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Migration Regulation Contagion

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Abstract

This paper examines the political economy of a selective immigration policy in a model with incomplete information on the characteristics of migrants. We address two questions: First, how does a selective immigration policy affect the number of immigrants which is admitted by the receiving country, and second, how does a selective immigration policy in one country affect immigration policies in other countries. We find (i) that countries with selective immigration policies *ceteris paribus* tend to admit more migrants than countries without such policies, and (ii) that neighboring countries will follow each other in implementing selective immigration policies. These theoretical findings are supported by evidence from an econometric panel analysis of immigration policies in 15 OECD countries in the period from 1980 to 2005.

Keywords: International migration; political economy of migration; skill-selective immigration policies.

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1 Introduction

While the structure and patterns of international migration in Europe and other OECD countries as well as the effects of immigration policies on these patterns are relatively well understood (e.g. Appleyard, 2001; Bertoli et al., 2009; Freeman, 2006; Neumayer, 2004; Hooghe et al., 2008; Venturini, 2007), the *emergence* of immigration policies has received surprisingly little attention. This is particularly disturbing, since standard economic models of international migration would predict that immigration increases the aggregate income of natives in the receiving countries (e.g. Dixit/Norman, 1980; Wong, 1995; Baldwin/Wyplosz, 2005). Hence, nations should not be expected to constrain the inflow of people. Obviously, reality contradicts such theory.

There are several available explanations to resolve this contradiction. First, the gains from migration are all but equally distributed: production factors which are substitutes for migrant labor lose, while production factors which are complements tend to win. The benefits and losses from migration therefore depend on the skill level and wealth of individuals. In real economies, the economic effects of migration essentially depend on labor market institutions: Models which consider wage rigidities and unemployment show that immigration can also result in an aggregate loss for the native population (e.g. Boeri/Brücker, 2005). This is particularly relevant in the European context with periodically widespread unemployment. Second, migration has an ambiguous impact on the welfare state: while migrants on average pay less taxes and are more than proportional affected by unemployment, the payments of migrants to pension schemes exceed the returns. Moreover, against the background of demographic change, they increase the workforce and mitigate age dependency rates. The impact of migration on the fiscal balance of the welfare state is therefore ambiguous (e.g. Bonin et al., 2000; Boeri et al., 2002). Finally, native welfare is also affected by the social integration of migrants. Criminality, ethnic and cultural differences, or simply xenophobia have beyond economic aspects an important impact on the perception of migration for different groups of the population. In all three dimensions – integration into the labor market, integration into the welfare state, and social integration – the effects of migration essentially depend on the human capital characteristics of migrants. The benefit-cost ratio tends to increase with education levels of migrants and other favorable human capital characteristics.

Hence, the political acceptance of migration significantly depends on the composition of the migrant population. During the 1990s and 2000s we can observe an increasing resentment against further immigration in many OECD

countries. New single-platform parties with an anti-migration agenda such as Jan Wilders in the Netherlands, the National Front in France, the FPÖ and the BZÖ in Austria and the Danish People's Party have successfully changed the political landscape, particularly in Europe. Furthermore, real or perceived security concerns, such as the war against terrorism, also shape popular opinion and accordingly migration policy (Dover, 2008). Thus, many ruling policy makers in immigration countries find themselves in a tension between the potential economic benefits of migration and popular resentment, which can drive them out of office if immigration creates too many problems. The answer to this policy dilemma has been a screening of migrants, permitting good migrants in, while keeping bad migrants out. The screening of migrants has a long tradition in traditional immigration countries such as Canada, Australia, New Zealand and, to a lesser extent, the US, but is more and more copied by other OECD countries such as the UK and the Czech Republic (see Bertoli et al., 2009; Facchini et al. 2008). More recently, the Bluecard initiative of the European Commission tries to tackle the same problem. Although there are many differences between the immigration policies in all these countries, they have in common that they grant residence and work permits on the basis of education, language skills and other human capital criteria, which should help to draw migrants which yield a net gain for the receiving country.

The objective of this paper is to examine some of the fundamental mechanics of selective immigration policies. We address two main questions: First, whether screening affects the number of migrants which are admitted by the country of destination. Second, whether the decision to opt for screening in one country has an impact on immigration policies in countries which are close substitutes as destinations for immigrants, e.g. neighboring countries or countries which have similar characteristics such as language. Opting for screening in one country might trigger countries which are close substitutes as destinations to opt for screening as well. The issue of regulation contagion is particularly important in the European Union (EU), where currently 27 member states run and administer separate and largely diverging immigration policy schemes, even though there may be a substantial overlap in policy objectives (Koff, 2005). Since an increasing share of the EU immigrants origin in non-EU countries, the question of coordinating immigration policies is high on the agenda of the EU Commission and the member states.

The current paper relates and contributes to two strands of literature. On the one hand and most obviously, our contribution adds to the political economy of migration and the empirics of skill-selective immigration policies. A substantial literature has meanwhile examined how individual preferences and interest groups affect immigration policies (Facchini/Mayda, 2008; 2009;

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Hainmueller/Hiscox, 2008; Mayda, 2006), how immigration policies affect the skill structure of migration patterns (e.g. Belot/Hatton, 2008; Bertoli et al., 2009; Brücker/Defoort, 2009; Freemann, 1992; 1995; Grogger/Hanson, 2008; Hooghe et al., 2008, Ortega/Peri, 2009) and how the skill composition of the immigrant population impacts labor markets and the welfare state (e.g. Boeri/Brücker, 2005; Boeri et al., 2002; Razin et al., 2009). In contrast to this literature, which examines domestic channels by which immigration may affect policy outcomes and vice versa, we adopt a broader perspective here: We consider how policy spillovers from other countries affect national immigration policies. In our view, this question becomes increasingly relevant since more and more countries tend to adopt skill-selective immigration policies, which may in turn exert pressure on countries which do not. On the other hand, we contribute to the literature on policy diffusion (e.g. Kato, 2003; Shipan/Volden, 2006; 2008; Simmons/Elkins, 2004; Volden et al., 2008), which to the best of our knowledge has largely ignored the impact of policy spillovers between competing destinations of immigrants so far.[1]

The simple model we present in this paper is not about the self-selection of migrants or the migration decision of individual agents. This paper is instead about the effects that the efforts by one host country to improve the benefits of migration have on other destination countries. To simplify issues, we treat the pool of potential migrants as given. The channel we highlight is that screening by one destination country alters the composition of the remaining migrant pool available to other (second mover) countries. Most important, and the particular focus of the analysis, is the fact that the actual migrant type is not directly observable for the administration in the destination country. Screening, testing, the allocation of points, etc., only give imperfect information on the true type of the migrant. Thus, immigration countries act under the constraint of imperfect information on the migrants' true characteristics.

