

Deole, Sumit S.; Zeydanli, Tugba

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

Sumit S. Deole*

GLO Fellow, TU Dortmund

Tugba Zeydanli†

University of Halle-Wittenberg

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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. 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. The causal impact of education on gender role attitudes is estimated by exploiting the exogenous variation in individuals' education induced by the compulsory school reforms undertaken in European countries in the second half of the 20th century. We find evidence 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, no evidence of effect heterogeneity is found concerning the 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

*Corresponding author. TU Dortmund, Department of Economics, Vogelpothsweg 87, D-44227 Dortmund. Tel.: +49 231 755 3572. Fax: +49 231 755 5404. Email: sumit.deole@tu-dortmund.de.

†Martin Luther University of Halle-Wittenberg, Department of Economics, Große Steinstraße 73, D - 06099 Halle (Saale), Germany. Tel.: +49 345 55 23381; Fax: +49 345 55 27191. Email: tugba.zeydanli@wiwi.uni-halle.de.

1 Introduction

Although women’s workforce involvement has observed a dramatic increase in many parts of the world, the situation remains far from ideal. For instance, while according to the latest Forbes list of billionaires (Forbes, 2020), three women ranked among the top twenty wealthiest people on the planet; however, the richest ”self-made” woman was ranked 211th on the list. Despite the recent improvements, women’s representation in national parliaments is also far from ”gender-equal”. In particular, the proportion of women in national parliaments in major Western countries is suboptimal, 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 (Inter-Parliamentary Union, 2019). Compared to the OECD average, these countries also report wider gender wage gap (OECD, 2019).¹

Vast social science literature is devoted to investigating the determinants of gender differences in various outcomes, which proposes deep-rooted discriminatory social norms and attitudes towards traditional gender roles as *potential* explanations (Alesina et al., 2013a; Bertrand, 2011; Blau and Kahn, 2017; Fortin, 2005; Vella, 1994). Although gender-based discrimination is generally socially unacceptable and covert, newer research shows that it 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).

In this paper, we investigate whether individuals’ education predicts their attitudes towards traditional gender roles. Our empirical setup mimics the emerging research demonstrating education’s pertinence in determining public attitudes towards immigration (Cavaille and Marshall, 2019; D’Hombres’ and Nunziata, 2016; Finseraas et al., 2018; Margaryan et al., 2019; Mazumder, 2019) and political institutions (Cheruvu,

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*”.

2020; Kunst et al., 2020; Siedler, 2010).^{3,4} To estimate the causal impact of education, we employ the instrumental variables (IV) estimation technique. For this purpose, 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 is conducted using two national panel datasets from the UK (British Household Panel Survey (BHPS)) and Switzerland (Swiss Household Panel (SHP)) and a repeated cross-sectional dataset comprising information from 13 Western European countries (European Social Survey (ESS)).

Our paper makes the following three contributions to the literature. First, by investigating whether individuals' education predicts their attitudes towards traditional gender roles, we underline education's role as a factor influencing the psychological and noncognitive attributes of gender inequalities. Pertinent for the transmission mechanism, 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.⁵ Second, our empirical analysis uses all available self-reported gender attitudes questions included in major surveys from Western Europe to give new insights into gender inequality in the region. More importantly, causally, we examine how education affects attitudes towards gender roles and work-life balance. We observe that an additional year of education yields egalitarian gender role attitudes across all outcomes. Being able to address this question is particularly crucial for designing policies to mitigate gender equality. Finally, we employ data from various countries to test whether education's gender-equal influence is valid and present across different cultures. In particular, we are interested in understanding whether the compulsory schooling reforms across countries would yield different effects on gender attitudes. Answering this question contributes to generalizing our results that education stimulates gender equality across divergent cultures. For this purpose, we also investigate whether education's impact on the individuals' gender role attitudes differs by their gender and religiosity.

The empirical analysis begins by discussing the results estimated using the ordinary least squares (OLS) method. We find a positive association between individuals' education level and their egalitarian attitudes towards traditional gender roles. The causal evidence estimated using the IV estimation strategy also shows

³Other notable research investigates education's impact on individuals' economic outcomes (Brunello et al., 2009; Grenet, 2013; Havari and Savegnago, 2014; Hofmarcher, 2019; Schneeweis et al., 2014) and demographic and health outcomes (Brunello et al., 2016; Cygan-Rehm and Maeder, 2013; Fort et al., 2016; Gathmann et al., 2015; Jürges et al., 2013; Kemptner et al., 2011; Kirdar et al., 2018; Hungerman, 2014; Tequame and Tirivayi, 2015; Wilson, 2017; Mocan and Pogorelova, 2017).

