The Effects of Social Health Insurance Expansion and Increased Choice on Perinatal Health and Health Care Use: Lessons from the Uruguayan Health Care Reform

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Abstract

In 2007 the Uruguayan government launched a reform aimed at expanding social health insurance to family-members of formal workers and to retirees. The policy increased insurance generosity -relative to the safety net alternative- and increased competition by allowing new beneficiaries to choose care from a set of private providers. Exploiting the phased-in implementation and the geographic variation in the intensity of the reform, we find that the expansion of social health insurance had a negligible effect on perinatal health and health care among adolescent mothers and their newborns. Our results do not support prior research showing health care quality improvements in settings with increased choice. We hypothesize that health care rationing by private providers due to rising wages, a smaller primary care infrastructure of private providers in low-income neighborhoods, and cultural and financial barriers may have accounted for the lack of positive effects.

Key words: social health insurance, provider choice, competition, birthweight, prenatal care, health reform, Latin America

JEL: D12, H51, I11, I12, I13, I14, I18, J13.

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1. Introduction

In the past decades, Latin America experienced a widespread process of health care reforms aimed at expanding access to health care and strengthening health systems. These reforms included the reorganization of health systems to address structural fragmentation, a decentralization of decision-making, improvements of regulatory functions, and a separation of financing and provider functions (Atun et al., 2015). In terms of health insurance structure, some countries, such as Costa Rica and Brazil, shifted from highly fragmented health insurance systems to integrated single-payer systems.[1] Others maintained semi-integrated systems but expanded coverage and/or guaranteed the provision of basic benefits to the poorest population. Such was the process followed by Chile, Colombia and Uruguay, and more shyly by Peru, Argentina, Dominican Republic, and Mexico. For the countries that implemented single-payer systems, there is some evidence that the reforms decreased infant and maternal mortality and rationalized the use of resources (Dow and Schmeer, 2003; Soares et al., 2017). There is little rigorous evidence, however, on the impacts of the reforms that expanded social security or public insurance. Unlike single-payer systems, social security health systems in Latin America rely on private provision and on provider competition. It is quite important to understand how these institutional arrangements affect the allocation of health care services and health outcomes.

In this paper, we assess the effects of one aspect of the health care reform launched in Uruguay as of 2007: the expansion of social security health insurance to groups of the population previously covered by the public safety net. Whereas the public safety net provided services only through public hospitals and clinics, social health insurance allowed beneficiaries to choose from a larger network of private and public providers, and to access care with higher

[1] Cuba also implemented a single-payer system earlier in the past century.

levels of per capita expenditure. The reform expanded the ability to choose a health provider to 808,000 new beneficiaries between 2007 and 2010 (a quarter of the Uruguayan population). Out of these, 256,000 were already purchasing services from private providers out of pocket. Thus, the reform expanded choice to 552,000 new beneficiaries in the period, an increase that was mainly due to the incorporation of children under the age of 18 of formal workers to their parents' social security scheme. We hypothesized that the expansion of social security to individuals previously uninsured or covered by the safety net would improve quality of care and health outcomes through two channels: by expanding access to a more generous health insurance and by increasing the quality of care through increased choice and competition.

Our identification strategy exploits the phased-in implementation of the reform to different groups of the population as well as the reform's differential intensity across geographic regions. Using Live Birth Certificates for Uruguay for the period 2002-2010, we find that the expansion in social security insurance increased the choice of private providers, but, against our initial hypothesis, had negligible effects on newborns' health and on the quality of access to perinatal health care.

Our findings contribute to several strands of literature. First, our paper is among a few to provide evidence of a recent market oriented health reform in Latin America aimed at making health care access universal and equitable. The reform imposed an important financial burden on the country, which within a few years from implementation increased health expenditure by 1 point of GDP. Understanding the value for money of the reform is by itself an important issue to focus on.

Second, our paper contributes to the recent literature on the effects of expanded choice and competition on quality of care and health outcomes. As argued by Goddard (2015), in settings with asymmetric information and incomplete contracts, competition may not be good

or bad per se, but may have quite distinct implications depending on the institutional framework. This includes the type of providers competing (insurers, hospitals, primary care providers), the nature of the agent ultimately making the choice (informed or uninformed patients, physicians), market regulations including price controls, the vertical integration between hospitals and primary care practices, or the type of procedure being chosen[2], among other features. Several recent papers provide evidence that the 2000's reforms that fostered hospital competition in the UK enhanced patient welfare, reduced patient mortality, improved health, and increased hospital management quality, without raising costs (Cooper et al., 2011; Gaynor and Town, 2012; Gaynor et al., 2013; Bloom et al., 2015; Gutacker et al., 2016; Gaynor et al., 2016). Kessler and McClellan (2000) also found that hospital competition improved welfare and reduced costs for Medicare patient in the US. While these papers suggest that competition can have unambiguous positive effects in markets with regulated prices, there is mixed evidence on its effects in markets where prices are flexible (Gaynor and Town, 2012). For example, Propper et al. (2004) show that the payer driven UK reform in the 1990s that promoted hospital competition led to higher mortality rates. The literature also provides suggestive evidence that competition and choice may not be welfare enhancing in poorly regulated health care markets. Private provision in these settings can be more inequitable and rely less on evidence-based care than public provision (Schleifer, 1998; Hart, 2003). A few papers show that in poorly regulated settings, or in settings where patients have poor information about quality, competition among private providers may lead to cream-skimming of patients (Hart et al., 1997), unnecessary testing and treatment, and violation of medical standards (Basu et al., 2012).

[2] Patients choosing high risk procedures may be more sensitive to mortality or readmission rates, whereas patients undergoing less critical interventions are more likely to react to other quality measures, such as health gains. The choice of the right measure of quality is important when trying to assess the effects of competition.

While the pro-competitive health care reform in Uruguay was set up in a highly regulated market, with fixed risk adjusted capitated payments to providers, we argue there were several institutional features that refrained it from having welfare enhancing effects. First, the reform attracted vulnerable and relatively uninformed populations that chose providers based on the level of flexible (although capped) copayments rather than quality. Second, because the private sector had a lower number of primary health care clinics in poor neighborhoods, a shift to the private sector may have negatively affected vulnerable women, at least during the first years of the reform. Third, the higher demand for private providers together with a fixed short run supply of physicians led to wage increases in the private sector, which, in a context of regulated capitated payments to providers, might have led to a rationing of services (lower length of visits, longer waiting lists).

2. The Health System in Uruguay

Prior to the reform in 2007, there were three sources of health insurance in Uruguay: public (or safety net) insurance, social security insurance, and private insurance. The first catered primarily to the low-income population, and offered coverage only through public clinics and hospitals. All formal workers in the private sector and some public-sector workers were entitled to social security health insurance. Social health insurance was financed through employer and employee contributions, and allowed beneficiaries to choose among a set of private not-for-profit health care organizations called Collective Medical Care Institutions (IAMC). Employees contributed 3% of their wages and employers 5% to finance social security insurance. The government collected funds through the Social Security Bank (BPS) and paid each Collective Medical Care Institution a unique regulated premium per beneficiary.[3] Those

[3] The monthly premium amounted to 959 Uruguayan pesos in August 2007, or 40 US dollars.

with no formal employment but economic means, as well as family members of well-off formal workers, bought private insurance out of pocket.[4]

In 2007 the Uruguayan government launched a reform with the aim of achieving universal access to health care services, equity in spending and funding, improvements in the quality of care, and a shift towards primary care (MSP, 2010a, 2010b). The main feature of the new scheme was the gradual expansion, between 2008 and 2016, of social health insurance to disabled dependents and children under the age of 18 of formal workers, spouses, partners, independent professionals, and retirees. Social health insurance, now organized around a national health solidarity fund called FONASA and administered by a new social health insurance authority (JUNASA), entitled beneficiaries to choose comprehensive health care from a network of not-for-profit Collective Medical Care Institutions (IAMC)[5] or from the public provider (State Health Care Administrator or ASSE). By law, FONASA providers could not reject any beneficiary requesting coverage. The first important expansion spanned the period 2008 and 2010, and included children under the age of 18 of formal workers. By the end of 2010, 808,000 new beneficiaries (mostly children) had joined the social security scheme (Arbulo et al., 2010). Out of these, 32% were beneficiaries that used to buy private insurance out of pocket, while the other 68% were individuals that were previously entitled to coverage in the public safety net or that were uninsured. By 2015, the reform had extended coverage to 1,640,000 new beneficiaries (around half of the Uruguayan population). Most new social health insurance beneficiaries chose to get care from private providers. Prior to the reform, in 2007, per capita health expenditure in the public safety net was about half the expenditure in the social security sector. By the end of 2010, both systems were spending similar amounts per capita, but perceived quality was higher for the private sector. In 2010 ninety percent of FONASA

[4] The monthly premium for an individual beneficiary was 1310 Uruguayan pesos in 2007 (55 US dollars).
[5] IAMC are closed networks that hire salaried physicians and own, for the most part, their own hospitals.

beneficiaries had chosen to get care from a private provider. Although the choice of the public provider increased in more recent years, private coverage continued to prevail, with a share of 82% among FONASA beneficiaries in 2015 (JUNASA, 2014).[6]

