The effect of women's rights on women's welfare: evidence from a natural experiment

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Abstract

Between the late 1960s and the 1990s vast changes in social norms and institutions took place in relation to women's rights. This paper explores the issue of whether women's rights have brought women higher welfare. Using individual level data on life satisfaction and focusing on the staggered timing of law changes on abortion rights in twelve European countries, I obtain an average treatment effect on the treated from differences-in-differences. The identification strategy uses the fact that exposure to women's rights varied by gender, country of residence and date of birth. I show that the extension of abortion rights is strongly linked to an increase in life satisfaction of women of childbearing age. The introduction of the pill in national public policies had an analogous effect, while mutual consent divorce laws decreased women's welfare. Being in a country with high maternity protection does not affect the results. These findings are true after controlling for age effects, unobserved heterogeneity across countries and time, and country-specific trends. It is robust to various econometric concerns.

Keywords: Natural experiment, Differences in differences, Women's rights, Welfare, Life satisfaction.

JEL: I38; J16

1 Introduction

An advertisement for an American department store in 1950[1]:

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 American family planning poster in the early 1940s[2]:

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.

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 visible and started to be influential and progressively represented in the political decision power. Equal treatment and reproductive rights became two hotly debated topics of political discourse and laws to address them 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 a priori bet on the direction and the size, if any, of these effects. The previous literature has elaborated theoretical models, but no empirical evaluation has been undertaken, to my knowledge. With some exceptions, economists generally expect a resulting increase in welfare from policies that removed binding constraints on women's choices.

In the public discourse the opinion is fragmented. It is not clear whether women have really achieved parity in the public sphere, and if this is the case, whether the double role of being the primary caregiver in the family and a worker has not just imposed a double burden on women.[3] It is often argued that women "could not have it all" and that they are not better off now.

As a first step in the assessment of the effect of women's rights, I focus on the policy of extending birth control rights, and in particular abortion rights, on women's welfare in Europe. Between the late 1960s and 2000, following two thousand years of non regulation and only 150 years of regulation[4], most Western countries legalised abortion with a large

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[1] Reported by Watkins (1998), p.9.

[2] Reported by Marks (2001), p.21.

[3] The debate talks about the "struggle to balance work and care-giving" (Institute for Women's Policy Research, online) and "*having-it-all* has changed to *work-life balance*" (*Guardian*, 13.9.2003).

[4] 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 through the Middle Ages to the eighteenth century. It was only in the nineteenth century that abortion came to be prohibited by law and contraception looked at negatively in most Western countries.

heterogeneity of timing and forms.

The theoretical channel of influence of birth control rights on welfare and utility considered here is that, by changing the predictability in the timing of childbearing, they affect the planning horizon that women consider when optimising their choices of investment in education and work. Thus an increase in birth control possibilities raises the optimal choice of education and improves the job market prospects of women. In addition, men could be better off when reproductive rights exist because they obtain a better match in the marriage market.

In a natural experiment, I exploit the exogenous natural variation given by the staggered and uncoordinated legalisation of abortion in twelve European countries between 1967 and 1998.

I concentrate on private benefits to individuals, stemming from changes to their set of incentives and choices, rather than on societal or "public good" effects. The analysis does not focus on the general effect of deriving utility (or disutility) purely from the fact that such laws exist, for instance, because the individual has a taste for individual rights, or because he considers abortion negatively on moral grounds and thus receives disutility. This does not engender systematic changes in behaviour.

The identification of the welfare effect comes from the fact that the exposure of an individual to abortion rights varied by gender, by country of residence and date of birth. A differences-in-differences estimator allows to identify the effect of laws passed by certain countries at different times and which affected particular groups of individuals, women of childbearing age *(treatment group),* with respect to control groups. Two control groups are identified. The first comprises women who were not exposed to abortion rights, 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. Controlling for age effects, it is shown that this set is comparable to the treatment group. The second control group consists of men in the same cohort of age as the treatment group (i.e. less than 50 years old) when birth control rights were introduced. Controlling for possible gender effects, the two groups are comparable for what concerns welfare in terms of life cycle events. Statistical techniques, the use of fixed effects and the inclusion of individual controls in the analysis make the results robust to elements of heterogeneity, such as additive systematic variation of life satisfaction across countries and, to some extent, across individuals.

The main finding is that women who could be affected by birth control rights, i.e. were 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 control experiments.

The magnitude of the welfare gain is equivalent to the gain from going up one level on a 12-category scale of income (this effect is largely constant along the income scale)

or of having higher rather than middle education. It is smaller (around one seventh) than the corresponding welfare loss from being unemployed or separated.

Additional sources of variation in the application of the policy are considered, such as the number of years that women have been exposed to the policy, their age when abortion was legalised, the "intensity" of rights, given by the distinction between partial and full abortion rights, and the role of religious institutions and religious membership. Religious institutions may have hindered the adoption or the application of the laws in some countries, while religious membership, by creating a parallel set of laws for the individual, may have affected the individual willingness to consider birth control rights as an option.

Concerns about the role of concurring policies in favour of women are addressed, examining the effect of the introduction of the pill in national public policies, changes to divorce law and the interaction of abortion rights with maternity leave policies provided in the same country.

In the empirical literature, there is no direct study of the link between reproductive rights and individual welfare. This paper appears to be the first to engage in the empirical evaluation of the effect of birth control rights on women's welfare.

The remainder of the paper is organised as follows. The next section reviews how the paper relates to the literature. Section 3 presents a simple model on the link between birth control rights and the individual optimal choice of education, from which empirical predictions can be derived. Section 4 describes the data and the empirical strategy. Section 5 presents the main empirical results. Section 6 extends the results in various directions. Section 7 concludes. The appendix lists the sources of all variables used in the analysis.

2   Related literature

There are two opposing perspectives in the economic literature on the theoretical link between welfare and birth control rights. The dominant one is that birth control and abortion rights have shifted out the frontier of available choices and thus could only increase women's welfare. The opposing view is that the same rights have weakened the bargaining position of women in the marriage market and lost protection for their gender-specific role of mother, ultimately losing welfare.

Goldin and Katz (2002) are an example of the first school. Their hypothesis is that the pill allowed sex without commitments and lowered the cost of delaying marriage. Thus it allowed young and single women to invest more in graduate and professional education and achieve better careers, and still obtain a good match in the marriage market. The match in marriage could even possibly be a better one, due to a "social multiplier effect" that made the market for "career women" thicker. They use this

framework to test whether in the United States access to the pill by women when they were 18 to 21 years old affected their decision to go to university and to marry later and find positive results.

The other strand in the literature emphasises the adverse welfare effects of birth control possibilities on women. Akerlof, Yellen and Katz (1996) set out to explain the "feminisation of poverty" in the United States as a consequence of birth control innovations. They link the large rise in out-of wedlock childbearing in the US between 1965 and 1990 and the decline of the "shotgun marriage", i.e. the bundling of sexual commitments with commitment devices. Their finding is only applicable to a specific set of circumstances. They focus on men who never prefer to marry and women who prefer marriage in every period. This obviously creates an initial mismatch and competition of women for the available men. Moreover women are homogeneous and cannot differentiate themselves in the marriage market (e.g. through education). When birth control exists, the equilibrium shifts to a situation where women lose the possibility of demanding marriage in exchange for sex, thus lose the transfers brought by marriage. Those who lose more are those who fail to adopt birth control and end up with a child born out of wedlock and without the income brought by a marriage. In this framework, when abortion and birth control became available, women invested more in human capital because they expected less rents from marriage.

According to Siow (2002) the parameter that is key to the welfare calculation is the relative supply of marriageable men to marriageable women . In general, as women are fecund for a shorter period than men, women are relatively scarce in the marriage market and innovations in birth control improve their welfare. Only if the supply of marriageable men is extremely scarce, with birth control technologies, fewer transfers are needed to induce women to cohabit or to get or to stay married and their welfare is decreased.

On the empirical side, there is no direct study of the link between reproductive rights and welfare, although there is a vast literature on unintended pregnancies and their negative effect on well-being of children and families, both mental and physical (e.g., Gruber, Levine and Staiger, 1999).

The use of staggered timing of the introduction of legalised abortion is not new in the literature, although so far it has been limited to the US and the *Roe vs. Wade* case. It has been used as a source of variation by Levine *et al* (1999) to study fertility effects, by Angrist and Evans (1998) to study the impact on female labour supply, and by Donohue and Levitt (2001) to examine the effect on criminality rates.

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 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 Easterlin (1974), Clark and Oswald (1994), Di Tella, MacCulloch and Oswald (2001), and Gruber and Mullainathan (2002).

3 The model

The three main channels through which birth control rights can affect individual utility are through a reduction in unwanted children, through a general empowerment of women, and through a better planning of education choices for women.

In the first channel, birth control rights diminish the portion of fertility that is undesired and lead to a direct increase of utility. This is trivial to model but difficult to test in the data. In future work, it would be interesting to include explicitly in the explanation two additional channels by which birth control laws can affect utility.

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 reproduction to women changed not only the individual choice of fertility. It also empowered women with a kit of social identities distinct from those related to the woman's role within the family. New social categories were born that women could confront themselves with. An attempt to identify this effect is made in the empirical analysis when dealing with Catholic versus Protestant countries, where the difference in the social categories considered appropriate for women is likely to be more pronounced.

The third channel lends itself to a clean analysis, both formally and empirically. Here I present a simple formalisation that links birth control laws, education choices and welfare changes for women and their partners, as it lends itself to predictions that can be tested by analysis.

There are N individuals of two types i, $i \in \{m, w\}$, where m denotes "man" and w "woman". The number of individuals of type m equals the number of type w. Each individual i maximises utility deriving from earnings (y_i) and the quality of the match in marriage (m_i), as defined below:

$$\underset{e}{Max} \left((1 - \rho) \, T \cdot \omega \cdot e_i - \frac{c \cdot e_i^2}{s_i} \right) + e_i^P$$

Individual income, y_i, is a function of the quantity of education e_i, its cost per unit c, the level of skills s_i that individuals are born with (with s_i uniformly distributed over $s \sim \left[s^L; s^H \right]$ in each period), the wage rate per unit of education and working time ω, the expected length of working life T, and an adjustment factor ρ which measures the

likelihood of unplanned fertility given country laws on birth control rights. The higher ρ, the higher the risk of an unwanted pregnancy for the individual. Since childbearing is known empirically to reduce both the length of working time and the wage[5], $(1 - \rho) T$ is the horizon over which individuals effectively plan their decisions and reap the benefits from their choice of education.

Country laws on birth control could possibly affect individual welfare in two ways. The "public good component" is the general effect of individuals deriving utility (or disutility) purely from the fact that such laws exist, regardless of whether the individual can make use of them. For instance, the individual may derive utility from it if he has a taste for individual rights, or he may receive disutility if he considers abortion negatively on moral grounds. The other component is a private utility one, affecting individual choices. The analysis on the parameter ρ identifies the private utility component of birth control rights, affecting the planning horizon of the individual. The public good component is not modelled here as it would not engender specific behavioural implications.

The quality of marriage, m_i, only depends on the quality of the partner as signalled by his or her education, e_i^P. I assume that the Becker model of marriage based on assortative matching holds (Becker, 1973).[6] In particular, prospective spouses do not choose themselves on the basis of randomly allocated love, but on their being similar in observable characteristics that signal underlying skills. I assume that investment in education is the only observable signal of skills. The equilibrium matching depends only on the relative position in the distribution of education (and skills) of that period, matching the most educated man with the most educated woman, and so on for all men and women in the ranking. This implies that i's choice of education determines both her stream of income and the type of person that she will marry.

The quality of marriage could in principle also include the utility from having children. 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 "production" of unplanned children. The implication is trivial. Introducing birth control rights reduces the probability of disutility from unplanned children.

Given the form of individual utility, the optimal investment in education is:

$$\frac{\partial y}{\partial e} = (1 - \rho) T \omega - 2 \frac{ce}{s} = 0$$

$$e^* = \frac{(1 - \rho) T \omega s}{2c}$$

The optimal investment in education is increasing in the length of working life, in the wage rate, and in skills. It is decreasing in the extent to which fertility cannot be

[5] See Waldfogel (1998) for a good review of results.