⁴A study that comes close to our focus is by Gulesci et al. (2019). The authors study the impact of compulsory schooling reforms in Turkey and find inter-generational spillover effects of the eldest daughter's increased education on mothers' attitudes about domestic violence, while boys' schooling does not show such an effect.

⁵For instance, Gang et al. (2013, p.13) note that most Western educational systems are designed quite explicitly to increase social tolerance.

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.

Our empirical analysis faces three critical challenges. The first challenge for estimating the causal effect of education on gender attitudes is 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 with egalitarian gender role attitudes can persevere to educate themselves and report higher education levels.⁶ We address these challenges by 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. In some countries, multiple reforms were observed in the post-WW II era, e.g., the UK implemented reforms in 1947 and 1972.

Third, while we ignore foreign-born individuals from the sample, for internal migrants, we assume that individuals' state of current residence is also their state of birth. This issue is particularly problematic for our results using Finnish and West German data and, as noted in Cavaille and Marshall (2019), can pose a threat to our identification.⁷

The fourth challenge arises from the demand for 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 need to have a sufficient number of individuals born before and after the treatment. Fortunately, we can use two

⁶For more discussion, see subsection 4.2

⁷We 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.

longitudinal household survey data from the BHPS and SHP and the repeated cross-sectional data from the ESS. To the best of our knowledge, survey questions recording individuals' gender attitudes are not available in other longitudinal household surveys or suffer from low-coverage issues that hindered our effort to assess the estimation results.⁸

2 Gender role attitudes in Europe

Recently, women have caught up with men in terms of years of completed education.⁹ In many countries, the increased women's education has also led to 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 (De Hauw et al., 2017; Esteve et al., 2012; Grow and Van Bavel, 2015). 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.¹⁰

Existing research devoted to analyzing the determinants of the gender differences in economic outcomes underlines the resolve of gender-biased societal norms and attitudes as potential explanations (Alesina et al., 2013a; Bertrand, 2011; Fortin, 2005; Vella, 1994). Various studies highlight the pertinence of gender-biased labor markets (Ceci et al., 2014; Polachek, 1987), 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; Cortes and Pan, 2018; Mandel and Semyonov, 2014). Given their importance, in this paper, we perform a thorough investigation of individuals' attitudes towards traditional gender roles.¹¹

The existing research can help us understand the state of gender role attitudes in OECD countries, their recent trends, and pertinent correlates. For instance, Fortin (2005) notes a substantial cross-country

⁸Detail explanation can be found in the data section.

⁹According to Eurostat, in the age range of 30-34, women (45%) are far more likely to complete tertiary education than men (34%) in EU-27 countries (https://ec.europa.eu/eurostat/statistics-explained/index.php/Gender_statistics#Education,2020_data)

¹⁰As per the *OECD Employment Outlook*, OECD countries reported an average gender wage gap of approximately 13% for 2019 (OECD, 2020). In particular, major Western countries report higher estimates than the OECD average, e.g., France (13.7%), Germany (15.3%), UK (16%), and the US (18.5%).

¹¹For 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.

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 supporting evidence that the attitudes towards gender roles became more egalitarian during the years 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 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, e.g., gender, age, education, income, and religiosity.¹² 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.^{13,14} Others suggest that the gender role attitudes may be formed outside the schooling system (Vella, 1994), often developed during the individual's youth (Vella, 1994), and strongly correlated with religiosity (Algan and Cahuc, 2006; Guetto et al., 2015; Guiso et al., 2003; H'madoun, 2010; Morgan, 1987; Lottes and Kuriloff, 1992; Voicu et al., 2009; Vella, 1994). 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 details on the compulsory schooling reforms in selected European countries.¹⁵ 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 by increasing the compulsory schooling age from 15 to

¹²For understanding the gender role attitudes among immigrants in European countries, *see* (Breidahl and Larsen, 2016).

¹³Lottes and Kuriloff (1992) also discuss that males and females perceive gender roles differently.

