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The Effect of Visa Policies on International Migration Dynamics

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Abstract

The effectiveness of migration policies has been widely contested, yet evidence has remained inconclusive due to conceptual and methodological limitations. Moreover, a general 'receiving country bias' in migration research, causes prior studies to focus on the effects of policies on inflows and fail to assess the simultaneous effect of policies on outflows. From a theoretical viewpoint, immigration restrictions reduce both inflows and outflows and, hence, overall circulation, which renders their effect on net migration theoretically ambiguous. To test this hypothesis, and using unique migration and visa datasets compiled by the DEMIG project (covering 38 countries over the 1973–2012 period), this paper assesses the short and long-term effects of travel visa policies on bilateral (country-to-country) inflow and outflow dynamics. The results suggest that travel visa policies significantly decrease inflows, but that this effect is to a large extent undermined by decreasing outflows of the same migrant groups. This seems to confirm that migration restrictions decrease circulation and tend to encourage long-term settlement, and thereby significantly reduce the responsiveness of migration to economic fluctuations in destination and origin societies.

Keywords: international migration, immigration policies, policy effects, migration determinants, circular migration

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Contents

1 Introduction

This paper examines the effect of travel visa requirements on bilateral immigration *and* emigration flows, using new databases with an unprecedented coverage in terms of countries and years. Focussing on the effects policies have on overall patterns of circularity, this paper aims to add much needed empirical evidence to the heated debate on the effectiveness of immigration policies.

Political salience of immigration has fast risen since the 1970s, despite the relative number of international migrants remaining remarkably stable: between 2.5 and 3 percent of the world population (Czaika and de Haas 2014). While this is a global phenomenon, it seems to particularly be the case for Western European countries, which might be partly explained by the transformation of Western Europe from an emigration region (colonisers, settlers, labour migrants and refugees migrating to the rest of the world) into a major global migration destination since the end of WWII. This geographical turnaround of global migration patterns has confronted Europe with unprecedented and largely unplanned immigration of increasingly diverse groups of migrants from non-European regions. Anglo-Saxon settler countries – United States, Canada, Australia and New Zealand – have also seen declining migration from Europe and increasing migration from non-European, particularly Asian and Latin American, countries.

Following the 1973 Oil Crisis and suspension of guest-worker programmes, Western European countries such as Germany, France, Belgium and the Netherlands experienced – generally unexpected and unintended – permanent settlement of large numbers of former 'guest workers' and other temporary immigrants, followed by large-scale family immigration (Castles and Kosack 1973; Entzinger 1985). The United States has also experienced an increase of largely spontaneous and often irregular migration of Mexican workers since the end of the Bracero recruitment agreement in 1964 (Cornelius et al. 2004; Durand, Massey and Zenteno 2001). In both Europe and the United States, ongoing demand for low-skilled labour in combination with growing efforts by governments to curtail such immigration, seems to have increased family and irregular migration. Also in the wealthy economies of Asia such as Japan, Korea, Taiwan, Singapore, and Malaysia, in the Arab Gulf States such as Saudi Arabia, Kuwait and the United Arab Emirates (UAE), and African migration magnets such as Côte d'Ivoire, Libya, Gabon, and South Africa, immigration, integration and settlement have become issues of increased political salience (cf. Castles, de Haas and Miller 2014).

Regular and irregular migration, and large-scale settlement of migrants in *apparent* defiance of border controls and immigration restrictions has sparked often heated political and academic debates on the effectiveness of immigration policies. Several scholars have argued that efforts of states to restrict immigration have often failed (Bhagwati 2003; Castles 2004; Cornelius et al. 2004; Düvell 2005). They argue that international migration is mainly driven by structural factors such as labour market demand, inequalities in wealth between rich and poor countries, and conflicts in origin countries. Migration policies will therefore only have limited effects. Furthermore, once a certain number of migrants have settled in the destination, social networks and the so-called 'migration industry' (recruiters, lawyers, smugglers and other intermediaries) tend to facilitate migration by lowering the costs and risks of moving (Castles, de Haas and Miller 2014; Krissman 2005; Massey 1990). Rather than affecting overall volumes of inflows, immigration restrictions would primarily change the *ways* in which people migrate. The overall migration volumes and long term trends they argue, are rather driven by broader economic, demographic and political processes in origin and destination countries.

Other scholars have argued that on the whole, state policies have been largely effective (Brochmann and Hammar 1999; Strikwerda 1999). Despite extensive media and academic attention to irregular and other forms of officially 'unwanted' migration, these scholars argue that the majority of

migrants abide by the rules and therefore the bureaucratic systems that regulate migration are largely effective, albeit not perfect. This seems to be partly confirmed by a growing number of quantitative studies indicating that immigration restrictions have a significant effect on inflows (Beine, Docquier and Özden 2011; Hatton 2005; Mayda 2010; Ortega and Peri 2013). However, given these restrictions should have at least *some* effect on inflows, the more appropriate question is *how big* these policy effects are compared to other migration determinants. For instance, Hatton (2009) investigated the determinants of asylum migration and concluded that the decline of asylum applications in the industrialised countries of Europe, North America and Australasia should largely be attributed to the decline of violence and terror in origin countries, and that more restrictive policies account for only about a third of the decline in applications since 2001.

Part of the controversy surrounding this issue seems spurious due to fuzzy definitions of policy effectiveness. These partly stem from confusion between policy discourses, policies on paper, policy implementation, and policy impacts. Three 'policy gaps' can be distinguished using these distinctions: the *discursive gap* (the discrepancy between public discourse and policies on paper); the *implementation gap* (the disparity between policies on paper and their implementation); and the *efficacy gap* (the extent to which implemented policies affect migration) (Czaika and de Haas 2013). In brief, policy making is often more nuanced than politicians' discourse might suggest. Although implemented policies seem to be the most appropriate yardstick to measure policy effectiveness, the generally 'tougher' discourses are often used in practice, which can lead to an overestimation of 'policy failure' (Czaika and de Haas 2013).

This leads to some methodological considerations. Firstly, it is questionable to what extent it is useful to talk in levels of 'general restrictiveness'. Immigration policies are usually about *selection* rather than controlling the total volume of migrants, despite politicians' discourses possibly suggesting the latter (de Haas, Natter and Vezzoli forthcoming). Immigration policies are typically a 'mixed bag' of various, incoherent and potentially contradictory laws, measures and regulations that target different migrant categories in different ways. For instance, while there has been a trend in recent decades of Western countries gradually liberalising policies towards high-skilled workers, students, and family migrants (cf. Bonjour 2011), this has coincided with increasing restrictions towards asylum seekers and low-skilled workers.[1]

Secondly, the gap between migration discourses and actual policies in the forms of laws, rules, measures, and practices gives reason to question the common assumption that immigration policies have generally become more restrictive in recent decades. For example, 'Fortress Europe' may be an apt characterisation of policies towards asylum seekers and refugees (Hatton 2004), but seems inappropriate for immigration policies of EU countries as a whole. There is also considerable variation through time, meaning there has not been a unilateral linear trend towards more or less restrictiveness (cf. de Haas, Natter and Vezzoli forthcoming; Ortega and Peri 2009).

From this, we can draw three methodological inferences. First, that appropriate measurement of the effects of migration policies requires consideration of implemented policies and *concrete policy instruments*. The second is the need to assess not only whether a particular migration policy has a significant effect, but also what the relative magnitude of this effect is *compared to other migration determinants* in origin and destination countries. Third, empirical studies on policy effectiveness should not only focus on the immediate effects of policy measures on the inflow of the migrant targeted by the

[1] Several scholars have argued that states, and liberal democracies in particular, face embedded constraints, in the form of constitutional norms and principles, which act to 'constrain the power and autonomy of states both in their treatment of individual migrants and in their relation to other states' (Hollifield 1992).

specific policy, but also consider the long-term effects as well as the 'knock-on' effects such measures can have on (other) migration flows, which may partly or entirely undermine the intended effects.

