The Subnational Effect of Aid on Civil Conflict

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Abstract

This thesis examines the effect of development aid on civil conflict at the sub-national level. Using georeferenced data on World Bank project locations, I estimate the effect of development aid on civil conflict events at the state and county levels in a sample of 36 countries. I use the interaction between a country's eligibility for aid from the International Development Association and the fraction of years a state or county received aid before its country becomes ineligible as an instrument for World Bank aid. Two stage least squares results suggest that aid reduces conflict incidence and onset at the county level, but has no effect on either incidence or onset at the state level. This effect is economically large and indicates that receiving aid reduces the probability of conflict incidence by 3.6% at the county level. Additionally, I find that aid has a greater conflict-reducing effect in high-elevation counties and counties close to the national border.

1 Introduction

The relationship between foreign aid and civil conflict has recently emerged as a salient topic in development economics and political economy. Foreign aid is often seen as a key policy tool for promoting development, and represents a significant flow of capital from rich to poor countries. In 2017 alone, Official Development Assistance from member countries of the OECD Development Assistance Committee (DAC) totaled USD 146.6 billion (OECD 2018). At the same time, however, many critics have questioned aid's effectiveness, and some have suggested that in certain cases it may do more harm than good (see e.g., Easterly 2006). Meanwhile, economists have begun to recognize the centrality of conflict — especially civil conflict — to the development process (Blattman and Miguel 2010). As Blattman and Miguel (2010), point out, more than half of all countries have experienced a civil conflict (an internal armed conflict resulting in at least 25 battle deaths per year) since 1960, with effects ranging from disruptive to devastating. Thus aid and conflict are (or should be) critical factors in the way social scientists and policy makers understand development. However, current evidence on the relationship between the two is inconclusive. Does aid ameliorate civil conflict by promoting development, as some have argued (see e.g., Collier and Hoeffler 2002)? Or is it possible that aid exacerbates or incites conflict by increasing incentives to fight? I aim to address these questions by examining the effect of aid on civil conflict events at the subnational level. Leveraging geo-referenced data on aid disbursements and violent events in a sample of 36 countries across several continents, I attempt to identify the subnational effect of aid on conflict.

I utilize an instrumental variables strategy to address potential endogeneity issues. The instrument is the interaction between an indicator for a country's eligibility to receive aid from the International Development Association (one of the two main lending arms of the World Bank) and the fraction of years a region receives aid before its country crosses the IDA eligibility threshold. The instrument has a strong first-stage relationship, and I will argue that it satisfies the exclusion restriction. Two spatial units are used in this analysis. The first is the ADM 1 level, the first administrative subdivision below the country-level, which is analogous to the state level in the United States. The second is the ADM 2 level, which corresponds to the second administrative subdivision below the country, and equivalently, the first subdivision below the ADM 1 level. This is analogous to the county level in the United States. For ease of interpretation, I will refer to the ADM 1 level as the state level and the ADM 2 level as the county level throughout this paper, even though the vast majority of countries in the sample do not use this terminology. At the state level, two-stage least squares point estimates are insignificant. At the county level, however, I find a large and significant negative effect of aid on conflict incidence and onset. These results are economically large and imply that aid receipt reduces the risk of conflict incidence at the county level by 3.6% and of conflict onset of 1.9%.

Although several previous studies have examined the link between aid and conflict, many of these studies investigate the effect of aid on conflict at the country level. This approach offers several advantages, most notably easy access to a variety of data sources. However, studies that take the country as the unit of analysis necessarily overlook many of the spatial and local dimensions of conflict, since all incidents of conflict and aid receipt are aggregated to the country level (Sundberg and Melander 2013). The implicit assumption of this approach is that aid disbursed in any location within a country can affect conflict anywhere else in the country. Such an assumption is problematic if aid has heterogeneous effects within countries, such that different types of aid and different recipient regions result in heteroegeneous effects. However, as discussed below, the existing literature on aid and conflict suggests a significant degree of heterogeneity in the effect of aid on conflict. Consequently, the average effect estimated by country-level studies may be merely the average of heterogeneous effects, and thus fail to identify the true "on the ground" impact. Moreover, the conflict-inciting or -reducing effect of aid on conflict may be so small and imprecisely identified that it is washed away at the country level. On the other hand, several other studies have taken a case study approach (see e.g., Crost et al. 2014). These studies examine the effect of a single development program or set of programs on one country or region. An important advantage of these studies is that their results can be situated within the institutional, geographical, and historical context they study. This allows for clearer interpretations of the causal mechanisms underlying the results, and elucidation of mechanisms that might not be predicted by standard theoretical frameworks. Moreover, by studying aid and conflict events within countries, they are able to examine within-country heterogeneity of the effect of aid on conflict. However, case studies must, to some extent, sacrifice external validity. This is especially true in the aid-conflict literature, as the types of aid and the way projects are implemented vary widely, as do the nature and behavior of conflicts.

I argue that my approach integrates the best of both approaches. By moving the unit of analysis to the subnational level, I can improve identification of local or regional effects that may not be visible at the country level. Such an approach isolates the effect of aid disbursed in a region on conflict in the same region. Consequently, I can identify the *local* effect of aid on conflict. Additionally, I can examine heterogeneity of the effect of aid on conflict within countries. Moreover, this approach avoids some of the external validity concerns associated with case studies that examine one country. The sample includes 36 countries across Asia, Europe, Latin America, the Middle East and North Africa, and Sub-Saharan Africa. Finally, since the spatial units of analysis are subnational, my analysis is able to exploit variation in aid between regions in the same country or state. Since my instrumental variables approach resembles a difference-in-differences approach in the first stage, the parallel trends assumption is important, as I will discuss in greater detail below. My approach offers an advantage in this regard, since regions within the same country are more likely to be better counter-factuals than different countries are.

In order to conduct a subnational analysis of the relationship between aid and conflict, I construct a dataset of conflict events and aid disbursements at the subnational administrative unit level. The main independent variable of interest is World Bank aid from the International Development Association (IDA) and the International Bank of Reconstruction and Development (IBRD). The IDA and IBRD are the two arms of the World Bank, and together finance its development projects and lending. The IBRD provides loans and guarantees to middle-income countries to promote development. The IDA provides concessional loans and grants to fund development projects in low-income and lower-middle-income countries. Development aid from the World Bank forms a non-negligible portion of global aid flows. For example, total aid commitments from the IDA and IBRD totaled more than $42 billion in 2017 (World Bank 2017). In the same year, total global disbursements of official development assistance totaled $163 billion (World Bank 2019). The AidData dataset covers the universe of IDA and IBRD projects approved between 1995 and 2014 (AidData 2017). 97% of projects are georeferenced, and every georeferenced project can be linked to a specific county region. I match project locations with disbursement data from the World Bank website to create a dataset of regional aid flows. The main dependent variable of interest is conflict events from the UCDP Georeferenced Event Dataset (UCDP GED) Global version 18.1 (Croicu and Sundberg 2017). UCDP GED attempts to cover all conflict events around the world between 1987 and 2017 that result in at least one fatality. All events in the UCDP GED are georeferenced, and 81% can be aggregated to the county level.

The present study is not without potential concerns. Just as the country may not be an appropriate unit of analysis for civil conflict, so may the region fail to conform to the actual spatial dimensions of conflict. Spatial spillovers of conflict and aid, especially across regions within countries, are almost certain. As a result, since this approach limits the effect of aid to only the region in which it is received, it may underestimate the true causal effect, especially at the finer county level. Additionally, as with any instrumental variables approach, concerns with the exclusion restriction are important. I argue below that the inclusion of controls, such as nighttime lights to capture income growth, and country-year fixed-effects ameliorate many of these concerns. I discuss these and other concerns in greater detail below.

2 Literature Review and Conceptual Framework

The cross-country literature on the relationship between aid and conflict has not yet reached any clear consensus, and generally finds different effects for different types of aid. Several cross-country studies find a negative association between official development assistance (ODA) and civil conflict. De Ree and Nillesen (2009) examine the effect of ODA on civil conflict in sub-Saharan Africa. Controlling for country fixed-effects and using GDP growth in donor countries as an instrument for aid flows, they find that aid inflows reduce the probability that ongoing civil conflicts will continue, but they do not find a significant effect on the probability of conflict onset. Similarly, Nielsen et al. (2011) investigate the effects of shocks to ODA flows on civil conflict onset. They find that negative aid shocks increase the probability of conflict onset, and argue that this result is evidence that negative aid shocks disrupt the balance of power between the government and rebel groups. On the other hand, Nunn and Qian (2014) investigate the effect of U.S. food aid on civil conflict at the country level, using U.S. wheat production as an instrument for food aid flows. They find that U.S. food aid increases the duration of ongoing civil wars but find no effect on civil conflict onset.

Although country-level studies tend to find significant effects of aid on conflict, these effects vary in direction and magnitude and appear to differ depending on the type of aid. Moreover, some of these studies suffer from serious identification concerns. Nielsen et al. (2011) utilize propensity score matching to approximate a random treatment of aid shocks. However, important omitted variables concerns remain and as one of the authors later observes, this matching technique may itself introduce bias (King and Nielsen 2018). Other papers, notably de Ree and Nillesen (2009) and Nunn and Qian (2014), employ instrumental variables approaches. However, it is questionable whether these studies satisfy the exclusion restriction. For example, de Ree and Nillesen (2009) use donor country GDP as an instrument for aid flows to recipient countries, but do not include year fixed-effects in their model. However, their instrument, donor country GDP, could be correlated with global trends that affect conflict. For example, global trends in economic growth, such as a global recession, could affect donor country GDP while simultaneously affecting conflict behavior in recipient countries directly. In this case, the exclusion restriction would be violated.

Nunn and Qian (2014) are able to avoid this particular concern by interacting a global time trend (U.S. wheat production) with a recipient country-level exposure variable (the fraction of years the country receives wheat aid from the U.S.). Even so, Christian and Barrett (2017) show that Nunn and Qian's empirical strategy is prone to bias for a different reason. Their reliance on a shift-share instrument, in which a common time trend is interacted with a cross-sectional constant, introduces an additional assumption for unbiasedness that is overlooked by the authors. As is discussed further below, the second stage dependent variable (conflict in this case) must exhibit parallel trends in the cross-sectional exposure variable. If this is not the case, 2SLS estimates may identify a spurious effect based on these parallel trends. For example, in Nunn and Qian's dataset, high-frequency aid recipient countries and low-frequency aid recipient countries experience non-parallel conflict trends over time. Specifically, the trend in conflict over time exhibits a pronounced U-shape in high-frequency recipient countries, but not in low-frequency recipient countries, while U.S. wheat production happens to exhibit a similar U-shape trend. Because trends in conflict are non-parallel between low- and high-frequency recipient countries, and because the trend in high-frequency countries is similar to the trend in U.S. wheat production, Christian and Barrett (2017) show that the instrument is biased towards identifying a positive relationship between aid on conflict. Moreover, they show that Nunn and Qian's instrument identifies a positive effect of aid on conflict even using a dataset generated on the assumption that aid reduces conflict. Not incidentally, the issues with Nunn and Qian‘s identification strategy are relevant to my own, as their strategy is conceptually similar to mine. However, I believe that my strategy offers an advantages. Since I am utilizing subnational spatial units, the counter-factual unit for parallel trends is not another country, which is likely to experience its own idiosyncratic time-varying shocks, but rather, another region in the same country or state. Regions in the same country or state are less likely to experience non-parallel trends.

