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Aid and the Rise and Fall of Conflict in the Muslim World

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ABSTRACT

The conflict following the Arab Spring is not the first wave of civil war in the Muslim world in recent time. From the mid-1980s to the end of the century, an average of one in 10 predominantly-Muslim countries experienced violent civil war in any given year. We provide a partial explanation for this statistic: a foreign aid windfall to poor, non-oil producing Muslim countries during the twin oil crises of the 1970s allowed the recipient states to become more repressive and stave off rebellion. When oil prices fell in the mid-1980s, the windfall ended, and the recipient countries experienced a significant uptick in civil war. To provide a causal interpretation we leverage a quasi-natural experiment of oil price induced aid disbursements which favored Muslim countries over non-Muslim countries. Our empirical findings are consistent with existing theories that foreign aid can "buy" stability.

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1 Introduction

British Foreign Secretary William Hague has called the wave of revolutions and civil war that swept across the Muslim world beginning in 2010 the most important event of the twenty-first century (Wintour and Watt, 2011). According to the United Nations, the ousting of the Qaddafi regime in Libya resulted in at least 30,000 fatalities (Laub, 2011), while the death toll in the insurrection against the Assad government in Syria has surpassed at least 60,000 since the fighting broke out in March 2011 (UNHCR, 2013). These political events in the Arab world have been compared to both the failed European revolutions of 1848 and the successful ones of 1989 (Springborg, 2011).

Yet this conflict, on the heels of the Arab Spring, is not the first wave of violent political action in the Muslim world in recent time. From the late 1980s to the end of the century, eight predominantly-Muslim countries experienced non-internationalized civil war of at least 1,000 battle deaths per year. (If the threshold is reduced to 25 battle deaths, the count rises to 21 countries.) As Figure 1 demonstrates, most of this activity was among non-oil producers, whose average propensity for heavy conflict was around 10% in any given year. In contrast, Muslim oil-producers have remained relatively stable.

In this article we offer a partial explanation for this relatively forgotten rise in conflict. We argue that foreign aid to developing, non-oil producing Muslim countries during the twin oil crises of the 1970s allowed the recipient states to stave off rebellions and maintain political stability. When oil prices fell in the mid-1980s, the windfall ended, and the recipient countries experienced a significant uptick in civil war. We use a difference-in-differences and instrumental variable research designs and make the case that the relationship is causal. Our empirical strategy exploits plausibly exogenous variation in world oil prices to

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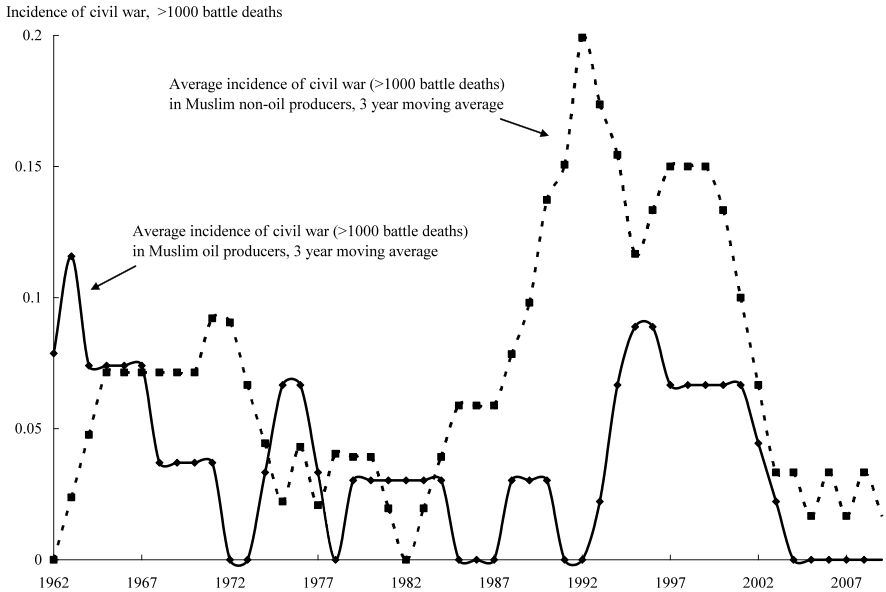

Figure 1: Conflict in Muslim countries.

explain variation in foreign aid inflows to Muslim non-oil producers; we show that this oil price-induced aid lowered the propensity of civil war (relative to non-Muslim non-oil producers).[1] We focus on Muslim aid recipients because they were the primary beneficiaries of the oil price induced aid disbursements from Gulf oil producers (e.g., Hunter, 1984; Kepel, 2002; Neumayer, 2003).

Our analysis begins with a series of transparent figures (with accompanying difference-in-differences estimates) that graphically demonstrate how our explanation linking aid to reduced conflict in Muslim non-oil producers is borne out in the underlying raw data. We follow this with more rigorous statistical analysis that confirms the trends in these figures. To mitigate concerns with endogeneity bias that often plagues cross-national studies of conflict we employ an instrumental variable strategy (Blattman and Miguel, 2010).

[1]Variants of this quasi-natural experiment have been used to study the effect of aid on macroeconomic variables (Werker *et al.*, 2009), as well as the impact of unearned foreign income (aid and remittances) on government survival (Ahmed, 2012). Neither of these studies examines the effect of aid on civil war.

In contrast to the usual instrumental variable approaches on the effect of aid (see Roodman, 2007), our two-stage least squares (2SLS) results gauge the within-country variation in conflict explained by oil-induced aid inflows. In particular, our instrument measures how exogenous changes in world oil prices are "propagated" to Muslim non-oil producers (relative to non-Muslim non-oil producers) via the aid channel. Our specifications control for the most prevalent explanations of civil war, such as economic growth, income, political institutions, ethnic fragmentation (and other time-invariant factors, such as geography, with country-fixed effects), and common temporal "shocks" (such as the Cold War with year fixed effects). Leveraging this quasi-natural experimental setup mitigates concerns associated with endogeneity bias and offers a unique and rare opportunity to gauge the effects of foreign aid on political stability. As we discuss later, our findings are robust to competing explanations arising from other financial flows (e.g., remittances) and the external influence of Arab oil producers.

Moreover, while our causal inferences apply to Muslim non-oil producers, the substantive implications can be generalized since we are careful to control for many time-varying and time-invariant (both observable and unobservable) effects associated with Islam that may differentially affect the incidence of conflict between Muslim and non-Muslim countries. These include country fixed effects (that account for any time-invariant "Muslim" effect), various differential trends (e.g., Muslim $\times$ Year, Muslim $\times$ Cold War), as well as different underlying sources of instability that may inflict Muslim countries relative to non-Muslim countries (e.g., economic development and structure, growth, population, level of democracy). In doing so, we can attempt — to the extent that the data permit — to isolate the causal effect of aid on conflict.

