UNIVERSIDADE FEDERAL DO CEARÁ

20 CAEN PÓS-GRADUAÇÃO EM ECONOMIA

SÉRIE
ESTUDOS ECONÔMICOS CAEN

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

Bladimir Carrillo Bermúdez
José Gustavo Féres

FORTALEZA • ABRIL • 2017

UNIVERSIDADE FEDERAL DO CEARÁ

PROGRAMA DE PÓS-GRADUAÇÃO EM ECONOMIA - CAEN

SÉRIE ESTUDOS ECONÔMICOS – CAEN

Nº 20

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

FORTALEZA – CE

ABRIL – 2017

More Doctors, Better Health?

Evidence from a Physician Distribution Policy*

Bladimir Carrillo
Departamento de Economia Rural – DER/UFV
bladimir.bermudez@ufv.br.

Jose Feres
Instituto de Pesquisa Econômica e Aplicada – IPEA-RJ
jose.feres@ipea.gov.br

Abstract

In 2013, the Brazilian government implemented one of the largest physician distri- bution programs on record. Using a difference-in-difference framework, we document that the number of primary care physicians increased by 60 percent in treated areas. Despite this increased supply of physicians, we find little evidence that the program led to better infant health, measured by low birth weight, prematurity and infant mortal- ity. These findings are essentially the same across a wide range of subgroups. We find suggestive evidence that the absence of family responses to the program is the primary source of these results.

Keywords: Primary care physicians; infant health; policy evaluation

JEL Codes: I12, I18, I38.

* Support for this research was provided by the Brazilian National Council for Scientific and Techno-
logical Development. We thank Prashant Bharadwaj, Marcelo Braga, Danyelle Branco, Diogo Brito, Carlos Charris, Joan Cost-Font, Francisco Costa, Micheliana Costa, Guilherme Diniz, Randall Ellis, Miguel Foguel, Wescley Freitas, Luis Galvis, Donna Gilleskie, Wilman Iglesias, Robert Kaestner, Paulo Matos, Matt Notowidigdo, Juan Trujillo, Cristiana Tristao, Raul Velilla, Jonathan Skinner, Douglas Staiger, Frank Sloan, Ian Trotter, and participants at various conferences and seminars for helpful comments.

1 INTRODUCTION

Improving infant health in a cost-effective way is a basic concern for policy makers. A prominent debate has focused on whether the government should be intervening on the supply-side or directly on the demand-side to achieve this goal. Many previous studies conducted in a variety of countries convincingly show that demand-side changes, such as expanding health insurance, lead to gains in a number of infant health outcomes (Aizer, 2007; Camacho and Conover, 2013; Chou et al., 2014). Yet, despite its importance in the public debate, there is little careful empirical research on the role of policies affecting primarily the supply side of market, especially in developing countries. Establishing the causal effects of both demand-side and supply-side interventions is crucial for the most efficient design of policies. This paper uses a large physician distribution program to provide new insights on the role of primary care physicians for infant health.

It is unclear whether increasing the number of primary care physicians can improve health. Theory suggests that prices of care should go down with an increase in the supply of physicians, encouraging greater consumption of care. In turn, more physician consultations during pregnancy may reduce the risk of poor birth outcomes and infant deaths. However, many families in areas underserved by primary care physicians already receive care from nurses or other alternative sources. So, increasing the supply of physicians will not necessarily lead to improvements in infant health unless the quality of care is significantly higher in physician's offices. In addition, families may not increase the number of visits to doctors at all for different reasons, including for example the opportunity cost of working or lack of information. Thus, whether increasing the availability of primary care physicians in fact translates into better infant health is an empirical question.

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

Our empirical strategy relies on the fact that the program was implemented in a limited set of municipalities. We use a difference-in-difference framework that compares the out- comes of treated and untreated municipalities before and after implementation of MPP. The identifying assumption underlying this statistical approach is that the outcomes of treated and untreated areas would experience similar trends over time absent the MPP's implementation. Although MPP status is not randomly assigned, we show that the outcomes of interest as well as a set of observable characteristics of treated and untreated municipalities were similar before the MPP. We also provide other pieces of evidence supporting the identifying assumption.

While the program was successful in recruiting physicians, it is uncertain whether the program was effective in increasing the number of physicians serving in BHUs. Since Brazil operates under a decentralized scheme, governments at the municipality level have consider- able autonomy to make decisions in hiring and firing public workers. A Federal law prohibits local governments from terminating the contracts of physicians enrolled in the program, but they retain discretion over physicians not linked to the program. If local administrations have incentives to substitute current physicians for MPP physicians, the program may be unable to increase the availability of primary care physicians. The popular press suggests that this is the case (see Jornal Nacional, March 4, 2017), but no study has formally investigated the extent to which the program affected physician supply.

Our first contribution, therefore, is to measure the relationship between MPP implementation and physicians. To do so, we compile detailed administrative records on the universe of physicians and construct panel data files at the municipality level before and after policy adoption. Using the difference-in-difference estimator, we document that treated municipalities experienced an unprecedented increase in the number of physicians serving in BHUs. The results indicate that program adoption led to an immediate and statistically significant increase of 0.10 in the number of BHU physicians per 1,000 residents. To place these results in perspective, the pre-intervention mean of BHU physicians per 1,000 residents is about 0.20. In fact, the introduction of policy was able to eliminate the remarkable physician gap between untreated and treated areas. The rich nature of these data allows us to estimate the effect of MPP separately for different types of physicians. The increase in the number of physicians is largely driven by family doctors and clinician/general medical practitioners, which is consistent with the target of policy.

Having documented a strong and robust "first stage", we then study MPP's overall impacts on infant health outcomes. We find little evidence that the program led to gains in infant health, measured by infant mortality, low birth weight and prematurity. The effects on these outcomes can usually be bounded to a tight interval around zero. For instance, we can rule out effects of MPP on low birth weight smaller than 1 percent of a standard deviation. We continue to find virtually zero policy effects when stratifying the sample according to baby's sex, maternal characteristics and observable municipality characteristics. The only exception to this overall pattern of null results is infant mortality from infectious and parasitic diseases. We find suggestive evidence that the program decreased infant mortality from these conditions by 0.09 deaths per 1,000 live births. But deaths due to these diseases are relatively rare for infants, and thus the magnitude of this effect is small. Yet taken in their entirety, the findings of this paper provide little evidence that increasing the number of primary care physicians leads to improvements in infant health.

A natural concern regarding the infant health analysis is selective mortality. If MPP implementation led to significant reductions in miscarriages and stillbirths, "saving" in part marginal babies that are more likely to have poor health outcomes, this will bias our estimates toward zero. We address this issue by directly examining how policy adoption affected fetal deaths. We do not find any evidence that fetal deaths decreased after MPP implementation in treated areas relative to comparison municipalities, casting doubt on the hypothesis of selective mortality. Moreover, if there were substantial reductions in miscarriages and still- births associated to the MPP introduction, then we should 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 towards this when we examine the total number of births. We also evaluate whether policy is associated with a higher rate of male births, which should occur whether selective mortality is a salient issue, as predicted by literature on "fragile" males (Almond and Mazumder, 2011; Eriksson et al., 2010; Kraemer, 2000). We find that there are not significant effects of MPP on sex ratio at birth in treated municipalities. Together, these results suggest that our main findings are in fact not driven by selective mortality.

We also consider other robustness checks. There is no evidence that our main results are driven by spillovers across nearby areas. Indeed, health outcomes in neighbors of treated municipalities evolved similarly before and after policy adoption compared to their counter- parts. Furthermore, the results hardly change when we use a difference-in-difference strategy across matched pairs of municipalities or when the observations are reweighted either by weights that depend directly on the propensity score or distances to treatment observations. If anything, these different estimation techniques make our estimates less precise, but the qualitative nature of our results remains essentially the same.

We then seek to understand the mechanisms underlying these results. In doing so, we consider available measures of primary and preventive care which were assumed to be affected by the program. If the use of basic health services increased with policy adoption, this would suggest that low returns to primary care physicians relative to other sources of care drive the null effects of policy on infant health. Alternatively, insignificant effects on the utilization of care would indicate that the absence of parental responses must be an explanation. We find no evidence of a change in prenatal care use or vaccinations. Indeed, these outcomes evolved similarly in treated and untreated municipalities before and after policy implementation. Although we cannot entirely conclude that this is all that is going on, these results suggests that the absence of family responses to the program is the primary source of our findings.

Our results are inconsistent with the traditional view that doctors "create their own demand", also known as supplier induced demand (Cromwell and Mitchell, 1986; Grytten and Sørensen, 2001; McGuire and Pauly, 1991; Rice, 1983). This literature indicates that an increase in the number of physicians per capita reduces the availability of patients and leads physicians to induce demand for health care services to maintain their income[1]. Specific features of the Brazilian setting may explain this result. First, remuneration of physicians in public health facilities is independent of the quantity and quality of services offered, so they do not have particular financial incentives for providing extra medical care to population. Second, one could think that language barriers may also have played a role in the demand for care in doctor's offices, since more than 50 percent of MPP physicians come from non-Portuguese speaking countries. If a significant fraction of women is reluctant to be attended by foreign physicians, then it may explain why MPP had no discernible effects on prenatal care visits. In any case, our findings are important for the development of policies that seek to improve infant health by reducing the geographic distribution imbalance of primary care physicians.

More generally, this study addresses the absence of quasi-experimental evidence on the effect of physicians on infant health in an emerging country context. Research on the causal effects of physicians is particularly relevant in middle-and low-income countries because these governments have fewer resources to implement public health policies. The few existing studies for the developing world have typically relied on cross-sectional comparisons between region-specific physician numbers and health outcomes while controlling for observable socio- economic and regional characteristics (Anyanwu and Erhijakpor, 2009; Frankenberg, 1995; Kamiya, 2010). Most of these studies find that increased supply of physician is associated with significant reductions in infant mortality. While often the best evidence available, these cross-sectional comparisons do not necessarily imply causation. If health professionals have strong preferences for working in more developed regions, where there are often more educated and higher quality parents, then standard techniques that fail to account for this sorting may substantially exaggerate the health benefits of physicians.

[1] This literature is based on the hypothesis that physicians have considerable influence on the extent and number of consultations as well as the quality of treatment provided (Grytten and Sørensen, 2001). In fact, literature has emphasized this as a factor explaining the increase in medical care spending in the U.S (Newhouse, 1992).

While previous research has focused on developed countries, the number of studies trying to exploit quasi-experimental variation in physician supply is limited even within these contexts.[2].Indeed, we are aware of only two such studies. The first one is that of Currie *et al*. (1995), who explore the impacts of fees paid to physicians on infant mortality in the United States. They find that policy is associated with lower infant mortality rates. Differently from the MPP, these authors focused on a policy that directly affected physician's incentives to provide extra medical care, which might have different implications than simply increasing the number of physicians in a given area. We are also aware of a study by Iizuka and Watanabe (2016), who provide estimates of the effect of a change in physician policy on infant health in Japan. They find that a large decline in the supply of hospital physicians is associated with poorer health outcomes. These results, however, are difficult to interpret because, as they show, the policy also led to significant reductions in other important health resources, including the number of hospitals open.

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

2 POLICY BACKGROUND

2.1 Brazilian Health System

The creation of the current health system was gradual, beginning with recognition of health- care as a citizen's right in the 1988 Brazilian constitution. Two years later, a series of laws created the Unified Health System (SUS, by its acronym in Portuguese), which provides free, universal access to preventive and curative care. A major innovation of the SUS was to decentralize health policy, where the different spheres of government (i.e., at the federal, state, and municipality level) have specific responsibilities in the provision of health services. The development and financing of national health policies is a responsibility at the federal level. In turn, municipalities are responsible for managing and providing primary health care services, while states provide technical and financial assistance. The creation of the SUS represented an unprecedented change in health policy. Prior to the SUS, only formal workers received health care provided by the Ministry of Health, while the other segments of population depended largely on philanthropic institutions and out-of-pocket expenses.[3]

The Basic Health Units (BHU) are public health centers through which the SUS provides accessible, affordable and primary health care. The goal of these centers is to provide health care

[2] Examples of studies that provide correlational evidence on the relationship between physicians and health for high-income countries include Aakvik and Holmas (2006), Auster et al. (1969) and Starfield (1991).
[3] See Paim et al. (2017) for a detailed review of the history of the Brazilian health system.

services to individuals, without the need for referral to other services, such as hospitals. The main services offered are prenatal care, general medical consultations, inhalations, injections, curatives, vaccines, collection of laboratory exams, dental treatment, and provision of basic medication. In 2012, there was one BHU for every 5,000 individuals.

2.2 The More Physician Program

Brazil is a developing country characterized by a highly unequal distribution of physicians. In 2012, the number of physicians per 1,000 residents was 1.6, but that figure was below 0.46 in 50 percent of municipalities, and 15 percent have a physician rate lower than 0.20. Only twenty percent of municipalities have a physician rate over 1, and five percent present a physician rate over 2. The number of physicians per capita was the lowest in the poorer, less populous and more distant municipalities. To place these figures in perspective, the average physician rate across the OECD countries is 3.7.

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

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

i) Extreme poverty rate over 20 percent;
ii) Being among the 100 municipalities with more than 80,000 inhabitants;
iii) Being located in the area of action of the Indigenous Special Sanitary District (ISSD);[4]

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

The Federal law 12,871/2013 allowed for eligible municipalities to voluntarily join the program. The remuneration of 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 to physicians. Program take-up was high, with the vast majority of eligible municipalities choosing to attend. Out of all 5,570 Brazilian municipalities, the program was implemented in approximately 4,102.

Having defined target areas, it establishes the maximum number of physicians that will be allowed to participate in the program based on the capacity of each municipality. A Law establishes an order of priority to select the physicians who will participate in the program. The participation is first offered to Brazilian and foreign physicians registered with the Regional Medical Council (CRM).[5] If vacancies remain after the choice of this first group, they will be offered to a second group, composed of Brazilian doctors trained abroad. The remaining vacancies are offered to a third group of foreign doctors trained outside the country.[6] If vacancies persist even after they are offered to these three groups, the Ministry of Health is allowed to make cooperation agreements with international organizations to fill the remaining positions.

