Is Money-Saving Preventive Care a Fairy Tale? Not for Early Childhood *

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Abstract

This paper quantifies the returns to investment in child health care by looking at the reduction in the number of hospitalizations and Emergency Room (ER) visits. Using administrative data from Colombia, between 2003-2007, we construct an individual-level panel that includes all health services used. We exploit the exogenous variation of a change in regulation that abolished fees for medical consultations for pregnant women and children under the age of one starting in February 2004. Using an instrumental variable approach, we find that an additional medical consultation visit reduces hospitalizations and ER visits by 40% and 46% respectively. Moreover, long-term analysis indicates that children who benefited from the reform had fewer consultation, hospitalizations, and ER visits during their early childhood. Robustness checks indicate that the effects are stronger for children who benefited from the policy in-utero. Additional exercises indicate that the effects are stronger specifically when preventive health care services were demanded and not just any type of medical consultation. A cost-benefit analysis concludes that the policy saves US $16.8 yearly per child, which account for 15% of the annual cost of the health insurance.

JEL codes: I12, I18

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1 Introduction

Health investment during early childhood has proven to have the highest returns, both in the short and the long run (Currie, 2008). In general, improvements in health outcomes have effects on children's cognitive development (Heckman, 2006), future health (Bozzoli, 2009; Strauss, 1998; Elo, 1992), and the accumulation of human capital throughout life (Currie, 2009; Oreopoulos, 2008). In addition to these undebatable private benefits, this paper provides evidence to nourish a controversial debate regarding the investments in preventive care as a mechanism to reduce future high-cost spendings for the health system. Cohen (2008) present a systematic review including 600 articles, published in refereed journals, to calculate cost-effectiveness ratios regarding preventive and treatment intervention. Their results demystify the conventional wisdom of preventive care as the magic bullet to prevent future spending in treatment health care. In addition, the authors highlight that the population targeted, their risk factors, and the frequency of the preventive care intervention are important aspects to determine the cost effectiveness of a particular preventive care intervention. Given these private and potential public benefits, child healthcare interventions become a first order topic of study for academia and policymakers. Academia should quantify the effects of such interventions to inform and recommend policymakers that need to make the right decision in terms of efficient allocation of public resources. In this sense, understanding how policy changes in the healthcare system can affect the demand for different health services is of high importance.

In this paper, we identify the returns to investment in child preventive healthcare by looking at the effects of attending medical consultations on the use of high-cost medical services, such as hospitalizations and Emergency Room (ER) visits. We examine Bogotá, Colombia, as an ideal case study for two main reasons. First, Colombia has very rich administrative data that enables us to construct an individual-level panel data-set of all the health services used over five years. Specifically, we use administrative data reported by the health service providers from 2003 to 2007, which includes information about consultations, ER visits, and hospitalizations for 128,278 individuals, giving us 416,864 observations. The individual panel allows us to capture unobservable heterogeneous factors that are essential inputs in the health production function, such as healthy behaviors and genetics, something novel in the literature for this type of question.

Second, in 2004, Colombia's health care system had a change in regulations that eliminated fees for services for medical consultation for pregnant women and infants younger than one-year-old. Taking advantage of this change in regulation, we exploit the policy reform as a quasi-experiment to test our question of interest. The identification strategy relies on the fact that this policy change gives us an exogenous variation in relative prices, and therefore, in the demand for health care services of eligible children. We use the eligibility criteria –being under the age of one after the amendment implementation– as an instrumental variable to predict the number of medical consultations that a child attended. Similar to the finding from the Oregon experiment by Finkelstein (2012) where a healthcare insurance expansion significantly increases health care utilization, in this case the cut in fee-for-services eliminated an entry barrier and increased the use of medical consultations. This intervention allows us to identify the effect of an exogenous increase of child preventive health care on the use of high-cost medical services, such as hospitalizations and ER visits.

We find that an additional medical consultation, as a result of the change in regulation, reduces hospitalization by about 40% (-0.29 standard deviations) and ER visits by 46% (-0.61 standard deviations), in a given year. We estimate that these results save U$16.8 per child yearly, which corresponds to a 15% of the cost of the health insurance. We also finds that the effects are stronger for children who benefited from the policy during their first year of life as well as in-utero. Additionally, we calculate the long-term benefits of the policy by following the children during their early childhood, finding that the cohort benefited by the policy, in the aggregate, went to fewer consultation, hospitalizations, and ER visits in their first years of life. These results suggest that investments in preventive child healthcare have long-lasting effects that improve long-term health. In addition, it becomes a politically attractive policy for those who appeal for a welfare state, as well as for those who want a smaller and less expensive one.

There is a wide variety of papers that have addressed this question, but this paper is novel given that it can exploit an exogenous variation to identify a causal effect with a panel data set that allows us to account for unobservable characteristics at an individual level. These two conditions enable us to have a very clean identification strategy to answer this question. Our paper is also unique in the sense that it focuses on early childhood, where the stakes are higher. Using early childhood is also beneficial for statistical purposes, given that the recommended number of visits determined by medical standard during pregnancy and the first years of life have a higher frequency compared to any other period of life. Table 1 has details on the number of medical visits recommended in each age cohort by the American Academy of Pediatrics. Finally, we are also the first to analyze the long-term effects and find lasting effects of a policy that induce the utilization of preventive care.

The paper is organized into eight sections. The second section presents the context of the health system in Colombia over the past two decades. The third one includes a review of the related literature. The fourth section presents the data used. The fifth and sixth section presents the empirical strategy, the results and robustness checks, respectively. The seventh section provides a cost-benefit analysis for the case of study. Finally, the eight section concludes.

2 Health System in Colombia

A fundamental change for Colombia in its social security scheme came in the nineties with the Law 100 of 1993, which expanded the coverage of health services and promoted the following six principles: efficiency, universality, solidarity, integrity, unity, and participation. To achieve universal health coverage, the system created two health care regimes that targeted individuals according to their labor market participation and their socioeconomic conditions: the Contributive Regime (CR), and the Subsidized Regime (SR).

The Contributive Regime includes all formal workers who contribute to their health insurance by the payroll tax. The Subsidized Regime, the public health insurance, targets individuals that do not have a formal job and that are poor and vulnerable according to their Poverty Index Score (SISBEN). At the time of the study, the Subsidized Regime covered a less comprehensive package of services than its objective. Although the government wants to cover the whole population under these two regimes, there is an uninsured group of individuals called the Vinculados. Nowadays, health coverage accounts for 96% of the population, becoming one of the main achievements of this reform.

