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Does Education Impact Female Employment?

The Use of Compulsory Schooling Reforms in Europe

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Abstract

This paper examines the impact of years of schooling on female employment decisions and on the intensity of employment by using the variation across countries and over birth cohorts in compulsory schooling reforms. The sample includes women aged 25 to 64 from five eurozone countries and the data covers the year of 2010. The findings suggest that years of schooling do not have a significant effect on the probability of female employment. Additionally, schooling is found to have a negative impact on the employment intensity. Some potential reasons behind these results are attitudes towards working women, household responsibilities, policies et cetera. The paper contributes to the literature about female employment and gender inequalities in the labor market.

Keywords: Employment, Education, Gender inequality, 2SLS, IV

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1. Introduction

Issues related to the labor market and gender equality are prominent in social and political discussions. Historically, employment rates for women in Europe have been significantly lower than those for men, and in fact they still are (Eurostat, 2014). In the recent decade and even more pronounced since the Lisbon Agenda in 2009, the European policy has turned some of its focus to increasing the number of women in employment. One reason for this shift is the aging population of many European countries which could lead to fiscal pressure and lower material living standards and thus calls for a larger labor force that could contribute to more sustainable social protection systems in the member countries (European Council, 2000).

Despite the fact that gender equality is one of the fundamental values in the European Union, the progress in the labor market has been relatively slow (European Commission, 2014a). In order to be able to further lower the gender gaps and increase female employment, it is essential for policy makers to have knowledge about the driving forces behind these inequalities so that policies can be designed to effectively deal with the prevailing issues. Indeed, it has been argued that policy makers intending to reduce the inequalities should focus on certain areas such as reducing the gender gaps in human capital endowments and improving the economic opportunities for women (World Bank, 2012). This paper will investigate one aspect of these relationships, namely the connection between the educational attainment of women and female employment.

It could be seen to be especially compelling to explore this relationship between education and employment in a European setting since both of these matters could be argued to have central roles in the member countries as they directly connect to the strategic goal of the European Union “to become the most competitive and dynamic knowledge-based economy in the world ... with more and better jobs” (European Parliament, 2000, n.p.). Additionally, the two areas are prominent in the European Union’s ten-year growth strategy “Europe 2020” in which they are covered by the two (out of five) headline targets that aim to increase employment and education (European Commission, 2015b).

The main purpose of this paper is to study the impact of schooling on female employment¹. In order to connect to existing theory concerning the labor supply, it will also briefly be investigated whether schooling has an effect on the intensity of female employment. The data that is used to analyze these matters covers the year of 2010 and originates from the Luxembourg Income Survey Database (LIS). The sample consists of females aged 25 to 64 who are currently not studying and who were born in one of the following five European countries: Greece, Germany, Ireland, Spain and the Netherlands.

¹ Employment is defined according to ILO.

² See Table B.1 in Appendix B for more details about the reforms.

These sample countries could be argued to be similar in several ways due to all being members of the same monetary union, but they also differ in many aspects. The levels of both education and female employment vary between the five countries, leading to a sample with a mixture of countries with low, middle and high rates, see Table A.1 and A.2 in Appendix A.

As frequently pointed out by economists, there is a risk that education might not be exogenous since it also represents individual characteristics that cannot be well defined and measured *et cetera*. In order to deal with the endogeneity, this paper will use the exogenous variation in years of schooling that came about due to compulsory schooling reforms² that took place during the mid to late 1900s. As a result of these reforms, years of compulsory schooling were increased by one year in Ireland, Germany and the Netherlands while increasing by two years in Spain and by three years in Greece (Brunello, Fort & Weber, 2009). As the reforms were implemented at different points in time and vary both over birth cohorts and across countries (and between states in Germany), the effect of increasing the years of compulsory schooling could be examined using a difference-in-difference framework.

The empirical models will be estimated using two-stage least squares (2SLS). In the first stage, schooling is regressed upon a dummy variable indicating whether or not an individual was exposed to a school reform. In the second stage, employment is regressed upon the instrumented variable of schooling. When considering the intensity, the ordinary employment variable is replaced by full-year full-time employment in the second stage.

The paper will primarily investigate two hypotheses that are based on the findings of previous empirical research. The first hypothesis states that the school reforms had a positive impact on years of schooling. This relationship will be investigated in the first stage of the analysis. The second hypothesis is that a woman's years of schooling have a positive impact on the probability that she is employed. Additionally, a third subordinate hypothesis will briefly be investigated. This one concerns the intensity of employment and states that schooling will have a positive effect on the probability of full-year full-time employment. This last hypothesis is based upon previous empirical findings suggesting that the substitution effect outweighs the income effect for women (Borjas, 2010).

The results suggest a positive relationship between the increase in years of compulsory schooling and women's years of schooling and this relationship holds even when different sub-samples are used. Nevertheless, the results do not support the hypothesis that female schooling increases the probability of employment since no significant relationship is found between these two. Hence, policies that increase female schooling

² See Table B.1 in Appendix B for more details about the reforms.

from low levels might not be successful in increasing the female employment rates. Potential reasons behind this could be policies concerning child care and taxes, lack of demand for skills learned in school, attitudes towards working women et cetera. Thirdly, a negative relationship is found between schooling and the probability of full-year full-time employment. Reasons behind this result could for instance be that the income effect outweighs the substitution effect and that women with more schooling might afford to work less.

The remainder of the paper will be structured as follows. The first section provides some background information and an overview of previous empirical research on the topic. Section 3 reviews theories behind an individual's decision to invest in human capital in the form of schooling along with theoretical predictions of the individual's decision to supply labor. Section 4 introduces the data used in the analysis. The methodology used to examine the relationships is then presented in section 5. The results from the estimations are presented in section 6 and the subsequent section 7 contains a number of robustness checks. The findings are discussed in section 8 and section 9 concludes.

2. Background

2.1 Setting the Scene – Gender Inequalities and the Female Labor Market

In order to present the context in which this paper takes place this section provides a brief overview of labor market statistics, gender inequalities and benefits from reducing such inequalities. According to Eurostat (2014), Germany and the Netherlands were two of the European Union member states with the highest female employment rates in 2013 while Greece had the lowest rate of all the European countries. With the female employment rates gradually increasing and doing so with a little more than four percent in the last decade, some progress in reducing the differences between the genders in the Euro area altogether could be seen. However, the female rates still remain well below the male employment rates, as seen in Figure 1 (Eurostat, 2014). Similar gender gaps in employment are also found using the data from LIS, see Table A.1 in Appendix A.

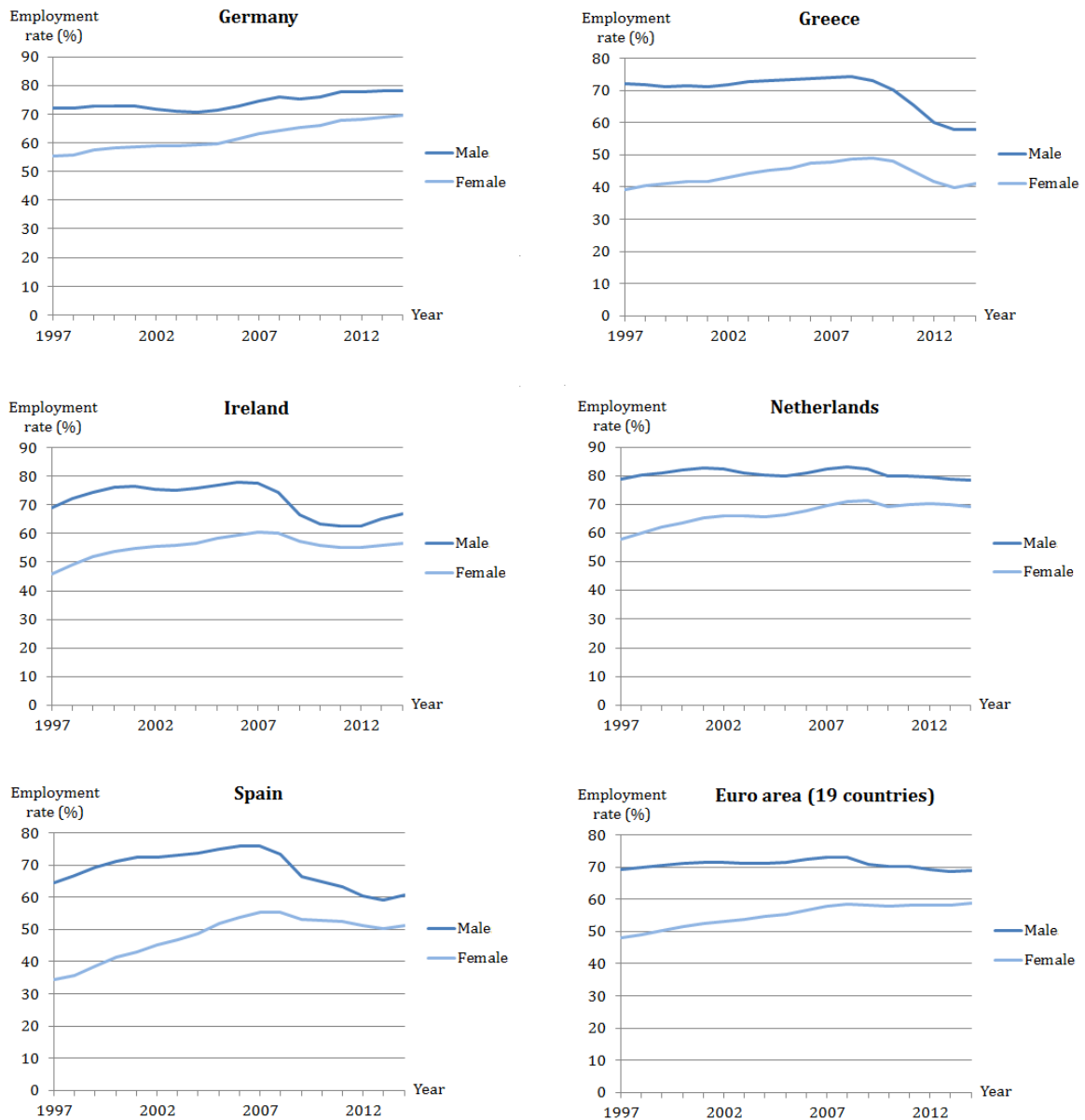


Figure 1. Employment Rate by Country

Note: The sample consists of individuals aged 20 to 64 years old.

Source: Eurostat (2015)

Female labor force participation rates³ have lately decreased in the developed economies and in the European Union for young individuals while increasing for adults. A potential reason behind the former drop could be that women are in general obtaining more education today than historically and thus stay longer in the education system (ILO, 2012). Also, the legislative changes in compulsory schooling age could contribute to the below average employment growth among young individuals (Pissarides et al. 2005). For adult women, the underlying reasons could instead be changes that to some

³ Labor force participation includes both individuals who are employed and those unemployed but seeking employment.

extent made it easier for women to combine household responsibilities with labor market participation (ILO, 2012).