[6] As Fernandez, Guner and Knowles (2001) describe, this model is strongly corroborated in the empirical literature.

planned because of country laws, and in the cost per unit of education.

I am abstracting here from the possible endogeneity of education to marriage considerations. In theory and in practice, matching considerations could endogenously affect each individual's choice of education (Coles, Mailath and Postlewaite, 1992). That is, an individual could decide to invest more in education purely out of concern for improving his or her position in the marriage market. Coles *et al* (1992) show that if this possibility is allowed, two effects take place in equilibrium. First, everyone would (weakly) increase their choice of education.[7] Second, the relative position of the individual would not change.[8] If all individuals take into account how their education choice affects their relative ranking and adjust their behaviour accordingly, the net effect of their decisions is that noone moves up or down in the ranking over time.[9] Thus the ranking and the matching in the marriage market are the same whether I consider this mechanism or not.

3.1   The effect of birth control rights: comparative statics

I focus on the role of birth control rights (i.e. abortion rights and access to the pill) in affecting the planning horizon that individuals face when optimising their choice of education, given their skills. I assume that the political action of granting birth control rights is a matter of political economy, governed by where the median voter posits himself towards these rights as a public good.

The factor ρ affects the duration of working life and varies both by gender and as a result of laws, with $\rho = \rho_i^{laws}$, with $laws \in \{NR, R\}$, where NR denotes "no rights" and R denotes "rights allowed". Analogously, I denote $e_i = e_i^{laws}$ and $y_i = y_i^{laws}$, respectively the level of education and income in the presence of the two sets of laws.

Men have by definition a zero probability of becoming pregnant, thus a perfect control on the length of their working life with respect to childbearing ($\rho_m = 0$). In contrast, women without birth control rights always have a less-than-perfect control on the timing of childbearing, $\rho_w \geq 0$ (with equality holding only if the woman chooses to abstain from sex). Birth control rights can bring to zero the probability of an unplanned pregnancy and introduces variation in the optimal choice of education of women. The education choices of men and women with the same level of skills generally differ.

Let us define:

- With and without birth control rights, men have $\rho_m^{NR} = \rho_m^R = 0$. Men's optimal choice of education is $e_m^R = e_m^{NR}$.

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[7] Property 2 in their model.

[8] Property 1 in their model.

[9] Coles *et al* (1992) derive their results for constant relative risk aversion utility functions, but show that they apply to a broader class of utility functions.

- Without birth control rights, women have $\rho_w^{NR} \geq 0$. Women's optimal choice of education is e_w^{NR}.

- With birth control rights, women have $\rho_w^R = 0$ and $\rho_w^R \leq \rho_w^{NR}$. Women's optimal choice of education is e_w^R.

Comparing the outcomes with and without birth control rights, the following results can be derived.

Proposition 1 : *With birth control rights, holding the level of individual skills constant, men's optimal investment in education does not change, while women's optimal investment is larger or equal than without rights, i.e. $e_w^R \geq e_w^{NR}$. It follows that women's income is higher or equal with rights than without them, $y_w^R \geq y_w^{NR}$, while men's income is unchanged, $y_m^R = y_m^{NR}$.*

Coming to the marriage market, in equilibrium, given their skills, all women increase their choice of education, such that their ranking in terms of education is preserved. The ranking of men is also unchanged, since their investment in education does not change. Thus positive assortative matching in the marriage market brings together the same highest-ranking woman with the same highest-ranking man as before, and so on down the ranking. The only difference is that men find a match in a better educated woman than before, while women find a match in a man with the same level of education as before. Proposition 2 then follows.

Proposition 2 : *With birth control rights, men's utility from the match in marriage is larger or equal than without rights, while women's utility is unchanged.*

The concluding result is the one that will be tested in the empirical analysis.

Proposition 3 : *With birth control rights, both men and women's utility is larger or equal than without rights. For women the effect goes through higher education and higher income. For men the effect goes through finding a better match in marriage.*

This simple model describes one possible mechanism that links birth control laws and welfare changes for women and their partners via their education choices. The model reaches conclusions similar to Goldin and Katz (2002) with regard to the education choice of women. In their work on the United States, they show that the availability of the pill has significantly increased the investment in education of young women and their wage. This is equivalent to a test of proposition 1.

The model presented here ignores issues of public goods that could affect one's utility, pre-existing wealth and other material and immaterial goods that could enter the utility function. It also ignores the value of redistributive transfers that can take place within

the marriage and which alter the spouses' bargaining power. It simply models marriage as an institution within which men and women derive the same amount of well-being from mutual care. The simplicity of the model is clearly a limit, but I want to focus on the change in the planning horizon of women, a mechanism that has not been spelled out formally in the literature.

A fundamental 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. A microfoundation for this could be nested in a household model with unequal bargaining power in favour of men linked to a system of political representation where men are over-represented.

4 Data and empirical strategy

4.1 Data description

A relatively recent development in economics is the possibility to directly evaluate the welfare effect of policies by analysing surveys of life satisfaction. The answers of nationally representative samples of individuals about their current life satisfaction are used as revealed subjective utility levels. As long as these surveys are carried out in an intertemporally constant way, they can provide a rich and consistent source of information on welfare trends.

This source of data has the unique advantage of matching welfare levels with individual characteristics. This combination of information allows to measure the effect of government interventions or market changes on the welfare of very specific groups of individuals.

To test for the effect of birth control rights on welfare I analyse individual-level data from the Eurobarometer survey for twelve European countries[10] for the period 1975-1998. The Eurobarometer has the unique advantage of providing consistent time series for European countries, which present a wider variation in legal changes with respect to abortion than the United States. This allows to identify the effect more precisely. It also makes the problem of omitted variables less likely. Other international social surveys do not provide data from as far back in time (ISSP since 1985, World Values Survey since 1980).

The Eurobarometer asks repeated cross-sections of individuals (totalling over 450,000) a question on happiness and one on life satisfaction. The two are strongly correlated and I use the data on life satisfaction, as it is more of a long-term indicator of welfare. The

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[10]Belgium, Denmark, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, and United Kingdom.

survey asks: "On the whole, are you very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the life you lead? Would you say you are...?" The answer could be provided in four categories (plus the "don't know" option): 1. Very satisfied, 2. Fairly satisfied, 3. Not very satisfied, 4. Not at all satisfied.[11]

The Eurobarometer also provides information on the gender of the respondent, his or her marital status, age, occupation and religious feeling or membership. In this analysis, exogenous characteristics like gender and age help identify the treatment and control groups. Characteristics that could be endogenously affected by the extension of rights are only used descriptively here, without claiming causation, to identify groups of individuals who exhibit higher levels of life satisfaction.

The question sometimes arises among economists of whether subjective survey data can and do provide true signals of welfare (Bertrand and Mullainathan, 2001). In theory, one possible measurement problem could arise from the framing of the question. In the Eurobarometer, the question is always framed in the same way over each country and year, and any other bias arising from framing, as long as is additive (language, ordering of questions and alternatives, wording, scaling of answers, social desirability of some answer with respect to others), is controlled for by including country and year fixed effects. Second, the setting of natural experiment (variation by a law change only applicable to a treatment group exogenously chosen by demography) allows predictive power in explaining an attitude, because the "treatability" is not systematically correlated either with the other observable characteristics or with the measurement error of the attitude variable.

Data on abortion laws are available in great detail from United Nations (2002). Coding changes in contraception policies has proved more problematic. Contrary to common expectations, data are hard to find. I have been able to collect somewhat reliable data on the year that the contraceptive pill was embodied in national public health policies, while I could not find data about the year that it was licensed in each country. Because of these caveats, I give more weight to the analysis of abortion rights, with a section on the possible influence of the pill.

4.2   Empirical strategy

4.2.1   Natural experiment

To evaluate the welfare effect of changes in abortion laws, a controlled experiment with randomized data is not available, but several conditions let identify it as a natural experiment. In the definition of Besley and Case (2000), "a natural experiment is often implicitly defined as a law change that affects outcomes for identifiable individuals who are otherwise indistinguishable from those not directly affected by the law change. Nat-

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[11]Answers have been recoded such that higher values reflect higher life satisfaction.

ural experiments thus have natural control groups with which to compare outcomes." As long as the policy exhibits enough variation and it affects a random selection of individuals, I can use it as a natural experiment. A classical concern when using law changes to estimate differences-in-differences effects is that policies may be endogenous. In the case of abortion laws, one would think that a common set of factors has motivated the law changes. Surprisingly, it appears that there is not such a unifying theme underlying legislation.

It is possible to show that there is no apparent link between the timing of abortion legalisation and factors that could reasonably affect the timing of legislation. The candidates are being a Catholic (or Orthodox) country, having more or fewer women in parliament, and the very measure of life satisfaction. The median year when countries enacted laws of full abortion rights is 1981, which divides early from late adopters. In table A.3 I separate countries for being above or below average life satisfaction (the average is 2.04). A cross-tabulation along the two dimensions shows that laws granting full abortion rights were equally spread in the four quadrants.

A cross-tabulation between the timing of adoption and the percentage of women in parliament in the year that full abortion rights were enacted also shows no biased pattern (table A.4). The same countries are equally spread out with regard to the prevalent religion refusing abortion or not. Among the early adopters, are France and Italy who are predominantly Catholic, and among the late adopters are Belgium and Greece, who are predominantly Catholic and Orthodox. Moreover, the inclusion of a country-specific linear trend in all estimations should capture country-specific trends in attitudes towards women's rights.

Finally, looking at the circumstances in which laws were passed shows different influences. 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 a natural 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.

Thus the idea that the timing of abortion legislation followed a very clear set of motives, be them political or societal, is not supported in the data. Certainly these changes did not happen in a vacuum, but what is important for the purpose of the analysis is that none of the possible factors is a good predictor of the law changes.

In addition, from a more technical point of view, most changes in laws occurred in the past and could not be affected by the dependent variable by way of reverse causation. Table A.1 shows the timing. In 1967 the United Kingdom liberalised abortion. In the 1970s, Denmark, France and Italy legalised abortion on request and (West) Germany, Greece and Luxembourg granted abortion rights on health grounds. In the 1980s the

Netherlands and Greece extended full rights and Portugal and Spain granted rights on health grounds. In the 1990s, Belgium and Germany extended rights to abortion on request. Ireland is the only country that has never amended its outright ban on abortion. Of the twelve countries in the sample, two, the UK and Denmark, extended full abortion rights before the recording of life satisfaction by the Eurobarometer had started. For three (Greece, Spain and Portugal), the Eurobarometer started to be collected only following their later accession to the EU, which happened later than the policy change. For the remaining six countries (leaving Ireland aside) the change in regime happened directly during the time that life satisfaction is recollected. The temporal lag ensures that there is no simultaneity between law changes and the recording of life satisfaction.

4.2.2 Differences-in-differences approach

Using treatment and control groups with differences-in-differences (DiD) over time allows establishing a clean measure of the policy impact. Abortion rights are a group-specific policy that only affects individuals of a certain gender and age (i.e. women of childbearing age). These factors are randomly allocated and individuals cannot self-select into the policy. Knowing the individual characteristics of respondents, it is possible to identify those who could be potentially treated *(eligible)*. Matching this information with the temporal and spatial variation of abortion policies in Europe naturally creates groups of *treated* individuals ("treatment group") and groups of *eligible but not treated* individuals ("control group"). In addition, since the treated are observed both before and after the treatment, it is possible to use the treated before the treatment as an additional control group for the treated after the treatment. In terms of the literature on program evaluation[12], I estimate the *average treatment effect on the treated* (ATE_1) , i.e. the average difference between treated and untreated outcomes across the population and over time within the same country.

The outcome variable, life satisfaction, does not show a systematic trend, as figure 1 shows. Country-specific linear and quadratic trends confirm this observation. Out of twelve countries, five report not significant trends, four a positive one (Italy, Luxembourg, Denmark, the Netherlands) and three a negative one (Belgium, Greece, Spain).