In this context, de Haas (2011) argued that it is useful to distinguish the effect of migration policies on the volume of inflows; spatial orientation of migration; composition of migration (legal channels and migrant characteristics); timing of migration; and reverse flows. Based on this, de Haas (2011) hypothesised four 'substitution effects' that can impact the effectiveness of immigration restrictions: (1) *spatial substitution* through the diversion of migration to other countries; (2) *categorical substitution* through a re-orientation towards other legal or illegal channels (cf. Czaika and Hobolth 2014); (3) *inter-temporal substitution* affecting the timing of migration such as 'now or never migration' in the expectation of future tightening of policies (see also Peach 1968; van Amersfoort 2011); and (4) *reverse flow substitution* if immigration restrictions also reduce return migration making the effect of restrictions on net migration ambiguous (de Haas 2011). Such substitution effects also show the need to consider the 'externalities' of *specific* policy measures that may go beyond short-term effects on *targeted* (e.g., asylum or family) migration categories by considering short *and* long-term effects of specific migration policies on other immigration and emigration flows that are not explicitly targeted by the policies. In other words, it is only by looking at policy effects on overall migration dynamics that we can obtain more fundamental and comprehensive insights into the role of policies in migration processes.

Prior studies on the macro-level determinants of international migration have yielded valuable insight into (the predominance of) economic migration determinants (cf. Hilderink et al. 2001; Jennissen 2003), but suffer from a number of methodological limitations. Many studies are limited by a 'single comparative design', meaning they explain variation in total immigration to a range of destination countries (Hilderink et al. 2001; Jennissen 2003; Zoubanov 2003; Zoubanov 2004). This creates a bias towards destination country-specific variables by ignoring the relevance of origin-country contexts. The few studies that differentiate several origin groups in one or separate destinations (cf. Faini and Venturini 1994; Rotte, Vogler and Zimmermann 1997) often suffer from the reverse problem. Furthermore, migration policy variables are rarely included in models or poorly operationalised, although a few recent studies have begun to include policy variables (Mayda 2010; Ortega and Peri 2013). Thielemann (2004) and Hatton (2009) are more focused studies assessing the effect of asylum policies on asylum applications, and consider a limited number of countries and shorter time periods. Most existing studies also focus on rather limited time periods, where a study of migration dynamics and the short- and long-term effect of policies would require data spanning several decades.

From a theoretical and empirical perspective, more comprehensive assessments of migration determinants including policy effects can only be achieved through a 'double comparative approach'; a simultaneous analysis of the migration of multiple origin groups to and from multiple destination countries.[2] This requires annual bilateral (country-to-country) migration data covering several decades to enable simultaneous assessment of the effect of origin and destination country migration determinants; as well as the inclusion of appropriate policy variables in empirical estimates. Recently, a number of innovative empirical studies have implemented such an approach to test the effects of migration policy variables on bilateral migration flows (Mayda 2010; Ortega and Peri 2013).

Yet through their focus on the impact of immigration policies on inflows, they do not assess the effect of immigration policies on flows in the opposite direction. This is problematic as the

[2] A similar approach was used by Van Tubergen et al. (2004) in studying the economic incorporation of immigrants in 18 Western countries. See: van Tubergen, F., I. Maas, and H. Flap. 2004. "The economic incorporation of immigrants in 18 western societies: Origin, destination, and community effects" *American Sociological Review* 69(5): 704-27.

effectiveness of policy restrictions can be undermined by 'reverse flow substitution': reducing inflows from particular origin countries and outflows to the same countries, thereby decreasing overall circulation. In other words, immigration restrictions may discourage migrants to return and hence push them into permanent settlement. This argument has been made in the context of the guest-worker policies implemented West-European countries (Entzinger 1985;Massey and Pren 2012), but has never been systematically tested in a comparative and longitudinal setting. We can therefore hypothesise that more liberal migration policies increase the overall responsiveness or 'elasticity' of migration to migration determinants such as economic growth and labour demand. Conversely, we may expect that a more liberal policy may not only increase inflows but also outflows. It is crucial to address such reverse flows effects in order to understand how policies affect migration *dynamics* and *circulation* over time. In brief, the danger of exclusively focusing on the inflow targeted by the policy is to over-estimate its net effect.

Another shortcoming of prior work on migration policy effectiveness is the implicit assumption that the effects of a change in migration policy in a more liberal direction 'mirror' the effects of a policy change in an opposite, more restrictive direction. There is reason however to hypothesise that policy restrictions and liberalisations have asymmetrical effects. While the lifting of a barrier may have more immediate effects, case studies suggest that the effects of restrictions may be smaller or may take more time to materialise. Especially as migrant networks facilitate the continuation of migration across legally closed borders, particularly through an increased reliance on family and irregular migration (cf. Böcker 1994; Massey and Pren 2012).

Many current policy interventions aim at stimulating circular migration through restricting migrants' access to rights and motivating return. Such policies however, may be based on flawed assumptions on the role of policies in migration processes. In fact, there is reason to hypothesise that restrictive immigration policies may actually achieve the opposite by reducing return and pushing migrants into permanent settlement. Measuring how policies affect bilateral inflows and outflows is therefore crucial for improving our understanding of the role of policies in migration processes and to provide policy-making with a more solid evidence basis.

2 Methodology

To fill these conceptual and empirical gaps, this paper assesses the short- and long-term effects of travel visa policy regimes on bilateral (country-to-country) immigration and emigration dynamics. The analysis draws on new databases that were collected as part of the DEMIG project.[3] Several pragmatic and analytical considerations underpinned our choice to use bilateral travel visa travel requirements to analyse policy effect. The first, pragmatic reason is the historical and geographical coverage of travel visa data. It is the only policy instrument for which a long data series for all countries covering the 1973–2012 period could be compiled. Migration policies are usually measured through the construction of migration policy indices based on an extensive review of changes in migration policies (cf. Czaika and de Haas 2013; de Haas, Natter and Vezzoli 2014). Notwithstanding the considerable potential of such indices in gaining insights into the nature and evolution of migration policies (cf. Mayda 2010; Ortega and Peri 2013), their main limitation is that they are general measures of overall restrictiveness and do not specify for particular origin countries. Visa data has the unique feature of being a *bilateral* (country-to-country) policy instrument, which is required to perform a double comparative analysis to

[3] See www.migrationdeterminants.eu.

test the effect of policy on flows of multiple origin groups and to and from a range of destination countries.

Second, travel visa data is available and reliable, as it is safe to assume that they are actually implemented. Our data originates from the Travel Information Manuals of the International Air Transport Association (IATA), a reliable source, published to provide airline companies with accurate, up-to-date information on actual policies so as to avoid them being confronted with carrier sanctions and other penalties by immigration authorities. Although the costs and difficulty of visa acquisition vary greatly, the introduction or lifting of a visa requirement constitutes a major policy change with real consequences. It would be ideal if we could quantify the difficulty of visa acquisition (for instance through measuring costs, waiting times or rejection rates), but such data would be very difficult to obtain and would significantly reduce the coverage in terms of years and countries.

Although travel visa regulations are meant for temporary visitors such as tourists or business visitors, it is undeniable that since the 1970s they have played an increasingly important role in preventing people from certain countries entering a national territory. For instance, in the 1980s and 90s Western European countries introduced travel visas for 'guest-worker' countries such as Turkey and Morocco in an obvious attempt to prevent people from joining their family in Europe. Many former 'guest-workers' entered formally as tourists with their passports, attaining work and residence permits after they obtained work.

Past research indicates the majority of migrants without residence documents entered regularly (cf. Düvell 2005; Schoorl et al. 2000). Once migrants stay longer than their formal tourist status allows (usually between three to six months) their stay becomes unauthorised. Once entered, migrants can find work (sometimes even legally), find shelter with family or friends, form new social and romantic relationships, and get practical and legal support, which all facilitate onward stay and settlement. The long-term outcome is that many unauthorised migrants eventually obtain residency through regularisation campaigns or 'amnesties' (cf. Fakiolas 2003; Levinson 2005; Zincone 2006). The recent history of immigration to Western Europe and the US has partly been one of regular entry, unauthorised overstay, and eventual regularisation. States have increasingly used visas as an instrument of up-front prevention of people arriving at all, which seems particularly effective for distant origin countries. Complementary to travel visas, destination countries have introduced carrier sanctions in the 1980s and 90s to prevent people without visas from boarding airplanes in the first place. States have not hidden that the combination of visas and carrier sanctions was an instrument to prevent people from entering and asking asylum (Neumayer 2006).

