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Essays in Health and Population Economics

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Chapter I

Introduction

Fertility and migration are key ingredients of population dynamics. They influence economic development and the well-being of societies. Understanding the factors behind these dynamics in different geographical and cultural contexts is essential to designing effective public policies.

This doctoral thesis consists of four research papers on Health and Population Economics. Three of them focuses on the dynamics of fertility in Uruguay—two of which carry out impact evaluations of relevant policy interventions—and the fourth looks at differences in health care use between natives and immigrants in Spain, exploring how the patterns of utilisation among foreign-born population evolve with age and the time spent in the host country.

Uruguay is an exceptional case in Latin America and the Caribbean regarding its demographic, social and political characteristics. The country started the demographic transition early and now has vital statistics indicators comparable to those observed in developed countries. These indicators include low fertility and crude birth rates, high life expectancy and low infant mortality rates. However, these indicators mask differences in reproductive behaviour across income and education levels. In particular, the adolescent fertility rate, concentrated in the poorest households, remained very high until 2014, when it sharply declined.

The first paper ("The short and long-term determinants of fertility in Uruguay", Chapter II) examines the main drivers of fertility in Uruguay over the last five decades. It separately looks at the factors that shape the fertility of women in three different reproductive stages: adolescence (15–19 years old),the intermediate stage (20–29 years old) and the late stage (30 years old and older). To this end, I collect long time series of demographic, social and economic variables country and department level and apply time series and panel data econometric techniques. The first type of exercise indicates the existence of a cointegration relationship between fertility and economic performance, education and infant mortality, with differences by reproductive stage. In particular, the study highlights the negative relationship between education and adolescent fertility, which has implications for the design of public policies. In addition, the study finds a negative relationship between income and fertility for women aged 20–29, which persists for women aged 30 and older. This may indicate that having children is perceived as an opportunity cost for women in this age group. The econometric analysis of the panel data, which uses department-level information and allows controlling for unobserved heterogeneity, confirms the relevant role of income for all groups of women and reinforces the key role of education for curbing teenage fertility. The negative association between fertility and employment rates for women aged 30 and older highlights the importance of the opportunity costs of motherhood and the considerable scope for more and better social protection policies (e.g., parental leave).

It is well known that Uruguay has a very high level of social development, reflected not only in one of the highest standards of living in the hemisphere, but also in a long history of remarkably progressive social policies. For example, Uruguay offers universal health care and high-quality, free prenatal care and family services. The country was also one of the first in Latin America and the Caribbean to legalise homosexual unions and cannabis for recreational use. This dissertation provides an evaluation of two of the pioneering policies adopted by the national authorities: the decriminalisation of abortion and a programme consisting in offering subdermal contraceptive implants to adolescent mothers after childbirth.

The second paper ("The impact of the legalisation of abortion on birth outcomes in Uruguay", ChapterIII) investigates the short-term impact on the quantity and quality of births of an abortion reform in Uruguay that legalised termination of pregnancy until the 12$^{\text{th}}$ week of pregnancy in the short run. I employ a differences-in-differences approach, comprehensive administrative records of births, and a novel identification strategy based on the planned or unplanned nature of pregnancies that came to term. My results suggest that this policy change has led to an 8% decline in the number of births from unplanned pregnancies, driven by the group of mothers aged between 20 and 34 years old who have secondary education. This decline has triggered an increase in the average quality of births in terms of more intensive prenatal control care and a lower probability of births among single mothers. Furthermore, I document a positive selection process of births because of the reform, as adequate prenatal control care and Apgar scores rose among the affected demographic group.

The third study ("Subdermal contraceptive implants and repeat teenage motherhood: Evidence from a major maternity hospital–based programme in Uruguay", Chapter IV) evaluates the impact of a programme offering a subdermal contraceptive implants and family planning counselling after an obstetric event on repeated adolescent motherhood. Teenage fertility is a social problem because of its private and public costs in countries of different development levels. Reductions in adolescent birth rates do not necessarily follow drops in overall fertility due to the demographic transition model. This paper analyses the impact of a subdermal contraceptive programme on repeat teenage motherhood. Using a regression discontinuity design, I find that the intervention reduced mothers' likelihood of having another child in the next 48 months by 10 percentage points. This reduction is not random, and I also identify small positive selection in subsequent births.

The last research work ("Immigrant assimilation in health care utilisation in Spain", Chapter V) studies the dynamics of the use of health services by Spanish immigrant population. Foreign-born population represented less than 1% of total inhabitants in Spain by mid-1990s. Nowadays, more than 12% of Spanish residents are migrants. Previous studies on this topic adopt an static perspective just focused on the recipiency of social benefits at a certain point of time. Nevertheless, grasping both the benefits and the costs of migration requires a perspective that explores how the relationship of foreign-born population and the welfare state evolves over time. Abundant evidence has tracked the labour market and health assimilation of immigrants, including static analyses of differences in how foreign-born and native-born residents consume health care services. However, we know much less about how migrants' patterns of health care usage evolve with time of residence, especially in countries providing universal or quasi-universal coverage. I investigate this process in Spain by combining all the available waves of the local health survey, which allows separately identifying period, cohort, and assimilation effects. I find that the evidence of health assimilation is limited and solely applies to migrant females' visits to general practitioners. Nevertheless, the differential effects of ageing on health care use between foreign-born and native-born populations contributes to the convergence of utilisation patterns in most health services after 20 years in Spain. Substantial heterogeneity over time and by region of origin both suggest that studies modelling future welfare state finances would benefit from a more thorough assessment of migration.

Chapter II

The short and long-term determinants of fertility in Uruguay†

II.1. Introduction

Uruguay's fertility behaviour has idiosyncratic features. The country was one of the pioneers of the demographic transition in Latin America and the Caribbean, with very early declines in both fertility and mortality. Its fertility rate was 2.7 children per woman in 1950, a figure that the continent did not reach until the end of the 20$^{\text{th}}$ century. Nevertheless, adolescent fertility—with its well-known negative public health and socioeconomic consequences—remained high until recently.[1] It peaked in 1997 at 74 births per thousand women, stabilised at approximately 60 births per thousand women in the following years and experienced a marked decline from 2014 to 2021, when it reached 26 births per thousand women (United Nations [United Nations], 2022).

The literature has attempted to explain the determinants of fertility at different stages of reproductive life using different conceptual frameworks, methodological

†This chapter is a joint work with and Patricia Triunfo and José-Ignacio Antón. A previous version of this work benefited from comments of Nicolás Bonino, Fernando Borráz, Elizabeth Bucacos and Wanda Cabella.

[1]Adolescent fertility is particularly relevant because of its impact throughout the life of teenage mothers: it is due to low educational attainment, poor labour market outcomes and poverty (Engelhardt et al., 2004; Fletcher & Wolfe, 2009; Hoffman & Maynard, 2008; López Gómez et al., 2016; Paranjothy et al., 2009; Varela Petito, 2004; Varela Petito et al., 2014b, 2014a). Teenage pregnancies are often unplanned (Antón et al., 2018; Buckles et al., 2019), receive less prenatal care and have worse birth outcomes on average (Joyce & Grossman, 1990; Kost & Lindberg, 2015; Kost et al., 2018; Moreira Wichmann, 2019).

tools and types of data. The results are ambiguous, possibly influenced by the different approaches mentioned above, which underlines the importance of providing new empirical evidence that sheds light on this phenomenon. Only a proper understanding of the factors driving fertility can allow policy makers to implement policies that either encourage or discourage fertility, depending on the context. While the adolescent birth rate was high until recently, the total fertility rate reached its minimum in 2020 at 1.48 births per woman, well below replacement level (World Bank, 2023). As a result, Uruguay is experiencing a rapid ageing process and, given its relatively high level of social protection for its per capita income, is inherently facing relevant fiscal pressures in the coming decades (Rofman et al., 2016).

The aim of this paper is to investigate the determinants of fertility among women aged 15–19 (teenage fertility), 20–29 (intermediate fertility) and 30 and over (late fertility). We use both time series analysis methods, based on data from 1968 to 2021, and panel data techniques, based on department-level statistical information from 1984 to 2019. The results of our time series econometric exercise show the existence of a cointegration relationship between fertility and economic performance (GDP per capita and female employment), education and infant mortality. Our findings indicate that income levels have a negative impact on fertility rates at various stages of the reproductive cycle. Education, on the other hand, appears to be an effective means of reducing teenage fertility rates. In terms of employment, our study highlights its negative correlation with fertility rates among women aged 30 and over, underscoring the significance of the opportunity cost of motherhood. We offer some explanations for these findings from the perspective of work–life balance problems and social policies to address them.

Overall, our research emphasizes the importance of considering multiple factors in exploring fertility rates and highlights the need for effective policies that support family planning and work–life balance.

The literature includes examples of in-depth studies covering several decades using the types of techniques employed here and specifically focused on the United States (Kearney & Levine, 2015b), Japan (Kato, 2021; Suzuki, 2019) and Italy (Cazzola et al., 2016). Nevertheless, to the best our knowledge, there are no studies specifically on either Uruguay or any other Latin American or Caribbean country. In addition, this work aims to contribute to the existing literature on the subject of fertility behaviour by providing additional evidence that cumulatively helps to increase our knowledge of its determinants.

After this introduction, the rest of the chapter unfolds as follows. The second section summarises the theoretical framework for studying fertility dynamics and explains the contributions of our work to the existing literature. The third and fourth sections cover the country and regional analyses, respectively, including a description of the methodology and data and presenting our results. Last, we discuss the main conclusions and implications of our study.

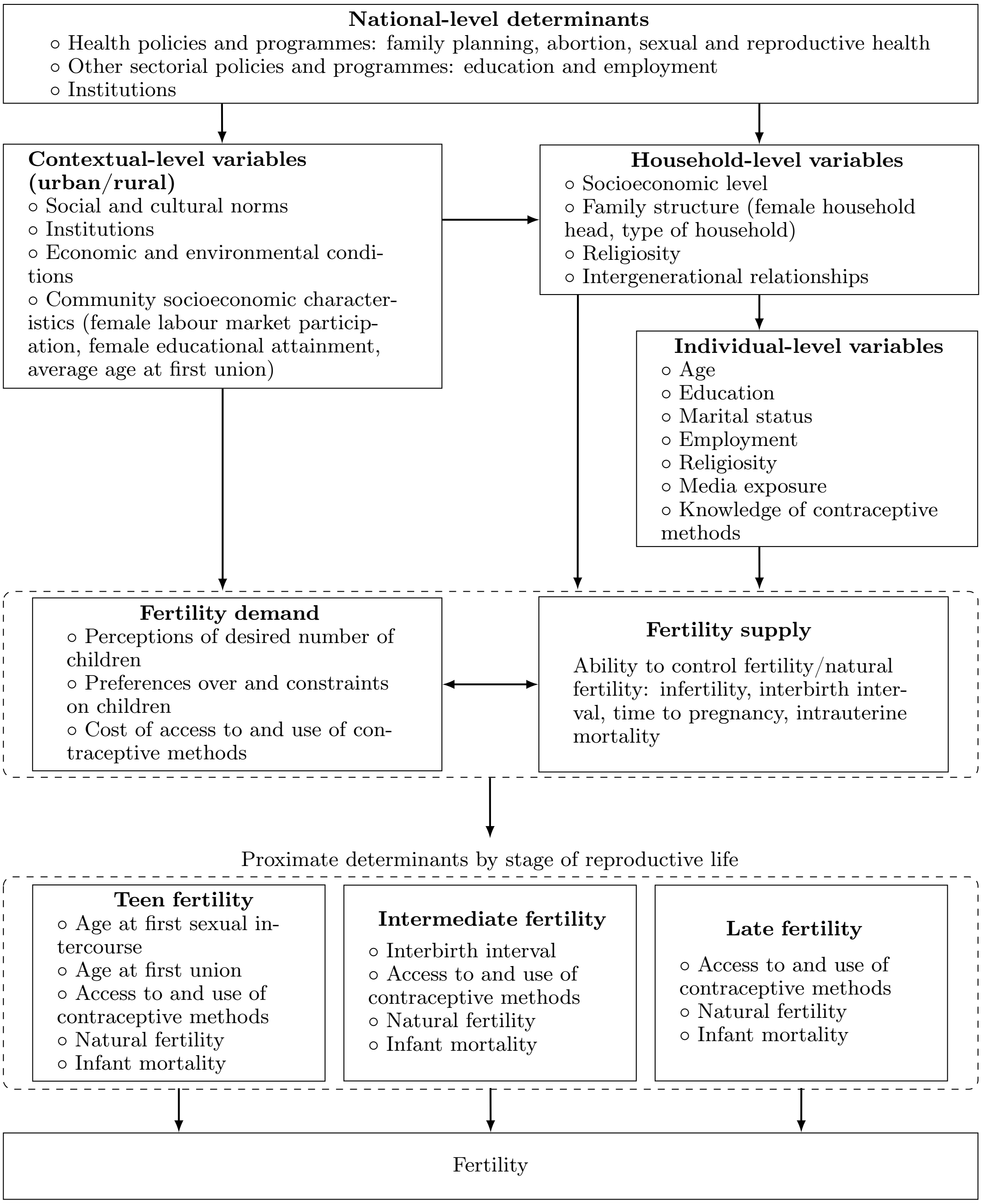
II.2. Theoretical framework and literature review

Studies of the determinants of fertility over reproductive life make use of different conceptual frameworks and operate at at least four different levels of analysis (national, community, household and individual). We summarise and systematise these approaches in Figure II.1, drawing on the seminal contribution of Davis and Blake (1956) and the later adaptations by Bongaarts (1978) and Ojakaa (2022).

The model of Davis and Blake (1956) proposes a set of intermediate determinants of fertility, inspiring the simplified approach of Bongaarts (1978) that focuses on so-called proximate determinants of fertility. These factors differ according to the stage of reproductive life and comprise three categories: exposure (nuptiality and age of sexual initiation), deliberate control of fertility (access to and use of birth-control methods and abortion) and natural fertility. The latter factor refers to the absence of contraception and depends on women's exposure and reproductive conditions (such as sexual abstinence, age at first sexual intercourse, coital frequency, miscarriages, infertility and breastfeeding). In turn, these causes and dynamics of fertility imply several empirically testable hypotheses.

First, the demographic transition theory (Farooq & Simmons, 1985; United Nations [UN], 1973) describes the shift from a population with high fertility and mortality to one with low fertility and mortality as a result of economic development. Originally used to explain the demographic change in Great Britain during the Industrial Revolution, it postulates that fertility decline is a consequence of a country's modernisation, economic growth and development, with the reduction in infant mortality and rural-to-urban migration being the main drivers.

Figure II.1. Determinants of fertility



Source: Author's elaboration from Bongaarts (1978), Davis and Blake (1956) and Ojakaa (2022).

Next, the so-called conventional structural hypothesis derives not only from the demographic transition theory but also microeconomic models and the threshold hypothesis. The former are the result of applying economic analysis, with rational choice as the main workhorse, to fertility, particularly to explain families' preferences for having children (Becker & Lewis, 1973; Easterlin, 1969; Leibenstein, 1975). This body of literature develops a supply–demand theoretical framework for fertility, where this variable is the result of the supply of children (number of surviving children in the absence of birth control), demand for children (due to preferences about the number of offspring) and cost of regulating births. The latter, the threshold hypothesis (UN, 1963), states that fertility will decline only after socioeconomic and health conditions have reached a certain level.

Third, the ideational or diffusionist hypothesis posits that the evolution of fertility is due to shifts in perceptions of, ideas about and attitudes towards birth control. These changes have their roots in the expansion or diffusion of family planning organizations and mechanisms and the increase in women's educational attainment. However, many authors emphasise that the operation of these forces requires the prior achievement of a certain level of socioeconomic development (Caldwell et al., 1992; Cleland, 2001; Cleland & Wilson, 1987; Fort et al., 2016; Hirschman & Guest, 1990).

Fourth, the relationship between fertility and female labour market participation has also received much attention from researchers. In particular, the so-called maternal role incompatibility hypothesis focuses on disentangling the potential and eventual problems of reconciling preferences over the number of children with working life prospects (Cramer, 1980; Lehrer & Nerlove, 1986; Spitze, 1988). Similarly, the societal response hypothesis argues that the existence of policies aimed at minimising conflicts between motherhood and female labour market participation (e.g., available and affordable childcare, generous parental leave or changes in attitudes towards working mothers) could prevent an increase in female employment from translating into a decline in fertility (Brewster & Rindfuss, 2000; Rindfuss et al., 2003).

The final approach is the institutional perspective, which focuses on the context, in particular the institutions, shaping fertility decisions. This perspective aims to reconcile the macro- and microeconomic perspectives.

Bridging the theoretical insights summarised above and empirical practice is challenging. It requires a search for variables that adequately approximate the different dimensions suggested by the theory. To capture the socioeconomic dimension, the variables most commonly chosen in the literature are GDP per capita or household expenditure (Buckles et al., 2021; Chatterjee & Vogl, 2016; Sobotka et al., 2011); unemployment (Cazzola et al., 2016; Currie & Schwandt, 2014); development indicators such as the Gini index, basic infrastructure and services and health and education expenditure (Bettio & Villa, 1998; Engelhardt & Prskawetz, 2004; Engelhardt et al., 2004); female educational attainment (either enrolment rates or average years of schooling by cohort) (Ainsworth et al., 1996; Sackey, 2005; Schultz, 1973; Vavrus & Larsen, 2003) and labour market indicators (women's labour market participation, female wages and the gender pay gap) (Kato, 2021).

The main proxies for the cultural dimension are age of sexual initiation, availability and use of contraceptive methods and household time allocation. The most widely used variables to approximate demographic aspects are population structure and mortality (total or infant). Finally, to operationalize institutional aspects and public policies, which cut across all the other dimensions, a popular strategy is to specify main milestones in the implementation of or drastic changes in public interventions, among others, related to family planning or education (Carr & Packham, 2017; Kearney & Levine, 2015a; Paton et al., 2020).

The previous empirical literature to which this study refers includes both time series and panel data analyses. The former type of research tends to highlight the role of macro-level determinants of fertility. Specifically, these studies emphasise the relevance of female labour force participation, unemployment (both male and female), infant mortality and women's education, among other factors. Nevertheless, this literature is inconclusive with respect to these determinants. A consensus is lacking regarding whether the relationships are causal (even in Granger's sense) or whether they could even be bidirectional (Audi & Ali, 2021; Chatterjee & Vogl, 2016; Kato, 2021; Sobotka et al., 2011).

The evidence on the effect of GDP per capita is more complex. Fertility appears to be procyclical, but it also tends to fall in the long term with economic growth and in the short run with recessions (Audi & Ali, 2021; Chatterjee & Vogl, 2016; Sobotka et al., 2011). The effect also differs across reproductive life stages, with the fertility of women aged 30 and over being the most sensitive to economic fluctuations. The related literature even discusses whether fertility actually declines after economic crises or, in contrast, whether such a development actually precedes the recorded output losses since it is extremely dependent on short-term expectations, as suggested by Buckles et al. (2021) for the United States. For this reason, i.e., the anticipatory behaviour of fertility, these authors are quite critical of the use of unemployment and other business cycle indicators as explanatory factors for fertility. By contrast, other studies such as Currie and Schwandt (2014), which link fertility and unemployment by cohort, suggest an important role for labour market prospects. In particular, they find that women aged 20–24 are the most affected group and that the negative impact increases over time due to the effect on childless women.

Previous works also make use of other measures of economic performance, such as women's wage levels, female labour market participation and the gender pay gap. For instance, Kato (2021), using data from 1980 to 2019 for Japan, with its low fertility rate and labour shortage (circumstances very far from the Uruguayan reality), suggests that women's average earnings have a negative impact on childbirth. Their results highlight the importance of the opportunity cost of having children and the need to design policies that improve work–life balance.

The inclusion of female education makes it possible to test the validity of the diffusion hypothesis. By way of example, the work of Audi and Ali (2021), employing time series from 1971 to 2014 for Tunisia, finds a negative impact of women's schooling level on fertility.

The United States followed a very similar path to Uruguay's. Although the decline was not monotonic, its total fertility rate more than halved between 1900 and 2017, and teenage fertility did not decline until recently. Buckles et al. (2019) examine the heterogeneity in fertility trends across different demographic groups. They find that the reduction in fertility in recent decades followed the reproductive behaviour of young and single women whereas married women and those above 30 saw an increase in their fertility. These authors' results also confirm the positive correlation between declines in fertility and in the proportion of unplanned pregnancies.

The literature using panel data exploits differences either between regions within the same national boundaries or among countries to shed light on the main determinants of fertility. Regarding the former, we can highlight the study by Kearney and Levine (2015b) for the United States, whose main finding is the role played by the expansion in access to family planning services, which explains 13% of the drop in teenage fertility between 1991 and 2010. The work of Cazzola et al. (2016) for Italy emphasises the importance of unemployment, especially in the case of male rates. The Japanese case has also received some attention. Suzuki (2019) finds that female wages have a nonnegligible negative impact on fertility whereas, according to the analysis of Kato (2021), differences in birth rates are due to childcare availability and female labour force participation.

Regarding cross-country literature, most of the existing works centre on developed countries. For instance, Sobotka et al. (2011) confirm the negative impact of economic crises on fertility rates. In terms of policies, D'Addio and d'Ercole (2006) show that social transfers that reduce the direct cost of children and provisions that allow mothers to better balance work and family have a significant impact on birth rates. The analysis of Kato (2021) comes to similar conclusions with regard to the economic environment but, surprisingly, opposite ones for family policies. The author also stresses the importance of female labour market conditions. Interestingly, Paton et al. (2020) find no effect of the expansion of sexual education on fertility. Among the studies focusing on developing regions, we can mention the work of Ojakaa (2022) for sub-Saharan Africa, which emphasizes the role of age at first marriage and contraceptive prevalence. For Latin America, Palloni and Rafalimanana (1999) analyse the relationship between infant mortality and fertility rates between 1920 and 1990 and find small positive effects of infant mortality on fertility.

The contribution of this work to the existing literature is twofold. First, it is the first study to focus specifically on Uruguay. This country has idiosyncratic characteristics: it shares many features with Latin America and the Caribbean (such as relatively high levels of inequality and labour market informality), but its level of social development is historically high, it reached the high-income country category less than a decade ago, and it was one of the first states in the hemisphere to complete its demographic transition (while teenage fertility remained high until very recently). We are not aware of any other research work exclusively devoted to a Latin American or Caribbean country. Second, by using the most recent and comprehensive data at the national and departmental level and state-of-the-art econometric techniques, we expand the empirical evidence on the main drivers of fertility at different stages of reproductive life. Thus, our study also aims to contribute to a better understanding of the overall dynamics of birth rates.

II.3. Time series country-level analysis

II.3.1. Data and methods

In this section, we carry out a descriptive analysis of the evolution of fertility at different stages of reproductive life (adolescents, women aged 20 to 29 and women aged 30 and over) based on historical time series. To this end, we collect information on the following covariates: GDP per capita, female employment rate (share of employed women in relation to female working-age population), female secondary gross enrolment rate (number of women in secondary education as a percentage of women aged 12–17) and infant mortality rate (number of deaths of children under one year of age, expressed per 1,000 live births). We use GDP per capita as a proxy for socioeconomic development. Our consideration of the female employment rate and secondary school enrolment allows us to test the validity of the diffusion hypothesis. These variables capture shifts in women's preferences over and attitudes towards fertility. In particular, educational attainment might improve women's access to information on contraceptive methods and increase their intrahousehold bargaining power. Infant mortality rate plays a key role in the demographic transition hypothesis and in other modern population theories. The relationship between fertility and infant mortality can be difficult to unravel because of various underlying mechanisms. These channels can range from purely physiological effects, where the death of an infant triggers resumption of mothers' menstruation and ovulation, thus increasing their likelihood of a new conception, to replacement or insurance mechanisms (whereby families aim for a specific number of surviving children beyond the desired family size), distortions in the market for potential partners and competition between children for maternal care and household resources (Palloni & Rafalimanana, 1999; Wolpin, 1998). Additionally, infant mortality can serve as an indicator of the quality of a country's health systems and level of access to medical care.

The availability of information on fertility and covariates over time varies considerably. Overall, in our econometric exercise, we are able to analyse teenage fertility over the period 1968–2021 and the rates of the other two age groups from 1978 to 2021. Reconstructing the time series of these variables is not trivial, requiring substantial effort and the combination of different sources. First, the adolescent fertility rate comes from statistical information from the Uruguayan National Institute of Statistics (Instituto Nacional de Estadística [INE], 2023e) and the World Development Indicators (WDI) (World Bank, 2023), whereas we obtain the other two fertility rates by combining vital statistics from the Ministry of Public Health and population projections from the National Statistics Institute (INE, 2023c; Ministerio de Salud Pública [MSP], 2023). We retrieve historical data on GDP per capita (in constant 1990 US$) from the Montevideo–Oxford Latin American Economic History Data Base (Economic and Social History Programme of the University of the Republic & Latin American Centre and the Department of International Development of the University of Oxford, 2023), and the female high school enrolment rate comes from the WDI (World Bank, 2023). We reconstruct our series on the female employment rate using information from Centro Latinoamericano de Economía Humana (1990) and INE (2023a).[2] Finally, we obtain the infant mortality rate for the period of interest from INE (2023d). Table II.1 shows the descriptive statistics of the variables included in our econometric exercise.

[2]The Uruguayan national household survey is not nationally representative until 1995. Nevertheless, since its inception, it has covered all municipalities with 5,000 inhabitants. Therefore, to construct homogeneous series, we restrict all the statistical information based on this database to the mentioned localities.

Table II.1. Summary statistics of time series data

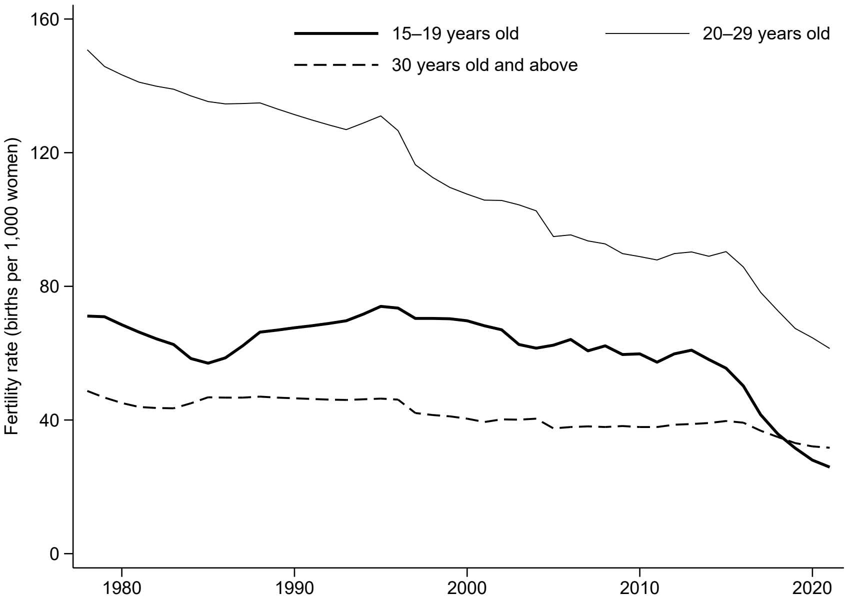
	Mean	Standard deviation	Minimum	Maximum
Fertility rate 15–19	61.9	10.8	25.9	74.0
Fertility rate 20–29	110.7	24.8	61.4	150.8
Fertility rate 30–49	41.6	4.5	31.7	48.7
GDP per capita	7,982.4	2,711.2	4,747.2	13,267.1
Female employment rate	89.7	22.0	61.7	131.3
Female high school enrolment	38.8	9.1	23.1	52.4
Infant mortality rate	23.5	15.7	6.2	61.9

Note: The number of observations is 54 (1968–2021) in all cases except for the 20–29 and 30+ fertility rates, where it is 44 (1978–2021).

Figure II.2 shows the evolution of the fertility rate by age group from 1978 to 2021 (the time window for which we have information on all of the groups). Different patterns emerge. First, the adolescent birth rate remained relatively high until 2015, when it experienced a rather abrupt fall. Second, the fertility rate of women between 20 and 29 years old underwent a sustained declined throughout the whole analysed period that accelerated in 2016. Last, the fertility of women aged 30 and above decreased at a much slower pace over the more than four decades considered in the analysis.

Naturally, the period covered by our analysis witnessed the enactment of several potentially relevant education- and health-related laws. Unfortunately, the degrees of freedom of our research design and the fact that some of the developments were contemporaneous prevent us from disentangling the causal effect of these policies. Below, we discuss the inclusion of a linear time trend and dummies for certain subperiods of time.

Figure II.2. Evolution of age-specific fertility rates in Uruguay (births per 1,000 women, 1976–2021)



Source: Author's analysis from INE (2023e) and World Bank (2023).

In principle, we aim to estimate the effect of the covariates of interest through the following linear model:

$$y_t = \mu + \delta' X_t + \xi_t \tag{II.1}$$

where y_t is the fertility rate (in natural logs), X_t represents a vector comprising the covariates mentioned above (in natural logs), and ϵ denotes the random disturbance term.

To proceed with the time series analysis, first of all, we must check whether the variables included in the analysis are stationary using the augmented Dickey–Fuller (ADF) (Dickey & Fuller, 1981) and Phillips–Perron (PP) (Phillips & Perron, 1988) unit-root tests. The latter is robust to heteroscedasticity and serial correlation and does not require specification of the form of the lag structure.

Second, having established the stationarity of the series, we examine the existence of cointegration between fertility and the variables described above using the tests proposed by Engle and Granger (1987), Johansen (1995), Pesaran and Shin (1999) and Pesaran et al. (2001).

Previous tests have been shown to be highly sensitive to the chosen model specifications. In third place, to address this issue, we decide to employ the autoregressive distributed lag (ARDL) model in our investigation of cointegration (Pesaran & Shin, 1999; Pesaran et al., 2001):

$$y_t = \alpha_0 + \alpha_1 t + \sum_{i=1}^{p} \phi_i y_{t-1} + \sum_{i=0}^{q} \beta_i' X_{t-i} + \epsilon_t \tag{II.2}$$

where t is a linear time trend, ϵ denotes the random disturbance term and p and q are the number of lags of the dependent and independent variables, respectively. In practice, we allow for a different structure of each variable (so that q can vary for each of the four covariates). We use the Bayesian information criterion (BIC) to determine the optimal number of lags (which may be different for each variable). The advantage of this model over other approaches is that it allows for mixed orders of cointegration and performs better with small samples.

Fourth, in the case of evidence of cointegration, we can rewrite equation II.2 as

$$\begin{aligned}\Delta y_t = \alpha_0 + \alpha_1 t - \gamma \left(y_{t-1} - \theta' X_{t-1}\right) + \sum_{i=1}^{p-1} \psi_{y_i} \Delta y_{t-1} + \omega' \Delta X_t \\ + \sum_{i=1}^{q-1} \psi_{X_i}' \Delta X_{t-i} + u_t\end{aligned} \tag{II.3}$$

where θ denotes the long-run coefficients (the equilibrium effects of the covariates on fertility); γ represents the error correction term (ECT)—the (negative) speed-of-adjustment coefficient, which measures how fast the dependent variable responds to deviations from the equilibrium relationship; and ψ_{y_i}, ω and ψ_{X_i} capture short-term fluctuations (unrelated to the long-term equilibrium).