Physicians registered with the CRM had priority in choosing the municipality in which they would carry out their responsibilities. In the case of foreign physicians who were trained abroad, the Ministry of Health chooses the municipality in which they would be allocated.

Participation conditions were flexible for foreigners. They were not asked to take Portuguese proficiency examinations for participation.

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

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

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

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

annual number of newly graduated physicians for satisfying the demand for these health professionals.

The program was extremely successful in recruiting physicians. Between 2013 and 2016, more than 18,000 physicians were enrolled in the program, a figure as large as the 60 percent of all physicians in BHUs during 2012. While this figure is suggestive, it is still unclear whether the goal of increasing the availability of BHU physicians was met. As hinted in the Introduction, the autonomy of local governments may jeopardize such a goal. In particular, some municipalities may have incentives to substitute current physicians for MPP physicians to increase the availability of resources for other purposes. The popular press suggests that this was the case, arguing that the increase in the number of BHU physicians was much lower than expected. Indeed, between 2013 and 2016, the number of BHU physicians increased by 11,000, a number 38 percent lower than expected. Although suggestive, this aggregated figure may simply reflect the fact that some physicians who already were in a BHU prior to policy implementation decided to join the program. In addition, that figure may be little informative on policy impacts because the supply of physicians may have increased during this period independently of MPP. We return to this issue below.

2.3 Factors Associated with Program Adoption

As discussed above, the Ministry of Health defined eligible areas based on demographic and socioeconomic characteristics. If variation in adoption is systematically related to municipality characteristics that are associated with differential trends in the outcomes of interest, then this could lead to spurious estimates of the effects of the program. To explore this issue, we compiled a set of geographic and pretreatment socioeconomic characteristics of municipalities, which are described in more detail in Appendix Table A1. We then use these predetermined characteristics to predict the probability that the municipality adopted the program by using probit and OLS regression models.

We present the results in Appendix Tables A3-A4. We find that MPP adoption is significantly associated with the number of physicians. On average, municipalities with lower pre-MPP physician rate are more likely to implement the program, which is consistent with the target of the policy. Program adoption was also more likely in the poorer and more populous municipalities. In addition, those municipalities that are part of legal Amazon region and municipalities with higher rural population share are also more likely to participate in the program. There is also a statistically significant positive association between local spending on Bolsa Família and program implementation.[7] We also find that a large set of characteristics do not have a statistically significant effect on treatment probability, including for example indigenous population rate, unemployment rate, Gini index and all geographic characteristics.

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

3 DATA AND EMPIRICAL APPROACH

3.1 Data

We use the microdata from the National System of Birth Records (SINASC), the National System of Mortality Records (SIM) and the National System of Health Facility Records (CNES). The first two databases are available for the period 2008-2015, while the last one is available for the 2008-2016 period.[8] Local administrations are responsible for collecting all these data and sending them to the Ministry of Health, which consolidates them finally in the SINASC, SIM or CNES databases. The first two data are collected yearly, while the latter is recorded monthly. Using these data, we assemble different municipality-level panel data files. We use bimonthly variation in our analysis because monthly data are noisy, particularly for infant health variables.[9,10] However, our results are very similar when we use monthly variation.[11]

---

[7] In particular, the Bolsa Familia program is a major social policy in which poor families receive a monthly cash transfer conditional on school attendance and health center visits. The monthly cash transfer from Bolsa Familia is equivalent to 40 percent of the monthly minimum wage.
[8] These data are available for years prior to 2008, but we do not include them in our analysis in an effort to focus on changes in outcomes that occurred around the MPP implementation. SINASC and SIM data were not available for 2016 when this study was conducted.
[9] We use “bimonth” to refer to a two-month period.
[10] In addition, the use of bimonthly variation considerably reduces the computational burden.
[11] See Appendix Table A13.

The SINASC is a rich source of data that covers all births in Brazil. This database includes information on exact birthdate, weeks of gestation, baby's sex, birth weight, and maternal characteristics such as marital status, age and education. 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.[12] Following the birth outcome literature, we focus on low birth weight (defined as birth weight less than 2500 grams) and prematurity (defined as gestation less than 38 weeks). Based on the information of the municipality in which the mother lives, we construct a municipality-level panel data file on birth records. We also aggregate maternal characteristics at the municipality-by-bimonth-by-year level to use them as controls in our main regressions.

The SIM provides comprehensive information on date and cause of death, birth date, race and gender, and mother's characteristics (education and age) are also provided for individuals who were under one year at death. 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 (WHO, 2010). The laws governing the collection of the death certificates are national and no burial can be performed without a death certificate. The SIM covers over 96 percent of all annual deaths inferred from demographic census.[13] Using this dataset, we focus on infant deaths per 1,000 live births, using again the municipality in which the mother lives as reference. To examine potential heterogeneities, we group causes of deaths into broad, mutually exclusive categories: infectious and parasitic diseases (4.7 percent), respiratory system diseases (5.2 percent), perinatal conditions (58 percent), congenital abnormalities (20 percent), and other diagnoses (12.1 percent).

Physician data come from the CNES. This database is basically a census of all public and private health facilities with detailed information on their human resources. Since MPP aims to increase the quantity of physicians in BHUs, we focus on the number of such physicians in each municipality over time. Other outcomes that we examine include physicians in private health facilities, and physicians in public health facilities (excluding those of BHUs). Exploring these outcomes may inform us on potential MPP externalities. There are municipalities with zero observations during the entire study period. This is rare for physicians in BHUs and other public health facilities (less than 3 percent), but more noticeable for physicians in private health facilities (about 60 percent). In a given panel dataset of physicians, we exclude those municipalities with zero observations during the

---

[12] 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.
[13] Information on death coverage from SIM are available at http://tabnet.datasus.gov.br/cgi/sim/ dados/cid10_indice.htm.

complete study period. Finally, to investigate the physicians most affected by the program, we assemble municipality-level panel datasets for different types of physicians in BHUs. The types of physicians we consider are gynecologists, clinicians, family physicians, pediatricians, and all other types.

The source for program data is from the Ministry of Health. We obtained individual records on all participant physicians, with information on whether the MPP contract ended and (if so) end date, municipality in which the physician was placed, and some demographic characteristics such as birthdate, sex, and country of origin. With this information, it is straightforward to identify treated municipalities. In addition, using these records and CNES data, we create a municipality-level panel of BHU physicians who were external to the program. Put differently, this is a panel of physicians who were not associated with the program during the post-intervention period. We use this outcome to assess potential externalities of program.

Additionally, we have a rich set of municipality-specific characteristics. These include GDP, percentage of indigenous population, Gini index, unemployment rate, illiteracy rate, share of rural population, number of inhabitants, social spending, and a set of geographical controls. We include interactions between this set of characteristics and time trends in our regressions to control for possible differences in trends across municipalities that are correlated to the municipality treatment effect.[14] For some of the time-varying characteristics, there is information for several years prior to policy implementation, so we can use them to test for differential pretreatment trends between untreated and treated areas. Appendix Table A1 describes the sources of these variables.

The sample means of key variables in each of the datasets used in this study are shown in Table A2. Physicians are measured per 1,000 residents. The average ratio of BHU physicians is 0.24, with a standard deviation of 0.27. Remarkably, there is a striking difference in this outcome pre- versus post-intervention period. The average during the pre-MPP period is 0.21, while the average during the post-MPP period is 0.30. This relatively large increase seems to be driven largely by those municipalities implementing the program. Indeed, the pre-MPP and post-MPP difference in this ratio among untreated municipalities is approximately 0.02, while among treated areas that figure is about 0.12 a difference of 0.10. This certainly crude difference-in-difference is almost identical to the MPP effects that we below estimate more formally.

On average, there are 87 births per municipality. About 9.5 percent of the births are classified as premature and 8.7 percent have a weight less than 2,500 grams. Six percent of babies are born to mothers with three or less years of schooling, and fifty percent are born to unmarried

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

mothers. Mothers are, on average, 25 years old. On average, there are 13 infant deaths per 1,000 live births. During the study period, there were about 300,000 infant deaths and 24 million births. Appendix Figures A2 through A4 show the evolution of birth and infant deaths outcomes over time in treated and untreated areas. During the pre-policy period, the trends in these outcomes look quite similar between both groups. There are no clear trends in birth outcomes, while infant mortality rate has declined over time.

3.2 Empirical Approach

To identify the relationship between MPP, physician and infant outcomes, we use the following specification:

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

where *outcome* is the dependent variable of interest, either an infant health or physician measure, for municipality *i* in bimonth *b* and year *t*. The independent variable of interest is the interaction of *T reatmenti*, which is an indicator variable for whether the municipality *i* adopted the program, and "Post", which denotes post-intervention observations starting September/October 2013. The covariates *Zi* interacted with a linear trend time include a set of pre-intervention municipality characteristics. Some specifications include state linear time trends. When the dependent variable is an infant health outcome, we control for maternal characteristics in addition.

The models include municipality fixed effects (*ηi*), which absorb any unobservable time-invariant factors, including initial conditions and persistent municipality characteristics such as geography, transport infrastructure and area-specific risks of diseases. Year-by-bimonth fixed effects (*µbt*) 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.[15] All our models use robust standard errors adjusted for clustering at the municipality level to account for serial correlation (Bertrand et al., 2004).

The coefficient *β* measures the effect of MPP on the outcome of interest. The primary identifying assumption of our statistical approach is that in the absence of the MPP, municipalities in the treatment and control groups would have experienced the same proportional changes in the outcomes of interest. Note that the inclusion of municipality and time fixed effects will strip out any time-invariant municipality-level factors and overall trends that might affect the outcomes. The

[15] We also estimate models that include municipality-by-bimonth fixed effects and find very similar results. For the interested reader, these results are presented in Appendix Table A12.

identifying assumption would be violated only if there were differential trends in time-varying determinants of outcomes across treated and untreated areas. By including differential trends parameterized as functions of a number of municipality- specific baseline characteristics, we control for observable determinants of MPP adoption that might be associated with differential trends in the outcomes of interest. However, as we show below, our results are not sensitive to the inclusion of these parameterized trends. More importantly, we provide several pieces of evidence supporting the identifying assumption that there were no major differential trends across municipalities that are spuriously correlated with the treatment effect.

Although the identifying assumption is not directly testable, we can use data prior to policy implementation to assess its plausibility. 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. Thus, we use pre-intervention data to estimate differential time trends in outcomes for treatment and control groups. We do this by estimating regressions on time trends interacted with a dummy for treated municipalities. If treated and untreated areas were on the same trends, the coefficient of this interaction variable should not be statistically significant.

The results of this straightforward exercise are presented in Table 1. The specification includes a time trend to allow for systematic general trends, and municipality fixed effects are also included to account for time-invariant unobservable characteristics. The estimates indicate that there is a time trend for some outcomes, but such a trend is statistically identical across treatment and control groups. Further, the estimated coefficients are small in magnitude. For instance, the mean trend is an annual decrease of 0.001 in preterm births, which is nearly 21 times larger than the estimated coefficient on the interaction term (in absolute value).

Appendix Table A5 provides an analogous exercise for other time-varying characteristics available in the vital statistics data. Columns (1)-(3) repeat the analysis using maternal characteristics as dependent variables. Column (4) examines the total number of births, while columns (5)-(6) consider fetal death outcomes. Columns (7)-(8) test for differential pre-trends in sex ratio at birth and prenatal visits. In no case are there statistically significant differences in pre-trends across treated and comparison areas.

To provide a more complete view of the plausibility of the identifying assumption, Appendix Table A6 also estimates differential time trends for a number of socioeconomic characteristics for which data were readily available at the municipality level during the pre-MPP period. Columns (1)-(4) show pre-trend checks for spending on Bolsa Família, education and health, and GDP, respectively. Testing for differential pre-trends in spending outcomes is useful because these variables may capture different dimensions of local policy that potentially affect the outcomes of interest. Examining GDP is also of special interest since it is a reasonable proxy for local development. Columns (5)-(8) consider a set of health infrastructure characteristics, including hospitals, beds, dental equipment and mammograms. Out of eight estimated coefficients of interest, none is statistically significant. Moreover, the magnitudes are very small. For instance, the estimated coefficient on the interaction of the trend in spending on health is only one-thirteenth as large as the annual mean trend in this outcome (0.003/0.069=1/13).

4 RESULTS

4.1 Effects of MPP on Physicians

We begin by examining graphically the relationship between policy adoption and the supply of physicians. We estimate an event-study version of equation (1) that includes indicators for nineteen bimonths before and after MPP adoption, interacted with the treatment group dummy. The bimonth zero is September/October 2013, when the policy was implemented. Figure 1 shows the event-study graph for BHU physicians, our main outcome of interest, plotting the respective coefficients and 95 percent confidence intervals. It provides an opportunity to better judge the validity of the difference-in-difference empirical design. The figure shows that, before the introduction of the program, there are no statically significant differential trends in BHU physicians. In the post-intervention period, there is a clear pat- tern indicating that the number of BHU physicians increased much more rapidly in treated municipalities than in the comparison group. Moreover, the event-study shows that such differential increase occurred immediately after implementation, peaking at the bimonth 10, and persisting for the rest of the post-intervention period. The lack of a statistically significant pre-trend is consistent with the findings in Section 3 and yields further support for the identifying assumption that the treatment and control municipalities would have experience similar changes in physician rate in the absence of MPP adoption.