The treatment group comprises of women who were of childbearing age when abortion legislation was enacted, and thus were exposed to the legislation when they could take advantage of it. I do not use (nor have) information on whether women effectively used abortion. I argue that the mere existence of abortion rights has made younger generations more satisfied with their life, because of the lesser constraints imposed on their life planning choices, even without exercising the option of using it. Thus it is the combination of date of birth, gender and country of residence which jointly determine whether each individual is exposed to the treatment. Since women interviewed are in

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[12]See Abadie (2003), Angrist, Imbens and Rubin (1996) and Wooldridge (2002).

the age range of 15 to 99 years, I can infer from their date of birth whether during their childbearing age they lived in an institutional setting allowing abortion. The combination generates an exogenous source of identification that is present for all countries except Ireland.

To illustrate the concept, let us set the end of childbearing age at 50^{13}, and let us take three women, all born in 1940, living in different countries: the United Kingdom, Italy and Belgium. Abortion rights were introduced respectively in 1967 in the UK, in 1978 in Italy and in 1990 in Belgium. The woman living in the United Kingdom was 27 in 1967 and she could plan much of her life with abortion rights. The woman living in Italy was 38 when abortion was introduced in her country, so she could benefit from the right's extension for a shorter portion of her life (twelve years). Finally, the woman in Belgium could not plan her life choices with abortion rights, because when abortion was introduced in Belgium in 1990, the same woman was 50. Thus the treatment group comprises women who were less than 50 when abortion rights were introduced, even if at the time of the survey they were over 50. The fact that they could plan part of their life under the regime with rights is what matters. For simplicity, these are defined as the "treatment group" throughout the analysis.

The principal control group is women who could not benefit from the rights either because they were over childbearing age when the law changed or because, despite being young, their country had not ruled in favour of abortion rights when they were interviewed. These are labelled here "women control group". Distinguishing between these two sub-groups does not yield significant differences, and therefore they are gathered in the same group in most of the analysis. Table 7 presents the estimates when the two groups are separated.

The individual nature of the data also allows to investigate whether men in the same cohort of age as the treatment group (i.e. less than 50 years old when abortion rights were introduced) were affected by the policy change. To the extent that they could be the partners of the treatment group, this is not unreasonable, and the model has shown that there could be an indirect effect through the marriage market.

Table A.5 reports summary statistics for baseline characteristics for individuals assigned to treatment and controls across all countries. The treatment and control groups are relatively homogeneous. Means and standard deviations are similar, confirming random assignment to the program across the population of the twelve European countries. Any effect stemming from different mean ages in the two groups is corrected by including age fixed effects. Besides, age effects convey relevant information since they consistently display a U-shaped trend with respect to reported life satisfaction, as per figure 3. Other personal characteristics apart from sex and age could in theory be affected by the treat-

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[13]I follow the medical literature in setting the end of childbearing age at 50 years old. Robustness checks in section 6 show that the results hold if an alternative age of 45 is adopted.

ment, thereby introducing heterogeneity in the model, and are therefore not included.[14]

The fact that Western Europe is relatively homogeneous with regard to the actual *use* of abortion rights ensures further comparability. Abortion is mainly used as a tool of family and career planning, rather than as a means to control family size as among married couples.[15] Moreover, abortion is provided at a symbolic or null cost in all of these countries[16], eliminating concerns of rationing or budget constraints.

To estimate ATE_1 with DiD one identifying assumption is needed.[17] It states that the average outcomes for treated and controls would have followed parallel paths over time in the absence of the treatment. The absence of trends in the outcome variable, life satisfaction, confirms this (see figure 1).

Once the empirical strategy has been defined, I can run reduced-form regressions of the following form to test the key prediction of the model (proposition 3):

$$
\begin{aligned}
W_{ict} &= \beta_1 \cdot X_{ct} + \beta_2 \cdot t_{1ict} + \beta_3 \cdot t_{2ict} + \beta_4 \cdot t_{3ict} + \beta_5 \cdot \eta_{ict} + \\
&\quad + \beta_6 \cdot \delta_c + \beta_7 \cdot \tau_t + \beta_8 \left(\delta_c \cdot Year \right) + \varepsilon_{ict}
\end{aligned} \tag{1}
$$

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 variable for living in a country that has extended legal abortion rights at time t, t_{1ict} is a dummy for belonging to the women treatment group, t_{2ict} comprises the women control group and t_{3ict} the men control group.

The data structure is pooled cross-sections over time. Since observations are thus independent but not identically distributed, standard errors are clustered by country and year in all specifications. This addresses concerns of aggregation bias (Moulton, 1990). The results are consistent and robust when the model is estimated with standard errors clustered by country only, in order to address another potential bias, that of potentially serially correlated outcomes (Bertrand *et al*, 2003).

To control for observed and unobserved heterogeneity, all regressions include age effects, modelled as ten-year age dummies[18] (η_{ict}); country effects (δ_c) to capture time-invariant differences between countries that passed such laws and those which did not;

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[14] The data reassuringly show that the relationship of life satisfaction with personal characteristics is consistent with the estimates by Blanchflower and Oswald (2000) and Gardner and Oswald (2001), which are based on other data.

[15] Henshaw (1990).

[16] David (1992).

[17] Underlying the estimation of ATE_1 are two further assumptions: stable unit treatment value and ignorability of treatment (Angrist, Imbens and Rubin, 1996). The first assumption rules out network effects and implies that access to rights by one's neighbours does not affect one's observed outcome. The assumption of ignorability of treatment requires that there is a random assignment of the treatment to individuals, or that, conditional on the factors that determine treatment, observed outcomes are independent of treatment itself. Here, once gender, age and country of residence are observed, an individual cannot self-select herself in or out of the program.

[18] The results are robust to the inclusion of the continuous variables "age" and "age squared" instead of cohort dummies.

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.

As the DiD estimator is unbiased only if the policy change is not systematically related to other factors that affect life satisfaction, I investigate the possibility of omitted variable bias by including other policies in favour of women: the inclusion of the pill in programs of national public health, mutual consent divorce laws and the degree of maternity benefits.

Three further sources of information are used to understand the effects. These are the variation in the spectrum of legislation from partial to full abortion rights, the institutional religious connotation of countries, which may have affected both the legislative process and its reception, and religious attitudes and membership of individuals.[19]

A possible concern about the identification of the effect may arise from the movement of people across countries, which could lead some women to have an abortion in countries different from those where they reside. If concrete, this possibility would induce a bias in my estimates. An estimate of the size of these cross-border movements can be inferred by the behaviour of Irish women. Ireland, where abortion is not legal, is neighboured by the UK, where it has been legal since 1967. Despite the publicity surrounding it, it is an opportunity that is costly both in terms of money, time and information needed, and thus only available to a small fraction of the population. UK and Ireland are likely to be the countries where this cost is lower, as the two countries share the same language, which facilitates the collection of information to flow, and it is relatively cheap to travel from one another. The *Abortion Statistics* published in the UK give numbers on abortion by residents and non-residents. Between 1979 and 1999, a number between two and six thousand women per year (0.4 to 0.7 percent of women in Ireland) had an abortion in England. These amounted to only 3.4 percent of all abortions undertaken in England. Moreover, it is important to remember that this would not affect the direction of my estimates. It would actually induce a downward bias in my estimates, which would then provide a lower bound for the effect.

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 interpretable as ordinal, but not cardinal, the coefficients from the ordered probit model are more correct than those from OLS. On the other hand marginal effects are more difficult to report parsimoniously in the case of a four-outcome variable. The linear probability model has the advantage that reporting and interpreting marginal effects is more straightforward. Angrist (2000) shows that if the focus is on directly interpretable causal effects rather than on structural parameters, the two approaches are largely comparable.

In this paper I report both sets of results for the main specification of the model in

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[19]It would also be interesting to analyse the relative variation in the actual use of abortion, but official statistics are non comparable, as France, Germany and Italy adopt different reporting methods than other European countries.

tables 1 and 2. Since they are totally consistent, table 3 and the following ones report estimates from the linear probability model only.

5 Main results

5.1 The welfare effect of abortion laws on women

Table 1 reports the estimates of the effect of abortion rights on life satisfaction from the linear probability model. Each column estimates the effect on different sets of treatment and control groups. By choosing control groups that are directly comparable with the treatment group, the coefficient on the treatment group yields the differences-in-differences estimate of the welfare gain (β_2 in equation 1). 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. The table reports the p-value of F-tests on the equality of the relevant coefficients at the bottom.

Column 1 shows the general result that women report on average more welfare than men, consistently with the previous literature.[20] Column 2 separates women by whether they lived with abortion rights when of childbearing age (treatment group) or not (control group). The control group includes women who did not benefit from these rights, either because they were not of childbearing age anymore, or because their country had not passed abortion laws at the time they were surveyed. 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, not significant coefficient.[21] A Wald test on the estimated difference between the treatment and control group of women is significant.

Comparing the estimated coefficient with those obtained on personal characteristics (income level, work status, level of education, marital status), it appears that the magnitude of the welfare gain from abortion rights is equivalent to the increase in welfare from moving one level upwards on a 12-category scale of income, which is largely constant along the income scale. It is also equivalent to the effect of having higher rather than middle education. It is smaller (around one seventh) than the corresponding welfare loss from being unemployed or separated.

Column 3 defines a control group of men who were in the same cohort of age as women in the treatment group. These are directly comparable to women in the treatment group

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[20] See Blanchflower and Oswald (2003).

[21] 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 are reported in table 7. They show that there is no difference in the welfare of the two groups.

for being at same stage of the life cycle, while they may differ for gender effects. The estimates are consistent with the previous specification. Men who lived under abortion rights when they were less than 50 years old do not show any significant difference with respect to other men. Women in the treatment group are on average significantly more satisfied than other women, than men of the same age, and than men who did not live under abortion rights. The difference in these effects is statistically significant.

Column 4 reports a control experiment. The previous results could in principle be the outcome of a cohort effect, whereby younger women are simply more satisfied than older ones. Since all regressions control for age effects, this is unlikely, but it is worthwhile to do a counter-experiment to formally check for this possibility. In column 4 I identify a "false" treatment group made of women who were younger than 50 years old at the time of the survey. They do not appear significantly more satisfied than other women.

Columns 5 and 6 investigate whether the treatment effect has been constant at the various ages at which women received abortion rights. 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 women are when the rights are introduced. If instead the positive effect of abortion rights is merely one of psychological empowerment from having more individual rights, this effect should be constant at any age. Column 5 reveals that the largest part of the positive welfare effect is indeed on women who received abortion rights when they were less than 35 years old. Women who received abortion rights while between 35 and 50 years old do not significantly report more welfare than the rest of the population. This is consistent with an effect going through better life planning of investment in education and desired fertility as the one spelt out in the model, more than with a psychological empowerment of women.

In an alternative specification, column 6 defines the treatment group by the number of years that a woman lived under abortion rights while of childbearing age. Both a linear and a quadratic term are introduced. The estimates show that having lived under abortion rights for longer has a positive and significant effect on women's welfare, albeit at a decreasing rate.

Finally, column 7 provides estimates of the same specification as in column 2, with standard errors clustered on country only. This is a more restrictive condition imposed on the data. Although the individual level identification strongly reduces the year-to-year autocorrelation of the law regime, the latter could induce an overstatement of the significance of the effects. The estimates when clustering on country confirms a significantly higher welfare for women in the treatment group.

In all regressions, the estimated effect of having abortion rights at the time of the survey is positive and significant. This points to a consistent increase in the average level of satisfaction in countries and times with abortion rights, analogously to a general public good. This is independent of the private benefits at the individual level. The

breakdown of this societal effect is investigated in section 6.6. If it were negative, it could indicate a strong aversion to birth control laws. As it is positive, one possible interpretation is that people enjoy living in a society that grants more individual rights. Another possibility is that birth control rights brought a change in social norms that made everyone more satisfied. By allowing women individual rights on their fertility, and indirectly on choices of family and work, various social norms broke up. Previously "unconventional" choices became socially acceptable, both for men and women. Akerlof and Kranton (2000)'s model of identity would argue at this point that utility increased because the reference point for social roles shifted closer to "unconventional" identities.

Table 2, columns 1 to 3, presents the ordered probit estimates and the marginal effects. These are consistent with those from the linear probability model both in terms of size and direction of the effects. Column 1 confirms that women report on average more welfare than men. Column 2 show that this effect is deriving from women in the treatment group, i.e. who lived with abortion rights at the time they were of childbearing age. This set of women reports to be "very satisfied" with 1.5 percentage points higher probability, while it has a negative probability to report lower levels of life satisfaction. Women in the control group have not been affected significantly. Column 3 confirm these results and adds that men of the same age as women in the treatment group were not affected significantly either.

