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Do Migrants Really Foster Trade?

The Trade-Migration Nexus, a Panel Approach 1960–2000

Christopher R. Parsons

The World Bank
Development Research Group
Trade and Integration Team
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Abstract

Despite the burgeoning empirical literature providing evidence of a strong and robust positive correlation between trade and migration, doubts persist as to unobserved factors which may be driving this relationship. This paper re-examines the trade-migration nexus using a panel spanning several decades, which comprises the majority of world trade and migration in every decade. First the findings common to the literature are reproduced. Country-pair fixed effects are then used to account for unobserved bilateral factors, the implementation of which removes *all* of the positive impact of migration on trade. In other words the unobserved factors, a leading candidate for which it is argued is international bilateral ties, are on average strongly and positively correlated with migrant networks. Dividing the world into the relatively affluent North and poorer South, the results show that migrants from either region *only* affect Northern exports to the South. This is intuitive since in general countries of the North export more differentiated products and information barriers between these regions are greatest. A country-level analysis further shows that migrants may both create and divert trade. Taken as a whole, the results demonstrate the large biases inherent in cross-sectional studies investigating the trade-migration nexus and highlight the extent to which previous results have been overstated.

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Christopher R. Parsons[1]

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[1]The Leverhulme Centre for Research on Globalisation and Economic Policy (GEP). The correspondence address is: Nottingham School of Economics, Clive Granger building, University Park, Nottingham, NG7 2RD, United Kingdom. Christopher Parsons may be contacted by e-mail at lexcrp1@nottingham.ac.uk, by phone on: +44 (0)1159 951 5620 or by fax: +44 (0)1159 951 4159.

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1. *Introduction*

Germany and Turkey have been in contact with one another, since at least the attempted expansion of the Ottoman Empire north of the Balkans, which culminated in the second siege of Vienna in 1683. Official diplomatic relations were marked however by the opening of the Berlin embassy in Constantinople in the 18th century, which led to increased labor mobility between the two cities and numerous subsequent trade agreements. In 2000, Germany remained the most important trading partner for Turkey, while Turkey represented the 17th most important trading partner for Germany - which at the time was the largest exporting nation in the world. Perhaps the most famous use of early migrant labor between the two nations was the joint imperial endeavor of constructing the Baghdad railway in the lead up to the Great War, "which was instrumental in forging a lasting Turco-German relationship" (McMurray 2001). Large numbers of Turkish workers began arriving in Germany in the 1960s however, to meet labor shortfalls exacerbated by the construction of the Berlin wall, which deprived West Germany of relatively cheap labor from the East. In 2000, the Turks in Germany represented the single largest diaspora in Europe and indeed the second largest South-North migration corridor in the world.[2] Similarly, the numbers of Germans in Turkey - most of which are ethnic Turks - is the second largest North-South corridor globally (Özden et al 2011).[3]

The Turkish-German case is a good example of an idiosyncratic international bilateral tie that is difficult to account for empirically. Such ties are underpinned by a complex combination of historical, political and cultural characteristics, which in turn are both the cause and the consequence of myriad past events. Although gravity models investigating the trade-migration nexus typically uncover a robust and positive relationship between these two forces of globalization, there clearly exists unobserved heterogeneity, which is not captured by standard gravity variables - as exemplified by the Turkish-German – which might be driving this relationship. Leading scholars also call into question the robustness of previous findings. Hanson (2007), for example, states "*it is difficult to draw causal inferences from these results, since immigration may be correlated with unobserved factors that affect trade, such as trading partners' cultural similarity or bilateral economic policies*" (pg. 43). Similarly Lucas (2006) is also sceptical since *"…reservations persist as to the potential for other, unobserved phenomena to be stimulating both trade and migration. ……Overall the estimated effects seem improbably large, though perhaps indicative of a very real underlying phenomenon*" (pg. 212).

This paper is the first to investigate the links between trade and migration in a panel spanning several decades, 1960-2000, which comprises the majority of world trade and migration in each period. The panel

[2] The largest South-North corridor is the Mexicans in the United States.
[3] The largest North-South corridor is the Americans in Mexico.

facilitates the implementation of time-varying country fixed effects to control for the common omission of multilateral resistance terms and, crucially, also for country-pair dummies to control for unobserved country-pair heterogeneity. Greater emphasis is placed upon bilateral trade flows to and from developing nations, while the time dimension of the panel is more comprehensive yielding better estimates of the longer term effects of migration on trade. Importantly, the effects of immigration *and* emigration on trade are assessed simultaneously, the absence of one of which tends to overestimate the importance of the other.

First the data are tested in repeated cross-sections and then the data are pooled, the results from which are consistent with the existing literature. The implementation of pair-wise fixed effects, used to account for 'bilateral ties', strips away the positive effect of migration on trade. Dividing the world into the relatively affluent North and poorer South, the results show that migrants from both regions only affect Northern exports to the South. This is intuitive since in general countries of the North export more differentiated products, while countries of the South more often export homogenous commodities. It is also between those regions that informational barriers are likely highest. Interacting the migrant variables further, at the country level, shows that migrants may both create and divert trade. These interactions also suggest that while the unobserved factors are generally positively correlated with the *direct* effects of migrants, the direction of the bias is less certain when the *indirect* impacts of migrants are considered. Taken as a whole, the results demonstrate the large biases inherent in cross-sectional studies investigating the trade-migration nexus and highlight the need to be cautious when interpreting previous findings. An international examination of the trade-migration nexus at the product level is absent from the existing literature. While this is beyond the scope of the current work, the results from this paper are strongly suggestive that this should be undertaken, without which it is difficult to draw ascertain the true mechanisms underpinning the trade-migration nexus.

2. *Literature Review*

Based on the premise that the greatest potential benefits to trade exist between countries which are the least similar (Winters 2003), then migrants (who by definition have experience of both locations), may be best placed to exploit those differences. Migrants are often bilingual, fluent in both their mother tongue and the language of their host nation. They may possess knowledge of the available products in both countries, about the local laws and regulations that govern the markets and the institutions that oversee their functioning. Migrants are ideally positioned to exploit opportunities for arbitrage and match buyers and sellers through their superior market knowledge, thereby lowering the transaction costs of trade. These arguments were first made by Gould (1994), whose seminal contribution paved the way for

numerous empirical papers which examine the trade-migration nexus. Gould distinguishes an *information* channel through which migrants reduce the transaction costs of trade, from a *preference* channel via which migrants foster trade flows through demanding domestically produced goods. Collectively, these two channels may be termed *direct immigrant links (direct links* henceforth), since they pertain to the effects of migrants whose country of birth relates to either the importing or the exporting nation. In other words migrants which directly affect trade flows either to or from their country of origin.

Rauch (2001), an advocate of the 'network' view of trade, stresses the role of business contacts and social networks that promote 'trusting' contractual arrangements and overcome informational asymmetries and informal trade barriers. These arguments are akin to Gould's transaction cost mechanism or information channel. Rauch and Trindade (2002) examine the extent to which concentrations of ethnic Chinese[4] – and not the absolute levels - foster trade. The key additional insight proffered by Rauch and Trindade therefore is that *third-party* migrants, the ethnicity of which pertains neither to the importing or exporting nation, may also promote bilateral trade flows. This is what Felbermayr, Jung and Toubal (2009) refer to as indirect effects.

The majority of papers in the trade-migration literature, implement gravity models and build upon Gould's insight to test these links in a variety of (predominantly OECD-centric) geographical settings, most commonly focusing upon a single country and her trading partners.[5] Head and Ries (1998) investigate immigrant-links in Canada, Dunlevy and Hutchinson (1999, 2001) examine historical data for the United States, Girma and Yu (2002) study the impact of migration on trade in the UK, Bryant et al (2004) for New Zealand, Blanes-Cristobel (2003) in Spain, White (2007b) for Denmark and Hatzigeorgiou (2010) in Sweden. At the *country* level, only three papers examine direct-links amongst groups of countries. Hatzigeorgiou (2009) examines a cross-section of 75 countries in 2000, while Felbermayr and Toubal (2008) implement a cross-section for the OECD in the same year. Felbermayr and Jung (2009) is the only paper to the knowledge of the author which implements a panel of countries (for 1990 and 2000).

Greater availability of disaggregated data has spurred ever more sophisticated empirical studies. Some focus upon the trade-migration nexus within a country, for example Combes et al (2005) for France. Yet another strand of the literature examines *states* or *provinces* trading with overseas country partners. Examples include Wagner et al (2002) for Canada, Co et al (2004), Bardhan and Guhathakurta (2005), Herander and Saavedra (2005), Dunlevy (2006) and Bandyopadhyay et al (2008) for the US and Peri and Requena (2010) for Spain.

[4] These concentrations are modelled as the cross-products of the share of ethnic Chinese in each trading partner.
[5] Migration data are notoriously weak and this focus can be explained by the paucity of the available migration data.

Pooled cross-section studies that center upon a single nation and her multiple trading partners generally uncover a significant and positive relationship between migrant stocks and bilateral trade flows. These findings seem robust to a number of different econometric specifications, time periods and alternative country settings; a combination of which accounts for the broad range of estimates obtained (Wagner et al 2002). And the ranges are indeed broad. Wagner et al (2002) in their survey find that the elasticities of exports and imports with respect to migration range from +0.02 to +0.16 and +0.01 to +0.31 respectively.[6] Given that the structure of the data in these studies militates against the inclusion of importer and exporter fixed effects however, an alternative explanation would be that these studies likely suffer from omitted variable biases.

