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# Secondary Schools and Teenage Childbearing: Evidence from the school expansion in Brazilian municipalities

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**Abstract**

This article investigates the effect of increasing secondary education opportunities on teenage fertility in Brazil. We construct a novel dataset to exploit variation from a 57% increase in secondary schools across 4,884 Brazilian municipalities between 1997 and 2009. We find that an increase of one school per 100 females reduces a cohort's teenage birthrate by between 0.250 and 0.563 births per 100, or a reduction of one birth for roughly every 50 to 100 students who enroll in secondary education.

JEL Codes: I20, I26, J13

Keywords: Secondary education, teenage childbearing, Brazil

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## 1. Introduction

A 2012 report by the World Bank on teenage pregnancy stresses the correlation between teenage childbearing and socioeconomic variables including poverty, inequality, public health expenditure and female labor force participation. The report shows that, despite substantial reductions in teenage pregnancy rates in virtually all countries, we continue to see rates differ vastly between high- and low-income countries, with Brazil—as a middle-income country—being placed somewhere in the middle of the distribution (World Bank, 2012).

Improved access to education, not explored in detail in the World Bank report, potentially provides an important channel through which teenage pregnancy and the above socioeconomic variables are correlated. Observational evidence shows a strong negative relationship between school availability and teenage childbearing in Brazil. We plot this relationship over time for Brazil in Figure 1. Between 1997 and 2009,[1] 9,402 secondary schools where introduced in Brazilian municipalities (a 57% increase), raising the average school density in municipalities from 1.06 per 100 teenagers to 1.54 per 100 teenagers. Over the same period, the birth rate for teenage girls[2] decreased by 21% from 8.1 births per 100 to 6.4 births per 100. This suggests an additional secondary school per 100 teenage females is associated with a decrease in births of 3.33 births per 100 teenage females. Evidence based on cross-sectional data shows a very similar picture. In Figure 2, we present the state-level relationship between school density and the rate of teenage childbearing. Based on this clear negative relationship we calculate that an additional secondary school per 100 teenage females is associated with a larger decrease in births of 17.9 births per 100 teenage females.[3]

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[1] In section 3.1., we provide the reasoning for the chosen period of 1997-2009 of this study.
[2] In this paper we define ‘teenage’ as ages 15–19 to match with the target entry age into secondary school.
[3] This estimate is based on a weighted linear fit, where the weights are the population of females aged 15-19 across Brazilian states.

In this paper, we investigate whether the negative association between secondary school availability and teenage childbearing is based on a causal relationship. We do this by looking at the effect of a large secondary school expansion across 4,884 Brazilian municipalities on teenage childbearing. Conditioning on municipal fixed effects and other determinants of school introductions, this expansion provides a plausibly exogenous source of variation in school availability. Our main results show that, on average, one additional secondary school per 100 females in the age cohort decreases teenage childbearing by between 0.250 and 0.563 births per 100 females. While being smaller than the effects that we measure using casual observation in Figures 1 and 2, our main estimates are statistically significant and economically relevant.

Our estimates rely on the assumption that the secondary school expansion is orthogonal to levels and trends in municipal teenage childbearing. We provide an analysis of the expansion and find that, controlling for variables reflecting school supply and demand, where and when a school is introduced is independent of variation in teenage childbearing. We also find no evidence of pre-trends in childbearing prior to school introductions. Our main estimates are robust to a number of different specifications and robustness checks, including estimates identified only from variation in the timing of school introductions across municipalities of similar size.

Brazil is particularly well suited for studying our research question. The school expansion that we examine constitutes one of the largest expansions of secondary schools on record. We use information from 13 waves of the annual Brazilian school census, containing detailed information on the universe of Brazilian schools, to create a new dataset reflecting the availability of secondary schools in every Brazilian municipality between 1997 and 2009. We combine this information with vital statistics data from Brazil capturing the universe of live births including information on the age of mothers at date of conception over the same period, creating a rich and unique dataset.

To our knowledge, this is the first paper to document and utilize data on the rapid growth of secondary schools across Brazil over the two decades starting in the 1990s.

This study contributes to a literature examining the relationship between education and fertility in young women. Lowering the explicit cost associated with education, by providing free school uniforms (Duflo, Dupas and Kremer, 2015) or the removal of school fees (Chicoine, 2020), leads to a greater number of girls in primary education and significantly reduces childbearing for young women in African countries. Increasing the time spent in education also affects teenage childbearing. In Chile, a greater number of full day, as opposed to half day, schools decreases the probability of adolescent motherhood (Berthelon and Kruger, 2011). Papers using variation from changes to mandatory schooling laws in high-income countries find that increases in the mandatory schooling age lead to a large and significant decrease in motherhood for young women in Norway, the US and the UK (Black, Devereux and Salvanes, 2008; Monstad, Propper and Salvanes, 2008; Geruso and Royer, 2018). However, a UK policy allowing young women to leave school six-months earlier does not lead to an increase in teenage motherhood (James and Vujic, 2019).

The findings in the literature indicate that there are multiple structural channels at work in the education-fertility relationship. School attendance restricts the time outside school available to young men and women (an *incarceration effect*), but education may also a change preferences or opportunity costs of young men and women (a *human capital effect*) (Black, Devereux and Salvanes, 2008).

We may also expect expanding access to schools to lead to changes in fertility through indirect channels. Secondary school education may increase a young adult's value in the marriage market (Fort, Schneeweis and Winter-Ebmer, 2016), or promote greater assortative matching, both of which may result in earlier childbearing. Previous studies have had limited success disentangling the relative importance of these different channels.

In this paper, we are mainly interested in testing for a causal relationship between the school expansion and teenage fertility, while we remain agnostic about the channels through which improving a community's school access might affect teenage childbearing. In addition to channels mentioned above, the introduction of a school may influence childbearing outcomes in difficult to identify, but nonetheless important ways. For example, improving access to secondary schools may disproportionately benefit human capital opportunities for girls relative to boys, and this may play a role in changing social norms towards early motherhood (Duflo, 2012). Our reduced form estimates reflect the net effect of all of these different channels.

We make two key contributions to the existing literature. First, our approach is different from several previous studies that exploit changes to mandatory school attendance age in countries with relatively good access to secondary schools, such as the US and Norway (Black, Devereux and Salvanes, 2008; Monstad, Propper and Salvanes, 2008). Increasing the school leaving age in these settings elicits a behavioral response from youths for whom the benefit of school attendance (subjective or objective) is low (Brunello, Fort and Weber, 2009; Fort, Schneeweis and Winter-Ebmer, 2016).[4] In contrast, the school expansion leads to a behavioral change from young men and women who attend school when access is good—but not when access is poor—without explicit compulsion. In affluent countries with good school access, we do not expect this margin of the population to respond to a change in mandatory schooling laws. For this reason, estimates based on mandatory schooling laws may be uninformative for inferring the effect of changes in

[4] Further, the observed effect of mandatory schooling laws on fertility is found to be context-dependent. Using changes in mandatory schooling laws, Fort, Schneeweis and Winter-Ebmer (2016) they find a negative relationship between education and fertility in England, but no such relationship for Continental Europe. They attribute differences in labour markets and marriage markets as potential sources of these differences.

access to schooling. Despite the stark difference in the approach chosen in this paper, our estimates are similar to the findings of studies using compulsory schooling. For example, compelling youths to attend school until age 16 reduces the probability of a teenage birth by 4.7% in the US and 3.5% in Norway (Black, Devereux and Salvanes, 2008). We find that one more school per hundred females reduces the rate of teenage births for a cohort by 1.4%.[5]

The second contribution comes from our focus on a middle-income, as opposed to high-income, country. Teenage childbearing in Brazil is significantly higher than in many high-income countries, but lower than in many of the poorest countries.[6] In addition, the focus on Brazil enables us to study a recent and rapid expansion of secondary schools over a relatively short time period, which is not possible in the context of high income countries, where such expansions have taken place decades earlier and often in a more gradual fashion.[7] We are therefore able to provide the first quantification of the role of expanding education access in the substantial reduction of teenage fertility rates observed across many middle-income countries over the last two decades. The expansion of secondary schooling in Brazil provides a blueprint for understanding the effect of the expansion of the educational system on fertility. The estimates provided here are therefore of relevance for a large group of low- and middle-income countries currently experiencing or on the brink of similar expansions of their educational systems.