To sum, the main result that emerges from these regressions is that, with the introduction of abortion rights, women who were able to incorporate abortion rights in their life planning experienced an increase in welfare. The rest of the population, and in particular other women who could not benefit from the rights, did not experience any effect. The effect is stronger, the younger the women were when abortion rights were introduced, and the longer the planning horizon they faced. Marginal returns start to decline after the woman is 35 years old. The possibility of a spurious correlation from women being happier than men and young women being happier than older women is ruled out by the results of a control experiment. All effects are significant over and above those of country and year fixed effects, country trends, and individual age affects.

6 Extensions

In this section the basic model is extended in various directions to take account of possible elements of heterogeneity and econometric concerns. The next section reviews the consistency of results on life satisfaction with the economic outcomes predicted by the formal model in section 3. Section 6.2 gives an ex-post description of who gained and who lost from these policies. Sections 6.3 reviews how other policies in favour of women's rights affect the analysis. Section 6.4 deals with variations in treatment effects along religious lines. Sections 6.5 addresses concerns of endogenous legislation. Section

6.6 presents a series of robustness checks and sensitivity analysis and finally, section 6.7 describes how the societal effect of the policy is distributed among the population.

6.1 Consistency with the economic model

The richness of individual level data is that it is possible to systematically match what individuals declare about their preferences with their personal characteristics and their life choices, such as education and work. This approach allows a "revealed preference" test of whether the model outlined in section 3 is consistent with the data.

In particular, I explore the validity of proposition 1 from the model. It states that with birth control rights, holding the level of individual skills constant, men's optimal investment in education does not change, while women's optimal investment is larger or equal than without rights. The same is expected to hold for income. Therefore I run three sets of regressions with a parallel design to equation 1 and columns 1 to 3 in table 1. The explanatory variables in the three cases are the same as in the main equation: abortion rights, a control for being a woman, the treatment and control groups, fixed effects and the country-specific linear trend, with errors clustered on country and year.

In the first set, I run a probit where the dependent variable is a dummy for receiving higher education (i.e. finishing school after 20 years old). Here the threshold age for benefiting of abortion rights and changing one's investment in higher education is set to 20 years old, for consistency between the timing of opportunities and choices. Column 1 shows that being a woman decreases the marginal probability of attaining higher education by 5.5 percentage points, but column 2 reveals that this probability improves for women in the treatment group relative to women in the control group (-4.1 percentage points versus -5.6 percentage points, respectively). This difference is statistically significant. A further breakdown of women in the control group (not reported in the table) highlights that women who received abortion rights but when they had completed their fertility cycle have a higher probability to attain a higher level of education than women who lived in countries with no abortion rights. Column 3 confirms these results and adds that men who lived in a country with abortion rights when less than twenty years old show a lower probability to achieve higher education as much as "treated" women, in a result that is difficult to explain in the framework adopted.

In the second set (columns 4 to 6), I run a probit for the effect of access to abortion rights on the probability of working. In this and in the last set I use the usual threshold age of 50 years old to define treatment and control groups. Women in general have 35.8 percentage points lower probability to be working than men (column 4). This probability reduces to 32.3 in the treatment group and 37.8 percentage points in the control group when they are disaggregated (column 5). Once the men treatment group is controlled for, in column 6, it appears that men who were less than fifty when abortion rights were introduced have 8.2 percentage point higher probability to be working than other

men, women in the treatment group have 26 percentage points lower probability, and the remaining women have 36.3 points lower probability. The better outcome of men exposed to abortion rights relative to other men is again explained as the result of a more efficient process of resource allocation. As in the previous set of regressions, when women in the control group are broken down in two subgroups, women who received abortion rights when they had completed their fertility cycle have a higher probability to be working than women who lived in a country with no abortion rights (not reported in the table). This points to the fact that the discrepancy between women with and without abortion rights is much less due to a cohort effect than expected.

The third and last set (columns 7 to 9) takes the twelve-category classification of personal income as the dependent variable and runs a linear probability model on the effect of abortion rights. Women are more likely to end up in lower classes of the income ranking (column 7). Women in the treatment group show consistently better outcomes than other women (column 8), but worse ones compared to men, in particular to men of their same age (column 9). As with the probability of working, "treated" men exhibit a higher income than other men, which points to a higher efficiency of resource allocation in these countries.

The empirical results are tightly consistent with Proposition 1 of the model, which asserts that abortion rights should raise female investment in education and female income. According to the estimates, women in general report a lower probability than men to achieve higher education, to work and to receive a higher income, respectively by 5.5, 35.8 and 50.8 percentage points. Women who had access to abortion rights when they were "young enough" to affect their choices of education and work consistently show an improvement in this probability with respect to all women, but yet a lower probability compared to men.

Men in the control group who lived in countries with abortion rights report a lower investment in education, while in theory no significant changes would be expected. This result is likely to deserve some more investigation. At the same time, men in the control group report an increased probability to work and they tend to receive a higher income. This can be explained as the result of a more efficient allocation of resources in these countries once individual rights to women are granted.

6.2 The distribution of gains and losses

Table 4 gives a descriptive, not causal, representation of which categories of women *within the treatment group* have gained or lost from the institutional change, according to their education level, working and marital status. The impact of the ability to control fertility on life planning and welfare may vary depending on individual skills, wages, education levels and marital status.

I do not claim causality between life satisfaction and the various indicators of marital

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status, level of education, type of occupation, on the grounds that these choices may be largely endogenous to the set of abortion rights available, as the model illustrates. Nevertheless, it is interesting to see which categories of women seem to have gained the most welfare.

Women who are married or cohabiting, women who work and women with high education have gained the most in terms of life satisfaction. Women who have also gained, although in a ratio of one-fifth compared to those who work. Women of childbearing age who are single, have low education and/or do not work, have a lower welfare than average, and show a lower life satisfaction than the average.

This gives an instant picture of the sides of the debate on the effect of women's rights on women's welfare. Women who could "have it all" give the highest evaluation of their welfare, while women who have lower skills or education and women who could not find a match in the marriage market give the lowest.

6.3 Other policies in favour of women

It is important to analyse whether the legalisation of abortion proxies for some other policy that occurred at the same time and affected women of childbearing age in particular. Alternatively, there could be some underlying factor affecting both abortion rights and individual life satisfaction at the same time. Other policies in favour of women's rights are natural candidates for these possibilities. Although the varied timing of abortion rights gives some protection against the likelihood that another factor exactly mimicked their pattern, I investigate this possibility.

I consider three policies that may have affected women differently than men: the inclusion of the pill in programs of national public policies, no-fault divorce laws and the provision of maternity leave.

Table A.1 shows that the first two sets of laws were legalised in European countries in a staggered manner over the period 1960s-1990s, although with a marked different timing across countries from that of abortion. Maternity leave protection increased markedly between 1969 and 1994, with a large jump at the end of the 1970s (Ruhm, 1998). This absence of synchronisation weakens the possibility that the effect of abortion rights is overlapping with that of another policy.

The pill was invented in the late 1950s and marketed for the first time in the US in 1960. With another staggered timing over 1960s-1990s it was embodied in European countries' public policies and provided by the national health systems.[22] The pill has put women more in control of their fertility. 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

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[22] As mentioned earlier, data on the coverage of the pill by national public policies are less precise, making estimates less reliable. This is why the analysis does not focus more directly on this change.

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. Thus, for the same reasons of abortion rights, it may have increased women's life satisfaction.

No-fault, or mutual consent, divorce laws made the position of women equal to that of men in the event of a divorce. By removing a constraint on the choice of women, they should have increased their life satisfaction. 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, Landes and Michael (1977) point out some of these ambiguous effects of marital instability on utility maximisation. If marriage is a contract with various non-contractible elements, making easier to break it 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 phase and in the marriage itself eventually lead to a utility from marriage which is lower at the time of dissolution than that expected at the time of marriage. Following these considerations, the expectation on the effect of having more divorce possibilities may as well be negative.

For maternity protection policies, I have data covering 1969 through 1994. Of the countries in my sample, Denmark, France, Germany, Greece, Ireland and Italy have made significant changes to their legislation. Ruhm (1998, 2000) has collected data that allow to compute the number of "full-pay" weeks of leave as the product of the number of weeks of paid leave (distinguishing between job-protected leave and not) by the average wage replacement rate. I examine how benefiting from long full-pay maternity leave (where long means "above the average") interacts with abortion rights. In principle, women who benefit from great maternity protection do not need abortion rights to optimally plan their education and work choices, as children do not limit the earning possibilities and working time of the women.[23] If this substitutability were perfect, an interaction of maternity policies and abortion rights would exhibit a zero estimated coefficient.

Table 5 reports the results for the three sets of policies. As before, all regressions control for age, country and year effects, include a country-specific linear trend, and standard errors are obtained from clustering the errors on country and year. The first column includes abortion, pill, divorce and maternity policies to evaluate the effect of abortion rights over and above them. Columns 2 and 3 report results for the role of the pill and mutual consent divorce changes affecting the life satisfaction of people exposed to them. Column 4 reports results on the interaction of abortion rights and maternity policies. Treatment and control groups are re-computed in an analogous way as with

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

abortion rights.

The regression in column 1 includes four policies and the groups of individuals who were affected by them: abortion rights, the pill as part of public policy, mutual divorce laws and high maternity leave protection. The estimate of the effect of abortion rights remain remarkably very close to those found in previous specifications. The low sensitivity of the abortion coefficients to the inclusion of additional variables suggests that the problem of collinearity is very low. Over and above the effect of divorce laws, the pill and maternity leave, extending abortion rights has a positive and significant effect of increasing life satisfaction. Women benefiting from no-fault divorce laws instead report a significantly lower life satisfaction. Women having access to the pill within the framework of national public policies are significantly more satisfied. The positive effect of the pill is not statistically distinguishable from that of abortion rights, as expected, while the effect of abortion and divorce right is. Having higher maternity leave protection does not affect life satisfaction significantly.

Columns 2 and 3 show that the pill and divorce laws taken alone have the same effect as when they are taken all together. This is further evidence of the low multicollinearity of the effects.

The effect of the pill is reported in column 2. It is negative and significant in its valuation as a public good, but women who had access to it while of childbearing age report a positive and significant effect on their welfare. The difference between the private and public benefit is statistically significant. The fact that, in general, the pill anticipated the extension of abortion rights points to a cumulative effect of the two policies.

The effect of no-fault divorce is positive and significant in general but negative and significant effect on women (column 3) . This is consistent with the conventional economic approach to divorce by Becker *et al* (1977).

High maternity protection policies appear not to have affected life satisfaction, neither *per se*, nor on the group of women who benefited from them during childbearing age (column 4).

Column 5 examines the further possibility of interaction of abortion rights with maternity leave policies. As outlined before, it may be that countries providing excellent maternity leave, i.e. for long enough periods and with a replacement wage rate close to unity, make the event of having a child, even if unplanned, neutral with respect to the woman's decision to study for higher education and work. With such a well designed maternity leave, abortion rights would be superfluous, unless there is a taste for planning the timing and spacing of children.[24] The results show that once the interaction of the two factors is allowed, maternity leave *per se* becomes significant in increasing general

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[24] Here I am abstracting from incentive effects on labour supply as well as demand, i.e. disincentives for firms to hire women of childbearing age.

welfare, but there are no specific effects stemming from the interaction of maternity protection and abortion rights. The positive effect of having abortion rights on the women treatment group is robust to this test. Although the coefficient is not significant, the fact that it is negative may weakly suggest that the two policies acted as relative substitutes. The same results are obtained using different components of maternity protection, such as the wage replacement rate, the number of weeks of job-protected leave and the number of non protected ones.

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. If it took one day and a lot of effort to do the washing in the 1930s, it now takes an hour and very little effort to do it with a washing machine. I would expect this effect to happen both for men and women, as it is not confined to household tasks. This can be proxied by a country-specific linear time trend, which has been controlled for throughout the whole analysis. Therefore, results should be interpreted as over and above the effort-saving effect of technological progress.

