Education, norms, and gender equality

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Abstract

Despite major developments in gender equality, differences between men and women's economic and social behaviors remain. Several studies demonstrate the importance of gender norms in explaining a significant part of the gender gap. But what shapes gender norms? I provide evidence on the role of education, considered to be a key factor to reach gender equality, in influencing attitudes on gender norms in two different domains: the labor market and household. Exploiting educational reforms in Europe, I find that mandatory education and years of education significantly reduces individuals' level of agreement on the gender norm that the man should be the breadwinner but not on the gender norm that the woman should be the homemaker. The result is consistent with the hypothesis that part of the "stalled revolution" in gender equality is because norms in the household are more rigid than in the labor market, and that educated women face a dilemma between a career and family, or a double burden where they continue to do the lion's share of household work.

Keywords: Gender equality; Education; Gender Norms; Labor market; Household Economics
JEL codes: D10; J16; I20

1 Introduction

Women have experienced several advancements in gender equality over the last decades, in particular in the labor market. Some of the explanations for this are access to birth control, the introduction of anti-discrimination laws, and changes in abortion and divorce laws (Goldin, 2006; Goldin, 2014). Despite the converging roles of women and men, however, gender gaps in the labor market and household remain substantial. Differences in wages, hours of work, and field of occupation persist in all OECD countries and cannot be completely explained by differences in schooling, experience, and job characteristics (Barigozzi et al., 2017; Blau and Kahn, 2006; Fortin, 2005; O'Neill, 2003).

A recent strand of literature focuses on the role of gender norms in creating and maintaining gender gaps. Norms refers to how society believes that individuals should behave (Michaeli and Spiro, 2017). They come with a set of prescribed behaviors that are costly to violate because they cause discomfort and anxiety in oneself and others, and therefore limit choices and behaviors (Akerlof and Kranton 2000; 2010). Gender norms can explain gender differences in household work, paid labor, the choice of fields of education, discrimination, risk-taking behavior, divorce rates, marriage rates, and fertility rates (Akerlof and Kranton, 2000; Bertrand et al., 2015a; Bittman et al., 2003; Brines, 1994; Evertsson and Nermo, 2004; Greenstein, 2000; Fernandez and Fogli,2009; Fortin, 2005; Hwang, 2016; Maxwell and Wozny, 2017; Zetterdahl and Hellström, 2015). The importance of gender norms makes a focus on the determinants of gender norms a crucial step to strive for gender convergence in the labor market (Bertrand, 2015b).[1]

Today, education is considered to be one of the most important tools to reach gender

1. Lippmann et al. (2016) provides a good summary of the explanations for the existence and persistence of gender norms. Women and men can have different comparative advantages that lead to a specialization in the household or labor market (Becker, 1973; 1974), due to biological differences or because different stages of a society's development require different types of economic activity (Alesina et al., 2013). Institutions can create incentives for more or

equality. Gender equality in education is the main target to reach the United Nation's third Millennium Development Goal: To promote gender equality and empower women (SADEV, 2011). Given the importance of gender norms in influencing individual's economic and social behavior, the degree to which educational policies that increase (mandatory) years of schooling also will increase gender equality depends on if and how education influence gender norms in the labor market and household.

In this paper, I present one of the first estimates of the causal effects of education on attitudes on gender norms in the labor market and household. I exploit educational reforms that increased mandatory education in 15 European countries between the 1930s and 1980s. The reforms create an exogenous variation in school-leaving age across countries and birth cohorts that allows me to estimate causal effects. I use data from the European Social Survey (ESS), which includes two statements that capture individuals' attitudes on gender norms: "When jobs are scarce, men should have more right to a job than women" and "A woman should be prepared to cut down on her paid work for the sake of her family." The two statements aim to capture attitudes on traditional gender norms where the man is the breadwinner and the woman is the homemaker.

My analysis proceeds in two steps. First, I estimate the reduced form effect of the reforms in mandatory education on attitudes on gender norms. This implies comparing attitudes on gender norms for the cohorts just affected by the reform with those not just affected by the reform. I find that, on a 5-point scale, individuals exposed to the reform have a significant 0.14 lower level of agreement on gender norms in the labor market but an insignificant

less specialisation and attitudes on gender that persist over time and are transmitted across generations (Esping-Andersen, 2009; Fernandez, 2011; Fernandez and Fogli, 2019; Lippman et al., 2016). It can also be due to the gender norms themselves, which continuously influence women and men's choices and behaviors (Akerlof and Kranton 2000; 2010).

0.02 lower level of agreement on gender norms in the household. Second, I estimate the causal effect of an additional year of education on attitudes on gender norms. Here, I use birth cohorts' different exposure to the reform as an instrumental variable for years of education.[2] The 2SLS estimates show a similar picture to the reduced form estimates. An additional year of education significantly reduces the level of agreement on traditional gender norms in labor market by 0.24 points and insignificantly reduces the level of agreement on traditional gender norms in household by 0.03 points. This suggests that some of the effects of mandatory education on attitudes on gender norms in the labor market can be explained by its influence on years of education. My results are robust to alternative model specifications and robustness checks. A placebo test where hypothetical reforms two years before or after the actual reforms are introduced also confirms that the effects are not driven by pre-trends or country-specific structural changes that also influence attitudes on gender identity norms.

Why should education matter for gender norms? Education increases labor market productivity, earnings, cognitive and non-cognitive skills, and the acquirement and response to information (Card, 2001; Meghir et al., forthcoming; Mocan, 2014; Lange, 2011; Oreopolos, 2006; Price and Simon, 2000). It also lowers criminality, increases civic participation, and lowers religiosity and superstition (Lochner and Moretti, 2004; Milligan et al., 2004; Mocan and Pogorelova, 2014). There is also some evidence that it matters for health (Chou et al., 2010; Gathmann et al., 2015; Grossman, 2006). My investigation starts from the premise

2. Angrist and Kruger (1991) is the first to use educational reforms to identify the causal effect of years of education on earnings in Germany. Since then, several papers use educational reforms in Europe to study the effects of an increase in years of education, including Brunello et al. (2009), Brunello et al. (2013), Borgonovi et al. (2010), Clark and Royer (2013), Fort et al. (2011), Grenet (2013), d'Hombres and Nunziata (2016), Mocan and Pogorelova (2014), and Pischke and Wachter (2008).

that education can give individuals a more open-minded attitude on what women and men are capable of, an increased sense of equality along social identities, and an increased cognitive ability and critical evaluation that leads to the re-evaluation of the cost and benefits of breaking gender norms. It also increases women's bargaining power, increases the opportunity cost for women staying at home, qualifies them to enter more skilled types of professions that men traditionally occupy, which can influence attitudes on women's labor market participation.

This study adds to a small but growing literature that documents the determinants of gender norms. Goldin and Katz (2002) find that the pill increased women's investment's in schooling and increased age at first marriage. Goldin (2006) argues that this is because the pill changed women's adult identities to be more influenced by career considerations rather than traditional gender roles. Fortin (2015) argues that the AIDS crisis created a shock that reversed some of the liberal effect of the pill and resulted to more conservative gender norms in the 1990s. Lippman et al. (2016) find that gender norms can be altered through gender-equalizing institutions, using the 41-year division of Germany as a natural experiment. Taking an intergenerational perspective, Fernandez et al. (2004) find that men growing up with working mothers are more likely to have working wives. Farre and Vella (2013) find a link between a woman's view on the role on role of women in the labor market and her children's views towards women in the labor market and their own labor market participation. Alesina et al. (2013) find that ethnicities and countries where ancestors used plough cultivation have lower female labor market participation rates even today. Olivetti et al. (2016) find that a woman's work behavior is influenced by her mother's and her friends' mothers' work behavior. Ljunge (2017) study the role of cultural dimensions using second-generation immigrants and find that pragmatism has a strong influence on gender norms.

