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THE IMPACT OF ABORTION LEGALISATION ON BIRTH OUTCOMES IN URUGUAY

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ABSTRACT

This work evaluates the impact of an abortion reform in Uruguay allowing free interruption of pregnancy until 12 weeks of gestation on the quantity and quality of births in the short run. We employ a differences-in-differences approach, a comprehensive administrative register of births and a novel identification strategy based on the planned or unplanned nature of pregnancies that end in births. Our results suggest that this policy induced an 8% decline in the number of births of unplanned pregnancies, driven by the group of mothers between 20 and 34 years old with secondary education. This increased the average quality of births in terms of more intensive prenatal control care and a lower probability of having a single mother. Furthermore, we document a positive selection process of births affected by the reform, as adequate prenatal control care and Apgar scores rose among the affected demographic group.

KEYWORDS: abortion, Uruguay, fertility, difference-in-differences.

JEL CLASSIFICATION: I12, I18, J13, J18.

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1. INTRODUCTION[1]

Uruguay is a small South American country with political and social peculiarities that distinguish it from other countries in the hemisphere. Whereas its vital statistics (high life expectancy and low fertility and infant mortality rates) are similar to those observed in the most industrialised nations and access to health care is universal, inequality levels and the extension of the informal economy are remarkable and not so far from the regional Latin American and Caribbean averages (see, for instance, Economic Commission for Latin America and the Caribbean, 2014).

The aim of this work is to evaluate the impact of a policy reform allowing the voluntary interruption of pregnancy in Uruguay that makes abortion possible before the twelfth week of gestation and entered into force on 3rd December 2012. The main hypothesis of the research is that this policy change might have a negative impact on the number of births, through a reduction of unplanned fertility and, if this change is non-random, a subsequent selection process on some birth quality outcomes.

The contribution of this work is threefold. First, it provides an evaluation of a policy intervention in Uruguay for which there is no empirical evidence so far. In the second place, almost all previous studies on the effects of abortion focus on developed countries, particularly the United States and Romania. The third contribution has to do with the identification strategy: whereas previous literature either relies on before-after estimates or exploits the spatial variation in access to voluntary interruption of pregnancy, we exploit the distinction between births from planned and unplanned pregnancies available in our database. Particularly, we rely on a unique administrative register of births in Uruguay, the Perinatal Information System (PIS), which comprises very precise and detailed time and spatial information on births.

[1] A first version of this paper was presented at an internal seminar at the Labour Market Area of the Department of Economics of the Johannes Kepler University Linz (Austria) and a regular seminar at the Department of Economics of University of the Republic in Montevideo (Uruguay). We thank Ana Balsa, Leonel Briozzo, Nicole Schneeweis, Michael Topf and Rudolf Winter-Ebmer for 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'.

Using a differences-in-differences (DID) approach, focusing on a relatively short time period (38 months between 2011 and 2014) centred on the date of the reform and assuming that only unplanned pregnancies are affected by the intervention and planned pregnancies serve as the control group, we estimate the causal effects of the depenalisation of abortion on fertility and birth quality outcomes. Our findings suggest that the introduction of the voluntary interruption of pregnancy in Uruguay resulted in a reduction of unplanned fertility of roughly 11% among women between 20 and 34 years old with secondary education. Overall, the observable characteristics of the births from unplanned pregnancies of these females (health indicators of new born children and their mothers and socio-demographic characteristics of the latter) are worse than average. Moreover, we find that a selection process operates in this reduction of births and, within the mentioned socio-demographic group, we find that the quality of births associated with unplanned pregnancies modestly improves in terms of better prenatal control care and higher Apgar scores.

The rest of the work is structured in four sections that follow this introduction. First, we provide a brief description of the reform and comment on the main insights of previous literature. The third section describes the database and the identification strategy and empirical methods. In the fourth place, we present and discuss the main results of the analyses. The last summarises the main findings of the research.

2. BACKGROUND AND PREVIOUS LITERATURE

As in other areas of social policy, Uruguay has been one of the pioneers in the region in terms of allowing voluntary interruption of pregnancy and it is currently the only country in the hemisphere apart from Cuba, Guyana and Mexico City where abortion on demand is legal (United Nations, 2014). Until the reform analysed here and with the exception of a brief lapse between 1934 and 1938, abortion was only permitted on the grounds of rape, maternal health and economic problems. .

