

The Effect of Visas on Migration Processes¹

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The effectiveness of migration policies has been widely contested. However, because of methodological and conceptual limitations, evidence has remained inconclusive. Moreover, prior studies focus on the effects of policies on inflows and fail to assess the simultaneous effect of policies on outflows. This is essential from a theoretical point of view as immigration restrictions may reduce both inflows and outflows and, hence, overall circulation. This renders the effect of immigration restrictions on *net* migration theoretically ambiguous. To fill this gap, and using unique migration and visa data from the Determinants of International Migration (DEMIG) project, this paper assesses the short- and long-term effects of travel visa policy regimes on bilateral immigration and emigration dynamics. The results suggest that travel visa policies significantly decrease inflows, but this effect is undermined by decreasing outflows of the same migrant groups. This confirms that migration restrictions decrease circulation and tend to encourage long-term settlement, and thereby sharply reduce the responsiveness of migration to economic fluctuations in destination *and* origin societies. We also identify asymmetric policy effects with migration flows declining only very gradually after a visa introduction but increasing almost immediately after visa removal.

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INTRODUCTION

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

Since the 1970s, the political salience of immigration has risen fast. While the relative number of international migrants has remained remarkably stable at levels between 2.5 and 3 percent of the world population over the past few decades (Czaika and de Haas 2013a), the issue of migration control has risen high on political agendas. Although this politicization of migration is a global phenomenon, this seems to be particularly the case for Western European countries. This might be partly explained by the fact that, since the end of World War II and in the wake of decolonization, Western Europe has transformed from a source of colonizers, settlers, labor migrants, and refugees to the rest of the world into a major global migration destination. This geographical turnaround of global migration patterns has confronted Europe with unprecedented and largely unplanned immigration of an increasingly diverse group of migrants from non-European regions. But also for the predominantly Anglo-Saxon settler countries — the United States, Canada, Australia, and New Zealand — declining immigration from Europe went along with increasing migration from non-Western countries, particularly Asian and Latin American societies.

Since the Oil Crisis in 1973 and the suspension of guest-worker programs, Western European societies such as Germany, France, Belgium, and the Netherlands experienced the — generally unexpected and unintended — permanent settlement of large numbers of former “guest workers” and other supposedly temporary immigrants, followed by large-scale family immigration (Castles and Kosack 1973; Entzinger 1985). At the same time, the United States has experienced the persistence and increase of largely spontaneous, often irregular migration of Mexican workers ever since the end of the Bracero recruitment agreement in 1964 (Durand, Massey, and Zenteno 2001; Cornelius et al. 2004). Both in Europe and the United States, persistent demand for low-skilled labor, in combination with growing efforts by governments to curtail such immigration, seems to have led to increased reliance on family migration and also an increase

in 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 in important African destination countries 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).

The continuation of regular and irregular migration and large-scale settlement of migrants in *apparent* defiance of border controls and immigration restrictions has sparked a heated debate on the effectiveness of immigration policies in politics and academia. Several scholars have argued that efforts of states to restrict immigration have often failed (Bhagwati 2003; Castles 2004; Cornelius et al. 2004; Düvell 2005). The argument is that international migration is mainly driven by structural factors such as labor market demand, inequalities in wealth between rich and poor countries as well as conflicts in origin countries. Migration policies will therefore only have limited effects. Furthermore, once a certain number of migrants has settled at the destination, social networks and the so-called "migration industry" (recruiters, employers, lawyers, smugglers, and other intermediaries) tend to facilitate migration by lowering the costs and risks of moving (Massey 1990; Krissman 2005). Rather than affecting overall volumes of inflows, immigration restrictions would therefore primarily change the *ways* in which people migrate, such as through an increased use of the family migration channel or irregular means of entry, but not, to a significant degree, the overall volumes and long term trends of migration, which are rather driven by broader economic, demographic, and political processes in origin and destination countries.

However, other scholars have argued that, on the whole, state policies have been largely effective (Brochmann and Hammar 1999; Strikwerda 1999). Despite extensive media and scholarly attention for irregular and other forms of officially "unwanted" migration, it can be argued that the vast majority of migrants abide by the rules and that bureaucratic systems set up to regulate migration are therefore largely effective, albeit not perfect. This position seems to be partly confirmed by a growing number of quantitative studies indicating that immigration restrictions have a significant effect on inflows (Hatton 2005; Mayda 2010; Beine, Docquier, and Özden 2011; Ortega and Peri 2013). However, although it seems rather obvious that restrictions should have at least *some* effect on inflows, the more appropriate questions are *how big* these policy effects are com-

