Migration to the US and Marital Mobility*

Rebekka Christopoulou[1]†, Dean R. Lillard[1,2,3], Daniel Lichter[4]

[1]*Ohio State University,* [2]*DIW-Berlin,* [3]*NBER,* [4]*Cornell University*

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Abstract

We combine survey data of British and German immigrants in the US with data of natives in the UK 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. In line with cross-country differences in the availability of educated spouses, and migrant-native differentials in the timing of marriage and financial maturity, we find that migration causes the likelihood of marrying-up to increase for women and to decrease for men. However, the way migrants self-select into migration and marriage dampens down these effects.

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†Email: Christopoulou.1@osu.edu

1 Introduction

Why people decide to migrate and marry and how these decisions affect their socio-economic status have important implications in both the home and host countries; e.g., implications about income inequality, female labor supply, the number of births and population growth, and the distribution of family resources. The extensive economics literature that studies marriage and migration emphasizes that both decisions involve high degrees of selectivity and, depending on this selectivity, they may have diverse effects on the decision-makers. An important drawback of this 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 effect on socio-economic mobility. To contribute evidence on this issue, 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 those of non-migrants who never left the home country (e.g. Abramitzky, Boustan, and Eriksson 2012). Most studies compare outcomes of migrants to outcomes of the native-born individuals or other co-ethnics living in the host country, and 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 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 select 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 with respect to wages depends on two different offsetting forces. On the one hand, there is a need for specialization in household production which generates a tendency for negative assortative mating. On the other hand, there is a need for joint consumption of household public goods which generates a tendency for positive assortative mating because there are returns to spouses having similar demands for 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).

Although the economics migration and marriage literatures have largely overlooked the possibility that individuals may take the decisions to migrate and marry jointly, other social science disciplines have long recognized that these two behaviors might be linked. The idea is that a person may migrate because it puts him in a marriage market with a bigger supply of 'good' potential spouses, or he may choose a spouse because it is easier or less costly to migrate if he is married to her. A set of sociological studies observe that women from developing countries often migrate in order to marry men living in richer countries (Constable 2004; Kim 2009), including compatriot men who had previously migrated abroad (Lievens 1999). Other studies 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). Albeit insightful, the evidence that this literature produces are merely suggestive, as they are either based solely on qualitative or descriptive data or they are based on statistical analysis that completely sidesteps self-selection into migration.

To our knowledge, Celikaksoy et al. (2006) is the only study in economics that examines the choice of spouse among migrants. Their evidence suggests that immigrants assort positively on education, even when they restrict the sample to immigrants who 'import' their spouses from their country of origin. However, this study shares many of the methodological shortcomings of the sociological literature. Because Celikaksoy et al. do not account for immigrant selectivity, they cannot identify the separate effects of the decisions to marry and migrate. Because they use data on each partner's education that were collected after marriage, their evidence ignores the possibility that a couple may have first married and then completed their education, which could cause spousal education levels to converge even if 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 study only uses data from the country to which people moved, its evidence sheds no light on what is arguably the most interesting counterfactual question - whether and to what degree marital sorting would 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. To do this, we combine survey data from Germany and the UK with survey data on German and British immigrants from the US. We use these data to 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 governments 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.

Our data show that observed marital mobility on the basis of education is roughly the same between British migrants and non-migrants, and it is somewhat higher for German migrants relative to non-migrants. However, when we control for marital and migration selectivity, mobility is lower for immigrant men than non-migrant men, and it is higher for migrant women than non-migrant women, irrespective of the country of immigrant origin. Our analysis suggests that these patterns arise because selectivity differs by sex, migration status, and country of origin. First, we find that unobserved characteristics related to marital appeal favor migrant men and disfavor migrant women 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, if market production is country-neutral and home production is country-specific (e.g. acculturating the children to country-specific values). 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 favorable provisions, e.g. they may select to marry-down as a entry-strategy 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 men marry and likely reach financial maturity later in life relative to US natives.

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 monthly Current Population Surveys (CPS) 1994-2010, from the 1994-2008 waves of the British Household Panel Survey (BHPS), and from the 1994-2009 waves of the German Socio-Economic Panel Study (SOEP). Since 1994 the CPS asks all respondents where they were born and when they arrived in the US. 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 draw data on respondents' level of education, socioeconomic characteristics (age, sex, race), and the relationship between household members. We use the latter data to match husbands and wives.