The relationship between foreign aid and political stability that we analyze cross-nationally in this article is starkly apparent in the case of Siad Barre's Somalia. From 1969 through 1990 Somalia received, on average, foreign aid equal to 18.5% of its GDP — from the Soviet Union, then the United States, plus the Arab League and the United Nations, among others (Besteman, 1996, p. 581). Following the loss of Soviet support, Barre dropped a scientific socialist platform and adopted clanism as a method for maintaining power. With increasing foreign aid (with a large share coming from Gulf donors), Barre favored the clans in his inner circle and bombed his rival clan's strongholds.

As the price of oil fell in the mid-1980s and Gulf aid declined, this policy would ultimately lead to his destruction as the excluded clans expressed their dissatisfaction with Barre's policy of divide-and-rule through armed insurgency. "Foreign aid," observed one Somalia scholar, "provided the glue that held the system together in spite of internal waste and corruption" (Adam, 1999, p. 175). Similar dynamics unfolded in other Muslim non-oil producers as we show in the empirical sections.

Our article advances the political economy literature in international relations, comparative politics, and development in a number of ways. First, we build on recent scholarship describing how negative aid shocks can contribute to civil war (e.g., Nielsen *et al.*, 2011). Our study, however, differs on several dimensions, as we show that higher levels of aid "buy" political stability, while subsequent declines engender a heightened incidence of conflict. As such we provide a dynamic account for the impact of aid on political stability. Second, we leverage a quasi-natural experimental setting — a rarity in international relations — to provide rigorous causal evidence linking foreign aid to civil war.[2] Third, the article is related to the literature on the resource curse and unearned rents, including foreign aid (Bueno de Mesquita and Smith, 2010; Robinson *et al.*, 2006; Ross, 2001). Fourth, by investigating the link between aid windfalls and conflict, the article contributes to our knowledge of the political consequences of aid and economic development more broadly (Bermeo, 2011; Besley and Persson, 2011; Collier and Hoeffler, 2002; Djankov *et al.*, 2008; Morrison, 2009; Przeworski *et al.*, 2000).

2 Aid and Conflict

Our empirical analysis is well situated in the literature on foreign aid and conflict. However, the theoretical expectations and empirical findings associated with aid and conflict remain mixed. On the one-hand, foreign aid is often viewed as a form of unearned government

[2]We are rigorous in our research design, for example, by showing that our treatment group of countries (i.e., Muslim non-oil producers) were "similar" to the control group of countries (i.e., non-Muslim non-oil producers) prior to the treatment (i.e., oil-priced aid shock). Moreover, as we show in the paper, our findings are robust to a variety concerns, such as unobserved spatial and temporal heterogeneity (e.g., effect of Cold War, region specific differential trends, etc.), alternate specifications and classifications of our treatment and control groups.

income that encourages rent-seeking behavior and predation. These accounts posit that unearned government income increases the "size of the pie," and if there are multiple groups dividing the pie, rent-seeking can contribute to increased fighting over it (Besley and Persson, 2011; Grossman, 1992). These ideas have been formalized in various models. Svensson (2000), for instance, develops a repeated game with stochastic shocks, where the increase in rent-seeking behavior arises due to coordination failure across the interest groups. Similarly, Hodler (2006) constructs a game-theoretic model where a surge in exogenous financial windfalls to the government (e.g., foreign aid, oil rents) fosters Cournot-type competition in the form of group fighting in ethnically fragmented societies and a subsequent decline in economic growth. As applied to autocratic states where groups are less likely to attain their share of the pie through non-violent political channels, a financial windfall is likely to raise internal domestic discontent. Consequently, higher levels of aid may encourage conflict.

Other studies, however, show that aid reduces the incidence of civil war by fostering economic growth and strengthening states' capabilities (Collier and Hoeffler, 2002; Miguel *et al.*, 2004). Inflows of aid have been situated within the rich literature on the resource curse and rentier state that suggests a causal link between financial windfalls (e.g., natural resource rents) and greater political stability (Bueno de Mesquita and Smith, 2010; Morrison, 2009; Ross, 2001). In these accounts, these windfalls constitute a source of non-tax income that can permit a government to behave less accountably to its population but still have sufficient funds to maintain support from its relevant constituents (e.g., military). The logic of the rentier state argument has been applied to unearned foreign income, most notably foreign aid. Moore (1998), for example, argues that as the share of government income from unearned income increases, state/society relations are less likely to be "characterized by accountability, responsiveness, and democracy" (p. 85). As a source of non-tax income, foreign aid can finance patronage, especially in autocracies where the "winning coalition" to maintain political stability is small (Bueno de Mesquita *et al.*, 2003).

Aid inflows may also facilitate political stability by stalling potentially destabilizing political reform (Casella and Eichengreen, 1996). In particular aid inflows may permit certain governments — especially

those with the incentive to do so, such as poor autocracies — to delay privately-costly (i.e., to the stability of the incumbent government) but potentially beneficial political reforms. Additionally, aid may mitigate conflict by providing a "cushion" to government spending from the downward pressures of negative economic shocks. Consequently, aid can provide governments with the resources necessary to make rebellion less attractive for opposition groups (Savun and Tirone, 2012).

A corollary to these arguments is that declines in aid can engender conflict. Nielsen *et al.* (2011) argue and leverage matching techniques to provide cross-national evidence that negative aid "shocks" accelerate the likelihood of civil war. Citing the commitment problem articulated by Powell (2004), Nielsen *et al.* argue that a severe decrease in government revenues (stemming from a rapid decline in foreign aid) can weaken the central government's capacity to effectively "buy off" rebel groups and contribute to "inefficient conflict."

Building on these insights, Ahmed and Werker (2012) develop a formal model between an incumbent government and rebel group(s) over the "state prize," including access to unearned government income. In their model they show a high level of aid raises the stakes for conflict, but makes an incumbent more capable of suppressing a revolt. As a result, the rebel group will not even try challenging the incumbent because the group will lose. At this higher level of aid, the incumbent can finance greater repression. In periods in which aid declines to a more moderate but still lucrative level, there is some greater chance for the opposition to win and a higher incidence of civil war.

These predictions of (1) less conflict during high periods of aid and (2) greater conflict during periods of reduced aid provides an analytical framework to understand the dynamics of aid and conflict observed in many Muslim aid recipients. We now turn to the empirics to provide causal evidence of such a relationship.

3 Identification Strategy

3.1 Quasi-Natural Experiment

Beginning in 1973, when the price of oil began to skyrocket, the Gulf nations of the Organization of the Petroleum Exporting Countries (OPEC) were extremely generous in their distribution of aid. Estimates

by Neumayer (2003, p. 134) suggest these countries doled out 1.5% of their GDP between 1974 and 1994, which amounted to 13.5% of all aid given out over this period. Neumayer (2003) provides econometric evidence that Gulf donors favored Muslim countries in the disbursement of this aid. No doubt some motivation was political: the Gulf countries were trying to quell unrest due to the huge inequality among their co-religionists (between the oil haves and have-notes), as well as to "assure them[selves] a clear position of dominance within the Muslim world" (Kepel, 2002, pp. 69–70).