There are further large gender differences in the division of market work and non-market work in European countries and women are found to spend significantly more time on housework compared to males (McGinnity et al. 2005). Even though female employment and wage rates in general are lower than those for males, women cannot be seen to lag behind in education (Eurydice, 2009). In fact, more than half of the university graduates in the European Union are females (European Commission, 2014a). In the specific dataset used in this paper, no substantial gender differences could be seen in years of schooling and the country averages are relatively similar for women and men aged 25 to 64, as seen in Table A.2 in Appendix A. Continuing to look specifically at the five sample countries, Figure 2 shows that the average of women’s years of schooling has been increasing in all countries since 1980.

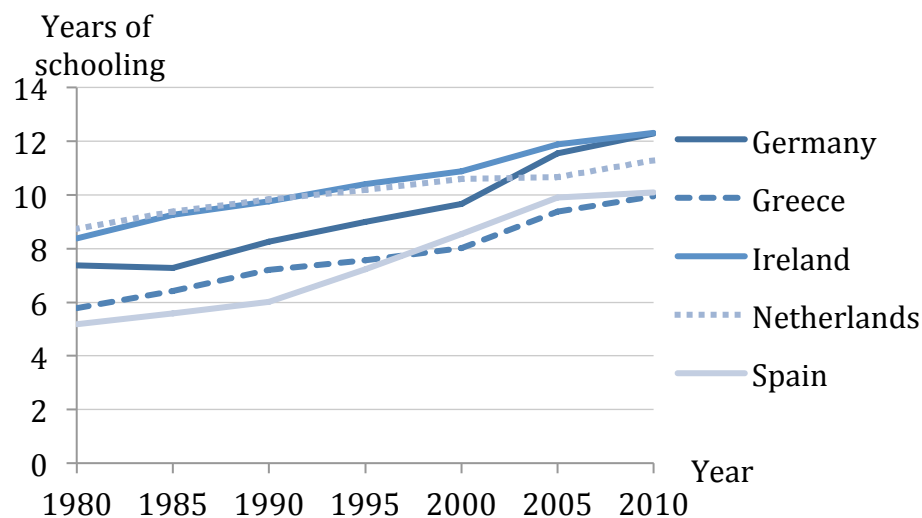


Figure 2. Average Years of Schooling

Note: The data is for women aged 25+.

Source: Barro & Lee (2010)

As several studies have shown, there are large benefits to reap from reducing the gender inequalities in the labor market. Findings suggest a positive correlation between the proportion of female workers in wage employment and economic development (World Bank, 2012). The greater diversity in leadership and subsequent innovation that would be realized if the skills of women were fully made use of in the economy would most likely increase the competitiveness of the countries (OECD, 2014). If female labor is not fully used or misallocated due to, for example, discrimination in the labor market or societal institutions, the economy could face economic losses (World Bank, 2012). As an example, ILO (2012) estimated significant economic contributions in all regions of the world if the gender gap would be lowered with global output increasing significantly as a result. Hence, obtaining more gender equality in the labor market is not only beneficial and important from a social perspective but also from an economic perspective.

2.2 Previous Research

This section presents an overview of previous studies on the subject. The use of years of schooling as a direct measure of educational attainment is often argued to result in endogeneity due to the so called “ability bias”, namely the fact that educational attainment also represents individual characteristics that cannot be well-defined and measured appropriately such as ability (Harmon & Walker, 1995; Wooldridge, 2009). The diversity of education systems, both within and between countries, further contributes to the idea that years of schooling might not be a correct measure of educational attainment (Griliches & Mason, 1972). The endogeneity problem of education could likewise arise due to the possibility that the current female labor force participation and employment rates could influence the schooling decisions of younger females, either in an encouraging or a discouraging manner.

Previous empirical studies have dealt with the endogeneity of education in different ways. One approach that has been adopted is to include measures of ability in the analysis by using ability proxies such as IQ tests (Blackburn, McKinley & Neumark, 1995; Griliches & Mason, 1972). If the estimated effect of education previously proxied for effects stemming from ability as well, the inclusion of a measure of ability should ultimately result in a decrease in the coefficient of education. Other studies⁴ have instead taken advantage of differences in schooling between twins or siblings. This approach essentially relies on the assumption that unobserved factors such as ability and motivation are inherent and thus common for these individuals (Harmon, Oosterbeek & Walker, 2003).

A third approach that has been used in several empirical studies is to deal with the endogeneity by exploiting exogenous variation in education arising due to compulsory schooling laws, which is also the approach adopted in this paper. This literature most prominently started with a well-known paper written by Angrist and Krueger in 1991 and has since then been used in various other studies. In their study, Angrist and Krueger (1991) used the quarter of birth of an individual as an instrument for education and exploited the fact that according to US schooling laws, children start school when they turn six years old and leave compulsory schooling once they reach the age of 16. Hence, individuals born late in the year should spend more time in school than those who were born early in the year. In contrast to the analysis of this paper, this kind of approach has mainly been used in the empirical work to estimate the returns to schooling, as Angrist and Krueger first did.

The idea of using school reforms as instruments for education has been widely used in studies of European countries as well. Several studies⁵ have examined the effect of education on wages in the United Kingdom by taking advantage of the exogenous changes in education that arose due to changes in schooling laws that increased the legal

⁴ See for example Ashenfelter and Krueger (1994) and Ashenfelter and Zimmerman (1997).

⁵ See for example Devereux and Hart (2008), Grenet (2013) and Harmon and Walker (1995).

minimum leaving age in the UK in 1947 and 1972. These papers suggest a positive rate of return but that the effect appears to be smaller for women than for men. Grenet (2013) argued that one potential reason for the positive effect on wages in the UK was that the reform induced individuals who would have dropped out of school in the absence of the reform to continue studying. Further studies by Meghir and Palme (1999, 2005) exploited the natural experiment nature of the increase in compulsory schooling in Sweden in the 1950s. The overall effect from increased schooling was found to be positive and the effect on earnings was found to be greater among individuals with lower education and low-educated parents. Additionally, similar approaches using school reforms as instrumental variables have been used in studies relating to Germany and France as well but no significant effect on wages could be found in these studies (Grenet, 2013; Pischke & von Wachter, 2008).

In contrast to the extensive literature on returns to education, empirical literature that mainly focuses on the causal effect of female schooling on employment is rather scarce. Additionally, no study was found that makes use of compulsory schooling reforms and that mainly focuses on this specific matter. Nevertheless, the common view among the studies that do examine the issue is that there seems to be a positive correlation between the likelihood of employment and the schooling level of a woman. To begin with, Heckman (1974) found that the probability that a woman works increased with her years of schooling, all else equal. Also, a cross-country study by OECD (2009) found benefits of education in that the likelihood of women (and men) to be employed in Germany, Greece, Ireland, Netherlands and Spain seemed to increase with educational attainment. Using the same database as in this paper (LIS), McLanahan (2004) found that the prospects of employment of a mother increased with her education level in a number of countries, including Germany and the Netherlands.

Furthermore, Jaumotte (2003) used another approach and analyzed the determinants of female labor force participation. Her findings suggest that female education has a significantly positive impact on female participation. This effect was seen to operate through the fact that education increased future earnings of women and also minimized the occurrence of specialization within couples. Oreopoulos (2006) conducted a Canadian study in which he found that individuals who obtained additional schooling were more likely to work. However, in contrast to the previously mentioned studies, Pischke and von Wachter (2008) argued that there was no employment effect of the increase in compulsory schooling in Germany. Additionally, Brunello, Fort and Weber (2009) who made use of school reforms in their analysis did not find that years of compulsory schooling affected an individual's employment decision significantly when looking at this issue as a robustness check.

Turning to the intensity of employment, findings from studies summarized by Borjas (2010) along with Bosworth, Dawkins and Stromback (1996) suggest that the relationship between wages and hours worked are slightly positive for women. In

general, the literature points to that the substitution effect dominates the income effect among working women. Further, Mincer (1974) argued that one could expect a positive correlation between years of schooling and the amount of weeks worked. On the other hand, Heckman and Macurdy (1980) found schooling of married women to have a suppressing effect on hours worked due to an increase in the demand for leisure.

3. Theory

This section provides an overview of the theoretical background of female education and employment. It is important to be aware of that theories generally have several limitations and as a consequence, do not represent perfect descriptions of the reality. Despite this, theories are examined as they are useful due to providing approximations of what one could expect in reality.

3.1 Human Capital Investment

In order to examine the impact of schooling on the female labor supply, it is essential to consider the question of why women decide to educate themselves and how this decision is made. According to theory, the basic idea is that benefits that are expected to accrue in the future are compared to costs that arise today and an individual is expected to invest in her human capital as long as the benefits exceed the costs. Even though individuals in general cannot be assumed to conduct formal calculations of costs versus future benefits, the expected future earnings are to some extent discounted in order to make an accurate comparison (Bosworth, Dawkins & Stromback, 1996; Ehrenberg & Smith, 2011; Oreopoulos, 2006). Present-oriented individuals who heavily discount the future are hence expected to invest less in their education compared to future-oriented individuals, other things equal (Ehrenberg & Smith, 2011). The costs to be taken into consideration mainly concerns opportunity costs, that is, the wage that the individual could be earning in the market if she was working instead of studying (Becker, 1975).

Another factor to consider is the influence of ability. To exclude measures of ability in estimations related to schooling is likely to result in biased estimates as pointed out by Griliches already in 1977. Individuals with high ability are likely to be more efficient in acquiring human capital and their returns to schooling are in general higher than for those with low ability. However, since high ability individuals could be expected to earn higher wages in the market regardless of their education level compared to those with low ability, the former could potentially face higher opportunity costs of education. Hence, individual ability might have an ambiguous effect on the decision to invest in schooling (Harmon, Oosterbeek & Walker, 2003). Based on this relationship, it is important to note that education might not solely be valuable in the labor market due to signaling traits like productivity, but could also function as a signal of other characteristics such as ability (Ehrenberg & Smith, 2011; Harmon, Oosterbeek & Walker, 2003).

In order to examine gender differences it could be useful to succinctly study a model of parental investment in the human capital of their children. The parents' decision of how much to invest in their children depends on a trade-off between consumption today against future consumption and the wealth of their children, with the latter being a function of the child's human capital endowments. If it is assumed that a family consists of parents and two children, a girl and a boy, the parents invest in the human capital of their children until the marginal cost (consumption of today) equals future marginal benefits. The marginal benefits will depend on the marginal utility of future consumption, remittance rate per unit of human capital, and the utility of the parents from a marginal increase in the human capital of their children. If the parents care more about the boy or if the remittance rate or the market return to investments in the human capital of boys is greater than for girls, the parents will choose to invest more in the boy (Alderman & King, 1998).

3.2 The Labor Supply Decision

Once the schooling decision has been explored, a second stage in order to analyze the impact of schooling on female employment is to examine the economic theory of a woman's decision to supply labor. This subsection starts with a short overview of the relationship between education and earnings, followed by a survey of the theoretical predictions of the female labor supply decision.