Finally, we carry out goodness-of-fit tests on the ARDL model, including tests for first-order autocorrelation (Breusch–Godfrey [Breusch, 1978; Godfrey, 1978] and Durbin–Watson [Durbin & Watson, 1950, 1951, 1971]), heteroscedasticity (White [1980], Breusch–Pagan [Breusch & Pagan, 1979] and Cook–Weisberg [Cook & Weisberg, 1983]), and normality (D'agostino et al., 1990). In addition, we examine Granger (1969) causality and compute impulse response functions and the error variance decomposition.

II.3.2. Results

According to the unit-root tests described above, whose results we present in the appendix (Table II.A.1), the variables included in our model are first-difference stationary—$I(1)$. We allow for a maximum of two lags because of the limited statistical power due to our sample size and number of variables. The results of the tests for cointegration (Tables II.A.2–II.A.4) indicate the existence of at least one cointegration relationship.

On the other hand, as mentioned earlier, the ARDL model for each series indicates the optimal lag structure to be as follows: (2,0,0,0,2) for teen fertility, (2,1,0,0,1) for intermediate fertility and (1,2,0,0,1) for late fertility.

In Table II.A.5, we present the results of the goodness-of-fit tests. They indicate that we cannot reject the null hypotheses of absence of serial correlation, homoscedasticity, and normality.

To check the stability of the parameters in our econometric models, we perform cumulative sum of squares tests for structural change (Brown et al., 1975). The plot shown in Figure II.A.1 indicates that the cumulative sum of the squared recursive residuals is approximately within the 95% confidence interval for the target value based on the null hypothesis of the parameter at each point all the time for the three fertility rates.

Since we observe relevant changes in the late fertility rate in 1996 and 2004 and an abrupt fall in all three rates from 2016, we include three dummy variables to account for these changes and ensure the stationarity of the time series.

Table II.2 presents the main results of our analysis. They show the existence of a long-term relationship between fertility and the covariates, with a statistically significant negative speed of adjustment. The mentioned error correction term indicates that the fertility rate adjusts to temporary deviations at a rate of between 17.5% and 25.4% per year, depending on the age group. Regarding the estimated long-run coefficients, which we interpret as elasticities, first, GDP per capita has a statistically significant effect, with a negative impact, on fertility only among women aged 30 and over. Second, the impact of the female employment rate is statistically different from zero and positive in all cases. A 1% increase in the share of employed women raises fertility by between 0.229% (women aged 20–29) and 0.475% (women aged 30 and over). Third, female high school enrolment exerts a statistically significant and positive effect on adolescent fertility and a negative one on that of women aged 30 and over. Fourth, infant mortality rate is relevant only for the latter group of women, with a positive statistically significant impact.

The existence of discrepancies between the short- and long-run coefficients simply indicates the complexity of the dynamic interactions between fertility and the covariates. Regarding adolescent fertility, we can interpret the short-term relationships as indicative of the necessary initial conditions for curbing this age-specific rate. Namely, a decline in teenage fertility requires an increase in women's educational attainment. Specifically, a 1% rise in female secondary school enrolment reduces the adolescent fertility rate by 0.379% with a two-year lag. However, this variable has a positive statistically significant effect for the other two age groups (0.143 for women aged 20–29 and 0.146 for women aged 30 and over). Nevertheless, this finding is consistent with the evidence reported by Fort et al. (2016) for England and continental Europe. For the former, these authors find support for a negative relationship between education and total fertility. This effect does not hold for mainland Europe. These authors suggest that this discrepancy might be due to the higher adolescent birth rate in England, where the increase in educational attainment associated with the expansion of compulsory schooling exerted an almost mechanical negative effect on fertility.

Finally, as mentioned above, the relationship between GDP per capita and fertility is far from simple, and in the short term, it may be the opposite of the inverse relationship that one expects in the long run. For example, economic or social crises may temporarily increase fertility due to uncertainty and the need for family support or in response to policies that encourage childbearing. In the long term, however, one anticipates an inverse relationship, associated with better access to education, increased employment opportunities and improved health care services. The effect also differs according to the stage of reproductive life, with the late stage being the most sensitive to economic fluctuations, as observed in Table II.2. For women aged 30 and over, we find a significant and negative relationship at time t (-0.171) and a positive one at $t-1$ (0.153).

The coefficient of the temporal dummy variable 2016–2021 accounts for a decrease of 0.110% in the adolescent fertility rate and of 0.049% in the intermediate fertility rate during the period of interest. We believe that this variable may capture changes that occurred in those years or in previous years. Uruguay launched several public policies that could potentially affect fertility, especially adolescent fertility, such as the following: the Sexual and Reproductive Health Law (2008) (Ley N.º 18.246. Ley sobre salud sexual y reproductiva, 2008), an expansion of the range of available contraceptives (including subdermal implants) from 2015 onward, and the creation of a network of sexual health service providers and the expansion of reproductive health services, including spaces for adolescents and the creation of a strategy for the prevention of unwanted adolescent pregnancies. The dummy variable is not significant for the late fertility rate, which may simply reflect that the aforementioned policies mainly targeted other groups. Regarding other temporal variables, a negative and significant trend in the adolescent fertility rate (-0.010) and an increase in subsequent fertility rates in 2004 (0.064) stand out. The latter could have to do with the economic growth after the 2002 crisis. For instance, Uruguay's GDP grew by 11.1% in 2004 (INE, 2023g).

According to Granger's (1969) causality criterion, we detect a bidirectional relationship between the adolescent fertility rate and the secondary school enrolment rate. Table III.3 shows that, in the past, the education variable was able to predict, in the Granger sense, the current rate of adolescent fertility and vice versa. This underlines the relevance of education as a policy measure to influence the adolescent fertility rate but not fertility across reproductive life stages.

Finally, to understand the relative importance of each factor in explaining the variability of the time series, we estimate impulse response functions (IRFs) of both the fertility rate and its determinants. IRFs describe the dynamic response of a system to a shock or impulse. It shows how the fertility reacts over time to a sudden change in one of its inputs. This information can be used to understand the transmission of shocks or policy interventions. Figure II.A.2 in the appendix shows that an increase in high school enrolment leads to a decrease in fertility in the short run but that the effect is relatively small and diminishes over time. However, the largest response corresponds to changes in fertility itself. These can be associated, for example, with shocks to fertility preferences or to fertility behaviour.[3]

[3] In principle, in orthogonal IRFs, the results depend on the order in which one includes the variables in the model. In practice, however, they are similar in all cases in our analyses, irrespective of the order.

Table II.2. Estimation results of the ARDL model

	(I)	(II)	(III)
	Fertility rate (in logs) of women aged		
	15–19	20–29	30 and above
Error correction term	-0.175^{***}	-0.254^{**}	-0.194^{**}
	(0.039)	(0.083)	(0.942)
Long-run relationships			
$\log(\text{GDP per capita})_t$	0.060	-0.022	-0.165^{***}
	(0.045)	(0.051)	(0.047)
$\log(\text{Female employment rate})_t$	0.302^{***}	0.229^{*}	0.475^{***}
	(0.076)	(0.099)	(0.108)
$\log(\text{Female high school enrolment})_t$	0.295^{***}	0.140	-0.171^{***}
	(0.068)	(0.068)	(0.055)
$\log(\text{Infant mortality rate})_t$	0.054	0.054	0.080^{*}
	(0.050)	(0.050)	(0.047)
Short-run relationships			
$\Delta(\log(\text{Age-specific fertility rate}))_{t-1}$	0.454^{***}	0.320^{*}	
	(0.112)	(0.165)	
$\Delta(\log(\text{GDP per capita}))_t$		-0.127^{*}	-0.171^{**}
		(0.069)	(0.073)
$\Delta(\log(\text{GDP per capita}))_{t-1}$			0.153^{*}
			(0.079)
$\Delta(\log(\text{Female high school enrolment}))_t$	0.050	0.143^{*}	0.146^{*}
	(0.091)	(0.078)	(0.082)
$\Delta(\log(\text{Female high school enrolment}))_{t-1}$	-0.379^{***}		
	(0.091)		
Year 1996			0.021
			(0.019)
Year 2004			0.064^{***}
			(0.021)
Years 2016–2021	-0.110^{***}	-0.049^{***}	0.007
	(0.025)	(0.014)	(0.014)
Linear time trend	-0.009^{***}	-0.004	0.002
	(0.003)	(0.004)	(0.003)
No. of observations	52	42	43
Mean of dependent variable	61.93	119.68	41.56
ARDL model structure	(2,0,0,0,2)	(2,1,0,0,1)	(1,2,0,0,1)

Notes: Standard errors in parentheses. All the models include an intercept. The structures of the three models in the table are ARDL(2,0,0,0,2), ARDL(2,1,0,0,1) and ARDL(1,2,0,0,1), respectively. *** significant at 1%; ** significant at 5%; * significant at 10%.

As an alternative measure to the IRFs, we present the forecast error variance decomposition with different time horizons for each model in Tables II.A.6–II.A.8.

The IRFs provide information on the dynamic response of the system but do not reveal the sources of variability in the time series. The main salient finding is that more than 75% of the variation in fertility in the long run is due to its own shocks rather than to its determinants. Anyway, it is worth highlighting the contribution of education by age group. Secondary school enrolment accounts for 10.8% of the variation in adolescent fertility in the first period, and its effect vanishes over time. In contrast, for women aged 20–29 and those aged 30 and over, the initial contribution is very low but grows over time (more than 8% after eight time periods).

Table II.3. Results of the Granger causality test

Equation		Women aged 15–19 χ^2	15–19 p-value	20–29 χ^2	20–29 p-value	30 and above χ^2	30 and above p-value
Δ log (Age-specific fertility rate)	Δ log (GDP per capita)	15.615	0.458	33.945	0.183	42.661	0.118
	Δ log (Female employment rate)	17.935	0.408	21.669	0.338	38.644	0.145
	Δ log (Female high school enrolment)	19.616	0.000	40.911	0.129	37.947	0.150
	Δ log (Infant mortality rate)	21.393	0.343	0.431	0.806	0.525	0.769
	All variables	36.464	0.000	14.113	0.079	11.990	0.152
Δ log (GDP per capita)	Δ log (Age-specific fertility rate)	0.547	0.761	0.464	0.793	2.256	0.324
	Δ log (Female employment rate)	0.172	0.918	0.126	0.939	0.406	0.816
	Δ log (Female high school enrolment)	0.686	0.710	0.652	0.722	0.699	0.705
	Δ log (Infant mortality rate)	16.503	0.438	14.417	0.486	14.225	0.491
	All variables	37.333	0.880	32.151	0.920	5.123	0.744
Δ log (Female employment rate)	Δ log (Age-specific fertility rate)	25.952	0.273	21.591	0.340	13.927	0.498
	Δ log (GDP per capita)	12.059	0.002	46.594	0.097	45.657	0.102
	Δ log (Female high school enrolment)	48.206	0.090	55.275	0.063	52.908	0.071
	Δ log (Infant mortality rate)	43.433	0.114	36.131	0.164	45.316	0.104
	All variables	19.084	0.014	12.498	0.130	11.552	0.172
Δ log (Female high school enrolment)	Δ log (Age-specific fertility rate)	74.073	0.025	21.333	0.344	22.475	0.325
	Δ log (GDP per capita)	16.055	0.448	0.213	0.899	0.185	0.912
	Δ log (Female employment rate)	18.124	0.404	2.603	0.272	23.554	0.308
	Δ log (Infant mortality rate)	0.765	0.682	26.545	0.265	24.514	0.294
	All variables	16.228	0.039	93.533	0.313	94.861	0.303
Δ log (Infant mortality rate)	Δ log (Age-specific fertility rate)	18.037	0.406	72.559	0.027	31.317	0.209
	Δ log (GDP per capita)	35.062	0.173	16.784	0.432	0.592	0.744
	Δ log (Female employment rate)	0.494	0.781	0.969	0.616	1.575	0.455
	Δ log (Female high school enrolment)	0.547	0.761	1.200	0.549	0.337	0.845
	All variables	92.138	0.325	16.008	0.042	11.151	0.193

Notes: The degrees of freedom refer to the number of constraints in the model (two in the case of the rows due to individual variables and eight in the case of the ones due to all variables).

II.4. Panel data department-level analysis

II.4.1. Data and methods

In this section, we make use of panel data for the country's 19 administrative units (departments) and the period 1984–2019 to provide additional insight into the dynamics of fertility in Uruguay. This strategy allows us to increase the statistical power of the analysis and control for time-constant department-level heterogeneity through fixed effects techniques, thereby mitigating endogeneity problems.

It is worth noting the existence of significant territorial disparities within the country. The department of Montevideo (which includes the country capital of the same name) concentrates 40% and 50% of the national population and GDP, respectively (Observatorio Territorio Uruguay [OTU], 2023). From a multidimensional perspective, although the human development index (HDI) experienced sustained progress across the whole country from 2008 to 2018, again, a nonnegligible gap between Montevideo and the rest of Uruguay is observable (OTU, 2023). In 1998, Montevideo was the only department to exhibit very high human development (above 0.800), while the rest of the regions had high HDI values (between 0.700 and 0.800). In 2018, apart from Montevideo, four other departments (Colonia, Maldonado, Flores and Florida) had crossed the threshold of very high human development. Cerro Largo, Rivera, Rocha, Treinta y Tres, Tacuarembó and Artigas were, in descending order, the areas with the lowest HDI values in Uruguay.

To build our database, we rely on a variety of sources, balancing the convenience of long series with the availability of statistical information. Whereas we can include a larger number of variables in this analysis than in our time series econometric exercise, the period covered here is shorter. As in the previous section, we model age-specific fertility rates as a linear function of several covariates. We compute department-level fertility rates from vital statistics (MSP, 2023) and population projections (INE, 2023f). First, the covariates include the demographic structure of the department through the percentage of each age group of total women aged 15–49, calculated from population projections (INE, 2023f). The second variable, derived from the national household survey (INE, 2023b), is the percentage of women in each age group married or in a union. To assess the impact of education, we consider a demand-side indicator, the average years of schooling of women in each age group, derived from (INE, 2023b). Fourth, using the same data source, we consider a set of variables that aim to capture the opportunity cost of having children, such as age-specific female employment rates. In fifth place, we also consider the gender gap, calculated as the ratio of women's average labour income to men's average earnings. Furthermore, our analysis considers average household disposable income per capita in national currency units (NCUs) at December 2010 prices. The infant mortality rate is computed from MSP (2023). Table II.4 shows the summary statistics of the variables included in our analyses.[4]

[4]As in the time series analysis, to rely on homogenous series, we limit our analysis of the Uruguayan household survey to municipalities with 5,000 inhabitants or more.

Table II.4. Summary statistics of regional panel data

	Mean	Standard deviation	Minimum	Maximum	Between-region standard deviation	Within-region standard deviation
Fertility rate 15–19	70.8	17.4	18.5	112.4	11.2	13.6
Fertility rate 20–29	123.0	26.7	55.2	186.9	11.5	24.3
Fertility rate 30+	42.0	8.1	24.8	71.0	3.8	7.2
% of women 15–19	14.3	0.9	12.0	16.5	0.7	0.6
% of women 20–29	25.0	1.4	21.7	29.3	1.0	1.0
% of women 30–49	45.3	2.3	37.9	50.2	1.5	1.7
% of women married 15–19	1.9	0.9	0.0	5.6	0.4	0.8
% of women married 20–29	22.1	3.2	10.2	32.0	1.4	2.9
% of women married 30–49	76.0	3.5	65.8	89.1	1.7	3.1
Average years of schooling of women 15–19	8.9	0.4	7.8	10.2	0.4	0.8
Average years of schooling of women 20–29	9.8	0.8	7.9	12.2	0.4	0.7
Average years of schooling of women 30–49	9.1	1.0	6.9	12.1	0.5	0.9
Employment rate of women 15–19	16.2	6.9	0.0	50.0	2.7	6.4
Employment rate of women 20–29	51.7	8.5	29.3	73.1	5.3	6.8
Employment rate of women 30–49	64.4	9.5	38.1	84.8	3.1	9.0
Gender pay gap	0.9	0.1	0.4	1.5	0.0	0.1
Average disposable income per capita	7,180.0	1,933.7	3,853.4	13,994.7	1,246.9	1,504.7
Infant mortality rate	14.7	7.5	0.0	46.4	1.7	7.3

Note: The number of observations is 665 (19 departments from 1984 to 2019).

Figure II.3 illustrates how the age-specific fertility rate fell across the whole national territory over the analysed period. Nevertheless, it also shows that the timing of this decline varies from one department to another.

The number of departments (19) is well below 50, which prevents us from using clustered standard errors to deal with serial correlation issues (Angrist & Pischke, 2008). We therefore follow the advice of Békés and Kézdi (2021) on dealing with few cross-sectional units in panel data: we employ Newey–West standard errors (Newey & West, 1987) that are robust to heteroscedasticity and autocorrelation up to the order suggested by the literature in our left-hand-side variable. In this respect, previous research emphasises that the error term of econometric models analysing the determinants of annual fertility rates tend to follow an AR(1) process (see, e.g., Brehm and Engelhardt [2015] and Prskawetz et al. [2009]). As a robustness check, we compute the standard errors following the procedure described by Driscoll and Kraay (1998), which additionally allows for cross-sectional dependence between departments.

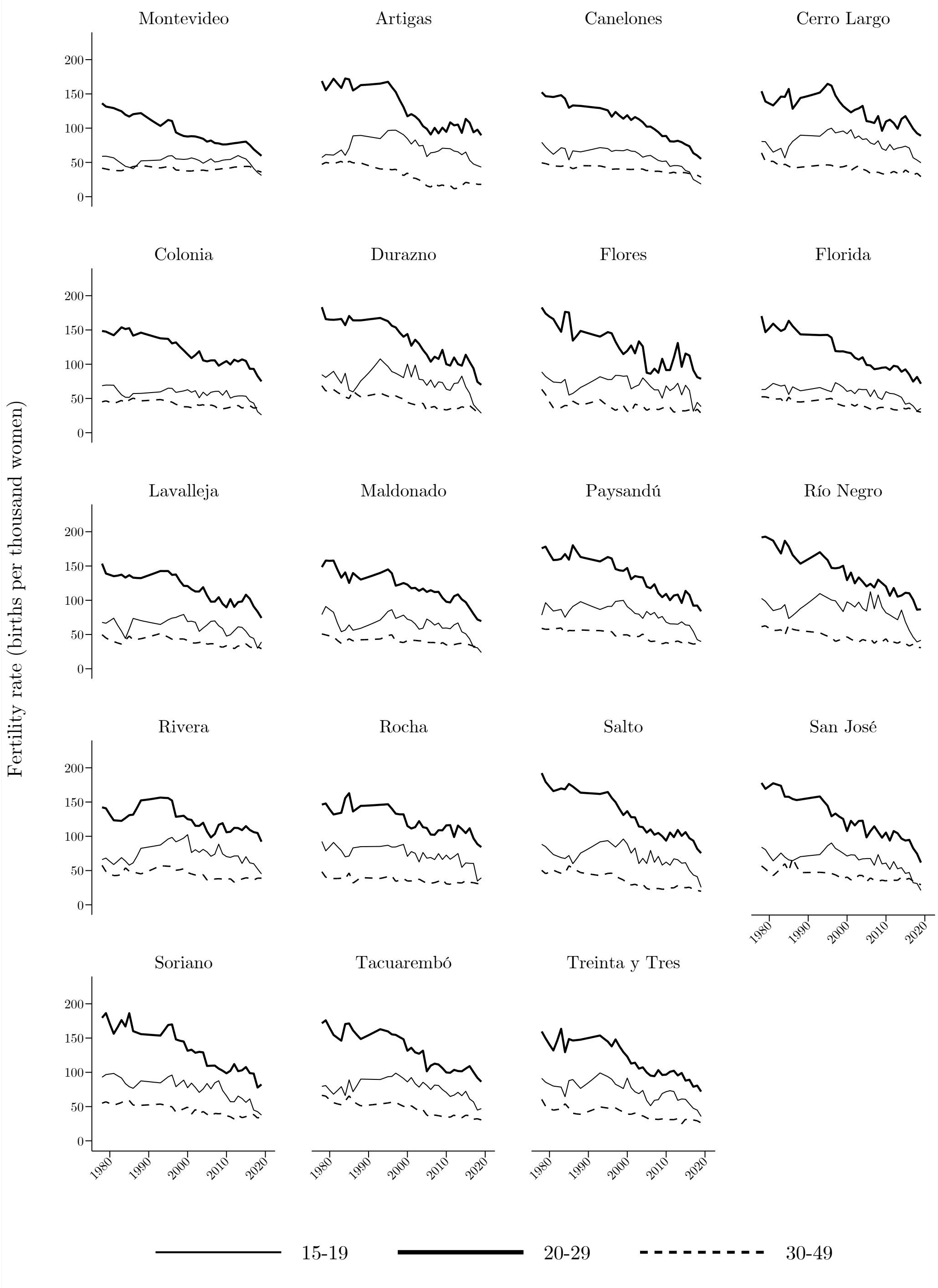
We consider all the variables in levels, without using the log transformation. In this setup, we do not have to worry about stationarity or normality (given the sample size). Furthermore, some departments exhibit zero values for certain variables.

Therefore, we estimate models of the following form:

$$y_{it} = \kappa + \lambda' Z_{it} + \eta_i + \tau_t + \upsilon_{it} \tag{II.4}$$

where κ is an intercept, y_{it} is the age-specific fertility rate in logs of department i in year t, Z_{it} is a vector containing the time-varying covariates of interest, η_i is a department fixed effect, τ_t is a year fixed effect and υ_{it} represents a time-varying disturbance. It is possible to group the 19 departments into six major geographical regions (metropolitan region, centre, east, northeast, littoral south and littoral north). Our specification allows us to include group-specific linear time trends. This provides a useful robustness check: we can assess whether the results simply follow pre-existing regional trajectories over time.

Figure II.3. Evolution of age-specific fertility rates by department in Uruguay (births per 1,000 women, 1976–2021)



Source: Author's analysis from MSP (2023) and World Bank (2023).

II.4.2. Results

Table II.5 shows the results of our fixed effects panel data analysis. The first column of the table presents the estimates of the model that includes both time and department fixed effects. The second one also includes region-specific linear time trends to assess the robustness of the results.

According to the estimation results, the share of women in each age group does not have a statistically significant effect on fertility, except that of women aged 15–19. In this case, a one-percentage-point increase in the share of teenagers among women of fertile age raises adolescent fertility by more than four per thousand points. In the context of a declining proportion of adolescents and fertility rates in this age group, a negative and significant coefficient would suggest that the reduction in adolescent fertility is more significant than what would be expected based on population aging alone. This finding may indicate that additional factors beyond demographic shifts, such as changes in social norms or increased access to contraception, are contributing to the decrease in adolescent fertility.

Age-specific nuptiality exerts a statistically significant positive impact on the fertility of women aged 20–29 and especially those aged 15–19. A one-percentage-point increase in the share of women who are married or in a union implies a rise in fertility of almost one point per thousand points for teenagers and about half a point for women aged 25–29. For women aged 30 years old and over, the model with regional time trends shows a statistically significant negative effect of this segment's share on their fertility. Although this finding is remarkable, this variable may capture couples' preferences regarding parenthood (e.g., delaying it for personal or professional reasons). In addition, individuals who marry at a later age are more likely to use contraceptive methods for family planning or health reasons.

The analysis also suggests that the average number of years of schooling leads to a statistically significant reduction in teenage fertility. In the remaining cases, the impact of this variable either is not statistically different from zero or is sensitive to the inclusion of regional trends.

Regarding the female employment rate, our results are consistent with those presented in the previous section. This variable has a negative significant effect on the fertility only of the oldest group of women. This pattern could have to do with the higher opportunity costs of having children and work–life balance problems for this demographic segment relative to the other, younger ones.

We employ the gender pay gap as an attempt to capture the relationship between women's reproductive and labour market behaviour and decisions. The lack of robustness of these results to the inclusion of regional linear time trends does not allow us to draw any relevant conclusions. Household income appears to have a statistically significant negative impact on the fertility of all segments of women. Several factors may explain this result: higher child-rearing expenses as income rises (e.g., a greater use of private education), larger opportunity costs or even cultural beliefs about parenthood that differ by socioeconomic level (e.g., better-off couples may decide to have a smaller number of children to gain more autonomy in their lives). A 1,000 NCU increase in household income raises the fertility of women aged 15–19 and 30 and over by one point per thousand. The effect is twice as large for women aged 20–29.

Last, infant mortality has a statistically significant effect on the fertility of women aged 20–29. The impact is null for adolescents and sensitive to the inclusion of regional time trends for the oldest segment of females. In societies with high mortality rates, in the early stages of the demographic transition, one would expect a fall in infant mortality to precede the decline in fertility. This is not the case in Uruguay, where we hypothesise that infant mortality may capture department-level differences in quality of life and access to health care. Families in the territories with the best conditions on these dimensions might show an increased willingness to have children because they perceive better future life chances for their offspring.

The results of the model using Driscoll–Kraay standard errors are remarkably similar to those of our main specification (Table II.A.9).

Table II.5. Estimation results of the panel data model for regional fertility rates

	(I)	(II)	(III)	(IV)	(V)	(VI)
	Fertility rate of					
	women aged 15–19		women aged 20–29		women aged 30 and above	
% of women in the age bracket	−4.336***	−4.253***	−0.410	−0.023	−0.012	0.045
	(1.036)	(0.909)	(0.884)	(0.798)	(0.457)	(0.347)
% of women married (age-specific)	0.852*	0.978**	0.445***	0.570***	−0.086	−0.132**
	(0.446)	(0.431)	(0.166)	(0.170)	(0.067)	(0.060)
Average years of schooling	−4.341***	−3.445**	0.716	0.500	4.390***	0.868
	(1.480)	(1.564)	(1.042)	(0.994)	(0.681)	(0.347)
Age-specific female employment rate	0.108	0.077	0.099	0.017	−0.327***	−0.150***
	(0.066)	(0.065)	(0.074)	(0.068)	(0.064)	(0.049)
Gender pay gap	9.676***	4.113	0.839	−3.580	4.356*	1.088
	(3.379)	(2.729)	(4.152)	(3.579)	(2.283)	(1.816)
Average disposable income per capita	−0.001	−0.001**	−0.002***	−0.002***	−0.001***	−0.001***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Infant mortality rate	−0.201*	−0.159	−0.275**	−0.244***	−0.083	−0.112**
	(0.110)	(0.102)	(0.110)	(0.097)	(0.062)	(0.053)
Year fixed effects	✓	✓	✓	✓	✓	✓
Department fixed effects	✓	✓	✓	✓	✓	✓
Region-specific linear time trends		✓		✓		✓
R^2	0.866	0.885	0.943	0.951	0.678	0.799
No. of observations	684	684	684	684	684	684
Mean of dependent variable	61.718	61.718	107.679	107.679	41.283	41.283

Notes: Standard errors robust to heteroscedasticity and first-order autocorrelation in parentheses. All the models include an intercept. *** significant at 1%; ** significant at 5%; * significant at 10%.

II.5. Conclusion

This study has examined a range of factors that influence fertility throughout the reproductive cycle, using time-series data from 1968 to 2021 and panel data from regional statistical sources from 1984 to 2019. We have focused on fertility across three stages: adolescent (15 to 19 years), intermediate fertility (20 to 29 years) and late fertility (30 to 49 years).

The analysis conducted in these pages has allowed testing some of the theoretical hypotheses put forward in the literature. Although the nature of this study is eminently descriptive, the use of multiple methods and remarkably long data series enhances our understanding of fertility behaviour.

Uruguay has atypical characteristics compared to most Latin American and Caribbean countries. It underwent the first demographic transition at an early stage. For decades, it has had quite low fertility rates among women aged 20 and over, but, until recently, experienced a relatively high teenage birth rate, especially among low socio-economic households. Moreover, most births take place outside of marriage, against a background of rising divorce rates. These features have led some observers to suggest that the country is undergoing a second demographic transition.

The work has considered a range of socio-economic indicators—GDP per capita and department-level average household income per capita—to test the relevance of some of the conventional structural or diffusion, maternal role incompatibility and institutional theories. Specifically, the study has used two economic indicators that reflect the social and economic conditions that shape the lives of Uruguayan women. On the one hand, GDP per capita is a better approximation of economic cycles, although the literature suggests the existence of time lags and differences throughout a woman's reproductive cycle. On the other hand, departmental average per capita income better captures the actual appropriation of economic output by households.

Previous literature suggests that the relationship between the level of income or GDP per capita and fertility is complex. In the long term, one expects a negative association, but shock-run shocks might shape this relationship, which may also vary by age group. In the time-series analysis, we have detected those differences between the short and the long run only for the fertility of women aged 30 years old and above. The results of the panel-data econometric exercise (which does not allow distinguishing between the short- and long-term effects) have indicated a negative association between income and fertility at all ages. These estimates may well capture greater availability and access to contraceptive methods, greater educational opportunities and an increase in the opportunity cost of having children.

Another relevant dimension that allows us to understand reproductive decisions is the female employment, because of the potential conflict between professional career and motherhood. The increase in women's employment may have a negative impact on fertility, as females might decide to postpone motherhood or reduce the number of children in order to prioritise their labour market performance. Contrary to expectations, our time-series analysis has shown a negative effect of female employment on fertility. Nevertheless, the econometric exercise using regional data, which allows controlling for unobserved heterogeneity and age-specific female employment rates, has revealed a negative impact. Such a result could reflect the opportunity costs of children, which raises the relevance of designing social protection policies that alleviate work-life balance problems, like making affordable childcare widely available or providing appropriate parental leave.

The relationship between education and its impact on fertility has received extensive attention in the specialised literature. Overall, existing studies suggest a negative association between schooling levels and fertility for a variety of reasons, ranging from a greater access to information, better job opportunities or changes in culture, social norms or preferences for motherhood. Our results have confirmed this relationship, particularly in the case of adolescent fertility. The possibility of curbing teenage motherhood by expanding education has emerged as a clear policy implication from this analysis.

Given Uruguay's stage of the demographic transition, as argued above, one should expect a positive or non-significant relationship between fertility and infant mortality. However, our results are not robust to the method of estimation and the analysed period. This lack of conclusive findings could indicate that such a variable reflects characteristics due to economic conditions and health care not captured by other covariates.

Finally, the sizeable fall in births in recent years—especially, among teenagers—might also have been the consequence of difference national policies implemented since 2008. Such government initiatives include the 2008 health care reform (which moved health care towards an integrated system), a new law on sexual and reproductive health in 2008, the setting of health care targets related to teenagers in 2010, the expansion of contraceptive methods fully or heavily subsidised by the health care system in 2011, the decriminalisation of abortion in 2012, initiatives to promote youth participation in civic life in 2014 or a new strategy to prevent teenage pregnancy in 2016. Whereas these policies have received a great deal of attention in various studies demonstrating their relevance (see, e.g., Antón et al. [2018], Cabella and Velázquez [2022], Ceni et al. [2021] or Balsa and Triunfo [2021]), our research design, constrained by the number observations and the contemporaneous nature of the mentioned interventions, cannot adequately include them in the analysis and disentangle their causal effects. Therefore, they should be the object of subsequent separate research works.

Appendix II

Table II.A.1. Results of unit-root tests

	In levels				In first differences			
	ADF test		PP test		ADF test		PP test	
	t	p-value	t	p-value	t	p-value	t	p-value
log (Fertility 15–19)	5.475	1.000	3.250	1.000	−2.871	0.049	−2.856	0.051
log (Fertility 20–29)	2.333	0.999	1.656	0.998	−3.680	0.004	−3.700	0.004
log (Fertility 30+)	0.412	0.982	−0.032	0.956	−4.647	0.000	−4.630	0.000
log (GDP per capita)	0.424	0.982	0.139	0.969	−5.109	0.000	−5.043	0.000
log (employment rate)	−1.639	0.463	−1.639	0.463	−6.962	0.000	−6.969	0.000
log (enrolment rate)	−0.089	0.951	−0.208	0.938	−5.777	0.000	−5.729	0.000
log (Infant mortality)	0.422	0.982	0.836	0.992	−9.662	0.000	−9.907	0.000

Notes: The results correspond to models with a constant and without a linear time trend. They remain the same when a linear time trend is included.

