The International Sources of Intrastate Conflict: United Nations Security Council Membership, Temporary Aid Shocks, and African Civil War Onset

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Abstract African countries that serve on the United Nations Security Council (UNSC) generally experience a temporary financial windfall of higher aid flows during their tenure. They, however, seem plagued by the "lottery winner's curse". The frequency of civil war onsets among these countries more than doubles from two percent to five percent during and in the year after UNSC membership. We argue that UNSC members receive a one-off, temporary positive aid shock in return for their favorable votes on the UNSC, which in turn increases their likelihood of experiencing a civil war onset. Panel data analyses of civil war onsets from 1945-1999 support our argument. Our results are robust to recipient fixed effects, time trends, the endogeneity of aid shocks, and alternative definitions of civil war onset and aid. Paradoxically, membership in the primary multilateral organization tasked with fostering civil peace increases the likelihood of civil war.

1. Introduction

Developing countries that serve on the United Nations Security Council (UNSC) often experience a temporary financial windfall during their two-year tenure. They generally receive higher flows of bilateral and multilateral aid in exchange for their favorable votes at the UNSC.[1] These countries, however, seem plagued by the "lottery winner's curse." They subsequently experience a substantial decrease in their economic and democratic performance (Bueno de Mesquita and Smith 2010). These outcomes are paradoxical since they contravene the United Nation's core goals of democracy promotion and economic development. We ask whether the lottery winner's curse also encompasses the United Nation's primary raison d'être – the maintenance of international peace and security. We do so by examining whether a country's likelihood of experiencing a civil war onset increases after winning election to the UNSC.

Following the logic of the lottery winner's curse to its full conclusion, we posit a *one-off, temporary positive aid shock* as the central mechanism that links UNSC membership to civil war onset. The existing literature, however, provides little direct theoretical or empirical guidance for building upon the aforementioned hypothesis. Most authors have tended to focus on the effect of *aid flows* on the onset of *civil conflict*, rather than on the influence of *aid shocks* on *civil war* onset.

Even so, there exist two seminal studies – Nielsen et al (2011) and Besley and Persson (forthcoming) – that more directly relate to our proposed hypothesis. Both offer seemingly conflicting mechanisms that link aid shocks to conflict onset. Nielsen et al (2011) empirically examine the effect of foreign aid shocks – extreme changes in the levels of foreign aid flows – on the onset of civil conflict, which they define as a binary variable that equals one if the

[1] See Kuziemko and Werker 2006, Dreher et al 2009a,b, 2010, Bueno de Mesquita and Smith 2010, and Lim and Vreeland 2011.

violent armed conflict results in at least 25 battle deaths. They find that negative foreign aid shocks increase the likelihood of civil conflict onset; positive foreign aid shocks do not influence the onset of civil conflict. Significantly, they remain agnostic on the effect of either positive or negative foreign aid shocks on civil war onset – which require a higher threshold of at least 1,000 battle deaths – concluding that aid shocks are "less predictive of large conflict onset" (Nielsen et al 2011, pp. 11). Besley and Persson (forthcoming) develop a commitment model that centers on shocks to account for the variance in the levels of political violence across countries. They use UNSC membership during the Cold War as an exogenous measure for positive shocks to aid flows. Importantly, their key independent variable has little or no effect on civil war onset. Moreover, though their theory concerns the effect of aid shocks on political violence, their empirical analysis focuses on *aid flows* rather than *aid shocks*. The upshot, then, is that the existing literature provides little theoretical or empirical guidance on the effect of UNSC membership on civil war onset.

We build on and make three explicit contributions to the literature. First, we distinguish one-off, temporary aid shocks from sustained aid shocks, and explicate the causal mechanism and theory linking one-off, temporary positive aid shocks to civil war onset. Second, we propose using *UNSC period* – a variable coded one for the years during and the year after UNSC membership, and zero otherwise – for African countries as an exogenous measure of temporary positive aid shocks. If one sought to test the impact of aid shocks on civil war onset, why not use a direct measure of aid shocks such as actual changes in annual aid flows, as per Nielsen et al (2011)? We argue that such direct measures suffer from potential problems of endogeneity. Donor countries might reduce aid flows to countries that are embroiled in a civil war, or they might increase aid flows in the hope of ending civil wars (de Ree and Nillesen 2009). The direction of causality might therefore run from civil wars to

foreign aid shocks, rather than the other way round.[2] UNSC membership is highly correlated with aid shocks (see Kuziemko and Werker 2006, Dreher et al. 2009a,b, 2010, and Lim and Vreeland 2010), but arguably uncorrelated with civil war. We focus only on African countries because election to the Security Council appears most idiosyncratic for the African region, and strictly enforced two-year term limits ensure the exogeneity of exit (Dreher and Vreeland 2010). To account for the delay between a country receiving increases in aid and descending into a civil war, we lag UNSC participation – coded one for the year of UNSC election and the years on the UNSC – by one year, which we then term *UNSC period*. Third, we present, to the best of our knowledge, the first systematic empirical analysis that uses a new comprehensive aid dataset to analyze the causal link between aid shocks and civil war onset.

We consider a panel of 46 African countries from 1945-1999. Data on civil war onsets come from Fearon and Laitin (2003). We begin with standard regression analyses. We use a rare-events logit regression model, and subsequently include fixed effects and cubic polynomial time variables. We find that African countries that are elected to the UNSC are three times more likely to experience a civil war onset in the years during and in the year after UNSC membership. Next, to directly address the possibility of a selection problem – that there exist countries that are both more prone to civil war and more likely to win election to the UNSC – we adopt a propensity score matching approach that involves pairing *UNSC period* country-year observations against observationally similar non-*UNSC period* country-year observations countries, and then comparing the average difference in civil war onsets. In addition to coarsening our data to reduce pre-matching imbalance among the covariates, we also use matching as a pre-processing step to reduce the sensitivity of our results to the

[2] It is arguable that donor aid is likely to be more responsive to civil conflict than civil war. In other words, we hypothesize that aid shocks are more endogenous to civil war onset than onset of civil conflict.

parametric functional forms specified in our earlier logistic regressions. We find that UNSC participation doubles the likelihood of civil war onset among African countries. Our results are robust to alternative definitions of civil war onset.

Finally, we use instrumental variables to directly mitigate the endogeneity problem and to test the causal mechanism – whether it is temporary positive or negative aid shocks – linking UNSC period to civil war onset. We obtain our aid variables from AidData (Nielson et al. 2010), which we then normalize by population.[3] Following Nielen et al. (2011), we define positive aid shocks as a dummy variable that equals one for all two-year average changes in commitment aid flows that are above the 85th percentile of all such aid changes, and 0 otherwise. Similarly, we define negative aid shocks as a dummy variable that equals one for all two-year average changes in commitment aid flows that are below the 15th percentile of all such aid changes, and 0 otherwise. We find that temporary positive aid shocks increase the likelihood of civil war onset. Moreover, the larger the temporary positive aid shock, the greater the probability that an African country will experience a civil war onset. Consistent with our theoretical predictions, we find that temporary negative aid shocks have no significant effect on the likelihood of civil war onset. Note that our results are robust to using a GDP normalized definition of aid per capita and alternative definitions of aid shocks and civil war onset.

The rest of the paper proceeds as follows. Sections 2 and 3 respectively review the literatures on global horse-trading and violent armed conflict. Section 4 outlines our argument, and section 5 details the empirical results. Section 6 concludes.

[3] We use population instead of GDP because the latter is endogenous to civil war onset.