pared to other migration determinants as well as how restrictions effect both inflows and outflows and, hence, the overall dynamics of migration. For instance, in his paper investigating the determinants of asylum migration, Hatton (2009) concluded that the decline of asylum applications over the 2000s in the industrialized countries of Europe, North America, and Australasia should be largely 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 about this issue seems spurious because of fuzzy definitions of policy effectiveness. These partly stem from a common confusion between (1) policy discourses, (2) policies on paper, (3) policy implementation, and (4) policy impacts. This differentiation allows us to distinguish three “policy gaps”: The *discursive gap* (the discrepancy between public discourses and policies on paper); the *implementation gap* (the disparity between policies on paper and their implemented policies); and the *efficacy gap* (the extent to which implemented policies affect migration) (Czaika and de Haas 2013b). In brief, the reality of policy making is often more nuanced than politicians’ discourses suggest. Although implemented policies seem to be the most appropriate yardstick to assess policy effectiveness, in practice, the generally “tougher” discourses are often used as a benchmark, which can easily lead to an overestimation of “policy failure” (Czaika and de Haas 2013b).

This leads to some methodological considerations. First, it is questionable to what extent it is useful to talk in terms of levels of “general restrictiveness.” In general, immigration policies are about *selection* rather than controlling the total volume of migrants, despite politicians’ discourses which may suggest the latter (de Haas, Natter, and Vezzoli 2014). 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 over the past decades there has been a trend in which most Western countries have gradually liberalized their policies toward high-skilled workers, students, and family migrants (cf. Bonjour 2011), this has gone along with frequently increasing restrictions toward asylum seekers and low-skilled workers.² Second, because

²Several 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, 577).

of the often considerable gap between migration discourses and actual policies in the forms of laws, rules, measures, and practices, there is reason to question the frequent assumption that immigration policies have generally become more restrictive over the past decades. For instance, “Fortress Europe” may be an adequate metaphor to characterize border control policies toward asylum seekers and refugees (Hatton 2004), but seems inappropriate to characterize the immigration policies of EU countries as a whole. There is also considerable variation over time, meaning that there has not been a unilateral linear trend toward more or less restrictiveness (cf. de Ortega and Peri 2009; Haas, Natter, and Vezzoli 2014).

From this, we can draw three methodological inferences. First, the appropriate measurement of the effects of migration policies requires us to focus on actually implemented policies and *concrete policy instruments*. The second inference 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 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); and timing of migration and reverse flows. On this basis, he hypothesized four hypothetical “substitution effects” which can limit the effectiveness of immigration restrictions: (1) *spatial substitution* through the diversion of migration to other countries; (2) *categorical substitution* through a reorientation toward other legal or illegal channels (for empirical evidence, see for instance Czaika and Hobolth 2016); (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 and make the effect of restrictions on net migration ambiguous. The existence of such substitution effects also shows the need to look at the “externalities” of *specific* policy measures by looking beyond the (short-term) effects on inflows of *targeted* (e.g., asylum, family) migration categories. This should be

achieved by considering (short *and* long-term) effects of specific migration policies on outflows of the targeted migrant category, as well as the effects on immigration and emigration of migrant groups which are not explicitly targeted by the policies. In other words, it is only by looking at policy effects on overall, long-term migration dynamics that we can obtain comprehensive insights in 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. First, many studies are limited by a “single comparative design,” which means that they explain variation in total immigration to a range of destination countries (Hilderink et al. 2001; Jennissen 2003; Zoubanov 2003, 2004). This research design creates a bias toward destination country-specific variables by ignoring the relevance of origin-country contexts. The few studies which differentiate several origin groups in one or separate destinations (cf. Faini and Venturini 1994; Rotte, Vogler, and Zimmermann 1997) suffer from the reverse problem. Second, migration policy variables are rarely included in models or poorly operationalized, although a few recent studies have started 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 more limited number of countries and shorter time periods. Third, most prior studies focus on rather limited time periods, whereas a proper study of migration dynamics and the short- and long-term effect of policies would require data spanning several decades.

From a theoretical and empirical point of view, more comprehensive assessments of migration determinants including policy effects can only be achieved through a “double comparative approach” implying simultaneous analysis of the migration of multiple origin groups to and from multiple destination countries.³ This requires annual bilateral (country-to-country) migration data covering several decades allowing for the 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 a double comparative design to test the effects of

³A similar approach has been used by van Tubergen, Maas, and Flap (2004) in studying the economic incorporation of immigrants in 18 Western countries.

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 reverse direction. This is a problem because the effectiveness of policy restrictions can be undermined by “reverse flow substitution effects” by not only reducing inflows from particular origin countries, but also reducing 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 in Western European countries (Entzinger 1985; Massey and Pren 2012), but has never been systematically tested in a comparative and longitudinal setting. Based on the above reasoning, we can hypothesize that more liberal migration policies increase the overall responsiveness or “elasticity” of migration to migration determinants such as economic growth and labor 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 flow effects in order to understand how policies affect migration *dynamics* and *circulation* over time. In brief, the danger of the usual exclusive focus on the inflow targeted by the policy is to overestimate 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. However, there is reason to hypothesize that policy restrictions and policy liberalizations have asymmetrical effects. While the lifting of a barrier may have a more immediate effects, case studies suggest that the effects of restrictions may be smaller or may take more time to materialize, particularly because 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).

