Received: 16 June 2016 | Revised: 9 February 2018 | Accepted: 21 February 2018

DOI: 10.1002/hec.3659

RESEARCH ARTICLE

WILEY Health Economics

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

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Funding information
Christian Doppler Laboratory 'Aging, Health and the Labor Market'; NORFACE research project "GIWeS-Globalisation, Institutions and the Welfare State"; Spanish Ministry of Science and Innovation, Grant/Award Numbers: CSO2013-47667-P and CSO2013-41828-R

JEL Classification: I12; I18; J13; J18

Abstract

This study 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 12th week of pregnancy in the short run. We 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. Our 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, we 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.

KEYWORDS

abortion, differences-in-differences, fertility, Uruguay

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 is legal (United Nations, 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 12th 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: 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.

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. 18987, which decriminalised abortion, and subsequent legal decrees came into force on 3rd December 2012. The reform makes it possible to terminate pregnancies up until the 12th 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 5-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, Gruber, and Staiger (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 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, Koenig, & Hindin, 2008).

[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, Rodríguez, & Berro, 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.

[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, Arribeltz, Alemán, Carril, & Rey, 2011).

The bulk of the empirical evidence on the effects of abortion on fertility outcomes originates from the United States (Ananat, Gruber, & Levine, 2007; Ananat et al., 2009; Angrist & Evans, 2000; Bitler & Zavodny, 2002; Charles & Stephens, 2006; Cook, Parnell, Moore, & Pagnini, 1999; Donohue III & Levitt, 2001, 2014; Gruber, Philip, & Staiger, 1999; Guldi, 2008; Joyce, 1987, 2003, 2009, 2010; Levine, Staiger, Kane, & Zimmerman, 1999; Rotz, 2013; Sorenson, Wiebe, & Berk, 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, 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 1 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 4 and 5.

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 drug usage (Charles & Stephens, 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, 2014; Gipson et al., 2008; Joyce, 2003, 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.

3 | DATA AND METHODS

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

[3]There are some notable exceptions such as Kane and Staiger (1996) and Levine, Trainor, and Zimmerman (1996).

[4]It 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.

(Díaz-Rossello, 1998; Fescina et al., 2010; Simini, 1999; World Health Organization, 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.

3.2 | Identification strategy

In order to explore the causal effect of abortion legislation on fertility outcomes, we employ a 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 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 5, 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 3 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

[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.

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:

$$\ln(births)_{gt} = \alpha + \beta \cdot Unplanned_g \cdot Abortion\ law_t + \delta \cdot Unplanned_g + \eta_t + \varepsilon_{gt}, \quad (1)$$

where $\ln(births)_{gt}$ 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 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 4.

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 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:

$$Outcome_{it} = \alpha + \beta \cdot Unplanned_i \cdot Abortion\ law_t + \delta \cdot Unplanned_i + \eta_t + \varepsilon_{it}, \quad (2)$$

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 1. Based on the availability of variables in the database, we focus on the following nine birth quality outcomes: birth weight (in 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 1 and 5 min, single mother, hypertensive mother, mother with eclampsia, and mother with pre-eclampsia.[8] Following the reasoning outlined above, we also include a transition variable in Equation 2.

[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 that there is no possibility of anticipation effects captured by the treatment variable.

[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.

Both models are estimated by ordinary least squares.[9] In order to take into account the possible intragroup correlation in 2, 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, Duflo, & Mullainathan, 2004). As there are only two groups, there is no evident completely satisfactory way to address this problem.[10] Therefore, as a robustness check, in Equation 1, we implement several versions of standard errors under the Newey–West estimator (Newey & West, 1987) that are robust to autocorrelation, the most likely time-series fertility pattern, 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 Model 2, 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]

Tables 1 and 2 present the main descriptive statistics of the samples employed in the analysis. Table 1 shows the mean and standard deviation of the variables of interest corresponding to the econometric exercise represented by Equation 1 for the quantity of births. Table 2 contains the same statistics for the variables used when—in Equation 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.

Figure 1 shows the evolution of the total number of births per month (in 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 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*-value of a test of equality of linear trends between planned and unplanned pregnancies before the law came into force using the 4-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.

4 | RESULTS

The results of estimating Equation 1 under different strategies are shown in Table 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

[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).