In the next step of our paper we test two hypotheses, which are derived from our theoretical considerations: First, whether countries which pursue a selective immigration policy admit more migrants than other countries do. Second, whether the fact that a country employs a selective immigration policy increases the probability that a neighboring country follows the same approach, i.e. whether we can observe clusters of countries with a selective

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[1]The only example we are aware of is Bertoli et al. (2009, Ch. 7) which addresses the question whether skill-selective immigration can result in a "Tragedy of the Commons", i.e. whether the increasing competition for high skilled immigrants might eventually exhaust the global pool of skilled labor. In contrast, we address the question whether for a given pool of skilled labor skill-selective immigration policies in one country reduce the average skill-level of the immigrant population in another country.

immigration policy. For this purpose, we use a new panel data set which covers migration flows into 15 OECD countries during the period from 1980 to 2005. This data set is complemented by data on immigration policies collected by Mayda/Patel (2004), which has been adjusted and extended for our analysis. Based on these data, we find robust evidence that (i) skill-selective immigration policies are correlated with larger immigration flows and that (ii) skill-selective immigration policies are correlated across countries which are close substitutes as destinations for migrants.

The remainder of the paper is organized as follows. Section 2 presents the simple model, which forms the basis of our further analysis. In Section 3 we confront the theoretical propositions of our paper with the available empirical evidence. Finally, Section 4 concludes.

2   A Simple Framework

Consider two countries, 1 and 2, facing a pool of n potential migrants from the rest of the world, where n is normalized to 1. Furthermore, assume that α of these potential migrants are undesirable, b for bad, migrants from the perspective of the two potential host countries, in the sense that they are unemployable, religious fanatics, criminal, welfare seekers or all of the above. While $(1 - \alpha)$ of the potential migrants are good migrants, g, in the sense that they generate a net-benefit for the receiving country and are thus desired migrants by both countries; note $\alpha \in [0, 1]$ and where α is common knowledge. However, a specific migrant's true type is unobservable to policy makers, only via some imperfect screening technology agents can be labelled such that

$$\Pr(label = g | type = g) = \Pr(label = b | type = b) = p, \qquad (1)$$

where p is known to the policy maker and accordingly we consider only $1 > p > \frac{1}{2}$. Finally, assume that policy makers in both countries – for example as driven by an election game, political competition or other events in the background – can only accept a maximum of k_i migrants of type b; while they, driven by a generally accepted net benefit from good migrants, want to maximize the total inflow, $m_{i,g}$, of g-types. Formally, they choose the total number of in-migrants, m_1 and m_2, such as to maximize:[2]

$$\max_{m_i} m_{i,g} \ s.t. \ m_{i,b} \leq k_i \ ; \ i = 1, 2 \,. \qquad (2)$$

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[2]In fact the two countries each face a linear programming problem, with a simple maximand, one constraint and the usual extreme point solutions. As will become clear below, the two programming problems will become inter-related via the effect on the migrant pool.

We have on purpose build the simplest of all frameworks to see what light it can shed on the fundamental question of regulation contagion.[3] Namely, under the above conditions, what are the total number of migrants, m_1 and m_2 that each country will be willing to admit? We will answer this question for three scenarios: First, no country administers a positive selection of migrants, i.e. no country uses the screening technology, p. Second, both countries apply the screening technology and only admit migrants labeled as g-types. Third, country one screens, while country two does not screen and moves second.

Before conducting the analysis, it is instructive briefly to highlight two central limitations of this simple model. First, we have not included screening costs into the above framework. Obviously, real-world selective migration polices impose administrative costs both on migrants and the destination country. For the policy maker objective this would imply an additional constraint in (2), say similar to a resource constraint, and none of the results of the model are affected. For the migrant, however, such screening costs would partly counterbalance the migration advantages (such as higher expected income). Thus screening costs would alter the pool of available migrants in a dynamic setting; however, for the static model examined here no qualitative changes occur. Second, we impose symmetry on the two potential destination countries throughout. This applies both to the strategic action space and to the pool of migrants. Only in their political constraints (e.g. k_i) we allow the countries to differ. A richer model would also separate the countries in terms of the *a priory* pool of potential migrants (say the attractiveness of the destination country). However, in order to focus on contagion effects we omit these aspects, having in mind that subsequent empirical testing controls for country fixed effects which absorb all time-invariant factors affecting the pool of immigrants or the costs of sceening (e.g. geography and similar factors).

In the benchmark case, labelled A, where neither of the two countries applies a screening technology we have the following simple situation. Permitting an inflow of m_i migrants results in αm_i bad migrants, and accordingly given the policy constraint, we have the maximizing migration choice of:

$$m_i^A = \frac{k_i}{\alpha} \; ; \; i = 1, 2 \, . \tag{3}$$

Next, consider a situation where both countries apply screening technology, p, and allow only those agents labeled as g-types to enter. In this case, denoted B, it must be that $(1-p)\alpha$ agents of the true type bad have been

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[3]Notice that the above government objective functions are independent on all accounts except for the common resource pool, so we depart from traditional Nash strategic interaction settings.

incorrectly labeled as good, while we have $p(1-\alpha)$ true good agents that have been correctly labeled as good. So there will be a total of $p(1-\alpha)+(1-p)\alpha$ agents carrying the label good, and accordingly the probability of getting a b-type agent is $p'_B = \frac{(1-p)\alpha}{p(1-\alpha)+(1-p)\alpha}$, and thus the permitted inflow of migrants in this case must be:

$$ m_i^B = \frac{k_i}{\alpha} \frac{p(1-\alpha)+(1-p)\alpha}{1-p} \; ; \; i=1,2 \,. \tag{4} $$

It is easy to verify that $\frac{p(1-\alpha)+(1-p)\alpha}{1-p} > 1$, and thus – as expected – with screening a larger total inflow of migrants is permitted.

