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ABSTRACT

We combine survey data on British and German immigrants in the US with data on natives in Britain and Germany to estimate the causal effect of migration on educational mobility through cross-national marriage. To control for selective mating, we instrument educational attainment using government spending on education in the years each person was of school-age. To control for selective migration, we instrument the migration decision using inflows of immigrants to the US during puberty and early adulthood. We find that migration causes men to marry up and women to marry down, but self-selection into migration and marriage dampens down these effects.

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1 Introduction

The decisions to marry and to migrate affect a wide range of outcomes of scientific and policy interest, such as income inequality, female labor supply, the number of births and population growth, and the distribution of family resources. In the extensive and largely independent economics literatures that study marriage and migration, researchers recognize that both decisions involve high degrees of self-selection based on a range of characteristics and, depending on this selectivity, they may have diverse effects on the decision-makers. An important drawback of the literature is that it examines marriage and migration independently, it does not account for the interplay between the two and, therefore, it fails to accurately identify their causal effect on socio-economic outcomes. To contribute evidence on this issue, we take education as a proxy of social status and economic well-being and we ask whether a migrant is more likely to marry a spouse of higher education than he would have if he had not migrated.

Migration research in economics treats the decision to migrate as an investment that depends on earnings differentials across countries net of migration costs (Sjaastad 1962). By comparing emigrants to non-migrants in the home country pre-migration, researchers have shown that this decision process produces migrants with select skills and characteristics (Chiquiar and Hanson 2005, Ibarraran and Lubotsky 2007, McKenzie and Rapaport 2010, Fernández-Huertas Moraga 2011). In combination with the causal effects of migration, this selectivity entails differential socio-economic trajectories for migrants relative to the population in both the origin and destination countries. Empirical studies, however, rarely compare the post-migration outcomes of migrants with outcomes of compatriots who did not migrate (e.g. Abramitzky, Boustan, and Eriksson 2012). Instead, studies typically compare migrants' outcomes to those of the native-born individuals or other co-ethnics who previously migrated to the host country. These studies test the extent to which migrants integrate into the host society or whether children of migrants are more or less upwardly mobile than children of natives. Much of this research relies on US data (Borjas 1993, 1995, 1996, 2002, Card 2005), although more recent studies have also used data from Australia (Chiswick, Lee and Miller 2005), Europe (Dustman, Glitz, and Vogel 2010, Dustman and Theodoropoulos 2010) and Canada (Aydemir, Chen, and Corak

2009). Depending on the nature of migrant selectivity, these studies estimate mixed effects of migration on migrants' economic status.

Marriage research in economics originates from the work of Becker (1974), who predicted that individuals can gain higher social or economic status through marital sorting, depending on whether mobility is measured on the basis of characteristics that are complements or substitutes in household production. For example, Becker argues that women are more likely to marry-up in terms of wages relative to men because men tend to specialize in market production and choose to marry women who specialize in home production (negative assortative mating). In contrast, marital mobility in terms of education is uncertain, since education encompasses characteristics that are both complements and substitutes in household production. In his extension of Becker's model, Lam (1988) argues that assortative mating (or homogamy) with respect to wages depends on two different offsetting forces. On the one hand, there are returns to specialization in household production which generates a tendency for negative assortative mating. On the other hand, joint consumption of household public goods generates a tendency for positive assortative mating because there are returns to spouses having similar demand for these public goods. Empirical studies generally find positive assortative mating on the basis of education but, consistent with Lam's prediction, there is mixed empirical support for the hypothesis of negative assortative mating on the basis of wages (Zimmer 1996; Nakosteen and Zimmer 2001; Zang and Liu 2003; Nakosteen, Westerlund, and Zimmer 2004). Irrespective of its direction, assortative mating is important not only because it determines the economic mobility of the spouses but also because its effect extends to their offspring (Chadwick and Solon 2002; Ermisch et al. 2006).

Of course, researchers have long recognized that individuals may take the decisions to migrate and marry jointly. For example, studies in sociology have observed that women from developing countries often migrate to richer countries with a bigger supply of 'good' potential spouses in order to marry men living there (Constable 2004; Kim 2009), including compatriot men who had previously migrated (Lievens 1999). Others examine whether migrants marry natives after they arrive to assimilate more rapidly in the culture and society of their host country (Qian and

Lichter 1991; Sassler 2005). Finally, a different set of studies discuss whether people, especially women, marry a foreigner while still in their home country to make it easier to move to another country - either because that country offers better labor market opportunities or because it offers other benefits such as better human rights (Watts 1983; Ortiz 1996; Piper 1999). More recently, economic research has examined whether migrants differ from natives on how they select their spouses. Celikaksoy et al. (2006) find that immigrants assort positively on education, even when they 'import' their spouses from their country of origin. Furtado and Theodoropoulos (2011) find that matching on education rather than ethnicity is more important for natives and those immigrants who arrived as young children, especially whites. Furtado (2012) focuses on second generation immigrants and shows that, when the distribution of educational attainment differs by ethnicity, individuals trade similarities in ethnicity for similarities in education when choosing spouses. Lafortune (2013) delves even deeper and shows that migrants who are forward looking will invest in education depending on their expectations of the marriage market in the host country.

Albeit insightful, the above studies suffer several shortcomings. First, because they fail to formally address either marital selectivity or immigrant selectivity or both, they cannot identify the separate effects of the two decisions on the outcomes of interest. Second, because they typically rely on cross-sectional data who are limited to people who are currently married, they cannot determine whether people invested in education after they married so that their education levels converged even when they were uncorrelated before marriage. Such behavior might plausibly occur if having a partner makes it easier to finance education or if a partner shares information about educational opportunities. Finally, because the studies only use data from the country to which people moved, their evidence sheds no light on what is arguably the most interesting counterfactual question - whether and to what degree would marital sorting differ had immigrants never left their home country.

In this paper, we aim to identify the causal effect of migration on marital mobility in terms of educational attainment. We combine survey data from Germany and the UK with survey data on German and British immigrants from the US. With these data we compare educational mobility through marriage among couples of natives living in the UK and Germany and couples living in the US where one partner is a British or German immigrant and the other partner is a US native. We estimate the probability that a migrant marries someone with more education, correcting for both migration and spouse selectivity. To purge the effect of marital sorting, we instrument for attained education using temporal variation in government spending on total education during the years each person was of school-age. By doing this, we avoid counting as marital mobility any correlation between the education of partners that arises because of how people select a spouse or the correlation that is the consequence of post-marriage educational attainment. To purge the effect of selective migration, we instrument the migration decision using variation in the number of British and German citizens who migrated to the US during the years each person was in puberty and early adulthood. These migrant inflows serve as a proxy for the extent of migration networks available to people who are deciding whether or not to move. Our identifying assumption is that higher migration flows lower the cost of migration but do not affect the probability of marrying a more educated US native.

At the observational level, marital mobility on the basis of education is roughly the same between migrant and non-migrant men, and it is somewhat higher for migrant women relative to non-migrants, especially Germans. However, when we control for marital and migration selectivity, mobility is higher for immigrant men than non-migrant men, and it is lower for migrant women than non-migrant women. Our analysis suggests that these patterns arise because selectivity differs by sex and migration status. First, we find that unobserved characteristics related to marital appeal favor migrant women and disfavor migrant men relative to their non-migrant counterparts. These results are consistent with Becker's prediction that men specialize in market production and women specialize in home production, assuming that this specialization generates equivalent specialization in mating strategies (i.e. men value non-marketable spousal traits and women value marketable traits). Second, we find that unobserved characteristics related to the migration decision decrease the probability of marrying up for migrants relative to their non-migrant counterparts. This result suggests that migrants exchange the education of their US spouses for other benefits, e.g. they may select to marry-down as a means to gain entry to the US. Finally, the causal effect of migration on mobility is consistent with the higher availability of educated spouses in the US than in the UK and Germany, and with the fact that British and German women marry later in life relative to their US competitors.

The paper is structured as follows: section 2 presents the data and some descriptive statistics, section 3 discusses the empirical strategy, and section 4 presents and discusses the results. A final section concludes the paper.

2 The Data

We draw data from the 1994-2010 monthly waves of Current Population S urveys (CPS); from the 1994-2008 waves of the British Household Panel Survey (BHPS); and from the 1994-2009 waves of the version of the German Socio-Economic Panel Study (SOEP) produced for the Cross National Equivalent File (Frick et al. 2007). Since 1994 the CPS asks all respondents where they were born and when they arrived in the US, which allow us to identify first-generation immigrants from Germany and the UK. All surveys ask respondents about their level of education, their socioeconomic characteristics (age, sex, race), and their relationship with other household members. Before we conduct any analysis with these data, we examine whether the probability of ever getting married differs by country and migration status. As Table 1 shows, we find that differences are small and in line with the constancy of the marriage probability worldwide[1] and the classification of marriage as a "cultural universal" (Brown 1991).