More generally, visa requirements can be seen as reflecting which migrants are 'desirable' in terms of their nationality and assumed migration motives, skills and socio-cultural background of migrants from those countries. There is plausibly a relation between travel visa regimes and other immigration restrictions towards particular nationalities. This also works in the other direction, with the lifting of visa requirements generally reflecting political *rapprochement* and policies in which citizens of those countries are increasingly welcomed. The introduction of visa requirements for citizens from some countries often accompanies the lifting for others. For instance, as most EU countries started to remove their internal boundaries with the Schengen agreement in 1985 and its full implementation in 1995, they became increasingly concerned about controlling external borders. This coincided with the introduction of visa requirements for an increasing number of non-European, countries. For instance, in 1990 and 1991 Italy and Spain introduced visa requirements for citizens of important origin countries such as Algeria, Morocco, Senegal, Tunisia and Turkey as part of a move to conform regulations to 'European community norms' (FocusMigration 2012: 3; OECD 1992: 77).

Governments do not generally conceal that they view visas as instruments to curb migration, in particular of asylum seekers. In 1992, Sweden introduced travel visas for Serbians, Montenegrins and Macedonians due to the increase in refugees of non-Bosnians from the former Yugoslavia (OECD 1994: 96). A year later, official Swedish government sources reported that 'the recent large inflow of Bosnians led the government to introduce a visa regime in June 1993 for Bosnia-Herzegovina' (OECD 1995: 121), and claimed this had a deterrent effect. In 2009, Canada introduced travel visas for Mexican citizens in response to tripling in refugee claims between 2005 and 2008 (GovernmentOfCanada 2009a), and for Czech citizens in response to increased refugee claims, particularly by Romas after visas were lifted in 2007 (GovernmentOfCanada 2009b). In August 1989, Turkey introduced a visa requirement for Bulgarian citizens in reaction to the inflow of over 320,000 Bulgarians of Turkish origin and Muslim religion since May 1989 (OECD 1990: 54; OECD 1992: 82). Turkish government sources claim that the reintroduction of visas for Bulgarians helped to slow down immigration (OECD 1992: 82).

These examples show that travel visas should be seen as a central component of the immigration policy toolbox. It is seen as an efficient 'upfront' way of preventing migrants from entering in the first place. It is a particularly attractive instrument for states, as visas restrictions can generally be implemented rather quickly through directives, decrees or other administrative measures, and generally do not require cumbersome legal changes.

2.1 Data

We use information on immigration and emigration *flows* drawing on the DEMIG C2C ('country-to-country') migration flow database, which contains annual bilateral flow data for 34 reporting countries (see Vezzoli, Villares-Varela and de Haas 2014). Flow data for four additional countries from UNDP (2010) was used. To our knowledge, this has yielded the largest bilateral migration flow database compiled to date. Bilateral immigration and emigration data is reported for 38 countries (no emigration flow data is reported by Canada, France or Moldova) on bilateral inflow from (and outflows to) approximately 190 countries between 1973 and 2011. Our migration flow data are based on a *country-of-citizenship* definition. This is a largely unambiguous criterion, and also the most appropriate one, because visa regulations are based on citizenship. The only limitation arises in the case of individuals holding dual citizenship.

In addition to migration inflows and outflows, we estimate the total migration circulation or 'turnover' (i.e., inflow plus outflow) and net flows (i.e., inflow minus outflow). This enables us to study the effect of travel visa policies on the volume of migration on inflows and outflows of citizens from targeted origin countries, as well as the effect of travel visa requirements on the overall rate circulation within bilateral dyads and net migration. Information on visa requirements was drawn from the IATA Travel Information Manuals,[4] and entered manually into a database, constituting a global panel of bilateral visa data for the period 1973–2012. The DEMIG VISA database contains information on country of visa issuance, nationality of the traveller, and whether a visa was require in a particular year. The binary visa policy was coded zero if no visa is required and one if a visa permit is required.[5] If no visa is required for *entering* the country it is considered an exemption, regardless of the period people are allowed to stay. It is not considered an exemption if holding residence permits in the country of visa

[4] The IATA travel manuals are released on a monthly basis. We have selected all January manuals from 1973–2012. The visa and exit requirements tracked only apply to travel visa/exit, excluding diplomatic or official passports and travel for business purposes (e.g. social visits, tourism, etc.).

[5] The original database includes individuals with the nationality of a 'blacklisted' country not allowed to travel to this country of destination. We have (re-)coded the visa policy variable for these dyads to one (instead of two).

issuance or other countries. We also ignore diplomatic passports or other exemptions that are not for regular tourist purposes.

Table 1 shows that about 35 percent of all 90,000 dyad-year observations covered by our bilateral migration database were visa-free, with visas required for the remaining corridors. A relatively low number (119) of dyad-year combinations concerned 'blacklisted' corridors, in which citizens could not even apply for a travel visa. We added these cases to the set of visa-constrained corridors. The data also shows that travel visa regimes are relatively stable. Over the 1973–2012 period, the 38 destination countries in our dataset introduced visas for 547 bilateral corridors and waived visa requirements for 612.

Table 1 Bilateral visa policy (38 visa issuing countries, 1973–2012)

	Frequency	Percentage
No visa required (no. of dyad-years)	31,615	35.01
Visa required (no. of dyad-years)	58,559	64.85
Blacklisted (no. of dyad-years)	119	0.13
Total (no. of dyad-years)	90293	100.00
Visa introductions (no. of incidences)	547	
Visa removals (no. of incidences)	612	

We included a number of control variables in our empirical estimates. Income data on *GDP per capita* and year-by-year *GDP per capita growth* are drawn come from the World Development Indicators (World Bank 2012). GDP per capita is gross domestic product divided by mid-year population. GDP is the sum of gross value added by all resident producers in the economy plus any product taxes, minus any subsidies not included in the value of the products. Data are in constant 2005 US dollars.

Because political circumstances are also likely to affect migration, we use Freedom House's cross-comparative assessment of global *political rights* and *civil liberties*. Since 1972, Freedom House publishes survey ratings and narrative reports on 195 countries and 14 related and disputed territories to monitor trends in democracy and track improvements and setbacks in freedom worldwide. On a one to seven scale high scores represent relatively few political rights and civil liberties, respectively.

We also included a few variables that proxy the nature of bilateral ties between countries. Data on *common currency* between the two countries of a dyad is based on information provided by Head et al (2008). Bilateral *distances* and information on *colonial ties* come both from the CEPII distance database.[6] Estimates of *population size* originate from UNPD population statistics.[7] We assume that common currency, distance, colonial ties and population size significantly affect the volumes of migration, and these therefore needed to be built in as controls. Relevant descriptive statistics on all variables are reported in the appendix (Table A-1).

2.2 Estimation strategy

[6] Distances are based on the population-weighted great circle formula that measures distance between large cities of the two countries (see www.cepii.fr/anglaisgraph/bdd/distances.htm).
[7] UNPD statistics: http://esa.un.org/unpd/wpp/index.htm.

In order to identify the effect of travel visa policy on various migration flow volumes and directions, we estimate the following migration model:

$$M_{ijt} = \beta_1 + \beta_2 policy_{ijt} + \beta_3 x_{ijt} + \beta_4 x_{it} + \beta_5 x_{jt} + \beta_6 D_j + \beta_7 O_i + \beta_8 T_t + u_{ijt}.$$

M_{ijt} captures the respective migration flow within an ij-dyad at time *t*, $policy_{ijt}$ indicating a time-varying binary variable on visa requirement. *X* captures a set of time-variant and dyad-, origin, and destination-specific control variables. *D* and *O* capture unobserved destination and origin heterogeneity. Finally, T controls for general time trends in international migration flows.

We assume $E(u) = 0$, and $cov\ (X, u) = 0$, but the visa $policy_{ijt}$ variable might be correlated with the error term u_{ijt}. This potential endogeneity can result from either reverse causality (when changes in migration flows lead to changes in visa policy), or omitted variables (if there are unobserved factors that simultaneously affect visa policy *and* migration flows) that can make OLS (ordinary least squares) estimates inconsistent.

