Development Aid & International Migration to Italy: Does Aid Reduce Irregular Flows?

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Abstract

In recent years, donors have claimed to tackle the root causes of migration from low-income countries using aid. While others have studied the effects of aid on regular migration, we test whether aid deters irregular migration to Italy using two innovative dependent variables: asylum applications and apprehensions at border. For asylum applications, the largest significant effect size implies we should expect one extra application for an additional $162,000 in bilateral aid. For border crossings, the only significant effect implies the marginal cost in bilateral aid is $1.8 million per deterred migrant. The conclusion that effect sizes are small is robust to different types of aid, measures of migration and various controls. We find robust evidence that irregular migration flows are significantly affected by conflict, poverty, and the pre-existing stocks from that country. Comparing our results to the existing aid-migration literature, we find similar effect sizes. The cost per deterred (regular) migrant is in the range $4-7 million. Statistically significant estimates for the effect of aid on regular migration are only found for sub-samples or specific specifications. In short, aid does not deter regular or irregular migration, so should be used for other purposes.

Keywords: Aid; Migration; Irregular migrants; containment development.

1 Introduction

The displacement crisis of recent years is unprecedented: the UNHCR (2017b) estimate that a record 65 million people were forced to leave their homes because of violence, conflict or natural disasters in 2016. Europe has seen some of the effects of this, with a record number of arrivals in 2014 and 2015 (Eurostat, 2017) and more than 10,000 lives lost in the attempt to cross the Mediterranean Sea (IOM, 2018). Amongst several policy responses, donors have announced large development aid packages as an attempt to address the 'root causes' of migration. This policy response has a long history (Carling and Talleraas, 2016), even if the scale of displacement is new. One recent example of high profile earmarked funds is the *European Union Emergency Trust Fund for stability and addressing root causes of irregular migration and displaced persons in Africa* (EUTF for Africa). The European Commission's (2016a) stated aim is to "...invest in long-term economic and social development, improving people's life and tackling the drivers of migration". With over 4 billion euro already committed this is a major initiative, but represents only part of a consistent strategy (Latek, 2016).

This study asks whether that policy lever is effective, specifically: has development aid reduced irregular migration to Italy? We only use one destination as there is limited comparability of national migration statistics (Raymer et al., 2013) and potentially large problems of double counting. There are three main entry points to Europe: Italy, Greece and Spain. We have chosen to study Italy as it has higher levels of irregular immigrants than Spain, and much more consistent flows than Greece (which saw a large spike in 2015). Two dependent variables are used, both of which capture flows: the number of asylum applicants and the number of apprehensions at border. By using both we are able to test the robustness of insights. A parsimonious set of controls is used in panel data for 147 countries over 14 years, with a now standard structural equation model approach to control for endogeneity.

A number of studies have examined the link between aid and migration, often expressing skepticism of any ability to reduce migratory flows (Clemens and Postel, 2018; De Haas, 2007). However, three recent empirical papers find a negative link, with more aid meaning lower immigration, in general (Lanati and Thiele, 2018), for rural aid specifically (Gamso and Yuldashev, 2018a) or for governance aid specifically (Gamso and Yuldashev, 2018b). On the other side, Berthélemy et al. (2009) found a positive effect of aid on migration stocks below a critical income threshold of PPP$ 7,300 and Menard and Gary (2018) confirmed an increase for specific donor-recipient pairs in the short-run.

Until recently, this empirical evidence has focused upon regular migration. This limitation has often been blamed on poor data availability. However, the expressed goal of aid is typically to reduce irregular migration, and this paper is able to judge aid according to its own criteria. Furthermore, irregular migration is far from trivial. For the last three years of data available (2014-16), inflows of regular migrants to Italy have numbered approximately in the range 230,000-250,000 (OECD, 2017). For irregular migrants, the range has been approximately between 150,000 and 180,000 over the same period (UNHCR, 2018; Frontex, 2018). In other words, irregular migration is both the type of migration that policy makers wish to discourage using aid, and important in terms of sheer scale. The only exceptions we are aware of to the broad focus on regular migrants are Vogler and Rotte (2000) and recent papers from Dreher et al. (2019) and Murat (2020). Vogler and Rotte (2000) look at determinants of asylum seekers to Germany, using bilateral aid as a proxy for bilateral contacts. Dreher et al. (2019) and Murat (2020) use data from UNHCR to measure 'refugee inflows' and asylum applications, respectively. However, there are a number of weaknesses that do not appear to have been overcome, discussed in greater detail in section 2 and 3.

Our main finding is that any effect of aid on irregular migration is small, and tends to be insignificant. In the baseline specification, the positive relationship between bilateral aid and asylum applications is found significant at the 5% level. However the effect size is small (a 1% increase in bilateral aid is associated with a 0.017% increase in asylum applications), and the result is not robust to other estimators. The effect of aid on border crossings is not consistently estimated to be either positive or negative, but is consistently small or not significant. We find the main drivers of irregular migration to Italy include the migrants' network at destination, income at origin, population and the presence of violent conflict at origin (this echoes the qualitative evidence, see Crawley et al., 2016). We also find that migration flows have a significant influence on the amount of both bilateral and total aid a country of origin receives: if endogeneity is not completely dealt with there will be a positive bias.

The remainder of this paper is composed as follows: section 2 provides a brief review of the literature on aid and migration; section 3 outlines the empirical approach and data; section 4 presents the results, with discussion and conclusion found in sections 5 and 6.

2 Theoretical Background and Literature Review

Why might we expect aid to influence the number of immigrants a country receives? From a theoretical perspective there are three main channels. First, neoclassical theories posit that the main driver of migration is the income differential between sending and receiving countries (Borjas, 1989). Thus if aid is able to reduce this differential by improving the incomes of those in recipient countries, aid would reduce the attractiveness of migration and so reduce the number of immigrants (Böhning and Schloeter-Paredes, 1994). This simple theory is closest to that espoused by politicians in their explanation of using aid to address the 'root causes'.

Second, theories of the migration transition recognise that migration has costs as well as benefits, thus leading to a non-linear relationship with economic development (Skeldon, 1997). The poorest are unlikely to be able to afford to migrate as they lack the necessary resources, while the richest are not attracted because of small income differentials. So aid at specific levels of income may contribute to loosening budget constraints enough to cause an increase in migration (De Haas, 2007). The turning point has been estimated in the range of $6,000 to $8,000 per capita in PPP (Clemens et al., 2014; Dao et al., 2018), with increased economic growth below that point expected to increase emigration.

Third, while the two proceeding theories essentially imply that aid has no differential effects depending upon who gives the aid, theories of the migrant network argue that bilateral ties cause greater migration (Massey, 1990). So, bilateral aid might reinforce these ties and cause greater migration from an aid recipient to that specific aid donor (Berthélemy et al., 2009). The channels for this may be in a general positive impression of a country, or in specific contacts made during the course of an aid project.

With these theories in mind, we now turn to the empirical evidence. The most important recent paper on the effect of aid on migration is Berthélemy et al. (2009), who find positive impacts on immigrant stocks for both bilateral aid and total aid. Specifically, a 10 percent increase in bilateral or total aid is associated with increases in migrant stocks of approximately 3 and 1.5 per cent respectively. This result comes from a gravity model covering 22 donors and 187 sending-countries for the year 2000. Using a simultaneous equation model, Berthélemy et al. (2009) attempt to correct for the endogeneity of aid by estimating its allocation as a function of GDP, population and institutional development at origin as well as bilateral trade and historical links. While a great step forward, the chosen migration variable is problematic as it measures stocks of regular migrants. They estimate the effect of aid (over 5-10 years) on immigration and emigration decisions that took place over a much longer time period. This problem is compounded because of endogeneity concerns. Bermeo and Leblang (2015) report that immigrant stocks are positively associated with aid allocation (possibly as immigrants are able to lobby for aid to their home country). Unless the effect of migrant stocks on aid allocation is controlled for, any estimate of the effect of aid on migration would be positively biased due to omitted variable bias. Given aid is found to have a positive effect, we are left to wonder whether this estimate is affected by the endogeneity of aid.

Menard and Gary (2018) also find a positive result of aid on migration, having dealt with some of the weaknesses of Berthélemy et al. (2009). They use data on migrant flows over 2000-2010, and add a third equation for trade flows. They find a 1% increase in aid is associated with a 0.18% increase in migration. This suggests that using flows as a dependent variable makes the positive effect of bilateral aid weaker: it is about 60% the size of Berthélemy et al.'s (2009) estimate. Menard and Gary (2018) also find a large effect of migration on aid: a 1% increase in migration is associated with a 0.72% increase in bilateral aid. This highlights the challenge of controlling for migration's effect on aid, as the reverse causality effect may be larger than the relationship of interest. The estimate of aid's effect on migrant flows may still be upwardly biased as the authors do not control for migrant stocks, which is likely to be positively associated with both aid and migration flows. Furthermore, Menard and Gary (2018) do not consider the effect of total aid, only bilateral aid.

The different effects of bilateral and total aid on migration are shown by Lanati and Thiele (2018). Using the standard 3SLS estimator, they show a small positive effect of bilateral aid on migration is completely offset by a larger negative effect of aggregate aid. While they emphasise the significance of these effects, the size is also important. When including year and country fixed effects, Lanati and Thiele (2018) report that a 1% increase in total aid, averaged over the three previous years, is associated with a reduction in emigration rates to OECD destinations of around 0.1%. No summary statistics are presented, but this appears to be a reduction of only around 0.004 standard deviations. Consistent with the above, their results show that migrant stocks (proxying a migrant's network) is a more robust determinant.

Whilst all of the papers above use regular migration, Dreher et al. (2019) address the specific case of refugees. They use data from the United Nations High Commissioner for Refugees (UNHCR) to examine the determinants of (positive) changes in the stocks of refugees for 141 origin countries over the 1976-2013 period, regardless of where they now reside. To account for reverse causality, the authors rely on instrumental variables. They use a recipient country's probability of receiving aid and government fractionalization as instruments, the latter measuring the fractionalization in the ruling coalition of a country in terms of party affiliation. They argue that greater fractionalization is positively correlated with the overall level of government spending in general, and provides exogenous variation in the amount of aid disbursed. Their findings suggest that aid has no significant impact on overall refugee stocks in the short term, but exerts a negative effect in the long run (after eleven years). When considering only OECD destinations, there is a positive short-run effect and the same negative long-run effect. When disaggregating aid into different types, humanitarian aid is reportedly more effective in reducing refugee flows in the short run: aid reduces refugee outflows as long as the share of humanitarian aid exceeds 9% of total ODA receipts.

Murat (2020) is another recent paper that moves away from a focus on regular migration. Using data on the flows of asylum seekers to many OECD destinations between 1993 and 2013, she estimates the effects of bilateral aid, and its interaction with sending country's GDP. Using system-GMM, there is a negative association between bilateral aid and asylum applications from poorer countries (a GDP per capita of less than $787 at 2005 prices). For those countries, a 1% increase in bilateral ODA is associated with a 0.05% decrease in asylum applications in the following year. There is mixed evidence over a longer time horizon, and for countries with a higher GDP per capita. There is no effect on regular flows.