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 probability that a person has not completed his educational attainment.

It is important to note that 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. However, because we know where each partner was born and where each partner's parents were born, we can split our immigrant sample by whether or not an immigrant married a US native (i.e. a US-born individual whose parents were also born in the US or in a country other than the home country of the immigrant spouse), a (first- or second-generation) compatriot immigrant, or an immigrant from a different country. We limit our sample to the first type of immigrant couples for two reasons. First, separate analysis of the three groups is hindered by small sample sizes.[1] Second, some fraction of couples of the latter two types, likely a non trivial fraction, took their migration decisions as a couple rather than as individuals, especially when they married each other before they migrated. This requires a complex model of the migration decision which would allow the migration of immigrant husbands and wives to be simultaneously determined. Although it is also possible for British and German migrants to have married US natives before they arrived in the US, we assume that this possibility is relatively low.

Table 1 describes the demographic characteristics of couples by sex and migration status and reports sample sizes. The data show that husbands and wives are similarly distributed among educational categories, 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 a degree of migrant selectivity in terms of education.

As we mentioned above, to correct our measure of educational mobility for marital selectivity we rely on variation in public spending in education during the years each person was of primary and secondary school age. 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.[2]

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

[1] 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.

[2] Ideally, one would like to use a more disaggregated measure of spending on educations, 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 is 0.998, while the correlation between spending in primary/secondary schools and post-secondary institutions is 0.996.

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 separate age-period 16-21 from age-period 22-30 because this is roughly 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. More specifically, Figure 2 orders individuals along the 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 that were interviewed in different years (i.e. born in different 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 in our analysis 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 host 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.[3]

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

3 Empirical strategy

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 another non-migrant. That is, we want to know whether $Prob(M_i = 1 \mid I_i = 1) \lesseqqgtr Prob(M_i = 1 \mid I_i = 0)$, 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 estimate equation (2) by gender on the pooled sample of migrants in the host country and non-migrants in the home country by a standard probit regression. The value $\widehat{\alpha}_1$ that we obtain is a 'naive' estimate of the migration effect on mobility since it could be contaminated with two types of bias. 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 measured before marriage; 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 outcomes. Although international evidence do not always support this premise with respect to education outcomes (see review by Hanushek 2003), new research insight 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.

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 health, confidence, favorable socioeconomic family background, or the ability to contribute to home production (e.g. to raise and acculturate children). By contrast, the predicted values of (3) are purged from the effects of such traits. The predicted values rely on the variation of our instrument variable, which does not capture changes in educational attainment of either spouse which happened after marriage, and is not related to characteristics (other than education) that make an individual an appealing spouse.

Because E_i has an ordered form and the error in the latent model is normally distributed, we can estimate the parameters by ordered probit. We run this regression on separate samples by country of residence at 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 $\widehat{E}_i$ derive 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 using the network of previous immigrants as an instrument that affects the migration decision but is orthogonal to marital mobility. 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 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). 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.[4] 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 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

[4]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).

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 1 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 2 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 3 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 in Germany and the UK spending when individuals were of primary-school age and spending in later years increase attained schooling by similar amounts, in the US only spending during primary-school age is important.

In Table 2 we correlate the residuals from these estimations between spouses and show that the correlation coefficients are positive and sizable, suggesting the presence of positive assortative mating on the basis of education across all groups. We also present the estimates of marital mobility in the absence of assortative mating ($\widehat{M_i}$). The data 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 British men and women, native British men, and native German men, would have been married to more educated spouses. By contrast, a smaller proportion of native British women, migrant German men and women, and native German women would have been married to more educated spouses. Among all groups, non-migrant British and German men would be the most mobile through marriage, since 84 percent of them would marry a more educated spouse. Migrant German men and non-migrant British and German women would be the least mobile through marriage, since only 13-14 percent of them would marry a more educated spouse.

These patterns suggest an important role for gender and migration in the determination of marital mobility. To obtain clearer evidence on this, in Table 4 we explicitly test whether and to what degree migration determines the marital mobility of British and German men and women. Essentially, Table 4 presents probit estimates of equation (2) on the following two samples: (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).