The remainder of this paper is organized as follows: In Section 2 we provide the background on the provision of secondary education in Brazil. In Section 3, we discuss the data to be used in the main analysis. In Section 4, we introduce the empirical strategy and show that the introduction

[5] Based on our preferred estimate of -0.565, and the corresponding mean cohort birth rate: $(-0.565 \times 100)/41.0$.
[6] The World Bank reports teenage birth rates (in births per 1000) of 82.2 for Brazil, 12.7, 28.1 and 46.2 for Germany the United Kingdom and the United States. The highest birth rate was for Niger, at 217.2 births per 1000 teens (World Bank, 2020).
[7] This limits in many instances the data that is available on the rollout of school expansions in these settings. Brazil provides a great setting for our study by being able to investigate the largescale school expansion making use of high-quality administrative data for the entire country.

of schools in Brazilian municipalities is uncorrelated with latent factors that might impact fertility decisions. In Section 5, we present and discuss the results. In Section, 6 we provide additional robustness checks. Section 7 concludes the article.

## 2. Background information

Secondary education in Brazil typically lasts for three years and is preceded by 9 years of primary school.[8] Of the 11,007 public secondary schools in 1997 for the municipalities in our study, 97% were under state control. State secretariats of education are responsible for the regulation and general management of secondary schools, including the recruitment of teachers and curriculum content (JBIC, 2005). There is no minimum age for initial enrolment to secondary school, but it is targeted at age 15, and students must have completed primary school first. There is no maximum age limit and, because of common late enrolment and grade retention in primary schools, age-grade mismatch at secondary school is frequent (Foureaux Koppensteiner, 2014). In 2010, about 30% of students in the first grade of secondary school were above the target age (IBGE, 2012).

Over the last two decades, many low- and middle-income countries have undergone an expansion of their secondary education system, driven by improvements in primary school completion rates and an increased demand for a more highly skilled workforce (World Bank, 2005). In Brazil, secondary schooling was an overlooked part of the education system until the beginning of the 1990s (Guimarães de Castro and Tiezzi, 2004). Prior to the 1990s, secondary education was highly geared to the elites in preparation for entrance to higher education and was considered of little relevance for the education of the broader population. Following the end of the military dictatorship, the introduction of the constitution of 1988 and the General Education Law

[8] In 2006, primary education was extended to nine years, with children regularly entering primary school in the year they turn six (before the end of March). For the most of our analysis before the mid-2000, primary education started at the age of seven and lasted for eight years, hence this change does not affect our cohorts of interest.

(LDB) of 1996 made access to secondary education a key aim on the political agenda, mandating it to be available (although not mandatory) for all those completing primary education. Following the rapid expansion of primary education, which lead to universal enrolment towards the end of the 1990s, secondary education started to expand rapidly as well (Marchelli 2010).

The expansion followed the increasing demand from larger numbers of final year primary students (Moore, DeStephano, Terway and Balwanz, 2008; Di Gropello, 2006; De Felizio, 2009)—we test this formally in Section 4. While the expansion in primary education was largely the responsibility of Brazilian municipalities, in part funded by federal resources through the programme FUNDEF (*Fundo de Manutenção e Desenvolvimento do Ensino Fundamental*), the 26 Brazilian states and the federal district were in charge of the expansion of secondary education using financial support from the federal program PROMED (*Programa de Melhoria e Expanção do Ensino Médio*). The explicit aim of PROMED was to address excess demand for secondary education from primary students (Monteiro de Barros and Mendes Najjar 2017).

The expansion resulted in a 57% increase in the number of secondary schools, from 16,562 in 1997 to 25,964 in 2009. This was driven primarily by a 68% increase in the number of publicly funded schools, from 11,007 to 18,526. The school expansion had a non-trivial impact on school access across Brazilian municipalities. In particular, there was a remarkable increase in the availability of schools to the poorer northern states of Brazil; Figure 3 maps the change in secondary school availability across municipalities.[9] In 1997, 316 municipalities, representing 6.5% of all Brazilian municipalities, had no secondary school. By 2009, this number had dropped to 12. There was also a notable 34% increase in the provision of private secondary education, from

[9] See appendix Figure A1 for the corresponding map of changes in teenage childbearing between 1997 and 2009.

5,555 to 7,438. The number of students in secondary schools increased from under 6.4 million in 1997 to 8.3 million in 2009 (INEP, 2003; INEP, 2011).[10]

## 3. Data

The primary data used in this study comes from two sources: the Brazilian school census (*Censo Escolar*) and Brazilian vital statistics data (*SINASC*) from the Ministry of Health. In addition, we use auxiliary data, including population estimates for Brazilian municipalities from the Brazilian Census Bureau, and municipal expenditure data from a variety of sources. Descriptive statistics for the key variables in our analysis are reported in the supplementary appendix (Table A1). As municipal boundaries changed over the period of interest, we base our analysis on 4,884 minimal comparable areas (see Appendix B for details). For simplicity, we continue to refer to the unit of observation as municipality.

### *3.1 Schooling data*

We use 13 waves of the Brazilian school census, collected annually for the Ministry of Education by the *Anisio Teixeira Institute of Research on Education* (INEP) which provides administrative data on the universe of schools in Brazil (Glewwe and Kassouf, 2012). The school census includes detailed information on the universe of public and private schools in Brazil, such as enrolment by grade, age and sex, information on the number of classes, the physical characteristics of the schools, as well as information on teachers.[11] The school census is available from 1995, but because of inadequate quality of the data, we discard the first two census years. We use the census data to create a dataset on the number of secondary schools, the number of classrooms and the number of students between 1997 and 2009—when the vast majority of the school expansion has

[10] This increase is mirrored by a steep increase in education expenditure; between 2000 and 2009, Brazil reported the largest increase in education spending as a percentage of total public expenditure for 33 countries for which data is available (OECD 2012).
[11] These data can be downloaded at the website of INEP (http://portal.inep.gov.br/microdados).

occurred—collapsing the data by municipality and year. By our definition, a new school is introduced in a municipality if a new unique school identification number appears in the school panel.[12] Information on municipality codes allows us to locate every school in Brazil to the corresponding municipality. The school census provides information on primary school enrolment, the availability of nursery classrooms, and the number of pre-school classrooms, which we use as control.