Regarding the possibility that it has been "feminism" that has driven both women's life satisfaction and the passing of laws liberalizing abortion, I prefer to give an empirical content to feminism as that of parity laws. Any other definition of feminism is not observationally distinguishable from that of a general or country-specific linear trend. On this 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."

6.3.1 Individual religiosity information as a test for omitted variable bias

A different approach to investigate the possibility of an omitted variable bias is to look at the effect of abortion rights on women who may be personally opposed to use abortion on religious grounds. Catholic, Greek Orthodox, Orthodox Jewish and Muslim religions explicitly ban abortion in all instances. Women who are strongly attached to these religions are subject to all women's rights influences experienced by the general treatment group, but they are likely not to derive any personal benefit from laws allowing abortion. If they do not exhibit higher life satisfaction, while the rest of the treated women does, this is additional evidence of the causality of abortion rights in increasing welfare. Therefore, in the framework of a wider analysis on religious effects, I single out women in the treatment group according to which denomination they regard themselves as belonging to. Column 4 in table 6 shows that treated women (i.e. who had abortion rights while of childbearing age) who define themselves as Greek Orthodox or Jewish are not any more satisfied than the rest of the population. Muslim women report a lower welfare than the average. Catholic women instead report a higher welfare. This is consistent with the literature on the sociology of religion, which argues that on the issue of family

planning Catholic women have disassociated themselves from the doctrine, even when strongly religious.

In general, this regression confirms that women who live in countries with abortion rights and are in childbearing age, but cannot make individual use of these rights because of their beliefs or the community they belong to, have not experienced any change in welfare. At the most, it has reduced it. This calls back into play the theory of Akerlof, Yellen and Katz (1996), when they argue that "women who, because of indecision or religious conviction have failed to adopt these innovations, have lost disproportionately". This result may be a valid proof that the effects found so far truly stem from birth control rights and not from some omitted underlying variable.

6.4 Variations in treatment effects by religion

Religious institutions

So far the analysis has assumed that the average treatment effect of extending abortion rights is the same across all countries. In reality, the effect of the treatment may differ across institutional settings.

This could be the case with religious institutions in Europe, in countries where they are predominant and they may interact with public policy. Europe has a strong Christian influence in its institutions, roughly equally divided between the Roman Catholic and the Protestant versions of Christianity. Other religious groups are quantitatively important, but they have had less impact on the institutional framework of European countries.

The two Christian churches have taken a different stance on abortion. The Roman Catholic Church sees abortion as a crime with no exception and anybody who has or facilitates an abortion is punished with excommunication from the Church. The Protestant Church (at least in my understanding) has not taken a formal stance on abortion and leaves it up to the individual and the couple to regulate this aspect of their life "according to their conscience". The Church of England shares the same approach[25]. Therefore it is predominantly Roman Catholic countries that have a more polarised view within society of reproductive freedom. They may comprise of 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 only abide to the state laws on this issue. It is interesting to analyse whether this polarised stance has induced systematic differences in the effects of abortion laws in countries where one or the other religion dominates.

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 (see table A.3).

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[25]See Brookes (1988, p. 154) for a description of the attitude of the Church of England.

To explore the extent of heterogeneous effects, columns 1 and 2 in table 6 run the basic specification separating countries that are predominantly Catholic from the others. The latter usually comprise a large share of Protestants, but not only, so I label them "non-predominantly Catholic"[26].

The two sets of countries show some differences in effects. In non-Catholic countries, women in the treatment group experienced a sizeable increase in welfare with respect to the rest of the population. Nevertheless, the general effect of having abortion rights at the time of the survey has a negative impact on reported welfare.

In Catholic countries instead, neither the treatment group, nor the control groups report a significant variation in welfare through abortion rights. There is instead a positive and significant effect from abortion rights in general.

This points to the concrete possibility that the Catholic church may have hindered the application of abortion laws or the realisation of their effects. In Catholic countries, where women achieved economic and political rights much later than in non-Catholic countries, prevailing social norms may have constrained the full effects of abortion laws on individual behaviour, and thus the life satisfaction associated with these choices. Put simply, women may have had individual birth control rights, but individual rights proved less effective when embedded in a culture where women had not achieved a social identity different from her role in the household. An opposite but consistent explanation is instead that the breakup in social norms was so large in Catholic countries that everyone benefited from it. It is not possible to distinguish between the two interpretations yet.

Individual religiosity

In table 6, columns 3 and 4, I analyse the possibility that the religious beliefs of the individual affect the extent to which she benefits from birth control rights.

Somewhat surprisingly, column 3 shows that women in the treatment group who declare themselves to "feel religious" report an additional positive, significant effect on welfare from birth control rights compared to the rest of the population. Using alternative indicators of religiosity, like religious attendance and the importance of religion in one's life, does not change this result in any way.

In column 4, women in the treatment group who declare themselves as belonging to a Catholic or a Protestant (including "Other Protestant") denomination report higher welfare. Greek Orthodox and Jewish women report respectively a positive and a negative sign, but these are not significant. Muslim women report a lower welfare. As these religions all strongly oppose abortion, these results have been interpreted in the previous section as the result of adhering to a religion that denies the validity of abortion, despite living in a country that allows it. Abortion rights have improved the welfare of women

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[26]The countries classified as *predominantly Catholic* are Belgium, France, Italy, Luxembourg, Ireland, Greece, Spain, and Portugal. *Non-predominantly Catholic* countries in the sample are the Netherlands, Germany, Denmark, and United Kingdom.

who were in an age when they could adopt them, but actual individual access to these rights is the joint outcome of country laws and the relative freedom granted by the religious denomination one wants to belong to. Catholic women represent an anomaly, as they report increased life satisfaction in spite of the fact that the doctrine bans abortion. Nevertheless, it is documented in the sociological literature that on issues of reproductive rights, even the most fervent Catholic women have adopted a stance of independence from the doctrine.

6.5   Robustness checks

Table 7 reports a number of robustness checks on the main result that women who could benefit from abortion rights report a higher evaluation of their own welfare.

Breakdown of control group of women

Throughout the analysis I consider the difference in difference effect on treated women with respect to women who could not benefit from the rights either because they were over childbearing age when the law changed or because, despite being young, their country had not ruled in favour of abortion rights when they were interviewed. Column 1 presents the estimates when the two groups are separated. It appears that women who lived in countries without abortion rights have a change in welfare with a coefficient of 0.055; women who did live in a country and year with abortion rights, but when they had completed their fertility cycle, have a coefficient of 0.061 (=0.118-0.057). Since the two effects are very close, it makes sense to group the two sets of women together, as this simplifies the interpretation of the difference in difference coefficients.

"False" vs. "true" law changes

Given the depth of information on abortion laws, I can identify a set countries where the law change was less effective in practice. United Nations (2002) states that before the law was changed, Greece, Belgium and the Netherlands were known to have widespread underground abortion and not to enforce the ban. Indeed, a regression of the basic model on these three countries shows no significant effect of abortion on women both in the treatment and control group (column 2). On countries where the law change was effective (column 3), instead, the basic model yields the traditional prediction of a positive effect on the treatment group.

This regression also shows a positive and significant effect on men in the same cohort of age as the women treatment group. This may point at a more precise effect on men, possibly going through marriage market effects, as indicated by the model in section 3.

Heterogeneity of rights

Over the years, the legislation on abortion rights has sometimes granted "partial rights", i.e. the right to abortion for health concerns relating either to the woman or to the child, including in the definition both physical and mental health, and other times

"full rights", i.e. the right to abort on socio-economic grounds or simply on request.[27] Some countries have granted partial rights only, others have later moved to full rights, others have leaped directly from no rights to full rights. Table A.1 gives details of the timeline of changes. Table 7, column 4 examines whether the effects of partial or full rights of abortion are different.

It appears that extending partial or full rights had a similar positive effect on women who could make use of these rights. The general effect on society of having abortion rights at the time of the survey is larger with partial rights than with full rights, but the two are not statistically different.

These effects are consistent with the commentaries of researchers on abortion. They observe that the leap to partial rights often opens some loophole or room for manoeuvre to obtain full rights in practice.[28] Once abortion is seen as a socially acceptable course of action and some rights are allowed for, arguments for abortion can be extended by the administering doctor to wider social considerations. In particular, doctors may authorise abortion for socio-economic reasons under the "mental health" protection ground of the woman.

One year before and one year after the law change

In column 5, I extract a sub-sample where the panel dimension is reduced to one year before and one after the law change for countries where the law changed between 1975 and 1998. This amounts to a much smaller sample of around 22,000 people. Despite the restrictiveness of the condition, the positive effect on the treatment group is significant and robust.

The duration of the welfare effect

Column 6 explores the possibility that results hinge on the "euphoria" effect immediately after the legalisation. Therefore I eliminate observations corresponding to the year when abortion was legalised and the following five years. Yet the positive effect on women who could use these rights is present even six years after their introduction, demonstrating a lasting feature.

Sensitivity analysis on the threshold age for end of childbearing

In column 7, I check the sensitivity of results to the choice of the threshold for the end of childbearing age. In the course of the analysis I have adopted 50 years of age as this is the limit usually adopted in the medical literature. The results are robust to adopting an age of 45 years.

Removing the autocorrelation of laws

There is the possibility that since the adoption of laws is positively serially correlated over time, the results on abortion rights at the time of the survey could have standard errors which are biased downwards (Bertrand *et al*, 2003). However, this would only

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[27] For details, see Appendix 1.

[28] 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.

affect the validity of the estimates of the societal effect of abortion. The estimate of the private benefit is derived from an individual level, cross-sectional identification and does not suffer from this bias.

The fact that all regressions cluster the observations on the country and year they were drawn from provides a powerful control for this possibility. Further checks using clustering on country in table 1 have reassured about the robustness of the identification to this bias.

As a further robustness exercise, I test for the possibility of under-rejection of the correct model by two different approaches. In one approach, as Bertrand *et al* suggest, I ignore the time series information and regress the dependent variable (individual welfare) on all relevant covariates and fixed effects employed in the estimation, except for the law change. The estimated residuals from the treatment countries only are divided among those corresponding to before and after the law and are regressed on the law dummy in a two-period panel. The results are shown in column 8. The effect of the law itself is positive and strongly significant. This may be a confirmation that the effect of abortion rights goes both through private effects on the individual, as well as through general or public good effects.

Selection effect, or "Donohue and Levitt effect"

Donohue and Levitt (2001) argue that the crime rate in the United States has declined significantly around 18 years after the legalisation of abortion, and that there is a causal relationship between the two events. Taken to the extreme, this argument could suggest that the results shown above may depend on fewer unhappy or unlucky people being born over time in countries allowing abortion, and thus to an increase in life satisfaction in these countries. When I subtract from the sample individuals born after abortion rights were introduced in each country, the positive effect of abortion rights on the treatment group is robust to this exclusion (regression not reported here).

6.6 Societal effects of the introduction of abortion rights

All previous results show a positive effect of living in a country with abortion rights on the evaluation of personal life satisfaction, with the only exception of Protestant countries. This parameter reveals a general effect that goes beyond whether the individual can actually make use of these rights. We would expect individuals who strongly oppose these rights to report a negative value of the estimated coefficient.

Table 8 investigates whether there is any systematic effect from personal covariates (age, gender, religion) on individual life satisfaction. The table reports seven sets of regressions of life satisfaction on a dummy for abortion rights at the time of the survey, alone and interacted with the relevant covariates. Each set of rows between thick lines represents a different regression. All regressions control for the individual being in the women treatment group or in the two control groups of men and women, for all fixed

effects and the trend. The third column reports the coefficient on the interaction, while the fourth column shows the estimated coefficient on abortion rights.

The findings are that man derive large marginal benefits from living in a country with abortion rights, while women in general do not (set 1). The benefits on women are concentrated only on those who could actually make use of abortion rights, and are therefore in the domain of private effects (set 2). The age of respondents does not have a significant effect over and above that of general age effects (set 3). Only individuals younger than 20 years old report a higher consideration of abortion rights.

In terms of political self-placement, left-wing individuals give surprisingly the lowest evaluation of their life satisfaction in presence of abortion rights and right-wing individuals give the highest (set 4).