When we allow mobility to include the effect of marital sorting, we find that German migrants are slightly more likely to marry up relative to their non-migrant counterparts, 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 irrespective of the country of migrant origin, migration is associated with lower marital mobility for men and higher marital mobility for women. In other words, there is something about the way migrants select their spouses that induces men to marry up and women to marry down. Had there been no marital sorting, migration would favor all migrant women relative to non-migrant women and it would disfavor all migrant men relative to non-migrant men. In fact, the size of this effect would be substantial. The results suggest that if all British men had stayed in the UK, then 85% of them would have been married to more educated spouses, whereas if all British men had migrated to the US, only 35% of them would have been married to more educated spouses. The corresponding effects for German men are equally sizable; 84% and 11% respectively. Conversely, if all British women had stayed in the UK, then only 14% of them would have married up, whereas if all of them had migrated to the US then 48% of them would have married up. The corresponding effects for German women are 13% and 41% respectively.

Although the probit results are informative, it remains unclear 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. In Table 5 we attempt to disentangle these effects by instrumenting the migration decision using inflows of British and German immigrants to the US during the time when individuals were forming their preferences about migration or they were forming the migration decision itself. More specifically, Table 5 reports estimates 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 (though for German women inflows over age 22-30 are statistically insignificant). Interestingly, for British women and German 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, suggesting that these individuals form their preferences about migration at later ages than others. 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 not statistically different than zero.

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 4 and 5), 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 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 6 presents the estimated selection effects and tests of their statistical significance. Marital selection effects are positive, sizeable, and statistically significant for all migrant men, and negative but less sizeable and not always statistically significant for migrant women. Migration selection effects are negative and statistically significant for the Germans, but they are weak and of ambiguous significance for the British. Specifically, for British males (females) $\widehat{\beta}_1 - \widehat{\delta}_1$ is positive (negative) and significant, but $\widehat{\alpha}_1 - \widehat{\gamma}_1$ is negative and 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, in the absence of spouse selectivity, migrant women are more likely to marry up relative to their non-migrant counterparts, whereas migrant men are more likely to marry down relative to their non-migrant counterparts. However, because of the way they select their spouses, migrant women reduce their probability of marrying up, while migrant men increase it. It follows that there is a component in the unobservables that drive marital selection that is sex and country-specific, such that it disfavors migrant women and it favors migrant men. Although it could be one of many characteristics that fit this profile, the ability to raise, educate, and acculturate children is plausibly country- and sex-specific and offers an explanation for our results. Becker argued that men specialize in market production and women specialize in home production. If market production is country-neutral but home production involves transmitting country-specific values to the offspring, then women are at a disadvantage in the US marriage market relative to their home marriage market. In this case, marital selectivity will cause migrant women to marry down relative to their non-migrant counterparts. Under the same rationale, men are equally competitive in the home and US marriage markets, but they value US wives less than native wives. In this case, marital selectivity will cause migrant men to marry up relative to their non-migrant counterparts.

The results also suggest that, in the absence of migration selectivity, both migrant women and migrant men are more likely to marry up relative to their non-migrant counterparts. 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. 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.

These 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 women and negative for men - 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 between migrants and US natives is different across men and 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 (available at the international online database of Integrated Public Use Microdata Series (IPUMS)). The 1980 wave of the US census reports data on the age at first marriage and country of birth of each surveyed individual. Using this data, Figure 4 compares kernel density estimates of the age at first marriage across British and German migrants and US natives. Figure 4 clearly shows that the time-windows of marriage completely overlap across migrant women and US men, but the corresponding overlap for migrant men and US women is small. The reason for this is that German and British men and women get married later than US natives, but women generally get married during a narrower time window relative to men. As a result, migrant women have time at their disposal to take advantage of the higher availability of educated partners in the US, but migrant men miss the window of opportunity and end up with the 'lemons'.

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 both of the above are true. 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, thus exacerbating further their disadvantage in the US marriage market.