This is the line of reasoning adopted by Felbermayr, Jung and Toubal (2009), who revisit Rauch and Trindade's evidence. These authors highlight Rauch and Trindade's omission of multilateral resistance terms[7] and argue this contributes to their large overestimate of the effect of Chinese migrants on trade, by a factor of between two and four. For this, Baldwin and Taglioni (2006) more broadly award the 'gold medal' mistake to which many papers in the wider gravity model literature fall foul.[8] This leads to biased estimates of trade costs and indeed of all other covariates, while further endogeneity arises due to measurement error in the economic mass variable. Felbermayr, Jung and Toubal (2009) however are restricted by the paucity of the available migration data such that they are constrained to repeated cross-section analysis. As such, they cannot control for unobserved pair-wise factors, which would provide one explanation for the unfeasibly large variance they obtain with their indirect network effect estimates.[9]

The more recent studies which implement state level data also uncover a complementarity between trade and migration but tend to be sounder empirically, estimating panel data and implementing importer and exporter fixed effects. Bandyopadhyay et al (2008) and Peri and Requena (2010) go still further, also implementing importer-exporter-pair effects to control for unobserved state-country pair-wise heterogeneity. These prove crucial in controlling for pair-wise heterogeneity in gravity models of international trade, as demonstrated by Cheng and Wall (2004). At the country level only Felbermayr and Jung (2009) control for country-pair unobserved heterogeneity (and multilateral resistances) using a panel for 1990 and 2000, which only covers North-South trade. These authors find a significant and positive

[6] The ranges presented here only include those studies that focus upon a single trading nation and her trading partners.
[7] For a lucid explanation of the impact of omitting these variables, readers are referred to Baldwin and Taglioni (2006).
[8] In addition to the 'gold medal' award, 'bronze' and 'silver' medals are doled out to papers that inappropriately deflate nominal values by US aggregate price index or else those that use the log of the average of trade flows as opposed to the average of their logs – when unidirectional trade flows are averaged.
[9] For example, their estimates of the trade creating effects of third-party migrant networks range from 8.1877*1018% for Japan to -100% for Saudi Arabia.

effect of migration on trade and their findings will serve as the benchmark for comparison for the results of this paper.

The implementation of fixed effects has successfully been used to solve a number of puzzling results in the gravity model literature. For example, Glick and Rose (2002) proffer a solution to the puzzle found by Rose (2000) whereby currency union membership was associated with an increase in trade of approximately 300%. Glick and Rose reduce this to around 100%, when pair-wise fixed effects are implemented. Similarly, Baier and Bergstrand (2007) control for countries selecting into trading and entering free trade agreements with one another. Once pair-wise fixed effects are included in their estimation these authors convincingly explain the large variation – including negative results - apparent in previous studies examining the effect of RTAs on bilateral trade flows. The current paper implements trading-pair fixed effects to control for international bilateral ties (amongst other unobserved pair-wise factors) to examine the trade-migration nexus.

The present paper is the first to implement a panel spanning several decades to investigate direct-links (*a la* Gould) and third party effects (*a la* Rauch), both separately and simultaneously; while also crucially controlling for unobserved pair-wise factors such as bilateral ties, which may influence both trade and migration. The focus also moves away from a single country (and her trading partners) and towards groups of countries. More emphasis is placed upon the relationship between trade and migration in the context of developing countries while the longer time dimension of the panel is also superior since most papers investigate immigrant-links in the years after 1980.[10] Significantly, the comprehensive migration data allow the effects of immigration *and* emigration on trade to be assessed simultaneously, the absence of one of which tends to overestimate the importance of the other.

The paper is structured as follows. The following section discusses the underlying mechanisms which are purported to drive the links between trade and migration and restates the basic framework so as to emphasize the role of the transaction costs – as opposed to preferences – which are economically more important. Section 4 outlines the specification of the empirical model, while Section 5 discusses the underlying data sources. Section 6 presents a repeated cross-section analysis and Section 7, a discussion of the issue of endogeneity. Observations are then pooled and fixed effects are added to show the nature and direction of the biases from cross-section estimates in Section 8. The analysis is then disaggregated at both the regional (Section 9) and the country level (Section 10) to highlight how dramatically the estimated effects of migration on trade change when fixed effects, used to control for unobserved bilateral ties, are considered. Lastly, Section 11 investigates the impact of third-party migrants on bilateral trade.

[10] Notable exceptions include Gould (1994) and the papers by Dunlevy and Hutchinson (1999 & 2001).

3. *Direct Links*

In this paper *direct-links* is the term used to capture those migrant-links which are formed between two trading nations, *i* and *j*, by migrants, whose country of birth is either *i* or *j*. In this section a brief discussion is provided as to how the main methods employed to capture these *direct-links* have evolved in the literature. In passing, a simple yet informative reinterpretation of the basic equation will be highlighted, which serves to emphasize the identification of the transaction cost or information channel (as opposed to a preference channel). This is an important distinction, since the information channel is more relevant economically since it is welfare enhancing. Lastly, a comparison of Gould's and Rauch's key migrant variables of interest, in the context of fixed effect models, leads to the conclusion that an additional (and unnecessary) restriction is imposed upon the parameters of the theoretically more intuitive approach.

Following Gould, most papers that investigate *direct-links*, typically regress the logarithm of a country's imports and/or exports upon the immigrant stock (and controls) of the host country (equations 1 and 2).[11]

1. $$lnX_{ij} = b_0 + \gamma_1 lnMIG_{ji} + \theta^{'} Z_{ij} + \varepsilon_{ij}$$

2. $$lnM_{ji} = b_0 + \beta_1 lnMIG_{ji} + \theta^{'} Z_{ij} + \varepsilon_{ij}$$

Where $\ln X_{ij}$ = the natural log exports from country *i* to country *j*, $\ln M_{ji}$ = the natural log of imports from country *j* to country *i*, $\ln MIG_{ji}$ = the natural log of the stock of immigrants from country *j* in country *i* and θ is a vector of coefficients for all remaining controls. In other words, bilateral trade is regressed upon unidirectional migration. In this framework it is assumed that immigrant (MIG_{ji}) preferences only affect destination country *i*'s imports (M_{ji}); while immigrants (MIG_{ji}) that lower the transaction costs of trade will affect both the import (M_{ji}) flows to and the export (X_{ij}) flows from country *i*. Therefore if $\beta_1 > 0$ and $\gamma_1 = 0$ then the preference channel is said to dominate but if $\beta_1 > 0$ and $\gamma_1 > 0$ then both mechanisms are prevalent. In this framework, MIG_{ji} is measured in absolute levels although *prima facie* it is not immediately obvious why this is appropriate.

This approach is flawed in at least two key ways. Foremost among these is the fact that immigrants/emigrants may establish importing *and* exporting businesses. Therefore, if $\beta_1 > 0$ and $\gamma_1 = 0$, this might be due to the fact that immigrants (MIG_{ji}) are importing goods to sell on or re-export as opposed to through a preference channel for consumption. So too might this be because emigrants (from the importing country *i*) abroad (MIG_{ij}) - which are not captured in this specification - establish exporting

[11] Throughout the paper, the first subscript always refers to the origin of persons or goods, while the second refers to the destination.

businesses in country *j*, that ship goods to country *i*, i.e. through the transaction cost mechanism. Notwithstanding these arguments, if immigration (MIG_{ji}) is found to influence both imports ($\beta_1>0$) and exports ($\gamma_1>0$), preferences might not be relevant at all, and the entire effect might be due to a reduction in transaction costs.

Hatzigeorgiou (2010) provides a useful reinterpretation of this basic approach, regressing instead unidirectional trade upon bilateral migration (equation 3).

3. $$lnM_{ji} = b_0 + \beta_1 lnMIG_{ji} + \gamma_1 lnMIG_{ij} + \theta^{'} Z_{ij} + \varepsilon_{ij}$$

Using the same intuition as above, Hatzigeorgiou argues that if $\gamma_1>0$, i.e. if emigrants from country *i* living in country *j*, foster trade flows from country *j* to country *i*, then this *must* be through the information channel since the preference channel cannot operate against the direction of trade. If $\beta_1>0$ however, this is hypothesized to capture both preference and transaction cost effects. This formulation places additional emphasis upon uncovering the relative importance of the economically more important and welfare enhancing transaction cost mechanism therefore.

Importantly, [3] also includes measures of both immigrant (Mig_{ji}) and emigrant stocks (Mig_{ij}). It is imperative to include both since - as enshrined in Ravenstein's (1885) fourth law of migration[12] - bilateral migrant flows beget further flows in the opposite direction, such that they will likely be positively correlated. Failing to include variables capturing both sides of the migration coin therefore, which is common – will likely bias results upwards.

Rauch and Trindade (2002), while abstracting from preference effects altogether, construct two variables to capture the effects of Chinese ethnic networks. The first is simply the log of the product of the population of Chinese in each trading partner: $\ln(POP_{ki}*POP_{kj})$ where $i\neq j$ and k=China.[13] Here we restrict the discussion to *direct links*, in which case either country *i* or country *j* need be China. This assumption is relaxed later when discussing *third-party* effects. This variable is assumed to capture "the total number of potential international connections between the ethnic [Chinese] populations of the trading partners" (pg. 119). The authors' second migration variable is constructed as the log of the cross-product of the shares of Chinese in both transacting countries, where the denominators for each share are the total resident populations, i.e. $ln\left(\frac{POP_{ki}}{respop_i} * \frac{POP_{kj}}{respop_j}\right)$, where $i\neq j$ and k=China. Here again, since discussion is limited to *direct links*, either country *i* or country *j* need be China. In this case, *POP* would refer to the

[12] This states that "each main current of migration produces a compensating counter-current".
[13] This allows for the possibility for $k=i$ and $k=j$, in which case we refer to direct links. If $k\neq i$ and $k\neq j$, the term *third-party effect* is instead used.

domestic population of China. Otherwise, *POP* would refer to the Chinese migrant population in either country *i* or *j*. This second variable is equivalent to the probability that any two migrants picked at random from countries *i* and *j* will be ethnically Chinese. This is used to capture a contractual reinforcement effect of migrant networks.

