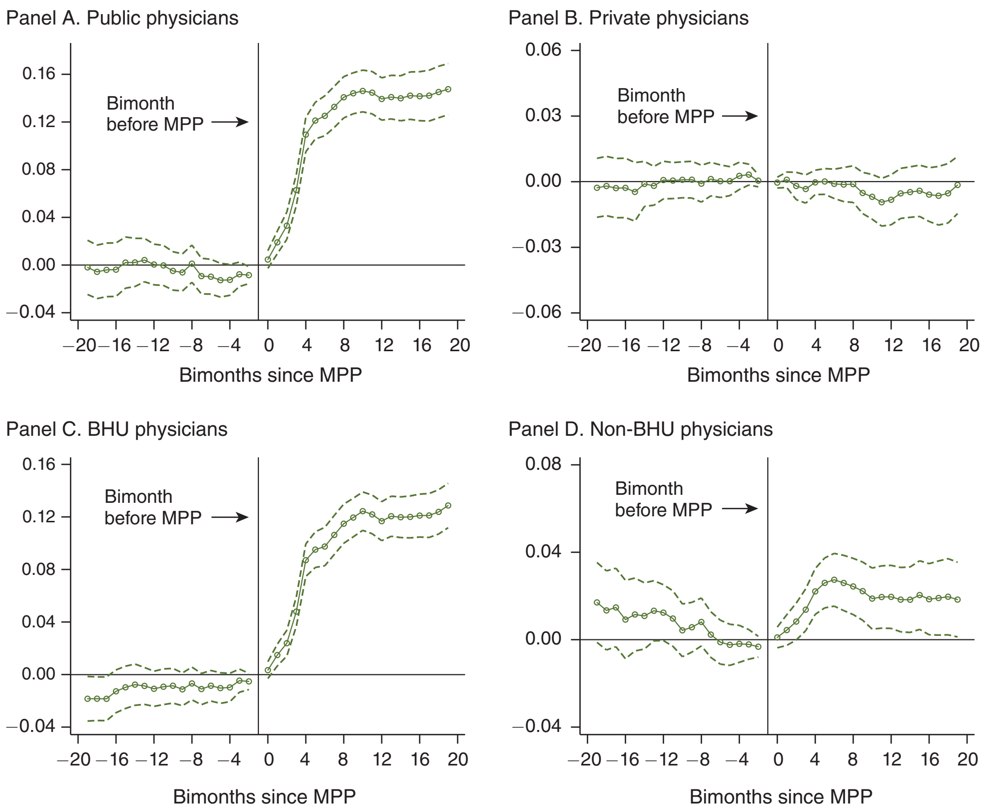

Figure 2. Effects of MPP on Physician Groups

Notes: These are event studies for physician outcomes (per 1,000 residents). The coefficients are estimates of β^k_{pre} and β^k_{post} of equation (2). 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 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.

