

THE EFFECT OF WOMEN'S RIGHTS ON WOMEN'S WELFARE: EVIDENCE FROM A NATURAL EXPERIMENT*

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This paper explores whether the welfare of women increased following the extension of women's rights between 1960s and 1990s. Using individual level data on life satisfaction and focusing on changes in birth control rights in twelve European countries, it shows that the extension of both abortion rights and the pill is strongly linked to an increase in life satisfaction of women of childbearing age. Birth control rights also increased women's investment in education, probability of working and income. Other women's rights have proved less beneficial. Mutual consent divorce laws decreased women's welfare. High maternity protection on the job has negligible effects.

What's college? That's where girls who are above cooking and sewing go to meet a man so they can spend their lives cooking and sewing.

(an advertisement for an American department store in 1950)

Modern life is based on control and science. We control the speed of our automobile. We control machines. We endeavor to control disease and death. Let us control the size of our family to ensure health and happiness.

(an American family planning poster in the early 1940s)¹

A key issue in public economics is to evaluate the effects of public policies. Between the late 1960s and the 1990s vast and deep changes in social norms and institutions took place in relation to women's rights. Women as a pressure group became influential in politics and equal treatment and reproductive rights laws were enacted in the majority of Western countries. Their goal was to improve women's welfare, and similarly to other public policy initiatives, they required funding to implement them. After ten to thirty years of such public policies, we can evaluate their effects.

There is no bet *a priori* on the direction and the size of these effects. With some exceptions, economists generally expect an increase in welfare from policies that remove binding constraints on choices. In the public debate the opinion is fragmented. Some qualitative evaluations report that women ended up with a double burden from being both the primary caregiver in the family and a worker and that ultimately women 'could not have it all'.

The data show that in the last thirty years women had fewer children, studied longer, worked more and earned a higher wage. The positive link between wage, education and welfare is generally assumed. Instead of imposing this assumption, I

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¹ The first quote is reported by Watkins (1998), the second by Marks (2001).

directly explore the perception of welfare based on self-evaluation by the subjects treated. I focus on the policy of extending birth control rights, i.e. abortion rights and the pill, on women's welfare in Europe.

Most Western countries legalised abortion between the late 1960s and 2000.² The pill was invented in the 1950s, marketed for the first time in the US in 1960, and embodied in national health systems in Europe later (some countries never adopted it). Although contraception has always existed, 'promoted as almost 100 per cent effective, the pill altered people's expectations about contraception and what it would achieve' (Marks, 2001). Its availability as part of the nationally financed public system meant that precise information became available to all women and that they could have access to it regardless of age, marital status, and financial constraints.

I exploit the variation given by the staggered and uncoordinated legalisation of abortion and the pill in twelve European countries between the 1960s and 1990s. The timing of the legislation could in principle be related to national characteristics, like the dominant religion or the share of women in parliament but none of these factors are by themselves good predictors of the actual law changes. Several historical conditions have played a considerable part in the process. Therefore I can treat the legislation as exogenous to the process determining women's life satisfaction. In the course of the analysis I explicitly address this issue and conduct a number of robustness tests.

The recent availability of surveys of life satisfaction on repeated cross-sections of individuals makes it possible to directly evaluate the welfare effect of policies. The answers of nationally representative samples of individuals about their current life satisfaction are used as revealed subjective utility levels and compared to traditional revealed preference indicators. I focus on how women changed their set of incentives and choices and derived private benefits from the policy changes. The identification of the welfare effect comes from the fact that the exposure of an individual to birth control rights varied by gender, country of residence and date of birth. A differences-in-differences estimator allows me to identify the effect of laws passed at different times in each country and which affected a particular group of individuals, women of childbearing age, with respect to women who were not exposed to abortion rights. This could be either because they lived in countries and years with abortion rights, but had completed the fertility cycle, or because their country did not legalise abortion. Treatment and control groups are comparable once age effects are controlled for.

The main finding is that women of childbearing age when the policy was introduced consistently show an increase in welfare, according to all specifications used. It is robust to a number of alternative specifications and the effect is sizeable.

Additional sources of variation in the policies are considered, like the number of years that women have been exposed to them, their age when abortion was legalised, the distinction between partial and full abortion rights, and the role of

² Reproductive rights were perceived as a path-breaking right in 1960s and 1970s, but they had been a privately regulated issue for almost two thousand years, from the ancient Greek to the eighteenth century. Only in the nineteenth century did abortion and contraception become discouraged if not prohibited in most Western countries.

religion in hindering the adoption or the application of the laws in some cases. Concerns about the role of concurring policies in favour of women are addressed, examining the effect of changes to divorce law and the interaction of abortion rights with maternity leave policies.

The next Section reviews how the paper relates to the literature and describes the channels through which birth control rights may affect welfare. Section 2 describes the data and the empirical strategy. Section 3 presents the main results and Section 4 some extensions. Section 5 concludes and the Appendix lists the sources of all variables.

1. Background

There are two opposing theoretical perspectives on the link between welfare and birth control rights. The prevalent one is that birth control and abortion rights have shifted out the frontier of available choices and increased women's welfare. Goldin and Katz (2002) describe the pill as lowering the cost of delaying marriage by allowing sex without commitments. It allowed young and single women to invest more in graduate and professional education and achieve better careers. At the same time it allowed them to keep a good match in the marriage market, and possibly an even better one, due to a 'social multiplier effect' that made the market for 'career women' thicker. They find evidence that in the US access to the pill by women when they were 18 to 21 years old increased the likelihood that they went to university and married later. In a theoretical model, Siow (2002) shows that the welfare of women always increases with birth control rights, except in the rare case when the supply of marriageable men is extremely scarce, as fewer transfers are needed to induce women into commitments.