Much current policy interventions aim at stimulating circular migration through restricting migrants’ access to rights and stimulating return. However, such policies may be based on flawed assumptions on the role of policies in migration processes. In fact, there is reason to hypothesize 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 as well as to provide policy making with a more solid evidence basis.

By also studying the effect of the *direction* of visa policy *changes* on migration flows, we hypothesize that visa introductions and removals may not lead to symmetric policy effects. On the one hand, the introduction of restrictive measures may trigger rather delayed (long-term) effects on migration flows partly due to the migration-facilitating function of migration networks which may lead to only very gradual decreases of migration after introduction of restrictions. On the other hand, the lifting of visa restrictions may rather have an almost immediate effect driven by the potential existence of “temporal substitution” upon visa removal, whereby people see migration as a “now or never” proposition because they may fear reintroduction of migration restrictions.

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 which we collected as part of the DEMIG project.⁴ Several pragmatic and analytical considerations underpinned our choice to use bilateral travel visa requirements to analyze policy effect. The first, pragmatic reason is the historical and geographical coverage of travel visa data. It is the only policy instrument for which we were able to compile long data series for all countries in the world covering the entire period between 1973 and 2012. 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 2013b; 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 that do not specify for individual origin countries. Visa data have the unique feature of being a *bilateral* (country-to-country) policy instrument that is available for all countries, which is required to perform a double comparative analysis to test the effect of policy on flows of multiple origin groups and to and from a range of destination countries.

⁴See www.migrationdeterminants.eu.

Second, travel visa data are a policy instrument for which information is available and reliable, because it is safe to assume that they are actually implemented. Our data originate from the Travel Information Manuals of the International Air Transport Association (IATA). This is a very reliable source of information. After all, the very reason for publishing these manuals is 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, it is safe to say that the introduction or lifting of a visa requirement is a major policy change with real consequences. It would have been 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 this 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, visa policies have played an increasingly important role in preventing people from certain countries of origin from entering the national territory. For instance, over the 1980s and 1990s, Western European countries introduced travel visa 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, traveling on their passports alone, and obtaining work and residence permits after they obtained work.

Prior research has indicated that the majority of migrants without residence documents have entered regularly (cf. Schoorl et al. 2000; Düvell 2005). Once migrants stay longer than their formal tourist status allows (usually between three and six months), their stay becomes unauthorized. 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 unauthorized migrants eventually obtain residency documents through regularization campaigns or “amnesties” (cf. Fakiolas 2003; Levinson 2005; Zincone 2006). The recent history of immigration to Western Europe and the United States has partly been one of regular entry, unauthorized overstay, and eventual regularization. States have therefore increasingly used visas as

an instrument of upfront prevention for people to come at all, which seems particularly effective for distant origin countries which are only reachable by air. Complementary to travel visas, destination countries have massively introduced carrier sanctions in the 1980s and 1990s 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 claiming asylum (Neumayer 2006).

More generally, one can see visa requirements as indicating which migrants are seen as “desirable” in terms of their national background and the assumed migration motives, skills, and sociocultural background of migrants from those countries. It is therefore plausible that there is a relation between travel visa regimes and other immigration restrictions toward 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 goes along with the lifting for others. For instance, as most EU countries started to remove their internal boundaries with the signature of the Schengen Agreement in 1985 and its full implementation in 1995, they became increasingly concerned about controlling external borders. This coincided with the introduction for visa requirements for an increasing range of non-European, particularly African and Asian 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” (OECD 1992, 77; FocusMigration 2012, 3).

Governments often do not conceal that they see visas as instruments to curb migration, in particular of asylum seekers. For instance, in 1992, Sweden motivated the introduction of travel visa regimes for Serbians, Montenegrins, and Macedonians by the strong growth in the number of refugees of non-Bosnians from 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 had a deterrent effect. In 2009, Canada introduced travel visas for Mexican citizens in response to tripling in refugee claims between 2005 and 2008 (Government of Canada 2009a). In the same year, Canada introduced visa requirements for Czech citizens, equally in reaction to a

strong increase in refugee claims, particularly by Roma, since visa restrictions were lifted in 2007 (Government of Canada 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; 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. They are a particularly attractive instrument for states, as visa restrictions can generally be imposed through directives, decrees, or other administrative measures, and generally do not require cumbersome legal changes and, hence, parliamentary and legal procedures, and can therefore be implemented rather quickly.

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 Haas 2014). We complemented these data with flow data for four additional countries from UNDP (2010). To our knowledge, this has yielded the largest bilateral migration flow database that has been compiled so far. Bilateral immigration and emigration data, reported by 38 countries (no emigration flow data are reported by Canada, France, and Moldova) on bilateral inflow from (and outflows to) about 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 take citizenship as a starting point. The only limitation arises in the case when individuals hold dual citizenship.

