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Does education predict gender role attitudes?: Evidence from European datasets

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Abstract: This paper presents the first empirical evidence of the causal impact of individuals' education on their attitudes towards traditional gender roles in Europe. We employ two national panel datasets from the UK and Switzerland and a repeated cross-sectional dataset with information from 13 Western European countries for the analysis. To estimate the causal impact of education on gender role attitudes, we exploit the exogenous variation in individuals' education induced by the compulsory school reforms implemented in European countries after World War II. Our results suggest that an additional year of education instigates egalitarian gender role attitudes equivalent of 0.1-0.3 of a standard deviation. While education's moderating effect is particularly prominent among women, we find no evidence of effect heterogeneity concerning individuals' religiosity. Our findings are robust to numerous checks performed and are briefly discussed for their policy relevance.

Keywords: Gender role attitudes, education, compulsory schooling reforms, IV strategy.

JEL Classification: J16, J78, C26

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1 Introduction

Although women’s workforce involvement has observed a dramatic increase recently in many parts of the world, the situation remains far from ideal. For instance, while three women ranked among the top twenty wealthiest people on the planet as per the latest *Forbes* list of billionaires (Forbes, 2020), the richest ”self-made” woman was ranked 211th on this list. Alike this situation, women’s representation in national parliaments in major Western countries is also suboptimal and far from ”gender-equal”, e.g., 25% in the US, 26.4% in the UK, 32.2% in France, 39.1% in Germany, as noted by the latest *Inter-Parliamentary Union* data (IPU, 2019). Despite the evidence on changing assortative mating patterns—women increasingly out-educate and out-earn their partners across the developed world (Esteve et al., 2012; Grow and Van Bavel, 2015; Klesment and Van Bavel, 2017; De Hauw et al., 2017), OECD countries still report a substantial gender wage gap (OECD, 2020).¹

Vast social science literature investigates determinants of gender differences in various outcomes. This research proposes deep-rooted discriminatory social norms and attitudes towards traditional gender roles as *potential* explanations (Vella, 1994; Fortin, 2005; Bertrand, 2011; Alesina et al., 2013a; Blau and Kahn, 2017). Although generally socially unacceptable and covert, newer research shows that gender-based discrimination can be implicit and often *unconscious* or *unintentional* on the discriminator’s part (Reuben et al., 2014). While various anti-discrimination policies are in place, implicit gender stereotyping is challenging to eliminate and is a primary reason why discrimination persists.² The stereotypical attitudes towards gender roles affect women’s careers in science (Reuben et al., 2014), cause them to pay more for credit (Alesina et al., 2013b), postpone motherhood (Bavel, 2010), adversely affecting their employment and gender pay gap (Fortin, 2005).

The existing literature extensively researches about education and its’ importance on several outcomes such as earnings, pensions, and further labor market outcomes (Brunello et al., 2009; Grenet, 2013; Fischer et al., 2020; Hofmarcher, 2021), demographic, health, religion and gender outcomes (Kempton et al., 2011; Brunello et al., 2013; Cygan-Rehm and Maeder, 2013; Jürges et al., 2013; Hungerman, 2014; Gathmann et al., 2015a,b; Tequame and Tirivayi, 2015; Brunello et al., 2016; Fort et al., 2016; Mocan and Pogorelova, 2017; Wilson, 2017; Kırdar et al., 2018; Erten and Keskin, 2019), and intergenerational transmission of education (Piopiunik, 2014; Gulesci et al., 2019). However, the effect of education on gender attitudes has not received sufficient attention. Notable exception include studies that account for the causal effect of education on gender attitudes concentrated on developing countries such as Turkey (Dinger et al., 2014) and China (Du et al., 2021). The former provides no evidence that women’s education altered attitudes toward gender inequality. However, the latter study concludes that another year of education leads

¹As per the *OECD Employment Outlook*, OECD countries reported an average gender wage gap of approximately 13% for 2019. The estimates for the listed Western countries are France (13.7%), Germany (15.3%), UK (16%), and the US (18.5%). Many observable characteristics are often proposed as explanations for the gender wage gap, e.g., occupational choice (Cortes and Pan, 2018; Croson and Gneezy, 2009), occupational segregation (Polachek, 1987), working experience (Light and Ureta, 1995), workforce interruptions (Mincer and Polachek, 1974), barriers to majoring in mathematically intensive academic science fields (Ceci et al., 2014). Notwithstanding, a substantial portion of the gender wage gap is still *unexplained* (Blau and Kahn, 2017, p.790).

²Blau and Kahn (2017, p. 831) define stereotyping or discrimination as ”the practice of judging an individual based on group characteristics, rather than upon his or her own merits”.

to have more gender equal approach in China.

In this paper, we investigate whether individuals' education predicts their attitudes towards traditional gender roles in the western societies. Pertinent for the transmission mechanism, we provide three complementary theoretical arguments that help us hypothesize education's causal impact on gender role attitudes. First, we refer to the research highlighting education's role as a cultural variable (Hainmueller and Hiscox, 2007; Gang et al., 2013), which holds that education reinforces the idea of equality of opportunities and increases social tolerance.³ Second, as many schools voluntarily promote sex education and schools are often the places for children to mingle with the opposite sex, we expect that increases in education are likely to introduce children to gender-equal perspectives (Beaumont and Maguire, 2013).⁴ Third, promoting gender equality discussions in the classroom creates egalitarian gender attitudes approach (Dhar et al., 2018).

Our empirical setup mimics the emerging research demonstrating education's pertinence in determining public attitudes towards immigration (D'Hombres' and Nunziata, 2016; Finseraas et al., 2018; Cavaille and Marshall, 2019; Mazumder, 2019; Margaryan et al., 2021) and political institutions (Siedler, 2010; Cheruvu, 2020; Kunst et al., 2020). To estimate the causal impact of education, we employ the instrumental variables (IV) estimation technique. To this end, we exploit the exogenous source of variation in individuals' education induced by the compulsory schooling reforms implemented in West European countries in the second half of the 20th century. The empirical investigation employs all available information on gender role attitudes present in European datasets. The datasets include a repeated cross-sectional dataset comprising 13 Western European countries, i.e., the *European Social Survey (ESS)*, and two national panel datasets: *British Household Panel Survey (BHPS)* and the *Swiss Household Panel (SHP)*.

Our paper makes the following three contributions to the literature. First, by investigating whether individuals' education predicts their attitudes towards traditional gender roles and work-life balance, we underline education's role as a factor influencing the psychological and noncognitive attributes of gender inequalities. Second, by applying all available self-reported gender attitudes questions included in major surveys from Western Europe, our empirical analysis gives new insights into gender inequality in the region. Being able to address this question is essential for designing policies aimed at mitigating gender inequality. In addition to that, we employ data from various countries to test whether education's gender-equal influence is valid and present across different cultures. Finally, we provide evidence how education is crucial on promoting gender equality in the society. To this end, we also investigate whether education's impact on gender role attitudes differs by gender and religiosity.

Our empirical analysis focused on studying the link between individuals' education and their gender role attitudes

³For instance, Gang et al. (2013, p.13) note that most Western educational systems are designed quite explicitly to increase social tolerance.