During the oil price boom, the aid these countries received looked like unearned income flowing to the state. Unlike foreign aid from the World Bank, for example, donors in the Gulf gave their money with comparatively few strings attached. Most aid was in the form of block grants to finance ministries. According to Hunter (1984): "the largest part of OPEC aid has still consisted of general balance of payment and budgetary support." Hallwood and Sinclair concur: "Most OPEC aid is given on very favourable terms and conditions from the recipient's point of view. A large proportion of this aid is given on a grant basis, otherwise loan terms are with low interest rates and long grace and amortisation periods" (1981, pp. 100–101). And while aid from Western donors has often been tied towards contracts with the donor country, "Arab aid has practically never been tied, with the exception of relatively unimportant specific loans and grants for oil purchases" (Neumayer, 2002, p. 15).

Two facts about the pattern of Gulf states' aid inform the basis of our research design. One, this aid favors other Arab and non-Arab Muslim recipients (Neumayer, 2003). Two, the aid is highly correlated with the price of oil. The programs only began in earnest following the oil crisis of 1973 and the aid fluctuations closely follow oil prices (Hallwood and Sinclair, 1981; Hunter, 1984). These two key facts form the basis of our quasi-natural experiment, in which Muslim countries were the unique recipients of an aid windfall that only lasted as long as the price of oil remained high. Since the aid was largely untied, and since the recipients were mostly non-democratic, this experiment should be of broader interest to scholars interested in the effect of unearned income in non-democratic regimes.

These two facts are apparent in the underlying data. Figure 2 plots average official development aid receipts across non-oil producing

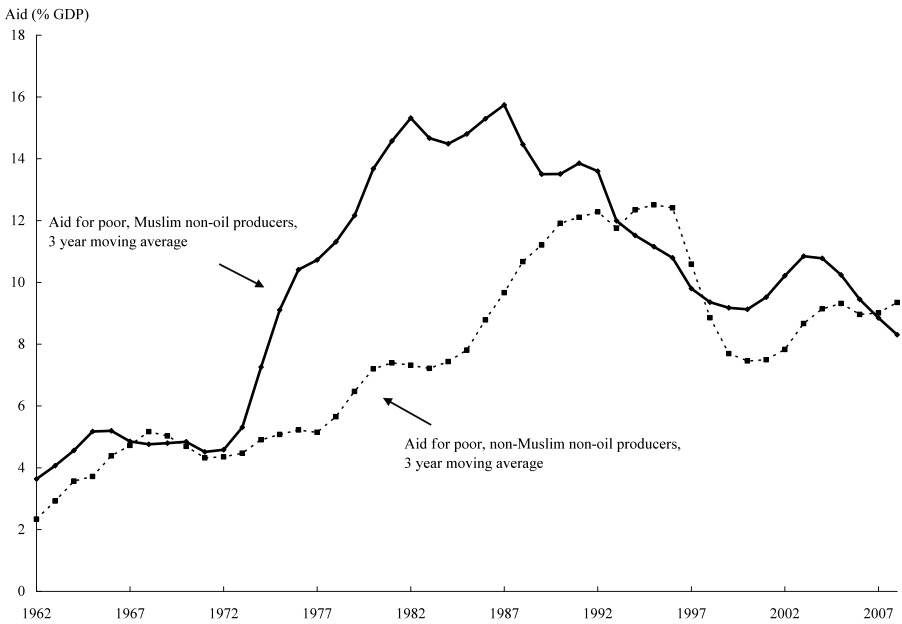

Figure 2: Foreign aid receipts by Muslim and non-Muslim countries, non-oil producers.

Muslim and non-Muslim countries since 1960.[3] This measure of foreign aid includes disbursements from DAC and non-DAC donors, which includes various Arab aid agencies (World Bank, 2011) (see Appendix A1 for the full definition). A country qualifies as Muslim if at least 70% of the population identify with the Islamic faith, although our findings are robust to alternate thresholds (see Table 4). As we show shortly, this

[3]Our measurement of the aid windfall here and throughout the paper uses *total ODA receipts* and, as such, does not require separating out Arab aid from non-Arab aid; the figures are therefore net of any changes in the disbursements of traditional donors or non-Arab, oil-producing donors. Unfortunately, bilateral aid data from specific Arab aid donors (e.g., Saudi Arabia, Kuwait, etc.) is unavailable, especially from the 1970s and 1980s. We restrict our analysis to a sample of developing countries (i.e., not "high income" according to the World Bank's classification) that do not produce any oil. Rich countries, of course, do not receive development assistance. Rather, they tend to be the donors. We exclude oil-producing countries (as defined by British Petroleum), since the impact of high oil prices will have a direct impact on a country's political economy that dwarfs any increase in foreign aid.

threshold ensures that the treatment group (Muslim non-oil producers) are comparable ("balanced") to the country group (non-Muslim non-oil producers) *prior* to the treatment period.

From 1960 to around 1973, average aid receipts in Muslim and non-Muslim countries were remarkably similar. This changed at the onset of the first oil shock. During the oil price boom between 1973 and until the mid-1980s, Muslim countries received substantially higher amounts of aid (percent of GDP) compared to non-Muslim countries. Over this period, on average, Muslim countries received 6.2 percentage points of GDP more aid than non-Muslim countries. Aid receipts in Muslim countries tapered off thereafter and for the most part since the early 1990s remained largely similar to inflows received in non-Muslim countries. As is apparent from the figure, Muslim countries experienced a windfall in foreign aid over the period that roughly corresponds to the years of high oil prices.

To show a clearer sense of this correlation, Figure 3 depicts movements in the price of oil (right axis) and superimposes the difference in foreign aid as a percentage of GDP between non-oil producing Muslim

Figure 3: Oil price and the aid windfall to Muslim countries, non-oil producers.

and non-Muslim countries (left axis). As is evident from the figure, the amount of bonus aid doled out to Muslim countries from Gulf donors tracks the price of oil. Moreover, difference-in-differences estimates from the high oil price period (1973–1985) and the surrounding low oil price periods (1960–1972, 1986–1999) between Muslim and non-Muslim countries reveal that these aid differentials are statistically significant and, at 5.8 percent of GDP (results reported in Appendix Table A5), nearly equal in magnitude to the first difference. The robust statistical relationship between oil price and aid tends to break down after 2000 due to a confluence of three potential causes in the aid allocation decision of Gulf oil producers: larger domestic populations requiring greater spending at home (rather than sending the government revenues abroad); the rise of sovereign wealth funds which encouraged saving; and higher allocations of aid to non-Muslim recipients from the traditional donors after 2000.[4] Thus, we limit our main analysis to the pre-2001 period.