How do we reconcile the literature's divergent findings? It is clear that the most positive effects are found when using migrant stocks as the dependent variable, or when excluding migrant stocks as a control. Such estimates are likely to be positively biased, given the endogeneity discussed above. Negative findings are found when including a broader set of controls, and using migrant flows as the dependent variable. Whilst these negative effects are occasionally found to be significant, they are invariably small.

Standing back, research into migration and aid has a common set of problems. Alongside persistent concerns about endogeneity, research into the effects (if any) of aid on migration is limited by data quality. There are no consistent and accurate data that measure the flow of irregular migrants by year, 'sending' country, any transition countries and destination country. Research in this area then faces a trade off. Dreher et al.'s (2019) approach is to use a dependent variable that is problematic (as discussed in section 3; the measure is constructed from stocks which are influenced by many factors such as historical migration and changes in immigration status) but has wide coverage: they sum all migrants with asylum status across all destination countries. This multi-destination approach is also taken by Murat (2020). We argue that our dependent variable more accurately captures the stated aim of donors (reducing flows of irregular migrants), but for a single gateway European country. This decision means we cannot detect any effects for irregular migrants taking other routes to Europe, or migrating to other continents. However, we are able to judge whether aid is successful according to the criteria that donors are most interested in, for an important and representative entry point. In part, differences stem from different goals and the solution to problematic data is often to use more than one source in order to gain a more accurate picture: as such we view our paper as complementary.

3 Empirical Approach

We test for the effect of aid on irregular migration using panel data of dyadic aid and migration flows between Italy and sending countries. The panel dataset covers 147 origin countries for the period 2003-2016 (see Table A1 for the list of sending countries, which is restricted to eligible recipients of aid). The empirical specification is based on a basic gravity model for migration (Vogler and Rotte, 2000), with the inclusion of aid from Italy and all other donors. The resulting empirical specification is:

$$\begin{aligned} MIGRATION_{it} = {} & \beta_1 ItalyODA_{it-1} + \beta_2 OtherODA_{it-1} \\ & + \beta_3 X_{it-1} + \beta_4 Z_{it-1} + \mu_i + \pi_t + \varepsilon_{it} \end{aligned} \tag{1}$$

Where *Italy ODA* is aggregate bilateral ODA from Italy; *Other ODA* is the total aggregate ODA received by the origin country excluding bilateral aid from Italy; X is a vector controlling for variables related to the country of origin; Z is a vector controlling for factors related to the selected destination, such as bilateral immigration policies. Subscripts i and t are for the country of origin and year respectively, with fixed effects denoted by μ and π. Given the focus on one destination only, a first concern is related to multilateral resistance (Bertoli and Moraga, 2013). Considering only dyadic variables would ignore the confounding influence that other destinations' attractiveness exerts on bilateral migration, thus leading to biased results (Hanson, 2010). However, year fixed effects absorb unobserved changes on alternative migration routes in any given year. In addition, we perform a specific robustness test including migration flows to other relevant destinations.

Other potentially important sources of endogeneity are home country conditions, bilateral links and reverse causality. *Home country conditions* refer to factors such as GDP per capita, a recent civil war or a natural disaster which would simultaneously make a country more likely to receive aid and make its citizens more likely to emigrate. If this is not controlled for, it may appear that there is a link between total migration and aggregate aid because they are both affected by such factors. *Bilateral links* relate to a specific dyadic link, such as the stock of migrants from a given country. This set of factors could positively influence both bilateral migration (as migrant stocks tend to attract further flows, due to network effects) and bilateral aid (Bermeo and Leblang, 2015). *Reverse causality* refers to the fact that a donor may disburse more aid to countries that send more migrants, or it might reduce assistance to the same countries punishing the lack of cooperation at stemming outflows.

Our strategy is two-fold. First, we include a series of robustness checks that include variables that might affect the aid-migration relationship. Second, to address endogeneity we adopt the now standard approach of Berthélemy et al. (2009) in using a structural equation model. We include two variables of interest: aid from Italy, and aid from all other donors. As we have no reason to think aid from other donors will be 'less' endogenous than Italian aid, we include each aid variable as a separate equation. Whilst most other literature recognizes the endogeneity of bilateral aid (e.g. Berthélemy et al., 2009; Lanati and Thiele, 2018; Menard and Gary, 2018; Murat, 2020) it is surprisingly rare for other aid to be considered as such. Because Italy is a main gateway to other European destinations, other donors are likely to be influenced by the migrant outflows from a country when allocating their aid.

Murat (2020) takes an alternative approach to tackling endogeneity by employing a system-GMM model. If the instruments are valid, this is able to provide unbiased estimates of the relationship of interest. Unfortunately, this does not appear to be the case. Hansen's J provides a test of instrument validity: a high p value is needed to reassure the researcher that the (often numerous) instruments are valid. As Roodman (2009, p.142) points out, even p values of 0.25 "should be viewed with concern". In Murat's (2020) core estimates (table 1), p values range from 0.068 to 0.198. Of the twenty results in the body of the paper, nine reject the null of correctly identified instruments at the 10% level. Further, only two of the twenty relevant p values are higher than 0.25, meaning most of the results should be viewed with concern. It is possible that the specification is at fault rather than the instruments per se, as an inability to convincingly pass the J-test could be caused by omitted variables. Murat (2020) does not include variables on conflict or colonial links, which could effect both aid and migration. Further, only bilateral aid between a given destination and source country is included. That will likely ignore the majority of aid, potentially biasing results and causing the problems in the Hansen test.

These results are particularly concerning, as it is understandably rare for researchers to present their least convincing results, and there are many possible specifications. There are some arbitrary decisions with regards the included regressors. For example, in a robustness check bilateral aid is included with lags of up to six years. A significant effect of aid on migration is found for the one-year and six-year lags (for low income countries), but it is not clear whether a specification with five-year or seven-year lags would find any significant effect of aid on migration[1]. The problem of 'researcher degrees of freedom' is compounded by the decisions over which instruments to use in GMM. Over 1,000 instruments are used by Murat (2020), but it is not clear which ones, for example the number of lags that were used. Murat's (2020) table 1 shows that the null hypothesis of no second-order serial correlation in disturbances (the standard Arellano-Bond test) is strongly rejected (p=0.002) in each case. The third-order test is sometimes significant at 10%. Depending on the lags used, the moment conditions and subsequent results may not be valid. Given these concerns, we do not use GMM as a method: the leading exponent of this approach does not appear to pass the relevant tests for instrument strength, and may not pass the relevant tests for autocorrelation.

3.1 Irregular Migration to Italy

The term 'irregular migrants' is used to refer to all the migrants that have reached Italy without having previously fulfilled immigration policy requirements. This includes all new irregular arrivals, whether or not they subsequently prove a legal right to obtain international protection, and it excludes non-geographical flows, such as new births and changes in the legal status. We focus on Italy because it is the port of entry for all migrants travelling on the Central Mediterranean Route (CMR). Figure 1 plots

[1]Many thanks to an anonymous referee for this point.

the numbers of border crossings for major European routes. In every year the Greek and Italian ports are the two main entry points. However, the Syrian crisis led to abnormally large inflows into Greece in 2015, making modelling that entry point more difficult. As such, focusing on Italy means focusing on one of the two main European gateways, and one without an obvious outlier. Averaging over the period, Greece accounted for 43% of yearly arrivals to Europe since 2009, while Italy accounted for 37% (using data from the European Border and Coast Guard Agency; see Frontex, 2018).

Figure 2 shows that regular flows to Italy have decreased since a high in 2007. This mirrors an increase in irregular flows, using either measure. This shows the value of directly investigating irregular migration: it has a different trend, and any effects of aid are unlikely to be inferred from previous studies that use regular migration.

There are practically only two ways in which asylum seekers can currently reach a European country: resettlement and undocumented migration. The former has become increasingly rare in EU states, with less than 30,000 resettled refugees in 2015 and 2016 combined (UNHCR, 2017a). This is dwarfed by the numbers claiming asylum: there were 1,255,600 first time asylum applications in 2015 alone (Eurostat, 2016). In response to the difficulties in using legal routes (Schuster, 2011), current refugees adopted a strategy of entering illegally before regularizing their status. In Italy between 2008 and 2016, less than 1% of refugees or those with international protection permits came into the country legally (i.e. through resettlement schemes). In other words, practically all of today's refugees were yesterday's irregular migrants. Over the same period, slightly under half of those claiming asylum were deemed eligible (Eurostat, 2017). We can infer from this that the category of asylum applicants is approximately equally split between future refugees and 'economic migrants'.

3.2 Description of Dependent Variable

The main innovation in this paper is using the flow of irregular migrants as our dependent variable, rather than the alternatives of changes in stocks of regular migrants or refugees. We use two such measures. First, from the UNHCR *Populations Statistics Database* we use the annual number of asylum applications lodged in Italy from 2003 to 2016. Second, we use the annual number of illegal crossings detected along the Italian segment of Europe's external border from the European Border and Coast Guard Agency (Frontex, 2018). The data record arrivals, and are available for 2009-2016.

Both variables have weaknesses. First, they are liable to over-counting as a single individual could apply for asylum and/or attempt border crossing multiple times. As far as asylum applications are concerned, the likelihood is limited as additional applications are not recorded if the first application has been lodged in the same reference period (mainly the calender year). Moreover, the EURODAC regulation establishes a database of fingerprints of asylum seekers and irregular migrants entering the Schengen area so that every new application can be verified against already existing data. In the case of border apprehensions we minimize the problem by focusing on the EU external border only, which excludes flows within the European Union. While aggregate numbers at the European level might be problematic as it is not clear how they relate to data published at the national level, this is not a problem if only the country level is considered (i.e. one destination). This is because an individual detected crossing into Italy might manage to continue his journey to be apprehended again in France. Thus figures at aggregate European level can be inflated by double counting, but our Italian data replacedareis less affected by the issue.

Second, the opposite problem may be at work for the number of detained migrants. While some may be detected multiple times, others may immigrate without being apprehended. This is expected to be a minor issue, as the largely maritime border of Italy makes it relatively easy to detect illegal crossings (Hanson and McIntosh, 2016). Technical reports from both the European Commission and the Border Agency have repeatedly confirmed that the likelihood of a migrant being smuggled into Europe undetected is very limited (European Commission, 2016b). Third, we are interested in where migrants are from, but this is imperfectly recorded. This is a larger problem for apprehensions, as 6.3% are recorded as originating from 'unspecified sub-Saharan' or 'unknown' countries. This means there is under-counting for some countries. For asylum applicants there is a better record, with only 1.56% of applications from individuals that are 'stateless' or of 'unspecified/not stated' origin. However, given the use of fixed effects, this would not bias estimates unless the likelihood for withholding information from authorities has varied differently for different origins between 2003 and 2016.