We also provide an analogous event-study for BHU physicians external to the program. The idea underlying this exercise is to assess the extent to which the program may have caused a "crowding-out" of physicians who were already working in a BHU prior to policy implementation. Certainly, these physicians may have joined the program during the post- intervention period and this may confuse program externalities with a purely mechanical effect. But if this occurred with the same probability for physicians in treated and untreated areas, then our difference-in-difference estimator will rule out such a mechanical effect. Remember that physicians in a treated municipality prior to policy were not allowed to join the program unless they were willing to be placed in a different municipality. This should diminish the possibility that these physicians are more likely to join the program relative to those physicians in untreated areas. While imperfect, this exercise may provide some initial evaluation of program externalities.

Figure 2 indicates that policy adoption is associated with a significant reduction in the number of BHU physicians who were external to the program. Indeed, treated municipalities experienced more rapid reductions in the number of non-MPP physicians relative to the pre-intervention period and comparison areas. This differential decline appears to emerge around bimonth 4 after the introduction of program, peaking at bimonth 13, and persisting for approximately eighteen bimonths. This visual evidence is consistent with a significant negative externality of the program on physicians. However, such an externality was not large enough to annihilate policy effects on the supply of primary care physicians. Indeed, Figure 1 shows large and persistent positive effects of program on the total number of BHU physicians.

Appendix Figures A6 through A8 report similar event-studies for different physician measures. We consider the number of public physicians, which include physicians in BHUs, hospitals or specialized clinics. The evidence for this outcome is consistent with the results in Figure 1. There does not appear to be a difference in the trend of these physicians during the pre-intervention period, but there is a marked divergence between treated and control municipalities after the introduction of program. The fact that the timing and magnitude of the estimated effects are very similar to that of BHU physicians suggests that this latter group of physicians is driving the aggregate results. We also examine public physicians, but excluding those from BHUs. By doing so, we find that policy adoption is associated with a small increase in the number of these physicians. This result may indicate some evidence for reallocation of physicians who were not linked to the program. Finally, Appendix Figure A8 shows no evidence that policy adoption is associated with changes in the number of physicians in private health facilities.

While the figures provide compelling evidence, we also formally present the regression results. Table 2 reports the estimates from equation (1), which confirms the graphical evidence. In addition to municipality and time fixed effects, column (1) controls for interactions between linear trends and municipality characteristics. The effect of MPP on BHU physicians is positive and significant (Panel A). The estimated coefficient implies that policy adoption resulted in a statically significant increase of 0.10 BHU physicians per 1,000 residents. The rate of BHU physicians in the treatment group increased by 0.12 over this period, so MPP is responsible for more than 80 percent of this increase. There seem to have been other factors causing increases in BHU physicians, but the bulk of the increases are the ones associated to the program. The results are consistent across different specifications (columns (2)-(7)). Controlling for specific state linear time trends and other differential trends, parameterized as functions of various observable baseline characteristics, leaves the estimated coefficient of interest virtually identical, which further points to the robustness of the finding. This provides very reassuring evidence that the results are unlikely to be driven by differential trends across treated and comparison municipalities.

The estimated effect is large. The results indicate that the number of physicians increased by 0.10 per 1,000 residents, relative to a pre-MPP mean of 0.20. To put this result in perspective, the mean difference in BHU physicians during the pre-intervention period between treated and comparison areas is 0.09 per 1,000 residents, suggesting that MPP introduction virtually eliminates the gap between both groups. Appendix Figure A1 provides visual evidence for this convergence process, showing that the rate of physicians between treated and untreated becomes equal in the post-intervention period.

Next, we examine the effect of policy on other physician outcomes. Panel B considers the effects of MPP on BHU physicians not linked to the program, finding a statistically significant reduction associated to the program. Indeed, policy adoption is associated to 0.5 decline in the number of these physicians. When we consider all public physicians, except those from BHUs, we find a positive coefficient on the interaction term of interest but far from statistically significant (Panel C). Panel D shows that there is no statistically significant effect of policy on the supply of private physicians. Moreover, the estimated coefficient of interest is very small in magnitude relative to the pre-MPP mean. This body of results is very robust to the inclusion of addition controls.

Table 3 examines the effects of MPP on BHU physicians according to medical specialty. The results suggest that MPP increases the rate of family physicians by about 0.096 (column (1)). The estimated effect is considerable. Relative to the pre-MPP mean of 0.12, the results indicate that the introduction of the program almost doubled the rate of family physicians. We also find evidence that the program increased clinicians per 1,000 inhabitants by 0.02 - a 21- percent increase at the pre-MPP mean. Columns (3)-(5) show a lack of correlation between MPP and pediatricians, gynecologist and all other types of BHU physicians. Overall, we find the effects of the largest magnitude for family physicians and clinicians.

The findings in this section suggest that policy implementation led to a large and robust increase in the number of BHU physicians. These effects are largely driven by family doctors and clinicians, with no evidence for other medical specialties. We take these results as evidence of a strong “first stage” that MPP increased the supply of primary care physicians in treated areas.

4.2 Effects of MPP on Infant Health

Having confirmed that MPP led to a sizeable increase in the supply of primary care physicians, we turn to the analysis of infant health outcomes, namely low birth weight, prematurity and infant mortality. Figures 3 through 5 plot the coefficients and 95 percent confidence intervals from event-studies that include indicators for 13 bimonths before and after of policy adoption, interacted with an indicator for “treatment”. The omitted category is the bimonth 2 prior to policy. The figures reveal that during the pretreatment period, the trends in all infant health outcomes we considered were in general similar between treated and untreated areas. Moreover, the vast majority of estimated coefficients are small in magnitude. Yet, the event-studies show no evidence for a change in the trends between treated and untreated municipalities during the post-intervention period, suggesting that policy adoption did not affect infant health outcomes.

Table 4 presents the corresponding difference-in-difference results from estimating equation (1). We find no statistically significant effects of the program on any of these infant health measures, confirming the graphical evidence. In addition, the estimated coefficients are very small in magnitude. For instance, the estimated coefficient of interest for prematurity is 0.0004, relative to a pre-MPP of 0.11. Importantly, note that these results are not driven by large standard errors. Indeed, our estimates suggest policy effects on these outcomes that can be bounded to a tight interval around zero. For example, we can rule out effects of MPP on low birth weight smaller than 1 percent of a standard deviation.

Appendix Table A7 explores potential heterogeneities according to baby’s sex and maternal characteristics. The results separately by gender do not reveal any evidence for significant effects of policy on infant health. We also stratify the sample by mother’s education (low and high education), mother’s age (< 20 yrs.) and marital status (unmarried and married). Across all these subsamples, we continue to find extremely small estimates tightly bound around zero.

Table 5 examines mortality results by cause of death. When we group causes of death into broad categories, we find only a marginally statistically significant effect of policy for infectious and parasitic diseases. The difference-in-difference estimate of -0.09 (standard error=0.05) implies that MPP introduction reduced infant mortality rates from this category by 0.09 deaths per 1,000 births. Although somewhat imprecisely estimated, the size of this effect is relatively large: 0.09 decline is about 10 percent of the pre-MPP mean infectious and parasitic mortality rate. But this disease-cause of mortality accounts for only a small portion of all infant deaths, so it does not make a difference in the aggregate result. Point estimates for other causes of deaths are not statistically significant and quantitatively very small relative to the pre-intervention mean.

Appendix Table A8 assumes that MPP was implemented in 2014 and allows the effects to vary over time. Again, there is no evidence that policy leads to better infant health. Point estimates are small and not statistically significant at the conventional levels of significance. One could argue that noise in bimonthly observations affects the precision with which the parameters of interest are estimated and thus the ability to detect significant effects. To examine this possibility, Appendix Table A9 presents the results of estimating the effects of policy using data aggregated at the municipality-by-year level. This exercise assumes that the program was implemented in 2014 and allows the effects to vary over time. All estimates continue to be indistinguishable from zero, suggesting no evidence for an effect of MPP on infant health.

Our focus is on the direct effect of MPP, but the policy could also affect other areas. For example, families might benefit from an increased supply of physicians in nearby areas if it implies a reduced waiting time and thus a lower relative cost of doctor visits. Although it seems unlikely that such spillovers are large enough to drive our main results, we assess whether they existed. Appendix Table A14 examines spillovers considering contiguous areas as neighbors. This analysis explores the average measures of infant health outcomes in the nearby municipalities. We find limited evidence of spillovers, with coefficients that are statistically insignificant and small in magnitude.

Overall, the results suggest that the health returns to the program are, at best, small. Next, we assess whether our results may be driven by selective mortality, an issue that emerges in any infant health analysis.[16] 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 Table 6. Column (1) explores policy effects on fetal death rate, which is calculated dividing fetal deaths by the number of potential births (births plus fetal deaths). Column (2) considers the natural logarithm of fetal deaths. Irrespective of how the dependent variable is measured, we find no evidence that policy led to reduced fetal deaths. Given this result, it is unsurprisingly 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). Columns (4) looks at the number of live births in natural logarithms. If the introduction of the program led to substantial reductions in miscarriages and stillbirths, then we should observe a higher number of live births during the post-intervention period

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

in treated areas relative to the comparison group. We do not find any evidence that this is the case. This is also true when we consider an expanded measure of births that includes fetal deaths (column (5)). 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. Columns (6)-(7) show no effect of policy on sex ratio at birth or the percentage of male 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. Appendix Table A15 tests this assumption by examining whether observable maternal characteristics changed after policy implementation in treated areas relative to comparison municipalities. Specifically, we run difference-in-difference regressions where maternal characteristics are dependent variables. If there were no compositional changes, point estimates on these regressions should be statistically insignificant and close to zero. This is exactly what we find. Aside from helping us rule out changes in the composition of women giving births, this result provides further indirect evidence that fetal selection is not a major issue in our setting.

4.3 Effects of MPP on Prenatal Care and Vaccination Coverage

Examining the use of primary and preventive care services may provide insights into the overall pattern of null health benefits of MPP. If the use of basic health services increased with policy implementation, then this would be evidence that low returns to primary care physicians relative to alternative sources of care drive the lack of effect of MPP on infant health. Alternatively, insignificant effects on the use of health services would suggest that the absence of parental responses to the program must be an explanation.

We then study the effects of MPP on prenatal care visits and vaccination coverage. These health investments are directly linked to the use of primary and preventive health services offered in BHUs. Exploring these outcomes is of interest in its own right. A significant body of work provides evidence that prenatal care visits are predictive of low birth weight and pre- maturity (Conway and Deb, 2005; Joyce, 1999; Klerman et al., 2001). In our data, attending at least four prenatal visits is correlated with 80 and 70 percent decrease in the probability of low birth weight and prematurity, respectively.[17] At the same time, vaccinations such as polio and measles have been shown to be

[17] We use all individual birth records and estimate OLS regressions of the relationship between the birth outcomes and attending at least four prenatal visits, condition on mother's education, mother's age, mother's civil status, baby's sex, and fixed effects for state and year of birth.

effective in preventing ill health and infant mortality, especially in developing countries where social safety nets are often limited.[18] We do not observe other measures of visit behaviors, but many of them vary plausibly in the same manner.

We begin by investigating whether prenatal care visits increased in treated areas relative to comparison municipalities. Appendix Figure A9 plots the coefficients and 95 percent confidence intervals from estimating an event-study version of equation (1) for prenatal care. We use the fraction prenatal care visits over four as our outcome of interest (the WHO-recommended minimum number of prenatal visits). There is no visual evidence of an increase in prenatal consultations in treated areas relative to comparison areas during the post-intervention period. The estimated coefficients fluctuate around zero and thus are not statistically significant.

Table 7 presents regressions results of the effect of MPP on prenatal care. When we include a limited set of interactions between municipality characteristics and linear time trends, we find a marginally statistically significant effect of MPP on prenatal care visits. The point estimate from this specification suggests that policy adoption led to a 0.39 percentage point increase in the fraction of mothers attending at least four prenatal visits. However, relative to the pre-intervention mean, this is a very small effect of less than 0.4 percent. When other controls are included, the coefficient of interest becomes slightly smaller and statistically insignificant.

We now investigate whether policy adoption increased immunization coverage in treated areas compared to control municipalities. We then use municipality-year API (National Program on Immunization) vaccination data for the 2008-2016 period. These vaccines include hepatitis B, BCG (against tuberculosis), MMR (measles, mumps, and rubella), rotavirus, polio, and yellow fever. Coverage for each outcome is measured as the number of doses divided by 1,000 infants. Appendix Figures A10 through A15 present the results from estimating event-study models. There is no graphical evidence indicating that MPP is associated with a statically significant increase in the use of these health services. All estimates are statistically insignificant, both before and after the introduction of program. In Table 8, we present formal difference-in-difference regressions of the MPP effects on immunization coverage. Confirming the graphical evidence, we do not find statically significant effects of the program on immunization.

Overall, Tables 7 and 8 suggest that the use of primary and preventive health services did not change after policy implementation in treated areas relative to comparison municipalities. This suggests that the absence of a parental response to the program may drive the overall pattern of null

[18] See, for example, BenYishay and Kranker (2015), Aaby et al. (2010, 2005), and Moulton et al. (2005).

health benefits we observe. Of course, this evidence is only suggestive, since we do not observe visits to BHUs.

4.4 Further Results and Robustness

We perform a number of other specification checks to test the robustness of our main results. In Appendix Tables A16-A17, we use different matching techniques to create similar treated and control municipalities, and estimate difference-in-difference regressions across this matched sample. To identify similar pairs, we use either propensity score or Mahalanobis-Metric matching. We also implement difference-in-difference regressions that reweight the observations either by entropy-weights (Hainmueller, 2012), or by weights that depend on the propensity score or distances to treatment observations (DiNardo et al., 1996; Heckman et al., 1998).[19] The results are broadly similar to the baseline across these different estimation strategies.

Appendix Tables A18-A19 present the results from specifications that include and mesoregion (137) and microregion (586) linear time trends instead of state linear trends.[20] For ease of comparison, column (1) in each table replicates the baseline specification. The findings continue to be essentially the same compared to the baseline. Column (4) uses a specification that includes rather state-by-bimonth-by-year fixed effects (27 states, 6 bimonths, 8/9 years). Our results are very robust even using this more demanding specification, leaving the coefficients of interest statistically similar to the benchmark specification.

