The Political Economy of Development and Organized Violence

Der Wirtschaftswissenschaftlichen Fakultät der
Gottfried Wilhelm Leibniz Universität Hannover
zur Erlangung des akademischen Grades

Doktor der Wirtschaftswissenschaften
- Doctor rerum politicarum -

genehmigte Dissertation

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geboren am 15. September 1988 in Pforzheim

2019

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Defense
11.04.2019

Abstract

This thesis analyses several political economy mechanisms surrounding economic development and development cooperation. In Chapter 1, I show that foreign policy alignment changes following leader changes in both recipient and donor countries impact the allocation of Official Development Aid. In addition, I highlight that leader changes are a natural breaking point for bilateral relations that make usually inconsequential actions matter. In Chapter 2, I find that development aid affects civil conflict dynamics within recipient countries in previously unknown ways. Aid seems to have an escalating effect on civil conflict during episodes of minor violence, but no effect on civil conflict once violence is widespread. In Chapter 3, I analyze the effect of territorial decentralization and centralization on economic development in a global sample of primary subnational units. I show that the economic benefits of these reforms are very context-specific. Centralization reforms increase economic development in Africa, while decentralization reforms have been successful in Asia. Additionally, there are differential effects within treated areas. Centralization reforms are less beneficial for areas that move to the political periphery, while decentralization yields bigger economic payoffs in countries that have local accountability in the form of local elections. Chapter 4 turns the focus to OECD countries and analyses how international migration affects the probability of terrorism. Even though increases in the foreign-born population increase the risk of terrorism, the effect is indistinguishable from increases in the native-born population. What is more, restricting the rights of migrants does not diminish but increase the effect of migrants on terror. In summary, the thesis highlights that several political economy mechanisms in the context of development and political conflict are much more context and time dependent than previously thought.

Keywords: Political economy, economic development, political violence

Acknowledgements

I arrived in Hannover nearly four years ago, in the spring of 2015, to start my Ph.D. studies. Right from the start – I was supposed to present my Master's thesis – three people made it clear to me that I essentially knew nothing. This was quite a rocky start; yet these three people eventually became not only dear co-workers, co-authors, and a supervisor, but friends. After having learned a lot due to the effort on their part and many others, it is now time to finish the next step and submit my dissertation. And, of course, it is high time to say thanks for four wonderful years. Without the critical feedback, great suggestions and ideas, and the general support I received from the many brilliant people I was fortunate to meet and now call friends, this dissertation would not have been possible. Let me apologize right away to all the people I should have thanked here, but unfortunately did not, in order to keep the acknowledgments from becoming a separate book.

First and foremost, I would like to thank my supervisor Martin Gassebner for being a brilliant teacher and giving me the chance to pursue my Ph.D. at his chair. I am more than grateful to him for constantly challenging my thinking and pushing me to become a better researcher, providing a great work environment, introducing me to his network, enabling me to attend many workshops and conferences, co-authoring with me, and helping me to develop my own research profile. I am truly blessed to know that Martin is always in my corner. Second, I want to thank Axel Dreher for being an excellent co-supervisor both during my Ph.D. and Master's studies in Heidelberg. Without Axel I would have probably never met Martin and many more great people I have had the joy to get to know and work with over the past four years. Much like Martin, Axel seems to have endless amounts of time for me and my research, which was and still is a great gift. Third, I want to thank Roland Hodler for being my informal supervisor, becoming a great co-author, hosting me on a research stay in St. Gallen, and offering me the chance to join his team in St. Gallen as a post-doc. I look forward to spending the next years with him. I also thank Philipp Sibbertsen for agreeing to chair my defense committee.

I owe a great debt of gratitude to my smart and funny co-workers and friends at the Institute of Macroeconomics at the University of Hannover: Arevik Gnutzmann-Mkrtchyan, Hinnerk Gnutzmann, Richard Bluhm, Melvin Wong, Martin Hoffstadt, Tobias Korn, and Andrea Cinque. Without Arevik pushing me to come to the point, Hinnerks company out in the cold, and Martin's, Tobias' and Andrea's hospitality and evenings out the dissertation would not only have been impossible, but also not nearly as much fun. Special thanks goes out to Richard and Melvin. Richard had to deal with the burden of sharing countless hours in the same office and commuter train rides between Hannover and Berlin with me. Sitting opposite of him for most of the time, he bore the brunt of my questions and estimation problems. Furthermore, he has become my partner in crime on many projects that should keep us busy for the foreseeable future. Richard, I promise to call less often now that this project has come to an end, but please don't be mad should I ever break this promise. Melvin is my academic brother who back in the days relieved me from being the only Ph.D. student at the chair and with whom I went through most of the coursework and take home exams; till we finally started our own and joint projects. I also want to thank my fellow Ph.D. students and the supervisory team of the Globalization and Development Ph.D. program; for the great feedback and smart ideas they provided at our workshops. I was privileged to learn a lot from this diverse group of researchers.

A special shout out is also required for the exceptionally smart and great people I had the pleasure to co-author with and whose work features in this thesis. I want to thank Sarah Langlotz, Kai Gehring, Christian Lessmann, and Tobias Rommel, for all their effort and countless discussions on our work. Tobi, I am sorry to tell you that you recently lost your spot as the person who has read most of my work to Martin and Richard, although you had this questionable honor ever since the first term paper in our undergraduate studies. I also want to thank all the amazing people I met at various conferences and workshops over the years. You provided me with critical feedback and memorable evenings throughout Europe and the United States; I am sure you know who you are.

Finally, I would like to thank my family. Especially my parents for always encouraging me to be headstrong and follow my own way; and of course my wife, Claire. Ever since we started sharing our paths in life, everything seems to come a lot easier. I thank you for providing everything your work-absorbed husband quite oftentimes forgot, for always having my back, for supporting and loving me without any reservations. It is you who made me start not following only my own ambitions, but working for the both of us, which is why I dedicate this dissertation to you.

Paul Schaudt
Berlin, February 9, 2019

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Chapter 1

Introduction

This thesis studies the political economy surrounding the allocation of Official Development Assistance (ODA) and the effect of ODA on conflict dynamics.[1] Furthermore, it tests some of the policy prescriptions included in development programs by multinational aid donors such as the World Bank and the International Monetary Fond (IMF), specifically administrative decentralizations. Finally, it analyzes whether organized political violence, in form of transnational terrorism, systematically spreads from developing to developed countries. This is an issue that has lead to renewed political tensions between classical donor and recipient countries of development aid.

Both development and development cooperation have long been associated with political conflict. A primary motivation to start development cooperation as we know it today, has been the clash between the United States and its allies and the former Soviet Union and its satellites during the last century. Western countries assumed, based on the arguments put forth by modernization theory, that infusing developing countries with capital would help them develop economically, which in turn would prevent them from becoming allies of the communist block. Similar to international conflict, civil conflict within developing countries has been identified as one of the main factors prohibiting economic development in many developing countries. Collier (2008) has gone as far as to label it 'development in reverse'.

After development cooperation produced unsatisfying results in terms of economic growth, donors started to attach more and more conditions to development aid. The idea was that aid would help develop countries economically only if those countries would have proper institutions and policies (e.g., Burnside and Dollar, 2000). Among the most prominent of those targeted policy prescriptions are the structural adjustment programs, which started in the 1980s. A central feature of those programs was privatization of state-owned enterprises and political and administrative decentralization to improve efficiency and spur economic growth. The effects of those reforms are mixed. Importantly, it remains unclear why those programs did not deliver the expected results. One argument regarding the failure of conditional aid programs is that they are in practice not always enforced due to political reasons both within recipient countries, and political consideration between donors and recipients (e.g., Dreher and Jensen, 2007).

Political goals between donor and recipient countries are constantly changing over

[1] In the remainder of the thesis, I will use ODA, development aid, foreign aid, and aid interchangeably.

time. Ranging from military alliances during the Cold War to providing support in the Global War on Terror, and the distribution of refugees and migration flows. Especially refugees and migrants have become a contested policy issue between donors and recipients since the Arab Spring and civil war in Syria and Iraq. Many European countries are currently negotiating deals with countries south of the Mediterranean to keep people out of Europe and offer substantial amounts of money in return. A central argument made by politicians in favor of those deals, is that migration imposes high security costs on potential host countries manifested in a higher probability of terrorism.

The primary contribution of the work presented here lies in identifying and empirically analyzing previously ignored political conflicts between donor and recipient countries that explain volatility in aid allocations, as well as the effect of development aid on conflict dynamics. Furthermore, the thesis will evaluate how effective decentralization programs are in promoting economic development or whether a more centralized organization is more beneficial for developing countries. What is more, the thesis explicitly tests whether the most recent political tensions between industrialized countries and developing countries regarding migration and the spread of terrorism are a measurable threat.

The remainder of the introduction outlines the specific research questions of each chapter as well as the contributions they make to the respective literatures. Chapter 2 investigates the allocation of bilateral development aid. We argue that shifts in the foreign policy alignment between a donor and a recipient country following leadership changes induce reallocations of bilateral aid. This is due to heightened uncertainty of recipients' behavior in the international arena. Utilizing data from the G7 and 133 developing countries between 1975 and 2012 and employing high dimensional fixed effects and control function models, we show that incoming leaders in recipient countries, which politically converge towards their current donors receive more aid commitments compared to those that diverge. Additionally, accounting for leader changes in donor countries, we find that incumbent recipient leaders have an opportunity to get even more aid when political change in donor countries moves them closer to the donor's foreign policy position. Thus, leadership turnover in recipient and donor countries makes otherwise inconsequential deviations in foreign policy alignment highly consequential for aid provision.

The implications of our findings are far-reaching. We provide evidence that factors related to aid allocation (e.g., voting in the United Nations General Assembly) might be more important during changes of leadership in either recipient or donor countries. Thus, our results tie into the research on the temporal variation in the importance of specific countries, for example, non-permanent United Nations Security Council membership (Kuziemko and Werker, 2006; Vreeland and Dreher, 2014). Furthermore, previous estimates of factors explaining aid allocation (Alesina and Dollar, 2000) might be even more severe during those periods and less important during others. Note that the usual fixed effects that are employed in aid allocation studies, such as country, year or recipient-donor, recipient-year and donor-year are not able to capture dyadic administration changes. Hence, studies tend to ignore this fickle periods in bilateral relations. However, using our data allows to control for it.

Chapter 3 analyzes the effect of bilateral ODA on civil conflict in recipient countries. It innovates upon previous research by explicitly modelling the effect of bilateral aid on conflict escalation and deescalation dynamics. We make three major contributions. First, we combine data on civil wars with data on low level conflicts in a new ordinal measure capturing the two-sided and multifaceted nature of conflict. Second, we develop a novel empirical framework. We propose a dynamic ordered probit estimator that allows for unobserved heterogeneity and corrects for endogeneity. Third, we identify the causal effect of foreign aid on conflict dynamics by predicting bilateral aid flows based on electoral outcomes of donor countries that are plausibly exogenous to conflict in recipient countries. We establish that the effect of foreign aid on the various transition probabilities is heterogeneous and can be substantial. Receiving bilateral aid raises the chances of escalating from small conflict to armed conflict, but we find little evidence that aid ignites conflict in truly peaceful countries.

This chapter highlights the importance of investigating factors influencing conflict within the dynamics of conflicts themselves to obtain proper estimates for those explanatory variables. Previous studies have mostly focused on the causal identification of first order effects for a variety of variables, such as aid (Nunn and Qian, 2014). However, as our study shows, state dependency is a major factor in conflict dynamics—conflict begets conflict. Thus, many of the relationships that have been uncovered so far (see Blattman and Miguel, 2010, for an overview) might be underestimated or overestimated with regard to specific conflict intensities. Identifying the actual conflict intensity on which the previously obtained first order effects actually obtain their power is a major challenge to inform policy recommendations.

In Chapter 4, we study the effect of territorial administrative reforms on local economic activity between 1990 and 2014 in a global data set covering 208 countries and territories. We establish a purely spatial approach to track all first order territorial administrative reforms occurring during our period of study. Furthermore, we locate all first order subnational administrative centers (cities) around the world. The two datasets provide the first comprehensive database on territorially reformed areas around the world together with their local administrative center. We test how territorial administrative reforms affect local economic activity combining our unique dataset with remotely sensed data on economic activity. We use panel difference-in-difference and triple difference-in-difference estimations to obtain their effects. We find that territorial decentralization has no significant effect on economic activity at the local level, while centralization reforms have a significant and meaningful positive effect on light intensity. Importantly, there is substantial regional variation. We find that the global finding is driven by the positive effects of centralizations in Africa, while territorial decentralizations have translated into sizable gains in local economic activity in Asia. In summary, the chapter shows that while there is a first order effect of territorial centralization on economic output, it is also sensitive to the context in which it occurs. Decentralizations are for example most effective if politicians are selected within subnational units, while centralizations are less beneficial in such settings. Furthermore, areas that experience an increase in the distance to their subnational administrative center gain less from centralization reforms, compared to those more centrally located.

Our results highlight that the conclusions of previous case studies about the effectiveness of administrative centralization and decentralization (e.g., Burgess et al., 2012; Grossman and Lewis, 2014) should be considered with care, since there is an abundance of heterogeneity in the effectiveness of those reforms depending on the federal-political context. Additionally, scholars should focus more on the distributional consequences within reformed areas, since there are sizable differences. The chapter also provides a practical solution to cope with non-stationary units in geospatial panel data studies. Using our splinter dataset, where splinters are defined as the smallest territorially unchanged area, allows researchers to control for the political affiliation of a specific geographic area over time, which might be especially important for researchers studying local conflict using geospatial data (e.g., Berman and Couttenier, 2015).

Chapter 5 of the thesis switches the focus to the classical donor countries within the Organization of Economic Cooperation and Development (OECD) and studies whether migration is related to organized violence. It focuses on the effect of migration on terrorism, since terrorism—unlike crime—is politically motivated. We analyze the causal effect of the foreign-born population residing in a country on the probability of a terrorist attack committed by a foreign national of that population in the host country. Our instrument for the stock of foreign-born population relies on the plausibly exogenous interactions of two sets of variables with respect to transnational terrorism in host countries. Variation across host-origin-dyads results from structural characteristics between the country of origin and the host, while variation over time is provided by changes in the push and pull factors between host and origin countries resulting from natural disasters. Using data for 20 OECD host countries and 183 countries of origin over the 1980-2010 period we show that the probability of a terrorist attack increases with a larger number of foreigners living in a country. However, we cannot reject the hypothesis that this scale effect is the same than the effect of domestic populations 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 stigmatize and exclude foreigners already living in a country increase rather than reduce the risk that foreign populations turn violent, as does terrorism committed against foreigners within the host country. Highly skilled migrant populations are associated with a significantly lower risk of terror compared to low skilled ones, while there is no significant difference between male and female migrants.

Our dyadic setup highlights that studies testing for the effect of migration on terrorism using monadic setups might be misleading since the scale effects of natives and foreigners are not separated. Furthermore, dyad-specific factors, such as conflict or colonial linkages between host and origin countries, might play a major role for the relationship between migration and terrorism that is not easy to disentangle—OECD host countries, for example, seem to be very different compared to developing host countries (Bove and Böhmelt, 2016).

Chapter 6 concludes the dissertation. It puts the different chapters into the broader context of the political economy of development and organized violence, highlights the lessons learned, and discusses open issues and potential ways to extend the research agenda laid out in this dissertation.

Chapter 2

Aid allocation

On the role of political leader pairs and foreign policy alignment changes.[*]

2-1 Introduction

Official Development Assistance (ODA) is an important source of financial liquidity for developing countries. If funds run dry, these countries face severe economic repercussions. As aid is not exclusively granted on need, both the size and the volatility of aid flows are subject to politics.[1] Long-term relations, such as colonial ties or geopolitical considerations (e.g., Alesina and Dollar, 2000; Collier and Dollar, 2002) and short-term shifts in the political importance of recipients, such as membership in the United Nations Security Council (UNSC) (e.g., Kuziemko and Werker, 2006; Dreher et al., 2009a,b), affect bilateral ODA flows. Apart from a recipient's international standing, its political positions matter as well. Disagreement between donors and recipients on policies significantly lowers aid flows, especially if issues are highly relevant for donors (Andersen et al., 2006; Dippel, 2015; Vreeland and Dreher, 2014). Donors even adjust access to liquidity strategically in order to influence elections in recipients countries. They increase bilateral aid to political friends during election years, thereby bolstering re-election prospects, while they decrease aid to political opponents (Faye and Niehaus, 2012).[2] Given the fact that donors actively try to use aid to keep their friends in power, it is surprising that we know only little about changes in aid allocation when this strategy fails, i.e., after leadership turnover: how do donors adjust aid provision following leader change?

Our chapter proposes an answer to this question. Leadership turnover – in both recipient and donor countries – is a source of uncertainty concerning future behavior in the international arena. Since the pursuit of foreign policy is usually the prerogative of the executive branch, leader change opens the door for large-scale

[*]This chapter is based on joint work with Tobias Rommel (Rommel and Schaudt, 2017).

[1]The various motives that influence aid allocation also pose challenges for the estimation of the causal effect of aid on various outcomes, such as consumption, investment or trade (Temple and de Sijpe, 2017).

[2]Similarly, the United States use their weight in the International Monetary Fund to provide loose conditions on credits (Dreher and Jensen, 2007) and in the World Bank to provide quicker loan disbursement (Kersting and Kilby, 2016) for political friends in the run-up to elections.

policy shifts. Nevertheless, re-alignment can go in both directions. New leadership does not automatically guarantee improved bilateral relations between donors and recipients. Therefore, the consequences of leader change for aid allocation are ambiguous ex ante. We argue that donors take foreign policy positions announced by recipients under increased scrutiny. Shifts in foreign policy following leader change work as an important source of information on which donors base their decisions regarding aid allocation. What is more, we argue that the effect of political re-alignment on aid allocation is not only present in case of leadership change in recipient countries, but is also consequential following leadership change in donor countries. Given that political relationships between states are reciprocal, changes in the head of executive of donor countries similarly increase uncertainty by discounting past behavior and therefore expectations about future relations. Hence, new donor leaders base aid disbursement on the foreign policy changes of recipient country governments. We expect that sizable reallocations of development aid occur after either recipient or donor leader change. Yet, the direction should depend on the foreign policy shifts of recipient countries towards donors. Leaders who signal political accord receive more aid; countries receive less aid if a leader signals political animosity.

Canada, for instance, takes recipients' foreign policy positions into account when it comes to aid provision. The Canadian International Development Agency (CIDA) explicitly states that they base aid disbursement on recipients' "needs, their capacity to manage development programs, and their alignment with Canadian foreign policy priorities" (CIDA, 2010, 3). In line with this notion, Ghana has always received sizable amounts of aid from Canada (Global Affairs Canada, 2015), but experienced a sharp decrease in 2009. Interestingly, this drop coincides with a change in leadership following the 2008 general elections. John Atta Mills defeated Nana Akufo-Addo in the second round run-off election held on December 28, 2008 by a margin of about .5% and was declared president on January 3, 2009. Uncertainty was high about the leadership's policy positions, which was further increased by the fact that Atta Mills had distanced himself from his mentor, former president Jerry Rawlings, during the campaign (Encyclopaedia Britannica, 2017). More importantly, alignment in the United Nations General Assembly between Canada and Ghana decreased by about 7 percentage points, indicating less support for Canada's foreign policy stance, which in turn was followed by cuts in aid.

To capture shifts in foreign policy, we rely on comparable measures of voting alignment in the United Nations General Assembly (UNGA) (Voeten, 2000). Voting in international organizations is a cost-effective way for donors to infer political accord or animosity of their recipients. Accordingly, UNGA voting patterns have frequently been used to proxy for political closeness between countries (e.g., Thacker, 1999; Barro and Lee, 2005; Bailey et al., 2017). Indeed, studies suggest that changes in heads of executive make a decisive difference when it comes to foreign policy proximity (Dreher and Jensen, 2013; Mattes et al., 2015). Yet, research has focused exclusively on either leadership changes in recipient countries only, or monadic (non-directed) position changes. We assert that leader changes in both recipient and donor countries affect bilateral relations and, consequently, aid allocation.

We focus on alignment changes that occur after leadership change in a dyadic donor-recipient leader pair, between leaders from the G7[3] and 133 developing countries from 1975 to 2012. Employing high dimensional fixed effects and control function models, we show that yearly alterations of foreign policy alignment have no significant effect on aid commitments from current donors, unless they occur in times of heightened uncertainty after leadership turnover. In line with our argument, the adjustment of foreign policy objectives after leader changes has a tremendous impact on aid commitments. Donors reward political convergence and punish divergence. These effects are different in substantial terms. Our findings suggest that leader changes in donor countries represent a 'window of opportunity' that recipients can use to attract gains in development aid, while recipient leader changes open predominantly a 'window of dis-opportunity' to forgo aid cutbacks. Focusing exclusively on monadic leadership changes in recipient countries is not able to capture this essential variation in the allocation of aid induced by leadership changes. Taken together, recipient country leaders have to decide early on how to align themselves with their international aid providers, as first impressions matter a great deal.

We proceed as follows: Section 2-2 presents our theoretical argument linking dyadic leadership change, political alignment, and aid allocation. Section 2-3 describes the data. Section 2-4 discusses our empirical strategy and results. Section 2-5 presents robustness tests. Section 2-6 concludes.

2-2 Leader Change and Aid

Donor countries have vested interests in political alignment with developing countries and thus care about which recipient leader is in power (Dreher and Jensen, 2007; Faye and Niehaus, 2012). As a consequence, leadership turnover in recipient countries endangers donors' interests, as it sets the stage for new foreign policy agendas. After inauguration a new recipient country government can adjust its foreign policy towards donors in three ways: keep relations unchanged, converge towards a common ideal position on international issues, or diverge. Because a new leader in a recipient country has the potential to change bilateral relations and the direction of this change is unclear ex ante, the leader in a donor country faces high uncertainty about the behavior of the recipient leader in the international arena, especially in the aftermath of leadership change.[4]

As a reaction to such changes in political alignment, we argue that donors reevaluate the current financial support they provide to a recipient country. A donor country possesses two options to alter its development cooperation: reward political friends with external revenues or deprive opponents of political and economic benefits. In general, donors have an incentive to bind new leaders early on by granting more development aid. Given budget constraints, they have a rationale to only reward politically aligned leaders with additional aid, however. To the contrary, donors hamper new recipient leaders by cutting aid if they perceive them as hostile. This deprives political foes of fungible revenues and handicaps their popularity and reelection chances early on.

[3]Canada, France, Germany, Italy, Japan, United Kingdom and United States

[4]Incoming political leaders have a wide range of effects, for instance regarding trade (McGillivray and Smith, 2004), economic growth (Jones and Olken, 2005), or democratization (Jones and Olken, 2009).

Whether a country under new leadership is a political friend or foe is difficult to evaluate in advance. Relying on ex ante characteristics, such as the foreign policy stances of leaders in the run-up to elections, may provide only an incomplete picture of an administration's foreign policy agenda. Past observed behavior should be heavily discounted as governments have private information that shape their foreign policy preferences as well as incentives to conceal their true intentions (e.g., Fearon, 1995, 1997). Additionally, audience costs change in conjunction with leadership turnover, effectively altering incentive structures for the leader after an election.[5] Lastly, the new leader may only imperfectly be bound to path-dependency or even have come to power by opposing the existing policy platform. Hence, the reaction of the donor hinges on the ex post conduct of the new leadership in the recipient country.

We argue that donor countries observe the behavior of new recipient country leaders during their first year in office, for example via voting alignment in the UNGA. Such votes cover a wide array of issues that allow political actors to estimate alignment tendencies and are thus a "record of how the state wants to be seen by others, the international norms it finds acceptable, and the positions it is willing to take publicly" (Mattes et al., 2015, 283). Voting in line with (or against) a donor's interests thus constitutes a cost-effective source of information that the donor can observe and use to determine if the other leader is more likely to be a friend or foe in the future. Another way to think about voting in the UNGA is in terms of revealed preferences. The donor might for example already have had talks to the new recipient leader and some understanding on foreign policy alignment, before the bulk of votes in the UNGA occurs during the last quarter of a year. In cases where the donor and the recipient have a good relationship we would expect more common votes in the UNGA than if they disagree on foreign policy issues.[6] In both cases, the initial trajectory of foreign relations should matter for the amount of political side payments the donor chooses to make (Kuziemko and Werker, 2006; Bueno de Mesquita and Smith, 2010).[7] Summing up, leadership change itself should not necessarily alter the allocation decision of the donor. Rather, the donor's willingness to provide ODA is shaped by the initial foreign policy positions that a new recipient leader takes.

H1: The effect of recipient country leadership change on aid flows is conditional on the political alignment new leaders establish towards their donor during their first year in power. Alignment with the donor increases aid flows; dis-alignment decreases aid flows.

Nevertheless, the very nature of political alignment is reciprocal. Therefore, the importance of foreign policy realignment does not solely originate from recipient country leadership changes. If a donor country leader enforces a new set of foreign policy objectives, its repercussions influence a recipient country's ability to pursue and implement its own policy goals. In other words, leadership changes in donor

[5] Arguably, a sitting leader wants to stay in power and is internally constrained by his domestic support groups (Moravcsik, 1997; Putnam, 1988; Bueno de Mesquita et al., 2003).

[6] This line of reasoning is closely linked to arguments developed by Vreeland and Dreher (2014) for voting in the UNSC.

[7] Note that such information becomes even more important if there is no prior observable behavior of an actor.

countries themselves shape bilateral foreign policy proximity. Thus, the pursuit of foreign policy goals is further confined by external constraints that arise from the behavior and power of other countries. In essence, both leaders matter for bilateral relations between countries. What is more, reacting to changes in donor countries might be in the interest of recipient countries. Internal constraints are fixed in the short run. Leaders are usually not able to change their support group – the electorate in democracies or the selectorate in autocracies – because the associated costs endanger their hold on power. Changes in the donor's foreign policy that emanate from leadership change thus open a window of opportunity for recipient countries to change bilateral relations, as external constraints on foreign policy decrease.

Consider that newly elected US presidents attempt to accomplish international success rather quickly. Barack Obama, for example, vowed to reset relations with the Middle East and reduce US interference in his Cairo speech, held shortly after his 2009 inauguration (New York Times, 2009). Donor leaders consider the reactions from the developing world as approval or dis-approval. A recipient country can either show willingness to work together or take a stance and openly oppose the new foreign policy agenda of the donor. In this sense, a change in donor leadership can provide other countries with the opportunity to reset relations or withdraw loyalty, respectively. If leaders welcome a new president and signal that they will work with them, they receive additional aid as part of a charm offensive. If a new leader in a donor country receives hostile signals from a recipient country's political leadership, aid flows decrease. In both cases we argue that first impressions matter a great deal and should influence the allocation of aid.

H2: Recipient country convergence towards a donor's foreign policy position after donor country leadership change increases aid flows; divergence decreases aid flows.

Leadership changes in both the recipient and the donor country reset personal relationships and domestic constraints on leaders opening windows of opportunity to fundamentally change foreign policy. In such situations, uncertainty in the bilateral relations between a donor and a recipient country rises and donor leaders make aid allocation decisions depending on ex post changes in foreign policy positions of recipient countries. Because donor countries have vested interests in political alignment, they reward political alignment and punish dis-alignment.

2-3 Data and Operationalization

Our dependent variable is Official Development Aid. In line with Faye and Niehaus (2012), we use ODA commitments instead of disbursements,[8] since disbursements in a given year might originate from projects granted earlier. Commitments on the other hand are targeted to a specific country in a given year. Hence, we can directly link them to shifts in political alignment between countries following leadership turnover. We take ODA commitments from the Development Assistance Committee

[8]Commitments are measured in millions of constant 2013 US$.

(DAC) database of the OECD (2017).[9] Because aid commitments are highly skewed, we use log-transformed values. We focus mainly on country dyads with positive aid flows to avoid arbitrary log-transformations. Nevertheless, we control for the inclusion of zeros as well as for selection effects in the robustness section.[10]

Our first independent variable is leadership change. We use data from the updated Archigos dataset (Goemans et al., 2009) to identify the heads of executive of each recipient and donor country. We code a change in leadership if the leader of country i in year t differs from the leader of country i in year $t-1$. If several leaders were in power in a country during a given year, we focus on the leader that has spent the highest fraction of days in office over the course of the respective year. As such, we assume that more days in office increase the capacity of a country's leader to shape foreign policy within a given year.[11] Assuming that foreign policy is 'high politics' and primarily influenced by the person running the executive branch, we define the head of the executive as the country's leader. In a next step, we use information on leadership changes in recipient and donor countries to construct dyadic leader changes. Our units of analysis are leader dyads. To illustrate this approach, consider that former President Barack Obama and former President Dilma Rousseff had formed the dyad between the United States and Brazil until May 12, 2016, until she was replaced by Michel Temer.[12]

Our analysis includes 133 recipient countries (see Table 2-A1) that – in tandem with the G7 donor countries – form 686 country dyads that engage in development cooperation over the 1975-2012 period. The panel is unbalanced since some recipient countries enter after 1975. Similarly, some donors only engage in development cooperation with a selected set of recipients. Given these limitations, our dataset includes 7505 donor-recipient-leader-pairs and 5010 dyadic leader changes. The median leader dyad lasts about five years. By construction, the shortest period is one year. The most durable leader dyads are between Germany under Chancellor Helmut Kohl and several recipient countries with a duration of 16 years; the exact time Kohl was in office. All G7 countries form administration dyads lasting longer

[9] ODA is defined as those "flows to countries and territories on the DAC list of ODA recipients and to multilateral institutions which are: i. provided by official agencies, including state and local governments, or by their executive agencies; and ii. each transaction of which: a) is administered with the promotion of the economic development and welfare of developing countries as its main objective; and b) is concessional in character and conveys a grant element of at least 25 per cent (calculated at a rate of discount of 10 per cent)" (OECD, 2017). Over the years the DAC has refined the ODA reporting rules to ensure accuracy and consistency among donors. The boundary of ODA has been carefully delineated, including: 1. Military aid: No military equipment or services are reportable as ODA. Anti-terrorism activities are also excluded. The cost of using donors' armed forces to deliver humanitarian aid is eligible. 2. Peacekeeping: Most peacekeeping expenditures are excluded in line with the exclusion of military costs. Some closely defined developmentally relevant activities within peacekeeping operations are included. 3. Nuclear energy: Reportable as ODA, provided it is for civilian purposes. 4. Cultural programs: Eligible as ODA if they increase cultural capacities, but one-off tours by donor country artists or sportsmen, and activities to promote the donors' image, are excluded.

[10] Note that 23% of the observations on bilateral aid flows are zero. This is mainly driven by the complete absence of development cooperation between Japan and several developing countries.

[11] This approach differs from Mattes et al. (2015) who use information on the leader who is in power in December for the entire year.

[12] Note that we would code a change for 2016 since Michel Temer has occupied more days in office than Dilma Rousseff. If he would have stepped down early and another person would have held office also for a shorter time than Mrs. Rousseff, we would have coded the change in 2017.

than 10 years, with the exception of the United States, due to presidential term limits.

The second independent variable is the change in foreign policy alignment between countries. We proxy changes of bilateral relations, using voting alignment in the United Nations General Assembly. Focusing on the UNGA has several advantages: data availability is generally very high because all sovereign countries have voting rights. Votes in the UNGA furthermore cover a wide array of issues that allow to proxy general alignment tendencies instead of ad hoc political liaisons (Mattes et al., 2015). Voting alignment has thus often been used to proxy political closeness. We measure voting alignment changes as the difference in the percentage of common yes and no votes between any two countries in one administration dyad between $t-1$ and t (Thacker, 1999; Faye and Niehaus, 2012). The data are provided by Voeten et al. (2017). Although this difference ranges empirically from -94 to +67 percentage points, such radical changes in bilateral relations are rather uncommon (Voeten, 2004; Hillman and Potrafke, 2015). Nevertheless, we test whether our results are sensitive to radical changes by restricting the scope of the alignment change in the robustness section. In addition, we make use of different measures that also include vote abstentions (Barro and Lee, 2005). Note also that Häge and Hug (2016) show that UNGA affinity scores are sensitive to the inclusion of consensus votes that systematically increase voting alignment between all country pairs. As we use changes in voting alignment, this should not affect our measure if the number of consensus votes does not change dramatically from year to year. In the main models, we use all votes since general foreign policy preferences are arguably more reliably revealed by all votes, as compared to only important votes (Andersen et al., 2006). Nonetheless, we test the robustness of our results and also include regular votes – votes that reoccur over UNGA sessions – and key votes (Kilby, 2009; Kersting and Kilby, 2016).

To isolate initial changes in foreign policy alignment from general long- and short-term alignment or dis-alignment tendencies between donor and recipient over time, we further include two variables into our baseline specification: in line with Faye and Niehaus (2012), we control for alignment between the former recipient and donor leader. For this we use average alignment over the past administration dyad instead of recipient leader dyads. This limits the maximum average alignment to 16 years, whereas Faye and Niehaus (2012) have cases where the alignment is averaged over nearly their entire sample period. For instance, Muammar al-Gaddafi ruled Libya from 1977 to 2011 and essentially covered the whole spectrum of political relationships with several G7 countries over those years. We argue that our dyadic measure of previous alignment is better able to capture past alignment because it does not blur the current relations by relations from decades ago that, in addition, were established by other administrations in donor countries. The effect of past mean alignment thus captures how well the previous administration dyad has worked with each other and explains path dependency in current bilateral relations. Moreover, we also include the lagged alignment level since it mechanically determines the possible range of re-alignment between t and $t-1$. Descriptive statistics of all variables used in the study are reported in Table 2-A2, sources and definitions in Table 2-A3.

2-4 Empirical Strategy and Findings

In our baseline specification (see eq. (2-1)) we regress the natural logarithm of ODA commitments at time t between the leader pair of donor country j to recipient country i on dyadic leader change, alignment changes and their interaction. The alignment change is defined as the difference in common votes between two countries from $t-1$ to t. The coefficient of interest is the interaction between leader change and changes in voting alignment, i.e., the corresponding change in voting alignment in the UNGA from $t-1$ (the last year of the outgoing leader in either one of the two countries) to t (the first year of the new leader in either one of the two countries). We expect a positive interaction effect of θ implying that alignment following a change in leadership increases aid flows, while dis-alignment decreases aid flows. ϕ captures the effect of lagged alignment. As such, it controls for the recent past of UNGA alignment in a dyad d, which determines the possible range of the change in voting alignment. ψ controls for past mean alignment of the previous administration dyad, to capture the overall relations between the two countries.[13] η is a vector including a set of additional donor and recipient control variables, such as GDP and population. α_{ij} are donor-recipient fixed effects capturing unobserved time-invariant heterogeneity for specific country dyads. Additionally, γ_t are year fixed effects to control for any global shocks that simultaneously affect alignment, leader change and aid commitments across all countries.

$$\begin{aligned} lnODA_{ijt} = {} & \beta \cdot leader_{ijt} + \delta \cdot \triangle alignment_{ijt} + \theta \cdot (leader_{ijt} * \triangle alignment_{ijt}) \\ & + \phi \cdot alignment_{ijt-1} + \psi \cdot meanalignment_{ijd-1} + \mathbf{X}'_{ijt}\boldsymbol{\eta} + \alpha_{ij} + \gamma_t + \epsilon_{ijt} \end{aligned} \tag{2-1}$$

Table 2-1 displays the results of this empirical strategy, when phasing in the different components of the regression model. Column 1 only includes dyadic leadership change. It shows that there is no unconditional effect of leadership turnover on ODA commitments from donor to recipient in a given donor-recipient pair; β is not statistically significant. Hence, the pooled leader change effect from either recipient or donor country does not affect aid allocation in a systematic way. In column 2, we only include the yearly change of voting alignment in the UNGA. The statistically significant positive effect highlights that convergence induces more aid. In column 3, we include our main independent variable – the interaction between changes in political alignment and leadership change. Dyadic leader changes with constant bilateral relations as well as yearly fluctuations in alignment in years without leadership turnover are both statistically insignificant. To the contrary, the interaction term is, as expected, positive and statistically significant. Voting convergence after either a donor or a recipient leader change is rewarded with more ODA commitments, while divergence is punished with aid cutbacks. Thus, the significant unconditional convergence effect is solely due to alignment changes after leader change. These findings show that leadership turnover itself does not change aid allocation patterns. Change in leadership becomes consequential only if it simultaneously changes the trajectory of foreign relations between countries.

[13] Note that lagged alignment and mean alignment of the past administration dyad are the same if the last administration dyad lasted only for one year. This is the case in 10 percent of our observation. In such cases no additional information is provided by the inclusion of the mean alignment variable.

Table 2-1 – Dyadic Leader Changes

	Dependent variable: *ln ODA commitments*			
	(1)	(2)	(3)	(4)
Dyadic Leader change	-0.027		-0.020	-0.005
	(0.024)		(0.024)	(0.079)
Alignment change		0.501**	-0.015	-0.012
		(0.209)	(0.214)	(0.350)
Leader change * realignment			1.393***	1.288***
			(0.317)	(0.382)
Last year alignment	0.577***	1.006***	0.712**	0.887*
	(0.218)	(0.329)	(0.324)	(0.528)
Past mean alignment	1.099***	0.813***	1.211***	0.838*
	(0.267)	(0.271)	(0.292)	(0.446)
Log GDP recipient	-0.136	-0.135	-0.131	
	(0.132)	(0.133)	(0.132)	
Log GDP donor	2.302***	2.286***	2.283***	
	(0.649)	(0.649)	(0.647)	
Log population recipient	0.804**	0.806**	0.810**	
	(0.340)	(0.340)	(0.341)	
Log population donor	0.147	0.278	0.459	
	(1.017)	(1.028)	(1.032)	
Adjusted R-squared	0.043	0.044	0.045	0.786
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,RY,DY
# of observations	16928	16928	16928	18571
# of dyads	668	668	668	681

Notes: Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In column 4, we further exploit the dyadic structure of our data by employing donor-recipient-pair, donor-year and recipient-year fixed effects. This approach enables us to control for other factors that vary on either donor or recipient countries over time and explain ODA allocation. Hence, unobserved heterogeneity is reduced to variables that vary within the dyads over time and are not explained by variables varying over donor and recipient by year, such as GDP or population size. A further benefit of this approach is that we do not decrease our sample size due to data availability of the control variables. The results show that the magnitude of the conditional alignment effect θ decreases slightly if we control for donor and recipient-specific factors.[14]

2-4.1 Differences between Recipient and Donor Leader Changes

In a next step, we investigate the conditional effect of leadership change and foreign policy realignment on the allocation of ODA commitments by differentiating between foreign policy changes that emanate either after a recipient or donor leader change

[14]The difference is not driven by the increasing sample size.

(see eq. (2-2)).[15] The results are displayed in Table 2-2.

$$\begin{aligned} lnODA_{ijt} = {} & \beta_1 \cdot recipient_{it} + \beta_2 \cdot donor_{jt} + \delta \cdot \triangle alignment_{ijt} \\ & + \theta_1 \cdot (recipient_{it} * \triangle alignment_{ijt}) + \theta_2 \cdot (donor_{jt} * \triangle alignment_{ijt}) \\ & + \phi \cdot alignment_{ijt-1} + \psi \cdot meanalignment_{ijd-1} + \mathbf{X}'_{ijt}\boldsymbol{\eta} + \alpha_{ij} + \gamma_t + \epsilon_{ijt} \end{aligned} \tag{2-2}$$

Column 1 illustrates that the specific type of leader change matters for aid allocation. While changes in donor countries are statistically insignificant, leadership turnover in recipient countries leads to less ODA on average. Taken at face value, this would imply that donors are cautious towards heads of executive that take over power in recipient countries. However, the results in column 3 qualify this effect. The interactions between voting alignment change and both recipient and donor leader change are positive and statistically significant. Furthermore, the sizable interaction effect offsets the negative effect of recipient leader change with no voting alignment change. Hence, convergence gets rewarded while divergence leads to a reduction in ODA commitments, regardless whether voting re-alignment is a reaction of recipient countries to a new leader in a donor country or a re-alignment of foreign policy after domestic leader change.[16]

Furthermore, the effects remain stable when we include donor-recipient-pair, donor-year and recipient-year fixed effects (column 4). A downside of this specification is that we cannot draw conclusions regarding the effect of leader change in instances where voting alignment is constant, since the fixed effects absorb the unilateral change variables.[17] The results are robust to a more conservative model including the similarity indexes (Helpman, 1987) of GDP and population size. The idea behind the inclusion of those indices is that countries more similar in GDP or population size might agree more on issues of trade or other issues potentially discussed in the UNGA, it could of course also be the other we around. We remain agnostic to both possibilities. [18] Taken together, these results strongly support our hypotheses.

[15]Note that β and θ have been changed to β_1 and β_2 as well as θ_1 and θ_2. Although theoretically possible, we do not include mutual leader changes as a separate category because they are empirically too infrequent.

[16]To test for autocorrelation, we reran all the models in Table 2-2 including lagged ODA commitments (results not reported). The lagged commitments are statistically significant, and have a point estimate up to 0.4 in the HDFE specification. A test for first order autocorrelation (Wooldridge, 2010; Drukker, 2003) cannot reject the null of no autocorrelation. Furthermore, a Fisher-test for a unit root in panel data using the Dickey-Fuller approach (Choi, 2001), utilizing up to 3 lags, neglects the presence of a unit root. We also included donor and recipient change and their respective interactions in separate regressions (results not reported). This leads to an increase in the magnitude and statistical significance of the single effects. Hence, our results are not driven by the simultaneous inclusion of both types of changes.

[17]The results are also robust to different forms of clustering (Cameron et al., 2011), such as clustering on donor, recipient and year or donor-recipient-pair and year. Since we only have 7 donors and our baseline results are stable, we cluster on the donor-recipient pair in the rest of our specifications.

[18]The similarity indexes are defined as follows: $SimilarityIndex(GDP)_{ijt} = 1 - (\frac{GDP_i}{GDP_i * GDP_j})^2 - (\frac{GDP_j}{GDP_i * GDP_j})^2$ and $SimilarityIndex(Population)_{ijt} = 1 - (\frac{Pop_i}{Pop_i * Pop_j})^2 - (\frac{Pop_j}{Pop_i * Pop_j})^2$. Results of the specification are reported in column 2 of Table 2-A4.

Table 2-2 – Dis-aggregate Leader Changes

	Dependent variable: *ln ODA commitments*			
	(1)	(2)	(3)	(4)
Recipient change	-0.104***		-0.098***	
	(0.032)		(0.032)	
Donor change	0.033		0.045*	
	(0.026)		(0.027)	
Alignment change		0.501**	0.002	0.034
		(0.209)	(0.219)	(0.350)
Recipient change * realignment			1.370***	1.187**
			(0.406)	(0.502)
Donor change * realignment			1.031***	0.877*
			(0.334)	(0.472)
Last year alignment	0.572***	1.006***	0.735**	0.937*
	(0.218)	(0.329)	(0.326)	(0.529)
Past mean alignment	1.082***	0.813***	1.127***	0.730*
	(0.266)	(0.271)	(0.284)	(0.434)
Log GDP recipient	-0.141	-0.135	-0.136	
	(0.132)	(0.133)	(0.132)	
Log GDP donor	2.254***	2.286***	2.246***	
	(0.648)	(0.649)	(0.646)	
Log population recipient	0.794**	0.806**	0.792**	
	(0.340)	(0.340)	(0.341)	
Log population donor	0.170	0.278	0.412	
	(1.017)	(1.028)	(1.030)	
Adjusted R-squared	0.044	0.044	0.046	0.786
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,RY,DY
# of observations	16928	16928	16928	18571
# of dyads	668	668	668	681

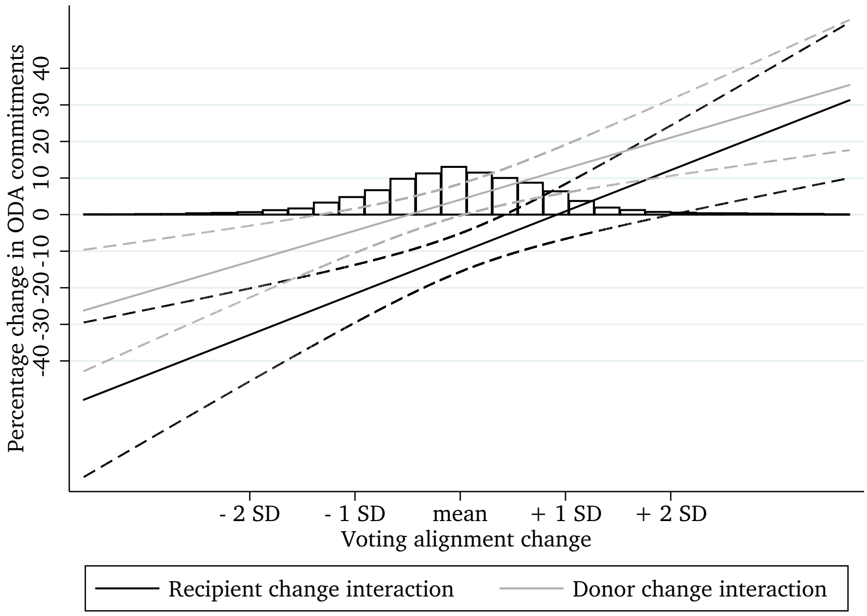
Notes: Leader change variables in column 4 are omitted due to fixed effects. Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To test if aid changes are indeed a political reaction, we test for other potential channels, such as trade, that vary between donor-recipient pairs over time. The bulk of bilateral trade between the G7 and other countries is driven by private firms that should care more about country-specific issues like property rights (absorbed by the fixed effects) and less about political alignment. Hence, we would expect no effect on bilateral trade resulting from our proposed mechanism, nor should trade impair our mechanism with regard to aid. This is exactly what we find. The inclusion of bilateral donor and recipient imports do not change our conditional alignment effect in Table 2-A4. Falsification tests, in which we replace ODA commitments with both donor and recipient imports, yield also no significant results (columns 4 and 5 of Table 2-A4).[19]

How consequential are these effects for recipient's revenue streams? To answer

[19]Testing for other channels is more difficult, as data availability is not sufficiently high for our sample. Remittances, for example, are only available on the recipient country level and not bilaterally before 2005.