Additional 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 not only to study the effect of travel visa policies on the volume of migration on inflows and outflows of citizens from targeted origin countries, but also to investigate the effect of travel visa requirements on the overall rate of circulation within bilateral dyads as well as their effects on net migration. Information on visa requirements was drawn from the International Air Transport

Association (IATA) *Travel Information Manuals*,⁵ and was 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 traveler, and whether a visa was required in a particular year. The binary visa policy was coded zero if no visa is required and one if a visa permit is required.⁶ As long as no visa is required for *entering* the country, we consider it as an exemption, regardless of the period people are allowed to stay. We do not consider visa exemptions for holders of residence permits in the country of visa issuance or other countries. We also ignore diplomatic passports or other exemptions that are not for regular touristic and other purposes.

Table 1 shows that about 35 percent of all 90,000 dyad-year observations covered by our bilateral migration database were visa-free whereas for the remaining corridors visas were required. A relatively low number (119) of dyad-year combinations concerned “blacklisted” corridors, in which case citizens could not even apply for a travel visa. We added these cases to the set of visa-constrained corridors. The data also show 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. World regions mostly affected by restrictive visa policy of those 38 destination countries are mostly countries in the Global South. On average, 98.3 percent (SD: 12.9) of country-years in South Asia and 93.0 percent (SD: 25.6) of country-years in Sub-Saharan Africa were visa constrained, followed by countries of the Middle East and North Africa with on average 87.2 percent (SD: 33.4) of all country-years being visa constrained. At the other end of the “global mobility divide” (Mau et al. 2015) are European and Central Asian countries (Mean: 36.3, SD: 48.1) and North America (Mean: 15.6, SD: 36.3).

We included a number of control variables in our empirical analysis. Income data on *GDP per capita* and year-by-year *GDP per capita growth* are drawn from the World Development Indicators (World Bank 2013).

⁵The IATA travel manuals are released on a monthly basis. We have selected all January manuals from 1973 to 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.).

⁶The original database includes individuals with the nationality of a “blacklisted” country, who are not allowed to travel to the country of destination. We have (re-)coded the visa policy variable for these dyads to one (instead of two).

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	64.86
Blacklisted (no. of dyad-years)	119	0.13
Total (no. of dyad-years)	90,293	100.00
Visa introductions (no. of incidences)	547	
Visa removals (no. of incidences)	612	

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 and 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, which monitor trends in levels of democracy and freedoms. Measured on a one to seven scale, high scores represent a low level of political rights and civil liberties.

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 are based on information provided by Head et al. (2010). Bilateral *distances* and information on *colonial ties* come from the CEPII distance database.⁷ Estimates of *population size* originate from UNPD population statistics.⁸ 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 S1.

Estimation Strategy

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

⁷Distances 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).

⁸UNPD statistics: <http://esa.un.org/unpd/wpp/index.htm>.

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

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 either result from reverse causality (when changes in migration flows lead to changes in visa policy) or from omitted variables (if there are unobserved factors that simultaneously affect visa policy *and* migration flows) 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 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 ($H_0: cov(P, \varepsilon) = 0$) on a 1 percent level ($p = 0.007$). This shows the need for an IV estimation method.

Our IV on the affinity of voting behavior of UN member states in the UN General Assembly captures the unobserved heterogeneity in the quality of bilateral relations which would otherwise be attributed to visa policies. The data for the variable *UN voting affinity* scores stem from the United Nations General Assembly Voting database (Strezhnev and Voeten 2013). UN affinity scores use binary data on approval or disapproval of an issue and range from -1 (least similar interests) to 1 (most similar interests).⁹ This binary and time-varying variable z_{ijt} is a valid instrument when the exclusion restriction ($cov(z, u) = 0$) holds, and is relevant when it is correlated with the endogenous explanatory variable ($cov(x, z) \neq 0$ with $x = visa$).

⁹The 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).

At the first stage, our two-stage least-square (2SLS) IV regression analysis estimates visa policy on a basis of our set of exogenous explanatory variables and the additional instrument z_{ijt} .¹⁰ Estimates reported in Table 2 show that our instrument *UN voting affinity* is relevant ($p = 0.000$) and passes the *F*-test on weak instruments. Additional to the policy values predicted at the first stage, our IV regression includes bilateral, origin- and destination-specific and time-variant control variables. 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.

RESULTS

Table 2 reports the estimated effects of travel visa requirements on bilateral migration flows. No matter whether visa policy is instrumented, the visa variable has a statistically significant effect on all migration variables. Yet 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.¹¹ 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 the overall circulation (“turnover”) to a similar extent as annual net inflows, that is, by about 26 percent.

The instrumented estimates (5)-(8) show that these estimates are (downward) biased. Visa policy instrumented by the UN voting similarity index significantly *increases* the estimated magnitude of the “visa effect” on migration. 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 results in a strongly negative effect on the overall circularity or “turnover” within

¹⁰In a two-stage least-square (2SLS) regression, an endogenous covariate in a regression model is regressed on all the exogenous variables in the model including one (or more) instruments. At the second stage, the endogenous covariate in the regression model is replaced with the predicted values of the endogenous variable from the first stage.