*3.2 Childbearing data*

Data on birth outcomes come from the microdata of Brazilian vital statistics, which cover approximately 45 million births occurring between 1997 and 2009. Vital statistics data are based on birth certificates issued by health institutions or midwifes attending homebirths and are collected through the states' health secretariats. The vital statistics microdata are publicly available through the System of Information on Life Births (SINASC) of the Brazilian public health system (DATASUS). These data provide information on the age and municipality of residence of the mother, as well as gestational length of the pregnancies, and the mother-reported race of the child.

For each year, we collapse these data to create a summary measure of births by municipality and mother's age at conception.[13] We calculate age at date of conception using information on gestational length recorded in the birth certificates to provide a municipal panel of births by mother's age at conception.[14] Brazilian vital statistics data show close to universal coverage of all occurring births; information from the 2010 population census shows that more than 99% of all births occurring between 2000 and 2010 were registered and entered into the vital statistics data

[12] We restrict the school count to schools that are active in a given year and report a positive number of enrolled students to limit measurement error in the school panel.
[13] This works in the Brazilian context, as the school year coincides with the calendar year.
[14] Using date of conception rather than date of birth of the child, allows us to capture the time when fertility decisions are taken and relate this to the timing of the introduction of schools.

we use. The advantage of using vital statistics data to learn about fertility in the population comes from the universal coverage of the data for the entirety of Brazilian municipalities over the period of interest. Information about the residence of the mother during pregnancy is particularly important, as information on the place of birth may be misleading if there is a discrepancy between place of residence of the mother and the place of occurrence of birth, which is more likely for relatively small municipalities that do not have clinics with birth facilities.

### *3.3 Auxiliary data*

*Population estimates*

The Brazilian Census Bureau (IBGE) provides official population estimates for each municipality based on the 1990 and 2000 census and the 1996 and 2006 population counts. These data provide population estimates by sex and age group that we use in all the regressions to account for cohort sizes.

*Municipality controls*

We use a rich set of municipality-level time-varying controls on the characteristics of the municipalities from a variety of sources. These include municipality GDP, and the fraction of municipality level expenditure on education, health, welfare, transportation and housing, provided by IBGE. In addition, we use information on the number of *Bolsa Família*[15] recipients and the total amount of *Bolsa Família* payments in the municipality. These data are available annually for the 1997–2009 period.[16] We provide details on the source of these data in the appendix (Table A2).

## 4. Empirical Strategy

### *4.1 Identification strategy*

[15] Bolsa Família is a cash transfer program of the Brazilian government for poor Brazilian families conditional on meeting requirements regarding school attendance and completion of vaccination schedules.
[16] The Bolsa Família programme was introduced in 2004. Data for municipal GDP is available from 1999.

Our outcome of interest is the teenage *birthrate*, denoted by $B_{it}$. $B_{it}$ is the cumulative number of births conceived between age 15 and age 19, per 100, in municipality $i$, by the cohort that is age 19 in year $t$:

$$B_{it} \equiv \frac{b_{it}^{19} + b_{it-1}^{18} + b_{it-2}^{17} + b_{it-3}^{16} + b_{it-4}^{15}}{females_{it}^{19}}. \tag{1}$$

where $b_{it}^{a}$ is the number of live births conceived by mothers of age $a$ in municipality $i$ and year $t$ and $females_{it}^{19}$ is the total number of females age 19 in municipality $i$ and year $t$.

We are interested in the effect that an increase in municipal school availability has on the birthrate. Our measure of school availability is referred to as secondary school density, denoted by $S_{it}$. This is calculated as the number of schools divided by the cohort size of females. Secondary school density, as with birth outcomes, is measured as the number schools per 100 females in the cohort.

Variation, to identify the intention-to-treat effect (ITT) of an increase in school density, comes from differences across municipalities in the within-municipality change in the number of secondary schools. To illustrate, we start the analysis with a stylized two-by-two difference-in-differences analysis (Table 1). We compare the change in the birthrate between 2000 and 2009 for municipalities that received at least one new school over this period (*treated*) and municipalities that did not receive a school over this period (*control*). Relative to the control municipalities, the treated municipalities saw an increase of 0.79 secondary schools per 100 students, roughly an 80 percent increase. While both control and treated municipalities experienced a decrease in birthrates between the two periods, the average decrease in treated municipalities is more accentuated with a difference of 0.65 births per 100 students enrolled. Attributing the difference in birthrate decrease to the difference in secondary schools increase, a unit change in school density decreases the teenage birthrate by 0.82 units (-0.65/0.79). As will be shown in Table 3, this basic calculation is similar to what we find in the full regression analysis.

Clearly, this conclusion relies on the assumption that school introductions do not vary systematically with changes in unobservables across municipalities that influence birth outcomes.[17] As an initial test, in Panel B of Table 1 we report the results of a falsification exercise, using birth outcomes for an older cohort group. The cohort aged 29 in 2009 would have been 20 years old in 2000; older than the secondary school target age group. It should be noted that this is an imperfect exercise; we cannot rule out that an older cohort was affected by the school introduction, as there is no age limit to enroll in secondary school, and there is frequent late enrolment into secondary schools. However, we expect to see an effect that is considerably less than that of the younger target-age cohort. As with the younger cohort, the older cohort experiences a reduction in birthrates over the two periods. However, there is no birthrate difference between treatment and control municipalities over time.

*4.2 Exogeneity of Brazil's secondary school expansion*

The interpretation of the conditional correlation between birth rate and school density as an intention-to-treat effect requires the identifying assumption that school introductions are conditionally random with respect to births, consistent with the controlled experiment above. In Section 2, we argue that supply-demand factors largely determine the school expansion across municipalities and over time. To examine this empirically, we estimate models of the between-municipality differences in the school expansion. We start by investigating the probability a municipality is part of the expansion, for which the outcomes is a binary indicator for receipt of a

[17] For example, we are interested in programs that include family planning components, such as the Family Health Program (Programa Saúde da Família). We find that the majority of the rollout of this federal Ministry of Health program happened around the millennium; we find no evidence of coordination with the expansion of secondary school, which was led by state education secretariats (Rocha and Soares 2010).

new school between the years 1998 and 2009. We then investigate the timing of the introduction of schools across municipalities. For municipalities that receive a school, the outcome is an ordinal variable equal to the number of years from 1997 that a school expansion is realized.[18] These two outcomes are regressed on 1997 municipal characteristics and a set of state dummy variables (Table 2).

Consistent with our identifying assumption, the correlation between the teenage birthrate and both the introduction of a secondary school or the timing of the expansion is neither significant nor large in magnitude. Rather, we find that supply-demand factors are important in explaining the school expansion. Municipalities with high secondary school density in 1997, which we interpret as a supply factor, are less likely to receive a school in the preceding periods and, if they do receive a school, the expansion takes place later than in municipalities with low initial school density. The municipality's enrolment rate in the final year of primary school, which we interpret as a demand factor, is positively correlated with how early the municipality receives a new secondary school, but does not determine the receipt of a school. This is consistent with a policy objective to equalize secondary school access across Brazilian municipalities, while giving priority to those municipalities that have a higher immediate demand (Soares, 1998).