In 1974, Mincer formulated an extensively used model which in its most common form models earnings (y) as a function of original earning capacity (y_0), years of schooling (S) and years of labor market experience (X), defined as age minus year of schooling minus six. Omitting subscripts for simplicity, the model is defined as follows:

$$\log y = \log y_0 + rS + \beta_1 X + \beta_2 X^2$$

The parameter r is referred to as the economic returns to education. Although the empirics are not unanimous, numerous empirical studies⁶ examining this relationship have found that education could be expected to have a positive effect on earnings, all else equal. This positive correlation is further in line with the results found using the LIS dataset, see Table A.3 in Appendix A. As mentioned by Mincer (1974), a wage increase due to investments in human capital could have an effect on the labor supply. Hence, the suggestive positive relationship between schooling and earnings will be used in the rest of this section in order to examine the potential impact of an increase in women's years of schooling on the theoretical predictions of female labor supply.

⁶ See for example Pritchett (2006) who provides a summary over several studies examining the Mincer-coefficient for schooling in a variety of countries. In OECD countries, the average rate of return was estimated to be seven percent.

The most basic labor supply theories⁷ predict that, in a static context, an individual will enter into employment as long as her reservation wage⁸ is greater than the market wage. She is then expected to supply labor until the marginal rate of substitution equals the wage rate. It follows from this that the higher the market wage, the higher is the probability that it will exceed the individual's reservation wage and consequently, the probability that the individual will work increases. This could further be seen to be in line with the predictions of the intertemporal substitution hypothesis that expect individuals to benefit from reducing non-market work and leisure in favor of working more when the wage rate is high (Borjas, 2010). However, if the real wage increases, the effect on the labor supply decision is expected to be ambiguous. Assuming that the individual is free to choose the number of hours worked herself, the subsequent increase in the cost of leisure and household work results in a substitution effect and the hours worked increase. Yet, a higher wage rate for a given amount of hours brings about an increase in income that ultimately results in a higher consumption of goods such as leisure. This income effect will work in the opposite direction of the substitution effect and decreases incentives to work. The overall impact of the wage increase will be determined by the dominating effect and cannot be predicted based on theory alone (Ehrenberg & Smith, 2011). Nevertheless, the income effect has been suggested to be greater the larger amount of hours an individual is working and when the individual is currently not working, an increase in the overall wage rate is expected to only result in a substitution effect (Borjas, 2010; Ehrenberg & Smith, 2011).

The basic models generally treat all the non-market work as leisure, but since production does not only take place in the market these models do not properly mirror the labor supply decision (Becker, 1965; Gronau, 1977; Mincer, 1962). On the contrary, there exist models, generally known as time allocation models, that distinguish between different time usages (Bosworth, Dawkins & Stromback, 1996; Gronau, 1977). According to these models, additional labor will be supplied as long as earnings enable the individual to purchase enough goods and services in order to make up for the losses in home production and leisure (Jaumotte, 2003). Modern theories that take both leisure and work at home into account stem from work by Mincer (1962) in which the labor supply decisions depend on individual profit maximizing in which individuals work when it is more profitable to do so compared to non-market alternatives. According to Mincer (1962), the probability that a (married) woman is employed will depend on the fraction of her life-time that she works. Hence, women with longer periods of labor force participation are more likely to work.

Further important factors in the female labor supply are productivity, comparative advantages and the elasticity of the labor supply. Women have historically had a

⁷ See for instance Blundell and MaCurdy (1999), Bosworth, Dawkins and Stromback (1996), Borjas (2010), and Ehrenberg and Smith (2011).

⁸ Reservation wage is the minimum wage rate which makes an individual indifferent between working and not working (Borjas, 2010).

comparative advantage in household production since it has generally been more efficient for them to engage in home production compared to market work (Becker, 1965; Borjas, 2010). The time inflexible and routine nature of the female household tasks could be seen to further limit the women's opportunities in employment and advancement (Coltrane, 2000; Goldscheider, 2000). The fact that time spent on these types of chores has been found to be greater for females with low educational attainment than for highly educated women, retards the employment possibilities of the former even more (Hook, 2010). As a result of these types of responsibilities, the elasticity of the labor supply is in general greater for women than for men (Borjas, 2010; Gronau, 1977; Jaumotte, 2003). This elasticity appears to be greater at the extensive than at the intensive margin, indicating that individuals seem to be more sensitive to wage changes when deciding whether to work or not than when they are already working in the market (Borjas, 2010).

In summary, theories of human capital investments predict that investments in schooling are expected to increase as long as the expected benefits are greater than the costs. Also, an individual is assumed to enter into employment as long as her reservation wage exceeds the prevailing market wage. Nevertheless, there are several factors affecting the female labor supply and the suggestive increase in earnings resulting from an increase in schooling could be expected to have an ambiguous effect on women's employment decisions.

4. Data

The following empirical analysis uses data that is provided by the Luxembourg Income Survey Database (LIS). LIS is the largest database available that provides harmonized microdata collected for multiple countries over a period of decades (LIS, 2011). The LIS dataset thus enables researchers to make cross-country comparisons that would not be possible without the harmonization. The database does not contain annual data but has rather adopted the approach of "waves" which makes the data more appropriate for cross-section analyses such as the one in this paper rather than time-series analyses (Atkinson, 2004). This paper pools together data from the fifth wave for the five sample countries and includes information from the year of 2010. The data for Ireland, Greece, Spain and the Netherlands originates from the European Union Survey on Income and Living Conditions (EU-SILC) while the base for the German data is the German Panel (GSOEP).

Since the variable of interest is female employment, the sample in the main model has been restricted to females aged 25 to 64 years. Although the working age is officially defined as 15 to 64 years (Eurostat, 2014), the age of 25 is used as the lower bound since at this age, the majority of individuals could be assumed to have entered the workforce, something which could not be assumed to hold for 15 year olds. Additionally,

individuals within this age span have not yet reached the legal retirement age in the included countries (European Commission, 2011b). To ensure that only individuals who have completed their schooling are included, those who are currently enrolled in full-time education are excluded from the sample. The sample is further limited to females born in the respective countries in order to decrease the possibility of including individuals that have not completed their primary education in the country and thus might not have been affected by the reform. The number of individuals with the above characteristics and complete data on the variables in the main empirical model⁹ amounts to 21,653 females.

Summary statistics of the data is provided in Appendix A. Table A.4 shows that the females included in the sample were on average born in 1964. Further, 62 percent of the women in the sample were potentially affected by a school reform. The variable depicting the years of schooling has been derived from a variable that reports the age at which the individual completed her full-time education. Since children start compulsory primary education at the age of six in all of the included countries (European Commission, 2014b), years of schooling is constructed by subtracting six from the age when education was completed. A top-bottom approach is then adopted in order to exclude individuals with less than six years of schooling since these could not have been enrolled in compulsory primary schooling. Furthermore, individuals with values above the 95th percentile are excluded in order to minimize the possibility that extreme values might bias the estimates. As seen in Table A.2 in Appendix A, the average years of schooling were the highest in the Netherlands while being the lowest in Greece with a difference between these two amounting to almost five years.

In order for the schooling variable to be valid, there are a number of assumptions needed relating to the school attendance of the individuals. Firstly, individuals are assumed to have completed their education as intended and without interruptions. Therefore, situations such as early or late entry, grade repetition or other interruptions to schooling are not considered in the following analysis. Further, individuals are expected to have obtained their education in the country (or region in the case of Germany) in which they have reported that they currently live in.

There could be potential issues related to this way of measuring schooling. As the variable builds on the age when the individual last attended full-time education, the possibility that some individuals decided to take a couple of years off sometime during their studies would mean that their values of schooling would be overestimated in the analysis. On the other hand, schooling would be underestimated for individuals who started school earlier than the official starting age. When the data is compared to data on years of schooling from other sources such as Barro and Lee (2010), the data is similar for Greece, Ireland and Spain, see Table A.2. However, the data in this table

⁹ See equation (1) and (2).

suggests that the years of schooling for Germany and the Netherlands might be overestimated in this paper. This could as previously mentioned be due to interruptions to schooling, measurement errors et cetera. If the data would be overestimated, the estimates could be biased. Nevertheless, it should be noted that the data from Barro and Lee (2010) presents the average years of schooling for all individuals older than 25 while the data for the sample of this paper only shows the average for individuals aged 25 to 64 and hence, the two datasets could only serve as approximate comparisons.

In order to get a quick overview of whether the data seems to be in accordance with theory, Table A.6 and A.7 in Appendix A present the percentage of women employed for 5-year intervals of schooling along with the percentage of women who were employed full-year full-time per interval. The first table is in line with the second hypothesis and suggests that the probability of being employed increases with years of schooling. However, when looking at the intensity of employment, proxied by working full-year full-time, it seems like the positive relationship reaches a peak at around 20 years of schooling. Even though these tables might indicate relationships between schooling and female employment and employment intensity, no causal relationships can be drawn from these tables. The potential causality will be investigated in the following sections.

5. Methodology

5.1 Econometric Analysis

Due to the endogeneity of education, an Ordinary Least Squares (OLS) estimation that includes education as an explanatory variable but does not control for ability would violate the standard assumption of orthogonal error terms and the estimates would hence be biased and inconsistent (Verbeek, 2012). A method used to obtain consistent estimates in this kind of situations is to find an instrumental variable that fulfills the two requirements of instrument relevance and instrument exogeneity. In other words, the instrument must be correlated with schooling but uncorrelated with the error term and any omitted variables (Wooldridge, 2009). As previously explained, the instrument used in this analysis will be the compulsory schooling reforms.

The main empirical model is based on the following equations:

$$(1) \text{Schooling}_{ijk} = \alpha_0 + \alpha_1 \text{Reform}_i + \alpha_2 \text{Country}_j + \alpha_3 \text{Cohort}_k + \alpha_4 \text{Trends}_{jk} + u_{ijk}$$

$$(2) \text{FemEmp}_{ijk} = \beta_0 + \beta_1 \text{Schooling}_i + \beta_2 \text{Country}_j + \beta_3 \text{Cohort}_k + \beta_4 \text{Trends}_{jk} + \varepsilon_{ijk}$$

A two-stage least square model is used in the analysis. In the first stage, equation (1), the dependent variable *Schooling* is the years of schooling of individual *i* from country *j* belonging to birth cohort *k*. The variable *Reform* serves as an instrumental variable for *Schooling* and indicates whether an individual was exposed to the reform or not. It is

equal to one if the individual was born after the first birth cohort affected by the reform as indicated in Table B.1 in Appendix B and is zero otherwise. Consequently, the instrumental variable varies both over cohorts and across countries. In Germany where the timing of the reform varied across states as well, the value of *Reform* depends both on the birth year of the individual and on the individual's region of residence.