Figure 2-1 – Marginal Effect of Leader Change, Conditional on Alignment Change



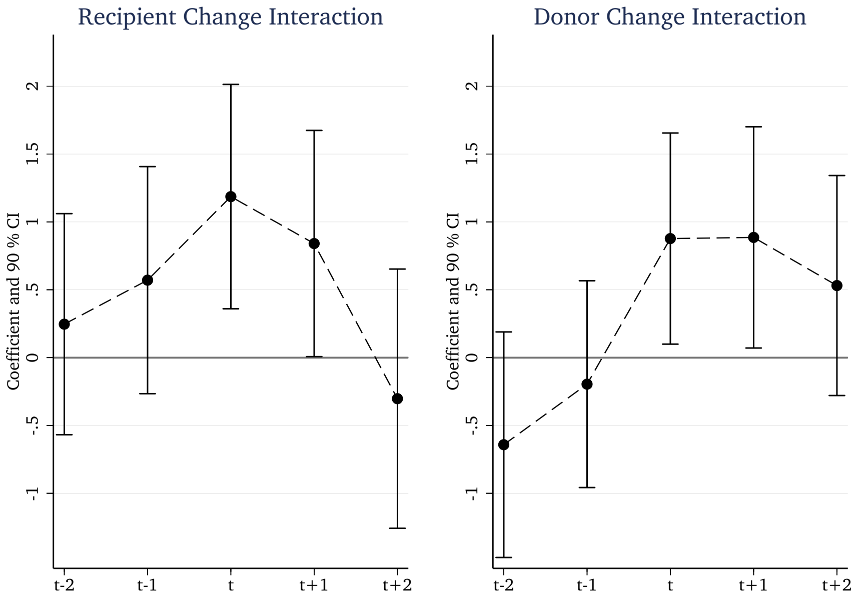
this question, we estimate the predicted change of ODA commitments in percentage points with respect to the change in voting alignment and the type of leadership turnover (based on model 3 in Table 2-2). The results are plotted in Figure 2-1. At the mean alignment change, representing marginal dis-alignment (see Table 2-A2), new recipient leaders receive 9.7% less ODA commitments in their first year. In the opposite case of donor leader change, they receive 3.8% higher ODA commitments.

If a newly inaugurated recipient leader, however, chooses to dis-align by one standard deviation – which is approximately a 8 percentage point decrease in voting alignment from one year to another – ODA commitments to this country shrink by 19.6%. Hence, decreasing political proximity with donor countries in the UNGA increases the negative effect of domestic leader change by about 10 percentage points for aid recipients. In case of donor leader change, dis-alignment seems to have no substantial effect. Conversely, foreign policy convergence gets rewarded with additional aid. A move towards the donor by one standard deviation results in 9.1% more ODA commitments. In substantial terms, these numbers show that signaling political accord or animosity matters a great deal in times of high uncertainty in bilateral relations, especially with regard to the economic implications of politically granted development aid.[20] Consider for example that the median aid recipient in our sample receives around $100m in development aid from the G7 annually. According to our results, if a new recipient country leader were to alter their foreign policy proximity to international aid providers by one standard deviation, the country would face a cut of 19.6%, i.e., almost $20m.

Summing up, political re-alignment after leader change is highly consequential for recipient countries. While new recipient leaders can mainly forgo cutbacks by aligning themselves with donors, existing recipient country leaders have an

[20] Note further that the size of the alignment change effect is much more pronounced in case of recipient leader change than for donor leader change. This is due to the fact that all donor countries react to recipient leader change at once, while only the affected donor reacts after donor leader change.

Figure 2-2 – Timing of the Conditional Alignment Effect



Notes: The underlying regression specifications are reported in Table 2-A5 in the Appendix. Standard errors are clustered at the donor-recipient dyad. The interaction between recipient leader change and alignment change at time t is statistically significant at the 5% level, the interaction between donor leader change and alignment change is significant at the 10% level.

opportunity to fill the public purse when a new donor leader enters office.

2-4.2 Timing

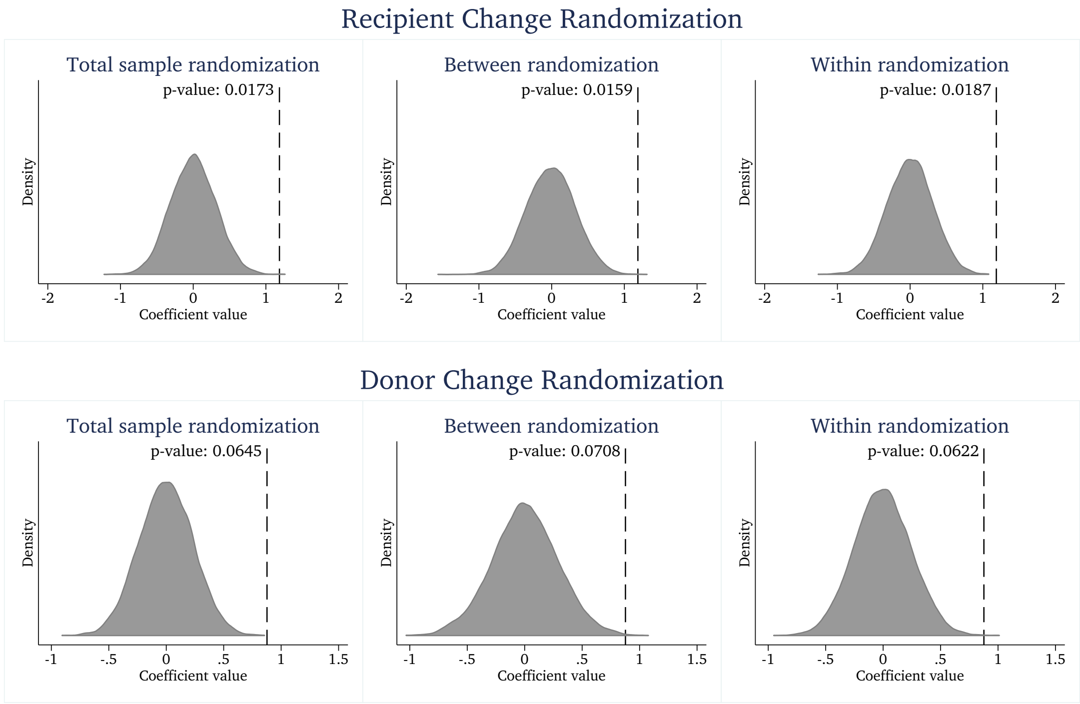
How lasting is the conditional alignment effect? If our argument were correct, future leader changes and their initial foreign policy shifts should not predict aid today. Nor should the initial foreign policy position taken by a new leader predict all future aid commitments. Instead, we would expect that the initial behavior becomes less relevant as soon as the donor-recipient pair gets a good estimate about how their relations actually are. To explore the time structure, we re-estimate the high dimensional fixed effect estimation (Table 2-2, column 4) using several leads and lags of our dependent variable.[21] Figure 2-2 plots the point estimates and their 90% confidence intervals of the recipient and donor leader interactions with UNGA voting changes.

Both interaction effects are not statistically different from zero before the year of the actual leader change t. This makes us confident, that it is indeed the initial foreign policy change of a new leader that has an effect on aid commitments rather than a general change in bilateral relations that is only accompanied by leader change. Likewise both interactions lose statistical significance two years after the respective leader change. Hence, the substantial effects are, as expected, rather short lived.

Nonetheless, it might still be the case that our results are driven by spurious correlation that covaries with the leader change interactions within the dyads over

[21]Because the median duration of the leader pair dyad is five years, we use two years prior to and after each leader change, in addition to the contemporaneous specification.

Figure 2-3 – Randomization of Leader Change



Notes: Distribution of point estimates for the interaction between recipient change and alignment change, based on Table 2-2, column 4. Each distribution corresponds to the different dependent-independent variable pairs, for the three different randomization procedures. Each distribution is constructed by repeating the randomization and estimation procedure 10000 times. The point coefficient of the actual estimation is depicted as a vertical line.

time. In order to test for this, we follow Hsiang and Jina (2014) and conduct a randomization test over all dimensions of our panel. More specifically we conduct three randomizations of our respective interaction terms on the basis of model 4 in Table 2-2: First, we randomize leader changes over the whole sample. Hence, a leader change in Kenya in 2000 can be assigned to Indonesia in 1990. Second, we randomize between dyads, thus keeping the time structure of the leader changes constant, which means that the entire leader change pattern of Kenya is, for example, assigned to Indonesia. This tests for spurious correlation arising from country or regional time trends, for example because the US closely monitors countries' voting behavior in the Middle East at the time of the wars in Iraq. Third, we randomize leader changes within each dyad, but not across dyads. Thus, leader changes in Kenya are shuffled around within Kenya. This randomization allows us to test if any unobserved dyadic-specific circumstances that vary over time drive the results, for instance conflicts over trade between countries that covary with the leader changes within dyads or covert operations between donors and recipients, such as CIA interventions (Berger et al., 2013). We expect that all randomization procedures produce a distribution of point coefficients centered around zero and that we should not reject the null more often than in our corresponding regression using the real data.

Figure 2-3 presents the kernel density function of the resulting point coefficients of the recipient leader and donor leader change interactions for each of the three randomization exercises, resulting from 10000 randomization iterations. The recipient change results are reported in the upper panel, while the donor change interaction results are plotted in the lower panel. The dotted line represents the obtained point coefficient from the actual data based on column 4 in Table 2-2. The reported Monte Carlo p-values report the fraction of t-statistics from the randomized data that exceed the absolute t-values for our coefficients of interest using the real data. In all cases, the estimated interaction terms using the real data exceed the obtained coefficient distributions obtained from the hypothetical scenarios. The p-values range between 0.0159 to 0.0187 for the recipient change interactions and 0.0622 and 0.0708 for the donor interactions. Thus they reaffirm the timing structure of our proposed mechanism. It is also further evidence that our results are not driven by any spurious correlation, either within or between panels. Hence, we are confident that it is indeed the leader change interaction that drives changes in ODA commitments between donors and recipients.

2-4.3 Scope

To evaluate the scope of the conditional alignment effect, we investigate how different institutional settings and types of leader transitions affect the alignment mechanism. We start by differentiating between legal and illegal leadership change. If donors care about the rule of law, they should oppose illegal power grabs by cutting financial support. We code illegal changes as irregular entries into office, for example via coups (Goemans et al., 2009). We do so only for recipient countries, as there are no illegal changes in the G7 countries in our sample. The results in Table 2-3 column 1 show a positive and statistically significant alignment change effect in both cases. Furthermore, a t-test fails to reject that the coefficients are equal.

In column 2, we interact our model with a proxy for political struggle, operationalized as years during which a country has had three or more heads of executive.[22] In such cases, the alignment change interaction becomes insignificant. This might point to the fact that donors are incapable of gaining enough information during very short executive tenures in recipient states. Thus, they are unable to figure out who they are dealing with and thus revert to their ‘standard’ aid allocation.

In column 3, we test whether domestic-support-group change in addition to leader change amplifies the effects from changes in voting alignment. Domestic-support-group changes follow the same logic as changes in the political orientation of the government (Potrafke, 2017). If the domestic support group changes, it is likely that different societal interest are primarily considered by the government.[23] Mattes et al. (2015) highlight that changes in the domestic support groups are the main driver of significant foreign policy re-alignment.[24] We adapt their specification

[22] About 1.6% of recipient change dyads fall under this classification.

[23] While domestic support group changes tell us little about the political orientation of the government, they tell us if switches in aggregated preferences occurred, thus highlighting our uncertainty argument. Another upside of domestic support group changes in comparison to ideology changes is that the latter are hard to grasp for a lot of recipient countries.

[24] According to Mattes et al. (2015) a change in support group concerns the societal foundation of the current leader’s rule. In democracies, for example, a leadership change that involves a change in the partisan composition of government is considered as a change in the support group. If leadership turnover occurs, but the new leader comes from the same party, no alteration of the societal foundation of a leader’s rule has taken place.

Table 2-3 – Scope of the Conditional Alignment Effect

	Dependent variable: *ln ODA commitments*				
			Mattes et al. 2015	*Dreher and Jensen 2013*	*Carter and Stone 2015*
	(1)	(2)	(3)	(4)	(5)
Last year alignment	0.745**	0.392	0.405	0.677**	0.659**
	(0.328)	(0.295)	(0.292)	(0.315)	(0.314)
Past mean alignment	1.104***	0.629**	0.568*	0.923***	0.889***
	(0.284)	(0.301)	(0.299)	(0.289)	(0.285)
	Legal change	*Without struggle*	*Support constant*	*During Cold War*	*Autocracy*
Recipient change	-0.092***	-0.091***	-0.129**	-0.129**	-0.104**
	(0.034)	(0.031)	(0.057)	(0.062)	(0.044)
Donor change	0.045*	0.040	0.090**	0.027	0.040
	(0.027)	(0.026)	(0.045)	(0.054)	(0.031)
Alignment change	0.039	-0.139	-0.100	-0.288	-0.033
	(0.220)	(0.206)	(0.199)	(0.356)	(0.220)
Recipient change * realignment	1.030**	1.277***	1.193*	1.328*	0.896*
	(0.445)	(0.393)	(0.675)	(0.712)	(0.535)
Donor change * realignment	1.023***	0.629**	0.449	0.761	1.148***
	(0.333)	(0.315)	(0.718)	(0.551)	(0.365)
	Illegal change	*Struggle year*	*Support change*	*After Cold War*	*Democracy*
Recipient change	-0.172**	-0.159	-0.067*	-0.077**	-0.095***
	(0.073)	(0.177)	(0.036)	(0.036)	(0.036)
Donor change			0.042	0.054*	0.056
			(0.037)	(0.032)	(0.045)
Alignment change				0.374	0.184
				(0.363)	(0.368)
Recipient change * realignment	1.196*	-1.772	1.595***	1.082**	1.702***
	(0.724)	(1.276)	(0.466)	(0.474)	(0.561)
Donor change * realignment			0.701*	1.241***	0.305
			(0.368)	(0.390)	(0.579)
Adjusted R-squared	0.045	0.031	0.034	0.054	0.054
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,Y	DR,Y
# of observations	16928	18571	18571	17477	17607
# of dyads	668	681	681	667	672

Notes: Column 1 includes GDP and population controls. Column 2 includes no additional controls. See Table 2-A6 for information on control variables in columns 3 to 5. Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

by including their core set of control variables in our dyadic setting (see Table 2-A6).[25] We find evidence in favor of our argument regardless of a simultaneous change in the support group of the leader – the interaction term is positive and statistically significant in both cases. At first glance the magnitude of the point estimate is higher in the case of domestic support group change. The t-test, however, indicates no difference between the coefficients. Thus, alterations in the conditions surrounding leader change do not seem to reduce the importance of first impressions.

We further differentiate between different eras as well as institutional settings (see Table 2-A3). In column 4, we adopt the Dreher and Jensen (2013) specification

[25] To reduce clutter, we do not report the coefficients of the additional control variables. They are, however, in line with the findings of previous research.

and differentiate between the Cold War and post-Cold War period,[26] but use all votes in the UNGA instead of focusing on key votes alone. In column 5, we subdivide the sample into democracies and autocracies. Carter and Stone (2015) have shown that donors prefer to provide political side payments to fellow democracies, since their own constituencies are more skeptical of financial support to autocracies compared to democracies. The interaction terms between leader change and changes in political proximity show the expected results, but reveal interesting variation in terms of effect size and statistical significance. For example, the donor change interaction is only significant in the post-Cold War period and for autocratic recipient countries. The recipient interactions are however not statistically different from one another between time periods. Interestingly, the point estimate of the interaction effect is about twice as large for democratically elected leaders. The presence of the interaction effects for both democratic and autocratic countries increases our confidence that we have not simply picked up lagged election effects (Faye and Niehaus, 2012), since many of the autocratic countries in our sample do not hold competitive elections. Again, there is no difference between the interactions of recipient leader change and alignment changes between autocracies and democracies. We attribute this in part to an imprecise estimate in the autocratic setting, driven by relatively few leader changes.

2-5 Robustness Tests

In this section we further probe the robustness of our findings. We check for problems of endogeneity and conclude that our results do not seem to be driven by reverse causality. We rule out selection effects on the dependent variable and use alternative measures of foreign policy alignment to demonstrate that our results are not subject to specific coding decisions. Finally, we show that the results are not driven by the allocation decisions of single donors.

2-5.1 Reverse Causality

Studies point to the fact that donors engage in vote buying (Dreher and Sturm, 2012; Carter and Stone, 2015), intervene in or influence elections in recipient countries (Faye and Niehaus, 2012), or use other means to oust unfavorable political leaders and regimes in order to achieve political and commercial objectives.[27] Hence, political convergence (or divergence) between a recipient and donor may depend on commitments (or threats) made by donors prior to leader turnover in a recipient state. The same problem applies to leader turnover in donor countries. A new US president may alter aid commitments made to recipients directly after inauguration, thus driving recipients to change their alignment strategies.

To tackle this issue we utilize an instrumental variables framework. Ideally we would instrument donor and recipient leader change as well as foreign policy alignment. Unfortunately, we lack instruments for foreign policy alignment and can only instrument leader changes. Bun and Harrison (2018), however, show that the interaction term between an exogenous and an endogenous variable is itself

[26] Voeten (2000) has shown that voting blocks are less stable after the end of the Cold War.

[27] Berger et al. (2013) provide a comprehensive list of United States' CIA interventions into the domestic politics of developing countries during the Cold War.

exogenous as long as there is no contemporaneous reverse causality, anticipation effects and the degree of endogeneity of the endogenous variables does not depend on the values of the exogenous one.

We follow Annen and Strickland (2017) and instrument donor leader changes with regular (executive and legislative) elections in donor countries. In addition, we include presidential term limits.[28] We instrument recipient leader changes using natural deaths of executive leaders (Jones and Olken, 2005) as well as legislative and executive elections.[29] The election data are taken from the National Elections Across Democracy and Autocracy (NELDA) database (Hyde et al., 2012).[30] Note that we only include 'regular' elections, which are elections that occur at their scheduled date and not elections that have been postponed or held after regular elections have been tempered with.[31]

Our identifying assumption is that none of these variables affects ODA commitments besides their effect via actual leader change and the foreign policy alignment that occurs in tandem. While this assumption is rather straight-forward in case of term limits, natural deaths, and election dates in donor countries, it could be more problematic for recipient countries. For one, Faye and Niehaus (2012) show that donors increase aid commitments to friendly regimes during election years, while they reduce aid to hostile regimes. Yet, their mechanism is conditional on alignment, for which we control. Hence, the conditional independence assumption should hold as long as we control for lagged alignment. We are also confident that the potential endogeneity in alignment should not depend on the values of our instrumented leader changes. Leader changes due to natural death should for example not affect the degree of potential endogeneity between alignment changes and ODA commitments within a given donor-recipient dyad.

Table 2-4 presents the four first stages of our 2SLS specification. Note that our instruments perform better in predicting donor leader change than recipient leader change, as shown by the adjusted R-squared in Table 2-4. This is not surprising, since elections in many recipient countries are not as competitive as in donor countries. Hence, they have less power in predicting leader change. Moreover, we cannot include donor and recipient year fixed effects since our instruments vary only by donor and recipient year.

Table 2-5 presents the second stage results of our instrumental variables approach. We report both 2SLS and control function results. Using regular 2SLS in column 1, we find that the donor-change interaction is positive and

[28]Term limits are only available for the United States. France introduced presidential term limits in 2008, but they have no predictive power for leader change in our sample that runs only until 2012.

[29]We depart from previous studies that exclusively focus on natural leader deaths. Despite the fact that such instances constitute exogenous variation, it is likely that a deceased leader's predecessor comes from the same party platform, was personally close to the former leader, and thus has less incentives to alter foreign policy dramatically. Hence, especially in cases of leadership turnover that occurs after leader death, our mechanism is least likely to manifest. On top of that, no donor leader has died a natural death in office within our sample. Hence, we cannot use natural deaths as an instrument for donor leader change.

[30]For detailed information on the data see Hyde et al. (2012) and the original application in Annen and Strickland (2017).

[31]Since we always code the leader with the most days in office during a year as the current leader, we lead elections occurring after July 1 by one year. By definition a new leader would not be coded for the current year and the change would occur in the following year.

Table 2-4 – Instrumental Variables: First Stages

	Dependent variables: *Leader Changes*			
	(1) *Recipient change*	(2) *Donor Change*	(3) *Recipient change *alignment*	(4) *Donor change *alignment*
Alignment change	-0.105**	0.281***	0.125***	0.192***
	(0.052)	(0.050)	(0.011)	(0.015)
Last year alignment	-0.284***	0.145**	0.067***	0.128***
	(0.068)	(0.058)	(0.009)	(0.013)
Past mean alignment	0.261***	0.257***	-0.088***	-0.148***
	(0.068)	(0.058)	(0.009)	(0.011)
Instruments				
Natural death of recipient leader	0.933***	0.020	0.001	-0.002
	(0.009)	(0.030)	(0.001)	(0.004)
Executive election (Recipient)	0.204***	0.036**	0.002	0.001
	(0.027)	(0.015)	(0.002)	(0.002)
Legislative election (Recipient)	0.018	0.022**	-0.001*	-0.001
	(0.015)	(0.011)	(0.001)	(0.001)
Leader term limit (Donor)	-0.019	1.194***	0.004*	-0.009***
	(0.026)	(0.009)	(0.002)	(0.001)
Executive election (Donor)	-0.006	0.048**	0.001	0.008***
	(0.012)	(0.020)	(0.001)	(0.001)
Legislative election (Donor)	0.009	0.172***	-0.000	-0.007***
	(0.007)	(0.007)	(0.000)	(0.001)
*Instruments*alignment change*				
Natural death of recipient leader	0.180**	-0.518*	0.869***	0.030
	(0.080)	(0.279)	(0.014)	(0.063)
Executive election (Recipient)	0.504**	0.131	0.084**	0.114
	(0.238)	(0.231)	(0.042)	(0.081)
Legislative election (Recipient)	-0.319***	-0.120	0.005	-0.021
	(0.121)	(0.144)	(0.021)	(0.031)
Leader term limit (Donor)	-0.087	0.633***	0.019	0.764***
	(0.239)	(0.105)	(0.040)	(0.019)
Executive election (Donor)	-0.055	2.194***	-0.007	0.000
	(0.142)	(0.170)	(0.025)	(0.050)
Legislative election (Donor)	0.034	-1.613***	0.023	0.213***
	(0.074)	(0.100)	(0.015)	(0.027)
Adjusted R-squared	0.093	0.320	0.228	0.427
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,Y
# of observations	15581	15581	15581	15581
# of dyads	668	668	668	668

Notes: Each column represents one of the first stages of model 1 Table 2-5. The Kleibergen-Paap F-stats over the four first stages are reported in Table 2-5. All specifications include GDP and population controls. Fixed effects: donor-recipient (DR), year (Y). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

statistically significant. It increases in size compared to the original effect (see column 3 in Table 2-2). The interaction between recipient country leader change and foreign policy alignment is not statistically significant. Note, however, that the recipient leader change interaction is estimated very imprecisely, and the interacted instruments do not really add exogenous variation (see the first stage results).[32]

Table 2-5 – Instrumental Variables: Second Stages

	Dependent variable: *ln ODA commitments*		
	(1)	(2)	(3)
	2SLS	*Control Function*	*Control Function*
Recipient change	-0.224*	-0.232*	-0.225*
	(0.116)	(0.120)	(0.119)
Donor change	0.032	0.013	0.027
	(0.054)	(0.053)	(0.054)
Alignment change	-0.064	-0.101	-0.282
	(0.398)	(0.244)	(0.241)
Recipient change * realignment	-0.291	1.423***	1.402***
	(1.787)	(0.423)	(0.418)
Donor change * realignment	1.840**	1.065***	0.927***
	(0.900)	(0.344)	(0.333)
Last year alignment	0.613	0.601*	0.234
	(0.376)	(0.344)	(0.345)
Past mean alignment	1.226***	1.263***	0.889**
	(0.354)	(0.311)	(0.312)
Control function Residuals			
Recipient change (residual)		0.155	0.142
		(0.125)	(0.124)
Donor change (residual)		0.042	0.040
		(0.062)	(0.064)
Within R-squared	0.047	0.050	0.039
Fixed Effects	DR,Y	DR,Y	DR,Y
F-stat IV (Kleibergen-Paap)	237.8	237.8	209.1
Obs	15576	15581	15581
Dyads	663	668	668

Notes: Columns 1 and 2 include GDP and population controls. Fixed effects: donor-recipient (DR), year (Y). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Since our instrument interactions do not add exogenous variation on the first stage, we focus on a control function approach, which increases efficiency, given mild assumptions (Wooldridge, 2010, 2015). Control functions do not need the residuals of our interaction instruments in order to produce consistent estimators. An obvious problem would however occur if our instrumented leader changes predict alignment change. In such a case it seems unlikely that the level residual can capture the endogeneity of leader changes in the levels as well as in the interactions. Column 1 of (Table 2-A7) shows that neither of our instrumented leader changes

[32]Nonetheless, the Hansen J-test of over-identification is rejected with a test statistic of 12.964 (p-value 0.1131).

predicts alignment changes. We are thus confident that the conditional independence assumption holds for the interaction as well.[33] Column 2 reports the control function estimates, where the standard errors are obtained from 999 bootstraps.[34] In this case both the donor and recipient interactions are positive, statistically significant, and comparable in size to the previous results. In addition, we follow Angrist and Pischke (2008) and exclude our control variables from the control function, since neither GDP nor population should add to the conditional independence between our instruments and leader change (see Table 2-5, column 3). Again, the results support our argument. All in all, it is not surprising that the obtained LATE does not differ much from the original results since donors do not seem to care too much about the circumstances surrounding recipient leader changes.

Lastly, because the identification of our interaction variables rests on the absence of anticipation effects of the alignment change (Bun and Harrison, 2018), we reestimate our core models with alignment change as the dependent variable and use lagged ODA commitments as well as interactions of leader change with lagged ODA commitments as independent variables (see columns 2 to 3 in Table 2-A7). We obtain a small level-coefficient of lagged ODA commitments on the alignment change, no effect for the recipient interaction with lagged ODA, and a small effect of the interaction between donor change and lagged ODA, which is consistent with the findings of Annen and Strickland (2017). Note however that none of these interaction effects is statistically significant if we include donor-year and recipient-year fixed effects.

2-5.2 Selection on the Dependent Variable

Due to the log-transformation, the results presented so far relate only to recipient countries that have already received aid from a donor. To rule out selection effects, we thus include donor-recipient pairs without previous aid flows, allowing us to test whether leader change can lead to the establishment of new development cooperation between a developing and a G7 country or to the complete abandonment of it, respectively.

Ideally we would run a proper two-stage model, but unfortunately we lack an instrument for the selection equation. Hence, we estimate an onset specification, in which the dependent variable is a dummy that is 1 if a country receives a positive amount of ODA commitments and zero otherwise (see Table 2-6, column 1). The sample consists only of donor-recipient dyads where there have been no ODA commitments in the last period. Concerning our variables of interest, only donor leader change has a statistically significant effect on the establishment of development cooperation with recipient countries if voting alignment stays constant. Most importantly, the interaction terms are not statistically significant. Political convergence after leadership turnover does lead to ODA commitments if they have

[33] Note that the non-findings have also implications for potential endogeneity in the alignment change variable. If we assume that our instrumented leader changes are indeed exogenous with respect to ODA commitments, and as Table 2-A7 shows do not predict alignment changes, then the endogeneity of alignment should not depend on the value of the instrumented leader changes, which is a necessary condition to identify the interaction following Bun and Harrison (2018).

[34] If we include the residuals of the interaction terms, which is not necessary in a control function, we obtain the same coefficients as in column 1.

Table 2-6 – ODA Selection and Zero ODA Commitments

	Dependent variable:			
	ODA onset	*ODA cont.*	*ln ODA commitments*	
	(1)	(2)	(3)	(4)
Recipient change	0.010	0.003	-0.049**	
	(0.015)	(0.003)	(0.020)	
Donor change	0.023**	0.001	0.029*	
	(0.011)	(0.004)	(0.017)	
Alignment change	0.061	0.031	0.313**	0.193
	(0.083)	(0.028)	(0.145)	(0.206)
Recipient change * realignment	-0.278	0.052	0.500*	0.788**
	(0.191)	(0.055)	(0.275)	(0.312)
Donor change * realignment	0.064	-0.002	0.356	0.184
	(0.114)	(0.044)	(0.220)	(0.249)
Last year alignment	-0.137	0.058*	0.727***	0.676**
	(0.107)	(0.034)	(0.211)	(0.339)
Past mean alignment	0.110	-0.000	0.681***	0.427*
	(0.111)	(0.033)	(0.187)	(0.249)
Donor GDP (ln)	0.116*	-0.009	1.358***	
	(0.065)	(0.025)	(0.426)	
Donor population (ln)	0.189	0.181***	1.338*	
	(0.224)	(0.060)	(0.697)	
Recipient GDP (ln)	-0.026	-0.011	-0.094	
	(0.021)	(0.009)	(0.085)	
Recipient population (ln)	-0.029	-0.011	0.179	
	(0.045)	(0.020)	(0.200)	
Adjusted R-squared	0.037	0.013	0.053	0.843
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,RY,DY
# of observations	4745	16938	21683	24176
# of dyads	426	673	745	768

Notes: Leader change variables in column 4 omitted due to fixed effects. Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

been zero in the past period.[35] We further test whether aid is cut completely between a donor and a recipient induced by alignment change after leader turnover (see Table 2-5, column 2). In this specification, none of the core variables is statistically significant. Hence, we conclude that the voting alignment mechanism after leadership change has no effect on the extensive margin of ODA commitments between donors and recipients.

Although we find no selection effects, we replicate columns 3 and 4 from Table 2-2 including zero ODA commitments (see Table 2-6, columns 3 and 4).[36] The main results support our argument. Nevertheless, the substantive as well as statistical

[35] We also tested if our mechanism induced any development cooperation in cases where a donor has never given aid to a recipient in the past. We do not find any effect on the interactions, but a small effect (0.067) on the unconditional recipient change indicator. The unconditional alignment change is also statistically insignificant in those cases.

[36] In order to log-transform this variable, we add $1 to each observation.

significance decreases compared to the results in Table 2-2. This is however not surprising. If the interaction of leader change and the political alignment does not have an effect on the extensive margin, including zeros biases the results for the intensive margins downward. Thus, foreign policy realignment is only consequential for recipients that already have established development cooperation.

2-5.3 Alignment, Leader Change and ODA Measures

In a next step, we test if our results are driven by the measurement of foreign policy realignment. First, we employ regular votes instead of all votes. This measure is based on recurring votes and therefore not dependent on the yearly fluctuations of the UNGA voting agenda (Bailey et al., 2017; Häge and Hug, 2016). Second, we focus only on key votes – votes deemed important by the US State Department – to test if recipients and donors act differently to issues considered as strategically important by the United States (Kersting and Kilby, 2016).[37] Third, we test if our results are driven by extreme shifts in foreign policy and run a trimmed least squares regression dropping the bottom and top 5% of the voting change observations. Lastly, we include vote abstentions into the UNGA voting alignment counting abstentions .5 (Barro and Lee, 2005).

The results largely support the robustness of the previous findings (see Table 2-7). The interaction between recipient leader change and the change in voting alignment is positive and statistically significant in all but one model. Only in case of key votes is the coefficient not statistically significant. At first sight this might seem puzzling. Yet, key votes are based on votes deemed important by the United States and might therefore always carry consequences, as suggested by the alignment change coefficient. The donor interaction effect in turn might be driven by the fact that other G7 leaders follow the US to different degrees.[38] Furthermore, key votes often cluster around certain events, like the Iraq War. Recipient country leaders might come to power and, simply by chance, not be able to signal alignment via key votes.[39] The interaction between donor leader change and the foreign policy alignment change is positive and statistically significant as long as we do not count abstentions. All in all, we find our results not to be driven by strong changes in voting alignment and robust to the different measures of UNGA voting alignment.

We also put the second part of our interactions under further scrutiny. So far we assigned a leader to a country year if he or she holds the majority of days in office during that year. While we believe this choice to be the most appropriate it is by no means the only justifiable approach. Mattes et al. (2015) for example make the argument that the leader in power during the last three months of a year is most likely to influence UNGA alignment, since most of the votes are cast during that

[37] Note that key votes are only available after 1984.

[38] Note that this is not a sample effect; we replicated our base specification Table 2-2 column 3 on the reduced key-vote sample – 1984 onwards – and obtain stable results.

[39] Since key votes are solely determined by the United States, we rerun column 2 using only the United States as a donor and utilize a simple time trend instead of the year fixed effects. Note that year fixed effects would absorb the US leader changes int his setting. In this case both interactions lose their statistical significance while yearly alignment changes enter significant (results not reported). This is not too surprising, since Carter and Stone (2015) have shown that the USA uses aid to influence voting behavior on key votes, thus introducing problems of endogeneity.

Table 2-7 – Alternative Alignment Change Specifications

	Dependent variable: *ln ODA commitments*				
	Regular votes	*Key votes*	*TLS 10%*	*Vote abstentions*	
	(1)	(2)	(3)	(4)	(5)
Recipient change	-0.091***	-0.070**	-0.094***	-0.101***	
	(0.032)	(0.034)	(0.033)	(0.032)	
Donor change	0.048*	0.068**	0.058**	0.046*	
	(0.027)	(0.027)	(0.027)	(0.027)	
Alignment change	0.320	0.667***	-0.142	-0.058	0.058
	(0.200)	(0.159)	(0.305)	(0.294)	(0.460)
Recipient change * realignment	1.208***	-0.103	1.217**	1.646***	1.283*
	(0.399)	(0.143)	(0.616)	(0.539)	(0.681)
Donor change * realignment	0.943***	0.289*	2.970***	0.972**	0.471
	(0.334)	(0.159)	(0.522)	(0.457)	(0.629)
Last year alignment	1.341***	1.179***	0.928**	0.595	0.984
	(0.309)	(0.159)	(0.394)	(0.443)	(0.784)
Past mean alignment	0.970**	-0.116	0.987***	0.882**	0.699
	(0.390)	(0.178)	(0.328)	(0.387)	(0.614)
Log GDP recipient	-0.131	0.051	-0.071	-0.137	
	(0.132)	(0.135)	(0.142)	(0.133)	
Log GDP donor	2.225***	1.769***	2.479***	2.309***	
	(0.647)	(0.677)	(0.613)	(0.652)	
Log population recipient	0.799**	0.589	0.842**	0.804**	
	(0.341)	(0.364)	(0.351)	(0.343)	
Log population donor	-0.074	3.186**	0.299	-0.109	
	(1.018)	(1.277)	(1.020)	(1.028)	
Adjusted R-squared	0.044	0.054	0.048	0.041	0.786
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,Y	DR,RY,DY
# of observations	16900	13495	15315	16928	18571
# of dyads	662	661	668	668	681

Notes: Regular votes (reoccurring votes) in column 1. Key votes in column 2. Top and bottom 5% of realignment excluded in columns 3. Alignment change includes vote abstentions in columns 4 and 5. Leader change variables in column 5 omitted due to fixed effects. Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

period (roughly 90% of the votes).[40] Thus we recode our leaders in office in several ways. First we exclusively consider leaders in power for the majorities of days during December. Second we focus on leaders in power during November and December. Third we only take leaders into account during the last three months of any year. Last we ignore all but the first or last leader during the last quarter. The correlation between the resulting recipient leader changes and our definition are between 0.60 and 0.76.[41] For the donor leader changes the correlations are between 0.64 and 0.86 respectively.[42] The results of the specifications using this alternative assignment of leader change are presented in Table 2-8.[43]

[40] They highlight that the majority of votes between 1946 and 2008 occur during December (around 75%), followed by November (roughly 15%) and October (approximately 4%).

[41] Specifically, 0.6032 for the December definition, 0.6389 for the November and December definition, 0.7191 for the last quarter definition, 0.7588 for the first leader during the last quarter and 0.7221 for the last leader during the last quarter.

[42] Correlation following the same definitions as in the recipient change example are 0.6401, 0.6975, 0.7976, 0.8559 and 0.7976.

[43] Note that we recoded the past mean alignment variable for each case, since the duration of administration pairs changes as soon as we redefine the leaders which are in power during a specific

Table 2-8 – Leader Change Definitions

	Dependent variable: *ln ODA commitments*				
		Majority in Office		*Last Leader*	*Newest Leader*
	December	*Nov.and Dec.*	*Last Quarter*	*Last Quarter*	*Last Quarter*
	(1)	(2)	(3)	(4)	(5)
Recipient change	-0.061*	-0.060*	-0.096***	-0.097***	-0.091***
	(0.035)	(0.033)	(0.032)	(0.032)	(0.032)
Donor change	0.004	0.022	0.037	0.072**	0.037
	(0.026)	(0.025)	(0.027)	(0.028)	(0.027)
Alignment change	0.573***	0.544***	0.499**	0.461**	0.487**
	(0.210)	(0.208)	(0.208)	(0.209)	(0.209)
Recipient change * realignment	0.453	0.894**	0.864**	0.974**	0.975**
	(0.364)	(0.391)	(0.391)	(0.405)	(0.385)
Donor change * realignment	0.411	0.285	0.486	0.578*	0.478
	(0.326)	(0.320)	(0.323)	(0.326)	(0.323)
Last year alignment	1.627***	1.627***	1.630***	1.624***	1.629***
	(0.317)	(0.318)	(0.318)	(0.318)	(0.318)
Past mean alignment	-0.470	0.196	0.248	-0.150	0.039
	(0.409)	(0.356)	(0.386)	(0.399)	(0.368)
Adjusted R-squared	0.043	0.043	0.044	0.044	0.044
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,Y	DR,Y
# of observations	16928	16928	16928	16928	16928
# of dyads	668	668	668	668	668

Notes: All specifications include GDP and population controls. Fixed effects: donor-recipient (DR), year (Y). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Columns 1 to 3 of Table 2-8 show that the obtained effects decrease in both magnitude and statistical significance compared to column 3 of Table 2-2 which is the corresponding specification using the majority in office definition (during the whole year rather then the specific month) to identify leader changes. In fact the interaction results vanish completely if we focus solely on the leader in power during December. If we consider only the first or last leader during the last quarter, results are similar to column 3. The donor change interaction in turn gains only statistical significance if we use the first leader of the last quarter definition. There are two likely explanations for these results. First it might be that new administrations that come in during the last two months of a year are not able to communicate their foreign policy preferences sufficiently during the year in question. The second explanation is that UNGA voting alignment works more in the way of revealed preferences. Hence alignment changes that coincide with leader changes at the end of the year are less consequential, since they proxy insufficiently for the relations during that year. That the effect reconstitutes itself as soon as we move closer to the majority of days in office definition for relevant leaders increases our confidence in the proxy character of the alignment measure. In fact the correlations between the first leader during the last quarter and our preferred leader assignment have the highest correlation (75.88% and 85.59%).

Next, we check if our results are only driven by changing ODA commitments decisions or if they also hold for actual ODA disbursements. Thus we replicate Table 2-2 using net ODA disbursements instead of ODA commitments. Table 2-9 shows that our obtained effects largely hold for aid disbursements as well, although the point coefficients become smaller and have reduced statistical significance. Note that the donor leader change interaction loses its statistical significance in the high

year.

Table 2-9 – Net ODA disbursements

	Dependent variable: *Log net ODA disbursements*			
	(1)	(2)	(3)	(4)
Recipient change	-0.070**		-0.066**	
	(0.029)		(0.029)	
Donor change	0.065***		0.073***	
	(0.022)		(0.023)	
Alignment change		0.389**	0.077	0.468
		(0.188)	(0.207)	(0.326)
Recipient change * realignment			0.798**	0.785*
			(0.389)	(0.445)
Donor change * realignment			0.701**	0.153
			(0.308)	(0.469)
Last year alignment	0.675***	1.000***	0.835***	1.246**
	(0.200)	(0.303)	(0.305)	(0.528)
Past mean alignment	0.671**	0.488*	0.663**	0.692
	(0.262)	(0.273)	(0.291)	(0.430)
Adjusted R-squared	0.052	0.051	0.052	0.811
Fixed Effects	DR,Y	DR,Y	DR,Y	DR,RY,DY
# of observations	15853	15853	15853	17218
# of dyads	661	661	661	670

Notes: Leader change variables in column 4 are omitted due to fixed effects. Columns 1 to 3 include GDP and population controls for donors and recipients. Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

dimensional fixed effects model in column 4.

2-5.4 Differences between Donors and Recipients

There is ample evidence that donors differ in the way they commit and disburse aid (Alesina and Dollar, 2000; Dietrich, 2016). The United States is famous for using aid to achieve geo-strategic goals, while France focuses prominently on former colonies. Closely related to this is the question whether changes in the aid commitments of individual donors are due to the changes in the average alignment with the G7 in general or if the results are truly driven by the dyad-specific changes in political proximity. We test the two issues jointly by including the average change in voting alignment with the G7 as an additional control and fully interacting our baseline model for the different donors (see Table 2-10).[44]

Regarding the interaction between recipient change and foreign policy alignment, we find that Canada, Germany, Great Britain, and the United States are the main drivers behind the reward and punishment mechanism following recipient leader change.[45] In case of alignment changes after donor leader change, we find statistically

[44]Note that we keep the time dummies separate, since they would overload the specification and absorb the donor change variable. Hence they only control for global shocks concerning all donors and recipients.

[45]This is surprising since both the United Kingdom and the United States have been shown to have a tendency to bypass aid in the first place (Dietrich, 2016), which should make them less

Table 2-10 – Differences between Donor Countries

	Dependent variable: *ln ODA commitments*						
	CAN	*FRA*	*GER*	*GBR*	*ITA*	*JAP*	*USA*
Recipient change	-0.098	0.023	0.020	0.179**	0.281**	0.060	0.076
	(0.072)	(0.076)	(0.020)	(0.077)	(0.125)	(0.122)	(0.063)
Donor change	-0.066	0.052	-0.094	-0.150**	-0.058	-0.129*	-0.010
	(0.070)	(0.074)	(0.106)	(0.076)	(0.063)	(0.077)	(0.057)
Alignment change	-2.008***	0.837	0.907	1.253***	-1.229	0.514	0.273
	(0.725)	(0.525)	(0.684)	(0.480)	(1.193)	(1.512)	(0.437)
Recipient change * realignment	2.258*	1.412	2.020***	2.546***	-3.395	1.291	1.238*
	(1.192)	(0.988)	(0.642)	(0.742)	(2.390)	(1.951)	(0.713)
Donor change * realignment	2.812***	0.310	3.487**	1.564*	1.385	3.742*	0.737
	(0.985)	(0.798)	(1.775)	(0.865)	(1.251)	(2.045)	(0.624)
Average G7 realignment	1.119	-0.489	-0.954	-1.148**	2.625***	-0.595	-0.332
	(0.724)	(0.599)	(0.698)	(0.531)	(0.851)	(0.974)	(0.307)
Last year alignment	0.639	1.338**	1.644**	1.548**	0.842	-0.234	0.901*
	(0.735)	(0.569)	(0.731)	(0.738)	(1.315)	(1.374)	(0.510)
Past mean alignment	2.302***	3.224***	2.333**	3.147***	-4.239***	8.659***	0.563
	(0.689)	(0.669)	(0.974)	(0.870)	(1.486)	(2.802)	(0.430)
Adjusted R-squared				0.085			
Fixed Effects				DR,Y			
# of observations				16900			
# of dyads				662			

Notes: GDP and population of donor and recipient countries are not reported. Fixed effects: donor-recipient (DR), year (Y). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

significant results for Canada, Germany, Great Britain, and Japan, while the rest of the G7 donors seem to exhibit no such behavior. France does not react to realignment after leader change, which is consistent with France's focus on former colonies (Alesina and Dollar, 2000). Despite not reacting to conditional signaling, Italy nevertheless goes along with the rest of the G7; the effect of average G7 realignment is positive and statistically significant. Although we do not find the same effects for every donor, we also do not find evidence against our theoretical argument. None of the interaction terms are negative and statistically significant. Rather, the results emphasize that different donors seem to vary with regards to the importance they place on realignment after leadership turnover. Most importantly, the results are not driven by a single donor.

To further check if single recipients drive our results, we perform leave-one-out tests. Here, we rerun our specification from column 4 in Table 2-2 excluding every recipient country once at a time. The point estimates of the recipient change-alignment interaction are plotted in Figure 2-A1 in the Appendix. All effects are positive and statistically significant. From this we can conclude that no single donor has enough leverage to drive our main finding. Additionally, Figure 2-A2 plots the corresponding donor change-alignment interaction. Apart from two exceptions, the results remain stable.

Summing up, our results are robust to a variety of specifications. We show that reverse causality, selection effects, and the measurement of political alignment do not conflate our results in a substantive way. Donor countries reward recipients with higher ODA commitments, if they come closer to their own position on internationally relevant and important policies. To the contrary, recipients that show political animosity after leader change are confronted with substantial aid cuts.

2-6 Conclusion

In this study, we analyze a new mechanism through which the G7 donors induce political aid cycles in recipient countries. We argue that donors place higher scrutiny on recipients' behavior in the UNGA after both donor and recipient leader change. In the aftermath of leadership turnover, otherwise inconsequential yearly fluctuations in voting alignment between recipients and donors lead to substantial effects on aid commitments.

We find that new recipient leaders that converge to a donor during their first year in office receive substantially more aid commitments compared to those that diverge from positions that donors take in the UNGA. We consistently find this conditional alignment effect in case of both recipient and donor leader change. The substantial size of the effect differs, however. While new recipient leaders mainly face the prospect of sizable cutbacks in case they dis-align from a donor, stronger alignment towards a new donor leader is seemingly an important strategy to increase ODA commitments. For the bulk of the alignment changes following leader change (around 78%) cutbacks range between 9.7% and 19.6% for dis-aligning new recipient leaders and amount to between 3.8% and 9.1% in increases for recipients that align themselves with a new donor leader.

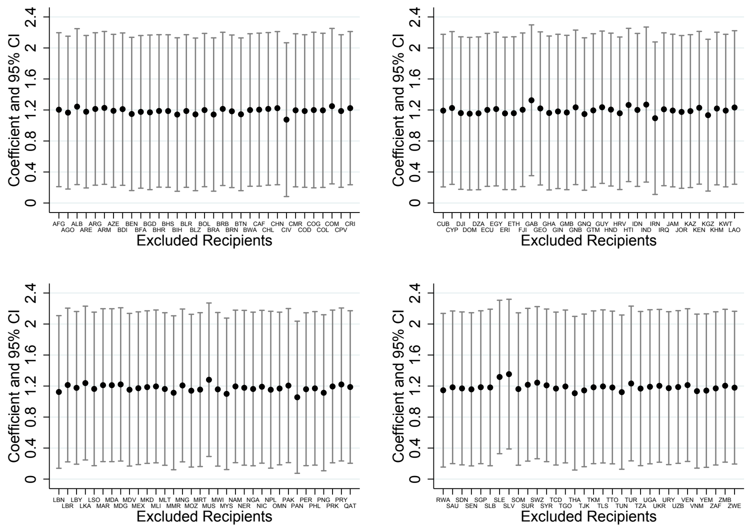
responsive to our proposed mechanism.

Moreover, aid increases after leader change are only short term as the alignment effect vanishes two years after leader change. Hence, initial changes in foreign policy of a new recipient leader – the first impression – determine the bilateral aid provision that a recipient country will receive from its donors only in the short term. We conclude that new recipient leaders must warily consider their first appearance on the international stage at the beginning of their incumbency. As their donors put their foreign policy positions under increased scrutiny, usually inconsequential changes in foreign policy result in sizable alterations of their aid commitments.