These weaknesses are outweighed by strengths. Both measures are based on flows rather than stocks, so they are unaffected by emigration or other changes in the status/residence of historical immigrants. Furthermore, they are of direct interest to donors. The border apprehensions measure represents the closest estimate of the real flow of irregular migrants while asylum applications count irregular migrants that are attempting to regularize their status after reaching Italy. There are several reasons to use both. In particular, illegal border crossing data are only available after 2008. Because journeys to Europe take up to three years (Crawley et al., 2016) and the 2009-2016 time frame is heavily affected by the 2008 financial crisis, a longer panel is preferable.

To give a sense of the data, figure 3 plots these dependent variables for the top nine sending countries in the sample. We also include 'change in refugee stocks' for comparison, which uses the same underlying data as Dreher et al. (2019) (see section 2). While we retain the restriction that changes must be positive, the 'Refugee Stocks' data used here only refer to Italy (whereas Dreher et al., 2019, aggregate all destination countries).

In figure 3 we limit the time period to nine years as the number of apprehensions at border is only available from 2009. All of the measures clearly show that irregular migration has risen sharply over the period. Our two chosen variables (asylum applications and apprehensions at border) often tell a similar story, though for Pakistan and Eritrea there is a less consistent picture. Across the dataset, there is a correlation coefficient between the two measures of 0.60 ($p < 0.0001$, $N = 1,351$).

Turning to refugee stocks, we see they are almost always lower than the number of asylum applications or the number of apprehensions in any given year. This is because refugee stocks only include asylum applications with a positive outcome. Using stocks to infer flows appears problematic, as illustrated by the Eritrean data. Stocks for 2010-2016 hover between 10,000 and 13,500. Looking at the change in stocks shows relatively large increases in 2010-2014 (between 139 and 1,321) but are capped at zero for 2015 and 2016. A very different picture is given by looking at the other possible dependent variables. In 2016, 20,721 Eritreans were detected at the border, with 7,457 asylum applications.

When comparing data used by Dreher et al. (2019) and this paper, there are a number of considerations. First, the obvious difference is that we consider one gateway country (Italy) and many 'sending' countries, whereas Dreher et al. (2019) aggregates all final destination countries where migrants have obtained asylum status, which can take several years. Second, as Dreher et al.'s (2019) underlying data are based on stocks, all positive changes are interpreted as an increase in the flow of refugees, whereas falls in stocks are replaced by zeros. However, changes in stocks are also influenced by births, deaths and changes of visa types. Third, refugees have, by definition, obtained asylum. This is in part a political process that reflects the relationship with countries of origin. Furthermore it excludes all pending applications, which in December 2016 for the EU totalled almost 900,000 (Eurostat, 2017). This may create bias, as the difference between pending and approved status can be very different for different countries of origin. Taken together, a flat level of stocks from a given country would be interpreted as no change in refugee arrivals. In reality it could be a steady stream of asylum seekers with negative asylum outcomes, a large number of pending applications, or a steady stream of successful applications in tandem with refugees gaining other visa types.

Our asylum variable is similar to Murat's (2020), capturing the flow of bilateral asylum applications. We differ by focusing on only one entry point (Italy), as it proxies for the flow of irregular migrants in Europe as a whole. The EURODAC regulation and 'Dublin regulations' (Djajić, 2014; Schuster, 2011) make it virtually impossible for asylum seekers to file their applications in countries other than their place of first entry (i.e., Italy, Greece and Spain) . The number of asylum applications in other European countries is largely determined by their geography, which limits their use in identifying the role of aid in affecting irregular migration.

3.3 Main Independent Variables of Interest

The main independent variable is yearly bilateral aid disbursed by Italy between 2002 to 2015. The more general effect of aid is captured by including all aid disbursed by other donors. In both cases, in-donor refugee costs and donors' administrative expenses are excluded.

3.4 Control Variables

Following the migration literature, we control for the main push and pull factors on migration. First, the stock of migrants from a given country captures current migrants' network. Second, we control for economic conditions at origin, while conditions at destination are absorbed by year fixed effects. At origin, we use GDP per capita and the unemployment rate. Including income at origin allows better accounting for budget constraints than simply controlling for the GDP ratio.[2] The unemployment rate controls for labour supply not absorbed internally.

Third, we capture demographic factors using the origin country's population size and dependency ratio. Fourth, we control for the political environment at origin through political rights and civil liberties as measured by Freedom House's *Freedom in the World index.* Fifth, to account for bilateral immigration policies, we use data from the DEMIG project (De Haas et al., 2014) to construct an index recording bilateral yearly changes in policy restrictiveness. Because immigration restrictions do not affect countries of origin to the same extent, we believe this allows a better control than simply including destination-year fixed effects. Each piece of legislation has been coded by DEMIG's researchers as 'more restrictive' (+1), 'less restrictive'(-1) or 'no changes' (0). We use the yearly sum to create a cumulative index for immigration policy changes. Because the DEMIG project only covers until 2013, additional information was collected from OECD International Migration Outlooks (OECD, 2015, 2016).

Sixth, to control for natural disasters, we use the Emergency Events Database (EM-DAT) created by the Centre for Research on the Epidemiology of Disasters (CRED), which provides a list of deadly natural events in given years. We also control for conflicts using data from the UCDP/PRIO Armed Conflict Dataset, compiled by the Uppsala Conflict Data Program (UCDP) at Uppsala University and the Centre for the Study of Civil War at the Peace Research Institute Oslo (PRIO), which records all episodes of armed conflict where at least one party is the government of a state. UCDP defines conflict as 'a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in a calendar year' (Strand et al., 2017, p. 9). Because the natural disaster database includes events of small intensity (i.e. from 100 people affected and above), the related variable counts the number of events, whereas the conflict variable is a dummy[3].

Finally, we measure 'distance' in terms of geography, history and language. We capture the similarity of languages using an index accounting for the number of common words out of a list of 1,000 items. Data on distances and languages have been drawn from the Centre d'Etudes Prospectives et d'Informations Internationales (CEPII) *GeoDist* and *Language* databases, respectively. To account for shared history and colonial linkages, we create a dummy variable that expands previous measures of colonial legacy (Rose, 2004). Drawing on post-colonial studies (Triulzi, 2006), we extend the concept to countries that were never formally part of the 'Italian Empire', but that were subjected to long term military invasions, such as Albania and Ethiopia. These are only estimated in the absence of country fixed effects.

3.5 Descriptive Statistics

Table 1 provides summary statistics, with full descriptions and sources in Appendix (Table A2).

4 Results

Baseline results are reported in Table 2, estimating (1) in 6 ways. Our chosen dependent variables (asylum applications and border apprehensions) are provided alongside estimates for regular migration, which is the current standard in the literature. Each specification is estimated using fixed effects for

[2]For instance, a ratio of 6 could correspond to very different scenarios in terms of migration hump: (i) a case where income at destination is $48,000 and origin $8,000; (ii) a case where income at destination is $30,000 and origin at $5,000. While GDP ratio would predict the same migration outcome in both cases, in the former an additional income increase is expected to lead to less emigration; whereas the opposite is true for the latter.

[3]Including both variables as dummies did not affect the results.

both year and origin, or just year. As outlined in section 2, we expect positive coefficients for both aid variables. Aid from all other donors is expected to loosen budget constraints given that only a minority of observations in our sample are found at income levels above $6-8,000. Bilateral Italian aid additionally captures network effects.

Looking at the main variables of interest, we see small coefficient estimates on aid variables in all specifications. The number of asylum applications has a positive relationship with both types of aid. Italian aid is found to be significant (at the 5% level) but the effect size is small: a 1% increase in Italian aid is associated with a 0.017% increase in the number of asylum applications. The number of border crossings sees a more mixed picture, with Italian and total aid having opposite signs. However, the effects are still small. For regular migrants, the most common measure in the literature, we see the largest effect size by excluding country fixed effects. A 1% increase in total aid is associated with a 0.03% fall in regular migration to Italy, an effect found significant at the 1% level. Once country fixed effects are included, the estimated effect size is much smaller and insignificant.

Previous migration has a much larger effect. A 1% increase in the number of immigrants already residing in Italy is associated with a 0.19% growth of asylum applications from that country. This effect size is over ten times larger than the equivalent for bilateral aid. The effect of the migrants' network is even larger for regular migrants, which hints at the changing importance of different countries in different kinds of immigration. The largest irregular flows over the period have been from Eritrea (114,211), Nigeria (96,871) and Syria (63,244) whereas the largest foreign-born communities residing in Italy in 2016 were from Romania (1.1 million), Albania (467,000) and Morocco (437,000). This explains why the coefficient on migration stocks is currently smaller for irregular migration. We might expect this to increase in the future, as these newer ties deepen.

Turning to economic conditions at origin, a 1% increase in GDP per capita at home is associated with an almost equal (0.95%) reduction in asylum applications. The same sign is retrieved for border apprehensions, though the coefficient is smaller and not significant. The unemployment rate at origin is not a significant factor in explaining within-country differences.

Turning to demographic factors, the dependency ratio (i.e. the share of population of working age, defined as 15-64) does not appear to have a strong influence on immigration to Italy, once country fixed-effects are included. Rather, population size at origin has the largest effect: a 1% increase in the home population is associated with a 7% growth in border crossings. The median yearly population growth in our sample is 1.47%, and so much of the increase in irregular migration numbers is explained by the model as a direct result of population growth. This does not apply to the number of asylum applications.

The political freedom in the country of origin is a significant factor in explaining differences between countries for all measures of migration, but fixed effects results show that it is not a significant factor in explaining within-country differences in irregular flows. Thus in the short run, changes in political freedom are not significantly related to either measure of irregular migration, but they do explain some of the variation between countries (if country dummies aren't allowed to soak up the majority of the differences).

By contrast Italian political conditions do matter: restrictive bilateral immigration policy is found to be significantly and positively correlated with asylum applications. There are two possible reasons why we might expect more restrictive law making to be associated with more asylum applications. First, it may be that this is simply reverse causality. If Italy responds to greater flows with harsher laws, an apparent positive association may be found. In this explanation any negative effect from stricter immigration laws, if it exists, is overwhelmed by reverse causality. Second, it may be that tougher laws on immigration means that more migrants attempt to regularise their status through the asylum process: as legal paths become more difficult, migrants opt for irregular routes before attempting to become legal residents. This fits with the pattern of the coefficients, as there is a large, positive and significant coefficient on asylum applications but an insignificant and negative effect on border crossings. The evidence is not sufficient to decide which explanation is correct, but it is clear that restrictive legislation is not an effective tool in the presence of other relevant drivers (Castles, 2004; Czaika and Hobolth, 2016). This is underlined by looking at regular migration: policy restrictions is found to have a positive and significant coefficient.