The other strand in the literature emphasises the adverse welfare effects on women from birth control. Akerlof *et al.* (1996) set out to explain the 'feminisation of poverty' in the US. The idea is that birth control innovations weakened the bargaining position of women in the marriage market. Losing the possibility of demanding a 'shotgun marriage' and the associated transfers, women end up with out-of-wedlock children and lower income. Akerlof *et al.* argue that ultimately all women lose welfare. In their framework, when birth control became available, women invested more in human capital because of lower expected rents from marriage.

In this paper I maintain the focus on private benefits to individuals, rather than on societal or 'public good' effects.³ The three main channels through which birth control rights can affect individual utility are a reduction in unwanted children, a general empowerment of women and a better planning of education choices for women. In the first channel, birth control rights do not have any effect on the number of 'planned' children and on the utility deriving from them (it does affect their timing and spacing only), while it affects the number of unplanned children. Introducing birth control rights reduces the probability of disutility from

³ Deriving (dis)utility from these rights on moral ground or because someone has a taste for individual rights is not analysed here.

unplanned children. This prediction is difficult to test in the data for the lack of counterfactual.

The second channel is that of changing the benchmark of social norms. Akerlof and Kranton (2000) describe the way that identity depends on the assigned social categories and on the extent to which one's given characteristics match the ideal of the assigned category. Granting individual rights on birth control to women changed not only the individual choice of fertility but also the benchmark itself. The social category of reference could then be different from the traditional one of primary caregiver in the family. A gain in utility arises when the distance between the actual choice and the benchmark diminishes. An attempt to identify this effect empirically is made when separating Catholic from Protestant countries, where social categories considered appropriate for women are likely to be different. Anticipating some results, the empirical analysis finds support for birth control rights changing the average welfare of all in Catholic countries, while in non-Catholic countries a significant effect is only found on treated women.

The third channel links birth control rights, education choices and welfare changes for women. It can be extended to include an effect on men and it lends itself to predictions that can be tested empirically. It is possible to write a simple model in which men and women derive utility from their income, which in turn depends on education. It is straightforward to show that without birth control rights (or when the woman does not want to exercise them) the optimal choice of education for women is lower than for men because the likelihood of unplanned fertility shortens the horizon over which to reap the benefits of education. Only if birth control rights exist and the woman is willing to use them, *can* the optimal choice of education for men and women be equal.⁴ Under the standard assumption that income and welfare are positively related, the welfare of women increases with birth control rights.⁵

The effect on men is the result of two counteracting effects. A positive effect on welfare is given by the fact that, in an equilibrium based on assortative matching of income/education, their partner is more educated than without birth control rights. A negative effect on welfare could stem from lower bargaining power within the household as the partner's income increases.

No empirical study of the link between reproductive rights and welfare exists yet, although there is a vast literature on unintended pregnancies and negative effects on the mental and physical well-being of children and families (Gruber *et al.*, 1999). Other studies have used the staggered timing of the legalisation of abortion in the US as a source of variation to study fertility effects (Levine *et al.*, 1999), the impact on female labour supply (Angrist and Evans, 1998) and to examine the effect on criminality rates (Donohue and Levitt, 2001).

This paper also connects to the literature on the analysis of the changing social structure of marriage. Edlund and Pande (2002) relate the decline in marriage to more left-wing voting for women through the shifting of the preference of the

⁴ Assuming that women are the prime carers for children and that education does not affect their ability to care for them, the optimal choice of education for men and women is only equal when women choose not to have children.

⁵ In the empirical analysis I avoid imposing this assumption and I explore it directly in the data.

median female voter towards more redistribution. Finally, the paper relates to an emerging literature in economics that infers welfare changes from self-reported well-being answers. Among others, see Di Tella *et al.* (2001) and Gruber and Mullainathan (2002).

2. Data and Empirical Strategy

2.1. *Data Description*

I analyse individual-level data from the Eurobarometer survey for twelve European countries collected between 1975 and 1998. The Eurobarometer asks over 450,000 individuals in repeated cross-sections the same question: ‘On the whole, are you *very/fairly/not very/not at all* satisfied with the life you lead?’. The answers are coded in four ordinal categories. The life satisfaction variable does not exhibit any systematic trend over the period, neither for men, nor for women.

On whether subjective survey data can and do provide true signals of welfare (Bertrand and Mullainathan, 2001), the fact that the ‘treatability’ is not systematically correlated either with other observable characteristics or with the measurement error of welfare allows predictive power in explaining an attitude. Moreover, the framing is constant and any additive bias is controlled for by country and year fixed effects.

Detailed data on abortion laws come from United Nations (2002). Precise and comparable data on contraception policies have proved more problematic to find, especially on the year of licensing of the pill in each country. Data on the year that the contraceptive pill started to be provided through the national public health system are used in the analysis.