In Appendix Tables A10-A11, we estimate the effects of MPP stratifying the sample according to the set of socioeconomic characteristics of municipalities. Looking at the number of physicians in BHUs, 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. We also find that the effects of policy on non-MPP physicians were the largest in poorer areas. Perhaps reflecting a reallocation effect, there is evidence that policy led to larger increases in the number of public, non-BHU physicians in poorer areas.

When we explore infant health outcomes, we do not find a consistent pattern. For each infant health outcome, we find evidence of a significant effect of policy in one subsample (out of 20). We find significant reductions associated to policy for prematurity in low unemployment rate areas. For low birth weight, there is only evidence of a significant decline in areas with high local social spending on education. Finally, we find a marginally statistically significant coefficient of interest, with

[19] See Appendix Figure A5 for descriptive statistics on the distribution of propensity scores in treated and untreated municipalities.
[20] Mesoregion and microregion are subdivisions that aggregates several municipalities of a given geographic area with similar economic and social characteristics. The Brazilian Bureau of Statistics (IBGE) created these subdivisions for statistical purposes.

the wrong expected sign, for infant mortality. This lack of a consistent pattern suggests that the few statistically significant estimated coefficients are most likely due to sampling error.

In Appendix Table A20, we investigate whether the implementation of MPP coincided with changes in other health resources. This could be the case if, for instance, MPP implementation encouraged municipality governments to increase local hospital size, for instance. Alternatively, local administrations might reduce health resources to increase the availability of public resources for other purposes. Using the number of hospitals, the number of beds, the number of complete dental equipment, and the number of mammograms as dependent variables (all measured per 1,000 inhabitants), we find insignificant policy effects on these outcomes, with estimated coefficients very small in magnitude relative to the mean.

5 CONCLUSION

A prominent public health policy debate has focused on whether the governments should be intervening on the supply-side or on the demand-side to improve infant health in a cost-effective way. This debate is particularly relevant for developing countries, where the resources for social policies are more limited. Many previous studies using rich quasi-experimental designs document that a variety of demand-side policies reduce the risk of poor infant health outcomes, but there is little careful empirical research on the role of supply side policies. This paper uses a large physician distribution program to shed new light on the role of primary care physicians in infant health in Brazil.

The intervention that we analyze recruited a substantial number of general medical practitioners and family doctors in basic health units, where families have free access to prenatal care and other medical consultations. Using a difference-in-difference empirical strategy, we first show that municipalities that adopted the program experienced an abrupt increase in the number of physicians serving in basic health units. Despite this, we find very little evidence that the program led to improved health outcomes for infants. Indeed, the effects of program on prematurity, low birth weight and infant mortality can be bounded to a tight interval around zero. Remarkably, these findings are essentially the same across subgroups from a wide range of municipality and maternal characteristics. The paper is able to show that selective mortality or other specific features of the empirical setting cannot explain these results.

This overall absence of benefits is surprising given that improving infant health was a major motivation and final goal of the program. A natural question that arises is whether this finding is driven by a lack of parental response to the program, or by low returns to primary care physicians relative to other alternative sources of care. Although our existing data do not allow us to resolve this question definitively here, we can bring some additional identifying information to bear. We find that prenatal care visits and vaccination coverage did not increase significantly after policy implementation in treated areas compared to control municipalities. This suggests that, although the policy was successful in raising the supply of primary care physicians, the affected parents did not respond by increasing their demand for care in physician's offices. Answering this "puzzling" behavior seems to be an interesting direction for future research.

References

Peter Aaby, Kathryn Hedegaard, Morten Sodemann, Ernesto Nhante, Jens Erik Veirum, Marianne Jakobsen, Ida Lisse, Henrik Jensen, and Anita Sandström. Childhood mortality after oral polio immunization campaign in Guinea-Bissau. *Vaccine*, 23(14):1746–1751, 2005.

Peter Aaby, Ces´ario L Martins, May-Lill Garly, Carlito Bal´e, Andreas Andersen, Amabelia Rodrigues, Henrik Ravn, Ida M Lisse, Christine S Benn, and Hilton C Whittle. Non-specific effects of standard measles vaccine at 4.5 and 9 months of age on childhood mortality: randomized controlled trial. *BMJ*, 341, 2010.

Arild Aakvik and Tor Helge Holmas. Access to primary health care and health outcomes: The relationships between GP characteristics and mortality rates. *Journal of Health Economics*, 25(6):1139–1153, 2006.

Daron Acemoglu, David H. Autor, and David Lyle. Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Midcentury. *Journal of Political Economy*, 112(3):497–551, 2004.

Anna Aizer. Public Health Insurance, Program Take-Up, and Child Health. *Review of Economics and Statistics*, 89(3):400–415, 2007.

Douglas Almond and Bhashkar Mazumder. Health Capital and the Prenatal Environment: The Effect of Ramadan Observance during Pregnancy. *American Economic Journal: Applied Economics*, 3(4):56–85, 2011.

John C Anyanwu and Andrew E O Erhijakpor. Health Expenditures and Health Outcomes in Africa. *African Development Review*, 21(2):400–433, 2009.

Richard Auster, Irving Leveson, and Deborah Sarachek. The Production of Health, an Exploratory Study. *The Journal of Human Resources*, 4(4):411–436, 1969.

Ariel BenYishay and Keith Kranker. All-Cause Mortality Reductions from Measles Catchup Campaigns in Africa. *Journal of Human Resources*, 50(2):516–547, 2015.

Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan. How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics*, 119(1):249– 275, 2004.

Kasey S Buckles and Daniel M Hungerman. Season of Birth and Later Outcomes: Old Questions, New Answers. *The review of economics and statistics*, 95(3):711–724, 2013.

Adriana Camacho and Emily Conover. Effects of Subsidized Health Insurance on Newborn Health in a Developing Country. *Economic Development and Cultural Change*, 61(3): 633–658, 2013.

John B. Casterline. Collecting data on pregnancy loss: A review of evidence from the world fertility survey. *Studies in Family Planning*, 20(2):81–95, 1989.

Shin-Yi Chou, Michael Grossman, and Jin-Tan Liu. The impact of National Health Insurance on birth outcomes: A natural experiment in Taiwan. *Journal of Development Economics*, 111:75–91, 2014.

Karen Smith Conway and Partha Deb. Is prenatal care really ineffective? Or, is the devil' in the distribution? *Journal of Health Economics*, 24(3):489–513, 2005.

Jerry Cromwell and Janet B Mitchell. Physician-induced demand for surgery. *Journal of Health Economics*, 5(4):293–313, 1986.

Janet Currie. Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development. *Journal of Economic Literature*, 47(1):87–122, 2009.

Janet Currie, Jonathan Gruber, and Michael Fischer. Physician Payments and Infant Mortality: Evidence from Medicaid Fee Policy. *The American Economic Review*, 85(2):106–111, 1995.

John DiNardo, Nicole M Fortin, and Thomas Lemieux. Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach. *Econometrica*, 64(5): 1001–1044, 1996.

Johan G Eriksson, Eero Kajantie, Clive Osmond, Kent Thornburg, and David J P Barker. Boys live dangerously in the womb. *American Journal of Human Biology*, 22(3):330–335, 2010.

Elizabeth Frankenberg. The effects of access to health care on infant mortality in Indonesia. Health *Transition Review*, 5(2):143–163, 1995.

Jostein Grytten and Rune Sorensen. Type of contract and supplier-induced demand for primary physicians in Norway. *Journal of Health Economics*, 20(3):379–393, 2001.

Jens Hainmueller. Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. *Political Analysis*, 20 (1):25–46, 2012.

James Heckman, Hidehiko Ichimura, Jeffrey Smith, and Petra Todd. Characterizing Selection Bias Using Experimental Data. *Econometrica*, 66(5):1017–1098, 1998.

Toshiaki Iizuka and Yasutora Watanabe. The Impact of Physician Supply on the Healthcare System: Evidence from Japan's New Residency Program. *Health Economics*, 25(11):1433– 1447, 2016.

Theodore Joyce. Impact of augmented prenatal care on birth outcomes of Medicaid recipients in New York City. *Journal of Health Economics*, 18(1):31–67, 1999.

Yusuke Kamiya. Determinants of health in developing countries: Cross-country evidence. 2010.

L V Klerman, S L Ramey, R L Goldenberg, S Marbury, J Hou, and S P Cliver. A randomized trial of augmented prenatal care for multiple-risk, Medicaid-eligible African American women. *American Journal of Public Health*, 91(1):105–111, 2001.

Sebastian Kraemer. The fragile male. *BMJ: British Medical Journal*, 321(7276):1609–1612, 2000.

Thomas G McGuire and Mark V Pauly. Physician response to fee changes with multiple payers. *Journal of Health Economics*, 10(4):385–410, 1991.

Lawrence H Moulton, Lakshmi Rahmathullah, Neal A Halsey, R D Thulasiraj, Joanne Katz, and James M Tielsch. Evaluation of non-specific effects of infant immunizations on early in- fant mortality in a southern Indian population. *Tropical Medicine & International Health*, 10(10):947–955, 2005.

Pablo A. Nepomnaschy, Kathleen B. Welch, Daniel S. McConnell, Bobbi S. Low, Beverly I. Strassmann, and Barry G. England. Cortisol levels and very early pregnancy loss in humans. *Proceedings of the National Academy of Sciences of the United States of America*, 103(10):3938–3942, 2006.

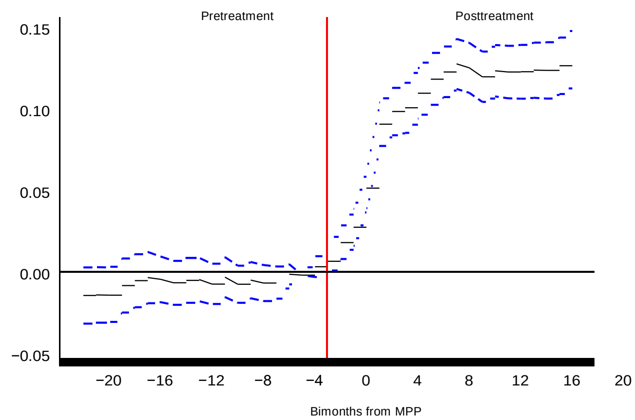
Joseph P Newhouse. Medical Care Costs: How Much Welfare Loss? *The Journal of Economic Perspectives*, 6(3):3–21, 1992.

Jairnilson Paim, Claudia Travassos, Celia Almeida, Ligia Bahia, and James Macinko. The Brazilian health system: history, advances, and challenges. *The Lancet*, 377(9779):1778– 1797, 2017.

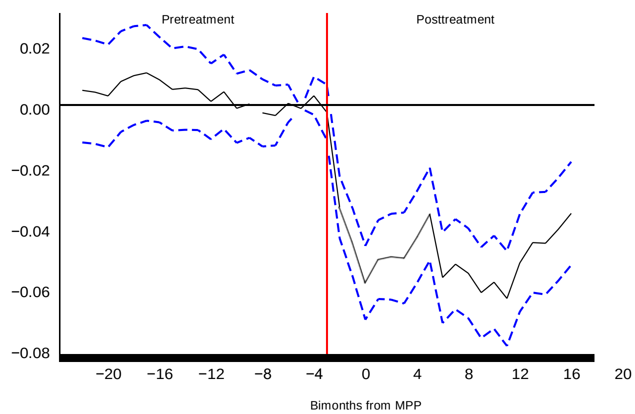
Thomas H Rice. The Impact of Changing Medicare Reimbursement Rates on Physician- Induced Demand. *Medical Care*, 21(8):803–815, 1983.

B Starfield. Primary care and health: A cross-national comparison. *JAMA*, 266(16):2268– 2271, 1991.

Figure 1 - Effects of MPP on physicians



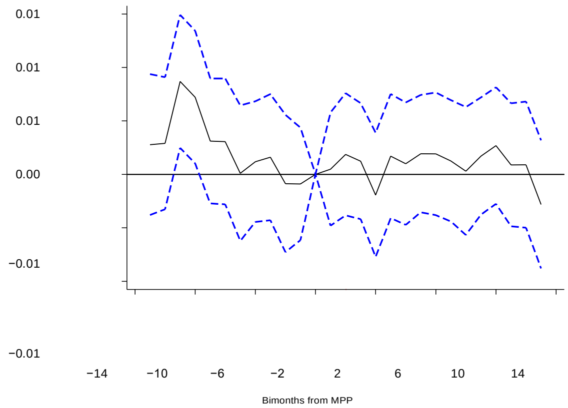
(a) BHU physicians



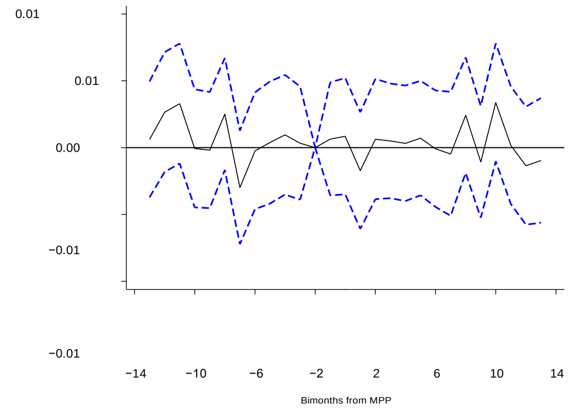
(b) Non-MPP, BHUphysicians

Notes. This is an event-study created by regressing the outcome of interest for a municipality-by-bimonth- by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coefficients for event-time, which plot the time path of the outcome of interest in treated versus untreated areas before and after of policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -2. Physician outcomes are measured per 1,000 residents. Non-MPP, BHU physicians refers to BHU physicians who were not linked to the program.

