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Working Paper

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CESifo Working Paper, No. 3705

Provided in Cooperation with:
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Suggested Citation: Stolz, Yvonne; Baten, Jörg (2012) : Brain drain in the age of mass migration: Does relative inequality explain migrant selectivity?, CESifo Working Paper, No. 3705, Center for Economic Studies and ifo Institute (CESifo), Munich

This Version is available at:
https://hdl.handle.net/10419/55316

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CESIFO WORKING PAPER NO. 3705
CATEGORY 4: LABOUR MARKETS
JANUARY 2012

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Abstract

Brain drain is a core economic policy problem for many developing countries today. Does relative inequality in source and destination countries influence the brain-drain phenomenon? We explore human capital selectivity during the period 1820-1909.We apply age heaping techniques to measure human capital selectivity of international migrants. In a sample of 52 source and five destination countries we find selective migration determined by relative anthropometric inequality in source and destination countries. Other inequality measures confirm this. The results remain robust in OLS and Arellano-Bond approaches. We confirm the Roy-Borjas model of migrant self-selection. Moreover, we find that countries like Germany and UK experienced a small positive effect, because the less educated emigrated in larger numbers.

JEL-Code: F220, J400, I210, N300.

Keywords: international migration, labor markets, human capital, economic history.

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Seminar and conference participants at the universities of Lund, Muenchen, London and Tuebingen, and in particular Martin Biewen, Ray Cohn, Kerstin Enflo, Joachim Grammig, Tim Hatton, Willi Kohler, Joachim Voth, two anonymous referees, and members of the Tuebingen economic history group provided valuable comments. Financial Support of the German Science Foundation (DFG), ESF GlobalEuroNet program and the EU HIPOD program is gratefully acknowledged, and data support of IPUMS, NAPP, Valeria Prayon, Matthias Blum, and Oliver de Marco. Eoin McLaughlin improved the language style.

For countries with substantial emigration rates, brain drain is a core economic policy problem.[1] The migration of Germans to the U.S. or Switzerland in the recent past, for instance, has been discussed as brain drain, because the high U.S. skill premia attract a large number of highly skilled Germans. Chiswick (2005) summarized that often the "best and the brightest" would leave their home country to emigrate to more promising labour markets.[2] In African countries, brain drain is perceived as an important issue, as well (Docquier, 2006). Although the health situation on the African continent is problematic, highly skilled African physicians leave and move in large numbers to the Western World because of higher returns to human capital. In a recent article in a leading medical journal, 'The Lancet', it was suggested that the recruitment of physicians from poor countries with high mortality ought to be treated as a criminal case because this would result in more people dying in the African source countries (Mills et al., 2008). Consistent with these approaches, we define 'brain-drain' as the phenomenon where, relative to the remaining population, a substantial number of more educated people emigrate.

What determines the selectivity of migrants? Among other explanatory variables, relative inequality has been stressed in the theory of self-selection. If inequality is higher in the destination than in the source country, we would expect highly skilled individuals to migrate, as Borjas (1987) formulated on the basis of Roy's self-selection model (Roy, 1951): higher inequality implies that the most educated receive higher relative wages. His views stimulated an excited debate because those who came to the U.S. from emigration countries with a high inequality like Mexico were expected to be negatively selected. That is, we would expect people with less education than the average person at home to migrate. We will call this theoretical approach "Roy-Borjas model" in the following, as Borjas applied the Roy

[1] This includes countries like Germany, which just recently was reported as net emigration country by a leading weekly journal ("die Zeit" 10/2010).

[2] Of course, in today's world of skill-selective immigration policies, incentives in source countries sometimes also impact on acquiring a good education in order to have the choice to migrate, even if the more educated individual does not migrate in the end. Furthermore, international migrants send remittances to their home countries that also have an important developmental effect. For example in the Philippines, remittances made up just over 10 percent of national income in 2007. In Mexico and India, the figures have even been higher.

Model to the process of migration (Borjas, 1987). We introduce a methodological innovation to the migration literature by using anthropometric inequality measures. These are based on a large project in which evidence was collected on human stature as a welfare indicator (Fogel, 1994; Steckel, 1995; Komlos and Baten, 1998; Blum and Baten, 2012, see section 3).

The question on the impact of relative inequality on the selectivity of international migrants remains so far unanswered. Bruecker and Defoort (2006) find a positive correlation between inequality in the home country and educational selectivity of migrants in the OECD for the 1980-2000 period and develop a theoretical model that explains that better educated people can cope with migration policy hurdles. Furthermore, they find that inequality impacts positively on the human capital selectivity of migrants. Feliciano (2005) studies 32 immigrant groups in the US labor market and compares them with their source countries regarding their education and inequality level. Her results were not consistent with the Roy-Borjas model. Belot and Hatton (2011), however, find evidence for a modified Roy model for OECD immigration during the past decades.

We contribute the analysis of a new and unique data set to this debate, as we can include international migrants from 52 source countries who went to five destinations in the Americas and in Europe during the era of mass migration (1820s-1900s). A migrant is defined as somebody born outside the destination country, hence no citizenship or ethnic definition contaminates the study of migration decision. The overall number of underlying individual observations is 6.2 million. We aggregate them by migration decade and by source and destination country pairs (minimum: 50 cases). We obtain 127 country pairs with sufficient underlying cases. Our evidence provides a unique setting to investigate the question at hand, because migration flows were not yet mainly determined by immigration policies, which nowadays shape migrant selectivity significantly.[3] We include U.S. data until 1900-1909, as

[3] Germany, for example, attracted relatively low-skilled migrants during the 1960s and thereafter, because of the immigration policies at that time that aimed at providing unskilled labor for factory work, and the family unification allowances during the following period. Ireland on the other hand, attracted highly skilled labor in the recent decades which is partly due to its immigration policy, and partly due to large amounts of foreign direct investment before the economic crisis of 2009.

the U.S. did not have strong immigration restrictions until 1919. Our Argentinean evidence covers only the migration until the decade of the 1880s, as Argentina was the first to impose strong immigration restrictions starting mainly in the 1890s (Timmer and Williamson, 1996; Sanchez-Alonso, 2008). The data studied here, therefore, provides relatively undistorted evidence of migrant self-selection. We include not only major transatlantic destination countries, but also European immigration targets such as the UK. Finally, we also study one destination country which had actually more emigration than immigration: Norway had significant immigration from Sweden, Finland, Denmark, Germany, Iceland, but also Italy, UK, US and Russia. The major transatlantic destination countries are represented in our sample by the U.S., Canada and Argentina. This study is the first general assessment of migrant selectivity during this most crucial period of human migration history: the age of mass migration.

We apply the age heaping approach which captures basic numeracy skills by looking at the share of people who are able to report an exact age. In previous studies, this measure has always been found highly correlated with other education indicators (see, for example Crayen and Baten, 2010a). It allows the calculation of the difference between migrants' numeracy and numeracy of the source country population. We use this differential as the dependent variable and regress it on a set of explanatory variables.

2a. Theory: the relationship between skill selectivity and inequality

On the micro level, economic theory implies that utility maximising individuals base their migration decisions on the benefits and costs of migration. Provided, the skill set a migrant incorporates is sufficiently applicable in the destination country, the expected yield from such a decision is the income gap between destination and home country multiplied by the probability of not being unemployed.[4] Migration costs comprise all the psychological, physical and material costs of the journey and subsequent settlement in a different

[4] During the late 19th century, labor markets were not much regulated; hence obtaining a job at low wage was typically possible.

environment. Since migration always requires a certain amount of cash or "out-of-pocket"-money (Liebig and Sousa-Poza, 2004: p. 128), and credit markets are often imperfect, a poverty constraint exists, as the poorest often simply cannot afford the cost. This restriction explains why, during the process of economic development, migration rises, when a country experiences initial economic growth. The poverty constraint loosens and more people can afford to migrate.

Migration costs increase with geographical and cultural distance, because travel and other costs (e.g. learning a language, religious differences etc) will be higher and the successful integration into the destination society might be more of a challenge. They decrease with growing diaspora communities in the target country, because friends and relatives living abroad might send remittances and provide valuable information, employment or other support for the newly arrived migrant.

The impact of all these determinants on migration decisions is relatively well-documented (Hatton and Williamson, 1998). What is less clear, however, is the question of what determines migrant selectivity. Borjas (1987) developed a framework based on the Roy Model to approach the issue of migrant selectivity (Roy, 1951). The basic model was originally formulated to explain occupational self-selection and its impact on inequality, when an individual has the possibility to choose between two options. Given that the skills are sufficiently correlated among occupations, the individual will select into the occupation that provides the highest expected earnings. Borjas (1987) adapted the model to migration decisions. Here, the migrant selects himself into migration to a certain destination country, when his skill set will realize more income in the destination labor market than in the domestic one. An underlying assumption is that the skills can be applied in both countries and are sufficiently valued in both labor markets. A second condition is a market with sufficient information so that migrants are able to respond to the incentive. The question is whether these assumptions are valid for the 19th century, our period of study. Previous migrants often informed their friends and relatives back home about the situation in the target country by

writing letters. While their letters were sometimes more optimistic than the real situation, they provided some insight as to the comparative welfare of skilled and unskilled workers. Moreover, a large number of migrants reversed their decision if the benefits were not as large as expected and returned home.

To sum it up, according to the model, whether a person with a given skill level actually moves or not depends *ceteris paribus* on the relative inequality of source and host country. Positive selection occurs when the destination displays a higher skill premium than the home country (see, for example German or African migration to the US in recent decades, or Russian Jews moving to 19th century U.S.). Negative selection occurs in the opposite case.

Belot and Hatton (2011) develop a variant of the Roy model to explain educational selectivity of migration flows into 29 OECD countries over the past decades. They also include immigration policy and poverty constraints. After controlling especially for a poverty constraint – as the poorest are not able to migrate – they obtain significant results for the link between inequality and selectivity the Roy model proposes. Moreover, they find cultural and geographic distance to be very important. Grogger and Hanson (2011) use data on emigrant stock by schooling and source country in the OECD. They confirm the Roy-Model in a sense that migrants are more educated than non-migrants and their selectivity is stronger, the higher the skill premium in the destination compared to the source country.

Other empirical studies, in contrast, did not confirm the Roy-Borjas model. Bruecker and Defoort (2006), for example, find a positive correlation between inequality in the home country and educational selectivity of migrants. They argue that this is caused by higher abilities of the educated to jump over immigration restriction hurdles. Moreover, they find the same correlation for host country inequality. Feliciano (2005) finds no effect of income inequality on human capital selectivity for 32 immigrant groups in the US labor market, which also does not correspond with the Roy-Borjas model prediction.[5] So far, there is no

[5] The Mexican case has attracted particular attention of scholars: Chiquiar and Hanson (2005) derive their data from the 1990 Mexico population and 1990 U.S. population censuses. They find that, while Mexicans are much

general agreement about the relationship between inequality and human capital selectivity of international migrants.