⁴The *European Commission* has long recognized the importance of gender equality as a fundamental principle of democracy (European Commission, 1996). Unsurprisingly, European countries undertook many measures to achieve gender equality. For instance, Parker et al. (2009) register that since the 1970s, most European countries have made sex education mandatory in schools (see Table 1 and 2 of the paper). Beaumont and Maguire (2013, p.11) present an expert view of the link between gender equality and sex education. In their report of the *European Parliament*, the authors describe that gender equality is more respected in countries where sexuality education is of higher quality.

faces three critical challenges worth addressing or at least must be mentioned. The first challenge comes from the issue of suspected endogeneity in the relationship of interest. We suspect two sources that make the relationship endogenous: selection bias and reverse causality. The individuals' education level is endogenous; individual-level characteristics, social environment, and cohort effects can shape their education decision. In addition to that, the possibility of reverse causality iterates that individuals, in particular women, with egalitarian gender role attitudes can persevere to educate themselves and report higher education levels (for more discussion, *see* subsection 4.2). We address these challenges using exogenous variation in education induced by compulsory schooling reforms within the restriction window. More specifically, we focus on respondents with birth years seven years before and seven years after the reform (Brunello et al., 2009).

The second challenge comes from the nature of the implementation of compulsory schooling reforms. Beyond increasing the compulsory schooling age, these reforms included other curricular changes in many countries. Brunello et al. (2009) note that in countries such as Belgium, Finland, France, and the Netherlands, the reforms accompanied a change in school design, typically the postponement of tracking. Therefore, we test our hypothesis on the ESS estimation sample with and without these countries and confirm that our main results are not sensitive to the inclusion of observations from these countries. Also, in some countries, multiple reforms were observed in the post-WW II era, e.g., the UK implemented reforms in 1947 and 1972. In our empirical analysis, we focus only on the 1972 reforms. Finally, while we ignore foreign-born individuals from the sample, for internal migrants, we assume that individuals' region of current residence is also their region of birth. However, individuals' region of current residence may differ from their region of birth, and dates of compulsory schooling reforms are also different between these regions of the same country. This issue is particularly problematic for our results estimated using state-level information of observations from Finland and West Germany, as noted in Cavaille and Marshall (2019), can pose a threat to our identification.⁵

The third challenge arises from the fact that our analysis demands individual-level data on various gender attitudes questions. Importantly, to exploit the compulsory schooling reform, we also need information on the individual's birth year and place of residence. Moreover, to apply our systematic restriction window, we require a sufficient number of observations of individuals born before and after the treatment. Fortunately, two longitudinal household survey datasets (BHPS and SHP) and the repeated cross-sectional data (ESS) contained this information. Nevertheless, to the best of our knowledge, other known longitudinal household surveys either do not contain all the necessary information, particularly gender attitudes questions, or suffer from low-coverage issues that hindered our effort to assess the estimation results (for more information, *see* subsection 4.1).

We first discuss the results estimated using the *ordinary least squares (OLS)* estimation strategy. We find a positive association between individuals' education level and their egalitarian attitudes towards traditional gender roles. The

⁵We test and confirm the robustness of our main results after excluding Finnish and West German observations from the estimation sample. Results are available upon request.

causal evidence estimated using the IV estimation strategy also shows that increased education predicts moderation of individuals' gender role attitudes. In terms of magnitudes, we find that an additional year of education instigates egalitarian gender role attitudes equivalent of 0.1-0.3 of a standard deviation. The heterogeneous effects analysis suggests that while education's moderating effects are particularly prominent among women respondents, no effect heterogeneity is found concerning the individuals' religiosity. Our findings are robust to numerous checks performed and are briefly discussed for their policy relevance.

2 Gender role attitudes in Europe

Recently, women have caught up with men in terms of years of completed education.⁶ In many countries, the increases in women's education have been large enough to cause a reversal of the gender education gap (Klesment and Van Bavel, 2017). A consequence of the increase in women's education is that women are increasingly entering into *hypogamy*, i.e., selecting a less educated partner (Esteve et al., 2012; Grow and Van Bavel, 2015; De Hauw et al., 2017). Despite the evidence that families with female breadwinners are on the rise (Raley et al., 2006; Vitali and Arpino, 2016), husbands consistently form the primary earner in most households (Klesment and Van Bavel, 2017), especially in households with children (Budig et al., 2012; Dotti Sani, 2015), contributing to the persistence of gender differences in various outcomes.

Extensive research lists determinants of gender differences in economic outcomes. This literature underlines the resolve of gender-biased societal norms and attitudes as potential explanations (Vella, 1994; Fortin, 2005; Bertrand, 2011; Alesina et al., 2013a). Furthermore, various studies highlight the pertinence of gender-biased labor markets (Polachek, 1987; Ceci et al., 2014), norms regarding motherhood decisions and childcare responsibilities (Budig et al., 2012; Dotti Sani, 2015), and gender differences in preference for education and occupation (Blau et al., 2013; Mandel and Semyonov, 2014; Cortes and Pan, 2018). Given their importance, in this paper, we perform a thorough investigation of individuals' attitudes towards traditional gender roles.⁷

We now describe the state of gender role attitudes in OECD countries, their recent trends, and pertinent correlates. To do this, we refer to the existing research. For instance, Fortin (2005) notes a substantial cross-country variation in anti-egalitarian gender views among the OECD countries and shows that these views have softened in recent cohorts. Using detailed Swiss data, Bornatici et al. (2020) also find that the gender role attitudes became more egalitarian during 2000-2017. Working on American data, Thornton and Freedman (1979) note that egalitarian attitudes among women increased between 1962 and 1977. In contrast, using more recent waves of the GSS, Cotter et al. (2011) shows that the increasing trend in egalitarian attitudes stopped in the mid-1990s, and gender role attitudes have changed

⁶According to Eurostat, in 2020, in the age range of 30-34, women are far more likely to complete tertiary education than men ((45% vs. 34%) in EU-27 countries (https://ec.europa.eu/eurostat/statistics-explained/index.php/Gender_statistics#Education, 2020 data))

⁷For instance, Fortin (2005) shows that the citizens' in-egalitarian gender role attitudes, especially their agreement with the statement "Do you agree that men should have more right to job than women when jobs are scarce?" are strongly correlated with female employment rates and the gender pay gap in OECD countries. Guetto et al. (2015) also show that egalitarian gender role attitudes are correlated with women's labor market decisions in European countries.

little since then. Blau and Kahn (2017) find a similar pattern for the gender pay gap in the US: the gender pay gap improved since the 1970s before the convergence slowed down in the 1990s, and the pay gap level is still substantial in the country.

