Conflict, Income Shocks, and Foreign Policy: Macro- and Micro-Level Evidence

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To Yunus Gibba

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Chapter 3: Stimulant or depressant? Resource-related income shocks and conflict

Chapter 4: Foreign interventions and community cohesion in times of conflict

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Introduction

Recent estimates show that "[b]y 2030, more than half of the world's poorest people will live in very poor countries that are fragile, affected by conflict, or experience high levels of violence."[1] In 2016, for instance, 489 million people suffered from hunger in countries exposed to conflict.[2] Large numbers of people being killed, injured or deprived of food and water are not the only devastating consequences of conflict and war. According to UNHCR, 44,400 people a day have to flee their homes because of conflict and persecution.[3] To understand what causes conflict with the aim of decreasing the risk of its occurrence, its duration, and its consequences, researchers have analyzed the determinants for many decades. Blattman and Miguel (2010) summarize this literature, highlight important limitations, and push for ways to proceed.[4] While the literature arrived at the consensus that income is one of the strongest determinants of conflict (e.g., Collier and Hoeffler, 1998; Fearon and Laitin, 2003; Blattman and Miguel, 2010), conflict has been found to be also one of the main obstacles to development (e.g., Collier, 1999; Abadie and Gardeazabal, 2003). Therefore, it is not surprising that conflict tends to be concentrated in low-income countries. Evidence on the direction of the effect, however, remains to be debated despite the new methods that have been proposed which help to disentangle the causal effects of potential drivers of conflict (see, e.g., Miguel et al., 2004; Berman and Couttenier, 2015).

The question of what drives conflict has more recently received much attention in the media. This is due to the refugee crisis that *reached* Europe in 2015, after it had *hit* many developing countries, which happen to be the neighbors of most conflict-ridden countries.[5] As the highest number of refugees originates from countries suffering from conflict and war, the question how to reduce the risk of conflict has been put on the politicians' decision-making table. Politicians in developed countries only started to realize the need

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[1]Source: https://blogs.worldbank.org/voices/how-we-re-fighting-conflict-and-fragility-where-poverty-deepest, accessed July 4, 2018.

[2]Source: https://www.wfp.org/news/news-release/hunger-conflict-zones-continues-intensify, accessed July 6, 2018.

[3]Source: http://www.unhcr.org/figures-at-a-glance.html, accessed July 4, 2018.

[4]For instance, Blattman and Miguel (2010) recommend the analysis of counterinsurgency strategies as a promising area of research of civil wars. In Chapters 3 and 4 of this thesis I consider the role of counterinsurgency strategies.

[5]In fact, 85% of the world's displaced people are still in developing countries. Source: http://www.unhcr.org/figures-at-a-glance.html, accessed July 1, 2018.

to react once the issue has affected them more directly. The German Chancellor Angela Merkel even confesses that "we have looked away for too long."[6] In line with the academic literature, the lack of development has been highlighted as an important root cause for conflict and refuge. The then-President of the European Commission José Manuel Barroso, for instance, stated that the European Union must continue their "political and development action to improve the living conditions in the countries of origin, working with them there, so that people do not have to flee their homes."[7] Consequently, many European countries decided to increase their development assistance in order to fight root causes of flight but also to respond to new challenges in their own countries on how to deal with refugee inflows. Whereas reducing poverty and the risk of conflict have of course always been the objectives of development cooperation, these objectives have now been discussed more loudly in the media.

To put it in the words of Jeremy Weinstein's keynote address at the *Household in Conflict Network* (HiCN) meeting in Brussels in November 2017 "Does social science belong at the decision-making table?". I think it should. Therefore, all the chapters of this thesis address specific policy measures that are of major political and economic importance. Broadly speaking, they all deal with the question of how foreign policy affects conflict and development.

The first two chapters take a macro level perspective, while the subsequent chapters consider the micro-foundations by looking at one specific country and context. Development economics has long been divided between the macro- and micro-development camp. However, already ten years ago, Rodrik (2008) observed the two camps to converge with regard to their policy mindset. Weighing external against internal validity, I decided to combine both the macro- and micro-based approach. While internal validity relates to the quality of causal identification, external validity cares about the generalizability. Micro level evidence in the form of randomized experiments is strongest on internal validity, but restricted to a particular context. Macro level approaches are usually weaker on internal validity, but given a credible identification, provide evidence for a broader population. Combining both approaches – that come with their costs and benefits – enables a comprehensive understanding of the overarching research question of this thesis.

In Chapter 1 – together with my coauthors Richard Bluhm, Martin Gassebner, and Paul Schaudt – I address the question of how development aid affects conflict in a cross-country panel analysis. This allows to derive a general picture of the overall effects of development aid on conflict. We also consider heterogeneous effects depending

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[6]Source: https://www.zeit.de/politik/deutschland/2018-03/angela-merkel-regierungserklaerung-bundestag, accessed July 2, 2018.

[7]Source: http://europa.eu/rapid/press-release_SPEECH-13-792_en.htmEuropean Commission, accessed July 2, 2018.

on the country's previous level of violence by accounting for the high persistence and state dependence of conflict and war. Income is one relevant channel through which development aid might affect conflict and consequently also refugee flows. Thus, in Chapter 2 of my thesis, I investigate the effects of development aid on GDP per capita growth jointly with Axel Dreher. This paper more generally adds to the literature on the effectiveness of development aid. In both chapters, I apply a new estimation strategy that allows to identify causal effects of aid on growth and conflict for a large set of recipient and donor countries over a long period of time. Macro level analyses like this probably relate more directly to what policy-makers care most about when it comes to foreign policy tools including the provision of development aid.

Yet, analyses at the macro level might not be sufficient to understand the mechanisms through which income affects conflict and under which conditions we can expect foreign policy measures to be effective. I turn to the micro level for the two subsequent chapters of this thesis. In particular, I focus on Afghanistan, which has been plagued by conflict for decades. This has resulted in a large number of people being affected, such as those that have to leave their homes. Afghan refugees currently make the second-largest population of refugees with 2.6 million people at the end of 2017.[8] Due to these devastating conditions, Afghanistan has been exposed to different foreign policy measures that aim at counterinsurgency, reconstruction and peace-building, making it an interesting case to analyze from the policymakers' perspective. The country also serves as a very practical case from the researchers' perspective. Despite that Afghanistan is one of the most severely conflict-ridden countries in the world, data at the household level as well as geocoded conflict data are available for almost the entire country.

Afghanistan is characterized by large inflows of development aid of different types including military-led projects, but also by the deployment of international military forces. These two strategies can be classified as either a "winning hearts and minds-oriented" approach or an approach based on peacekeeping and counterinsurgency strategies. Both approaches are common foreign policy tools that became increasingly important during the past half-century, as illustrated in Figure 1. The figure plots Official Development Assistance (ODA) from all donor countries to all recipient countries over the 1960-2014 period in tandem with the number of ongoing peacekeeping missions (PKM) from the United Nations (UN), regional inter-governmental organization, states or ad hoc groups of states.[9] We can observe a constantly rising trend in development aid as measured by total net ODA, in particular within the last two decades, and a rising number of foreign peacekeeping missions, with a downward trend only in the most recent years.

While every conflict is different, the latter two chapters help to identify core features

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[8]Source: http://www.unhcr.org/statistics/unhcrstats/5b27be547/unhcr-global-trends-2017.html, accessed July 1, 2018.

[9]The list of PKM provided by Mullenbach (2013) includes the mission of the International Security Assistance Force (ISAF) in Afghanistan, on which I elaborate in Chapter 4.

FIGURE 1

Aid and ongoing peacekeeping missions
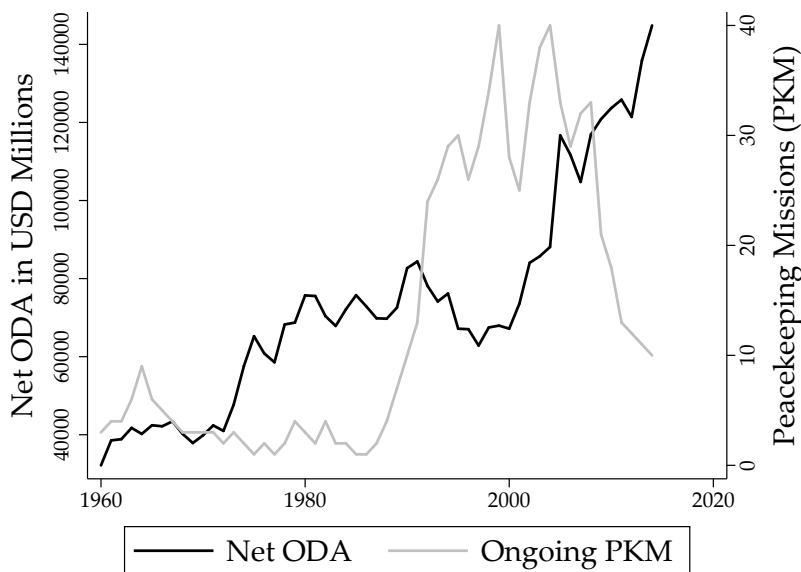
Notes: The figure plots the evolution of ODA and PKM over the 1960-2014 period. ODA is measured in net disbursements in constant US Dollars, Millions (ODA: Total Net, constant 2016 prices) from OECD (2018). Data on ongoing peacekeeping missions is from Mullenbach (2013) and covers third-party peacekeeping missions from UN, regional inter-governmental organization, state or ad hoc group of states.

and mechanisms to allow for a better understanding of heterogeneities across different conflict or country contexts. Chapters 3 and 4 shed light on i) specific features that play a decisive role in how income affects conflict and ii) the conditions under which we can expect foreign policy measures to be effective. These chapters also highlight that the different mechanisms are case-specific and that we cannot derive conclusions for all countries, all income sources or all types of conflict without accounting for these different features and environments.

In Chapter 3 of this thesis, Kai Gehring, Stefan Kienberger, and I consider how opium-related income shocks affect the incidence and intensity of conflict. This analysis relates to the previous chapters by investigating the effect of income as a main driver of conflict and by identifying more fine-grained mechanisms. In addition, the relation between (illegal) resource-related income shocks and conflict is likely to be affected by foreign policy choices. This is the case because opium-related income shocks are driven by changes in demand in Western countries and thus in the international opium price and because the international community is involved in the enforcement of laws against the production, distribution or use of illegal products.

In the final chapter of this thesis, Chapter 4, I turn to the second type of foreign policy measures as illustrated in Figure 1. I expand on households' coping strategies and consider how community cohesion and local institutions are affected by the presence of

Western forces in Afghanistan. In her keynote address at the *Development Economics and Policy Conference* in Zurich in June 2018, Rohini Pande emphasized the need to invest in the *invisible infrastructure* in order to achieve development. As she notes "this also implies recognizing and responding to social norms and underlying power structures." In most parts of Afghanistan, power traditionally tends to be local. This makes it an interesting case to analyze when it comes to the effectiveness of state-building approaches as envisaged by the foreign military intervention. While public policy often ignored structures of human interactions, they represent the "glue that holds society together" (Janmaat, 2011). Literature has argued that social cooperation is beneficial for development (Knack and Keefer, 1997), and for reducing the risk of conflict (Collier and Hoeffler, 2004b). Therefore, in order to create an environment in which development projects can work and where reconstruction efforts can be made, we need to understand how institutions at the local level are affected by foreign interventions.

Figure 2 summarizes how the four chapters of this thesis relate to each other. All four chapters consider determinants of conflict and development in a globalized world and try to shed more light on the effectiveness of different foreign policy measures. Obviously, this figure plots just an extract of the true underlying processes. It serves as a stylized overview of the different elements that are in the focus of this thesis. There are many more potential drivers of conflict and development, many more channels, and different ways of interconnectedness of all these elements. Some of those elements that are not part of this thesis but which I analyze in related studies are indicated in light grey font.

In Langlotz and Potrafke (2016), for instance, we consider the effect of aid on military expenditures as one alternative or additional channel through which conflict might be affected.[10] In Dreher et al. (2018) we go one step further in responding to the policymakers' current main question of how to deal with increased refugee flows. We analyze the effects of development aid on refugee flows to the world and to OECD donor countries in particular.[11]

The figure highlights another common aspect of the four chapters of this thesis, which is the attempt to analyze causal effects. The dark shaded arrows plot the direction of causality that I address in my analyses. Arrows in light grey point to alternative directions of how the elements might be interconnected. In this way, the arrows in light grey also indicate the potential for reversed causality with which I have to deal with in all chapters of this thesis.

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[10]This relates to the discussion of the fungibility of development aid. Development aid by definition is not meant to end in the military budget of a recipient. When accounting for extreme outliers, our results do not point to a diluting effect of aid to the military on average.

[11]We find evidence for aid to reduce refugee flows after four three-year periods, which seem to be driven by very long-term effects of aid on growth.

FIGURE 2
The interplay of Chapters 1-4
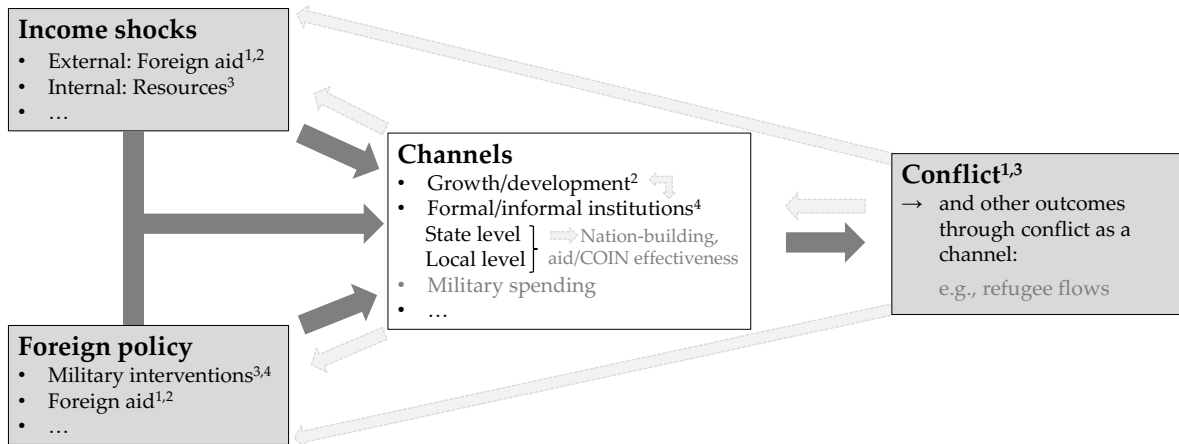
Empirical Approach

In all four chapters, I follow recent trends in the literature and apply estimation strategies that address endogeneity concerns with the aim of deriving causal estimates. These different strategies include *high-dimensional fixed effects*, *instrumental variable (IV)* and *reduced-form* approaches, and a *regression discontinuity design*. Depending on the data and level of analysis, I apply different estimation techniques as I describe in more detail in the following.

The data that I exploit comes from various levels, starting with the cross-country level in Chapters 1 and 2. In both chapters, I do, however, exploit an IV strategy, which is based on information at the dyadic level, i.e., country-pair level. My coauthor Axel Dreher and I proposed a new IV for aid, which I use in both chapters. More precisely, we exploit the interaction of an exogenous time-varying variable that varies at the donor-year level with an endogenous variable that varies at the donor-recipient level. Using the interaction of an exogenous with an – at least to some extent – endogenous variable has recently become increasingly popular, as this strategy allows to derive causal estimates of the interaction term given that one controls for the levels of the interaction (see Nizalova and Murtazashvili, 2016; Bun and Harrison, 2018). This strategy resembles a difference-in-difference-like (DiD) setup and the discussion of the exclusion restriction follows this line of reasoning. Most prominently, Nunn and Qian (2014) have applied this strategy in their study on US food aid and conflict and Werker et al. (2009) in their study on aid from Arab oil-producing donors and growth in Muslim recipient countries. In Chapters 1 and 2, we go one step further in bringing Nunn and Qian's IV strategy to the dyadic

level by using a "gravity-style" equation as put forward by Frankel and Romer (1999) and Rajan and Subramanian (2008). We thus combine two approaches, the DID-like setup as in Nunn and Qian (2014) with a gravity-style equation. The first two chapters elaborate on the effects of total bilateral aid at the recipient-year level such that the IV has to be aggregated in order to make use of it at the monadic level. Given the complexity of this IV setup, I apply different aggregation techniques. To ensure that the variation in the IV at the recipient-year level originates from the exogenous part of the dyadic IV, I run simulation tests and determine under which conditions the different techniques lead to the same estimates. In Chapter 1, we deal with further complexity as we are interested in the effects of an endogenous regressor under the presence of heterogeneity and first-order dynamics in a non-linear setting. Therefore, we combine *correlated random effects* and a *control function approach* with a *dynamic panel ordered probit model.* To do so, we extend the approaches by Wooldridge (2005) and Giles and Murtazashvili (2013) and – to the best of our knowledge – are the first to apply this in a dynamic ordered setting.

With an increasing popularity of a new identification strategy, there also comes more criticism, as most recently put forward by Christian and Barrett (2017). They show that non-linear trends in the time-varying variable can be problematic and propose placebo tests based on randomization inferences, which I apply in my analyses to rule out this valid concern. Despite this recent critic on DID-type IV estimators in panel data, IV strategies in general face at least two other main criticisms. First and most obviously, the excludability of the instrument, which cannot be tested. Second, on whether the Local Average Treatment Effect (LATE) is representative and can be generalized. The ways how I measure income shocks are already specific, as neither aid nor income from the illegal opium business directly affect all households in a country. They are even more specific if we consider the exogenously induced changes of these income sources when applying IV strategies. Notwithstanding, these two income sources represent a non-negligible part of the income for which causal effects can be measured. This would be impossible for overall income. What is more, these parts of overall income depend on national and international policy choices and can thus be changed. This again relates to the keynote by Jeremy Weinstein pushing for the analysis of actual policy measures, which can indeed be influenced.

After having combined dyadic data with monadic data in Chapters 1 and 2, I turn to the micro level in Chapters 3 and 4. I use subnational data and apply different units of analysis starting from the ADM1 (province) to ADM2 (districts), to the village, and household level. This allows me to exploit even further estimation strategies including *spatial econometrics* and *high-dimensional fixed effects*. In both chapters, I also make use of the interaction between an exogenous time-varying variable with a variable that varies over space and apply this in a reduced-form and IV approach. Additionally, I make use of a *natural experiment* in the form of a *geographic regression discontinuity design.* In

particular, I follow the recent literature (e.g., Dell, 2010; Dell et al., 2017) and exploit a geographic boundary that assigns households to a treated and control area "in an as-if random fashion" (Keele et al., 2015, p. 127). In the last two chapters, I apply multiple identification strategies, which allow me to compare the LATEs and to get a better sense of the size of the true causal effects.

While the causal analysis is very helpful in many means, we should still not forget that policymakers might be even more interested in the analysis of "purposeful policy actions" and not only of the exogenously driven part. According to Rodrik (2012, p. 148) – who refers to the empirical literature on policies and growth – "an equally important limitation of IV is that what we are typically interested in knowing is the impact of purposeful policy action." Following his line of argument, by applying IV methods, we answer a rather different question, as the exogenous component covers "policy interventions that governments did not adopt purposefully." However, given that the *official* and the true underlying (*unofficial*) purpose might deviate, focusing on conditional correlations can still be misleading. Taking the example of ODA, which "is administered with the promotion of the economic development and welfare of developing countries as its main objective."[12] However, as extensively shown by the literature, ODA is frequently given for political reasons rather than because of the official claims (amongst others, Kuziemko and Werker, 2006; Dreher et al., 2009). Ordinary least squares (OLS) regressions would thus analyze the effect of politically driven aid and we could not derive policy implications for aid, which is given according to economic needs. IV strategies help to disentangle "what causes what" by netting out the part that is motivated by *official* and *unofficial* purposes of the respective policy measure. To the extent that both *official* and *unofficial* (often unknown) purposes correlate with a bulk of observable and unobservable factors that likely bias the result, there are good reasons to apply a strategy based on causal identification. In any case, when interpreting results and deriving policy implications from OLS and IV estimates, we have to take these arguments seriously and carefully think what part of the policy measure we *can* and *want* to capture in the analysis.

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[12]Source: http://www.oecd.org/dac/stats/officialdevelopmentassistancedefinitionandcoverage.htm, accessed July 3, 2018.

Summary of the chapters

The four chapters contribute to different strands of the literature. They add to the literature on the determinants of conflict by providing causal evidence at the macro- and micro-level and by identifying new features and heterogeneities that help to solve the puzzle of why shocks to different income sources – including (illegal) resources and the provisions of foreign aid – affect conflict differently in different environments. The third chapter more specifically adds to the literature on the resource curse and in particular to the strand focusing on the resource-conflict nexus and illegal resources. The thesis adds to the literature on the effectiveness of foreign policy tools as development aid and military interventions. The empirical results presented in this thesis thus help to derive implications for the effects of foreign policy on conflict, institutions, and nation-building more broadly. In the following, I summarize the main empirical strategy, the major contributions, and findings of each of the four chapters.

Chapter 1: Fueling conflict? (De)Escalation and bilateral aid

Joint work with Richard Bluhm, Martin Gassebner, and Paul Schaudt[13]

In Chapter 1 of the thesis, we study the effects of bilateral foreign aid on conflict escalation and de-escalation in 125 developing countries over the 1975 to 2010 period. We add to the literature on the determinants of conflict (e.g., Collier and Hoeffler, 1998; Hegre and Sambanis, 2006) and to the literature on development aid and conflict in particular (e.g., De Ree and Nillesen, 2009; Savun and Tirone, 2012). Both strands of literature have not arrived at a consensus yet.

Theoretically, the relationship between aid and conflict is ambiguous as it is the case for income and conflict more generally. This is because rising opportunity costs, increasing state capacity, and greater gains from capturing the state are all plausible consequences of development assistance. This relation is not only disputed on theoretical grounds, but also empirically. Convincing evidence is limited either to single countries (e.g., the Philippines, Crost et al., 2014), to specific types of aid (e.g., US food aid, Nunn and Qian, 2014), or a combination of both (e.g., US military aid in Colombia, Dube and Naidu, 2015). Credible evidence on the role of overall bilateral aid on conflict for a large set of aid recipient and donor countries is lacking. While the theoretical literature has highlighted the dynamic nature of conflict, the empirical literature still lags behind.

We combine data on civil wars with data on low-level conflicts in a new ordinal measure capturing the multifaceted nature of conflict. The measure is based on the standard UCDP-PRIO classifications of civil conflict and war, which we complement with observations from the Cross-National Time-Series Data Archive (CNTS) (Banks and Wilson, 2015) on government purges, assassinations, riots, and guerrilla warfare to

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[13]This paper is available as CESifo Working Paper No. 6125 (Bluhm et al., 2016).

capture small conflict. Neglecting smaller conflicts likely pollutes previous estimates of the effect of aid on conflict, as we expect heterogeneous effects according to the intensity of conflict. Since aid flows tend to disproportionately flow to the birth region of the current ruler (Dreher et al., 2015, see), this is likely to translate into civil discontent that can find its expression in smaller acts of violence. In addition, small conflicts allow rebels to get an estimate of how easily collective action problems can be overcome and provide information about the government's repressive capabilities. As any violent behavior questions the state's monopoly of violence, small conflicts as we define them can considered the most basic definition of civil conflict. We argue that foreign aid may exacerbate violent tendencies in such environments but not when society is truly at peace.

Since identification of endogenous regressors and their partial effects under the presence of heterogeneity and first-order dynamics is tricky in non-linear settings, we develop a novel empirical framework. We propose a dynamic ordered probit estimator that allows for unobserved heterogeneity and corrects for endogeneity. To model the ordered conflict outcome, we combine correlated random effects and a control function approach with dynamic panel ordered probit models. We also contribute to the literature by identifying the causal effect of foreign aid on conflict by predicting bilateral aid flows based on electoral outcomes of donor countries that are exogenous to recipients, which is based on the strategy derived in Chapter 2. We find that the effect of foreign aid on the various transition probabilities is heterogeneous. Receiving bilateral aid raises the chances of escalating from small conflict to armed conflict, but we find no evidence that aid ignites conflict in truly peaceful countries.

Chapter 2: Aid and growth. New evidence using an excludable instrument
Joint work with Axel Dreher[14]

The second chapter of the thesis, which is joint work with Axel Dreher, relates to the first one to the extent that income is considered one of the most important determinants of conflict. More generally, the chapter contributes to the large literature on the effectiveness of development aid (e.g., Doucouliagos and Paldam, 2009; Clemens et al., 2012). Most of this literature, however, does not provide unbiased estimates of aid and growth, or does derive causal estimates, but only for a a subset of donors, recipients, or types of aid as discussed before in the summary of Chapter 1.

While not being the first that provide IV estimates in regressions of growth on aid, we argue to provide evidence for a much broader set of countries over a longer period than previous studies with convincing identification strategies. For instance, Galiani et al. (2017) use an IV which results in a very particular LATE, which is likely not to be

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[14]This paper is available as Heidelberg University Discussion Paper No. 635 (Dreher and Langlotz, 2017).

representative.[15] Werker et al. (2009) or Ahmed (2016) on the other hand restrict their analysis to particular sets of donors or recipients from the beginning.

We propose an excludable instrument to test the effect of overall bilateral foreign aid on economic growth in a sample of 96 recipient countries over the 1974-2009 period. More precisely, we interact donor government fractionalization with a recipient country's probability of receiving aid. The literature and our results show that fractionalization increases donors' aid budgets through increased government expenditures, representing the over-time variation of our instrument. This can be explained by logrolling behavior. The probability of receiving aid introduces variation across recipient countries. Given that the effect of the potentially endogenous variable is controlled for, the interaction of the endogenous variable with an exogenous one can be interpreted as exogenous under mild conditions (see, e.g., Bun and Harrison, 2018; Nizalova and Murtazashvili, 2016). This chapter contributes to the literature by introducing a new IV which can be used to address a large number of questions in the aid effectiveness literature (see, e.g., applied by Ziaja, 2017).

Our IV results show no significant effect of aid on growth in the overall sample. We also investigate the effect of aid on consumption, savings, and investments. We also find no significant effect of aid on any of these outcomes. This result might also helps to understand why we find no conflict-reducing effect of aid in Chapter 1, given that we find no evidence for income being positively affected. In a related study on aid and refugees, which is joint work with Axel Dreher and Andreas Fuchs (Dreher et al., 2018), we see, however, that the effects on refugee flows seem to be driven by very long-term effects of aid on growth after four three-year periods. If at all, these results point to aid being effective only in the very long-term.

Chapter 3: Stimulant or depressant? Resource-related income shocks and conflict

Joint work with Kai Gehring and Stefan Kienberger[16]

Chapters 3 and 4 now turn to the micro level, in particular, to the context of Afghanistan. In Chapter 3 – together with Kai Gehring and Stefan Kienberger, – I consider the effect of opium-related income shocks on conflict and investigate the underlying mechanisms. In analogy to Chapter 1, it also adds to the literature on the determinants of conflict by investigating the effects of income from a micro level perspective (as, for instance, Dube and Vargas, 2013; Berman and Couttenier, 2015). In this chapter, we try to provide explanations for why the literature on resource-related income shocks and conflict has not arrived at a consensus yet. We, therefore, elaborate on the role of specific features of

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[15]They instrument aid flows with the International Development Association's (IDA) threshold for receiving concessional aid. The variation in this IV is rare such that the estimate is driven by few countries that pass the threshold once.

[16]This paper is available as Heidelberg University Discussion Paper No. 625 (Gehring et al., 2018).

the income source and the conflict environment.

We combine temporal variation in international drug prices with new data on spatial variation in opium suitability.[17] In analogy to the previous chapters, this empirical strategy resembles a DID-like setup. Rather than only using the international price of heroin (a product made from opium), which might be endogenous given that Afghanistan is the main producer of opium, we also rely on price changes in complements to heroin. We make use of the international prices of heroin and complementary drugs in an IV and reduced form setting. We amplify this strategy with two alternative IV strategies based on climatic differences and changes in legal opioid prescriptions in the United States.

District level results show a very robust conflict-reducing effect of higher drug prices over the 2002-2014 period, both in a reduced-form setting and with the three different IVs. We provide evidence for two main mechanisms. First, we argue and show that the relevance of contest effects depends on the degree of violent group competition over valuable resources. To test this, we consider whether a district is dominated by the Taliban or the government, ethnically mixed or features foreign military bases. We argue that a conflict-inducing effect of a resource-shock is more likely if there were different groups violently fighting for suitable areas (as it is the case in Colombia, see Angrist and Kugler, 2008; Mejia and Restrepo, 2015). In Afghanistan, we find no evidence for strong group competition for suitable districts.

Second, we provide evidence along the opportunity cost hypothesis by showing that opium profitability has stronger effects in districts that account for a higher share of value added along the production chain measured by the presence of opium markets, labs and trafficking routes. This affects both the intensive margin (higher revenues) as well as the extensive margin (more people benefiting). This finding is backed by identifying positive effects of increases in opium profitability on various measures of household living standards by using household level data. Lastly, we find that the effect begins to matter after the dissolution of large armed militias due to an exogenous policy change by the international coalition. This relates to the role of foreign military interventions in times of conflict, which I exploit in more detail in Chapter 4.

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[17]Kienberger et al. (2017) introduced the measure on opium suitability.

Chapter 4: Foreign interventions and community cohesion in times of conflict

Single-authored[18]

In the final chapter of my thesis, I consider the role of foreign policy measures in times of conflict on the *invisible infrastructure*.[19] In particular, in Chapter 4, I analyze how foreign military interventions relate to the internal cohesion of communities and the role of local institutions in Afghanistan. I consider the role of the International Security Assistance Force (ISAF), which has been one of the largest coalitions in NATO's history. In an environment where state institutions are unstable or even lacking, community cohesion and local governance play a central role. Power in rural Afghanistan indeed tends to be at the local level.

As it is well accepted that local communities are relevant partners in counterinsurgency, development cooperation and post-conflict reconstruction activities, it is important to understand these institutions in order to derive effective policy measures. The chapter also investigates the interplay between the two main foreign policy approaches during conflict and war, the "winning hearts and minds-oriented" approach versus the counterinsurgency approach. So far, the role of ties within communities has received little attention in the literature on the effects of either strategy. Studies have focused on wartime information (e.g., Berman and Matanock, 2015; Wright et al., 2017), or attitudes towards and collaboration with pro-government forces including international troops versus insurgents (e.g., Lyall et al., 2013; Hirose et al., 2017; Schutte, 2017; Child, 2018).

I apply three different estimation techniques in my analysis, which covers the period from 2005 to 2014 and roughly 90% of the country. I exploit household level information from two surveys and apply a high-dimensional fixed effects panel analysis.[20] The second technique relies on an interaction with an income shock that introduces exogenous variation in the need to rely on support and thus allows analyzing heterogeneous effects according to the presence of foreign military forces. Third, I make use of the step-wise regional enlargement of ISAF's mandate in a geographical regression discontinuity design. I exploit the fact that the boundary between the northern regional command – where ISAF has been deployed to first – and the rest of the country – where the mandate enlargement took place with a time lag – splits households into a control and treatment area in an "as-if-random" assignment.

The findings of the three different approaches all suggest that households in districts where foreign military forces are present receive less help from others in their community, have less trust in community councils and participate less in those councils. I find no

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[18]This paper is soon available as Heidelberg University Discussion Paper (Langlotz, 2018).

[19]The literature which elaborates on the effects of conflict on social capital is increasing. See Bauer et al. (2016) for a summary.

[20]I rely on data from the National Risk and Vulnerability Assessment and the Survey of the Afghan People, which are both large-scale representative household surveys.

evidence for a crowding out of these traditional institutions by new (and more formal) institutions.[21] Rather my results suggest that development aid becomes less effective in those regions, where the international military forces are present. This fits the anecdotal evidence and points again to the need of "recognizing and responding to social norms and underlying power structures" as Rohini Pande argued in her keynote. This paper relates to the previous chapters by shedding more light on the micro-foundations, which help to explain why development aid or foreign military interventions might not be as effective as intended for.

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[21]Note that the empirical (e.g., Acemoglu et al., 2014; Guiso et al., 2016; Dell et al., 2017; Lowes et al., 2017) and theoretical (e.g., Bowles and Gintis, 2002; Acemoglu and Robinson, 2017) literatures provide mixed results on whether strong state capacity is a complement or substitute of governance and cooperation at the community level.

Chapter 1

Fueling conflict? (De)Escalation and bilateral aid

Joint work with Richard Bluhm, Martin Gassebner, and Paul Schaudt

Abstract

This paper studies the effects of bilateral foreign aid on conflict escalation and deescalation. 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 by predicting bilateral aid flows based on electoral outcomes of donor countries that are exogenous to recipients. 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.

1.1. 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., US food aid, Nunn and Qian, 2014) or both (e.g., US 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 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 paper. 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. (2015), 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 (following Chapter 2; Frankel and Romer, 1999; Rajan and Subramanian, 2008). This type of identification strategy is now common in the trade and migration literature 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.[1]

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.

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[1]In Chapter 3 I will expand on the channels of how income shocks affect conflict from a micro perspective.

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 paper is organized as follows. Section 1.2 discusses the related literature and provides the theoretical background. Section 1.3 introduces our new ordinal conflict measure. Section 1.4 outlines our empirical model and identification strategy. Section 1.5 presents the empirical results and Section 1.6 discusses a battery of robustness checks. Section 1.7 concludes.

1.2. Related literature

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., Chapter 2; Rajan and Subramanian, 2008; Clemens et al., 2012). The literature on income shocks and conflict is also instructive. While evidence at the country level has not arrived at a consensus yet (e.g., Miguel et al., 2004; Bazzi and Blattman, 2014), Berman and Couttenier (2015) find negative income shocks to 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 (Collier and Hoeffler, 2007; Langlotz and Potrafke, 2016). Although official development aid excludes military aid by definition, receiving aid relaxes the government's budget constraint if aid is sufficiently fungible.

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, 2008). 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 US 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.

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., 2012a; 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.[2] This could be the case for at least two reasons. First, aid is not distribution-neutral (see, e.g., Dreher et al., 2015, 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.

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

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 as an instrument for bilateral aid flows which could work through a variety of other channels, such as trade or foreign direct investment (FDI). A noteworthy exception are Nunn and Qian (2014) who use lags of US wheat production interacted with each recipient's frequency of receiving aid as an instrument for US food aid.[3] 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 is done in Chapter 2, which first introduced 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.

1.3. 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 1.A (Tables 1.6 to 1.8).

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[3]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.

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 (BRD) and then to create dummy variables indicating the surpassing of one of two thresholds (25 or 1,000 BRD) 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."

FIGURE 1.1
Distribution of conflict intensities
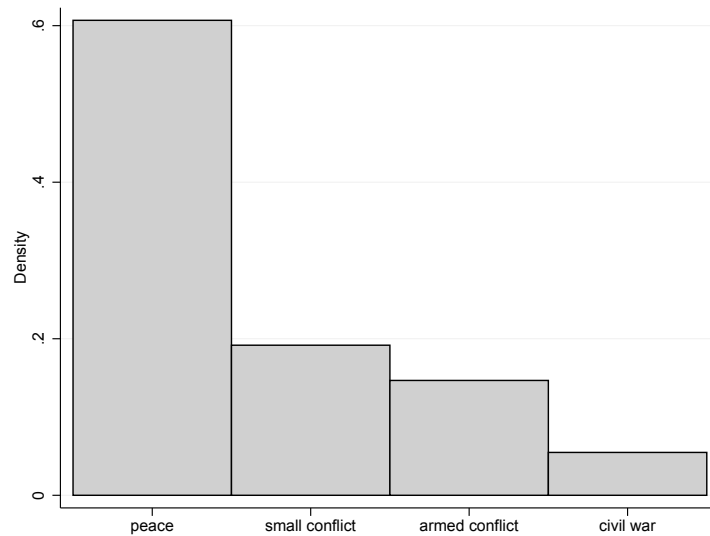
Notes: Illustration of the unconditional distribution of the ordinal conflict measure. There are 3,014 peace years, 739 small conflict years, 544 armed conflict years, and 203 civil war years in our sample.

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 BRD per annum. We call conflicts that reach this state but do not exceed 1,000 BRD in a given year 'armed conflict.' At the top, we add a category called 'civil war' if there are more than 1,000 BRD. 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).[4] All of these categories are manifestations of civil conflict,

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[4]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

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 of which is the government (two-sided, state-centered).[5] 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 1.1 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 BRD to create intermediate levels of armed civil conflict (e.g., Esteban et al., 2012a; 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 1.C presents the case of Sri Lankan Civil War to illustrate the benefits of our coding in more detail.

TABLE 1.1
Unconditional transition matrix

From State	To 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%.

Table 1.1 shows the unconditional transition probabilities as they are observed in our data. This simple exercise already allows us to make three worthwhile points. First, the

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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 as civil war.

[5]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, accessed July 9, 2018).

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).

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).

1.4. Empirical strategy

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.[6] 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

$$c_{it}^* = \mathbf{x}_{it}'\boldsymbol{\beta} + \mathbf{h}_{i,t-1}'\boldsymbol{\rho} + (\mathbf{x}_{it} \otimes \mathbf{h}_{i,t-1})'\boldsymbol{\gamma} + \mu_i + \epsilon_{it}, \tag{1.1}$$

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 types of

---

[6]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.

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:

$$
\begin{aligned}
\Pr[c_{it} = j|\mathbf{x}_{it}, h_{j+p,i,t-1} = 1] =& F\left[\alpha_{j+1} - \mathbf{x}_{it}'\boldsymbol{\beta} - \rho_{j+p} - (\mathbf{x}_{it} \times h_{j+p,i,t-1})'\boldsymbol{\gamma}_{j+p} - \mu_i\right] \\
&- F\left[\alpha_j - \mathbf{x}_{it}'\boldsymbol{\beta} - \rho_{j+p} - (\mathbf{x}_{it} \times h_{j+p,i,t-1})'\boldsymbol{\gamma}_{j+p} - \mu_i\right],
\end{aligned}
$$
$$(1.2)$$

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

$$
\begin{aligned}
\frac{\partial}{\partial x_k}\left(\Pr[c_{it} = j|\mathbf{x}_{it}, h_{j,i,t-1} = 1]\right) =& (\beta_k + \gamma_{j,k})\left(f\left[\alpha_j - \mathbf{x}_{it}'\boldsymbol{\beta} - \rho_j - (\mathbf{x}_{it} \times h_{j,i,t-1})'\boldsymbol{\gamma}_j - \mu_i\right]\right. \\
&\left.- f\left[\alpha_{j+1} - \mathbf{x}_{it}'\boldsymbol{\beta} - \rho_j - (\mathbf{x}_{it} \times h_{j,i,t-1})'\boldsymbol{\gamma}_j - \mu_i\right]\right),
\end{aligned}
$$
$$(1.3)$$

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 dependence in state j as follows:

$$
S_j = (NT)^{-1} \sum_i^N \sum_t^T \left(\Pr[c_{it} = j|\mathbf{x}_{it}, h_{j,i,t-1} = 1] - \sum_{r \neq j} \omega_{rj} \Pr[c_{it} = j|\mathbf{x}_{it}, h_{r,i,t-1} = 1]\right),
$$
$$(1.4)$$

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.

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 (IV) 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

$$c_{1it}^* = \mathbf{z}_{1it}'\boldsymbol{\beta}_1 + \beta_2 a_{2it} + \mathbf{h}_{1i,t-1}'\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + u_{1it}, \tag{1.5}$$

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 Equation 1.5 and from now on estimation runs over $t = 1, \ldots, T$.

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

$$a_{2it} = \mathbf{z}_{1it}'\boldsymbol{\alpha}_1 + \mathbf{z}_{2it}'\boldsymbol{\alpha}_2 + \mu_{2i} + \lambda_{2t} + u_{2it}, \tag{1.6}$$

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 $b_{2i}|\mathbf{z}_i \sim \mathcal{N}(0, \sigma_{b_2}^2)$ and $\mathbf{z}_i \equiv (\mathbf{z}_{1it}', \mathbf{z}_{2it}')' \equiv (\mathbf{z}_{i1}', \mathbf{z}_{i2}', \dots, \mathbf{z}_{iT}')'$ is a vector of all strictly exogenous variables in all time periods. Plugging this into Equation 1.6 gives

$$a_{2it} = \mathbf{z}_{1it}'\boldsymbol{\alpha}_1 + \mathbf{z}_{2it}'\boldsymbol{\alpha}_2 + \bar{\mathbf{z}}_i'\boldsymbol{\psi} + \lambda_{2t} + \nu_{2it}, \tag{1.7}$$

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 Equation 1.7 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 $Var(u_{2it}) = \sigma_{u_2}^2$ implies that we can rewrite our model of interest as

$$c_{1it}^* = \mathbf{z}_{1it}'\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}_{1i,t-1}'\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + \omega u_{2it} + \epsilon_{1it}, \tag{1.8}$$

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 $b_{1i}|\mathbf{z}_i, \mathbf{h}_{i0}, \boldsymbol{\nu}_{2i} \sim \mathcal{N}(\mathbf{z}_i'\boldsymbol{\delta}_0 + \mathbf{h}_{i0}'\boldsymbol{\delta}_1 + \boldsymbol{\nu}_{2i}'\boldsymbol{\delta}_3, \sigma_d^2)$. 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 $d_{1i}|\mathbf{z}_i, \mathbf{h}_{i0}, \boldsymbol{\nu}_{2i} \sim \mathcal{N}(0, \sigma_d^2)$. Plugging this into Equation 1.8 gives the final equation

$$\begin{aligned} c_{1it}^* = \ &\mathbf{z}_{1it}'\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}_{1i,t-1}'\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \omega \nu_{2it} \\ &+ \lambda_{1t} + \mathbf{z}_i'\boldsymbol{\delta}_0 + \mathbf{h}_{i0}'\boldsymbol{\delta}_1 + \boldsymbol{\nu}_{2i}'\boldsymbol{\delta}_3 + d_{1i} + \epsilon_{1it}, \end{aligned} \tag{1.9}$$

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., $\boldsymbol{\beta}_1/\sqrt{(1 + \sigma_{d_1}^2)}$ 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 Equation 1.7, 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 Equation 1.9. 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.[7]

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.

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. Chapter 2 shows 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 under mild conditions, provided we include country- and time-fixed effects.

---

[7]We conserve degrees of freedom by splitting the two vectors, so that in the case of the exogenous variables we have $\mathbf{z}_i^+ = (\mathbf{z}_{i1}', \mathbf{z}_{i2}', \ldots, \mathbf{z}_{iR}', \bar{\mathbf{z}}_i^{+\prime})'$ 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 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).

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.[8] For the few donors with FPTP systems – Canada, the United Kingdom, and the United States – we use legislative fractionalization as our preferred source of exogenous variation.[9]

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 inherent in our approach and discuss the concern raised by Christian and Barrett (2017) on non-linear trends in the time series of the interacted instruments.

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:

$$a_{3ijt} = \theta_0 g_{3jt} + \theta_1(g_{3jt} \times \bar{p}_{3ij}) + \xi_0 l_{3jt} + \xi_1(l_{3jt} \times \bar{p}_{3ij}) + \mu_{3ij} + \lambda_{3t} + \varepsilon_{3ijt}, \qquad (1.10)$$

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

---

[8]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.

[9]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 US without a material impact on the results.

effects, μ_{3ij}. We then aggregate the predicted bilateral aid from Equation 1.10 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 Equation 1.7.

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.

1.5. Results

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.[10] These results are not intended to

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[10]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

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):

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

(1.11)

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 United States).[11]

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. Equation 1.11 predicts that this increases the aid to GDP ratio by about 0.01 percentage points 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 in any given year.[12]

Reduced form of aid

We now turn to country level estimates of the first stage relationship. Table 1.2 shows three reduced form regressions for aid to GDP which we obtain by estimating the equivalent fixed effects model of Equation 1.7. The residuals from these models are

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fixed effects regressions as long as selection is ignorable.

[11]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 United States) results in amendments to the budget. The parties negotiating these amendments are likely to have different preferences over which countries should receive aid.

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

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 1.2

First stage regressions with generated IV

	Dependent Variable: Aid to GDP		
	(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 Equation 1.11. Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Two things stand out in Table 1.2. 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.[13] 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 1.B report the regression coefficients and the confidence intervals we obtain when we drop each donor (Figure 1.2) or each recipient (Figure 1.3) 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 1.2. 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.[14]

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[13] 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 US districts with constructed values.

[14] Other factors, such as global economic crises, may both depress aid and lead to more fragmented governments in rich countries. However, if these factors uniformly affect all recipients in a given year,

Table 1.9 in Appendix 1.B includes United Nations General Assembly (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.

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 1.3 reports the regression results and Table 1.4 shows the associated average partial effects of aid on different transitions.

Consider the regressions in Table 1.3 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

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they are captured by the time effects.

TABLE 1.3

Second stage ordered probit regressions, CRE and CF

	Dependent Variable: Ordered Conflict		
	(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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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. One might argue that this inclusion introduces a bad control issue. However, given the similarity in our point coefficients between the different models, we do not think that this is a major problem. 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 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 1.3. 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 1.4

Average partial effect of aid on transition probabilities

| **From State** | **To 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 Conflict	-2.867**	1.439**	1.358**	0.070
	(1.359)	(0.701)	(0.646)	(0.048)
Armed Conflict	-1.379	-0.539	1.333	0.585
	(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 1.3. 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 1.4 reports estimates of APEs for a one percentage point change in aid on the various transition probabilities (see Equation 1.3 in Section 1.4). 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 BRD.

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.[15] 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).[16]

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.

Persistence, state dependence, and duration

Table 1.5 shows the average transition probabilities as they are predicted by our preferred specification.[17] 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 Equation 1.4 apart from the weights. Recall that state dependence measures the effect of the state on itself after accounting

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[15]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.

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

[17]Table 1.5 can be directly compared to the observed data shown in Table 1.1 and the difference between these two is a basic measure of goodness of fit.

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).[18] It is conceptually distinct from persistence which, in theory, could be entirely driven by observed and unobserved characteristics.

TABLE 1.5
Estimated transition probabilities and state dependence

From State	To 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 1.3. Panel bootstrap standard errors in parentheses, computed with 200 replications. The upper four rows sum to 100%. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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.

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).[19] The expected duration of peace is about five years.

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

[19]To see the equivalence, recall that the hazard rate is the probability that the current state will end,

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).[20]

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 receipts is constant over time. This is not a concern in our case, although it is an issue in related work.[21]

Figure 1.4 in Appendix 1.B show 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 is left unchanged. Note that we ignore the first stage variability.

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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.

[20]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.

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

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 1.5 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.

1.6. 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 1.B.

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 1.10 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 1.10 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 approaches.[22]

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[22]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.

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.

Definition of variables

We now turn to the sensitivity of our results with respect to the operationalization of our key variables. In Table 1.11 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[23] but fewer than 25 BRD 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 BRD. 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 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 United Kingdom, and the United States. 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

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[23]Source: START (2016).

they are important to the United States).[24] 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 1.12 shows, we obtain quantitatively identical results for all four perturbations.

Additional controls

In Table 1.13 we extend the set of control variables. Column 1 examines the 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.[25] 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 1.13. 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.[26]

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 with the part of our instrument that is solely driven by exogenous variation.[27] 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.

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

[25]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.

[26]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 1.3 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.

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

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 US food aid differs from the results of overall aid presented here. Table 1.14 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 US food aid is instrumented with US wheat production interacted with the probability of receiving US aid. However, we exchange their binary conflict indicator with our ordinal measure of conflict and include the appropriate interactions.[28] In line with their results, we find that US 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 US food aid. This should not be the case. Donor fractionalization of the 28 DAC donor countries should not predict US food aid. Likewise, wheat production in the United States should not predict total ODA disbursed by the 28 DAC donors, but only a small part of US overall aid. Table 1.15 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 US food aid highlighted previously.

1.7. Conclusion

This paper 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 paper 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 that a conflict escalates from a low level of political violence to armed conflict, we find little evidence

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[28]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 1.14).

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.

Appendices to Chapter 1

1.A. Sample

TABLE 1.6

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 1.7

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.

1.B. Additional regressions

TABLE 1.8
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: Recipient 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 1.9
Robustness: First stage

	Dependent Variable: Aid to GDP				
	(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 Equation 1.11. Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.10
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
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 1.11
Robustness: Alternate measures of conflict and foreign aid

	(1) w/ Terror	(2) only 25 BDs	(3) UCDP-PRIO	(4) w/ OOF	(5) No Anglo Saxon	(6) (5) w/ OOF
			Perturbations on LHS or RHS:			
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 (columns 5 and 6) excludes Canada, the United Kingdom, and the United States. 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 1.12

Robustness: Leave-one-out test for small conflict coding

	Dependent Variable: Ordered Conflict			
	(1)	(2)	(3)	(4)
	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 1.13
Robustness: Additional covariates

	(1)	(2)	(3)	(4)	(5)	(6)
			Dependent Variable: Ordered Conflict			
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 ($\hat{v}_{2it}$)	-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 1.14

Comparison: Our results vs. Nunn and Qian (2014)

	Dependent Variable: Ordered Conflict		
	(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 1.15

Falsification test

	Dependent Variable:	
	(1)	(2)
	US 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FIGURE 1.2
Leave-one-out test: Donors
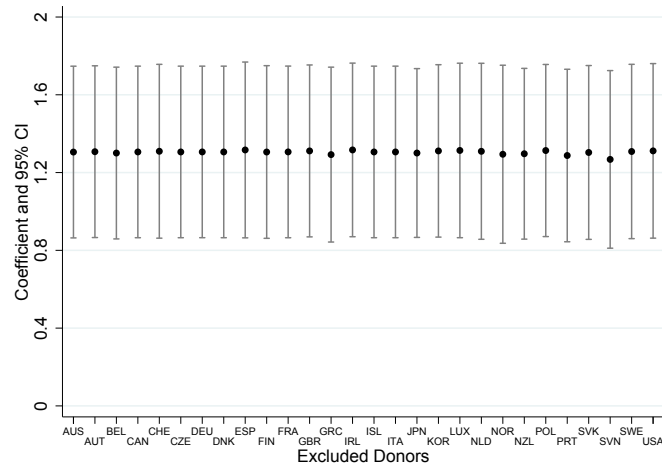
Notes: Each point in the figure represents the result of a regression of actual on predicted aid shares where one of the DAC donors has been excluded from the bilateral sample. Cluster robust standard errors are provided as error bars.

FIGURE 1.3
Leave-one-out test: Recipients
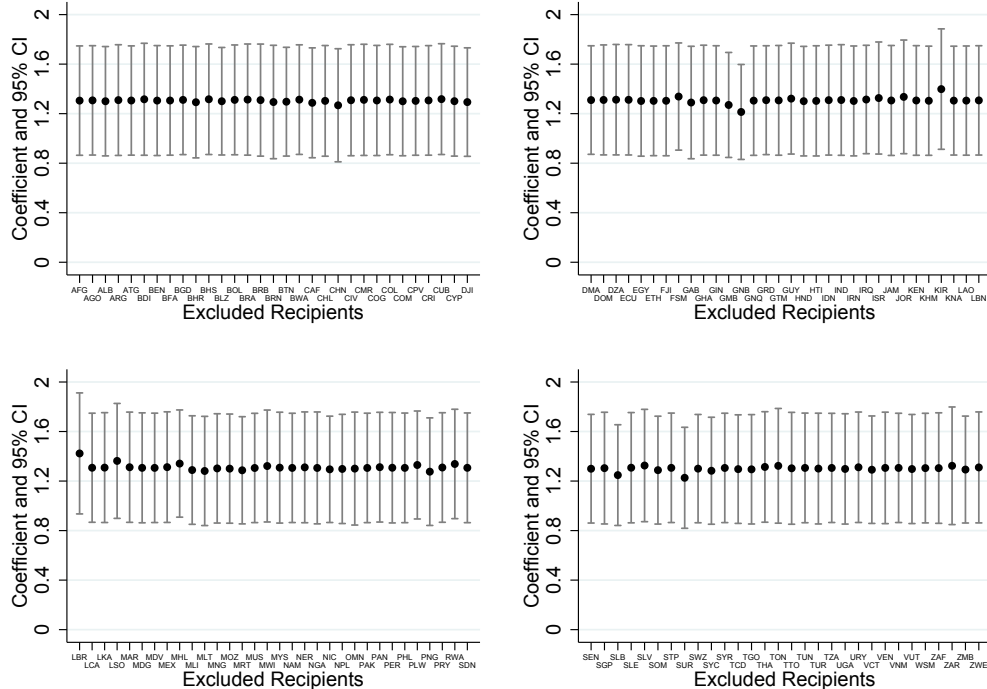
Notes: Each point in the figure represents the result of a regression of actual on predicted aid shares where one of the recipients has been excluded from the bilateral sample. Cluster robust standard errors are provided as error bars.

FIGURE 1.4
Parallel trends
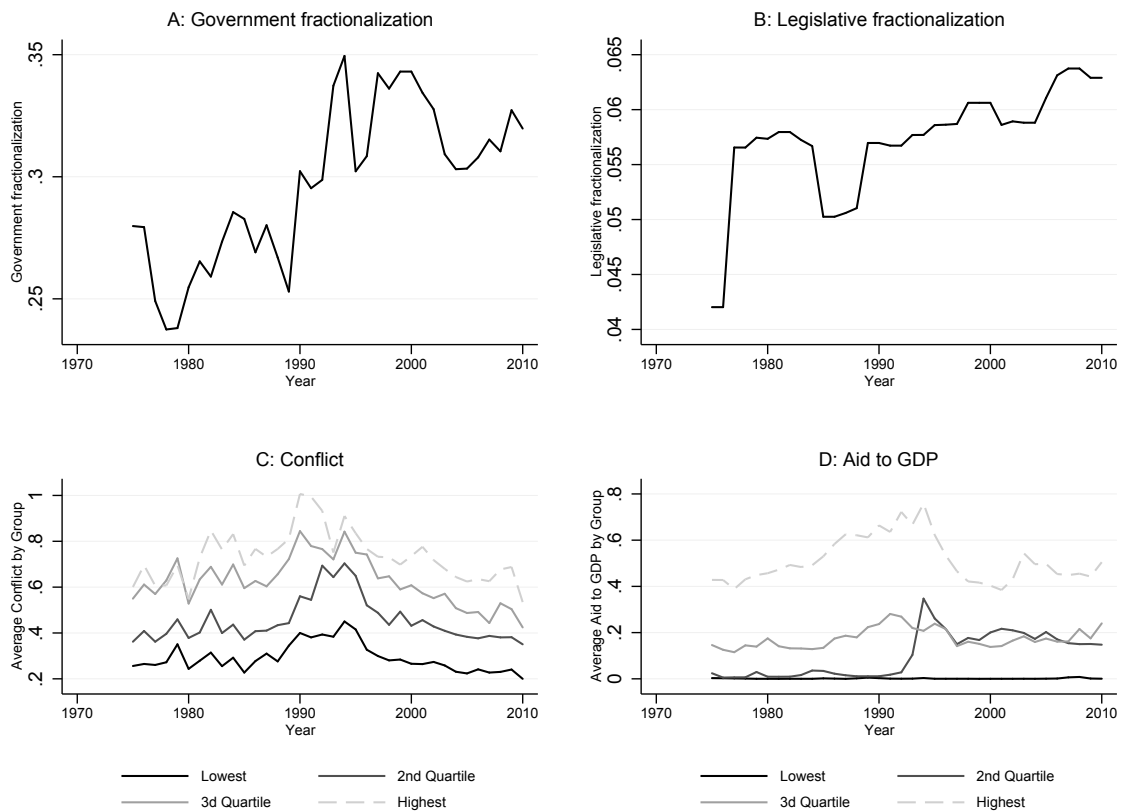
Notes: The figures show the time series of average government fractionalization of donors (panel A), average legislative fractionalization of donors (panel B), conflict in recipient countries grouped by their probability to receive aid (panel C), and average aid to GDP ratios in recipient countries grouped 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 1.5
Randomization test
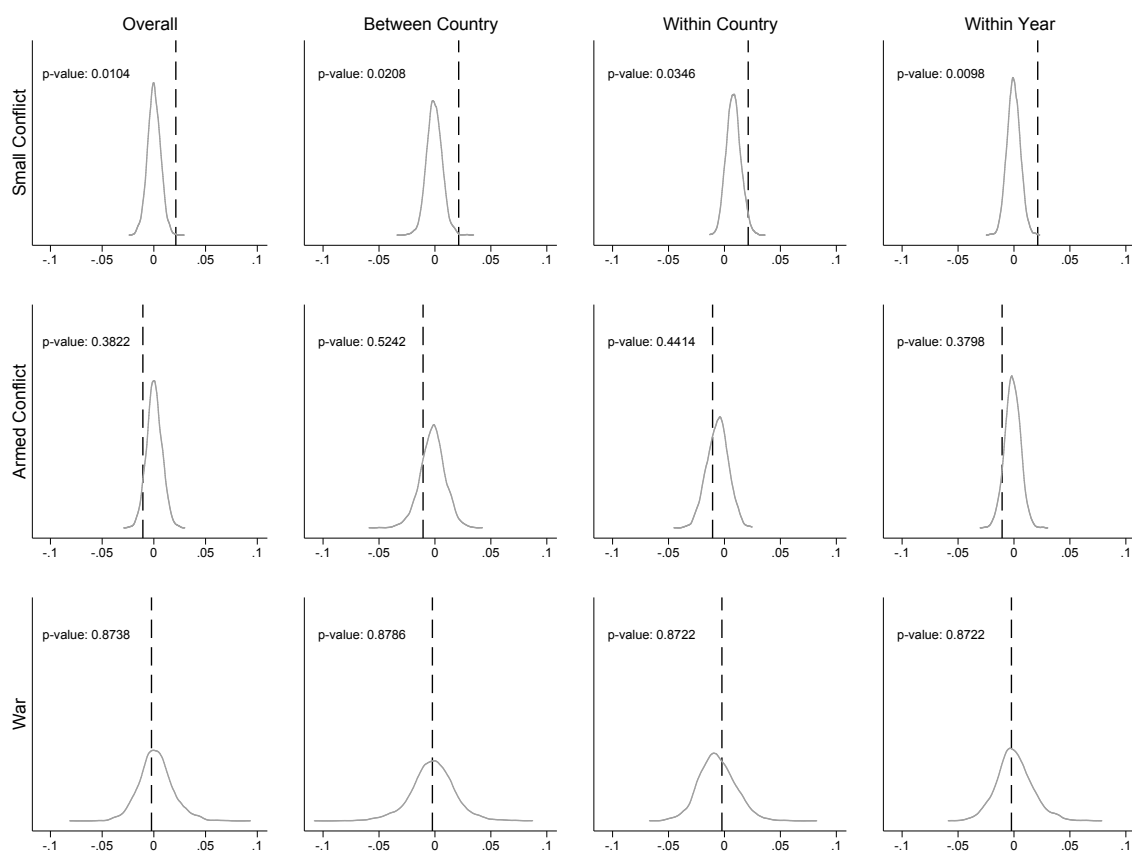
Notes: The figure shows the distribution of point coefficients based on 5,000 Monte Carlo replications per randomization strategy as described in the text. The *p*-values are estimated as the proportion of times that the absolute value of the *t*-statistics in replication data exceed the absolute value of the original *t*-statistic.

1.C.   Short case study: Sri Lanka

Figure 1.6 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 1.6
Conflict dynamics in Sri Lanka
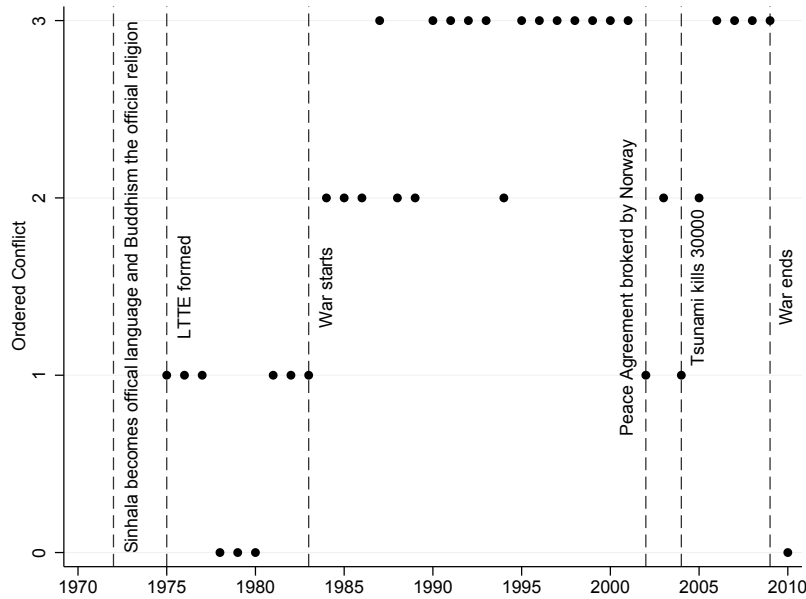
The political conflict between the Sinhalese (about 73.8% of the population) and the Tamils (about 18% of the population, concentrated in the northeast of the country), has been lingering in Sri Lanka since the independence from the British Empire in 1948. The conflict started escalating in 1970 when 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.[29] 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).[30]

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

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[29]See http://www.cfr.org/terrorist-organizations-and-networks/sri-lankan-conflict/p11407, accessed July 9, 2018.

[30]See http://www.aljazeera.com/focus/blanktemplate/2008/11/2008111061193133.html, accessed July 9, 2018.

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 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.[31] 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.[32] Yet, in both cases violence never ceased but failed to reach the threshold of 25 BRD. In 2002 there have still been several clashes between LTTE fighters and government soldiers, although both groups tried to adhere to the peace agreement.[33] In 2004 rioters burned down outlets of the government friendly splinter group who seceded from the LTTE (Banks and Wilson, 2015).[34]

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.

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[31]Source: http://www.nytimes.com/2009/05/19/world/asia/19lanka.html?_r=2&ref=global-home, accessed July 9, 2018.

[32]See http://www.cfr.org/terrorist-organizations-and-networks/sri-lankan-conflict/p11407, accessed July 9, 2018.

[33]Heidelberg Institute for International Conflict Research (HIIK) 2002: https://hiik.de/konfliktbarometer/bisherige-ausgaben/, accessed July 9, 2018.

[34]HIIK 2004: https://hiik.de/konfliktbarometer/bisherige-ausgaben/, accessed July 9, 2018.

Chapter 2

Aid and growth. New evidence using an excludable instrument

Joint work with Axel Dreher

Abstract

We use an excludable instrument to test the effect of bilateral foreign aid on economic growth in a sample of 96 recipient countries over the 1974-2009 period. We interact donor government fractionalization with a recipient country's probability of receiving aid. The results show that fractionalization increases donors' aid budgets, representing the over-time variation of our instrument, while the probability of receiving aid introduces variation across recipient countries. Controlling for country- and period-specific effects that capture the levels of the interacted variables, the interaction provides a powerful and excludable instrument. Making use of the instrument, our results show no significant effect of aid on growth in the overall sample. We also investigate the effect of aid on consumption, savings, and investments, and split the sample according to the quality of economic policy, democracy, and the Cold War period. With the exception of the post-Cold War period (where abundant aid reduces growth), we find no significant effect of aid on growth in any of these sub-samples. None of the other outcomes are affected by aid.

2.1.   Introduction

In a previous paper we began with an apology for adding yet another paper investigating the effect of foreign aid on economic growth to what is already a long list of articles (Dreher et al., 2016). We frankly admitted that we were unable to provide an unbiased estimate of aid's effect on growth – as is true for most of the preceding literature. Since then, a number of innovative contributions have added to our understanding of whether and to what extent aid causally affects growth and institutions. Jackson (2014) suggests using natural disasters in countries receiving aid from the same donor as an instrument. Galiani et al. (2017) instrument aid flows with the International Development Association's (IDA) threshold for receiving concessional aid.   While interesting and innovative, we remain unconvinced of these identification strategies. Jackson's suggestion of increased short-term aid for countries unaffected by disaster as a consequence of disasters in other aid recipient countries from the same donor, while empirically powerful, lacks a theoretical foundation, and is thus potentially spurious.[1]   Galiani et al.'s instrument could be correlated with growth for reasons other than aid, as countries' rates of growth might be influenced by factors other than aid at the time they exceed the IDA's income threshold.[2] The lack of a plausibly excludable instrument for aid in a large sample of donor and recipient countries continues to plague the aid effectiveness literature at large. The question of whether aid affects recipient countries' economic growth thus remains wide open.[3]

In this paper, we aim to fill this gap. We are inspired by the identification strategies of Werker et al. (2009), Nunn and Qian (2014), and Ahmed (2016). These studies rely on plausibly excludable variables that do not vary at the recipient country level and interact it with a proxy for the probability of receiving aid. We borrow from Ahmed (2016) who exploits variation in the composition of the United States' House of Representatives to instrument US aid in explaining recipient country democracy. To the extent that fractionalization leads to larger government budgets and larger overall budgets lead to an increase in the aid budget, fractionalization can serve as a powerful instrument. In line

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[1]On the significance of "false positives," see Chaudoin et al. (2016).

[2]This would hold even if the decision to pass the IDA's income threshold could not be manipulated by aid-receiving governments. Consider a reform-oriented government that achieves substantially higher growth rates for some years that eventually lead to passing the exogenous threshold. Growth dynamics will be different in these years compared to the years in which the country does not grow, even with an exogenous income threshold. What is more, governments can manipulate GDP data, which makes reaching the threshold potentially endogenous (see, Kerner et al., 2017, who show this for aid-dependent countries). Galiani et al. test for these possibilities. Using a smoothed income trajectory to rule out the effect of shocks they find results that are similar to their main analysis. They find no evidence of data manipulation. However, their sample only covers 35 countries. Dreher and Lohmann (2015) focus on regional growth within countries. Their instrument for aid is an interaction of the IDA income threshold with a region's probability to receive aid, in a sample of 21 countries.

[3]Among prominent recent attempts to investigate the effect of aid, Clemens et al. (2012) do not use instruments and Brückner (2013) relies on rainfall and commodity price shocks, which can easily violate the exclusion restriction. See Werker (2012) and Doucouliagos (2016) for recent surveys.

with Nunn and Qian (2014) and Ahmed (2016) we introduce variation at the recipient country level by interacting fractionalization with the share of years a country receives aid from its donors.[4] To the extent that variables correlated with donor fractionalization do not affect recipients' rates of growth differently in regular and irregular recipients of aid, controlled for country- and period fixed effects and a battery of control variables, the resulting instrument is excludable. Contrary to Nunn and Qian (2014) and Ahmed (2016), we focus on growth rather than democracy or conflict, and aid from a group of major donors rather than (food) aid from the United States exclusively. Other than Werker et al. (2009), we focus on a broad set of donor countries. As we outline in more detail in Section 2.2, we investigate the link between government fractionalization and the effectiveness of aid as a chain of cause-and-effect relationships. Starting with the effect of fractionalization on government budgets, we further illustrate the relation between overall budgets and aid budgets.