Identification also depends on the independence between school introductions and trends in birth outcomes. We investigate pre-trends in birth outcomes for municipalities that received secondary schools by performing an event study-style analysis,[19] constructing visual plots reflecting time-demeaned births relative to the periods just before and after a school introduction:

[18] In the case of municipalities that receive more than one school, this outcome reflects receipt of the first school.
[19] The complexity in conducting an event study in this framework is that some municipalities experience multiple 'events' by having schools introduced at multiple points in time. We simplify the analysis and focus on only the first 'event' (i.e. the first observed change in the number of secondary schools). We do not attempt to infer a causal relationship from this exercise, the purpose is only to examine pre-trends.

$$B_{it} = \sum_{d=-13}^{13} \lambda_d 1[t - e_i = d] + \vartheta_i + \mu_{it}. \quad (2)$$

The outcome, $B_{it}$, is the cohort birth rate in municipality $i$ in year $t$. The event for municipality $i$ is the year in which a change in the number of secondary schools is first observed, $e_i \in [1997, 2009]$, and the indicator function $1[t - e_i = d]$ takes a value of 1 when the difference between year $t$ and $e_i$ is $d$ and 0 otherwise. The parameters $\lambda_d$ reflect the average (demeaned) birthrate $d$ periods away from the event, $d = 0$, normalizing $\lambda_{-1} = 0$. $\vartheta_i$ reflects the mean birth rate for municipality $i$ and within-municipality deviations from the mean are captured by $\mu_{it}$. Estimated values of $\lambda_d$, for $d \in [-4, 4]$ are plotted in Figure 4, for classroom density (4a) and the birthrate (4b) as outcomes (bars around point estimates reflect a 95% confidence interval). From the resulting figures we conclude that there are no systematic trends in municipal rates of teenage childbearing prior to the introduction of a secondary school.[20]

*4.3 Estimation Strategy*

The difference-in-difference estimates of Table 1 ignore variation in the timing of school introductions and do not control for time-varying observables that may explain changes in birthrates. To address this, our primary analysis is based on the following regression equation:

$$B_{it} = \alpha S_{it-4} + \gamma PE_{i,t-5} + \boldsymbol{X}'_{it}\boldsymbol{\Lambda} + year_t + \delta_t^s + \eta_i + \epsilon_{it}. \quad (3)$$

The outcome is the cohort birth rate for municipality $i$ in year $t$, as defined in Equation (1). The explanatory variable of interest, $S_{it-4}$, is secondary school density in municipality $i$ in year $t - 4$ (when the cohort reflected in $B_{it}$ was 15).[21] $PE_{i,t-5}$ is the enrolment rate for students in the final-

[20] In Appendix C2 we provide further event-study analysis using a subsample of the municipalities and birth rates for specific mother ages. As with Figure 4, we do not find any systematic evidence of pre-trends.

[21] We focus on the school density when the cohort was the target age for starting secondary school (age 15). This raises the potential concern of underestimating the effect of the school expansion on teen childbearing. Considering other measures such as density at age 16 or mean number of schools in teenage years yields results that are similar in magnitude.

year primary classes at year $t-5$.[22] $\boldsymbol{X_{it}}$ is a vector of municipal controls including the number of pre-schools, total male to female sex ratio, total and per-recipient Bolsa Família transfers, and—both in logs and four-year log-differences—municipal GDP and public spending (welfare, education,[23] health, transportation, and housing). Unobservable heterogeneity is captured by a year trend, $year_t$, a state × year specific shock, $\delta_t^s$, a municipal fixed effect, $\eta_i$, and a time-varying municipal component, $\epsilon_{it}$.

The resulting estimate, $\hat{\alpha}$, reflects the intention-to-treat effect of a one unit increase in secondary school density on teenage birthrates. Municipal differences of the within-variation in the secondary school expansion between the years 1997 and 2005 identify this coefficient.[24] For an unbiased estimate of $\alpha$ we assume strict exogeneity between the error term and school introductions. This assumption is supported by the analysis in Section 4.2. We subject this assumption to further tests and specifications in sections 5 and 6. The remarkable stability of our results across specifications indicate to us that this assumption is plausible.

## 5. Results

Estimates for Equation (3) are presented in Table 3. The coefficient of interest, $\alpha$, is reported in the first row [*Secondary school density (t-4)*]; estimates for selected control variables in $\boldsymbol{X_{it}}$ and $PE_{i,t-5}$ are reported in the remaining rows. All regressions include a year trend and municipality

[22] The first observation for $PE_{i,t-5}$ (at $t = 2001$) is constructed from the 1996 wave of the Brazilian School Census.
[23] This largely captures municipal spending on pre-primary and primary education, rather than on secondary education (which is the responsibility of the states). Excluding education expenditure leaves the estimates virtually unchanged. Results available from authors.
[24] In an earlier version of this paper, we instrumented secondary school enrolment using the expansion of secondary schools in year t-4. This has the benefit of normalizing the estimates to be in terms of per student enrolled, rather than "school" which is not a well-defined unit of measurement and can vary in size across municipalities. It has the disadvantage in that it may ignore important effects of introducing schools into a municipality that arise through channels other than school enrolment. The results reported here are qualitatively similar to our previous findings (Foureaux Koppensteiner and Matheson, 2019).

fixed effects, municipality-clustered standard errors are reported in parenthesis.[25] For each specification, we report both the within-variation R-squared and between-variation R-squared.

Column 1 shows results of the benchmark specification including all municipalities in the sample. Based on this estimate, for a unit increase in school density we expect to see a decrease of 0.269 births per 100. If the mechanism operated entirely through school enrolment, we can get an intuitive sense of the magnitude to interpret this effect. An increase in school density leads to an average enrolment increase of 28.87 pupils per 100 cohort females.[26] Given this, for every 107 students enrolled we expect to see a reduction of one birth (28.87/-0.269). In Column 2 we include municipal spending information and state-specific year trends. The addition of control variables increases the within-variation explained by approximately 50 percent (a 6.5 percentage point increase). However, we observe very little change in the estimated value of $\alpha$ relative to the specification in Column 1.

In Columns 3 to 5 we further weaken the identification assumption by restricting the sample to municipalities that are more similar. In Column 3, the sample excludes 34 municipalities with populations greater than 500,000.[27] Estimates do not meaningfully change from the previous columns. In Column 4, we further restrict the sample to the 2,550 municipalities with populations between 10,000 and 100,000. In Column 5 we further exclude from the sample all municipalities that did not receive a new school between 1997 and 2005 (Column 5). As these are the years from which our identifying variation comes, the final specification relies on the relatively weak

[25] The estimates reported in Table 3 are considerably more precise than the estimate reported in Table 1. This is largely attributable to the fact that the regression analysis allows us to exploit variation in the timing of the school expansion across municipalities, whereas the difference-in-differences analysis of Table 1 relies only on variation in school expansion participation.
[26] Regression results on corresponding student enrolment rates are reported in supplementary appendix Table C1.
[27] The 34 cities with a population above 500,000 stand out from the vast majority of municipalities with their very large populations and extreme population density, making these municipalities not easily comparable to the majority of municipalities in Brazil.

identifying assumption that the timing of school introduction is strictly exogenous, rather than whether a school is introduced or not. The magnitude of our estimates increases in the final two specifications, indicating an effect of between -0.563 and -0.499. Column 5, which requires the weakest identifying assumptions, implies that there is one fewer teenage childbirth for every 48 students enrolled due to the increase in secondary school provision (23.8/-0.499).

