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Selection Bias in Heterogeneous Returns to College in Highly Stratified Educational Systems in Europe

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Abstract

Information on economic returns to college education has indisputable value, yet most existing estimates in Europe unrealistically assume the homogeneity of treatment effects across the population or the ignorability assumption. In this paper we relax those assumptions by using the newly developed local instrumental variable method for heterogeneous treatment effects due to unobservables presented in general form in Heckman, Urzua, and Vytlacil (2006). The method is applied to the measurement of earnings returns to college education for 28-to-38-year-olds in Austria, Czech Republic, Germany, Poland, and Slovakia (and the United Kingdom as well, as an additional point of reference) based on pooled EU-SILC data from 2005 and 2011, when data on parental background were also collected. Our results reveal positive self-selection, or ability bias, across countries and for both men and women in most, though not all, modeled conditions. In a world of essential heterogeneity, we find that the marginal returns to education for those who achieved some kind of college degree are substantively larger than the estimated returns for those who did not go to college, as well as for estimates generated from a Mincer-type model using the same data. We suggest, though cannot demonstrate, that the highly stratified nature of these educational systems create mechanisms for students to be more realistic about their educational and economic prospects, which may contribute to the size of positive self-selection estimated.

Introduction

For decades, the study of economic returns to education has been a cornerstone of research on the accumulation and distribution of human capital and its impact on economic growth. Within the human capital framework, Mincer (1974) developed a widely accepted earnings equation to empirically estimate the rate of returns to schooling, under the assumption of homogeneity. The Mincer equation – which uses OLS regression with logged earnings as the dependent variable and years of schooling as a primary independent variable, along with a separable quadratic function for work experience – was regarded as "one of the great success stories of modern labor economics" (Willis 1986: 526). Studies of Mincer returns have tracked wage premiums by educational level, over time, between countries and regions, by gender, and have been cited by scholars thousands of times (e.g. Psacharopoulos 1973, 1985, 1994; Psacharopoulos & Patrinos 2004). In the European Union, where national differences in labor market conditions are large despite the single market, Mincer rates of return are still widely used in policy debates on equity and efficacy of investments in education (European Commission 2010; Glocker & Steiner 2011).

While there have been various attempts to address the endogeneity problem of education in earnings function, the validity of the overall Mincerian endeavor has been cast into serious doubt by Heckman, Lochner, and Todd (2006), who rejected the Mincer model on both theoretical and methodological grounds. At issue in that critique was that the commonly estimated Mincer rates of return using survey data may be different from the ''true'' causal effect under a hypothetical scenario of random assignments. As is well-established in the social stratification literature, education is itself an attained characteristic that is unevenly distributed by social class, ability, and other factors. Because of this, there may be selection bias in terms of the kinds of people who select (or are selected) into college – based on their observable and unobservable characteristics – that enable them to earn more in the labor market. From this view, the typical person who goes to college chooses to do so because of the economic benefits the degree will bring due to his/her higher unobserved endowments, such as ability and effort, whereas others with different

endowments do not perceive the same economic returns to college (Card 1999). This is called "ability bias" or, simply, "selection bias." As Heckman (2001) pointed out, the presence of individual heterogeneity and self-selection gives rise to a sorting gain, which biases the standard estimator for the causal effect of schooling with observational data.

Contemporary research on the causal impact of higher education on earnings has identified two major assumptions that generate selection bias (Heckman, Lochner, and Todd 2006). Neither of these assumptions, which are taken for granted in Mincer-type analyses, may hold true: (1) that the effect of education is homogeneous across different members of a population of individuals with the same observed attributes; and (2) the ignorability assumption, or selection on observables, which assumes that the distribution of college education in a population is random after controlling for relevant observed covariates. The first "homogeneity assumption" is highly implausible in real world conditions because the earnings benefit different people would receive by the treatment condition can vary greatly due to the "essential heterogeneity" (Heckman, Urzua, and Vytlacil 2006) of an individual's life condition. The second ignorability assumption (Morgan and Winship 2007) is also problematic due to the role of ability or family background in systematically affecting an individual's chances of receiving the treatment condition.

Much previous work in sociology on heterogeneous treatment effects (HTE) of higher education (e.g., Brand and Xie 2010; Tsai and Xie 2008) has relied on the ignorability assumption, i.e., it is assumed that those who attend college and those who do not are not systematically different, conditional on observed covariates. In this paper, we relax that assumption by using the newly developed instrumental-variable method for HTE in economics developed by Heckman, Urzua, and Vytlacil (2006). This new method was also used in Tsai and Xie (2011), who found heterogeneous treatment effects of higher education on earnings in Taiwan; their empirical results revealed distinct patterns by gender, with positive selection bias most clearly shown among women rather than men.

In this paper we also adopt the approach of Heckman, Urzua, and Vytlacil (2006) to measure heterogeneous treatment effects of higher education on earnings. The empirical context of our study is Central Europe – Germany,

Austria, Poland, Slovakia and the Czech Republic, along with the United Kingdom as an additional point of reference. Though we have the ambition of analyzing all European countries possible within the EU-SILC data, we begin our analysis with Central European countries because of the similarity of their educational systems, in addition to having similarly open and integrated economies and labor markets. After overviewing those countries, the paper then explains the methods and data in more detail, before presenting the results of our study.

Empirical Context

While it would be wonderful to focus on a large set of European countries, the substantial computational demands of our methodological approach requires that we begin with a smaller number of countries. The benefit of this, however, is that we can control for institutional factors at the country level. The countries under study neighbor each other, have strong trade inter-linkages and thus experience similar business cycles, and share similarities in labor market flexibility and the female labor participation (about 55% in Germany and Austria, and 50% in Poland, Slovakia and the Czech Republic). Male industrial employment is also high in all of these countries, ranging from 39% in Austria to 51% in Slovakia (World Bank Indicators 2013). Besides variance in unemployment rates across the countries, there are many similarities in their macroeconomic conditions (Eurostat 2013).

Educational systems structure the educational opportunities of pupils and the ways families make key educational and labor market decisions (Buchmann & Dalton 2002). Sociologists have for decades compared the degree of stratification of educational systems, and have commonly grouped the countries in this study as sharing the characteristics of early tracking and a high degree of institutional stratification and vocational specificity (Shavit & Müller, 1998; Allmendinger 1989; Kerckhoff 2001). For example, Matějů et al (2007) recently compared key indicators of secondary educational systems across OECD countries, such as the number of school types, and enrollment in vocational and academic programs, as well as indicators of tertiary education, such as net enrollment rates, private and public spending, and financial aid. What they found was that, across OECD countries, indicators of the stratification and vocational

specificity of secondary and tertiary education are correlated with each other, as well as that all of the countries in this study fall within the category of having highly stratified educational systems.

In terms of specifics, already at the end of primary education (in 4th to 6th grade, depending on the country), pupils in Germany, Austria, Slovakia and the Czech Republic take high-stakes tests to determine how they will be sorted into one of three different educational tracks. They can apply to highly selective grammar schools (*gymnasium*), which provide pupils with the best credentials and training for higher education, as well as to less academic schools at the lower secondary level. In Austria, Czech Republic and Slovakia, the alternative to long-form (typically 8-year) grammar schools are to continue studies in basic schools or transfer to lower vocational schools. In Germany, in addition to grammar schools, pupils can also apply to a *Realschule* (an intermediate track covering the 5th to 9th grades), or *Hauptschule* (the lower track), the latter of which provides basic general education and usually covers the 5th to 9th grades. In Poland, all pupils in lower secondary education attend a grammar school, though pupils must apply to them on a competitive basis at about the age of 12 and there are major differences between schools in quality and prestige.

In Austria, Czech Republic and Slovakia, pupils who did not succeed in attending the long-form grammar schools can apply again to grammar schools that cover upper secondary education, or apply to upper technical schools or apprenticeship programs, the latter provide the least likely chances of attending higher education. Similarly, after completing a grammar school in Poland at the lower secondary level, pupils apply at the upper secondary level to attend the more prestigious *liceum*, which prepare pupils the most for higher education, or technical schools that provide more vocationally-specific education. In Germany, pupils generally attend the same school through lower and upper secondary education; pupils attending the *Hauptschule* and *Realschule* often complete their education with vocational and apprenticeship programs that provide direct linkages to the labor market, often in the form of on-the-job training. In all countries, opportunities for university attendance depend greatly on the type of secondary school attended and the credentials achieved in secondary education.

At the level of tertiary education, these countries have not experienced

the same degree of educational expansion as in, for example, the United Kingdom. In the latter, over 47% of males and females aged 30-34 have attained tertiary education, compared to only 39% in Poland, 32% in Germany, 26% in Austria and the Czech Republic, and 24% in Slovakia (Eurostat 2013). Analyses of Mincerian returns to education in the countries under study have not found that educational expansion dilutes the economic value of education. In Germany, for example, data from the German Socio-Economic Panel suggests that returns to education declined until the late 1990s and increased significantly afterwards, even as educational expansion continued (Gebel & Pfeiffer 2007). In post-communist Poland, Slovakia and the Czech Republic, returns to education increased rapidly with the introduction of the market economy even as their tertiary educational systems also expanded rapidly (Filer, Jurajda & Planovsky 1999).

Theoretical considerations

These similarities in educational systems and in socio-economic context inform the theoretical considerations of this study. Recall that human capital theory posits that schooling causally affects earnings positively, since it raises workers' skills and productivity, and thus their value on the labor market (Becker 1964; Blau and Duncan 1967; Mincer 1974). However, alternative views of the school-earnings link is that it is not necessarily causal, since a part of the purported effect of schooling on earnings may be spurious as a result of other factors that drive how different people are selected into college. There are in fact two sources of selection at work: social selection due to family background effects, and self-selection (or ability bias), due to the effect of unobservables at the individual level.

On the one hand, analyses of Mincerian returns to college are blind to the different propensities of people to achieve college education based on social background. The Mincerian model provides biased average returns to schooling because, for example, the observed sample of those who attained college education could be itself a product of selection by socio-economic status of the background family, such that reported coefficients may not estimate the economic return of the educational investment, but rather the "return" of the

intergenerational reproduction of social status from parents to children. Individuals from socio-economically advantaged backgrounds are the most likely to attain college education and earn more on the labor market, but it is likely that they would have relatively high earnings anyways even if they did not achieve college education, due to the effects of social networks and cultural advantages that accrue to individuals of advantaged families. The sociological literature on the effects of family background on educational attainment that undergird this hypothesis of negative selection bias is enormous (e.g., Breen and Goldthorpe 1997; Buchmann and DiPrete 2006; Lucas 2001; Morgan 2005; Raftery and Hout 1993). Brand and Xie (2010) did find evidence of negative social selection in the United States, though they select on observables, as mentioned previously.

On the other hand, educational decisions of pupils and families, such as what secondary education tracks to apply for and whether to go to college, can be viewed as rational decisions based on the evaluation of the monetary costs and benefits of education, which are unique to each individual based on his or her endowments, such as ability and effort (Carneiro, Hansen, and Heckman 2003; Carneiro and Heckman 2002; Carneiro, Heckman, and Vytlacil 2011; Heckman, Urzua, and Vytlacil 2006; Heckman and Vytlacil 1999, 2000, 2005). In this rational choice view, individuals who would benefit the most from college education are most likely to attend college, while those who determine that the benefits of college, when subtracting out the cost of tuition and opportunity costs, would not substantially increase their life prospects are less likely to attend. According to this hypothesis of positive selection bias, the Mincerian coefficients are biased estimates of returns to education, because the coefficient for schooling assumes that all members of the population would benefit by that degree, while in fact individuals who did not decide to attend college determined on the basis of unobservables that they would benefit to a lesser degree. In other words, the average person who indeed attains college education should have higher earnings returns to college than the marginal student who is ambivalent between going or not. An important implication of the positive selection hypothesis is that policy efforts to expand higher education systems are not necessarily profitable, since the addition of each additional student would lead to incrementally smaller private returns.

There is another line of reasoning giving credence to the positive selection hypothesis. Kerckhoff (1976, 1977, 2001), building on the distinction between 'contest' and 'sponsored' mobility proposed earlier by Turner (1960), argued that proponents of the social psychological model of educational attainment did not pay adequate attention to the structural constraints that individuals take into account when making important decisions about their future educational and occupational goals. What Kerckhoff repeatedly argued is that in highly stratified educational systems – such as those we are focusing on in this paper – students are more likely to make more "realistic" decisions about their educational and life prospects compared to students in less stratified systems. The reason is that given the more frequent or more life-consequential high-stakes testing that occurs in such stratified educational systems, students receive more feedback about their abilities, other endowments and prospects than in school systems like those in the United States with a 'contest' type of mobility, where students are allowed more space to explore and dream about "unrealistic" life trajectories.

Methodology

Following Heckman, Urzua, and Vytlacil (2006), we estimate heterogeneous treatment effects of college education on earnings by considering two groups of factors affecting college attendance: observed attributes due to family background (i.e., inequality of educational opportunities due to ascribed characteristics), and unobserved individual heterogeneity (i.e., a general concept encompassing such factors as ability, aspiration, effort and other endowments). We estimate a selection model that consists of two equations: (1) an earnings outcome equation; and (2) a selection equation predicting the individual's "propensity score" of receiving college education. The two equations can be expressed as

$$Y_i = \beta_i D_i + \gamma X_i + U_i, \qquad (1)$$

$$P_i(Z_i) = \text{Prob}(D_i = 1) = F(Z_i\delta), \qquad (2)$$

where Y_i is earnings in the logarithm form; i (= 1, …, n) is the subscript denoting person i; D_i is an *endogenous* dummy variable denoting whether or not person i

attended college (D_i = 1 if yes; D_i = 0 if no); βi represents the *heterogeneous* return to college attendance, after controlling for the effects of X_i, a vector of other earnings determinants including constant, year of survey, and work experience; γ is a vector of coefficients for X_i; U_i is the disturbance component of log earnings which includes such unobserved factors as ability, effort, and market luck; Z_i is a vector of observed exogenous covariates including gender (though we also run the model for each gender separately as well), father's education, mother's education, father's occupational status, and the instrumental variable (number of siblings); $P_i(Z_i)$ is the person's propensity score of receiving college education; δ is a vector of coefficients for Z_i; F is an inverse link function that transforms the index function, $Z_i\delta$, into a probability. Note that the following decision rule is used to predict binary selection into college:

$$D_i = 1 \text{ if } D_i^* > 0; \; D_i = 0 \text{ otherwise,}$$

$$D_i^* = P_i(Z_i) - U_{Di} \qquad (3)$$

where D^*_i is an unobserved latent variable indicating the net gain to person i from receiving college education; U_{Di} is the unobserved individual-specific component in the selection equation.

Within this framework, $P_i(Z_i)$ and U_{Di} in the schooling selection equation may be interpreted as observed and unobserved costs of education, respectively. The higher the propensity score $P_i(Z_i)$, the more advantaged the family background, thus the lower the observed costs of education, and the larger the person's educational opportunity. By contrast, the larger the unobserved individual heterogeneity U_{Di}, the larger the unobserved costs of education, and the less likely it is that the person receives college education. If $P_i(Z_i) = U_{Di}$, then person i is assumed to be indifferent between going to college or not.

We use the software developed by Heckman, Urzua, and Vytlacil (2006) to estimate treatment effects of college education on earnings at different levels of U_D – i.e., at different latent levels of resistance to college education. This is called the "marginal treatment effect," or MTE. Under both parametric and semiparametric-LIV approaches, all treatment parameters of concern can be identified by using weighted averages of the MTE. In other words, once MTE is known, we can estimate the weighted average treatment effect either for the

entire population or for a subpopulation. When the effect is averaged for the entire population, it is called the average treatment effect (ATE). When the effect is averaged for the subpopulation of those who attended college, it is called the average treatment effect of the treated (TT). Likewise, when the effect is averaged for the subpopulation of those who did not attend college, it is called the average treatment effect of the untreated (TUT).

Finally, will decompose the conventional bias (i.e., the difference in magnitude between OLS and ATE estimators; the OLS estimator is a Mincerian-type coefficient using our dependent and explanatory variables) into two components: the selection bias (i.e., the mean bias of selection on observed characteristics in the absence of college education) and the sorting gain (i.e., the average additional college premium for persons who attend college relative to that for persons who do not attend college). More important, if TT is greater than TUT, then there is a positive selection (or a sorting gain); conversely, if TT is smaller than TUT, then there is a negative selection (or a sorting loss).

Data

To our knowledge, very few scholars have applied the Heckman LIV heterogeneous treatment effects model to returns to education in a subset of European countries, including those examined here. For example, Flossmann and Pohlmeier (2007) survey empirical evidence of causal returns to education in Germany, but the studies reviewed using the IV-approach provide only average returns, and while in their own analysis they find positive sorting gain, but under the ignorability assumption. While they note the importance of the LIV approach (in the model used here), they do not report any studies on Germany that have used it, citing the non-existence of sufficient data sources. To our knowledge, Moffitt (2009) provides the only marginal treatment effect estimates for higher education in the UK.

Indeed, the key reason for the paucity of literature is the difficulty and large data constraints of the statistical model, which requires a minimum sample size of about 2600 young respondents per country with complete and detailed information on personal income, education, parental education and occupation,

work experience, as well as an instrumental variable that may affect college attainment but does not directly affect future earnings. In addition, these variables ought to be standardized across countries.