The literature on the role of schooling on attitudes on gender norms is small and inconclusive. Friedman et al. (2011) find positive effects of education on domestic violence,

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whereas Gulesci and Meyerson (2012) and Dincer et al. (2014) find no effect on women's attitudes on gender equality. This paper is perhaps the most related to Erten and Keskin (2017), who study the role of education in shaping different dimensions of domestic violence in Turkey. They find that education has an effect on the psychological violence and financial control that women in rural areas experience. The authors study if changing attitudes on gender norms explains this. Out of seven questions on gender norms, they find an effect in only two of the seven questions, and conclude that the effect of education is instead driven by the labor market channel. Key differences between this and earlier studies on education and gender attitudes is that I use educational reforms in a large number of countries, thus limiting the worry that the effect of the reform or educational attainment is due to other contemporaneous changes at that time, or additional changes that the reform caused, such as a change in the quality of teachers or improved school curricula. I also have questions specifically designed to capture the gender norms in the labor market and household, which allows me to study and contrast the effect of education in these domains. Furthermore, as I have reforms dating back to the 1930s, I am able to use a sample of individuals from a wide age group and not only short to medium run effects. I am also able to include both women and men in the analysis and study heterogeneous effects, which several of the earlier studies do not.

The results in this study highlight that education matters for attitudes on gender norms regarding women and men's equal rights to be a breadwinner, but not when it comes to women's obligations to prioritize the family over paid work and to be a homemaker. The findings are in line with the notion that household work is the strongest form of "doing gender" and more rigid than other gender norms, and that this is part of the reason for the "stalled revolution" in gender equality (Hochschild, 1990; West and Zimmermann, 1987). It supports Goldin's (1997) findings that women, in particular college-educated women, have an increased wish to combine a career and family and Bertrand et al.'s (2010) results that motherhood creates a sharp trade-off between career and family for US female top profes-

sionals. Similarly, Sasser (2005) finds that time constraints due to family responsibilities creates an earnings gap due to reduced working hours. Finally, it is also in line with the argument that the reason why women's gains in equality has not increased women's wellbeing (Stevenson and Wolfers, 2009) is because women now have to succeed as being good mothers, wives, and have careers at the same time (Benabou ad Tirole, 2007). At the aggregate level, it is in line with Hwang's (2016) argument that slow changing norms in the household explains recent demographic changes in countries where there have been vast improvements of women's economic success, such as the decline in the marriage and fertility rates among the educated. From a policy perspective, the results highlights an overlooked limitation of increasing education, especially when the goal is to increase gender equality, as it does not appear to address the issue of inequality in the household. It also broadly suggest that policies that more specifically aim to influence gender norms in the household, such as paid paternity leave, is needed to increase equality further.

The rest of the paper is organized as follows. Section 2 presents the data. Section 3 presents the empirical strategy. Section 4 presents and discusses the results. Section 5 concludes.

2 Data

The empirical analysis uses data from three rounds of the ESS. The ESS data has been collected biannually since 2000, and each wave contains a nationally representative sample of 1,500 individuals aged fifteen and above in a large number of European countries.

There are two reasons to use the ESS data. The first reason is that in the waves of 2002, 2008, and 2010, the ESS asked how much the respondent agrees with two statements related to the respondent's attitudes on gender norms. The statements are 1) "When jobs are scarce, men should have more right to a job than women" and 2) "A woman should be prepared to cut down on her paid work for the sake of her family." Each question allows five possible answers: Agree strongly, Agree, Neither agree nor disagree, Disagree, and Disagree

strongly. Similar to Bertrand et al. (2015a), I interpret a larger degree of agreement on the first statement as expressing the view that it is more important for men than for women to be employed in the labor market. That is, that men are supposed to be the breadwinners in the household.[3] I interpret a larger degree of agreement on the second statement as expressing the view that women have an obligation to prioritize the family over work. That is, that women are supposed to be the homemakers in the household.[4] I recode the answer alternatives to each statement so that Strongly Agree = 4, Agree = 3, Neither Agree nor Disagree = 2, Disagree = 1, and Strongly disagree = 0. In other words, a higher value indicates that the individual's attitude is in accordance with the gender norms that it is more important for men to be employed in the labor market relative to women and that women have an obligation to prioritize family over work.

The second reason to use ESS data is the large sample size. The final sample consists of individual-level data from 15 European countries that carried out a total of 18 educational reforms between the 1930s and 1980s. The variation in the number of years of mandatory education is between +1 and +4 years. The countries are Austria, Belgium, Denmark, Ger-

3. Fortin (2005) finds that it is one of the most important explanatory factors when it comes to cross-differences in female employment rates.

4. One may be concerned that the second variable does not directly capture attitudes on the traditional gender role of women as homemakers. For example, it is possible that it captures how much the individual believes that the family should take priority over work for both women and men (Park et al, 2012). Unfortunately, the ESS does not ask whether men should be prepared to cut down on paid work for sake of family. Nonetheless, changes in attitudes when it comes to the latter question does capture the opinion about women's role in the household.

many (West), Finland, France, Greece, Ireland, Italy, The Netherlands, Portugal, Spain, UK- England, UK- Scotland, UK- Northern Ireland. The reforms were implemented at the national level, but there was regional variation in the timing in Finland and Germany. Appendix Table A1 provides a summary of the reforms, and the Technical Appendix describes the reforms in more detail.[5] I exclude non-natives to ensure that individuals went through the educational system in the country they live in. [6] I also exclude individuals that are currently in full-time education. To ensure that the estimates are not confounded by other contemporaneous changes in the country that affected different birth cohorts' attitudes on gender norms, I include individuals born up to four years before and after the pivotal cohort first affected by the reform. Due to the timing of the reforms, this means that the sample includes individuals between the ages of 20 and 80. The final sample size is around 10.000 individuals.

[TABLE 1 ABOUT HERE]

Table 1 presents the summary statistics by country and the cohorts affected by the reform on the two statements on gender norms and the individual controls. The table highlights two interesting facts about attitudes on gender norms. First, there are stark differences in self-reported attitudes on gender norms between countries. It is possible that cultural differences and institutions can explain this. The Nordic countries have less traditional gender norms and what can be considered more of a dual-earner model. Policies and norms encourage a more equally shared division of work in the household and labor market. For example, formal childcare provision is a social right (Plantenga and Remery, 2009), and the compensation for

5. There is one reform per country, except for Portugal where there are four reforms.

6. For both Finland and Germany I use current region of residence as a proxy for region of residence at the time of the educational reform.

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parental leave is high. Furthermore, fathers in Finland are entitled to more than one month of paid paternal leave (the same is true for fathers in Germany). These policies may both reflect gender norms regarding work and family and influence gender norms. Countries such as Germany, the UK, and Spain have intermediate gender norms. They have unpaid or short paid parental leave, low provision of paid paternity leave (except Germany), and expensive childcare provision (UK and Spain). These policies are less egalitarian than the ones in the Nordic countries (Fahlen, 2016).[7] Second, attitudes differ on the two gender norms. Overall, individuals have a lower level of agreement on gender norms in the labor market than gender norms in the household. The sample average for the former is 1.27, and for the latter it is 2.00. This pattern is found in all countries, albeit to varying degrees. Although these are differences only in means for the two questions, it supports the notion that the norm of the man as the breadwinner is less rigid than the notion of the woman as the homemaker, which is in line with the argument that household work is the main expression of doing gender.