We pool data from all monthly CPS surveys and keep records for couples of first-generation immigrants and US natives. We combine the CPS data with comparable data from all BHPS and SOEP couples who are both native-born. We exclude from our sample anyone who migrated before age 18 to reduce the chance that a person migrated not because he chose to do so but because his parents chose to migrate. We also exclude individuals surveyed when they were 21 or younger because our primary focus is on educational mobility and we want to reduce the

[1] Using data from the Demographic Yearbooks of the United Nations on 97 industrial and agricultural countries societies, Fisher (1989) reports that between 1972 and 1981, 93.1% of women and 91.8% of men were married by age 49.

probability that a person was still in school.

The CPS data do not include information on when couples married. Consequently, we cannot differentiate between immigrants who married before they came to the US and immigrants who married after they arrived. We do know, however, where each partner was born and where each partner's parents were born. Thus, we can restrict our immigrant sample to include only first-generation immigrants from the UK or Germany who are married to a US native (i.e. a US-born individual whose parents were also born in the US or in a country other than the UK or Germany). We exclude other types of immigrants (e.g. those married to a first- or second-generation compatriot immigrant, or an immigrant from a different country) mostly because the size of those samples are small.[2] We also exclude these immigrants because they likely took their migration decisions as a couple rather than as individuals. To model the educational mobility of such couples requires a complex model of the migration decision which would allow the migration of immigrant husbands and wives to be simultaneously determined. Of course, it is also possible for British and German migrants to have married US natives before they arrived in the US, as is the case of women who met and married U.S. servicemen stationed in their home-country during and after World War II and entered the US as "War Brides".[3] In the following section, we explain that our identification method addresses the potential bias that this source of endogeneity might cause.

Table 2 describes the demographic characteristics of couples by sex and migration status and reports sample sizes. The data show that the distribution of education of husbands is broadly

[2]Among all British and German migrants in our sample, about 68% are married to US natives, 31% are married to compatriots, and less than 1% are married to migrants from other countries.

[3]It is estimated that approximately 115,000 immigrants entered the US under the provisions of the War Brides Act of 1945; the Alien Fiancees and Fiances Act of 1946; and the Soldier Brides Acts of 1946 and 1947 (INSUS 1950). These brides entered the country within a narrow time window (in the decade following the end of WWII) and belonged to a narrow birth cohort. Based on a small survey of British war brides conducted in 1989, the average age of the brides at the time of marriage was twenty-three and the age of their US husbands was twenty-five (Virden 1996). These characteristics allow us to estimate an upper bound of the share of War Brides in our data. Migrant women who arrived in the US between 1945 and 1955 and were younger than 36 at the time of arrival comprise 15.8% of the total British females and 21.6% of the German females. More recent data from the UK International Passenger Survey (IPS) indicates that, on average between 1982 and 2012, only twenty-five percent of British citizens leaving the UK between 1982 and 2012 said they were doing so primarily to join or accompany their family or (co-ethnic or other) partner. Most of them said that they migrated to work. While the IPS does not report primary reasons by country of destination, the relative strength of the US economy suggests that UK immigrants probably come to the US to work. The same logic applies to immigrants from Germany.

similar to the distribution of education of wives, thus hinting the potential for marital sorting. The data also show that migrants and their American spouses are generally more educated than natives still living in the home country, suggesting that migrants are a more selected sample of their native population in terms of education.

As noted above, to correct educational mobility for marital selection we rely on variation in public spending on education during the years each person was of primary and secondary school age.[4] We collect data on the amount local, state, and federal governments spent on education at all levels as a percentage of GDP from Chantrill (2011a) for the UK, and from Chantrill (2011b) for the US. For Germany we get the data from Diebolt (1997) for periods 1920-1937 and 1950-1989 and from Eurostat for the period 1990-2009. For the war period 1939-1950 in Germany we estimate public spending on education using out-of-sample predictions from a simple regression of the German public spending on education on the US public spending on education.

To correct the probability of migrating to the US for potential migrant selectivity we rely on time-varying information on the number of immigrants who arrived in the US in the years each person was age 16-21 and 22-30. We obtain this information from the US Yearbooks of the Immigration and Naturalization Service. We measure the average inflow during late puberty and early adulthood because this is when people likely start to think about and potentially begin to form plans to migrate. We separately measure inflows for the earlier and later age-periods to capture variation that occurs when people make their education and labor market participation decisions, respectively. We use flows rather than the stock of British immigrants in the US because we could find no consistently defined time-series data that measures the stock.

To show how these data vary over time and age, we plot, in Figures 1 and 2 education spending by country and inflows of British and German migrants to the US. Figure 1 plots the raw data series across calendar years and Figure 2 plots the data for our analysis sample after they are assigned to each individual. Specifically, Figure 2 orders individuals along the

[4]Ideally, one would like to use a more disaggregated measure, e.g. by level of education or geographic region/state. Long time-series on such disaggregated variables are not available. Snyder and Dillow (2011) provide separate data series on public and private education spending in the US by level of education from 1970 to 2010. Using those data we find that the correlation between total private and public spending and between spending in primary/secondary schools and post-secondary institutions exceed 0.9.

horizontal axis by the age they were at the time of the survey and plots on the vertical axes the mean spending during the years a person was of school-age and mean migration inflows to the US during the years a person was ages 16-21, and 22-30. Both instruments vary across individuals of different ages and across individuals of the same age who were interviewed in different calendar years.

Finally, we include measures of the average per-capita GDP in the US, the UK, and Germany during the years each respondent was a child, a teenager, and young adult. Apart from predicting educational attainment, GDP is often used in the classic push-pull migration framework to control for the effect of economic development on an individual's decision to migrate. That approach posits that unfavorable (economic, political, and social) conditions in the home country push people to the host country, while favorable conditions in the host country pull people from their home country. We draw these data from Maddison (2006).

Because we pool data from repeated cross-sections from the host (US) and home countries (UK and Germany), we construct new sample weights so that our pooled samples are representative of the population in the home country in the year of the interview. We construct population weights with population data by year (of survey), age and sex from the the World Health Organization mortality database.[5]

3 Empirical strategy

The long route to identifying the interdependencies between migration and educational marital mobility is to estimate, as jointly dependent, (i) the investment in education, (ii) the probability of migration, (iii) the timing of migration, (iv) the probability of marriage, (v) the timing of marriage, (vi) the choice of spouse based on education, and (vii) the choice of spouse based on nationality. Because we could not find the full set of data one would need to estimate this structural system (especially data on the timing of marriage), we condense it to two equations that model the probability to migrate and the joint probability of marrying and marrying a

[5]For each sex s, year t, and age-group k, we calculate population weights as: $(population_{stk}/population_{st})/(sample\ size_{stk}/sample\ size_{st})$.

native with higher education. A number of factors makes us confident that our model works well to answer the questions of interest while at the same time it benefits from simplicity. First, as we showed in Table 1, while migrants are a bit less likely to have ever been married than non-migrants, the difference is less than two percentage points. This makes us confident that differences in the probability of marriage by migration status will not significantly bias our results. Second, our instrument for marital mobility addresses both educational and ethnic homogamy and nets out differences in educational attainment that might have occurred either after a person married or in anticipation of future migration. Third, our instrument for the probability of migration addresses its endogeneity with the probability of marriage, and it also makes the relative timing of the decision to marry and migrate largely irrelevant. We provide detailed explanations below.

Let E_i^* be the latent variable that denotes the desired level of education of individual i. E_i^* is continuous but unobservable. We observe only the actual choice E_i of the individual which is censored into C educational alternatives of increasing levels, with $c \in \{1, 2, ..., C\}$. The observed censored variable is a function of the latent variable, such that: $E_i = c \;\; if \;\; \psi_{c-1} < E_i^* < \psi_c, \; \psi_0 = -\infty, \; \psi_C = +\infty$. Using the values of E_i we can define marital mobility M_i to equal 1 if a person's education is less than the education of his/her spouse and 0 otherwise. Formally, we set:

$$M_i = \begin{cases} 1 \quad if \; E_j > E_i \\ \\ 0 \quad if \; E_j \leq E_i \end{cases} \quad where \; j \; is \; the \; spouse \; of \; i. \tag{1}$$

Our goal is to evaluate whether a person who migrated to the US and married a US native is more or less likely to experience marital mobility than a person who did not migrate and married a fellow non-migrant. That is, we want to know whether $Prob(M_i = 1 \mid I_i = 1) \lesseqgtr Prob(M_i = 1 \mid I_i = 0)$, where $I_i = 1$ if a person immigrated to the US. To answer that question we model the joint probability of marrying and marrying a person with higher education, as follows:

$$Prob(M_i = 1) = \alpha_0 + \alpha_1 I_i + \sum_k \alpha_{2k} X_{ki} + \epsilon_i \tag{2}$$

where X denotes K exogenous variables; α are parameters to be estimated; and ϵ denotes a normally distributed error term. We use a standard probit model to estimate equation (2), by gender, on the pooled sample of migrants in the host country and non-migrants in the home country. The value $\widehat{\alpha}_1$ that we obtain is a 'naive' estimate of the migration effect on mobility which is potentially biased because of two types of selection. Specifically, the probability of marrying-up may differ by migration status (i) if migrants select their spouses based on a different set of unobserved characteristics, or they have a different degree of marital selectivity, relative to non-migrants (differential assortative mating bias); and (ii) if migrants self-select into migration based on unobserved characteristics that affect their choice of spouse or their own marital appeal (migration selection bias). To find the causal effect of migration on marital mobility, we need to remove both types of bias.