Moreover, the issue has not been investigated from a historical and a broad international perspective until now. Wegge (2002) and Abramitzky et al. (2009) provide valuable studies on country cases and Cohn (2009) studies the early skill composition of mainly English, German, and Irish migrants to the United States 1820-1860 using the occupational composition of migrants as a proxy. Cohn makes clear that it was the migrants themselves, who declared the occupations. They sometimes tended to make exaggerated statements about their social and occupational status at home. In a review of Cohn's book, Kampfhoefner (2009) suggested to complement this approach with the age-heaping method. Mokyr (1983) pioneered the technique for the Irish case (see also Ó Gráda, 1986, for Baltic migrants to Dublin). Mokyr (1983) confirms that early migrants often reported occupations with high social status, but found that age heaping was significantly higher among Irish migrants than among the Irish population. While this is true for the whole pre-famine period, age-heaping on emigrant ships that arrived during the famine years was even higher. Also using education-based proxies, Abramitzky, Boustan and Eriksson (2009) find negative selection of Norwegian migrants to the US in the period 1850-1914, while Long and Ferrrie (2010) observe more upward than downward mobility among those who moved from the UK to the U.S. during the 1880s to 1900s. They made sure in a multiple factor analysis that this was not an effect of the move itself. They emphasized as a caveat that the mobility criterion -- the change between one of four broad occupational categories – implies that those who were already in the highest category (white collar) had no further possibility of upward mobility. Wegge (2002) finds in her study of the German principality of Hesse-Cassel no strong migrant selectivity as the poor were hindered by poverty constraints from moving whereas the

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less educated than US natives, they are better educated than the average Mexican, indicating a positive selection. Moraga (2011) also studied self-selection of Mexican migrants to the US using Mexican household level data of 2000-2004. Moraga finds a negative selection of Mexican immigrants, directly contradicting the previous results of Chiquiar and Hanson.

wealthier had no incentives to migrate. Hence she finds mainly those with medium skilled occupations went across the Atlantic, namely those who could transfer their skills easily and were not bounded by poverty constraints (Wegge, 2002; Hatton, 2010).

We extend those valuable historical studies by using the age-heaping indicator and by focusing on five destination countries and 52 source countries, offering additional systematic insights on this issue, taking a long-run, international approach for the 1800s - 1900s period.

2b. Other determinants of migrant selectivity

We expect transport costs and poverty constraints to play an important role. The log distance from the source country capital to the destination country capital multiplied with the decade-specific cost is included in the regressions below to proxy migration costs.[6] As the inhabitants of many poor countries and the poor in medium-income countries simply could not afford the transatlantic journey and many could not even afford migration within Europe, we need to control for poverty constraints. As the poverty constraint might be less binding for a journey to a country which is closer, we multiply the logarithm of the distance with the measure of poverty to allow for varying intensity of this effect.

Other important components in the model are chain migration effects and remittances that earlier migrants might provide. Previously migrated friends or relatives sent home not only money, but also information about the destination country, which decreases the perceived risk of migration (Cohn, 2009).

In some European countries the travel costs of the poor were even paid by the municipal communities which wanted to avoid the social transfers (von Hippel, 1984; Bade, 2008). This contributed to less positively selected migrants.

How can we measure poverty constraints? We use the specification suggested by Hatton and Williamson (2002) which was widely accepted in the literature: the ratio of gini

[6] The distance measure as well as data on colonial ties and common languages is taken from http://www.cepii.fr/anglaisgraph/bdd/distances.htm . On the decade-specific costs, see Sanchez-Alonso (2008).

coefficient divided by GDP per capita squared (Maddison, 2009).[7] This measure reflects the reality quite well, because both higher inequality (in the numerator) and lower GDP per capita (in the denominator) increase the constraining effect of poverty.

Apart from economic incentives, political, cultural and religious factors might also play a role. In German historiography, the democratic revolution attempt in 1848 and its aftermath generated an outflow of highly educated individuals, who continued to play a role in American policies. We test whether the democracy situation in the destination country, relative to the source country, might have an impact on the selectivity of migrants.

Eastern European migration was partly shaped by religious factors. The Jewish minority experienced strong discrimination in the Russian Empire during this period, which reached its maximum in the pogrom waves of the 1880s. During the 1880s, the mass exodus of more than two million Russian Jews began. But already before, a modestly numbered stream of highly skilled Jews left the country. Even if Boustan (2007) found that the largest share of migration can also be explained by wage gaps and other economic variables she agrees that there was an extra dip of Russian and Jewish migration in the early 1890s caused by religious persecution. Hence the pronounced selectivity was not only caused by economic incentives, but reinforced by religious persecution. Therefore, we control for such occasions whenever possible in our regressions.

Finally, we control for common language and colonial ties. On the one hand, having to acquire a new language requires higher human capital of migrants than being able to use the mother tongue. On the other hand, advanced human capital can be more easily transferred between countries sharing the same language. This would suggest a positive effect on selectivity. While the sign of the effect is not clear, we would expect skill selection to differ for country pairs with the same language. Colonial ties often show the same features. A common culture and common institutions will make it easier for the migrant to adapt to the

[7] Where GDP was not available, we used imputations based on anthropometric values, see Baten and Blum 2010. This method exploits the fact that for historical data, the biological standard of living proxies living standards quite satisfactorily.

new environment. In the case of migration from India to Britain, the type of colonial migrant might have been quite often government officials who went to the colonies to work in the administration or military. Their families might later have returned to Britain, in which case we would expect them to be more numerate than the source country population.

3a. Methodology: skill selectivity

Age heaping is a method that uses the share of persons who report their exact age, as opposed to those who round erroneously, as an indicator for basic numeracy (Mokyr, 1983; Crayen and Baten 2010a, and 2010b). This indicator has been widely applied recently (A'Hearn, Baten and Crayen, 2009; de Moor and van Zanden, 2008; Clark, 2007; Humphries and Leunig, 2009; Cinnirella, 2008; O'Grada, 2006). A'Hearn, Baten and Crayen (2009) have shown that within societies characterized by a lower level of human capital, the frequency of people stating their age erroneously is higher than in more developed societies. The tendency is to mention a convenient multiple of five instead of the exact age, which becomes evident in the frequency distribution of the age data. The ratio of the frequency of multiples of five in relation to the frequency of all mentioned numbers is defined as the Whipple Index.[8] The ABCC index employed in this paper is a simple linear transformation of the Whipple index. It represents the estimated percentage share of the population who reported an exact age (A'Hearn, Baten and Crayen, 2009).

$$(1)\quad ABCC = \left(1 - \frac{(Wh - 100)}{400}\right) \times 100 \text{ if } Wh \geq 100; \text{ else } ABCC = 100$$

The ABCC index correlates strongly with literacy rates, schooling and other human capital indicators, a relation which remains relatively stable across time and space and is robust when applied to different types of data sources. Generally, the age heaping approach is considered a viable method to capture human capital in empirical studies. The great advantage

[8] The optimum is 100, i.e. an equal distribution of mentioned ages throughout the population, the extreme of 500 occurs, if everybody mentions a multiple of five only.

of age heaping is the great variety of sources, where evidence can be drawn from and its coherent construction over space and time (Crayen and Baten, 2010a).

Our dependent variable which measures human capital selectivity is constructed as the difference of the mean ABCC Index of migrants 'Mig' and the mean ABCC of the source country population per decade. The latter includes both migrants and stayers:

$$S_{ijt} = Mig_{ijt} - (Mig+Stayer)_{it} \quad (2)$$

where S_{ijt} is the selectivity of migrants from country i to j in migration decade t.

In our study, the source country numeracy is calculated as a weighted share of stayers and migrants if the migration rates reach a substantial number, since during the time before the migration decision, the migrants' human capital still was part of the source country environment.

The argument might arise that results are biased, if the census taking process in home and target country differ or if the states are differently institutionalized and therefore gather their citizens' ages with different frequency. However, Crayen and Baten (2010a) have shown that the number of previous censuses taken as a proxy for institutionalized state-authority does not have a significant impact on the outcomes of the ABCC Index. Moreover, we will control for destination and source country fixed effects, which captures unobserved source and destination country specific effects.

Another possible concern relates to the numeracy of migrants, which is based on questions posed years after migration, since in the meantime the migrant could have acquired further skills in the destination country. We did, however, counter-check our results with a sample of migrants that were obtained from ship lists, directly after arrival in the destination countries, and the correlation was very close (see below for further details).

All of our numeracy differentials are arranged by estimated decade of migration. The ABCC of the migrants is collected from data of censuses taken in the destination country. As the census does not provide time of migration, we first used the age information to calculate birth cohorts and then assumed that the majority of the migrants migrated in their second

decade of life to estimate the migration decade. In the census data, the year of immigration is not noted. All previous migration studies found that a significant majority migrated when they were around age 15-35, except for some children and a small number of older persons. We therefore argue that the period of migration decision must have been mostly two decades after birth and use this to calculate migration decades. This assumption has been counter-checked with lists created on ships, and we found it justified: The ages 15-35 are in majority by far. Even more importantly, the numeracy by decade and country is almost exactly the same when looking at ship lists (with known time of migration) and census data. Comparing all passenger lists of ships arriving to New York between 1860 and 1895 the correlation of ABCC values by country and decade with the census evidence is 0.6 (p= 0.00, N=105).[9] We then used the ABCC values of the source countries, which were also first organized by birth decades and then shifted by two decades to reflect the estimated decade of migration (Crayen and Baten, 2010a).[10] The difference between the numeracy of the migrants and the source country estimate is the main dependent variable, as explained above.

Finally, we need to address the issue of return migration. If census years were mostly at the end of our period, then we would have a mixture of return and permanent migrants in the one or two decades before the census year, and only permant migrants in the earlier decades. For modern data, Lubotsky (2007) has recently argued that temporary migrants can be very differently selected. How evenly spread are our census years? Fortunately, the census years are relatively evenly distributed over the era of mass migration. The U.S. data which accounts for the largest body of evidence starts already in 1850, and the numbers of migrants before 1850 was much smaller than later on. For Argentina, our first census evidence was

[9] We included all ship lists which were provided by the transcriber's guild (New York arrivals: http://www.immigrantships.net/nycarrivals1_6.html). Unfortunately, the number of observations is much smaller than in the case of census data – only some 300,000 compared to 6.2 million that we study here based on the census data -- hence we did not perform the same analysis with the ship lists. The advantage of ship list evidence is the possibility to determine the human capital status (and age) directly at arrival. One disadvantage is that it includes temporary migrants or travellers who returned home after a few months, but still the comparison to census data provides valuable insights. We thank Oliver de Marco for his immense contribution to this point.

[10] We only use the age group 23-72, to avoid age effects caused by selective mortality of the older age groups thereby obtaining up to a maximum of five cohorts with each census (those aged 23-32, 33-42, …, 62-72).

from 1869, and Argentina did not have mass immigration until the late 1850s. Also for the other countries census dates are fairly spread over the relevant period. How large might the extent of bias from differences of temporary and permanent migrants have been? The implications of Lubotsky's recent study for our paper are that temporary migrants can be indeed quite selective. He found that in today's U.S., many temporary migrants were negatively selected, and after they returned, the remaining migrants were a different selection.[11] The question is, however, whether those effects of today's period could also apply to the 19th century, when transport costs were much higher. We can put this to the test to a certain extent. While there exist no official statistics, we are able to compare two groups, migrants identified in the census and migrants identified on shiplists. Those who were recorded on shiplists clearly included both temporary and permanent migrants, whereas the early cohorts of the census-based migration evidence might contain mostly permanent migrants. We were interested in the question whether the migrant selectivity in the two sources differs, which can shed light on the question raised above. For this, we used a data set of country-migration decade observation (US data), and subtracted the numeracy selectivity values of shiplist-based migrants from census-based migrants. We regressed this differential on a set of migration decade dummies to see whether there was bias in some of the decades. As a result, we find that the coefficient was sometimes positive and sometimes negative, but there was never a significant difference (Table 1). The overall arithmetic difference was –2.21, whereas the weighted difference (weighted by number of migrants) was plus 1.36 percent across all decades. Hence among the more prominent migrant groups (Ireland etc.) there was probably no negative selectivity among temporary migrants, whereas there was some among the smaller immigrant groups.

Table 1: Regression of differences between census-based and shiplist-based numeracy selectivity estimates (Column 1), and comparison between selectivity by time period since migration (Columns 2 to 5)

[11] Lutbotsky (2007) uses longitudinal earnings data from Social Security records to study the effect of selective emigration on the measured progress of immigrants to the US, whereas previous studies on income achievements were using cross-sections which suffered from temporary migrant selectivities.