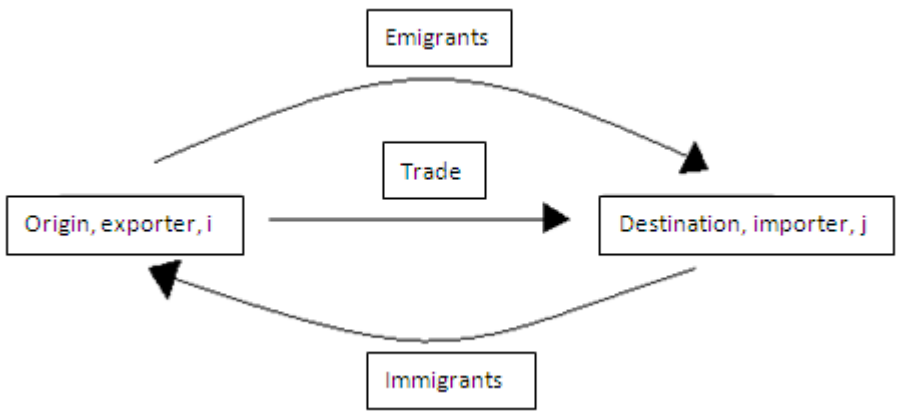
In a log-linear framework however, due to the additive property of logs, both these latter variables are equivalent to each other should importer and exporter fixed effects be implemented; since the 'respop' terms dropout due to the implementation of importer and exporter fixed effects. Moreover, $\ln POP_{ki} = \ln MIG_{ki}$ and $\ln POP_{kj} = \ln MIG_{kj}$, if $k \neq i,\ k \neq j$. In other words, with the inclusion of importer and exporting country fixed effects the migration variable formulations of Rauch and Trindade are equivalent to those used throughout the remainder of the literature; but with one important difference.

In [3], the two migration variables have separate coefficients β_1 and γ_1. However, with the inclusion of country *i* and country *j* fixed effects, the two migrant variables of Rauch and Trindade both reduce to $\xi_1 \ln CHIN_i + \xi_1 \ln CHIN_j$. In other words, an additional restriction is placed upon the coefficients of these variables. In the absence of importer and exporter fixed effects, Rauch and Trindade's migration variables are more intuitively appealing theoretically; not least the probabilistic variable constructed using the shares of ethnic Chinese in each trading partner. With the implementation of fixed effects however, these theoretically more appealing variables actually impose an additional restriction on the regression coefficients, making them less attractive.

The foregoing discussion highlights three factors which are deemed necessary to take account of in any study of trade and migration. Firstly, for the sake of identification, it is superior to regress unidirectional trade upon bilateral migration, in order to better isolate the impact of the economically more important information channel. Since $X_{ij}=M_{ij}$, it is only necessary to include either imports or exports. Secondly, this formulation accounts for both sides of the migration coin, which is necessary since otherwise the coefficients on the remaining migration variable will likely be biased upwards. Lastly, with the inclusion of importer and exporter fixed effects, immigration and emigration are better modeled in levels as opposed to any notion of shares, which might initially seem more appealing theoretically but which actually place an additional restriction upon the estimate coefficients. While these factors may be viewed as tenets which should be adhered to, the foregoing discussion also highlights the difficulty in meaningfully distinguishing the transaction cost and preference mechanisms. Since great weight is placed upon these hypotheses in the literature, they will be referred to in passing, although this paper focuses upon the extent to which migrants foster bilateral trade flows *whatsoever*.

Due to the varying notation used in the literature, it proves prudent to specify the notation used throughout this paper before proceeding further (see figure 1). Crucially, what follows takes aggregate exports as the left-hand side variable, as opposed to imports in (1)-(3). This is simply the result of exports being specified in the underlying data, but all the foregoing arguments hold due to the symmetry in the trade and migration data. In terms of trade, country *i* is always the exporting country, while country *j* is always the importer. On the migration side, country *i* sends emigrants to country *j*, while country *j* sends immigrants to country *i*. In other words, emigrants travel from *i* to *j* in the same direction as trade (exports from *i* to *j*). Conversely, immigrants travel from *j* to *i*, against the flow of goods.

Figure 1



4. Specification

The gravity model in its simplest incarnation - having accounted for economic mass - predicts that trade between two countries is a decreasing function of the barriers to trade between them; relative to the average barrier of both regions to trade with the rest of the world. Anderson and Van Wincoop (2003) derive a gravity framework assuming perfect competition and product differentiation at the country level. Their derivation allows a theoretically appropriate measure of both trading partner's average barrier with the rest of the world, termed multilateral trade resistance variables. Goods are differentiated by place of origin and each region completely specializes in producing a single good, the supply of which is fixed. Identical and homothetic preferences are approximated by a CES utility function and prices differ between countries due to trade costs - bilateral barriers to trade – which are unobservable, such that:

4. $$p_{ij} = p_i t_{ij}$$

Where: p_{ij} = the price of goods from region *i* sold in region *j*, p_i = the exporters supply price and t_{ij} = the unobservable barrier to trade. The price index terms, or equivalently, the terms for multilateral trade resistance, are a function of trade restrictions with *all* trading partners and are given by:

5. $$P_j = \left[\sum_i \left(\beta_i p_i t_{ij}\right)^{(1-\sigma)}\right]^{1/(1-\sigma)}$$

Where additionally: β_i = is a positive distribution factor, a price scale factor and σ = the elasticity of substation between the goods. The authors succeed in deriving an intuitive version of the gravity model, based on the crucial assumption that trade barriers are symmetric, i.e. that $t_{ij}=t_{ji}$:

6. $$x_{ij} = \frac{y_i y_j}{y^W}\left(\frac{t_{ij}}{P_i P_j}\right)^{1-\sigma}$$

Where additionally: x_{ij} = the nominal value of exports from country *i* to country *j*, y_i = the GDP of country *i*, y_j = the GDP of country j and y^W = world income. The key insight of the model is that trade between countries *i* and *j* depends not only upon nations' size and the bilateral barriers between them, but also upon the multilateral resistance of countries *i* and *j* with the rest of the world. If either country's multilateral resistance increases with the rest of the world, then they will have the incentive to trade relatively more with one another. Cross-sectional models incorporating importer and exporter fixed effects will suitably account for these additional price terms (see Rose and Van Wincoop (2001) and Feenstra (2004)). In panel analyses however, country-time-varying fixed effects are required (see Baldwin and Taglioni (2006) or Baier and Bergstrand (2007)).

4.1 Trade Costs

Trade costs have large welfare implications and migrants matter for trade because they can potentially lower trade costs through by reducing informational asymmetries (Anderson and van Wincoop 2004).[14] In the preceding gravity set-up, migrants enter the non-observable trade cost function, t_{ij}. In order to meaningfully isolate migrant's impact however, it is crucial to account for each additional component that has been found important in explaining trade costs to avoid omitted variable bias**.**

Anderson and van Wincoop (2004) identify several broad categories of trade costs. Transport costs include direct, freight and insurance charges, as well as the indirect costs which include storage, inventory and preparation costs. Next there are country specific wholesale and retail distribution costs. Policy barriers include domestic tariff and non-tariff barriers, as well as international commitments, for example

[14] Migrants' preferences for domestically produced goods also bolster bilateral trade flows (Combes et al 2005), but importantly these links do not beget (efficiency and therefore) welfare gains.

membership of the WTO or regional trade agreements. While linguistic, currency and security barriers are all self-explanatory; information costs comprise search, legal and regulatory costs.

4.2 Empirical Specification

The success of identifying the extent to which migrants affect bilateral trade flows depends upon: successfully modeling the various trade costs outlined in the previous section and ensuring that the correct empirical model is used; one that controls for multilateral resistance terms and crucially also for unobserved pair-wise heterogeneity, in order to account for bilateral ties between trading nations.

As is common in the literature, direct transport costs are modeled using a measure of geodesic distance and a dummy variable which equals one if a country-pair shares a common border. Further dummy variables are included which take the value one if country-pairs share joint membership of an RTA, speak the same official language or share the same currency, legal system or a colonial history. Colonial ties will account, in part, for the extent to which countries share similar institutions. This will also likely capture some historical aspect of migrants' network effect. Lastly, information cost barriers are modeled using variables which capture *direct links* (and later *third-party effects*), which are hypothesized to bridge informational asymmetries.

The non-observable trade cost variable, t_{ij} modeled as a linear combination (which is standard in the literature), is given by equation 7:

7. $t_{ij} = {mig_{ij}}^{\beta 1} mig_{ji}^{\beta 2} cont_{ij}^{\beta 3} {lang_{ij}}^{\beta 4} {col_{ij}}^{\beta 5} rta_{ij}^{\beta 6} leg_{ij}^{\beta 7} curr_{ij}^{\beta 8}$

Substituting [7] into [6], taking logs and adding importer and exporter fixed effects, yields:

8. $lnX_{ij} = \beta_0 + \beta_1 ln(mig_{ij}) + \beta_2 ln(mig_{ji}) + \beta_3 cont_{ij} + \beta_4 lang_{ij} + \beta_5 col_{ij} + \beta_6 rta_{ij} + \beta_7 leg_{ij} + \beta_8 curr_{ij} + \varphi_i + \gamma_j + \varepsilon_{ij}$

X_{ij} is a measure of aggregate exports. φ is the vector of exporter fixed effects, γ the corresponding vector of importer fixed effects and ε_{ij} is the error term, which is assumed to be log normally distributed. In cross-sections, these importer and exporter fixed effects capture the multilateral price index terms in addition to measures of national income. Although this strategy militates against obtaining separate coefficient estimates for the economic mass variables, any measurement error associated with them should drop out. Fixed effects prove useful since they additionally capture country-level unobserved

heterogeneity including indirect transport costs, wholesale and retail distribution costs, belonging to the WTO, the quality of institutions, domestic regulations, for example a nation's customs procedures or the ease of obtaining the required documentation to trade, infrastructure and geography, levels of corruption and domestic security protocol. Additionally, other channels through which migrants could potentially influence trade will also be controlled for, for example through accounting for any rise in the stock of human capital.