In our assessment, the data source that best satisfies these criteria is the EU-SILC survey (EU Statistics on Income and Living Conditions), which is a survey instrument that collects cross-sectional and longitudinal microdata on income, poverty, social exclusion and living conditions across EU countries. The Czech Republic, Poland, and Slovakia began participating in EU-SILC in 2005, joining Germany, Austria and other EU-15 countries at that time. The core of the instrument is the measurement of income (often both personal and household) at a very detailed level. Importantly, the EU-SILC surveys in 2005 and 2011 contain an ad-hoc module on the intergenerational transfer of poverty, which included information on family background (father's and mother's education and occupation, as well as the number of siblings when young), without which the analysis would not be possible. Because the 2005 and 2011 datasets have insufficient sample size independently, we pool the two datasets together and include in the model a dummy variable for survey year.

Similar to previous studies of selection bias in returns to education, we restrict the sample to younger respondents aged 28-38 years old inclusive, with non-zero income, and with complete information on parental background and other required variables. The achieved sample size for each country is listed in Table 1.

Education. The key treatment variable in our study is "college," which is a dichotomous variable indicated by a dummy variable = 1 if the highest level of education attained is any kind of tertiary education (unfortunately, more detailed information is not available across both time periods, because data for 2005 does not differentiate BA and higher degrees), 0 if no tertiary education was attained. This may appear crude, but is the only possible measure available. It is also important to keep in mind that a dummy variable of this kind, indicating the fulfillment of a key educational qualification valued on the labor market, is advantageous over the construction of a continuous variable (estimated from the highest level of education attained), due to the major qualitative differences between educational programs in stratified educational systems that last the

same number of years but lead to different qualifications.

Earnings. EU-SILC asks detailed information about wage and non-wage earnings and employment conditions for heads of households and their spouses or partners. For this analysis we calculate the natural log of personal monthly gross earnings from employment and self-employment sources only. Average monthly earnings are based on information about the number of months the respondent worked in the last year. EU-SILC questions on earnings are not truncated or employ categorical responses; rather, to reduce measurement error, respondents are often encouraged to provide detailed information from pay slips and other personal records containing factual earnings data.

Parental education. The 2005 and 2011 waves are the only EU-SILC surveys to include questions on parental education and occupation. Because of concerns about between-country differences in the response categories used, we decided to recode information on parental education into dichotomous dummy variables indicating whether the father (variable *faedu*) and mother (variable *maedu*) attained college education = 1 or not = 0.

Fathers' occupation. We also include in our model indicators of father's occupational status, based on single-digit ISCO response categories. We employ two dummy variables, *Dfaclass1* (indicating employment in a service-sector) and *Dfaclass2* (indicating employment in a blue-collar job), with the reference category being military occupations and respondents with missing data.

Gender is coded as 1=female, 0 = male.

Survey year is a dummy variable coded as 2011=1, 2005=0.

Work experience. Some countries in EU-SILC, including all of the ones here, include a direct measure of years of work experience, which we view as superior to the ad hoc calculation of Mincerian experience. Work experience is measured as a continuous variable of years worked.

Instrument. The instrumental variable for this analysis is number of siblings the respondent had at the age of 14, which is possible to calculate based on slightly different questions in the 2005 and 2011 surveys. We chose this instrument because it is one of the few possible candidates in the dataset that are correlated with the treatment variable – number of siblings is known to be negatively correlated with educational attainment (Blake 1989; Hanushek 1992),

while not affecting personal earnings (e.g. Kessler 1991).

Table 1 provides descriptive statistics for all six countries. As can be observed, the percent of college respondents in the sample generally matched official statistics for these countries, though not in an exact way. The degree of expansion of tertiary education over the course of a generation can be observed by comparing the percent of parents with college education with that of the respondents. In that regard, the Czech Republic has done relatively little compared to its neighbors to expand opportunities for college education. Table 1 also indicates that the sample has a larger representation of men due to the fact that men are more likely than women to have non-zero income. The sample size for Austria is relatively small, and as a result, it was not possible to run the model for Austrian women only, as we got a fatal error due to insufficient sample size. For similar reasons, we were unable to include a number of other European countries originally planned in our research, such as Scandinavian ones, due to insufficient sample size for those countries.

(Table 1 here)

Results

We first estimate using a probit model the propensity of receiving college education for every observation in the sample for men and women separately in each country (Table 2). Recall that the higher the propensity score P(Z), the more inducement to college education because of explanatory variable Z. We can observe in table 2 that the variables representing father's and mother's education have coefficients in the propensity score model indicating a lower degree of inducement into college (compared with fathers and mothers with college education, which is the reference category), consistent with the literature on the sociology of education. In addition, in Figures 1-6 we also graph the distribution of the propensity score for both the treated group and the untreated group, separately for men, women and together. As can be inferred from the figures, the distribution of the propensity score for the treated and untreated groups overlap considerably.

(Table 2 here)

(Figures 1-6 here)

We report our main results by providing parametric and semiparametric estimates of the weighted average marginal treatment effects for specific populations: the average treatment effect (ATE) for the entire population, for the subpopulation of those who attended college (the average treatment effect of the treated, or TT), and for the subpopulation that did not attend college (the average treatment effect of the untreated, or TUT). We present these results for each country for both sexes (table 3), as well as for women (table 4) and for men (table 5).

We present both parametric and semiparametric estimates due to the sensitivity of the estimates to the different assumptions each approach utilizes. The parametric approach estimates the marginal treatment effect under the assumption of a joint trivariate normal distribution for errors in a switching regression setup – the two error terms in the earnings equation under the two treatment regimes, and the error term in selection. The preferred semiparametric approach presented in Carneiro, Heckman, and Vytlacil (2011) does not invoke this assumption, and capitalizes on the fact that the expected value of Y in the earnings function depends on the propensity score P(Z) so that P(Z) serves as a local instrumental variable (LIV). In our results, semiparametric and parametric estimates are generally consistent in terms of indicating diminishing marginal returns to education (though the slope of the estimates can differ considerably), but with larger standard errors in the semiparametric estimates.

Tables 3-5 report estimates of various returns to college for parametric and semiparametric selection models for ATE, TT, and TUT, as well as the local IV estimates and conventional OLS estimates from the same data. As can be observed, OLS estimates for women are higher than men in most countries (because women without a college degree have disproportionately lower earnings compared to men in the same situation), except for Slovakia, where the estimates are slightly larger for men. A bootstrapping method was used to carry out tests of significance for differences between any pair of parameter estimates of concern. Most importantly, the tables also report and decompose conventional

selection bias, termed simply "bias" (calculated as OLS–ATE) into two components: the estimates of ability bias (OLS–TT) and the sorting gain (= TT–ATE).

(Tables 3, 4 and 5 here)

There are a number of important findings that we can infer from these results. First, for most countries and for both genders, the estimates for returns to college exhibit positive selection, in that the returns to college for those who indeed went to college (TT) are much larger than the average treatment effect for the overall population (ATE), and larger still for those who did not go to college (TUT). For example, in the pooled sample of Austrian men and women together, the estimated marginal returns for those who went to college is 1.132, compared to .685 for those who did not. With reference to the average treatment effect, there is a quite substantial sorting gain of .32. The overall observation of positive self-selection holds, generally speaking, for both parametric and semiparametric approaches.

In all countries and in both genders, the difference in the marginal returns to education among those who went to college (TT) are much larger than the corresponding OLS estimates, indicating a very strong bias on unobservables, such as ability, effort and educational aspirations. This ability bias indicates that students with the highest aptitude do indeed go onto college and due to those endowments end up earning considerably more than Mincer-type OLS estimates would have us believe. The size of these differences in the countries examined underscores that the Mincer coefficient is not informative on the economic returns to schooling that those who go to college actually achieve, and thus should also not be used as a factor in educational and labor market policies.

To better view the degree to which the returns for those who go to college are different from the average marginal rates of return (TT-ATE), this sorting gain (or loss) is depicted in Figure 7 by country, gender and approach. While sorting gains are prevalent, a small negative selection bias is apparent in the parametric approach for British data. The difference, however, is very small and not statistically significant, and therefore should be interpreted to suggest that,

in our model, there are practically no differences between the returns to college between those who indeed attained or failed to attain college education. Similarly, there seems to be practically no difference in returns to college between Slovak men who went or did not go to college. Further, positive selection appears to be most prevalent among both sexes in Austria and the Czech Republic, which also happen to be the countries with the least expansive tertiary education systems, as only 26% of young adults in both countries have attained college education.

(Figure 7 here)

Lastly, figures 8-13 graph, as a function of unobserved heterogeneity (U_D), the estimated weights from our data from semiparametric models for the average treatment effect (ATE), the average treatment of the treated (TT), and the average treatment effect of the untreated (TUT). ATE averages the marginal treatment effect evenly, and is thus constant. Note that smaller coefficients of unobserved heterogeneity indicate high propensities for attending college, while large coefficients mean the opposite. What is important to observe is that the slope of TT at low values of U_D are particularly steep for both genders in the Czech Republic and Austria, meaning that in both countries men and women with very high ability are strongly sorted into college, while the download slope is a bit less steep in the other countries. Another perspective, however, is that while the Austrian and Czech educational systems appear to efficiently sort students into college by ability, educational opportunity is not widespread: in these countries there are many individuals that did not go to college despite having high propensities for college education. These educational systems appear significantly supply-constrained, in that it is not low ability, but rather high marginal costs of schooling, that limits their returns to education.

(Figures 8-13 here)

Conclusion

In this paper we have undertaken the challenging enterprise of applying an instrumental-variable method for heterogeneous treatment effects from

unobservables in observational data in EU-SILC countries with similarly stratified educational systems. We find strong evidence in support for positive self-section into college due to those unobservables, which produce marginal rates of return for those with any college education than would be expected in a Mincer-type model taking into account family background. In nearly all conditions, ability bias is large and significant, with the implication that the OLS model does not provide realistic estimates of the returns that those with a college education actually achieve. In conditions of essential heterogeneity, individuals with different endowments have different returns, which we bring to stark relief by presenting weights for marginal treatment effects for those who went to college and those who did not.

The disadvantage of our approach is that we rely on an instrumental variable, number of siblings, which is assumed to be associated with college education (which we show is the case in Table 2) but only indirectly, not directly, associated with earnings indirectly through education. While these associations can be externally confirmed, they are not verifiable within the framework of the model. Assuming the validity of our instrument, we show that in most empirical conditions examined there are substantial heterogeneous treatment effects due to unobservables between those who have attained a college education degree and those who have not. When weighing the advantages and disadvantages of different approaches to returns to education, we still believe that our semiparametric local instrumental variable approach is superior in terms of identifying heterogeneous treatment effects due to unobservables and conditional on observables, than other models used in the literature than invoke the ignorability assumption.

We view this paper as the start of an even larger undertaking of applying the model to all EU-SILC countries that satisfy the data requirements. The benefit of a larger sample of countries will be the ability to compare heterogeneous treatment effects across heterogeneous educational regimes, and thus the possibility of assessing what cross-national patterns may exist in the effects of unobservables on marginal returns to education in Europe.

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Table 1. Descriptive Statistics by Country

Variable	Austria	Czech Republic	Germany	Poland	Slovak Republic	U. K.
College-educated (%)	26.26	18.88	44.50	30.95	24.72	46.06
Income	2497.15	759.29	2453.47	557.63	554.79	2966.53
	(1686.95)	(567.76)	(1591.94)	(540.84)	(461.97)	(4438.30)
LN(Income)	7.62	6.44	7.58	6.04	6.11	7.70
	(.70)	(.64)	(.78)	(.79)	(.68)	(.81)
Age	33.51	33.26	33.84	32.96	32.83	33.45
	(3.12)	(3.14)	(3.15)	(3.18)	(3.18)	(3.11)
Work experience, years	13.65	11.46	12.49	10.25	11.50	13.11[a]
	(4.95)	(4.28)	(5.07)	(4.83)	(4.63)	(4.46)
Number of Siblings	1.54	1.21	1.13	1.67	1.50	1.46
	(1.45)	(.87)	(1.06)	(1.55)	(1.10)	(1.78)
Survey year (%)						
2011	47.48	57.21	47.65	44.22	51.68	33.41
2005	52.52	42.79	52.35	55.78	48.32	66.59
Gender (%)						
Men	55.75	54.11	50.06	54.11	53.90	49.50
Women	44.25	45.89	49.94	45.35	46.10	50.50
Father's education (%)						
Primary and lower	36.45	35.16	5.60	26.73	14.41	40.90
Secondary	47.94	46.03	47.95	58.93	68.99	18.60
College and higher	10.23	10.28	32.70	8.24	11.05	14.52
Missing	5.37	8.52	13.75	6.11	5.55	25.98
Mother's education (%)						
Primary and lower	38.42	39.92	14.81	30.65	22.00	56.38
Secondary	36.54	52.48	59.18	58.68	69.76	9.91
College and higher	4.74	6.21	16.25	7.54	6.79	13.76
Missing	20.30	1.38	9.77	3.13	1.45	19.94
Father's Occupation (%)						
Non-manual	40.90	30.30	46.66	21.38	30.71	44.75
Manual	52.35	58.45	40.20	64.18	58.80	23.09
Armed forces	.38	1.04	.49	1.56	.56	.32
Missing	6.38	10.21	12.66	12.88	9.92	31.84
Sample size (N)	2,384	2,898	4,321	8,026	3,386	3,430

Note: Numbers in parentheses are standard errors;
[a] This statistics is derived from information provided by 2011 respondents; the 2005 respondents did not provide information on this variable.

Introduction

For decades, the study of economic returns to education has been a cornerstone of research on the accumulation and distribution of human capital and its impact on economic growth. Within the human capital framework, Mincer (1974) developed a widely accepted earnings equation to empirically estimate the rate of returns to schooling, under the assumption of homogeneity. The Mincer equation – which uses OLS regression with logged earnings as the dependent variable and years of schooling as a primary independent variable, along with a separable quadratic function for work experience – was regarded as "one of the great success stories of modern labor economics" (Willis 1986: 526). Studies of Mincer returns have tracked wage premiums by educational level, over time, between countries and regions, by gender, and have been cited by scholars thousands of times (e.g. Psacharopoulos 1973, 1985, 1994; Psacharopoulos & Patrinos 2004). In the European Union, where national differences in labor market conditions are large despite the single market, Mincer rates of return are still widely used in policy debates on equity and efficacy of investments in education (European Commission 2010; Glocker & Steiner 2011).

While there have been various attempts to address the endogeneity problem of education in earnings function, the validity of the overall Mincerian endeavor has been cast into serious doubt by Heckman, Lochner, and Todd (2006), who rejected the Mincer model on both theoretical and methodological grounds. At issue in that critique was that the commonly estimated Mincer rates of return using survey data may be different from the ''true'' causal effect under a hypothetical scenario of random assignments. As is well-established in the social stratification literature, education is itself an attained characteristic that is unevenly distributed by social class, ability, and other factors. Because of this, there may be selection bias in terms of the kinds of people who select (or are selected) into college – based on their observable and unobservable characteristics – that enable them to earn more in the labor market. From this view, the typical person who goes to college chooses to do so because of the economic benefits the degree will bring due to his/her higher unobserved endowments, such as ability and effort, whereas others with different

endowments do not perceive the same economic returns to college (Card 1999). This is called "ability bias" or, simply, "selection bias." As Heckman (2001) pointed out, the presence of individual heterogeneity and self-selection gives rise to a sorting gain, which biases the standard estimator for the causal effect of schooling with observational data.

Contemporary research on the causal impact of higher education on earnings has identified two major assumptions that generate selection bias (Heckman, Lochner, and Todd 2006). Neither of these assumptions, which are taken for granted in Mincer-type analyses, may hold true: (1) that the effect of education is homogeneous across different members of a population of individuals with the same observed attributes; and (2) the ignorability assumption, or selection on observables, which assumes that the distribution of college education in a population is random after controlling for relevant observed covariates. The first "homogeneity assumption" is highly implausible in real world conditions because the earnings benefit different people would receive by the treatment condition can vary greatly due to the "essential heterogeneity" (Heckman, Urzua, and Vytlacil 2006) of an individual's life condition. The second ignorability assumption (Morgan and Winship 2007) is also problematic due to the role of ability or family background in systematically affecting an individual's chances of receiving the treatment condition.

Much previous work in sociology on heterogeneous treatment effects (HTE) of higher education (e.g., Brand and Xie 2010; Tsai and Xie 2008) has relied on the ignorability assumption, i.e., it is assumed that those who attend college and those who do not are not systematically different, conditional on observed covariates. In this paper, we relax that assumption by using the newly developed instrumental-variable method for HTE in economics developed by Heckman, Urzua, and Vytlacil (2006). This new method was also used in Tsai and Xie (2011), who found heterogeneous treatment effects of higher education on earnings in Taiwan; their empirical results revealed distinct patterns by gender, with positive selection bias most clearly shown among women rather than men.

In this paper we also adopt the approach of Heckman, Urzua, and Vytlacil (2006) to measure heterogeneous treatment effects of higher education on earnings. The empirical context of our study is Central Europe – Germany,

Austria, Poland, Slovakia and the Czech Republic, along with the United Kingdom as an additional point of reference. Though we have the ambition of analyzing all European countries possible within the EU-SILC data, we begin our analysis with Central European countries because of the similarity of their educational systems, in addition to having similarly open and integrated economies and labor markets. After overviewing those countries, the paper then explains the methods and data in more detail, before presenting the results of our study.

Empirical Context

While it would be wonderful to focus on a large set of European countries, the substantial computational demands of our methodological approach requires that we begin with a smaller number of countries. The benefit of this, however, is that we can control for institutional factors at the country level. The countries under study neighbor each other, have strong trade inter-linkages and thus experience similar business cycles, and share similarities in labor market flexibility and the female labor participation (about 55% in Germany and Austria, and 50% in Poland, Slovakia and the Czech Republic). Male industrial employment is also high in all of these countries, ranging from 39% in Austria to 51% in Slovakia (World Bank Indicators 2013). Besides variance in unemployment rates across the countries, there are many similarities in their macroeconomic conditions (Eurostat 2013).

