

Dreher, Axel; Gassebner, Martin; Schaudt, Paul

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The effect of migration on terror: Made at home or imported from abroad?

Axel Dreher

Department of Economics, Heidelberg University; University of Goettingen; CEPR; CESifo; KOF Swiss Economic Institute

Martin Gassebner

Institute of Macroeconomics, University of Hannover; CESifo; KOF Swiss Economic Institute

Paul Schaudt

Department of Economics, University of St. Gallen

Abstract. We analyze how a country's immigrant population—defined as the stock of people born abroad—affects the probability of a terrorist attack in the host country. Using data for 20 OECD host countries and 183 countries of origin over the 1980–2010 period our OLS and 2SLS regressions show that the probability that immigrants from a specific country of origin conduct a terrorist attack in their host country increases with a larger number of foreigners from such countries living there. However, this scale effect does not differ from the effect domestic populations have on domestic terror. We find scarce evidence that terror is systematically imported from countries with large Muslim populations or countries where terror networks prevail. Policies that exclude foreigners already living in a country increase rather than reduce the risk that foreign populations turn violent, and so do terrorist attacks against foreigners in their host country. Highly skilled migrants are associated with a significantly lower risk of terror compared with low skilled ones, while there is no significant difference between foreign-born men and women.

Résumé. L'effet de l'immigration sur le terrorisme: pur produit domestique ou importé de l'étranger? Dans cet article, nous analysons la façon dont la population immigrée d'un pays, c'est à dire la population née à l'étranger, a une incidence sur la probabilité d'une attaque terroriste dans le pays d'accueil. En s'appuyant sur les données de 20 pays d'accueil de l'OCDE et 183 pays d'origine, nos régressions à moindres carrés

Corresponding author: Axel Dreher, mail@axel-dreher.de

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ordinaire (MCO) et à doubles moindres carrés (2SLS) montrent que la probabilité que des immigrants d'un pays d'origine particulier commettent une attaque terroriste dans leur pays d'accueil augmente dès lors que la population immigrée de même nationalité est importante au sein du même pays. Néanmoins, cet effet d'échelle ne diffère en rien de celui que peut avoir les populations nationales sur le terrorisme local. Nous n'avons pas dégagé de preuves évidentes corroborant le fait que le terrorisme est systématiquement importé de pays à vaste population musulmane ou de pays où règnent de nombreux réseaux terroristes. Les politiques excluant les étrangers vivant déjà dans un pays ont plutôt tendance à augmenter le risque que cette population ne se tourne vers la violence que de le réduire, et il en va de même pour les attaques terroristes visant les étrangers dans leur pays d'accueil. En matière de terrorisme, les immigrants hautement qualifiés sont associés à un risque sensiblement moindre que les immigrants peu qualifiés, et il n'y a pas de différence significative entre les hommes et les femmes nés à l'étranger.

JEL classification: D74, F22, F52, P48

....[T]he attacks of September 11, 2001, showed that some [immigrants] come to the United States to commit terrorist acts, to raise funds for illegal terrorist activities, or to provide other support.

—George W. Bush (2001)

Immigration and Jihad go together. One is the consequence of the other and dependent upon it.

—Imam Abu Baseer¹

1. Introduction

In 2017, US President Donald Trump banned citizens from a number of Muslim-majority countries from immigrating to the United States, with the explicit purpose of preventing terrorists to enter. Indeed, plenty of anecdotes and opinion-based writings, in concert with a number of descriptive evaluations of terrorist events link immigrants to a higher risk of terrorism within the host country.² Systematic evidence on whether or not immigration increases terrorism is, however, surprisingly scarce.³ In this paper, we aim to provide

1 Abu Baseer is a leading religious supporter of al Qaeda (Leiken 2004). Cited in (Paz 2002, p. 73).

2 A particularly prominent example of opinion-based “analysis” is Malkin’s (2002) bestseller *Invasion*, suggesting a range of discriminatory measures against immigrants to prevent the migration of terror. According to (Camarota 2002, p. 5), “there is probably no more important tool for preventing future attacks on US soil than the nation’s immigration system.” In the raw data that we use for this paper, an average person born abroad is four times more likely to commit a terrorist act compared with the average citizen of a country.

3 The only systematic statistical analysis we are aware of reports a negative correlation between migration and terrorist attacks (Bove and Böhmelt 2016). While not focusing on migration, Milton et al. (2013) show that the number of refugees hosted in a country are correlated with a larger number of terrorist

such evidence. We analyze how a country's immigrant population—defined as the stock of people born abroad—affects the probability of a terrorist attack in the host country.⁴

We estimate OLS and 2SLS regressions for 20 OECD host countries and 183 countries of origin over the 1980–2010 period. The analysis is at the host–origin–year level, which allows us to take account of dyad-specific fixed effects and test how origin- and host–origin-specific features mediate the effect of foreign populations on terror. Our instrumental variables regressions predict the stock of foreigners with the interaction between two sets of variables. Variation across host–origin dyads results from structural characteristics between the country of origin and the host, while variation over time (and dyads) originates from changes in push and pull factors between host and origin countries resulting from natural disasters. Controlling for the levels of these variables and fixed effects for dyads and years, the interactions provide powerful and arguably excludable instruments. As we explain in some detail below, the intuition of our instrumentation strategy is in analogy to a difference-in-differences estimator, where we assume changes in the number of disasters in host and origin countries to differentially affect terror in countries with different structural characteristics exclusively due to the number of migrants there (rather than via any omitted variables).

The nature of our data and methods implies two caveats that we would like to discuss up front. The first is endogeneity. There is evidence that terrorism leads to a backlash against migrants (Gould and Klor 2016) and that immigration policies are shaped by the perceived threat of terror.⁵ What is more,

attacks. Other previous studies that address the link between terror and migration either examine the effect of terror on migration (e.g., Dreher et al. 2011) or employ data on terrorists with immigration status rather than relying on systematic cross-country time-series data on migration and terror attacks (Kephart 2005, Leiken 2004, Leiken and Brooke 2006). Studies focusing on terrorists with immigration background find a close link between immigration and terrorism. Given that they do not examine overall flows of immigration but only those cases in which immigrants have been involved in terrorist activity, these studies do not provide an accurate picture of the relation between migration and terrorism.

- 4 We use the terms migrants, foreigners and foreign-born populations to refer to them but have no data to differentiate these facets.
- 5 Countries threatened by terrorist attacks respond to this threat to their values by diminishing the very rights they aim to protect in the first place (Dreher et al. 2010). An area particularly prone to human rights restrictions is immigration and asylum policy. Plenty of evidence suggests that stricter immigration and visa policies are a preferred reaction to terrorist attacks (Fitzpatrick 2002, Martin and Martin 2004, Avdan 2014). After the September 11, 2001, (hereafter 9/11) attacks on the United States, then-US President George W. Bush issued a presidential directive introducing stricter immigration policies to combat terrorism. The Department of Homeland Security (DHS) was founded in 2003, explicitly linking immigration policies to anti-terrorism

a large number of omitted variables is potentially correlated with the number of terror attacks and the stock of immigrants. While our instrumental variable closely follows other work in the recent migration literature, so that we think our results are as credible as any of those,⁶ bullet-proof identification remains impossible in our setting, and one can always think of potential violations of our identifying assumptions. The absence of bullet-proof identification implies that our results should be taken with a grain of salt. At the same time, the importance of the questions we investigate here makes us believe that conditional correlations will be of interest to a wide audience of scholars and policy-makers.

The second caveat of our analysis relates to the focus on countries and years rather than on individual migrants and terrorists. Such data do not allow us to test whether individual migrants have turned into terrorists. Instead, they allow us to test the effect of migration on the overall risk of terror. Migrants can arguably affect terror in a number of ways. First, migrants can turn into terrorists themselves. Second, their presence can affect the probability that others turn violent. Third, larger networks of migrants from the same country, including friends and family, might reduce the risk that foreigners already living in the country turn violent. Fourth, the inflow of people with anti-Western sentiment might make migrants of the second or third generation living in a country turn violent, and thus increase the risk of terror even if the additional migrants themselves do not commit terrorist acts. Our focus on country-level data thus also implies advantages, as only such data are suited to test these aggregated net effects of migration on terror.

A skeptical reader might also criticize that we focus on stocks of foreigners rather than flows. However, based on terrorists' *vitas* summarized in the previous literature, in the vast majority of cases, foreigners committing terror have lived in the country they attack for an extended period of time.⁷

strategies (Kerwin 2005). A number of additional discriminatory measures have since been implemented, among them exceptional powers to the Attorney General to detain foreigners without a hearing and proof of guilt if there are "reasonable grounds to believe" these foreigners are involved in terrorist activity, ethnic profiling and required registration for certain groups of entrants—in particular from Muslim states (Spencer 2007). As Spencer (2007) summarizes, France, Germany, Spain and the UK, among others, have similarly tightened immigration laws or procedures in response to the terrorist attacks of 9/11.

6 In particular see Alesina et al. (2016) and Docquier et al. (2016).

7 Camarota (2002) investigates how 48 foreign-born Islamic terrorists entered and remained in the United States in the 1993–2001 period. Leiken (2004) focuses on 212 suspected and convicted terrorists in North America and Western Europe from 1993 to 2003. Kephart (2005) covers the immigration histories of 94 terrorists operating in the United States in the 1990–2004 period, while Leiken and Brooke (2006) coded 373 terrorists belonging to organizations with global reach over the years 1993 to 2004. For example, the metro and rail bombings in Paris during the mid-1990s have been conducted by "legal" French

Rather than entering as a terrorist, it seems that the bulk of future terrorists immigrate without the intention to be involved in terrorism and only later become terrorists. They get into contact with terrorists living in their host country or when returning to their country of origin for holiday or business.⁸

In the empirical analysis below, we therefore test whether and to what extent the stock of foreigners living in a country is related to the level of terror, rather than focusing on recent entrants. Focusing on stocks rather than flows comes with an additional advantage. Larger networks of foreigners already living in a country facilitate further immigration. Higher numbers of foreigners thus facilitate the actions of terrorists as well, given that they might find it easier to enter and live in the country, potentially illegally.

We are interested in whether foreign nationals living in a host country lead to a higher probability of terrorist attacks originating from nationals of this country in their host country. Arguably, the absence of such a pure “scale effect” would be surprising. An increasing number of people living in a country mechanically increases the probability to observe violence originating from that group (Jetter and Stadelmann 2019). Such correlation is comparable to those between the size of the domestic population living in a country and the number of terrorist attacks pursued by them (Krueger and Malečková 2003). In light of the scale effect population size has on domestic terror according to the previous literature, the absence of a positive correlation between the number of foreigners and the number of attacks pursued by foreigners would imply that foreigners are less likely to become terrorists than the domestic population. It is therefore important to put the effect of foreigners on the probability of foreign attacks in perspective and provide a comparison with how the number of native-born affects terrorism by those native-born.⁹

Muslim citizens of Algerian origin (Leiken 2004). The leader of the French cell responsible for the bombings, Khaled Kelkal, e.g., immigrated to France from Algeria as an infant in the 1970s (Leiken 2004). In these and all of the other examples provided in Kephart (2005), immigration happened many years before the involvement in any terrorist activity. The three future 9/11 hijackers from the Hamburg cell came to Germany as legal immigrants and only later came in contact with fundamentalist networks (Leiken 2004). A more recent example is Najim Laachraoui who is alleged to be involved in the suicide terrorist attack on Brussels’s airport in March 2016 (as well as in the Paris attacks of November 2015). Laachraoui was born in Morocco but migrated to Belgium as a child (www.nbcnews.com/storyline/brussels-attacks/najim-laachraoui-what-we-know-about-suspected-bomb-maker-n543996, accessed November 13, 2016).