3 Identification strategy

The research design consists of selecting a span of birth cohorts who were the first to be subject to the post-reform educational system and the last to be subject to the pre-reform educational system. Thus, each individual is assigned to either the pre-reform group (non-treated) or the post-reform group (treated).

3.1 Reduced form estimation

I first explore whether there is an effect of reforms in mandatory education on attitudes on gender norms. In the reduced form model, the relationship between reform status and

7. Note, however, that due to differences in the timing of the reforms, one should not make any strong conclusions from the comparison of differences in means across countries.

attitudes on gender norms is the overall effect that includes all mechanisms that connect mandatory education and attitudes on gender norms. Thus, the reduced form estimates show if there is any effect of mandatory education on attitudes on gender norms. The model is:

(1) $$Attitudes_{ikct} = \beta_0 + \beta_1 Reform_{kc} + \beta_2 X_{ikct} + \mu_c + \mu_t + \epsilon_{ikct}$$

$Attitudes_{ikct}$ is the self-reported attitude on gender norms of individual i born in year k and country c, interviewed in survey year t. It lies between 0 and 4, and I use the whole five-point scale in the analysis.[8] A fall in $Attitudes_{ikct}$ means that the individual has a less traditional view of the gender norm of interest. $Reform_{kc}$ takes the value 1 if the individual born in year k is in in the treated group affected by a reform in mandatory education in country c and 0 otherwise. The vector X_{ikct} captures individual characteristics and includes the individual's age, age squared, gender, whether the individual belongs to a minority group, whether the individual's mother is an immigrant, and type of residence dummies. μ_c and μ_t are country-specific and survey-specific dummies, respectively. A crucial assumption of the identification strategy is that, conditional on the covariates in equation (1), the timing of the reform is uncorrelated with other changes in the country, for example, cultural, social, and new policies or institutional changes, that also influence gender norms. To ensure that the assumption is fulfilled, equation (1) includes country-specific linear and quadratic birth cohort trends that control for contemporaneous changes in time-varying unobservable factors at the country level. The trends are measured relative to the pivotal cohort first affected by the reform and are allowed to differ on either side of the reform threshold. The estimated

8. I follow Ferrer-i-Carbonell and Frijters (2004) who study subjective well-being data and show that it makes qualitatively little difference whether one assumes ordinality or cardinality. In the sensitivity analysis I also use (IV) ordered probit and (IV) probit.

standard errors are clustered at the country by birth cohort level.[9]

The coefficient of interest is β_1, which identifies the effect of the reforms in mandatory education among the group of compliers, i.e. those affected by the reform. A non-zero estimate of β_1 would indicate that there is an effect of reforms in mandatory schooling on the level of agreement on traditional gender norms.

I estimate equation (1) with a sample of treated and non-treated individuals born before and after the first affected (pivotal) cohort. A larger bandwidth increases sample size and allows the models to be estimated with power. However, it increases the risk that other country-specific circumstances affect the birth cohorts in ways that also influence their attitudes on gender norms and bias the estimate. In the main analysis, I use a bandwidth that includes the four cohorts born before and after the pivotal cohort in order to ensure that the treated and non-treated individuals are comparable. Since individuals cannot chose their year of birth, it is unlikely that exposure to the educational reform is correlated with other unobserved individual characteristics that would also determine attitudes on gender norms.

3.2 2SLS estimation

As discussed, the reduced form effect is the overall effect of all mechanisms that links reforms in mandatory education with attitudes on gender norms. I next use information on years of education to study the effect of educational attainment on gender norms. The methodological challenge when estimating the causal effect of education on attitudes on gender norms is that education can be related to unobservable individual and family characteristics that are also related to attitudes on gender norms. Also, if attitudes on gender

9. In Portugal's case, I include reform-specific country dummies, country-reform-specific birth cohort trends and cluster at the reform and cohort level. In the case of Germany, I cluster at the region and birth cohort level.

norms influence society's capacity to use its pool of talent and grow, then gender norms, the level of economic development, and educational attainment in the country are endogenous. As a result, an OLS model that includes years of education as an independent variable may introduce an omitted variable bias that suggests that more education leads to a change in attitudes on gender norms when education may not have an actual causal effect per se. Any conclusions on the role of education in forming gender norms based on the findings in an OLS model may therefore be incorrect. Thus, it is necessary to use an exogenous change in education. I therefore next use an IV framework where I instrument years of education with a dummy that takes the value 1 if the individual is in the treated group and 0 if the individual is in the control group. The model is:

$$(2) \qquad Attitudes_{ikct} = \beta_0 + \beta_1 Education_{ikct} + \beta_2 X_{ikct} + \mu_c + \mu_t + \epsilon_{ikct}$$

$$(3) \qquad Education_{ikct} = \alpha_0 + \alpha_1 Reform_{kc} + \alpha_2 X_{ikct} + \mu_c + \mu_t + \epsilon_{ikct}$$

Here, equation (2) is the second stage, and equation (3) is the first stage. Similar to the reduced form framework, $Attitudes_{ikct}$ is the self-reported attitude on gender norms of individual i born in year k and country c, interviewed in survey year t. Here, $Education_{ikct}$ is the endogenous variable educational attainment. It is the number of years of education, and I instrument it with the exogenous $Reform_{kc}$ variable. As before, the vector X_{ikct} captures individual characteristics and includes the individual's age, age squared, gender, whether the individual belongs to a minority group, whether the individual's mother is an immigrant, and type of residence dummies. μ_c and μ_t are country-specific and survey-specific dummies, respectively. I also include country-specific linear and quadratic birth cohort trends. The standard errors are clustered at the country by birth cohort level. [10]

10. I treat the case of Portugal and Germany the same as in the reduced form model.

An important assumption in the IV framework is that reforms in mandatory education only affects attitudes on gender norms through the effect on years of education. However, it is possible that there were other changes in the educational system at the time of the reform (Garrouste, 2010; Fort, 2006). For example, the reforms in the UK also included an increase in the number of teachers, school buildings, and infrastructures. In Greece, the reform also included new textbooks and curricula. In Denmark and Spain, the reforms also included a new comprehensive school (d'Hombres and Nunziata, 2016). As a result, some of the reforms also include an overall increase in the quality of education that may influence attitudes on gender norms. This speaks to the necessity to include linear and non-linear country-specific trends, as these capture the potential confounding factors. In this case, the $Reform_{kc}$ variable captures the common change due to all reforms. If all reforms include the same change in other types of educational inputs, then I estimate the joint effects of an increase in the years of education and the increase in the quality of education (d'Hombres and Nunziata, 2016). However, Brunello et al. (2013b) develop and implement a test that supports the validity of the educational reforms as an IV for educational attainment.[11]

11. The educational system as an institution may also affect attitudes on gender norms, for example, through the learning environment and if teachers and the curriculum encourage gendered behavior, such as leadership and competition. One noteworthy example is former East Germany, which made several institutional reforms and introduced policies specifically designed to promote gender equality up until the unification. Examples of this are work-balance programs, kindergarten and other childcare facilities (Bauernschuster and Rainer, 2012). This changed gender roles in former East Germany and much of this persists today (Lippmann et al., 2016). At the time of the unification, there was an educational reform in former East Germany where mandatory education was lowered from 10 to 9 years. It is possible that this change coincided with other reforms that discouraged gender equality, as former East Germany adopted former West Germany's institutions and policies. As a result, East Germany is excluded from the analysis. While possible, it is unlikely that all the

4 Results

4.1 Educational reforms and attitudes on gender norms

4.1.1 Reduced form results

Table 2 presents the reduced form estimation results on the effect of mandatory schooling reforms on attitudes on gender norms. Panel A and B show the results for gender norms in the labor market and household, respectively. Column (1) shows the results when only reform status is included as a variable in addition to country and survey year dummies. Column (2) adds individual controls. Column (3) adds country-specific cohort linear and quadratic trends. It is the preferred model specification.