Confirming the importance of push factors, the presence of violent conflicts in the country of origin positively affects the number of asylum applications lodged in Italy. Conflict is associated with a 53% increase in applicants from a given country. It also explains variance in the number of border apprehensions between countries, but not short-run fluctuations within countries. Contrary to the positive effect of armed conflict, natural disasters do not seem to trigger more migration to Italy. It is found negative in every specification, which adds weight to the argument that natural disasters tend to foster short distance migration rather than international flows (Piguet et al., 2011).

The counter-intuitive negative signs retrieved for colonial ties and language proximity can be explained by the limited colonial experience of Italy and by most irregular migrants originating from countries that do not share linguistic roots with Italy. More than 40% of irregular migrants originated from countries where the language proximity index is zero. Language barriers and lack of shared history do not seem to work as a deterrent to irregular migration to Italy.

4.1 Robustness Checks

We now turn to a series of robustness checks, which are found in tables 3-7 and figure 4. The main variables of interest are largely unaffected by controlling for income levels, multilateral resistance, FDI, remittances, trade, deeper lags of ODA variables and when estimating using 3SLS. In table 3 we test the robustness of the results by splitting the sample into high and low income countries, in line with the migration transition hypothesis. We use a GDP per capita cut off of $8,000 at Purchasing Power Parity (Clemens et al., 2014; Dao et al., 2018). Above this threshold increases in income are thought to have a negative effect on migration, whereas below it additional income is expected to increase migration. When the sample is restricted to low income countries, we find a consistent pattern for all dependent variables. Bilateral aid has a positive effect on migration, while aid from all other donors decreases migration. This is found strongly significant for regular migration, but the effect size is small: a 1% increase in total aid is associated with a 0.04% decrease in regular immigration to Italy.

When considering ‘high’ income countries, we find a less consistent pattern. Aid from all other donors is positively and significantly related to asylum applications, while bilateral aid is found to have a weakly significant negative effect on the number of border crossings. The inconsistency in the higher income sample is perhaps a function of the smaller sample size. The main consistent finding is again of small effect sizes. The only strongly significant finding for the variables of interest is that we should expect a 0.044% fall in regular migration to Italy for an additional 1% in aid from other donors. This finding is at odds with the migration-hump hypothesis for aid, as it implies extra finance for low income countries decreases regular migration. Looking at each of the three dependent variables, we do not find evidence that aid loosens the budget constraint in low income countries.

In table 4 we provide an additional control for multilateral resistance to migration, beyond the strategy of using year fixed effects (see Parsons, 2012, for a justification of this method). Our results are potentially biased if the attractiveness of Italy as a destination has changed, relative to other countries. In order to control for such an effect, we introduce three additional variables (one per dependent variable), estimated for both the full and low income samples. These capture contemporary flows to relevant alternative destinations: the number of asylum applications lodged in other DAC donors, the number of border apprehensions at other ports of entry to Europe, and the total regular migration flow to other DAC members.

The variables of interest remain largely unchanged. Bilateral aid is again positive and significant for asylum applications, while the negative effect of total aid on regular migration from countries below 8,000 PPP$ is also robust to multilateral resistance. It should be noted that all three additional controls are found positive and significant for the full sample. This means that movements on other routes are complementary, suggesting that drivers in the countries of origin have been dominating over pull factors in specific destinations.

In table 5 we add controls for FDI and remittances received by the countries of origin to our baseline specification. This helps control for other international transfers that may affect both aid and migration. We find a similar overall pattern. Comparing the variables of interest to the baseline specification, we see that the effect of bilateral aid on asylum applications is still positive, but loses significance and is about half the size. The negative effect of non-Italian aid on regular migration is now significant also for the full sample.

Looking at the new variables included, it is perhaps surprising that there are opposite signs for private remittances and foreign investments. While their impact has been questioned (De Haas, 2005), both types of international transfers could conceivably loosen the budget constraint. However, we find opposite signs for each dependent variable. In only one case is there a significant effect: FDI on border crossings from low income countries. This is also the specification with the lowest sample size. We treat this set of results with caution, especially when combined with the well-known problems with the reliability of remittances data (Alvarez et al., 2015).

In table 6 we include the country of origin's trade openness (total imports and exports on GDP) as well as the relevance of bilateral trade with Italy (as a share of total trade). Menard and Gary (2018) find that higher integration into international trade of source countries is associated with lower migration. By contrast, Berthélemy et al. (2009) find that bilateral exports from destination to origin country can contribute to the attraction effect. We do not find a robust link between migration and either total or bilateral trade. The variables of interest are estimated in the now familiar fashion: all effect sizes are small, with a weakly significant effect for bilateral aid on asylum applications. Likewise, non-Italian aid is found to have a significant negative effect on regular migration, but only for a subset of countries. Neither is estimated with a consistent sign across the two irregular migration variables.

Next, we consider aid's effect on migration over a longer time period. It is plausible that aid affects migration with a lag of over one year. Figure 4 plots estimated coefficients retrieved in the baseline specification but lagging aid variables by between 1 and 8 years, keeping all other covariates unchanged. The choice of 8 years is driven by *Ln(Border)* panel spanning over 9 years. The top row in the figure shows bilateral aid, while the bottom row plots total aid from all other donors. Each migration dependent variable is given in a separate column. Starting from asylum applications on the left, only one instance is significant at 5% (bilateral aid lagged one year). For border apprehension, the positive effect of others' aid is significant at 5% for 3 and 5 year lags. However, the estimated coefficients are remarkably similarly over time. The estimated effect of aid on migration is mostly small and insignificant, regardless of the lag length.

In table 7 we present the results from the 3SLS regressions. This is now the standard approach for addressing endogeneity concerns that stem from the joint determination of aid and migration. We include separate equations for bilateral aid received from Italy and total aid from all other donors. We follow Clist (2011) and related literature when choosing a parsimonious set of controls. Equation (1) is augmented by:

$$\begin{aligned} ODAItaly_{jit-1} = \beta_1 Migration_{ijt} + \beta_2 GDPcapita_{it-1} + \beta_3 Population_{it-1} \\ + \beta_4 PolFreedom_{it-1} + \beta_5 Conflict_{it-1} + \beta_6 NatDisaster_{it-1} \\ + \beta_7 ItaExports_{jit-1} + \mu_i + \pi_{t-1} + \varepsilon_{jit-1} \end{aligned} \tag{2}$$

$$\begin{aligned} OtherODA_{it-1} = \beta_1 Migration_{ijt} + \beta_2 GDPcapita_{it-1} + \beta_3 Population_{it-1} \\ + \beta_4 PolFreedom_{it-1} + \beta_5 Conflict_{it-1} + \beta_6 NatDisaster_{it-1} \\ + \beta_7 TradeOpen_{it-1} + \mu_i + \pi_{t-1} + \varepsilon_{jit-1} \end{aligned} \tag{3}$$

Where *ItaExports* measures bilateral exports from Italy to origin countries as a proxy for Italy's commercial interests; *TradeOpen* measures the trade openness of recipient countries.

From the 3SLS estimates, the main message is that the variables of interest are not significant. The familiar pattern is replicated: a positive link between bilateral aid and asylum applications and a negative relationship between other donors' aid and regular migration for low income countries. However, the main message is simply that aid effects are small and/or not significant.

Other factors retrieve more robust associations. The role of the migrants' network is found to be positive for all dependent variables, and always significant for the smaller sample. The effect size is largest for regular flows, confirming patterns discussed earlier. Similarly, the coefficients for income at origin remains significant and negative for asylum applications, and population is again found to be an important determinant of border apprehension. The presence of conflict is also confirmed as a robust determinant of asylum applications.

Looking at the determinants of aid (see tables A3 and A4 in Appendix), our concerns regarding the endogeneity of total aid are confirmed. All measures of migration to Italy are significantly associated with total ODA from all other donors, and in the cases of asylum applications and regular entries this is also true for Italian ODA. Aid is generally given in response to higher predicted numbers of immigrants to Italy. The only exception is recipient countries with GDP per capita below 8,000 PPP$ that fail to prevent their citizens illegally crossing Italian borders. This would be in line with the increasing practice, within the EU, of linking development aid to measures of border externalization (e.g. Oette and Babiker, 2017). Recipient and transit countries are made responsible for containing outflows in exchange of financial support. Despite the problem of endogeneity, estimates of the variables of interest are largely unaffected.

5 Discussion

Rather than highlight specific results that are found to be significant in a specific regression, we attempt to look at the whole body of evidence presented. We do this in order to lower the risk that we cherry pick significant but fragile results. We'll start by considering each of the dependent variables in turn.

For asylum applications the coefficients on Italian aid are always found to be positive, with effect sizes ranging from 0.01 to 0.27. In the twelve estimates reported it is significant at the 5% level three times, and at the 10% level twice more. The largest significant coefficient is 0.020, meaning that a 1% increase in bilateral aid from Italy to a given country is associated with a 0.020% increase in the number of asylum applications. To give an example of what this means consider the case of Pakistan in 2016, which was the second largest Italian aid recipient and the second largest origin country for asylum applications. The results imply that increasing Italian aid by 1% (i.e. $438,484) would equal an increase of around 2.70 asylum applications. The parameter sizes equate one additional asylum application with an extra $162,198 (2010 prices) in bilateral aid. While this is statistically significant, it is a small effect. We also estimate the effect of total aid received by origin countries from all other donors, with effect sizes that range from 0.26 to -0.044. It is found significant only once, at the 5% level, with a coefficient of 0.025. This evidence provides very little support for donors' arguments that aid deters irregular migration, at least in the short run when it is measured using asylum applications.

For the number of border apprehensions we find small effect sizes with inconsistent signs. The only significant effect is found in table 3, where bilateral aid has a negative and weakly significant effect on border crossings for countries with a per capita income of over 8,000 PPP$. To give a sense of the effect size consider the case of Iraq in 2016, which was the top bilateral aid recipient and the second largest sending country within the higher-income sub-sample. The coefficient implies that additional $510,507 would reduce border apprehensions by 0.27 units. The cost per deterred border crossing is then $1,846,653 (2010 prices). This effect size is relatively high compared to other specifications, and so can be thought of as bounding the range of true effect size. It is also worth noting that this single significant finding is found for a subset (countries with a per capita income over 8,000 PPP$) and when using the dependent variable with smaller coverage. As such, the statistical power is smaller. More generally, the consistent picture is of very small effect sizes from aid on border crossings compared to other determinants of migration.

Comparing the above with results for regular migration, a clear difference emerges. The coefficient of total aid is negative in all estimates for lower income countries. Leaving aside the 3SLS estimate, the other coefficients are always significant and fall within a narrow range (-0.044 to -0.038). The 3SLS coefficient is larger (though not significant): a 1% increase in aid disbursements is associated with a 0.075% decrease in regular migration flows. Using Pakistan as an example, these effects imply an increase in aid from other donors of $46 million is associated with an eleven unit decrease in regular migrant flows to Italy. The largest effect we find for regular migration then implies a figure of over $4 million per deterred migrant. Thus while the sign is more consistently estimated, we also find small effect sizes for regular migration.