The National Health Security Council is in charge of defining the regulation of the health system in Colombia. In 2004, the Council introduced the Agreement 260, which eliminated the fees for services for prenatal care services and medical consultations for children under the age of one, who belonged to the Contributive Regime[1]. For this study, it is important to understand that: before this Agreement, all children from the CR that attended a medical consultation had to pay a fee-for-service, regardless of their age. After the Agreement, only children above the age of one have to pay a fee-for-service. There are no changes in fee for children in the Subsidized Regime.

Additionally, in 2006 the Law 1098, known as "The Code for Children and Teenagers," promoted in Article 27 the right to comprehensive healthcare for children under five years old, giving priority to those who belonged to the Subsidized Regime. Given the overlap of this regulation with the change in regulation of 2004, we have restricted the empirical exercises to the sample before 2006 to have a clean identification strategy regarding our policy change of study.[2]

The Agreement of 2004, analyzed in this paper, eliminates part of the income restrictions to medical care by exempting the pregnant mothers and children under one year of age from paying fees for services, eliminating an entry barrier to the use of health services. Therefore, the changes made by the regulation allows us to analyze the different behaviors of individuals in the use of health services.

3 Literature Review

Health is one of the elements that most significantly affects an individual's life, as it relates positively to productivity and the acquisition of human capital (Currie, 2009; Heckman, 2006; Strauss, 1998). Health status is of particular importance during early childhood: evidence shows that a child's health affects her through different channels during later years of his life. The most direct channel is through future health (Bozzoli, 2009; Strauss, 1998; Elo, 1992). Besides, health in early childhood affects human capital accumulation in a significant way (Oreopoulos, 2008). The literature has found that healthier children attend school more regularly (Miguel, 2004; Case, 2005) and have better cognitive development because they can acquire skills more easily (Cohodes, 2016; Heckman, 2006; Case, 2005; Diette, 2000). Furthermore, health in early childhood also relates positively to future productivity (Black, 2007; Behrman, 2004) and income (Smith, 2009).

[1]There are two different fees charged to individuals that demand health services: first, a fee-for-service to control excessive use of medical consultation services, this applies only to CR beneficiaries, and second fee, a co-payment to finance part of the hospitalization services and specialized exams provided that is applied to individual in both regimes. ER visits do not require any payment from the patient.

[2]results are available upon request

The long-term effects of investments in early childhood health have motivated policymakers to look for mechanisms that improve children's health conditions. One of them is preventive care services, which seeks to tackle the causes of the illness before the disease develops. Academic literature in medicine has reported a negative correlation between use of primary care and the rates of hospitalizations (Cheng, 2010; Macinko, 2010; Kringos, 2013). In addition, a literature review by Rosano (2013) finds that, from 51 peered reviewed papers between 1990 and 2010, 72.5% of the results suggest a negative and significant correlation between primary care and hospitalizations. Moreover, Dafny (2005) find that a public insurance expansion increases also the number of hospitalizations. These results suggest that giving access, increase the use of hospitalizations and outweighs the efficiency effect of more primary care.

In this paper we focus and estimate the efficiency effect, as there is no change in the coverage rate or access to hospitalizations, but only on preventive care. To the best of our knowledge, our paper is the first one to explore the relationship between preventive and treatment care that achieves to, both, exploit an exogenous variation and control for individual unobservable characteristics by using an individual panel data set. Our paper is also unique in the sense that it is the only one that focuses on early childhood and that analyzes long-term effects of the policy.

The importance of preventive care gives relevance to the study of its determinants. In general, the main determinants seems to be the individuals budget constraint. Some papers (Finkelstein, 2012; Aron-Dine, 2013; De la Mata, 2012) show how becoming insure significantly increases health care utilization. Anderson (2012) find that people who do not have health insurance are more likely to delay seeking medical care, which prevents the early detection of diseases and therefore the proper treatment of them. Other barriers, like information, do not seem to be the problem, as Sautmann (2016) find, in the context of Africa, that the barrier to optimal care seeking is cost and not information. More closely related to this paper, other articles analyze the introduction of fees for services in the health care demand of the general population (Augurzky, 2006; Kiil, 2014) and find a significant reduction of its usage. These works relate to the seminal paper by Manning (1987), who perform an experiment and finds positive estimates for the price elasticity of demand for medical services, as well as for the income elasticity of demand. Moreover, this paper finds that the price elasticity for children is not statistically different from zero, which is different from what we find in our paper for a different country of study.

The potential reduction in hospitalization product of an investment in preventive care also points out a possible benefit in public finances. Curative care is commonly the costliest service in the system, which means that avoidable hospitalizations are increasing the cost of the system unnecessarily. It seems logical to think that if preventive care reduces unnecessary hospitalization, the extra expenses in primary care can end up saving money to the system overall as they reduce high cost medical services. Indeed, this logic is very common to hear from politicians during campaigns (Cohen, 2008). However, except for some exceptional cases, the idea of cost-saving investment in preventive care seems to be a myth; most studies find that an increase in primary care does not decrease overall spending in the system (Cohen, 2009; Horwitz, 2013; McWilliams, 2017). Some find that this investment has positive effects on care, but an increase in overall spending (Kringos, 2013), while others find greater expenses with no significant reduction on diseases rates (Iizuka, 2017). Moreover, these effects are not exclusive to hospitalizations, as increases in ER visits have been found too (Taubman, 2014). In the best cases, overall monetary benefits are minimum (Maciosek, 2010), which seems to point at the conclusion that investment in preventive care makes people healthier and allows them to live better lives, but doesnt save the system any money.

Our paper finds a positive effect of preventive care, and we believe our main difference is that most of the papers in the literature focus on adults health or overall population. We, however, focus on early childhood, where this sort of investment has proven to have the highest returns in many dimensions. Our study adds one more dimension to this list, as we find to be the case that the system saves reduces spending due to more use of medical consultation. In addition, we believe to have an edge over other studies of this sort because, as mentioned previously, we exploit an exogenous shock while controlling for unobservable characteristics with panel data over several years.

4 Data

The Individual Register of Provision of Services (RIPS, by its acronym in Spanish) is defined by Resolution 3374 of 2000 from the Ministry of Health as "the minimum set of basic data that the General System of Social Security of Health requires for different processes. Its structure and characteristics have been unified and standardized for all entities. This record contains the identification of the health service provider, the patient, the service itself, and the diagnose that led to the provision of the service."

The RIPS includes four types of services provided: consultations, ER visits, hospitalizations, and procedures. In this paper, we use the first three. Each use of one of these types of medical services generates a detailed record of the patient. Specifically, each record includes a random identification number, age, sex, type of health regime to which she belongs, dates in which she got in and out of a hospitalization, institution that provided the service, and the diagnose that was given.