Educational systems structure the educational opportunities of pupils and the ways families make key educational and labor market decisions (Buchmann & Dalton 2002). Sociologists have for decades compared the degree of stratification of educational systems, and have commonly grouped the countries in this study as sharing the characteristics of early tracking and a high degree of institutional stratification and vocational specificity (Shavit & Müller, 1998; Allmendinger 1989; Kerckhoff 2001). For example, Matějů et al (2007) recently compared key indicators of secondary educational systems across OECD countries, such as the number of school types, and enrollment in vocational and academic programs, as well as indicators of tertiary education, such as net enrollment rates, private and public spending, and financial aid. What they found was that, across OECD countries, indicators of the stratification and vocational

specificity of secondary and tertiary education are correlated with each other, as well as that all of the countries in this study fall within the category of having highly stratified educational systems.

In terms of specifics, already at the end of primary education (in 4th to 6th grade, depending on the country), pupils in Germany, Austria, Slovakia and the Czech Republic take high-stakes tests to determine how they will be sorted into one of three different educational tracks. They can apply to highly selective grammar schools (*gymnasium*), which provide pupils with the best credentials and training for higher education, as well as to less academic schools at the lower secondary level. In Austria, Czech Republic and Slovakia, the alternative to long-form (typically 8-year) grammar schools are to continue studies in basic schools or transfer to lower vocational schools. In Germany, in addition to grammar schools, pupils can also apply to a *Realschule* (an intermediate track covering the 5th to 9th grades), or *Hauptschule* (the lower track), the latter of which provides basic general education and usually covers the 5th to 9th grades. In Poland, all pupils in lower secondary education attend a grammar school, though pupils must apply to them on a competitive basis at about the age of 12 and there are major differences between schools in quality and prestige.

In Austria, Czech Republic and Slovakia, pupils who did not succeed in attending the long-form grammar schools can apply again to grammar schools that cover upper secondary education, or apply to upper technical schools or apprenticeship programs, the latter provide the least likely chances of attending higher education. Similarly, after completing a grammar school in Poland at the lower secondary level, pupils apply at the upper secondary level to attend the more prestigious *liceum*, which prepare pupils the most for higher education, or technical schools that provide more vocationally-specific education. In Germany, pupils generally attend the same school through lower and upper secondary education; pupils attending the *Hauptschule* and *Realschule* often complete their education with vocational and apprenticeship programs that provide direct linkages to the labor market, often in the form of on-the-job training. In all countries, opportunities for university attendance depend greatly on the type of secondary school attended and the credentials achieved in secondary education.

At the level of tertiary education, these countries have not experienced

the same degree of educational expansion as in, for example, the United Kingdom. In the latter, over 47% of males and females aged 30-34 have attained tertiary education, compared to only 39% in Poland, 32% in Germany, 26% in Austria and the Czech Republic, and 24% in Slovakia (Eurostat 2013). Analyses of Mincerian returns to education in the countries under study have not found that educational expansion dilutes the economic value of education. In Germany, for example, data from the German Socio-Economic Panel suggests that returns to education declined until the late 1990s and increased significantly afterwards, even as educational expansion continued (Gebel & Pfeiffer 2007). In post-communist Poland, Slovakia and the Czech Republic, returns to education increased rapidly with the introduction of the market economy even as their tertiary educational systems also expanded rapidly (Filer, Jurajda & Planovsky 1999).

Theoretical considerations

These similarities in educational systems and in socio-economic context inform the theoretical considerations of this study. Recall that human capital theory posits that schooling causally affects earnings positively, since it raises workers' skills and productivity, and thus their value on the labor market (Becker 1964; Blau and Duncan 1967; Mincer 1974). However, alternative views of the school-earnings link is that it is not necessarily causal, since a part of the purported effect of schooling on earnings may be spurious as a result of other factors that drive how different people are selected into college. There are in fact two sources of selection at work: social selection due to family background effects, and self-selection (or ability bias), due to the effect of unobservables at the individual level.

On the one hand, analyses of Mincerian returns to college are blind to the different propensities of people to achieve college education based on social background. The Mincerian model provides biased average returns to schooling because, for example, the observed sample of those who attained college education could be itself a product of selection by socio-economic status of the background family, such that reported coefficients may not estimate the economic return of the educational investment, but rather the "return" of the

intergenerational reproduction of social status from parents to children. Individuals from socio-economically advantaged backgrounds are the most likely to attain college education and earn more on the labor market, but it is likely that they would have relatively high earnings anyways even if they did not achieve college education, due to the effects of social networks and cultural advantages that accrue to individuals of advantaged families. The sociological literature on the effects of family background on educational attainment that undergird this hypothesis of negative selection bias is enormous (e.g., Breen and Goldthorpe 1997; Buchmann and DiPrete 2006; Lucas 2001; Morgan 2005; Raftery and Hout 1993). Brand and Xie (2010) did find evidence of negative social selection in the United States, though they select on observables, as mentioned previously.

On the other hand, educational decisions of pupils and families, such as what secondary education tracks to apply for and whether to go to college, can be viewed as rational decisions based on the evaluation of the monetary costs and benefits of education, which are unique to each individual based on his or her endowments, such as ability and effort (Carneiro, Hansen, and Heckman 2003; Carneiro and Heckman 2002; Carneiro, Heckman, and Vytlacil 2011; Heckman, Urzua, and Vytlacil 2006; Heckman and Vytlacil 1999, 2000, 2005). In this rational choice view, individuals who would benefit the most from college education are most likely to attend college, while those who determine that the benefits of college, when subtracting out the cost of tuition and opportunity costs, would not substantially increase their life prospects are less likely to attend. According to this hypothesis of positive selection bias, the Mincerian coefficients are biased estimates of returns to education, because the coefficient for schooling assumes that all members of the population would benefit by that degree, while in fact individuals who did not decide to attend college determined on the basis of unobservables that they would benefit to a lesser degree. In other words, the average person who indeed attains college education should have higher earnings returns to college than the marginal student who is ambivalent between going or not. An important implication of the positive selection hypothesis is that policy efforts to expand higher education systems are not necessarily profitable, since the addition of each additional student would lead to incrementally smaller private returns.

There is another line of reasoning giving credence to the positive selection hypothesis. Kerckhoff (1976, 1977, 2001), building on the distinction between 'contest' and 'sponsored' mobility proposed earlier by Turner (1960), argued that proponents of the social psychological model of educational attainment did not pay adequate attention to the structural constraints that individuals take into account when making important decisions about their future educational and occupational goals. What Kerckhoff repeatedly argued is that in highly stratified educational systems – such as those we are focusing on in this paper – students are more likely to make more "realistic" decisions about their educational and life prospects compared to students in less stratified systems. The reason is that given the more frequent or more life-consequential high-stakes testing that occurs in such stratified educational systems, students receive more feedback about their abilities, other endowments and prospects than in school systems like those in the United States with a 'contest' type of mobility, where students are allowed more space to explore and dream about "unrealistic" life trajectories.

Methodology

Following Heckman, Urzua, and Vytlacil (2006), we estimate heterogeneous treatment effects of college education on earnings by considering two groups of factors affecting college attendance: observed attributes due to family background (i.e., inequality of educational opportunities due to ascribed characteristics), and unobserved individual heterogeneity (i.e., a general concept encompassing such factors as ability, aspiration, effort and other endowments). We estimate a selection model that consists of two equations: (1) an earnings outcome equation; and (2) a selection equation predicting the individual's "propensity score" of receiving college education. The two equations can be expressed as

$$Y_i = \beta_i D_i + \gamma X_i + U_i, \qquad (1)$$

$$P_i(Z_i) = \text{Prob}(D_i = 1) = F(Z_i\delta), \qquad (2)$$

where Y_i is earnings in the logarithm form; i (= 1, …, n) is the subscript denoting person i; D_i is an *endogenous* dummy variable denoting whether or not person i

attended college (D_i = 1 if yes; D_i = 0 if no); βi represents the *heterogeneous* return to college attendance, after controlling for the effects of X_i, a vector of other earnings determinants including constant, year of survey, and work experience; γ is a vector of coefficients for X_i; U_i is the disturbance component of log earnings which includes such unobserved factors as ability, effort, and market luck; Z_i is a vector of observed exogenous covariates including gender (though we also run the model for each gender separately as well), father's education, mother's education, father's occupational status, and the instrumental variable (number of siblings); $P_i(Z_i)$ is the person's propensity score of receiving college education; δ is a vector of coefficients for Z_i; F is an inverse link function that transforms the index function, $Z_i\delta$, into a probability. Note that the following decision rule is used to predict binary selection into college:

$$D_i = 1 \text{ if } D_i^* > 0; \; D_i = 0 \text{ otherwise,}$$

$$D_i^* = P_i(Z_i) - U_{Di} \qquad (3)$$

where D^*_i is an unobserved latent variable indicating the net gain to person i from receiving college education; U_{Di} is the unobserved individual-specific component in the selection equation.

Within this framework, $P_i(Z_i)$ and U_{Di} in the schooling selection equation may be interpreted as observed and unobserved costs of education, respectively. The higher the propensity score $P_i(Z_i)$, the more advantaged the family background, thus the lower the observed costs of education, and the larger the person's educational opportunity. By contrast, the larger the unobserved individual heterogeneity U_{Di}, the larger the unobserved costs of education, and the less likely it is that the person receives college education. If $P_i(Z_i) = U_{Di}$, then person i is assumed to be indifferent between going to college or not.

We use the software developed by Heckman, Urzua, and Vytlacil (2006) to estimate treatment effects of college education on earnings at different levels of U_D – i.e., at different latent levels of resistance to college education. This is called the "marginal treatment effect," or MTE. Under both parametric and semiparametric-LIV approaches, all treatment parameters of concern can be identified by using weighted averages of the MTE. In other words, once MTE is known, we can estimate the weighted average treatment effect either for the

entire population or for a subpopulation. When the effect is averaged for the entire population, it is called the average treatment effect (ATE). When the effect is averaged for the subpopulation of those who attended college, it is called the average treatment effect of the treated (TT). Likewise, when the effect is averaged for the subpopulation of those who did not attend college, it is called the average treatment effect of the untreated (TUT).

Finally, will decompose the conventional bias (i.e., the difference in magnitude between OLS and ATE estimators; the OLS estimator is a Mincerian-type coefficient using our dependent and explanatory variables) into two components: the selection bias (i.e., the mean bias of selection on observed characteristics in the absence of college education) and the sorting gain (i.e., the average additional college premium for persons who attend college relative to that for persons who do not attend college). More important, if TT is greater than TUT, then there is a positive selection (or a sorting gain); conversely, if TT is smaller than TUT, then there is a negative selection (or a sorting loss).

Data

To our knowledge, very few scholars have applied the Heckman LIV heterogeneous treatment effects model to returns to education in a subset of European countries, including those examined here. For example, Flossmann and Pohlmeier (2007) survey empirical evidence of causal returns to education in Germany, but the studies reviewed using the IV-approach provide only average returns, and while in their own analysis they find positive sorting gain, but under the ignorability assumption. While they note the importance of the LIV approach (in the model used here), they do not report any studies on Germany that have used it, citing the non-existence of sufficient data sources. To our knowledge, Moffitt (2009) provides the only marginal treatment effect estimates for higher education in the UK.

Indeed, the key reason for the paucity of literature is the difficulty and large data constraints of the statistical model, which requires a minimum sample size of about 2600 young respondents per country with complete and detailed information on personal income, education, parental education and occupation,

work experience, as well as an instrumental variable that may affect college attainment but does not directly affect future earnings. In addition, these variables ought to be standardized across countries.

In our assessment, the data source that best satisfies these criteria is the EU-SILC survey (EU Statistics on Income and Living Conditions), which is a survey instrument that collects cross-sectional and longitudinal microdata on income, poverty, social exclusion and living conditions across EU countries. The Czech Republic, Poland, and Slovakia began participating in EU-SILC in 2005, joining Germany, Austria and other EU-15 countries at that time. The core of the instrument is the measurement of income (often both personal and household) at a very detailed level. Importantly, the EU-SILC surveys in 2005 and 2011 contain an ad-hoc module on the intergenerational transfer of poverty, which included information on family background (father's and mother's education and occupation, as well as the number of siblings when young), without which the analysis would not be possible. Because the 2005 and 2011 datasets have insufficient sample size independently, we pool the two datasets together and include in the model a dummy variable for survey year.

Similar to previous studies of selection bias in returns to education, we restrict the sample to younger respondents aged 28-38 years old inclusive, with non-zero income, and with complete information on parental background and other required variables. The achieved sample size for each country is listed in Table 1.

Education. The key treatment variable in our study is "college," which is a dichotomous variable indicated by a dummy variable = 1 if the highest level of education attained is any kind of tertiary education (unfortunately, more detailed information is not available across both time periods, because data for 2005 does not differentiate BA and higher degrees), 0 if no tertiary education was attained. This may appear crude, but is the only possible measure available. It is also important to keep in mind that a dummy variable of this kind, indicating the fulfillment of a key educational qualification valued on the labor market, is advantageous over the construction of a continuous variable (estimated from the highest level of education attained), due to the major qualitative differences between educational programs in stratified educational systems that last the

same number of years but lead to different qualifications.

Earnings. EU-SILC asks detailed information about wage and non-wage earnings and employment conditions for heads of households and their spouses or partners. For this analysis we calculate the natural log of personal monthly gross earnings from employment and self-employment sources only. Average monthly earnings are based on information about the number of months the respondent worked in the last year. EU-SILC questions on earnings are not truncated or employ categorical responses; rather, to reduce measurement error, respondents are often encouraged to provide detailed information from pay slips and other personal records containing factual earnings data.

Parental education. The 2005 and 2011 waves are the only EU-SILC surveys to include questions on parental education and occupation. Because of concerns about between-country differences in the response categories used, we decided to recode information on parental education into dichotomous dummy variables indicating whether the father (variable *faedu*) and mother (variable *maedu*) attained college education = 1 or not = 0.

Fathers' occupation. We also include in our model indicators of father's occupational status, based on single-digit ISCO response categories. We employ two dummy variables, *Dfaclass1* (indicating employment in a service-sector) and *Dfaclass2* (indicating employment in a blue-collar job), with the reference category being military occupations and respondents with missing data.

Gender is coded as 1=female, 0 = male.

Survey year is a dummy variable coded as 2011=1, 2005=0.

Work experience. Some countries in EU-SILC, including all of the ones here, include a direct measure of years of work experience, which we view as superior to the ad hoc calculation of Mincerian experience. Work experience is measured as a continuous variable of years worked.

Instrument. The instrumental variable for this analysis is number of siblings the respondent had at the age of 14, which is possible to calculate based on slightly different questions in the 2005 and 2011 surveys. We chose this instrument because it is one of the few possible candidates in the dataset that are correlated with the treatment variable – number of siblings is known to be negatively correlated with educational attainment (Blake 1989; Hanushek 1992),

while not affecting personal earnings (e.g. Kessler 1991).

Table 1 provides descriptive statistics for all six countries. As can be observed, the percent of college respondents in the sample generally matched official statistics for these countries, though not in an exact way. The degree of expansion of tertiary education over the course of a generation can be observed by comparing the percent of parents with college education with that of the respondents. In that regard, the Czech Republic has done relatively little compared to its neighbors to expand opportunities for college education. Table 1 also indicates that the sample has a larger representation of men due to the fact that men are more likely than women to have non-zero income. The sample size for Austria is relatively small, and as a result, it was not possible to run the model for Austrian women only, as we got a fatal error due to insufficient sample size. For similar reasons, we were unable to include a number of other European countries originally planned in our research, such as Scandinavian ones, due to insufficient sample size for those countries.

(Table 1 here)

Results

We first estimate using a probit model the propensity of receiving college education for every observation in the sample for men and women separately in each country (Table 2). Recall that the higher the propensity score P(Z), the more inducement to college education because of explanatory variable Z. We can observe in table 2 that the variables representing father's and mother's education have coefficients in the propensity score model indicating a lower degree of inducement into college (compared with fathers and mothers with college education, which is the reference category), consistent with the literature on the sociology of education. In addition, in Figures 1-6 we also graph the distribution of the propensity score for both the treated group and the untreated group, separately for men, women and together. As can be inferred from the figures, the distribution of the propensity score for the treated and untreated groups overlap considerably.

(Table 2 here)

(Figures 1-6 here)

We report our main results by providing parametric and semiparametric estimates of the weighted average marginal treatment effects for specific populations: the average treatment effect (ATE) for the entire population, for the subpopulation of those who attended college (the average treatment effect of the treated, or TT), and for the subpopulation that did not attend college (the average treatment effect of the untreated, or TUT). We present these results for each country for both sexes (table 3), as well as for women (table 4) and for men (table 5).

We present both parametric and semiparametric estimates due to the sensitivity of the estimates to the different assumptions each approach utilizes. The parametric approach estimates the marginal treatment effect under the assumption of a joint trivariate normal distribution for errors in a switching regression setup – the two error terms in the earnings equation under the two treatment regimes, and the error term in selection. The preferred semiparametric approach presented in Carneiro, Heckman, and Vytlacil (2011) does not invoke this assumption, and capitalizes on the fact that the expected value of Y in the earnings function depends on the propensity score P(Z) so that P(Z) serves as a local instrumental variable (LIV). In our results, semiparametric and parametric estimates are generally consistent in terms of indicating diminishing marginal returns to education (though the slope of the estimates can differ considerably), but with larger standard errors in the semiparametric estimates.