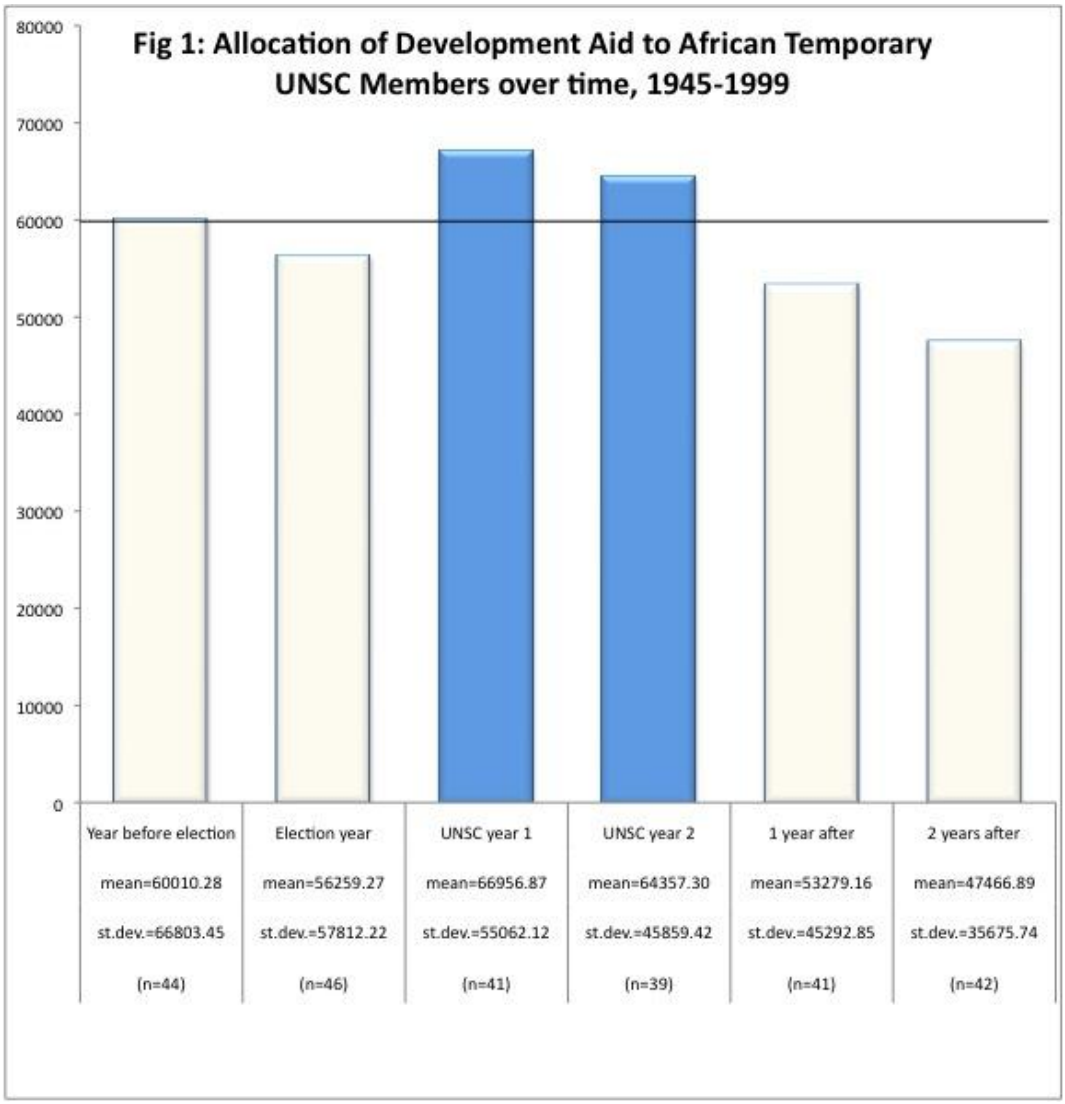
2. Global Horse-Trading: UNSC Votes for Foreign Aid

The literature on the global-horse trading of UNSC votes for development aid generally focuses on Japan and the United States and their political manipulation of international organizations. It finds that temporary UNSC members receive more multilateral aid during their UNSC term from international financial institutions (IFI) that either Japan or the United States dominate. Lim and Vreeland (2011) find that elected UNSC members receive approximately 25 percent more loans than non-UNSC members during their UNSC tenure from the Asian Development Bank, an institution that Japan dominates. Dreher et al. (2009a,b, 2010) demonstrate that temporary UNSC members receive more loans with fewer conditionalities from the World Bank and the International Monetary Fund – institutions where the United States is the largest vote-holder. Also, Kuziemko and Werker (2006) show that elected UNSC members receive more United Nations Development Program aid than non-members through UNICEF, where the United States is the largest contributor. Note that global horse-trading also occurs at the bilateral level – temporary UNSC members also receive more American and Japanese bilateral aid (Kuziemko and Werker 2006, Frankel and Gibbons 2011).

More relevant for the purposes of this paper is the pattern of aggregate aid commitments per person that temporary African UNSC members receive over time (see Figure 1).[4] These countries receive US$60,000 in gross aid commitments in the year before election to the UNSC, an amount that is similar to the average gross aid commitment of US$59,589 that they receive throughout the sample. In the election year, gross aid

[4] As we will detail in the empirical section below, we choose to focus on aid commitments rather than disbursements because pre-2002 data for disbursements tend to be less reliable (Nielsen et al 2011). We normalize our aid variables by population rather than GDP because the latter is endogenous to civil war onset (Steinwald 2011).

commitments fall slightly to US$56,000. During the first and second years of membership on the UNSC, the gross amount of aid commitments rises to US$67,000 and US$64,000 respectively. In other words, elected UNSC members receive a temporary positive aid shock of approximately 17 percent during their term on the Security Council. Exogenous term limits subsequently force them to exit the Security Council. The gross amount of aid commitments then falls to US$53,000 and US$47,000 in the first and second years after the UNSC term ends, representing a temporary negative aid shock of 24 percent.



Note: The horizontal line shows the mean aid commitment to temporary African UNSC member countries across the entire sample (59589, n=1323).

The spike in gross aid commitments received during the UNSC term and the sharp decline in gross aid commitments in the year after UNSC membership reflect the aggregate effect of the time taken to assemble both bilateral and multilateral loan commitments: 0.5 – 2.5 years (see Figure 2). Note that loan commitments represent the rate-determining step in the entire loan process, and that loan disbursements can often take place immediately after the loan has been approved (see Figure 2). We therefore expect both a correlation between the amount of aid committed and disbursed to temporary UNSC members, as well as a strong correlation between the timing of these commitments and disbursements. Bilateral aid commitments generally require less time to assemble: typically 4 to 12 months (Perry 2009, pp. 64). In contrast, the project loan approval process for multilateral aid agencies usually spans 18 to 24 months (Lim and Vreeland 2011). Since donor countries such as the United States and Japan commence the project loan approval process during the UNSC election year, this translates to temporary UNSC members receiving significant increases in aid during their years on the UNSC (Dreher et al 2009a,b, Lim and Vreeland 2011). When elected members leave the UNSC after their term ends, donors cease providing extra aid, and this results in aid amounts falling precipitously – relative to the years on the UNSC – in the year after UNSC membership.

To summarize, the global horse-trading literature provides two important insights:

(1) Temporary UNSC members experience a *temporary positive aid shock* during their term on the UNSC

(2) Temporary UNSC members experience a *temporary negative aid shock* one year after their UNSC term ends.

