Online Appendix to:

Migration to the US and Marital Mobility

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1 Previous literature

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).

Although researchers have long recognized that individuals may take the decisions to migrate and marry jointly, to this date they have failed to formally model and test this joint relationship. 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, these studies fail to formally address either marital selectivity or immigrant selectivity or both. Thus, they cannot identify the separate effects of the two decisions on the outcomes of interest. Further, 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. Our empirical exercize addresses all these limitations.

2 Tests of performance and robustness

Some aspects of our baseline 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.[1] Both new measures of mobility

[1] Using the sample variance in this way allows for equivalent shifts in raw and estimated mobility. Further,

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 A1 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 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 A2 and the bivariate probit estimates in Table A3. In all cases, the estimates remain qualitatively robust. Quantitative differences are most apparent in the instrument coefficients, which

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.

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.[2]

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

[2]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 A2). 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).

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 A4 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. [3]

[3] 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 A4, 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

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).

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Table A.1: 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 A.2: 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 A.3: 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 A.4: 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.