The fact that Western Europe is relatively homogeneous with regard to the actual *use* of birth control rights ensures comparability. Abortion is mainly used as a tool of family and career planning rather than as a means to control family size among married couples (Henshaw, 1990). Moreover, abortion and the pill are provided at a symbolic or null cost in all of these countries (David, 1992), eliminating concerns of rationing or budget constraints.

2.2. *Empirical Strategy*

By using the introduction of birth control rights as a *treatment* and the fact that such policies have been adopted at different points in time by the countries considered, the question can be addressed within the framework of a standard evaluation problem.⁶ Since the policies exhibit a sizeable variation and affect a random selection of individuals chosen by demography, they provide a natural setting to estimate differences-in-differences effects.

A concern is that there may be a unifying theme underlying legislation that could make the policies endogenous to the other variables considered. The idea that the timing of abortion legislation followed a very clear set of motives, be them

⁶ See Heckman *et al.* (1999).

political or societal, is not supported by the data. The correlation between the timing of adoption of birth control rights and the life satisfaction in the country, the dominant religion or the number of women in parliament is generally low. Certainly these changes did not happen in a vacuum, but no single factor is by itself a good predictor of the law changes. Historical circumstances have played a large role.⁷ The Appendix reports the timing. In addition, most changes in laws occurred before the recording of life satisfaction and could not be affected by the dependent variable by way of reverse causation.

The combination of date of birth, gender and country of residence determine who is exposed to the treatment, such that individuals cannot self-select into the policy. Birth control rights can only affect individuals of a certain gender and age, that is, women of childbearing age. Matching this information with the temporal and spatial variation of birth control policies in Europe naturally creates groups a *treatment group*, i.e. women who were of childbearing age⁸ when birth control rights were introduced, and a *control group*, which comprises either

- (i) women who were over childbearing age when the law changed or
- (ii) women of any age who were living in countries where birth control rights were not present when they were interviewed.⁹

Since the timing of policies is different, different cohorts are contrasted in each country. The treatment is to be able to take advantage of the rights and not actually to use them. Being able to plan one's life with access to these rights is what matters.

The estimation compares the difference of self-reported life satisfaction between treated and control individuals when the policy was in operation, to the same difference calculated at a point in time previous to the introduction of the policy. This is the *effect of treatment on the treated*, i.e. the average difference between treated and untreated outcomes across the population and over time within the same country.

It can be written as:

$$E(\Delta_t | R = 1) = E(W_t^R - W_t^{NR} | R = 1)$$

where W represents the outcome of interest (welfare), R (for 'Rights') is the treatment, the superscripts R and NR refer to the treatment status ('Rights' and 'No Rights' respectively) and R is the indicator for treatment.

The identifying restriction is that the average outcomes for treated and controls would have followed parallel paths over time in the absence of the treatment, i.e.

⁷ Germany passed full rights as the outcome of a negotiation with the more liberal East Germany's legislation following the unification. In Greece, Spain and Portugal it was an adaptation of the national *corpus legis* to the European one at the time of the accession into the European Union (although it was not a required step, as the opposite behaviour of Ireland shows). In Italy a Constitutional Court ruling opened the way to a more open legislation.

⁸ I follow the medical literature in setting the end of childbearing age at 50 years. The results hold if an alternative age of 45 is adopted.

⁹ Distinguishing between these two sub-groups does not yield differences that are statistically significant and thus are gathered in the same group in the analysis.

the mean change in the no-programme outcome measure is the same for treated and non-treated.

$$E(W_{t_1}^{NR} - W_{t_0}^{NR} | R = 1) = E(W_{t_1}^{NR} - W_{t_0}^{NR} | R = 0)$$

where t_0 and t_1 are points in time before and after the introduction of the policy ($t_1 > t_0$).

The life satisfaction variable measured for women presents no cohort effects but marked age effects. The coefficients in a regression of life satisfaction on ten-year cohorts of women, plus the usual fixed effects, reveal that the effects from each cohort are not statistically different from one another. Instead life satisfaction displays a U-shaped trend with respect to age, with a minimum around 55 years old, similar to previous studies.¹⁰ If we estimate the effect of age on life satisfaction allowing it to be country-specific, the equality of the coefficients cannot be rejected at the 1% level. Welfare can thus be described as a function of age and the participation in the programme:

$$W = g(\text{age}) + \beta l(R = 1) + u$$

where $g(\cdot)$ is a general function of *age* of individuals, common across countries, β is the change in welfare occurring for treated individuals, l is the indicator function for being in the treatment group and u is a country-specific unobservable.

The effect β is identified by the differences-in-differences estimator:

$$\begin{aligned} & [E(W_{t_1}^R | \text{age}_{t_1}) - E(W_{t_0}^{NR} | \text{age}_{t_0})] | (R = 1) \\ & - [E(W_{t_1}^{NR} | \text{age}_{t_1}) - E(W_{t_0}^{NR} | \text{age}_{t_0})] | (R = 0) \end{aligned}$$

which is equivalent to

$$E(\Delta_t | \text{age}_t, R = 1) = E(W_t^R - W_t^{NR} | \text{age}_t, R = 1).$$

This measures the average gain in welfare for women who were exposed to birth control rights compared to what they would have experienced in the base state.¹¹

In the absence of individual panel data, the distribution of the benefits *within* the treatment group remains unknown. Characteristics that could be endogenously affected by the extension of rights (marital status, education, occupation) are only used descriptively (without claiming causation) to identify groups of individuals who exhibit higher levels of life satisfaction.