In the third – asymmetric – scenario, C, where only country 1 screens, while country 2 does not screen and moves second, it must be that from the perspective of country 1 this situation is identical to scenario B. Thus

$$ m_1^C = \frac{k_1}{\alpha} \frac{p(1-\alpha)+(1-p)\alpha}{1-p} \,. \tag{5} $$

In contrast, country 2 faces an altered mix of potential migrants which it permits to enter at random. The composition of the reminder migrant pool features $(\alpha - m_1^C p'_B)$ b-types and $(1-\alpha) - m_1^C(1-p'_B)$ g-types. Or, put differently, since country 1 by maximizing $m_{1,g}$ has drawn k_1 bad migrants out of the pool and has accepted a total of m_1^C migrants, the probability of drawing a bad migrant from the reminder pool must be:

$$ p''_C = \frac{\alpha - k_1}{1 - m_1^C} = \frac{(1-p)\alpha(\alpha - k_1)}{(1-p)\alpha - k_1 p(1-\alpha) - k_1(1-p)\alpha} \,. \tag{6} $$

Accordingly, country two maximizes the inflow of good agents when setting the total inflow of migrants at:

$$ m_2^C = \frac{k_2}{\alpha} \frac{(1-p)\alpha - k_1 p(1-\alpha) - k1(1-p)\alpha}{(1-p)(\alpha - k_1)} \,. \tag{7} $$

Inspection of (7) and (3) discloses that $m_2^C < m_2^A$. Thus when country 1 applies a screening technology, country 2 permits fewer total migrants. Since country 2 still ends up with the same number of b-type agents it must arrive at a strictly lower number of the beneficial g-types. Accordingly country 2 must be worse off.

Two immediate policy conclusions can be made. First, the unilateral implementation of a screening procedure in country 1 increases the total permitted inflow of migrants into country 1 *and* reduces the total permitted inflow of migrants into country 2, thus one observes a tightening of policy

in country 2 following a green card regulation in country 1. Second, the nearhand solution for country 2 is, of course, also to implement a screening procedure, which would bring us to case B, where both countries permit larger numbers of migrants and receive larger numbers of the beneficial g-type migrants.

Finally, the effects of changes in the exogenous variables of the above model lead to highly intuitive comparative static results. For example, it can be shown that an improvement of the initial quality composition of the migrant pool, lower α, or an increase in tolerance levels, i.e. an increase in the policy constraints, k_1 and k_2, or a better screening technology, p, lead *ceteris paribus* to the country in question permitting larger total migrant inflows.

3   Empirical Evidence

From this simple theoretical framework we can derive two hypotheses, which can be falsified empirically: First, countries which pursue a selective immigration policy admit more migrants than countries which do not – anything else being equal. Second, if one country opts for a selective immigration policy, other countries may follow this example. The latter hypothesis is expected to hold particularly for countries in the same geographical region since international migration is heavily concentrated regionally. However, due to falling transport and communication costs the role of distance is eroding over time. Countries which have similar characteristics in terms of language, culture and economic opportunities, might therefore be considered as close substitutes from the perspective of potential migrants. We therefore consider different classifications for the substitution relationships below.

A detailed proof of both hypotheses is hardly possible at present, since only a small number of countries follow selective immigration policies consistently (see Bertoli et al., 2009; Chaloff/Lemaitre, 2009; Mayda/Patel, 2004). Moreover, the variance of immigration policies over time is relatively low. Nevertheless, on the basis of the available data we provide some first evidence.

3.1   The data set

Our analysis is based on annual gross immigration flows into fifteen OECD countries in the period 1980 to 2005, which gives altogether 390 balanced

panel observations.[4] The migration data for the years 1995 to 2005 are taken from the International Migration Dataset (IMD) provided by the OECD (2009). For the period 1980 to 1994 we used the data set collected and organized by Mayda (2007) which is based on OECD data as well. Since the data sources and methods of collection are the same, we merged these two data sources. For a detailed description of the approach, see also Mayda (2007) and Ortega/Peri (2009).

The immigration data are based on national population registers and residence permits, which can be considered as relatively accurate measures for the legal entry of foreign nationals. While the OECD makes an effort (especially since 1995) to maintain a consistent definition of immigrants across countries, some differences in the definition of migrants across countries remain. An important one is that some countries define immigrants on the basis of the place of birth and others on the basis of nationality. While this inconsistency can make a pure cross-country comparison inaccurate, it is a less severe problem in our case of fixed effects regressions where the within transformation controls for cross-country differences in measurement – at least if these differences in definitions and measurement concepts are time-invariant.

Categorizing skill-selective immigration policies is also difficult, since in most countries many channels of immigration exist which are in turn subject to many legal and other regulatory changes over time. We base our analysis on major differences in skill-selective immigration policies and major policy reforms in order to capture the main effects. For this purpose we use the Mayda/Patel (2004) data set which has been updated by Ortega/Peri (2009). The Mayda/Patel (2004) data set documents the main characteristics of immigration policies in several OECD countries (between 1980 and 2000) and the year of changes in their legislations. Ortega/Peri (2009) have updated this database to the year 2005. Following the same methodological approach, we classified immigration policies in New Zealand based on information provided by the home office (New Zealand, 2010) and OECD (2003). In addition, we use information on major policy reforms in selected countries presented in Bertoli et al. (2009) for cross-checking the information from the other sources. Using this information, we calculated an index of skill-selective immigration policies, which has a value from one (no skill-selective immigration policies at all) to ten (consistent skill-selective immigration policies).