We provide evidence of an important mechanism explaining the volatility of development aid, beyond the effect of elections (Faye and Niehaus, 2012) or political importance due to temporary membership in the UNSC (Kuziemko and Werker, 2006). Politically motivated aid has been shown to be less effective in promoting growth (Dreher et al., 2018) and politically committed aid increases aid volatility that induces a heightened risk of civil conflict (Nielsen et al., 2011). Our results thus highlight that more scrutiny is required to dis-entangle development aid from politically motivated side payments that may have detrimental effects for developing countries.

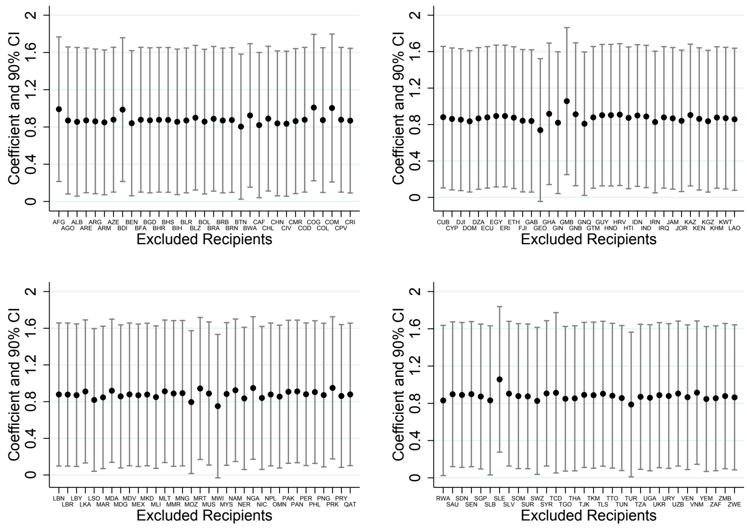
Appendix A:

Figure 2-A1 – Leave-One-Out Test for Recipient Change Interaction



Note: Reported are point coefficients of the interaction between recipient change and alignment change and the corresponding 95% confidence intervals, based on column 4 in Table 2-2.

Figure 2-A2 – Leave-One-Out Test for Donor Change Interaction



Note: Reported are point coefficients of the interaction between donor change and alignment change and the corresponding 90% confidence intervals, based on column 4 in Table 2-2.

Table 2-A1 – List of Recipient Countries, in Alphabetical Order

Afghanistan, Albania, Algeria, Angola, Argentina, Armenia, Azerbaijan, Bahamas, Bahrain, Bangladesh, Barbados, Belarus, Belize, Benin, Bhutan, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Brunei Darussalam, Burkina Faso, Burundi, Cambodia, Cameroon, Central African Republic, Chad, Chile, China, Colombia, Comoros, Congo-Brazzaville, Costa Rica, Croatia, Cuba, Cyprus, Djibouti, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Ethiopia, Fiji, Gabon, Gambia, Georgia, Ghana, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, India, Indonesia, Iran, Iraq, Ivory Coast, Jamaica, Jordan, Kazakhstan, Kenya, Korea (North), Kuwait, Kyrgyzstan, Laos, Lebanon, Lesotho, Liberia, Libya, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nepal, Nicaragua, Niger, Nigeria, Oman, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Qatar, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, Solomon Islands, Somalia, South Africa, Sri Lanka, Sudan, Suriname, Swaziland, Syria, Tajikistan, Tanzania, Thailand, Timor-Leste, Togo, Trinidad and Tobago, Tunisia, Turkey, Turkmenistan, Uganda, Ukraine, United Arab Emirates, Uruguay, Uzbekistan, Venezuela, Vietnam, Yemen, Zambia, Zimbabwe.

Table 2-A2 – Descriptive Statistics

	N	Min	Mean	Max	SD
ODA commitments	18,571	0.01	67.49	19,721.40	251.32
ODA commitments (Log)	18,571	-4.61	2.18	9.89	2.37
Administration dyads	18,571	1.00	–	7,507	–
Administration change	18,571	0.00	0.27	1.00	0.44
Recipient change	18,571	0.00	0.13	1.00	0.34
Donor change	18,571	0.00	0.20	1.00	0.40
Alignment change	18,571	-0.94	-0.00	0.67	0.08
Voting alignment	18,571	0.00	0.62	1.00	0.23
Past mean voting alignment	18,571	0.00	0.62	1.00	0.22
Administration dyad duration	18,571	1.00	5.93	16.00	3.65
Donor GDP (log)	17,401	20.09	21.45	23.30	0.81
Recipient GDP (log)	16,928	11.51	16.72	22.97	1.82
Donor population (log)	17,401	10.08	11.21	12.65	0.71
Recipient population (log)	17,095	4.95	9.06	14.10	1.70
Similarity Index (GDP)	16,928	0.00	0.06	0.50	0.11
Similarity Index (Population)	17,095	0.00	0.21	0.50	0.16
(Donor) Imports in million USD (Log)	18,571	-13.82	-2.88	13.00	8.99
(Recipients) Imports in million USD (Log)	18,571	-13.82	-5.58	12.13	9.42

Table 2-A3 – Variables and Sources

Variable	Source
ODA commitments	OECD (2015)
ODA commitments (Log)	OECD (2015)
Administration dyads	Archigos (Goemans et al., 2009)
Administration change	Archigos (Goemans et al., 2009)
Recipient change	Archigos (Goemans et al., 2009)
Donor change	Archigos (Goemans et al., 2009)
Administration dyad duration	Archigos (Goemans et al., 2009)
Alignment change	Voeten et al. (2017)
Voting alignment	Voeten et al. (2017)
Past mean voting alignment	Voeten et al. (2017)
Donor GDP (log)	PWT 7.1 (Heston et al., 2012)
Recipient GDP (log)	PWT 7.1 (Heston et al., 2012)
Donor population (log)	PWT 7.1 (Heston et al., 2012)
Recipient population (log)	PWT 7.1 (Heston et al., 2012)
(Donor) imports in million USD (Log)	UN Comtrade (2017)
(Recipients) imports in million USD (Log)	UN Comtrade (2017)
GDP per capita (Log)	PWT 7.1 (Heston et al., 2012)
Democracy	Polity IV (Marshall et al., 2016)
Political system transition	Polity IV (Marshall et al., 2016)
Military alliance (United States)	Mattes et al. (2015)
Military alliance (Russia)	Mattes et al. (2015)
Domestic support group change (Donor)	Mattes et al. (2015)
Domestic support group change (Recipient)	Mattes et al. (2015)
Same political colour dummy	DPI (Beck et al., 2001)
Natural death of a leader (Recipient)	Jones and Olken (2005)
Executive elections (Donor)	NELDA (Hyde et al., 2012)
Executive elections (Recipient)	NELDA (Hyde et al., 2012)
Legislative elections (Donor)	NELDA (Hyde et al., 2012)
Legislative elections (Recipient)	NELDA (Hyde et al., 2012)
Presidential term limits (USA)	NELDA (Hyde et al., 2012)

Table 2-A4 – Other Channels

	Dependent variable:				
	(1)	(2)	(3)	(4)	(5)
	ODA com.	ODA com.	ODA com.	Donor Imports	Recipient Imports
Alignment change	-0.267	0.105	0.041	-0.745	-0.630
	(0.306)	(0.355)	(0.349)	(0.550)	(0.449)
Recipient change * realignment	0.954**	1.096*	1.200**	-0.801	0.285
	(0.455)	(0.575)	(0.502)	(0.742)	(0.416)
Donor change * realignment	0.777*	0.832*	0.872*	0.548	0.480
	(0.423)	(0.501)	(0.472)	(0.605)	(0.470)
Last year alignment	0.316	0.968*	0.951*	-1.449	-1.269*
	(0.391)	(0.524)	(0.530)	(1.026)	(0.729)
Past mean alignment	0.950**	0.703	0.732*	0.275	1.207**
	(0.371)	(0.446)	(0.434)	(0.562)	(0.547)
Lagged ODA commitments	0.338***				
	(0.017)				
Similarity Index (GDP)		3.386			
		(2.077)			
Similarity Index (Population)		1.742			
		(2.849)			
Donor Imports (Log)			0.015		
			(0.010)		
Recipient Imports (Log)			-0.006		
			(0.009)		
Adjusted R-squared	0.813	0.790	0.786	0.975	0.986
Fixed Effects	DR,RY,DY	DR,RY,DY	DR,RY,DY	DR,RY,DY	DR,RY,DY
# of observations	17858	16923	18571	18571	18571
# of dyads	673	663	681	681	681

Notes: All dependent variables are log-transformed. Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2-A5 – Timing of the Conditional Alignment Effect

	Dependent variable: *ln ODA commitments*				
Leader change-alignment interaction	*2 years prior*	*1 year prior*	*leader change*	*1 year after*	*2 years after*
Alignment change	0.7876*	0.5832	0.0342	0.3903	0.5907
	(0.4014)	(0.3987)	(0.3497)	(0.3446)	(0.3742)
Recipient change * realignment	0.2464	0.5709	1.1865**	0.8407*	-0.3027
	(0.4947)	(0.5081)	(0.5020)	(0.5061)	(0.5800)
Donor change * realignment	-0.6419	-0.1957	0.8773*	0.8859*	0.5312
	(0.5046)	(0.4626)	(0.4723)	(0.4949)	(0.4921)
Last year alignment	1.3252**	1.1019*	0.9370*	1.3890**	1.5363***
	(0.6232)	(0.5932)	(0.5288)	(0.5431)	(0.5779)
Past mean alignment	0.1643	0.1839	0.7297*	0.2997	0.0633
	(0.4321)	(0.4198)	(0.4342)	(0.4201)	(0.4092)
Fixed Effects	DR,RY,DY	DR,RY,DY	DR,RY,DY	DR,RY,DY	DR,RY,DY
Adjusted R-squared	0.783	0.785	0.786	0.791	0.794
# of observations	17103	17858	18571	17322	16568
# of dyads	681	681	681	681	681

Notes: Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2-A6 – Additional Variables in Table 3

Specification	Variables	Source
Mattes et al. (2015)	Democracy (if PolityIV ¿= 6)	Teorell et al. (2013)
	Political system transition	Teorell et al. (2013)
	USA defense pact	Gibler (2009)
	RUS defense pact	Gibler (2009)
Dreher and Jensen (2013)	Donor GDP per capita	Heston et al. (2012)
	Recipient GDP per capita	Heston et al. (2012)
	Political color	Beck et al. (2001)
Carter and Stone (2015)	Democracy dummy	Teorell et al. (2013)
	Donor GDP per capita	Heston et al. (2012)
	Recipient GDP per capita	Heston et al. (2012)
	Same political color	Beck et al. (2001)

Table 2-A7 – Granger Causality

	Dependent variables: *Alignment Change*			
	2SLS	*OLS*	*OLS*	*OLS*
	(1)	(2)	(3)	4
Lagged ODA			0.0003	0.0005*
			(0.0004)	(0.0003)
Recipient change	-0.0026		-0.0033*	
	(0.0059)		(0.0020)	
Donor change	-0.0013		-0.0037**	
	(0.0031)		(0.0016)	
Recipient change * lagged ODA			0.0004	0.0000
			(0.0006)	(0.0003)
Donor change * lagged ODA			0.0011**	-0.0004
			(0.0005)	(0.0003)
Last year alignment	-0.8423***	-0.8320***	-0.8378***	-0.8713***
	(0.0223)	(0.0213)	(0.0218)	(0.0358)
Past mean alignment	0.5272***	0.5185***	0.5224***	0.2101***
	(0.0205)	(0.0197)	(0.0200)	(0.0238)
Donor GDP (log)	-0.0132	-0.0143	-0.0090	
	(0.0099)	(0.0093)	(0.0095)	
Recipient GDP (log)	0.0005	-0.0002	-0.0004	
	(0.0030)	(0.0023)	(0.0024)	
Donor population (log)	-0.2580***	-0.2496***	-0.2617***	
	(0.0207)	(0.0195)	(0.0201)	
Recipient population (log)	0.0030	-0.0008	0.0012	
	(0.0063)	(0.0061)	(0.0062)	
Adjusted R-squared	0.5476	0.5621	0.5655	0.8909
Fixed Effects	DR,Y	DR,Y	DR,DY,RY	
Obs	15576	16337	16337	17858
Dyads	663	668	662	673

Notes: Fixed effects: donor-recipient (DR), year (Y), recipient-year (RY), donor-year (DY). Robust standard errors in parentheses, clustered on donor-recipient dyad. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Chapter 3

Development Aid and Conflict

Taking conflict dynamics seriously *

3-1 Introduction

Civil conflict is not only one of the main obstacles to development, it also tends to be concentrated in poor countries. About half of all developing countries experienced an armed conflict in which at least 25 people died in a given year over the past four decades – directly or indirectly affecting close to four billion people. At the same time, poor and badly governed states prone to conflict need and receive substantial amounts of development assistance. Bilateral aid averaged about 5% of recipient GDP over the same period, but does this aid appease or fuel conflict?

A large and growing literature examining this question has failed to generate a consensus. Theoretically, the relationship is ambiguous as rising opportunity costs, increasing state capacity, and greater gains from capturing the state are all plausible consequences of development assistance. The empirical evidence is equally divided: several studies find that aid helps, while others maintain that it obstructs peace. Credible evidence is usually limited to specific regions or countries (e.g., the Philippines, Crost et al., 2014), specific types of aid (e.g., U.S. food aid, Nunn and Qian, 2014) or both (e.g., U.S. military aid in Columbia, Dube and Naidu, 2015). Devising a convincing identification strategy for bilateral aid has proven difficult given the well-known limitations of cross-country data.

Another notable divide between the theoretical and empirical literature is that the latter pays little attention to the dynamics of conflict. Empirically, conflict is usually considered to be a binary state, although recent theory stresses the importance of smaller conflicts (e.g., Bueno de Mesquita, 2013), different types of violence (e.g., Besley and Persson, 2011b), and conflict cycles (e.g., Rohner et al., 2013; Acemoglu and Wolitzky, 2014). Most papers distinguish between the onset and continuation of conflict, but studying these two transitions separately is an imperfect substitute for analyzing an inherently dynamic problem (Beck et al., 1998). More fundamentally, there is no empirical sense of escalation or deescalation among different conflict intensities when the ordinal nature of conflict is disregarded. Only the case of a switch from peace to conflict and vice versa is usually accounted for. These distinctions matter. As we show in the following, small scale conflicts below

*This chapter is based on joint work with Richard Bluhm, Martin Gassebner and Sarah Langlotz (Bluhm et al., 2018).

the usual minimal threshold of 25 battle-related deaths often start a cycle of violence. In contrast, a civil war never broke out in a society that was completely at peace in the year before.

Establishing the *causal* effect of bilateral aid on the escalation and deescalation of conflict is the key objective of this chapter. In essence, we conjecture that neglecting smaller conflicts pollutes most existing estimates of the effect of aid on conflict. To see this, consider the argument that foreign aid incites violence because some groups inevitably profit more from the added financial flows than others. Hodler and Raschky (2014) and Dreher et al. (2018), for example, show that funds tend to disproportionately flow to the birth region of the current ruler. This is likely to translate into civil discontent which can find its expression in smaller acts of violence with comparatively low opportunity costs. Any violent behavior questions the state's monopoly of violence, satisfying what can be considered the most basic definition of civil conflict. Small conflicts thus act as a signal to the government that some part of society is not content with the current provision, or division, of public goods. In addition, they help potential rebels to get an estimate of how easily they can overcome collective action problems and provide information about the government's repressive capabilities. Foreign aid, in turn, may exacerbate violent tendencies in such environments but not when society is truly at peace.

Our empirical analysis introduces three novelties in order to identify these dynamics. First, we propose a new measure of conflict which captures the gradations of civil violence from peace over intermediate categories to fully fledged civil wars. Second, we develop a dynamic ordered probit framework which allows us to estimate escalation and deescalation probabilities for multiple states. In our approach, the onset, continuation, and the duration of each realization of civil violence are all well defined. We then extend this basic framework to account for unobserved heterogeneity (quasi fixed effects) and correct for the endogeneity of aid (based on Rivers and Vuong, 1988; Wooldridge, 2005; Giles and Murtazashvili, 2013). Third and most importantly, we identify the effect of aid on conflict using characteristics of the electoral system of donor countries. We interact political fractionalization of each donor with the probability of receiving aid to predict bilateral aid flows in a "gravity-style" aid equation (Frankel and Romer, 1999; Rajan and Subramanian, 2008; Dreher and Langlotz, 2015). This type of identification strategy is now common in the trade and migration literatures but usually relies on structural characteristics of both partner countries. We solely use the variation arising from electoral outcomes in donor countries combined with the likelihood of receiving aid.

Our main results show that the causal effect of foreign aid on the various transition probabilities is heterogeneous and, in some instances, sizable. Foreign aid has a very different effect on the probability of experiencing conflict, depending on whether a society was entirely peaceful, already in turmoil, or mired in major civil conflict.

Aid does not seem to harm recipient countries by causing conflict across the board. While all estimates suggest that bilateral aid tends to fuel conflict, we find scarce evidence suggesting that foreign aid leads to new eruptions of conflict or that it drives the escalation towards (or the continuation of) civil wars. At face value, the positive signs are also at odds with rising opportunity costs, although it remains difficult to delineate the exact channels.

Our findings suggest that aid can be harmful when given to countries already experiencing violent turmoil just short of the conventional definition of civil conflict. In those cases we find *i)* a strong negative effect on the probability of transitioning back to peace, *ii)* an elevated risk of continued violence, and *iii)* a non-trivial probability of escalating into armed conflict. Donor countries have to be aware of the unintended consequences of giving aid to countries with lingering conflicts.

Our results underscore the importance of carefully modeling the dynamics of conflict. This echoes the recent literature (e.g., Bazzi and Blattman, 2014; Nunn and Qian, 2014; Berman and Couttenier, 2015) but our analysis goes several steps further and generates new insights. Escalation or deescalation, i.e., the switching among different conflict intensities, is a dynamic process and the established binary peace-war typology hides important heterogeneity. What is often coded as peace is not actually peaceful and what influences the decision to fight differs in these situations.

The remainder of the chapter is organized as follows. Section 3-2 discusses the related literature and provides the theoretical background. Section 3-3 introduces our new ordinal conflict measure. Section 3-4 outlines our empirical model and identification strategy. Section 3-5 presents the empirical results and Section 3-6 discusses a battery of robustness checks. Section 3-7 concludes.

3-2 Related Literature

3-2.1 Civil Conflict and Foreign Aid

The direction of the overall effect of aid boils down to how it changes the calculus of citizens and governments. For citizens, aid may alter the opportunity costs of fighting (e.g., Becker, 1968; Collier and Hoeffler, 2004b). For governments, aid may increase state capacity (Fearon and Laitin, 2003; Besley and Persson, 2011a) and/or increase the value of capturing the state (e.g., Grossman, 1991). Variants of these theories incorporate both channels and try to distinguish between two opposing income effects: having less to fight over but fewer outside options versus fighting over a larger pie but having more to lose. As a result of this heterogeneity, the overall sign of the effect of aid remains theoretically ambiguous. We now briefly discuss these channels one by one.

Foreign aid affects the opportunity costs of fighting. If aid improves the provision of public goods, then it directly decreases the incentives of engaging in violent activities (Becker, 1968). Aid may also alter opportunity costs indirectly through economic growth. However, the large empirical literature on aid and growth finds little or at best weak evidence in favor of this channel (e.g., Rajan and Subramanian, 2008; Clemens et al., 2012; Dreher and Langlotz, 2015). The literature on income shocks and conflict is also instructive. Bazzi and Blattman (2014) find no evidence of an effect of export price shocks on conflict at the country-level, while Berman and Couttenier (2015) add that negative income shocks predict conflict at the subnational level.

Foreign aid may increase state capacity. When aid improves public resources, the government is likely to put more effort into controlling these resources (Fearon and Laitin, 2003). Greater control over resources increases its capability to suppress conflict and higher state capacity lowers the risk of conflict by reducing the likelihood of successful capture (Besley and Persson, 2011a). It thus diminishes the expected value of rebellion. Part of the state capacity effect could run through military spending. Although official development aid excludes military aid by definition, receiving aid relaxes the government's budget constraint if aid is sufficiently fungible (Collier and Hoeffler, 2007).

Foreign aid raises the stakes. Standard contest theory argues that the state is a price that rebels want to capture (e.g., Grossman, 1991). It predicts that conflict becomes more likely when aid receipts are higher as the expected gains from fighting increase. Such arguments are pervasive in the literature on conflict over natural resources and many other contests. However, the equilibrium level of conflict may be independent of the income level if the revenue and opportunity cost effects cancel out (Fearon, 2007). Dal Bó and Dal Bó (2011) show that the relative size of these effects depend on the labor and capital intensity of production, while Besley and Persson (2011b) introduce a model where they depend on the cohesiveness of political institutions. When aid acts like a resource windfall in weak states, it raises violence and repression in equilibrium. Hence, it matters where development aid actually goes and how easily it can be appropriated by rebels, either directly by intercepting aid deliveries or indirectly by imposing "revolutionary taxation."

Most studies in the literature on civil conflict find that aid appeases (e.g., De Ree and Nillesen, 2009; Savun and Tirone, 2011; Ahmed and Werker, 2015). Recently, however, evidence to the contrary has been accumulating (e.g., Besley and Persson, 2011b; Nunn and Qian, 2014; Dube and Naidu, 2015). Nunn and Qian (2014), for example, argue that food aid can be used as rebel financing since it can be captured almost instantly. Their results show that U.S. food aid prolongs the duration of conflict but does not predict conflict onset. Rising opportunity costs can also lead to an adverse effect of aid. Crost et al. (2014) show that municipalities in the Philippines which are about to receive more aid experience increased rebel activity. Rebels anticipating the impending change in incentives sabotage aid, since successful aid programs reduce support for their cause.

3-2.2 Cycles of Violence

The cyclical nature of conflict is receiving increasing attention. Recent theories aim to account for escalation and deescalation cycles in a unified framework. Besley and Persson (2011b) emphasize that one-sided violence by an incumbent aiming to stay in power gives rise to multiple states of violence, ranging from peace over repression to civil war. Rohner et al. (2013) and Acemoglu and Wolitzky (2014) present models where recurring conflicts can happen by accident but are often started when there is a break down of trust or signals are misinterpreted. They only end when beliefs are updated accordingly. Once such a cycle starts, persistence may simply be the product of continuously eroding outside options which suggests that stopping violence becomes more difficult as conflicts intensify. The empirical literature lags behind this development. Even if studies account for different intensity levels, they usually analyze them separately and thus cannot deliver a full description of the underlying dynamics.

Small conflicts matter for a proper understanding of conflict cycles. They are often the starting point for further escalation and can be an integral part of rebel tactics. Political economy models highlight the importance of collective action and information problems that have to be overcome to engage in organized violence, revolution, or civil war (Esteban et al., 2012; Bueno de Mesquita, 2013). Small conflicts can help to overcome these problems by delivering an estimate on how many others are willing to fight the government. Theoretically, small conflicts can be considered a signaling device, where potential rebels try to determine the type of their government or vice versa (Acemoglu and Wolitzky, 2014). Minor violent actions do not have the same opportunity costs as civil war. They allow groups of individuals to question the monopoly of violence without investing too much into the fight and may be strategic substitutes to conventional warfare in a long-standing rebellion (Bueno de Mesquita, 2013). Empirically, these situations are very different from peace. Without accounting for small-scale conflicts, estimates of onset probabilities are likely to be biased by mixing truly peaceful societies with already violent and volatile environments.

A neglect of small conflicts is particularly worrying when it comes to the impact of aid on conflict. The effect of aid may very well be heterogeneous and depend on the level of violence.[1] This could be the case for at least two reasons. First, aid is not distribution-neutral (see, e.g., Dreher et al., 2018, who show that Chinese aid disproportionately flows to the birth region of African leaders). Greater aid flows may increase pre-existing discontent over the allocation of resources. Due to logistical reasons aid is given more often to peaceful regions or regions of low conflict intensity. If aid is primarily targeted at such regions, resentment may fortify in unprivileged areas, where violence persists. Opportunity costs erode and rebels controlling such a region may be able to recruit others more easily. Second, if a country is entirely peaceful, the government is less likely to divert development aid or freed-up funds to the military. If there is a lingering conflict, on the other hand, the incumbent government might continue to invest in the military to repress or discourage rebellion (Besley and Persson, 2011a). Hence, the effect of aid on state capacity differs depending on the level of violence.

3-2.3 Causal Identification

The simultaneity of aid and conflict makes causal identification notoriously difficult. The strong correlation of low GDP per capita and civil strife is one of the most robust findings in the literature (e.g., Fearon and Laitin, 2003; Blattman and Miguel, 2010). Underdevelopment – with all that it entails – is the *raison d'être* of development aid. As a result, the effect of aid is likely to be biased upwards if aid is primarily given to countries in need, or biased downwards if donors are driven by political motives (as documented by, e.g., Kuziemko and Werker, 2006) or reduce aid in light of the logistical challenges created by conflict. Biases could also result from third factors influencing aid and conflict simultaneously, such as political and economic crises, or (systematic) measurement errors.

Much of the literature follows Clemens et al. (2012) and addresses the endogeneity problem by lagging aid. This is meant to rule out reverse causality and avoid bad-quality instruments (arguably without much success). Others follow the advice of Blattman and Miguel (2010) and focus on causal identification with single instruments. However, most instruments proposed so far are either weak or not exogenous: De Ree and Nillesen (2009), for example, use donor country GDP

[1] For instance, Collier and Hoeffler (2004a) argue that aid is especially effective in post-conflict scenarios.

as an instrument for bilateral aid flows which could work through a variety of other channels, such as trade or FDI. A noteworthy exception are Nunn and Qian (2014) who use lags of U.S. wheat production interacted with each recipient's frequency of receiving aid as an instrument for U.S. food aid.[2] We extend the spirit of their identification strategy to all major bilateral donors, with the explicit aim of drawing conclusions that go beyond the (limited) effects of food aid given by one large donor. Much of the ground work has been done in Dreher and Langlotz (2015) who first introduce political fractionalization interacted with the probability of receiving aid as an instrument for bilateral aid flows in the context of growth regressions. We describe this strategy in more detail below.

3-3 Data

We study the occurrence of civil violence in 125 developing countries over the period from 1975 to 2010. We first discuss our measure of conflict, and then the operationalization of aid and the covariates. A list of the included countries and summary statistics of all variables can be found in Appendix A (Tables 3-A1 to 3-A3).

3-3.1 An Ordinal Measure of Conflict

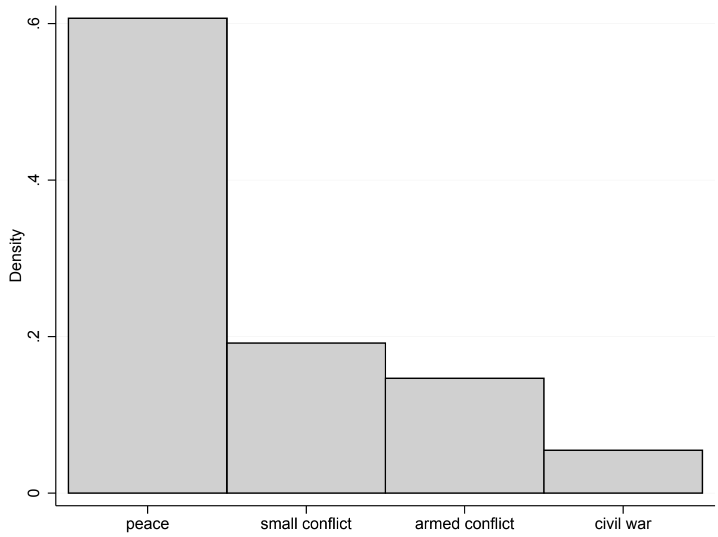
A distinct feature of the civil conflict literature is its crude measurement of conflict. The industry standard is to first count the number of battle-related deaths (BDs) and then to create dummy variables indicating the surpassing of one of two thresholds (25 or 1,000 BDs) for the first time (conflict onset) or for any given year other than the first (continuation or ending). Clearly, a key concern motivating this choice is noise in the underlying raw data and theoretical ambiguity about what constitutes "conflict."

We propose a new ordinal measure of conflict with four states. For comparability, we begin with the standard UCDP-PRIO measure of civil conflict ('internal armed conflict', Gleditsch et al., 2002). UCDP-PRIO defines civil conflict as a contested incompatibility that concerns the government or a territory in which armed force between two parties, one of which is the government, and results in at least 25 BDs per annum. We call conflicts that reach this state but do not exceed 1,000 BDs in a given year 'armed conflict.' At the top, we add a category called 'civil war' if there are more than 1,000 BDs. At the bottom, we complement the data with observations from the Cross-National Time-Series Data Archive (CNTS) on government purges, assassinations, riots and guerrilla warfare (Banks and Wilson, 2015).[3] All of these

[2]A different strategy is proposed by Werker et al. (2009) and Ahmed and Werker (2015), who use oil prices to instrument aid flows from oil-producing Muslim to non-oil producing Muslim countries.

[3]The precise definitions of our variables from the Databanks User's Manual are as follows. Purges: Any systematic elimination by jailing or execution of political opposition within the ranks of the regime or the opposition. Assassinations: Any politically motivated murder or attempted murder of a high government official or politician. Riots: Any violent demonstration or clash of more than 100 citizens involving the use of physical force. Guerrilla Warfare: Any armed activity, sabotage, or bombings carried on by independent bands of citizens or irregular forces and aimed at the overthrow of the present regime. Note that Besley and Persson (2011b) took a similar approach when they added one-sided state repression (purges) as an intermediate category to what we define

Figure 3-1 – Distribution of Conflict Intensities



Notes: Illustration of the unconditional distribution of the ordinal conflict measure. There are 3,014 peace years, 739 small conflict years, 544 armed conflict years, and 203 civil war years in our sample.

categories are manifestations of civil conflict, albeit on a lower intensity level. We only include observations of the CNTS data that are comparable to the type of conflict we consider in the above categories, i.e., conflicts between two parties one being the state (two-sided, state-centered).[4] Only a truly peaceful society is coded zero. As a whole, the countries in our sample spend about one third of all years in conflict at various intensities and about two thirds of all years in peace. Figure 3-1 shows a histogram of the intensity distribution.

A key advantage of our approach is that the number of armed conflicts and civil wars in our sample are identical to the UCDP-PRIO measure. Hence, our results are comparable with existing studies and differ mainly due to the definition of peace. We distinguish between truly peaceful observations and those with irregular violence below the conventional thresholds. This conservative approach of changing existing measures implies that our ordinal measure is comparable and easy to understand. We avoid weighting procedures such as those used by the composite index of the CNTS data set. We also deliberately refrain from mixing flow and stock variables to measure different conflict intensities, such as taking the cumulative amount of BDs to create intermediate levels of armed civil conflict (e.g., Esteban et al., 2012; Bazzi and Blattman, 2014). Measures including both flow and stock variables do not allow us to study escalation and deescalation since they have absorbing terminal states. Appendix B presents the case of Sri Lankan Civil War to illustrate the benefits of our coding in more detail.

Table 3-1 shows the unconditional transition probabilities as they are observed

as civil war.

[4]In the case of riots this may not be obvious from the variable definition, but the large riots recorded in the CNTS data usually involve violent clashes between anti-government protesters with (pro-)government forces. They are what incumbents react to with repression. For a prototypical example, see Yemen in 2011 (`http://www.nytimes.com/2011/02/15/world/middleeast/15yemen.html`).

Table 3-1 – Unconditional Transition Matrix (in %)

	To State			
From State	Peace	Small Conflict	Armed Conflict	Civil War
Peace	87.26	10.69	2.06	0.00
Small Conflict	43.85	48.13	6.78	1.24
Armed Conflict	11.28	8.46	70.30	9.96
Civil War	1.49	5.97	23.88	68.66

Notes: The table reports the raw transition matrix estimated using the same balanced sample of 125 countries over 36 years that is used in the main analysis (4,500 observations imply 4,375 transitions). Rows sum to 100%.

in our data. This simple exercise already allows us to make three worthwhile points. First, the cyclical nature of conflicts is clearly visible but there is not a single country in our data set where peace immediately preceded civil war. Second, our coding of small conflict achieves a credible and important separation of the lower category. Peace is now very persistent and, if anything, a transition to a small conflict is most likely. Small conflict is a fragile state which often reverts back to peace, is not particularly persistent, but does sometimes erupt into more violent states. Third, higher intensity conflicts are once again more persistent. These observations match up well with the literature, in particular, the use of irregular means to increase mobilization for a future conventional campaign and increased persistence as outside opportunities erode (Bueno de Mesquita, 2013).

3-3.2 Bilateral Aid Flows and Controls

Our main independent variables are two types of flows disbursed by 28 bilateral donors of the OECD Development Assistance Committee (DAC): Official Development Aid (ODA) and Other Official Flows (OOF). ODA refers to flows that are *i)* provided by official agencies to developing countries and multilateral institutions, *ii)* have economic development and welfare as their main objective, and *iii)* have a concessional character. The last condition reflects that the grant element should be at least 25%. OOF includes flows by the official sector with a grant element of less than 25% or flows that are not primarily aimed at development. We use net ODA flows which include loan repayments since these reduce the available funds. In the robustness section, we also consider multilateral aid.

The data for government and legislative fractionalization (in donor countries) are from Beck et al. (2001). For the set of core controls, we follow Hegre and Sambanis (2006) by including the log of population to capture the scale effect inherent in conflict incidence and the log of GDP. We later also use the Polity IV score to account for institutional quality and a democracy dummy indicating if the Polity score is equal or above six. We control for a measure of political instability, that is, a dummy coded one if a country has experienced a change in its Polity IV score of at least three points. We also include the regional Polity IV score to proxy for the democratic values of the neighborhood (Gates et al., 2006) and allow for spillovers from neighboring countries with dummies indicating if at least one neighbor had a small conflict, armed conflict or war during a given year (Bosker and de Ree, 2014).

3-4 Empirical Strategy

3-4.1 Conflict Histories

We now develop an empirical framework that captures the ordinal nature of conflict, allows for a rich specification of conflict histories and includes variables that have history-dependent effects.

Dynamic switches among multiple states cannot be meaningfully estimated with linear models. Beck et al. (1998) show that separately specifying models of onset and ending of war is equivalent to a dynamic model of war incidence. However, many more linear models would be needed to study the transition among multiple states. The result would be unstable parameter estimates that are inefficiently estimated, potentially biased, and difficult to interpret. Further, if we believe that there is an underlying latent variable ('conflict') which is observed as an ordered outcome, then separate regressions can violate known parameter restrictions.[5] Hence, a non-linear framework is needed.

Some notation is in order to help fix ideas. As typical in an ordered setting, we observe a conflict outcome c_{it} which takes on $J+1$ different values in country i at time t. A specific outcome is $j \in \{0, 1, \ldots, J\}$. The outcomes are ordered by intensity (i.e., peace, small conflict, armed conflict, civil war) and are generated by a continuous latent variable c^*_{it} with J cut points $\alpha_1 < \cdots < \alpha_j < \cdots < \alpha_J$ to be estimated later. The first outcome is $c_{it} = 0$ if $-\infty < c^*_{it} < \alpha_1$, the intermediate outcomes are $c_{it} = j$ if $\alpha_j < c^*_{it} < \alpha_{j+1}$ with $0 < j < J$, and the last outcome is $c_{it} = J$ if $\alpha_J < c^*_{it} < \infty$.

Next, define the associated $J \times 1$ vector of one period conflict histories as $\mathbf{h}_{i,t-1} \equiv (h_{1,i,t-1}, \ldots, h_{j,i,t-1}, \ldots, h_{J,i,t-1})'$. The typical element of $\mathbf{h}_{i,t-1}$ is $h_{j,i,t-1} \equiv 1[c_{i,t-1} = j]$, that is, an indicator of whether the past outcome is identical to outcome j.

Contrary to the standard approach, our latent variable model of interest has a full set of history-dependent effects

$$c^*_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + \mathbf{h}'_{i,t-1}\boldsymbol{\rho} + (\mathbf{x}_{it} \otimes \mathbf{h}_{i,t-1})'\boldsymbol{\gamma} + \mu_i + \epsilon_{it} \quad , \quad (3\text{-}1)$$

where $\mathbf{x}_{it}$ is a column vector of regressors without a constant, $\mathbf{h}_{i,t-1}$ is defined above, and the Kronecker product simply accounts for all possible interactions between $\mathbf{x}_{it}$ and $\mathbf{h}_{i,t-1}$. We include country level unobserved effects, μ_i, whose identification we discuss below. Typically we will partition the vector $\mathbf{x}_{it} = (\mathbf{x_1}'_{it}, \mathbf{x_2}'_{it})'$, so that some variables are history-dependent and others are not (e.g., proxy controls and time dummies). We are only interested in the estimated coefficients inasfar as they define the relevant probabilities.

Conditional on the covariates and the conflict history we have three different

[5] This is a version of the misnamed "parallel regression assumption" in ordered probit models. If the outcome is an ordered response, then the predicted probabilities of falling below a certain cut point *must* be increasing in the outcome j for all values of the covariates (Wooldridge, 2010, p. 658). If all the coefficients can vary in each state, then this meaningless result cannot be ruled out.

types of outcome probabilities: $\Pr[c_{it} = 0|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}] = \Pr[c^*_{it} \leq \alpha_1|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}]$, $\Pr[c_{it} = j|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}] = \Pr[\alpha_j < c^*_{it} \leq \alpha_{j+1}|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}]$, and $\Pr[c_{it} = J|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}] = \Pr[c^*_{it} > \alpha_J|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}]$. We have to be more explicit in the notation since we are interested in the transition and continuation probabilities of the various states. For simplicity, just focus on the j-th intermediate outcome where $0 < j < J-1$, then w.l.o.g. we can define continuation, escalation and deescalation from an initial state $j+p$ to outcome j as:

$$\begin{aligned}\Pr[c_{it} = j|\mathbf{x}_{it}, h_{j+p,i,t-1} = 1] =& F\left[\alpha_{j+1} - \mathbf{x}'_{it}\boldsymbol{\beta} - \rho_{j+p} - (\mathbf{x}_{it} \times h_{j+p,i,t-1})'\boldsymbol{\gamma}_{j+p} - \mu_i\right] \\ &- F\left[\alpha_j - \mathbf{x}'_{it}\boldsymbol{\beta} - \rho_{j+p} - (\mathbf{x}_{it} \times h_{j+p,i,t-1})'\boldsymbol{\gamma}_{j+p} - \mu_i\right],\end{aligned} \tag{3-2}$$

where we have escalation if $p < 0$, continuation if $p = 0$ and deescalation if $p > 0$. The case of $p = 0$ is often also called 'persistence.' $F(\cdot)$ is some continuous symmetric c.d.f. which is defined by the distribution of the error terms, ϵ_{it}.

The purpose of this entire exercise is to be able to define the partial effect of a particular $x_{k,it} \in \mathbf{x}_{it}$ on one of the transition probabilities defined above. It should now be straightforward to see that these are the derivatives of a particular probability with respect to $x_{k,it}$. For example, in the case of continuing in the past state j we have

$$\begin{aligned}\frac{\partial}{\partial x_k}\left(\Pr[c_{it} = j|\mathbf{x}_{it}, h_{j,i,t-1} = 1]\right) =& (\beta_k + \gamma_{j,k})\left(f\left[\alpha_j - \mathbf{x}'_{it}\boldsymbol{\beta} - \rho_j - (\mathbf{x}_{it} \times h_{j,i,t-1})'\boldsymbol{\gamma}_j - \mu_i\right]\right. \\ &\left.- f\left[\alpha_{j+1} - \mathbf{x}'_{it}\boldsymbol{\beta} - \rho_j - (\mathbf{x}_{it} \times h_{j,i,t-1})'\boldsymbol{\gamma}_j - \mu_i\right]\right),\end{aligned} \tag{3-3}$$

where $f(\cdot)$ is the p.d.f. of $F(\cdot)$.

We still lack a formal definition of state-dependence. In binary models, state dependence is the probability of an event happening when the event happened before minus the probability of the event when it did not happen before net of all other observed and unobserved factors. With ordered outcomes it is no longer that simple. We need to account for the fact that there are several ways of entering into a particular state. Inspired by the labor literature (Cappellari and Jenkins, 2004), we estimate state-dependence as the difference between experiencing a particular state if it has occurred before and a weighted average of the ways of entering this state when it has not occurred before.

Formally, define state dependence in state j as follows:

$$S_j = (NT)^{-1}\sum_i^N\sum_t^T\left(\Pr[c_{it} = j|\mathbf{x}_{it}, h_{j,i,t-1} = 1] - \sum_{r\neq j}\omega_{rj}\Pr[c_{it} = j|\mathbf{x}_{it}, h_{r,i,t-1} = 1]\right), \tag{3-4}$$

where the weights, ω_{rj}, are the normalized class frequencies (the number of observations that can potentially make the switch, normalized to sum to unity). We expect state dependence to increase with higher conflict intensities. The higher the level of conflict, the more difficult it becomes to leave states that have a destructive nature.

3-4.2 Dynamic Ordered Probit with Endogeneity

Identification of endogenous regressors and their partial effects under the presence of heterogeneity and first-order dynamics is tricky in non-linear settings. Researchers often opt for linear instrumental variable methods to keep things simple, but here we trade simplicity for a better understanding of the dynamics.

To model the ordered conflict outcome, we combine correlated random effects (CRE) and a control function (CF) approach with dynamic panel ordered probit models. Dynamic models with correlated random effects where all regressors are strictly exogenous have been studied by Wooldridge (2005), among others, and endogeneity was introduced into these types of dynamic binary choice models by Giles and Murtazashvili (2013). To the best of our knowledge, we are the first to employ a CRE approach with an endogenous regressor in an dynamic ordered setting. Note that this approach does not work with unbalanced panels. In the robustness section, we also specify linear models for comparison.

We incorporate two specific features into the general formulation from the preceding section. First, we add an endogenous regressor (the ratio of bilateral aid to GDP) and, second, we interact this variable with the one-period conflict history. We do not consider other interactions. Hence, our model of interest becomes

$$c^*_{1it} = \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \beta_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + u_{1it} \quad , \quad (3\text{-}5)$$

where $\mathbf{z}_{1it}$ is a column vector of strictly exogenous variables, a_{2it} is the endogenous aid to GDP ratio, λ_{1t} are time dummies, and everything else is defined as before. We added subscripts to each variable or vector if they belong to the main equation of interest (1) or the reduced form (2). We assume that the model is dynamically complete once the first-order dynamics are accounted for and that the error term is free of serial correlation. The process starts at $s < 0$ and is observed over $t = 0, \ldots, T$. We always lose the first period, so in eq. (3-5) and from now on estimation runs over $t = 1, \ldots, T$.

The endogenous aid to GDP ratio has the following linear reduced form

$$a_{2it} = \mathbf{z}'_{1it}\boldsymbol{\alpha}_1 + \mathbf{z}'_{2it}\boldsymbol{\alpha}_2 + \mu_{2i} + \lambda_{2t} + u_{2it} \quad , \quad (3\text{-}6)$$

where $\mathbf{z}_{2it}$ is a vector of instruments that is relevant and excluded from the main equation. Our instrument is generated from bilateral regressions. We discuss its construction in detail in the next section. Note that under mild conditions a generated instrument works just like a regular instrument: the parameters are estimated consistently and the limiting distributions are the same (see Wooldridge, 2010, p. 125). Hence the standard errors need not be adjusted, they are only likely to be noticeably biased in small samples.

We assume that the reduced form heterogeneity can be expressed as $\mu_{2i} = \bar{\mathbf{z}}'_i\boldsymbol{\psi} + b_{2i}$, where $b_{2i}|\mathbf{z}_i \sim \mathcal{N}(0, \sigma^2_{b_2})$ and $\mathbf{z}_i \equiv (\mathbf{z}'_{1it}, \mathbf{z}'_{2it})' \equiv (\mathbf{z}'_{i1}, \mathbf{z}'_{i2}, \ldots, \mathbf{z}'_{iT})'$ is a vector of all strictly exogenous variables in all time periods. Plugging this into eq. (3-6) gives

$$a_{2it} = \mathbf{z}'_{1it}\boldsymbol{\alpha}_1 + \mathbf{z}'_{2it}\boldsymbol{\alpha}_2 + \bar{\mathbf{z}}'_i\boldsymbol{\psi} + \lambda_{2t} + \nu_{2it} \quad , \quad (3\text{-}7)$$

where $\nu_{2it} = b_{2i} + u_{2it}$ is the new composite error term. It is well known that the coefficients on the time-varying covariates in eq. (3-7) are numerically equivalent to the linear fixed effects model, making this a very robust specification (Wooldridge, 2010, p. 332).

Following Rivers and Vuong (1988) and Giles and Murtazashvili (2013), joint normality of (u_{1it}, u_{2it}) conditional on $\mathbf{z}_i$ with $Var(u_{1it}) = 1$, $Cov(u_{1it}, u_{2it}) = \tau$, and $Var(u_{2it}) = \sigma^2_{u_2}$ implies that we can rewrite our model of interest as

$$c^*_{1it} = \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + \omega u_{2it} + \epsilon_{1it}, \quad , \tag{3-8}$$

where we define $\omega = \tau/\sigma_{u_2}$.

Note that $u_{1it} = \omega u_{2it} + \epsilon_{1it} = \omega(\nu_{2it} - b_{2i}) + \epsilon_{1it}$, so our equation of interest is contaminated by both the first stage errors and the associated unobserved heterogeneity. The role of ν_{2it} is to "correct" for the contemporaneous endogeneity between the two equations, while b_{2i} allows for feedback from the unobserved effect in the reduced form.

If we let $b_{1i} = \mu_{1i} - \omega(\nu_{2it} - u_{2it})$ be the composite unobserved effect, then the key question in non-linear dynamic models is what assumptions do we make about how the composite heterogeneity relates to the initial conditions $\mathbf{h}_{i0}$, the covariates $\mathbf{z}_i$ and the reduced form errors in all periods $\boldsymbol{\nu}_{2i}$?

Following Giles and Murtazashvili (2013), we assume that $b_{1i}|\mathbf{z}_i, \mathbf{h}_{i0}, \boldsymbol{\nu}_{2i} \sim \mathcal{N}(\mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3, \sigma^2_d)$. This homoskedastic normal distribution implies that the composite heterogeneity is a linear function: $b_{1i} = \mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3 + d_{1i}$ where $d_{1i}|\mathbf{z}_i, \mathbf{h}_{i0}, \boldsymbol{\nu}_{2i} \sim \mathcal{N}(0, \sigma^2_d)$. Plugging this into eq. (3-8) gives the final equation

$$\begin{aligned} c^*_{1it} = {} & \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \omega\nu_{2it} \\ & + \lambda_{1t} + \mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3 + d_{1i} + \epsilon_{1it}, \end{aligned} \tag{3-9}$$

which can be estimated by standard random effects ordered probit along with the cut points α_j which will result in scaled parameters (e.g., $\boldsymbol{\beta}_1/\sqrt{(1+\sigma^2_{d_1})}$ and so on, assuming the usual normalization of $Var(\epsilon_{1it}) = 1$ is applied).