¹¹Estimates in this log-transformed model are interpreted as (semi-)elasticities, which implies that a change in the binary visa policy variable results in a $[e^\beta - 1] * 100$ percentage change in the migration flow variable.

TABLE 2 (CONTINUED)
 VISA POLICY EFFECT ON BILATERAL MIGRATION FLOWS, 38 DESTINATION COUNTRIES, 1973–2012

Dependent variable: log of annual: Estimator	Inflow FE	Outflow FE	Turnover FE	Net flow FE	Inflow IV	Outflow IV	Turnover IV	Net flow IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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 ² (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** (0.000)	-0.271** (0.000)	-0.271** (0.000)	-0.269** (0.000)
F-test					40.92	43.34	43.13	40.37

Notes: In IV regressions, visa variable has been instrumented 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. Standard errors in parentheses: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

bilateral corridors. We estimate the average turnover to be about 75 percent lower in visa-restricted corridors. The effect on net inflows (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 reduces not only inflows but also outflows and, hence, overall circulation. The estimates strongly indicate that visa policies affect 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 some of the time-varying unobserved heterogeneity across dyads is captured by our UN voting similarity measure, we are confident of a systemic visa policy effect which 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 increase migration. Yet high income growth rates in origin countries increase emigration, which might provide indirect evidence for “transition theories” (Zelinsky 1971; Skeldon 2012) which hypothesize that rapid growth and concomitant socioeconomic transformations can paradoxically boost emigration. Discrepancies in political rights and civil liberties between origin and destination countries seem to increase migration flows. Population size also shows the expected significant and positive signs.

If migration restrictions decrease circularity, we should see that migration becomes less responsive to changes in economic conditions in origin and destination countries if visas are introduced because of increasing costs and risks of migrating. For instance, if migrants become unemployed, they are less likely to return if there is a risk of not being able to re-migrate because of migration restrictions. To investigate the responsiveness of migration to economic fluctuations, we interact GDP growth rates in both origin and destination countries with the binary travel visa policy variable, keeping everything else constant.¹² Table 3 reports relatively strong effects of economic growth cycles on migration to and from destination countries if mobility is not constrained by visa requirements. In a visa-free corridor, a decrease in a destination’s growth rate by one percent-

¹²In 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).

age point also decreases migration inflows by about 1 percent and increases outflow (including return) by about 2 percent on average. This corroborates the idea that free migration flows are strongly related to business cycles and labor demand.

However, such “market mechanisms” are either fully (for inflows) or largely (for outflows) neutralized when visa policy restrictions are introduced. This shows that visa barriers tend to drastically reduce the responsiveness of migration to economic conditions and fluctuations in destination countries. For instance, a one percentage point increase in an origin countries’ growth rate increase return outflows by about 1 percent on average in visa-free corridors, but only by about 0.5 percent in corridors which are visa-constrained; that is, 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 countries.

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

TABLE 3
VISA POLICY AND ECONOMIC CYCLES: SOME INTERACTION EFFECTS (1973–2012)

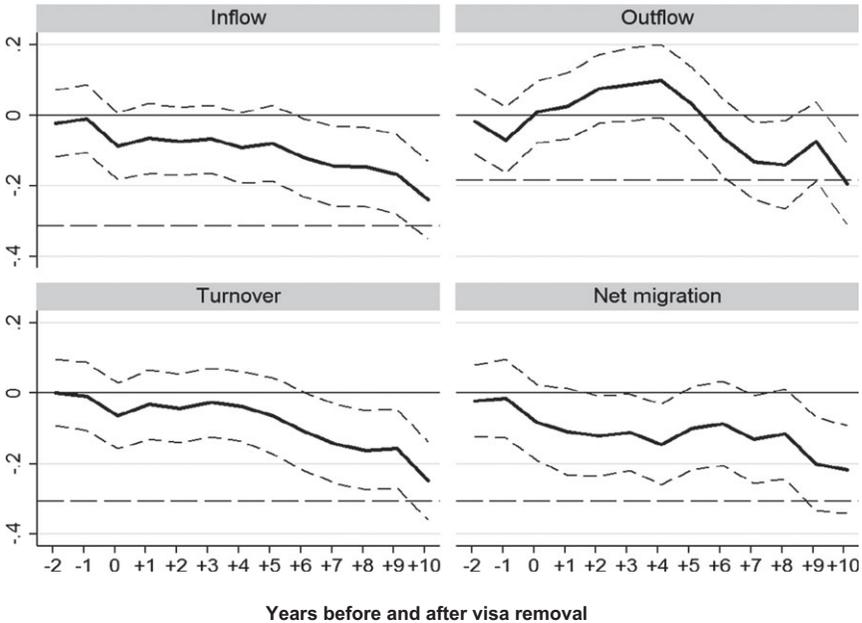
DV: log of flow	(1) Inflow	(2) Outflow	(3) Turnover	(4) Net migration
Visa	-0.297** (0.039)	-0.188** (0.037)	-0.295** (0.040)	-0.257** (0.041)
GDP growth (dest)	0.009** (0.003)	-0.019** (0.003)	-0.000 (0.003)	0.029** (0.004)
Visa × GDP growth (dest)	-0.008* (0.003)	0.009* (0.003)	-0.004 (0.003)	-0.025** (0.004)
GDP growth (origin)	0.003 (0.002)	0.009** (0.002)	0.004* (0.002)	-0.001 (0.002)
Visa × GDP growth (origin)	-0.001 (0.002)	-0.004* (0.002)	-0.001 (0.002)	0.001 (0.003)
Income gap (dest – origin)	0.017** (0.005)	0.015** (0.004)	0.020** (0.005)	0.009+ (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 ² (within)	0.19	0.15	0.19	0.14
Number of dyads	3,290	3,290	3,290	3,254