Concerning the determinants of citizens' gender role attitudes, researchers underscore the importance of numerous individual-level characteristics.⁸ For instance, Algan and Cahuc (2006, 397-398) show that higher education and income are associated with egalitarian gender role attitudes, whereas being male and aged increases the likelihood of in-egalitarian gender role attitudes. In addition, Lottes and Kuriloff (1992) discuss that males and females perceive gender roles differently. Fodor and Balogh (2010) also find that women express more egalitarian gender role attitudes than men. Others suggest that the gender role attitudes may be formed outside the schooling system (Vella, 1994), often developed during the individuals' youth (Vella, 1994), and strongly correlated with their religiosity (Morgan, 1987; Lottes and Kuriloff, 1992; Vella, 1994; Guiso et al., 2003; Algan and Cahuc, 2006; Voicu et al., 2009; Guetto et al., 2015). A broad reading of this research suggests that, as many religious norms focus on the gendered division of labor within the family, religious respondents are likely to hold traditional gender role attitudes and make religious women less likely to join the labor market.

3 Compulsory schooling reforms

Table 1 provides an overview of the compulsory schooling reforms in selected European countries.⁹ These countries, also surveyed in the ESS, include Austria, Belgium, Switzerland, Germany, Denmark, Spain, Finland, France, Great Britain, Greece, Ireland, Italy, Netherlands, and Sweden. For instance, Austria implemented the compulsory schooling reforms in 1962, which affected individuals born on or after 1947 by increasing the compulsory schooling age from 14 to 15 years. Similarly, the reforms in Great Britain took place in the year 1972, affecting those born in 1957, more precisely those born on or after September 1957, by increasing the compulsory schooling age from 15 to 16 years. In Switzerland, the reform took place in 1970 and affected individuals born in 1971.

Beyond cross-country variations in reforms' implementation dates, they also varied by regions and the length of schooling they affected. For example, in Germany, Finland, and Sweden, reform dates varied by region. The ten West German states implemented reforms in different years, as summarized in Table 1, and uniformly increased compulsory schooling by one year (from 8 years to 9 years) across all states. ESS collects detailed information on the individuals' state of residence in Germany, which we use for the empirical investigation. In Finland and Sweden, the implementation of reforms was at the municipal level and was completed progressively.

In Finland, the implementation of reforms took place between 1972 and 1977 and extended compulsory schooling

⁸For understanding the gender role attitudes among immigrants in European countries, *see* Breidahl and Larsen (2016).

⁹Baseline results are estimated using the information on affected cohorts noted in Brunello et al. (2009). However, the reforms implementation dates slightly differ in other research on the topic, e.g., (*see* Cygan-Rehm and Maeder, 2013). To test the robustness of our findings, we employ this alternative information and confirm that the main results hold.

from 6 to 9 years (*see* Pekkarinen, 2008). As Pekkarinen describes, in relatively underdeveloped Northern and Eastern regions of Finland, the implementation of reforms began early and were also more effective due to lower initial education levels. In contrast, the reforms had little impact in most urbanized Southern and Western regions of the country as some portions of the reforms were already in place before the official implementation date (*see* Pekkarinen, 2008, 815). Sweden implemented the reforms progressively on the Municipality level. In 1962, the reform extended compulsory schooling from 7 (or 8) to 9 years affecting the cohort born in 1950.¹⁰ Since ESS does not collect information on the individual’s municipality of residence, we employ a broader definition of the affected cohorts following Brunello et al. (2009) and D’Hombres’ and Nunziata (2016).

The reforms also varied in terms of the length of schooling increased. In most countries, reforms increased compulsory schooling by one year. These include Austria, Switzerland, West Germany, Great Britain, Ireland, the Netherlands, and Sweden. In Spain and Denmark, schooling increased by two years, and in Finland, Greece, and Italy, it increased by three years. In Belgium, the reforms increased schooling by four years.

4 Data and Methodology

4.1 Data

Our empirical analysis uses two national panel datasets from the UK and Switzerland and a cross-national dataset comprising information from West European countries. These datasets include the BHPS, SHP, and ESS.¹¹ The BHPS spans from 1991 to 2008 and provides information on British residents’ individual, household, job, and attitudes-related subjects.¹² Like BHPS, SHP collects job and living conditions-related information of individuals and households from Switzerland. The analysis considers SHP waves between the years 2003 to 2019. Finally, the ESS surveys residents of 32 predominantly European countries and provides information on their various attitudes and beliefs. We focus on the years between 2002 to 2016. Depending on the availability of information on gender role attitudes and schooling reforms details, the baseline results are estimated using the individual-level data from 13 West European countries for the ESS survey waves II, IV, and V.

Outcome variables

We test our hypothesis using all possible gender role attitudes recorded in the surveys. The BHPS records the individual responses to the following six gender role attitudes and preferences toward work and family life:

¹⁰However, Meghir and Palme (2005) note that many Swedish schools had already extended schooling before the reforms’ official implementation in 1962.

¹¹We used the following two criteria to select datasets for our study: 1) availability of outcome variables in the survey, 2) and availability of a sufficient number of observations, especially in the pre-treatment period. While the *Household, Income and Labour Dynamics in Australia (HILDA)* does not include information on individuals’ gender role attitudes, the following datasets suffer from the low pre-treatment observation count: *German General Social Survey (ALLBUS)*, *German Socio-Economic Panel (SOEP)*, *International Social Survey Programme (ISSP)*, and *Panel Study of Income Dynamics (PSID)*.

¹²*Understanding Society* was launched in 2008 as a continuation of the BHPS; however, it incorporated only a small portion of the BHPS’s coverage. Therefore, our analysis only considers BHPS as it covers a more extended period.

- *A pre-school child suffers if the mother works,*
- *Family life suffers if the woman works full-time,*
- *A woman and her family would all be happier if the woman works,*
- *Both husband and wife should contribute to the household income,*
- *Having a full-time job is the best way for a woman to be an independent person,*
- *A husband's job is to earn money; a wife's job is to look after the home and family..*

The responses to these questions range from *1-strongly agree* to *5-strongly disagree*. The survey questions are recorded biannually. Following Balbo and Arpino (2016), we extend the survey's coverage by replacing the missing values with the mean of the observed values from the two adjacent waves of the missing observation.

Unlike BHPS, the SHP contains only one survey question capturing individuals' gender role attitudes, and it is as follows:

- *Pre-school child suffers if mother works.*

While this statement is phrased similar to BHPS statement #1 noted above, the responses fall on a ten-pointer scale ranging from *1-strongly disagree* to *10-strongly agree*. We reverse the responses' order so that higher values on this variable indicate more egalitarian gender role attitudes. The SHP has covered the survey question annually since the year 2003.

Finally, the ESS contains the following two gender role attitudes:

- *Do you agree that women should be prepared to cut down on paid work for sake of family?,*
- *Do you agree that men should have more right to job than women when jobs are scarce?*

The individual responses to these questions range from 1 (*Strongly agree*) to 5 (*Strongly disagree*). The gender role attitudes are present in the ESS survey waves of the following years: 2004, 2008, and 2010.

To streamline the interpretation and draw a general conclusion across different outcomes, we follow Kan (2007) and construct a gender role attitude index (*GA index* hereafter) for the surveys that recorded multiple gender role attitudes, especially for the BHPS and ESS datasets.^{13,14} To do this, we recode component items in the same direction so that a low (high) score indicates inequalitarian (egalitarian) gender role attitudes. Table 2 provides summary statistics of the BHPS and ESS GA indexes, along with information on the components used to construct these indexes. After satisfying Cronbach's alpha for BHPS (0.71) and ESS (0.72), we continue our analyses with the standardized GA index with the mean 0 and variance 1.