In the second stage, equation (2), *Employment* is regressed upon *Schooling*, with the latter variable being the prediction from the first stage regression. The dependent variable in the second stage, *Employment*, is a dummy variable that equals one if the female is employed and equals zero otherwise. As the model has a binary dependent variable, a linear probability model (LPM) is used. In the LPM, one estimates the probability that the dependent variable equals to one, that is, the probability that the individual is employed. Therefore, the parameter β_1 measures the change in probability that employment is equal to one, due to changes in schooling, all else equal.

It should be mentioned that when using LPM, it is possible to obtain fitted probabilities that is either greater than one or less than zero which do not make sense as probabilities only take values in the interval [0,1]. Nevertheless, due to focusing on the average causal effect rather than predictions, this issue is not crucial in this specific analysis. Further, when using LPM the model will by definition suffer from heteroskedasticity and hence, robust standard errors are used in the analysis (Angrist & Pischke, 2009; Wooldridge, 2009). The standard errors are furthermore clustered at the country level since the standard errors could be assumed to be independent across countries but correlated within. This since it is likely that the schooling and employment decisions of individuals from the same country are correlated as they share background characteristics and are exposed to the same type of domestic shocks and environment et cetera. Additionally, the standard assumption of normal error terms is violated when using LPM as the error term rather has the binomial distribution. Nevertheless, the LPM is robust to non-normality and provides consistent and unbiased estimates with binary dependent variables in spite of this violation (Ramanathan, 2002).

It has been discussed among economists whether to use LPM or alternative estimation methods such as probit when estimating models with binary dependent variables. Both LPM and probit have their advantages and disadvantages. One main difference is that in LPM, the marginal effects are assumed to be constant while a probit model allows for marginal effects of diminishing magnitudes (Wooldridge, 2009). Hence, the LPM results are relatively uncomplicated and easy to interpret and could provide good estimates of the average effect while the results obtained using probit could be trickier, especially in IV models (Söderbom, 2009; Wooldridge, 2002). Further, a commonly mentioned benefit with the probit model is that by assuming the standard normal distribution function, this model ensures that the estimated probabilities only take on values between zero and one (Verbeek, 2012). However, this is a rather strong assumption and as discussed above, predictions are not especially relevant for this analysis.

It is also important to keep in mind that even if the conditional expectation function (CEF) would be nonlinear, there is no guarantee that probit would be the appropriate model. Similar to estimating a linear model when the CEF is nonlinear, using the incorrect type of nonlinear model would not give the true value of the marginal effects. It follows from this brief discussion that neither of the two approaches are flawless. However, this paper will be using the LPM rather than any nonlinear approach. The main reasons behind this choice are the fact that the LPM does not rely upon substantial distributional assumptions, ease of interpretation, and that the discussed drawbacks with LPM are not extremely relevant in this specific analysis that focuses on the causal effect. As Pischke wrote in a blog post, “there is a lot to be said for sticking to a linear regression function as compared to a fairly arbitrary choice of a non-linear one” (2012, n.p.).

As much of the theory concerns the intensity of employment, this paper will also briefly explore this matter. In order to examine the effect of schooling on employment intensity, equation (1) is again used as the first stage while the second stage is replaced with the following equation:

$$(3) FYFT_{ijk} = \gamma_0 + \gamma_1 Schooling_i + \gamma_2 Country_j + \gamma_3 Cohort_k + \gamma_4 Trends_{jk} + \epsilon_{ijk}$$

The binary dependent variable *FYFT* indicates whether or not an individual has been working full-year full-time during the reference period of 2010. Full-year is defined as being employed during all 52 weeks of the year and includes paid leaves. Full-time is defined as working at least 30 hours a week. If the variable equals to zero, the individual has not been employed full-year and/or full-time.

In order to control for unobserved country and birth cohort heterogeneity, country and cohort fixed effects are included in the regression models. The inclusion of the trend variable is derived from the important assumption of parallel trends that must hold in order for the difference-in-difference approach to be valid (Angrist & Pischke, 2009; European Commission, 2011a). This assumption requires that the average years of schooling for those affected and not affected should have followed similar patterns in absence of the reforms (Abadie, 2005). If the trends are not similar between the sample countries, this should be controlled for in the regressions. Figure A.1 in Appendix A presents a plot depicting the average years of schooling for each birth cohort up until five years before the implementation of the reforms for each country. As the trends between the countries appear to deviate from each other to some extent, this suggests that country-specific linear trends need to be incorporated in order to control for such differences. As a result, the variable *Trend* that includes country-specific linear trends¹⁰ is added to the main regressions. Finally, no weights are used in the estimation as

¹⁰ The trends are constructed by taking birth cohort minus 1930. The year of 1930 is chosen as the base as it is the first birth year in the sample. Still, any other year could have been used instead.

weighting could lead to results that are misleading and difficult to interpret, especially when the model suffers from inherent heteroskedasticity (Angrist & Pischke, 2009).

There is a possibility that all individuals did not react in the same way to the changes in compulsory schooling. If this is the case, it could affect for whom the effect of the reforms is measured. For instance, there was presumably a subpopulation that stayed in school longer solely because they were forced to do so as a result of the reforms. These might have been individuals with low expected returns to education, as they would probably have left school earlier than individuals who expected high returns. Therefore, the estimates obtained from 2SLS might not necessarily mirror the potential outcomes for the entire population. In fact, under the assumptions of independence, exclusion, monotonicity and the existence of a first stage (further explained in the following section), IV estimates the average impact of increased schooling for the individuals whose schooling decisions were altered as a result of the reforms, the so called compliers (Angrist & Pischke, 2009). This is generally known as the local average treatment effect (LATE). This effect will not automatically correspond to the average treatment effect (ATE) measuring the impact of the reform on a randomly drawn individual or the average treatment effect on the treated (ATET) measuring the average effect for all those affected by the reforms (Kennedy, 2008). Consequently, heterogeneity in the individual reactions to the reforms has an impact on the type of the effect that is captured in the IV estimation and the subsequent impact on the external validity is hence important to bear in mind.

5.2 Assumptions

This subsection presents and defines a number of necessary assumptions. First of all, a key assumption is that the reforms did not affect the employment decisions of women except through years of schooling. This is generally known as the exclusion restriction and means that the school reforms should not have had any effect on other factors such as the quality of education (Wooldridge, 2009; Hoogerheide, Block & Thurik, 2012). For instance, the reforms might have led to an increased demand for teachers which could have resulted in the hiring of non-qualified teachers. If this had an effect on the quality of teaching, the validity of the exclusion restriction would be threatened (Lundborg, Nilsson & Rooth, 2014). Nevertheless, this study relies on the findings of Brunello et al. (2013) who investigated the correlation between several European compulsory schooling reforms and the quality of education and found no significant relationship between these.

In addition to the exclusion restriction, the IV model used in the analysis also requires that everyone who was affected by the compulsory schooling reforms was affected in the same manner. Therefore, all the individuals who were exposed to the reforms are expected to have experienced an increase in the years of compulsory schooling. This is commonly known as the monotonicity assumption. Further, the IV should be independent, meaning that exposure to the school reforms is assumed to have been as

good as random conditional on the birth cohort and country fixed effects included in the model. Additionally, it is assumed that the reforms did not affect the composition of the treatment and control groups (Angrist & Pischke, 2009). Hence, people are not expected to have migrated due to the increase in years of compulsory schooling. It is further assumed that the countries did not take any preventive actions or adjusted the teaching methods as a result of the compulsory schooling reforms (Ashenfelter, 1978).

6. Results

6.1 OLS Results

In order to have something to compare the 2SLS results with, baseline results are obtained by estimating equation (2) with OLS. The results are presented in Table 1. Using this method, years of schooling are found to have a small but significantly positive effect on female employment. The result does only change negligible and when rounded off no difference could be seen between the specifications that include or exclude country-specific linear trends. Still, as previously argued, these results are likely to be biased and inconsistent due to endogeneity but could provide useful as a base.

Table 1. OLS Results

| | Dependent variable: Employment | |
|--------------|--------------------------------|--------------------|
| | (1) | (2) |
| Schooling | 0.023** (0.005) | 0.023** (0.005) |
| Observations | 21,653 | 21,653 |
| F-statistic | | |
| Trend | | ✓ |

Notes: Country and birth cohort fixed effects are included. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

6.2 First Stage Results

The results from the first stage, equation (1), estimated with 2SLS are presented in Table 2. In this stage, *Schooling* was regressed upon the instrument variable *Reform*. The results imply a significant and positive relationship between the increase in years of compulsory schooling and years of schooling. One important requirement for using the school reforms as an instrumental variable is that the relationship between the reforms and years of schooling should be strong enough. A weak instrument could lead to a severe bias, incorrect test statistics and distorted confidence intervals (Wooldridge,

2009). A rule of thumb commonly used by economists is that a first stage F-statistic¹¹ greater than ten is required in order to avoid the issue of weak instruments (Staiger & Stock, 1997). According to Stock and Yogo (2005), this criterion could be seen to correspond to a five percent test that the 2SLS bias would in the most extreme case be ten percent or less. In the preferred specification (6) that includes country and birth cohort fixed effects along with country-specific linear trends, the F-statistic is 49.32 and *Reform* could hence be seen to be a valid instrument.

As seen in Table 2, the magnitude of the estimated effect of the school reforms on years of schooling changes depending on the different control variables that are included. When including country and birth cohort fixed effects there is a relatively large difference between the estimate of 0.576 and the estimation from the first specification that does not include any control variables, amounting to 2.322. Following the expectations that years of schooling differ across countries and over cohorts, some of the effect found in the first specification could be due to a resulting omitted variable bias. By including country and birth cohort fixed effects, the heterogeneity is controlled for and the estimate becomes more precise as the standard error of the estimate decreases. The estimated effect of the reform is further lower and amounts to an increase in schooling with 0.168 years when the country-specific linear trends are included in specification (6). This decrease could mean that some of what was believed to be a first stage effect in the specifications without trends could instead have resulted from that the countries were following different linear trends. The standard error of the estimate in specification (6) is smaller than without trends and the estimate obtained is even more precise.

Table 2. First Stage Results

| | Dependent variable: Schooling | | | | | |
|---------------------------|-------------------------------|---------------------|--------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Reform | 2.322*** (0.373) | 2.315*** (0.297) | 1.075** (0.549) | 0.748*** (0.179) | 0.576*** (0.058) | 0.168*** (0.021) |
| Observations | 21,653 | 21,653 | 21,653 | 21,653 | 21,653 | 21,653 |
| F-statistic | 31.06 | 48.62 | 3.07 | 14.01 | 79.88 | 49.32 |
| Country fixed effect | | ✓ | | | ✓ | ✓ |
| Birth cohort fixed effect | | | ✓ | | ✓ | ✓ |
| Trend | | | | ✓ | | ✓ |

Notes: The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors are clustered at the country level and presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

¹¹ In a test of the hypothesis that the instrument does not enter the first stage equation.

