Opportunity Cost, State Capacity, and the Economic Drivers of Civil Conflict

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Abstract

Although the literature is near-unanimous in finding that per capita income and civil conflict are negatively associated, unambiguous interpretations of how exactly the two are connected remain elusive. Two theories claim to explain this association: the first proposes that citizens in poor countries face a lower opportunity cost of fighting, and the other argues that poor governments lack the resources to suppress rebellion. Much of the literature treats these causal mechanisms as mutually exclusive. In this paper, I draw on existing literature to construct a model which treats these two mechanisms as parallel processes and explains how they interact. I then test my argument by using instrumental variables to identify exogenous variation in the incomes of citizens and governments. I find mixed evidence in support of my argument. While shocks to citizens' incomes and government income are both associated with a higher likelihood of civil conflict, I find no evidence that the two are mutually reinforcing in this regard. I conclude by interpreting these results and suggesting avenues for future research.

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1 Introduction

An extensive body of political science and economics literature has sought to explore the economic conditions which drive civil conflict. While not denying that ethnic hostilities, political tensions, and other social factors play significant roles in civil conflict, economic theories of civil conflict argue that certain economic conditions are often sufficient for civil conflict to

occur, whereas grievances, no matter how strong, are neither necessary nor sufficient. This literature is near-unanimous in finding that per capita income and civil conflict are negatively associated. Indeed, Hegre and Sambanis (2006) identify low income and growth as the most robust predictors of civil conflict from among dozens of other variables.

However, unambiguous interpretations of how exactly low income and growth are connected to civil conflict remain elusive. This relationship is typically explained using one of two theories. The first, which I will call the "opportunity cost" theory, proposes that income and conflict are negatively associated because the opportunity cost of fighting is low in poor countries—in other words, citizens of poor countries simply have less to lose by staging a rebellion (Collier and Hoeffler, 1998, 2004). The second theory, which I will call the "state capacity" theory, argues that income and conflict are negatively associated because poor governments lack the resources necessary to suppress rebellions (Fearon and Laitin, 2003). Much of the income-conflict literature treats these two proposed causal mechanisms as mutually exclusive. While some authors acknowledge that both mechanisms may have an effect, most focus their theories and empirical analyses around one mechanism or the other.

I consider the possibility that these explanations need not be mutually exclusive. I build upon Chassang and Padro i Miquel's (2009) model of economic shocks and civil conflict to propose how the opportunity cost and state capacity mechanisms might function in parallel, and how their underlying causal mechanisms are related. I use this extended model to derive three hypotheses. I then test these hypotheses by using instrumental variables to identify exogenous variation in the incomes of citizens and governments. I find mixed evidence in support of my argument. Shocks to citizens' incomes and shocks to government income are strongly and significantly associated with a higher likelihood of civil conflict, suggesting that the opportunity cost and state capacity mechanisms are not mutually exclusive. I find no evidence that shocks to citizens' and government incomes are mutually reinforcing. I conclude, however, that these results are not robust. Finally, I reevaluate my research design

in light of these results, and derive a few suggestions for future research on the relationship between income and conflict.

2 Literature Review

2.1 Two theories of civil conflict

Early civil conflict literature disagreed on how to interpret the association between low income and civil conflict. Most related income to conflict in terms of one of two theories: the "opportunity cost" theory and the "state capacity" theory. These theories have since formed the basis for an extensive literature investigating the economic drivers of civil conflict.

The opportunity cost theory was developed in two papers by Paul Collier and Anke Hoeffler (Collier and Hoeffler, 1998, 2004). Collier and Hoeffler (1998, 2004) construct a theory in which the economic incentives faced by prospective rebels constitute the decisive factor behind the occurrence of civil conflict. For a civil conflict to occur, armed rebellion must provide rebels with the chance to make themselves materially better-off than they would be otherwise. Collier and Hoeffler interpret income and growth as proxies for the opportunity cost of fighting faced by rebels, and argue that income and conflict are negatively related because the opportunity cost of fighting is lower in poor countries, where rebels have less to lose.

The state capacity theory originated in an influential paper by James Fearon and David Laitin. Instead of focusing on rebels' incentives, Fearon and Laitin (2003) propose that the occurrence of civil conflict depends on whether the state possesses the military and administrative capabilities necessary to defeat an insurgency—characteristics which are labeled "state capacity" in later literature. This theory leads Fearon and Laitin to interpret per capita income as a proxy for state capacity, and argue that income and civil conflict are neg-

atively related because the governments of poor countries lack the state capacity necessary to maintain order (Fearon and Laitin, 2003; Fearon, 2008).

2.2 Two impediments to our understanding

Perhaps the largest obstacle impeding our understanding of the income-conflict association is the fact that the opportunity cost and state capacity mechanisms are observationally equivalent. The same negative association between income and conflict could be construed as evidence for either mechanism. Though several scholars have proposed that both mechanisms may simultaneously drive the negative association between low income and civil conflict, in practice most of the civil conflict literature treats these two mechanisms as mutually exclusive. Since the theoretical literature tends to explore only one causal mechanism at a time, it is unable to suggest which empirical evidence would support the existence of one mechanism versus the other (Blattman and Miguel, 2010). The empirical literature, in turn, either remains agnostic as to which mechanism drives the income-conflict association, or treats the two mechanisms as mutually exclusive. Furthermore, neither the theoretical nor empirical literature takes seriously the possibility that the opportunity cost and state capacity mechanisms may be mutually reinforcing.

A second impediment to our understanding of the income-conflict association is the possibility that both the opportunity cost and state capacity theories are wrong to posit a straightforward causal relationship, and that income is in fact endogenous with respect to conflict. Such arguments are not without merit. One possibility is that both conflict and income are jointly influenced by some other variable. For example, Besley and Persson (2008, 2010) contend that the opportunity cost of fighting, state capacity, and the occurrence of civil conflict are jointly determined by a state's decision to invest in developing its own administrative capabilities in the long term. They find that states which anticipate civil conflict choose instead to invest in short-term defensive capabilities. Another possibility is

that conflict causes low income, perhaps by destroying physical capital and human capital. The expectation of future conflict might also dissuade investment. For example, Collier and Hoeffler (2004) find that past conflict is associated with lower economic growth in the future. Ray and Esteban (2017) identify several other papers corroborating this finding. The threat of endogeneity has led scholars interested in explaining to income-conflict association to search for situations in which income variation is clearly exogenous.

2.3 Economic shocks

To circumvent these two obstacles, scholars have narrowed their focus onto the association between negative economic shocks and civil conflict. Negative economic shocks (hereafter, *shocks*) provide an ideal setting for studying the income-conflict association for two reasons.

First, economic shocks can often be attributed to specific causes. This cause is sometimes a discrete event. For example, Besley and Persson (2011) use natural disasters to identify exogenous shocks to economic production and aid flows. In other cases, the cause of a shock can be identified using various instruments and proxies. For example, Miguel et al. (2004) use changes in rainfall as an instrument for income growth in less-developed, agrarian countries. Brückner and Ciccone (2010) and Bazzi and Blattman (2014) use changes in world commodity prices as proxies for income shocks.

Second, these instruments and proxies used to identify economic shocks often enjoy the added advantage of identifying exogenous variation in a specific type of income, rather than aggregate income. For example, Dube and Vargas (2013) use changes in cocoa and oil prices in Colombia to proxy for shocks to income accruing to labor and capital, respectively. De Ree and Nillesen (2009) use changes in U.S. GDP to identify exogenous variation in American aid flows, and by extension, income of the recipient governments. By identifying shocks to different kinds of income, these instruments and proxies can help discern between

conflict provoked via the opportunity cost mechanism or state capacity mechanism. This, in turn, helps us investigate whether the opportunity cost and state capacity mechanisms are mutually exclusive or mutually reinforcing.

In tandem with these empirical advances, scholars have begun extending the opportunity cost and state capacity theories by constructing models explaining the association between economic shocks and civil conflict. However, the empirical analysis of the shock-conflict association has largely outpaced its theoretical analysis. Aside from papers by Chassang and Padro i Miquel (2009) and Dal Bó and Dal Bó (2011), extensions of the opportunity cost and state capacity theories incorporating economic shocks have remained relatively informal. The informal nature of these theories has made it difficult to consider how the opportunity cost and state capacity mechanisms might be related.

3 Theory and Hypotheses

3.1 Economic shocks, opportunity cost, and civil conflict

Chassang and Padro i Miquel (2009) provide the most detailed extension of the opportunity cost theory in the context of economic shocks. Their model is based on the rationalist model of war developed by Fearon et al. (1995). The rationalist model proposes that since war is destructive by nature, there should exist some bargain that both actors would prefer to the outcome of a costly war. However, bargaining often fails to yield a mutually-preferred settlement. Fearon et al. (1995) identify commitment problems as among the main reasons why bargaining so often fails and leads to war. They define a commitment problem as a situation where a mutually-preferred bargain exists, but one actor faces an incentive to renege on the bargain later on. The presence of a commitment problem prevents that actor from making a credible commitment to honor the bargain, thereby rendering the bargain

unenforceable. Consequently, bargaining fails and war ensues.

Following this logic, Chassang and Padro i Miquel (2009) (hereafter, *C&P*) construct a model which seeks to explain the effect of economic shocks on civil conflict in terms of the incentives faced by prospective rebels. In this subsection, I will reconstruct this model and derive its key propositions following the steps outlined by C&P.

Consider a model with two groups: Group 1 and Group 2. Each group begins the game with an initial endowment of land L and labor l. Group 1 begins the game with an initial endowment of $1 - \lambda$ units of land, and Group 2 begins with $1 + \lambda$ units of land. There is a total of 2 units of land. Each group has 1 unit of labor l. Each group can use their land and labor to produce crops according to the following production function:

$$C(\theta, L, l) = \theta L l$$

where θ represents land productivity. So a group controlling both units of land could produce a maximum of 2θ units of crops.

Instead of farming, each group can also divert $c \in (0, 1)$ units of labor toward fighting to take the other group's land, leaving $1-c$ units of labor left over for farming. The opportunity cost of fighting is equivalent to the income foregone by fighting. Assuming the group in question wins and controls both units of land, the opportunity cost of fighting is given by $2\theta c$.

However, fighting is not inevitable. Instead of fighting, Group 1 and Group 2 can attempt to avert war by transferring land such that both groups prefer the bargain to war. We assume that each group has complete information and so knows whether a mutually-preferred bargain exists. C&P demonstrate that as long as Group 1 and Group 2 can see the entire set of bargains they mutually prefer to war, then unequal landholdings alone are not sufficient for war to occur. So for the sake of simplicity, from now on we will focus on the case where

each group controls one unit of land.

Instead, whether or not a bargain can sustain peace depends on the size of each group's offensive advantage. If one group's offensive advantage is sufficiently large, then a commitment problem prevents the groups from reaching a bargain that both prefer to war (see Chassang and Padro i Miquel (2009) for the proof).

Suppose now that the game takes place over an infinite number of periods t. At the start of each period, each group retains its land holdings from the previous period, and can bargain to avert war. If no bargain can be reached, then the groups fight for one period. The winner gets all the loser's land and production, and enjoys it for every future period.

Suppose also that land productivity varies, perhaps due to weather shocks or changes in the price of crops. Suppose that land productivity is a random variable with a well-defined expected value, $E(\theta) = \bar{\theta}$, and whose cumulative distribution function is given by $F(\theta)$. In each period t, θ_t is independently drawn, taking some value on the interval $(0, +\infty)$. Since both groups have perfect information, we assume that they start each period by accurately observing the value of θ_t, and then reaching a stable bargain if one exits. If no such bargain exists, then the groups fight, and the winner enjoys all land and production in every future period.