The base specification for the regression model is the following:

$$\begin{aligned} W_{ict} = & \alpha + \beta_0 x_{ct} + \beta_1 g_{1ict} + \beta_2 g_{2ict} + \beta_3 \eta_{it} \\ & + \beta_4 \delta_c + \beta_5 \tau_t + \beta_6 (\delta_c \text{Year}) + \varepsilon_{ict} \end{aligned} \quad (1)$$

where W_{ict} denotes a four-category indicator of welfare of individual i living in country c in year t , x_{ct} is an indicator for living in a country that has birth control

¹⁰ Blanchflower and Oswald (2004) report a minimum in the late 30s-early 40s in Britain.

¹¹ The differences-in-differences estimator gives an estimate of the *direct effects* of the policy on individuals explicitly treated. It is possible that the rights operate with network effects and that access to rights by one's neighbours affects one's welfare, for instance that the welfare of a woman in the control group is enhanced by her daughter's treatment. This idea is not developed here.

rights at time t , g_{1ict} identifies treated women, g_{2ict} is a dummy for the women in the control group and η_{it} are dummies for the age of the individual at time t in ten-year spans.¹²

To control for observed and unobserved heterogeneity, all regressions include country effects (δ_c) to capture time-invariant differences between countries that passed such laws and those which did not; year effects (τ_t) to control for general trends in extending abortion; a country-specific linear trend to allow country effects to change over time, to capture country-specific trends in attitudes towards women's rights.¹³ Since observations are independent but not identically distributed, to correct for aggregation bias (Moulton, 1990) standard errors are clustered by country and year in all specifications.¹⁴

The fact that women may travel to a different country to have an abortion could confound the identification. If concrete, this possibility would only induce a downward bias in my estimates, which would then be conservative, without affecting their direction. Despite the publicity surrounding these cross-border movements, they are costly in terms of money, time and information and are really only available to a small fraction of the population. The UK and Ireland can provide an idea of the relevance of this effect. Ireland has never allowed abortion while the UK has legalised it since 1967. Travelling costs are low and the collection of information is easy thanks to the same language. Yet, even if all abortion performed on non-residents in the UK were carried out on Irish women, this would amount to treating 0.4 to 0.7% of women in Ireland (ONS, 2001).

All regressions have been run both as an ordered probit and as a linear probability model (LPM). Since the categories of the dependent variable are ordinal, the coefficients from the ordered probit model are more correct than those from OLS. On the other hand marginal effects are difficult to report parsimoniously for a four-outcome variable. Angrist (2000) shows that if the focus is on directly interpretable causal effects rather than on structural parameters, the two approaches are largely comparable. Thus I report marginal effects when describing the magnitude of the effect and results from the linear probability model in all other cases.

3. Main results

3.1. *The Effect on Welfare*

Table 1 reports the estimates of the effect of birth control rights on life satisfaction from the linear probability model.¹⁵

¹² Dummies for being ≤ 19 , 20–29, 30–39, ... 60–69, ≥ 70 years old. The results are robust to the inclusion of the continuous variables 'age' and 'age squared' instead of the above.

¹³ A quadratic trend yields no additional information.

¹⁴ The results are substantially unchanged when standard errors are clustered by country to address the other potential bias of potentially serially correlated outcomes (Bertrand *et al.*, 2003).

¹⁵ All regressions control for age, country and year effects, a country-specific linear trend, and cluster the errors on country and year to obtain robust standard errors. The F-tests on each set of controls, including fixed effects, reject throughout all regressions that they could be jointly null.

Table 1
The Effect of Abortion Rights on Welfare, (differences-in-differences estimates)

	Pill			Abortion rights			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Depvar: Life Satisfaction (mean = 3.043)							
Pill in public policy at time of survey	-0.165 (3.79)	-0.196 (4.52)	-0.196 (4.54)	-0.185 (4.29)			
Abortion rights at time of survey	0.080 (3.91)				0.076 (3.74)	0.077 (3.76)	0.083 (4.11)
All women	0.016 (4.19)						
Treatment group: women with rights when childbearing age		0.027 (6.75)			0.028 (6.90)		
Control group: women without rights or not of childbearing age with rights		-0.028 (4.14)	-0.034 (4.80)	-0.031 (4.62)	-0.005 (0.72)	-0.007 (1.02)	-0.005 (0.73)
Treatment group: women who received rights when <i>less than 25 years old</i>			0.041 (8.98)			0.039 (7.81)	
Treatment group: women who received rights <i>between 25 and 35 years old</i>			0.031 (5.39)			0.042 (7.30)	
Treatment group: women who received rights <i>between 35 and 50 years old</i>			-0.002 (0.26)			0.002 (0.42)	
Treatment group: No. years that women enjoyed rights				0.004 (5.61)			0.007 (7.00)
Treatment group: No. years that women enjoyed rights - squared				-9.40e-5 (3.51)			-2.71e-4 (5.39)
Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country effects, Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.13	0.13	0.13	0.13	0.13	0.13	0.13
Obs.	459,953	459,953	459,953	459,342	459,953	459,953	459,476

Notes: t-statistics in parentheses from standard errors adjusted for clustering (250 country-year clusters). Sample period is 1975–98 for 12 countries. Fixed effects are significant at the 1% level in all specifications.