In order to graphically illustrate the effect of the reforms, the coefficients from regressing *Schooling* upon a serie of dummies and controlling for country-specific linear trends and birth cohort and country fixed effects are plotted in Figure 3. The dummy variables included indicate different years before and after the implementation of the reforms. As could be seen in the figure, there seems to be an overall jump at the year of implementation of the reforms which could lend support to the finding that the instrument positively affected individual schooling. However, the increase in schooling is not immensely apparent. This could for instance be due to measurement errors in variables such as the timing of reform implementation and year of birth or due to the possibility that individuals who completed school at an early age were likely to find ways to opt out of school even after the reforms (Stephens & Yang, 2014).

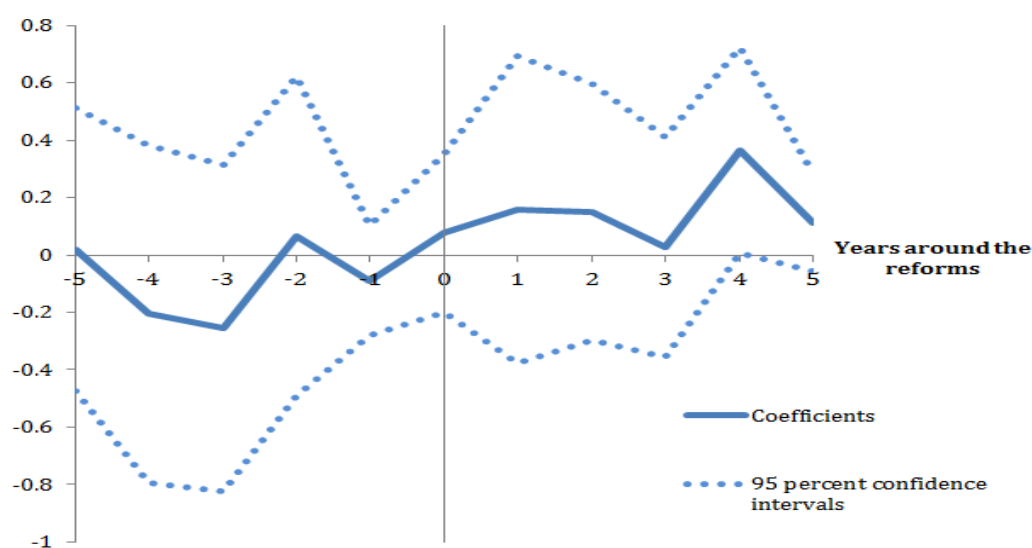


Figure 3. First Stage Plot

Notes: The category “-5” is defined as individuals born at least five years before the first birth cohort affected and the category “5” contains individuals born at least five years after the first reform cohort.

Consequently, the reforms are found to have a significant and positive effect on years of schooling in the first stage, equation (1). The significance of the coefficient of the variable *Reform* does not change when different subsamples are used or when one experiments with the inclusion of country and birth cohort fixed effects and trends. When the sample is altered regarding the age range and whether or not the individuals were born in the country or if they are currently enrolled in education or not, the estimated coefficient and its level of significance does not change remarkably, see Table 3 and Table 4.

Table 3. First Stage: Different Subsamples

| | Dependent variable: Schooling | | | |
|---------------------|-------------------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| Reform | 0.164*** (0.019) | 0.145*** (0.024) | 0.181*** (0.016) | 0.168*** (0.021) |
| Observations | 24,637 | 22,355 | 23,845 | 21,653 |
| F-statistic | 56.70 | 28.96 | 106.29 | 49.32 |
| 25-64 years old | ✓ | ✓ | ✓ | ✓ |
| Born in the country | | ✓ | | ✓ |
| Not enrolled | | | ✓ | ✓ |

Notes: Trends and country and birth cohort fixed effects are used. Robust standard errors are clustered at the country level and presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 4. First Stage: Different Age Ranges

| | Dependent variable: Schooling | | | |
|-----------------|-------------------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| Reform | 0.165*** (0.022) | 0.227*** (0.021) | 0.168*** (0.021) | 0.147*** (0.184) |
| Observations | 22,510 | 16,064 | 21,653 | 20,030 |
| F-statistic | 45.51 | 96.30 | 49.32 | 38.54 |
| 20-64 years old | ✓ | | | |
| 25-54 years old | | ✓ | | |
| 25-64 years old | | | ✓ | |
| 30-64 years old | | | | ✓ |

Notes: Country and birth cohort fixed effects are used. Country specific linear trends are included. Robust standard errors which are clustered at the country level in parentheses. *** Significant at the 1 percent level ** Significant at the 5 percent level. * Significant at the 10 percent level.

6.3 Second Stage Results

The main results from equation (2) are presented in Table 5. Although the specification with trends is the preferred one, results without country-specific linear trends are also presented in the same table. Without trends, the result does not differ greatly from the result obtained with OLS and schooling is found to have a relatively small positive effect and is significant at the ten percent level. However, the results from 2SLS and OLS differ in the preferred specification (2) when the country-specific linear trends are included and it seems like the OLS regression overestimated the correlation between schooling and employment. When using 2SLS, schooling could not be found to have a significant

effect on female employment. The coefficient from the preferred specification (2) is positive, but no causal interpretations can be made due to the lack of significance.

Table 5. Second Stage: Effect of Schooling on Employment

| | Dependent variable: Employment | |
|--------------|--------------------------------|------------------|
| | (1) | (2) |
| Schooling | 0.067* (0.039) | 0.121 (0.102) |
| Observations | 21,653 | 21,653 |
| Trend | | ✓ |

Notes: Country and birth cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors are clustered at the country level and presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

It should be noted that the regression estimates in Table 5 are obtained by using the Stata command for 2SLS since this is more appropriate as the standard errors and the subsequent test statistics are invalid when manually computed. This since the manual two-step method does not account for the fact that an estimated regressor is used in the second stage. However, the coefficients are the same¹² in both methods (Wooldridge, 2009).

The IV estimates could also be obtained by dividing the coefficient obtained from running *Reform* on *Employment* (the reduced form) with the first stage coefficient. In order to investigate if this holds, Table 6 presents the results from the reduced form. If including all the decimals obtained in the estimation, it could be seen that the estimates of the effect of schooling on employment are indeed the same in the two approaches.

Table 6. Reduced Form

| | Dependent variable: Employment | |
|--------------|--------------------------------|------------------|
| | (1) | (2) |
| Reform | 0.039 (0.025) | 0.020 (0.019) |
| Observations | 21,653 | 21,653 |
| Trend | | ✓ |

Notes: Country and cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors are clustered at the country level and presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

¹² The manually computed results are presented in Table A.8 in Appendix A.

When changing the outcome of interest to the dummy variable indicating whether an individual reported to have been working full-year full-time (*FYFT*), the second stage results are negative and significant at the five and ten percent level depending on exclusion or inclusion of trends, see Table 7. Regarding *FYFT* as a proxy for intensity of employment, these findings suggest a negative relationship between schooling and female employment intensity.

Table 7. Full-year Full-time Employment

| | Dependent variable: | | | |
|--------------|---------------------|--------------------|---------------------|---------------------|
| | Schooling | FYFT | Schooling | FYFT |
| Reform | 0.577*** (0.057) | | 0.169*** (0.021) | |
| Schooling | | -0.035* (0.019) | | -0.215** (0.064) |
| Observations | 21,593 | 21,593 | 21,593 | 21,593 |
| F-statistic | 81.29 | | 51.22 | |
| Trend | | | ✓ | ✓ |

Notes: Country and birth cohort fixed effects are included. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

6.4 Heterogeneity Analysis

This section will briefly investigate if the results found in the previous sections remain if focusing on a specific group within the sample or if there might be heterogeneity. The sample will in this subsection be restricted to those most likely to have been affected by the reforms in an attempt to increase the precision of the estimates. Previous research¹³ has suggested that individuals who grow up in a family with low socioeconomic status are more likely to have a low level of education. Hence, these individuals could also be more likely to be the ones who were induced to change their schooling behavior due to the reforms. Following the suggestive evidence of a positive correlation between education and income as well, a possible proxy to indicate whether an individual has a low socioeconomic background could be the education level of the parents. Hence, a dummy variable that equals to one if at least one of the parents has low educational attainment is generated. Low education is defined as having completed at most lower secondary education (ISCED 2) or at most ten years of schooling in the case of Greece. Due to data limitations on parental schooling, only Germany, Greece and Spain could be used in this analysis.

¹³ See for example Haveman and Wolfe (1995).

As seen in Table 8, restricting the sample to individuals who potentially have a background of low socioeconomic status results in a first stage effect that is still positive but larger than when looking at the whole sample in Table 2. Perhaps more noteworthy, the second stage result is as before positive but it is now significant at the 1 percent level. Consequently, it seems like schooling has an impact on employment for women with low socioeconomic background while not having any effect for the whole sample. One of many potential reasons behind this finding could be that individuals with a family of higher socioeconomic status might receive some help from their parents to get a job through their contacts et cetera while this might not be possible for the others whose parents might not have the possibility to help their children in this way. Therefore, individual characteristics and attainments such as schooling might matter more for the possibility of employment for the latter group as they cannot rely on contacts and so on to be offered a job. Still, these findings are only based on three countries and could as a consequence not be straightforwardly compared to the main results.

Table 8. Low Socioeconomic Status

| | Dependent variable: | |
|--------------|---------------------|---------------------|
| | Schooling | Employment |
| Reform | 0.227*** (0.045) | |
| Schooling | | 0.120*** (0.012) |
| Observations | 9,187 | 9,187 |
| F-statistic | 16.82 | |

Notes: Country-specific linear trends and country and birth cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors are clustered at the country level and presented in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

7. Robustness Checks

In order to evaluate the stability of the results this section provides the results from a number of robustness checks. Firstly, in order to control for the fact that a number of variables such as partner status, number of children and number of household members could be expected to affect the schooling decision of a woman, a set of variables are included in the first stage regression as a robustness control. The inclusion of these variables does not change the determining power of the instrument, which remains significant and with a similar size of the coefficient, see Table 9.

Table 9. Robustness Check

| | Dependent variable: Schooling |
|-----------------------------|-------------------------------|
| Reform | 0.238*** (0.024) |
| Partner | 0.004** (0.002) |
| Number of children | 0.0014 (0.090) |
| Number of household members | -0.203** (0.076) |
| Observations | 21,534 |
| F-statistic | 82.10 |

Notes: Country-specific linear trends and country and birth cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not currently enrolled. Robust standard errors which are clustered at the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

From Figure 3 that graphically displays the first stage relationship, one could as previously mentioned see a tendency of an increase in the average years of schooling after the reforms were implemented but the figure did not fully suggest a clear jump at the time of the implementations. Lundborg, Nilsson and Rooth (2014) argued that there is a possibility that there are measurement errors in the reform indicator resulting from difficulties to define the correct starting date of the reform and from that some individuals might not be in the correct grade according to their age. This error was argued to be more likely to pose a problem for cohorts close to the reform implementations. These cohorts could also be problematic due to other reasons such as the possibility that additional resources were granted to schools around the time of the reform in order for them to be able to cope with the changes. As a result, the expected jump at the implementation of the reform might not be clear. Hence, the robustness of the finding that the school reforms are valid instruments is tested by excluding cohorts close to the reform implementations in a separate regression. As shown in Table 10, the first stage relationship does not change remarkably when excluding three cohorts: one birth cohort before the implementation, the first cohort affected and the second cohort affected. The coefficient is still significant and positive even though it became slightly larger. Thereby, the result from the preferred specification might, if anything, underestimate the effect of the reforms on the individual schooling level.