Religious individuals are more positive towards abortion (set 5), relatively religious men more than religious women (set 6). Protestant and Catholics also show a positive general effect from living in a country with abortion rights, while the effect is negative for Muslim and neutral for Greek Orthodox and Jewish (set 7).

7   Conclusion

In this paper I have explored the issue of whether women's rights have brought women a higher welfare and satisfaction with their lives. I have focused my analysis on birth control rights. The effect of a change in birth control rights on welfare could operate through (at least) three different channels: through a reduction in unwanted children, through a better planning of education choices for women and through a more general empowerment of women. Although all three channels are likely to be relevant in practice, the one that operates through the optimal investment in education is explored in more detail as it lends itself to predictions that can be tested by quantitative analysis.

I have analysed the evaluations of life satisfaction by over 450,000 individuals in twelve European countries between 1975 and 1998. Both abortion rights and the endorsement of the pill by national public policies were introduced with a staggered and plausibly exogenous timing between 1967 and the 1990s. I have linked gender, birth year and country of residence of the individual in a unique framework aimed at determining whether the individual could be affected by birth control rights, or in other words, was *treated*. The differences-in-differences framework has yielded a clean estimate of the effect of birth control rights on individual welfare, in particular on that of women. The richness of data allowed to disentangle the welfare effect deriving from birth control rights from possibly confounding effects such as gender and age effects, effects due to the country and the year the person lives in, and to general trends within the country.

The main finding is that following abortion rights, women who were effectively exposed to the policy (i.e. of childbearing age at the time the policy was introduced)

consistently registered an increase in welfare. In terms of magnitude, the welfare gain of women in the treatment group is equivalent to the gain from going up one level on a 12-category scale of income (this effect is largely constant along the income scale) or of having higher rather than middle education. It is smaller (around one seventh) than the corresponding welfare loss from being unemployed or 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, the longer they were exposed to them. Marginal returns start to decline after the woman is 35 years old. The possibility of a spurious correlation from all women being happier than men and young women being happier than older women is ruled out by the results of a control experiment. These effects are robust to alternative specifications and to the inclusion of age controls, country effects, year effects, and country-specific linear trends.

Life satisfaction effects are consistent with changes operating through economic choices. The formalisation shows that if birth control rights affect the planning horizon for women's choices of education and work, we can expect that the liberalisation of birth control rights is followed by an increase in women's investment in education, in a higher probability of working and in a higher income level. These three effects are strongly supported in the data.

I have explored the strength of the result along several dimensions, taking into account possible elements of heterogeneity and econometric concerns. The identifying assumption of the estimation is that no other shock has happened on the same countries and years contemporaneously to changes in abortion rights. Since changes in abortion laws exhibit quite a large variation among European countries, it is unlikely, although it cannot be ruled out, that some event has exactly mimicked the time and geographic pattern of abortion laws with effects on the same treatment group. To test for this possibility, two policies that are likely to have brought large changes in women's welfare in the same years are analysed: the legalisation of mutual consent divorce and the introduction of the pill in national public policies. This paper has not focused on them directly because data are less reliable on these policies, but including them in the framework shows the robustness of the effect of abortion rights on welfare. The analysis shows that the introduction of the pill in national public policies had a positive effect on women's welfare, while mutual consent divorce laws decreased women's welfare. It is plausible to argue that abortion rights and the pill had a cumulative effect on welfare. The negative effect of mutual consent divorce is consistent with the economic theory of it.

In addition to these two policies, the interaction of maternity leave policies with abortion rights is investigated, but there are no convincing results on the substitutability between the two or of an independent effect of maternity policies on women's welfare.

Heterogeneity of treatment effects is considered along institutional differences, reli-

gious characteristics, and variations in the intensity of rights. Econometric concerns of endogenous legislation are discussed but they do not appear to apply. Finally, a series of robustness checks and sensitivity analysis is presented.

These findings are interesting both from a historical point of view and in the context of developing countries. One of the goals of development policy is the empowerment of women. Considering that a third of countries, mainly developing ones, representing a quarter of the world population, do not have abortion rights at all, these results may provide some guidance on the effects of opening up to these rights. Despite institutional differences that make the results not directly applicable to other societies, this analysis suggest an important link between providing individual rights like birth control to women and favouring their empowerment in other fields.

The next step of the analysis will be to collect data on other policies that may have affected women's welfare, in particular on their access to contraceptive services, their rights in the labour market, and investigate the effect of these policies on welfare as well as on economic and demographic variables.

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8   Appendix: Data sources and description

All individual level data come from the *Mannheim Eurobarometer Trend File.*

12 COUNTRIES: Belgium, Denmark, France, Germany, Greece (since 1981), Ireland, Italy, Luxembourg, Netherlands, Portugal (since 1985), Spain (since 1985), UK.

YEARS: 1975-1998 if not otherwise stated.

LIFE SATISFACTION: four-category variable, based on the question: "On the whole, are you very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the life you lead?". The possible answers are recoded as 4=very satisfied and 1=not at all satisfied.

TREATMENT GROUP: women of childbearing age when abortion legislation was enacted.

WOMEN CONTROL GROUP: women who were not exposed to abortion rights during their childbearing age. It comprises women who lived in a country which had not legalised abortion, or who were over childbearing age when the law changed. The two groups exhibit non-distinguishable effects.

MEN CONTROL GROUP: men in the same cohort of age as women in the treatment group.

PERSONAL CHARACTERISTICS: dummies on gender, whether the person is working, married/cohabiting versus single, his level of education (low, middle, high), his personal income on a 12 point scale.

AGE: ten-year age dummies (age less than 20, 20-29, 30-39, 40-49, 50-59, 60-69, over 70).

FEEL RELIGIOUS: dummy equal to one if the person answers "religious" to any of two questions: "Whether you do or you don't follow religious practices, would you say that you are... (religious, not religious, an agnostic, an atheist)" or: "Independently of whether you go to church or not, would you say you are... (a religious person, not a religious person, a convinced atheist)". The two questions are asked in different waves.

RELIGIOUS DENOMINATION: the Eurobarometer asks: "Do you regard yourself as belonging to a religion? If yes, which of them?". Answers are coded separately for Catholic, Greek Orthodox, Jewish, Muslim, Protestant and Other Protestant (comprising Church of England and others).

LEFT-RIGHT SELF PLACEMENT: dummy derived from the question: "In political matters people talk of "the left" and "the right". How would you place your views on this scale? 1. Left ...10. Right." Answers are coded as "Left" if between 1 and 4, as "Centre" between 5 and 6, and "Right" between 7 and 10.

CATHOLIC COUNTRIES: countries with a majority of Roman Catholics (Greek Orthodox for Greece), as in parentheses. These are Belgium (75%), France (83-88%), Italy ("predominantly"), Luxembourg ("predominantly"), Ireland (91.6%), Greece (98%), Spain (94%), and Portugal (94%). Non-Catholic countries in the sample are the Nether-

lands (31%), Germany (34%), Denmark (3%), and the UK (predominately Anglican). Source: *CIA Intelligence Factbook 2002.*

ABORTION LAWS: source: United Nations (2002).

NO-FAULT DIVORCE: source: Edlund, Haider and Pande (2003).

DATE OF INCLUSION OF ORAL PILL IN THE NATIONAL HEALTH PLANNING: sources: Jones *et al* (1989), United Nations (2002).

MATERNITY LEAVE BENEFITS: the number of weeks of job-protected paid leave due to pregnancy multiplied by the average wage replacement rate. These data have been kindly provided by Christopher Ruhm.

PERCENTAGE OF WOMEN IN PARLIAMENTS: source: Inter-Parliamentary Union (1995).

Table A.1: Women's rights in Europe, 1967-2000

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

Note: European countries that do not appear here are not included in the analysis because of lack of data on life satisfaction. Details of all sources are provided in the Appendix.

Table A.2: Abortion laws in the world, 1997

Abortion restrictiveness	Number of countries	% Population	Cumulative % pop.
To save the woman's life	54	25	25
Physical health Mental health	23 20	10 4	14
Socio-economic grounds On request	6 49	20 41	61

Source: Rahman, Katzive and Henshaw (1998).

Table A.3: Timing of abortion laws and correlations with life satisfaction

		Average life satisfaction for countries	
		More satisfied	Less satisfied
Timing of laws	Early	UK (1967) Denmark (1973)	France (1975) Italy (1978)
	Late	Netherlands (1981) Belgium (1990)	Greece (1986) Germany (1995)

Table A.4: Timing of abortion laws and the percentage of women in Parliament

		Average women in parl. in year of liberalisation of abortion rights	
		Less women than avg.	More women than avg.
Timing of laws	Early	France (1975)	UK (1967) Denmark (1973) Italy (1978)
	Late	Belgium (1990) Greece (1986)	Netherlands (1981) Germany (1995)

Table A.5: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Women (51.6% of the sample)					
Life satisfaction (1-4 scale)	237,505	3.054	0.774	1	4
Age	237,134	43.1	17.9	15	99
Personal income position	171,157	6.270	3.255	1	12
Low Education	236,195	0.418	0.493	0	1
High Education	236,195	0.135	0.342	0	1
Working	237,505	0.346	0.476	0	1
In a couple (married or de-facto)	237,505	0.604	0.489	0	1
Men (48.4% of the sample)					
Life satisfaction (1-4 scale)	222,448	3.040	0.770	1	4
Age	222,184	42.6	18.0	15	99
Personal income position	165,860	6.790	3.190	1	12
Low Education	221,201	0.372	0.483	0	1
High Education	221,201	0.190	0.392	0	1
Working	222,448	0.630	0.483	0	1
In a couple (married or de-facto)	222,448	0.341	0.474	0	1
Women treatment group (33.8% of the sample)					
Life satisfaction (1-4 scale)	149,173	3.049	0.764	1	4
Age	148,877	36.4	13.2	15	80
Personal income position	107,528	6.940	3.184	1	12
Low Education	148,429	0.347	0.476	0	1
High Education	148,429	0.166	0.372	0	1
Working	149,173	0.442	0.497	0	1
In a couple (married or de-facto)	149,173	0.641	0.480	0	1
Women control group (17.8% of the sample)					
Life satisfaction (1-4 scale)	88,332	3.061	0.790	1	4
Age	88,257	54.4	19.1	15	99
Personal income	63,629	5.138	3.052	1	12
Low Education	87,766	0.537	0.499	0	1
High Education	87,766	0.083	0.276	0	1
Working	88,332	0.185	0.388	0	1
In a couple (married or de-facto)	88,332	0.540	0.498	0	1
Men control group (31.6% of the sample)					
Life satisfaction (1-4 scale)	139,752	3.025	0.760	1	4
Age	139,571	36.0	13.4	15	99
Personal income	104,185	7.368	3.147	1	12
Low Education	138,984	0.300	0.458	0	1
High Education	138,984	0.221	0.415	0	1
Working	139,752	0.729	0.444	0	1
In a couple (married or de-facto)	139,752	0.592	0.491	0	1
Life satisfaction - for whole sample	460,144	3.047	0.772	1	4
- for indiv with low edu	181,090	2.932	0.810	1	4
- for indiv with middle edu	150,869	3.089	0.750	1	4
- for indiv with high edu	73,942	3.186	0.724	1	4
- for indiv who works	222,471	3.069	0.746	1	4
- for indiv who does not work	188,183	2.991	0.816	1	4
- for single indiv	112,128	3.029	0.759	1	4
- for in-couple indiv	284,143	3.088	0.762	1	4
- with respect to income group	337,129	Monotonically increasing			
Country level					
Country-years with full abortion rights	250	0.51	0.50	0	1
Country-years with partial abortion rights	250	0.28	0.45	0	1

Table 1: The effect of abortion rights on welfare, differences-in-differences estimates

Depvar: Life Satisfaction	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Abortion rights at the time of the survey	0.087 (4.36)	0.076 (3.74)	0.067 (3.06)	0.096 (4.62)	0.078 (3.82)	0.083 (4.11)	0.076 (1.73)
All women	0.016 (4.20)						
Treatment group: women with abort. rights when childbearing age		**0.028 (6.90)**	**0.043 (3.52)**				**0.028 (2.12)**
Control group: women without abort. rights or not of childbearing age with abort. rights		-0.005 (0.72)	-4.15e-4 (0.005)		-0.007 (1.07)	-0.005 (0.73)	-0.005 (0.22)
Control group: men with abort. rights in the same cohort of age as treatment group			0.018 (1.41)				
Control experiment: Women less than 50 years old				0.005 (0.36)			
Treatment group: women who received abort. rights when less than 25 years old					0.029 (6.41)		
Treatment group: women who received abort. rights between 25 and 35 years old					0.030 (6.14)		
Treatment group: women who received abort. rights when over 35 years old					0.001 (0.16)		
Treatment group: No. years that women enjoyed abort. rights						0.007 (7.00)	
Treatment group: No. years that women enjoyed abort. rights - squared						-2.71e-4 (5.39)	
Age effects, Country effects, Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Country-year	Country-year	Country-year	Country-year	Country-year	Country-year	**Country**
P-value of F-tests: Women T group - women C group		0.00	0.00			0.07	0.08
Women T group - men C group		0.00	0.00				
R2	0.95	0.95	0.95	0.95	0.95	0.95	0.95
Obs	459,953	459,953	459,953	237,505	459,953	459,953	459,953

Notes: t-statistics in parentheses, from standard errors adjusted for clustering on country and year (except for column 7). Sample period is 1975-1998 for 12 countries. Columns 1-7 report Linear Probability Model estimations. Fixed effects are significant at the 1% level in all specifications. Definitions of all variables are in the Appendix.