If a group plays peace, its expected payoff is given by:

$$\theta_t + \delta V^P$$

This expression is fairly intuitive. The first term is the output generated using one unit of land and one unit of labor in period t. The second term, δV^P is the discounted value of peace (if it is sustainable), where $\delta \in (0, 1)$ is a time discount factor. If the groups remain at peace for the rest of the game, then each group enjoys its single unit of land for every future

period. So the value of sustained peace is given by:

$$V^P = \frac{\bar{\theta}}{1-\delta}$$

If a group decides to attack in a first strike, its expected payoff is given by:

$$P\big(2\theta_t(1-c) + \delta V^V\big)$$

This expression is intuitive as well. With probability P, the attacker wins and seizes the other group's land for a total of two units of land. Since the attacker expends c units of labor fighting, it has $(1-c)$ units of labor left over for production. So in period t, the attacker produces $2\theta_t(1-c)$ units of output. The second term, δV^V is the discounted value of victory (should it be won) in every future period. The winning group gets to enjoy both units of land for every future period. So the value of victory in every future period is given by:

$$V^V = \frac{2\bar{\theta}}{1-\delta}$$

Relating the expected payoffs of peace versus attacking, we see that peace is possible when the following condition is satisfied:

$$\theta_t + \delta V^P > P\big(2\theta_t(1-c) + \delta V^V\big)$$

In other words, peace is possible if the payoff from peace in every period (LHS) is greater than the expected payoff from fighting in the first period and living with the outcome in every future period (RHS). Rearranging this condition, we get the following equation:

$$\theta_t\big(1 - 2P(1-c)\big) > \delta[PV^V - V^P] \tag{1}$$

It is now clear that this peace condition is violated if an economic shock causes θ_t to drop low enough.

We may now solve for the most efficient subgame perfect equilibrium for this model. C&P explain that the most efficient subgame perfect equilibrium involves players following a threshold strategy in which they play peace if θ_t is above some threshold $\widetilde{\theta}$, and fight if θ_t is below that threshold $\widetilde{\theta}$. This optimal threshold $\widetilde{\theta}$ is the lowest value of θ_t for which the above peace condition holds.

To solve for the most efficient subgame perfect equilibrium, we start with the peace condition (equation 1). Let us begin by obtaining the continuation value of peace *in equilibrium*, $\widetilde{V^P}$. C&P explain that $\widetilde{V^P}$ is obtained by solving following equation:

$$\widetilde{V}^P = F(\widetilde{\theta})\frac{1}{2}\big[2E(\theta \mid \theta < \widetilde{\theta})(1-c) + \delta V^V\big] + \big(1 - F(\widetilde{\theta})\big)\big[E(\theta \mid \theta > \widetilde{\theta}) + \delta \widetilde{V}^P\big]$$

Begin by looking at the first term. $F(\tilde{\theta})$ is the probability that θ_t falls below the threshold. $1/2$ is the probability of winning a symmetric conflict, in which neither player enjoys an offensive advantage. The term in brackets is the lifetime value of victory. Now, look at the second term. $1 - F(\tilde{\theta})$ is the probability that θ_t does not fall below the threshold, and so peace is possible. The bracketed term is the lifetime value of peace.

The solution to this equation is given by:

$$\widetilde{V}^P = \frac{\bar{\theta}}{1-\delta} - \frac{cF(\widetilde{\theta})E(\theta \mid \theta < \widetilde{\theta})}{1 - \delta\big(1 - F(\widetilde{\theta})\big)}$$

Note that the continuation value of peace *in equilibrium* has two parts. The first term is the value of peace should it continue forever. The second term is the expected cost of war, should it occur.

The value of victory in equilibrium is $\frac{2\bar{\theta}}{1-\delta}$, as before.

Plugging the threshold $\widetilde{\theta}$, the continuation value of peace in equilibrium, and the continuation value of victory in equilibrium in to the peace condition (equation 1), we get the following:

$$\widetilde{\theta}\big(1-2P(1-c)\big) = \delta\left[P\frac{2\bar{\theta}}{1-\delta} - \frac{\bar{\theta}}{1-\delta} + \frac{cF(\widetilde{\theta})E(\theta \mid \theta < \widetilde{\theta}}{1-\delta(1-F(\widetilde{\theta}))}\right]$$

Dividing both sides by $\big(1-2P(1-c)\big)$ and simplifying the bracketed term, we arrive at an equation that implicitly defines the optimal threshold $\widetilde{\theta}$ as:

$$\widetilde{\theta} = \frac{\delta}{1-2P(1-c)}\left[(2P-1)\frac{\bar{\theta}}{1-\delta} + \frac{cF(\widetilde{\theta})E(\theta \mid \theta < \widetilde{\theta}}{1-\delta(1-F(\widetilde{\theta}))}\right] \tag{2}$$

We can prove that this threshold $\widetilde{\theta}$ exists in a few steps. We know three things about the RHS of the threshold equation. First, we know that it is continuous. Second, we know that it is strictly positive. Third, we know that it has a loose upper bound, which is reached when the value of playing peace in equilibrium, $\widetilde{V^P}$, is zero. This upper bound is given by $\frac{\delta P}{1-2P(1-c)}\frac{2\bar{\theta}}{1-\delta}$. We also know something about the LHS of the threshold equation. We know that $\widetilde{\theta}$ can take any value in the interval $(0, +infty)$.

Therefore, there must be some values of $\widetilde{\theta}$ such that LHS $>$ RHS, and other values of $\widetilde{\theta}$ such that LHS $<$ RHS. By the above argument, C&P arrive at the following proposition, slightly paraphrased here:

Proposition 1. *For sufficiently low values of P, the most efficient subgame perfect equilibrium of the dynamic game with economic shocks is given by a stationary threshold strategy, where the threshold is the smallest solution to equation 1. For sufficiently high values of P, every equilibrium involves war at $t = 1$ for any realization θ_1.*

In other words, we see that war occurs in equilibrium if an economic shock brings the value

of θ_t low enough, assuming neither group enjoys an overwhelming offensive advantage.

The intuition behind Proposition 1 is straightforward when put in the context of citizens staging a rebellion against their government. Following an economic shock, citizens could spend that period farming, as they normally would. However, the returns from farming will be quite low. These low returns correspond to a low opportunity cost of fighting faced by citizens. Alternatively, citizens could spend the current period fighting the government for more land, with the knowledge that land productivity will regress to the mean in future periods. Consequently, citizens are better off in the long run fighting during the current period to acquire land which will provide high returns to farming in the future. So citizens fight the government for the duration of the economic shock.

If this model provides an accurate representation of the incentives faced by prospective rebels, then we should observe more civil conflict following shocks to citizens' incomes. So from Proposition 1, I derive my first hypothesis:

Hypothesis 1. *Shocks to citizens' incomes will increase the likelihood of civil conflict by lowering the opportunity cost of fighting.*

C&P also derive a second proposition, which I include here because it will become important in the next subsection. Paraphrasing slightly, Proposition 2 states that:

Proposition 2. *The threshold $\widetilde{\theta}$ defined by equation 2 is increasing in P.*

This is evident just from looking at the RHS of the threshold equation. In the term $\frac{\delta}{1-2P(1-c)}$, P is multiplied by -1 and is in the denominator. So increasing P makes the denominator smaller, which makes the whole term bigger. In the bracketed term, P is positive, so increasing P makes the bracketed term bigger. These two terms make up the RHS, which defines the threshold $\widetilde{\theta}$.

3.2 Economic shocks, state capacity, and civil conflict

Extensions of the state capacity theory in the context of economic shocks are somewhat less formalized. Nielsen et al. (2011) provide the most detailed of these extensions. While Nielsen et al. (2011) seek to explain the effect of foreign aid shocks on civil conflict in particular, their underlying theory should apply to any economic shock which reduces the government's income.

The informal argument articulated by Nielsen et al. (2011) is based on Powell (2006)'s extension of the rationalist model of war developed by Fearon et al. (1995). Powell attributes the commitment problems identified by Fearon et al. (1995) to sudden changes in the players' relative power. Such a change provides the strengthened player with an increased probability of victory. As a result, the weakened player must offer a better bargain to avoid being attacked. However, the weakened actor has an incentive to renege on that bargain should it regain power. The weakened actor is thus unable to credibly commit to uphold its end of the bargain. Knowing this, the strengthened actor chooses to attack while it possesses a heightened victory probability.

Nielsen et al. (2011) explicitly links such offensive advantages to the government's finances. By depriving the government of funds, an aid shock weakens the government, and makes citizens stronger in relative terms. Nielsen et al. (2011) is vague in explaining precisely how government funds lead to decreased state capacity, an issue to which I return in the discussion section. For now, I will assume that a cash-strapped government is weaker because it cannot afford to pay its soldiers and procure equipment.

In terms of Powell (2006), this shock to government income gives the citizens an offensive advantage. To avoid war, the weakened government must placate citizens with a better bargain than before. However, the government has an incentive to renege on that bargain once aid flows resume and the government regains funds. As a consequence, the government

cannot credibly commit to uphold its end of the bargain. Knowing this, citizens choose to attack while they possess a heightened offensive advantage.

It is not too complicated to formalize this argument and incorporate it into C&P's original model from the previous section. By doing so, we can deduce how the opportunity cost and state capacity mechanisms might interact, reshaping the incentives faced by prospective rebels.

First, let us rename the groups. We will name Group 1 the Citizens, and Group 2 the Government (hereafter, I capitalize "the Citizens" and "the Government" when referring to actors in this model, and leave them uncapitalized when referring to "the citizens" or "the government" in a general sense). Suppose that the expected value of land productivity is now given by $\bar{\theta} = 1/2$. Second, suppose that the Citizens' and the Government's land productivities move independently of one another. We denote the Citizens' land productivity in period t by θ_t^C , and denote the Government's land productivity in period t by θ_t^G. We assume that each group has perfect information, and accurately observes the other group's θ_t at the start of each period.

Nielsen (2012) suggests that citizens' offensive advantage is inversely related to the government's income. So we redefine the Citizens' victory probability during period t as the following piecewise function:

$$P_t^C(\theta_t^G) = \begin{cases} 1 - \theta_t^G & \text{if } 0 < \theta_t^G \leq 1 \\ 0 & \text{if } \theta_t^G > 1 \end{cases} \tag{3}$$

Now, when θ_t^G equals its expected value $\bar{\theta} = 1/2$, the Citizens do not enjoy an offensive advantage: $P_t^C(1/2) = 1/2$. But note that when the Government experiences an economic shock, the falling value of θ_t^G causes the Citizens' victory probability to rise, giving the Citizens a temporary offensive advantage over the Government. Nielsen's (2012) argument does

not address the effect of citizens' income shocks on the government's victory probability. Therefore, we will assume that a shock to the Citizens' income does not affect the Government's offensive advantage, and will only focus on solving for the Citizens' equilibrium strategy.

If the Citizens enjoy a sustained peace, their expected payoff is given by:

$$\theta_t^C + \delta V^P$$

If the Citizens attack, their expected payoff is given by:

$$P_t^C(\theta_t^G)\big(2\theta_t^C(1-c) + \delta V^V\big)$$

Combining these expressions, we see that the Citizens refrain from attacking the Government if the following condition is satisfied:

$$\theta_t^C + \delta V^P > P_t^C(\theta_t^G)\big(2\theta_t^C(1-c) + \delta V^V\big)$$

Rearranging as in the previous subsection, this becomes:

$$\theta_t\big(1 - 2P_t^C(\theta_t^G)(1-c)\big) > \delta[P_t^C(\theta_t^G)V^V - V^P] \tag{4}$$

We may now solve for the Citizens' equilibrium strategy. As in the original C&P model, the Citizens' equilibrium strategy is a threshold strategy in which they play peace if θ_t^C is above some threshold $\widetilde{\theta}$, and fight if θ_t^C is below that threshold $\widetilde{\theta}$. We can find the value of that threshold $\widetilde{\theta}$ by following the steps detailed by C&P for their original model.

To solve for the most efficient subgame perfect equilibrium, we start with the new peace

condition (equation 4). Once again, we begin by obtaining the continuation value of peace *in equilibrium*, $\widetilde{V^P}$, by solving following equation:

$$\widetilde{V}^P = F(\widetilde{\theta})\frac{1}{2}\big[2E(\theta \mid \theta < \widetilde{\theta})(1-c) + \delta V^V\big] + \big(1 - F(\widetilde{\theta})\big)\big[E(\theta \mid \theta > \widetilde{\theta}) + \delta \widetilde{V}^P\big]$$

The solution to this equation is given by:

$$\widetilde{V}^P = \frac{1}{2(1-\delta)} - \frac{cF(\widetilde{\theta})E(\theta \mid \theta < \widetilde{\theta})}{1 - \delta\big(1 - F(\widetilde{\theta})\big)}$$

Note that the first term on the RHS of the equation above is now $\frac{1}{2(1-\delta)}$ instead of $\frac{\bar{\theta}}{1-\delta}$, because I stated earlier that $E(\theta_t^C) = E(\theta_t^G) = 1/2$ in this extended version of C&P's model.