¹³Some examples of using this methodology include Sweeting et al. (2014) and Balbo and Arpino (2016).

¹⁴The baseline results are estimated using GA indexes. To show that the paper's central message is not conditional on creating the index, in section 5.1, we also discuss the results estimated separately for each gender role attitude.

Sample construction

To obtain our estimation sample, we apply sample restrictions employed in the extensive literature on the topic (see Brunello et al., 2009; Cygan-Rehm and Maeder, 2013). Accordingly, the sample is first restricted to natives respondents, i.e., we drop first- and second-generation immigrants. We further limit the sample to the working-age individuals (between 26 and 65 years of age) as their gender role attitudes have practical consequences for the women’s labor market outcomes.¹⁵ Following the advice of Brunello et al. (2009) about systematic window sample, we use 7-years restriction windows around the treatment year. In other words, we focus on individuals with birth years seven years before and seven years after the reform.¹⁶ The symmetric window restriction allows us to obtain similar sample sizes with comparable characteristics, simultaneously ensuring us against systematic changes that could occur if a long time window is considered. For instance, a sample with a longer bandwidth window (let us say 20 years) may include post-reform observations that are systematically different (living under a different institutional setting, etc.) than those surveyed 20 years before the reform. The approach diminishes the impact of unaccounted confounding factors. Moreover, the sample restriction also reduces the age effect arising from comparing too young and too old generations in the sample.

Explanatory variables

We consider several exogenous variables as relevant control variables for our analysis. Our explanatory variable of interest is a continuous variable representing the individuals’ years of schooling. The BHPS and SHP do not report the precise number of completed years of education; thereby, we construct schooling years based on the corresponding educational category. We control for cohort-specific fixed effects by using dummy variables indicating the individuals’ year of birth. We account for the regional variation in gender role attitudes by controlling region dummies for BHPS, canton dummies for SHP, and country dummies for ESS. Finally, the model includes survey year dummies to account for survey year-specific changes in gender role attitudes. For the ESS, we also control for the country-specific birth trend and country-related macroeconomic variables such as GDP per capita and inflation rate.

Table 2 presents the summary statistics of the variables employed. The statistics are presented as comparisons of means between the treated group and the control group. The treated group comprises individuals born on or after the reform, while the control group consists of individuals born before the reforms. We additionally report normalized differences in means of our experimental groups. In columns (1)-(3), the statistics are reported for the BHPS data, columns (4)-(6) summarize the SHP data, and columns (7)-(9) describe the ESS data.

The summary of the gender role attitudes recorded in the BHPS suggests that the treated individuals report more egalitarian gender views than the control group, except for the statement *A woman and family would all be happier if she works*. For this outcome, we find that both groups report statistically indistinguishable gender role attitudes.

¹⁵To test the robustness, we relax this restriction and re-estimate the baseline results and confirm that the restriction is not essential for the paper’s central message. The results are available upon request.

¹⁶In subsection 4.3, we relax the seven-year restriction to a ten-year restriction window.

Concerning *BHPS GA index*, we also find that treated individuals report higher values than the control group. They report statistically significant different gender role attitudes and are more gender-egalitarian than the control group. The statistics presented in columns (4)-(6) do not find a statistically significant difference in the experimental groups' means of gender role attitudes. Like the BHPS, the summary of the ESS data presented in columns (7)-(9) also shows that the treated individuals report more egalitarian gender role attitudes than the control group individuals.

Next, we discuss the covariates used in the empirical investigation. First, we notice that the treated group reports larger mean education levels than the control group observations in all datasets. A reason for this difference is the compulsory schooling reforms. However, we also observe that the treated group's average age is much lower than the control group's average age. Together, these observations may highlight a cohort effect as the younger generation is more likely to pursue higher education than the former generation. The age difference may also explain why the treated units report a larger means of outcome variables than control units. For instance, younger individuals are also likely to hold egalitarian gender role attitudes than older individuals. We revisit these issues in the empirical methodology section. The table also summarizes the individuals' socio-economic characteristics, such as marital status, gender, and working status.

4.2 Methodology

The empirical investigation begins by presenting the *ordinary least squares* (OLS) estimates of the association between the individuals' education and their gender role attitudes. Essentially, we estimate the following equation:

$$GA_{it} = \alpha schooling_{it} + \theta cohort_i + \beta year_t + \gamma region_{it} + u_{it} \quad (1)$$

where GA_{it} is the GA index of the individual i at time t , $schooling_{it}$ represents years of schooling of an individual i at time t , $cohort_i$ stands for cohort fixed-effects, $year_t$ is for survey year fixed-effects, $region_{it}$ takes into account region fixed-effects, and u_{it} is the error term.^{17,18} Under this framework, we exploit regional and time variation in education in 7 years pre-and post-reform. For the ESS data analysis, we employ the following additional variables: linear and quadratic country-specific birth cohort trends in the equation as different countries implemented reforms in different years, country-specific characteristics such as GDP per capita, unemployment rate, and woman's labor force participation rates.

However, we suspect that the OLS estimate of the simple association between the individuals' education level and gender role attitudes, i.e., α , is endogenous for several reasons. First, individuals' education level is not exogenous, but it depends on individuals' choices and social norms contemporaneous to the individuals' formative years. For instance,

¹⁷The post-reform increase in education is likely to affect individuals' marriage decisions, employment status, and income. Controlling for these factors might lead to the "bad controls" identification problem as elaborated in Angrist and Pischke (2009).

¹⁸To deal with serial correlation and heteroskedasticity, we cluster standard errors by region level.

being a female was associated with lower educational outcomes historically, but at the same time, females have only recently converged to males' years of schooling, as noted earlier. Second, the association between education and gender role attitudes may work in both directions, and there is a potential problem of reverse causality in the variables of interest. Intuitively, as motivated earlier, individuals with higher education may report more egalitarian gender role attitudes than their low-educated counterparts. Simultaneously, individuals with egalitarian gender role attitudes are also likely to continue to educate themselves and report higher education levels, e.g., the discussion of education and gender role attitudes of younger cohorts noted earlier. Especially true among female respondents, women with more egalitarian gender role attitudes may also indicate egalitarian views towards education and decide to be highly educated than their counterparts who support in-egalitarian gender role attitudes. Finally, the attitudes toward gender roles may depend on parental characteristics, such as their education, working status, school environment, lifestyle changes, and social norms, otherwise unobserved in empirical investigations.