To investigate this further, we performed a Hausman-Wu endogeneity test with regard to the visa policy variable. The test assumes that under the null hypothesis, both OLS and instrumental variable (IV) estimators are consistent, whereas, under the alternative hypothesis, the OLS estimator is not consistent, while IV (2SLS) remains consistent. Therefore, we should expect that under the alternative hypothesis, the two estimates are significantly different. Applied to our data, the Hausman-Wu endogeneity test rejects the null hypothesis ($H0$: $cov(P, \varepsilon) = 0$) on a one percent level ($p = 0.007$). This shows the need for an IV estimation method.

Our IV on the affinity of voting behaviour of UN member states in the UN General Assembly captures the unobserved heterogeneity in the quality of bilateral relations that would otherwise be attributed to visa policies. The data for the variable *UN voting affinity* scores stems from the United Nations General Assembly Voting database (Strezhnev and Voeten 2013). UN affinity scores uses binary data on approval or disapproval of an issue and range from –1 (least similar interests) to 1 (most similar interests).[8] This variable satisfies the key condition that a valid IV variable should affect the central explanatory variable (*visa requirement*) but not the dependent variables (*immigration* and *emigration*). We assume that for our binary and time-varying instrument z_{ijt} the exclusion restriction ($cov(z, u) \neq 0$) holds, and that it is also valid, that is, correlated with the endogenous explanatory variable (*visa requirement*).

At the first stage, our 2SLS regression analysis estimates visa policy on a basis of our set of exogenous explanatory variables and the additional instrument z_{ijt}. Estimates reported in Table 2 show that our instrument *UN voting affinity* is highly significant ($p = 0.000$), which makes us confident that predicted values of the visa variable are uncorrelated with the error term in our subsequent migration model (1). Additional to the policy values predicted at the first stage, our IV regression includes bilateral, origin- and destination-specific and time-variant control variables. In order to capture some of the unobservable origin and destination heterogeneity, we further include destination dummy variables that, for instance, capture different definitions of a migrant (different register systems), additional to various origin and time variables.

[8] The calculation of UN affinity scores is based on the S algorithm as $1 - 2 \cdot d/d_{max}$, where d is the sum of metric distances between votes by dyad members in a given year and d_{max} is the largest possible metric distance for those votes (see Signorino and Ritter 1999).

3 Results

Table 2 reports the estimated effects of travel visa requirements on bilateral migration flows. Regardless of whether a visa policy is instrumented, the visa variable has a statistically significant effect on all migration variables. However, instrumenting the visa variable makes considerable difference in terms of the magnitude of the effects. Without taking into account the potential endogeneity of visa policies, we estimate the inflows in visa-required corridors about 27 percent lower than visa-free corridors on average.[9] Outflows are also significantly lower in visa-required corridors, although on a somewhat lower level of around 17 percent. Adding up migration flows in either direction, we find that visas reduce both the overall circulation ('turnover') and annual net flows by about 26 percent.

The instrumented estimates (5)-(8) however, show that these estimates are (downward) biased. Visa policy instrumented by the UN voting similarity index significantly *increases* the magnitudes of the 'visa effect' on flows. Visa-free inflows are on average 67 percent higher than visa-restricted inflows. Visa-effects on reverse flows are even stronger with average outflows being 88 percent lower if immigration is visa-restricted. The negative effect of visa on both inflows *and* outflows therefore result in a strongly negative effect on the overall circularity or 'turnover' within bilateral corridors. We estimate the average turnover to be about 75 percent lower in visa-restricted corridors. The effect on net flows (inflows minus outflows) is comparatively modest with visa-restriction having a net immigration reducing effect of about 38 percent.

These results provide strong evidence for our hypothesis that the imposition of travel visa requirements reduce not only inflows but also outflows, and thus, overall circulation. The estimates indicate that visa policies affects migration independently not only from control variables but also from unobservable factors that may also affect and reflect the other dimensions of bilateral relations between origin and destination countries. Because our UN voting similarity measure captures some of the time-varying unobserved heterogeneity across dyads, we are confident there is a systemic visa policy effect that reduces international migration in either direction.

Other contextual variables generally show the expected sign. Low income in origin countries and higher income in destination countries increases migration. High growth rates in origin countries increase emigration, which may reflect that fast-growing economies offer more prospect for people to return. Similarly, discrepancies in the provision of political rights and civil liberties between origin and destination countries increase migration flows. Population size also shows the expected significant and positive signs.

[9] Estimates in this log-transformed model are interpreted as (semi-)elasticities, which implies that a change in the binary visa policy variable results in a $[e^{\beta} - 1] * 100, i.e. \approx \beta$, percentage change in the migration flow variable.

Table 2 Migration flows and visa policy (1973-2011)

	Dependent variable: Log of annual:	Inflow	Outflow	Turnover	Net-flow	Inflow	Outflow	Turnover	Net-flow
	Estimator	FE	FE	FE	FE	IV	IV	IV	IV
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bilateral	Visa requirement	-0.313**	-0.184**	-0.306**	-0.306**	-1.123**	-2.154**	-1.405**	-0.476**
		(0.016)	(0.015)	(0.016)	(0.020)	(0.130)	(0.142)	(0.131)	(0.162)
	Common currency	-0.034	0.037	-0.037	-0.218**	-0.051	0.012	-0.061	-0.204**
		(0.038)	(0.037)	(0.038)	(0.049)	(0.042)	(0.046)	(0.042)	(0.053)
Origin	Income per capita	-0.019**	-0.003	-0.020**	-0.014**	-0.028**	-0.035**	-0.033**	-0.012**
		(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
	Growth rate income p.c.	0.002**	0.006**	0.003**	-0.000	0.002**	0.005**	0.003**	0.000
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	Political rights	0.015**	0.028**	0.017**	0.018**	0.015*	0.051**	0.020**	0.008
		(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)
	Civil liberties	0.020**	-0.026**	0.015*	0.026**	0.027**	-0.009	0.024**	0.031**
		(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)
	Population size	0.000**	0.000**	0.000**	0.000**	0.000**	0.000**	0.000**	0.000**
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Destination	Income per capita	0.013**	0.035**	0.021**	0.000	0.031**	0.078**	0.045**	0.010*
		(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
	Growth rate income p.c.	0.004*	-0.014**	-0.003	0.013**	-0.002	-0.023**	-0.009**	0.008**
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
	Political rights	0.081**	0.047**	0.106**	0.059**	0.121**	0.198**	0.171**	0.041*
		(0.011)	(0.010)	(0.010)	(0.014)	(0.015)	(0.017)	(0.016)	(0.020)
	Civil liberties	-0.130**	-0.031*	-0.131**	-0.112**	-0.099**	-0.055**	-0.105**	-0.104**
		(0.013)	(0.013)	(0.013)	(0.017)	(0.016)	(0.018)	(0.016)	(0.020)
	Population size	0.000**	0.000**	0.000**	0.000**	0.000**	0.000**	0.000**	0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	Time FE	Yes	yes	yes	yes	yes	yes	yes	yes
	Constant	Yes	yes	yes	yes	yes	yes	yes	yes
	Observations	52,362	52,362	52,362	47,692	40,447	40,447	40,447	36,977
	Number of dyads	3,290	3,290	3,290	3,254	3,141	3,141	3,141	3,105
	R-sq (within)	0.19	0.15	0.19	0.14	0.12	0.10	0.09	0.12
	Hausman test	(0.000)	(0.000)	(0.000)	(0.000)				
	First stage regression: instrument for visa policy								
	UN affinity score					-0.271***	-0.271***	-0. 271***	-0.269***
						(0.000)	(0.000)	(0.000)	(0.000)

Note: Standard errors in parentheses: ** p<0.01, * p<0.05, + p<0.1. In IV regressions, visa variable has been instrument by UN affinity score variable. The instrument is a moving average of annual UN affinity score: affinity= 0.4*affinity_lag1 + 0.3*affinity_lag2 + 0.2* affinity_lag3 + 0.1*affinity_lag4

If migration restrictions decrease circularity, we should see that migration becomes less responsive to changes economic conditions in origin and destination countries. For instance, if migrants become unemployed, they are less likely to return if there is a risk of not being able to re-migrate. To investigate the responsiveness of migration to economic fluctuations, we interacted GDP growth rates in both origin and destination countries with our binary travel visa policy variable, keeping everything else constant (Table 3).[10] We find that the usual strong effects of economic growth on migration to and from destination countries are either fully (inflows) or largely (outflows) neutralised by visa policy restrictions. This shows that visa barriers may drastically reduce the responsiveness of migration to economic conditions and fluctuations in origin and destination countries. Even when origin countries are thriving economically, travel barriers may prevent migrants from returning. It therefore seems safe to say that visa requirements decrease the overall responsiveness or 'elasticity' of migration to economic trends in both origin and destination counties.