In addition to investigating the effect of aid on growth, this paper's contribution is the introduction of an instrument for aid from a large number of donors and years that can be used to address a substantial number of questions in the aid effectiveness literature, such as in Chapter 1 to investigate the effect of aid on conflict. Though still new, our instrument has already been used, for instance, in Ziaja (2017) in the context of democracy.[5] We suggest a number of additional research questions where we think our instrument helps overcoming the endogeneity of aid in the conclusion.

We describe our data and method in more detail in Section 2.3. To foreshadow our results (shown in Section 2.4), we find that the interaction of government fractionalization and a country's probability of receiving aid is a powerful instrument for aid. Using this instrument, we find no positive effect of aid on growth in the overall sample. Section 2.5 splits the sample in a number of important dimensions – the quality of economic policy, democracy, and the Cold War – and tests whether the impact of aid differs across these groups. With the exception of the post-Cold War period (where abundant aid reduces growth), we find no significant effect of aid on growth in any of these sub-samples. We also investigate the effect of aid on components of GDP rather than growth (in Section 2.6). Savings, investment, and consumption are all unaffected by aid. The final section summarizes and concludes the paper.

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[4]Werker et al. (2009) focus on aid from Arab donors and rely on a binary indicator identifying Muslim recipient countries, which are more likely to receive such aid compared to non-Muslim countries.

[5]Variants of it have been used to instrument International Monetary Fund loans, see Lang (2016) and Gehring and Lang (2018).

2.2.   The argument

Most of the previous literature pursues one of three strategies to identify the effect of aid on growth. One group of papers relies on instruments that relate to the size of the recipient country's population (as a proxy for the ease to exercise power, e.g., Rajan and Subramanian, 2008). A second group of papers focuses on bilateral political relations, for example employing voting coincidence in the UNGA to instrument for aid (Bjørnskov, 2014). The third uses internal instruments and estimates difference or system GMM regressions (Minoiu and Reddy, 2010). Each of these strategies is misguided. Population size can affect growth through many channels that researchers cannot control for and is thus not excludable (Bazzi and Clemens, 2013). Lagged levels and differences of aid are also hardly excludable to growth, invalidating them as (internal) instruments. Political-relations based variables might be excludable, but to the extent that the motive for granting aid affects the outcome, the resulting LATE reflects the effects of politically motivated aid rather than those of all aid (Dreher et al., 2016).

A couple of recent papers suggest alternative identification strategies, based on interactions between an excludable instrument and a potentially endogenous variable (Werker et al., 2009; Nunn and Qian, 2014; Ahmed, 2016). Of these, only Werker et al. (2009) investigate the question that we address in this paper – the effect of foreign aid on economic growth. Werker et al. make use of oil price fluctuations that substantially increase the aid budgets of oil-producing Arab donors, in particular to Muslim countries. Specifically, their instrument for Arab aid is the interaction of the oil price with a binary indicator for Muslim recipient countries, which receive the bulk of Arab donors' aid. They find recipient country growth to be unrelated to aid. While we are convinced of Werker et al.'s identification strategy, their results can hardly be generalized to represent the effects of aid more broadly. As they point out, their results show the LATE for oil-price-induced increases in aid to Muslim countries, which might be unrepresentative of aid from a broader set of donors to a broader set of recipients. In particular, the modalities of aid delivery as well as the political motivations of this aid might reduce its effectiveness, as might the specific set of policies and institutions in the largely authoritarian recipient countries of aid from Arab donors (Werker et al., 2009; Dreher et al., 2016). We rely on Werker et al.'s identification strategy, closely following Nunn and Qian (2014) and, in particular, Ahmed (2016), but focusing on aid's effect on growth for a large set of aid donors and recipients, over a long period of time.

We rely on two additional strands of previous literature to motivate our instrument for aid. The first investigates the effect of government fractionalization on governments' budgets. Roubini and Sachs (1989) propose that coalition governments will be more reluctant to reduce expenditures compared with single-party governments, as each party of the coalition will resist pressure to cut expenditure in its own area, even if they are

in favor of overall spending cuts. Volkerink and de Haan (2001) and Scartascini and Crain (2002) show that legislature fragmentation increases governments' expenditures. We make use of the relationship between fractionalization and government budgets, hypothesizing that the larger budgets arising due to fractionalization increase aid budgets, which in turn affect aid disbursements at the recipient country level. Most importantly, controlling for period-fixed effects, recipient-fixed effects, and other control variables, government fractionalization in donor countries is arguably excludable in growth regressions at the recipient country level.

The second well-established strand of literature we draw from addresses the relationship between overall government budgets and their aid budgets. Brech and Potrafke (2014) and Round and Odedokun (2004) show that overall expenditures as a share of GDP significantly determine aid budgets. Interestingly, in line with our hypothesis in this paper, Round and Odedokun's (2004) regressions excluding government expenditures show that government fractionalization increases aid budgets, "apparently to satisfy the various interests of the coalition" (p. 308).[6] Obviously, larger overall aid budgets increase aid disbursements to recipient countries, on average (e.g., Dreher and Fuchs, 2011).

We use fractionalization interacted with the probability of receiving aid as our instrument for bilateral aid, and argue that it is excludable to recipient country growth. As Nunn and Qian (2014, p. 1632, 1638) explain, this holds even though the probability of receiving aid itself is endogenous. As they point out, the resulting regressions resemble a difference-in-difference approach, where we compare the effect of aid on growth in regular and irregular recipients of aid as donor fractionalization changes. We explain our identification strategy in more detail in the next section, where we introduce our data and method of estimation.

One might consider two alternative instruments resulting from our hypothesized transmission channels: government expenditures and aid budgets. These instruments are, however, not necessarily excludable, given that growth shocks in recipient countries could directly affect donors' aid budgets (and thus their overall budgets), while growth shocks in non-recipient countries might not. For example, Rodella-Boitreaud and Wagner (2011) show that donors' total aid budgets increase with natural disasters in developing countries, indicating that donors adjust their total aid budget in response to shocks rather than merely reallocating aid while holding budgets constant. We therefore do not use government expenditures and aid budgets as instruments.[7]

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[6]Overall government budgets and government fractionalization do not turn out to be robust determinants of aid budgets in the large-scale robustness analysis in Fuchs et al. (2014). Their regressions however include various measures of fractionalization and fiscal policy at the same time, setting a high bar on the identification of the individual effects.

[7]Some previous papers rely on aid budgets as an instrument for aid. One example is Hodler and Raschky's (2014) analysis of how aid affects nightlight at the regional level. See, Temple and Van de Sijpe's (2017) analysis of how aid affects various components of GDP for a discussion on how endogeneity

2.3. Method and data

Our growth models follow the approach in Clemens et al. (2012). However, Clemens et al. do not use instruments, but claim to address the endogeneity of aid by differencing the regression equation and lagging aid, so that it can reasonably be expected to cause growth rather than being its effect. Their estimates could still be biased in either direction. For example, donors might grant more aid to an incoming reform-oriented government. Increased growth resulting from reforms could then spuriously be attributed to the increases in aid. On the other hand, donors might give more aid to countries where they anticipate that shocks will reduce future growth rates (Dreher et al., 2016). This is in line with Roodman (2015), who finds that Clemens et al. (2012) fail to remove contemporaneous endogeneity. This is why we see the need of using a new IV strategy.

We base our analysis on Clemens et al.'s permutations of Burnside and Dollar (2000), the study that has arguably gained the most attention in the literature on aid and growth.[8] In terms of timing, our preferred specifications follow Clemens et al. (2012) and assume that disbursed aid takes one four-year period to become effective in increasing or decreasing economic growth. In all tables we also report contemporaneous effects of aid on growth within the same four-year period. We estimate the regressions with country-fixed effects rather than in first differences.[9] Our preferred empirical model is at the recipient-period level:

$$Growth_{i,t} = \beta_1 Aid_{i,t-1} + \beta_2 Aid_{i,t-1}^2 + \boldsymbol{X}_{i,t}\beta_3 + \beta_4\eta_i + \beta_5\tau_t + \epsilon_{i,t'} \qquad (2.1)$$

where $Growth_{i,t}$ is recipient country i's average yearly real GDP per capita growth over a four-year period t.[10] $Aid_{i,t-1}$ denotes the amount of net aid (in % of GDP) disbursed by the 28 bilateral donors of the OECD's DAC in the previous period. Some

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can be alleviated by filtering out common factors that have heterogeneous effects on the variable of interest. In Chauvet and Ehrhart's 2015 analysis of aid's effect on firm growth in 29 developing countries they instrument for aid using fiscal revenue as a share of donors' GDP (interacted with joint religion or colonial history). When we use aid budgets instead of fractionalization (interacted with the probability of receiving aid) as an instrument our main results are unchanged. The Kleibergen-Paap first stage F-statistics are strong, as one might expect.

[8]We rely on Minasyan's (2016) update of these data until 2009. We replicated our main analyses with Clemens et al.'s 2012 permutations of Rajan and Subramanian (2008) instead. Our results are unchanged.

[9]Clemens et al. (2012) seem to prefer a measure of early-impact aid over all aid. This measure has been shown not to be a robust predictor of growth (Rajan and Subramanian, 2008; Bjørnskov, 2014; Roodman, 2015). What is more, a major drawback with this measure is that disaggregated aid disbursements are not available for the entire period, so that disbursements have to be estimated based on commitments. Data on commitments in the earlier periods also suffer from severe underreporting, which is not addressed in Clemens et al. (2012) (see OECD/DAC CRS Guide, Coverage Ratios, accessed on March 3, 2014: http://www.oecd.org/dac/stats/crsguide.htm). We therefore prefer to focus on overall aid.

[10]We include recipient countries that have been on at least one "DAC List of ODA Recipients" between 1997 and 2013. Appendix 2.B shows these countries. The results are unchanged when we instead estimate the aid-growth relationship in a dyadic setting.

specifications also include aid squared to test for decreasing returns to aid, following Clemens et al. (2012). η_i represent recipient-country-fixed effects, τ_t period-fixed effects, and $\epsilon_{i,t}$ the error term. Standard errors are bootstrapped based on pairwise recipient country clusters.[11]

All regressions include the set of contemporaneous control variables used in Burnside and Dollar (2000), which we denote as $\boldsymbol{X}_{i,t}$: Initial GDP/capita, Ethnic Fractionalization, Assassinations, Ethnic Fractionalization*Assassinations, dummies for Sub-Saharan Africa and East Asia, Institutional Quality, M2/GDP (lagged), and Policy.[12] Some words of caution are in order. The instrumental variables approach that we explain in more detail below does not rely on these control variables – our instrument does not violate the exclusion restriction in their absence. We thus face a trade-off between increasing the efficiency of the estimator and introducing bias via the potential endogeneity of the control variables and their correlation with predicted aid. While we include the control variables in the main analysis, note that our results are qualitatively unchanged when we exclude them.[13]

A skeptical reader might also be concerned about the Nickell bias arising from the inclusion of initial GDP per capita. When we exclude initial GDP per capita, our results remain robust. When we correct for the bias by applying the procedure developed by Bruno (2005a,b) for unbalanced dynamic panel models using the Anderson-Hsiao and Arellano-Bond estimators, our results are equally unchanged, irrespective of whether or not we include the remaining covariates.

We estimate a zero stage regression at the donor-recipient-period level as follows:

$$Aid_{i,j,t} = \gamma_1 FRAC_{j,t} * p_{i,j} + \epsilon_{i,j,t} \tag{2.2}$$

$Aid_{i,j,t}$ denotes the amount of aid (in % of GDP) from donor j disbursed to recipient i in period t. We predict bilateral aid with the interaction of donor fractionalization

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[11]However, even though we are using a constructed instrument, IV standard errors are consistently estimated as long as the second stage error term is not correlated with our donor-recipient-specific instrument ($FRAC_{j,t} * p_{i,j}$) from the zero stage regression (Wooldridge, 2010). In line with Atkinson and Cornwell (2014) we also employ wild bootstrap at the second stage to test robustness (using cgmwildboot, Cameron et al., 2008). Standard errors are based on the bootstrapped p-values as these rather than standard errors are pivotal. Our results do not change when using alternative bootstrap approaches or when not bootstrapping standard errors.

[12]To reduce clutter, we do not show them in the main tables. Note that the time-invariant variables are removed here (as in Clemens et al., 2012) through the inclusion of country-fixed effects. Also note that we do not control for Burnside and Dollar's measure of good policy, given that improvements in policy might be an important transmission channel by which aid affects growth. We lose about 200 observations when we include the good policy indicator. Our results, however, do not depend on its exclusion. While the first stage F-statistics are somewhat lower in the reduced sample, the coefficients of interest are within the respective Anderson-Rubin 90%-confidence intervals. We also estimate regressions including an imputed good policy indicator to avoid losing observations. Our results are again unchanged. Appendix 2.A reports the sources and definitions of all variables, while we show descriptive statistics in Appendix 2.C. Appendix 2.E reports the full specifications for the main regressions.

[13]See Table 2.10 in the Appendix 2.D.

$FRAC_{j,t}$ and the probability of receiving aid $p_{i,j}$, which varies across donor-recipient pairs and periods.[14] Standard errors in Equation 2.2 are clustered at the donor-recipient country level. One might be concerned about potential direct effects of the probability of receiving aid on economic growth. However, our growth regressions control for the effect of the probability of receiving aid as well as the level of donor fractionalization through the inclusion of recipient-country- and period-fixed effects. Given that the effect of the potentially endogenous variable is controlled for, the interaction of the endogenous variable with an exogenous one can be interpreted as being exogenous (Nunn and Qian, 2014; Nizalova and Murtazashvili, 2016; Bun and Harrison, 2018).

As an alternative approach to construct our instrument, we include the levels of the interaction term as well as time- and country-fixed effects in Equation 2.2 and predict aid relying on γ_1. Taking the coefficient from the interaction term (γ_1) ensures that we construct our instrument from using exogenous variation only. Our first- and second-stage results remain the same to the extent that we control for the same factors in the first- and second-stage regressions. One might be concerned that the two approaches differ if donor fractionalization depended on donor-recipient pair characteristics. While we consider this unlikely, to ensure that our results do not depend on this modeling choice, we add the levels of the interaction term and donor-recipient-fixed effects to Equation 2.2 in a robustness test.[15]

We also compare the different modelling choices of the zero stage in case of one endogenous variable in a simulation analysis. What is more, we compare the findings to the approach when predicting aid relying on all coefficients of the zero stage regression including the levels of the interacted instrumental variable, country-pair- and time-fixed effects. In balanced samples, we find these different methods to lead to the same second stage results. Note that after aggregating over all donors, the donor-recipient-specific probability is then captured by recipient-country-fixed effects (when proceeding as in Equation 2.2). When instead controlling for time-fixed effects in the zero stage, the probability is captured by the time-fixed effects at this level. The donor-specific time-varying measure of government fractionalization is the same across recipients and is consequently captured in the time-fixed effects after we have aggregated the data over all donors. The only variation that remains at the first- and second-stage level

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[14]Instead of exploiting the contemporaneous variation of our instrumental variable, we could as well lag fractionalization (and its interaction) to allow for sufficient time between aid commitments and their disbursement. We do, however, prefer to focus on contemporaneous values, in line with the previous literature. When we lag fractionalization by one four-year period, our second stage results are unchanged. The instrument's power in the first stage is slightly below ten for contemporaneous aid in the linear specification and above ten for the other three specifications. As a falsification test, we also used fractionalization one period in the future interacted with the probability of receiving aid. Reassuringly, the first stage F-statistic is below one, indicating the lack of power of future fractionalization.

[15]We compare the zero stage results and corresponding second stage results when excluding (column 1) or including the levels of the interaction term and donor-recipient-fixed effects (column 2) in Table 2.11 in Appendix 2.D. The second stage results are unchanged.

is the exogenous variation introduced by the interaction term. This holds irrespective of the three different modelling choices: a) including only the interacted instrument as in Equation 2.2; b) predicting aid relying on γ_1 from a zero stage regression which also includes the levels of the interacted instruments and fixed effects; and c) the same regression as in b) but predicting aid from all coefficients.[16]

One might also be concerned about the fact that we do not control for the second stage covariates in the dyadic Equation 2.2. The dyadic zero stage equation constructs an instrument from exogenous variation, which we then use in the usual 2SLS procedure at the recipient level. After aggregating over all donors, we use the constructed instrument and control for the second stage covariates in the first stage regression. Thus, the remaining variation is the exogenous variation introduced by our constructed instrument conditional on all second stage covariates.

The intuition of our approach is that of a DiD approach, where we investigate a differential effect of donor fractionalization on the amount of aid to countries with a high compared to a low probability of receiving aid. The identifying assumption is that growth in countries with differing probabilities of receiving aid will not be affected differently by changes in fractionalization, other than via the impact of aid, controlling for recipient-country- and period-fixed affects and the other variables in the model. In other words, as in any DiD setting, we rely on an exogenous treatment and the absence of different pre-trends across groups. Controlled for period-fixed effects, donor-government fractionalization cannot be correlated with the error term and is thus clearly exogenous to aid. In order for different pre-trends to exist, these trends across countries with a high compared to a low probability to receive aid would have to vary in tandem with period-to-period changes in donor fractionalization. Given that donor fractionalization follows no obvious trend in our data, we consider this implausible. Following Christian and Barrett (2017) we plot the variation in government fractionalization in tandem with the variation in aid and growth for two different groups that are defined according to the mean of the probability to receive aid. Figure 2.4 in Appendix 2.F plots these graphs. They give no reason to believe that the parallel trend assumption is violated in our case.[17]

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[16]With the single dyadic instrument that we have here, there would be a further alternative, which does not require the zero stage or gravity-like approach. Starting at the donor-recipient-period level, we could aggregate the interaction between fractionalization and the probability to receive aid over all donors and take this as an instrument at the recipient-period level for aid (see Equation 2.1). The equivalence between the zero stage approach and this alternative is given for a single dyadic instrumental variable (when including more dyadic instrumental variables, the zero stage accounts for a weighting of these separate instruments). As we control for the levels of the interaction term through the inclusion of fixed effects at the recipient-period level, we are left with one dyadic instrument here. Indeed this approach leads to identical results compared to the zero stage approach.

[17]More precisely, the probability-specific trends in aid and growth, respectively, seem rather parallel across the regular recipients (those with a probability to receive aid that is above the mean) and the irregular recipients (with the probability to receive aid being below the mean). There is also no obvious non-linear trend in regular compared to irregular recipients that is similar for aid and growth. What is more, these trends do not overlap with the trend in government fractionalization. In analogy to Christian

In order to ensure that our result is not driven by omitted variables that affect regular and irregular recipients of aid differently, we also control for recipient country characteristics such as economic freedom and trade (in % of GDP), both as a level and interacted with the probability of receiving aid, respectively. The effect of aid on growth is unaffected and F-statistics remain around the threshold of ten. Moreover, the dyadic instrument remains strong at the zero stage regression when controlling for a number of donor and recipient country characteristics as economic freedom, ideology, overall trade, bilateral imports and exports and donor GDP per capita growth.[18]

We aggregate Equation 2.2 across donors for each recipient and period, resulting in the fitted value of aid as a share of GDP at the recipient-period level (in analogy with Rajan and Subramanian, 2008, for example):

$$\hat{Aid}_{i,t} = \sum_j \left[\hat{\gamma_1} FRAC_{j,t} * p_{i,j} + \epsilon_{i,j,t} \right] \tag{2.3}$$

We then instrument $Aid_{i,t-1}$ in Equation 2.1 with our constructed instrument $\hat{Aid}_{i,t-1}$ from Equation 2.3 at the recipient-period level.[19] We instrument $Aid^2_{i,t-1}$ with the square of predicted aid to GDP from the first stage, following Wooldridge (2010, p. 268). Our results are robust when we instead use the square of fitted aid to GDP (from Equation 2.3) as an instrument for aid squared (from Equation 2.1).

A priori, it is unclear whether legislature or government fractionalization is more suitable as an instrument. As Ahmed (2016) points out for the United States, the "funding and allocation of bilateral economic aid involves both the executive branch and Congress" and the same is true for the other donor countries in our sample. As it is the government that drafts the budget plan and not the legislature, we measure donor fractionalization as the probability that two randomly-chosen deputies from among the parties forming the government represent different parties (Beck et al., 2001). This would come at the disadvantage that there is no variation in government fractionalization for the United States and Canada across our period of observation. We therefore replace government fractionalization with legislature fractionalization for these countries.[20] Our results are

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and Barrett (2017), our identification strategy would be at risk in the presence of a non-linear trend in government fractionalization that is similar to the trends in aid and growth for the group of regular recipients. A common trend in all three variables, that is not different for regular and irregular recipients would, to the contrary, be captured by our time-fixed effects.

[18]The detailed results are available on request.

[19]This follows Rajan and Subramanian (2008) and – in the context of trade rather than aid – Frankel and Romer (1999). Our results are unchanged when we include donor-recipient-pair and period-fixed effects in the zero stage regression (with first stage F-statistics becoming stronger). They are also unchanged when we instead replace $Aid_{i,t-1}$ in Equation 2.1 with $\hat{Aid}_{i,t-1}$ predicted from a first stage regression that includes donor-recipient pair and period fixed effects as well as the control variables from the second stage.

[20]Unsurprisingly, government fractionalization in Canada and the United States is constant. While most DAC donor countries have parliamentary systems with proportional representation, there are exceptions (e.g., plurality voting system in Canada and presidential elections in the United States).

unchanged when we (i) do not replace these values, (ii) omit the two countries, and (iii) use legislature instead of government fractionalization for all countries.

We proxy a country's probability of receiving aid with the percentage of years the country received aid from a particular donor over the sample period, following Ahmed (2016) and Nunn and Qian (2014). Specifically, the probability of receiving aid from a particular donor j is $\bar{p_{i,j}} = \frac{1}{36} \sum_{y=1}^{36} p_{i,j,y}$, with $p_{i,j,y}$ indicating whether recipient i received positive amounts of aid from donor j in year y. To test robustness, we alternatively included the probability to receive aid over each four-year period (and its interaction with fractionalization) rather than those over the whole sample period.[21]

FIGURE 2.1

Probability to receive aid and average aid, 1974-2009
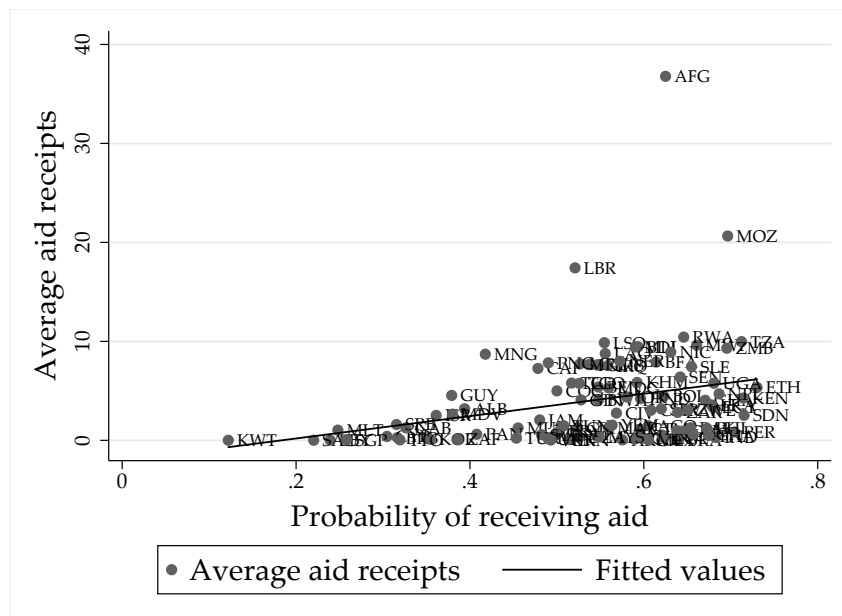
We argue that the extent to which changes in aid budgets affect aid receipts depends on a country's probability of receiving aid. Both Nunn and Qian (2014) and Ahmed (2016) show that the probability of receiving aid is indeed significantly correlated with the amount of US (food) aid a country receives. The same holds for our sample, for a broad set of donors, as can be seen in Figure 2.1. The Figure plots the average probability of receiving aid (i.e., recipient i's probability of receiving aid from any donor over the whole sample period) on the horizontal axis and the average aid received from all donors as a percentage of GDP on the vertical axis. The correlation between the two is 0.31, significant at the 1% level. For example, the figure shows that Afghanistan received aid in 63% of the years in the 1974-2009 period, amounting to about 37% of its GDP. On

The United Kingdom and France also differ from the remaining donors as they lack proportional representation. However, in both countries government fractionalization varies. In a robustness test, we also replace government fractionalization with legislature fractionalization for the United Kingdom and France. Results at the different stages remain unchanged.

[21]Our results do not depend on this choice.

the lower end of the scale, Kuwait received 0.0085% of its GDP as aid, and received aid in 12% of the years in the sample.

To establish the link between fractionalization and aid disbursements in our sample, we proceed with re-estimating specifications from the previous literature, illustrating this link with our data, at the donor-recipient-period level.[22]

Table 2.12 in Appendix 2.D closely follows the regressions in Scartascini and Crain (2002), and Roubini and Sachs (1989), respectively, but includes our measure of fractionalization rather than theirs. The dependent variable is annual central government expenditure as a share of GDP for the 28 donor countries in our sample over the 1974-2009 period, focusing on four-year averages, as in our main regressions. As can be seen, government expenditures increase significantly with fractionalization, at the 1% level of significance. The estimated effect of an increase in fractionalization from zero to one is in the range of a 0.85–2.8 percentage point increase in central government expenditures (with a sample average of 32.30%).

Figure 2.2 shows the partial leverage plot for fractionalization corresponding to the regression of column 1 in Table 2.12. The figure shows that the results are not driven by obvious outlying observations.[23]

We next turn to the effect of government budgets on aid budgets. Table 2.13 shows how an increase in central government expenditures translates into larger aid budgets, broadly following the regressions of Fuchs et al. (2014). The results show that an increase in central government expenditures by one percentage point increases governments' aid budgets by between 0.002 and 0.006 percentage points, at the 1% level of significance. For the average country in our sample this amounts to a maximum increase of 1.5% of its government's aid budget. Put differently, a one standard deviation increase in expenditures translate into a 0.06 percentage point increase in the aid budget to GDP ratio, which represents 24% of its standard deviation.

Figure 2.3 shows the partial leverage plot between government expenditures and aid budgets, based on column 1 of Table 2.13. The figure suggests that an outlying observation (representing Italy over the 1974–1977 period) potentially affects the result. When we remove this observation our results are however unchanged, suggesting a high positive correlation between central government expenditures and aid budgets. Arguably, larger aid budgets will translate into larger aid disbursements at the individual country level, on average.

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[22]We focus on the donor-recipient-period as this is the framework we use to predict aid (see equations 2.2 and 2.3).

[23]When we restrict the sample to those observations that we can use in the growth regressions below, results in Table 2.12 stay robust. The same holds for those in Table 2.13 below.

FIGURE 2.2

Fractionalization and central government expenditures, 1974-2009

coef = 2.7874889, (robust) se = .25391181, t = 10.98
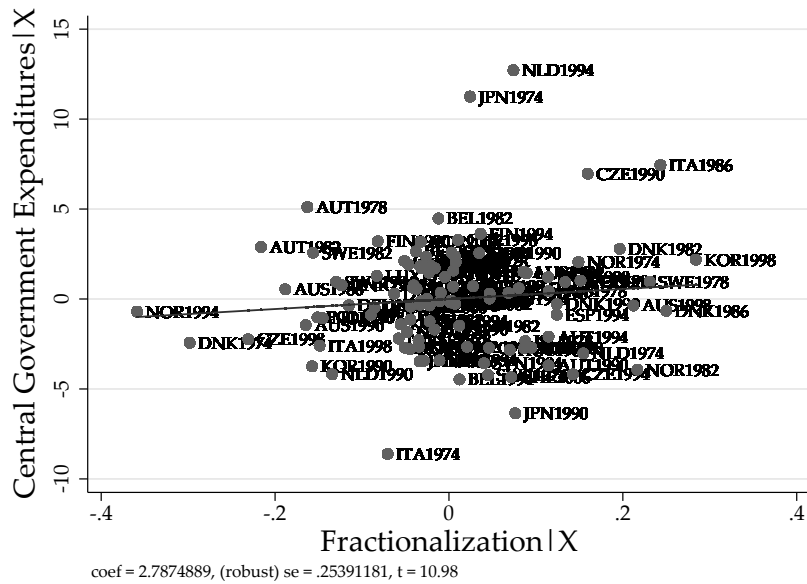
Notes: Based on Table 2.12, column 1.

FIGURE 2.3

Central government expenditures and aid budgets, 1970-2009 period

coef = .00226887, (robust) se = .00012657, t = 17.93
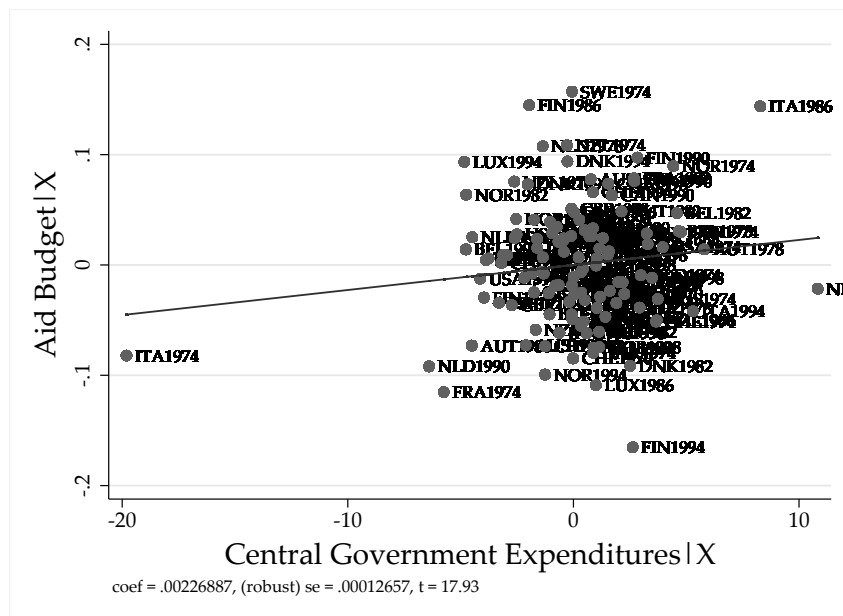
Notes: Based on Table 2.13, column 1.

2.4. Main results

Table 2.1 shows the results for our main specifications, estimated with OLS for comparison. As can be seen, GDP per capita growth is not significantly correlated with contemporaneous aid (column 1).[24] There is no evidence of a non-linear relationship, as indicated by the insignificant squared term in column 2. In line with Clemens et al. (2012), the impact of aid on growth turns stronger when aid is lagged, as can be seen in columns 3 (without aid squared) and 4 (including aid squared). The coefficient for lagged aid is more than twice the estimate in the comparable regressions in Clemens et al. (2012).[25] The regression shows that an increase in lagged aid by one percentage point of GDP is accompanied by higher growth of a magnitude of 0.25 percentage points in the linear (column 3) and 0.30 in the non-linear regression for the average country (column 4).[26] Note that the squared term in column 4 is again not significant at conventional levels, indicating no evidence that the effect of aid on growth is decreasing in aid. Arguably, these estimates are not causal, as omitted variables could easily explain the correlations.

TABLE 2.1
Aid and growth, 1974-2009, OLS

	(1)	(2)	(3)	(4)
Aid/GDP	0.049	-0.058	0.250**	0.311***
	(0.059)	(0.123)	(0.120)	(0.096)
Aid/GDP squared		0.003		-0.002
		(0.004)		(0.005)
Log Initial GDP/capita	-2.949***	-3.049***	-3.330***	-3.287***
	(0.619)	(0.696)	(0.588)	(0.631)
Assassinations	-0.013	-0.005	-0.221	-0.220
	(0.187)	(0.185)	(0.189)	(0.188)
Ethnic*Assassinations	-0.688	-0.700	0.015	0.010
	(0.809)	(0.802)	(0.556)	(0.554)
M2/GDP (t-1)	-0.009	-0.008	-0.002	-0.002
	(0.006)	(0.006)	(0.008)	(0.007)
Aid lagged	No	No	Yes	Yes
Number of observations	739	739	636	636
Adjusted R-Squared	0.151	0.153	0.197	0.196

Notes: Data are averaged over four years at the recipient-period level. Recipient- and period-fixed effects are included. Standard errors are in parentheses (clustered at the recipient country level; significance levels: * 0.10, ** 0.05, *** 0.01). Models are based on Burnside and Dollar (2000).

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[24]Note that to facilitate comparison we restrict the sample to those observations that are also included in the 2SLS regressions below.

[25]Specifically, their estimated coefficient is 0.096 (in column 4 of their Table 7), which is however not significant at conventional levels.

[26]The coefficient for the linear aid term is 0.361 and for aid squared -0.008 in the comparable regression in Clemens et al. (2012), both significant at the 5% level (in column 7 of their Table 7).

Before discussing the IV results presented in Table 2.2, it is important to note that the interaction of donor fractionalization and the probability of receiving aid is statistically significant at the 1% level in the zero stage regression (Equation 2.2, Table 2.11 in Appendix 2.D). The corresponding F-statistic of the interaction term is 104.62. Obviously, when taking the alternative approach to Equation 2.2 by including donor-recipient pair fixed effects the respective F-statistic drops to a lower value, 20.57, which is still clearly above the threshold of ten. The coefficient of the dyadic instrument in Equation 2.2 amounts to 0.363 with a standard deviation of 0.035. An increase in fractionalization from zero to one thus increases bilateral aid to recipient countries that receive aid in all years by 0.363 percentage points of GDP. The dyadic instrument provides the exogenous variation that we use to calculate the exogenous part of bilateral aid (in % of GDP). After aggregating over all donors, we use the sum of fitted bilateral aid (fitted aid to GDP, over all 28 DAC donors) in order to measure its causal effect on growth at the recipient-period level.

Table 2.2 shows the results at the recipient-period level using fitted aid to GDP as an instrument for actual aid. The control variables from Table 2.1 are included in all first- and second-stage regressions, but we exclude them from the table to reduce clutter.[27] Column 1 focuses on contemporaneous aid, instrumented with $\hat{Aid}_{i,t}$, in analogy to Equation 2.3. The table also shows the corresponding first stage results. As can be seen in the table, the Cragg-Donald and Kleibergen-Paap first-stage F-statistics are above Staiger and Stock (1997) rule-of-thumb threshold of ten.[28] The underidentification test (Kleibergen-Paap LM statistic) clearly rejects the Null hypothesis that the equation is underidentified.

Column 2 includes aid squared, which we instrument with the square of predicted aid to GDP of the first-stage. The test statistics given in column 2 of Table 2.2 refer to this instrument; statistics for aid itself are equivalent to those shown in column 1. The results show strong first-stage F-statistics; underidentification is again easily rejected.

Columns 3 and 4 show results for our preferred specifications, replacing contemporaneous values of aid with their lagged values (Equation 2.1). The statistics indicate that for the linear and squared term the instrument for aid is strong. The results show no significant effect of aid or aid squared on growth. There is no evidence that aid causally affects growth.[29] The significant correlations shown in Table 2.1 and

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[27]Appendix 2.E shows the full results.

[28]Stock and Yogo (2005) propose more specific sets of critical values for weak identification tests based on the number of endogenous regressors, the number of instruments and the acceptable maximum bias of the 2SLS relative to OLS regression or the maximum Wald test size distortion. For example, a 20% 2SLS size distortion of a 5% Wald test is associated with a critical value of 6.66 and a lower value of 4.42 for a 20% LIML (limited information maximum likelihood) size distortion.

[29]We also used logged aid/GDP rather than the level of aid along with its square, which allows for a decreasing marginal effect of aid even though it does not allow its effect to change sign. Our results are unchanged.

Aid and growth. New evidence using an excludable instrument

TABLE 2.2
Aid and growth, 1974-2009, IV

	(1)	(2)	(3)	(4)
	Panel A: Second stage			
Aid/GDP	-0.298	-0.254	-0.087	0.002
	(0.430)	(0.336)	(0.379)	(0.342)
Aid/GDP squared		-0.011		-0.018
		(0.011)		(0.014)
Aid lagged	No	No	Yes	Yes
Level of Govfrac	Yes	Yes	Yes	Yes
Level of Probability	Yes	Yes	Yes	Yes
Number of observations	739	739	636	636
Kleibergen-Paap F stat.	15.881	9.586	20.393	28.113
Kleibergen-Paap LM stat.	0.000	0.000	0.000	0.008
	Panel B: First stage			
Fitted Aid/GDP	5.208***	5.208***	5.483***	5.483***
	(1.308)	(1.308)	(1.215)	(1.215)
Squared predicted Aid/GDP		1.195***		1.126***
		(0.386)		(0.212)
	Panel C: Zero stage			
Fractionalization*Probability		0.363***		
		(0.035)		

Notes: Data are averaged over four years at the recipient-period level. Recipient- and period-fixed effects are included. First- and second-stage include as control variables: Log initial GDP/capita, Assassinations, Ethnic*Assassinations, and M2/GDP (lagged). Pairs cluster bootstrap standard errors with 500 replications are in parentheses in the second stage regressions (clustered at the recipient country level). Standard errors are in parentheses in the first stage regressions (clustered at the recipient country level). Models are based on Burnside and Dollar (2000). The first stage statistics reported in columns 2 and 4 refer to the squared aid term. The statistics for the linear term in columns 2 and 4 are identical to columns 1 and 3, respectively. Standard errors are in parentheses in the zero stage regression (clustered at the donor-recipient level). Significance levels: * 0.10, ** 0.05, *** 0.01.

in Clemens et al. (2012) are thus likely to be spurious. Potentially, donors anticipate growth-promoting policies – due to more reform-oriented politicians assuming power, for example – and increase their aid to such countries.

We conclude that there is no evidence that aid increases growth and offer a number of explanations. First, aid or growth might not be measured precisely enough to capture the effects of aid in a rather small sample of less than 800 observations. Second, even if aid would be measured precisely, the small number of observations implies that our tests are underpowered. In order for our tests to show an effect of aid if it was actually there with an 80% probability we would require more than 6000 observations rather than the sample of roughly 800 that we have.[30] This is an unfortunate feature that we share with

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[30]This high number of required observations is driven by our fixed effects setting, as both country-

the aid effectiveness literature at large (Ioannidis et al., 2017).[31] Third, the effects of aid might be spread over different horizons, and our four-year averages might be inadequate to capture these effects.[32] Fourth, aid might be effective in some groups of countries but not in others, and our pooled sample could hide such effects. We turn to this in the next section. Finally, of course, aid might simply not increase growth.

2.5. Heterogeneous effects of aid

Our instrumental variable regressions estimate the effect of variation in bilateral aid flows that go disproportionately to regular and irregular recipients of aid as a result of differences in government fractionalization. We have no reason to believe that the LATE cannot be generalized to be representative of bilateral aid more broadly. However, the previous literature suggests that the effects of aid vary across a recipient country's policies and institutions. Most importantly, it has been suggested that aid is effective in countries with good economic policies (Burnside and Dollar, 2000), in democracies (Svensson, 1999), or after the end of the Cold War (Headey, 2008), but not otherwise. All of these interactions have been shown to be fragile (e.g., Doucouliagos and Paldam, 2009), but none of these earlier studies investigates causal relationships. Rather than introducing interaction effects, we split the sample according to the median of Burnside and Dollar's (2000) good policy index (based on inflation, the budget balance, and openness to trade), Cheibub et al. (2010)'s binary indicator of democracy, and the years before 1991 and after 1990, respectively.

Table 2.3 shows the results. As can be seen, aid has no significant linear effect on growth in any of the samples. With one exception, the results also show that there is no significant non-linear effect of aid on growth. The exception is the regression in column 6 where we split along the Cold War dimension. Aid squared is significant (at the 5% level) after the end of the Cold War. However, the coefficient is negative with a level effect that is also negative, indicating that if aid had any effect at all it would reduce growth.

Overall, our results show no positive effects of aid on growth in any of the sub-samples and a negative effect of abundant aid on growth after the Cold War period.

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and time-fixed effects in tandem with the set of covariates capture most of the variation in the dependent variable so that the variation caused by aid conditional on these variables is rather small.

[31] According to Ioannidis et al. (2017), only about 1% of the 1779 estimates in the aid and growth literature surveyed have adequate power (see also, Doucouliagos, 2016).

[32] A detailed analysis of longer lags is beyond the scope of this paper. When we include further lags of our aid variables, the second lag stays insignificant (8 years), but there is some evidence that growth might increase with even longer lags (from 12 years on) in line with Dreher et al. (2018). The number of observations in these regressions is however comparably low, and we did not investigate the robustness of these results.

TABLE 2.3

Aid and growth, 1974-2009, IV, different samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Policy		Democracy		Cold War	
	bad	good	no	yes	<1991	>1990
Panel A: Linear Effect						
Aid/GDP (t)	0.734	-0.747	-0.228	0.579	-0.604	-1.220
Number of observations	252	487	421	264	304	435
Aid/GDP (t-1)	0.521	-0.228	-0.408	2.005	-0.296	0.023
Number of observations	198	438	341	246	224	412
Panel B: Nonlinear Effect						
Aid/GDP (t)	0.747	-0.726**	-0.221	0.584	-1.323	-1.018
Aid/GDP squared (t)	0.001	-0.003	-0.001	-0.005	0.080	-0.026**
Number of observations	252	487	421	264	304	435
Aid/GDP (t-1)	0.642	-0.226	-0.391	1.991**	-0.863	0.163
Aid/GDP squared (t-1)	-0.038	-0.000	-0.005	-0.023	0.079	-0.009
Number of observations	198	438	341	246	224	412

Notes: Data are averaged over four years at the recipient-period level. Recipient- and period-fixed effects are included. The first- and second-stages include as control variables: Log initial GDP/capita, Assassinations, Ethnic*Assassinations, and M2/GDP (lagged). The bad/good policy sample includes countries below/above the median according to the Burnside-Dollar good policy index. Democracy is measured with the binary indicator of Cheibub et al. (2010). Pairs cluster bootstrap standard errors with 500 replications are used (clustered at the recipient country level; significance levels: * 0.10, ** 0.05, *** 0.01). Models are based on Burnside and Dollar (2000).

2.6. Where does the aid go?

In the final substantive section of the paper we investigate the effects of aid on components of GDP, with the aim of testing where aid is spent. The insignificant effect of aid on GDP per capita growth could be the result of aid being spent on consumption rather than investment. Alternatively, aid could increase investment, but investments might be ineffective in increasing economic growth. The policy implications of these results would be substantially different.[33]

We investigate the effect of aid on investment, overall consumption, private sector consumption, and government consumption. We also investigate the effect of aid on domestic savings, testing whether aid inflows are substituted by equivalent decreases in domestic savings. Specifically, we focus on gross capital formation (in % of GDP), household final consumption expenditure (in % of GDP) and government final consumption expenditure (in % of GDP), with overall consumption being the sum of the two, and gross domestic savings (in % of GDP). We use the same covariates and timing as in our aid-growth regressions above.

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[33]Werker et al. (2009) find aid from Arab donors to be consumed rather than invested in large parts. They also show that domestic savings decrease with increased aid inflows.

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TABLE 2.4

Aid and other outcomes, 1974-2009, IV, different samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	**Policy**		**Democracy**		**Cold War**	
		bad	good	no	yes	<1991	>1990
Investment							
Aid/GDP (t)	0.444	0.553	0.713	0.165	1.842	-0.374	-4.316
Number of observations	722	243	479	408	262	294	428
Aid/GDP (t-1)	0.851	0.889	1.072	0.598	3.882	1.231	-1.519
Number of observations	620	191	429	331	243	216	404
Savings							
Aid/GDP (t)	-0.821	0.875	-0.207	0.831	-1.776	0.703	-3.726
Number of observations	727	247	480	412	263	298	429
Aid/GDP (t-1)	-1.350	-0.200	-0.968	-0.700	-2.439	2.846	-9.624
Number of observations	625	194	431	334	245	219	406
Overall consumption							
Aid/GDP (t)	0.850	-0.656	0.227	-0.765	1.767	-0.688	3.760
Number of observations	726	246	480	412	263	297	429
Aid/GDP (t-1)	1.328	0.295	0.951	0.723	2.264	-2.862	9.593
Number of observations	623	192	431	334	244	217	406
Government consumption							
Aid/GDP (t)	-0.168	-0.551	-0.685	-1.021	0.737	-0.926	0.078
Number of observations	726	246	480	412	263	297	429
Aid/GDP (t-1)	0.476	0.132	0.233	0.422	0.624	-0.602	3.314
Number of observations	623	192	431	334	244	217	406
Private consumption							
Aid/GDP (t)	1.022	-0.105	0.917	0.257	1.044	0.238	3.708
Number of observations	726	246	480	412	263	297	429
Aid/GDP (t-1)	0.859	0.164	0.729	0.301	1.683	-2.260	6.311
Number of observations	623	192	431	334	244	217	406

Notes: The dependent variables are – all as a percentage of GDP – Overall Consumption, government final consumption expenditure (Gov. Consumption), household final consumption expenditure (Private Consumption), gross capital formation (Investment), and gross domestic savings (Savings). The coefficients shown refer to contemporaneous and lagged Aid as a percentage of GDP. The bad/good policy sample includes countries below/above the median according to the Burnside-Dollar good policy index. Democracy is measured with the binary indicator of Cheibub et al. (2010). Data are averaged over four years at the recipient-period level. Recipient- and period-fixed effects are included. The first- and second-stages include as control variables: Log initial GDP/capita, Assassinations, Ethnic*Assassinations, and M2/GDP (lagged). Pairs cluster bootstrap standard errors with 500 replications are used (clustered at the recipient country level; significance levels: * 0.10, ** 0.05, *** 0.01). Models are based on (Burnside and Dollar, 2000).

Table 2.4 shows the results. As can be seen, aid has no significant effect on any of the variables in any period. Specifically, there is no effect of aid on consumption, savings or investment in the overall samples, countries with good or bad policies, democratic or

undemocratic countries, or during or after the Cold War period. Overall, our results therefore contrast with those of the previous literature. Boone (1996), for example, reports that aid increases consumption, but not savings and investment. Werker et al. (2009) find that household and government consumption both increase with aid, that savings decrease with aid, and investment is unaffected (all focusing on Arab donors and the recipients of their aid exclusively). Temple and Van de Sijpe (2017) confirm the positive impact of aid on total consumption, which seems to be driven mainly by household consumption. This shows the importance of the choice of identification strategy, as well as the sample of donors and recipients, for testing the effect of aid on the outcomes of interest.

2.7.   Conclusion

This paper has proposed an excludable instrument to identify whether and to what extent foreign aid affects economic growth. Cross-sectional variation arises due to changes in aid disbursements following differences in donor countries' government fractionalization. Temporal variation is introduced by interacting fractionalization with the probability of a certain country receiving aid. The approach resembles a difference-in-difference approach, the difference being that our treatment variable (fractionalization) is a continuous rather than a binary indicator.

Using aid disbursement data for all bilateral donors of the OECD's DAC to a maximum of 96 recipient countries over the 1974-2009 period, we find our instrument to be powerful. For the average recipient country this represents roughly quadrupling the amount of current (bilateral) aid. In contrast, countries that receive aid only half of the time can expect an increase in aid inflows of 0.183 percentage points. Applying the instrument to our growth models, we find bilateral aid to be ineffective in increasing economic growth in the overall sample and various sub-samples, split along the quality of economic policies, democracy, and the Cold War period. In the years after the end of the Cold War, we find growth to decrease with abundant aid. We also investigate the effect of aid on savings, consumption, and investment, and do not find any effect of aid in the overall sample or our sub-samples.

Our results show that bilateral aid has no robust effect on short-term growth. We would like to stress that this finding does not imply that aid is necessarily ineffective. One might argue that aid is measured imprecisely, and standard errors are too large. Statistical power might be too low for the estimators to find a significant effect, even if it would be there (Ioannidis et al., 2017). We agree that these are two possible explanations for our insignificant results. We still believe that it is important to show, and publish, these results, as the published literature on the effectiveness of aid tends to be over-optimistic, due to institutional biases of the authors in the aid effectiveness literature

and the well-known bias of journals to publish (only) significant results (Doucouliagos and Paldam, 2009; Doucouliagos, 2016). As the lack of power pertains independent of the significance of the results, there is arguably no reason to dismiss ours on the grounds of large standard errors, compared to a number of recent papers finding significant (and positive) results. We therefore urge readers to evaluate this paper on its methodological improvements over the previous literature, rather than its results.

At least one other important reason can explain the insignificant results: Donors pursue a multitude of objectives when granting aid, with economic growth being just one of them. To the extent that donors prioritize geostrategic goals over developmental ones, the effects of "true" developmental aid will be higher than those of all aid (Dreher et al., 2016). Aid would then need to be evaluated based on progress towards its "true" goals. While we did not investigate such outcomes here, the effects of aid on a number of alternative outcomes have been documented, including on terror (Azam and Thelen, 2008), voting behavior in international organizations (Vreeland and Dreher, 2014), and conflict (Chapter 1; Nunn and Qian, 2014).

We would like to conclude this paper by pointing to a number of important questions that could be addressed with our instrumental variables strategy, for a large number of donors and years. The effect of aid on formal and informal institutions, economic freedom, conflict, terrorism, migration, and the size of the shadow economy, among others, has been investigated in a large number of papers. All of these questions face the problem of endogeneity between aid and the variable of interest. Our instrument is well-suited to address this problem, as has been demonstrated in Chapter 1 for conflict, and in Ziaja (2017) for democracy. In providing an instrumental variable that is suitable to address the endogeneity of aid in a broad setting of questions, we hope to contribute in providing a more nuanced understanding of the various causal effects the aid might have.

Appendices to Chapter 2

2.A.   Definitions and sources

TABLE 2.5

Definitions and sources

Variable	Description	Data Source
Agency	Dummy 1 if there are national aid agencies operating independently from the Ministry of Foreign Affairs (Donor).	Fuchs et al. (2014)
Aid/GDP	ODA Total Net, current prices (USD) in % of recipient GDP, aggregated over all 28 bilateral DAC donors.	OECD, World Bank (2014)
Aid Budget/GDP	Donor ODA Total Net, current prices (USD) – to all recipients divided by donor GDP in current prices.	OECD, World Bank (2014)
Central Government Expenditures/GDP	Central government expenditures (% of GDP) annual % of GDP (Donor).	IMF/GFS (2014)
Closed Lists	When proportional representation is 1, closed list gets a 1 if voters cannot express preferences for candidates within a party list, 0 if not (Donor).	Database of Political Institutions (Beck et al., 2001)
Log Colony	Log of the population of former colonies on DAC list of ODA recipients (1997-2013), 0 if no colonial history (Donor).	Own calculations based on Fuchs et al. (2014)
Democracy	Dummy 1 if recipient country is a democracy.	Cheibub et al. (2010)
Donor Exports	Log value of Exports from donor to recipient country in US Dollars (constant 2005 USD).	IMF (DOTS)
Donor GDP/capita Growth	GDP per capita growth (annual %) (Donor).	World Bank (2014)
Economic Freedom	Economic Freedom, chain linked index.	Fraser Institute
Donor GDP Growth	GDP growth (annual %) (Donor).	World Bank (2014)
Government Consumption/GDP	General government final consumption expenditure (% of GDP).	World Bank (2014)
Fractionalization (Frac)	The probability that two deputies picked at random from among the government parties will be from different parties.	Database of Political Institutions (Beck et al., 2001)
Investment/GDP	Investment – gross capital formation (% of GDP).	World Bank (2014)
Log GDP/capita	Log of donor GDP per capita (constant 2005 USD).	World Bank (2014)
Log Population	Log of population total (Donor).	World Bank (2014)

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Overall Consumption in % of GDP	Overall consumption: sum of private and government consumption (% of GDP).	Own construction based on World Bank (2014)
Political Globalization	KOF Political Globalization Index composed of embassies in country (25%), membership in international organization (27%), participation in UN Security Council missions (22%), international treaties (26%).	Dreher (2006), updated in 2013
Population (Share>64)	Population ages 65 and above (% of total) (Donor).	World Bank (2014)
Presidential	Dummy 1 for a presidential country (Donor).	DPI (Beck et al., 2001)
Private Consumption/GDP	Household final consumption expenditure (% of GDP).	World Bank (2014)
Probability over all Periods	The probability of receiving aid from a particular donor j within the whole observation period from 1974-2009.	Own construction based on ODA Total Net Data from OECD (2014), Table DAC2a
Recipient Exports	Log value of Exports from recipient to donor country in US Dollars (constant 2005 USD).	IMF (DOTS)
Savings/GDP	Gross domestic savings (% of GDP).	World Bank (2014)
Total Seats	Total seats in the legislature or in the case of bicameral legislatures, the total seats in the lower house (Donor).	Database of Political Institutions (Beck et al., 2001)
Trade Openness	Trade (% of GDP) (Donor).	World Bank (2014)
Unemployment	Unemployment, total (% of total labor force) (national estimate) (Donor).	World Bank (2014)

Burnside and Dollar 2000 specification (4-year periods)

Assassinations	Average number of assassinations in a given period.	Banks and Wilson (2012, 2007)*
Ethnic*Assass- inations	Interaction between Assassinations and Ethnolinguistic Fractionalization.	Banks and Wilson (2012, 2007), Easterly and Levine (1997), Roeder (2001)*
Budget Balance	Overall budget balance, including grants. Measured as cash surplus/deficit (% of GDP).	World Bank (2005,2007), IMF (IFS) 2005*
Ethnolinguistic Fractionalization	Ethnolinguistic Fractionalization in a country in a given period.	Easterly and Levine (1997), Roeder (2001)*
GDP/capita Growth	GDP per capita growth (%) based on constant local currency.	World Bank 2007*

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Inflation	Natural log of (1+consumer price inflation).	World Bank (2005, 2007), IMF (2005)*
Institutional Quality	First non-missing value of the ICRG composite index [0, 10].	ICRG*
Log Initial GDP/capita	Logarithm of initial GDP per capita in International prices.	Penn World Tables 6.2*
M2/GDP	Lagged Money and quasi-money (% of GDP).	World Bank (2007)*
Openness	Wacziarg and Welch (2008) extension of the initial Sachs and Warner (1995) openness index.	Wacziarg and Welch (2008), updated by Clemens et al. (2012)*
Policy Index	Good policy index based on budget balance/GDP, inflation and trade openness (cf. Burnside and Dollar 2000).	Calculation based on Clemens et al. (2012)
Region Dummies	Dummies for Sub-Saharan Africa and East Asia.	Clemens et al. (2012)*

Notes: *Our source is Clemens et al. (2012), www.cgdev.org/doc/Working20Papers/CRBB-Replication-Files.zip, accessed January 22, 2014. More details can be found in the "Technical Appendix to Counting chickens when they hatch: Timing and the effects of aid on growth," www.cgdev.org/doc/Working20Papers/counting_chickens_technical_appendix.pdf, accessed January 1, 2014. Data for the most recent period are from Minasyan (2016). The variables listed below the Burnside and Dollar (2000) specification are recipient-specific characteristics.

2.B. Sample

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Table 2.6

Included donor countries, in alphabetical order

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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.

<div align="center">

Table 2.7

Included recipient countries, in alphabetical order

</div>

Afghanistan, Albania, Algeria, Angola, Argentina, Barbados, Benin, Bolivia, Botswana, Brazil, Burkina, Faso, Burundi, Cambodia, Cameroon, Central African Rep., Chad, Chile, China, Colombia, Dem. Rep. Congo, Rep. Congo, Costa Rica, Cote d'Ivoire, Cyprus, Dominican Republic, Ecuador, Egypt, El Salvador, Ethiopia, Gabon, Gambia, Ghana, Guatemala, Guinea, Guyana, Haiti, Honduras, India, Indonesia, Iran, Iraq, Israel, Jamaica, Jordan, Kenya, Korea, Kuwait, Laos, Lebanon, Lesotho, Liberia, Libya, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Mauritania, Mauritius, Mexico, Mongolia, Morocco, Mozambique, Nepal, Nicaragua, Niger, Nigeria, Pakistan, Panama, Papua New Guinea, Paragua, Peru, Philippines, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, South Africa, Sri Lanka, Sudan, Syria, Tanzania, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, Uruguay, Venezuela, Yemen, Zambia, Zimbabwe

2.C.   Descriptive statistics

<p align="center">TABLE 2.8
Descriptive statistics: Table 2.1</p>

	Obs.	Mean	SD	Min	Max
GDP p.c. growth	739	1.56	3.78	-32.42	17.05
Aid/GDP	739	3.60	4.81	-0.15	47.91
Probability of Receiving Aid	739	0.55	0.13	0.12	0.73
Log Initial GDP/capita	739	7.99	1.01	5.14	10.80
M2/GDP, lagged	739	6.36	22.14	0.02	236.92
Institutional Quality	641	4.48	1.61	1.58	9.50
Assassinations	739	0.30	1.01	0.00	11.50
Ethnolinguistic Franctionalization	739	0.46	0.29	0.00	0.93
Sub-Saharan Africa	739	0.41	0.49	0.00	1.00
East Asia	739	0.08	0.27	0.00	1.00
Policy Index (Burnside & Dollar 2000)	501	1.65	0.84	-2.61	2.63
Democracy (Cheibub)	739	0.39	0.47	0.00	1.00
Polity IV	713	0.43	6.63	-10.00	10.00
Fractionalization (Donor)	739	0.34	0.25	0	0.81

Notes: Government fractionalization is replaced with legislature fractionalization for the United States and Canada. Sample based on column 1 in Table 2.1.

Table 2.9

Descriptive statistics: Tables 2.4, 2.12, and 2.13

	Obs.	Mean	SD	Min	Max
Panel A: Variables Table 2.4					
Government Consumption/GDP	693	14.40	6.03	3.92	49.86
Private Consumption/GDP	693	69.28	15.74	17.70	180.81
Overall Consumption/GDP	693	83.68	15.43	32.29	194.85
Investments/GDP	689	21.75	7.82	4.42	68.28
Savings/GDP	694	16.32	15.41	-94.85	67.71
Panel B: Variables Table 2.12					
Central Government Expenditure/GDP	19869	32.30	9.64	11.90	54.01
Fractionalization (Donor)	19869	0.39	0.25	0.00	0.81
GDP Growth (annual Trade Openness	19869	76.51	36.98	17.80	201.48
Log Population	19869	15.98	1.50	12.30	18.64
Population (share>64)	19869	13.14	2.80	4.18	18.10
Log GDP/capita	19869	10.11	0.52	8.53	11.00
Closed Lists	19869	0.61	0.49	0.00	1.00
Total Seats in the Legislature	19869	231.99	147.73	58.00	669.75
Presidential	19869	0.07	0.25	0.00	1.00
Unemployment	16647	6.94	4.42	1.80	22.30
Panel C: Variables Table 2.13					
Aid Budget/GDP	21838	0.43	0.25	0.00	1.03
Central Government Expenditure/GDP	21838	31.79	9.89	11.90	54.01
Log GDP/capita	21838	10.24	0.30	9.58	11.00
Aid Agency	21838	0.37	0.48	0.00	1.00
Log Colony	21838	11.48	8.39	0.00	21.34
Political Globalization	21838	86.44	10.55	53.67	97.91

Notes: Government fractionalization is replaced with legislature fractionalization for the United States and Canada. Sample based on column 1 for Tables 2.4, 2.12, and 2.13.

2.D.   Additional regressions

TABLE 2.10

Aid and growth, 1974-2009, IV, no covariates

	(1)	(2)	(3)	(4)
	Panel A: Second Stage			
Aid/GDP	-0.128	0.080	0.064	0.079
	(0.463)	(0.453)	(0.386)	(0.489)
Aid/GDP squared		-0.019		-0.001
		(0.014)		(0.017)
Aid lagged	No	No	Yes	Yes
Level of Govfrac	Yes	Yes	Yes	Yes
Level of Probability	Yes	Yes	Yes	Yes
Number of observations	739	739	636	636
Kleibergen-Paap F stat.	16.054	8.912	19.856	9.434
Kleibergen-Paap LM stat.	0.000	0.003	0.000	0.002
	Panel B: First Stage			
Fitted Aid/GDP	5.421***	5.421***	6.047***	6.047***
	(1.354)	(1.354)	(1.358)	(1.358)
Squared predicted Aid/GDP		2.247***		2.046***
		(0.753)		(0.667)
	Panel C: Zero Stage			
Fractionalization*Probability		0.363***		
		(0.035)		

Notes: Data are averaged over four years at the recipient-period level. Recipient- and period-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications are in parentheses in the second stage regressions (clustered at the recipient country level). Standard errors are in parentheses in the first-stage regressions (clustered at the recipient country level). The first-stage statistics reported in columns 2 and 4 refer to the squared aid term. The statistics for the linear term in columns 2 and 4 are identical to columns 1 and 3, respectively.

TABLE 2.11

Zero-stage, alternative approaches

	(1)	(2)
	Panel A: Zero Stage	
Fractionalization*Probability	0.363***	0.213***
	(0.035)	(0.053)
Kleibergen-Paap F stat.	104.62	20.572
K-P LM stat. p-val.	0.000	0.000
	Panel B: Second Stage	
Aid/GDP	-0.298	-0.298
	(0.430)	(0.430)
Controlling for the levels of the interaction	No	Yes
Controlling for time FE, country pair FE	No	Yes

Notes: Data are averaged over four years at the donor-recipient-period level in the zero-stage regression and at the recipient-period level in the second stage regression. Standard errors are in parentheses in the zero-stage regression (clustered at the donor-recipient level). Pairs cluster bootstrap standard errors with 500 replications are in parentheses in the second stage regressions (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE 2.12

Fractionalization and central government expenditures, 1974-2009, OLS

	(1) Scartascini & Crain (2002)	(2) Roubini & Sachs (1989)
Fractionalization	2.787***	0.848***
	(0.254)	(0.199)
Log Population	-11.918***	
	(0.999)	
Trade Openness	-0.045***	
	(0.005)	
Population (share>64)	1.560***	
	(0.047)	
Log GDP/capita	-10.967***	
	(0.520)	
Closed Lists	2.549***	
	(0.182)	
Total Seats	0.038***	
	(0.002)	
Presidential	6.929***	
	(0.460)	
Central Gov. Exp./GDP (t-1)		0.147***
		(0.008)
GDP Growth (annual percent)		-0.306***
		(0.024)
Unemployment		0.385***
		(0.022)
Number of observations	19869	18795
Adjusted R-squared	0.881	0.933

Notes: Data are averaged over four years at the donor-recipient-period level. Donor- and period-fixed effects are included. Standard errors are in parentheses (clustered at the donor-recipient country level; significance levels: * 0.10, ** 0.05, *** 0.01). Model (1) is based on Scartascini and Crain (2002), Model (2) on Roubini and Sachs (1989).

TABLE 2.13

Central government expenditures and aid budgets, 1970–2009, OLS

	(1)	(2)	(3)	(4)	(5)
Central Gov. Exp./GDP	0.0023***	0.0062***	0.0020***	0.0062***	
	(0.0001)	(0.0002)	(0.0001)	(0.0002)	
Aid Budget/GDP (t-1)	0.6044***		0.6242***		0.6304***
	(0.0040)		(0.0034)		(0.0040)
Log GDP/capita	0.4017***	0.5426***	0.4495***	0.5657***	0.3026***
	(0.0154)	(0.0152)	(0.0135)	(0.0163)	(0.0088)
Aid Agency	0.0543***	0.0854***	0.0363***	0.0779***	0.0549***
	(0.0022)	(0.0040)	(0.0024)	(0.0042)	(0.0017)
Log Colony	-0.1400***	-0.6614***	-0.1009***	-0.6516***	-0.0468***
	(0.0073)	(0.0093)	(0.0058)	(0.0100)	(0.0044)
Political Globalization			0.0043***	0.0019***	
			(0.0002)	(0.0003)	
Fractionalization					0.0308***
					(0.0034)
Number of observations	21838	21838	21838	21838	35263
Adjusted R-squared	0.940	0.887	0.943	0.888	0.943

Notes: Data are averaged over four years at the donor-recipient-period level. Donor- and period-fixed effects are included. Standard errors are in parentheses (clustered at the donor-recipient country level; significance levels: * 0.10, ** 0.05, *** 0.01). Models are based on Fuchs et al. (2014).

2.E. Full regressions

TABLE 2.14

Full regressions

	(1)	(2)	(3)	(4)
Panel A: OLS				
Aid/GDP	0.049	-0.058	0.250**	0.311***
	(0.059)	(0.123)	(0.120)	(0.096)
Aid/GDP squared		0.003		-0.002
		(0.004)		(0.005)
Log Initial GDP/capita	-2.949***	-3.049***	-3.330***	-3.287***
	(0.619)	(0.696)	(0.588)	(0.631)
Assassinations	-0.013	-0.005	-0.221	-0.220
	(0.187)	(0.185)	(0.189)	(0.188)
Ethnic*Assassinations	-0.688	-0.700	0.015	0.010
	(0.809)	(0.802)	(0.556)	(0.554)
M2/GDP (t-1)	-0.009	-0.008	-0.002	-0.002
	(0.006)	(0.006)	(0.008)	(0.007)
Aid lagged	No	No	Yes	Yes
Number of observations	739	739	636	636
Adjusted R-Squared	0.151	0.153	0.197	0.196
Panel B: IV				
Aid/GDP	-0.298	-0.254	-0.087	0.002
	(0.430)	(0.336)	(0.379)	(0.342)
Aid/GDP squared		-0.011		-0.018
		(0.011)		(0.014)
Log Initial GDP/capita	-4.135**	-4.847**	-4.268***	-4.924***
	(1.789)	(1.966)	(1.360)	(1.271)
Assassinations	0.049	0.078	-0.204	-0.179
	(0.288)	(0.285)	(0.275)	(0.255)
Ethnic*Assassinations	-0.802	-0.863	-0.007	-0.068
	(1.080)	(1.069)	(0.970)	(0.909)
M2/GDP (t-1)	-0.004	-0.003	0.000	0.003
	(0.009)	(0.009)	(0.009)	(0.009)
Aid lagged	No	No	Yes	Yes
Level of Govfrac	Yes	Yes	Yes	Yes
Level of Probability	Yes	Yes	Yes	Yes
Number of observations	739	739	636	636
Kleibergen-Paap F stat.	15.881	9.586	20.393	28.113
Kleibergen-Paap LM stat.	0.000	0.000	0.000	0.008

Notes: Notes: Data are averaged over four years at the recipient-period level. Recipient- and period-fixed effects are included. Standard errors are in parentheses in columns 1–4 (clustered at the recipient country level, significance levels: * 0.10, ** 0.05, *** 0.01); pairs cluster bootstrap standard errors with 500 replications are used in columns 5–8 (clustered at the recipient country level; significance levels: * 0.10, ** 0.05, *** 0.01). Models are based on Burnside and Dollar (2000).