Tables 3-5 report estimates of various returns to college for parametric and semiparametric selection models for ATE, TT, and TUT, as well as the local IV estimates and conventional OLS estimates from the same data. As can be observed, OLS estimates for women are higher than men in most countries (because women without a college degree have disproportionately lower earnings compared to men in the same situation), except for Slovakia, where the estimates are slightly larger for men. A bootstrapping method was used to carry out tests of significance for differences between any pair of parameter estimates of concern. Most importantly, the tables also report and decompose conventional

selection bias, termed simply "bias" (calculated as OLS–ATE) into two components: the estimates of ability bias (OLS–TT) and the sorting gain (= TT–ATE).

(Tables 3, 4 and 5 here)

There are a number of important findings that we can infer from these results. First, for most countries and for both genders, the estimates for returns to college exhibit positive selection, in that the returns to college for those who indeed went to college (TT) are much larger than the average treatment effect for the overall population (ATE), and larger still for those who did not go to college (TUT). For example, in the pooled sample of Austrian men and women together, the estimated marginal returns for those who went to college is 1.132, compared to .685 for those who did not. With reference to the average treatment effect, there is a quite substantial sorting gain of .32. The overall observation of positive self-selection holds, generally speaking, for both parametric and semiparametric approaches.

In all countries and in both genders, the difference in the marginal returns to education among those who went to college (TT) are much larger than the corresponding OLS estimates, indicating a very strong bias on unobservables, such as ability, effort and educational aspirations. This ability bias indicates that students with the highest aptitude do indeed go onto college and due to those endowments end up earning considerably more than Mincer-type OLS estimates would have us believe. The size of these differences in the countries examined underscores that the Mincer coefficient is not informative on the economic returns to schooling that those who go to college actually achieve, and thus should also not be used as a factor in educational and labor market policies.

To better view the degree to which the returns for those who go to college are different from the average marginal rates of return (TT-ATE), this sorting gain (or loss) is depicted in Figure 7 by country, gender and approach. While sorting gains are prevalent, a small negative selection bias is apparent in the parametric approach for British data. The difference, however, is very small and not statistically significant, and therefore should be interpreted to suggest that,

in our model, there are practically no differences between the returns to college between those who indeed attained or failed to attain college education. Similarly, there seems to be practically no difference in returns to college between Slovak men who went or did not go to college. Further, positive selection appears to be most prevalent among both sexes in Austria and the Czech Republic, which also happen to be the countries with the least expansive tertiary education systems, as only 26% of young adults in both countries have attained college education.

(Figure 7 here)

Lastly, figures 8-13 graph, as a function of unobserved heterogeneity (U_D), the estimated weights from our data from semiparametric models for the average treatment effect (ATE), the average treatment of the treated (TT), and the average treatment effect of the untreated (TUT). ATE averages the marginal treatment effect evenly, and is thus constant. Note that smaller coefficients of unobserved heterogeneity indicate high propensities for attending college, while large coefficients mean the opposite. What is important to observe is that the slope of TT at low values of U_D are particularly steep for both genders in the Czech Republic and Austria, meaning that in both countries men and women with very high ability are strongly sorted into college, while the download slope is a bit less steep in the other countries. Another perspective, however, is that while the Austrian and Czech educational systems appear to efficiently sort students into college by ability, educational opportunity is not widespread: in these countries there are many individuals that did not go to college despite having high propensities for college education. These educational systems appear significantly supply-constrained, in that it is not low ability, but rather high marginal costs of schooling, that limits their returns to education.

(Figures 8-13 here)

Conclusion

In this paper we have undertaken the challenging enterprise of applying an instrumental-variable method for heterogeneous treatment effects from

unobservables in observational data in EU-SILC countries with similarly stratified educational systems. We find strong evidence in support for positive self-section into college due to those unobservables, which produce marginal rates of return for those with any college education than would be expected in a Mincer-type model taking into account family background. In nearly all conditions, ability bias is large and significant, with the implication that the OLS model does not provide realistic estimates of the returns that those with a college education actually achieve. In conditions of essential heterogeneity, individuals with different endowments have different returns, which we bring to stark relief by presenting weights for marginal treatment effects for those who went to college and those who did not.

The disadvantage of our approach is that we rely on an instrumental variable, number of siblings, which is assumed to be associated with college education (which we show is the case in Table 2) but only indirectly, not directly, associated with earnings indirectly through education. While these associations can be externally confirmed, they are not verifiable within the framework of the model. Assuming the validity of our instrument, we show that in most empirical conditions examined there are substantial heterogeneous treatment effects due to unobservables between those who have attained a college education degree and those who have not. When weighing the advantages and disadvantages of different approaches to returns to education, we still believe that our semiparametric local instrumental variable approach is superior in terms of identifying heterogeneous treatment effects due to unobservables and conditional on observables, than other models used in the literature than invoke the ignorability assumption.

We view this paper as the start of an even larger undertaking of applying the model to all EU-SILC countries that satisfy the data requirements. The benefit of a larger sample of countries will be the ability to compare heterogeneous treatment effects across heterogeneous educational regimes, and thus the possibility of assessing what cross-national patterns may exist in the effects of unobservables on marginal returns to education in Europe.

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Table 1. Descriptive Statistics by Country

Variable	Austria	Czech Republic	Germany	Poland	Slovak Republic	U. K.
College-educated (%)	26.26	18.88	44.50	30.95	24.72	46.06
Income	2497.15	759.29	2453.47	557.63	554.79	2966.53
	(1686.95)	(567.76)	(1591.94)	(540.84)	(461.97)	(4438.30)
LN(Income)	7.62	6.44	7.58	6.04	6.11	7.70
	(.70)	(.64)	(.78)	(.79)	(.68)	(.81)
Age	33.51	33.26	33.84	32.96	32.83	33.45
	(3.12)	(3.14)	(3.15)	(3.18)	(3.18)	(3.11)
Work experience, years	13.65	11.46	12.49	10.25	11.50	13.11[a]
	(4.95)	(4.28)	(5.07)	(4.83)	(4.63)	(4.46)
Number of Siblings	1.54	1.21	1.13	1.67	1.50	1.46
	(1.45)	(.87)	(1.06)	(1.55)	(1.10)	(1.78)
Survey year (%)						
2011	47.48	57.21	47.65	44.22	51.68	33.41
2005	52.52	42.79	52.35	55.78	48.32	66.59
Gender (%)						
Men	55.75	54.11	50.06	54.11	53.90	49.50
Women	44.25	45.89	49.94	45.35	46.10	50.50
Father's education (%)						
Primary and lower	36.45	35.16	5.60	26.73	14.41	40.90
Secondary	47.94	46.03	47.95	58.93	68.99	18.60
College and higher	10.23	10.28	32.70	8.24	11.05	14.52
Missing	5.37	8.52	13.75	6.11	5.55	25.98
Mother's education (%)						
Primary and lower	38.42	39.92	14.81	30.65	22.00	56.38
Secondary	36.54	52.48	59.18	58.68	69.76	9.91
College and higher	4.74	6.21	16.25	7.54	6.79	13.76
Missing	20.30	1.38	9.77	3.13	1.45	19.94
Father's Occupation (%)						
Non-manual	40.90	30.30	46.66	21.38	30.71	44.75
Manual	52.35	58.45	40.20	64.18	58.80	23.09
Armed forces	.38	1.04	.49	1.56	.56	.32
Missing	6.38	10.21	12.66	12.88	9.92	31.84
Sample size (N)	2,384	2,898	4,321	8,026	3,386	3,430

Note: Numbers in parentheses are standard errors;
[a] This statistics is derived from information provided by 2011 respondents; the 2005 respondents did not provide information on this variable.

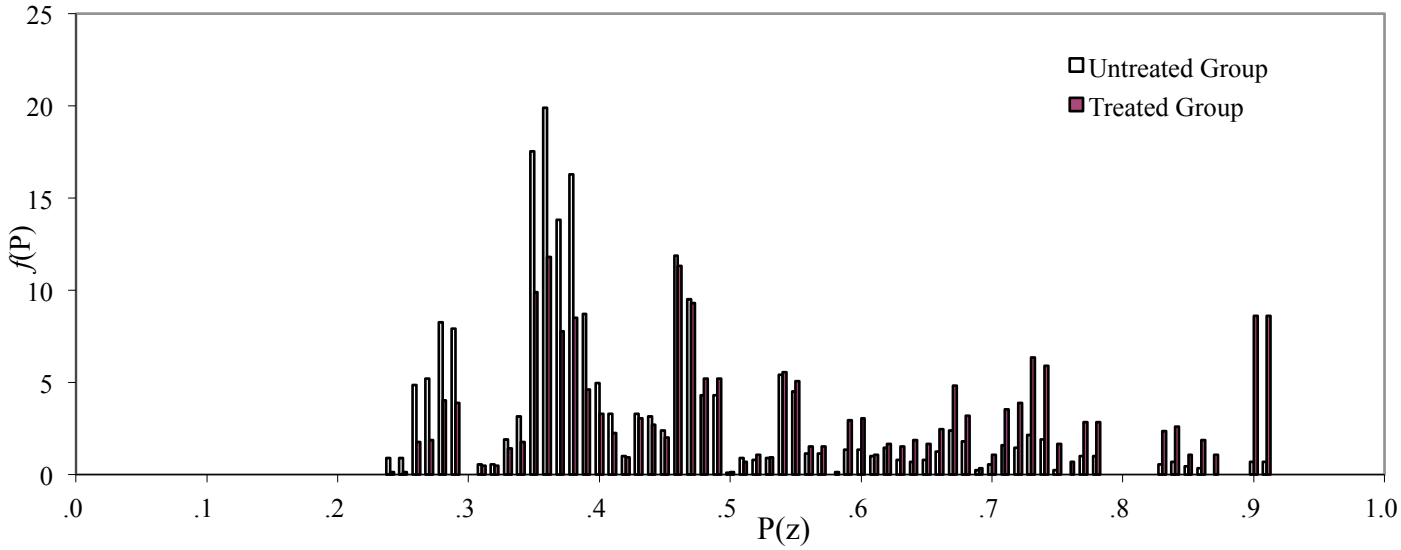
Table 2. Estimates of Probit Models Predicting College Attended

	Austria (N=2,384)		Czech (N=2,898)		Germany (N=4,321)		Poland (N=8,026)		Slovak (N=3,386)		U. K. (N=3,430)	
Independent Variable(Z)	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
Father's Education(Ref: high level)												
Low level	-.571*	-.524*	-.753*	-.072	-.354*	-.632*	-.656*	-.484*	-.620*	-.575*	-.592*	-.736*
	(.148)	(.165)	(.164)	(.176)	(.140)	(.140)	(.118)	(.122)	(.183)	(.183)	(.109)	(.110)
Middle level	-.563*	-.390*	-.794*	-.244	-.460*	-.490*	-.536*	-.387*	-.481*	-.369*	-.475*	-.533*
	(.130)	(.146)	(.132)	(.141)	(.069)	(.071)	(.093)	(.100)	(.119)	(.126)	(.115)	(.121)
Missing	-.707*	-.319	-1.153*	-.705*	-.630*	-.536*	-.705*	-.635*	-.615*	-.690*	-.678*	-.681*
	(.259)	(.309)	(.281)	(.279)	(.108)	(.112)	(.150)	(.160)	(.229)	(.251)	(.119)	(.118)
Mather's Education(Ref: high level)												
Low level	-.903*	-1.183*	-.925*	-1.605*	-.336*	-.758*	-1.087*	-1.225*	-.970*	-.998*	-.354*	-.652*
	(.197)	(.217)	(.174)	(.180)	(.111)	(.110)	(.112)	(.122)	(.170)	(.187)	(.106)	(.109)
Middle level	-.654*	-.845*	-.553*	-1.073*	-.231*	-.455*	-.550*	-.705*	-.498*	-.716*	.001	-.316*
	(.191)	(.207)	(.153)	(.162)	(.084)	(.083)	(.090)	(.105)	(.130)	(.156)	(.137)	(.138)
Missing	-.682*	-1.151*	-.559	-1.115*	-.507*	-.615*	-1.185*	-.820*	-.772*	-.834*	-.582*	-.968*
	(.204)	(.227)	(.375)	(.401)	(.128)	(.131)	(.193)	(.200)	(.344)	(.399)	(.125)	(.127)
Father's Occupation (Ref: Army & Missing)												
Manual	.295	.407	.025	.152	.344*	.329*	.413*	.394*	.193	.294	.094	.212*
	(.216)	(.253)	(.222)	(.225)	(.092)	(.104)	(.082)	(.082)	(.150)	(.166)	(.075)	(.076)
Non-Manual	.104	-.111	-.233	-.453*	.019	.033	-.012	-.073	-.269	-.047	-.249*	-.062
	(.221)	(.255)	(.220)	(.224)	(.093)	(.103)	(.075)	(.073)	(.147)	(.163)	(.089)	(.086)
Numbers of Sibling	-.025	-.078*	-.109*	-.099*	-.003	-.056*	-.134*	-.124*	-.120*	-.137*	-.069*	-.004
	(.028)	(.037)	(.050)	(.055)	(.027)	(.028)	(.020)	(.017)	(.035)	(.035)	(.028)	(.028)

Note: Numbers in parentheses are standard errors; * $p < .05$(two-tailed tests).

Figure 1. Density of the Propensity Score for Austrian Data

Both sexes



Men

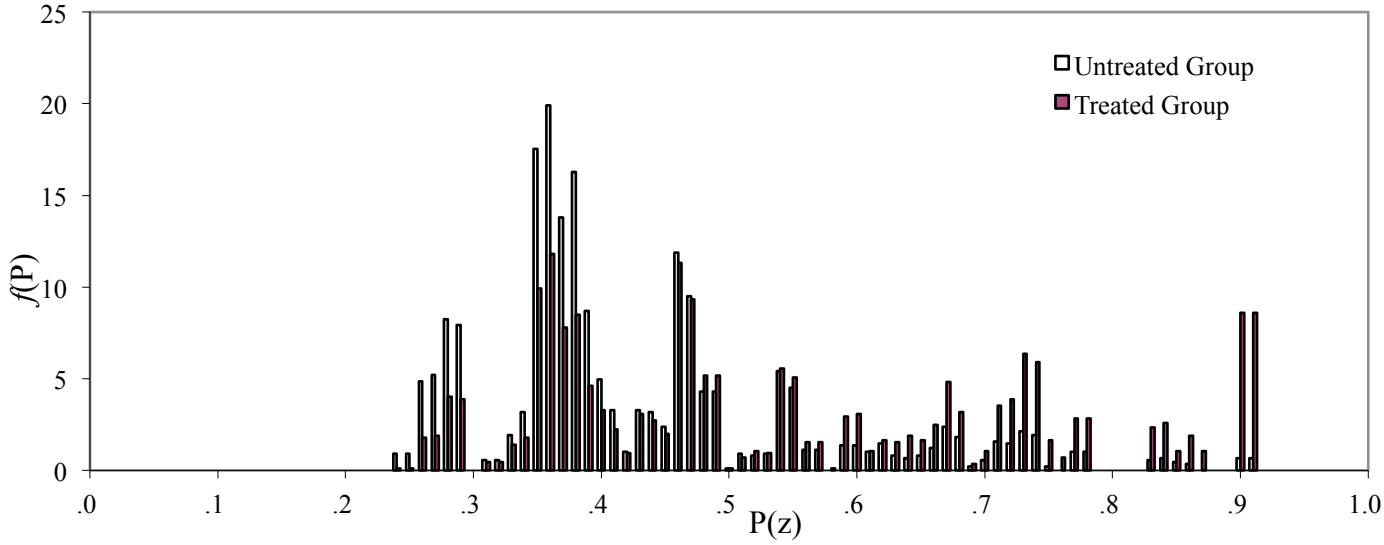
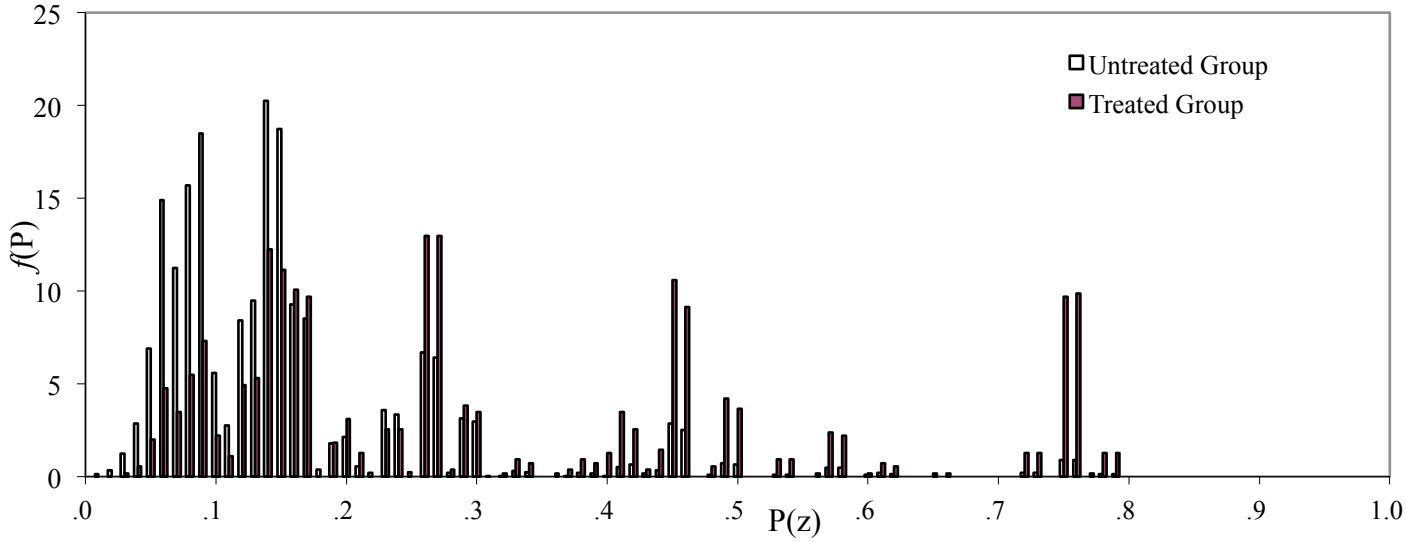
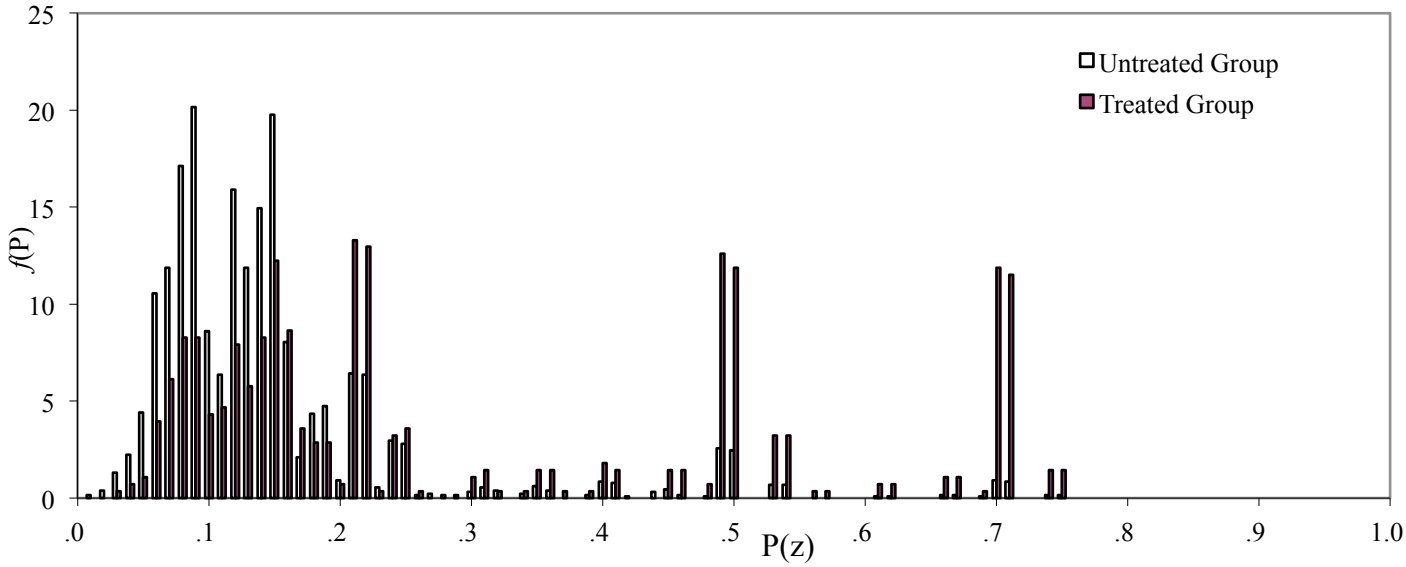