3.2 Exogeneity and Pre-Treatment Balance

Figures 2 and 3 underlie our empirical strategy to identify the effect of aid on political stability. Such an identification strategy is attractive since the world price of oil should not differentially affect the internal economic and political conditions across similar non-oil producing aid recipients. In the lead-up to the first oil price shock, Muslim and non-Muslim countries did not differ significantly on observable economic (GDP per capita) and political (POLITY, executive constraints, and incidence of civil war) conditions. From a causal inference standpoint, this is noteworthy as it means that the typical Muslim non-oil producing aid recipient (our "treated" sample) was similar to non-Muslim oil producing aid recipients (our "control" sample) prior to the reception of the oil-price induced aid shock ("treatment"). Consequently, differences in political outcomes (e.g., civil war incidence) in the post-treatment period between Muslim and non-Muslim countries can be attributed to the treatment effect (i.e., oil priced induced aid inflows), conditional on other potential explanations (e.g., economic growth, global shocks, country-specific time invariant characteristics such as colonial legacy, etc.). These potential confounders are controlled for in our baseline 2SLS specifications. Moreover, to rigorously account for other factors that may

[4]Further details available from the authors upon request.

potentially explain divergences in outcomes in the post-treatment period, we also control for pre-existing conditions (e.g., political institutions in 1972) and interact them with the time-varying component of our instrument (i.e., oil prices). By including these controls in our baseline specification, we control for a large set of factors that might also explain the differential propensity of conflict between Muslim and non-Muslim non-oil producers. In doing so, we are able to isolate the causal effect of aid on conflict. And while our causal inferences apply to our treatment group of Muslim non-oil producers in the strictest sense, the implications of our empirical analysis are broadly generalizable to all aid recipients (since we control for a large set of time-varying and time-invariant characteristics).

4 Aid and Internal Conflict

4.1 Measuring Conflict

We now turn to the effect of the aid windfall on internal conflict in the recipient countries. To measure political violence, we use armed conflict data (ACD) from UCDP/PRIO spanning the period 1960–2008 (Gleditsch *et al.*, 2002; updated by Harbom *et al.*, 2008). ACD has been widely used in empirical studies of civil war (e.g., Besley and Persson, 2011; Collier *et al.*, 2003; Miguel *et al.*, 2004; Nielsen *et al.*, 2011). Following a number of prominent papers in the empirical literature on civil war, particularly recent ones with fixed effects, we evaluate the impact of aid on the incidence of conflict (e.g., Collier and Hoeffler, 1998; Elbadawi and Sambanis, 2000; Besley and Persson, 2011; Nunn and Qian, 2014). Our main threshold of civil war involves non-internationalized internal war involving at least 1,000 battle deaths per year (although the findings are robust to using the lower threshold of 25 battle deaths). This threshold level of violence is consistent with two-sided conflicts involving a government and well-armed rebel groups and has been used accordingly in related studies (e.g., Kalyvas and Balcells, 2010; Savun and Tirone, 2012). The measure of the incidence of civil war is a binary variable equal to one if the relevant battle death threshold has been met, and zero otherwise. (Appendix A describes all the variables and data sources used in this article.)

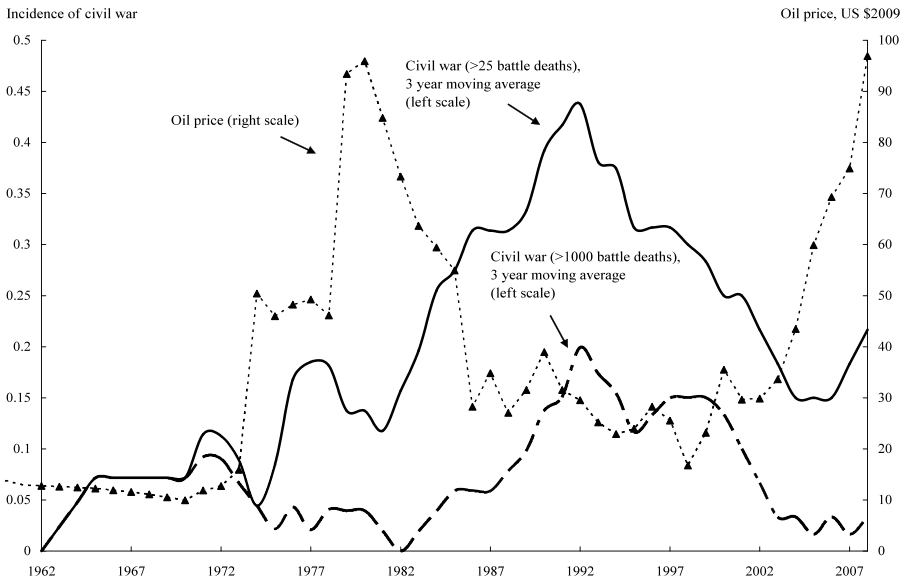

Figure 4: Incidence of civil war in non-oil producing Muslim aid recipients.

4.2 Trends in the Raw Data: Aid and Conflict Differentials

Figure 4 depicts the overall trends in oil prices and the incidence of civil war in our sample of developing, non-oil producing Muslim countries. At both thresholds of civil war, conflict remained relatively stable and low until the early 1980s during a period of high oil prices (and therefore higher aid inflows). The propensity for conflict in these countries picked up dramatically from 1982 through the late 1990s, when oil prices declined (and with them, aid inflows).

Since these recipients do not produce any oil, the positive correlation between oil prices and political stability (i.e., lower incidence of civil war) is unlikely to operate directly via oil prices, which might on their own go in the opposite direction as high commodity prices can lead to discontent and riots — as the 2008 food crisis showed so vividly. Rather, as we show below, it operates through the oil-price-induced foreign aid disbursed by Gulf oil producers to developing, non-oil-producing Muslim countries.

Figure 5 examines the relationship between the aid differential and the conflict differential across Muslim and non-Muslim countries in the

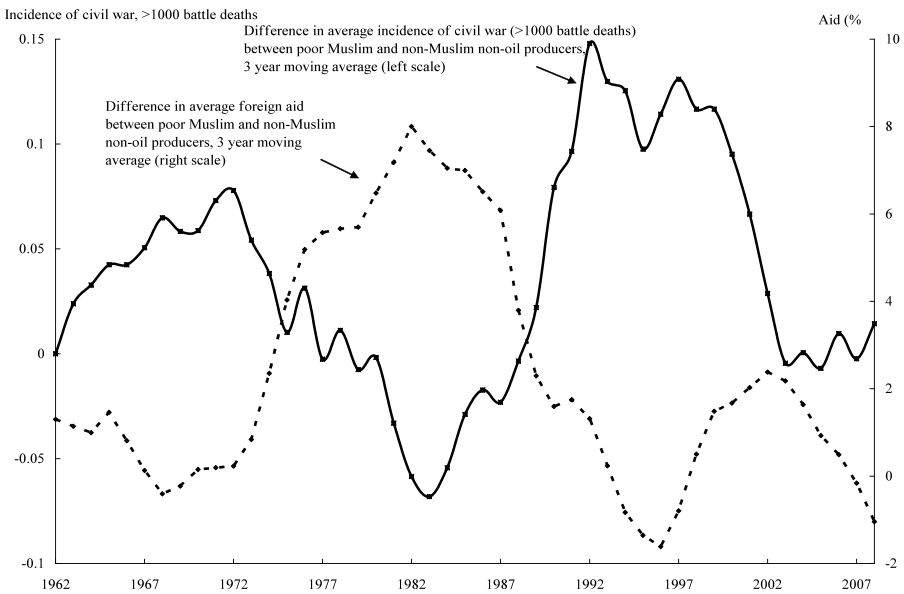

Figure 5: The Muslim/Non-Muslim foreign aid and conflict differentials, non-oil producers (at least 1,000 battle deaths per year).

sample. Muslim countries experienced less conflict than non-Muslim countries when they received comparatively more aid. As the aid differential reversed due to lower oil prices from the mid-1980s until the early 2000s, Muslim countries were substantially more likely to experience civil war.