The empirical literature that developed to test Becker's predictions on assortative mating typically estimates the degree of marital sorting using earnings regressions from samples of married couples. Controlling for observed factors and characteristics, such as schooling, age, and work experience, the literature interprets the correlations of the ensuing residuals of the spouses as indexes of marital selectivity. A set of studies use post-marriage earnings and characteristics (Zimmer 1996, Zhang and Liu 2003), while others use pre-marriage earnings and characteristics (Zimmer and Nakosteen 2001, Nakosteen, Westerlund, and Zimmer 2004). The purpose of this latter approach is to net out the effect of post-marriage developments that may cause spouse wages to converge or diverge. In the spirit of this approach, we estimate educational attainment observed after marriage using variation from an instrumental variable which is not only measured before marriage but it is also independent of one's expectation to migrate; i.e, public spending on education averaged over the years each person was of school age. Our choice of instrument relies on the premise that higher budgetary allocations are effective at improving educational outcomes. Although international evidence does not always support this assumption (see Hanushek 2003), recent research suggests that the relationship between public education spending and educational outcomes is positive and statistically significant in countries with good governance (Rajkumar and Swaroop 2008). All three countries we study here fall into this category. As is standard in the literature, we specify education spending as a share of GDP while keeping GDP per capita constant, thus controlling for economic cycles.

We estimate the following model of demand for education:

$$E_i = \theta_0 + \theta_1 Y_i + \sum_k \theta_{2k} X_{ki} + \phi_i \tag{3}$$

where Y is the instrumental variable; θ denotes parameters to be estimated; and ϕ denotes a normally distributed error term. The estimated residuals of (3) embody traits that influence not only the individual's potential for educational attainment but also his attractiveness to potential spouses. For example, a large positive residual may reflect a range of traits that are visibly appealing such as exceptional ambition, mental and physical health, confidence, favorable socioeconomic family background, the ability to contribute to home production, and a range of cultural or ethnic characteristics. The effects of such traits are not present in the predicted values of (3) because these rely on variation in spending that is unrelated to characteristics (other than education) that make an individual an attractive spouse. As importantly, our instrument is also independent of changes in educational attainment of either spouse which happened due to marriage (e.g. by resources or information sharing between spouses), and of investments in education of forward-looking individuals who anticipated access to a different marriage market because they planned to migrate.

Because E_i has an ordered form and the error in the latent model is assumed to be normally distributed, we can estimate the parameters by ordered probit. We run this regression on separate samples by country of residence in the survey year. We do not estimate this equation separately by sex or by immigrant status because we want to make sure that the resulting predicted values $\widehat{E}_i$ draw from the same distribution and can be compared across spouses. We use these values to define a new measure of marital mobility:

$$\widehat{M}_i = \begin{cases} 1 & if\ \widehat{E}_j > \widehat{E}_i \\ 0 & if\ \widehat{E}_j \leq \widehat{E}_i \end{cases} \tag{4}$$

We then re-model the joint probability of marrying and marrying a person with higher education using $\widehat{M}_i$ as the dependent variable, as follows:

$$Prob(\widehat{M}_i = 1) = \beta_0 + \beta_1 I_i + \sum_k \beta_{2k} X_{ki} + \varepsilon_i \tag{5}$$

As before, we estimate equation (5) by gender on the pooled sample of migrants and non-migrants using probit regression. The value $\widehat{\beta}_1$ that we obtain is now net of marital selectivity effects, but it is still potentially contaminated with migration selection effects.

Our last step is to address migrant selectivity by modeling the probability that a person migrates. For this exercise, we use the network of previous immigrants as an instrument that affects the migration decision but is orthogonal to marital mobility. We assume that the network of migrants affects the migration decision because it is correlated with the cost of migration. It is easier for newly arrived migrants to navigate a new culture if they can tap into a larger migration network that may provide advice on getting a visa, travel information, housing and financial support, help with the host language, and help with navigating local government bureaucracies and other services (Carrington at al., 1996; Bauer et al., 2002; Munshi, 2003). The network of migrants is orthogonal to our measure of marital mobility because we only look at cross-national marriages. We plausibly assume that the size of a given migrant community in the US is not correlated with the probability that a migrant who belongs to that community marries a US native who is more educated than the migrant himself. Our instrument also addresses potential simultaneity in migration and marriage that arises in those cases when marriage took place in the country of immigrant origin and before the migration decision. Those British and German immigrants who met and married their US spouses in their home country (e.g. the War Brides) are unlikely to have based their migration decision on networks of co-ethnic migrants.[6] Our instrument, therefore, safely separates the association between migration and marital mobility that arises because migrants marry US spouses in order to migrate to the US (migration selection effect) from the association that arises because migrants marry US spouses as a result of migrating to the US (causal effect of migration).

With Z denoting our instrument variable, we specify the probability of being a migrant as follows:

$$Prob(I_i = 1) = \vartheta_0 + \vartheta_1 Z_i + \sum_k \vartheta_{2k} X_{ki} + \varphi_i \tag{6}$$

This allows us to re-model the joint probability of marrying and marrying a person with higher education in two further ways:

$$Prob(M_i = 1) = \gamma_0 + \gamma_1 \widehat{I_i} + \sum_k \gamma_{2k} X_{ki} + \nu_i \tag{7}$$

$$Prob(\widehat{M_i} = 1) = \delta_0 + \delta_1 \widehat{I_i} + \sum_k \delta_{2k} X_{ki} + \upsilon_i \tag{8}$$

Using bivariate probit, we estimate equations (6) and (7), and equations (6) and (8), as systems of simultaneous equations with jointly determined errors, where γ and δ are the respective structural parameters.[7] Under instrument validity, $\widehat{\gamma_1}$ and $\widehat{\delta_1}$ capture the effect that being a migrant would have on the probability of marrying a more-educated spouse if the migration

[6] For example, researchers have documented that the War Brides, who probably form the majority of migrants in our sample who entered the US married, did not move into an existing immigrant population or settle in ethnic enclaves. Rather, they were welcomed by, and often moved in with, the families of their husbands. The war brides did not rely on co-ethic immigrant networks even for help with basic practicalities of their migration process. They received advice and assistance with paperwork by the American Red Cross and ofter their transportation to the U.S. was arranged and paid by the U. S. government (Virden 1996).

[7] Alternative to using IV methods, one can also estimate our structural model with matching techniques. We decided against using matching because we had very few proxy variables available. To be appropriate for our empirical exercise, proxy variables should affect both the decision to migrate and the decision to marry a spouse of a given education level, but they should not be affected by the decision to migrate (Rosenbaum and Rubin, 1983). Because migration likely affects many of the socioeconomic characteristics of individuals that are measured post-migration (e.g. household size, income), only a few of the available variables can serve as proxies; e.g., age and race. Relying on such proxies to carry out the matching estimation would likely violate a key aspect of the strong ignorability assumption; i.e., that, after controlling for the proxies, marital mobility should be independent of the selection into migration. We also prefer IV estimation over matching because, even when good proxies and good instruments are available, evidence suggests that the IV method outperforms the matching method (see, for example, McKenzie, Stillman, and Gibson 2010).

decision solely depended on migration networks. The coefficient $\widehat{\gamma}_1$ is net of migration selectivity effects, but it is still potentially contaminated with marital selection effects. The coefficient $\widehat{\delta}_1$ is net of both migration and marital selectivity effects and reflects the causal effect of migration on marital mobility.

Linear combinations of the coefficients in equations (2), (5), (7), and (8) provide estimates of the degree and the direction in which marital and migration selectivity change the causal effect of migration on marital mobility. Specifically, differences $\widehat{\alpha}_1 - \widehat{\beta}_1$ and $\widehat{\gamma}_1 - \widehat{\delta}_1$ approximate the marital selection effect, while differences $\widehat{\alpha}_1 - \widehat{\gamma}_1$ and $\widehat{\beta}_1 - \widehat{\delta}_1$ approximate the migration selection effect. In both cases, the implied linear cross-model restriction is $\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1 = 0$. To test this restriction, we re-estimate (2), (5), (6) and (7), and (6) and (8) as a system of equations, where we combine the parameter estimates and associated (co)variance matrices into one parameter vector and one simultaneous (co)variance matrix.

4 Results

4.1 The migration effect on educational mobility through marriage

In Table 2 we showed that British and German migrants who marry US natives and live in the US are generally more educated than British and German natives who never migrate and marry non-migrants. Further, the US natives who are married to British and German migrants are also more educated than the British and German natives who never migrate. To further examine these patterns, in Table 3 we present indicators of assortative mating and marital mobility (M_i) by immigrant status and sex. The data show that the correlation of the educational attainment between spouses is highest for German natives (0.57), and in all other cases it is lower and roughly equivalent (about 0.39-0.46). However, women are more likely to marry more educated spouses than men are, irrespective of their migration status. Specifically, the marital mobility rate for all migrant women and British native women is 37-40%, whereas the corresponding rate for all migrant men and British native men is only 22-26%. For German natives the mobility rates are relatively lower, consistent with the higher degree of assortative mating, but again women are more likely to marry more educated spouses than men are (with probabilities 0.24 and 0.20, respectively).