8 Note that there is substantial variation over time in the stock of foreigners in our sample. The median change from one period to another is 25%. The fifth percentile of the distribution of changes in this stock is -20% ; the 95th percentile is 214%.

9 We would, however, like to stress from the outset that we have no instrument for the domestic population so that our comparative analysis does not claim to identify causal effects.

It is also important to understand what factors influence this scale effect. We analyze three groups of variables: the political and economic environment in the host country, characteristics of the origin country and the composition of migrant stocks.¹⁰ First, we hypothesize that a host country's policies and environment are crucial in the fight against terror. One important dimension concerns the extent to which immigrants are integrated into the culture and society of their host country (Leiken 2004, Rahimi and Graumans 2015). Well-integrated foreigners are less likely to engage in terror against their host country population. Tensions among the host and foreign populations, to the contrary, will increase the propensity (of foreigners and native-born, arguably) to engage in terrorist activity (Findley et al. 2012). Most important, we expect terrorist groups to have an easier time recruiting foreigners for the fight against the host country's population if they themselves are the target of political violence from the domestic population.

Furthermore, we expect immigrants' prospects to earn their living and obtain positions of respect in their host countries to be crucial. Policies aimed at forced integration—putting pressure on immigrants to assimilate, learn the language of their host country, or change the way they dress or exercise their religion—can turn either way. To the extent these policies are successful and result in better-integrated immigrants, they can help to reduce terror in the future. Yet restrictions and pressure on immigrants on areas of their lives they deem important can as well raise resistance and alienation and thus achieve the opposite effect (see Fouka 2020).

A second important dimension of host country policies concerns immigration and integration. Policies on immigration and integration are officially, at least in part, designed to reduce the risk of terror. It is, however, not clear if stricter immigration policies do in fact reduce the probability that foreigners commit terror, since their effect on foreigners already living in the host country is not well understood. Such policies could be perceived as acts of repression, racism and humiliation by foreigners already residing in the host country, leading to alienation and resistance and thereby increasing terror. While we cannot test these mechanisms directly, we can test if stricter immigration and integration policies reduce the risk that migrants engage in terror against their host country.

We also allow for the possibility that migrants from different countries engage in terrorist activity to a different extent. Anecdotal evidence suggests that foreigners with Muslim background are particularly likely to engage in terrorist activity (e.g., Camarota 2002). As Enders and Sandler (2006) point out, the marginal costs of terrorism are particularly low in countries with large Muslim populations, while resources required to conduct terror are plenty. The immigration of people from Muslim-majority countries could thus be one channel by which migration affects terror. We test whether the effect of

10 Kis-Katos et al. (2014) document that the determinants of terrorism can be heterogeneous.

immigrants from Muslim-majority countries differs from those of other countries. We also test whether immigrants from countries where terrorist networks prevail are more likely to be involved in terror and to what extent migrants are more prone to engage in terrorism if the host country is engaged in military conflict with the country of origin.¹¹ Conflict has been shown to either directly increase the risk of a country's citizens being involved in terrorist activity or to make them more violent in general (Montalvo and Reynal-Querol 2005, Esteban et al. 2012, Campos and Gassebner 2013). Regarding terror, Bove and Böhmelt (2016) provide evidence of a spatial spillover among countries. They show that countries closer to countries rich in terror are more likely to experience terror themselves (with "closer" being measured by the number of migrants from a country, among others). Hence, we expect foreigners born in countries with populations involved in substantial terrorist activity or with large terrorist networks present to be particularly violent.

Finally, we investigate whether the composition of migrant populations affects whether or not migration causes terror. The role of gender and education has received some attention in the previous literature. While the earlier literature tends to characterize women as victims of terror, more recent discussions acknowledge their role as perpetrators as well (Agara 2015). We therefore examine the role of foreign-born men and women separately in addition to investigating their joint effect. We have, however, no clear hypothesis regarding the importance of gender for the effect of migration on transnational terror. The role of education is equally unclear. While many believe poverty and lack of education to be among the root causes of terrorism, parts of the previous literature have shown that terrorists are often well educated compared with their peers (Krueger and Malečková 2003).

Our results show that terror becomes more likely with a larger number of foreigners living in a host country. This scale effect relating larger numbers of foreigners to more attacks does not, however, imply that foreigners are more likely to become terrorists compared with the domestic population. When we calculate the effect of a larger native-born population on the probability of terror attacks by native-born, we find this effect to be of comparable size. Overall, we thus conclude that a rising stock of foreigners living in a country does not increase the risk of terror more than does domestic population growth.

We find that domestic policies relating to the integration and rights of immigrants affect the probability that foreigners turn violent. More specifically, our analysis demonstrates that restrictions on migrants' rights and stricter integration laws increase the effect that the size of migrant populations have on terror. It seems that stricter policies segregating foreigners already living in a country lead to alienation and resistance, increasing the risk of terror arising from those populations rather than reducing it. Host country policies

11 As Leiken (2004, p. 87) puts it: "For the production of terrorists what could be more ideal than Algeria—with its modern history of violent political struggle and a vicious fundamentalist resistance movement?"

thus affect terror in ways other than commonly perceived. What is more, we find that a larger number of attacks against foreigners in the host country increases the risk of terror by foreigners there.

Our results also show that highly skilled migrants are associated with a significantly lower risk of terror compared with low skilled ones, while there is no significant difference in terror arising from male compared with female migrant populations. With some exceptions, we do not find migrants coming from Muslim-majority countries and those coming from countries with particularly pronounced terrorist networks or high domestic terror activity to increase the likelihood of terror compared with other foreign populations.

We outline our data in section 2 and our empirical strategy in section 3. Section 4 shows the main results, section 5 tests robustness and section 6 concludes and discusses policy implications.

2. Data

We aim to test whether a larger number of foreigners from a particular country increases the probability of terrorist attacks by people of that nationality in their host country. We define $TERRO\!R_{hot}$ as a binary indicator that is one if at least one terrorist attack is conducted by nationals of origin o in host country h during year t .¹² Our main variable of interest ($FOREIGNERS_{hot}$) is the log number of foreigners born in country o and living in country h at time t . While a pure scale effect of a larger number of foreigners living in a country on terror attacks pursued by people of that nationality would be unsurprising, we are interested in how the effect compares to terrorist attacks committed by the domestic population.

We construct our terror indicator from the *International Terrorism: Attributes of Terrorist Events* (ITERATE) database (Mickolus et al. 2017). ITERATE is the only database that provides data on transnational terrorist acts, including information about the nationality of perpetrators and victims.¹³

Our data on foreign-born populations are taken from the Institut für Arbeitsmarkt- und Berufsforschung's (IAB) brain-drain dataset (Brücker et al. 2013). The IAB defines immigrants as the number of foreign-born individuals

12 Note that we use a binary indicator since 99.5% of our dyad year observations show no transnational terror events. Of the remainder, around 80% are one, 15% are between two and four and the remaining 5% range between five and 17 incidents.

13 Mickolus et al. (2017, p. 2) define transnational terrorism as “the use, or threat of use, of anxiety-inducing, extra-normal violence for political purposes by any individual or group, whether acting for or in opposition to established governmental authority, when such action is intended to influence the attitudes and behavior of a target group wider than the immediate victims and when, through the nationality or foreign ties of its perpetrators, its location, the nature of its institutional or human victims, or the mechanics of its resolution, its ramifications transcend national boundaries.”

aged 25 and older living in a country other than the country they were born (not distinguishing between “regular” migrants and refugees).¹⁴ The data are based on harmonized census data of 20 OECD host countries. The dyadic data include the stocks of immigrants from 187 countries of origin in the host countries in five-year intervals over the 1980–2010 period. Compared to other datasets, the main advantage of the IAB data is that they provide a complete time series for each host–origin pair.¹⁵ Since the stock of foreigners typically evolves slowly over time, we linearly interpolate the years in between the five-year intervals. We expect this to introduce random noise, while allowing us to exploit yearly variation in the terrorist data.¹⁶

Figure 1 gives a first impression of the data. The left panel shows the number of transnational terrorist attacks by *FOREIGNERS* in OECD host countries (light grey line) over the 1980–2010 period. As can be seen, the number of attacks steadily decreased over time, with total numbers in a decade ranging from 479 in the 1980s to 138 in the 1990s and 45 in the 2000s.

The figure also shows the number of terrorist attacks of OECD *NATIVES* on *FOREIGNERS* in their country as well as of *NATIVES* on *NATIVES* (“domestic terror”).¹⁷

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- 14 The exception is Germany, for which data on the foreign-born population before 2009 are unavailable, so that a citizenship-based definition of foreigners is used (Brücker et al. 2013, p. 3). Germany differs also as an origin country, since the migrant stocks of East and West Germany in other countries have been aggregated prior to unification. The same procedure was implemented for South and North Yemen. For a more detailed discussion of the IAB harmonization procedure, see Brücker et al. (2013).
- 15 This is important, since observations for the stock of migrants missing from other data sources in particular countries and (different) years are unlikely to be missing at random, but rather for reasons that could be correlated with terror itself. The downside is that we do not observe the foreign-born population aged 24 or younger, but the correlation with data from the World Bank (Özden et al. 2011) that include those migrants is 0.95 in the set of years reported by both sources (1980, 1990, 2000).
- 16 To the extent that the timing between changes in the stock of foreigners and terror is smoothed, this could, however, result in attenuation bias. To test this possibility, we report results without interpolation in section 5. We also test robustness by excluding host-country and origin-country observations where inflows or outflows of migrants surge due to the effect of refugee crises (and noise introduced by linear interpolation is consequently most severe).
- 17 We calculate the number of *NATIVES* by subtracting the number of *FOREIGNERS* from the host country’s total population, taking data on total population from the World Bank (2016). These data include foreigners, according to the World Bank’s definition of the series: “Total population is based on the de facto definition of population, which counts all residents regardless of legal status or citizenship. The values shown are midyear estimates” (World Bank 2016). Unfortunately, these data do not allow us to

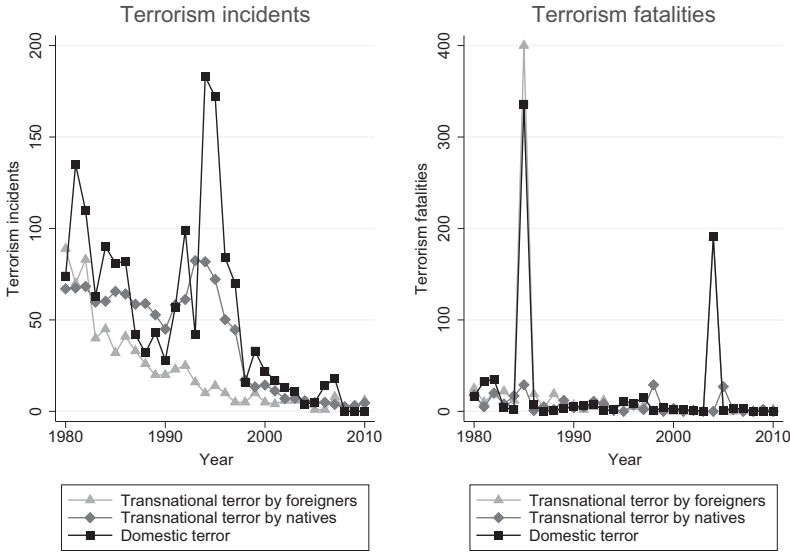


FIGURE 1 Terror incidents and fatalities in the OECD over time
 NOTES: The figure shows the number of transnational (ITERATE) and domestic terror events (Enders et al. 2011 and Gaibulloev et al. 2012 based on GTD) over time. We have omitted 9/11 from the graph so that the movements in other years are more visible.