The stability of the estimates across these various specifications provides additional support for our identification strategy. If there were time varying characteristics that both influence birth outcomes and are correlated with school introductions, then the estimates would be expected to change as we restrict municipalities to those that are more "similar" in terms of school introductions. Indeed, we find that the estimates are remarkably consistent across the different samples, lending additional credibility to the identification strategy.

We also investigate heterogeneous effects on teenage births by municipality characteristics. We focus on two sets of municipality characteristics: measures that reflect the state of the local economy (the ratio of wages for workers with at least secondary education, versus below secondary education and the municipal unemployment rate) and measures of population density and dispersion (population per $km^2$ of land and proportion of rural students enrolled). Estimates, corresponding to our preferred specification of Column 4 in Table 3, are stratified by the median values for each of these variables (see Table C3 in Appendix C). We do not find substantial differences in estimates across the different municipal characteristics. The effect of school density on birth rates is almost identical for municipalities in which the observed earnings for secondary education is above the median versus those below the median. There is a small difference for municipalities in which the local unemployment rate is high versus those in which it is low. Less dense municipalities (as measured by population per $km^2$ and rural student enrolment) also tend to have a larger effect.

## 6. Testing for potential threats to the identification strategy

The preliminary analysis in Section 4 is consistent with the assumption of exogeneity of the secondary school expansion across municipalities is plausible. The stability of estimates across different sub-samples of municipalities reinforces this finding. In this section, we discuss additional threats to the identification strategy and how we test for these. We explore further the possibility that the expansion of secondary schools is correlated with other municipal-level programs not controlled for by the municipal spending variables we include in Equation (3).

### *6.1 Unobserved municipality characteristics*

One concern for the causal interpretation of the estimates in Table 3 is that there may be unobserved municipal characteristics that are correlated with teenage births and influence a municipality's receipt of a secondary school, leading to a spurious correlation between birth rates and school density. We investigate this using two strategies. First, a binary indicator for the introduction of a secondary school in year $t$ is regressed on the lag of birth rates, $B_{it-1}$ (Panel A, Table 4). Second, a binary indicator for the introduction of a secondary school is regressed on the percent change in the number of births between year $t$ and $t-4$, $(B_{it} - B_{it-4})/B_{it-4}$ (reported in standard deviations, Panel B, Table 4). Regressions also include the lagged value of primary school enrolment, $PE_{i,t-1}$, the vector of controls $X_{it}$, as well as state-time trends and municipal fixed effects. For display purposes, coefficients and standard errors are multiplied by 100.

These results from these regressions are consistent with school introductions being conditionally independent of birth rates. Correlations between municipal birthrates and school introductions are small and statistically insignificant.

*6.2 Births to older mothers*

As there is no explicit age restriction to enrolling into secondary education in Brazil, there is no age cut-off at which we can say *treatment* is received or not. However, as the majority of secondary school students enroll between the ages of 15 and 17, we expect the secondary school expansion to have less an effect on the fertility of older women compared to younger women. Finding a qualitatively similar effect on births at ages 30 and older may indicate that unobservable factors are influencing municipal childbearing more generally.

To test this, we regress the municipal births for women age 15 to 40 ($b_{it}^{a}$, $a = 15, \ldots, 40$) on the contemporaneous school introductions. Regressions include age-specific male and female sex-ratios, but otherwise contain the same control variables as in Equation (3). The conditional correlations between contemporaneous school density and the percent change in age-specific births are reported in Figure 5. Estimates for all groups over 23 years of age are statistically insignificant and small in magnitude. We interpret this as further evidence that there are no unobserved municipal changes that affect both childbearing and the introduction of secondary schools.

*6.3 Selective migration*

Migration might lead us to estimate a negative value for $\alpha$ if the introduction of a secondary school induces an outflow of sexually active teens, creating a spurious negative relationship between school introductions and teenage childbearing. In Appendix C3 we provide a detailed analysis of migration patterns in Brazil, using information from the 2010 Brazilian population census available publically through IPUMS International. These data allow us to analyze migration patterns for a subset of the Brazilian municipalities (831 municipalities). We summarize the key findings here.

Migration data from the population census demonstrates the following: a) relative to other migrant and non-migrant women, women who migrate between ages 10 and 18 are significantly more likely to be teenage parents; b) migrants tend to leave municipalities with low school expansion in favor of municipalities with high school expansion; c) the difference in school expansion between the destination and origin municipality is higher for teenage mothers than for non-teenage mothers. These stylized facts all work against finding a negative relationship between school density and teenage birthrates. As a result, migration may lead to an underestimate of the true magnitude. Therefore, the results reported in Table 3 should be considered a lower bound on the parameter $\alpha$.

## 7. Concluding Remarks

In this article, we investigate whether an increase in secondary education access, through an increase in the municipal density of secondary schools, impacts the fertility decisions of teenagers in the middle-income context of Brazil. Our estimates are consistent with the hypothesis that the school expansion played an important role for teenage fertility in Brazil. A one-unit increase in secondary school density in municipalities leads to an average decrease in a cohort's teenage birth rate of between 0.250 and 0.563 births per 100. These estimates suggest, under some assumptions, a reduction of one birth for roughly every 50 to 100 students who enroll in secondary education due to the expansion.

The data we use in this study have the advantage of near-universal coverage of births, and significant variation from a large school expansion across 4,884 municipalities. However, a limitation of these data is that we cannot link birth outcomes to schools at an individual level. Therefore, our results should not be interpreted as capturing the effect of school enrolment on an individual's outcome. Rather, we measure the intention-to-treat effect, reflecting the availability of secondary education opportunities in a municipality.

The findings in this paper also contribute to a literature that looks at the impact that expanding education access has in developing communities. Several studies have previously looked at the quasi-random variation from the primary school expansion in Indonesia. This expansion is found to have a substantial impact on education and adult wages (Duflo, 2001, 2004). In addition to labor market outcomes, women have fewer children as adults (Akresh, Halim, and Kleemans, 2018). Providing evidence from an RCT, Burde and Linden (2013) find that introducing a primary school in Afghan villages significantly increases academic participation and performance for both boys and girls. However, the gains for girls are disproportionately large, significantly reducing the enrolment and the performance gap between boys and girls. An analysis of Brazil over the same period as our study finds a strong association between teenage fertility and income inequality within a municipality (Chiavegatto Filho and Kawachi, 2015), and an inverse relationship between regional teenage pregnancy and regional measures of the human development index (Ferreira Vaz, et al., 2016). Our analysis provides some insight into the relative importance of access to secondary education in these relationships and adds to this observational evidence by providing evidence on a pathway through which school access may change gender-based inequalities in a municipality. If secondary access affects both fertility and social mobility, the school expansion we document may lead to fundamental changes in income inequality in the longer-run.

The between-state variation in teenage pregnancy rates (Figure 2) may be also reflect broader differences in economic and social issues. Kearney and Levine (2012) argue that the substantial geographic variation in US teen pregnancy rates reflects geographic differences in economic opportunity—both perceived and real. It is being on a low economic life trajectory that leads teenage girls to have children. In the context of the current study, by decreasing the geographic distance necessary to attend secondary school, the Brazilian expansion improved opportunities for secondary school attendance. In this sense, our estimated negative relationship between schools and teenage childbearing is consistent with Kearney and Levine's findings and improving access to secondary schooling may improve the (perceived) life trajectory for young women.