Table 2: The effect of abortion rights on welfare, DiD estimates, ordered probit

Depvar: Life Satisfaction	(1)	(2)	(3)
Abortion rights at the time of the survey	0.138	0.121	0.108
	(4.31)	(3.70)	(3.06)
All women	0.028		
	(4.68)		
Treatment group: women with abort. rights when childbearing age		**0.047**	**0.069**
		(7.20)	**(3.65)**
Control group: women without abort. rights or not of childbearing age with abort. rights		-0.005	-0.001
		(0.53)	(0.007)
Control group: men with abort. rights in the same cohort of age as treatment group			0.025
			(1.32)
Age effects	**Yes**	**Yes**	**Yes**
Country effects, Year effects	**Yes**	**Yes**	**Yes**
Country-specific linear trend	**Yes**	**Yes**	**Yes**
P-value of F-tests:			
Women T-group - women C-group		0.00	0.00
Women T-group - men C-group			0.00
Pseudo R2	0.07	0.07	0.07
Obs	459,953	459,953	459,953

Marginal effects from probit				
Abortion rights at time of survey on	Y_3: Very satisfied	0.043	0.038	0.034
	Y_2: Fairly satisfied	-0.008	-0.007	-0.007
	Y_1: Not very	-0.024	-0.021	-0.018
	Y_0: Not at all	-0.011	-0.010	-0.009
All women on	Y_3: Very satisfied	0.009		
	Y_2: Fairly satisfied	-0.002		
	Y_1: Not very	-0.005		
	Y_0: Not at all	-0.002		
Women in treatment group on	Y_3: Very satisfied		**0.015**	**0.022**
	Y_2: Fairly satisfied		**-0.004**	**-0.006**
	Y_1: Not very		**-0.008**	**-0.011**
	Y_0: Not at all		**-0.003**	**-0.005**
Women in control group on	Y_3: Very satisfied		-0.018	2.74e-4
	Y_2: Fairly satisfied		4.35e-4	-6.81e-5
	Y_1: Not very		0.001	-1.43e-4
	Y_0: Not at all		4.08e-4	-6.30e-5
Men in control group on	Y_3: Very satisfied			-0.008
	Y_2: Fairly satisfied			0.002
	Y_1: Not very			0.004
	Y_0: Not at all			0.002

Notes: t-statistics in parentheses, from standard errors adjusted for clustering on country and year. Sample period is 1975-1998 for 12 countries. F-tests for the joint significance of fixed effects are significant at the 1% level in all specifications. Definitions of all variables are provided in the Appendix.

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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Abortion rights at the time of the survey	0.011	0.011	0.012	-0.026	-0.061	-0.105	0.733	0.640	0.526
	(1.49)	(1.48)	(1.59)	(2.65)	(5.37)	(8.76)	(2.74)	(2.40)	(1.94)
All women	-0.055			-0.358			-0.508		
	(24.50)			(48.35)			(27.78)		
Treatment group: women with abort. rights when less than 20 years old		-0.041	-0.065						
		(13.63)	(15.34)						
Control group: women without abort. rights or older than 20 years old when received abort. rights		-0.056	-0.063						
		(22.94)	(25.70)						
Treatment group: women with abort. rights when childbearing age					-0.323	-0.260		-0.402	-0.211
					(39.33)	(19.70)		(17.99)	(3.11)
Control group: women without abort. rights or not of childbearing age with abort. rights					-0.378	-0.363		-0.687	-0.632
					(40.65)	(34.24)		(16.02)	(15.46)
Control group: men with abort. rights in the same cohort of age as treatment			-0.054			0.082			0.224
			(11.78)			(6.75)			(3.32)
Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P-value of F-tests: women T group – women C group		0.00	0.02		0.00	0.00		0.00	0.00
Women T group – men C group		0.00	0.00		0.00	0.00		0.00	0.00
Pseudo-R2/R2	0.12	0.12	0.12	0.26	0.26	0.26	0.19	0.19	0.19
Obs	445,866	445,866	445,706	459,953	459,953	459,953	337,017	337,017	337,017

Notes: t-statistics reported in parentheses, from standard errors adjusted for clustering on country and year. Sample period is 1975-1998 for 12 countries. Columns (1)-(4) report probit estimations with marginal probabilities. Columns (5) and (6) report estimates from Linear Probability Model regressions. The personal income position is coded on a scale 1 to 12. Definitions of all variables are provided in the Appendix.

Table 4: The distribution of gains and losses by marital status, education, working status

	Depvar: Life Satisfaction		
	(1)	(2)	(3)
Abortion rights at the time of the survey	0.074	0.086	0.090
	(3.64)	(4.26)	(4.45)
All women	0.004	0.012	0.021
	(0.68)	(2.34)	(5.20)
Treatment group and **single**	-0.084		
	(11.38)		
Treatment group and **in couple**	0.094		
	(13.48)		
Treatment group and **working**		0.036	
		(5.47)	
Treatment group and **non-work.**		-0.022	
		(3.26)	
Treatment group and **high edu**			0.076
			(11.81)
Treatment group and **low edu**			-0.059
			(11.03)
Age effects	**Yes**	**Yes**	**Yes**
Country effects, Year effects	**Yes**	**Yes**	**Yes**
Country-specific linear trend	**Yes**	**Yes**	**Yes**
P-value of F-tests			
Treatment gp single- Treatment gp in couple	0.00		
Treatment gp working- Treatment gp non working		0.00	
Treatment gp high edu - Treatment gp low edu			0.00
R2	0.95	0.95	0.95
Obs	459,953	459,953	457,396

Notes: t-statistics reported in parentheses, from standard errors adjusted for clustering on country and year. Sample period is 1975-1998 for 12 countries. No causality is clamed in these estimates, but purely a description of the distribution of gains and losses. "In couple" includes the two categories of being married and living in a de-facto couple. Fixed effects are jointly significant at one percent level in all specifications above. Definitions of all variables are provided in the Appendix.

Table 5: Other policies in favour of women

Treatments:	All rights	Pill in national public policy	Divorce	Maternity policies	Abortion. rights *and* maternity policies
	(1)	(2)	(3)	(4)	(5)
Abortion rights at the time of the survey	0.069				0.085
	(3.19)				(4.36)
All women	0.006	-0.028	0.043	0.011	-0.002
	(0.44)	(4.14)	(3.99)	(1.94)	(0.25)
Treatment group: women with abort.	**0.016**				**0.044**
rights when childbearing age	**(1.99)**				**(4.49)**
Pill in public policy	-0.136	-0.196			
	(3.41)	(4.52)			
Treatment group: women with pill in	**0.055**	0.055			
public policy when childbearing age	**(7.95)**	(8.36)			
Divorce rights	0.127		0.172		
	(3.21)		(4.48)		
Treatment group: women with divorce	**-0.056**		-0.034		
rights	**(3.62)**		(2.98)		
High maternity protection	0.044			0.040	0.054
	(1.46)			(1.14)	(1.85)
Treatment group: women with high	**-0.003**			0.004	-0.005
maternity protection when childbearing age	**(0.34)**			(0.69)	(0.39)
T group (Abortion rights) * T group					-0.018
(Maternity policy)					(1.25)
Age effects	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Country effects	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Year effects	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Country-specific linear trend	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
P-value of F-tests					
T group abort.- T group divorce	0.00				
T group abortion- T group pill	0.00				
T group abortion- T group maternity prot.	0.18				0.00
T group pill– pill.		0.00			
Treatment gp divorce– divorce			0.00		
T group maternity pol.-interaction term				0.00	
T group maternity policy - maternity policy					0.07
R2	0.95	0.95	0.95	0.95	0.95
Obs	414,351	459,953	459,953	414,351	414,351

Notes: t-statistics in parentheses, from standard errors clustered on country and year. Sample period is 1975-1998 for 12 countries. Fixed effects are jointly significant at 1 percent level in all specifications. In column 3 the treatment group is as for abortion rights, but in reference to data on contraception; it comprises women who, when the pill became part of public policies, were less than fifty. The indicator of maternity protection used is the number of weeks of job-protected paid leave times wage replacement rate. Definitions of all variables are provided in the Appendix.

Table 6: Variations in treatment effects by religious variables

	Depvar: Life Satisfaction			
	Non-Catholic country	Catholic country	Individual feeling religious	Individual religious denomin.
	(1)	(2)	(3)	(4)
Abortion rights	-0.034	0.112	0.071	0.082
	(1.52)	(3.96)	(3.23)	(3.98)
Treatment group: women with abort. rights	0.051	0.031	0.029	
when childbearing age	(4.49)	(1.78)	(2.23)	
Treatment group and feel religious			0.042	
			(5.47)	
Treatment group and Catholic				0.029
				(4.37)
Treatment group and Greek Orthodox				0.037
				(1.54)
Treatment group and Jewish				-0.039
				(0.86)
Treatment group and Muslim				-0.133
				(2.65)
Treatment group and Protestant				0.041
				(4.32)
Treatment group and other Protestant				0.071
				(6.81)
Control group: women without abort. rights or	-0.014	0.004	-4.26e-4	-0.007
not of childbearing age with abort. rights	(1.07)	(0.42)	(0.05)	(1.00)
Control group: men with abort. rights in the	0.002	0.027	0.017	-0.005
same cohort of age as treatment group	(0.19)	(1.45)	(1.36)	(0.83)
Age effects	**Yes**	**Yes**	**Yes**	**Yes**
Country effects, Year effects	**Yes**	**Yes**	**Yes**	**Yes**
Country-specific linear trend	**Yes**	**Yes**	**Yes**	**Yes**
P-value of F-tests:				
Women T group – women C group	0.00	0.04		
Women T group – men C group	0.00	0.38		
R2	0.96	0.94	0.95	0.95
Obs	178,618	281,335	459,953	459,953

Notes: t-statistics reported in parentheses, from standard errors adjusted for clustering on country and year. Sample period is 1975-1998 for 12 countries. Countries are classified as Catholic is they have a relevant majority of Catholics. These are Belgium, France, Italy, Luxembourg, Ireland, Greece, Spain, and Portugal. Non-Catholic countries are the Netherlands, Germany, Denmark, and UK. Fixed effects are jointly significant at one percent level in all specifications above. Definitions of all variables are provided in the Appendix.