Panel A in Table 2 presents the impact of reforms in mandatory education on gender norms in the labor market. In column (1), which reports the effect of the reforms without any additional controls besides country dummies and survey year dummies, the reduced form estimate suggests that the effect of reforms in mandatory education is a significant fall in the level of agreement on gender norms in the labor market by 0.11 points on a five-point scale. Column (2) adds individual controls. This slightly decreases the size of the estimate, which shows that the reforms reduced the level of agreement on traditional gender norms in the labor market by 0.09 points. In column (3), which adds country-specific linear and quadratic cohort-specific trends, the effect of the reforms increases. The point estimate shows that the effect of reforms in mandatory schooling is a fall the level of agreement on gender norms in the labor market by 0.14 points. The effect is significant at the 1 percent level.

Panel B in Table 2 presents the impact of reforms in mandatory education on gender norms in the household. In column (1), the reduced form estimate suggests that the effect

educational reforms in Europe that I consider were also purposely re-designed to increase gender equality.

of reforms in mandatory education is a significant fall in the level of agreement on gender norms in the household by 0.07 points on a five-point scale. Column (2) adds individual controls. Interestingly, the estimate falls in size to 0.04 and is no longer significant. Column (3) adds country-specific linear and quadratic cohort-specific trends. This further lowers the size of the effect to a fall in attitudes by 0.02 points. The effect remains insignificant.

Among the unreported estimates, I also find gender differences in attitudes on gender norms in the labor market but not in the household. Women have a 0.22-point lower level of agreement with the norm that men are the breadwinners. However, in regard to the norm that women are the homemakers the difference in the level of agreement between women and men is an insignificant 0.01 points.[12] In other words, women and men tend to disagree on women's rights in the labor market but not on women's obligations in the household.

Overall, the reduced form estimates suggest a significant effect of reforms in mandatory education on attitudes on gender norms in the labor market and an insignificant effect on attitudes on gender norms in the household. In other words, the many reforms in mandatory education in Europe during the 20th century appear to have had an impact on attitudes on gender norms in the labor market, but not for gender norms in the household. The results also highlight the importance of including individual characteristics and country- and cohort-specific trends in the model.

[TABLE 2 ABOUT HERE]

4.1.2   Placebo test

A concern may be that the reform variable picks up some time trend or structural changes in each country instead of a true treatment effect of the reform in mandatory education. To study this, I perform a placebo test in the line of Black et al. (2008) and introduce a

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12. Estimates available upon request.

hypothetical reform in mandatory educational in each country. I construct a new variable that captures the placebo reform and add it to the main model.[13] Table 3 presents the results from this exercise. Column (1) gives the baseline reduced-form estimates of the effect of the reform on attitudes on gender norms. Column (2) adds a placebo reform two years in the past. Column (3) adds a placebo reform two years in the future. Adding a placebo reform two years before or after does not alter the reduced-form estimates for the true reform, and the coefficients of the placebo reforms are not statistically significant. The results from the placebo reform exercise suggest that the true years of the reforms are meaningful determinants of gender norms in the labor market but not the household, and that pre-trends or country-specific structural changes are not driving the results.

[TABLE 3 ABOUT HERE]

4.2 Years of education and attitudes on gender norms

The reduced form effect captures the sum of all effects of the reform in mandatory education on gender norms. In this section, I study a potential mechanism by analyzing the effect of years of education on gender norms. Here, I use reform exposure as an instrument for years of education in an instrumental variable framework.

4.2.1 First-stage estimates

It is crucial for the IV framework that the national reforms create an exogenous increase in educational attainment for the cohorts affected by the reform. To evaluate the first-stage relationship between reform status and years of education, I estimate the OLS model presented in equation (3). Table 4 presents the results. Panel A shows the results when

13. I have to include the true reforms in the model since the placebo and true reform overlap for some cohorts.

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looking at the gender norms in the labor market, and Panel B displays the results when examining gender norms in the household.[14] Column (1) shows the results when only reform status is included as a variable in addition to country and survey year dummies. Column (2) adds individual controls. Column (3) adds country-specific cohort linear and quadratic trends.

[TABLE 4 ABOUT HERE]

The results reveal that reform status is highly predictive of an individual's years of education. The correlation is positive and statistically significant in all models. In the preferred model specification (column 3), the average increase in educational attainment as a result of being exposed to a mandatory educational reform is 0.6 years. This is a slightly larger estimate than, for example, Brunello et al. (2009) and d'Hombres and Nunziata (2016) who find effects in the size of 0.3-0.4 and 0.35 years, respectively, but I use information from three ESS waves and they use six ESS waves.[15]

Figures 1 and 2 present visuals of the positive jump in educational attainment due to the reform. Figure 1 shows the average educational attainment for cohorts born four years before and after the pivotal cohort first affected by the reform. Figure 2 shows the estimated effect of the reform on educational attainment for the same window of cohorts. It is the average educational attainment, net of covariates. I calculate it using the residuals of years of schooling before and after the reform after I remove the influence of variables and cohort birth

14. The sample size differs slightly but nearly all individuals answer both questions.

15. When I use all six waves of the ESS, the first-stage results are around 0.4. This is in line with other studies and I am therefore not concerned that the individuals that answer the questions on gender norms is a sample unrepresentable of the whole population.

trends. As expected, both Figure 1 and 2 present the jump in the educational attainment that is the result of the educational reform.

[FIGURES 1 AND 2 ABOUT HERE]

4.2.2 IV results

Table 5 presents the 2SLS estimates from using exposure to educational reforms as an instrumental variable for educational attainment. Here, the reforms in mandatory education create an exogenous increase in educational attainment, which allows for causal interpretations of the estimates. The table's organization is similar to that of the reduced form results presented in Table 2. Table 5 also presents the F-statistic of the instrument. It is above the recommended threshold of 10 in all model specifications (Staiger and Stock, 1997). I am therefore not concerned with having a weak instrument.

[TABLE 5 ABOUT HERE]

Panel A, column (1) reports that an additional year of education reduces the level of agreement on gender norms in the labor market by 0.14 points on a five-point scale. Column (2) adds individual controls. This slightly increases the size of the estimate, which shows that an additional year of education reduces the level of agreement on traditional gender norms in the labor market by 0.19 points. Column (3) adds country-specific linear and quadratic cohort-specific trends. This increases the size of the effect further, and the point estimate shows that an additional year of education reduces the level of agreement on gender norms in the labor market by 0.24 points. The effect is significant at the 1 percent level.

Panel B, column (1) reports that an additional year of education reduces the level of agreement on gender norms in the household by 0.09 points on a five-point scale. The effect is significant at the 1 percent level. Column (2) adds individual controls. The estimate remains similar in size but is no longer significant. Column (3) adds country-specific linear and quadratic cohort-specific trends. This lowers the size of the effect to a fall by 0.03 points.