The figure shows that the bulk of attacks are committed by *NATIVES* within their own countries both against fellow *NATIVES* (black line) and against *FOREIGNERS* (dark grey line). Attacks by *NATIVES* on either *NATIVES* or *FOREIGNERS* exceed those by *FOREIGNERS* most of the time. Fatalities from these attacks are typically infrequent, as can be seen from the right panel of figure 1. There are two exceptions. The first spike in the figure represents an attack on Air India Flight 182 in 1985, resulting in 331 fatalities. The second is due to attacks on a subway in Madrid in 2004 (we have omitted 9/11 from the graph so that the movements in other years are more visible).

To put these numbers in perspective, table 1 reports the yearly average and total number of terrorist attacks in each OECD country during the 1980–2010

distinguish separate effects of migrants returning to an OECD country that have potentially been exposed to terrorism while being abroad. Note that ITERATE includes exclusively terrorist events in which the location, perpetrator and victim do not have the same nationality. Terror conducted by *NATIVES* of country h within h thus captures exclusively events in which *NATIVES* attack *FOREIGNERS*. Domestic attacks are those where both the perpetrator and the victim originate from the country in which the attack takes place (taken from Enders et al. 2011 and Gaibulloev et al. 2012 based on data from the Global Terrorism Database, GTD).

TABLE 1
Decomposition of terror incidents, 1980–2010

Host country	Sum of terror incidents (total)	Average number of terror incidents per year	Percentage committed by native-born	Percentage committed by foreign-born	Terror committed per million native-born	Terror committed per million foreign-born
Australia	21	0.68	0.71	0.29	0.03	0.06
Austria	62	2.032	0.71	0.29	0.19	1.07
Canada	31	1.00	0.52	0.48	0.02	0.11
Chile	66	2.13	0.95	0.05	0.15	0.16
Denmark	29	0.94	0.59	0.41	0.11	1.89
Finland	0	0.00	0.00	0.00	0.00	0.00
France	468	15.10	0.67	0.33	0.18	1.36
Germany	753	24.29	0.87	0.13	0.28	0.77
Greece	323	10.42	0.89	0.11	0.93	2.28
Ireland	28	0.90	0.18	0.82	0.05	3.29
Luxembourg	3	0.10	0.33	0.67	0.10	0.68
Netherlands	76	2.45	0.63	0.37	0.11	0.90
New Zealand	5	0.16	1.00	0.00	0.05	0.00
Norway	11	0.35	0.64	0.36	0.05	0.67
Portugal	68	2.19	0.90	0.10	0.20	0.60
Spain	411	13.26	0.91	0.09	0.31	0.68
Sweden	25	0.81	0.64	0.36	0.06	0.39
Switzerland	70	2.23	0.58	0.42	0.22	0.88
UK	747	24.10	0.92	0.08	0.40	0.62
United States	305	9.84	0.60	0.40	0.02	0.21
Average	175.5	5.65	0.66	0.29	0.17	0.83

NOTES: Results are based on the average number of native-born and foreigners within the host countries during the 1980–2010 period. The total number of terror attacks refers to the sum of terror attacks committed within the host country, by nationals against nationals (Enders et al. 2011 and Gaibullov et al. 2012), by nationals against foreigners (ITERATE 2015) and by foreigners within the host country regardless of the targets' nationality (ITERATE 2015).

period, along with the percentage of those numbers committed by *NATIVES* compared with by *FOREIGNERS*. The table shows that the large majority of attacks originate from *NATIVES*. However, when we focus on the number of attacks by *NATIVES* and *FOREIGNERS* per one million people, the number of attacks by foreigners dominates by a factor of four. Specifically, for every one million people, 0.17 terrorist attacks are conducted on average by *NATIVES* per country and year, while the corresponding number for *FOREIGNERS* is 0.83. The table also illustrates that though the probability that the average individual becomes a terrorist is low, terror events are frequent. Over the sample period, Germany experienced 753 events. Of those incidents, 97 were committed by foreigners, while the rest were perpetrated by German citizens, either against foreigners (215) or against other Germans (441). There were 470 events in France (154 committed by foreigners), 412 in Spain (35 committed by foreigners) and 319 in Greece (36 committed by foreigners). The maximum number of foreign terror attacks in the host countries of our sample in a single year is 35 in the United States, in 1982. In our universe of host countries,

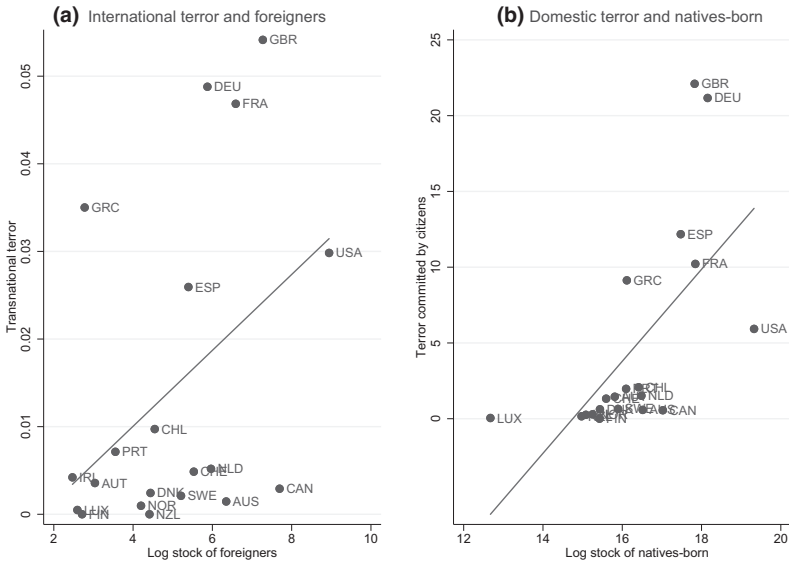


FIGURE 2 Transnational and domestic terror incidents across the OECD
 NOTES: Panel (a) plots the yearly average (log) stock of the foreign-born population against average international terror. Panel (b) shows domestic terror and the average (log) size of the native-born population.

there are 10 attacks by foreigners in the median year (1996): four attacks in Germany and three attacks in both France and the United States.¹⁸

Figure 2 further illustrates the scale effects of foreign and domestic populations with respect to terror. The left panel shows that the number of attacks by *FOREIGNERS* increases with the stock of migrants living in an OECD country. According to the right panel of figure 2, the number of *NATIVES* living in an OECD country is positively correlated with the number of terrorist attacks by *NATIVES*. Both correlations are unsurprising.

18 Specifically, in Germany, a UK national affiliated with the Irish Republican Army (IRA) fired mortar grenades towards UK military barracks. The other three attacks were conducted by Turkish citizens against Turkish facilities. In France, two attacks were conducted by Algerians affiliated with the Islamic Armed Group Algeria, of which one was a bombing attack on a commuter train in Paris killing four people and injuring 84. The third attack in France in that year was prevented by the authorities (an Iranian citizen who planned a terror attack against Israeli facilities). In the United States, two attacks were committed by Cuban nationals. One was an arson attack against an attorney representing the widow of a leftist guerrilla, the other a “sniping at a building.” The third terror attack involved a Romanian citizen who was arrested while trying to smuggle arms to conduct a terrorist attack.

3. Empirical strategy

3.1. Base specification

Our analysis is dyadic and focuses on terror in host country h from people born in origin country o , in year t . This allows us to hold constant all confounders that do not vary across host–origin dyads over time as well as testing a number of hypotheses that relate to the origin of migrants. We test the effect of *FOREIGNERS* on *TERROR* with the following baseline specification, using a linear-probability model (and clustering standard errors at the host–origin dyad):

$$TERROR_{hot} = \beta FOREIGNER_{hot} + \mathbf{X}'_{hot}\psi + \eta_{ho} + \gamma_t + \epsilon_{hot}, \quad (1)$$

where \mathbf{X}'_{hot} is a set of time-varying control variables, η_{ho} are dyadic host–origin fixed effects that control for the general relations between countries (e.g., historical conflict), γ_t are year fixed effects and ϵ_{hot} is an error term.

In our main specifications, we assume that terrorist attacks react to changes in our explanatory variables in the same year. This is likely to be the case if terrorist attacks are largely based on short-term changes that foreigners expect to affect their situation in the future or if the attacks are direct reactions to recent policy changes. We rerun all specifications including explanatory variables as (lagged) five-year moving averages to allow for longer lags between changes in policies and outcomes and the actions of terrorists, among other tests for robustness.

Following the previous literature on bilateral terror (Blomberg and Rosendorff 2009, Neumayer and Plümper 2009, Plümper and Neumayer 2010), we include the natural logarithm of host and origin GDP as well as their populations as our basic control variables.¹⁹ The resulting dataset covers more than 102,000 dyadic observations from 183 origin countries in 20 OECD countries, over the 1980–2010 period. Our basic regressions ignore the obvious problem of reversed causality and omitted variable bias, though our dyad fixed effects arguably capture some of them.

We proceed by including a number of interactions to test more nuanced hypotheses:

$$TERROR_{hot} = \beta FOREIGNER_{hot} + \theta(FOREIGNER_{hot} \times INT_{ho,t-1}) + \delta INT_{ho,t-1} + \mathbf{X}'_{hot}\psi + \eta_{ho} + \gamma_t + \epsilon_{hot}, \quad (2)$$

where $INT_{ho,t-1}$ represents the variables that we hypothesized to change the effect of *FOREIGNERS* on *TERROR* above.²⁰ These variables are moving

19 We test the robustness of our findings below by including additional control variables that have been identified as robust correlates of terrorism in previous work (Gassebner and Luechinger 2011).

20 Note that some of them vary across dyads and time, while others are constant across either host or origin countries, as we explain below. Online appendix A reports the exact definitions and sources of all variables, while appendix B

averages over five years, as we expect foreigners to react to a country's (recent) general trend in policies rather than to year-to-year changes. We lag them by one period, since we assume that the effect of these variables on how migration affects terror is not likely to be immediate.

First, we measure conflict ($CONFLICT_{ho,t-1}$) with the fraction of years a host–origin pair is in a military conflict over the $t-5$ to $t-1$ period, based on data taken from the UCDP/PRIO Armed Conflict Dataset V.4-2015 (Gleditsch et al. 2002, Pettersson and Wallensteen 2015).

We include host–origin-specific indicators of (changes in) the restrictiveness of immigration, migrant rights and repression and integration, broadly following the approach of Mayda (2010) and Ortega and Peri (2013). As they do, we measure changes in “restrictiveness” with respect to the first year in our sample, based on data from the dyad-specific DEMIG database of the International Migration Institute (DEMIG 2015, De Haas et al. 2015).²¹ In the initial year (1980, for most of the dyads in our sample), we code restrictiveness as “zero.”²² In each following year, we count the number of policies that make migration more or less restrictive. We then add (subtract) the number of policies that make migration more (less) restrictive in each year. The resulting indicator rises in years in which the number of more restrictive policies exceeds the number of policies that make migration less restrictive. The indicator falls in years in which liberalization dominates.

We measure policies that either regulate the rights of foreigners living in the respective host country or the degree of surveillance and sanctions employed against them ($RIGHTS_{hot}$ and $SANCTIONS_{hot}$).²³ Higher scores imply that integration policies are more restrictive, fewer rights are granted and surveillance is more extensive. $RIGHTS_{hot}$ covers policy measures that affect

shows descriptive statistics. Appendix C shows the countries included in our sample.