Figure 2. Density of the Propensity Score for Czech Data

Both sexes



Men



Women

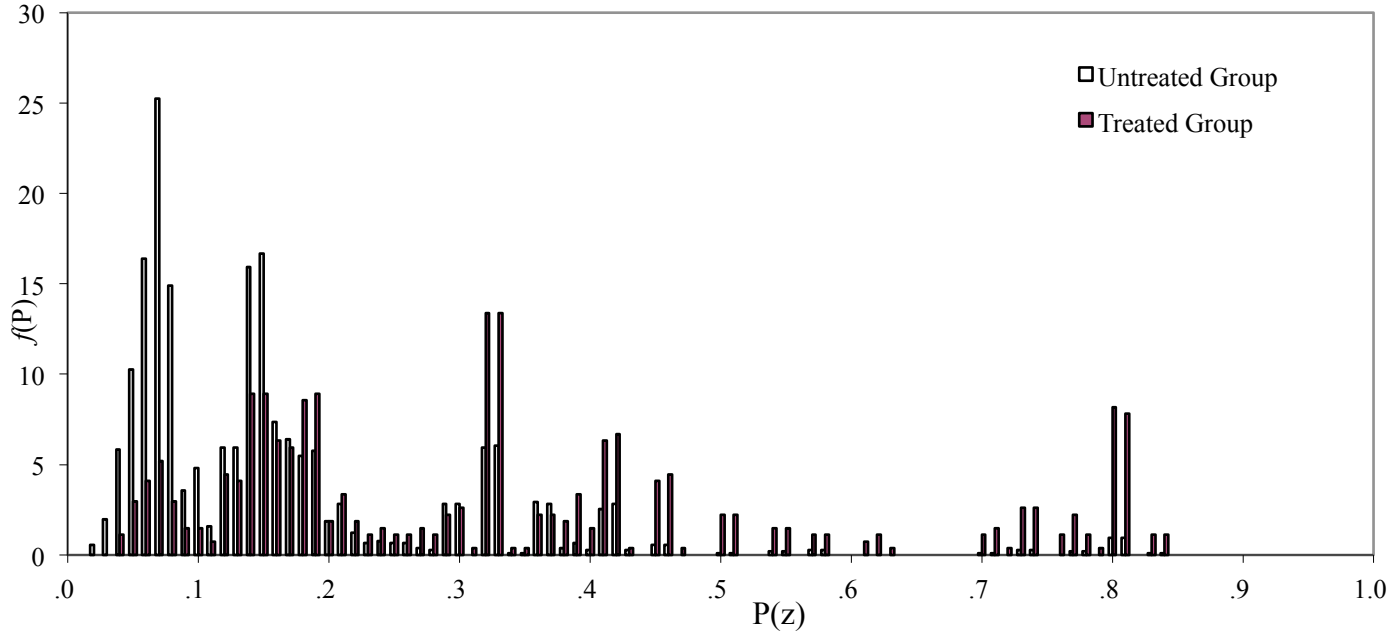
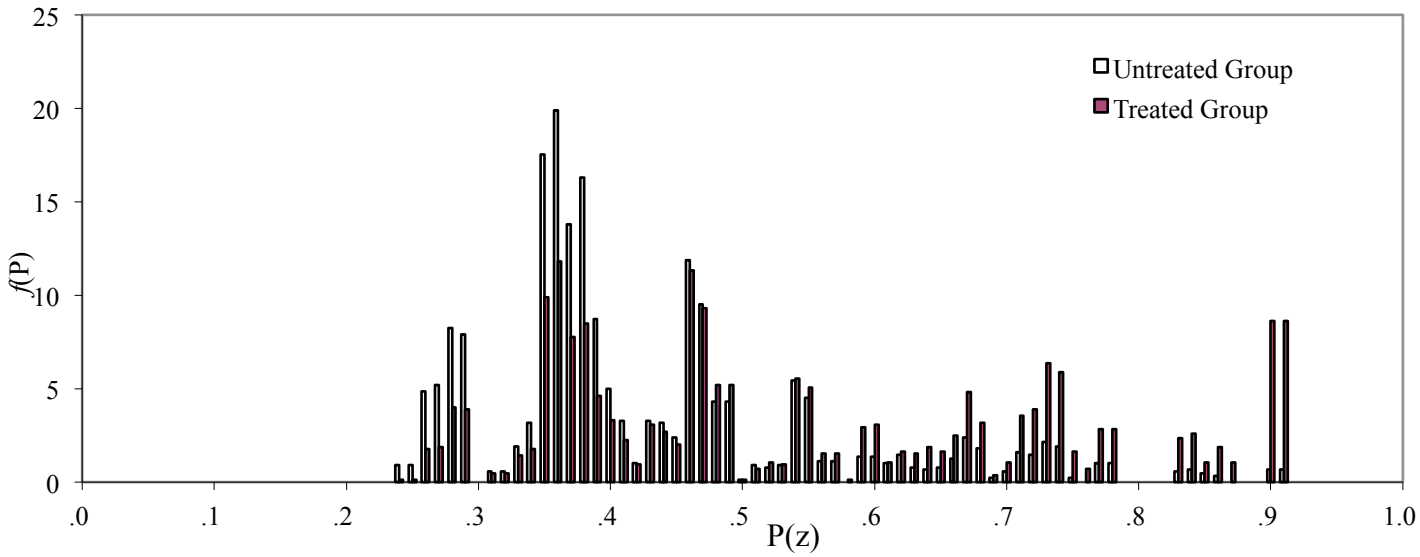
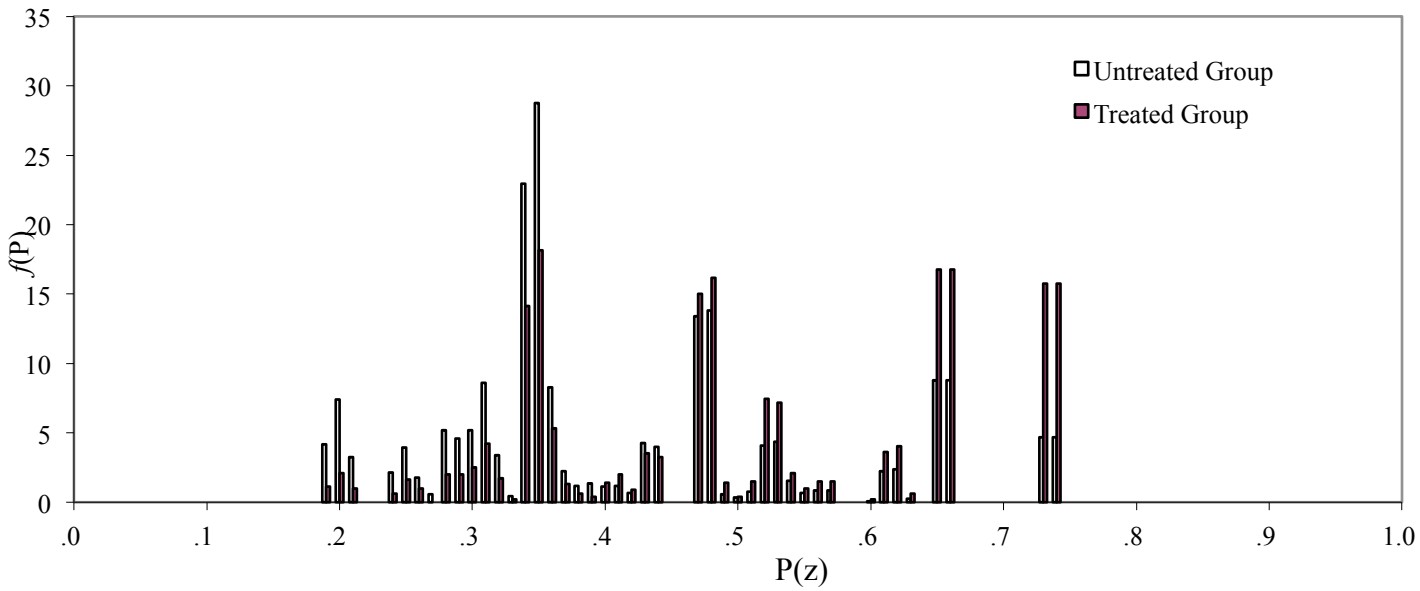


Figure 3. Density of the Propensity Score for German Data

Both sexes



Men



Women

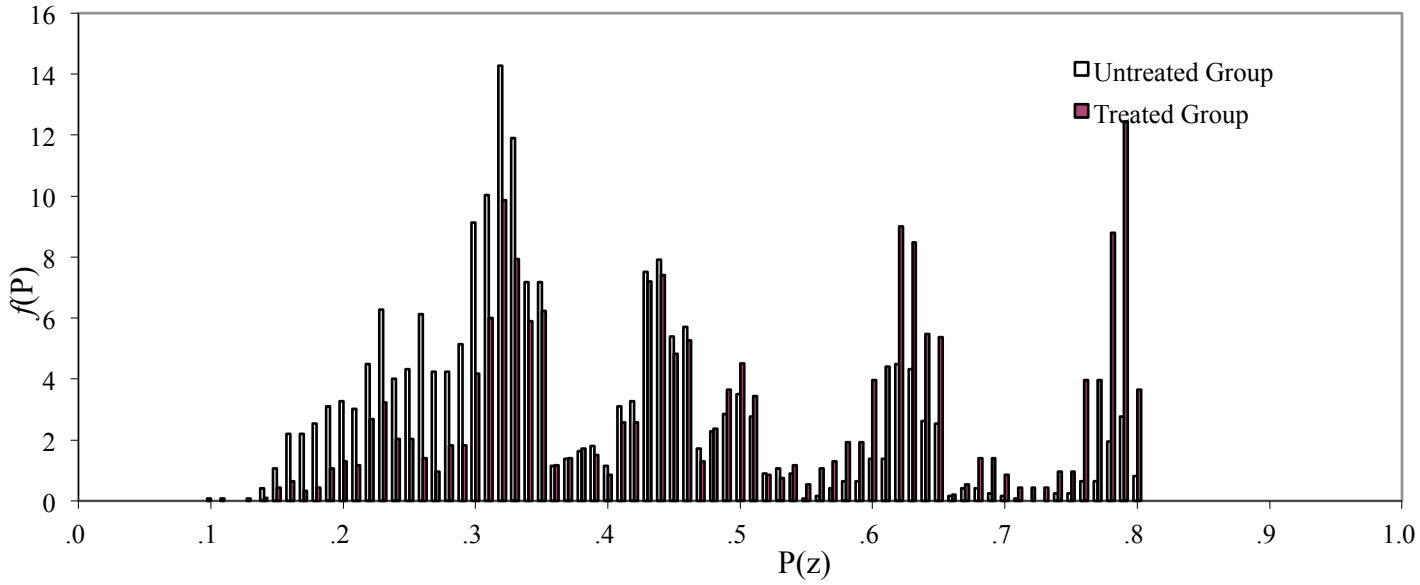
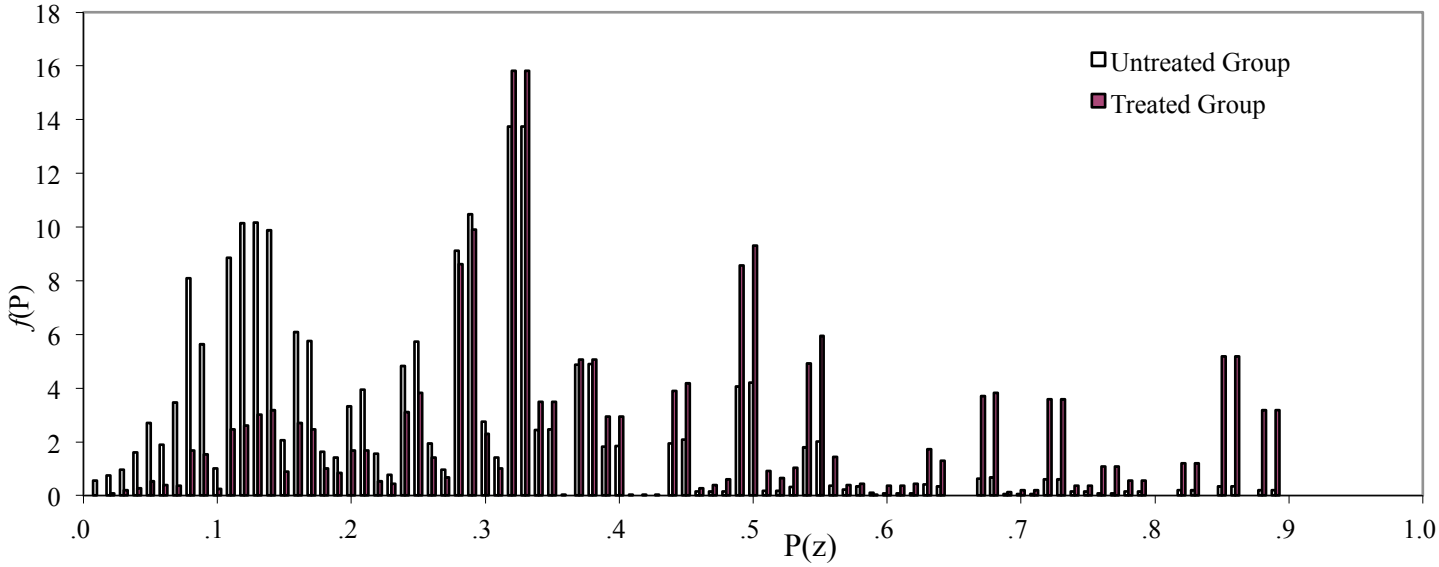
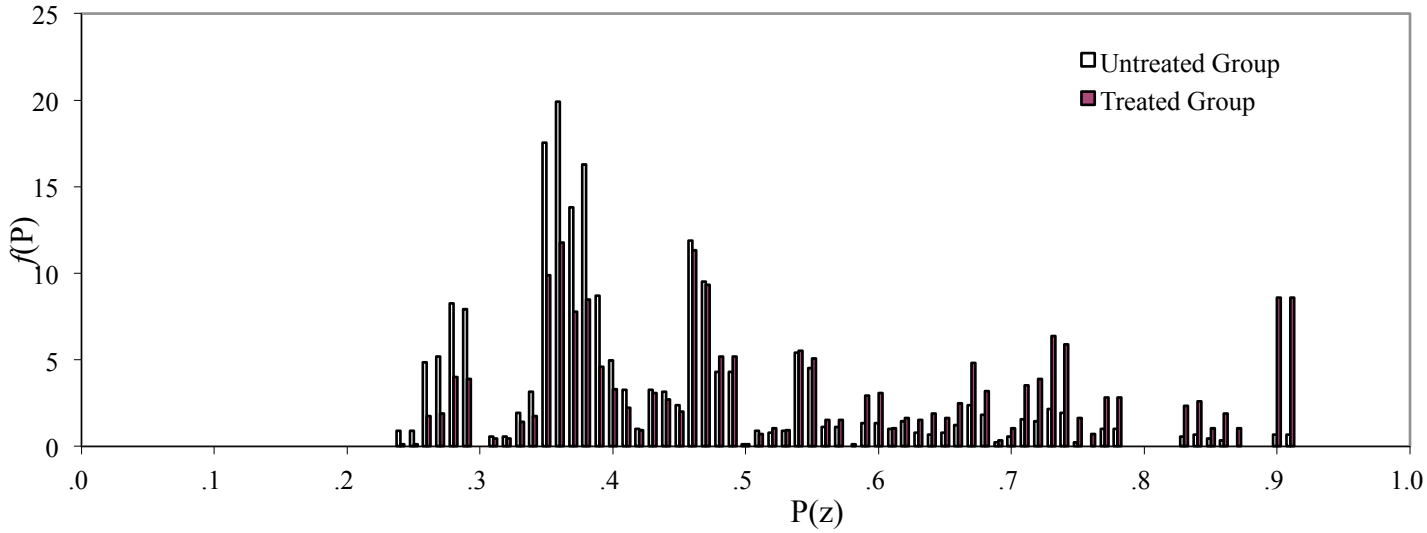