Table II.A.2. Results of Johansen test for cointegration

	Fertility rate of women aged					
	15–19		20–29		30 and above	
	Statistic	5% critical value	Statistic	5% critical value	Statistic	5% critical value
Trace						
Rank = 0	72.235	68.52	79.843	68.52	80.780	68.52
Rank = 1	41.241	47.21	45.862	47.21	50.889	47.21
Rank = 2	19.058	29.68	25.459	29.68	27.221	29.68
Rank = 3	5.535	15.41	10.221	15.41	11.291	15.41
Rank = 4	0.106	3.76	0.012	3.76	0.309	3.76
Maximum eigenvalue						
Rank = 0	30.993	33.46	33.982	33.46	29.891	33.46
Rank = 1	22.183	27.07	20.403	27.07	23.668	27.07
Rank = 2	13.523	20.97	15.238	20.97	15.930	20.97
Rank = 3	5.429	14.07	10.209	14.07	10.983	14.07
Rank = 4	0.106	3.76	0.012	3.76	0.309	3.76

Note: If rank = 0, there is no cointegration relationship; if rank = 1, there is at least one cointegration relationship, and so on.

Table II.A.3. Results of the Engle and Granger test for cointegration

	Fertility rate of women aged					
	15–19		20–29		30 and above	
	t-statistic	p-value	t-statistic	p-value	t-statistic	p-value
ADF test	−3.841	0.015	−3.941	0.011	−4.836	0.000
PP test	−3.867	0.014	−3.936	0.011	−4.830	0.000

Figure II.A.1. Cumulative sum of squares tests for parameter stability

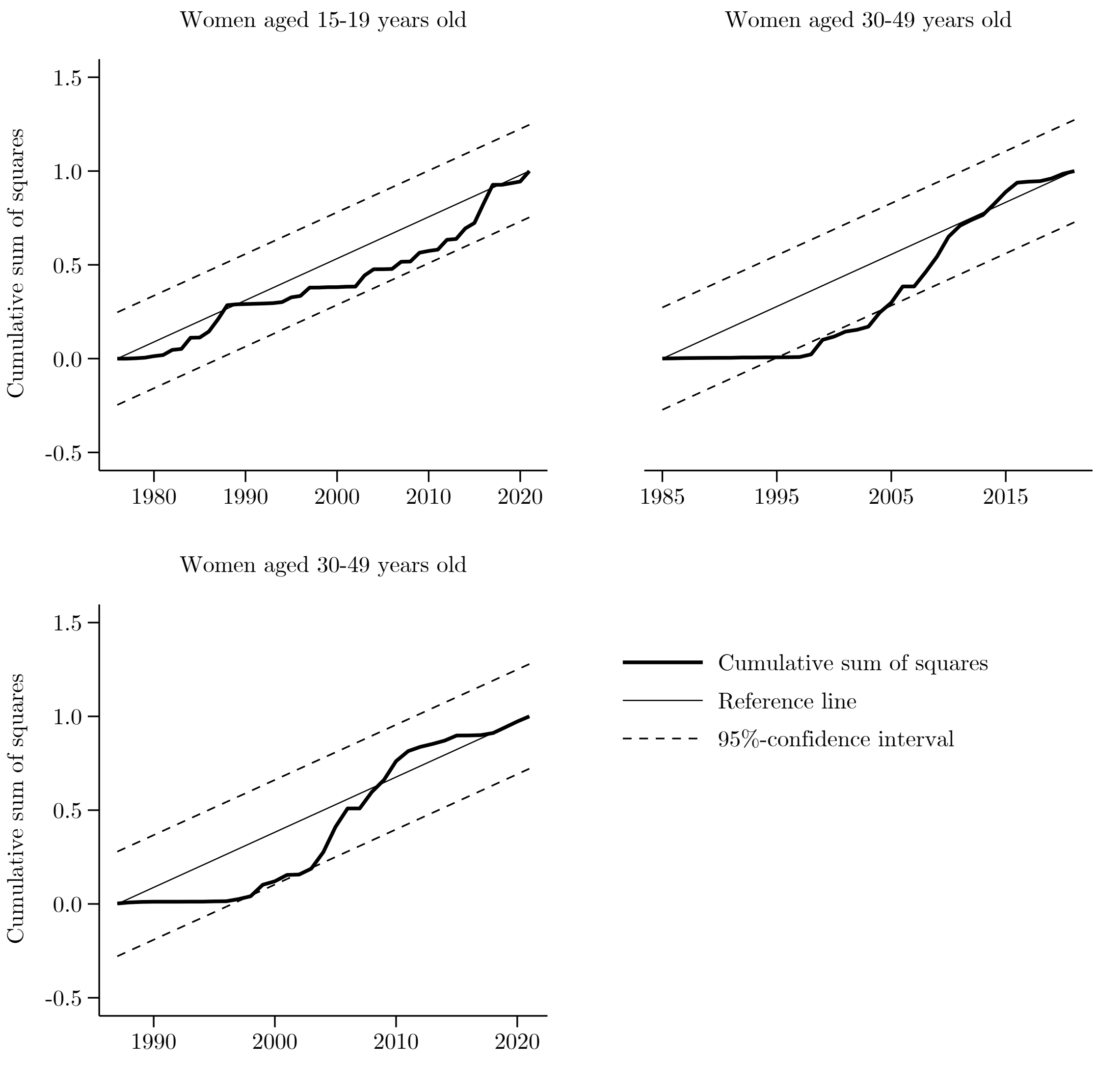


Table II.A.4. Results of Pesaran, Shin and Smith test for cointegration

	Fertility rate of women aged		
	15–19	20–29	30 and above
F-statistic	6.242	3.138	3.968
p-value $I(0)$	0.003	0.113	0.001
p-value $I(1)$	0.016	0.310	0.005
t-statistic	4.452	3.043	1.594
p-value $I(0)$	0.004	0.106	0.307
p-value $I(1)$	0.045	0.353	0.606
Residual degrees of freedom	41	31	29
Model degrees of freedom	10	10	12
No. of observations	52	42	42
ARDL model structure	(2,0,0,0,2)	(2,1,0,0,1)	(1,0,0,0,1)

Table II.A.5. Results of tests of goodness of fit

	Fertility rate of women aged					
	15–19		20–29		30 and above	
	Statistic	*p*-value	Statistic	*p*-value	Statistic	*p*-value
First-order autocorrelation test (Breusch–Godfrey)	2.163	0.149	0.410	0.527	1.570	0.220
First-order autocorrelation test (Durbin–Watson)	1.736	0.195	0.295	0.591	1.099	0.303
Heteroscedasticity test (White)	52.000	0.435	42.000	0.427	43.000	0.428
Heteroscedasticity test (Breusch–Pagan/Cook–Weisberg)	0.934	0.334	0.511	0.475	0.034	0.854
Normality test (D'Agostino et al.)	0.750	0.703	2.773	0.250	0.033	0.984

Note: The structures of the three models in the table are ARDL(2,0,0,0,2), ARDL(2,1,0,0,1) and ARDL(1,2,0,0,1), respectively.

Table II.A.6. Variance decomposition of VAR model for fertility rate of women between 15 and 19 years old

Time period	log (Fertility rate 15–19)	log (GDP per capita)	log (Female employment rate)	log (Female high school enrolment)	log (Infant mortality rate)
1	1.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2	0.766	0.050	0.031	0.108	0.044
	(0.090)	(0.051)	(0.035)	(0.060)	(0.046)
3	0.751	0.083	0.052	0.084	0.030
	(0.103)	(0.073)	(0.051)	(0.056)	(0.030)
4	0.760	0.082	0.052	0.079	0.027
	(0.115)	(0.082)	(0.055)	(0.059)	(0.029)
5	0.764	0.078	0.055	0.074	0.029
	(0.115)	(0.076)	(0.060)	(0.057)	(0.025)
6	0.767	0.077	0.055	0.073	0.029
	(0.115)	(0.073)	(0.061)	(0.056)	(0.024)
7	0.767	0.076	0.055	0.072	0.030
	(0.116)	(0.073)	(0.062)	(0.056)	(0.024)
8	0.768	0.076	0.055	0.071	0.030
	(0.117)	(0.073)	(0.062)	(0.056)	(0.024)

Note: Standard errors in parentheses.

Table II.A.7. Variance decomposition of VAR model for fertility rate of women between 20 and 29 years old

Time period	log (Fertility rate 20–29)	log (GDP per capita)	log (Female employment rate)	log (Female high school enrolment)	log (Infant mortality rate)
1	1.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2	0.955	0.001	0.012	0.024	0.008
	(0.036)	(0.007)	(0.016)	(0.025)	(0.015)
3	0.929	0.005	0.013	0.049	0.005
	(0.062)	(0.013)	(0.023)	(0.050)	(0.011)
4	0.901	0.017	0.012	0.064	0.006
	(0.088)	(0.036)	(0.026)	(0.067)	(0.016)
5	0.876	0.032	0.011	0.073	0.008
	(0.109)	(0.057)	(0.028)	(0.077)	(0.020)
6	0.860	0.041x	0.011	0.079	0
	(0.122)	(0.070)	(0.029)	(0.083)	(0.023)
7	0.850	0.046	0.010	0.083	0.010
	(0.132)	(0.078)	(0.030)	(0.089)	(0.025)
8	0.843	0.050	0.010	0.087	0.010
	(0.140)	(0.084)	(0.031)	(0.093)	(0.026)

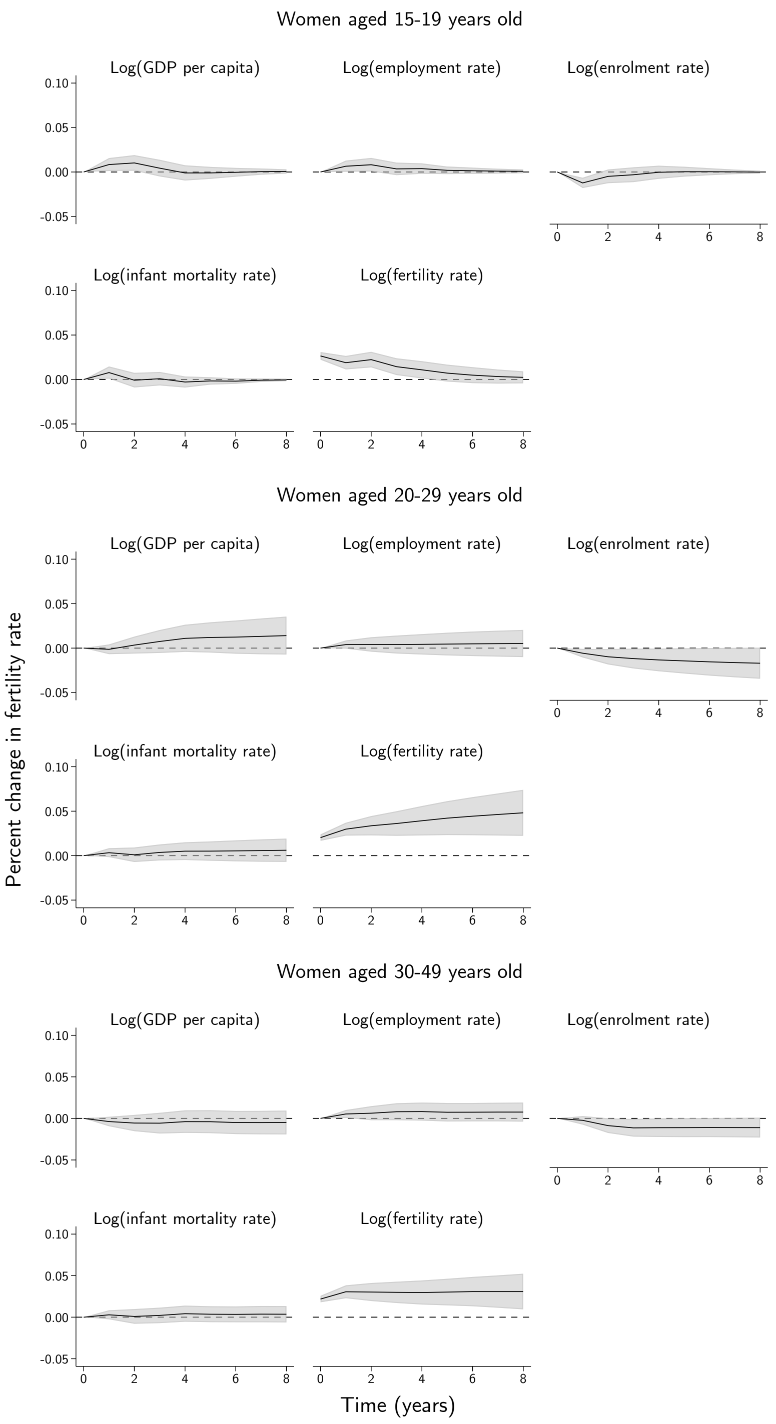
Note: Standard errors in parentheses.

Table II.A.8. Variance decomposition of VAR model for fertility rate of women aged 30 years old or more

Time period	log (Fertility rate 20–29)	log (GDP per capita)	log (Female employment rate)	log (Female high school enrolment)	log (Infant mortality rate)
1	1.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2	0.962	0.010	0.019	0.004	0.006
	(0.033)	(0.018)	(0.021)	(0.010)	(0.013)
3	0.920	0.018	0.027	0.032	0.004
	(0.068)	(0.035)	(0.036)	(0.041)	(0.011)
4	0.881	0.021	0.036	0.057	0.004
	(0.100)	(0.046)	(0.051)	(0.064)	(0.013)
5	0.862	0.020	0.042	0.070	0.007
	(0.119)	(0.048)	(0.062)	(0.077)	(0.020)
6	0.853	0.018	0.043	0.078	0.007
	(0.129)	(0.049)	(0.067)	(0.084)	(0.023)
7	0.847	0.019	0.044	0.082	0.008
	(0.135)	(0.053)	(0.070)	(0.089)	(0.024)
8	0.842	0.019	0.045	0.085	0.008
	(0.141)	(0.056)	(0.073)	(0.092)	(0.026)

Note: Standard errors in parentheses.

Figure II.A.2. Impulse response functions to a one-standard-deviation shock to covariates and fertility itself



Note: The grey-shaded areas indicate 90% confidence intervals.

Table II.A.9. Estimation results of panel data model for regional fertility rates with Driscoll–Kraay standard errors

	(I)	(II)	(III)	(IV)	(V)	(VI)
	Fertility rate of					
	women aged 15–19		women aged 20–29		women aged 30 and above	
% of women in the age bracket	−4.336***	−4.253***	−0.410	−0.023	−0.012	0.045
	(1.421)	(1.014)	(0.990)	(0.698)	(0.539)	(0.427)
% of women married (age-specific)	0.851*	0.978**	0.445***	0.570***	−0.086	−0.132**
	(0.422)	(0.363)	(0.131)	(0.115)	(0.060)	(0.046)
Average years of schooling	−4.341**	−3.445**	0.716	0.500	4.390***	0.868
	(1.556)	(1.574)	(1.096)	(1.108)	(0.911)	(0.678)
Age-specific female employment rate	0.108*	0.077	0.099*	0.017*	−0.327***	−0.150**
	(0.053)	(0.055)	(0.056)	(0.056)	(0.082)	(0.056)
Gender pay gap	9.676**	4.113	0.839	−3.580	4.356*	1.088
	(3.698)	(2.992)	(2.113)	(4.881)	(3.840)	(1.756)
Average disposable income per capita	−0.001	−0.001**	−0.002***	−0.002***	−0.001***	−0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Infant mortality rate	−0.201	−0.159	−0.275*	−0.244**	−0.083	−0.112**
	(0.140)	(0.123)	(0.053)	(0.138)	(0.105)	(0.047)
Year fixed effects	✓	✓	✓	✓	✓	✓
Department fixed effects	✓	✓	✓	✓	✓	✓
Region-specific linear time trends		✓		✓		✓
R^2	0.866	0.885	0.943	0.951	0.678	0.799
No. of observations	684	684	684	684	684	684
Mean of dependent variable	61.718	61.718	107.679	107.679	41.283	41.283

Notes: Standard errors robust to heteroscedasticity, first-order autocorrelation and cross-sectional dependence in parentheses. All the models include an intercept. *** significant at 1%; ** significant at 5%; * significant at 10%.

Chapter III

The impact of the legalisation of abortion on birth outcomes in Uruguay†

III.1. Introduction

As in other areas of social policy, Uruguay has been one of the pioneers in Latin America and the Caribbean in legalising the voluntary termination of pregnancy, being one of the few places in the region (along with Cuba, Guyana and Mexico City) where abortion on demand was legal (UN, 2014). The aim of this work is to explore the impact of a policy reform that legalised voluntary interruption of pregnancy in this country up until the 12$^{\text{th}}$ week of gestation. Our main hypothesis is that this policy change might have had a negative impact on the number of births, through a reduction in unplanned fertility, and, if this change is nonrandom, it might have led to a subsequent selection process on some birth quality outcomes.

This work makes three contributions to the existing literature. First, it provides an evaluation of a policy change in Uruguay for which there is so far no empirical evidence. In the second place, almost all previous studies on the effects of abortion legalisation have focused on developed countries, particularly, in the United States and Romania. The third contribution is linked to the identification strategy:

†This chapter, joint work with José-Ignacio Antón and Patricia Triunfo, was published in *Health Economics* (Antón et al., 2018). This chapter benefited from comments of Ana Balsa, Leonel Briozzo, Cecilia Noboa, Nicole Schneeweis, Michael Topf, Rudolf Winter-Ebmer, Sally Wright and participants at the 30$^{\text{th}}$ Annual Conference of the European Society of Population Economics and internal seminars at the Department of Economics of Johannes Kepler University Linz (Austria) and the Deparment of Economics of the University of the Republic in Montevideo (Uruguay).

Whereas previous literature either relies on before–after estimates or exploits the spatial variation in access to the voluntary interruption of pregnancy, we exploit the distinction between births from planned and unplanned pregnancies that are available in our database. Particularly, we rely on a unique set of administrative records of births in Uruguay, the Perinatal Information System (PIS), which provides very precise and detailed time and spatial information on births.

We employ a differences-in-differences (DID) approach to estimate the causal effect that the depenalisation of abortion has on fertility and birth quality outcomes, focusing on a relatively short time frame (38 months between 2011 and 2014) centred on the date of the reform and assuming that only unplanned pregnancies are affected by the policy change, whereas planned ones serve as the control group. Our findings suggest that the introduction of the voluntary interruption of pregnancy in Uruguay leads to a reduction in unplanned fertility of around 11% among women aged between 20 and 34 years old with secondary education. Overall, the observable characteristics of births from unplanned pregnancies of these women (health indicators of newborn children and their mothers and the latter's socio-demographic characteristics) are worse than average. Moreover, we find that a selection process operates in this reduction in births and within the mentioned socio-demographic group, whereby the quality of births associated with unplanned pregnancies modestly improves in terms of better prenatal control care and higher Apgar scores.

III.2. Background and previous literature

Up until the current reform, with the exception of a brief hiatus between 1934 and 1938, abortion was only permitted in Uruguay on the grounds of congenital foetal anomalies incompatible with life, rape, risk of maternal death, or economic problems.[1] Law No. 18.987, which decriminalised abortion, and subsequent legal decrees came into force on 3rd December 2012. The reform makes it possible to

[1]In these situations, the judge could exonerate both doctors and mothers. In practise, the Uruguayan authorities applied the exemptions in the law in a very restricted way, and the channels to perform an abortion with legal and medical safety were limited to rape and the aforementioned two medical reasons (Rodríguez et al., 2009). For instance, in 2009, when there were 49,152 births, they only authorised 66 interruptions (50 linked to foetal anomalies incompatible with life). In 2016, there were 9,719 legal abortions under the new law.

terminate pregnancies up until the 12^{th} week of gestation, with all associated costs covered by the Ministry of Public Health. The abortion procedure is intended to be chemical, administering misoprostol. Women who wish to terminate a pregnancy must appear before a board of three health care professionals, who provide them with detailed information on their decision (the risks of the procedure, alternative options, and the social support programmes available for maternity or adoption). After a five-day waiting period, women can confirm their decision, and then the procedure is scheduled. Females aged under the age of 18 are allowed to decide for themselves if the mentioned three-member board approves it.[2] The reform was the result of a lengthy debate, which included event a presidential veto on a similar bill in 2008. Given the extremely sensitive nature of the topic in the country, the government made big efforts to ensure that the health care system was completely ready to conduct abortion procedures by the time the law entered into force (a date widely known long in advance).

The main hypothesis for this work is that legalisation of abortion leads to a decline in births from unplanned pregnancies, which can affect the quality of the average birth (a selection process occurs on certain birth outcomes) in an ambiguous way. First, Economic Theory predicts that lowering the costs of abortion might have a positive effect on both pregnancies and terminations, with an ambiguous effect on fertility and a negative effect on unwanted fertility. According to the model of Ananat et al. (2009), women make their fertility decisions sequentially. First, to become pregnant and, second, to abort or give birth. Those choices depend on the expected payoff, with the latter decision made with more complete information on birth quality outcomes than the original decision to try to become pregnant. In the second stage, the negative effect of abortion on the unwanted number of births can lead to an improvement in child outcomes through several channels: the existence of a child quantity–quality trade-off (Becker & Lewis, 1973), the greater

[2] It is worth mentioning that, since 2002, there has existed an organisation called Health Initiatives Against Induced Abortion in Unsafe Conditions, formed by a group of health care professionals linked to the main public maternity hospital. It has provided counselling to women wanting to abort (both before and after termination) with the aim of reducing the risk of injury associated with unsafe abortion within the legal framework (Briozzo, 2007, 2008; Briozzo et al., 2002, 2006, 2007). Although they cannot provide misoprostol or any other abortive drug, there is some evidence that an informal market for these products has flourished (López Gómez et al., 2011).

likelihood for women of programming fertility consistently with their educational and labour market plans (Angrist & Evans, 2000), and a lower probability of inadequate prenatal care due to the unwanted nature of the pregnancy (Grossman & Jacobowitz, 1981; Grossman & Joyce, 1990; Joyce & Grossman, 1990; Rosenzweig & Schultz, 1983). Nevertheless, if access to abortion—or the likelihood of interrupting the pregnancy—is not independent of the mothers' characteristics, the net effect on average child outcomes can be negative and compensate for the former effect, as reported by Pop-Eleches (2006) for some outcomes in Romania. Finally, it is also worth mentioning that the reduction in unplanned fertility can itself raise the quality of the average birth, given that the quality outcomes of unwanted births tend to be significantly worse than wanted ones (Gipson et al., 2008).

The bulk of the empirical evidence on the effects of abortion on fertility outcomes originates from the United States (Ananat et al., 2007, 2009; Angrist & Evans, 2000; Bitler & Zavodny, 2002; Charles & Stephens, Jr., 2006; Cook et al., 1999; Donohue, III & Levitt, 2001, 2004; Gruber et al., 1999; Guldi, 2008; Joyce, 1987, 2004, 2009, 2010; Levine et al., 1999; Rotz, 2013; Sorenson et al., 2002). Furthermore, several studies study the consequences of changes in the abortion regime in Romania(Mitrut & Wolff, 2011; Pop-Eleches, 2006, 2010), with one piece of comparative research focused on Eastern European countries (Levine & Staiger, 2004) and another further study centred on Nepal (Valente, 2014). Overall, previous research suggests that legislation facilitating abortion is likely to prompt a drop in both fertility rates and unwanted fertility.[3] This process is often accompanied by a positive selection of births, in the sense that the decline in fertility is concentrated among those pregnancies with worse characteristics than average, which are terminated. Therefore, children born after abortion legalisation often have mothers with better characteristics and enjoy higher welfare levels in later stages of life than the average child who was born before laws legalising abortion.

The existing literature analyses not only the characteristics of mothers and families but also the short-, middle-, and long-term outcomes of children. The short-run variables considered in previous studies mainly include birth weight,

[3] There are some notable exceptions such as (Kane & Staiger, 1996) and (Levine et al., 1996).

perinatal mortality, and variables associated with the mothers' or households characteristics. However, even in those cases, the findings from previous research are not totally homogenous. For instance, Gruber et al. (1999) reports the positive effects of the decriminalisation of abortion in the United States on variables related to household characteristics (e.g., the proportion of single mothers or household socio-economic status), whereas the impact on the incidence of low birth weight is not significant. According to Bitler and Zavodny (2002), this policy led to a decline in adoptions. Pop-Eleches (2006) finds that the ban on abortion in Romania in 1966 had a greater effect on highly educated women compared with lower educated females. For the same country, Mitrut and Wolff (2011) find that the legalisation of abortion in 1990 had a positive effect on weight at birth and lowered the number of abandoned children. Valente's (2014) research on Nepal does not find any impact on observable investments in neonatal health of the reduction in fertility associated with abortion facilities.

The medium-run outcomes explored in the literature comprise variables such as anthropometric measures or infant mortality. For example, Gruber et al. (1999) report that abortion reduced the infant mortality rate (under one year old) in the United States, whereas the findings of Mitrut and Wolff (2011) for Romania suggest no impact on weight-for-height and height-for-age Z-scores for children at ages four and five.

Many authors have also focused on long-term child outcomes, such as their educational achievements, employment,earnings, or criminality. In these cases, most of the literature reports positive effects on child and family well-being. For instance, Gruber et al. (1999) find that abortions reduce the probability of poverty and receiving welfare benefits. Ananat et al. (2009) report a positive effect on not being a single parent and education outcomes but no significant impact on employment probability or the likelihood of being imprisoned.[4] In a similar vein, other authors find abortion legalisation has positive effects on the wages of children from disadvantaged backgrounds (Rotz, 2013) or a reduction in the probability of

[4]Tt is worth mentioning the findings of Pop-Eleches (2006), who finds that the abortion ban in Romania led to a positive selection of births if mothers' characteristics are not taken into account (as fertility increased more among urban and highly educated women than among poor females). On average, children outcomes (schooling level and labour market success) improved, but when controlling for mothers' observable characteristics, the authors find a decline in the same indicators.

drug usage (Charles & Stephens, Jr., 2006). Finally, it is worth mentioning the controversial findings regarding the impact of abortion on youth criminality in the United States (Donohue, III & Levitt, 2001, 2004; Gipson et al., 2008; Joyce, 2004, 2009, 2010; Sorenson et al., 2002).

Therefore, even if, in principle, we expect to find a decline in fertility and an improvement in the quality of births, given the mixed nature of findings from previous studies, the existence, extent, and direction of the selection effect should not be taken for granted.

III.3. Data and methods

III.3.1. Database

The data source used in this study is the PIS, a set of administrative records that provide precise time and spatial information on births, including the characteristics of mothers, pregnancies (such as the weeks of gestation), and newborns (Diaz-Rossello, 1998; Fescina et al., 2010; Simini, 1999; World Health Organization [WHO], 2010). The PIS aims to monitor maternal, perinatal, and child health in Latin America and the Caribbean. It draws from clinical forms commonly used in gynaecology and neonatology that are filled in by health care professionals.

Our analysis uses the PIS from 2011 to 2014. As the register's coverage was not complete for the whole country at the beginning of the period, we focus on the 15 largest maternity hospitals in Montevideo, the capital of Uruguay. They account for more than 90% and 50% of the births in the city and nationwide, respectively, during the period of analysis.

We must make several observations regarding the period of analysis and sample selection. As mentioned above, because abortion is only allowed within the first 12 weeks of gestation, we focus on those pregnancies (which end in a birth) at the 13th week of gestation, when abortion is no longer legally possible (unless under exceptional circumstances). Particularly, bearing in mind that the actual date of birth of pregnancies that reach the 13-week threshold at the same time can differ because of different periods of gestation (roughly from 28 to 42 weeks), those births that reached 13 weeks of gestation after 8th June 2014, must be excluded.

Otherwise, there could be births reading that number of weeks after that point that could correspond to 2015, whose data are not available in the database.[5]

This means the period of analysis covers slightly more than 19 months after the legislation entered into force, which constitutes a time window of 38 months (152 weeks), symmetric with respect to 3rd December 2012, which includes all the births that reached 13 weeks of gestation between 20th June 2011 and 18th May 2014. The period of analysis was characterised by economic stability and the absence of other major policy changes. Subsection 3.2 provides additional details on the time window. Overall, we use 93,762 births that are collapsed into 304-week group observations in the first part of our analysis, focused on birth quantity. When we look at birth quality outcomes for the group of women aged between 20 and 34 years old with secondary education (among whom we find evidence of an impact on fertility), the sample size shrinks to 24,630 births.

III.3.2. Identification strategy

In order to explore the causal effect of abortion legislation on fertility outcomes, we employ a differences-in-differences (DID) strategy exploiting the information in the PIS database. Our identification strategy is novel and is based on information about the planned or unplanned nature of the pregnancy. Gynaecologists ask their female patients during their visits whether the pregnancy is planned or not, and they record that information in the system. In order to obtain the causal impact of the reform, we need to assume that the legal changes in abortion laws described above only affect unplanned pregnancies. Therefore, planned pregnancies serve as a control group. The planned or unplanned nature of a pregnancy, even if not a random variable, is considered orthogonal to the abortion legislation that came into force in December 2012. It might be the case that abortion affects whether births are wanted or unwanted. According to Ananat et al. (2009), lowering the costs of abortion can lead to a higher number of both pregnancies and abortions, with an ambiguous effect on fertility. When a woman becomes pregnant, she

[5] In other words, for the same conception day in 2014, there are pregnancies that ended in a birth in 2014 and others in 2015. Therefore, to consider the whole 2014 could lead to an overestimation of the total number of births, and, if the weeks of pregnancy are correlated with the planned or unplanned nature of the pregnancy (a key variable in our identification strategy), this approach could bias our estimates.

receives more information about the costs and benefits of childbirth that might change her decision on whether to have the child or not. Therefore, whether a birth is wanted or not can be affected by the abortion regime, and the number of unplanned pregnancies might increase because of the lower costs of interrupting the them. Although these information reasons are in principle less relevant in the case of planned pregnancies, there is still room for unexpected events in the first 12 gestation weeks that can make possible that the legalisation of abortion can negatively affect the number of births from planned pregnancies. For instance, a breakup, a partner's decease, a mother's sudden serious health problem, or congenital foetal anomalies detected can lead to the termination of some initially planned pregnancies and compromise our identification strategy. In Section III.4, we discuss in detail the possible threats to the validity of our identification strategy, with special emphasis on prenatal genetic testing. We argue that, at least, the impact recovered by the estimates can be considered as a lower bound of the impact of the new abortion policy.

The DID approach requires only that, in the absence of the treatment (the policy intervention allowing legal abortion), both groups would have evolved in a parallel way (i.e., the parallel trends assumption). Time fixed-effects control for the influence of common shocks affecting both planned and unplanned pregnancies. Regarding group-specific shocks, during the short time window considered in the analysis (roughly three years), it is unlikely that Uruguay saw major changes in the patterns of pregnancy planning for cultural or sociological reasons that might otherwise explain eventual changes in fertility outcomes. The assumption of the reform's lack of impact on planned births is not directly testable. We comment on this issue in further detail in the discussion section.