¹⁴Fodor and Balogh (2010) also find that women express more egalitarian gender role attitudes than men.

¹⁵The primary source of information for the compulsory schooling reforms provisions is Brunello et al. (2009).

16 years.¹⁶ In Switzerland, the reform took place in 1970 and affected individuals born in 1971. Beyond cross-country variations in implementation dates, the compulsory schooling reforms also varied by regions and the length of schooling they affected.

For example, in Germany, Finland, and Sweden, reform dates varied by region. For instance, 10 West German states implemented reforms in different years, as summarized in Table 1. These reforms uniformly increased compulsory schooling by one year (from 8 years to 9 years) in all German states.^{17,18} In Finland and Sweden, the implementation of reforms was at the municipal level and was completed progressively. In Finland, the reforms extending compulsory schooling from 6 to 9 years were implemented between 1972 and 1977 (*see* Pekkarinen, 2008). As Pekkarinen describes, while the implementation of reforms in Finland began in relatively underdeveloped Northern and Eastern regions, the reforms also had the most impact due to lower initial education levels. In contrast, the reforms had little impact in most urbanized Southern and Western regions of the country, where they were also adopted in advance in some form before the official implementation date (*see* Pekkarinen, 2008, 815). For instance, 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 1950.¹⁹ Since the ESS does not provide 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 German states, Great Britain, Ireland, Netherlands, and Sweden. However, in Spain and Denmark, schooling increased by two years. In Finland, Greece, and Italy, schooling increased by three years. In Belgium, the reforms increased schooling by four years.

¹⁶To be precise, the reform affected individuals born on or after September 1957, while those born before September 1957 were not subjected to law.

¹⁷The ESS collects detailed information on the individuals' state of residence in Germany, which we use for the empirical investigation.

¹⁸Baseline results are estimated using the information on affected cohorts, as noted in Brunello et al. (2009). However, other researchers have implemented different reform dates, e.g., (*see* Cygan-Rehm and Maeder, 2013). We verify our findings' robustness by employing this alternative information and confirm that the main results hold.

¹⁹However, Meghir and Palme (2005) note that many Swedish schools had already extended schooling before the reforms' official implementation in 1962.

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 *British Household Panel Survey (BHPS)*, *Switzerland Panel Survey (SHP)*, and *European Social Survey (ESS)*.²⁰ The BHPS spans from 1991 to 2008 and provides information on individual, household, job-related, and attitudes-related subjects.²¹ Like the BHPS, the SHP also collects information on the job and living conditions of individuals and households in Switzerland. The analysis considers the SHP waves between the years 2003 to 2019. Finally, the ESS surveys the individuals from 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:

- *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..*

²⁰The following two criteria are used to select the datasets: 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 the outcome variable(s) of interest, 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)*.

²¹*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 the BHPS as it covers a more extended period.

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), the survey’s coverage is extended by replacing the missing values with the mean of the observed values from the two adjacent waves of the missing observation.

Unlike the 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-point scale ranging from *1-strongly disagree* to *10-strongly agree*. We reverse the responses’ order so that higher values on this variable indicate egalitarian gender role attitudes. The SHP has asked 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 empirical analysis employs information on gender role attitudes present in the ESS survey waves from 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, i.e., the BHPS and ESS datasets.^{22,23} To do this, we recode component items in the same direction so that a low (high) score indicates inegalitarian (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.

Sample construction

²²Other examples of using this methodology include Sweeting et al. (2014) and Balbo and Arpino (2016).

²³The 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 separately for each gender role attitude.

We obtain our estimation sample by applying sample selection 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.²⁴ Following Brunello et al. (2009)’s advice 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.²⁵ 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’s say 20 years) may include post-reform observations that are systematically different (that they live 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 explanatory variables. Our 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 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, whereas the control group consists of individuals born before the reforms were implemented. We additionally report normalized differences in means of our experimental groups. In

²⁴To 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.

²⁵In subsection 4.3, we relax the seven-year restriction to a ten-year restriction window.

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 group 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. A similar observation holds for the *BHPS GA index* constructed using six gender role attitudes from the BHPS. 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, $region_i$ takes into account region fixed-effects, and u_{it} is the error term.^{26,27} 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, 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. This is 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. In equation (2), we first regress the compulsory schooling reform indicator on the endogenous regressor ($schooling_{it}$) and estimate the predicted years of schooling $\widehat{schooling}_{it}$. The *reform* is a binary

²⁶The 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).