In terms of financing, the new social security scheme increased workers' contributions from 3% of wages to between 4.5% and 8.0% depending on whether the worker has dependent children and/or a non-working spouse or partner. This increase was not enough, however, to compensate the growth in social security expenditures, and the government has been contributing to the system, since its inception, with general revenues. These contributions amounted to 7% of total social insurance expenditures in 2008 and to 17% in 2015. In parallel, the budget assigned by the government to the public provider (ASSE) increased by 46% in real terms between 2007 and 2010, and by 89% in real terms between 2007 and 2015, despite a decline in the share of the population covered by the public safety net insurance. The purpose of this budgetary effort was to raise the per capita expenditure in the public insurance to that of social health insurance (MSP / PAHO, 2010).[7] Finally, private providers are paid by JUNASA an age-and-gender risk-adjusted capitated fee[8] plus a pay-for-performance component for each beneficiary.[9]

[6] Another important change in terms of governance, though less important for the purpose of this paper, was the separation in 2007 of the State's regulatory role from the role of provider. The public provider, ASSE, was dissociated from the orbit of the Ministry of Public Health and was constituted as a decentralized organization.
[7] Zumar (2013) analyzed the budgetary long-term effects of the reform through simulation models. She concluded that the expansion of the social health insurance would increase the public sector deficit over time in a scenario with an annual GDP growth of around 2%, but would be sustainable over time in more optimistic scenarios that assumed a GDP growth rate of 6%, and increases in labor market formality and labor force participation. GDP grew 6.4% per year in average between 2008 and 2010, but only 3% in average between 2011 and 2016.
[8] Contributions for children below the age of 1 and individuals older than 65 are between 3.5 and 6.5 times the baseline per capita fee (corresponding to a male aged 20 to 44). The monthly premium paid for a male beneficiary aged 20 to 44 (base category) amounted to 419 Uruguayan pesos in January 2008, or 18 US dollars.
[9] This component introduced by the reform aimed at reorienting the health system towards primary care, disease prevention and early diagnosis, particularly in the area of reproductive health. Late in 2008, the Uruguayan government began to compensate health care institutions for adhering to a set of "primary care goals" associated with a series of performance indicators. In the area of maternity care, one of the objectives was to increase the share of pregnancies with an initial visit in the first trimester and at least six prenatal visits before delivery. Another objective included improving the registration of medical records of pregnant patients in the national Perinatal Information System (JUNASA, 2010; González et al., 2010.).

The expansion of social health insurance implied several fundamental changes in access to benefits and in the financing of health care spending. First, it caused a reduction in out of pocket health expenditure for families who previously paid individual premiums for private health insurance and were now entitled to social insurance (crowding out effect). The net income effect (including tax increases) for these families depended on their levels of income and family size: the reform favored lower income families (who had to pay smaller social security contributions) and families with more children. In addition, the reform had an arguably ambiguous effect over families who were previously receiving care from the public provider and became new beneficiaries of the social health insurance (expansion effect). Financially, these families experienced a small increase in social security contributions because they were low income. However, the reform entitled these families to higher quality health care services, at least when measured in terms of the public health expenditure directed to these households. Llambi et al. (2010) showed that the new health insurance scheme increased in 2008 the fraction of public health expenditure (net of contributions) directed to households in the lower deciles of income and decreased the fraction directed to households in the upper deciles. In particular, the 1st decile increased participation in public health expenditure by 1 percentage point (from 11.5% in 2005 to 12.5 in 2008) and the 10th decile lost participation by a similar magnitude (from 7.5% to 6%).

3. Research Question, Identification and Methodology

In this paper we explore whether the expansion in social security health insurance to populations previously covered by the public safety net increased prenatal and perinatal health care access and outcomes. Our treatment of interest is the option to choose care from a set of public or private providers relative to a system that only provided care through a public safety net. This choice can affect quality in at least three ways. First, beneficiaries were given the option to get care from providers that initially spent a higher per capita amount on health

services. While by 2010 the per capita expenditure in the safety net had equaled that of the social security system, during the first years of the reform, the social security system had a more generous coverage. Second, if beneficiaries can assess quality ex-ante, the average quality of care accessed and the average health outcomes for these beneficiaries should improve through sorting after expanding choice. Furthermore, in a competitive setting with complete information about quality, economic theory predicts that providers will restructure their services to attract patients. We expect private providers to offer services that are of higher quality, more efficient, accountable, and catered to the population needs than those offered by the public provider. If quality is non-observable, however, competition may not lead to higher quality outcomes. Because we are unable to assess the visibility of quality, the sign and magnitude of the effect of higher choice and competition ends up being an empirical issue.

We first approach our research question by exploiting the fact that children of formal workers below the age of 18 were the first large group of beneficiaries favored by the expansion of social security between 2008 and 2010. We thus compare the differences in use of prenatal care and perinatal outcomes between mothers aged 17 and mother aged 19 before and after January 2008, when this stage of the reform took place. We truncate the analysis in 2010 because after this year new women of other age groups began to benefit from the social security expansion, confusing the identification strategy. The underlying assumption is that the trends observed in the outcomes of mothers aged 19 are a good counterfactual for those of mothers aged 17.[10] The first equation of interest is:

$$Y_{igt} = \lambda_t + \mu_g + \sum_{\substack{t=2002,\\ t\neq 2007}}^{t=2010} \delta_{0t} D_{igt} + X'_{igt}\rho + \varepsilon_{igt} \qquad (1)$$

[10] Our data does not provide information on the mother's date of birth, only on mother's age at the time of delivery. Because we do not know whether a mother aged 18 at the time of delivery was eligible for social security insurance throughout the full pregnancy or not, we prefer to exclude mothers aged 18 from the analysis.

where Y_{igt} is a perinatal health care measure or health outcome for child i born to mother of age g (17 vs. 19) in year t. The first outcome we consider is whether the birth occurred in a private hospital. Other outcomes include birthweight, low birthweight, premature birth, and four indicators of health care use, onset of prenatal care in the first trimester of pregnancy, if the mother had at least three, or at least six prenatal visits during her pregnancy, and delivery by Cesarean section. λ_t is a vector of year fixed effects that captures annual differences (common to both age-groups) in the dependent variable relative to the year 2007 and μ_g are age-group specific dummies capturing time invariant differences between adolescent mothers aged 17 and adolescent mothers aged 19. D_{igt} are dummy variables defined for each year 2002-2006 and 2008-2010, that take the value of 1 if the mother gave birth at year t and was aged 17 at the time of birth, and 0 otherwise. X_{igt} is a set of variables that capture mother characteristics such as education (incomplete primary school, incomplete secondary school, complete secondary school), marital status (married or cohabiting) trimester of gestation, and department (geographic region) fixed effects. We also include time-varying regional (department-level) characteristics such as unemployment, fraction of population that completed middle school, fraction of population that completed high school, and fraction of household owners. For observations with missing information, we impute the value of that variable with the average value in the sample and include a binary indicator that takes the value of 1 if the record is missing and 0 otherwise. ε_{igt} stands for a pregnancy-specific error term. δ_t are the year-specific difference in difference (DD) parameters of interest. We verify the parallel trends assumption by testing whether these parameters equal zero for the years prior to the reform (years 2002-2006), where 2007 is the omitted category. Equation (1) is estimated by ordinary least squares (OLS) with Huber White robust standard errors.

The introduction of additional policies in the period may limit the validity of the difference in difference analysis. First, between 2005 and 2010 the Uruguayan government implemented a comprehensive tobacco control campaign that decreased disproportionately tobacco use among

younger cohorts (Triunfo et al., 2016), decreased smoking among pregnant women and increased birthweight (Harris et al., 2015). Second, by the end of 2008 pregnant women under the age of 18 were entitled to receive family allowances regardless of their formal participation in the labor market. There is evidence that family allowances in Uruguay increased birthweight, a result consistent with improved maternal nutrition during pregnancy (Amarante et al. 2016).[11] Third, expenditure per capita in the safety net public insurance experienced a sharp increase after the reform, almost closing the gap with private providers a few years after the reform. This increase may have materialized in improvements in the quality of public provision in the safety net and may have improved outcomes in the control group. Both the tobacco campaign and the allowances to adolescent mothers could bias the DD estimates upwards, while increases in per capita expenditure in the public safety net could bias the DD estimates downwards.