Law No. 18987 that depenalised abortion and subsequent legal decrees came into force on 3rd December 2012. The reform makes it possible to interrupt pregnancy

before the twelfth week of gestation, with all the associated costs covered by the Ministry of Public Health. The procedure is intended to be chemical through the use of misoprostol. Women who wish to abort must appear before a commission of three healthcare professionals to receive detailed information about 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 the procedure is scheduled. Females under 18 years old can be allowed to decide on their own if allowed by the three-member commission.[2]

The main hypothesis of the work is that abortion legalisation leads to a decline of births from unplanned pregnancies, which can affect to the quality of the average birth (a selection process on certain birth outcomes) in an ambiguous way. Firstly, Economic Theory predicts that the lowering the costs of abortion interruption might have a positive effect on both pregnancies and abortions, 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 (firstly, to become pregnant and, secondly, to abort or give birth) on the basis of their expected payoff, with the choice of giving birth or aborting made under more complete information on birth quality outcomes than the pregnancy decision. In the second place, the negative effect of abortion on the unwanted number of births can lead to an improvement in child's outcomes through several channels: the existence of a child quantity-quality trade-off (Becker and Lewis, 1973), the higher likelihood for women of programming fertility consistently with their educational and labour market plans (Angrist and Evans, 2000) and a lower probability of inadequate pre-natal care due to unwantedness (Grossman and Jacobowitz, 1981; Rosenzweig and Schultz, 1983; Grossman and Joyce, 1990; Joyce and Grossman, 1990). Nevertheless, if the access to abortion or the likelihood of interrupting the pregnancies is not independent of mothers' characteristics, in whose case the net effect on average child's outcomes can be negative and

[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 healthcare professionals linked to the main public maternity hospital providing counselling to women wanting to abort (both before and after interruption) with the aim of reducing the risk of injury associated with unsafe abortion within the legal framework (Briozzo *et al*., 2002, 2006 and 2007; Briozzo, 2007 and 2008). 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).

compensate the former effect (Pop-Eleches, 2006). Finally, it is also worth mentioning that the reduction of unplanned fertility can raise the quality of the average birth by itself given that the quality outcomes of unwanted births tend to be significantly worse than others (Gipson *et al*., 2008).

The bulk of the empirical evidence on the effects of abortion on fertility outcomes refers to the United States (Joyce, 1987, 2003, 2009 and 2010; Cook *et al*., 1999; Gruber *et al*., 1999; Levine *et al*., 1999; Angrist and Evans, 2000; Donohue and Levitt, 2001 and 2004; Bitler and Zavodny, 2002; Sorenson *et al*., 2002; Charles and Stephens, 2006; Ananat *et al*., 2007 and 2009; Guldi, 2008; Rotz, 2013), with several studies focused on Romania (Pop-Eleches, 2006 and 2010; Mitrut and Wolff, 2011), a comparative research (Levine and Staiger, 2004) and another one on Nepal (Valente, 2012). In the first place, overall, these works suggest that legislation facilitating abortion implies a drop in fertility rates and unwanted fertility.[3] This process is accompanied often by a process of positive selection of births, in the sense that the decline of fertility concentrates in those pregnancies with worse characteristics (either associated to newborns or mothers) than average, which are interrupted. Therefore, those children born after the reform come from mothers with better characteristics and enjoy higher welfare levels in later stages of life than the average birth before laws allowing abortion. However, findings are not homogeneous: there is still a lot of controversy regarding the relationship between abortion and youth crime, the impact of abortion on some birth quality outcomes is nil, there is no significant selection in the case of Nepal and even in the Romanian case before the ban on abortion of 1966 it seems to be associated with a negative selection of births on certain characteristics, that is, the reduction was larger in births with better outcomes (high education and urban residence) than the average one. Therefore, even if, in principle, we expect to find a decline in fertility and an improvement in the quality of births, the existence, extent and signs of the selection effect should not be taken for granted.

[3] There is some remarkable exception like Kane and Staiger (1996).