Since they share an institutional and political context, and spatial proximity, they are more likely to be affected similarly by time-varying shocks. Nonetheless, I implement tests of the parallel trends assumption to check that this assumption is satisfied. I argue that results from these tests suggest that, if biased at all, my 2SLS estimates are biased **against** finding that aid reduces conflict.

Similar to the cross-country studies, case studies of foreign aid and violence have reached conflicting conclusions. Crost, Felter, and Johnston (2014) study the effect of a large development program in the Philippines. Exploiting the program's eligibility threshold, they use a regression discontinuity design to find that the program increased conflict casualties in its early stages. They argue that this result is consistent with anecdotal evidence that rebels attempted to sabotage the program, fearing its success would weaken their popular support. On the other hand, Berman, Shapiro, and Felter (2011) find that U.S. reconstruction spending in Iraq during and after the Iraq War reduced violence in recipient areas. In another case study of Iraq, Iyengar, Monten, and Hanson (2011) find that labor expenditures by U.S. development programs reduced labor-intensive insurgent violence. Both studies exploit variation in the funding of reconstruction projects in Iraq to estimate the effect of reconstruction spending on conflict behavior. Iyengar et al. (2011) use a difference-in-differences strategy, exploiting variation in spending preferences among different military units and variation in overall funding authorized by Congress to estimate the effect of additional labor expenditures. Similarly, Beath, Christia, and Enikolopov (2017) examine the effects of a randomized controlled trial on insurgency in Afghanistan. They find that the development program reduced violence in most recipient regions, but increased violence in border regions. Wood and Sullivan (2015) examine the effect of humanitarian aid on conflict using spatially disaggregated data on aid and conflict events in a sample of over 20 African countries. They use a genetic matching algorithm to match treatment regions (which receive humanitarian aid) and control regions (which receive no aid) and find that humanitarian aid increases rebel violence.

Several of these studies (Beath et al. 2017; Crost et al. 2014; Iyengar et al. 2011) exploit experimental or quasi-experimental variation and thus appear to be less afflicted by identification concerns. However, these case studies of aid and conflict invite questions of external validity. In Crost et al. (2014), the key result — that violence increases prior to project implementation — is tied to an idiosyncratic feature of the projects: a community decision-making process preceding implementation. Similarly, case studies in Iraq and Afghanistan such as Berman et al. (2011), Iyengar et al. (2011), and Beath et al. (2017) examine insurgent violence in the context of a military intervention by an external power, a scenario which differs from other common cases of civil conflict. Despite these concerns, these case studies tend to find an ameliorative effect of aid on conflict, with the notable exception of Crost et al. (2014). They point to a plethora of theoretical mechanisms. Ex ante incentives for rebels to sabotage aid projects, labor market dynamics, varying incentives for local vs. foreign insurgents among others — all of these are potential causal pathways for the effect of aid on conflict.

The existing literature offers disparate and at times conflicting results. Although several studies have found significant negative effects of aid on conflict duration and onset, several others have reached opposite conclusions. This suggests that heterogeneity analysis, as well as attention to the context surrounding project implementation, is important. The literature also points to a diverse array of on-the-ground mechanisms, which renders simple theoretical frameworks of aid and conflict problematic, and suggests that uniform effects of aid on conflict should not be expected. Notwithstanding this word of caution, it is still useful to outline some basic, informal theoretical frameworks for understanding the dynamics between conflict and aid. These framework should be thought of as attempts to elucidate several of the most important possible mechanisms for aid to affect conflict. Two basic effect pathways jump out.

The first is the opportunity cost effect. To the extent that foreign aid alleviates poverty and raises incomes, it will increase the opportunity cost of fighting relative to producing. As a result, agents will reallocate labor away from soldiering and toward production (Grossman 1991). This effect aligns with the well-established cross-country correlation between poverty and conflict (Blattman and Miguel 2010). However, this effect is premised on a causal condition — foreign aid raises local incomes — that may or not exist in reality. To the extent that at least some projects have no effect on, or even reduce incomes, this causal mechanism will have limited explanatory power.

The second is the ”rapacity effect” (Dube and Vargas 2013). Aid inflows may incite conflict by increasing the size of the prize to be won in a civil conflict (Grossman 1991; Garfinkel and Skaperdas 2007). As aid inflows increase the returns to violent expropriation, agents will reallocate labor towards fighting and away from production. This effect is most plausible when aid introduces tangible assets that can be seized by local agents and then consumed or sold. Thus we may expect tangible forms of aid, such as food aid or humanitarian aid, to be most vulnerable to escalations in rapacity (Nunn and Qian 2014). It is difficult to determine whether this effect is likely to be present for development aid, as development aid does not always take the form of tangible materials. There are other potential mechanisms related to the way foreign aid can upset the local status quo. To the extent that aid projects do in fact promote social, political, or institutional change, there may be a backlash effect, as agents unhappy with these changes resort to violence to resist them.

In addition to these basic effect pathways, there are several additional mechanisms that may be important. For example, aid may incite conflict, even without a rapacity effect, if aid is seen by rebel groups as a challenge to their authority or a tool that will increase loyalty to the central government. Since rebel groups often rely on support or acquiescence from the local population, we may expect them to be sensitive to anything which will threaten the loyalty of the public. This is exactly the mechanism that Crost et al. (2014) identify: rebels sabotage aid projects out of fear that the success of these projects will increase support for the government. In addition, several other studies point out that attacks on aid projects become more likely when rebel groups view the projects as means for the central government to consolidate popularity (Fast 2010; Stoddard, Harmer and DiDomenico 2009). On the other hand, to the extent that aid projects are effective in increasing support for the central government, aid may reduce incentives to join rebel groups. This may occur even without an observable improvement in local economic conditions — the perception that economic conditions are improving, or that the central government is providing services may itself reduce incentives to rebel. For example, Berman et al. (2011) present a model of insurgency in which the central government can attempt to control a population and reduce insurgency by using force or providing services. As government service provision increases, noncombatants increase their loyalty to the government and their loyalty to rebel groups falls. As "rational peasants," they interpret increased government service provision as an increase in the expected utility they derive from central government control relative to rebel control, and thus see it as in their own interest to increase cooperation with the government.

Consequently, there are a number of potential causal mechanisms, and theory provides no clear predictions about the relationship between aid and conflict. Moreover, it is possible that one or more of these effects may be in play in any given locale. This framework suggests, above all, that heterogeneous effects should be expected, and that the effects of aid on conflict may vary widely across recipient regions, project types, and conflict categories.

3 Data

Three main datasets are used in the analysis: data on administrative units, conflict events, and aid projects. Administrative unit data are taken from the Database of Global Administrative Areas (GADM 2018). GADM maps the spatial boundaries of all known administrative units around the world. In particular, I aggregate spatial data on aid and conflict to the ADM 1, or state, level and ADM 2, or county, level. As mentioned above, I refer to the ADM 1 level as the state level and the ADM 2 level as the county level throughout this paper for ease of understanding. Likewise, for controls for population, precipitation, temperature, and nighttime lights, I take spatial averages across the region. Data on conflict come from the UCDP Georeferenced Event Dataset (UCDP GED) Global version 18.1 (Croicu and Sundberg 2017). UCDP GED covers all conflict events around the globe from 1987 to 2017. UCDP GED bases its data collection around a specific definition of a conflict event. A conflict event is only included if it involves at least one organized actor, at least one fatality, and occurs in a specific time and place. Consequently, non-fatal conflict-related events, such as troop movements and arrests, are omitted. Moreover, fatal conflict events are not included if they do not involve at least one identifiable organized actor. Mob violence and riots are thus not covered by UCDP GED. The dataset also covers interstate conflict events (between two different states). Since my analysis focuses on civil conflict, I drop all interstate conflict events (less than 1% of the observations in the dataset).

The dataset traces events with a dyad focus. It reports conflict events for dyads (actor-actor pairs, or actor-civilian pairs) that have crossed the UCDP threshold for an armed conflict of 25 battle deaths in a year, during some year. That is to say, if a dyad crosses this threshold in only one year, then conflict events associated with the dyad are recorded for all years, regardless of whether they cross the threshold in that year. At the same time, if a dyad never reaches this threshold, then it is never classified as a civil conflict by UCDP GED and its conflict events never appear in the dataset. This dyad focus is useful in that it includes only events that occur between warring parties that engage in substantial conflict. However, it has the possibility to introduce bias if the frequency of conflict events below this threshold is systematically correlated with aid receipt, conditional on controls.

UCDP GED is compiled through a rigorous coding process. In the first pass, coders search global newswire reports for instances of fatal conflict events. In the second pass, these news reports are crossreferenced with other local and specialized news sources, such as reports from NGOs, local monitoring organizations, and Truth and Reconciliation Commissions. These initial passes are followed by automated checks, and human corrections to flagged errors. UCDP GED is fully georeferenced. All conflict events are assigned to the lowest possible identifiable level of spatial aggregation. The most precise location is the village or town, and the highest level of spatial aggregation is the country. 95% of all conflict events in UCDP GED fall within a specific state, while 81% fall within a specific county.

The dataset offers several advantages for the study of the subnational effecs of aid on conflict. First, and most importantly, precise geocoding allows for a low level of spatial aggregation and analysis. Since the vast majority of events fall within a specific state, I do not lose much of the sample due to geographic imprecision. Second, UCDP GED offers an internally consistent definition of conflict across space and time. Thus, we can study conflict events from disparate regions and across time periods and be sure that each event fits the same definition of conflict Finally, UCDP GED compares favorably with similar datasets, both in terms of its coverage, internal consistency, and quality of geocoding (Eck 2012).

Data on World Bank aid projects comes from the most recent version of AidData's All World Bank IDA/IBRD dataset (AidData 2017). This dataset covers the universe of World Bank projects funded through the IBRD and IDA and approved between 1995 and 2014. 5,881 unique projects are included, of which 5,684 (97%) are geocoded, totalling roughly USD 390 billion in geocoded disbursements and USD 630 billion in geocoded commitments. The AidData dataset contains the World Bank project ID, geocoded project location(s), and information about the sector of the project. All geocoded projects can be linked to specific villages or towns (and thus all can be aggregated to the state and county level). I link this dataset with disbursement data obtained from the World Bank website to create a project-location-year level disbursement dataset. Disbursement data covers project in- and out-flows between 1995 and 2017, and are measured in constant 2017 US dollars.