Figure 4. Density of the Propensity Score for Polish Data

Both sexes



Men



Women

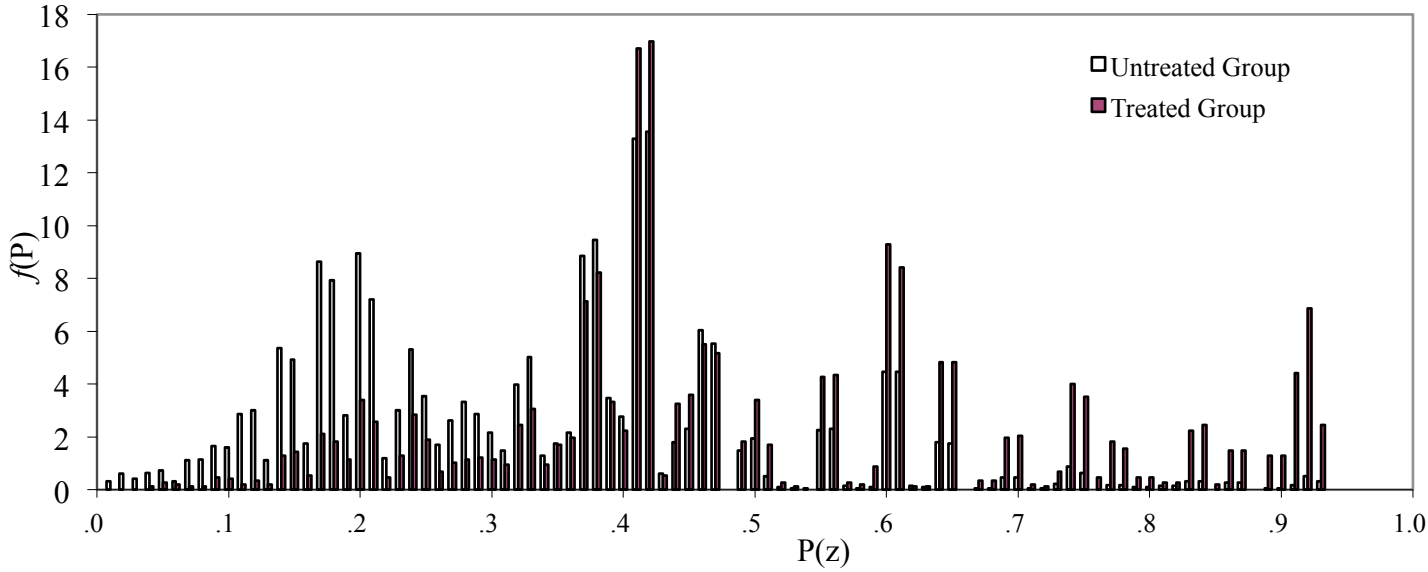
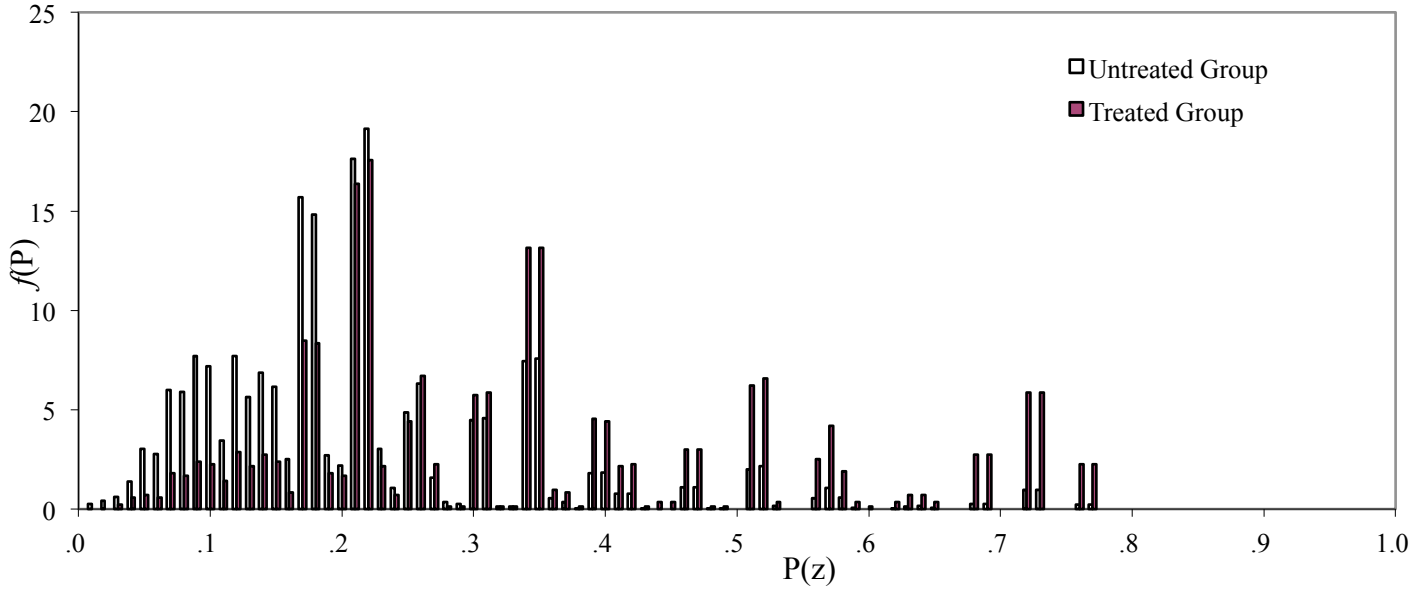
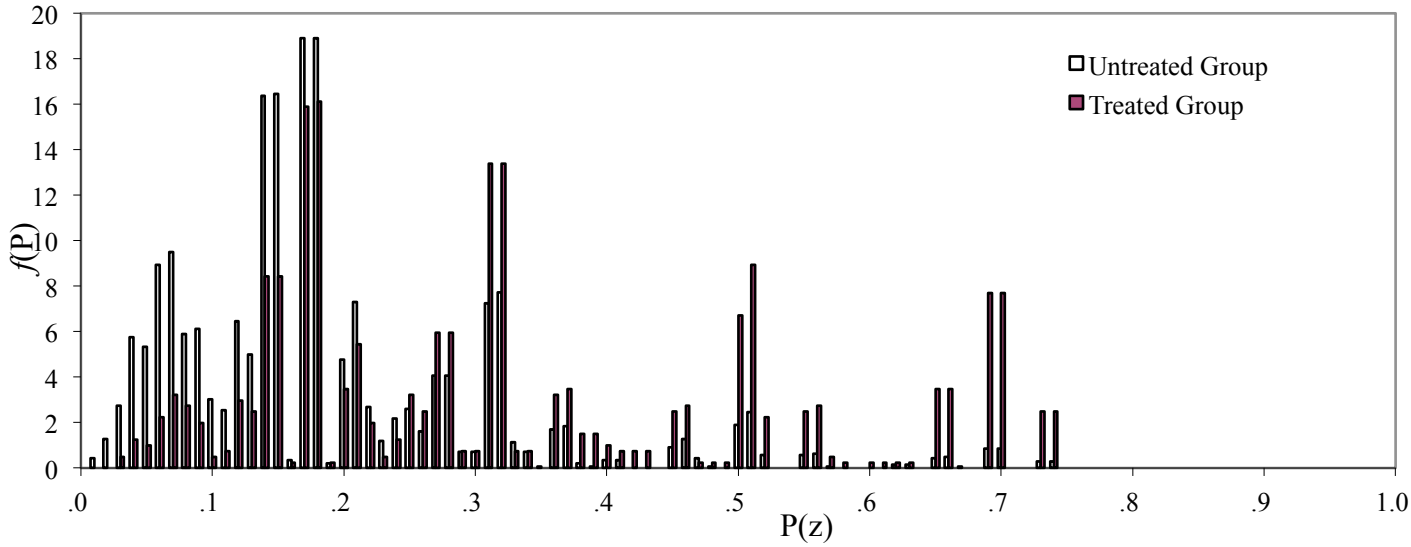


Figure 5. Density of the Propensity Score for Slovak Data

Both sexes



Men



Women

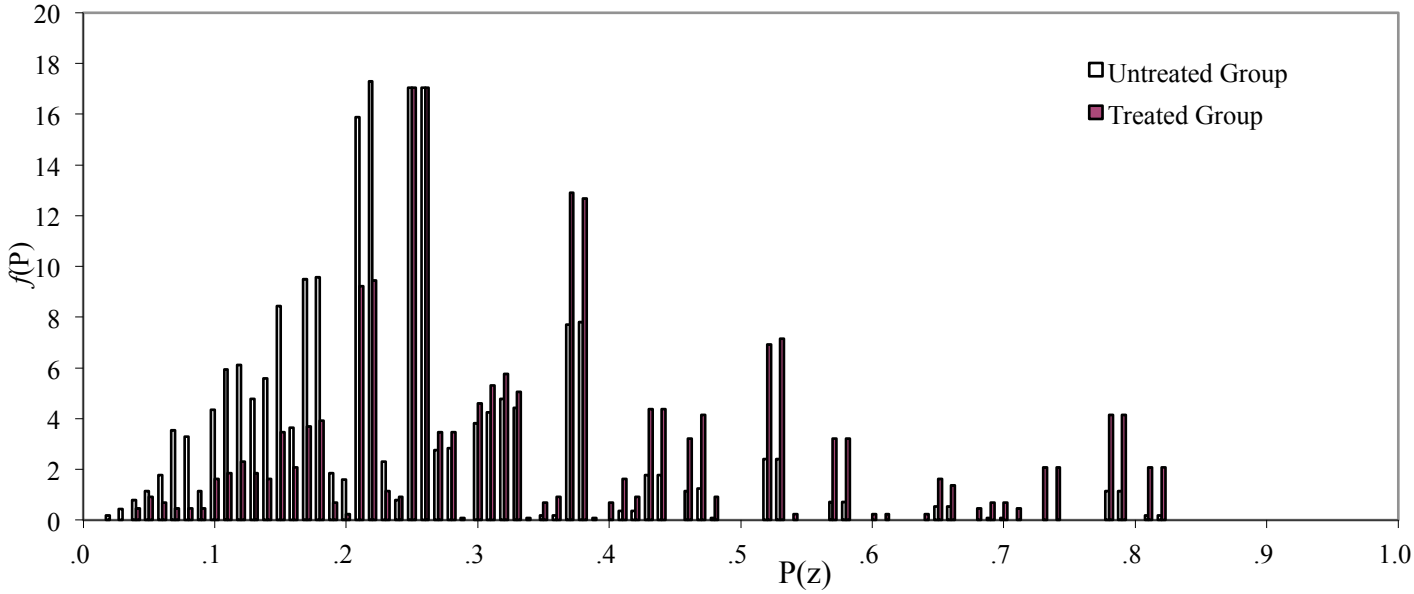


Figure 6. Density of the Propensity Score for British data

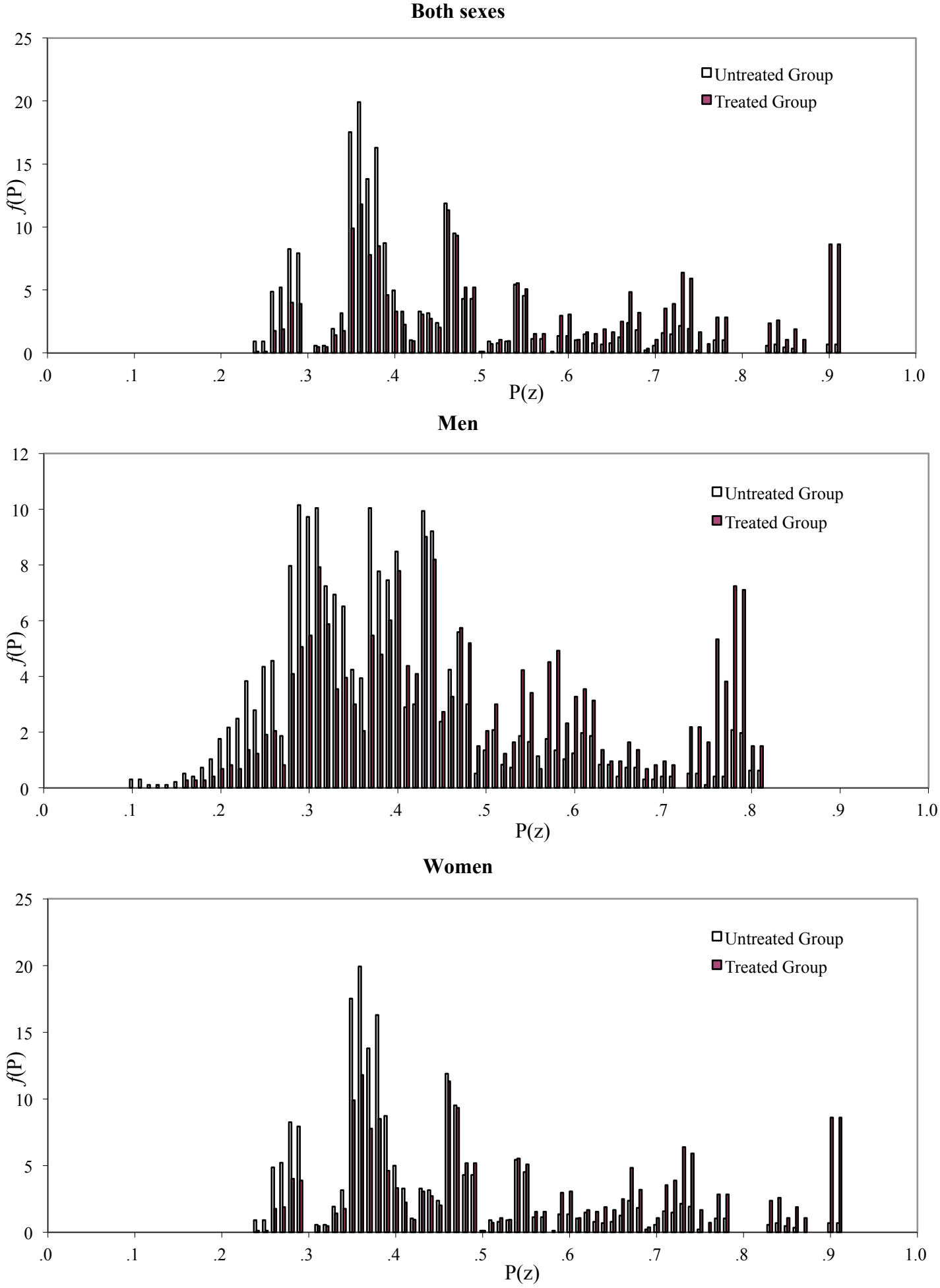


Table 3. Estimates of Treatment Parameters by Parametric and Semi-parametric Approaches for 6 Countries: Both Sexes

Treatment Parameter	Austria (N=2,366)		Czech (N=2,894)		Germany (N=4,321)		Poland (N=8,010)		Slovak (N=3,376)		U. K. (N=3,428)	
	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-
ATE	.807*	.810*	.812*	.715*	.737*	.638*	1.032*	.942*	.643*	.593*	.984*	.944*
	(.115)	(.169)	(.069)	(.123)	(.074)	(.075)	(.038)	(.045)	(.072)	(.083)	(.090)	(.102)
TT	.968*	1.132*	1.031*	.931*	.827*	.736*	1.147*	1.089*	.673*	.613*	.954*	1.047*
	(.127)	(.150)	(.090)	(.102)	(.088)	(.083)	(.054)	(.052)	(.088)	(.081)	(.093)	(.106)
TUT	.745*	.685*	.732*	.636*	.659*	.550*	.966*	.855*	.627*	.585*	1.003*	.877*
	(.122)	(.200)	(.070)	(.145)	(.071)	(.082)	(.042)	(.059)	(.075)	(.108)	(.100)	(.117)
OLS	.342*	.342*	.458*	.458*	.504*	.504*	.605*	.605*	.327*	.327*	.418*	.418*
	(.032)	(.032)	(.027)	(.027)	(.022)	(.022)	(.017)	(.017)	(.023)	(.023)	(.026)	(.026)
IV (Sibling)	.812*	.800*	.951*	.870*	.752*	.653*	1.091*	1.029*	.655*	.590*	.965*	1.026*
	(.117)	(.177)	(.081)	(.100)	(.077)	(.075)	(.043)	(.041)	(.077)	(.076)	(.089)	(.102)
Bias	-.465*	-.468*	-.355*	-.256*	-.233*	-.134	-.427*	-.337*	-.316*	-.266*	-.566*	-.526*
	(.119)	(.172)	(.074)	(.126)	(.078)	(.079)	(.042)	(.048)	(.076)	(.086)	(.093)	(.105)
Ability bias	-.625*	-.789*	-.573*	-.473*	-.323*	-.232*	-.541*	-.484*	-.346*	-.285*	-.536*	-.628*
	(.131)	(.153)	(.098)	(.105)	(.090)	(.086)	(.057)	(.055)	(.091)	(.084)	(.096)	(.109)
Sorting gain	.160	.320	.218	.216	.090	.098	.114	.147*	.030	.019	-.030	.102
	(.171)	(.227)	(.116)	(.159)	(.115)	(.112)	(.066)	(.069)	(.114)	(.116)	(.129)	(.147)