3. DATA AND METHODS

3.1. DATABASE

The data source used in this work is the Perinatal Information System (PIS), a unique dataset: it is an administrative register that provides very precise time and spatial information on births, including characteristics of mothers, pregnancies (such as the weeks of gestation) and newborns (Díaz-Rosello, 1998; Simmini, 1999; Fescina *et al*., 2010; 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 healthcare professionals and the information is then entered into the PIS.

Our analysis makes use of the PIS from 2011 to 2014. As the coverage of the register was not complete in 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 town and the whole country, respectively, during the period of analysis.

We must make several observations regarding the period of analysis and sample selection. As mentioned above, abortion is only allowed within the first 12 weeks of gestation. Therefore, we focus our attention on those pregnancies (which end in a birth) at the thirteenth week of gestation, when abortion is no longer legally possible (under non-exceptional circumstances). Particularly, keeping in mind that the birth day 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 reach 13 weeks of gestation after 8 June 2014 must be excluded because there could be births that reached that number of weeks after that point that could correspond to year 2015, which is not available in the database. That means that we have a period of something more than 19 months after the legislation entered into force. Therefore, in our analysis we consider a time window of 38 months (152 weeks) symmetric with respect to 3rd December 2012, which includes all births that reached 13 weeks of gestation between 20th June 2011 and 18th May 2014. In sub-section 3.2, we provide additional details. 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 in the group of women aged 20 to 34 years old with secondary education (among whom we find evidence of an impact), the sample size shrinks to 24,630 births.

3.2. IDENTIFICATION STRATEGY

In order to explore the causal effect of the abortion legislation on fertility outcomes, we employ a differences-in-differences (DID) strategy exploiting the information contained in the PIS database. Our identification strategy is novel and it is based on the information about the planned or unplanned nature of the pregnancy. Gynaecologists ask women during their visits whether the pregnancy is planned or not and they enter that information in the system. In order to obtain the causal impact of the reform, we need to assume that the legal changes can affect only 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 put in force in December 2012. It might be the case that abortion affects the unwantedness of births. According to Ananat *et al*. (2009), lowering the costs of abortion can lead to both a higher number of pregnancies and abortions, with an ambiguous effect on fertility. When a woman becomes pregnant, she receives more information about the costs and benefits of child birth and might change her decision on having or not the child. However, even if the unwantedness of births can be affected by the abortion regime and the number of unplanned pregnancies might increase because of the relatively lower costs of interrupting the births, in principle, there is no reason for expecting that pregnancies that were initially planned might change because of the possibility of abortion.

The DID approach only requires that, in the absence of the treatment (the policy intervention allowing abortion), the evolution of both groups would have been parallel (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 explain eventual changes in fertility outcomes. The assumption of absence of impact of the reform on planned births is not

directly testable. However, aiming to shed some light on the plausibility of our identification strategy, 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 1 when the reform entered into force and 0 otherwise. The coefficient of this binary variable is not significant (p-value equals 0.253), which is certainly reassuring.

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

$$\ln\left(\text{births}\right)_{gt} = \alpha + \beta \cdot \text{Unplanned}_g \cdot \text{Abortion law}_t + \delta \cdot \text{Unplanned}_g + \eta_t + \varepsilon_{gt} \qquad [1]$$

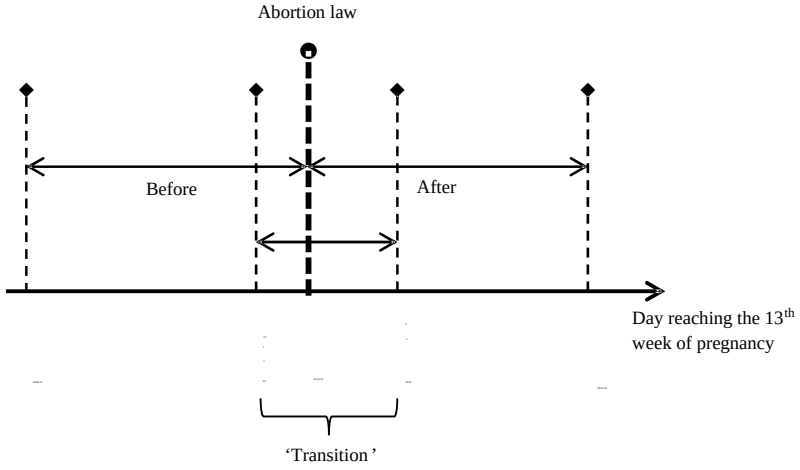
where $ln(births)_{gt}$ represents the logarithm of the number of births of the group g (planned or unplanned) in time period t; α is an intercept; the variable $Unplanned_g$ is a group dummy variable that takes the value zero for the series of planned pregnancies and one for the series of unplanned ones; $Abortion\ law_t$ is a time dummy taking the value one when the legislation allowing voluntary interruption 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, covers the causal effect of the abortion legislation, particularly the average treatment effect, on the number of births. As we look at the number of births per week as our left-hand-side (LHS) variable, we do not include any additional controls to estimate the main effect of the law. Later on, as long as we further carry out a separate analysis by mothers' age and education level and most of the observable characteristics of births (even age and schooling) can be considered as outcomes, so they would represent bad controls in the sense of Angrist and Pischke (2008).