Notes: GDP growth variables are lagged by one year. Visa policy is not instrumented in this analysis. Robust standard errors in parentheses: ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

decrease overall circularity. However, this does not yet provide an adequate estimate for the effect of *changes* in travel visa policies through the introduction or removal of visa requirements. After all, because visa regimes are relatively stable, as visa requirements do not change very often. The above results therefore largely represent average differences in bilateral migration flows between visa-constrained and visa-free corridors rather than dynamic visa policy effects affecting migration over time. 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 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 introduction adjustment processes after a visa policy change has been introduced. This procedure also enables us to assess possible *asymmetric policy effects* by analyzing whether the effects of *introductions* and *removals* of travel visa requirement mirror each other, or whether these are substantially different.

Figure I (and Appendices S2 and S3) displays 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 publicly announced well in advance. This may be different for general border closures which are often well-known beforehand and which people can anticipate, for instance around independence of countries (cf. Vezzoli 2015). Concerning the post-introduction period, we find that it takes a relatively long time before this effect of visa introductions on immigration and emigration becomes significant. Although inflows already go down in the same year when a visa is introduced, it takes more than five years until numbers decline in a statically significant way. After 10 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

Figure I. 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 10 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 percent confidence interval.

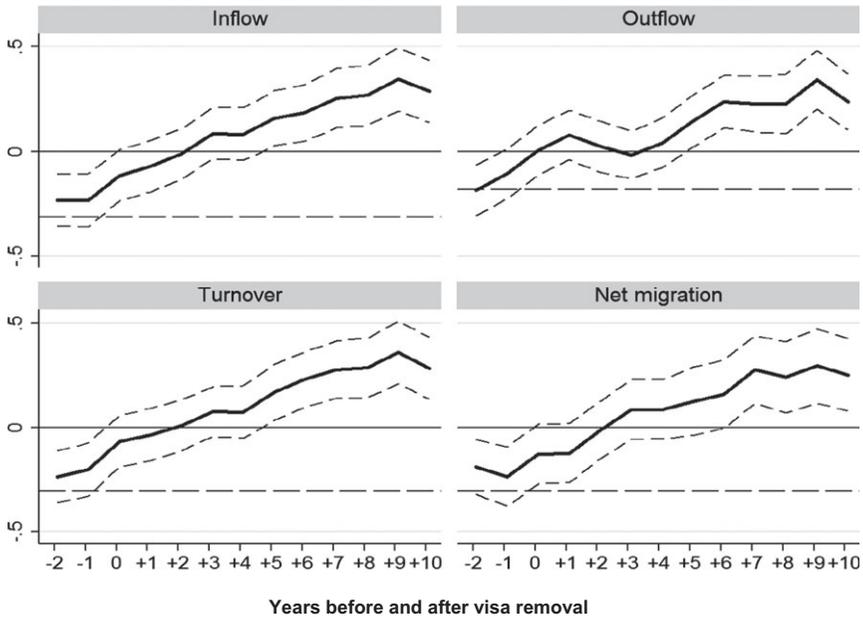
corridors. While emigration slightly increases after visa introduction, it takes almost six to seven years until outflows have declined significantly.

These protracted visa introduction effects on in- and outflows are similarly reflected in the effects on overall circulation (turnover) and net migration. A theoretically plausible explanation of these delayed and partial effects of visa introductions is that migrant networks tend to facilitate migration across formally closed borders by decreasing the costs and risks of migrating, for instance through shifts from labor to family migration or irregular migration channels. This migration-facilitating role of transnational social ties “cushions” the effect of migration restrictions and explains why strong and immediate shifts in the volume or direction of migration often do not occur. It is therefore only on the longer term that we may generally expect policy effects to take hold. This can also explain that such post-visa introduction effects are only partial, and remain smaller than the average effect of visa-restricted corridors. For instance, family

migration through networks can explain that migration over formally closed borders continues for decades.