Our results echo those from a recent attempt to look at aid's effect on refugee numbers. Dreher et al. (2019) find that the total aggregate aid received by a source country has no significant changes in overall refugee outflows in the short term. Over a longer horizon (12 to 15 years) they estimate total aid deters refugees stocks: an increase in total ODA/GDP by one percentage point is associated with a 33% decrease in refugee flows to the OECD. Prima facie this appears to be a very large effect, but let's consider what this would mean for Pakistan. A one percentage point increase in aid/GDP in 2003-2005 is approximately $1.4 billion (in 2010 prices), which their estimates equate with a drop in refugee stocks in 2016 of 1,178. The estimated cost of deterring one migrant is about $1.2 million. Thus while the long-term effect is found weakly significant and appears large, the actual effect size is unlikely to persuade donors that this is a good use of scarce aid resources.

Murat's (2020) results also imply large costs per deterred migrant, despite focusing on different kinds of aid and migration. She finds a significant negative association between bilateral aid and asylum applications, for sending countries with GDP per capita below $787 (2005 prices). The largest effect is for countries in the lowest quintile (below $411 GDP per capita, in 2005 prices), and equates a 1% increase in bilateral aid with a 0.05% reduction in asylum applications in the following year. Using the median values for migration and aid in that quintile, this translates into a 0.007 drop in asylum applications for an additional $74,100 spent in bilateral ODA. The cost per deterred asylum application is just over $10.5 million. There is a range. It reduces to $3.6 million if only the most recent year (2013) is considered, and it increases to $18.6 million if only Italy is considered as a destination country.

Comparing our findings to other previous work, we inevitably compare to studies that focus on the effect of aid on regular migration. The results are remarkably similar in magnitude despite differences in aim, approach, and the accompanying discussion. Consider the significant and negative effect of total aid on regular migration to OECD countries reported by Lanati and Thiele (2018). We don't find this to be significant; a difference which can partly be attributed to the potential endogeneity of total aid. The negative effect we consistently found in a fixed effects model from total aid on regular migration to Italy is not robust to 3SLS estimates including a third equation for non-Italian aid. Regarding effect sizes, coefficients cannot be directly compared because of different dependent variables: we use logged irregular flows whereas Gamso and Yuldashev (2018a) and Lanati and Thiele (2018) use emigration rates. This means an illustration is required to compare effect sizes.

While Gamso and Yuldashev (2018a) refer to rural aid and Lanati and Thiele (2018) refer to total aid, they both find a 1% increase in (that kind of) aid is associated with a decrease in emigration rates of approximately 0.1%. Taking again the case of Pakistan in 2016, aid's potential deterring effect translates into a reduction of the regular emigration rate from 0.00764% to 0.00763% for an additional $46 million in total aid. Keeping population constant, this would lead to a 15-unit decrease in regular migration flows to Italy from Pakistan, from 14,745 to 14,730 immigrants. This is more than $3 million in aid per deterred migrant (at 2010 prices). Our own estimates lead to a cost per deterred regular migrant that ranges from $4 to $7 million (2010 prices). While the coefficient sizes may appear to be different, the resulting estimates of cost per deterred migrant are remarkably similar.

Our results also support recent findings that financial constraints have a more limited effect than previously thought (Dao et al., 2018). We do not find support for the idea that by loosening budgetary constraints foreign aid causes more migration (De Haas, 2007). Instead, we consistently find a negative association between income at origin and irregular migration to Italy, which is significant in most specifications. Higher per-capita GDP in the country of origin leads to reductions in flows to Italy. Contrary to the prediction of the migration hump (Berthélemy et al., 2009; Clemens et al., 2014), we find the negative relationship with irregular migration to Italy is stronger for countries of origin below the 8,000 PPP$ per capita threshold.

The role of income levels may of course be rather different for irregular migration than for regular migration. One difference is the much higher costs of illegal routes. Although the increased demand has recently lowered the price (Europol, 2016), it is estimated that a journey from Libya to Italy relying on smugglers costs on average $1,500 in current prices (Reitano and Tinti, 2015), whereas the cheapest journey from Eritrea could cost as much as $3,700 (Hamood, 2006). With prices so high, relatively small gains in income are of little consequence to irregular migrants. Rather it is poverty, networks and conflict which drive irregular migrants, a set of findings which are robust across our specifications.

Turning to bilateral aid, our results agree with previous work that found individual donor countries have very limited ability to effect their immigration flows (Lanati and Thiele, 2018). Whilst Berthélemy et al. (2009) found a strong positive effect, papers that use migrant flows as their dependent variable find smaller effects (Menard and Gary, 2018). Further, research that found a positive effect from aid on migration typically didn't control for bilateral migrant stocks. Once the immigrants' network and related attraction effect are accounted for, evidence shows that the effect of bilateral aid is negligible compared to that of the migrants' network (Lanati and Thiele, 2018). In this regard, our findings for Italy are also consistent with previous research on asylum seekers in Germany and Western Europe (Neumayer, 2005; Vogler and Rotte, 2000). Both Neumayer (2005) and Vogler and Rotte (2000) used bilateral aid as a proxy for bilateral contacts between donors and countries of origin in a fixed effect model and found no significant effects on the flows of asylum seekers.

6 Conclusion

A lot has been written on the link between aid and regular migration, often in response to donors' claims that aid is able to address the root causes of migration. We add to this literature by using new data on irregular migration, asking whether aid is an effective deterrent of asylum applications and border apprehensions in Italy. Our consistent finding, across dependent variables, types of aid and various specifications, is that the estimated effect size of development aid on migration is small. Inconsistently, effects are found to be significant.

While we contribute new data to the debate, we are also subject to at least four of the limitations of previous work. First, the strategies to deal with endogeneity are not wholly satisfactory. The direction of the bias is known, but this problem is a persistent theme. Second, while we provide new data on irregular migrants, the distinction between transit and origin countries is still unexplored. Our solution is to examine one transit country, but much is not yet understood about the roles of different countries. For our specific question that may give misleading answers. Although donors tend to focus more resources on countries of origin (Czaika and Mayer, 2011), it is possible that donors successfully affect migration by targeting transition countries (Dreher et al., 2019). Third, the effects of different types of aid is unknown. It may be that a specific subset of aid is effective at reducing migration; an effect that could be hidden by looking at total volumes. Separating this from fragile sub-group analysis will be particularly difficult, as a search for this type of effective aid could easily inadvertently resemble p-hacking. Fourth, the timing of any effects is difficult to model. It is likely that sustained aid disbursement affects migration over a longer period. As discussed in section 3, there is a trade off between adopting appropriate measures of migration flows and data coverage. We've opted for new data which is both more accurate and relevant, but face lower coverage. Further research would ideally address some of these problems.

We contribute to the debate on aid's effectiveness at deterring migration by using new data on irregular flows to Italy. We find consistent evidence of very small effects. An apparent 'attraction effect' of bilateral aid is not found; the migrants' network plays a much larger role. For total aid there is again a very small effect. We find no robust evidence that supports either a positive effect through loosening budget constraints, nor a negative effect through closing income differentials. Further, the relevance of budgetary constraints and the migration-hump hypothesis for irregular migration are also challenged by the effect of GDP per capita in source countries. The most optimistic case we can make from the evidence presented here is that the cost in bilateral aid per deterred border crossing is $1.8 million (for countries with a per capita income of over 8,000 PPP$). For regular migrants, the cost per deterred regular migrant is in the range of $4-7 million, which is similar to previous work. The most significant effect from the asylum data implies the opposite effect: we should expect one extra application for every additional $162,198 in bilateral aid. To conclude, aid does not appear to be an effective tool for deterring either regular or irregular migration.

Figure 1: Apprehensions At Border On Main Irregular Migration Routes To Europe, 2009-2016