Three additional points should be made. In the first place, 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 whose mothers have been exposed at least 12 weeks to the new law.[4] Therefore, we focus on what happens 12 weeks after the coming into force of the law. The eventual effect of the

[4] In this framework, the first women totally treated are those who reach the 13 weeks of gestation, 12 weeks after the reform came into force. Even assuming complete access to abortion facilities as soon as the law started to be effective, it is possible that the exposure of a woman that reached the 13 weeks of gestation a few days after the reform and that of a female exposed for 12 weeks is actually very different.

reform during such a period is captured by an additional variable we add 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.[5] In the second place, aiming to assess how plausible the parallel trends assumption is as one of the main devices for checking the robustness of the DID estimations suggested by Angrist and Pischke (2008), we include a group-specific linear time trend. Even if parallel trajectories of groups are not observed, this allows us to obtain consistent estimates under the assumption of parallel growth of the series (Mora and Reggio, 2015). Finally, aiming to shed some additional light on the validity of the identification strategy used in the analysis, we carry out two falsification tests -estimating the effect of two 'placebo' interventions-, described in Section 4. Aiming to summarize the information provided above, we outline the time window used in the analysis in Figure 1.

Figure 1. Time window used in the analysis



Source: Authors' elaboration.

The model presented above is estimated for the whole sample of births considered and, in a separate way, 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 decline. In other words, the reduction of births might affect some groups more than others, being concentrated on potential

[5] It 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.

mothers and children with certain characteristics. As mentioned in section 2, previous empirical evidence is not clear and unequivocal about the expected direction of the selection. Aiming to disentangle the existence of such a selection process, we estimate the following reduced-form model based on individual birth data:

$$\text{Outcome}_{it} = \alpha + \beta \cdot \text{Unplanned}_i \cdot \text{Abortion law}_t + \delta \cdot \text{Unplanned}_i + \eta_t + \varepsilon_{it} \qquad [2]$$

where $Outcome_{it}$ denotes a certain outcome of birth i, which takes place in period t. The rest of the terms of the equation have the same meaning as in equation [1]. Based on the availability of variables in the database, we focus on the following 9 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 one minute and at five minutes, single mother, hypertensive mother, mother with eclampsia and mother with pre-eclampsia.[6] Following the reasoning stated above, we add a transition variable as well in equation [2].

Both models are estimated by ordinary least squares (OLS).[7] 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 (Bertrand *et al*., 2004; Angrist and Pischke, 2008). As we only have two groups, there is no completely robust way to address this problem.[8] Therefore, as a robustness check, in equation [1], we implement several versions of standard errors under the Newey-West estimator (Newey and West, 1987) which are robust to autocorrelation, the most likely time-series fertility pattern, up to a

[6] According to the Kessner criterion, 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 birth.

[7] OLS 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 while the same property in the linear regression model only needs the absence of omitted relevant variables (Angrist and Pischke, 2008).

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

certain order.[9] Particularly, 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 $0.75T^{1/3}$, T being the number of available periods of time, although we consider different numbers of periods. In the case of model [2], in order to control for the possible serial correlation at the group level, we collapse the dataset at the time-group level and, using the mean of the variables at that level and weighting by the number of births of each group in each time period, we implement the mentioned Newey-West estimator. Also, we carry out the estimation using weeks as time units, although the results obtained considering months are exactly the same.[10]

The main descriptive statistics of the samples used in the analysis are included in Tables 1 and 2. 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, whereas Table 2 contains the same statistics for the variables used when we explore the effect of the abortion legislation on the quality outcomes of births of women aged 20 to 34 with complete secondary education in equation [2]. From this descriptive stuff, we can see that the prevalence of births from unplanned pregnancies is very 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 below 20 years old.