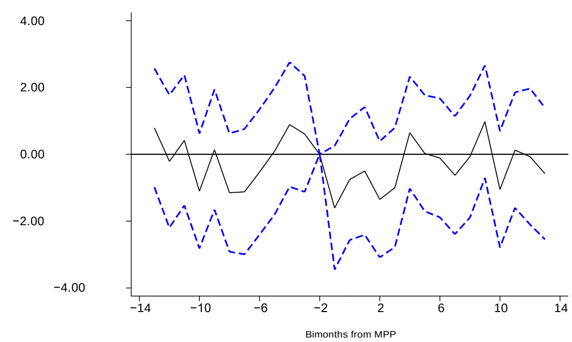
Figure 2 - Effects of MPP on infant health



(a) Fraction preterm births



(b) Fraction low birth weight



(c) Infant mortality rate

Notes. This is an event-study created by regressing the outcome of interest for a municipality-by-bimonth- by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, maternal characteristics, state linear time trends and the full set of municipality Notes. This is an event-study created by regressing the outcome of interest for a municipality-by-bimonth- by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, maternal characteristics, state linear time trends and the full set of municipality characteristics interacted with linear trends. The observations are weighted by the number of births. The figure reports the coefficients for event-time, which plot the time path of the outcome of interest in treated versus untreated areas before and after of policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category is -2

Table 1 - Treatment-Control differences in pre-MPP time trends in main outcomes

	BHU physicians	Other public physicians	Private physicians	Preterm	Low birth weight	Infant mortality rate
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Time Trend	0.0003	-0.0004	0.0005	-0.0001	0.0000	0.0223
	[0.0003]	[0.0004]	[0.0004]	[0.0001]	[0.0000]	[0.0149]
Linear time trend	0.0001	0.0009	0.001	0.0021	0.0000	-0.0799
	[0.0003]	[0.0004]***	[0.0004]**	[0.0001]***	[0.0000]	[0.0140]***
N	185171	165741	73819	188851	188851	190230
R-squared	0.735	0.883	0.915	0.327	0.141	0.215

Notes. This table presents the results from OLS regressions run at the municipality-level predicting the physician and infant outcomes of interest using pre-MPP data. All regressions include municipality-fixed effects. Physician outcomes are measured per 1,000 residents. Other public physicians refers to all public physicians but excluding those in BHUs. Regressions for birth and infant death outcomes are weighted by the number of births. The number of observations differ across physician outcomes because municipalities with zero values during the entire period are excluded from the regression estimation. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 2 - The effect of MPP on physicians

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A: BHU physicians			
Post × Treatment	0.1068	0.0998	0.101	0.1009	0.1026	0.1027	0.1041
	[0.0062]***	[0.0061]***	[0.0061]***	[0.0061]***	[0.0061]***	[0.0061]***	[0.0060]***
N	294054	294054	289712	284852	283502	283502	283502
R-squared	0.686	0.691	0.689	0.692	0.693	0.693	0.694
				Panel B: Non-MPP, BHU physicians			
Post × Treatment	-0.0473	-0.0551	-0.0533	-0.0514	-0.0496	-0.0496	-0.0472
	[0.0060]***	[0.0058]***	[0.0058]***	[0.0058]***	[0.0058]***	[0.0058]****	[0.0057]**
N	294047	293939	289567	284713	283364	283364	283364
R-squared	0.682	0.688	0.686	0.691	0.692	0.692	0.693
				Panel C: Other public physicians			
Post × Treatment	0.0013	0.0064	0.0088	0.0073	0.0081	0.008	0.0111
	[0.0087]	[0.0087]	[0.0084]	[0.0085]	[0.0085]	[0.0085]	[0.0088]
N	263278	258372	254217	250170	249112	249112	249112
R-squared	0.862	0.866	0.861	0.863	0.863	0.863	0.864
				Panel D: Private physicians			
Post × Treatment	-0.0042	-0.0042	-0.0047	-0.004	-0.0033	-0.0035	-0.0033
	[0.0062]	[0.0063]	[0.0064]	[0.0064]	[0.0064]	[0.0064]	[0.0062]
N	117241	116647	113248	112546	112384	112384	112384
R-squared	0.904	0.905	0.881	0.881	0.881	0.882	0.882
Linear time trend interacted with:							
Basic characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-MPP BHU physicians	No	Yes	Yes	Yes	Yes	Yes	Yes
Other economic characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Geographic characteristics	No	No	No	Yes	Yes	Yes	Yes
Social spending	No	No	No	No	Yes	Yes	Yes
Spending on Bolsa familia	No	No	No	No	No	Yes	Yes
State linear trends	No	No	No	No	No	No	Yes

Notes. Physician outcomes are measured per 1,000 residents. Non-MPP, BHU physicians refers to BHU physicians who were not linked to the program. Other public physicians refers to all public physicians but excluding those in BHUs. Each coefficient is from a differ- ent regression. All regressions control for municipality and bimonth-year fixed effects. Basic characteristics include pre-MPP per capita GDP, log of population, and illiteracy rate. Other economic characteristics include indigenous population rate, Gini Index, unemploy- ment rate, and rural population rate. Geographic characteristics include municipality area, altitude, distance to capital, temperature, rainfall, legal Amazon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education and health. 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 brackets) are clustered at the municipality level. Significance: * p < 0.10** p < 0.05, *** p < 0.01.

Table 3 - The effect of MPP on BHU physicians by medical specialty

	Family doctor	Clinicians	Gynecologist	Pediatricians	All other specialties
	(1)	(2)	(3)	(4)	(5)
Post × Treatment	0.0932	0.0167	0.0005	-0.0042	-0.0108
	[0.0036]***	[0.0055]***	[0.0042]	[0.0042]	[0.0068]
Pre-MPP mean	0.128	0.0949	0.025	0.056	0.0582
N	276378	210545	87603	67937	64666
R-squared	0.509	0.622	0.400	0.697	0.68

Notes. Physician outcomes are measured per 1,000 residents. Each coefficient is from a different regression. All regressions control for municipality and bimonth-year fixed effects. Regressions include also state linear time trends as well as 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 brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table 4 - The effect of MPP on infant health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Prematurity							
Post × Treatment	0.002	0.0015	0.0011	0.0009	0.0002	0.0002	0.0001	0.0004
	[0.0014]	[0.0014]	[0.0014]	[0.0014]	[0.0014]	[0.0014]	[0.0014]	[0.0014]
N	266181	261329	261329	257484	253240	252001	252001	252001
R-squared	0.336	0.338	0.344	0.333	0.333	0.333	0.333	0.341
	Panel B: Low birth weight							
Post × Treatment	0.0001	0.0001	0.0001	-0.0001	-0.0002	-0.0002	-0.0001	-0.0003
	[0.0006]	[0.0006]	[0.0006]	[0.0007]	[0.0007]	[0.0007]	[0.0007]	[0.0007]
N	266181	261329	261329	257484	253240	252001	252001	252001
R-squared	0.129	0.13	0.13	0.122	0.12	0.12	0.12	0.121
	Panel C: Infant mortality rate							
Post × Treatment	0.0958	0.128	0.1193	0.006	0.0509	0.0375	0.0421	0.0016
	[0.2280]	[0.2297]	[0.2305]	[0.2369]	[0.2363]	[0.2362]	[0.2364]	[0.2369]
N	267072	262128	261329	257484	253240	252001	252001	252001
R-squared	0.061	0.061	0.062	0.059	0.056	0.056	0.056	0.057
Basic characteristics × linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-MPP BHU physicians × linear trend	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Maternal characteristics	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Other economic characteristics × linear trend	No	No	No	Yes	Yes	Yes	Yes	Yes
Geographic characteristics × linear trend	No	No	No	No	Yes	Yes	Yes	Yes
Social spending × linear trend	No	No	No	No	No	Yes	Yes	Yes
Spending on Bolsa familia × linear trend	No	No	No	No	No	No	Yes	Yes
State linear trends	No	No	No	No	No	No	No	Yes

Notes. Each coefficient is from a different regression. All regressions control for municipality and bimonth-year fixed effects. Basic char- acteristics include pre-MPP per capita GDP, log of population, and illiteracy rate. Maternal characteristics include education (less than 4 years rate), age, and unmarried. Other economic characteristics include indigenous population rate, Gini Index, unemployment rate, and rural population rate. Geographic characteristics include municipality area, altitude, distance to capital, temperature, rainfall, legal Ama- zon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education and health. The observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table 5 - The effect of MPP on infant mortality rate by cause

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

Notes. Each coefficient is from a different regression. All regressions control for municipality and bimonth-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 Inter- national Statistical Classification of Diseases and Related Health Problems 10th Revsion (ICD-10). Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table 6 - The effect of MPP on fetal deaths, births and sex ratio

	Fetal death rate	ln(# fetal deaths)	(Fetal + infant deaths)/ (fetal deaths + births)	ln(# births)	ln(# births + fetal deaths)	Sex ratio	Fraction male births
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treatment	-0.1032	-0.0025	-0.101	-0.0032	-0.0033	0.007	0.0012
	[0.2085]	[0.0219]	[0.3239]	[0.0041]	[0.0041]	[0.0044]	[0.0009]
N	252001	84690	252001	252001	252001	248292	252001
R-squared	0.066	0.937	0.083	0.995	0.995	0.052	0.021

Notes. Each coefficient is from a different regression. All regressions control for municipality and bimonth-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. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 7 - The effect of MPP on prenatal care visits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treatment	0.0039	0.0029	0.003	0.0028	0.0016	0.0018	0.0023	0.0027
	[0.0024]*	[0.0024]	[0.0024]	[0.0024]	[0.0024]	[0.0022]	[0.0022]	[0.0026]
Pre-MPP mean	0.900	0.900	0.900	0.900	0.900	0.900	0.900	0.900
N	266181	261329	261329	257484	253240	252049	252049	252049
R-squared	0.71	0.71	0.711	0.711	0.699	0.701	0.701	0.705
Basic characteristics × linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-MPP BHU physicians × linear trend	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Maternal characteristics	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Other economic characteristics × linear trend	No	No	No	Yes	Yes	Yes	Yes	Yes
Geographic characteristics × linear trend	No	No	No	No	Yes	Yes	Yes	Yes
Social spending × linear trend	No	No	No	No	No	Yes	Yes	Yes
Spending on Bolsa família × linear trend	No	No	No	No	No	No	Yes	Yes
State linear trends	No	No	No	No	No	No	No	Yes

Notes. Dependent variable is fraction mothers attending at least four prenatal care visits. Each coefficient is from a different regression. All regressions control for municipality and bimonth-year fixed effects. Basic characteristics include pre-MPP per capita GDP, log of population, and illiteracy rate. Maternal characteristics include education (less than 4 years rate), age, and unmarried. Other economic characteristics include indigenous population rate, Gini Index, unemployment rate, and rural population rate. Geographic characteristics include municipality area, altitude, distance to capital, temperature, rainfall, legal Amazon region dummy, and semiarid region dummy. Social spending includes pre-MPP spending on education and health. The observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

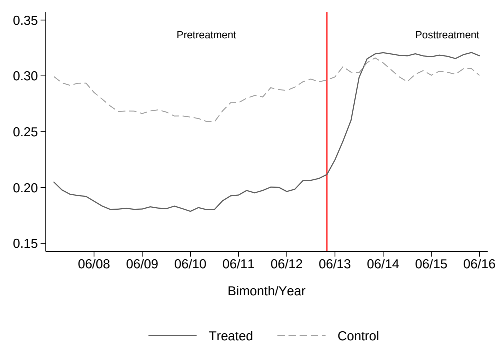
Table 8 - The effect of MPP on immunization coverage

	Hepatitis B	BCG	MMR	Rotavirus	Polio	Yellow fever
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treatment	1.2606	-0.4409	0.9601	1.1856	1.0017	1.3902
	[1.0129]	[1.1660]	[1.0087]	[0.8961]	[0.9828]	[1.1854]
Pre-MPP mean	106.14	90.24	108.55	94.73	107.78	90.84
N	47373	46810	47375	47372	47371	37498
R-squared	0.312	0.597	0.316	0.369	0.329	0.616

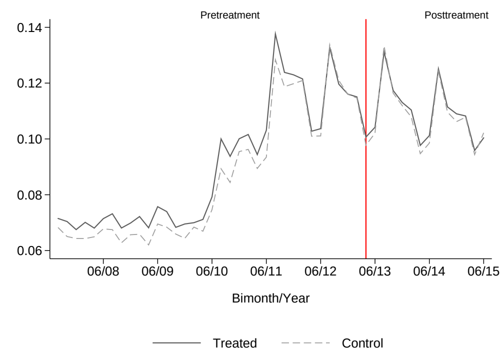
Notes. Each coefficient is from a different regression. All regressions control for municipality and year fixed effects. Regressions include also state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. Immunization coverage is computed as the number of doses per 100 infants. Analysis is based on municipality- by-year panel data covering the 2008 through 2016 period. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

A Appendix

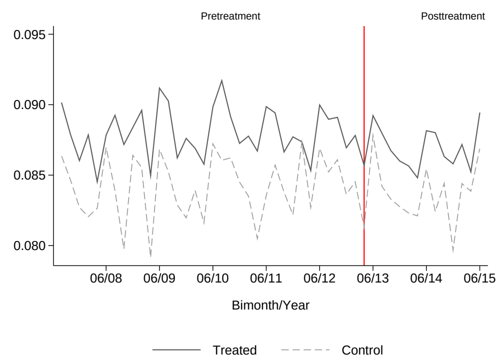
Figure A.1 - Trends in physician and infant outcomes



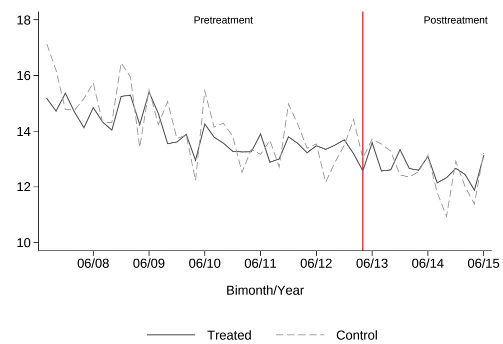
(a) BHU physicians (per 1,000 residents)