Figure 2: Multilateral Aid Agency Project Lending Cycle (from Lim and Vreeland 2011)

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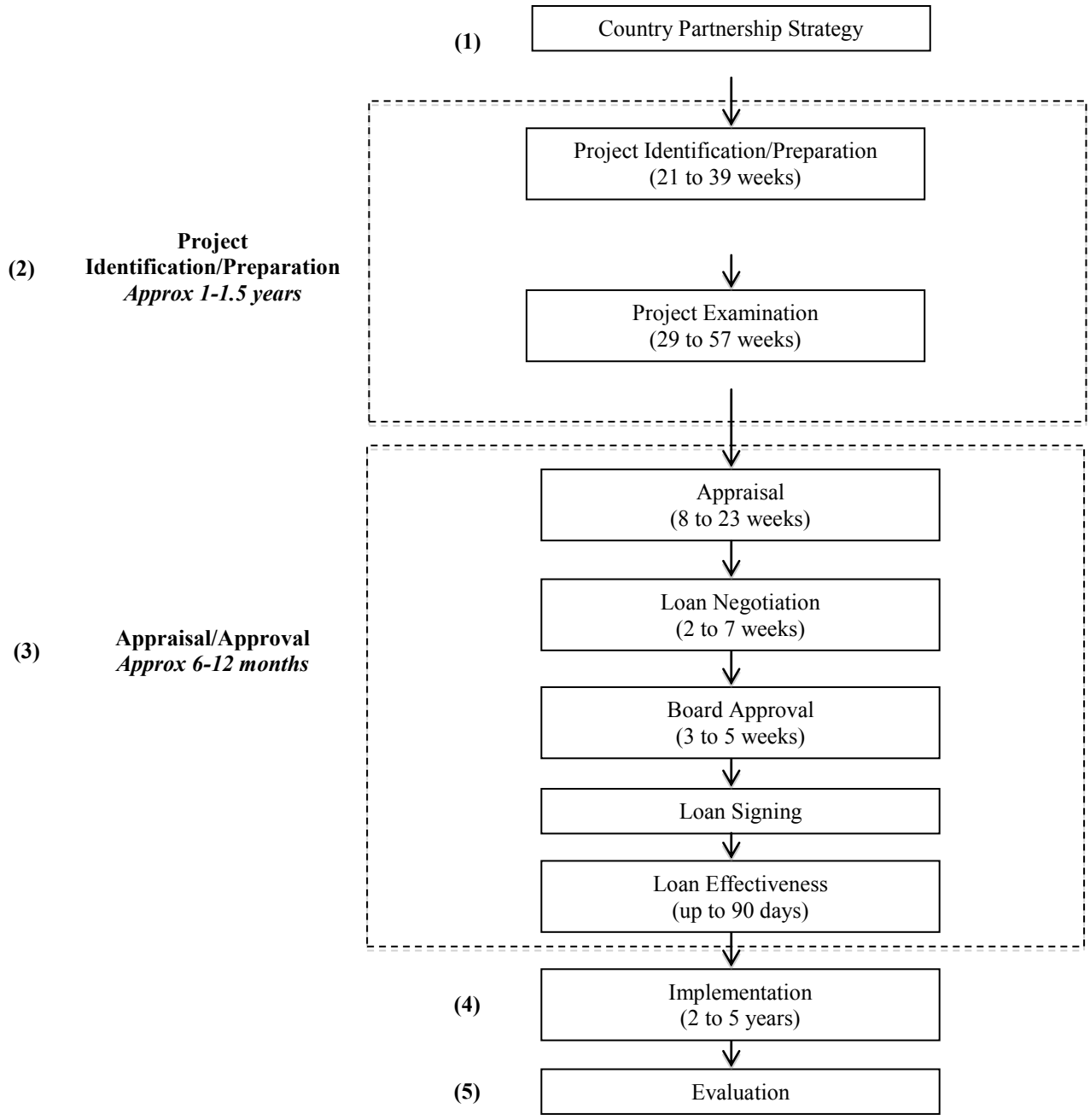


Note: From initiation until commitment (disbursement) takes approximately 1.5 to 2.5 years

3. Foreign Aid Shocks and Civil War Onset

The current literature has tended to focus on the effect of *aid flows* on the onset of *civil conflict*, rather than on the influence of *aid shocks* on *civil war* onset.[5] Most authors discount the effect of *aid volatility* on conflict onset, focusing instead on the influence of *increased aid flows* (Nielsen et al 2011),

But why should scholars focus on aid shocks? Nielsen et al. (2011) make a compelling case for distinguishing between aid shocks, aid flows, and aid volatility. They recount how aid to developing countries is characterized by volatility rather than stability – development aid can be up to 40 times more volatile than government revenue (Bulir and Hamann 2006, pp. 7). More importantly, the literature on aid volatility generally agrees that aid volatility significantly decreases economic growth and increases the risk of civil war (see Arcand and Chauvet 2001, Bulir and Hamann 2003, 2006). So scholars who ignore aid volatility omit a potentially important variable in their analyses.

Still, aid volatility does not distinguish between the "different downstream effects of positive versus negative fluctuations" (Nielsen et al. 2011, pp. 3). The literature on aid flows suggests different causal mechanisms through which severe changes in the amount of aid

[5] At the outset, it is important to note that the existing literature generally treats *civil wars* as conceptually similar to but substantively different from *civil conflicts*. The causal mechanisms that govern civil conflicts are conceived of as also applying to civil wars, with the fundamental distinction between the two phenomena being one of order of magnitude – violent armed conflicts are defined as outbreaks of violence that result in at least 25 battle deaths, whereas civil wars have a higher threshold of at least 1,000 battle deaths (Lacina and Gleditsch 2005). In the process of outlining the theoretical section of our paper, we adhere to the traditional convention and use the terms *civil conflict* and *civil war* interchangeably when referencing conceptual contributions from the literature.

received could affect the dynamics of civil war onset. These differing mechanisms can be grouped into two categories using the conflict-onset framework established by Collier and Hoeffler (2004) of *greed* versus *grievance*. Consider the case of positive aid flows. First, positive aid flows can induce conflict because rebels are greedy: aid might increase the spoils of war, and thus provide greater incentives for rebel groups to capture the government (Azam 1995, Grossman 1991). Alternatively, aid might engender grievances because recipient governments transfer the additional resources to favored groups, thereby exacerbating local inequalities and agitating rebel groups to retaliate (Esman and Herring 2003). In contrast, positive aid flows might stabilize countries and lower the propensity of civil war onset in two ways: First, aid might strengthen the state and deter rebels from launching attacks (Collier and Hoeffler 2007). Second, recipient governments might use the additional funds to provide side payments to rebel groups to stave off civil war. Figure 3 summarizes these opposing viewpoints.

Figure 3: Foreign Aid Shocks and Civil War Propensity

	Positive Aid Flows: Increases State Capacity	***Negative Aid Flows: Decreases State Capacity***
Increase civil war propensity	**Rebel Greed:** Increased spoils of war incentivizes rebels to launch attacks **Rebel Grievance:** Increased government transfers to favored groups exacerbates local grievances **State Capacity:** Government launches pre-emptive strike; Government confident to respond against rebel attacks	**Rebel Greed:** Government cannot credibly commit to providing side-payments, rebels launch attack **State Capacity:** Rebel group launches pre-emptive strike; Government inability to respond to rebel attacks emboldens rebels to launch massive attack
Reduce civil war propensity	**Rebel Greed:** Governments make side-payments to rebel group **State Capacity:** Rebel group deterred from attacking strengthened state	**Rebel Greed:** Reduced spoils of war disincentivizes rebels to launch attacks **State Capacity:** Weakened state negotiates truce with rebel group

Even so, there exist two seminal studies – Nielsen et al (2011) and Besley and Persson (forthcoming) – that more directly relate to our proposed hypothesis. Each explicates a theoretical model that centers on the effect of aid shocks on the ability of governments and rebels to credibly commit to maintaining the existing distribution of resources under the status quo. Both also offer conflicting mechanisms that link aid shocks to conflict onset. Nielsen et al (2011) find that negative aid shocks increase the onset of civil conflict; they find no effect for positive aid shocks. In contrast, Besley and Persson (forthcoming) use UNSC membership during the Cold War as an exogenous measure for positive shocks to aid flows, and find that positive aid shocks increase the onset of civil conflict.