A two-step approach means *i)* we first estimate the reduced form in eq. (3-7), obtain an estimate of the residuals $(\hat{\nu}_{2it})$ and the reduced form errors in all periods $(\hat{\boldsymbol{\nu}}_{2i})$, and then *ii)* plug these into eq. (3-9). The standard errors are bootstrapped over both stages to account for the estimation of the residuals in the first step. Note that the CF approach does not require interactions with the residuals unlike IV methods, making it somewhat less robust but potentially much more efficient (Wooldridge, 2010, p. 128).

In our case T is large which has two major implications. First, adding a new time-varying control variable means adding T additional regressors. Second, the initial conditions problem is not likely to be severe. Rabe-Hesketh and Skrondal (2013) provide simulation results for different ways of specifying the conditional density of the unobserved effect in the dynamic binary probit model. Inspired by their study, we experimented with constraints that can be placed on the two sequences $\mathbf{z}_i$ and $\hat{\boldsymbol{\nu}}_{2i}$. Our results suggest that allowing only the first few periods to have an independent effect and constraining the rest to the time averages yields results that are almost indistinguishable from the full model.[6]

[6] We conserve degrees of freedom by splitting the two vectors, so that in the case of the exogenous variables we have $\mathbf{z}^+_i = (\mathbf{z}'_{i1}, \mathbf{z}'_{i2}, \ldots, \mathbf{z}'_{iR}, \bar{\mathbf{z}}^{+'}_i)'$ where $R < T$ and $\bar{\mathbf{z}}^+_i = \frac{1}{T-R-1}\sum_{t=R+1}^{T}\mathbf{z}_{it}$ is the time average after period R. The residual sequence, $\boldsymbol{\nu}^+_{2i}$, is computed

The average partial effects (APEs) are derivatives of the expectation of our specification with respect to the distribution of b_{1i} (see Blundell and Powell, 2004; Wooldridge, 2005). The APEs can be different for each t. We usually average across all observations to obtain a single estimate.

3-4.3 Identification

We use political fractionalization in donor countries interacted with the probability of receiving aid as our primary source of exogenous variation at the donor-recipient level. Dreher and Langlotz (2015) show that government fractionalization interacted with this probability is a strong instrument for bilateral aid. Government fractionalization is defined as the probability that any two randomly-chosen deputies of the parties forming the government represent different parties (Beck et al., 2001).

The motivation for this instrument comes from three different strains of literature. First, government or legislative fractionalization has been shown to positively affect government expenditures (Roubini and Sachs, 1989). Within a coalition government, logrolling during the budgeting process will lead to higher overall government expenditures. Second, higher government expenditures also imply higher aid budgets (Brech and Potrafke, 2014). Third, higher aid budgets translate into higher aid disbursements (Dreher and Fuchs, 2011). The interaction with the probability of receiving aid then introduces variation across recipients. An interaction of this endogenous probability with an exogenous variable is itself exogenous, provided we include country and time fixed effects.

Most studies analyzing the effects of political fractionalization on government spending focus on parliamentary systems with proportional representation. This is because coalition governments are more likely to be generated by some systems rather than others. Electoral rules, in particular first-past-the-post (FPTP) rules, define if government can be fractionalized at all or if there is a single-party government which negotiates the budget process in some form of reconciliation process with the legislative body. Persson et al. (2007) present a model along these lines where majoritarian elections usually lead to single party government and less spending in equilibrium than proportional elections. Hence, we prefer government fractionalization over fractionalization of the legislature as an instrument in parliamentary systems with proportional representation.[7] For the few donors with FPTP systems – Canada, the UK, and the U.S. – we use legislative fractionalization as our preferred source of exogenous variation.[8]

Just as in Nunn and Qian (2014), our identification strategy can be related to a difference-in-difference (DiD) approach. We essentially compare the effects of aid induced by changes in political fractionalization in donor countries among regular and irregular aid recipients. We later also examine the parallel trends assumption

analogously. Our results are not sensitive to the choice of R, as long as the first period is allowed to have its own coefficients. We typically set $R = 4$. We also included $\mathbf{z}_{i0}$ to little effect (as suggested by Rabe-Hesketh and Skrondal, 2013).

[7] Legislative fractionalization is defined similarly to government fractionalization. It gives the probability of randomly picking two deputies from the legislature that belong to different parties.

[8] France is an interesting case as it is a mixed system with two-round runoff voting. However, both government and legislative fractionalization vary for France. In a robustness test we also treat France in the same way as Canada, the UK, and the U.S. without a material impact on the results.

inherent in our approach.

Applying this in a bilateral setting requires aggregating the bilateral variation in the instruments to the recipient-year level. We opt for a regression approach in which we predict aid bilaterally from the best linear combination of the two interacted instruments and then aggregate the bilateral predictions. Specifically, we predict aid from donor j to recipient i in year t in a bilateral regression:

$$a_{3ijt} = \theta_0 g_{3jt} + \theta_1(g_{3jt} \times \bar{p}_{3ij}) + \xi_0 l_{3jt} + \xi_1(l_{3jt} \times \bar{p}_{3ij}) + \mu_{3ij} + \lambda_{3t} + \varepsilon_{3ijt} \quad , \quad (3\text{-}10)$$

where g_{3jt} is government fractionalization, l_{3jt} legislative fractionalization and $\bar{p}_{3ij}$ is the pairwise probability of receiving aid. As discussed above g_{3jt} is typically zero in FPTP systems. For an identification consistent with our theoretical framework we set all FPTP observations of $g_{3jt} = 0$. Analogously, we set $l_{3jt} = 0$ in non-FPTP systems. Hence, we utilize only the system-relevant political fractionalization. The time-invariant probability is defined as $\bar{p}_{3ij} = \frac{1}{T}\sum_t^T \mathbf{1}[a_{3ijt} > 0]$, so that it contains the fraction of years in which recipient i received a positive amount of aid from donor j. We again added subscripts to indicate that this equation (3) precedes the others with index (2) and (1). We do not need to control for the endogenous level of $\bar{p}_{3ij}$ as it is captured by the recipient-donor fixed effects, μ_{3ij}. We then aggregate the predicted bilateral aid from eq. (3-10) across all donors in order to get predicted aid as a share of GDP at the recipient-year level. Hence, $\hat{a}_{2it} = \sum_j \hat{a}_{3ijt}$ is the instrument in eq. (3-7).

We may worry about what variation actually ends up in our constructed instrument. To be clear, it consists of three different components: *i)* the estimated donor-recipient fixed effects aggregated over all donors, or $\sum_j \hat{\mu}_{3ij}$, *ii)* the estimated effects of those donor characteristics that do not vary across recipients and the time dummies aggregated over all donors, or $\sum_j \hat{\theta}_0 g_{3jt} + \sum_j \hat{\xi}_0 l_{3jt} + J\hat{\lambda}_{3t}$, and, finally, *iii)* the exogenous variation introduced by the two interaction terms aggregated over all donors, or $\sum_j \hat{\theta}_1(g_{3jt} \times \bar{p}_{3ij}) + \sum_j \hat{\xi}_1(l_{3jt} \times \bar{p}_{3ij})$. The first two are potentially endogenous, but we control for their influence in the estimation that follows. Donor fractionalization is the same across all recipients and will be swept out by the fixed effects (or time-averages) in the reduced form equation. Similarly, everything but the interaction terms will be swept out by the recipient effects and time effects.

Consider the influence of colonial ties for example. If a former colony receives more aid from its former colonizer, then this will be captured by a higher donor-recipient fixed effect and a higher probability to receive aid. Moreover, former colonizers may be more likely to intervene and act as "peacemakers." Both issues are no threat to our identification strategy, since these level effects are absorbed at the various stages. Our exclusion restriction would only be violated if a change in the political fractionalization of a former colonizer would lead to a different change in aid flows given to regular recipients as opposed to irregular recipients *and* this change in fractionalization would make the former colonizer more likely to intervene in one of these two groups. However, even this concern is mitigated by our exclusive focus on internal civil conflicts.

3-5 Results

3-5.1 Bilateral Estimation

We begin by briefly discussing the bilateral regression which we use to construct the instrument. Recall that we regress aid received by each recipient from a particular donor on political fractionalization, its interaction with the probability of receiving aid, and a full set of country and time fixed effects. We estimate these models with the fraction of aid in GDP as the dependent variable (not in logs, since negative flows occur when loan repayments exceed new inflows).

The regression is estimated over 4,116 bilateral donor-recipient relations for which we have data, yielding a total of 129,348 observations.[9] These results are not intended to be interpreted causally on their own. They purely serve to "translate" the exogenous variation in donor characteristics into changes in aid disbursements at the recipient level, depending on how strongly a recipient depends on aid from each particular donor.

The estimated coefficients of our variables of interest are as follows (standard errors are reported in parentheses below):

$$\hat{a}_{3ijt} = \cdots - \genfrac{}{}{0pt}{}{0.043}{0(0.014)} g_{3jt} + \genfrac{}{}{0pt}{}{0.227}{0(0.058)} (g_{3jt} \times \bar{p}_{3ij}) + \genfrac{}{}{0pt}{}{2.564}{0(1.407)} l_{3jt} - \genfrac{}{}{0pt}{}{2.936}{0(1.426)} (l_{3jt} \times \bar{p}_{3ij}). \tag{3-11}$$

The coefficients on the interaction terms are highly significant. Note that the negative sign on the second interaction coefficient is misleading. In both cases, increasing political fractionalization leads to more aid disbursements for nearly all of the sample. Interestingly, fractionalized parliamentary systems give more aid to regular recipients, whereas divided majoritarian systems give more aid to irregular recipients (which is in line with the result in Ahmed, 2016, for the case of the U.S.).[10]

The effects of political fractionalization are not as large as a cursory glance at the coefficients may suggest. To see this, consider a 10 percentage points increase of political fractionalization in a donor country when a recipient receives aid about two thirds of the time. Eq. (3-11) predicts that this increases the aid to GDP ratio by about 0.01 percentage point for aid from proportional systems ($0.1\times[-0.043+0.227\times 2/3] \approx 0.01$) and about 0.06 percentage points for aid from majoritarian systems ($0.1\times[2.564-2.936\times 2/3] \approx 0.06$). The increase in majoritarian systems tends to be larger, in part because it is estimated based solely on three of the biggest donors. We clustered standard errors at the donor-recipient level. The cluster-robust F-statistic of the interaction terms is about 10.83. Note that the constructed instrument will turn out to be considerably stronger once we aggregate to the country level, since we then add up many of these small changes in the aid to GDP ratio of recipients

[9] We do not constrain this estimation to the balanced sample we use later on for two reasons: *i)* in order to get the best possible estimate of this relationship, and *ii)* unbalancedness is not a problem in fixed effects regressions as long as selection is ignorable.

[10] An explanation could be that government fractionalization works mainly via its effect on the general budget and hence affects the volume of receipts of regular beneficiaries, while legislative fractionalization (e.g., divided government in the U.S.) results in amendments to the budget. The parties negotiating these amendments are likely to have different preferences over which countries should receive aid.

in any given year.[11]

3-5.2 Reduced Form of Aid

We now turn to country level estimates of the first stage relationship. Table 3-2 shows three reduced form regressions for aid to GDP which we obtain by estimating the equivalent fixed effects model of eq. (3-7). The residuals from these models are used as control functions in the main specifications which we estimate further below. The sample is now balanced at $T = 36$ (minus the initial period) and $N = 125$. This constitutes a much larger sample relative to the typical study in this field which often focuses exclusively on Sub-Saharan Africa or loses observations due to the inclusion of many controls. Our data contains countries experiencing some of the most severe and longest-running civil conflicts (e.g., Afghanistan, Iraq, Pakistan and many more).

Table 3-2 – First Stage Regressions with Generated IV

	Dependent Variable: Aid to GDP		
VARIABLES	(1)	(2)	(3)
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	1.352***	1.234***	1.233***
	(0.088)	(0.067)	(0.068)
Selected Controls			
Log GDP per capita		-5.089***	
		(0.845)	
Log GDP			-5.114***
			(0.806)
Log Population			6.084***
			(2.306)
Additional Controls			
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Summary Statistics			
Kleibergen-Paap F-statistic IV	233.5	336.2	331
$N \times T$	4375	4375	4375
T	35	35	35
N	125	125	125
Within-R^2	0.0412	0.0739	0.0763

Notes: The table shows the results of first stage regressions using a linear two-way fixed effects model. The instrument is the sum of predicted bilateral aid over all donors ($\sum_j \hat{a}_{3ijt}$) from eq. (3-11). Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[11] We repeated this estimation using net aid including Other Official Flows (OOF). The results are qualitatively and statistically similar (not reported, available on request).

Two things stand out in Table 3-2. First, the estimated coefficients on the instruments in all columns are always larger than one. Depending on the specification, a one percentage point increase in the predicted aid to GDP ratio leads to about a 1.3 percentage point increase in actual aid to GDP. Adding other controls moves the estimated coefficients a bit closer to unity. The size of the coefficient is not unusual. Related approaches in the trade and migration literature often yield coefficients that are sometimes below or near unity (Frankel and Romer, 1999) and sometimes considerably larger (Alesina et al., 2016). If the constructed instrument over-predicts the quantity in question, then the coefficient will be below unity, and *vice versa*. Not surprisingly, our aggregation of predicted bilateral flows tends to undershoot actual aid to GDP ratios and therefore has a multiplier above unity. Second, the aggregated instrument is highly relevant. The cluster-robust F-statistics always exceed the conventional level of about ten by an order of magnitude, which is also not unusual in comparable applications.[12] Hence, it seems safe to conclude that aggregating many small changes in aid induced by electoral outcomes in donor countries interacted with the probability of receiving aid constitutes a powerful instrument of development aid.

No single donor or recipient is driving this result. Two graphs in Appendix A report the regression coefficients and the confidence intervals we obtain when we drop each donor (Figure 3-A1) or each recipient (Figure 3-A2) one at a time in the bilateral sample, aggregate the data to the country-level, and re-run the first stage regression. The estimates vary only within an extremely narrow band. A similar question regarding the strength of our instrument is whether this association is driven mainly by recipients with a highly fragmented donor pool. The variation of aid induced by changes in divided donor governments is likely to be higher for recipients with many active donors. To investigate this, we measure donor fragmentation by a Herfindahl index and the combined share of the three largest donors. We then interact predicted aid to GDP with a dummy indicating whether the recipient has a higher donor fragmentation than the sample mean. The coefficients on predicted aid to GDP and the first stage F-statistics are qualitatively similar to those in Table 3-2. The interaction term itself is insignificant, irrespective of whether we use the Herfindahl or the share of the three largest donors. Hence, our instrument does not draw its power from any one donor, any one recipient, or settings where many donors are active at the same time.

A number of other concerns could be raised regarding the strength and validity of our identification strategy. Fractionalized governments and legislatures could be giving more aid to countries that are politically closer, more open to trade or that receive a lot of foreign direct investment. Any (conditional) correlation of our instrument with these variables might weaken the strength of our instrument and could violate the exclusion restriction in some circumstances. However, note that a violation of the exclusion restriction requires not only that fractionalization-induced aid disbursements vary in tandem with other variables and that these variables determine conflict, it also requires that these other variables have heterogeneous effects on regular and irregular aid recipients.[13]

[12] Without added controls Frankel and Romer (1999) report an F-statistic of 98.01 for their predicted trade shares. In a completely different context, Gordon (2004) reports F-statistics up to 291 when instrumenting actual changes in Title 1 spending per pupil in U.S. districts with constructed values.

[13] Other factors, such as global economic crises, may both depress aid and lead to more

Table 3-A4 in Appendix A includes UNGA voting alignment (based on ideal points as in Bailey et al., 2017), trade openness, and FDI inflows over GDP as additional controls into the first stage regressions. We now limit the sample to the subset of countries that is covered by the added variables. Column (1) re-estimates our base specification from above. Columns (2) to (4) progressively add the additional controls. The last column includes all added controls. The strength of our instrument is virtually unaffected. The F-statistic of the instrument varies between 30 to 70. Likewise, the estimated coefficients of predicted aid are very stable around 1.3. Closer voting alignment and more openness increase aid flows, while the coefficient on FDI flows is not significant at conventional levels. Adding all variables increases the model fit by about six percentage points. While these measures clearly matter for aid allocation, they do not capture the exogenous variation that is contained in our instrument.

3-5.3 Baseline Results

We focus on a basic set of controls in our main specifications but allow for (fixed) unobserved country heterogeneity, unobserved time effects, and instrument our time-varying variable of interest. All of these three measures take care of omitted variables and contemporaneous endogeneity. We present two sets of estimates for our baseline results. Table 3-3 reports the regression results and Table 3-4 shows the associated average partial effects of aid on different transitions.

Consider the regressions in Table 3-3 first. In column (1) we show the estimates without additional controls, next we add GDP per capita, and then we allow GDP and population to have different effects in the last column. The results are interesting in a couple of respects. The coefficients of aid to GDP and its interactions with the lagged states are virtually the same across all three specifications (even though the underlying scale factors differ). The regressions suggest *i)* that the intensifying effect of aid on conflict is stronger if the country experienced a small conflict in the year before, and *ii)* that the effect is not statistically different from the base level (i.e., peace in the previous year) for higher conflict intensities. We also find reasonably strong evidence of the endogeneity of aid. The residuals from the first stage have the opposite signs and similar magnitudes as the coefficients on the base level. This suggests that we would find no evidence of an effect of aid on conflict, if we would not correct for endogeneity (this is indeed the case). In control function methods, testing the null that the coefficient on the residuals is zero corresponds to a Hausman test of endogeneity which does not depend on the first stage, hence the reported bootstrap standard errors will be conservative. Nevertheless, we can reject the null of endogeneity at the 10% significance level.

We prefer column (3) since it accounts for scale effects (conflicts with more battle-related deaths occur in larger countries) and measures the net effect of higher aid intensity at a given income level. Nevertheless, none of the coefficients on the selected time varying controls are significant. Most existing studies use pooled methods (including the sensitivity analysis by Hegre and Sambanis, 2006) which rely on between-country differences. Given that recipient level CREs and conflict histories are included in all of our specifications, log GDP (whether per capita or

fragmented governments in rich countries. However, if these factors uniformly affect all recipients in a given year, they are captured by the time effects.

Table 3-3 – Second Stage Ordered Probit Regressions, CRE and CF

	Dependent Variable: Ordered Conflict		
VARIABLES	(1)	(2)	(3)
Aid to GDP (a_{2it})	0.0728*	0.0729	0.0721
	(0.0432)	(0.0491)	(0.0468)
Residuals ($\hat{\nu}_{2it}$)	-0.0847*	-0.0865*	-0.0863*
	(0.0442)	(0.0501)	(0.0480)
Interactions with Lagged States			
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0220***	0.0209**	0.0212**
	(0.00792)	(0.00841)	(0.00866)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.00843	-0.0104	-0.0106
	(0.0187)	(0.0191)	(0.0191)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00229	-0.00139	-0.00229
	(0.0240)	(0.0252)	(0.0248)
Lagged States			
Small Conflict ($h_{1,i,t-1}$)	0.582***	0.578***	0.576***
	(0.0744)	(0.0752)	(0.0794)
Armed Conflict ($h_{2,i,t-1}$)	2.110***	2.098***	2.107***
	(0.181)	(0.185)	(0.190)
Civil War ($h_{3,i,t-1}$)	3.429***	3.406***	3.424***
	(0.227)	(0.230)	(0.241)
Selected Controls			
Log GDP per capita		0.253	
		(0.339)	
Log GDP			0.289
			(0.310)
Log Population			-0.0478
			(0.509)
Additional Controls			
Recipient CRE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Residual CRE	Yes	Yes	Yes
Initial States	Yes	Yes	Yes
Summary Statistics			
$N \times T$	4375	4375	4375
T	35	35	35
N	125	125	125

Notes: The table shows the results of an ordered probit model with correlated random effects and a control function approach. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

not) and log population do not seem to contribute much additional information. Note that we defer the discussion of the lagged states to the next subsection where we analyze the persistence and duration of conflicts at various intensities.

We have strong reasons to trust the estimates presented in Table 3-3. We allow for quasi-fixed effects, first-order multi-state dynamics, and correct for contemporaneous heterogeneity. In theory, additional controls may help justifying the identifying assumptions regarding the instrument but there is no *ex ante* reason to expect that our estimates are still biased. Including more variables also comes at a cost as we described earlier. Each additional variable consumes several degrees of freedom due to how the unobserved heterogeneity is modeled. We return to the issue of additional controls in the robustness section.

Table 3-4 – Average Partial Effect of Aid on Transition Probabilities

	To State			
From State	Peace	Small Conflict	Armed Conflict	Civil War
Peace	-1.639	1.154	0.475	0.010
	(1.056)	(0.743)	(0.317)	(0.009)
Small	-2.867**	1.439**	1.358**	0.070
Conflict	(1.359)	(0.701)	(0.646)	(0.048)
Armed	-1.379	-0.539	1.333	0.585
Conflict	(1.174)	(0.474)	(1.099)	(0.498)
Civil War	-0.401	-0.970	-0.618	1.989
	(0.387)	(0.734)	(0.551)	(1.494)

Notes: Based on column (3) in Table 3-3. Panel bootstrap standard errors in parentheses, computed with 200 replications. Rows sum to zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To assess the magnitude of the implied effects we have to turn to partial effects as opposed to estimated coefficients. Table 3-4 reports estimates of APEs for a one percentage point change in aid on the various transition probabilities (see eq. (3-3) in Section 3-4). Note that – by definition – each row sums to zero. Although all estimates above the diagonal are positive and those below negative, we find no statistically significant evidence in favor of an effect of aid on conflict when countries are entirely at peace or engaged in a conflict with more than 25 BDs.

Aid has significant adverse effects in volatile environments which are not entirely peaceful but also not (yet) fully engaged in armed conflict. There, more aid makes peace less likely, but a continuation of small conflict and a transition to armed conflict more likely. A one percentage point increase in the ratio of foreign aid to GDP leads to about a 1.4 percentage point increase in the probability of transitioning from small conflict to armed conflict.[14] The same increase in aid also significantly increases the likelihood of remaining in a small conflict (by about 1.4 percentage points) and makes a transition to peace much less likely (about -2.9 percentage points).[15]

[14] We might be concerned that the effect of aid on the transition from small conflict to armed conflict is driven by a small subset of observations. However, there are about 50 switches behind this estimate and more than 300 observations behind each of two lower switches.

[15] The size of the estimated effects are also in line with recent estimates by Besley and Persson

The size of this effect is best understood in conjunction with a typical change in aid flows. The average aid to GDP ratio in our sample is about 5% and the within standard deviation is also close to 5% (when we exclude recipients who receive more than half their GDP in foreign aid, e.g., Liberia 2008, Palau 1994, 1995). Mali, for example, experienced a one standard deviation increase in its aid to GDP ratio in 1994 when the share of aid to GDP increased from about 8% to 13%. At the same time, there was an escalation from small conflict to armed conflict. Consistent with this observation, our model predicts an increase in the probability of transitioning from small conflict to armed conflict of about 7 percentage points. Aid increases of this magnitude are rare. Only in about 3% of the sample they exceed five percentage points but changes around one percentage point are more common (about 14% of the sample). In Uganda, for example, aid increased by about one percentage point on two occasions (1981 and 2002). In both cases, the country experienced an escalation of conflict.

3-5.4 Persistence, State-Dependence and Duration

Table 3-5 shows the average transition probabilities as they are predicted by our preferred specification.[16] The diagonal of this matrix shows the predicted persistence rates and the off-diagonal elements are the escalation and deescalation probabilities, respectively. Note that we define persistence and continuation in analogy, so that persistence is simply the estimated probability of remaining in a particular state. The matrix provides nearly all the terms needed to estimate state dependence as in eq. (3-4) apart from the weights. Recall that state dependence measures the effect of the state on itself after accounting for observed and unobserved differences in the population (e.g., the destructive effects of unemployment, after netting out that the unemployed may have different characteristics than the employed).[17] It is conceptually distinct from persistence which, in theory, could be entirely driven by observed and unobserved characteristics.

We find strong evidence of state dependence in each of the four states, even after controlling for observed and unobserved heterogeneity. The bootstrapped standard errors are many times smaller than the estimated effects of each state. State dependence in armed conflict and civil war is moderately high and very similar (we cannot reject the null that these two estimates are the same). For both types of conflict, the sheer fact that a country finds itself in conflict implies that the probability of remaining in conflict rises by about 30 percentage points. Comparing these estimates with the persistence probabilities shown on the diagonal is particularly instructive. State dependence accounts for the bulk of persistence in armed conflict and civil war, but much less so in small conflict and peace. Note that the literature typically combines armed conflicts and civil war which would increase our estimates of persistence (and probably also of state-dependence) in the combined state.

(2011b), Crost et al. (2014), and Nunn and Qian (2014). However, De Ree and Nillesen (2009) find that an increase in aid flows by 10% decreases the probability of continuation of conflict by about eight percentage points.

[16] Table 3-5 can be directly compared to the observed data shown in Table 3-1 and the difference between these two is a basic measure of goodness of fit.

[17] The literature typically distinguishes between three sources of state dependence: heterogeneity, serial correlation, and true state dependence.

Table 3-5 – Estimated Transition Probabilities and State Dependence

	To State			
From State	Peace	Small Conflict	Armed Conflict	Civil War
Peace	79.954***	16.344***	3.657***	0.045*
	(1.902)	(1.536)	(0.739)	(0.024)
Small Conflict	61.751***	27.463***	10.496***	0.290**
	(2.857)	(2.293)	(1.454)	(0.126)
Armed Conflict	21.783***	32.690***	39.749***	5.778***
	(4.412)	(2.268)	(4.388)	(1.246)
Civil War	3.485	13.835***	51.102***	31.578***
	(2.215)	(3.186)	(3.173)	(4.941)
State Dependence	40.794***	8.890***	32.380***	30.765***
	(2.693)	(1.635)	(4.326)	(4.872)

Notes: Based on column (3) in Table 3-3. Panel bootstrap standard errors in parentheses, computed with 200 replications. The upper four rows sum to 100%. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Taking a truly dynamic approach allows us to bridge another distinction that is often drawn in the conflict literature: event models versus duration models. First-order Markov models can be compared to discrete time duration models with a constant hazard rate (e.g., Cappellari and Jenkins, 2004).[18] The expected duration of peace is about five years. Most conflicts are relatively short-lived on average. Small conflicts last about 1.4 years, armed conflict about 1.7 years, and civil wars about 1.5 years. We are predicting conflicts that last longer than three years only after about the 95th percentile (and longer than five years after the 99th percentile). This may seem short compared to other findings in the literature but it is worth bearing in mind that we distinguish between different types of conflict that are often lumped together. A conflict cycle that goes from small over armed conflict to outright civil war and back is perfectly compatible with the duration typically found in the literature (e.g., Collier and Hoeffler, 2004b).[19]

3-5.5 Identification Assumptions and Falsification

Our local average partial effect compares the effects of politically induced differences in bilateral aid between regular and irregular aid recipients. This raises the question whether the parallel trends assumption inherent in DiD approaches is satisfied, or if spurious non-linear trends are at work. Our identification strategy is only valid if the heterogeneous response to the instrument generated by the regularity of aid

[18]To see the equivalence, recall that the hazard rate is the probability that the current state will end, or $\Pr(T_i = t | T_i \geq t)$. A discrete time-homogeneous Markov chain has a constant hazard rate with a well defined expectation. The probability of exiting a particular state is geometrically distributed with $\Pr[T_i = t] = p_{ii}^{t-1}(1 - p_{ii})$. The expected survival time in state i is simply $\mathbb{E}[T_i] = 1/(1 - p_{ii})$ and the quantile function is $Q(r) = F^{-1}(r) = \ln(1 - r)/\ln(p_{ii})$ where r is the percentile of interest.

[19]Also note that our estimates under-predict persistence relative to the observed data, in part because we average out the effects of observed and unobserved heterogeneity.

receipts is constant over time. This is not a concern in our case, although it is an issue in related work.[20]

Figure 3-A3 in Appendix A shows the time series of our political measures in the upper panel, and the time series of conflict and the aid to GDP ratios split by quartiles of the probability to receive aid. While our measures of political fractionalization, bilateral aid, and conflict are trending up towards the middle of the studied period, the trends are remarkably parallel at the different levels of aid dependency. Only non-linear trends of aid and conflict in highly aid-dependent countries which coincide with trends in donor fractionalization would be a threat to our identification strategy. Such trends are absent in our data, while the common trends are absorbed by the time dummies.

We also conduct several placebo tests. Our finding that aid leads to an escalation of conflict rests on the coincident timing of politically-induced aid flows and the observed conflict histories. Randomizing aid flows along various dimensions allows us to break this temporal structure. As an added advantage we also obtain Monte Carlo p-values for our coefficients of interest. We shuffle the data along four dimensions. We randomly re-assign the aid to GDP ratio by exchanging i) all observations in the sample, ii) the entire time series between countries, iii) years within countries, and iv) countries within years. The rest of the data are left unchanged. Note that we ignore the first stage variability. Randomizing this stage as well would introduce weak IV bias and lead to non-central distributions. All of the four randomizations break our causal chain. The aid flows of another country, time period, or both cannot possibly have caused the observed conflict but spurious trends along particular dimensions would persist.

Figure 3-A4 reports the results from 5,000 Monte Carlo simulations per randomization strategy. For each placebo test, we report the distribution of the coefficients on the interaction terms. The results are unambiguous. Our findings are not driven by global trends, cross-sectional dependence, or selection of countries into regular aid receipts. Our estimated coefficients on the interaction of aid with small conflict are far to the right of simulated distributions, with exact p-values that are considerably smaller than 0.05. Consistent with our main results, we find no evidence that the effect of aid is different in societies that experienced an armed conflict or a civil war in the preceding year.

3-6 Extensions

We present a number of extensions which subject our main findings to several robustness checks and perturbations. First, we compare the ordered probit estimator to standard linear models. Second, we examine the sensitivity of our results to the underlying definition of the key variables. Third, we include a variety of additional controls. Finally, we examine the role of multilateral and humanitarian aid. We only briefly survey the results; all corresponding tables are relegated to Appendix A.

[20]See Christian and Barret (2017) who show that non-linear trends could be driving the positive effect of food aid on conflict reported in Nunn and Qian (2014).

3-6.1 Linear Estimation

The proposed dynamic ordered probit model is reasonably demanding to estimate and one might be concerned that our findings are driven by the structure we impose on the data ('identification by functional form'). Table 3-A5 addresses this issue. Here we ignore the ordinal nature and estimate our base specification using different linear approaches. Recall that least squares is not suitable for ordinal outcomes if the number of outcomes is not large and the error distribution is not approximately normal, among other issues.

All first order effects of aid on conflict are similar to the non-linear models. Column (1) in Table 3-A5 shows that, just as in the non-linear models, we find no effect if we estimate the fixed effects OLS counterpart to our dynamic specification when ignoring the endogeneity of aid. Column (2) then uses a control function approach to correct for the endogeneity of aid and recovers a positive first order effect of aid on all conflict outcomes. Column (3) illustrates the well-known equivalence of control function (CF) and instrumental variables (IV) approaches.[21]

The models with interaction terms confirm our initial findings. As columns (5) and (6) show, once we correct for the endogeneity of aid, the estimated coefficient is positive and significant. The coefficients on the three interaction terms are numerically similar, no matter if we use the control function estimator or not. In column (6), when we use a standard IV approach, the interaction effects become much less precisely estimated while the signs and magnitudes are broadly stable. The CF estimator requires only one first stage estimation to correct for popular transformations (such as squares or interactions) of the endogenous variable. The IV estimator instead requires us to generate many additional instruments to run as many additional first stage regression as we have interaction terms. As a result, the IV estimator is much less efficient but imposes fewer assumptions (Wooldridge, 2010, pp. 128–129). Given the stability of the estimated coefficients, this difference appears to be immaterial in our case.

3-6.2 Definition of Variables

We now turn to the sensitivity of our results with respect to the operationalization of our key variables. In Table 3-A6 we alter the construction of our conflict and aid measures. Column (1) addresses the potential concern that while our newly developed measure is a step forward, we might not have gone far enough. One type of violence which we have so far neglected is terrorism. We now include country-year observations with a positive number of terror attacks[22] but less than 25 BD in the category one (small scale conflict) of our ordinal measure. In column (2) we combine categories two and three, since several studies only distinguish between peaceful countries and countries with more than 25 BDs. In both cases the results are qualitatively similar to our main findings.

Next, we compare our approach to the 'industry standard', where peace and small conflict are combined in one category. This eliminates the possibility to distinguish between truly peaceful countries and countries that experience small conflict. In

[21]In static models CF and IV approaches yield numerically identical results. However, here we specify the first stage of the CF estimator without controlling for the lagged states.

[22]From START (National Consortium for the Study of Terrorism and Responses to Terrorism) (2016).

line with our expectations, neither the level estimates nor the interaction effects are statistically significant in column (3). This is also true for the APEs.

Column (4) changes the definition of aid. So far, we have only focused on ODA. Here we include OOF to capture a broader concept of financial inflows from abroad, which does not affect our results. In columns (5) and (6) we exclude Canada, the UK and the U.S. We do so for two reasons. First, for those three countries we use legislative fractionalization rather than government fractionalization as an IV for bilateral aid. In order to rule out that our results depend on this choice, we estimate our preferred specification for the remaining 25 DAC donors. Second, these three donors could differ from the rest of the DAC donors in how they disburse aid to countries in conflict (e.g., if they are important to the U.S.).[23] Column (5) uses ODA, while Column (6) uses ODA with OOF. In each case, the estimated coefficients and APEs are in line with our preferred specification.

Last but not least, we code variants of the small conflict category by excluding one of the constituting variables each time (e.g., riots, assassinations). Our results are not driven by one single dimension of small conflict. As Table 3-A7 shows, we obtain quantitatively identical results for all four perturbations.

3-6.3 Additional Controls

In Table 3-A8 we extend the set of control variables. Column (1) examines influence of conflict in the immediate regional neighborhood. We find little evidence of spillover effects, although such peer effects are generally difficult to identify. Columns (2) to (5) examine if political institutions affect the link between aid and conflict. This comes at the cost of a reduced sample.[24] None of the political variables alter our main results. Column (6) shows that GDP growth makes conflict less likely but does not affect the relationship between aid and conflict.

We strongly prefer our baseline estimates with country and time effects over the results reported in Table 3-A8. Many of the added variables can be considered "bad controls" in the sense that they themselves could be outcomes of development aid. As cases in point, political instability, classification as a democracy, or GDP growth have all been causally linked to aid in the past. The inclusion of outcomes on the right hand side creates a selection problem which can completely distort the estimated causal effect.[25]

3-6.4 Multilateral, Humanitarian, and Food Aid

Multilateral aid is typically a bit less than one third of all aid. To estimate its influence, we first calculate the correlation of multilateral aid as a share of GDP with aggregated predicted aid to GDP (our instrument) and then the correlation

[23]Our second stage results are also not driven by individual recipients.

[24]The Polity IV score is not available for cases of foreign "interruption" (code -66) and lacks data for island countries. We lose, e.g., Afghanistan, Iraq, Cambodia, and Lebanon.

[25]See Angrist and Pischke (2008) for a discussion of this problem. A similar reasoning could be used to prefer the short specification in column (1) of Table 3-3 over the other two columns. Note that the inclusion of log GDP and log population hardly makes a difference in the estimates and both variables have insignificant coefficients, so that this distinction is immaterial for our main results.

with the part of our instrument that is solely driven by exogenous variation.[26] The correlation of multilateral aid to GDP with aggregated predicted aid to GDP is 0.46, but falls to 0.05 when the exogenous component is isolated. Hence we conclude that multilateral aid is certainly important and correlates with bilateral aid but not with our identifying variation.

We now consider the role of humanitarian and – its main component – food aid. Although humanitarian aid protects vulnerable populations, it is also easily captured by rebel groups and thus directly affects the opportunity costs of fighting. Humanitarian aid represents about 6.5% of overall aid in our sample. Here too the partial correlation of the exogenous component of predicted bilateral aid with humanitarian aid is close to zero (0.02), suggesting that our results are not driven by (unobserved) humanitarian aid.

Next, we analyze if the effect of U.S. food aid differs from the results of overall aid presented here. Table 3-A9 presents the results of simple replication and modification exercises using the data from Nunn and Qian (2014). Column (1) shows that our results are qualitatively similar in the matched sample of 103 recipient countries over the period from 1975 to 2007. In column (2), we then replicate a version of their main specification, where U.S. food aid is instrumented with U.S. wheat production interacted with the probability of receiving U.S. aid. However, we exchange their binary conflict indicator with our ordinal measure of conflict and include the appropriate interactions.[27] In line with their results, we find that U.S. food aid increases the probability of conflict across the board. Column (3) then removes the top category from our dependent variable. This hardly affects our conclusions.

Last but not least, we conduct a falsification test to figure out if the identifying variation overlaps between our estimates of the effect of total ODA and the established effect of U.S. food aid. This should not be the case. Donor fractionalization of the 28 DAC donor countries should not predict U.S. food aid. Likewise, wheat production in the U.S. should not predict total ODA disbursed by the 28 DAC donors, but only a small part of U.S. overall aid. Table 3-A10 shows that this is reflected in the data. Hence, our primary finding that bilateral development aid promotes the continuation of small conflicts and an escalation of small to armed conflicts is quite different from the local average partial effect of U.S. food aid highlighted previously.

3-7 Concluding Remarks

This chapter studies the effects of development aid on conflict. While there is a large literature on the topic, it typically separates the onset of a conflict from its continuation and neglects smaller acts of violence. This misses important dynamics which our chapter makes an effort to expose.

Our results show that the effects of bilateral aid are heterogeneous with respect to the different intensity levels of conflict. Whereas aid increases the probability

[26] We regress our instrument on a full set of time and country fixed effects, and obtain the residual.

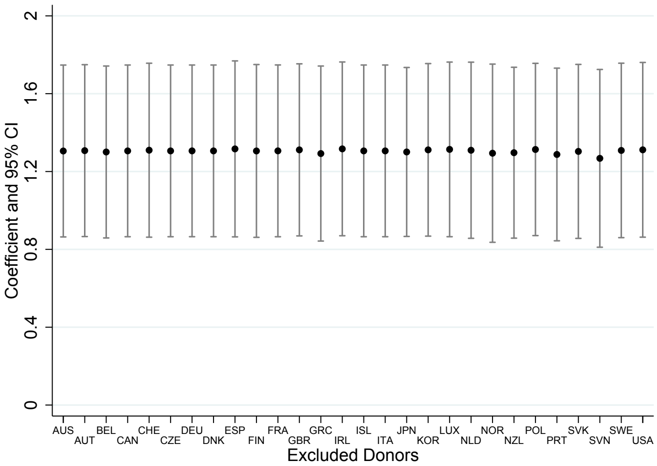
[27] Note that our framework does not allow for the large set of controls used in Nunn and Qian (2014). However, the corresponding OLS coefficients only vary in a narrow band, no matter if we specify the original long regression or the short regression (as in column (2) of Table 3-A9).

that a conflict escalates from a low level of political violence to armed conflict, we find little evidence in favor of an adverse effect of aid in truly peaceful countries. Aid does also not seem to affect the transition probabilities once a country experiences armed conflict or civil war. These results underline the importance of separating truly peaceful situations from countries exposed to small conflict. If we do not account for this distinction, we would fail to detect an effect of aid on conflict.

These findings call for care when devising aid policies for countries affected by conflict. Particular care has to be exercised when aid is given to countries where turmoil is already present but armed conflict has not yet erupted. Our results suggest that aid might be more harmful than helpful in these situations, despite best intentions. Our analysis focuses on overall official development assistance. Future research could examine what type of assistance can be given to countries with persistent low-intensity conflicts so as to actually foster peace. Achieving this goal will require more research on the exact channels at play.

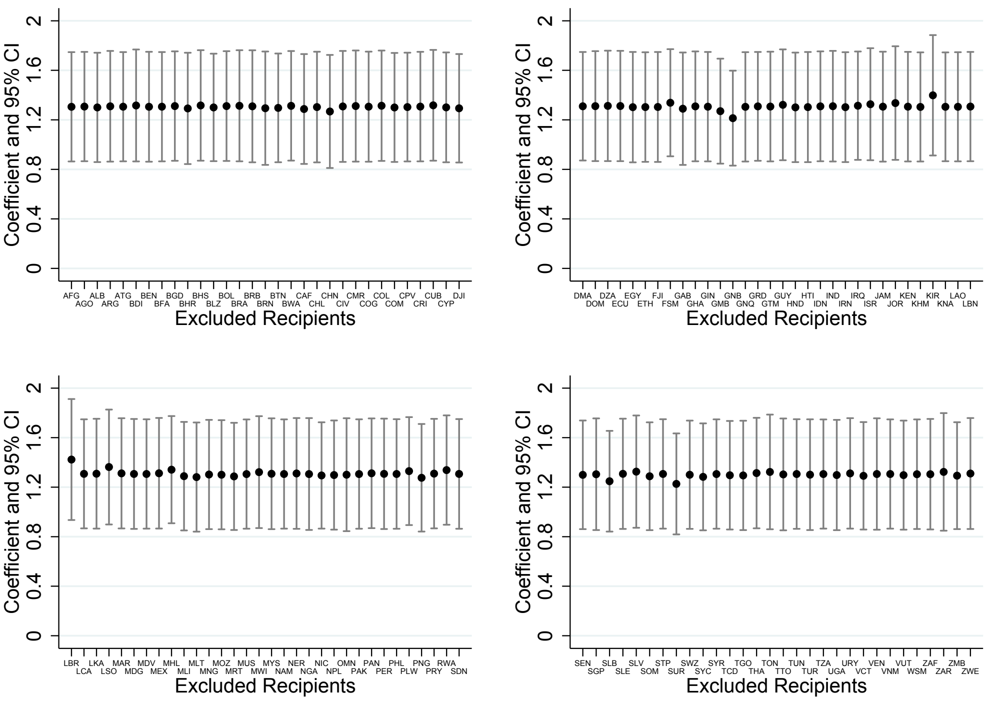
Appendix A:

Figure 3-A1 – Leave-One-Out Test: Donors



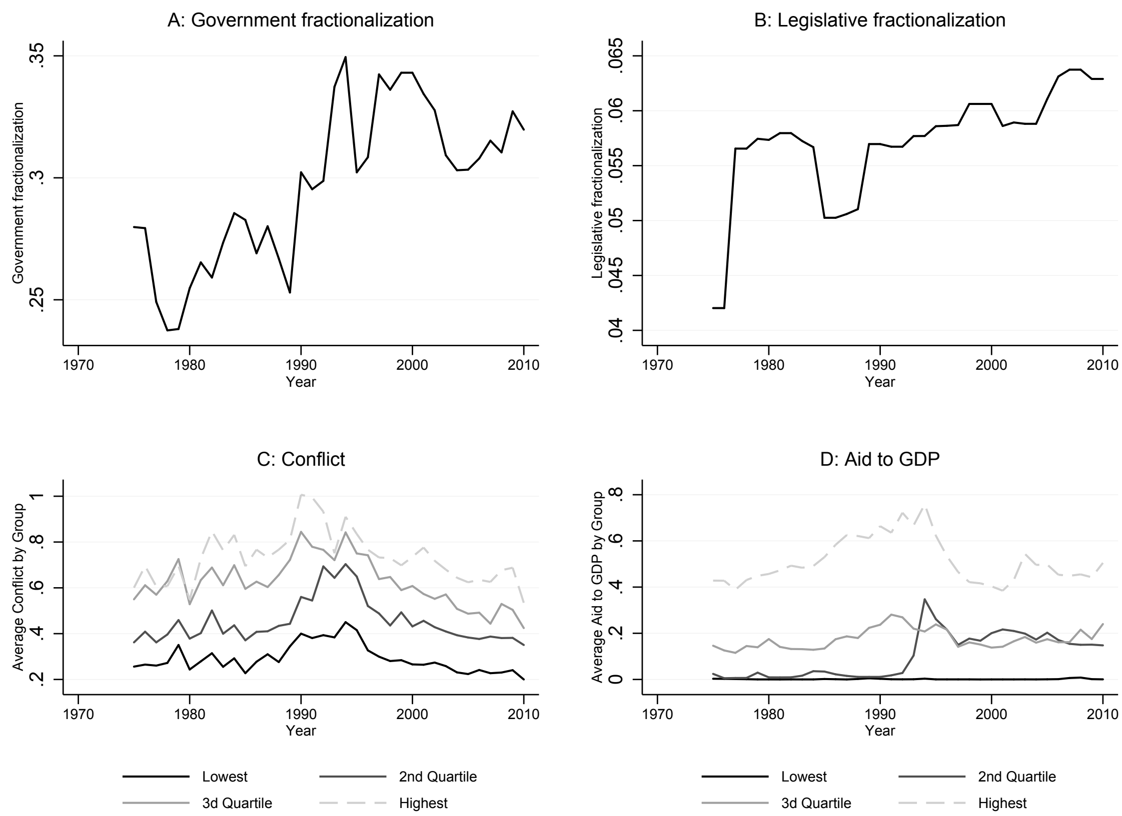
Notes: Each point in the figure represents the result of a regression of actual on predicted aid shares where one of the DAC donors has been excluded from the bilateral sample. Cluster robust standard errors are provided as error bars.