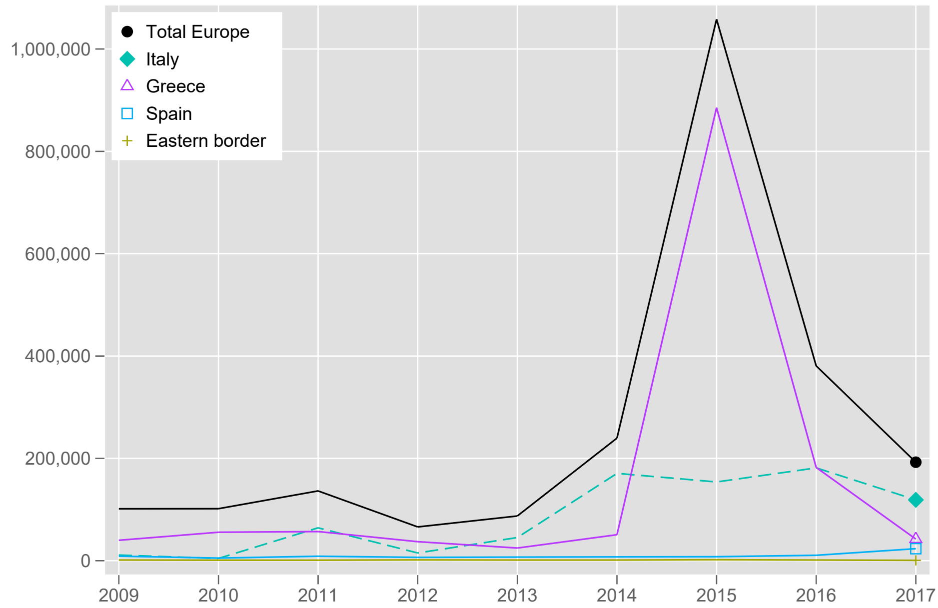


Figure 2: Aggregate Immigrant Inflows From Non-DAC Countries, 1998-2018

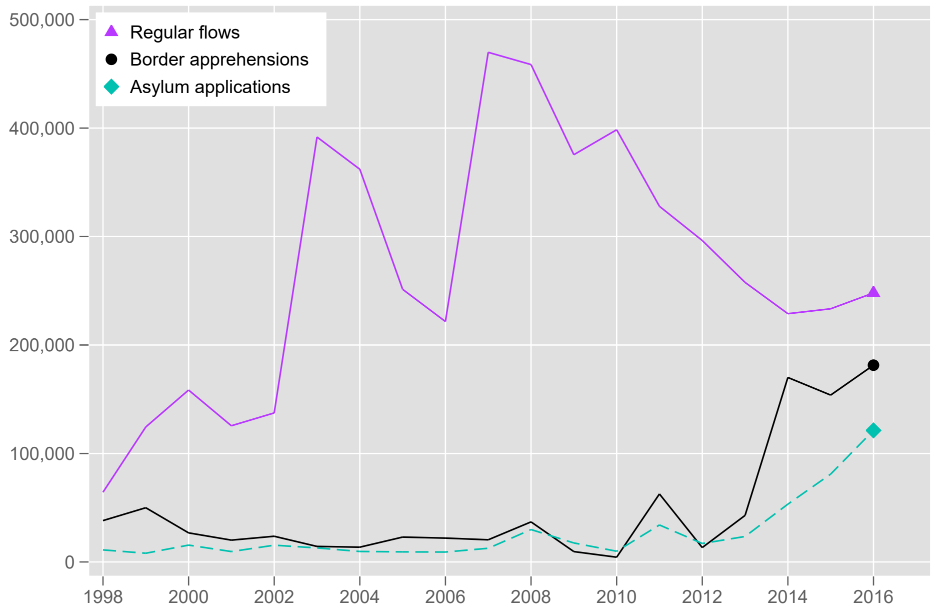
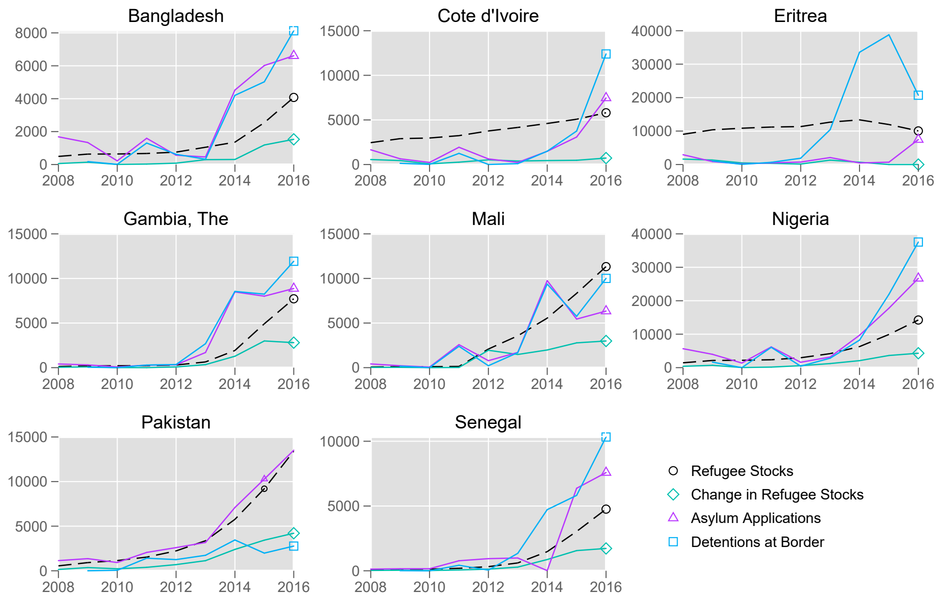


Figure 3: Three Measures of 'Migration' to Italy for 9 Top 'Sending' Countries, 2008-2016



Note: the figure shows 95% Confidence Intervals

Figure 4: Estimated coefficients lagging aid variables one to eight years

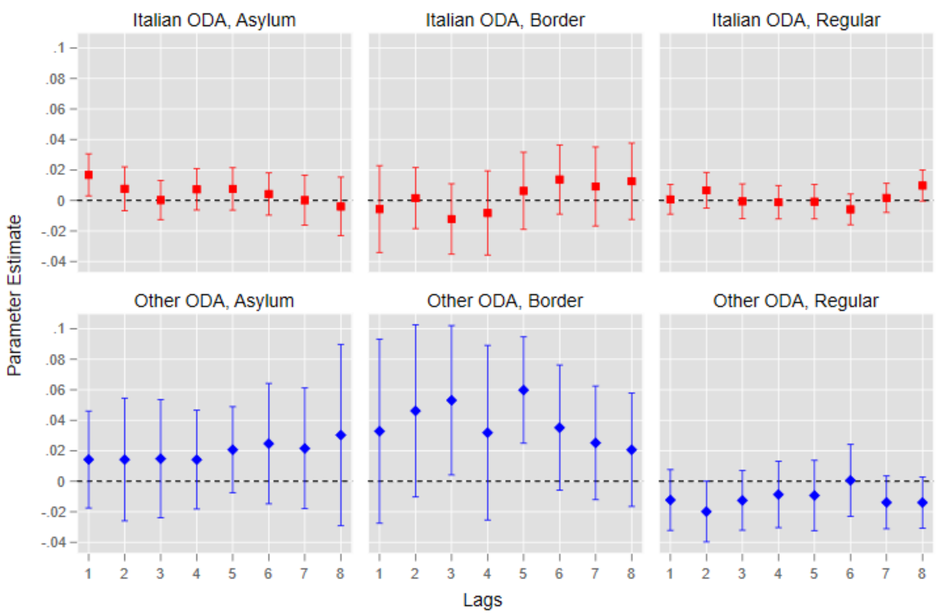


Table 1: Descriptive Statistics

	Obs.	Mean	SD	Min	Max
Ln(Regular)	2248	4.48	2.72	0.00	12.51
Ln(Asylum)	2365	1.75	2.34	0.00	10.19
Ln(Border)	1359	1.30	2.48	0.00	10.59
Ln(ItalyODA)	2378	8.81	6.89	0.00	20.70
Ln(Other ODA)	2378	16.21	7.28	0.00	23.92
Ln(Immig. Stock)	2241	6.43	3.07	0.00	13.97
Ln(GDPpc Origin)	2271	8.10	1.32	5.27	11.89
Unemployment Origin	2128	9.28	6.83	0.10	37.60
Ln(Pop Origin)	2375	15.25	2.27	9.16	21.04
Dep. Ratio Origin	2210	64.07	19.72	16.33	112.97
Political Freedom Origin	2338	7.46	3.75	2.00	14.00
Immig. Policy Restrict.	2377	19.18	6.00	5.00	26.00
Nat Disaster Origin	2378	0.65	0.73	0.00	3.78
Conflict Origin	2378	0.24	0.43	0.00	1.00
Ln(Dist)	2379	8.47	0.86	5.44	9.78
Colony	2379	0.04	0.18	0.00	1.00
Language Prox	2365	0.13	0.15	0.00	1.00

Table 2: Baseline regressions

	Ln(Asylum)		Ln(Border)		Ln(Regular)	
	OLS	FE	OLS	FE	OLS	FE
Ln(Italy ODA)	0.012	0.017**	-0.012	-0.0056	0.00084	0.00074
	(0.017)	(0.0070)	(0.023)	(0.014)	(0.0069)	(0.0049)
Ln(Other ODA)	0.0040	0.014	0.00085	0.033	-0.033***	-0.012
	(0.022)	(0.016)	(0.030)	(0.031)	(0.0077)	(0.010)
Ln(Immig. Stock)	0.54***	0.19**	0.40***	0.19	0.85***	0.50***
	(0.069)	(0.090)	(0.081)	(0.12)	(0.022)	(0.087)
Ln(GDPpc Origin)	-0.11	-0.95**	-0.074	-0.23	-0.14***	-0.33*
	(0.15)	(0.40)	(0.17)	(0.80)	(0.046)	(0.18)
Unemployment Origin	0.019	0.024	0.024	-0.0076	0.011**	0.015
	(0.014)	(0.020)	(0.020)	(0.022)	(0.0049)	(0.012)
Ln(Pop Origin)	-0.13	-0.36	0.046	7.33***	0.099***	0.78*
	(0.096)	(0.65)	(0.11)	(2.27)	(0.033)	(0.42)
Dep. Ratio Origin	0.017**	0.021	0.029***	-0.013	-0.0076***	0.0022
	(0.0086)	(0.016)	(0.010)	(0.040)	(0.0029)	(0.0073)
Political Freedom Origin	0.10***	0.082	0.071*	-0.022	0.027***	0.080***
	(0.033)	(0.056)	(0.040)	(0.10)	(0.010)	(0.026)
Immig. Policy Restrict.	0.55***	0.68***	0.26	-0.18	-0.0021	0.15***
	(0.18)	(0.19)	(0.22)	(0.20)	(0.049)	(0.041)
Nat Disaster Origin	-0.17	-0.024	-0.49**	-0.14*	0.011	0.019
	(0.15)	(0.056)	(0.19)	(0.082)	(0.053)	(0.025)
Conflict Origin	0.81***	0.37**	0.93***	0.26	-0.045	0.014
	(0.27)	(0.15)	(0.32)	(0.20)	(0.070)	(0.057)
Ln(Dist)	-0.32		-0.12		0.067	
	(0.20)		(0.24)		(0.074)	
Colony	-0.025		-0.040		-0.13	
	(0.67)		(0.88)		(0.16)	
Language Prox	-2.69***		-4.00***		0.30	
	(0.80)		(0.84)		(0.29)	
Year FE	x	x	x	x	x	x
Origin FE		x		x		x
Observations	1994	1994	1143	1143	1956	1956
R^2	0.57	0.20	0.46	0.24	0.94	0.19
Timeframe	2003-16		2009-16		2003-16	

Notes: clustered standard errors in parentheses. OLS includes Year fixed effects, FE includes both Year and Origin fixed effects. Unless otherwise specified in the text, all regressors are one-year lagged. * p<0.1, ** p<0.05, *** p<0.01

Table 3: Robustness check: Income levels

	Ln(Asylum)		Ln(Border)		Ln(Regular)	
Sample's income level:	LOW	HIGH	LOW	HIGH	LOW	HIGH
Ln(Italy ODA)	0.017*	0.016	0.010	-0.019*	0.0046	-0.0066
	(0.0093)	(0.011)	(0.021)	(0.011)	(0.0055)	(0.0085)
Ln(Other ODA)	-0.035	0.025**	-0.012	0.0066	-0.044***	-0.0045
	(0.025)	(0.011)	(0.24)	(0.0093)	(0.013)	(0.013)
Ln(Immig. Stock)	0.45***	-0.039	0.31	0.031	0.63***	0.17*
	(0.13)	(0.10)	(0.30)	(0.031)	(0.10)	(0.086)
Ln(GDPpc Origin)	-1.57***	-0.63	-1.32	-0.59	-0.50**	-0.15
	(0.54)	(0.51)	(1.42)	(0.81)	(0.24)	(0.31)
Unemployment Origin	0.066**	-0.0094	-0.032	0.010	0.0074	0.015
	(0.031)	(0.025)	(0.060)	(0.013)	(0.020)	(0.013)
Ln(Pop Origin)	0.17	-0.16	16.0***	1.16	0.68	0.53
	(1.57)	(0.51)	(4.00)	(0.91)	(0.93)	(0.34)
Dep. Ratio Origin	0.019	0.024	0.014	0.014	0.0021	-0.0041
	(0.016)	(0.017)	(0.073)	(0.022)	(0.0091)	(0.013)
Political Freedom Origin	0.17**	-0.10**	-0.023	0.00039	0.091**	0.038*
	(0.073)	(0.046)	(0.13)	(0.10)	(0.036)	(0.023)
Immig. Policy Restrict.	0.96***	0.54***	-1.11**	0.076	0.61**	0.020
	(0.27)	(0.20)	(0.46)	(0.098)	(0.30)	(0.035)
Nat Disaster Origin	0.024	-0.040	0.047	-0.11*	0.0084	0.015
	(0.075)	(0.076)	(0.13)	(0.066)	(0.035)	(0.042)
Conflict Origin	0.42***	0.18	0.32	0.28	-0.056	0.24*
	(0.15)	(0.29)	(0.30)	(0.19)	(0.058)	(0.13)
Year FE	x	x	x	x	x	x
Origin FE	x	x	x	x	x	x
Observations	1109	885	596	547	1098	858
R^2	0.29	0.17	0.40	0.082	0.29	0.15
Timeframe	2003-16		2009-16		2003-16	