- 21 An obvious alternative to DEMIG is the International Migration Policy and Law Analysis (IMPALA) Database (Beine et al. 2016), which, however, currently covers only 10 years from nine countries.
- 22 While one might expect the level of restrictions to be more important than changes in restrictions, such country-specific levels are taken account of by the host–origin fixed effects included in our regressions (with which the initial level of policies would be perfectly correlated).
- 23 $RIGHTS_{hot}$ covers policies that fall into DEMIG's categories recruitment/assisted migration program, resettlement programs, language, housing and cultural integration programs, access to social benefits and socio-economic rights, access to justice and political rights, access to permanent residency and access to citizenship (DEMIG 2015). $SANCTIONS_{hot}$ refers to surveillance technology/control powers, identification documents, detention, carrier liabilities, employer liabilities and other sanctions (DEMIG 2015). $RIGHTS_{hot}$ ($SANCTIONS_{hot}$) ranges from -21 to 10 (-30 to 36) in our sample.

government agreements about worker recruitment, programs that resettle refugees, migrants' access to language programs or financial assistance, as well as religious and cultural integration programs, among others. Examples for policies covered by $SANCTIONS_{hot}$ are controls on the movement and migration status of people (like the construction of fences or introduction of fingerprinting), rules on identification documents, procedures and criteria for the detention of foreigners and employment permits.

We also use an integration policies index ($INTEGRATION_{hot}$), constructed in the same way as the $RIGHTS_{hot}$ and $SANCTIONS_{hot}$ indices and covering restrictions on the naturalization of non-native speakers, preferential naturalization for native-born of particular countries and regulations of permanent residency or work permits, among others (DEMIG 2015). Higher values on the index imply more restrictive policies.

Furthermore, we aim to test the effect of the host country's immigration policies. Our indicator is an ordinal measure of the restrictiveness of immigration policies, again based on the DEMIG (2015) database. $IMMIGRATION_{hot}$ captures regulations of border and land controls, as well as legal entry and stay. Again, higher values represent more restrictive policies.

Our final set of political variables varies exclusively at the host-country level. $TERRORFOREIGN_{ht}$ measures the number of terrorist attacks by $NATIVES$ against foreigners in host country h and year t . $RELIGIOUS TENSIONS_{ht}$ is taken from the International Country Risk Guide (PRS Group 2016), ranging between one and six, with higher values representing fewer tensions. It measures "the domination of society and/or governance by a single religious group that seeks to replace civil law by religious law and to exclude other religions from the political and/or social process; the desire of a single religious group to dominate governance; the suppression of religious freedom; the desire of a religious group to express its own identity, separate from the country as a whole" (PRS Group 2016).

Next we turn to the composition of the foreign-born stocks (again relying on IAB data). We separately include the stock of foreign men and women to test gender-related differences. We also separate foreigners by their skills—low, medium and high.

Finally, we turn to characteristics of the origin country. We interact the bilateral stock of foreigners with a binary indicator for countries with predominantly Muslim population, according to the CIA World Factbook.²⁴ We also include a binary indicator that measures the degree of domestic terror in a country of origin. This indicator is one for countries that are in the highest quintile of the distribution of domestic terror over our sample of countries and years.

24 Available at www.cia.gov/library/publications/the-world-factbook/fields/2122.html (last accessed August 11, 2016).

3.2. Identification

A key problem for estimating the effect of the stock of foreigners on the likelihood of transnational terrorism is endogeneity. Dreher et al. (2011) show that terrorism affects migration. What is more, terrorism and migration are both correlated with a large number of variables that cannot all be controlled for in our regressions, despite the inclusion of host–origin fixed effects. To address this endogeneity, we follow recent advances in the migration, development and labour literature (Feyrer 2009, Beine et al. 2011, Artuç et al. 2015, Alesina et al. 2016). Our instrument relies on the interaction between two sets of variables. One makes use of the observation that migration is pushed by natural disasters in countries of origin and pulled by the absence of natural disasters in host countries (Drabo and Mbaye 2015, Berlemann and Steinhardt 2017). Differences in the number of natural disasters hitting the host and origin countries in our sample at any point in time provide variation in migration—and thus the stock of foreigners—over time and between countries. Disasters could of course affect terror through variables other than migration as well. For example, Jetter (2017) shows that disasters crowd out media coverage about terrorist events, in turn making attacks less likely. This is why we interact the number of disasters in host and origin countries with a number of structural variables that make it more or less likely that disaster-induced shocks affect the flow of migrants between any two countries. We assume that emigrants who leave their home country as a consequence of disasters prefer to move to countries with which they share a (past or present) colonial relation, a common language (spoken by at least 9% of the population) and a common border. We also assume they prefer countries that are less distant (measured by the logged great circle distance between their capitals in kilometres) and in which they have pre-existing networks (measured as the log of the stock of foreigners in 1960 of people from the same country).

We interact the structural variables with the vector of the total number of natural disasters in host ($DISASTER_{ht}$) and origin countries in a given year ($DISASTER_{ot}$), assuming that natural disasters in origin countries increase the importance of push factors for migration (Artuç et al. 2015, Docquier et al. 2016), while natural disasters within host countries reduce the weight of pull factors. We use data on natural disasters provided by the Emergency Events Database (EM-DAT) (Guha-Sapir et al. 2016). EM-DAT includes all disasters where at least 10 people died, at least 100 people were affected, a state of emergency was declared, or a call for international assistance was made. Natural disasters cover five subcategories—geophysical, meteorological, hydrological, climatological, biological and extraterrestrial. In each year, there are 2.4 (1.7) disasters in the average host (origin) country in our sample, with a maximum of 34 (37).²⁵

25 EM-DAT collects data from a number of different sources, including UN agencies, non-governmental organizations, research institutes, insurance

Note that we do not assume that countries are hit by these disasters at random. Some countries are more likely to be hit than others, due to their geographical and climatic conditions. Controlled for year fixed effects that capture events that affect the likelihood of disasters across all countries at a particular time and host–origin fixed effects that take account of fixed geographical or climatic conditions, the timing of disasters in any particular year and (origin as well as host) country is, however, plausibly exogenous to dyadic terror events between any pair of countries.²⁶ What is more, we control for the number of disasters in the host and origin countries in the first and second stage regressions, so that the levels of disasters are not part of our instrument.

The intuition behind the interacted instruments is that of a difference-in-differences approach: we investigate a differential effect of dyad-specific pull and push factors on the number of terrorist attacks in a year with fewer or more disasters.²⁷ We assume that any bias resulting from the (potential) endogeneity of the push and pull factors with respect to terror is the same in countries with different numbers of disasters. At the same time, the existence of alternative channels by which disasters affect terror would not threaten the consistency of the estimated interaction term, except in the case that such omitted variables are also correlated with the push and pull factors. While we control for likely determinants of terror potentially affected by disasters, it is impossible to rule out that other such variables exist. That would be the case if terror depended on omitted variables that are correlated with differences in the number of natural disasters and differences in the structural characteristics at the same time, even controlling for dyad-specific and year fixed effects. As one example, assume that newspapers report more frequently about disasters in countries that are less distant. If reporting about terror was consequently crowded out only in these countries and terrorist groups reduce the number of attacks in such countries (in the entire year of the disaster) but not in others,

companies and press agencies. See www.emdat.be/explanatory-notes for methodological details.

26 Climatic conditions might change over the 30 years we consider in our sample. A skeptical reader might expect these changes to be correlated with omitted variables that are in turn correlated with the probability of terror between origin and host countries. When we replace the host–origin fixed effects with host–origin–decade fixed effects our estimate stays significant and increases in size. Arguably, within-decade changes in (slow-moving) climatic conditions are unlikely to affect our results.

27 We follow the previous literature and use the number of natural disasters rather than disaster outcomes such as deaths or destruction (Docquier et al. 2016), since the latter two are more likely to be correlated with terrorist activity in the origin or host country, e.g., blocking relief organizations from distributing emergency relief.

the exclusion restriction would be violated. While we consider this unlikely,²⁸ it cannot be ruled out. For readers who remain skeptical of our identification strategy, we would like to stress from the outset that our key results do not depend on whether we focus on OLS or 2SLS. They can thus conveniently be interpreted as conditional correlations rather than causal estimates if the reader prefers to do so.²⁹

Our first-stage regression is then as follows:³⁰

$$\begin{aligned}
 & \text{FOREIGNERS}_{hot} \\
 &= \frac{0.0927^{***}}{(0.0297)} \text{DISASTER}_{ht} + \text{DISASTER}_{ht} \\
 & \quad \times \left(\frac{0.0072}{(0.0067)} \text{COLONY}_{ho} - \frac{0.0144^{**}}{(0.0045)} \text{LANGUAGE}_{ho} \right. \\
 & \quad - \frac{0.0224}{(0.0174)} \text{BORDER}_{ho} - \frac{0.0066^{**}}{(0.0032)} \text{DISTANCE}_{ho} \\
 & \quad \left. + \frac{0.0050^{***}}{(0.0006)} \text{FOREIGNERS}_{1960_{ho}} \right) - \frac{0.0040}{(0.0371)} \text{DISASTER}_{ot} \\
 & \quad + \text{DISASTER}_{ot} \times \left(- \frac{0.0320^{***}}{(0.0118)} \text{COLONY}_{ho} \right. \\
 & \quad \left. + \frac{0.0116}{(0.0116)} \text{LANGUAGE}_{ho} - \frac{0.0293^{**}}{(0.0147)} \text{BORDER}_{ho} \right)
 \end{aligned}$$

28 To test whether our results are driven by omitted variables that are systematically correlated with the stock of foreign-born populations over time within dyads or across dyads at any specific time, we randomly assigned stocks of migrants in these two dimensions. First, we assign the stock of foreigners of each particular year to a random year for the same dyad. Second, we assign the stock of foreigners of one dyad in each year to a random dyad in the same year. Figure D-1 in online appendix D shows the point coefficients resulting from 5,000 randomizations for each of the two procedures in concert with the p-value testing whether the randomized coefficients are identical to the main result. As can be seen, the coefficients are centred around zero and significantly different from the main results.

29 We report most of these OLS regressions in online appendix D. While the magnitudes of the estimates differ to some extent from the 2SLS results reported in the main text, our key results are unchanged.

30 Some of the coefficients in equation (3) might not match the reader's expectations. However, the regressions control for the levels of the structural variables through the inclusion of fixed effects. When we exclude the dyadic fixed effects, we find that migration is more likely with pre-existing networks, shorter distances and with a common language, as one might expect. We provide the full set of coefficients in column 1 of table D-1 in online appendix D. We show the corresponding second stage of the regression in column 1 of table 3.

$$\begin{aligned}
& + \frac{0.0051}{(0.0041)} DISTANCE_{ho} - \frac{0.0045^{***}}{(0.0012)} FOREIGNERS1960_{ho} \Big) \\
& + \mathbf{X}'_{hot} \boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot}.
\end{aligned} \tag{3}$$

The second-stage regression explaining terror is:

$$\begin{aligned}
TEROR_{hot} = & \beta \widehat{FOREIGNER}_{hot} + \rho DISASTER_{ot} \\
& + \delta DISASTER_{ht} + \mathbf{X}'_{hot} \boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot}.
\end{aligned} \tag{4}$$

Recall that we control for the number of disasters in host and origin countries in the second stage of our regressions, so that they are not used as instruments. The levels of the structural variables do not vary over time and are thus captured by the host–origin fixed effects η_{ho} .