²⁷To deal with serial correlation and heteroskedasticity, we cluster standard errors by region level.

indicator taking the value 1 if an individual was affected by the reform and zero otherwise. In the second step, we replace the predicted years of schooling values $\widehat{schooling}_{it}$ in equation (1) (instead 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. The next section presents the first stage results and provides 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, it must be noted 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.

In our analysis, the identification depends on the following assumptions: (a) highly gifted students tend to stay in school longer; (b) compulsory schooling reforms affect only education level and have no impact on gender role attitudes; (c) individuals working (living) in an egalitarian environment may have a more egalitarian gender role attitude than individuals working (living) in an in-egalitarian environment; (d) compulsory schooling has an exogenous effect on individuals' years of schooling, as it depends on the person's year of birth, but the educational level they attain endogenously depends on their choice, and finally (e) in some of our selected countries (e.g., Germany and Finland) the compulsory schooling reforms are implemented in different time frames, this is not related with the general level of education. 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.

As we estimate the *local average treatment effect* (LATE), our compliers are individuals who causally get affected by the compulsory school reforms. The most crucial assumption to be satisfied for the LATE to have an unbiased estimate 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. We do this by regressing the reform dummy on the probability of graduating from elementary school. We record negative but statistically insignificant coefficients for all datasets.²⁸

5 Results and discussion

5.1 Main results

The baseline results are presented in Table 3. In column (1), we present the estimates of the 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 graphical examination of the first stage results. In other words, in Figure 2, we study whether compulsory schooling affects individuals' education and that the IV is valid. 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 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, a small moderation in the education effect is observed, 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

²⁸The results are available upon request.

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, education matters for shaping individuals' attitudes towards gender roles and work preferences.

The baseline results were estimated using GA indexes. 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, especially for the gender role attitudes recorded in all datasets, except for one 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 associated with the individuals' larger belief systems, e.g., their 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.²⁹ These subgroup-specific heterogeneous effects are documented in Table 4 and Table 5. In most specifications, we find that increases in education yield more egalitarian gender role attitudes among females than males across all datasets. Concerning the individuals' religiosity, our results do not find any evidence of heterogeneous effects.

²⁹Recent research by Hungerman (2014) find that higher levels of education lead to lower levels of religious affiliation later in life. This result provides an additional supporting argument for considering heterogeneous effects associated with the individuals' religiosity.

5.3 Robustness checks

This subsection presents robustness checks of our main results. First, we re-estimate our baseline results after changing the bandwidth window of sample restriction (Brunello et al., 2009; Cygan-Rehm and Maeder, 2013). The results in Table-7 show the estimates of the sample with a 10-year window. The results confirm a positive and sizable effect of education increases on the individuals' gender role attitudes. Second, we re-estimate the BHPS results using the individuals' *school leaving age* instead of *Years of schooling*. Exceptionally, besides individuals' completed education noted above, BHPS also recorded their school-leaving age. Although the number of observations reporting their school leaving age is much lower, it provides a useful check for our endogenous variable of interest. Our results report a statistically significant first stage regression (see Table 6), 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 movement against women's sexual harassment at workplaces (*#MeToo* movement) gained momentum worldwide, larger questions concerning women's workplace experiences emerged. Extensive research on the topic shows that the origins of the ever-present gender gap are deep-rooted in discriminatory social norms regarding traditional gender roles. This paper 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. We also found that education's moderating effects are particularly prominent among women and that the individuals' religiosity does not intervene in the relationship. The finding that education increases do not induce gender-equal attitudes in males is relevant for education policy. It questions the assumption that the Western education system increases social tolerance and highlights the need to reformulate the curriculum to reinforce gender-equal attitudes among males effectively.