One drawback of the AidData dataset is that it does not provide information on how funds are disbursed between project locations when there are multiple project locations associated with a given project. For example, if a project has three different project locations, I cannot track how the disbursement is distributed among the three regions. I follow the aid literature and assign disbursements equally across all locations or in proportion to the population of the recipient regions. As is shown below, both methods produce essentially identical estimates. Additionally, the raw disbursements data displays a strong right skew. Therefore, I take the inverse hyperbolic sine of aid amounts for use in the regressions: $sinh^{-1}(x) = ln(x+\sqrt{x^2+1})$. The inverse hyperbolic sine transformation is similar to a log transformation, but offers the advantage that it is defined at zero, and therefore it is not necessary to add one to the raw data. The interpretation of an inverse hyperbolic sine-transformed variable is essentially identical to that of a log-transformed variable.

Since my identification strategy exploits variation in the timing of crossing the International Development Association's eligibility threshold, my sample includes only countries that cross the threshold during the sample period (1999-2017). This results in a sample of 36 countries. Four of these countries do not possess a level of administrative subdivision below the state, so the county sample consists of 32 countries. The sample includes 11 countries in Sub-Saharan Africa, two in the Middle East and North Africa, three in Europe, 18 in Asia, and two in Latin America. The full list of sample countries along with their year of crossing the IDA threshold is displayed in table 1 below. Countries not included in the county level sample are marked with a star. The sample includes a total of 647 states and 6773 counties.

The primary outcomes of interest are conflict incidence and onset. Conflict incidence is used to estimate the overall probability of a conflict event. Conflict incidence is a binary indicator variable which is equal to one if a region experiences conflict in year t. On the other hand, conflict onset measures the appearance of new conflict events in a region. Conflict onset is an indicator variable which equals one when a region experiences conflict in year t, while experiencing no conflict in $t-1$. If a region experiences conflict at time $t-1$, the onset variable is set to missing since conflict onset is impossible in this case. Summary statistics of aid and conflict data at both the state and county levels can be found in tables 1 and 2 below.

In addition to these primary datasets, I also include data on regional population, annual precipitation, and nighttime lights to construct control variables. Population data are taken from the Center for International Earth Science Information Network's (CIESIN) Gridded Population of the World (GPW), Population Count version 4.11 dataset (CIESIN 2018). The data were downloaded as rasters and then aggregated to the regional level. Since CIESIN GPW only contains population estimates at 5-year intervals (for the years 2000, 2005, 2010, 2015, as well as projections for 2020), I use linear interpolation to fill missing years. Annual precipitation data are from the Climactic Research Unit's Time Series (CRU TS) dataset version 4.02. CRU TS has global coverage from 1901 to 2017, and is available as a raster dataset at a resolution of 0.5°by 0.5°. Just as with the population data, the raster data is aggregated to the regional level to create a region-year dataset. Finally, nighttime lights data come from the Defense Meteorological Satellite Program Operational Light Scan (DMSP-OLS) dataset (DMSP-OLS. I use the stable lights product, which measures cloud-free nighttime light emission, filtering out light from ephemeral events such as fires. Data values range from 1 to 63, where 1 is the lowest level of light emission. These data are also downloaded as raster data and averaged at the administrative unit level.

4 Empirical Approach

The primary challenge of estimating the effect of development aid on civil conflict is the problem of endogeneity. This issue could operate through a number of avenues. First, there may be reverse causality. Donors may change their decision making based on the conflict status of a given region, and thus aid flows may be determined by conflict. This causality may function either positively or negatively. If donors aim to promote peace through aid provision, we may expect more aid to flow to conflict-ridden regions. On the other hand, conflict presents difficulties for the transfer of aid, project implementation, and aid worker safety. As a result, we may also expect less aid to flow to regions which experience conflict. Apart from reverse causality, simple OLS regression will be biased if there are unobserved factors that affect conflict which are also correlated with aid. A number of factors, such as geography, per capita income, public services, and ethnic fractionalization could confound this relationship.

To deal with potential endogeneity issues, I utilize an instrumental variables approach to estimate the causal effect of aid on conflict. Following recent contributions in the aid and growth literature, I exploit exogenous variation in World Bank aid due to a country's crossing the IDA eligibility threshold (see Dreher and Lohmann 2015; Galiani et al. 2017). Since 1987, eligibility for IDA lending has been determined by a GNI per capita income limit. This limit is adjusted for inflation each year. Figure 1 displays the evolution of the IDA threshold over time. Countries that cross the threshold become ineligible for further commitments from the IDA (IDA 2001; Galiani et al. 2017). As Galiani et al. (2017) demonstrate, countries that cross the threshold subsequently experience large declines in World Bank aid, which are not compensated for with increases in aid from other sources.

However, it is possible that crossing the threshold is itself endogenous to conflict: countries experiencing conflict may experience lower growth rates, and thus be less likely to cross the threshold. To address this potential endogeneity at the country-year level, I follow Dreher and Lohmann (2015) who use as the excluded instrument an interaction between a dummy for being above the IDA threshold with the fraction of years a region receives aid before crossing the threshold. This can be thought of as a treatment intensity variable. Regions which receive aid in many years before crossing the threshold are likely to be more exposed to changes in IDA status. On the other hand, irregular recipient regions are less exposed as they rarely receive IDA aid. As a result, we expect regular recipient regions to experience larger declines in World Bank aid after their country crosses the IDA threshold, compared to irregular recipient regions. Thus, the strategy resembles a difference-in-differences approach in the first stage, wherein high frequency recipient regions are the treatment group and low frequency recipient regions are the control. The preferred regression specification is as follows:

$$C_{ict} = \beta A_{ict} + \gamma X_{ict} + \delta_{ct} + \eta_i + \epsilon_{ict} \tag{1}$$

$$A_{ict} = \alpha(F_i \times I_{ct}) + \gamma X_{ict} + \delta_{ct} + \eta_i + \epsilon_{ict} \tag{2}$$

Equation (2) is the first stage of the 2SLS model, and equation (1) is the second stage. Regions are indexed by i, countries are indexed by c, and years are indexed by t. A, the independent variable of interest in the second stage, is World Bank aid, measured as the inverse hyperbolic sine of disbursements or a binary indicator for receiving any aid. C is the conflict variable of interest, which is a binary variable for conflict incidence or onset. $F \times I$ is the excluded instrument – the interaction between F, the fraction of years a region received aid before crossing the threshold and I, an indicator for whether a country is above the IDA threshold. X is a vector of time-varying regional controls. δ is a country-year fixed effect, and η is a region fixed effect. Consequently, all unobserved factors that vary over time in the same way across the country are absorbed. Unobserved, time-invariant regional characteristics are also absorbed. Note that in some specifications at the county level, I include state-year fixed-effects. Since all county regions are nested within a larger state region, this controls for all unobserved factors that vary over time uniformly within the state.

In the second stage, I regress conflict at time t on the predicted value of World Bank aid from the first stage at time t, vectors of time-varying controls at the region level, country-year fixed-effects (or state-year fixed-effects in regressions at the county level), and region (state or county) fixed effects. The independent variable of interest is A. We are interested in whether the coefficient β is significantly different from 0. If β is positive, the estimate suggests that aid increases conflict (given that the identification assumptions are satisfied). Likewise, if β is negative, the estimate implies that aid reduces conflict.

In the first stage regression, I regress World Bank aid on the interaction term between a dummy for being above the IDA threshold and the fraction of years the region received aid before crossing the threshold. The instrument satisfies the exclusion restriction if the instrument only influences changes in conflict through changes in aid. Thus, the instrument must not affect conflict directly, and must not be correlated with any omitted time varying factors that change in different ways across different regions within the same country (or state, at the county level) and that also affect conflict. In other words, the additional effect on conflict of crossing the IDA threshold for regular recipient regions compared to irregular recipient regions within the same country (or at the county level, within the same state region) occurs only because of changes in aid. Since my preferred specification includes region fixed-effects as well as country-year fixed-effects (or state-year fixed-effects at the county level), the exclusion restriction will only be violated if there is some time-varying shock at the region level that varies systematically for regular and irregular recipient regions within the same country or state after crossing the IDA threshold. One potential channel may be income. We may expect regular and irregular recipient regions to possess systematically different characteristics such that they experience differential growth patterns or income trends when their country reaches a certain income level. For example, a country may cross the IDA income threshold due only to economic growth in regular recipient regions, while income in irregular recipient regions remains stagnant. To address this concern, I will include nighttime luminosity, a well-known correlate of economic growth, as a region-year level control. In addition, I will control for other time-varying characteristics, such as precipitation and population.

5 Results

I now discuss results from my analysis. Tables 4 and 5 below display results from Ordinary Least Squares (OLS) regressions of conflict incidence and onset on aid at the state and county levels. The regressions are estimated using a linear probability model, with heteroskedasticity-robust standard errors clustered at the region level. The primary aid variables of interest are a binary variable for receiving any aid and the amount of aid received (inverse hyperbolic sine-transformed). These variables capture the extensive margin and the intensive margin of the effect of aid on conflict, respectively. Thus, the interpretation of the aid receipt variable is the effect of receiving any aid relative to receiving none at all. Similarly, the interpretation of the aid amount variable is the effect of a doubling of the amount of aid received. Given that aid is likely endogenous to conflict, these OLS estimates should only be interpreted as correlations. Table 3 displays estimates of OLS regressions of conflict incidence and onset on aid at the state level. Each regression includes country by year fixed-effects as well as region fixed-effects. There does not appear to be any significant correlation between aid and conflict at the state level. Point estimates are small, insignificant and close to 0. For example, the coefficient in column 3 of table 4 implies that receiving aid is associated with a 0.15% increase in the probability of conflict incidence. Similarly, the coefficient in column 2 of table 4 suggests that doubling the amount of aid received is associated with a 0.01% increase in the probability of conflict incidence.

Table 5 reports results of OLS regressions of conflict on aid at the county level. Each regression includes state-year fixed-effects and region fixed-effects. These results indicate a slight but significant positive correlation between aid and both conflict incidence and onset. However, all point estimates are close to zero and economically small. For example, the point estimate in column 3 of table 5 suggests that receiving aid is associated with a 0.4% increase in the likelihood of conflict incidence, roughly 10% of the sample mean. Likewise, the estimate in column 2 indicates that doubling aid received is associated with a 0.03% increase in the probability of incidence. Point estimates from the regressions of conflict onset on aid are similar, although slightly smaller. Whereas aid receipt is associated with a 0.4% increase in the probability of experiencing any conflict, it is only associated with a 0.28% increase in the probability of the appearance of a new conflict.

Now, I move to results of the first stage regressions. Tables 6 and 7 display results at the state and county levels, respectively. The dependent variable is aid, and the independent variable of interest is the excluded instrument – the interaction between the fraction of years a region received aid before crossing the IDA eligibility threshold and a dummy for being above the threshold. Since we expect regular recipient regions to witness larger declines in aid after the IDA threshold is crossed, compared to irregular recipient regions, we expect the coefficient on the interaction term to be negative. In table 7, which displays estimates of regressions of aid on the interaction term at the county level, the coefficient on the instrument is negative, large, and significant in all specifications. For example, the coefficient in column 3 of table 7 suggests that a county that receives aid in all years before crossing the threshold experiences a 38% decline in the probability of receiving aid after the threshold is crossed, compared to a county that receives aid in no year before crossing the threshold. In each specification, the F-statistic on the instrument is large and well above the weak instrument threshold of 10. Results are qualitatively similar in table 6, which reports first stage regressions at the state level. Overall, these results indicate a strong first-stage relationship between the excluded instrument and the endogenous variable.