We use records from children under the age of five for the period 2003 to 2007 in the city of Bogotá. Even though the policy affected the whole country, Bogotá was taken as the area of study for three reasons: i) it is the capital, and it concentrates approximately 18% of the total population of the country, ii) considering a more concentrated geographic area allows us to have more certainty about the homogeneity of the policy implementation, and iii) the completeness and quality of the administrative data is better for Bogotá compared to the rest of the country. These elements allow us to identify in a much cleaner way the effects of the policy.

However, using data only from Bogotá may have some external validity implications that are worth discussing. According to ENDS 2005, the frequency by which people seek for medical care services is higher in Bogotá than in the other regions of the country. In addition, better-educated moms, like the ones in Bogotá, take their children more often to the doctor. The direction in which this fact affects the effects of the policy is not clear, as different stories are plausible. For instance, mothers could be less sensitive to the policy in Bogotá because they already took their children enough times to the doctor, which would suggest that our findings are a lower bound. On the other hand, mothers could be more sensitive to the policy because they are more interested in their children's health care, which would imply that we are finding an upper bound. Aside from that, Bogotá is a city with high costs of transportation in terms of time because of the vehicular congestion. This could discourage the use of healthcare, which would suggest that our results are a lower bound. However, the number of hospitals *per capita* in Bogotá is higher than in other regions, which means that the probability of having a health center near is higher, which would cancel to a certain extent the extra costs of transportation mentioned above. In the end, there are plausible stories that suggest that, regarding external validity, we could be finding an upper or lower bound, and therefore, although there is nothing conclusive, this must be taken into account.

This administrative data enable us to construct an individual-level unbalanced panel with 416,864 observations that correspond to 128,278 children. Appendix B includes some details of the cleaning process of the database. The main panel we build is unbalanced for two reasons. The first one is because of the difference in the date of birth, that makes us see some individual during more periods than others. This issue is addressed in the robustness checks by equaling the number of periods we see all individuals. The second reason is that we only observe an individual when they use at least one health service in a period of time. That is, when an individual did not go to either consultations, hospitalizations or emergency room visits in the unit of time established (semester or year) we do not observe it in that period. One can see how the bigger the time window is, the less unbalanced the panel is, as it becomes more unlikely that an individual has not used a single health service during that period. In addition, it is important to recall that newborn babies are supposed to go to consultations every month, so we do not believe this to be very problematic. However, in the robustness checks we build a balanced panel, imputing zeros were there are no observations, in order to see that the results still hold and alleviate any concerns.

For the purpose of this paper, we take into account the date in which the Agreement was implemented: February 5th, 2004. Only children that were born between February 5th of 2003 and February 5th of 2004, who are between 0 and 1 years of age before and after February 5th, 2004 were considered to enter the panel. Children that are 1 year old around the date of the policy change, become the best comparison groups for this evaluation. Using this data, we estimate the effect of the change in fees as an exogenous variation over consultations demand and take advantage of this to causally address the impact of more preventive health care in the demand of curative services.

Figure 1 shows stylized facts that compare the number of medical consultations, hospitalizations, and ER visits by treatment and control in each age group from 0 to 4 for the children that are part of the panel. The black and the white bars in the figure corresponds to the average demand of a specific service for the treatment and control group, respectively. The data shows that the treatment group goes to more consultations in their first year of life than the control group. Also, from the second year of life onwards, the number of consultations for the treatment group decrease, while this service increase for the control group. This decreasing trend in services is also true for hospitalizations and ER visits. This stylized fact summarize our main result.

To test the consistency in terms of the demand for services reported in our panel, we compare the frequency of attention to health services in each of these stages of early childhood with respect to the pediatric standard reported in Table 1. The black bar presented in Figure 2 shows the number of medical visits according to the pediatric standard and the white bar shows the current number of medical visits that are reported in our data in each age group for two different time windows: the semester and the year. The ratio of current number of visits with respect to the recommended standard is 0.76

Finally, we include a preview of our results in Figure 3 that shows the demand for services from the control (dotted line) and treatment (solid line) groups for a period before and after the reform depicted with the vertial line in period 3. For the figure it is clear that the treated group increases the number of medical consultations relative to the control after the policy was implemented. On the other hand, the average number of hospitalizations and ER visits are higher for the control group relative to the treatment group after the implementation of the policy.

5 Empirical Strategy

We want to test whether attending to medical consultations in the first year of life can reduce the number of hospitalizations and/or ER visits in the future. An initial and naive fixed effect model would be the following:

$$H_{it+1} = \alpha + \beta C_{it} + \theta X_{it} + \mu_i + \lambda_t + \epsilon_{it} \tag{1}$$

Where H_{it+1}, is the number of hospitalizations or ER visits to which an individual i attended in the time window $t+1$ (month, trimester, semester or year). Our dependent variable of interest is denoted by C_{it} and it stands for the number of medical consultations to which an individual i attends in time window t.

Health status is potentially affected by fixed and time-varying individual characteristics such as the type of health regime to which the child belongs; these characteristics are included in the vector X_{it}. The panel data allows the inclusion of individual fixed effects μ_i , which control for individuals time-invariant unobservable characteristics, such as parental behavior, genetics, and in general, baseline health status that affect future health conditions. λ_t corresponds to the time fixed effects and ϵ_{it} corresponds to the idiosyncratic error.

The fact that our outcome variable is from $t+1$ solves concerns of reverse causality, were hospitalizations or ER visit incite medical consultations in the same period. However, the estimation described earlier can still have endogeneity problems. As we argued before, the Colombian case provides a useful setting to explore this empirical question because of an exogenous change in price due to the removal of fees for medical consultations. This variation in price can induce a higher demand for medical consultations for children under one year of age, without affecting the costs of hospitalizations or ER visits.

To solve the endogeneity problem, we use an instrumental variable strategy, by taking the exogenous component of the reform in terms of the timing and age of the children covered. Specifically, being eligible for treatment, that is being under one year of age after February 2004, is used as an instrument for the number of medical consultations that a child attends. The first stage regression, presented in equation (2), will use the exogenous treatment to predict the use of consultation services results.

$$C_{it} = \theta + \delta T_{it} + \rho X_{it} + \mu_i + \lambda_t + u_{it} \tag{2}$$

Where C_{it} takes the value of 1 for children who were born on the 5th of February 2004, and 0 for children that were born on the 5th February 2003. All other children in between those dates of birth will have a treatment value proportional to the time of treatment they were exposed to during their first year of life. Lets take the example of a child that was born on the 5th of May of 2003, at the time of the amendment she will be nine months, so she will still be eligible for a cut in the fee for service for 3 months. In this case, the treatment variable will take the value of 0.25.