Equation 8 is estimated using the least squares dummy estimator (LSDV henceforth). Although algebraically analogous to the standard panel 'within' estimator, the LSDV estimator yields an R^2 which may serve as the basis for comparison, for the cross-section and pooled results in this paper and the rest of the estimates in trade gravity literature more broadly. In the presence of many zero trade flows however; Santos Silva and Tenreryo (2006) demonstrate how heteroskedastic residuals may lead to inconsistent results, in which case they argue the Pseudo-Poisson Maximum Likelihood estimator is appropriate. The LSDV estimator is nevertheless chosen for analysis since there are no zero observations whatsoever in the underlying trade data. There exist missing values however. The key question therefore is whether these represent true zero values, in which case they need to be handled with care; or whether they are actual missing values. Since aggregate trade data are used, there is every reason to believe that the majority of these 'missing values' are indeed missing. This is especially so in the later period to which the trade data refer, since small values are not reported due to the financial constraints faced by the authors (see below). Indeed, the conclusion of private correspondence with the authors of the dataset was that these values should be treated as missing, since it was argued, it was a far bigger assumption to assume these missing values are all zero. For the sake of robustness, several samples, which have varying degrees of missing values, are estimated to ensure that their presence do not lead to spurious results.

5. *Data*

The paper draws upon data from three main sources. Migration data are obtained from Özden et al (2011), which details five origin-destination matrices that comprise every nation state, major territory and dependency from across the globe (226*226). The dataset is based upon the foreign-born concept of migration and each matrix corresponds to one of the last five completed census rounds, 1960-2000.[15] The data correspond to economic migrants and every effort has been made to remove refugees. Although the proportion of illegal migration captured in the dataset is unknown, it is still likely that a fairly large proportion of illegal migrants are captured in national censuses. This dataset is superior to those previously estimated in terms of its broad global coverage and the number of decades to which it refers. It

[15] In this paper, the version of the data chosen from Ozden et al (2011) is that which equates the migration data to a specific year, for example, 1970 or 1980 as opposed to the version which pertains to census rounds or decades, for example 1994-2005.

is therefore the most appropriate dataset to best capture the second great wave of international migration (of the modern era).

Trade data are taken from Feenstra et al (2005), which provides data calculated from the UN *Comtrade* database[16] for the period 1962-2000. The strength of these data lie in imports being preferred over exports - since they are frequently considered to be more accurate - the extent to which the authors clean the dataset by comparing the import and export data of each bilateral trade flow and the clear documentation the authors provide as to the adjustments made, which facilitates an accurate matching of the migration to the trade data. Since the earliest year to which the trade data refer is 1962, the migration data for 1960 is assumed to be comparable for this decade, under the assumption that these trade flows would have been similar to those two years hence. In cases where countries need to be aggregated to equate them to a trading entity, migrations between these countries are removed from the dataset. Once matched, the dataset comprises 178 countries in total.

The trade data are divided into two distinct periods, 1962-1983, for which data are complete (i.e. 178*178 countries) and 1984-2000. For this latter period bilateral trade flows values at less than $100,000 are omitted i.e. missing. Moreover, complete data are only available for 72 countries, which are reduced to 68 once the necessary aggregations have been made.[17] For this more recent period, the trade data are available for 68*178 countries therefore. The sample selected for estimation comprises those trade flows for those 68 countries for which data are available in each period (i.e. 68*68), termed sample 1. Additional samples are also estimated to check for robustness. Sample 2, relates to the largest possible sample (68*178), sample 3 comprises origins with fewer than 30 missing observations and sample 4 comprises those nations with fewer than 20 missing observations. The percentage of world migration and trade captured in each sample is detailed below (see table 1).[18] The list of countries in each sample can be found in the appendix.

Table 1. The percentage of world trade and migration captured in each sample in every decade

Decade	Samp. 1 %WT	Samp. 1 %WM	Samp. 2 %WT	Samp. 2 %WM	Samp. 3 %WT	Samp. 3 %WM	Samp. 4 %WT	Samp. 4 %WM
1960	88	80	94	88	76	69	69	50
1970	90	76	95	83	79	68	72	51

[16] See: http://comtrade.un.org
[17] For example Belgium, Belgium-Luxembourg and Luxembourg to a single entity over time, named Belgium-Luxembourg.
[18] Note that the denominator used when calculating each migration figure in table 1, refer to the total 178 countries in the sample, in other words those countries/regions for which internal migration has been removed.

1980	90	70	95	75	71	61	65	52
1990	94	69	97	73	80	58	74	51
2000	94	55	97	64	76	48	69	44

Source: Author's Calculations. %WT and % WM refer to the percentage of world trade and migration in each sample respectively.

The remaining covariates, geodesic distance, contiguity, common language, shared colonial relationship, belonging to a regional trade agreement, common legislation, common currency and GSP are all taken from Head, Mayer and Ries (2010).[19]

6. Cross-section Results

Table 2 Direct Link Cross-Section Regressions, 1960-2000

Dependent Var.: Log Exports			**Migrant Links**		
	2000	**1990**	**1980**	**1970**	**1960**
Log immigrants	0.051***	0.044***	0.046**	0.071***	0.054***
	(0.012)	(0.013)	(0.019)	(0.018)	(0.018)
Log emigrants	0.074***	0.078***	0.054***	0.060***	0.068***
	(0.012)	(0.013)	(0.018)	(0.018)	(0.019)
Log distance	-1.017***	-0.939***	-1.119***	-0.886***	-0.755***
	(0.055)	(0.047)	(0.075)	(0.070)	(0.077)
Contiguity	0.163	-0.066	-0.345	-0.257	0.103
	(0.139)	(0.14)	(0.215)	(0.221)	(0.22)
Common Language	0.188*	-0.051	0.001	0.090	0.062
	(0.100)	(0.096)	(0.16)	(0.154)	(0.161)
Colony	0.302**	0.481***	0.895***	1.036***	0.810***
	(0.146)	(0.147)	(0.207)	(0.199)	(0.226)
RTA	0.277***	0.086	-1.007***	-0.276	-0.741***
	(0.092)	(0.095)	(0.162)	(0.198)	(0.179)
Common Legislation	0.313***	0.439***	0.134	0.159	0.213**

[19] See: http://www.cepii.fr/anglaisgraph/bdd/gravity.htm

	(0.061)	(0.063)	(0.103)	(0.098)	(0.108)
Common Currency	-0.594***	0.316	1.053*	1.350*	1.070***
	(0.109)	(0.445)	(0.588)	(0.72)	(0.337)
Importer (i)/Exporter (j) dummies	YES	YES	YES	YES	YES
Observations	3,795	3,456	3,382	3,215	2,870
R^2	0.809	0.788	0.664	0.683	0.699

The dependent variable is the log of bilateral exports. All regressions include importer and exporter fixed effects. Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively. Cluster robust standard errors are provided in parentheses.

The results of cross-section regressions, based on equation 8, are presented for the years 1960-2000 for sample 1 (see table 2). Across all years, the regressions explain at least approximately 70% of the total variation in bilateral exports, which is typical. The coefficient on the distance variable is around 1, which is what theory predicts. Sharing a common border or a common language have little effect upon trade, a result explained by the positive correlations of these variables with both migration variables, the inclusion of other covariates which account for the variation in these variables and the implementation of importer and exporter fixed effects. There is a very strong impact of sharing a colonial heritage, although this effect decreases over time, as historical network effects deteriorate and institutions diverge from one another.[20] Sharing common legislative origins are also found to significantly bolster trade in three of the five periods. The coefficients on the common currency and the RTA variables are very unstable however, being both significantly positive and negative across the years. This is due to endogeneity bias as argued by Glick and Rose (2002) and Baier and Bergstrand (2007) respectively.

Turning to the key variables of interest, the immigrant and emigrant variables are significant in every decade and the coefficients are remarkably stable over time. In 2000, a 10% increase in immigrants and emigrants is associated with bilateral trade increasing by 0.5% or 0.7% respectively. In other words, an increase in the global stock of 8,890,000 immigrants/emigrants is associated with an increase in world trade of $29bn and $42bn respectively, or $3,280 or $4,760 per immigrant/emigrant. In terms of the hypotheses which have featured so strongly in the literature, the coefficient on immigrants might be interpreted as a measure of one side of the transaction cost channel - since bilateral migration is regressed upon unidirectional trade. Nevertheless since the coefficient on the immigrant stock variable is not statistically larger than the coefficient on the stock of emigrants in any decade no firm conclusions can be drawn with regards separating the two mechanisms.

[20] The erosion of colonial links is well documented in Head, Mayer and Ries (2010)

7. Endogeneity

The importer and exporter fixed effects used in the regressions in table 1 will control for endogeneity bias in relation to the commonly omitted multilateral resistance terms, as well as any influence of migration among other countries on the trade between *i* and *j*. In a panel however, additional unobserved pair-wise or country-pair-time-varying influences may exist which are correlated with the error term ε_{ij}, and which subsequently give rise to selection or an omitted variable bias. Of the three types of endogeneity that might exist between trade and migration this is the principle cause for concern.

All efforts have been made to reduce measurement error since the two continuous variables, trade and migration, are from official sources and the dummy variables are also taken from an authoritative dataset. With regards simultaneity bias, sufficient evidence exists from previous studies that causality runs from migration to trade. Hatzigeorgiou (2010) argues that trade is not a key determinate of migration and further highlights Gould's test of causality which suggests that immigration precedes trade. Furthermore, Felbermayr and Jung (2009) find that causality runs from migration to trade following a regression based F-test of strict exogeneity. Peri and Requena (2010) using 2SLS, implement historical immigrant enclaves as instruments for contemporaneous migration and 'provide robust and consistent evidence that a causal effect from immigrants to export flows for Spanish provinces…' (pg 11). Lastly, Gould argues that immigration flows are subject to binding quotas such that migration stocks are more likely to be exogenous than their comparable bilateral trade flows.

Omitted variable or selection bias remains worrisome however. Papers investigating the determinants of migration by-and-large implement specifications with similar variables to those in the trade gravity model literature. For example, economically larger countries, those in closer proximity or country-pairs sharing a colonial heritage all tend to trade and exchange more migrants[21] with one another (Ortega and Peri 2009). In other words, the observed characteristics which drive both trade and migration are similar. Indeed, the R^2 in the handful of papers which investigate the determinants of international migration at the macro level, are typically quite low, for example around 25% in Mayda (2007) which suggests that there exists significant unobserved heterogeneity. In contrast, gravity models of international trade typically have an R^2 of between 60-80% (Baier and Bergstrand 2007). The question therefore is whether the unobserved components in determining trade flows are correlated with the determinants of international migration. As intimated by the Turkish-German example at the start of this paper, the answer is likely to be in the affirmative.