To overcome these problems, we exploit the geographic dimension of the reform. There are 19 geographic units (departments) in Uruguay, which showed different levels of public health care coverage for adolescent mothers in the years before the reform (2002-2007). Our hypothesis is that those departments with a low fraction of private coverage among adolescents (greater public coverage) in these years had more to gain from the expansion of social security, and were more likely to increase the volume of beneficiaries choosing a private provider. Thus, we expect the expansion of social security to have had stronger effects in terms of health outcomes and health care utilization in these regions. Defining the variable *Public coverage* in department r (C_r) as the fraction of mothers under the age of 18 with public coverage in the period 2002-2007, we estimate a triple difference model that provides full nonparametric control for age-group specific time

[11] In addition, in 2008 there was an increase in the family allowance per child. While this increase was the same regardless of the age of the mother, younger mothers and their offspring may have benefitted more due to their higher vulnerability (childbearing among adolescents is disproportionately higher among low income women). We try to control for vulnerability by including measures of education, but we could still be missing some margin.

effects that are common across departments, time-varying department effects and age-specific department effects. The equation of interest is:

$$Y_{igrt} = \mu_{gt} + \lambda_{rt} + \eta_{rg} + \sum_{\substack{t=2002 \\ t\neq 2007}}^{2010} \delta_{1t} D_{igt} * C_r + X'_{igrt}\zeta + v_{igrt} \quad (2)$$

where Y_{igrt} is an outcome variable for child i born to mother in age group g (17 vs. 19) in region r and year t. The parameters μ_{gt}, λ_{rt}, and η_{rg} represent fixed effects for mother's age group (17 vs. 19), year dummies, region fixed effects, interactions of age group and year dummies, interactions of year dummies and the variable *Public coverage before the reform* (C_r), and an interaction between age group and C_r. The variables we are interested in are the interactions between age group, C_r, and the nine year dummies. δ_t is a vector of parameters depicting the triple difference effects of interest. The coefficients of δ_t for the years prior to the reform allow us to assess the parallel trends assumption between groups of mothers more likely to have benefitted from the reform (adolescents in regions with lower private provision) and those less likely to have benefitted from it. X_{igrt} includes the same set of control variables defined for equation (1). v_{igrt} stands for a pregnancy-specific error term.

All models in the triple difference analysis are estimated by ordinary least squares (OLS) with standard errors clustered at the region and age group level (38 clusters). This level of clustering allows for the estimation of arbitrary correlation of errors across years within age group and regions.

In addition to the mother-specific outcomes reported above, we analyze an outcome at the population level, fertility. We define fertility as the ratio of births in a particular age-group and region to the total number of women in that age-group and region.

4. Data

We analyze birth registries from the National Registry of Live Birth Certificates, which have full coverage in Uruguay. The live birth certificate is completed by the treating physician based on the woman's clinical history. The microdata on births and deaths in Uruguay during the period 1996 to 2011 were harmonized and validated as a result of a collaborative project between the National Institute of Statistics (INE), the Ministry of Public Health and the Population Unit of the University of the Republic.

As of 2008, the registration of birth certificates changed from paper to electronic format. While this change had no impact on the registration of birth outcomes, it significantly affected the coding of maternal characteristics, such as education or marital status. These variables increased their missingness after 2008 and were harder to interpret due to noisy responses. For this reason, whenever we were able to match the ID of the mother, we imputed education and marital status information from the Perinatal Information System, a nationwide electronic registry operating in many prenatal care clinics in Uruguay since 1990.

We study the period 2002 to 2010 in order to cover pre- and post-reform years. Although we have post-2010 data, we chose to truncate the data in 2010 to avoid distorting the identification strategy, as an increasing proportion of women above the age of 18 became entitled to the NHI after 2010. In a robustness check we rerun the analysis including three years after 2010.

From an initial population of 436,455 births in the period spanning 2002-2010 for the full country, we excluded 48,061 corresponding to multiple births, births with birthweight below 500 grams or below 25 weeks of gestation, births with no information on the mother's age, and births with missing values for the dependent or explanatory variables of interest. Of the remaining 388,394 observations, 32,653 were births to mothers aged 17 or 19. We excluded

births to mothers aged 18 because we did not have information on mother's date of birth and were unable to assess the extent to which these women were exposed to the choice of a private provider throughout some or none of the pregnancy.

Table 1 shows descriptive statistics for the variables considered in the analysis. We define two types of outcomes: perinatal health outcomes and use of prenatal health services. Health outcomes include birthweight (measured in grams), low birthweight or LBW (defined as birthweight below 2500 grams) and prematurity (less than 37 weeks of gestation). Health services include whether the woman had at least three and at least six prenatal visits and whether she initiated prenatal care in the first trimester. In addition to being specific goals set by the Ministry of Health, Balsa and Triunfo (2012) show large effects of these variables on birth outcomes in Uruguay. We also consider whether the mother had a delivery by cesarean section.

[TABLE 1 ABOUT HERE]

Table 1 shows that prior to the reform, mothers aged 17 had a slightly smaller rate of deliveries in private hospitals than mothers aged 19 (22% vs. 26%). The reform increased private sector deliveries at a higher rate for under-aged mothers. By 2010 36% of mothers aged 17 were delivering in a private hospital relative to 33% for mothers aged 19.

The children of mothers aged 17 have a slightly lower average birthweight, and a higher likelihood of low birthweight and prematurity than those of older mothers. Younger mothers are also less likely to initiate care in the first trimester and to have a C-section. When comparing the pre- and post-reform periods, we see improvements in the indicators of prenatal care and perinatal outcomes for mothers of all age groups. As expected, mothers aged 19 are more likely to have finished high school, although more than 90% of mothers have not completed high school by the time they give birth. A few mothers are married at delivery, but between 40% and 60% cohabit with the child's father.

5 Results

5.1 Main Results

Results for the difference in differences analysis are displayed in Table 2. Each column depicts a different regression, with column (1) explaining the probability of delivering in a private hospital, columns (2) to (4) analyzing perinatal health outcomes (birthweight, low birthweight, and prematurity, respectively) and columns (5) to (8) exploring health care utilization measures (onset of prenatal care in the 1st trimester, at least 3 prenatal care visits, at least 6 prenatal care visits, and delivery by C-section). We also estimate a regression at the adolescent-age-regional level explaining fertility decisions (column 9).[12] The rows in Table 2 show the interactions between an indicator of the mother being aged 17 at the time of birth and the year of delivery. While not shown, each regression includes a full non-parametric specification of age and year fixed effects as well as region (department) fixed effects, controls for mother's education and marital status, newborn's gender, gestation quarter, and department time varying characteristics (unemployment rate, education, and house ownership).

Column (1) provides evidence that the health reform was associated with a substantial increase in the use of private providers by adolescents relative to other mothers between 2008 and 2010. The gap for giving birth at a private hospital between a mother aged 17 and one aged 19 increased by 7.2 percentage points in 2008 (relative to 2007), by 10.3 percentage points in 2009, and by 10.9 percentage points in 2010. While prior to the reform older mothers were

[12] We run a regression of fertility rate on age dummies, year dummies and interactions of age-group and year effects, controlling for regional fixed effects and for time varying regional controls (unemployment, education, fraction of household owners).

more likely to give birth in a private hospital, the reform reverted the sign of the difference (see Figure 1 and the coefficients on the age-year interactions in Table 2 for the pre-reform period). While the pre-reform trends are not exactly parallel between our treatment and control groups, the widening of the gap prior to the reform, if anything, would be playing against our estimates. On the other hand, the lack of statistical significance in most of the pre-reform interactions in columns (2)-(9) suggests that 19-year old mothers are a good counterfactual of younger women in terms of the health outcomes and health care measures analyzed.

[INSERT TABLE 2 HERE]

[INSERT FIGURE 1 HERE]

In spite of the observed increase in the use of private health care by younger mothers, we see no evidence of changes in health outcomes or perinatal health care access following the reform. Most coefficients are statistically insignificant and small. The exceptions are the coefficients on the likelihood of having at least three prenatal care visits ($p<0.05$) and on the likelihood of having at least six prenatal care visits ($p<0.1$) for 2010, which exhibit negative signs. When assessing the post-reform aggregate effect for the years 2008-2010 (see last row in Table 2), we cannot reject the hypothesis that the reform had no effect on health outcomes or health services utilization.

As mentioned, the double difference analysis may not capture the causal effect of the reform due to the introduction of other policies in the period that affected either treated or control mothers. Table 3 presents the results of a triple difference analysis that exploits, in addition, regional differences in private coverage of adolescent mothers prior to the reform. Those areas with fewer adolescent mothers in the private sector prior to the reform were more likely to expand private adolescent coverage as of 2008. As in Table 2, each column in Table 3 represents a regression on a particular outcome. The table shows only the triple interactions

between an indicator of being aged 17 at the time of birth, year of delivery, and geographic-intensity of public coverage prior to the reform. Each regression includes, in addition, a full non-parametric specification of age, year and regional effects, as well as controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership). Figure 2, panels a) to i), provides a graphical analysis for each outcome in Table 3.

[INSERT TABLE 3 HERE]

[INSERT FIGURE 2 HERE]

Panel a) in Figure 2 shows that adolescent mothers in regions with higher public coverage prior to the reform were more likely to expand usage of private services, especially in years 2009 and 2010. Adolescent mothers in regions with a rate of public coverage 10 percentage points above average prior to the reform, increased their likelihood of delivering in a private hospital by between 5 and 6 percentage points in 2009 and 2010. The figure also shows that, in years prior to the social security expansion, the trend in use of private hospitals at delivery between young adolescent mothers in regions with a high fraction of public coverage and older adolescent mothers in other areas was not exactly parallel. While we acknowledge this identification problem, the trend differential was negative prior to the reform and the sign changed after the expansion of social security. If anything, this identification problem would be underestimating the effect of the reform on the uptake of private providers.

In terms of health care outcomes, the triple difference analysis shows no statistically significant evidence of reform effects on birthweight or low birthweight, although we find some evidence of increases in prematurity in the first year after the reform. Adolescent mothers delivering in areas with a pre-reform public coverage 10% higher than average increased the likelihood of a premature delivery by 1.8 percentage points in 2008 relative to older mothers in

other areas. Results also show a negative and marginally significant ($p<0.1$) coefficient on having at least three prenatal care visits in 2010, but no other statistically significant effects on health care use after the reform. When analyzing aggregate effects for the period 2008-2010, we find that both the effects on private delivery and prematurity remain statistically significant, but the effect on having at least three prenatal visits loses significance.