To what degree are the above patterns due to assortative mating? To answer this question we calculate new measures of assortative mating and marital mobility using the residuals and the predicted values from the probit estimates of equation (3). We estimate this equation separately on the pooled CPS data of US natives and British and German immigrants, the BHPS data of British natives, and the SOEP data of German natives. Table 4 presents the results. In all samples, respondents attained more education if, during the years they were of school age, their government spent a larger share of GDP on education (holding GDP per capita constant). The estimated effect is higher in the UK than in Germany and the US. However, while attained schooling rises when governments spend more in either early or later schooling years in Germany and the UK , in the US attained schooling is higher only when the government spends more when people are of primary-school age.

In Table 3 we use the residuals for each partner in a couple from (3) and show that their correlation coefficients are positive and sizable. Recall that these residuals measure attained education that is not explained by public education spending, thus the correlations suggest the presence of positive assortative mating on the basis of education across all groups. We also use the residuals from (3) to compute net marital mobility ($\widehat{M_i}$) (given by equation (4)), which measures prevalence of people who married a more educated spouse after removing variation in education predicted by public education spending. The results suggest that, if spouses had not selected each other on the basis of traits correlated to their educational attainment, then a larger share of migrant men and women, and of non-migrant women would have been married to more educated spouses. By contrast, a smaller proportion of non migrant men would have been married to more educated spouses. Among all groups, non-migrant British and German women would be the most mobile through marriage, since 86 percent of them would marry a more educated spouse. Non-migrant British and German men would be the least mobile through marriage, since only 15-16 percent of them would marry a more educated spouse.

These patterns suggest that there may be gender-specific differences in how migration affects marital mobility. To obtain clearer evidence on this, in Table 5 we explicitly test whether and to what degree migration determines the marital mobility of British and German men and women. Table 5 presents the coefficients on the migration indicator from equations (2) and (5) that are estimated on: (i) the sample of British immigrants who are married to US natives and live in the US (from the CPS data) pooled together with the sample of British natives who are married to compatriots and live in the UK (from the BHPS data); and (ii) the sample of German immigrants who are married to US natives and live in the US (from the CPS data) pooled together with the sample of German natives who are married to compatriots and live in Germany (from the SOEP data). In the first and third columns marital mobility is defined as M_i (raw) and in the second and fourth columns it is defined as $\widehat{M_i}$ (estimated).

In columns (1) and (3), when we do not adjust our mobility measure for the effect of marital sorting, we find that German migrants are more likely to marry up relative to their non-migrant counterparts (though for German men the differences are small), whereas British migrants are as likely to marry up as their non-migrant counterparts. When we purge out the marital sorting effect we find that, marital mobility is strongly associated with migration and the effects are similar for migrants from both countries. Men who migrate are much more likely to marry a more educated woman while women who migrate are much less likely to marry a more educated man. In other words, there is something about the way migrants select their spouses that induces men to marry down and women to marry up. Had there been no marital sorting, migration would favor all migrant men relative to non-migrant men and it would disfavor all migrant women relative to non-migrant women. In fact, the size of this effect would be substantial. The results suggest that if all British men had stayed in the UK, then only 15% of them would have married up, whereas in the extreme case that all of them had migrated to the US then 68% of them would have married up. The corresponding effects for German men are 16% and 89% respectively. Conversely, if all British women had stayed in the UK, then 86% of them would have been married to more educated spouses, whereas if all British women had migrated to the US, only 59% of them would have been married to more educated spouses. The corresponding effects for German women are equally sizable; 86% and 64% respectively.

We next attempt to disentangle whether the estimated migration effect on $\widehat{M_i}$ is due to the act of migration per se or whether it is because individuals who migrate differ in unobserved ways that affect their probability of marrying up. Table 6 reports results from the estimation of simultaneous equations (6) and (7), and simultaneous equations (6) and (8), by seemingly unrelated bivariate probit regression. As before, in the first and third columns marital mobility is defined as M_i and in the second and fourth columns it is defined as $\widehat{M_i}$.

The first-stage regressions produce positive coefficients on the instruments in both the British and the German samples. The likelihood that an individual migrates to the US increases with the mean annual inflow of compatriot migrants to the US both over the time individuals were of age 16-21 and over the time they were 22-30. Interestingly, for men the coefficients on migration inflows measured over the age of 16-21 are significantly higher than those on inflows measured over the age of 22-30 (which are not statistically different from zero), while the pattern for women is the opposite, suggesting that men form their preferences about migration at earlier ages than women. In all cases, the Wald test rejects the hypothesis that the migration decision is exogenous to marital mobility. The only exception to this is when we define marital mobility as M_i in the British sample, but in this case we also find that the effect of migration on marital mobility is statistically insignificant.

Interestingly, relying on exogenous variation in the migration decision does not significantly affect the estimated effect of migration on marital mobility in the British sample. Not only are the migration coefficients qualitatively robust across probit and bivariate probit models (Tables 5 and 6), but also the resulting marginal effects of migration on marital mobility remain very similar in scale. In contrast, instrumenting the migration decision does make a difference in most of the results from the German sample. The implication is that the difference between the naive and causal migration effects on marital mobility is driven mostly by marital selectivity in the British sample, and by both marital and migration selectivity in the German sample.

To facilitate comparison, Table 7 presents the estimated selection effects and tests of their statistical significance. Marital selection effects are negative for men and positive for women, and in all cases sizable and statistically significant. Migration selection effects are negative for both men and women, but they are weaker for the British relative to the Germans. In fact, for the British $\widehat{\beta}_1 - \widehat{\delta}_1$ is negative and significant, but $\widehat{\alpha}_1 - \widehat{\gamma}_1$ is positive and statistically insignificant. However, as for all other groups, the difference between the two is statistically zero (the Wald test fails to reject the parameter restriction $\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1 = 0$ in all cases).

4.2 Discussion of the estimated effects

Our results suggest that, because of the way they select their spouses, migrant men reduce their probability of marrying up, while migrant women increase it. It follows that there are sex and country-specific unobservable characteristics that drive marital selection, such that they disfavor migrant men and they favor migrant women. Although it could be one of many unobservables that fit this profile, preferences of spousal traits are plausibly country and sex-specific and offer an explanation for our results. Becker argued that men specialize in market production and women specialize in home production and his argument is valid even today.[8] If this entails that husbands determine the social status of the family, then men can afford to marry down without loss of socioeconomic status and can select their wives on the basis of other traits, especially traits related to home production. For example, men may select wives who are young, healthy, fertile, and can run a household. Correspondingly, if women cannot determine the social status of the family, they will prefer husbands who are well-educated, ambitious, and can earn a living. This gender difference in mate selection preferences and the resulting patterns of female *hypergamy* and male *hypogamy* has been extensively documented in the social sciences (Hadfield and Sprecher 1995, Cashdan 1996). Evolutionary psychologists claim that the difference is inherent and serves family survival, while social learning theorists claim that it appears in male-dominated societies and should fade as women gain equal rights to men.

[8] For example, the German government has, until relatively recently, set welfare and public support policies according to a "male breadwinner" model. That is, the institutional structure in German provides strong financial and social incentives for couples to divide labor so that men specialize in market production and women specialize in home production. Starting around 2000, German social policymakers began to reform institutions using a different model (Meyer 1998). For information on the US, one can look at the Panel Study of Income Dynamics (PSID) - Child Development Supplement/Transition to Adulthood (CDS-TA) surveys. The CDS-TA surveys interviews the person in the PSID household who identifies herself/himself as the 'primary' care-giver (PCG) of each child. Data from the 2002 and 2007 waves show that biological, adoptive, or step mothers comprise over 90 percent of self-identified care-givers.

In either case, this difference is consistent with our results because it implies that migrant men are at a disadvantage in the US marriage market whereas migrant women are not. Specifically, studies that examine the earning trajectories of immigrants in the US show that, although they eventually assimilate fully in the native population, during their first years in the US they suffer a large earnings penalty relative to equally experienced natives. In fact, it may take up to 20 years for this penalty to disappear (Lubotsky 2007). As a result, migrant men are less competitive in the US marriage market than in their home market and, thus, they marry down relative to their non-migrant counterparts. The earnings penalty and the delay in financial maturity is less of a problem for migrant women because they are not typically expected to be the primary bread-winners of the household. Quite the contrary, migrant women may possess advantageous traits which their US competitors do not possess. Researchers have often documented a preference of US men for immigrant wives from the old world who retain traditional patriarchal family values, as opposed to US women who adopted materialism, liberal individualism, and feminism earlier on (Honig 1998). Evidence that such preferences exist is the presence of International Marriage Brokers (IMB) - an industry which specializes in facilitating marriage between US native men and foreign women and is, essentially, the modern version of the 'mail-order bride' industry that dates back to the 1800s. From this viewpoint, migrant women are more competitive in the US marriage market relative to their home marriage market and, thus, they marry up relative to their non-migrant counterparts

Our results also suggest that, in the absence of migration selectivity, both migrant women and migrant men are more likely to marry up relative to non-migrants. However, because of the way they self-select into migration, migrant men and women reduce their probability of marrying up. This implies that unobserved characteristics that drive selection into migration (e.g. cultural characteristics, risk preferences, and language skills) make migrants willing to marry less educated US spouses in exchange for other favorable provisions. For example, US spouses may provide access to US residency and accelerate their assimilation in the US native community, e.g. by helping them finding employment. Furtado and Theodoropoulos (2009, 2010) show that, indeed, cross-national marriage increases employment rates for immigrants, and does so not only because of the legal status acquired through marriage but also through native networks. Our finding that migration selection effects are weaker for British than for German migrants is consistent with this explanation. It is arguably more difficult for German migrants to assimilate in the US than it is for British migrants, since British immigrants are both culturally and linguistically more similar to US natives than German migrants. Likewise, the natives' sentiments towards immigrants also depend on cultural and linguistic proximity. For example, a nationwide poll in 1944 about which migrants the U.S. should allow to enter the country showed that British immigrants were at an advantage, topping the list of preferred foreigners with 68% positive votes. In that same poll, German immigrants ranked 7th, gathering only 36% positive votes (Simon and Alexander 1993). Reports from German War Brides also reveal that in the years following the end of WWII German immigrants faced discrimination and were occasionally harassed as "Nazis" (Shukert and Schibetta 1988). In contrast, British brides reported to have experienced hardly any discrimination after they arrived in the country (Virden 1996).