**Data availability statement**

The data and code underlying this article are can be accessed at shorturl.at/coGY9.

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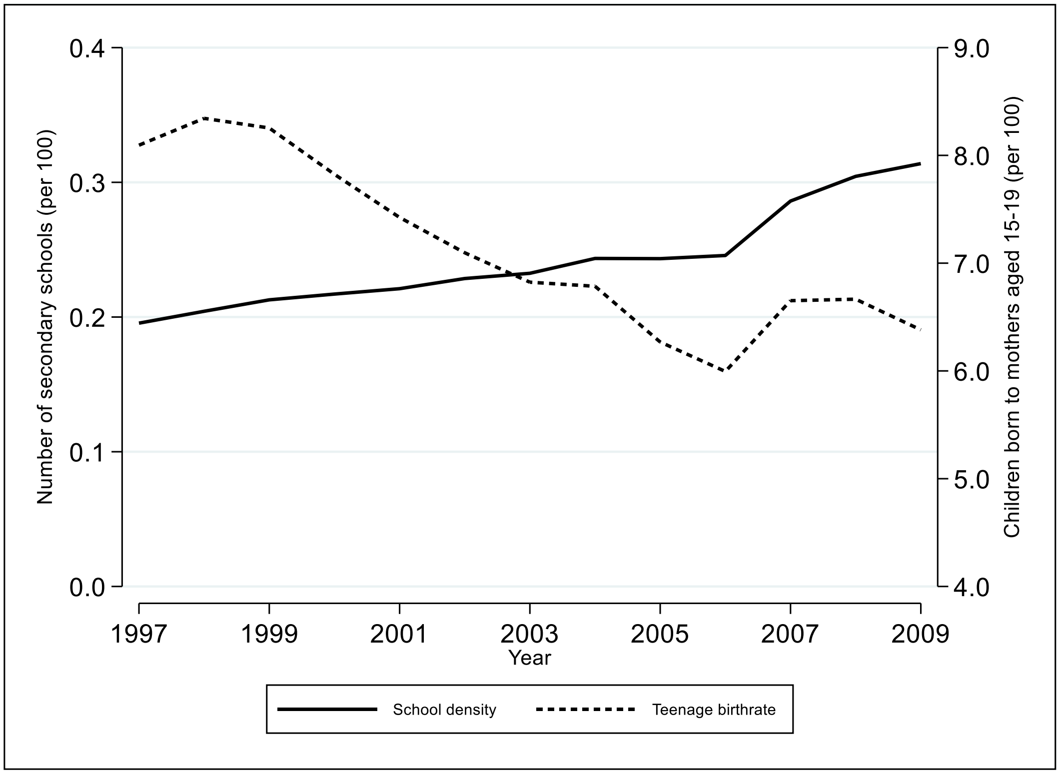
Figures

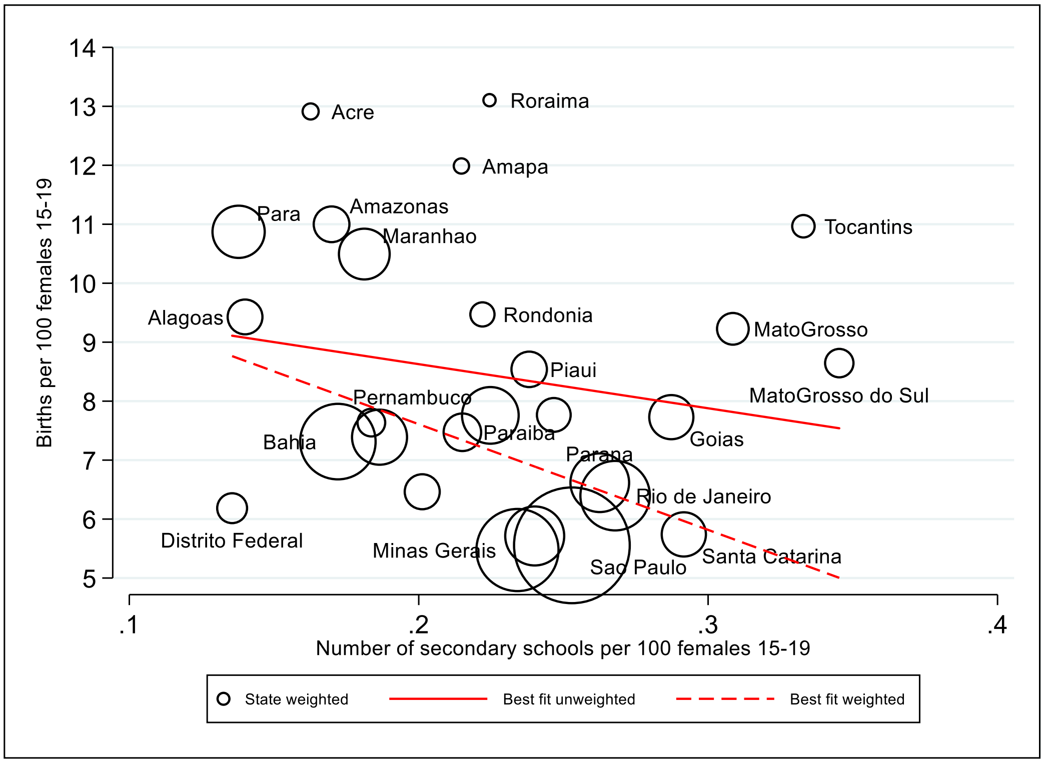
Figure 1: Secondary school density and teenage birthrate over time, Brazil




Notes: This figure shows the annual number of secondary schools and births for the period 1997 to 2009 per 100 females age 15–19.

Source: School data come from the 1997–2009 waves of the Brazilian School Census; official population estimates from the Brazilian Census Bureau; births by age of conception from Brazilian Vital Statistics.

Figure 2: Secondary school density and teenage birthrate across Brazil, 2002

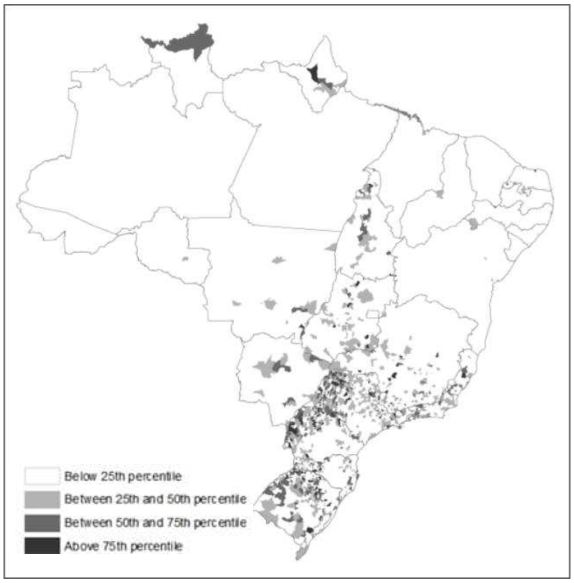



Notes: Data for 2002 cross-section. Marker size weighted by population of females aged 15–19. Broken line shows linear fit weighted by population size, solid line shows unweighted linear fit. Births are for mothers aged 15–19.