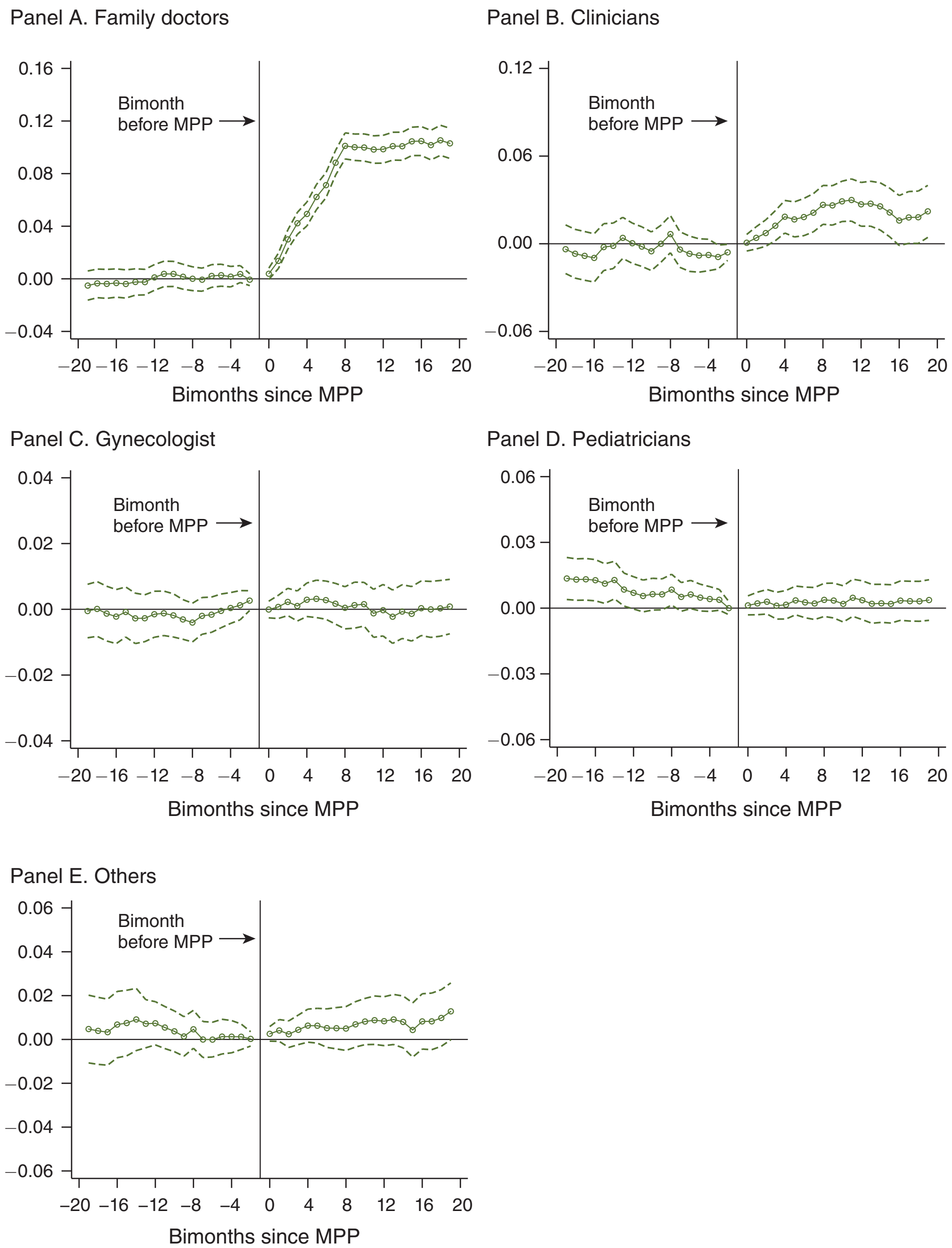

Figure 3. Effects of MPP on Physician by Specialty

Notes: These are event studies for physician outcomes (per 1,000 residents). The coefficients are estimates of β^k_{pre} and β^k_{post} of equation (2). 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 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.

facilities after MPP implementation. Column 3 reveals a strong and statistically significant effect of the program on physicians in BHUs, with an estimated coefficient of 0.105 and a 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 which the parameter is estimated, we can 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 for the total number of physician. 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 large 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 further put this result in perspective, the mean difference in BHU physicians during the pre-intervention period between treated and comparison areas is about 0.09 per 1,000 residents, suggesting that MPP introduction virtually closed the gap between both groups. As highlighted earlier, online 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). These results suggest that MPP led to increases in the rate of family physicians by about 0.075 (standard error $=$ 0.004). This estimated effect is large in magnitude relative to a pre-MPP mean of 0.18. The event-study graph in Figure 3, panel A shows substantial heterogeneities with respect to the 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 from the baseline mean). We also find 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.[25]