In order to explore the effect of the reform on fertility, we estimate the following reduced-form econometric model:

$$\log\left(\text{births}_{gt}\right) = \alpha + \beta\left(\text{unplanned}_g \cdot \text{abortion law}_t\right) + \gamma \text{unplanned}_g + \eta_t + \varepsilon_{gt} \quad \text{(III.1)}$$

where $\log\left(\text{births}_g t\right)$ represents the natural logarithm of the number of births of group g (planned or unplanned) in time period t; α is an intercept; the variable unplanned_g is a group dummy variable that takes the value zero for the series of planned pregnancies and one for the series of unplanned ones; abortion law_t is a time dummy taking the value one when the legislation allowing voluntary termination of pregnancy is in force and zero otherwise; η_t denotes time fixed-effects; and ε_{gt} is a random disturbance. The parameter of interest is β, which, under the parallel trends assumption, captures the causal effect of the abortion legislation—particularly, the average treatment effect—on the number of births. Within this framework, with the number of births per week as our left-hand-side variable, we do not include any additional controls to estimate the main effect of the law, because we subsequently perform a separate analysis by mothers' age and education level. Furthermore, most of the observable characteristics of births (even age and schooling) can be considered as outcomes, so they would be "bad controls" in the sense of Angrist and Pischke (2008).

Three additional points should be made. First, a particularly relevant date in the analysis is when women who effectively give birth reach 13 weeks of gestation. By then, abortion is not legally possible. In order to recover a reasonably homogeneous treatment effect, we focus our attention on those births of mothers who have been exposed to the new law for at least 12 weeks.[6] We therefore focus on what happened 12 weeks after the law came into force. The eventual effect of the reform during such a period is captured by an additional variable added to Equation III.1 that we call transition$_{gt}$, which is simply a binary variable that takes the value one for unplanned pregnancies during those 12 first weeks and zero otherwise.[7] Second, aiming to assess how plausible the parallel trends assumption is as one of the main tools for checking the robustness of the DID estimations suggested by Angrist and Pischke (2008), we include a group-specific linear time trend. Even if the parallel trajectories of groups are not observed, this allows obtaining consistent estimates under the assumption of the series' parallel growth (Mora & Reggio, 2017). Finally, to shed some additional light on the validity of the identification strategy used in the analysis, we perform two falsification tests—we estimate the effect of two "placebo" interventions—described in Section III.4.

[6] Within this framework, the first women fully treated are those reaching 13 weeks of gestation, 12 weeks after the reform came into force. Even assuming complete access to abortion facilities as soon as the law became effective, the exposure of a woman reaching 13 weeks of gestation a few days after the reform came into force and that of another female exposed for 12 weeks may actually be very different.

[7] This variable can be thought of as a treatment effect for the first 12 weeks, an interaction between a dummy variable for such periods and the unplanned group dummy. This also ensures there is no possibility of anticipation effects captured by the treatment variable.

The model presented above is estimated for the full sample of births considered and, then, separately, for each age-education group. For those groups where we find a drop in fertility, we verify whether there is a selection of births on observable characteristics underpinning such a decline. In other words, the reduction in births might affect some groups more than others, being concentrated on potential mothers and children with certain characteristics. As mentioned in Section III.2, previous empirical evidence is ambiguous about the expected direction of the selection. To unravel the existence of such a selection process, we estimate the following reduced-form model based on individual birth data:

$$\text{outcome}_{it} = \alpha + \beta\left(\text{unplanned}_i \cdot \text{abortion law}_t\right) + \gamma\text{unplanned}_i + \eta_t + \varepsilon_{it} \quad \text{(III.2)}$$

where outcome_{it} denotes a certain outcome of birth i, which takes place in period t. The rest of the equation's terms have the same meaning as in Equation III.1. Based on the availability of variables in the database, we focus on the following nine birth quality outcomes: birth weight (in natural logs), premature birth (fewer than 37 weeks of gestation), adequate prenatal care according to the Kessner Index or the criteria of the Uruguayan Ministry of Public Health, the Apgar score at one and five minutes, single mother, hypertensive mother, mother with eclampsia, and mother with preeclampsia.[8] Following the reasoning outlined above, we also include a transition variable in Equation III.2.

Both models are estimated by ordinary least squares.[9] In order to take into account the possible intragroup correlation in both models, we cluster standard errors at the time-group level. However, it is possible that serial correlation within groups is relevant, which might inflate standard errors (Angrist & Pischke, 2008; Bertrand et al., 2004). As there are only two groups, there is no evident completely

[8]According to the Kessner criteria, a mother receives adequate prenatal care if there is a prenatal care visit in the first quarter and at least nine contacts by the end of the pregnancy (Kotelchuck, 1994). The Ministry of Public Health of Uruguay has a target of a first control in the first quarter and at least six visits before the birth.

[9]Ordinary least squares are preferred over other alternatives that could fit certain right-hand side variables, such as the Poisson or the negative binomial regression model, because the requirement for consistency of the latter is more demanding than in the case of the linear regression. Particularly, under those types of models, consistent estimates of the parameter of interest require certain assumptions on the functional form of the perturbation to be fulfilled whereas the same property in the linear regression model only needs the absence of omitted relevant variables (Angrist & Pischke, 2008).

satisfactory way to address this problem.[10] Therefore, as a robustness check, in Equation III.1, we implement several versions of standard errors under the Newey–West estimator (Newey & West, 1987) that are robust to autocorrelation up to a certain order.[11] In particular, when controlling for serial correlation, we focus on the results based on the criterion of Newey and West (1994), who suggest controlling for a number of lags equal to $0.75T^{1/3}$, with T being the number of available time periods, although we consider different numbers of periods in the analysis. In the case of the second model, in order to control for the possible serial correlation at the group level, we collapse the data set at the time-group level, and using the mean of the variables at that level and weighting by the number of births in each group in each time period, we implement the mentioned Newey–West estimator. We also carry out the estimation using weeks as time units, although results with months are basically the same.[12]

Table III.1 and III.2 present the main descriptive statistics of the samples employed in the analysis. Table III.1 shows the mean and standard deviation of the variables of interest corresponding to the econometric exercise represented by Equation III.1 for the quantity of births. Table III.2 contains the same statistics for the variables used when—in Equation III.2—we explore the effect of abortion legislation on the quality outcomes of births of women aged from 20 to 34 years old with secondary education. From these descriptive statistics, we can see that the prevalence of births from unplanned pregnancies is considerably different across demographic groups. Before the intervention, they accounted for less than 25% of total births, but their weight is much more relevant among women with primary education or under the age of 20.

[10] With more than 50 clusters (groups), one can cluster standard errors at the group level, which are therefore robust to serial correlation of unknown form. Unfortunately, there is no equivalent method for implementing a similar strategy with only two groups.

[11] According to the literature, serial correlation is common in time-series fertility. See, for instance and among many others, Prskawetz et al. (2009) and Brehm and Engelhardt (2015).

[12] The statistical significance of the coefficients is exactly the same, although the size of the coefficients might obviously change. These results are available from the authors upon request.

Table III.1. Descriptive statistics of the variables used in the analysis of the quantity of births

	Planned births			Unplanned births		
	Mean before [standard deviation]	Mean after [standard deviation]	Difference (standard error)	Mean before [standard deviation]	Mean after [standard deviation]	Difference (standard error)
No. of births	225.00	235.05	10.05***	172.59	155.04	−17.55***
	[17.51]	[16.81]	(2.78)	[13.56]	[15.35]	(2.35)
No. of births of mothers aged	6.79	6.93	0.14	13.89	12.14	−1.75***
under 20 with primary education	[2.47]	[2.72]	(0.42)	[4.51]	[3.35]	(0.64)
No. of births of mothers aged	17.12	18.26	1.14*	32.89	29.95	−2.95***
under 20 with secondary education	[3.82]	[4.37]	(0.67)	[6.28]	[5.03]	(0.92)
No. of births of mothers aged	22.99	21.05	−1.93	29.13	23.97	−5.16***
20–34 with primary education	[4.64]	[4.82]	(0.77)	[5.24]	[4.68]	(0.81)
No. of births of mothers aged	94.97	98.96	3.99**	67.50	62.64	−4.85***
20–34 with secondary education	[9.78]	[9.79]	(1.59)	[8.02]	[8.13]	(1.31)
No. of births of mothers aged	46.39	48.89	2.50**	10.93	9.39	−1.54**
20–34 with tertiary education	[7.13]	[6.58]	(1.11)	[3.71]	[3.57]	(0.59)
No. of births of mothers aged	3.30	3.49	0.18	4.58	3.51	−1.07***
35 or over with primary education	[1.95]	[1.73]	(0.30)	[2.22]	[2.06]	(0.35)
No. of births of mothers aged	17.12	18.26	1.14*	32.89	29.95	−2.95***
35 or over with secondary education	[3.82]	[4.37]	(0.67)	[6.28]	[5.03]	(0.92)
No. of births of mothers aged	16.67	18.93	2.26***	3.14	2.67	−0.47*
35 or over with tertiary education	[4.16]	[4.73]	(0.72)	[1.64]	[1.53]	(0.26)
No. of observations (weeks)	76	76		76	76	

Note: Standard deviations of the variables in brackets and standard errors of the differences in means of the variables in parentheses. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

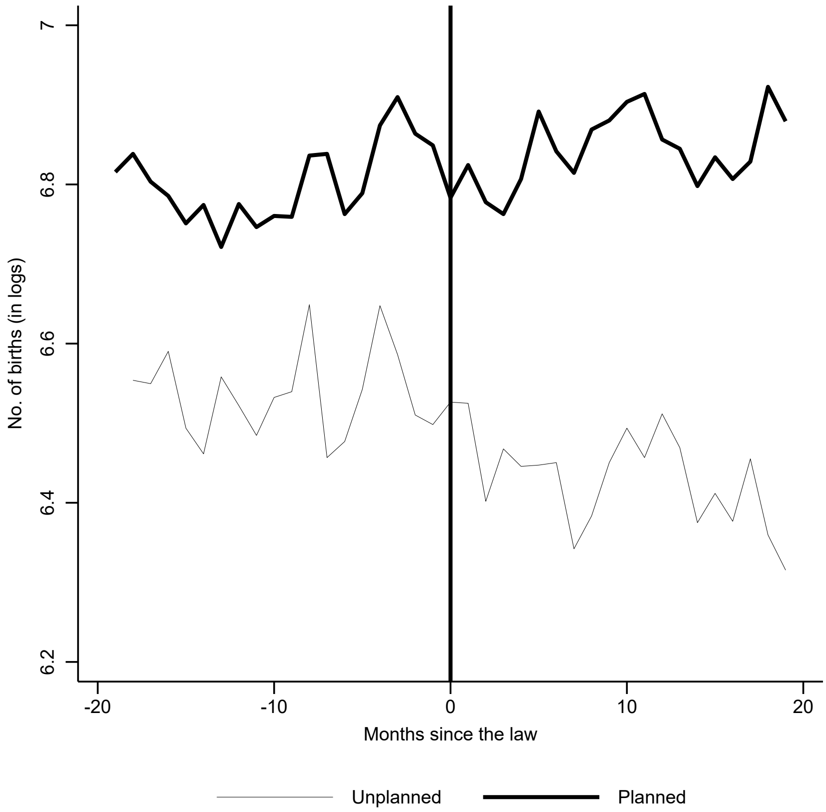
Table III.2. Descriptive statistics of the variables used in the analysis of the quality of births of mothers aged 20 to 34 with secondary education

	Planned births			Unplanned births		
	Mean before [standard deviation]	Mean after [standard deviation]	Difference (standard error)	Mean before [standard deviation]	Mean after [standard deviation]	Difference (standard error)
Birth weight (kg)	3.289	3.299	0.011	3.256	3.285	0.029**
	[0.571]	[0.575]	(0.009)	[0.592]	[0.570]	(0.012)
Premature	0.089	0.085	−0.004	0.093	0.090	−0.003
	[0.285]	[0.279]	(0.005)	[0.291]	[0.287]	(0.006)
Adequate prenatal care	0.691	0.722	0.031***	0.455	0.508	0.053***
(Kessner Index)	[0.462]	[0.448]	(0.007)	[0.498]	[0.500]	(0.010)
Adequate prenatal care	0.823	0.849	0.026***	0.585	0.632	0.046***
(Ministry of Public Health)	[0.382]	[0.358]	(0.006)	[0.493]	[0.482]	(0.010)
Apgar at 1 minute	8.465	8.499	0.034*	8.449	8.518	0.068***
	[1.220]	[1.206]	(0.020)	[1.231]	[1.146]	(0.024)
Apgar at 5 minutes	9.585	9.604	0.018	9.558	9.603	0.045**
	[0.998]	[1.012]	(0.017)	[1.000]	[1.014]	(0.020)
Single mother	0.098	0.110	0.011**	0.243	0.254	0.011
	[0.298]	[0.312]	(0.005)	[0.429]	[0.435]	(0.009)
Hypertensive mother	0.020	0.020	0.001	0.025	0.019	0.005*
	[0.138]	[0.141]	(0.002)	[0.155]	[0.138]	(0.003)
Mother with preeclampsia	0.032	0.035	0.002	0.030	0.037	0.007**
	[0.177]	[0.183]	(0.004)	[0.171]	[0.189]	(0.003)
Mother with eclampsia	0.001	0.001	0.000	0.002	0.001	−0.001
	[0.026]	[0.035]	(0.000)	[0.044]	[0.032]	(0.001)
No. of observations (births)	7,218	7,531		5,130	4,761	

Note: Standard deviations of the variables in brackets and standard errors of the differences in means of the variables in parentheses. In some of the variables, the number of observations is slightly lower because of missing values. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Figure III.1 shows the evolution of the total number of births per month (in natural logs) during the period of interest. Although the figure is descriptive and there seems to be a pre-existent trend, the graph suggests a decline in the pattern of births from unplanned pregnancies.

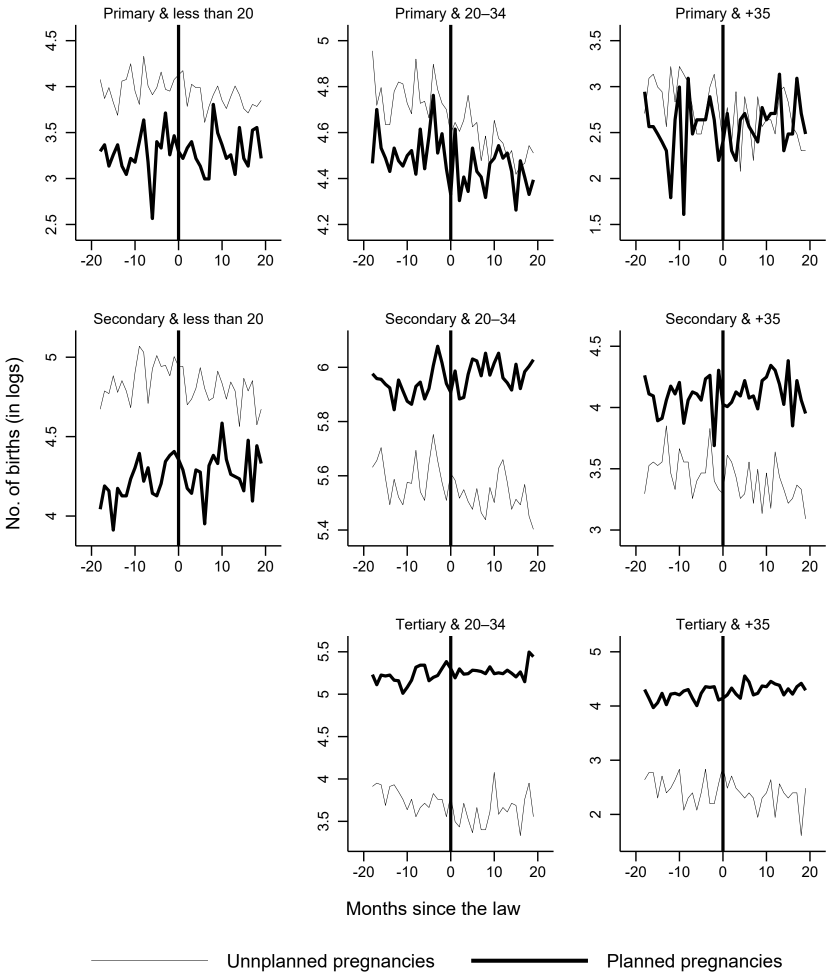
Figure III.1. Evolution of the number of births before and after the law



Note: Months represents groups of four weeks. The p-value of the difference between the trends of the time series depicted in the figure before the law came into force is 0.209.

Figure III.2 depicts the same relationship by age-education group, revealing the different relevance of unplanned pregnancies across demographic groups. Below both figures, we present the p-values of a test of equality of linear trends between planned and unplanned pregnancies before the law came into force using the four-week data used for building the graphs. We find that the difference is not statistically significant at the 10% level in the series of total births and in all the subpopulations of mothers with the exception of the women between 20 and 34 years old and tertiary education. Nevertheless, this group only accounts for less than 5 and 2% of total and unplanned births, respectively, before the abortion law and the treatment is not significantly different from zero when we include in regressions a group-specific linear time trend. The results of the econometric analysis excluding this group remain unchanged. Although it is speculative to infer a clear outcome from the graph, the figure suggests a decline in the number of births of women aged between 20 and 34 years old who finished secondary education (a core group in terms of fertility, representing 41.4% of total births before the abortion legislation in our database) since the law came into force.

Figure III.2. Evolution of the number of births before and after the law by mothers' age group and education



Note: Months represent groups of four weeks. By columns and from up to bottom, the p-value of the difference between the trends of the monthly time series depicted in the figure before the law came into force for each group are 0.993 and 0.633 (1^{st} column); 0.639, 0.575 and 0.001 (2^{nd} column); 0.402, 0.546, and 0.199 (3^{rd} column).

III.4. Results

The results of estimating Equation III.1 under different strategies are shown in Table III.3 They suggest that the abortion legislation has had a negative impact on the number of births. However, after controlling for a group-specific linear time trend, the negative impact falls from 17% to 8%. The outcomes of the analysis are robust to the consideration of serial correlation up to order four (consistent with the rule of thumb mentioned above), and, reassuringly, the falsification tests add further confidence over the existence of a causal effect of this health policy intervention. Furthermore, when we perform the analysis considering different orders of autocorrelation (up to order 12) and including the dependent variable in levels, we obtain comparable results.[13] As mentioned above, we implement two "placebo" interventions. The first "placebo" (placebo A) law consists in a "treatment" applied on unplanned births to the 12 weeks prior to the coming into force of the abortion law. The second one (placebo B) looks at what happens with unplanned births during 12 weeks in 2012 corresponding to the first 84 days of our period of treatment but a year earlier (roughly, from the end of February to the end of May). Reassuringly, neither of the fake treatments is statistically significant.

[13] These results are available in the appendix (Tables III.A.1–III.A.3).

Table III.3. Effect of abortion legislation on the number of births (in logs)

	(I)	(II)	(III)	(IV)	(V)
Treatment	−0.171***	−0.081**	−0.081**	−0.103**	−0.078**
	(0.017)	(0.038)	(0.017)	(0.048)	(0.037)
Placebo A				−0.032	
				(0.034)	
Placebo B					0.011
					(0.041)
Week fixed effects	✓	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓	✓
Group-specific linear time trend		✓	✓	✓	✓
Control for serial correlation			Until AR(4)	Until AR(4)	Until AR(4)
R^2	0.933	0.936	0.936	0.936	0.936
No. of observations	304	304	304	304	304
Mean of dependent variable	5.264	5.264	5.264	5.264	5.264

Note: Standard errors robust to heteroscedasticity and the corresponding type of serial correlation in parentheses. All the specifications include a constant and a transition variable. The group dummy is a binary variable that takes the value zero if it is a planned pregnancy and one if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

In the second place, as outlined in Section III.3, we repeat the analysis separately for each age-education group of women in order to identify which demographic collective is driving the results presented above. Our results clearly indicate that the fall in fertility is exclusively driven by the group of women aged between 20 and 34 years old with secondary education. The results shown in Table III.4 do not control for serial correlation for reasons of brevity, but the results remain the same when we controlling for it.[14] In the next section, we present several arguments that might explain why this demographic group is the only one affected by the reform. We also comment there on the possibility that the abortion policy may have some influence on planned births for several reasons—for instance, prenatal genetic screening tests before the 12$^{\text{th}}$ week of pregnancy.

[14] As in the previous case, in this robustness check, we consider fourth-order autocorrelation as the baseline, and we check whether the results are robust, allowing autocorrelation up to the order of 12. These results are available from the authors upon request.

Table III.4. Effect of abortion legislation on the number of births (in logs) by age and education

	(I) Under 20 & primary	(II) Under 20 & secondary	(III) 20–34 & primary	(IV) 20–34 & secondary	(V) 20–34 & tertiary	(VI) 35 or over & primary	(VII) 35 or over & secondary	(VIII) 35 or over & tertiary
Treatment	0.002	−0.055	0.023	−0.109**	−0.054	−0.245	−0.057	−0.180
	(0.188)	(0.109)	(0.111)	(0.052)	(0.165)	(0.324)	(0.204)	(0.243)
Week fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓	✓	✓	✓	✓
Group-specific linear time trend	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.747	0.828	0.659	0.901	0.942	0.523	0.771	0.926
No. of observations	304	304	304	304	304	292	304	297
Mean of dependent variable	2.183	3.138	3.162	4.368	3.050	1.214	2.332	1.933

Note: Standard errors robust to heteroscedasticity in parentheses. All the specifications include a constant and a transition variable. The group dummy is a binary variable that takes the value zero if it is a planned pregnancy and one if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Even if there is no selection effect of births based on either observable or unobservable characteristics, the fact that the decline of births only affects a very specific and particular demographic group might have some effect on the characteristics of the average birth in the country. Considering this question, we look at the average characteristics of births of the affected group of mothers (women aged from 20 to 34 years old who completed secondary education) compared with the average characteristics of the rest of births (Table 5). With a very few exceptions, there are no major differences between the quality of births between both groups. Births from unplanned pregnancies—whose weight declines—are characterised by less adequate prenatal care and a higher proportion of single mothers than those resulting from planned pregnancies. Although statistically significant, the size of the differences in terms of the proportion of births whose mothers suffered from eclampsia (larger for the affected group) is small and of little relevance in economic terms. These features suggest that, ceteris paribus, the abortion reform prompted an improvement in the quality of the average birth in terms of prenatal care examinations.

As outlined in Section III.3, the second part of the analysis focuses on the qualitative outcomes of births among those women who are affected by the intervention according to the results shown above. We therefore estimate Equation III.2 for the affected group of births in order to see whether an underlying selection of births is operating here. It is worth mentioning that the previous literature does not provide a "shortcut" hypothesis on how the decline in fertility due to the legalisation of abortion can affect birth outcomes, in the sense that either a positive or a negative selection process might be observed. The results of our analysis (Table III.6) suggest that there is only a slight positive selection of births in terms of prenatal control care and the Apgar score.[15] The probability of receiving adequate prenatal care according to the Kessner criteria increases by 5% points and 4.2% if we follow the definition of the Ministry of Public Health of Uruguay. Meanwhile, the reform

[15]For these four cases, in order to control for the existence of serial correlation, we repeat the analysis by collapsing the data into cells and using as right-hand-side variable the cell means weighted by the number of births corresponding to each cell. This yields exactly the same coefficients as those included in Table III.6 but allows us to deal with serial correlation using the Newey–West estimator. The results of this exercise, available from the authors upon request and not reproduced in the text for reasons of space, are very similar to the ones shown here.

Table III.5. Differences in means of observable characteristics between births from unplanned pregnancies of mothers aged 20 to 34 with secondary education and the rest of births before the reform

	(I) Births from unplanned pregnancies of women aged 20–34 with secondary education	(II) Rest of births	(III) Difference ([III] = [I] − [2])
Birth weight (in logs)	1.159	1.155	0.004
	(0.003)	(0.001)	(0.004)
Premature births	0.093	0.098	−0.005
	(0.004)	(0.002)	(0.005)
Adequate prenatal care visits (Kessner)	0.455	0.597	−0.142***
	(0.007)	(0.003)	(0.008)
Adequate prenatal care visits (Ministry)	0.585	0.729	−0.144***
	(0.007)	(0.003)	(0.007)
Apgar one min (in logs)	2.124	2.119	0.005
	(0.003)	(0.001)	(0.003)
Apgar five min (in logs)	2.257	2.254	0.003
	(0.002)	(0.001)	(0.002)
Single mother	0.243	0.171	0.073***
	(0.006)	(0.002)	(0.006)
Hypertensive mother	0.025	0.021	0.003
	(0.002)	(0.001)	(0.002)
Pre-eclampsia	0.032	0.030	0.003
	(0.002)	(0.001)	(0.003)
Eclampsia	0.002	0.001	0.001**
	(0.001)	(0.000)	(0.000)
No. of observations	5,130	24,785	

Note: Standard errors in parentheses. The number of observations is lower in some variables because of missing values. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

has a positive impact on the Apgar score at one and five min of 2.2% and 1%, respectively.

Table III.6. Effects of abortion legislation on qualitative birth outcomes among mothers aged 20 to 34 with secondary education

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
	Birth weight (in logs)	Premature	Adequate prenatal care (Kessner)	Adequate prenatal care (Ministry)	Apgar one min. (in logs)	Apgar five min. (in logs)	Single mother	Hyperthensive mother	Pre-eclampsia	Eclampsia
Treatment	0.006	−0.001	0.052**	0.041**	0.022***	0.010**	0.009	−0.008	−0.003	−0.002
	(0.008)	(0.013)	(0.023)	(0.018)	(0.008)	(0.004)	(0.018)	(0.007)	(0.008)	(0.001)
Week fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Group-specific linear time trend	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R^2	0.006	0.006	0.062	0.075	0.008	0.007	0.043	0.007	0.006	0.007
No. of observations	24,613	24,630	24,146	24,095	24,442	24,447	24,117	24,630	24,630	24,630
Mean of dependent variable	1.169	0.089	0.617	0.745	2.130	2.260	0.162	0.021	0.034	0.001

Note: Standard errors clustered at the week-group level between parentheses. All the specifications include a constant and a transition variable. The group dummy is a binary variable that takes the value zero if it is a planned pregnancy and one if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

III.5. Discussion

There are several issues regarding our results that should be commented here: the plausibility of the exogeneity of the planned or unplanned nature of pregnancies (with respect to the possibility of abortion) and other threats to the validity of our identification strategy, the reasons that may explain why women aged between 20 and 34 years old are the only affected group and the absence of large impacts of the new abortion policy and other possible implications of the legalisation of the voluntary termination of pregnancy not covered here.

Our first set of comments discusses how plausible are the assumptions behind our identification strategy. An essential ingredient in our research design is due to the exogeneity of the planned or unplanned nature of the pregnancy. In this respect, it is worth mentioning that there are several contributions regarding the endogeneity of desired births (a different issue from planned births). In this respect, several studies (Ananat et al., 2009; Kane & Staiger, 1996) suggest that abortion could act as an insurance, in the sense that people (obviously, not those planning a birth) could be less careful regarding contraceptive methods as there is still the option of terminating a pregnancy. However, this issue would only affect unplanned births and not planned ones, and therefore, it is not a source of concern for us. A second relevant element to discuss here has to do with the possibility of terminating planned pregnancies before reaching the 12^{th} week of gestation. The Uruguayan Ministry of Public Health recommends that the first antenatal visit (where the future mother is usually asked about the planned nature of the birth) is scheduled before the first 12 weeks of pregnancy—particularly, as soon as the woman notices she has missed her period. In our database, the question about the nature of the birth is usually asked by the gynaecologist in the first prenatal care visit. It is therefore possible that the policy intervention depenalising abortion also has a negative effect on planned births, in the case of unexpected events, such as a breakup, the partner's decease, or a mother's sudden serious health problem before reaching the legal time limit. It might be particularly interesting the effect that prenatal genetic testing before the 12^{th} week can have on planned births. However, several issues might attenuate the seriousness of this problem. First, as many other countries, before the decriminalisation of voluntary abortion, Uruguay considered both congenital foetal anomalies incompatible with life and health conditions that seriously threat mother's life as "justified" causes of abortion (both mothers and doctors could be exempted with from any punishment, with even a committee at the Ministry of Health deciding on these cases ex-ante). [16] Therefore, any abortion related to information provided by prenatal testing detecting congenital foetal anomalies incompatible with life should have had a similar effect on births either before or after the abortion law. Second, the 12-week limit was not set by chance: It aimed to prevent eugenic abortions, terminations linked to congenital anomalies compatible with life (Johnson, 2011).[17] However, recent advances in prenatal genetic testing based on non-invasive techniques allow detecting some genetic conditions like trisomy 21 or Turner syndrome—at earliest—around the 10th week, leaving room for interruptions linked to some foetal anomalies compatible with life. Nevertheless, given that the results of these tests are not immediate (usually takes around 10–12 days) and the time constraints imposed by both the law (the 12-week limit and the five-day waiting period for reflecting about the abortion), it is difficult to legally perform a legal abortion associated to the detection of these types of congenital anomalies.[18] Third, the rate of congenital anomalies

[16] The same applies to rape, although the new law extended the possibility of abortion until the 14th week.

[17] For instance, in some countries, like Spain, which permits voluntary abortion until week 12, the interruption for congenital anomalies is allowed up until the 22nd week.

[18] The public health care suppliers schedule the first antenatal test between the 11th and 13th week of gestation since 2014. It consists in a blood test—triple screening—and a nuchal translucency ultrasound intended to detect trisomy 13, 18, and 21, with only the latter chromosomic anomaly is compatible with life. Given the legal and technological time constraints stated in the main text, it is virtually impossible to terminate a pregnancy within the standard times and protocols in the public health care. The chances are only slightly better in the private sector: This sort of blood test was not available in the country until as late as in February 2013, with only one private medical centre, which sent the blood samples to a lab in the United States, offering it by August 2013. Not everyone could afford it by then, as its price, US$1,600, represented more than 60% of the average national monthly wage. This screening does not provide a definitive answer to the existence of foetal anomalies, so doctors recommend undergoing a chorionic villus sampling or an amniocentesis when the probability of genetic disorders detected by these tests is high. The latter procedures are invasive and not exempted from miscarriage risks, so they are mainly intended for females with a personal or family history of a genetic condition or certain high-risk pregnancies. They are usually carried out between the 11th and 14th week of gestation, with higher miscarriage risks if performed earlier. The results of these screenings can take from few days to several weeks, depending on the condition on the genetic disorders tested. In 2013 and without actual data on this phenomenon, Uruguayan gynaecologists did not agree on the relevance of the new screening procedures based on blood sampling in triggering legal terminations of pregnancies and statistical information on that issue was lacking. Although

among newborns in Uruguay before the law was quite low. For instance, their incidence at a major private hospital—the British Hospital—was 12‰of live births in 2003–2005 (Bonino et al., 2006). Finally, there are quite many anomalies that can only be detected after the 12th-week limit or which detection is usually done much later (for instance, structural anomalies [National Institute for Health and Care Excellence, 2017]). Although the aforementioned reasons suggest that the effect of abortion on planned pregnancies should be minimal, if it existed, our estimates would be downward biased, and we would be underestimating the true (negative) effect of abortion on fertility. This is a limitation of our analysis that must be highlighted, although, as mentioned, in the worst of the cases, the true impact of abortion would be larger than the one estimated here. Lastly, in order to ensure the relevance of our results, we carry out several tests that can shed some light on the validity of our identification strategy. First, we adjust a linear time trend to the series of planned births and introduce a dummy indicating when the abortion law comes into force. If this fictitious variable were statistically significant and negative that would indicate that the trend of planned births changes and that the abortion law could have negatively affected planned births. Specifically, we regress the weekly number of planned births in the 152 weeks included in the analysis on a linear time trend and a dummy variable that takes the value one when the reform entered into force and zero otherwise. Reassuringly, the coefficient of this binary variable is not significant (the p-value is 0.253). In the second place, unrelated to the absence of effects of abortion policies on planned pregnancies (see the discussion above), the suitability of our identification strategy is also supported by the two different "placebo" tests described above. Neither the first nor the second "placebo" interventions is statistically significant, which reinforces our confidence on the approach adopted.[19]

a doctor based at the only private centre offering this procedure in 2013 thought that it could have an influence (even though the confirmatory analysis are performed after the 12th week), other gynaecologists believed that it was quite unlikely that this issue was triggering terminations (Barquet, 2013). Moreover, it is also sensible to think that most of pregnancies of women with a personal or family history of a genetic condition are not very likely to be planned, so part of the effect of these medical factors should fall on unplanned gestations. Therefore, it is reasonable to conclude that the importance of these factors might be limited, because, at best, only part of high-risk pregnant women can have accessed to those procedures in such a way they could have legally aborted prior to the 12th week of gestation.