Table 3 Business cycles and visa policy effects (1973-2011)

	(1)	(2)	(3)	(4)
DV: Log of flow:	Inflow	Outflow	Turnover	Net migration
Visa	-0.297**	-0.188**	-0.295**	-0.257**
	(0.039)	(0.037)	(0.040)	(0.041)
GDP growth (dest)	0.009**	-0.019**	-0.000	0.029**
	(0.003)	(0.003)	(0.003)	(0.004)
Visa x GDP growth (dest)	-0.008*	0.009*	-0.004	-0.025**
	(0.003)	(0.003)	(0.003)	(0.004)
GDP growth (origin)	0.003	0.009**	0.004*	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Visa x GDP growth (origin)	-0.001	-0.004*	-0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)
Income gap (dest – origin)	0.017**	0.015**	0.020**	0.009+
	(0.005)	(0.004)	(0.005)	(0.005)
Other controls	yes	yes	yes	yes
Time FE	yes	yes	yes	yes
Constant	yes	yes	yes	yes
Observations	52,362	52,362	52,362	47,692
R-sq (within)	0.19	0.15	0.19	0.14
Number of dyads	3,290	3,290	3,290	3,254

Note: Robust standard errors in parentheses: ** p<0.01, * p<0.05, + p<0.1. GDP growth variables are lagged by one year.

4 Asymmetric policy effects: visa introductions versus removals

The above analysis has provided evidence that visa restrictions establish a significant barrier for international migration flows *in either direction* and decrease overall circularity. This however, does not provide an adequate estimate for the effect of *changes* in travel visa policies through the introduction or removal of visa requirements. After all, as visa regimes are relatively stable – visa requirements do not change much very often – the above results largely reflect cross-sectional variation. In order to understand short- to medium-term effects of changes in visa policies, we modify our empirical model by including a series of lead and lag dummy variables that may capture inter-temporal dynamics of

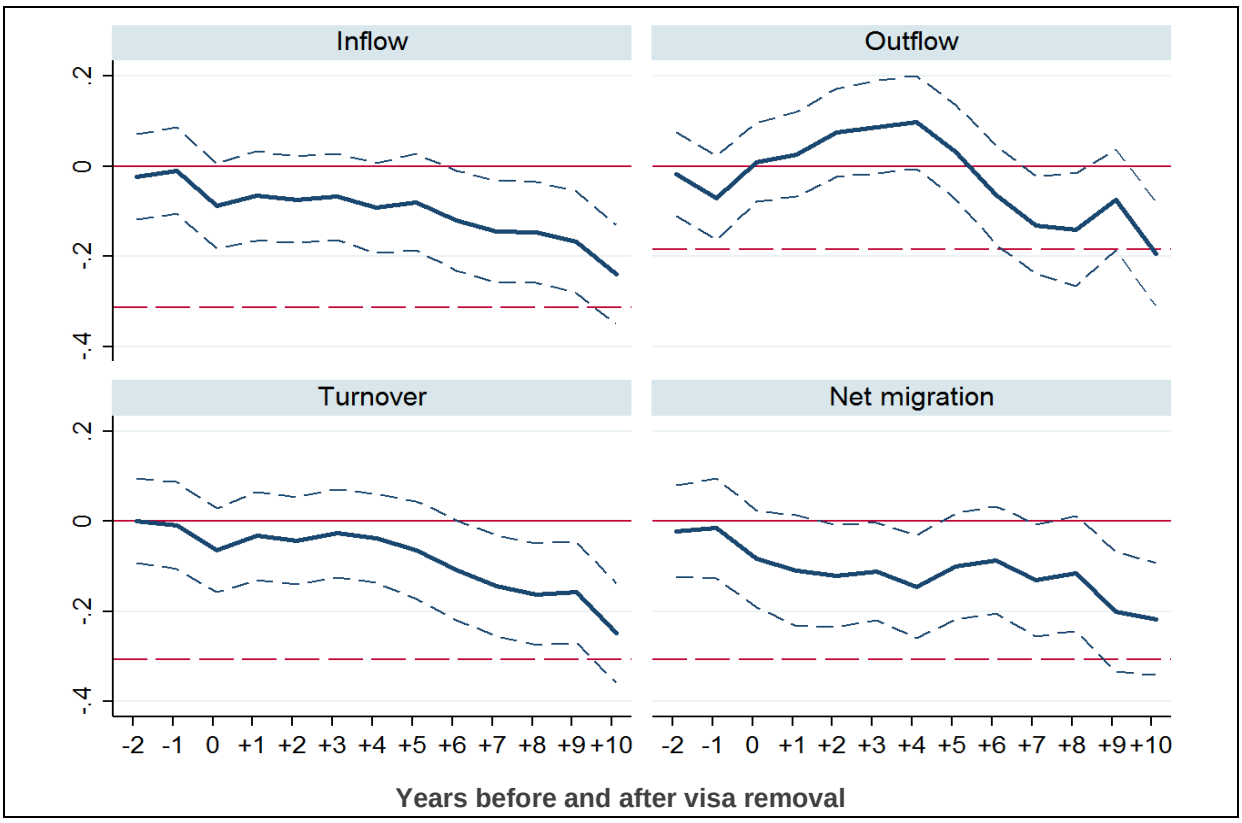
[10] In this specification, visa policy is not instrumented due to the lack of further instruments and the methodological complications in instrumenting multiple endogenous variables. Therefore, specifications (1)-(4) in Table 3 are based on the benchmark specifications (1)-(4) of the FE model (Table 2).

migration flows through an *anticipation* effect of an upcoming change in visa regulations and/or an *adaptation* effect after visa policy has changed.

To measure this, we include two lead dummies for the two years *before* a policy change and ten lag dummies capturing the respective years after a policy change took place. This procedure is suitable to assess inter-temporal substitution effects ('now or never migration' in anticipation of the forthcoming introduction of a visa) and post-introduction adjustment processes of a visa policy change. This procedure also enables us to assess *asymmetric policy effects* by analysing whether the effects of *introductions* and *removals* of travel visa requirement mirror each other or are substantially different.

Figure 1 (and Tables A-2 and A-3) display the effects on migration flows before and after the introduction of travel visa. We find no significant inter-temporal substitution or 'anticipation' effect, which may imply that people do not seem to respond to the introduction of visa requirements in the near future by migrating before it is too late. This may be explained by the fact that the introduction of requirements can often be unexpected and are generally not announced publicly well in advance. Concerning the post-introduction period, we find it takes a relatively long time for immigration and emigration flows to respond significantly to the introduction of visas. Although inflows decrease in the same year a visa is introduced, it takes more than five years until numbers have declined in a statically significant way. After ten years, inflows are about 20 percent lower than levels before the visa introduction, which is about three-quarters of the average long-term difference of about 26 percent (see FE estimation in Table 2, model 1) between visa-free and visa-restricted corridors.