Column 1 shows the general result that on average women report more welfare than men, consistently with the previous literature.¹⁶ Column 2 reports the differences-in-differences effect of having access to the pill. The estimated coefficients show that women in the treatment group systematically report higher levels of life satisfaction than the rest of the population. This is not true of other women, who report a negative and significant coefficient.¹⁷ The Wald test for the estimated difference between treatment and control group of women is significant at the 1% level.

Column 3 investigates whether the treatment effect has been constant at the various ages at which women received access to the pill. Since younger women face a longer horizon over which to reap the benefits of education and fertility planning, the effect of birth control rights should be stronger, the younger the women when the rights are introduced. If instead the positive effect of birth control rights is merely one of psychological empowerment from having more individual rights, this effect should be constant at any age. Column 3 reveals that the largest part of the positive welfare effect is indeed on women who received access to the pill when they were less than 35 years old (the difference between receiving them before being 25 or 35 is not statistically significant). Women who received the pill between 35 and 50 years old do not report any significant change in welfare. This is consistent with an effect going through better life planning of investment in education and desired fertility, more than with a psychological empowerment of women.

In an alternative specification, column 4 defines the treatment group by the number of years that a woman had access to the pill while of childbearing age. Both a linear and a quadratic term are introduced. The estimates show that the welfare effect of having access to the pill is increasing and concave in the number of years. The maximum is reached after 21 years of access to the pill; after that it declines slowly.

Columns 5 to 7 apply the same regressions to the treatment 'having access to abortion rights'. Both quantitatively and qualitatively the results are very similar to those of the pill. The main difference is that the women in the control group report a negative but not significant effect. The coefficients on the treatment and control group are statistically different. As with the pill, women reaped the maximum effect on welfare if they had abortion rights before being 35 years old. After that the effect is insignificant. The effect is concave but with earlier gains and a faster decline with respect to the pill. The maximum effect of having access to abortion rights is after 13 years of treatment.

In all regressions, the estimated effect of abortion being allowed at the time of the survey is positive and significant whereas the effect of the pill being available is negative and significant. These effects are analogous to those of general public goods affecting the average level of satisfaction, independently of the private benefits at the individual level, but cannot be explained without further assumptions.

¹⁶ See Blanchflower and Oswald (2004).

¹⁷ Estimates of the same specification where the control group of women is broken down between women who did not enjoy abortion rights because of their age and those whose country did not provide abortion rights show that there is no difference in the welfare of the two groups.

3.2. *Economic Significance*

To describe the order of magnitude of the effect on welfare, I report the marginal effects from an ordered probit computed at the mean. Table 2 shows that being treated with access to the pill during childbearing age increases the probability of declaring being *very* satisfied with one's life by 1.5 percentage points. It decreases the probability of being *fairly* satisfied by 0.4 percentage points, of being *not very* satisfied by 0.8 points and being *not at all* satisfied by 0.3 percentage points.

Comparing the estimated effect with those obtained from personal characteristics, the welfare gain from access to the pill is equivalent to the return from achieving higher rather than middle education and a third of the size of the gain from being married or cohabiting. It is approximately one tenth of the effect of going up one level on a 12-category scale of income. It is one third of the corresponding welfare loss from being unemployed and one seventh of the loss from being separated.

4. Extensions

In this Section the basic specification is extended to take account of possible elements of heterogeneity and econometric concerns. These results are not reported in the Tables for brevity but are available on request.

4.1. *Men as a Control Group*

To the extent that they are partners of the treatment group, men could be affected by the policy changes and thus exhibit *indirect* welfare effects. An additional *men control group* can be identified, comprising men in the same cohort of age as the treatment group. They are directly comparable to women in the

Table 2
The Size of the Effect of the Pill on Welfare

Depvar:	Marginal effects from being...			
	Women in treatment group	Women in control group	Married or cohabiting	Separated
Y_4 : Very satisfied	0.015	-0.014	0.040	-0.101
Y_3 : Fairly satisfied	-0.004	0.003	-0.008	-0.003
Y_2 : Not very satisfied	-0.008	0.007	-0.022	0.069
Y_1 : Not at all satisfied	-0.003	0.003	-0.009	0.036
	Unemployed	Going up one income level	Having higher education*	Having low education*
Y_4 : Very satisfied	-0.047	0.173	0.016	-0.021
Y_3 : Fairly satisfied	0.010	-0.040	-0.004	0.005
Y_2 : Not very satisfied	0.026	-0.093	-0.009	0.011
Y_1 : Not at all satisfied	0.011	-0.039	-0.003	0.006

Notes: marginal effects from two probit estimations, one on treatment and control groups and one on personal characteristics. Both include age effects, country and year fixed effects, country linear trend. * = compared to middle education.

treatment group for being at the same stage of the life cycle, while they may differ for gender effects. When included in the base specification they do not show any significant difference with respect to other men, while the effects on treated and control women are unchanged. This could be read as an indication that there is no effect, or alternatively as the result of two balancing effects: higher welfare from more educated and more satisfied partners, lower welfare from lower bargaining power in the household.

4.2. *Consistency with the Economic Model*

The data allow a test of whether declared welfare matches revealed preference indicators that are associated with it. I apply the same policy experiment with differences-in-differences to observe whether women with birth control rights exhibit different choices of education, work and income, with the same specification as equation 1.¹⁸ I use probit when the dependent variable is the probability to achieve higher education or to work.