Turning now to my main two-stage least squares (2SLS) results, Tables 8 and 9 report 2SLS linear probability model estimates of the effect of aid on conflict at the state and county level, respectively. Just as in the previous regressions, these regressions include country-year fixed-effects at the state level and state-year fixed-effects at the county level, and standard errors are clustered at the region level. In table 8, which reports results at the state level, 2SLS estimates of the effect of aid on conflict incidence are insignificant. Point estimates of the effect on conflict incidence are positive while estimates of the effect on conflict onset are negative. The coefficients on aid are much larger in magnitude than the point estimates in the OLS regressions. For example, the coefficient in column 3 of table 8 indicates that receiving aid results in a 1.3% increase in the probability of experiencing conflict at the regional level. On the other hand, the analogous estimate from the OLS regressions suggests only a 0.15% increase.

Table 9 displays estimates of the effect of aid on conflict onset at the county level. Here, results suggest that aid reduces conflict incidence and onset. The point estimates of the effect of each of three aid variables on incidence are significant at the 5% level. Moreover, these results are large and economically significant. Consider the coefficient in column 3.

The estimate reflects the effect of aid on conflict at the extensive margin, and suggests that receiving any aid reduces the probability of conflict by 3.63%, or roughly 92% of the sample mean. Estimates of the effect of the amount of aid on conflict are likewise significant. The coefficient in column 1 suggests that doubling the amount of aid a county receives results in a 0.29% reduction in the probability of conflict, roughly 7% of the sample mean. Interestingly, the effect of aid on incidence at the extensive margin is many times larger than its effect at the intensive margin — in this case roughly 13 times larger.

Turning now to the effect of aid on conflict onset, we notice a similar pattern. The estimated effect of aid on onset is large, although only marginally significant, and suggests that receiving any aid results in a 1.9% reduction in the probability of the appearance of new conflict events in the county (column 6). This estimate is quite large: 110% of the magnitude of the sample mean. On the other hand, while the estimates of the effect of aid at the intensive margin are also marginally significant, they are much smaller. The estimate in column 4 suggests that doubling the amount of aid received in the county level results in only a .15% reduction in the likelihood of conflict onset. This amounts to roughly 9% of the sample mean. Just as before, the effect of aid is roughly 13 times greater at the extensive margin.

Overall, 2SLS estimates point to a large and significant effect of aid on conflict at the county level, such that receiving aid reduces the probability of conflict incidence and onset by roughly the sample mean. It is important to note that this effect is many times stronger at the extensive margin. The main conflict-reducing effect of aid seems to come from a region receiving a new aid project relative to having none at all, rather than increases in funding of existing projects. Moreover, these estimates point in the opposite direction than the OLS results (in table 5) which suggest a positive correlation between aid and conflict. However, results at the state level are insignificant in all specifications. Additionally, the magnitude of the 2SLS point estimates are greater at the county level than at the state level in all specifications. This trend is more pronounced in the conflict incidence regressions, in which the county-level estimates are multiple times larger in absolute value than the state-level estimates.

In tables 10 and 11, I estimate 2SLS linear probability models using an alternative conflict specification. These regressions include a control for lagged conflict incidence. As other studies have noted (see e.g., Nunn and Qian 2015), this specification may better account for the high level of persistence of conflict over time. An additional benefit of this specification is that it no longer becomes necessary to set conflict onset to missing when the region experiences conflict in the previous year. Rather, the lagged conflict variable will control for the mechanical negative correlation between lag conflict and conflict onset (conflict in the previous periods makes conflict onset impossible). However, these results may be susceptible to the Nickell bias, which results from the inclusion of lags of the dependent variable in a fixed-effects model. Consequently, I consider my main 2SLS estimates to be more reliable, although it is still useful to check whether these estimates are robust to an alternative specification. At both the state and county levels, the results are qualitatively similar. At the state level, in table 10, the coefficient on aid is insignificant in all specifications just as in the main 2SLS regressions in table 8. The magnitude of the point estimates are similar though slightly smaller. In table 11, which displays results of regressions at the county level, the results for incidence are very similar to those in the main 2SLS estimates in table 9. Column 3 of table 11, for example, implies that receiving aid reduces the probability of conflict by 3.43%, essentially equivalent to the 3.63% point estimate identified in the main specification. On the other hand, the results for conflict onset lose their significance in this specification. Point estimates from the alternative specification are slightly smaller, for example, the estimation of the effect at the extensive margin in column 6 of table 11 implies that aid reduces the probability of onset by 1.46%, slightly smaller than the 1.87% estimate in the main regression (table 9).

Next, I proceed to investigate the validity of the instrument. The most important concern for my instrument is that the parallel trends assumption is violated. As Christian and Barrett

(2017) point out, with a shift-share instrument such as my own, it is possible that the second-stage effect identified by the instrument is due to differential trends in conflict between the treatment group (high-frequency aid recipients) and the control group (low-frequency aid recipients) that are spuriously correlated over time with crossing the IDA threshold. If this is the case, the instrumental variables estimation identifies only these differential conflict trends, rather than any true relationship between aid and conflict. To avoid this concern, it must be the case that there are no differential trends in conflict among high-frequency and low-frequency aid recipients before treatment (i.e., before crossing the IDA threshold). To test this assumption, I implement a standard test of the parallel trends assumptions for a continuous treatment intensity variable. I interact leads and lags of the IDA crossing date with the fraction of years a region receives aid before crossing the IDA threshold (Fraction Aid). The coefficient on each interaction term represents the differential level of conflict between low- and high-frequency recipient regions a given number of years before or after crossing the IDA threshold. I include the same set of controls and fixed-effects as are included in the main analysis. The existence of many significant coefficients in the pre-treatment period would suggest that the parallel trends assumption is violated, and hence that 2SLS estimates may be only identifying these differential pre-trends, rather than identifying the true effect of the endogenous regressor.

However, this test is complicated by the fact that countries cross the IDA threshold in different years, and so the treatment periods are not identical across regions. To address this complication, I run the test for windows of various lengths surrounding the crossing date. However, as the window length increases, fewer regions are included, rendering the tests less informative. It should be noted that all regions have at least two pre-treatment periods and two post-treatment periods. Results from these regressions are displayed in tables 12-15. In tables 12 and 13, which display results of the test at the state level for conflict incidence and onset respectively, none of the coefficients on the pre-treatment interaction terms enter significantly except for the coefficient on the interaction term three years before treatment in column 3 of table 12, which is marginally significant and positive. However, the overall lack of significance in the pre-treatment period suggests no cause for concern about the parallel trends assumption at the state level.

Tables 14 and 15 display results of the same test at the county level for incidence and onset respectively. As mentioned above, these tables include the full set of controls included in the main regressions as well as state-year fixed-effects. Here there appears to be a anticipation effect. Coefficients on the interaction term for the the last year of the pre-treatment period enter positively and significantly in every regression in both tables. This anticipation effect extends to the second year before treatment in columns 1-3 of table 14, which displays estimates for incidence. The window of four pre-treatment and four-post-treatment years (column 3) indicates significant coefficients for all pre-treatment years. The estimates imply that in the years immediately before the instrument goes into effect, high-frequency recipient regions were more likely to experience conflict than low-frequency recipient regions. For example, the coefficient on the interaction term one year before the instrument goes into effect in column 1 of table 15 indicates that a county which received aid in all years before crossing the IDA threshold has 2.4% greater probability of experiencing conflict than a region which received aid in no years before crossing, in the last pre-treatment year. So there seems to be clear evidence that the parallel trends assumption is not satisfied at the county level. Is this evidence that the causal effect identified by the IV estimation — that aid reduces the probability of conflict at the county level — is spurious? I argue that it is not.

First, we must consider the case where non-parallel trends would result in a spurious negative relationship between aid and conflict identified by IV estimation. In this case, high-frequency recipient regions would experience lower differential conflict trends relative to low-frequency recipient regions in the pre-treatment period, and higher or parallel trends afterwards. In other words, the coefficients on the pre-treatment interaction terms would be negative and significant, and the coefficients on the post-treatment interaction terms would be positive or zero. Note that the instrument takes higher values for high-frequency recipient regions in the years after crossing. For example, a region which receives aid in 25% of years before crossing would take a value of 0.25 in the post-treatment period, whereas a region which receives aid every year before crossing would take a value of 1 in the post-treatment period. As a result, the non-parallel pre-trends mentioned above would result in the instrument being positively correlated with conflict. Intuitively, this is because high-frequency regions experience a lower probability of conflict before crossing and a higher or equivalent conflict tendency after, and take higher values of the instrument upon crossing. Since the instrument is negatively correlated with aid (as is shown in the first stage regressions in tables 6 and 7) and positively correlated with conflict, the instrument will identify a false negative relationship between aid and conflict.

This scenario would appear to be confirmed if the parallel trends test revealed significant and negative coefficients on the interaction terms in the pre-treatment period. However, the tests of the parallel trends assumption suggest the opposite: high-frequency recipient regions actually have a higher probability of conflict during the years immediately before treatment. Consequently, the tests do not confirm that the negative causal effect at the county level identified by the IV results is spurious. Rather, they seem to suggest that 2SLS estimates at the county level are biased *against* identifying a negative relationship between aid and conflict. Consequently, it appears that the estimates identified in my main 2SLS results should actually be taken as a lower bound of the magnitude of the conflict-reducing effect of aid at the county level.

Now, I turn to heterogeneity analysis. First, I examine the possibility of differential effects by elevation. A number of studies have suggested a link between terrain ruggedness — the presence of mountains and hills, which provide places for rebels to hide and are difficult for central government forces to penetrate — and conflict, most famously Fearon and Laitin (2003). Consequently, we may expect the effect of aid on conflict to be different in regions with different terrain, namely between high and low elevation regions. To investigate, I estimate my main 2SLS regressions separately for regions with average elevation above the median in the sample, and those below. These results are shown in tables 16-19. Columns 1-3 include only below-median elevation regions, and columns 4-6 contain only above-median elevation regions. At the state level, shown in tables 16 and 17, estimates are insignificant in all regressions. However, at the county level, estimates indicate that aid reduces conflict incidence and onset in high-elevation counties, but has no significant effect in low-elevation counties. Moreover, this conflict-reducing effect in high-elevation counties is substantially larger than the effect identified in the main 2SLS estimates. Estimates for the effect on incidence and onset at the county level are shown in tables 18 and 19, respectively. For example, the estimate in column 6 of table 18, which identifies the effect of receiving any aid on conflict incidence in high elevation regions, implies that aid receipt reduces conflict by 9.4% — almost triple the size of the average effect identified in the main 2SLS analysis. This estimate is significant at the 1% level. Similarly, the coefficient on aid amount is several times larger than the average effect identified in the main estimates. Results for conflict onset are similar, and are also several times larger than the average effect. For example, column 6 of table 19 indicates that aid receipt reduces conflict onset by 4.7%, roughly two and half times the size of the average effect from table 8. Thus, it appears that aid greatly reduces conflict in high-elevation regions but has no effect in low-elevation regions.