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The effect remains insignificant, which indicates that an additional year of education has an insignificant effect on the level of agreement on gender norms in the household.

Overall, the 2SLS estimates are in line with the argument that higher education has an effect on attitudes on gender norms in the labor market but not on gender norms in the household. The size of the effect of an additional year of education on gender norms in the labor market is similar to the gender difference in the attitude on the same gender norm. Again, the results also highlight the importance of controlling for individual characteristics and country- and cohort-specific trends in the model.[16]

The difference between the reduced form and 2SLS estimates when it comes to gender norms in the labor market is fairly large. This is most likely because not everyone in the affected cohorts are compliers of the reform, i.e. they would have more than the mandated number of years of education even in the absence of the reform. In this case, the reform has a lower impact on years of schooling. Indeed, the first-stage results suggests that the reforms increased years of education by an average of 0.6 years. This is lower than the increase in years of mandatory education in all reforms, which vary between +1 and +4 years.

4.3   Gender differences

So far I have focused on the average impact of education. Since men and women may have had different possibilities to continue their education during the time of the reforms included in the analysis, it is possible that the effect on one gender drives the effect of mandatory education and years of education on gender norms. For example, Gathmann et al., (2015) find large gender differences in the effect of mandatory education and years

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16. I include both linear and quadratic trends in the main specification, but I have performed all analysis with linear or quadratic trends only. The results do not change. Estimates are avaliable upon request.

of education on health. It is also possible that education only matters for one gender's attitudes because women and men may be affected differently. To examine whether the effect differs for women and men, I interact the endogenous variable years of education and the instrumental variable reform with a gender dummy. Column (2) in Table 6 presents both the reduced form and 2SLS estimates. Interestingly, I find no gender difference in the effect of the reforms in mandatory education or years of education on attitudes on gender norms in the labor market and household.

[TABLE 6 ABOUT HERE]

4.3.1 The importance of exogenous variation

Simple correlations between years of education and attitudes on gender norms may produce biased results. A simple way to assess this is to compare the 2SLS estimates with the naive OLS estimates. In the OLS model, I ignore the potential endogeneity of education and enter years of education as a regressor in the model. Table 7 shows the OLS estimates. The organization of the table is the same as before. Panel A displays the results for gender norms in the labor market and Panel B shows the results for gender norms in the household.

[TABLE 7 ABOUT HERE]

The OLS results for the relationship between years of education and attitudes on gender norms in the labor market show a significant negative relationship. The coefficient estimate is around -0.06 and significant at the 1 percent level in all model specifications. The OLS results for the relationship between years of education and gender norms in the household (Panel B) also show a significant negative and significant relationship, but the size of the estimate is half the size at -0.03.

In all model specifications, the OLS estimates are different than the 2SLS estimates. In the preferred model specification, the OLS estimate suggests that an additional year of education reduces the level of agreement on gender norms in the labor market by 0.06 points

on a five-point scale. In contrast, the 2SLS estimate shows a much larger effect of 0.24 points. This is a stark difference, which suggests that the OLS estimates are biased against finding a role of years of education for attitudes on gender norms in the labor market and highlights the need for exogenous variation in education to identify causal effects. On the other hand, the difference between the OLS and the 2SLS estimates on the effect of education on attitudes on gender norms in the household is not as much about size, but significance of the estimates. Here, the results suggest that OLS estimates are biased toward finding a significant role of education on attitudes on gender norms in the household and highlights the need for exogenous variation in education to identify insignificant causal effects.

The 2SLS estimate is identified using the compliers for the reform, i.e., those whose final education increases due to the reform. Earlier studies using school reforms as an instrument for educational attainment typically find larger treatment effects than those from OLS (Oreopoulos, 2006). Card (2001) and Lang (1993) suggest that this is because the 2SLS estimate is identified using the compliers of the reform, whereas the OLS estimate captures the average effect for the whole population, given that there are no omitted variables and measurement bias. Since the reforms I consider concern changes in mandatory education for grades 4-9, it is likely that the compliers are individuals who would have had low educational attainment before the reform, perhaps due to credit constraints or more need for immediate work. It is also possible that the reform affects these individuals differently in terms of attitudes on gender norms. As a result, a possible explanation why the 2SLS estimates are different from the OLS estimates is that the local average treatment effect (LATE) that I identify is different than the average treatment effect (ATE) on the whole population. This implies that the results are not generalizable to the whole population. On the other hand, Oreopoulos (2006) compares school reforms of different scopes and finds that the ATE and the LATE only differ a small amount, and that the main reason why the OLS estimates are smaller than the 2SLS estimates is not due to differences in the population of the individuals affected by the instrument.

4.4 Robustness checks

I estimate different versions of the main reduced form and IV model to test the robustness of my results. As mentioned in the identification strategy, the choice of bandwidth of the pre- and post-reform cohorts introduces a trade-off between efficiency and bias. To study the results' sensitivity to the choice of bandwidth, I decrease and increase the bandwidth of the control and treatment groups to include individuals born 3 and 5 years before and after the pivotal cohort. Table 8, columns (1) to (2) present the results for both the reduced form and 2SLS estimates. The effects of the reforms in mandatory education and years of education are always significant for gender norms in the labor market and insignificant for gender norms in the household. The F-statistic of the instrument falls below 10 when the bandwidth is 3 years. This is most likely due to the reduction in the sample size.

Next, I add variables that are arguably affected by education and related to attitudes on gender norms: marriage status, number of household members, having children at home, labor market status, and being very religious. Table 8, column (3) presents the results. The estimates of the effect of reforms in mandatory education and years of education on gender norms in the labor market remain similar in size and significant. The estimates of the effect of reforms in mandatory education and years of education on gender norms in the household falls slightly and remains insignificant. Column (4) presents the results when I include the sample weights provided in the ESS data. The size of the estimates of the role of reforms in mandatory education and years of education on gender norms in the labor market falls slightly but remain significant. The effects of mandatory education and years of education on gender norms in the household remain insignificant. Column (5) presents the results when I exclude the pivotal cohort first affected by the reform. Since the implementation of the reform may have taken some time there may be a mix of treated and non-treated individuals in the pivotal cohort for each reform. Since this excludes a large proportion of individuals that arguably were the most affected by the reform and reduces the overall sample size, the F-statistic of the instrument falls to below 10. However, the interpretation

of the results remain the same. Column (6) presents the results when I use an ordered probit or IV ordered probit model to relax the assumption that the distance between the answer alternatives are all equal. This allows me to test whether the assumption in the main model that the outcome variables are continuous matters for the estimated effect. Column (7) presents the results from when I construct binary outcome variables that take the value 1 if the individual answers that they Strongly Agree, Agree, or Neither agree nor disagree with the statement, and 0 otherwise (Disagree or Strongly Disagree). The interpretation of the results do not change when I use an (IV) ordered probit or (IV) probit model.[17]

[TABLE 8 ABOUT HERE]

5 Conclusion

Education is considered a key tool to reach gender equality and a growing literature finds that gender norms can explain some of the gender differences in women and men's economic and social behaviors.

In this paper, I present one of the first estimates of the importance of education in shaping attitudes on gender norms. In particular, this is the first paper to study the separate role of education on gender norms in the two spheres of the labor market and household. My empirical analysis uses birth cohorts' exposure to educational reforms in 15 European countries between the 1930s and 1980s to estimate the reduced form effect of mandatory education on attitudes on gender norms. I then exploit the variation in exposure to the educational reform to identify the effect of years of education on attitudes on gender norms in an IV framework. I find evidence that an increase in mandatory education and years of education lowers the level of agreement on traditional gender norms in the labor market

17. The IV ordered probit model is estimated with the Conditional Mixed Process (CMP) command in STATA, developed by Roodman (2011).