Table 10. Excluding Birth Cohorts around the Reform

| | Dependent variable: Schooling |
|--------------|-------------------------------|
| Reform | 0.223*** (0.029) |
| Schooling | |
| Observations | 19,849 |
| F statistic | 47.07 |

Notes: Country-specific linear trends and country and birth cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Furthermore, the use of school reforms as an instrument requires that the reforms only had an effect on the cohorts born after the first cohorts affected as specified in Table B.1 in Appendix B. In order to examine whether the reforms did have an impact on individuals born before these years, a placebo test is performed. In the first version of this test, a dummy variable that indicates whether an individual was born the year before first birth cohort affected in each of the countries is included. The second version of the test contains an additional dummy variable for individuals born two years before the first birth cohorts affected. As shown in Table 11, the coefficients for these dummies are not significant. Hence, these findings suggest that those who were born before the implementations of the school reforms were indeed not affected by the reforms.

Table 11. Placebo Test

| | Dependent variable: Schooling | |
|-------------------------------|-------------------------------|---------------------|
| | (1) | (2) |
| Reform | 0.158*** (0.023) | 0.170*** (0.025) |
| Birth cohort one year before | -0.068 (0.111) | -0.057 (0.106) |
| Birth cohort two years before | | 0.087 (0.119) |
| Observations | 21,653 | 21,653 |
| F-statistic | 39.26 | 38.28 |

Notes: Country-specific linear trends and country and birth cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Additionally, the sample being used in the analysis consists of pooled data from multiple countries that is treated as one single population. In order to be able to use the timing of the implementations in different countries in a similar manner as when having natural experiments with regional variance, the conditional impact of the school reforms on schooling should not vary between the countries that are being examined (Brunello, Fort, & Weber, 2009). Hence, omitting one of the countries should not change the first stage result significantly. As seen in Table 12, the value and significance of the coefficient from the first-stage regression do not change remarkably when excluding the countries separately. However, when excluding Germany, the F-statistic becomes low and the problem of a weak instrument arises. This suggests that the German sample includes valuable information that contributes to the first stage effect found in the full sample. On the other hand, when excluding the Netherlands, the F-statistic increases. This could suggest that the Dutch sample includes relatively much noise and when it is excluded, the relationship thus becomes more apparent.

Table 12. Excluding Countries

| | Dependent variable: Schooling | | | | |
|------------------|-------------------------------|-------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Reform | 0.148*** (0.030) | 0.179* (0.101) | 0.178*** (0.028) | 0.185*** (0.013) | 0.156*** (0.031) |
| Observations | 18,257 | 18,426 | 19,622 | 16,412 | 13,895 |
| F-statistic | 18.56 | 2.36 | 30.68 | 145.76 | 18.59 |
| Country excluded | GR | DE | IE | NL | ES |

Notes: Country-specific linear trends and country and birth cohort fixed effects are used. The sample is restricted to females aged 25-64 who are born in the specific countries and not enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

8. Discussion

The main hypothesis that schooling has a positive effect on the likelihood of employment could not be confirmed in the main analysis. Rather, schooling was found to have no significant effect on the likelihood of female employment. Furthermore, the results suggest that schooling has a negative impact on the probability that a woman works full-year full-time. This section discusses these results and a couple of potential reasons behind them.

8.1 No Effect on Employment

The suggestive fact that female schooling does not have an impact on the likelihood of employment could be seen as somewhat surprising since one could expect that the human capital endowments of an individual would matter for the probability of

employment. However, it could be that schooling alone is not enough for increasing the prospects of employment. Perhaps, the form of investment in human capital that increases the likelihood of employment is not schooling but other types of investments. It could be that the skills learned in formal schooling are not very attractive to employers, who might prefer other forms of education such as on the job training instead. In fact, in a Eurobarometer survey from 2014, about one fourth of the Europeans did not think that their education had provided them with the skills required in order to find a position corresponding to their qualifications (European Commission, 2014c). One potential issue could be that females' incentives to invest in human capital especially intended for the market might be relatively low. As pointed out by Mincer (1974), women are less likely to invest in their human capital through on the job training since their expected work lives are shorter than those of the men due to the traditional role of women as bearing and taking care of the children et cetera. Therefore, the absence of a relationship between schooling and employment could be due to the possibility that schooling might not contribute to the skills demanded in the labor market.

It could also be meaningful to consider the possibility that women do not to enter the labor market either by their own choice or due to other responsibilities. According to a survey conducted by Eurostat (2013), the primary reason reported by about half of the female respondents aged 25 to 64 to why they did not apply for jobs was family or personal responsibilities which among other things included looking after children and incapacitated adults. For males in the same age range, only around five percent claimed that they did not seek employment due to family or personal responsibilities. The same story goes for working part-time. While the main reason for women was family and personal responsibilities, the primary reason for males was the fact that they could not find a full-time job (Eurostat, 2013). Hence, there seems to be a large difference between the genders, which in turn could be seen to be in accordance with theory in which females stay out of the labor force due to household work. Even though it could be argued that progress has been made in dividing up the responsibilities for household production, it does still seem to hinder women from entering the labor force to the same extent as males. If this female behavior is independent of the education level of the woman, this could be a potential reason behind why a significant relationship between schooling and employment is not found.

Further underlying factors could be attitudes toward gender equality and stereotypes. According to a European survey from 2015, the majority of the Europeans believed that if the mother was a full-time employee, this had a negative impact on the family life. If this is the common belief, it could be that women choose to stay at home instead of working, independent of their schooling level. The fact that half of the Europeans still held the view that men are less competent than women at performing household tasks could also be seen to lend support to the view that females might have a comparative advantage in household production as previously discussed. Hence, it could be that there

is no relationship between schooling and employment since the attitudes of society towards working females potentially impede women from entering into employment, regardless of their years of schooling.

However, the fact that there also seemed to be an overall opinion that females have the wish to enter the labor market and make a career for themselves could indicate that there might be other people involved in women's employment decisions (European Commission, 2015a). In contrast to the labor supply models discussed in the theory section, there is a possibility that the decision is not only made by the woman. Joint labor supply models generally predict that the male is the one who determines the overall labor supply of the household. Several studies¹⁴ have investigated these predictions and have found that a potential reason behind this could be that females are expected to have less bargaining power than men due to the distribution of income within households, unequal earning power, access to economic resources, fall-back utility¹⁵ et cetera. Hence, if it rather is the man in the household that makes the woman's employment decision, it could be that the decision is based on other reasons and that the female's schooling is not one of the important driving forces behind whether she is employed or not.

Other factors that could affect a woman's decision to enter into employment are different types of policies. Policies aiming to address the gender gap in employment must target the underlying set of demographic, economic and behavioral factors (ILO, 2012). It is well established that tax systems distort the choices of individuals and that high marginal tax rates effectively reduce the labor supply. However, taxes could be argued to have a greater distortionary effect on married females compared to other individuals. For married women, the marginal tax rate that matters in their decision to join the labor force or not is the average tax rate on the earnings of the second earner. Among the countries examined in this paper, it is only in Greece that single and second earners are taxed equally. The Netherlands is close to equality while Germany and Spain tax second earners around 50 percent more than single earners. The country with the most unequal tax rates is Ireland in which the second earner faces a high relative tax burden that is more than double the one for singles (Jaumotte, 2003).

Since the female labor supply is argued to be more elastic than the male supply, there is a possibility that high marginal taxation could have an especially significant effect on their decision to enter into employment. Hence, a more neutral tax treatment of second earners could be expected to boost female employment (Jaumotte, 2003). Accordingly, it could be that unequal tax systems such as the one in Ireland discourage women from entering into employment even though they might have high education. This could be

¹⁴ See for example Blundell and MaCurdy (1999), Bosworth, Dawkins and Stromback, (1996), Ehrenberg and Smith (2011), Killingworth (1983), and Lundberg, Pollak and Wales (1997).

¹⁵ The utility as a non-member of the family.

one possible explanation for the lack of relationship between schooling and employment.

Another type of policies that could affect the employment decision of females is policies related to child care such as different kinds of subsidies and benefits. Empirical findings on child care policies has found that child care subsidies increase the number of women in the labor force (Jaumotte, 2003). Further evidence provided by for example Gustafsson and Stafford (1992) showed a positive effect of the Swedish public child care on the labor force participation of mothers. Hence, if a country does not have proper family policies, this could hinder women from entering into employment even though they might have many years of schooling. Of the five countries analyzed, public expenditures on formal daycare measured in percentage of GDP was the highest in Germany and Greece (0.4 percent) while being the lowest in Spain (0.1 percent), compared to the average of 0.5 percent for the European countries in the study by Jaumotte (2003). Child benefits were also below average in all the sample countries except in Germany (Jaumotte, 2003). Consequently, policies concerning child care and benefits could from a European perspective be relatively limited in the five sample countries, indicating that this could potentially impede women from entering into employment. In line with this, Europeans mentioned that they believed that two of the most effective ways to increase the number of women in the labor market were to increase the accessibility of child care and to make it easier for women to combine work with household and care responsibilities (European Commission, 2015a).

Another reason behind the insignificant result could be potentially low returns to schooling. If the returns to schooling are low, as they have been found to be in some relatively recent studies such as the one by Pischke and von Wachter (2008), one could expect that increased schooling might not have any large effects on the labor supply either. Hence, the possible presence of low returns to schooling might contribute to the lack of relationship between schooling and the probability of female employment. Further, as the IV estimates are measured for those who altered their behavior due to the reforms and who would likely have dropped out of school at the former minimum leaving age otherwise, the expected returns for these individuals could be assumed to be lower than for those who would have chosen to stay in school even in the absence of the reforms. With this in mind, the insignificant result could seem more plausible.

As previously mentioned, increasing women's human capital endowments and their economic opportunities were believed to be two possible ways to increase gender equality. Therefore, it is in addition compelling to shortly discuss the potential impact of the finding that schooling does not seem to have a significant effect on the probability of employment. One straightforward conclusion would be that policies that aim at increasing female employment rates should not plan to do so by increasing female schooling from low levels. Consequently, increasing compulsory schooling even further does not appear to be an effective strategy if one believes in the results of this paper.

Policy makers should rather focus their measures on other areas related to female employment in order to increase the employment rates. They could also combine school reforms with other policies. The different potential reasons behind the result that have been discussed above could possibly serve as a starting point for the discussion of which other areas to target instead of directly targeting compulsory schooling.