The effects are shown in Table 3 and are largely comparable for the three dependent variables. Women always show a lower probability than men to study and work and a lower income, but treated women perform better than the average and the control group fare worse. These differences are statistically significant at the 1% level.

Being a woman decreases the marginal probability of attaining higher education by 5.5 percentage points but this probability falls to 3.2 and 4.1 percentage points for women who respectively had access to the pill and to abortion (columns 1 to 3). Control women are the least likely to achieve higher education. In the second set (columns 4 to 6), women in general have 35.8 percentage points lower probability of being in work than men, but this probability falls to 34.9 for women with the pill and 32.3 for women with abortion rights. In the third set (columns 7 to 9) the dependent variable is personal income on a 1 to 12 scale. Women are more likely to end up in lower classes of the income ranking. Again, women with birth control rights have a much better standing compared to control women.

The estimates show that women who had access to birth control rights when they were 'young enough' changed their education and work choices. The empirical results are tightly consistent with the economic framework according to which birth control rights are expected to raise female investment in education, labour supply and income.

4.3. *Distribution of Gains and Losses*

It would be useful to derive some results on the distribution of the welfare gain *within* the treatment group. Unfortunately we do not know the characteristics of the *same* women before and after the treatment, thus we cannot say whether their choices of marital status, education, occupation were endogenous to the set of

¹⁸ The only difference is that 20 years is the threshold age for being treated when analysing education to keep consistency between the timing of opportunities and choices.

Table 3
Consistency with the Economic Model: the Effect of Abortion Rights on Education, Probability of Working and Level of Income

Depvar:	Prob(having higher education)			Prob(working)			Personal income position		
	(1)	Pill (2)	Abortion rights (3)	(4)	Pill (5)	Abortion rights (6)	(7)	Pill (8)	Abortion rights (9)
Pill in public policy at the time of the survey	-0.015 (1.49)	-0.016 (1.30)		0.004 (0.26)	-0.003 (0.20)		2.561 (4.26)	2.291 (3.97)	
Abortion rights at the time of the survey	0.011 (1.44)		0.011 (1.48)	-0.026 (2.62)		-0.061 (5.37)	0.820 (2.99)		0.640 (2.40)
All women	-0.055 (24.50)			-0.358 (48.35)			-0.507 (27.57)		
Treatment group: women with birth control rights when childbearing age (or when ≤20 years old for education)		-0.032 (11.66)	-0.041 (13.67)		-0.349 (44.94)	-0.323 (39.33)		-0.411 (18.69)	-0.402 (17.99)
Control group: women without birth control rights or had them past childbearing age (or >20 years old for education)		-0.063 (29.24)	-0.056 (22.87)		-0.382 (26.20)	-0.378 (40.65)		-0.858 (19.81)	-0.687 (16.02)
<i>Age effects</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Country effects, Year effects</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Country-specific linear trend</i>									
Pseudo-R ² /R ²	0.12	0.12	0.12	0.26	0.26	0.26	0.19	0.19	0.19
Obs.	445,706	445,866	445,866	459,953	459,953	459,953	337,017	337,017	337,017
Clusters	238	238	238	250	250	250	228	228	228

Notes: t-statistics reported in parentheses, from standard errors adjusted for clustering on country and year. Sample period is 1975–98 for 12 countries. Columns 1–6 report marginal probabilities from probit estimations, columns 7–9 estimates from Linear Probability Model regressions. The personal income position is on a scale 1 to 12.

available rights. In the absence of longitudinal data on life satisfaction of individuals, it is only possible to give a descriptive, not causal, representation of some categories of women who have gained or lost from the institutional change.

Under both sets of birth control rights, women who are married or cohabiting, women who work and women with high or middle education ('women who could have it all') gained the most in terms of life satisfaction. Treated women who are single are no different from the control group with regard to welfare. Treated women with low education and/or who do not work have lower welfare than average. Treated women in the two lowest categories of income show the biggest loss of all (-0.24 percentage points), treated women in the two average categories report the average coefficient and treated women in the two highest categories report the largest gain (20 to 21 percentage points). All effects are statistically significant.

4.4. *Other Policies in Favour of Women*

The possibility of omitted variable bias is investigated by including other policies in favour of women that changed in the same years: mutual consent divorce laws and maternity benefits. Treatment and control groups are re-computed in an analogous way to birth control rights, according to whether women could take advantage of them when the law changed.¹⁹ The base specification is otherwise the same as in equation 1.

No-fault, or mutual consent, divorce laws removed the disparities between men and women in the event of a divorce. These laws might have increased women's life satisfaction by removing a constraint on choices. On the other hand, since the existence of divorce is associated with being able to renege on a previous choice, its welfare effects may also be negative. Becker *et al.* (1977) point out some ambiguous effects of marital instability on utility maximisation. If marriage is a contract with various non-contractible elements, making it easier to break leads the couple to be 'reluctant to invest in skills or commodities specific to their marriage if they anticipate dissolution: having children and working exclusively in the nonmarket sector are two such marriage-related activities' (Becker *et al.*, 1977). Less time and fewer resources invested both in the search and in the marriage itself eventually lead to lower utility from marriage at the time of dissolution than expected at the time of marriage. The estimates show that the effect of no-fault divorce is negative and significant on treated women, and positive and significant on society as a whole.