(b) Fraction preterm births

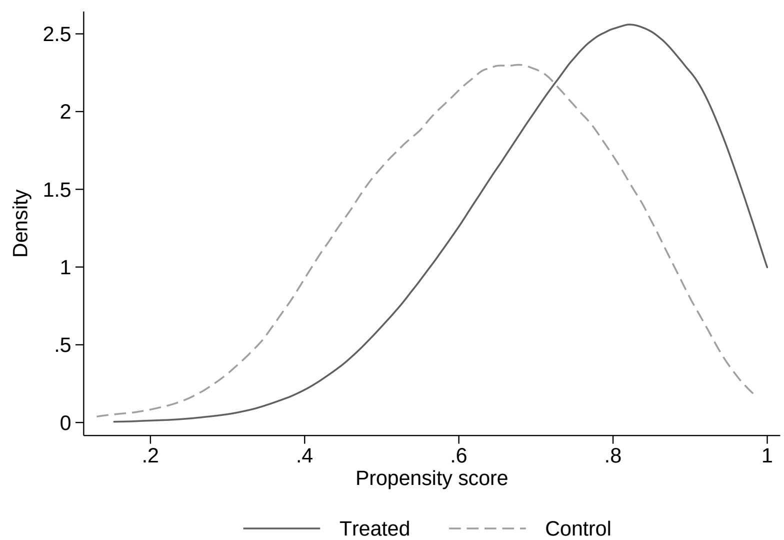


(c) Fraction low birth weight



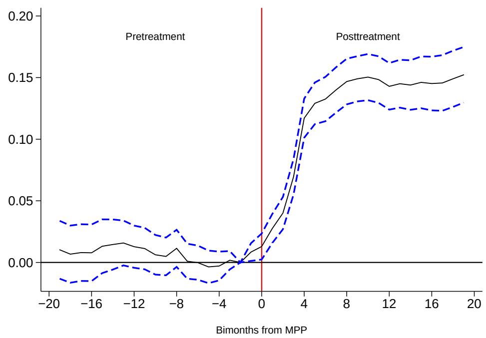
(d) Infant mortality rate

Figure A.2 - Propensity score distributions

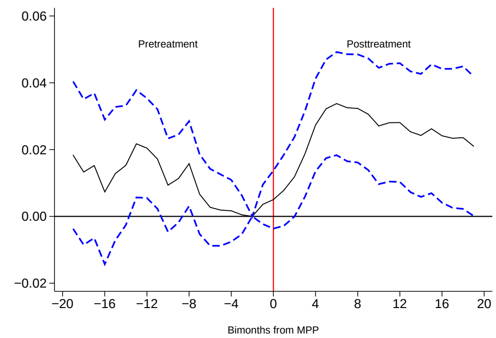


Notes. Figure shows kernel density estimates Epanechnikov kernel for the full estimation sample. The bandwidth is 0.046 for untreated and treated municipalities. We construct propensity scores by esti-mating a probit model with the binary dependent variable equal 1 if a municipality implemented the MPP using the following pretreatment covariates: BHU physicians, log of per capita GDP, log of pop-ulation, illiteracy rate, indigenous population rate, Gini Index, unemployment rate, rural population rate, municipality area; altitude, distance to capita; temperature, log of rainfall, Legal Amazon dummy indicator, semiarid area dummy indicator, log of per capita spending on Bolsa Familia, log of per capita spending on education, and log of per capita spending on health. This yields estimates of the propensity of treatment, $p_i = P(Treatment = 1|X_i)$.

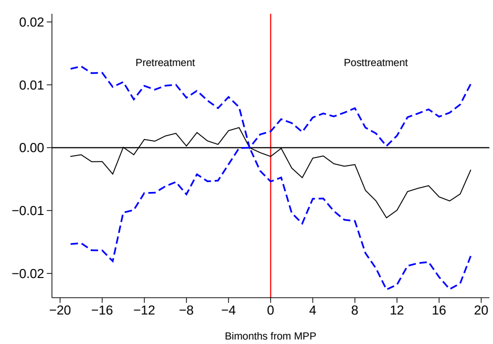
Figure A.3 - Effects of MPP on other physician outcomes



(a) Public physicians



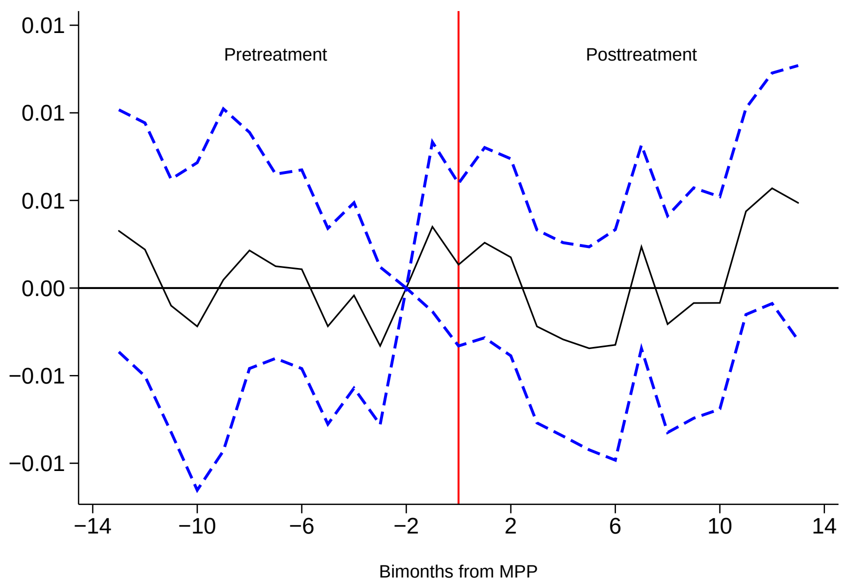
(b) Public physicians (except those in BHUs)



(c) Private physicians

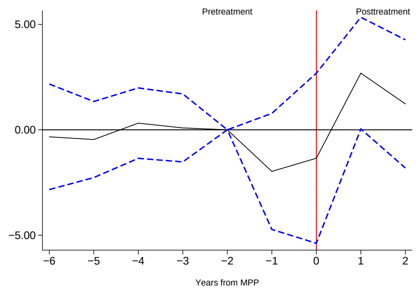
Notes. This is an event-study created by regressing the outcome of interest for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, municipality fixed effects, state linear time trends and the full set of municipality characteristics interacted with linear trends. The figure reports the coeÿcients for event-time, which plot the time path of the outcome of interest in treated versus untreated areas before and after of policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category -2. Physician outcomes are measured per 1,000 residents.

Figure A.4 - Effects of MPP on prenatal visits by bimonth

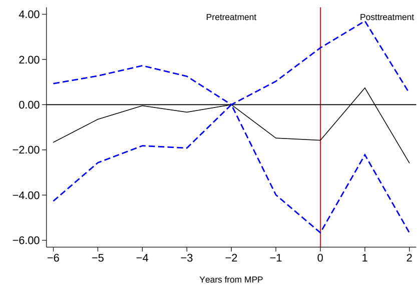


Notes. This is an event-study created by regressing fraction at least four antenatal visits for a municipality-by-bimonth-by-year cell on a full set of event time indicators interacted with an indicator for "treatment", and on a set of controls. The controls include bimonth-by-year fixed effects, munic-ipality fixed effects, maternal characteristics, state linear time trends and the full set of municipality characteristics interacted with linear trends. The observations are weighted by the number of births. The figure reports the coeÿcients for event-time, which plot the time path of the outcome of interest in treated versus untreated areas before and after of policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The bimonth in which the MPP was introduced is normalized to zero. The omitted category -2.

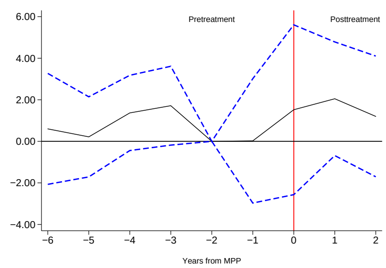
Figure A.5 - Effects of MPP on immunization coverage



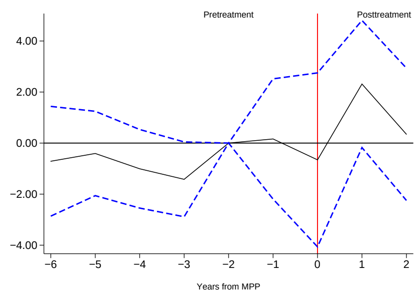
(a) Hepatitis B



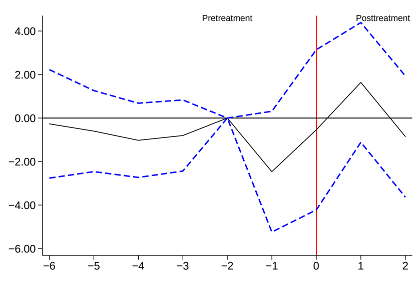
(b) BCG



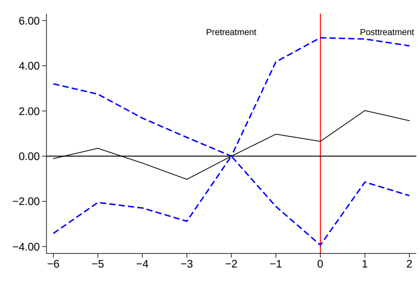
(c) MMR (measles, mumps, and rubella)



(d) Rotavirus



(e) Polio



(f) Yellow fever

Notes. This is an event-study created by regressing the outcome of interest for a municipality-by-year cell on a full set of event time indicators interacted with an indicator for “treatment”, and on a set of controls. The controls include year fixed effects, municipality fixed effects, maternal characteristics, state linear time trends and the full set of municipality characteristics interacted with linear trends. Immunization coverage is computed as the number of doses per 100 infants. The figure reports the coeÿcients for event-time, which plot the time path of the outcome of interest in treated versus untreated areas before and after of policy implementation. The dashed lines represent 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. The year 2014 is normalized to zero. The omitted category -2.

Table A1 - Sources of data

Data	Source	Coverage
Physician records	Brazilian Ministry of Health	2008-2016
Birth and death records	Brazilian Ministry of Health	2008-2015
Fetal deaths	Brazilian Ministry of Health	2008-2015
Immunization: hepatitis B, BCG, MMR, rotavirus, polio, and yellow fever	National Program on Immunization	2008-2016
Local hospital capacity		
Number of hospital		
Number of beds	Brazilian Ministry of Health	2008-2016
Number of complete dental equipment		
Number of mammograms		
Socioeconomic characteristics		
Population		
Illiteracy rate		
Indigenous population rate		
Rural population rate	Demographic Census	2010
Gini index		
Unemployment rate		
Other socioeconomic characteristics		
Spending on Bolsa Familia		2007-2012
Spending on education	IPEA	2007-2011
Spending on health		2007-2011
Gross Domestic Product (GDP)		2007-2010
Time-invariant characteristics		
Municipality area		
Altitude		
Distance to capital		
Temperature	IPEA	
Rainfall		
Legal Amazon region		
Semiarid region		

Table A2: Summary statistics

	Mean	Standard Deviation	N
Physician outcomes:			
BHU physicians	0.24	0.27	294108
Other public physicians	0.43	0.51	263278
Private physicians	0.14	0.23	117241
Infant outcomes:			
Births	87.12	517.89	268560
Fraction prematurity	0.09	0.047	266720
Fraction low birth weight	0.08	0.034	266720
Infant mortality rate	13.61	19.39	268560
Maternal characteristics:			
Fraction at least 4 prenatal visits	0.90	0.08	266720
Fraction education years < 4	0.05	0.07	266720
Fraction unmarried	0.49	0.18	266720
Age	25.83	1.47	266703
Municipality characteristics:			
Municipality area	1542.69	5714.71	5505
Altitude	4.12	2.92	5505
Distance to capital	253.19	163.63	5505
Temperature	22.51	2.99	5465
Ln(Rainfall)	4.7	0.34	5465
Fraction legal Amazon region	0.13	0.34	5597
Fraction semiarid region	0.2	0.4	5597
Ln(population)	9.41	1.15	5565
Illiteracy rate	15.81	9.75	5565
Indigenous population, (%)	0.72	4.34	5565
Gini Index	0.5	0.06	5565
Unemployment rate, (%)	6.34	3.67	5549
Rural population, (%)	36.62	21.8	5497
Ln(per capita spending on Bolsa Familia)	1.21	0.82	27821
Ln(per capita spending on education)	5.34	0.36	20472
Ln(per capita spending on health)	5.08	0.48	20450
Ln(per capita GDP)	1.39	0.7	22256

Notes. Physician outcomes are measured per 1,000 residents.