[25] 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

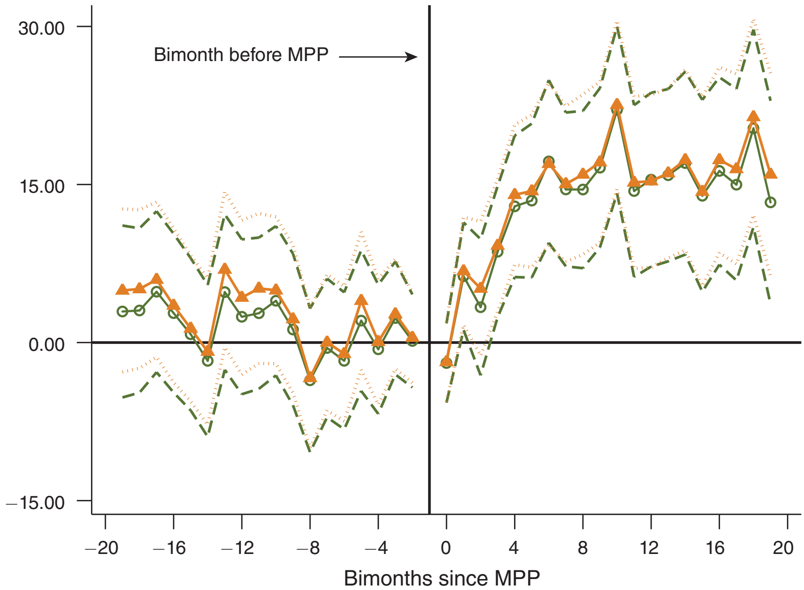

Figure 4. Effects of MPP on Doctor Visits

Notes: These are event studies for the number of doctor visits (per 1,000 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.

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.

IV. Relationship between MPP and Utilization

A. *Results for 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 4 plots the coefficients and 95 percent confidence intervals from estimating the event-studies

(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 3.

Table 4—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
Observations	300,510	290,790	286,416	285,012	285,012
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 parentheses) are clustered at the municipality level.

for the number of doctor visits per 1,000 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 3 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 2 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), we find that MPP led to an average increase of 11 ambulatory visits to physicians per 1,000 residents (Table 4, column 5).

We next estimate the changes in doctor visits by patient's age. These results are shown in Table 5 and Figure 5. 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. In general, the estimated effects are not statistically different across all age groups examined. The point estimates, however, are jointly informative and provides suggestive (but not definitive) evidence that the effects are somewhat more pronounced for infants under 1 year of age (a 10 percent increase), followed by children aged 1–5 (a 7 percent increase), and by individuals aged 15–40 (a 6.8 percent increase).

Overall, these patterns indicate that the introduction of MPP increased doctor visits. To interpret the magnitude of these results, we compute the implied marginal effect of physicians. Given that MPP led to an increase of 18 percent in physician rate, our calculations imply that a 1 percent increase in the number of doctors as

Table 5—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
Observations	285,012	285,012	285,012	285,012	285,012	285,012

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 parentheses) are clustered at the municipality level.

result of MPP would increase ambulatory visits to physicians by about 0.3 percent.[26] The magnitude of the effect is strikingly similar to that estimated in cross-sectional studies by Stano et al. (1985), who estimate an elasticity of 0.28 using US data, and by Busato and Künzi (2008), who find an elasticity of 0.30 for Switzerland. Other studies focusing on the demand for surgeons tend to find an elasticity with respect to provider supply ranging between 0.1 and 0.20 (Fuchs 1978, Wilensky and Rossiter 1983, Cromwell and Mitchell 1986).

B. *Results for Prenatal Care*

Figure 6 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 care 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 6, panel B reveal a

[26] While the identifying assumption of the difference-in-difference is sufficient to estimate the impact of the program, a causal interpretation of this elasticity requires that the program had no effects on the measures of health care utilization other than by increasing the supply of physicians. In particular, if MPP affected the quality of physicians by attracting younger and less experienced doctors, then this is likely to bias downward our estimated elasticity if physician quality positively affects utilization.