	(1)	(2)	(3)	(4)	(5)
Migration cohorts	All	All	All	All	1870s
Less than 20 years		-0.96	-0.67	-0.84	-0.55
in U.S.		(0.290)	(0.378)	(0.291)	(0.747)
Migration dec 1810	-5.67				
	(0.523)				
Migration dec 1820	-1.01				
	(0.856)				
Migration dec 1830	0.84				
	(0.804)				
Migration dec 1840	0.58				
	(0.860)				
Migration dec 1850	-3.41				
	(0.291)				
Migration dec 1860	-1.85				
	(0.566)				
Migration dec 1870	-1.99				
	(0.556)				
Migration dec 1880	2.14				
	(0.641)				
Migration dec 1890	-1.21				
	(0.637)				
Country FE	N	N	Y	N	N
Time FE	N	N	N	Y	N
Constant	-1.21	-2.17***	-7.53***	0.49	-3.80
	(0.637)	(0.000)	(0.000)	(0.599)	(0.005)
N	105	205	205	205	70
R-sq	0.04	0.01	0.65	0.04	0.00

In column 1, the year indicates the migration decade estimate (1810 for 1810-19 etc). Reference category is 1900s. Dependent variable is the difference between migrant selectivity according to shiplists in the U.S. (which includes both temporary and permanent migrants) and the migrant selectivity according to census. Migrant selectivity is always the difference between the numeracy of migrants and source country population in a specific decade. In column 2 to 5, the dependent variable is migrant selectivity according to the U.S. censuses of 1900 and 1910. In the column 2 to 4, both censuses are pooled, because the attention is focused on the difference between migrants more and less than 20 years in the United States. In column 5, the censuses are not pooled, because the focus is here on the comparison of migrants of the 1870s migration decade only, who were either "permanent" migrants (longer than 20 years in the U.S) or "mixed" (permanent and temporary) migrants (shorter than 20 years in the U.S.).

Moreover, we used the time of migration variable which is available in the U.S. censuses of 1900 and 1910. We distinguished migrants who were less than 20 years and those who were more than 20 years in the United States, which is a frequently used threshold. Within those two groups, we calculated the difference between their ABCC value and the one of the country and birth decade they came from. This yielded 205 country-cohort observations, each expressed as the difference between migrant ABCC and source country ABCC. This variable was regressed on a dummy variable "Less than 20 years in the U.S.", whereas the reference categories were migrants who were at the time of the census more than

20 years in the United States (Table 1, Col. 2-4). As we do not know whether the former migrants stayed later-on, it is a comparison between both permanent and temporary migrants on the one hand and permanent migrants on the other. There is always a small negative effect of less than 1 percent ABCC, but it is never significant.[12] In sum, if there was an issue of return migrant selectivity, it was probably small for this period.

3b. Estimating Inequality

A second methodological question was the measurement of relative inequality. Although Borjas' original model looks at the standard deviation of wages, most recent studies on the model use Gini coefficients of the income distribution, because they are available for a large number of countries since about 1980. The underlying assumption is that wage variation and overall income Gini coefficients correlate.[13] For the 19th century, data on inequality is scarce (see van Zanden et al., 2011; Blum and Baten, 2011), therefore we use an innovative approach to capture this independent variable, that builds on the work of these authors.

Our core independent variable is inequality in the destination country minus inequality in the source country, constructed with an anthropometric method. Baten (2000) argued that the coefficient of variation of human stature is correlated with overall inequality in a society, and that it can be used as proxy measure, especially where income inequality indicators are lacking. The correlation has been confirmed in further analyses, for example by Pradhan et al. (2003), Moradi and Baten (2005) and van Zanden et al. (2011). This method has been widely used in the economic history literature (Sunder, 2003; Guntupalli and Baten, 2006). The idea is that heights reflect nutritional conditions during early childhood and youth. As wealthier people have better access to food, medical resources and shelter, their children tend to be taller than those of the poorer strata of the population. Hence, the variation of height of a certain cohort may be indicative of income distribution during the decade of their birth.

[12] Another test compared those who were in the U.S. for 10-20 years and those in the U.S. 20-30 years taking them from the two censuses of 1900 and 1910 separately. The migration took place in both cases during the 1870s. As result, the mixed group had a selectivity of -0.55 percent, compared to "permanent" group.

[13] Belot and Hatton (2011) use wage differences measured in the wages for occupations that normally require some skills versus some that do not

Yet, while a correlation with income does exist, this correlation is only partial, since some important inputs are not traded on markets but are provided as public goods. These lead to modest deviations between purchasing power-based and height-based inequality measures. Deaton (2003) and Pradhan et al. (2003) have highlighted the importance of measures of health inequality in general. Heights capture important biological aspects of the standard of living (Komlos, 1985; Steckel, 1995). Also in migration decisions a proxy that captures overall welfare is relevant, because individuals not only maximize their income but also health and longevity.[14]

Table 2: Some examples of anthropometric gini coefficients and skill premia, by country and decade

Country	Migration decade	Height gini	Skill prem.	Country	Migration decade	Height gini	Skill prem.	Country	Migration decade	Height gini	Skill prem.
UK	1880	34	1.66	Austria	1900	44	1.60	Russia	1880	49	2.48
Italy	1860	37	2.00	Italy	1850	44	2.00	Russia	1890	49	2.09
UK	1870	37	1.60	Netherld.	1890	44	1.32	France	1870	49	1.83
UK	1860	39	1.67	Germany	1880	45	1.44	Belgium	1880	50	1.67
Netherld.	1900	39	1.32	Germany	1840	45	1.62	Austria	1890	50	1.60
UK	1900	40	1.51	Italy	1890	45	1.65	France	1860	50	1.59
UK	1890	40	1.58	Germany	1830	45	1.46	Russia	1830	51	2.42
UK	1840	40	1.67	Poland	1830	45	1.85	Spain	1880	51	1.83
Italy	1870	41	2.00	Russia	1910	45	2.10	Russia	1860	51	2.43
Netherld.	1860	41	1.32	Russia	1900	45	1.91	Spain	1860	51	2.04
Germany	1900	41	1.59	Poland	1860	45	2.31	Russia	1840	52	2.32
France	1900	41	1.84	Germany	1850	46	1.79	Austria	1870	52	1.60
Italy	1840	41	2.01	UK	1910	46	1.50	Austria	1840	53	1.60
Italy	1880	41	1.81	France	1880	46	1.67	Spain	1870	53	1.95
Italy	1900	41	1.84	Belgium	1900	46	1.65	Belgium	1870	53	1.67
UK	1850	42	1.67	Italy	1910	46	1.81	France	1840	53	1.76
Netherld.	1840	42	1.32	Austria	1850	46	1.60	Spain	1900	53	2.00
Netherld.	1910	42	1.32	Netherld.	1870	46	1.32	Russia	1870	53	2.65
Netherld.	1830	43	1.32	Poland	1840	47	2.09	Russia	1850	54	2.60
France	1890	43	1.49	Belgium	1890	47	1.60	Belgium	1860	54	1.66

[14] To illustrate the effect of social inequality on the mean height and height variance of the population, consider two different allocations of resources after birth (on the following, see Moradi and Baten 2005): All resources are perfectly equally distributed among the people in society versus a situation where there exists an unequal distribution of resources. In the first case, the height distribution only reflects genetic factors. Despite perfect equality, we observe a *biological variance* of (normally distributed) heights because individuals are differently endowed with their genetic inheritance. In the latter case, the unequal allocation of resources allows some individuals to achieve their genetic maximum height because they have access to resources, while others who do not will stay shorter. In practice, while most height distributions are normally distributed or very close to normal, the variance of the distribution is larger than in the case of social equality.
Moradi and Baten (2005) have estimated the relationship between income inequality and height CV for 14 African countries and 29 five-year periods, controlling for the differences in income definition and population coverage. They found that height CV was significantly and positively correlated with the Gini coefficients of income.

Netherld.	1880	43	1.32	Belgium	1910	47	1.58	France	1850	55	1.68
Poland	1890	43	2.13	Belgium	1830	47	1.67	Belgium	1840	57	1.66
Germany	1860	43	1.47	Poland	1870	47	2.09	Spain	1850	58	2.02
France	1830	43	1.74	Austria	1860	47	1.60	Spain	1910	59	2.00
France	1910	43	1.48	Poland	1850	48	2.37	Belgium	1850	59	1.67
Netherld.	1850	44	1.32	Germany	1870	48	1.38	Spain	1890	65	2.09
Poland	1880	44	1.99	Austria	1880	48	1.60	Spain	1840	70	2.13
Germany	1910	44	1.55								
Average		41	1.63			46	1.72			54	1.96

Sources: Blum and Baten (2011). Included are only cases for which both height ginis and skill premia are available.

All in all, the relationship between Gini coefficient of income and height gini is well-established but was never before applied in the migration literature. The validity of height Ginis can also be counter-checked by comparison with other inequality evidence, such as skill premia, as Blum and Baten (2011) recently did. The authors kindly provided their data set, so skill premia can be compared with height ginis here. In Table 2, some of the height ginis are presented in ascending order, jointly with evidence about skill premia during the 19th century. Skill premia are defined in this study as the wage ratio between a skilled worker in the building trades, and an unskilled one. If we take the average skill premium of the first column – which represents the cases where height ginis are low – also skill premia are low on average; much lower than the skill premia of columns 2 and 3, which are those cases in which height ginis are middle and high. There are clearly some outliers, but overall there is a correlation between the two measures (Correlation coefficient in the full Blum Baten sample is 0.48, p-value 0.000).[15]

Were the destination or the source countries more unequal during the era of mass migration? If we calculate the Gini coefficient weighted by the number of observations, we obtain 40.5 for the source countries and 40.3 for the destination countries. Hence there is

[15] Clearly, the skill premium as a measure of inequality does not cover the entire economy and the assumption that inequalities between skilled and unskilled building workers reflects skill premia in other sectors of the economy might not always hold. On the one hand, sources of income like subsistence farming, household production, public goods and black market economies are not captured by skill premia. Those latter parts of the economy, however, can be covered quite well with anthropometric measures. On the other hand, wage differences are expected to result in differences in biological living standards.

almost no difference between the two. If we include only those observations which can be included in the regressions below (Table 5, Column 1), the result is identical.

Among the destination countries not represented in the skill premia data set, Norway and Canada had low anthropometric inequalities with values between 41 and 48, whereas the U.S. inequality was not low. Unfortunately, we do not have skill premia for Ireland separately, so we cannot compare this important source country. The overall inequality of UK was in the medium range. Poland, for example, had relatively low inequality. As the UK had higher inequality than Poland, we would expect more skilled migration from Poland to the UK.

While the anthropometric inequality measures allow to cover many countries and decades, we were curious whether the results would be confirmed with other, non-anthropometric inequality measures, even if those might be only available for a subset of observations. Those are based on direct income gini coefficients, ginis estimated based on the wage-to-GDP proxy suggested by Williamson, and the share of income received by the richest fraction of the population (van Zanden et al., 2011, explain how those indicators are transformed to become comparable). We test below whether those non-anthropometric inequality measures yields the same results as our basic specification.

4. Data

For measuring human capital selectivity of migrants, it is necessary to measure both the human capital of migrants and of the population of the source country. For the migrants, we use data sets from the IPUMS and the North Atlantic Population Projects that provide 100 percent census samples for the late 19th century for a number of countries, and smaller samples for other countries.[16] We only use information on individuals that are older than 23, because younger people still are more aware of their age. For numeracy of source countries,

[16] See data appendix.

we use published national censuses of a great number of countries that were originally compiled by Crayen and Baten (2010a).[17]

Geographically, we cover 52 source countries in Europe, Latin America, Asia, Asia-Pacific and Africa to the US, the UK, Canada, Argentina and Norway as destination countries (Table 3). With our five destination countries, our panel allows to observe 127 country pairs. For some countries, we observe both immigration and emigration. The global nature of our data set allows an in-depth analysis of international migration during the 19th century. The migration decades range from the period 1820 - 1829 up to the period 1900 – 1909.[18] Cases with less than 50 observations are excluded. The U.S. immigration before the 1880s is better documented than thereafter, because the NAPP project provided a 100% sample of the U.S. census in 1880, and smaller samples before and after.