[19]As stated in the main text, the "placebo" laws do not guarantee that the abortion policy

In the second place, there are several reasons that might explain why women aged between 20 and 34 years old is the only demographic group affected by the reform. The first explanation has to do with the fact that women between 20 and 34 years old represent the main fertility group, adding up to more than 70% of total fertility. In particular, more than four out of 10 births correspond to females in this age group with secondary education. Second, as mentioned in Section III.4, since 2002, a decade before the abortion law was passed, there has been a group of health professionals (Health Initiatives Against Induced Abortion in Unsafe Conditions) based at the main maternity hospital in Montevideo (the Pereira Rossell Hospital) advising women wishing to terminate their pregnancies and seeking above all to guarantee the safety of abortions. In such an environment, one could find the pharmacological means for doing so (mainly, misoprostol) in the black market (López Gómez et al., 2011).

Therefore, the possibility of chemical abortion in the decade prior to the reform, even if not widespread, existed to a certain extent. Although there are no statistics available on this issue, one may speculate that the access to abortion was not randomly distributed across demographic groups. Given that the Pereira Rossell Hospital is a public centre mainly providing health services to people from low socio-economic backgrounds, it is possible that women with medium and high levels of education, with higher resources, were less likely to access to the facilities of Health Initiatives or were not willing to risk participating in a system targeting low-income population.[20] Third, one should bear in mind that it is not surprising to find that the impact of abortion or contraceptive measures differs by socio-economic group (see, among many others, Angrist and Evans [2000], Bitler and

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does not affect planned pregnancies, but this test is an overall assessment of the plausibility of the parallel trend assumption looking at the time series during a short window before the actual treatment. The plausibility of the non-impact of abortion decriminalisation on planned pregnancies is grounded in the two first issues raised in the discussion section: the theoretical discussion on the previous literature and the linear trend planned pregnancies seem to follow, which is unaffected by the law. It is also worth mentioning that the second "placebo" test rules out the possibility of anticipation to the law's enactment.

[20]This programme, based at the Pereira Rossell Hospital, and which existed before the reform, mainly treated low-education groups. The females attending this public health centre, mostly with a low socio-economic status, would have had better access to abortion than women treated at other hospitals. In order to further explore this issue, we repeat our analyses only for the births at the Pereira Rossell Hospital, not finding any statistically significant effect for any group. These results are presented in the supplementary material (Table III.A.4).

Zavodny [2002], Guldi [2008], Levine et al. [1999], Mitrut and Wolff [2011] and Zavodny [2004]). Although we argue above that it is not likely that the legalisation of abortion has a substantial effect on planned pregnancies should not be a major source of concern, this issue might affect socio-demographic groups in a different way, as, for instance, they differ in their tastes and probabilities of having high-risk pregnancies. Closely related to the remarks made above, there are several possible explanations for the absence of any dramatic effects of the reform. A first argument lies on the operation of Health Initiatives programme prior to the legalisation of abortion, which not only favoured the existence of a social climate supporting voluntary interruption of pregnancy, but it also somehow made it easier to access the procedure for interested women. Furthermore, the health technology existing at the time of around the reform allowed for most illegal abortions to be carried out with pharmacological means, leaving more leeway for the possibility of nonlegal terminations than in the American or the Romanian cases several decades earlier. In this respect, the modest results reported here are quite similar to the findings for Nepal, legalising abortion in 2002. Regarding the limited positive effects on outcomes of newborns, it is worth mentioning that Uruguay is already a high-income country—part of the Organisation for the Co-operation and Economic Development (OECD)—, with a relatively high level of economic and social development (Economic Commission for Latin America and the Caribbean [Economic Commission for Latin America and the Caribbean], 2014). Therefore, increases and gains in magnitudes like birth weight might not be as impressive as might be in other countries starting off from a much lower position. There is evidence of other policies in the country that contributed to increase prenatal care visits but which did not result in an increase of birth weight (Balsa & Triunfo, 2015), so it is not unusual to have found effects on some magnitudes although not on others.

There is a last additional issue to be mentioned. First, the effects of the decriminalisation of abortion may go beyond those reported here. Given that unsafe abortion is considered one of the main risk factors for maternal mortality, it is reasonable to expect that legalising abortion should have contributed to a reduction in unsafe illegal abortions. Although there is no hard empirical evidence on this issue, according to the Uruguayan Ministry of Public Health, there were only two maternal deaths resulted from abortion practices during the first two years the reform, and both cases were linked to illegal abortions (Quian, 2015).

III.6. Conclusion

A groundbreaking abortion reform for Latin America and the Caribbean, allowing for the voluntary termination of pregnancy during the first 12 weeks of gestation came into force in Uruguay in late 2012. In the first two years after the introduction of the new law, the number of voluntary interruptions of pregnancy equalled 15,176 (Ministerio de Salud Pública, 2014, 2015). In 2014, this meant an abortion rate of 12 per 1,000 women aged between 15 and 45 years old, a similar level to that in countries like Spain, Portugal, and Italy (UN, 2014).

This work has explored the impact of this policy intervention on both quantitative and qualitative fertility outcomes. The main results obtained here suggest a decline in fertility associated with an 11% reduction in the number of births resultant from unplanned pregnancies among women aged between 20 to 34 years old with secondary education. Given that the quality of births in this group of females before the reform was below average, other things being equal, the decline should have helped to improve the average birth outcomes. Moreover, we have found that the reduction in births is not orthogonal to some observable birth quality outcomes, but there is a positive selection process regarding adequate prenatal control care and the Apgar score. Further research is required in order to unravel the effect of abortion on middle- and long-term socio-economic indicators of children and adult economic outcomes as well as the possible positive effects on the safety conditions of abortions practised in the country.

Appendix III

Table III.A.1. Effect of abortion legislation on the number of births (in levels)

	Total			Women aged 20–34 & secondary		
	(I)	(II)	(III)	(IV)	(V)	(VI)
Treatment	-31.139^{***}	-13.383^{*}	-13.383^{*}	-10.245^{***}	-8.257^{**}	-8.257^{*}
	(3.251)	(7.240)	(7.298)	(1.994)	(4.141)	(4.691)
Week fixed effects	✓	✓	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓	✓	✓
Group-specific linear time trend		✓	✓		✓	✓
Control for serial correlation			Until AR(4)			Until AR(4)
R^2	0.937	0.940	0.940	0.904	0.904	0.904
No. of observations	304	304	304	304	304	304

Note: Standard errors robust to heteroscedasticity and the corresponding type of serial correlation in parentheses. All the specifications include a constant and a transition variable. The group dummy is a fictitious takes the value 0 if it is a planned pregnancy and 1 if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table III.A.2. Effect of abortion legislation on the number of births (in logs) assuming different structures of auto-correlation

	Total			Women aged 20–34 & secondary			
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VI)
Treatment	−0.081**	−0.081**	−0.081**	−0.109*	−0.109*	−0.109*	−0.109*
	(0.037)	(0.039)	(0.036)	(0.055)	(0.056)	(0.058)	(0.056)
Week fixed effects	✓	✓	✓	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓	✓	✓	✓
Group-specific linear time trend		✓	✓		✓	✓	✓
Control for serial correlation	AR(1)	Until AR(8)	Until AR(12)	AR(1)	Until AR(4)	Until AR(8)	Until AR(12)
R^2	0.936	0.936	0.936	0.904	0.904	0.904	0.904
No. of observations	304	304	304	304	304	304	304

Note: Standard errors robust to heteroscedasticity and the corresponding type of serial correlation in parentheses. All the specifications include a constant and a transition variable. The group dummy is a fictitious takes the value 0 if it is a planned pregnancy and 1 if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table III.A.3. Effect of abortion legislation on qualitative birth outcomes among mothers aged 20–34 with secondary education (robustness check using aggregate data and controlling for serial correlation)

	(I)	(II)	(III)	(IV)
Treatment	0.052**	0.041**	0.022*	0.010*
	(0.028)	(0.023)	(0.009)	(0.006)
Week fixed effects	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓
Group-specific linear time trend	✓	✓	✓	✓
Control for serial correlation	Until AR(4)	Until AR(4)	Until AR(4)	Until AR(4)
R^2	0.893	0.924	0.573	0.556
No. of observations	304	304	304	304

Note: Standard errors robust to heteroscedasticity and the corresponding type of serial correlation in parentheses. All the specifications include a constant and a transition variable. The group dummy is a fictitious takes the value 0 if it is a planned pregnancy and 1 if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table III.A.4. Effect of abortion legislation on the number of births (in logs) by age and education

	(I) Total	(II) Under 20 & primary	(III)	(IV) Under 20 & secondary	(V) 20–34 & primary	(VI) 20–34 & secondary	(VII) 35 or over & primary
Treatment	−0.011	−0.014	−0.007	0.065	0.017	0.389	0.102
	(0.072)	(0.259)	(0.153)	(0.149)	(0.122)	(0.390)	(0.360)
Week fixed effects	✓	✓	✓	✓	✓	✓	✓
Group dummy	✓	✓	✓	✓	✓	✓	✓
Group-specific linear time trend	✓	✓	✓	✓	✓	✓	✓
R^2	0.881	0.717	0.807	0.723	0.604	0.648	0.642
No. of observations	304	303	304	304	304	257	247

Note: Standard errors robust to heteroscedasticity in parentheses. All the specifications include a constant and a transition variable. The group dummy is a binary variable that takes the value zero if it is a planned pregnancy and one if it is an unplanned one. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Chapter IV

Subdermal contraceptive implants and repeat teenage motherhood: Evidence from a major maternity hospital–based programme in Uruguay†

IV.1. Introduction

Uruguay was one of the first countries in Latin American and the Caribbean to undergo a demographic transition. Its total fertility rate (TFR) reached 2.9 children per woman in 1960–1965, very close to the European TFR, with a relatively high adolescent fertility rate (AFR) of 63 children per thousand female adolescents between 15 and 19 years old, considerably higher than that in Europe (37) but lower than that in the United States (82). The following decades witnessed a decoupling of the two indicators. While the TFR experienced a pronounced drop, the AFR remained stable. Since 2016, the latter has substantially declined, reaching 28 births per thousand adolescents aged 15 to 19 in 2020. This constitutes a milestone in Uruguayan demographics (Cabella et al., 2019).

Teenage motherhood is a problematic phenomenon and an important public health matter for nations of different development levels. A sizeable number of works have extensively documented its association with poor social, health and economic outcomes for both parents and children (Aizer et al., 2020; Boden et

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al., 2007; Branson & Byker, 2018; Duncan et al., 2018; Engelhardt et al., 2004; Francesconi, 2008; Hoffman & Maynard, 2008; Johansen et al., 2020; Rodríguez Vignoli et al., 2017; Varela Petito, 2004; Varela Petito et al., 2014a). Although the literature is still scarce and focused mainly on the United States, several studies show the effectiveness of expanding family planning clinics in reducing adolescent pregnancy (Branson & Byker, 2018; Kelly et al., 2020; Strupat, 2017) and expanding access to long-acting reversible contraception (LARC) (Ceni et al., 2021; Lindo & Packham, 2017; Luca et al., 2021) and short-acting methods (Ananat & Hungerman, 2012; Bailey, 2006; Bentancor & Clarke, 2017; Guldi, 2008) such as subcutaneous implants and intrauterine devices and the pill or the morning-after pill, respectively.

In addition, while the effects of abortion legalisation on adolescent fertility seem ambiguous (Angrist & Evans, 2000; Clarke & Mühlrad, 2021; Levine et al., 1999), the impact of abstinence-based education programmes is apparently null (Carr & Packham, 2017; Fox et al., 2019; Kearney & Levine, 2015b). Regarding contraceptive methods, cohort studies point out the greater efficacy, adherence and satisfaction of LARC methods than of short-acting contraception among adolescents (American College of Obstetricians and Gynecologists, 2018; Arribas-Mir et al., 2009; Diedrich et al., 2017; Raine et al., 2011; Rosenstock et al., 2012; Short et al., 2011). However, though adoption of the former has increased remarkably in the last decade, they remain much less used than the latter (Finer et al., 2012; Gomez Ponce de Leon et al., 2019; Luca et al., 2021).

This work analyses the impact of a subdermal contraceptive implant programme on repeat teenage motherhood in Uruguay from November 2015 to December 2016. The intervention was carried out by the country's largest maternity unit, which offered a subdermal implant after an obstetric event to mothers under 20 years old. This long-term birth control method consists of placement of a small flexible plastic rod (about the size of a matchstick or hairpin) under the skin in a woman's upper arm. The device releases the hormone progestogen into the bloodstream, which prevents women from becoming pregnant. With a duration of up to five years, the effectiveness of this method is above 99%. We make use of hospital administrative data of the universe of births and employ a regression discontinuity design (RDD) that exploits the reduction in the likelihood of receiving an offer of an implant among mothers above the age cut-off of 20 years old. We find that the programme reduced mothers' probability of having another child in the next 48 months by 10 percentage points. This intention-to-treat (ITT) effect implies an approximately one-third decrease in the fertility rate of the corresponding group before the hospital implemented the programme. Our results hold in a number of robustness checks in which we consider different model specifications and placebo tests.

To the best of our knowledge, this work is the first to address the effects of this specific type of postpartum intervention. It also widens the empirical evidence, which we discuss in detail in the next section, on the effectiveness of public health interventions in curbing adolescent fertility, particularly outside high-income economies.

The rest of the chapter unfolds as follows. Section IV.2 discusses the previous literature and frames our work within it. Section IV.3 discusses our research design, including the institutional setting, data and empirical strategy. Section IV.4 presents the results of our analyses and discusses their external validity. Section IV.5 summarises and discusses the main implications of the research.

IV.2. Background and related literature

The empirical evidence regarding the effectiveness of different strategies for reducing teenage motherhood is ambiguous. The outcomes of the strategies vary by both the type of programme/birth control method and the context of implementation. Several studies (Ananat & Hungerman, 2012; Bailey, 2006; Guldi, 2008) examine the effects of the availability of the first contraceptive pill (Enovid) in the US, authorised in 1960 by the Food and Drug Administration (FDA). The pill was revolutionary not only because of its greater effectiveness compared to that of other existent contraceptive methods but also because it prevented conception from the moment of intercourse. The studies find that the pill's availability had a negative impact on young women's fertility in the United States (Ananat & Hungerman, 2012; Bailey, 2006; Guldi, 2008).

The evidence on the impact of abortion legalisation is mixed. The results of this literature range from findings of no effect to evidence of reductions of 13 percentage points in teenage motherhood (Angrist & Evans, 2000; Antón et al., 2018; Levine et al., 1999). A lack of consensus also applies to the relevance of laws that restrict minors' access to voluntary termination of pregnancy (Bitler & Zavodny, 2001; Joyce & Kaestner, 1996; Joyce et al., 2006; Levine, 2003; Myers & Ladd, 2020).

Several studies explore the effects of access to free emergency contraception (such as Prevent, approved by the FDA in 1997, which in most states required a prescription until 2006). Durrance (2013) and Gross et al. (2013) provide empirical evidence suggesting negligible effects on adolescent fertility and an increase in sexually transmitted diseases. In Chile, which used to have one of the most restrictive abortion laws in the world, the introduction of emergency contraception reduced teenage birth rates by 1.5 to 3 percentage points (Bentancor & Clarke, 2017). This larger effect in Chile than in countries with more liberal laws suggests a certain degree of substitution between abortion and emergency contraception.

Compared to alternatives that require user intervention (such as condoms or contraceptive pills), LARC methods—the use and accessibility of which have increased in recent years—have proved highly effective in preventing pregnancy. Several studies on the United States document sizeable reductions in teenage fertility rates—particularly in poor areas—as a result of the expansion of family planning facilities offering counselling, access to contraception and referrals to social services (Kelly et al., 2020; Lindo & Packham, 2017; Luca et al., 2021; Packham, 2017). In an experimental evaluation, Luca et al. (2021), like us, focus on repeat adolescent pregnancy. They find that a comprehensive intervention targeted at teenagers and comprising LARCs reduced repeat pregnancy by half, with one-third of this effect being due to the increase in access to these types of contraceptive methods through the programme.

Although scarcer, empirical evidence from outside high-income economies is quite supportive of these kinds of broad strategies. Recent literature confirms the success of policies that combine family planning, counselling and access to LARCs in reducing teenage fertility in South Africa (Branson & Byker, 2018)), with even intergenerational benefits detected in Indonesia (Strupat, 2017) and Ecuador (Galárraga & Harris, 2021). In contrast to such comprehensive programmes, the abstinence-based programmes created by several US states and targeted at adolescents have yielded, at best, null impacts on pregnancy and abortion rates and the prevalence of sexually transmitted diseases (Carr & Packham, 2017; Fox et al., 2019; Kearney & Levine, 2015b).

Last, the evidence of the impact of different policies on adolescent fertility in Uruguay is scarce. Antón et al. (2018) find an 8% reduction in unplanned births in the short run as a result of abortion legalisation. While these authors find no impact on teenagers, the work of Cabella and Velázquez (2022) suggests that the decriminalization of voluntary interruption of pregnancy would have reduced the adolescent birth rate by 4%. Ceni et al. (2021), using an event study and exploiting the timing of the rollout and regional differences in the availability of subcutaneous implants at state-owned hospitals, show that the theoretical possibility of accessing this contraceptive method through public health care led to an overall reduction of 3 percentage points in the birth rate of teens and women covered by this health system. This decline was notably high among adolescents and nulliparous women.

IV.3. Research design

IV.3.1. Institutional setting

The backbone of the Uruguayan health care system is a public national health insurance programme that ensures universal coverage through the combination of public and private production of services (Balsa & Triunfo, 2021). Residents can choose between the two types of providers, and 39% were covered by the former in December 2021 (MSP, 2022). Mobility between the two kinds of delivery systems is rare and subject to certain restrictions (e.g., overall, one cannot switch providers before a lapse of two years).

Uruguay included subcutaneous implants within the publicly funded basket of contraceptive methods at the end of 2014, but they became available only at hospitals in the public network. Several follow-up and satisfaction surveys show the method had a high acceptance rate, a low failure rate (2.5 cases per 1,000 users) and a high continuity rate (92%) (Aguirre et al., 2017; Tristant et al., 2019). The Ministry of Public Health initially paid around US$ 10 for each device (as part of a large batch). To date, access to the implants, including their placement, has been free for Uruguayan women. For the hospital, the cost, corresponding to the wage of a public sector specialist practitioner for the time it takes to advise the patient about the procedure and place the device, is approximately US$ 8 (Sindicato Médico del Uruguay, 2022). Therefore, at most, the overall fiscal cost of the procedure is approximately US$ 18 per unit.

Located in the capital city (Montevideo), Pereira Rossell Hospital Centre (CHPR by its Spanish acronym) is part of the network of public care centres and hosts the largest maternity unit in Uruguay. According to hospital birth records (Ministerio de Salud Pública, 2022), in 2014, it concentrated approximately 15% and 40% of births in the country and in the public system, respectively. CHPR treats mainly the low-income population. Therefore, it is unsurprising that the educational attainment of people giving birth there is substantially lower than average—in 2014, only 26% of women giving birth at the centre had completed lower secondary school, relative to the national average of 70% among new mothers overall—or that almost 25% of Uruguayan mothers aged less than 20 years old had their children at the hospital.

From 1st November 2015 to 31st December 2016, CHPR developed a free programme offering post-obstetric gestagen subdermal implant placements in the hospital's adolescent postpartum ward. This room accommodated mothers under 20 years old, but its patients could occasionally include older mothers depending on hospital needs (e.g., lack of space in other wards). The intervention aimed at providing counselling on family planning and offering implants of the contraceptive method within the first 48 hours after an abortion, childbirth or caesarean section and before hospital discharge. In 2017, the programme changed, expanding to other population groups. According to Lacerda et al. (2019), who did a follow-up survey of postpartum adolescents who opted for the implants between November 2015 and December 2016 and between January 2018 and January 2019, participants reported high levels of satisfaction (76%) and adherence to the method (97%), although only 15% declared no adverse effects (with the main reported side effects being amenorrhoea and irregular menstrual bleeding).

IV.3.2. Data

In our empirical analysis, we use administrative data from the Perinatal Information System (SIP by its Spanish acronym) (Ministerio de Salud Pública, 2022), a birth register with virtually universal coverage across Uruguay that contains detailed information on newborns, pregnancies and mothers (Diaz-Rossello, 1998; Simini, 1999; WHO, 2010). It draws data from clinical forms filled out by health care professionals and is commonly used in gynaecology and neonatology. We focus on births between 1st November 2015 and 31st December 2016, the programme's period of operation. Our analysis also considers births in the interval 1st November 2013–31st December 2014 for one of the robustness checks.

Due to the collaboration of health professionals at CHPR, we can match mothers who gave birth at the institution during the period of interest with information on subdermal implants provided by the maternity ward while the programme was running. As we argue below, receiving an implant is a policy outcome, but we use this information to discuss the strength of our identification strategy in isolating the effects on teenage fertility. Given that the target of this action was women giving birth under 20 years old, we look at a 60-month interval centred on the date of the 20th birthday.

Our main variable of interest is fertility after the eventual implant offer. Therefore, we explore how mothers who were more likely to receive an offer because they were below the age cut-off compare with those who were less likely to have access to subdermal implants as they were older than 20. We take advantage of the longitudinal nature of the database to assess the former group's probability of having another child in the next 48 months after receiving the offer of the contraceptive implant. This is the longest period that we can cover at the time of writing of this chapter. As a result, the available sample for our baseline analysis comprises 2,755 mothers. We present the main descriptive statistics of the variables considered in the empirical exercise in the appendix (Table IV.A.1).

It is worth highlighting that repeat motherhood among teenagers is common in Uruguay. Almost 44% of mothers aged less than 20 years who gave birth in 2014 (the year before the programme started) had another child in the following four years (Ministerio de Salud Pública, 2022).

IV.3.3. Empirical strategy

For our empirical strategy, we use an RDD that exploits the fact that the programme targeted women below 20 years old. As explained above, sometimes women above the age threshold were allocated into the adolescent postpartum ward, where the subdermal implant was offered. This means that some women above the cut-off point were exposed to the treatment (i.e., offered the implant) and even received the implant. Consequently, the discontinuity is not sharp, but the probability of being offered the implant is substantially higher for mothers below the age threshold.

Moreover, we cannot observe who actually was able to receive the implant. We can only verify whether a new mother belongs to the theoretical target group (i.e., if she was less than 20 years old at the time of delivery). Consequently, we can estimate an ITT effect at the local level based on the mentioned age cut-off point. Note that the installation of the subcutaneous implant does not represent the treatment but an outcome. If we abstract from removals or failures, installation of this contraceptive device, which is more than 99% effective, almost mechanically implies null fertility during the implant duration.[1]

We also do not know which women received counselling from the staff (as explained above, an ingredient of the specific intervention). In principle, even if we cannot separate the effect of the implant from that of such counselling, this issue is not problematic: it simply means that the latter is part of the programme. It is worth mentioning that contraception is widely available in the country. The Uruguayan health authorities heavily subsidise access to several methods, and the morning-after pill and abortion were legal by the time the intervention began. Subdermal implants even started to be available in the public health sectors (through primary care) at approximately the same time that the CHPR deployed the programme. Again, this does not interfere with our research design since we are not assessing the impact of the availability of implants but rather the effects of a very specific postpartum intervention comprising them. The availability of other birth control methods is indeed relevant, but it is part of the context in which the programme was implemented. In the results section, we discuss how these context-related issues might affect the external validity of our findings.

To estimate the ITT effect, we focus on mothers whose age was slightly less and slightly more than 20 years old (the cut-off point) when they gave birth, i.e., between 1st November 2015 and 31st December 2016, the period when the

[1] If we knew to whom CHPR offered an implant, we could estimate the local average treatment effect using a fuzzy RDD where the age cut-off serves as an instrumental variable for being offered an implant. Unfortunately, this information is not available.

programme ran. The main outcome of interest O_i is fertility, specifically a binary indicator equal to one if the mother gave birth to another child in the 48 months following the implant offer. The explanatory variable of primary interest is T_i, a dummy variable equal to one if the mother's age (A_i) at the time of birth was below 20 years (c).

We estimate the ITT by ordinary least squares through the following regression:

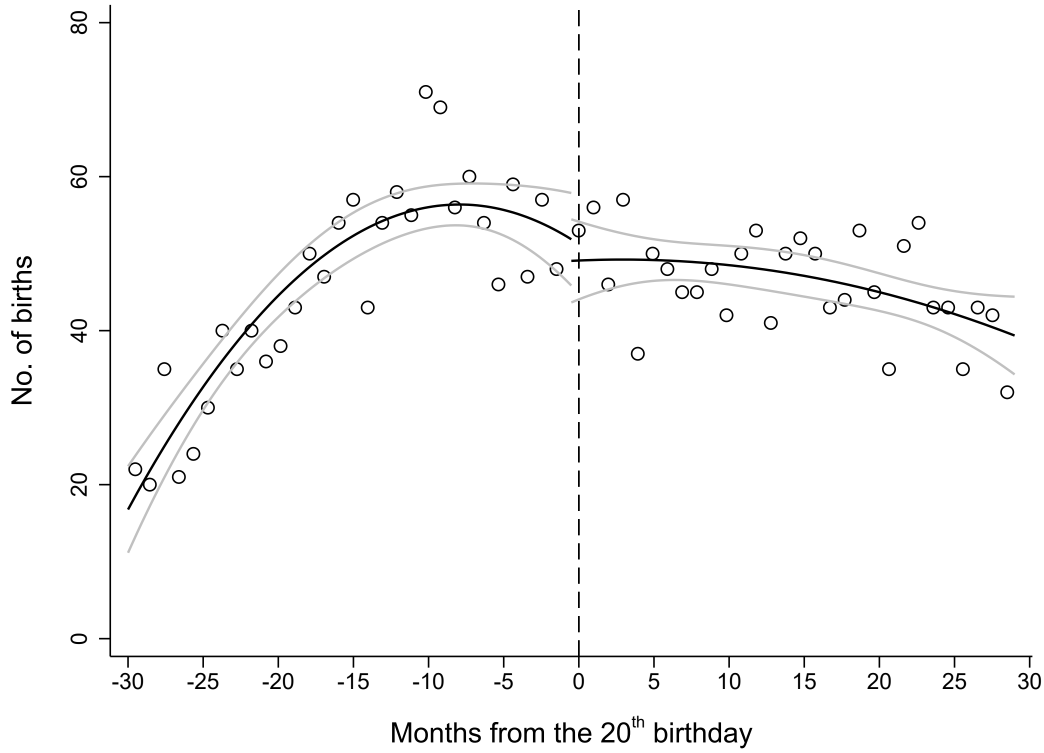
$$\begin{aligned} O_i = \beta_0 + \beta_1 T_i + \beta_2(A_i - c) + \beta_3(A_i - c)^2 + \beta_4(A_i - c) \times T_i + \\ \beta_5(A_i - c)^2 \times T_i + \Theta X_i + \varepsilon_i. \end{aligned} \tag{IV.1}$$

We allow for different quadratic time trends in age (second-order polynomials) before and after the birthday cut-off and control for a set of covariates on the mother's sociodemographic characteristics and observable birth outcomes when the implant was eventually offered (X_i). The latter include marital status, education, parity, gestation length, Apgar score at one minute, number of prenatal controls and month fixed effects. The parameter of interest is β_1, which tells us the effect of being below the cut-off point on the outcome of interest. Our main specification considers mothers who gave birth during the period when the programme was running over the 30 months before and after they reached 20 years old.

The source of identification of the policy's ITT effect is the reduction in new mothers' likelihood of receiving an implant offer after the 20-year-old cut-off is crossed. In other words, the policy application will be as good as randomised in the neighbourhood around the discontinuity threshold if the research design satisfies certain conditions. The first condition is that there must not be any manipulation of the forcing variable by mothers. This is extremely unlikely in our set-up as the programme neither was public nor benefited from advertising. Furthermore, even in the improbable case that mothers were aware of the programme, it is hard to imagine that women in the private sector would have moved to the public network to receive the implant if we bear in mind the costs of teenage motherhood and the costs of this procedure.

Regarding the first condition, if fertility sharply changed at the cut-off point, this would indicate that some sort of manipulation must be going on, with some selection process in place that threatened the validity of our research design. This is not the case here. We find that the average number of births per month does not vary around the cut-off (Figure IV.1). We also perform the density test suggested by McCrary (2008), the result of which does not allow us to reject the hypothesis that there is no shift at the 20-year-old threshold (p-value = 0.312).

Figure IV.1. Number of births per month by months from 20$^{\text{th}}$ birthday



Note: The figure shows the number of births around the cut-off of 20 years old when mothers could have received the implant offer. We estimate a quadratic separately at both sides of the cut-off. Grey lines are point-wise 90% confidence intervals.

The second condition is that there not be any correlation between an observation's being below the cut-off point and the factors affecting the outcome. We assess whether there is any discontinuity in the average values of the observable covariates through the lens of the specification outlined above. They correspond to pre-treatment characteristics since they pertain to mothers' demographic and socioeconomic characteristics and birth outcomes before the implant was offered. We do not find any evidence of a shift in these predetermined characteristics at the relevant age threshold (Table IV.1). We also carry out the permutation test suggested by Canay and Kamat (2017) and arrive at the same conclusion (p-values of

0.662, 0.397, 0.844, 0.454, 1.000, 0.657 and 0.671 for each of the seven variables in Table IV.1, respectively). Therefore, we have no reason to expect any discontinuity in the relevant unobservable factors at the cut-off.

Table IV.1. Covariate balance: Evaluation of the discontinuity in the covariates at the cut-off

	(I) Lower secondary and above	(II) Single mother	(III) Higher-order birth	(IV) Pre-term birth	(V) Apgar score at 1 min.	(VI) Birth weight	(VII) No. of prenatal controls
Intention to treat (age $<$ 20)	-0.012	0.072	0.073	0.028	0.143	24.799	-0.038
	(0.052)	(0.051)	(0.053)	(0.033)	(0.169)	(67.606)	(0.372)
Quadratic form	✓	✓	✓	✓	✓	✓	✓
Interaction	✓	✓	✓	✓	✓	✓	✓
No. of observations	2,755	2,755	2,755	2,755	2,755	2,755	2,755
Mean of dependent variable	0.317	0.285	0.372	0.263	0.107	3,180	7.886

Notes: *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. Robust standard errors are in parentheses. The model includes only mothers who gave birth between 30 months before and 30 months after their 20th birthday. All the variables refer to the time of the birth when the mother could have received the implant offer.