By this same logic, the value of victory in equilibrium is now given by $\frac{2}{2(1-\delta)}$, which I leave unsimplified for now to make simplification of the new threshold equation easier.

Plugging the threshold $\widetilde{\theta}$, the future value of peace in equilibrium, and the future value of victory in equilibrium in to the peace condition (equation 4) and setting both sides equal, we get the following:

$$\widetilde{\theta}\big(1 - 2P_t^C(\theta_t^G)(1-c)\big) = \delta\left[P_t^C(\theta_t^G)\frac{2}{2(1-\delta)} - \frac{1}{2(1-\delta)} + \frac{cF(\widetilde{\theta})E(\theta \mid \theta < \widetilde{\theta})}{1 - \delta\big(1 - F(\widetilde{\theta})\big)}\right]$$

Dividing both sides by $\big(1 - 2P_t^C(\theta_t^G)(1-c)\big)$ and simplifying the bracketed term, we arrive at an equation that implicitly defines the optimal threshold $\widetilde{\theta}$ as:

$$\widetilde{\theta} = \frac{\delta}{1 - 2P_t^C(\theta_t^G)(1-c)}\left[\big(P_t^C(\theta_t^G) - 1\big)\frac{1}{2(1-\delta)} + \frac{cF(\widetilde{\theta})E(\theta \mid \theta < \widetilde{\theta})}{1 - \delta\big(1 - F(\widetilde{\theta})\big)}\right] \tag{5}$$

We can prove that this threshold exists using the argument laid out by C&P. We know three things about the RHS of the threshold equation. First, we know that it is continuous.

Second, we know that it is strictly positive. Third, we know that it has an upper bound, now given by $\frac{\delta}{1-2(1-c)}\frac{1}{1-\delta}$. This upper bound is achieved when $V^P = 0$ and $P_t^C \approx 0$. We also know something about the LHS of the threshold equation. We know that $\widetilde{\theta}$ can take any value in the interval $(0, +\infty)$.

Therefore, there must be some values of $\widetilde{\theta}$ such that LHS > RHS, and other values of $\widetilde{\theta}$ such that LHS < RHS.

We have therefore established a new version of Proposition 1, which states:

Proposition 1a. *For sufficiently low values of $P_t^C(\theta_t^G)$, the most efficient subgame perfect equilibrium of the dynamic game with economic shocks is given by a stationary threshold strategy, where the threshold is the smallest solution to equation 4. For sufficiently high values of $P_t^C(\theta_t^G)$ every equilibrium involves war at $t = 1$ for any realization θ_1^C.*

Now, we see that a war is inevitable following a sufficiently large negative shock to government income θ_t^G, even if there is no negative shock to citizens' income θ_t^C. This is because a shock to government income increases the citizens' offensive advantage $P_t^C(\theta_t^G)$ to such an extent that they are better off fighting, no matter their current land productivity. From this revised version of Proposition 1, I derive my second hypothesis:

Hypothesis 2. *Shocks to government income will increase the likelihood of civil conflict by temporarily raising citizens' victory probability.*

We can also derive a new version of Proposition 2:

Proposition 2a. *The threshold $\widetilde{\theta}$ defined by equation 5 is increasing in $P_t^C(\theta_t^G)$.*

This means that when θ_t^G falls, $P_t^C(\theta_t^G)$ rises, causing the threshold $\tilde{\theta}$ to rise. So following a shock to θ_t^G, a smaller shock to θ_t^C is necessary to provoke war.

In other words, the Citizens' threshold land productivity is increasing in citizens' victory probability. But now, note that citizens' offensive advantage is decreasing in the Govern-

ment's land productivity. So a shock to the Government's land productivity gives the Citizens a temporary offensive advantage, which consequently raises the Citizens' threshold land productivity below which war ensues. So if we accept Nielsen's proposition that government income shocks increase citizens' offensive advantage, it logically follows from this extended model that government income shocks determine how large a shock to citizens' income is necessary to provoke war. This leads to my third hypothesis:

Hypothesis 3. *Alongside a shock to government income, a smaller shock to citizens' income is necessary to provoke civil conflict.*

4 Measurement and Data

To test these three hypotheses, I must identify measurable variables corresponding to the occurrence of civil conflict, shocks to the opportunity cost of fighting, and shocks to state capacity. Since both the opportunity cost and state capacity theories come from large-N, cross-country studies, I will use country-years as my unit of analysis.

4.1 Measuring civil conflict

In both my extended model and the model by Chassang and Padro i Miquel (2009) from which it is derived, the Citizens and the Government begin the game in a state of peace, and fight for only one period. My theory does not pertain to the continuation or destructiveness of civil conflict. So to test my theory, I would like a measure of civil conflict that focuses only on the beginning of civil conflict.

A measure known as "civil conflict onset" satisfies these criteria. According to this measure, if civil conflict begins a given country year, the civil conflict onset variable takes a value of "1." For all successive years of civil conflict, the civil conflict onset variable takes a value of

"0." For country-years for which there is peace, the civil conflict onset variable also takes a value of "0." Measuring conflict by its onset has two desirable properties. First, it is the most prevalent dependent variable in the literature explaining the outbreak of civil conflict. Second, it is the dependent variable used by Miguel et al. (2004), whose dataset I use as the base for my own.

I will adhere to the UCDP/PRIO dataset's definition of civil conflict as a "contested incompatibility which concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle deaths" (Gleditsch et al., 2002).

4.2 Measuring opportunity cost shocks

My first hypothesis predicts that economic shocks cause conflict by lowering the opportunity cost of fighting faced by citizens. In this case, an opportunity cost shock is represented by the income citizens forgo by fighting. So to test Hypothesis 1, I need a measure which proxies for this foregone income. As an explanatory variable, I would also like this measure to satisfy two additional criteria. First, it must be precise—to proxy for the opportunity cost of fighting, this measure must not also pick up variation in government income. Second, it must be exogenous—this measure must have a good instrument available.

To measure opportunity cost shocks, I will use the identification strategy developed by Miguel et al. (2004), who use GDP growth as their main explanatory variable, instrumented by rainfall growth. Rainfall meets both restrictions for a good instrument identified by Angrist and Krueger (2001). First, rainfall and GDP growth are highly correlated in agrarian countries which lack extensive irrigation systems, where much of the population relies on rain-fed agriculture for income. Second, rainfall meets the exclusion restriction—it is a natural process which is unaffected by civil conflict, nor does it affect civil conflict through any other

mechanism. So GDP growth instrumented by rainfall satisfies my exogeneity criterion. This measure also satisfies my precision criterion—since Miguel et al. (2004) find that rainfall has no significant effect on government income, I can be sure that this explanatory variable will measure only changes in the opportunity cost of fighting, and not changes in state capacity.

The weakness of this identification strategy is that rainfall cannot instrument for growth in all countries during all periods. Later analyses by Ciccone (2011) and Miguel and Satyanath (2011) find that rainfall only works as an instrument for growth in sub-Saharan African countries before the year 2000. Nevertheless, growth instrumented by rainfall is still the best measure of opportunity cost shocks for the purposes of this thesis, because no other measure employed in the literature targets the opportunity cost mechanism so precisely.

I will define my explanatory variables and instruments measuring opportunity cost shocks as follows. $GDPgrowth_{it}$ is the proportional change in GDP in country i from years *t-1* to *t*. $RainfallGrowth_{it}$ is the proportional change in rainfall in country i from years *t-1* to *t*. $RainfallGrowth_{i,t-1}$ is the proportional change in rainfall in country i from years *t-2* to *t-1*.

4.3 Measuring state capacity shocks

My second hypothesis predicts that economic shocks cause civil conflict by lowering state capacity, thus weakening the government's ability to suppress a rebellion. More precisely, economic shocks are said to lower state capacity by depriving the government of funds. So to test Hypothesis 2, I will need an explanatory variable that measures shocks to government income. As before, this explanatory variable must measure only the causal mechanism of interest—in this case, state capacity shocks. It must also have a good instrument with which to ensure exogeneity.

Since the purpose of Nielsen et al. (2011)'s informal model is to describe how aid shocks affect civil conflict, changing aid flows are the natural choice to proxy for government in-

come shocks. I will define my explanatory variable measuring government income shocks as $AidGrowth_{it}$, which denotes the proportional change in aid received by country i from year *t-1* to year t.

Although the overall relationship between aid and conflict is quite complicated and context-specific, most literature linking aid growth to conflict via the state capacity mechanism in particular finds that aid and conflict are negatively related, all else equal (De Ree and Nillesen, 2009; Collier et al., 2009; Savun and Tirone, 2011, 2012).

Furthermore, I must be confident that $AidGrowth_{it}$ proxies for changes in government income without affecting the opportunity cost of fighting. Since most aid is fungible, changing aid flows are essentially equivalent to changes to government income (Feyzioglu et al., 1998). In addition, since the empirical relationship between aid flows and GDP growth is relatively weak, it is unlikely that aid significantly affects citizens' income and the opportunity cost of fighting, especially in the short-run.

To use $AidGrowth_{it}$ as an explanatory variable, I must also be sure that changes in aid are exogenous with respect to civil conflict. After all, donors might adjust aid flows based on their expectations of civil conflict in recipient countries. To satisfy the exogeneity criterion, I must also have an instrument which identifies exogenous variation in aid growth.

To measure exogenous shocks to government income, I will use a country's membership on the United Nations Security Council during the past two years as instruments for aid flows received by that country. I will define these instruments as $UNSCmember_{i,t-1}$, which takes a value of "1" if country i was on the Security Council during the previous year, and $UNSCmember_{i,t-2}$ which takes a value of "1" if country i was on the Security Council two years ago. I choose to lag these variables based on the assumption that it takes time for donors to adjust their aid flows upon learning the composition of the Security Council.

Security Council membership satisfies both criteria for a good instrument. First, Kuziemko

and Werker (2006) demonstrates that Security Council membership and foreign aid receipts are highly correlated, finding that countries on the UN Security Council receive significantly higher levels of foreign aid during their terms as nonpermanent members. Second, Security Council membership is assigned to the rotating members according to a somewhat-random process involving regional quotas and arbitrary term lengths. Furthermore, world events that temporarily increase the Security Council's importance (and increase aid flows to the rotating members) are also random with respect to a rotating member's accession a year or more earlier. On account of this randomness, Security Council membership is unlikely to affect a country's likelihood of civil conflict onset except via aid flows. Reverse causality is also less of a concern in the case of civil conflict onset, because it is harder for donors to predict the onset of conflict rather than its continuation, which is often obvious. So Security Council membership satisfies the exclusion restriction necessary for a good instrument.

4.4 Data

My unit of analysis is country years. Since using rainfall growth as an instrument for GDP growth only works for the sample of country-years used by Miguel et al. (2004), I use their dataset as my base dataset. I then augment the Miguel et al. (2004) dataset with data on aid flows and Security Council membership assembled by Nielsen et al. (2011), who uses the AidData database for the underlying aid data (Tierney et al., 2011). My final dataset is comprised of data on 41 sub-Saharan African countries from 1981 through 1999, for a total of 779 country-year observations. Summary statistics and data sources are displayed below in Table 1.