Table 4. Estimates of Treatment Parameters by Parametric and Semi-parametric Approaches for 6 Countries: Men

Treatment Parameter	Austria (N=1,326)		Czech (N=1,563)		Germany (N=2,163)		Poland (N=4,370)		Slovak (N=1,815)		U. K. (N=1,693)	
	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-
ATE	.776*	1.014*	.773*	.661*	.609*	.542*	1.029*	.877*	.546*	.486*	1.099*	1.039*
	(.160)	(.231)	(.099)	(.155)	(.100)	(.142)	(.057)	(.091)	(.100)	(.111)	(.120)	(.122)
TT	.864*	1.087*	1.115*	.801*	.653*	.646*	1.203*	1.176*	.535*	.497*	1.087*	1.119*
	(.203)	(.234)	(.137)	(.144)	(.105)	(.152)	(.079)	(.083)	(.119)	(.103)	(.132)	(.150)
TUT	.734*	.975*	.641*	.603*	.570*	.426*	.942*	.730*	.552*	.481*	1.108*	.980*
	(.154)	(.262)	(.095)	(.174)	(.101)	(.144)	(.062)	(.116)	(.104)	(.135)	(.128)	(.132)
OLS	.257*	.257*	.448*	.448*	.448*	.488*	.570*	.570*	.330*	.330*	.345*	.345*
	(.037)	(.037)	(.034)	(.034)	(.027)	(.027)	(.025)	(.025)	(.032)	(.032)	(.034)	(.034)
IV (Sibling)	.831*	1.080*	1.011*	.787*	.608*	.515*	1.133*	1.067*	.540*	.482*	1.091*	1.092*
	(.183)	(.219)	(.122)	(.142)	(.100)	(.141)	(.065)	(.076)	(.104)	(.098)	(.128)	(.132)
Bias	-.519*	-.758*	-.325*	-.213	-.161	-.094	-.459*	-.307*	-.216*	-.156	-.754*	-.694*
	(.165)	(.234)	(.105)	(.158)	(.104)	(.144)	(.063)	(.094)	(.105)	(.116)	(.125)	(.127)
Ability bias	-.607*	-.831*	-.666*	-.352*	-.205	-.199	-.633*	-.606*	-.205*	-.167	-.741*	-.774*
	(.206)	(.237)	(.141)	(.148)	(.109)	(.154)	(.082)	(.087)	(.124)	(.108)	(.137)	(.153)
Sorting gain	.088	.073	.341*	.139	.044	.105	.174	.299*	-.011	.010	-.013	.080
	(.259)	(.328)	(.169)	(.211)	(.145)	(.207)	(.098)	(.124)	(.156)	(.152)	(.178)	(.193)

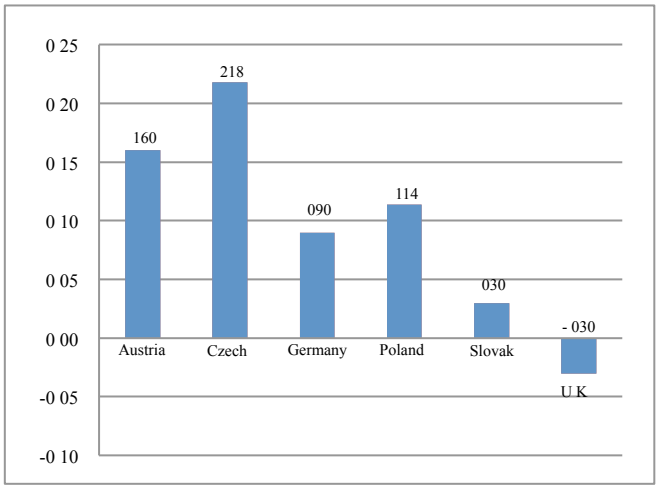
Table 5. Estimates of Treatment Parameters by Parametric and Semi-parametric Approaches for 6 Countries: Women

Treatment Parameter	Austria (N=1,028)		Czech (N=1,321)		Germany (N=2,153)		Poland (N=3,626)		Slovak (N=1,549)		U. K. (N=1,732)	
	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-	Para-	Semi-
ATE			.854*	.760*	.882*	.785*	1.055*	1.027*	.762*	.710*	.880*	.860*
			(.092)	(.138)	(.093)	(.098)	(.060)	(.066)	(.097)	(.148)	(.119)	(.143)
TT			.902*	1.050*	.983*	.804*	1.100*	1.012*	.837*	.777*	.856*	.902*
			(.119)	(.132)	(.116)	(.120)	(.077)	(.831)	(.121)	(.140)	(.126)	(.154)
TUT			.836*	.660*	.800*	.769*	1.019*	1.034*	.711*	.684*	.895*	.829*
			(.094)	(.165)	(.093)	(.107)	(.066)	(.090)	(.102)	(.192)	(.125)	(.160)
OLS			.466*	.466*	.556*	.556*	.637*	.637*	.318*	.318*	.490*	.490*
			(.043)	(.043)	(.036)	(.036)	(.024)	(.024)	(.033)	(.033)	(.038)	(.038)
IV (Sibling)			.888*	.950*	.936*	.794*	1.078*	1.131*	.790*	.714*	.916*	.833*
			(.107)	(.124)	(.102)	(.105)	(.063)	(.064)	(.104)	(.138)	(.133)	(.179)
Bias			-.388*	-.295*	-.327*	-.229*	-.418*	-.390*	-.444*	-.391*	-.391*	-.370*
			(.102)	(.145)	(.100)	(.105)	(.064)	(.070)	(.103)	(.151)	(.113)	(.148)
Ability Bias			-.436*	-.585*	-.427*	-.247*	-.463*	-.375*	-.519*	-.459*	-.366*	-.412*
			(.127)	(.138)	(.121)	(.125)	(.081)	(.086)	(.126)	(.145)	(.132)	(.158)
Sorting gain			.048	.290	.100	.018	.045	-.015	.075	.068	-.025	.042
			(.150)	(.191)	(.148)	(.155)	(.097)	(.106)	(.155)	(.204)	(.173)	(.210)

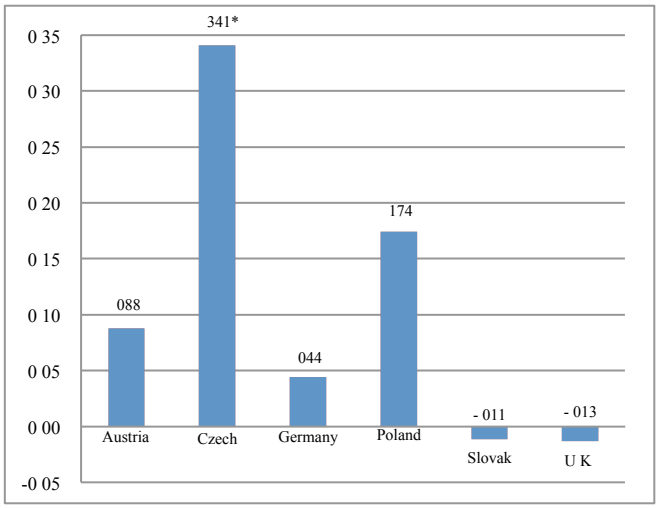
Figure 7. Estimates of Sorting Gain or Loss

Parametric Approach

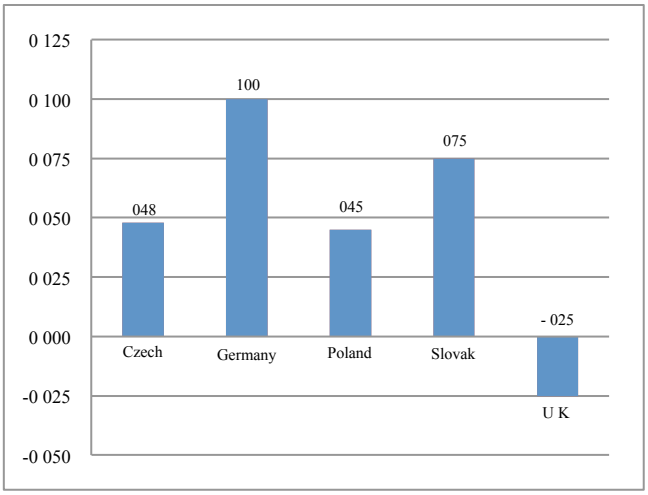
Both sexes



Men

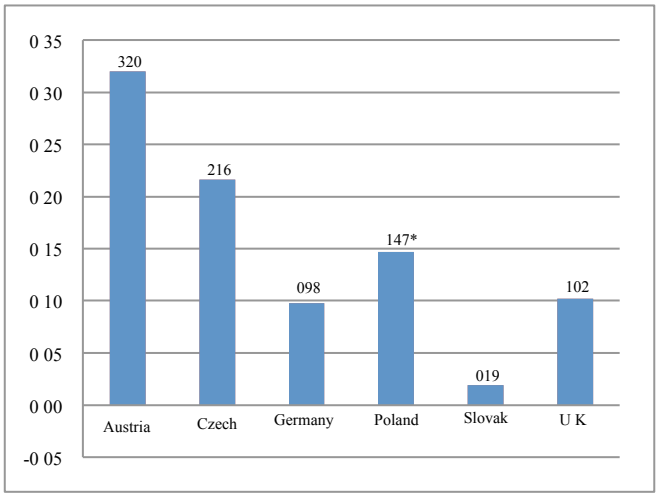


Women

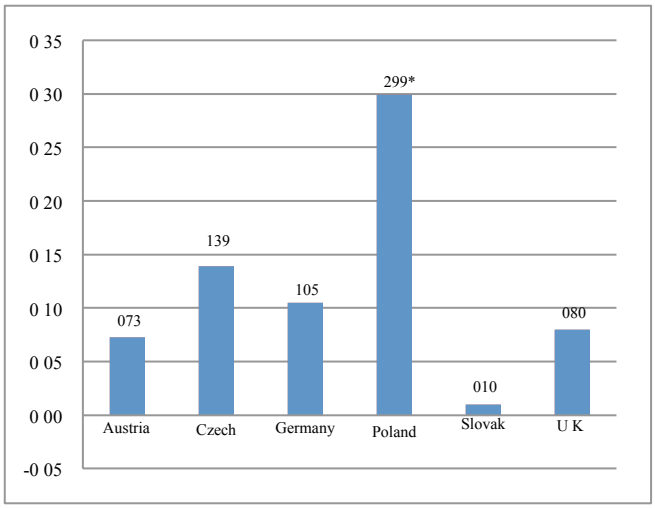


Semi-parametric Approach

Both sexes



Men



Women

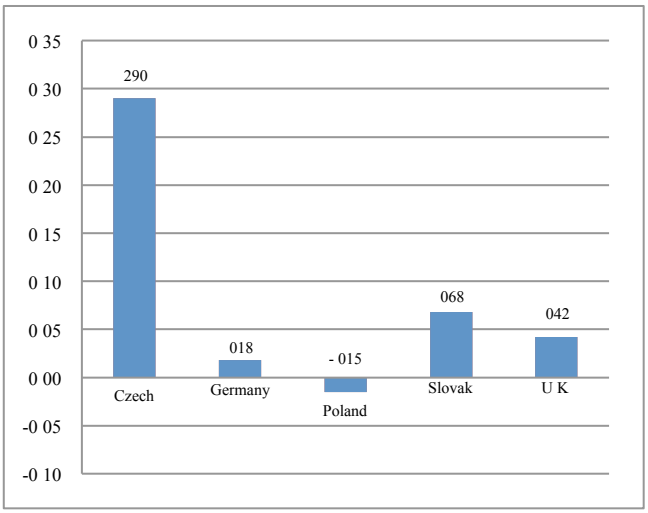
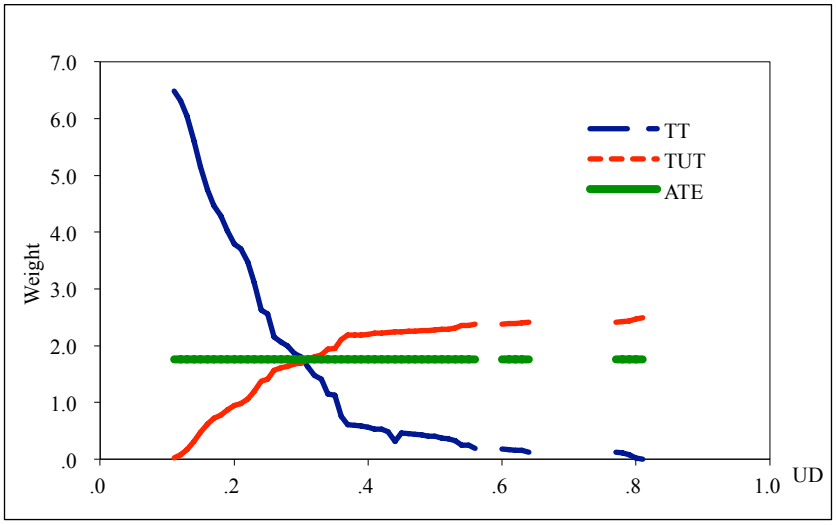
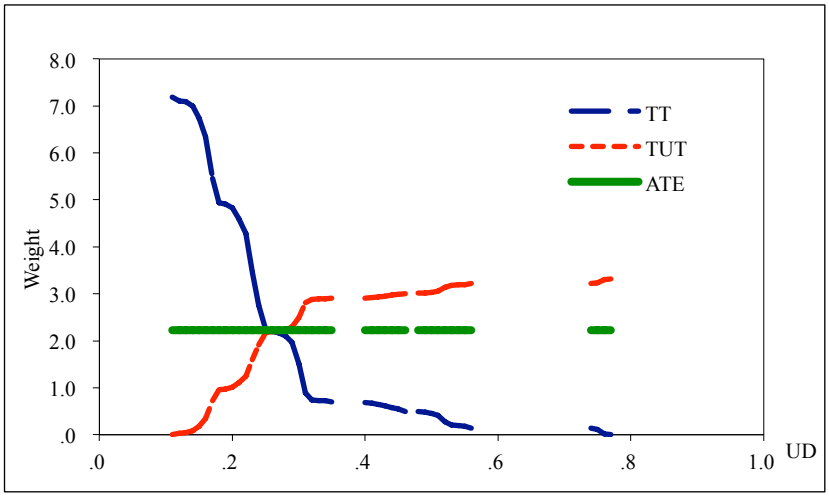