[21]A variable capturing colonial links proves to be a key predictor of migrant stocks but not of migrant flows when variables for migrant networks are included in estimation.

A complex combination of historical, political and cultural characteristics underpins international bilateral relations. These characteristics are too complex to lump together under a single heading, or capture using variables common to the gravity model literature. No doubt they have the potential to select country-pairs into trading with, or permitting migration between, one another however, as in Turkish-German case. These characteristics likely also constitute the fundamentals underpinning international bilateral ties. The direction of bias given the omission of these characteristics (on trade and migration) is indeterminate however. This is perhaps best exemplified by the fact that bilateral ties need not be congenial for trade and migration to exist. Countries with 'good' bilateral ties might experience trade and migration (Brazil and Japan), trade and no significant migration (United Kingdom and Mexico), migration and no significant trade (Sweden and Serbia and Montenegro) or negligible trade and migration (France and Bhutan). However, so too can 'bad' bilateral ties give rise to similar outcomes. Poor relations still underpin trade and migration between Iran and the United States.[22] Strained ties between Germany and Myanmar, while not resulting in significant migration, still give rise to significant trade. The Cuban diaspora in the US is the 11th largest South-North corridor in the world (Özden et al 2011)[23] and negligible trade and migration occur between Israel and Malaysia.

These deep-rooted (and yet unobservable) historical, cultural or political country-pair characteristics, may thus be positively or negatively correlated with both trade and migration, which in turn might lead to either *over-* or *under-estimates* of the effect of migration on trade. However, (as similarly argued by Baier and Bergstrand (2007)) if these characteristics are fundamental in nature and have endured over time, then they will likely affect the levels of trade and migration (relative to their potential), as opposed to recent changes in trade and migration. If true, then these deterministic characteristics will be predominantly cross-sectional in nature and can be largely accounted for with country-pair fixed effects; the implementation of which will also control for the endogeneity of the RTA and Currency Union dummies.

8. Panel Results

In a panel framework, equation 8 can be rewritten as:[24]

9. $$lnX_{ijt} = \beta_0 + \beta_1 \ln(mig_{ijt}) + \beta_2 \ln(mig_{jit}) + \beta_3 cont_{ijt} + \beta_4 lang_{ijt} + \beta_5 col_{ijt} + \beta_6 rta_{ijt} + \beta_7 leg_{ijt} + \beta_8 curr_{ijt} + \varphi_{it} + \gamma_{jt} + \tau_{ij} + \varepsilon_{ijt}$$

[22] In 2000, the Iranian diaspora in the United States was the 22nd largest, while the United States was the 21st most important export market for Iran.
[23] In 2000, the 577 Americans recorded as residing in Cuba represented the fifth largest diaspora in the Caribbean Island.
[24] Here the left-hand side is not divided through by the product of the national income variables *a la* Baier and Bergstrand (2007), since no restriction of unitary income elasticises is imposed.

In addition to the fixed effects to control for multilateral resistances which now have a *t* subscript, the term, τ_{ij}, is a vector of bilateral *i* and *j* fixed effects. Four regressions are presented (see table 3). The first includes importer, exporter and year dummies and can be thought of as the simple model. The next implements importer/exporter-time-varying dummies, which as well as appropriately accounting for deflating current prices through the time dummies also control for multilateral resistances (Baldwin and Taglioni 2006). The last two specifications include both importer/exporter-time-varying and pair-wise dummies but differ in how the pair-wise dummies are constructed. The results in the third column, implement fixed effects (termed *pair*) on country-pairs, regardless of the direction of trade. The dummy for the Franco-Belgian bilateral tie for example therefore equals one whether France exports to Belgium or Belgium exports to France. In the last column, separate pair-wise dummies are used for each direction of bilateral trade flows (termed *pairid*). *Pair* fixed effects are justified if trade costs really are symmetric. *Pairid* fixed effects are analogous to the standard 'within' panel estimator. All robust standard errors are clustered by country-pair.

The simple model yields results familiar from the literature and comparable to the cross-section results in table 1. The log of immigrants and emigrants are both highly significant and with the coefficient on emigrants larger than that of immigrants, which is expected due to the addition of preferences operating in the same direction as trade. The results in the second column, which additionally control for multilateral resistances, are similar to those in the first column, although the standard errors are marginally broader. In other words, - with the exception of the coefficient on the currency union variables - there seems to be little bias resulting from failing to account properly for multilateral resistance. That is not to detract from the results of Felbermayr, Jung and Toubal (2009) however, whom provide convincing evidence that a failure to include importer and exporter fixed effects in cross-section analyses leads to significant biases.

Table 3 Direct Link Pooled Regressions, 1960-2000

Dependent Var.: Log Exports	**Migrant Links**			
	1	**2**	**3**	**4**
Log immigrants	0.061***	0.055***	-0.023*	-0.023*
	(0.009)	(0.010)	(0.012)	(0.013)
Log emigrants	0.073***	0.070***	-0.011	-0.009
	(0.009)	(0.010)	(0.012)	(0.015)
Log distance	-0.929***	-0.938***	.	.
	(0.043)	(0.045)	.	.

Contiguity	-0.063	-0.107	.	.
	(0.130)	(0.137)	.	.
Common Language	0.057	0.0467	.	.
	(0.094)	(0.097)	.	.
Colony	0.690***	0.748***	.	.
	(0.150)	(0.153)	.	.
RTA	-0.166***	-0.015	0.486***	0.490***
	(0.065)	(0.073)	(0.069)	(0.074
Common Legislation	0.249***	0.262***	.	.
	(0.057)	(0.058)	.	.
Common Currency	0.123	0.319*	0.637***	0.640***
	(0.180)	(0.188)	(0.150)	(0.157)
Importer /Exporter dummies (i/j)	YES			
Year dummies (t)	YES			
Importer/Exporter-time-varying dummies (it/jt)		YES	YES	YES
Symmetric pair dummies (ij)			YES	
Asymmetric pair dummies (ij)				YES
Observations	16,718	16,718	16,718	16,718
R^2	0.734	0.780	0.866	0.901

The dependent variable is the log of bilateral exports. All regressions include importer and exporter fixed effects. Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively. Robust standard errors are provided in parentheses.

When the *pair* or symmetric fixed effects are additionally included, the results change dramatically. The RTA and common currency dummies are now highly positive and similar to those obtained by Glick and Rose (2002) who found a coefficient of 0.74 and Baier and Bergstrand (2007) who estimated the impact of RTAs on trade to be 0.68. Since Rose (2002) uses symmetric fixed effects, these results would vindicate his approach.

Most importantly for the purposes of this paper however, are the results on both migration variables. No effect of emigrants is found *whatsoever* and the coefficient on the immigrant variable is actually negative,

suggesting that a 10% rise in immigrants is associated with a 0.2% fall in trade. Theory suggests that the unobserved bilateral factors, which are captured here using country-pair fixed effects, may be both positively and negatively correlated with migrant networks. The empirical results clearly demonstrate however, that on average these unobserved bilateral factors are strongly positively correlated with the migration variables, such that their imposition removes the positive impact of migration on trade. Previous estimates which fail to control for these factors should be treated with caution therefore. One possible explanation is offered by Diaz-Alejandro (1970), who argues that migrants might start producing in the destination country those goods that they previously demanded from abroad. More simply, this might be a pure demand effect such that immigrants continue consuming destination country products once they have left the origin country. The results in the final column, using asymmetric pair-wise fixed effects, those akin to the standard 'within' estimator, yield similar results. According to the theory, trade costs are treated symmetrically. Clearly, in reality this might not be the case however. The foregoing results would suggest that empirically, at the aggregate level at least, it is not important which set of fixed effects are used since the results are not significantly different from one another.

Since no study to the knowledge of the author, has crucially controlled for the age on arrival of migrants, little evidence currently exists as to the persistence of the effect of migrants upon trade over time. Since the estimated panel contains observations at ten-year intervals, one interpretation of the results is that they more adequately pick up the long-run relationship between trade and migration, a steady-state estimate once capital has had time to adjust. It might also be the case that migrants only facilitate trade between those countries which are absent from the sample. Given the proportions of trade and migration covered in the sample however - which also include many countries for which positive effects have been found in the existing literature – this also seems unlikely.

Of course the implementation of fixed effects is no panacea, although this is the strategy adopted to control for the endogeneity bias of currency unions and regional trade agreements elsewhere in the literature. First they treat both positive and negative correlations of the unobserved pair-wise factors with the migrant network variables as symmetric. A further cause for concern is attenuation bias. Should one of the right-hand side variables be poorly measured, this would lead to a classic error-in-variables problem whereby differencing the panel data biases the resulting estimates towards zero. This is especially the case should the variable in question be largely time-invariant. However, in terms of both the aggregate immigrant and emigrant stocks and the bilateral pair-wise migration corridors, there have been dramatic changes over time, such that this is not a cause for concern. Moreover, the classic error-in-variables generally leads to inconsistent estimates of all the βs and since the estimates of the other

explanatory variables are strictly in line with previous estimates, this provides indirect evidence that the estimates can be trusted.

8.1 Robustness

Lastly, it might be the case that some of the missing values, which we have every reason to believe are true missing values, are in fact zero. If so, then a failure to account for the heteroskedastic residuals, which arise from numerous zero values in the regressor, might lead to inconsistent results. Table 4 provides a summary of how many missing values exist in every decade.

Table 4 % Missing values in sample 1, 1960-2000

Decade	% Missing
1960	**27**
1970	**18**
1980	**13**
1990	**12**
2000	**9**

Source: Author's Calculations.