Moreover, we can check whether being younger than 20 affects the probability of receiving an implant. Otherwise, we should not expect any negligible nonspurious effect on fertility. Reassuringly, using the specification in equation IV.1, we find that being below the threshold raises the likelihood of receiving an implant by approximately 8 percentage points (Table IV.2).[2]

Table IV.2. Effects on the probability of receiving a subdermal implant after giving birth

	(I)	(II)	(III)
Intention to treat (age $<$ 20)	0.077**	0.074*	0.078**
	(0.039)	(0.039)	(0.039)
Quadratic form	✓	✓	✓
Interaction	✓	✓	✓
Controls		✓	✓
Month fixed effects			✓
No. of observations	2,755	2,755	2,755
Mean of dependent variable	0.157	0.157	0.157

Notes: *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. The estimated coefficients reflect the change in the probability of receiving a subdermal implant from 1st November 2015 to 31st December 2016. Robust standard errors are in parentheses. The model includes only mothers who gave birth between 30 months before and 30 months after their 20th birthday.

In the next section, jointly with the main results, we present two further analyses. First, if the programme of subdermal implants is successful in lowering fertility, it is important to investigate whether the average quality of subsequent births increases. The reduction in the number of births might not be random; i.e., subdermal implants could prevent pregnancies with potentially worse outcomes. Several studies document this mechanism in the case of abortion legalisation. To explore this issue, we focus on the quality of subsequent births. We use equation IV.1, replacing fertility (our left-hand-side variable) with different outcomes of those births that occurred after exposure to the implant offer.

We also show the results of several robustness checks. First, we compute the ITT using local linear and quadratic regression. Second, we check the sensitivity of

[2]Figure IV.A.1 in the appendix illustrates the effect of the discontinuity graphically. The p-value of the difference is below 0.05.

our findings to the use of wider and narrower time intervals around the threshold of 20 years old. Last, we assess the impact of two different placebo interventions: first, we estimate the effect of mothers' being below 20 years old one year before the programme started (1st November 2013–31st December 2014), and second, we look at the impact of mothers' being below the threshold at private hospitals (where subdermal implants were not available until several years later) during the same time window in which the programme ran at CHPR.

IV.4. Results

We present our estimation results in four steps. We first discuss the effects of mothers' being below the age cut-off (i.e., the ITT effect) on their fertility. We next focus on the impact of the programme across different groups of mothers and then investigate whether there is any selection of subsequent births on observables due to the decrease in fertility. Last, we show the results of several robustness checks that include two placebo tests.

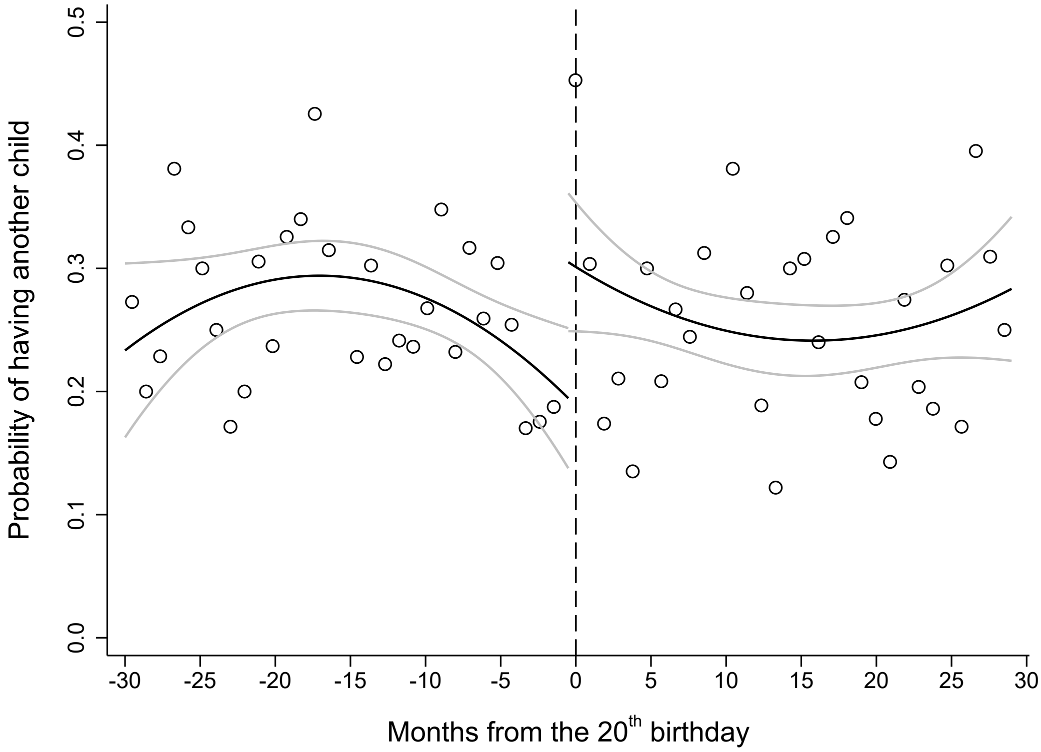
Figure IV.2, which allows for different quadratic trends before and after 20 years, suggests that the programme reduced fertility in the subsequent 48 months by approximately 10 percentage points.[3]

Table IV.3 summarises the findings of the econometric analysis, which confirm the graph's results. Column 1 corresponds to the graphical analysis, whereas the second model adds control covariates and the third includes month fixed effects. The results confirm those shown in the figure, i.e., a reduction of 10 percentage points in mothers' likelihood of having another child in the following four years after giving birth. This is a sizeable decrease in fertility. The probability of similar mothers in 2014, the year before the programme started, having another child in the next 48 months after giving birth was 32.7%. Thus, the programme cut fertility by nearly one-third.

Using seemingly unrelated regression techniques, we cannot reject that this estimate (in absolute value) and the rate of adoption of implants (shown in Table IV.1)

[3] The p-value of the difference at the cut-off is below 0.05. Note that, contrary to conventional wisdom, a statistically significant result at a certain level of difference does not require us to compute nonoverlapping confidence intervals at the same threshold (Julious, 2004; MacGregor-Fors & Payton, 2013).

Figure IV.2. Effect on mothers' probability of having another child in the next 48 months after giving birth by months from 20$^{\text{th}}$ birthday



Note: The figure shows women's probability of having another child in the next 48 months after the birth when they could have received an implant offer. We estimate a quadratic separately at both sides of the cut-off. Grey lines are point-wise 90% confidence intervals.

are statistically different. One could in principle expect that the latter would be significantly larger than the former if the implants replaced other contraceptive methods. These results would suggest that this substitution effect was negligible and that the devices themselves played a relevant role. This should not be totally surprising if we bear in mind that almost half of teenagers who gave birth a year before the programme began became pregnant again in the next four years. Another possible explanation has to do with the existence of measurement error in the register of implants (which we have to manually match to birth registers).

Last, it is possible that mothers' contact with the qualified and committed health professionals overseeing the programme might have resulted in an increased use of other contraceptive methods in their next pregnancy since these medical staff offered counselling on other contraceptive measures as well. This could particularly be the case among mothers who had certain medical conditions (diabetes, hypertension, or major depression, among others), who were on certain medications that doctors recommend not to combine with implant devices, or who did not like the devices' side effects.[4] The support from previous literature for these kinds of approaches (as highlighted in Section IV.2) and the fact that the programme's design and implementation hinged on quite high motivation levels among medical staff underscore the plausibility of this mechanism. Unfortunately, we do not have actual information on this matter (similarly to how we do not know who received an implant offer). However, although we cannot disentangle the role of the different potential channels, note that this staff contact element is an intrinsic ingredient of this specific programme.

Table IV.3. Effects on women's probability of having another child in the next 48 months after giving birth

	(I)	(II)	(III)
Intention to treat (age $<$ 20)	-0.113^{**}	-0.104^{**}	-0.102^{**}
	(0.048)	(0.048)	(0.048)
Quadratic form	✓	✓	✓
Interaction	✓	✓	✓
Controls		✓	✓
Month fixed effects			✓
No. of observations	2,755	2,755	2,755
Mean of dependent variable	0.263	0.263	0.263

Notes: *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. The estimated coefficients reflect the change in the probability of having another child 48 months after the birth when the mother could have received the implant offer. Robust standard errors are in parentheses. The model includes only mothers who gave birth between 30 months before and 30 months after their 20$^{\text{th}}$ birthday.
Source: Authors' analysis from SIP.

To study the impact of the programme across different types of mothers, we stratify the sample on several relevant dimensions measured at the time of birth. Table IV.4 displays the impact of the programme by mothers' education, marital

[4]For an example, see the guidelines of the British National Health System (2021) or the Mayo Clinic (2021).

status, parity and gestation length. The main findings of this analysis reveal that the effect is negative and statistically different from zero for mothers with low educational attainment, those with a partner at the time of birth, those who had already had children and those who had a premature birth. Although our results suggest that the programme is particularly effective in curbing fertility among mothers from disadvantaged social backgrounds, one should interpret them with caution because of the relatively low statistical power (i,e., the low number of observations in certain categories) when we split the total sample. Indeed, using seemingly unrelated regressions, we cannot reject the hypothesis of homogeneous effects across the categories of each of the four stratification variables.

Table IV.4. Heterogeneity in the effects of the programme

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
	Education		Marital status		Parity		Gestation length	
	Primary or less	Lower secondary and above	Single	With partner	First birth	Higher-order birth	Pre-term birth	Full-term birth
Intention to treat (age < 20)	−0.144**	0.006	−0.108	−0.103*	−0.050	−0.197**	−0.256*	−0.083
	(0.062)	(0.082)	(0.089)	(0.059)	(0.060)	(0.081)	(0.130)	(0.051)
Quadratic form	✓	✓	✓	✓	✓	✓	✓	✓
Interaction	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
No. of observations	1,771	786	874	1,730	1,729	1,026	294	2,461
Mean of dependent variable	0.286	0.207	0.270	0.255	0.256	0.276	0.259	0.264

Notes: *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. The estimated coefficients reflect the change in the probability of having another child 48 months after the birth when the mother could have received the implant offer. Robust standard errors are in parentheses. The model includes only mothers who gave birth between 30 months before and 30 months after their 20th birthday. All the variables refer to the time of the birth when the mother could have received the implant offer.

Table IV.5 presents the analysis of the selection effects. We look at whether the characteristics of subsequent births in the next 48 months after the mother's receipt of the implant differ from those of subsequent births to mothers above the age threshold when they gave birth. The only variable on which we detect a statistically significant effect is the share of single mothers. This outcome shows a 14-percentage-point decline. The impact is not significantly different from zero in the rest of variables that we examine, and therefore, the reduction in fertility induced by the programme will have corresponded to positive selection in subsequent births, as well. Nevertheless, one should bear in mind that the sample available for this exercise shrinks dramatically because barely a quarter of women gave birth to another child during the 48-month window.

Table IV.5. Selection effects: Programme's impact on the characteristics of subsequent births

	(I) Lower secondary and above	(II) Single mother	(III) Pre-term birth	(IV) Apgar score at 1 min.	(V) Birth weight	(VI) No. of prenatal controls
Intention to treat (age < 20)	−0.010	−0.143*	−0.032	0.090	−56.198	−0.232
	(0.077)	(0.086)	(0.065)	(0.275)	(116.843)	(0.629)
Quadratic form	✓	✓	✓	✓	✓	✓
Interaction	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓
No. of observations	842	842	842	842	842	842
Mean of dependent variable	0.195	0.195	0.106	8.494	3,239	7.545

Notes: *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. Robust standard errors are in parentheses. The model includes only mothers who gave birth between 30 months before and 30 months after their 20$^{\text{th}}$ birthday. All the variables refer to the time of the first birth, in the next 48 months, after the one when the mother could have received the implant offer.

Table IV.6 shows our robustness checks. In our baseline specification, we favour a quadratic in age since the specialised literature discourages the use of higher-degree polynomials because of their large sensitivity to the order of the polynomial and poor coverage of confidence intervals Gelman and Imbens (2019).[5] In columns (I) and (II), we assess whether our main results vary when we make use of local linear and polynomial regressions with optimal bandwidth selection (Calonico et al., 2019). The results remain qualitatively and quantitatively the same. The second sensitivity analysis involves modifying the bandwidth. Neither narrowing (column [III]) nor widening the window (column [IV]) around the cut-off alters the estimated impact of the programme in a relevant way.

Our final robustness assessment comprises two placebo tests (columns [V] and [VI]). The first one focuses on a similar time window but one year earlier (i.e., we look at the effect among mothers who gave birth when they were less than 20 years old at CHPR between 1st November 2013 and 31st December 2014). The second test focuses on mothers who gave birth at private hospitals—where subdermal implants were not available—during the period when the programme operated at CHPR. Reassuringly, neither of the estimated coefficients for the associated placebo policies is significantly different from zero.

[5] In any case, the use of a cubic and a quartic function also indicates a negative effect on fertility, specifically, a reduction of 14.2 and 21.1 percentage points, respectively.

Table IV.6. Robustness checks

	(I) Local linear regression	(II) Local quadratic regression	(III) 48-month interval	(IV) 72-month interval	(V) Placebo 1: previous year	(VI) Placebo 2: private hospitals
Intention to treat (age $<$ 20)	-0.091^{**}	-0.113^{**}	-0.119^{**}	-0.086^{*}	-0.013	-0.038
	(0.048)	(0.048)	(0.053)	(0.044)	(0.051)	(0.040)
Quadratic form			✓	✓	✓	✓
Local linear form	✓					
Local quadratic form		✓				
Controls	✓	✓	✓	✓	✓	✓
Interaction	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓
No. of observations	7,211	2,981	2,365	3,102	3,027	4,330
Mean of dependent variable	0.214	0.214	0.261	0.258	0.362	0.255

Notes: *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. The estimated coefficients reflect the change in the probability of having another child 48 months after the birth when the mother could have received the implant offer or the timing of the placebo intervention. Robust standard errors are in parentheses. The number of observations effectively used in the estimators for columns (I) and (II) are 1,712 and 2,981, respectively.

We conclude this section by commenting on possible external validity concerns. First, using an RDD implies that we can estimate a local ITT effect. Nevertheless, the relevance of the group affected by the discontinuity (teenagers) ensures the pertinence of the analysis. A second potential issue is that the programme seeks to prevent repeat teenage pregnancy. This is not a rare phenomenon—as mentioned above, before the programme began, approximately one-third of the mothers around the discontinuity threshold had another child in the following four years—but it certainly does not represent the whole universe of adolescent births. Nevertheless, even if these mothers already have children, repeat teenage pregnancy is still a quantitatively important issue that has been specifically explored in previous research elsewhere (Luca et al., 2021).

Moreover, there may be specific barriers to realizing the potential benefits of subdermal implants for other groups (nulliparous women). For instance, adolescents from disadvantaged social backgrounds rarely use preventive services where doctors could offer the method, and they might not be easy to reach using informational campaigns. However, authorities might consider monetary incentives (Heil et al., 2012) given the high burden from unintended births, which are very common among teenage mothers (Bearak et al., 2022), and doctors could advise them to attend preventive check-ups where they would be offered the implant.

Third, our findings concern a single hospital centre; nevertheless, CHPR is the largest maternity hospital in the country and mostly attends to the population groups in which teenage pregnancy is most prevalent. Specifically, this centre concentrates 24% of total teenage births and 40% of those births at public hospitals in the country (50% and 77% in Montevideo, the capital city, respectively).

Fourth, the impact of the intervention in places where repeat adolescent motherhood is less frequent than in Uruguay should be smaller. Conversely, in countries where contraception is not as widespread or pregnancy termination or the morning-after pill are not available, the impact of this type of intervention could be even larger. In this respect, we would expect that the transferability of the policy to other Latin American and Caribbean countries (where access to contraception and abortion is overall much lower than in Uruguay) is potentially high.

Finally, it is difficult to conjecture what the impact of this intervention would be if it were implemented in the private sector. On the one hand, it is possible that adolescent mothers from upper middle- and upper-class socioeconomic backgrounds (the main users of private health care) would be more likely to accept the implant. Furthermore, this population segment, on average, exhibits a lower fertility rate. On the other hand, the fact that it is quite likely that teenagers from better-off families who decide to carry a pregnancy to term are strongly selected (in that they not only avoided the use of contraception but also ruled out abortion), so they could be more reluctant to adopt birth control methods of any kind (e.g., because of strong religious motivations). Therefore, the likely impact of implementing this programme in the private sector is quite unclear.

IV.5. Conclusion

Teenage motherhood is a societal challenge due to the costs that it imposes on both parents and children, and it affects both low- and high-income regions. This work analyses the impact of a subdermal contraceptive programme on repeat teenage motherhood in Uruguay. Using an RDD, we find that mothers experienced a 10% reduction in their probability of having another child in next 48 months after the implant offer. This figure is extremely close to the findings of the randomised trial of Luca et al. (2021) in both qualitative and quantitative terms.

Our results suggest that the programme is cost-effective. We do not know the exact amount of resources invested in the intervention, but the approximate figures provided in Subsection **??** indicate that the overall cost is around US$ 18 per unit. This figure is dwarfed by the high burden of unintended pregnancies. Although no figures for Uruguay are available, based on the cost of a live birth in Chile and some assumptions drawn from studies for the United States, we estimate the cost of an unintended pregnancy that includes prenatal and postnatal care until 60 months after delivery to amount to US$ 2,707.[6]

[6] Also located in the Southern Cone, Chile exhibits a level of economic development, medical technology and quality of public health care similar to Uruguay's. The public health care system in Chile resorts to modern forms of management such as quasi-market arrangements and subcontracting. This results in much better availability of information on medical costs in Chile than in Uruguay. Specifically, the cost of a delivery, including hospitalization, in Chile in 2016 was US$ 1,510 (Fondo Nacional de Salud, 2023). Our estimation relies on the following assumptions. First, the cost of a live birth in Chile is 3.5 times lower than that in the United States (Trussell et al., 2013). We assume that this proportion holds for the total cost including prenatal and post-

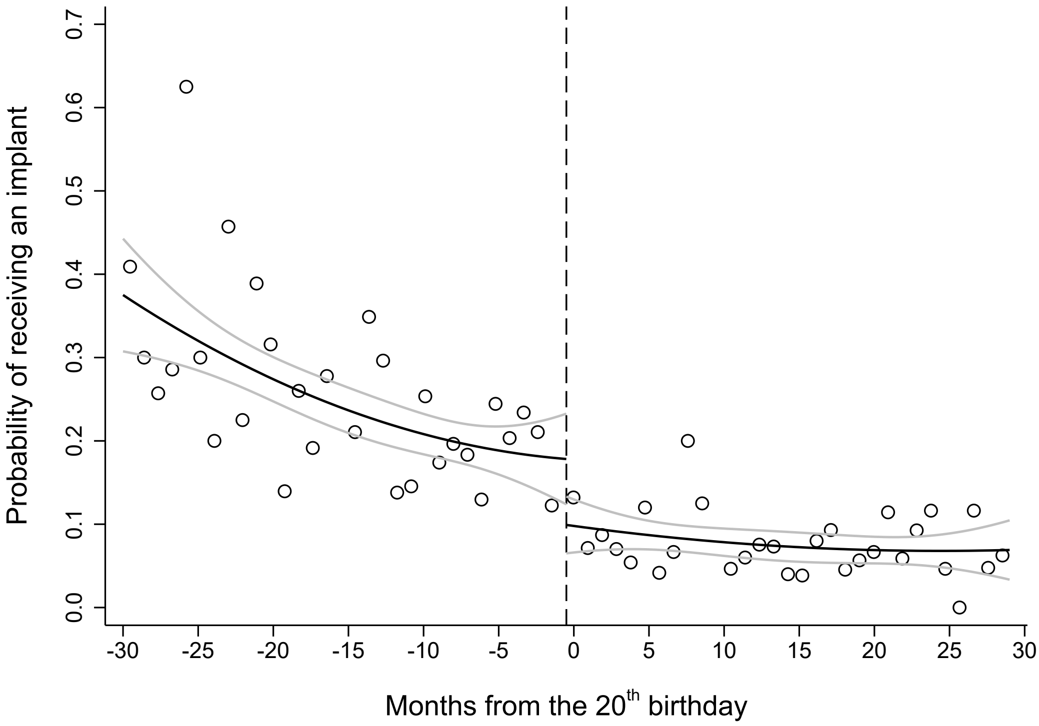
Our study has several limitations. First, we cannot totally disentangle the impact of the subdermal implants from the other possible mechanisms through which the programme could have affected fertility, such as better information on other contraceptive methods. Nevertheless, when we compare the estimated change in fertility to the estimated rate of implant adoption, we cannot rule out that the effects of the programme could be partly attributable to the latter. Second, our research does not address the impact on sexually transmitted diseases. It is possible that part of the adopters switched to implants from barrier contraceptive methods, which could lead to a rise in these types of infections.

Third, further research should address the medium- and long-run impact of the programme on outcomes such as educational attainment or positive spillovers on other younger children in the family due to the reduction in fertility. Fourth, the benefits of the programme identified by this work do not cover the likely increased well-being associated with subdermal implants relative to that associated with other methods. Last, it is still unclear whether this method the most cost-effective contraception measure, particularly relative to intrauterine devices (Farah et al., 2022; Guerra et al., 2015; Henry et al., 2014; Mavranezouli, 2008; Ngacha & Ayah, 2022; Trussell et al., 2013). This judgement critically depends on adherence rates, which could well be country specific.

natal care. Second, the share of the total costs (comprising delivery and prenatal and postnatal care) in Chile is the same as in the United States (taken from Sonfield and Kost [2015]. Third, we adjust for mistimed pregnancy (many unintended births often just come earlier than desired) using the same factor as [Trussell et al., 2013] for females aged 18–19 years old in the United States. Note that the final figure probably seriously underestimates the total cost of unintended pregnancy because it excludes private and external costs due to the impact of teenage birth on mothers and children's socioeconomic outcomes (e.g., those associated with the negative impact on mothers' educational attainment).)

Appendix IV

Figure IV.A.1. Effect on women's probability of receiving an implant after giving birth by months from 20th birthday



Note: The figure shows women's probability of receiving an implant after giving birth from 1st November 2015 to 31st December 2016. We estimate a quadratic separately at both sides of the cut-off. Grey lines are point-wise 90% confidence intervals. *Source*: Authors' analysis from SIP.

Table IV.A.1. Descriptive statistics

	Mean	Standard deviation
Implant after birth	0.157	0.364
Another child in the next 48 months	0.263	0.440
Age	20.066	1.315
Single	0.317	0.465
Missing marital status	0.055	0.228
Lower secondary or above	0.285	0.452
Missing education	0.072	0.258
Previous births	0.372	0.484
Pre-term birth	0.107	0.309
Apgar score at 1 minute	8.366	1.430
Birth weight (g)	3,180.248	602.969
No. of prenatal controls	7.886	3.256
No. of observations	2,755	

Chapter V

Immigrant assimilation in health care utilisation in Spain†

V.1. Introduction

Migrants are common in societies of many different development levels (International Organization for Migration [IOM], 2019). Migrants' overall success largely depends on their assimilation into the host society, including in how they access and use social services. Although immigration exerts overall beneficial effects on destination countries (International Monetary Fund [IMF], 2020, Chapter 4), its impact on the sustainability of the welfare state is a subject of debate. These effects most likely depend on migrants' characteristics, the specific features of welfare states, and the time horizon used to measure service use (Barrett & McCarthy, 2008; Christl et al., 2022; Giulietti, 2014; Hansen et al., 2017; Nannestad, 2007). In turn, as long as migration increases the ethnic heterogeneity of societies, it can also shape natives' attitudes towards resource redistribution and the welfare state itself (Alesina et al., 2021b, 2021a; Dahlberg et al., 2012; Facchini et al., 2016).

This chapter explores the patterns of migrants' assimilation into health care in Spain. In particularly, we evaluate how length of residence affects migrants' use of this service, considered the cornerstone of the Spanish welfare state. We also explore the extent of convergence in health care utilization patterns between

†This chapter is a joint work with and Patricia Triunfo and José-Ignacio Antón. This work benefited from comments of Rodrigo Ceni, Rafael Grande, Rafael Muñoz de Bustillo and participants at the 41st Health Economics Association Conference on a first draft.

native and migrant populations, in terms of both assimilation and differential ageing processes.

To pursue these goals, we employ different econometric models to exploit four Spanish health surveys (conducted from 2010 to 2020). These surveys contain information on respondents' time of residence in the country and include more than 80,000 natives and nearly 8,000 foreign-born individuals. This setup allows us to identify cohort, period, and assimilation effects and to carry out separate analysis by gender. Our findings suggest relevant migrant cohort effects in many services, i.e., foreign-born populations use some types of health care less than natives in their first years in the country. These results are consistent with both the healthy migrant effect and the persistence of information problems and language barriers.

Assimilation—in this context, the increase in health care utilization with the number of years in the host country—is only relevant for visits to general practitioners (GPs) by female migrants. We do not observe large differences by region of origin, although the statistical power of our analyses substantially decreases when examining different groups of migrants separately. Though the effect is modest, we argue that this outcome might be due to partial health assimilation and limited socio-economic progress, given that the use of health care services in Spain is higher among individuals with high educational attainment.

Even if migrants use less health care at arrival and their assimilation is quite limited, the age patterns of their utilisation are different than among locals. As a result, after 20 years in Spain, migrants tend to exhibit similar levels of effective recourse to health services as the native population, with a few exceptions. Particularly, migrant women visit GPs more often than natives after 10 years and some cohorts of foreign-born females make higher use of emergency care.

Overall, our results suggest that the differences between comparable native and migrant populations in terms of health care utilisation are relatively minor when the latter arrive to the country, and that after some time, their behaviour becomes considerably similar. Nevertheless, we identify some services where health consumption seems higher among foreign-born women. We argue that this result might indicate a modest extra cost in terms of social spending due to migration. Further simulations assessing the costs and benefits of migration in Spain might profit from these findings.

To our knowledge, this study is the first to explore immigrant assimilation based on health care use in Spain. Whereas several papers provide evidence on how foreign-born populations exhibit better health than locals on arrival, how this gap tends to close with time of residence in the country (Rivera et al., 2013, 2015), and how foreign-born populations exhibit similar or lower health service utilization (see, e.g., the surveys of Llop-Gironés et al. [2014], Norredam et al. [2009] and Sarría-Santamera et al. [2016], among many others), no previous work addresses this question linked to the dynamics of health care use.

Whereas the literature on immigrant health (Antecol & Bedard, 2015; Giuntella & Stella, 2016; Hamilton et al., 2011) and specially labour market assimilation (Abramitzky et al., 2020; Bodvarsson & Van der Berg, 2013; Lalonde & Topel, 1997) is abundant, few studies consider how patterns of immigrant health care use evolve with time of residence in the host country. Studies focused on the U.S. indicate no difference in take-up rates of Medicaid means-tested benefits between natives and migrants, whose participation rises with the time spend in the country (Borjas & Hilton, 1996), as well as higher initial health expenditures among Latino migrants than locals, with certain evidence of convergence for Latino migrants who get American citizenship (Vargas Bustamante & Chen, 2011).

It is also worth summarising the findings from countries providing universal access to health care. Even though, overall, these studies suggest a lower use of non-emergency health care services by migrants (vs. natives) on arrival and a certain catch-up process, this literature is not unanimous. For instance, migrant males in Canada less frequently contact a doctor on arrival, but their levels of health care usage rise to match natives after 6–8 years in the country (McDonald & Kennedy, 2004). According to Wadsworth (2013), the differences in health care use between migrants and natives in the United Kingdom and Germany are not large. In the U.K., foreigners make a little more use of GP services (but not hospitals) than natives. Their change in usage with time spent in the country does not follow a systematic pattern. In the case of Germany, if anything, migrants exhibit lower rates of utilisation of health care (both general practitioners and hospital services), but their rates seem to converge with those of natives with time of residence in the country. Finally, migrants' usage of primary care emergency services in Norway exhibits substantial variation over groups and is higher than natives' usage (perhaps at the expense of less effective access to other types of health care), and it decreases with length of residence in the country (Småland Goth & Godager, 2012).[1]

The remainder of the chapter unfolds as follows. The second section provides some theoretical and institutional background for the analysis, while the third and fourth sections describe the database and methods employed in the analysis, respectively. Section 5 presents the empirical results and discusses their implications. Finally, in Section 6 we offer some concluding remarks and pathways from future research.

V.2. Background

Like most developed countries, Spain provides virtually universal health care coverage to every resident in the country. National authorities extended this entitlement in 2000 to undocumented migrants, with the sole requirement being registration in a local population census (with no legal consequences). Nevertheless, in practical terms, information problems or fear of retaliation due to their irregular status, jointly with the hesitancy of some regional authorities (who are responsible for health care delivery) to provide foreigners without legal residence in Spain with health cards, could hamper actual access to the National Health System (NHS) among some segments of migrants.

In September 2012, in the middle of the Great Recession, the Spanish government restricted the access of undocumented foreigners to primary care (with the exceptions of minors, pregnant women, and anyone who needed emergency care). The reform still allowed the affected groups to purchase insurance for a monthly fee of 60 € (individuals below 65) and 157 € (individuals above 65). Some regional governments also promptly passed legislation to protect the affected populations and provide more beneficial insurance conditions, close to those before the reform. These restrictions might have resulted in less actual access to these services and

[1] Schober and Zocher (2022), who only consider asylum-seekers (a very specific subset of the foreign-born population) and evaluate them five years after arrival, find that the effect of time of residence in Austria on health expenditures is negative for refugees and not clear for economic migrants: the evolution of the results for stayers is different than for the rest of the foreign-born population in this group.

worse health outcomes (Jiménez-Rubio & Vall-Castelló, 2020; Juanmarti Mestres et al., 2021).

The change in Spain's central government in 2018, after a motion of no confidence, led to the restoration of the situation prior to the limitations on migrants' health care access. In practice, similar obstacles apparently persist because of legal loopholes (Villarreal, 2019). Bruquetas-Callejo and Perna (2020) even argue that migrants' entitlement to health care in Spain has been more of a political talking point than a subject with substantial differences between the two main political parties.

Previous literature on the theoretical reasons for expecting assimilation (or non-assimilation) in health care is scant. A first reason that increased use of health services may correspond with years living in the host country, particularly relevant in contexts without universal coverage, is the higher probability of improving health care access with longer residence in a region (Antecol & Bedard, 2015; McDonald & Kennedy, 2004; Vargas Bustamante & Chen, 2011). A second pertinent argument has to do with the existence of the "healthy migrant effect" and a process of negative assimilation in health, documented by recent studies focused on Spain (Rivera et al., 2013, 2015). Similarly, during their first years in the country, migrants might face cultural and even linguistic barriers (Pena Díaz, 2016), whose relevance should decrease with their time spent in the country. A related argument refers to the role of information and knowledge on the functioning of health care systems, again likely to increase with time of residence in the host state (Devillanova, 2008). This issue could have both a negative effect on visits to GPs on arrival and a positive one on the use of emergency care (Småland Goth & Berg, 2010; Småland Goth et al., 2010). The existence of assimilation in this area is mostly an empirical question, since there are other factors that might act in the opposite direction. For instance, keeping in mind previous studies on the impact of income on health care demand, a non-linear but overall negative trend (Antón & Muñoz de Bustillo, 2010), assimilation of migrants in this domain might mitigate the the increase in health care use—see, among many others, the survey of Antecol and Bedard (2015).