[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]See for instance and among many others, Prskawetz, Mamolo, and Engelhardt (2010) 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 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 [17.51]	235.05 [16.81]	10.05*** (2.78)	172.59 [13.56]	155.04 [15.35]	−17.55*** (2.35)
No. of births of mothers aged under 20 with primary education	6.79 [2.47]	6.93 [2.72]	13.89 (0.42)	12.14 [4.51]	−1.75*** [3.35]	(0.64)
No. of births of mothers aged under 20 with secondary education	17.12 [3.82]	18.26 [4.37]	1.14* (0.67)	32.89 [6.28]	29.95 [5.03]	−2.95*** (0.92)
No. of births of mothers aged 20–34 with primary education	22.99 [4.64]	21.05 [4.82]	−1.93 (0.77)	29.13 [5.24]	23.97 [4.68]	−5.16*** (0.81)
No. of births of mothers aged 20–34 with secondary education	94.97 [9.78]	98.96 [9.79]	3.99** (1.59)	67.50 [8.02]	62.64 [8.13]	−4.85*** (1.31)
No. of births of mothers aged 20–34 with tertiary education	46.39 [7.13]	48.89 [6.58]	2.50** (1.11)	10.93 [3.71]	9.39 [3.57]	−1.54** (0.59)
No. of births of mothers aged 35 or over with primary education	3.30 [1.95]	3.49 [1.73]	0.18 (0.30)	4.58 [2.22]	3.51 [2.06]	−1.07*** (0.35)
No. of births of mothers aged 35 or over with secondary education	14.99 [3.90]	15.67 [4.49]	0.68 (0.68)	8.32 [2.99]	7.30 [2.77]	−1.01** (0.47)
No. of births of mothers aged 35 or over with tertiary education	16.67 [4.16]	18.93 [4.73]	2.26*** (0.72)	3.14 [1.64]	2.67 [1.53]	−0.47* (0.26)
Number 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.

***Differences significant at 1%

**significant at 5%

*significant at 10%.

correlation up to order 4 (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" 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 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.

In the second place, as outlined in Section 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 4 do not control for serial correlation for reasons of brevity, but the results remain the same when we controlling for it.[14]

[13]These results are available in the supplementary material for this article (Tables S1–S3).

[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 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 (grams)	3,289	3,299	11	3,256	3,285	29**
	[571]	[575]	(9)	[592]	[570]	(12)
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 (Kessner Index)	0.691	0.722	0.031***	0.455	0.508	0.053***
	[0.462]	[0.448]	(0.007)	[0.498]	[0.500]	(0.010)
Adequate prenatal care (Ministry of Public Health)	0.823	0.849	0.026***	0.585	0.632	0.046***
	[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 pre-eclampsia	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)
Number of observations (births)	7,218	7,521		5,130	4,761	

Note. Standard deviations of the variables in brackets and standard errors of the differences in the means of the variables in parentheses. In some of the variables, the number of observations is slightly lower because of missing values.

***Differences significant at 1%

**significant at 5%

*significant at 10%.

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 12th week of pregnancy.

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 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 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 6) suggest that there is only a slight positive selection of births

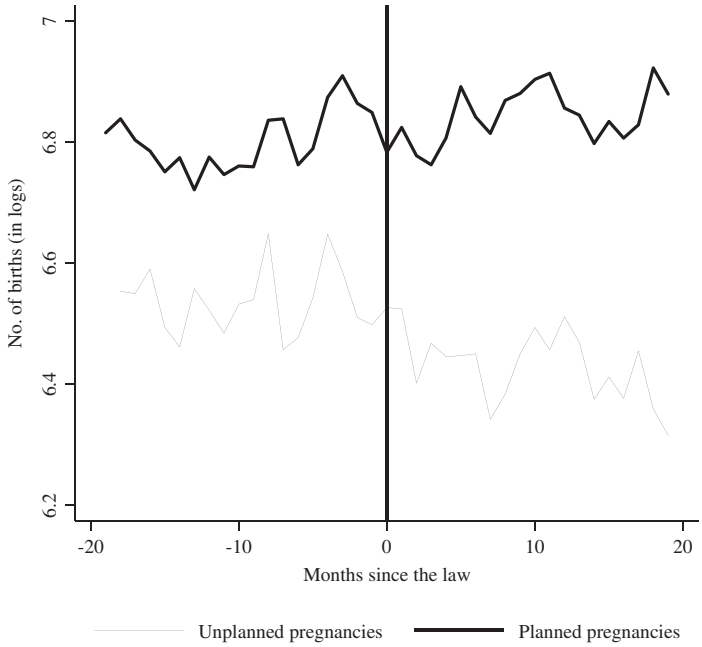


FIGURE 1 Evolution of the number of births before and after the law

Note. Months represents groups of 4 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.