Table 3: Underlying number of cases by source country

Country	Cases	Country	Cases	Country	Cases
Ireland	1877232	Mexico	45828	Barbados	1841
Germany	1719228	Netherld.	42674	India	1208
UK	978433	Austria	32834	Iceland	1159
Canada	467201	France	29777	Uruguay	1050
Sweden	205227	Russia	26044	Greece	845
Norway	138013	Portugal	11362	Brazil	844
Belgium	101223	Luxembg.	10902	Hong Kong	812
China	86092	Spain	9274	Turkey	812
Switz.ld	75371	Hungary	8589	Romania	751
Czech	60458	Finland	8021	Jamaica	670
Denmark	53816	Cuba	4683	Japan	548
Italy	51385	Australia	2229	Bermuda	430
US	47985	Chile	1978	Bolivia	244
Poland	46183				

Sources: see data appendix.

[17] In Appendix D, we show the age distribution 43-82 of the UK population (census 1881) and of UK immigrants to the U.S. in the American census of 1880. They exhibit the typical spikes at ages that are multiples of five. These are more extreme among the migrants, reflecting a negative selectivity in this case.

[18] In Appendix Table D.1, the average number of underlying observations is reported for each source country, decade, and destination country.

5a. How did migrant selectivity develop during this period?

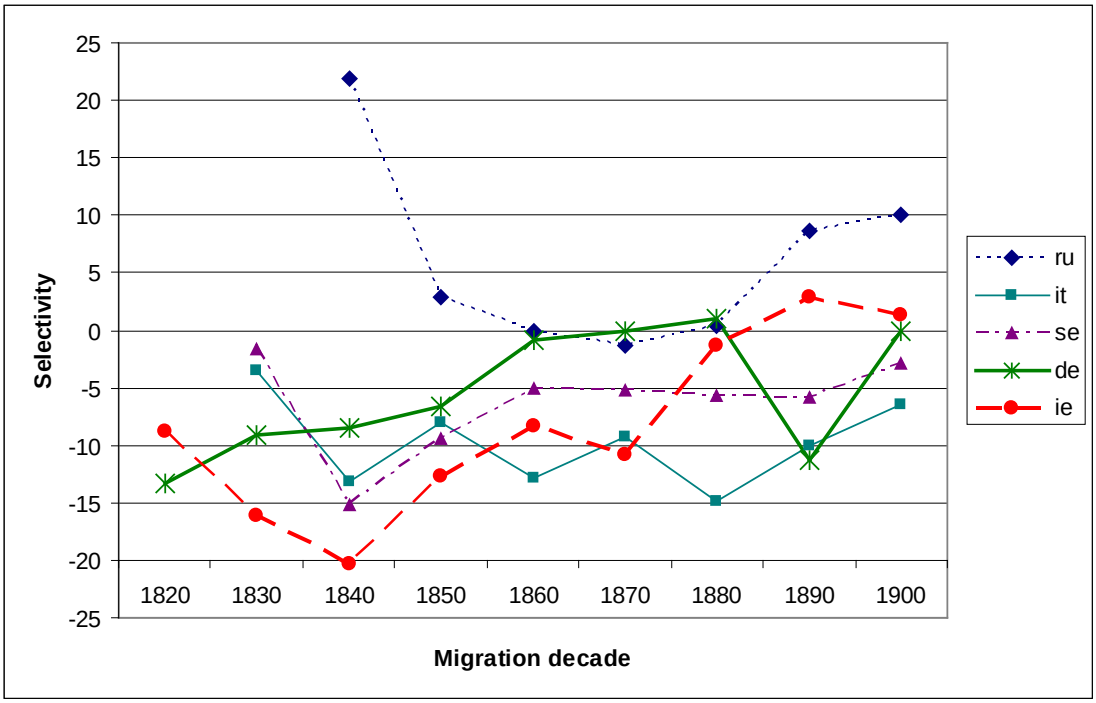
We first take a closer look at our dependent variable, which is defined as the numeracy of migrants minus the numeracy in the source country (both in percent). For the investigated period, average numeracy was almost equal in the source countries (90 percent), and in the destination countries (89 percent). The average numeracy of migrants was slightly lower, namely 87 percent (arithmetic mean by source country), or 86 percent (mean weighted by migrant numbers). On average, there was, thus, no numeracy brain drain, but rather a mathematical brain gain for the source countries, because migrants who left in the 19th and early 20th century were slightly less numerate than the remaining population. The difference is, however, small so that it is more meaningful to look at the variation of brain drain and brain gain between countries and over time and to study the determinants. In the following, we analyze some prominent examples of emigrant countries sending migrants to the U.S. and UK. We arrange all numeracy values by migration decade.

The largest migrant flows to the United States in this period came from Germany and Ireland. These migrants were mainly negatively selected for the early cohorts of our sample (Figure 1, Panel A).[19] We actually find 6-13 percent lower numeracy among those Germans migrating during the 1820s-1850s. Irish migrants display a stronger negative selectivity, perhaps due to the Great Famine years, since remittances sent over by previous migrants were also used by the less educated to leave the country. Those who migrated in the "hungry 1840s" display a value that is 20 percent lower than those, who stayed in Ireland. Over time, this negative selectivity diminishes and eventually dissolves completely for the migration cohorts 1880-1900.[20]

[19] We consider Ireland separately, although it was part of the British Empire, because the characteristics of Irish migrants were different.

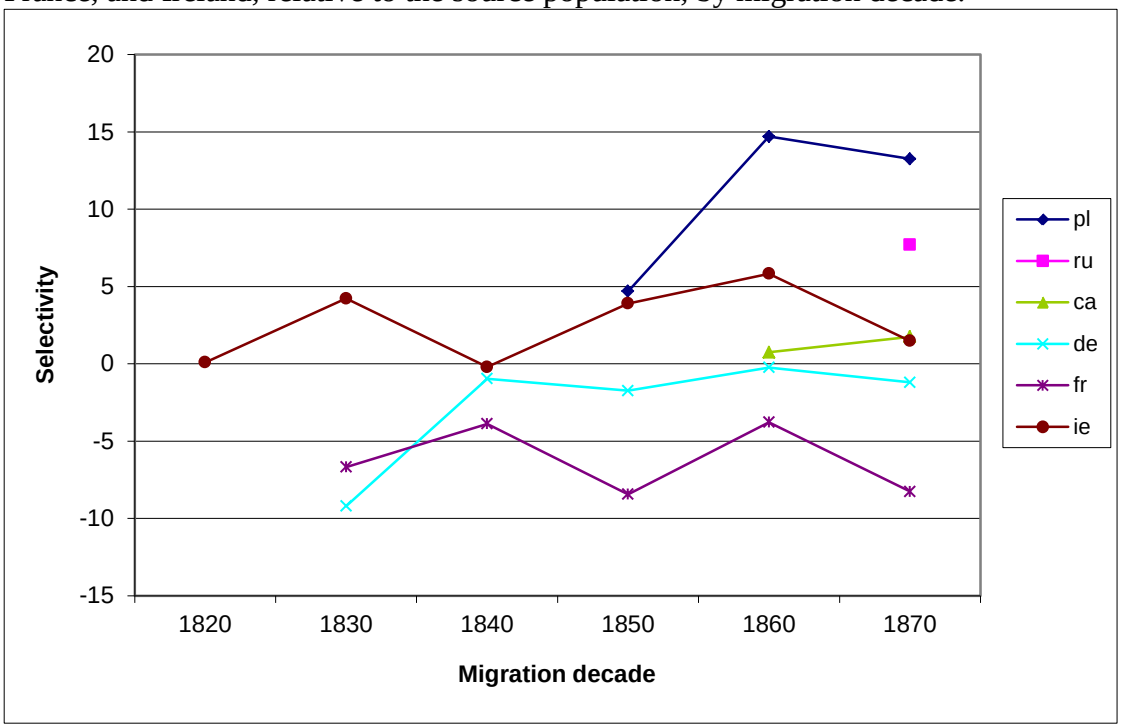
[20] Except for the small dip in German selectivity, this might have been caused by the economic crisis of the early 1890s initiated by the Baring crisis.

Figure 1: Panel A: Selectivity among U.S. immigrants from Germany and Ireland ("old migration countries"), as well as from Russia, Italy and Sweden relative to the source population, by migration decade.



Sources: see data appendix.
The year on the horizontal axis indicates the beginning year of a migration decade

Figure 1, Panel B: Selectivity among UK immigrants from Poland, Russia, Canada, Germany, France, and Ireland, relative to the source population, by migration decade.



Sources: see data appendix.

The year on the horizontal axis indicates the beginning year of a migration decade.

Among the "new immigration areas" in Eastern and Southern Europe – and the Nordic countries such as Sweden – the development is quite different (Figure 1, panel A). The Swedes and Italians show a modest negative selectivity over the whole period with no major changes. In contrast, the Russian immigrants initially are very positively selected (although numbers of migrants were small). The earliest cohorts migrating in the 1840s are more than 20% more numerate than their compatriots staying at home.[21] This is partly due to the fact that large shares of Russian immigrants were Jews, who have a reputation for being better educated than the overall population. Additionally, the high costs of migration from Eastern Europe translated in highly skilled first-wave migrants. Afterwards, there are probably strong "friends and relatives"-effects at work, also supported by remittances, as illustrated by the fact that the strong positive selectivity of the first decades decreases among the later cohorts.

As a second example of a migration destination, England is a particularly interesting case (Figure 1, Panel B). Here, immigration is predominantly Irish in the first cohorts. These individuals are on average slightly positively selected (between 0 and 5 percent). Therefore, Ireland experiences some brain drain to England, but a brain gain migration to the US. Also, Poland and Russia, and to a lesser extent Canada suffer from brain drain effects due to migration to England. France and Germany, in contrast, did not experience brain drain with their modest migration flows to England.

In sum, although migrants are on average slightly negative selected, the variation between countries is large. Especially during the mid-19th century waves of migration, some of the main source countries display negative migrant selectivity partly caused by payments of source country government institutions who wanted to send away the poorest, and partly financed by remittances of earlier migrants (especially important for the Irish migration, see Cohn 2009, for the German case see Bade 2008). In contrast, Eastern European migrants are quite positively selected.

[21] The immigration cohort of the 1830s would have been even more positively selected, but we removed it from the figure due to quite small sample size, in order not to provide an inadequate impression. Thanks to Ray Cohn for his important comment on this.

5b. What determines migrant selectivity?

We first estimate the factors determining migrant selectivity in an OLS framework using the following equation:

$$(3) \quad S_{ijt} = Inequ_{ijt} + \log(Mig_{ijt-1}) + PovConst_{it} + Log\,(Dist_{ij}) + PovConst_{it} * Log\,(Dist_{ij}) + Dem_{ijt} + Language_{ij} + Colony_{ij} + Civil\ War_{it} + Relative\ Democracy_{ijt} + Fixed\ Effects_i + Fixed\ Effects_j + Fixed\ Effects_t$$

Skill-selectivity of migrants S_{ijt} observed in destination country *j* from source country *i* in migration decade *t* is our dependent variable. The prominent explanatory variable is $Inequ_{ijt}$, which is the relative Gini coefficient from destination and source country in a given migration decade. This is the coefficient of interest, when analyzing the Roy-Borjas relationship. Next, we control for a set of standard migration variables that could also impact on migrant selectivity, such as the friends-and-relatives-effect, which we proxy with the log of the absolute immigrants observed in destination country *j* from source country *i* in t-1, $log(Mig_{ijt-1})$ that is, in the decade previous to the estimated migration decade. $PovConst_{it}$ is a variable that controls for the development level of the source country *i* as a possible poverty constraint. The log distance, $Log\,(Dist_{ij})$, between the capitals of source and destination countries is also included. The poverty constraint is also interacted with log distance, since we would expect the poverty constraint to be more binding in case of a transatlantic journey then in the case of intra-European migration because of the lower material and psychological migration costs in the latter case. Additionally, we control for common language, $Language_{ij}$ and colonial ties, $Colony_{ij}$.[22] And finally, we control for $Relative\ Democracy_{ijt}$ of source and destination country and $Civil\ War_{it}$ at the time of emigration in the source country.