2.F. Parallel trends

FIGURE 2.4
Parallel trends

A: Fractionalization

B: Aid/GDP

C: Growth

——— Below mean probability ——— Above mean probability
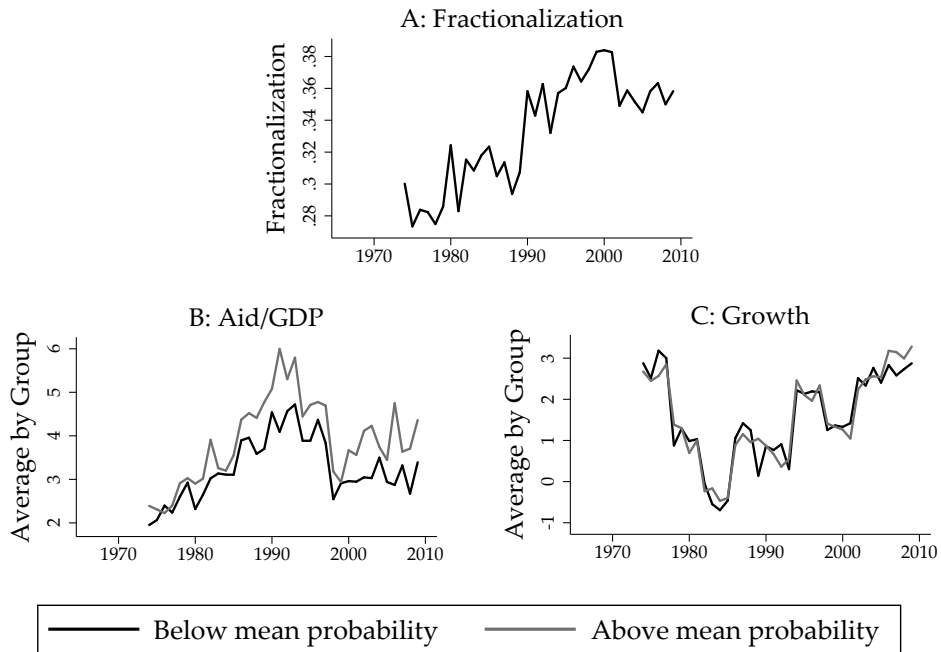
Notes: Panel A shows how government fractionalization (replaced by legislature fractionalization for the United States and Canada) varies over time. Panel B is the average aid to GDP-ratio within the group that is below the mean of the probability to receive aid (black line) and the group that is above the mean (grey line) over time. Panel C is the average real GDP per capita growth rate within these two groups over time. For the construction of the averages we use observations from the sample of column 1 for Table 2.1.

Chapter 3

Stimulant or depressant? Resource-related income shocks and conflict

Joint work with Kai Gehring and Stefan Kienberger

Abstract

We combine temporal variation in international drug prices with new data on spatial variation in opium suitability to examine the effect of opium profitability on conflict in Afghanistan. District level results indicate a conflict-reducing effect over the 2002-2014 period, both in a reduced-form setting and with three different instrumental variables. We provide evidence for two main mechanisms. First, the importance of contest effects depends on the degree of violent group competition over valuable resources. By using data on the drug production process, ethnic homelands, and Taliban versus pro-government influence, we show that on average group competition for suitable districts is relatively low in Afghanistan. Second, we highlight the role of opportunity costs by showing that opium profitability positively affects household living standards, and becomes more important after a sudden rise in unemployment due to the dissolution of large armed militias after an exogenous policy change.

3.1. Introduction

An important strand of the resource-curse literature examines how resource-related income shocks are linked to conflict (e.g., Brückner and Ciccone, 2010; Morelli and Rohner, 2015; Berman et al., 2017). Yet, we've only begun to understand the micro-foundations behind the resource-conflict-nexus. After focusing on the aggregate country level for many years, recent contributions at the micro level have discovered large heterogeneities across different commodities and countries (e.g., Dube and Vargas, 2013).

Our paper makes three main contributions, which relate to the the contest or rapacity hypothesis – where fights about valuable resources increase conflict – and the opportunity cost effect – where higher resource prices improve living conditions and lower conflict. First, we augment the contest hypothesis by highlighting that one has to consider the degree of competition between groups that fight for valuable resources. Afghanistan is an ideal setting to analyze the role of group competition, as it comprises many ethnic groups, but at the same time the conflict is mainly between two opposing sides since 2001. Our results show that higher opium prices reduce conflict incidence and intensity in Afghanistan. We provide evidence that, in line with our hypothesis, there is on average little between-group competition about opium production sites, and further show that the conflict-reducing effect of opium is stronger in districts that are more plausibly dominated by the Taliban. On the contrary, existing evidence for Colombia suggests a rapacity effect as rising cocaine prices lead to more conflict (Angrist and Kugler, 2008; Mejia and Restrepo, 2015). Both findings are in line with our hypothesis as in the setting of Colombia, rival groups compete for cocaine production grounds.

Second, we further examine and verify the main hypothesis in Dube and Vargas (2013). They show that positive price changes of relatively more labor-intensive goods reduce conflict because they increase the opportunity costs of joining rebel groups and engaging in fighting. Afghanistan, which is characterized by a weak labor market and by a large share of people working in agriculture, provides a good example to cross-validate the external validity of this important hypothesis. There are only two main crops that are feasible to produce all across the country; opium, which is very labor-intensive, and wheat, which requires less labor (Mansfield and Fishstein, 2016).[1] Accordingly, a relative decline in opium prices causes marginal producers to shift towards wheat production and decreases labor demand.[2] In absence of good alternatives, joining a rebel group like the

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[1]Our analysis does not explicitly consider other crops. We do not neglect their importance in certain areas, especially when they are intercropped (i.e., when farmers can combine their cultivation on the same land) and when they allow cultivation over two or three seasons per year. However, as each individual crop is negligible in importance compared to opium and the cultivation of these alternatives is restricted to certain areas, we assume that shocks to the profitability of these crops are not systematically biasing the effect of the exogenous opium profitability.

[2]According to UNODC (2004) between 80% to 90% of landowners and farmers decide on their own what they plant, which will usually be the most profitable crop.

Taliban is one of the few options (e.g., Bove and Elia, 2013). We use survey data to verify that opium profitability indeed matters for well-being at the household level, and show that the apparent reliance on opium increases after an exogenous policy shock that deprived people of an important alternative source of income.

Exploring those mechanisms helps to understand the role of opium in the Afghan conflict, which caused more than 100.000 battle-related deaths since 2002. In addition to learning more about this individual case, lessons from this conflict might turn out to be useful for other cases. Afghanistan resembles other countries, in that it has a weak government that cannot effectively enforce its monopoly of violence as well as a high level of ethnic fractionalization and weak labor market with few opportunities of formal employment.

Third, we establish causality through the combination of novel data with different identification strategies. In particular, by combining temporal variation in international drug prices with a new dataset on spatial variation in opium suitability (Kienberger et al., 2017), we can observe changes in opium profitability across time and districts. We exploit patterns in consumer preferences in the drug market and use the international price of heroin (which is made of opium) and of complements to heroin, along with local opium prices in a reduced-form setting to verify the causal interpretation of our findings. In addition, we propose two alternative identification strategies in an instrumental variable setting, based on climatic differences and changes in legal opioid prescriptions in the United States. All these strategies lead to the same result. We find that a higher opium profitability consistently reduces both conflict incidence and intensity.

Our dataset allows us to identify if this effect is indeed driven by changes in opportunity costs. First, we use different waves of the National Risk and Vulnerability Assessment (NRVA) to show that the gains from higher opium profitability reach the average household. We find consistently higher food consumption and living standards using various indicators, suggesting that a more profitable opium economy increases the opportunity costs of fighting. Second, we georeference data provided by the United Nations Office for Drugs and Crime (UNODC) on drug markets, labs, and potential trafficking routes (see among other reports, UNODC, 2016). We argue that districts which do not only cultivate opium in its raw form, but also process and trade it can capture a larger share of the value added along the supply chain. This affects both the intensive margin (higher revenues) as well as the extensive margin (more people benefiting). We conceptualize this by using simple indices and network-based variants of market access (Donaldson and Hornbeck, 2016). If there was strong between-group competition in the Afghan drug market, we would expect more fighting in those districts. The conflict-reducing effect is, however, even stronger in those districts that feature further processing steps and have high drug-market access. This is in line with an explanation based on the opportunity costs of fighting, and implausible if there was strong between-group

competition in the drug market (as it is apparently the case in Colombia, see, Angrist and Kugler, 2008; Mejia and Restrepo, 2015).

We complement the evidence on little competition between rival groups for suitable districts in Afghanistan by relying on the geographical distribution of ethnic homelands (Weidmann et al., 2010). We compute whether a district is ethnically mixed and how many ethnic groups it features. Building on the literature about the role of ethnic groups for conflict (e.g., Esteban et al., 2012b), we would expect no or a smaller conflict-reducing effect in ethnically heterogeneous districts if there was strong violent competition between those groups. We find no significant differences, supporting our hypothesis of limited group competition.

Furthermore, our hypothesis on the role of group competition for suitable districts would predict that the conflict-reducing effects should be the strongest in districts that are plausibly dominated by one group. We exploit the fact that after 2001 almost all fighting occurs between pro-government groups including Western military forces and the Taliban.[3] We use maps on the historical Taliban presence and the homelands of the Pashtun ethnic group, as well as data about the location of foreign military bases and main cities to measure whether a district is (i) more plausibly controlled by the Taliban or (iii) controlled by the government or foreign military.[4] With regard to the first point, we find that the conflict-reducing effect after 2001 is stronger in areas that are more likely controlled by the Taliban. This supports qualitative evidence about the ideological turn of the group towards protecting opium farmers and their apparently strong relations with the opium economy. The finding is also in line with anecdotal evidence that the group is acting as a stationary bandit, which maximizes revenues in the districts it controls.

Regarding the second point, we analyze the role of the Afghan government and foreign military more specifically. The degree to which the illegality of opium influences conflict decisively depends on actual government strategies, i.e., control and enforcement. In Mexico, for instance, the culmination of violence in recent years coincides with the government taking stricter actions and enforcement, potentially breaking up existing and more peaceful equilibria among drug cartels and the government (for studies on Mexico see, for instance, Dell, 2015; Mejía et al., 2015). Illegality increases profits and the risk of production, as well as fostering the creation of organized criminal groups, that fight for suitable areas and rents. Another dimension of government influence is that if rules are enforced, this creates an incentive for opium farmers to cooperate and finance the Taliban who offer protection against those measures. Consequently, this can lead to more fights between Taliban and government (Peters, 2009), and a smaller or no conflict-reducing effect.

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[3]This is based on the UCDP Georeferenced Event Dataset (GED).

[4]The Taliban are initially a Pashtun group (although not exclusively anymore), so that Pashtun presence makes it easier to establish a presence of the Taliban in a district.

The foreign coalition officially takes a strong stance on drugs in general and opium in particular. Several United Nations Security Council (UNSC) bulletins claim resolute actions against drug producers and traffickers. According to our data, we find no evidence of a heterogeneous effect according to the presence of the foreign military forces. This is in line with statements by the US military leadership who do not regard "anti-drug enforcement" as part of their agenda.[5] To measure government influence, we follow Michalopoulos and Papaioannou (2014) and use the distance to the capital as a measure of the Afghan government's influence. To account for the specific topography of Afghanistan, we do not only measure the distance in a straight-line, but also compute two- and three-dimensional road distances and estimated travel times to Kabul and other big cities. We generally find no moderating effect of any of those approximations. The influence of the government, leading to potential conflict with producers of *de jure* illegal crops, seems to be confined to a radius of about 75 km or 2 hours travel time to Kabul.

Finally, we use a policy change in the Western military strategy to further shed light on the difficulties associated with nation-building during and after a military intervention, and suggest some important trade-offs. In a nutshell, between 2001 and approximately 2005 the Western forces financially supported warlords and local strongholds to build a strong anti-Taliban coalition. Estimates report that several hundred thousands of men were armed and became part of those militias, and that more than 60% of the provincial governors "were leaders of armed groups and most of the remaining ones had links to the latter" during that time (Giustozzi, 2009, p. 91). Around 2005, the Western coalition switched their strategy towards a nation-building approach that attempted to pacify and "clean" Afghan politics by putting pressure on the Afghan government to force political leaders and governors to give up and abandon their connection to the militias (Giustozzi, 2009, p. 94ff.). In the following, many trained and armed men who were part of those groups lost a substantial source of income and the reliance on other income sources like opium production assumedly increased. By exploiting the approximate timing of this change, we can show that the connection between drug profitability and conflict indeed becomes much stronger after 2005. This suggests a trade-off between reducing the influence of non-state armed groups and fighting the production of an illegal resource at the same time. Both policy goals apparently cannot be achieved simultaneously.

We proceed as follows. Section 3.2 discusses the relevant theoretical considerations

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[5]The official views are visible in, for instance, the 2004 UNSC Resolution 1563 stressing "the importance of extending central government authority to all parts of Afghanistan, [...], and of combating narcotics trade and production" (see http://unscr.com/en/resolutions/doc/1563, accessed June 14, 2018). When asked about the actual approach of the military, Jean-Luc Lemahieu, who was head of the UNODC in Afghanistan from 2009 to 2013, is quoted as saying "drug control wasn't a priority." Other sources at the US government are quoted with an informal bargain that they "would not pursue top Afghan allies who were involved in the drug trade." Source: http://www.rollingstone.com/politics/news/afghanistan-the-making-of-a-narco-state-20141204, accessed June 14, 2018.

and the related literature, Section 3.3 introduces the data, and Section 3.4 the empirical strategy. The main results are presented in Section 3.5. We investigate heterogeneity of the results and the underlying channels in Section 3.6. We discuss sensitivity tests in Section 3.7. Section 3.8 summarizes and provides policy implications.

3.2. Theoretical considerations and contributions to the literature

Theoretical considerations

From a theoretical perspective, it is *ex ante* unclear in which direction income in general, and opium-related income in particular, affects conflict. The existing literature mainly distinguishes between two channels, the opportunity costs mechanism (e.g., Grossman, 1991) and the contest model (e.g., Hirshleifer, 1988, 1989, 1995). The first theory hypothesizes that with a rise in income the opportunity costs of fighting increase, leading to, on average, less violence. For an individual, joining or supporting anti-government troops like the Taliban, becomes less attractive after an increase in the profitability of opium. In contrast, the contest model, or rapacity effect, would predict that with higher opium profitability, highly suitable territories become relatively more attractive, because the potential gains from fighting are greater. This would predict relatively more fighting in attractive districts when opium prices are high.

In Afghanistan, the main alternative to growing poppies is considered to be growing wheat (UNODC, 2013; Lind et al., 2014) or, if neither is sufficiently attractive, joining a rebel group.[6] Many studies suggest that growing poppies is generally far more profitable and the gross wheat-to-opium income-ratio ranges between 1:4 to 1:27 (UNODC, 2005, 2013). Nevertheless, Mansfield and Fishstein (2016) criticize this over-simplified approach for focusing on gross instead of net returns, and ignoring differences in the production process. We consider this a valid criticism, in particular against the background of the evidence provided by Dube and Vargas (2013) that labor intensity is crucial to understand the effect of resource shocks on conflict. The differences in Afghanistan are indeed large. Mansfield and Fishstein (2016, p. 18) report "opium requiring an estimated 360 person-days per hectare, compared to an average of only 64 days for irrigated wheat." This leads to two important implications.

First, whether opium is profitable (and more profitable than an alternative, e.g., wheat) depends on the price in the respective year and differs between districts, also because it is costlier than wheat in terms of inputs like fertilizer. Mansfield and Fishstein (2016) report that there were years where opium was profitable across nearly all locations

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[6]Bove and Elia (2013, p. 538) even write that "in Afghanistan individuals may choose between opium cultivation and joining an anti-government group."

they examined, and other years where this depends on the specific location. This supports our empirical strategy exploiting that price changes have heterogeneous effects depending on the suitability of soil. Second, other crops are plausible alternatives when considering the net returns. We take this into account by controlling for shocks on wheat profitability in a similar manner as we do for opium. Due to the differences in labor intensity and legal status, we expect different effects of both shocks. The effect of a positive wheat shock on opportunity costs is ambiguous. While the income of some producers and farmers increases, most farmers grow wheat as a staple crop and many households are net buyers of wheat (Mansfield and Fishstein, 2016). The net effect on opportunity costs is hence a combination of both a positive and a negative effect and thus remains an open empirical question.

Studying the production process highlights one reasons why lower opium prices can lead to more conflict. If opium becomes relatively less profitable compared to wheat, some marginal (small or large) landowners will decide to switch to the less labor-intensive wheat production. This will decrease the demand for labor. For those Afghans owning some land, it means that they loose a potentially more lucrative alternative or complementary source of income in addition to cultivating crops for subsistence. Tenant farmers and cash-croppers do not even have this alternative or back-up option; for them joining anti-government groups, who pay a minimal salary might be the only viable alternative.[7]

In Afghanistan, a further channel linking illegal crop production and conflict, is producers turning to rebel groups which offer protection against eradication or expropriation, in exchange for some form of a taxation. According to a survey conducted in southern Afghanistan, more than 65% of the farmers and traffickers stated that protection of opium cultivation and of trafficking is the main opium-related activity of the Taliban (Peters, 2009). UNODC (2013, p. 66) states that "[i]n some provinces, notably those with a strong insurgent presence, some or all farmers reported paying an opium tax" in the form of land or road taxes. If the Taliban were to use these revenues to expand their battle activities, this could amplify conflict.[8] Depending on whether the group acts as stationary or roving bandits (De La Sierra, 2015), they could also establish monopolies of violence to sustain taxation contracts and try to avoid conflict when the profitability of the taxable resources is higher. Wright (2018) argues that the tactics of rebel groups depend on their and the state's capacity as well as on outside options available to civilians, which can all be affected by income shocks. While rebel tactics are not the focus of our study, we distinguish between different types of violence in a robustness test. Taken

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[7]Several sources speak of ten US Dollar per month as the wage offered by the Taliban (more than in the official army), e.g., https://www.wired.com/2010/07/taliban-pays-its-troops-better-than-karzai-pays-his/ and Afghan officials are cited as wanting to turn "ten-dollar-Taliban" around (https://www.cleveland.com/world/index.ssf/2009/08/afghan_leaders_move_toward_rec.html, accessed June 14, 2018).

[8]See also http://www.huffingtonpost.com/joseph-v-micallef/how-the-Taliban-gets-its_b_8551536.html, accessed June 14, 2018.

together, a positive price shock on opium production could lead to less conflict through an opportunity cost channel or to more conflict via higher expected gains from fighting and financing rebel activity.

Contributions to the literature

We contribute to different strands of the literature. First, we add to the large literature on resource-related income shocks and conflict. Empirically, income is often found to be one of the strongest correlates of violence (e.g., Fearon and Laitin, 2003; Collier and Hoeffler, 2004b; Blattman and Miguel, 2010). Most recent studies exploit income shocks induced by international commodity price changes or rainfall fluctuations that affect local production and income levels, and can in turn also affect the level of conflict. However, studies at the cross-country macro level (e.g., Miguel et al., 2004; Brückner and Ciccone, 2010; Bazzi and Blattman, 2014) and subnational level (e.g., Caselli and Michaels, 2013; Dube and Vargas, 2013; Berman and Couttenier, 2015; Berman et al., 2017) are still far from reaching a consensus. One plausible reason is that the majority of these papers do not consider the different features of resources and income sources and the role of violent group competition.[9]

Second, our analysis adds to the scarce causal evidence on the effect of illegal commodities. Despite the importance of the illicit economy, in particular in many developing and conflict-ridden societies, the literature provides very limited evidence on the effects of illegal commodity shocks on conflict. One notable exception is Dell (2015), who uses a regression discontinuity design to identify a causal relationship between drug trafficking, the political approaches to cope with it, and drug-related violence at the municipal level in Mexico. Our paper is similar to the extent that we also foster the understanding of the causal effect of drug cultivation and related activities on the behavior of people in affected regions. Closely related to our paper is the work by Angrist and Kugler (2008) and Mejia and Restrepo (2015), who exploit demand and supply shocks to cocaine and find a positive relationship with conflict in the Colombian context. Mejia and Restrepo (2015) show that when cocaine production was estimated to be more profitable the number of homicides increases. This effect is stronger in municipalities with a high suitability to grow cocaine, while a higher profitability of alternative crops such as cocoa, sugar cane, and palm oil tends to reduce violence. We augment these findings by showing that it is not only labor intensity and illegality per se, but that

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[9]Ross (2004) and Lujala (2009) differentiate between various types of resources, but do not address endogeneity. Ross (2004) analyses 13 cases and provides evidence on a relationship between oil, non-fuel minerals, and drugs with conflict. Lujala (2009) finds a negative correlation of conflict with drug cultivation, but suggests a conflict-increasing effect of gemstone mining and oil and gas. La Ferrara and Guidolin (2007) analyze the effect of conflict on diamond production, i.e., the opposite direction of causality. Gehring and Schneider (2016) show that oil shocks do not lead to violent conflict, but their distribution can foster separatist party success in democracies.

the nature of violent group competition over valuable resources moderates the resource-conflict-relationship. Additionally, we argue that illegality can matter if it is actually enforced by the government, which can lead to conflict with the producers and create support for cartels or rebel groups. With that said, we also contribute to the literature on the provision of state-like institutions by non-state actors (e.g., De La Sierra, 2015). This relates to problems of imposing rules upon occupied territory in general (Acemoglu et al., 2011) and establishing a credible government in a poor and economically constrained environment (Berman et al., 2011). Our results highlight the importance of distinguishing between de jure illegality and de facto enforcement. In Afghanistan, our results support anecdotal evidence that the government apparently takes a very loose stance on drug enforcement outside areas directly surrounding Kabul. Our results also hint towards the role of local Taliban groups as stationary bandits. Qualitative evidence emphasizes that the Taliban collect taxes from opium farmers and traffickers and even implement conflict-solving mechanisms within the districts under their control to minimize violence that would potentially disturb the profitable production process. Our contribution thus stresses the need to consider differences between the types of resources as well as local circumstances like market structures before drawing general conclusions about the effect of resources on conflict.

An important strand of literature emphasizes existing cleavages between ethnic groups as an important driver of conflict (e.g., Esteban and Ray, 2008; Besley and Reynal-Querol, 2014; Morelli and Rohner, 2015; Michalopoulos and Papaioannou, 2016). We argue that since 2001 there is on average little competition between ethnic groups about suitable districts in Afghanistan, because the conflict is between two major sides, and ethnic groups and tribes have to choose to support one side or the other. This is supported by the fact that our results do not differ between mixed and more or less ethnically fractionalized districts.

We also add to the emerging literature on conflict and violence in Afghanistan. For instance, Sexton (2016) uses plausibly random variation in the allocation of US counterinsurgency aid to show that more aid leads to more conflict in contested districts. In an experimental set up, Lyall et al. (2013) study the determinants of International Security Assistance Force (ISAF) support and show that harm caused by Western forces increases support for the Taliban. Trebbi and Weese (2016) propose a new method to study the internal organization of rebel groups in Afghanistan, supporting that the Taliban are by far the most important group. Condra et al. (2018) show that the Taliban try to undermine electoral institutions with attacks, but minimize direct harm to civilians. In contrast, most of the conflicts we capture within our sample period are between rebels and pro-government groups rather than against civilians.

Evidence on the relationship between opium and conflict is scarce, despite the fact that opium accounts for the largest share of profits in Afghanistan (Felbab-Brown, 2013)

and, according to UNODC (2009), one out of seven Afghans is somehow involved in cultivation, processing or trafficking. Opium represents an important source of income for at least 15% of Afghans, with a higher share in rural areas. Two studies address opium production and conflict in Afghanistan empirically. Bove and Elia (2013) show a negative correlation between conflict and opium prices for a sample of 15 out of 34 provinces and monthly data over the 2004-2009 period. Our paper augments the findings in Bove and Elia (2013) with a larger sample, longer time period and more systematic identification strategies. Lind et al. (2014) find a negative impact of Western casualties on opium production over the 2002-2007 period, and no effect in the opposite direction. Compared to the focus on Western casualties we can provide a more comprehensive measurement of conflict, and our different strategies allow us to carve out the direction of causality more clearly.[10] Our results seem to be at odds with Berman et al. (2011), who find no positive correlation between unemployment and insurgency attacks for Afghanistan, Iraq and the Philippines. The difference might be explained by their focus on the 2008-2009 period, the use of other outcome variables and their reliance on a fixed effects strategy without exogenous variation. Our findings based on household level data from the NRVA over the 2005-2012 period show that opium profitability coincides with households being better off.

3.3. Data

Conflict data: We use the UCDP Georeferenced Event Dataset (GED) as our primary source for different conflict indicators.[11] This dataset includes geocoded information (based on media reports) on the "best estimate of total fatalities resulting from an event" (Sundberg and Melander, 2013; Croicu and Sundberg, 2015), with specific information about the types of fighting (one-sided, state-based, non-state) and the actors involved as illustrated in Table 3.8.[12] In our sample period, 94% of the events covered by UCDP are

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[10]As the ISAF "is not directly involved in the poppy eradication or destruction of processing facilities, or in taking military action against narcotic producers" (see ISAF mandate: http://www.nato.int/isaf/topics/mandate/index.html), the authors argue that Western casualties are more exogenous compared to the total number of casualties. Nevertheless, the 2004 United Nations Security Council Resolution 1563, for instance indicates that Western forces were involved in eradication during the 2002-2007 period (see „extending central government authority to all parts of Afghanistan, [...], and of combating narcotics trade and production," http://unscr.com/en/resolutions/doc/1563, accessed June 4,2018).

[11]We prefer this over data from the Armed Conflict Location & Event Data Project (ACLED), because ACLED is only available for the 2004-2010 period, thus reducing the sample by half, and is reported to be less reliable for Afghanistan (e.g., Eck, 2012).

[12]An event is defined as "[a]n incident where armed force was [used] by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Sundberg and Melander, 2013; Croicu and Sundberg, 2015). These battle-related deaths include dead civilians and deaths of persons of unknown status. For more details see Appendix 3.A.

fights between the Afghan government and the Taliban (so-called state-based violence). Less than 4% of all cases are classified as one-sided with the Taliban as the perpetrator and civilians as the victims. We differentiate between these different types in Section 3.7.

Our analysis is at the district level (ADM2). There are 398 districts, which belong to 34 provinces (ADM1) as presented in Figure 3.11 in Appendix 3.C. There is no perfect threshold in the casualty number that identifies a conflict as relevant.[13] We report results for thresholds of 5, 25, 50, and 100 battle-related deaths (BRD), and the log of the number of BRD per district-year as a continuous conflict measure.[14] Weidmann (2015) documents some under-reporting of media-based conflict data in areas with low population density compared to the SIGACTS data (Significant Activities), which are based on military reports and not publicly available. Media-based datasets could also be downward biased with regard to the intensity of conflict, especially in high conflict areas. Using different thresholds, each somehow arbitrary, along with a continuous measure of BRD alleviates these concerns, ensures transparency and allows us to capture conflict at the local level in a comprehensive way. We also use population-weighted and unweighted suitabilities to test potential differences with regard to population density, a jackknife approach (i.e., drop one province at a time) to account for the influence of high-conflict areas, and consider different conflict types in robustness tests. None of this suggests that the choice of conflict indicator introduces a systematic bias in our results.[15] To further verify the reliability of the UCDP GED data, Figure 3.23 in Appendix 3.G shows a high correlation with a subjective conflict indicator derived from the NRVA household survey.

Opium suitability index: We exploit a novel data set measuring the suitability to grow opium based on exogenous underlying information about land cover, water availability, climatic suitability, and soil suitability. Conceptually, the index is comparable to suitability indices by the Food and Agricultural Organization (FAO). It was developed in collaboration with UNODC, and is described in detail in a publication in a geographical science journal (Kienberger et al., 2017). The left hand side of Figure 3.1 plots the distribution of the opium suitability index across Afghan districts. While an index of one would indicate perfect suitability in terms of land cover, water availability, climatic suitability, and suitability of soils, an index of zero means that the district is least suitable for growing opium. Given that it is generally possible to grow opium in many parts of

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[13]Although it is standard at the macro level to only use the two thresholds of 25 and 1000, the latter threshold is evidently not appropriate for an analysis at the district level.

[14]Berman and Couttenier (2015) use a one-BRD threshold. However, the grid cell level at which they work is of a much smaller size than the ADM2 level. For our size, we consider five BRD a good threshold to detect small conflict, whereas a one-BRD threshold might suffer from misreporting and falsely coding conflict.

[15]We also use the SIGACTS data on the events direct fire, indirect fire, and improvised explosive device (IED), for which we received access by Shaver and Wright (2016) to verify the reliability of UCDP GED data. The results for all three types of events point in the same negative direction and are available on request.

Afghanistan and that it is "renewable," this suitability can also be understood as the actual "resource" that varies across districts. We weigh the suitability with the population density, but this does not affect our results.

FIGURE 3.1

Distribution of crop suitability across districts (weighted by population)
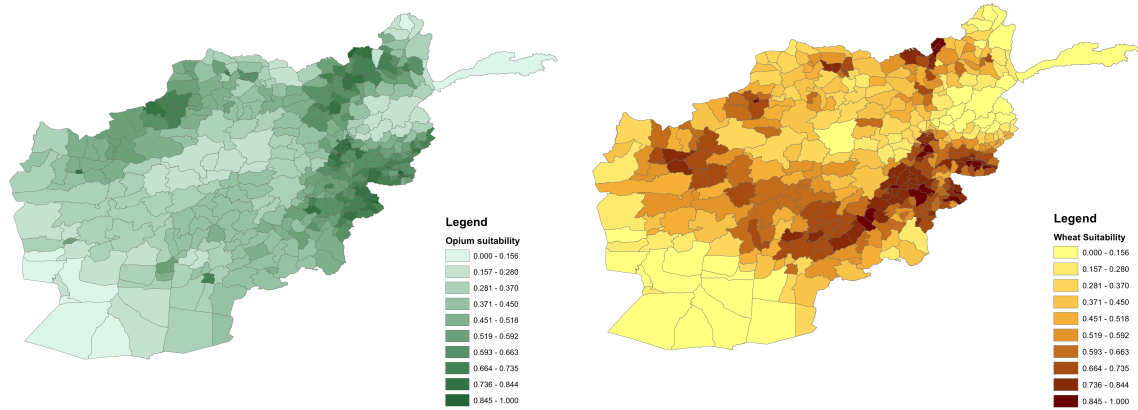
(A) Opium suitability (Kienberger et al., 2017)   (B) Wheat suitability (*FAO GAEZ*)

Drug prices: For the measure of the external price shock we rely on international drug price data from the European Monitoring Center for Drugs and Drug Addiction (EMCDDA), which provides data for a large number of drugs in European countries (also including Turkey that is crossed by many drug trafficking routes). We take the mean prices for each country-year and calculate the average across all countries for which data are available, in order to eliminate the effects of country-specific shocks. The average variation should be a clearer estimate of global demand shocks.[16] Local price data on opium are derived from the annual Afghanistan Opium Price Monitoring reports by UNODC. The international price is the price for heroin, which is an opiate derived from morphines that are extracted from the opium poppy.[17]

Drug cultivation and drug revenues: Information on actual opium cultivation and opium yield is retrieved from the annual UNODC Opium Survey reports. District level cultivation are estimates derived from province level cultivation data from UNODC survey questionnaires and remote sensing methods. We calculate actual opium production at the district-year level from opium cultivation and the respective yields, which vary by year

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[16]In the robustness section (Section 3.7), we try alternative definitions by taking price deviations from the long-term mean. Our results are not affected by this choice.

[17]EMCDDA provides data on white and brown heroin. The bulk of heroin consumed in Europe is brown heroin, which is also much cheaper than white heroin. Besides being less common, white heroin is only reported by a small number of European countries and is also likely to be consumed in fewer countries. Both types are products of opium poppies and the correlation between white and brown heroin prices is 0.49.

and region. Opium revenues equal opium production in kg multiplied with the yearly Afghan farm-gate prices (fresh opium at harvest time, country-average) in constant 2010 Euro/kg. For the regression analysis we take the logarithm of the revenues.

Survey Data: We use the NRVA survey waves conducted in 2005, 2007/08 and 2011/12 (CSO, 2005, 2007/08, 2011/12) to better test the opportunity cost channel at the household level. These are nationally representative and include between 21,000 and 31,000 households as well as covering from 341 to 388 of the 398 official districts in Afghanistan. We harmonize data from three different waves to construct indicators based on food consumption and expenditures, household assets, and a self-reported measure on the household's economic situation.

Other data: As covariates we take the average luminosity computed using nighttime satellite data as a proxy for development (Henderson et al., 2012) and population (Henderson et al., 2018), which is computed using estimates from the Gridded Population of the World, Version 4 (GPWv4), dataset. Development and population are potentially affected by our outcome variable conflict and thus potentially bad controls. Accordingly, we take the (pre-determined) lagged values, and use them only for robustness. Using district-fixed effects and only within-district variation ensures that our main estimations are not affected by cross-sectional differences in population size. Similar to Harari and La Ferrara (2018) we use an index that captures inter-annual variations in drought conditions, the vegetation health index (VHI) provided by the FAO (Van Hoolst et al., 2016). In contrast to precipitation data (which are of low quality in Afghanistan) this requires no assumptions about the linearity of the effect and directly measures drought conditions. Additional time-invariant data on geographic conditions and further potentially relevant factors are used to identify mechanisms and for robustness tests. To analyze heterogeneous effects, we georeferenced district level information about opium production in labs, opium markets, and trafficking, as well as on military and government presence that are explained in the respective sections (Sections 3.6 and 3.7). All variables and their sources are described in detail in Appendix 3.A and descriptive statistics are reported in Appendix 3.B.

3.4. Identification strategy

Estimating equation and identification

Our baseline specification focuses on the reduced-form intention-to-treat (ITT) effect. We prefer this specification because opium cultivation data are district level estimates by UNODC derived from province level data that might exhibit considerable measurement error.[18] To circumvent these concerns we combine temporal price variation with district level data on the suitability to grow opium to compute the reduced-form effect. In addition, we use actual opium revenues (and cultivation) to assess the size of our effect in an IV setting. This approach resembles Bartik- or shift-share-like instruments that combine cross-sectional variation with variation in a times series (as in Nunn and Qian, 2014, and in Chapters 1 and 2). Our baseline equation at the district-year level over the 2002 to 2014 period is:

$$conflict_{d,t} = \beta\, opium\, profitability_{d,t-1} + \zeta\, wheat\, shock_{d,t-1} + \tau_t + \delta_d + \tau_t\delta_p + \varepsilon_{d,t}. \quad (3.1)$$

Standard errors are clustered at the district level, but results are robust to different choices including the use of province level clusters and a wild-cluster bootstrap approach (Appendix 3.F, Table 3.32). The outcome variable, $conflict_{d,t}$, is the incidence or the intensity of conflict in district d in year t based on the different thresholds. Our "treatment" variable $opium\, profitability_{d,t-1}$ measures the relative extent of the shock induced by world market price changes in t-1 conditional on the exogenous district-specific suitability to grow opium in district d. More specifically, $opium\, profitability_{d,t-1}$, and analogously $wheat\, shock_{d,t-1}$, are defined as:

$$opium\, profitability_{d,t-1} = drug\, price_{t-1} \times opium\, suitability_d,$$
$$wheat\, shock_{d,t-1} = wheat\, price_{t-1} \times wheat\, suitability_d.$$

We include wheat-related income shocks ($wheat\, shock_{d,t-1}$), since wheat is the main (legal) alternative crop that farmers grow throughout Afghanistan. This allows us to identify differential effects for two types of income shocks, one affecting the main legal and the other the main illegal crop. $Wheat\, shock_{d,t-1}$ uses variation in the international wheat price interacted with the suitability to produce wheat ($wheat\, suitability_d$). The effect of wheat price shocks on income is ambiguous, as Afghanistan also imports large amounts of wheat. In fact, Afghanistan contributes less than 1% to the global wheat

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[18]As stated by the UNODC (2015, 63) "[d]istrict estimates are derived by a combination of different approaches. They are indicative only, and suggest a possible distribution of the estimated provincial poppy area among the districts of a province." Assuming the measurement error is normal, this would bias our estimations towards zero. In case the precision of estimates is also affected by conflict and suitability, however, the bias is hard to predict.

supply, which is why we follow the literature and consider the international price as exogenous (e.g., Berman and Couttenier, 2015). Note, that our main results regarding *opium profitability* all hold without including this variable.

FIGURE 3.2

Time structure of price effects
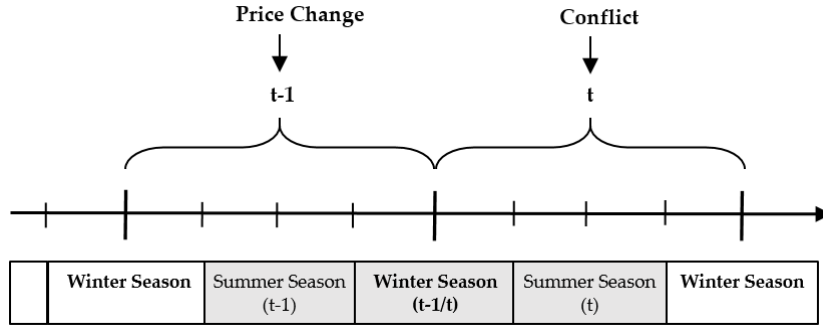
Market price changes can plausibly influence opium cultivation and revenues in the same and the following year, as Figure 3.2 illustrates. There are two main growing seasons for opium in Afghanistan, the winter season starting in fall and the summer season starting around March (Mansfield and Fishstein, 2016). Our preferred specification assumes the largest effect of *opium profitability* on conflict one year later. Price changes in (*t-1*) are most likely to affect cultivation decisions in summer(*t-1*), winter(*t-1/t*) and summer(*t*), as well as affecting labor demand and revenues in both (*t-1*) and (*t*). Using prices in (*t-1*) accounts for the fact that producers require time to update their information set and adjust production, and often receive their remuneration in advance (Mansfield and Fishstein, 2016).[19] Taking contemporaneous prices in (*t*) is conceptually difficult with yearly price and conflict averages. Using the price in (*t*) would introduce reverse causality, as price changes later in the year can be affected by conflict earlier in the year. Moreover, it is unclear how quickly changes in world market prices transmit into changes at the local Afghan level. For these reasons, we prefer the lagged value, however, using prices in (*t*) yields comparable results as shown in Appendix 3.E.

It might be problematic to use the international opium (heroin) price p_{t-1}^O because Afghanistan contributes a large share of the global opium production (UNODC, 2013b). More specifically, we would be worried about omitted variables OV_{t-1} that affect both opium production and p_{t-1}^O, as well as $conflict_{d,t}$, differentially, conditional on the time-invariant *opium suitability*$_d$. Problematic omitted variables would be any time-varying factors that affect high and low suitability districts differentially and follow a similar pattern as the heroin price.

One example of such an omitted variable could be changes in district-specific government institutions. Assuming that well-working government institutions are bad for

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[19]Caulkins et al. (2010, p. 9) also suggest that "the largest driver of changes in hectares under poppy cultivation is not eradication or enforcement risk, but rather last year's opium prices."

opium production and trafficking, the effect of better institutions on production would be more negative in highly suitable regions. If good institutions also lead to less conflict, omitting this variable would bias our estimated effect upwards. Note that as we find a negative relationship we are more concerned about a potential downward bias. This could, for instance, occur if endogenously decided eradication campaigns correlate positively with the heroin price, and are less likely to take place in highly suitable areas that could be more risky to enter for government forces. If eradication campaigns also cause conflict, this would lead to a spurious correlation biasing the coefficient for $opium\,profitability_{d,t-1}$ downwards. Based on the notorious ineffectiveness of eradication policies (see, Rubin and Sherman, 2008; Felbab-Brown, 2013; Mejía et al., 2015), this possibility seems rather unlikely, yet there could be other biasing factors.

Note that we are less worried about overall opium supply shocks in Afghanistan. While these shocks would of course affect world market prices, this variation is captured by year-fixed effects. The year-fixed effects τ_t capture, for instance, yearly changes in crop diseases or shifts in anti-drug policies to the degree that they affect all districts in Afghanistan in the same way. District-fixed effects δ_d account for time-invariant unobservable characteristics at the district level. Province-times-year-fixed effects $\tau_t\delta_p$ account in addition for time-varying unobservables at the province level. These can include institutions provided by ethnic group or tribal leaders or warlords, which are in many provinces more important than the central government. As a large share of the drug trade is organized at the ethnic or provincial level (Giustozzi, 2009), changes in those institutions plausibly affect both conflict and opium production. Identification in this setting then relies only on within-province variation in a particular year due to differences in opium suitability.

Moreover, although our main specification does not rely on control variables, Appendix 3.F shows that the results also hold with using $X_{d,t}$ and $X_{d,t-2}$, vectors of district level time-varying covariates including climate conditions and some baseline covariates frequently used in other conflict regressions such as luminosity (as a proxy for GDP) and population. Climate conditions are exogenous to conflict and can plausibly be used as contemporaneous values. We lag the luminosity and population twice with the aim to use a pre-determined value and to mitigate the bad control problem.

Table 3.9 in Appendix 3.B shows that low and high suitability differ in some covariates X_d, as for instance in the distance to Kabul or ethnic group distribution. We would be worried if time-varying omitted variables would affect opium prices and conflict differentially depending on those covariates. A problematic case would be if high suitability districts, for reasons unrelated to opium, experience an increase in conflict over the sample period, and low suitability districts exhibit no change. The decrease in prices would then on average be associated with increasing conflict in highly suitable districts, even though it was caused by other characteristics that distinguish high from

low suitability districts. By interacting the complete set of time-invariant covariates X_d both with a linear time trend or flexibly with time-fixed effects τ_t, we capture any such bias to the extent that it is based on those observable differences (see Appendix 3.F).

Next, we would be concerned if non-linear long term trends in prices correlate with long term trends in conflict that are driven by omitted variables and differ between low and high suitability districts. We thus take the issues raised in, e.g., Goldsmith-Pinkham et al. (2018) and Christian and Barrett (2017) about the role of cross-sectional differences and spurious non-linear trends seriously. However, section 3.6 verifies that, even though prices decline in early years as well, the trends only start to differ after an exogenous change in Western policy around 2005, which increased the reliance of the local population on opium revenues. Moreover, we alleviate this concern in different ways. First, Appendix 3.F shows the results with de-trended opium prices, which exhibit less variation but support the main finding. Second, we randomize the prices across years and find that random assignment yields to no significant relationship with coefficients being distributed around zero. Third, section 3.4 examines price trends and suggests that long term trends are mostly driven by demand rather than supply factors. Nonetheless, the next section introduces an additional identification strategy exploiting the relationship of opium with complementary drugs to alleviate remaining concerns.

Identification using price changes

In order to assess the direction of any remaining potential bias, we gather price data for a variety of drugs that are used as complements to heroin. We exploit the fact that prices of complements depend on the same demand shifters (DS), but the biasing effect of a district level change in opium supply q^O_{t-1} (potentially caused by an omitted variable) points in the opposite direction for the complement price than for the heroin price because of the negative cross-price elasticity. More formally,

$$p^O_{t-1} = f(\overset{(+)}{DS'_{t-1}}, \overset{(-)}{q^O_{t-1}}, \overset{(+)}{q^C_{t-1}}),$$

$$p^C_{t-1} = f(\overset{(+)}{DS'_{t-1}}, \overset{(+)}{q^O_{t-1}}, \overset{(-)}{q^C_{t-1}}).$$

Accordingly, a bias resulting from problematic omitted variables that affect opium supply would distort the estimated coefficient b in different directions for the opium and complement prices. Formally, the expectations for a coefficient estimate from a regression on conflict in the presence of a bias become:

$$E[b^O] = \beta + \gamma \times \frac{\rho(opium\, price_{t-1} \times suit_d, OV_{t-1} \times suit_d)}{\text{Var}(opium\, price_{t-1} \times suit_d)}, \tag{3.2}$$

$$E[b^C] = \beta \times \frac{\sigma^O}{\sigma^{\epsilon^C} + \sigma^O} + (-\varpi) \times \gamma \times \frac{\rho(complement\, price_{t-1} \times suit_d, OV_{t-1} \times suit_d)}{\text{Var}(complement\, price_{t-1} \times suit_d)}. \tag{3.3}$$

b^O and b^C are the estimates using the opium and complement price, whereas β is the "true" parameter. σ^O is the standard deviation of the opium price, and ϵ^C indicates the influence of exogenous supply side shocks on the complement price. ϖ is a parameter that is positive if the cross-price elasticity is negative, i.e., if two goods are complements $(-\varpi) \le 0$. Hence, the equations show two things. Attenuation bias moves the complement estimate towards zero, as $\frac{\sigma^O}{\sigma^{\epsilon^C} + \sigma^O} \le 1$. At the same time an omitted variable would bias the complement coefficient in the opposite direction as compared to the opium coefficient. Appendix 3.D provides the derivation and explains the necessary assumptions.

We show that for this comparison to be helpful, three main criteria need to be fulfilled.

1. We need to be able to identify complements for which the negative cross-price elasticity with opium is sufficiently high.

2. We require complements for which large supply side shocks are unrelated to district level supply side shocks for opium in Afghanistan. This enables us to treat supply side shocks as random noise (ϵ^C), which does only attenuate the coefficient towards zero.

3. The degree to which drug prices are affected by common demand shifters (a change in overall income of consumers, a shift in consumers' preferences about drugs, or the number of buyers in the drug market) must be sufficiently high relative to ϵ^C.

To the extent that these criteria are fulfilled, we can derive the following: If both estimates have the same sign this strongly signals that the true effect also points in the same direction due to the opposing directions of the omitted variable bias. If both exhibit a negative coefficient, we can distinguish between two scenarios, a) a downward or b) an upward bias in the opium estimate. In case a) the complement coefficient is more positive than the opium coefficient, because both attenuation bias and OVB move it towards zero. If the complement coefficient is more negative than the opium price, this suggests that the opium coefficient is upward biased (scenario b). In this case, the opium estimate can be interpreted as an upper bound of the true negative effect. Although the intuition is provided in Equations 3.2 and 3.3, we also validate this strategy using a Monte Carlo simulation, described in detail in Appendix 3.D.

Regarding the identification of complements, we exploit the fact that drugs are classified as stimulants (uppers) or depressants (downers), with heroin being in the latter

category. There is a consensus among experts about a high share of polydrug users, in particular users that combine a stimulant and a depressant (EMCDDA, 2016). We gather data on changes in the prices of three depressants that are regarded as complements to opium: cocaine, amphetamine, and ecstasy (EMCDDA, 2016). Leri et al. (2003, p. 8) conclude that the "prevalence of cocaine use among heroin addicts not in treatment ranges from 30% to 80%," making it a "strong" complement. This can take place in form of "speed-balling" (mixing heroin and cocaine), consuming the two jointly or with a time lag (e.g., weekend versus workday drug consumption). Moreover, cocaine supply is also most clearly exogenous to supply shocks in Afghanistan, as production exclusively takes place in South America and there is nearly no overlap with regard to trafficking routes (suggested by low cocaine seizures in Asia, see, UNODC, 2013b).[20] Thus, cocaine most clearly fulfills conditions 1 and 2.

One disadvantage of focusing on one complement is that supply side shocks for any individual complement ϵ^C could have a relatively large influence compared to common demands shifters. Using an index of the average normalized prices of the three upper drugs instead has the advantage of reducing the influence of individual supply side shocks, making it more likely that condition 3 is fulfilled. Hence, we use the cocaine price alone as well as a complement index. We find comparable results using either the cocaine price or the index. We will also argue that the movement of prices (and expert opinions) indicate that long term price changes are more strongly driven by demand-side factors, which also alleviates concerns about omitted variables and their suitability-specific effect on opium supply and prices.[21]

International prices, local prices, and local revenues

In the following, we (i) discuss the movements of prices over our sample period, (ii) show that international prices of complements correlate positively with the international heroin price, (iii) international prices translate into economically relevant changes in the local price in Afghanistan and, (iv) that they affect opium revenues at the district level in Afghanistan. Figure 3.3 displays the variation in the international prices of heroin, cocaine, the complement index, as well as the Afghan price in constant 2010 Euro per gram. The local opium farm-gate prices at harvest time in Afghanistan are the ones most likely to be driven by opium supply side effects in Afghanistan. The international heroin

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[20]There is also no evidence suggesting that ecstasy and amphetamines are produced in Afghanistan, but there is vague evidence on amphetamine-type stimulants (ATS) being seized in the Middle East (UNODC, 2013b). Afghanistan is never mentioned in this regard and not included in the list of countries of provenance (UNODC, 2013b).

[21]The price of substitutes can also be positively correlated with the opium price as both prices increase if general demand, preferences or the number of buyers increases. However, when opium supply decreases, the opium price would increase and the price of the substitute would also increase. Hence, we cannot distinguish the demand shock from the second, potentially endogenous, relationship with Afghan opium supply, as both point in the same direction.

price is a result of demand and supply in both the world market and within Afghanistan. Finally, the complement index captures shifts in demand for the three complementary drugs and largely eliminates individual supply shocks by using the average.

The graph provides several important insights. First, there are variations between the years, but overall all prices decline over time. The common pattern suggests that on average the development of prices over time is more strongly driven by common demand factors, and not by a shock to an individual drug. Interviews with experts at EMCDDA also support this view. Second, there is an overall positive correlation between the international heroin price, the complement index and the cocaine price (significant at the 1% level), validating our assumption of the cross-price elasticity being sufficiently high. As expected, the index exhibits less variation than the cocaine price. Third, local Afghan prices also correlate positively with the international heroin price. This indicates that despite end-customer market prices being multitudes higher than local prices, international price changes also translate into economically meaningful changes at the country of origin.

FIGURE 3.3
Variation in international and local prices over time
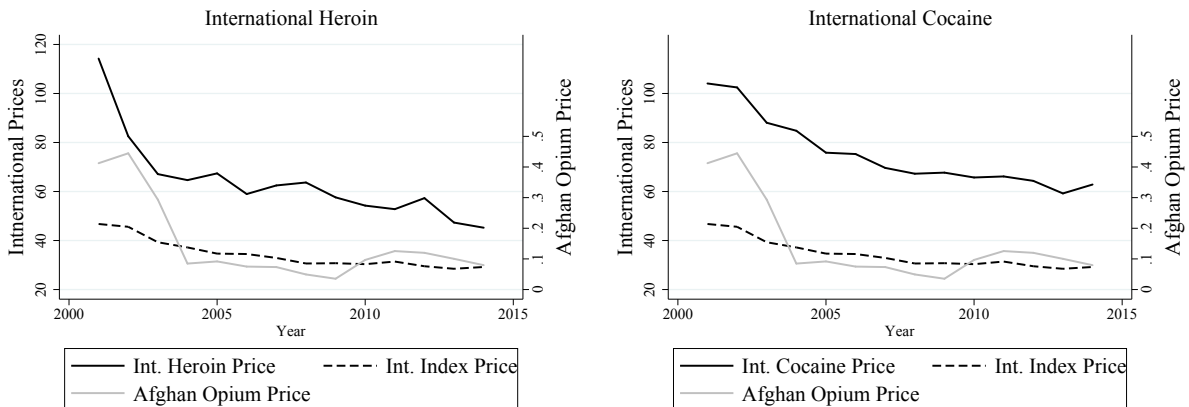
After showing that the international heroin price is positively correlated with the international complement price index and with the local opium price, we proceed and quantitatively test whether international price changes translate into changes in actual opium revenues at the district level.[22] Some reports indicate that an amount of opium worth 600 US Dollars can have a street value of more than 150,000 US Dollars.[23] Consequently, we want to see whether market consumer price changes have a statistically and economically significant effect at the local level. We therefore run the empirical model as defined in equation (1) but with the actual revenues from opium cultivation as the dependent variable (in logarithms). Opium revenues are defined as the production in kg

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[22]In Appendix 3.F in Table 3.26 we replace revenues with opium cultivation in hectares.

[23]See http://www.rollingstone.com/politics/news/afghanistan-the-making-of-a-narco-state-20141204, accessed June 14, 2018.

multiplied with the Afghan opium farm-gate price at harvest in constant 2010 Euro/kg. Table 3.1 presents the results considering lagged effects in column 1. In column 2 we consider both lagged and contemporaneous effects by taking the moving average over (t) and (t-1).

TABLE 3.1

Effect of international price changes on opium revenues, 2002-2014

	Outcome: (t)	Outcome: (t) + (t-1)
	(1)	(2)
Opium Profitability (t-1)	2.336***	2.489***
	(0.827)	(0.749)
Wheat Shock (t-1)	-0.406	-0.123
	(0.460)	(0.418)
Number of observations	5149	5085
Adjusted R-Squared	0.482	0.565

Notes: The dependent variable opium revenues is in logarithms. Column 1 presents lagged effects. Column 2 reports lagged and contemporaneous effects by defining the outcome as the moving average, i.e., (revenues(t)+revenues(t-1))/2. Opium Profitability is defined as the interaction between the normalized international heroin drug price (in logarithms) and the suitability to grow opium. Standard errors clustered at the district level are displayed in parentheses. Significance levels: * 0.10 ** 0.05 *** 0.01.

We see that external price shocks, measured by the interaction of international heroin price with the suitability to grow opium, in line with our proposed mechanism lead to an increase in local opium revenues in the same and following year (compared to Figure 3.2). These results are significant at the 1% level in columns 1 and 2. Quantitatively, a 1% increase in the international heroin price leads to about a 2.4% increase in revenues for those districts where opium suitability reaches one (perfect suitability). For districts characterized by the mean suitability (0.53) the effect would roughly decrease by half (0.53*2.40=1.27), but the elasticity is still bigger than one.[24] As a placebo test, it is reassuring that despite a positive correlation in the two suitability indices, the wheat shock has no effect on opium revenues.

Visualizing the identification strategy

As our identification relies on the interaction term $opium\,profitability_{d,t-1}\ =\ drugprice_{t-1} \times suitability_d$, our setting resembles a difference-in-difference approach. The main effects of the two levels of the interaction term ($drug\,price_{t-1}$, $opium\,suitability_d$) are captured by the district- and time-fixed effects in our model. We expect the effect of international price shocks on opium cultivation and revenue to be larger in districts that

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[24]This estimation does not include province-times-year-fixed effects as the actual cultivation data, from which revenues are calculated, is gathered at the province level and district level data are estimated based on these underlying data (like any other estimation using cultivation data).

are more suitable to grow opium compared to districts with a low suitability. Figure 3.4 illustrates this with two maps showing the district level opium suitability overlaid with the distribution of conflict across Afghanistan for two selected years. 2004 is (and follows) a year of high prices and thus higher opium profitability (left graph) and 2009 a year of lower prices (right graph). It becomes immediately clear that lower prices are associated with more widespread and more intense conflict, and higher prices with less conflict, indicating support for the opportunity cost hypothesis at the country level. Our identification, however, relies on within-district variation over time conditional on suitability. This intuition becomes clear when comparing the relative change in conflict for different levels of opium suitability. Districts with a higher suitability experience a much higher increase in conflict when prices and opium profitability decline. This is most evident in the north, northeast, and east. Although these are only correlations, they help to understand the variation that we exploit in our analysis in the next section.

FIGURE 3.4

Intensity of conflict in districts with high and low opium suitability
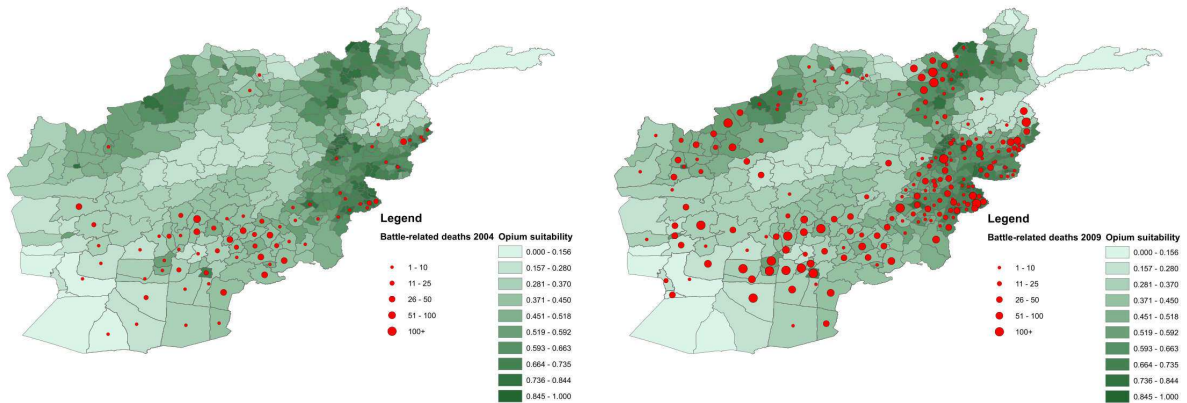
(A) Conflict (2004): High opium prices (t/t-1)   (B) Conflict (2009): Low opium prices (t/t-1)

3.5. Results

Main results

We now turn to our main results in Table 3.2. We report results for different dependent variables, where column 1 uses the continuous measure (log BRD) and columns 2 to 5 define conflict as a binary indicator with increasing thresholds of battle-related deaths. Panel A reports results using the interaction of the local opium price with the suitability to grow opium as the measure for opium profitability. In panel B we replace the local price with the international heroin price (our baseline specification), and panels C and D report results using the complement price index and for robustness the international cocaine

price. All regressions include only wheat shock and province-times-year-fixed effects as control variables. Our results do not rely on the inclusion of control variables as can be seen in Appendix 3.F, where we show that inferences are robust across various different specifications. Turning to the results, the regression coefficients are very much in line with our graphical inspection in Figure 3.4. Already when using the local opium prices, which introduces endogeneity, we find constantly negative coefficients. When turning to our baseline specification in panel B, the negative effect of the opium profitability on conflict intensity and incidence is more pronounced than in panel A. The coefficients are significant at the 5% to 10% level for the first four specifications. They turn insignificant when considering only conflict events with more than 100 deaths, which is what we expect given the low number of such high scale events and the higher degree of state dependence (as discussed in Chapter 1). A 10% increase in the international heroin price translates into 7% fewer battle-related deaths in perfectly suitable districts. Note that a price increase of 10% is only slightly above the average annual price change of 8.8%.

To verify whether this negative effect can be causally interpreted, we now turn to the results using our complement prices. In panels C and D we find that the point estimates using the complement price index and the cocaine price are both negative. The fact that both estimates are negative reassures us that the true effect is also negative. Furthermore, the fact that the estimates using the complement prices are more negative – and statistically significant at the 1% level in columns 1 to 4 – indicates that the coefficients using the heroin price are (marginally) upward biased and provide an upper bound of the true negative effect. Accordingly, the true effect might be more negative than the coefficients using the heroin price. For all further computations we proceed with this more "conservative" specification.

When turning to the main legal alternative crop – wheat – we observe a positive coefficient in most regressions. Though, contrary to opium price-related shocks, the point estimates of wheat price-related shocks sometimes switch signs and turn negative. Bearing in mind that contrary to opium, wheat is relatively less labor intensive and often also imported from abroad. The fact that most households are net buyers of wheat (Mansfield and Fishstein, 2016) and are thus negatively affected by price increases could explain the positive coefficients.[25]

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[25]Chabot and Dorosh (2007) use the NRVA household survey and state that in the 2003 wave calorie intake through wheat consumption amounts to 60% of total calorie consumption pointing to the high reliance on this crop.

TABLE 3.2

Main results using normalized drug prices, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Local Opium Price					
Opium Profitability (t-1)	-0.346***	-0.096***	-0.094***	-0.076**	-0.042**
	(0.107)	(0.033)	(0.032)	(0.029)	(0.018)
Wheat Shock (t-1)	0.366***	0.100***	0.095***	0.047	-0.013
	(0.120)	(0.037)	(0.035)	(0.031)	(0.017)
Adjusted R-Squared	0.649	0.502	0.484	0.454	0.311
Panel B: International Heroin Price (Baseline)					
Opium Profitability (t-1)	-0.675**	-0.167*	-0.191**	-0.147*	-0.040
	(0.296)	(0.090)	(0.085)	(0.075)	(0.037)
Wheat Shock (t-1)	0.307**	0.088**	0.077**	0.034	-0.010
	(0.123)	(0.039)	(0.036)	(0.031)	(0.019)
Adjusted R-Squared	0.649	0.501	0.484	0.454	0.310
Panel C: International Complement Price					
Opium Profitability (t-1)	-0.947***	-0.249***	-0.237***	-0.203***	-0.086**
	(0.308)	(0.094)	(0.086)	(0.076)	(0.041)
Wheat Shock (t-1)	0.221*	0.063	0.060	0.016	-0.023
	(0.128)	(0.040)	(0.037)	(0.033)	(0.020)
Adjusted R-Squared	0.651	0.502	0.484	0.455	0.311
Panel D: International Cocaine Price					
Opium Profitability (t-1)	-0.461**	-0.116*	-0.124**	-0.102**	-0.026
	(0.199)	(0.059)	(0.057)	(0.051)	(0.025)
Wheat Shock (t-1)	0.305**	0.087**	0.078**	0.033	-0.010
	(0.120)	(0.038)	(0.035)	(0.030)	(0.018)
Adjusted R-Squared	0.650	0.502	0.484	0.454	0.310

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) and the suitability to grow opium. The number of observations is equal across all panels (5174). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Instrumental variable

In a next step, we use IV regressions, where we instrument the endogenous variable (opium revenues) with two different IVs, opium profitability and a measure for climate conditions – the vegetation health index (VHI) (similar to Miguel et al., 2004; Nillesen and Verwimp, 2009). While the reduced form approach in Table 3.2 presents the ITT effect, we identify the LATE for compliers in Table 3.3. Having an alternative source of exogenous variation enables us to compare the LATE of the different instrumental variables. This step also allows us to quantify the size of the effect in an economically meaningful way. Note that we still prefer the reduced form results presented in Table 3.2, as the data on opium cultivation and thus revenues are estimates only and there might be non-random measurement error in the data. As in Table 3.1, we do not include province-times-year-fixed effects as district level opium revenue data are estimates from province level data.

Panel A of Table 3.3 reports second stage IV results where we instrument opium revenues with opium profitability measured by the interaction of the international heroin price with the suitability to grow opium. We find a negative coefficient for opium revenues in all columns in panel A, significant at the 10% level in columns 1 and 2 as well as close to significance at conventional levels in column 3. The IV results reveal that the opium profitability is a strong instrument as indicated by the Kleibergen-Paap F-statistic, which clearly exceeds the critical threshold of ten proposed by Staiger and Stock (1997). Parallel to panel A, we instrument opium revenues with the VHI in panel B and find our results to remain robust. In particular for columns 1 to 3 coefficient estimates are close across both panels, indicating that the LATEs are similar. In panel C we use both instruments jointly and again find quantitatively comparable effects. Results turn out to be significant in columns 1 to 4. The three estimates reported in column 1 across all panels show that an increase of opium revenues by 10% leads to a decrease in the number of battle-related deaths of about 2%.

The last specification with two instruments allows us to conduct an overidentification test, which helps to assess the validity of the instruments. The Hansen over-identification test-statistics support the validity of the instruments in all columns. Panel D presents corresponding first stage results. While opium profitability positively affects opium revenues as we already know from Table 3.1, the VHI negatively affects revenues, which is reasonable as droughts deteriorate cultivaton and yield of opium.

To sum up, we get similar results using two rather different sources of exogenous variation. This is reassuring regarding the quantitative size of the IV estimates, as well as for the validity of our main identification strategy. What is more, in Appendix 3.F we show that this finding holds when using a further IV, which is based on changes in legal opioid prescription in the United States (Table 3.30). Different combinations of the three

instruments point to similar results. We also show IV results for a different timing and for opium cultivation in Appendix 3.F.

TABLE 3.3

First and second stage IV results for opium revenue (t-1), 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Opium Profitability (t-1) as IV					
(log) Revenue (t-1)	-0.153*	-0.044*	-0.040	-0.018	-0.004
	(0.083)	(0.025)	(0.025)	(0.019)	(0.008)
Number of observations	5104	5104	5104	5104	5104
Kleibergen-Paap F stat.	16.382	16.382	16.382	16.382	16.382
Panel B: VHI (t-1) as IV					
(log) Revenue (t-1)	-0.184*	-0.049	-0.049	-0.052*	-0.016
	(0.107)	(0.032)	(0.031)	(0.027)	(0.014)
Kleibergen-Paap F stat.	9.634	9.634	9.634	9.634	9.634
Panel C: Opium Profitability & VHI (t-1) as IV					
(log) Revenue (t-1)	-0.162**	-0.045**	-0.042**	-0.027*	-0.007
	(0.071)	(0.021)	(0.021)	(0.016)	(0.008)
Kleibergen-Paap F stat.	11.753	11.753	11.753	11.753	11.753
Hansen J p-val.	0.800	0.896	0.806	0.226	0.371

Panel D: First stage results			
	Panel A	Panel B	Panel C
Opium Profitability (t-1)	2.922***		2.798***
	(0.722)		(0.721)
VHI (t-1)		-0.013***	-0.012***
		(0.004)	(0.004)

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Taken together, we find that opium profitability is an important determinant of conflict incidence and intensity in the ITT and IV estimation. Our findings are in line with the results for positive income shocks in Berman and Couttenier (2015) and they support the conclusions in Dube and Vargas (2013) that the labor intensity of a resource compared to alternatives is a decisive factor. However, our results seem to be at odds with the conclusion in Mejia and Restrepo (2015) that an income shock for an illegal resource is related to more conflict. While coca has a similar labor intensity as the alternative crops cacao, palm oil, and sugar cane in Colombia (Mejia and Restrepo, 2015), opium cultivation is much more labor-intensive than all alternative crops. The next section will

further elaborate on the role of local monopolies of violence and the absence of group competition as potential explanations for the differences, suggesting that illegality per se is not the decisive factor moderating the effect on conflict. We will also dig deeper into identifying whether the effect is driven by increased opportunity costs of fighting by looking at household level survey data.

3.6.   Mechanisms and transmission channels

Opportunity costs at the household level

Whereas the tests above provide an indication of the potential profits in a particular district, a second important question is to what degree individual households and farmers actually benefit from a higher opium profitability. To exploit this individual dimension we use different waves of an Afghan nationally-representative household survey, the National Risk and Vulnerability Assessment (NRVA). We construct several indicators of households' living standards, in accordance with the literature. This allows us to analyze whether opium profitability translates into better living standards, which would provide evidence for the opportunity cost hypothesis. Figure 3.5 plots the coefficients for opium profitability for seven different regression models with the outcome variable indicated in the legends. We find evidence that dietary diversity increases when households experience a positive opium profitability and that more households are food secure. The positive effect of a higher profitability of opium is also visible when we consider food expenditures.[26]

We turn to indicators that are not as volatile as food consumption. In years following high opium prices, households in districts with a higher opium suitability benefit more from the price increases also in terms of assets that they hold. The last indicator "Economically Improved" is a self-reported measure, which turns out to be affected in the same direction as the other indicators of living standards. If households are better off economically, there is less need to fight as the opportunity costs of fighting do indeed increase with a higher opium profitability. The corresponding regression results are presented in Table 3.17.[27] In line with our main findings at the district level and our hypothesized mechanisms, the household level opportunity costs of fighting increase with a higher opium profitability. We do not think it is feasible to distinguish whether factors at the household or group level are more important. Rather we provide support for mechanisms at both levels.