8.2 Negative Effect on Full-year Full-time

The finding that an increase in schooling results in a decrease in the probability of full-year full-time employment could also be slightly surprising as previous literature suggested that higher schooling led to an increased amount of market work. Possible explanations for the contrasting result of this paper can be drawn from the suggestive fact that more schooling could be expected to lead to an increase in the wage rate, as supported both by numerous empirical studies and the regression in Table A.3 in Appendix A. In view of the theoretical predictions mentioned earlier, there was argued to be both an income and substitution effect affecting the labor supply of an individual when she experienced a wage increase. The income effect led to a decrease in the hours worked while the substitution effect increased the hours, and the ultimate impact of increased schooling could be argued to be ambiguous due to these contradicting forces. The finding that the employment intensity, proxied by full-year full-time employment, is negatively correlated with schooling could suggest that the income effect might outweigh the substitution effect for the specific sample investigated.

Another rationale behind the negative relationship could relate to assortative mating with respect to education. There are several studies¹⁶ that have suggested that individuals are more likely to marry individuals with similar levels of schooling. Hence, highly educated females could be expected to partner with highly educated males. Assuming that the results from the Mincer model are valid, increased schooling would yield a higher wage rate. Combining these concepts, it could be that females with many years of schooling marry men with high wage rates. As women generally¹⁷ have lower wages than men, the cost of female leisure is likely to be lower than that of male leisure. If the woman is free to choose her amount of time worked herself, she might reduce her time worked in the market as a consequence. Accordingly, females with more schooling could be expected to be able to work less due to the relatively high income of their partners and the lower opportunity cost of women.

In a similar manner as when regarding women's employment decisions, attitudes and stereotypes could be underlying factors to the negative relationship between schooling and the intensity of employment. If women decide to enter into employment regardless of the view of society on working females, they might alter their decisions relating to the intensity of their employment instead. As mentioned earlier, full-time working mothers

¹⁶ See for instance Boulier and Rosenzweig (1984) and Greenwood et al. (2014).

¹⁷ In Europe, the average gender pay gap amounts to approximately 16 percent (European Commission, 2014d).

were perceived to negatively affect the family life. Hence, if working females feel that society hold this view, it could be that they decide to work but to not work full-time in order to become more socially accepted. However, if women with lower education earn less than highly educated females, it could be that the former must work full-time in order to be able to pay rent, buy groceries, support their families et cetera. Thus, there is a possibility that those with lower schooling might not afford to act in accordance to the opinions of the others who believe that women should work less but rather need to work as much as possible to make it all go around financially.

A further reason behind the negative result could be that women might find part-time work especially attractive as it enables them to combine commitments both at work and at home (Silim & Stirling, 2014). Hence, if highly educated women earn more than those with less education, the former could afford to both work and still fulfill the commitments at home by adjusting their amount of working time while the latter might not be able to do so due to financial constraints. Additionally, it could be that well-educated females have better prospects than low-educated of being offered beneficial employment contracts in which they are freer to choose their own working hours.

8.3 Data, Methodology and Measurement Errors

This subsection considers the potential impact of issues such as the quality of the data and the sample. Starting with the sample, a fundamental element in order to obtain a sample whose properties mirror those of the whole population of interest is to have a random sample. Even if surveys practice random sampling procedures it could be questioned whether the respondents really do represent all types of people living within each country. If not, the lack of data from certain groups of people could potentially lead to results that are not representative for the whole population of interest. Hence, the convenience sample of females born in the five countries, aged 25 to 64 and who are currently not enrolled in education might not represent all individuals in the countries with these specific characteristics. Thus, one should be careful when considering the external validity of the results.

Additionally, there is a possibility that some of the assumptions made in the analysis might not fully hold in reality. Some of the assumptions are not easily testable and hence, it is difficult to know for certain if they are true or not. For example, the requirement of no systematic migration due to the school reforms could potentially have been violated. However, it is not easy to test whether those who migrated around the time at which the reforms were implementations did it as a consequence of the changes in the years of compulsory schooling or due to other reasons unrelated to the school reforms. As no related data is available in LIS, this could not be tested in this paper.

There could also be issues related to the choice of econometric model. In contrast to OLS that produces both consistent and unbiased estimates under a certain set of assumptions, the estimates obtained using 2SLS are in general consistent but biased.

This bias could be seen to arise due to the fact that the first stage is fitted and hence contain randomness. The bias will be more severe, the weaker the instrument is. Nevertheless, when the sample is large, the 2SLS estimates are likely to be close to the causal effect (Angrist & Pischke, 2009). Due to the relatively large sample size in this analysis and the rather high F-statistic of the instrument, one could expect that the 2SLS bias should not be a great cause for concern. However, the fact that the standard errors are clustered but that the number of clusters is low could be another matter contributing to the risk that the estimations might suffer from a bias (Angrist & Pischke, 2009). Essentially, the fundamental issue with the model choice is that the true model is unknown.

The estimation could also suffer from measurement errors. One could assume that as the analysis is based on measures of objective facts rather than subjective conditions, the measurement errors should be relatively modest. People could be assumed to remember whether they worked last year or not, how much they did work et cetera. However, there is still a risk that an individual's answers do not perfectly correspond to the true values for the specific individual, but also contain some elements of error in spite of their objective nature. This could be a result of various factors such as selective memory, misunderstanding of the questions or the type of answers that were asked for. For example, individuals might not remember the exact age at which they completed their full-time education and instead answered with an approximate value. This would in turn lead to measurement errors in the schooling variable that is used in the analysis. Respondents could also have had difficulties remembering the amount of hours worked per week or weeks worked per year and hence, answered incorrectly on these survey questions that form the basis for the full-year full-time variable. These kind of errors could potentially threaten the quality of the answers and hence, the validity of the analysis.

9. Concluding Remarks

This paper examined one possible policy approach to increase female employment rates and hence to decrease gender inequalities in Europe. To investigate this, data from 2010 for women aged 25 to 64 from five eurozone countries was used in order to examine the impact of schooling on women's employment decisions and on their employment intensity. This was done in a 2SLS framework by exploiting the variation over birth cohorts and across countries in reforms increasing the years of compulsory schooling.

The findings imply that the school reforms had a positive effect on years of schooling, all in accordance with the first hypothesis. Further, the results suggest that a woman's schooling has no significant effect on the probability of employment. This was not in line with the common view on the matter and with the hypothesis of a positive relationship. After performing a number of robustness checks, it could be argued that the results were relatively stable. The lack of a relationship between schooling and employment

could be due to the possibility that schooling is not the kind of human capital that is demanded and rewarded in the labor market. It could also be that family responsibilities and attitudes towards working women impede women from entering into employment, irrespectively of their years of schooling. Different types of policies related to child care and taxes might be additional obstacles for women to be employed. If it is difficult to combine work in the market with household responsibilities, females might have to stay at home regardless of whether they are well-educated or not. Tax systems that are less neutral with respect to the taxing of second earners could also stagnate women's entering into employment and hence, possibly underlie the insignificant relationship between employment and schooling.

As gender equality is one of the fundamental values in the European Union and as some of the focus of the union's policies currently is on increasing female employment rates, the finding that women's schooling does not seem to have an effect on the likelihood of employment could have valuable policy implications. According to the results, policy makers should not try to increase the female employment rates by increasing schooling from low levels since that does not appear to be especially efficient. However, the result does not altogether reject the efficiency of policies related to schooling as these could still be efficient if, for example, designed in other ways or if combined with other policies.

When considering the intensity of employment, a negative relationship was found between schooling and the intensity, proxied by full-year full-time employment. This result was likewise not in accordance with the hypothesis of a positive relationship that was based upon previous empirics. Potential reasons could be drawn from the suggestive fact that women with more schooling in general could be expected to have higher wages than low-educated women. Hence, the former might afford to alter their working hours due to various reasons such as more flexible employment contracts. The negative relationship could also be due to a dominating income effect, assortative mating and differences in the opportunity cost of leisure.

9.1 Future Research

There are a number of ways in which other studies aiming to investigate similar matters could be revised. One approach would be to have access to a more extensive dataset that contains data on migration, country of schooling, actual years of schooling, the prevalence of grade repetition et cetera. This would for example make it possible to investigate the potential presence of migration around the timing of the reforms. It would also be interesting for future research to examine the relationship between schooling and female employment and work intensity over time by using time-series data.

Moreover, future research could further investigate whether the impact of schooling differs between individuals in different parts of the income distribution, something that

could only briefly be done in this paper due to data limitations. Further, as no relationship was found between schooling and employment there might be other forms of human capital investments that could affect the likelihood of female employment. Hence, future studies could for instance investigate the relationship between on the job training and individuals' employment decisions. Further, this paper examined the possibility to decrease gender inequalities by increasing female employment through increases in compulsory schooling. It would also be interesting to investigate other potential ways to decrease the inequalities.

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Appendix A: Figures and Tables

Table A1. Employment Rate by Gender

| Country | Year | Females | | | Males | | |
|-------------|------|--------------|---------------------|-----------|--------------|---------------------|-----------|
| | | Observations | Employment rate (%) | Std. dev. | Observations | Employment rate (%) | Std. dev. |
| Germany | 2004 | 6,016 | 71.6 | 45.09 | 5,622 | 84.5 | 36.20 |
| | 2007 | 5,765 | 73.5 | 44.15 | 5,260 | 85.7 | 35.05 |
| | 2010 | 6,139 | 74.4 | 43.63 | 5,388 | 85.3 | 35.43 |
| Greece | 2004 | 3,591 | 51.1 | 50.00 | 3,398 | 81.9 | 38.51 |
| | 2007 | 4,068 | 55.1 | 49.74 | 3,886 | 81.1 | 39.19 |
| | 2010 | 3,440 | 48.2 | 49.97 | 3,337 | 68.7 | 46.36 |
| Ireland | 2004 | 3,221 | 55.4 | 49.71 | 3,062 | 77.3 | 41.92 |
| | 2007 | 2,588 | 55.6 | 49.69 | 2,455 | 72.0 | 44.92 |
| | 2010 | 2,077 | 53.3 | 49.90 | 1,931 | 64.8 | 47.76 |
| Netherlands | 2004 | 5,869 | 71.8 | 45.02 | 5,665 | 86.7 | 33.95 |
| | 2007 | 6,603 | 70.4 | 45.66 | 6,387 | 86.7 | 33.97 |
| | 2010 | 6,403 | 71.9 | 44.94 | 6,132 | 86.7 | 33.99 |
| Spain | 2004 | 9,144 | 49.7 | 50.00 | 8,739 | 80.2 | 39.81 |
| | 2007 | 8,786 | 57.2 | 49.48 | 8,357 | 79.5 | 40.36 |
| | 2010 | 8,290 | 54.9 | 49.76 | 7,921 | 71.8 | 44.99 |

Notes: The subsample contains individuals aged 25-64 who are born in the country and not currently enrolled in education.