The aforementioned selection effects mask the causal effect of migration on marital mobility. After we purge out the selection effects, we find that the causal effect of migration is positive for men and negative for women - a finding which, at first sight, seems contrary to expectations. One would expect that the causal effect of migration would be positive for all migrants because mean educational attainment in the US has been consistently higher than in Germany and the UK over the period that the individuals in our sample were moving to the US and getting married. We show this clearly in Figure 3 using data from Barro and Lee (2012). The data suggest that, by moving to the US, all migrants got access to a marriage market where the average candidate spouse was more educated than in the home marriage market.

The gender difference in our causal estimates can be explained if the gap in the timing of marriage differs between migrant and US native women. Because the surveys that we use for the analysis provide no information on the timing of marriage, we obtained relevant information from the US census.[9] The 1980 wave of the US census reports data on the age at first marriage

[9]Available at the international online database of Integrated Public Use Microdata Series (IPUMS).

and country of birth of each surveyed individual. Using these data, Figure 4 compares kernel density estimates of the age at first marriage across British and German migrants and US natives and clearly shows that both migrant men and migrant women get married around two years later than US natives and that US women marry in a narrower time window than all others. From Table 2, we also know that women marry older men and men marry younger women. From this it follows that, if there was no selectivity at all, migrant men could take advantage of the higher availability of educated partners in the US because their target-group of potential US wives would not decrease as they delayed their marriage. In contrast, migrant women would miss the window of opportunity to marry up since their target group of potential spouses would decrease both in size and in quality as they delayed marriage.

The observed difference in the age of first marriage between migrants and US natives could be either because of country-specific norms, or because the decision to migrate further delays their marriage. Data from the United Nations Economic Commission for Europe (UNECE) Statistical Database suggest that, while both of them are true, country-specific norms seem more important for women. These data show that in 1980 the mean age at first marriage in the UK was 23 for women and 25.3 for men. The corresponding numbers for British immigrants from the IPUMS database are 23.4 for women and 26.2 for men. Similarly, in 1980 the mean age at first marriage in Germany was 23.4 for women and 26.1 for men. The corresponding numbers for German immigrants from the IPUMS database are 23.5 for women and 25.8 for men. These data suggest that migrant British women and migrant German men and women follow closely the norms regarding the timing of marriage from their country of origin. Migrant British men delay their marriage decision by approximately an extra year relative to non-migrants.

4.3 Tests of performance and robustness

Some aspects of our analysis may cause concern. First, although the benefits associated with educational attainment and, by extension, with educational mobility through marriage may differ across countries, our analysis implicitly assumes that they are comparable. For example, we assume that a German who has a high-school degree will be better off migrating to the US and marrying a US native with a post-secondary qualification than not migrating and marrying a compatriot who also has a high school degree. In reality, whether or not this is true depends on the monetary and non-monetary returns to a German high school degree relative to the returns to a post-secondary qualification in the US. While we acknowledge this limitation, we expect that, in our sample, sex-specific country differences in the returns to education (monetary and social returns combined) are not as prevalent across aggregate educational levels as they are across types of education (e.g. vocational vs. general) and fields of specialization (e.g. humanities vs. sciences) within aggregate educational categories. If these latter differences are not systematic, at the mean of each educational category they will tend to cancel out.

To obtain supporting evidence on this we test the robustness of our estimates to two more conservative measures of mobility. First, we collapse the education categories from the five used so far down to three (primary, secondary, and higher) and re-calculate mobility using our standard definition (i.e. we set mobility to equal one if education of spouse>education of self, and zero otherwise). Second, we use the original five educational categories but set mobility to equal one if the education of the spouse is higher than the education of self plus the sample variance in educational attainment.[10] Both new measures of mobility are more restrictive and thus more plausibly comparable across countries than the one we used in the baseline analysis. We find that the results are highly robust when we use these alternative measures. For brevity, we confine ourselves to reporting the averages of the new mobility measures and the estimated selection effects (the full set of results are available upon request). In Table 8 we show that, while the size of the selection effects differ in absolute value, the results remain qualitatively robust (though the estimates become noisier for German females).

Our identification strategy may also cause some concern. The migration inflows which we use to identify migration selectivity are aggregations of individual behavior which (depending

[10]Using the sample variance in this way allows for equivalent shifts in raw and estimated mobility. Further, because the variance of educational attainment is higher than one, the definition of raw mobility requires a gap of at least one educational category between spouses. An individual with primary education is mobile if s/he marries a spouse who has at least an upper secondary education, an individual with lower secondary education is mobile if s/he marries a spouse who has at least post-secondary education; an individual with upper-secondary education is mobile if s/he marries a spouse with at least tertiary education; and individuals with post-secondary or tertiary education are never mobile.

on the age each individual migrated) may include the migrants in our sample. Because of this, the predictive power of our instruments may reflect exogenous correlated effects (Manski 1993, 2000). That is, it may reflect that the migrants in our sample and the migrants in our instrument may decide to migrate because they have unobserved similar characteristics or because they are exposed to the same institutional or contextual factors ('Manski's reflection problem'). To partly account for such unobserved common factors in the models reported thus far, we control for ten or five-year birth-cohort fixed effects. Because our instruments vary by year of birth, including single-year cohort dummies would entail perfect multicollinearity. As a robustness test, we now switch to using a full set of age and survey-year dummies, which correct for exogenous correlated effects to the extend that such effects vary across the age and survey-year dimensions.

For completeness, we re-estimate both the probit and the bivariate probit models using the new fixed-effects specification. We present the resulting probit estimates in Table 9 and the bivariate probit estimates in Table 10. In all cases, the estimates remain qualitatively robust. Quantitative differences are most apparent in the instrument coefficients, which increase in economic and statistical significance in all cases apart from German males. In that sample, the coefficients on migration inflows measured over the age of 22-30 become higher than those on inflows measured over age 16-21, which are now statistically equal to zero. These changes in the instrument coefficients also spill over to the effect of migration on marital mobility, which becomes higher for women and somewhat lower for males. The most notable differences are in the sample of British women, where the migration effect on raw mobility becomes positive and significant, and in the sample of German females, where the migration effect on estimated mobility remains negative but is now highly significant. On the whole, however, the inclusion of the fixed effects does not alter the main patterns in the results.[11]

[11]The reason why we present the fixed-effects specification as part of our robustness analysis and not as our main result is a practical one. The fixed-effects specification causes separation problems so that the bivariate probit does not achieve convergence to a maximum likelihood. For this reason, to estimate the fixed-effect specifications in many cases we had to change the set of controls in our models (compare notes of Tables 5 and 9). Importantly, in all cases, when we include fixed-effects we are unable to jointly estimate equations (2), (5), (6) and (7), and (6) and (8), and thus to conduct the tests for the parameter restrictions. Similar problems with bivariate probit estimations have been reported by other researchers (e.g. Freedman and Sekhon 2010).

A further source of potential concern is that the bivariate probit regressions provide no diagnostics for instrument performance. At times, economic studies that use bivariate probit obtain diagnostics from 2SLS estimates (see, for example, Evans and Schwab 1995). While the 2SLS estimation provides the opportunity to thoroughly test the validity and explanatory power of the instruments, it is not the appropriate method to use when the dependent variables are binary. The incorrect assumption of linearity for a relationship which is in fact non-linear will yield least squares estimates that have no known distributional properties (so that statistical inferences are unreliable), are sensitive to the range of the data, may substantially mis-estimate the magnitude of the true effects, and systematically produce probability predictions outside the 0-1 range. For these reasons, although we present 2SLS diagnostics, we do so with reservation.