We address the endogeneity in regressor by implementing the instrumental variable strategy (*IV Strategy*). That is, we exploit the exogenous source of variation induced by the compulsory schooling reforms undertaken in European countries in the post- *World War II* era and examine the direction of causality from education to gender role attitudes. We do this by employing the standard *Two-Stage least-squares* (TSLS) approach. We construct a binary indicator *reform* taking the value 1 if an individual was affected by the reform and zero otherwise. In equation (2), we first regress the compulsory schooling reform indicator on the endogenous regressor (*schooling_{it}*) and estimate the predicted years of schooling $\widehat{schooling_{it}}$. In the second step, we replace the predicted years of schooling $\widehat{schooling_{it}}$ in equation (1) (in place of *schooling_{it}*) and estimate the impact of years of schooling on the individuals' gender role attitudes.

$$schooling_{it} = \lambda_1 reform_i + \lambda_2 cohort_i + \lambda_3 year_t + \lambda_4 region_i + \epsilon_{it} \quad (2)$$

The identification strategy requires that the first stage regression yields a statistically significant result, and the correlation between *reform_i* and the years of schooling is not weak. In the next section, we discuss the first stage results and provide supporting evidence of the instrument variable's validity. As the exclusion restriction assumption can not be tested directly, we provide an in-depth discussion. The assumption requires that the compulsory schooling reforms affect gender role attitudes exclusively through individuals' increased education level. First, we note that the reforms affected individuals based on their birth cohorts. As the reform dates were set after individuals were born, parents could not make childbearing decisions based on the reform, except in Switzerland. The reform might also lead to another systematic change in the school system, such as affecting school quality or increasing assortative mating (Black and Devereux, 2011; Holmlund et al., 2011). The change in these channels potentially has an impact on gender role attitudes. Brunello et al. (2009) are also concerned about the instrument's internal validity, and their

analyses do not reject the validity. Furthermore, we assume that our inclusion of region and cohort fixed effects to control for region-specific or cohort-specific differences in social norms, family orientations, or religious affiliation helps us to address the exclusion restriction concern.

In our analysis, the identification depends on the following assumptions. First, we assume that compulsory schooling reforms affect individuals' education level only and have no impact on their gender role attitudes. Second, the reforms have an exogenous effect on individuals' years of schooling to the extent that the effect depends on their year of birth. At the same time, we allow individuals to decide the type and level of education endogenously, i.e., highly gifted students tend to stay in school longer. Third, it is likely that individuals working (living) in an egalitarian environment also have a more egalitarian gender role attitude than individuals working (living) in an in-egalitarian environment. Finally, in some sample countries (e.g., Sweden and Finland), the compulsory schooling reforms were implemented at the municipality level and at different times. As we lack information on the respondents' municipality of birth, we assume that our main results do not depend on this lack.¹⁹

As we estimate the *local average treatment effect* (LATE), our compliers are individuals who are causally affected by the compulsory school reforms. The most crucial assumption to be satisfied for the LATE to provide unbiased estimates is the assumption of monotonicity (Angrist and Pischke, 2009), which requires that individuals are affected by the reform in the same manner. In other words, no one drops out of school or attends secondary school instead of continuing their education to high school (i.e., reducing education). We test for this assumption by checking whether the reform led to a reduction in education. For this exercise, we regress the reform dummy on the probability of graduating from elementary school. We record negative but statistically insignificant coefficients for all datasets (results available upon request).

5 Results and discussion

5.1 Main results

The baseline results are presented in Table 3. In column (1), we present the estimates of OLS regressions. We find that higher education is associated with individuals' egalitarian gender role attitudes in almost all specifications. In terms of magnitude, a year increase in education is associated with approximately a 0.05 to 0.08 standard deviation increase in GA indexes, statistically significant at the 1% level.

Next, we discuss the results of the IV estimation strategy. The discussion begins with the visual examination of the first stage graphical results. Figure 2 shows whether compulsory schooling affects individuals' education and that the IV is valid separately for all datasets and countries present in the estimation sample. The general tendency of graphs is to reveal a higher education for younger cohorts and a discrete jump for the first birth cohort after the

¹⁹Our results are robust excluding observations from these countries.

reform. In all countries, we observe a jump of roughly 0.5 to 1.3. However, the post-reform increase in education is not permanent, and in some countries, we observe relatively smaller education effects, e.g., Belgium, Great Britain, Netherlands, and Sweden.

Columns (2)-(4) present the results of the IV estimation strategy. In column (2), we discuss the first stage statistics, which suggest that compulsory reforms increase years of education by approximately one year in the BHPS dataset, 0.57 years in the SHP dataset, and by 0.33 years in the ESS dataset. The results are statistically significant in all specifications. Moreover, we report that the first stage *F-statistics* are above 10 in all specifications, which, as advised by Staiger and Stock (1997), supports the IV’s validity. Column (3) of Table 3 presents the second stage estimates. We find that increases in individuals’ education moderate their gender role attitudes in all datasets. A one-year increase in education increases the GA index by 0.3 sd in the UK, 0.19 sd in Switzerland, and 0.1 sd in the ESS. All LATE estimators are statistically significant at 1% level. In other words, we conclude that education matters for shaping individuals’ attitudes towards gender roles and work preferences.

Now, we show the results separately for each gender role attitude. Table 8 presents the results. A broad reading of the table suggests that, in most specifications, the results are qualitatively similar to the baseline estimates discussed earlier, except for a statement capturing the individuals’ gender role attitudes in the BHPS dataset. We find that increases in education make respondents agree with the following two statements: *Both husband and wife should contribute to the household income* and *Having a full-time job is the best way for a woman to be an independent person*. Our findings indicate that while education induces egalitarian attitudes towards women’s labor market participation in Britain, it instigates inegalitarian expectations towards women’s equal contribution to the household income.

5.2 Heterogeneous effects

This section examines whether and to what extent gender role attitudes differ across population subgroups. As noted earlier, male and female respondents perceive gender roles differently (Lottes and Kuriloff, 1992) and that these attitudes are likely to be associated with their larger belief systems, e.g., religiosity (Morgan, 1987; Lottes and Kuriloff, 1992). In response, we estimate the heterogeneous effects associated with the individuals’ gender and religiosity. We do this by estimating the baseline regressions separately for men and women respondents and religious and non-religious respondents. To define the individuals’ religiosity, we employ the survey question asking the following: “How often do you attend religious services apart from special occasions?” Using this information, we create a dummy variable taking the value of 1 if they attend religious services and 0 for those who “never” attend religious services.²⁰ These subgroup-specific heterogeneous effects are documented in Table 4 and Table 5. We find that while increases in education yield more egalitarian gender role attitudes among females across all datasets. For

²⁰Recent research by Hungerman (2014) find that higher levels of education lead to lower levels of religious affiliation later in life, a result providing additional supporting evidence for considering heterogeneous effects associated with the individuals’ religiosity.

the BHPS dataset, we find supporting evidence among males, whereas we steer clear of making a definitive conclusion for male data in other datasets due to less than ten first stage *F-statistics*. Concerning the individuals’ religiosity, our results do not find any evidence of heterogeneous effects.