The index of skill-selective immigration policies is reported in Annex Ta-

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[4]We are grateful to Anna Maria Mayda and Giovanni Peri for providing information and access to their datasets.

ble A1. As can be seen there, the traditional immigration countries Australia, New Zealand and Canada achieve the highest scores. In the beginning of the sample period, these countries effectively influenced the skill level of immigrants by admitting only migrants from sending countries which are characterized by high income levels and high human capital endowments, i.e. the Western European countries, the United States and Canada. Canada was the first among these countries which formally introduced a point-based system which regulated the entry of immigrants explicitly by human capital criteria such as education, occupation, age, language proficiency and work experience in 1967. Similar systems have been adopted by Australia in 1989 and by New Zealand in 1991. These systems have been reformed over time in order to improve the human capital characteristics of immigrants and their labor market performance. The picture is more mixed in the United States, where many channels for entering the country exist. The most important reform to improve the skill mix of the migrant population has been the adoption of the 1990 Immigration Act there. This Immigration Act established the H1B visa category, which explicitly opened a channel for the immigration of high skilled workers. However, at some 65,000 visas p.a. the number of workers which immigrate under this category is modest. Although many other channels exist which facilitate the immigration of high skilled individuals in the US, the skill-selective immigration policies are diluted inter alia by a large amount of illegal immigration. Still, immigration policies are much more skill-selective in the US than in the European countries covered by our sample. Many European countries have not yet any skill-selective immigration policies in place or they have only conducted minor reforms such as Germany during the Schröder government. The most comprehensive reform in Europe was adopted by the UK, which overhauled the immigration policies by introducing a point-based system.[5]

Considering a policy index instead of simple dummy variables has the main advantage that we can exploit policy changes for our analysis, which provides a much higher variance than simply including dummy variables. Although a higher variance of immigration policies would improve the analysis, we obtain a considerable variation in immigration polices both across and within countries in our sample (see Annex Table A1).

For analyzing policy spillovers, we have to decide which countries are close substitutes as migration destinations and which are not. To ensure robustness of results, we decided to include two different measures for this purpose. The first measure is simply based on geographical neighborhood,

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[5]For an overview on skill-selective immigration policies in the OECD, see also Bertoli et al. (2009), Mayda/Patel (2004) and Chaloff/Lemaitre (2009).

i.e. countries are considered as substitutes if they have a common border or, in the case of islands, are characterized by geographical proximity. In addition to this criteria, our second measure takes other characteristics into account, i.e. common language, culture etc. In this case, countries with English as the main native language (AUS, CA, NZL, the UK and the US) form a cluster as well as the Nordic countries (DK, NOR, SWE). Based on these two measures, we calculate for the countries which are considered as close substitutes a skill-selective immigration policy index, where each country considered as a substitute is weighted by its population size (see Annex Table A2).

Based on considerations derived from gravity models which explain trade and factor movements, we consider the real GDP per capita and population size as control variables. Our source for the GDP per capita variable are the Penn World Tables 6.1 (Heston et al., 2009) and for the population figures the World Bank Development Indicators 2010 (World Bank, 2010).

The descriptive statistics for all variables considered in the regressions are displayed in Annex Table A3.

3.2 Does screening yield more migration?

The first hypothesis states that countries which pursue skill-selective immigration policies tend to admit more migrants than others do. We examine this in a simple regression model which estimates the impact of our index for skill-selective immigration policies on the gross migration rate. More specifically, the regression model is specified as

$$m_{it} = \alpha_1 S_{i,t-1} + \eta' \mathbf{x}_{i,t-1} + e_{it}, \tag{8}$$

where m_{it} is the gross migration rate of destination country i, $S_{i,t-1}$ the index of skill-selective immigration policies, which is scaled between one and ten, $\mathbf{x}_{i,t-1}$ a vector of control variables, η the corresponding vector of coefficients, and μ_{it} the disturbance term. α_0 denotes the constant, α_1 the parameter of interest, i ($i = 1, 2, \dots 15$) is the destination country index and t ($t = 1, 2, \dots 25$) the time index. We thus explain the migration rate by the lagged index of skill-selective immigration policies and the lagged values of the control variables. We have chosen lagged values assuming that migrants form their expectations based on past values of the relevant institutional and economic variables. Note that Blanchflower/Oswald (2004) have shown that simple OLS models achieve similar results as ordered logit models already for three-point scales. Our skill-selective immigration policy index is measured at a ten-point scale, thus the results of our OLS and fixed effects models can be expected to be sufficiently accurate.

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The error term e_{it} is specified as a two-way error component model with fixed country and fixed time effects, i.e. as

$$e_{it} = \theta + \mu_i + \tau_t + \varepsilon_{it}, \tag{9}$$

where θ denotes a constant, μ_i denotes a country-specific fixed effect, τ_t a time-specific fixed effect and $\varepsilon_{it} \sim N(0, \sigma^2)$ is white noise. While the country-specific fixed effects capture all time-invariant factors which affect migration decisions such as geographical distance, language and culture, the time-specific fixed effects control for all time-varying factors which are common to all cross-sections in one time period such as joint macroeconomic shocks or transport and communication costs which fall over time. We test stepwise for the significance of the country-specific and time-specific fixed effects.

The other control variables are derived from the well-established gravity model of trade and factor mobility. We consider here three variables as controls: the gross domestic product (GDP) per capita at purchasing power parities and constant prices, the population size of the destination country and a deterministic time trend. The GDP per capita serves as an approximation for expected earnings in the receiving country and should thus affect the scale of migration positively. We expect that population size affects the gross migration rate negatively since the migration rate is already normalized by the population of the destination country. The expectation of a negative effect of population size follows from the fact that larger countries tend to have lower shares of external trade and factor mobility than smaller countries, other things being equal, since there is more room for internal trade and factor mobility in larger countries. Note that geographical distance and other time-invariant variables which are relevant in gravity equations drop out since we consider country-specific fixed effects.

Our final model is thus specified as

$$\ln m_{it} = \alpha_1 S_{i,t-1} + \alpha_2 \ln y_{i,t-1} + \alpha_3 \ln pop_{i,t-1} + e_{it}, \tag{10}$$

where $\ln y_{i,t-1}$ is the log of the GDP per capita measured in purchasing power parities and constant prices, $\ln pop_{i,t-1}$ the log of the population in the destination country.

As a robustness check, we estimate the model also in dynamic form, i.e. as

$$\ln m_{it} = \sum_{j=1}^{N} \gamma_j \, m_{i,t-j} + \alpha_1 S_{i,t-1} + + \alpha_2 \ln y_{i,t-1} + \alpha_3 \ln pop_{i,t-1} + e_{it}, \tag{11}$$

where j indexes the time lag and γ_j the coefficient on the lagged migration rate. The number of lags is chosen based on the significance level, i.e. we include all lags that turn out significant at least at the 10% level.