Significantly, both Nielsen et al. and Besley and Persson *do not find evidence* that aid shocks influences civil war onset.[6] This should not surprise for two reasons. First, both studies do not directly address problems of endogeneity – while aid shocks cause civil wars, it is also possible that donors reduce or increase aid flows in response to civil war onsets. Second, both studies do not examine the hypothesis that we have proposed – that UNSC members receive a one-off, temporary positive aid shock in return for their favorable votes on the UNSC, which in turn increases their likelihood of experiencing a civil war onset. Note that Nielsen et al. look at the effect of aid shocks on violent armed conflict (more than 25 battle deaths) rather than civil war onset (more than 1000 battle deaths). Similarly, Besley and Persson focus their empirical analysis on changes in the level of political violence – from peace to repression to civil war – rather than on civil war onset per se. They also look at *aid flows* rather than *aid shocks*.[7] The upshot is that the existing literature provides little direct theoretical or empirical guidance for building upon our proposed hypothesis – that UNSC members receive a one-off, temporary positive aid shock in return for their favorable votes on the UNSC, which in turn increases their likelihood of experiencing a civil war onset.

[6] Nielsen et al. (2011) find their negative aid shocks variable to be significant only at the 10 percent level, and conclude that aid shocks are "less predictive of large conflict onset". Besley and Persson (forthcoming) find that their key independent variable has little or no effect on civil war onset. See Table 1, Columns 6 and 7 and Table 2, Columns 3 and 4. The results in Table 1 are obtained using a fixed effects logit model and are only significant at the 10 percent level. Table 2 reports results from a linear probability model with fixed effects; these are insignificant.

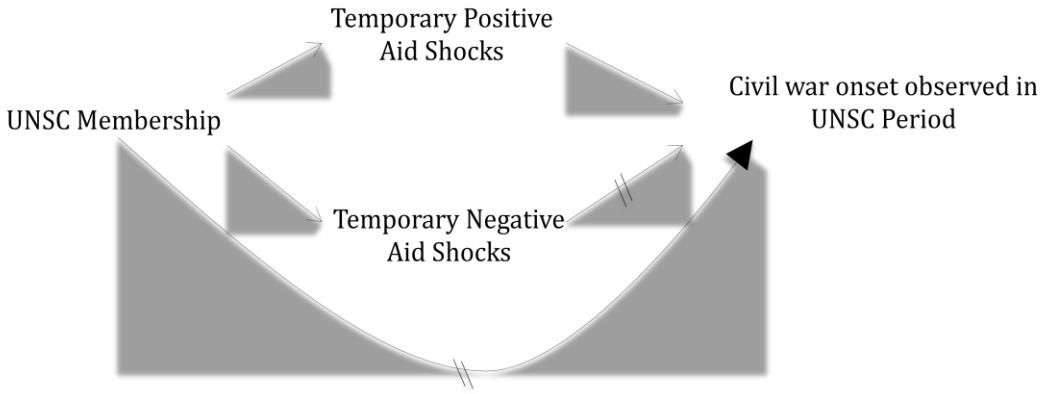
[7] See Table 2, Column 6. The dependent variable is Log Aid Disbursements from OECD countries.

4. The Argument

Overview

Drawing on the insights from the global horse-trading and civil war literatures, we hypothesize that African countries that are elected to the UNSC will receive a *positive aid shock* during their term on the UNSC and a *negative aid shock* one year after UNSC membership. Because these aid shocks pertain to vote-buying at the UNSC, they are likely to be *temporary* in nature, a fact that has important consequences for the effect of the aid shocks on the likelihood of civil war onset. Specifically, we argue that both *temporary positive aid shocks* increase the probability of civil war onset, whereas *temporary negative aid shocks* have no effect on the likelihood of civil war onset. Moreover, the timing of civil war onset is likely to be idiosyncratic because countries vary in when they receive the positive aid shock during their UNSC term, and because governments vary in how quickly they can coordinate an attack against rebel groups in response to a positive aid shock. Consequently, we predict that African countries that are elected to the UNSC will experience a higher likelihood of civil war onset in the years during and in the year after UNSC membership. Figure 4 summarizes these arguments.

Figure 4: Foreign Aid Shocks and Civil War Propensity



Mechanism

We conceive of the state as disaggregated into two competing factions – the government and rebels. Foreign aid flows comprise an integral component of the government's annual budget, and, together with the country's tax base, determine the government's power relative to the rebels. For example, foreign aid comprised more than 30 percent foreign of Mali's budget during the 1970s ad 1980s (Nielsen et al. 2011, pp. 1); aid flows comprised 99 percent of the Democratic Republic of Congo's GDP in 2003. Aid shocks change the government's annual budget and hence the distribution of power between the government and rebels.

Suppose that the government possesses complete information about the size and nature (temporary or permanent) of the aid shocks they experience in a given year. The government will update its beliefs about the distribution of power in the immediate aftermath of an aid shock. Rebels may not be privy to such information and will rely on government actions to infer the size and nature of the aid shock that the government has received. Specifically, rebels conduct regular probes that consist of small-scale attacks against the government, and rely on the latter's response to update their belief about the true underlying distribution of power. Rebels will escalate their attacks against the government if they conclude from their probe that the true underlying distribution has shifted in their favor; they will continue to maintain the status quo if they believe that no change has occurred. The key implication of this set of assumptions is that while both the government and the rebels are aware of the true underlying distribution of power prior to an aid shock, only the government is aware of the new underlying distribution of power in the *immediate aftermath* of the aid shock.

What are the implications of this story for the incentives of both the government and rebels in the immediate aftermath of a temporary aid shock? We argue that a *temporary positive aid shock* will increase the likelihood of civil war for two reasons.

First, a government that receives a temporary positive aid shock knows that the relative distribution of power has temporarily shifted in its favor. It must then weigh the prospect of utilizing the temporary boost to its resources to wage a civil war against the rebels – forgoing the ability to consume today the flow of resources made available from the preexisting equilibrium in return for a larger flow of resources tomorrow – against the prospect of continuing to receive the present flow of resources in perpetuity. Whether the government will indeed do so will depend on its discount rate and its belief about its likelihood of emerging victorious from the civil war.

Second, even if the government does not initiate a civil war, it is more likely to respond to regular rebel probes of the existing distribution of power with overwhelming violence, in part because the additional resources at its disposal increases its confidence ex-ante of securing a victory over the rebel group. The counterfactual argument here is that in the absence of the temporary positive aid shock, the government would have exercised more restraint against the regular rebel probe, or might have agreed to a redistribution of resources. Civil war would thus have been averted.

What about the scenario in which the government receives a *temporary negative aid shock*? We argue that receiving such a shock will not increase the likelihood of civil war.

The relative distribution of power temporarily shifts in favor of the rebels when a government receives a temporary negative aid shock. The government observes this new but temporary shift in the balance of power, whereas the rebel groups are kept in the dark. Because the government knows that it is temporarily weakened, it is unlikely to escalate its attacks against the rebels during that period, and will instead seek to maintain the status quo

by concealing from the rebels its momentary weakness. To the extent that the government is successful in doing so, then should the rebels happen to launch a probe in the year of the negative aid shock, they will conclude that the true underlying distribution of power has remained unchanged. The rebels are therefore unlikely to follow up on their initial probe by escalating their attacks.