5.2 Robustness and Sensitivity

We conducted several sensitivity checks for the triple difference analysis. First, we re-estimated the model without adjusting for pregnancy-level characteristics (mother's education, marital status, gestation quarter, newborn's gender) and department time-varying characteristics. Results are very close to those in Table 3 (see Appendix Table 1), suggesting that unobserved changes in the composition of pregnant mothers are unlikely to lead our results.

Second, we expanded the set of treatment and control mothers. In a first approach, we defined treated mothers as those aged 16 to 17 at the time of delivery and control mothers as those aged 19 to 20 at delivery. We still observe a positive and statistically significant effect of the reform on the likelihood of prematurity in 2008 (though slightly smaller in size) and a negative effect of the reform on the likelihood of having at least three prenatal control visits, both in 2008 and 2010 (see Appendix Table 2). We also re-estimated the triple difference regression using the full sample of women between 14 and 45 years old at birth, with the exception of those aged 18 (results are displayed in Appendix Table 3). We observe a positive and marginally significant effect of the reform on prematurity ($p<0.1$) for 2009 and a negative and marginally significant effect on the likelihood of having at least 3 prenatal control visits in 2010. On the other hand, we now observe a positive effect on birthweight and a negative effect on the likelihood of low birthweight in 2008.

Third, we expanded the period of analysis to allow for some additional years post-reform. Concretely, we re-estimated the original triple differences model on the set of mothers aged 17 and 19 between 2002 and 2013. Note that between 2011 and 2013 the social security system extended benefits to couples of formal workers with children. The incorporation was gradual: the benefit extended in 2011 to couples of formal workers with at least three children, in 2012 to couples with at least two children, and in 2013 to couples with at least four children. Because the likelihood that a 19 year old has three children is relatively small, we can still be quite confident about the 2011 results. The identification strategy becomes more blurry as additional years are added. We present results in Appendix Table 4.[13] We continue to observe the positive effects on prematurity in 2008 and 2009. However, these effects are reverted in 2011. The coefficient on the 2011 triple interaction (when analyzing prematurity) is now negative, statistically significant and of similar size (but opposite sign) than the average 2008-2009 effect. We also observe in 2011 an increase in birthweight of 30 grams for a 10% increase in the intensity of private coverage prior to the reform, and a corresponding decrease in low birthweight.

Finally, we merged our data with data from the Perinatal Information System (SIP) and re-estimated our triple difference regressions using the same outcomes, but coming from a different source (SIP). SIP has more information on the process of perinatal care and on birth outcomes than Birth Registries. However, it does not have universal coverage, although coverage has increased notoriously over the years. To avoid confusing changes in pregnancy composition due to SIP expansion with reform induced changes, we compared, for each outcome, SIP averages with Birth Registry averages and found fairly good overlaps in all the series between 2006 and 2010. Our regressions using SIP cover thus only this period. Results

[13] The specification controls for pregnancy- and mother-characteristics, but not for time varying regional controls. We showed before that the exclusion of time varying regional controls had no effect on the results.

are quite consistent with those from Birth Registries: we find a statistically significant increase in prematurity in 2008, and negative and statistically significant coefficients on the likelihood of having at least three prenatal visits for 2009 and 2010. However, the triple difference parameters are now non-significant when explaining private coverage, a result due to large standard errors. We also assess new outcomes, such as mother and baby length of stay in hospital after birth and whether the mother smoked during pregnancy. We find a small increase in hospital length of stay for the newborn and a decrease in length of stay for the mother. We also find a marginally significant and positive coefficient when explaining smoking during the first trimester of pregnancy for births occurring in 2008.

6. Discussion

Our triple difference analysis shows that the social security expansion resulted, as expected, in an increase in the use of private providers by adolescents after 2008 in departments with low pre-reform private coverage. However, we do not find support for the hypothesis that the pro-choice reform led to improved perinatal health outcomes or to better use of perinatal health care. On the contrary, we find some evidence that during the first years, the shifts to private providers resulted in increases in prematurity and decreases in the probability of having at least three prenatal care visits. These findings suggest that the expansion of provider choice, at least during the first years of the reform, may have negatively affected the most vulnerable women, which are most likely to have inadequately controlled pregnancies.

Our results differ from those in the recent literature that support a positive association between pro-choice reforms and quality of health care provision. Several institutional features may explain this difference. First, in the period right after the implementation of the reform private providers had a smaller primary care network in disadvantaged neighborhoods than public providers. Providers in Uruguay are closed organizations with full time salaried staff and

geographically concentrated clinics. Cultural or financial barriers may have made it more difficult for vulnerable women to navigate the private system. For example, families that used to drop by at safety net clinics in their neighborhoods may have found it harder to understand how to schedule appointments or how to choose physicians in facilities located further away from their homes. Transportation costs may have also influenced their decisions to seek care. This hypothesis is in line with the findings in Bhalotra et al. (2017), who showed that the expansion in primary care facilities associated with the Family Health Program in Brazil during the 90's contributed significantly to improve birth-related outcomes.

Second, while social security insurance pays fixed capitated payments to providers, providers in Uruguay can discretionary set copayment levels, subject to a regulated cap. Conditional on the choice of provider, copayments do not directly affect access to prenatal care, as most prenatal care services are free from out of pocket charges.[14] However, because beneficiaries must select for a lock-in period of three years an organization that will be accountable for all the care received, flexible copayments introduce some price competition into the system. A comparison of information on enrollment and on web-based listings of copayments by provider, suggests that new beneficiaries in Uruguay were more likely to select providers with low copayments. If new beneficiaries are more responsive to price than quality, there is no reason to expect the pro-choice reform to improve provider quality. Moreover, women of lower socioeconomic status may end up choosing providers of lower quality if they are more responsive to copayments than other women. Prior literature has shown that pro-choice reforms can negatively affect quality of care when prices are flexible (Propper et al., 2004).

[14] Prenatal care was exempted from copayments in January 2006.

Third, the reform changed the relative price of health care labor. Wages in the Uruguayan private health care sector are set according to agreements between the medical unions and the providers.[15] The increased demand for private health care services that followed the reform expanded the demand for medical inputs. Given a fixed supply of physicians, this resulted in higher wages and in an augmented ratio of wages to costs. The nature of private providers may have also fed this process. Private providers in Uruguay are not-for-profit institutions owned by physicians' cooperatives, by physician unions, and by patients' cooperatives. The residual claimants are either salaried physicians or managers of the institutions, who seek to maximize wages, among other objectives. Figure 3 shows that wages in the health care sector increased by 17% in real terms between July 2007 and December 2010, while capitated payments to providers, fixed by the regulator, decreased 3% in real terms. The ratio of wages to total costs for private providers changed from 56% in January 2008 to 63% in January 2011. Informal qualitative evidence suggests that private providers rationed services by increasing waiting lists and decreasing length of consultations. Our results are consistent with those in Fleitas (2017), who explores the effects of removing lock-in restrictions on beneficiary mobility across providers during the first years of the Uruguayan reform on physician's wages and provider quality. Quality is measured by the hours worked by high skilled relative to low skilled physicians. Fleitas finds that the reform resulted in increased returns to skills for physicans, but not in increases in average quality, an effect he attributes to a very inelastic supply of high quality physicians in the short run.

Our core analysis studies only the first three years of the reform. Because of the magnitude of the changes involved, the adjustments required from providers and beneficiaries may have delayed potential benefits from the new system during these years. Our (less rigorous)

[15] Since 2008 the medical unions also negotiate public sector wages with the government, but public wages are still substantially lower than those in the private sector.

assessment for the following years shows some evidence of improvements in health outcomes in 2011 (higher birthweight and lower rates of prematurity), but not robust across the years.

Social security insurance expenditure increased by 131% in real terms between 2007 and 2010 to accommodate the 808,000 new beneficiaries that adopted the new scheme. A quick back of the envelope computation[16] suggests that the expansion of social security between 2007 and 2010 increased social security expenditure by 500 million dollars, about 1% of GDP. Considering that 32% of beneficiaries were already paying for private insurance out of pocket, and that the government was paying prior to the reform for the coverage of those in the safety net, the incremental costs of the reform exceeded the 300 million US dollars. If prenatal services are indicative of other health care services, our results suggest that the reform was cost-ineffective, at least during the first years. Unfortunately, prenatal health care is among the few services in the country with digital mandatory registries and we are unable to analyze other services. We cannot dismiss improvements in other health care areas and leave the task of studying health care services that go beyond the first level of care for a future research agenda. The perception of a quality gap between private and public provision is stronger for specialty care and copayments are likely to pay an important role in access to other type of care.

The amount spent by the government on safety net insurance also increased between 2007 and 2010 by 54% in real terms. Because the number of beneficiaries covered by the safety net decreased, per capita expenditure on the public safety net insurance almost doubled in the period, approximately reaching the levels of social security insurance in 2010. This budget increase may have improved the quality of services in the safety net, reducing the incremental

[16] Our computation assumes that the expansion of social security between 2008 and 2010 reached 256,000 beneficiaries that substituted social security for private insurance, 209,000 that were listed as users of the public safety net in 2007, around 306,000 that were not listed as public or private users, and 38,000 corresponding to population increase. We estimate that social security expenditure per capita was around 688 US dollars between 2008 and 2010 and safety net insurance expenditure was around 340 US dollars in 2007.

impact of social security insurance. We still believe that this change in safety net generosity does not invalidate our initial hypothesis proposing an association between more choice and better quality of care. Furthermore, the fact that expenditure per capita was almost equal by 2010 may have contributed to identify the effects of insurance generosity from insurance choice, had we found positive impacts of the social security expansion throughout the period.