The differential effects of aid on political violence are large and statistically significant. Table 1 presents difference-in-differences estimates for the incidence of civil war between periods of high and low oil prices and across Muslim and non-Muslim aid recipients. Prior to the oil crisis of 1973, Muslim countries were approximately 5 percentage points more likely than non-Muslim countries to experience civil war (although, this difference is not statistically significant). During the period of high oil prices, Muslim countries were 2 percentage points less likely than non-Muslim countries to be highly unstable. As the aid windfall went away, the propensity for civil war jumped dramatically in Muslim countries: it was 9 percentage points more likely than in non-Muslim countries.

Table 1: Incidence of Civil War in muslim and non-muslim non-oil producers, by periods of low and high oil prices.

	Non-muslim	Muslim	Difference	Std error	Difference-in-differences	Std error
Before: 1960–1972	0.02	0.06	0.04	(0.054)		
During: 1973–1985	0.05	0.03	−0.02	(0.022)	−0.06	(0.045)
After: 1986–1999	0.04	0.14	0.10	(0.065)	0.11	(0.058)
Post: 2000–2008	0.02	0.03	0.01	(0.027)	−0.08	(0.051)

Note: Standard errors are clustered by country reported in parentheses.

The difference-in-differences estimates imply that the aid windfall of 1973–1985 made Muslim countries 7 percentage points relatively more stable compared with the underlying propensity in non-Muslim countries. The end of the windfall engendered a relative rise in high intensity political violence as Muslim countries became 11 percentage points more likely to be engaged in two-sided violence, while non-Muslim countries became slightly more (around 1 percentage point) more stable. This effect is statistically significant (p-value = 0.054). For the period after 2000, when higher oil prices prevailed, the propensity for high intensity civil war fell in Muslim countries (relative to the prior period), but they were as stable as non-Muslim countries.

The validity of the difference-in-differences strategy assumes that there are parallel trends across Muslim and non-Muslim countries. Yet this may not be the case, for example, due to unobserved trends between these two groups of countries (e.g., due to differences in fertility) or differential trends by geographic region (e.g., regional "waves" of democratization). Fortunately, the difference-in-differences estimates in Table 1 are robust to the inclusion of a *Muslim* × *Year* trend and a vector of regional dummies interacted with a year trend as controls (e.g., *Africa* × *Year*, *Middle East* × *Year*, etc.). The former controls for any unobserved temporal trend between Muslim and non-Muslim countries,

while the latter accounts for unobserved temporal trends across regions (results reported in Table A6).

4.3 Instrumental Variable Results

4.3.1 Setup

While Figures 3 and 5 capture the underlying dynamics linking oil prices to aid inflows, and that aid to civil war, they do not control for recipient characteristics that might mediate the effect of aid on conflict (e.g., economic growth). To address this concern more rigorously, we use a 2SLS setup to identify the causal effect of oil prices on conflict via the aid channel and limit our analysis to developing countries that do not produce any oil. Specifically, we estimate the following reduced form setup:

$$\text{First stage: } \text{Aid}_{it} = \alpha + \beta^* \text{Muslim}_i \times \text{p(oil)}_t + \gamma^* X_{it} + \delta_i + \delta_t + \varepsilon_{it}$$
$$\text{Second stage: } \text{Civil War}_{it} = a + b^* \text{Aid}_{it} + c^* X_{it} + d_i + d_t + e_{it}$$

where Civil War$_{it}$ measures the incidence of high intensity civil war in country i in year t; Aid$_{it}$ is foreign aid (% GDP); Muslim$_i$ is a dummy variable equal to 1 if a country is Muslim; p(oil)$_t$ is the world price of oil in year t; X_{it} is a vector of time-varying recipient characteristics, δ_i and δ_t are a set of country and year fixed effects respectively, and ε_{it} is a stochastic error term. The standard errors are corrected for serial correlation with the Newey–West procedure (for efficiency purposes, due to the small effective sample size) for 1-period autocorrelation. We also run the results clustering at the level of the instrument, or *Muslim**year, and the standard errors shrink (results not reported).[5] So as to *not* estimate the *direct* effect of oil prices on conflict, our sample is restricted to 87 developing, non-oil producing countries. (Table A3 in the data appendix provides summary statistics.)

The instrument, which interacts whether an aid recipient is Muslim with the price of oil, identifies the differential effect of oil prices on aggregate foreign aid inflows (from both DAC and non-DAC donors) into Muslim countries (relative to non-Muslim countries), and thus

[5]This means that results we report using Newey–West standard errors are more *conservative* estimates of the effect of aid on conflict.

captures movements in the aid differential in Figure 3.[6] In essence, the instrument measures how exogenous changes in world oil prices are propagated to Muslim non-oil producers via the aid channel. The inclusion of country fixed effects in both stages controls for observed and unobserved time-invariant characteristics that affect a country's receipts of foreign aid (e.g., colonial relationship, distance), and propensity for conflict (e.g., ethnic fragmentation, past history of violence, geography), and subsumes the main effect of Muslim_i in the interaction term since a country's classification as Muslim is time-invariant. In the second stage regression, the inclusion of these fixed effects means the coefficient b gauges the within-country effect of aid on civil war. Finally, the inclusion of year fixed effects accounts for common temporal trends/shocks that impact all countries (e.g., oil shocks, Cold War) and subsumes the main effect of p(oil)_t.

The vector X_{it} contains recipient characteristics that affect both the allocation of aid in the first stage and incidence of civil war in the second stage, including: economic development (log GDP per capita), growth (GDP per capita growth), and population size (in logarithmic units). For example, existing studies find that poorer countries (e.g., Fearon and Laitin, 2003) and those experiencing slower (negative) growth are more prone to conflict (e.g., Savun and Tirone, 2012).

To account for the possibility that being a Muslim country proxies for structural demographic/economic and political conditions that might be differentially affected by the oil price shock, both regressions also control for the share of rural population in 1972 (i.e., prior to the treatment period) and whether the country was autocratic in 1972 (defined as a Polity2 score of −5 or lower), each interacted with the price of oil. The economic and demographic variables are drawn from World Bank (2011) and the measure of autocracy is from Marshall *et al.* (2010).

4.3.2 Core results

Table 2 presents the effect of oil price-induced aid flows on civil war. Columns 1–3 show that oil prices explain variation in aid receipts in

[6]Unfortunately, data on annual bilateral aid disbursements from non-DAC Gulf oil donors to specific countries are unavailable.

Table 2: Effect of oil price-induced aid on civil war.