A further factor that would reinforce the disadvantage of immigrant men in the US marriage market is the possibility that they reach financial maturity later than US native men. Considering that migration involves establishing a life in a new environment and competing in a new labor market where discrimination is potentially present, this is a reasonable possibility. If, in addition to getting married at older ages, single migrant men are less well-off relative to US natives, they are even less attractive as husbands to US native women. In contrast, the delay in financial maturity is less of a problem for migrant women, as women are not typically expected to be the primary bread-winners of the household. Studies that examine the assimilation trajectories of immigrants in the US with respect to earnings provide supportive evidence. They show that, although immigrants eventually assimilate fully in the native population with respect to earnings, during their first years in the US they suffer a large earnings penalty relative to equally experienced natives. In fact, recent evidence from longitudinal data suggest that the immigrant-native earnings gap closes by half as fast as typical estimates from repeated cross sections suggest. According to this evidence, it takes immigrants up to 20 years to make the same earnings as natives (Lubotsky 2007).

4.3 Tests of performance and robustness

Some aspects of our identification strategy potentially cause concern. One concern stems from the fact that migration inflows are aggregations of individual behavior which (depending 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 account for such unobserved common factors, we re-estimate our models using a fixed effects specification. Because our instruments vary by age and year of survey, we include a full set of age and survey-year fixed-effects.[5] We thus identify causality of the migration networks by using variation in the inflows of migrants between age-groups within a survey year.

For completeness, we re-estimate both the probit and the bivariate probit models using the fixed-effects specification. We present the new probit estimates in Table 7 and the new bivariate probit estimates in Table 8. In all cases, the results remain qualitatively robust. Further, quantitative differences concern mostly the instrument coefficients and do not spill over to the estimated migration effects. The most notable difference is in the German sample, where the coefficients on migration inflows become significantly smaller. In contrast, for British men the coefficients on migration inflows measured over the age of 22-30 become three times higher. In all cases, the coefficients on migration inflows measured over the age of 22-30 become significantly higher than those on inflows measured over the age of 16-21, suggesting that individuals tend to form their preferences about migration mostly during young adulthood. On the whole, however, the inclusion of the fixed effects does not alter the main patterns in the results.[6]

A further source of 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

[5]See Nakajima (2007), and references therein, for a list of health economics studies that also treat Manski's reflection problem with fixed-effects.

[6]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 4 and 8). 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).

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 grossly misestimate 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 9 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 low enough to suggest weak identification), and in most cases 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 1.7 and 3 percentage points. Albeit plausible, these marginal effects appear suspiciously constant across samples. For example, the effects of the instruments appear to be equal in the samples of British women and German men. According to the bivariate probit estimates, however, these effects should be significantly higher for German men than British women. The implication is 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 also explains 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 sample of British men when mobility is measured as M_i, even though the migration effect on marital mobility is statistically insignificant. An additional contradiction in the results of the two tests appears in the sample of German men (again when mobility is measured as M_i).

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, men cannot take advantage of the availability of more educated candidate wives in the US because they reach financial maturity and they marry later than native US men, they miss the window of opportunity to catch the 'good' spouses, and they end up with the 'lemons'. Second, unobserved characteristics that drive marital sorting favor mobility for migrant men and disfavor mobility for migrant women - a result that can be explained if men specialize in market production which rewards country-neutral unobservables, while women specialize in home production which rewards country-specific unobservables (e.g. the ability to raise and acculturate children). Third, 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 favorable provisions, 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.

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Appendix: Tables and figures

Table 1: 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 2: 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. (1)	0.30	0.42	0.39	0.39
Prob(mobility=1)				
raw values	0.24	0.40	0.26	0.37
predicted from eq. (1)	0.29	0.53	0.84	0.14
B. German				
Correlation of education across spouses				
raw values	0.39	0.41	0.57	0.57
residuals from eq. (1)	0.38	0.32	0.53	0.53
Prob(mobility=1)				
raw values	0.22	0.39	0.20	0.24
predicted from eq. (1)	0.13	0.31	0.84	0.14

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

Table 3: Ordered probit regression of educational attainment on education spending

	CPS		BHPS		SOEP	
Mean ed. spending:						
over age 5-17	0.041***		0.145***		0.062***	
	[0.005]		[0.029]		[0.012]	
over age 5-12		0.045***		0.083***		0.026***
		[0.004]		[0.023]		[0.010]
over age 13-17		-0.004		0.062***		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, birth-cohort fixed effects. * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 4: Probit regression of marital mobility on migration