I also examine differential effects by distance to the border. A number of studies point to border regions as being more exposed to conflict for a number of regions. Since these regions are often distant from central government power centers, they often serve as a haven for rebels. Civil conflicts may spill over across borders, especially when the countries on both side of the border share an ethnic group, or when an ethnic group is split by a border (see e.g., Michalopoulos and Papaioannou 2016, for a recent study). I examine this source of potential heterogeneity by splitting my sample by distance of the region's centroid from the national border. This is likely a better sample cut than including only regions that abut the national border, because especially at the county level, there are many small regions that are very close to the national border but that do not abut it.

Tables 20-23 displays results from these regressions. Of these, tables 20 and 21 report results for regressions of conflict incidence and conflict onset at the state level, respectively. Estimates are positive in the sample of regions close to the border, and negative in the sample of regions distant from the border. However, results at the state level are also insignificant in every specification. On the other hand, Tables 22 and 23 show regressions of incidence and onset respectively at the county-level. Columns 1-3 report results from regions below the median distance from the border, while columns 4-6 report results from regions above. Estimates of the effect of aid on both conflict incidence and onset are significant and negative in regions close to the border, but are insignificant in regions further from the border. The point estimates of the effect of aid on conflict in regions closer to the border are greater in magnitude than the average effect from the main estimates. For example, the coefficient in column 3 of table 22, which identifies the effect of aid on conflict incidence in regions close to the border, indicates that receiving aid reduces conflict incidence by 4.87%, larger than the 3.63% average effect identified by the main 2SLS estimates (table 9). The results are similar for onset. Aid receipt reduces conflict onset by 3.16% in regions close to the border (column 3, table 23), nearly twice the size of the 1.87% estimate from the main 2SLS regression.

Finally, I also investigate differential effects by regional area. This can help elucidate the possibility of the attenuation of the effect of aid on conflict through space. This may occur for a number of reasons. For example, aid may produce offsetting spillover effects, such that it reduces conflict in the locality that receives it, but induces it in neighboring regions. In such a case, the local effect of aid on conflict is aggregated together with the spillover effects, resulting in a null average effect at larger units of analysis. To examine this question, I split the sample by the median surface area. I then run 2SLS regressions separately on each sample cut. Results from these regressions are reported in tables 24-27.

Tables 24 and 25, which report results at the state level, do not indicate any significant effect among smaller units or larger ones. However, in table 26, which reports the results of conflict incidence regressions at the county level, coefficients on aid are negative and significant at the 1% level in smaller regions. There appears to be no significant effect in larger regions. These coefficients are large: for example, the effect of receiving aid in a small region is an 8.73% reduction in the probability of conflict incidence (table 26, column 3). Likewise, the point estimates on the amount of aid are large, and suggest that doubling aid in a small region is associated with a 0.74% reduction in the likelihood of conflict. On the other hand, in table 27, which displays results of conflict onset regressions, aid is insignificant in all specifications. However, the point estimates in columns 1-3, on the sample of small regions, are an order of magnitude larger than the point estimates from the sample of larger regions, in columns 4-6. Although the onset results are not as conclusive as the incidence results, they too are suggestive of a larger effect of aid on conflict in smaller regions.

6 Discussion

2SLS estimates of the effect of aid on conflict indicate that aid reduces conflict incidence and onset at the county-level, although this relationship is somewhat weaker for conflict onset. Aid has a larger conflict-reducing effect in high-elevation counties and counties close to the national border, and these estimates are significant for both conflict incidence and onset. The magnitude of the effect of aid on conflict also appears to be greater in smaller regions. Moreover, estimates of the effect of aid on conflict at the extensive margin are may times larger than the estimated effect at the intensive margin. What interpretations do these results suggest?

These results are consistent with a number of interpretations. The first is the opportunity cost mentioned above: development aid, by promoting growth, alleviating poverty, and raising incomes, increases the opportunity costs of fighting relative to working for a wage. However, the fact that the effect of aid is concentrated at the extensive margin seems problematic for this theory. Presumably, greater amounts of aid are more likely to be correlated with a stronger impact on growth than the mere presence of aid projects. If development aid indeed reduces conflict through its effect on growth, then we would expect the amount of aid (i.e., the intensive margin) to display a strong effect on conflict, not only the presence of aid projects. Thus, I argue that this interpretation is not very strongly supported.

Another possible interpretation is similar to the insurgency-service provision model proposed by Berman et al. (2011). Development aid projects are a visible form of central government service provision. Locals observe the presence of aid projects, and consequently, their expected utility of central government control increases relative to the expected utility they derive from rebel control. They consequently join rebel groups in smaller numbers, and increase support for the central government. Thus rebel activity dissipates and conflict falls. This interpretation seems consistent with a large effect on the extensive margin. The presence of an aid project in a region, relative to none being present before, may have a large impact on locals' perceptions of (and hence their expected utility derived from) government control. On the other hand, further increases in aid spending are likely to be less noticeable and thus may have a smaller effect. Moreover, one reasonable prediction stemming from this interpretation is that, since aid reduces conflict through its effect on insurgency, it should have greater conflict-reducing effects in regions which are prone to such conflict. This seems consistent with the patterns identified in the heterogeneity analysis: development aid seems to have a greater conflict-reducing effect in regions which are geographically prone to insurgency, namely, high-elevation, mountainous regions, and border regions.

Finally, these results seem to point to the existence of offsetting spatial spillovers of aid on conflict. That is to say, aid may reduce conflict on average at its point of impact, but its spillovers may increase conflict in surrounding locales. As a result, the effect of aid is more likely to be null in larger spatial units. This corresponds with the finding that aid reduces conflict at the county level, but has no significant effect at the state level. It is also consistent with the result that the effect of aid on reducing conflict incidence is greater in smaller counties than larger ones: the smaller the county is, the less likely it is that offsetting spatial spillovers will fall within its borders. Future versions of this paper should include spatial lags of aid to address this question directly.

7 Conclusion

Using an instrumental variables strategy, I attempt to estimate the causal effect of aid on conflict incidence and onset at the local level. My results suggest that aid reduces conflict incidence and onset significantly at the county level, although this result is weaker for conflict incidence. This effect is much stronger on the extensive margin. There does not appear to be any significant effect of aid on either conflict onset or incidence at the state level. Moreover, I find that aid has a greater conflict-reducing effect in high-elevation counties and regions close to the national border. These findings appear to be consistent with a government service provision-insurgency model of the relationship between aid and conflict, in which increased service provision reduces support for rebels and causes levels of conflict to fall. Further research should include spatial lags of aid to address spatial spillovers.

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Tables and Figures

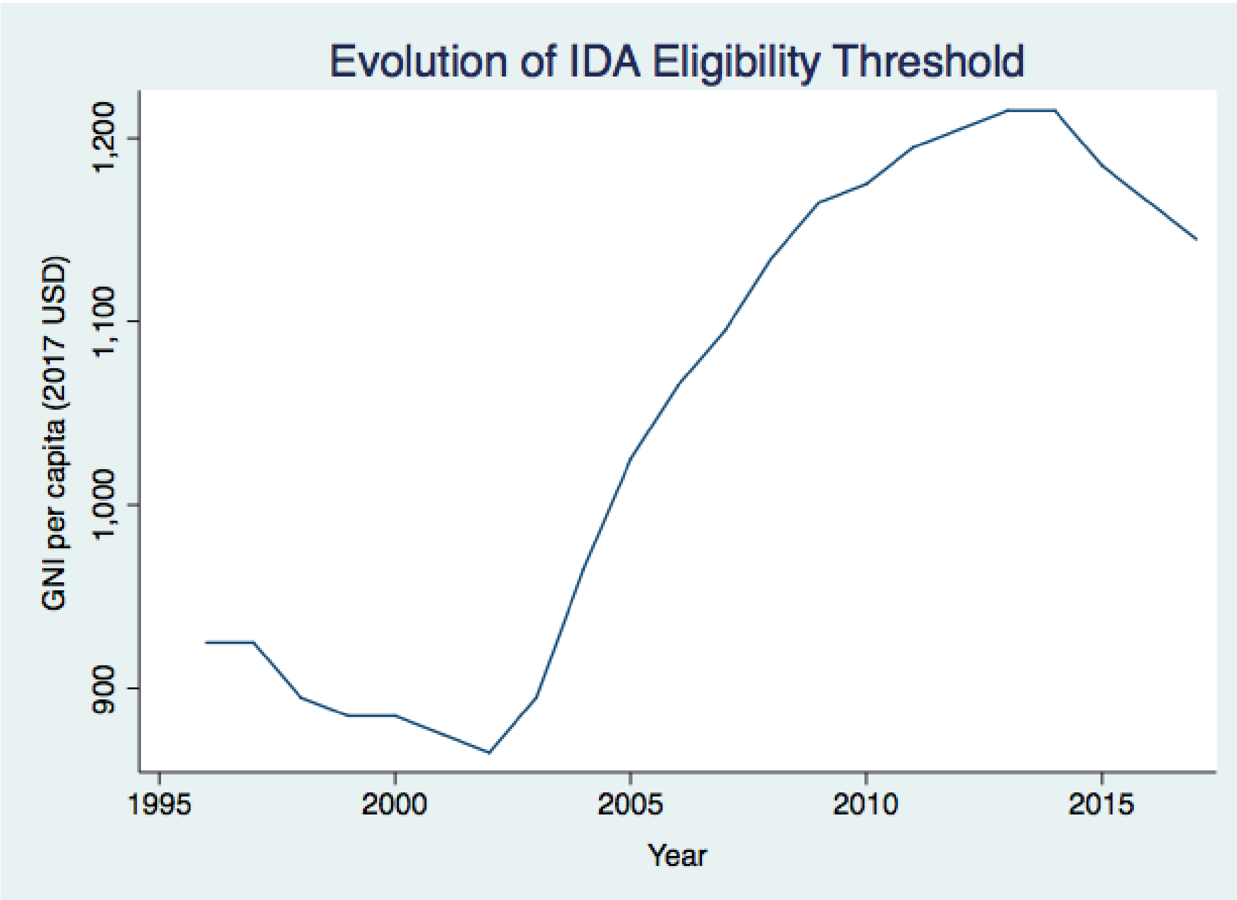

Figure 1. IDA Eligibility Threshold Over Time

Table 1. Sample Countries

Countries with Year of Crossing			
Name of Country	Year	Name of Country	Year
Angola	2005	Moldova*	2007
Armenia*	2003	Myanmar	2013
Azerbaijan	2005	Mongolia	2006
Bhutan	2003	Mauritania	2011
China	2000	Nigeria	2010
Cote d'Ivoire	2012	Pakistan	2012
Cameroon	2006	Papua New Guinea	2007
Congo (Republic)	2006	Sudan	2009
Equatorial Guinea	2001	Solomon Islands	2012
Georgia	2003	Sao Tome and Principe	2011
Ghana	2008	Tajikistan	2013
Guyana	2005	Turkmenistan*	2003
Honduras	2001	Timor-Leste	2006
Indonesia	2003	Ukraine	2003
India	2010	Uzbekistan	2010
Lesotho*	2005	Vietnam	2010
Laos	2012	Yemen	2012
Sri Lanka	2003	Zambia	2008

Table 2. Summary Statistics (State-level)

	mean	sd	min	max	count
Incidence	0.141	0.348	0	1.000	11120
Onset	0.048	0.214	0	1.000	10038
Aid Amount (Pop)	9321.944	29543.491	0	543216.427	11120
Aid Amount (Loc)	9330.514	26964.017	0	513965.740	11120
IHS Aid Amount (Pop)	11.942	6.475	0	20.806	11120
IHS Aid Amount (Loc)	12.151	6.533	0	20.751	11120
Any Aid	0.787	0.409	0	1.000	11120

Raw aid amounts are in thousands of 2017 US dollars.