Figure 3-A2 – Leave-One-Out Test: Recipients



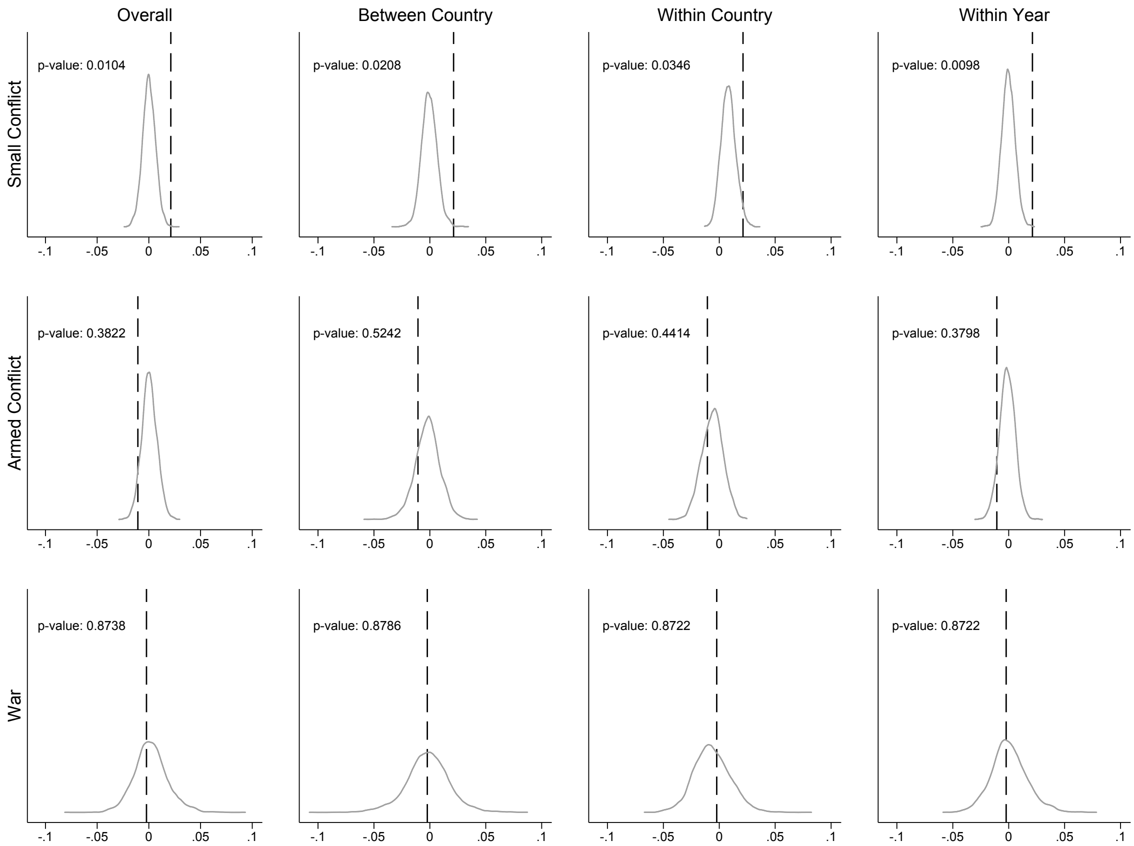
Notes: Each point in the figure represents the result of a regression of actual on predicted aid shares where one of the recipients has been excluded from the bilateral sample. Cluster robust standard errors are provided as error bars.

Figure 3-A3 – Parallel Trends



Notes: The figures shows the time series of average government fractionalization of donors (panel a), average legislative fractionalization of donors (panel b), conflict in recipient countries grouped by their probability to receive aid (panel c), and average aid to GDP ratios in recipient countries grouped by their probability to receive aid (panel d). Conflict measures the probability of experiencing any type of conflict (ranging from small conflict to civil war).

Figure 3-A4 – Randomization Test



Notes: The figure shows the distribution of point coefficients based on 5,000 Monte Carlo replications per randomization strategy as described in the text. The p-values are estimated as the proportion of times that the absolute value of the t-statistics in replication data exceed the absolute value of the original t-statistic.

Table 3-A1 – Included Donor Countries, in Alphabetical Order

Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, United Kingdom, United States.

Table 3-A2 – Included Recipient Countries, in Alphabetical Order

Afghanistan, Albania, Algeria, Angola, Antigua and Barbuda, Argentina, Bahamas, Bahrain, Bangladesh, Barbados, Belize, Benin, Bhutan, Bolivia, Botswana, Brazil, Brunei Darussalam, Burkina Faso, Burundi, Cambodia, Cameroon, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Congo, Costa Rica, Cuba, Cyprus, Democratic Republic of Congo, Djibouti, Dominica, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Ethiopia, Fiji, Gabon, Gambia, Ghana, Grenada, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, India, Indonesia, Iran, Iraq, Israel, Ivory Coast, Jamaica, Jordan, Kenya, Kiribati, Lao, Lebanon, Lesotho, Liberia, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Marshall Islands, Mauritania, Mauritius, Mexico, Micronesia, Mongolia, Morocco, Mozambique, Namibia, Nepal, Nicaragua, Niger, Nigeria, Oman, Pakistan, Palau, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Rwanda, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadine, Samoa, Sao Tome and Principe, Senegal, Seychelles, Sierra Leone, Singapore, Solomon Islands, Somalia, South Africa, Sri Lanka, Sudan, Suriname, Swaziland, Syria, Tanzania, Thailand, Togo, Tonga, Trinidad and Tobago, Tunisia, Turkey, Uganda, Uruguay, Vanuatu, Venezuela, Vietnam, Zambia, Zimbabwe.

Table 3-A3 – Summary Statistics

Variables	Mean	SD	Min	Max	N
Panel A: Bilateral Data					
Aid to GDP (in percent)	0.19	1.40	-5.68	228.67	131964
Aid to GDP (with OOF, in percent)	0.19	1.49	-25.71	228.67	131964
Government Fractionalization	0.30	0.27	0.00	0.83	141789
Legislative Fractionalization (FPTP only)	0.06	0.17	0.00	0.69	151906
Probability to Receive	0.46	0.37	0.00	1.00	152208
Probability to Receive (with OOF)	0.45	0.36	0.00	1.00	152208
Panel B: Country Data					
Aid to GDP (in percent)	4.95	8.84	-2.95	241.69	4500
Aid to GDP (with OOF, in percent)	5.10	9.10	-10.89	241.69	4500
Log of GDP	16.19	2.10	11.39	22.97	4500
Log of Population	8.17	2.24	2.50	14.11	4500
Log of GDP per capita	7.96	1.12	5.08	11.49	4500
Polity IV (revised)	-0.14	6.79	-10.00	10.00	3670
Political Instability	0.18	0.39	0.00	1.00	3723
Regional Polity IV	-0.56	5.79	-9.00	10.00	3723
Neighbor in Small Conflict	0.40	0.49	0.00	1.00	4500
Neighbor in Armed Conflict	0.34	0.47	0.00	1.00	4500
Neighbor in War	0.16	0.36	0.00	1.00	4500

Notes: All measures of foreign aid to GDP have a maximum well in excess of 200%. This maximum is driven by Palau. Together with other pacific islands, Palau is part of the Compact of Free Association with the United States and receives foreign assistance greatly exceeding its GDP. Without Palau, the maximum falls to slightly above 100% (due to Liberia). Negative numbers are repayments of loans.

Table 3-A4 – Robustness: First Stage

	Dependent Variable: Aid to GDP				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	1.319***	1.384***	1.244***	1.318***	1.307***
	(0.219)	(0.165)	(0.228)	(0.219)	(0.171)
Selected Controls					
Log GDP	-4.042***	-3.980***	-4.222***	-4.045***	-4.151***
	(0.968)	(0.962)	(0.907)	(0.966)	(0.913)
Log Population	4.855**	6.029**	5.531**	4.923**	6.505***
	(2.393)	(2.460)	(2.227)	(2.397)	(2.306)
UNGA Voting Alignment		2.084***			1.793***
		(0.525)			(0.473)
Trade Openness			0.045***		0.040***
			(0.010)		(0.009)
FDI Inflows / GDP				0.037	0.021
				(0.028)	(0.024)
Additional Controls					
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Summary Statistics					
Kleibergen-Paap F-statistic IV	36.12	70.39	29.76	36.34	58.22
Within-R^2	0.113	0.145	0.152	0.114	0.176
$N \times T$	3080	3080	3080	3080	3080
T	35	35	35	35	35
N	88	88	88	88	88

Notes: The table shows the results of first stage regressions using a linear two-way fixed effects model. The instrument is the sum of predicted bilateral aid over all donors ($\sum_j \hat{a}_{3ijt}$) from eq. (3-11). Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3-A5 – Robustness: Different Linear Estimation Schemes

	Estimation Method:					
	(1) FE-OLS	(2) CRE-CF	(3) FE-2SLS	(4) FE-OLS	(5) CRE-CF	(6) FE-2SLS
VARIABLES						
Aid to GDP (a_{2it})	-0.0011	0.0104*	0.0114*	-0.0012	0.0103*	0.0116*
	(0.0011)	(0.0055)	(0.0058)	(0.0009)	(0.0054)	(0.0061)
Residuals ($\hat{\nu}_{2it}$)		-0.0117**			-0.0117**	
		(0.0059)			(0.0060)	
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)				0.0058**	0.0059*	0.0077
				(0.0028)	(0.0033)	(0.0073)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)				-0.0108	-0.0107	-0.0125
				(0.0120)	(0.0122)	(0.0162)
Civil War ($a_{2it} \times h_{3,i,t-1}$)				-0.0026	-0.0025	-0.0096
				(0.0054)	(0.0130)	(0.0104)
Lagged States						
Small Conflict ($h_{1,i,t-1}$)	0.2506***	0.2501***	0.2486***	0.2271***	0.2263***	0.2174***
	(0.0306)	(0.0308)	(0.0306)	(0.0342)	(0.0355)	(0.0439)
Armed Conflict ($h_{2,i,t-1}$)	1.1201***	1.1193***	1.1231***	1.1707***	1.1695***	1.1841***
	(0.0797)	(0.0813)	(0.0789)	(0.0996)	(0.1000)	(0.1144)
Civil War ($h_{3,i,t-1}$)	1.7902***	1.7896***	1.7899***	1.8116***	1.8105***	1.8457***
	(0.0856)	(0.0962)	(0.0835)	(0.0878)	(0.0962)	(0.1027)
Summary Statistics						
$N \times T$	4375	4375	4375	4375	4375	4375
T	35	35	35	35	35	35
N	125	125	125	125	125	125

Notes: All columns include recipient and time fixed effects. Clustered standard errors in parentheses for all columns but column (2) and (5), where we report panel bootstrap standard errors in parentheses, computed with 200 replications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3-A6 – Robustness: Alternate Measures of Conflict and Foreign Aid

	Perturbations on LHS or RHS:					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	w/ Terror	only 25 BDs	UCDP-PRIO	w/ OOF	No Anglo Saxon	No Anglo Saxon w/ OOF
Aid to GDP (a_{2it})	0.0832*	0.0324	0.0571	0.0467	0.272	0.106
	(0.0453)	(0.0401)	(0.0426)	(0.0418)	(0.194)	(0.0655)
Residuals ($\hat{\nu}_{2it}$)	-0.0905**	-0.0442	-0.0539	-0.0607	-0.296	-0.122*
	(0.0443)	(0.0407)	(0.0433)	(0.0431)	(0.197)	(0.0671)
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0101	0.0197**		0.0209**	0.0308**	0.0234**
	(0.0100)	(0.00814)		(0.00836)	(0.0148)	(0.00954)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0172	-0.00814	-0.0258	-0.0113	-0.0209	-0.0127
	(0.0202)	(0.0167)	(0.0201)	(0.0181)	(0.0329)	(0.0197)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00747		-0.0202	-0.00331	-0.0284	-0.00194
	(0.0254)		(0.0264)	(0.0178)	(0.0475)	(0.0215)
Lagged States						
Small Conflict ($h_{1,i,t-1}$)	0.741***	0.531***		0.575***	0.578***	0.573***
	(0.0775)	(0.0788)		(0.0809)	(0.0807)	(0.0819)
Armed Conflict ($h_{2,i,t-1}$)	2.448***	2.260***	2.088***	2.105***	2.120***	2.114***
	(0.220)	(0.189)	(0.173)	(0.185)	(0.196)	(0.185)
Civil War ($h_{3,i,t-1}$)	3.798***		3.334***	3.434***	3.478***	3.442***
	(0.266)		(0.229)	(0.239)	(0.253)	(0.240)
Summary Statistics						
$N \times T$	4375	4375	4375	4375	4375	4375
T	35	35	35	35	35	35
N	125	125	125	125	125	125

Notes: All columns include the log of GDP, log population, the initial states, CRE at the recipient level, residual CRE, time fixed effects. No Anglo Saxon excludes Canada, the UK and the U.S. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3-A7 – Robustness: 'Leave-One-Out' Test for Small Conflict Coding

	Dependent Variable: Ordered Conflict			
	(1)	(2)	(3)	(4)
VARIABLES	No Assassinations	No Guerrilla Warfare	No Purges	No Riots
Aid to GDP (a_{2it})	0.0774	0.0600	0.0933*	0.0630
	(0.0509)	(0.0434)	(0.0510)	(0.0469)
Residuals ($\hat{\nu}_{2it}$)	-0.0866*	-0.0688	-0.107**	-0.0695
	(0.0516)	(0.0446)	(0.0523)	(0.0479)
Interactions with Lagged States				
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0159*	0.0170**	0.0218**	0.0134*
	(0.00884)	(0.00797)	(0.00880)	(0.00785)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0137	-0.00960	-0.0105	-0.0200
	(0.0181)	(0.0184)	(0.0191)	(0.0196)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00855	-0.00459	-0.00326	-0.0125
	(0.0288)	(0.0217)	(0.0255)	(0.0271)
Lagged States				
Small Conflict ($h_{1,i,t-1}$)	0.584***	0.383***	0.601***	0.766***
	(0.0773)	(0.0729)	(0.0785)	(0.0914)
Armed Conflict ($h_{2,i,t-1}$)	2.059***	1.953***	2.115***	2.157***
	(0.182)	(0.174)	(0.190)	(0.184)
Civil War ($h_{3,i,t-1}$)	3.391***	3.266***	3.431***	3.443***
	(0.232)	(0.227)	(0.240)	(0.245)
Summary Statistics				
$N \times T$	4375	4375	4375	4375
T	35	35	35	35
N	125	125	125	125

Notes: All columns include the log of GDP, log population, the initial states, CRE at the recipient level, residual CRE, time fixed effects. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3-A8 – Robustness: Additional Covariates

	Dependent Variable: Ordered Conflict					
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Aid to GDP (a_{2it})	0.0462	0.0422	0.0296	0.0401	0.0449	0.0413
	(0.0381)	(0.0387)	(0.0395)	(0.0391)	(0.0388)	(0.0374)
Residuals ($\hat{\nu}_{2it}$)	-0.0596	-0.0561	-0.0431	-0.0552	-0.0598	-0.0532
	(0.0392)	(0.0371)	(0.0392)	(0.0375)	(0.0373)	(0.0377)
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0212***	0.0226**	0.0251**	0.0239**	0.0242**	0.0198**
	(0.00803)	(0.0113)	(0.0102)	(0.0113)	(0.0113)	(0.00838)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0127	-0.0111	-0.00683	-0.0107	-0.00775	-0.0165
	(0.0202)	(0.0205)	(0.0209)	(0.0192)	(0.0191)	(0.0170)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00432	-0.00278	-0.000690	-0.000633	-0.000977	-0.00462
	(0.0248)	(0.0275)	(0.0255)	(0.0270)	(0.0276)	(0.0239)
Added Controls						
Neighbor in Small Conflict	0.128*					
	(0.0673)					
Neighbor in Armed Conflict	0.0623					
	(0.0811)					
Neighbor in Civil War	0.165*					
	(0.0877)					
Political Instability		0.218***				
		(0.0769)				
Polity IV (revised)			-0.0102			
			(0.00830)			
Regional Polity IV				0.0118		
				(0.0159)		
Democracy					-0.334***	
					(0.124)	
GDP Growth						-1.043***
						(0.293)
Summary Statistics						
$N \times T$	4375	3708	3672	3708	3708	4375
N	125	103	102	103	103	125

Notes: All columns include the log of GDP per capita, the lagged states, the initial states, CRE at the recipient level, residual CRE, time fixed effects. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3-A9 – Comparison: Our Results vs. Nunn and Qian (2014)

	Dependent Variable: Ordered Conflict		
VARIABLES	(1)	(2)	(3)
	Aid to GDP	U.S. Food aid	U.S. Food aid
Aid (a_{2it})	0.0714	0.0129*	0.0114*
	(0.0614)	(0.00731)	(0.00663)
Residuals ($\hat{\nu}_{2it}$)	-0.0851	-0.0126*	-0.0112*
	(0.0587)	(0.00721)	(0.00656)
Interactions with Lagged States			
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0272**	-0.000720	-0.000711
	(0.0133)	(0.000808)	(0.000823)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.00235	-0.000322	0.00150
	(0.0234)	(0.000694)	(0.00108)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00211	0.000105	
	(0.0248)	(0.00112)	
Lagged States			
Small Conflict ($h_{1,i,t-1}$)	0.566***	0.701***	0.655***
	(0.0814)	(0.0774)	(0.0788)
Armed Conflict ($h_{2,i,t-1}$)	2.057***	2.041***	2.156***
	(0.176)	(0.149)	(0.157)
Civil War ($h_{3,i,t-1}$)	3.348***	3.268***	
	(0.216)	(0.209)	
Selected Controls			
Log GDP per capita	0.252	0.589	0.479
	(0.342)	(0.638)	(0.561)
Additional Controls			
Recipient CRE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Residual CRE	Yes	Yes	Yes
Initial States	Yes	Yes	Yes
Summary Statistics			
Kleibergen-Paap F-statistic IV	24.73	10.59	10.59
$N \times T$	3296	3296	3296
T	31	31	31
N	103	103	103

Notes: The table shows the results of an ordered probit model with correlated random effects and a control function approach. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3-A10 – Falsification Test

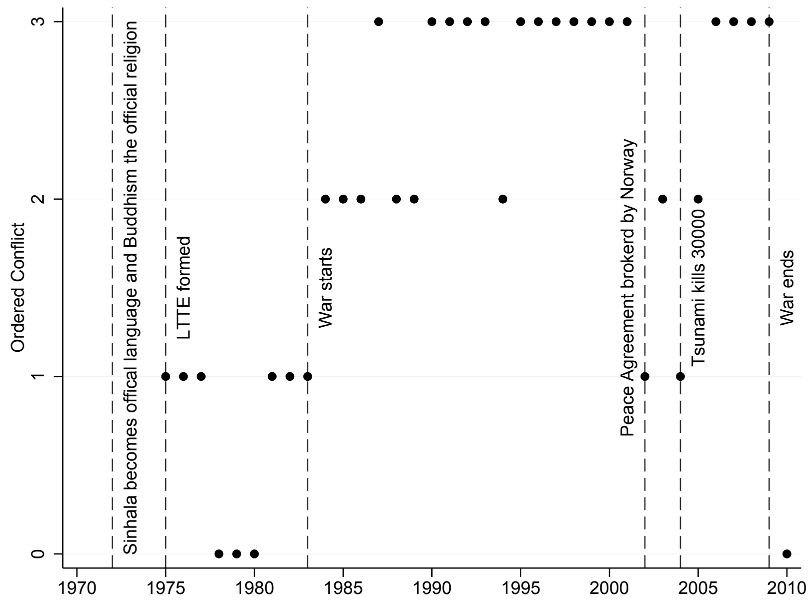
	Dependent Variable:	
	(1)	(2)
VARIABLES	U.S. Food aid	Aid to GDP
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	2.125	
	(3.126)	
Nunn and Qian (2014) IV		-0.0000156
		(0.0000327)
Selected Controls		
Log GDP per capita	-58.7451	-4.6827***
	(41.6526)	(0.8736
Additional Controls		
Country FE	Yes	Yes
Time FE	Yes	Yes
Summary Statistics		
Within-R^2	0.0460	0.1116
$N \times T$	3193	3193
T	31	31
N	103	103

Notes: The table shows the results of first stage regressions using a linear two-way fixed effects model. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B: Short Case Study – Sri Lanka

Figure 3-B1 illustrates the dynamics of the civil conflict in Sri Lanka from 1975 to 2010 as captured by our measure. Sri-Lanka is an ideal case for two reasons: First, the conflict went through all conflict intensities. Second, the conflict turned violent in the mid-1970s right around the start of our sample and ended in 2010 at the end of our sample period.

Figure 3-B1 – Conflict Dynamics in Sri Lanka



The political conflict between the Sinhalese (about 73.8% of the population) and the Tamils (about 18% of the population, concentrated in the northeast of the country), has been lingering in Sri Lanka since the independence from the British Empire in 1948. The conflict started escalating in 1970 when the new constitution declared Sinhala as the official language and defined Buddhism as the official religion. The reaction of the Tamil (mainly Christians and Hindus with their own language) followed in 1972 when Ceylon became officially recognized as the Republic of Sri Lanka.[28] The Tamils formed the Tamil New Tigers Group to set up a separate homeland *Tamil Eelam* in the northeast of Sri Lanka which was accompanied by heavy riots (Banks and Wilson, 2015).[29]

In 1975, the New Tigers Group re-named itself the Liberation Tigers of Tamil Eelam (LTTE) spurring harsh responses by the government. Notice that while the UCDP-PRIO still codes the country as peaceful, our residual category of small conflicts already picks up the escalating violence. In 1978 the LTTE was outlawed. Interestingly, this coincides with a drop in our conflict measure to zero. The next escalation occurred in 1981, when riots erupted in Jaffna and a state of emergency was declared. Finally, in 1983 the first guerrilla attack, an ambush, was conducted

[28] See `http://www.cfr.org/terrorist-organizations-and-networks/sri-lankan-conflict/p11407`.

[29] See `http://www.aljazeera.com/focus/blanktemplate/2008/11/2008111061193133.html`.

by the LTTE, resulting in the death of 13 soldiers. The incident led to the eruption of riots and the killing of hundreds of people. The year 1984 then marks the first armed conflict observation in the UCDP-PRIO data set (category two in our measure).

The UCDP-PRIO data set does a good job for most of the following years in which the conflict is varying between armed conflict and civil war until the military defeat of the LTTE in 2009.[30] There are, however, two observations, one in 2002 and the other in 2004, in which UCDP-PRIO codes a peace observation. In both cases what follows is an armed conflict observation, and in 2006 a civil war observation. The two "peace" observations which in our approach fall into the small conflict category coincide the ceasefire mediated by Norway in 2002 and the split of LTTE, after which one part formed a pro-government party. The second slump in conflict intensity was 2004, in which more than 30,000 citizens died during the tsunami.[31] Yet in both cases violence never ceased but failed to reach the threshold of 25 BD. In 2002 there have still been several clashes between LTTE fighters and government soldiers, although both groups tried to adhere to the peace agreement.[32] In 2004 rioters burned down outlets of the government friendly splinter group who seceded from the LTTE (Banks and Wilson, 2015).[33]

Summing up, our measure captures the cyclical nature of the civil conflict between the LTTE and the government of Sri-Lanka rather well. Sri-Lanka was never actually completely at peace from 1981–2009 until the military defeat of the LTTE.

[30] New York Times 2009: http://www.nytimes.com/2009/05/19/world/asia/19lanka.html?_r=2&ref=global-home.

[31] See http://www.cfr.org/terrorist-organizations-and-networks/sri-lankan-conflict/p11407.

[32] Heidelberg Institute for International Conflict Research (HIIK) 2002: http://www.hiik.de/en/konfliktbarometer/pdf/ConflictBarometer_2002.pdf.

[33] HIIK 2004: http://www.hiik.de/en/konfliktbarometer/pdf/ConflictBarometer_2004.pdf.

Chapter 4

Administrative Reforms

The Economic Effects of Territorial Reforms.[*]

> *"There are certain things I would not do if I were to start again. One of them is the abolition of local government [..]"*
>
> — Julius Nyerere, former President of Tanzania, 1984

4-1 Introduction

The increasing availability of geospatial data and advances in causal identification have revived research on the benefits and drawbacks of decentralization policies. While the overwhelming majority of existing studies uses country-level data to study the effects of fiscal or political decentralization on a variety of outcomes,[1] a more recent literature focuses explicitly on the spatial dimension of administrative reforms, i.e., changes in sub-national borders and the number, size and location of administrative units. Burgess et al. (2012), for example, show that the creation of new districts in Indonesia accelerates the exploitation of common pool resources, Asher and Novosad (2015) study investment decisions after a decentralization reform in India, and Bazzi and Gudgeon (2015) analyze civil conflict in the newly created Indonesian districts. The two types of administrative reforms are often linked: territorial decentralization often (but not always) coincides with political decentralization (Grossman and Lewis, 2014; Grossman et al., 2017).

Decentralization became a policy mantra in the late 1980s and continues to play an important political role today. One of the key prescriptions of international institutions advising developing countries has been to put more responsibility on the local level, e.g., through fiscal decentralization or the creation and empowerment of new districts.[2] Decentralization and devolution is thought to be a powerful tool for

[*]This chapter is based on joint work with Richard Bluhm and Christian Lessmann (Bluhm et al., 2019).

[1]E.g., Davoodi and Zou (1998) study the relationship between decentralization and growth, Cai and Treisman (2005) analyze if decentralization leads to fiscal responsibility. Bardhan and Mookherjee (2000) investigate how prone local governments are to corruption (see also Fisman and Gatti, 2002).

[2]See, for example, Chapter 5 in the World Development Report 1999/2000 which focuses entirely on decentralization reforms (World Bank, 2000) or Fedelino (2010) for a survey of IMF advice on the macro-aspects of decentralization reforms.

improving local governance and public goods provision, as well as managing conflict. Ideally, decentralization enables groups with different preferences (often different ethnic groups) to achieve their preferred public policy and increases government accountability. This notion aligns well with the classic theory of federalism. A major implication of these models is that heterogeneous countries should feature a greater number of homogeneous districts (Tiebout, 1956; Oates, 1972, 1999; Panizza, 1999; Alesina et al., 2004).[3]

Decentralization reforms are not a panacea. An important political economy literature stresses that decentralization reforms raise the risk of elite capture by increasing the proximity between local governments and local elites (Prud'homme, 1995; Bardhan and Mookherjee, 2000). Research on comparative development in the long run also tends to be more critical of the potential gains to be reaped from decentralization reforms, at least when they occur from a low base of political centralization. Acemoglu and Robinson (2012) argue that a high degree of historical political centralization is an essential ingredient in creating the institutions underpinning long-run development. This argument is supported by robust evidence on the gains from pre-colonial political centralization in Africa (Gennaioli and Rainer, 2007; Osafo-Kwaako and Robinson, 2013; Michalopoulos and Papaioannou, 2013). In light of these different channels, we consider it an empirical question whether decentralization reforms have positive effects on economic development and, if so, under which conditions they materialize.

Individual country studies shed some light on the mechanisms at play. Several studies document positive effects of decentralization or the creation of new districts on a range of outcomes. Asher and Novosad (2015) study three newly created districts in India and show that local control over political institutions increases investment into education and fosters economic activity. Kosec and Mogues (2016) analyze decentralization in Ethiopia using a regression discontinuity design, finding that decentralization increases the delivery of services which are priority for the central government. Others document negative effects, particularly in terms of the management of public goods. Lipscomb and Mobarak (2017) find that municipality proliferation increases negative externalities (downstream river pollution) in Brazilian communities. Burgess et al. (2012) show that creation of new districts increases deforestation in Indonesia and suggest that this effect runs through local politicians permitting more (illegal) logging. Previous cross-country studies tackling territorial reforms have mostly focused on administrative unit proliferation, i.e., simple counts derived from administrative registers, and confirm some of these findings (Grossman et al., 2017). Unlike spatial approaches, using cross-country variation does not allow the study of local effects and, perhaps most importantly, obscures that a single reform may imply that some administrative units are split while others are simultaneously being merged.

In this chapter, we provide the first global study of the effects of territorial reforms on local economic activity from 1990 to 2014. A major benefit of our approach is that it allows us to simultaneously analyze the effect of territorial

[3]Thus, administrative territorial reform might be a particular attractive policy for African countries whose national borders were determined by colonial powers (Alesina et al., 2011; Michalopoulos and Papaioannou, 2013, 2016). While national boundaries are firmly established today, the internal organization of countries is malleable and reformed often. In fact, the developing world has seen a steep rise in administrative units since the early 1990s.

decentralization and centralization of primary subnational units. The global scope also allows us to examine effect heterogeneity and regional variation in the transmission channels. In our context, 'primary' refers to the boundaries of the highest sub-national jurisdictions, such as departments, states, or provinces as defined by the Global Administrative Unit Layers (GAUL) project.[4] Territorial decentralizations yield smaller regions, centralizations larger ones. Using the GAUL data we construct a universe of micro-regions or splinters which represent the smallest unit whose borders have never been reformed in our observation period. We use satellite-derived measures of economic activity (night lights) to measure the effect of sub-national border changes on economic activity within these regions.

The key contributions of the chapter are threefold. First, we establish the effects of administrative reforms in a global sample of districts. Our identification strategy relies on a panel difference-in-difference approach combined with an event study design. This allows us to draw general conclusions about the efficacy of such reforms not limited to a specific country or region. Second, we develop a new spatial approach to trace and classify territorial reforms around the globe. Thus, we are able to track which areas have been reformed and unreformed over the period from 1990 until 2014. Third, using these new data, we explore the effect of territorial reforms in different political settings and regions.

First-order administrative reforms predominantly occur in younger states, as well as low and middle income countries. The modern territorial structure of industrialized economies is an outcome of many reforms that happened in the past. An interesting exception is Germany, which following re-unification in 1990 reorganized the 15 districts of the former German Democratic Republic into 5 states (*Bundesländer*). The borders of West-German states have also changed in the past. The last reform occurred when new, larger states were created based on the occupation zones of the Allied Powers after WWII.[5] First-order reforms are rare among the highly-developed countries existing today. Our analysis therefore focuses predominantly on the developing world, specifically Africa and Asia.

Administrative reforms in Sub-Saharan Africa have an interesting history (see Olowu, 2001, for more details). Prior to independence, many countries on the subcontinent went through a period of intensified decentralization reforms, mainly as a response to the crumbling legitimacy of the colonial state. After independence, many African leaders embarked on nation-building projects with little regard for local institutions (see the epigraph to this chapter). Single party states emerged and many countries became heavily centralized both in terms of political power and the configuration of administrative units, often maintaining local government only in name. Reforms became inevitable once the crisis of African socialism, import substitution policies and unfavorable commodity dependence started to undermine the power base of these centralized states in the late 70s and 80s. Structural adjustment policies in turn usually included decentralization packages and emphasized lean (central) government. In practice, however, the central state still maintained control by appointing local officials during this period. Finally, after the "third wave of democratization" in the 1990s (Diamond, 1996), the pace of decentralization reforms increased substantially and brought with it a resurgence of local power.

[4]The GAUL data is compiled by the UN Food and Agricultural Organization (FAO).

[5]Note that these reforms are outside our sample.

The administrative history of Asia is less uniform. Although Asia is well-known for its strong central states, it is also the home to many multi-ethnic federal states (such as Indonesia or Malaysia). There too, we observe a clear trend towards decentralization and district proliferation. Younger nation states with a colonial history tend to reform their districts more often than older countries. India, Bangladesh and Indonesia went through several administrative reforms, whereas countries like Japan have had a stable administrative structure for decades. India is a particularly interesting case in terms of heterogeneity, since its state borders deliberately follow linguistic divisions (Asher and Novosad, 2015).

Our main findings are as follows. Territorial centralization reforms increase light intensity of micro-regions in the full sample, while we cannot reject the null that decentralization reforms have no first order effects. Our event study implies that it takes about 4 years until centralization reforms yield positive effects. Furthermore, we document substantial effect heterogeneity across regions and political systems. While territorial centralizations have positive effects on economic activity in African regions, Asian regions gain significantly from decentralization reforms. A centralization reform in Africa increases economic activity as measure by night light by about 34 percent in the subsequent years, while a decentralization in Asia increases activity by roughly 70 percent. We find suggestive and interesting results using alternative conditioning factors. For example, we classify the micro-regions forming parts of a decentralization reforms as mother (child) and centralizations as absorbing (absorbed). We also condition on constitutional rules, and consider the proximity of micro-regions to the administrative center. Our findings suggest that while there are first-order effects of territorial reforms on economic output, they are sensitive to the context in which they occur. Africa's weaker states stand to gain more from territorial centralization than Asia's centralized states who, in turn, benefit from territorial decentralization.

The remainder is structured as follows. Section 4-2 outlines our new approach to track and classify territorial reforms, discusses the data, and our variables of interest. Section 4-3 explains our empirical strategy and discusses the identifying assumptions. Section 4-4 presents our main findings. Several extensions are reported in Section 4-5. Section 4-6 offers concluding remarks.

4-2 Data

4-2.1 Tracking Administrative (Territorial) Reforms

We propose a purely spatial approach to identify and classify all first order territorial administrative reforms across the globe.[6] The method is based on authoritative vector data included in the GAUL database. The GAUL project obtains its data from other UN agencies, member states, and its users. The current 2015 version contains vector data for all countries across the globe from 1990 until 2014. The GAUL database follows clear and well-documented procedures for how district reforms are recorded in the data and how the increasing availability of better vector data is incorporated retrospectively into the entire data set. Note, however,

[6]This approach is related to Egger et al. (2017), who independently developed similar ideas in the context of German municipal mergers. Our approach is considerably more general though, since it applies to all kinds of territorial reforms.

that the data are incomplete in some cases, such as Indonesia. We complement the GAUL data with several additional sources, e.g., administrative registers and maps from official government records. We also manually code and geo-reference all administrative centers (cities) from administrative registers for each primary subnational unit for each country in each year. Administrative capitals are a useful tool to cross check the vector layer data, and make sure that we do not miss any reforms. More information about our approach to code the cities and the corrections we implement to data can be found in section B of the Appendix.[7]

Our approach to finding reforms is straightforward. We iteratively identify administrative reforms as changes in the area of a district. For any pair of two years, we create the spatial intersection of the two vector data sets. This creates new areas or new affiliations whenever a border is moved, deleted or created. We then cycle forward by intersecting the result of the previous intersection with the next year of official data and so on. During each iteration, we record the current district identifier and add it to an identification string which in the last year contains 24 (i.e. $2014 - 1990$) identifiers. We obtain two data sets in this manner. The first is a spatial data set of micro-regions, which in the final year contains the smallest spatial unit whose borders were not reformed in all of the preceding years. We call this unit a splinter. Figure 4-A1 shows the entire splinter universe of 3,293 splinters that make up our sample. The second is a kind of phylogenetic or evolutionary tree for each contemporary splinter, summarizing its entire history of district affiliations and its respective administrative center back till 1990. Note that splinters only result from border reforms that cross-cut borders from the previous year. If borders are simply abolished no new splinter will be created but the regions identity changes. Thus, only the combination of the spatial splinter data set and the phylogenetic tree can identify all administrative reforms in a general and consistent manner.

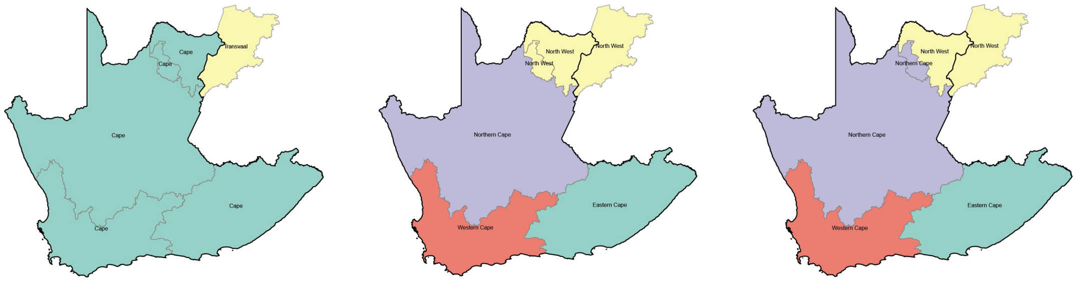
Figure 4-1 provides a small snapshot of the two data sets created in this process. It illustrates the reform history of Cape Province in South Africa from 1992 onward (the green area in panel A). The province was split into four new districts in 1994 (panel B). Three of the successor districts are simply subareas of the former Cape Province, while the fourth district (North-West) was created with some areas of both the former Cape Province and the former Transvaal – the neighboring province to the north east (the yellow area in panel A). Furthermore, part of the North-West district has been reassigned to the Northern Cape in 2005 (see yellow area in panel B turned purple in Panel C). As a result, all splinters of Cape Province are affiliated with at least two different districts over this period: one prior to 1994 and (at least) one from 1994 onward (see panel D). There are two things to notice. First, if our data set would end in 1995 we would have exactly four splinters in place of the original Cape province. Second, the fact that some area of the Cape Province gets lumped together with some part of another province does not create a new splinter, while the reassignment of part of an area within the former Cape Province to another creates a new splinter (e.g., reasigning part of North-West to Northern Cape).

This approach has several advantages which distinguish our paper from the rest of the literature. First, previous micro-studies of administrative reforms have restricted their attention to specific countries and types of reforms, e.g., district splits within Indonesia (Burgess et al., 2012). In the Indonesian case districts were split within

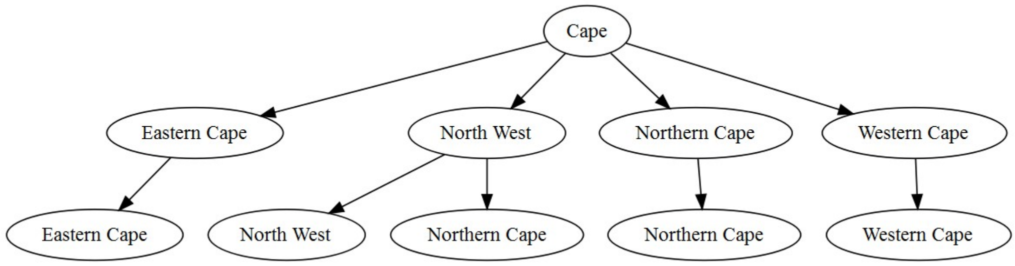
[7]Note that we have to exclude several countries due to errors in the vector data. They include, India, Indonesia, Guinea Bissau, Guinea, Lebanon, Chad and Serbia.

Figure 4-1 – Reform History of Cape Province, South Africa

(a) Cape province in 1992 **(b)** Cape province in 1995 **(c)** Cape province in 2005



(d) The evolutionary tree



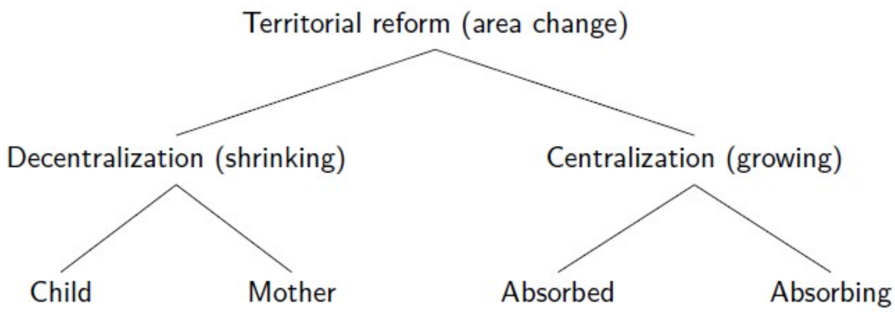
Notes: Panel a to c illustrate initial and successor districts of the Cape Province in South Africa. Panel d) illustrates the evolutionary tree for the splinters which were formerly part of Cape Province, South Africa. The last level represents the situation after the 2005 reform.

their original borders, allowing the authors to count the number of districts within a province. Cases like the North-West District in South Africa would not be captured by such an approach. Second, cross country approaches usually rely on country level data of the number of administrative units (Grossman et al., 2017). Hence they are not able to track subtle changes among existing districts, such as the 2005 land swap between the Northern Cape and the North West districts in South Africa (Figure 4-1). Third, our splinters are natural units of observation. By construction, they form a balanced panel of internally unreformed units. Note that other spatial units, such as grid cells, do not exhibit this property, as borders would run through such cells (as in Baskaran and Blesse, 2018). Finally, disregarding measurement errors in the spatial data, the splinter universe allows us to quantify the precise area under treatment for each reform.

4-2.2 Classifying Territorial Reforms

We classify all administrative reforms at the splinter level as follows. First, whenever a splinter j changes its regional affiliation i, and the area of the region the splinter is affiliated with changes, then we code a reform. The area of a region is simply the sum of splinter areas affiliated with it in a given year (or $A_{it} = \sum_j^J A_{ijt}$ which can be constructed by using the information on j and i in the phylogenetic tree). The requirement of an area change is important to distinguish territorial reforms from mere name changes.[8] In a second step we classify each reform either as a decentralization or centralization. Decentralizations are defined as affiliation changes in which the area change for splinter j is negative, which implies that it is located in a smaller administrative unit in t as compared to $t-1$. Centralizations are defined as affiliation changes in which the area change for splinter j is positive, i.e., in cases where the splinter is located in a bigger administrative unit in t as compared to $t-1$. The reform of the Northern-Cape in 2005 represents such a case (Figure 4-1). Note that in our baseline 'bigger' and 'smaller' refer exclusively to area, not population. This classification identifies 552 splinters as treated of which 435 are territorial decentralization reforms and 171 are centralization reforms.[9]

Figure 4-2 – Classification Tree



Notes: The figure depicts the classification tree for each reform type.

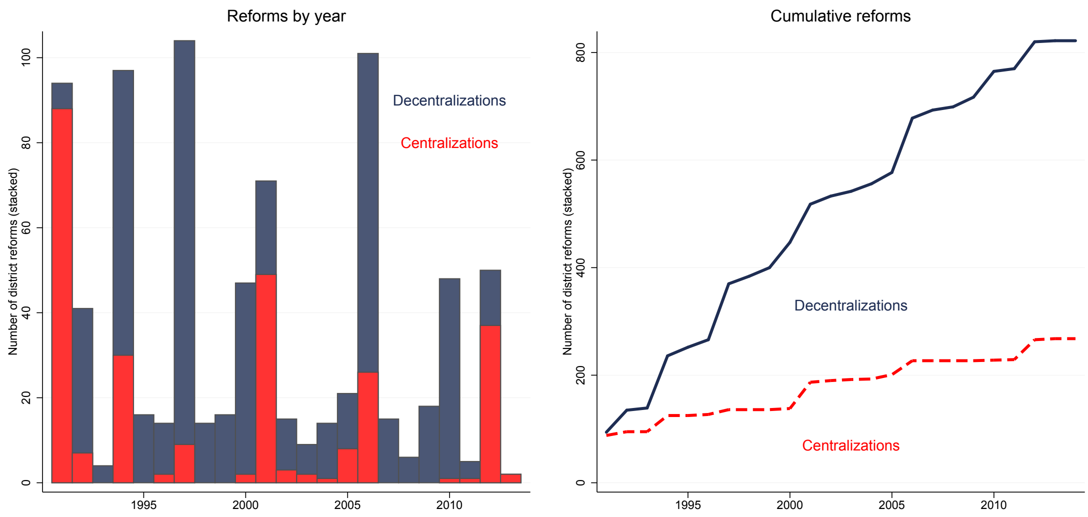
Next, we use the geocoded administrative centers to further categorize each reform. Following Bazzi and Gudgeon (2015), we assign each decentralized splinters a 'mother' and 'child' dummy. We consider splinters that keep their administrative capital as mother regions and splinters that receive a new administrative capital as new offspring or children. In the case of the 1994 Cape Province split, the Western Cape is the mother region, since it keeps Cape Town as its administrative center, while all other splinters in Figure 4-1 are defined as children. We similarly categorize the territories forming parts of a centralization reform into 'absorbing' and 'absorbed' splinters. Absorbing splinters are those that keep their administrative capitals following the centralization (or merger), while absorbed splinters are those that change their capital (also see Egger et al., 2017). In the case of the 2005 reform of the Northern Cape and North-West region in South Africa, the splinter with the history *Cape* $\longrightarrow$ *Northern Cape* $\longrightarrow$ *Northern Cape* is classified as the absorbing region since Kimberly remains its administrative center, while the splinter with the history *Cape* $\longrightarrow$ *North West* $\longrightarrow$ *Northern Cape* has been absorbed.[10] This classification divides the decentralization reforms into 231 mother splinters and 204 child splinters, whereas centralization reforms consist of 58 absorbing and 86 absorbed splinters. Figure 4-2 summarizes this typology.

[8]We test the sensitivity of our results using several thresholds of area changes to test if our results are driven by small border adjustments below.

[9]Note that some of the splinters have been treated multiple times, an issue we return to in Section 4-3.

[10]In cases where a new administrative center is created for all splinters treated we assign 'mother' and 'absorbing' to splinters for which the new capital is within the union of previously affiliated splinters.

Figure 4-3 – Trends in Decentralization and Centralization Reforms



Notes: Illustration of the frequency and type of administrative reforms. The left panel shows the frequency by reform type in each year. The right panel shows the accumulation of reforms over time. The frequencies count the number of treated districts.

Our framework is general in the sense that it can accommodate complicated cases where parts of an existing region join another region, e.g, the reform of the Northern Cape and North-West region in South Africa. In such a setting, we observe both a decentralization and a centralization reform. Figure 4-3 provides an overview of the global reform activity in our sample and Table 4-A1 list each country that has been reformed, as well as the year of reform.

4-2.3 Outcomes

We focus on local economic activity to assess the impact of territorial reforms. Official income statistics, even if they are available for sub-national regions in developing countries, suffer from several well-known problems, such as measurement errors, the lack of regional purchasing power parities, and aggregation issues once regional borders change. For these reasons, we use an outcome derived from satellites which measure economic activity uniformly across the globe.

Our primary measure of local economic activity is the density (intensity) of light in a given district. Henderson et al. (2012) show that average light intensity is a good proxy for national GDP in poor countries. Chen and Nordhaus (2011), Hodler and Raschky (2014), Michalopoulos and Papaioannou (2013, 2016), Lessmann and Seidel (2017) and several others since then extend this finding to the subnational level. In an application which is particularly relevant for our context, Pinkovskiy (2017) shows that there are distinct discontinuities of light at national borders.

Night lights observed by US weather satellites are recorded on a scale of 0 to 63 DN, where DN is a digital number which does not map directly to a physical quantity. The National Oceanic and Atmospheric Administration (NOAA) processes the daily cloud-free pictures into an annual composite and provides the data on a resolution of 30 arc seconds (about 1 km $\times$ 1 km at the equator). We follow best-practice in the literature by defining light intensity or luminosity as $\ln(light/area + 0.01)$, where *light* is the area-weighted sum of light in each district and area is a districts area in km^2. Area-weighting the sums corrects for the curvature of the earth, while taking logarithms after adding a small constant allows us to focus on relative changes in light intensity without losing observations with no light. The results can be interpreted as if light per capita were on the left hand side of the regression, as long as we control for the log of population density.

4-3 Empirical Strategy

4-3.1 Estimation Framework

Our identification approach relies on panel data difference-in-difference (DiD) estimates. Panel DiD estimates are commonly used to evaluate the impact of policies in observational studies but impose strong assumptions, such as common parallel trends and constant treatment effects.

We estimate the effect of territorial reforms using the following model as a baseline

$$y_{ijt} = \beta D_{ijt} + \gamma C_{ijt} + \delta p_{ijt} + \mu_{ij} + \lambda_t + \varepsilon_{ijt} \tag{4-1}$$

where y_{ijt} is the log of light density, D and C are treatment indicators for centralizations and decentralizations, repsectively, p_{ijt} is the log of population density,[11] μ_{ij} are splinter fixed effects and λ_t are year fixed effects. β and γ are our coefficients of interest.

Our treatment indicators remains one for all post treatment years, so that we exclude all splinters that have been treated more than once. At a later stage we specify an alternative count variable for splinters that experience several reforms.[12] Our DID estimates thus capture the effects within a treated splinters compared to never treated splinters or unreformed first order administrative districts. Note that we exclude splinters that are treated with both times of reforms, since the proper reference group becomes convoluted. The main reason is that the degree of the reform becomes more important. A splinter could for example be heavily decentralized and experience only a small centralization later on. To properly capture those effects we would have to include separate estimates for continuous treatments, which would not be straight forward to interpret.