Of course, should the government fail to conceal its momentary weakness and falter in its attempts to deal with the small attack, then the rebels would wrongly conclude that the true underlying distribution of power is in their favor. To the extent that the rebels have a high discount factor and are confident of emerging victorious from a civil war, they are likely to be willing to forgo the present stream of benefits that accrue from the existing distribution of resources, in favor of a higher stream of future benefits dictated by a redistribution of resources. The rebels are therefore more likely to continue escalating their attacks and less likely to settle their differences with the government. Because the government knows that the temporary negative aid shock has only weakened it temporarily, and that aid levels are likely to stabilize in the near future, it is more likely to refuse to yield to the rebel incursion. Note that our argument is not that temporary negative aid shocks will never lead to a civil war onset. Rather, we expect there to be a smaller number of civil war onsets that began as a result of UNSC-induced temporary negative aid shocks relative to temporary positive aid shocks, for the reasons that we have outlined above.

To summarize, we observe a civil war because the effect of the temporary positive aid shock under information asymmetry is to alter the dynamics of bargaining between the government and rebels in such a way as to render one or both less likely to concede to a settlement. The number of battle deaths is more likely to rise past the 1000-mark threshold, and we are thus more likely to observe a civil war onset following a temporary positive aid shock.

Observable Implications

At this point, it is important to stress that the timing of UNSC-induced civil war onsets is likely to be idiosyncratic. This occurs primarily because donor countries vary in their preferred choice of aid channels – bilateral or multilateral – through which to engage in vote-trading at the UNSC. Recall that bilateral aid commitments generally require 4 to 12 months to assemble, whereas multilateral aid commitments require 18 to 24 months. Consequently, recipient countries vary in when they receive the positive aid shock during their UNSC term, and hence in when they experience a civil war onset.

Moreover, governments vary in their pre-aid shock mobilization capacity. Consider the scenario of a temporary positive aid shock. Governments with greater pre-existing mobilization capacity would be able to immediately channel the increased aid flows to wage war against rebel groups, whereas those with less developed pre-existing mobilization capacity would require some lead time before launching an attack. Since countries receive an increase in aid flows either during the first or second year on the UNSC, we consequently observe civil war onsets during the years during and the year after UNSC membership.

Hypotheses

(1) African countries that are elected to the UNSC are more likely to receive a *temporary positive aid shock* during their term on the UNSC.

(2) African countries that are elected to the UNSC are more likely to receive a *temporary negative foreign aid shock* that one year after UNSC membership.

(3) African countries that are elected to the UNSC are more likely to experience the onset of civil war in the years during and in the year after UNSC membership.

5. Data, Research Design, and Methodology

In this section, we present quantitative evidence supporting our three hypotheses. We begin by describing the data and presenting descriptive evidence of our broad hypothesis that African countries that are elected to the UNSC are more likely than other African countries to experience civil war onset in the years during and in the year after UNSC membership. We then test the hypothesis using more rigorous methods. Finally, we present preliminary evidence suggesting that UNSC-induced civil war onsets occur mainly through temporary positive aid shocks.

5.1 Average civil war onset and UNSC participation

Data on civil war onset come from Fearon and Laitin (2003); data on UNSC participation are from the United Nations website. After a list-wise deletion of observations with missing data (on the control variables), we are left with an unbalanced panel that covers 46 African countries from 1945 to 1999, for a total of 1,742 country-year observations. Our dependent variable is civil war onset. Civil war is a rare event: the mean number of civil war onsets is 0.019, and the median is zero. During and in the year of UNSC membership, 1.89 percent of all country-year observations experienced the outbreak of civil war, totaling 0.4 percent of all civil wars in Africa. Twenty-two of the 46 African countries in our sample experienced at least one civil war; Algeria, Angola, Burundi, Chad, Ethiopia, Rwanda, Somalia, Sudan, Uganda, and Zimbabwe experienced multiple civil wars. Table 1 provides a summary of the descriptive statistics.

Our primary independent variable of interest is *UNSC period*, coded 1 for the years countries serve on the UNSC and the year after UNSC membership. Table 2 above lists the Africa countries that have served in the UNSC. Only nine African countries have yet to serve on the UNSC: Angola, Central African Republic, Chad, Eritrea, Lesotho, Malawi, South

Africa, and Swaziland. Five of these nine African countries have never experienced civil war: Eritrea, Lesotho, Malawi, Mozambique, and Swaziland.

Table 1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max	Unit	Original source	# Obs
Civil War Onset	0.019	0.136	0	1	Binary	Fearon and Laitin (2003)	1742
UNSC period	0.092	0.290	0	1	Binary	http://www.un.org/sc/members.asp	1742
Prior war	0.153	0.360	0	1	Binary	Fearon and Laitin (2003)	1742
GDP per capita	1.284	1.247	0.215	11.08	Log	Fearon and Laitin (2003)	1699
Log (population)	8.554	1.220	5.598	11.71	Log	Fearon and Laitin (2003)	1740
Log (% mountainous)	1.573	1.410	0	4.421	Log	Fearon and Laitin (2003)	1742
Noncontiguous state	0.014	0.119	0	1	Binary	Fearon and Laitin (2003)	1742
Oil exporter	0.141	0.348	0	1	Binary	Fearon and Laitin (2003)	1742
New State	0.048	0.214	0	1	Binary	Fearon and Laitin (2003)	1742
Instability	0.160	0.367	0	1	Binary	Fearon and Laitin (2003)	1734
Democracy	-4.119	5.485	-10	10	Ordinal	Fearon and Laitin (2003)	1727
Ethnic fractionalization	0.611	0.257	0.036	0.925	Fraction	Fearon and Laitin (2003)	1742
Religious fractionalization	0.424	0.231	0	0.783	Fraction	Fearon and Laitin (2003)	1742
Aid per population	59588	60281	0.246	660899	Fraction	Fearon and Laitin (2003)	1323

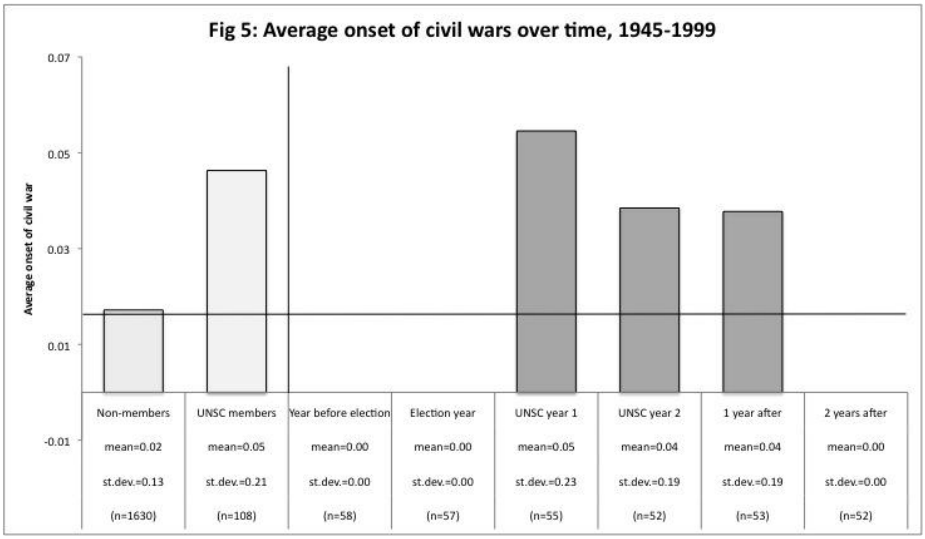
Table 2: UNSC participation for the African Group (countries experience the onset of civil war during *UNSC Period* in bold) 1946-1999