We later show our instrumental variable is relevant: it has strong predictive power over the endogenous variable, consultations. Then, we use the predicted value of consultation from the first stage to run our second stage as follows:

$$H_{it+1} = \alpha + \beta \hat{C_{it}} + \theta X_{it} + \mu_i + \lambda_t + \epsilon_{it} \tag{3}$$

Where $\hat{C_{it}}$ is the number of medical consultations predicted by the first stage to which a child i attends in the time window t (semester or year). All other variables are defined as in equation (1). In this estimation, the coefficient of interest is $\hat{\beta}$, which captures the effect of going to an additional medical consultation in a semester or year on the number of hospitalizations or ER visits that an individual attended to in the following semester or year.

Finally, we also want to estimate the long-term health effect of the policy by looking at the impact of being treated over the total number of consultations, hospitalizations and emergency visits that a child has attended between the ages of 1 to 5. Therefore, we need to aggregate all variables into a cross-section of children. Type of health regime, sex and an age cohort variable, according to the year of birth of the child, will be included in the vector X_{it}. The following equation shows our long-term specification:

$$H_i = \alpha + \beta T_i + \theta X_i + \lambda_t + \epsilon_{it} \tag{4}$$

Here, H_i is now the total number hospitalizations, ER visits or consultations that the child i attended from when she turned one, until the end of the age-individual panel. From this estimation we would expect that our coefficient of interest $\hat{\beta}$ is negative an statistically significant, showing that in the long-term children with more treatment will have a lower rate of use of medical services in general.

6 Results

6.1 Main Results

Tables 2 show our naive OLS and Fixed Effects (FE) results for hospitalizations and ER visits. According to the OLS model in the yearly specification, an additional medical consultation decreased the number of hospitalizations in 0.009, and according to the FE model, it did it in 0.015 in the yearly specification. Both of these effects are significant at the 1% level. In the case of ER visits, the OLS model shows a decrease of 0.037 in the yearly specification and the FE model a decrease of 0.070, both of these results significant at the 1% level. The results are consistent between time window specifications in direction and significance. However, even though the lagged dependent variable and the FE help to clean some of the endogeneity, there could still be some biases that will be controlled with the instrumental variable technique.

For the 2SLS model, we begin by estimating the first stage, where we predict the number of consultations attended with the program eligibility status of an individual. Results are shown in Table 3 . We find that being a child under 1 year of age after February 2004, that is, to have access to free of fee-for-service consultations, increase the number of consultations by 2.84 in the yearly specification. This is significant at the 1% level. This result is consistent for every time window specification of the first stage. In addition, we find that the instrument relevance condition is satisfied with a strong F of excluded instruments for all time windows specifications.

The results of the second stage for the 2SLS model for hospitalizations and ER visits are shown in Table 3. We find, in the yearly specification, that an extra medical consultation decreases the number of hospitalizations by 0.039, which is equivalent to a 40% drop. In the case of ER visits, we find that, in the yearly specification, with an additional consultation, the number of ER visits decreased by 0.160, which is equivalent to a 46% drop. Results are consistent in both hospitalizations and ER visits for all time window specifications and are significant at the 1% level. The 2SLS coefficients are bigger compared to the OLS and FE, confirming that the problem of endogeneity was underestimating our initial results. Therefore, our paper enriches the evidence in the literature by providing a rigorous identification strategy that estimates a causal effect clean of different sources of endogeneity.

Finally, Table **??** looks at the long-term effect of the policy, showing that the children that were treated at least during one month during their first year of life, compared to the ones that were not treated at all, reported having goneto fewer hospitalizations, ER visits and medical consultations between their first five years of life. In particular, these children went to 0.31 fewer consultations, 0.017 fewer hospitalizations and 0.030 fewer ER visits, all significant at least at the 10% level.

These results suggest that the policy benefits go beyond the period in which the children were treated. The decrease in hospitalizations and ER visits, as a result of an initial increase in preventive care investment during the early stages of life, show a considerable gain in efficiency of the use of resources in the health system. In general, we interpret this decrease in the use of services in the long-run as the desired outcome: having healthier individuals.

6.2 Robustness checks

In this section, we will present five different additional robustness check exercises of our results. First, we present two robustness checks that deal with concerns in terms of the construction of the panel due to the nature of the data. Then, we include three additional robustness checks that helps to uncover the channels behind the impact: preventive care, pre-natal care and stronger exposure to the change in relative prices.XXX Standardized results contrasting all the exercises are showed in Table 5.

The first robustness check addresses one possible concern for the validity of our results regarding the length of the panel. Children with no treatment are seen more times throughout the panel, from 2003 to 2007. On the other hand, children that received treatment are only seen from 2004 on-wards. This difference in the length of the panel could induce to have a downward bias in our results. To tackle this concern, we transform the panel so that treated and non-treated children are seen an equal amount of years. In this process, only 2,066 observations are lost, about 0.02% of the sample, which correspond to non-treated children who were seen in 2007. Table 6 shows the instrumental variable results for hospitalizations and ER visits and the first stage, where we find that the relevance condition is satisfied. We find that both hospitalizations and ER visits, in all time window specifications, present a negative coefficient which is significant at the 1% level. Table 5 allows us to compare the standardized coefficient of this adjusted panel (presented in column 2) with the original results (presented in column 1) for the yearly time window specification: both panels show very similar results.

For our second robustness check, we constructs a yearly balanced panel and run our main results. As explained in the data section, the nature of our data set produces an unbalanced panel as we only observe individuals if they used at least one of the three health services in the period of time analyzed. To check if there is any important bias in doing this, we build a balanced panel for the yearly time window by imputing zeros in all periods where an individual that appears in our panel does not appear in a record of the health system. The results appear in Table 7 showing a relevant first stage and a negative and significant effect over hospitalizations and ER visits. In this case, we found larger standardized coefficients, but close enough to the main estimation result. The larger magnitude of the effect in the balanced panel could be explained by the additional zeros that are imposed in the balanced panel to the control group, suggesting that our main results are a lower bound.

As our third robustness check, we only take into account the medical consultations that correspond to a preventive care diagnose according to the ICD-10 code reported by the doctor. As expected, we find stronger results, reinforcing that the channel through which we argue the effect takes place.