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Prob(being a migrant)	-0.055	-2.214	0.053	1.874
	[0.079]	[0.092]***	[0.039]	[0.068]***
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.24	0.35	0.39	0.48
Prob(being a migrant)=0	0.26	0.85	0.38	0.14
B. German				
Prob(being a migrant)	0.218	-3.261	0.400	1.066
	[0.071]***	[0.140]***	[0.035]***	[0.045]***
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.26	0.11	0.38	0.41
Prob(being a migrant)=0	0.20	0.84	0.24	0.13

Notes: Regressions on the British sample control for race, age, and age squared of self and spouse; household size; survey year dummies; birth cohort dummies; and average GDP per capita in the UK during age 16-21 and 22-30 (regressions on British males also control for average GDP per capita in the US during age 0-15). Regressions on the German sample control for age and age squared of self; household size; survey year dummies; birth cohort dummies; 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 males also control for average GDP per capita in the US during age 16-21 and 22-30, average GDP in Germany during age 0-15, and for age of spouse instead for age squared of self). Huber/White robust standard errors are in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Bivariate probit regression of marital mobility on migration

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Second-stage: Prob(mobility=1)				
Prob(being a migrant)	0.112	-2.678	0.409	2.225
	[0.983]	[0.254]***	[0.487]	[0.095]***
First-stage: Prob(being a migrant=1)				
Mean inflow of British migrants:				
over age 16-21	0.292	0.308	0.164	0.175
	[0.0763]***	[0.084]***	[0.061]***	[0.061]***
over age 22-30	0.228	0.268	0.232	0.256
	[0.112]**	[0.120]**	[0.088]***	[0.090]***
Wald test of exogeneity	0.033	5.010	0.544	34.91
	(0.855)	(0.025)**	(0.461)	(0.000)***
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.29	0.35	0.52	0.56
Prob(being a migrant)=0	0.26	0.85	0.36	0.14
B. German				
Second-stage: Prob(mobility=1)				
Prob(being a migrant)	1.366	-1.623	1.374	1.825
	[0.402]***	[0.290]***	[0.393]***	[0.080]***
First-stage: Prob(being a migrant=1)				
Mean inflow of German migrants:				
over age 16-21	0.753	1.026	0.355	0.432
	[0.284]***	[0.300]***	[0.146]**	[0.149]***
over age 22-30	3.547	4.817	0.287	0.431
	[0.814]***	[0.810]***	[0.271]	[0.273]
Wald test of exogeneity	8.319	34.21	5.174	149.0
	(0.004)***	(0.000)***	(0.023)**	(0.000)***
Estimated prob(mobility=1) if:				
Prob(being a migrant)=1	0.68	0.44	0.74	0.64
Prob(being a migrant)=0	0.20	0.84	0.24	0.13

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

Table 6: Wald-test of statistical significance of selection effects and parameter restrictions

		Males			Females		
		Effect	X^2	(p-value)	Effect	X^2	(p-value)
A. British							
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	2.159	365.8	(0.000)	-1.821	506.3	(0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	2.790	9.100	(0.002)	-1.816	12.85	(0.000)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-0.167	0.030	(0.856)	-0.356	0.560	(0.455)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	0.464	6.380	(0.011)	-0.351	37.45	(0.000)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	-0.631	0.530	(0.466)	-0.005	0.000	(0.992)
B. German							
Marital selection effect	$\widehat{\alpha}_1 - \widehat{\beta}_1$	3.479	498.7	(0.000)	-0.666	122.3	(0.000)
	$\widehat{\gamma}_1 - \widehat{\delta}_1$	2.989	44.45	(0.000)	-0.451	1.150	(0.284)
Migration selection effect	$\widehat{\alpha}_1 - \widehat{\gamma}_1$	-1.148	9.640	(0.002)	-0.974	6.490	(0.011)
	$\widehat{\beta}_1 - \widehat{\delta}_1$	-1.638	45.92	(0.000)	-0.759	180.3	(0.000)
Parameter restriction	$\widehat{\alpha}_1 - \widehat{\beta}_1 - \widehat{\gamma}_1 + \widehat{\delta}_1$	0.490	1.410	(0.235)	-0.215	0.290	(0.588)