Table 1: Summary Statistics

VARIABLES	Number of Observations	Mean	Standard Deviation	Minimum	Maximum	Source
Civil conflict onset	555	0.069	0.253	0	1	UCDP/PRIO Armed Conflict Dataset
Civil war onset	625	0.037	0.188	0	1	UCDP/PRIO Armed Conflict Dataset
Civil conflict incidence	743	0.268	0.443	0	1	UCDP/PRIO Armed Conflict Dataset
Civil war incidence	743	0.167	0.373	0	1	UCDP/PRIO Armed Conflict Dataset
GDP growth (proportional change)	743	-0.005	0.071	-0.474	0.670	Penn World Table
Aid growth (proportional change)	734	0.304	1.640	-1	9.79	AidData
Rainfall growth (proportional change)	743	0.018	0.209	-0.550	1.677	Global Precipitation Climatology Project
Security Council membership	743	0.057	0.231	0	1	Nielsen et al. (2011)
GDP in 1981	743	1.164	0.901	0.316	4.832	Penn World Table
Polity2 score	743	-3.608	5.554	-10	9	Polity IV
Ethnic fractionalization	743	0.655	0.237	0.036	0.925	Fearon and Laitin (2003)
Religious fractionalization	743	0.487	0.186	0	0.783	Fearon and Laitin (2003)
$> 1/3$ export revenue from fuels	743	0.118	0.323	0	1	World Bank (WDI)
Log population	743	8.750	1.207	5.717	11.71	Fearon and Laitin (2003)
Terrain ruggedness	743	1.578	1.433	0	4.421	Fearon and Laitin (2003)

5 Estimation Procedures

Since my identification strategy involves instrumental variables and a dichotomous independent variable, I use probit models with endogenous regressors for my main analyses. A drawback of probit models is that they do not allow for fixed effects. So to test whether my main results are driven by characteristics specific to certain countries or years, I fit linear probability models with fixed effects as one of my robustness tests.

My main equation takes the following form:

$$\begin{aligned} Pr(ConflictOnset_{it} = 1) = \Phi\big(&\alpha_0 + \alpha_1 GDPgrowth_{it} \\ &+ \alpha_2 AidGrowth_{it} \\ &+ \alpha_3 (GDPgrowth \times AidGrowth)_{it} \\ &+ X_{it}\beta\big) \end{aligned}$$

where $\Phi(\cdot)$ denotes the standard normal cumulative distribution function, and X_{it} is a vector of controls.

My first-stage equations take the following forms:

$$GDPgrowth_{it} = a_0 + a_1 RainfallGrowth_{it} + a_2 RainfallGrowth_{i,t-1} + \varepsilon_{1,it}$$

$$AidGrowth_{it} = a_3 + a_4 UNSCmember_{i,t-1} + a_5 UNSCmember_{i,t-2} + \varepsilon_{2,it}$$

where the ε_{it} are error terms.

Each of the explanatory variables in the main equation above corresponds to one of my three hypotheses. To test Hypotheses 1, I include $GDPgrowth_{it}$, instrumented by $RainfallGrowth_{it}$ and $RainfallGrowth_{I,t-1}$. To test Hypothesis 2, I include $AidGrowth_{it}$ instrumented by $UNSCmember_{i,t-1}$ and $UNSCmember_{i,t-2}$. To test Hypothesis 3, I include an interaction term, $(GDPgrowth \times AidGrowth)_{it}$. It is technically neither necessary nor sufficient to have a statistically significant interaction term in a probit model to detect an interactive effect between two variables (Berry et al., 2010). Nevertheless, Brambor et al. (2006) suggests including an interaction term anyway.

I use two methods to test my hypotheses. First, I check whether the sign and statistical significance of the estimated coefficients on each explanatory variable are statistically significant. Second, I calculate the predicted probabilities of civil conflict onset at different values of the explanatory variables. I use this second method because I fit a nonlinear model, which means that the effect of each explanatory variable may vary in size and statistical significance depending on its value. For technical reasons, papers fitting nonlinear models with instrumented explanatory variables do not typically estimate predicted probabilities or effect sizes. I can do so in this paper, however, because the new "endogenous regression models" included in Stata 15 allow the user to estimate predicted probabilities and effect sizes in nonlinear models with endogenous regressors.

Finally, I use control variables from Fearon and Laitin (2003). which have become standard in the conflict literature. These controls include a country's level of democracy, ethnic fractionalization, religious fractionalization, a dummy for having a large proportion of exports from fuels, population, and terrain ruggedness. GDP at the beginning of the dataset (1981) is also included as a control.

6 Main Results

6.1 First-stage regression results

Table 2: First-Stage Regression Results

VARIABLES	(1) GDP growth (proportional change)	(2) Aid growth (proportional change)
Rainfall growth (proportional change)	0.05***	
	(0.02)	
Rainfall growth (lagged) (proportional change)	0.04***	
	(0.01)	
UNSC member (lagged once)		-0.05
		(0.08)
UNSC member (lagged twice)		-0.21**
		(0.09)
Constant	0.00	0.23***
	(0.00)	(0.04)
Number of observations	551	551

Standard errors in parentheses, clustered at country level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. χ^2 or F-statistics were not available for first-stage regressions.

The results of my first-stage regressions are displayed above in Table 2. In these models, each endogenous explanatory variable is regressed on its instruments using Ordinary Least Squares. The above results let us asses whether each instrument is strongly associated with its corresponding explanatory variables. The stronger the association, the more fully an instrument satisfies the "strong first stage" condition characteristic of a good instrument.

Model 1 in Table 2 regresses GDP growth on current and lagged rainfall growth. The rainfall growth variables and GDP growth display a strong, positive association, consistent with the findings of Miguel et al. (2004). A 1% increase in $RainfallGrowth_{it}$ is, on average,

associated with a 4.8% increase in $GDPgrowth_{it}$, all else equal. This estimate is significant at the 1% level, with a 95% confidence interval extending from 1.58% to 8.1%. A 1% increase in $RainfallGrowth_{i,t-1}$ is associated with a 3.6% increase in $GDPgrowth_{it}$. Note that these effects refer to increases in the GDP growth *rate*, and not in aggregate GDP from t to *t-1*. These results suggest that rainfall growth satisfies the first criterion for being a good instrument.

Model 2 regresses aid growth on whether a given country was on the UN Security Council during the two previous years. The data demonstrate a strong negative association between UN Security Council membership and aid growth. A country on the Security Council during year *t-1* experiences a 5.3% lower aid growth rate than does a similar country not on the Security Council. This estimate is not statistically significant, with a p-value of 0.484. A country on the Security Council during year *t-2* experiences a 21% lower aid growth rate than does a similar country that was not on the Security Council. This estimate is quite statistically significant, with a p-value of 0.016, and a 95% confidence interval extending from -38% to -3.9%. These results indicate that the association between Security Council membership and aid growth is quite strong, even though the direction of association is the opposite of what I expected. I will now move on to evaluating the main regression results.

6.2 Main regression results

Table 3: Probit Analyses of Civil Conflict Onset

VARIABLES	(1) GDP Growth Only	(2) GDP Growth Only	(3) Aid Growth Only	(4) Aid Growth Only	(5) Both	(6) Both
GDP growth (proportional change)	-9.04	-10.43**			-4.09	-4.47
	(6.56)	(4.80)			(3.82)	(3.46)
Aid growth (proportional change)			-0.88***	-0.89***	-0.85***	-0.85***
			(0.12)	(0.11)	(0.15)	(0.14)
GDP growth × Aid growth					0.65	0.50
					(0.57)	(0.45)
GDP in 1981		-0.33		-0.14		-0.13
		(0.20)		(0.11)		(0.10)
Polity 2 score (lagged)		0.02*		0.01		0.01
		(0.01)		(0.01)		(0.01)
Ethnic fractionalization		0.23		0.14		0.11
		(0.39)		(0.19)		(0.17)
Religious fractionalization		-0.75		-0.37		-0.34
		(0.56)		(0.27)		(0.25)
> 1/3 export revenue from fuels		-0.01		-0.00		0.00
		(0.26)		(0.11)		(0.11)
Log population (lagged)		0.04		0.02		0.02
		(0.07)		(0.03)		(0.03)
Terrain ruggedness		0.05		0.03		0.03
		(0.06)		(0.03)		(0.03)
Constant	-1.17	-0.92	-0.31	-0.19	-0.00	-0.22
	(0.50)	(0.57)	(0.32)	(0.29)	(0.00)	(0.31)
Wald χ^2	1.90	52.81	53.48	106.70	134.64	153.12
Number of observations	555	555	555	555	555	555

Standard errors in parentheses, clustered at country level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Wald χ^2 statistics for joint significance of the coefficients are included instead of F statistics, but are interpreted the same way.

My main regression results are displayed above in Table 3. Each model uses a probit specification, with instrumented GDP growth and instrumented aid growth as the explanatory variables, and civil conflict onset as the dependent variable. Model 1 in Table 3 includes only $GDPgrowth_{it}$, the explanatory variable measuring shocks to citizens' incomes. Model 3 includes only $AidGrowth_{it}$, the explanatory variable measuring shocks to government income. Model 5 includes both explanatory variables and an interaction term. Models 2, 4, and 6 add controls to Models 1, 2, and 3, respectively.

Results for the instrumented GDP growth explanatory variable are inconsistent, and at first seem to diverge from our expectations. When $GDPgrowth_{it}$ is the only explanatory variable in Model 2, its estimated coefficient is negative and statistically significant at the 1% level. This suggests that countries experiencing higher levels of GDP growth are less

likely to experience the onset of civil conflict than are countries experiencing lower levels of GDP growth, all else equal. Yet when $GDPgrowth_{it}$ is included alongside $AidGrowth_{it}$ in Models 5 and 6, its coefficients are positive and not statistically significant. These initial results suggest that the effect of shocks to citizens' incomes on civil conflict onset become inconsequential in the presence of shocks to government income. However, we will see in the next subsection that this apparent loss of significance is misleading.

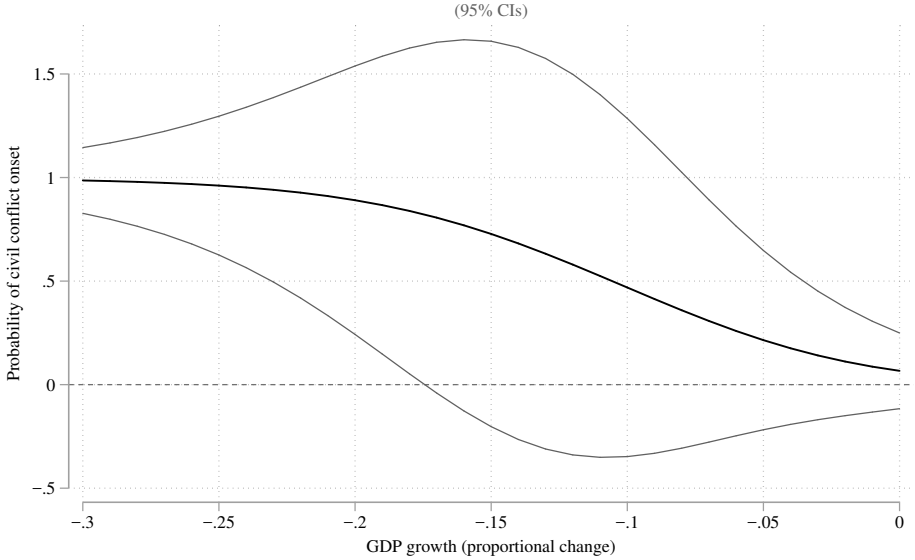
Results for the instrumented aid growth explanatory variable are consistent in all models, and match my expectations. The estimated coefficient on $AidGrowth_{it}$ is negative and statistically significant at the 1% level in all models. This suggests that countries experiencing higher levels of aid growth are less likely to experience the onset of civil conflict than are countries experiencing lower levels of aid growth, all else equal.

Results for the interaction term between GDP growth and aid growth diverge from my expectations. The coefficient on this interaction term is positive and not statistically significant in Models 5 and 6. This suggests that the effect of citizens' income shocks does not increase when a shock to government income occurs simultaneously.

Since all the models in Table 3 are probit models, we cannot infer much more about the effects of each explanatory variable from their estimated coefficients. To more closely examine the effects of each explanatory variable and evaluate Hypotheses 1 through 3, we must look at the predicted probabilities of civil conflict onset and effect sizes associated with each variable. The following figures 1-3 were generated using Model 6 from Table 3.

6.2.1 Testing Hypothesis 1

Fig. 1: Citizens' Income Shocks and the Probability of Civil Conflict Onset



In Figure 1[1], we see that the predicted probability of civil conflict onset rises as the level of GDP growth, $GDPgrowth_{it}$, becomes more negative. In other words, larger shocks to GDP are associated with a higher probability of civil conflict onset. Also note that the confidence interval around the predicted probability of civil conflict narrows as $GDPgrowth_{it}$ becomes more negative. While small shocks are associated with somewhat higher predicted probabilities of civil conflict onset, these estimates are not statistically significant. By contrast, large shocks are associated with much higher predicted probabilities of civil conflict onset, with very high levels of statistical significance.