Notes: clustered robust standard errors in parentheses. LOW: recipients with GDPcapita below 8,000 PPP$. HIGH: recipients with GDPcapita above 8,000 PPP$. * p<0.1, ** p<0.05, *** p<0.01

Table 4: Robustness check: Multilateral Resistance

	Ln(Asylum)		Ln(Border)		Ln(Regular)	
Sample's Income Level	Full	LOW	Full	LOW	Full	LOW
Ln(Italy ODA)	0.018**	0.020*	-0.0072	0.010	0.00043	0.0042
	(0.0076)	(0.010)	(0.014)	(0.021)	(0.0049)	(0.0055)
Ln(Other ODA)	0.013	-0.031	0.027	0.0040	-0.0092	-0.044***
	(0.015)	(0.024)	(0.031)	(0.23)	(0.0098)	(0.013)
Ln(Immig. Stock)	0.18**	0.40***	0.18	0.31	0.51***	0.64***
	(0.084)	(0.13)	(0.12)	(0.30)	(0.083)	(0.10)
Ln(GDPpc Origin)	-0.83**	-1.38**	-0.086	-1.07	-0.37**	-0.55**
	(0.39)	(0.53)	(0.80)	(1.44)	(0.17)	(0.23)
Unemployment Origin	0.015	0.050*	-0.0081	-0.037	0.0084	0.0059
	(0.020)	(0.030)	(0.022)	(0.059)	(0.011)	(0.020)
Ln(Pop Origin)	-0.61	-0.023	7.03***	15.3***	0.59	0.52
	(0.63)	(1.42)	(2.23)	(3.91)	(0.42)	(0.95)
Dep. Ratio Origin	0.030**	0.029*	-0.011	0.021	0.0060	0.0051
	(0.015)	(0.016)	(0.040)	(0.072)	(0.0073)	(0.0094)
Political Freedom Origin	0.057	0.14**	-0.044	-0.048	0.083***	0.094**
	(0.053)	(0.069)	(0.10)	(0.13)	(0.026)	(0.036)
Immig. Policy Restrict.	0.61***	0.97***	-0.23	-1.28***	0.19***	0.62**
	(0.18)	(0.28)	(0.21)	(0.46)	(0.042)	(0.30)
Nat Disaster Origin	-0.019	0.038	-0.14	0.028	0.018	0.0089
	(0.054)	(0.073)	(0.082)	(0.12)	(0.025)	(0.035)
Conflict Origin	0.30**	0.37***	0.23	0.28	0.014	-0.053
	(0.14)	(0.14)	(0.20)	(0.30)	(0.056)	(0.058)
Ln(Asylum) DAC	0.32***	0.36***				
	(0.068)	(0.11)				
Ln(Border) other routes			0.14**	0.15		
			(0.056)	(0.096)		
Ln(Regular) DAC					0.18***	0.10
					(0.060)	(0.062)
Year FE	x	x	x	x	x	x
Origin FE	x	x	x	x	x	x
Observations	1979	1101	1143	596	1956	1098
R^2	0.23	0.32	0.25	0.41	0.20	0.29
Timeframe	2003-16		2009-16		2003-16	

Notes: clustered robust standard errors in parentheses. LOW: sample includes recipients with GDPcapita below 8,000 PPP$. * p<0.1, ** p<0.05, *** p<0.01

Table 5: Robustness check: other international transfers

	Ln(Asylum)		Ln(Border)		Ln(Regular)	
Sample's Income Level	Full	LOW	Full	LOW	Full	LOW
Ln(Italy ODA)	0.011	0.0100	-0.0048	-0.0023	0.0038	0.0066
	(0.0076)	(0.0098)	(0.014)	(0.020)	(0.0058)	(0.0068)
Ln(Other ODA)	0.0092	-0.038	0.032	0.038	-0.024**	-0.044***
	(0.018)	(0.028)	(0.031)	(0.28)	(0.0099)	(0.015)
Ln(Immig. Stock)	0.21**	0.49***	0.17	0.29	0.53***	0.60***
	(0.10)	(0.14)	(0.12)	(0.32)	(0.10)	(0.14)
Ln(GDPpc Origin)	-1.42**	-2.05***	0.23	-0.78	-0.35	-0.68*
	(0.59)	(0.74)	(1.10)	(1.93)	(0.25)	(0.35)
Unemployment Origin	0.024	0.050	0.017	-0.016	0.019	0.0041
	(0.023)	(0.036)	(0.024)	(0.072)	(0.013)	(0.020)
Ln(Pop Origin)	0.87	-0.25	10.2***	16.3***	1.96***	0.32
	(1.15)	(1.84)	(2.36)	(4.28)	(0.57)	(1.07)
Dep. Ratio Origin	0.022	0.027	0.0057	0.032	-0.00067	0.0090
	(0.015)	(0.017)	(0.041)	(0.081)	(0.0075)	(0.011)
Political Freedom Origin	0.039	0.13*	-0.0075	0.010	0.071***	0.095**
	(0.054)	(0.074)	(0.11)	(0.13)	(0.027)	(0.038)
Immig. Policy Restrict.	0.62***	0.90**	-0.20	-0.97*	0.087*	0.60
	(0.20)	(0.37)	(0.20)	(0.51)	(0.046)	(0.51)
Nat Disaster Origin	-0.0078	0.039	-0.078	0.11	0.010	0.0025
	(0.062)	(0.084)	(0.084)	(0.13)	(0.025)	(0.038)
Conflict Origin	0.31**	0.34**	0.15	0.11	-0.030	-0.092
	(0.15)	(0.14)	(0.23)	(0.37)	(0.059)	(0.067)
Total FDI	0.026	0.048	-0.082	-0.18**	-0.013	-0.013
	(0.053)	(0.050)	(0.059)	(0.085)	(0.018)	(0.017)
Total Remittances	-0.010	-0.030	0.024	0.028	0.012	0.0062
	(0.035)	(0.084)	(0.041)	(0.17)	(0.014)	(0.025)
Year FE	x	x	x	x	x	x
Origin FE	x	x	x	x	x	x
Observations	1685	952	988	530	1653	942
R^2	0.20	0.28	0.26	0.40	0.21	0.27
Timeframe	2003-16		2009-16		2003-16	

Notes: clustered robust standard errors in parentheses. LOW: sample includes recipients with GDP per capita below 8,000 PPP$. * p<0.1, ** p<0.05, *** p<0.01

Table 6: Robustness check: trade flows

	Ln(Asylum)		Ln(Border)		Ln(Regular)	
Sample's Income Level:	Full	LOW	Full	LOW	Full	LOW
Ln(ItalyODA)	0.020**	0.022	-0.00069	0.016	-0.0030	-0.0026
	(0.0090)	(0.014)	(0.017)	(0.035)	(0.0060)	(0.0069)
Ln(Other ODA)	0.012	-0.044	0.038	-0.073	-0.017	-0.038***
	(0.021)	(0.028)	(0.033)	(0.29)	(0.012)	(0.012)
Ln(Immig. Stock)	0.16	0.51**	0.22	0.68***	0.46***	0.59***
	(0.10)	(0.21)	(0.14)	(0.18)	(0.11)	(0.17)
Ln(GDPpc Origin)	-1.23**	-1.72**	-0.47	-1.68	-0.16	-0.32
	(0.57)	(0.68)	(1.13)	(1.54)	(0.24)	(0.27)
Unemployment Origin	0.036	0.086**	-0.0038	-0.045	0.026**	0.019
	(0.024)	(0.042)	(0.022)	(0.085)	(0.013)	(0.023)
Ln(Pop Origin)	-0.37	2.57	6.09**	17.4***	0.78*	0.55
	(0.73)	(2.25)	(2.35)	(3.99)	(0.40)	(1.15)
Dep. Ratio Origin	0.028	0.012	-0.012	-0.0060	0.0089	0.011
	(0.019)	(0.021)	(0.042)	(0.068)	(0.0082)	(0.010)
Political Freedom Origin	0.029	0.088	-0.068	-0.13	0.074***	0.070*
	(0.060)	(0.082)	(0.11)	(0.13)	(0.028)	(0.037)
Immig. Policy Restrict.	0.69***	0.94**	0.040	-0.86	0.14***	1.21***
	(0.19)	(0.40)	(0.25)	(0.60)	(0.045)	(0.25)
Nat Disaster Origin	0.000068	0.039	-0.11	0.13	0.038	0.040
	(0.065)	(0.089)	(0.091)	(0.16)	(0.024)	(0.038)
Conflict Origin	0.42**	0.41**	0.36	0.25	0.033	0.0076
	(0.18)	(0.18)	(0.26)	(0.46)	(0.060)	(0.055)
Bilateral trade (%)	0.074	-0.0080	0.044	0.021	-0.0095	-0.017
	(0.052)	(0.060)	(0.073)	(0.14)	(0.012)	(0.024)
Trade Open. (GDP%)	-0.091	-0.27	0.37	-0.040	-0.098	-0.0013
	(0.29)	(0.46)	(0.63)	(1.18)	(0.15)	(0.23)
Observations	1578	792	901	420	1553	789
R^2	0.23	0.35	0.24	0.46	0.20	0.32