To test that our instruments can be plausibly excluded as direct determinants of educational mobility through marriage, we calculate the Basman/Sargan X^2 statistic under the null that they are uncorrelated with the error term. To test whether our instruments have weak explanatory power, we calculate the F statistic under the null that the instruments are jointly statistically insignificant. Finally, we calculate the Wooldridge's robust score test under the null that the migration decision is exogenous to marital mobility, which is equivalent to the Wald test in the bivariate probit regression. Table 11 presents the instrument coefficients from the first-stage 2SLS estimates along with the diagnostic statistics. In all cases, the estimates are qualitatively robust in comparison to the ones produced by the bivariate probit and the diagnostic tests generally corroborate the good performance of the estimations. The Sargan test results indicate that the instruments are valid, the F-statistic is always statistically significant (though for men low enough to suggest weak identification), and the Wooldridge test fails to reject exogeneity.

A number of patterns in these results add to our reservation about the linear probability model. First, the estimates suggest that if the mean annual inflow of immigrants to the US increases by 10,000 during the youth of British and Germans, then the probability that they will migrate to the US increases by between 0.3 and 2.5 percentage points. Albeit plausible, the OLS coefficients appear to contradict those produced by the probit methods. For example, these coefficients are higher for British men than German men, while the bivariate probit estimates suggest the opposite. It is, therefore, plausible that the least squares method fails to capture important non-linearities and, thus, underestimates the true effect of migration networks on the migration decision of German men. In turn, this would also explain why the F-statistic appears weak. Further inconsistencies between the linear and non-linear models appear in the results of the exogeneity test. Unlike the Wald test of the bivariate probit, the Wooldridge test produced after the 2SLS procedure fails to reject exogeneity of the migration decision in the British sample when mobility is measured as M_i, even though the migration effect on marital mobility is statistically insignificant. [12]

5 Conclusion

In this paper we have tested whether the decision of British and German individuals to migrate to the US and marry a US spouse provides them with better opportunities for educational mobility through marriage. Our analysis showcases that migration and marriage are jointly determined decisions which involve complex selection mechanisms. We show that, by migrating to the US, British and German migrants access a marriage market that offers more opportunities for educational mobility through marriage relative to their respective marriage markets in the home countries. However, this does not guarantee higher marital mobility rates for migrants relative to non-migrants.

A number of factors work against the positive prospects of the US marriage market. First, migrant women take little advantage of the availability of more educated candidate husbands in the US because they marry later than native US women. Thus, absent any selection effects, they miss the window of opportunity to catch the 'good' spouses, and they end up with the 'lemons'. Further, unobserved characteristics that drive marital sorting disfavor mobility for migrant men and favor mobility for migrant women - a result that can be explained if men specialize in market

[12] Horrace and Oaxaca (2006) show that bias and inconsistency in the OLS estimators of the linear probability model (LPM) increase with the share of LPM predicted probabilities that fall outside the unit interval. In the models we report in Table 11, the sample share of the predicted probabilities that lie outside the unit interval is 3.9% for British males, 1.4% for British females, 1.1% for German males, and 1.9% for German females.

production and select spouses with traits that help in home production, while women specialize in home production and select spouses with traits that help in market production. Under this scenario, migrant men who face an earnings penalty in the US are at a disadvantage in the US marriage market, while migrant women who may possess competitive non-marketable traits (e.g. more traditional values) may be at an advantage in the US marriage. In addition, we find that unobserved characteristics that drive selection into migration disfavor mobility for both migrant men and women, suggesting that immigrants may be willing to marry-down in order to marry-in. That is, they may exchange education of their US spouses for other benefits, such as help with assimilation in the US native community or access to US residence. The end-product of the above effects is that migrating to the US and marrying a US native pays off (in terms of educational mobility through marriage) for the Germans but not for the British.

These results provide a basis for taking into account immigrant selectivity when designing immigration policy. For example, if the objective is to encourage skilled immigration, policy-makers may consider loosening the rules for granting legal status to migrant spouses of natives. If the objective is the social integration of immigrants, policy-makers may consider programs which could act as substitutes for native contacts, such as programs that help immigrants to find a job.

Of course, our results cannot be generalized. Our analysis has relied on immigrants who marry natives in the host country and has overlooked those who marry co-ethnics. Marital and migration selection effects are likely different for this latter group of immigrants, and would be worth exploring. Our analysis has also relied on immigrants from two European countries which differ in culture and language but are fairly similar in other important dimensions. For example, travel and visa costs from Britain and Germany to the US are fairly similar. Moreover, the economies of both countries have been growing at roughly similar rates in the same period of time. In fact, their growth patterns have followed closely those of the U.S. Further work in this area should examine immigrants from less developed countries and immigrants who face varying entry costs. Finally, because of data problems, our measure of marital mobility has been one-dimensional. Although education is widely used as an key indicator of socio-economic status, monetary measures of well-being have more straightforward economic interpretation. Thus, an interesting question for future research, given that appropriate data become available, is whether the causal effect of migration on marital mobility is sensitive to alternative definitions of mobility, e.g. in terms of income, wealth,or occupation.

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Appendix: Tables and figures

Table 1: Share of ever-married population over age 40 by country, sex, and migration status

	All	Males	Females	Source
British immigrants in the US	94.99	93.85	95.80	CPS
German immigrants in the US	94.46	91.75	96.00	CPS
British natives in the UK	93.67	92.89	94.34	BHPS
German natives in Germany	92.24	91.31	93.09	SOEP

Table 2: Weighted means and frequencies of selected variables

	Migrant/native couples in US				Native couples in home country	
	Migrant husbands	US wives	Migrant wives	US husbands	Husbands	Wives
A. British						
Age	44.0	42.2	54.6	56.5	48.1	46.0
Non-whites	.004	.020	.014	.039	.029	0.027
Education completed						
primary	0.71	0.48	2.44	1.28	19.06	22.77
lower secondary	1.12	1.07	2.62	3.07	7.66	8.87
upper secondary	20.87	23.40	39.44	25.32	27.10	28.92
post-secondary	24.92	19.90	28.50	25.54	31.97	26.93
higher	52.38	55.15	27.01	44.79	14.21	12.52
Household size	2.95	2.95	2.65	2.65	3.01	3.01
Observations	1159	1159	1422	1422	34141	34141
B. German						
Age	55.9	52.6	59.9	61.0	49.1	46.6
Education completed						
primary	2.65	1.58	4.07	1.67	2.07	2.61
lower secondary	3.34	4.68	3.36	4.98	44.63	42.78
upper secondary	21.31	21.68	46.26	30.27	32.07	37.95
post-secondary	28.42	26.08	28.73	32.50	9.46	9.03
higher	44.28	45.97	17.58	30.58	11.77	7.62
Household size	2.69	2.69	2.48	2.48	2.94	2.94
Observations	507	507	2115	2115	94815	94815

Notes: We have created five aggregated educational categories using 13 categories from the BHPS, 16 categories from the CPS, and years of completed education from the SOEP, to avoid small sex-specific cell sizes.

Table 3: Measures of assortative mating and marital mobility

	Migrant/native couples in US		Native couples in home country	
	Migrant husband rel. to US wife	Migrant wife rel. to US husband	Husband rel. to wife	Wife rel. to husband
A. British				
Correlation of education across spouses				
raw values	0.42	0.46	0.44	0.44
residuals from eq. (3)	0.30	0.43	0.39	0.39
Prob(mobility=1)				
raw values	0.24	0.40	0.26	0.37
based on eq. (4)	0.71	0.47	0.15	0.86
B. German				
Correlation of education across spouses				
raw values	0.39	0.41	0.57	0.57
residuals from eq. (3)	0.38	0.32	0.53	0.53
Prob(mobility=1)				
raw values	0.22	0.39	0.20	0.24
based on eq. (4)	0.87	0.69	0.16	0.86

The residual of ordered probit were calculated as described by Machin and Steward (1990, pp. 346-347).

Table 4: Ordered probit regression of educational attainment on education spending

	CPS		BHPS		SOEP	
Mean ed. spending:						
over age 5-17	0.041***		0.133***		0.062***	
	[0.005]		[0.029]		[0.012]	
over age 5-12		0.045***		0.073***		0.026***
		[0.004]		[0.023]		[0.010]
over age 13-17		-0.004		0.061***		0.035***
		[0.003]		[0.019]		[0.009]
Observations	3749217	3749217	108426	108426	202029	202029

Controls: mean GDP per capita over age 5-17, sex, age fixed effects, five-year birth-cohort fixed effects.

* p<0.1; ** p<0.05; *** p<0.01.