Table 5 Pooled Regressions to test for robustness

Dependent Var.: Log Exports	**Migrant Links**			
	Sample 2	**Sample 1 No 1960**	**Sample 3**	**Sample 4**
Log immigrants	-0.005	-0.012	-0.025**	-0.024*
	(0.013)	(0.016)	(0.013)	(0.014)
Log emigrants	-0.018	-0.009	0.016	0.020
	(0.012)	(0.017)	(0.016)	(0.020)
RTA	0.439***	0.498***	0.380***	0.429***
	(0.068)	(0.067)	(0.069)	(0.081)
Common Currency	0.522***	0.506***	0.458***	0.380***
	(0.146)	(0.112)	(0.118)	(0.129)

Importer/Exporter-time-varying dummies (it/jt)	YES	YES	YES	YES
Asymmetric pair dummies (ij)	YES	YES	YES	YES
Observations	30,625	13,848	10,387	7,876
R^2	0.654[a]	0.681[a]	0.814[a]	0.833[a]

The dependent variable is the log of bilateral exports. All regressions include importer and exporter fixed effects. Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively. Robust standard errors are provided in parentheses. [a] denotes that the estimated R^2 is not comparable to the R^2 in other tables since it is calculated using the standard within estimator as opposed to the least squares dummy variable estimator. This is to ensure consistency within the table since it is not possible to use the LSDV for sample 1 since it is beyond the limits of Stata.

Clearly the greatest number of missing values is in 1960, which is expected given the timing of the onset of globalization. In order to test for the inclusion of the missing values, table 5 provides further estimates. Column 2, estimates equation [9] on sample 2, the full sample of 68*178 countries. Column 3 again estimates sample 1, but this time excluding the year 1960, the year which comprises the greatest proportion of missing values. Column 4 provides estimates for sample 3, a sub-sample which comprises only those origins with fewer than 30 missing values; while column 5 presents the estimates for sample 4, a sub-sample which comprises the fewest missing values, with the regression estimated on those exporters which report fewer than 20 missing values. The results are remarkably stable across all five samples. None of the estimates on either the currency union or the RTA variables are statistically different from one another and no statistically significant effect of emigrants on exports is found in any of the samples. Similarly, the estimated impact of immigrants on trade is either negative or insignificantly different from zero across all five samples. The results in table 5 therefore lend credibility to the main estimates, since it is clear the results are not an artifact of either sample selection or the inclusion of the missing values.

9. Regional Results

Given the weight of evidence in the literature to date, the results pooling observations across the entire sample are very surprising. Given Winters' insight that the least similar countries have the greatest potential for trade however, the sample is next divided into the relatively affluent North[25] and comparatively poorer South. Two regressions are estimated for each regional combination. The first column presents results akin to column 2 of table 3, when country-time varying dummies are included but country-pair fixed effects are not. The results in the second column present the most restrictive specification, which then additionally include asymmetric (*pairid)* fixed effects (see table 6).

[25]The countries of the 'North' refer to those countries that have been consistently wealthy throughout the period i.e. the countries of the OECD minus the Czech Republic, Hungary, Korea, Mexico, Poland the Slovak Republic and Turkey.

The first set of regional results, those from columns (1), omit controls for unobserved pair-factors and again produce the results typically found in the literature. The coefficient on distance is again around minus 1, the negative impact of which disproportionately affects Southern exporters. While again the contiguity variable is insignificant across all specifications, the role of common language, a proxy for cultural proximity, is significantly positive for trade between the North and the South. Sharing a colonial heritage is again found to strongly influence trade, except in the case of Southern exports to the North. Similarly, sharing a common legislative system positively impacts trade except for Northern exports to the South. While few inferences can be drawn with regards the RTA or common currency variables, due to the well-documented endogeneity bias which exists in their presence, positive coefficients result. Importantly, in the absence of pair-wise fixed effects, all the coefficients on the immigrant and emigrant stock variables are highly significant and positive with the exception of the effect of Northern immigrants on Southern exports.

Again however, the results for all four regional combinations alter dramatically once controls are added for unobserved heterogeneity. Regional trade agreements are then only found to positively influence exports between countries of the North. Similarly, sharing a common currency is only found to boost Northern exports to Southern countries. The most startling results however again concern the migration variables. Immigrants and emigrants are only found to influence aggregate bilateral trade flows for exports from the North to the South and even in this case the estimates are smaller than many papers in the literature would suggest. Since the degree of differentiation of exported goods is likely far higher from countries of the North, and since informational asymmetries are likely highest between countries of the North and the South; migrants might be expected to influence trade the most between the North and the South *a priori*. The failure to uncover an impact of migration upon trade is most surprising in the context of North-North trade. Here, over 98% of the variation in bilateral trade is explained, the trade and the migration data are of the highest quality and exports are likely the most differentiated. One plausible explanation for this result would be that information is more readily available about the countries of the North such that migrants cannot exert much influence in terms of bridging informational asymmetries.

Table 6 Direct Links Regional Regressions, 1960-2000

Dependent Var.: Log Exports	**Migrant Links**							
	N-N (1)	**N-N (2)**	**S-N (1)**	**S-N (2)**	**N-S (1)**	**N-S (2)**	**S-S (1)**	**S-S (2)**
Log immigrants	0.040***	0.010	-0.005	-0.005	0.083***	0.033*	0.048***	-0.029
	(0.015)	(0.016)	(0.022)	(0.034)	(0.017)	(0.017)	(0.017)	(0.030)

Log emigrants	0.040**	0.011	0.106***	0.016	0.054***	0.055**	0.058***	-0.016
	(0.018)	(0.018)	(0.019)	(0.022)	(0.018)	(0.026)	(0.016)	(0.034)
Log distance	-0.824***	.	-1.041***	.	-0.863***	.	-1.318***	.
	(0.062)	.	(0.092)	.	(0.080)	.	(0.081)	.
Contiguity	0.084	.	0.487	.	0.162	.	-0.112	.
	(0.085)	.	(0.476)	.	(0.477)	.	(0.206)	.
Common Language	0.044	.	0.329**	.	0.334**	.	0.080	.
	(0.087)	.	(0.156)	.	(0.150)	.	(0.155)	.
Colony	0.406***	.	0.163	.	0.525***	.	0.932*	.
	(0.134)	.	(0.211)	.	(0.178)	.	(0.508)	.
RTA	0.403***	0.267***	0.085	0.115	0.245*	0.253	-0.095	0.096
	(0.074)	(0.059)	(0.198)	(0.216)	(0.148)	(0.178)	(0.165)	(0.267)
Common Legislation	0.311***	.	0.295***	.	-0.044	.	0.216**	.
	(0.063)	.	(0.098)	.	(0.081)	.	(0.095)	.
Common Currency	0.305***	0.199	1.504**	1.093*	1.226**	0.36	0.000	0.831
	(0.125)	(0.123)	(0.613)	(0.644)	(0.506)	(0.394)	(0.423)	(0.741)
Importer /Exporter time-varying dummies (it/jt)	YES	YES	YES	YES	YES	YES	YES	YES
Asymmetric pair dummies (ij)	.	YES	.	YES	.	YES	.	YES
Observations	2,088	2,088	4,187	4,187	4,269	4,269	6,174	6,174
R^2	0.959	0.985	0.788	0.930	0.847	0.894	0.671	0.857

The dependent variable is the log of bilateral exports. All regressions include importer and exporter fixed effects. Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively. Robust standard errors are provided in parentheses.

The only paper, with estimates directly comparable to those presented here however, are those provided by Felbermayr and Jung (2009), since, as argued throughout the paper, cross-sectional results should be viewed with caution given that other bilateral factors, which might drive both trade and migration cannot be accounted for. They estimate a panel of North-South trade and migration for 1990 and 2000 using the

'within' estimator, which yields a coefficient on the stock of immigrants from the South in the North of 0.112, (with a standard error of 0.043). Given the various differences between the approached adopted, the results here are considered consistent with their findings.[26]

10. Country Results

The results so far relate to the average effect of both immigrants and emigrants upon trade, either across countries (table 2), over time (tables 3 and 5) or across regions and time (table 6). In each case, controlling for unobserved pair-wise factors removes most if not all of the positive affect of migration upon trade. The results may be further decomposed by interacting the immigrant variable with the 68 destinations and the emigrant variable by the 68 origins (equation 10), to yield the average effects of these variables for specific countries over time.

$$10.\ lnX_{ijt} = \beta_0 + \pi_1 \sum_{j=1}^{j=68} ln\left(\left(mig_{jit}\right) * destination_j\right) + \pi_2 \sum_{i=1}^{i=68} ln\left(\left(mig_{ijt}\right) * origin_i\right) + \beta_1 cont_{ijt} + \beta_2 lang_{ijt} + \beta_3 col_{ijt} + \beta_4 rta_{ijt} + \beta_5 leg_{ijt} + \beta_6 curr_{ijt} + \varphi_{it} + \gamma_{jt} + \tau_{ij} + \varepsilon_{ijt}$$

Where, π_1 and π_2 are coefficient vectors for the interaction variables. This exercise pushes the data to the very limit, since only four/five observations are available for each country. The goal then, is not to draw firm inferences with regards point estimates at the country level, but rather to get a better sense of the direction of bias which results from failing to take account of unobserved pair-wise heterogeneity. The statistically significant coefficients for the immigrant interactions from [10], from both including and omitting τ_{ij} are presented in Table 7. Due to their similarity, the emigrant interactions are presented in Appendix 2 for the sake of brevity

Table 7 again serves to highlight the disparity in the results when unobserved factors are omitted. In the absence of pair-wise fixed effects, 35 countries have statistically significant immigrant-interaction point estimates, while 33 countries have statistically significant emigrant-interaction point estimates. While the overwhelming majority of these point estimates are positive, importantly, some statistically significant and negative coefficients also result i.e. trade diversion. Although this finding is largely absent from the literature it seems wholly plausible since networks might well; organize the production of a good that was previously imported from elsewhere, source these goods from third-party countries or otherwise find more profitable destinations for goods. In order to do this, direct migrant networks might tap into their

[26] Felbermayr and Jung omit the effect of Northern migrants residing in Southern countries, which might bias their estimate upwards. Their time period is shorter, they take the average of trade flows over time to smooth the trade data, implement the geometric average of trade flows in estimation while their migration data only captures migrants 25 and over.

wider international networks, which although an interesting possibility is beyond the scope of the current study.