5.3 Robustness checks

This subsection presents robustness checks of our main results. First, we re-estimate our baseline results after extending the bandwidth window of sample restriction to a 10-year window (Brunello et al., 2009; Cygan-Rehm and Maeder, 2013). Table-7 present the results, which confirm a positive and sizable effect of education on the individuals’ gender role attitudes. Second, we apply alternative definitions of the years of schooling. Exceptionally, BHPS records the individuals’ completed education as well as their school-leaving age. Therefore, in Table 6, we present the BHPS results estimated using individuals’ *school leaving age* instead of *Years of schooling*. While fewer reported their school leaving age, results in large missing values in the estimation sample, its use provides a useful check for our endogenous variable of interest. Our results report a statistically significant first stage results, and the second stage reports findings similar to our baseline results.

6 Conclusion

Despite considerable improvements in recent decades, the gender gap in various outcomes is still a major concern among policymakers. In 2017, when the worldwide movement against women’s sexual harassment at workplaces (*#MeToo* movement) gained momentum, broader questions concerning women’s workplace experiences emerged. The extensive existing research links the gender gap’s origins to deep-rooted discriminatory social norms regarding traditional gender roles. Using diverse international datasets, in this paper, we investigated whether individuals’ education has a moderating effect on these norms. Our analysis demonstrated that increases in education instigated egalitarian gender role attitudes in European countries. Furthermore, the findings indicated that education’s moderating effects are particularly prominent among women and that the individuals’ religiosity does not intervene in the relationship.

Our finding that education predicts individuals’ attitudes towards traditional gender roles and work-life balance bears relevance for egalitarian gender policies. In particular, recent research notes that *COVID-19 pandemic* may have persistent implications for gender equality. Unlike past recessions, Alon et al. (2020) note that the pandemic had an enormous impact on sectors with high female employment shares. According to the emerging empirical research, the pandemic-led change in the working environment (e.g., working-from-home possibility) not only disproportionately favored males (Bonacini et al., 2021b), but increased child-caring needs due to subsequent closures of schools and daycare centers are having an additional adverse effect on the productivity of working mothers (Deole et al., 2021), further widening the gender wage gap (Bonacini et al., 2021a). While flexible work arrangements are here to stay

long after the pandemic is over, Alon et al. (2020) predict that its impact on working mothers is likely to be persistent due to high returns to experience in the labor market. We argue that the disproportionate impact of COVID-19 on gender-related social norms and the gender gap in labor market outcomes provides an avenue for future research. In particular, future research can investigate whether households with highly educated couples shared child-caring responsibilities equally? Had egalitarian sharing of household responsibilities moderated the disproportionate effects of the crisis on working mothers?

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Tables and Figures

TABLE 1: Reform dates and affected cohorts

Country	(1) Reform date	(2) First cohort affected	(3) Δ comp. schooling yrs
Austria	1962	1947	14 to 15
Belgium	1983	1969	14 to 18
Switzerland	1970	1971	14 to 15
West Germany			
Schleswig-Holstein	1956	1941	14 to 15
Hamburg	1949	1934	14 to 15
Niedersachsen	1962	1947	14 to 15
Bremen	1958	1943	14 to 15
Nordrhein-Westfalen	1967	1953	14 to 15
Hessen	1967	1953	14 to 15
Rheinland-Pfalz	1967	1953	14 to 15
Baden-Württemberg	1967	1953	14 to 15
Bayern	1969	1955	14 to 15
Saarland	1964	1949	14 to 15
Denmark	1971	1957	14 to 16
Spain	1970	1957	12 to 14
Finland	1974	1963	13 to 16
France	1959	1953	14 to 16
Great Britain	1972	1957	15 to 16
Greece	1975	1963	12 to 15
Ireland	1972	1958	14 to 15
Italy	1963	1949	11 to 14
Netherlands	1975	1959	15 to 16
Sweden	1962	1950	14/15 to 15/16

Note: This table shows the country-specific compulsory schooling reform dates and the first cohort that was affected by these reforms. Online Appendix A gives a detail description of all reforms. Source: Brunello et al. (2009).

TABLE 2: Summary statistics

	(1) Control	(2) BHPS Treated	(3) Norm. Diff.	(4) Control	(5) SHP Treated	(6) Norm. Diff.	(7) Control	(8) ESS Treated	(9) Norm. Diff.
Gender role attitudes									
BHPS GA index (<i>Outcome 1</i>)	18.90	19.28	-0.38***						
A pre-school child suffers if the mother works	2.88	3.10	-0.22***						
Family life suffers if the woman works full-time	3.00	3.14	-0.14***						
A woman and family would all be happier if she works	2.85	2.85	0.00						
Both husband and wife should contribute to the hh income	3.42	3.36	0.06***						
Having job is the best way for a woman to be independent	3.14	3.07	0.08***						
A husband earns money; a wife looks after the home	3.60	3.76	-0.16***						
Child suffers with working mother (<i>Outcome 2</i>)				5.25	5.17	0.09			
ESS GA index (<i>Outcome 3</i>)							6.86	7.05	-0.19***
Women should be prepared to cut down on paid work for sake of family							3.06	3.17	-0.11***
Men should have more right to job than women when jobs are scarce							3.73	3.90	-0.16***
Explanatory variables									
Years of schooling	9.57	10.03	-0.47***	9.14	10.36	-1.22***	12.47	13.36	-0.88**
Age	45.74	38.30	7.45***	40.41	33.12	7.28***	52.43	45.20	7.23***
Year of birth	1953.09	1962.53	-9.44***	1965.67	1974.68	-9.01***	1955.16	1962.41	-7.25**
Heterogeneity checks									
Female	0.53	0.54	-0.01	0.59	0.55	0.04***	0.53	0.530	-0.00
Religiosity	0.33	0.30	-0.03***	0.68	0.62	0.06***	0.66	0.63	0.03***
Supplementary socio-economic information									
Married	0.79	0.79	0.01	0.68	0.53	0.15***	0.68	0.63	0.05***
Working	0.79	0.81	-0.01*	0.89	0.30	-0.01	0.59	0.72	-0.13**
Unemployed	0.04	0.03	0.01**	0.01	0.01	0.00	0.02	0.03	-0.01
Inactive	0.17	0.16	0.00	0.10	0.08	0.01*	0.39	0.25	0.14***
5 quantiles of HH income	3.14	3.06	0.07***	3.15	3.11	0.04	3.30	3.37	-0.06**
Number of persons in HH	3.18	3.39	-0.21***	3.41	2.88	0.53***	2.61	2.97	-0.36***
Observations (N)	29208	39927	69135	7716	5002	12718	8160	9804	17964

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category. The sample includes up to seven birth cohorts before and after a reform.