The second stage explaining the various interactions with terror becomes:

$$\begin{aligned}
TEROR_{hot} = & \beta \widehat{FOREIGNER}_{hot} + \delta INT_{ho,t-1} \\
& + \theta (\widehat{FOREIGNER}_{hot} \times INT_{ho,t-1}) + \rho DISASTER_{ot} \\
& + \delta DISASTER_{ht} + \mathbf{X}'_{hot} \boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot}.
\end{aligned} \tag{5}$$

These regressions use an additional set of instruments: We instrument both $FOREIGNERS_{hot}$ and $FOREIGNERS_{hot} * INT_{ho,t-1}$ with the instruments of equation (3) as well as with the interaction of these instruments with $INT_{ho,t-1}$.³¹ Note that we have no suitable instruments for the levels of the interacted variables themselves. This implies that we do not test whether these variables directly increase or reduce the risk of terror. Under mild assumptions, we can nevertheless estimate how these variables change the effect of the stock of foreigners on terror. As in any interaction model, the interpretation of our estimates is again similar to a difference-in-differences model. The interaction investigates the effect of these variables on terror for different stocks of foreigners. As long as the effect of the (instrumented) stock of foreigners on terror is exogenous, and the degree of bias for any of the variables does not depend on the stock of foreigners, the estimate for the coefficient of the interaction term is consistent.³² The first assumption—the exogeneity of the stock of foreigners—depends on the validity of our instruments (which we have discussed above). The second assumption is the so-called parallel trends assumption, implying that any bias resulting from the (potential) endogeneity of the variables entering the interaction with the

31 This follows Wooldridge (2010, p. 143 onwards).

32 Bun and Harrison (2019) and Nizalova and Murtazashvili (2016) provide details on the identifying assumptions and formal proofs. Also see appendix S.4 in Dreher et al. (2019).

stock of foreigners is the same for any level of this stock. We investigate this assumption in figure D-2 in online appendix D.³³

4. Results

4.1 Conditional correlations, native- and foreign-born populations

Column 1 of table 2 shows the results of the baseline regression, estimated with OLS (equation (1)). As can be seen, the probability of a transnational terrorist attack decreases with the GDP of the origin country and increases with the size of its population, at the 1% level of significance. Both results are in line with the previous literature.³⁴ Just like Gassebner and Luechinger (2011), we find no significant effect of host country GDP and population.

The results also reflect the positive scale effect already visible in figure 2. At the 1% level of significance, the number of terrorist attacks increases with the number of migrants living in a country. The coefficient implies that an increase in the number of migrants by 1% comes with an increase in the probability of terrorist activity from that group of 0.001 percentage points, on average. In order to put the magnitude of this scale effect into perspective, we proceed by comparing it to the effect of the size of the domestic population on domestic terror.

We are interested in whether the stock of *NATIVES* affects the probability of terror against either other *NATIVES* or *FOREIGNERS* to a different extent compared with how the stock of *FOREIGNERS* affects the probability of transnational terror. Rather than estimating separate models, we nest the regressions using interaction terms for the host country variables (native-born stock of people and host GDP) so that we can directly compare their magnitudes.

We include dyads of the host country with itself and replace the number of foreigners with the log stock of native-born there (i.e., when $h = o$). We do not include origin-country GDP in this regression, as it would be undefined for the domestic terror dyads.

The upper panel in column 2 of table 2 reports the average effect of the number of native-born (on domestic terror) and foreigners (on transnational terror). The lower panel shows the differential effect of *NATIVES* compared with the pooled estimate. According to the results, there is no statistically significant difference among the two sets of regressions. The average scale effect

33 The figure depicts the trends of our potentially endogenous confounding variables over the quartiles of the migrant stocks that we have predicted based on the first stage of our regression (in column 1 of table D-1). The figure shows no obvious differences in these trends across the respective sub-samples. The exception is our indicator for conflict, where we are thus less confident that the coefficient of the interaction term is estimated consistently.

34 See, for example, Li and Schaub (2004) and Li (2005) on how GDP affects terror and Burgoon (2006) on population.

TABLE 2
 Terror and migration comparing native-born and foreigners, 1980–2010, OLS

	Terror indicator (1)	Terror indicator (2)	Terror count (3)	Severe terror indicator (4)	Severe terror count (5)	Terror fatalities count (6)
Log GDP host	0.0032 (0.0040)	0.0062** (0.0029)	0.0926** (0.0464)	0.0028*** (0.0010)	0.0034** (0.0017)	-0.0393 (0.0623)
Log stock foreigners	0.0013*** (0.0003)	0.0036*** (0.0008)	0.0120*** (0.0033)	0.0014*** (0.0005)	0.0024*** (0.0009)	0.0275* (0.0162)
Log GDP origin	-0.0021*** (0.0007)					
Log population host	0.0125 (0.0093)					
Log population origin	0.0077*** (0.0026)					
Citizen interaction						
Log GDP host		-0.4291** (0.1863)	-10.9431** (5.0809)	0.0103 (0.0168)	0.0648 (0.0463)	-10.0921 (10.8919)
Log stock		0.1823 (0.9179)	19.1166 (19.5007)	0.0889 (0.0983)	0.1927 (0.3089)	142.6256 (138.8912)
R-squared	0.0035	0.0216	0.0401	0.0019	0.0023	0.0025
Fixed effects	HO, Y	HO, Y	HO, Y	HO, Y	HO, Y	HO, Y
Observations	102,760	123,380	123,380	123,380	123,380	123,380

NOTES: The dependent variable in columns (1) and (2) is binary and indicates that at least one transnational attack occurs in a year. Column (3) uses the number of transnational attacks per year. In column (4) the binary indicator is one if a transnational terror attack occurs in a given year which results in at least one person wounded or killed. Column (5) uses the number of those attacks per year. Column (6) counts the number of fatalities. In the case of native-born, domestic attacks are also included. Robust standard errors clustered on host–origin dyad in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host–origin fixed effects and Y are year fixed effects.

of the total population on the probability of terror is positive and significant at the 1% level. However, while the point coefficient estimating the difference for terror originating from the native-born population compared with the total population is large, this difference is not significant at conventional levels.

Column 3 of table 2 replaces the binary dependent variable with the number of attacks in a country dyad and year. Again, the difference between the baseline effect of average terror and terror by *NATIVES* is not statistically significant. When we calculate the elasticity at the sample mean of transnational terror incidents (0.028), we find that a 1% increase in the stock of foreigners increases the number of terrorist attacks by 0.43 percent. These numbers are not easily comparable to the scale effects for the domestic population shown in the previous literature. Studies with a monadic setting typically find a positive effect of population size on terror, but coefficients vary greatly in size and significance (see Gassebner and Luechinger 2011). They are, however, not directly comparable to our setting as they combine scale effects for perpetrators and victims. Most dyadic studies focus on GDP and GDP per capita and thus only implicitly control for population. The exception is Neumayer and Plümper (2009). According to their results, a 1% change in the perpetrator population leads to an increase in the expected number of attacks of 0.45 percent. In their unilateral analysis, Savun and Phillips (2009) obtain an elasticity of one for the expected number of domestic attacks with respect to the domestic population.

One might argue that even if we find no difference in the quantity of terror attacks committed by native-born and foreigners, the number of victims resulting from foreign attacks might be higher. We test this in a sample containing only those terrorist attacks in which at least one person was either wounded or killed. The results shown in columns 4 (for the occurrence of at least one terrorist event) and 5 (for the number of attacks) show again no statistical difference between foreigners and native-born. Based on our conditional correlations we thus conclude that the scale effect of foreign populations—while positive and significant—is comparable to those associated with domestic populations. We also replicated these regressions using the number of terror fatalities as the dependent variable (column 6). The migrant stock is only marginally significant in this regression. Given that fatalities involve a larger degree of randomness than the occurrence of an attack, this is unsurprising.

4.2. Instrumental variables results

Table 3 shows the main results of our instrumental variables regressions. As can be seen from column 1, the average effect the stock of foreigners has on transnational terror is substantially larger compared with the OLS regression above (in column 1 of table 3) and is significant at the 1% level.³⁵ The

35 The first-stage results are shown in equation (3) as well as in table D-1 in online appendix D. Kleibergen–Paap F-statistics indicate the power of our instruments,

TABLE 3
 Terror and migration, baseline, 1980–2010, 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)
Log GDP host	-0.0644*** (0.0167)	-0.0689*** (0.0200)	-0.0683*** (0.0201)	-0.0692*** (0.0201)	-0.0608*** (0.0174)	
Log GDP origin	0.0073** (0.0030)	0.0066** (0.0033)	0.0064* (0.0033)	0.0069** (0.0034)	0.0055** (0.0028)	
Log population host	0.0986*** (0.0263)	0.1026*** (0.0298)	0.1016*** (0.0300)	0.0972** (0.0289)	0.0942*** (0.0265)	
Log population origin	-0.0247*** (0.0084)	-0.0280*** (0.0098)	-0.0282*** (0.0099)	-0.0328*** (0.0108)	-0.0279*** (0.0093)	
Natural disaster host	-0.0002 (0.0002)	-0.0002 (0.0002)	-0.0002 (0.0002)	-0.0003 (0.0002)	0.0000 (0.0002)	
Natural disaster origin	-0.0013*** (0.0003)	-0.0011*** (0.0003)	-0.0011*** (0.0003)	-0.0012*** (0.0003)	-0.0010*** (0.0003)	
Log stock foreigners	0.0443*** (0.0091)	0.0430*** (0.0100)	0.0439*** (0.0102)	0.0438*** (0.0102)	0.0368*** (0.0088)	0.0360*** (0.0129)
Log net ODA			-0.0006*** (0.0002)			
UNGA alignment				-0.0420*** (0.0124)		
Log imports origin from host					-0.0018*** (0.0005)	
Log imports host from origin					-0.0008*** (0.0002)	
R-squared	0.00737	0.0072	0.0072	0.0070	0.0070	0.1045
Fixed effects	HO, Y	HO, Y	HO, Y	HO, Y	HO, Y	HO, HY, OY
Kleibergen–Paap F-stat. IV	15.91	13.19	12.77	13.22	14.18	6.429
Observations	102,760	91,621	91,621	91,621	91,621	91,621

NOTES: The dependent variable is binary and indicates that at least one transnational attack occurs in a year. Robust standard errors clustered on host–origin dyad in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host–origin fixed effects, Y are year fixed effects, HY are host–year fixed effects and OY are origin–year fixed effects.

coefficient implies that a 1% increase in the stock of foreigners increases the probability of a terrorist attack by 0.044 percentage points, on average.³⁶ The estimated Local Average Treatment Effect (LATE) captures the effect of those migrants that have been induced to migrate by natural disasters in host and origin countries. While such disasters are unlikely to have a direct effect on terrorists' desire to move to a particular country, the resulting flows of migrants facilitate the flow of terrorists as well. The larger the numbers of migrants from a particular country of origin to a specific host country, the easier it is for terrorists to hide among the crowd. What is more, the resulting larger networks of foreigners residing in the host countries make it easier for terrorists to find shelter there or receive other support—financial and logistical. The sheer presence of a larger number of foreigners from a particular country makes it easier for terrorists from the same country to remain in cover. We thus assume that the push and pull factors covered in our model affect the move of (present and future) terrorists in concert with other migrants. To the extent that disasters affect the flow of terrorists to a lower degree than other migrants, we would underestimate the total effect of migration on terror.

While the literature seems to accept that our instrument is exogenous to labour market outcomes (Feyrer 2009, Beine et al. 2011, Artuç et al. 2015, Docquier et al. 2016, Alesina et al. 2016), one might question its excludability in our setting, as discussed above. The further columns of table 3 thus test the robustness of our results to the inclusion of those dyad- and year-specific variables that are most likely to threaten our identifying assumptions. To rule out that differences in results are due to differences in the number of observations rather than the effect of additional variables, we hold the sample constant across these regressions. Column 2 shows results for this reduced sample without additional control variables. The point coefficients are almost identical.

In column 3, we control for net official development assistance given by a host country to a country of origin. Foreign aid is given to reduce terror, and terror affects aid (Fleck and Kilby 2010, Dreher and Fuchs 2011), while aid in turn affects migration (Dreher et al. 2019). For similar reasons, we control for—in column 4—voting coincidence between host and origin in the United Nations General Assembly (Dreher and Gassebner 2008) and—in column 5—for bilateral imports and exports (Egger and Gassebner 2014). While these variables enter with significant coefficients, the effect of the stock of foreigners hardly changes.