Descriptive statistics are displayed in Table 4. The migrant selectivity variable is distributed between -27.9 and +52.0 numeracy points with an unweighted mean of -4.3, indicating that on average, migrants were slightly negatively selected. Relative Gini

[22] http://www.cepii.fr/anglaisgraph/bdd/distances.htm, last accessed 15.10.2010

coefficients are distributed between -28 and 22. The raw migrant share variable displayed some left skewness, which is why we take the logs.

Table 4: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Migrant selectivity	303	-4.3	8.2	-27.9	52.0
Relative inequality (anthrop.)	303	-3.9	8.2	-28.0	22.0
Relative inequality (non anthrop.)	82	1.0	12.7	-42.4	28.6
Ln migrant share	303	0.6	2.1	-5.0	4.6
Poverty constraint	303	0.9	0.3	0.0	2.1
Ln distance	303	3.0	1.1	0.5	4.6
Ln dist*pov. constr.	303	2.7	1.6	0.0	8.3
Relative democr.	303	1.3	3.5	-5.7	10.0
Common language	303	0.2	0.4	0.0	1.0
Colonial r'ship	303	0.2	0.4	0.0	1.0

Note: only the cases are included for which all explanatory variables (Table 5, Col 1) did not contain missing values. The variable “Relative inequality (non anthrop.)” refers to Table 7, Column 1. Sources: see data appendix.

In our estimations, we always employ destination and source country fixed effects to capture country specific political and socio-cultural characteristics as well as the income situation in destination and source countries.

As a result, *relative inequality* plays a consistent role in determining migrant selectivity (Table 5). The coefficients of this variable are positive and have the expected sign in all five specifications. The results confirm the relationship proposed by Roy (1951) and Borjas (1987). In the first regression, we include the Russian emigration, although it might have been partly determined by religious factors, as explained in the previous section. In the second to fifth column, it is excluded and our results remain the same. In column 2, 4 and 5 we tested fixed effects models in order to control for unobserved heterogeneity (which is otherwise controlled with country dummies). The coefficient for relative inequality is robust

in this specification, as well.[23] In column 4, we assess whether inequality matters only together with the friends and relatives effect, or only together with poverty constraints, which is not the case.

Table 5: Baseline regressions of human capital selectivity (numeracy migrant in % - numeracy source country in %)

	(1)	(2)	(3)	(4)	(5)
Estimation method	RE	FE	RE	FE	FE
Source countries excluded	None	Russia	Russia	Russia	Russia
Relative inequality dest - source	0.16***	0.17***	0.15**	0.13**	0.11*
	(0.003)	(0.004)	(0.017)	(0.034)	(0.063)
Friends & relatives (previous mig.)	-0.48	-0.68	-0.43	-1.53**	
	(0.204)	(0.286)	(0.186)	(0.013)	
Poverty constraint (Gini/GDP sq.)	-0.26**	0.24	-0.19*	-0.10	
	(0.025)	(0.109)	(0.081)	(0.503)	
Ln distance	-2.07***		-1.70**		
	(0.010)		(0.029)		
Ln distance * poverty constraint	0.06*	-0.10**	0.04	0.01	
	(0.099)	(0.023)	(0.240)	(0.854)	
Relative democracy	-0.45				
	(0.428)				
Common Language	2.65		3.93**		
	(0.127)		(0.047)		
Colonial relationship	1.07		0.70		
	(0.562)		(0.751)		
Civil War				-2.19**	
				(0.035)	
Destination	Yes		Yes	No	
Source	Yes		Yes	No	
Time	Yes	No	Yes	Yes	Yes
Destination-source country pairs	No	Yes, FE	No	Yes, FE	Yes, FE
Constant	1.28	-2.11*	-4.16	-3.11*	-1.11
	(0.803)	(0.078)	(0.440)	(0.085)	(0.490)
Observations	303	300	291	300	376
R-squared (between)	0.854	0.0292	0.858	0.0579	0.0530
R-squared (within)	0.160	0.0915	0.216	0.235	0.114
R-squared (overall)	0.593	0.0406	0.590	0.0379	0.0402

P-values based on robust standard errors are included in brackets. Migration decades 1820s-1900s are included. Column 1 and 3 are estimated with random effects models (but country dummies included), col. 2, 4 and 5 are based on fixed effects estimates. Hausman test P-Value (0.2345) suggests, however, that a random effect estimator is consistent and efficient. Sources: see data appendix.

[23] However, the Hausman test indicates that the random effects model is consistent and efficient (see notes to Table).

Is the coefficient of relative inequality economically meaningful? One method of measuring economic significance is to consider the effects of one standard deviation of the explanatory variable. If we multiply the standard deviation of inequality (8.21) with its coefficient (0.17, col. 2 in Table 5), we obtain 1.39. This is roughly 17% of the standard deviation of the dependent variable (standard deviation: 8.16), which means that it can explain roughly one sixth of the standard deviation of the dependent variable. This amount is not very large, but it is also not negligible. If we do the same with the coefficient of the non-anthropometric relative inequality variable below (Table 7, column 1: 0.29, and Table 4, line 3), we obtain 3.68, which is around 45% of the standard deviation of the dependent variable. This is a substantial share, indicating economic significance.

The other variables had much less consistent effects. *The friends and relatives effect* has always the expected negative sign, but is only statistically significant in specification 4, Table 5 (and in Appendix Table D.2, column 1). This indicates some impact of already existing networks on the skill selectivity of new migrants, but the insignificance of many coefficients suggests that this effect was not very systematic. The provision of information and remittances might have encouraged also less positively selected individuals to migrate in some of the cases.

The *poverty constraint* variable renders also no consistent results. It mainly has a negative sign and is twice significant. While Hatton and Williamson (1998) found that it was a determinant of migration flows, but human capital selectivity does not seem to be consistently related to this variable.

We calculated a time variant measure of economic distance costs.[24] The strong decline of transport cost with the arrival of the steamship innovation features prominently here. *Log* $(Dist_{ij})$ has a negative sign and becomes significant. This result might seem counter-intuitive, but the result might be due to the fact that the majority of the variance in the distance variable

[24] We took the passenger cost estimates by Sanchez-Alonso (2008), and calculated the cost for distance unit for each decade. This is then multiplied with actual distances between population centres in the countries. (distance measures from http://www.cepii.fr/)

stems from the difference in trans-Atlantic versus intra- European migration. We observe better selected individuals in European destination countries than in the U.S. or Argentina, for example, perhaps because the risky environment of the New World deterred skilled migrants.[25]

Relative democracy is controlled for based on the estimates of democracy produced by the Polity IV project.[26] It indicates the openness of democratic institutions in a country and is measured on a scale of -10 (low) to 10 (high). We subtracted the democracy score of the destination country from the one of the source country to obtain the relative democracy variable. One might expect that the more educated were attracted by higher democracy values in the destination country, relative to the source country (on the significant attraction of migrants of any skill, see Bertocchi and Strozzi 2008). This variable turns out to be insignificant, too. The politically motivated migration might have been too small in number during the 19th century, or it was probably not sufficiently restricted to the more educated strata.

Finally, we tested *common language* and *colonial relationships*, and found positive effects for language. A common language might have been more useful for the more educated who usually have a comparative advantage with words and skills, rather than with brawn. Colonial ties do not seem to matter. Finally, in column 4 of Table 5 we test for a potential effect of *civil war* in the country of origin, which turns out negative and significant. This

[25] This is supported by the fact that when old world destinations are not included below in Column 5 of Appendix Table D.2, the variable gets a large positive value (p-value 0.103). As a second possible explanation, it could be speculated that migrants within Europe expected more skill competition in European destination labor markets. Figures 1 and 2 indicate this particularly for the case of Irish emigrants.
We also include an *interaction term between economic distance and poverty constraint*, but it turns out mostly insignificant. In Table B.2 in the appendix we also tested whether the effect of distance and poverty constraint differ in the first half of the 19th century, because as shipping technology improved, travelling across the Atlantic became less time consuming and costly. Therefore an interaction term of distance and the time dummies of the first half of the 19th century was included (Appendix Table B.2). The result was a significant negative coefficient, indicating that in the early period, when migration was more costly, the impact of distance on migrant selectivity was even more negative than thereafter. This might be slightly more in line with the risk interpretation above, because in the early period the New World was still perceived as a risky world. An interaction term with the poverty measure and the first half of the 19th century did not render significant results.
[26] Marshall, Monty G., and Jaggers, K. (2008): Polity IV Project: data set.
http://www.systemicpeace.org/polity/polity4.htm

variable is taken from the Correlates of War Project and Uppsala Conflict Data Project. It is coded as a dummy variable, turning 1 if civil war broke out in a given country and period. [27]

5c. Tests of robustness, WLS, and direction of causality issues

As a first test for robustness, we omitted the German, Irish and English migrants to test if our result were mainly driven by these very large immigrant groups. The results do not differ very much, and the Roy-Borjas forces remain strong (Appendix Table D.2, columns 1-3).[28]

An alternative robustness test is to weigh the observations with the square root of the number of migrants underlying each unit in a WLS regression which leads to more efficient estimates. The results in Table 6, column 1 to 4, are consistent with previous estimates: the relative inequality is a significant determinant of migrant selectivity. One potential disadvantage of weighted regressions is that a few source countries account for the majority of migrants; and thus they receive most of the weight in the estimates.[29]

Finally in column 5 of Table 6, we test the alternative measure for the friends-and-relatives effect that we described in the notes to the Table, namely the number of migrants relative to home country population. Using this different specification does not change the

[27] Civil war is defined as sustained combat with at least 1,000 battle-related deaths per year that takes place between the armed forces of a government and forces of another entity for central control or for local issues. Military and civilian deaths are counted. Source: Correlates of War Project and Uppsala Conflict Data Project. http://www.correlatesofwar.org/

[28] If the Irish are omitted, the friends-and-relatives effects turns small and insignificant, this is neatly consistent with the literature that argued that this effect was particularly important for Irish migration (Cohn 2009). Next, we exclude the Irish and the Italian migrants, because here, the migration flows were very large in comparison to the home country population. Another way of validating the robustness of our results is to exclude migration to European destination countries (Appendix Table D.2, column 5). The result is that even if we look at only transatlantic migration, our results stay robust.

[29] While in the OLS estimations, common language had a positive effect on human capital selectivity, the WLS regressions suggest the opposite. Here the Irish, who were negatively selected and shared a common language with North Americans, gain a strong weight, because their N was large. A former colonial relationship also seems to be of importance in WLS: this variable turns highly significant and positive in all estimations, indicating that people, who migrated into a country to which they were linked by colonial ties, were more positively selected than migrants moving to other destination countries. This might have been caused partly by re-migration of the families of former colonial officials or similar special factors of the colonial administration. We also assessed whether the difference between migrant numeracy and source country numeracy might depend on the level of source country numeracy. Those coming from a high education background might have been more likely to be negatively selected, even if we have seen many counter-examples in the Figures discussed above. We therefore include a term "ABCC level source country", which indeed turns significant but did not change the main results (Table 6, column 3). In a similar exercise to evaluate the properties of the dependent variable, we included only those in which the source country numeracy deviates from the optimum of 100 percent (Table 6, column 4). This removes some 35 cases, relative to column 2, but again the coefficients do not change.

significance of the Borjas variable, and most other significance levels also do not change (distance is the exception).