Finally, our analysis does not address changes in welfare purely due to financial changes resulting from the reform. High income families who used to pay for private health insurance out-of-pocket were probably financially worse off due to the tax increase, but for middle income families the extent to which taxes exceeded previous out-of-pocket premiums depends on whether they purchased private insurance, on their level of income and the number of children. The financial status of very low income families who were previously uninsured or covered by the safety net and were now eligible for social security insurance was relatively unchanged in terms of taxes (a lower bound of the income distribution was exempted from the tax increases). However, it is important to note that new beneficiaries of the social security health insurance who used to be uninsured or used the public safety net probably increased their out-of-pocket costs after 2007 due to copayment charges in services other than prenatal care. The effects of copayments should be an important element to consider when assessing other health care services. While this analysis goes beyond the scope of the present paper, it should also be considered when assessing the impact of the reform.

7. Conclusions

During the past decades, Latin America has witnessed significant reform processes in the health care sector. These reforms include the Plan AUGE in Chile, the Seguro Popular in Mexico, the Act 100 of Colombia, the Integrated Health System in Brazil, the deregulation of Social Workers' Institutions in Argentina, and the Integrated National Health System in

Uruguay. These new plans were aimed at creating and strengthening solidarity pillars, promoting universal provision of a basic package of services, and offering universal access to care through the creation of unique systems (Mesa Lago, 2005; Mendez and Lopez Vanegas, 2010; Filgueira, 2014; Atun et al., 2014).

The Uruguayan health care reform initiated in 2007 pointed in this direction, expanding social health insurance and entitling new beneficiaries to choose services from a set of private and public providers. In the years following 2007, a large number of individuals shifted their source of care from public health clinics and hospitals to private providers. In this paper, we seek to answer whether the social security expansion and the associated increases in health expenditure and in the choice of health care providers improved the quality of perinatal care and health outcomes. We exploit the phased-in design of the social health insurance expansion, and, specifically, the fact that during the first three years, only children of formal workers under the age of 18 or disabled children were entitled to the insurance. The incorporation of other groups of women of childbearing age did not occur until 2011. The design allows us to use a methodology of double and triple differences, taking advantage also on the differential access to private coverage of younger adolescent mothers in the pre-reform years.

Unlike other investigations exploring the effects of choice on health care, our results show that higher expenditure and more choice did not lead to improved health care quality or to better perinatal health outcomes. We find some evidence, although not sufficiently robust, that the reform increased prematurity in the first year post-reform and decreased the likelihood of having at least three prenatal care controls. We attribute these results to the smaller network of primary care clinics in disadvantaged neighborhoods for private relative to public providers, to the lack of response of new beneficiaries to quality measures (whereas there was a clear shift towards providers offering low or no copayments), and to an increase in the weight of wages on total costs, which may have led to rationing (increased waiting times for appointments and

shorter visits). We hypothesize that the not-for-profit status of private providers, in addition to a shortage of skilled physicians (Fleitas, 2018), may also be behind the wage increases and the observed changes in quality. Future research should further explore these hypotheses, as well as the impact of the reform on health care services beyond the first level of care.

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Table 1. Summary Statistics

	Age 17 Pre-reform 2002-2007	Age 17 Post-reform 2008-2010	Age 19 Pre-reform 2002-2007	Age 19 Post-reform 2008-2010
Birth and health care outcomes				
Delivered in private hospital	0.221	0.364	0.262	0.330
Birthweight in grams (mean and std.dev)	3117	3151	3157	3195
	(538)	(553)	(538)	(533)
Low Birthweight (< 2500 grams)	0.098	0.094	0.088	0.081
Prematurity (<37 weeks)	0.106	0.104	0.092	0.087
Onset of prenatal care at 1st trimester	0.317	0.407	0.341	0.441
At least 3 prenatal care visits	0.922	0.926	0.915	0.929
At least 6 prenatal care visits	0.730	0.757	0.733	0.773
At least 9 prenatal care visits	0.415	0.457	0.429	0.478
C-section	0.204	0.242	0.219	0.258
Maternal characteristics				
Education< Elementary school	0.106	0.079	0.094	0.080
Elementary≤ Education <High school	0.853	0.887	0.819	0.840
Education >= High school	0.042	0.034	0.087	0.080
Education missing	0.038	0.041	0.036	0.056
Married	0.102	0.058	0.140	0.076
Marital status missing	0.003	0.140	0.003	0.133
Cohabitation	0.399	0.481	0.451	0.560
No information on living arrangements	0.002	0.031	0.002	0.026
Other pregnancy information				
Newborn´s sex	0.518	0.518	0.511	0.499
Gestation in 1st trimester	0.236	0.238	0.237	0.228
Gestation in 2nd trimester	0.247	0.248	0.261	0.265
Gestation in 3rd trimester	0.252	0.257	0.252	0.245
Gestation in 4th trimester	0.265	0.257	0.249	0.262
Department-level demographics				
% unemployed (mean)	0.060	0.038	0.061	0.038
% graduating from middle school (mean)	0.694	0.698	0.694	0.701
% graduating from high school (mean)	0.344	0.353	0.345	0.359
% household owner (mean)	0.677	0.627	0.674	0.626
N	8,771	4,581	12,767	6,534

Table 2. Difference in differences: mothers aged 17 vs mothers aged 19, by year (N=32653)

	Coverage	Health Outcomes			Health Care Use				Fertility
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	At least 6 prenatal care visits	Cesarean section	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-reform									
Age =17*2002	0.044**	41.971	-0.009	-0.024*	0.043*	0.008	0.025	0.002	-0.013***
	(0.018)	(25.527)	(0.014)	(0.014)	(0.022)	(0.013)	(0.021)	(0.019)	(0.005)
Age =17*2003	0.029	31.738	-0.006	-0.006	0.004	-0.004	-0.025	0.013	-0.005
	(0.019)	(25.704)	(0.014)	(0.015)	(0.022)	(0.013)	(0.021)	(0.020)	(0.005)
Age =17*2004	0.045**	40.044	-0.021	-0.029*	0.004	-0.010	-0.038*	0.014	0.002
	(0.019)	(26.133)	(0.014)	(0.015)	(0.023)	(0.013)	(0.021)	(0.020)	(0.005)
Age =17*2005	0.035*	55.770**	-0.013	-0.016	0.016	0.016	-0.006	-0.008	-0.003
	(0.020)	(26.360)	(0.014)	(0.015)	(0.023)	(0.013)	(0.021)	(0.021)	(0.005)
Age =17*2006	0.007	5.737	-0.007	-0.010	0.007	-0.010	-0.013	-0.015	0.001
	(0.019)	(25.910)	(0.014)	(0.015)	(0.023)	(0.013)	(0.021)	(0.020)	(0.006)
Post-reform									
Age =17*2008	0.072***	17.025	-0.003	-0.006	0.022	0.001	-0.012	-0.022	0.004
	(0.020)	(25.705)	(0.014)	(0.015)	(0.023)	(0.013)	(0.020)	(0.020)	(0.005)
Age =17*2009	0.103***	7.981	0.003	-0.006	0.000	-0.008	-0.022	0.007	-0.001
	(0.020)	(26.262)	(0.014)	(0.014)	(0.023)	(0.013)	(0.021)	(0.021)	(0.005)
Age =17*2010	0.109***	35.856	-0.013	-0.020	-0.018	-0.026**	-0.038*	0.016	0.007
	(0.020)	(25.891)	(0.013)	(0.014)	(0.023)	(0.012)	(0.020)	(0.021)	(0.004)
Avg effect post reform									
Age=17*I(Year>2008)	0.094***	20.350	-0.004	-0.011	0.001	-0.011	-0.024	0.000	0.003
	(0.016)	(21.348)	(0.011)	(0.012)	(0.019)	(0.010)	(0.017)	(0.017)	(0.004)

* p<.1, ** p<.05, ***p<.01. Robust standard errors in parentheses. Regression includes a full non-parametric specification of age and year effects, geographic area (department) fixed effects, and controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership). Omitted year: 2007.