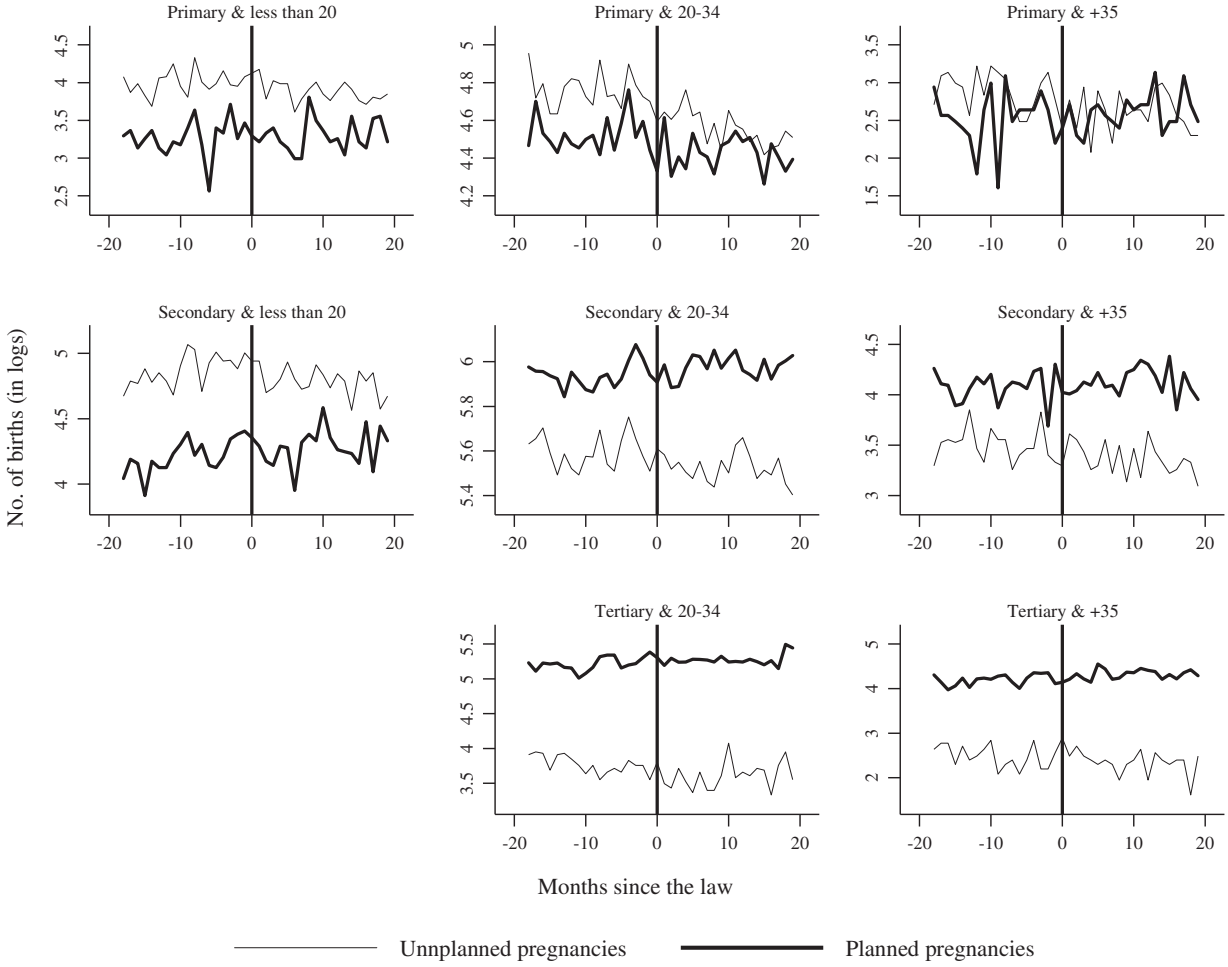
in terms of prenatal control care and the Apgar score.[15] The probability of receiving adequate prenatal care according to the Kessner criteria increases by 5% points and 4.2% if we follow the definition of the Ministry of Public Health of Uruguay. Meanwhile, the reform has a positive impact on the Apgar score at 1 and 5 min of 2.2% and 1%, respectively.

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 (see for instance, 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 12th 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

[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 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.



Note: Months represent groups of 4 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 (1st column); 0.639, 0.575 and 0.001 (2nd column); 0.402, 0.546, and 0.199 (3rd column).

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

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 12th 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 5-day waiting period for reflecting about the abortion), it is difficult to legally perform a legal abortion associated to the detection of these types

[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.