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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	(4) Sources
Austria	1962	1947	14 to 15	Brunello et al. (2009)
Belgium	1983	1969	14 to 18	Brunello et al. (2009)
Switzerland	1970	1971	14 to 15	
West Germany				
Schleswig-Holstein	1956	1941	14 to 15	Brunello et al. (2009)
Hamburg	1949	1934	14 to 15	Brunello et al. (2009)
Niedersachsen	1962	1947	14 to 15	Brunello et al. (2009)
Bremen	1958	1943	14 to 15	Brunello et al. (2009)
Nordrhein-Westfalen	1967	1953	14 to 15	Brunello et al. (2009)
Hessen	1967	1953	14 to 15	Brunello et al. (2009)
Rheinland-Pfalz	1967	1953	14 to 15	Brunello et al. (2009)
Baden-Württemberg	1967	1953	14 to 15	Brunello et al. (2009)
Bayern	1969	1955	14 to 15	Brunello et al. (2009)
Saarland	1964	1949	14 to 15	Brunello et al. (2009)
Denmark	1971	1957	14 to 16	Brunello et al. (2009)
Spain	1970	1957	12 to 14	Brunello et al. (2009)
Finland	1974	1963	13 to 16	
France	1959	1953	14 to 16	Brunello et al. (2009)
Great Britain	1972	1957	15 to 16	
Greece	1975	1963	12 to 15	Brunello et al. (2009)
Ireland	1972	1958	14 to 15	Brunello et al. (2009)
Italy	1963	1949	11 to 14	Brunello et al. (2009)
Netherlands	1975	1959	15 to 16	Brunello et al. (2009)
Sweden	1962	1950	14/15 to 15/16	Brunello et al. (2009)

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.

TABLE 2: Summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Control	Treated	Norm. Diff.	Control	Treated	Norm. Diff.	Control	Treated	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>)									
Women should be prepared to cut down on paid work for sake of family	9.57	10.03	-0.47***	9.14	10.36	-1.22***	12.47	13.36	-0.88**
Men should have more right to job than women when jobs are scarce	45.74	38.30	7.45***	40.41	33.12	7.28***	52.43	45.20	7.23***
	1953.09	1962.53	-9.44***	1965.67	1974.68	-9.01***	1955.16	1962.41	-7.25**
Explanatory variables									
Years of schooling	0.53	0.54	-0.01	0.59	0.55	0.04***	0.53	0.530	-0.00
Age	0.33	0.30	-0.03***	0.68	0.62	0.06***	0.66	0.63	0.03***
Year of birth									
Heterogeneity checks									
Female	0.79	0.79	0.01	0.68	0.53	0.15***	0.68	0.63	0.05***
Religiosity	0.79	0.81	-0.01*	0.89	0.30	-0.01	0.59	0.72	-0.13**
Supplementary socio-economic information									
Married	0.04	0.03	0.01**	0.01	0.01	0.00	0.02	0.03	-0.01
Working	0.17	0.16	0.00	0.10	0.08	0.01*	0.39	0.25	0.14***
Unemployed	3.14	3.06	0.07***	3.15	3.11	0.04	3.30	3.37	-0.06**
Inactive	3.18	3.39	-0.21***	3.41	2.88	0.53***	2.61	2.97	-0.36***
5 quantiles of HH income	29208	39927	69135	7716	5002	12718	8160	9804	17964
Number of persons in HH									
Observations (N)									

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.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		IV Estimates			N
		<u>1st Stage</u>	<u>2nd Stage</u>	<u>F-test</u>	<u>Weak IV Robust 95% CI</u>	
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)	(2)	(3)	(4)	(5)	(6)	(7)
	Gender	OLS	IV Estimates		F-test	Weak IV Robust 95% CI	N
			<u>1st Stage</u>	<u>2nd Stage</u>			
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)	(2)	(3)	(4)	(5)	(6)	(7)
	Gender	OLS	IV Estimates				N
			<u>1st Stage</u>	<u>2nd Stage</u>	<u>F-test</u>	<u>Weak IV Robust 95% CI</u>	
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)	(2)	(3)	(4)	(5)	(6)
	OLS	IV Estimates			N	
		<u>1st Stage</u>	<u>2nd Stage</u>	<u>F-test</u>	<u>Weak IV Robust 95% CI</u>	
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		F-test	Weak IV Robust 95% CI	N
		<u>1st Stage</u>	<u>2nd Stage</u>			
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.