For maternity protection policies, I examine how benefiting from 'full-pay maternity leave'²⁰ interacts with birth control rights. In principle, the two could be substitutes.²¹ Women who benefit from good maternity protection do not need birth control rights to plan optimally their education and work choices because

¹⁹ For divorce law there is obviously no threshold age for being *treated*.

²⁰ Computed as the number of weeks of paid leave times the average wage replacement rate. The results are robust to alternative indicators of maternity protection (wage replacement rate, number of weeks of job-protected as well as non-protected leave).

²¹ Women are expected to bear the main impact of maternal leave changes as, even where parental leave equally applies to fathers, it is they who take the majority of the leave (95%).

children, even if unplanned, would be neutral with respect to the decision to study for higher education and work. In this context birth control would be superfluous unless there is a taste for planning the timing and spacing of children.

There is scant evidence that the two policies acted as substitutes. High maternity protection did not affect the welfare of women receiving it and no specific effects arise from the interaction of birth control rights with 'long' maternity policies (where long means 'above the average'). Nevertheless their effect should be seen in the context of a general equilibrium model, where disincentives for firms to hire women of childbearing age bite more, the more generous maternity policies are.

When the four policies are included as different treatments in the same regression, they show little collinearity. The welfare effect of birth control rights over and above the others remains positive and significant and the effect of abortion and the pill is not statistically distinguishable from one another. Another possible factor affecting the life satisfaction of women could be technological progress, which decreased the effort required to perform most tasks, including household tasks. This effect is captured by the country-specific linear time trend that all regressions control for. On the possibility that it is 'feminism' that drove both women's life satisfaction and the adoption of birth control rights, Becker (1981, p. 251) argues 'the [women's] movement is primarily a response to other forces that have dramatically changed the role of women rather than a major independent force in changing their role.' Without giving it empirical content, it is not observationally distinguishable from that of a general or country-specific linear trend.

4.4.1. *Individual religiosity information as a test for omitted variable bias*

Catholic, Greek Orthodox, Orthodox Jewish and Muslim religions explicitly ban birth control in the form of pill and abortion in all instances. The fact that some religions oppose outright birth control rights and that religious women may voluntarily give up these rights even when their country allows them provides another interesting natural experiment. Within the same country we can find women otherwise comparable whose religion exposes them to different sets of rights. If treated women from these religions do not exhibit higher life satisfaction than the control group, while the rest of the treated women does, it further proves the causality of birth control rights on welfare.

The data confirm that treated women who define themselves as Greek Orthodox or Jewish are not any more satisfied than the control group. Muslim women report a loss in welfare compared to the control group. This is consistent with the idea of Akerlof *et al.* (1996) that women who choose not to take advantage of rights may be worse off than without rights. Catholic women instead report a higher welfare, in line with rest of the treatment group. This is consistent with findings from the sociology of religion that on family planning even the most fervent Catholic women adopted a stance of independence from the doctrine. These results may be a further test that the effects found so far truly arise from birth control rights and not from some omitted underlying variable.

4.5. *Variations in Treatment Effects by Religion*

The effect of the treatment may differ across institutional settings. Europe has deep institutional roots in Christianity. While the Roman Catholic and the Greek Orthodox Churches see the pill as not acceptable and abortion as a crime to be punished with excommunication, the Protestant Church and the Church of England leave it to conscience. Consequently societies that are predominantly Catholic or Orthodox²² tend to have a very polarised view of reproductive freedom. In the same countries may coexist non-religious people who abide to the laws of the state, Catholics who abide to the Catholic Church law and Catholics who despite their religion choose to follow the state laws on this issue. Since the starker contrast between state and church law is on abortion, I examine whether the effect of abortion rights differs across the two settings.

While the main results are unchanged in both sets of countries, Catholic countries present a much larger general effect of abortion rights at the time of the survey. It may suggest that these countries, where women achieved economic and political rights later, experienced a wider societal effect of breakup of social norms from the law change. In non-Catholic countries instead the effect is concentrated on private gains to the treated individuals.

Concerns about whether the Catholic church has retarded the *timing* or the *content* of laws instead do not hold, as predominately Catholic countries are equally present among early adopters (France, Italy), as among late adopters (Belgium, Greece) in granting full rights, i.e. abortion on demand.

4.6. *Robustness Checks*

- (1) United Nations (2002) states that before abortion laws changed, Greece, Belgium and the Netherlands were known to have widespread underground abortion and not to enforce the ban. Thus we can distinguish ‘false’ from ‘true’ law changes. Indeed, a regression of the basic model on these three countries shows no significant effect of abortion on women neither on the treatment, nor on the control group. On countries where the law change was effective, instead, the basic model yields an even stronger positive welfare effect on the treatment group.
- (2) Abortion rights have been granted in two basic forms, ‘partial rights’, i.e. effective only in case of a health threat to the woman or to the child, and ‘full rights’, i.e. the right to abort on socio-economic grounds or simply on request. Some countries have granted partial rights only, others have later moved to full rights, others have leapt directly from no rights to full rights. A regression with an indicator for each set of rights shows that the effect is positive and not statistically different on the women treated. This is not surprising as partial rights often allow obtaining full rights in practice.

²² These are Belgium, France, Italy, Luxembourg, Ireland, Greece, Spain, and Portugal. *Non-predominantly Catholic* countries in the sample are the Netherlands, Germany, Denmark, and UK (from *CIA Intelligence Factbook 2002*).