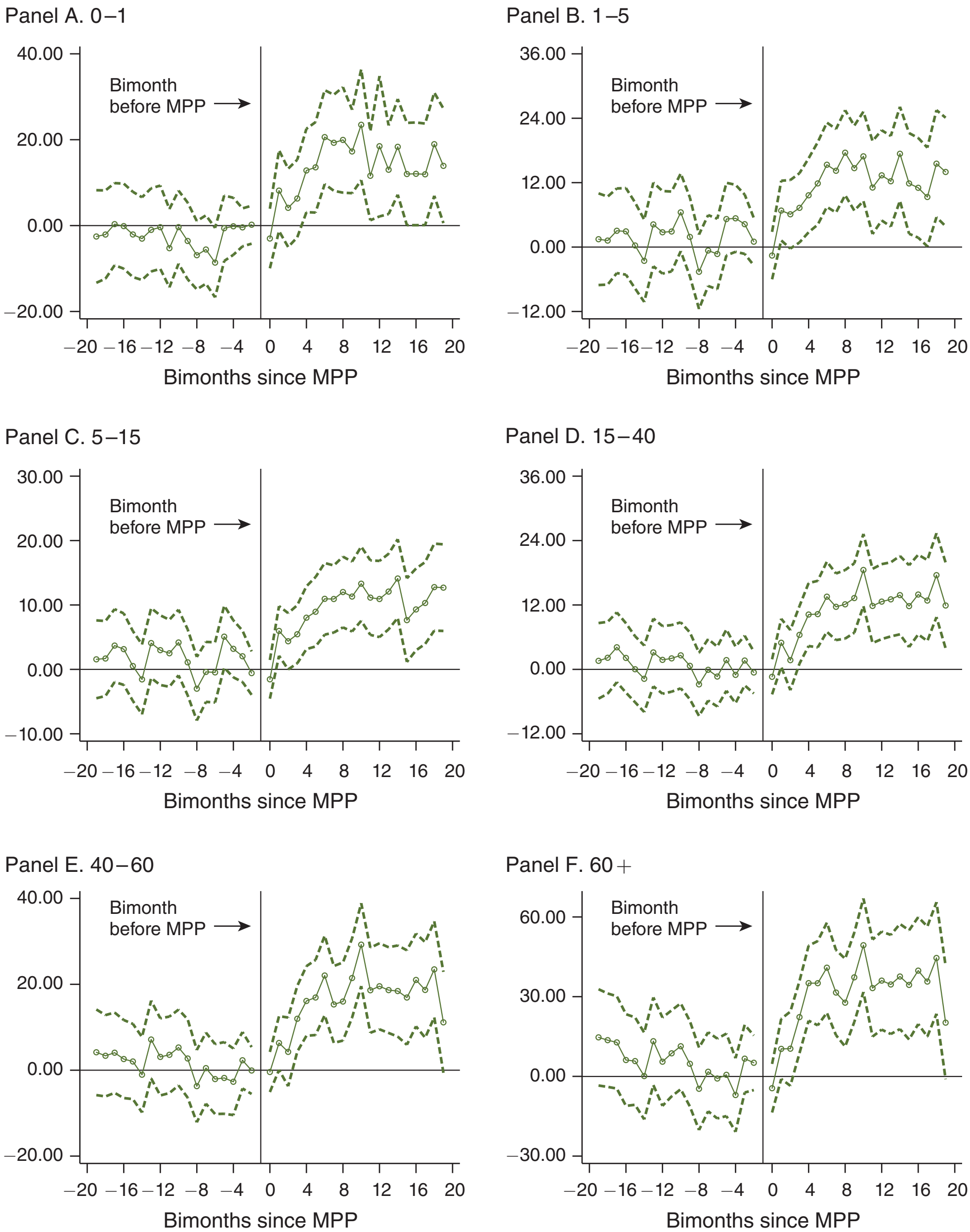

Figure 5. Effects of MPP on Doctor Visits by Age

Notes: These are event studies for visit outcomes (measured per 1,000 residents). The coefficients are estimates of β^k_{pre} and β^k_{post} of equation (2). 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 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.

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 1,000 or 4 percent relative to the pre-MPP mean. This increase becomes