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[26]We also construct food expenditure adjusted for spatial price differences using the Paasche or Laspeyres price indexes, since households in different districts face different prices. Results are robust to this choice as can be seen in Table 3.17.

[27]Results are also robust when accounting for household survey weights as presented in Figure 3.14.

Figure 3.5

Effect of opium profitability (t-1) on living standard indicators in year (t)
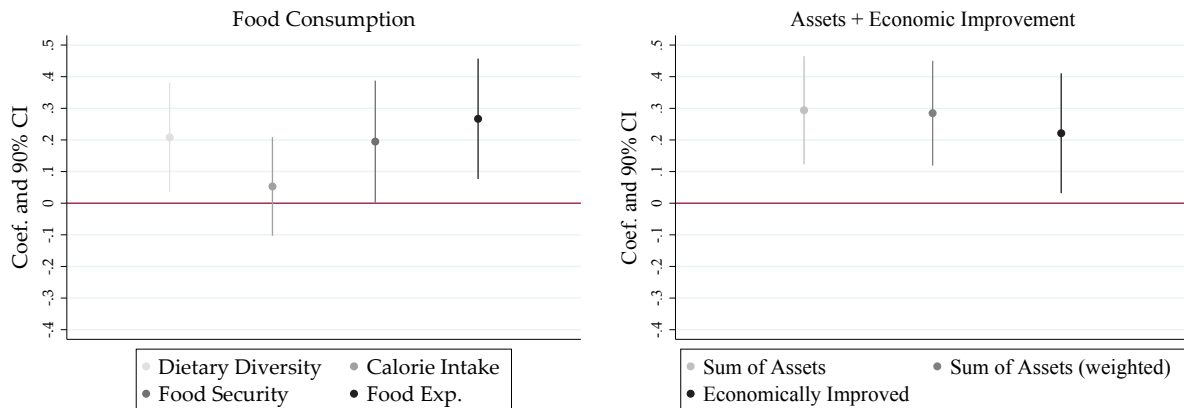
Opportunity costs and group competition at the district level

If there was competition among producers (between cartels or rival groups), we would expect that in districts which feature not only raw production but also intermediate steps along the value-chain (like processing, trading or trafficking), rents associated with opium and thus the gains from fighting are higher. In line with contest theory, the conflict-decreasing effect of positive income shocks would be relatively smaller in these districts. In contrast, if there was no or little violent group competition, higher profits would increase the opportunity costs of fighting even more in those districts that can extract a larger share of the value added. To be able to test this formally, we require proxies for the potential share of value added per district.

For this endeavor, we georeference data on whether a district contains a heroin or morphine lab, an opium market (major or sub-market) or whether it is crossed by potential drug trafficking routes. Figure 3.6 shows some of the data, and Appendices 3.A and 3.H provide all sources. The information is to a large extent based on UNODC reports. Profit margins are higher further up the production chain, markets create additional jobs and revenue, and trafficking routes allow raising income through some form of taxation or road charges. While it is important to keep in mind that there is no reliable information about yearly changes in trafficking routes and opium markets or labs, it is more precise to think of these variables as proxies. Nevertheless, we find it plausible that with little eradication efforts and limited state capacity, most of the locations and trafficking routes would remain relevant throughout the sample period. In particular, we create four indicators measuring the existence or sum of markets and processing labs in a district and whether a district is on a plausible trafficking route that would not need to cross areas of other ethnic groups.

As a second group of measures, we proxy for the role and connectivity of a district in the whole drug production and trafficking network using a market access approach adapted from Donaldson and Hornbeck (2016). The assumption is that in addition to capturing a larger share of value added, when a district itself features more markets, being surrounded by other districts with many markets increases the district's probability to extract rents (i) when transporting the raw product to markets, (ii) processing it in laboratories, and (iii) when trafficking the final product out of the country. This measure takes account of production chains and the interconnectedness of the production network, which should provide a more precise measure of potential profit opportunities and extractable rents related to the opium economy. We also compute more common market access variables using economic development (or population) as proxies for the economic importance of districts as consumer markets. As the importance of Afghan consumers for opium-related profits is negligible, a significant interaction with these placebo measures could indicate that our drug market access captures a spurious relationship and not meaningful variation in the share of extractable rents from the opium economy. Market access for a district i is computed as $MA_i = \sum_{j=1}^{N} dist_{i,j}^{-\theta} W_j$. W_j is the importance of district j proxied using either the number of drug markets or mean luminosity (or population). $dist_{i,j}$ are the distances between the district and the other districts and θ is the factor discounting other districts that are further away. We use a factor of 1 as in Donaldson and Hornbeck (2016). To take account of the topography and mountainous terrain in Afghanistan, we compute distances using the two-dimensional road network (Market Access 2D) as well as a three-dimensional road network when adjusting for elevation (Market Access 3D).

Table 3.4 begins with considering interactions with variables that signal if a district is able to extract more or less of the value-added along the production chain. The results in panel A indicate that the link between the profitability of opium production and conflict is more pronounced in districts that account for a potentially larger share of the value chain. This is visible in the negative interaction effects for all four indicators. All coefficients are negative, and with the exception of the indicator focusing only on laboratories significant at the 5% or 1% level. Panel B presents interaction results using the market access measures. In line with our hypothesis, we find a negative interaction effect when using the proxy computed specifically for the drug market, but no relationship when computing the indicator based on luminosity as a proxy for general economic development. Although the measures employed might contain measurement error, the consistent results across all indicators suggest that the conflict-reducing effect is driven by opportunity costs. It also further suggests that there is no large scale violent group competition about the most profitable districts.

There are at least two possible explanations. First, if the producers are at the same time the local leaders of a rebel group (the Taliban) they are facing a trade-off between

FIGURE 3.6
Mechanisms and channels

(A) Market access using opium markets

(B) Ethnicities, military & Taliban (1996)
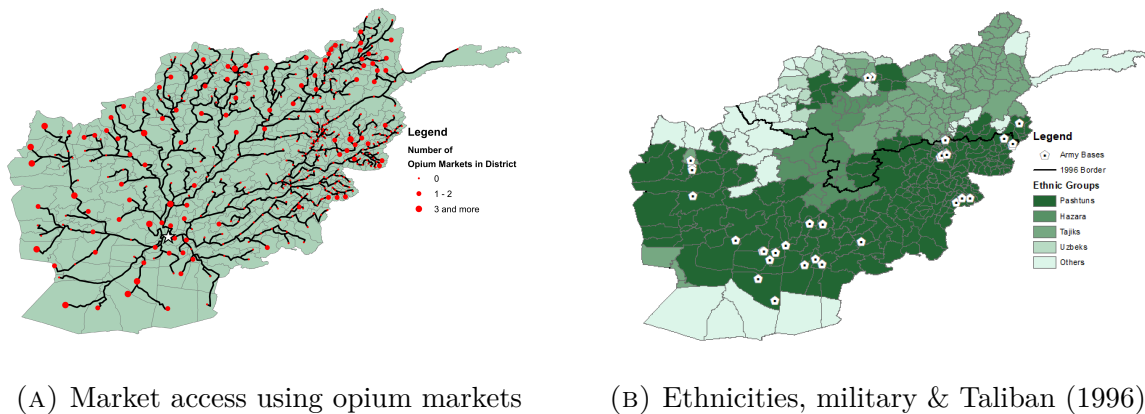
Notes: On the left hand side, the dots indicate district-specific centroids, and the black lines are the shortest road connections to the other centroids in the network. To compute market access, the same computation is done for every centroid in the district, leading to different optimal road connections. The distances are then used as weights and multiplied with the importance of the respective network members, in this case the number of drug markets. Sources: UNODC (2016), Open Street Map and Afghanistan Information Management Service (AIMS).
The right hand side map shows the four major ethnic groups in Afghanistan (Source: GREG). The white symbols with the black dots indicate the location of a foreign military base, for which we could track location, opening and closing date (sources in detail in Appendices 3.A and 3.H.). The area south of the thick black line was controlled by the Taliban prior to 2001 (Dorronsoro, 2005).

the gains from opium production and the gains from fighting the Afghan government or Western forces. Fighting or attacks in the same district are harmful for production by impeding works in the field, destroying production sites or drawing attention and thus increasing the likelihood of eradication measures. All else equal, a higher profitability of opium production relatively increases the incentives to maintain peace (or at least some form of truce). Second, the fact that these districts cover additional steps in the production chain also means that more workers benefit from the increases in profitability, either through more jobs or higher wages, leading to a larger increase in opportunity costs of fighting (respectively decrease when the price drops).

Mechanisms at the group level

The existing qualitative academic literature as well as reports, newspaper articles, and our previous results suggest no large scale (violent) competition amongst suppliers in Afghanistan.[28] Areas and the respective drug production as well as the trafficking process are either controlled by the Taliban and the local elites cooperating with them, or by the (internationally recognized) government, the Western forces and other groups associated

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[28]Note that this does not mean there is no competition at the small-scale level between individual farmers and sharecroppers. What is important is that there is a local (district or province level) elite that has established control over the district.

TABLE 3.4

Opportunity costs proxied by share of value added, 2002-2014

	(1)	(2)	(3)	(4)
	Panel A: Opium Markets, Labs, Smuggling			
	Major/Sub Market	**Sum of All Markets**	**Any Lab**	**Ethnic Traff. Route**
Opium Profitability (t-1)	-0.472	-0.480	-0.590*	0.105
	(0.314)	(0.306)	(0.312)	(0.358)
Opium Prof. (t-1)*X	-0.845**	-0.521**	-0.502	-1.734***
	(0.415)	(0.255)	(0.557)	(0.487)
	Panel B: Market Access (Network Approach)			
	Opium Market		**Luminosity**	
	2D	**3D**	**2D**	**3D**
Opium Profitability (t-1)	1.489	1.496	-0.902**	-0.899**
	(1.140)	(1.130)	(0.434)	(0.433)
Opium Prof. (t-1)*X	-0.470**	-0.474**	0.035	0.035
	(0.232)	(0.231)	(0.041)	(0.041)

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Opium Market 2D and 3D range between [2.24,11.23] and [2.21,11.22], so the marginal effects are always negative as well. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables X see Appendix 3.A. The number of observations is 5174 in every regression, the adjusted R-squared varies between 0.649 and 0.652. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

with it. We hypothesize that the degree of violent supplier (group) competition is decisive in moderating the relationship between resource profitability and instability. This also helps explain why our results point in the opposite direction than those from Angrist and Kugler (2008) and Mejia and Restrepo (2015) in the Colombian setting. Rather than the *de jure* legal status in its own right (as suggested in Mejia and Restrepo, 2015), local monopolies of violence and the actual enforcement of the law matter.

There are no reliable time-varying data about Taliban-dominated territory or actual government or Western military control for our whole sample period. Nevertheless, using a variable estimating contemporaneous group control would be endogenous anyway, and thus be problematic as part of an interaction term. We thus prefer to rely on time-invariant and pre-determined variation prior to the start of our sample period. To approximate the strength of government institutions and the presence of Western military (and law enforcement), we compute distances to the seat of government and assemble information about foreign military bases. Michalopoulos and Papaioannou (2014) also use distance as a measure of government influence, and Lind et al. (2014) uses distance

to Kabul as an indicator for low law enforcement and weak state institutions.

In order to proxy for Taliban control, we also gather information on whether Pashtuns, one of Afghanistan's major ethnic groups, are present in a district (using data from Weidmann et al., 2010), and whether the district has been controlled by the Taliban in 1996 (Dorronsoro, 2005). In both types of districts, government influence is potentially less strong. Trebbi and Weese (2016, p. 5) argue that support for the Taliban as the main insurgent group is best explained by ethnic boundaries. Anecdotal evidence and personal conversations with experts indicate that ethnic institutions are more relevant compared to the official government in Pashtun areas. For areas under Taliban control before 1996, we expect that due to the common past the Taliban will, all else equal, find it easier to expand their power again in those areas. We use a variety of different sources for these variables, ranging from maps provided by experts at the UN, to American military data, satellite images and newspaper reports. Figure 3.6 visualizes the data, and Appendix 3.H documents the steps involved in the construction and all sources in detail.

In uncontested districts, the Taliban also have higher incentives to maintain peace to avoid distorting the production process, the more so the higher the profitability of production. A local farmer describes that in a prominent opium growing area "the Taliban have a court there to resolve people's problems" and despite their presence "the security situation is good for the people living there."[29] Other sources verify the link between the Taliban and the drug production process, sometimes even providing seeds, tools and fertilizer. A local Taliban leader is described as "just one of dozens of senior Taliban leaders who are so enmeshed in the drug trade."[30] In contrast to other countries, there is no strong competing producer or trafficker group. It is rather the case that "the drug cartel is the Taliban."[31] Trebbi and Weese (2016, p. 5) also suggest that "insurgent activity in Afghanistan is best represented by a single organized group."

Drug producers apparently have little to fear from the government. Researchers describing their fieldwork in Badakhshan "observed neither restrictions to poppy farmers nor any repercussions or a need to hide the fields from outsiders" and in areas supposedly controlled by the government, "officials at all levels are benefiting from the proceeds from drug trafficking" (Kreutzmann, 2007, p. 616). Despite the official government claims that "poppy cultivation only takes place in areas controlled by the Taliban," a US counter-narcotics official in Afghanistan reports that "(president) Karzai had Taliban enemies who profited from drugs, but he had even more supporters who did."[32] This suggests

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[29]See http://www.rollingstone.com/politics/news/afghanistan-the-making-of-a-narco-state-20141204, accessed June 14, 2018.

[30]See https://www.nytimes.com/2017/10/29/world/asia/opium-heroin-afghanistan-taliban.html and https://thediplomat.com/2016/10/how-opium-fuels-the-talibans-war-machine-in-afghanistan/, both accessed June 14, 2018.

[31]See, https://qz.com/859268/americas-failed-war-on-drugs-in-afghanistan-is-threatening-to-doom-its-war-on-terror-as-well/, accessed June 14, 2018.

[32]See, http://www.rollingstone.com/politics/news/afghanistan-the-making-of-a-narco-state-20141204,

that the Taliban have an active interest in an undisturbed drug production process, and that the government has little power or interest to engage in the enforcement against the *de jure* illegal production.

Table 3.5, panel A shows that the conflict-reducing effect is actually stronger in districts that are more likely to be under Taliban control (columns 1-3). Columns 4 and 5 suggest that whether a district is ethnically mixed or features a large number of ethnic groups does not influence the relationship between opium and conflict.[33] This is also the case when we consider districts that in 1996 were partly controlled by the Taliban and partly under the control of a member group of the Northern Alliance (column 6). Panel B shows results for interactions with military bases, and several approximations for government influence using the distance to Kabul. None of the interaction coefficients in panel B turns out to be significant. That we also find no significant effect for any of the distance measures could be due to the fact that the relation is not linear. In Table 3.6 we dig deeper into the influence of the government by constructing binary indicators for whether a district is within a specific proximity to Kabul or other main cities. This suggests that the influence of the Afghan government seems indeed to be confined to districts within a small radius of 75 km or approximately two hours driving distance to Kabul.

Our results do not rule out that local Taliban forces use part of the revenue extracted from the opium business to finance anti-government conflict and attacks. Local revenues could partly be used for violent operations if there are relevant targets within a district. Of course, revenues need not fully remain within the district, and could be pooled to enable countrywide operations. Figure 3.7 does not indicate such a mechanism at the large scale. On average, an increase in opium revenue correlates with a decrease in casualties. We also show a regression aggregating all our data at the provincial level and again find a negative coefficient for opium profitability (see, Table 3.16). We would have expected the opposite pattern if higher revenues in one district shifted conflict to neighboring districts or to other provinces in the country.

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accessed June 14, 2018. The same source also reports a case where a drug trafficker possessed a letter of safe passage from a counter-narcotics police leader, and a new director of an anti-corruption agency was revealed to be a formerly convicted drug trafficker.

[33]In Appendix 3.F we reconstruct measures on ethnic groups and in particular presence of Pashtuns by relying on the NRVA 2003 household survey. While the 2003 wave is likely not to be nationally representative, it serves as a suitable proxy to using the GREG dataset. Results are robust to relying on household level information of native languages (see, Table 3.38).

TABLE 3.5
Territorial control and ethnic groups, 2002-2014

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Taliban control & ethnic groups						
	Pashtuns	Taliban Territory 1996		Ethnic Groups		Mixed
		All Regions	No North	1 if Mixed	Number	Territory 1996
Opium Profitability (t-1)	0.312	-0.207	-0.221	-0.763**	-0.695**	-0.977*
	(0.365)	(0.372)	(0.365)	(0.370)	(0.298)	(0.568)
Opium Prof. (t-1)*X	-1.723***	-1.013**	-1.063**	0.130	-0.265	0.114
	(0.412)	(0.477)	(0.491)	(0.421)	(0.977)	(0.280)
Panel B: Presence of government & Western forces						
	Any Military	Distance to Kabul			Travel Time to Kabul	
	Base	Linear	Road 2D	Road 3D	Road 2D	Road 3D
Opium Profitability (t-1)	-0.6669**	-0.2858	-0.5448	-0.5484	-0.6578	-0.6589
	(0.2977)	(0.4923)	(0.5009)	(0.5020)	(0.4383)	(0.4385)
Opium Prof. (t-1)*X	-0.1872	-0.0012	-0.0002	-0.0002	0.0037	0.0038
	(0.5553)	(0.0013)	(0.0012)	(0.0011)	(0.0432)	(0.0430)

Notes: Linear probability model with province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables X see Appendix 3.A. The number of observations is 5174 in every regression, the adjusted R-squared varies between 0.649 and 0.653. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 3.6
Government control, 2002-2014

| | Linear Distance | | Travel Time 3D | |
| | 1 if < 75 | 1 if < 100 | 1 if < 2 | 1 if < 3 |
	(1)	(2)	(3)	(4)
Panel A: Proximity to Kabul				
Opium Profitability (t-1)	-0.826***	-0.782**	-0.893***	-0.826**
	(0.308)	(0.313)	(0.314)	(0.325)
Opium Profitability (t-1)*X	1.693**	0.712	1.685**	0.588
	(0.800)	(0.667)	(0.671)	(0.508)
Panel B: Proximity to other main cities				
Opium Profitability (t-1)	-0.685**	-0.535	-0.576*	-0.557
	(0.327)	(0.345)	(0.319)	(0.343)
Opium Profitability (t-1)*X	-0.014	-0.463	-0.435	-0.308
	(0.527)	(0.456)	(0.564)	(0.507)

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables in X see Appendix 3.A. Other main cities are Kandahar, Kunduz, Jalalabad, Hirat and Mazari Sharif. The number of observations is 5174 in every regression, the adjusted R-squared varies between 0.650 and 0.651. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Exogenous change in policy

Finally, we exploit an important policy change in the foreign coalition's military strategy. This helps us to verify the importance of the opium economy in providing jobs. More importantly, this section sheds some light on the effectiveness of nation-building efforts and foreign military interventions, linking our study to the literature on nation building, as for instance Berman et al. (2011) for Iraq, Dell and Querubin (2018) for Vietnam, and Chapter 4 for Afghanistan. These studies often consider a distinction between strategies focusing on the use of firepower and military force, and strategies based on winning "hearts and minds" by investing money and providing services and public goods like security. Obviously, each conflict is different, but nonetheless studying the successes and failures often can provide important lessons for the future and other contexts. In Afghanistan, the coalition forces initially provided strong financial support to existing warlords and local strongholds from roughly 2001 to 2005 to build a strong anti-Taliban coalition. Rough estimates speak of several "hundred thousands of men" being armed as part of local militias, and more than 60% of provincial governors being "leaders of armed groups and most of the remaining ones had links to the latter" (Giustozzi, 2009,

FIGURE 3.7

Variation in total opium revenue and total battle-related deaths
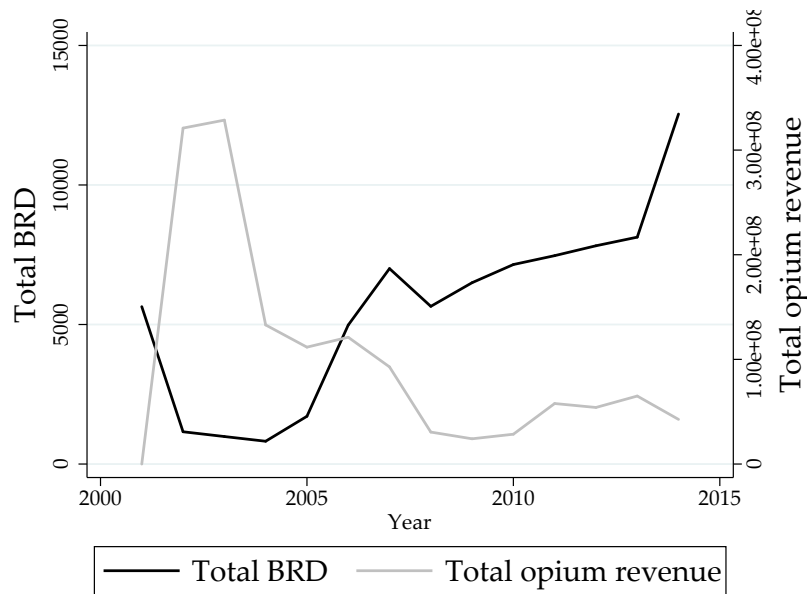
p. 91). Around 2005, the coalition switched their strategy towards a nation-building approach that attempted to pacify and "clean" Afghan politics. In this process, intense pressure on the Afghan government forced political leaders and governors to abandon their connection and support to the militias and many trained and armed men lost their main source of income (Giustozzi, 2009, p. 94 ff.). This change in strategy also coincides with the resurgence of the Taliban.

There is an analogy to the order of events in Iraq, where the de-Baathification process dissolved the Iraqi army and stopped all senior and mid-level party officials from joining the new army and security services. Various experts assess that this "drove many of the suddenly out-of-work Sunni warriors into alliances with a Sunni/anti-American insurgency" that later joined forces like ISIS, speak of the "pervasive role played by members of Iraq's former Baathist army" and estimate that "25 of ISIS's top 40 leaders once served in the Iraqi military."[34]

We want to see whether the policy change created similar problems in Afghanistan, especially because the coalition forces at the same time repeatedly declared their aim to fight opium production (e.g., UNSC Resolution 1563). Dissolving the militias eliminated many reasonably paid jobs, which should increase the reliance on income from the opium economy. We exploit the approximate timing of this change in Figure 3.8, which shows

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[34]See http://time.com/3900753/isis-iraq-syria-army-united-states-military/, https://www.reuters.com/investigates/special-report/mideast-crisis-iraq-islamicstate/, https://www.independent.co.uk/news/world/middle-east/how-saddam-husseins-former-military-officers-and-spies-are-controlling-isis-10156610.html and http://nationalpost.com/news/world/how-the-catastrophic-american-decision-to-disband-saddams-military-helped-fuel-the-rise-of-isil, accessed June 14, 2018. A detailed report about "Lessons of De-Baathification in Iraq" is by Sissons and Al-Saiedi, available at https://www.ictj.org/publication/bitter-legacy-lessons-de-baathification-iraq, accessed June 14, 2018.

that the connection between drug profitability and conflict becomes much stronger following 2005. This highlights an important trade-off between "cleaning" the state and non-state armed groups as well as fighting the production of an illegal resource at the same time. In contrast to Berman et al. (2011), who rely on survey results about unemployment for just two years of observations, we thus find further evidence of an opportunity-cost-based mechanism.

FIGURE 3.8

Variation in conflict across high and low suitability districts over time
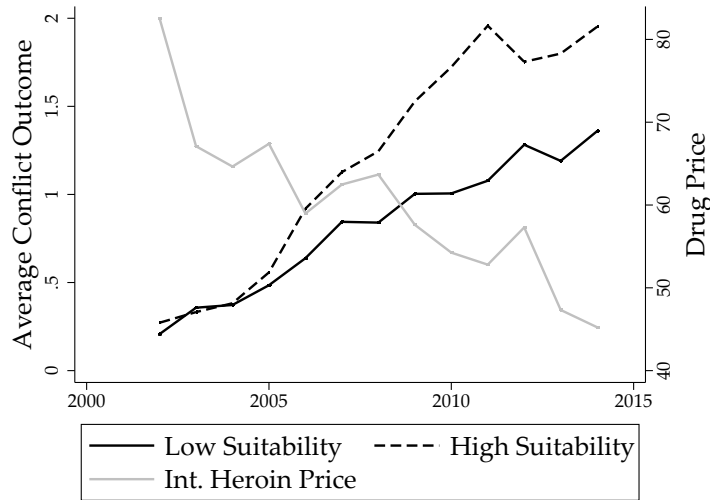
Notes: To assign a district to low or high suitability, we use a cut-off of 0.4. See Appendix 3.F, Figure 3.18 for an alternative cut-off of 0.3. Inferences do not depend on this choice.

3.7. Further results and sensitivity analysis

This section explores further results and the sensitivity of our main findings to different specification choices. All tables and figures related to the results discussed in this section are reported in Appendices 3.E and 3.F.

Timing of shocks: In a first step, we consider different lag structures in our main analysis. We do so by first including opium profitability in periods $t + 1$, t, and $t - 1$ at the same time in Table 3.12, with $t + 1$ testing for pre-trends. Second, we compare our main findings for contemporaneous and lagged effects separately in Table 3.13. Table 3.12 shows, as we hypothesize, that opium profitability in t and $t - 1$ indicates the conflict-reducing effect, while international opium prices in $t+1$ interacted with the suitability to grow opium have no significant effect on conflict. This is reassuring and supports the causal order and mechanism that we hypothesize. Table 3.13 shows that including

the contemporaneous and lagged variables individually yields similar coefficients, with slightly larger coefficients for our preferred timing $(t-1)$.

Types of fighting: To better understand the types of violence and the actors involved, Table 3.8 presents descriptive statistics. As can be seen, almost all events reported by UCDP are conflicts between the Taliban and the Afghan government, i.e., two-sided violence involving the state. Table 3.14 reports our baseline results for all battle-related deaths in column 1 and compares these to a more distinct analysis of who is fighting by looking at the actors and deaths per conflict side. Column 2 considers all casualties that are caused by Taliban violence against civilians. Results on this type of violence – that represents only 4% of all casualties – show a smaller and statistically insignificant negative coefficient. Columns 3 to 5 cover the majority of violent events, those between Taliban and government. In all specifications, irrespective of whether we look at total casualties (column 3) or only casualties on one side (column 4 and 5), we find a persistently negative effect. This exercise provides evidence for a robust conflict-reducing effect of opium income on two-sided violence throughout our observation period.

Empirical model: First, we show our main results with a less restrictive set of fixed effects in Table 3.19 for the different prices in panels A to D. The results using only district- and year-fixed effects all point in the same direction, with somewhat smaller coefficients. Again, all four prices consistently indicate a negative effect, both when looking at conflict intensity and incidence. The larger effects in absolute terms in our baseline results (Table 3.2) with province-times-year-fixed effects suggest that these succeed in eliminating biasing variation.

Next, we consider heterogeneous effects of opium profitability on onset and ending of conflict events. Acemoglu and Wolitzky (2014) and Chapter 1 point to the importance of differentiating between the probability of switching from one conflict state to another as, for instance, from peace to conflict versus from conflict to peace.[35] Thus, we also measure the effects for conflict incidence (panel A), onset (panel B), and ending (panel C) in separate models. Results are presented in Table 3.20. Panel A verifies our main finding

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[35]Berman and Couttenier (2015), for instance, argue that conflict persistence is very low at their level of analysis (a cell equivalent to 55 times 55 kilometers at the equator) compared to country level data. Consequently, they do not include the lagged dependent and rather estimate separate models for onset and ending. We report transition probabilities of the different conflict intensities from peace to war in Table 3.10. As compared to transition probabilities at the macro level, as seen in Chapter 1, they are comparably low. Given that our variable of interest is an interaction term, which we interact with further variables to identify mechanisms, we would face a high complexity if we also interacted it with the district's lagged state of conflict, in analogy to Chapter 1. In addition, other than at the macro level, there is no standard in how to define thresholds of different intensities of conflict. Besides this, in Afghanistan we do not have a comparable level to "truly peaceful" as in Chapter 1. For these reasons, we prefer to account for heterogeneous effects according to the intensity of conflict by comparing results across the different definitions of conflict (columns 2-5 in most tables).

with a linear probability model by showing similar results when using conditional logit. In panel B we find that opium profitability consistently reduces the likelihood of a conflict onset for conflict measured up to a threshold of 25 battle-related deaths. For conflict ending, we only find a significantly positive effect for smaller conflicts. These result indicate that a positive income shock and more opium cultivation raise the likelihood that an ongoing small conflict ends, and reduces the likelihood that conflicts break out.

Modifications of the treatment variable: We use multiple modifications of our treatment variable, both by replacing the drug prices and the crop suitabilities with alternative measures. Tables 3.21 and 3.22 are equivalent to our main results presented in Table 3.2 apart from the fact that drug prices are not normalized in Table 3.21 and in Table 3.22 the prices are not in logarithms. In Table 3.23, we use the deviation of the international prices from their long-term mean.[36] This is a first attempt to rule out that our results are driven by the long-term negative trend in international drug prices as visible in Figure 3.3. We find our results to be unaffected by all these choices. With regard to the suitability, we replace the population-weighted suitability for opium and wheat with an unweighted version (see Table 3.24). Weighting is important as population density differs strongly across Afghanistan, but causes potential bad control problems due to endogenous migration. While the wheat shock turns insignificant, the results for opium profitability remain unaffected for all specifications.

Finally, we dichotomize the levels of the interaction. This reduces the complexity of the DiD-like interpretation. In panel A of Table 3.25 we dichotomize the suitability based on the sample median. This allows us to interpret a price increase for two groups of districts, i.e., suitable (above the median) and less suitable (below the median). In panel B both variables are dichotomized based on the respective sample median. The coefficient in panel A indicates that a 10% increase in prices leads to about a 2.3% decrease in battle-related deaths in districts with a high suitability. Panel B finds that changing from a low- to a high-price-period reduces deaths by about 50% in districts with high suitability. Across all columns, the results are robust to this adaption.

Outcome and timing (reduced form and IV): In sections 3.4 and 3.5 we show that there is a strong effect on opium revenues. We now replace opium revenues with opium cultivation in hectares. Table 3.26 supports the positive effect of a higher opium profitability on opium cultivation, with positive coefficients that are marginally insignificant in column 1 and significant at the 5% level in column 2 when considering both periods that are most likely affected by the price change. This is not surprising as opium revenues are affected through changes in price and produced quantity, and

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[36]Specifically, we use the mean over the entire observation period. Due to data restrictions we cannot calculate the mean over a longer term.

cultivation only by the latter.

We then turn to the IV results. Table 3.27 shows IV results when replacing the endogenous variable revenue in $t-1$ with cultivation in $t-1$. Both panel A reporting second stage results and panel B reporting the corresponding first stage results support our findings. To account for the different timing as shown in Figure 3.2, we show the second and first stage results when we replace revenue in $t-1$ with the moving average of revenue in $t-1$ and t in Tables 3.28 and 3.29. The two IVs, opium profitability and VHI, are again strong as indicated by the F-statistic. The overidentification test cannot be rejected, supporting the validity of the instruments. Lastly, we show different combinations of the two main instruments in tandem with the time-varying legal opioid prescription (interacted with opium suitability) as a third instrument (see Tables 3.30 and 3.31). Legal opioids can theoretically increase heroin demand through addictions or substitute the illegal drug. The negative coefficient in the first stage shows that more legal prescriptions are linked to a lower opium price. Climate conditions are useful as they are exogenously assigned and do not follow a clear trend, and legal prescriptions are useful as they are mostly driven by US-specific factors clearly unrelated to Afghanistan (Dart et al., 2015). All different combinations lead to highly comparable second stage results and both the F-statistics and over-identification tests support the power and validity of the instruments. Having alternative sources of exogenous variation also enables us to compare the LATE of the different IVs. We find that the local effects do not differ much either in terms of magnitude or with regard to statistical significance.

Standard errors: In a next step, we use different choices on how to cluster standard errors. In the baseline models we used the district level, allowing for serial correlation over time within a district. In Table 3.32 we use two-way clustering, i.e., district and year clusters in panel A and province and year clusters in panel B (Cameron et al., 2011). Clustering at the province level is problematic as the number of clusters might be too small, which can lead to the over- or under-rejection of the null hypothesis (Cameron and Miller, 2015). Instead, we use the wild-cluster bootstrap method with the null imposed with 1000 replications and Webb's weights (Webb, 2013), which has been shown to provide valid inference even for few clusters. Figure 3.15 plots the distribution of the bootstrap estimates. The null hypothesis of no effect is rejected both when using the international heroin price or the complement price index at least at the 5% level.

Covariates and trends: Our specifications so far only include wheat shock and different fixed effects as covariates. It is natural to first compare these results to not including this main covariate. In Table 3.33 we find our results to remain robust to excluding the wheat shock, with coefficients slightly increasing. To account for the persistence of conflict, we include the lagged dependent variable in a next step. Opium

profitability remains negative in all columns and statistically significant in columns 1 to 3 (see Table 3.34). In Table 3.35 (panel A) we add a baseline set of pre-determined covariates such as luminosity and population as well as an exogenous measure of droughts, the VHI. In further specifications (panel B), we also allow for time-varying effects of these time-invariant control variables.[37] One concern with our specification is that the time trends in prices interact not only with opium suitability, but also with other district characteristics. One way to model this is by adding interactions between these characteristics and a time trend. Another more flexible way is to interact the time invariant control variables with year dummies (panel C). This last specification allows for fully flexible trends interacting with a wide range of district features. The coefficients of our treatment variable are remarkably stable, changing from -0.675 in the baseline (column 1, panel B, Table 3.2) to -0.694 in the most flexible specification for conflict intensity. They also remain significant with p-values below at least 0.1 for all conflict proxies (with the sole exception of the category "war").

Sensitivity to outliers: In Table 3.36 we drop potential outliers. In panel A we exclude all border districts from the specification as they could be either very different to other districts or shocks in neighboring countries could affect border districts in a different fashion. For instance, we expect a large share of trafficking to occur close to the border. This could drive the results if international price increases would not reach the average farmer but only the traders, which are closer to the final customer along the supply chain. We find that our results are not driven by this particular group of districts. In panel B we drop the two southern provinces Kandahar and Helmand and find our results to remain robust to this choice. These provinces are of specific interest for a number of reasons. First, the Taliban had their origins in the southern region and are thus likely to still have a strong support base here. Second, these provinces are known to be the largest producers of opium. Third, because of their direct connection to Pakistan, which is not only important in relation to trafficking routes but is also a major base of military support for the Taliban.

Apart from these rather obvious heterogeneous groups we systematically investigate whether results are driven by a particular province or year. Figure 3.16 reports the coefficients and the 90% confidence intervals when we drop each year or province at the time. All coefficients remain stable and within a narrow band.

Randomization: One of the important points raised by Christian and Barrett (2017) is that non-linear trends in the time series of Bartik/shift-share like instruments can

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[37]The set of time-invariant covariates includes Ruggedness, Ethnic Trafficking Route, Pashtuns, Mixed Ethnic Groups, Taliban Territory 1996, Mixed Territory 1996, Distance Linear, Distance 2D and 3D, Travel Time 2D and 3D (all distances to Kabul).

be problematic. We address this by looking at prices of different drugs and different versions of these prices (detrended, log vs. non-log). To further rule out that the results are driven by non-linear trends, we implement further randomization placebo tests. We first randomize the time-varying variable (international heroin price) across years, and in a second specification randomize the district-specific suitability across districts. We would be worried if any of these specifications would create a negative effect similar in magnitude to our treatment effect. Figure 3.17 plots the distribution of the coefficients generated by 5'000 randomizations per test along with the actual coefficient. We can also use this to conduct a randomization inference (RI) exercise, in which we compare how many of the random draws generate coefficients that are more negative than ours in order to compute an RI p-value. Reassuringly, we find that if the treatment was randomized according to the two different strategies, the simulated coefficients are always centered around zero. The p-values computed using two-sided symmetric randomization inference are 0.021.

Taken together, our results (i) do not depend on the choice of a linear or non-linear model or (ii) on the level at which we cluster standard errors, (iii) are robust to several modifications of the treatment and outcome variable, (iv) are barely affected and actually more negative when accounting for comprehensive sets of covariates, (v) are not driven by obvious outliers or particular provinces, and (vi) survive randomization placebo tests.

3.8. Conclusion

This paper provides new evidence on the conflict-resource-curse and the effects of resource-related income shocks (e.g., Van der Ploeg and Rohner, 2012; Berman and Couttenier, 2015; Morelli and Rohner, 2015; Berman et al., 2017) by shedding light on the micro-foundations of the underlying mechanisms. For this purpose, we focus on Afghanistan, which is a practical case from a researcher's perspective for at least three main reasons. First, despite the high intensity of conflict, we were able to collect a rich dataset including household level information. Second, different conflict actors can be identified and distinguished. Third, the country is characterized by a high variation across space with regard to resources, the distribution of ethnic groups and government influence. Although any conflict is distinct, we believe this case provides important lessons for other settings. Many conflict-ridden countries struggle with weak government enforcement, are ethnically diverse with difficulties in forming stable coalitions, feature a weak labor market with heavy reliance on one specific product, and often face obstacles in nation-building associated with foreign interventions.

Overall our reduced-form results show that, on average, a 10% increase in opium prices decrease the number of battle-related deaths by about 7% in districts with the highest possible suitability to grow opium. These results are robust to using different

international and local prices and to exploiting the relationship between opium and complement drugs. It seems that our baseline specification using heroin prices is – if one is worried about potential biases – most likely an upper bound of the true negative effect. The reduced-form results using international prices are quantified using IV results that also exploit exogenous weather changes (like e.g., Brückner and Ciccone, 2010) and legal opioid prescriptions in the United States. All IVs yield local average treatment effects that are comparable in size. Finally, several robustness tests address the potential risk that the overall downward trend in drug prices correlates with suitability-specific trends in other variables (Christian and Barrett, 2017).

The results add to the literature in several important ways. First, they augment the scarce literature on the effect of illegal resource-shocks (Angrist and Kugler, 2008; Mejia and Restrepo, 2015), and document that these shocks need not necessarily induce conflict. Second, by comparing the effects of opium- to wheat-related shocks we support Dube and Vargas's (2013) seminal study for Colombia by showing that labor intensity matters in determining the effect of resource-related income changes. Third, we verify the relevance of opportunity costs as a mechanism both by relying on district- and household level data. We thus emphasize the degree to which a high share of Afghans indeed rely on "illegal" income sources in satisfying basic needs.

Fourth, we highlight the importance of market structures, group competition and monopolies of violence for analyses of resource-related shocks. This is particularly relevant for illicit products, where the *de jure* illegality pushes up margins and supports the creation of rival cartels, which are willing to accept the related risks and exploit the potential for profits. The conflict in Afghanistan after 2001 is largely between two factions, the government plus associated groups on one side and the Taliban on the other side (see Trebbi and Weese, 2016). Consequently, we assess whether a district is likely to be under Taliban or government control. We find that in districts where the Taliban are more likely to have a monopoly of violence, the conflict-reducing effect is stronger. This supports qualitative evidence that the rebel group has given up its prior anti-drug stance and is actively involved in the drug trade, acting as a kind of stationary bandit (De La Sierra, 2015).

Fifth, we highlight the role of government enforcement. The reduction in conflict caused by a higher opium profitability is partly explained by the apparently loose enforcement of the "illegality" of opium production. Enforcement could be driven by both the willingness and the ability of the government. Our results suggest that the only areas where government control is sufficiently strong to engage in enforcement are within a limited area around the capital Kabul (comparable to Michalopoulos and Papaioannou, 2014, for Africa). If the government enforces the rules, farmers have an incentive to support the Taliban in exchange for protection of their opium-related activities. This in turn leads to higher potential for conflict with pro-government groups. In line with our

theoretical considerations, we find no conflict-reducing effect in those districts that are close to Kabul.

While we do not claim that our findings can explain the conflict in Afghanistan in all its complexity, they augment existing insights (e.g., Bove and Elia, 2013; Lyall et al., 2013; Lind et al., 2014; Condra et al., 2018). Although we cannot make strong claims beyond our observation period, the findings are in line with the spread of conflict in Afghanistan in the last years that featured falling prices and lower opium profitability. We use results at the province- and country-level to verify that higher opium revenues do, on average, not seem to spill-over and create conflict in other parts of Afghanistan. In a context with weak labor markets and few outside opportunities, depriving farmers of their main source of income by enforcing rules through eradication measures has to be weighted against the impact on households and the risk of fueling conflict. At the same time, it is, of course, also too simplistic and naive to conclude that opium production is "good" and should not be considered a potential problem.

Instead, we aim to highlight the importance of understanding the underlying trade-offs in order to derive sound policy measures that consider the "unseen" or unexpected indirect consequences of those choices. Our results suggest that the dissolving of militia groups, which provided employment and income sources for many Afghans, intensified the reliance on opium as an income source. There are, of course, good reasons for demilitarizing societies as well as for tackling illicit economies in attempts of nation-building (relating to Berman et al., 2011; Dell and Querubin, 2018). Nonetheless, if there are no attractive and feasible alternatives, both aims are hard to achieve individually and even more so simultaneously.

Appendices to Chapter 3

3.A. Definition of the variables

Any Lab: The information on the existence of laboratories used for processing opium is based on UNODC reports regarding drug markets, labs, and trafficking routes (e.g., UNODC, 2006/07, 2014, 2016). As described in Section 3.H, we georeference the maps in the reports to assign coordinates to the labs, and later compute district averages. For this variable, we count all type of heroin laboratories. It takes on the value 1 if there is at least one lab in a district i, and 0 otherwise.

Any Military Base: We use information from Wikipedia's GeoHack program for the more well-known bases and on news articles, and Wikimapia and Google Maps satellite data for the less well-documented ones. The approach is described in detail in Section 3.H. Note that we are most likely not capturing all existing locations, as we did not receive the exact information about opening and closing for all military bases. Opening and closing dates were coded with the available information, if there was no information about shutting down a base we assume it is still active. The variable takes on the value one if there is at least one open military base in a district i in year t, and 0 otherwise.

Battle-Related Deaths (BRD): The best (most likely) estimate of total fatalities resulting from an event, with an event being defined as "[an] incident where armed force was by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date." A direct death is defined as "a death relating to either combat between warring parties or violence against civilians." Note that UCDP GED only includes BRD of events that belong to a dyad ("two conflicting primary parties or party killing unarmed civilians") that reached in total at least 25 BRD within one year. If the dyad generated events with less then 25 BRD in the previous or subsequent years, they are still counted if the dyad had reached the 25 BRD threshold in another year. We construct a continuous measure (log of BRD) and binary outcomes from all BRD of any party or any type of violence (state-based, non-state or one-sided violence). To capture the lowest level of conflict in a binary measure, we classify a district-year observation with at least five BRD *small conflict*. We then increase the threshold to 10 for the next level of conflict intensity (*low conflict*). In analogy to the threshold used in macro level analyses, we call a district-year observation *conflict* if there are more than 25 BRD. At the top, we take a threshold of 100 BRD for the most severe level of violence what we call *war*. Since UCDP GED provides information on the parties and the type of violence we also construct specific outcome measures according to those categories. Besides different measures of incidence, we also construct measures on onset and ending. We define conflict onset as the incidence of a conflict in a district, where there was no conflict in the previous year ($Conflict_{i,t} = 1|Conflict_{i,t-1} = 0$). Years of ongoing

conflict are set to missing. In analogy, a conflict ending is defined when conflict persisted in the previous year but not anymore in the current year ($Conflict_{i,t} = 0|Conflict_{i,t-1} = 1$). We also set the ending variable missing for observations which have been at peace in the previous year and remained in peace in the current year, following the standards in the literature. From UCDP GED (Sundberg and Melander, 2013; Croicu and Sundberg, 2015).

Calorie Intake and Food Insecurity: The woman's questionnaire provides amounts, frequencies and sources of a large set of food items, which we use to construct measures on calorie intake and food insecurity. We multiply amounts consumed with kcal values for that food item to get total household calorie intake. The kcal values are provided by the CSO and The World Bank (2011). To get a binary indicator on food insecurity we use the reference value of 2100 calories per day as recommended by the FAO. Total household daily calorie intake is divided by the number of members that were resident and ate at least dinner regularly in the household during the last seven days to get per capita measures. Source: NRVA (CSO, 2005,2007/08,2011/12).

Consumer Price Index (CPI): From OECD (2016) for the Euro area (19 countries) and from World Bank (2016) for remaining countries (2010 = 100).

Dietary Diversity: According to Wiesmann et al. (2009, p. 5) "Dietary diversity is defined as the number of different foods or food groups eaten over a reference time period, which in my case is one week, not regarding the frequency of consumption." We classify the different food items from the survey into eight food groups as explained in Wiesmann et al. (2009). These groups are staples, pulses, vegetables, fruit, meat/fish, milk/dairy, sugar, and oil/fat. The variable varies between zero and eight, with eight indicating a high food diversity. Source: NRVA (CSO, 2005,2007/08,2011/12).

Distance/Proximity to Kabul (capital) and Kandahar, Kunduz, Jalalabad, Hirat, and Mazari Sharif (next five largest cities): We use the shapefiles provided by the Afghan statistical authority on the 398 Afghan districts. Note that the shapefiles available at www.gadm.org do not reflect the current status of administrative division in Afghanistan, and instead we use the one from Empirical Studies of Conflict (ESOC) Princeton (https://esoc.princeton.edu/files/administrative-boundaries-398-districts). To compute the distances, we first create the centroid of each district polygon. To compute road distances we combined road shapefiles from the official Afgan authorities with street maps from open street map, which were improved by voluntary contributors to close gaps in the official maps. 3D-distances were computed using elevation data from the US Geological Survey (https://lta.cr.usgs.gov/GMTED2010, accessed July 9, 2018). We add the elevation information to the shapefile containing the roads, and then compute and save three-dimensional distances. We then use the network analyst in ArcGIS to set up a network between all district centroids, clipping centroids that do not overlap with a street in that district that is closest with regard to the as-the-bird-flies distance.

Then, we compute the most efficient routes using road distances in two- and three-dimensions. The distances are saved in a matrix and exported in a table that is further processed in Stata. For the variable "distance to other main cities" we use the minimum distance to any of the five cities. For travel time we use the distinction of roads in three classes (motorways, rural, urban), and assign commonly used values for average traveling speed for that road type based on three sources. The first source is UNESCAP (http://www.unescap.org/sites/default/files/2.4.Afghanistan.pdf, page 14) which assumes that the speed on motorways is 90 km/h and on urban roads 50 km/h. The second source is IRU (https://www.iru.org/apps/infocentre-item-action?id=560&lang=en) which states no limits except for urban areas with 50 km/h. The 3rd source is WHO (http://apps.who.int/gho/data/view.main.51421) reporting 90 km/h for rural. We choose the following average traveling speeds, assuming that no strictly enforced limits and little traffic on motorways (120 km/h), and accounting for some (90 km/h-10 km/h) and moderate traffic in cities (50-20 km/h). Thus our main choice is the following. Motorways: 120 km/h, rural: 80 km/h, urban: 30 km/h. These choices are not perfect, but we verify that our results hold with other variations as well.

For the proximity to Kabul and other main cities we also define binary indicators for the distance being smaller than 75 km (1 if < 75) or smaller than 100 km (1 if < 100). In analogy to these categories we construct indicators for the travel time to Kabul or one of the other main cities falling below 2 or 3h.

Drug Prices (International): The data are average prices per gram in constant (2010) EU across all available countries in Europe. We use data on different drugs: amphetamines, cocaine, ecstasy, heroin (brown). To construct the average price of alternative drugs we use a mean of the three upper drugs amphetamines, cocaine, and ecstasy. For the analysis we convert all drug prices into constant 2010 EU per gram. We then normalize the prices by using a linear min-max function such that all prices vary between 0 and 1. From European Monitoring Centre for Drugs and Drug Addiction (EMCDDA).

Economically Improved: This variable refers to the question "How do you compare the overall economic situation of the household with 1 year ago?" 1 indicates much worse, 2 slightly worse, 3 same, 4 slightly better, and 5 much better. This is a self-reported measure of the household. Source: NRVA (CSO, 2005,2007/08,2011/12).

Ethnic Groups: We have used the GIS-coordinates of all ethnic groups in the "georeferencing of ethnic groups" (GREG) dataset Weidmann et al. (2010). It relies on maps from the classical "Soviet Atlas Narodov Mira" from 1964, and is very extensively used for the construction of ethnolinguistic fractionalization indices. GREG is a georeferenced dataset containing the coordinates of the group boundaries of 1120 ethnic groups. One advantage and disadvantage of the data is that it is capturing group locations in the 1960s. This is an advantage as it ensures that the boundaries are not

endogenous to changes during our period of observation. It is partly a disadvantage if groups and countries changed over time. In Afghanistan, the country boundary did not change. Ethnic group populations certainly change to some degree over time, so that all variables more precisely capture the historic homelands of ethnic groups rather than the current settlement areas. Our variable Pashtuns is coded in the following way. The GREG polygons can contain more than one ethnic group. Our binary indicator takes on the value one if Pashtuns are present to any degree in a district i, regardless of whether they were the majority group. The idea behind this is that the Taliban are initially a Pashtun group (although not exclusively anymore), so that Pashtun presence could make it easier to establish a presence of the Taliban in a district. We also construct two measures on whether a district is ethnically mixed, first by using the the number of ethnic groups and second by generating a binary indicator, which takes a value of 1 if the number of ethnic groups is larger 1.

Ethnic Trafficking Route: This variable combines information about unofficial border crossings from UNODC with information about the homelands of ethnic groups from the (GREG) dataset (Weidmann et al., 2010). It takes on the value of 1 if there is a potential trafficking route leading from a district to at least one unofficial border crossing point without crossing the ethnic homeland of another group. The underlying intuition is that trafficking is cheaper and significantly easier to conduct, and the accruing additional profits higher, if there is no need to cross the area of other ethnic groups to transport over the border.

Food Expenditures (Paasche/Laspeyres): Following the literature, we include food items from all possible sources, i.e., purchased food or food in form of gifts etc. We use the section on food consumption from the NRVA women's questionnaire as this section offers precise amounts per food item. The food items are merged to local prices, which are provided in a separate section of the NRVA, the district questionnaire. Prices vary at the district level. We show three food expenditure measures, which are all measured in constant 2011 prices, i.e., prices of the 2011/12 survey wave. Only food items that appear in all three waves are included to build the measure.

The first measure "Food Exp. 2011 Prices" does not account for spatial price differences. "Food Exp. 2011 Prices, Paasche" and "Food Exp. 2011 Prices, Laspeyres" adjust for spatial price differences, since households in different districts face different prices. Missing values of district prices are replaced by the province median, which in case of missing values has been replaced by the national median price. For close to all reported food items prices have been given in the district questionnaire. Information on food and drinks consumed outside the house (from the male survey section) are also included in the total food expenditure measures (adjusted for inflation and regional price differences depending on the measure). Expenditures are meaured in per capita terms by dividing the total household food expenditure with the number of households (resident

and ate at least dinner regularly in the household during the last seven days). Source: NRVA (CSO, 2005,2007/08,2011/12).

Inflation, GDP Deflator: GDP deflator for the United States with 2010 as the base year. From World Bank (2016).

Insecurity/Violence Shock: The share of sampled households per district that have experienced a shock due to insecurity/violence according to the NRVA survey (CSO, 2005,2007/08,2011/12).

Legal Opioids: The data are collected using a variety of sources. The reason is that most single publications did not cover our whole sample period, and that we want to cross-verify the numbers. A main source is the US CDC Public Health surveillance report 2017, available at https://stacks.cdc.gov/view/cdc/47832. Other important sources were Manchikanti et al. (2012); Kenan and Mack (2012); Dart et al. (2015).

Local Opium Price: Local price data on opium is derived from the annual Afghanistan Opium Price Monitoring reports UNODC. These reports include (monthly) province level dry opium prices by farmers and by traders as well as country-wide yearly data on fresh opium farm-gate prices, that are weighted by regional production. The province level opium prices of farmers and traders are highly correlated, with a correlation coefficient close to 1 (0.998). The correlation between the country level farm-gate price and the province level farm-gate price is 0.66, significant at the 1% level. While the province level prices are only available from 2006 to 2013 and for a subset of provinces, they are still very helpful in identifying whether international prices are correlated with local prices. We use the country-wide yearly data on fresh opium farm-gate prices in Afghanistan interacted with the suitability as one proxy for opium profitability in our regressions in Table 3.2, Panel A.

Luminosity: Proxy for GDP and development. The yearly satellite data are cloud-free composites made using all the available smooth resolution data for calendar years. The products are 30 arc second grids, spanning -180 to 180 degrees longitude and -65 to 75 degrees latitude. A number of constraints are used to select the highest quality data for entry into the composites: Data are from the center half of the 3000 km wide OLS swaths. Lights in the center half have better geolocation, are smaller, and have more consistent radiometry. Sunlit data and glare are excluded based on the solar elevation angle, Moonlit data based on a calculation of lunar illuminance. Observations with clouds are excluded based on clouds identified with the OLS thermal band data and NCEP surface temperature grids. Lighting features from the aurora have been excluded in the northern hemisphere on an orbit-by-orbit manner using visual inspection. From Version 4 DMSP-OLS nighttime lights time series, National Oceanic and Atmospheric Administration-National Geophysical Data Center (NOAA/NGDC, https://www.ngdc.noaa.gov, 2013). We take the logarithm.

Markets (Major/Sub) and Sum of all Markets: The information on opium markets is based on UNODC reports regarding drug markets, labs, and trafficking routes (e.g., UNODC 2006/07, 2014, 2016). The first variable takes on the value one if there is at least one major or sub-market in district i, and 0 otherwise. The second variables counts the sum of all opium markets in a district (both sub and major).

Market Access: Market access for a district i is computed as $MA_i = \sum_{j=1}^{N} dist_{i,j}^{-\theta} W_j$. W_j is the importance of district j proxied using either the number of opium markets or mean luminosity (or population). $dist_{i,j}$ are the distances between the district and the other districts and θ is the factor discounting other districts that are further way. We use a factor of 1, as in Donaldson and Hornbeck (2016). To take account of the topography and mountainous terrain in Afghanistan, we compute distances using the two-dimensional road network (Market Access 2D) as well as a three-dimensional road network when adjusting for elevation (Market Access 3D).

Mixed/Taliban Territory 1996: The book by Dorronsoro (2005) provides a map indicating the territory of the Taliban in 1996 and of other major groups of the Northern Alliance (Dschunbisch-o Islami, Dschamiat-i Islami, Hizb-i Wahdat). We georeferenced the map and aligned it with the district boundaries. In many cases, the division was quite clearly aligned or overlapping with a district boundary, in the other cases we chose the closest district boundary. We classify a district as a Mixed Territory if it is part of the Taliban 1996 territory and part of the territory of any of the three groups belonging to the Nothern Alliance. The binary indicator on Taliban Territory that we create take on the value one if a district belongs to the territory that was occupied or under the control of the Taliban in 1996. A second indicator (Taliban Territory 1996 - No North) is defined whether the district exclusively occupied by the Taliban and is characterized by no presence of the Northern Alliance. More details can be found in Dorronsoro (2005) and Giustozzi (2009).

Opium Cultivation and Revenues: Opium cultivation in hectares. Data at the district level is an estimate from the data at the province level. We use logged values for opium cultivation and for revenues. From opium cultivation and the respective yields we were able to calculate actual opium production at the district-year level. We also constructed opium revenues by multiplying opium production in kg with the fresh opium farm-gate prices at harvest time in constant 2010 EU/kg. From the Annual Opium Poppy Survey (UNDCP, 2000) and Afghanistan Opium Survey (UNODC, 2001-2014).

Opium Suitability: Proxy for potential of opium production based on exogenous underlying information about land cover, water availability, climatic suitability, and soil suitability. It was developed in the context of a study in collaboration with UNODC, and is described in detail in a publication in a geographical science journal (Kienberger et al., 2017). The environmental as well as climatic suitability to cultivate opium poppy (Papaver somniferum) is characterized by different factors such as the prevailing physio-

geographical and climatic characteristics using climatic suitability based on the EcoCrop model from Hijmans et al. (2001). The factor determined to be most important by experts is land cover (S1, 0.41 – the sum of the weights equals 1.0), followed by water availability (S2, 0.28) and climatic conditions (S3, 0.21) respectively. This is in line with additional studies previously carried out by UNODC and described in the World Drug Report (2011) for Myanmar. From Kienberger et al. (2017). The data and the index itself was modeled on a $1km^2$ resolution and then aggregated to the district units by an area weighted mean approach. The original indicator values were normalized using a linear min–max function between a possible value range of 0 and 100 to allow for comparison and aggregation. Only the land cover indicator was normalized integrating expert judgments through an Analytical Hierarchy Process (AHP) approach. The four indicators were then subsequently aggregated applying weighted means (weights were verified through expert consultations building on the AHP method). None of the input factors constituting the index is itself to a major degree affected by conflict, which is the outcome variable. Consequently, the index values by district can be considered as exogenously given.

We weight the opium and wheat suitabilities with the (lagged) population distribution within the districts. This is helpful as, for instance, the south features large desert areas and at the same time concentrated areas with dense population, and accounting for the suitability in uninhabited desert areas might be misleading (our results are not significantly affected by this choice).

Population: A minimally-modeled gridded population data collection that incorporates census population data from the 2010 round of censuses. Population estimates are derived by extrapolating the raw census estimates to a series of target years and are provided for the years 2000, 2005, 2010, 2015, and 2020. We use the ipolated data from 2000 till 2015. We take the logarithm. From the Center for International Earth Science Information Network – CIESIN – Columbia University. 2016. Gridded Population of the World, Version 4 (GPWv4): Administrative Unit Center Points with Population Estimates. Palisades, NY: NASA Socioeconomic Data and Applications Center (SEDAC). Source: http://dx.doi.org/10.7927/H4F47M2C, accessed October 5, 2017.

Ruggedness: The data on terrain ruggedness is the same that was used in Nunn and Puga (2012), although we use it on a more disaggregated level. We calculate the average ruggedness index for every district. While ruggedness refers to the variance in elevation, we also use raw elevation data from the NASA Shuttle Radat Topography Mission (SRTM) data set. The data set and a detailed documentation are available at http://diegopuga.org/data/rugged/.

Southern Provinces: Dummy variable which turns one for districts located in one of the two provinces Kandahar and Hilmand, and 0 otherwise.

Sum of Assets (weighted): The number of assets the households possess over a set of assets that is constant over the 3 survey waves. This set consists of Radio/Tape, Refrigerator, TV, VCR/DVD, Sewing Machine, Thuraya (any phone), Bicycle, Motorcycle, Tractor/Thresher, Car. Sum of Assets weighted is the sum of asset weighted by the proportion of households not possessing the specific item. Source: NRVA (CSO, 2005, 2007/08, 2011/12).

Travel Time to Kabul and other main cities: Hours required to travel from district centroid to Kabul. For details about distance computation see Distances.

Vegetation Health Index (VHI): We use the Vegetation Health Index (VHI) of FAO (Van Hoolst et al., 2016). VHI is a composite index joining the Vegetation Condition Index (VCI) and the Temperature Condition Index (TCI, Kogan 1995). Low values of VHI represent drought conditions. This is a combination of low values of the observed VCI (relatively low vegetation) and higher values of the TCI (relatively warm weather). For details see Van Hoolst et al. (2016). The VHI is calculated from data of Advanced Very High Resolution Radiometer (AVHRR) sensors on board of the National Oceanic and Atmospheric Administration (NOAA) and Meteorological Operational Satellite (METOP) satellites. It is superior to simply using precipitation data, which do not directly measure drought conditions, require assumptions about the linearity of the effect and, in particular in Afghanistan, have severe limitations in terms of quality and resolution. The index is based on earth observation data and is available on a monthly basis with a resolution of 1 km^2. As cultivation and harvest times differ within Afghanistan, we use the yearly average. The remote sensing based index is operationally used to monitor drought conditions in the Global Early Warning System (GEWS), low VHI values indicate drought conditions.

Wheat Price (International): Source is the International Monetary Fund Primary Commodity Prices database (IMF, 2017). IMF reports benchmark prices which are representative of the global market. They are determined by the largest exporter of a given commodity. The prices are period averages and are in nominal US dollars (2005 as baseline).

Wheat Suitability: The FAO-GAEZ (2012) model provides for each crop/Land Utilization Type (LUT) a comprehensive soil suitability evaluation for all the soil units contained in the Harmonized World Soil Database (HWSD). This is done by the use of individual soil quality ratings (SQ). Seven different SQs are calculated and are combined in a soil unit suitability rating (SR, %). The SR represents the percentage of potential yield expected for a given crop/LUT with respect to the soil characteristics present in a soil map unit of the HWSD and is depending on input/management level. Source: Global Agro-ecological Zones (GAEZ v3.0) by the Food and Agriculture Organization of the United Nations (FAO-GAEZ 2012). Details are provided on the website http://www.fao.org/nr/gaez/about-data-portal/

agricultural-suitability-and-potential-yields/en/, accessed October 12, 2016. Move to the section "Agro-ecological suitability and productivity" to find the suitability we use and access the data portal for downloads.

3.B. Descriptive statistics

	Obs.	Mean	Stand. Dev.	Min	Max
BRD					
(log) All	5174	1.11	1.54	0.00	8.20
Small Conflict	5174	0.31	0.46	0.00	1.00
Low Conflict	5174	0.23	0.42	0.00	1.00
Conflict	5174	0.14	0.34	0.00	1.00
War	5174	0.03	0.18	0.00	1.00
(log) Taliban-Civilians	5174	0.08	0.37	0.00	4.14
(log) Taliban-Government	5174	1.05	1.52	0.00	8.20
(log) Government BRD by Taliban	5174	0.53	0.94	0.00	8.03
(log) Taliban BRD by Government	5174	0.77	1.33	0.00	6.39
(log) Opium Profitability					
Int. Heroin	5174	-1.52	0.66	-4.61	-0.00
Local Opium	5174	-1.04	0.70	-4.61	0.01
Int. Complement	5174	-1.30	0.56	-3.17	-0.00
Int. Cocaine	5174	-1.15	0.67	-4.61	-0.00
Distance to Kabul					
Linear	5148	277.05	181.54	0.00	817.64
Road 2D	5174	345.03	212.05	0.00	959.78
Road 3D	5174	347.47	213.08	0.00	964.48
Travel Time 2D	5174	7.53	5.91	0.00	28.40
Travel Time 3D	5174	7.57	5.94	0.00	28.45
Market Access					
Opium Market 2D	5174	4.47	1.10	2.24	11.23
Opium Market 3D	5174	2.63	0.69	1.33	6.93
Luminosity 2D	5174	6.51	4.86	1.85	41.26
Luminosity 3D	5174	6.47	4.84	1.85	41.24
continued on next page					

Table 3.8 continued

All other variables

(log) Wheat Shock	5174	-0.48	0.46	-2.11	0.01
Opium Suitability	5174	0.53	0.18	0.00	1.00
Wheat Suitability	5174	0.55	0.23	0.00	1.00
(log) Cultivation	5174	1.38	2.15	0.00	6.91
(log) Opium Revenue	5149	4.26	5.83	0.00	16.98
Luminosity	4776	0.49	3.03	0.00	58.01
Vegetation Health Index (VHI)	5173	124.08	23.20	51.28	191.99
(log) Population	5174	3.96	1.24	0.44	9.58
Ruggedness in 1000	5148	299.18	216.54	4.48	877.01
Any Military Base	5174	0.04	0.20	0.00	1.00
Major/Sub Market	5174	0.27	0.44	0.00	1.00
Sum of all Markets	5174	0.40	0.85	0.00	8.00
Any Lab	5174	0.13	0.34	0.00	1.00
Ethnic Trafficking Route	5174	0.52	0.50	0.00	1.00
Mixed Territory 1996	5174	0.04	0.20	0.00	1.00
Taliban Territory 1996	5174	0.58	0.49	0.00	1.00
Pashtuns	5174	0.74	0.44	0.00	1.00
Ethnicity - 1 if Mixed	5174	0.59	0.49	0.00	1.00
Number Ethnic Group	5174	1.93	0.97	1.00	5.00

Notes: The sample is based on the specification in Table 3.2, column 1.

TABLE 3.8

Type of violence and fighting parties

	Frequency (1)	Percent (2)
	Conflict Dyads	
Government of Afghanistan - Taliban	14,853	93.93
Taliban - Civilians	614	3.88
Government of United States of America - al-Qaida	125	0.97
	Type of violence	
State-based violence	15,084	95.39
Non-state violence	631	3.99
One-sided violence	98	0.62

Notes: Summary on types of violence provided by UCDP GED between 2002-2014.

TABLE 3.9

Balancing tests: High and low opium suitable districts

	Mean Values per Group		P-Value
	High Suitability	Low Suitability	
Ruggedness in 1000	286.052	342.550	0.000
Distance to Kabul - Linear	248.425	371.647	0.000
Distance to Kabul - Road 2D	311.787	454.068	0.000
Distance to Kabul - Road 3D	314.037	457.126	0.000
Travel Time to Kabul - Road 2D	6.560	10.693	0.000
Travel Time to Kabul - Road 3D	6.597	10.755	0.000
Pashtuns	0.780	0.602	0.000
Mixed Ethnic Groups	0.538	0.742	0.000
Number Ethnic Groups	1.830	2.247	0.000
Mixed Territory 1996	0.030	0.075	0.000
Taliban Territory 1996	0.593	0.527	0.000
Ethnic Trafficking Route	0.557	0.409	0.000
BRD 2000	14.308	11.075	0.172
Luminosity 2000	0.160	0.213	0.322
(log) Population 2000	3.974	2.654	0.000
Wheat Suitability	0.609	0.371	0.000

Notes: Sample based on Table 3.2, column 1. To assign a districts to low or high suitability, we use a cut-off of 0.4. In Table 3.35 we control for an interaction of all the variables (above the separating line) with a time trend or with time-fixed effects.

TABLE 3.10

Unconditional transition matrix

	1 if 0	1 if >0	1 if >10	1 if >25	1 if >100
1 if 0	87.49	7.55	2.46	1.85	0.64
1 if >0	36.86	35.41	15.81	9.76	2.17
1 if >10	23.46	30.19	19.81	23.27	3.27
1 if >25	19.90	13.21	16.64	36.54	13.70
1 if >100	19.25	7.55	4.15	28.68	40.38

Notes: Sample based on Table 3.2, column 1.

3.C.   Geographical overview

Afghanistan and its neighboring states

Notes: Opium is reported to be mostly trafficked through Iran, Pakistan as well as through Turkmenistan according to UNODC.

Elevation and mountainous terrain in Afghanistan

Notes: The central and north-eastern part of Afghanistan feature the most mountainous terrain. Mountains are correlated with opium suitability, for instance very high altitude areas with a lot of snow are obviously unsuitable, but generally opium can be produced in many places as our map for the suitability indicator shows. We will run regressions with and without the border districts, as well as regressions controlling for elevation or ruggedness (in a flexible way interacted with year dummies) to account for potentially time-varying effects of these factors. Source for elevation data: US Geological Survey (USGS) Global Multi-resolution Terrain Elevation Data 2010 (GMTED2010), available at https://lta.cr.usgs.gov/GMTED2010, accessed June 4, 2018. Source for ADM1 administrative data is www.gadm.org, accessed June 4, 2018.