Table 7: Probit regression of marital mobility on migration with full set of fixed effects

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Prob(being a migrant)	-0.059	-2.188	0.050	2.021
	[0.075]	[0.084]***	[0.040]	[0.074]***
B. German				
Prob(being a migrant)	0.218	-2.841	0.390	1.102
	[0.071]***	[0.120]***	[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 4. Regressions on German males also control for household size; average GDP per capita in Germany during age 0-15, 16-21 and 22-30; average GDP per capita in the US 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 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 8: Bivariate probit regression of marital mobility on migration with a full set of fixed effects

	Males		Females	
	Raw mobility	Estimated mobility	Raw mobility	Estimated mobility
A. British				
Second-stage: Prob(mobility=1)				
Prob(being a migrant)	-0.332	-2.568	0.359	2.231
	[1.036]	[0.200]***	[0.522]	[0.099]***
First-stage: Prob(being a migrant=1)				
Mean inflow of British migrants:				
over age 16-21	0.220	0.258	0.197	0.199
	[0.074]***	[0.077]***	[0.068]***	[0.067]***
over age 22-30	0.668	0.678	0.282	0.294
	[0.193]***	[0.189]***	[0.101]***	[0.103]***
Wald test of exogeneity				
	0.075	6.340	0.362	16.76
	(0.784)	(0.012)**	(0.547)	(0.000)***
B. German				
Second-stage: Prob(mobility=1)				
Prob(being a migrant)	1.590	-2.397	1.474	1.731
	[0.400]***	[0.276]***	[0.384]***	[0.086]***
First-stage: Prob(being a migrant=1)				
Mean inflow of German migrants:				
over age 16-21	0.371	0.320	0.069	0.070
	[0.217]*	[0.224]	[0.018]***	[0.020]***
over age 22-30	2.122	1.991	0.149	0.171
	[0.675]***	[0.698]***	[0.042]***	[0.040]***
Wald test of exogeneity	10.95	4.781	6.343	112.9
	(0.001)***	(0.029)**	(0.012)**	(0.000)***

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

Table 9: 2SLS first-stage: 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	0.030	0.030	0.017	0.017
	[0.012]**	[0.012]**	[0.005]***	[0.005]***
during age 22-30	0.020	0.020	0.022	0.022
	[0.010]**	[0.010]**	[0.007]***	[0.007]***
F-test of joint instrument significance	3.216	3.216	15.25	15.25
	(0.040)**	(0.040)**	(0.000)***	(0.000)***
Sargan test of overidentification	1.022	0.576	0.126	0.315
	(0.312)	(0.448)	(0.722)	(0.575)
Wooldridge's test of exogeneity	27.13	784.5	0.701	634.4
	(0.000)***	(0.000)***	(0.402)	(0.000)***
B. German				
Mean inflow of German migrants:				
during age 16-21	0.017	0.017	0.028	0.028
	(0.010)	(0.010)	(0.011)**	(0.011)**
during age 22-30	0.022	0.022	0.030	0.030
	(0.008)***	(0.008)***	(0.018)*	(0.018)*
F-test of joint instrument significance	4.113	4.113	3.286	3.286
	(0.016)**	(0.016)**	(0.037)**	(0.037)**
Sargan test of overidentification	1.989	0.029	0.120	2.456
	(0.158)	(0.864)	(0.728)	(0.117)
Wooldridge's test of exogeneity	0.021	27.99	55.32	32.85
	(0.885)	(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 instrument variables