When a country experiences 0% GDP growth from years *t-1* to *t*, its predicted probability of civil conflict onset is 7%, all else equal. A 95% confidence interval around this estimate extends from -12% to 25%. The mean value of GDP growth in the sample is approximately 0%.

All else equal, when a country experiences a 10% drop in GDP from years *t-1* to *t* ($GDPgrowth_{it} =$

[1] All figures constructed using the "plotplain" scheme by Bischof (2017)

-0.10), its predicted probability of civil conflict onset rises to 47%. This estimate is not statistically significant, with a 95% confidence interval extending from -35% to 130%. A comparable shock coincided with the onset of civil conflict in Burundi in 1995.

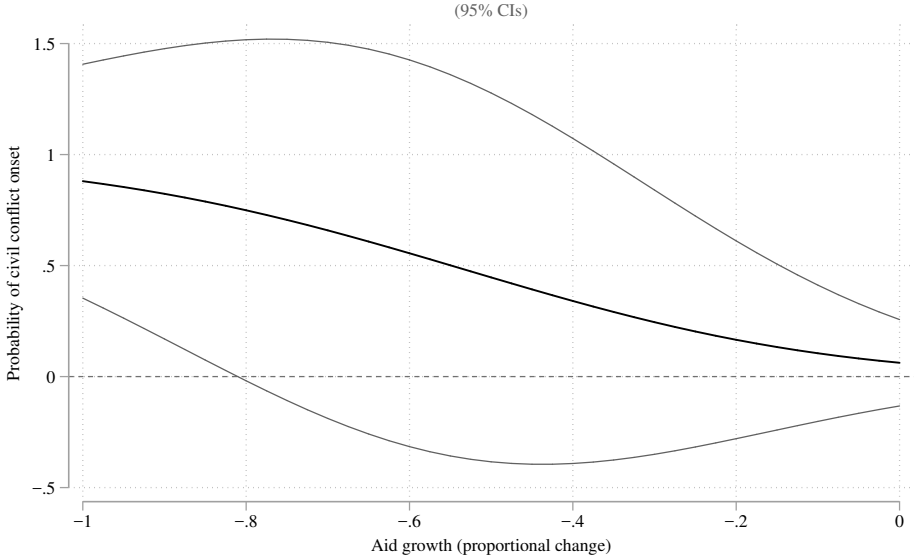
All else equal, when a country experiences a 20% drop in GDP from years *t-1* to *t* ($GDPgrowth_{it} = -0.20$), its predicted probability of civil conflict onset rises to 89%. This estimate is statistically significant at the 1% level, with a 95% confidence interval extending from 24% to 154%. While this may seem like an unrealistically large shock, a shock of this magnitude actually coincided with the onset of civil conflict in Sierra Leone in 1992.

All else equal, when a country experiences a 30% drop in GDP from years *t-1* to *t* ($GDPgrowth_{it} = -0.30$), its predicted probability of civil conflict onset rises to 99%. This estimate is statistically significant at the 0.1% level, with a 95% confidence interval extending from 83% to 114%. A comparable shock coincided with the onset of civil conflict in Guinea-Bissau in 1998.

Overall, small shocks to GDP exhibit a positive, but not statistically significant effect on the probability of civil conflict onset. Larger shocks to GDP have a positive, increasingly significant effect on the probability of civil conflict onset. I therefore reject the null hypothesis that GDP growth and civil conflict onset are unrelated. This suggests that large shocks to citizens' income substantially and significantly increase the likelihood of civil conflict onset.

6.2.2 Testing Hypothesis 2

Fig. 2: Government Income Shocks and the Probability of Civil Conflict Onset



In Figure 2, we see that the predicted probability of civil conflict onset rises as the level of aid growth, $AidGrowth_{it}$, becomes more negative. In other words, larger aid shocks are associated with higher probabilities of civil conflict onset. As was the case with $GDPgrowth_{it}$, the confidence interval around the predicted probability of civil conflict narrows as the size of the shock increases. While small aid shocks are associated with somewhat higher predicted probabilities of civil conflict onset, these estimates are not statistically significant. However, large shocks are associated with much higher predicted probabilities of civil conflict onset, with very high levels of statistical significance.

When a country experiences 0% aid growth from years *t-1* to *t*, its predicted probability of civil conflict onset is 6%. A 95% confidence interval around this estimate extends from -13% to 26%. The mean level of aid growth in the sample is approximately 7%. While the aid shocks in the following examples might seem large to the point of being unrepresentative, substantial fluctuations in aid are in fact relatively common.

All else equal, when a country experiences a 60% drop in aid from years *t-1* to *t* ($AidGrowth_{it} =$

-0.60), its predicted probability of civil conflict onset rises to 56%. This estimate is not statistically significant, with a 95% confidence interval extending from -32% to 143%. An aid shock of this magnitude coincided with the onset of civil conflict in Angola in 1984.

All else equal, when a country experiences an 80% drop in aid from years *t-1* to *t* ($AidGrowth_{it} = -0.80$), its predicted probability of civil conflict onset rises to 75%. This estimate is just shy of significance at the 5% level, with a 95% confidence interval extending from -1.9% to 152%.

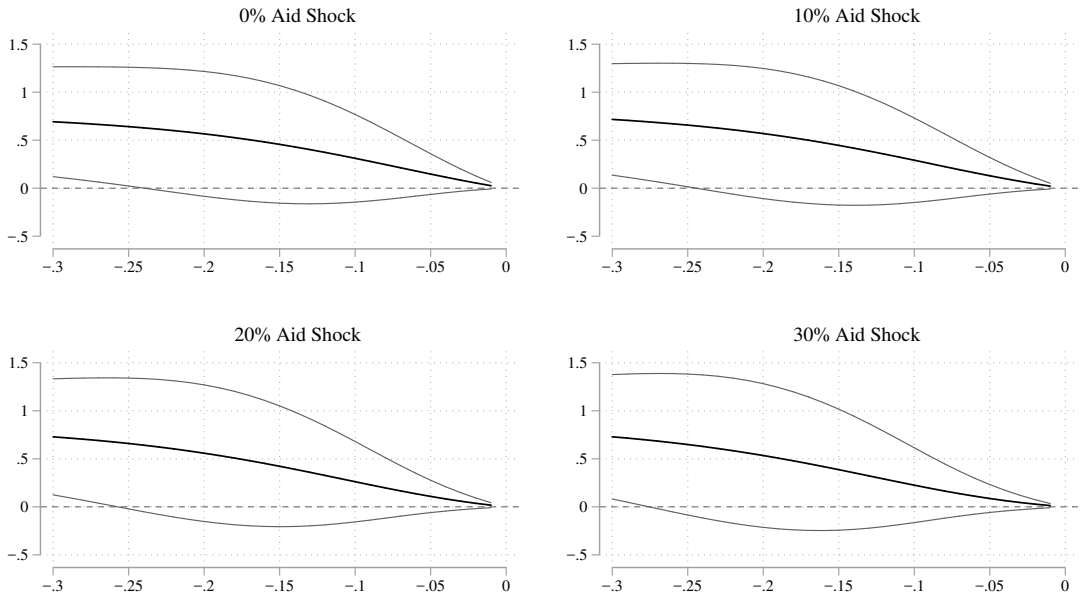
All else equal, when a country experiences a 100% drop in aid from years *t-1* to *t* ($AidGrowth_{it} = -1.00$), its predicted probability of civil conflict onset rises to 88%. This estimate is significant at the 1% level, with a 95% confidence interval extending from 35% to 140%.

Overall, we see that small and medium aid shocks have a positive but not statistically significant effect on the probability of civil conflict. The largest aid shocks have a positive, highly significant effect on the probability of civil conflict onset. I therefore reject the null hypothesis that aid growth and civil conflict onset are unrelated. That being said, the effect of aid shocks on the likelihood of civil conflict onset seems to be smaller than that of shocks to GDP, since a larger aid shock is necessary to attain a given predicted probability of civil conflict onset.

6.2.3 Testing Hypothesis 3

Fig. 3: Effects of Citizens' Income Shocks Alongside a...

(95% CIs)



Y−axis: Change in probability of civil conflict onset, X−axis: GDP growth (proportional change)

First, note that the plots above in Figure 3 depict the *effects* of a given decrease in GDP on the predicted probability of civil conflict onset, whereas the previous two plots charts the predicted probability of civil conflict onset associated with given decreases in GDP. In the plots above, we see two things. First, we see that larger decreases in GDP from years t-1 to t are associated with larger increases in the predicted probability of civil conflict onset. Second, we see that a given decrease in GDP is associated with a somewhat larger increase in the predicted probability of civil conflict onset in the presence of a simultaneous aid shock.

Let us consider the effect of a 10% drop in GDP from years *t-1* to *t* at various levels of aid growth.

To begin, suppose that aid is unchanged from the previous year ($AidGrowth_{it} = 0.00$). All else equal, when a country experiences a 10% drop in GDP, its predicted probability of civil conflict onset rises by 31 percentage points compared to when GDP growth is 0%. This

estimate is not statistically significant, with a p-value of 0.18, and a 95% confidence interval around this estimate extending from -15% to 76%.

Second, suppose that aid has dropped 10% ($AidGrowth_{it} = -0.10$). Now, all else equal, when a country experiences a 10% drop in GDP, its predicted probability of civil conflict onset rises by 29 percentage points. This estimate is not statistically significant, with a p-value of 0.19, and a 95% confidence interval extending from -15% to 73%.

Third, suppose that aid has dropped 20% ($AidGrowth_{it} = -0.20$). Now, all else equal, when a country experiences a 10% drop in GDP, its predicted probability of civil conflict onset rises by 26 percentage points. This estimate is not statistically significant, with a p-value of 0.22, and a 95% confidence interval extending from -16% to 68%.

Finally, suppose that aid has dropped 30% ($AidGrowth_{it} = -0.30$). Now, all else equal, when a country experiences a 10% drop in GDP, the predicted probability of civil conflict rises by 22 percentage points. This estimate is not statistically significant, with a p-value of 0.25, and a 95% confidence interval extending from -16% to 62%.

Notice that a 10% drop in GDP causes a smaller increase on the probability of civil conflict when a larger aid shock is in progress. Also notice that the estimated effect of a 10% drop in GDP loses statistical significance when a larger aid shock is in progress. However, notice that the confidence intervals on all of these estimates overlap. So the difference in these effect sizes is not large enough to be considered statistically significant, even though it is substantively interesting. I therefore fail to reject the null hypothesis that the effect size of a shock to GDP is increasing with the size of a simultaneous shock to aid. This seems to refute the idea that a smaller shock to citizens' income is necessary to provoke civil conflict when a shock to government income occurs simultaneously.

7 Robustness Tests

I will now investigate whether my findings are robust. In particular, I would like to answer two questions. First, to what extent do my empirical procedures consistently measure the relationships between economic shocks and civil conflict onset? Second, to what extend do my empirical procedures consistently measure the relationship between economic shocks and related types of internal conflict?