Country	UNSC period
Algeria	1968-1969; 1988-1989
Benin	1976-1977
Botswana	1995-1996
Burkina Faso	1984-1985
Burundi	**1970-1971**
Cameroon	1974-1975
Congo	1986-1987
Djibouti	**1993-1994**
Egypt	1945-1946; 1949-1950; 1961-1962;1984-1985;1996-1997
Ethiopia	1967-1968; 1989-1990
Gabon	1978-1979; 1998-1999
Gambia	1998-1999
Ghana	1962-1963; 1986-1987
Guinea	1972-1973
Guinea Bissau	**1996-1997**
Ivory Coast	1964-1965; 1990-1991
Kenya	1973-1974; 1997-1998
Liberia	1961-1962
Libya	1976-1977
Mali	1966-1967
Mauritania	1974-1975
Mauritius	1977-1978
Morocco	1963-1964; 1992-1993
Namibia	1999-2000
Niger	1980-1981
Nigeria	**1966-1967; 1978-1979; 1994-1995**
Rwanda	1994-1995
Senegal	**1968-1969; 1988-1989**
Sierra Leone	1970-1971
Somalia	1971-1972
Sudan	1972-1973
Tanzania	1975-1976
Togo	1982-1983
Tunisia	1959-1960; 1980-1981
Uganda	**1966-1967; 1981-1982**
Zambia	1969-1970; 1979-1980; 1987-1988
Zimbabwe	**1983-1984; 1991-1992**

Never members : Angola, Central African Republic, Chad, Eritrea, Lesotho, Malawi, Mozambique, South Africa, Swaziland,

Sources: http://www.un.org/sc/members.asp

Is *UNSC period* correlated with onset of civil wars? Figure 5 presents descriptive evidence concerning the pattern of civil war onsets over time. Consider first the left-hand side of the figure. 5 percent of *UNSC period* country-year observations experience a civil war onset, compared to 2 percent for non-*UNSC period* ones. Next, consider the right-hand part of the figure. The average onset of civil wars for all African countries is 0.02. There are no civil war onsets leading up through the UNSC election year.[8] The mean number of civil wars spikes to 0.05 in the first year on the UNSC, and remains constant at 0.04 in the second year on the UNSC and in the year after the UNSC term ends. A t-test indicates that the difference between civil war onsets that occur during the *UNCS period* and those that occur at other times is statistically significant at the 0.05 level (t-statistic=-2.40). The descriptive statistics thus support the hypothesis that African countries that are elected to the UNSC experience a higher likelihood of civil war onset in the years during and in the year after UNSC membership.



Note: The horizontal line shows the mean onset of civil wars across the entire sample of African countries (0.019, n=1,738).

[8] Dreher and Vreeland (2010) find that civil war onset is a negative predictor of UNSC membership. Ongoing civil wars do not significantly predict UNSC membership.

5.2 Regression Analyses

We now turn to testing our hypothesis using regression analysis. The dependent variable is civil war onset and the independent variable is *UNSC period*. Our control variables come from Fearon and Laitin (2003). We run two regression models – rare events logit, and conditional logit to account for country fixed effects. We implement rare events logit to account for the fact that our dependent variable is a dichotomous one, with only 1.8 percent of our observations coded as one (see King and Zeng 2001, 2002). Conditional logit addresses time-invariant country-specific heterogeneity. For each of these models, we begin by including a set of control variables (lagged one year) from Fearon and Laitin (2003). We then include cubic polynomial time variables to account for time trends (Carter and Signorino 2010). All specifications use robust standard errors clustered by country.

Table 3 presents the regression results. The coefficient on the key independent variable is positive and statistically significant across all four econometric specifications at the 0.05 level of significance. Our preferred specification for the rare events logit estimator is the one that includes the cubic polynomial time variables (Table 3, Column 2). *UNSC period* is strongly and positively correlated with civil war onset (b=1.106, p=0.034). Holding the other control variables constant at their median values, a country that gets elected to the UNSC is three times more likely to experience a civil war during the *UNSC period* – its risk of civil war onset increases from 1.6 percent to 4.7 percent.[9] Our preferred overall specification is the conditional logit with fixed effects estimator (Table 3, column 4), for which our key independent variable is statistically significant at the 0.05 level (b=1.373, p=0.054). This result supports our interpretation that our findings are not driven by

[9] Results obtained using CLARIFY software. See King, Tomz, and Wittenberg (2000). The change in the risk of civil war onset is 0.029, and the 95% confidence interval is from 0.001 to 0.102.

unobserved, between-country heterogeneity, and that our hypothesized UNSC effect operates at the country-level.

With regards to the other control variables, prior war, GDP per capita, population, mountainous terrain, noncontiguous state, and new state are significantly correlated with civil war onset. GDP per capita is negative and significant for the fixed effect logit specifications, which confirms the general consensus within the civil war literature that increased state capacity reduces the probability of civil war onset. Similar to Nielsen et al (2010), we do not attempt to interpret this result as our regression models were designed primarily to estimate the causal effect of *UNSC period* on civil war onset.

Table 3: The Impact of UNSC period on Onset of African Civil War

	(1) Rare Events Logit	(2) Rare Events Logit	(3) Fixed Effects Logit	(4) Fixed Effects Logit
UNSC period	0.956**	1.106**	1.337**	1.373**
	(0.478)	(0.52)	(0.526)	(0.54)
Prior war	-1.458	-1.722**	-4.131***	-4.340***
	(0.932)	(0.856)	(0.731)	(0.752)
GDP per capita	-0.229	-0.452	-2.448***	-3.305***
	(0.26)	(0.407)	(0.941)	(0.991)
Log (population)	0.282	0.264	4.561***	10.392
	(0.202)	(0.237)	(1.57)	(6.544)
Log (% mountainous)	0.268**	0.297**		
	(0.133)	(0.141)		
Noncontiguous state	2.209**	2.049*		
	(1.01)	(1.125)		
Oil exporter	0.326	0.205	1.298	1.454
	(0.556)	(0.745)	(0.802)	(0.916)
New State	1.397**	2.329***	2.400*	2.801**
	(0.676)	(0.842)	(1.226)	(1.332)
Instability	0.665	0.643	0.363	0.37
	(0.473)	(0.511)	(0.578)	(0.639)
Democracy	0.023	0.023	-0.004	0.054
	(0.043)	(0.056)	(0.081)	(0.097)
Ethnic fractionalization	-0.888	-0.737		
	(0.826)	(0.804)		
Religious fractionalization	0.549	-0.098		
	(0.927)	(1.089)		
Constant	-6.431***	-6.018*		
	(1.697)	(3.567)		
Country Dummies	NO	NO	YES	YES
Cubic polynomial time	NO	YES	NO	YES
Propensity score	NO	NO	NO	NO
Number of obs.	1684	1684	809	809

5.3 The Selection Problem

Because civil war onset is a negative predictor of UNSC membership, one might plausibly question whether there exist countries that are both more prone to civil war and more likely to serve on the UNSC. To further address the possibility of a selection problem, we employ a propensity score matching approach suggested by Rosenbaum and Rubin (1983)[10]. Propensity score matching artificially simulates a randomized experiment by creating control and treatment groups through matching on observables. Consequently, these two groups have similar observable characteristics, and differ only in the treatment variable. Any statistically significant difference in the outcome variable between both groups can then be attributed to the treatment, to the extent that the groups do not differ in unobservable characteristics. We define the treatment as *UNSC period*, which is a dummy that equals one for the years during and the year after UNSC membership, and 0 otherwise.[11] The outcome variable is civil war onset.

Implementing propensity score matching consists of two steps. We first calculate the propensity score for *UNSC period* – the probability that a country will receive the treatment – using a conditional logit model to account for the fact that only two countries can be selected from the African region per year. We include a range of variables proposed by Dreher and Vreeland (2010) as significant determinants of UNSC membership for African countries: rotation, population size, and change in GDP growth rate.[12] All controls are lagged by one

[10] See also Imai and van Dyk (2004), Smith and Todd (2005), and Dehejia (2005).