Table 3. Summary Statistics (County-level)

	mean	sd	min	max	count
Incidence	0.039	0.194	0	1.000	114240
Onset	0.017	0.129	0	1.000	111670
Aid Amount (Pop)	894.492	5037.086	0	256413.161	114240
Aid Amount (Loc)	896.805	4539.546	0	223767.545	114240
IHS Aid Amount (Pop)	4.646	6.624	0	20.055	114240
IHS Aid Amount (Loc)	4.741	6.735	0	19.919	114240
Any Aid	0.337	0.473	0	1.000	114240

Raw aid amounts are in thousands of 2017 US dollars.

Table 4. No Correlation Between Aid and Conflict at State Level (OLS Estimates)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Onset	(5) Onset	(6) Onset
Aid Amount (Pop)	0.000178			0.000102		
	(0.000863)			(0.000712)		
Aid Amount (Loc)		0.000116			0.0000133	
		(0.000847)			(0.000691)	
Any Aid			0.00155			-0.00293
			(0.0111)			(0.00929)
Log Population	0.112**	0.112**	0.112**	0.0422	0.0422	0.0420
	(0.0513)	(0.0514)	(0.0514)	(0.0311)	(0.0311)	(0.0311)
Precipitation	0.0179	0.0179	0.0179	0.00394	0.00395	0.00400
	(0.0202)	(0.0202)	(0.0202)	(0.0188)	(0.0188)	(0.0188)
Lag Lights	0.00661	0.00665	0.00667	0.0157	0.0158	0.0159
	(0.0260)	(0.0260)	(0.0260)	(0.0154)	(0.0154)	(0.0154)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9705	9705	9705	8813	8813	8813

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5. Positive Correlation Between Aid and Conflict at County Level (OLS Estimates)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Onset	(5) Onset	(6) Onset
Aid Amount (Pop)	0.000278*			0.000179		
	(0.000168)			(0.000114)		
Aid Amount (Loc)		0.000287*			0.000183*	
		(0.000164)			(0.000110)	
Any Aid			0.00400*			0.00282*
			(0.00220)			(0.00148)
Log Population	-0.00720	-0.00709	-0.00715	0.0100	0.0101	0.0101
	(0.0238)	(0.0238)	(0.0238)	(0.0126)	(0.0126)	(0.0126)
Precipitation	-0.0113	-0.0113	-0.0113	-0.000698	-0.000690	-0.000716
	(0.00979)	(0.00979)	(0.00979)	(0.00833)	(0.00833)	(0.00833)
Lag Lights	-0.00449	-0.00449	-0.00448	-0.00131	-0.00131	-0.00131
	(0.00397)	(0.00397)	(0.00397)	(0.00242)	(0.00242)	(0.00242)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	101595	101595	101595	99402	99402	99402

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6. First Stage Estimates (State-level)

	(1) Aid Amount (Pop)	(2) Aid Amount (Loc)	(3) Any Aid
Fraction of Years Aid x Above	-9.464***	-9.924***	-0.729***
	(0.570)	(0.581)	(0.0367)
Log Population	-1.286	-1.919	-0.130
	(1.214)	(1.215)	(0.0815)
Precipitation	0.201	0.310	0.0185
	(0.267)	(0.317)	(0.0216)
Lag Lights	0.608	0.556	0.0244
	(0.484)	(0.520)	(0.0334)
Country-Year FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
First Stage F-statistic	276.0	292.2	394.1
Observations	9705	9705	9705

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7. First Stage Estimates (County-level)

	(1) Aid Amount (Pop)	(2) Aid Amount (Loc)	(3) Any Aid
Fraction of Years Aid x Above	-4.755***	-4.958***	-0.375***
	(0.204)	(0.209)	(0.0148)
Log Population	0.366	0.00115	0.0152
	(0.762)	(0.787)	(0.0561)
Precipitation	0.0898	0.0517	0.0130
	(0.253)	(0.259)	(0.0190)
Lag Lights	0.161	0.165	0.0100
	(0.138)	(0.142)	(0.0103)
Region FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
First Stage F-statistic	542.3	562.8	637.6
Observations	101595	101595	101595

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8. No Effect of Aid on Conflict at State Level (2SLS LPM Estimates)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Onset	(5) Onset	(6) Onset
Aid Amount (Pop)	0.000986			-0.00105		
	(0.00306)			(0.00227)		
Aid Amount (Loc)		0.000940			-0.00100	
		(0.00292)			(0.00217)	
Any Aid			0.0128			-0.0135
			(0.0397)			(0.0293)
Log Population	0.113**	0.113**	0.113**	0.0418	0.0412	0.0413
	(0.0509)	(0.0509)	(0.0510)	(0.0312)	(0.0311)	(0.0311)
Precipitation	0.0178	0.0177	0.0177	0.00413	0.00426	0.00415
	(0.0202)	(0.0202)	(0.0202)	(0.0188)	(0.0188)	(0.0188)
Lag Lights	0.00622	0.00630	0.00651	0.0165	0.0164	0.0162
	(0.0263)	(0.0263)	(0.0262)	(0.0156)	(0.0155)	(0.0155)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	276.0	292.2	394.1	236.2	250.6	335.5
Observations	9705	9705	9705	8813	8813	8813

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9. Aid Reduces Conflict Incidence at County Level (2SLS LPM Estimates)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Onset	(5) Onset	(6) Onset
Aid Amount (Pop)	-0.00286**			-0.00148*		
	(0.00135)			(0.000787)		
Aid Amount (Loc)		-0.00274**			-0.00142*	
		(0.00130)			(0.000755)	
Any Aid			-0.0363**			-0.0187*
			(0.0171)			(0.00994)
Log Population	-0.00733	-0.00837	-0.00782	0.0102	0.00964	0.00990
	(0.0242)	(0.0243)	(0.0242)	(0.0127)	(0.0127)	(0.0127)
Precipitation	-0.0113	-0.0115	-0.0111	-0.000691	-0.000756	-0.000577
	(0.00982)	(0.00982)	(0.00983)	(0.00834)	(0.00834)	(0.00834)
Lag Lights	-0.00431	-0.00432	-0.00440	-0.00123	-0.00123	-0.00128
	(0.00399)	(0.00399)	(0.00399)	(0.00243)	(0.00243)	(0.00243)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	542.3	562.8	637.6	503.9	522.2	595.1
Observations	101595	101595	101595	99402	99402	99402

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10. Alternative Conflict (State-level)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Onset	(5) Onset	(6) Onset
Aid Amount (Pop)	0.000877			-0.0000487		
	(0.00269)			(0.00255)		
Aid Amount (Loc)		0.000836			-0.0000465	
		(0.00257)			(0.00243)	
Any Aid			0.0114			-0.000632
			(0.0349)			(0.0330)
Log Population	0.0915**	0.0919**	0.0918**	0.0870***	0.0869***	0.0869***
	(0.0448)	(0.0448)	(0.0449)	(0.0313)	(0.0314)	(0.0314)
Precipitation	0.0176	0.0175	0.0176	0.00849	0.00849	0.00849
	(0.0204)	(0.0204)	(0.0204)	(0.0177)	(0.0177)	(0.0177)
Lag Lights	0.00534	0.00541	0.00560	0.0163	0.0163	0.0163
	(0.0235)	(0.0234)	(0.0233)	(0.0159)	(0.0159)	(0.0158)
Lagged Conflict	0.122***	0.122***	0.121***	-0.333***	-0.333***	-0.333***
	(0.0222)	(0.0222)	(0.0222)	(0.0159)	(0.0159)	(0.0159)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	276.3	292.5	393.9	276.3	292.5	393.9
Observations	9705	9705	9705	9705	9705	9705

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11. Alternative Conflict (County-level)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Onset	(5) Onset	(6) Onset
Aid Amount (Pop)	-0.00270**			-0.00115		
	(0.00124)			(0.000881)		
Aid Amount (Loc)		-0.00259**			-0.00111	
		(0.00119)			(0.000845)	
Any Aid			-0.0343**			-0.0146
			(0.0157)			(0.0112)
Log Population	-0.00666	-0.00765	-0.00713	0.00729	0.00687	0.00709
	(0.0218)	(0.0218)	(0.0218)	(0.0165)	(0.0166)	(0.0166)
Precipitation	-0.0112	-0.0113	-0.0110	0.00283	0.00279	0.00292
	(0.00963)	(0.00964)	(0.00964)	(0.00830)	(0.00830)	(0.00830)
Lag Lights	-0.00378	-0.00379	-0.00388	-0.00105	-0.00105	-0.00109
	(0.00370)	(0.00370)	(0.00369)	(0.00281)	(0.00281)	(0.00281)
Lagged Conflict	0.0972***	0.0973***	0.0972***	-0.275***	-0.275***	-0.275***
	(0.0107)	(0.0108)	(0.0108)	(0.00770)	(0.00770)	(0.00770)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	543.1	563.6	638.3	543.1	563.6	638.3
Observations	101595	101595	101595	101595	101595	101595

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12. Parallel Trends Test (State-level)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Incidence	(5) Incidence
Fraction Aid x 6 Years Before					0.0650
					(0.0584)
Fraction Aid x 5 Years Before				0.00374	0.0128
				(0.0629)	(0.0645)
Fraction Aid x 4 Years Before			0.0702	0.0629	0.0719
			(0.0485)	(0.0503)	(0.0516)
Fraction Aid x 3 Years Before		0.0640	0.0774*	0.0623	0.0713
		(0.0402)	(0.0416)	(0.0472)	(0.0486)
Fraction Aid x 2 Year Before	0.0124	0.0221	0.0355	0.0415	0.0505
	(0.0419)	(0.0427)	(0.0453)	(0.0545)	(0.0560)
Fraction Aid x 1 Year Before	0.00565	0.0154	0.0296	0.0440	0.0530
	(0.0481)	(0.0490)	(0.0508)	(0.0588)	(0.0600)
Fraction Aid x Year of	0.0172	0.0270	0.0374	0.00252	0.0115
	(0.0446)	(0.0464)	(0.0505)	(0.0577)	(0.0599)
Fraction Aid x 1 Years After	0.0268	0.0369	0.0483	0.0329	0.0419
	(0.0448)	(0.0469)	(0.0501)	(0.0600)	(0.0619)
Fraction Aid x 2 Years After		0.0501	0.0615	0.0780	0.0870
		(0.0481)	(0.0498)	(0.0602)	(0.0624)
Fraction Aid x 3 Years After			0.0310	0.0460	0.0551
			(0.0449)	(0.0538)	(0.0555)
Fraction Aid x 4 Years After				-0.0380	-0.0289
				(0.0594)	(0.0614)
Fraction Aid x 5 Years After					0.0176
					(0.0253)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Percent of Regions	100	100	92.12	77.43	75.12
Observations	9705	9705	8940	7515	7290