Table 8 shows the first and second stage results. The first stage regressions results show the relevance condition of the instrument remains satisfied. Once more, both hospitalizations and ER visits show negative results in all their time window specifications, and all are significant at the 1% level. Although at first sight, effects do seem to be stronger, and once we standardize the coefficients, it is possible to compare among them. The effects found in this exercise for hospitalizations and ER visits are greater by 36% compared to the original coefficients. This result supports the claim that the effects of the consultation over hospitalizations and ER visits take place because a preventive medical visit allows for the adoption of healthy behavior that avoids the disease from appearing. Nevertheless, once we correct the standard errors in the standardized estimation, the effect over ER visits is lost. This imply that, even though preventive health care matters in absolute terms to reduce ER visits, the effect is not large enough to dominate, different to the case of hospitalizations where preventive care matters the most. This helps to uncover the channel of our impact: preventive health care reduces the need for curative health care understood as hospitalizations

A fourth additional check we are interested in is between children that were not treated and those who were treated in their first year of life as well as in their pre-natal period, given that the policy removed the fees for prenatal care services. This means that we want to compare the children born before the policy was implemented with those born nine months after the policy started. We would expect this comparison to deliver bigger results than the ones obtained in the main exercise of this paper since our treatment group includes children with more comprehensive coverage. Table 10 shows the result for the pre-natal exercise. In the case of hospitalization, all-time window specifications have a valid instrument and show a negative and significant result. However, in the case of ER visits, we see that there is a positive effect in the semester window and a negative one in the yearly window, but not statistically significant. We need to further explore this result that show opposite signs.

Table 10 shows the result for the pre-natal exercise. All-time window specifications have a valid instrument. In the case of hospitalizations, we found negative and significant results at the 1% level. However, in the case of ER visits, we only found negative and significant results in the yearly time window and at a 10% significance level, while the semester time window for this outcome does not show a effect different from zero. This implies that exposure during pregnancy matters for hospitalizations, but does not have that much of an effect avoiding visits to the emergency room. When checking the standardized results, nevertheless, the impact of pregnancy treatment is positive and significant for both outcomes, but not bigger relative to the main result.

7 Cost Effectiveness Analysis

One important motivation for promoting preventive health care is the reduction of costs in the healthcare system. If the increase in preventive health care leads to a reduction in costly medical services (hospitalizations and ER visits), as shown in the results, then it is possible that the system as a whole faces lower costs. Two opposite forces interact in this cost analysis: on the one hand, the increase in consultations leads to higher costs for the system but, on the other hand, the increase in consultations causes a reduction in ER visits and hospitalizations, avoiding their costs. To evaluate which force prevails, we combine the main results of the paper with the costs established by the Social Insurance Institute (ISS for its acronym in Spanish). The costs and calculations are shown in Table 12.

First, each consultation has a cost of U$ 2.5. Taking into account that the change in policy increased consultations per child in 2.84 per year, as shown in the first stage of the IV regression of the original panel, the cost per child for the system increased by U$ 7.2 per year. In the case of ER visits, the costs for each visit is U$ 3.6. As every extra consultation decreases the ER visits in 0.16, this means that the program avoids each year 0.45 visits yearly per child. All of these translates in savings of U$ 1.6 yearly per child.

Hospitalizations are by far the costliest service: U$ 43.8 per night. To make a correct cost-benefit analysis, it is important to take into account that on average, each child spends 4.6 nights hospitalized, which means that the cost is much higher than what the night cost suggests. The results show that the policy reduces hospitalizations in 0.039 for every extra consultation, which means that yearly it is reducing hospitalization by 0.11 per child. Hence, taking in to account the decrease in hospitalizations, the cost of hospitalization per night, and the average nights spent in the hospital, the monetary savings in this service are U$ 22.4 yearly per child.

With these calculations we find that, in balance, the policy saves U$ 16.8 per child every year, which means that not only the policy pays for itself, but it even ends up alleviating part of the system financial burden. Taking into account that in Colombia, around 650,000 children are born every year, we estimate that the policy has saved approximately 11 million dollars per year for the State.

Bearing this in mind, the abolition of fees for services appears to be a highly cost-effective policy. In addition, we want to stress that we are not taking into account in this analysis the reduction of hospitalization and consultation found in the long run, which means that the financial benefits found here are probably being underestimated. However, it is important to insist that even if the cost-benefit analysis was not positive, the policy could still be worth implementing as we are not taking into account all other long term benefits and positive externalities, such as the effects on human capital, productivity, and future health, already mentioned in the related literature.

We find, then, that the investment in child preventive health care as a public policy with an unusual characteristic: it appeals to the whole political spectrum, because its benefits appeal to those who fight a welfare state, and its financial benefits appeal to those who seek for a smaller state. In other words, the policy is not only effective but also politically viable.

8 Conclusions

The objective of this study is to establish a relation between the use of preventive care services (consultations) and high-cost medical services (hospitalizations and ER visits). To do so, we take advantage of a change in the regulation that affected the cost of access to the health services for children under one year of age. We propose a 2SLS model, which used the program eligibility as an instrument for the number of consultations that a child attends to, given the exogenous date of the reform implementation. The estimations show that an extra medical consultation visit, as a result of the amendment benefits, reduces hospitalization by 40% (or 0.039 units) and ER visits by 46% (or 0.16 units), in the year window specification. These effects survive to various robustness checks. We also found that the effects are stronger for children who were also exposed to the policy during their prenatal period. We found that effects are larger to those services refering exclusively to preventive health care. Results for pregnancy treatment also show a positive effect, but with a lower magnitude. In addition, we find the policy has positive long term effects, as children who were treated in their year 0 of age, went to fewer consultations, hospitalizations and medical consultations when they were 1 to 5 years old.

We also found the policy to be cost-effective, as the increase in costs generated by the use of more consultations are more than compensated by the decrease in cost product of fewer hospitalizations and ER visits. Our estimates indicate that the system saves U$16.8 for every child per year. However, focusing only on short term cost-effectiveness of the policy leaves aside important long term benefits and positive externalities that must be taken into account. Healthier children have better productivity, human capital, and health in the long term future. The abolition of fees for preventive health services seems, therefore, to be an effective policy measure for improving the health of the people and lowering the costs of the system.

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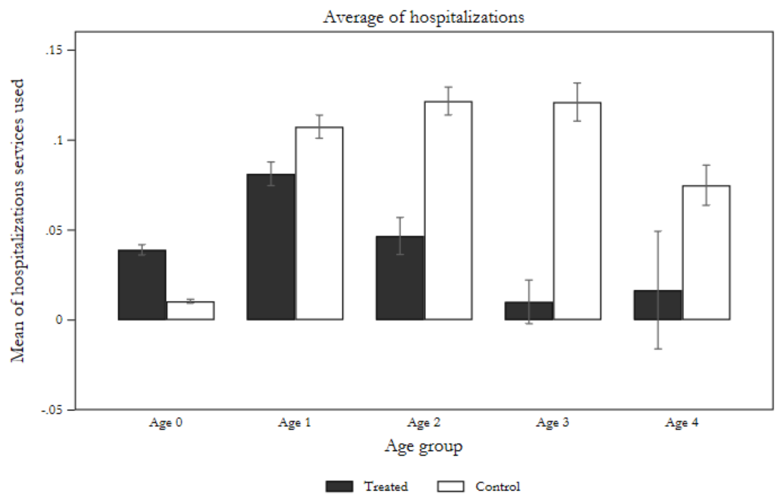
Table 1: Recommended Number of Medical Visits by Age Group