We also estimate an event window DiD which allows the effects of the reforms to phase in and out over a pre-defined period (as in Freyaldenhoven et al., 2018).

[11] The log of population density is calculated in the same way as the log of light density. Spatial population data are taken from the ‘Global Human Settlement Layer’ (GHSL) provided by the European Commission (CIESIN, 2015). Note that the GHSL only provides detailed spatial population estimates for the reference years 1975, 1990, 2000 and 2015. We linearly interpolate the population data between those reference years.

[12] Standard DiD approaches are not able to tackle dynamic treatment effects which leads to invalid estimates (Lechner, 2015).

Formally, we specify

$$
\begin{aligned}
y_{ijt} = \ & \sum_{s}^{S} \Delta\beta_s D_{ij,t+s} + \beta_{-S-1} D_{ij,t-S-1} + \\
& \sum_{s}^{S} \Delta\gamma_s C_{ij,t+s} + \gamma_{-S-1} C_{ij,t-S-1} + \delta p_{ijt} + \mu_{ij} + \lambda_t + \varepsilon_{ijt}
\end{aligned}
\tag{4-2}
$$

where all the variables are defined as before but $s = \{-S, -S+1, -S+2, \ldots, 0, \ldots, S-2, S-1, S\}$ so that we span a window of $2S+1$ years around the treatment date. The dummies $D_{ij,t-S-1}$ and $C_{ij,t-S-1}$ indicate that a decentralization or centralization occurred more than S years in the past. As a normalization, we do not include dummies for reforms that are more than S periods in the future. Our coefficients of interest are β_s and γ_s which capture the treatment effects of decentralization and centralization in the years around the treatment. This event study approach also has the benefit that pre- and post-treatment periods are symmetric, a property that has recently shown to bring estimates closer to experimental benchmarks (Chabé-Ferret, 2015).

We also investigate effect heterogeneity using different conditioning variables. The general approach is similar in each case, so that we only illustrate it here for the case of changes in administrative capitals. Testing for differences among regions that retain or lose their capital essentially amounts to a triple-DiD estimator for each reform type

$$
\begin{aligned}
y_{ijt} = \ & \beta_1 D_{ijt} + \beta_2 (D_{ijt} \times \mathit{Child}_{ij}) + \\
& \gamma_1 C_{ijt} + \gamma_2 (C_{ijt} \times \mathit{Absorbing}_{ij}) + \delta p_{ijt} + \mu_{ij} + \lambda_t + \varepsilon_{ijt}
\end{aligned}
\tag{4-3}
$$

where we add the interaction of decentralization reforms with an indicator on whether the splinter obtained a new capital (*Child*) and the interaction of centralization reforms with an indicator of whether it retains its capital (*Absorbing*). The coefficients of interest in this setup are β_2 and γ_2. While we could also allow the treatment effects to phase in and out in this expanded setting, it would require us to estimate a large number of coefficients even for moderate window sizes.

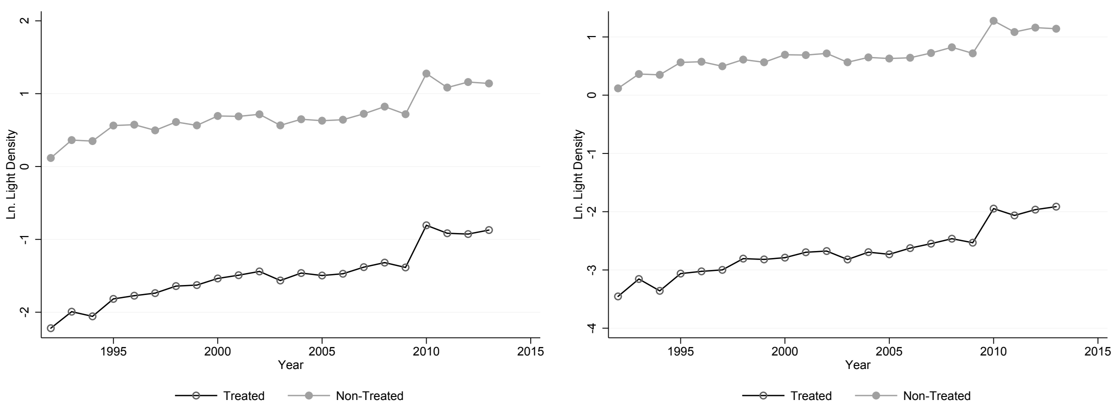
4-3.2 Identifying Assumptions

For DiD estimates to be valid they need to satisfy two central assumptions. First we need to assume common trends, or bias stability, which means that differences in the expected potential non-treatment outcomes over time are not related to the subsequent treatment groups. Second we need to assume exogeneity of our control variables and pre-treatment outcomes. This implies that the timing of the treatment is exogenous. If both assumptions are satisfied we are able to recover the average treatment effect on the treated (ATT).

Parallel trends: We provide suggestive evidence that the parallel trend assumption holds in Figure 4-4, which plots the trends of average log light density in our treatment and control groups over time. Figure 4-4 shows that the average light density of the control group is substantially higher compared to the treatment groups, but the trends are similar. The level effect is easily explained by the

Figure 4-4 – Light Density Trends Between Treated and Non-Treated Splinters

(a) Territorial Decentralizations **(b)** Territorial Centralizations



Notes: Panel a reports the trends of log light density between the control group and the group of splinters experiencing decentralization. Panel b reports the trends of log light density between the control group and the group of splinters experiencing centralizations.

composition of reforming countries. Most industrial countries do not conduct any reforms during our sample, thus the control group contains many rich first order districts. If we demean light density within splinters, the differences vanish (see Figure 4-A2 in the Appendix).[13] We return to this issue when presenting the results of the event study design, but note here already that we do not detect pre-trends there either.

Exogeneity of controls and pre-treatment outcomes: To test whether the assumption of exogenous controls is reasonable in our case, we subset our sample to those 430 splinters that are treated only once and calculate the time until treatment (as the treatment year minus 1992). The time until treatment is then regressed on the initial light density of our splinters and several control variables. The results are presented in Table 4-1. The table shows that several variables predict the time until treatment if we do not employ country fixed effects (column 1 & 2 of Table 4-1). However, as soon as we include country fixed effects, none of the initial values can predict the time until treatment.[14]

We also test for static selection into treatment. Without evidence of static selection into treatment, we could rely on simpler estimation methods. Here we convert our sample into a cross-section of splinters and regress an ever-treated dummy on the same initial value variables as in the previous table.

Table 4-A2 in the Appendix reports the result of the regressions, again excluding and including country fixed effects. Only the initial distance to the district capital of a splinter robustly predicts whether a splinter is ever treated.[15] Level effects are not

[13]Note that all our regression specifications include splinter fixed effects, which means that the level differences between splinters are controlled for by design.

[14]Note that all those attributes will be partialled out by splinter fixed effect present in all of our specifications.

[15]We also tested if there are different underlying selection mechanisms for centralization reforms or decentralization reforms. We could not find any differences between selection into decentralization or centralization by replicating Table 4-1 and Table 4-A2 for both types separately.

Table 4-1 – Exogenous Timing of Treatment

	Dependent Variable: Time until treatment (t = 8.36)			
	Without country FEs		*With country FEs*	
	(1)	(2)	(3)	(4)
	Splinter characteristics			
Initial Light Density	-0.9369	-0.9116	-0.0060	-0.0007
	(0.1364)***	(0.1339)***	(0.0889)	(0.0882)
Initial Distance to District Capital	-0.0083	-0.0064	0.0006	0.0009
	(0.0028)***	(0.0032)**	(0.0011)	(0.0012)
Initial Population	0.8276	0.6374	0.0280	0.0382
	(0.1897)***	(0.2427)***	(0.0726)	(0.0627)
	District characteristics			
Initial Area		-0.0033		-0.0016
		(0.0048)		(0.0013)
Initial Population		0.2043		-0.0340
		(0.3004)		(0.1745)
Observations	430	430	430	430
R^2	0.140	0.145	0.864	0.864

Notes: Standard errors are clustered at the splinter level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

surprising, since without them we could run a simple first difference specification. This is also intuitive given the high amount of decentralization reforms in our sample. Areas that are further away from the centers of the political administration are more likely to become independent subnational units.

4-4 Main Results

Standard DiD: Our first set of results focuses on the effects of territorial reforms in general. Column 1 of Table 4-2 reports our baseline specification as stated by equation (4-1) using a standard panel-DiD specification without splinters which are treated multiple times. We find that centralization reforms increase economic activity, measured as the log of light density, on the treated splinters. The effect is highly statistically significant and economically meaningful. Territorial centralization reforms, on average, lead to 28.83% increase in economic activity for the entire period subsequent to the reform. For territorial decentralization reforms, however, we cannot reject the null hypothesis of no effect in the global data.

Column 2 replicates the specification of Column 1 including splinters which were reformed more than once. Note that we still ignore the consecutive treatments, i.e., the reform dummies remain one after the first treatment. The results remain qualitatively and quantitatively similar. Column 3 relaxes this by using a count variable for the number of reforms and the type experienced by each splinter. Again, the results hardly change. Finally, column 4 includes separate estimates for dummies for each number and type of treatment. Our main effects remain the same, but we now find a negative effect for splinters that are decentralized for the third time.

The results are available upon request.

Table 4-2 – Difference in Difference Results

	Dependent Variable: Ln Light Density			
Independent vars:	(ST) Treat Dummy (1)	(MT) Treat Dummy (2)	(MT) Treat Count (3)	(MT) Separate Treat Dummy (4)
Decentralization	0.1290	0.0983	0.0796	0.0998
	[0.1012]	[0.1084]	[0.1087]	[0.1024]
Centralization	0.2533	0.2562	0.2495	0.2567
	[0.0541]***	[0.0630]***	[0.0598]***	[0.0631]***
	Additional treatments			
2nd Decentralization				0.0882
				[0.2495]
3rd Decentralization				-0.1528
				[0.0414]***
2nd Centralization				0.0560
				[0.0651]
Obs.	68532	69544	69544	69544
Splinter-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Adj. R^2	0.976	0.976	0.976	0.976

Notes: The log of population density is included as a control variable in all specifications. *ST* refers to the single-treatment sample, *MT* to the multiple treatment sample. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note that this is is only a rough test of whether the exclusion of multiple treatments changes the results, which does not seem to be the case. Causal estimation of repeated treatment effects in panel regressions is considerably more complicated (Lechner, 2015). In the remainder of the paper we exclude all splinters that are treated more than once.[16]

Event Study Design: Our previous specifications where simple panel DiD models reporting the average effect of territorial reforms based on very different pre- and post-treatment periods. Reforms carried out in the early 1990s, for example, have very long post-treatment and comparably short pre-treatment periods. The opposite is the case for reforms undertaken in the late 2000s and early 2010s.

To test if our effects take time to fade in, out, or both, we re-estimate our baseline specification using an event-window design, in which we force the pre- and

[16] A potential problem with our the spatial approach to identifying territorial reforms is measurement error in the original vector data. If borders are drawn imprecisely from year to year, we could falsely detect reforms which are only immaterial shifts on the map. We replicate our baseline results introducing thresholds of area changes in order to test if our results are driven by measurement error. Table 4-A3 in the Appendix shows that our baseline results are not driven by small territorial reforms, but are stable at reasonable values of the area threshold. We also test if our results are driven by any particular country, within our treatment group, but find no systematic evidence for this (see Figure 4-A3). An exception is Uganda, which has a lot of influence on the decentralization coefficient. Uganda's decentralization reforms are well documented (Grossman and Lewis, 2014).

Table 4-3 – Timing of the Effects (A): Centralizations

	Dependent Variable: Ln Light Density						
	Event sequence size						
	$S = 2$	$S = 3$	$S = 4$	$S = 5$	$S = 6$	$S = 7$	$S = 8$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pre-treatment						
t_{-2}	0.0870	0.0942	0.0995	0.0960	0.0884	0.0708	0.0810
	[0.1071]	[0.1245]	[0.1298]	[0.1243]	[0.1187]	[0.1163]	[0.1184]
t_{-1}	0.0289	0.0351	0.0397	0.0372	0.0285	0.0118	0.0212
	[0.0908]	[0.1063]	[0.1122]	[0.1074]	[0.1036]	[0.1024]	[0.1064]
	Post-treatment						
Treat	0.0717	0.0804	0.0846	0.0821	0.0752	0.0575	0.0686
	[0.0865]	[0.1030]	[0.1063]	[0.1008]	[0.0941]	[0.0902]	[0.0924]
t_1	0.1043	0.1129	0.1190	0.1159	0.1086	0.0921	0.1022
	[0.0747]	[0.0899]	[0.0942]	[0.0882]	[0.0834]	[0.0818]	[0.0861]
t_2	0.1061	0.1156	0.1214	0.1193	0.1111	0.0945	0.1060
	[0.1074]	[0.1232]	[0.1273]	[0.1202]	[0.1140]	[0.1113]	[0.1146]
t_3		0.1341	0.1411	0.1388	0.1323	0.1147	0.1257
		[0.0863]	[0.0909]	[0.0851]	[0.0809]	[0.0792]	[0.0849]
t_4			0.2127	0.2109	0.2039	0.1874	0.1981
			[0.0736]***	[0.0707]***	[0.0648]***	[0.0625]***	[0.0623]***
t_5				0.3370	0.3310	0.3143	0.3260
				[0.0762]***	[0.0749]***	[0.0753]***	[0.0746]***
t_6					0.3258	0.3096	0.3210
					[0.1040]***	[0.1033]***	[0.1017]***
t_7						0.3724	0.3848
						[0.1179]***	[0.1155]***
t_8							0.2559
							[0.1091]**
Obs.	68532	68532	68532	68532	68532	68532	68532
Splinter-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.976	0.976	0.976	0.976	0.976	0.976	0.976

Notes: The log of population density is included as a control variable in all specifications. S refers to the size of the pre- and post-treatment sequence. The coefficients for decentralizations for the same specification are reported in Table 4-A4. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

post-treatment periods to be of equal length (see eq. (4-2) in Section 4-3). The recent econometric literature highlights that symmetric DiDs are often closer to experimental benchmarks compared to standard panel DiDs (Chabé-Ferret, 2015). The downside of the approach is that the data requirements are higher (longer panels) and the results are less tractable.

Table 4-3 reports estimates of our centralization treatment by pre- and post-treatment period for symmetric sequences around the treatment ranging from 2 to 8. The sequence number represents the number of pre- and post-treatment periods that are interacted with our $Ever-centralized$ indicator. The results we obtain for decentralizations are reported in Table 4-A4 in the Appendix; for which we find no significant effects. Note that for ease of representation, we only report the first two pre-treatment periods t_{-2} and t_{-1}. The remaining pre-treatment periods are not reported, but are statistically insignificant. We also omit the dummy equaling unity for all post-treatment periods outside the respective sequence, while the dummy equaling unity for all pre-treatment periods outside the sequence is the omitted category.[17]

Columns 1 to 7 of Table 4-3 show several interesting patterns. First and foremost, the effects of centralizations seem to take some time to phase in. The gains in economic activity usually occur after the first three years and remain persistent. Note that this is independent of the window size we employ. Second, if the window size gets very short (column 1 & 2) the effect turns insignificant, which is not too surprising. If the economic gains of centralizations takes time to phase in reducing the post-treatment period to only 3 years or less will mask the positive long term effect of centralizations.

In summary our baseline estimates suggest that there are economic gains from centralization, while we find no discernible effect for decentralizations. Furthermore, the effects of the reforms take time to manifest and are persistent.

4-5 Extensions

We now focus on several important extensions of our main results. First, we use our unique global dataset to test if our effects are homogeneous across different regions. Second, we analyze if all splinters involved in either decentralizations or centralizations are affected in the same way. Third, we investigate if our obtained effects are homogeneous across political systems. Finally, we test if political proximity to a subnational administrative center (or capital) has direct benefits on economic output.

Regional Heterogeneity: We now test if there are qualitative differences in how decentralizations and centralizations affect light intensity between Africa and Asia, in which the majority of territorial reforms occur and that have been the primary focus of previous research (e.g., Burgess et al., 2012; Bazzi and Gudgeon, 2015; Grossman and Lewis, 2014; Grossman et al., 2017). Table 4-4 replicates our baseline specification for treated splinters in the two regions separately.[18]

Columns 1 and 2 of Table 4-4 show the results for both the single treatment and multiple treatment sample for Africa. The results imply that African regions gain from centralization, which is in line with our baseline (see Table 4-2). In Asia the coefficients change significantly. We can no longer reject the null for centralizations and the magnitude of the coefficients is only about one fifth of the size compared to the full and African sample. Instead, decentralizations are statistically significant and have a large positive effect (about 70%) on economic activity. Again the results of the first reform remain stable if we estimate separate coefficients for consecutive reforms separately (columns 2 & 4). Summing up, it seems that countries in Africa and Asia have had very different payoffs from territorial reforms. The positive effects of decentralization documented in several case studies are often based on Asian countries that behave differently compared with African regions. These heterogeneities are highly important for policy considerations. For the remainder of the paper we will always test for differences in the two regions and try to uncover what drives those substantial differences.

[17] The post-treatment period dummy is usually positive and statistically significant.

[18] Note that we only subset the treated splinters via continent to keep our global control group.

Table 4-4 – Regional Heterogeneity

	Dependent Variable: Ln Light Density			
	Africa		*Asia*	
Independent vars:	(ST) Treat Dummy (1)	(MT) Separate Treat Dummy (2)	(MT) Treat Dummy (3)	(MT) Separate Treat Dummy (4)
Decentralization	-0.0184	-0.0352	0.5491	0.5407
	[0.0929]	[0.0909]	[0.2121]**	[0.2136]**
Centralization	0.2853	0.2889	0.0563	0.0571
	[0.0457]***	[0.0586]***	[0.1237]	[0.1237]
	Additional treatments			
2nd Decentralization		-0.1954		0.9423
		[0.1305]		[0.3419]***
3d Decentralization		-0.1806		
		[0.0418]***		
2nd Centralization		0.0895		
		[0.0607]		
Obs.	65210	66024	61492	61492
Splinter-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Adj. R^2	0.977	0.977	0.974	0.974

Notes: The log of population density is included as a control variable in all specifications. *ST* refers to the single-treatment sample, *MT* to the multiple treatment sample. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We also test the timing of centralization and decentralization reforms within the two regions. Table 4-A5 in the Appendix confirms the overall timing structure for centralizations reported in Table 4-3 for Africa. Table 4-A6 shows that the effect of decentralizations in Asia is more immediate, usually right after the reform occurs.[19]

New vs. Absorbed Districts: Next we test whether there is a qualitative difference for the child and absorbing splinters involved in a territorial reform. Table 4-5 reports our results focusing on the single treatment case for our full, African, and Asian treatment samples. Columns 1 to 3 show that our baseline estimates become slightly smaller once we differentiate between the different splinters involved in a reform.

In the full sample (column 1), the coefficient for decentralizations turns out to be significantly positive. Splinters with the former district capital (mothers) gain from decentralization while the child districts don't. Concerning centralizations the effects do not depend on the classification of a splinter as absorbing or absorbed.

Again there are qualitative differences between Asia and Africa. While *child* splinters seem to gain less economic activity in Africa compared to their mother districts, the opposite is the case for Asia. Furthermore we find that *absorbing*

[19]We also checked if the results are sensitive to the reform size within the two regions. Table 4-A7 and Table 4-A8 in the Appendix, show that this is not a concern.

Table 4-5 – Triple-DiD Results

	Dependent Variable: Ln Light Density		
	All	*Africa*	*Asia*
	(1)	(2)	(3)
Decentralization	0.1646	0.0268	0.4766
	[0.0648]**	[0.0675]	[0.2138]**
Centralization	0.2108	0.2441	0.0578
	[0.0667]***	[0.0584]***	[0.1237]
Decentralization × *Child*	-0.0888	-0.0900	0.1924
	[0.1384]	[0.1148]	[0.2670]
Centralization × *Absorbing*	0.1774	0.1582	
	[0.1085]	[0.1035]	
Observations	68532	65210	61294
Country-Splinter-FE	Yes	Yes	Yes
Country-Year-FE	Yes	Yes	Yes
Adj. R^2	0.976	0.977	0.974

Notes: The log of population density is included as a control variable in all specifications. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

splinters seem to gain more from centralization reforms compared to those that have been absorbed, which is in line with evidence on German municipality mergers (Egger et al., 2017). However, the triple-DiD estimates are not precise enough to reject the null for the interaction terms. Since we do not observe centralizations in Asia, we cannot distinguish between absorbing and absorbed here.[20]

Unitary vs. Federal Systems: Our spatial approach delivers a simple and objective classification of administrative reforms. However, it also lumps together reforms occurring in very different political systems. To add this information, we supplement our splinter level panel data set with two country level variables from the Institutions and Elections Project (or IAEP, from Wig et al., 2015). *Govstruct* identifies unitary systems, confederations, and federations. Unitary systems are countries with a strong central government and few or no regional structures. Confederations are countries with strong regional governments and weak central states. Federations are countries where strong central governments coexist with subordinate provincial governments and/or semi-autonomous regions. *Regstruct* records how regional representatives are selected. They can either be elected autonomously, be appointed by the central government, or they do not exist. Table 4-6 shows the distribution of these categories over all country-years in our sample, as well as for Asia and Africa where the bulk of our variation comes from, separately. Clearly, African countries tend to select their subnational representatives at the subnational level, while Asian countries predominantly appoint subnational representatives via the central government.

For now we will focus on the electoral dimension and run another triple-DiD. Restricting oneself to the '*elected*' dimension has two advantages: First, our model stays traceable, since we only have two triple-DiD coefficients of interest. Second,

[20]Note that we cannot allow for multiple treatments in this specification, since a splinter is not necessarily classified in the same way for consecutive reforms.

Table 4-6 – IAEP Matrix

	Selection of Regional Government		
	No regional gov't	Central gov't	Autonomous
		Panel a) All countries	
Unitary system	497	1132	892
Federal system	–	127	717
		Panel b) Only Africa	
Unitary system	85	343	431
Federal system	–	20	146
		Panel c) Only Asia	
Unitary system	109	550	137
Federal system	–	45	142

Notes: The data is from the Institutions and Elections Project (or IAEP, from Wig et al., 2015) There are no confederations (apart from Switzerland, which is never treated in our sample and hence omitted) in our sample. The observation are country-years.

the electoral dimension measures if there are trade-offs between economies of scales and preference heterogeneity, usually associated with larger units (Alesina et al., 1995).

Table 4-7 – Territorial Reforms and Local Power

	Dependent Variable: Ln Light Density		
	All	*Africa*	*Asia*
	(1)	(2)	(3)
Decentralization	0.0991	-0.0018	0.4124
	[0.0789]	[0.0987]	[0.2057]**
Centralization	0.2969	0.3640	-0.1609
	[0.0685]***	[0.0676]***	[0.2428]
Decentralization × *Elected*	0.0581	-0.0251	0.3335
	[0.0809]	[0.0595]	[0.0684]***
Centralization × *Elected*	-0.1207	-0.2417	0.3146
	[0.1037]	[0.0493]***	[0.2037]
Observations	68532	65210	61294
Splinter-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Adj. R^2	0.976	0.977	0.974

Notes: The log of population density is included as a control variable in all specifications. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-7 reports the results of our triple-DiD, interacting the *elected* dummy with our treatment indicators. Column 1 shows that the triple-DiD estimates are not statistically significant based on our full sample. Centralizations have their usual unconditional effect, which in this case is for countries that either have no regional government or appoint it. Again we face substantial regional heterogeneity. Column 2 confirms the Alesina et al. (1995) hypothesis for Africa. Centralizations carried out in countries in which regional governments are not elected have economic gains of around 43%, while those with regional elected governments only gain about 13%. In Asia we find evidence for the same argument, but again on the decentralization effect. Decentralizations in countries without regionally elected government gain only about half the economic activity compared to those in which regional governments are elected. This might be especially surprising given that African countries tend to have comparably more regional elected political bodies compared to Asian countries, although they do not always have much political power.

Proximity to the Administrative Center: Territorial reforms do not only change the composition of people and the size of subnational units, they also affect how close the next administrative center, or city is. Hence, parts of the effects might be explained by changes in the distance to the administrative center. Moving closer to the administrative center might increase the access to public goods, or investments a splinter receives, while moving further away might turn a splinter into the political periphery. To proxy for the effects of political connectedness or administrative proximity, we run a triple-DiD using the distance change between a centroid of a splinter to its current and former administrative center as proxy for changes in the administrative proximity induced by territorial reforms.

Table 4-8 – Proximity to the Administrative Center

	Dependent Variable: Ln Light Density		
	All	*Africa*	*Asia*
	(1)	(2)	(3)
Decentralization	0.1045	-0.0630	0.6992
	[0.1323]	[0.1007]	[0.2193]***
Centralization	0.2987	0.3104	-0.4631
	[0.0523]***	[0.0569]***	[0.0333]***
Decentralization $\times$ Δ*Dist AC*	-0.0003	-0.0007	0.0026
	[0.0004]	[0.0005]	[0.0018]
Centralization $\times$ Δ*Dist AC*	-0.0007	-0.0006	-0.0030
	[0.0003]**	[0.0004]	[0.0000]***
Observations	67256	64792	61052
Splinter-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Adj. R^2	0.976	0.977	0.974

Notes: The log of population density is included as a control variable in all specifications. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-8 reports the results of the administrative proximity triple-DiD. We find that splinters that move further away from the administrative center during a centralization policy do indeed lose economic activity compared to those that do not. In fact the benefits of centralizations are zero once a administrative center moves more than 400 kilometers away. This is, however, only the case for a small group of splinters. Regional differences between Asia and Africa are again substantial. In Africa, the triple-DiD estimates are not statistically different from zero. In Asia in turn centralizations are now always related to a loss in economic activity which is amplified by an increase in the distance to a splinters capital. For decentralizations we can not reject the null of the triple-DiD estimate.

4-6 Conclusion

This paper studies the effect of administrative territorial reforms on economic activity. Based on data on the borders of first-order subnational administrative units, we trace changes in the territory over time. Our data set consists of literally all countries in the world and covers the period from 1992 to 2014. Our unit of observation are micro-regions (splinters) that are the smallest subnational territories never been reformed during our observation period. These regions might have a history of being a result of a territorial decentralization or centralization reform. In case of decentralizations, the micro-regions have had a larger size in the past; in case of centralizations the micro-regions haven been merged with another one. Note that our approach is able to capture multiple treatments which occur in several countries. Based on this information, we estimate how territorial administrative reforms impact economic activity measured by satellite nighttime lights, using (triple) DiD panel estimators as well as event study designs.

Our results are as follows. We find that territorial centralizations lead to a roughly 30% increase in economic activity in the centralized areas. We cannot confirm any stable relationship between decentralizations and economic activity in the full sample of countries. Our event study implies that the effect of centralizations take around 4 years to phase in. Importantly, our global analysis reveals substantial regional variation in the effects of territorial reforms on economic activity. Centralizations are primarily beneficial in Africa, while decentralizations have proven successful to increase economic activity in Asia.

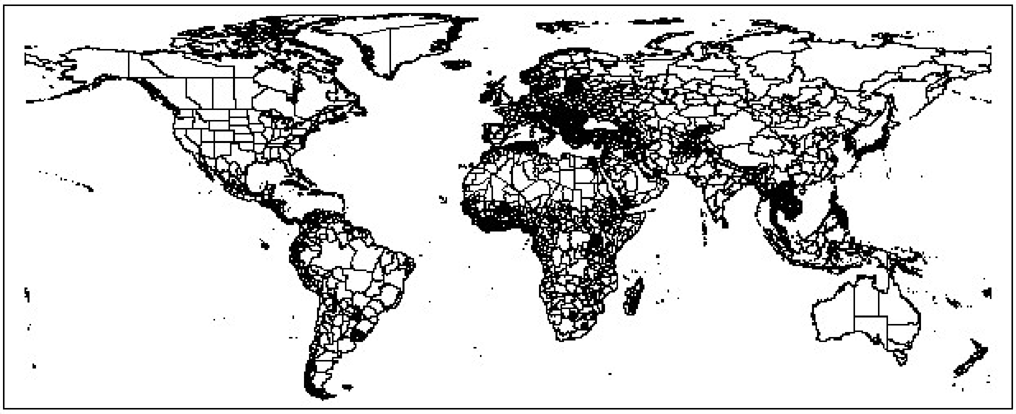
We investigate how administrative reforms interact with the broader federal-political structure of a country, and how proximity to administrative centers affect our reformed splinters. We find that local power matters substantially. Decentralizations are most beneficial if local governments are selected on the regional level. Contrary, centralizations are less beneficial if local governments are elected locally. Thus, it seems that territorial administrative reforms work best if they fit to the general federal-political structure of a country. Regarding political proximity, we find that areas that are moved further away from the local center of power, gain substantially less from centralizations reforms compared to those that are more centrally located. However, these distinctions do not solve the puzzle around the major differences between Asia and Africa.

Given the large heterogeneity of the effects, we cannot derive clear cut policy conclusions. An important result is that there is no clear unconditional positive effect in favor of either decentralization or centralization reforms. The effectiveness of reforms are highly context specific. Our results imply that existing case studies have little external validity. More research into the topic is of vital importance.

Appendix

4-6.1 Additional Tables and Figures

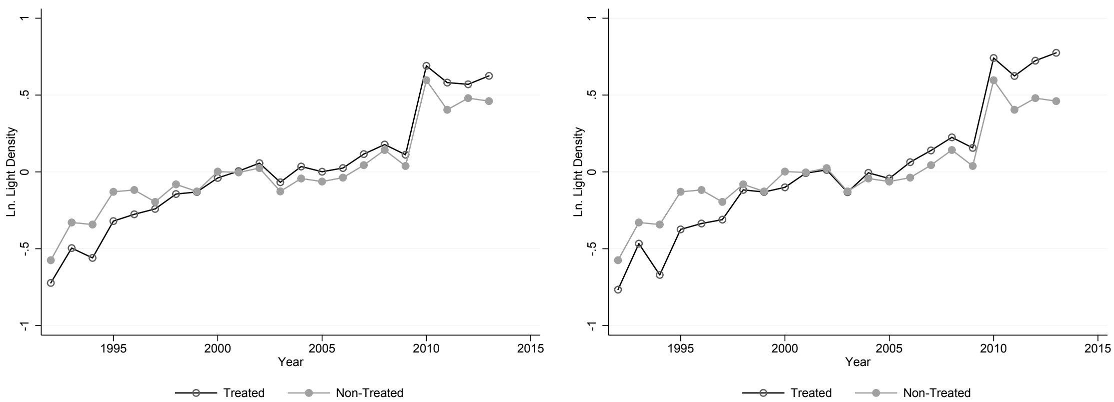
Figure 4-A1 – Splinter Sample



Notes: The figure illustrates the primary subnational administrative unit trace splinters generated from subnational border changes between 1990 and 2014.

Figure 4-A2 – Splinter Demeaned-Light Density Trends Between Treated and Non-Treated

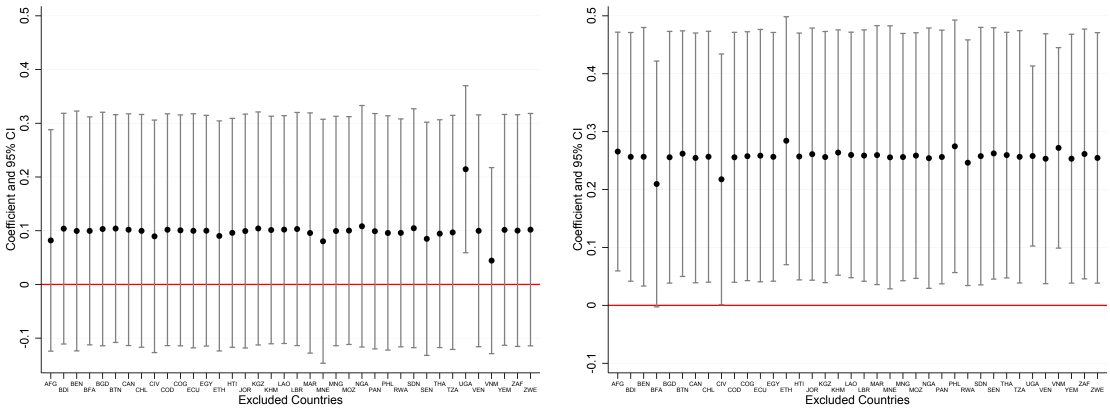
(a) Decentralization **(b)** Centralization



Notes: Panel A reports the trends of log light density between the control group and the group of splinters experiencing decentralization. Panel B reports the trends of log light density between the control group and the group of splinters experiencing centralizations.

Figure 4-A3 – Leave-One-Out Test

(a) Decentralization **(b)** Centralization



Notes: Panel A reports the coefficients and 95% intervals of the decentralization estimate of column 1 of our baseline specification Table 4-2 excluding on treated country at the time. Panel B reports the coefficients and 95% intervals of the centralization estimate of column 1 of our baseline specification Table 4-2 excluding on treated country at the time

Table 4-A1 – Countries, Reforms and Districts

Reformed country	**Reform year**	**Districts before**	**Districts after**	**Fraction reformed**	**Fraction decentralized**	**Fraction centralized**
Afghanistan	1995	31	32	0.0364	0.0364	0
Afghanistan	2004	32	34	0.0560	0.0560	0
Bangladesh	1993	4	5	0.223	0.223	0
Bangladesh	1998	5	6	0.310	0.310	0
Bangladesh	2010	6	7	0.246	0.246	0
Benin	1999	6	12	0.974	0.974	0
Bhutan	1992	18	20	0.508	0.365	0.144
Burkina Faso	2001	45	13	0.989	0	0.989
Burundi	1991	15	16	0.0645	0.0645	0
Burundi	1998	16	17	0.0553	0.0553	0
Côte d'Ivoire	1991	50	10	1	0	1
Côte d'Ivoire	1997	10	16	0.640	0.515	0.125
Côte d'Ivoire	2000	16	18	0.190	0.190	0
Côte d'Ivoire	2001	18	19	0.0968	0.0968	0
Côte d'Ivoire	2012	19	14	0.516	0.0130	0.503
Cambodia	1995	24	25	0.0946	0.0946	0
Cambodia	1997	25	26	0.0713	0.0713	0
Canada	1999	12	13	0.348	0.348	0
Chad	2000	14	28	0.835	0.835	0
Chile	2007	13	15	0.167	0.167	0
Congo	1995	9	10	0.216	0.216	0
Congo	2002	10	11	0.000721	0.000721	0
Congo	2003	11	12	0.000610	0.000610	0
Congo, Dem. Rep.	1997	9	11	0.108	0.108	0
Ecuador	1998	22	23	0.134	0.134	0
Ecuador	2007	23	25	0.126	0.126	0
Egypt	2009	26	27	0.0141	0.0141	0
Ethiopia	1994	13	10	0.986	0.145	0.841
Ethiopia	1998	10	11	0.000933	0.000933	0
Gambia	2003	7	6	0.0104	0	0.0104

Continued on next page

Table 4-A1 – continued from previous page

Reformed country	**Reform year**	**Districts (before)**	**Districts (after)**	**Fraction reformed**	**Fraction decentralized**	**Fraction centralized**
Guinea	1996	7	8	0.167	0.167	0
Guinea-Bissau	1991	8	9	0.0256	0.0256	0
Haiti	2003	9	10	0.116	0.116	0
India	2001	31	34	0.308	0.308	0
Indonesia	2000	26	27	0.0412	0.0412	0
Indonesia	2001	27	31	0.310	0.310	0
Indonesia	2003	31	32	0.0517	0.0517	0
Indonesia	2005	32	33	0.0332	0.0332	0
Jordan	1996	8	12	0.583	0.583	0.000304
Kyrgyzstan	1999	6	7	0.231	0.231	0
Kyrgyzstan	2000	7	8	0.00108	0.00108	0
Laos	1995	17	18	0.143	0.143	0
Laos	2006	18	17	0.143	0	0.143
Lebanon	1993	5	6	0.192	0.192	0
Liberia	2000	13	14	0.165	0.165	0
Liberia	2001	14	15	0.205	0.205	0
Mali	1991	8	9	0.262	0.262	0
Mongolia	1996	21	22	0.0735	0.0735	0
Morocco	1997	7	15	0.710	0.710	7.89e-05
Nigeria	1997	31	37	0.269	0.267	0.00189
Panama	1997	10	11	0.384	0.384	0
Panama	1998	11	12	0.215	0.215	0
Philippines	1995	15	16	0.201	0.201	0
Philippines	2002	16	17	0.218	0.158	0.0603
Rwanda	2006	12	5	1	0.0233	0.977
Senegal	2002	10	11	0.432	0.388	0.0439
Senegal	2008	11	14	0.494	0.494	0
South Africa	1994	4	9	0.818	0.816	0.00199
Sudan	1991	18	9	0.991	0	0.991
Sudan	1994	9	26	0.991	0.991	0
Sudan	2006	26	25	0.197	0.0474	0.150
Sudan	2011	25	15	0.0695	0.0695	0
Tanzania	2002	25	26	0.0904	0.0904	0
Tanzania	2012	26	30	0.276	0.276	0
Thailand	1994	73	76	0.0883	0.0883	0
Uganda	2005	69	70	0.00827	0.00827	0
Uganda	2006	70	77	0.193	0.193	0
Uganda	2007	77	80	0.0358	0.0358	0
Uganda	2009	80	87	0.183	0.183	0
Uganda	2010	87	112	0.358	0.357	0.000994
Venezuela	1998	23	24	0.00145	0.00145	0
Vietnam	1992	44	53	0.358	0.355	0.00360
Vietnam	1997	53	61	0.158	0.158	0
Vietnam	2004	61	64	0.144	0.139	0.00516
Yemen	1994	19	20	0.0313	0.0313	0
Yemen	2004	20	21	0.0304	0.0304	0
Zimbabwe	1997	8	10	0.0859	0.0859	0

Notes: The figure list the years in which a country is treated with spatial administrative reform, as well as the number of districts before and after reform. Additionally it list the fraction of districts treated with any reforms, decentralization and centralization.

Table 4-A2 – Static Selection into Treatment on Initial Values

	Dependent Variable: Ever treated			
	Without country FEs		*With country FEs*	
	(1)	(2)	(3)	(4)
	Splinter characteristics			
Initial Light Density	-0.0803	-0.0801	-0.0118	-0.0116
	(0.0040)***	(0.0136)***	(0.0070)*	(0.0081)
Initial Distance to District Capital	0.0007	0.0007	0.0004	0.0004
	(0.0001)***	(0.0003)***	(0.0001)***	(0.0002)**
Initial Population	0.0801	0.0338	0.0145	0.0180
	(0.0058)***	(0.0345)	(0.0061)**	(0.0138)
	District characteristics			
Initial Area		-0.0000		0.0000
		(0.0002)		(0.0001)
Initial Population		0.0487		-0.0038
		(0.0380)		(0.0136)
Observations	2112	2112	2106	2106
R^2	0.198	0.202	0.624	0.624

Notes: Standard errors are clustered at the splinter level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A3 – Main Results Including Area Change Thresholds

	Dependent Variable: Ln Light Density			
	(1)	(2)	(3)	(4)
	Δ5%	Δ10%	Δ25%	Δ50%
Decentralization	0.1231	0.1200	0.1171	0.0936
	[0.1011]	[0.1028]	[0.1104]	[0.0736]
Centralization	0.2561	0.2434	0.2596	0.2491
	[0.0529]***	[0.0538]***	[0.0478]***	[0.0549]***
Observations	68268	67872	66860	65232
Splinter-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Adj. R^2	0.976	0.976	0.976	0.976

Notes: The log of population density is included as a control variable in all specifications. Table 4-A7 and Table 4-A8 show the results for Africa and Asia separately. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A4 – Timing of the Effects (B): Decentralizations

	Dependent Variable: Ln Light Density						
	Event sequence size						
	$S = 2$	$S = 3$	$S = 4$	$S = 5$	$S = 6$	$S = 7$	$S = 8$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pre-treatment						
t_{-2}	-0.0147	-0.0124	-0.0100	-0.0087	0.0069	0.0240	0.0311
	[0.0971]	[0.1064]	[0.1223]	[0.1367]	[0.1529]	[0.1645]	[0.1758]
t_{-1}	0.0194	0.0226	0.0244	0.0258	0.0417	0.0590	0.0661
	[0.0970]	[0.1077]	[0.1241]	[0.1390]	[0.1556]	[0.1674]	[0.1794]
	Post-treatment						
$Treat$	0.0180	0.0218	0.0255	0.0271	0.0441	0.0620	0.0698
	[0.0985]	[0.1064]	[0.1220]	[0.1364]	[0.1520]	[0.1631]	[0.1747]
t_1	0.0112	0.0158	0.0189	0.0209	0.0374	0.0555	0.0635
	[0.0812]	[0.0895]	[0.1051]	[0.1196]	[0.1352]	[0.1462]	[0.1577]
t_2	-0.0191	-0.0140	-0.0102	-0.0085	0.0088	0.0269	0.0350
	[0.1118]	[0.1210]	[0.1376]	[0.1525]	[0.1690]	[0.1805]	[0.1920]
t_3		0.0008	0.0051	0.0071	0.0241	0.0426	0.0502
		[0.1315]	[0.1483]	[0.1629]	[0.1793]	[0.1911]	[0.2022]
t_4			0.0199	0.0221	0.0402	0.0590	0.0675
			[0.1438]	[0.1585]	[0.1748]	[0.1863]	[0.1976]
t_5				0.1661	0.1849	0.2042	0.2123
				[0.1566]	[0.1724]	[0.1837]	[0.1944]
t_6					0.1362	0.1561	0.1646
					[0.1978]	[0.2090]	[0.2192]
t_7						0.2252	0.2341
						[0.2143]	[0.2247]
t_8							0.2599
							[0.2208]
Obs.	68532	68532	68532	68532	68532	68532	68532
Splinter-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.976	0.976	0.976	0.976	0.976	0.976	0.976

Notes: The log of population density is included as a control variable in all specifications.S refers to the size of the pre- and post-treatment sequence. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A5 – Timing of the Effects Africa: Centralizations

	Dependent Variable: Ln Light Density						
	Event sequence size						
	$S = 2$	$S = 3$	$S = 4$	$S = 5$	$S = 6$	$S = 7$	$S = 8$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pre-treatment						
t_{-2}	0.1041	0.1065	0.1060	0.1017	0.0877	0.0672	0.0706
	[0.1123]	[0.1310]	[0.1372]	[0.1319]	[0.1266]	[0.1244]	[0.1222]
t_{-1}	0.0336	0.0359	0.0343	0.0303	0.0151	-0.0050	-0.0018
	[0.0983]	[0.1146]	[0.1208]	[0.1162]	[0.1114]	[0.1102]	[0.1095]
	Post-treatment						
$Treat$	0.0831	0.0857	0.0840	0.0798	0.0660	0.0448	0.0482
	[0.0918]	[0.1111]	[0.1169]	[0.1115]	[0.1053]	[0.1012]	[0.0993]
t_1	0.1278	0.1308	0.1298	0.1256	0.1114	0.0912	0.0935
	[0.0760]*	[0.0932]	[0.0989]	[0.0927]	[0.0878]	[0.0863]	[0.0861]
t_2	0.1255	0.1286	0.1278	0.1235	0.1084	0.0880	0.0917
	[0.1163]	[0.1342]	[0.1395]	[0.1321]	[0.1260]	[0.1232]	[0.1223]
t_3		0.1394	0.1387	0.1343	0.1201	0.0988	0.1022
		[0.0935]	[0.0992]	[0.0930]	[0.0883]	[0.0863]	[0.0867]
t_4			0.2418	0.2372	0.2232	0.2027	0.2057
			[0.0654]***	[0.0625]***	[0.0587]***	[0.0575]***	[0.0592]***
t_5				0.3864	0.3725	0.3518	0.3555
				[0.0687]***	[0.0725]***	[0.0753]***	[0.0802]***
t_6					0.3720	0.3516	0.3553
					[0.1058]***	[0.1071]***	[0.1103]***
t_7						0.4151	0.4191
						[0.1277]***	[0.1311]***
t_8							0.2945
							[0.0927]***
Obs.	65210	65210	65210	65210	65210	65210	65210
Splinter-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.977	0.977	0.977	0.977	0.977	0.977	0.977

Notes: The log of population density is included as a control variable in all specifications. S refers to the size of the pre- and post-treatment sequence. The coefficients for decentralizations for the same specification are reported in Table 4-A4. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A6 – Timing of the Effects Asia: Decentralizations

	Dependent Variable: Ln Light Density						
	Event sequence size						
	$S=2$	$S=3$	$S=4$	$S=5$	$S=6$	$S=7$	$S=8$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pre-treatment						
t_{-2}	0.0588	0.0976	0.1871	0.2168	0.2891	0.3587	0.4102
	[0.1715]	[0.1999]	[0.2314]	[0.2705]	[0.2849]	[0.3222]	[0.3518]
t_{-1}	0.1493	0.1890	0.2780	0.3081	0.3808	0.4505	0.5024
	[0.1599]	[0.1882]	[0.2146]	[0.2536]	[0.2645]	[0.3000]	[0.3282]
	Post-treatment						
$Treat$	0.1656	0.2091	0.3031	0.3354	0.4104	0.4821	0.5359
	[0.1545]	[0.1837]	[0.2126]	[0.2509]	[0.2586]	[0.2912]*	[0.3175]*
t_1	0.2437	0.2873	0.3817	0.4143	0.4889	0.5607	0.6147
	[0.1740]	[0.2041]	[0.2326]	[0.2721]	[0.2795]*	[0.3115]*	[0.3363]*
t_2	0.2976	0.3415	0.4355	0.4683	0.5432	0.6146	0.6687
	[0.1850]	[0.2156]	[0.2430]*	[0.2836]	[0.2923]*	[0.3247]*	[0.3504]*
t_3		0.3866	0.4810	0.5137	0.5887	0.6604	0.7140
		[0.2399]	[0.2671]*	[0.3078]*	[0.3173]*	[0.3505]*	[0.3751]*
t_4			0.4868	0.5201	0.5959	0.6681	0.7225
			[0.2913]*	[0.3333]	[0.3443]*	[0.3765]*	[0.3999]*
t_5				0.6660	0.7418	0.8143	0.8687
				[0.3802]*	[0.3932]*	[0.4266]*	[0.4502]*
t_6					0.8409	0.9133	0.9678
					[0.4188]**	[0.4521]**	[0.4751]**
t_7						0.9888	1.0434
						[0.4454]**	[0.4682]**
t_8							1.0411
							[0.4834]**
Obs.	61294	61294	61294	61294	61294	61294	61294
Splinter-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.974	0.974	0.974	0.974	0.974	0.974	0.974

Notes: The log of population density is included as a control variable in all specifications. S refers to the size of the pre- and post-treatment sequence. The coefficients for decentralizations for the same specification are reported in Table 4-A4. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A7 – Africa: Main Results Including Area Change Thresholds

	Dependent Variable: Ln Light Density			
	(1)	(2)	(3)	(4)
	Δ5%	Δ10%	Δ25%	Δ50%
Decentralization	-0.0191	-0.0292	-0.0421	-0.0245
	[0.0965]	[0.0944]	[0.0976]	[0.0665]
Centralization	0.2853	0.2586	0.2679	0.2540
	[0.0457]***	[0.0523]***	[0.0480]***	[0.0553]***
Observations	65122	64924	64352	63362
Splinter-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Adj. R^2	0.977	0.977	0.977	0.977

Notes: The log of population density is included as a control variable in all specifications. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4-A8 – Asia: Main Results Including Area Change Thresholds

	Dependent Variable: Ln Light Density			
	(1)	(2)	(3)	(4)
	Δ5%	Δ10%	Δ25%	Δ50%
Decentralization	0.5718	0.5737	0.6306	0.3569
	[0.2185]***	[0.2206]**	[0.2354]***	[0.1800]**
Centralization	0.0472	0.2124	0.3275	0.2940
	[0.1453]	[0.1798]	[0.2315]	[0.3808]
Observations	61140	60964	60634	60128
Splinter-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Adj. R^2	0.974	0.974	0.974	0.974

Notes: The log of population density is included as a control variable in all specifications. Standard errors are clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4-6.2 Coding Administrative Centers (State Capitals)

To identify and geo-code our administrative centers we proceed in several steps. First, we collect a list of primary subnational administrative centers (state capitals) from the 'statoids' database (Law, 2002). The statoids database is the most comprehensive database of first and second order administrative districts. It aggregates data from a variety of sources, such as administrative registers and comprehensive encyclopedia, such as the Encyclopedia Britannica.[21] It is worth mentioning that the statoids database and GAUL vector data do not always agree on which subnational unit is the primary administrative one. In such cases we manually check the district and general administrative layering of a country by consulting several sources, e.g., the CIA Factbook (Central Intelligence Agency, 2018).