Table 5: Correcting for marital selectivity
Coefficient on migration indicator from probit model of marital mobility

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Migrant	-0.043	1.942	0.037	-1.723
	[0.081]	[0.070]***	[0.040]	[0.063]***
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.24	0.68	0.38	0.54
Prob(being a migrant)=0	0.26	0.15	0.37	0.86
B. German				
Migrant	0.207	3.103	0.398	-1.032
	[0.070]***	[0.118]***	[0.035]***	[0.055]***
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.26	0.89	0.38	0.64
Prob(being a migrant)=0	0.20	0.16	0.24	0.86

Notes: Regressions on the British sample control for race of self and spouse; age; household size; birth cohort dummies; and average GDP per capita in the UK during age 16-21 and 22-30. Regressions on British females also control for age squared and age of spouse. Regressions on the German sample control for age of self and spouse; household size; and birth cohort dummies. Regressions on German males also control for average GDP per capita in Germany during age 16-21 and 22-30 and interactions between average GDP in the US during ages 16-21 and 22-30 and inflows of German immigrants to the US during that same age. Regressions on German females also control for average GDP per capita in the US during age 0-15 and 16-21 and average GDP in Germany during age 0-15. Huber/White robust standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Correcting for marital and migration selectivity
Selected coefficients from bivariate probit model of marital mobility

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Second-stage: Prob(mobility=1)				
Migrant	-0.055	2.513	-0.161	-1.484
	[1.073]	[0.210]***	[0.728]	[0.127]***
First-stage: Prob(being a migrant=1)				
Mean inflow of British migrants:				
over age 16-21	0.380	0.413	0.168	0.160
	[0.094]***	[0.100]***	[0.065]***	[0.056]***
over age 22-30	0.114	0.157	0.217	0.231
	[0.095]	[0.105]	[0.066]***	[0.071]***
Wald test of exogeneity	0.0001	9.864	0.075	5.292
	(0.991)	(0.002)***	(0.784)	(0.021)**
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.24	0.82	0.31	0.59
Prob(being a migrant)=0	0.26	0.15	0.37	0.85
B. German				
Second-stage: Prob(mobility=1)				
Migrant	1.494	3.975	1.397	-0.926
	[0.458]***	[0.264]***	[0.413]***	[0.722]
First-stage: Prob(being a migrant=1)				
Mean inflow of German migrants:				
over age 16-21	0.458	0.512	0.027	0.042
	[0.184]**	[0.173]***	[0.049]	[0.049]
over age 22-30	0.314	0.438	0.109	0.113
	[0.364]	[0.372]	[0.035]***	[0.056]**
Wald test of exogeneity	7.433	13.70	4.868	0.023
	(0.006)***	(0.000)***	(0.017)**	(0.879)
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.72	0.97	0.75	0.67
Prob(being a migrant)=0	0.20	0.16	0.24	0.86

Notes: Controls are as in Table 5. Huber/White standard errors are in brackets; probability values are in parentheses.

Table 7: Test of statistical significance of selection effects and parameter restrictions

		Males		Females	
		Value	Wald-test	Value	Wald-test
A. British					
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	-1.985	298.9 (0.000)	1.760	647.7 (0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	-2.567	4.950 (0.026)	1.323	3.170 (0.075)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	0.011	0.000 (0.991)	0.198	0.080 (0.782)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-0.571	13.46 (0.000)	-0.239	5.270 (0.022)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	0.582	0.300 (0.584)	0.437	0.360 (0.550)
B. German					
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	-2.896	455.6(0.000)	1.431	508.6 (0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	-2.480	23.25 (0.000)	2.323	6.340 (0.012)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-1.288	8.970 (0.003)	-0.998	6.240 (0.012)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-0.872	17.09 (0.000)	-0.106	0.020 (0.879)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	-0.416	0.830 (0.362)	-0.892	0.990 (0.320)

Note: Probability values of the Wald X^2test-statistic are in parentheses.

Table 8: Testing robustness to alternative definitions of educational marital mobility

		Males		Females	
		Value	Wald test	Value	Wald test
Education categories: primary, secondary, higher					
A. British					
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	-3.201	587.2 (0.000)	1.956	697.6 (0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	-3.631	7.850 (0.005)	1.766	3.380 (0.066)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-0.018	0.000 (0.988)	-0.040	0.000 (0.966)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-0.448	31.39 (0.000)	-0.229	2.560 (0.109)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	0.430	0.130 (0.721)	0.189	0.040 (0.842)
Prob(mobility=1)	Migrants	0.14		0.25	
	Non-migrants	0.20		0.30	
B. German					
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	-5.429	578.8 (0.000)	2.337	2155 (0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	-4.846	58.89 (0.000)	5.140	148.9 (0.000)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-1.214	5.390 (0.020)	-1.139	10.44 (0.000)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-0.631	3.420 (0.064)	1.664	82.63 (0.000)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	-0.583	1.000 (0.317)	-2.803	47.72 (0.000)
Prob(mobility=1)	Migrants	0.12		0.29	
	Non-migrants	0.08		0.13	
Mobility=1 if education of spouse> education of self+ Var(education of self)					
A. British					
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	-0.729	16.66 (0.000)	3.454	8.450 (0.004)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	-0.014	0.000 (0.991)	2.885	4.460 (0.034)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-1.019	0.820 (0.364)	-0.132	0.110 (0.744)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-0.306	1.030 (0.309)	-0.700	6.560 (0.010)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	-0.713	0.350 (0.556)	0.568	1.420 (0.234)
Prob(mobility=1)	Migrants	0.09		0.11	
	Non-migrants	0.11		0.17	
B. German					
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	-1.581	157.4 (0.000)	1.234	335.6 (0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	-1.681	12.25 (0.000)	1.836	35.00 (0.000)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-0.809	3.240 (0.072)	-1.681	41.96 (0.000)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-0.909	10.26 (0.001)	-0.909	10.26 (0.001)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	0.100	0.050 (0.818)	-0.602	4.320 (0.038)
Prob(mobility=1)	Migrants	0.08		0.14	
	Non-migrants	0.03		0.07	

Note: Probability values of the Wald X^2test-statistic are in parentheses.

Table 9: Correcting for marital selectivity using age and year fixed-effects
Coefficient on migration indicator from probit model of marital mobility

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Migrant	-0.041	1.944	0.049	-1.972
	[0.077]	[0.066]***	[0.040]	[0.074]***
B. German				
Migrant	0.208	2.834	0.390	-1.102
	[0.071]***	[0.117]***	[0.035]***	[0.043]***

Notes: All regressions control for a full set of age dummies and year of survey dummies. The regressions on the British sample also control for the variables described in Table 5. Regressions on the German sample control for household size and average GDP per capita in Germany during age 16-21 and 22-30. Huber/White robust standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Correcting for marital and migration selectivity using age and year fixed-effects
Selected coefficients from bivariate probit model of marital mobility

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Second-stage: Prob(mobility=1)				
Migrant	-0.441	2.338	0.806	-2.126
	[1.111]	[0.199]***	[0.368]**	[0.099]***
First-stage: Prob(being a migrant=1)				
Mean inflow of British migrants:				
over age 16-21	0.454	0.466	0.224	0.200
	[0.095]***	[0.099]***	[0.085]***	[0.087]**
over age 22-30	0.270	0.304	0.340	0.331
	[0.108]**	[0.119]**	[0.117]***	[0.122]***
Wald test of exogeneity	0.133	5.740	3.927	6.755
	(0.716)	(0.017)**	(0.047)**	(0.009)***
B. German				
Second-stage: Prob(mobility=1)				
Migrant	1.522	2.339	1.474	-1.731
	[0.460]***	[0.267]***	[0.384]***	[0.086]***
First-stage: Prob(being a migrant=1)				
Mean inflow of German migrants:				
over age 16-21	0.003	0.005	0.069	0.070
	[0.028]	[0.028]	[0.018]***	[0.020]***
over age 22-30	0.144	0.166	0.149	0.171
	[0.065]**	[0.069]**	[0.042]***	[0.040]***
Wald test of exogeneity	7.648	5.917	6.344	112.9
	(0.006)***	(0.015)**	(0.012)**	(0.000)***

Notes: Controls are as in Table 9. Huber/White standard errors are in brackets; probability values are in parentheses.

Table 11: 2SLS first-stage regression of Prob(being a migrant=1)

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Mean inflow of British migrants:				
during age 16-21	0.025	0.025	0.016	0.016
	[0.008]***	[0.008]***	[0.005]***	[0.005]***
during age 22-30	0.010	0.010	0.021	0.021
	[0.008]	[0.008]	[0.005]***	[0.005]***
F-test of joint instrument significance	6.588	6.588	14.66	14.66
	(0.001)***	(0.001)***	(0.000)***	(0.000)***
Sargan test of overidentification	2.352	0.097	0.495	0.007
	(0.125)	(0.755)	(0.481)	(0.931)
Wooldridge's test of exogeneity	12.82	614.9	25.57	43.61
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
B. German				
Mean inflow of German migrants:				
during age 16-21	0.017	0.017	0.003	0.003
	(0.005)***	(0.005)***	(0.004)	(0.004)
during age 22-30	0.008	0.008	0.009	0.009
	(0.006)	(0.006)	(0.003)***	(0.003)***
F-test of joint instrument significance	5.166	5.166	27.22	27.22
	(0.006)***	(0.006)***	(0.000)***	(0.000)***
Sargan test of overidentification	1.349	1.277	2.345	0.694
	(0.245)	(0.258)	(0.126)	(0.405)
Wooldridge's test of exogeneity	21.465	229.6	25.00	5994
	(0.000)***	(0.000)***	(0.000)***	(0.000)***

Notes: Controls are as in Table 4. Huber/White standard errors are in brackets; probability values are in parentheses.