FIGURE 2: First Stage: effect of reform on years of education

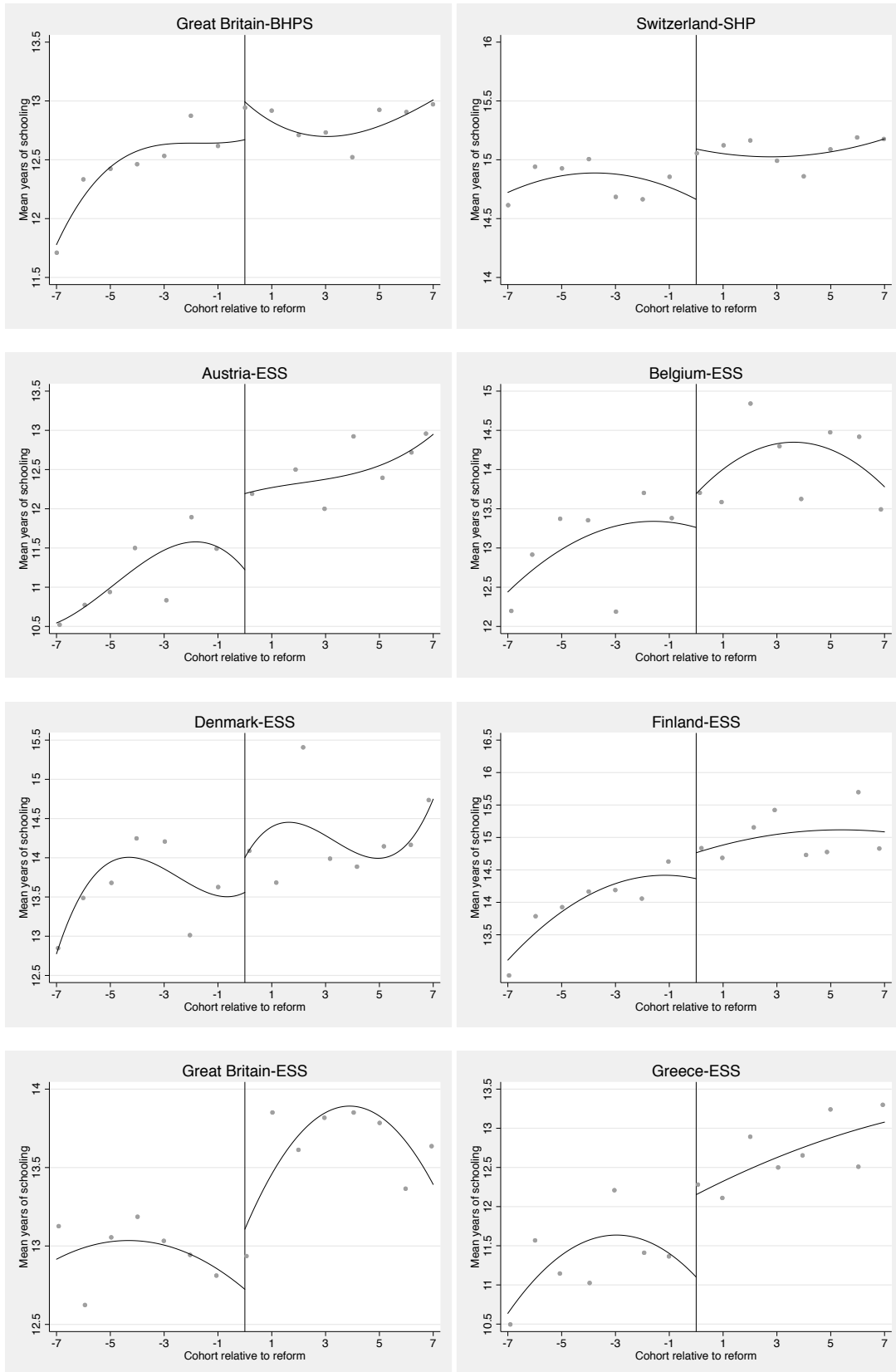


FIGURE 2: First Stage: effect of reform on years of education

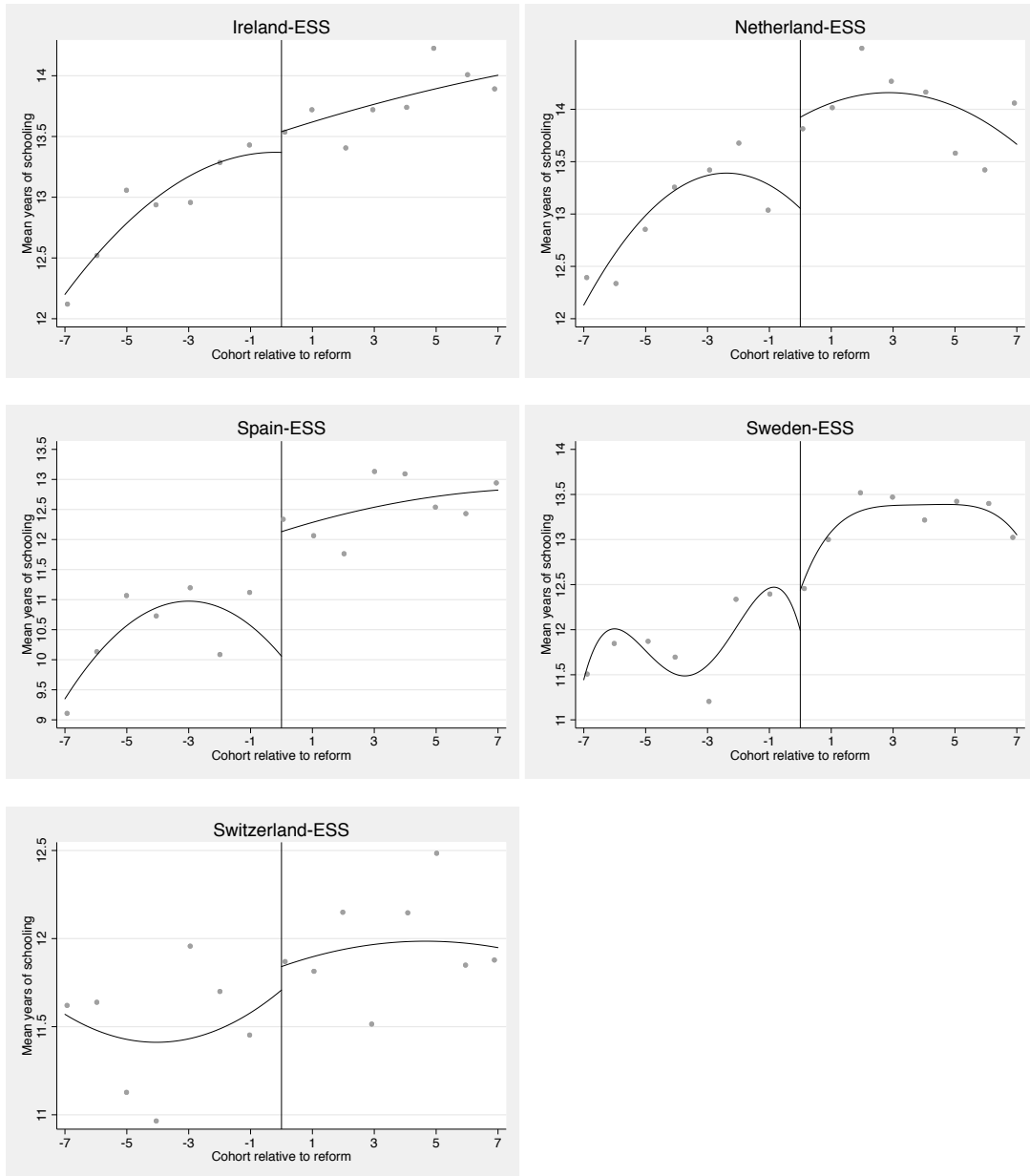


TABLE 3: **Baseline Results**
(DVs: GA indexes)

	(1) OLS	(2) 1 st Stage	(3) 2 nd Stage	(4) IV Estimates F-test	(5) Weak IV Robust 95% CI	(6) N
a) BHPS data						
Years of schooling	0.016*** (0.001)	0.920*** (0.070)	0.316*** (0.031)	171.3	[0.261, 0.386]	68,233
b) SHP data						
Years of schooling	0.085*** (0.003)	0.573*** (0.080)	0.190*** (0.055)	51.20	[0.103, 0.288]	12,718
c) ESS data						
Years of schooling	0.0531*** (0.001)	0.352*** (0.122)	0.124*** (0.034)	17.3	[0.056, 0.191]	17,964

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category. For the ESS, we also control for the country-specific birth trend and country-related macro economic variables.