7.1 Including country and year fixed effects

Table 4: LPM FE Analyses of Civil Conflict Onset

VARIABLES	(1) GDP Growth Only	(2) Aid Growth Only	(3) Both	(4) GDP Growth Only	(5) Aid Growth Only	(6) Both
GDP growth (proportional change)	-1.70		-1.18	0.26		0.32
	(1.58)		(1.53)	(1.77)		(1.74)
Aid growth (proportional change)		-0.55	-0.58		-0.54	-0.55
		(0.42)	(0.46)		(0.39)	(0.45)
GDP growth × Aid growth			0.45***			0.19
			(0.17)			(0.14)
GDP in 1981	-0.02**	-0.02**	-0.02**	-1.33	-1.26	-1.21
	(0.01)	(0.01)	(0.01)	(0.84)	(0.77)	(0.79)
Polity 2 score (lagged)	0.00	0.00*	0.00*	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ethnic fractionalization	0.04	0.06	0.04	-1.91	-1.75	-1.68
	(0.08)	(0.07)	(0.07)	(1.64)	(1.51)	(1.54)
Religious fractionalization	-0.14	-0.15*	-0.14*	-23.46	-22.25*	-21.45
	(0.09)	(0.09)	(0.08)	(14.52)	(13.34)	(13.62)
$> 1/3$ export revenue from fuels	0.00	-0.00	0.01	0.04	0.04	0.04
	(0.03)	(0.03)	(0.04)	(0.09)	(0.09)	(0.09)
Log population (lagged)	0.01	0.00	0.00	-0.76	-0.72	-0.69
	(0.01)	(0.01)	(0.01)	(0.51)	(0.47)	(0.48)
Terrain ruggedness	0.01	0.02	0.01	0.35**	0.35**	0.33**
	(0.01)	(0.01)	(0.01)	(0.17)	(0.15)	(0.16)
Constant	0.07	0.21*	0.22*	20.18	19.20	18.47
	(0.08)	(0.12)	(0.12)	(13.04)	(12.00)	(12.26)
Country fixed effects	NO	NO	NO	YES	YES	YES
Year fixed effects	NO	NO	NO	YES	YES	YES
Wald χ^2	15.53	18.96	25.92	n/a	n/a	n/a
Number of observations	555	551	551	551	551	551

Standard errors in parentheses, clustered at country level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Wald χ^2 statistics for joint significance of the coefficients are included instead of F statistics, but are interpreted the same way.

The main drawback of fitting probit models for my main estimations is that nonlinear models like probit cannot include fixed effects. Consequently, my main empirical procedures

leave open the possibility that the relationships between GDP growth, aid growth, and civil conflict onset are driven by effects specific to certain countries or years in the sample. To explore this possibility, I fit linear probability models featuring country and year fixed effects. Unfortunately, using linear probability models causes a decrease in significance even without fixed effects, because linear probability models necessarily predict probabilities below zero and above one. This is evident in Models 1, 2, and 3 of Table 4, which are fit using linear probability models without fixed effects. While the directions of the effects are negative as predicted, the estimated effects of GDP growth and aid growth on the probability of civil conflict onset are not statistically significant. Adding fixed effects to these linear probability models decreases the significance of the estimates even further. This is evident in Models 4, 5, and 6 of Table 4, which are fit using linear probability models with fixed effects. This suggests that the relationships between GDP growth, aid growth, and civil conflict onset are partially driven by effects specific to certain countries or years in the sample.

7.2 Economic shocks and civil war onset

Table 5: Probit Analyses of Civil War Onset

VARIABLES	(1) GDP Growth Only	(2) GDP Growth Only	(3) Aid Growth Only	(4) Aid Growth Only	(5) Both	(6) Both
GDP growth (proportional change)	-12.89***	-13.32***			-13.20***	-13.46***
	(1.51)	(1.45)			(2.33)	(2.05)
Aid growth (proportional change)			-0.54	-0.62	-0.18	-0.15
			(1.34)	(1.05)	(0.74)	(0.77)
GDP growth × Aid growth					1.08*	1.04*
					(0.59)	(0.58)
GDP in 1981		-0.10		-0.19		-0.07
		(0.08)		(0.24)		(0.07)
Polity 2 score (lagged)		0.00		0.01		0.00
		(0.01)		(0.02)		(0.01)
Ethnic fractionalization		0.05		0.23		0.02
		(0.19)		(0.50)		(0.18)
Religious fractionalization		0.01		0.03		-0.02
		(0.22)		(0.50)		(0.20)
$> 1/3$ export revenue from fuels		0.04		0.05		0.05
		(0.13)		(0.33)		(0.14)
Log population (lagged)		0.02		0.04		0.03
		(0.04)		(0.10)		(0.04)
Terrain ruggedness		0.03		0.08		0.02
		(0.03)		(0.09)		(0.03)
Constant	-0.72***	-0.79**	-1.43	-1.68	-0.68**	-0.86*
	(0.22)	(0.37)	(1.66)	(2.19)	(0.33)	(0.48)
Wald χ^2	72.72	134.02	0.16	15.02	103.80	198.77
Number of observations	625	621	621	621	621	621

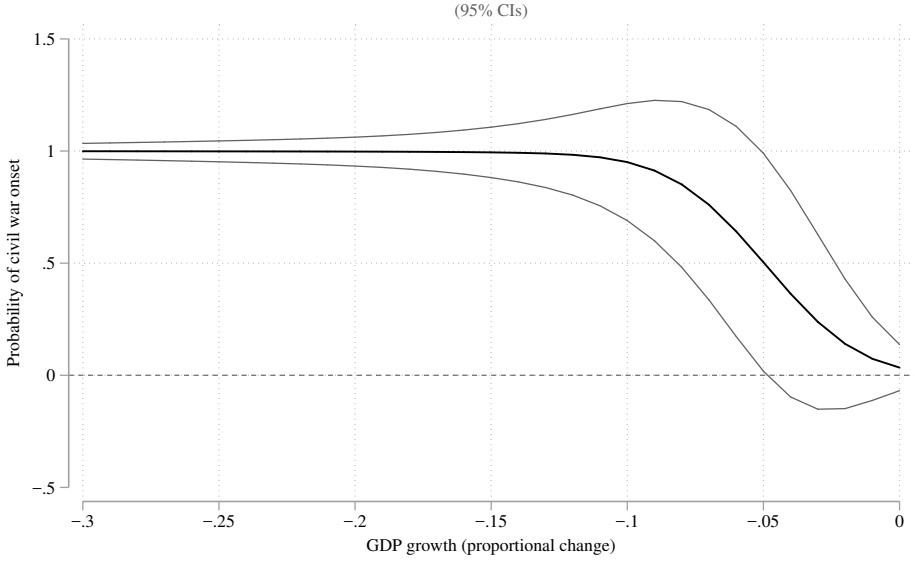
Standard errors in parentheses, clustered at country level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Wald χ^2 statistics for joint significance of the coefficients are included instead of F statistics, but are interpreted the same way.

No part of my argument suggests that the mechanisms linking economic shocks to civil conflict onset should be any different from those linking shocks to civil *war* onset. Civil war and civil conflict are differentiated by their severity. According to the UCDP/PRIO dataset's definition, civil conflict is said to occur if at least 25 battle-related deaths occur in a given country-year. By contrast, civil war is said to occur if at least 1000 battle-related deaths occur in a given country-year.

To test whether my empirical procedures also predict civil war onset, I fit models in Table 5 which use civil war onset as dependent variables, but are otherwise identical to the models used to obtain my main results. In Models 1 and 2 of Table 5, GDP growth is negatively associated with civil war onset at the 0.1% level. In Models 3 and 4, aid growth is not associated with civil war onset at any conventional significance level. Peculiarly, the interaction

term in Models 5 and 6 is positive and significant at the 10% level. These initial results suggest that shocks to government income are weaker predictors of civil *war* onset than they are of civil conflict onset. This suggestion is corroborated when looking at predicted probabilities and effect sizes. The following figures 3-6 were generated using Model 6 from Table 5.

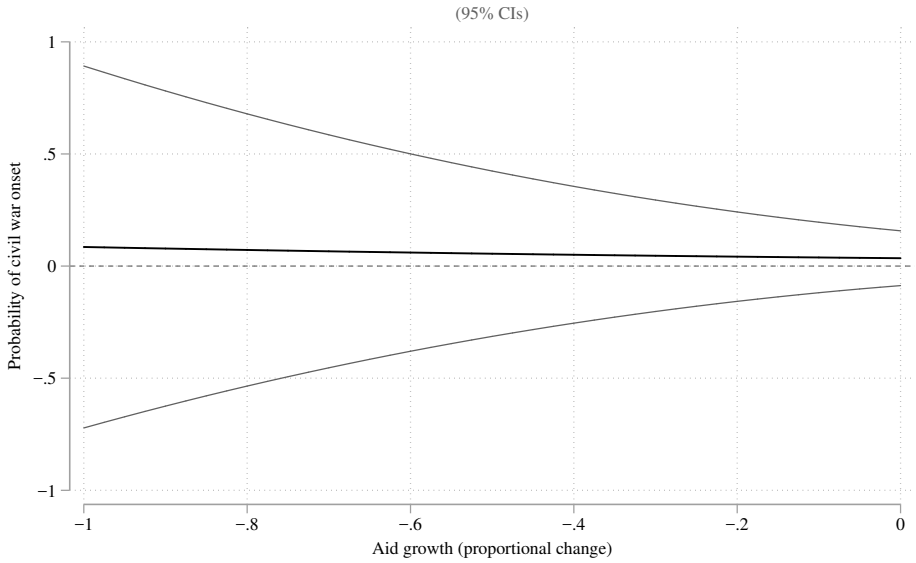
Fig. 4: Citizens' Income Shocks and the Probability of Civil War Onset



In Figure 4, note that the predicted probability of civil war onset rises as the level of GDP growth becomes more negative. In other words, larger shocks are associated with higher probabilities of civil conflict onset. Also note that the confidence interval around the predicted probability of civil war onset narrows as the size of the shock increases.

Comparing this plot to Figure 1 in the main results section, two things are worth noticing. First, small shocks to GDP growth seem to have a larger effect on the predicted probability of civil *war* onset than on the predicted probability of civil *conflict* onset. Second, the predicted probabilities of civil war onset at various values of GDP growth (Figure 4) exhibit consistently narrower confidence intervals (and higher significance) than do the predicted probabilities of civil conflict onset (Figure 1).

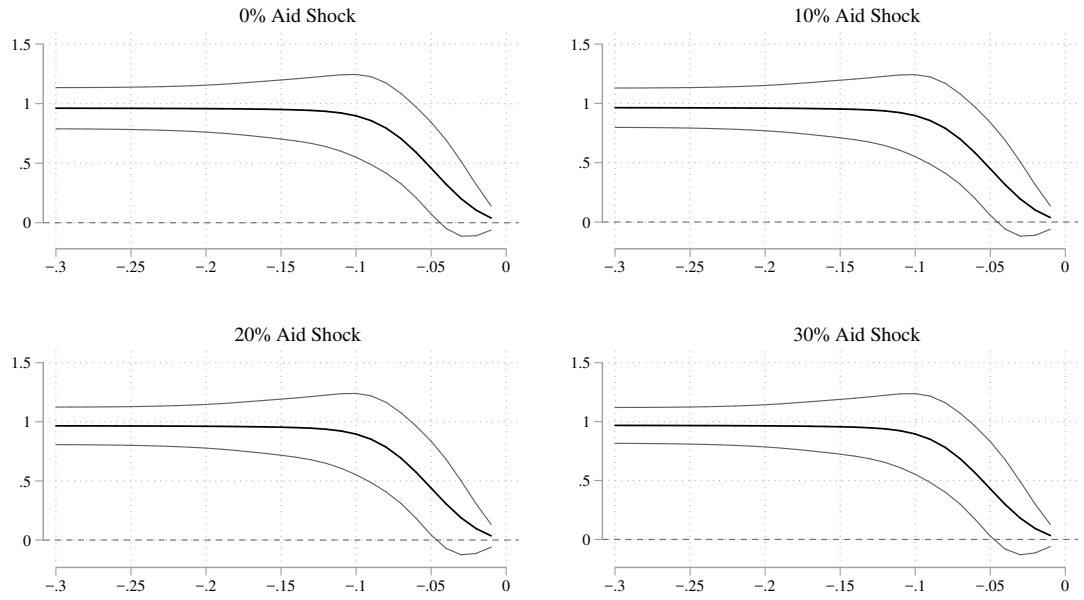
Fig. 5: Government Income Shocks and the Probability of Civil War Onset



In Figure 5, note that the predicted probability of civil war onset barely changes as the level of aid growth becomes more negative. Larger shocks to aid are not associated with a higher probability of civil conflict. Also note that the confidence interval around the predicted probability of civil war onset only widens as the size of the shock increases. Aid shocks seem to never exert a statistically significant effect on the probability of civil war onset.

Fig. 6: Effects of Citizens' Income Shocks Alongside a...