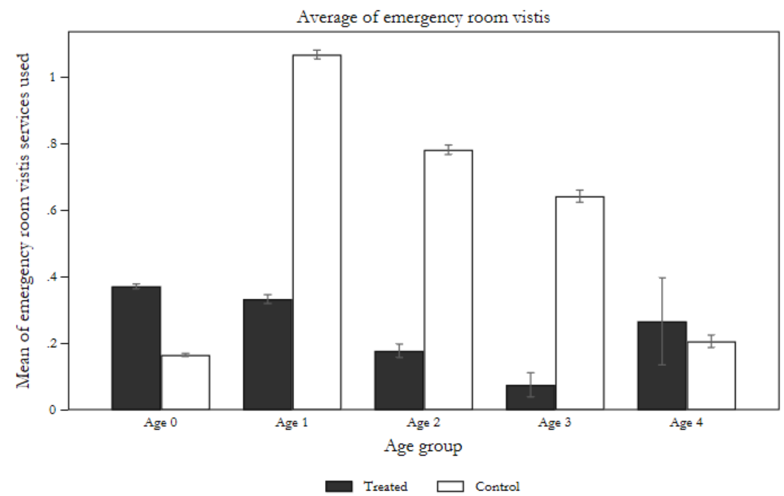
Age group	Number of visits	Frequency per semester	Months of visits
0-1	6	3	1, 2, 4, 6 and 9
1-2	3	1.5	12, 15 and 18
2-3	2	1	24 and 30
3-5	2	0.5	36 and 48

Note: Periodicity schedule recommended for preventive pediatric health care from American Academy of Pediatrics, 2019.

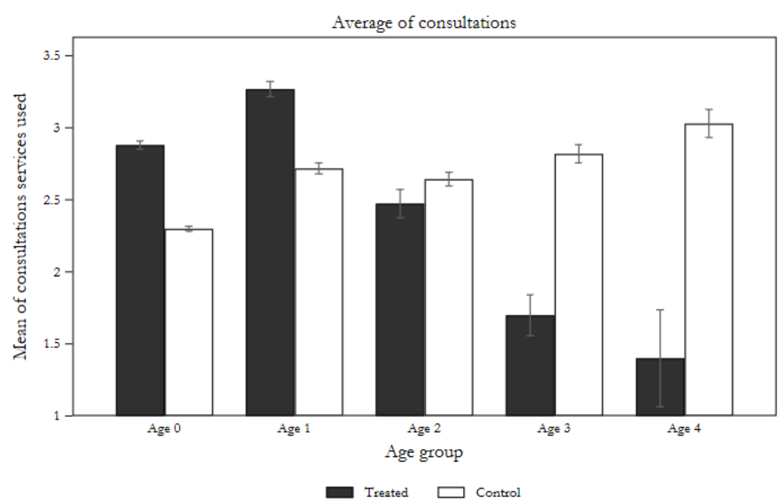
Figure 1: Avareage Medical Services by Age Group



(a) Hospitalizations



(b) Emergency room visits



(c) Consultations

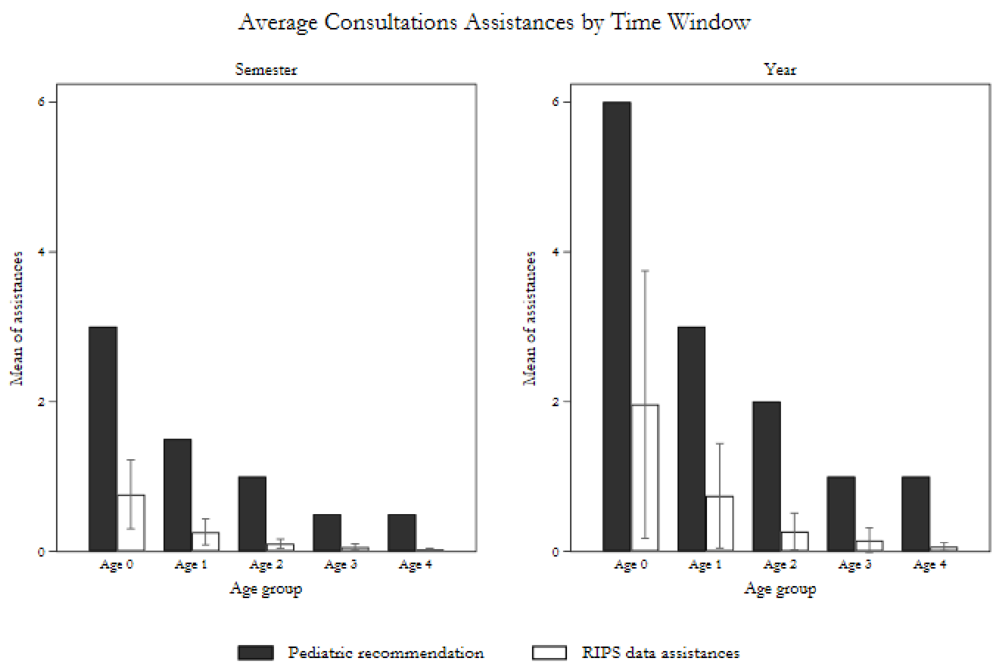
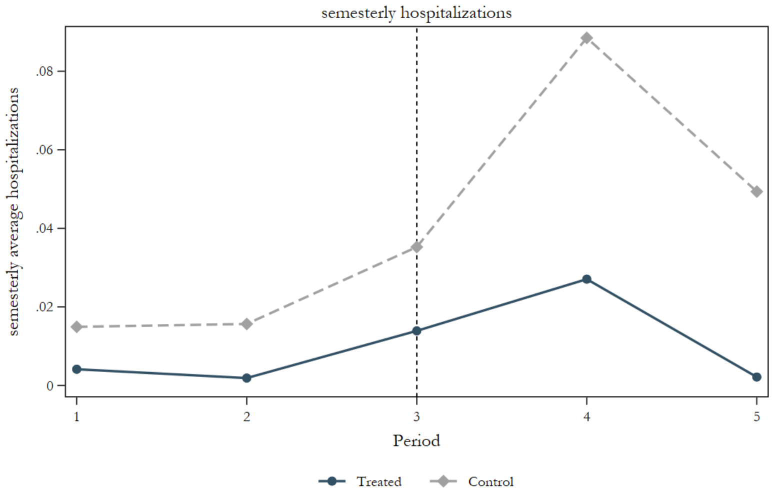
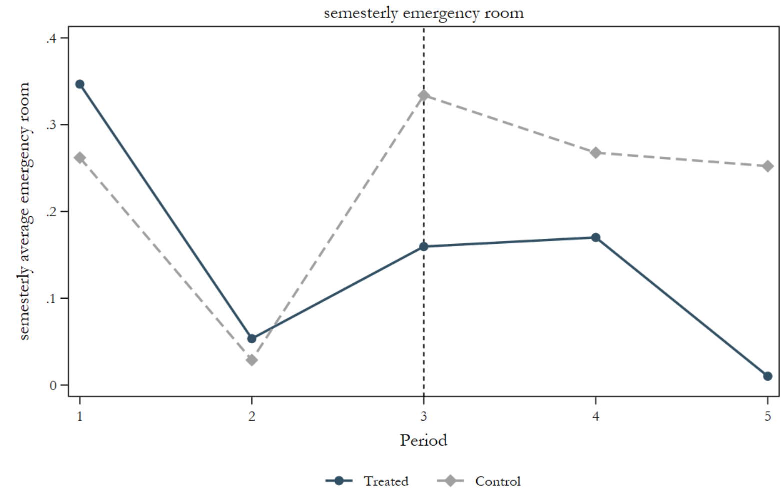