A comprehensive test of the properties of time series indicated that the main series as well as the residuals do not display unit root problems. The Fisher test for unbalanced panels, as well as the Hadri-LM-test for the three largest source countries in a balanced panel, were calculated and suggest that our series do not suffer from non-stationarity.

Table 6: Regression of human capital selectivity, weighted by number of underlying observations

	(1)	(2)	(3)	(4)	(5)
Estimation method	WLS	WLS	WLS	WLS	WLS
Included abcc range	All	All	All	<100	<100
Friends and relatives, relative to	Migrants	Migrants	Migrants	Migrants	Source country
Relative inequality dest - source	0.15**	0.14**	0.17***	0.16**	0.11*
	(0.017)	(0.018)	(0.002)	(0.012)	(0.071)
Friends & relatives (previous mig.)	-0.03	-0.03	0.16	-0.44	0.17
	(0.908)	(0.909)	(0.527)	(0.143)	(0.352)
Poverty constraint (Gini/GDP sq.)	-0.19	-0.19	-0.10	-0.08	0.22
	(0.167)	(0.163)	(0.456)	(0.685)	(0.196)
Ln distance	-2.63***	-2.64***	-1.96***	-2.58***	-0.38
	(0.000)	(0.000)	(0.009)	(0.001)	(0.701)
Ln distance * poverty constraint	0.06*	0.06*	0.03	0.03	-0.07
	(0.090)	(0.088)	(0.384)	(0.492)	(0.114)
Relative democracy	0.30				
	(0.453)				
Common Language	-1.10	-1.10	-1.23	-0.52	0.80
	(0.447)	(0.447)	(0.365)	(0.745)	(0.773)
Colonial relationship	5.57***	5.57***	5.81***	6.52***	6.76**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.021)
ABCC level source country			-0.71***		
			(0.000)		
Destination	Yes	Yes	Yes	Yes	Yes
Source	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes
Constant	4.17	2.53	71.15***	4.95	0.01
	(0.374)	(0.632)	(0.000)	(0.389)	(0.999)
Observations	303	312	312	267	201
R-squared	0.64	0.64	0.71	0.65	0.70

P-values based on robust standard errors are included in brackets. Migration decades 1820s-1900s are included. Russia excluded. Sources: see data appendix.
The line "Friends and relatives relative to" defines the concept which we applied to calculate the friends and relatives effects: We focus on two relative measures of the "friends-and-relatives" factor: (1) the number of migrants relative to the home country population (used in Column 5) and (2) the number of migrants of a given nationality, relative to the total migrant population in a destination country (used in Column 1 to 4 and other Tables). Both refer to the decade preceding migration of the individuals whose selectivity we aim to explain. Concept (1) would measure the number of potential relatives who could send remittances, per capita of the

source country. Assuming that the wealth of all previous immigrants would be similar, this is a convincing indicator. However, it has considerable data requirements, and population estimates in some of the source countries are not available. We could construct this measure only for 201 country pairs, for which relative inequality and other explanatory variables were available.

Table 7: Regression of human capital selectivity on non-anthropometric inequality measures

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation method	RE	RE	FE	WLS	WLS	WLS
Relative inequality dest - source	0.29**	0.28*	0.24**	0.34*	0.34*	0.38*
	(0.024)	(0.058)	(0.029)	(0.097)	(0.097)	(0.080)
Friends & relatives (previous mig.)	-0.24	-0.23	-0.24	0.16	0.16	0.37
	(0.657)	(0.729)	(0.813)	(0.738)	(0.738)	(0.391)
Poverty constraint (Gini/GDP sq.)	-11.47**	-11.53**	-5.49	-16.74**	-16.74**	-11.84
	(0.043)	(0.021)	(0.321)	(0.032)	(0.032)	(0.146)
Ln distance	-1.95	-2.12		-4.13***	-4.13***	-3.58**
	(0.244)	(0.213)		(0.008)	(0.008)	(0.012)
Ln distance * poverty constraint	2.26	2.41	0.23	5.93***	5.93***	5.02**
	(0.269)	(0.235)	(0.928)	(0.006)	(0.006)	(0.023)
Relative democracy	-0.38	1.61*		-0.47		
	(0.745)	(0.068)		(0.540)		
Common Language	0.74	0.64		-0.08	-0.08	-1.45
	(0.835)	(0.887)		(0.981)	(0.981)	(0.603)
Colonial relationship	-1.78	-1.87		-2.08	-2.08	-1.25
	(0.537)	(0.691)		(0.411)	(0.411)	(0.622)
Civil War		-1.10	-0.50			
		(0.668)	(0.841)			
ABCC level source country						-0.74
						(0.103)
Destination	Yes	Yes	No	Yes	Yes	Yes
Source	Yes	Yes	Yes, FE	Yes	Yes	Yes
Time	No	No	No	Yes	Yes	Yes
Constant	-4.34	7.79	-0.84	5.35	61.01***	87.60***
	(0.632)	(0.390)	(0.789)	(0.424)	(0.000)	(0.000)
Observations	82	82	82	82	82	82
R-square between	0.866	0.867	0.0275			
R-square within	0.215	0.216	0.232			
R-square overall	0.788	0.789	0.0318	0.79	0.79	0.81

Source: Sources: see data appendix. Van Zanden et al. (2011) recently presented estimates of global inequality based on both anthropometric and non-anthropometric inequality measures, and they kindly provided the latter to us.

Table 8: Arellano Bond dynamic panel regressions

	(1)	(2)
Relative skill premium dest - source	0.13***	0.10**
	(0.002)	(0.018)
Relative inequality dest - source	0.21	0.09
	(0.107)	(0.475)
Friends & relatives (previous mig.)	0.17	-0.44
	(0.808)	(0.557)
Poverty constraint (Gini/GDP sq.)		-0.06
		(0.783)
Log distance		-5.12**
		(0.039)
Ln distance * poverty constraint		0.00
		(0.984)
Constant	-3.50***	12.81*
	(0.000)	(0.094)
Observations	228	228
No(instruments)	45	48
p-value of Wald chi2	0.002	0.000

Migration decades 1820s-1900s are included. Russia excluded. We use the entire lag structure for instrumentation, i.e. starting from the (t-2) lag of the difference for the levels equation, and the (t-1) lag of the level for the difference equations. Arellano-Bond test for AR(2) in first differences. Prob > z: 0.19. The Sargan test of overidentifying restrictions yielded a chi2 of 47.26 (Prob > chi2 = 0.23). Sources: see data appendix.

As a further robustness test, we assessed whether the results would be confirmed using non-anthropometric inequality measures. As explained above, Van Zanden et al. (2011) recently presented estimates of global inequality based on both anthropometric and non-anthropometric inequality measures, and we include the latter in Table 7. Those non-anthropometric inequality measures actually yield the same signs for the Borjas variable as our basic specification and their coefficient is even somewhat larger, which is probably caused by the different set of countries and decades that can be covered with this alternative measure of relative inequality. This result is a very strong confirmation of robustness of the main results of interest here. Moreover, we also performed a robustness test about the accuracy of migration decade estimates.[30]

[30] Previously, our assumption was that, for example, those rounding on 40 would do so from true ages symmetrically lower or higher (for example, ages 38 to 42 being rounded to 40). For the robustness test, we now

Next we considered the problem of endogeneity. Theoretically, one could imagine that a massive exodus of a large and highly selected part of a population would influence relative inequality of the source country, since relative inequality should ceteris paribus decline, if, for example, a large share of unskilled workers leaves. In most countries, however, the requirement of a large share of the population leaving is not fulfilled, since emigration rates were normally below 5 percent per decade. Exceptions are Ireland, from where in some decades more than 10 percent left, and Italy right before WWI (Hatton and Williamson, 1998). This means that a problem of reverse causality might arise as a large, and strongly selective migrant flow might in turn affect income distribution in the source country. The only migration flows that were so large are the Irish and the Italian. If we exclude them from our regressions, the results stay robust (Appendix Table D.2, column 4). Hence, we conclude that the direction of causality issue is not affecting our results generally.

Finally, we tested whether our results also hold when a Generalized Method of Moments estimator is applied (Arellano and Bond, 1991). While this method is conventionally used in dynamic settings to account for the likely endogeneity of lagged dependent variables, it basically generates a large number of instrumental variables from lagged first difference values of the dependent variable. This estimation in first differences is of advantage, because it allows us to make sure that trend correlation is not a problem here. Again, the relative inequality coefficients turn out robust (Table 8).

To conclude, a wide range of econometric techniques suggests that relative inequality had an effect on migrant selectivity as measured by relative numeracy with the age heaping method. There is some evidence -- although more limited -- on friends-and relatives-effects,

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hypothesized that only ages 37 to 41 rounded to 40. In a second step, we did the same with 39 to 43. We are well aware that this introduces artificial measurement error, because the symmetric rounding is the more likely factual behavior. Hence the results would be expected to yield slightly less significant results, but a small variation should not completely change the results. This is exactly what happened. Of the coefficients, 100 percent kept the same sign and the same order of magniturde, and almost all remained significant. In very few cases, the p-values of statistical significance went slightly over 0.10, as we would have expected since we are introducing artificial measurement error in this robustness test.

colonial relationships and common language, whereas counteracting forces might have rendered the effects of economic distance and democracy mostly insignificant.

Conclusion

In this study, we assessed the selectivity of migrants in the era of mass migration. We focus not only on the main transatlantic migration destinations, but also on two European destination countries, the UK and Norway. No less than 52 source countries could be included, with 127 country pair flows. The underlying data set is based on 6.2 million individual migrants.

The main model tested is the Roy Model of self-selection (1951) that Borjas applied to the process of migration (1987). It states a relationship between skill selectivity of migrants and relative inequality of source and destination countries, measured with an anthropometric indicator here. We confirm the influence of these economic migration incentives after controlling for a large number of other variables such as “friends and relatives effects”, poverty constraints, economic distance, relative democracy, common language and colonial relationships. Even if we used non-anthropometric inequality measures, regressions actually yield the same signs for the Borjas variable as our basic specification and the coefficient is even somewhat larger. This study has been the first general assessment of migrant selectivity during this most crucial period of human migration history, using large samples that included a variety of different source and destination countries.

It is crucial to understand the brain-drain processes between source and destination countries, because the stock of human capital determines future growth capabilities. Brain drain effects have not been systematically studied for the era of mass migration of the mid-to-late 19th century with large international samples before. In the case of mid-19th century mass migration history, we also noted some arithmetic brain gains for the source countries, since those who left Scandinavia or central Europe around mid-century were often less numerate than the remaining population. There could have been, for instance, marginal positive human capital growth effects in Germany or in some Scandinavian countries, because the average

numeracy should have increased with migration, due to negatively selected emigration. In contrast, Eastern Europe lost a large number of the numerate population, and the migration effects might have been *ceteris paribus* negative. Clearly, also a large number of other factors were at work, which is why these effects should not be seen in isolation.

Can we draw a wider pricture, comparing today's migration with the era of mass migration? Two main differences are obvious. Firstly, in the world today, immigration policies play a much stronger role than in the 19th century, making it very hard for many unskilled potential migrants to jump over immigration hurdles. Hence each consideration of relative inequality incentives has to be *ceteris paribus*. A second major difference in this big picture is that today many migrants come from countries with higher inequality. This fact fueled the Borjas debate about potentially problematic selectivity of migrants coming from Mexico to the United States in the recent past. In the 19th and early 20th century, this was clearly different, because source and destination country inequalities were almost equal. However, the debate about migrant education in the U.S. before WWI was also mostly motivated by immigration from countries in Southern and Eastern Europe which were characterised by relatively high inequality. Even if the absolute level of education was the main issue of debate, the Borjas forces were also influential in the background. In sum, when designing immigration policies today, both the recent and the historical experience can provide important insights about the economic forces which determine the selectivity of migration.