Figure 1 Visa introduction and migration flow adjustments

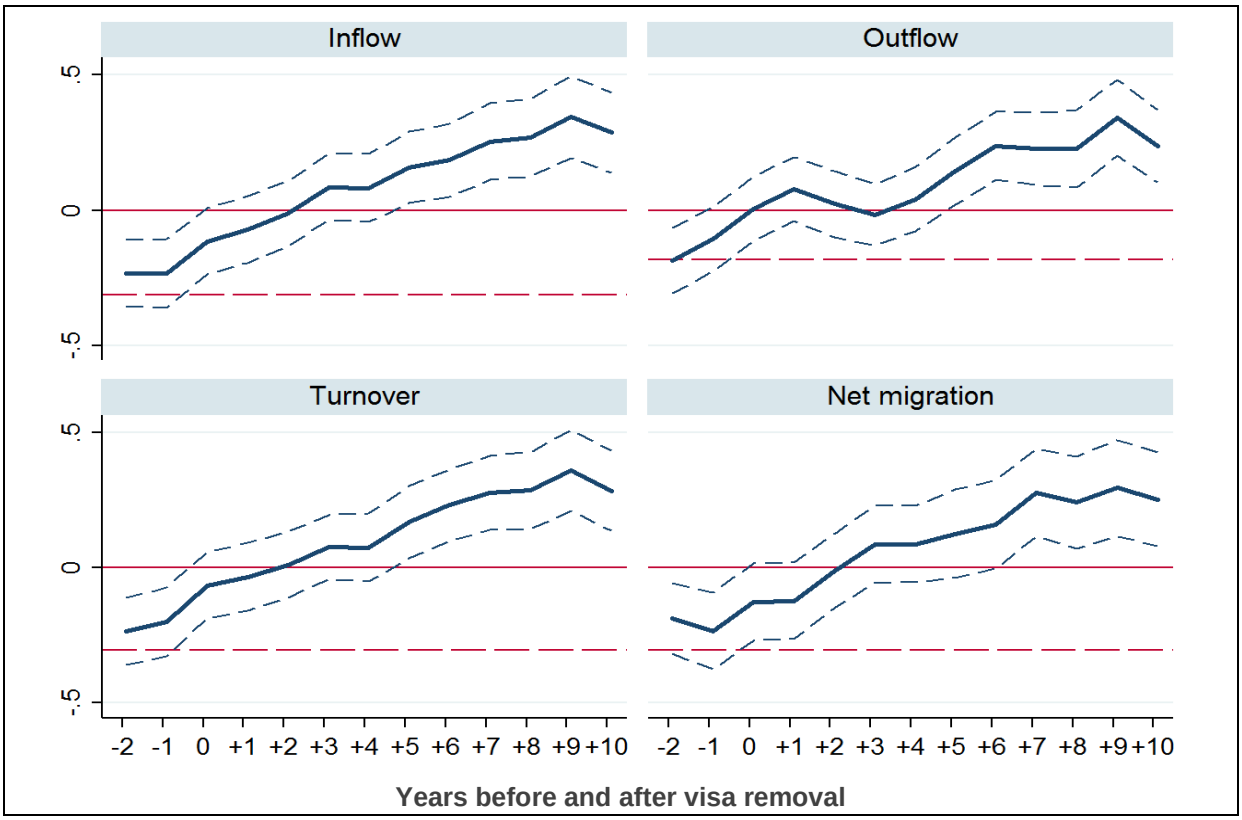


Note: Estimates of average deviation in migration flows of corridors in which visa requirement has been introduced from flows in visa-free corridors (zero line) between two years before and ten years after visa introduction. Horizontal long-dashed lines represent FE estimates of visa policy variable irrespective of time leads and lags (Table 2, models 1-4). Short-dashed lines reflect 95% confidence interval.

While emigration increases slightly after visa introduction, it takes six to seven years until outflows have declined significantly. These protracted visa introduction effects on in- and outflows are similarly reflected in the turnover and net flow trajectories. These delayed and partial effects of visa introductions can be partly explained by the fact that migrant networks tend to facilitate migration across formally closed borders by decreasing the costs and risks of migrating. This makes strong and immediate shifts in the volume or direction of ongoing migration processes unlikely. It is therefore only in the longer term that we may expect policy effects to take hold, and such effects are only partial due to the continuation of migration, for instance, through family migration.

On the contrary, the *removal* of a visa requirement has an immediate effect on inflows (and to a smaller extent on outflows) by increasing the average inflow by almost 30 percent after 3 years (Figure 2). Three years after removal of a visa requirement, immigration reaches the average long-term levels of visa-free corridors. This shows that the adjustment process after visa removals is much swifter than for visa introduction. This *asymmetric policy effect* becomes even stronger if we consider that in the case of the removal of visa requirements, immigration, emigration and 'turnover' do not converge towards long-term levels. Instead, all flows tend to 'overshoot' to much higher levels. This rapid increase only seems to reach a tipping point after about nine years.

Figure 2 Visa removal and migration flow adjustments



Note: Estimates of average deviation in migration flows of corridors in which visa requirement has been removed from flows in visa-free corridors (zero line) for the period two years before and ten years after visa removal. Long-dashed lines represent FE estimates of visa policy variable irrespective of time leads and lags (Table 2, models 1-4). Short-dashed lines reflect 95% confidence interval.

This 'over-shooting effect' of migration after the removal of travel barriers may be explained by two effects. First, visa removals enable people who already had a desire to immigrate, but considered it too difficult or costly, to migrate. Such effects also seem to exist in other migration policy domains, such as the temporary immigration surges after the removal of migration restrictions for countries in

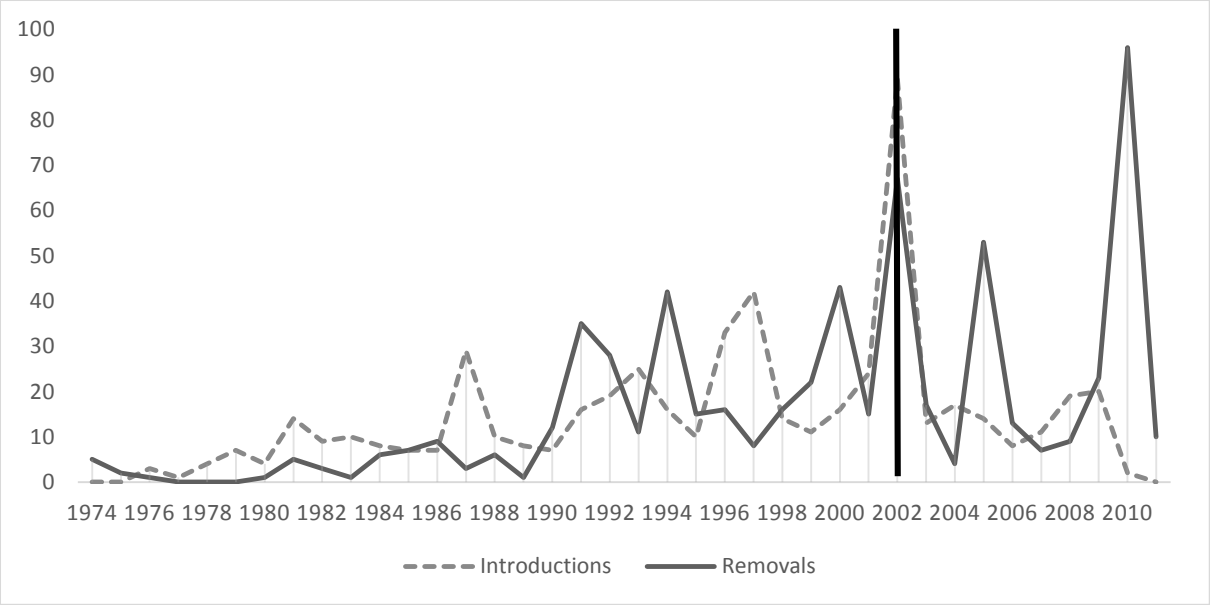
Central and Eastern Europe. A second factor may be that the removal of visa restriction may motivate some people to seize the opportunity – 'now or never' considerations – based on fears that the more liberal mobility regime may not persists for a long time, as was the case when Turkey re-introduced visas for Bulgarian citizens in 1989. While we did not find such inter-temporal substitution effects for visa introduction, these effects may be more relevant for visa removals. Third, such effects may be re-inforced when 'pioneer migrants' who left immediately after the visa removal are followed by subsequent 'network migrants' whose move is facilitated through social contacts and information provided by prior migrants.