Table 3. Triple differences: mothers aged 17 vs mothers aged 19 in regions with different intensity of public coverage, by year (N=32653)

	Coverage	Health Outcomes			Health Care Use				Fertility
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	At least 6 prenatal care visits	Cesarean section	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-reform									
Age =17*2002*intensity	0.201	9.553	-0.025	-0.004	0.198	0.041	0.237*	-0.100	-0.011
	(0.156)	(96.542)	(0.055)	(0.067)	(0.163)	(0.059)	(0.130)	(0.095)	(0.030)
Age =17*2003*intensity	0.274**	-177.558	0.093	0.125*	0.130	-0.020	0.009	0.083	-0.008
	(0.128)	(147.586)	(0.086)	(0.067)	(0.108)	(0.056)	(0.138)	(0.143)	(0.030)
Age =17*2004*intensity	0.222*	160.919	-0.014	-0.015	-0.017	0.074	0.107	-0.108	-0.012
	(0.131)	(113.100)	(0.071)	(0.071)	(0.104)	(0.070)	(0.125)	(0.133)	(0.027)
Age =17*2005*intensity	0.121	-49.760	-0.052	0.037	0.082	-0.021	0.153	-0.133	-0.017
	(0.105)	(104.285)	(0.057)	(0.063)	(0.090)	(0.053)	(0.111)	(0.141)	(0.017)
Age =17*2006*intensity	0.142	0.644	-0.019	0.026	-0.111	0.086**	0.039	-0.070	-0.002
	(0.121)	(122.595)	(0.060)	(0.065)	(0.121)	(0.040)	(0.081)	(0.116)	(0.027)
Post-reform									
Age =17*2008*intensity	0.122	-46.100	0.019	0.181**	0.014	-0.069	-0.020	-0.090	-0.012
	(0.151)	(96.278)	(0.083)	(0.072)	(0.118)	(0.065)	(0.106)	(0.106)	(0.018)
Age =17*2009*intensity	0.514***	-121.178	0.032	0.110	0.023	0.010	0.142	0.030	-0.002
	(0.125)	(145.533)	(0.060)	(0.073)	(0.144)	(0.048)	(0.123)	(0.110)	(0.026)
Age =17*2010*intensity	0.587***	-145.816	0.044	0.064	-0.003	-0.103*	0.140	-0.075	-0.029
	(0.138)	(125.908)	(0.045)	(0.063)	(0.175)	(0.060)	(0.143)	(0.142)	(0.021)
Avg effect post reform									
Age=17*I(Year>2008) *intensity	0.403***	-103.501	0.031	0.119**	0.011	-0.055	0.085	-0.047	-0.015
	(0.109)	(83.738)	(0.051)	(0.050)	(0.118)	(0.045)	(0.107)	(0.101)	(0.017)

* p<.1, ** p<.05, ***p<.01. Clustered standard errors (at the department and age level) in parentheses. Regression includes a full non-parametric specification of age, year and geographic area (department) effects, plus controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership). Omitted year: 2007.

Figure 1. Deliveries in private hospitals (%)

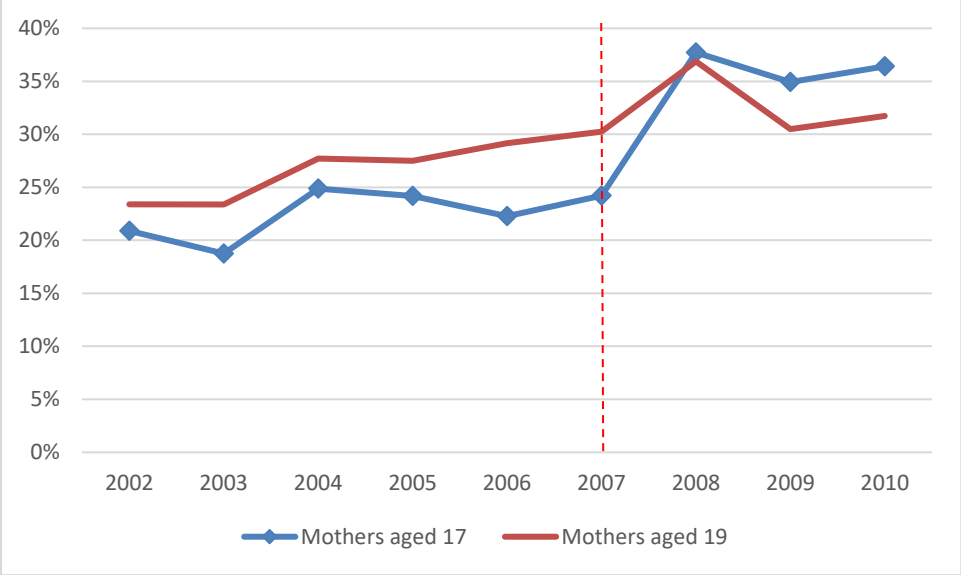
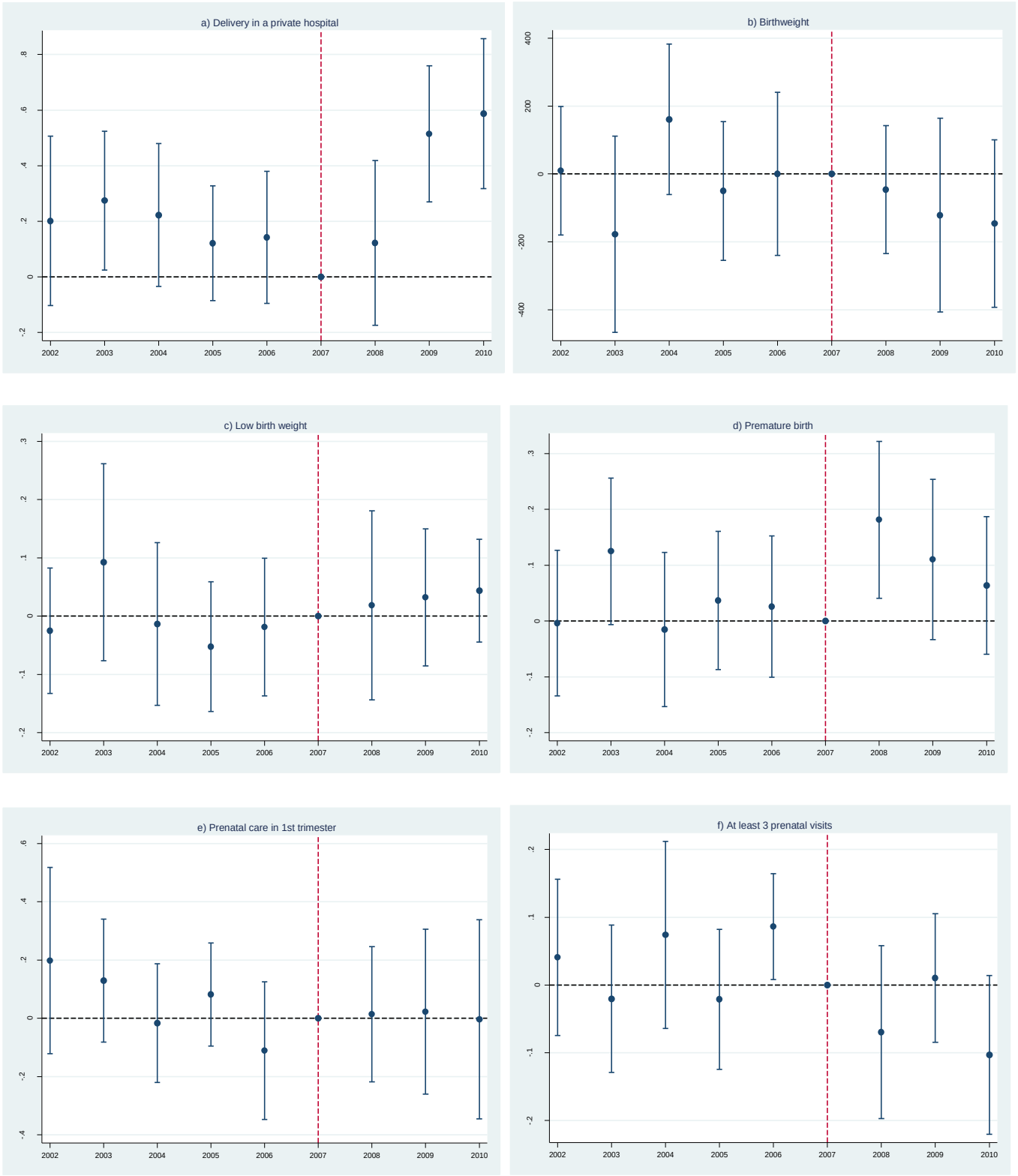
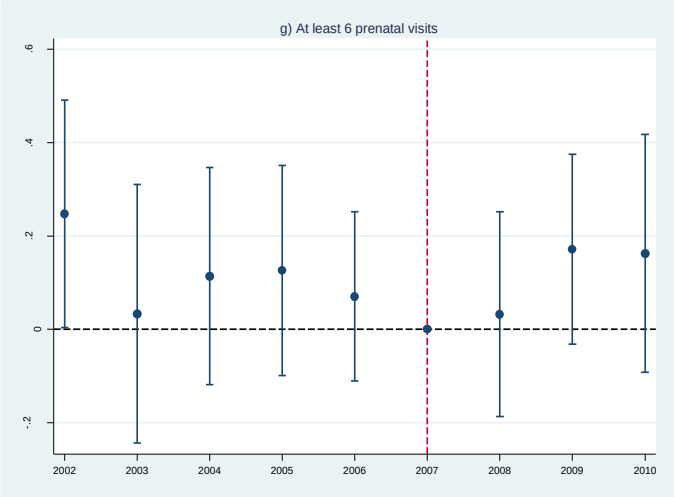
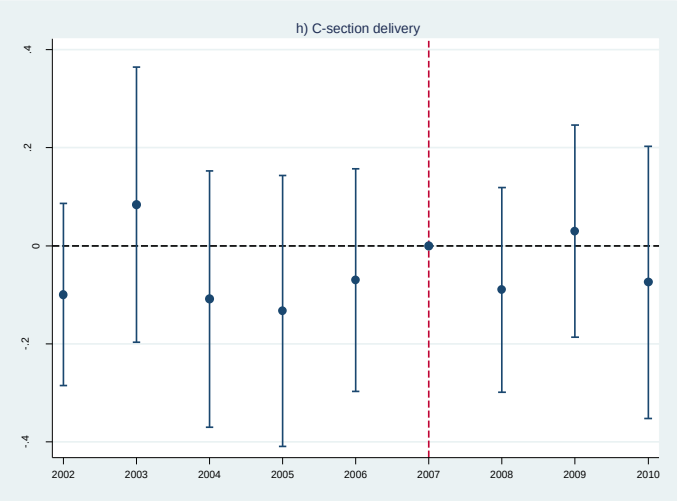