Figure 8: Marginal Treatment Effects in Austria

Both sexes



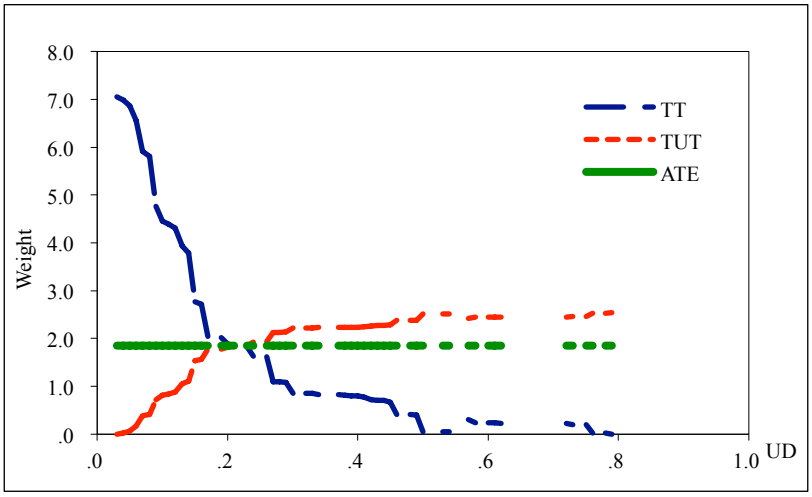
Men



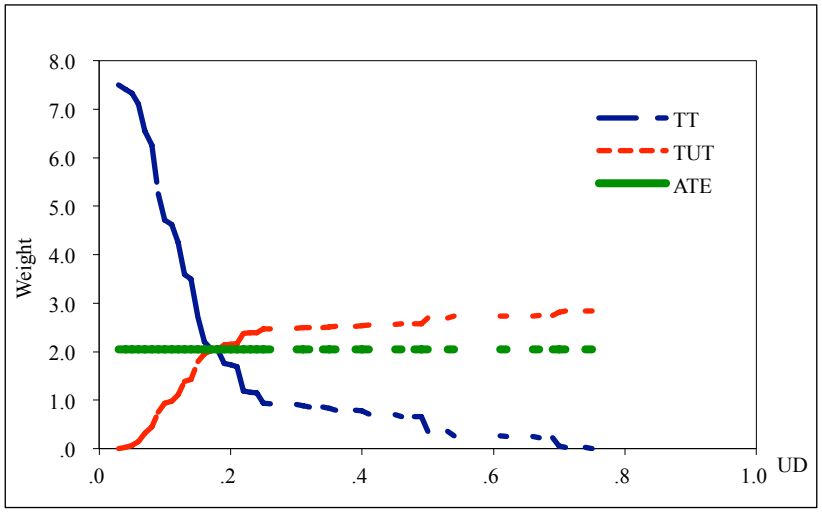
Women

Figure 9: Marginal Treatment Effects in the Czech Republic

Both sexes



Men



Women

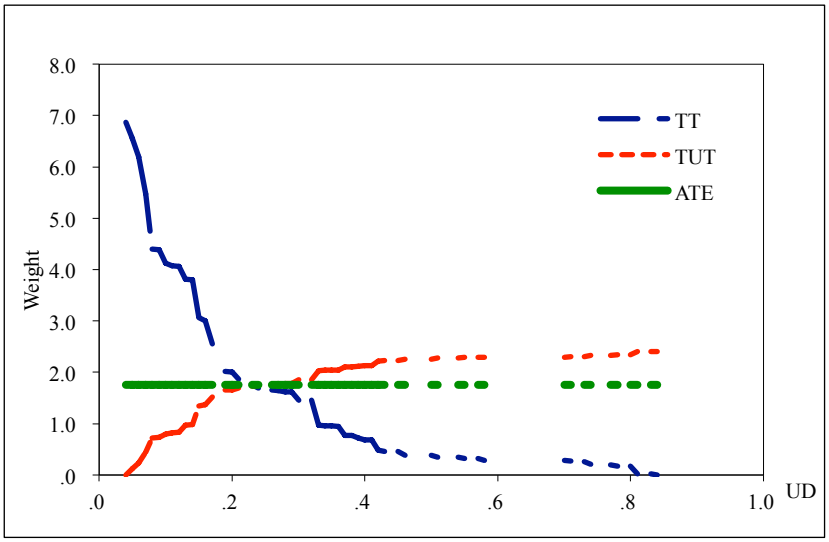


Figure 10: Marginal Treatment Effects in Germany

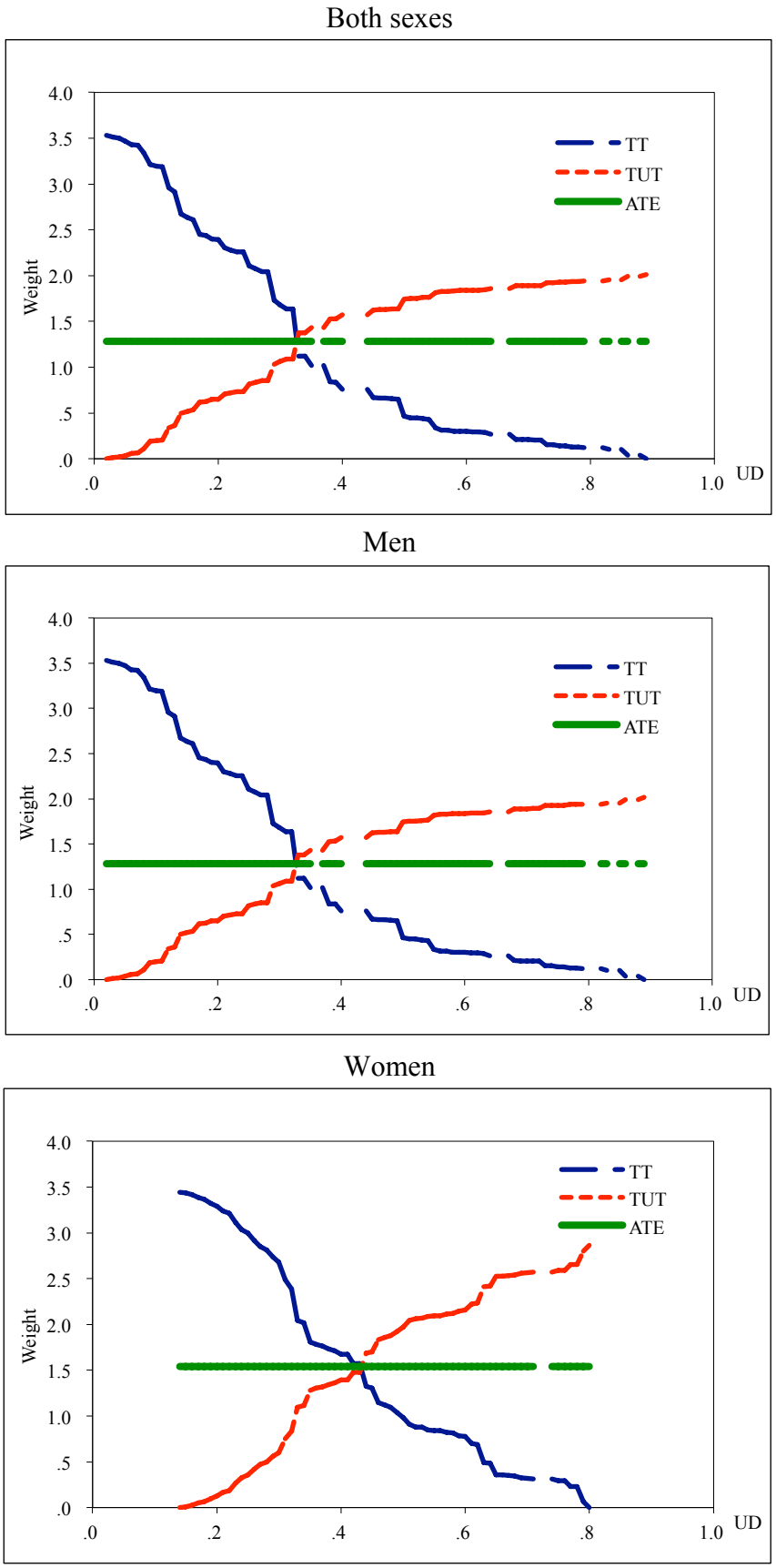
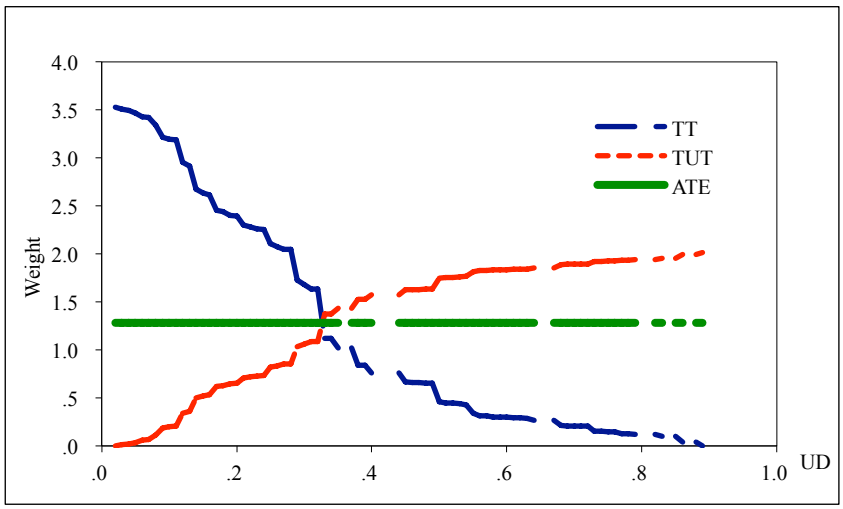
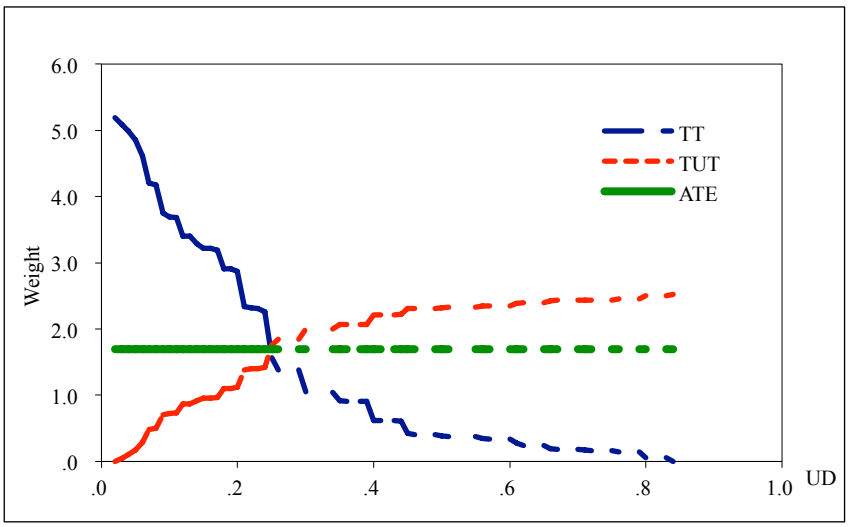


Figure 11: Marginal Treatment Effects in Poland

Both sexes



Men



Women

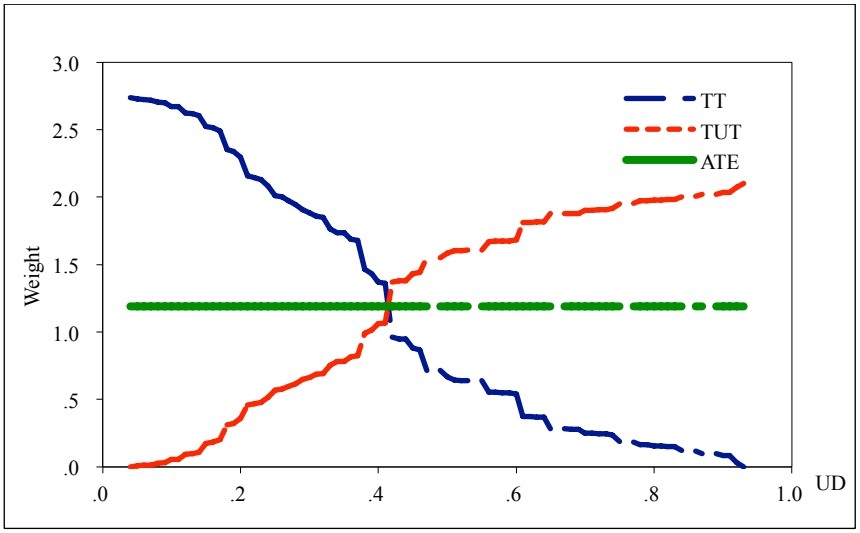
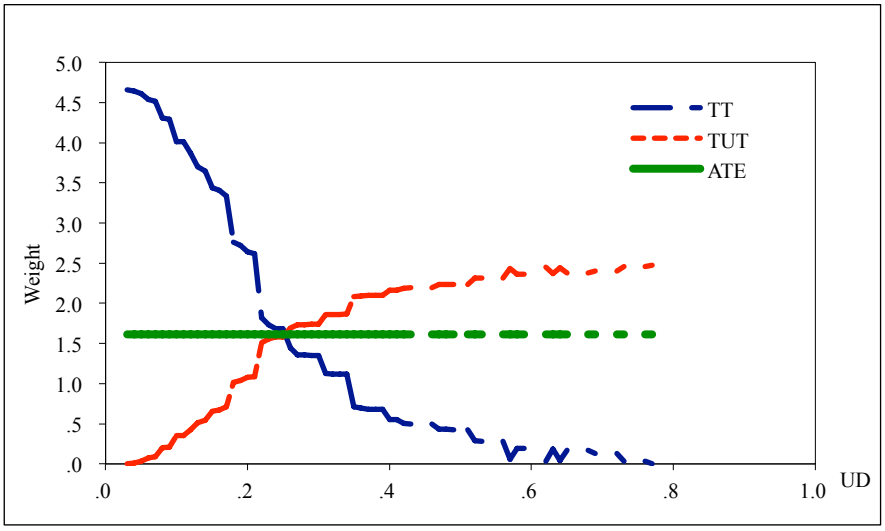
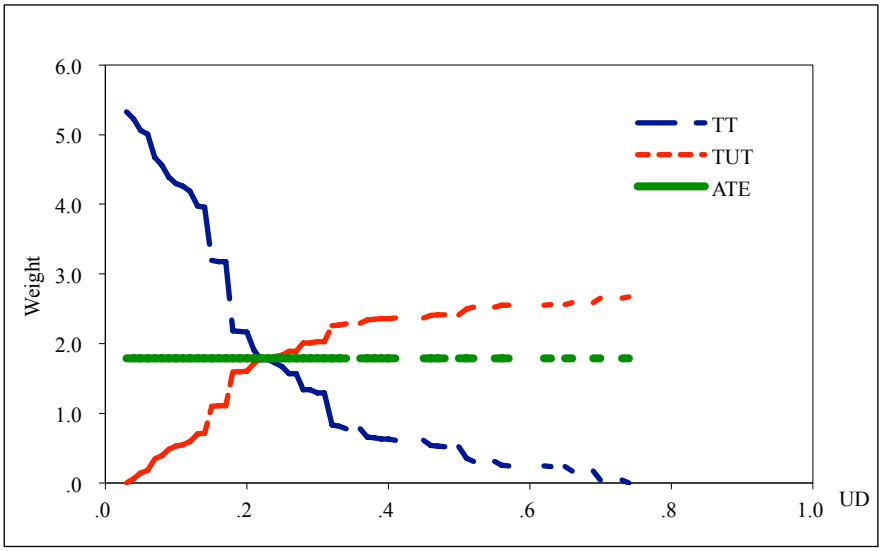


Figure 12: Marginal Treatment Effects in Slovakia

Both sexes



Men



Women

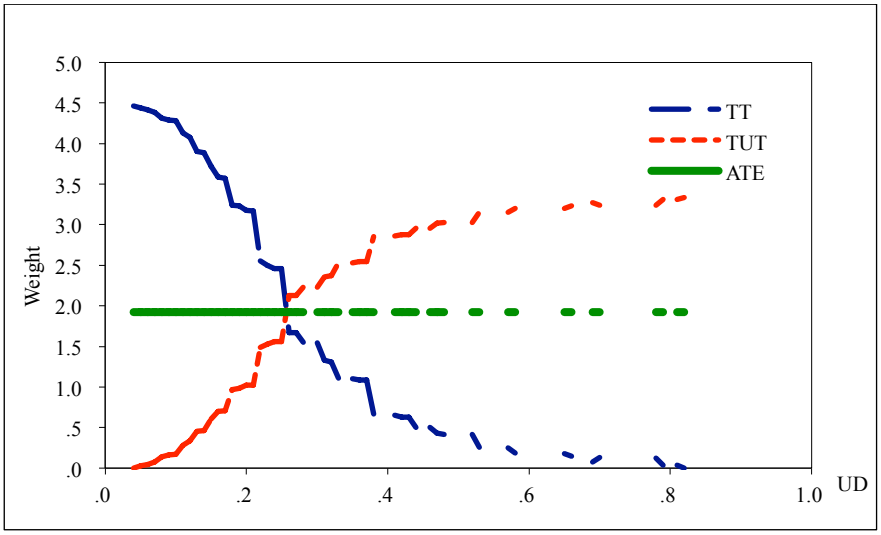
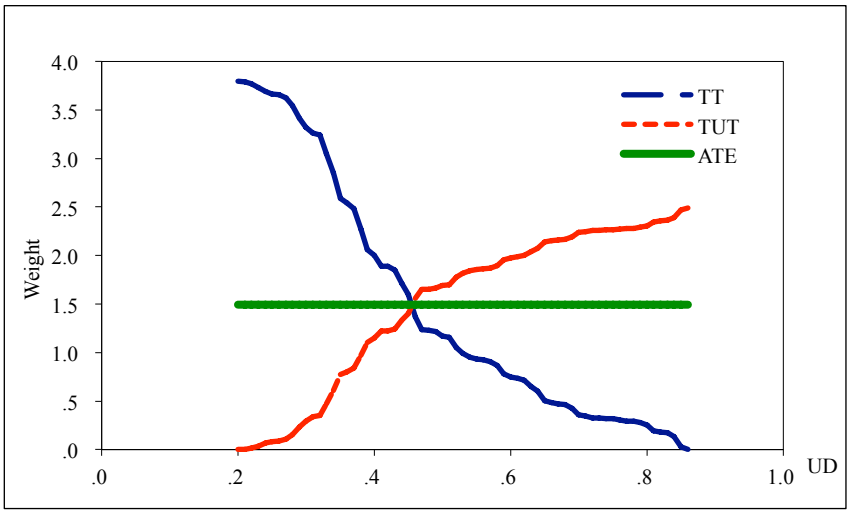
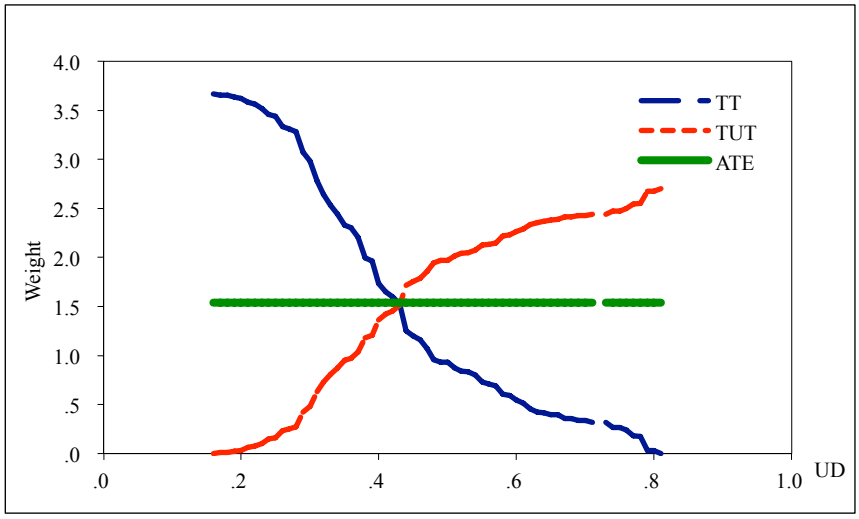


Figure 13: Marginal Treatment Effects in the United Kingdom

Both sexes



Men



Women