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but not for gender norms in the household. These results are robust to several model specifications, robustness checks, and a placebo test.

The results add knowledge on several important aspects regarding education and gender equality. First, the many educational reforms in Europe during the 20th century that increased mandatory education have had a long-lasting impact on today's attitudes on gender norms in the labor market but not in the household. From a policy perspective, this result highlights a benefit and, interestingly, an important and overlooked limitation of policies that aim to increase education. Without complementary policies that change gender norms in the household, the "stalled revolution" on female empowerment and gender equality may remain. Second, I demonstrate a causal effect of years of education on attitudes on gender norms in the labor market but not the household. This creates a dilemma for educated women who aspire for a career but also feel the obligation to be the homemaker in the household. Together, the results provide supporting evidence for the hypothesis that reason for the remaining gender gap in countries where there has been large improvements in gender equality is, at least partly, due to the fact that gender norms in the household are less malleable than norms in the labor market. This may, in turn, lead to demographic changes in terms of falling marriage and fertility rates and increasing divorce rates, as recently experienced in several countries worldwide.

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7   Figures and Tables

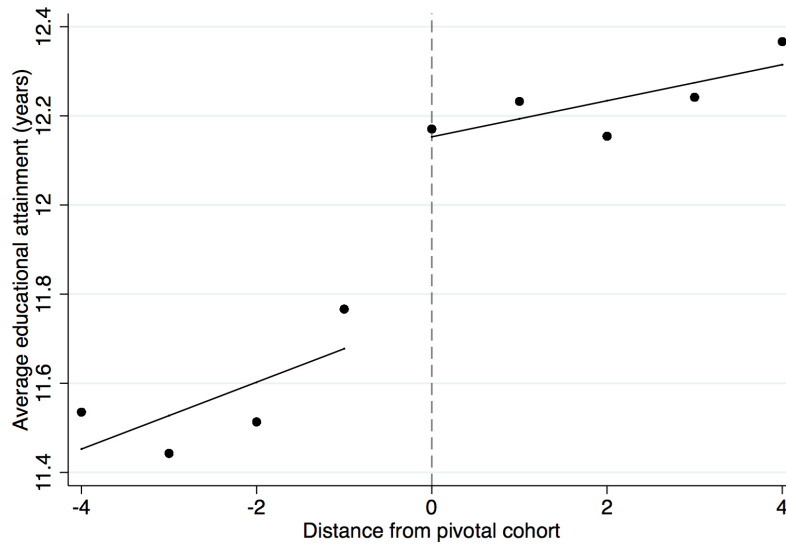Figure 1. Reform exposure and years of education for the 4+/-4 window
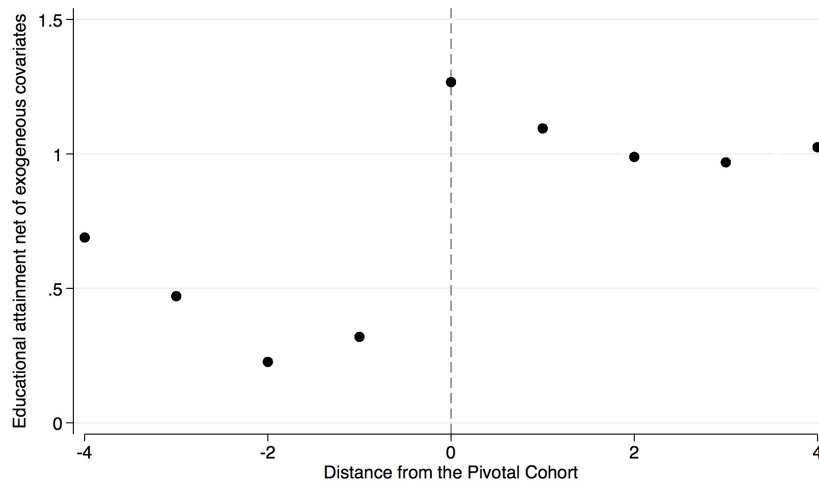

Figure 2. Effect of the reform, net of covariates

TABLE 1:

SUMMARY STATISTICS, BY COUNTRY AND REFORM

	(1) Labor Market	(2) Household	(3) Education	(4) Age	(5) Women	(6) Big city	(7) Small city	(8) Country-side	(9) Minority	(10) Mother
Austria	1.57	2.34	12.2	53.9	0.48	0.28	0.26	0.45	0.01	0.0.06
Belgium	1.16	1.58	13.9	38.8	0.49	0.22	0.23	0.56	0.02	0.08
Germany	1.28	2.12	13.5	55.7	0.50	0.22	0.35	0.43	0.00	0.06
Denmark	0.52	1.09	14	50.5	0.48	0.34	0.34	0.32	0.01	0.02
Finland	0.64	1.28	14.9	43.7	0.48	0.31	0.28	0.41	0.08	0.06
France	1.12	2.07	11.8	54.8	0.51	0.27	0.31	0.43	0.02	0.07
Greece	1.92	2.15	12.0	45.3	0.60	0.62	0.15	0.24	0.03	0.01
Ireland	1.20	1.88	13.4	50.3	0.59	0.28	0.21	0.51	0.01	0.01
Italy	2.13	2.93	10.2	56.0	0.59	0.18	0.65	0.17	0.00	0.00
The Netherlands	1.71	1.97	11.01	71.0	0.53	0.30	0.21	0.48	0.00	0.048
Portugal 1	1.89	2.65	6.28	63.38	0.00	0.41	0.25	0.34	0.01	0.01
Portugal 2	1.56	2.54	6.14	59.4	1.00	0.39	0.28	0.32	0.01	0.01
Portugal 3	1.53	2.46	7.26	52.5	0.62	0.37	0.29	0.34	0.01	0.01
Portugal 4	1.26	2.11	11.47	27.9	0.57	0.43	0.31	0.25	0.02	0.02
Spain	1.16	2.30	11.8	50.5	0.053	0.31	0.26	0.43	0.02	0.01
UK - England	1.14	1.91	13.3	49.5	0.53	0.29	0.47	0.24	0.03	0.05
UK - Scotland	1.12	1.82	13.6	48.0	0.55	0.23	0.46	0.30	0.03	0.04
UK - Northern Ireland	1.22	1.78	12.1	48.6	0.74	0.38	0.39	0.26	0	0.09
Total	1.27	2.00	11.8	49.3	0.55	0.34	0.29	0.37	0.01	0.03

Notes: This table presents the country mean and overall mean of the two outcome variables of interest and the individual controls. Columns (1) and (2) provide the means of the two outcome variables of interest: "When jobs are scarce, men should have more right to a job than women" and "A woman should be prepared to cut down on her paid work for the sake of her family." It ranges between 0 and 4. Columns (3) and (4) list the mean number of years of education and age, respectively. Column (5) shows the proportion women in the sample. Columns (6) - (8) shows the proportion who lives in a big city, small city, and country-side. Column (9) and (10) give the proportion minority groups in the country and who has an immigrant mother, respectively. Source: European Social Survey waves 2002, 2008, and 2010. In Portugal's care, there are four reforms.