(95% CIs)



Y−axis: Change in probability of civil war onset, X−axis: GDP growth (proportional change)

Two things are of interest in Figure 6. First, we see that larger decreases in GDP from years *t-1* to *t* are associated with larger increases in the predicted probability of civil war onset. Second, however, we see that in each sub-plot, a given decrease in GDP is associated with the same increase in the predicted probability of civil war onset regardless of whether an aid shock occurs simultaneously. This indicates that there is no interactive effect between shocks to GDP and shocks to aid in causing civil war onset.

7.3 Civil conflict incidence

Table 6: Probit Analyses of Civil Conflict Incidence

VARIABLES	(1) GDP Growth Only	(2) GDP Growth Only	(3) Aid Growth Only	(4) Aid Growth Only	(5) Both	(6) Both
GDP growth (proportional change)	-2.95	-2.80			-2.81	-2.24
	(4.99)	(5.36)			(4.57)	(4.93)
Aid growth (proportional change)			-0.39	-0.59	-0.06	-0.24
			(1.45)	(1.23)	(1.44)	(1.74)
GDP growth × Aid growth					1.38**	1.26
					(0.69)	(0.96)
GDP in 1981		-0.22		-0.28		-0.33
		(0.20)		(0.38)		(0.29)
Polity 2 score (lagged)		0.01		0.00		0.00
		(0.02)		(0.01)		(0.01)
Ethnic fractionalization		0.75		0.53		0.63
		(0.82)		(0.91)		(0.86)
Religious fractionalization		-0.93		-0.71		-0.85
		(0.83)		(0.99)		(0.88)
¿ 1/3 export revenue from fuels		0.06		0.08		0.08
		(0.67)		(0.51)		(0.62)
Log population (lagged)		0.26*		0.19		0.24
		(0.16)		(0.24)		(0.20)
Terrain ruggedness		0.25**		0.18		0.22
		(0.13)		(0.22)		(0.17)
Constant	-0.63***	-3.20***	-0.50	-2.08	-0.64	-2.73
	(0.16)	(1.17)	(0.67)	(2.86)	(0.40)	(2.21)
Wald χ^2	0.35	20.39	0.07	39.16	7.52	24.91
Number of observations	743	743	730	730	730	730

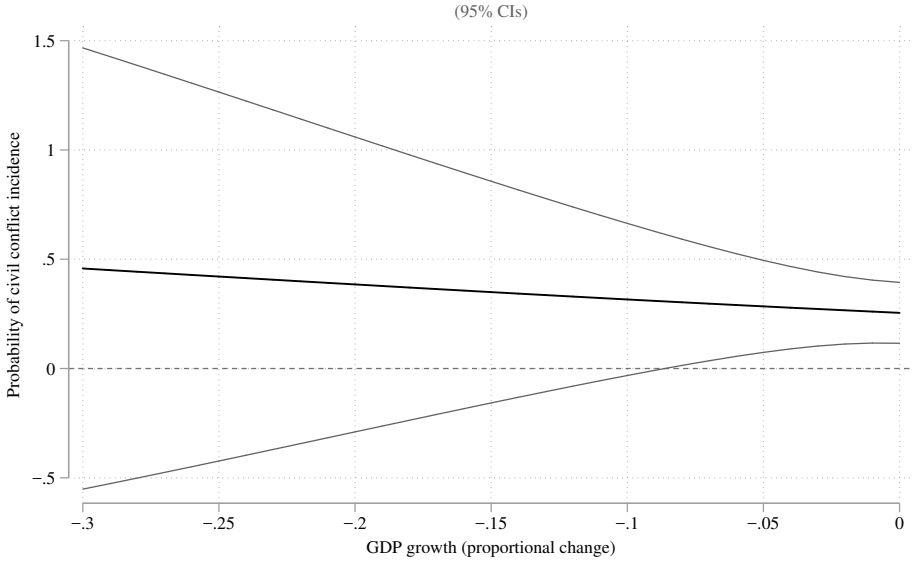
Standard errors in parentheses, clustered at country level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Wald χ^2 statistics for joint significance of the coefficients are included instead of F statistics, but are interpreted the same way.

Though the theories articulated by Chassang et al (2009) and Nielsen et al (2012), and by this paper try to explain the onset of conflict, the wider literature on the economic drivers of civil conflict explores the *incidence* of conflict as well. "Civil conflict incidence" is the broadest measure of civil conflict used in the large-N literature, not distinguishing between the onset or continuation of conflict. According to this definition, the variable $ConflictIncidence_{it}$ takes a value of "1" for country-year observations in which over 25 battle-related deaths occurred, regardless of whether the conflict began in that year. To test whether my argument and empirical procedures explain the incidence of civil conflict as well as its onset, I fit additional models which use $ConflictIncidence_{it}$ as a dependent variable, but are otherwise the same as the models in the main results section.

In Models 1 and 2 of Table 6, GDP growth is negatively but not significantly associated

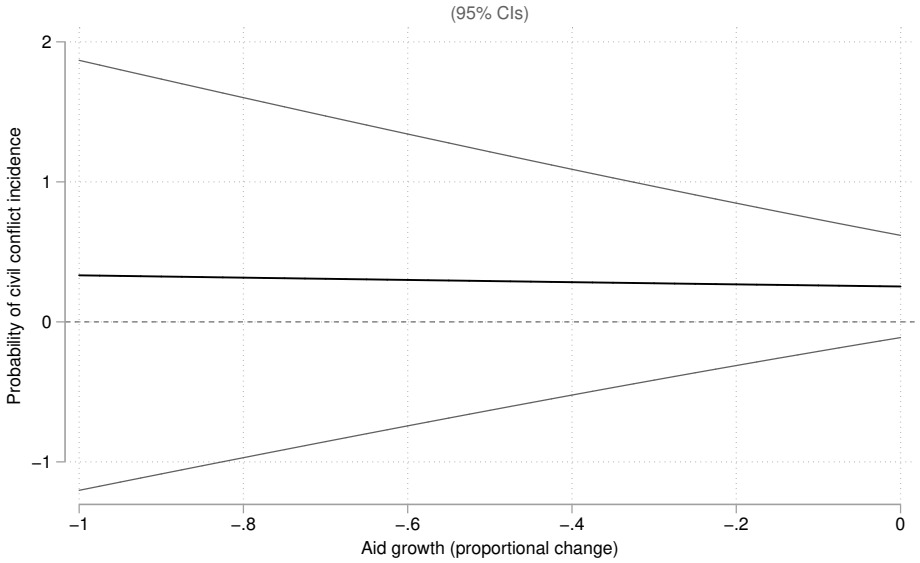
with civil conflict incidence. In Models 3 and 4, aid growth is negatively but not significantly associated with civil conflict incidence. In Models 5 and 6, both GDP growth and aid growth are negatively but not significantly associated with civil conflict incidence. The interaction term is positive and barely significant at the 5% level. These initial results suggest that GDP growth and aid growth are poor predictors of civil conflict incidence, a suggestion which is corroborated when looking at predicted probabilities and effect sizes. The following figures 7-9 were generated using Model 6 from Table 6.

Fig. 7: Citizens' Income Shocks and the Probability of Civil Conflict Incidence



In Figure 7, note that the predicted probability of civil conflict incidence rises only somewhat as the level of GDP growth becomes more negative. In other words, larger shocks are associated with a modestly higher probability of civil conflict incidence. Also note that only the smallest shocks to GDP are significantly associated with a heightened predicted probability of civil conflict incidence. As the size of the shock increases, the confidence intervals around each predicted probability grow progressively wider as the estimates lose statistical significance.

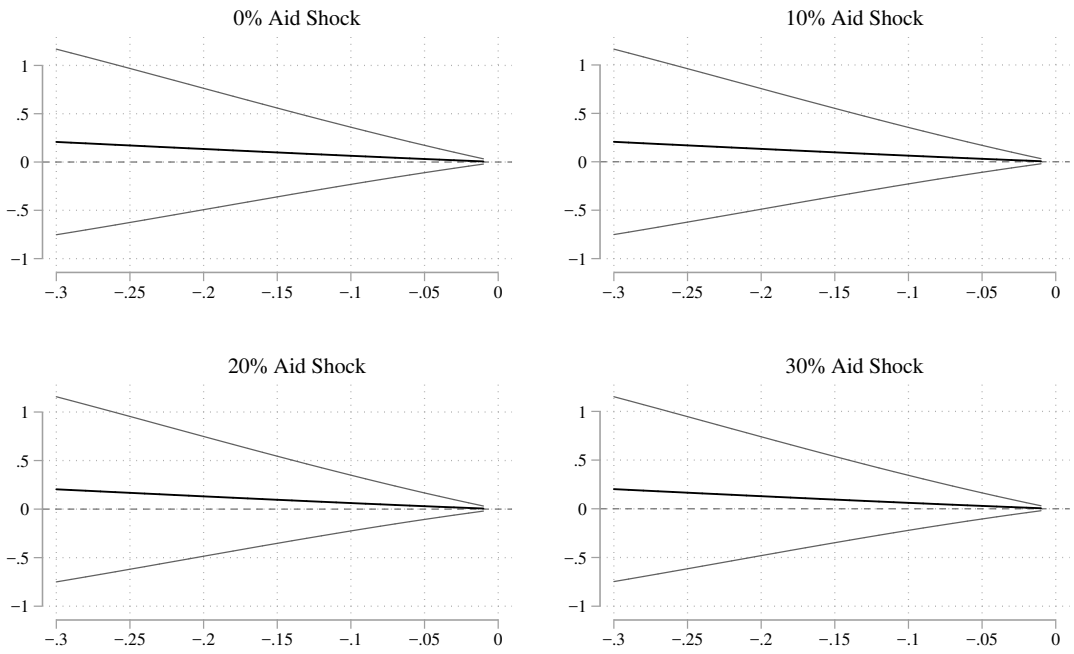
Fig. 8: Government Income Shocks and the Probability of Civil Conflict Incidence



In Figure 8, note that the predicted probability of civil conflict incidence barely changes as the level of aid growth becomes more negative. All of the predicted probabilities are statistically insignificant.

Fig. 9: Effects of Citizens' Income Shocks Alongside a...

(95% CIs)



Y−axis: Change in probability of civil conflict incidence, X−axis: GDP growth (proportional change)

Two things are noteworthy in Figure 9. First, note that a given decrease in GDP is associated with roughly the same increase in the predicted probability of civil conflict incidence regardless of whether an aid shock occurs simultaneously. Second, note that larger decreases in GDP from years *t-1* to *t* are associated with larger increases in the predicted probability of civil conflict incidence. However, the effect of a given decrease in GDP on the predicted probability of civil conflict incidence is consistently smaller than the effect on the predicted probability of civil conflict onset (Figures 3, 4, 5, 6).

Taken together, these results suggest that income shocks of any kind are more relevant to the onset of conflict rather than its incidence. They also suggest that only shocks to citizens' incomes have any significant effect on conflict incidence.