Table A.2. Years of Schooling by Gender

| Country | Year | Females | | | | Males | | | |
|-------------|------|--------------|--------------------|-----------|-----------|--------------|--------------------|-----------|-----------|
| | | Observations | Years of schooling | Std. dev. | Barro-Lee | Observations | Years of schooling | Std. dev. | Barro-Lee |
| Germany | 2010 | 3,278 | 14.7 | 3.67 | 12.3 | 3,056 | 14.9 | 4.42 | 13.1 |
| Greece | 2010 | 3,396 | 11.6 | 4.50 | 10.0 | 3,285 | 12.0 | 4.47 | 10.7 |
| Ireland | 2010 | 2,031 | 12.1 | 2.99 | 12.3 | 1,895 | 12.0 | 3.34 | 12.0 |
| Netherlands | 2010 | 5,241 | 16.4 | 4.05 | 11.3 | 4,773 | 17.5 | 4.33 | 11.9 |
| Spain | 2010 | 7,758 | 11.9 | 4.54 | 10.1 | 7,512 | 11.8 | 4.44 | 10.4 |

Notes: Years of schooling have been top-bottom coded which indicates that values above 6 years and above the 95th percentile are excluded. The sample includes individuals aged 25-64 who are born within each country and not currently enrolled in education. The data from Barro-Lee (2010) shows the average years of total schooling, age 25+.

Table A.3. Returns to Schooling

| | Dependent variable: | |
|--------------|---------------------|------------------------|
| | Schooling | Paid employment income |
| Reform | 0.191*** (0.050) | |
| Schooling | | 0.325** (0.160) |
| Observations | 12,669 | 12,669 |
| F-statistic | 11.82 | |

Notes: Trends and country and birth cohort fixed effects are included. The sample is restricted to females aged 25-64 who are born in the specific countries and not currently enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table A.4. Summary Statistics: Year of Birth

| Country | Sample size | Mean birth cohort | Std. dev. |
|-------------|-------------|-------------------|-----------|
| Germany | 6,252 | 1963 | 10.57 |
| Greece | 3,440 | 1964 | 11.01 |
| Ireland | 2,077 | 1965 | 11.12 |
| Netherlands | 6,403 | 1963 | 10.57 |
| Spain | 8,290 | 1964 | 10.63 |
| Total | 26,462 | 1964 | 10.70 |

Notes: The sample is restricted to females, aged 25-64 who are born in the countries and not currently enrolled in education.

Table A.6. Employment Rate by Years of Schooling

| Years of schooling | Observations | Average employment rate (%) | Std. dev. |
|--------------------|--------------|-----------------------------|-----------|
| <5 | 211 | 18.5 | 2.68 |
| 6-10 | 5,671 | 38.5 | 0.65 |
| 11-15 | 8,911 | 63.2 | 0.51 |
| 16-20 | 5,267 | 78.1 | 0.57 |
| 21-25 | 1,349 | 81.8 | 1.05 |
| 26-30 | 244 | 78.7 | 2.63 |

Notes: The sample contains females aged 25-64 who are born in the countries and not currently enrolled in education.

Table A.7. Full-year Full-time Employment Rate by Years of Schooling

| Years of schooling | Observations | Average full-year full-time employment rate (%) | Std. dev. |
|--------------------|--------------|---|-----------|
| <5 | 211 | 11.37 | 2.19 |
| 6-10 | 5,659 | 24.24 | 0.57 |
| 11-15 | 8,877 | 33.83 | 0.50 |
| 16-20 | 5,254 | 44.21 | 0.69 |
| 21-25 | 1,348 | 39.02 | 1.33 |
| 26-30 | 244 | 39.75 | 3.14 |

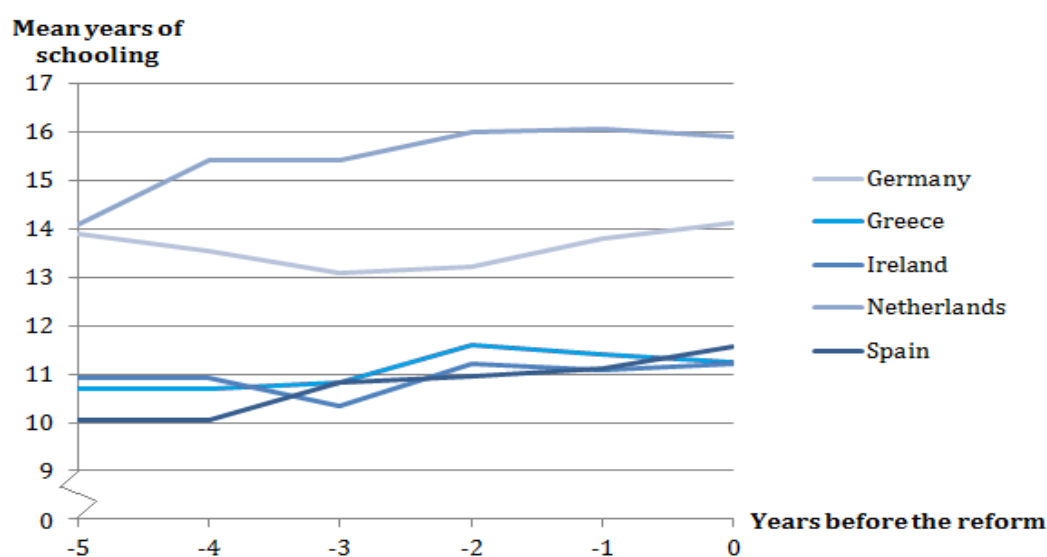
Notes: The sample contains females aged 25-64 who are born in the countries and not currently enrolled in education.

Table A.8. Manual IV

| | Dependent variable: | |
|----------------|---------------------|------------------|
| | Schooling | Employment |
| Reform | 0.168** (0.024) | |
| $\hat{\delta}$ | | 0.121 (0.113) |
| Observations | 21,653 | 21,653 |

Notes: $\hat{\delta}$ is derived from the fitted values from the first stage regression. Trends and country and birth cohort fixed effects are included. The sample is restricted to females aged 25-64 who are born in the specific countries and not currently enrolled in education. Robust standard errors that are clustered on the country level in parentheses. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Figure A.1. Country-specific Trends



Appendix B: School Reforms

Table B.1. Compulsory Schooling Reforms

| Country | Year of the reform | First birth cohort affected | YCE ^a before | YCE ^a after | Age at school entry |
|------------------------|--------------------|-----------------------------|-------------------------|------------------------|---------------------|
| Germany | | | | | |
| Schleswig-Holstein | 1956 | 1941 | 8 | 9 | 6 |
| Hamburg | 1949 | 1934 | 8 | 9 | 6 |
| Lower Saxony | 1962 | 1947 | 8 | 9 | 6 |
| Bremen | 1958 | 1943 | 8 | 9 | 6 |
| North Rhine-Westphalia | 1967 | 1953 | 8 | 9 | 6 |
| Hesse | 1967 | 1953 | 8 | 9 | 6 |
| Rhineland-Palatinate | 1967 | 1953 | 8 | 9 | 6 |
| Baden-Württemberg | 1967 | 1953 | 8 | 9 | 6 |
| Bavaria | 1969 | 1955 | 8 | 9 | 6 |
| Saarland | 1964 | 1949 | 8 | 9 | 6 |
| Greece | 1975 | 1963 | 6 | 9 | 6 |
| Ireland | 1972 | 1958 | 8 | 9 | 6 |
| Netherlands | 1975 | 1959 | 9 | 10 | 6 |
| Spain | 1970 | 1957 | 6 | 8 | 6 |

Source: Brunello, Fort & Weber, 2009.

^aYears of compulsory education.

Additional information about the compulsory school reforms

Germany

In 1964, the Agreement on the Unification of the School System, also called Hamburg Accord, was signed. The agreement aimed to uniform school starts and increased the compulsory schooling up to grade nine. The increase was introduced at various times in different states and was not universally in place until 1970 (Pischke, 2003; Pischke and von Wachter, 2008).

Greece

The Greek Parliament increased compulsory schooling by three years (from six to nine years) in 1975. Those who were potentially affected by the reform were the children who turned 12 in 1975, meaning that the first birth cohort affected was those born in 1963 (Brunello, Fort & Weber, 2009).

Ireland

The school leaving age in Ireland was raised by one year to 15 in 1972. Individuals who were potentially affected by this reform were those who were 14 year olds in 1972.

Hence, the cohort of 1958 is treated as the first birth cohort affected (Brunello, Fort & Weber, 2009).

Netherlands

In 1975, the former three-year educational programs were made into four-year programs and compulsory schooling leaving age was increased by one year from the age of 15 to 16, leading to a total of ten years of compulsory schooling. Following the approach by Brunello, Fort and Weber (2009), the first cohort affected is assumed to be those born in 1959.

Spain

In the 1970 General Act on Education and Financing of Education Reform (LGE), the years of compulsory schooling were increased from six to eight years. Those who were born in 1957 and onwards were likely to be affected by this reform (Brunello, Fort & Weber, 2009).

Appendix C: Definitions

Table C1. Variable List

| Name | Label | Definition |
|-----------|--|--|
| AGE | Age in years | |
| CTRYBRTH | Country of birth | Information is provided by the respondent about the country where she was born. |
| EDCAGE | Age when completed education | Age of individual when last attended continuous full-time education. |
| EDDAD_C | Education of father | Highest education level of father in a country-specific format being defined as highest year/degree or level of education which has been either completed or attended. |
| EDMOM_C | Education of mother | Highest education level of mother in country-specific format being defined as highest year/degree or level of education which has been either completed or attended. |
| EMP | Employed | Indicator of employment, which is in line with the ILO definition of employment, for the current period. |
| ENROLL | Currently enrolled in education | Indicator of current educational enrollment (in or above primary level program). |
| FYFT | Full-year full-time | A variable indicating whether a person has been working full-year full-time during the reference year. Full-year means that the person has been employed during all 52 weeks of the reference year (including periods on paid leave from employment), and full-time is defined as working 30 hours per week. |
| ILE | Paid employment income | Monetary and non-monetary payments received in counterpart for dependent employment. |
| NCHILDREN | Number of own children living in household | Number of (biological, adoptive or step) children of the individual who are presence in the household. |
| NHHMEM | Number of household members | The number of persons in the survey unit. |
| PARTNER | Partner | An individual is considered to have a partner if he/she is married or in consensual union. |

Source: LIS Variable Definition List, Available at: <http://www.lisdatacenter.org/our-data/lis-database/documentation/> [Accessed 20 February]

C.2 Abbreviations

2SLS - Two-stage least squares

ATE - Average treatment effect

ATET - Average treatment effect on the treated

GDP - Gross domestic product

ILO - International Labour Organization

IV - Instrumental variable

LATE - Local average treatment effect

LIS - Luxembourg Income Survey

LPM - Linear probability model

OECD - Organisation for Economic Co-operation and Development

OLS - Ordinary least squares