In a second step we collect the locations of our administrative cities, i.e., the longitude and latitude of the city centroids using the 'OpenStreetMap' (OSM) (OpenStreetMap contributors, 2017) and the Google Maps API service (see Google Maps API, 2017). OSM and the Google API provide the absolute majority of all coordinates without any problems. OSM has several advantages; the most important one is that it returns more information about the coordinate 'place' match compared to Google. Specifically, OSM allows us to check if we received the actual city centroid or the centroid of a district that has the same name.[22] Unfortunately, not all cities can be coded automatically. In cases in which it is not possible we coded the city centroids manually. In Uganda, for example, we had to manually code 50 out of 112 administrative centers.

To asses the quality of our coordinates we implemented a three-step quality assessment. First, we use our universe of splinters constructed from the GAUL vector data and test if all districts active within a year contain only one administrative center as it should be by construction. Second, we manually check individual city coordinates if we do not obtain a unique place match in OSM. Furthermore we randomly check city coordinates for which we obtain an OSM match. Finally, we match out panel of subnational administrative capitals with the Global Rural-Urban Mapping Project (GRUMP) (Balk et al., 2006; GRUMP, 2017). The GRUMP data set is constructed and maintained by the Socioeconomic Data and Applications Center, part of the NASA's Earth Observing System Data and Information System and it obtains location as well as population information about nearly all settlement points on earth.[23] We spatially match our coordinates with the GRUMP database using a buffer of 3km to allow for small differences in the coordinates between our administrative centers and the settlement data. If administrative centers are not matched to the GRUMP settlement data, we qualitatively check each of those cases again.[25] Note that all three steps are iterated until we do not encounter further

[21]We are not the first to use the database to code subnational units. Grossman et al. (2017) also use it as there primary source to count district proliferation.

[22]Note, that is is quite common that districts are named after their administrative center

[23]The latitude and longitude information are gathered by consulting multiple sources, including the Digital Chart of the World, City Population database, World Gazetteer and Falling Rain (Balk et al., 2006; GRUMP, 2017).[24] The data set of GRUMP was collected between 2005 and 2010, in which geo-referencing sources provided merely a low level of quality (Balk et al., 2006; GRUMP, 2017). To tackle this issue, a revision of Version 1 was operated and released in 2017 (Balk et al., 2006; GRUMP, 2017).

[25]We also look up and correct mismatches in city names between GRUMP and our

problems in any of the steps.

Issues arising during our coordinate assignment procedure and the quality assessment can be grouped into three broad categories specifically related to one of the three steps. Most of the problems appear in our initial match with our splinter universe. The main reasons for a mismatch between the GAUL vector data and the statoids database result from different codings of districts and timing. For example the GAUL database codes the three geographic regions of Malawi as the first order administrative districts, while statoids documents the districts— second order administrative districts following GAUL. After consulting various sources, we could confirm that the three geographical regions have never been political units. Thus, we extract the actual districts out of the second order administrative district vector data from GAUL and use them to form our splinter universe in Malawi. Following this procedure we do no loner obtain any mismatches. Note that combining the two datasets increases the quality of our splinter universe significantly, since it allows us to automatically find potential problems in either dataset. If both datasets agree, a potential coding error in the raw data is much less likely. The second issue arising between GAUL and statoids is the timing of reforms. GAUL codes a border reform during the year it occurred, no matter the timing. Statoids on the other hand relies on a multitude of sources, which either report the exact date, or use some aggregation rule. If there are timing issues in our match, we always use the GAUL definition and impose the administrative center status on the Statoids data. Note that we can still allow for different coding of early and late reforms in our analysis, since splinters are time invariant, and the reform data can be adjusted later on. The GAUL - statoids match identifies some districts, which do not host an administrative center at all, e.g. ‘Bujumbura Rural’ in Burundi who’s administrative center is Bujumbura located in ‘Bujumbura Urban’. For those cases we code a dummy noting that a splinter (or district) has an externally located administrative center.

Problems with OSM, Google and GRUMP are of a more limited variety. They are mostly related to imprecise coding of coordinates or multiple matches. In those cases we qualitatively check the coordinates using official maps, and reassign the coordinates accordingly.

administrative centers.

Chapter 5

Terrorism and international migration

The role of migration policies and origin country characteristics[*]

> *...the 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 for terrorist operations, here and abroad..*
>
> George W. Bush, 2001

> *Immigration and Jihad go together. One is the consequence of the other and dependent upon it.*
>
> Imam Abu Baseer[†]

5-1 Introduction

There is clear and systematic evidence that 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. Arguably, it is easier to restrict the rights of foreigners in order to increase the (perceived) security of a country's natives than to restrict the rights of these natives (i.e., voters) themselves.

[*] This chapter is based on joint work with Axel Dreher and Martin Gassebner (Dreher et al., 2017).

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

Plenty of evidence suggests that stricter immigration and visa policies are a preferred reaction to terrorist attacks (Fitzpatrick, 2002; Martin and Martin, 2003; Avdan, 2014).[1] After the September 11, 2001 (hereafter 9/11) attacks on the United States, U.S. President George W. Bush issued a Presidential Directive introducing stricter immigration policies to combat terrorism. The new Department of Homeland Security (DHS) was founded in 2003, incorporating the former Immigration and Naturalization Service (INS). The new Department explicitly links immigration policies to anti-terrorism 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 hearings and proof of guilt if there is '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). In the 2016 US-Presidential election, the Republican candidate promised to ban all Muslims from immigration to the United States if he were to win the election. Directly after his inauguration he issued a travel ban for six predominantly Muslim countries.

The United Kingdom equally tightened immigration policies in the wake of 9/11, most notably with the introduction of the Anti-terrorism, Crime and Security Act 2001 (Spencer, 2007). Under the Act, the Secretary of State for the Home Department is allowed to order the detention of foreigners based on mere suspicion of terrorist involvement, without trial.[2] As Spencer (2007) summarizes, France, Germany, and Spain, among others, have similarly tightened immigration laws or procedures in response to the terrorist attacks of 9/11.

In light of these reactions to terror, evidence that liberal immigration and integration policies or the number of foreigners living in a country increase terrorism is surprisingly scarce. The only systematic statistical analysis we are aware of reports a negative correlation between migration and terrorist attacks (Bove and Böhmelt, 2016).[3] Other previous studies that address the link between terror and migration either examine the effect of terror on migration (e.g., Dreher and Fuchs, 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. The absence of a causal investigation about whether and to what extent migration induces terror is an important gap in the literature.

We fill this gap and analyze the effect of immigration on terrorist attacks in an instrumental variable setting. We predict the stock of foreigners with the interactions between two sets of variables. Variation across host-origin-dyads results from structural characteristics between the country of origin and the host, while

[1] Also see Bandyopadhyay and Sandler (2014) game-theoretic model on immigration policy and counterterrorism.

[2] The act was deemed unlawful in 2004, which is why the Prevention of Terrorism Act 2005 was passed, allowing the Home Secretary to impose 'control orders' on everyone suspected of being involved in terrorism.

[3] There is, however, evidence that the number of refugees hosted in a country are correlated with a larger number of terrorist attacks (Milton et al., 2013).

variation over time (and dyads) originates from changes in push and pull factors between host and origin countries resulting from natural disasters.[4] Controlling for the levels of these variables and fixed effects for dyads and years, the interactions provide a powerful and excludable instrument. As we explain in some detail below, the intuition of our instrumentation strategy is in analogy to a difference-in-difference estimator, where we assume changes in the number of disasters to differentially affect terror in countries with different structural characteristics exclusively due to the number of migrants there (rather than via any omitted variables).[5]

Our data include 20 OECD host countries and 183 countries of origin over the 1980-2010 period. This focus on countries and years – rather than individual migrants and terrorists – has a number of advantages but also comes at some cost. On the downside, most importantly, our 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. Most obviously, migrants can turn into terrorists themselves. However, their presence can also affect the probability that others turn violent. As one example, 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. As another, 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. Only country-level data, such as those that we use here, are suited to test these broader effects of migration on terror.

We find 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 imply that foreigners are more likely to become terrorists compared to the domestic population. When we calculate the effect of a larger native population on the probability of terror attacks by natives, 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 refine the basic analysis in several ways and analyze how politics and economics, origin country characteristics, and the composition of migrant populations mediate the effect of migration on terror. We also test whether and to what extent immigration and integration policies change the effect of foreigners on terror. Our results show that domestic policies relating to the integration and prospects of immigrants as well as immigration policies affect the probability that foreigners turn violent. More specifically, our analysis demonstrates that restrictions on migrants' rights and stricter immigration laws increase the effect of migrants 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 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

[4]This follows previous literature on migration (see Alesina et al., 2016; Docquier et al., 2016).

[5]We use the term migrants and foreign born populations interchangeably, since our data does not allow us to be more specific (see Section 5-4).

there.

Our results show that highly skilled migrants are associated with a significantly lower risk of terror compared to low skilled ones, while there is no significant difference in terror arising from male compared to 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 to increase the likelihood of terror compared to other foreign populations.

The Section 5-2 discusses the previous evidence linking immigration to terrorism and introduces our hypotheses. We outline our data in Section 5-3 and our empirical strategy in Section 5-4. Section 5-5 shows the main results, Section 5-6 tests robustness, and Section 5-7 concludes and discusses policy implications.

5-2 Terror and Migration

While there is no evidence of a systematic effect of immigration on terrorism, plenty of anecdotes and opinion-based writings, in concert with a number of descriptive evaluations of terrorist events exist.[6] Somewhat systematic evidence is offered in the few studies analyzing the vitas of known or suspected terrorists. Among these, 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 1993-2003. 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 1993-2004.

All these studies find that terrorism is strongly associated with immigration. Camarota (2002, p.5) consequently concludes that 'there is probably no more important tool for preventing future attacks on U.S. soil than the nation's immigration system.' However, based on terrorists' vitas summarized in the previous literature, in the vast majority of cases, foreigners committing global terrorism have lived in the country they attack for an extended period of time rather than entering and immediately engaging in an attack.[7] 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.

[6]A particularly prominent example of opinion-based 'analysis' is Malkin (2002) bestseller *Invasion*, suggesting a range of discriminatory measures against immigrants to prevent the migration of terror.

[7] For example, the metro and rail bombings in Paris during the mid-1990s have been conducted by "legal" French 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 Brussel'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 (http://www.nbcnews.com/storyline/brussels-attacks/najim-laachraoui-what-we-know-about-suspected-bomb-maker-n543996, accessed November 13, 2016).

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. Larger 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, 2017). 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 natives affects terrorism by those natives.

It is also important to understand what factors influence this scale effect. We analyze three groups of potential confounders: the political and economic environment in the host country, characteristics of the origin country, and the composition of migrant stocks.[8] 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 natives, arguably) to engage in terrorist activity (Findley et al., 2012; Gould and Klor, 2016). Most importantly, 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, 2016).

A second important dimension of host country policies concerns immigration. Policies on immigration 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

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

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 policies reduce the risk that migrants engage in terror against their host country when immigration restrictions are put in place.

Second, 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). 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 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[9] 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. 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 males and females 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 to their peers (Krueger and Malečková, 2003).

5-3 Data

We aim to test whether a larger number of foreigners from a particular country increases the probability of terrorist attacks from people of that nationality in their host country. We define $TERROR_{hot}$ as a binary indicator that is one if at least

[9] 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?'

one terrorist attack is conducted by nationals of origin o in host country h during year t.[10] 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. 2014). ITERATE is the only database that provides data on global terrorist acts, including information about the nationality of perpetrators and victims.[11]

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 aged 25 years and older living in a country other than the country they were born (not distinguishing between 'regular' migrants and refugees).[12] 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.[13] Since the stock of foreigners typically evolves slowly over time, we linearly interpolate the years in between the five-year intervals.[14] We expect this to introduce random noise, while allowing us to exploit yearly variation in the terrorist data. We report results without interpolation to test robustness in Section 5-6.[15]

Figure 5-1 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

[10]Note that we use a binary indicator since 99.5 percent of our dyad-year observations show no transnational terror events, while of the remainder, around 80 percent are one, 15 percent are between 2 and 4, and the remaining 5 percent range between 5 and 17 incidents.

[11]Mickolus et al. (2014: 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.'

[12]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).

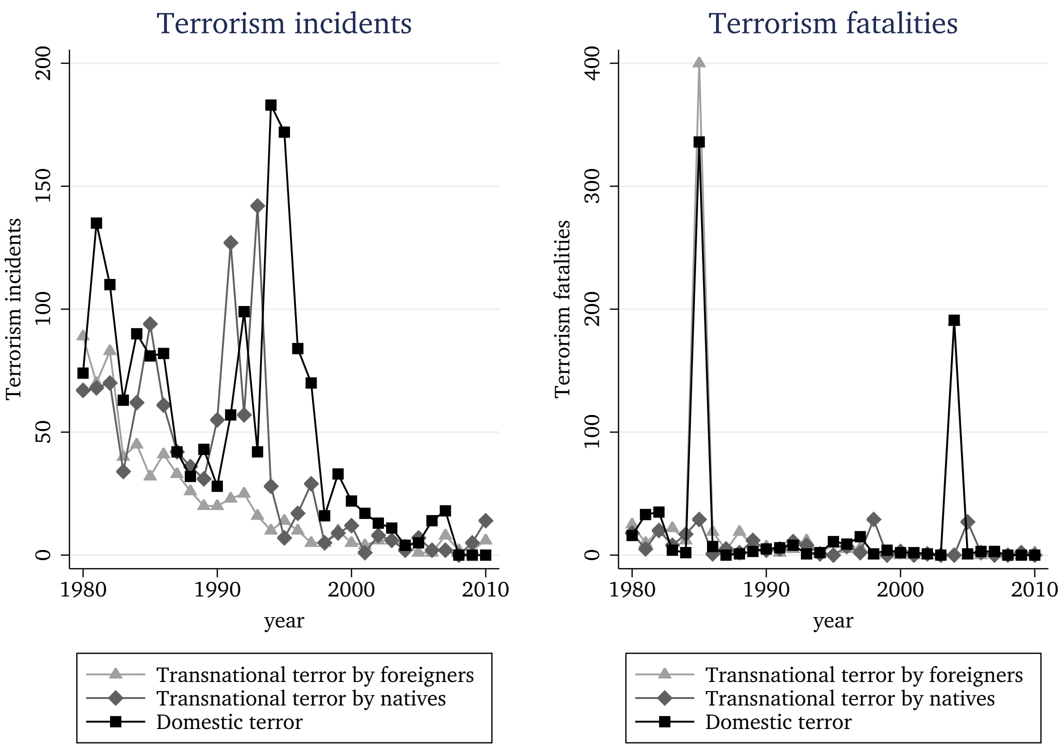
[13]This is important, since observations for the stock of migrants missing from other data sources in particular countries and (different) years are unlikely to be missing at random, but rather for reasons that could arguably be correlated with terror itself. The downside is that we do not observe the 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).

[14]The differences in reported Migration stocks are between -22% and 200% in 95% of the observations

[15]We also test robustness by excluding host- 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).

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.

Figure 5-1 – 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 also shows the number of terrorist attacks of OECD $NATIVES$ on $FOREIGNERS$ in their country, as well as from $NATIVES$ on $NATIVES$ ('domestic terrorism').[16]

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 from $NATIVES$ on either $NATIVES$ or $FOREIGNERS$ exceed those from $FOREIGNERS$ most of the time. Fatalities from these attacks are typically infrequent, as can be seen from the right panel of Figure 5-1. 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

[16] 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" (World Bank 2016). ITERATE exclusively includes 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 exclusively captures events in which $NATIVES$ attack $FOREIGNERS$. Domestic attacks are those where both the perpetrator and the victim originate from the country the attack takes place (taken from Enders et al. (2011) and Gaibulloev et al. (2012) based on data from the Global Terrorism Database, GTD).

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).

Table 5-1 – Decomposition of Terror Incidents, 1980-2010

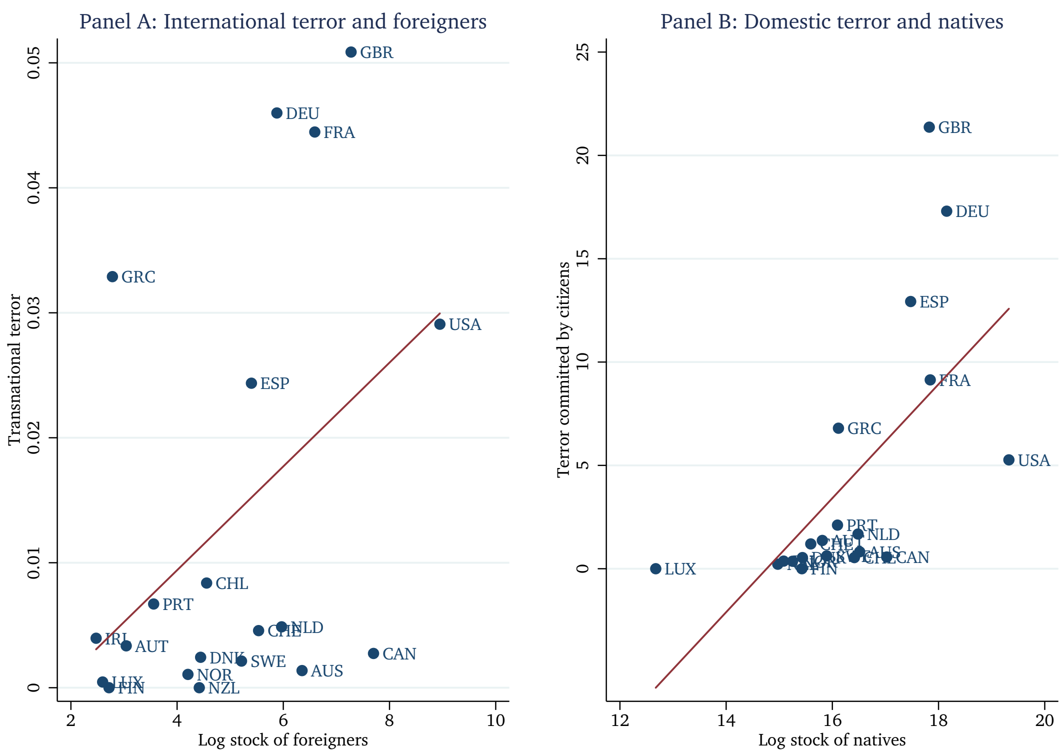
Host-countries	Sum of terror incidents (total)	Average amount of terror incidents	Percentage committed by native born	Percentrage committed by foreign born	Terror committed per million native born	Terror committed per million foreign born
Australia	24	0.774	0.75	0.25	0.039	0.061
Austria	63	2.032	0.71	0.29	0.197	1.070
Canada	32	1.044	0.54	0.46	0.023	0.109
Chile	67	2.170	0.96	0.04	0.153	0.157
Denmark	31	1.009	0.62	0.38	0.123	1.890
Finland	0	0.000	–	–	0	0
France	471	15.183	0.67	0.33	0.182	1.360
Germany	753	24.295	0.87	0.13	0.276	0.773
Greece	319	10.291	0.88	0.12	0.915	2.280
Ireland	31	1.000	0.26	0.74	0.073	3.290
Luxembourg	3	0.112	0.43	0.57	0.149	0.676
Netherlands	75	2.419	0.63	0.37	0.105	0.901
New Zealand	5	0.161	1.00	0.00	0.051	0.000
Norway	13	0.419	0.69	0.31	0.069	0.672
Portugal	68	2.198	0.90	0.10	0.201	0.595
Spain	412	13.305	0.92	0.08	0.313	0.680
Sweden	29	0.935	0.69	0.31	0.081	0.391
Switzerland	70	2.260	0.59	0.41	0.223	0.884
UK	748	24.133	0.92	0.08	0.401	0.619
United States	305	9.830	0.60	0.40	0.024	0.206
Average	176	5.679	0.72	0.28	0.180	0.831

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

To put these numbers in perspective, Table 5-1 reports the average and total number of terrorist attacks in each OECD country per year during the 1980-2010 period, along with the percentage of those numbers committed by $NATIVES$ compared to $FOREIGNERS$. 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, 0.18 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 very 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, there are 10 attacks by foreigners in the median year (1996): four attacks in Germany and three attacks in France and the United States, respectively.[17]

Figure 5-2 further illustrates the scale effects of foreign and domestic populations with respect to terror. The left panel shows that the number of attacks from $FOREIGNERS$ increases with the stock of migrants living in an OECD country. According to the right panel of Figure 5-2, the number of $NATIVES$ living in an OECD country is positively correlated with the number of terrorist attacks from $NATIVES$. Both correlations are unsurprising.

Figure 5-2 – Transnational and Domestic Terror Incidents across the OECD



Notes: The figure plots the average log of migrants against international terror (Panel A) and domestic terror (Panel B) per country, respectively.

[17]Specifically, in Germany, a U.K. national affiliated with the Irish Republican Army (IRA) fired mortar grenades towards U.K. 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 4 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.

5-4 Empirical Strategy

5-4.1 Base Specification

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):

$$TERROR_{hot} = \alpha + \beta FOREIGNER_{hot} + \mathbf{X}'_{hot}\boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot} \tag{5-1}$$

where $\mathbf{X}_{hot}$ is a set of time-varying control variables, η_{ho} are dyadic host-origin fixed effects, 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.[18] 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. Migrants might choose their host country according to the risk of experiencing terror, but potentially also according to the ease of pursuing attacks there. A large number of omitted variables are arguably related to both terror and migration as well. We still report these basic results for comparison.

We proceed by including a number of interactions that test the more nuanced hypotheses introduced above:

$$\begin{aligned} TERROR_{hot} = \alpha + \beta FOREIGNER_{hot} + \theta(FOREIGNER_{hot} * INT_{ho,t-1}) \\ + \delta INT_{ho,t-1} + \mathbf{X}'_{hot}\boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot} \end{aligned} \tag{5-2}$$

where $INT_{ho,t-1}$ represents the variables that we hypothesized to change the effect of $FOREIGNERS$ on $TERROR$ in Section 5-2 above.[19] These variables are moving averages over five years, as we expect foreigners to react to a country's (recent) general trend in policies rather than 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.

[18] We test the robustness of our findings by including additional control variables that have been identified as robust correlates of terrorism below (Gassebner and Luechinger, 2011).

[19] Note that some of them vary across dyads and time, while others are constant across either host or origin countries, as we explain below. Appendix A reports the exact definitions and sources of all variables, while Appendix B shows descriptive statistics. Appendix C shows the countries included in our sample.

First, we measure conflict ($CONFLICT_{ho,t-1}$) with the fraction of years a host-origin pair is in a military conflict over the $t-5-t-1$ 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 indicators of 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).[20] In the initial year (1980, for most of the dyads in our sample), we code restrictiveness as 'zero'. 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 $SANCTIONS_{hot}$).[21] 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 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 natives 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.

[20]An obvious alternative to DEMIG is the International Migration Policy and Law Analysis (IMPALA) Database (Beine et al., 2016), which however currently covers only ten years from nine countries.

[21]$RIGHTS_{hot}$ 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.

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, undated, 2016), ranging between 1 and 6, 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, undated, 2016).

Next 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.[22] 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.

Finally, 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.

5-4.2 Identification

The main problem for estimating the causal effect of the stock of foreigners on the likelihood of transnational terrorism is endogeneity. Dreher and Fuchs (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. OLS estimates of terrorism on migration stocks are therefore likely to be biased.

To address this endogeneity, we closely follow recent advances in the migration, development, and labor literature (Feyrer, 2009; Beine et al., 2011; Artuç et al., 2015; Alesina et al., 2016). Our instrument relies on the interactions between two sets of variables. The interaction exploits variation across host-origin-dyads that results from differences in whether or not a country of origin and the host share a language or the distance between them, among others. Variation over time (and between countries) results from differences in the number of natural disasters that hit a country at any point in time. Our first-stage regression is as follows:[23]

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

[23] see full set of coefficients is provided in column 1 Table 5-D1. The coefficients depicted in the equation correspond to the second stage in column 1 of Table 5-3.

$$FOREIGNERS_{hot} = \alpha + \Big(-\frac{0.0320^{***}}{(0.0118)} COLONY_{ho} + \frac{0.0116}{(0.0116)} LANGUAGE_{ho} - \frac{0.0293^{**}}{(0.0147)} BORDER_{ho} + -\frac{0.0051}{(0.0041)} DISTANCE_{ho} - \frac{0.0045^{***}}{(0.0012)} FOREIGNERS1960_{ho}\Big) * DISASTER_{ot} - \frac{0.0040}{(0.0371)} DISASTER_{ot} + \Big(\frac{0.0072}{(0.0067)} COLONY_{ho} - \frac{0.0144^{***}}{(0.0045)} LANGUAGE_{ho} - \frac{0.0224}{(0.0174)} BORDER_{ho} - \frac{0.0066^{**}}{(0.0032)} DISTANCE_{ho} + \frac{0.0050^{***}}{(0.0006)} FOREIGNERS1960_{ho}\Big) * DISASTER_{ht} + \frac{0.0927^{***}}{(0.0297)} DISASTER_{ht} + \mathbf{X}'_{hot}\boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot}. \tag{5-3}$$

The pull and push factors between host and origin countries in our regressions include a binary indicator showing whether or not the host and origin countries share a (past or present) colonial relation, a common language (spoken by at least nine percent of the population), a common border, as well as the logged great circle distance between their capitals (in kilometers), and the log of the bilateral stock of foreigners in 1960 to capture preexisting networks. The levels of these structural variables do not vary over time and are thus captured by the host-origin fixed effects η_{ho}.

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 EM-DAT (Guha-Sapir et al., 2016). EM-DAT includes all disasters where at least ten 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 sub-categories – 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).[24] 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. However, we control for the number of disasters in the host and origin countries in the first and second stage regressions. 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 number of disasters in any particular year and (origin as well as host) country is plausibly exogenous to dyadic terror events between any pair of countries.[25] The intuition behind the

[24]EM-DAT collects data from a number of different sources, including UN agencies, non-governmental organizations, research institutes, insurance companies, and press agencies. See http://www.emdat.be/explanatory-notes (accessed December 28, 2017) for methodological details.

[25]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

interacted instruments is that of a difference-in-difference 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.[26]

A natural disaster in a country of origin makes migration to the OECD overall more attractive if this country is closer, has traditional migrant communities, or shares colonial and cultural ties. The dyadic characteristics would then be crucial in determining how many people affected by the disaster decide to migrate to a specific host country. In turn, disasters in host countries make them less attractive.[27]

The second-stage regression explaining terror then looks as follows:

$$\begin{aligned} TERROR_{hot} = \alpha + \beta \widehat{FOREIGNER}_{hot} + \rho DISASTER_{ot} + \delta DISASTER_{ht} \\ + \zeta INT_{ho,t-1} + \mathbf{X}'_{hot}\boldsymbol{\psi} + \eta_{ho} + \gamma_t + \epsilon_{hot}. \end{aligned} \tag{5-4}$$

Our identification strategy rests on two assumptions.[28] The first assumption is the plausible exogeneity of natural disasters with respect to terror in a dyad and year, conditional on the variables in the models. The random timing of disasters in any year and the inclusion of year and dyad-fixed effects support this assumption. Our second assumption is that any endogeneity of the push and pull factors due to omitted variable bias must be independent of disasters. In other words, 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. 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. For example, migrants could choose their host countries in response to natural disasters in a way that depends on omitted variables that in turn affect terror. Given that we control for dyad-specific and year fixed effects, we consider this possible, but unlikely.[29]

size. Arguably, within-decade changes in (slow-moving) climatic conditions are unlikely to affect our results.

[26]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.

[27]Note that 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.

[28]Bun and Harrison (2018) and Nizalova and Murtazashvili (2016) provide details on the identifying assumptions and formal proofs. Also see Appendix S.4 in Dreher et al. (2019).

[29]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 point in 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 5-D1 in the Appendix 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 centered around zero and significantly different

The second stage explaining the various interactions with terror is:

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

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 $INT_{ho,t-1}$.[30] 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-difference 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. [31] 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 stock of foreigners is the same for any level of this stock. We investigate this assumption in Figure 5-D2 in the Appendix.[32]

5-5 Results

5-5.1 Descriptive Evidence, Native and Foreign Born Populations

Column 1 of Table 5-2 shows the results of the baseline regression, estimated with OLS (equation 1 above). 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 one percent level of significance. Both results are in line with the previous literature.[33] Just like Gassebner and Luechinger (2011), we find no significant effect of host country GDP and population.[34]

The results also reflect the positive scale effect already visible in Figure 5-2. At the one percent level of significance, the number of terrorist attacks increases with

from the main results.

[30]This follows (, p.143 onwards).

[31]See again the references we refer to in footnote 25.

[32]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 5-3). The figure shows no obvious differences in these trends across the respective sub-samples. The exception is our indicator for conflict, were we are thus less confident that the coefficient of the interaction term is estimated consistently.

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

[34]Also see Jetter (2017).

the number of migrants living in a country. The coefficient implies that an increase in the number of migrants by one percent comes with an increase in the probability of terrorist activity of 0.001 percentage points. 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 against $FOREIGNERS$ to a different extent compared to 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 stock of people and host GDP) so that we can directly compare their magnitudes.

Table 5-2 – Terror and Migration Comparing Natives and Foreigners, 1980-2010, OLS

	(1) Terror indicator	(2) Terror indicator	(3) Terror count	(4) Severe terror indicator	(5) Severe terror count	(6) Terror fatalities count
Log GDP host	0.0032	0.0062**	0.0926**	0.0028***	0.0034**	-0.0393
	(0.0040)	(0.0029)	(0.0464)	(0.0010)	(0.0017)	(0.0623)
Log stock foreigners	0.0013***	0.0036***	0.0120***	0.0014***	0.0024***	0.0275*
	(0.0003)	(0.0008)	(0.0033)	(0.0005)	(0.0009)	(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**	-10.9431**	0.0103	0.0648	-10.0921
		(0.1863)	(5.0809)	(0.0168)	(0.0463)	(10.8919)
Log stock		0.1823	19.1166	0.0889	0.1927	142.6256
		(0.9179)	(19.5007)	(0.0983)	(0.3089)	(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
Obs	102760	123380	123380	123380	123380	123380

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 natives, also domestic attacks are 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, Y are year fixed effects.

We include dyads of the host country with itself and replace the number of foreigners with the log stock of natives 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 5-2 reports the average effect of the number of natives (on domestic terror) and foreigners (on transnational terror). The lower panel shows the differential effect of $NATIVES$ compared to the pooled estimate. According to the results, there is no statistically significant difference among the two sets of regressions. The average scale effect of the total population on the probability of terror is positive and significant at the one percent level. However, while the point coefficient estimating the difference for terror originating from the native population compared to the total population is large, this difference is not significant at conventional levels.

Column 3 of Table 5-2 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 one percent 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 one percent 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 natives 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 natives. 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.

5-5.2 Causal Evidence

Table 5-3 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 than in the OLS regression above (in column 1 of Table 5-3) and is significant at the one percent level.[35] The coefficient implies

[35]The first-stage results are shown in equation (3) above as well as in Table 5-D1 in the Appendix. Kleibergen-Paap F-statistics indicate the power of our instruments, ranging from 12.8 to 15.9. The correlation between our predicted stocks of migrants and actual migrant stocks is

that a one percent 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. In contracts if disasters affect the flow of terrorist migrants to a lower degree than other migrants, we might underestimate the total effect of migration on terror.

Table 5-3 – Terror and Migration, Interactions, 1980-2010, 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)
Log GDP host	-0.0644***	-0.0689***	-0.0683***	-0.0692***	-0.0608***	
	(0.0167)	(0.0200)	(0.0201)	(0.0201)	(0.0174)	
Log GDP origin	0.0073**	0.0066**	0.0064*	0.0069**	0.0055**	
	(0.0030)	(0.0033)	(0.0033)	(0.0034)	(0.0028)	
Log population host	0.0986***	0.1026***	0.1016***	0.0972***	0.0942***	
	(0.0263)	(0.0298)	(0.0300)	(0.0289)	(0.0265)	
Log population origin	-0.0247***	-0.0280***	-0.0282***	-0.0328***	-0.0279***	
	(0.0084)	(0.0098)	(0.0099)	(0.0108)	(0.0093)	
Natural disaster host	-0.0002	-0.0002	-0.0002	-0.0003	0.0000	
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	
Natural disaster origin	-0.0013***	-0.0011***	-0.0011***	-0.0012***	-0.0010***	
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	
Log stock foreigners	0.0443***	0.0430***	0.0439***	0.0438***	0.0368***	0.0360***
	(0.0091)	(0.0100)	(0.0102)	(0.0102)	(0.0088)	(0.0129)
Log net ODA			-0.0006***			
			(0.0002)			
UNGA Alignement				-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
Obs	91621	91621	91621	91621	91621	91621

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.

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 Figure 5-D3 and Figure 5-D4 in the Appendix). The coefficient changes most notably when we exclude Turkey as a country of origin. The point estimate is not statistically different from the main results, however.

While the literature seems to accept that our instrument is exogenous to labor 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. The further columns of Table 5-3 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 – in column 4 – for voting coincidence between host and origin in the United Nations General Assembly (Dreher and Gassebner, 2008), as well as – 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 control for all factors that do not vary between dyads over time.[37]Again, the result is similar. We are thus confident that our identifying assumptions are not threatened by omitted variables that do not vary at the dyad-year level.[38]

Table 5-4 turns to the alternative definitions of our dependent variable, in line with Table 5-2 above. Across the regressions, the scale effect of foreigners on terror remains significant at the one percent 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 natives and we have no additional instrument for the size of the domestic population that would allow this comparison.

Table 5-5 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, integration and immigration policies, the degree of terror against foreigners in

[37]We show the first stage regression of the high dimensional fixed effects specification in Table 5-D1 (column 2).

[38]Again see Figure D-1 in the Appendix 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 5-3. 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% percent larger (i.e., $0.0443/(0.0377 + 0.0443)$) than the impact of the observed variables to explain away the entire effect of migration.

Table 5-4 – Terror and Migration, Alternative Definitions, 1980-2010, 2SLS

	(1) Terror count	(2) Severe terror indicator	(3) Severe terror count	(4) Terror fatalities count
Log GDP host	-0.1488***	-0.0226**	-0.0341**	0.0197
	(0.0520)	(0.0094)	(0.0144)	(0.0439)
Log GDP origin	0.0180**	0.0028*	0.0040*	-0.0046
	(0.0085)	(0.0015)	(0.0022)	(0.0081)
Log population host	0.2079***	0.0372***	0.0547***	-0.1689
	(0.0676)	(0.0134)	(0.0210)	(0.1843)
Log population origin	-0.0569**	-0.0107**	-0.0158**	-0.0331
	(0.0231)	(0.0048)	(0.0073)	(0.0276)
Natural disaster host	-0.0009	-0.0001	-0.0002	0.0004
	(0.0007)	(0.0001)	(0.0001)	(0.0004)
Natural disaster origin	-0.0027***	-0.0005**	-0.0008***	-0.0020
	(0.0008)	(0.0002)	(0.0003)	(0.0021)
Log stock foreigners	0.1009***	0.0172***	0.0261***	0.0424
	(0.0313)	(0.0059)	(0.0091)	(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	102760	102760	102760	102760

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, Y are year fixed effects.

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).

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 is independent from military conflict between the origin and host countries.

Columns 2-5 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

Table 5-5 – Terror and Migration, Interactions, 1980-2010, 2SLS

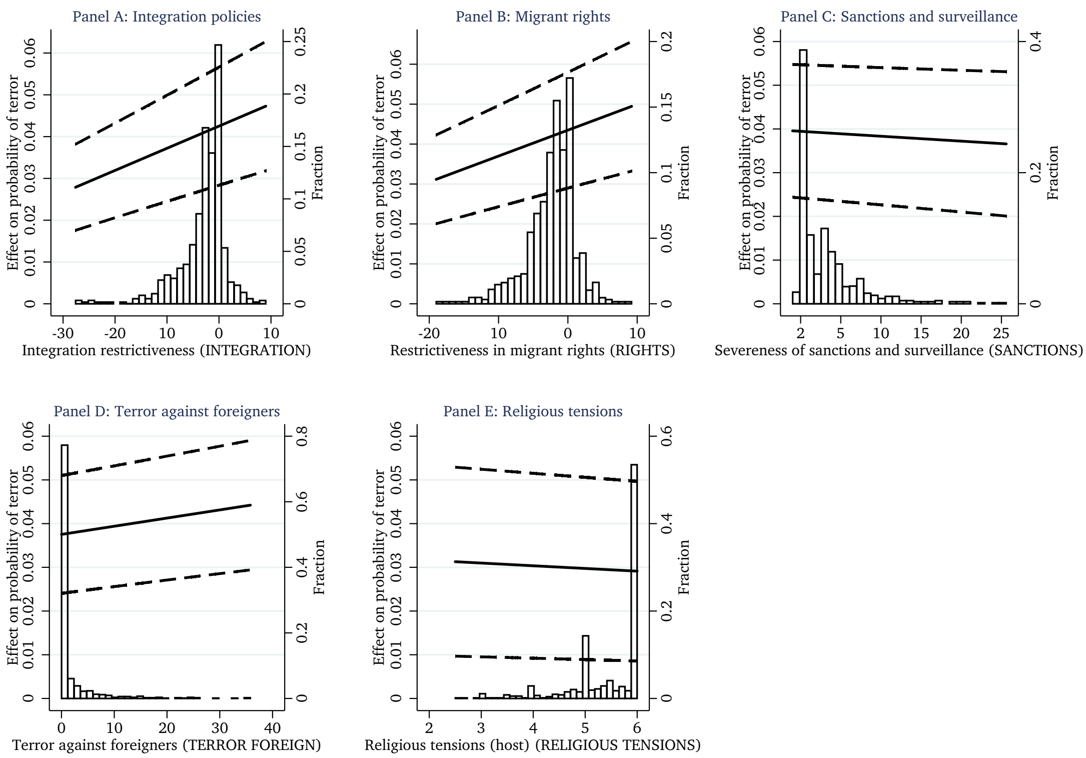
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log GDP host	-0.0643***	-0.0718***	-0.0741***	-0.0616***	-0.0621***	-0.0535***	-0.0570**
	(0.0167)	(0.0173)	(0.0178)	(0.0166)	(0.0164)	(0.0150)	(0.0264)
Log GDP origin	0.0073**	0.0072***	0.0074***	0.0064**	0.0069**	0.0060**	0.0025
	(0.0030)	(0.0028)	(0.0028)	(0.0028)	(0.0029)	(0.0026)	(0.0024)
Log population host	0.0985***	0.1293***	0.1276***	0.0976***	0.1028***	0.0936***	0.1072***
	(0.0263)	(0.0298)	(0.0299)	(0.0253)	(0.0259)	(0.0247)	(0.0404)
Log population origin	-0.0246***	-0.0323***	-0.0322***	-0.0233***	-0.0237***	-0.0215***	-0.0092
	(0.0084)	(0.0088)	(0.0090)	(0.0078)	(0.0081)	(0.0076)	(0.0075)
Natural disaster host	-0.0002	-0.0002	-0.0002	-0.0001	-0.0001	-0.0002	-0.0000
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Natural disaster origin	-0.0013***	-0.0010***	-0.0010***	-0.0012***	-0.0013***	-0.0012***	-0.0005*
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0002)
Log stock foreigners	0.0442***	0.0425***	0.0435***	0.0395***	0.0421***	0.0375***	0.0328**
	(0.0091)	(0.0086)	(0.0088)	(0.0093)	(0.0092)	(0.0082)	(0.0136)
Additional variable	Conflict	Integration	Migrant rights	Migrant sanctions	Immigration	Terror vs. foreigners	Religious tensions
Variable coefficient	-0.0049	-0.0030***	-0.0038***	0.0001	0.0001	-0.0013***	0.0020
	(0.0167)	(0.0005)	(0.0007)	(0.0005)	(0.0004)	(0.0003)	(0.0016)
Interaction coefficient	-0.0015	0.0005***	0.0006***	-0.0001*	0.0001	0.0002***	-0.0006*
	(0.0027)	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0003)
R-squared	0.00737	0.00612	0.00637	0.00712	0.00723	0.00731	0.00510
Kleibergen-Paap F-stat. IV 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	102760	102760	102760	102760	102760	102760	89020

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.

on immigration (column 5).[39]

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 one and ten percent level of significance, respectively.

Figure 5-3 – Marginal Effects Corresponding to Table 5



Notes: The figures show the marginal effects of the significant interactions from Table 5-5 with bars indicating the distribution of the underlying data (right scale).

Figure 5-3 shows the marginal effects of the significant interactions. Panel A of Figure 5-3 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 one percent 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 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 one percent 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 (an 16.7% increase).

Panel C turns to the effect of migrant surveillance and sanctions. While the effect

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

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 one percent increase of the stock of foreigners leads to an increase of 0.0391 in the likelihood of terror compared to 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 to 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 one percent increase in the stock of foreigners increases the probability of a terrorist attack committed by foreigners by 0.0379 percentage points on average. The corresponding increase is 0.0442 percentage points at the maximum value of terror against foreigners (15.5% higher compared to the mean). To the contrary, while the effect of (the absence of) religious tensions is statistically significant (Panel E), the difference of a one percent 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).

5-5.3 Composition of Migrant Populations

We proceed with testing whether the composition of migrants matters. Column 1 of Table 6 separately investigates male and female migrants. Column 2 distinguishes migrants with low, medium, and high skills.[41] As an additional set of instruments for the stock of female and male 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.[42] For the stock of low, medium and high skilled migrants, we add interactions of our 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.[43]

The results of Column 1 show that the risk of terror increases with the number of male immigrants, at the one percent level of significance, but not with the number of female immigrants. The coefficients of the two groups are, however, not statistically

[40]As an illustration, consider France. According to the DEMIG (2015) data, France introduced 18 additional restrictions on immigration over the 1991-1994 period. This 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]he IAB database defines the skill levels as follows: Low skilled individuals have received lower secondary, primary or no schooling. Medium skilled migrants have obtained a high school diploma or equivalent certificate. High skilled immigrants have tertiary education (Brücker et al., 2013, p.4)

[42]We predict the number of male and female migrants with our instruments and then use the predicted migrant stocks as instruments for the second stage.

[43]Figure 5-D5 in the Appendix shows that our predictions for the different groups match the actual values well.

Table 5-6 – Gender and Skill Level, 1980-2010, 2SLS

	(1)	(2)
Log GDP host	-0.0234***	-0.0214**
	(0.0087)	(0.0106)
Log GDP origin	0.0025	-0.0038
	(0.0016)	(0.0025)
Log population host	0.0611***	0.0812***
	(0.0201)	(0.0308)
Log population origin	-0.0072	-0.0140**
	(0.0046)	(0.0064)
Natural disaster host	-0.0003	0.0003
	(0.0002)	(0.0003)
Natural disaster origin	-0.0011***	-0.0006***
	(0.0003)	(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	102760	102760

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.

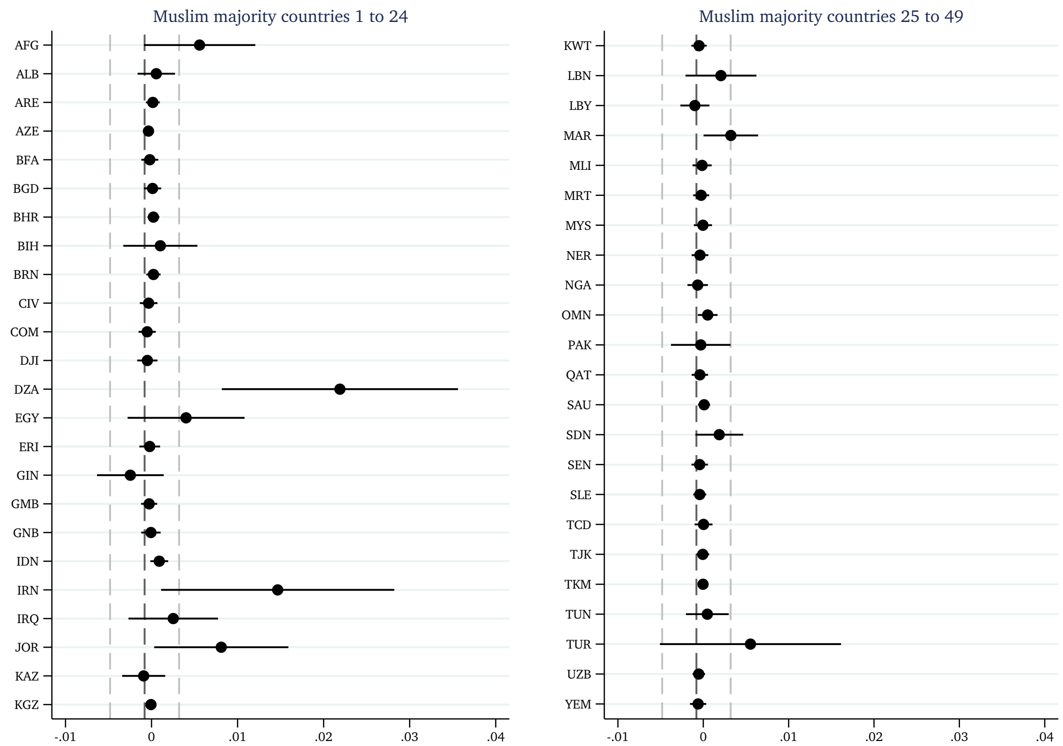
different from each other (p-value: 0.54). Column 2 shows that the risk of terror increases with low skilled immigrants, but decreases with highly skilled immigrants, the difference between the two being significant at the one percent level. While the previous literature has often argued that terrorists are well educated compared to 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 labor quotas generally reduce terrorist attacks in the host country.