* p¡0.10, ** p¡0.05, *** p¡0.010

Table 7: Robustness check: 3SLS estimates

	Ln(Asylum)		Ln(Border)		Ln(Regular)	
	Full	LOW	Full	LOW	Full	LOW
Ln(Italy ODA)	0.19	0.27	-0.063	-0.20	-0.20	0.058
	(0.21)	(0.27)	(0.18)	(0.25)	(0.13)	(0.13)
Ln(Other ODA)	0.16	0.26	-0.70	0.33	0.28	-0.075
	(0.28)	(0.41)	(0.97)	(1.30)	(0.18)	(0.25)
Ln(Immig. Stock)	0.14	0.21**	0.44	0.44*	0.37***	0.56***
	(0.15)	(0.10)	(0.31)	(0.25)	(0.14)	(0.070)
Ln(GDPpc Origin)	-1.12**	-1.28***	1.70	-0.94	-0.78**	-0.32
	(0.53)	(0.43)	(2.87)	(1.18)	(0.31)	(0.24)
Unemployment Origin	0.019*	0.053*	-0.055	-0.13	0.012*	0.0094
	(0.011)	(0.031)	(0.075)	(0.087)	(0.0063)	(0.018)
Ln(Pop Origin)	0.35	2.64	6.23***	12.8***	1.18**	0.30
	(0.67)	(2.26)	(2.02)	(3.83)	(0.47)	(1.44)
Dep. Ratio Origin	0.014	0.023	-0.0056	-0.0072	-0.0035	0.0019
	(0.014)	(0.018)	(0.034)	(0.039)	(0.0099)	(0.011)
Political Freedom Origin	0.12***	0.35***	-0.025	0.024	0.089***	0.11**
	(0.039)	(0.079)	(0.074)	(0.11)	(0.025)	(0.051)
Immig. Policy Restrict.	0.51***	0.38	0.070	0.19	0.079	0.46*
	(0.15)	(0.48)	(0.44)	(0.65)	(0.10)	(0.26)
Nat Disaster Origin	-0.11	-0.13	-0.14	0.13	0.053	-0.039
	(0.082)	(0.14)	(0.12)	(0.15)	(0.052)	(0.065)
Conflict Origin	0.31***	0.31**	0.36	-0.0091	-0.024	-0.062
	(0.12)	(0.14)	(0.27)	(0.27)	(0.081)	(0.077)
Year FE	x	x	x	x	x	x
Origin FE	x	x	x	x	x	x
Observations	1928	967	1098	486	1898	964
R^2	0.77	0.77	0.65	0.80	0.90	0.96
Timeframe	2003-16		2009-16		2003-16	

Notes: standard errors in parentheses. LOW: sample includes recipients with GDPcapita below 8,000 PPP$. * p<0.10, ** p<0.05, *** p<0.010

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A Appendices

Table A1: List of included countries

Recipient			
Afghanistan	Cuba	Lesotho	Sao Tome and Principe
Albania	Cyprus	Liberia	Saudi Arabia
Algeria	Djibouti	Libya	Senegal
Angola	Dominican Republic	Lithuania	Serbia
Argentina	Ecuador	Macedonia, FYR	Sierra Leone
Armenia	Egypt, Arab Rep.	Madagascar	Singapore
Azerbaijan	El Salvador	Malawi	Solomon Islands
Bahamas	Equatorial Guinea	Malaysia	South Africa
Bahrain	Eritrea	Maldives	Sri Lanka
Bangladesh	Estonia	Mali	St. Lucia
Barbados	Ethiopia	Malta	St. Vincent and the Grenadines
Belarus	Fiji	Mauritania	Sudan
Belize	Gabon	Mauritius	Suriname
Benin	Gambia, The	Mexico	Swaziland
Bhutan	Georgia	Moldova	Tajikistan
Bolivia	Ghana	Mongolia	Tanzania
Bosnia and Herzegovina	Guatemala	Montenegro	Thailand
Botswana	Guinea	Morocco	Timor-Leste
Brazil	Guinea-Bissau	Mozambique	Togo
Brunei Darussalam	Guyana	Myanmar	Tonga
Bulgaria	Haiti	Namibia	Trinidad and Tobago
Burkina Faso	Honduras	Nepal	Tunisia
Burundi	Hong Kong SAR, China	Nicaragua	Turkey
Cabo Verde	India	Niger	Turkmenistan
Cambodia	Indonesia	Nigeria	Uganda
Cameroon	Iran, Islamic Rep.	Oman	Ukraine
Central African Republic	Iraq	Pakistan	United Arab Emirates
Chad	Israel	Panama	Uruguay
Chile	Jamaica	Papua New Guinea	Uzbekistan
China	Jordan	Paraguay	Vanuatu
Colombia	Kazakhstan	Peru	Venezuela, RB
Comoros	Kenya	Philippines	Vietnam
Congo, Dem. Rep.	Kuwait	Qatar	West Bank and Gaza
Congo, Rep.	Kyrgyz Republic	Romania	Yemen, Rep.
Costa Rica	Lao PDR	Russian Federation	Zambia
Cote d'Ivoire	Latvia	Rwanda	Zimbabwe
Croatia	Lebanon	Samoa	

Table A2: Variables description and sources

Name	Description	Source	Unit
Border	Number of illegal border crossing detected per origin country (2009-16)	Frontex	Detection
Asylum	Number of Asylum applications filed in Italy per origin country	UNHCR	Applicant
Regular	Number of migrants obtaining a residence permit in Italy	OECD	Individual
Immigrant stock	Number of foreign nationals residing in Italy	OECD and ISTAT	Individual
Italy ODA	Aggregate gross disbursement from Italy to origin country	OECD/CRS	USD 2015
Other ODA	Total aggregate gross disbursement received by origin country excluding bilateral aid from Italy	OECD/CRS	USD 2015
Pop Origin	Total population in origin country	WDI	Individual
Dependency Ratio Origin	Share of population below15 and above 64	WDI	Rate
Unemployment Origin	Unemployment rate in origin country, ILO modelled	WDI	Rate
GDPpc Origin	per capita GDP in origin country at 2010 constant USD	WDI	USD 2010
Political Freedom Origin	Political Rights and Civil Liberties are both measured on a one-to-seven scale, with 1 representing the highest degree of freedom, and then summed so that the total index varies from 2 to 14	Freedom House	Index
Immig. Policy Restrict.	Each legislative measure is coded (+1 = more restrictive; -1 = less restrictive) and summed to obtain a yearly cumulative index per origin country	Own based on DEMIG	Index
Conflict Origin	Dummy for violent conflict registered in a given year in origin country (0 = no conflict)	UCDP-PRIO	Dummy
Nat Disaster Origin	Number of natural hazardous events with more than 100 people affected registered	EM-DAT	Event
Distance	GPS distance between Italy and Origin country (capital cities)	CEPII	Km
Language Prox	Unadjusted Value of Linguistic proximity (ASJP), from 0 to 1 (1 = same language)	CEPII	Index
Colony	Dummy for colonial linkages and common history (0 = No)	Own	Dummy
FDI inflows	Gross FDI inflows into origin countries at 2010 constant USD	WDI	USD 2010
Total remittances	Total inflow of remittances into origin countries at current prices	World Bank	USD current
Bilateral Exports	Exports from Italy to origin countries (as share of origin country GDP)	Own based on UNCOMTRADE	Rate
Bilateral Trade	Share of trade between Italy and origin countries (as share of total trade of country of oring)	Own based on UNCOMTRADE	Rate
Trade Openness	Total exports and imports of origin countries (as share of origin country GDP)	Based on WDI	Rate

Table A3: Robustness check: 3SLS estimates for bilateral ODA equations

	Ln(Italy ODA)		Ln(Italy ODA)		Ln(Italy ODA)	
	Full	LOW	Full	LOW	Full	LOW
Ln(Italy exp)	0.079	0.13	0.39**	0.47**	0.12	0.20
	(0.13)	(0.18)	(0.17)	(0.19)	(0.14)	(0.18)
Ln(GDPpc Origin)	0.33	1.13	-1.59	0.95	-0.16	0.32
	(0.72)	(1.27)	(1.08)	(1.89)	(0.70)	(1.04)
Ln(Pop Origin)	-1.27	-4.07	-1.72	3.57	-1.95*	-3.86
	(1.07)	(2.60)	(4.97)	(11.2)	(1.11)	(2.53)
Political Freedom Origin	-0.16*	-0.35**	-0.031	0.059	-0.18**	-0.19
	(0.083)	(0.17)	(0.10)	(0.15)	(0.088)	(0.13)
Nat Disaster Origin	0.34**	0.47**	-0.14	0.030	0.28*	0.43**
	(0.15)	(0.22)	(0.18)	(0.26)	(0.16)	(0.22)
Conflict Origin	-0.095	-0.13	-0.067	-0.29	0.18	0.11
	(0.31)	(0.42)	(0.36)	(0.44)	(0.30)	(0.37)
Ln(Asylum)$_t$	0.66**	0.60				
	(0.27)	(0.55)				
Ln(Border)$_t$			0.085	-0.47		
			(0.65)	(0.77)		
Ln(Regular)$_t$					1.01***	0.30
					(0.37)	(0.49)
Year FE	x	x	x	x	x	x
Origin FE	x	x	x	x	x	x
Observations	1928	967	1098	486	1898	964
R^2	0.82	0.73	0.89	0.84	0.81	0.74
Timeframe	2003-16		2009-16		2003-16	

Notes: standard errors in parentheses. LOW: sample includes recipients with GDPcapita below 8,000 PPP$. * p<0.10, ** p<0.05, *** p<0.010

Table A4: Robustness check: 3SLS estimates for total ODA equations

	Ln(Other ODA)		Ln(Other ODA)		Ln(Other ODA)	
	Full	LOW	Full	LOW	Full	LOW
Trade Open. (GDP%)	-0.0022	-0.0031	-0.0083	0.0045***	-0.0024	-0.0028
	(0.0031)	(0.0022)	(0.0053)	(0.0016)	(0.0032)	(0.0019)
Ln(GDPpc Origin)	2.57***	0.37	3.51***	-0.37	1.90***	-0.52
	(0.52)	(0.48)	(0.84)	(0.35)	(0.51)	(0.33)
Ln(Pop Origin)	-2.33***	-5.89***	-8.60**	1.45	-3.32***	-5.45***
	(0.79)	(1.01)	(3.99)	(2.09)	(0.83)	(0.84)
Political Freedom Origin	-0.15**	-0.40***	0.084	-0.044	-0.23***	-0.29***
	(0.063)	(0.067)	(0.084)	(0.028)	(0.067)	(0.043)
Nat Disaster Origin	0.15	0.040	0.12	-0.0078	0.084	0.063
	(0.12)	(0.086)	(0.15)	(0.050)	(0.12)	(0.072)
Conflict Origin	-0.13	-0.10	-0.13	0.0047	0.23	0.23*
	(0.23)	(0.17)	(0.29)	(0.083)	(0.22)	(0.12)
Ln(Asylum)$_t$	0.88***	0.73***				
	(0.20)	(0.21)				
Ln(Border)$_t$			1.16**	-0.24*		
			(0.52)	(0.14)		
Ln(Regular)$_t$					1.60***	0.47***
					(0.28)	(0.16)
Year FE	x	x	x	x	x	x
Origin FE	x	x	x	x	x	x
Observations	1928	967	1098	486	1898	964
R^2	0.90	0.58	0.93	0.89	0.90	0.70
Timeframe	2003-16		2009-16		2003-16	

Notes: standard errors in parentheses. LOW: sample includes recipients with GDPcapita below 8,000 PPP$. * p<0.10, ** p<0.05, *** p<0.010