TABLE 3 Effect of abortion legislation on the number of births (in logs)

	(1)	(2)	(3)	(4)	(5)
Treatment	−0.171***	−0.081**	−0.081**	−0.103**	−0.078**
	(0.017)	(0.038)	(0.037)	(0.048)	(0.037)
Placebo I				−0.032	
				(0.034)	
Placebo II					0.011
					(0.041)
Mean of the dependent variable	5.264	5.264	5.264	5.264	5.264
R^2	0.933	0.936	0.936	0.936	0.936
No. of observations	304	304	304	304	304
Week fixed effects	Yes	Yes	Yes	Yes	Yes
Group dummy	Yes	Yes	Yes	Yes	Yes
Group-specific linear time trend	No	Yes	Yes	Yes	Yes
Control for serial correlation	No	No	Until AR(4)	Until AR(4)	Until AR(4)

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%

**significant at 5%

*significant at 10%.

of congenital anomalies.[18] Third, the rate of congenital anomalies 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, Gómez, Cetraro, Etcheverry, & Pérez, 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

[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 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.

TABLE 4 Effect of abortion legislation on the number of births by age and education

	(1) Under 20 & primary	(2) Under 20 & secondary	(3) 20–34 & primary	(4) 20–34 & secondary	(5) 20–34 & tertiary	(6) 35 or over & primary	(7) 35 or over & secondary	(8) 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)
Mean of the dependent variable	2.183	3.138	3.162	4.368	3.050	1.214	2.332	1.933
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
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group-specific linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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. In some demographic groups, some weeks are excluded because the number of births recorded is zero and the log transformation cannot be applied.

***Significant at 1%

**significant at 5

*significant at 10%.

TABLE 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

	(1) Births from unplanned pregnancies of women aged 20–34 with secondary education	(2) Rest of births	(3) Difference [(3) = (1)−(2)]
Birth weight (in logs)	8.067	8.063	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 1 min (in logs)	2.124	2.119	0.005
	(0.003)	(0.001)	(0.003)
Apgar 5 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)

Note. Standard errors in parentheses.

***Significant at 1%

**significant at 5%

*significant at 10%.

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

	(1) Birth weight (in logs)	(2) Premature	(3) Adequate prenatal care (Kessner)	(4) Adequate prenatal care (Ministry)	(5) Apgar 1 min (in logs)	(6) Apgar 5 mins (in logs)	(7) Single mother	(8) Hypertensive mother	(9) Pre-eclampsia	(10) 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)
Mean of the dependent variable	8.077	0.089	0.617	0.745	2.130	2.260	0.162	0.021	0.034	0.001
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
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group-specific linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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%

**significant at 5%

*significant at 10%.

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]

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 2, 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 & Evans, 2000; Bitler & Zavodny, 2002; Guldi, 2008; Levine et al., 1999; Mitrut & Wolff, 2011; 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 Economic Co-operation and Development), with a relatively high level of economic and social development (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 2 years the reform, and both cases were linked to illegal abortions (Quian, 2015).

[19]As stated in the main text, the "placebo" laws do not guarantee that the abortion policy 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 S4).

6 | CONCLUSIONS

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 2 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 (United Nations, 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.

ACKNOWLEDGEMENTS

Previous versions of this paper were presented at an internal seminar at the Labour Market Area of the Department of Economics of the Johannes Kepler University Linz (Austria) in 2015, a regular seminar at the Department of Economics of University of the Republic in Montevideo (Uruguay) in 2016 and the 30th Annual Conference of the European Society of Population Economics in Berlin (Germany) in 2016. We thank Ana Balsa, Leonel Briozzo, Cecilia Noboa, Nicole Schneeweis, Michael Topf, Rudolf Winter-Ebmer, Sally Wright, the editor Thomas DeLeire, and an anonymous referee for their helpful comments on a previous draft of this work. Antón gratefully acknowledges funding from the Spanish Ministry of Science and Innovation (research projects CSO2013-41828-R and CSO2013-47667-P), the NORFACE research project "GIWeS-Globalisation, Institutions and the Welfare State", and the Christian Doppler Laboratory "Aging, Health and the Labor Market."

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SUPPORTING INFORMATION

Additional Supporting Information may be found online in the supporting information tab for this article.

How to cite this article: Antón J-I, Ferre Z, Triunfo P. The impact of the legalisation of abortion on birth outcomes in Uruguay. *Health Economics*. 2018;27:1103–1119. https://doi.org/10.1002/hec.3659