5-5.4 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 to all other (i.e., non-Muslim) countries. As before, we instrument the stock of foreigners with the interaction 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 5-4 – Marginal Effects of Majority Muslim Countries



Notes: The vertical solid line is the average partial effect of the estimate for migration from the average non-Muslim-majority country; the dotted lines show the 95% confidence interval. The figure shows the additional effect for each Muslim-majority country, in tandem with the 95% confidence interval.

Figure 5-4 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 percent 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').[44] The two exceptions are Algeria and Iran, at the ten percent level of significance. Compared to the (insignificant) average effect of foreigners from non-Muslim countries, the marginal effect for Algeria implies that a one percent 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 from Algerian fundamentalists who participated 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.[45] 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 such diffusion (Bove and Böhmelt, 2016). As above, we therefore test whether migrants from 'terror-rich' countries show different effects of terror compared to migrants from the average "non-terror-rich" country.

Our first proxy for the existence of terror networks is a binary indicator variable $TERRORRICH_o$ for each country that is located within the top quintile of the overall terrorist incident distribution of the GTD dataset.[46] Again, we interact these dummies with our migration variable. Figure 5-5 shows that five countries have average partial effects that are higher than the reference group, at least at the ten percent level of significance. Compared to 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.[47] 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. Immigration from terror-rich countries is thus not responsible for the overall effect of migration on terror.

Our second proxy for the existence of terrorist networks identifies the ten most active terrorist groups, in terms of incidents perpetrated globally, and that operate in at least five countries (using GTD data).[48] We then identify the three countries

[44] Jetter and Stadelmann (2017) show that the probability that Muslims become terrorists is smaller compared to non-Muslims once population size is accounted for.

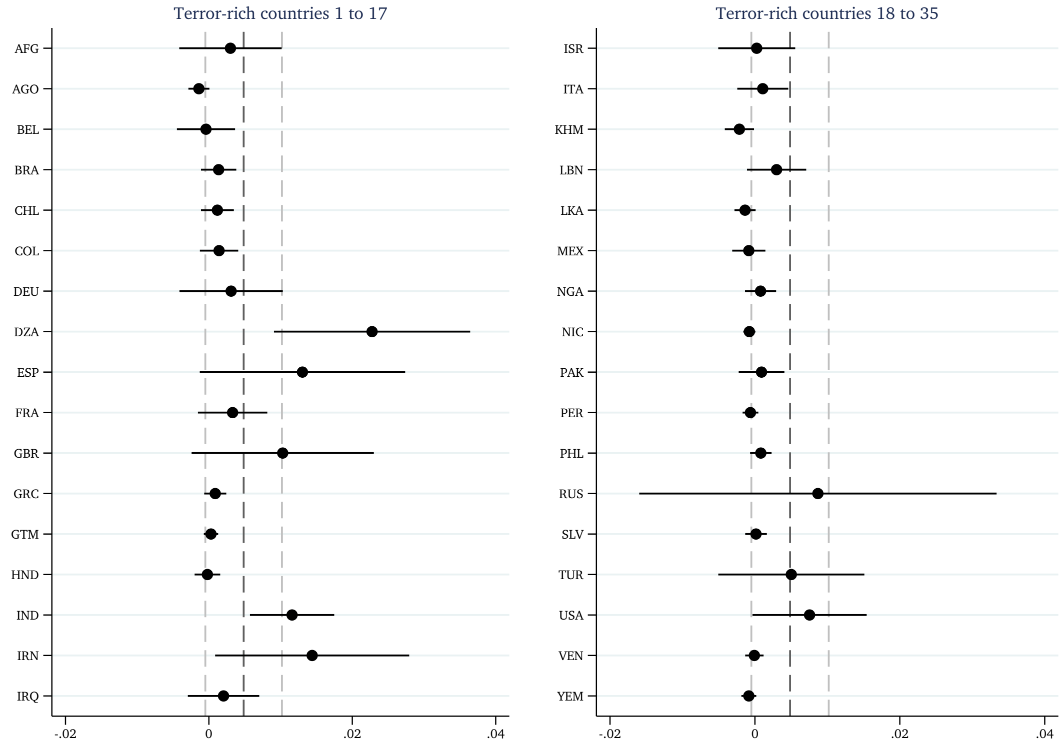
[45] There is no dominant terror organization behind these attacks in Germany, while one third of the French attacks were conducted by Islamic Jihad organizations. In our sample, Algerian terrorists conducted 34 terror attacks in total, while citizens of Iran conducted a total of 80 attacks.

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

[47] Some background for Algeria and Iran was given above. In most of the 15 attacks in India, 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 U.K. citizens by Muslim extremists. Spanish nationals were involved in 17 attacks in France, 10 attacks in Italy and a total of 43 attacks in our sample (34 were the responsibility of ETA). 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.

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

Figure 5-5 – Marginal Effects of '*Terror Rich*' Countries



Notes: The vertical solid line is the average partial effect of the estimate for the average non-'terror rich' country; the dotted lines show the 95% confidence interval. The figure shows the additional effect for each 'terror rich' country, in tandem with the 95% confidence interval.

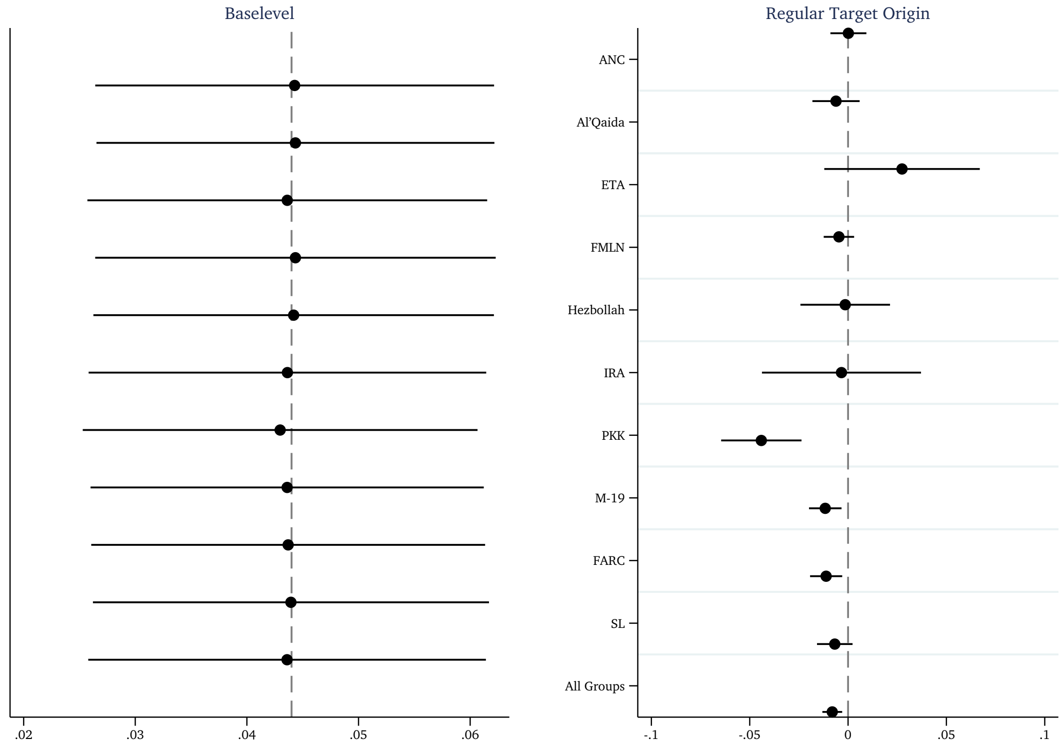
in which each group commits most of their attacks over our sample countries and period. We list these groups and countries in Table 5-D2 in the Appendix, in descending order of group activity.

Figure 5-6 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 (left) and the interaction coefficient for the countries with the most active terrorist networks (right) for each of the ten most active transnational terror groups.[49] 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 – which include Germany, Turkey and Iraq – is smaller than the effect of migrants from all other countries. Again, there is thus no evidence that migration from countries with large terror networks accounts for the overall effect of migration on terror.[50]

[49] In the pooled specification all countries of Table-A1 are coded as network-countries.

[50] We also tested for 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 5-D6 in the Appendix). 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 to 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

Figure 5-6 – Marginal Effect of Migrants from Countries with Terrorist Networks



Notes: 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 (left) and the interaction coefficient for the countries with the most active terrorist networks (right) for each of the ten most active transnational terror groups (with the 95% confidence interval).

In summary, 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).

5-6 Tests for Robustness

In summary, we find a positive scale effect of larger foreign populations. 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 include all explanatory variables – rather than just the interaction variables – as (lagged) five-year moving averages to allow for longer lags between changes in

(Table 5-D3 in the Appendix).

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 five percent 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 precipitation from their decade averages.[51] A drawback of this approach is a substantial loss in the number observations.

Sixth, we estimate the first and second stage including fixed effects for origin-years and host-years instead of just years (in concert with the dummies for each dyad). We consequently rely exclusively on within-dyad variation to identify our coefficients. 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 5-D4 (for the main regressions) and Table 5-D5 (for the separate regressions according to gender and skills) in the Appendix. Most of our results turn out to be robust to all modifications. The effect of a one percent 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 5-3 above, that we reproduce in Table 5-D4 for comparison ('no moving average').

With respect to the interaction terms, terror from natives 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 5-D5 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 5-D6 presents the results of a nested model in which we allow for different average partial effects between the 1980s, 1990s and the 2000s. We find that the effect of the stock of foreigners is statistically different in the 1990s and 2000s compared to 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

[51] We thank Sven Kunze for sharing his temperature and precipitation data (Kunze, 2017).

millennium compared to 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 5-D7 show that the effect remains significant when 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 foreign born 20 years prior within a dyad).

5-7 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 been caused by expectations that a larger number of foreigners living in a country increases the risk of terrorist attacks in the host country. This chapter 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 to the domestic population. When we calculate the effect of a larger population of natives on the number of times natives attack foreigners or other natives, we find this effect to be of comparable magnitude.

Second, we test whether migrants from countries with large terrorist networks or from Muslim-majority countries affect the risk of terror differently, and whether and to what extent host country immigration and integration policies mediate the risk arising from foreigners. We find scarce evidence that terror is systematically imported from countries with large Muslim populations, or countries rich in terror.

We also 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 from 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 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 favor 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 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.

Appendix A: Sources and Definitions

Table 5-A1 – Sources and Definitions

Variable	**Source**	**Definition**
Transnational terror attacks	Mickolus et al. (2017)	Sum of yearly incidents of terror attacks from nationals of an origin country within the host country.
Domestic terror attacks	Enders et al. (2011); Gaibulloev et al. (2012)	Terror from nationals against nationals within the country.
Terror against foreigners (by natives)	Mickolus et al. (2017)	Terror from nationals against foreigners within the host country.
Transnational terror dummy	Mickolus et al. (2017)	Dummy that is one if at least one terror attack was committed by a national of an origin country within the host country during a year.
Transnational terror attacks (severe)	Mickolus et al. (2017)	Sum of yearly incidents of terror attacks from nationals of an origin country within the host country, in which at least one victim was wounded or killed.
Transnational terror dummy (severe)	Mickolus et al. (2017)	Dummy that is one if at least one severe terror attack was perpetrated by a national of an origin country within the host country during a year.
Log of foreign-born residents	IAB Database, Brücker et al. (2013)	Log of total bilateral foreign-born residents from an origin country.
Log of foreign-born male residents	IAB Database, Brücker et al. (2013)	Log of total bilateral foreign-born male residents from an origin country.
Log of foreign-born female residents	IAB Database, Brücker et al. (2013)	Log of total bilateral foreign-born female residents from an origin country.
Log of foreign-born residents low skilled	IAB Database, Brücker et al. (2013)	Log of total bilateral foreign-born low skilled residents from an origin country.
Log of foreign-born residents medium skilled	IAB Database, Brücker et al. (2013)	Log of total bilateral foreign-born medium skilled residents from an origin country.
Log of foreign-born residents high skilled	IAB Database, Brücker et al. (2013)	Log of total bilateral foreign-born high skilled residents from an origin country.
Log of natives	World Bank (2016), IAB Database, Brücker et al. (2013)	Log of total population minus the total foreign-born resident stock.
Log of Foreigners 1960	Özden et al. (2011)	Log of foreign citizens from the 1960s.
Common border	Head et al. (2010)	Dummy for shared border.
Common language	Head et al. (2010)	Dummy that is one if at least 9% of the host population speak the language of the origin country.
Current/former colony	Head et al. (2010)	Dummy that is one if the origin country ever was a colony of the host country.
Log of distance	Head et al. (2010)	Log of Distance in km between host and origin country.

Continued on next page

Table 5-A1 – continued from previous page

Variable	Source	Definition
Natural disaster (host)	Guha-Sapir et al. (2016)	Sum of natural disasters in host country. Includes all disasters where at least ten 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 sub-categories – geophysical, meteorological, hydrological, climatological, biological, and extraterrestrial.
Natural disaster (origin)	Guha-Sapir et al. (2016)	Sum of natural disasters in origin country. Includes all disasters where at least ten 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 sub-categories – geophysical, meteorological, hydrological, climatological, biological, and extraterrestrial.
Temperature deviation (origin)	Kunze (2017)	Temperature deviations from the decade mean.
Precipitation deviation (origin)	Kunze (2017)	Precipitation deviations from the decade mean.
Log GDP (host)	World Bank (2016)	Log of GDP in constant 2010 US$ of the host country.
Log GDP (origin)	World Bank (2016)	Log of GDP in constant 2010 US$ of the origin country.
Log population (host)	World Bank (2016)	Log of total population in the host country.
Log population (origin)	World Bank (2016)	Log of total population in the origin country.
Bilateral conflict dummy	UCDP Armed Conflict Dataset (V.4-2015), Gleditsch et al. (2002); Pettersson and Wallensteen (2015)	Dummy that is one if host and origin country are engaged in military conflict, both as primary or supporting actors.
Religious tensions (host)	PRS Group, undated (2016)	Religious tension indicator (ranking 1 to 6), measures the degree to which religious issues are politicized in a country. Higher values mean fewer tensions.
GDP per capita growth (host)	World Bank (2016)	Log of GDP per capita growth in host country.
Integration index	DEMIG (2015)	Index of integration restrictiveness. Rolling stock of the net count of more restrictive policy measures (DEMIG policies that are labeled integration under the variable "$pol_{a}rea$").

Continued on next page

Table 5-A1 – continued from previous page

Variable	Source	Definition
Migrant rights index	DEMIG (2015)	Index of migrant rights restrictiveness. Rolling stock of the net count of more restrictive policy measures (DEMIG policies that are related to access of social programs, labor access and residence under the variable "pol_tool").
Migrants surveillance & sanction index	DEMIG (2015)	Index of surveillance & sanction restrictiveness. Rolling stock of the net count of more restrictive policy measures (DEMIG policies that are related to sanctions, surveillance measures, like regular reporting, and liabilities under the variable "pol_tool").
Immigration index	DEMIG (2015)	Index of immigration restrictiveness. Rolling stock of the net count of more restrictive policy measures (DEMIG policies that are labeled integration under the variable "pol_area").
Muslim country dummy	Central Intelligence Agency (2018)	Dummy that is one if Islam is the majority religion of a country.
Terror rich country dummy	Enders et al. (2011); Gaibulloev et al. (2012)	Dummy that is one if a country is in the top quintile of the domestic terror distribution over the whole sample.
Log of net ODA in constant 2015 US$	OECD (2017)	Log of net ODA commitments in constant 2015 US$ from the host to the origin country.
UNGA voting alignment	Voeten et al. (2017)	UNGA voting alignment, common votes share including abstentions
Log of Imports host from origin	Fouquin and Hugot (2016)	Log of imports the host country imports from the origin country.
Log of Imports origin from host	Fouquin and Hugot (2016)	Log of imports the origin country imports from the host country.

Appendix B: Descriptive Statistics

Table 5-B1 – Descriptive Statistics

Variable	Mean	SD	Min	Max	N
Dependent Variables					
Tranational Terror Attacks	0.01	0.13	0.00	17.00	102760
Domestic Terror Attacks	2.52	9.67	0.00	135.00	102760
Terror from nationals against foreigners	1.86	4.85	0.00	35.80	102760
Transnational Terror Dummy	0.00	0.06	0.00	1.00	102760
Transnational Terror Attacks (Severe)	0.00	0.05	0.00	7.00	102760
Transnational Terror Dummy (Severe)	0.00	0.03	0.00	1.00	102760
Independent Variables					
Log of Foreign Born Residents	5.05	3.51	0.00	16.04	102760
Log of Foreign Born (male)	4.47	3.33	0.00	15.43	102760
Log of Foreign Born (female)	4.34	3.37	0.00	15.25	102760
Log of Foreign Born (low skilled)	4.11	3.21	0.00	15.48	102760
Log of Foreign Born (medium skilled)	3.95	3.15	0.00	14.78	102760
Log of Foreign Born (high skilled)	4.11	3.23	0.00	14.09	102760
Instrumental Variables					
Common Border	0.02	0.13	0.00	1.00	102760
Common Languge	0.14	0.35	0.00	1.00	102760
Current/former Colony	0.04	0.21	0.00	1.00	102760
Log of Distance	8.68	0.82	4.09	9.88	102760
Natural disaster (Host)	2.36	4.52	0.00	34.00	102760
Natural disaster (Origin)	1.69	3.19	0.00	37.00	102760
Temperature Deviation (Origin)	0.42	0.50	0.00	9.35	86571
Precipitation Deviation (Origin)	10.09	12.50	0.00	120.00	91060
Log Bilateral migrant stock 1960	3.46	3.31	0.00	14.62	102760
Control Variables					
Log GDP (Host)	26.81	1.36	23.43	30.34	102760
Log GDP (Origin)	23.63	2.42	18.10	30.34	102760
Log Population (Host)	16.36	1.38	12.81	19.55	102760
Log Population (Origin)	15.43	2.07	9.65	21.01	102760
Bilateral Conflict Dummy	0.00	0.03	0.00	1.00	102760
Religious tensions (Host)	5.61	0.64	2.67	6.00	89020
GDP per capita growth (Host)	2.05	1.58	-2.29	8.97	102628
Integration Index	-3.11	4.71	-27.60	9.00	102760
Migrant rights Index	-2.52	3.66	-19.00	9.20	102760
Migrants survaliance & sanction Index	3.27	4.21	-1.00	25.60	102760
Immigration Index	-1.69	3.76	-18.80	8.00	102760
Muslim Country Dummy	0.25	0.43	0.00	1.00	102760
Terrorrich Country Dummy	0.19	0.39	0.00	1.00	102760
Ethnic tensions (Host)	4.91	0.90	2.00	6.00	89020
Log of net ODA in constant 2015 US$	-7.22	7.42	-13.82	9.50	101132
UNGA alignment	0.74	0.15	0.00	1.00	93,190
Log of imports host from origin	22.24	7.19	0.00	27.87	102760
Log of imports origin from host	2.37	6.37	0.00	26.55	102760

Appendix C: List of Countries

Table 5-C1 – Host Countries (and First Year of Inclusion):

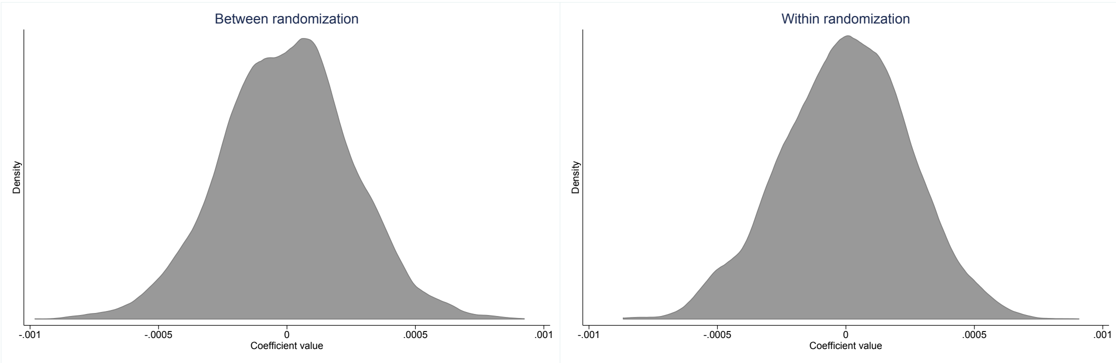
Australia 1980, Austria 1980, Canada 1980, Chile 1980, Denmark 1980, Finland 1980, France 1980, Germany 1980, Greece 1980, Ireland 2010, Luxembourg 1980, Netherlands 1980, New Zealand 1980, Norway 1980, Portugal 1980, Spain 1980, Sweden 1980, Switzerland 1980, United Kingdom 1980, United States 1980.

Table 5-C2 – Origin Countries:

Afghanistan, Albania, Algeria, Andorra, Angola, Antigua and Barbuda, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahamas, Bahrain, Bangladesh, Barbados, Belarus, Belgium, Belize, Benin, Bhutan, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Brunei Darussalam, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Congo-Brazzaville, Congo-Kinshasa, Costa Rica, Croatia, Cuba, Cyprus, Czech Republic, Denmark, Djibouti, Dominica, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Georgia, Germany, Ghana, Greece, Grenada, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hungary, Iceland, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Ivory Coast, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kiribati, Korea South, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Luxembourg, Macedonia, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Marshall Islands, Mauritania, Mauritius, Mexico, Micronesia, Moldova, Mongolia, Morocco, Mozambique, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Oman, Pakistan, Palau, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Samoa, Sao Tome and Principe, Saudi Arabia, Senegal, Seychelles, Sierra Leone, Singapore, Slovakia, Slovenia, Solomon Islands, South Africa, Spain, Sri Lanka, Sudan, Suriname, Swaziland, Sweden, Switzerland, Tajikistan, Tanzania, Thailand, Timor-Leste, Togo, Tonga, Trinidad and Tobago, Tunisia, Turkey, Turkmenistan, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States of America, Uruguay, Uzbekistan, Vanuatu, Venezuela, Vietnam, Yemen, Zambia, Zimbabwe

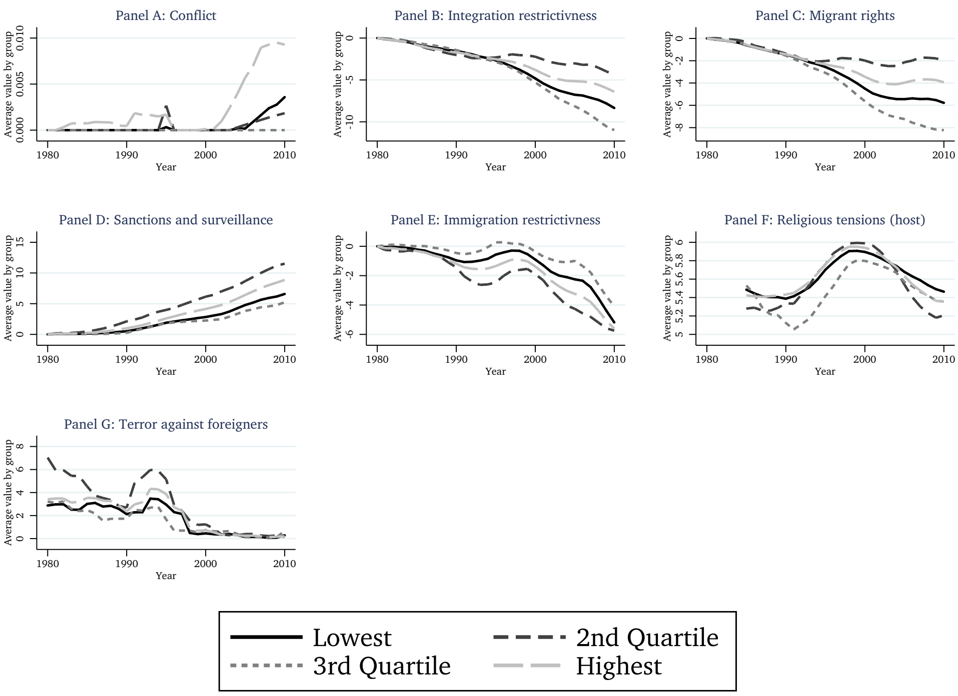
Appendix D: Figures and Tables

Figure 5-D1 – Randomization Test



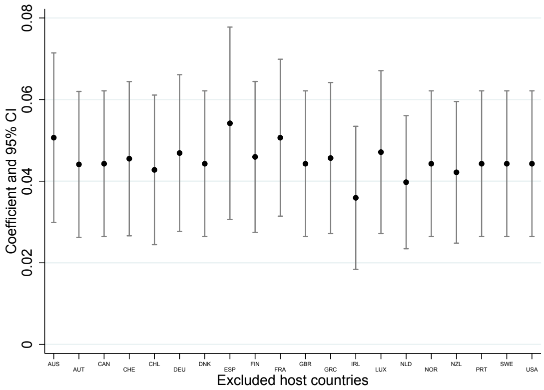
Notes: Coefficient distributions are obtained from 5,000 randomizations and depict the base level effect (corresponding to column 1 of Table 5-3). Note that the point coefficient estimated from the actual data is 0.0443 and is thus not shown in the graphs.

Figure 5-D2 – Parallel Trends of Interaction Variables



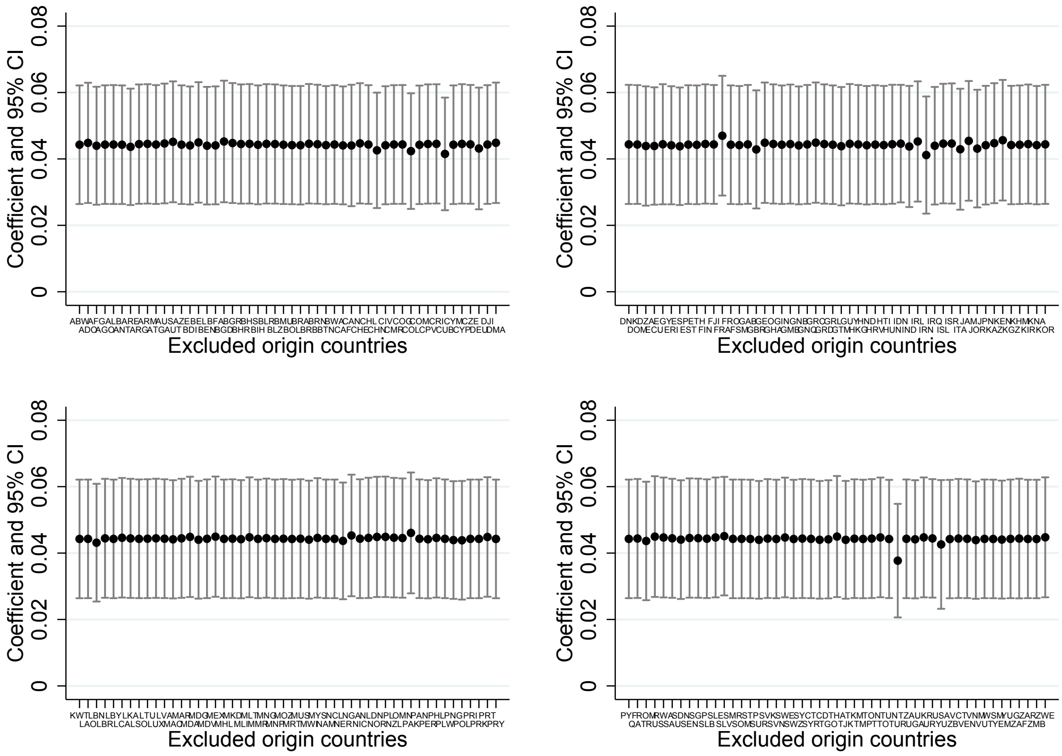
Notes: Panels A to G show the trends in the interaction variables of Table 5-5 over the different quartiles of the predicted migrant stocks (based on column 1 of Table 5-3.

Figure 5-D3 – Leave-One-Out Test (Host Countries)



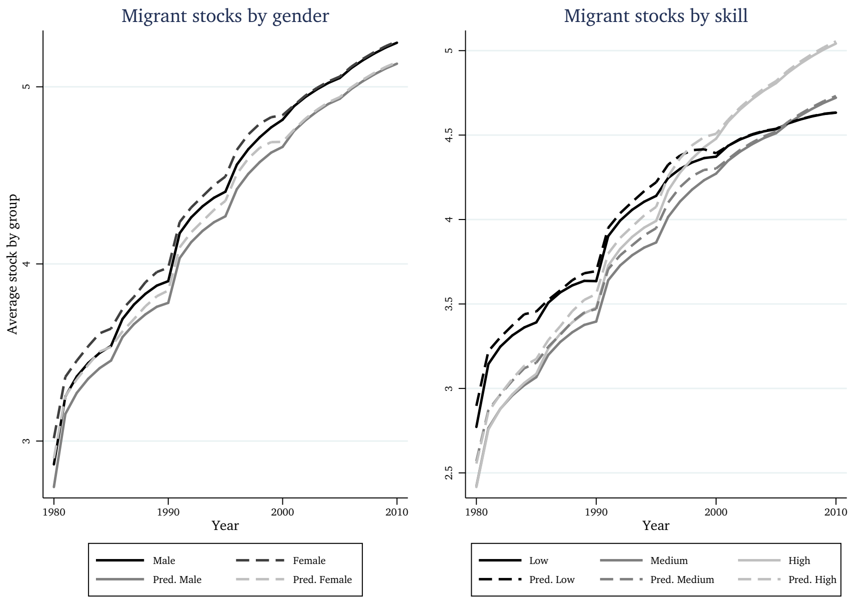
Notes: Depicts the point coefficients of the stock of foreigners and the 95% confidence interval based on column 1 of Table 5-3 for regressions excluding the respective host country.

Figure 5-D4 – Leave-One-Out Test (Origin Countries)



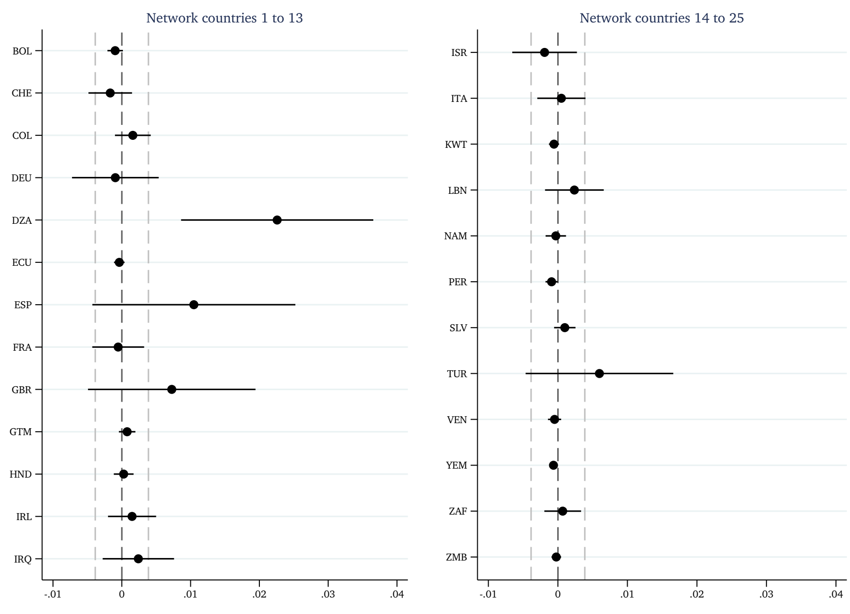
Notes: Depicts the point coefficients of the stock of foreigners and the 95% confidence interval based on column 1 of Table 5-3 for regressions excluding the respective origin country.

Figure 5-D5 – Parallel Trends (by Gender and Skill Level)



Notes: The left figure shows the average of actual and predicted migrant stocks by gender within our 20 host countries over time. The right figure shows average stocks by skill level.

Figure 5-D6 – Marginal Effects of Individual $Network$ Compared to $Non-Network$ Countries



Notes: The vertical solid line is the average partial effect of the reference group (all non-network countries), the dotted lines the 95% confidence interval. The figure shows the additional effect for each country with large terrorist networks, in tandem with the 95% confidence interval.

Table 5-D1 – First Stage Results (Gravity Specification)

	First Stages		
	(1) Log migrants	(2) Log migrants	(3) Log migrants
Log GDP host	1.5391***		
	(0.1653)		
Log GDP origin	-0.2064***		
	(0.0516)		
Log population host	-1.8788***		
	(0.3789)		
Log population origin	0.7011***		
	(0.1359)		
Natural disasters host	0.0927***		
	(0.0297)		
Natural disasters origin	-0.0040		
	(0.0371)		
Interactions with Natural Disasters in Host countries			
Colony host	0.0072	-0.0285***	-0.0211*
	(0.0067)	(0.0108)	(0.0119)
Common border	-0.0224	-0.0324	-0.0279
	(0.0174)	(0.0280)	(0.0258)
Common language	-0.0144***	0.0159***	0.0219***
	(0.0045)	(0.0044)	(0.0049)
Log distance	-0.0066**	0.0114***	0.0148***
	(0.0032)	(0.0041)	(0.0040)
Migrant stock 1960	-0.0050***	-0.0010	-0.0024***
	(0.0006)	(0.0006)	(0.0006)
Interactions with Natural Disasters in Origin countries			
Colony host	-0.0320***	-0.0406***	-0.0447***
	(0.0118)	(0.0114)	(0.0122)
Common border	-0.0293**	0.0041	-0.0047
	(0.0147)	(0.0158)	(0.0173)
Common language	0.0116	0.0278**	0.0319***
	(0.0116)	(0.0114)	(0.0109)
Log distance	0.0051	-0.0037	-0.0101*
	(0.0041)	(0.0041)	(0.0055)
Migrant stock 1960	-0.0045***	-0.0043***	-0.0053***
	(0.0012)	(0.0012)	(0.0013)
R-squared	0.4240	0.9679	0.9604
Fixed effects	HO,Y	HO,HY,OY	HO,HY,OY
Observations	102,760	91,621	115,320

Notes: Column 1 shows the first stage corresponding to column 1 of Table 3 (including host-origin and year fixed effects). Column 2 includes fixed effects for origin-year, host-year and origin-host (Column 1, row 6, in Table 5-D4). Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host-origin fixed effects, Y are year fixed effects, HY are host-year fixed effects, and OY are origin-year fixed effects.

Table 5-D2 – Network Countries by Transnational Terror Group

ANC	Al-Qaida	ETA	FMLN	Hezbollah	IRA	PKK	M-19	FARC	SL
South Africa	Iraq	Spain	El Salvador	Lebanon	United Kingdom	Turkey	Colombia	Colombia	Peru
Zambia	Algeria	France	Guatemala	Israel	Ireland	Germany	Venezuela	Venezuela	Bolivia
Botswana	Yemen	Italy	Honduras	Kuwait	France	Iraq	Ecuador	Peru	Colombia

Notes: The table lists the three countries in which terror groups are most active, in descending order.

Table 5-D3 – Network Countries by Transnational Terror Group

Group:	ANC	Al-Qaida	ETA	FMLN	Hezbollah	IRA	PKK	M-19	FARC	SL	All Groups
p-value:	0.2634	0.4459	0.9682	0.2426	0.8345	0.7056	0.9444	0.5109	0.8953	0.9894	0.5304

Notes: The p-values refer to a two sided t-test testing whether the coefficients of $\beta_4 - \beta_3$ are identical, based on the following specification: $TERROR_{hot} = \alpha + \beta(FOREIGNERS_{hot}) + \beta_2(FOREIGNERS_{hot} * NETWORK_o) + \beta_3(FOREIGNERS_{hot} * GROUPACTIVITY_t) + \beta_4(FOREIGNERS_{hot} * NETWORK_o * GROUPACTIVITY_t) + \xi(NETWORK_o * GROUPACTIVITY_t) + \rho DISASTER_{ot} + \delta DISASTER_{ht} + \mathbf{X}'_{\mathbf{hot}}\bar{} + \eta_{ho} + \gamma_t + \epsilon_{hot}$

Table 5-D4 – Tests for Robustness

	Interaction of foreigners with:					
	None	Integration	Migrant rights	Migrant sanctions	Terror vs. foreigners	Religious tensions
All moving averages	0.0426***	0.0005***	0.0006***	0.0000	0.0001***	-0.0006*
(five years)	(0.0090)	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0003)
No moving averages	0.0443***	0.0005***	0.0005***	-0.0001**	0.0002*	-0.0013**
	(0.0091)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0006)
Period averages	0.0415***	0.0006***	0.0007***	-0.0001**	0.0002***	-0.0007**
(five years)	(0.0078)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
Exclude outliers	0.0394***	0.0005***	0.0006***	-0.0000	0.0001***	-0.0006*
	(0.0085)	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0004)
Additional instruments	0.0397***	0.0005***	0.0005***	-0.0001	0.0002***	-0.0004
	(0.0087)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)
High dimensional FE	0.0227***	0.0001***	0.0002**	-0.0001	0.0002***	-0.0007
	(0.0084)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0005)
Severe terror incidents	0.0172***	0.0002***	0.0003***	-0.0000	0.0001***	-0.0002
	(0.0059)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0002)

Notes: Regressions are variants of those shown in Table 5-5. $None$ shows the coefficient of $logmigrants$, without interaction. The remaining columns show the coefficient of the interaction. $Allmovingaverages$ includes all explanatory variables as five-year moving averages (lagged by one year). $Nomovingaverages$ uses yearly values for the interaction variables, rather than five-year moving averages. $Periodaverages$ uses averages over five years (but no moving average). Exclude outliers excludes the five percent largest changes in migration in our sample. $Additionalinstruments$ adds deviations in temperature and precipitation as interaction variables to our set of instruments. $HighdimensionalFE$ includes fixed effects for origin-year, host-year and origin-host. $Severeterrorincidents$ involve at least one victim wounded or killed.

Table 5-D5 – Gender and Skills, Tests for Robustness

	Gender Specification			Skill Specification			
	Male	Female	p-value Male/Female	Low	Medium	High	p-value Low/High
All moving averages	0.0071*	0.0137***		0.0480***	0.0234**	-0.0724***	
(five years)	(0.0037)	(0.0052)	0.3989	(0.0125)	(0.0117)	(0.0216)	0.0002
Period averages	0.0109**	0.0128*		0.0567***	0.0200	-0.0695***	
(five years)	(0.0048)	(0.0066)	0.8537	(0.0145)	(0.0123)	(0.0197)	0.0001
Exclude outliers	0.0116**	0.0092		0.0296***	0.0144	-0.0344*	
	(0.0045)	(0.0060)	0.7924	(0.0095)	(0.0112)	(0.0188)	0.0144
Additional instruments	0.0180***	0.0047		0.0497***	0.0102	-0.0451**	
	(0.0055)	(0.0060)	0.1838	(0.0135)	(0.0135)	(0.0213)	0.0019
Severe terror incidents	0.0056*	0.0057		0.0180**	0.0083	-0.0220	
	(0.0032)	(0.0038)	0.9873	(0.0085)	(0.0080)	(0.0146)	0.0624

Notes: The p-values correspond to t-tests testing the equality in coefficients for male and female migrants and low and medium skilled migrants. We do not show results for $No moving averages$ and $High dimensional$ FE. Given that we do not include interactions, there are no moving averages in any of the regressions. When we include the additional fixed effects, the first-stage F-statistic is insufficiently low, so we do not report these (insignificant) results in the table. $Severe terror incidents$ involve at least one victim wounded or killed.

Table 5-D6 – Different Time Periods, 2SLS

	(1) Terror indicator	(2) Terror count	(3) Severe terror indicator	(4) Severe terror	(5) Terror fatalities count
Log stock foreigners	0.0282***	0.0686***	0.0105**	0.0161**	0.0074
	(0.0072)	(0.0246)	(0.0049)	(0.0077)	(0.0113)
Period Interactions					
Log stock foreigners in 1990s	-0.0024***	-0.0050***	-0.0010***	-0.0016***	-0.0085
	(0.0004)	(0.0014)	(0.0003)	(0.0005)	(0.0072)
Log stock foreigners in 2000s	-0.0034***	-0.0068***	-0.0014***	-0.0021***	-0.0058
	(0.0005)	(0.0016)	(0.0003)	(0.0005)	(0.0081)
R-squared	0.00580	0.00364	0.00185	0.00145	0.000008
Kleibergen-Paap F-stat. IV	19.59	19.59	19.59	19.59	19.59
Fixed effects	HO,Y	HO,Y	HO,Y	HO,Y	HO,Y
Observations	102760	102760	102760	102760	102760

Notes: The dependent variable in column (1) is binary and indicates that at least one transnational attack occurs in a year. Column (2) counts the number of transnational attacks per year. In column (3) 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 (4) uses the number of those attacks per year. Column (5) counts the number of fatalities. Control variables (GDP and population of host origin, and natural disasters in host and origin) are included. Note that the base level of the period dummies is absorbed by the time dummies. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. HO are host-origin fixed effects, Y are year fixed effects.

Table 5-D7 – Terror and Lagged Migration Stocks, 2SLS

	(1)	(2)	(3)	(4)
Log GDP host	-0.0199**	0.0180**	0.0014	-0.0010
	(0.0083)	(0.0077)	(0.0094)	(0.0080)
Log GDP origin	-0.0017	-0.0041***	0.0003	0.0003
	(0.0017)	(0.0016)	(0.0012)	(0.0026)
Log population host	0.0412***	-0.0087	0.0115	0.0210
	(0.0149)	(0.0132)	(0.0077)	(0.0259)
Log population origin	-0.0113*	-0.0049	0.0022	0.0033
	(0.0060)	(0.0041)	(0.0038)	(0.0042)
	5 year lag	10 year lag	15 year lag	20 year lag
Natural disasters host	0.0000	-0.0002	-0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Natural disasters origin	-0.0008***	-0.0006***	0.0000	-0.0001
	(0.0003)	(0.0002)	(0.0001)	(0.0001)
Log stock foreigners	0.0259***	0.0139***	0.0008	-0.0024
	(0.0069)	(0.0046)	(0.0045)	(0.0095)
R-squared	0.0056	0.0040	0.0019	0.0005
Kleibergen-Paap F-stat. IV	19.17	23.27	16.20	6.406
Fixed effects	HO,Y	HO,Y	HO,Y	HO,Y
Observations	89020	74200	57560	39980

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

Chapter 6

Concluding Remarks

This thesis analyzes political conflicts related to development cooperation and tests some of the assumptions underlying development cooperation as well migration as a potential channel through which organized violence could spill over from developing to developed countries. The different chapters highlight that volatility of development aid is related to political changes within donor and recipient countries, and that development aid itself can influence conflict dynamics within recipient countries. Furthermore, we show that decentralization reforms proposed by international organizations are context dependent. Finally, we provide evidence that violence can spill over from developing countries to developed ones via migration, but we cannot reject that the effect of migration on terrorism within developed countries is the same as from increases in the native population of those countries.

The results of Chapter 2 show that leader changes within donor and recipient countries induce sizable changes in the allocation of development aid, following usually inconsequential changes in voting behavior within the United Nations General Assembly. Recipient and donor leader changes have markedly different effects. While donor leader changes provide a window of opportunity for recipients to gain additional funds if they politically converge toward their donor, recipient leader changes present a window of dis-opportunity, where recipients can only forego cuts in aid they receive if they converge politically. Furthermore, we provide evidence that leader changes in both recipient and donor countries are natural breaking points for bilateral relations. The findings have both academic and political relevance. For scholars studying bilateral relations between administrations of different countries, our findings highlight that political change is not only important in developing countries, but also within highly institutionalized democracies, where checks and balances should provide foreign policy stability. In short, leader changes matter everywhere and influence the public finances of developing countries. Policywise, our results highlight the importance of isolating aid allocation from politics which have nothing to do with development cooperation. Especially, given the fact that financial volatility has been shown to hinder economic development, it should be avoided that aid adds to the problem.

The point is closely related to the findings of Chapter 3, which shows that development aid can actually increase the risk that a small scale civil conflict within recipient countries escalates to armed conflict or war. Improving on previous measures and estimating the causal effect of aid on conflict dynamics allows us to partial out the effect of aid and the state dependence of conflict itself. We can show that in sum bilateral aid matters only in some circumstances, but has no effect once violence is widespread. One of the main reasons for this finding is that civil conflict much like unemployment exhibits very high state dependence. Thus, once a conflict has actually erupted it is very hard to overcome. Our findings have broad implications for empirical conflict research, that so far mostly ignores state dependence. Advances in geospatial-analysis that allow researchers to zoom into the affected regions should incorporate the dynamic framework we propose. The policy implications from our work are far reaching as well. Donor countries should better monitor which governments get bilateral aid and when. Institutional considerations have been implemented in the allocation of development aid for several decades the monitoring of violence especially at smaller stages is however still largely absent. Given our findings, this is especially worrisome, since aid seems to increase the risk of conflict escalation.

Monitoring seems also to be required when recommending administrative reforms. Chapter 4 shows that district proliferation does not promote economic development in general, but needs to fit the political circumstances of the country in question. In general we find substantial regional heterogeneity in the effects of territorial reforms on economic activity. While decentralization reforms have been successful in promoting economic activity in Asia, centralization reforms have been beneficial in Africa. Furthermore, we find that local power and political proximity to the administrative centers matter. Decentralizations are most beneficial if local politicians are elected, while the opposite is true for centralizations. During territorial centralizations, areas that are further removed form the administrative hub gain less in economic activity compared to those more centrally located. However, none of the political mechanisms analyzed can explain the substantial differences between Africa and Asia. Hence, further research is necessary to uncover what drives the different results. In general our results should caution researchers to rely on case study evidence of either the success or failure of specific territorial administrative reforms for general conclusions. Similarly, international organizations should refrain from prescribing one-size-fits-all policy recommendation for heterogeneous countries.

In Chapter 5 of the thesis we switch the focus to the classical donor countries within the OECD and study whether migration is related to organized violence, specifically terrorism. The fear of terrorism has led many OECD countries to tighten their migration policies and reduce migration in general. Thus depriving people of many developing countries of the opportunity to develop skills and earn money abroad. Our results show that while there is a positive effect related to the amount of foreign-born on terrorism within host countries, the obtained effect is primarily explained by scale. We cannot confirm that increases in the foreign born population increase the risk of terrorism more than increases in the native population. What is more, we find that policies negatively targeting migrants actually increase the risk of terror. Hence, it is counterproductive policy to make life for migrants harder within OECD countries, since it does not make them more safe, but deprive them of talent and deprive people in developing countries of economic gains. This is not to say that migration does not create economic losers within host countries, but the security concern with respect to terrorism is not a tradeoff with potential economic gains both within industrialized and developing countries.

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