Column 6 includes host-year and origin-year fixed effects instead of the fixed effects for years. In tandem with the dyad fixed effects, we thereby

ranging from 12.8 to 15.9. The correlation between our predicted stocks of migrants and actual migrant stocks is about 0.3, illustrating that a substantial share of migrant stocks is explained by our instruments.

36 This result is not driven by any particular host or origin country (see figures D-3 and D-4 in online appendix D). The coefficient changes most notably when we exclude Turkey as a country of origin. The point estimate is not, however, statistically different from those of the main results.

TABLE 4

Terror and migration, alternative definitions, 1980–2010, 2SLS

	Terror count (1)	Severe terror indicator (2)	Severe terror count (3)	Terror fatalities count (4)
Log GDP host	−0.1488*** (0.0520)	−0.0226** (0.0094)	−0.0341** (0.0144)	0.0197 (0.0439)
Log GDP origin	0.0180** (0.0085)	0.0028* (0.0015)	0.0040* (0.0022)	−0.0046 (0.0081)
Log population host	0.2079*** (0.0676)	0.0372*** (0.0134)	0.0547*** (0.0210)	−0.1689 (0.1843)
Log population origin	−0.0569** (0.0231)	−0.0107** (0.0048)	−0.0158** (0.0073)	−0.0331 (0.0276)
Natural disaster host	−0.0009 (0.0007)	−0.0001 (0.0001)	−0.0002 (0.0001)	0.0004 (0.0004)
Natural disaster origin	−0.0027*** (0.0008)	−0.0005** (0.0002)	−0.0008*** (0.0003)	−0.0020 (0.0021)
Log stock foreigners	0.1009*** (0.0313)	0.0172*** (0.0059)	0.0261*** (0.0091)	0.0424 (0.0427)
R-squared	0.00419	0.00251	0.00190	0.00001
Kleibergen–Paap F-stat. IV	15.91	15.91	15.91	15.91
Fixed effects	HO,Y	HO,Y	HO,Y	HO,Y
Observations	102,760	102,760	102,760	102,760

NOTES: The dependent variable in column (1) counts the number of transnational attacks per year. In column (2), we use a binary indicator that is one if a transnational terror attack occurs in a given year which results in at least one wounded or killed victim. Column (3) uses the number of those attacks per year. Column (4) counts the number of fatalities. Robust standard errors clustered on host–origin dyad in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host–origin fixed effects and Y are year fixed effects.

control for all factors that do not vary between dyads over time and factors that are constant between all dyads with a particular host or origin in a given year.³⁷ Again, the result is similar. We are thus confident that our identifying assumptions are not threatened by omitted variables that do not vary between dyads in a given year.³⁸

37 We show the first stage regression of the high dimensional fixed effects specification in table D-1 (column 2) of online appendix D.

38 Again, see figure D-1 in online appendix D for the (lack of) systematic importance of unobserved dyad-specific variation over time or time-specific variation across dyads. We also gauged the importance of omitted variable bias following the approach of Altonji et al. (2005) and Bellows and Miguel (2009). Specifically, we compared the relative impact that unobserved variables would need to have on our coefficients of interest relative to observed ones in order to fully account for our results. For instance, the estimated effect of migration on terror is 0.0443 according to column 1 of table 3. The coefficient decreases to 0.0377 when we omit all control variables. This suggests that omitted variable bias would need to be in the opposite direction and almost 54% larger (i.e., $0.0443/(0.0377 + 0.0443)$) than the impact of the observed variables to explain

Table 4 turns to the alternative definitions of our dependent variable, in line with table 2. Across the regressions, the scale effect of foreigners on terror remains significant at the 1% level. The exception is column 4, where we focus on fatalities arising from transnational terror, with an insignificant coefficient. Fatalities in OECD countries are too random to be predicted with any accuracy in our dyadic setting.

We find that the stock of foreigners increases the occurrence of terror and severe terror as well as the number of terror events and severe terror events. Ideally, we would like to compare these scale effects to those of the domestic population in our instrumental variable setting as well. However, our instrument is not suited to predict changes in the stock of native-born, and we have no additional instrument for the size of the domestic population that would allow this comparison.

4.3. Conditional effects: Conflict, migration policies and domestic tensions

Table 5 tests if the effect of the stock of foreigners depends on whether these foreigners migrated from countries that are engaged in military conflict with their host, on integration and immigration policies and on the degree of terror against foreigners in the host countries and religious tensions there. We include the interaction of the respective variable with migration as an additional regressor (which we instrument with the interaction of the respective variable and our interacted set of instruments as outlined above). The table shows the coefficients θ of the interaction terms in equation (2) along with the coefficients of the variables we interact the stock of foreigners with (δ).

Column 1 tests the importance of military conflict between the host and origin countries for how the stock of foreigners affects terror. To this end, we introduce $CONFLICT_{hot}$ and its interaction with the number of foreigners. The coefficient is negative but not precisely estimated. It thus seems that the effect of foreigners on terror in the OECD is independent from military conflict between the origin and host countries, on average.

Columns 2 to 5 introduce the variables measuring the policies and outcomes of immigration and integration policies and their interactions with the number of foreigners. We find that laws putting pressure on migrants to integrate (column 2) and stronger restrictions of foreigners' rights (column 3) increase the probability of terror associated with a rising number of foreigners in a country. Stricter sanctions on migrants seem to reduce the threat of terror associated with the number of foreigners (column 4), while we do not find a significant interaction with restrictions on immigration (column 5).³⁹

away the entire effect of migration. Also note that our results are robust—though smaller in magnitude—throughout the different specifications when we use OLS instead of 2SLS (see online appendix E) or use log changes in the stock of foreign-born populations instead of levels (see table D-2 of the online appendix).

39 Results are similar when we estimate the regressions with OLS. The exceptions are the interactions with religious tensions (which turns insignificant) and

TABLE 5
Terror and migration, interactions, 1980–2010

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log GDP host	-0.0643*** (0.0167)	-0.0718*** (0.0173)	-0.0741*** (0.0178)	-0.0616*** (0.0166)	-0.0621*** (0.0164)	-0.0535*** (0.0150)	-0.0570** (0.0264)
Log GDP origin	0.0073** (0.0030)	0.0072*** (0.0028)	0.0074*** (0.0028)	0.0064** (0.0028)	0.0069** (0.0029)	0.0060** (0.0026)	0.0025 (0.0024)
Log population host	0.0985*** (0.0263)	0.1293*** (0.0298)	0.1276*** (0.0299)	0.0976*** (0.0253)	0.1028*** (0.0259)	0.0936*** (0.0247)	0.1072*** (0.0404)
Log population origin	-0.0246*** (0.0084)	-0.0323*** (0.0088)	-0.0322*** (0.0090)	-0.0233*** (0.0078)	-0.0237*** (0.0081)	-0.0215*** (0.0076)	-0.0092 (0.0075)
Natural disaster host	-0.0002 (0.0002)	-0.0002 (0.0002)	-0.0002 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0002 (0.0002)	-0.0000 (0.0002)
Natural disaster origin	-0.0013*** (0.0003)	-0.0010*** (0.0003)	-0.0010*** (0.0003)	-0.0012*** (0.0003)	-0.0013*** (0.0003)	-0.0012*** (0.0003)	-0.0005* (0.0002)
Log stock foreigners	0.0442*** (0.0091)	0.0425*** (0.0086)	0.0435*** (0.0088)	0.0395*** (0.0093)	0.0421*** (0.0092)	0.0375*** (0.0082)	0.0328** (0.0136)
Additional variable	Conflict	Integration	Migrant rights	Migrant sanctions	Immigration	Terror vs. foreigners	Religious tensions
Variable coefficient	-0.0049 (0.0167)	-0.0030 (0.0005)	-0.0038*** (0.0007)	0.0001 (0.0005)	0.0001 (0.0004)	-0.0013*** (0.0003)	0.0020 (0.0016)
Interaction coefficient	-0.0015 (0.0027)	0.0005 (0.0001)	0.0006*** (0.0001)	-0.0001* (0.0001)	0.0001 (0.0000)	0.0002*** (0.0000)	-0.0006* (0.0003)
R-squared	0.00737	0.00612	0.00637	0.00712	0.00723	0.00751	0.00510
Kleibergen–Paap F-stat. IV	14.51	16.23	15.27	13.18	13.70	15.96	11.81
Fixed effects	HO, Y	HO, Y	HO, Y	HO, Y	HO, Y	HO, Y	HO, Y
Observations	102,760	102,760	102,760	102,760	102,760	102,760	89,020

NOTES: The dependent variable is binary and indicates that at least one transnational attack occurs in a year. Robust standard errors clustered on host–origin dyad in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host–origin fixed effects, Y are year fixed effects.

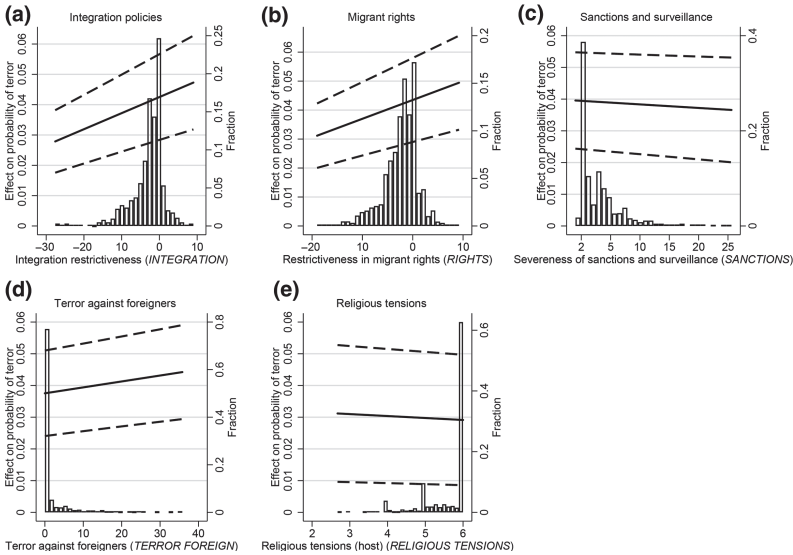


FIGURE 3 Marginal effects corresponding to table 5
 NOTE: The figures show the marginal effects of the significant interactions from table 5 with bars indicating the distribution of the underlying data (right scale).

Columns 6 and 7 focus on terror against foreigners and (the absence of) religious tensions in the host country. As can be seen, terror against foreigners (column 6) and religious tensions (column 7) increase the scale effect of migrants on terror, at the 1% and 10% levels of significance, respectively. We further elaborate on these results in online appendix F. Table F-1 shows that an increase in the stock of migrants from a particular country also increases terror *against* foreigners from that country; table F-2 reports that such violence against foreigners from a specific country increases the effect of the stock of foreigners on terror conducted by this group of foreigners. According to the coefficients shown there, terror against foreigners explains a large share of the effect that a specific group of foreigners have on the number of terror attacks in their host country. In light of these results, it seems that tensions and violence against foreigners are important explanations for terror originating from foreigners.

Figure 3 shows the conditional marginal effects of the significant interactions (Brambor et al. 2006). Panel (a) of figure 3 shows that immigration policies substantially affect the risk of terror arising from any given stock of migrants in a country. At the mean value of the integration index, a 1% increase of the stock of foreigners increases the probability of a terrorist event by 0.0409 percentage points on average. The corresponding increase is 0.0473

immigration restrictions (which is significant and positive). We also tested an interaction with GDP per capita growth, but found no statistically significant effect of the interaction.

percentage points when integration restrictions are maximal (which is a 14.6% increase). Results are similar when we focus on migrant rights instead (panel (b)): at the mean value of migrant rights, a 1% increase in the stock of foreigners increases the likelihood of terror by 0.0419 percentage points on average and at the maximum value by 0.0495 percentage points (a 16.7% increase).