Table 1 presents the regression results. The first regression is estimates by pooled OLS, i.e. with a common constant ignoring country-specific and time-specific fixed effects. The positive and highly significant coefficient for the skill-selective immigration policy index suggests that skill-selective immigration policies are positively correlated with a higher migration rate. As expected, the GDP per capita level of the destination country affects the migration rate positively, while the population size variable exerts a negative effect.

Table 1 about here

The first regression is, however, biased and inconsistent if country-specific fixed effects affect the migration behavior of individuals. Regression (2) therefore includes country-specific fixed effects. The $F(14, 374)$-test statistic is 93.10, which rejects the Null of no country-specific fixed effects at the 1% level. The coefficient on the indicator for skill-selective immigration policies still appears positive and highly significant. Similarly, as in the first regression, we obtain the expected positive sign for the coefficient of the GDP variable and a negative sign for the coefficient of the population size variable. Note that also the scale of the coefficient for the skill-selective immigration policy variable is similar to the pooled model in the fixed-effects model. The R^2 suggests that our model explains about 40% of the within variation in our data.[6]

In regression (3) we consider not only country-specific but also time-specific fixed effects which absorb the variance of shocks common to all cross sections in a time-period. Including time-specific fixed effects is a safe way to control for omitted time-varying variables. The $F(24, 348)$-test statistic is 1.86, which rejects the Null of no time-specific fixed effects at the 1% level. Moreover, we use the Prais-Winston estimator which calculates panel-corrected standard errors, i.e. standard errors which correct for panel-specific heteroscedasticity and contemporaneous correlation. Beck/Katz (1995; 1996) provide Monte-Carlo evidence that the Prais-Winston estimator is preferable

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[6]Note that the R^2 statistic of the pooled and the fixed effects model are not comparable since the first refers to the overall variance in the data and the second to the within variance.

to Feasible Generalized Least Square (FGLS) estimators in fixed effects regressions since the latter tends to underestimate the standard errors in panels which have a similar group and time dimension compared to ours. The Wald-test statistics suggest that both panel-specific heteroscedasticity and contemporaneous correlation are present in our data. As can be seen in Table 1, the coefficient on the index for skill-selective immigration policies is still positive and significant at the 1% level. Again, we obtain the expected and significant results for the control variables. The R^2 statistic indicates that the model considering country-specific and time-specific fixed effects can explain 48% of the within variance in our data.

Finally, in the fourth regression, we have specified the model in dynamic form considering two lagged values of the dependent variable. We consider the first and the second lags of the dependent variable since further lags do not appear significant in our data. The model is estimated again with country- and time-specific fixed effects, which both turn out significant in our data. As before, we estimate the model using the Prais-Winston procedure which enables us to calculate panel-corrected standard errors. One might argue that simultaneous equation bias may affect the results of a dynamic model with fixed effects (Nickell, 1981). However, this bias is of order $1/T$, such that it is rather small in our case with 24 observations over time. Monte Carlo evidence suggests that standard OLS estimators considering fixed effects are preferable to Generalized Methods of Moments (GMM) estimators in a panel of our cross-sectional and time dimension (Judson/Owen, 1999).

As can be seen in column (4) of Table 1, the short-term coefficient on our skill-selective immigration policy variable is significant at the 5% level and has a value of 0.04. The long-term coefficient[7] has a value of 0.12 and is thus comparable to the values obtained for this variable in the static estimates of the model. For the control variables we obtain again the expected signs and significant coefficients.

All together, we find a robust correlation between skill-selective immigration policies and the scale of migration. This holds both for the static specification of the model which considers country- and time-specific fixed effects and for the dynamic model which considers lagged values of the dependent variable. Since we applied a log-log specification of the model, we can interpret the results as elasticities. With respect to the variable of interest, the immigration policy index increasing the index by one score increases the

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[7]The long-term coefficients of the dynamic model refer to the long-run equilibrium of the model which is achieved after the adjustment to economic shocks has been completed. The long-run coefficients of the model are calcuated as $\alpha_x^* = \alpha_x/(1 - \sum_{j=1}^{N} \gamma_j)$, where α_x^* denotes the long-term coefficient of an explanatory variable x and α_x the short-term coefficient.

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gross migration rate by between 9.0% and 11.4% in the static regressions.[8] In the dynamic regression, increasing the immigration policy index by one score increases the gross migration rate by 4% in the short run and by 12.4% in the long run. Altogether, the scale of the coefficient is remarkably stable across the different regressions. However, since the scores of the immigration policy index are a constructed measure the reader should be aware that the quantitative results have to be taken with a grain of salt.

Note that the fixed model identifies the parameters via the within variation in the data, i.e. via changes in the relevant policy variables holding time-invariant differences across countries constant which may affect the scale of migration in one way or another. Similarly, the models considering also time-fixed effects absorb the variance resulting from shocks common to all countries. Unobserved heterogeneity across countries or across time periods can thus not bias our results. Nevertheless, although our findings are consistent with the hypothesis derived from our theoretical model, we are reluctant to draw causal inference from our findings. A robust correlation between a high migration rate and lagged skill-selective immigration policies does not necessarily imply that skill-selective immigration policies are the cause of a higher immigration rate. As a robustness check we have also estimated the model with skill-selective immigration policies on the left-hand side and the migration rate on the right-hand side (i.e. reverse causality), but we obtained no significant results for the migration rate in this specification. We thus conclude that our findings are a strong hint that skill-selective immigration policies – other things being equal – trigger higher migration rather than the other way round.

3.3   Do we observe migration regulation contagion?

Consider now the second hypothesis, i.e. that a selective immigration policy in one country triggers other countries to follow the same approach. These spillover effects are of course more relevant for countries which are close substitutes as destination countries. As outlined above, we use two indicators for substitution links between countries. The first one is simply based on geographical proximity ($SPILLOVER1_{i,t-1}$). It assumes that immigration policies in one country are affected by immigration policies in countries with which it shares a common border or a common coast. The second indicator is based both on common border or common coast and on further criteria such as common language, culture and other links between countries, which may

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[8]As an example, if the gross migration rate initially is 0.10, then increasing the immigration policy index by one score increases the gross migration rate to between 0.109 and 0.114.

result in substitution relations ($SPILLOVER2_{i,t-1}$). Since spatial correlation might be an issue in our data, we consider the first lags of the explanatory variables in our regressions.