V.3. Data

Our analysis makes use of the National Health Survey (NHS), waves 2011–2012 and 2017 (Spanish Statistical Office [Spanish Statistical Office], 2022b), and the European Health Interview Survey (EHIS), waves 2014 and 2020 (Ministry of Health, 2022), administered by the SSO. These databases are the first health surveys in Spain that include precise information on the timing of immigrants' arrival in the country. The EHIS began much later than the NHS (whose first wave corresponds to 1987), but the EHIS is designed to be fully comparable to the NHS. Consequently, local authorities have discontinued the NHS, which they carried out roughly each three years until 2014. Both sources are representative of the resident population in the country aged at least 15 years old, at the regional level. Each wave includes approximately 24,000 households and follows a three-stage stratified sampling design, since it only selects one adult person from each household—households are randomly chosen from each census section—for an interview. Apart from basic socio-demographic characteristics, the questionnaires in both types of surveys cover detailed and comparable self-reported information on health status and health care utilisation. The main differences between the two sources is that the earlier questionnaire includes additional items on quality of life (e.g., information on social support) and additionally interviews a minor living each household. Hereafter, we refer to both questionnaires as national health surveys (NHSs).

For the purpose of this investigation, we pool the samples of adults in the four waves mentioned above. We identify migrant status by looking at the country of birth rather than citizenship, because naturalization processes in Spain differ widely by state of origin (e.g., they are much shorter for people from some Latin American and Caribbean countries). Regarding health care use, we focus on the following items related to health services use: number of visits to general practitioners (GPs) in the last four weeks, number of visits to specialist doctors in the last four weeks, number of hospitalisations in the last 12 months, and number of times the person used emergency care in the last 12 months.

The resulting sample, after dropping the observations with missing values on any of the variables included in the analysis (1.1% of cases), comprises 80,122 native and 7,807 migrant adults. Using survey weights, the latter group represents

13.8% of the sample. Thanks to ad hoc agreements with the Spanish institutions responsible for granting access to the data (the SSO and the Ministry of Health, respectively), we are able to distinguish among foreign-born individuals from different regions of origin. The most relevant groups in demographic terms are migrants from Latin America and the Caribbean (43.4% of all the foreign-born adult population), those from European countries other than the European Union 15 (EU15) countries (19.7%), those from Africa (17.5%), and those from EU15 countries (13.6%). In contrast to other countries like Sweden or Germany, where refugees represent an important part of of the foreign population, the motivations for immigration to Spain are overwhelmingly economic, with the exception of EU15 migrants, who are mainly attracted by the benevolent climate and the lower cost of living than their countries of origin (Spanish Statistical Office, 2022a).[2]

We show the main summary statistics of the sample in Table V.1. It includes both the variables used in our analyses of health care use assimilation and those considered for exploring the channels through which such a process takes place (several health outcomes like self-perceived health, overweight status, and mental health problems).

[2] There is a small share of migrants from the Southern Cone of Latin America who immigrated to Spain for political reasons in the 1980s (Muñoz de Bustillo & Antón, 2010).

Table V.1. Descriptive statistics

	Means (standard deviations) Natives	Migrants
No. of visits to a GP (last four weeks)	0.361	0.299
	(0.767)	(0.637)
No. of visits to a specialist (last four weeks)	0.137	0.100
	(0.546)	(0.445)
No. of hospitalisations (last year)	0.131	0.120
	(1.438)	(1.661)
No. of visits to emergency care	0.471	0.509
	(1.403)	(1.267)
Good or very good health (last year)	0.715	0.770
	(0.452)	(0.421)
Overweight (body mass index $\geq$ 25)	0.529	0.500
	(0.499)	(0.500)
Mental health problems (last year)	0.118	0.075
	(0.322)	(0.264)
Female	0.505	0.535
	(0.500)	(0.499)
Age	49.480	40.321
	(19.173)	(14.504)
Married	0.575	0.565
	(0.494)	(0.496)
Low education	0.619	0.507
	(0.486)	(0.500)
Medium education	0.196	0.325
	(0.397)	(0.468)
High education	0.185	0.168
	(0.389)	(0.374)
Employed	0.452	0.533
	(0.498)	(0.499)
Unemployed	0.116	0.200
	(0.320)	(0.400)
Inactive	0.433	0.266
	(0.495)	(0.442)
Arrived before 1996		0.133
		(0.339)
Arrived between 1996 and 2007		0.623
		(0.485)
Arrived after 2007		0.244
		(0.430)
Less than 5 years since migration		0.131
		(0.338)
5–9 years since migration		0.245
		(0.430)
10 or more years since migration		0.624
		(0.484)
No. of observations	80,122	7,807

Note: The number of observations is lower in the case of overweight (82,856) and mental health problems (87,902).

V.4. Methods

In order to disentangle the effect of foreigners' length of residence on their patterns of health care use, we adopt the empirical strategy utilized by Antecol and Bedard (2006, 2015) and Giuntella (2016) for analysing health assimilation. Specifically, we estimate equations of the following form:

$$Y_i = X_i\beta + A_i\delta + C_i\gamma + T_i\pi + \varepsilon_i, \tag{V.1}$$

where Y_i denotes a health care variable relevant to person i, X_i a vector of socio-demographic control variables (region, degree of urbanisation, and a cubic of age, fully interacted with migrant status, education, and marital and activity status.), A_i a vector of dummy variables indicating how long an immigrant has lived in Spain (set equal to 0 for locals and excluding a category that serves as reference), C_i a vector of dummy variables identifying the arrival cohort (which takes the value 0 for natives), T_i a vector of dummy variables capturing the survey year, and ε_i a random disturbance.

We estimate equation V.1 by Ordinary Least Squares (OLS) in our baseline analysis. We are not interested in prediction but in the marginal effects of the dummies due to cohort and assimilation variables. In this respect, OLS estimates are consistent under weaker assumptions than others obtained from count data models (Angrist & Pischke, 2008). In any case, in Subsection V.5.5, devoted to robustness checks, we present the results obtained using count data models.

As there are relevant differences in health and health care use by gender, we estimate the equation of interest separately for men and women. As in the case of health status (Antecol & Bedard, 2015; Giuntella & Lonsky, 2022), ethnicity might also play a role in health services utilisation. Whereas most of the population born in Spain is white, there is a considerable heterogeneity in the ethnic composition of migrant adults. This is due to the relevance of migration from Latin America, the Caribbean, and Africa. Previous research has identified relevant differences in the patterns of migrant health care use by state of origin (Llop-Gironés et al., 2014). For this reason, we additionally re-estimate our model comparing locals with migrants from the EU15, the rest of Europe, Latin America, and the Caribbean and Africa. Unfortunately, considering these groups separately largely reduces the available samples, which makes the estimates quite imprecise.

Pooling several cross-sections and both migrants and natives allows us to separately identify cohort, assimilation, and period effects. We consider three arrival cohorts based on Spain's recent economic and social conditions: before 1996 (when massive immigration began), 1996–2007 (a period of strong economic growth before the financial crisis), and 2008–2020 (just after the start of the Great Recession). In order to study assimilation, we take into account three intervals of length of stay in Spain: 0–4, 5–9, and 10 or more years. In order to identify the model, we omit the first category of time of residence. The coefficients due to arrival cohort indicate the differences in health care utilisation between migrants and natives at 0–4 years since migration. Those associated with the two dummies of length of stay in the country (5–9 and 10 or more years) capture the change in health care use for migrants with time spent in Spain. Combining the coefficients of the interaction between age and migrant status, the arrival cohort, and the time since arrival allow us to calculate how the migrant-native gap in health care use evolves over time. We can identify the period effect thanks to the inclusion of natives in the sample.

In principle, the coefficients of the binary variables due to migrant cohorts would indicate the gap in health care demand between locals and migrants on arrival at 0 years old. Therefore, to make the interpretation of the results easier, we centre age at 15 years old (the lowest age at which we can observe individuals in our database), so those parameters capture the difference between foreign-born individuals at 0–4 years since migration and native population at that age.

We explicitly refrain from introducing variables controlling for health status in the left-hand side of equation, as those sort of variables are jointly determined with utilisation of health care services, and both are part of the process of immigrants' assimilation to their host countries. In order to explore the potential role of these factors in shaping the use patterns of health services, we further estimate the role of immigrant assimilation in a set of health outcomes (self-reported health status, overweight status, and prevalence of mental health problems).

Selection of return migration represents a potential threat to identification. For instance, if foreign-born populations who go back to their country of origin have worse health (and higher demand for health care) than stayers, the estimated coefficients would be inconsistent (downward biased). Regrettably, although return migration became a very relevant phenomenon during the Great Recession and its aftermath (Izquierdo et al., 2016; Larramona, 2013), there is little available evidence on the relationship between this phenomenon and health status for Spain. A survey of studies by Antecol and Bedard (2015), focused on other countries, suggests mixed results, so the experience of other societies does not provide a clear guide here. If, as shown by Abramitzky et al. (2014) in the case of the labour market, negative self-selection of return migration were the norm, our estimates of assimilation would be a lower bound for the actual effect of the number of years spent by migrants in Spain.

V.5. Results

V.5.1. Main results

We display the main results of the econometric analysis in Table V.2, which shows the estimates due to visits to GPs and specialists, and V.3, which refers to hospital admissions and emergency care. Regarding the number of visits to GPs, we observe no difference in the frequency of use between locals and migrants on arrival in the case of males. Nevertheless, we observe that two cohorts of foreign-born women make a less intense use of health services that their native counterparts. The length of residence in Spain does not affect the utilisation of this service in the case of foreign-born men, but it increases after 10 years in Spain by 0.102 visits among migrant women. The size of this assimilation effect is not negligible: it represents nearly a quarter of the average number of visits to GPs. With regards to contacts with specialist doctors, assimilation is absent among both sexes, Nevertheless, it is worth mentioning that, on arrival, the female migrant cohort arriving between 2008 and 2020 exhibited a lower rate of utilisation of this service than natives. This effect (0.057 visits less) constitutes approximately one-third of the average number of contacts with specialists in Spain.

Table V.3 shows the results for hospitalisations and emergency care. They reveal no difference between male migrants on arrival and locals for the former variable, but all the cohorts of foreign-born women exhibit substantially higher hospital admissions than comparable natives.[3] Also, on arrival, two cohorts of migrant women exhibit a lower use emergency services than female natives. We do not observe any cohort or assimilation effects.

[3]This result probably reflects the fact that migrants' fertility rates on arrival are higher than natives' rates (Spain has one of the lowest native fertility rates in the world), correlated with the ones observed in their country of origin and decreasing with time spent in Spain (Grande & Del Rey, 2017).

Table V.2. Age, immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.025***	0.000	0.018***	0.005
	(0.006)	(0.006)	(0.005)	(0.003)
$Age^2/100$	−0.038***	0.008	−0.029***	−0.004
	(0.012)	(0.013)	(0.010)	(0.006)
Age^3 /10000	0.024***	−0.004	0.014**	0.000
	(0.008)	(0.008)	(0.006)	(0.004)
Age × migrant	−0.003	0.029**	−0.005	0.006
	(0.013)	(0.013)	(0.007)	(0.007)
Age/100 × migrant	0.003	−0.061**	0.010	−0.014
	(0.031)	(0.031)	(0.016)	(0.016)
Age/10,000 × migrant	−0.001	0.035	−0.006	0.009
	(0.022)	(0.022)	(0.011)	(0.011)
Immigrant arrival cohort				
Pre–1996	0.022	−0.145*	0.022	−0.086
	(0.060)	(0.075)	(0.039)	(0.061)
1996–2007	0.051	−0.091	0.024	−0.073
	(0.054)	(0.062)	(0.035)	(0.052)
2008–2020	0.029	−0.123**	−0.007	−0.067**
	(0.046)	(0.055)	(0.030)	(0.033)
Time of residence in Spain				
5–9 years	−0.048	0.018	−0.014	−0.014
	(0.039)	(0.043)	(0.024)	(0.036)
10 or more years	−0.022	0.102**	−0.008	0.054
	(0.041)	(0.046)	(0.028)	(0.045)
Adjusted R^2	0.052	0.032	0.031	0.033
No. of observations	40,936	46,993	40,936	46,993
Mean of dependent variable	0.293	0.410	0.107	0.156

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.3. Age, immigrant arrival cohort and assimilation effects (OLS) in hospital stays and visits to emergency care

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.043**	0.031	0.059***	0.032**
	(0.017)	(0.019)	(0.010)	(0.014)
$Age^2/100$	−0.081**	−0.056	−0.130***	−0.087***
	(0.033)	(0.037)	(0.020)	(0.027)
Age^3 /10000	0.049***	0.032	0.086***	0.062***
	(0.019)	(0.021)	(0.011)	(0.016)
Age × migrant	−0.042	−0.009	−0.030	0.057**
	(0.038)	(0.016)	(0.024)	(0.023)
Age/100 × migrant	0.110	0.003	0.067	−0.115**
	(0.099)	(0.042)	(0.054)	(0.049)
Age/10,000 × migrant	−0.080	0.004	−0.051	0.069**
	(0.072)	(0.031)	(0.037)	(0.032)
Immigrant arrival cohort				
Pre–1996	0.173	0.243***	0.190	−0.302*
	(0.182)	(0.090)	(0.142)	(0.166)
1996–2007	0.240	0.170**	0.115	−0.185
	(0.233)	(0.072)	(0.108)	(0.152)
2008–2020	0.230	0.116**	0.035	−0.240**
	(0.261)	(0.053)	(0.092)	(0.122)
Time of residence in Spain				
5–9 years	−0.170	−0.032	−0.001	−0.071
	(0.221)	(0.034)	(0.070)	(0.102)
10 or more years	−0.232	−0.007	0.050	0.062
	(0.239)	(0.067)	(0.078)	(0.109)
Adjusted R^2	0.007	0.001	0.012	0.014
No. of observations	40,936	46,993	40,936	46,993
Mean of dependent variable	0.118	0.142	0.403	0.548

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.
Source: Authors' analysis from national health surveys.

V.5.2. Heterogeneity by origin

In this subsection, we explore how the results on assimilation to health care differ by country of origin. Specifically, we look at foreign-born populations in the four most relevant groups of migrants: EU15, other European countries, Latin America, and the Caribbean and Africa.[4] Because of sample size limitations, in many cases the estimated coefficients are not statistically different from zero, though they are not statistically different from our main results either. Therefore, we reproduce those results in the supplementary appendix and we comment on the main salient findings here.

First, regarding EU15-born individuals (Table V.A.1 and V.A.2), the most interesting finding is the positive effect of assimilation on female health care use, which does not only apply to GPs but also to specialists. Second, the results for other European migrants (Table V.A.3 and V.A.4) are completely in line with the ones presented in Section V.5.1. Third, in the case of foreign-born populations from Latin America and the Caribbean (Table V.A.5 and V.A.6), we found that time spent in Spain has a positive effect on women's visits to GPs and specialists, but a negative impact on hospitalisations. Last, with regards to Africans (Table V.A.7 and V.A.8), the length of residence does not seem to affect health care use, but the estimated coefficients are not statistically different from those reported in the total sample.

V.5.3. Health care use in the middle run

As the number of years spent in Spain might impact the use of health care, and the utilisation patterns for these services might be different for migrants and natives, it is possible that the implications of migration for aggregate health spending vary over time. In this respect, our analysis suggests that on arrival, migrants are not an unusual burden in terms of health spending, since their rates of utilisation are no higher than their native counterparts. The only exception is hospitalisations in the case of women. In this subsection, based on those estimates, we assess whether

[4]The rest of the immigrants represent less than 6% and less than 1% of the foreign-born and total populations, respectively. They include individuals from very different origins, like the United States or Asia. For these reasons, our analysis does not consider them.

this pattern changes after the foreign-born individuals have spent 10 years or more in Spain.

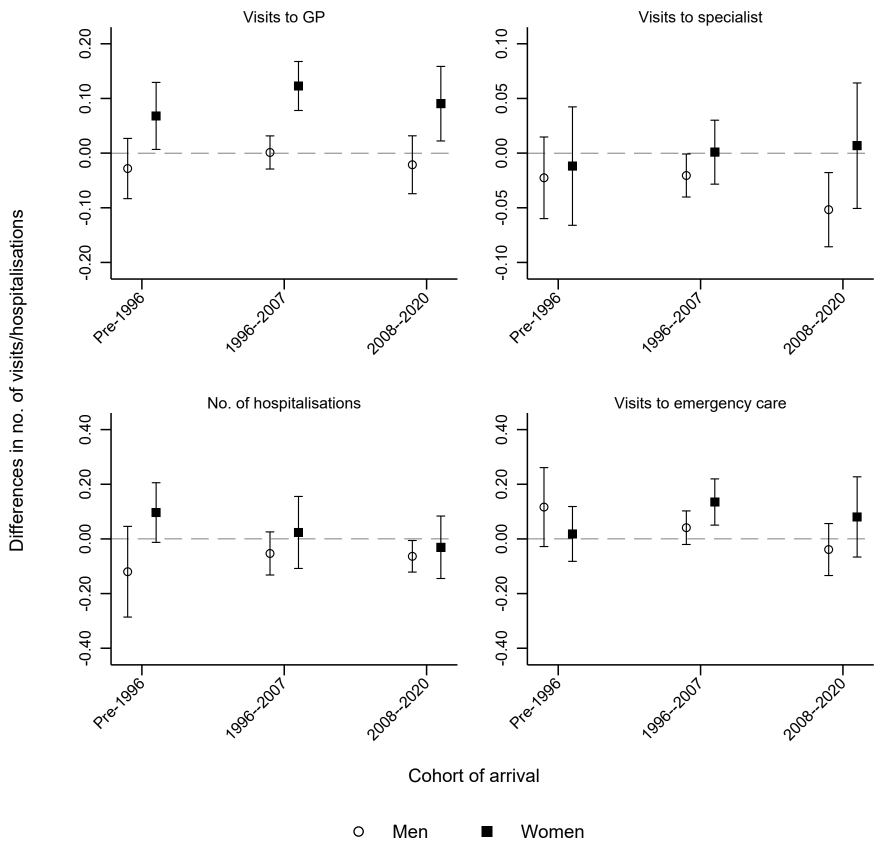
Figure V.1 displays the differences in health care use between 35-year-old migrants who have lived in Spain for ten years or more and locals by type of service, sex, and arrival cohort. The results indicate that, after a decade in Spain, foreign-born males do not use health care differently than natives, with the exception of specialist doctors and two arrival cohorts whose rates of utilisation are lower than natives' rates. Concerning migrant women, their number of visits to GPs is higher than natives' visits, and the same applies to the level of their use of emergency care, for migrants who arrived between 1996 and 2007.

In the appendix (Figure V.A.1–V.A.4), we present the results of separate analyses for four different groups of migrants by region of origin (EU15, other European countries, Latin America, and the Caribbean and Africa). This exercise reveals substantial heterogeneity. For instance, some cohorts of EU15 males exhibit lower rates of use of health care services than natives, in terms of visits to GPs, specialists, and emergency care, 10 years or more after arrival. Differences in the levels of health care utilisation between locals and foreign-born populations from other European countries are null, with the exception of the male cohort that arrived before 1996, which has more visits to specialists than comparable natives. Individuals from Latin America and the Caribbean drive the main results: after at least 10 years in Spain, the number of GP visits among females is higher than native women, and the same applies to two female migrant cohorts in terms of emergency care. Last, the only gaps between African migrants and locals are a higher use of primary care by the 1996—2007 female cohort, a slightly lower number of visits to specialists by males arriving before 1996, and a lower number of hospitalisations among the latest migrant cohort compared to their native counterparts.

These results illustrate that the impact of migration on health spending can vary depending on the time horizon considered and the origin of the foreign-born population. A quick back-of-the-envelope calculation, based on the total number of uses of public health services (Ministerio de Sanidad, 2022) and the cost estimated by some regional health authorities for each service (Resolución STL/353/2013, 2013), exemplifies this point. After 10 years or more of living in Spain, migrants who arrived between 1996 and 2007 required roughly 6% higher health care spending than natives. Similarly, African individuals from the 2007–2020 arrival cohort show a lower level of consumption of these services than locals.

Another interesting outcome that emerges from this picture is that the joint effect of assimilation and differential ageing tends to neutralise the initial differential use of health services by some foreign-born cohorts. In other words, we observe a process of convergence in terms of health care utilisation between migrants and locals, which in certain cases even results in higher rates of utilisation by migrants.

Figure V.1. Differences in health care use between 35-year-old migrants after 10 years in Spain by arrival cohort



Note: The graph shows point estimates and 90%-level confidence intervals. We assume that migrants enter the country 10 years ago or earlier.
Source: Authors' analysis from results in Table V.2 and V.3.

V.5.4. Channels of assimilation

Although the assimilation described above is somewhat limited, it is interesting to dig deeper into the potential mechanisms driving this process, even in a speculative fashion. The first possible channel is health assimilation. In this respect, using a similar econometric specification, we explore the effect of time of residence in Spain on the probability of reporting good health, overweight status, or mental health problems (Table V.4). Our results suggest a pattern of negative health assimilation. In the case of females, the probability of being overweight increases by almost 8 percentage points 10 years after arrival, which is compatible with our findings revealing a higher number of visits to GPs.

The higher use of health care services among migrant women is not at odds with the findings of previous literature on Spanish immigration. Whereas the employment rates and earnings of foreign-born, working-age populations tend to increase with time of residence in Spain (Amuedo-Dorantes & de la Rica, 2007; Izquierdo et al., 2010), their occupational assimilation is incomplete (Fernández-Macías et al., 2015; Rodríguez-Planas & Nollenberger, 2016; Simón et al., 2014). Bearing in mind that health care use seems to decrease with occupational attainment in Spain (Lostao et al., 2011), our results—indicating a higher number of visits to GPs by female migrants who have lived longer in Spain—would align with these labour market developments.

In a related argument, job quality, even leaving aside pay, is substantially worse for Spanish immigrants than for locals (Díaz-Serrano, 2013; Fernández & Ortega, 2007; Gálvez-Iniesta, 2022; Gamero Burón, 2010). Recent studies document that poor working conditions might have a detrimental effect on health similar to that of unemployment (Chandola & Zhang, 2017; Wang et al., 2022). Obviously, our assessment of health assimilation is imperfect (conditioned by the survey), so job quality might be a plausible mechanism for our findings.

A last potential channel has to do with acculturation. A relevant number of studies highlight the relevance of language and culture in migrants' health care access (Fassaert et al., 2009; Ndumbi et al., 2018; Pena Díaz, 2016; Småland Goth et al., 2010; Sorensen et al., 2019; Thomas, 2016). Our results are in line with this literature in that the foreign-born population segment experiencing the most intense assimilation processes are Latin Americans and Caribbeans, followed by

Europeans, while the number of years in Spain does not seem to affect the rates of utilisation among Africans very much. Africans are arguably the most culturally distant migrant group from Spanish locals.

Table V.4. Immigrant arrival cohort and assimilation effects (OLS) in health outcomes

	(I)	(II)	(III)	(IV)	(V)	(VI)
	Good health		Overweight		Mental health problems	
	Men	Women	Men	Women	Men	Women
Immigrant arrival cohort						
Pre–1996	−0.056	0.072	−0.060	−0.086	0.008	0.028
	(0.046)	(0.045)	(0.063)	(0.056)	(0.023)	(0.029)
1996–2007	0.001	0.037	−0.020	−0.043	−0.019	0.009
	(0.036)	(0.037)	(0.054)	(0.048)	(0.018)	(0.024)
2008–2020	0.000	0.089***	−0.041	0.002	−0.012	−0.037*
	(0.031)	(0.032)	(0.046)	(0.041)	(0.014)	(0.021)
Time of residence in Spain						
5–9 years	−0.069***	−0.024	0.049	0.064**	0.024**	−0.021
	(0.026)	(0.028)	(0.039)	(0.032)	(0.010)	(0.019)
10 or more years	−0.077***	−0.033	0.060	0.078**	0.048***	−0.027
	(0.027)	(0.030)	(0.041)	(0.036)	(0.013)	(0.021)
Adjusted R^2	0.156	0.178	0.121	0.134	0.042	0.059
No. of observations	40,936	46,993	39,508	43,348	40,926	46,976
Mean of dependent variable	0.765	0.681	0.605	0.445	0.073	0.150

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects and controls for age (introduced though a third-degree polynomial fully interacted with migrant status), degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

V.5.5. Robustness checks

In this section, we comment on the results of several robustness checks that test the sensitivity of our results to different methodological choices. First, we re-estimate all our models using a Poisson regression model (Table V.A.9 and V.A.10 in the online appendix). Like OLS, this approach yields consistent estimates without requiring any further function of the error term (Blackburn, 2014; Wooldridge, 2010). [5] Reassuringly, the results are qualitative and qualitatively similar to our baseline estimations (Table V.A.11 and V.A.12).[6]

Our second robustness exercise consists of performing our estimations while including only those individuals who solely have public health insurance, as a way of isolating our results from the eventual distortions due to the different normative changes in the last decade. Again, our results hold, even though the degree of precision diminishes because of the smaller sample. [7] The last sensitivity check explores whether our results vary when we limit our analysis to individuals aged less than 65 years old (Table V.A.13 and V.A.14). This methodological choice, which might help to ameliorate the eventual bias due to return migration, does not seem to have any influence on our results beyond reducing the native sample in a non-trivial way.

V.6. Conclusion

Immigration's relationship with the welfare state demands substantial attention from both policy makers and society as a whole. The access and use of health care by foreign-born populations represents a matter of relevance because of their effect on public finances, but also in terms of ensuring an adequate integration of

[5]This model is not efficient in cases of over-dispersion. Nevertheless, other alternatives like the negative binomial regression model, which is more efficient if the error term follows the assumed functional form, result in biased and inconsistent estimates in case of the violation of such properties.

[6]One should bear in mind that, roughly, the coefficients of the Poisson regression model indicate the percentage change in the left-hand-side variable associated with a one-unit change in the right-hand-side variable.

[7]Although insurance is an endogenous variable, the share of the population with only this coverage, almost all migrants and more than 60% of locals, is representative enough to be interesting on its own.

immigrants in the host country. The results of the assessment of both issues might differ on migrants' arrival and after a longer term of residence.

This research contributes to the literature by providing detailed evidence on the process of assimilation in health care utilisation by immigrants in Spain. Our findings suggest, first, that some population segments of foreign-born populations, on arrival, use part of these services less than comparable natives, which is compatible with the healthy migrant effect. Furthermore, such use increases with the time living in Spain, but only in the case of female migrants and visits to GPs. Although this suggests that the evidence of assimilation is not very strong, it is not negligible either: public health spending represented more than 30% of total expenditures in this area in 2020 (Ministerio de Sanidad, 2022; Resolución STL/353/2013, 2013). As a result of this limited assimilation and the different impact of age on the rates of health care utilisation, the patterns of consumption for these services by migrants converge with the natives' patterns and even surpass them for some female migrant cohorts and services. Note that the impact of the time spent in the host country (assimilation) and the differential effect of age on the variable of interest represent distinct phenomena. We can separate them in our analysis, thanks to the use of several waves of the database and the consideration of both natives and migrants in the main specification.

Our results suggest that, even if the gap between migrants and natives in health care use is narrow, assimilation plays a non-negligible role. The existence of substantial heterogeneity by migrant group and time passed since arrival might influence the estimates of migrants' impact on public finances. As a consequence, we believe that researchers aiming to judge welfare state sustainability and those that cover migration could benefit from a more detailed modelling of the patterns of access to social services by foreign-born populations.

Appendix V

Table V.A.1. Age, immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist (foreign-born population from EU15 countries)

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.027***	0.000	0.020***	0.004
	(0.006)	(0.006)	(0.006)	(0.003)
$Age^2/100$	−0.043***	0.009	−0.033***	−0.004
	(0.012)	(0.013)	(0.011)	(0.006)
Age^3 /10000	0.026***	−0.004	0.016***	0.000
	(0.008)	(0.008)	(0.006)	(0.004)
Age × migrant	0.037	0.034	−0.014	−0.040**
	(0.023)	(0.038)	(0.012)	(0.019)
Age/100 × migrant	−0.084	−0.079	0.021	0.073*
	(0.052)	(0.081)	(0.025)	(0.038)
Age/10,000 × migrant	0.056	0.049	−0.011	−0.042*
	(0.036)	(0.054)	(0.017)	(0.023)
Immigrant arrival cohort				
Pre–1996	−0.017	−0.313	0.151	0.093
	(0.100)	(0.258)	(0.122)	(0.117)
1996–2007	−0.086	−0.308	0.107	0.064
	(0.091)	(0.214)	(0.120)	(0.108)
2008–2020	−0.152	−0.199	0.074	0.151
	(0.096)	(0.140)	(0.081)	(0.107)
Time of residence in Spain				
5–9 years	−0.071	0.079	−0.016	−0.026
	(0.083)	(0.108)	(0.067)	(0.053)
10 or more years	−0.109	0.289*	−0.044	0.116*
	(0.090)	(0.174)	(0.079)	(0.070)
Adjusted R^2	0.053	0.034	0.033	0.033
No. of observations	37,967	43,279	37,967	43,279
Mean of dependent variable	0.302	0.415	0.112	0.160

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.2. Age, immigrant arrival cohort and assimilation effects (OLS) in hospital stays and visits to emergency care (foreign-born population from EU15 countries)

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.044**	0.026	0.062***	0.030**
	(0.018)	(0.022)	(0.010)	(0.014)
$Age^2/100$	−0.083**	−0.047	−0.138***	−0.084***
	(0.034)	(0.041)	(0.020)	(0.028)
Age^3 /10000	0.050***	0.027	0.090***	0.061***
	(0.019)	(0.023)	(0.012)	(0.016)
Age × migrant	−0.144	−0.038*	−0.039	−0.021
	(0.126)	(0.022)	(0.058)	(0.049)
Age/100 × migrant	0.400	0.063	0.101	0.018
	(0.347)	(0.046)	(0.113)	(0.096)
Age/10,000 × migrant	−0.300	−0.033	−0.081	−0.002
	(0.258)	(0.029)	(0.070)	(0.060)
Immigrant arrival cohort				
Pre–1996	0.873	0.286**	0.142	0.026
	(0.738)	(0.141)	(0.329)	(0.340)
1996–2007	0.889	0.224*	−0.109	−0.093
	(0.797)	(0.128)	(0.332)	(0.334)
2008–2020	0.922	0.167	−0.152	0.036
	(0.994)	(0.108)	(0.296)	(0.291)
Time of residence in Spain				
5–9 years	−1.027	0.038	0.132	0.134
	(1.138)	(0.059)	(0.143)	(0.155)
10 or more years	−1.194	−0.008	0.027	0.215
	(1.067)	(0.064)	(0.125)	(0.158)
Adjusted R^2	0.010	0.001	0.013	0.016
No. of observations	37,967	43,279	37,967	43,279
Mean of dependent variable	0.123	0.141	0.397	0.538

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.
Source: Authors' analysis from national health surveys.