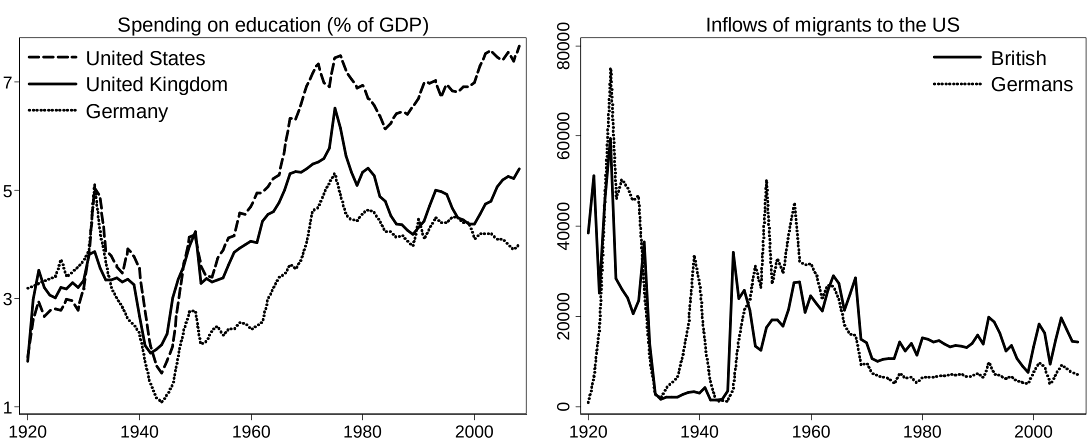


Figure 2: Scatter plots of instruments and age

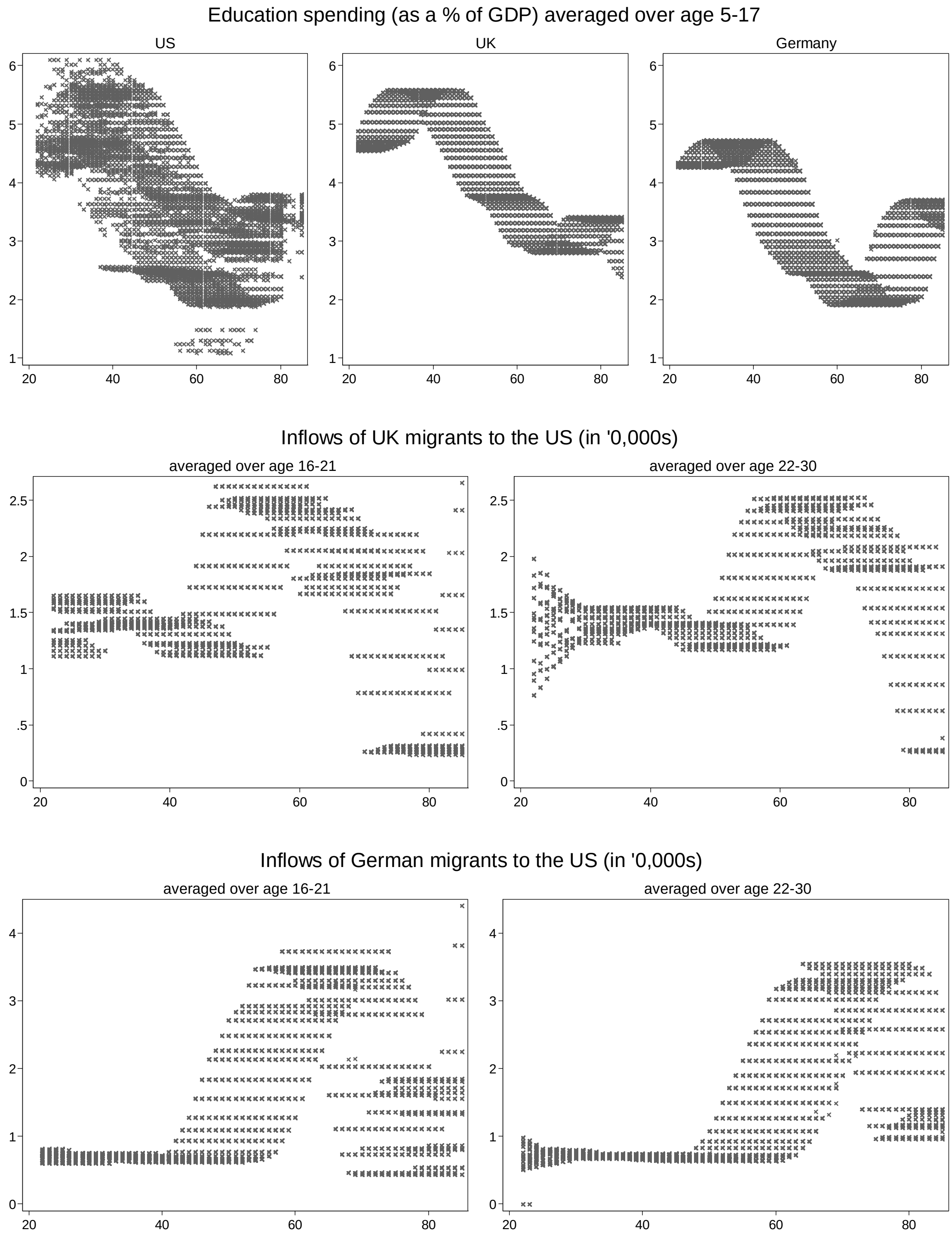
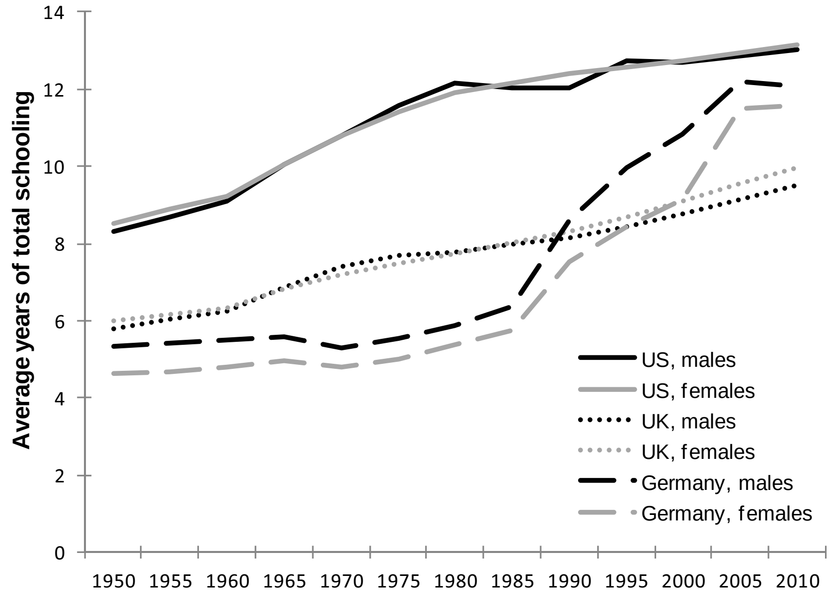