Dependent variable:	Foreign aid (% GDP)		Incidence of civil war (at least 1000 battle deaths)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Method of estimation:	OLS Muslim	OLS Non-muslim	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Oil price (2009 US$)	0.07	0.005									
	(0.019)	(0.010)									
Muslim * Oil price			0.089								
			(0.018)								
Foreign aid (% GDP)				−0.022	−0.032	−0.022	−0.023	−0.013			
				(0.008)	(0.011)	(0.008)	(0.008)	(0.006)			
Foreign aid per capita									−0.001		
									(0.0005)		
Foreign aid										−0.0002	
										(0.00007)	
Log foreign aid											−0.287
											(0.099)
Remittances (% GDP)					0.006						
					(0.003)						
Assassinations						0.011					
						(0.006)					
Terrorist fatalities							0.0003				
							(0.0001)				
Recipient characteristics			Y	Y	Y	Y	Y	Y	Y	Y	Y
Country fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects			Y	Y	Y	Y	Y	Y	Y	Y	Y
F-statistic on instrument			25.28		22.58	25.25	26.07	12.85	10.97	29.81	11.45
Number of observations	701	2395	2277	2277	1279	2277	1933	2914	2178	2178	2172

Note: Newey-West corrected robust standard errors with upto 1 lag autocorrelation reported in parentheses. Recipient characteristics include: log GDP per capita, GDP per capita growth (% annual), log population, POLITY in 1972 × p(oil), and Percent rural × p(oil). These coefficients plus country and year fixed effects and a constant are not reported. Specification (8) estimated over full sample period (upto 2008) and instrument is adjusted for post-2000 period. In Specification (9), foreign aid per capita is measured in 2008 US$. In Specification (10), foreign aid is measured in millions of 2008 US$, and Specification (11) is equal to log(1+foreign aid).

Muslim non-oil producers only. Columns 1 and 2 capture the dynamics underlying the correlation between the aid differential and oil prices in Figure 3. In particular, for the sample restricted to Muslim non-oil producers, the price of oil has a robust positive and statistically significant effect on aid inflows (Column 1). In contrast, oil prices have an inconsequential effect on aid inflows in non-Muslim non-oil producers (Column 2). Combining the samples, Column 3 shows the first stage regression describing the effect of oil prices on foreign aid. The coefficient estimate on the instrument implies that oil prices have a positive and statistically significant effect in raising aid inflows to Muslim non-oil producers: a $10 increase in oil prices raises aid flows in Muslim recipients by nearly 0.9 percent of GDP. The instrument's F-statistic (25.28) exceeds the threshold for weak instruments of 10 (Staiger and Stock, 1997).

Turning to the second stage regression, Columns 4–11 show that instrumented aid lowers the incidence of high intensity conflict. In Column 4, a one percentage point increase in aid lowers the incidence of civil war by 2.2 percentage points; alternatively, a standard deviation change in aid in Muslim recipients lowers the incidence of violent civil war by about one standard deviation (equal to 18 percentage points).[7] Columns 5–7 add controls for remittances, assassinations, and terrorism-related fatalities, respectively, and serve as robustness checks for alternate explanations that we discuss in the penultimate section. The robust negative effect of aid on conflict holds in a sample through the period 2008 (Column 8) although the estimated effect is attenuated due to the breakdown in the oil price and aid relationship in the 2000s.

Finally, the results do not hinge on the measurement of foreign aid. Columns 9–11 show that aid has a negative effect on conflict when measured in per-capita terms (Column 9), in levels of US dollars (Column 10), and in logarithmic units (Column 11). The coefficient estimate in Column 9, for instance, implies that a $10 increase in aid per capita lowers the likelihood of conflict by 1 percentage point.

[7] In an alternate specification that clusters the standard errors by Muslim-year (which accounts for the potential correlation of errors within Muslim countries over time), the coefficient on aid is -0.021 (SE = 0.003).

4.3.3 Alternate specifications

The results in Table 2 hold across a variety of alternate specifications (results reported in Table 3). For instance, instrumented aid reduces the likelihood of civil war in a specification where the second stage is estimated via a probit regression (Column 1). Varying the mix of fixed effects does not affect the substantive findings either (Columns 2–4). Rather, the estimated effect of aid tends to be larger than the core results in Table 2. For example, in a specification without any country or year fixed effects (but which does appropriately control for the constitutive terms of the instrumental variable in both the first and second stage regressions), a 1 percentage point increase in aid (% GDP) lowers the incidence of civil war by 2.8 percent (Column 4). Thus our preferred fixed effect specifications should be viewed as conservative estimates of the causal effect of aid on conflict. Moreover, our results are robust in specifications that control for the potential budgetary effects of aid on conflict, such as a total government expenditures and fuel imports (results not reported).

Of course, there is no reason to assume that contemporaneous aid only has an effect on the incidence of civil war. For instance, some aid could be saved and used to stall rebellion in future years (Savun and Tirone, 2012). Columns 5–7 show that instrumented aid from 1, 2, and 4 years in the past exhibits a robust, negative effect on the incidence of contemporaneous conflict. Unsurprisingly, the effect tends to decay over time as the coefficient estimates decline in size as the lag length increases. From Table 2, for example, a one percentage increase in contemporaneous aid (% GDP) decreases the incidence of civil war by about 2.2 percentage points. In contrast, aid from 2 or 4 years in the past reduces the incidence of civil war in the current year by 1.9% and 1.1%, respectively. Finally, the core results also hold with 2 and 4 year moving averages of foreign aid receipts (Columns 8 and 9).[8]

4.3.4 Accounting for unobserved heterogeneity

A potential concern with these findings is that factors unrelated to foreign aid disbursements could account for the differential conflict

[8]The results also hold with 2 and 4 year moving average of aid measured in per capita terms.

Table 3: Effect of oil price-induced aid on civil war, alternate specifications.

Dependent variable:	Incidence of civil war (at least 1000 battle deaths)								
Method of estimation	IV Probit (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)	2SLS (7)	2SLS (8)	2SLS (9)
Foreign aid	−0.117	−0.025	−0.021	−0.027					
	(0.014)	(0.009)	(0.008)	(0.011)					
Foreign aid in *t-1*					−0.021				
					(0.007)				
Foreign aid in $t-2$						−0.019			
						(0.007)			
Foreign aid in $t-4$							−0.011		
							(0.006)		
Foreign aid, 2 year moving average								−0.021	
								(0.007)	
Foreign aid, 4 year moving average									−0.018
									(0.007)
Additional controls									
Muslim	0.241	0.070		0.074					
	(0.187)	(0.028)		(0.031)					
Oil price (2009US$)	0.007		0.001	0.002					
	(0.010)		(0.001)	(0.001)					
Recipient characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country fixed effects	N	N	Y	N	Y	Y	Y	Y	Y
Year fixed effects	N	Y	N	N	Y	Y	Y	Y	Y
F-statistic on instrument	25.28	14.68	24.21	11.14	28.05	29.65	34.77	29.65	36.59
Number of observations	2277	2277	2277	2277	2242	2164	2006	2163	2005