TABLE 2:
Reduced form results

	(1)	(2)	(3)
Panel A: "When jobs are scarce, men should have more right to a job than women."			
Reform	-0.105***	-0.091***	-0.142***
	(0.0222)	(0.0353)	(0.0465)
Observations	10,090	9,961	9,961
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."			
Reform	-0.0663***	-0.0382	-0.0196
	(0.0216)	(0.0341)	(0.0449)
Observations	10,082	9,954	9,954
Country	Yes	Yes	Yes
Year	Yes	Yes	Yes
Individual controls	No	Yes	Yes
Trends	No	No	Yes

Notes: This table presents the effect of being exposed to an educational reform on attitudes on gender norms in the labor market and household. The outcome variables are the levels of agreement with the two statements. They range between 0 and 4. Reform is a dummy variable that takes the value 1 if the individual's cohort was exposed to the reform and 0 otherwise. Country is country-specific dummies. Year is survey-specific dummies. Individual controls are age, age squared, gender, belonging to a minority group in the country, if the mother is an immigrant, and area of residence dummies. Trends are country-specific linear and quadratic birth cohort trends. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE 3:

PLACEBO TEST: INTRODUCING HYPOTHETICAL REFORMS AROUND THE ACTUAL REFORM

	(1) Baseline results	(2) -2 years	(3) +2 years
Panel A: "When jobs are scarce, men should have more right to a job than women."			
Reform	-0.142***	-0.146***	-0.102***
	(0.0465)	(0.0468)	(0.0266)
Placebo reform		-0.0385	-0.0406
		(0.0341)	(0.0750)
Observations	9,961	9,961	9,961
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."			
Reform	-0.0196	-0.0157	-0.0431
	(0.0449	(0.0454)	(0.0490)
Placebo reform		0.0369	-0.0865
		(0.0623)	(0.0757)
Observations	9,954	9,954	9,954
Country	Yes	Yes	Yes
Year	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
Trends	Yes	Yes	Yes

Notes: This table presents the reduced-form estimates from a placebo test where the year of the actual reform in each country is moved back two years or forward two years. The outcome variables are the levels of agreement that the individual reports to the two statements on traditional gender norms. They lie between 0 and 4. Columns (2) and (3) provide the first-stage estimates on the effect of reform exposure when the placebo reform is added to the model. Country is country-specific dummies. Year is survey-specific dummies. Individual controls are age, age squared, gender, belonging to a minority group in the country, if the mother is an immigrant, and area of residence dummies. Trends are country-specific linear and quadratic birth cohort trends. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4:

FIRST-STAGE ESTIMATES OF THE EFFECTS OF THE EDUCATIONAL REFORM ON
THE NUMBER OF YEARS OF EDUCATION

	(1)	(2)	(3)
Panel A: "When jobs are scarce, men should have more right to a job than women."			
Reform	0.738***	0.470***	0.565***
	(0.0793)	(0.125)	(0.166)
Observations	10090	9961	9961
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."			
Reform	0.756***	0.474***	0.572***
	(0.0878)	(0.124)	(0.166)
Observations	10082	9954	9954
Country	Yes	Yes	Yes
Year	Yes	Yes	Yes
Individual controls	No	Yes	Yes
Trends	No	No	Yes

Notes: This table presents the effects of the reform on the number of years of education for the main sample. The sample includes individuals born four years before and after the pivotal cohort. Country are country-specific dummies. Year are survey-specific dummies. Individual controls are age, age squared, gender, belonging to a minority group in the country, if the mother is an immigrant, and area of residence dummies. Trends are country-specific linear and quadratic birth cohort trends. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 5:
IV RESULTS

	(1)	(2)	(3)
Panel A: "When jobs are scarce, men should have more right to a job than women."			
Education	-0.142***	-0.194**	-0.242***
	(0.0306)	(0.0808)	(0.0979)
F-statistic of instrument	86.50	14.17	11.58
Observations	10090	9961	9961
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."			
Education	-0.0898***	-0.0806	-0.0343
	(0.0295)	(0.0722)	(0.0776)
F-statistic of instrument	86.84	14.58	11.98
Observations	10082	9954	9954
Country	Yes	Yes	Yes
Year	Yes	Yes	Yes
Individual controls	No	Yes	Yes
Trends	No	No	Yes

Notes: This table presents the effect of the number of years of education on attitudes on gender norms in the labor market and household. The outcome variables are the levels of agreement with the two statements. They range between 0 and 4. Education is years of education. It is instrumented with national reforms that changed the mandatory number of years of education. Country is country-specific dummies. Year is survey-specific dummies. Individual controls are age, age squared, gender, belonging to a minority group in the country, if the mother is an immigrant, and area of residence dummies. Trends are country-specific linear and quadratic birth cohort trends. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE 6:
DIFFERENCES BETWEEN WOMEN AND MEN

	(1) Baseline results	(2) Gender
Panel A: "When jobs are scarce, men should have more right to a job than women."		
Reform	-0.142***	-0.149***
	(0.0465)	(0.0520)
Reform×Female		0.013
		(0.052)
Education	-0.242***	-0.336**
	(0.0979)	(0.144)
Education×Female		0.165
		(0.113)
Observations	9961	9961
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."		
Reform	-0.0196	-0.0322
	(0.0449)	(0.051)
Reform×Female		0.0230
		(0.0505)
Education	-0.0343	-0.0595
	(0.0776)	(0.102)
Education×Female		0.0525
		(0.0781)
Observations	9954	9954
Country	Yes	Yes
Year	Yes	Yes
Individual controls	Yes	Yes
Trends	Yes	Yes

Notes: This table presents the effect of the number of years of education on self-reported attitudes toward gender norms in the labor market and household. The outcome variables are the levels of agreement that the individual reports to the two statements on traditional gender norms. They lie between 0 and 4. Country is country-specific dummies. Year is survey-specific dummies. Individual controls are age, age squared gender, belonging to a minority group in the country, if the mother is an immigrant, and area of residence dummies. Trends are country-specific linear and quadratic birth cohort trends. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** p<0.01, ** p<0.05, * p<0.1.

OLS ESTIMATES OF THE CORRELATION BETWEEN EDUCATION AND ATTITUDES ON
GENDER NORMS

	(1)	(2)	(3)
Panel A: "When jobs are scarce, men should have more right to a job than women."			
Education	-0.0615***	-0.0594***	-0.0595***
	(0.0027)	(0.0028)	(0.0028)
Observations	10,090	9,961	9,961
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."			
Education	-0.0378***	-0.0348***	-0.0346***
	(0.0028)	(0.0028)	(0.0028)
Observations	10,082	9,954	9,954
Country	Yes	Yes	Yes
Year	Yes	Yes	Yes
Individual controls	No	Yes	Yes
Trends	No	No	Yes

Notes: This table presents the correlation between the number of years of education and attitudes on gender norms in the labor market and household. The outcome variables are the levels of agreement to the two statements. They range between 0 and 4. Education is years of education. Country is country-specific dummies. Year is survey-specific dummies. Individual controls are age, age squared, gender, belonging to a minority group in the country, if the mother is an immigrant, and area of residence dummies. Trends are country-specific linear and quadratic birth cohort trends. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE 8:
SENSITIVITY ANALYSIS