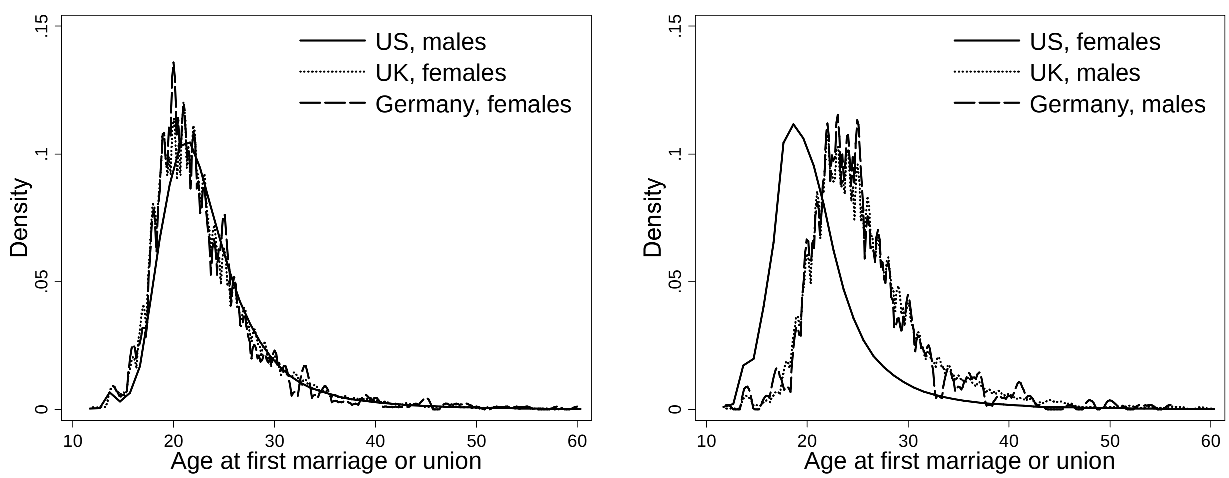


Figure 3: Educational attainment by country and sex



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.