Note: Newey-West corrected robust standard errors with up to 1 lag autocorrelation reported in parentheses. Foreign aid measured as a share (%) of GDP. Recipient characteristics include: log GDP per capita, GDP per capita growth (% annual), log population, POLITY in 1972 × p(oil), and Percent rural × p(oil). These coefficients are not reported. Depending on the specification, country and year fixed effects and a constant are not reported. In Columns 5–7, aid in $t-j$ is instrumented with MUSLIM × Oil price in $t-j$, where $j = 1, 2, 4$. In Columns 8 and 9, 2 and 4 year moving average aid are instrumented with 2 and 4 year moving averages of MUSLIM × Oil price.

propensities across Muslim and non-Muslim non-oil producers. One such argument is the role of Cold War international politics on conflict (Kalyvas and Balcells, 2010). However, positing that the end of the Cold War can explain the rise in civil war misses the point of the treatment and control groups: one would have to argue that the end of the Cold War differentially affected instability in Muslim countries. Yet Cold War politics — in particular interventions by United States and the Soviet Union — did not discriminate on religion. Moreover, the rise of the civil conflict began by the mid-1980s, well *before* the Cold War had ended. Econometrically, the inclusion of year fixed effects subsumes the effects of temporal factors that affect all countries in sample, such as the Cold War and post-Cold War period. Nevertheless to account this potential differential effect, we include the interaction of a Muslim dummy and Cold War dummy (*Muslim* $\times$ *Cold War*) as an additional control. As Column 1 in Table 4 shows, the robust effect of aid on conflict holds in such a specification. Furthermore, this differential effect does not exhibit a robust effect on conflict.

Another concern is that Muslim countries are inherently more prone to instability. The research designs mitigate this concern. For instance, the inclusion of country fixed effects in the 2SLS regressions estimates the within-country variation of aid on conflict (and thus eliminates the proposed "Muslim" effect). Thus, one would have to come up with an alternative explanation for why Muslim countries should suddenly become relatively more unstable right when the price of oil happened to fall if not through the budgetary effects of foreign aid. Of course, it is plausible that this proposed Muslim effect might be time-varying, due to unobserved divergences in demographics, religious intensity, and/or regional "diffusion" (on conflict) between Muslim and non-Muslim countries. Empirically, this worry is not borne out. In a specification that accounts for this unobserved differential effect (with the inclusion of a *Muslim* $\times$ *Year* trend interaction as an additional control), the robust negative effect of aid on conflict remains (Table 4, Column 2).

Skeptics may worry further that unobserved differential trends across regions (rather than between Muslim and non-Muslim countries) are driving the results, for example, due to differences in the diffusion of democracy across geographic regions. To allay this concern, Columns 3 and 4 in Table 4 present results from specifications that control for a vector differential trends by region. Column 3 controls for a vector

Table 4: Effect of oil price-induced aid on civil war, Accounting for unobserved heterogeneity and alternate classifications.

Dependent variable:	Incidence of civil war (at least 1000 battle deaths)								Onset of civil war	
	Unobserved heterogeneity				All conflict	Alternate definition of Muslim				
						MUSLIM60	MUSLIM80	% MUSLIM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Foreign aid (% GDP)	−0.021	−0.022	−0.017	−0.023	−0.023	−0.022	−0.009	−0.017	−0.003	−0.010
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.005)	(0.006)	(0.003)	(0.005)
Additional controls										
Muslim × cold war	−0.013									
	(0.050)									
Muslim × year		0.001								
		(0.002)								
Region FE × cold war			Y							
Region FE × Year				Y						
Recipient characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Country fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
F-statistic on instrument	17.73	26.82	24.79	27.37	25.28	29.7	20.88	24.66	25.28	15.29
Number of observations	2277	2277	2277	2266	2277	2277	2277	2277	2277	1151

Note: Estimation via 2SLS. Newey-West corrected robust standard errors with up to 1 lag autocorrelation reported in parentheses. Recipient characteristics include: log GDP per capita, GDP per capita growth (% annual), log population, POLITY in 1972 × p(oil), and Percent rural × p(oil). These coefficients plus country and year fixed effects and a constant are not reported. In Columns 3 and 4, the region fixed effects are Africa, Asia, Middle East, Europe, North America, and South America (the reference category is Australia). In Columns 1 and 3, Cold War is an indicator variable equal to 1 for any observation during the Cold War (before 1990) and 0 for any observation thereafter. In Column 5, the dependent variable (for all conflict) measures the incidence of either a internationalized or non-internationalized internal conflict exceeding at least 1,000 battle deaths. Columns 6 and 7 instrument for aid interaction of oil prices with a dummy equal to 1 if the percentage of Muslims in the population exceeds 60% and 80%, respectively. In Column 8, aid is instrumented with the interaction of oil prices and each country's percentage of Muslims in the population. In Columns 9 and 10, the dependent variable is the *onset* of civil war (at least 1,000 battle deaths). In Column 10, the sample is restricted to years of a negative aid shock.

of region dummies (e.g., Asia, Africa, etc.) interacted with a Cold War dummy, while Column 4 does so with a year trend. In both specifications, instrumented aid continues to exhibit a robust negative effect on conflict.

Of course, there may be cross-border economic and political factors that are correlated with oil prices (but not necessarily with foreign aid) that may affect conflict propensity in developing, non-oil producing Muslim countries. We discuss these alternate explanations and our robustness checks below.

4.3.5 Alternate classifications

The core 2SLS results are robust to alternate classifications of the key variables. For example, Column 5 in Table 4 shows that instrumented aid reduces the likelihood of both high intensity internationalized and non-internationalized civil war. The results also do not hinge on the threshold classifying a country as being Muslim (and therefore its inclusion in the treatment group).[9] For example, Column 6 instruments for aid using the interaction of oil prices and a dummy variable equal to 1 for any non-oil producing country where Muslims constitute 60 or more percent of the population. In this specification, the estimated effect of instrumented aid remains unchanged from the core results in Table 2. Furthermore, aid has a robust negative effect on conflict when it is instrumented using a Muslim threshold of 80% (Column 7), although the effect is slightly less statistically significant since there are fewer countries in the treatment group. In fact, the main results do not hinge on using any particular cutoff threshold. In Column 8, we instrument for aid using each country's percentage of Muslim citizens. This ranges from 0% (e.g, Bolivia, Swaziland) to 100% (e.g., Mauritania, Somalia). In this specification, instrumented aid has a robust negative effect on conflict, although its effect is slightly smaller than the core results in Table 2.

Finally, Columns 9 and 10 report the effect of aid on conflict onset. Across the entire sample, on average, aid lowers the onset of high intensity conflict but the effect is not statistically significant. This weak effect may mask the differential (and asymmetric) impact of positive

[9]Percentages are drawn from Fearon and Laitin (2003).

and negative aid "shocks" on conflict onset. In particular, Nielsen *et al.* (2011) show that negative aid shocks raise the likelihood of conflict while the converse (i.e., a positive aid shock) may not necessarily decrease the likelihood of conflict in equal magnitude. Indeed, in a sample restricted to years of negative aid shocks (i.e., decline in aid receipts from the previous year), higher amounts of aid lower the likelihood of conflict onset (Column 10), a finding consistent with Nielsen *et al.* (2011).