	(1) -3/+3 years	(2) -5/+5 years	(3) Endogenous variables	(4) Including weights	(5) Excluding pivotal cohort	(6) (IV) Ordered probit	(7) (IV) Probit
Panel A: "When jobs are scarce, men should have more right to a job than women."							
Reform	-0.169***	-0.107***	-0.142***	-0.141***	-0.134**	-0.156***	-0.111*
	(0.0545)	(0.0412)	(0.0472)	(0.0525)	(0.0643)	(0.046)	(0.566)
Education	-0.327**	-0.210**	-0.314**	-0.211**	-0.327*	-0.214***	-0.182***
	(0.114)	(0.0895)	(0.140)	(0.0866)	(0.174)	(0.0365)	(0.066)
Observations	7,630	12,251	9,426	9,961	8,873	9,993	10,149
F-statistic of instrument	7.06	12.00	7.34	13.47	4.91		
Panel B: "A woman should be prepared to cut down on her paid work for the sake of her family."							
Reform	-0.0513	-0.0261	-0.0115	0.0247	0.0263	-0.0123	-0.0568
	(0.0527)	(0.0401)	(0.0456)	(0.0497)	(0.0620)	(0.0443)	(0.0565)
Education	-0.0975	-0.0505	-0.0246	0.0362	0.00392	-0.0241	-0.101
	(0.101)	(0.0767)	(0.0961)	(0.0744)	(0.116)	(0.0775)	(0.092)
Observations	7,626	12,237	9,418	9,954	8,864	9,4984	10,149
F-statistic of instrument	7.42	12.36	7.97	14.07	4.91		
Controls in main specification	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the effect of reform exposure and number of years of education on attitudes on gender norms. The main specification includes country-specific and year dummies, individual controls, and trends. Columns (1) and (2) include a selected sample born within a bandwidth before and after the pivotal cohort. Column (3) adds the plausibly endogenous variables marriage status, household size, having a child at home, and labor market status to the main model. Column (4) adds sample weights provided by ESS. Column (5) excludes the pivotal cohort first affected by the reform. Column (6) and (7) presents the estimate coefficients when using an ordered (IV) probit model or (IV) probit model. Robust standard errors are in parenthesis, clustered at the country-by-birth cohort level. *** p<0.01, ** p<0.05, * p<0.1.

8 Appendix

TABLE A1:
SUMMARY OF REFORMS

Country	Change in number of years of mandatory education	Year of implementation	First affected cohort
Austria	8 to 9	1962	1951
Belgium	8 to 12	1983	1969
Denmark	7 to 9	1971	1957
Finland (Southern)	6 to 9	1976	1965
Finland (Eastern)	6 to 9	1974	1963
Finland (Northern)	6 to 9	1972	1961
France	8 to 10	1967	1953
Germany (Schleswig-Holstein)	8 to 9	1956	1941
Germany (Hamburg)	8 to 9	1949	1934
Germany (Niedersachsen)	8 to 9	1962	1947
Germany (Bremen)	8 to 9	1958	1943
Germany (Nordrhein-Westfalen)	8 to 9	1967	1953
Germany (Hessen)	8 to 9	1967	1953
Germany (Rheinland-Pfalz)	8 to 9	1967	1953
Germany (Baden-Wrttemberg)	8 to 9	1967	1953
Germany (Bayern)	8 to 9	1969	1955
Germany (Saarland)	8 to 9	1964	1949
Greece	6 to 9	1975	1963
Ireland	8 to 9	1972	1958
Italy	5 to 8	1963	1950
The Netherlands	7 to 9	1950	1936
Portugal	3 to 4	1956 (boys)	1945 (boys)
	3 to 4	1960 (girls)	1949 (girls)
	4 to 6	1964	1956
	6 to 9	1986	1981
Spain	6 to 8	1970	1957
UK - England	8 to 9	1972	1958
UK - Northern Ireland	8 to 9	1972	1958
UK - Scotland	8 to 9	1972	1959

Notes: This table describes the change in mandatory education for the reforms included in the empirical analysis. Please see the Technical Appendix for more information.

9 Technical Appendix

The following section describes the educational reforms in more detail.

9.1 Austria

The 1962 School Ammendment Act increased the mandatory number of years of education from 8 to 9. Starting age remained the same, but leaving age increased from 14 to 15. The law came into effect in 1966. The first affected cohorts were born in 1951 (Mocan and Pogorelova, 2014).

9.2 Belgium

The Loi du Juni 1983 reform increased the mandatory number of years of education from 8 to 12. Starting age remained the same, but leaving age increased from 14 to 18. The first affected cohorts were born in 1969 (d'Hombres and Nunziata, 2016).

9.3 Denmark

The 1971 reform increased mandatory education from 7 to 9 years. Starting age remained the same, but leaving age increased from 14 to 16. The first affected cohorts were born in 1957 (Fort, 2006).

9.4 Finland

The 1972-1977 reform created regional variation in the implementation of Finland's educational reform, which increased mandatory education from 6 to 9 years. Starting age remained the same, but leaving age increased from 13 to 16. I consider three regional variations. Southern Finland implemented the reform in 1976, and the first affected cohorts were born in 1965. Eastern Finland implemented the reform in 1974, and the first affected cohorts

were born in 1963. Northern Finland implemented the reform in 1972, and the first affected cohorts were born in 1961 (d'Hombres and Nunziata, 2016).

9.5 France

The 1959 Berthoin Reform increased mandatory education from 7 to 9 years. It was implemented in 1967. Starting age remained the same, but leaving age increased from 14 to 16. The first affected cohorts were born in 1953 (d'Hombres and Nunziata, 2016).

9.6 Germany

There were regional variations in the implementation of the educational reform, which increased mandatory education in former West Germany from 8 to 9 years. Starting age remained the same, but leaving age increased from 14 to 15. The timing of the reform and affected cohorts is taken from Pischke and von Wachter (2008).

9.7 Greece

The Greek Parliament increased mandatory education by 3 years in 1975. The first affected cohorts were born in 1963 (Brunello et al., 2013).

9.8 Ireland

The 1972 educational reform increased mandatory education from 8 to 9 years. The first affected cohorts were born in 1958 (Fort, 2006).

9.9 Italy

The 1963 reform made junior high school mandatory. Compliance was not instantaneous, however, and it was not until 1976 that the proportion of children who attended junior high

school was close to 100 percent. The first affected cohorts were born in 1950 (d'Hombres and Nunziata, 2016).

9.10 Portugal

Portugal underwent four educational reforms between 1956 and 1986. The first reform was implemented in 1956 and increased mandatory education from 3 to 4 years for boys born in 1945 and after. The second reform was implemented in 1960 and increased mandatory education from 3 to 4 years for girls born in 1949 and after. The third reform was implemented in 1964 and increased mandatory education from 4 to 6 years for individuals born in 1957 and after. The fourth reform was implemented in 1986 and increased mandatory education from 6 to 9 years for individuals born in 1981 and after (d'Hombres and Nunziata, 2016). For Portugal, country and reform-specific dummies and country and reform-by-birth cohort trends are used.

9.11 The Netherlands

The 1950 reform increased mandatory education from 7 to 9 years. The first cohort affected by the reform were born in 1936 (d'Hombres and Nunziata, 2016).

9.12 Spain

The 1970 General Act on Education and Financing of Educational Reform (LGE) increased mandatory education from 6 to 8 years. The first affected cohorts were born in 1957 (Brunello et al., 2013).

9.13 UK: England, Wales, and Northern Ireland

The 1973 reform increased mandatory education from 9 to 10 years. The first affected cohorts were born in 1958 (Fort, 2006).

9.14   UK: Scotland

The 1976 educational reform increased mandatory education from 9 to 10 years. The first affected cohorts were born in 1959 (d'Hombres and Nunziata, 2016).