Source: School data come from the 2002 wave of the Brazilian School Census; official population estimates from the Brazilian Census Bureau; births by age from Brazilian Vital Statistics.

Figure 3: Classroom density by percentile

1997




2009

Source: Brazilian School Census 1997 and 2009. Percentiles held constant at 1997 cut-offs.

Figure 4: Event study for cohort births and classroom density

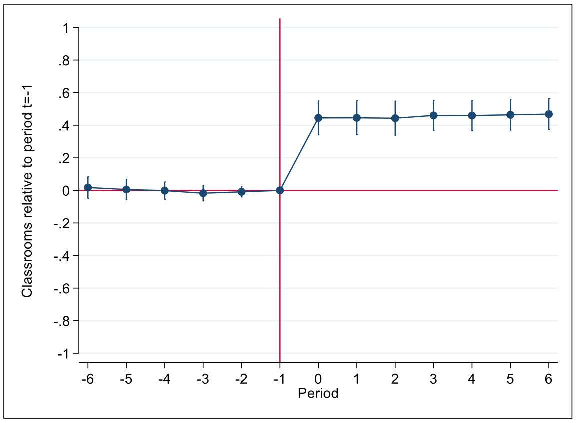



Figure 4a: Classrooms per 100 youth age 15–19

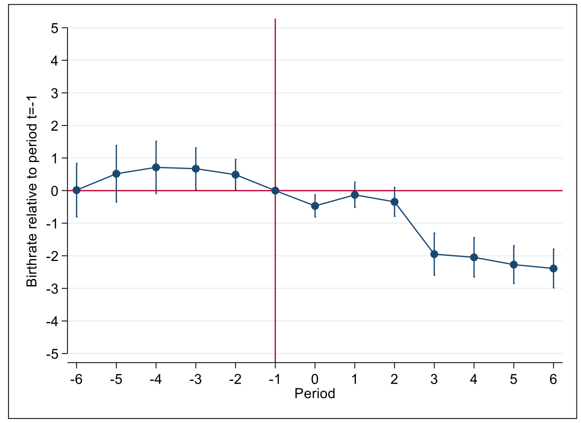
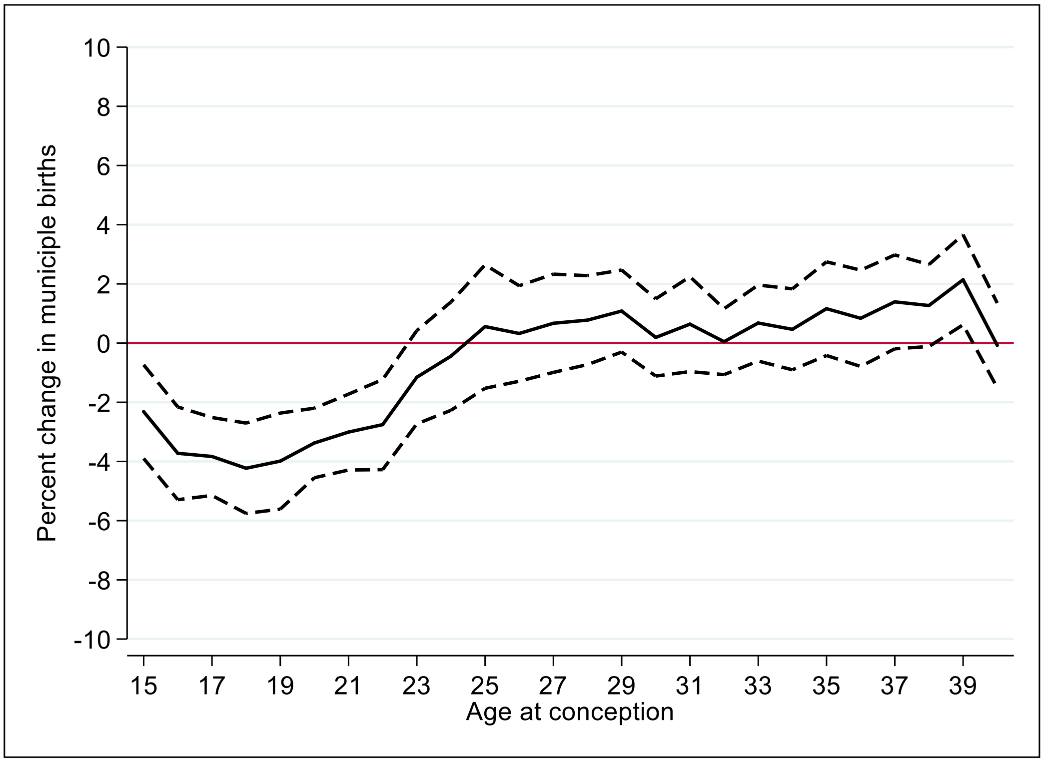



Figure 4b: Cohort birth rate (age 15–19)

Notes: These figures plot the coefficients from an event study; the first observed secondary school introduction in a municipality is the “event”. Bars indicate 95% confidence intervals. All estimates condition municipality fixed effects. Includes all municipalities that received at least one new school over time (3,470 municipalities).

Figure 5: Contemporaneous secondary schools and births (percent change) by age




Notes: This figure plots the coefficients from a regression of the number of live births for each age group on contemporanious secondary schools. The solid line plots these coefficients divided age-group specific means for the outcome ($\times 100$). Dashed lines show the corresponding 95% confidence interval. All regressions condition on lagged primary school enrolment, preschool classrooms, male and female age-specific population size, municipality expenditures, municipality and year fixed effects.

Table 1: Simple difference in differences

| | Births per 100 | | | Schools per 100 | | |
|---|---|---|---|---|---|---|
| | 2000 | 2009 | Difference | 2000 | 2009 | Difference |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| A. Cohort births age 15–19 | | | | | | |
| Control municipality | 43.93 | 36.48 | -7.45 | 1.13 | 1.15 | 0.02 |
| | | | (0.54) | | | (0.01) |
| Treated municipality | 44.56 | 36.47 | -8.10 | 1.06 | 1.88 | 0.81 |
| | | | (0.56) | | | (0.03) |
| Difference | 0.64 | -0.01 | -0.65 | -0.06 | 0.73 | 0.79 |
| | (0.68) | (0.65) | (0.66) | (0.03) | (0.03) | (0.02) |
| B. Cohort births age 25–29 | | | | | | |
| Control municipality | 46.10 | 41.66 | -4.44 | | | |
| | | | (0.59) | | | |
| Treated municipality | 47.33 | 42.89 | -4.45 | | | |
| | | | (0.43) | | | |
| Difference | 1.24 | 1.23 | -0.01 | | | |
| | (0.65) | (0.45) | (0.72) | | | |

Notes: This table shows a the difference in cohort births between ages 15 and 19 per 100 (Panel A), and between ages 25 and 29 per 100 (Panel B). Sample includes municipalities with populations >10,000 and <500,000. Estimates weighted by population size. *Treated municipalities* received at least one new secondary school between 2000 and 2009, *control municipalities* did not receive a new school between 2000 and 2009.