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Controls included are lag lights, log of population, and precipitation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13. Parallel Trends Test (State-level)

	(1) Onset	(2) Onset	(3) Onset	(4) Onset	(5) Onset
Fraction Aid x 6 Years Before					0.0594
					(0.0448)
Fraction Aid x 5 Years Before				-0.0178	-0.00921
				(0.0384)	(0.0392)
Fraction Aid x 4 Years Before			0.0429	0.0379	0.0465
			(0.0479)	(0.0442)	(0.0454)
Fraction Aid x 3 Years Before		0.0233	0.0260	0.0205	0.0288
		(0.0295)	(0.0305)	(0.0321)	(0.0342)
Fraction Aid x 2 Year Before	-0.0333	-0.0274	-0.0228	-0.0135	-0.00528
	(0.0419)	(0.0420)	(0.0433)	(0.0510)	(0.0516)
Fraction Aid x 1 Year Before	-0.0108	-0.00484	-0.0000783	0.00886	0.0172
	(0.0369)	(0.0371)	(0.0380)	(0.0422)	(0.0431)
Fraction Aid x Year of	-0.00284	0.00314	0.00679	-0.0479	-0.0396
	(0.0423)	(0.0433)	(0.0456)	(0.0443)	(0.0457)
Fraction Aid x 1 Years After	0.0429	0.0492	0.0552	0.0576	0.0662
	(0.0414)	(0.0417)	(0.0432)	(0.0484)	(0.0491)
Fraction Aid x 2 Years After		0.0488	0.0544	0.0694*	0.0779*
		(0.0355)	(0.0358)	(0.0388)	(0.0407)
Fraction Aid x 3 Years After			0.0193	0.0306	0.0392
			(0.0440)	(0.0487)	(0.0494)
Fraction Aid x 4 Years After				-0.0524	-0.0438
				(0.0402)	(0.0408)
Fraction Aid x 5 Years After					0.0209
					(0.0173)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Percent of Regions	98.15	98.15	90.57	75.89	73.57
Observations	8813	8813	8159	6880	6657

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Controls included are lag lights, log of population, and precipitation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14. Parallel Trends Test (County-level)

	(1) Incidence	(2) Incidence	(3) Incidence	(4) Incidence	(5) Incidence
Fraction Aid x 6 Years Before					-0.0000286
					(0.0112)
Fraction Aid x 5 Years Before				0.00652	0.00506
				(0.00985)	(0.0103)
Fraction Aid x 4 Years Before			0.0246**	0.00350	0.00147
			(0.00973)	(0.00988)	(0.0102)
Fraction Aid x 3 Years Before		0.0120	0.0170*	-0.00264	-0.00410
		(0.00912)	(0.00971)	(0.00990)	(0.0103)
Fraction Aid x 2 Year Before	0.0165**	0.0182**	0.0247**	-0.000686	-0.00445
	(0.00787)	(0.00820)	(0.00986)	(0.00984)	(0.0101)
Fraction Aid x 1 Year Before	0.0423***	0.0440***	0.0550***	0.0280***	0.0273**
	(0.00843)	(0.00877)	(0.0105)	(0.0107)	(0.0111)
Fraction Aid x Year of	0.0286***	0.0303***	0.0386***	0.00231	0.00123
	(0.00797)	(0.00835)	(0.0100)	(0.00958)	(0.00992)
Fraction Aid x 1 Years After	-0.00117	0.000589	0.00435	-0.00816	-0.00938
	(0.00768)	(0.00791)	(0.00948)	(0.0101)	(0.0104)
Fraction Aid x 2 Years After		0.00940	0.0147*	0.00689	0.00553
		(0.00729)	(0.00889)	(0.00969)	(0.0100)
Fraction Aid x 3 Years After			0.0180*	0.0119	0.0101
			(0.00946)	(0.0101)	(0.0104)
Fraction Aid x 4 Years After				-0.00447	-0.00627
				(0.00833)	(0.00871)
Fraction Aid x 5 Years After					-0.0112
					(0.00776)
State-Year FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Percent of Regions	100	100	93.21	78.24	77.54
Observations	101595	101595	94695	79485	78780

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Controls included are lag lights, log of population, and precipitation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15. Parallel Trends Test (County-level)

	(1) Onset	(2) Onset	(3) Onset	(4) Onset	(5) Onset
Fraction Aid x 6 Years Before					-0.00743
					(0.00969)
Fraction Aid x 5 Years Before				0.00319	0.00183
				(0.00758)	(0.00775)
Fraction Aid x 4 Years Before			0.0181**	-0.000523	-0.00249
			(0.00824)	(0.00737)	(0.00757)
Fraction Aid x 3 Years Before		0.0113	0.0147*	0.00545	0.00425
		(0.00804)	(0.00818)	(0.00842)	(0.00886)
Fraction Aid x 2 Year Before	0.0105	0.0121*	0.0164**	0.00344	-0.000206
	(0.00685)	(0.00688)	(0.00813)	(0.00777)	(0.00777)
Fraction Aid x 1 Year Before	0.0236***	0.0252***	0.0322***	0.0246***	0.0240***
	(0.00690)	(0.00693)	(0.00816)	(0.00859)	(0.00877)
Fraction Aid x Year of	0.00551	0.00714	0.0108*	-0.00414	-0.00512
	(0.00534)	(0.00541)	(0.00644)	(0.00579)	(0.00615)
Fraction Aid x 1 Years After	-0.00579	-0.00411	-0.00229	-0.00727	-0.00830
	(0.00569)	(0.00561)	(0.00662)	(0.00701)	(0.00719)
Fraction Aid x 2 Years After		0.00905	0.0132*	0.0104	0.00915
		(0.00579)	(0.00684)	(0.00735)	(0.00752)
Fraction Aid x 3 Years After			0.0145*	0.0132*	0.0120
			(0.00755)	(0.00797)	(0.00832)
Fraction Aid x 4 Years After				-0.00446	-0.00559
				(0.00577)	(0.00599)
Fraction Aid x 5 Years After					-0.000283
					(0.00676)
State-Year FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Percent of Regions	99.84	99.84	93.08	78.10	77.41
Observations	99402	99402	92690	77733	77028

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Controls included are lag lights, log of population, and precipitation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16. Average Elevation 2SLS Estimates (State-level)

	(1)	(2)	(3)	(4)	(5)	(6)
	Incidence: Low	Incidence: Low	Incidence: Low	Incidence: High	Incidence: High	Incidence: High
Aid Amount (Pop)	-0.000443			0.00178		
	(0.00502)			(0.00410)		
Aid Amount (Loc)		-0.000412			0.00174	
		(0.00467)			(0.00399)	
Any Aid			-0.00561			0.0237
			(0.0636)			(0.0541)
Log Population	0.0846	0.0843	0.0843	0.158**	0.159**	0.159**
	(0.0583)	(0.0579)	(0.0578)	(0.0724)	(0.0727)	(0.0725)
Precipitation	0.0310	0.0311	0.0311	0.0000980	0.000167	0.000463
	(0.0234)	(0.0234)	(0.0234)	(0.0461)	(0.0462)	(0.0462)
Lag Lights	0.0264	0.0263	0.0262	-0.0287	-0.0288	-0.0283
	(0.0285)	(0.0284)	(0.0283)	(0.0377)	(0.0377)	(0.0372)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	135.7	141.7	184.7	139.3	147.6	207.2
Observations	5070	5070	5070	4590	4590	4590

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17. Average Elevation 2SLS Estimates (State-level)

	(1) Onset: Low	(2) Onset: Low	(3) Onset: Low	(4) Onset: High	(5) Onset: High	(6) Onset: High
Aid Amount (Pop)	-0.00424			0.000430		
	(0.00408)			(0.00277)		
Aid Amount (Loc)		-0.00394			0.000419	
		(0.00378)			(0.00270)	
Any Aid			-0.0529			0.00565
			(0.0506)			(0.0363)
Log Population	0.0615*	0.0595	0.0590	0.0360	0.0364	0.0362
	(0.0369)	(0.0365)	(0.0363)	(0.0422)	(0.0425)	(0.0424)
Precipitation	0.0327	0.0332	0.0330	-0.0427	-0.0427	-0.0426
	(0.0209)	(0.0209)	(0.0208)	(0.0463)	(0.0463)	(0.0463)
Lag Lights	0.0357**	0.0353**	0.0345**	-0.0183	-0.0183	-0.0182
	(0.0170)	(0.0169)	(0.0168)	(0.0309)	(0.0308)	(0.0306)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	94.20	100.5	134.6	138.7	146.1	199.6
Observations	4592	4592	4592	4152	4152	4152

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18. Aid Reduces Conflict Incidence in High-Elevation Counties (2SLS LPM Estimates)

	(1) Incidence: Low	(2) Incidence: Low	(3) Incidence: Low	(4) Incidence: High	(5) Incidence: High	(6) Incidence: High
Aid Amount (Pop)	0.00149			-0.00749***		
	(0.00165)			(0.00231)		
Aid Amount (Loc)		0.00143			-0.00723***	
		(0.00157)			(0.00224)	
Any Aid			0.0192			-0.0941***
			(0.0211)			(0.0288)
Log Population	-0.00222	-0.00163	-0.00193	0.00561	0.00304	0.00343
	(0.0410)	(0.0409)	(0.0409)	(0.0282)	(0.0282)	(0.0281)
Precipitation	0.000459	0.000533	0.000250	-0.0483***	-0.0488***	-0.0487***
	(0.0134)	(0.0134)	(0.0134)	(0.0163)	(0.0163)	(0.0162)
Lag Lights	0.000832	0.000812	0.000838	-0.00427	-0.00431	-0.00484
	(0.00535)	(0.00535)	(0.00535)	(0.00649)	(0.00649)	(0.00644)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	276.5	297.0	329.0	225.5	225.2	262.0
Observations	51690	51690	51690	48720	48720	48720

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19. Aid Reduces Conflict Onset in High-Elevation Counties (2SLS LPM Estimates)