TABLE 4: **Heterogeneous effects by gender**
(DVs: GA indexes)

	(1) Gender	(2) OLS	(3) 1 st Stage	(4) 2 nd Stage	(5) F-test	(6) Weak IV Robust 95% CI	(7) N
a) BHPS data							
Years of schooling	<i>Women</i>	0.009*** (0.002)	0.425*** (0.107)	0.533*** (0.148)	59.8	[0.327, 1.07]	31,406
	<i>Men</i>	0.026*** (0.001)	1.282*** (0.093)	0.261*** (0.027)	128.9	[0.212, 0.319]	36,827
b) SHP data							
Years of schooling	<i>Women</i>	0.111*** (0.004)	1.012*** (0.110)	0.200*** (0.042)	85.14	[0.121, 0.287]	7,266
	<i>Men</i>	0.092*** (0.004)	-1.963 (1.287)	0.037 (0.024)	2.325	[-0.067, 0.623]	5,452
c) ESS data							
Years of schooling	<i>Women</i>	0.057*** (0.002)	0.562*** (0.165)	0.126*** (0.037)	11.55	[0.059, 0.484]	9,521
	<i>Men</i>	0.052 (0.002)	0.060 (0.038)	0.072* (0.034)	0.110	[-0.001, 0.145]	8,443

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category. For the ESS, we also control for the country-specific birth trend and country-related macro economic variables.

TABLE 5: **Heterogeneous effects by religiosity**
(DVs: GA indexes)

	(1) Gender	(2) OLS	(3) IV Estimates 1 st Stage	(4) IV Estimates 2 nd Stage	(5) F-test	(6) Weak IV Robust 95% CI	(7) N
a) BHPS data							
Years of schooling	<i>Religious</i>	0.021*** (0.003)	0.989*** (0.153)	0.285*** (0.064)	41.51	[0.177, 0.445]	11,698
	<i>Non-religious</i>	0.021*** (0.002)	0.858*** (0.117)	0.337*** (0.057)	53.61	[0.242, 0.480]	25,666
b) SHP data							
Years of schooling	<i>Religious</i>	0.049*** (0.003)	0.469*** (0.136)	0.117*** (0.031)	11.83	[0.115, 0.357]	7,889
	<i>Non-religious</i>	0.041*** (0.002)	0.121 (0.151)	0.084** (0.034)	0.637	[-0.574, 0.367]	4,127
c) ESS data							
Years of schooling	<i>Religious</i>	0.053*** (0.002)	0.452*** (0.218)	0.124*** (0.056)	4.3	[0.0184, 3.179]	6,311
	<i>Non-religious</i>	0.052*** (0.003)	0.306*** (0.151)	0.102*** (0.034)	4.08	[-0.013, 5.403]	11,617

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category. For the ESS, we also control for the country-specific birth trend and country-related macro economic variables.

TABLE 6: **Using school leaving age for BHPS data**
(Sensitivity analysis I, DVs: GA indexes)

	(1) OLS	(2) IV Estimates 1 st Stage	(3) IV Estimates 2 nd Stage	(4) F-test	(5) N	(6) Weak IV Robust 95% CI
Years of schooling	0.053***	0.614*** (0.015)	0.555*** (0.071)	75.28 (0.136)		[0.324, 0.814] 5,632

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category.

TABLE 7: Using 10-year restriction window
(Sensitivity analysis II, DVs: GA indexes)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV Estimates				N
		1 st Stage	2 nd Stage	F-test	Weak IV Robust 95% CI	
a) BHPS data						
Years of schooling	0.016***	1.136*** (0.0009)	0.252*** (0.061)	346.7 (0.020)	[0.216, 0.294]	81,460
b) SHP data						
Years of schooling	0.085*** (0.003)	0.699*** (0.084)	0.190*** (0.046)	69.41	[0.067, 0.285]	15,990
c) ESS data						
Years of schooling	0.054***	0.338*** (0.001)	0.100*** (0.101)	11.83 (0.024)	[0.094, 0.559]	25,162

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category. For the ESS, we also control for the country-specific birth trend and country-related macro economic variables.

TABLE 8: Baseline estimates separately for each gender role attitude

	(1) OLS	(2) 1 st Stage	(3) IV Estimates 2 nd Stage	(4) F-test	(5) Weak IV Robust 95% CI	(6) N
a) BHPS data						
A pre-school child suffers if the mother works	0.011*** (0.001)	0.915*** (0.070)	0.464*** (0.041)	169.2	[0.365, 0.524]	68,136
Family life suffers if the woman works full-time	0.018*** (0.002)	0.917*** (0.070)	0.362*** (0.034)	169.9	[0.365, 0.524]	68,140
A woman and family would all be happier if she works	0.001 (0.002)	0.916*** (0.070)	0.071*** (0.022)	169.4	[0.031, 0.120]	68,128
Both husband and wife should contribute to the hh income	-0.021*** (0.002)	0.918*** (0.070)	-0.064*** (0.022)	170.2	[-0.138, -0.049]	68,138
Having job is the best way for a woman to be independent	-0.009*** (0.002)	0.917*** (0.070)	-0.062*** (0.022)	169.9	[-0.109, -0.022]	68,149
A husband earns money; a wife is looks after the home	0.056*** (0.002)	0.919*** (0.070)	0.357*** (0.031)	170.9	[0.283, 0.404]	68,154
b) SHP data						
Pre-school child suffers if mother works	0.085*** (0.003)	0.573*** (0.080)	0.190*** (0.055)	51.20	[0.103, 0.288]	12,718
c) ESS data						
Women should be prepared to cut down on paid work for sake of family	0.044*** (0.002)	0.334*** (0.102)	0.101*** (0.025)	10.83	[0.027, 0.723]	25,171
Men should have more right to job than women when jobs are scarce	0.061*** (0.002)	0.334*** (0.102)	0.074*** (0.024)	10.83	[0.047, 0.708]	25,171

Note: *, **, *** indicate the 10%, 5%, and 1% significance levels, respectively. Standard errors are reported in parentheses. For each set of dummies (year, birth year, and region) we leave out the first dummy as the excluded category. For the ESS, we also control for the country-specific birth trend and country-related macro economic variables.