[9] See, for instance and among many others, Prskawetz *et al*. (2010) and Brehm and Engelhardt (2015).
[10] The statistical significance of the coefficients is exactly the same although, naturally, the size of the coefficients might 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				Unplanned			
	Before		After		Before		After	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
No. of births	225.00	17.51	235.05	16.81	172.59	13.56	155.04	15.35
No. of births of mothers aged less than 20 years old with primary education	6.79	2.47	6.93	2.72	13.89	4.51	12.14	3.35
No. of births of mothers aged less than 20 years old with secondary education	17.12	3.82	18.26	4.37	32.89	6.28	29.95	5.03
No. of births of mothers aged 20-34 years old with primary education	22.99	4.64	21.05	4.82	29.13	5.24	23.97	4.68
No. of births of mothers aged 20-34 years old with secondary education	94.97	9.78	98.96	9.79	67.50	8.02	62.64	8.13
No. of births of mothers aged 20-34 years old with tertiary education	46.39	7.13	48.89	6.58	10.93	3.71	9.39	3.57
No. of births of mothers aged 35 years old or more with primary education	3.30	1.95	3.49	1.73	4.58	2.22	3.51	2.06
No. of births of mothers aged 35 years old or more with secondary education	14.99	3.90	15.67	4.49	8.32	2.99	7.30	2.77
No. of births of mothers aged 35 years old or more with tertiary education	16.67	4.16	18.93	4.73	3.14	1.64	2.67	1.53
Number of observations (weeks)	152		152		152		152	

Source: Authors‘ analysis of PIS data.

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

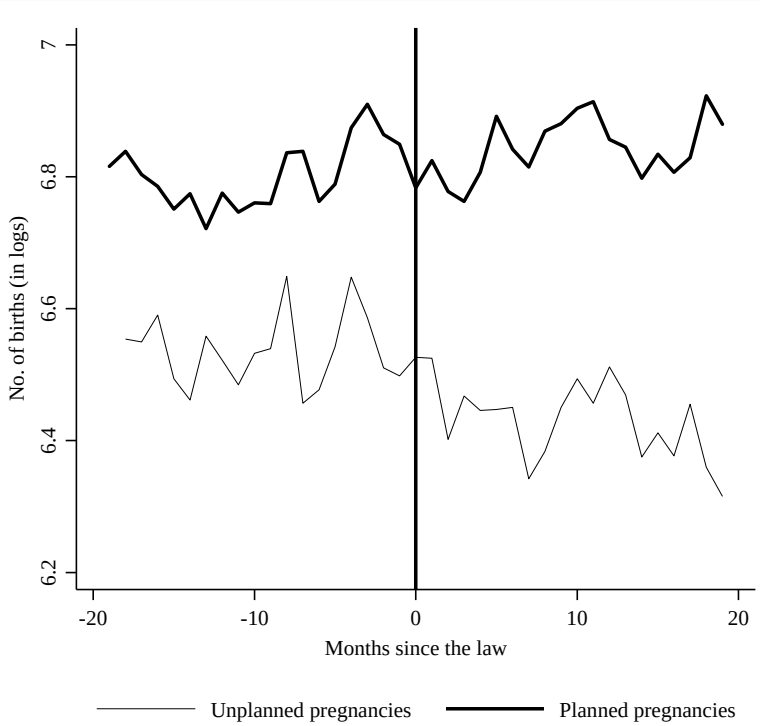
	Planned				Unplanned			
	Before		After		Before		After	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Birth weight (grams)	3,289	571	3,299	575	3,256	592	3,285	570
Premature	0.089	0.285	0.085	0.279	0.093	0.291	0.090	0.287
Adequate prenatal care (Kessner Index)	0.691	0.462	0.722	0.448	0.455	0.498	0.508	0.500
Adequate prenatal care (Ministry of Public Health)	0.823	0.382	0.849	0.358	0.585	0.493	0.632	0.482
Apgar at 1 minute	8.465	1.220	8.499	1.206	8.449	1.231	8.518	1.146
Apgar at 5 minutes	9.585	0.998	9.604	1.012	9.558	1.000	9.603	1.014
Single mother	0.098	0.298	0.110	0.312	0.243	0.429	0.254	0.435
Hypertensive mother	0.020	0.138	0.020	0.141	0.025	0.155	0.019	0.138
Mother with pre-eclampsia	0.032	0.177	0.035	0.183	0.030	0.171	0.037	0.189
Mother with eclampsia	0.001	0.026	0.001	0.035	0.002	0.044	0.001	0.032
Number of observations (births)	7,218		7,521		5,130		4,761	