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

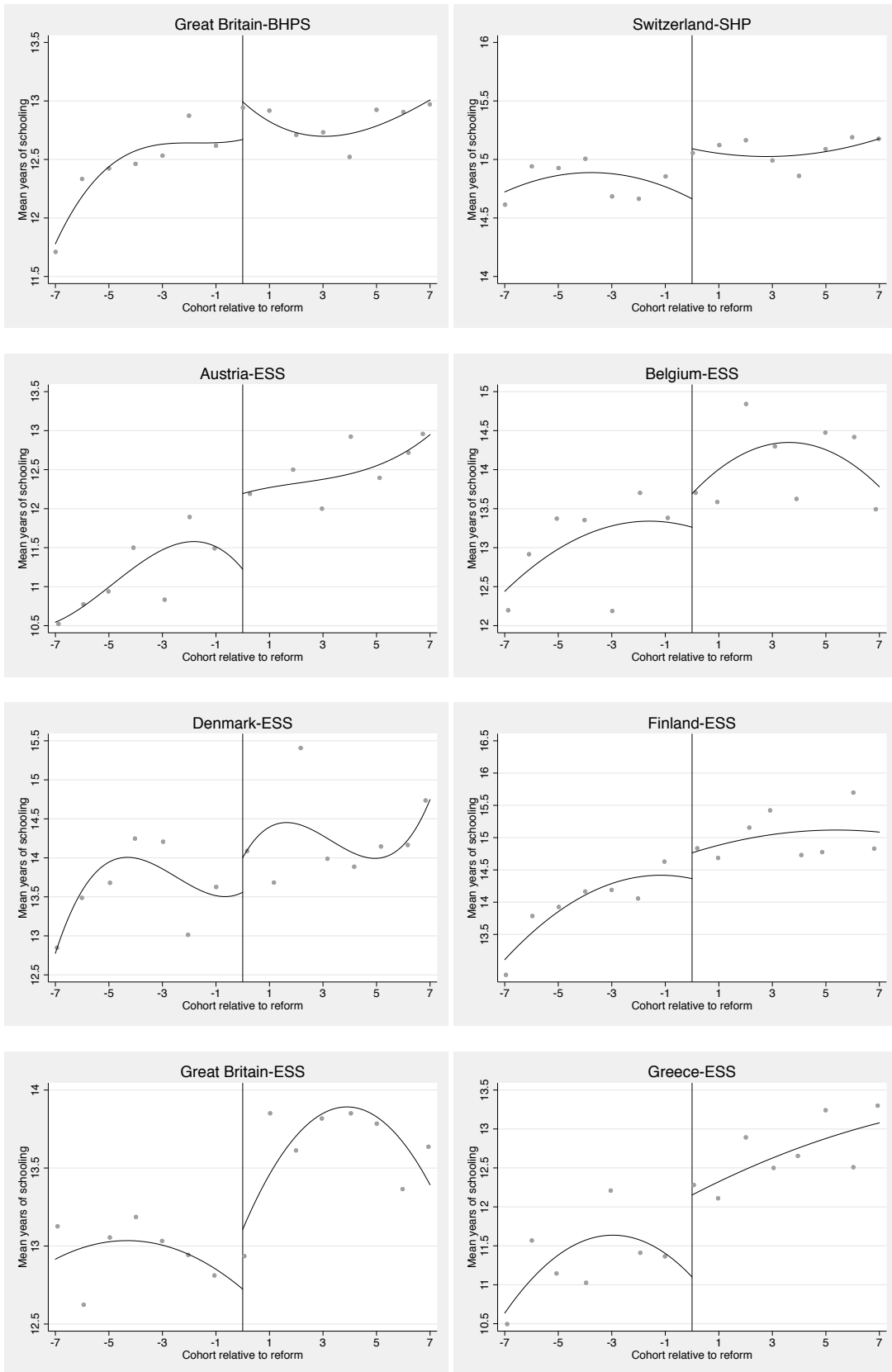


FIGURE 2: First Stage: effect of reform on years of education

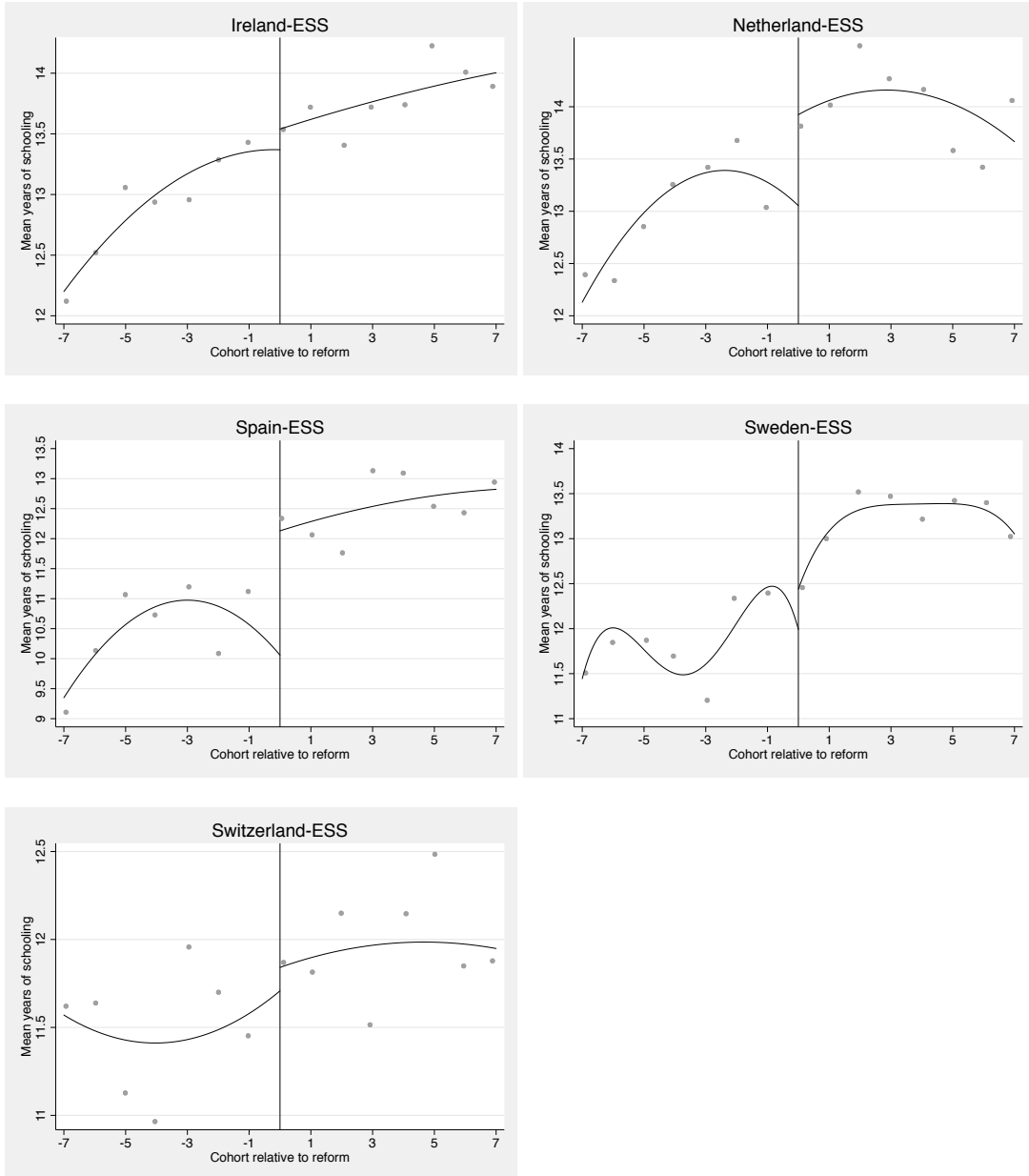


TABLE 8: Baseline estimates separately for each gender role attitude

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		IV Estimates		Weak IV Robust 95% CI	N
		1 st Stage	2 nd Stage	F-test		
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.

TABLE 9: **Baseline Results with Socio-economic characteristics**
(DVs: GA indexes)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV Estimates		F-test	Weak IV Robust 95% CI	N
		<u>1st Stage</u>	<u>2nd Stage</u>			
a) BHPS data						
Years of schooling	0.002* (0.001)	1.018*** (0.066)	0.298*** (0.027)	235.9	[0.249, 0.356]	68,233
b) SHP data						
Years of schooling	0.121*** (0.004)	0.356*** (0.053)	0.306*** (0.090)	44.45	[0.249, 0.356]	12,718
c) ESS data						
Years of schooling	0.0537*** (0.005)	0.281 (0.314)	0.024 (0.08)	0.804	[-0.402, 0.918]	2,585

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.