Table V.A.3. Age, immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist (foreign-born population from European countries other than the EU15)

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.027***	−0.001	0.020***	0.004
	(0.006)	(0.006)	(0.006)	(0.003)
$Age^2/100$	−0.043***	0.010	−0.033***	−0.003
	(0.012)	(0.013)	(0.011)	(0.006)
Age^3 /10000	0.026***	−0.005	0.016***	0.000
	(0.008)	(0.008)	(0.006)	(0.004)
Age × migrant	0.038	0.011	−0.019	0.001
	(0.032)	(0.031)	(0.014)	(0.024)
Age/100 × migrant	−0.112	−0.026	0.035	−0.008
	(0.079)	(0.079)	(0.028)	(0.061)
Age/10,000 × migrant	0.095	0.023	−0.025	0.011
	(0.061)	(0.064)	(0.018)	(0.050)
Immigrant arrival cohort				
Pre–1996	0.061	−0.221	0.188*	0.153
	(0.164)	(0.167)	(0.106)	(0.291)
1996–2007	−0.036	−0.227**	0.019	−0.121
	(0.115)	(0.103)	(0.066)	(0.106)
2008–2020	−0.040	−0.252***	0.041	−0.073
	(0.106)	(0.097)	(0.059)	(0.083)
Time of residence in Spain				
5–9 years	−0.041	0.196***	0.049	0.035
	(0.083)	(0.073)	(0.032)	(0.051)
10 or more years	0.036	0.126	0.107***	0.127
	(0.081)	(0.078)	(0.035)	(0.079)
Adjusted R^2	0.052	0.034	0.033	0.033
No. of observations	38,065	43,480	38,065	43,480
Mean of dependent variable	0.301	0.413	0.111	0.159

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.4. Age, immigrant arrival cohort and assimilation effects (OLS) in hospital stays and visits to emergency care (foreign-born population from European countries other than the EU15)

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age	0.040**	0.028	0.062***	0.034**
	(0.018)	(0.021)	(0.010)	(0.014)
$Age^2/100$	−0.077**	−0.050	−0.138***	−0.092***
	(0.033)	(0.041)	(0.020)	(0.028)
Age^3 /10000	0.047**	0.029	0.090***	0.065***
	(0.019)	(0.023)	(0.012)	(0.016)
Age × migrant	0.013	0.011	0.025	0.090
	(0.019)	(0.022)	(0.051)	(0.060)
Age/100 × migrant	−0.033	−0.040	−0.049	−0.216
	(0.052)	(0.057)	(0.116)	(0.149)
Age/10,000 × migrant	0.025	0.038	0.022	0.160
	(0.045)	(0.045)	(0.081)	(0.115)
Immigrant arrival cohort				
Pre–1996	−0.106	−0.019	0.262	−0.946***
	(0.091)	(0.107)	(0.350)	(0.345)
1996–2007	−0.057	−0.063	−0.077	−0.419
	(0.046)	(0.058)	(0.193)	(0.323)
2008–2020	−0.054	−0.048	−0.240	−0.547***
	(0.040)	(0.048)	(0.164)	(0.207)
Time of residence in Spain				
5–9 years	0.008	0.081	0.013	0.131
	(0.034)	(0.056)	(0.116)	(0.264)
10 or more years	0.029	0.075	0.083	0.376
	(0.039)	(0.054)	(0.127)	(0.244)
Adjusted R^2	0.008	0.001	0.013	0.015
No. of observations	38,065	43,480	38,065	43,480
Mean of dependent variable	0.118	0.140	0.400	0.542

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.5. Age, immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist (foreign-born population from Latin America and the Caribbean)

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.025***	0.002	0.020***	0.006*
	(0.006)	(0.006)	(0.006)	(0.003)
$Age^2/100$	−0.040***	0.005	−0.032***	−0.006
	(0.012)	(0.013)	(0.010)	(0.006)
Age^3 /10000	0.024***	−0.002	0.016***	0.001
	(0.008)	(0.008)	(0.006)	(0.004)
Age × migrant	−0.022	0.059***	−0.004	0.013
	(0.022)	(0.021)	(0.015)	(0.011)
Age/100 × migrant	0.053	−0.120**	0.014	−0.024
	(0.053)	(0.049)	(0.037)	(0.025)
Age/10,000 × migrant	−0.037	0.070**	−0.010	0.013
	(0.039)	(0.035)	(0.028)	(0.017)
Immigrant arrival cohort				
Pre–1996	0.065	−0.222**	0.016	−0.184***
	(0.104)	(0.110)	(0.070)	(0.065)
1996–2007	0.137	−0.204**	0.035	−0.128***
	(0.093)	(0.089)	(0.057)	(0.048)
2008–2020	0.122*	−0.179**	−0.037	−0.114***
	(0.071)	(0.077)	(0.054)	(0.039)
Time of residence in Spain				
5–9 years	−0.095	0.035	−0.031	0.033
	(0.068)	(0.067)	(0.047)	(0.033)
10 or more years	−0.097	0.131*	−0.077	0.080**
	(0.074)	(0.067)	(0.051)	(0.039)
Adjusted R^2	0.052	0.033	0.032	0.034
No. of observations	38,772	44,790	38,772	44,790
Mean of dependent variable	0.300	0.416	0.112	0.160

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.6. Age, immigrant arrival cohort and assimilation effects (OLS) in hospital stays and visits to emergency care (foreign-born population from Latin America and the Caribbean)

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.038**	0.028	0.059***	0.031**
	(0.017)	(0.021)	(0.010)	(0.014)
$Age^2/100$	−0.073**	−0.050	−0.133***	−0.086***
	(0.033)	(0.040)	(0.020)	(0.027)
Age^3 /10000	0.046**	0.029	0.087***	0.062***
	(0.019)	(0.022)	(0.012)	(0.016)
Age × migrant	0.000	−0.016	0.002	0.048
	(0.014)	(0.014)	(0.036)	(0.034)
Age/100 × migrant	0.005	0.031	−0.003	−0.090
	(0.034)	(0.030)	(0.086)	(0.074)
Age/10,000 × migrant	−0.007	−0.021	0.002	0.047
	(0.025)	(0.020)	(0.066)	(0.050)
Immigrant arrival cohort				
Pre–1996	−0.072	0.313***	−0.070	−0.040
	(0.058)	(0.110)	(0.186)	(0.265)
1996–2007	−0.045	0.162**	0.073	−0.120
	(0.042)	(0.073)	(0.174)	(0.232)
2008–2020	−0.037	0.129**	0.051	−0.107
	(0.035)	(0.064)	(0.125)	(0.189)
Time of residence in Spain				
5–9 years	0.081**	−0.081*	−0.094	−0.085
	(0.033)	(0.046)	(0.130)	(0.150)
10 or more years	0.041	−0.106**	−0.035	0.011
	(0.033)	(0.048)	(0.153)	(0.165)
Adjusted R^2	0.008	0.001	0.012	0.015
No. of observations	38,772	44,790	38,772	44,790
Mean of dependent variable	0.118	0.141	0.402	0.549

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.7. Age, immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist (foreign-born population from Africa)

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age	0.027***	−0.001	0.020***	0.004
	(0.006)	(0.006)	(0.006)	(0.003)
$Age^2/100$	−0.043***	0.010	−0.032***	−0.003
	(0.012)	(0.013)	(0.011)	(0.007)
Age^3 /10000	0.026***	−0.005	0.016***	0.000
	(0.008)	(0.008)	(0.006)	(0.004)
Age × migrant	−0.038	−0.060	0.002	0.016
	(0.028)	(0.039)	(0.008)	(0.022)
Age/100 × migrant	0.088	0.103	−0.010	−0.044
	(0.065)	(0.090)	(0.016)	(0.051)
Age/10,000 × migrant	−0.062	−0.053	0.007	0.036
	(0.044)	(0.062)	(0.011)	(0.036)
Immigrant arrival cohort				
Pre–1996	0.003	0.293	−0.038	0.107
	(0.118)	(0.227)	(0.062)	(0.277)
1996–2007	0.081	0.507**	−0.013	0.152
	(0.110)	(0.210)	(0.063)	(0.282)
2008–2020	0.100	0.272	0.005	0.066
	(0.098)	(0.194)	(0.048)	(0.143)
Time of residence in Spain				
5–9 years	−0.012	−0.069	−0.025	−0.211
	(0.089)	(0.139)	(0.048)	(0.218)
10 or more years	0.047	0.080	0.025	−0.197
	(0.090)	(0.154)	(0.055)	(0.281)
Adjusted R^2	0.052	0.032	0.031	0.033
No. of observations	40,936	46,993	40,936	46,993
Mean of dependent variable	0.293	0.410	0.107	0.156

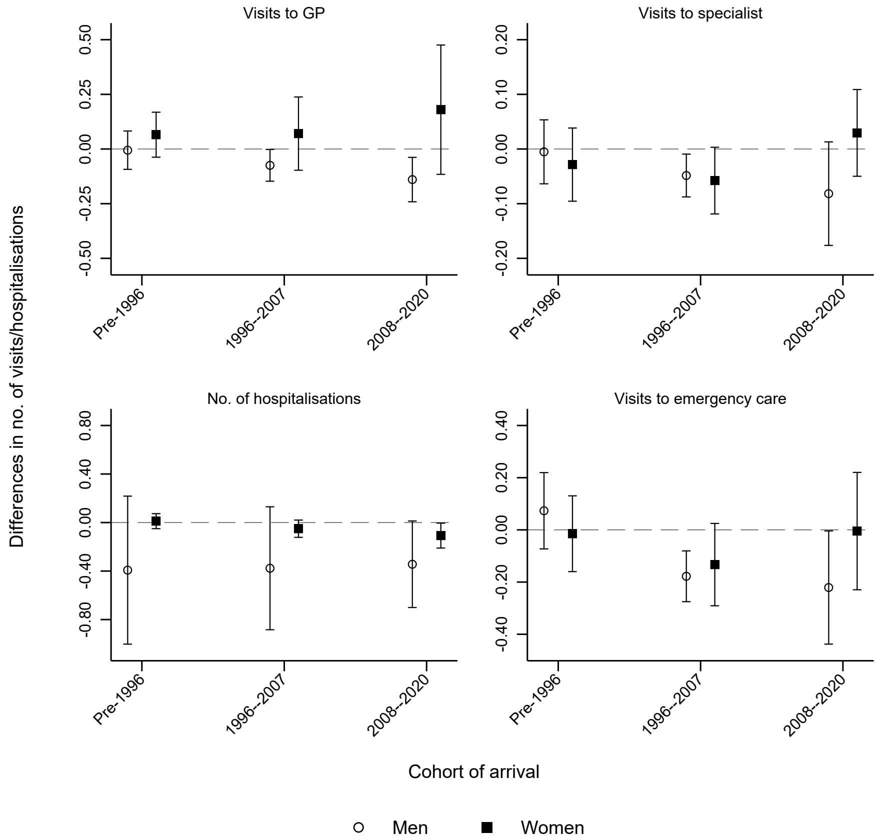
Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.8. Age, immigrant arrival cohort and assimilation effects (OLS) in hospital stays and visits to emergency care (foreign-born population from Africa)

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age	0.039**	0.029	0.060***	0.030**
	(0.018)	(0.021)	(0.010)	(0.014)
$Age^2/100$	−0.074**	−0.052	−0.134***	−0.084***
	(0.033)	(0.041)	(0.020)	(0.028)
Age^3 /10000	0.046**	0.030	0.088***	0.061***
	(0.019)	(0.023)	(0.012)	(0.016)
Age × migrant	0.002	0.028	−0.082	0.126**
	(0.027)	(0.108)	(0.060)	(0.054)
Age/100 × migrant	−0.014	−0.137	0.170	−0.273**
	(0.069)	(0.305)	(0.142)	(0.129)
Age/10,000 × migrant	0.014	0.121	−0.110	0.188**
	(0.054)	(0.230)	(0.099)	(0.094)
Immigrant arrival cohort				
Pre–1996	0.033	0.590	0.438	−0.417
	(0.058)	(0.449)	(0.364)	(0.315)
1996–2007	0.033	0.531	0.200	−0.113
	(0.061)	(0.334)	(0.221)	(0.287)
2008–2020	−0.031	0.265	0.246	−0.390
	(0.055)	(0.282)	(0.230)	(0.244)
Time of residence in Spain				
5–9 years	0.035	−0.145	0.127	−0.270
	(0.027)	(0.130)	(0.174)	(0.256)
10 or more years	−0.010	0.206	0.176	−0.298
	(0.042)	(0.418)	(0.173)	(0.271)
Adjusted R^2	0.008	0.002	0.012	0.016
No. of observations	38,245	43,327	38,245	43,327
Mean of dependent variable	0.118	0.147	0.404	0.544

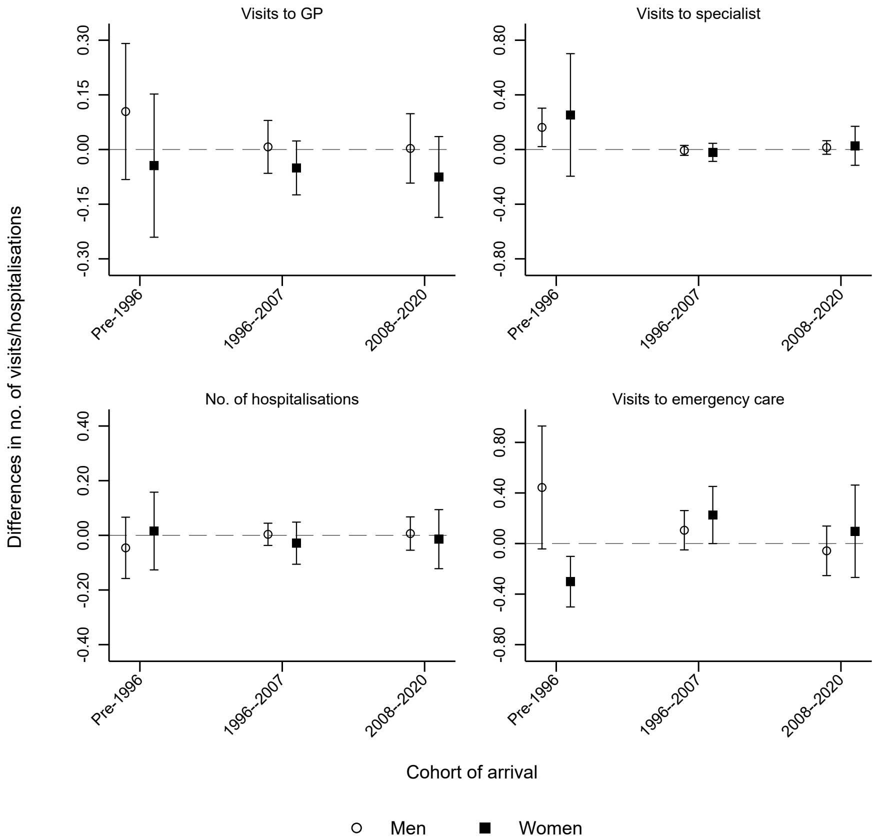
Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Figure V.A.1. Differences in health care use between 35-year-old migrants after 10 years in Spain by arrival cohort (foreign-born population from EU15 countries)



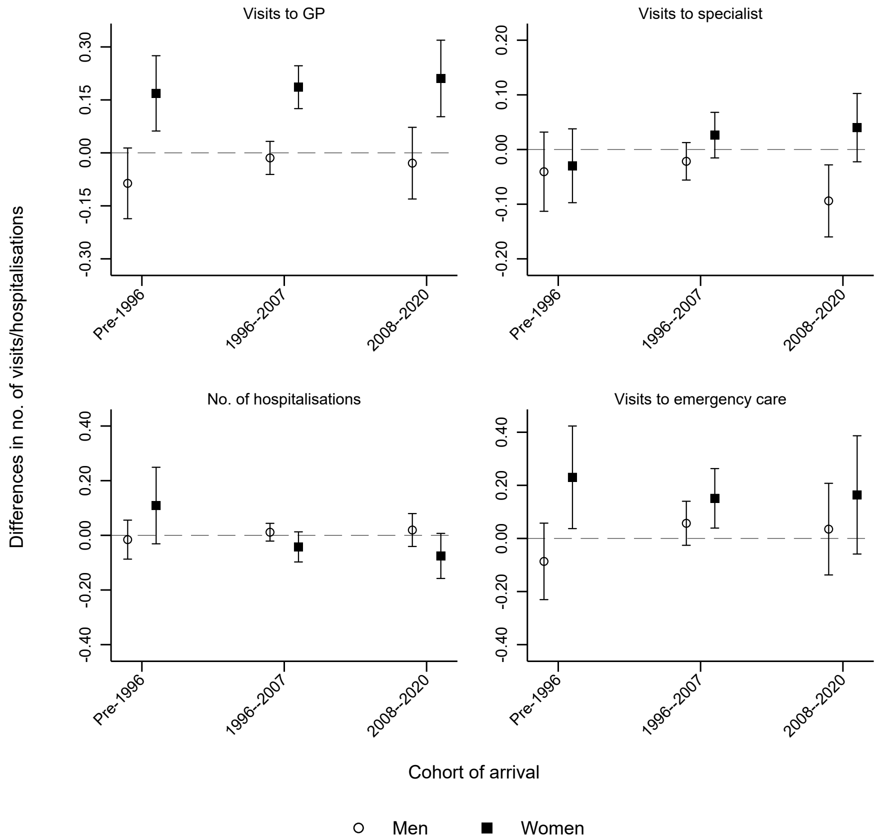
Note: The graph shows point estimates and 90%-level confidence intervals. We assume that migrants enter the country 10 years ago or earlier.

Figure V.A.2. Differences in health care use between between 35-year-old migrants after 10 years in Spain by arrival cohort (foreign-born population from European countries other than EU15)



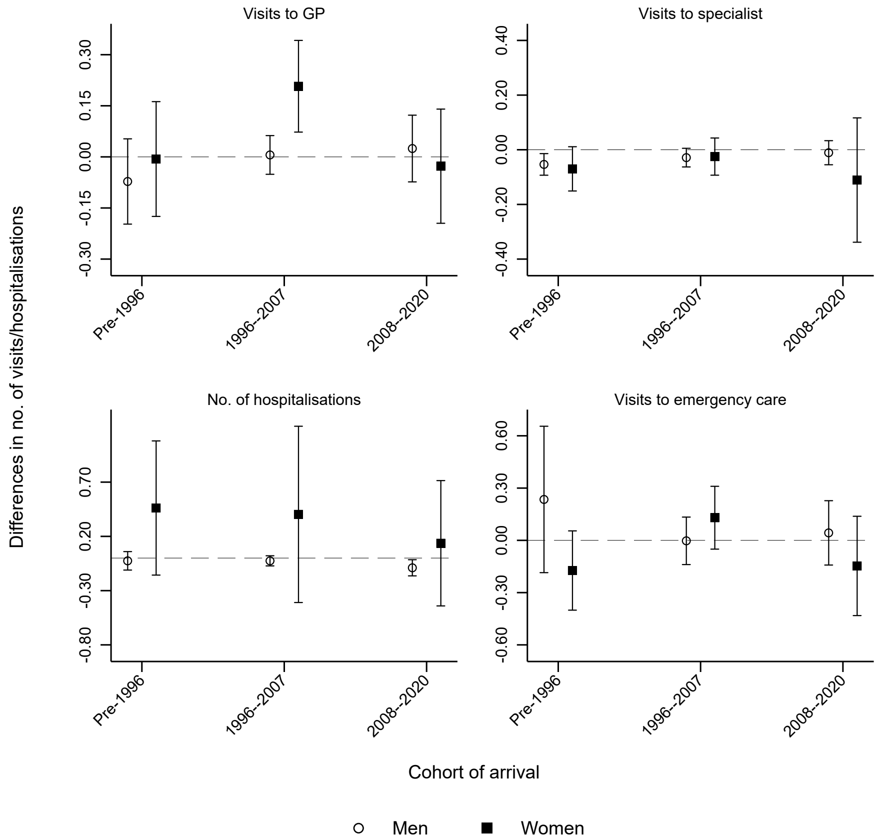
Note: The graph shows point estimates and 90%-level confidence intervals. We assume that migrants enter the country 10 years ago or earlier.
Source: Authors' analysis from results in Table V.A.3 and V.A.4.

Figure V.A.3. Differences in health care use between 35-year-old migrants after 10 years in Spainby arrival cohort (foreign-born population from Latin America and the Caribbean)



Note: The graph shows point estimates and 90%-level confidence intervals. We assume that migrants enter the country 10 years ago or earlier.
Source: Authors' analysis from results in Table V.A.5 and V.A.6.

Figure V.A.4. Differences in health care use between 35-year-old migrants after 10 years in Spain by arrival cohort (foreign-born population from Africa)



Note: The graph shows point estimates and 90%-level confidence intervals. We assume that migrants enter the country 10 years ago or earlier.
Source: Authors' analysis from results in Table V.A.5 and V.A.6.

Table V.A.9. Robustness checks (I): Immigrant arrival cohort and assimilation effects (Poisson-regression estimates) in visits to GP and specialist

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.096***	0.003	0.205***	0.051**
	(0.021)	(0.015)	(0.050)	(0.023)
$Age^2/100$	−0.133***	0.021	−0.342***	−0.062
	(0.041)	(0.029)	(0.097)	(0.044)
Age^3 /10000	0.068***	−0.014	0.175***	0.022
	(0.024)	(0.017)	(0.055)	(0.026)
Age × migrant	−0.035	0.079**	−0.095	0.067
	(0.052)	(0.038)	(0.100)	(0.065)
Age/100 × migrant	0.062	−0.169**	0.193	−0.142
	(0.112)	(0.082)	(0.211)	(0.137)
Age/10,000 × migrant	−0.033	0.100*	−0.124	0.090
	(0.074)	(0.055)	(0.139)	(0.090)
Immigrant arrival cohort				
Pre–1996	0.246	−0.389*	0.681	−0.480
	(0.298)	(0.236)	(0.578)	(0.531)
1996–2007	0.361	−0.251	0.609	−0.383
	(0.278)	(0.210)	(0.540)	(0.485)
2008–2020	0.129	−0.378*	−0.340	−0.645*
	(0.244)	(0.197)	(0.498)	(0.349)
Time of residence in Spain				
5–9 years	−0.271	0.075	−0.367	−0.271
	(0.203)	(0.149)	(0.394)	(0.350)
10 or more years	−0.184	0.282*	−0.516	0.136
	(0.207)	(0.152)	(0.386)	(0.371)
R^2	0.053	0.034	0.039	0.035
No. of observations	40,936	46,993	40,936	46,993
Mean of dependent variable	0.293	0.410	0.107	0.156

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses. R^2 is the squared coefficient of correlation between the actual and the fitted values, as suggested by Zheng and Agresti (2000).

Table V.A.10. Robustness checks (I): Immigrant arrival cohort and assimilation effects (Poisson-regression estimates) in hospital stays and visits to emergency care

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.319***	0.031	0.130***	0.060***
	(0.116)	(0.019)	(0.023)	(0.021)
$Age^2/100$	−0.553**	−0.056	−0.287***	−0.161***
	(0.220)	(0.037)	(0.046)	(0.040)
Age^3 /10000	0.308**	0.032	0.187***	0.113***
	(0.123)	(0.021)	(0.026)	(0.024)
Age × migrant	−0.214	−0.009	−0.047	0.102***
	(0.181)	(0.016)	(0.057)	(0.037)
Age/100 × migrant	0.582	0.003	0.110	−0.208***
	(0.479)	(0.042)	(0.136)	(0.080)
Age/10,000 × migrant	−0.436	0.004	−0.088	0.125**
	(0.360)	(0.031)	(0.098)	(0.053)
Immigrant arrival cohort				
Pre–1996	0.890	0.243***	0.361	−0.521*
	(0.603)	(0.090)	(0.326)	(0.289)
1996–2007	1.297**	0.170**	0.189	−0.316
	(0.660)	(0.072)	(0.252)	(0.260)
2008–2020	1.048	0.116**	−0.014	−0.399**
	(0.755)	(0.053)	(0.207)	(0.202)
Time of residence in Spain				
5–9 years	−1.015	−0.032	−0.006	−0.131
	(0.721)	(0.034)	(0.197)	(0.187)
10 or more years	−1.579**	−0.007	0.116	0.088
	(0.786)	(0.067)	(0.212)	(0.197)
R^2	0.025	0.002	0.014	0.015
No. of observations	39,944	44,820	39,944	44,820
Mean of dependent variable	0.115	0.134	0.403	0.550

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses. R^2 is the squared coefficient of correlation between the actual and the fitted values, as suggested by Zheng and Agresti (2000).

Table V.A.11. Robustness checks (II): Immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist (population with just NHS coverage)

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.026***	0.001	0.019***	0.005
	(0.006)	(0.006)	(0.006)	(0.003)
$Age^2/100$	−0.041***	0.008	−0.030***	−0.003
	(0.013)	(0.013)	(0.011)	(0.006)
Age^3 /10000	0.025***	−0.004	0.015**	−0.001
	(0.008)	(0.008)	(0.006)	(0.004)
Age × migrant	−0.008	0.028**	−0.005	0.011
	(0.013)	(0.014)	(0.008)	(0.008)
Age/100 × migrant	0.017	−0.060*	0.008	−0.025
	(0.031)	(0.032)	(0.017)	(0.017)
Age/10,000 × migrant	−0.012	0.035	−0.004	0.016
	(0.022)	(0.023)	(0.012)	(0.012)
Immigrant arrival cohort				
Pre–1996	0.023	−0.119	0.027	−0.086
	(0.063)	(0.078)	(0.041)	(0.064)
1996–2007	0.064	−0.060	0.033	−0.076
	(0.057)	(0.065)	(0.037)	(0.054)
2008–2020	0.057	−0.096	−0.001	−0.077**
	(0.050)	(0.059)	(0.032)	(0.034)
Time of residence in Spain				
5–9 years	−0.054	−0.001	−0.013	−0.016
	(0.043)	(0.046)	(0.026)	(0.038)
10 or more years	−0.033	0.080*	−0.012	0.041
	(0.046)	(0.048)	(0.030)	(0.048)
Adjusted R^2	0.054	0.032	0.032	0.033
No. of observations	36,497	43,294	36,497	43,294
Mean of dependent variable	0.298	0.419	0.104	0.151

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.12. Robustness checks (II): Immigrant arrival cohort and assimilation effects (Poisson-regression estimates) in hospital stays and visits to emergency care (population with just NHS coverage)

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.046**	0.030	0.059***	0.032**
	(0.019)	(0.021)	(0.011)	(0.015)
$Age^2/100$	−0.088**	−0.055	−0.130***	−0.088***
	(0.036)	(0.040)	(0.021)	(0.028)
Age^3 /10000	0.053***	0.031	0.085***	0.063***
	(0.020)	(0.023)	(0.012)	(0.017)
Age × migrant	−0.045	−0.008	−0.026	0.059**
	(0.041)	(0.018)	(0.026)	(0.024)
Age/100 × migrant	0.119	0.000	0.057	−0.123**
	(0.107)	(0.047)	(0.059)	(0.052)
Age/10,000 × migrant	−0.088	0.006	−0.044	0.078**
	(0.077)	(0.035)	(0.041)	(0.035)
Immigrant arrival cohort				
Pre–1996	0.207	0.254***	0.172	−0.266
	(0.209)	(0.097)	(0.153)	(0.176)
1996–2007	0.275	0.173**	0.110	−0.160
	(0.266)	(0.075)	(0.116)	(0.162)
2008–2020	0.270	0.112**	0.035	−0.217*
	(0.300)	(0.057)	(0.098)	(0.132)
Time of residence in Spain				
5–9 years	−0.199	−0.032	0.003	−0.090
	(0.255)	(0.037)	(0.076)	(0.111)
10 or more years	−0.280	−0.008	0.058	0.025
	(0.276)	(0.072)	(0.085)	(0.116)
Adjusted R^2	0.007	0.001	0.012	0.014
No. of observations	36,497	43,294	36,497	43,294
Mean of dependent variable	0.120	0.145	0.411	0.559

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.13. Robustness checks (III): Immigrant arrival cohort and assimilation effects (OLS) in visits to GP and specialist (population aged less than 64 years old)

	(I)	(II)	(III)	(IV)
	No. of visits to GP		No. of visits to specialist	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.029***	0.046***	0.011	0.016**
	(0.008)	(0.012)	(0.007)	(0.008)
$Age^2/100$	−0.048**	−0.114***	−0.005	−0.034
	(0.022)	(0.031)	(0.017)	(0.022)
Age^3 /10000	0.031*	0.097***	−0.008	0.024
	(0.018)	(0.026)	(0.014)	(0.018)
Age × migrant	0.023	−0.009	0.007	−0.016
	(0.022)	(0.029)	(0.015)	(0.017)
Age/100 × migrant	−0.071	0.038	−0.024	0.051
	(0.063)	(0.080)	(0.042)	(0.048)
Age/10,000 × migrant	0.064	−0.043	0.022	−0.048
	(0.056)	(0.069)	(0.036)	(0.041)
Immigrant arrival cohort				
Pre–1996	0.017	−0.103	0.022	−0.035
	(0.065)	(0.078)	(0.041)	(0.064)
1996–2007	0.021	−0.018	0.011	−0.037
	(0.056)	(0.066)	(0.036)	(0.052)
2008–2020	−0.010	−0.069	−0.022	−0.035
	(0.049)	(0.058)	(0.033)	(0.034)
Time of residence in Spain				
5–9 years	−0.041	0.016	−0.012	−0.019
	(0.038)	(0.044)	(0.025)	(0.037)
10 or more years	−0.022	0.081*	−0.011	0.039
	(0.041)	(0.047)	(0.028)	(0.046)
Adjusted R^2	0.023	0.020	0.024	0.033
No. of observations	30,295	31,240	30,295	31,240
Mean of dependent variable	0.236	0.357	0.092	0.149

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

Table V.A.14. Robustness checks (III): Immigrant arrival cohort and assimilation effects (Poisson-regression estimates) in hospital stays and visits to emergency care (population aged less than 64 years old)

	(I)	(II)	(III)	(IV)
	No. of hospitalisations		No. of visits to emergency care	
	Men	Women	Men	Women
Age effects and interactions				
Age	0.045***	0.030	0.053***	0.105***
	(0.014)	(0.040)	(0.015)	(0.027)
$Age^2/100$	−0.082***	−0.058	−0.110***	−0.280***
	(0.027)	(0.108)	(0.039)	(0.067)
Age^3 /10000	0.049**	0.039	0.066**	0.220***
	(0.020)	(0.092)	(0.033)	(0.052)
Age × migrant	0.063	0.075	0.003	0.046
	(0.050)	(0.076)	(0.046)	(0.052)
Age/100 × migrant	−0.197	−0.233	−0.033	−0.092
	(0.161)	(0.220)	(0.124)	(0.145)
Age/10,000 × migrant	0.198	0.207	0.042	0.056
	(0.167)	(0.191)	(0.103)	(0.125)
Immigrant arrival cohort				
Pre–1996	0.065	−0.222**	0.016	−0.184***
	(0.104)	(0.110)	(0.070)	(0.065)
1996–2007	0.137	−0.204**	0.035	−0.128***
	(0.093)	(0.089)	(0.057)	(0.048)
2008–2020	0.122*	−0.179**	−0.037	−0.114***
	(0.071)	(0.077)	(0.054)	(0.039)
Time of residence in Spain				
5–9 years	−0.183	−0.036	−0.008	−0.086
	(0.225)	(0.035)	(0.072)	(0.105)
10 or more years	−0.227	−0.002	0.063	0.053
	(0.243)	(0.078)	(0.081)	(0.111)
Adjusted R^2	0.005	0.001	0.016	0.014
No. of observations	30,295	31,240	30,295	31,240
Mean of dependent variable	0.087	0.129	0.390	0.556

Notes: *** significant at 1% level; ** significant at 5% level; * significant at 10% level. All specifications include an intercept, year and region fixed effects, degree of urbanisation, education and marital and activity status. Heterokedasticity-robust standard errors in parentheses.

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