Note: In some of the variables, the number of observations is slightly lower because of missing values.

Source: Authors‘ analysis of PIS data.

Figure 2 shows the evolution of the total number of births (in logs) during the period of interest. Albeit the figure is descriptive and there seems to be some pre-existent trend, the graph is quite suggestive and seems to point to a decline in the pattern of births from unplanned pregnancies. Figure 3 depicts the same relationship by age-education group, making clear the different relevance of unplanned pregnancies across demographic groups. Although it is risky to infer a clear outcome from the graph, the evolution of the number of births of women aged 20 to 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) is also suggestive of a reduction in the number of births since the entry into force of the law.

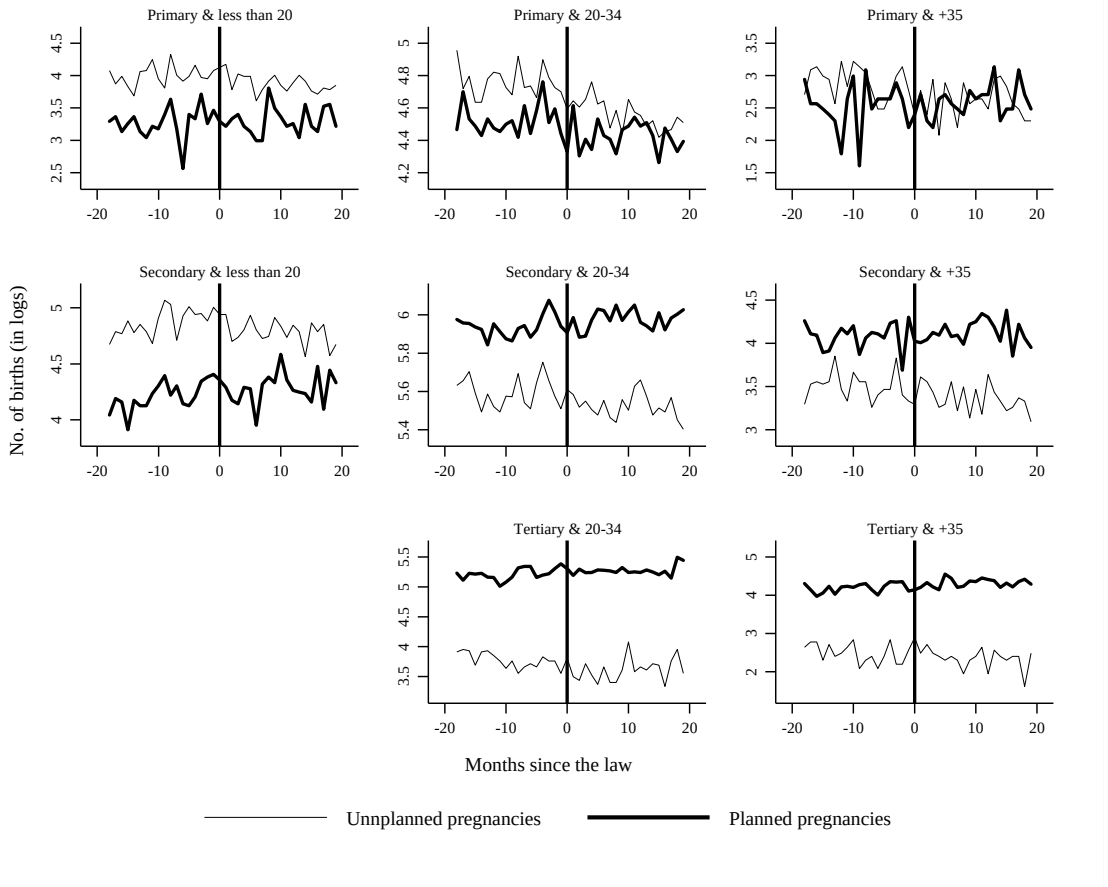
Figure 2. Evolution of the number of births before and after the law



Note: Months represents groups of 4 weeks.