Table 7: Results for Country-level Immigrant Interactions

Country	Mig. Stock	Coeff. No τ_{ij}	Sig.	Coeff. Inc. τ_{ij}	Sig.	Country	Mig. Stock	Coeff. No τ_{ij}	Sig.	Coeff. Inc. τ_{ij}	Sig.
Australia-Norfolk Is.	240,778	0.20	***	-0.02		Hong Kong SAR, China	287,117	0.11	***	-0.01	
Philippines	1,237,433	0.19	***	0.05		Oman	75,655	0.11	*	-0.08	
Venezuela	162,537	0.19	***	-0.01		Slovenia	13,231	0.11	**		
Angola	172,856	0.18	**	-0.19	*	South Africa-Botswana-Lesotho-Namibia-Swaziland	203,721	0.11	**	-0.03	
Chile	342,971	0.18	***	-0.12		United Arab Emirates	17,467	0.09	*	-0.09	
Malaysia	494,239	0.18	***	0.04		Pakistan	5,621,668	0.09		-0.14	*
Peru	231,434	0.18	***	-0.07		Tunisia	426,364	0.09	***	-0.07	
Dominican Rep.	330,979	0.17	***	0.03		Norway	192,642	0.09	**	0.07	
Israel	125,829	0.15	**	0.04		Thailand	168,634	0.09	***	-0.07	
Ecuador	221,108	0.15	***	-0.10		Finland	316,196	0.06	*	0.16	*
Mexico	3,607,198	0.15	***	-0.10		Greece	938,753	0.06	**	0.00	
New Zealand	242,460	0.15	***	-0.02		Turkey	1,733,521	0.05	*	-0.11	*
Indonesia-East Timor-Maldives	742,776	0.14	***	-0.03		Germany	2,840,813	-0.06	**	0.04	
Russia	796,637	0.14	***			Belgium-Luxembourg	311,149	-0.06	**	-0.10	
Colombia	723,293	0.14	***	-0.08		Netherlands	670,888	-0.08	**	-0.03	
Canada	1,115,927	0.14	***	-0.21	***	France-Monaco-Andorra	1,045,326	-0.08	***	-0.09	
Qatar	1,586	0.12	*	-0.06		Saudi-Arabia	56,140	-0.09	*	-0.17	**
Argentina	253,322	0.12	**	-0.13		Czechoslovakia	1,014,715	-0.10	*	-0.18	

Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively.

When unobserved pair-wise factors are accounted for, again the majority of these positive effects disappear. It was argued in the endogeneity section, that in theory, the direction of the bias from failing to account for the unobserved heterogeneity could work in either direction. This plays out, for example the point estimate for Finland increases from 0.06 to 0.16 following the inclusion of τ_{ij}. Conversely, the coefficient for Canada falls from 0.14 to -0.21. However, the global and regional estimates suggest that overall; the unobserved bilateral factors are positively correlated with migrant networks on average, such that their inclusion dramatically removes most of the effect of migration upon trade. This story is again borne out by these results, since the vast majority of positive estimates are found to be insignificant or indeed significant and negative when fixed effects are included.

11. Third Party Effects

So far, attention has been focused upon *direct* links. It is equally plausible however; that migrant networks exist between trading pairs which pertain to a country (of birth) *k*, which is neither the importing ($k \neq i$) nor exporting nation ($k \neq j$) i.e. that a *third-party* effect exists which is driving the observed coefficients.[27] Continuing from the discussion in Section 3, these third party effects can be modeled as $\ln(MIG_{ki} * MIG_{kj})$ where $i \neq j$ and $k \neq i$ *and* $k \neq j$. This is another important contribution of this paper, since despite only 68 countries being chosen for analysis; the comprehensive migration dataset permits third-party effects pertaining to potentially all (178) countries of the world to be included in estimation (see Equation 11).

$$11.\ lnX_{ijt} = \beta_0 + \beta_1 \ln(mig_{ijt}) + \beta_2 \ln(mig_{jit}) + \delta_1 \sum_{k=1}^{k=178} \ln(mig_{kjt} * mig_{kit}) + \beta_3 cont_{ijt} + \beta_4 lang_{ijt} + \beta_5 col_{ijt} + \beta_6 rta_{ijt} + \beta_7 leg_{ijt} + \beta_8 curr_{ijt} + \varphi_{it} + \gamma_{jt} + \tau_{ij} + \varepsilon_{ijt}$$

Where additionally, ς_1 is a vector containing the coefficients for the interactions of the third-party effects. Table 8 again presents the statistically significant point estimates for the third-party effects estimated in [11] while both omitting and including τ_{ij}. The estimates of the indirect effects of migrants are more stable in comparison with the existing literature, whether or not pair-wise fixed effects are included in the regression. Prior to the inclusion of τ_{ij}, there are 30 positive and 29 negative point estimates; suggesting in comparison with direct effects third-party networks are more likely to divert trade.

It is not as immediately obvious why pair-wise fixed effects should be implemented when testing for third-party effects. Felbermayr, Jung and Toubal (2009) argue that third-party effects should be exogenous to bilateral trade flows between countries *i* and *j*, since the migrants pertain to countries *k*.[28] However, if migrants from a country k are believed to foster trade between countries *i* and *j* then there might be unobserved bilateral factors between *i* and *j*, which encourage migrants from *k* to select into migrating into those countries in the first place. In fact, controlling for pair-wise unobserved factors again drastically alters the results, with only 15 point estimates surviving. In the case of third-party effects however, the direction of the bias from excluding controls for the unobserved pair-wise factors is far from clear, since many estimates are both biased up and down. This result contrasts nicely with the estimates of the direct results. In that case, the direction of the bias was on average positive, which might be expected since international bilateral ties are more generally 'good' as opposed to 'bad' and in turn these good relationships might be expected to be positively correlated on average with migrant networks. In the case of indirect networks however, the results suggest a far more complex relationship. Arguably, these complexities can only be identified with richer more disaggregated data.

[27] Felbermayr, Jung and Toubal (2009) are credited as having been the first to highlight the difference between direct and indirect links.

[28] Furthermore they argue that these third-party effects must operate though the transaction cost channel.

Table 8: Results for Country-level Indirect Interactions

Country	Mig. Stock	Coeff. No τ_{ij}	Sig.	Coeff. Inc. τ_{ij}	Sig.	Country	Mig. Stock	Coeff. No τ_{ij}	Sig.	Coeff. Inc. τ_{ij}	Sig.
Latvia	54,554	0.12	***	0.04		Iceland	19,205	0.00		-0.02	*
Cameroon	41,478	0.07	***	0.04		Myanmar	214,829	0.00		0.03	**
United Kingdom	3,649,716	0.06	***	-0.12	**	Antigua-British Virgin Is.-Dominica-Grenada-Montserrat-St Kitts-Anguilla-St Lucia-St Vincent	91,348	-0.01		-0.02	*
Nepal	612,465	0.06	***	0.02		Peru	231,434	-0.02		0.04	*
Japan	564,264	0.05	***	0.02		Egypt	1,021,589	-0.02		0.04	*
Iran	446,783	0.05	***	0.04		Jamaica-Turks and Caicos-Cayman Is.	459,221	-0.02	**	-0.01	
India	6,361,587	0.05	*	-0.04		Bahamas	16,859	-0.02	*	0.01	
Romania	976,563	0.05	*	0.06		Bhutan-Brunei	36,197	-0.02	**	0.00	
Czechoslovakia	1,014,715	0.05	*	0.03		Philippines	1,237,433	-0.03	*	0.00	
Guatemala	192,359	0.04	**	0.00		Congo	17,777	-0.03	*	-0.03	
France-Monaco-Andorra	1,045,326	0.04	**	0.04		Greenland	5,418	-0.03	**	0.00	
Guyana	134,275	0.04	***	0.00		Gibraltar	10,984	-0.03	**	-0.01	
Germany	2,840,813	0.04	**	-0.02		Ghana	158,859	-0.03	*	0.00	
Trinidad and Tobago	138,159	0.04	***	0.00		Portugal	1,373,239	-0.03	**	0.01	
Denmark-Faeroe Islands	203,767	0.04	***	0.03		Australia-Norfolk Is.	240,778	-0.03	*	0.00	
Guinea-Bissau-Cape Verde-Sao Tome y Principe	86,609	0.03	***	0.00		Laos	156,926	-0.03	**	-0.01	
Samoa	38,689	0.03	***	0.01		Venezuela	162,537	-0.03	***	0.00	
Belgium-Luxembourg	311,149	0.03	*	0.00		Mexico	3,607,198	-0.04	***	0.00	
Bosnia and Herzegovina	159,234	0.03		0.12	***	Colombia	723,293	-0.04	**	-0.02	
North Korea	403,285	0.03	***	0.01		Central African Republic	3,725	-0.04	**	0.00	
Brazil	300,387	0.03	**	0.00		Democratic Republic of Congo	41,717	-0.04	***	-0.02	
Seychelles	4,276	0.03	*	0.04	**	Malta	90,044	-0.04	***	-0.02	
Korea	1,211,822	0.03	**	0.00		Suriname	103,078	-0.04	***	-0.02	
Spain	1,631,399	0.03	*	0.01		Falklands	647	-0.04	*	-0.02	
Papua New Guinea	17,593	0.03	*	-0.02		Israel	125,829	-0.04	**	-0.02	
Sudan	140,239	0.02	*	-0.02		Mongolia	2,118	-0.04	***	0.01	
Netherlands Antilles-Aruba	65,811	0.02	***	0.02		Djibouti	1,369	-0.04	***	-0.03	
Bahrain	9,983	0.02	*	0.00		Gambia	8,351	-0.05	***	-0.03	
Thailand	168,634	0.02	*	0.02		Vietnam	640,323	-0.05	***	-0.02	
Ecuador	221,108	0.02	*	0.03	**	Slovenia	13,231	-0.05	***	-0.01	
Macao	424,121	0.02	***	0.01	*	Saudi Arabia	56,140	-0.05	***	-0.05	***
Algeria	1,354,783	0.01		-0.01		Estonia	38,333	-0.06	**	-0.05	*
Syria	274,244	0.01		-0.04	**	Slovakia	106,463	-0.06	***	-0.15	***
Malaysia	494,239	0.00		-0.03	**	Kazakhstan	538,338	-0.08	**	0.01	

Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively.