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Appendix A (not to be included in the published version, but available in the internet)

Data Appendix

Migrant numeracy: Difference between the numeracy of migrants and the joint weighted numeracy of stayers and migrants (except for countries in which migration was small, see the longer footnote in the 'skill selectivity' section). Numeracy in the source countries are from Crayen and Baten (2010a). On the sources of migrants: Census evidence was available for Argentina (1869, 1895) – sample of circa 2 percent; Canada (1871, 1881-100%, 1901); Norway (1865, 1875, 1900); England (1851, 1881); US (1850, 1860, 1870, 1880-100%, 1900, 1910). Sources of those censuses: On the U.S. except 1880: Ruggles, Steven, Matthew Sobek, and Trent Alexander, et al. *Integrated Public Use Microdata Series: Version 3.0* [Machine-readable database]. Minneapolis, MN: Minnesota Population Center [producer and distributor], 2004. On Argentina: Somoza, J. and Lattes, A. (1967. Muestras de los dos primeros censos nacionales de población, 1869 y 1895. Documento de Trabajo No 46, Instituto T. Di Tella, CIS, Buenos Aires. On all other samples: North Atlantic Population Project and Minnesota Population Center. NAPP: Complete Count Microdata. NAPP Version 2.0 [computer files]. Minneapolis, MN: Minnesota Population Center [distributor], 2008. [http://www.nappdata.org].
We used the migration numbers in Ferenczi and Willcox to identify the countries in which the migration rate exceeded one percent per decade to a given target country (in most cases, there was only one target country with such substantial migration). For the periods before 1870, we used the stock of migrants in the target countries, and compared overlapping numbers between Ferenczi and Willcox and census data in order to make sure that the differences in counting (Ferenczi and Willcox focus on migration statistics, hence an Irish migrant to Canada might have finally gone to the U.S; the census stock excludes those who died between migration and census taking). But the correspondence between both sources was quite good. For example, for the 1860s Ferenczi and Willcox list some 700,000 migrants from the UK (incl. Ireland) to the U.S., whereas the stock in the 1880s that we estimated to have migrated during the 1860s was 660,000. We then calculated the weighted average of numeracy of stayers and migrants. Only for very few cases we had to assume similar values to the ones of other migrants (for example, we assumed that Spanish migration to Brazil in the 1880s was similar to the one to Argentina in the 1880s etc.).

Relative Inequality Dest-Source (anthrop.):: height gini of the destination county minus the height gini of the source county, from Blum and Baten (2012), Estimating Inequality with Anthropometric Indicators, for an online version, see http://www.wiwi.uni-tuebingen.de/cms/fileadmin/ Uploads/Schulung/Schulung5/ Joerg/Baten_Blum_skpr100331a.pdf, last accessed March 31st, 2010.
Moradi and Baten (2005) recommended the following formula for translating height CVs into 'height ginis' (ibid: p.29), which we will use below:

(3) $$Gini_{it}=-33.5+20.5*CV_{it}$$

Relative Inequality (non-anthrop.): Gini coefficients of income inequality of the destination county minus the Gini coefficient of the source county, from van Zanden et al. (2011), friendly provided by the authors. The authors include all estimates of income inequality: direct estimates based on tax and other statistical surveys, indirect estimates based on the share of income of the richest fraction of the population, and indirect estimates based on the ratio between average income and the wages of unskilled workers (the 'Williamson method').

Friends and relatives (previous mig.): The share of migrants from one source country in one destination country, relative to the total number of migrants in this destination country. It was calculated with migrant data sets cited above under *Migrant numeracy*. In Table 6, column 5, this variable is defined as the number of migrants in country j from country i, relative to the population in country i. Population comes from Maddison (2009). When census years were less than two decades from each other, we used linear interpolation for the number of migrants for the latter measure, and population figures were also interpolated between Maddison's years of observation, but only if sensible start year population figures were available.

Poverty constraint: Height ginis divided by GDP per capita squared (multiplied with 1 million), Height ginis are based on Blum and Baten (2011, see *Relative* above), GDP on Maddison (2009), and for those countries for which values were lacking we used the imputations first done by Baten and Blum (2010), see http://www.wiwi.uni-tuebingen.de/cms/fileadmin/Uploads/Schulung/Schulung5/Joerg/baten_blum_ht_100331a.pdf last accessed March 31st, 2010.

Distance: Log distance is taken from http://www.cepii.fr/anglaisgraph/bdd/distances.htm last accessed March 31st, 2010. The distance was then multiplied with the passenger cost estimates by Sanchez-Alonso (2008) to account for the decline in distance costs.

Colonial ties: see *distance;* it is a dummy which is one if source and destination countries had a colonial relationship at some point in time.

Common language: see *distance.* It is a dummy which is one if the population majority in the source and destination countries had the same language.

Relative democracy: Evidence from the Polity IV project, see Marshall, Monty G., and Jaggers, K.(2008): Polity IV Project: data set. http://www.systemicpeace.org/polity/polity4.htm last accessed March 31st, 2010. We took the overall democracy index. The values vary between –10 and 10, and we took the value in the destination country minus the one of the source country.

Civil War data is from the Correlates of War Project, see Singer, J. David and Melvin Small (1972): The Wages of War, 1816-1965: A Statistical Handbook. New York. Or see http://www.correlatesofwar.org last accessed March 31st, 2010. It is a dummy variable which is 1 if a country had a civil war conflict with at least 1,000 battle deaths per year.

Internet Appendix B (not to be included in the published version): additional Tables

Appendix Table B.2: Regression of migrant selectivity: interaction with early/late period

	(1)	(2)
Estimation method	RE	RE
Source countries excluded	Russia	Russia
Gini Destination Premium	0.15***	0.15***
	(0.006)	(0.005)
Friends&Family (Ln Migr)	-0.51	-0.46
	(0.198)	(0.230)
Poverty Constraint	-0.20*	-0.18
	(0.061)	(0.106)
Ln distance	-1.54*	-1.74**
	(0.062)	(0.037)
Ln distance * poverty constraint	0.04	0.04
	(0.208)	(0.284)
Relative democracy	-0.41	-0.44
	(0.421)	(0.393)
Common Language	4.08**	3.99**
	(0.012)	(0.014)
Colonial relationship	0.73	0.88
	(0.706)	(0.665)
Early * distance	-1.14**	
	(0.029)	
Early * poverty constraint		0.00
		(0.987)
Destination fixed eff.	Yes	Yes
Source fixed eff.	Yes	Yes
Time fixed eff.	Yes	Yes
Constant	-0.18	-4.02
	(0.972)	(0.432)
Observations	291	291
R-squared (between)	0.864	0.864
R-squared (within)	0.216	0.201
R-squared (overall)	0.592	0.587

Note: As a threshold value for early/late, we used 1850, because the freight rate index by Harley (1998, also reprinted in O'Rourke and Williamson 1999) showed a much stronger decline of freight rates after 1850.

Not for publication: Appendix C: Methodology and basic concepts of age heaping (Internet Appendix)

We study numerical abilities in this article, which are an important component of overall human capital. In order to provide estimates of very basic components of numeracy, we will apply the age heaping methodology.[31] The idea is that in less developed countries of the past, only a certain share of the population was able to report the own age exactly when census-takers, army recruitment officers, or prison officials asked for it. The remaining population reported a rounded age, for example, 40, when they were in fact 39 or 41. In today's world of obligatory schooling, passports, universities, birth documents, and bureaucracy, it is hard to imagine that people did not know their exact age. But in early and less organized societies this was clearly different. The typical result is an age distribution with spikes at ages ending in a five or a zero and an underrepresentation of other ages, which does not reflect the true age distribution. There was also some heaping on multiples of two, which was quite widespread among children and teenagers and to a lesser extent among young adults in their twenties. This shows that most individuals actually knew their age as teenagers, but only in well-educated societies were they able to remember or calculate their exact age again later in life.[32]

To give an example of rounding on multiples of five, the census of Mexico City 1790 reports 410 people aged 40, but only 42 aged 41. This was clearly caused by age heaping. Apolant (1975, p. 333) gives individual examples of age misreporting: Joseph Milan, who appeared in February 1747 as a witness in an Uruguayan court, should have been 48 years old, according to one judicial record. However, in the same year, but in another judicial record, he declares his age to be '45 years'. Demographers see this age misreporting as a problem when calculating life expectancies and other population statistics. But exactly this misreporting enables us to approximate numerical abilities of historical populations. The ratio between the preferred ages and the others can be calculated by using several indices, one of them being the Whipple index.[33] To calculate the Whipple index of age heaping, the number of persons reporting a rounded age ending with 0 or 5 is divided by the total number of people, and this is subsequently multiplied by 500. Thus, the index measures the proportion of people who

[31] For more detailed surveys on the age heaping methodology see A'Hearn, Baten and Crayen (2009).
[32] At higher ages, this heaping pattern is mostly negligible, but interestingly somewhat stronger among populations who are numerate enough not to round on multiples of five.
[33] A'Hearn, Baten and Crayen (2009) found that this index is the only one that fulfils the desired properties of scale independence (a linear response to the degree of heaping), and that it ranks samples with different degrees of heaping reliably.

state an age ending in a five or zero, assuming that each terminal digit should appear with the same frequency in the 'true' age distribution.[34]

$$(1)\ Wh = \left(\frac{\sum (Age25 + Age30 + ...Age60)}{1/5 \times \sum Age23 + Age24 + Age25 + .. + Age62)} \right) \times 100$$

For an easier interpretation, A'Hearn, Baten, and Crayen (2009) suggested another index, which we call the ABCC index.[35] It is a simple linear transformation of the Whipple index and yields an estimate of the share of individuals who correctly report their age:

$$(2)\ ABCC = \left(1 - \frac{(Wh - 100)}{400}\right) \times 100 \text{ if } Wh \geq 100 \text{; else } ABCC = 100 .$$

The share of persons able to report an exact age turns out to be highly correlated with other measures of human capital, like literacy and schooling, both across countries, individuals, and over time (Bachi 1951, Myers 1954, Mokyr 1983, A'Hearn, Baten, and Crayen 2009). A'Hearn, Baten, and Crayen (2009) found that the relationship between illiteracy and age heaping for less developed countries (LDCs) after 1950 is very close. They calculated age heaping and illiteracy for not less than 270,000 individuals who were organized by 416 regions, ranging from Latin America to Oceania.[36] The correlation coefficient with illiteracy was as high as 0.7. The correlation with the PISA results for numerical skills was even as high as 0.85, hence the Whipple index is more strongly correlated with numerical skills. They also used a large U.S. census sample to perform a very detailed analysis of this relationship. They subdivided by race, gender, high and low educational status, and other criteria. In each case, they obtained a statistically significant relationship. Remarkable is also the fact that the coefficients are relatively stable between samples, i.e., a unit change in age heaping is associated with similar changes in literacy across the various tests. The results are not only valid for the U.S.: In any country with substantial age heaping that has been studied so far, the correlation was both statistically and economically significant.

In order to assess the robustness of those U.S. census results and the similar conclusions drawn from late 20th century LDCs, A'Hearn, Baten, and Crayen (2009) also assessed age heaping and literacy in 16 different European countries between the Middle Ages and the early 19th century. Again, they found a positive correlation between age heaping and

[34] A value of 500 means an age distribution with ages ending only on multiples of five, whereas 100 indicates no heaping patterns on multiples of five, that is exactly 20 percent of the population reported an age ending in a multiple of five.

[35] The name results from the initials of the authors' last names plus Greg Clark's, who suggested this in a comment on their paper. Whipple indexes below 100 are normally caused by random variation of birth rates in the 20th century rich countries. They are not carrying important information, hence normally set to 100 in the ABCC index.

[36] See A'Hearn, Baten and Crayen (2009), Appendix available from the authors.

literacy, although the relationship was somewhat weaker than for the 19th or 20th century data. It is likely that the unavoidable measurement error when using early modern data caused the lower statistical significance.