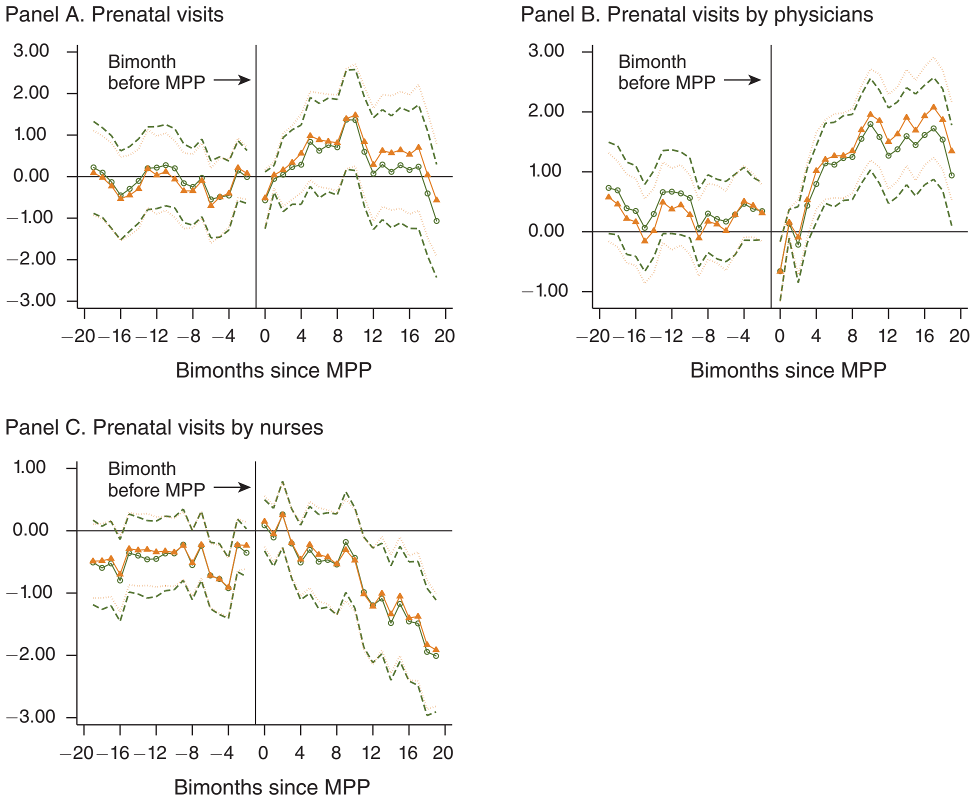

Figure 6. Effects of MPP on Prenatal Care

Notes: These are event studies for prenatal care visits (measured per 1,000 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 corresponds 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.

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 1,000. 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.

The results from estimating event-studies for the number of prenatal care visits by nurses are shown in Figure 6, 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 1,000, 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

Table 6—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
Observations	300,510	290,790	286,416	285,012	285,012
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
Observations	300,510	290,790	286,416	285,012	285,012
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
Observations	300,510	290,790	286,416	285,012	285,012
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 parentheses) are clustered at the municipality level.

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 was 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 receive is indistinguishable from zero.