Table A3: Determinants of MPP adoption (OLS models)

	(1)	(2)	(3)	(4)	(5)
BHU physicians	-0.2307	-0.0887	-0.0867	-0.0934	-0.0848
	[0.0297]***	[0.0296]***	[0.0301]***	[0.0308]***	[0.0313]***
Ln(per capita GDP)		-0.0701	-0.0612	-0.0608	-0.0348
		[0.0134]***	[0.0138]***	[0.0143]***	[0.0168]**
Ln(population)		0.115	0.1192	0.1216	0.1143
		[0.0053]***	[0.0065]***	[0.0068]***	[0.0079]***
Illiteracy rate		0.0042	0.0026	0.0024	0.001
		[0.0012]***	[0.0013]**	[0.0013]*	[0.0014]
Indigenous population rate			0.0013	0.0013	0.0018
			[0.0012]	[0.0015]	[0.0016]
Gini Index			0.1758	0.1361	0.0669
			[0.1212]	[0.1250]	[0.1304]
Unemployment rate			0.002	0.002	0.001
			[0.0019]	[0.0020]	[0.0020]
Rural population rate			0.0011	0.0012	0.001
			[0.0004]***	[0.0004]***	[0.0004]***
Municipality area				0.0000	0.0000
				[0.0000]	[0.0000]
Altitude				-0.0031	-0.0034
				[0.0031]	[0.0031]
Distance to capital				0.0000	0.0000
				[0.0000]	[0.0000]
Temperature				-0.0018	-0.0013
				[0.0054]	[0.0054]
Ln(Rainfall)				-0.0367	-0.0335
				[0.0375]	[0.0376]
Legal Amazon region				0.2396	0.2292
				[0.0814]***	[0.0822]***
Semiarid region				0.0009	0.0064
				[0.0278]	[0.0280]
Ln(per capita spending on Bolsa Familia)					0.0359
					[0.0169]**
Ln(per capita spending on education)					0.0246
					[0.0256]
Ln(per capita spending on health)					-0.0731
					[0.0188]***
N	5462	5461	5380	5290	5264
R-squared	0.088	0.151	0.151	0.153	0.155

Notes. All regressions include state fixed effects. Robust standard errors are reported in brackets. BHU Physician is measured per 1,000 residents. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A4: Determinants of MPP adoption (probit models)

	(1)	(2)	(3)	(4)	(5)
BHU physicians	-0.6539	-0.1546	-0.1572	-0.1722	-0.1601
	[0.0879]***	[0.0879]*	[0.0890]*	[0.0914]*	[0.0928]*
Ln(per capita GDP)		-0.2348	-0.2033	-0.2186	-0.1584
		[0.0453]***	[0.0466]***	[0.0482]***	[0.0580]***
Ln(population)		0.4397	0.445	0.4348	0.4201
		[0.0233]***	[0.0266]***	[0.0283]***	[0.0343]***
Illiteracy rate		0.0133	0.0082	0.0069	0.001
		[0.0045]***	[0.0048]*	[0.0050]	[0.0053]
Indigenous population rate			0.0086	0.0056	0.0066
			[0.0078]	[0.0073]	[0.0073]
Gini Index			0.5111	0.3002	0.0313
			[0.4089]	[0.4230]	[0.4418]
Unemployment rate			0.0058	0.0061	0.0021
			[0.0067]	[0.0069]	[0.0070]
Rural population rate			0.0032	0.0033	0.0026
			[0.0013]**	[0.0013]**	[0.0014]*
Municipality area				0.0000	0.0000
				[0.0000]***	[0.0000]***
Altitude				-0.0089	-0.0103
				[0.0109]	[0.0110]
Distance to capital				-0.0001	-0.0001
				[0.0002]	[0.0002]
Temperature				0.0006	0.0029
				[0.0189]	[0.0190]
Ln(Rainfall)				-0.1613	-0.1655
				[0.1465]	[0.1473]
Legal Amazon region				0.8522	0.819
				[0.2542]***	[0.2552]***
Semiarid region				-0.0013	0.0081
				[0.1033]	[0.1042]
Ln(per capita spending on Bolsa Familia)					0.1309
					[0.0556]**
Ln(per capita spending on education)					0.1608
					[0.0925]*
Ln(per capita spending on health)					-0.2436
					[0.0813]***
N	5381	5380	5299	5240	5214
R-squared	0.0769	0.1407	0.1398	0.1443	0.1464

Notes. All regressions include state fixed effects. Robust standard errors are reported in brackets. BHU Physician is measured per 1,000 residents. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A5 - Treatment-Control differences in pre-MPP time trends in other outcomes (1)

	Mother's education (fraction < 4 years)	Fraction unmarried	Mother's age	ln(# births)	Fetal death rate	ln(# fetal deaths)	Sex ratio	At least 4 prenatal visits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Time Trend	0.0001	-0.0003	-0.0009	0.0001	-0.003	-0.0002	0.0000	0.0000
	[0.0001]	[0.0006]	[0.0009]	[0.0003]	[0.0104]	[0.0010]	[0.0002]	[0.0001]
Linear Time Trend	-0.0015	-0.0085	0.0209	0.0000	0.0078	0.0011	-0.0001	-0.0004
	[0.0001]***	[0.0003]***	[0.0007]***	[0.0003]	[0.0098]	[0.0009]	[0.0002]	[0.0001]***
N	188851	188851	188840	188851	189017	63793	185641	188851
R-squared	0.712	0.612	0.754	0.995	0.147	0.946	0.062	0.734

Notes. This table presents the results from OLS regressions run at the municipality-level predicting outcomes using only pre-MPP data. All regressions include municipality-fixed effects. Fetal death rate is computed as fetal deaths / (births +fetal deaths). Observations are weighted by the number of births. Robust stan-dard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A6 - Treatment-Control differences in pre-MPP time trends in other outcomes (2)

	ln(per capita spending on Bolsa Familia)	ln(per capita spending on education)	ln(per capita spending on health)	ln(per capita GDP)	Hospitals	Beds	Complete dental equipment	Mammograms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treatment	-0.009	-0.003	0.005	-0.000 2	0.0001	0.0004	-0.0005	-0.0001
	[0.005]	[0.006]	[0.007]	[0.005]	[0.0001]	[0.0015]	[0.0008]	[0.0001]
Linear time trend	0.137	0.069	0.064	0.038	0.0003	0.0013	0.0064	0.0005
	[0.004]***	[0.004]***	[0.006]***	[0.005]***	[0.0001]***	[0.0014]	[0.0007]***	[0.0001]***
N	27821	20472	20450	22256	122696	103530	172910	39054
R-squared	0.977	0.939	0.873	0.989	0.879	0.932	0.705	0.708

Notes. This table presents the results from OLS regressions run at the municipality-level predicting the outcomes using pre-MPP data. All regressions include municipality-fixed effects. Columns (1)-(4) are based on unbalanced municipality-by-year panel of data. Information for spending on education and health are available for the 2007-2011 period. Data on Bolsa Familia spending are available for the 2007-2012 period. Data on GDP is available for 2007-2010. The outcomes in columns (5)-(8) are measured per 1,000 residents. The number of observations varies across outcomes in columns (5)-(8) because municipalities with zero values during the entire period are excluded from regression estimation. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A7 - The of MPP on infant health according to baby's sex and maternal characteristics

	Male	Female	Mother's education < 4 years	Mother's education > 4 years	Unmarried	Married	Mother's age < 20	Mother's age > 20
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Prematurity				
Post × Treatment	0.0013	-0.0004	0.0031	0.0000	- 0.001	0.0007	0.0027	-0.0001
	[0.0014]	[0.0015]	[0.0046]	[0.0013]	[0.0017]	[0.0015]	[0.0020]	[0.0013]
N	248825	248292	175136	251872	242246	248638	230857	251424
R-squared	0.248	0.227	0.115	0.325	0.235	0.224	0.172	0.3
				Panel B: Low birth weight				
Post × Treatment	0.0009	-0.001	-0.0002	-0.0003	- 0.0005	- 0.0007	0.0006	-0.0004
	[0.0008]	[0.0009]	[0.0031]	[0.0007]	[0.0011]	[0.0009]	[0.0014]	[0.0007]
N	248825	248292	175136	251872	242246	248638	230857	251424
R-squared	0.073	0.078	0.051	0.115	0.084	0.076	0.049	0.116
				Panel C: Infant mortality rate				
Post × Treatment	0.1418	-0.0998	-0.1126	-0.0897	-	-	-0.4316	0.0198
	[0.3407]	[0.3224]	[2.2259]	[0.2306]			[0.5402]	[0.2687]
N	252001	252001	251565	252001			252001	252001
R-squared	0.043	0.038	0.066	0.05			0.032	0.055

Notes. Each coeÿcient is from a different regression. Municipality and bimonth-year fixed effects are included in all specifications. Regressions include also maternal characteristics, state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. When the sample is stratified by the maternal characteristic X, then the variable X is excluded from the regression. Mother's marital status is not available for death records. Observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A8 - The effect of MPP on infant health - Allowing the effects to vary over time

	Prematurity (1)	Low birth weight (2)	Infant mortality rate (3)
1(2014 year) × Treatment	0.0001	-0.0002	-0.1026
	[0.0014]	[0.0008]	[0.2959]
1(2015 year) × Treatment	0.0001	0.0000	0.201
	[0.0016]	[0.0008]	[0.2997]
N	252049	252049	252049
R-squared	0.341	0.121	0.057

Notes. Each coeÿcient is from a different regression. All regressions control formunicipality and bimonth-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 term 1(.) represents an indicator for year. The observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01

Table A9 - The effect of MPP on infant health - municipality-by-year data

	Prematurity (1)	Low birth weight (2)	Infant mortality rate (3)
1(2014 year) × Treatment	0.0002	0.0000	0.0285
	[0.0015]	[0.0009]	[0.2921]
1(2015 year) × Treatment	0.0004	0.0000	0.2734
	[0.0017]	[0.0008]	[0.2908]
N	42120	42120	42120
R-squared	0.627	0.468	0.318

Notes. Each coeÿcient is from a different regression. All regressions control for municipality and 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 term 1(.) represents an indicator for year. The observations are weighted by the number of births. Analysis is based on municipality-by-year panel data covering the 2008 through 2015 period. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A10 - The effect of MPP on physicians by pre-MPP socioeconomic characteristics

		Gini Index	Unemployment rate	Illiteracy rate	Rural population rate	Population size	GDP (per capita)	BHU physicians (per capita)	Spending on Bolsa Familia (per capita)	Spending on education (per capita)	Spending on health (per capita)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: BHU physicians	Low	0.1279 [0.0112]***	0.1407 [0.0119]***	0.1049 [0.0119]***	0.0888 [0.0100]***	0.1727 [0.0123]***	0.1456 [0.0116]***	0.1024 [0.0071]***	0.1639 [0.0124]***	0.1706 [0.0121]***	0.1694 [0.0121]***
	High	0.087 [0.0094]***	0.0841 [0.0089]***	0.0881 [0.0075]***	0.1209 [0.0109]***	0.0467 [0.0068]***	0.0586 [0.0082]***	0.127 [0.0129]***	0.0526 [0.0056]***	0.0527 [0.0073]***	0.0518 [0.0075]***
Non-MPP, BHU physicians	Low	-0.0363 [0.0108]***	-0.031 [0.0114]***	-0.0266 [0.0112]**	-0.0314 [0.0095]***	-0.0458 [0.0117]***	-0.0643 [0.0109]***	-0.0378 [0.0064]***	-0.0326 [0.0118]***	-0.0415 [0.0115]***	-0.0485 [0.0114]***
	High	-0.0604 [0.0090]***	-0.0571 [0.0084]***	-0.0661 [0.0071]***	-0.0544 [0.0102]***	-0.0424 [0.0067]***	-0.0344 [0.0080]***	-0.0501 [0.0124]***	-0.0519 [0.0055]***	-0.0415 [0.0071]***	-0.0388 [0.0074]***
Other public physicians	Low	0.0028 [0.0137]	0.0076 [0.0172]	0.0197 [0.0193]	0.0028 [0.0155]	0.0154 [0.0149]	0.0333 [0.0134]**	0.0043 [0.0130]	0.0077 [0.0180]	0.0124 [0.0141]	0.0241 [0.0144]*
	High	0.0086 [0.0144]	0.0226 [0.0134]*	0.021 [0.0118]*	0.0117 [0.0123]	0.0105 [0.0170]	0.0183 [0.0207]	0.0082 [0.0154]	-0.0158 [0.0135]	0.0158 [0.0171]	0.0132 [0.0167]
Private physicians	Low	-0.0132 [0.0100]	-0.0154 [0.0089]*	0.0027 [0.0091]	0.0003 [0.0095]	-0.041 [0.0265]	-0.0451 [0.0187]**	-0.0006 [0.0082]	-0.0134 [0.0117]	-0.0432 [0.0207]**	-0.0487 [0.0264]*
	High	0.0205 [0.0089]**	0.0252 [0.0141]*	-0.0019 [0.0086]	-0.0157 [0.0133]	0.0023 [0.0098]	0.0036 [0.0082]	-0.0134 [0.0114]	0.0146 [0.0149]	0.004 [0.0105]	0.0032 [0.0089]

Notes. Each coeÿcient is from a separate regression. Physician outcomes are measured per 1,000 residents. Non-MPP, BHU physicians refers to BHU physicians who were not linked to the program. Other public physicians refers to all public physicians but excluding those in BHUs. All regressions control for municipality and bimonth-year fixed effects. Regressions include also state linear time trends as well as the full set of interactions between municipality characteristics and a linear time trend. Low and High denote, respectively, the first and third tertiles of the distribution of the municipality characteristic. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A11 - The effect of MPP on infant health by pre-MPP socioeconomic characteristics

		Gini Index	Unemployment rate	Illiteracy rate	Rural population rate	Population size	GDP (per capita)	BHU physicians (per capita)	Spending on Bolsa Familia (per capita)	Spending on education (per capita)	Spending on health (per capita)
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:											
Prematurity	Low	-0.0005	-0.0043	-0.0021	0.0013	0.0038	0.001	0.0028	-0.002	0.0015	0.0028
		[0.0019]	[0.0022]*	[0.0019]	[0.0018]	[0.0024]	[0.0024]	[0.0023]	[0.0020]	[0.0020]	[0.0023]
	High	0.0012	-0.0008	0.0000	-0.0032	-0.0003	0.0002	0.0004	-0.0003	0.0003	0.0001
		[0.0023]	[0.0021]	[0.0024]	[0.0028]	[0.0019]	[0.0018]	[0.0019]	[0.0021]	[0.0019]	[0.0019]
Low birth weight	Low	0.0005	-0.0009	-0.0004	0.0001	-0.0015	-0.0001	-0.0001	-0.0034	-0.0024	-0.0009
		[0.0013]	[0.0013]	[0.0011]	[0.0010]	[0.0018]	[0.0015]	[0.0014]	[0.0015]**	[0.0017]	[0.0017]
	High	0.0006	-0.0012	0.0005	0.0004	0.0000	-0.0001	-0.0006	-0.0006	-0.0003	-0.0001
		[0.0011]	[0.0011]	[0.0012]	[0.0014]	[0.0010]	[0.0010]	[0.0012]	[0.0011]	[0.0010]	[0.0010]
Infant mortality rate	Low	0.3133	-0.2212	-0.4281	0.1244	1.4512	1.2763	0.1851	0.0391	0.6912	1.1527
		[0.4484]	[0.5068]	[0.3100]	[0.2821]	[0.8295]*	[0.7180]*	[0.5339]	[0.5766]	[0.7517]	[0.7527]
	High	-0.0341	0.0491	0.4727	0.5602	-0.3613	-0.1789	-0.2649	-0.1025	-0.3525	-0.3878
		[0.4260]	[0.3847]	[0.5481]	[0.6769]	[0.2925]	[0.2817]	[0.3773]	[0.3403]	[0.2894]	[0.2921]