12. *Conclusion*

Do migrants really foster trade? The answer based on the results in this paper would be a tentative *Yes*. Abstracting from the question of *how* migrants foster trade, the key question asked in this paper is whether migrants foster trade *whatsoever*. Implementing a panel which comprises the majority of world trade and migration in each period, the core results show that a failure to account for unobserved pair-wise heterogeneity, which in turn is positively correlated with migrant networks, lead to a substantial overestimate of the impact of migration on trade. The estimates imply that in the longer run, migration has a negligible or indeed negative impact on trade. Dividing the world into the relatively affluent North and poorer South, the results show that migrants from either region *only* affect Northern exports to the South. Further, the country level results suggest that migrants may both create and divert international trade. Taken as a whole, the results demonstrate the large biases inherent in cross-sectional studies investigating the trade-migration nexus and highlight the need to be cautious when interpreting previous findings. The surprise therefore is not that few positive effects are found, but rather that such unambiguously positive and robust results have featured so prominently in the literature to date. While an international examination of the trade-migration nexus at the product level is absent from the existing literature, the results from this paper strongly suggest that this should be a research priority should the complexities of the trade-migration nexus be truly understood.

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Appendix 1: List of Countries in each sample

Sample 1

Algeria	Czech Republic	Indonesia-East Timor-Maldives	Morocco	Romania	Tunisia
Angola	Czechoslovakia	Iran	Netherlands	Russia	Turkey
Argentina	Denmark-Faeroe Islands	Ireland	New Zealand-Cook Islands-Niue-Tokelau	Saudi Arabia	United Arab Emirates
Australia-Norfolk Is.	Dominican Republic	Israel	Nigeria	Singapore	United Kingdom
Austria	Ecuador	Italy	Norway	Slovakia	United States-Puerto Rico-US Virgin Is.
Belgium-Luxembourg	Finland	Japan	Oman	Slovenia	Venezuela
Brazil	France-Monaco-Andorra	Kazakhstan	Pakistan	South Africa-Botswana-Lesotho-Namibia-Swaziland	Vietnam
Bulgaria	Germany	Korea	Peru	Soviet Union	Fm. Yugoslavia
Canada	Greece	Kuwait	Philippines	Spain	
Chile	Hong Kong SAR, China	Libya	Poland	Sweden	
China	Hungary	Malaysia	Portugal	Switzerland-Liechtenstein	
Colombia	India	Mexico	Qatar	Thailand	

Sample 2

Afghanistan	Central African Republic	Georgia	Kyrgyz Republic	North Korea	St. Helena
Albania	Chad	Germany	Laos	Norway	St. Pierre and Miquelon
Algeria	Chile	Ghana	Latvia	Oman	Sudan
Angola	China	Gibraltar	Lebanon	Pakistan	Suriname
Antigua-British Virgin Is.-Dominica-Grenada-Montserrat-St Kitts-Anguilla-St Lucia-St Vincent	Colombia	Greece	Liberia	Panama	Sweden
Argentina	Congo	Greenland	Libya	Papua New Guinea	Switzerland-Liechtenstein
Armenia	Costa Rica	Guatemala	Lithuania	Paraguay	Syria

Australia-Norfolk Islands	Cote d'Ivoire	Guinea	Macao	Peru	Tajikistan
Austria	Croatia	Guinea-Bissau-Cape Verde-Sao Tome y Principe	Macedonia	Philippines	Tanzania
Azerbaijan	Cuba	Guyana	Madagascar	Poland	Thailand
Bahamas	Cyprus	Haiti	Malawi	Portugal	Togo
Bahrain	Czech Republic	Honduras	Malaysia	Qatar	Trinidad and Tobago
Bangladesh	Czechoslovakia	Hong Kong SAR, China	Mali	Reunion-Comoros	Tunisia
Barbados	Democratic Republic of Congo	Hungary	Malta	Romania	Turkey
Belarus	Denmark-Faeroe Islands	Iceland	Guadeloupe-Martinique	Russia	Turkmenistan
Belgium-Luxembourg	Djibouti	India	Mauritania	Rwanda	Uganda
Belize	Dominican Republic	Indonesia-East Timor-Maldives	Mauritius	Samoa	Ukraine
Benin	Ecuador	Iran	Mexico	Saudi Arabia	United Arab Emirates
Bermuda	Egypt	Iraq	Moldova	Senegal	United Kingdom
Bhutan-Brunei	El Salvador	Ireland	Mongolia	Serbia	United States-Puerto Rico-US Virgin Is.
Bolivia	Equatorial Guinea	Israel	Morocco	Seychelles	Uruguay
Bosnia and Herzegovina	Estonia	Italy	Mozambique	Sierra Leone	Uzbekistan
Brazil	Ethiopia-Eritrea	Jamaica-Turks and Caicos-Cayman Is.	Nepal	Singapore	Venezuela
Bulgaria	Falkland Islands	Japan	Netherlands	Slovakia	Vietnam
Burkina Faso	Fiji-Tonga-Nauru	Jordan	Netherlands Antilles-Aruba	Slovenia	Yemen
Burma	Finland	Kazakhstan	New Caledonia-French Polynesia-Wallis and Futuna	Somalia	Fm. Yugoslavia
Burundi	France-Monaco-Andorra	Kenya	New Zealand-Cook Islands-Niue-Tokelau	South Africa-Botswana-Lesotho-Namibia-S	Zambia
Cambodia	French Guiana	Kiribati-Vanuatu-Tuvalu-Solomon Islands	Nicaragua	Soviet Union	Zimbabwe

Cameroon	Gabon	Korea	Niger	Spain	
Canada	Gambia	Kuwait	Nigeria	Sri Lanka	

Sample 3

Argentina	Czech Republic	India	Mexico	Russia	Thailand
Australia-Norfolk Is.	Denmark-Faeroe Islands	Indonesia-East Timor-Maldives	Morocco	Singapore	United Kingdom
Austria	Finland	Ireland	Netherlands	Slovakia	United States-Puerto Rico-US Virgin Is.
Belgium-Luxembourg	France-Monaco-Andorra	Italy	New Zealand-Cook Islands-Niue-Tokelau	Slovenia	Fm. Yugoslavia
Brazil	Germany	Japan	Norway	Spain	
Canada	Greece	Kazakhstan	Pakistan	Sweden	
Colombia	Hong Kong SAR, China	Malaysia	Portugal	Switzerland-Liechtenstein	

Sample 4

Argentina	Czech Republic	Hong Kong SAR, China	Malaysia	Russia	Sweden
Belgium-Luxembourg	Denmark-Faeroe Islands	Ireland	Netherlands	Singapore	Switzerland-Liechtenstein
Brazil	France-Monaco-Andorra	Italy	Norway	Slovakia	Thailand
Canada	Germany	Japan	Pakistan	Slovenia	United Kingdom
Colombia	Greece	Kazakhstan	Portugal	Spain	United States-Puerto Rico-US Virgin Is.

Appendix 2: Results for country-level emigrant interactions

Country	Mig. Stock	Coeff. No τ_{ij}	Sig.	Coeff. Inc. τ_{ij}	Sig.	Country	Mig. Stock	Coeff. No τ_{ij}	Sig.	Coeff. Inc. τ_{ij}	Sig.
Venezuela	162,537	0.30	***	0.08		Czech Republic	170,763	0.11	**		
Mexico	3,607,198	0.22	***	0.00		New Zealand-Cook Islands-Niue-Tokelau	242,460	0.11	***	-0.02	
Colombia	723,293	0.21	***	0.07		Indonesia-East Timor-Maldives	742,776	0.10	***	0.06	
Nigeria	142,818	0.21	***	-0.12		Tunisia	426,364	0.09	*	-0.23	
Algeria	1,354,783	0.20	***	-0.01		Norway	192,642	0.09	***	0.02	
Chile	342,971	0.18	***	-0.07		Portugal	1,373,239	0.08	***	0.13	
Slovakia	106,463	0.18	***			Pakistan	5,621,668	0.07	***	0.15	*
Thailand	168,634	0.17	***	-0.10		Turkey	1,733,521	0.07	**	0.02	
Slovenia	13,231	0.16	***			Qatar	1,586	0.06		-0.31	*
United Arab Emirates	17,467	0.15	**	0.06		Malaysia	494,239	0.06	**	0.07	
Ecuador	221,108	0.15	***	0.10		Ireland	897,049	0.06	**	-0.06	
Kazakhstan	538,338	0.15	*			Australia-Norfolk Is.	240,778	0.05		-0.10	*
Dominican Republic	330,979	0.15	**	-0.04		Japan	564,264	0.04		0.10	**
Hong Kong SAR, China	287,117	0.14	***	0.02		Singapore	140,112	0.04		0.15	*
Oman	75,655	0.14	**	0.04		Italy	4,087,945	-0.02		0.09	*
Peru	231,434	0.14	***	-0.14		France-Monaco-Andorra	1,045,326	-0.05		0.13	*
Russia	796,637	0.14	**			Germany	2,840,813	-0.05	*	0.05	
Israel	125,829	0.14	*	0.03		Belgium-Luxembourg	311,149	-0.07	***	-0.03	
Philippines	1,237,433	0.13	***	0.03		Czechoslovakia	1,014,715	-0.13	***	-0.05	
Greece	938,753	0.11	***	-0.01							

Superscripts ***, **, * denote statistical significance at 1, 5 and 10% respectively.