Visa removals are also likely to encourage emigration along the same corridors. Although this variable measures the departure of citizens from a particular origin country irrespective of their destination, it is safe to assume that this strongly correlates with return to the origin country (see Vezzoli, Villares-Varela and de Haas 2014). This effect on emigration can be explained in similar ways as the 'overshooting' effect for immigration. First, the removal of visa requirements may neutralise fears amongst those migrants who already had a wish to return, but did not do so out of fear of not being able to travel back to visit family and friends or to re-migrate. Second, the emigration-increasing effect is likely to be amplified by network effects. In other words, the removal of visa requirements leads to a rapid increase in overall circulation along bilateral corridors because it reduces costs and risks of movement.

4.1 Measuring the difference-in-difference effect of visa introduction and removal

To further investigate the existence of *asymmetric visa policy effects* we analysed whether visa policy changes have significantly different effects on migration when compared with counterfactual situations in which there has been no visa policy change. To perform this type of analysis we select only those countries where a certain policy change has occurred in 2002, which is the year where for all 38 countries under consideration most visa introductions (89) and removals (67) have taken place since 1974 (Figure 3).

Figure 3 Visa introductions and removals since 1974 (38 destination countries)



Each bilateral dyad affected by the visa policy change in 2002 is then matched with a number of 'similar' dyads that were not affected by the same policy change in 2002 (and the five years after). The average difference in migration outcomes across the two groups is compared to estimate the respective effect of a visa policy change. This difference-in-difference (DID) estimation overcomes the problem of missing data by measuring outcomes and covariates for both the dyads that have seen a policy change ('treated' dyads) and the dyads without a change in visa regulation in the same period ('untreated' dyads). DID compares 'treated' and 'untreated' control groups in terms of changes in migration outcomes M over time relative to the outcomes observed before policy change occurred.

$$\Delta\Delta = E\left(M_{post}^{T} - M_{pre}^{T} | T = 1\right) - E\left(M_{post}^{C} - M_{pre}^{C} | T = 0\right)$$

Since we have enough 'un-treated' dyads available to match with dyads that have seen a policy change, and by assuming that differences in implementing the policy change are based on differences in observed characteristics, the corresponding effect of the policy change can be assessed even if the policy change itself is not random.

We then combine the DID estimation with the propensity score matching (PSM) to better match control and treatment units on pre-intervention characteristics. Hereby, the propensity score can be used to match treated and untreated units in years before a policy change occurred, and the impact of the policy change is calculated across treated and matched control units within the common support.[11] PSM involves the construction of a 'statistical control group' by estimating the probability of a policy change on the basis of observed characteristics unaffected by the policy change. This is done on the basis of a vector of observable characteristics X in the three years (1999-2001) before the policy change has taken place. Propensity scores are calculated as the probability for a policy change, conditional on observable characteristics X:[12]

$$P(X) = \Pr(T = 1|X)$$

We use non-parametric kernel matching, which creates a weighted average of all non-affected dyads, to construct the counterfactual match for each policy-affected dyad.

Results based on this counterfactual analysis largely confirm our previous finding of asymmetric visa policy effects (Table 4). For the first five years (2002-2007) after removal of a visa requirement in 2002 we find a significantly positive and robust effect on inflows and, to a lesser extent, also on outflows. Effects on both the overall circulation as well as net inflows are particularly strong and indicate for significantly increasing migration rates in both directions *after* the introduction of a visa waiver.

[11] Unlike PSM alone, the DID estimator allows for unobserved heterogeneity (the unobserved difference in mean counterfactual outcomes between treated and untreated units) that may affect policy change (and thus, a potential selection bias), assuming that these unobserved factors do not vary over time.

[12] Rosenbaum and Rubin (1983) show that, under certain assumptions, matching on $P(X)$ is as good as matching on X.

Table 4 Visa removal effect: DID estimation with propensity score matching

	INFLOW		OUTFLOW		TURNOVER		NET FLOW	
VISA	Before	After	Before	After	Before	After	Before	After
REMOVAL	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NO	268.92	377.24	42.74	54.24	216.71	282.70	128.82	173.14
	(45.58)	(34.27)	(19.79)	(13.83)	(67.20)	(46.81)	(41.56)	(29.00)
YES	396.47	786.89	160.87	270.24	479.47	1102.58	141.24	542.80
	(49.54)	(30.74)	(18.09)	(11.93)	(62.51)	(40.92)	(38.66)	25.312
Δ	127.55*	409.65***	118.13***	216.01***	262.76***	819.88***	12.42	369.66***
	(67.32)	(46.03)	(26.81)	(18.27)	(91.78)	(62.18)	(56.77)	(38.50)
Δ IN Δ	**282.10*** **		**97.88*** **		**557.12*** **		**357.24*** **	
	(81.56)		**(32.44)**		**(110.86)**		**(68.59)**	

Note: Means and Standard Errors are estimated by linear regression. Standard errors in parentheses. Inference: *** p<0.01; ** p<0.05; * p<0.1. DID-estimator with Kernel propensity score matching. Covariates included. Year of intervention (visa removal): 2002. Period before: 1999-2001. Period after: 2002-2007. No. of dyads with visa removal in 2002: 68. Propensity score is estimated at the baseline.

On the other hand, migration flows show a less clear direction in the five years after introduction of a visa requirement (see Table 5). While gross and net inflows are negatively affected, outflows and overall circulation show negative but not significant differences in levels before and after a restrictive visa policy intervention. This results corroborates our earlier finding on the delayed effects of restrictive migration policy change, which may be explained to the migration-facilitating function of migration networks which can reduce the effectiveness of policy restrictions.

Table 5 Visa introduction effect: DID estimation with propensity score matching

	INFLOW		OUTFLOW		TURNOVER		NET FLOW	
VISA	Before	After	Before	After	Before	After	Before	After
INTRO	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NO	261.68	437.58	154.06	153.49	390.27	478.03	84.50	176.12
	(39.52)	(26.84)	(15.74)	(10.67)	(40.03)	(28.06)	(16.45)	(11.66)
YES	62.40	90.82	16.63	28.717	73.37	109.50	40.11	52.07
	(38.67)	(25.79)	(15.40)	(10.15)	(39.27)	(25.88)	(16.14)	(10.64)
Δ	-199.28***	-346.76***	-137.43***	-124.77***	-316.89***	-368.52***	-44.40***	-124.05***
	(55.29)	(37.22)	(22.02)	(14.72)	(56.07)	(38.18)	(23.05)	(15.79)
Δ IN Δ	**-147.48*** **		**12.66**		**-51.632**		**-79.66*** **	
	(66.65)		**(26.49)**		**(67.84)**		**(27.93)**	

Note: Means and Standard Errors are estimated by linear regression. Standard errors in parentheses. Inference: *** p<0.01; ** p<0.05; * p<0.1. DID-estimator with Kernel propensity score matching. Covariates included. Year of intervention (visa introduction): 2002. Period before: 1999-2001. Period after: 2002-2007. No. of dyads with visa introduction in 2002: 89. Propensity score is estimated at the baseline.

5 Conclusion

Although the effectiveness of migration policy has been subject of heated debate, evidence has remained inconclusive because of conceptual and methodological limitations, as well as the lack of adequate migration and policy data. Reflecting the 'receiving country bias', the one-sided research focus of policy effects on immigration flows ignores the effects of policies on reverse flows. This reflects the lack of adequate empirical tests to measure the effects of policies on migration flows in *either direction*. Also, prior studies have not taken into account the potential asymmetry of policy effects, which is the

hypothetical possibility that the introduction and removal of policy restriction may have different effects.

To partly fill these gaps, and drawing on unique new datasets containing an unprecedented range of bilateral migration flow data (DEMIG C2C) and data on travel visa requirements (DEMIG VISA) covering 38 countries over the 1973-2012 period, this paper analysed the simultaneous effects of the introduction and removal of travel visa requirements on the volume and timing of immigration and emigration and how these effects interfere with economic migration determinants. The results showed that visa restrictions significantly decrease immigration *and* emigration. In other words, the immigration reducing effect is partly counterbalanced by its emigration reducing effect. This confirms the hypothesis that immigration restrictions have significant *reverse flows substitution effects* by decreasing circularity. Although this data does not allow for the analysis of actual migration behaviour at the micro-level, our macro-level findings seem to be in line with evidence from surveys and case studies that immigration restrictions can push migrants into permanent settlement.