	(1) Onset: Low	(2) Onset: Low	(3) Onset: Low	(4) Onset: High	(5) Onset: High	(6) Onset: High
Aid Amount (Pop)	0.000864			-0.00378***		
	(0.00102)			(0.00120)		
Aid Amount (Loc)		0.000827			-0.00365***	
		(0.000980)			(0.00115)	
Any Aid			0.0111			-0.0470***
			(0.0131)			(0.0148)
Log Population	0.0304**	0.0308**	0.0306**	0.00888	0.00765	0.00764
	(0.0154)	(0.0154)	(0.0154)	(0.0214)	(0.0215)	(0.0214)
Precipitation	0.00262	0.00266	0.00250	-0.0155	-0.0158	-0.0157
	(0.0115)	(0.0115)	(0.0115)	(0.0120)	(0.0120)	(0.0120)
Lag Lights	0.000981	0.000967	0.000976	-0.00135	-0.00137	-0.00167
	(0.00347)	(0.00347)	(0.00347)	(0.00378)	(0.00378)	(0.00376)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	270.0	289.0	320.5	199.3	199.0	235.7
Observations	50774	50774	50774	47456	47456	47456

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20. 2SLS LPM Estimates: Distance to Border (State-level)

	(1) Incidence: Close	(2) Incidence: Close	(3) Incidence: Close	(4) Incidence: Far	(5) Incidence: Far	(6) Incidence: Far
Aid Amount (Pop)	0.00575			-0.00270		
	(0.00459)			(0.00406)		
Aid Amount (Loc)		0.00553			-0.00255	
		(0.00440)			(0.00385)	
Any Aid			0.0726			-0.0355
			(0.0571)			(0.0535)
Log Population	0.128*	0.131*	0.130*	0.154**	0.153**	0.153**
	(0.0671)	(0.0674)	(0.0674)	(0.0724)	(0.0721)	(0.0719)
Precipitation	0.0290	0.0296	0.0302	0.0155	0.0161	0.0162
	(0.0252)	(0.0252)	(0.0253)	(0.0327)	(0.0328)	(0.0328)
Lag Lights	-0.0404	-0.0399	-0.0382	0.0237	0.0236	0.0231
	(0.0268)	(0.0266)	(0.0261)	(0.0337)	(0.0336)	(0.0336)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	89.69	94.52	133.1	195.8	210.0	286.7
Observations	4800	4800	4800	4860	4860	4860

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21. 2SLS LPM Estimates: Distance to Border (State-level)

	(1) Onset: Close	(2) Onset: Close	(3) Onset: Close	(4) Onset: Far	(5) Onset: Far	(6) Onset: Far
Aid Amount (Pop)	0.000699			-0.00292		
	(0.00296)			(0.00339)		
Aid Amount (Loc)		0.000678			-0.00274	
		(0.00287)			(0.00319)	
Any Aid			0.00878			-0.0378
			(0.0371)			(0.0439)
Log Population	0.0501	0.0504	0.0504	0.0558	0.0537	0.0542
	(0.0362)	(0.0364)	(0.0364)	(0.0450)	(0.0447)	(0.0445)
Precipitation	-0.00403	-0.00402	-0.00386	0.0257	0.0264	0.0265
	(0.0242)	(0.0242)	(0.0242)	(0.0298)	(0.0298)	(0.0298)
Lag Lights	-0.00842	-0.00834	-0.00814	0.0283	0.0282	0.0277
	(0.0200)	(0.0199)	(0.0196)	(0.0217)	(0.0217)	(0.0217)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	85.86	91.11	126.2	147.5	156.8	212.7
Observations	4396	4396	4396	4367	4367	4367

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22. Aid Reduces Conflict Incidence in Counties Close to the Border (2SLS LPM Estimates)

	(1) Incidence: Close	(2) Incidence: Close	(3) Incidence: Close	(4) Incidence: Far	(5) Incidence: Far	(6) Incidence: Far
Aid Amount (Pop)	-0.00409**			-0.00186		
	(0.00197)			(0.00188)		
Aid Amount (Loc)		-0.00385**			-0.00181	
		(0.00185)			(0.00183)	
Any Aid			-0.0487**			-0.0247
			(0.0232)			(0.0250)
Log Population	0.00781	0.00616	0.00645	-0.0153	-0.0159	-0.0154
	(0.0250)	(0.0249)	(0.0248)	(0.0368)	(0.0369)	(0.0368)
Precipitation	-0.00940	-0.00949	-0.00957	-0.00795	-0.00808	-0.00778
	(0.0220)	(0.0220)	(0.0220)	(0.0106)	(0.0106)	(0.0106)
Lag Lights	-0.00164	-0.00167	-0.00178	-0.00886	-0.00884	-0.00887
	(0.00376)	(0.00375)	(0.00375)	(0.00795)	(0.00795)	(0.00795)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	174.6	187.5	229.2	375.3	380.2	408.2
Observations	50040	50040	50040	50820	50820	50820

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 23. Aid Reduces Conflict Onset in Counties Close to the Border (2SLS LPM Estimates)

	(1) Onset: Close	(2) Onset: Close	(3) Onset: Close	(4) Onset: Far	(5) Onset: Far	(6) Onset: Far
Aid Amount (Pop)	-0.00267**			-0.000654		
	(0.00119)			(0.00108)		
Aid Amount (Loc)		-0.00251**			-0.000638	
		(0.00112)			(0.00105)	
Any Aid			-0.0316**			-0.00869
			(0.0141)			(0.0143)
Log Population	0.0124	0.0113	0.0114	0.00716	0.00695	0.00716
	(0.0186)	(0.0187)	(0.0186)	(0.0180)	(0.0180)	(0.0180)
Precipitation	0.00816	0.00808	0.00800	0.000159	0.000113	0.000218
	(0.0171)	(0.0171)	(0.0171)	(0.00934)	(0.00935)	(0.00934)
Lag Lights	-0.00228	-0.00230	-0.00236	0.000120	0.000131	0.000121
	(0.00252)	(0.00252)	(0.00251)	(0.00463)	(0.00463)	(0.00463)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	162.9	174.4	216.1	348.7	353.2	378.6
Observations	48962	48962	48962	49738	49738	49738

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 24. 2SLS Estimates by Area (State-level)

	(1) Incidence: Small	(2) Incidence: Small	(3) Incidence: Small	(4) Incidence: Big	(5) Incidence: Big	(6) Incidence: Big
Aid Amount (Pop)	0.00166			0.000337		
	(0.00375)			(0.00513)		
Aid Amount (Loc)		0.00156			0.000331	
		(0.00354)			(0.00503)	
Any Aid			0.0215			0.00448
			(0.0485)			(0.0682)
Log Population	0.124*	0.125*	0.126*	0.0615	0.0616	0.0615
	(0.0696)	(0.0691)	(0.0689)	(0.0613)	(0.0617)	(0.0614)
Precipitation	0.00218	0.00173	0.00202	0.0335	0.0335	0.0335
	(0.0225)	(0.0225)	(0.0225)	(0.0358)	(0.0358)	(0.0358)
Lag Lights	0.0181	0.0182	0.0186	-0.0463	-0.0463	-0.0462
	(0.0209)	(0.0208)	(0.0206)	(0.0439)	(0.0438)	(0.0438)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	216.4	234.6	317.9	73.48	74.70	103.0
Observations	4620	4620	4620	4995	4995	4995

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 25. 2SLS Estimates by Area (State-level)

	(1) Onset: Small	(2) Onset: Small	(3) Onset: Small	(4) Onset: Big	(5) Onset: Big	(6) Onset: Big
Aid Amount (Pop)	-0.00104			-0.00134		
	(0.00280)			(0.00398)		
Aid Amount (Loc)		-0.000987			-0.00132	
		(0.00265)			(0.00393)	
Any Aid			-0.0135			-0.0177
			(0.0361)			(0.0524)
Log Population	0.0660	0.0651	0.0647	0.00921	0.00839	0.00907
	(0.0461)	(0.0459)	(0.0458)	(0.0396)	(0.0402)	(0.0397)
Precipitation	-0.00309	-0.00282	-0.00303	0.0161	0.0162	0.0162
	(0.0187)	(0.0186)	(0.0186)	(0.0371)	(0.0371)	(0.0371)
Lag Lights	0.0350**	0.0349**	0.0346**	-0.0392	-0.0395	-0.0397
	(0.0156)	(0.0155)	(0.0153)	(0.0343)	(0.0343)	(0.0343)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	186.7	202.6	268.4	58.15	59.09	85.47
Observations	4389	4389	4389	4295	4295	4295

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 26. Aid Reduces Conflict Incidence in Smaller Counties (2SLS LPM Estimates)

	(1) Incidence: Small	(2) Incidence: Small	(3) Incidence: Small	(4) Incidence: Big	(5) Incidence: Big	(6) Incidence: Big
Aid Amount (Pop)	-0.00741***			0.000844		
	(0.00282)			(0.00157)		
Aid Amount (Loc)		-0.00704***			0.000818	
		(0.00267)			(0.00152)	
Any Aid			-0.0873***			0.0111
			(0.0328)			(0.0207)
Log Population	-0.00782	-0.00880	-0.00832	-0.000190	0.000222	0.0000329
	(0.0407)	(0.0406)	(0.0405)	(0.0317)	(0.0316)	(0.0317)
Precipitation	-0.00799	-0.00879	-0.00842	-0.0167	-0.0166	-0.0167
	(0.0123)	(0.0122)	(0.0122)	(0.0132)	(0.0132)	(0.0132)
Lag Lights	-0.000315	-0.000349	-0.000266	-0.0210**	-0.0210**	-0.0210**
	(0.00435)	(0.00434)	(0.00431)	(0.00949)	(0.00949)	(0.00948)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	102.9	110.9	141.0	465.6	464.3	511.0
Observations	48630	48630	48630	51705	51705	51705

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 27. 2SLS Estimates by Area (County-level)

	(1) Onset: Small	(2) Onset: Small	(3) Onset: Small	(4) Onset: Big	(5) Onset: Big	(6) Onset: Big
Aid Amount (Pop)	-0.00279			-0.000356		
	(0.00175)			(0.000771)		
Aid Amount (Loc)		-0.00266			-0.000345	
		(0.00166)			(0.000747)	
Any Aid			-0.0331			-0.00467
			(0.0206)			(0.0101)
Log Population	0.0141	0.0137	0.0140	0.0181	0.0179	0.0180
	(0.0179)	(0.0179)	(0.0179)	(0.0168)	(0.0169)	(0.0168)
Precipitation	0.00440	0.00412	0.00424	-0.00648	-0.00649	-0.00644
	(0.00926)	(0.00924)	(0.00924)	(0.0113)	(0.0113)	(0.0113)
Lag Lights	0.000412	0.000402	0.000435	-0.00404	-0.00404	-0.00406
	(0.00285)	(0.00285)	(0.00284)	(0.00530)	(0.00530)	(0.00529)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat	100.2	107.7	135.2	425.8	423.3	472.3
Observations	48172	48172	48172	49956	49956	49956

Standard errors in parentheses

Standard errors are robust to heteroskedasticity and clustered at the region level. Aid amount is the inverse hyperbolic sine of raw disbursements. Lag Lights are inverse hyperbolic sine transformed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$