Figure 2: Avareage Assistances to Consultations for Time Windows

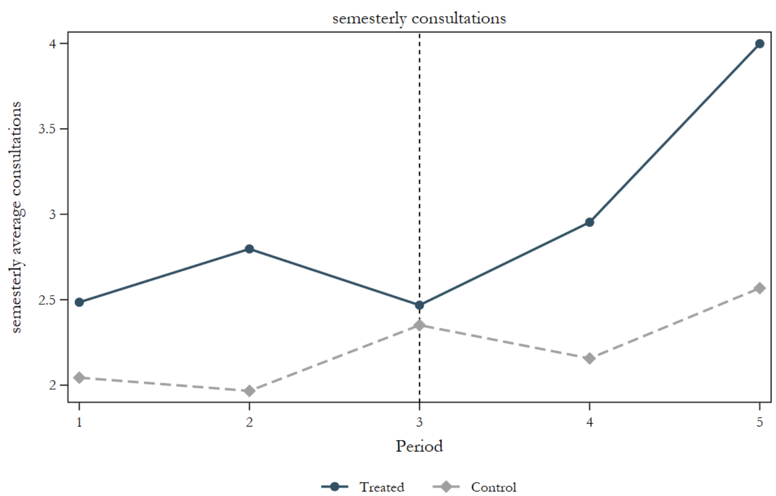
Figure 3: Semesterly Evolution of Medical Services



(a) Hospitalizations



(b) Emergency room visits



(c) Consultations

Table 2: OLS and Fixed Effects Regressions

Time window	Semester	Year	Semester	Year
		OLS		
	Hospitalizations		Emergencies	
Consultations	-0.013***	-0.009***	-0.053***	-0.037***
	(0.001)	(0.001)	(0.001)	(0.001)
R^2	0.039	0.049	0.072	0.138
		FE		
	Hospitalizations		Emergencies	
Consultations	-0.017***	-0.015***	-0.080***	-0.070***
	(0.001)	(0.001)	(0.001)	(0.002)
R^2	0.046	0.074	0.165	0.269
Observations	111,456	69,656	111,456	69,656
Individuals	39,995	27,913	39,995	27,913
Consultations mean	2.174	2.750	2.174	2.750
Outcomes mean	0.063	0.097	0.300	0.344

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Main Estimation IV Regressions

Time window	Semester	Year
	First stage	
Treatment	1.323***	2.844***
	(0.034)	(0.053)
F of excluded instruments	1,530.32	2,921.25
R^2	0.038	0.129
Observations	110,749	69,075
Individuals	39,288	27,332
	Hospitalizations	
Consultations	-0.041***	-0.039***
	(0.003)	(0.002)
R^2	0.012	0.037
Observations	110,749	69,075
Individuals	39,288	27,332
	Emergency room visits	
Consultations	-0.231***	-0.160***
	(0.008)	(0.005)
R^2	-0.058	0.163
Observations	110,749	69,075
Individuals	39,288	27,332

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Long-run Treatment Effects Over Services in Ages 1 to 5

Long run effect over	Consultations	Hospitalizations	Emergencies
Treatment[a]	-0.311***	-0.017*	-0.030**
	(0.062)	(0.010)	(0.015)
R^2	0.115	0.067	0.033
Observations	58,106	58,106	58,106

[a] Treatment is defined as the percentage of the child's exposition in months. Exposition is higher the more months the child live as treated during it's first year. The child is treated when is under 1 after february 2004. Then, maximum exposition is reach when the child was born after february 2004.

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for sex, the type of health regime and cohort fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Estandardized Yearly Especification Effect Coefficients

Standardized impact	Main result	Length panel robustness	Balanced panel robustness	Preventive care	Pregnancy treatment	Contributive subsample
Hospitalizations	-0.286***	-0.286***	-0.390***	-0.366***	-0.239***	-1.435***
	(0.016)	(0.016)	(0.023)	(0.044)	(0.011)	(0.440)
Emergency room visits	-0.613***	-0.618***	-0.836***	-0.048	-0.292***	-3.754***
	(0.018)	(0.019)	(0.031)	(0.046)	(0.012)	(0.989)
Observations	69,075	66,951	69,075	40,906	139,565	12,781
Individuals	27,332	26,783	27,332	24,101	27,913	5,676

Note: Comparisson between the main result and the robustness checks results. Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors for the standardized coefficient in parenthesis. All regressions are estimated with individual controls for sex, the type of health regime and time fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: **Robustness Check for Adjustment in the Length of the Panel**

Time window	Semester	Year
	First stage	
Treatment	1.333***	2.845***
	(0.034)	(0.053)
F of excluded instruments	1,537.06	2,893.07
R^2	0.039	0.131
Observations	108,209	66,951
Individuals	38,848	26,783
	Hospitalizations	
Consultations	-0.043***	-0.039***
	(0.003)	(0.002)
R^2	0.003	0.033
Observations	108,209	66,951
Individuals	38,848	26,783
	Emergency room visits	
Consultations	-0.240***	-0.161***
	(0.008)	(0.005)
R^2	-0.085	0.160
Observations	108,209	66,951
Individuals	38,848	26,783

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Robustness Check for Estimation With the Balanced Panel

Time window	Year
	First stage
Treatment	2.812***
	(0.037)
F of excluded instruments	5,828.00
R^2	0.241
Observations	139,565
Individuals	27,913
	Hospitalizations
Consultations	-0.028***
	(0.001)
R^2	0.003
Observations	139,565
Individuals	27,913
	Emergency room visits
Consultations	-0.067***
	(0.003)
R^2	0.093
Observations	139,565
Individuals	27,913