Empirically, we test the hypothesis that skill-selective immigration policies affect countries which are close substitutes as destinations for migrants by estimating the following model:

$$S_{it} = \beta_1 SPILLOVER_{i,t-1} + \eta' \mathbf{x}_{it} + u_{it}, \tag{12}$$

where S_{it} denotes as before the index for skill-selective immigration policies, $SPILLOVER_{i,t-1}$ a weighted index for the skill-selective immigration policies in countries which are considered as close substitutes as destinations for migrants, $\mathbf{x}_{it}$ a vector of control variables and η the related vector of coefficients. The error term u_{it} is specified as before as a two-way error component model with fixed country- and time-specific effects.

We thus explain skill-selective immigration policies by an index of skill-selective immigration policies in countries considered as close substitutes as destinations for migrants weighted by their population size and some control variables. Beyond the country- and time-specific fixed effects, we use the GDP per capita as a control variable, such that we estimate in static form as

$$S_{it} = \beta_1 SPILLOVER_{i,t-1} + \beta_2 \ln y_{i,t-1} + u_{it}. \tag{13}$$

and in dynamic form as

$$S_{it} = \sum_{j=1}^{N} \gamma_j\, S_{i,t-j} + \beta_1 SPILLOVER_{i,t-1} + \beta_2 \ln y_{i,t-1} + u_{it}. \tag{14}$$

We decided not to consider population size here since there is no obvious reason why population size should affect immigration policies.[9] We estimated all regressions using the first and the second measures for policy spillovers.

Table 2 about here

Table 2 reports the regression results for the first spillover indicator. We find a robust correlation between the regulation of immigration by human capital criteria in one country and similar policies in neighboring countries.

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[9]Population size turns out to be insignificant in all regressions explaining skill-selective immigration policies carried out by us.

In the pooled OLS regressions – i.e. regression (1) in Table 2 – the coefficient on the weighted index of skill-selective migration policies in neighboring countries is highly significant and at a value of 0.73 sizable. Note that we can interpret these coefficients only as linear correlations since both variables are indexes. In the regressions with country-specific fixed effects the coefficient on the index of skill-selective migration policies in substitute countries is still significant at the 1% level and large. The $F_{(14,373)}$-test statistics of 229.6 indicates that the country fixed effects are jointly significant at the 1% level. The regression diagnostics suggest that we with our model can explain 47% of the within variation of the data in regression.

In the regressions with country- and time-specific fixed effects the coefficient on the variable of interest, the index of skill-selective immigration policies, is still significant at the 1% level although the scale of the coefficient declines relative to the previous regressions. The $F_{(24,373)}$-test statistic is 1.86 in regression (2) which suggests that the time-fixed effects are jointly significant. Moreover, we have used the Prais-Winston estimator in regressions (3) which enables us to report panel-corrected standard errors which consider panel-specific heteroscedasticity and contemporaneous correlation in the error terms. The Wald-χ^2-statistics suggest that both are present in our data. The regression diagnostics indicate that we can explain about 50% of the within variation in regression (2).

Finally, we apply a dynamic specification of our model in regression (4). We consider only one lag of the dependent variable in the specification presented in Table 2 since further lags have turned out to be insignificant. The short-term coefficient is significant at the 5% level in regression (4). The long-run coefficient[10] is at 0.89 in regression (4) somewhat larger than in the static specifications of the model. The $F_{(24,333)}$-test statistic for the specification of the dynamic model with country- and time-specific fixed effects is 1.08, which cannot reject the Null of no time-specific fixed effects. We therefore present the model which only considers country-specific fixed effects. Again we report panel-corrected standard errors which correct for panel-specific heteroscedasticity and contemporaneous correlation. The regression diagnostics suggest that we can explain 91% of the within variation of the data with the dynamic model.

Table 3 about here

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[10]As before, the long-term coefficients of the dynamic model are calculated as $\beta_x^* = \beta_x/(1 - \sum_{j=1}^{N} \gamma_j)$, where β_x^* denotes the long-term coefficient of an explanatory variable x and β_x the short-term coefficient.

The findings we obtain on the basis of the second spillover index, i.e. the weighted index if skill-selective immigration policies in countries which are considered as close substitutes, are remarkably similar to our first findings, which are based on the simple criteria of geographical neighborhood. In the pooled OLS model, the coefficient for the variable of interest is at 0.99 somewhat larger than that for the first indicator and also significant at the 1% level. Again, the country-specific fixed effects turn out to be highly significant. The coefficient on the skill-selective immigration policies in countries which are close substitutes is, however, at 1.11 substantially larger than that for the variable which is based on the neighborhood criteria (0.62) in the fixed effects regressions. The time-specific fixed effects turn out to be insignificant both in the static and the dynamic specification of the model. The long-run coefficient in the dynamic specification of the model is at 1.66 again substantially larger than that of the other policy indicator (0.89). Note again that these results refer to linear correlations which are hard to interpret quantitatively since they depend on the scaling of the index variables.

Nevertheless, skill-selective immigration policies seem to be closely correlated across clusters of countries, which supports the second theoretical prediction of our model. In our broader definition of the substitution criteria, which beyond geographical proximity also covers language and cultural links, we find an even stronger correlation. This is true for all specifications of the model, i.e. under consideration of country- and time-specific fixed effects and in the dynamic specification of the model.

The high value of the correlation coefficient can be traced back to the selective immigration policies in neighboring countries such as Australia and New Zealand, and Canada and the US. The few European countries which pursue a selective immigration policy at present have introduced these policies only some years ago, hence the weight of those countries is not large in our sample. Our results therefore only have a preliminary character. We will get more insights on our hypothesis if more countries in Europe start to apply a selective immigration policy. This would also us allow to consider country-specific fixed effects and use the variance in the time dimension for the identification of our parameters. Nevertheless, the large coefficient and high significance of the parameter for selective immigration policies in neighboring countries is a strong hint that migration regulation contagion is an issue – at least in our sample.