Figure 1: Raw data used to derive instruments

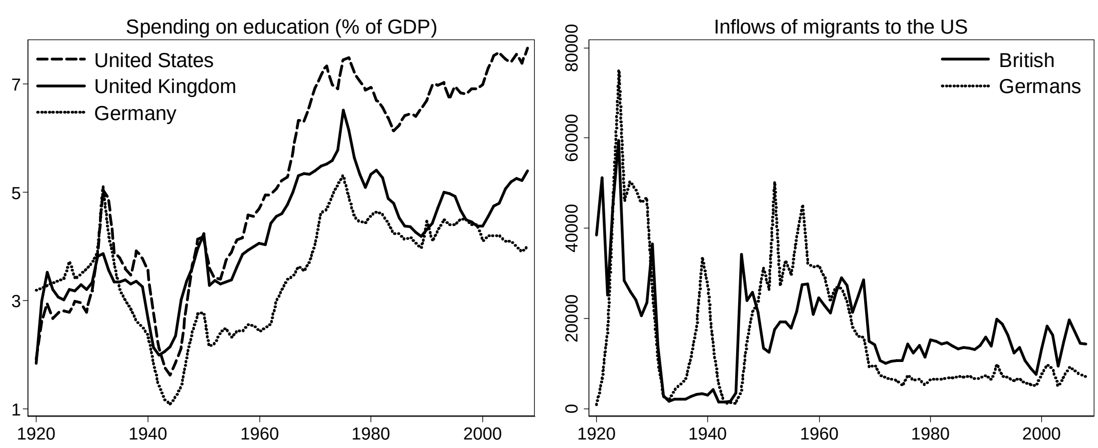


Figure 2: Scatter plots of instruments and age

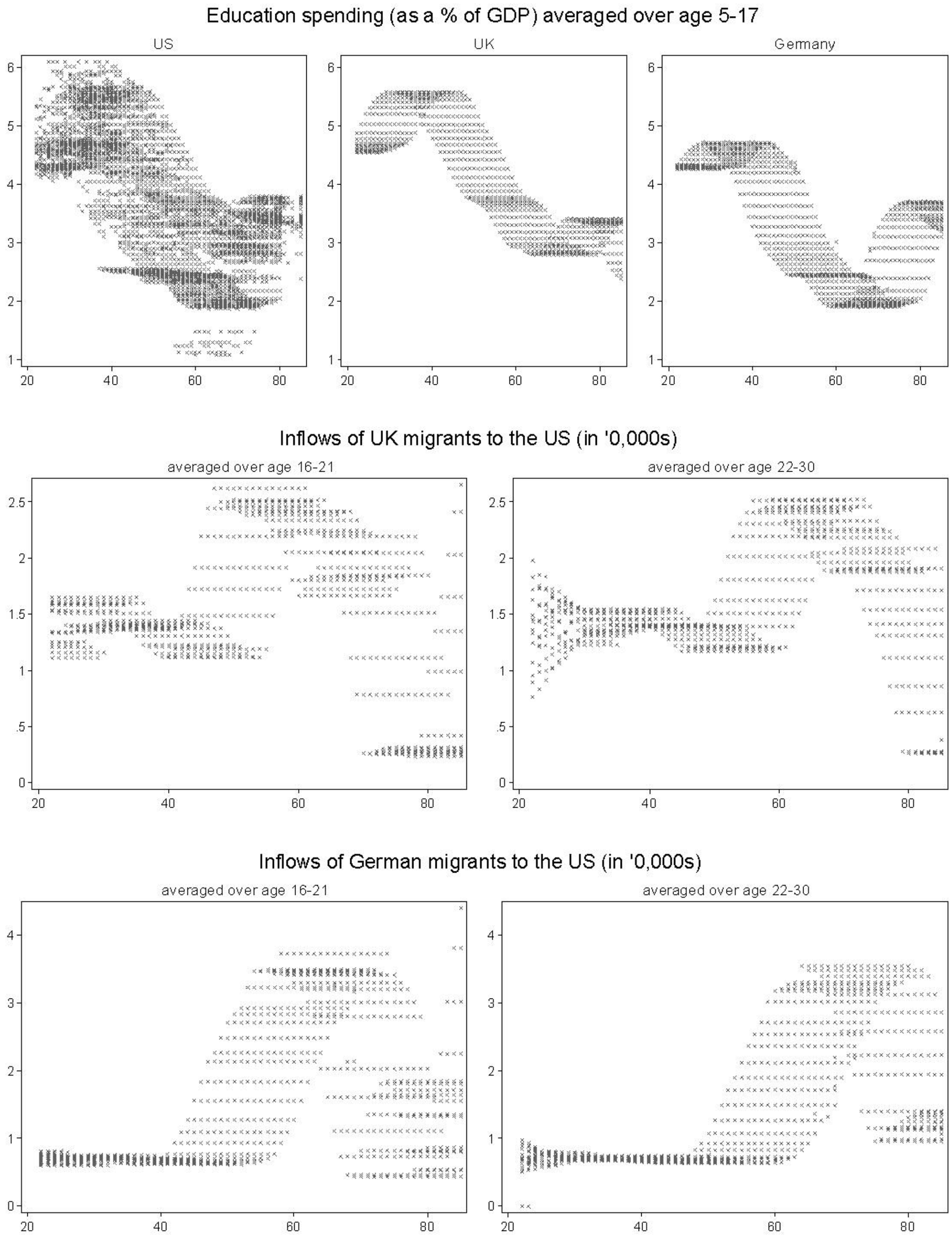
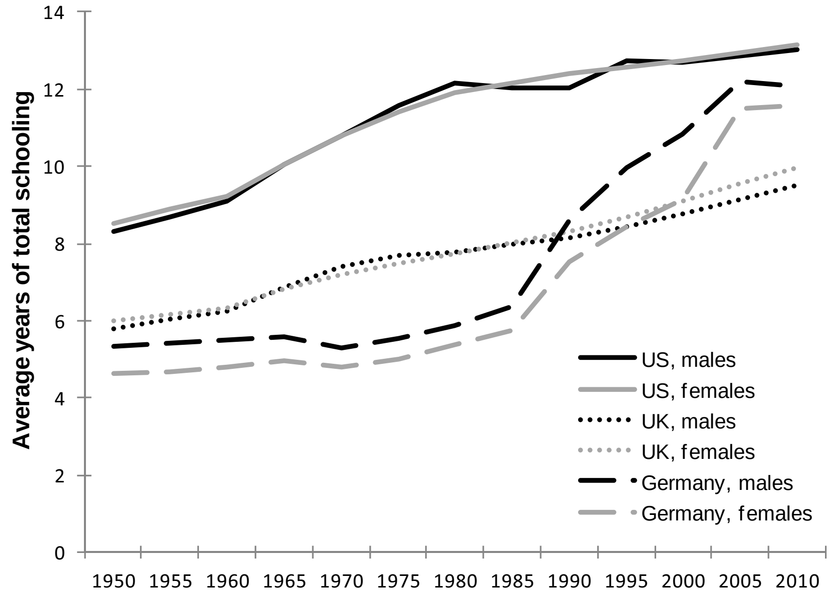
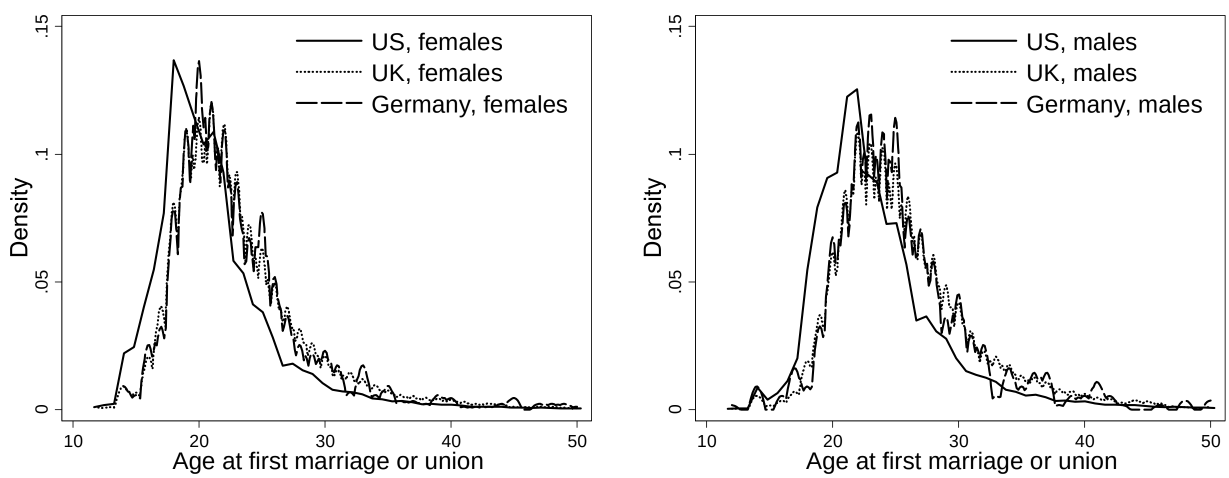


Figure 3: Educational attainment by country and sex



Source: Barro and Lee (2010)

Figure 4: Kernel density estimates of age at first marriage of US residents in 1980 by country of birth



Source: IPUMS international online database. Notes: Kernel=epanechnikov, bandwidth=0.3.