Once abortion is seen as a socially acceptable course of action and some rights are allowed for, the 'mental health' protection ground for abortion can be extended to include wider reasons.²³

- (3) Donohue and Levitt (2001) argued that the crime rate in the US has declined significantly as a result of the legalisation of abortion as fewer unwanted children were born. This would suggest that the population contained as a result a higher proportion of 'happy' people, biasing the results. The positive effect of abortion rights on the treated is robust to excluding from the sample individuals born *after* abortion rights were introduced.

5. Conclusion

This paper evaluates the private benefits to women arising from changes to their set of incentives and choices from birth control policies: abortion rights and the endorsement of the pill in national public policies. It analyses answers by over 450,000 individuals from 12 European countries between 1975 and 1998 reporting a self-evaluation of their life satisfaction.

The main finding is that following the introduction of birth control rights, women who could take advantage from the policy (that is, they were of childbearing age at the time the policy was introduced) consistently registered an increase in welfare. The magnitude of the welfare gain is equivalent to the return from achieving higher rather than middle education and a third of the size of the gain from being married or cohabiting. It is approximately one tenth of the effect of going up one level on a 12-category scale of income. It is one third of the corresponding welfare loss from being unemployed and one seventh of the loss from being separated. Other women and men have not reported any significant effect.

The effect on women in the treatment group is stronger, the younger were the women when they received birth control rights and the longer they were exposed to them. Marginal returns start to decline after the woman is 35 years old, or after 21 years of pill/13 years of abortion rights.

Life satisfaction effects are consistent with changes operating through economic choices. The data strongly confirm that birth control rights caused an increase in women's investment in education, probability of working and income level.

Women professing religions that are firmly against birth control rights did not exhibit a change in their welfare.

At the same time, other women's rights have been less beneficial. The analysis shows that mutual consent divorce laws have decreased women's welfare, while granting high maternity protection in the workplace did not have significant effects, possibly because of negative feedback effects on the 'employability' of women.

Econometric concerns of endogenous legislation are discussed but they do not appear to apply. A series of robustness checks and sensitivity analysis is presented.

²³ This is often argued in the case of Spain nowadays, which only grants partial rights, but where abortion is effectively available on demand in private clinics.

In addition to their historical importance, these results may provide some guidance on the effects of granting women's rights towards development goals of empowerment. A third of the countries in the world, mainly developing ones, representing a quarter of the world population, do not have birth control rights at all and similar statistics apply to women's rights in general. Although institutional differences have to be taken into account before applying them to other societies, this analysis suggests an important link between providing individual rights like birth control to women, favouring their empowerment in other fields and increasing their welfare.

A question not addressed in this paper is why birth control rights are not always granted, if it is true that they generally improve welfare. It would be interesting to endogenise the law as part of a political economy process. One possible explanation could be linked to the median voter having different preferences from the 'average' one, motivated by a political system where men are over-represented. Another extension of the analysis is to identify the distribution of the gains and losses from the policy. Panel data at the individual level would be required to estimate this effect.

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Appendix: Data Description and Sources

Eurobarometer (version Mannheim Eurobarometer Trend File 1970–99)

Countries: Belgium, Denmark, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, United Kingdom.

Years: 1975–98 except Greece (1981–98) and Spain and Portugal (1985–98).

Life satisfaction: four-category variable based on the question: 'On the whole, are you very/fairly/not very/not at all satisfied with the life you lead?'. The distribution of the answers is 'Very satisfied' 27.63%, 'Fairly satisfied' 54.09%, 'Not very satisfied' 13.63% and 'Not at all satisfied' 4.65%.

Personal characteristics: dummies on gender, whether the person is working, married/cohabiting or single, level of education,²⁴ personal income on a 12-point scale.

Age: dummies for age less than 20, 20–29, 30–39, 40–49, 50–59, 60–69, over 70.

Religious denomination: the Eurobarometer asks: 'Do you regard yourself as belonging to a religion?'. Answers are coded separately for Catholic, Greek Orthodox, Jewish, Muslim, Protestant and Other Protestant (including Church of England).

Sources of policies

Abortion laws: from United Nations (2002); see also Rahman *et al.* (1998).

Date of inclusion of the pill in national health planning: from Jones *et al.* (1989), United Nations (2002).

No-fault divorce: from Edlund *et al.* (2003).

Maternity leave benefits: data kindly provided by Christopher Ruhm; see Ruhm (1998).

Percentage of women in parliaments: from Inter-Parliamentary Union (1995), *Women in Parliaments 1945–1995*, Geneva.

²⁴ Based on age when finished full-time education: *low* ≤ 15, *middle* 16–19, *high* ≥ 20.

Table A1
Women's Rights in Europe, 1960–98

	Abortion partial rights (health grounds)	Abortion full rights (on request)	Pill as part of public policy	No-fault divorce
Belgium		1990	1973	1975
Denmark		1973	1973	1969
France		1975	1967	1975
West Germany	1976	1995	1975	1976
Greece	1978	1986	1980	1983
Ireland	no	no		1995
Italy		1978	1971	1971
Luxembourg	1978			1975
Netherlands		1981	1969	1971
Portugal	1984		1976	1975
Spain	1985		1978	1981
United Kingdom		1967	1961	1971

Note. Sources are provided in the Appendix.

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