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Robustness Check for Only Preventive Care Consultations

Time window	Semester	Year
	First stage	
Treatment	0.446***	0.943***
	(0.016)	(0.025)
F of excluded instruments	773.71	1,440.70
R^2	0.034	0.059
Observations	110,749	69,075
Individuals	39,288	27,332
	Hospitalizations	
Consultations	-0.122***	-0.118***
	(0.010)	(0.007)
R^2	-0.124	-0.065
Observations	110,749	69,075
Individuals	39,288	27,332
	Emergency room visits	
Consultations	-0.685***	-0.482***
	(0.031)	(0.018)
R^2	-0.710	-0.156
Observations	110,749	69,075
Individuals	39,288	27,332

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Robustness Check for Treated Children Including Pre-natal Period

Time window	Semester	Year
	First stage	
Treatment	0.868***	1.674***
	(0.046)	(0.072)
F of excluded instruments	353.38	542.30
R^2	0.037	0.101
Observations	78,287	45,744
Individuals	36,810	24,101
	Hospitalizations	
Consultations	-0.025***	-0.038***
	(0.008)	(0.005)
R^2	0.023	0.008
Observations	73,584	40,906
Individuals	36,810	24,101
	Emergency room visits	
Consultations	0.046***	-0.009
	(0.016)	(0.008)
R^2	0.056	0.182
Observations	73,584	40,906
Individuals	36,810	24,101

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Robustness Check for Treated Children Including Pre-natal Period by Donut Hole

Time window	Semester	Year
	First stage	
Treatment	0.908***	1.640***
	(0.046)	(0.071)
F of excluded instruments	389.59	535.35
R^2	0.040	0.108
Observations	77,482	45,004
Individuals	36,425	23,716
	Hospitalizations	
Consultations	-0.032***	-0.039***
	(0.007)	(0.005)
R^2	-0.002	-0.007
Observations	72,820	40,199
Individuals	36,425	23,716
	Emergency room visits	
Consultations	-0.005	-0.014*
	(0.014)	(0.008)
R^2	0.115	0.179
Observations	72,820	40,199
Individuals	36,425	23,716

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Main Estimation IV Regressions only for Contributive Regime

Time window	Semester	Year
	First stage	
Treatment	0.293***	0.327***
	(0.063)	(0.084)
F of excluded instruments	21.83	15.01
R^2	0.054	0.064
Observations	24,222	12,781
Individuals	10,366	5,676
	Hospitalizations	
Consultations	-0.131***	-0.227***
	(0.042)	(0.070)
R^2	-0.831	-2.143
Observations	24,222	12,781
Individuals	10,366	5,676
	Emergency room visits	
Consultations	-0.800***	-1.047***
	(0.175)	(0.276)
R^2	-3.959	-12.358
Observations	24,222	12,781
Individuals	10,366	5,676

Note: Authors calculations. Data obtain from the Ministry of Social Protection. Robust standard errors in parenthesis. All regressions are estimated with individual controls for the type of health regime and individual and time fixed effects according to the time window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 12: Cost Benefit Balance

Service		Price
General medical consultation		US \$ 2.5
Day of hospitalization		US \$ 43.8
Emergency room visit		US \$ 3.6
	Cost increase	
2.84 Consultations	$\times$ US \$ 2.5	= +US \$ 7.2
	Cost reduction	
-0.11 Hospitalizations	$\times$ US \$ 43.8 $\times$ 4.61 Days	= −US \$ 22.4
-0.45 Emergency room visits	$\times$ US \$ 3.6	= −US \$ 1.6
Total savings in the medical system		= −US \$ 16.8

Note: Each service cost is estimated per child. Cost obtained from ISS 2001 Manual of Tariffs. The price is estimated using the ISS 2001 reference price + 35% measured in 2017 US dollars.

Appendix

A: How are ages created?

The RIPS data set that we use contains the diagnostic report from services in hospitals and medical centers in Bogotá for children in early childhood. Then, each observation in the original data set is a medical service deliver to an specific child, where the information of the variables comes from the reports made by the worker in charge. One of the questions that must be filled in the report is the current age. For these reason, we have the exact age of the child when the medical service was deliver. Nevertheless, we do not have the date of birth of the child, leaving us only with a yearly age in each report. As using the individual identifier we are able to build a panel, we have to deal with possible miss reports in the ages.

To deal with the issue, we clean the ages deleting every report where the change in years does not match with the change in dates between services for the same child. If the days past between one service an another exceeds a year and the age does not change, that observation is drop. Also, we delete observations where the age change negatively between two services. We manage to identify miss reports calculating when the child must have been in an specific age using the last reported date. We delete an observation if the time difference between the maximum reported date and the date of one service exceeds the one between reported ages. For example, if the time past between the last date and the reported date is more than three years, but the ages only changes in one year, we drop that observation. Finally, we do this cleaning for every age group, checking if the dates distance are consistent with changes in reported ages. The resulting variable allows us to use the age as a good measure for exposition to the policy after the change in co-payments date.

B: Cleaning RIPS Data

1. First, we drop the observations that are repeated. It is probable that the cause of this is not a very sick individual but a repeated report on the database. Then, to clean for this, we dropped the observation that had the same diagnose in the same date, for the same individual. 57,568 observations were dropped, which was a 7.8% of the observations.

2. The change in regulation, in which the children under one do not have to pay for medical consultations, can make the Health Provider Company misreport the childs age. Also, sometimes the children are registered with the mothers id and can generate that two brothers have the same id. With the purpose of dealing with these two problems, we identify the children who do not have coherent age reports throughout the years. Cleaning for this, 125,169 observations were deleted, which were an 18.8% of the database.

3. To clean even more for bad implementation of the policy, which is not our goal to measure, once we have identified the observations that should have been treated, we drop all the observations that report a medical consultation cost value greater than zero. Cleaning for this, 5,963 observations were deleted, which was a 1.1% of the database.

4. We also deleted the individuals that went to an abnormal quantity of medical consultations, to clean for outliers. For this, all the children that went to more than 24 consultations per year, which is more than two per month, were dropped. 14,141 observations were deleted, which were a 2.1% of the database.

5. This leaves us with a panel of 542,026 observations from 128,278 individuals. Then we create the time window variables and panel, where we count the number of times we observe an individual using a service in a certain period of time. Some individuals only appear in one period, so we cannot estimate their fixed effect in this panel. These observations are dropped. Notice that the longer the time window, the less probability that an individual appears in more than one period; this generates that the panels of longer time window lose more individuals and observations. In the end we are left with 238,043 observations from 58,489 individuals in the monthly panel, XX observations from 58,489 individuals in the trimester, 111,456 observations from 38,858 individuals in the semester, and 69,656 observations from 27,888 individuals in the yearly.