4 Conclusion

In this paper we present a very simple model which analyzes the political economy of a selective immigration policy, which tries to screen migrants by human capital criteria. All results of our model depend on four simple assumptions: First, that all immigration policies only have incomplete information on the true type of immigrants and therefore must admit a certain share of migrants which are not welcome; second, that the population values migrants differently, i.e. the population expects that there are certain migrants which provide a net gain to the economy and the society, while others do not; third, that the population accepts the inflow of migrants from which it expects that they are a net burden to the economy and the society only to a certain critical extent and fourth, that the composition of the migrant population in one country affects the composition of the pool of migrants left for other countries. A selective immigration policy which regulates migration by human capital criteria yields – if we accept these four assumptions – a higher number of migrants compared to countries which do not opt for a selective immigration policy, and it reduces the number of migrants in countries which do not adopt a screening of migrants. Finally, within our theoretical framework "not adopting" a selective immigration policy is not a sensible strategy since countries can improve welfare if they opt for a screening of migrants.

Out of these simple theoretical considerations we derive two hypotheses, which can be tested empirically: First, countries with a selective immigration policy will admit more migrants and, second, the application of a selective immigration policy in one country increases the probability that other countries, particularly in the same geographical area, will adopt the same approach. We tested both propositions with the help of simple regression models in 15 immigration countries of the OECD during the period 1980 to 2005. Skill-selective immigration policies are measured by an index variable, which allocates between 1 and 10 scores for immigration policies. This variable has a relatively high variance, which helps to identify potential effects of immigration policies. Moreover, we apply to measures for skill-selective immigration policies in countries which are close substitutes as destinations for immigrants, one simply based on geographical proximity, and another one which in addition considers language and cultural similarities. Indeed, we find (i) that countries with a higher degree of skill-selective immigration policies measured by our policy index have a significantly higher net immigration rate, and (ii) that skill-selective immigration policies are closely correlated with countries which are substitutes as destinations for migrants. These findings turn out to be robust in different specifications of the regression models, i.e. under consideration of country- and time-specific fixed

effects and in static as well as in dynamic specifications – controlling for various other factors. Since the variance of immigration policies is relatively low in our sample, we suggest to interpret our results with caution. Even though we find correlations which support our theoretical predictions, and even though we include lagged policy variables, we still hesitate to draw conclusive causal inference from this evidence. In our view these findings are, however, a strong first hint that migration regulation contagion exists. Future research will have to re-address these issues.

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Tables

Table 1: Explaining the immigration rate

	(1)		(2)		(3)		(4)	
	standard errors in parentheses							
$\ln m_{i,t-1}$							0.862	***
							(0.076)	
$\ln m_{i,t-2}$							-0.185	**
							(0.076)	
$S_{i,t-1}$	0.106	***	0.114	***	0.090	***	0.040	**
	(0.013)		(0.025)		(0.022)		(0.020)	
$\ln y_{i,t-1}$	0.860	***	1.937	***	1.127	***	0.400	**
	(0.119)		(0.156)		(0.239)		(0.165)	
$\ln pop_{i,t-1}$	-0.347	***	-0.367	***	-3.977	***	-1.493	***
	(0.019)		(0.057)		(0.510)		(0.448)	
constant	-4.113	***	16.745	***	14.439	***	5.754	***
	(1.629)		(4.034)		(2.168)		(2.171)	
observations	390		390		390		375	
country FE	no		yes		yes		yes	
time FE	no		no		yes		yes	
R^2	0.54		0.39		0.48		0.78	

Notes: ***,**,* denote significance at the 1%, 5% and 10% level.The dependent variable is the log of the gross immigration rate, $\ln m_{it}$, in all regressions. Regression (1) is estimated by pooled OLS. Regression (2) is estimated with country-specific fixed effects. The $F(14, 357)$-test statistic is 93.10, which rejects the Null of no country-specific fixed effects at the 1% level. Regression (3) is estimated with country- and time-specific fixed effects. The $F(23, 333)$-test statistic is 2.23, which rejects the Null of no time-specific effects at the 1% level. Regression (4) is estimated with country- and time-specific fixed effects. The $F(23, 317)$-test statistic is 1.62, which rejects the Null of no time-specific fixed effects at the 5% level. We report panel-corrected standard errors assuming panel specific heteroscedasticity and contemporaneous correlation of the standard errors in regressions (4) and (5). In regression (4), the Wald-$\chi^2(41)$-statistic for the model assuming independent errors is 4,114, for the model assuming heteroscedastic errors it is 7,202, and for the model assuming heteroscedastic errors and contemporaneous correlation it is 164,868. In regression (5), the Wald-$\chi^2(42)$-statistic for the model assuming independent errors is 9,577, for the model assuming heteroscedastic errors it is 14,618, and for the model assuming heteroscedastic and contemporaneous errors it is 66,215.

Table 2: Explaining skill-selective immigration policies (1)

	(1)		(2)		(3)		(4)	
	standard errors in parentheses							
$S_{i,t-1}$							0.929	***
							(0.034)	
$SPILLOVER1_{i,t-1}$	0.733	***	0.616	***	0.494	***	0.063	**
	(0.038)		(0.051)		(0.054)		(0.027)	
$\ln y_{it}$	-1.194	***	0.623	***	-1.859	***	0.128	**
	(0.332)		(0.204)		(0.95)		(0.057)	
constant	13.177	***	-4.997	***	21.890	***	-1.394	***
	(3.396)		(-2.006)		(5.044)		(0.518)	
observations	390		390		390		375	
country FE	no		yes		yes		yes	
time FE	no		no		yes		no	
R^2	0.50		0.47		0.50		0.91	

Notes: ***,**,* denote significance at the 1%, 5% and 10% level. The dependent variable is the index of skill-selective immigration policies, S_{it}, in all regressions. Regression (1) is estimated by pooled OLS.– Regression (2) is estimated with country-specific fixed effects. The $F(14, 373)$ -test statistic is 229.61***, which rejects the Null of no country-specific fixed effects at the 1% significance level. Regression (3) is estimated with country- and time-specific fixed effects. The $F(24, 348)$-test statistic is 1.86***, suggesting that the time-specific fixed effects are significant at the 1% level. Regression (4) is estimated with country-specific fixed effects but no time-specific fixed effects since the $F(24, 333)$-test statistic of 1.08 indicates that time-specific fixed effects are not significant here. We report panel-corrected standard errors assuming panel-specific heteroscedasticity and contemporaneous correlation of the standard errors in regressions (3) and (4). In regression (3), the Wald-$\chi^2(41)$-statistic for the model assuming independent errors is 8,071, for the model assuming heteroscedastic errors it is 12,695, and for the model assuming heteroscedastic errors and contemporaneous correlation it is 1,25e+06. In regression (4), the Wald-$\chi^2(17)$-statistic for the model assuming independent errors is 45,732, for the model assuming heteroscedastic errors it is 67,977, and for the model assuming heteroscedastic and contemporaneous errors it is 7.15e+06.