Besides decreasing overall levels of circulation, we also found that immigration restrictions severely reduce the responsiveness of migration to economic growth virtually down to zero. In other words, visa requirements partly neutralise business cycle effects. Taking into account the close association between economic growth and the level of immigration in visa-free corridors that exists, this indicates that, besides interrupting circulation and encouraging long-term settlement, visa restrictions severely reduce the responsiveness of migration to economic fluctuations in destination *and* origin societies.

The analysis also found evidence that policy effects are highly asymmetrical. While the introduction of restrictive measures had a delayed effect, the lifting of restrictions have an almost immediate effect. After the introduction of visa requirement, levels of immigration only go down gradually. Even after 10 years we still see significantly higher levels of immigration and emigration compared to average levels in visa-required migration corridors. It is likely that the migration-facilitating function of migration networks partly explain these delayed effects and the only very gradual decreases of migration after introduction of restrictions.

On the contrary, migration flows respond almost immediately after the removal of visas, with levels of immigration and emigration reaching the average levels of visa-free corridors after one to three years, after which they temporarily 'overshoot' these levels for several years. This may indicate the existence of 'temporal substitution effects' upon visa removal, whereby people partake in 'now or never' migration because they may fear re-introduction of migration restrictions. Such hypotheses would however, need further investigation using micro-level data. Such temporal surges of migration did not occur in anticipation of the introduction of visas. This may be explained by the fact that visa introductions are generally not announced well in advance as is the case with major reforms.

In sum, this paper found substantial evidence for the hypothesis that the immigration reducing effect of immigration restrictions is partly undermined by its reducing effect on reverse (emigration) flows across a vast range of migration corridors, thereby decreasing overall circulation and encouraging long-term settlement. Another undesired effect may be that visas requirements partly neutralise business cycle effects, which makes migration much less responsive to economic growth. The paper was not able to assess the extent to which visa restrictions compel migrants to migrate through irregular channels (categorical substitution) or divert migration through other itineraries routes or deflect migration towards other destination countries (spatial substitution). Such effects may further undermine the effectiveness of immigration restrictions, and need to be investigated in future analyses. Future analyses should also test of the effects of other policy measures in order to gain a more comprehensive picture of the role of policies in migration processes.

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Annex

Table A-1: Descriptive statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
In-flow	109176	873.819	7015.414	0	630416
Out-flow	88272	534.073	5704.169	0	393884
Gross-flow	84025	1472.438	13122.74	0	813541
Net-flow	84025	355.320	4043.207	-179355	599361
Visa requirement	90293	0.650	0.477	0	1
UN affinity	91613	0.670	0.262	-1	1
Income level (p.c., dest, in ‘000)	106886	17.981	9.701	0.346	56.285
Income level (p.c. origin, in ‘000)	93399	7.009	9.752	0.055	67.554
Income growth (p.c., origin, in ‘000)	93503	2.222	5.764	-50.290	92.586
Income growth (p.c., dest, in ‘000)	106548	2.001	3.240	-30.694	13.589
Political rights (dest)	104290	1.218	0.794	1	6
Civil liberties (dest)	104292	1.388	0.825	1	5
Political rights (origin)	95843	3.543	2.221	1	7
Civil liberties (origin)	95860	3.537	1.916	1	7
Colonial ties	108683	0.031	0.174	0	1
Distance (in ‘000 km)	108683	7.318	4.682	0.077	19.648
Population size (origin)	103253	36.090	126.825	0.016	1311.798
Population size (origin)	108683	22.788	28.280	0.179	188.694
Common currency	108683	0.0144	0.119	0	1

Table A-2: Visa introduction and migration flows (1973-2011)

Dependent variable: Log of annual:	(1) Inflow	(2) Outflow	(3) Turnover	(4) Net migration
Lead 2	-0.024	-0.017	0.002	-0.022
	(0.041)	(0.039)	(0.041)	(0.051)
Lead 1	-0.010	-0.070+	-0.009	-0.015
	(0.040)	(0.039)	(0.040)	(0.049)
Introduction	-0.088*	0.010	-0.064	-0.083+
	(0.040)	(0.039)	(0.040)	(0.050)
Lag 1	-0.066	0.026	-0.032	-0.109*
	(0.041)	(0.040)	(0.041)	(0.051)
Lag 2	-0.074+	0.075+	-0.043	-0.121*
	(0.042)	(0.041)	(0.042)	(0.052)
Lag 3	-0.067	0.087*	-0.026	-0.111*
	(0.042)	(0.041)	(0.042)	(0.052)
Lag 4	-0.092*	0.098*	-0.038	-0.145**
	(0.042)	(0.041)	(0.042)	(0.052)
Lag 5	-0.080+	0.031	-0.064	-0.100+
	(0.043)	(0.041)	(0.043)	(0.053)
Lag 6	-0.120**	-0.064	-0.108*	-0.086
	(0.044)	(0.042)	(0.044)	(0.054)
Lag 7	-0.145**	-0.131**	-0.142**	-0.130*
	(0.045)	(0.043)	(0.044)	(0.055)
Lag 8	-0.147**	-0.140**	-0.162**	-0.116+
	(0.050)	(0.049)	(0.050)	(0.062)
Lag 9	-0.168**	-0.074	-0.157**	-0.200**
	(0.053)	(0.051)	(0.053)	(0.065)
Lag 10	-0.240**	-0.195**	-0.247**	-0.217**
	(0.055)	(0.053)	(0.054)	(0.067)
Lag >10	-0.535**	-0.332**	-0.523**	-0.513**
	(0.019)	(0.019)	(0.019)	(0.024)
Other controls	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Constant	yes	yes	yes	yes
Observations	54,986	54,986	54,986	50,007
R-sq (within)	0.20	0.15	0.20	0.15
Number of dyads	3,300	3,300	3,300	3,269

Note: Standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1. Full table is available on request.

Table A-3: Visa removal and migration flows (1973-2011)

Dependent variable: Log of annual:	(1) Inflow	(2) Outflow	(3) Turnover	(4) Net migration
Lead > 2	-0.299**	-0.129**	-0.278**	-0.301**
	(0.046)	(0.043)	(0.047)	(0.049)
Lead 2	-0.234**	-0.186**	-0.235**	-0.188**
	(0.063)	(0.062)	(0.064)	(0.067)
Lead 1	-0.233**	-0.107+	-0.200**	-0.234**
	(0.065)	(0.061)	(0.065)	(0.072)
Removal	-0.114+	0.006	-0.065	-0.126+
	(0.063)	(0.060)	(0.062)	(0.073)
Lag 1	-0.071	0.079	-0.034	-0.123+
	(0.063)	(0.060)	(0.064)	(0.072)
Lag 2	-0.012	0.022	0.011	-0.011
	(0.062)	(0.063)	(0.062)	(0.070)
Lag 3	0.086	-0.017	0.076	0.087
	(0.062)	(0.058)	(0.061)	(0.073)
Lag 4	0.083	0.041	0.075	0.088
	(0.064)	(0.060)	(0.064)	(0.072)
Lag 5	0.158*	0.145*	0.169*	0.126
	(0.067)	(0.063)	(0.068)	(0.083)
Lag 6	0.186**	0.239**	0.231**	0.160+
	(0.069)	(0.064)	(0.068)	(0.083)
Lag 7	0.254**	0.227**	0.277**	0.277**
	(0.072)	(0.068)	(0.070)	(0.083)
Lag 8	0.269**	0.228**	0.286**	0.241**
	(0.073)	(0.073)	(0.073)	(0.087)
Lag 9	0.344**	0.342**	0.359**	0.295**
	(0.077)	(0.072)	(0.076)	(0.091)
Lag 10	0.287**	0.236**	0.285**	0.252**
	(0.076)	(0.068)	(0.076)	(0.088)
Other controls	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Constant	yes	yes	yes	yes
Observations	54,989	54,989	54,989	50,010
R-sq (within)	0.20	0.15	0.20	0.15
Number of dyads	3,300	3,300	3,300	3,269

Note: Standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1. Full table is available on request.