FIGURE 3.11
ADM1 level (provinces) of Afghanistan

Notes: The figure plots the 34 provinces (ADM1 level). Source: Central Statistical Office Afghanistan, available at http://afghanag.ucdavis.edu/country-info/about-afghan.html, accessed July 9, 2018.

3.D. Identification using complement prices

Assume that we estimate a regression

$$conflict_{d,t} = b \times drug\,price_{t-1} \times suit + \tau_t + \delta_d + \varepsilon_{d,t}, \tag{3.4}$$

but the true regression is

$$conflict_{d,t} = \beta \times drug\,price_{t-1} \times suit + \tau_t + \delta_d + \gamma \times suit * OV_{t-1} + \vartheta_{d,t}. \tag{3.5}$$

The drug prices (of opium and complements) depend on the following factors: i) changes in demand, to which we refer to as common demand shifters (DS'), ii) changes in opium supply (q^O), and iii) changes in the supply of the complement (q^C).

Accordingly, we have

$$p^O_{t-1} = f(\overset{(+)}{DS'_{t-1}}, \overset{(-)}{q^O_{t-1}}, \overset{(+)}{q^C_{t-1}}) \tag{3.6}$$

and

$$p^C_{t-1} = f(\overset{(+)}{DS'_{t-1}}, \overset{(+)}{q^O_{t-1}}, \overset{(-)}{q^C_{t-1}}). \tag{3.7}$$

The omitted variable OV_t varies at the time level, in our case by year. To be relevant for our estimation, we assume that OV has a nonzero effect on the outcome, i.e., $\gamma \neq 0$, and the effect varies conditional on the suitability ($suit$). $suit \sim [0,1]$, with higher values indicating a higher suitability for production. At the same time, OV must also affect opium supply and in turn opium prices, again differentially conditional on $suit$. More formally, in case $E[q^O_{t-1}, suit_d \times OV_{t-1}] \neq 0$, a potentially problematic bias could arise. Given the negative point estimates in our regression analysis when we use the opium price, we would be worried about a downward bias in the coefficient b, which could lead to the false rejection of the null hypothesis. However, what we show in the following is relevant for both an upward and a downward bias. Note that $\frac{\partial q^O_{t-1}}{\partial(suit_d \times OV_{t-1})} < 0$, so that $\frac{\partial p^O_{t-1}}{\partial(suit_d \times OV_{t-1})} > 0$. We exploit the fact that the bias resulting from the omitted variable (conditional on suitability) through its effect on opium supply works in different directions for the complement than it does for opium: $\frac{\partial p^C_{t-1}}{\partial OV_{t-1}} = (-1)\frac{\partial p^O_{t-1}}{\partial OV_{t-1}}$.

How does this help us in the causal interpretation of our estimations? We describe the opium price and the price of complements, how the two relate to each other, and under which assumptions we can use the relationship between the two to better understand causality. We then verify and illustrate this relationship and its implications with a Monte Carlo simulation. We are particularly interested in the relationship between the two prices, and a potential suitability-specific effect of an omitted variable on opium supply.

Opium price:

Consider the price of opium as a linear function:

$$p^O_{t-1} = DS_t - q^O_{t-1} + \varpi \times q^C_{t-1} + \epsilon^O_{t-1},$$

where the factors directly influencing supply can be distinguished as

$$q^O_{t-1} = X_{t-1} + \eta \times \sum_{d=1}^{D} \text{suit}_d \times \text{OV}_{t-1} + \epsilon^{q^O}_{t-1}.$$

ϖ indicates to which degree opium and the complement are related ($\varpi \sim \mathcal{U}[0,1]$), i.e., how strong the cross-price elasticity is. The second equation means that the opium supply in year t is influenced by factors X_{t-1} like temperature and precipitation that are unrelated to the suitability-specific effect of the omitted variable, and by the suitability-specific shock caused by the omitted variable.[38] η indicates the degree to which the omitted variable influences opium supply and the opium price ($\eta \sim |\mathcal{N}(0, \sigma^2)|$). We sum up over all districts d, and assume that the omitted variable has a stronger effect on high suitability districts. Furthermore, we make one important assumption: We assume that supply shocks of the complement q^C_{t-1} can be related to overall opium supply, but are exogenous to district level differences in supply in Afghanistan, $\rho(q^C_{t-1}, suit_d \times OV_{t-1}) = 0$. We further validate this assumption by considering both an index of complements, as well as cocaine for which supply and trafficking routes (and related shocks) clearly differ from opium (heroin). Accordingly, both the term X_{t-1} and q^C_{t-1} are captured by year-fixed effects τ_t in Equation 3.4, and can be omitted without affecting the estimation of b. $\epsilon^O_{t-1} \sim \mathcal{N}(0, \sigma^2)$ is an iid error term. Assuming for simplicity that demand shifters and potentially endogenous opium supply influence the price in an additive manner yields

$$p^O_{t-1} = DS_t - \eta \times \sum_{d=1}^{D} \text{suit}_d \times \text{OV}_{t-1} + \epsilon^O_{t-1}. \tag{3.8}$$

with $\eta \sim |\mathcal{N}(0, \sigma^2)|$ being the degree to which the omitted variable influences p^O_{t-1} and ϵ^O_{t-1} being an iid error term.

Complement price:

The price of the complement is

$$p^C_{t-1} = DS_t - q^C_{t-1} + \varpi \times q^O_{t-1} + \epsilon^C_{t-1},$$

and inserting q^C_{t-1} leads to

$$p^C_{t-1} = DS_t + \varpi \times (X_{t-1} + \eta \times \sum_{d=1}^{D} \text{suit}_d \times \text{OV}_{t-1} + \epsilon^O_{t-1}) - q^C_{t-1} + \epsilon^C_{t-1}.$$

For a negative cross-price elasticity, ϖ is positive: as q^O increases, the price of opium decreases, polydrug consumers have more money available, the demand for the

---

[38]We simplify and just use X instead of summing up over all potential factors weighted by their importance.

complement increases, which leads to a price increase in the complement. To be problematic, a supply-side shock must be caused by an omitted variable and must be suitability-specific. The main feature that we exploit is that the effect of those shocks, which might also be correlated with conflict, affect the opium and the complement price in different directions.[39] As above, the assumption that supply shocks of the complement are exogenous to district level differences in supply in Afghanistan means that the terms X_{t-1} and q_{t-1}^C are captured by year-fixed effects τ_t and can be dropped. This results in the following equation:

$$p_{t-1}^C = DS_t + \varpi \times \eta \sum_{d=1}^{D} \text{suit}_d \times OV_{t-1} + \epsilon_{t-1}^O + \epsilon_{t-1}^C. \tag{3.9}$$

with $\varpi \sim \mathcal{U}[0,1]$ being the degree to which supply side shocks to opium affect the price of the complement(s). With a negative cross-price-elasticity, as for complement goods, it holds that $\varpi > 0$, i.e., a positive supply shock to the good decreases the price of the good, and increases demand and the price of the complement. The error term is $\epsilon_{t-1}^C \sim \mathcal{N}(0, \sigma^2)$. If the additional random noise ϵ_t^C becomes too large, the complement price becomes less informative and less useful as this would dominate the former part of the equation.

Opium and complement price:

Accordingly (focusing on those parts relevant for the coefficient estimate that are not captured by FE), we have:

$$p_{t-1}^O = DS_t - \eta \times \sum_{d=1}^{D} \text{suit}_d \times OV_{t-1} + \epsilon_{t-1}^O, \tag{3.10}$$

$$p_{t-1}^C = DS_t + \varpi \times \eta \sum_{d=1}^{D} \text{suit}_d \times OV_{t-1} + \epsilon_{t-1}^O + \epsilon_{t-1}^C. \tag{3.11}$$

Subtracting Equation 3.10 from Equation 3.11 gives

$$p_{t-1}^C = p_{t-1}^O + (\varpi \times \eta + \eta) \times \sum_{d=1}^{D} \text{suit}_d \times OV_{t-1} + \epsilon_{t-1}^C. \tag{3.12}$$

Equation 3.12 shows that p^C can be considered as the opium price plus a difference in the

---

[39]One simplifying assumption we make is that adjustment effects take time, for instance until the next year. Over time, the quantity of the complement that is produced will of course adjust to the higher price and increase as well, which will limit the price increase until a new equilibrium is reached. It also matters whether a supply shock is temporary or persistent.

bias and an iid error term ϵ^C_{t-1}, which we can treat as additional random measurement error in a regression on conflict. We can then write the prices at the district-year level using Equation 3.10 and Equation 3.11 as

$$p^O_{d,t} = DS_t - \eta \times suit_d * OV_{t-1} + \epsilon^O_{d,t-1}, \tag{3.13}$$

$$p^C_{d,t} = DS_t + \varpi \times \eta \times suit_d \times OV_{t-1} + \epsilon^C_{d,t-1}. \tag{3.14}$$

We can then compare the three estimating equations

$$conflict_{d,t} = b^{True(1)} \times p^O_{t-1} \times suit_d + \gamma \times suit_d \times OV_{t-1} + \tau_t + \delta_d + \vartheta_{d,t}, \tag{3.15}$$

$$conflict_{d,t} = b^{Opium(2)} \times p^O_{t-1} \times suit_d + \tau_t + \delta_d + \vartheta^O_{d,t}, \tag{3.16}$$

$$conflict_{d,t} = b^{Complement(3)} \times p^C_{t-1} \times suit_d + \tau_t + \delta_d + \vartheta^C_{d,t}. \tag{3.17}$$

Equation 3.15 is the "true" regression and equations 3.16 and 3.17 "short" equations in the sense Angrist and Pischke (2008) use true and short. Short equations do not capture the effect of the omitted variable, and thus yield biased coefficients b. Inserting the terms from above yields

$$conflict_{d,t} = b^{True(1)} \times p^O_{t-1} \times suit_d + \gamma \times suit_d \times OV_{t-1} + \tau_t + \delta_d + \vartheta_{d,t}, \tag{3.18}$$

$$conflict_{d,t} = b^{Opium(2)} \times (DS_t - \eta \times suit_d \times OV_{t-1} + \epsilon^C_{t-1}) \times suit_d + \tau_t + \delta_d + \vartheta^O_{d,t}, \tag{3.19}$$

$$conflict_{d,t} = b^{Complement(3)} \times (DS_t + \varpi \times \eta \times suit_d \times OV_{t-1} + \epsilon^C_{t-1}) \times suit_d + \tau_t + \delta_d + \vartheta^C_{d,t}. \tag{3.20}$$

This means the coefficients in the short regressions are

$$b^{Opium} = \beta - \eta \times suit_d \times OV_{t-1} + \vartheta^O_{d,t},$$

$$b^{Complement} = \beta + (\varpi \times \eta \times suit_d \times OV_{t-1}) - \beta \times \frac{\sigma^{\epsilon^C}}{\sigma^C + \sigma^{\epsilon^C}} + \vartheta^C_{d,t}.$$

The expectation for the coefficients when regressing both prices in the short regressions on conflict are

$$E[b^{Opium}] = \beta + \gamma \times \frac{\rho(opiumprice_{t-1} \times suit_d, OV_{t-1} \times suit_d)}{\text{Var}(opium\,price_{t-1} \times suit_d)} \tag{3.21}$$

and

$$E[b^{Complement}] = \beta - \beta \times \frac{\sigma^{\epsilon C}}{\sigma^C + \sigma^{\epsilon C}} + (-\varpi) \times \gamma \times \frac{\rho(opium\, price_{t-1} \times suit_d, OV_{t-1} \times suit_d)}{\mathrm{Var}(opium\, price_{t-1} \times suit_d)}$$

$$\implies$$

$$E[b^{Complement}] = \beta \times \frac{\sigma^C}{\sigma^C + \sigma^{\epsilon C}} - \varpi \times \gamma \times \frac{\rho(opium\, price_{t-1} \times suit_d, OV_{t-1} \times suit_d)}{\mathrm{Var}(opium\, price_{t-1} \times suit_d)}. \quad (3.22)$$

The first term in Equation 3.22 indicates the attenuation bias, moving the coefficient towards zero, as the complement price is only a noisy proxy for the opium price. The second term shows that a potential omitted variable bias points in the opposite direction for the two prices, as $-\varpi \leq 0$ and $\varpi \sim \mathcal{U}[0,1]$. We can see that if $\varpi = 0$, the estimate would not be affected by omitted variable bias at all; it would however also not be informative about the opium price. For the case $\varpi = 1$, i.e., perfect complements in the sense that the good's price reacts to changes in the supply of the complement as strong as to changes in its own supply, the omitted variable bias would point in the opposite direction and be of exactly equal size for both prices.

Equations 3.21 and 3.22 make it very obvious which properties we would want from a "useful" complement.

- Common demand shifters must affect both prices simultaneously, so that $\frac{\sigma^C}{\sigma^C + \sigma^{\epsilon C}}$ remains close to 1, i.e., the complement price is informative about the opium price.

- Supply side shocks to the complement must be exogenous to suitability-specific shocks in Afghanistan such that we can ignore their influence for the estimation.

We can conclude the following:

- Estimates using the complement price will be attenuated towards zero, making it less likely to find a significant effect.

- Omitted variable bias shifts both coefficients in opposite directions. Accordingly, if the true effect is zero, one of the estimates should be larger, and one smaller than zero. It is unlikely that both are negative (or positive), if the true effect is not negative or positive (Scenario A and B in the simulation below).

- If both coefficients are negative (positive) and the opium coefficient is more negative (positive), this indicates a downward (upward) bias in the opium coefficient. The complement coefficient is an upper (lower) bound of the true negative (positive) effect (Scenario C in the simulation below).

- If both coefficients are negative (positive) and the complement coefficient is more negative (positive), this indicates an upward (downward) bias in the opium coefficient. The opium coefficient is an upper (lower) bound of the true negative (positive) effect (Scenario D in the simulation below).

Taken together, our exercise serves two purposes. First, we can test whether both coefficients are significantly larger or smaller than zero. If this is the case, we can be confident about the sign of the true effect. In addition we can test which coefficient is further away from zero to assess the direction of OVB and estimate an upper or lower bound of the true effect.

We can also illustrate and show this using a simulation. In a Monte Carlo simulation, we can draw parameters from general distributions to account for the fact that we do not know the true cross-price elasticity, the size of random errors, and the degree to which omitted variables influence the endogenous parameter.

Simulation:

The Monte Carlo approach simulates four different scenarios, which vary by featuring an upward or downward bias and by having a true estimate that is either 0 or -1. As we cannot observe the true data generating process, we simulate a very general data generating process to assess the validity of our approach. We assume for the common demand shifters $DS_t \sim |\mathcal{N}(0, \sigma^2)|$. Moreover, we use $\eta \sim U[0, 1]$ for different degrees of endogeneity and $\varpi \sim |\mathcal{N}(0, \sigma^2)|$ for different cross-price elasticities.

The outcome, conflict in district d at time t is then

$$
\begin{aligned}
conflict_{d,t} = \alpha + \beta^{True} \times drug\,price_{t-1} \times suit_d + \tau_t + \delta_d + \\
\gamma \times suit_d \times OV_{t-1} + \vartheta_{d,t},
\end{aligned}
\tag{3.23}
$$

with $\vartheta_{d,t} \sim \mathcal{N}(0, \sigma^2)$. We add a positive constant α to always ensure a positive outcome, but this is not necessary and not biasing the results. In each round, we draw 1000 observations, clustered in 100 districts with 10 time periods, and compute all variables. We then in each row estimate the following.

$$
conflict_{d,t} = b^{True(1)} \times p_{t-1}^O \times suit_d + \gamma \times suit_d \times OV_{t-1} + \tau_t + \delta_d + \vartheta_{d,t},
\tag{3.24}
$$

$$
conflict_{d,t} = b^{Opium(2)} \times p_{t-1}^O \times suit_d + \tau_t + \delta_d + \vartheta_{d,t}^O,
\tag{3.25}
$$

$$
conflict_{d,t} = b^{Complement(3)} \times p_{t-1}^C \times suit_d + \tau_t + \delta_d + \vartheta_{d,t}^C.
\tag{3.26}
$$

We want to understand how the likelihood that $b^{complement} < b^{opium}$ depends on the direction of omitted variable bias and how likely it is that both point estimates

significantly differ from the true parameter value.

The simulation is repeated 5,000 times and we store b^1, b^2 and b^3 in each round, as well as SE^1, SE^2 and SE^3, and $pval^1$, $pval^2$ and $pval^3$. We then compute the likelihood that across all these different data generating processes i) the estimates using complement prices are more negative than the estimates using opium prices, ii) both prices yield a negative point estimate, and iii) both prices yield a negative point estimate and are significantly different from the true parameter value at the 5% level. These estimations are run for four different cases:

A. $\beta = 0$ and $\gamma = 1$.
B. $\beta = 0$ and $\gamma = -1$.
C. $\beta = -1$ and $\gamma = 1$.
D. $\beta = -1$ and $\gamma = -1$.

The implications would be the same for positive values of β. The simulation results verify the two main insights from above. First, if the true parameter value is 0, the likelihood that both the estimate using opium and using the complement are significantly smaller than zero is comparable to the false rejection rate. In simple terms, under rather general conditions it is extremely unlikely that both estimates would be significantly negative if the true effect is 0. Second, if the true parameter value is -1, we can compare the point estimates using the complement and the opium price to assess the direction of potential omitted variable bias. If the estimate using the opium price is downward biased, it is extremely unlikely that the point estimate using the complement price is more negative. If the point estimate using the complement price is in such a scenario more negative, the likelihood that both estimates are more negative than the true parameter value is extremely small. Our estimations reveal that both point estimates are negative and significantly different from zero with small p-values, and that the point estimate using the complement price is more negative. Thus, it is extremely unlikely that the true parameter value is equal or larger than zero, and it is likely that the estimates using the opium price is an upper bound of the true negative effect. Table 3.11 shows these results in detail for the scenarios A to D. Based on the table and Figures 3.12 and 3.13, scenario D seems to fit our data best. In that case, the estimate using the opium price is an upper bound of the true negative effect.

More specifically, the table and figures illustrate several important aspects. The figures clearly illustrate the differences between using the opium price or a complement price. First, we can see that in the majority of cases one of the estimates is higher, and one lower than the true value. Consequently, testing whether both are more positive or negative than 0 (or any value Z) is a good indication about the direction of the causal effect (or it being higher or lower than Z). It is apparent that the estimates using the complement are more dispersed and on average closer to zero. The dispersion comes from draws with a weaker cross-price elasticity, in which case the relationship with the

treatment and outcome are also weaker. The estimates are closer to zero compared to the opium estimates due to attenuation (see equation 3.22).

The first rows of Table 3.11 verify that the simulation works as intended. The parameter estimates using the true regression is close to the true relationship, the hypothesis of it being different is only rejected at the 5% level in 5% of the cases, about equal to the false rejection rate (row 4). Due to the omitted variable bias that we created, the estimates of the "short" regressions using opium or the complement alone differ quite often significantly from the true β. So how to make use of this strategy? First, one can check whether the estimate using the complement price is more negative or more positive than the estimate using the opium price. In our case it is more negative, clearly suggesting that scenario B or D (upward bias) is the relevant one for us. In B and D this can happen in 0.965 and 0.751% of all draws, whereas in A or C only in 0.033 and 0.075% of all cases.

Scenario B is based on a true effect of 0, scenario D on a negative true effect of -1. The second aspect we can consider is the likelihood of both estimates being more negative than the true effect. Rows 6 and 7 show that the likelihood that both are more negative is around 10% only, the likelihood that they are both significantly more negative even smaller.[40] As in our case, the estimate using the complement is more negative than using opium, we are also interested in the likelihood that this happens **and** both are significantly more negative than the true value. The last row shows that this likelihood is about equal to the false rejection rate.[41]

This is what we exploit in our case. First, estimates using opium (international heroin) prices, as well as prices using cocaine and an index of three complements are all negative. Consequently, it is unlikely that the true effect is not negative. Second, because both complement estimates are more negative, the (less negative) estimate using opium serves as an upper bound for the true negative effect. This is best visible in the right hand side of Figure 3.13.

---

[40]This likelihood is slightly higher with an upward bias in scenario A or C as we focus here on the likelihood of estimates being more negative. Considering simply whether both are significantly different (positive or negative), yields similar values for all scenarios.

[41]0.073 and 0.008, close to 5% if the true value is 0, and 0.032 and 0.028 if the true value is -1. This is due to the fact that in the former case (B) both "short" estimates are more often negative than in the latter case (D). The reasons is the attenuation of the complement estimate towards zero, making it less likely to be more negative than -1 in scenario D.

TABLE 3.11
Simulation

	A $\beta=0$ & Downward bias (1)	B $\beta=0$ & Upward bias (2)	C $\beta=-1$ & Downward bias (3)	D $\beta=-1$ & Upward bias (4)
$\bar{b}$(true)	-0.000	0.001	-0.999	-1.000
$\bar{b}$(opium)	-0.369	0.374	-1.369	-0.633
$\bar{b}$(complement)	0.248	-0.247	-0.296	-0.802
p [$b(true) \neq < \beta$]	0.051	0.052	0.048	0.051
b(opium)) $< \beta \wedge$ b(complement) $< \beta$	0.232	0.110	0.221	0.102
p [b(complement) $< \beta \wedge$ b(opium) $<\beta$]	0.087	0.036	0.091	0.035
b(opium) $<$ b(complement)	0.967	0.035	0.925	0.249
b(opium) $<$ b(complement) $< \beta$	0.018	0.099	0.069	0.057
p [b(opium)$<$ b(complement) $< \beta$]	0.015	0.028	0.065	0.010
b(complement) $<$ b(opium)	0.033	0.965	0.075	0.751
b(complement)$<$ b(opium) $< \beta$	0.214	0.011	0.151	0.045
p [b(complement)$<$ b(opium) $< \beta$]	0.073	0.008	0.032	0.028

Notes: Simulations with 5'000 repetitions. b(true) is the estimate from the true regression, i.e., one taking account of omitted variable bias (upward or downward). b(opium) is the estimate using the opium price, and b(complement) using the complement price. Row five gives an idea in which scenario we are in, looking at whether the estimate using opium or its complement is more negative. Combining row 5 and rows 8 and 9 indicates the likelihood of both estimates being negative given an upward or downward bias scenario. p( ) indicates that coefficient estimates are significantly different/more negative than the true value at the 5% level.

Stimulant or depressant? Resource-related income shocks and conflict

FIGURE 3.12

Simulations with true parameter estimate $\beta=0$. A: Downward bias, left side, B: Upward bias, right side

Stimulant or depressant? Resource-related income shocks and conflict
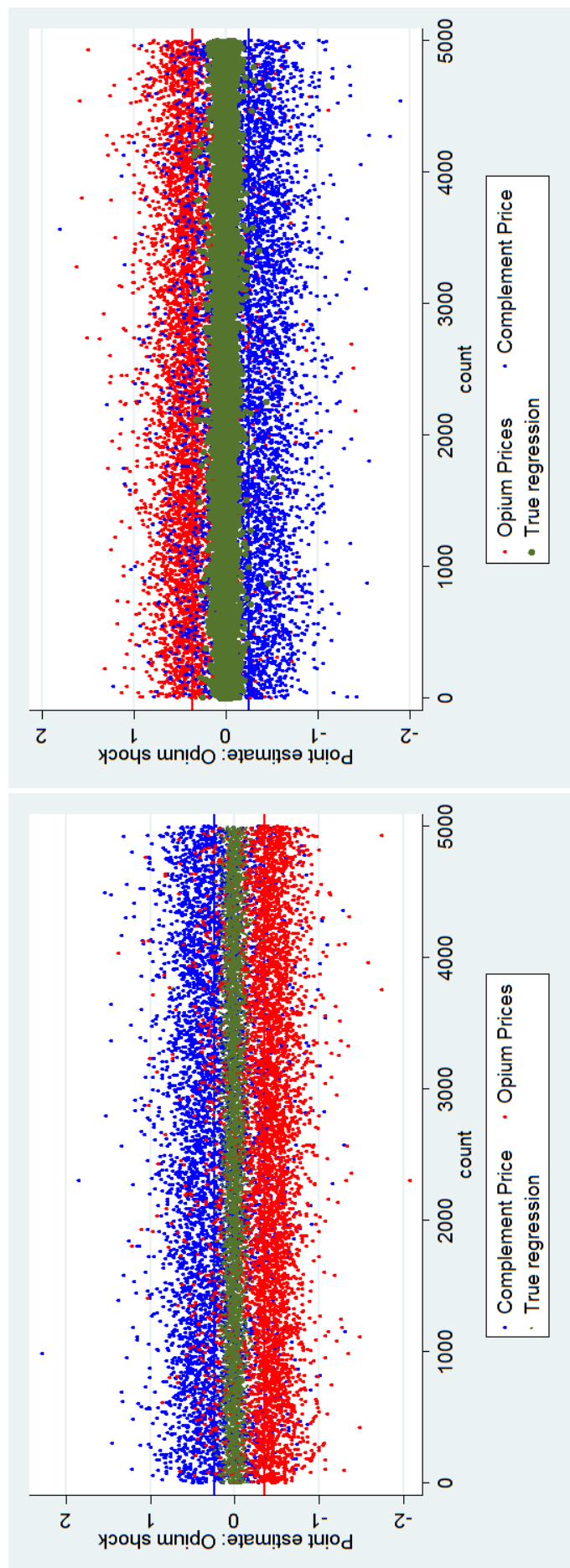
Figure 3.13

Simulations with true parameter estimate $\beta=-1$. C: Downward bias, left side, D: Upward bias, right side
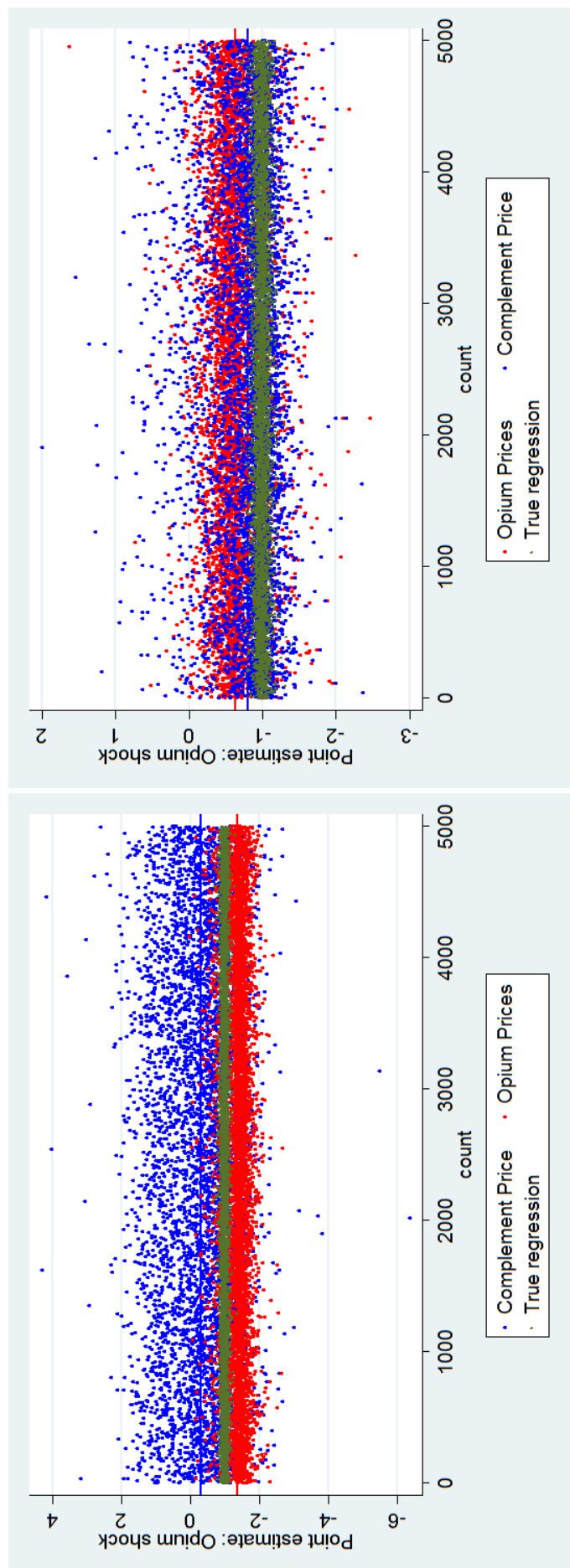
3.E.   Further results

Different timing of the shocks

TABLE 3.12

Leads and lags, 2002-2014

	Wheat shock: Not included (1)	Wheat shock: (t-1) (2)
Opium Profitability (t+1)	-0.066	0.011
	(0.251)	(0.254)
Opium Profitability (t)	-0.660**	-0.670**
	(0.320)	(0.319)
Opium Profitability (t-1)	-0.773***	-0.585*
	(0.289)	(0.314)
Number of observations	4776	4776
Adjusted R-squared	0.648	0.649

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is the logarithm of BRD in (t). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 3.13

Timing of shocks, 2002-2014

	(log) BRD (1)	1 if ≥ 5 (2)	1 if ≥ 10 (3)	1 if ≥ 25 (4)	1 if ≥ 100 (5)
	Panel A: Contemporaneous effect				
Opium Profitability (t)	-0.608**	-0.168**	-0.161**	-0.130*	-0.021
	(0.246)	(0.075)	(0.074)	(0.066)	(0.033)
Wheat Shock (t)	0.443***	0.092*	0.115***	0.064*	0.010
	(0.154)	(0.050)	(0.043)	(0.038)	(0.024)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.454	0.310
	Panel B: Lagged effect				
Opium Profitability (t-1)	-0.675**	-0.167*	-0.191**	-0.147*	-0.040
	(0.296)	(0.090)	(0.085)	(0.075)	(0.037)
Wheat Shock (t-1)	0.307**	0.088**	0.077**	0.034	-0.010
	(0.123)	(0.039)	(0.036)	(0.031)	(0.019)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.484	0.454	0.310

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Types of fighting

<div align="center">

TABLE 3.14

Types of fighting, 2002-2014

</div>

Conflict Actor:	All	Taleb.-Civil.		Taleb.-Gov.	
BRD:	Any	Both	Both	Taleb.	Gov.
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-0.675**	-0.098	-0.677**	-0.539***	-0.521*
	(0.296)	(0.069)	(0.302)	(0.187)	(0.274)
Wheat Shock (t-1)	0.307**	0.012	0.362***	0.134*	0.257**
	(0.123)	(0.026)	(0.124)	(0.079)	(0.115)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.200	0.658	0.555	0.596

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is the log of BRD in (t) for a specific type of conflict operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Regressions at the province level

TABLE 3.15

Effect of income shocks on opium revenues, province level, 2002-2014

	Outcome: (t)	Outcome: (t) and (t-1)
	(1)	(2)
Opium Profitability (t-1)	5.885*	5.461*
	(3.199)	(3.074)
Wheat Shock (t-1)	2.238	2.184
	(1.901)	(2.030)
Number of observations	442	442
Adjusted R-Squared	0.609	0.679

Notes: Linear probability models with province- and year-fixed effects. The dependent variable opium revenues in (t) is in logarithms. Standard errors clustered at the province level are displayed in parentheses. Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.16

Normalized drug prices, province level, 2002-2014

	Local Opium Price	International Heroin Price	Complement Price	International Cocaine Price
	(1)	(2)	(3)	(4)
Opium Profitability (t-1)	-0.290	-0.717	-1.101*	-0.661*
	(0.299)	(0.566)	(0.647)	(0.368)
Wheat Shock (t-1)	0.083	-0.027	-0.179	-0.105
	(0.385)	(0.404)	(0.401)	(0.409)
Number of observations	442	442	442	442
Adjusted R-Squared	0.723	0.724	0.726	0.726

Notes: Linear probability models with province- and year-fixed effects. The dependent variable is the log of BRD in (t). Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) as indicated in the column heading and the suitability to grow opium. Standard errors are in parentheses (clustered at the province level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Regressions at the household level: Living standards

TABLE 3.17

Living standard indicators, household level, 2002-2014

	(1)	(2)	(3)
	Dietary Diversity	**Calorie Intake**	**Food Security**
Panel A: Food consumption			
Opium Profitability (t-1)	0.571**	143.915	0.182*
	(0.289)	(256.750)	(0.110)
Wheat Shock (t-1)	0.144	103.729	-0.000
	(0.124)	(98.066)	(0.035)
Number of observations	72224	71634	71634
Adjusted R-Squared	0.371	0.139	0.197
Panel B: Food expenditures			
	Food Exp.	**Food Exp. Paasche adj.**	**Food Exp. Laspeyres adj.**
Opium Profitability (t-1)	698.905**	788.172**	750.822**
	(303.057)	(312.228)	(314.647)
Wheat Shock (t-1)	346.351***	366.635***	263.161**
	(109.688)	(110.396)	(112.203)
Number of observations	72643	72643	72635
Adjusted R-Squared	0.225	0.196	0.217
Panel C: Assets			
	Sum of Assets	**Sum of Assets weighted**	**Economically improved**
Opium Profitability (t-1)	0.925***	0.614***	0.431*
	(0.327)	(0.217)	(0.225)
Wheat Shock (t-1)	-0.066	-0.015	-0.228***
	(0.112)	(0.072)	(0.082)
Number of observations	72447	66620	70670
Adjusted R-Squared	0.323	0.336	0.249

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable in (t) is operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. All food expenditures are in 2011 prices. Standard errors are in parentheses (clustered at the district-year level). Significance levels: * 0.10 ** 0.05 *** 0.01.

FIGURE 3.14

Effect of opium profitability (t-1) on living standard indicators in (t), accounting for household survey weights
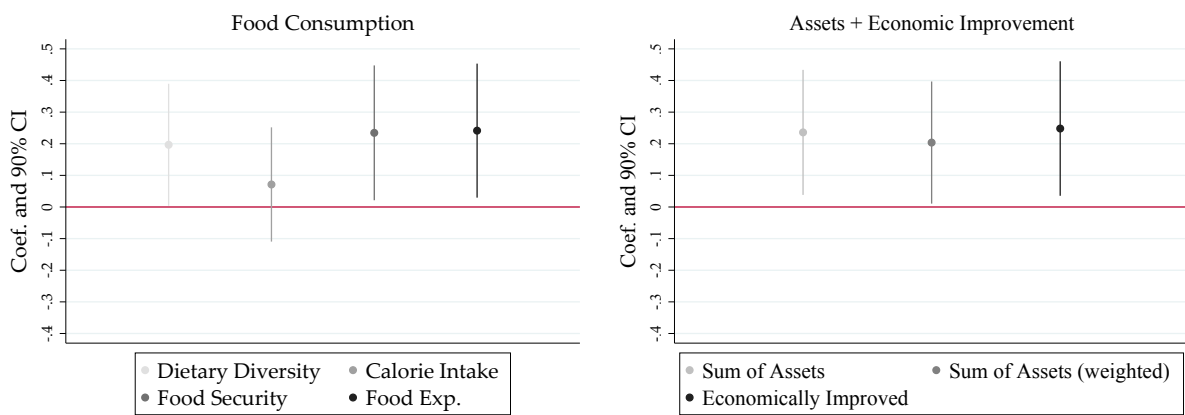
Notes: The figure shows results of 7 separate regressions in analogy to Table 3.17. The difference is that we include household survey weights in the regressions. Results are also robust to using robust standard errors rather than clustering at the district-year level (see Table 3.18).

TABLE 3.18

Living standard indicators, household level, robust SE, 2002-2014

	(1)	(2)	(3)
	Dietary Diversity	**Calorie Intake**	**Food Security**
Panel A: Food consumption			
Opium Profitability (t-1)	0.571***	143.915	0.182***
	(0.140)	(154.958)	(0.055)
Wheat Shock (t-1)	0.144***	103.729*	-0.000
	(0.049)	(58.041)	(0.018)
Number of observations	72224	71634	71634
Adjusted R-Squared	0.371	0.139	0.197
Panel B: Food expenditures			
	Food Exp.	**Food Exp. Paasche adj.**	**Food Exp. Laspeyres adj.**
Opium Profitability (t-1)	698.905***	788.172***	750.822***
	(142.659)	(139.481)	(139.569)
Wheat Shock (t-1)	346.351***	366.635***	263.161***
	(44.827)	(44.586)	(43.308)
Number of observations	72643	72643	72635
Adjusted R-Squared	0.225	0.196	0.217
Panel C: Assets			
	Sum of Assets	**Sum of Assets weighted**	**Economically improved**
Opium Profitability (t-1)	0.925***	0.614***	0.431***
	(0.168)	(0.121)	(0.112)
Wheat Shock (t-1)	-0.066	-0.015	-0.228***
	(0.053)	(0.037)	(0.037)
Number of observations	72447	66620	70670
Adjusted R-Squared	0.323	0.336	0.249

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable in (t) is operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. All food expenditures are in 2011 prices. Robust standard errors are in parentheses. Significance levels: * 0.10 ** 0.05 *** 0.01.

3.F.   Sensitivity analysis

Empirical model

TABLE 3.19

Normalized prices, district- and year-fixed effects, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Local Opium Price					
Opium Profitability (t-1)	-0.280***	-0.097***	-0.078***	-0.026	0.000
	(0.091)	(0.028)	(0.026)	(0.021)	(0.012)
Wheat Shock (t-1)	0.318***	0.084***	0.074**	0.049*	0.010
	(0.106)	(0.032)	(0.030)	(0.026)	(0.015)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.562	0.423	0.410	0.389	0.264
Panel B: International Heroin Price (Baseline)					
Opium Profitability (t-1)	-0.451**	-0.132**	-0.122*	-0.050	-0.010
	(0.209)	(0.064)	(0.063)	(0.051)	(0.023)
Wheat Shock (t-1)	0.289***	0.080**	0.067**	0.044*	0.008
	(0.110)	(0.032)	(0.031)	(0.027)	(0.016)
Adjusted R-Squared	0.561	0.421	0.409	0.389	0.264
Panel C: Complement Price					
Opium Profitability (t-1)	-0.707***	-0.217***	-0.172***	-0.091*	-0.028
	(0.222)	(0.068)	(0.064)	(0.050)	(0.023)
Wheat Shock (t-1)	0.207*	0.053	0.050	0.032	0.003
	(0.114)	(0.033)	(0.032)	(0.028)	(0.016)
Adjusted R-Squared	0.563	0.423	0.410	0.390	0.264
Panel D: International Cocaine Price					
Opium Profitability (t-1)	-0.363***	-0.102**	-0.088**	-0.046	-0.012
	(0.138)	(0.042)	(0.041)	(0.034)	(0.015)
Wheat Shock (t-1)	0.268**	0.075**	0.065**	0.040	0.006
	(0.108)	(0.032)	(0.030)	(0.026)	(0.016)
Adjusted R-Squared	0.562	0.422	0.410	0.390	0.264

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.20

Conditional logit: incidence, onset and ending, 2002-2014

	1 if ≥ 5 (1)	1 if ≥ 10 (2)	1 if ≥ 25 (3)	1 if ≥ 100 (4)
Panel A: Incidence				
Opium Profitability (t-1)	-6.376***	-6.823***	-6.849**	-3.148
	(1.764)	(2.519)	(3.249)	(6.672)
Wheat Shock (t-1)	-0.171	2.204**	2.631*	1.154
	(1.087)	(1.117)	(1.466)	(3.084)
Number of observations	4407	3510	2431	806
Pseudo R-Squared	0.350	0.272	0.272	0.213
Panel B: Onset				
Opium Profitability (t-1)	-4.505***	-6.076**	-5.729*	-1.719
	(1.686)	(2.375)	(3.092)	(5.601)
Wheat Shock (t-1)	1.062	2.860***	1.725	0.162
	(1.020)	(1.109)	(1.424)	(2.721)
Number of observations	2953	2739	1995	714
Pseudo R-Squared	0.170	0.136	0.149	0.149
Panel C: Ending				
Opium Profitability (t-1)	4.053**	0.445	-0.446	-9.784
	(1.698)	(2.430)	(2.939)	(8.124)
Wheat Shock (t-1)	0.363	-0.457	-1.357	-1.915
	(1.150)	(1.558)	(2.059)	(4.696)
Number of observations	1931	1195	730	207
Pseudo R-Squared	0.105	0.077	0.102	0.161

Notes: Conditional logit model with year- and district-fixed effects. The dependent variable is conflict onset/ending in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Modifications of the treatment variable: Drug prices

TABLE 3.21

Non-normalized drug prices, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Local Opium Price					
Opium Profitability (t-1)	-0.644***	-0.166***	-0.165***	-0.143***	-0.079***
	(0.200)	(0.059)	(0.056)	(0.052)	(0.030)
Wheat Shock (t-1)	0.341***	0.095**	0.090**	0.041	-0.016
	(0.121)	(0.038)	(0.035)	(0.031)	(0.017)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.650	0.502	0.484	0.454	0.311
Panel B: International Heroin Price (Baseline)					
Opium Profitability (t-1)	-2.103**	-0.503*	-0.550**	-0.465**	-0.183*
	(0.835)	(0.256)	(0.233)	(0.206)	(0.108)
Wheat Shock (t-1)	0.289**	0.085**	0.075**	0.030	-0.016
	(0.128)	(0.040)	(0.037)	(0.033)	(0.020)
Adjusted R-Squared	0.649	0.501	0.483	0.454	0.310
Panel C: Complement Price					
Opium Profitability (t-1)	-4.023***	-1.016**	-0.982***	-0.870***	-0.371**
	(1.337)	(0.399)	(0.364)	(0.329)	(0.176)
Wheat Shock (t-1)	0.221*	0.065	0.062*	0.016	-0.023
	(0.130)	(0.040)	(0.037)	(0.033)	(0.020)
Adjusted R-Squared	0.651	0.502	0.484	0.455	0.311
Panel D: International Cocaine Price					
Opium Profitability (t-1)	-3.594***	-0.888**	-0.871***	-0.780**	-0.318**
	(1.229)	(0.363)	(0.334)	(0.302)	(0.159)
Wheat Shock (t-1)	0.220*	0.066*	0.062	0.015	-0.023
	(0.130)	(0.040)	(0.038)	(0.033)	(0.020)
Adjusted R-Squared	0.651	0.502	0.484	0.455	0.311

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the drug prices (in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.22

International heroin price, price not in logarithms, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-6.970***	-1.665**	-1.781***	-1.438***	-0.553**
	(2.232)	(0.696)	(0.618)	(0.537)	(0.277)
Wheat Shock (t-1)	0.853**	0.250**	0.222**	0.107	-0.037
	(0.354)	(0.112)	(0.106)	(0.092)	(0.050)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-squared	0.649	0.501	0.483	0.454	0.310

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized international price (prices are not in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.23

International heroin price in deviations, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-6.197*	-1.434	-1.567*	-1.387*	-0.620*
	(3.136)	(0.875)	(0.887)	(0.747)	(0.350)
Wheat Shock (t-1)	0.303**	0.089***	0.080**	0.032	-0.017
	(0.122)	(0.031)	(0.036)	(0.027)	(0.020)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.310

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between international price deviations (from the mean) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Modifications of the treatment variable: Suitability

TABLE 3.24
Unweighted suitabilities, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-0.988***	-0.249***	-0.244***	-0.188**	-0.031
	(0.290)	(0.093)	(0.088)	(0.077)	(0.041)
Wheat Shock (t-1)	0.173	0.036	0.049	0.014	0.006
	(0.149)	(0.043)	(0.043)	(0.040)	(0.024)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-squared	0.650	0.501	0.483	0.454	0.310

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized international prices (in logarithms) and the unweighted suitability to grow opium (in analogy for wheat). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Modifications of the treatment variable: Dyadic DiD

TABLE 3.25

DiD, dyadic treatment, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Suitability dichotomized					
Opium Profitability (t-1)	-0.229***	-0.052*	-0.041	-0.042*	-0.017
	(0.087)	(0.027)	(0.026)	(0.022)	(0.013)
Wheat Shock (t-1)	0.072	0.027*	0.026*	0.007	-0.014**
	(0.051)	(0.016)	(0.015)	(0.013)	(0.006)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.648	0.500	0.482	0.453	0.311
Panel B: Suitability and Heroin Price dichotomized					
Opium Profitability (t-1)	-0.397***	-0.107***	-0.090**	-0.071**	-0.029
	(0.117)	(0.038)	(0.035)	(0.030)	(0.018)
Wheat Shock (t-1)	0.099	0.029	0.043	0.007	-0.025**
	(0.092)	(0.028)	(0.029)	(0.024)	(0.012)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.311

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Suitability (for opium and wheat) dichotomized according to the sample median in panel A. Suitability (for opium and wheat) and international prices (for heroin and wheat) dichotomized according to the sample median in panel B. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Outcome and timing (Reduced form and IV)

TABLE 3.26

Effect of income shocks on opium cultivation, 2002-2014

	Outcome: (t)	Outcome: (t)+(t-1)
	(1)	(2)
Opium Profitability (t-1)	0.483	0.705**
	(0.307)	(0.308)
Wheat Shock (t-1)	-0.173	-0.070
	(0.171)	(0.168)
Number of observations	5174	5174
Adjusted R-Squared	0.399	0.488

Notes: The dependent variables opium cultivation is in logarithms. Column 1 presents lagged effects. Column 2 reports lagged and contemporaneous effects by defining the outcome as the moving average, i.e., (revenues(t)+revenues(t-1))/2. Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) and the suitability to grow opium. Standard errors clustered at the district level are displayed in parentheses. Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.27

IVs for opium cultivation, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Second Stages					
Opium Profitability (t-1) and VHI (t-1) as IVs					
(log) Cultivation (t-1)	-0.469**	-0.132**	-0.125**	-0.083*	-0.022
	(0.213)	(0.063)	(0.063)	(0.048)	(0.022)
Number of observations	5173	5173	5173	5173	5173
Kleibergen-Paap F stat.	9.947	9.947	9.947	9.947	9.947
Hansen J p-val.	0.708	0.644	0.724	0.448	0.496
Panel B: First Stages					
Cultivation in (t-1)					
Opium Profitability (t-1)	0.811***	0.811***	0.811***	0.811***	0.811***
	(0.260)	(0.260)	(0.260)	(0.260)	(0.260)
VHI (t-1)	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Adjusted R-Squared	0.385	0.385	0.385	0.385	0.385

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium cultivation is in (t-1). Opium Profitability (t-1) and VHI (t-1) are used as IVs. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.28

IVs for opium revenues, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Opium Price Shock (t-1) as IV					
(log) Revenue:(t)+(t-1)	-0.173*	-0.049*	-0.045	-0.020	-0.004
	(0.099)	(0.030)	(0.030)	(0.022)	(0.010)
Number of observations	5085	5085	5085	5085	5085
Kleibergen-Paap F stat.	11.047	11.047	11.047	11.047	11.047
Panel B: VHI (t-1) as IV					
(log) Revenue:(t)+(t-1)	-0.374	-0.099	-0.099	-0.105	-0.032
	(0.299)	(0.084)	(0.084)	(0.079)	(0.033)
Number of observations	5084	5084	5084	5084	5084
Kleibergen-Paap F stat.	2.610	2.610	2.610	2.610	2.610
Panel C: Opium Price Shock and VHI (t-1) as IVs					
(log) Revenue:(t)+(t-1)	-0.193**	-0.054*	-0.051*	-0.029	-0.007
	(0.098)	(0.029)	(0.029)	(0.021)	(0.010)
Number of observations	5084	5084	5084	5084	5084
Kleibergen-Paap F stat.	6.170	6.170	6.170	6.170	6.170
Hansen J p-val.	0.374	0.464	0.413	0.079	0.266

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium revenues is operationalized as the moving average between (t) and (t-1). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.29

Corresponding first stage results for revenues (t)+(t-1), 2002-2014

	Opium Profitability	VHI	Opium Profitability, and VHI
	(1)	(2)	(3)
Opium Profitability (t-1)	2.489***		2.436***
	(0.749)		(0.748)
VHI (t-1)		-0.007	-0.005
		(0.004)	(0.004)

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is opium revenues. Opium revenues is operationalized as the moving average between (t) and (t-1). The corresponding IVs are indicated in the column heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.30

Alternative IVs for revenue (t-1), 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Opium Profitability (t-1) and VHI (t-1) as IVs					
(log) Revenue (t-1)	-0.162**	-0.045**	-0.042**	-0.027*	-0.007
	(0.071)	(0.021)	(0.021)	(0.016)	(0.008)
Number of observations	5103	5103	5103	5103	5103
Kleibergen-Paap F stat.	11.753	11.753	11.753	11.753	11.753
Hansen J p-val.	0.800	0.896	0.806	0.226	0.371
Panel B: Legal Opioids (t-1) as IVs					
(log) Revenue (t-1)	-0.193**	-0.058**	-0.046*	-0.025	-0.009
	(0.086)	(0.025)	(0.024)	(0.017)	(0.007)
Number of observations	5104	5104	5104	5104	5104
Kleibergen-Paap F stat.	13.050	13.050	13.050	13.050	13.050
Panel C: Legal Opioids (t-1) and VHI (t-1) as IVs					
(log) Revenue (t-1)	-0.192**	-0.056**	-0.046**	-0.030*	-0.010
	(0.075)	(0.022)	(0.021)	(0.016)	(0.007)
Number of observations	5103	5103	5103	5103	5103
Kleibergen-Paap F stat.	10.431	10.431	10.431	10.431	10.431
Hansen J p-val.	0.947	0.819	0.943	0.334	0.596

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium revenues are in (t-1). The corresponding IVs are indicated in the panel heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.31

Corresponding first stage results for revenues, 2002-2014

	Opium Profitability and VHI	Legal Opioids	Legal Opioids and VHI
	(1)	(2)	(3)
Opium Profitability (t-1)	2.798***		
	(0.721)		
VHI (t-1)	-0.012***		-0.011***
	(0.004)		(0.004)
Legal Opioids (t-1)		-15.384***	-14.878***
		(4.259)	(4.271)

Notes: Linear probability models with year- and district-fixed effects. The dependent variable is opium revenue in (t-1). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Standard errors

TABLE 3.32

Standard errors clustered at different levels, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: Clustered at district and year level					
Opium Profitability (t-1)	-0.675*	-0.167	-0.191*	-0.147*	-0.040
	(0.365)	(0.103)	(0.103)	(0.082)	(0.044)
Wheat Shock (t-1)	0.307**	0.088***	0.077**	0.034	-0.010
	(0.105)	(0.025)	(0.030)	(0.024)	(0.016)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.484	0.453	0.310
Panel B: Clustered at province and year level					
Opium shock (t-1)	-0.675*	-0.126	-0.191*	-0.147*	-0.040
	(0.365)	(0.104)	(0.103)	(0.082)	(0.044)
Wheat shock (t-1)	0.307**	0.144***	0.077**	0.034	-0.010
	(0.105)	(0.033)	(0.030)	(0.024)	(0.016)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.550	0.484	0.453	0.310

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are clustered as indicated in the panel heading. Significance levels: * 0.10 ** 0.05 *** 0.01

FIGURE 3.15

Wild-cluster bootstrap (clustered at the province level)
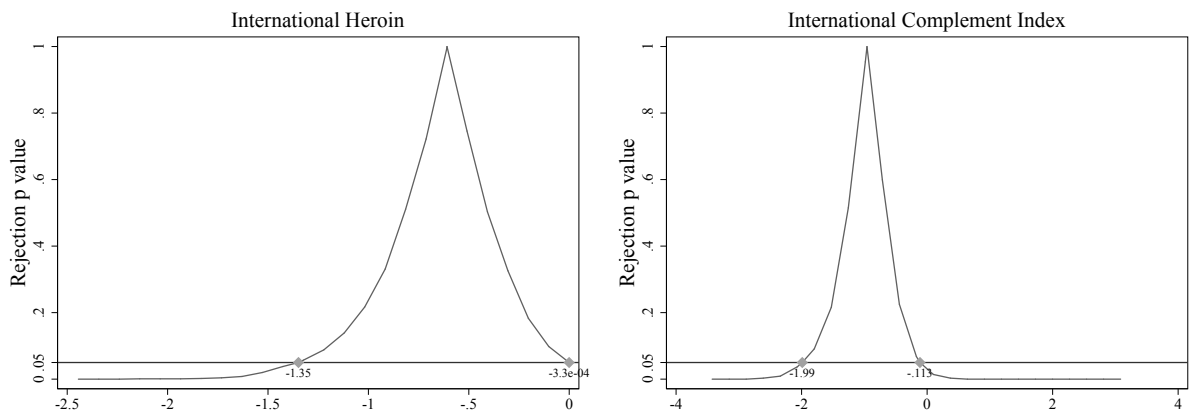
Notes: Figures show the distribution of bootstrap estimates. The dependent variable is the (log) of BRD. Regressions correspond to Table 3.2 column 1 (panels B and C). The number indicate the left and right 95% confidence interval. The test of the the null hypothesis at the 5% level is whether this intervall contains 0.

Covariates and trends

TABLE 3.33

No wheat shock included, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-0.923***	-0.238***	-0.253***	-0.175**	-0.031
	(0.279)	(0.084)	(0.079)	(0.069)	(0.030)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.310

Notes: Linear probability modelswith province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

TABLE 3.34

Lagged dependent, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-0.455*	-0.140*	-0.160**	-0.102	-0.021
	(0.252)	(0.084)	(0.076)	(0.065)	(0.032)
Wheat Shock (t-1)	0.260**	0.083**	0.068**	0.031	-0.008
	(0.103)	(0.036)	(0.032)	(0.027)	(0.015)
Dependent (t-1)	0.236***	0.114***	0.153***	0.228***	0.207***
	(0.023)	(0.019)	(0.023)	(0.027)	(0.040)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.670	0.508	0.496	0.482	0.340

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 3.35

Including covariates, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)

Panel A: Baseline covariates

Opium Profitability (t-1)	-0.595**	-0.177**	-0.188**	-0.132*	-0.014
	(0.275)	(0.086)	(0.082)	(0.070)	(0.038)
(log) Wheat shock (t-1)	0.282**	0.093**	0.077**	0.028	-0.019
	(0.129)	(0.041)	(0.037)	(0.032)	(0.019)
VHI (t)	0.000	-0.000	-0.000	0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Luminosity (t-2)	0.018	0.005	0.002	-0.003	-0.000
	(0.020)	(0.006)	(0.006)	(0.005)	(0.003)
(log) Population (t-2)	1.417	-0.478	0.037	0.611	0.789**
	(3.472)	(0.911)	(0.900)	(0.958)	(0.308)
Adjusted R-Squared	0.650	0.501	0.483	0.453	0.311

Panel B: Baseline covariates, time-invariant covariates×trend

Opium Profitability (t-1)	-0.680**	-0.182**	-0.199**	-0.175**	-0.030
	(0.269)	(0.084)	(0.081)	(0.068)	(0.039)
(log) Wheat shock (t-1)	0.269**	0.091**	0.082**	0.025	-0.016
	(0.130)	(0.041)	(0.037)	(0.032)	(0.020)
VHI (t)	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)
Luminosity (t-2)	0.014	0.004	0.002	-0.004	-0.001
	(0.020)	(0.006)	(0.006)	(0.005)	(0.003)
(log) Population (t-2)	-0.652	-0.910	-0.719	-0.232	0.910**
	(3.469)	(0.948)	(0.956)	(0.934)	(0.380)
Adjusted R-Squared	0.653	0.504	0.487	0.461	0.317

Panel C: Baseline covariates, time-invariant covariates×time dummies

Opium Profitability (t-1)	-0.754***	-0.209**	-0.222**	-0.186**	-0.040
	(0.289)	(0.089)	(0.087)	(0.073)	(0.042)
(log) Wheat shock (t-1)	0.276*	0.090**	0.081**	0.034	-0.020
	(0.141)	(0.043)	(0.041)	(0.036)	(0.022)
VHI (t)	-0.000	-0.001	-0.000	0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Luminosity (t-2)	0.011	0.003	0.001	-0.005	-0.000
	(0.020)	(0.006)	(0.006)	(0.005)	(0.003)
(log) Population (t-2)	-0.881	-0.868	-0.775	-0.352	0.860**
	(3.540)	(0.988)	(0.978)	(0.941)	(0.380)
Adjusted R-Squared	0.654	0.503	0.486	0.462	0.314

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. The set of time-invariant covariates includes Ruggedness, Ethnic Trafficking Route, Pashtuns, Mixed Ethnic Groups, Taliban Territory 1996, Mixed Territory 1996, Distance Linear, Distance 2D and 3D, Travel Time 2D and 3D (all distances to Kabul). The number of observation is 5173 in panel A, and 5174 in panels B and C. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

Outlier analysis

TABLE 3.36

Drop potential outliers, 2002-2014

	(log) BRD	1 if ≥ 5	1 if ≥ 10	1 if ≥ 25	1 if ≥ 100
	(1)	(2)	(3)	(4)	(5)
Panel A: No border districts					
Opium Profitability (t-1)	-0.601**	-0.160	-0.161*	-0.146*	-0.014
	(0.304)	(0.098)	(0.096)	(0.086)	(0.055)
Wheat Shock (t-1)	0.348**	0.113**	0.106**	0.027	-0.019
	(0.167)	(0.052)	(0.050)	(0.043)	(0.027)
Number of observations	3718	3718	3718	3718	3718
Adjusted R-Squared	0.678	0.523	0.513	0.483	0.342
Panel B: No Southern provinces (Kandahar and Hilmand)					
Opium Profitability (t-1)	-0.674**	-0.174*	-0.215**	-0.118	-0.007
	(0.311)	(0.096)	(0.091)	(0.078)	(0.033)
Wheat Shock (t-1)	0.319**	0.088**	0.072*	0.033	0.003
	(0.127)	(0.041)	(0.038)	(0.032)	(0.017)
Number of observations	4732	4732	4732	4732	4732
Adjusted R-Squared	0.620	0.480	0.458	0.407	0.255

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. In panel A all border districts are exlcuded and in panel B all districts in the two provinces Kandahar and Hilmand are excluded. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01.

FIGURE 3.16

Leave one out - year and province
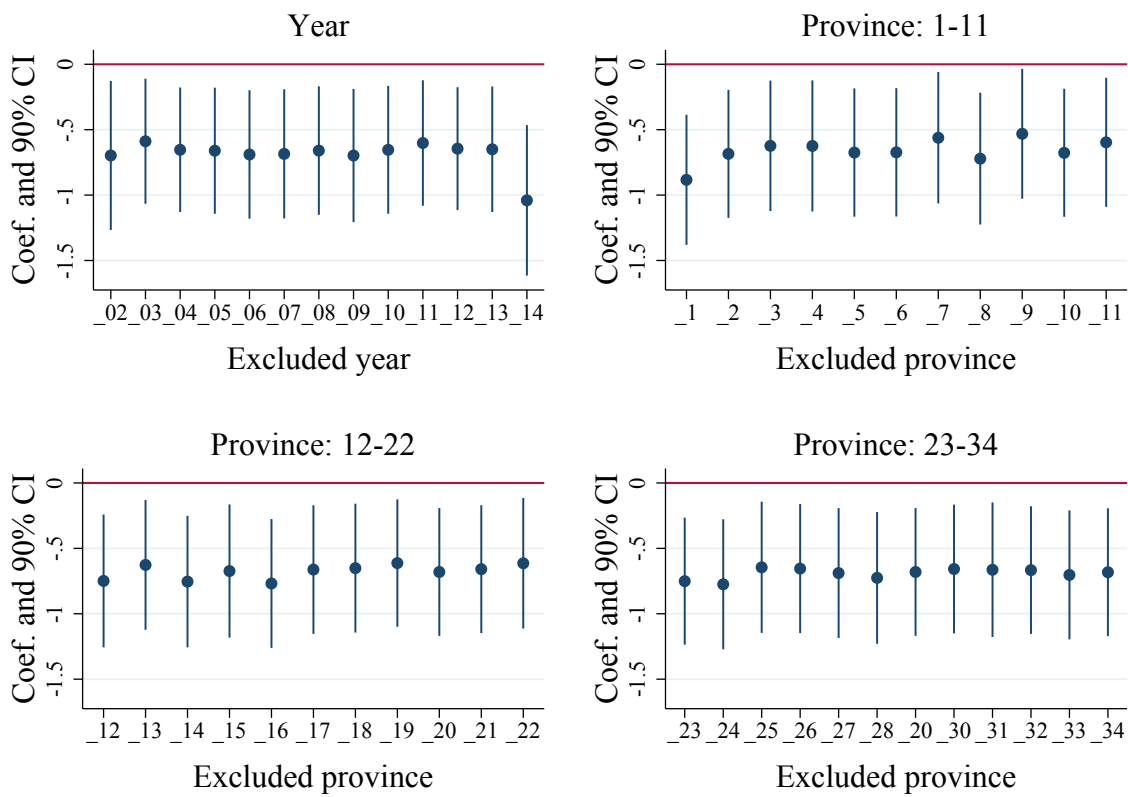
Notes: This figure shows results for 47 separate regressions in analogy to panel B's column 1 of Table 3.2, where we leave out one year or one province at the time. This also alleviates concerns whether particular outliers in the cross-sectional variation drive our result.

Randomization

FIGURE 3.17

Randomization: Heroin price and opium suitability
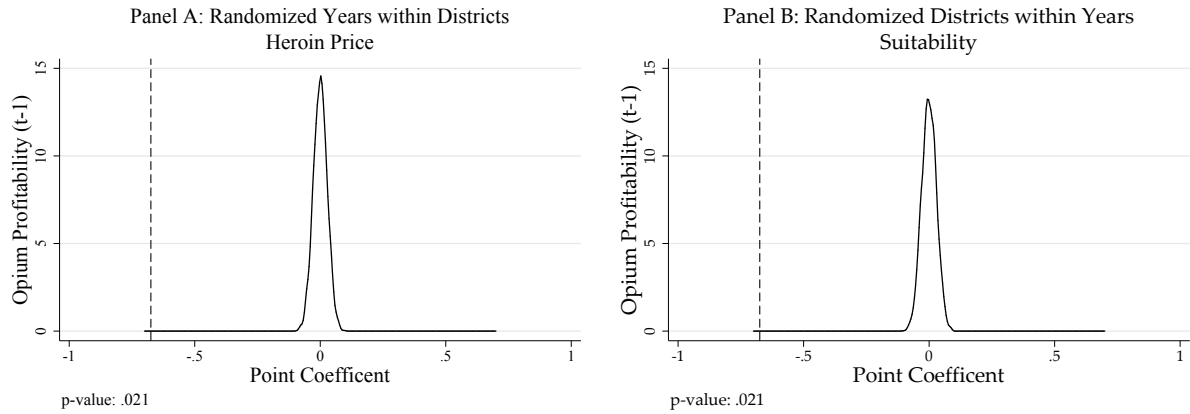
Notes: This figure plots the distribution of the coefficients generated by 5'000 randomizations, with panel A randomly reordering prices across years within districts and multiplying with the actual suitability and panel B reordering the suitability across districts and multiplying with the actual price in the respective yes. Based on the regression model in panel B's column 1 of Table 3.2. For this placebo test, we want to see whether the randomized coefficients are centered around zero, and what share of the draws turn out to be more negative than the actual treatment coefficient. This share is used to compute the randomization inference p-value shown in the bottom of the graph.

Robustness for Table 3.4 and Table 3.5

TABLE 3.37

Opportunities costs proxied by share of value added, 2002-2014

	Market Acess Population 2D	Market Acess Population 3D	Sum Markets and Lab
	(1)	(2)	(3)
Opium Profitability (t-1)	-0.6533**	-0.6534**	-0.5368*
	(0.3027)	(0.3027)	(0.3015)
Opium Profitability (t-1)*X	-0.2188	-0.0002	-0.1882
	(0.2678)	(0.0003)	(0.1292)
Number of observations	5174	5174	5174
Adjusted R-Squared	0.649	0.649	0.650

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables *X* see Appendix 3.A. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 3.38

Ethnic groups measured by NRVA, 2002-2014

	Any Pasthuns	Share Pasthuns	Ethnic Groups	
			1 if Mixed	Number
	(1)	(2)	(3)	(4)
Opium Profitability (t-1)	-0.062	-0.283	-0.403	-0.380
	(0.402)	(0.359)	(0.380)	(0.529)
Opium Profitability (t-1)*X	-1.157***	-1.005*	-0.524	-0.179
	(0.433)	(0.577)	(0.423)	(0.223)

Notes: Linear probability models with province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables *X* see Appendix 3.A. For this table the different measures on ethnic groups are derived from the NRVA 2003, which is not nationally representative, but serves as a suitable proxy for ethnic group distribution. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Robustness for Figure 3.8.

FIGURE 3.18

Variation in conflict across high and low suitable districts over time
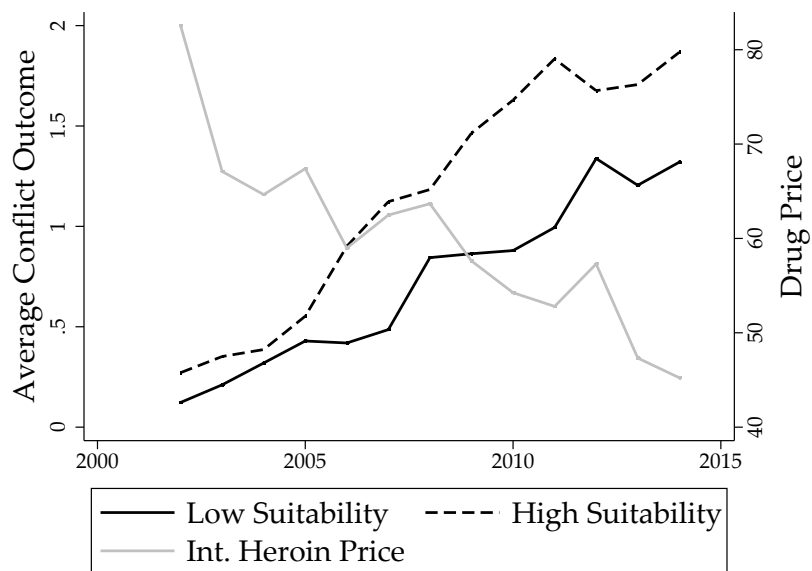
Notes: To assign a district to low or high suitability, this figure uses an alternative cut-off of 0.3.