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 three years (Figure II). 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 toward 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. Although we might hypothesize that, after this tipping point, they may decrease to long-year averages, we cannot test this with the available data methodology.

Figure II. 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 10 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 percent confidence interval.

This “over-shooting effect” of migration after the removal of travel barriers may be explained by three factors. First, visa removals enable people who already had a desire to immigrate, but considered it too difficult, dangerous or costly, to migrate. Such a release of “latent migration desires” also seem to exist in other migration policy domains, such as the temporary emigration surges after the removal of emigration restrictions in formerly Communist countries in Central and Eastern Europe. Second, the removal of visa restrictions may motivate some people to seize the opportunity out of “now or never” considerations based on fears that the more liberal mobility regime may not persist for a long time — as was the case when Turkey, for instance, reintroduced visas for Bulgarian citizens in 1989 or in the case of Ecuador, where within six to 18 months after the introduction of universal freedom in 2008, visa requirements were reintroduced for citizens from China, Afghanistan, Bangladesh, Eritrea, Ethiopia, Kenya, Nepal, Nigeria, Pakistan, and Somalia (Acosta Arcarazo and Freier 2015). The fact that we did not find such inter-temporal substitution effects for visa introduction suggests that such effects are more relevant for visa removals. Third, such “overshooting” effects may be reinforced when “pioneer migrants” who left immediately after the visa removal are followed subsequently by family members and other “network migrants” whose move is facilitated through social contacts and information provided by earlier migrants.

Visa removals are also likely to encourage reverse flows (proxied by emigration) within the same bilateral (country-to-country) corridors. Although this variable measures the departure of citizens from a particular origin country irrespective of their destination, and some may move on to another destination country, it is safe to assume that most return to the origin country (see Vezzoli, Villares-Varela, and Haas 2014). This “overshooting” effect of visa removals on emigration can be explained in similar ways as the respective effect for immigration. First, the removal of visa requirements may neutralize fears among those migrants who already had a latent wish to return, but did not do so out of fear of not being able to travel back to visit family and friends in the destination country or to re-migrate if the return is not successful. 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.

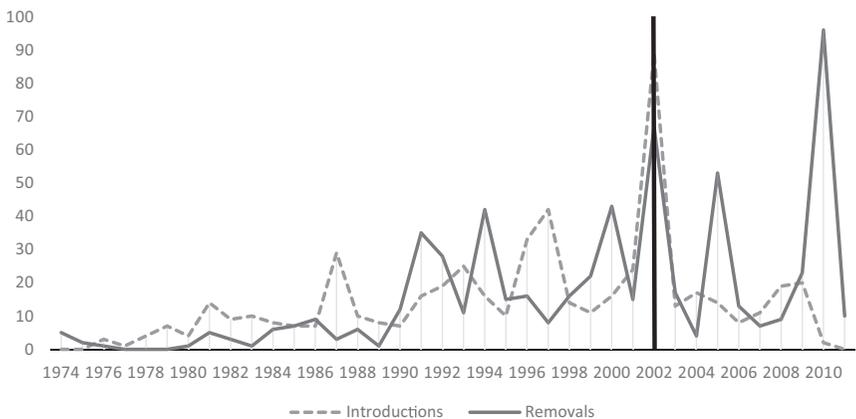
Measuring the Difference-in-difference Effect of Visa Introduction and Removal

To further investigate the existence of *asymmetric visa policy effects*, we analyzed whether the removal or introduction of visa requirements has significantly different effects on migration compared to counterfactual situations in which there has been no visa policy change.

To perform this type of analysis, we analyze only those countries in which a policy change has occurred in 2002. This is the year in which for all 38 countries under consideration the highest number of visa introductions (89) and removals (67) have occurred since 1974 (Figure III). Each bilateral dyad which is 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” groups of country dyads in terms of changes in migration outcomes M over time relative to the outcomes observed before policy change occurred.

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

Figure III. Visa Introductions and Removals Since 1974 (38 Destination Countries).



As 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 combine the DID estimation with a propensity score matching (PSM) to match control and treatment units on pre-intervention characteristics. 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.¹³ Propensity score matching (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 :¹⁴

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

We use nonparametric 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 (see 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.

On the other hand, the *introduction* of a visa requirement has a less straightforward effect on migration (see Table 5). While gross and net inflows are negatively affected, outflows and overall circulation show non-

¹³Unlike 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.

¹⁴Rosenbaum and Rubin (1983) show that, under certain assumptions, matching on $P(X)$ is as good as matching on X .