V. Relationship between MPP and Infant Health

A. *Results for Overall Infant Health Outcomes*

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

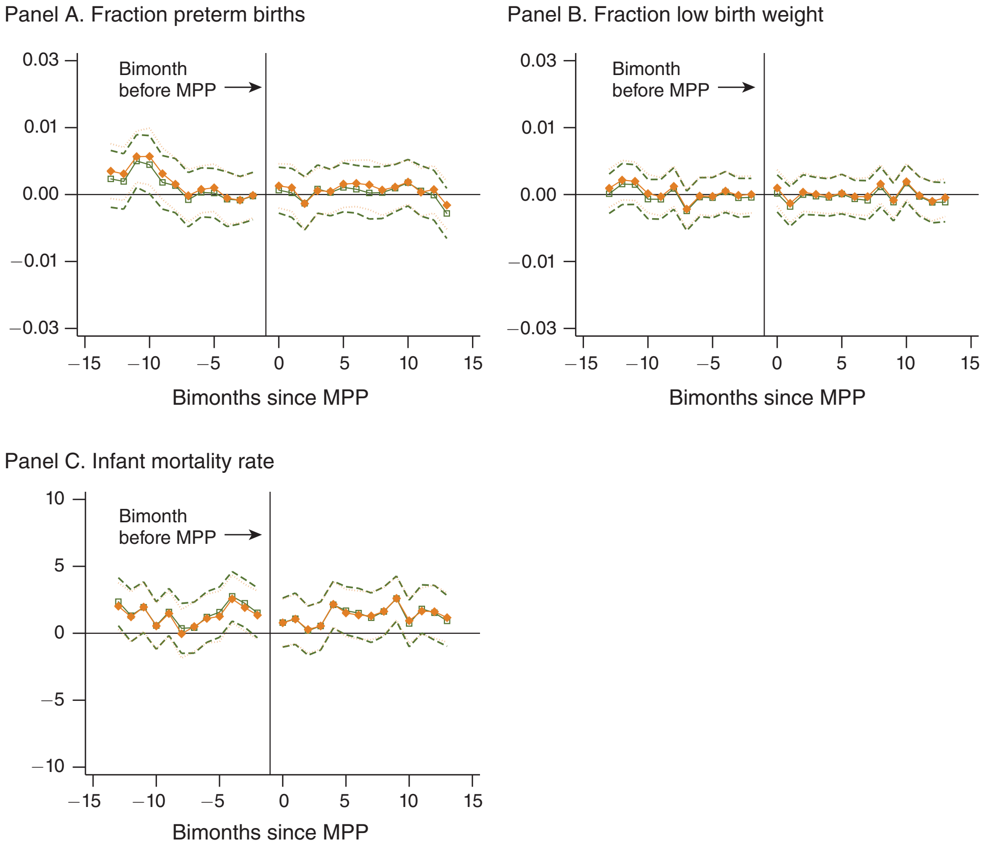

Figure 7. Effects of MPP on Infant Health

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 corresponds 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.

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 7 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 the full specification 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

Table 7—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
Observations	266,720	257,733	253,904	252,665	252,665	252,665
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
Observations	266,720	257,733	253,904	252,665	252,665	252,665
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
Observations	268,608	258,480	254,592	253,344	252,665	252,665
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 panels A and B are a proportion of preterm births and proportion of low birth weight babies, respectively. The 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 four years of schooling, and proportion of births by unmarried mothers. The observations are weighted by the number of births. Robust standard errors (reported in parentheses) are clustered at the municipality level.

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.

Online Appendix Table A.5 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

TABLE 8—THE EFFECT OF MPP ON INFANT HEALTH ACCORDING TO BABY'S SEX AND MATERNAL CHARACTERISTICS

	Male (1)	Female (2)	Mother's education < 4 years (3)	Mother's education > 4 years (4)	Unmarried (5)	Married (6)	Mother's age < 20 (7)	Mother's age > 20 (8)
Panel A. Prematurity								
Post × Treatment	0.0013 (0.0014)	−0.0004 (0.0015)	0.0031 (0.0046)	0.0000 (0.0013)	−0.001 (0.0017)	0.0007 (0.0015)	0.0027 (0.0020)	−0.0001 (0.0013)
R^2	0.248	0.227	0.115	0.325	0.235	0.224	0.172	0.3
Observations	248,825	248,292	175,136	251,872	242,246	248,638	230,857	251,424
Panel B. Low birth weight								
Post × Treatment	0.0009 (0.0008)	−0.001 (0.0009)	−0.0002 (0.0031)	−0.0003 (0.0007)	−0.0005 (0.0011)	−0.0007 (0.0009)	0.0006 (0.0014)	−0.0004 (0.0007)
R^2	0.073	0.078	0.051	0.115	0.084	0.076	0.049	0.116
Observations	248,825	248,292	175,136	251,872	242,246	248,638	230,857	251,424
Panel C. Infant mortality rate								
Post × Treatment	0.1418 (0.3407)	−0.0998 (0.3224)	−0.1126 (0.2259)	−0.0897 (0.2306)	–	–	−0.4316 (0.5402)	0.0198 (0.2687)
R^2	0.043	0.038	0.066	0.05			0.032	0.055
Observations	252,001	252,001	251,565	252,001			252,001	252,001

Notes: Each coefficient is from a different regression. Municipality and bimonth-by-year fixed effects are included in all specifications. Regressions also include 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 parentheses) are clustered at the municipality level.

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, online Appendix Table A.6 presents results 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.

B. *Results for Infant Health by Maternal Characteristics and Mortality by Cause of Death*

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 years), 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 into broad categories, we find only a marginally statistically significant effect

Table 9—The Effect of MPP on Infant Mortality Rate by Cause

	Infectious and parasitic diseases (1)	Respiratory diseases (2)	Perinatal conditions (3)	Congenital abnormalities (4)	Other diagnoses (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
Observations	252,001	252,001	252,001	252,001	252,001

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 Revision (ICD-10). Robust standard errors (reported in brackets) are clustered at the municipality level.