Notes. Each coeÿcient is from a separate regression. All regressions control for municipality and bimonth-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. Low and High characteristics represent, respectively, the first and third tertiles of the distribution of the municipality characteristic. Observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A12 - The effect of MPP on physician and infant health - Municipality-specific seasonal variation

	BHU physicians	Non-MPP BHU physicians	Other public physicians	Private physicians	Prematurity	Low birth weight	Infant mortality rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treatment	0.1047	-0.0477	0.0112	-0.0034	0.0001	-0.0003	-0.078
	[0.0064]***	[0.0061]***	[0.0093]	[0.0066]	[0.0014]	[0.0007]	[0.2525]
N	283502	283364	249112	112384	252049	252049	252049
R-squared	0.701	0.7	0.867	0.883	0.401	0.214	0.158

Notes. Each coeÿcient is from a dfferent regression. Physician outcomes are measured per 1,000 residents. Non-MPP, BHU physicians refers to BHU physicians who were not linked to the program. Other public physicians refers to all pub-lic physicians but excluding those in BHUs. These regressions show the robustness of the main results to the inclusion of municipality-by-bimonth fixed effects. The regressions also control for state linear time trends as well as the full set of interactions between municipality characteristics and a linear time trend. For birth and infant death outcomes, the regres-sions control in addition for maternal characteristics and weight the observations by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A13 - The effect of MPP on physician and infant health - Municipality-by-month-by-year data

	BHU physicians	Non-MPP BHU physicians	Other Public physicians	Private physicians	Prematurity	Low birth weight	Infant mortality rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treatment	0.1041	-0.0471	0.0111	-0.0033	0.0001	-0.0005	-0.0427
	[0.0060]***	[0.0057]***	[0.0087]	[0.0062]	[0.0013]	[0.0007]	[0.2435]
N	566999	566882	498247	224783	497160	497160	497160
R-squared	0.683	0.682	0.86	0.879	0.224	0.064	0.03

Notes. Each coeÿcient is from a different regression. Physician outcomes are measured per 1,000 residents. Non-MPP BHU physicians refers to BHU physicians who were not linked to the program. Other public physicians refers to all public physicians but excluding those in BHUs. These regressions show the robustness of the main results to the use of municipality-by-month-by-year data rather than bimonthly data. The regressions include municipality and month-by-year fixed effects. Other controls include state linear time trends as well as the full set of interactions between municipality characteristics and a linear time trend. For birth and infant death outcomes, the regressions in addition control for maternal characteristics and weight the observations by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A14 - Spillovers: Neighboring municipalities

	Prematurity	Low birth weight	Infant mortality rate
	(1)	(2)	(3)
Post × Treatment	0.0007	0.0006	0.0425
	[0.0010]	[0.0005]	[0.2221]
N	251953	251953	251953
R-squared	0.486	0.175	0.076

Notes. Each coeÿcient is from a different regression. All regressions control for municipality and bimonth-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. Regressions are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01

Table A15 - The effect of MPP on maternal characteristics

	Education < 4 years	Unmarried	Age
	(1)	(2)	(3)
Post × Treatment	-0.0006	0.0002	-0.0098
[0.0011]		[0.0064]	[0.0144]
N	252001	252001	252001
R-squared	0.708	0.717	0.728

Notes. Each coeÿcient is from a different regression. All regressions control for municipality and bimonth-year fixed effects. Regressions include also state linear time trends as well as the full set of interactions between municipality characteristics and a linear time trend. Observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A16 - The effect of MPP on physician supply - Matching estimations

	Baseline	Mahalanobis matching	Nearest neighbors based on propensity score	Kernel Matching	Inverse propensity score weighted estimates	Entropy balancing weighted estimates
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: BHU physicians			
Post × Treatment	0.1041	0.0973	0.1000	0.0998	0.1104	0.0895
	[0.0060]***	[0.0054]***	[0.0048]***	[0.0047]***	[0.0060]***	[0.0046]***
N	283502	223144	241766	241874	241874	283448
R-squared	0.694	0.667	0.67	0.67	0.699	0.675
			Panel B: Non-MPP, BHU physicians			
Post × Treatment	-0.0472	-0.0465	-0.0427	-0.0428	-0.0432	-0.0436
	[0.0057]***	[0.0052]***	[0.0045]***	[0.0045]***	[0.0057]***	[0.0043]***
N	283364	222980	241602	241710	241710	283310
R-squared	0.693	0.655	0.66	0.661	0.691	0.664
			Panel C: Other public physicians			
Post × Treatment	0.0111	0.0115	0.0108	0.0116	0.0132	0.0072
	[0.0088]	[0.0107]	[0.0092]	[0.0092]	[0.0091]	[0.0089]
N	249112	195665	207532	207532	207532	249058
R-squared	0.864	0.838	0.845	0.845	0.831	0.88
			Panel D: Private physicians			
Post × Treatment	-0.0033	0.0015	0.0025	0.0029	0.001	-0.0002
	[0.0062]	[0.0088]	[0.0078]	[0.0079]	[0.0070]	[0.0077]
N	112384	80213	83561	83561	83561	112330
R-squared	0.882	0.76	0.759	0.757	0.75	0.852

Notes. Physician outcomes are measured per 1,000 residents. Non-MPP, BHU physicians refers to BHU physicians who were not linked to the program. Other public physicians refers to all public physicians but excluding those in BHUs. Each coeÿcient is from a different regression. All regressions control for municipality and bimonth-year fixed effects. Regressions include also state linear time trends as well as the full set of interactions between municipality characteristics and a linear time trend. Columns (2)-(3) weight control observations by the frequency with which the observation is used as a match. Colum (4) weights control observations by the overall weight given to the matched observations. Column (5) weights the observations by (treatment/ propensity score)+(1-treatment/1-propensity score) (DiNardo et al. 1996; Heckman et al. 1998). Column (6) weights the control group observations using entropy-balancing weights (Hainmueller 2012). Columns (2)-(5) trim the sample to those with estimated propensity scores between 0.1 and 0.9 (Crump et al. 2009). The number of observations differ across outcomes because municipali-ties with zero values during the entire period are excluded from the regression estimation. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A17 - The effect of MPP on infant health - Matching estimations

	Baseline	Mahalanobis matching	Nearest neighbors based on propensity score	Kernel Matching	Inverse propensity score weighted estimates	Entropy balancing weighted estimates
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Prematurity			
Post × Treatment	0.0004	-0.0005	-0.0012	-0.0012	-0.0012	0.0006
	[0.0014]	[0.0016]	[0.0013]	[0.0013]	[0.0012]	[0.0019]
N	252001	198435	214862	214953	214953	252001
R-squared	0.341	0.235	0.246	0.246	0.223	0.352
			Panel B: Low birth weight			
Post × Treatment	-0.0003	-0.0003	-0.0004	-0.0004	-0.0005	-0.0005
	[0.0007]	[0.0009]	[0.0009]	[0.0009]	[0.0008]	[0.0010]
N	252001	198435	214862	214953	214953	252001
R-squared	0.121	0.079	0.081	0.081	0.073	0.127
			Panel C: Infant mortality rate			
Post × Treatment	0.0016	-0.4381	-0.0879	-0.0927	-0.0508	-0.0475
	[0.2369]	[0.3627]	[0.3040]	[0.3045]	[0.2904]	[0.3313]
N	252001	198435	214862	214953	214953	252001
R-squared	0.057	0.04	0.041	0.042	0.039	0.058

Notes. Each coeÿcient is from a different regression. All regressions control for municipality and bimonth-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. Columns (2)-(3) weight control observations by the frequency with which the observation is used as a match. Colum (4) weights control observations by the overall weight given to the matched observations. Column (5) weights the observations by (treatment/propensity score)+ (1-treatment/1-propensity score) (DiNardo et al. 1996; Heckman et al. 1998). Column (6) weights the control group observations using entropy-balancing weights (Hainmueller 2012). Columns (2)-(5) trim the sample to those with estimated propensity scores between 0.1 and 0.9 (Crump et al. 2009). Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * p < 0.10 ** p < 0.05, *** p < 0.01.

Table A18 - The effect of MPP on physician supply - Alternative specifications

	Baseline	Mesoregion time trends	Microregion time trends	State-by-bimonth-by-year fixed effects
	(1)	(2)	(3)	(4)
		Panel A: BHU physician		
Post × Treatment	0.1041	0.1031	0.1013	0.107
	[0.0060]***	[0.0060]***	[0.0059]***	[0.0061]***
N	283502	283502	283502	283502
R-squared	0.694	0.696	0.704	0.696
		Panel B: Non-MPP, BHU physicians		
Post × Treatment	-0.0472	-0.0469	-0.0466	-0.0459
	[0.0057]***	[0.0058]***	[0.0056]***	[0.0058]***
N	283364	283364	283364	283364
R-squared	0.693	0.695	0.702	0.695
		Panel C: Other public physicians		
Post × Treatment	0.0111	0.0103	0.012	0.0102
	[0.0088]	[0.0083]	[0.0082]	[0.0090]
N	249112	249112	249112	249112
R-squared	0.864	0.866	0.871	0.865
		Panel D: Private physicians		
Post × Treatment	-0.0033	-0.0025	-0.0059	-0.0029
	[0.0062]	[0.0060]	[0.0053]	[0.0065]
N	112384	112384	112384	112384
R-squared	0.882	0.885	0.894	0.882

Notes. Physician outcomes are measured per 1,000 residents. Each coeÿcient is from a different regression. Non-MPP,BHU physicians refers to BHU physicians who were not linked to the program. Other public physicians refers to all public physicians but excluding those in BHUs. All regressions control for municipality and bimonth-by-year fixed effects. Column (1) includes also state linear time trends as well as the full set of interactions between municipality characteristics and a linear time trend. Columns (2)-(4) use different type of region-specific time trends instead of state linear time trends. 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 brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A19 - The effect of MPP on infant health - Alternative specifications

	Baseline	Mesoregion time trends	Microregion time trends	State-by-bimonth-by-year fixed e ects
	(1)	(2)	(3)	(4)
		Panel A: Prematurity		
Post × Treatment	0.0004	-0.0005	-0.0008	0.0006
	[0.0014]	[0.0013]	[0.0012]	[0.0013]
N	252001	252001	252001	252001
R-squared	0.341	0.349	0.362	0.366
		Panel B: Low birth weight		
Post × Treatment	-0.0003	-0.0004	-0.0002	-0.0003
	[0.0007]	[0.0007]	[0.0006]	[0.0007]
N	252001	252001	252001	252001
R-squared	0.121	0.122	0.124	0.127
		Panel C: Infant mortality rate		
Post × Treatment	0.0016	0.0722	0.1121	-0.0184
	[0.2369]	[0.2351]	[0.2407]	[0.2375]
N	252001	252001	252001	252001
R-squared	0.057	0.058	0.06	0.063

Notes. Each coeÿcient is from a different regression. All regressions control for municipality and bimonth-by-year fixed effects. Column (1) includes also maternal characteristics, state linear time trends and the full set of interactions between municipality characteristics and a linear time trend. Columns (2)-(4) use different type of region-specific time trends instead of state linear time trends. Observations are weighted by the number of births. Robust standard errors (reported in brackets) are clustered at the municipality level. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A20 - The effect of MPP on local hospital capacity

	Hospitals	Beds	Complete dental equipment	Mammograms
	(1)	(2)	(3)	(4)
Post × Treatment	0.0024	-0.0131	-0.0079	-0.0046
	[0.0026]	[0.0424]	[0.0060]	[0.0036]
Pre-MPP mean	0.076	1.525	0.482	0.0262
N	186648	155365	261114	58205
R-squared	0.808	0.813	0.858	0.584

Notes. Each coeÿcient is from a different regression. The outcomes are measured per 1,000 residents. All regressions control for municipality and bimonth-by-year fixed effects. Specifications include also 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 brackets) are clustered at the municipality level. Significance:* $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

References

Richard K Crump, V Joseph Hotz, Guido W Imbens, and Oscar A Mitnik. Dealing with limited overlap in estimation of average treatment effects. Biometrika, 96(1):187–199, 2009.

John DiNardo, Nicole M Fortin, and Thomas Lemieux. Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach. Econometrica, 64 (5):1001–1044, 1996.

Jens Hainmueller. Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. Political Analysis, 20 (1):25–46, 2012.

James Heckman, Hidehiko Ichimura, Jerey Smith, and Petra Todd. Characterizing Selection Bias Using Experimental Data. Econometrica, 66(5):1017–1098, 1998.

In 2013, the Brazilian government implemented one of the largest physician distribution programs on record. Using a difference-in-difference framework, we document that the number of primary care physicians increased by 60 percent in treated areas. Despite this increased supply of physicians, we find little evidence that the program led to better infant health, measured by low birth weight, prematurity and infant mortality. These findings are essentially the same across a wide range of subgroups. We find suggestive evidence that the absence of family responses to the program is the primary source of these results.

Av. da Universidade, 2700 • Benfica • Fone/Fax:(085) 3366.7751
Cep: 60.020-181 • Fortaleza - CE - Brasil
http://www.caen.ufc.br • e-mail: sec_caen@caen.ufc.br