Table 7: Robustness checks

	Breakdown of control groups	"False" law changes	"True" law changes	Partial vs. Full rights	One year before & after law changes	Euphoria after rights?	Sensitivity: end childb. age at 45 years old	Ignoring time series information
			Depvar: Life Satisfaction					Depvar: see note
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abortion rights at the time of the survey	0.118	0.178	0.033		-0.075	0.076	0.069	0.007
	(5.82)	(5.77)	(1.42)		(6.46)	(2.47)	(3.23)	(3.79)
Treatment group: women with abortion rights when childbearing age	**0.023**	**-0.006**	**0.065**		**0.048**	**0.054**	**0.044**	
	5.92	**(0.36)**	**(4.65)**		**(2.90)**	**(3.61)**	**(3.76)**	
Control group: women without abort. rights or not of childb. age with abort. rights		-0.005	0.001	-3.78e-4	-0.014	0.010	-0.005	
		(0.49)	(0.13)	(0.05)	(0.69)	(1.11)	(0.63)	
1 Control group: women in countries with no abort. rights	0.055							
	(5.61)							
2 Control group: women with abort. rights only after childbearing age	-0.057							
	(8.05)							
Control group: men with abort. rights in the same cohort of age as treatment group		-0.051	0.046	0.017		0.034	0.013	
		(3.01)	(3.11)	(1.39)		2.18	(1.11)	
Partial abortion rights				0.086				
				(2.29)				
Full abortion rights				0.064				
				(2.69)				
Treatment group with <u>partial</u> abortion rights				0.049				
				(3.98)				
Treatment group with <u>full</u> abortion rights				0.040				
				(3.19)				
Age, Country, Year effects, Country trend	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**	**In 1st stage**
P-value of F-tests: women T gp – women C gp	0.00; 0.00	0.94	0.00	0.00; 0.00	0.04	0.00	0.00	
Women T group – men C group		0.00	0.00	0.00; 0.00		0.00	0.00	
R2; Obs	0.95; 459,953	0.95; 122,867	0.95; 337,086	0.95; 459,953	0.95; 22,842	0.95; 366,170	0.95; 459,953	1e-4; 196,023

Notes: t-stats in parentheses, from standard errors clustered on country and year. Sample period is 1975-1998 for 12 countries. In column 1, I create two separate control groups. In columns 2 and 3, I separate countries that did not have an effective law change because had widespread underground abortion before the law changed (Greece, Belgium, and Netherlands). Column 4 examines whether partial and full rights have the same effect. In column 5, the panel dimension is reduced to one year before and one after the law change for countries that changed the law between 1975 and 1998. In column 6, I eliminate observations from the year when abortion was legalised and the following <u>five</u> years. In column 7, I check the sensitivity of results to the threshold set for the end of childbearing age. Column 8 results from a two-step procedure aimed at removing the autocorrelation of laws. In the 1st step, data on life satisfaction are regressed on all variables except the law change dummy. The estimated residuals *from the treatment countries only* are regressed on the law dummy in a two-period panel (before-after the law).

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Table 8: Societal effect of abortion rights

Depvar: Life Satisfaction	Set of individual characteristics interacted with abortion rights	β, t-stats	β (abortion rights at time of survey), t-stats
1. By gender	Abortion rights * women	-0.120 (10.19)	0.129 (6.00)
2. By having been treated or not	Abortion rights * women treatment group	0.009 (0.89)	0.101 (4.54)
	Abortion rights * women control group	-0.065 (10.04)	
	Abortion rights * men treatment group	-0.016 (1.49)	
3. By age cohorts	Abortion rights* less than 20 years old	0.091 (2.86)	0.058 (1.79)
	Abortion rights*20-29 years old	0.027 (0.89)	
	Abortion rights*30-39 years old	-0.002 (0.06)	
	Abortion rights*40-49 years old	0.008 (0.30)	
	Abortion rights*50-59 years old	-0.002 (0.08)	
	Abortion rights*60-69 years old	0.007 (0.37)	
4. Political self-placement	Abortion rights*left-wing	-0.026 (4.08)	0.025 (1.11)
	Abortion rights*right-wing	0.121 (16.53)	
5. Whether *i* feels religious	Abortion rights*feel religious	0.070 (8.94)	0.057 (2.69)
6. By gender, whether *i* feels religious	Abort* women *religious	0.047 (5.17)	0.057 (2.69)
	Abort* men *religious	0.098 (11.85)	
7. By religious membership	Abort*Protestant	0.035 (4.04)	0.049 (2.16)
	Abort*Catholic	0.045 (5.83)	
	Abort*Greek Orthodox	0.038 (1.30)	
	Abort*Jewish	0.004 (0.11)	
	Abort* Muslim	-0.158 (4.54)	
Always controlling for: **Being in treatment and control groups** **_Age, Country, Year effects, Country trend_** All regressions: R2= 0.95, Obs= 459,953.			

Notes: t-statistics in parentheses, from standard errors adjusted for clustering on country and year. Sample period is 1975-1998 for 12 countries. Each set of individual characteristics is interacted in different regressions with abortion rights at time t in a regression that controls for abortion rights, being in the treatment group, women/men control groups, age, country, year fixed effects, country specific linear trend, and with clustered errors. Definitions of all variables are provided in the Appendix.

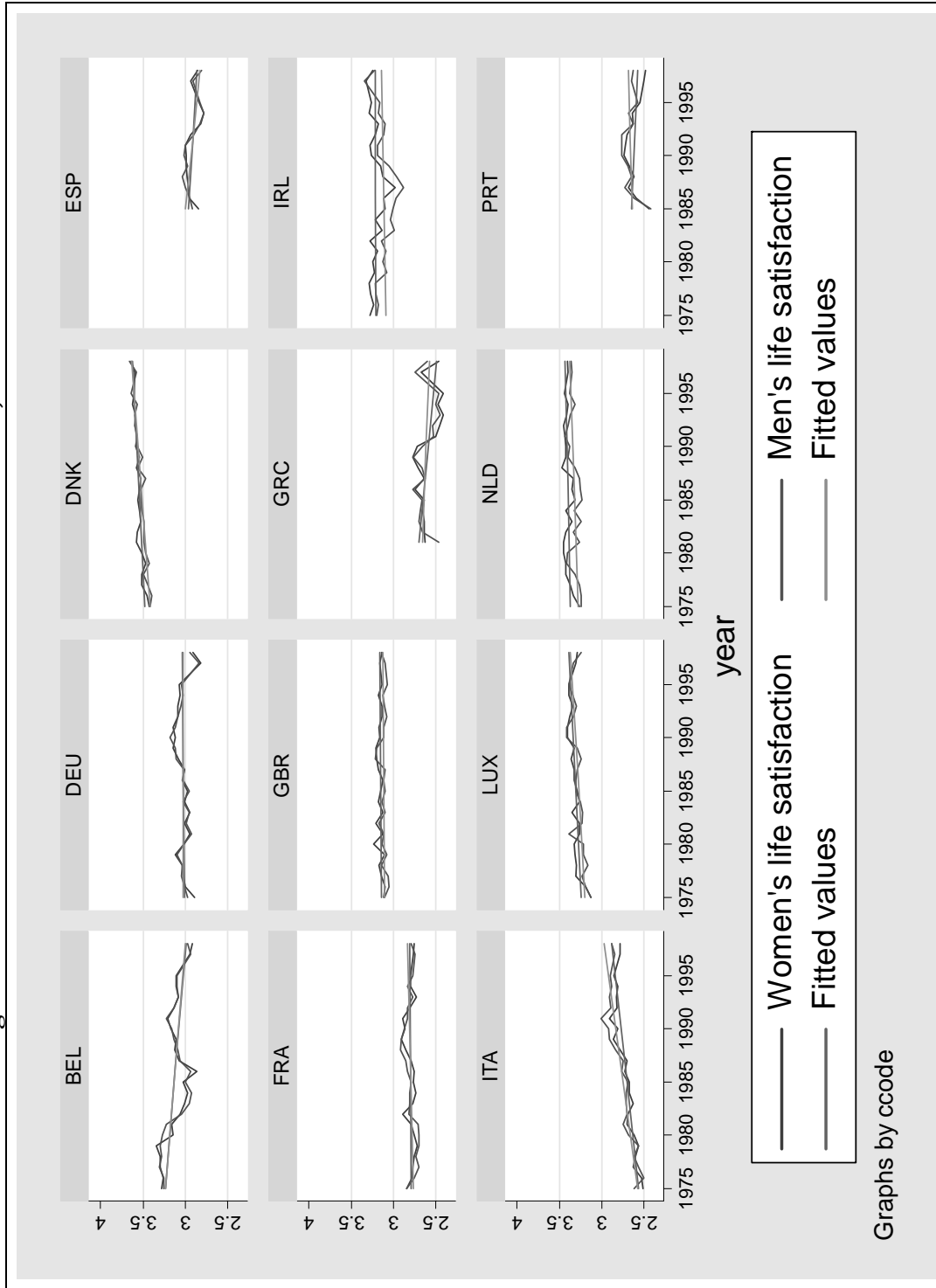## Figure 1: Trends in life satisfaction of men and women, 1975-1998

Graphs by ccode

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Figure 2: Distribution of the categories of the dependent variable (individual life satisfaction)
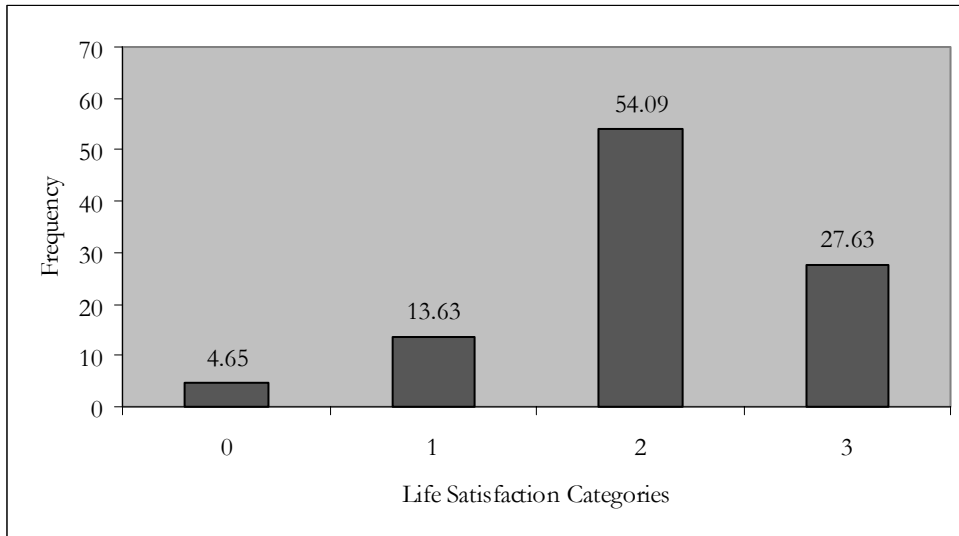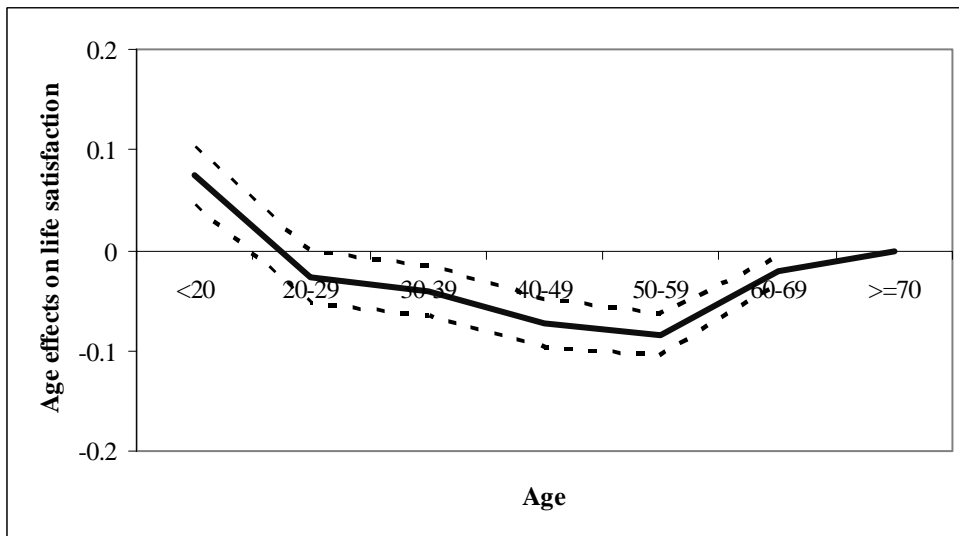

Figure 3: Distribution of age fixed effects on life satisfaction

Notes: coefficients on 10-year age dummies in an estimation of life satisfaction on 10-year age dummies, personal controls, country fixed effects, year fixed effects, country-specific linear trend, with standard errors clustered by country and year.