Table 2: Between municipality variation in school expansion

| *Outcome* | (1) *New school indicator* | | (2) *Timing of new school* | |
|---|---|---|---|---|
| | Coefficient | Standard error | Coefficient | Standard error |
| Teenage birth rate | -0.0007 | (0.0018) | 0.0057 | (0.0109) |
| Enrolment rate (primary year 8) | 0.0003 | (0.0003) | -0.011*** | (0.0019) |
| Secondary schools per 100 | -0.064*** | (0.0047) | 0.322*** | (0.0405) |
| Pre-school rooms per 100 | 0.0000 | (0.0000) | -0.0008 | (0.0007) |
| Male/female sex ratio (teen) | 0.066** | (0.0257) | -0.1122 | (0.1844) |
| Public spending—Total | -0.0012 | (0.0032) | -0.044** | (0.0180) |
| Public spending—Welfare | 0.0031 | (0.0024) | 0.0026 | (0.0150) |
| Public spending—Education | 0.0061 | (0.0049) | 0.0179 | (0.0287) |
| Public spending—Health | -0.0031 | (0.0045) | 0.0059 | (0.0253) |
| Public spending—Transport | -0.004** | (0.0019) | -0.0032 | (0.0106) |
| Public spending—Housing | -0.0021 | (0.0024) | -0.0079 | (0.0146) |
| Population per km2 | 0.0000 | (0.0000) | 0.0000 | (0.0001) |
| Municipality size (<10,000 excluded) | | | | |
| 10,000-49,999 | 0.247*** | (0.0205) | 0.329** | (0.1328) |
| 50,000-99,999 | 0.271*** | (0.0397) | -0.0884 | (0.2282) |
| 100,000-499,999 | 0.184*** | (0.0526) | -0.3055 | (0.3068) |
| >=500,000 | 0.0425 | (0.1016) | 0.1856 | (0.7288) |
| Log(total population) | 0.095*** | (0.0135) | -0.437*** | (0.0816) |
| | | | | |
| R2/Pseudo R2 | 0.3774 | | 0.055 | |
| Observations | 4,884 | | 3,160 | |
| | | | | |
| | F-stat | p-value | Chi2 | p-value |
| Joint significance—Municipal spending | 1.34 | 0.2345 | 8.51 | 0.2029 |
| Joint significance—State dummy variables | 9.36 | 0.0000 | 226.8 | 0.0000 |

Notes: This table reports the results of a regression of two variables reflecting the school expansion. Column 1 reports a linear probability regression in which the outcome is a binary indicator equal to one if a new school was introduced to a municipality in the period 1998–2009, and 0 otherwise. Column 2 reports an ordinal logit regression in which the outcome reflects the timing of a municipality's first school new school (number of years after 1997). Only municipalities that received a new school during the period1998–2009 are including in the second regression. Both regressions include state dummy variables. Population size is based on the average total population between 1997 and 2009. Robust standard errors are reported in parenthesis. ***, **, and * denote statistical significance at 1%, 5% and 10%.

Table 3: Regression of municipal cohort birthrate on municipal secondary school density

| *Outcome: Number of births conceived between age 15–19, per 100 females* | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Secondary school density (*t-4*) | -0.269*** | -0.250** | -0.252** | -0.563*** | -0.499*** |
| | (0.115) | (0.112) | (0.112) | (0.196) | (0.200) |
| *Control variables* | | | | | |
| Primary enrolment (*t-5*)† | -0.014*** | -0.015*** | -0.015*** | -0.013** | -0.011* |
| | (0.003) | (0.004) | (0.004) | (0.005) | (0.006) |
| Pre-school rooms‡ | 0.001* | 0.001** | 0.007*** | 0.001 | -0.001 |
| | (0.000) | (0.000) | (0.002) | -(0.004) | -(0.004) |
| Male/Female ratio | 19.264*** | 18.753*** | 18.698*** | 19.406*** | 19.769*** |
| | (1.025) | (1.000) | (1.002) | (1.161) | (1.234) |
| Bolsa Família (log) | | -0.985*** | -1.093*** | -0.618*** | -0.803*** |
| | | (0.114) | (0.123) | (0.176) | (0.192) |
| Bolsa Família (pre recipient) | | 0.018 | 0.021 | 0.041** | 0.036** |
| | | (0.015) | (0.015) | (0.016) | (0.017) |
| Year (linear trend) | X | X | X | X | X |
| Municipal fixed effects | X | X | X | X | X |
| Municipal spending and GDP | | X | X | X | X |
| State by year effects | | X | X | X | X |
| Municipalities in sample | All | All | Pop <500k | Pop 10k−100k | Pop 10k–100k + new school 1997–2005 |
| R2 (within) | 0.130 | 0.195 | 0.195 | 0.296 | 0.316 |
| R2 (between) | 0.078 | 0.052 | 0.050 | 0.050 | 0.054 |
| Observations | 43,941 | 43,941 | 43,635 | 22,576 | 17,963 |
| Municipalities | 4,884 | 4,884 | 4,850 | 2,550 | 2,037 |

Notes: This table reports the results of regressing cohort births, from age 15 to age 19, per 100 females in period $t$ on the number of secondary schools per 100 in period $t-4$. In addition to the reported variables, estimates in Columns 2–5 condition log-municipality expenditures and the change in log-municipality expenditures. The third column of results omits municipalities with populations greater than 500,000 from the sample. Columns 4–6 restrict to municipalities with populations greater than 10,000 and less than 100,000. Columns 5 and 6 additionally restrict to municipalities that received at least one new school in the periods 1997–2009 and 1997–2005, respectively. Municipality-clustered standard errors are reported in parenthesis. ***, **, and * denote statistical significance at 1%, 5% and 10%.
†*Primary enrolment* is the lagged number of students enrolled in year 8 of primary school in period $t-5$ in each municipality.
‡*Pre-school room* reflects the contemporaneous number of municipal pre-school classrooms, a proxy for pre-school availability.

Table 4: Regression of municipal school introduction on lagged municipal birth trends

| *Outcome: New secondary school indicator* | | | | |
|---|---|---|---|---|
| **A** | (1) | (2) | (3) | (4) |
| Births per 100, 15–19 years *(t-1)* | 0.0012 | 0.0002 | -0.0004 | 0.0029 |
| | (0.0130) | (0.0130) | (0.0396) | (0.0490) |
| Observations | 43,955 | 43,649 | 22,578 | 17,963 |
| Municipalities | 4,884 | 4,850 | 2,509 | 1,996 |
| **B** | | | | |
| Births per 100 at age 15−19, 5-year growth rate | 0.0008 | 0.0008 | -0.0010 | -0.0018 |
| | (0.0012) | (0.0012) | (0.0018) | (0.0025) |
| Observations | 43,952 | 43,646 | 22,578 | 17,963 |
| Municipalities | 4,884 | 4,850 | 2,509 | 1,996 |
| Municipalities in sample | All | Pop <500k | Pop 10k−100k | Pop 10k–100k + new school 1997–2005 |

Notes: This table reports the results of regressing a binary variable, indicating a new school introduced to a municipality in year $t$, on the number of births in period $t-1$ (Panel A), and the growth rate in teen births over the previous 4 years (Panel B). Coefficients are multiplied by 100 for display purposes. Growth in teen births calculated as the average annual percent change in cohort births between *t* and *t-4*. Estimates condition on lagged primary school enrolment, nursery and preschool classrooms, municipality expenditures, municipality and year fixed effects. Municipality-clustered standard errors are reported in parenthesis.