Figure 2: Triple Difference Effects




g) At least 6 prenatal visits
.6
.4
.2
0
-.2
2002
2003
2004
2005
2006
2007
2008
2009
2010


h) C-section delivery
.4
.2
0
-.2
-.4
2002
2003
2004
2005
2006
2007
2008
2009
2010

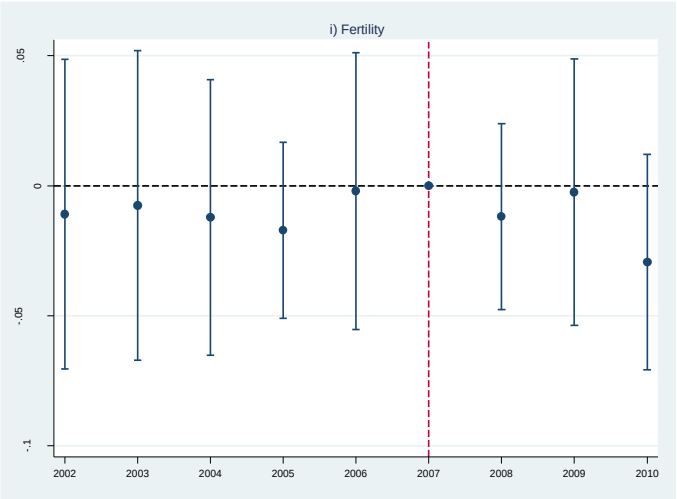
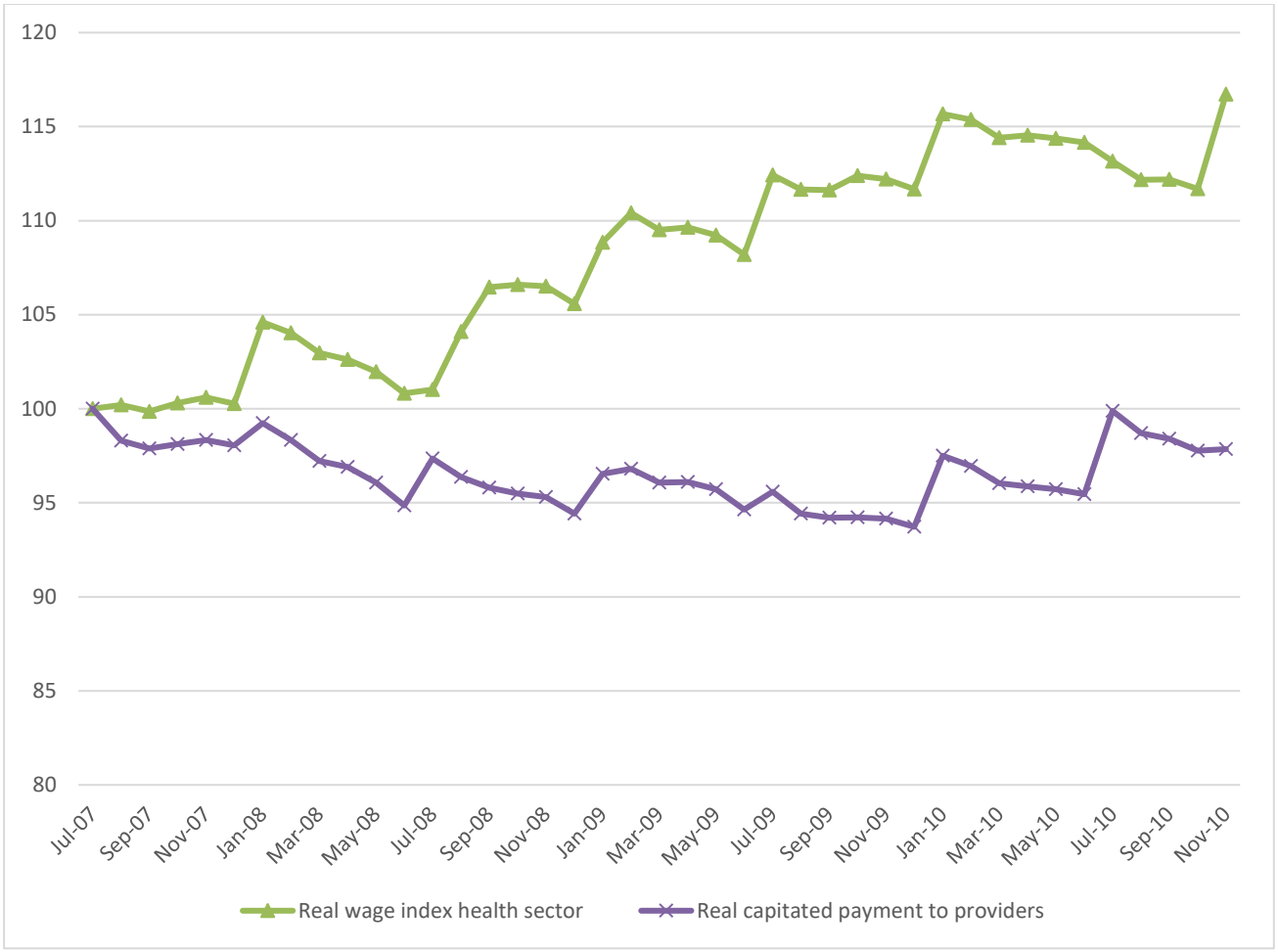

i) Fertility
.05
0
-.05
-.1
2002
2003
2004
2005
2006
2007
2008
2009
2010

Figure 3: Trends in physician wages and capitated payments to providers



Appendix Table 1. Triple differences: mothers aged 17 vs mothers aged 19 in regions with different intensity of public coverage, by year. Regressions excluding individual-level and geographic time-varying controls (N=32653).

	Coverage	Health Outcomes			Health Care Use			
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	At least 6 prenatal care visits	Cesarean section
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform								
Age =17*2002*intensity	0.250	32.045	-0.031	-0.007	0.213	0.054	0.261**	-0.101
	(0.164)	(98.632)	(0.058)	(0.063)	(0.152)	(0.054)	(0.115)	(0.104)
Age =17*2003*intensity	0.330**	-150.500	0.087	0.122*	0.144	-0.012	0.021	0.078
	(0.128)	(147.744)	(0.092)	(0.070)	(0.101)	(0.055)	(0.136)	(0.147)
Age =17*2004*intensity	0.204	176.150	-0.017	-0.013	-0.021	0.061	0.078	-0.110
	(0.162)	(110.446)	(0.082)	(0.074)	(0.097)	(0.073)	(0.122)	(0.154)
Age =17*2005*intensity	0.160	-34.095	-0.056	0.036	0.086	-0.019	0.157	-0.133
	(0.105)	(101.019)	(0.055)	(0.062)	(0.092)	(0.052)	(0.112)	(0.152)
Age =17*2006*intensity	0.168	9.392	-0.019	0.026	-0.110	0.083**	0.035	-0.068
	(0.112)	(128.745)	(0.061)	(0.063)	(0.119)	(0.039)	(0.076)	(0.114)
Post-reform								
Age =17*2008*intensity	0.136	-59.523	0.022	0.182**	0.006	-0.074	-0.026	-0.097
	(0.157)	(92.102)	(0.082)	(0.073)	(0.122)	(0.066)	(0.108)	(0.107)
Age =17*2009*intensity	0.537***	-110.520	0.031	0.110	0.022	0.007	0.137	0.026
	(0.126)	(145.630)	(0.058)	(0.073)	(0.138)	(0.051)	(0.124)	(0.111)
Age =17*2010*intensity	0.633***	-135.514	0.043	0.064	0.006	-0.096*	0.154	-0.073
	(0.148)	(125.600)	(0.040)	(0.061)	(0.166)	(0.056)	(0.140)	(0.141)

* p<.1, ** p<.05, ***p<.01. Clustered standard errors (at the department and age level) in parentheses. Regression includes a full non-parametric specification of age, year and geographic area (department) effects, plus controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership).