Source: Authors' analysis of PIS data.

Figure 3. Evolution of the number of births before and after the law by mothers' age group and education



Note: Months represent groups of 4 weeks.

Source: Authors' analysis of PIS data.

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, when we control for a group-specific linear time trend, the negative impact declines from 17 to 8%. The outcomes of the analysis are robust to the consideration of serial correlation up to order 4 (consistent with the rule of thumb mentioned above) and, reassuringly, the falsification tests yield more confidence about the existence of a causal effect of this health policy intervention. We also perform the analysis considering different orders of autocorrelation (up to order 12) and also the

dependent variable in levels and obtain similar results.[14] As mentioned above, we implement two 'placebo' interventions. The first 'placebo' law consists in a 'treatment' applied to the 12 weeks prior to the coming into force of the law and the second looks at what happens during 12 weeks in 2012 corresponding to the first 12 weeks of our period of treatment but obviously a year earlier. Neither of them is statistically significant, which is comforting.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.171***	-0.081**	-0.171***	-0.081**	-0.103**	-0.078**
	(0.017)	(0.038)	(0.020)	(0.037)	(0.048)	(0.037)
Placebo I					-0.032	
					(0.034)	
Placebo II						0.011
						(0.041)
Mean of the LHS variable	5.264	5.264	5.264	5.264	5.264	5.264
R^2	0.933	0.936	0.933	0.936	0.936	0.936
No. of observations	304	304	304	304	304	304
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Group dummy	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	No	Yes	No	Yes	Yes	Yes
Control for serial correlation	No	No	Until AR(4)	Until AR(4)	Until AR(4)	Until AR(4)

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. 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 dummy which takes the value zero if it is a planned pregnancy and one if it is an unplanned one.

Source: Authors' analysis of PIS.

In the second place, as explained in the 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 shown above. Our results clearly indicate

[14] These results are available from the authors upon request.

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 brevity, but, when we control for the existence up to serial autocorrelation, the results remain the same.[15]

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Less than 20 & primary	Less than 20 & secondary	20-34 & primary	20-34 & secondary	20-34 & tertiary	35 or more & primary	35 or more & secondary	35 or more & secondary
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 LHS 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
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. Standard errors robust to heteroscedasticity in parentheses. All the specifications include a constant and a transition variable. The group dummy is a dummy which 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 was zero and the log transformation cannot be applied.

Source: Authors' analysis of PIS.

There are several reasons that might explain why this demographic group is the most affected by the reform. The first one has to do with the fact that women between 20 and 34 years old mean the main fertility group, representing more than 70% of total fertility. Particularly, more than 4 out of 10 births correspond to females of this age group with secondary education. In the second place, as mentioned in section 2, since

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

2002, a decade before the abortion law, there have existed a group of health professionals (*Health Initiatives Against Induced Abortion in Unsafe Conditions*) based at the main maternity hospital of Montevideo (Pereira Rossell Hospital) advising women willing to interrupt pregnancies and trying to guarantee above all, the safety of abortions. In such an environment, one could find the pharmacological means for doing so (mainly, misoprostol) the black market (López Gómez *et al*., 2011). Therefore, the possibility of chemical abortion in the decade prior the reform, even if not widespread, existed to certain extent. Although there are no statistics available on this issue, one can speculate that access to abortion was not randomly distributed across demographic groups. Given that the Pereira Rossell Hospital is a public centre that mainly serves low-resources population, it is possible that the access of women with medium and high levels of education, with higher resources, did not access to the facilities of *Health Initiatives* or were not willing to risk to participate in a system targeted at low-income population.

Even if there is no selection effect of births 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. In order to get an idea of how the average birth can be affected, we look at the average characteristics of births of the affected group of mothers (women aged 20 to 34 years old who completed secondary education) versus the rest of births (Table 5). Overall, there are no large differences between the quality of births in both groups with a very few exceptions. The births associated to unplanned pregnancies (whose weight declines) are characterised by less adequate prenatal controls and a higher proportion of single motherhood than the rest of births as a whole. Though 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 induced an improvement in the quality of the average birth in terms of prenatal controls.