5 Discounting Alternative Explanations

The primary goals of the statistical analysis in the previous section are to demonstrate the relationship between the increase in aid received by non-oil producing Muslim countries and internal conflict, and to argue that the aid can explain some of the decrease and subsequent increase in conflict. A potential concern with the findings from this exercise is that factors correlated with oil prices that are independent of foreign aid flows may be driving the differential effects in conflict propensity (i.e., potential violations of the "exclusion restriction"). We investigate the main potential alternative explanations below.

5.1 Remittances

At the same time Gulf oil producers were disbursing aid, they also hosted labor from non-oil producing Muslim countries. These workers in turn remitted income back home. Like aid, remittances to non-oil producing Muslim countries are correlated with the price of oil (Ahmed, 2012). Unlike aid, remittances are received by households and not by the government, so the link between remittances and conflict is not as direct. For instance, remittances may "buy" stability via a substitution effect between welfare goods and patronage (Ahmed, 2012) and also decrease the risk of conflict by fostering economic growth (Regan and Frank, 2014). Alternatively, higher remittance income should enrich individuals and may fuel conflict (e.g., by increasing demands for political liberalization). Adjudicating these divergent predictions therefore remains an empirical matter. As we show in Column 5 of Table 2, controlling for remittances does not affect the substantive finding of instrumented aid on civil war. Moreover, controlling for remittances

Table 5: Difference-in-differences estimates, accounting for alternate explanations.

Dependent variable	Incidence of civil war (at least 1,000 battle deaths)					
	(1)	(2)	(3)	(4)	(5)	(6)
During:	−0.054	−0.067	−0.062	−0.070	−0.059	−0.075
1973–1985	(0.038)	(0.047)	(0.048)	(0.051)	(0.045)	(0.059)
After:	0.111	0.125	0.120	0.132	0.114	0.104
1986–1999	(0.059)	(0.057)	(0.058)	(0.060)	(0.055)	(0.053)
Post:	−0.042	−0.092	−0.092	−0.099	−0.088	−0.113
2000–2008	(0.048)	(0.053)	(0.053)	(0.056)	(0.051)	(0.055)
Additional controls	Remittances				Assassinations	Terrorist fatalities
Excluded countries		Lebanon	Morocco	Morocco Lebanon		

Note: Standard errors are clustered by country reported in parentheses.

in the difference-in-differences estimates (Table 5, Column 1) does not affect our basic findings from Table 1. These estimates are slightly less robust than those reported in Table 1 due to a smaller sample size since remittance data is spotty in the 1970s. On balance, we conclude that excluding remittance inflows does not constitute omitted variable bias.

5.2 Exporting Islamic Extremism

Perhaps the most troublesome concern is that oil prices allowed Gulf oil producers to affect the internal politics of non-oil producing Muslim countries, independent of the foreign aid channel. After all, when the price of oil was high, OPEC countries were not only giving money away, they were exporting politics. In particular, there were three major players whose influence benefited from the high oil prices: Ayatollah Khomeini in Iran, Muammar Gaddafi in Libya, and the religious hard-liners in Saudi Arabia. Khomeini tried to export the Iranian revolution, Gaddafi funded insurgencies of all sorts, and the Saudis pushed Wahhabi beliefs on Muslims around the world (Kepel, 2002).

We account for the potential effects of external meddling in two ways (see Appendix B for an expanded discussion). First, our results are robust when we exclude conflicts that featured some possibility of influence from Arab oil producers (i.e., Lebanon and Morocco) from

our empirical analysis. Second, we account for assassinations and terrorist related fatalities as proxies for extremism to test whether extremism produced alongside the foreign aid windfalls can explain the patterns of conflict. We repeat our empirical analysis controlling for each country's annual number of assassinations (available from Banks, 2010) and terrorist related fatalities (from START, 2014). Controlling separately for assassinations and terrorist fatalities (Table 2, Columns 6 and 7) does not appreciably change the pattern between the foreign aid windfall and high intensity, two-sided civil war.[10] The difference-in-differences estimates remain largely unchanged (Table 5, Columns 5 and 6). On balance, accounting for the potential effects associated with the "exporting" of Islamic extremism does not affect our substantive findings.

6 Conclusion

The recent upheaval across the Muslim world has sparked interest in understanding some of its causes. In this article, we refer back to the heightened instability many Muslim non-oil producing countries experienced from the mid-1980s to 2000 to provide robust empirical evidence that oil-price induced foreign aid accounts for some of the political dynamics in these countries. We show that these aid inflows reduced the incidence of civil war in developing, non-oil producing Muslim countries.

This main finding offers several avenues for future research. For instance, this article shows that higher levels of aid "buy" political stability, while declines engender a heightened incidence of conflict. As such our paper offers a dynamic account for the effects of foreign aid on political stability, as aid can strengthen the capacities of governments (especially in autocracies) in the short-run but lay the seeds for future conflict once aid receipts decline. This insight could be developed further, both in formal theories and empirical testing. For instance, on the latter, rather than examining the contemporaneous or one year lagged effect of aid on conflict (as is the norm in the conflict literature), alternate regression specifications could include additional lags and/or

[10]Unsurprisingly, the total number of terrorist related fatalities is positively and significantly associated with the incidence of civil war (Table 2, Column 7).

leads of aid as independent variables of interest. Moreover, the type of aid that we empirically model in this article (i.e., primarily untied) suggests that future research ought to evaluate whether and how the composition of aid (e.g., tied versus untied) differentially affects conflict.

With respect to contemporary politics, our findings provide a backdrop to understand some of the emerging dynamics underlying the political upheaval in the Middle East and North Africa this decade. By early 2015 Tunisia and Egypt had seen popular revolutions, Libya and Syria civil war, Yemen and Bahrain civil uprisings, and a dozen other countries protests of varying strength. None of these headline countries perfectly fits our pattern of the non-resource endowed, aid-flooded but repressive recipient government. But they are not — with the potential exception of Libya, which, for its modern history has made a practice of being an exception — inconsistent with our story: the authoritarian regimes had enough unearned income to maintain stability. However by 2010, foreign aid in these countries (notably Egypt, Syria, Tunisia, and Yemen) was reaching its lowest levels in the past 40 years, which heightened the risk of political upheaval. In Egypt, foreign aid has fallen dramatically since 1990. In 1990, aid inflows comprised around 13% of GDP; by 2009 aid inflows amounted to less than 1% of GDP. Similarly, external rents received by Yemen declined: from 8.3% of GDP in 1990 to around 1% of GDP in 2009.

In most cases of regime change in the Arab Spring, the political activity was largely peaceful, and thus did not even lead to the 1,000 battle deaths necessary to qualify as two-sided conflict in our dataset. But a moment happened, for so many reasons, which made the lingering autocracy of these countries a target. Decades after they might have — in the absence of the oil price induced aid doled out by Gulf oil producers — these countries made rapid (potential) steps toward democratization, bringing the average level of democracy in Muslim countries that much closer to the average level in non-Muslim countries, which (among non oil-producers) had last been equal in 1975.

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