Panel (c) turns to the effect of migrant surveillance and sanctions. While the effect is significant over the entire range of the distribution, the change of the conditional effect is small in quantitative terms: at the mean value of the index, a 1% increase of the stock of foreigners leads to an increase of 0.0391 in the likelihood of terror, compared with an 0.0366 increase at the maximum (corresponding to a 6.6% reduction). Although the interaction points into the opposite direction, the marginal effect is substantially smaller compared with the integration and rights interactions discussed above.

Overall, we conclude that migration policies play an important role in the fight against terror. The optimal mix, however, is crucial. Countries that put too much pressure on immigrants to integrate and that restrict their rights are likely to achieve the opposite of what they aim for, at least in the short run. Immigrants already living in the country might turn against their host and get increasingly violent.⁴⁰

The effect of terror against foreigners is also substantial (panel (d)). At the mean value of terror against foreigners, a 1% increase in the stock of foreigners increases, on average, the probability of a terrorist attack committed by foreigners by 0.0379 percentage points. The corresponding increase is 0.0442 percentage points at the maximum value of terror against foreigners (15.5% higher compared with the mean). To the contrary, while the effect of (the absence of) religious tensions is statistically significant (panel (e)), the difference of a 1% increase in the stock of foreigners at the mean of religious tensions is hardly distinguishable from that at the maximum (0.0294 vs. 0.0291).

4.4. Composition of migrant populations

We proceed with testing whether the composition of migrants matters. Column 1 of table 6 investigates male and female migrants separately. Column 2 distinguishes between migrants with low, medium and high skills.⁴¹ As an

40 As an illustration, consider France. According to the DEMIG (2015) data, France introduced 18 additional restrictions on immigration over the 1991–1994 period. They included prohibiting foreign graduates from gaining employment in France and suppressing work permits for asylum seekers. In 1994, France restricted the access and right of residence for Algerians (DEMIG 2015). France suffered a spell of terrorism in the following year, with at least one attack per year committed by an Algerian citizen over the 1995–1999 period.

41 The IAB database defines the skill levels as follows: (i) low skilled individuals have received lower secondary, primary or no schooling, (ii) medium skilled migrants have obtained a high school diploma or equivalent certificate and (iii) high skilled immigrants have tertiary education (Brücker et al. 2013, p. 4).

TABLE 6
Gender and skill level, 1980–2010, 2SLS

	(1)	(2)
Log GDP host	-0.0234*** (0.0087)	-0.0214** (0.0106)
Log GDP origin	0.0025 (0.0016)	-0.0038 (0.0025)
Log population host	0.0611*** (0.0201)	0.0812*** (0.0308)
Log population origin	-0.0072 (0.0046)	-0.0140** (0.0064)
Natural disaster host	-0.0003 (0.0002)	0.0003 (0.0003)
Natural disaster origin	-0.0011*** (0.0003)	-0.0006*** (0.0002)
Log stock (male)	0.0160*** (0.0054)	
Log stock (female)	0.0093 (0.0071)	
Log stock (low skilled)		0.0459*** (0.0122)
Log stock (medium skilled)		0.0161 (0.0136)
Log stock (high skilled)		-0.0506** (0.0237)
R-squared	0.0079	0.0033
Kleibergen–Paap F-stat. IV	30.84	9.968
Fixed effects	HO,Y	HO,Y
Observations	102,760	102,760

NOTES: The dependent variable is binary and indicates that at least one transnational attack occurs in a year. Robust standard errors clustered on host-origin dyad in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host-origin fixed effects, Y are year fixed effects.

additional set of instruments for the stock of male and female migrants, we add the interaction of our instruments with the share of male migrants from a country of origin to a specific host country over the entire sample period. For the stock of low-, medium- and high-skilled migrants, our additional instruments are interactions of the original instruments with the shares of low- and medium-skilled workers among each dyad over the sample period. As can be seen in table 6, our instruments are relevant.⁴²

The results of column 1 show that the risk of terror increases with the number of male immigrants, at the 1% level of significance, but not with the number of female immigrants. The coefficients of the two groups are, however, not statistically different from each other (p-value: 0.54). Column 2 shows that the risk of terror increases with low skilled immigrants, but decreases with highly skilled immigrants, the difference between the two being significant at

⁴² Figure D-5 in online appendix D shows that our predictions for the different groups match the actual values well.

the 1% level. While the previous literature has often argued that terrorists are well educated compared with their peers (Krueger and Malečková 2003), the same does not seem to hold for highly skilled immigrant populations in general. This is in line with the game theoretical model of Bandyopadhyay and Sandler (2014), showing that increases in skilled labour quotas generally reduce terrorist attacks in the host country.

4.5. Investigating possible channels

We investigate two main channels through which migration can be expected to affect terror. Migration is most frequently attributed to terror because it supposedly increases the inflow of religious extremists or directly allows terrorists to enter the country (rather than affecting the general risk of terror arising from any foreigner living in the country).

As we have no data on religious extremists, we instead focus on migrants from Muslim-majority countries which are often perceived to be linked with Islamic terror (Gould and Klor 2016). We therefore test whether migrants from countries with a Muslim-majority population affect the risk of terror differently from the average non-Muslim majority country in our sample. In order to allow this comparison between countries, rather than within dyad pairs over time exclusively, we replace the dyad fixed effects with dummies for individual host countries. Rather than pooling all Muslim-majority countries, we include dummies for each individual country in this group and interact them with our migration variable. The resulting coefficients can be interpreted as the difference in the average partial effect of migrants from Muslim-majority countries compared with all other (i.e., non-Muslim) countries. As before, we instrument the stock of foreigners with the interactions of natural disasters and the pull and push factors introduced above. We also control for the structural variables in the first and second stage since they are no longer absorbed by dyadic fixed effects in this setting.

Figure 4 plots average partial effects for each Muslim-majority country and the effect of the reference group (i.e., the average non-Muslim country), along with a 95% confidence interval (shown as vertical line on the x-axis). Overall, foreigners from Muslim countries do not differ in how they affect terror in their host country from the average non-Muslim country (“Reference Group”).⁴³ The two exceptions are Algeria and Iran, at the 10% level of significance. Compared to the (insignificant) average effect of foreigners from non-Muslim countries, the marginal effect for Algeria implies that a 1% increase in the stock of Algerian migrants increases the likelihood of terror on average by 2.1 percentage points in the average OECD country. The corresponding effect for Iranian migrants is 1.5 percentage points. The effect of Algerian migrants can mainly be attributed to attacks by Algerian fundamentalists who participated

43 Jetter and Stadelmann (2019) show that the probability that Muslims become terrorists is smaller compared with non-Muslim individuals once population size is accounted for.

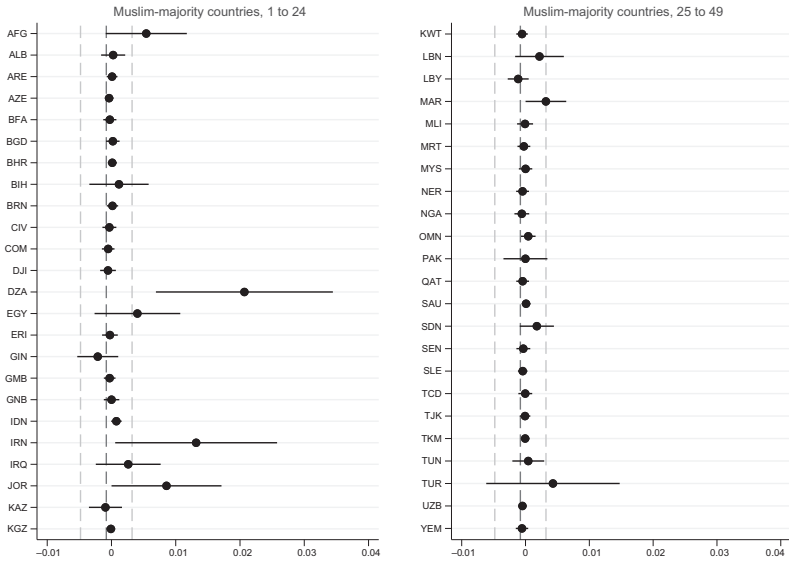


FIGURE 4 Marginal effects of Muslim-majority countries
 NOTES: The vertical dark dashed line is the average partial effect of the estimate for migration from the average non-Muslim majority country; the light dashed lines show the 95% confidence interval. The figure shows the additional effect for each Muslim-majority country, in tandem with the 95% confidence interval.

in 12 attacks in France in the late 1980s to mid-1990s. The effect of Iranians is driven by 18 attacks against each France and Germany in the 1980s and early 1990s by Iranian nationals.⁴⁴ Overall, there is little evidence that migration affects terror because it increases the inflow of religious extremists.

We proceed with testing whether migration from origin countries with prevailing terror networks can explain the effect of overall migration on terror. To the extent that terrorism spreads from countries with such networks, migration might be one vehicle of diffusion (Bove and Böhmelt 2016). As above, we therefore test whether migrants from “terror-rich” countries show different effects of terror compared with migrants from the average “non-terror rich” country.

Our first proxy for the existence of terror networks is a binary indicator variable $TERROR - RICH_o$ for each country that is located within the top quintile of the overall terrorist incident distribution of the GTD

44 There is no dominant terror organization behind these attacks in Germany, while one third of the French attacks were conducted by Islamic Jihad organizations. In our sample, Algerian terrorists conducted 34 terror attacks in total, while citizens of Iran conducted a total of 80 attacks.

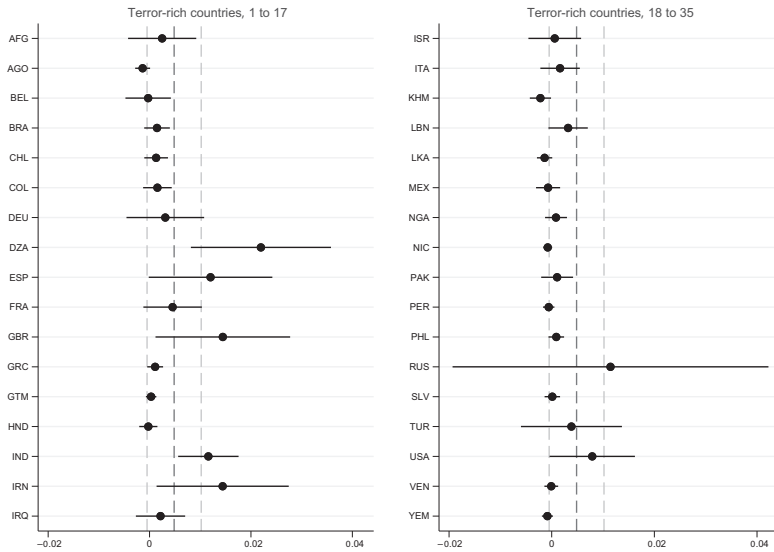


FIGURE 5 Marginal effects of *terror-rich* countries

NOTES: The vertical dark dashed line is the average partial effect of the estimate for the average non-“terror-rich” country; the light dashed lines show the 95% confidence interval. The figure shows the additional effect for each “terror-rich” country, in tandem with the 95% confidence interval.

dataset.⁴⁵ Again, we interact these dummies with our migration variable. Figure 5 shows that five countries have average partial effects that are higher than the reference group, at least at the 10% level of significance. Compared with migrants from the average “non-terror rich” country, migrants from Algeria, Iran, India, Spain and Turkey are all more likely to increase the likelihood of a terrorist attack, while migrants from Angola and Cambodia are less likely than the reference group to affect terror.⁴⁶ Overall, there is no sweeping evidence indicating that the exclusion of immigrants based on the degree of terror in their country of origin could reduce terror substantially.