Age heaping has also been compared to other human capital indicators, for example, primary schooling rates. The widest geographical sample studied so far was created by Crayen and Baten (2010a), who were able to include 70 countries for which both age heaping and schooling data (as well as other explanatory variables) were available. They found in a series of cross-sections between the 1880s and 1940s that primary schooling and age heaping were closely correlated, with R-squares between 0.55 and 0.76 (including other control variables; see below). Again, the coefficients were relatively stable over time. This large sample also allowed the examination of various other potential determinants of age heaping. To assess whether the degree of bureaucracy, birth registration, and government interaction with citizens are likely to influence the knowledge of one's exact age, independently of personal education, the authors used the number of censuses performed for each individual country for the period under study as an explanatory variable for their age heaping measure. Except for countries with a very long history of census-taking, all variations of this variable turned out insignificant, which would suggest that an independent bureaucracy effect was rather weak. In other words, it is sometimes the case that societies with a high number of censuses had high age awareness. But, at the same time, these societies were also early in introducing schooling and this variable clearly had more explanatory power in a joint regression than the independent bureaucracy effect. Crayen and Baten also tested whether the general standard of living had an influence on age heaping tendencies (using height as well as GDP per capita to serve as a proxy for welfare) and found a varying influence: in some decades, there was a statistically significant correlation, but in others there was none. Cultural determinants of age heaping were also observable, but their strongest influence was visible in East Asia, not in the Latin American countries under study in this article.

In this article, we employ the ABCC measure of age heaping, computing indexes for different countries and birth decades. In order to do so, we use the age groups 23-32, 33-42, etc.[37] we omitted the age range from 63 to 72, as this age group offers too few observations, especially for the 17th and 18th centuries, when mortality was relatively high.[38]

[37] An advantage of this method is to spread the preferred ages, such as 25 or 30, more evenly within the age groups and it adjusts also for the fact that more people will be alive at age 50 than at age 54 or at age 55 than at age 59 (Crayen and Baten 2010b).

[38] Given that young adults aged 23 to 32 round partly on multiples of two rather than five, we use the adjustment method suggested by Crayen and Baten (2010a) to increase the Whipple value (minus 100) by 24 percent, before calculating the ABCC measure.

An advantage of the age heaping methodology is that age statements are more widely available than other human capital proxies like signature ability or school attendance. As Reis (2008) argues, the age heaping measure is a very basic measure of human capital. Therefore, it is especially valid to study human capital development in Latin America in the 17th and 18th centuries when more advanced human capital indicators were quite scarce and reflected only the skills of the elite.

Not for publication: Appendix C: Literacy as an alternative measure of skill selectivity

Interestingly, while some specialized studies have used the occupational structure and age heaping of migrants as indicators, literacy of migrants was not used before. Unfortunately, literacy of immigrants at arrival was only assessed in the U.S. starting in 1899, when the U.S. public grew concerned with the educational status of recent mass immigration from Southern and Eastern Europe, and those lists are not available as individual data sets.

Literacy was also recorded in the censuses between 1850 and 1910, but the comparison between the literacy of immigrants in the U.S. and the population in the source country is difficult for a number of reasons. Firstly, literacy in source countries was recorded using a number of different definitions. Some sources recorded literacy of the adult population, whereas the majority recorded those aged 15 and older, 10 and older or even six years and older.[1] Many statistics report just one number for the whole population which makes it impossible to calculate literacy of age groups or to obtain time series by birth cohorts. Secondly, literacy of individuals coming from different linguistic backgrounds is always difficult to measure. Even if census takers were instructed to record literacy in any language and not only in the official language of the destination country, migrants from different language families could still have declared themselves illiterate when they were asked by census takers. We compared literacy and age heaping from the census data of the different migrant groups in the United States directly. Migrants with a Romanic-language background, namely Italy and Portugal, displayed average numeracy values. However, they had significantly lower literacy rates than one would expect according to their average numeracy. Thirdly, although the vast majority arrived as young adults, a part of the migrants came as children and teenagers to the United States. Already for the mid-19th century, Cohn (2009) reports roughly one quarter arriving as children. When we look at the literacy of persons with migration background in the census some years later, we therefore have to be aware that many of them acquired literacy when they already lived in the United States. So the literacy performance is not only influenced by selective migration but also by age structure and schooling possibilities for migrants. To make things even more complicated, the U.S. was often the destination for migrants coming from countries with lower schooling (Eastern and Southern Europe), but also from countries with better schooling than the U.S., such as Sweden, Norway and so on. The children of those migrant families might have “lost” some of the schooling they would have obtained in their source countries if they had not migrated. Therefore, there exist various biases of different directions which are difficult to quantify. For these reasons, the study of U.S. migrant selectivity based on literacy is too difficult at the

present stage of knowledge. Fortunately, the age heaping techniques provides a feasible alternative to study this important issue.

Appendix D

Appendix Table D.1: Average number of underlying cases for each decade, destination and source country, by migration decade and destination country

Destination	1820	1830	1840	1850	1860	1870	1880	1890	1900
Argentina		109	265	428	721	655	623		
Canada	197	10664	8723	7228	5220	4352	381	421	
Norway	527	878	1490	2511	2120	2002	1798	1655	
UK	1451	1208	1611	515	411	376			
US	915	13006	24900	32064	35651	30703	989	941	655

Notes: For example, 109 was the average number of cases of all source countries that provided migrants to Argentina in the 1830s.

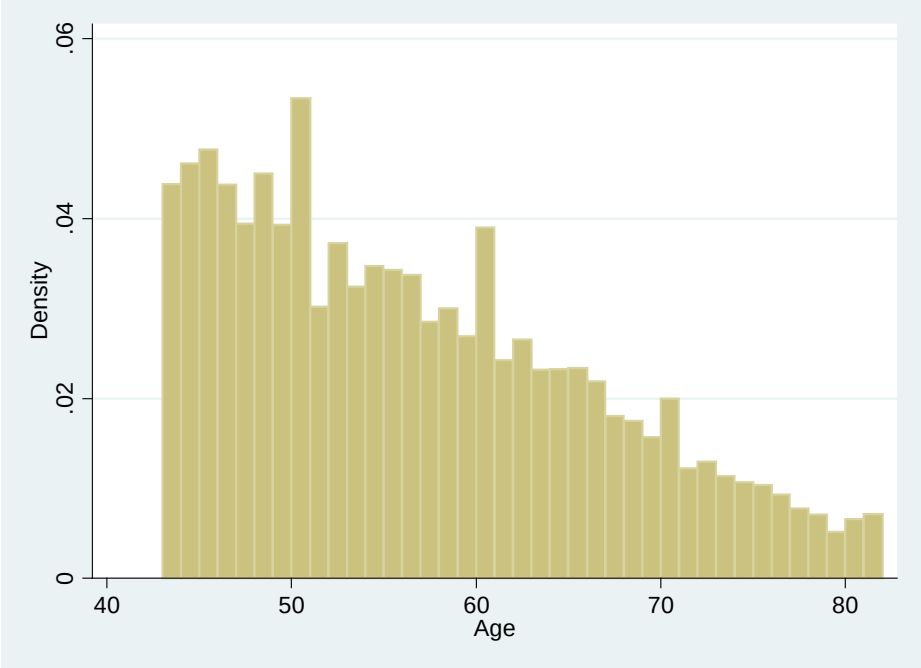
Sources: see data appendix.

Appendix Table D.2: Robustness of human capital selectivity regression: excluding some of the source and destination countries

	(1)	(2)	(3)	(4)	(5)
Countries excluded	Germany	Ireland	UK	Ireland & Italy	Old World
Source or Destination	Source	Source	Source	Source	Destination
Relative inequality dest - source	0.14**	0.13**	0.14**	0.11*	0.17***
	(0.019)	(0.041)	(0.023)	(0.084)	(0.005)
Friends & relatives (previous mig.)	-0.74*	-0.14	-0.57	-0.17	-0.09
	(0.059)	(0.666)	(0.136)	(0.595)	(0.803)
Poverty constraint (Gini/GDP sq.)	-0.14	-0.17	-0.19*	-0.11	-0.18
	(0.286)	(0.128)	(0.095)	(0.332)	(0.603)
Ln distance	-1.96**	-0.79	-1.67**	-0.36	5.45
	(0.023)	(0.339)	(0.037)	(0.662)	(0.103)
Ln distance * poverty constraint	0.03	0.03	0.04	0.01	0.04
	(0.444)	(0.368)	(0.237)	(0.748)	(0.712)
Common Language	4.99**	5.17***	3.13	6.20***	2.49
	(0.028)	(0.007)	(0.156)	(0.001)	(0.105)
Colonial relationship	1.26	-1.93	-0.05	-3.13	5.64**
	(0.603)	(0.414)	(0.983)	(0.172)	(0.018)
Destination	Yes	Yes	Yes	Yes	Yes
Source	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes
Constant	-0.14	-6.92	-0.90	-10.02*	-29.39**
	(0.984)	(0.252)	(0.890)	(0.094)	(0.018)
Observations	272	281	277	263	229
R-square between	0.861	0.879	0.864	0.885	0.917
R-square within	0.227	0.158	0.188	0.170	0.217
R-square overall	0.594	0.607	0.588	0.620	0.616

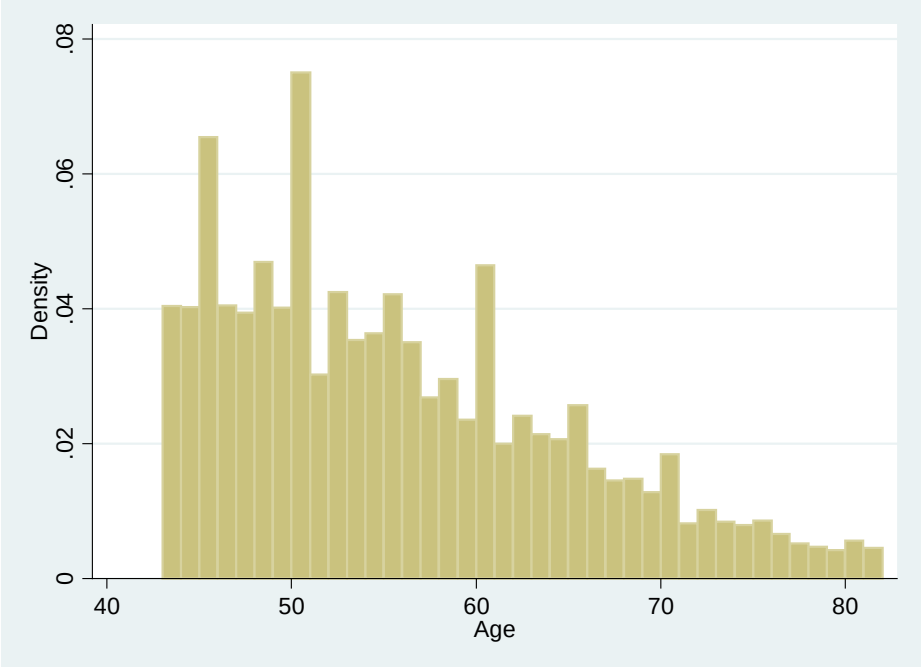
P-values based on robust standard errors are included in brackets. Migration decades 1820s-1900s are included. Russia excluded. Sources: see data appendix. All estimates are random effects.

Appendix Figure D.1 Panel A: Age distribution of 23 to 82 year old population in the UK, 1881 census



Sources: see data appendix.

Appendix Figure D.1 Panel B: Age distribution of 23 to 82 year old immigrant population from the UK, living in the US, 1880 census



Notes to Appendix Figure D.1 Panel A and B: we performed logit regressions of the migrant status on numeracy by decades. The migrant variable always rendered a negative, highly significant coefficient. Hence, migrants in this panel have a statistically significant lower chance of being numerate than the source country population of the UK.

Sources: see data appendix.