3.G.   Additional maps

FIGURE 3.19

Opium suitability, opium markets, and processing labs
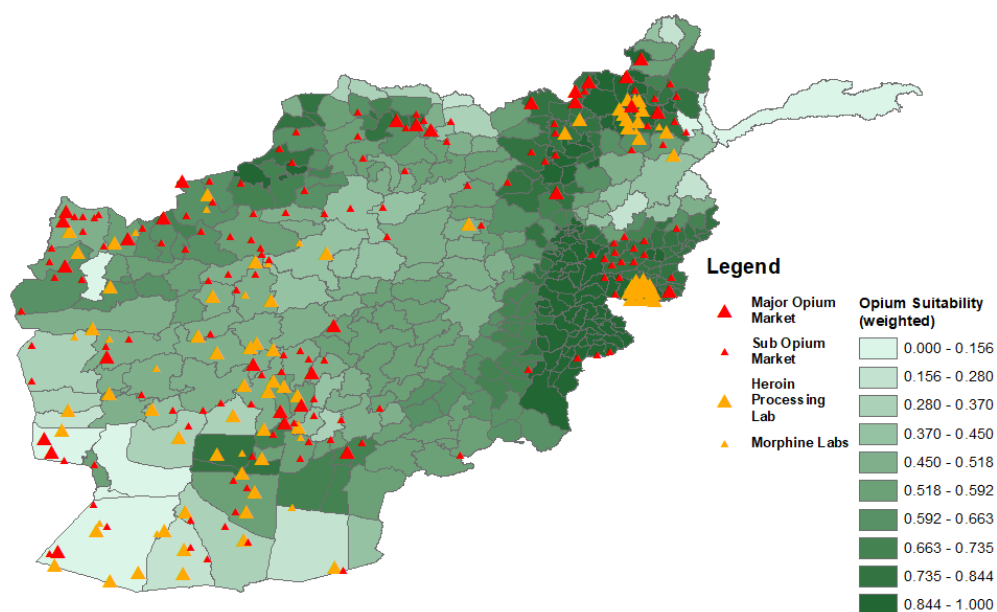
Notes: The figure plots the population weighted suitability. Market and lab information based on UNODC. Opium suitability based on Kienberger et al. (2017).

FIGURE 3.20
Ethnic groups
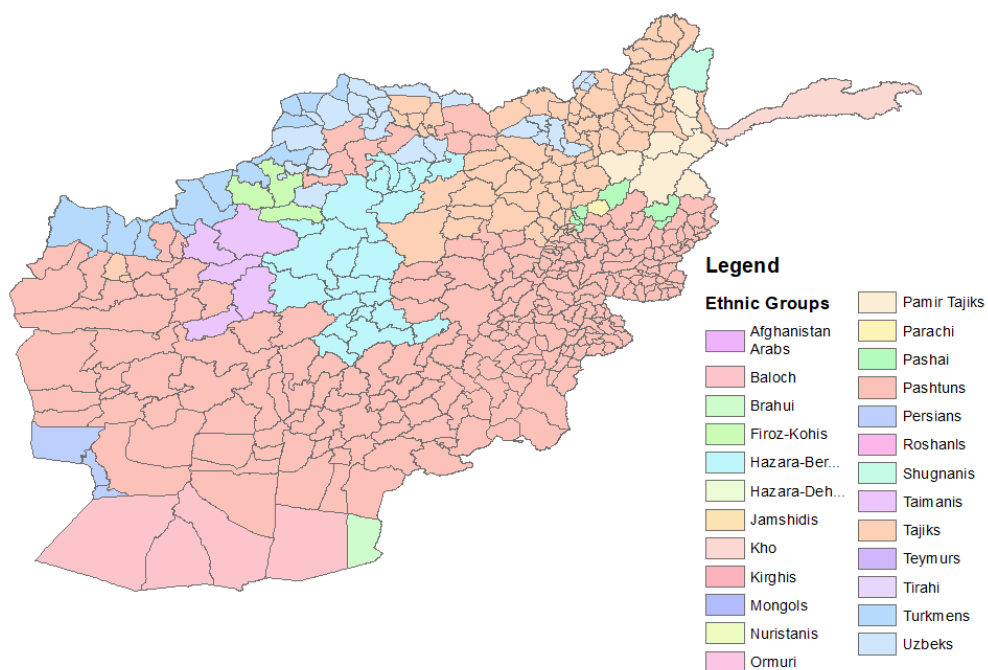
Notes: Distribution of ethnic groups (homelands) in Afghanistan. Note that these are partly overlapping polygons, i.e., some districts feature more than one group even though this is not visible in the map, but we account for this in later estimations. Source: GREG (Weidmann et al., 2010).

FIGURE 3.21
Road network
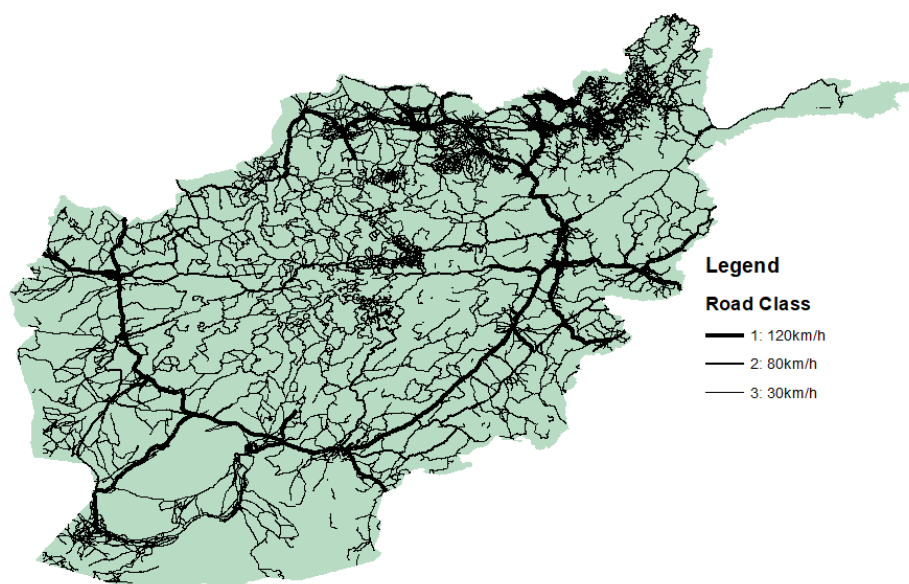
Notes: The road network in Afghanistan distinguishing in highways (assumed speed 120 km/h), rural roads (ass. speed 90 km/h), and urban roads (ass. speed 50 km/h). The distinction in road types and the choice of average speed is not decisive for our results.

FIGURE 3.22
Elevation, 2D and 3D route
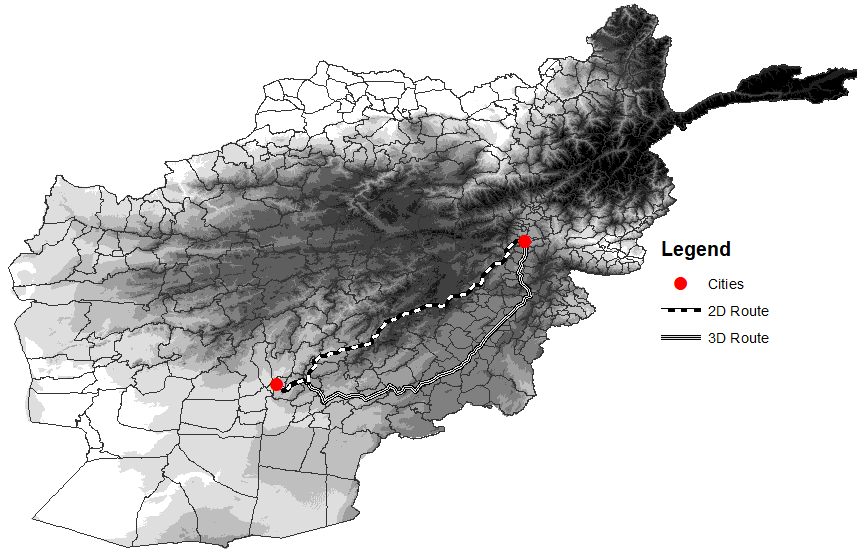
Notes: The intensity of black indicates the elevation in Afghanistan. The white-black dashed line shows the shortest road distance between to district centroids. The second white/black line indicates the shortest distance when accounting for elevation differences along the roads. In particular the central part of Afghanistan is very mountainous, which can have a large effect on transportation costs and travel time.

FIGURE 3.23
Distribution of objective and subjective conflict indicators, 2002-2014
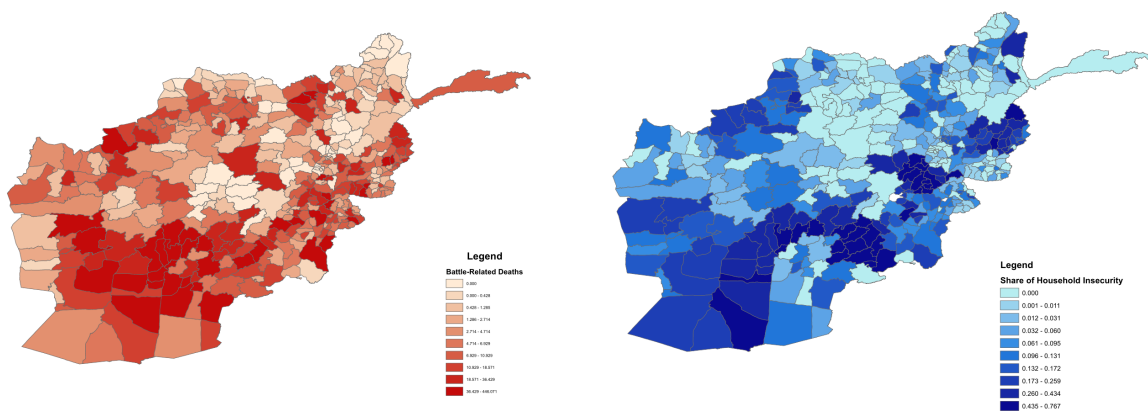
(A) Number of battle-related deaths (UCDP GED)

(B) Share of households experiencing insecurity shock (NRVA)

FIGURE 3.24
Political control in Afghanistan in the fall of 1996
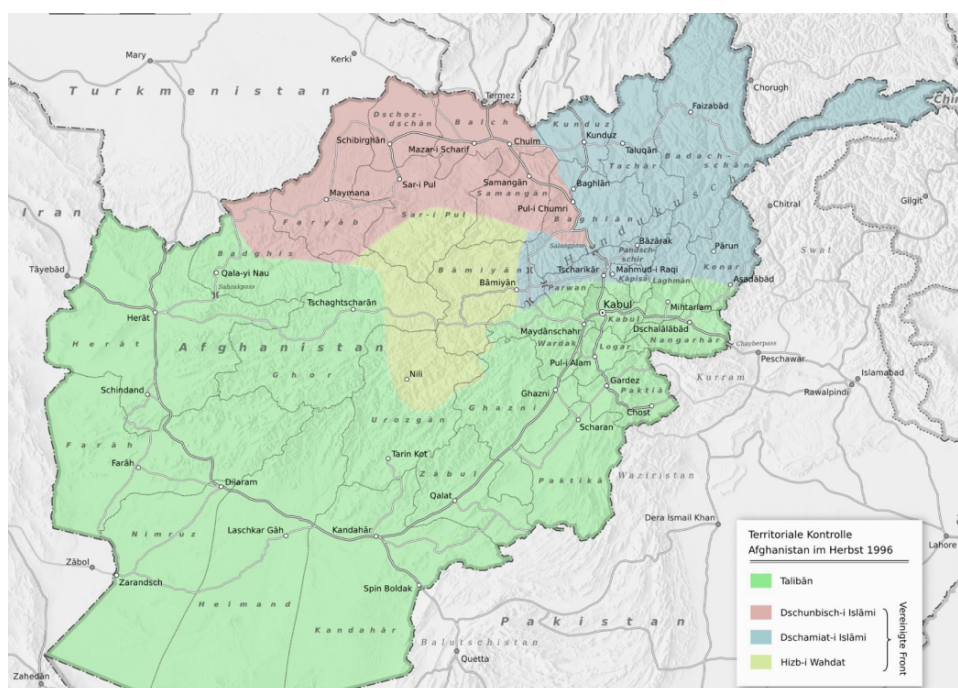
Notes: The figure is an excerpt from a book by Dorronsoro (2005). We georeference the green area as the area formerly under Taliban control, and the other three polygons as not under Taliban control.

3.H.   Data coding and map generation

Processing and trafficking

There is little to no information that is publicly available on trafficking routes that might be used to smuggle opium through and out of the country. Nevertheless, the UN Office on Drugs and Crime creates and contains spatial maps in its public reports. We were able to digitize a UNODC map from 2007 (about the middle of our sample period) by taking image files of the maps themselves and georeferencing specific points on the images (border points) to a geographically accurate projection of Afghanistan. This process was continued until the map and the images matched perfectly. We then digitized the data contained in the image about the important roads used for trafficking, and the other variables such as main opium markets, heroin processing labs.

Original UNODC map (2007)
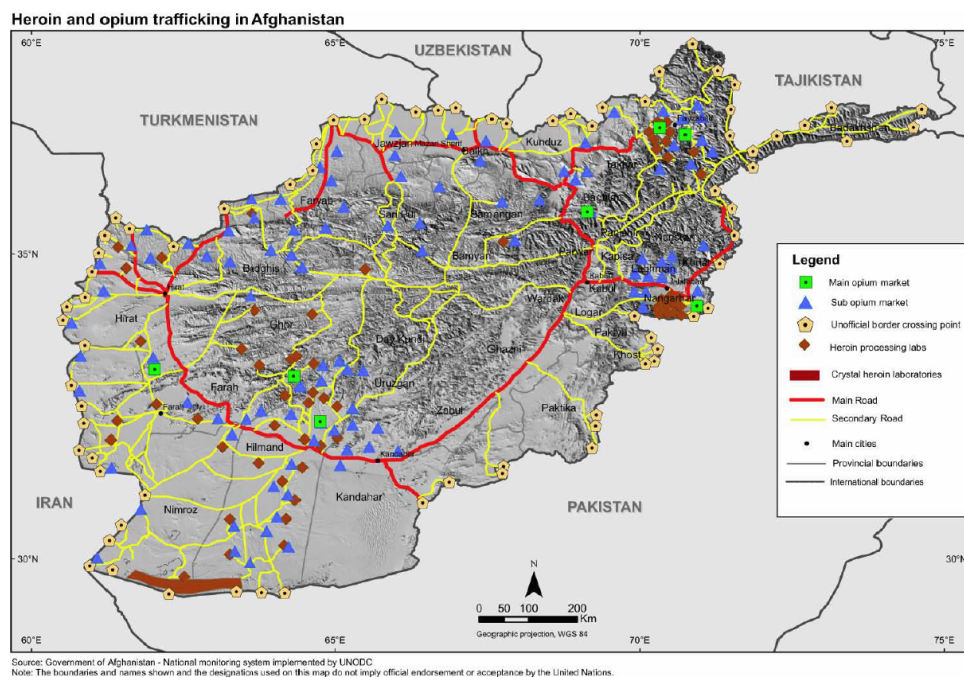
Map making process: The source of the original map comes is the UNODC's 2007 Afghanistan Opium Survey. The map depicts major and secondary roads, main cities, opium markets, border crossing points, and processing labs. We also used the 2009 Afghanistan Opium Survey to cross-validate the data points. In almost all cases, there were no changes between the two years. In case the 2009 map identifies additional markets or labs we added these as data points. Given that the location of illegal markets and labs will always contain some measurement error and could be moved over time, our aim is to code variables that measure the potential for a trafficking route, border crossing, market or lab. This means that the indicators that we create are time-invarying, also due to

the availability of data. We interact the binary indicators extracted from the map with an exogenous variable, so that the interaction term can be interpreted as causal under relatively mild circumstances.

Superimposed maps

In the next step of the process, we match the borders of the image and the georeferenced (Coordinate system GCS WGS 1984) shapefile for Afghan authorities (ESOC Princeton, https://esoc.princeton.edu/files/administrative-boundaries-398-districts). This way, we are accurately overlaying the data points and not simply making an educated guess as to where to place the points. Below are the two final digitized maps based on the UNODC data, overlaid with the district data. The binary indicators that we use in Section 3.6 on heterogeneous effects are coded as one if the respective feature is present within the boundaries of the district polygon at least once. Alternatively, we use the number of feature per district, e.g., for opium markets.

FIGURE 3.27
Final map 1
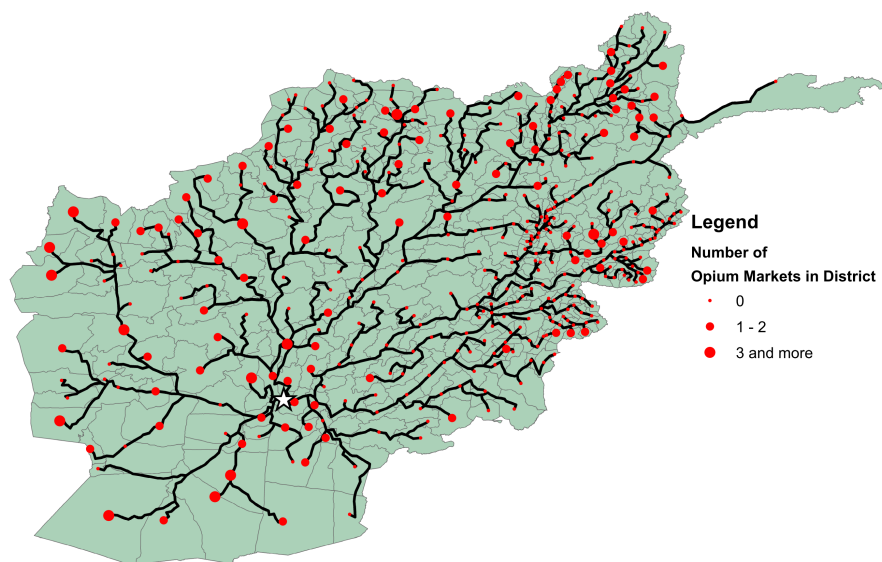
Notes: The dots indicate district-specific centroids, and the black lines are the shortest roads connections to the other centroids in the network. To compute market access, the same computation is done for every centroid in the district, leading to different optimal road connections. The distances are then used as weights and multiplied with the importance of the respective network members, in this case the number of drug markets. Sources: UNODC (2016), Open Street Map and Afghanistan Information Management Service (AIMS).

FIGURE 3.28
Final map 2
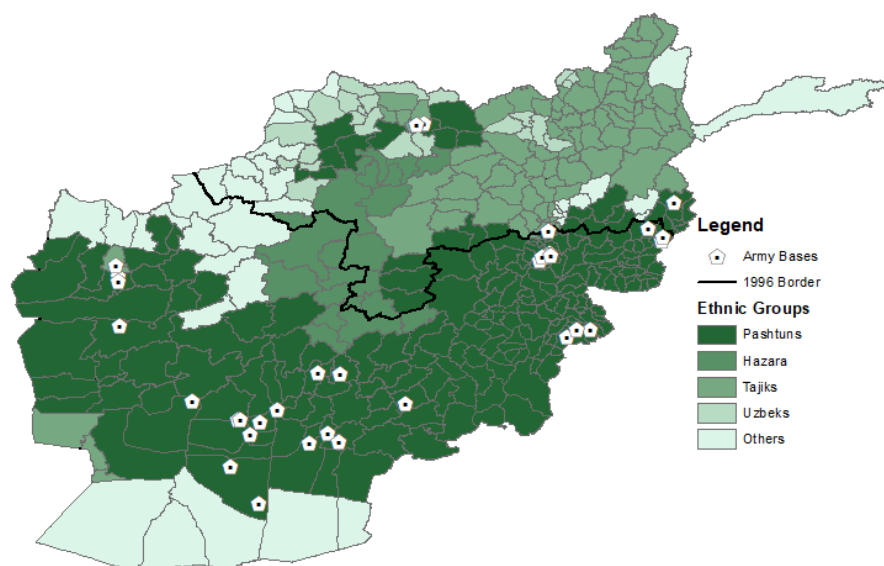
Notes: The map shows the four major ethnic groups in Afghanistan in different shades of green (Source: GREG). The white symbols with the black dots indicate the location of a foreign military base, for which we could track location, opening and closing date (sources in detail in the appendix). The area south of the thick black line was controlled by the Taliban prior to 2001. (Dorronsoro, 2005).

Major military bases

This section describes how we determine the locations of major known military bases in Afghanistan. There are nearly 400 foreign military bases in Afghanistan, but the bases mostly release no official information as to their geographic location for security reasons. In order to find this information, we compile data from different sources about the most relevant bases to include, where exactly (latitude and longitude coordinates) these bases are (or were; many are now closed). We rely on information from Wikipedia's GeoHack program for the more well-known bases and on news articles, Wikimapia and Google Maps satellite data for the less well-documented ones. News articles were useful in this case because they are often allowed to publish the district in which these bases are located; from there, we were able to look for these bases by referencing photos of the bases (if available) with available satellite data to verify their location. Below, we show the table with the locations of the about 50 bases that we could identify. The exact locations are blackened out for confidentiality reasons, even though we are convinced none of this information is confidential and could be misused or endanger soldiers. Without access to confidential NATO and US military information this is the best data we could assemble. It is certainly not a complete list of bases, which introduces considerable measurement error to the indicator variable we create based on it. At the same time, we have no reason to expect this measurement error to be non-normal.

The following table shows the available data for about 50 bases that we deemed to be the most important foreign bases in Afghanistan over the last 15 years. We list the name, type, location (coordinate system CGS WGS 1984), militaries present (countries of origin), district in which the base is located, date opened and closed (a "." in the opened or closed section means there is either no data for closure time or that the base is still open. See Field 9 for explanatory notes in these cases), and general notes of interest.

Main bases and relevant information (1/2)

OBJECTID*	Base Name	Installation Type	Militaries Present	Lat	Lon	District
1	Delaram	FOB	USMC			Delaram
2	Leatherneck	Camp	USMC			Nahri Saraj
3	Kabul International Airport	Camp	ISAF, Turkish Army, US Army, USMC, USAF, Mongolian Armed Forces			Kabul
4	Kandahar Airfield	Airfield	RAF, USAF, US Army			Kandahar
5	Shindand Airbase	Airbase	USAF, AAF			Shindand
6	Bagram Airfield	Airfield	US Army, USAF			Bagram
7	Bastion	Camp	British Army, RAF, Royal Navy (RN), Royal Marines (RM), USMC, Estonian Land Forces, Danish Defence, Tonga Defence Services			Nahri Saraj
8	Price	MOB	RM, British Army, Danish Defence, US Army, USMC			Nahri Saraj
9	Lashkar Gah	MOB	British Army, RM			Lashkargah
10	Eggers	Camp	NATO, US Army, USMC, US Air Force, Australian Army, New Zealand Army, Turkish Army, French Army, Mongolian Armed Forces			Kabul
11	Salerno	FOB	US Army, USAF, US Navy			Khost (Matun)
12	Chapman	FOB	US Special Operation Command, US Army, CIA			Khost (Matun)
13	Marmal	Camp	German Army, German Navy, German Air Force, Royal Netherlands AF, Swedish Air Force, US Army, Mongolian Armed Forces			Mazar-e Sharif
14	Dwyer	Camp	USMC, British Army, RM			Garmsir
15	Rhino	Camp	USMC, US Navy, US Army, USAF, SASR			Garmsir
16	Holland	Camp	Australian Army, New Zealand Army, US Army, Royal Netherlands Army, ANA			Tarin Kot
17	Black Horse	Camp	US Army, Canadian Army			Kabul
18	Dogan	Camp	<Null>			Kabul
19	Invicta	Camp	Italian Army			Kabul
20	Julien	Camp	Canadian Army			Kabul
21	Julien	Camp	Canadian Army			Kabul
22	Phoenix (Qargha)	Camp	US Army			Kabul
23	Souter	Camp	British Army			Kabul
24	Warehouse	Camp	Canadian Army			Khost (Matun)
25	Pucino	Camp	USSOCOM			Mandozayi
26	Clark	Camp	US Army			Waygal
27	Blessing	Camp	US Army, USMC			Nari
28	Bostick	FOB	US Army			Sarkani
29	Joyce	FOB	US Army			Asadabad
30	Wright	Camp	US Army			Bagram
31	Albert	Camp	US Army			Bagram
32	Blackjack	Camp	US			Bagram
33	Bulldog	Camp	US			Bagram
34	Civilian	Camp	US			Bagram
35	Cunningham	Camp	US			Bagram
36	Gibraltar	Camp	US			Bagram
37	Warrior	Camp	US			Bagram
38	Pratt	Camp	US Army			Mazar-e-Sharif
39	Spann	Camp	US Army			Mazar-e-Sharif
40	Baker	Camp	Australian Army			Daman
41	Nathan Smith	Camp	Canadian Army, US Army			Kandahar
42	Hadrian	Camp	Royal Netherlands Army			Deh Rawod
43	Russell	Camp	Australian Army			Tarin Kot
44	Hamdullah	FOB	USMC, British Army, RM			Sangin
45	Arena	Camp	Italian Army, Italian Air Force, US Army			Hirat
46	Stone	Camp	Carabinieri, US Army			Hirat
47	Vianini	Camp	Italian Army			Hirat
48	Losano	Camp	RNLAF, US Army, USAF			Kandahar
49	Lagman	FOB	US Army, US Navy, Romanian Army, ANA			Qalat
50	Shorabak	Camp	ISAF, US, Britain, Denmark, Estonia, Tonga			Lashkargah
51	Pasab (Wilson)	FOB	US Army			Panjwayi

Main bases and relevant information (2/2)

Opened	Closed	Field9	Notes	Shape *
2009	2014	<Null>	<Null>	Point
2008	2014	<Null>	Regional Command Southwest Headquarters	Point
2001	.	Open	ISAF Headquarters, ISAF Joint Command Headquarters, Headquarters for RC-Capital	Point
2001	.	Open	RC-S headquarters	Point
2004	2014	<Null>	<Null>	Point
2001	.	Open	Largest US base in Afghanistan, RC-East Headquarters	Point
2006	2014	<Null>	Main British base and formerly home to Task Force Helmand	Point
2006	2014	<Null>	<Null>	Point
2006	2014	<Null>	<Null>	Point
2006	2014	<Null>	NATO Training Mission – Afghanistan Headquarters	Point
2003	2013	<Null>	<Null>	Point
2001	.	Open	Major CIA and Special Operations counter-insurgency outpost	Point
2005	.	Open	<Null>	Point
2007	2009	<Null>	<Null>	Point
2001	2002	<Null>	First Marine land base in Afghanistan	Point
2006	2013	<Null>	<Null>	Point
2008	2013	<Null>	<Null>	Point
2002	2015	<Null>	<Null>	Point
2006	2012	Close unk, camp was open in 2012	<Null>	Point
2003	2005	<Null>	Reopened as a Counterinsurgency Academy in April 2007	Point
2007	.	Open	Reopened as a Counterinsurgency Academy in April 2008	Point
	.	Open	Opening unknown	Point
2007	2014	Slated to close in 2014	<Null>	Point
2002	2014	Slated to close in 2014, Canada withdrew all troops at this time	<Null>	Point
2002	2013	<Null>	<Null>	Point
	.	Open unk, close unk	<Null>	Point
2002	2011	<Null>	<Null>	Point
2006	2012	<Null>	<Null>	Point
2002	2013	Close unk, camp was open in 2013	<Null>	Point
2001	.	Close unk	<Null>	Point
2004	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
	2012	Open unk, close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
	2012	Open unk, close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
2003	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
2004	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
2002	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
	.	Open	Located in/related to Bagram Airfield, opening date unknown	Point
	2014	Open unk	<Null>	Point
	2014	Open unk, between 2001 and 2004	<Null>	Point
2006	2015	<Null>	Located in/related to Kandahar Airfield	Point
2003	2013	<Null>	<Null>	Point
	2013	Open unk, task force Uruzgan started 2006	<Null>	Point
2005	2013	<Null>	<Null>	Point
2007	2014	<Null>	<Null>	Point
2012	.	Open	<Null>	Point
Before/in 2008	2014	<Null>	<Null>	Point
Before/in 2006	2012	<Null>	<Null>	Point
	.	Open unk, close unk	Located in/related to Kandahar Airfield	Point
2004	2014	<Null>	<Null>	Point
2005	.	Open	ISAF logistics hub	Point
	2014	Open unk, slated to close in 2014	<Null>	Point

Confirming the location of these districts using satellite

This is an example of what the Wikimapia satellite imagery we used to locate bases looks like. This is an image of Base Blackhorse, which is now closed. We were able to locate this as Base Blackhorse by first searching for the camp on wikimapia which offered two possible locations (approximately 9 miles away from each other) where the base could be. After we discovered in a news report that the base was located next to an Afghan National Army base, which was itself located on the site of the Pul-e-Charkhi-Prison, we were able to determine the definitive location of the prison and thus the location of the base.

Definitions and explanation of how each base was found. Below, we have laid out the definitions for what each type of base exists in Afghanistan and explained how we determined the specific locations for each base we included. The base definitions are important to know because the type of base is a good indicator of its size. Though this was of course not the only criteria we used to determine whether or not a specific base should be represented on the map, it was important for weeding out those that are not included (for example, we included no firebases on account of their temporary and generally small size). Below this, we provide more detail about specific bases whose locations we were not able to get from the GeoHack database, in which bases are supposed to have had multiple confirmations. These bases were found using satellite data and through available news reports, photos and satellite imagery. All definitions below are adapted or directly from Wikipedia to provide a rough idea about the types of military bases that exist in Afghanistan. We do not rely on the distinctions and simply code whether there is an open base or not.

Additional information about bases (from wikipedia):

- Definition FOB - A forward operating base (FOB) is any secured forward military position, commonly a military base, that is used to support tactical operations. A FOB may or may not contain an airfield, hospital, or other facilities. The base may be used for an extended period of time. FOBs are traditionally supported by Main Operating Bases that are required to provide backup support to them. A FOB also improves reaction time to local areas as opposed to having all troops on the main operating base.

- Definition MOB - A MOB is a term used by the United States military defined as a permanently manned, well protected base, used to support permanently deployed forces, and with robust sea and/or air access.

- Definition COP - A combat outpost is a detachment of troops stationed at a distance from the main force or formation, usually at a station in a remote or sparsely populated location, positioned to stand guard against unauthorized intrusions and surprise attacks; the station is occupied by troops, it is usually a small military base or settlement in an outlying frontier, limit, political boundary or in a foreign country.

- Definition Firebase - A temporary military encampment to provide artillery fire support to infantry operating in areas beyond the normal range of fire support from their own base camps.

- Definition Camp - A semi-permanent facility for the lodging of an army. Camps are erected when a military force travels away from a major installation or fort during training or operations, and often have the form of large campsites.

- Definition Base - A facility directly owned and operated by or for the military or one of its branches that shelters military equipment and personnel, and facilitates training and operations. In general, a military base provides accommodations for one or more units, but it may also be used as a command center, a training ground, or a proving ground. In most cases, a military base relies on some outside help in order to operate. However, certain complex bases are able to endure by themselves for long periods because they are able to provide food, water and other life support necessities for their inhabitants while under siege.

All locations are taken from Wikimedia's GeoHack program if available. We do not consider Firebases and COPs, which are smaller and often temporary outposts. In addition, we found or updated the information for the following cases:

1. COP/FOB Zangabad has been coded as FOB Pasab. This was the most likely location for a forward operating base close the Zhari/Panjwayi district border. Exact location determined as such using Wikimapia satelite imagery. It is coded as being in the district of Panjwayi.

2. Camp/FOB Hadrian location determined using Wikimapia satellite imagery.

3. Camp Russell location determined using Wikimapia satellite imagery in relation to Camp Holland.

4. Camp Arena, Camp Vianini, and Camp Stone are each in roughly the same area. Using Wikimapia imagery, we assume that Camp Arena, the only camp with an Italian Air Force presence, is located at the airfield in Hirat. Camp Vianini and Camp Stone were assigned their locations using Wikimapia imagery as well. We believe Camp Vianini to be at the location we chose based on the fact that an Italian artillery regiment was attacked at that location and we believe the Italian Army was the only major force at Camp Vianini. Camp Stone, which has multiple country forces at its location, is expected to be south of the airport and Camp Arena, according to Wikimapia data.

5. Camp Blackhorse determined using Wikimapia and various sources citing the camp to be adjacent to the Pul-e-Charkhi ANA compound.

6. Camp Clark determined using Wikimapia satellite imagery.

7. Camp Warehouse determined using Wikimapia satellite imagery.

8. Camp Phoenix location determined using google maps and Wikimapia satellite data.

9. Camp Invicta located using Wikimapia satellite data.

10. FOB Hamidullah located using Wikimapia satellite data. In Wikimapia, the location is described as FOB Nolay, the previous name of the base.

11. Camp Blessing located using Wikimapia satellite data.

12. FOB Joyce located using satellite data and with news articles stating that FOB Joyce is within/very close to the village of Serkanay.

13. Camp Wright located using Wikimapia and Google Maps satellite data; it is listed as "USA Army Base" on the Wikimapia site.

Final map of located military installations
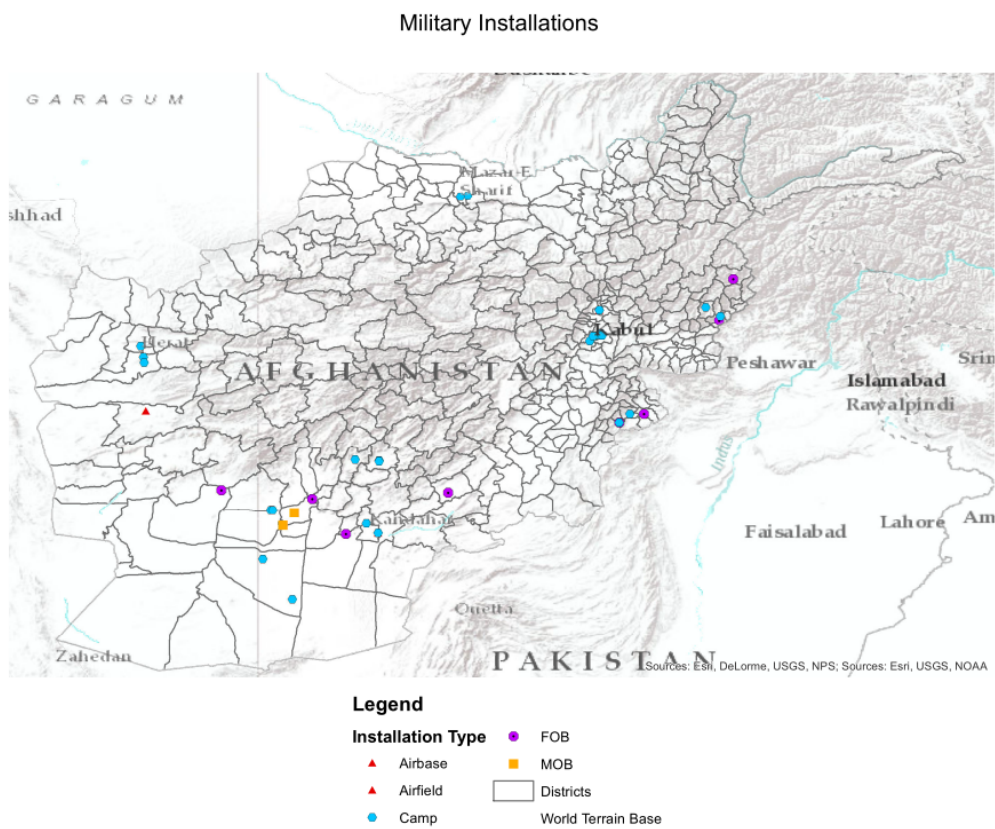
This map shows the geographic location of the bases that we identified. Some bases are not visible in this view as a result of closely overlapping with other bases, in which case the map displays only one symbol.

Chapter 4

Foreign interventions and community cohesion in times of conflict

Single-authored

Abstract

This paper analyses how foreign military interventions relate to the internal cohesion of communities and the role of local institutions in times of conflict. I consider the case of Afghanistan where households have been exposed to conflict for decades. Given an environment where formal institutions are unstable or even lacking, the local community becomes more important. Relying on support from others in the community is thus a common strategy for households to cope with different types of shocks. At the same time, the success of security missions crucially depends on cohesion within communities as they are relevant partners in counterinsurgency and reconstruction activities. I address endogeneity by applying different estimation techniques, including a geographic regression discontinuity design. My findings suggest that households in districts where foreign military forces are present receive less help from others in their community, have less trust in community councils and participate less in those councils.

4.1. Introduction

A large literature argues that social interactions in communities are unlikely to change from one day to another. Nunn (2008), Guiso et al. (2016), and Dell et al. (2017), for instance, show that social cohesion is a slow process with deep historical roots. Contrary to this, numerous studies provide evidence that also short- and medium-term shocks can affect social interactions and cooperation (see among others, Bellows and Miguel, 2009; Fearon et al., 2009).

In this paper, I analyze the short-term effects of foreign military interventions on the cohesion within communities and the role of local institutions in times of conflict. In particular, I consider the role of the International Security Assistance Force (ISAF) in Afghanistan, which "was one of the largest coalitions in history and is NATO's most challenging mission to date."[1] ISAF takes a counterinsurgency (COIN) approach (e.g., Dorn, 2011) to achieve its mission of enhancing security and creating a safe environment for reconstruction and nation-building.[2] Afghanistan – apart from its humanitarian and political relevance – serves as a practical case from a statistical point of view. Despite being one of the most severely conflict-ridden countries in the world, data on social interactions at the household level as well as geocoded conflict data are available for almost the entire country. Given the high intensity of conflict for decades, households have already adopted mechanisms to deal with the never ending insecurity. In this setting, "ISAF deployment is a relative new phenomenon [...] and its strategies change continually," which makes it possible to "disentangle the response of old coping strategies to a new type of event" (Bove and Gavrilova, 2014, p. 113).

In an environment where state institutions are unstable or even lacking, community cohesion plays a central role. According to Jones and Muñoz (2010) we need to better understand the role of such institutions since power in rural areas of Afghanistan tends to be local. This is not only relevant from the perspective of households, but also when it comes to the success of counterinsurgency and security missions. It is well accepted that local communities are relevant partners in counterinsurgency and post-conflict reconstruction activities. Receiving information about insurgents is an important resource during wars and civilians are therefore approached and (ab)used to share sensitive wartime information (e.g., Berman and Matanock, 2015; Lyall et al., 2015; Wright et al., 2017).[3] This applies as well to insurgents, whose rebellion rests on the

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[1]"At its height, the force was more than 130,000 strong, with troops from 51 NATO and partner nations." Source: https://www.nato.int/cps/en/natohq/topics_69366.htm, accessed February 8, 2018.

[2]It differs from a pure peacekeeping mission. However, Friis (2010) argues peacekeeping and counterinsurgency "seem to be converging and share some commonalities," such that a clear distinction is not always possible.

[3]"Civilian information about the identity or location of rebels, or even about local terrain and customs, makes government attacks much more effective at controlling territory (by capturing, killing, or intimidating rebels). We call this an information-centric framework" (Berman and Matanock, 2015).

support of the communities. Local and international development actors usually also rely on communities for an effective implementation of the projects. For instance, the National Solidarity Program (NSP) implements development projects in cooperation with the communities and thereby tries to strengthen local self-governance.[4] Community level ties are thus a prerequisite for many policy measures to be effective. However, according to anecdotal evidence, ISAF – while being dependent on the communities – has detrimental effects on social cohesion. Cohn (2009, p. 3), for instance, argues that ISAF "helped to undermine and marginalize the important role played by village elders in Afghan culture."

Given that the conflict in Afghanistan is an inherently long-term phenomenon, it correlates with almost any possible outcome, as it is the case for the deployment of international forces and community cohesion. I propose different estimation techniques to get as close as possible to measuring causal effects. I combine georeferenced data on the presence of ISAF and geocoded conflict events with household level data from the National Risk and Vulnerability Assessment (NRVA) and the Survey of the Afghan People (Asia Foundation), which are both large and nationally-representative household surveys.

First, by using information at the household level, I apply high-dimensional fixed effects that capture a large part of the omitted variables that likely bias my results. Second, I exploit exogenous variation in the need to rely on community cohesion induced by climatic shocks and consider the interaction effect of this income shock with the presence of international military forces.[5] While this approach does not allow to analyze the direct effect of foreign security missions on community cohesion, it enables me to investigate heterogeneous effects of short-term income shocks depending on whether ISAF is present in a district or not. Under mild conditions, the interaction effect can be regarded as exogenous as I control for the levels of the interaction term (e.g., Bun and Harrison, 2018; Nizalova and Murtazashvili, 2016).[6] Third, I make use of the mandate enlargement of ISAF during the period from December 2003 until October 2006 in a geographical regression discontinuity (GRD) design. I exploit the fact that the boundary between the

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[4]NSP development aid is provided via community councils. If there is a lack of social cohesion, the assistance might not be distributed most efficiently. Participation in such programs is also less likely when there is less social cohesion.

[5]There is increasing literature on how income shocks affect conflict. See for instance Brückner and Ciccone (2010) and Bazzi and Blattman (2014) for studies at the macro level as well as Berman and Couttenier (2015), Berman et al. (2017), and Chapter 3 for studies at the micro level. These studies exploit variation in international commodity prices or weather conditions to instrument changes in income.

[6]A recent critique by Christian and Barrett (2017) is that non-linear trends in the time series of the interacted instruments can be problematic. For the second estimation technique, I only consider a cross-section and the exogenous shocks are household-specific, although driven by region-specific weather shocks. In my case, I would thus be rather concerned by omitted variables, that correlate with the outcome differentially according to the presence of ISAF. Given that I cannot rule out this concern, I propose the geographical regression discontinuity design as an alternative estimation technique. This allows me to test for differences according to ISAF presence in a variety of observable factors between districts close to the treatment boundary.

northern regional command – where ISAF has been deployed to first – and the rest of the country – where the mandate enlargement took place with a time lag – splits households into a control and treatment area in an "as-if-random" assignment.[7]

The paper contributes to different strands of the literature. First, it adds to the literature on conflict and social cohesion. According to Fearon et al. (2009, p. 288), the standard approach in measuring social cohesion "[i]nvolves surveying households to assess levels of trust, patterns of community activity, and the extent of associational life." In analogy, Chan et al. (2006, p. 290) summarize social cohesion as a "set of attitudes and norms that include trust, a sense of belonging and the willingness to participate and help." Following this literature, I rely on different indicators of community cohesion, including participation in local community councils, trust in such councils and whether households receive help from others in their community, which can be regarded as a proxy for prosocial behavior.

Bauer et al. (2016) summarize the current stage of this literature in a meta-analysis covering 23 articles. The studies vary with respect to their analytical approach and outcome variables. While some studies exploit (repeated) household questionnaires as I do (see, e.g., Bellows and Miguel, 2009; De Luca and Verpoorten, 2015), others apply incentivized lab-in-field experimental games (see, e.g., Fearon et al., 2009; Voors et al., 2012; Gilligan et al., 2014). Bauer et al. (2016) conclude from their meta-analysis that violence induces cooperation and prosocial behavior across different outcome measures.[8] None of the studies they consider is on Afghanistan. Besides adding another country-case, I augment this literature strand by highlighting the role of foreign security missions in how community cohesion is affected in times of conflict.

Second, the paper contributes to the literature on the effectiveness of security missions in achieving peace and providing an environment for reconstruction and nation-building. The deployment of external forces in war contexts in the form of security and peacekeeping missions is a common policy tool, but the analyses of the effects of these policy measures are limited to specific outcomes. Most obviously, many studies focus on violence or peace as the outcome of interest (e.g., Gilligan and Sergenti, 2008; Hultman et al., 2013; Ruggeri et al., 2013). Others analyze changes in household attitudes towards and collaboration with pro-government forces including international troops as compared to insurgents given that they are exposed to violence by either party (e.g., Lyall et al., 2013; Hirose et al., 2017; Schutte, 2017).

A long list of studies shows, for instance, that social cooperation is beneficial for development (see, e.g., Knack and Keefer, 1997), and could thus be a channel on how security missions affect their main objectives of reconstruction, development, and

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[7]I discuss the suitability of this approach in detail in Sections 4.4 and 4.5.

[8]The literature has also pointed to in-group and out-group effects. For instance, for the Rwandan context, Pinchotti and Verwimp (2007) find that while within group ties are strengthened, they are weakened between (ethnic) groups.

peace. Collier and Hoeffler (2004b) more directly find that decreases in social cohesion can contribute to conflict. In line with that, Gilligan et al. (2014) find that social cohesion implies a strong potential for recovery. Despite acknowledging that the success of counterinsurgency activities, post-conflict reconstruction, and nation-building efforts depends on community cohesion, evidence on the effects of such missions on social cohesion is scarce. Iyengar et al. (2017, p. 7) conclude from a systematic review on Afghanistan that "evidence on community cohesion in the existing literature was too limited to draw a conclusion and in many studies was not even considered." Closely related to my analysis are the studies by Dell and Querubin (2018) and Weidmann and Zürcher (2013). Dell and Querubin (2018) exploit a discontinuity of two different military strategies applied in the Vietnam War, one relying on overwhelming firepower and the other more on a "hearts-and-minds-oriented" approach. The authors identify worse effects on security and local government administration of the first strategy relative to the latter. Weidmann and Zürcher (2013) show how conflict affects social cohesion and attitudes about the warring parties in Northern Afghanistan. Their study relates to the effects of foreign military interventions to the extent that the authors differentiate between who is fighting, including ISAF forces. The authors find that attitudes change, but that these effects do not extend to changing the trust or cooperation within communities. While the results provide important insights, the authors do not derive any causal estimates. Additionally, their sample covers only four out of 398 districts over the 2007-2009 period.

Third, I add to the literature on the role of aid in "winning hearts and minds" of the local population (e.g., Berman et al., 2011). Recent studies on Afghanistan elaborate on the effects of military-led aid projects (Sexton, 2016; Child, 2018), aid provided through the National Solidarity Program (NSP) (Beath et al., 2016), or development aid more broadly (Böhnke and Zürcher, 2013). Studies on Afghanistan, but also other countries (e.g., Crost et al., 2014), provide mixed results. Aid can indeed be effective in building pro-government support from communities, but can also lead to more violence (see also Chapter 1 for evidence at the macro level). My study relates to this literature strand as these two strategies, "winning hearts and minds" through aid and approaches based on the deployment of military forces (with different degrees in applying force) have to be considered in tandem. The mission of such military interventions is to secure an environment such that reconstruction efforts can be made. In my analysis, I will account for the presence of aid and reconstruction programs. I also investigate heterogeneous effects of aid effectiveness and its acceptance by communities depending on whether ISAF is present or not.

This analysis differs from the mentioned studies to the extent that their focus is often on peacekeeping missions after war or on violence committed by foreign forces. I consider the presence (conditional on violence), which does not necessarily coincide with violence. Besides this, studies at the community or household level focus on attitudes towards the

government versus insurgents and information sharing with either warring party. In this study, I elaborate on how within-community ties are affected, ties which form the "glue that holds society together" (Janmaat, 2011). Finally, many related studies consider a small fraction of the country, which is due to survey data being available only for this particular subset (as it is often the case in randomized experiments). However, this constrains external validity even within the country. This study, on the contrary, provides evidence for more than 90% of the country's districts over the 2005-2014 period. The results of the different estimation approaches all point to the same finding. In line with anecdotal evidence, I find that ISAF presence has a negative effect on different measures of community cohesion.

I proceed as follows. In Section 4.2 I discuss the theoretical mechanisms at the local level. Section 4.3 introduces the data and section Section 4.4 the three different identification strategies. Results of each strategy are presented in Section 4.5 along with robustness checks. Potential mechanisms at place are discussed in Section 4.6. Finally, Section 4.7 summarizes results and highlights policy implications.

4.2. Mechanisms at the local level

In many conflict-ridden countries, the community- or village-level plays a central role as state institutions are often unstable or even lacking (e.g., Arjona, 2014). Social and political institutions are therefore often provided by local leaders. In Afghanistan, local entities are traditionally governed by local non-state actors, i.e., by the elders of the village and the so-called shura or jirga, which is a community (village) council (e.g., Asia Foundation, 2007; Jones and Muñoz, 2010).[9] Shura refers to "meetings by lead representatives of factions, clans, families, militias, or other units relevant to resolution of a problem or class of problems" and they are "generally convened for the purpose of discussion and collective decision making" (Asia Foundation, 2007, p. 23).[10] For instance, dispute solving most commonly takes places at this level. Whereas the state is not regarded as legitimate in many regions in Afghanistan, the shura and village elders qualify as legitimate protectors. While the "custom and informal customary dispute resolution in civil matters is explicitly recognized by Afghanistan's Civil Code (1976) and Civil Procedure Law (1977)," agreements which are solely concluded through these

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[9]In the following I will use the term shura when I refer to these traditional community councils. While shura is the Arabic word, jirga is the Pashto word. "Historically, a jirga is a temporary council established to address specific issues, while a shura is a more permanent consultative council. In practice, however, the two terms are often used interchangeably" (Jones and Muñoz, 2010, p. 21). "Unless the village is big Jirga/Shura is usually made up of representatives from more than one village (village cluster, district, valley, or tribal segment)" (Asia Foundation, 2007, p. 23).

[10]This also involves development activities, in which the shura became more involved after the fall of the Taliban. Many donors of aid projects are consulting and working with the traditional shuras (Asia Foundation, 2007).

informal councils are not legally binding (Wardak, 2016, p. 15).

The literature has provided numerous hypotheses about how communities react when they are exposed to shocks or external threats. In the NRVA survey, about 60% of households suffer from any type of shock including income shocks caused by climatic changes or price changes but also shocks induced by insecurity and violence. Households that are exposed to negative shocks are more likely to ask for help from others at the local level when state institutions are not present. I therefore expect the exposure to common threats to be linked to an increase in community cohesion (in line with Bauer et al., 2016). In Afghanistan, communities have set up so-called *Arbakai* or *Chalweshtai*, which are community police forces that implement the decisions of the local shura to deal with threats.[11] In line with this anecdotal evidence is Jennings and Sanchez-Pages' (2017) theoretical model, which shows that an "external threat stimulates social capital as there now exists a protective reason to invest in it" (p. 158). The authors additionally argue that the relation depends on the intensity of the threat and differs between rich and poor societies.

While this study relates to previous work on how threats like conflict affect social capital, it focuses in particular on how foreign military interventions affect community cooperation and how the general link between community cooperation and negative shocks depends on the presence of the foreign military.

When ISAF enters the territory, we can expect different mechanisms to occur, depending on whether they enter territory formerly in the hands of the government versus the insurgents, and thus uncontested versus contested territory. As ISAF assists the Afghan government in counterinsurgency activities, their deployment could indicate higher levels of pro-government presence and control (as argued by Sexton, 2016), which could go along with higher levels of perceived security. However, in case military installations are strategically located to insecure areas or in case these bases become the target of Taliban attacks and thus attract conflict, their presence might be associated with an increased risk of contestation. According to the data provided by UCDP GED within the period of observation, fighting takes place almost exclusively between Taliban on the one side and pro-government forces including ISAF on the other side.[12] This is why I frame conflict as contestation since two opposing conflict actors fight for control over a territory.

While violence usually does not directly involve the communities, households are still affected by the surrounding insecurity and are exposed to power shifts at the community- and district-level (e.g., Weidmann and Zürcher, 2013). Fights between both warring parties could be regarded as a common threat. In contested areas, households face a

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[11]There are a number of alternative terms or definitions of such neighborhood watch schemes, local protectors or local defenders (Jones and Muñoz, 2010).

[12]The share of this type of violence by all reported battle-related deaths makes up 95%.

higher uncertainty about who controls the area in the future. They can neither rely on the government nor the rebel leaders for longer-term support in times of economic hardship. Just like for common negative income shocks one could expect that social cohesion is increasing. However, while the threat can be regarded as common as it introduces higher levels of insecurity, it must not be common in the sense of which conflict actor is supported by the community members. For this reason, one could expect the opposite effect of contestation if cleavages begin to emerge. Households are no longer confronted by one common threat, but rather, face a new actor and are thus exposed to two rival groups that fight for control. This mechanism is likely to be amplified by the fact that both warring parties seek for wartime information within the communities (e.g., Berman and Matanock, 2015; Lyall et al., 2015; Wright et al., 2017). Households might not know anymore whom they can trust as they don't know with whom their neighbors are cooperating. This is likely to affect the legitimacy of village leaders and the shura. As Weidmann and Zürcher (2013) point out, exposure to conflict between pro-government forces and insurgents can thus lead to an erosion of the community's social glue.[13] There is another mechanism at place in contested districts which might also result in less community cohesion. To get control, both conflict actors "need the support of the population to win" (Jones and Muñoz, 2010, p. 5). They might increase the provision of institutions like public goods (Arjona, 2014) and protection (Tilly, 1985). The increased support by both groups could affect the relevance of the local institutions and could weaken the informal ties between community members.

If the foreign military enters uncontested districts which are under government control and remain uncontested, one could expect an increased provision of formal institutions and infrastructure via undisturbed reconstruction efforts. This might render informal institutions at the community level less important as they are crowded out by more formal (state) institutions. The empirical (e.g., Acemoglu et al., 2014; Guiso et al., 2016; Dell et al., 2017; Lowes et al., 2017) and theoretical (e.g., Bowles and Gintis, 2002; Acemoglu and Robinson, 2017) literature, however, provides mixed results on whether strong state capacity is a complement or substitute of governance and cooperation at the community level. What is more, according to anecdotal evidence, ISAF has been criticized not to coordinate with the locals and to rather bypass the local shuras in decision-making processes, which increases the confusion as to who has control.[14] This might result in less cohesion when cohesion is measured by participation and trust in those traditional councils. However, it might also raise skepticism towards ISAF.[15] Child

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[13]Schutte et al. (2018) introduce fear as a mediator of how conflict affects cohesion. Assuming that ISAF introduces fear, this concept can be related to the context of how security missions affect cohesion.

[14]ISAF's interpreters are usually not representatives of the population. Rather they are commonly from educated and wealthy households.

[15]Another criticism, which led to more skepticism, is that favoritism occurred and the way ISAF spends money has not been transparent, which caused – perceived – rising inequalities.

(2017, p. 8) highlights another reason for an increased skepticism based on the perception of reconstruction activities as they "can be unwelcome by some community members on ideological grounds." This negative perception likely spills over to foreign personnel in general, whether being development workers or part of the military. Böhnke and Zürcher (2013) indeed find that – if at all – development projects lead to a more negative perception of foreigners. Child (2017) further distinguishes between projects that are more political like education as compared to health projects, with the latter inducing less resistance. Since military forces are clearly linked to a political mission, one can assume more resistance.[16] Thus, even if ISAF leads to an increased provision of formal support mechanisms and new institutions, households might not rely on these because of negative perceptions. The traditional local institutions would then not be crowded out by new formal institutions.

Given these different mechanisms that can occur, the net effect remains to be empirically tested. While anecdotal evidence points to the fact that the foreign military intervention leads to the erosion of local institutions, there is a lack of quantitative evidence. I, therefore, exploit various estimation techniques to get close to measuring a causal effect on how ISAF affects community cohesion. Before I present the details of the identification strategy in Section 4.4, I introduce the data in the following section. In Section 4.6 I expand on potential explanations and mechanisms.

4.3. Data

All variables listed in the following are described in more detail in Appendix 4.C with descriptive statistics being presented in Appendix 4.D.

Household level: I derive most of the data from the National Risk and Vulnerability Assessment (NRVA), a survey of Afghan households conducted in 2005, 2007/08, 2011/12.[17] While the three waves are comparable in many questions, they differ in some important ones. I describe the harmonization procedure along with the definitions of the variables in Appendix 4.C.

The surveys were conducted by the Ministry of Rural Rehabilitation and Development (MRRD) and the Central Statistics Office (CSO) with the support from the European

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[16]Child (2017) backs this concern with insights from field interviews which point to projects causing more resistance when they are tied to the military.

[17]The survey has been continued and since 2013 renamed to Afghanistan Living Conditions Survey (ALCS). I requested the recent datasets but after one year still haven't received them. Note also that the first wave was conducted in 2003 already. For reasons of comparability of the survey structure and design, the 2003 wave is only used for balancing tests in the RDD approach and not included in the panel regressions. Only starting with the second wave in 2005, the survey is designed to be nationally representative. The 2003 wave includes a much smaller number of households and a much smaller set of variables. It still provides important information that I use to validate the GRD.

Union. The three waves cover between 21'000 and 31'000 households in 341 between 388 districts (of a total of 398 districts in 34 provinces).[18]

The NRVA includes data on shocks that the household experienced within the last 12 months as for instance insecurity, opium eradication, price and various climatic shocks. I construct a binary measure taking on the value of one if the household experienced any shock, which might be endogenous, and a second measure on exogenous climatic shocks.[19] In the same section, the survey provides very specific information on coping strategies that households apply as a response to the different shocks. The list of potential coping strategies covers 26 measures with some being suitable to proxy community cohesion.[20]

I construct different indicators for community cohesion. *Community Help* is an indicator taking the value of one if the household received help from others in the community. Similar to this variable, I build a wider measure including both *Community Help* and whether the household received a loan from friends or family *Community Help+Loans.* These proxy variables are not ideal for capturing the entire spectrum of social cohesion, though they capture an important part, which is prosocial behavior. Bauer et al. (2016), for instance, classify the typical outcome variables into different categories with one being prosocial behavior. Note that I cannot differentiate between different motivations of a community member to provide help. Whether the decision to help is motivated by altruism or reciprocity is not possible to disentangle from this analysis.[21] Besides this dimension, many studies consider social group participation or community participation as relevant outcome variables. I, therefore, construct the alternative measure *Council Member* that is based on community behavior and in particular on the participation in community councils.[22]

The survey also provides information on household composition, assets, education,

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[18]"The number of rural communities or villages in Afghanistan is a matter of interpretation. The Central Statistics Office counts 40,020 rural villages, while the NSP counts 24,000 communities" (Nixon, 2008).

[19]The latter takes a value of one if the household has been hit by one of the following climatic shocks: Earthquakes, landslides/avalanches, flooding, late damaging frosts, heavy rains preventing work, severe winter conditions, and hailstorms.

[20]When using these variables I control for the household having experienced a shock to account for the survey design. Without doing this my results could be driven by differences in the exposure to shocks and not by the coping behavior.

[21]These variables represent the supply side. However, by controlling for different shocks that household experienced I account for the demand of community support. In particular I also account for covariate shocks induced by climate shocks, which usually demand for the community rather than single households to cope with it. This is due to the fact that most households are working in agriculture and are dependent on the surrounding households because of, for instance, irrigation systems.

[22]The question in the survey is: "Is anyone in your family a member of the following decision making bodies in your community?" Following Iyengar et al. (2017) I build a measure on whether the community shura is asked for dispute solving mechanisms. The question in the survey is: "How was the dispute over land or housing solved?" with the possible responses: "With help of court" or "With help of neighborhood representatives or village authority." However, the latter measure turns out to be missing for more than 97% of the sample. The survey does not allow to derive measures that capture the output side of these councils. Though, while *Community Help* and *Community Help+Loan* represent the output or supply side of community cohesion in general, *Council Member* rather represents the input side.

sources of income, food consumption, and expenditures. Furthermore and very relevant for the analysis are the survey questions on aid. One important development program that has been introduced in some areas of the country in 2003 and thus around the same time as the ISAF mandate had been enlarged to the north was the National Solidarity Program (NSP) created by the Afghan MRRD and funded by the World Bank as well as bilateral donor countries.[23] The NSP created so-called Community Development Council (CDC) at the community level to implement infrastructure or agriculture projects in collaboration with the community and to strengthen community level governance. The NSP works together with different international groups and NGOs that support CDCs in implementing these development projects.[24] According to Beath et al. (2016, p. 8) the program served as an "implicit state-building function in establishing the government as a benevolent provider of public goods and services." The extent to which CDCs complement (or substitute) traditional shuras differs across districts and time and the success depends on how they can cooperate with the traditional institutions. While in 2005 in Nangahar one member states that "CDCs are different from other shuras or jirgas in that they plan and organize development projects" (Nixon, 2008), it has also been stated that they are involved in dispute resolution. However, in communities where a traditional shura exists, the CDCs engaged in dispute-solving mostly in collaboration with them (Nixon, 2008). Still, problems of opposition from the traditional shura or powerful individuals have been reported (Asia Foundation, 2007). Unfortunately, I have information on the villages that participated in the program only for the wave in 2005, where households have been asked whether there exists a CDC in their community and whether they participated in it.

As a second source for household level data, I rely on the Survey of the Afghan People conducted by the Asia Foundation, which is another nationally-representative household survey. Given that this dataset is only available from 2007 onward, I can only use it for the panel analysis and not for the regression discontinuity design (RDD), as the discontinuity is based on a policy change, which leaves no treatment variation for the years covered by the survey. Yet, the survey is useful as it includes information on trust and confidence in the community councils (shura), from which I construct two further proxies of social cohesion, i.e., *Trust in Council* and *Confidence in Council*. It allows me to validate my results based on the NRVA survey.

Regional level: The main variable of interest, *ISAF* presence, is proxied by three different indicators. First, I exploit the stage-wise enlargement of the mandate as illustrated in Figure 4.3 and create an indicator variable for districts that fall within the north of the country (stage 1). Second, I construct a binary variable indicating

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[23]By 2008, the program covered two-thirds of the communities in the country (Nixon, 2008).

[24]See, e.g., http://www.afghanwarnews.info/development/NSP.htm for more details, accessed June 22, 2018.

whether a Provincial Reconstruction Team (PRT) is located in district i or in its neighboring districts. PRTs are "small teams of military and civilian personnel working in Afghanistan's provinces to provide security for aid works and help humanitarian assistance or reconstruction tasks in areas with ongoing conflict or high levels of insecurity."[25] Both measures come at the cost that they do not vary after 2006, with one exception of the creation of a PRT in 2010. I account for this by focusing on the cross-section for the year 2005 in two of the three estimation strategies. In the panel fixed effects regression, the variation comes only from switches between the first and second survey wave (2005 and 2007/08) within those districts, where ISAF has been deployed to.[26] While being under NATO's (ISAF's) authority, the aim of the 26 joint civil-military units goes beyond the military domain. They provide support for local partners and ministries in governance issues and, according to NATO (2008), take part in meetings of community councils (shuras). Since PRTs include a military component and are often even placed within military bases of the respective ISAF lead nation, I use them as another proxy for the presence of foreign military personnel.[27] One of the most common criticisms is in fact that their civilian personnel appears in the same uniform as the military personnel and thus it is impossible to distinguish between the different purposes.[28] Third, I follow Sexton (2016) and Hirose et al. (2017) and use the presence of a military base in a district as an alternative measure, which varies over time.[29] In analogy to the presence of a PRT, I construct a binary variable indicating whether there is at least one military base in district i or in any of its neighboring districts.[30] More information on ISAF's involvement in Afghanistan is provided in Appendix 4.B.

Since the level of contestation is one of the most obvious confounding factors in the analysis of how the presence of security interventions affects community cohesion, I proxy for contested territory by using data on conflict. Besides, I am interested in heterogeneous effects given the level of contestation. I measure contestation based on three different data sources. UCDP GED provides geocoded data on battle-related deaths derived from media reports (Sundberg and Melander, 2013). For Afghanistan and the period of observation, these events either cover two-sided violence between Taliban and government forces or one-sided violence committed by the Taliban against civilians. However, about 95% of

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[25]Source: https://www.nato.int/docu/review/2007/issue3/english/art2.html, accessed April 1, 2018. Depending on the lead nation, PRTs differ in size, structure and guidance.

[26]For robustness, I run the same regressions but replace district- with province-fixed effects and allow for a comparison within provinces between districts that are characterized by a PRT and those that are not.

[27]Eronen (2008) states that on average civilians represent only 5% of the personnel in PRTs.

[28]"NGOs have been hesitant to work with the PRTs and have called for their roles to be clarified." (Asia Foundation, 2007, p. 30)

[29]Note that this measure in not complete, since exact geographic locations of most bases are kept secret. I thus focus on the data of large military bases as described in Chapter 3.

[30]For robustness, I restrict the indicator variable on the presence of the district only, respectively for the presence of a military base and a PRT.

the events within the 2005-2014 period are classified as fighting between pro-government forces and the Taliban. I, therefore, refer to *Contestation* as it is likely that in districts where the two groups are fighting, they fight for control. Given the concerns raised with media-based conflict data (as discussed in Weidmann, 2015, 2016, and summarized in Chapter 3), I also rely on conflict events recorded by international forces, secured by Shaver and Wright (2016). This dataset covers significant activities (SIGACTS), classified into three types of events, direct and indirect fire attack (DF and IDF), and improvised explosive device (IED).[31] While direct fire attacks are close combat events characterized by the use of weapons as small arms or rocket-propelled grenades, indirect fire attacks can be launched from great distances and because of that are also likely to be less precise. The latter includes mortars and rockets and can be heard within a quite large surrounding, thus creating broader attention. Whereas the first two types involve fighters, improvised explosive devices are associated with less risk for the perpetrators. They are often placed around roads and directed against moving targets, for instance pro-government convoys.[32]

Besides these objective conflict measures, I use information at the household level on insecurity shocks and also aggregate this to shares of households affected at the district level.[33]

Further variables: I control for *Nightlight*, the Vegetation Health Index (*VHI*) and *Aid* at the district level. Nightlight (in logarithms) is used to proxy for district level GDP, which has become the standard approach if district level GDP is not available (Henderson et al., 2012). This also proxys for population density (Henderson et al., 2018), and the access to markets and infrastructure which is a relevant factor for the neediness of community support. I use the Vegetation Health Index (*VHI*) provided by the FAO as an objective indicator of climatic shocks as it measures droughts. Besides this, to proxy for the presence of foreign civilian personnel apart from the National Solidarity Program that introduced the CDCs, I include geocoded aid (*Aid*) provided by AidData at the yearly level.[34] More precisely, I include aid commitments provided by the World Bank

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[31]Note that the SIGACTS version I use does not allow to distinguish between the conflict sides as it covers the total events per district-year for each of the three types. To get information on Western casualties from hostile encounters involving Western ISAF forces or US forces in Operation Enduring Freedom I also refer to data from iCasualties.org (2016). Anecdotal evidence suggests that Taliban often hits Western soldiers on their daily ways to and from the military bases. One could therefore also use it to proxy the presence of Western forces. The correlation between the two variables is, however, only 0.2. This could also be driven by the availability of the casualty numbers at the province level only. iCasualties.org provides some information on more precise locations, though this covers only a small subset of events, which I regard as too incomplete to exploit this variation at the district level.

[32]These definitions follow Eynde et al. (2017) and Sonin et al. (2017).

[33]Averages of the objective and subjective conflict measures are presented in Appendix 4.F. This comparison also serves as a verification of the conflict data that I apply. As can be seen, both objective and subjective conflict indicators are quite highly correlated.

[34]For cross-sectional analyses of the 2005 wave I include whether there is a CDC in the community

(including IBRD and IDA) and aid provided by all donors as reported by the Afghanistan Recipient System.