45 We focus on GTD rather than ITERATE because we are interested in overall terror at the origin-country level rather than in exclusively transnational terror exposure or the terror against specific groups.

46 Some background for Algeria and Iran was given above. In most of the 15 attacks by Indians, the victims were Indian nationals. Sikh extremists conducted the majority of these attacks, with several attacks pertaining to the Kashmir conflict, split equally between the United States, United Kingdom and Canada. Towards the end of our sample, three attacks in the United Kingdom were directed against UK citizens by Muslim extremists. Spanish nationals were involved in 17 attacks in France, 10 attacks in Italy and 43 attacks in our sample (34 were the responsibility of ETA). A total of 145 attacks were conducted by Turkish nationals, 39 of which occurred in France and 20 in Germany. More than half of the attacks are related to the Turkish–Armenian conflict.

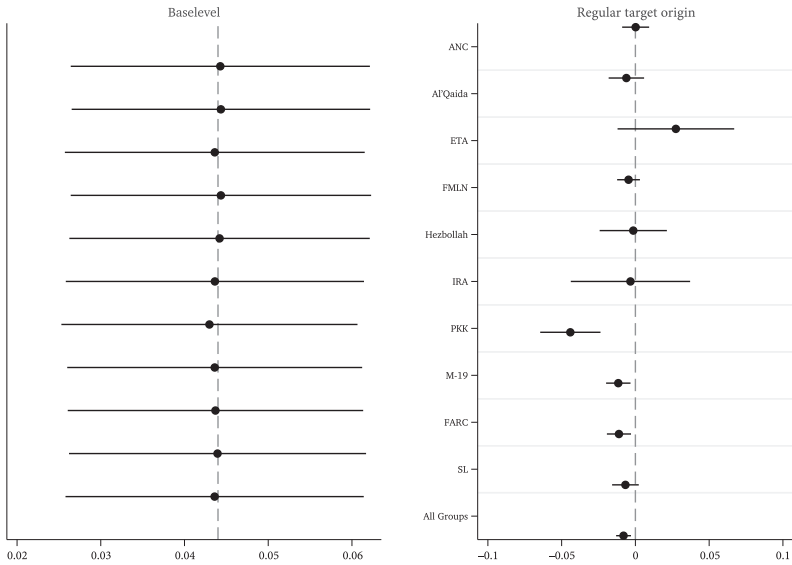


FIGURE 6 Marginal effect of migrants from countries with terrorist networks
 NOTES: The figure shows the average partial effects of the baseline estimate for how the stock of foreigners affects terror without the countries that the respective group is most active in (left) and the interaction coefficient for the countries with the most active terrorist networks (right) for each of the 10 most active transnational terror groups (with the 95% confidence interval). The vertical dashed line shows the baseline estimate in the left panel and marks a zero effect in the right panel.

Immigration from terror-rich countries does thus not seem to be responsible for the overall effect of migration on terror.

Our second proxy for the existence of terrorist networks identifies the 10 most active terrorist groups, in terms of incidents perpetrated globally, and that operate in at least five countries (using GTD data).⁴⁷ We then identify the three countries in which each group commits most of their attacks over our sample countries and period. We list these groups and countries in table D-3 of online appendix D, in descending order of group activity.

Figure 6 shows the average partial effects of the baseline estimate for how the stock of foreigners affects terror without the countries that the respective group is most active in (left) and the interaction coefficient for the countries with the most active terrorist networks (right) for each of the 10 most active transnational terror groups. The figure shows that the effect of migration on terror is not statistically different for most of these countries. The exceptions are countries where the Kurdistan Workers’ Party (PKK) is most active. It turns out that the effect of migrants from these countries—Germany, Turkey and Iraq—is smaller than the effect of migrants from all other countries. Again,

47 While the GTD provides information on the group that commits an attack it does not provide the nationality of the perpetrators.

there is no evidence that migration from countries with large terror networks accounts for the overall effect of migration on terror.⁴⁸

On balance, our analysis does not provide support for widely held beliefs that terror is imported from Muslim-majority countries or countries with large terrorist networks. It seems that the effect that foreign populations have on terror is largely independent of their country of origin but rather is related to the sheer size of the overall (foreign) population (the scale effect).

5. Tests for robustness

In summary, we find a positive scale effect of larger foreign populations on the probability of transnational terrorism within host countries. We find this scale effect to be more severe when migrants are situated in host countries where terror against foreigners is prevalent and religious tensions abound, when migrant rights are restricted and integration laws get tougher. The risk of terror is lower when sanctions against migrants become more frequent. We find no significant difference between male and female immigrants on the risk of terror. Highly skilled immigrants reduce the risk of terror, while low skilled immigration increases it.

We test the robustness of these results in a number of dimensions. First, we use all explanatory variables—rather than just the interaction variables—as (lagged) five-year moving averages to allow for longer lags between changes in policies and outcomes and the actions of terrorists. Second, we use yearly values for our interaction variables, rather than five-year moving averages. Third, we test whether and to what extent the linear interpolation of the migration data affects our results. Instead of interpolating, we use averages over five years (but no moving average). Fourth, we test whether and to what extent our results are driven by dyads in which the stock of the foreign-born population changes substantially, for example due to refugee crises and the resulting surge in immigrants. Specifically, we exclude the dyads that experience the biggest 5% of changes in migration within our sample. Fifth, we employ additional instrumental variables to test if our results hold for a broader LATE. Following Beine and Parsons (2015), we add to our set of instruments the interaction of the yearly deviations in temperatures and

48 We also tested differences for every country in each group. Again, we do not find systematic differences overall, but once more obtain a differential effect for Algeria (figure D-6 in online appendix D). We also test if migration from countries in which terror groups have a strong presence (commit a lot of attacks) has a stronger effect on terror in years in which those groups are more active compared with migration from countries where the groups are less active, using a triple interaction. Again, we do not find any evidence that migrants from countries with larger terrorist activities affect terror more strongly than migrants from the average country without such networks (table D-4 in online appendix D).

precipitation from their decade averages.⁴⁹ A drawback of this approach is a substantial loss in the number of observations.

Sixth, we replace the year fixed effects with fixed effects for origin-year and host-year in the first and second stages. Finally, we test whether our results for the interacted variables are driven by our focus on all terror events rather than focusing on severe events only. Hence, we (again) replace all terror events with terror events during which at least one victim was wounded or killed.

We show the results from these tests in table D-5 (for the main regressions) and table D-6 (for the separate regressions according to gender and skills) of online appendix D. Most of our results turn out to be robust to all modifications. The effect of a 1% increase in the stock of migrants on the probability of transnational terrorist attacks ranges between 0.023 percentage points (when we include the additional fixed effects) and 0.043 percentage points (when we use moving averages throughout). This is similar to the main estimate of 0.043 percentage points from column 1 of table 3, which we reproduce in online appendix table D-5 for comparison (“no moving average”).

With respect to the interaction terms, terror from native-born against migrants in the respective host country robustly increases the effect of migrants on transnational terror, while the religious tensions interaction holds in only four of the robustness tests. Regarding policies, it turns out that while the harmful effect of strict integration policies and restrictions of migrant rights prevails in all regressions, the beneficial effect of strict sanctions turns insignificant in five out of the seven additional regressions. There is thus no robust evidence that stricter policies reduce the risk of terror, while there is robust evidence that they increase terror. Table D-6 of online appendix D confirms our previous finding with respect to the gender and skill composition of foreigners.

We conclude this section with two extensions. First, we test if the effect of migration on terror varies over different periods of time, across the different definitions of dependent variables. Table D-7 of online appendix D presents the results of a nested model in which we allow for different average partial effects between the 1980s, 1990s and 2000s. We find that the effect of the stock of foreigners is statistically different in the 1990s and 2000s compared with the 1980s, while the effect does not differ between the 1990s and 2000s. According to the estimates shown in column 1, the scale effect is about 10% lower in the 1990s and about 14% lower after the turn of the millennium compared with the 1980s. The overall effect, however, stays positive and significant at all times.

Our second extension increases the time that we allow for migrant stocks to affect terror. Rather than focusing on the immediate effect of migrants on terror, we investigate their effect after five, 10, 15 and 20 years. The results of table D-8 of online appendix D show that the effect remains significant when

⁴⁹ We thank Sven Kunze for sharing his temperature and precipitation data (Kunze 2017).

we lag the stock of migrants by five and 10 years, but is much reduced in magnitude. There is no significant effect for the deeper lags. We take this as evidence that the effect we measured in the main analysis pertains to the presence of the migrant stocks themselves, rather than any long-term effects that arise from their persistent presence in a country (such as potentially violent second generations whose size should correlate with the stock of immigrants from 20 years ago within a dyad).

6. Conclusions

Over the last 15 years, a number of countries have substantially tightened immigration laws and introduced policies putting pressure on migrants to integrate into their host countries, including restrictions on migrants' rights as well as surveillance and sanctions. These changes have in part been motivated by expectations that a larger number of foreigners living in a country increases the risk of terrorist attacks in the host country. This paper has put these expectations to the data, for 20 OECD host countries and 183 countries of origin over the 1980–2010 period.

First, we tested the hypothesis that the stock of foreigners residing in a country leads to a larger number of terrorist attacks. Our results show that the probability of a terrorist attack increases with a larger number of foreigners living in a country. This scale effect relating larger numbers of foreigners to more attacks does not imply, however, that foreigners are more likely to become terrorists compared with the domestic population. When we calculate the effect of a larger population of native-born on the number of times native-born attack foreigners or other native-born, we find this effect to be of comparable magnitude. Second, we test whether and to what extent stricter policies on immigration and integration change the effect of migrant stocks on terror. Contrary to the expectations of politicians, introducing strict laws that regulate the integration and rights of migrants does not seem to be effective in preventing terror attacks by foreign-born residents. Terrorist attacks have made politicians across the Western world severely diminish the very rights they aim to protect (Dreher et al. 2010), without, it seems, achieving the desired increase in security. To the contrary, repressions of migrants already living in the country alienate substantial shares of the population, which overall increases rather than reduces the risk of terror. We find a similar result with respect to terrorism against foreigners in their host country, which we also found to increase the risk of terror originating from the stock of foreigners.

We also test whether migrants from countries with large terrorist networks or from Muslim-majority countries affect the risk of terror differently. We find scarce evidence that terror is systematically imported from countries with large Muslim populations, or countries rich in terror. Singling out migrants from such countries by imposing stricter policies or barring them from entering entirely thus seems an ineffective policy.

We conclude with two qualifications. First, our results are based on data for the group of migrants from a particular country and the number of terrorist attacks by nationals from this country. This has the advantage that we can estimate how the risk of terror is affected by a larger number of migrants, but does not allow to test whether specific migrants are engaged in terrorist events. Such analysis would require more detailed (individual-level) data than are currently available for a large sample of countries and years.

Second, an analysis of whether or not migration should be restricted has to involve a broader calculation of its costs and benefits (Fitzpatrick 2002). Driving fast on motorways leads to accidents and fatalities, planes crash and people die and more people living in cities leads to a larger number of murder cases. Few people favour strict bans on motorways and planes, or cities. In a similar vein, a larger number of people leads to a higher risk that some of them engage in terror. This holds for native- and foreign-born populations alike and by itself hardly qualifies as reason to ban migration (or population growth). Rather, the increased risk of terror has to be weighed against the many other—positive and negative—effects that come with immigration. We leave such analysis for future research.

Supporting information

Additional supporting information can be found in the online version of this article.

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