TABLE 4
VISA REMOVAL EFFECT: DIFFERENCE-IN-DIFFERENCE ESTIMATION WITH PROPENSITY SCORE MATCHING

Visa Removal	Inflow		Outflow		Turnover		Net flow	
	Before (1)	After (2)	Before (3)	After (4)	Before (5)	After (6)	Before (7)	After (8)
No	268.92 (45.58)	377.24 (34.27)	42.74 (19.79)	54.24 (13.83)	216.71 (67.20)	282.70 (46.81)	128.82 (41.56)	173.14 (29.00)
Yes	396.47 (49.54)	786.89 (30.74)	160.87 (18.09)	270.24 (11.93)	479.47 (62.51)	1102.58 (40.92)	141.24 (38.66)	542.80 (25.31)
Δ	127.55* (67.32)	409.65*** (46.03)	118.13*** (26.81)	216.01*** (18.27)	262.76*** (91.78)	819.88*** (62.18)	12.42 (56.77)	369.66*** (38.50)
Δ in Δ	282.10*** (81.56)		97.88*** (32.44)		557.12*** (110.86)		357.24*** (68.59)	

Notes: 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. Means and standard errors are estimated by linear regression. Standard errors in parentheses. Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

TABLE 5
 VISA INTRODUCTION EFFECT: DIFFERENCE-IN-DIFFERENCE ESTIMATION WITH PROPENSITY SCORE MATCHING

Visa Intro	Inflow		Outflow		Turnover		Net flow	
	Before (1)	After (2)	Before (3)	After (4)	Before (5)	After (6)	Before (7)	After (8)
No	261.68 (39.52)	437.58 (26.84)	154.06 (15.74)	153.49 (10.67)	390.27 (40.03)	478.03 (28.06)	84.50 (16.45)	176.12 (11.66)
Yes	62.40 (38.67)	90.82 (25.79)	16.63 (15.40)	28.717 (10.15)	73.37 (39.27)	109.50 (25.88)	40.11 (16.14)	52.07 (10.64)
Δ	-199.28*** (55.29)	-346.76*** (37.22)	-137.43*** (22.02)	-124.77*** (14.72)	-316.89*** (56.07)	-368.52*** (38.18)	-44.40*** (23.05)	-124.05*** (15.79)
Δ in Δ	-147.48*** (66.65)		12.66 (26.49)		-51.63 (67.84)		-79.66*** (27.93)	

Notes: 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. Means and standard errors are estimated by linear regression. Standard errors in parentheses. Inference: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

significant differences before and after a restrictive visa policy intervention. Yet these findings corroborate our earlier finding on the delayed effects of restrictive migration policy change, which may be explained by the migration-facilitating function of migration networks which tend to reduce the effectiveness of policy restrictions.

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 on immigration ignores the effects of policies on reverse flows and overall circulation. This is reflected in the lack of adequate empirical test 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 possibility that the introduction and removal of restriction may have different effects and are not necessarily mirroring each other.

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 analyzed 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 these data do not allow for the analysis of actual migration behavior at the micro-level, our macro-level findings seem to be in line with evidence from numerous prior 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 high responsiveness of (unrestricted) migration to economic growth virtually down to zero. Visa requirements thus largely neutralize business cycle effects. Taking into

account the close association between economic growth and the level of immigration in visa-free corridors, this indicates that, besides interrupting circulation and encouraging long-term settlement, visa restrictions severely reduce the responsiveness or “elasticity” 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 has an almost immediate effect. After the introduction of a visa requirement, levels of immigration only go down gradually. Even 10 years after the introduction of visas, 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 migrant 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 “inter-temporal substitution effects” upon visa removal, whereby people involve in a “now or never” migration because they may fear reintroduction of migration restrictions. Such inter-temporal substitution may also interact with the release of a “latent” migration potential, in which people who were in situations of “involuntary immobility” (cf. Carling 2002) seize the opportunity once it occurs. It would be useful to further investigate these hypotheses 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 other major reforms of immigration policies and border regimes, such as the introduction of migration restrictions that have often accompanied decolonization.

In sum, this paper found robust evidence for the hypothesis that the immigration-reducing effect of immigration restrictions is to a significant extent undermined by its emigration-reducing effect through discouraging the return of migrants, decreasing overall circulation and encouraging long-term settlement. Another related counter-productive effect may be that visa requirements partly neutralize business cycle effects, which makes migration much less responsive to economic growth. While during periods

of economic growth visa restrictions may hinder migrants to come, during economic downturns visa restrictions may discourage migrants to return.

Future analyses should also test of the effects of other policy measures to gain a more comprehensive picture of the role of policies in migration processes. For instance, because of data limitations this 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 toward other destination countries (spatial substitution). Such effects may further undermine the long-term effectiveness of immigration restrictions, and although some evidence exists on these (Czaika and Hobolth 2016), they deserve to be further investigated in future analyses. However, this paper unequivocally shows that visa restrictions significantly reduce the circulation of migrants. This highlights the importance of considering the impact of migration policies on both inflows *and* outflows. The common, one-sided focus on inflows prevents us from gaining comprehensive insights into how policies affect migration processes.

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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of this article at the publisher's web site:

Appendix S1. Descriptive statistics, definitions, and sources of main variables.

Appendix S2. Visa introduction and migration flows (1973–2012).

Appendix S3. Visa removal and migration flows (1973–2012).