Appendix Table 2. Triple differences: mothers aged 16-17 vs mothers aged 19-20 in regions with different intensity of public coverage, by year (N=65847)

	Coverage	Health Outcomes			Health Care Use			
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	At least 6 prenatal care visits	Cesarean section
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform								
Age =17*2002*intensity	0.211*	24.479	-0.056	-0.038	0.021	0.001	0.165*	-0.133
	(0.122)	(108.639)	(0.053)	(0.052)	(0.120)	(0.043)	(0.092)	(0.119)
Age =17*2003*intensity	0.313**	9.270	0.016	0.035	-0.024	-0.063	0.011	-0.212
	(0.118)	(138.593)	(0.065)	(0.075)	(0.062)	(0.037)	(0.114)	(0.145)
Age =17*2004*intensity	0.229*	130.272*	-0.048	-0.010	-0.147**	-0.008	-0.015	-0.254**
	(0.120)	(76.710)	(0.051)	(0.042)	(0.060)	(0.055)	(0.091)	(0.111)
Age =17*2005*intensity	0.179*	-6.985	-0.061	-0.007	0.005	-0.044	0.099	-0.096
	(0.094)	(92.131)	(0.054)	(0.056)	(0.081)	(0.054)	(0.100)	(0.076)
Age =17*2006*intensity	0.118	-31.306	-0.059	-0.021	-0.041	-0.025	0.005	-0.242**
	(0.083)	(116.725)	(0.048)	(0.061)	(0.068)	(0.038)	(0.090)	(0.105)
Post-reform								
Age =17*2008*intensity	0.217	79.775	-0.021	0.119**	0.011	-0.117**	-0.056	-0.140
	(0.144)	(66.689)	(0.045)	(0.052)	(0.113)	(0.048)	(0.081)	(0.126)
Age =17*2009*intensity	0.482***	117.980	-0.056	0.066	-0.202*	0.016	0.048	-0.180
	(0.094)	(107.648)	(0.038)	(0.048)	(0.106)	(0.027)	(0.091)	(0.139)
Age =17*2010*intensity	0.664***	-40.395	-0.022	0.052	-0.135	-0.130***	0.033	-0.216
	(0.126)	(96.994)	(0.036)	(0.041)	(0.140)	(0.034)	(0.079)	(0.166)

* p<.1, ** p<.05, ***p<.01. Clustered standard errors (at the department and age level) in parentheses. Regression includes a full non-parametric specification of age, year and geographic area (department) effects, plus controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership).

Appendix Table 3. Triple differences: mothers aged 14-17 vs mothers aged 19-45 in regions with different intensity of public coverage, by year (N=399041)

	Coverage	Health Outcomes			Health Care Use			
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	At least 6 prenatal care visits	Cesarean section
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform								
Age =17*2002*intensity	0.238**	82.902	-0.073	0.007	-0.020	0.007	0.045	-0.100
	(0.117)	(105.372)	(0.045)	(0.055)	(0.081)	(0.047)	(0.103)	(0.064)
Age =17*2003*intensity	0.349***	162.032	-0.043	0.003	-0.037	-0.012	0.043	-0.003
	(0.103)	(121.679)	(0.052)	(0.057)	(0.056)	(0.040)	(0.117)	(0.086)
Age =17*2004*intensity	0.308***	136.185**	-0.032	0.008	-0.078	-0.002	-0.021	-0.042
	(0.105)	(64.989)	(0.045)	(0.038)	(0.048)	(0.040)	(0.085)	(0.097)
Age =17*2005*intensity	0.203***	46.799	-0.043	0.041	-0.003	-0.007	0.023	-0.096*
	(0.064)	(84.384)	(0.049)	(0.040)	(0.075)	(0.039)	(0.092)	(0.048)
Age =17*2006*intensity	0.162**	49.895	-0.005	0.057*	-0.086	-0.003	-0.027	-0.036
	(0.077)	(86.613)	(0.044)	(0.033)	(0.053)	(0.035)	(0.094)	(0.061)
Post-reform								
Age =17*2008*intensity	0.331**	261.418***	-0.083**	0.054	-0.009	-0.026	-0.019	-0.018
	(0.128)	(65.311)	(0.037)	(0.040)	(0.101)	(0.037)	(0.057)	(0.065)
Age =17*2009*intensity	0.589***	128.553	-0.008	0.062*	-0.096	-0.003	0.009	0.022
	(0.096)	(77.447)	(0.032)	(0.033)	(0.107)	(0.029)	(0.064)	(0.098)
Age =17*2010*intensity	0.574***	110.564	-0.042	0.051	-0.086	-0.050*	0.028	0.008
	(0.122)	(93.585)	(0.042)	(0.036)	(0.119)	(0.026)	(0.060)	(0.115)

* p<.1, ** p<.05, ***p<.01. Clustered standard errors (at the department and age level) in parentheses. Regression includes a full non-parametric specification of age, year and geographic area (department) effects, plus controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership).

Appendix Table 4. Triple differences: mothers aged 17 vs mothers aged 19 in regions with different intensity of public coverage, by year (N=46003).

Six years post-reform

	Coverage	Health Outcomes			Health Care Use			
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	At least 6 prenatal care visits	Cesarean section
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform								
Age =17*2002*intensity	0.201	55.132	-0.031	0.000	0.170	0.048	0.239**	-0.139
	(0.158)	(108.527)	(0.059)	(0.061)	(0.150)	(0.059)	(0.115)	(0.105)
Age =17*2003*intensity	0.265**	-92.232	0.058	0.122	0.122	-0.018	0.025	0.038
	(0.124)	(147.147)	(0.087)	(0.073)	(0.103)	(0.064)	(0.153)	(0.141)
Age =17*2004*intensity	0.250	225.248**	-0.045	-0.027	0.020	0.071	0.110	-0.140
	(0.167)	(107.788)	(0.072)	(0.065)	(0.100)	(0.070)	(0.124)	(0.141)
Age =17*2005*intensity	0.118	-58.818	-0.027	0.048	0.064	0.008	0.125	-0.174
	(0.124)	(106.888)	(0.062)	(0.063)	(0.088)	(0.064)	(0.116)	(0.141)
Age =17*2006*intensity	0.180*	28.698	-0.024	0.037	-0.084	0.115**	0.067	-0.070
	(0.106)	(145.833)	(0.061)	(0.058)	(0.109)	(0.045)	(0.090)	(0.112)
Post-reform								
Age =17*2008*intensity	0.133	-37.357	0.021	0.185***	0.021	-0.031	0.029	-0.113
	(0.165)	(84.860)	(0.076)	(0.067)	(0.121)	(0.066)	(0.106)	(0.100)
Age =17*2009*intensity	0.513***	-87.338	0.033	0.117*	0.026	0.019	0.171*	-0.020
	(0.127)	(146.743)	(0.056)	(0.066)	(0.128)	(0.041)	(0.093)	(0.111)
Age =17*2010*intensity	0.590***	-128.836	0.046	0.057	0.010	-0.077	0.168	-0.105
	(0.132)	(132.372)	(0.044)	(0.055)	(0.152)	(0.055)	(0.118)	(0.141)
Age =17*2011*intensity	0.528***	297.488***	-0.119**	-0.153**	0.117	-0.005	0.148	-0.251*
	(0.154)	(97.210)	(0.054)	(0.060)	(0.169)	(0.066)	(0.125)	(0.131)
Age =17*2012*intensity	0.652***	85.506	-0.053	0.010	0.043	-0.018	0.134	-0.089
	(0.224)	(122.259)	(0.062)	(0.062)	(0.196)	(0.060)	(0.149)	(0.132)
Age =17*2013*intensity	0.655***	93.930	-0.035	0.046	0.133	-0.042	0.154	0.083
	(0.228)	(124.254)	(0.064)	(0.064)	(0.193)	(0.042)	(0.121)	(0.201)

* p<.1, ** p<.05, ***p<.01. Clustered standard errors (at the department and age level) in parentheses. Regression includes a full non-parametric specification of age, year and geographic area (department) effects, plus controls for mother's education, marital status, newborn's gender, and gestation quarter.

Appendix Table 4. Triple differences: mothers aged 17 vs mothers aged 19 in regions with different intensity of public coverage, by year.

SIP Data (N=14690).

	Coverage	Health Outcomes			Health Care Use			
	Delivery in a private hospital	BW	LBW	Prematurity	Onset of prenatal care 1st trimester	At least 3 prenatal care visits	Mother's Length of Stay	Smoked in 1st trimester
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform								
Age =17*2006*intensity	0.380	300.763*	-0.038	-0.018	-0.354**	-0.094	1.044	-0.008
	(0.346)	(175.979)	(0.099)	(0.109)	(0.165)	(0.078)	(2.012)	(0.170)
Post-reform								
Age =17*2008*intensity	0.260	177.440	0.035	0.172**	-0.058	-0.135	-7.953**	0.168*
	(0.234)	(128.523)	(0.060)	(0.070)	(0.172)	(0.081)	(3.477)	(0.097)
Age =17*2009*intensity	0.393	10.236	-0.006	0.087	-0.142	-0.118**	-16.872***	0.150
	(0.266)	(185.964)	(0.089)	(0.100)	(0.170)	(0.057)	(5.225)	(0.172)
Age =17*2010*intensity	0.554	20.182	0.059	0.057	-0.129	-0.207**	-0.531	0.124
	(0.368)	(175.306)	(0.071)	(0.078)	(0.235)	(0.087)	(2.049)	(0.157)

* p<.1, ** p<.05, ***p<.01. Clustered standard errors (at the department and age level) in parentheses. Regression includes a full non-parametric specification of age, year and geographic area (department) effects, plus controls for mother's education, marital status, newborn's gender, gestation quarter, and geographic time-varying characteristics (unemployment, education, house ownership).