[11] Alternatively, one could define the treatment as "the year that an African country is elected to the UNSC" and the outcome as "the average number of civil war onset during *UNSC period*". We choose not to do so because

[12] Some argue that only significant predictors should be included as covariates in matching. See Hopkins and Simmons (2005) and Ho, Kosuke, King, and Stuart (2007).

year to minimize post-treatment bias. We find that all controls are significant at the 0.01 level (see Table 4a).

4a) Estimating the propensity of UNSC membership (conditional logit, conditioned on year)

Variable	Significant predictors
Rotate	-3.520***
	(-0.508)
Population share (t-1)	0.357***
	(0.068)
GDP change	0.259***
	(0.092)
Number of observations:	1481
Log likelihood:	-391.8
Pseudo R2:	0.06

To improve balance among the covariates between the treated and control groups, we coarsen the data using a Monotonic Imbalance Bounding algorithm, which allows users to define ex-ante the maximum imbalance allowed for the set of covariates (see Iacus, King, and Porro 2008 and Blackwell, Iacus, King, and Porro 2010). The $\mathbf{l}_1$ statistic, which provides a measure for the degree of imbalance in the pre-matched covariates, decreases from 0.83 to 0.73, indicating an improvement in the overall balance.[13]

Next, we match each treated observation against a non-treated observation with a similar propensity score. Note that there are several propensity score estimators that one can use. A common basic idea underlies all these estimators. For the context of this paper, for each country *i* in the *UNSC period* group, a comparable group of non-*UNSC period* observations has to be found. Matches are constructed on the basis of a neighborhood $\aleph(e_i)$, where e_i is the propensity score for country i, estimated from the conditional logit regression above. Let N_0 denote the number of non-treated observations and N_1 denote the number of treated observations. Then, we define country j as those non-*UNSC period* members who are

[13] 0 indicates perfect balance, and 1 maximal imbalance.

neighbors to country *i*, for whom $e_j \in \mathrm{N}(e_j)$, i.e. the set of countries $A_i = \{j \mid e_j \in \aleph(e_j)\}$. The effect of treatment for each observation *i* in the group of *UNSC period* members is estimated by subtracting the weighted average of the outcome of non-*UNSC period* members from the outcome of the treatment observation *i* (see Heckman et al., 1999):

$$Y_{i1} - \sum_{j=1}^{N_0} w(i,j) Y_{j0} \,. \qquad [1]$$

Matching estimators differ in the weights $w(i,j) \in [0,1]$ with $\sum_{j=1}^{N_0} w(i,j)$ for the members of the comparison group. We implement three-to-one nearest neighbor matching.

Tables 4b and 4c report the results from a post-matching balancing test for both the non-coarsened and coarsened matching samples respectively. Both reveal that the differences in the means between the covariates for the treated and control groups are statistically significant for the unmatched sample but statistically insignificant for the matched sample, suggesting that we have successfully matched on observables. Importantly, the differences in the means between the covariates for the treated and control groups for both the unmatched and matched samples are of greater statistical significance for the non-coarsened sample than the coarsened one. This indicates that coarsening has indeed reduced the imbalance among the pre-matched and matched covariates.

We estimate the effect of *UNSC period* on civil war onset – the average effect of the treatment on the treated – for both the non-coarsened and coarsened samples by taking the average difference across the matched observations. Table 4d reports the results. Focusing on the coarsened sample, we find that *UNSC period* increases average civil war onset by 3.8 percent, a finding that is significant at the 0.05 level (t-statistic=2.04). Substantively, since the mean number of civil wars for all African countries is 0.019, African countries that are elected to the UNSC experience a doubling in the average number of civil war onsets in the years during and after UNSC membership.

4b) Balancing test for matching variables: Uncoarsened Data

		Mean UNSC period obs.	Mean non-UNSC period obs.	t-statistic
Rotate	Unmatched sample:	0.359	0.452	4.07***
	Matched sample:	0.349	0.355	0.2
Population share (t-1)	Unmatched sample:	8.915	8.603	2.95***
	Matched sample:	8.915	8.856	0.41
GDP change	Unmatched sample:	7.391	7.326	0.94

4c) Balancing test for matching variables: Coarsened Data

		Mean UNSC period obs.	Mean non-UNSC period obs.	t-statistic
Rotate	Unmatched sample:	0.359	0.401	2.47***
	Matched sample:	0.349	0.339	0.98
Population share (t-1)	Unmatched sample:	8.915	8.674	2.36**
	Matched sample:	8.915	8.86	0.39
GDP change	Unmatched sample:	7.391	7.367	0.34
	Matched sample:	7.391	7.322	0.75

4d) Comparison of mean Civil War Onset (with and without propensity score matching):

	Uncoarsened		Coarsened	
	Unmatched sample:	**Matched sample:**	Unmatched sample:	**Matched sample:**
UNSC period	0.473	**0.473**	0.473	**0.473**
non-UNSC period	0.154	**0.011**	0.013	**0.009**
Difference	0.032	**0.036**	0.034	**0.038**
(std. error)	0.012	**0.019**	0.012	**0.019**
t-statistic	2.75***	**1.90***	2.93***	**2.04****

5.4 Integrating Matching with Regression Analyses

Following Ho, Imai, King, and Stuart (2007), we include the propensity score as a control variable on our preferred specification (Table 3, Column 4) to reduce the sensitivity of our results to the parametric form imposed by the regression models.[14] The core idea is to use matching as a nonparametric preprocessing step to remove unmatched observations prior to implementing regular regressions models. Table 5 presents our findings. We obtain similar results to our regular regressions in Table 3. *UNSC period* increases in both statistical and substantive significance (b=1.979, p=0.004) when compared to the baseline preferred specification in Table 3, Column 4 (b=1.373, p=0.054). Overall, the robustness of our results to alternative model specifications increases our confidence that a causal relationship exists between *UNSC period* and civil war onset.

[14] See Copelovitch 2010 and Davis 2010 for empirical applications of Ho, Imai, King, and Stuart (2007).

Table 5: The Impact of UNSC membership on Onset of African Civil War, using Pre-processed matched data

	Fixed Effects Logit
UNSC period	1.979***
	(0.647)
Prior war	-7.628***
	(2.772)
GDP per capita	-4.636**
	(1.87)
Log (population)	38.256**
	(17.494)
Log (% mountainous)	
Noncontiguous state	
Oil exporter	1.564
	(1.631)
New State	3.920**
	(1.808)
Instability	0.487
	(0.884)
Democracy	0.12
	(0.139)
Ethnic fractionalization	
Religious fractionalization	
Constant	
Country Dummies	YES
Cubic polynomial time	YES
Propensity score	YES
Number of obs.	690

5.5 Positive or Negative Aid Shocks?

Our findings reveal a positive correlation between *UNSC period* and civil war onset. To the extent that *UNSC period* is indeed an exogenous measure of temporary aid shocks, then our findings plausibly suggest a causal relationship between temporary aid shocks and civil war onset. Our analyses thus far, however, do not distinguish among alternative causal mechanisms that account for how UNSC membership affects civil war onset. Specifically, it remains agnostic with regards to whether it is *temporary positive aid shocks* or *temporary negative aid* shocks that increase the likelihood of civil war onset.