4.4. Identification strategy

Estimation strategy 1: Panel regression

I consider the household level as the main level of analysis for the three different estimation techniques that I present in the following.[35] Since households are not being tracked over the three survey waves I apply a quasi-panel structure at the household level following Ciarli et al. (2015).[36] I pool the independent cross sections and account for time- and district-fixed effects. The basic empirical panel data model is the following:

$$CC_{i,d,t} = \beta ISAF_{d,t} + \theta c_{d,t-1} + \boldsymbol{X'}_{d,t-1}\boldsymbol{\gamma} + \boldsymbol{H'}_{i,d,t}\boldsymbol{\mu} + \tau_t + \delta_d + \epsilon_{i,d,t}. \qquad (4.1)$$

$CC_{i,d,t}$ represents one of the measures for community cohesion of household i in district d in year t from the NRVA survey or the Survey of the Afghan people.[37] $ISAF_{d,t}$ is an indicator of whether ISAF is present in the district, measured by the mandate enlargement, the presence of PRTs or military bases. The variable $c_{d,t-1}$ captures the degree of contestation in the previous year. Whether the region is contested is measured by the number of battle-related deaths (in logarithms) from UCDP GED. For robustness, I replace this measure with the three different attack types, DF, IDF, and IED from SIGACTS (Shaver and Wright, 2016). $X_{d,t-1}$ is a vector of predetermined district level control variables including aid, VHI, and nightlight. $H_{i,d,t}$ is a vector of household level covariates. Due to the structure of the survey, I cannot apply predetermined household specific characteristics. I, therefore, follow Chauvet and Ehrhart (2015) and aggregate each household control over all households at the district level and exclude household i.[38] These variables include household living standards measured by household food consumption, whether households earn income from agricultural work, receive remittances, and whether they have taken a loan. The latter is of particular importance as to proxy for the need to rely on community support. Following Dell et al.

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as provided by the household survey.

[35]I apply Linear Probability Models (LPM) in all regressions.

[36]In all regressions I include household survey weights to ensure results to be representative for the Afghan population.

[37]In the latter case I do not control for $H_{i,d,t}$ as the survey does not provide comparable questions to the NRVA. Note that for robustness, I exclude this set of controls also for regressions using the NRVA data.

[38]Since this captures variation over time, it is conceptually different from a district-fixed effect. In a cross-section, it does, however, get very close to a district-fixed effect. Notwithstanding, I prefer this technique to including these variables at the household level as they could be potentially bad controls. Results are robust to not including covariates.

(2017) I also account for household characteristics as age and sex of household head, number of all household members, and number of children living in the household. As some of these variables could be transmission channels and therefore bad controls, their inclusion can cause a bias of the estimates of interest. I therefore consider results without household-specific control variables and rather rely only on pre-determined district level control variables in $t-1$ and fixed effects.[39]

τ_t and δ_d are time- and district-fixed effects. They are important to the extent that I must control for the need to rely on the community. Due to the lack of data on institutions, district- and year-fixed effects allow to control for some part of that variation. District-fixed effects account for instance for the distance to major cities, which is used as a proxy for the presence or legitimacy of central government institutions (e.g., Lind et al., 2014). Using this strategy allows me to rule out some part of the omitted variable bias. However, I cannot claim causality.

Estimation strategy 2: Interaction with exogenous shocks

In a second step, I consider the interaction between $ISAF_d$ (and c_d) with an exogenous shock as shown in equation 4.2:

$$CC_{i,d} = \beta S_{i,d} * ISAF_d + \alpha S_{i,d} * c_d + \eta S_{i,d} + \omega ISAF_d + \boldsymbol{X'}_d \boldsymbol{\gamma} + \boldsymbol{H'}_{i,d} \boldsymbol{\mu} + \epsilon_{i,d}. \quad (4.2)$$

$S_{i,d}$ measures the exogenous income shock that varies at the household level, which is a combination of different climatic shocks.[40] Roughly 70% of the Afghan population receive at least some part of their income from agricultural activities. Climatic shocks thus represent a major threat to household income, especially in rural areas.[41] Given that these exogenous income shocks increase the need to rely on support from either formal or informal institutions, I exploit this variation to consider heterogeneous effects depending on the presence of ISAF and the degree of contestation. $ISAF_d$ and c_d are defined as in equation 4.1. As discussed before $ISAF_d$ can lead to heterogeneous effects that can be further amplified by the level of conflict in a district. I therefore also consider the triple interaction $S_{i,d} * c_d * ISAF_d$.[42] I restrict my analysis to the year 2005, as this survey wave includes most information on community cohesion and I can exploit the boundary

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[39]Despite being a potentially bad control I include whether households in a district have taken a loan to proxy for the need to rely on help from others. I do, however, also run regressions without any covariates to test robustness.

[40]It is a binary indicator variable taking on the value of one if the household has been hit by one of the following climatic shocks: Earthquakes, landslides/avalanches, flooding, late damaging frosts, heavy rains preventing work, severe winter conditions, and hailstorms. I define this variable in more detail in Appendix 4.C.

[41]In the three waves of the NRVA this share varies between 60 and 80% for rural households and between 50 and 65% for all households.

[42]When including the triple interaction I take account of all the levels and the interaction of the different pairs of the levels, respectively.

between the northern command and the rest of the country as I will do in the regression discontinuity.[43]

I control for the same set of variables as in equation 4.1 and again cluster at the district level. Given that I am interested in the interaction term with the exogenous income shock, these controls should not alter the results of the coefficients of the interaction. This estimation technique comes at the cost that only the interaction term can be considered exogenous and one cannot deduce the effect of ISAF presence independently from the shock. This leads over to the third estimation technique, which I will present in the following.

Estimation strategy 3: Geographic regression discontinuity

The third technique follows the approach by Card and Krueger (2000), Dell (2010) and Dell et al. (2017). I exploit a geographic boundary as a regression discontinuity. The main assumption is that a geographic or administrative boundary assigns households to a treated and control area "in an as-if random fashion" (Keele et al., 2015, p. 127).

I exploit the sequential enlargement of ISAF's mandate as envisaged by the Bonn Agreement, first to the north of the country (including 9 out of 34 provinces) and later to the remaining country. After NATO took command of ISAF in August 2003, UNSC Resolution 1510 on October 13 in 2003 announced the enlargement of ISAF's mandate to the north to support the government beyond the capital Kabul. As shown in Figure 4.3 and discussed in more detail in Appendix 4.B, the process of taking command over the entire country was split into four stages, with stage 2 to 4 being implemented after the NRVA household survey in 2005 had been conducted. While the decision of starting in the north has likely not happened at random, the provincial borders that form the treatment boundary can be regarded "as-if random" to the extent that they have not been systematically placed according to the level of conflict and social cohesion. Besides this, they are also not overlapping with the homelands of different ethnic groups, which would be a concern since ethnicity is an important determinant of community cooperation (see, Dell et al., 2017). According to Giustozzi (2008, p. 21), "[p]rovincial boundaries were drawn in such a way as to divide communities and create multi-ethnic and multi-tribal administrative units." Additionally, the timing of the subsequent stages can be regarded as random, since "[t]here is unlikely to be further expansion of ISAF until more assets are available in country for it, namely, close air support, fixed-wing and rotary-wing lift capability, special forces capability and logistical support" as stated in the report of the secretary-general of the UN in December 2003 (UNSC, 2003).

Yet, there are differences across the northern districts from the rest of the country.

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[43]With more than 200,000 individuals the NRVA 2005 was the largest household survey that has ever been carried out in Afghanistan (MRRD and CSO, 2007).

The biggest concern would be differences relating to security or territorial control. These factors likely correlate both with the outcome and the placement of the troops. To validate the RD design, three main assumptions have to be fulfilled.

First, the main identifying assumption is that all relevant factors besides ISAF treatment vary smoothly at the treatment boundary, which creates the discontinuity in the treatment of interest. While it is likely that many factors are not balanced across all northern districts as compared to all remaining districts of the country, I can show that households close to the border (within a bandwidth of 50 km) can indeed be regarded as comparable (according to a large set of observable factors). I will discuss this in more detail when I present balancing tests in Section 4.5.

Second, one has to rely on the assumption that the province borders are relevant to the treatment of interest. According to Eynde et al. (2017), administrative borders in Afghanistan are relevant for the security provision and insurgency. ISAF is split into broad regional commands (North, South, East, West), which are again split into commands of the different NATO and partner nations. Forces of one nation did not cross regional commands of others – with few exceptions as for instance in case of consultations of the lead personnel – because of their own risk and for not getting into the responsibilities of other lead nations.[44] At the same time administrative boundaries, while being relevant for the treatment, might come along with other compound treatments (as discussed in Keele and Titiunik, 2015). Given that my baseline geographic regression discontinuity (GRD) results rely on households from 66 districts from 14 provinces it seems rather unlikely that in all these political units reforms took place at the same time, which furthermore coincide with the timing of the mandate expansion. For robustness, I exclude 100 km-segments (covering treated and non-treated) of the boundary at the time so that results can not be driven by a single area where a potential compound treatment could actually explain the discontinuity. To the extent that potential but *irrelevant* (in that regard that they are not of interest to this analysis) treatments occur in both periods, before and after the *relevant* treatment, balancing tests for the pretreatment period allow to infer whether these *irrelevant* treatments cause a potential bias. As stated before, ISAF gets involved in the reconstruction, for instance, through PRT or NSP projects. While I can control for aid and show that general aid is not distributed differently across the treatment boundary, my treatment effect can still result from a combined treatment of the presence of military personnel and related aid. In Section 4.6, I have a closer look on how aid and military presence relate to each other. As I cannot prove the *Compound Treatment Irrelevance* assumption, I have to rely as well on inferences from the two alternative identification strategies presented in the previous two subsections.

Third, one has to rule out selective sorting. Taliban insurgents could for instance move across the border as a response to the deployment of ISAF forces to the north. If

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[44]According to Eynde et al. (2017, p. 16) "ISAF forces were also constrained by district boundaries."

this was the case, one would assume that along with the insurgent relocation, violent attacks would be relocated. If this affected community cohesion, we would misinterpret the treatment effect to the extent that changes in community cohesion would stem from shifts in conflict rather than because of the presence of foreign military forces. To rule out that the results are driven by relocation of insurgency, I replace the outcomes of community cohesion with different measures of conflict relying on both measures from UCDP GED and SIGACTS for the year 2005. I will test and discuss the validity of these assumptions in detail in Section 4.5.

I am restricted to the cross-section of the NRVA survey wave in 2005 as the next wave of the household survey (2007/08) took place right after the mandate had been expanded to the entire country. There would be no differential treatment assignment left. The estimation equation for the regression discontinuity is the following:

$$CC_{i,v,d} = \alpha + \beta treat_d + f(geo\ location_v) + \boldsymbol{X'}_d\boldsymbol{\gamma} + \boldsymbol{H'}_{i,v,d}\boldsymbol{\mu} + \sum_{s=1}^{n} seg_v^s + \epsilon_{i,v,d}. \quad (4.3)$$

$CC_{i,v,d}$ measures community cohesion of household i living in village v of district d. $treat_d$ takes a value of one if the district is in one of the northern provinces, i.e., where ISAF has been present at the latest since the end of 2004. $f(geo\ location_v)$ is the RD polynomial, which takes on different functions of the geographic location of household i in village v. For the 2005 survey, I was able to get information on longitude and latitude at the village level.[45] I assign all households in the same village to the same linear distance.[46] Following Gelman and Imbens (2018), I use local linear (and quadratic) RD polynomials rather than polynomials of higher order and limit the analysis to households located within different bandwidths of the boundary (50 km, 75 km, 100 km).[47] While the boundary forms a multi-dimensional discontinuity in longitude and latitude, I also apply a one-dimensional forcing variable, which is defined as the linear distance between the border and the household's village. In Appendix 4.E, I restrict the analysis to households in districts that are direct neighbors with respect to the border rather than taking all households of villages that fall within the different bandwidths.[48] Following Dell (2010) and Dell et al. (2017), I include border segment fixed effects seg_v^s. They split the entire

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[45]I don't have geocoded data on the location of the villages for the subsequent surveys.

[46]Having information on the more precise locations of households at the village level rather than the district level allows for a much higher number of mass points (as discussed in Cattaneo et al., 2017).

[47]Since I account for household survey weights, I do not account for triangular or epanechnikov kernel weights. Effectively, I apply a uniform kernel. This choice seems reasonable given that I use a local linear estimation within specific bandwidths. According to Cattaneo et al. (2017, p. 50) "[e]mploying a local linear estimation with bandwidth h and the uniform kernel is therefore equivalent to estimating a simple linear regression without weights using only observations whose distance from the cutoff is at most h." As compared to a global RD, I do not include households far away from the boundary and thus need no differential weighting of the observations according to their distance, since all are relatively close. For robustness I disregard household survey weights (Appendix 4.E, Table 4.24).

[48]Note that for robustness I also include the interaction of the RD polynomial with the treatment, which allows for different slopes at both sides of the boundary (see Appendix 4.E).

border into equally sized segments and take on a value of one if the village is closest to segment *s* and zero otherwise. I apply segments of 100 and 200 km. This allows comparing households in treated and control groups within the same segment of the border. Figure 4.1 shows where the boundary is located along with the 200 km segments and the three different bandwidths. The figure also highlights districts that are not included in the 2005 survey wave, as shaded by the grey dotted areas.[49]

FIGURE 4.1

Boundary, segments, and bandwidths
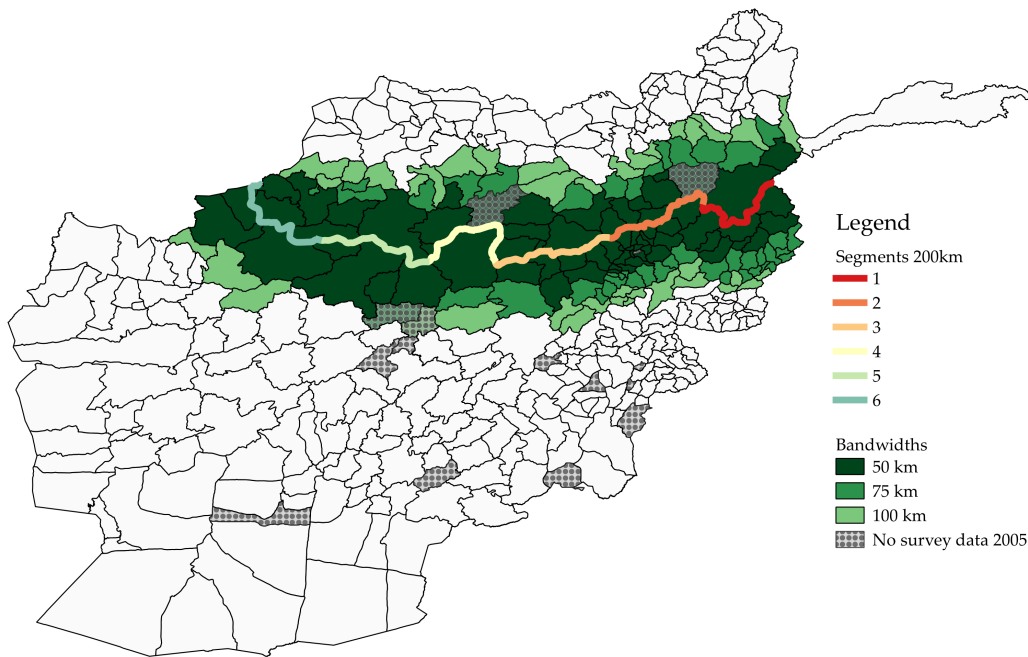
Notes: The boundary splits the country into the northern command (treated), where ISAF's mandate has been extended to in December 2003 (completed end of 2004) and the rest of the country (control), where ISAF has been deployed to after the 2005 survey has been conducted. Highlighted are the six boundary segments à 200 km, the three groups of bandwidths and the districts for which there is no survey data available in the 2005 survey wave.

Since households in the Kabul province fall within the larger bandwidths and ISAF has been present there since 2001, I present results for a restricted sample where I exclude Kabul. For robustness, I also exclude households in a few more areas where Western forces have been present prior to the official mandate enlargement according to anecdotal evidence (Eronen, 2008).[50] This could flaw my results as I want to identify the effect of the presence of foreign military forces.[51] X_d and $H_{i,d}$ are again vectors of pre-determined

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[49]In Appendix 4.F I show that these missing districts are not particularly prone to conflict.

[50]I describe this in more detail in Appendix 4.B.

[51]I cannot rule out that military forces have been present in some areas for which I don't have data for, though in these cases, if it is not in the form of a permanent base or PRT, I would not expect strong effects on community cohesion. Furthermore I exclude segments of the boundary at the time for robustness.

district level control variables and household level control variables (district level mean of all households in district d apart from household i). Standard errors are clustered at the district level in the baseline regressions.[52]

4.5. Main results

Results 1: Panel regressions

I now turn to the regression results, starting with the baseline panel regressions. Columns 1 to 3 of Table 4.1 present results for the two NRVA waves (2005 and 2007/08) and columns 4 to 6 for all three NRVA waves until 2012. I define ISAF presence according to three variables, *Mandate* (enlargement), *PRT* and military *Base*. After the end of 2006, all stages of the mandate enlargement have been completed. Thus, the variable *Mandate* takes a value of one for all observations after 2006. On the contrary, the presence of a military *Base* still varies over time. With regard to the PRTs, only one PRT has been established later than 2006, which is under the command of Turkey in the district Shibirghan of province Jawzjan. When interpreting results based on the presence of PRTs, one has to keep in mind that it captures basically no variation after 2006 as it is the case for the variable *Mandate.* Thus, results are driven by switches in ISAF presence in the earlier years of the panel.[53] This is one reason why I restrict the analysis to the two waves of the NRVA in columns 1 to 3. The second reason is that starting from 2011, the transition from ISAF command to Afghan forces began (see for more details Appendix 4.B and Figure 4.4). I account for this by excluding the tranches which have first been part of the transition process in columns 4 to 6.

The degree of *Contestation* is measured by the predetermined number of battle-related deaths from UCDP GED. In Appendix 4.E, Table 4.15, I exchange this measure with the three different types of attacks from SIGACTS. In all specifications, I find a negative relation of ISAF presence and the likelihood that a household receives help from the community. The coefficient turns insignificant for military bases in column 6. I also find a clear negative relation of pre-determined contestation and community cohesion, which increases in coefficient size and significance when not including the indicators for ISAF presence. To the extent that fighting between Western forces and insurgents occurs where Western forces are present – either permanently or occasionally – the negative coefficients point to the same inference as it is the case for the different measures of *ISAF* presence. Interestingly, the unconditional correlation of *Contestation* with *Mandate* and

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[52]For robustness I also cluster at the village- and province-level (see Appendix 4.E).

[53]For robustness of the PRT measure I run the same regressions but exchange district-fixed effects with province-fixed effects to allow for a comparison across districts but within provinces. Results are reported in Table 4.14 and support the negative finding of the presence of a PRT on community cohesion.

TABLE 4.1

Panel results, Community Help, 2005-2008 and 2005-2012

	2005-2008			2005-2012		
	Mandate	PRT	Base	Mandate	PRT	Base
	(1)	(2)	(3)	(4)	(5)	(6)
ISAF	-0.085**	-0.069**	-0.046**	-0.061**	-0.056**	-0.018
	(0.034)	(0.027)	(0.023)	(0.030)	(0.027)	(0.027)
Contestation (t-1)	-0.021**	-0.024**	-0.029***	-0.023***	-0.024***	-0.027***
	(0.009)	(0.010)	(0.009)	(0.007)	(0.008)	(0.008)
Observations	50123	50123	50123	55865	55865	55865
Adj. R-squared	0.302	0.302	0.301	0.294	0.294	0.293
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
District, Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is community cohesion measured by Community Help. ISAF presence is defined according to the column heading. All regressions include district- and year-fixed effects. The set of control variables includes aid (t-1), nightlight (t-1) and VHI (t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

PRT is only around 0.2 but close to 0.5 for military *Base*.[54] Results are robust to not including any covariates or to increasing the list of covariates as presented in Appendix 4.E, Table 4.12.

Table 4.2 turns to measures of trust and confidence in community councils (shura). Such information is not available in the NRVA survey. I, therefore, rely on the Survey of the Afghan People. Again, households are not being tracked and I apply a quasi-panel. However, the data only begins in 2007. In this setting, I define ISAF presence according to the existence of a military *Base* rather than by the mandate enlargement or the presence of a PRT, as there is no variation left in the 2007-2014 period and district-fixed effects would capture the entire variation.[55] Columns 1 and 3 present results for the full dataset and columns 2 and 4 account for the transition of ISAF to the local Army, which started in 2011. The restricted sample therefore excludes all districts where the transition from ISAF to local forces has already taken place. As it is not clear whether one should expect different effects of the presence of Afghan forces as compared to foreign forces, I present results for both samples.

In line with results presented in Table 4.1, the presence of a Military *Base* is negatively associated with community cohesion, measured by confidence and trust in community councils. The coefficient estimates are smaller and less significant when including districts,

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[54]Despite multicollinearity concerns, I present results including both ISAF presence and *Contestation*, since contestation is the most obvious confounding factor and its exclusion would likely cause an omitted variable bias. Notwithstanding, Appendix 4.E shows results when excluding contestation.

[55]District-fixed effects would capture the entire variation in PRTs apart from the single PRT in the district Shibirghan, that was installed in 2010. In particular, since PRTs differ a lot with respect to the lead nation, I do not want to deduce general effects from this single variation after 2006 in the measure based on PRT presence.

TABLE 4.2

Panel results, Trust and Confidence in Councils, 2007-2014

	Confidence		Trust	
	(1)	(2)	(3)	(4)
ISAF	-0.059	-0.126*	-0.098***	-0.177***
	(0.047)	(0.075)	(0.024)	(0.048)
Contestation	0.001	-0.000	0.001	0.002
	(0.002)	(0.003)	(0.002)	(0.003)
Observations	56664	28940	48998	29427
Adj. R-squared	0.046	0.058	0.063	0.071
Control variables	Yes	Yes	Yes	Yes
District, Year FE	Yes	Yes	Yes	Yes
Restricted sample	No	Yes	No	Yes

Notes: The dependent variable is indicated in the column heading. ISAF presence is defined according to the presence of a military base. All regressions include district- and year-fixed effects. The set of control variables includes aid (t-1), nightlight (t-1) and VHI (t-1). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

where ISAF has passed the command to the Afghan Army (see columns 1 and 3). This might suggest that the level of community cohesion recovers after ISAF forces withdraw from the district and that the Afghan lead is not as harmful for community cohesion as compared to the lead by foreign troops.[56]

Despite controlling for different fixed effects and pre-determined control variables, the coefficient estimates presented in this section can, however, not be interpreted as causal.

Results 2: Interaction with exogenous shocks

In order to get closer to measuring causal effects, I consider heterogeneous effects of short-term income shocks depending on whether ISAF is present or not. I exploit variation induced by climatic shocks, which present a common threat as the majority of households derive income from agricultural activities. While I cannot infer the direct effect of foreign security missions on community cohesion from these results, this empirical strategy allows me to interpret the coefficient estimates of the interaction with the shock as exogenous. This is because I control for the endogenous level of the interaction term (ISAF presence) (e.g., Nizalova and Murtazashvili, 2016; Bun and Harrison, 2018).

I do this in tandem with analyzing heterogeneous effects of negative income shocks according to the district's intensity of conflict. Ignoring the degree of contestation could again confound my results. I restrict this analysis to the cross-section of 2005 for three reasons. First, it is the most detailed wave with respect to variables that proxy community cohesion. Second, this wave is characterized by the most significant variation of the main

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[56]The result remain negative but turn insignificant when including province-times-year-fixed effects as presented in Appendix 4.E, Table 4.13.

variable of interest across space. Third, I can only apply the GRD for this wave and I want to allow for a comparison of the results between these two techniques that get more closely to causal analyses.

TABLE 4.3

Climatic shocks, heterogeneous effects, 2005

	ISAF Mandate		PRT		Military Base	
	(1)	(2)	(3)	(4)	(5)	(6)
Shock (t-1)	0.073***	0.054***	0.073***	0.057***	0.070***	0.063***
	(0.015)	(0.014)	(0.012)	(0.012)	(0.011)	(0.012)
Shock*Contestation (t-1)	0.034*	0.009	0.035*	0.007	0.022	0.004
	(0.020)	(0.018)	(0.020)	(0.018)	(0.019)	(0.017)
Shock*ISAF (t-1)	0.009	0.028	0.019	0.036	0.048	0.001
	(0.019)	(0.020)	(0.023)	(0.029)	(0.031)	(0.020)
Observations	30916	29785	30916	29785	30916	29785
Adj. R-squared	0.048	0.183	0.048	0.183	0.049	0.183
Control variables	No	Yes	No	Yes	No	Yes
Jointly Significant	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is community cohesion measured by Community Help. Contestation is measured by the number of battle-related deaths. The even numbered column numbers include the controls hh food insecurity, hh agricultural income, hh remittances, hh loan and district-fixed effects. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Table 4.3 shows the results for the cross-section of 2005 with the three different measures of ISAF presence excluding the triple interaction. While almost no interaction term turns out to be significant on its own, they are all jointly significant. Given that all interaction terms are positive, this result indicates that climatic shocks lead to more community cohesion, irrespective of the level of contestation and the presence of ISAF. However, the results do not allow to interpret whether ISAF on its own is increasing or decreasing community cohesion.

In a second step, I consider the triple interaction of the shock, contestation, and ISAF presence. This choice is motivated by the theoretical considerations outlined in Section 4.2. The influence of ISAF presence likely depends on the level of contestation. For ease of interpretation, I show plots rather than regression results. Figure 4.2 presents marginal effects of the exogenous shock given the level of contestation. The left graph presents the marginal effect for districts where ISAF has not yet been present and the right graph for districts in the north where ISAF's mandate has been extended to before 2005. The positive effect of the income shock on community cohesion in areas without ISAF presence is increasing in the intensity of contestation, turning significant at higher levels of contestation. On the other hand, in the north of the country, the interaction effect points in the opposite direction. With a higher intensity of contestation, it becomes less likely that households can rely on help from others in their community given that

ISAF is present. In Appendix 4.E, I show that this opposing effect due to the presence of ISAF is robust to replacing the dependent variable with *Community Help+Loan*.[57] I also show regression results for the triple interaction using the different measures of contestation (Table 4.16). This result is in line with the negative findings of ISAF presence on community cohesion in the panel regressions (see Section 4.5). While the left graph is in line with the summary drawn by Bauer et al. (2016) that violence induces cooperation, this must not be the true in case of a foreign military intervention as can be seen in the right graph.

FIGURE 4.2
Triple interaction, Community Help, 2005
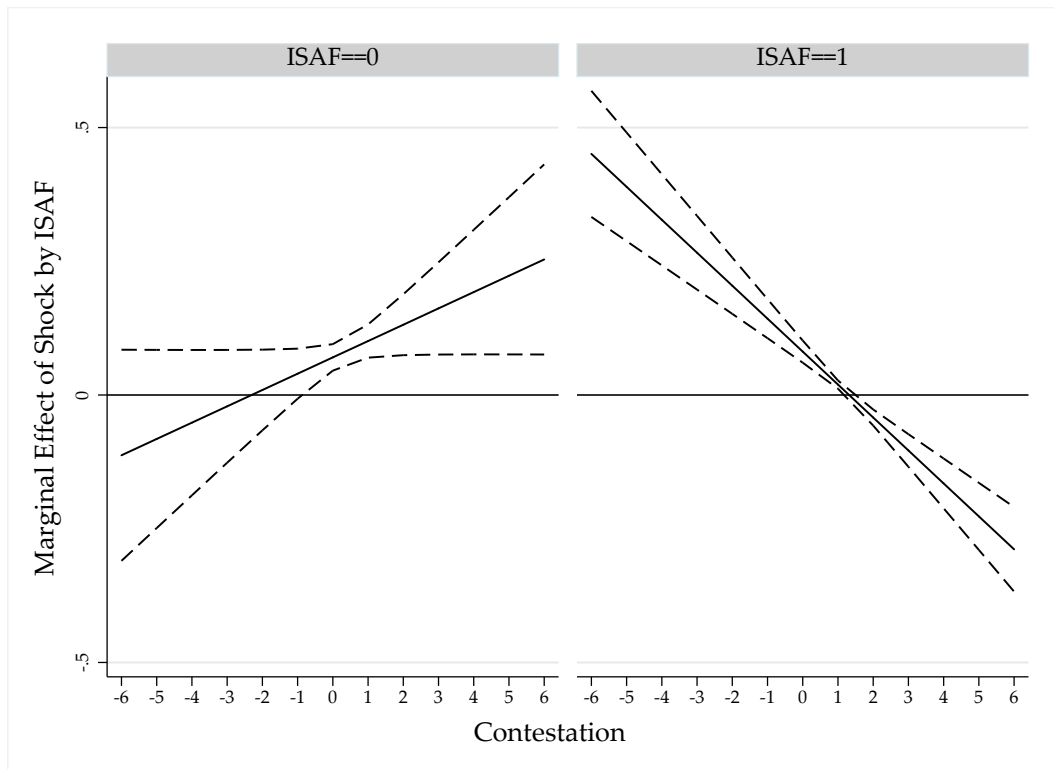
Notes: The figure presents results for one regression with the triple interaction of shock, contestation, and ISAF presence. The two graphs show the marginal effect of the shock as contestation changes for the two types of districts (ISAF versus no ISAF). Contestation is measured by the logarithm of battle-related deaths and ISAF presence by the mandate enlargement to the north. Shock is the indicator variable of whether a household has been exposed to a negative climatic shock. Marginal effects are plotted along with 90% confidence intervals.

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[57]There is no significant triple interaction effect for *Council Member*.

Results 3: Geographic regression discontinuity

Before turning to the treatment effect of ISAF's mandate enlargement, I assess the plausibility of the identifying assumptions of the GRD. The main identifying assumption is that all relevant factors besides ISAF treatment vary smoothly at the boundary. To test this, I regress pre-determined household level variables, pre-determined district level time-varying variables and district level time-invariant variables on the treatment.[58] I do not rely on simple mean comparisons for treatment and control group as the geographic heterogeneity in this RDD requires a different strategy (e.g., Keele and Titiunik, 2015; Dell et al., 2017). This is due to the fact that the balance is likely to change as one moves along the boundary. I therefore apply the local linear estimation as described in Equation 4.3 by using pretreatment and time-invariant (geographic) characteristics as the outcome variables. Results are presented in Table 4.4 in panels A-E with a bandwidth of 50 km, which represents the baseline bandwidth as I will discuss in the following. While all regressions include segment fixed effects, I ignore control variables since some of those are the outcome variable in the balancing test. Given that Western forces have been temporarily present before 2003 in some of the districts, as most obviously (and in this case even permanently) in Kabul, I consider the *restricted sample.*

It is reassuring to see that variables at the household and district level all show no significant differences according to the treatment. According to Table 4.4, households in districts close to the boundary thus seem comparable according to the available set of observable factors. In Appendix 4.E, I report balancing tests at the district- rather than the household level since many of these factors vary only at the district level. I also report more detailed balancing tests on the main outcome variable *Community Help* for 2003 across different bandwidths. These can be regarded as placebo tests given that in 2003 there was not yet such a treatment boundary according to the mandate enlargement.[59] All results support the fact that these factors vary smoothly at the treatment boundary.

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[58]While the 2003 NRVA survey serves well for balancing tests, I do not include it in the panel regressions or apply a DiD as the survey design and structure differ too much from the subsequent NRVA surveys. Nonetheless, the 2003 data is the best I can find to run balancing tests on pretreatment variables at the household level to assure that the two groups of treated and control are comparable. As for the 2005 wave I have the information on village level longitude and latitude.

[59]However, as discussed before in some areas there is evidence of the presence of international forces at the time of the NRVA 2003 survey. Ideally, I would have survey data from before 2001.

Table 4.4

Regression discontinuity, balancing tests

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Conflict (2002)						
	Insecurity		log	Fire		IED
	HH	District	BRD	Direct	Indirect	Attack
ISAF treat	-0.038	-0.081	0.243	0.155	0.320	0.170
	(0.027)	(0.129)	(0.365)	(0.131)	(0.241)	(0.154)
Observations	1540	1630	1630	1630	1630	1630
Adj. R-squared	0.007	0.284	0.278	0.077	0.108	0.127
Panel B: Government/Western forces/NGOs (2002/03)						
	Military	Employed by			Aid	
	Bases	Military	State/NGO	WB	Total	High
ISAF treat	0.773	0.010	-0.005	-1.019	-0.912	-0.237
	(0.702)	(0.011)	(0.020)	(7.967)	(1.893)	(0.313)
Observations	1630	1630	1630	1630	1630	1630
Adj. R-squared	0.127	0.010	0.015	0.256	0.084	0.065
Panel C: Geography and territory						
	Rugged-ness	Wheat Suit.	Opium Revenue	Travel Time	Share Rural	Territory Control
ISAF treat	-118.580	0.130	1019.175	123.975	-0.003	-0.597
	(125.470)	(0.130)	(631.327)	(188.044)	(0.020)	(0.386)
Observations	1630	1630	1630	1630	1630	1630
Adj. R-squared	0.500	0.275	0.376	0.314	0.090	0.763
Panel D: Ethnicity and household size (2003)						
	Pashtuns	No. Ethnic Groups	Native Langugage			HH Members
			Dari	Pashto	Uzbeki	
ISAF treat	0.343	0.528	-0.081	0.016	0.085	0.074
	(0.262)	(0.518)	(0.128)	(0.161)	(0.138)	(0.562)
Observations	1630	1630	1630	1630	1630	1630
Adj. R-squared	0.332	0.347	0.681	0.593	0.513	0.035
Panel E: Further variables (2002/03)						
	VHI	Shock		Popu-lation	Nighlight	Wheat Cons.
		Climate	Any			
ISAF treat	4.412	0.034	0.049	14.995	1.149	3.265
	(6.161)	(0.139)	(0.108)	(64.016)	(0.975)	(2.599)
Observations	1630	1630	1630	1630	1630	1570
Adj. R-squared	0.302	0.036	0.027	0.333	0.264	0.040
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	No	No	No	No	No
Restricted sample	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is indicated in the column heading. 200 km segment-fixed effects are included. All regressions are on the restricted sample. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Another concern of this approach is that there could be selective sorting as discussed in Section 4.4. Taliban insurgents could for instance move across the boundary as a response to the deployment of ISAF forces to the north. If this were the case, one would assume that along with the insurgent relocation conflict would be relocated. To rule out that the results are driven by the relocation of insurgency, I replace the 2005 outcomes of community cohesion with different measures of conflict at the same time. Results are presented in panel D of Table 4.27 (Appendix 4.E). I find no evidence in support of this concern. None of the conflict outcomes are affected by the treatment close to the boundary in the year 2005.

Table 4.5 turns to the treatment effects on community cohesion in districts close to the boundary. For the purpose of comparison, I first focus on the variable *Community Help*. Panels A to D differ in the way the RD polynomial is specified as indicated in the panel headings. Results are provided for three different bandwidths, 50 km (baseline), 75 km and 100 km. I chose the optimal bandwidth "in a data-driven, automatic way to avoid specification searching and ad-hoc decisions" (Cattaneo et al., 2017, p. 52).[60] In all regressions I include border segment-fixed effects in line with Dell (2010) and Dell et al. (2017) and a minimum set of control variables. Results without segment-fixed effects and control variables (or further control variables) are reported in Table 4.21 (Appendix 4.E). Even columns differ from odd columns to the extent that I exclude households of provinces which have potentially been characterized by the presence of foreign forces before the mandate enlargement has been implemented. The restricted sample most importantly also excludes Kabul province, which differs not only because of the presence of ISAF since 2001. In all four panels the same picture emerges. ISAF presence reduces community cohesion measured by *Community Help*. Coefficient estimates are of comparable size (in particular in panels A,C, and D) and increase in size the smaller the bandwidth. In terms of effect size, households in the treated area are 6 to 12% less likely to receive help from others in their community.

Table 4.6 presents results for alternative outcome variables *Community Help+Loan* and *Council Member*, with the latter being comprised of membership in the shura (community council) or community development council (CDC). I present results for the most rigorous specification of Table 4.5 (i.e., controlling for segment-fixed effects, covariates and taking the restricted sample) across the three different bandwidths. For both alternative outcome variables, we see the same direction of the effect, with coefficients being significant for all bandwidths and irrespective of taking a linear polynomial in the distance or in longitude and latitude (with two exceptions). The effect on *Community Help+Loan* is higher than on the main outcome variable, which

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[60]Due to household survey weights I cannot apply the RDD Stata commands (rdrobust, rdbwselect) for my main regressions, though when ignoring survey weights, rdbwselect determined 40-50 km as the optimal bandwidth for the different outcome variables.

TABLE 4.5

Regression discontinuity, Community Help, 2005

	(1)	(2)	(3)	(4)	(5)	(6)
	Bandwidth 50		Bandwidth 75		Bandwidth 100	

Panel A: Linear polynomial in distance to boundary

ISAF treat	-0.093**	-0.121**	-0.082*	-0.095**	-0.064*	-0.082**
	(0.045)	(0.052)	(0.042)	(0.044)	(0.035)	(0.037)
Adj. R-squared	0.079	0.095	0.064	0.065	0.058	0.057

Panel B: Linear polynomial in longitude and latitude

ISAF treat	-0.059**	-0.080***	-0.052*	-0.060**	-0.047*	-0.058**
	(0.025)	(0.028)	(0.028)	(0.029)	(0.026)	(0.029)
Adj. R-squared	0.078	0.093	0.065	0.064	0.059	0.056

Panel C: Quadratic polynomial in distance to boundary

ISAF treat	-0.094**	-0.120**	-0.085**	-0.096**	-0.062*	-0.081**
	(0.045)	(0.052)	(0.041)	(0.043)	(0.035)	(0.037)
Adj. R-squared	0.079	0.095	0.064	0.065	0.059	0.057

Panel D: Quadratic polynomial in longitude and latitude

ISAF treat	-0.096**	-0.113**	-0.075**	-0.088**	-0.059*	-0.080**
	(0.046)	(0.046)	(0.037)	(0.039)	(0.034)	(0.036)
Adj. R-squared	0.084	0.098	0.066	0.065	0.061	0.058
Observations	3554	3148	7495	5882	11810	8426
Number of clusters	74	64	120	103	166	144
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is Community Help. 200 km segment-fixed effects are included. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

is not surprising as *Community Help+Loan* is comprised of whether the household has received help from others in the community (*Community Help*) or received loans from family or friends. Households in the north are 16-28% less likely of receiving this type of support. The likelihood that a household is a member in a community council (either in the traditional shuras or the recently emerging CDCs) is also 12 to 18% lower as compared to the districts where ISAF has not yet been present.[61]

Results are robust to further alterations of the RD estimation equation (robustness checks are reported in Appendix 4.E). I first exclude segment-fixed effects and covariates. Second, I apply shorter segments of 100 rather than 200 km. Third, I account for a larger set of covariates including household characteristics. Fourth, I define the treatment by the direct neighborhood of a district to the treatment boundary. Fifth, I include the

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[61]More detailed results on the different outcomes including alternative specification choices are reported in Appendix 4.E in Tables 4.19 and 4.20.

TABLE 4.6

Regression discontinuity, alternative outcomes, 2005

	Community: Help+Loan			Council Member		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Linear polynomial in distance to boundary						
ISAF treat	-0.281***	-0.223***	-0.188***	-0.121	-0.176**	-0.161*
	(0.066)	(0.056)	(0.053)	(0.087)	(0.075)	(0.093)
Adj. R-squared	0.220	0.172	0.153	0.220	0.133	0.075
Panel B: Linear polynomial in longitude and latitude						
ISAF treat	-0.174***	-0.162***	-0.163***	-0.117*	-0.127**	-0.108
	(0.046)	(0.041)	(0.042)	(0.063)	(0.057)	(0.069)
Adj. R-squared	0.213	0.171	0.154	0.221	0.135	0.074
Observations	3148	5882	8426	3148	5882	8426
Number of clusters	64	103	144	64	103	144
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	50	75	100	50	75	100

Notes: The dependent variable is Community Help. 200 km segment-fixed effects are included. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. In the regressions on *Council Member* I additionally control for the presence of a council. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

interaction of the treatment with the forcing variable, which allows for different slopes at both sides of the boundary.[62] Sixth, as in the two first estimation strategies I account for the pre-determined level of contestation and also interact it with the treatment as presented in Table 4.22.[63] I do so for the different measures of conflict using data from NRVA, UCDP GED, and SIGACTS. Seventh, I cluster standard errors at alternative levels, including a wild-cluster bootstrap approach (Table 4.23 and Figure 4.6). Lastly, I apply a jackknife procedure and drop households of both treatment and control group within a boundary segment (Figure 4.7). Results are robust to any of these choices.

Taken together, the results of the different estimation approaches all point to the same finding. ISAF presence – measured by the enlargement of the mandate to the north, the presence of PRTs or military bases – has a negative effect on different measures of community cohesion. This is in line with anecdotal evidence that ISAF erodes institutions at the local level.

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[62]All these alterations are reported in Table 4.21.

[63]The interaction of contestation with the treatment does not provide evidence for a clear pattern.

4.6.   Potential mechanisms

In this section, I turn to potential mechanisms that help to explain the above finding. Since the GRD gets closest to measuring causal effects, I analyze potential channels by relying on this estimation technique.   I replace the dependent variable with a long list of alternative outcome variables in 2005.   In Table 4.27 (Appendix 4.E), I consider four different groups of channels, i) government employment and support (versus informal agricultural activities), ii) increased living standards, iii) provision of aid and infrastructure, and iv) the intensity of conflict.   Theoretically, one could argue that improvements in most of these categories render the community support less important. However, the literature has not arrived at a consensus yet on how more formal institutions supplement or complement rather informal institutions and how conflict should be affected.

There is hardly any evidence of a treatment effect on the variables presented in panels A-D. As discussed before, community cohesion does not seem to be affected because of changes in insecurity. I find no robust effect on the different measures of contestation or insecurity in panel D. The treatment does not turn out to be significant for any of these variables. I also find no evidence for a positive effect on households relying on the state as a coping strategy, which I proxy by either "worked on relief programs from Government/NGOs/International Organizations" or "joined the military" (column 1, panel A). Furthermore, there is no significant effect on loans that households take (including formal loans from banks or NGOs). Assuming that nightlight proxys for development and thus infrastructure, there is also no significant improvement as it is the case for different measures of household living standards. The only significant finding is that households participate less in any cash for work program from the National Emergency Employment Program (NEEP), National Solidarity Program (NSP) or other cash for work and income generation projects.[64] When keeping in mind that these programs often involve foreign staff, the finding would be in line with what Böhnke and Zürcher (2013) and Child (2017) argue.

However, since this result is based on only one significant finding (out of 20 regressions), I further investigate the acceptance and effectiveness of aid programs. I do so to rule out that my results are driven by an out-crowding of rather informal (traditional) ties in the community because of an increased supply of formal alternatives and thus a reduced need to rely on the former. First, I investigate the effectiveness of aid from the World Bank interacted with the treatment to identify potential heterogeneities. While the treatment did not infer changes in aid volumes according to Table 4.27, panel C, aid

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[64]Since this can simply be due to the fact that there are fewer programs, I control for the presence of a CDC and lagged aid. Results remain robust to this. However, when I include the self-reported statement that there was no such program or that the household didn't know of it, the effect turns insignificant (it remains negative though).

might be more or less effective when ISAF is present. ISAF's mission states to increase security "so that the Afghan Authorities as well as the personnel of the United Nations and other international civilian personnel engaged, in particular, in reconstruction and humanitarian efforts, can operate in a secure environment."[65] I consider nightlight and household living standards as the outcome in the aid effectiveness analysis. As can be seen in Figure 4.8, however, ISAF seems to reduce the effectiveness of development aid provided by the World Bank according to a variety of outcome measures.[66] This is in line with anecdotal evidence provided by Child (2017) from his field interviews, which points to projects causing more resistance when they are tied to the military. This again relates to the discussion of the compound treatment with military presence and the provision of aid as two parts of the treatment. Both represent the presence of foreign personnel, which is – and is perceived to be – aligned with the government.

Second, I have a closer look at my outcome variable *Council Member*, which is composed of the membership in the traditional shura and the CDC initiated by the NSP, with the latter being much more closely linked to the government and the involvement of foreign staff. So far, I analyzed the participation in any of the two councils jointly as both represent community participation. The distinction allows me to derive conclusions about the acceptance of the NSP, which aims at strengthening local governance but also at increasing government control. Table 4.7 presents results for participation in the CDC in panel A, and in the traditional shuras in panel B. The joint effect shown in Table 4.21 seems to be driven by the membership in the CDCs. While columns 1 and 2 refer to the baseline sample of the GRD, columns 3 and 4 restrict the analysis to those villages which have a CDC or shura.[67]

These findings all suggest that community support is not crowded out by formal state support or by the increased effectiveness of development aid projects, which render community support less important. They also indicate that institutions set up by the state often in partnership with foreign NGOs or military personnel seem less welcome. Since I have no data on attitudes, I cannot dig deeper into this when using the GRD. However, these results support the general picture derived from the literature that considers attitudes and either violence committed by ISAF (e.g., Lyall et al., 2013; Schutte, 2017) or the provision of aid (e.g., Child, 2017).[68] It also fits anecdotal evidence, as for instance

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[65]Source: https://www.nato.int/isaf/topics/mandate/unscr/resolution_1510.pdf, accessed April 9, 2018.

[66]The marginal effect of aid is more negative for all outcome measures when the household lives in the treated area where ISAF is present. The marginal effect turns significant in three of these cases (wheat consumption, expenditures, and food security).

[67]In about 50% respectively, with more than 80% of the households living in a village/community where there is either a CDC or a traditional shura.

[68]Beath et al. (2016) identifies generally positive effects of the NSP program on economic outcomes and support for the government, but not in regions close to Pakistan, where external insurgents are involved which do not rely on the local population for support. In my analysis ISAF is also an external force, though different from the external insurgents discussed by Beath et al. (2016). The difference

TABLE 4.7

Regression discontinuity, Council Member, 2005

	Full sample		If council=1	
	(1)	(2)	(3)	(4)
Panel A: CDC				
ISAF treat	-0.152**	-0.097**	-0.379***	-0.241*
	(0.062)	(0.038)	(0.123)	(0.122)
Observations	3148	3148	1731	1731
Adj. R-squared	0.170	0.170	0.120	0.120
Panel B: Traditional Shura				
ISAF treat	0.029	-0.032	-0.062	-0.296
	(0.085)	(0.058)	(0.110)	(0.178)
Observations	3148	3148	1687	1687
Adj. R-squared	0.171	0.177	0.187	0.196
200km segments	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes
GRD type	Linear	Long & Lat	Linear	Long & Lat

Notes: The dependent variable is membership in either the CDC or traditional shura. Results are provided for the 50 km bandwidth. 200 km segment-fixed effects are included. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

by General McChrystal, who notes "we face not only a resilient and growing insurgency; there is also a crisis of confidence among Afghans – in both their government and the international community – that undermines our credibility" (Jones and Muñoz, 2010, p. 8). In analogy, Giustozzi (2008, p. 35) points out that the deployment of troops has been interpreted as increased repression by local communities.

My findings contribute to the literature and anecdotal evidence by highlighting that not only the perceptions of and collaboration with the insurgents or the government can change, but also that ties within communities are adversely affected. What is more, my findings indicate that the negative effect of the presence of international forces is not dependent on the level of violence. Neglecting the role of foreign interventions can lead to mixed findings on how violence affects community cohesion and on how development aid can be effective in winning hearts and minds and achieving reconstruction efforts.

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between my finding and their finding for regions not close to Pakistan could be driven by the different time horizons. While I account only for the short-term effects Beath et al. (2016) considers longer term effects. Also anecdotal evidence points to first skepticism among communities, which later turned into trust into this program (Nixon, 2008).

4.7. Conclusion

In this paper, I analyze whether and how the presence of foreign military forces relates to community cohesion in times of conflict. I consider Afghanistan, which has been exposed to conflict for decades and where households had to adopt coping strategies to deal with the never-ending insecurity. In an environment where state institutions are unstable or even lacking, community cohesion and cooperation plays a central role as a coping mechanism. This is not only relevant from the perspective of households, but also with regard to the success of security missions and development projects. In particular, I consider the role of one of the largest military coalitions in NATO's history, the International Security Assistance Force (ISAF).

I propose three different estimation techniques to get as close as possible to estimating causal effects. First, I rely on household level quasi-panel data from the National Risk and Vulnerability Assessment (NRVA) and the Survey of the Afghan People (from the Asia Foundation) and apply high-dimensional fixed effects and pre-determined control variables to capture an important part of the omitted variables that might bias the results. Second, I investigate how exogenous income shocks affect the level of community cohesion differently according to the presence of ISAF. Third, I exploit a geographic regression discontinuity. I make use of the step-wise enlargement of ISAF's mandate as envisaged by the Bonn Agreement. UNSC Resolution 1510 on October 13 in 2003 announced the enlargement of ISAF to the northern regional command to support the government beyond the capital Kabul. While the first stage was completed in October 2004, stages 2 to 4 have been implemented after the NRVA household survey in 2005 had been conducted. The 2005 NRVA household survey wave allows the comparison of households close to the boundary between the northern regional command (treated area) and the rest of the country (control area) as if they were randomly assigned.

The findings suggest that households in districts where foreign military forces are present receive less help from others in their community, have less trust in community councils and participate less in those institutions. This finding is robust across the different estimation techniques and to numerous robustness checks. I also provide evidence that this is not due to a crowding-out of informal institutions by an increased provision of formal institutions.

It is well accepted that local communities are relevant partners in postwar reconstruction, counterinsurgency, and peace-building. Yet, prior work has focused on attitudes and collaborative behavior with either insurgents or pro-government groups including foreign military personnel. The role of ties within communities has received much less attention, though. When the community's social glue is eroded because of the foreign military intervention, this can harm the effectiveness of security missions, reconstruction and development projects, and consequently nation-building.

Appendices to Chapter 4

4.A. Origins of administrative borders

Historical origins: Afghanistan has a long history of military occupations and interventions by foreign countries, including Great Britain (colonial empire), the Soviet Union and more recently the United States.[69] After the attempts by the British to control the country through the first (1839-1842) and second (1878-80) Anglo-Afghan War, the British decided to turn the country into a buffer state. By the end of the 19th century, the British pushed for a formal border between Afghanistan and British India (today it marks the border between Afghanistan and Pakistan). Mortimer Durand negotiated the Durand Line Treaty with Abdur Rahman, who was Emir of Afghanistan from 1880 to 1901. The Durand line forms a boundary that is largely not recognized by Afghanistan and which divided the Pashtun population in half. With regard to the northern border, agreements with the Russian government took place in 1885. The greater part of the northern border is demarcated by rivers (Oxus river, now known as the Amu Darya) (Omrani, 2009). According to Giustozzi (2008), Abdur Rahman set the basis for what became Afghanistan's administration. Abdur Rahman also introduced smaller provinces than before and replaced local rulers with his own representatives. "Abdur Rahman was also the first ruler to start the policy of deporting whole communities to far-off regions" (Giustozzi, 2008, p. 5), a practice that has been continued until 1959. The rulers aimed at creating a mix of ethnicities in order to regain support for the central government. In particular, Pashtun tribes have been exposed to this practice and have been deported to the northern regions.

In general, administrative units within Afghanistan have been repeatedly reorganized. King Nadir Shah, who reigned Afghanistan from 1929 to 1933, split the country into eight provinces, which were under the power of the central government. The command was going from province to district and to sub-district level. This system was dominant until a major reform of the administrative boundaries has been undertaken in 1963 (Gopalakrishnan, 1982). This reform reorganized the country into 28 provinces and set the basis for today's administrative divisions.[70]

The historical and political gazetteers for Afghanistan indicate that the borders have often been demarcated by geographic features such as rivers or mountains. According to Giustozzi (2008), the Afghan state throughout tried to apply "divide and rule" tactics: "Provincial boundaries were drawn in such a way as to divide communities and create multi-ethnic and multi-tribal administrative units, making it difficult for the local

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[69]Figure 4.16 plots the directions and major fighting territories of the Soviet invasion from 1979-1989.

[70]See http://www.iranicaonline.org/articles/afghanistan-xi-admin and pahar.in/wpfb-file/1985-historical-and-political-gazeteer-of-afghanistan-vol-6-kabul-and-se-afghanistan-s-pdf/, both accessed June 8, 2018.

population to come together and influence or oppose government" (Giustozzi, 2008, p. 21). This indicates that the administrative units are not a construct of ethnic or tribal homelands and have rather even been constantly changed.

Recent reorganization: A more recent reorganization took place in June 2005, where the Afghan Ministry of the Interior assigned 398 districts to 34 provinces. Prior to this change, the country was divided into 329 districts and 32 provinces.[71] In most cases, province boundaries have not been affected by this new reorganization, with the exception of the creation of two new provinces (Daikondi and Panjshir). In most cases, districts have been split and in few cases reassigned to another (new) province. Only in case of two districts such a transfer took place at the GRD treatment boundary, shifting these two districts from treatment to control group. The two districts Kahmard and Sayghan have first been part of Baghlan province (northern command) and then in 2005 been assigned to Bamyan province (eastern command).[72] Given that NATO was deployed to the north before the administrative reform took place, I assume these two districts to be treated in 2005 as they have belonged to the Baghlan province and thus have been part of the first stage of the mandate enlargement to the north.[73] However, for robustness I first exclude them as well as the province Bamyan and, second, rerun all regressions using the *new treatment boundary* that is based on this shift of the two districts.[74] Apart from these administrative units no other units have been shifted in a way that they crossed the *treatment boundary* given the administrative reorganization in 2005.[75]

This change occurred just right before the NRVA 2005 wave has been conducted. While starting from this wave, households have been assigned to the new list of districts, this was not so the case for the 2003 wave, which I use for balancing tests. In the latter case, I used the village geocodes (longitude and latitude) and matched those to the new administrative units, i.e., the 398 districts.

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[71]Source: http://www.aims.org.af/services/mapping/geo_codes/398_dist_matching_to_329.xls and http://www.statoids.com/uaf.html, accessed June 11, 2018.

[72]Source: http://www.aims.org.af/services/mapping/geo_codes/398_dist_matching_to_329.xls. I compared the shapefiles for 329 and 398 districts provided by e.g., https://esoc.princeton.edu/country/afghanistan, accessed June 9, 2018.

[73]In June 28, 2004 the establishment of 4 PRTs in the North has been announced including Baghlan (https://www.nato.int/cps/en/natohq/topics_69366.htm, accessed June 9, 2018).

[74]The new boundary is plotted for comparison in Figure 4.9.

[75]Note that Bamyan province is anyhow excluded in the most rigorous specifications as there are indications of ISAF presence before the mandate enlargement to the east officially took place.

4.B.   Nato involvement in Afghanistan

General facts: Following the Bonn Agreement in 2001, ISAF was tasked to support the Afghan government in securing Kabul and its surroundings exclusively.[76] At that time it was under the lead of individual NATO allies, with the lead being based on a six-month national rotation. The NATO took the lead of ISAF in Afghanistan on August 11, 2003 with the main objective "to enable the Afghan government to provide effective security across the country and develop new Afghan security forces to ensure Afghanistan would never again become a safe heaven for terrorists."[77] ISAF supported the Afghan National Security Forces (ANSF) in conducting security operations and in counterinsurgency activities, with the aim at increasing the capacity and capabilities of the Afghan forces. Another objective was to improve governance and socio-economic development and to create sustainable stability. 51 NATO and partner nations were involved with 130,000 strong troops at its height. Originally the international forces were deployed to Kabul, though the presence was subsequently enlarged as described in the following. UNSC Resolution 1510 "[a]uthorizes expansion of the mandate of the International Security Assistance Force to allow it, as resources permit, to support the Afghan Transitional Authority and its successors in the maintenance of security in areas of Afghanistan outside of Kabul and its environs, so that the Afghan Authorities as well as the personnel of the United Nations and other international civilian personnel engaged, in particular, in reconstruction and humanitarian efforts, can operate in a secure environment, and to provide security assistance for the performance of other tasks in support of the Bonn Agreement."[78] At the end of 2006, the expansion over the entire country has been completed.

Mandate enlargement: In the following I provide a summary of the enlargement of ISAF's mandate split into four stages according to the four regional commands as presented in Figure 4.9, with stage 1 starting in the north of the country to stage 4 (covering the entire country).[79]

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[76]The Bonn Agreement established the Afghan Interim Authority (AIA) with Hamid Karzai as Chairman.

[77]Source: https://www.nato.int/cps/en/natohq/topics_69366.htm, accessed April 9, 2018.

[78]Source: https://www.nato.int/isaf/topics/mandate/unscr/resolution_1510.pdf, accessed April 9, 2018.

[79]Source: https://www.nato.int/cps/en/natohq/topics_69366.htm, accessed April 2, 2018.

FIGURE 4.3

ISAF mandate expansion

Notes: This figure presents the expansion if the ISFA mandate. Source: https://www.gov.uk/government/publications/uks-work-in-afghanistan/the-uks-work-in-afghanistan, accessed June 27, 2018.

Stage 1: To the North	
December 31, 2003	taking command over PRT in Kunduz as a pilot
June 28, 2004	announced establishment of 4 PRTs in the North
	(Mazar-e-Sharif, Meymana, Feyzabad, Baghlan)
Oct. 1, 2004	process completed: present in 9 northern provinces
Stage 2: To the West	
February 10, 2005	announced enlargement to the West
May 31, 2006	process began
	taking command over PRT/bases in Herat and Farah
September, 2006	two more PRTs become operational (Ghor, Baghdis)
	present in 50% of Afghanistan's territory:
	9 northern provinces + all western provinces
Stage 3: To the South	
December 8, 2005	plan for stage 3 endorsed
July 31, 2006	process began
	command expanded over 6 provinces including 4 PRTs
	(Daykundi, Helmand, Kandahar, Nimruz, Uruzgan,Zabul)
	covering 3/4 of Afghanistan's territory (total of 13 PRTs)
Stage 4: To the East	
October 5, 2006	final stage implemented
	responsibility of entire country

With regard to the exact timing when stage 2 (west) began, there is some mixed evidence. The earliest date that is mentioned is May 2005, which would be just shortly before the NRVA survey has been conducted (June to August 2005). This date is in contrast to what is noted on the NATO website and to official numbers of when PRTs fall under the command of ISAF or when they have been opened by ISAF. To still eliminate any concerns, I exclude these western provinces in the GRD for robustness (see Table 4.26). Results are not affected by this. Note also, that even if ISAF started to be present earlier than what the official numbers claim, I don't expect effects to occur within a month (when the survey has been conducted). Moreover, most of the questions I use from the NRVA refer to the last 12 months.

Provincial Reconstruction Teams (PRT): This unit has been created by a program called Coalition Humanitarian Liaison Cells before the first stage of the mandate enlargement took place. They have then been assigned to NATO command and been renamed into PRT, with different nations taking the lead of the 26 units. Originally, PRTs were US-funded and directed and "[t]hese cells were made up of five to ten Army Civil Affairs Officers who manned small outposts in the provinces of Afghanistan where Coalition Forces were present."[80] Because of the different lead nations, they lack an overarching strategy and differ in size, structure and guidance. In general, these units were set up to provide support to other actors for reconstruction, development and humanitarian assistance. The principal role of the PRTs in this respect was to build Afghan capacity, support the growth of governance structures and promote an environment in which governance can improve. Since some PRTs have been active before the mandate enlargement began, I account for this in my analysis when looking at the *restricted sample*. According to Eronen (2008), the first PRTs were established in 2003 in Gardez, Kunduz, Bamyan, and Mazar-e Sharif. I exclude the regions for robustness in the untreated group.[81]

Transition to Afghan forces: ISAF started in 2011 to pass over responsibility to the Afghan forces with the transition being completed at the end of 2014. The gradual transition process, in Pashtu and Dari called "Inteqal", was split into 5 tranches. The process is displayed in Figure 4.4 below. I digitilized Eynde et al.'s (2017) map of the transition process. Because of a lack of information on the exact transition ceremonies, the authors use the announcement of the transition stages by president Karzai. For more details on this process see Eynde et al. (2017) who analyze the effects of the transition from international lead to Afghan lead on insurgent activity and counterinsurgent effectiveness.

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[80]source: http://www.understandingwar.org/provincial-reconstruction-teams-prts, accessed April 6, 2018.

[81]For the Bamyan province this is also stated in other sources, as for instance http://www.nzdf.mil.nz/news/media-releases/2013/20130405-codbma.htm, accessed May 22, 2018.

In this paper, I account for that by looking at the *restricted sample*, which excludes districts where the transition already took place. Note that this is only relevant for the panel regression analysis.

FIGURE 4.4
Security transition from ISAF to Afghan Army
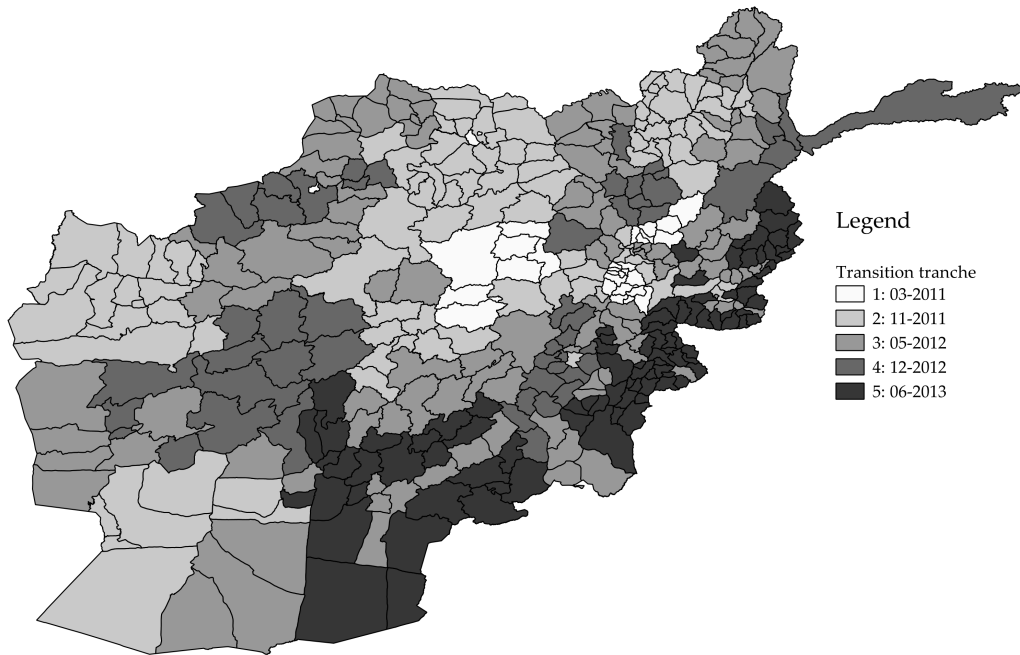
Notes: This figure illustrates the security transition tranches from ISAF to the Afghan Army starting in 2011 and being completed in December 2014 with the transition ceremony (end of ISAF involvement). Highlighted are the announcements of the transition tranches by president Karzai, not the actual completion of the respective transition. Source: Eynde et al. (2017).

The transition process also included the phasing out of all PRTs by the end of 2014 with their functions being handed over either to the government, development actors or to the private sector. After ISAF's mission was completed when the transition ended, a new non-combat mission was launched already in January 2015, the so-called *Resolute Support*. This mission's objectives are to provide further training, advise and support to the Afghan security forces. Western forces are therefore continuously present.

Apart from ISAF

Operation Enduring Freedom (OEF): In October 2001, the US-led coalition OEF started a military campaign. Dorn (2011, p. 18) describes OEF's main goal as to defeat terrorists, in particular, al-Qaeda and the Taliban, and that it "uses primarily a warfighting strategy." It is different from ISAF also for other reasons, as for instance with regard to its lacking authorization by the UNSC. Neither the invasion nor the creation of OEF have been authorized. However, several UNSC resolutions acknowledged OEF, such that it became clear that the intervention is not illegal.[82] Within the first two years where ISAF was a rather small force composed of 5,000 and restricted to Kabul, "OEF continued operating throughout the country, though its permanent presence was limited to the Kabul region and a few bigger cities in the east and southeast of the country" (Eronen, 2008, p. 3). Since this could bias my results, I exclude those locations where I have information on their presence before the 2005 survey wave has been conducted (again only for the control group). In Table 4.26, I also exclude the eastern command from the GRD for robustness. Note that the south where they have most bases is not included in the GRD as this is outside of the applied bandwidths. US facilities as of January 2005 are plotted in Figure 4.11. While the first couple of PRTs were under the lead of the OEF –according to Eronen (2008) Gardez, Kunduz, Bamyan and Mazar-e Sharif – their lead has been passed to ISAF throughout its mandate expansion. As noted before, I exclude Bamyan province in the regression analysis when I refer to the restricted sample as it would be part of the GRD but in the control group.

United Nations Assistance Mission in Afghanistan (UNAMA): The UNAMA was established by the UNSC (Resolution 1401 of March 28, 2002). It aims at strengthening the foundations of a constitutional democracy in Afghanistan. Different than ISAF and OEF, around 80% of the staff are Afghan nationals (Dorn, 2011). It, therefore, does not represent an entirely foreign intervention. Besides that, "UNAMA, for its part, has at present only a small cadre of uniformed personnel in Afghanistan and very little ability to use force" (Dorn, 2011, p. 18). It works together with the foreign military, development and humanitarian agencies, and the Afghan government, though it "does not dictate security policy, and focuses instead on developing governing capacity, democratic institutions, respect for human rights, and sustainable development."[83]

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[82]Source: http://pom.peacebuild.ca/AfghanistanPeaceOperation.shtml, accessed June 27, 2018.
[83]Source: http://pom.peacebuild.ca/AfghanistanPeaceOperation.shtml, accessed June 27, 2018.

4.C.   Definitions and sources

NRVA dataset

The data has been collected at three different levels; the household level (with both male and female questionnaires), the community level (shura), and the district level for price data. The surveys are statistically representative to the provincial level, which is not the unit of analysis that I apply. Following Child (2017), I regard the data at the district level to yield reasonable approximations for district level inference since sample sizes at the district level are quite large. For randomization, Afghanistan was divided into strata (the 34 provinces plus the urban areas), and in each stratum, a number of clusters (primary sample units - PSU) of 12 HH were randomly selected to achieve a balanced sample across strata. The large difference of the population size across strata has required a deviation from the balanced sample (for very large and very small strata) as controlled for by the use of sampling weights. The household selection follows a quasi-random process: The total number of dwellings in a community (PSUs) was divided by 12. The resulting number was to account for the distance between two interviewed households to spread the information collected within a PSU. For more details see CSO (2005, 2007/08, 2011/12).

Definitions and sources

Aid: Data on aid is derived from AidData (2017). I use data on World Bank aid (WB Aid), which includes IBRD and IDA, as well as on aid by all donors coded by the Afghanistan Recipient System (Total Aid) (Goodman et al., 2016). AidData provides aid commitments. For both types of aid, I include all sectors and all location types and take AidData's geographical exactness of 1 and 2. The mean of aid by all donors is about three times as high as WB aid only. Both measures are correlated with a correlation coefficient of 0.68. I also construct "High Aid" (WB), which takes a value of one if the district receives more WB aid than the mean district in a particular year.[84]

Age/Sex (HH Head): From the NRVA (CSO, 2005, 2007/08, 2011/12). Sex takes a value of 1 for female HH heads.

Agricult. Income: The dummy is equal to 1 if the household receives any income from agriculture or works in agriculture. From the NRVA (CSO, 2005, 2007/08, 2011/12).

Any CDC/Shura: The dummy is equal to 1 if the household lives in a village/community where there is a shura/CDC. From the NRVA (CSO, 2005).