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 online Appendix Figure A.4 make clear the lack of a statistically significant correlation between MPP adoption and infant mortality by cause of death.

C. *Selective Mortality*

Next, we assess whether our results may be driven by selective mortality, an issue that emerges in any infant health analysis (Currie 2009). 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 online Appendix Table A.7. Column 1 of online Appendix Table A.7 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 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 online Appendix Figure A.5 show that our results are unlikely to be driven by substantial reductions in miscarriages and stillbirths.

D. *Composition of Births*

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. In online Appendix Table A.8 and Figure A.6, we test 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.

VI. Robustness Checks

In this section, we briefly discuss several potential threats to internal validity and robustness checks, including different estimation strategies, controls for region-specific time trends, changes in other health resources, and heterogeneous treatment effects.

A. *Matching Estimation Strategies*

One concern with our differences-in-differences framework is that it may confound the treatment effects with preexisting differences in time trends across treated and untreated areas. Although we do not find any evidence for significant differences in pre-trends, we use different matching techniques to increase the comparability of the municipalities and minimize the risk of differential trends in unobservables. To implement this additional test, we use either propensity score or Mahalanobis-Metric matching to identify similar pairs and then estimate differences-in-differences regressions across the matched sample. We also run 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, Fortin, and Lemieux 1996; Heckman et al. 1998).[27] In general, the results from these different estimation strategies are broadly similar to the baseline (see online Appendix Tables A.9–A.11).

B. *Controlling for Region-Specific Time Trends*

Our baseline results are based on a specification that includes state-specific linear time trends, which help control for preexisting differential trends across states in factors affecting the outcomes of interest. As another test for pre-existing time trends, we run models that include mesoregion (137) and microregion (586) linear time trends instead of state linear trends. Both mesoregion and microregion are levels of disaggregation finer than a state, so including these trends may be a more effective way of accounting for any pre-existing differential trends.[28] Columns 2–3 of Appendix Tables A.12–A.14 show that our results are robust to including either mesoregion-specific or microregion-specific linear time trends. As an alternative test, column 4 shows the results from 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.

C. *Changes in Other Health Resources*

We now evaluate whether the introduction of MPP coincided with changes in other health resources. This could be the case if, for example, MPP implementation encouraged municipality governments to increase local hospital size. Alternatively, local administrations might reduce health resources to increase the availability of public resources for other purposes. To address this concern, we examine the relationship between MPP and local health resources, including hospitals, hospital beds, ultrasound machines, dental equipment, and X-ray machines (all measured per 1,000 inhabitants). There is no evidence that the policy is associated with changes in these health resources (See online Appendix Figure A.7 and Table A.15).

D. *Heterogeneous Treatment Effects*

We also investigate if our results mask any heterogeneous effects with respect to the distribution of characteristics across treated and untreated areas. To do so, we stratify the sample according to the set of socioeconomic characteristics of municipalities. These results are shown in online Appendix Tables A.16–A.18.

[27] See online Appendix Figure A.8 for descriptive statistics on the distribution of propensity scores in treated and untreated municipalities. It shows the distribution of the probability of adopting MPP conditional on the set of geographical and socioeconomic characteristics.

[28] Mesoregion and microregion are subdivisions that aggregate 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.

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 the 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.

We do not find a consistent pattern when we consider infant health outcomes. 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.

VII. Final Remarks

This paper has offered new evidence on the extent to which increases in physician supply affect healthcare 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 of 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 increased doctor visits across all age groups and led to greater utilization of doctors for prenatal care. However, these physicians replaced nurse visits for prenatal care without increasing the overall number of visits women receive. As a result of this shift in the healthcare provider, one would have expected positive effects on infant health if the quality of care provided by physicians is significantly higher compared to that provided by nurses. Yet, our analysis reveals very little evidence that the program led to improvements in birth weight, gestation, or infant mortality. The estimates are precise enough to conclude that the returns to the program in terms of these widely used metrics of infant health must be small.

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, Carr-Hill, and Dixon 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, Kendall, and Goodman 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 the supply of primary care physicians and does not address the question of whether access to more specialized physicians is effective in improving infant health outcomes. For example, Currie, Gruber, and Fischer (1995) finds 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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