Table 5. Differences in means of observable characteristics between births fromunplanned pregnanciesof 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 controls (Kessner)	0.455	0.597	-0.142***
	(0.007)	(0.003)	(0.008)
Adequate prenatal controls (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.

Source: Authors' analysis of PIS data.

As outlined in section 3, the second part of the analysis focuses on looking at the qualitative outcomes of births of those women who are affected by the intervention according to the results shown above. Therefore, we estimate equation [2] for the affected group of births in order to see if an underlying selection of births is operating here. It is worth mentioning that previous literature does not provide a short-cut hypothesis of how the decline of fertility owed to the legalisation of abortion can affect birth outcomes, in the sense that, as mentioned in Section 2, either a positive or a negative selection process might be observed. The results of our analysis (Table 6) suggest that there is only slight positive selection of births in terms of prenatal control

care and Apgar score.[16] The probability of receiving adequate pre-natal controls according to the Kessner criteria increases in 5 percent 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 Apgar scores at 1 and 5 minutes of 2.2 and 1%, respectively.

There are two additional issues to be mentioned. First, the effects of the depenalisation may be beyond the ones reported here. As long as unsafe abortion is considered as one the main risk factors of maternal mortality, the depenalisation of abortion should have helped to reduce illegal interruptions made under bad health and safety conditions. Although there is no hard empirical evidence on this issue, according to the Ministry of Public Health, there were only two maternal deaths caused by abortion practices in the first two years the reform was in force and both were linked to illegal abortions (Quian, 2015). In the second place, the absence of spectacular effects of the reform could be associated with the Health Initiatives Against Induced Abortion in Unsafe Conditions group, which not only did not favour the existence of a social climate supporting voluntary interruption of pregnancy but also somehow facilitated the procedure for those women interested.

[16] 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.

Table 6. Effect of abortion legislation on qualitative birth outcomes among mothers aged 20 to 34 years old with secondary education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Birth weight (in logs)	Premature	Adequate prenatal controls (Kessner)	Adequate prenatal controls (Ministry)	Apgar 1 min (in logs)	Apgar 5 mins (in logs)	Single mother	Hypertensive 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)
Mean of the LHS 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
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%. 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 dummy which takes the value zero if it is a planned pregnancy and one if it is an unplanned one.

Source: Authors' analysis of PIS.

5. CONCLUSIONS

On 3rd December 2012, a seminal abortion reform for Latin America and the Caribbean, allowing for voluntary interruption of pregnancy within the first 12 weeks of gestation, came into force in Uruguay. In the first two years under the new law (from December 2012 to December 2014), the number of voluntary interruptions of pregnancy was 15,176 (Ministerio de Salud Pública, 2014 and 2015). In the year 2014, this meant an abortion rate of 12 interruptions per 1,000 women between 15 and 45 years old, a level similar to that in countries like Spain, Portugal or Italy (United Nations, 2014).

This research 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% decline in the number of births from unplanned pregnancies of women aged 20 to 34 years old with secondary education. Given that the quality of births of this group before the reform was below average, per se and others things being equal, the decline should have contributed to the improvement of the outcomes of the average birth. Moreover, we find that the reduction of births is not orthogonal to some observable birth quality outcomes, but there is a positive selection process regarding adequate prenatal control care and Apgar score. Further research is needed in order to disentangle the effect of abortion on middle- and long-term socio-economic indicators of children and economic outcomes of adults and possible positive effects on the safety conditions of abortions realised in the country.

It is also worth mentioning that the social, political and health environment in Uruguay at this time was very different from the one found in the United States in the 70s. Firstly, even if abortion was not legal prior to the reform, there was some room for pregnancy interruption under medical advice through the action of some professionals based at the main public maternity hospital. Secondly, the technology existing in the 2000s allowed that most of the illegal abortions were carried out employing pharmacological means (Sanseviero, 2003), leaving a larger room for the possibility of non-legal interruptions than in the American case several decades earlier. These factors can also contribute to explain why the effects of the reform are concentrated on a specific age-education group.

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