As a preliminary test of the causal mechanism linking UNSC membership and civil war onset, and to obtain unbiased estimates for the key independent variable, we employ the seemingly unrelated bivariate probit model, which is a structural equation model that is functionally equivalent to a maximum likelihood variant of the two-stage least squares (2SLS) estimator (Greene 2000, pp. 852-855). Our choice of regression model is appropriate due to the binary nature of both our outcome variable and single endogenous regressor.[15]

Our aid variables are taken from AidData (Nielson et al 2010), a project-level aid database that provides a more comprehensive record of annual aid flows than OECD's aid database. We first aggregate the project-level aid variables to the country-year level, and focus on commitments rather than disbursements because AidData's pre-2002 disbursement data proves unreliable (Nielsen et al 2011, pp. 6). Following Nielsen et al (2011), we define a positive aid shock as a dummy variable that equals one for all two-year average changes in commitment aid flows that are above the 85th percentile of all such aid changes, and "0" otherwise. Similarly, a negative aid shock is defined as a dummy variable that equals one for

[15] The instrumental variables probit model is appropriate only for a binary outcome variable with a continuous endogenous regressor.

all two-year average changes in commitment aid flows that are below the 15th percentile of all such aid changes, and 0 otherwise. In other words:

$$aidshock_t = [(aid/pop_t - aid/pop_{t-1}) + (aid/pop_{t-1} - aid/pop_{t-2})]/2$$

$$positiveaidshock_t = \begin{cases} 1; \text{ if aidshock}_t > 85th \text{ percentile} \\ 0; \text{otherwise} \end{cases}$$

$$negativeaidshock_t = \begin{cases} 1; \text{ if aidshock}_t < 15th \text{ percentile} \\ 0; \text{otherwise} \end{cases}$$

Table 6 presents the regression results using UNSC membership to instrument for temporary positive aid shocks. UNSC membership is strongly correlated with positive aid shocks (b=0.384, p=0.000), and predicted aid shocks are strongly correlated with civil war onset (b=3.085, p=0.000). The rho correlation parameter from the Wald test of exogeneity is very significant (chi-squared=0.000<0.05), and we therefore reject the null hypothesis that the error terms in the structural and reduced-form equations for the single endogenous variable are uncorrelated (see Wooldridge 2002, pp. 472-477). In other words, positive aid shocks are indeed endogenous to civil war onset, and our choice of instrumental variables as an identification strategy is therefore an appropriate one. Holding the other covariates constant at their median values, the marginal effect of receiving a positive aid shock is to increase the probability of civil war onset by 0.83, an effect that is statistically significant at the 0.01 level (p=0.000).

Next, we implement the seemingly unrelated bivariate probit model using alternative definitions of temporary positive aid shocks (80th and 90th), the results of which are presented in Table 7 below. UNSC membership is strongly correlated with positive aid shocks at the 0.01 level and predicted aid shocks are strongly correlated with civil war onset at the 0.01 level across all specifications. Interestingly, the marginal effect of predicted positive aid shocks on civil war onset increases as the severity of the predicted positive aid shock increases, suggesting that more extreme aid shocks increase the probability of civil war onset.

Table 6: Seemingly Unrelated Bivariate Probit Instrumental Variables Model for Positive Aid Shocks

	1st Stage	2nd Stage
UNSC membership	0.384***	
	(0.147)	
Predicted aid shocks		3.085***
		(0.165)
Prior war	-0.255	-0.660**
	(0.171)	(0.316)
GDP per capita	0.024	-0.350***
	(0.042)	(0.123)
Log (population)	-0.137***	0.207*
	(0.047)	(0.111)
Log (% mountainous)	-0.117***	0.190***
	(0.04)	(0.044)
Noncontiguous state	0.117	0.518
	(0.532)	(0.632)
Oil exporter	-0.231	0.199
	(0.154)	(0.224)
Instability	0.017	0.254
	(0.14)	(0.166)
Democracy	-0.006	0.022
	(0.01)	(0.015)
Ethnic fractionalization	-0.263	0.086
	(0.23)	(0.252)
Religious fractionalization	-0.385	-0.457
	(0.243)	(0.385)
Constant	-8.955*	-1.417
	(5.32)	(5.042)
Country Dummies	NO	NO
Cubic polynomial time	YES	YES
Number of obs.	1116	1116

Table 7: Seemingly Unrelated Bivariate Probit Instrumental Variables Model for Various Positive Aid Shocks Definitions

	80th percentile	85th percentile	90th percentile
UNSC membership	0.375***	0.384***	0.423**
	(0.127_	(0.147)	(0.176)
Predicted positive aid shocks	2.918***	3.085***	2.832***
	(0.152)	(0.165)	(0.183)
Marginal effect of positive aid shocks	0.69***	0.75***	0.73***
Wald test of exogeneity (p value)	0	0	0

The results are less clear regarding temporary negative aid shocks. The year after UNSC membership proved a weak instrument for temporary negative aid shocks – it did not satisfy the instrument relevance criteria. This should not surprise, as there are only two country-year observations that experienced a civil war onset in the year after UNSC membership.

Overall, these results provide tentative evidence for our causal mechanism linking UNSC membership to civil war onset. There is strong evidence suggesting that temporary positive aid shocks increase the likelihood of civil war onset, and that this effect is greater the more extreme the aid shock. Due to data limitations, we do not have enough evidence to either accept or reject the hypothesis that temporary negative aid shocks increase the likelihood of civil war onset, though our analysis seems to suggest that temporary negative aid shocks have no effect on civil war onset.

5.5 Robustness Checks

We implement our core specifications using alternative definitions of civil war onset obtained from the meta-dataset in Sambanis (2004), and found our results remain the same.[16] We also implement the seemingly unrelated bivariate probit model using a GDP normalized definition of aid per capita, and with OECD disbursement data, and results hold. That our results are robust to these additional checks increases our confidence in our hypothesis.

A cursory survey of the 7 cases of civil war that occurred in the years during and in the year after UNSC membership suggests that governments either initiated civil wars in an effort to crush rebel groups, or responded with overwhelming force to regular rebel attacks (see Table 7). The anecdotal evidence therefore supports state capacity arguments that temporary positive aid flows increase the propensity for civil war.

[16] These results will be made available in the replication materials.

Table 7: Survey of Cases of Civil War Onset in Year After UNSC Membership

Country	Served on UNSC	Civil War Onset	Details	Aid Shocks?	Fits Hypothesis
Burundi	1970-1971	1972	Hutu officers initiate rebellion, Tutsi government begin genocide	N/A	**N/A**
Djibouti	1993-1994	1993	Afar rebels attack government installations, government responds	**Yes**	**Yes**
Guinea Bissau	1996-1997	1998	President Viera fires armed forces chief-of-staff Ansumane Mane, accusing him of supporting separatists in Casamance, Senegal. An army mutiny follows with most of the army's 6000 soldiers joining the rebels. Troops from Senegal and Guinea (Conarky) move to protect the president	**Yes**	**Yes**
Nigeria	1966-1967	1967	Biafra secessionist movement, Northern leaders coup	**Yes**	**No**
Senegal	1988-1989	1989	Escalation of existing low-level conflict	**Yes**	**Yes**
Uganda	1981-1982	1981	Begins with an attack on army installation by the NRA rebel group, government responds	**Yes**	**Yes**
Zimbabwe	1983-1984	1983	Mugabe's Fifth Brigade deployed to crush Matebeland uprisings	**Yes**	**Yes**

Note: These cases derive from Fearon and Laitin (2003).

6. Conclusion

Our main results demonstrate that African countries that are elected to the UNSC will experience a higher likelihood of civil war onset in the years during and in the year after UNSC membership. We argue that this observed effect is due to the temporary positive aid shocks that these countries receive when they serve on the UNSC. Our additional tests provide tentative evidence that temporary positive aid shocks increase the likelihood of civil war onset. Due to data limitations, we make no causal claim about the effect of temporary negative aid shocks on civil war onset.

Future research should focus on testing whether temporary aid shocks differ both substantively and conceptually from sustained aid shocks. One possible avenue would be to code for aid shocks in a manner that distinguishes temporary aid shocks from sustained ones, and analyzing the effect of such aid shocks on both violent armed conflict and civil war onsets. Alternatively, one could analyze the extent to which civil wars that result from temporary aid shocks differ from those that arise from sustained aid shocks, with regards to the duration of conflict and the number of battle deaths.

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