Table 3: Explaining skill-selective immigration policies (2)

	(1)		(2)		(3)		(4)	
	standard errors in parentheses							
$\ln S_{i,t-1}$							0.896	***
							(0.034)	
$SPILLOVER2_{i,t-1}$	0.989	***	1.112	***	0.985	***	0.173	***
	(0.029)		(0.068)		(0.077)		(0.045)	
$\ln y_{it}$	-0.045		-0.011		1.127	***	0.018	
	(0.237)		(0.183)		(0.165)		(0.058)	
constant	0.474		-0.262		16.140	***	-0.705	
	(2.495)		(1.871)		(2.377)		(0.500)	
observations	390		390		390		375	
country FE	no		yes		yes		yes	
time FE	no		no		yes		no	
R^2	0.75		0.55		0.57		0.91	

Notes: ***,**,* denote significance at the 1%, 5% and 10% level. The dependent variable is the index of skill-selective immigration policies, S_{it}, in all regressions. Regression (1) is estimated by pooled OLS. Regression (2) is estimated with country-specific fixed effects. The $F(14, 373)$ -test statistic is 130.71*** which rejects the Null of no country-specific effects at the 1% level. Regression (3) is estimated with country- and time-specific fixed effects. The $F(24, 348)$-test statistic in regression (3) is 0.87, suggesting that the time-specific fixed effects are not significant. Regression (4) is estimated with country-specific fixed effects but no time-specific fixed effects since the $F(24, 333)$-test statistic of 0.87 indicates that time-specific fixed effects are not significant. We report panel-corrected standard errors assuming panel-specific heteroscedasticity and contemporaneous correlation of the standard errors in regressions (3) and (4). In regression (3), the Wald-$\chi^2(41)$-statistic for the model assuming independent errors is 9,505, for the model assuming heteroscedastic errors it is 18,669, and for the model assuming heteroscedastic errors and contemporaneous correlation it is 790,328. In regression (4), the Wald-$\chi^2(17)$-statistic for the model assuming independent errors is 47,432, for the model assuming heteroscedastic errors it is 57,159, and for the model assuming heteroscedastic and contemporaneous errors it is 1.81e+06.

Table A1: Index of skill-selective immigration policies

	AUS	BE	CA	DK	FR	DE	JAP	LX	NL	NOR	NZL	SWE	SWI	UK	USA
1980	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1981	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1982	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1983	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1984	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1985	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1986	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1987	5	2	6	1	1	1	2	1	1	2	5	2	2	3	5
1988	5	2	7	1	1	1	2	1	1	1	5	2	2	4	5
1989	6	2	7	1	1	1	3	1	1	1	5	2	2	4	5
1990	6	2	7	1	1	1	3	1	1	1	5	2	2	4	5
1991	6	2	7	1	1	1	3	1	1	1	6	2	2	4	6
1992	6	2	7	1	1	1	3	1	1	1	6	2	2	5	6
1993	6	2	8	1	1	1	4	1	1	1	6	2	2	5	6
1994	6	2	8	1	1	1	4	1	1	1	6	2	2	5	6
1995	6	2	8	1	1	1	4	1	1	1	7	2	2	5	6
1996	6	2	8	1	1	1	4	1	1	1	7	2	2	5	6
1997	6	2	9	1	1	1	4	1	1	1	7	2	2	5	6
1998	6	2	9	1	1	1	4	1	1	1	8	2	2	5	6
1999	7	2	9	1	1	1	5	1	1	1	8	2	2	5	6
2000	7	2	9	1	1	2	5	1	1	1	8	2	2	5	6
2001	7	2	10	1	1	2	6	1	1	1	8	2	3	5	6
2002	7	2	10	1	1	2	6	1	1	2	8	2	3	5	6
2003	7	2	10	1	1	2	6	1	1	2	8	2	3	5	6
2004	7	2	10	1	1	2	6	1	2	2	8	2	3	5	6
2005	8	2	10	1	1	2	6	1	2	2	8	2	3	5	6

Sources: Own calculations based on Mayda/Patel (2004), Ortega/Peri (2009),
Bertoli et al. (2009) and the New Zealand Home Office (2010).

Table A2: Countries with close substitution relations

destination country	neighborhood criteria	broader substitution criteria
AUS	NZL, JAP	CAN, NZL, JAP, UK, USA
BE	DE, LX, FR, NL	DE, LX, FR, NL
CAN	USA	AUS, NZL, UK, USA
DK	DE, NOR SWE	DE, NL, NOR, SWE
FR	BE, DE, LX, SWI, UK	BE, DE, LX, SWI, UK
DE	BE, DK, LX, NL, SWI	BE, DK, LX, NL, SWI
JAP	AUS, NZL	AUS, NZL
LX	BE, DE, FR, NL	BE, DE, FR, NL
NL	BE, DE, LX	BE, DE, DK, LX
NOR	DK, SWE	DK, SWE
NZL	AUS, JAP	AUS, CAN, JAP, UK, USA
SWE	DK, NOR	DK, NOR
SWI	DE, FR	DE, FR
UK	BE, FR	AUS, BE, CAN, FR, NZL, USA
USA	CAN	AUS, CAN, NZL, UK

Table A3: Descriptive statistics

Variable	Obs.	Mean	Std.Dev.	Min	Max
$\ln m_{it}$	390	1.604	0.838	-0.393	3.361
S_{it}	390	3.187	2.414	1	10
$SPILLOVER1_{it}$	390	2.958	2.297	1	10
$SPILLOVER2_{it}$	390	3.198	2.117	1	8
$\ln y_{it}$	390	10.188	0.259	9.642	11.173
$\ln pop_{it}$	390	9.687	1.601	5.898	12.597

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