Any Shock: Households have been asked whether they experienced any shocks (including insecurity, climatic shocks, price shocks etc.) within the last 12 months. From

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[84]For more information see https://www.aiddata.org/data/world-bank-geocoded-research-release-level-1-v1-4-2 and https://www.aiddata.org/data/afghanistan-aims-geocoded-research-release-level-1-v1-1-1, accessed July 12, 2018.

the NRVA (CSO, 2005, 2007/08, 2011/12).

Cash/Food for Work: The dummy is equal to 1 if the household participates in any cash (or income generating) or food for work programs. From the NRVA (CSO, 2005).

Confidence/Trust in Councils: From the Survey of the Afghan People (Asia Foundation, 2007-2014). "I would like to ask you about some officials, institutions and organizations in our country. I will read these out to you. As I read out each, please tell me how much confidence you have in each of the institutions and organizations and officials to perform their jobs. Do you have a great deal of confidence, a fair amount of confidence, not very much confidence, or no confidence at all in Community Shuras/ Jirgas." The question on trust is phrased in analogy. The variables take a value of one if the household has a great deal of confidence or a fair amount of confidence, zero if not very much confidence or no confidence at all, and missing if refused or don't know.

Contestation: I derive measures on conflict (contestation) from different sources. Battle-related deaths are from UCPD/GED (Sundberg and Melander, 2013). IED, Direct Fire and Indirect Fire are from SIGACTS, provided by Shaver and Wright (2016) at the district-year level. Insecurity is a subjective conflict measure from the NRVA survey on whether households have experienced an insecurity shock within the last 12 months (CSO, 2005, 2007/08, 2011/12). The first four measures are used in logarithms, while the latter (*Insecurity*) gives the share of households per district or takes a value of one for households exposed to this shock.

Economic Improve: This variable refers to the question "How do you compare the overall economic situation of the household with 1 year ago?" 1 indicates much worse, 2 slightly worse, 3 same, 4 slightly better, and 5 much better. This is a self-reported measure of the household. From the NRVA (CSO, 2005,2007/08,2011/12).

Employment: From the NRVA survey (CSO, 2005, 2007/08, 2011/12) on whether the household is employed by the military, state or NGOs. *Employed by State/NGO* takes a value of one if the household "Worked on relief programmes from Government/NGOs/International Organisations." *Employed by Military* takes a value of one if the household "Joined military." *Employed by Gov.* takes a value of one if household is employed by or receives benefits/pension from the government.

Ethnicity/Language: I derive information on ethnicity and languages from two different sources. One is the NRVA 2003 survey wave (CSO, 2003), which includes a question on the native language spoken by the household. I include the shares of households speaking one of the three main languages (Dari, Pashto, Uzbeki). The second source is the "georeferencing of ethnic groups" (GREG) dataset from Weidmann et al. (2010). It relies on maps from the "Soviet Atlas Narodov Mira" from 1964. It contains the coordinates of the group boundaries of ethnic groups. I define two variables from the latter dataset; one indicator variable taking the number of one if Pashtuns are in the districts and another variable counting the number of ethnic groups.

HH Members/Children: Number of household members in total and number of children in household. From the NRVA (CSO, 2003, 2005, 2007/08, 2011/12).

ISAF: I construct three different measures. *Mandate* enlargement takes a value of one for the northern region from 2005 on, the value switches to one for the remaining regions from 2006 on, i.e., for the survey wave of 2007/08.[85] This indicator is based on the mandate enlargement as presented in Figure 4.3. Data on the location, opening and lead nations of PRTs is derived from https://www.nato.int/isaf/topics/prt/index.html and https://www.nato.int/cps/ua/natohq/topics_69366.htm (both accessed June 26, 2018). Data on military bases is the same used in Chapter 3. As described in Chapter 3, we use information from Wikipedia's GeoHack program for information on rather well-known bases and rely on news articles, Wikimapia and Google Maps satellite data for the less well-known ones. Due to the lack of public information because of security reasons, this dataset does not capture all existing locations and therefore it introduces some measurement error. However, as discussed in Chapter 3 there is no reason to believe that the measurement error is non-normal. The variable takes a value of one if there is at least one open military base in a district i in year t. For more details see Appendix 3.H (Chapter 3). For PRTs and bases I construct measures on whether they are present in district i or its neighboring districts.

Loan: From the NRVA (CSO, 2005, 2007/08, 2011/12). The dummy is equal to one if the household responds with yes to the following question: "Have you or any household member taken a loan in the last year?"

Nightlight: Data on nightlight varies at the district-year level. Version 4 DMSP-OLS Nighttime Lights composites from AidData (2017). As in Chapter 3, I use nightlight in logarithms.

Opium Revenue: Opium revenues is derived from cultivation in hectares and the respective yields. Cultivation data at the district-year level is an estimate from the data at the province level. After multiplying cultivation with yield, I constructed opium revenues by multiplying opium production in kg with the fresh opium farm-gate prices at harvest time in constant 2010 EU/kg. From the Annual Opium Poppy Survey (UNDCP, various years) and Afghanistan Opium Survey (UNODC, various years).

Population: Population count (UN adjusted values) from Gridded Population of the World v4 (GPWv4) (AidData, 2017). GPWv4 depicts the distribution of human population across the globe.

Remittances: The dummy is equal to 1 if the household receives any remittances. From the NRVA (CSO, 2005, 2007, 2011/12).

Ruggedness: The data on terrain ruggedness comes from Nunn and Puga (2012). For more details see http://diegopuga.org/data/rugged/ (accessed June 30, 2018). I

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[85]My sample starts in 2005 because of data availability, such that I cannot code the first stage to begin in 2004.

define it by 1000 to keep coefficients in a readable size.

Share Rural: From the NRVA 2003 survey wave to get pre-determined values at the district level (CSO, 2003). District level shares of rural population.

Shock: I use the following shocks to construct the binary indicator variable measuring climatic shocks: Earthquakes, Landslides/avalanches, Flooding, Late damaging frosts, Heavy rains preventing work, Severe winter conditions, Hailstorms. Households have been asked whether they experienced any of these shocks within the last 12 months. From the NRVA (CSO, 2005, 2007/08, 2011/12).

Travel Time: Estimated travel time to the nearest city of 50,000 or more people in year 2000 (Nelson, 2008). Global Environment Monitoring Unit - Joint Research Centre of the European Commission, Ispra Italy. Available at http://forobs.jrc.ec.europa.eu/products/gam/ (AidData, 2017).

Territorial Control: The data comes from Dorronsoro (2005), who provides a map on the territorial control of the Taliban in 1996 and of other major groups of the Northern Alliance (Dschunbisch-o Islami, Dschamiat-i Islami, Hizb-i Wahdat). More details on the georeferencing of this variable can be found in Appendix 3.H (Chapter 3).

Vegetation Health Index (VHI): I use the Vegetation Health Index (*VHI*) provided by the FAO (Van Hoolst et al., 2016) as an objective indicator of climatic shocks. The index can be used as a proxy for droughts as low values indicate drought conditions. For more details see Appendix 3.A (Chapter 3).

Wheat Suitability: The FAO-GAEZ (2012) model provides for each crop/Land Utilization Type a comprehensive soil suitability evaluation for all the soil units contained in the Harmonized World Soil Database. Source: Global Agro-ecological Zones (GAEZ v3.0) by the Food and Agriculture Organization of the United Nations (FAO-GAEZ 2012). Details are provided on the website http://www.fao.org/nr/gaez/about-data-portal/agricultural-suitability-and-potential-yields/en/ (accessed July 9, 2018).

Living standards

In the following I describe in detail the construction of each indicator which I use to measure living standards. For most indicators I follow Deaton and Zaidi (2002), D'Souza and Jolliffe (2013) and Wiesmann et al. (2009). All these variables are derived from the NRVA survey (CSO, 2005, 2007/08, 2011/12).

Asset Index: Rather than applying principle component analysis I use the number of assets the household possesses (constant over waves). This set consists of Radio/Tape, Refrigerator, TV, VCR/DVD, Sewing Machine, Thuraya (any phone),

Bicycle, Motorcycle, Tractor/Thresher, Car. This is done without using any weights representing the quality of the asset because of lack of information. I therefore prefer this transparent and easy-to-interpret measure.

Calorie Intake and Food Insecurity: I restrict the construction of the calorie intake on information provided in section 15 of the NRVA household survey, which is part of the woman's questionnaire and contains amounts, frequencies and sources of a large set of food items. Unfortunately, I could not include for instance how much food they received in the course of food-for-work programs as no amounts are provided.[86] I use kcal values provided by the CSO and The World Bank (2011).[87] Amounts consumed are then multiplied by kcal values for that type of food and the sum represents the total household calorie intake.

Besides including the calorie intake as a continuous variable, I construct a binary indicator of food insecurity. For an individual the reference value would be 2100 calories per day as recommended by the FAO. To evaluate whether each individual in the household would reach the threshold I divide the total household daily calorie intake by the number of members that were resident and ate at least dinner regularly in the household during the last seven days. I adjust this number of resident household members by how many guest meals have been reported and how many person-meals have been eaten outside home.

Food Consumption Expenditures: As discussed in Deaton and Zaidi (2002), consumption- or expenditure-based measures are regarded to be more appropriate as compared to income as they are smoother as well as less variable (e.g., due to seasonality). Besides that reason, income sources among the poor are usually more spread and thus difficult to measure, especially when households draw income from self-employment or are subsistence farmers (Deaton and Zaidi, 2002, p. 14; Jolliffe et al., 2004, p. 558). Finally, households might be more willing to give information about their expenditures as compared to their income situation (Jolliffe et al., 2004, p. 558). Following Deaton and Zaidi (2002) I include food items from all possible sources (purchased, gifts, etc.). The NRVA survey includes a separate section of local prices at the district level which are merged to the household level dataset on food consumption (section 15, women's questionnaire).

I adjusted for spatial price differences, since households in different districts face different prices. I use the Paasche and Laspeyre's Price indices to account for that. As underlined in the literature (Deaton and Zaidi, 2002, p. 42) the median is preferred to the mean due to its lower sensitivity to outliers, which might have been caused by misunderstandings about values etc. For missing values regarding district prices I

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[86]Note, however, that only few households participated in any such programs.

[87]For a few items, i.e., number of eggs, nan pieces and maize(corn) I use kcal values reported in http://siteresources.worldbank.org/AFGHANISTANEXTN/Resources/305984-1326909014678/8376871-1334700522455/NRVA0708-Quality.pdf, accessed June 30, 2018.

have generated the province median, which in case of missing values has been replaced by the national median price. For almost all the reported food items in the women's questionnaire prices have been given by the district questionnaire. Food expenditure is measured in constant prices (I use both 2005 and 2011/12 prices). While the 2005 wave includes more districts, the 2011/12 wave is more complete with respect to price data availability for each food item.[88] I only include food items that are surveyed in all three waves to allow for comparability across waves.

I add expenditures (adjusted for inflation and regional price differences) of food and drinks consumed outside home from the men's questionnaire.[89] Unfortunately, I could not account for guest meals as it is not clear of which food items they are composed of. As for the calorie intake I measure per capita expenditures by dividing the total household food consumption measure with i) the number of households (resident and ate at least dinner regularly in the household during the last seven days), and ii) the number of resident household members adjusted by guest meals.

Food Consumption Score: The Food Consumption Score has been developed by the World Food Program as an alternative measure of food diversity. The food consumption score differs from the simple food diversity measure to the extent that each food group gets a weight representing the food group's quality. I therefore multiply the frequency of each food group with those weights and take the sum over all food groups. Food frequency, in this context, is defined as the frequency (in terms of days of consumption over a reference period) that a specific food item or food group is eaten at the household level. For a detailed description see Wiesmann et al. (2009) and World Food Programme.[90]

Food Diversity: According to Wiesmann et al. (2009) "Dietary diversity is defined as the number of different foods or food groups eaten over a reference time period, which in my case is one week, not regarding the frequency of consumption." I categorize food items into eight food groups following Wiesmann et al. (2009) and the World Food Programme. These groups are staples, pulses, vegetables, fruit, meat/fish, milk/dairy, sugar, and oil/fat. The variable varies between zero and eight, with eight indicating a high food diversity and thus higher standards of living.

Wheat Consumption: The Afghan food consumption is to a large extent based on wheat consumption. I construct a continuous variable representing the per capita wheat consumption within a household. According to D'Souza and Jolliffe (2013), calorie intake from wheat makes up more than half of total calorie intake.

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[88]When using constant 2005 prices I replace missing prices for few food items with the 2011/12 data.

[89]Unfortunately no amounts and sources on drinks consumed at home are provided in the 2005 survey such that I also disregard those for the 2007/08 and 2011/12 survey as well.

[90]See http://documents.wfp.org/stellent/groups/public/documents/manual_guide_proced/wfp197216.pdf, accessed June 30, 2018.

4.D.   Descriptive statistics

TABLE 4.8
Descriptives, 2005-2012

	Observations	Mean	Stand. Dev.	Min	Max
Community Help	55865	0.17	0.37	0.00	1.00
Community Help+Loans	55865	0.25	0.44	0.00	1.00
Council Member	55865	0.12	0.32	0.00	1.00
Climate Shock	55865	0.36	0.48	0.00	1.00
Any Shock	55865	0.60	0.49	0.00	1.00
Insecurity	55865	0.10	0.30	0.00	1.00
(log) BRD	55865	1.22	1.64	0.00	6.63
IED	55865	7.45	24.24	0.00	450.00
Direct Fire	55865	17.33	80.43	0.00	1625.00
Indirect Fire	55865	6.77	21.46	0.00	433.00
ISAF Mandate	55865	0.64	0.48	0.00	1.00
PRT	55865	0.30	0.46	0.00	1.00
Base	55865	0.29	0.45	0.00	1.00

Notes: Sample based on Table 4.1, columns 4-6. For the definition of the variables see Appendix 4.C.

TABLE 4.9
Descriptives, 2005, Bandwidth 50 km

	Observations	Mean	Stand. Dev.	Min	Max
Community Help	3554	0.05	0.23	0.00	1.00
Community Help+Loans	3554	0.17	0.38	0.00	1.00
Council Member	3554	0.28	0.45	0.00	1.00
Climate Shock	3554	0.46	0.50	0.00	1.00
Any Shock	3554	0.56	0.50	0.00	1.00
Insecurity	3554	0.01	0.07	0.00	1.00
(log) BRD	3554	0.07	0.46	0.00	4.04
IED	3554	0.08	0.39	0.00	4.00
Direct Fire	3554	0.46	3.29	0.00	40.00
Indirect Fire	3554	0.38	1.89	0.00	18.00
ISAF Mandate	3554	0.32	0.46	0.00	1.00
PRT	3554	0.22	0.42	0.00	1.00
Base	3554	0.21	0.41	0.00	1.00

Notes: Sample based on Table 4.5, column 1. For the definition of the variables see Appendix 4.C.

TABLE 4.10

Descriptives, all variables, 2005, Bandwidth 50 km

	Observations	Mean	Stand. Dev.	Min	Max
Aid (WB)	3554	0.73	0.63	0.03	2.45
Aid (AFG)	3554	11.37	20.35	0.36	85.36
VHI	3554	125.71	19.02	76.90	164.05
(log) Nightlight	3554	-5.38	2.57	-6.91	1.46
Loan	3554	0.46	0.50	0.00	1.00
Remittances	3554	0.12	0.33	0.00	1.00
Agricult. Income	3418	0.76	0.43	0.00	1.00
Any CDC	3554	0.57	0.49	0.00	1.00
Any Shura	3554	0.51	0.50	0.00	1.00
CDC Member	3554	0.13	0.34	0.00	1.00
Shura Member	3554	0.16	0.37	0.00	1.00
Age (hh head)	3190	44.73	13.13	0.00	99.00
Sex (hh head)	3240	0.01	0.10	0.00	1.00
HH Members	3539	7.36	2.71	1.00	22.00
HH Children	3190	7.34	2.62	1.00	22.00
Employed by Gov.	3418	0.09	0.29	0.00	1.00
Employed by Military	3554	0.01	0.12	0.00	1.00
Employed by State/NGO	3554	0.00	0.06	0.00	1.00
Cash for Work	3526	0.05	0.23	0.00	1.00
Food for Work	3457	0.04	0.18	0.00	1.00
Pashtuns	3554	0.42	0.49	0.00	1.00
No. Ethnic Groups	3554	2.23	0.98	1.00	4.00
Native Language: Dari	3329	0.63	0.40	0.00	1.00
Native Language: Pashto	3329	0.14	0.26	0.00	1.00
Native Language: Uzbeki	3329	0.15	0.30	0.00	1.00
Economic Improve	3488	2.76	0.90	1.00	5.00
Wheat Consumption	3554	23.18	12.78	0.00	99.00
Food expenditure	3554	1316.51	812.79	0.00	9729.89
Dietary Diversity	3533	6.43	1.54	1.00	8.00
Food Insecurity	3488	0.23	0.42	0.00	1.00
Sum Assets	3554	1.28	1.04	0.00	8.00
Ruggedness	3554	414.37	211.67	17.21	855.89
Wheat Suitability	3554	0.43	0.23	0.01	0.87
Opium Revenue	3554	469.05	899.41	0.00	3361.81
Opium Eradication	3554	0.06	0.23	0.00	1.00
Travel Time	3554	549.11	366.70	88.52	1965.92
Share Rural	3554	0.98	0.09	0.57	1.00

Notes: Sample based on Table 4.5, column 1. For the definition of the variables see Appendix 4.C.

TABLE 4.11

Descriptives, Survey of the Afghan People, 2007-2014

	Observations	Mean	Stand. Dev.	Min	Max
Confidence	56664	0.70	0.46	0.00	1.00
Trust	47628	0.80	0.40	0.00	1.00
(log) BRD	56664	0.74	3.77	-4.61	8.20
Base	56664	0.34	0.47	0.00	1.00
Aid (WB)	49678	1.24	3.07	0.00	23.26
Aid (AFG)	56664	6.25	18.08	0.00	134.90
VHI	56664	129.69	22.23	61.30	191.99
(log) Nightlight	47644	-3.09	3.84	-6.91	4.06

Notes: Sample based on Table 4.2, column 1. For the definition of the variables see Appendix 4.C.

4.E. Additional results

Panel regressions

TABLE 4.12

Panel results, NRVA, 2005-2008 and 2005-2012

	2005-2008			2005-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Enlargement						
ISAF	-0.145***	-0.121***	-0.108***	-0.119***	-0.095***	-0.095***
	(0.036)	(0.036)	(0.035)	(0.032)	(0.032)	(0.031)
Contestation (t-1)		-0.022**	-0.020**		-0.024***	-0.021***
		(0.010)	(0.009)		(0.008)	(0.007)
Adj. R-squared	0.291	0.300	0.306	0.285	0.294	0.303
Panel B: PRT						
ISAF	-0.116***	-0.111***	-0.060**	-0.099***	-0.095***	-0.049*
	(0.035)	(0.034)	(0.028)	(0.033)	(0.032)	(0.026)
Contestation (t-1)		-0.026**	-0.026***		-0.025***	-0.024***
		(0.012)	(0.010)		(0.009)	(0.008)
Adj. R-squared	0.289	0.300	0.304	0.283	0.294	0.302
Panel C: Military Bases						
ISAF	-0.070*	-0.063	-0.049*	-0.039	-0.038	-0.013
	(0.040)	(0.041)	(0.027)	(0.035)	(0.035)	(0.028)
Contestation(t-1)		-0.036***	-0.030***		-0.031***	-0.027***
		(0.011)	(0.009)		(0.009)	(0.007)
Adj. R-squared	0.290	0.302	0.310	0.285	0.296	0.307
Observations	51260	46116	41289	56995	51851	46781
Control variables	No	No	Yes	No	No	Yes
District, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Restricted Sample	No	Yes	No	No	Yes	No

Notes: The dependent variable is community cohesion measured by Community Help. The set of control variables includes aid (t-1), VHI (t-1), nightlight (t-1), hh shock, food insecurity, agricultural income, remittances, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.13

Panel results, Survey of the Afghan People, 2007-2014

	(1)	(2)	(3)
Panel A: Confidence in Council			
ISAF	-0.061	-0.015	-0.017
	(0.047)	(0.068)	(0.061)
Contestation	0.001	0.002	-0.001
	(0.002)	(0.002)	(0.003)
Observations	56676	56664	28940
Adj. R-squared	0.046	0.069	0.079
Panel B: Trust in Council			
ISAF	-0.097***	-0.026	-0.106
	(0.024)	(0.039)	(0.066)
Contestation	0.001	0.001	-0.003
	(0.002)	(0.002)	(0.003)
Observations	48998	48997	29427
Adj. R-squared	0.063	0.092	0.097
Control variables	No	Yes	Yes
District,Year FE	No	Yes	Yes
Province*Year FE	No	Yes	Yes
Restricted sample	No	No	Yes

Notes: The dependent variable is indicated in the panel heading. The set of control variables includes aid (t-1), VHI (t-1) and nightlight(t-1). Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.14

Panel results, PRT using province FE, 2005-2008 and 2005-2012

	2005-2008		2005-2012	
	(1)	(2)	(3)	(4)
ISAF	-0.029**	-0.029***	-0.030***	-0.030***
	(0.011)	(0.005)	(0.011)	(0.005)
Contestation (t-1)	-0.017***	-0.017***	-0.017***	-0.017***
	(0.005)	(0.002)	(0.005)	(0.002)
Observations	50123	50123	55865	55865
Adj. R-squared	0.280	0.280	0.275	0.275
Control variables	Yes	Yes	Yes	Yes
Province,Year FE	Yes	Yes	Yes	Yes
SE cluster	District	Robust	District	Robust

Notes: The dependent variable is community cohesion measured by Community Help. ISAF presence is defined according to the presence of a PRT in district *i* or its neighboring districts. All regressions include province- and year-fixed effects. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level in columns 1 and 3 and robust standard errors in column 2 and 4.). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.15

Panel results, alternative conflict measures, 2005-2008 and 2005-2012

	2005-2008			2005-2012		
	Mandate	**PRT**	**Base**	**Mandate**	**PRT**	**Base**
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IED Explosion						
ISAF	-0.086**	-0.066***	-0.053*	-0.065**	-0.058**	-0.007
	(0.034)	(0.026)	(0.029)	(0.032)	(0.026)	(0.032)
Contestation (t-1)	-0.058***	-0.059***	-0.065***	-0.040***	-0.040***	-0.044***
	(0.015)	(0.015)	(0.015)	(0.013)	(0.012)	(0.012)
Adj. R-squared	0.304	0.304	0.303	0.295	0.295	0.294
Panel B: Direct Fire						
ISAF	-0.098***	-0.083***	-0.061**	-0.069**	-0.065**	-0.026
	(0.034)	(0.026)	(0.029)	(0.031)	(0.026)	(0.032)
Contestation (t-1)	-0.015	-0.020*	-0.024**	-0.026***	-0.027***	-0.029***
	(0.011)	(0.012)	(0.011)	(0.009)	(0.009)	(0.009)
Adj. R-squared	0.301	0.301	0.299	0.294	0.294	0.293
Panel C: Indirect Fire						
ISAF	-0.100***	-0.081***	-0.041	-0.071**	-0.063**	-0.010
	(0.034)	(0.026)	(0.026)	(0.032)	(0.025)	(0.029)
Contestation (t-1)	-0.015	-0.017	-0.021*	-0.021**	-0.021**	-0.025***
	(0.011)	(0.011)	(0.012)	(0.010)	(0.009)	(0.010)
Adj. R-squared	0.301	0.301	0.299	0.293	0.293	0.292
Observations	50123	50123	50123	55865	55865	55865
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
District, Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Conflict measures are derived from SIGACTS. The dependent variable is community cohesion measured by Community Help. ISAF presence is defined according to the column heading. All regressions include district- and year-fixed effects. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Heterogeneous effects given an exogenous shock

TABLE 4.16

Triple interaction, different conflict measures, 2005

	log BRD (1)	IED Attacks (2)	Fire Direct (3)	Fire Indirect (4)
Panel A: No Control variables				
Shock (t-1)	0.073***	0.081***	0.051***	0.069***
	(0.015)	(0.016)	(0.018)	(0.015)
ISAF	-0.013**	-0.018***	-0.018**	-0.016***
	(0.005)	(0.006)	(0.007)	(0.005)
Shock*ISAF	0.009	0.005	0.031	0.014
	(0.019)	(0.020)	(0.022)	(0.019)
Contestation	-0.004	-0.009***	-0.007*	-0.006***
	(0.003)	(0.003)	(0.004)	(0.002)
Shock*Contestation	0.034*	0.024	0.050**	0.040
	(0.020)	(0.028)	(0.021)	(0.028)
ISAF*Contestation	-0.001	0.010*	0.006	0.004
	(0.003)	(0.006)	(0.005)	(0.005)
Shock*ISAF*Contestation	-0.095***	-0.042	-0.050	-0.159***
	(0.022)	(0.038)	(0.038)	(0.035)
Observations	30916	30916	30916	30916
Adj. R-squared	0.048	0.045	0.051	0.049
Panel B: Control variables				
Shock (t-1)	0.070***	0.080***	0.051***	0.070***
	(0.015)	(0.016)	(0.018)	(0.016)
ISAF	-0.009	-0.015**	-0.012	-0.008
	(0.006)	(0.006)	(0.007)	(0.007)
Shock*ISAF	0.011	0.008	0.030	0.012
	(0.020)	(0.021)	(0.023)	(0.020)
Contestation	0.005	-0.001	0.002	0.011
	(0.004)	(0.007)	(0.005)	(0.013)
Shock*Contestation	0.031	0.020	0.046**	0.031
	(0.019)	(0.027)	(0.021)	(0.026)
ISAF*Contestation	-0.011	-0.000	-0.003	-0.016
	(0.007)	(0.008)	(0.007)	(0.016)
Shock*ISAF*Contestation	-0.092***	-0.043	-0.047	-0.155***
	(0.022)	(0.039)	(0.037)	(0.035)
Observations	29785	29785	29785	29785
Adj. R-squared	0.053	0.048	0.055	0.054

Notes: The dependent variable is community cohesion measured by Community Help. Panel B includes as control variables aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

FIGURE 4.5

Triple interaction, Community Help+Loan, 2005
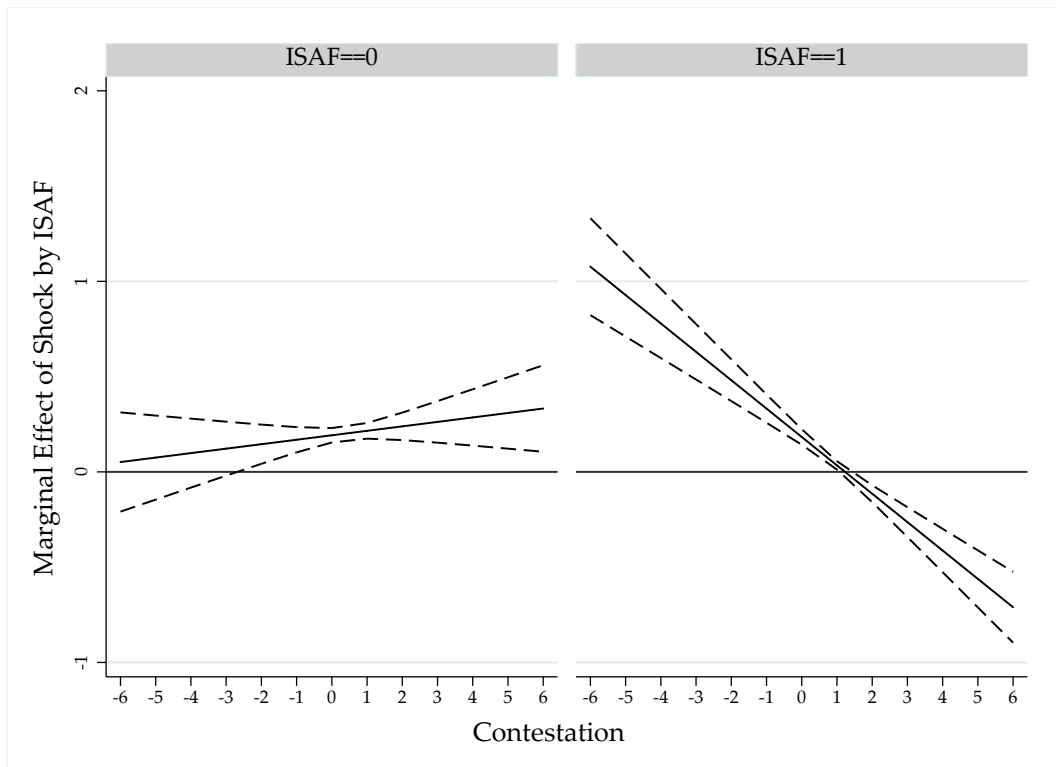
Notes: The two graphs show the marginal effect of the shock as contestation changes for the two types of districts (ISAF versus no ISAF). Contestation is measured by the logarithm of battle-related deaths. Income shock is the indicator variable of whether a household has been exposed to a negative income shock measured by climatic shocks. Marginal effects are plotted along with 90% confidence intervals.

Regressions discontinuity

TABLE 4.17

Regression discontinuity, balancing tests at district level

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
	log	Fire		IED	VHI	Nighlight
	BRD	Direct	Indirect	Attack		
ISAF treat	0.194	-0.002	0.202	0.126	-2.236	0.665
	(0.226)	(0.014)	(0.208)	(0.128)	(6.830)	(0.765)
Adj. R-squared	0.098	-0.029	-0.056	-0.061	0.142	0.013
			Panel B			
	Aid		Military	Wheat	Popu-	Share
	WB	Total	Bases	Suit.	ation	Rural
ISAF treat	0.465	-0.025	0.576	0.109	27.772	-0.016
	(1.157)	(0.133)	(0.582)	(0.130)	(64.370)	(0.016)
Adj. R-squared	0.059	-0.073	-0.061	0.208	0.169	-0.083
			Panel C			
	Rugged-	Opium	Travel	Territory	Pashtuns	No. Ethnic
	ness	Revenue	Time	Control		Groups
ISAF treat	-217.095*	859.860	26.495	-0.264	0.326	0.433
	(122.084)	(558.612)	(173.864)	(0.379)	(0.203)	(0.534)
Adj. R-squared	0.516	0.164	0.285	0.710	0.261	0.169
Observations	51	51	51	51	51	51
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is indicated in the column heading of each panel. Regressions are at the district level. Robust standard errors are in parentheses. Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.18

Regression discontinuity, placebo tests, Community Help, 2003

	50 km Bandwidth	75 km Bandwidth	100 km Bandwidth
	(1)	(2)	(3)
Panel A: Linear polynomial in distance to boundary			
ISAF treat	-0.055	-0.046	-0.029
	(0.037)	(0.037)	(0.029)
Adj. R-squared	0.021	0.008	0.009
Panel B: Linear polynomial in longitude and latitude			
ISAF treat	-0.043	-0.033	-0.023
	(0.029)	(0.028)	(0.027)
Adj. R-squared	0.020	0.008	0.010
Observations	1630	2471	3483
Number of clusters	51	86	114
200km segments	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes

Notes: The dependent variable is community cohesion measured by Community Help. All regressions include as control variables hh shock and segment-fixed effects. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.19

Regression discontinuity, Community Help+Loan, 2005

	(1)	(2)	(3)	(4)	(5)	(6)
	Bandwidth 50		Bandwidth 75		Bandwidth 100	
Panel A: Linear polynomial in distance to boundary						
ISAF treat	-0.244***	-0.281***	-0.217***	-0.223***	-0.162***	-0.188***
	(0.059)	(0.066)	(0.054)	(0.056)	(0.053)	(0.053)
Adj. R-squared	0.197	0.220	0.169	0.172	0.157	0.153
Panel B: Linear polynomial in longitude and latitude						
ISAF treat	-0.153***	-0.174***	-0.158***	-0.162***	-0.143***	-0.163***
	(0.041)	(0.046)	(0.038)	(0.041)	(0.040)	(0.042)
Adj. R-squared	0.193	0.213	0.169	0.171	0.160	0.154
Panel C: Quadratic polynomial in distance to boundary						
ISAF treat	-0.234***	-0.273***	-0.209***	-0.218***	-0.157***	-0.187***
	(0.060)	(0.067)	(0.056)	(0.058)	(0.052)	(0.054)
Adj. R-squared	0.198	0.221	0.170	0.173	0.158	0.154
Panel D: Quadratic polynomial in longitude and latitude						
ISAF treat	-0.213***	-0.228***	-0.175***	-0.184***	-0.128***	-0.159***
	(0.059)	(0.064)	(0.050)	(0.050)	(0.049)	(0.050)
Adj. R-squared	0.198	0.217	0.172	0.173	0.165	0.157
Observations	3554	3148	7495	5882	11810	8426
Number of clusters	74	64	120	103	166	144
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is Community Help+Loan. 200 km segment-fixed effects are included. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Table 4.20

Regression discontinuity, CDC/Shura Member, 2005

	(1)	(2)	(3)	(4)	(5)	(6)
	Bandwidth 50		Bandwidth 75		Bandwidth 100	

Panel A: Linear polynomial in distance to boundary

ISAF treat	-0.086	-0.121	-0.179**	-0.176**	-0.143*	-0.161*
	(0.083)	(0.087)	(0.070)	(0.075)	(0.084)	(0.093)
Adj. R-squared	0.191	0.220	0.110	0.133	0.066	0.075

Panel B: Linear polynomial in longitude and latitude

ISAF treat	-0.085	-0.117*	-0.123**	-0.127**	-0.093	-0.108
	(0.061)	(0.063)	(0.052)	(0.057)	(0.064)	(0.069)
Adj. R-squared	0.191	0.221	0.110	0.135	0.066	0.074

Panel C: Quadratic polynomial in distance to boundary

ISAF treat	-0.072	-0.114	-0.138*	-0.152*	-0.141	-0.161*
	(0.079)	(0.084)	(0.073)	(0.077)	(0.087)	(0.094)
Adj. R-squared	0.192	0.221	0.119	0.140	0.066	0.075

Panel D: Quadratic polynomial in longitude and latitude

ISAF treat	-0.135	-0.180*	-0.183***	-0.185**	-0.135*	-0.145
	(0.087)	(0.092)	(0.068)	(0.072)	(0.079)	(0.087)
Adj. R-squared	0.193	0.224	0.114	0.141	0.068	0.077
Observations	3554	3148	7495	5882	11810	8426
Number of clusters	74	64	120	103	166	144
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is CDC/Shura Member. 200 km segment-fixed effects are included. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. I additionally control for the presence of a council. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

Table 4.21

Regression discontinuity, alternative specifications, 2005

	Segment FE		Covariates	
	No	**12 a 100 km**	**No**	**Long Set**
	(1)	(2)	(3)	(4)
Panel A: Linear polynomial in distance to boundary				
ISAF treat	-0.108**	-0.119**	-0.109**	-0.107**
	(0.050)	(0.054)	(0.052)	(0.047)
Adj. R-squared	0.091	0.104	0.065	0.110
Observations	3148	3148	3148	2446
Number of clusters	64	64	64	64
Panel B: Linear polynomial in longitude and latitude				
ISAF treat	-0.065**	-0.076**	-0.061**	-0.102***
	(0.029)	(0.033)	(0.024)	(0.033)
Adj. R-squared	0.089	0.102	0.063	0.115
Observations	3148	3148	3148	2446
Number of clusters	64	64	64	64
Panel C: Direct neighbors				
ISAF treat	-0.089***	-0.114***	-0.081*	-0.131***
	(0.031)	(0.030)	(0.039)	(0.047)
Adj. R-squared	0.117	0.180	0.114	0.166
Observations	1986	1986	1986	1599
Number of clusters	28	28	28	28
Panel D: Distance to boundary interaction with treatment				
ISAF treat	-0.109**	-0.118**	-0.107**	-0.110**
	(0.049)	(0.053)	(0.051)	(0.049)
Adj. R-squared	0.090	0.104	0.065	0.110
Observations	3148	3148	3148	2446
Number of clusters	64	64	64	64
200km segments	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes

Notes: The dependent variable is community cohesion measured by Community Help. The long set of control variables includes aid(t-1), VHI(t-1), nightlight(t-1), military bases(t-1), presence of a CDC, distance to Kabul, hh shock, hh head age, hh head sex, hh members, hh number of children, food insecurity, agricultural income, remittances, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.22

Regression discontinuity, control for Contestation, 2005

	(1)	(2)	(3)	(4)	(5)	(6)
	Insecurity		UCDP	Fire		IED
	HH	District	log BRD	Direct	Indirect	Attack

Panel A: Linear polynomial in distance to boundary
Control for Contestation

ISAF treat	-0.121**	-0.123**	-0.122**	-0.123**	-0.121**	-0.138***
	(0.052)	(0.051)	(0.052)	(0.056)	(0.052)	(0.051)
Contestation (t-1)	-0.032	0.205	0.006	0.006	0.001	0.075***
	(0.093)	(0.291)	(0.023)	(0.039)	(0.025)	(0.014)
Adj. R-squared	0.095	0.096	0.095	0.095	0.095	0.110

Panel B: Linear polynomial in distance to boundary
Control for Contestation and Interaction with treatment

ISAF treat	-0.122**	-0.127**	-0.123**	-0.130**	-0.121**	-0.139***
	(0.052)	(0.053)	(0.052)	(0.053)	(0.051)	(0.051)
Contestation (t-1)	-0.025	0.188	0.021	-0.060	-0.008	0.079***
	(0.114)	(0.303)	(0.040)	(0.079)	(0.035)	(0.014)
ISAF*Contestation	-0.038	1.015	-0.025	0.093	0.039	-0.047
	(0.115)	(1.338)	(0.052)	(0.090)	(0.057)	(0.051)
Adj. R-squared	0.094	0.096	0.095	0.096	0.095	0.110

Panel B: Linear polynomial in longitude and latitude
Control for Contestation

ISAF treat	-0.081***	-0.081***	-0.080***	-0.080**	-0.081***	-0.099***
	(0.028)	(0.028)	(0.028)	(0.034)	(0.028)	(0.027)
Contestation (t-1)	-0.030	0.185	0.004	-0.001	0.002	0.076***
	(0.094)	(0.294)	(0.025)	(0.044)	(0.025)	(0.014)
Adj. R-squared	0.093	0.093	0.092	0.092	0.092	0.108

Panel B: Linear polynomial in longitude and latitude
Control for Contestation and Interaction with treatment

ISAF treat	-0.081***	-0.084***	-0.079***	-0.091***	-0.092**	-0.098***
	(0.028)	(0.029)	(0.029)	(0.032)	(0.036)	(0.028)
Contestation (t-1)	-0.025	0.169	0.009	-0.090	-0.011	0.079***
	(0.115)	(0.307)	(0.044)	(0.092)	(0.038)	(0.013)
ISAF*Contestation	-0.027	0.928	-0.009	0.131	0.055	-0.029
	(0.115)	(1.339)	(0.055)	(0.119)	(0.075)	(0.059)
Adj. R-squared	0.092	0.093	0.092	0.095	0.094	0.108
Observations	3148	3148	3148	3148	3148	3148
Number of clusters	64	64	64	64	64	64
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is community cohesion measured by Community Help. The set of control variables includes aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. 200 km segments included in all regressions. Contestation is measured as indicated in the column heading. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.23

Regression discontinuity, different ways of clustering SE, 2005

|	(1)	(2)	(3)
SE Cluster:	District	Village	Robust
Panel A: Linear polynomial in distance to boundary			
ISAF treat	-0.121**	-0.121***	-0.121***
	(0.052)	(0.039)	(0.020)
Panel B: Linear polynomial in longitude and latitude			
ISAF treat	-0.080***	-0.080***	-0.080***
	(0.028)	(0.022)	(0.013)
Observations	3148	3148	3148
Adj. R-squared	0.093	0.093	0.093
200km segments	Yes	Yes	Yes
Control variables	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes

Notes: The dependent variable is community cohesion measured by Community Help. The set of control variables includes aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.24

Regression discontinuity, no household weights, 2005

	(1)	(2)	(3)	(4)	(5)	(6)
	Community Help		Community Help+Loan		Council Member	
ISAF treat	-0.093**	-0.121**	-0.244***	-0.281***	-0.093	-0.154
	(0.045)	(0.052)	(0.059)	(0.066)	(0.092)	(0.095)
Adj. R-squared	0.079	0.095	0.197	0.220	0.158	0.190
Observations	3554	3148	3554	3148	3554	3148
Number of clusters	74	64	74	64	74	64
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is indicated in the column heading. 200 km segment-fixed effects are included. All regressions are including households within the 50 km bandwidth. The set of control variables include aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

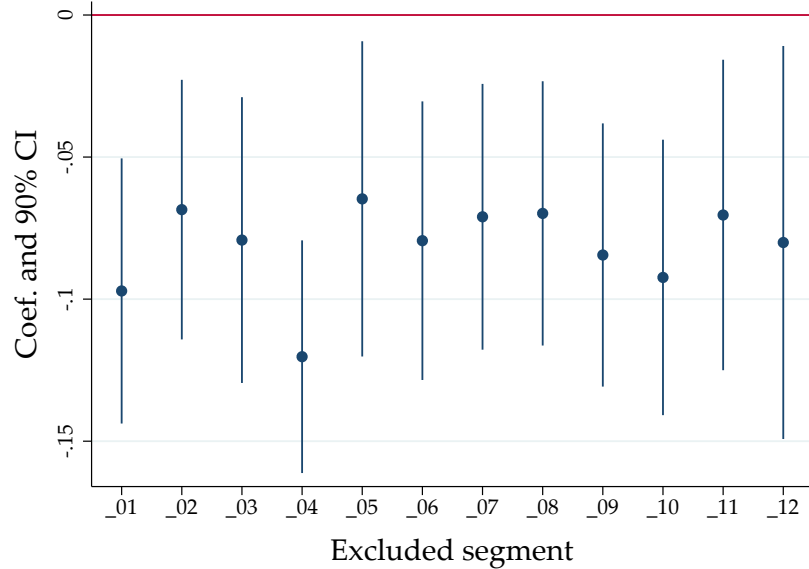FIGURE 4.6
Wild-cluster bootstrap
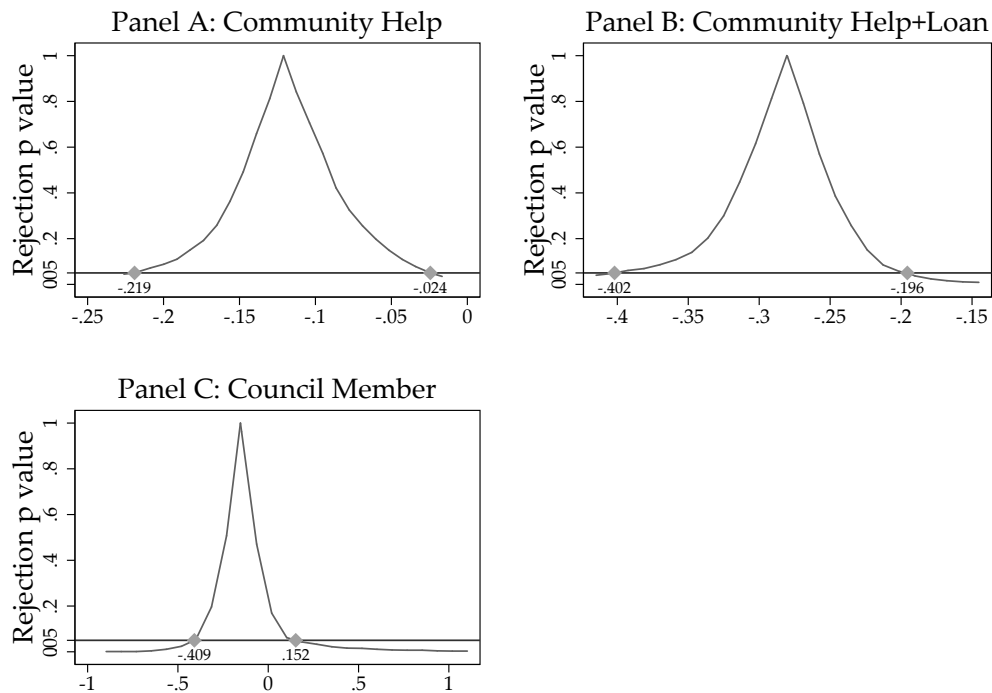
Notes: All panels show the distribution of bootstrapped estimates for province level clustered standard errors with the null imposed with 1'000 replications. The panel heading indicates the dependent variable. Results are shown for the most rigorous specification, including 200 km segment-fixed effects and the set of control variables for the restricted sample as in column 4 of Table 4.5. The numbers indicate the left and right 95% confidence interval. The Null hypothesis at the 5% level is whether this interval contains 0.

TABLE 4.25
Regression discontinuity, new boundary, 2005

	(1)	(2)	(3)	(4)	(5)	(6)
ISAF treat	-0.094**	-0.114**	-0.121**	-0.049	-0.064*	-0.080***
	(0.047)	(0.054)	(0.052)	(0.030)	(0.035)	(0.028)
Observations	3555	3208	3148	3555	3208	3148
Adj. R-squared	0.084	0.096	0.095	0.082	0.094	0.093
200km segments	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	No	Yes	Yes	No	Yes	Yes
Exclude 2 districts	No	No	Yes	No	No	Yes
Border	New	New	Old	New	New	Old

Notes: The dependent variable is community cohesion measured by Community Help. The set of control variables includes aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Column 1, 2, 4, 5 apply the new boundary after administrative reorganization. Column 3 and 6 apply the old boundary but exclude 2 districts (Kahmard and Sayghan), which have been shifted across the border after the change of administrative units in 2005. For more details on the administrative reorganization see Appendix 4.A. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

FIGURE 4.7

Regression discontinuity, drop a boundary segment at the time, 2005

Notes: The figure plots coefficient estimates of the treatment variable for 12 separate regressions. Regressions are as in Table 4.5, panel B, column 2.

TABLE 4.26

Regression discontinuity, exclude western/eastern command, 2005

|	(1)	(2)	(3)	(4)
	No western Command		No Eastern Command	
ISAF treat	-0.174**	-0.137*	-0.061	-0.065***
	(0.077)	(0.069)	(0.039)	(0.022)
Observations	2785	2785	1483	1483
Adj. R-squared	0.108	0.106	0.053	0.054
200km segments	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes
GRD type	Linear	Long & Lat	Linear	Long & Lat

Notes: The dependent variable is community cohesion measured by Community Help. The set of control variables includes aid(t-1), VHI(t-1), nightlight(t-1), hh shock, loan. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

TABLE 4.27

Regression discontinuity, potential mechanisms, 2005

	(1)	(2)	(3)	(4)	(5)
Panel A: Government Employment + Coping					
	Cope State Military	Loan	Gov. Employ.	Agricult. Income	Opium Eradication
ISAF treat	-0.022	-0.146	-0.064	0.218	-0.007
	(0.016)	(0.137)	(0.045)	(0.141)	(0.032)
Observations	3262	3148	3084	3084	3262
Adj. R-squared	0.064	0.093	0.057	0.035	0.141
Panel B: Living Standards					
	Wheat Consumpt.	Food Expend.	Dietary Diversity	Food Insecurity	Sum of Assets
ISAF treat	-5.103	-297.066	0.261	-0.110	0.079
	(4.358)	(310.410)	(0.600)	(0.213)	(0.272)
Observations	3262	3262	3224	3185	3262
Adj. R-squared	0.036	0.065	0.242	0.075	0.015
Panel C: Aid + Economic Improvement					
	Cash for Work	Any CDC	Aid WB	Nighlight	Economic Improve
ISAF treat	-0.105**	-0.280	-0.307	-0.710	0.144
	(0.046)	(0.212)	(0.421)	(1.035)	(0.176)
Observations	3233	3262	3262	3262	3179
Adj. R-squared	0.025	0.144	0.111	0.451	0.089
Panel D: Conflict + Insecurity					
	HH Insecurity	HH Theft	(log) BRD	Fire Direct	Indirect
ISAF treat	-0.014	0.001	0.204	0.006	0.265
	(0.013)	(0.008)	(0.152)	(0.411)	(0.232)
Observations	3262	3262	3262	3262	3262
Adj. R-squared	0.023	0.000	0.238	0.068	0.077
200km segments	Yes	Yes	Yes	Yes	Yes
Control variables	No	No	No	No	No
Restricted sample	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is indicated in the column heading of each Panel. Standard errors are in parentheses (clustered at the district level). Significance levels: * 0.10 ** 0.05 *** 0.01

FIGURE 4.8

Heterogeneous effects of aid according to ISAF presence
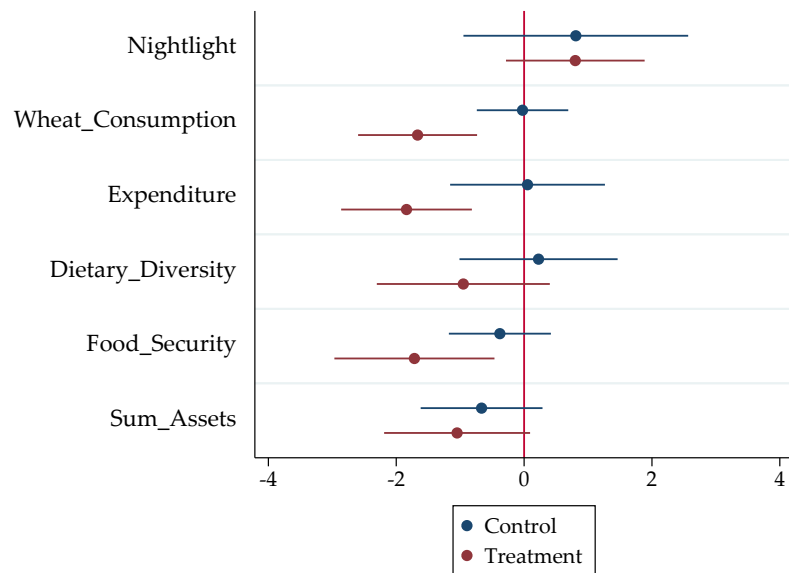
Notes: The figure plots the marginal effects of aid on various outcome variables (as indicated on the y-axis) depending on whether ISAF is present (Treatment, in red) or not (Control, in blue). The effects are measured in in standard deviations. The regressions follow the baseline GRD estimation strategy as presented in Table 4.5, column 2. The outcome is replaced with measures on nightlight and living standards in 2005, and the treatment is interacted with Aid from the WB in (t-1).
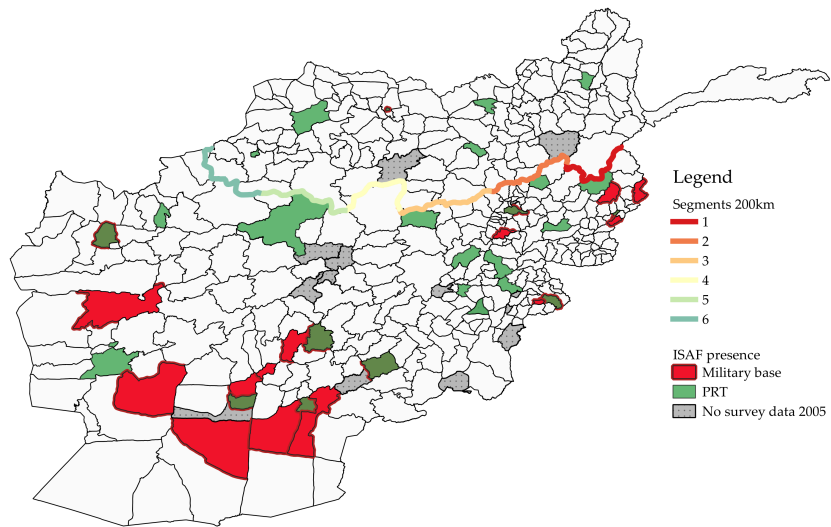
4.F.   Additional maps
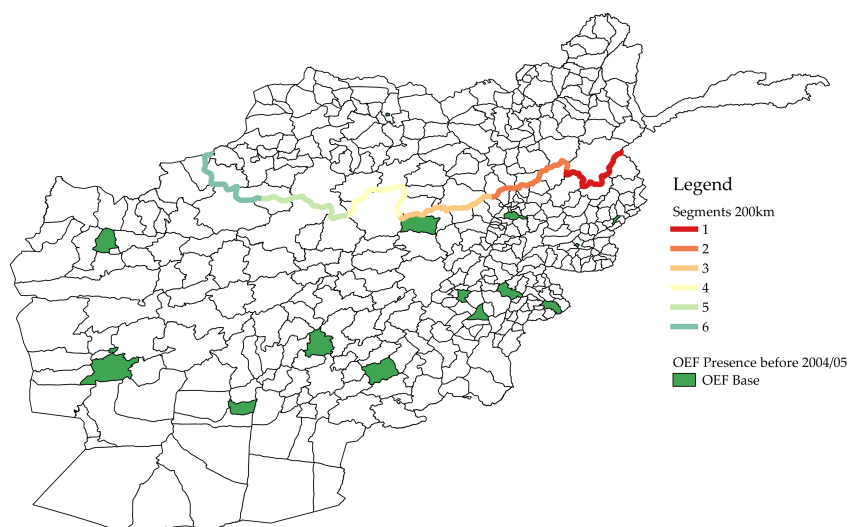
FIGURE 4.9
Regional commands and province names

Notes: The boundary splits the country into the northern command (treated), where ISAF's mandate has been extended to in December 2003 (completed end of 2004) and the rest of the country (control), where ISAF has been deployed to after the survey wave of 2005 has been conducted. I plot the new boundary after the administrative reorganization in 2005 as described in Appendix 4.A. Highlighted are the four regional commands as described in https://www.globalsecurity.org/military/ops/oef_orbat_isaf_091000.htm (accessed June 27, 2018). The shapefile for the 34 provinces is from https://data.humdata.org/dataset/afg-admin-boundaries, accessed June 27, 2018.

FIGURE 4.10
Presence of military bases and PRTs

Notes: The boundary splits the country into the northern command (treated), where ISAF's mandate has been extended to in December 2003 (completed end of 2004) and the rest of the country (control), where ISAF has been deployed to after the survey wave of 2005 has been conducted. Highlighted are the six boundary segments of 200 km, and the districts for which there is no survey data available in the 2005 survey wave. Districts highlighted in red (and surrounded by red) are characterized by a military base (any time within my sample period) and districts marked in green show the location of a PRT. Note that in some districts, as for instance in the district Masar-e Scharif, both a PRT and a military base is present. While the data on PRTs in complete, the data on military bases has to be considered with some caution since it covers not the entire spectrum. For more details on the collection of this data see Gehring et al. (2018).

FIGURE 4.11
OEF bases before 2005

Notes: The map highlights districts with US facilities (including minor facilities) from OEF as of January 1, 2005. Source: https://www.globalsecurity.org, accessed March 25, 2018.

FIGURE 4.12
Battle-related deaths: Mean value 2005-2012
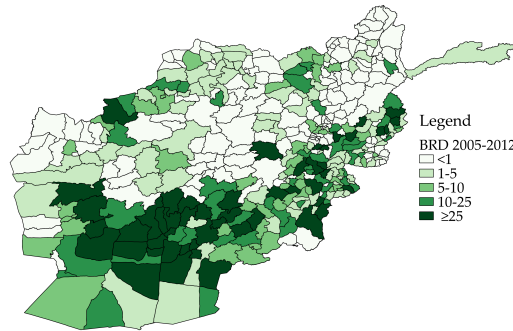
Notes: The figure plots the distribution of the number of battle-related deaths (no logarithms) from UCDP GED in averages per district over the 2005-2012 period.

FIGURE 4.13
Alternative conflict measures: Mean values 2005-2012

(A) Insecurity (NRVA)

(B) IED (SIGACTS)

(C) IDF (SIGACTS)

(D) DF (SIGACTS)
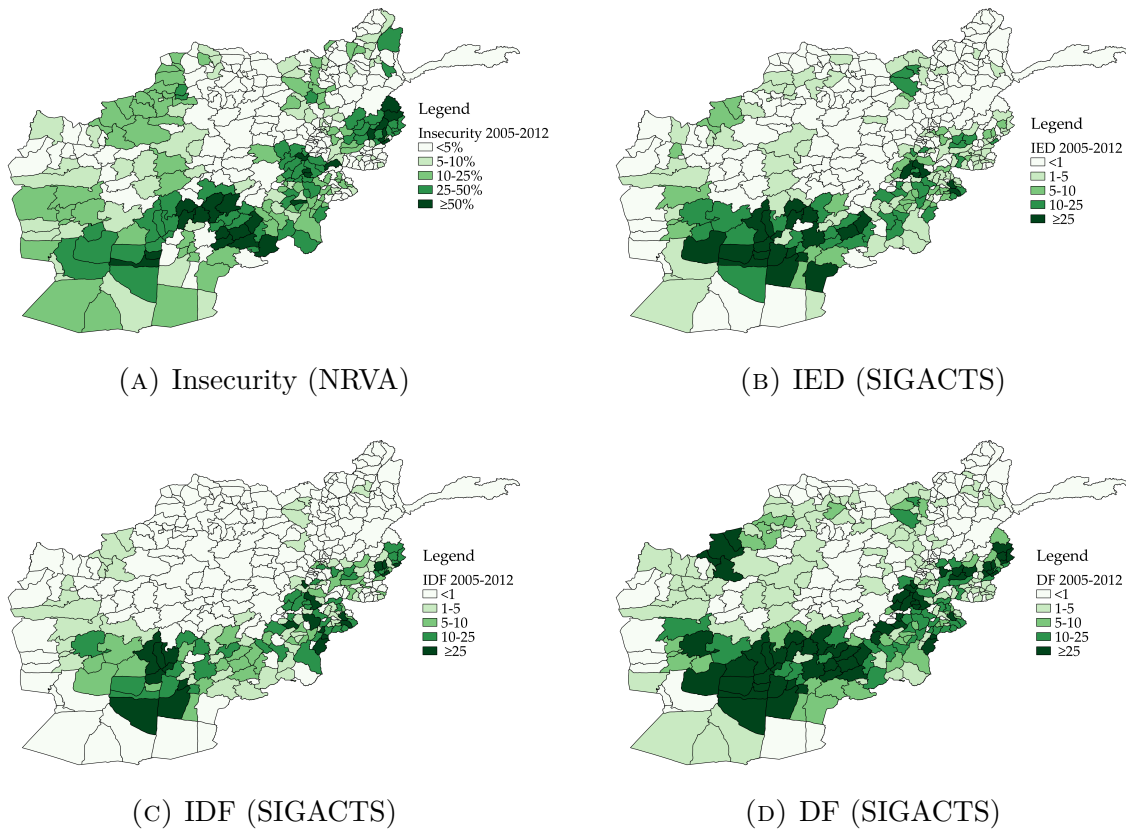
Notes: The figures plot the distribution of alternative conflict measures provided by NRVA (panel A) and SIGACTS (panels B-D). While panel A shows a subjective conflict measure (Insecurity: share of households per district that experienced an insecurity shock), panels B-D cover events tracked by the military and provide numbers (no logarithms) of three types of events: IED, Indirect Fire (ID) and Direct Fire (DF). All values are averages per district over the 2005-2012 period.

FIGURE 4.14

Conflict and missing survey data
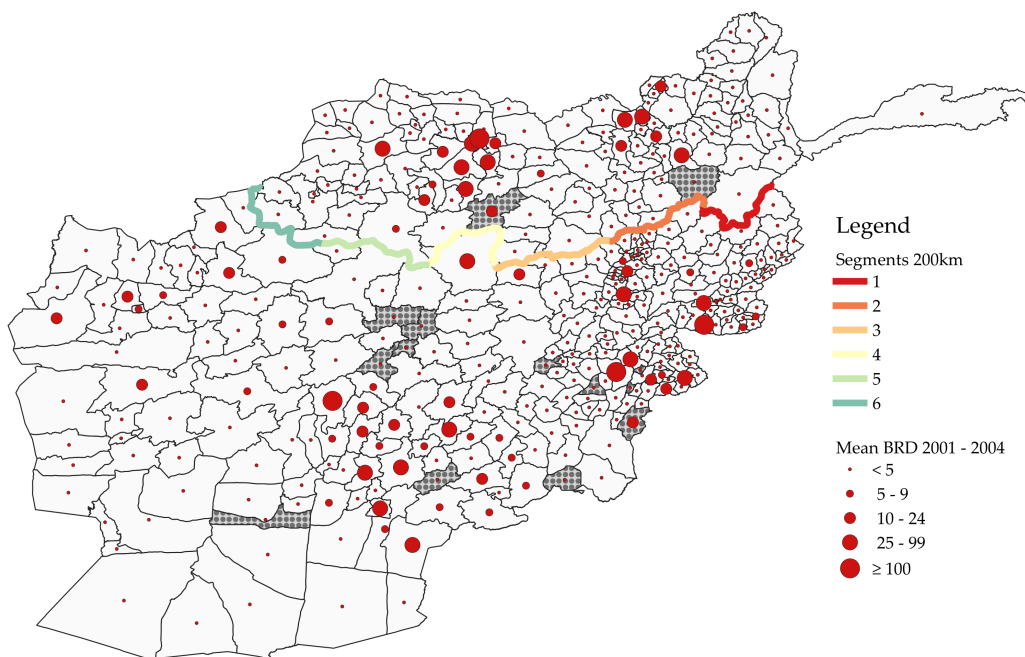
Notes: The boundary splits the country into the northern command (treated), where ISAF's mandate has been extended to in December 2003 (completed end of 2004) and the rest of the country (control), where ISAF has been deployed to after the survey wave of 2005 has been conducted. Highlighted are the six boundary segments a 200 km, and the districts for which there is no survey data available in the 2005 survey wave. Districts in dottet grey are missing in the NRVA dataset. The red dots present the conflict intensity measured by the number of battle-related deaths (BRD). The dots present the mean BRD per district over the four prior years to 2005 (2001-2004).

FIGURE 4.15
Territorial control 1996
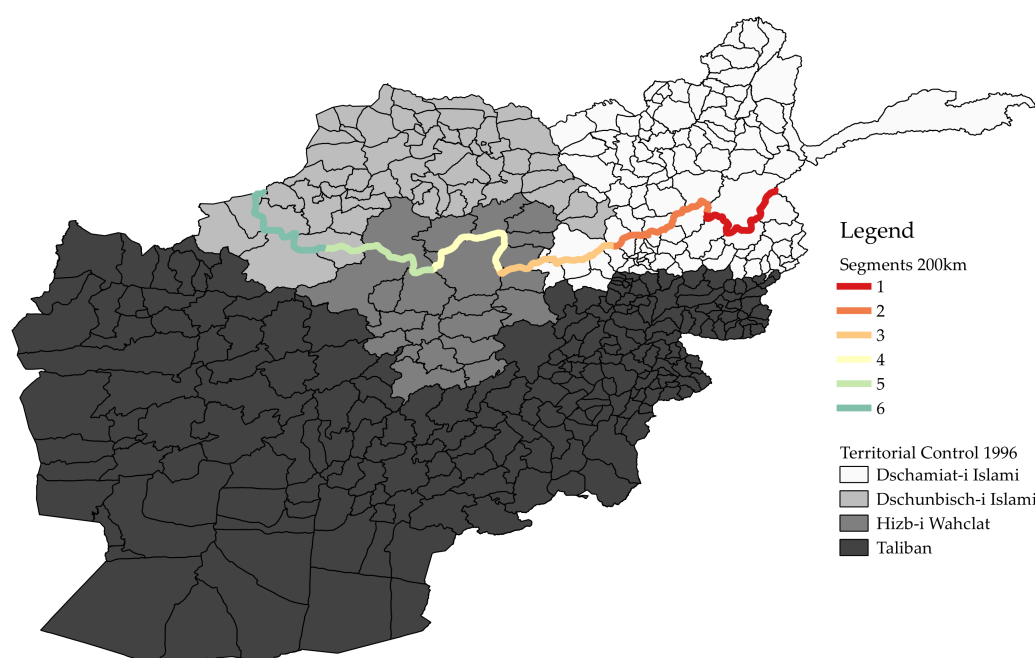
Notes: The source for the classification of the territorial control in 1996 is Dorronsoro (2005).

FIGURE 4.16
Soviet invasion 1979-1989
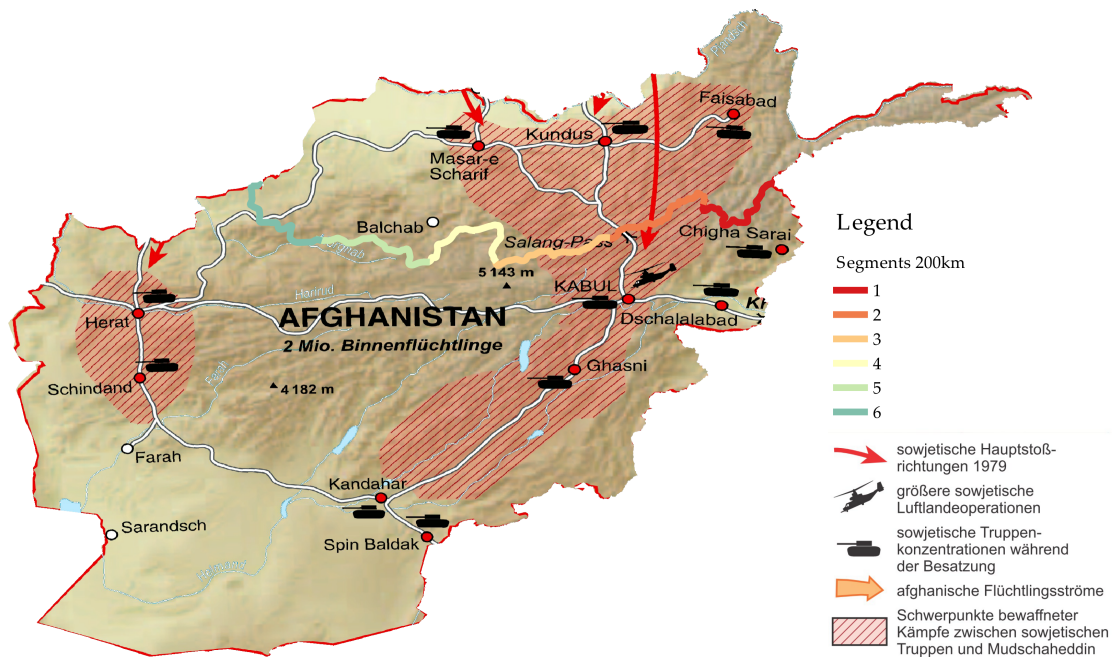
Notes: I georeferenced the map on the soviet invasion from http://www.zmsbw.de/html/
einsatzunterstuetzung/downloads/0592404.pdf (accessed June 26, 2018) and overlaid it with the
shapefile from https://esoc.princeton.edu/country/afghanistan for the 398 districts. This allows for
the inclusion of the treatment boundary in this original map on the soviet invasion. The red arrows
show the main directions of the invasion and the fighting (orange arrows as indicated in the legend
are not on the map as they show the direction of refugee flows out of the country). Helicopters
present bigger airborne landing operations and battle tanks show main troop concentration while
the occupation. The red dashed areas are the focal points of fighting between soviets troops and
the mujaheddin.

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