7.4 Civil war incidence

Table 7: Probit Analyses of Civil War Incidence

VARIABLES	(1) GDP Growth Only	(2) GDP Growth Only	(3) Aid Growth Only	(4) Aid Growth Only	(5) Both	(6) Both
GDP growth (proportional change)	-7.03***	-7.16**			-7.03***	-6.51*
	(2.71)	(3.06)			(2.91)	(3.50)
Aid growth (proportional change)			-0.22	0.05	0.03	0.43
			(1.33)	(1.48)	(1.35)	(1.04)
GDP growth × Aid growth					1.39**	1.49
					(0.66)	(1.18)
GDP in 1981		0.00		0.12		-0.09
		(0.17)		(0.19)		(0.16)
Polity 2 score (lagged)		-0.00		-0.00		-0.00
		(0.02)		(0.02)		(0.02)
Ethnic fractionalization		0.92		0.91		0.75
		(0.78)		(0.84)		(0.84)
Religious fractionalization		0.02		0.13		0.09
		(0.79)		(0.88)		(0.74)
¿ 1/3 export revenue from fuels		0.17		0.23		0.19
		(0.60)		(0.65)		(0.56)
Log population (lagged)		0.24		0.25		0.22
		(0.17)		(0.18)		(0.18)
Terrain ruggedness		0.22*		0.22		0.19
		(0.13)		(0.15)		(0.16)
Constant	-0.89***	-4.15***	-0.94	-4.25***	-0.95***	-3.84*
	(0.16)	(1.31)	(0.57)	(1.34)	(0.30)	(2.03)
Wald χ^2	6.73	47.91	0.03	20.52	20.41	65.60
Number of observations	743	743	730	730	730	730

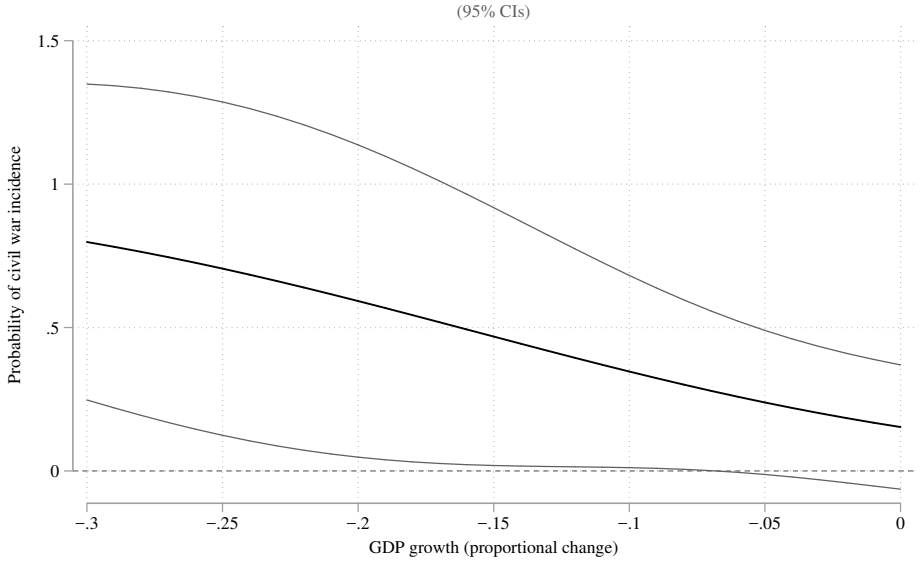
Standard errors in parentheses, clustered at country level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Wald χ^2 statistics for joint significance of the coefficients are included instead of F statistics, but are interpreted the same way.

"Civil war incidence" is an analogous measure to civil conflict incidence, the only difference being the threshold above which the indicator variable takes a value of "1." Whereas $ConflictIncidence_{it}$ takes a value of "1" if at least 25 battle-related deaths occurred for a given country-year observation, $WarIncidence_{it}$ takes a value of "1" only if at least 1000 battle-related deaths occurred for a given country-year observation.

To test whether my argument and empirical procedures explain and predict civil war incidence, I fit additional models which use $WarIncidence_{it}$ as a dependent variable, but are otherwise identical to the models in the main results section (Table 1). In Models 1 and 2 of Table 7, GDP growth is negatively and significantly associated with civil war incidence at the 1% and 5% levels respectively. In Models 3 and 4, Aid growth is negatively but not significantly associated with civil war incidence. In Models 5 and 6, GDP growth is negatively

and significantly associated with civil war incidence at the 5 and 10% levels, respectively. Aid growth is negatively but not significantly associated with civil war incidence. The interaction term is positive and significant at the 5% level in Model 5, but is positive and insignificant in Model 6. These initial results suggest that GDP growth is a good predictor of civil war incidence, but Aid growth is not. The following figures were generated using Model 6 from Table 7.

Fig. 10: Citizens' Income Shocks and the Probability of Civil War Incidence



In Figure 10, note that the predicted probability of civil war incidence rises as the level of GDP growth becomes more negative. In other words, larger shocks are associated with a higher probability of civil war incidence. Also note that the confidence interval around the predicted probability of civil war onset narrows as the size of the shock increases.

Fig. 11: Government Income Shocks and the Probability of Civil War Incidence

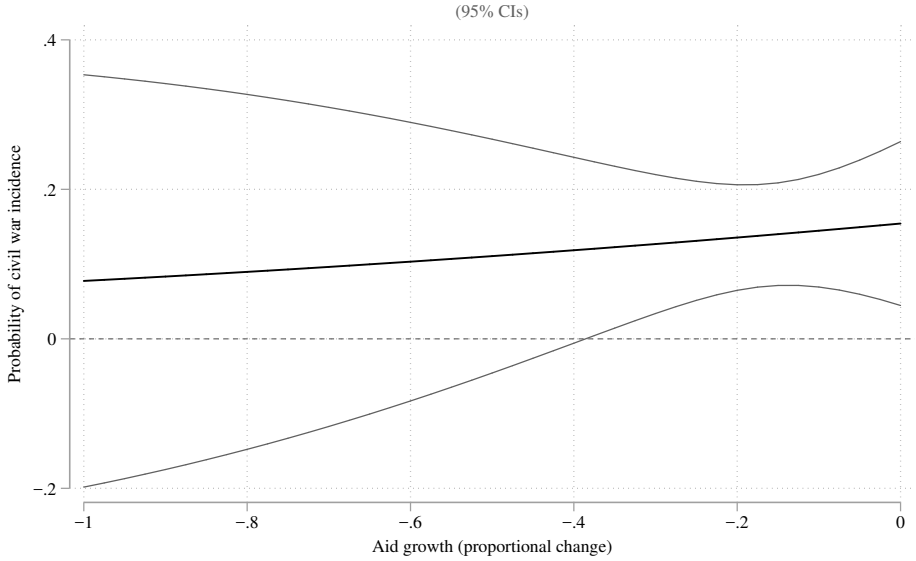
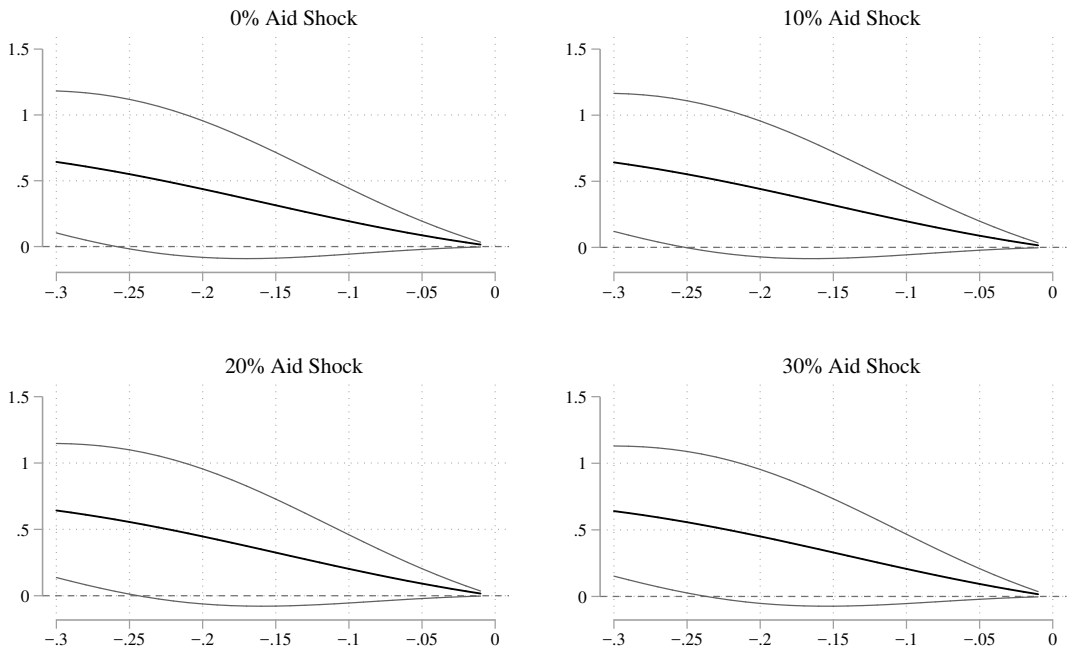


Figure 11 displays a strange relationship. The predicted probability of civil war incidence appears to fall as the level of aid growth becomes more negative. In other words, larger shocks are associated with a lower probability of civil conflict onset. Also note that the confidence interval around the predicted probability of civil war incidence narrows at first as the size of the shock increases, but then widens.

Fig. 12: Effects of Citizens' Income Shocks Alongside a...

(95% CIs)



Y−axis: Probability of Civil War Incidence, X−axis: GDP growth (proportional change)

Two things are noteworthy in Figure 12. First, note that larger decreases in GDP from years *t-1* to *t* are associated with larger increases in the predicted probability of civil conflict incidence. Second, note that a given decrease in GDP is associated with almost exactly the same increase in the predicted probability of civil conflict incidence, regardless of whether an aid shock occurs simultaneously. This suggests that that there is no interactive effect between GDP growth and Aid growth in predicting civil war incidence.

Taken together, these results of these robustness tests suggest that income shocks of any kind are much more relevant to the onset of civil war (and civil conflict) rather than its incidence. They also suggest that only shocks to citizens' incomes have any significant effect on civil war incidence.

8 Discussion

8.1 Discussion of main results

My main results offer some support for Hypotheses 1 and 2, but no support for Hypothesis 3. The results of my robustness tests suggests some additional caveats to consider in interpreting my main results.

I find moderate-to-strong support for Hypothesis 1, which predicts that shocks to citizens' incomes will make civil conflict more likely. My initial results appear contradictory. When instrumented GDP growth is the only explanatory variable considered, I observe a negative, highly significant association between instrumented GDP growth and the probability of civil conflict onset. On the other hand, when instrumented GDP growth is included alongside instrumented aid growth, I observe an insignificant association between instrumented GDP growth and the probability of civil conflict onset. However, I find a more consistent and nuanced relationship between instrumented GDP growth and the probability of civil conflict onset when evaluating the predicted probability of civil conflict onset at various values of GDP growth. I find that the predicted probability of civil conflict onset rises as the level of GDP growth becomes more negative. In other words, larger shocks to GDP are associated with a higher probability of civil conflict onset. I also find that these predicted probabilities gain statistical significance as shock size rises. While small shocks are associated with somewhat higher predicted probabilities of civil conflict, these estimates are not statistically significant. By contrast, large shocks are associated with much higher predicted probabilities of civil conflict onset, with high levels of statistical significance.

These findings are consistent with the opportunity cost explanation of the shock-conflict association, which states that shocks to citizens' incomes raise the likelihood of civil conflict by lowering the opportunity cost of fighting faced by prospective rebels. They also suggest that the opportunity cost mechanism only causes conflict through exceptionally large shocks

to citizens' incomes. In other words, an income shock can only lead citizens to fight by depriving them of a large portion of their income, to the point where citizens have little left to lose by fighting. Citizens are not motivated to fight by the prospect of slightly higher returns to fighting compared to labor during small economic downturns.

I find moderate-to-strong support for Hypothesis 2, which predicts that shocks to government income will make civil conflict more likely. I observe a negative, highly significant association between instrumented aid growth and the probability of civil conflict onset, regardless of whether aid growth is the only explanatory variable included. I find a more nuanced relationship between instrumented aid growth and the probability of civil conflict onset when evaluating the predicted probability of civil conflict onset at various values of aid growth. I find that the predicted probability of civil conflict onset rises as the level of aid growth becomes more negative. In other words, larger shocks are associated with a higher probability of civil conflict onset. I also find that these predicted probabilities gain statistical significance as shock size rises. While small aid shocks are associated with somewhat higher predicted probabilities of civil conflict onset, these estimates are not statistically significant. By contrast, large shocks are associated with much higher predicted probabilities of civil conflict onset, with high levels of statistical significance.

These findings are consistent with the state capacity explanation of the shock-conflict relationship, which states that shocks to government income make civil conflict more likely by diminishing the government's ability to suppress rebellion. They also suggest shocks to government income must be exceptionally large to affect state capacity—in general, governments are not endangered by small income shocks. One possible explanation is that governments smooth their expenditure over time, saving when government income grows, and using those savings to supplement lost income during economic shocks. Another explanation is that governments may cut security-related expenditure last when facing economic shocks to preserve their ability to suppress rebellion.

I find no support for Hypothesis 3, which predicts that following a shock to government income, a smaller shock to citizens' incomes is necessary to provoke conflict. In other words, Hypothesis 3 predicts that a shock to citizens' incomes has a larger effect on the probability of civil conflict when a shock to government income occurs simultaneously. This finding seems to refute the idea that a smaller shock to citizens' incomes is necessary to provoke civil conflict when a shock to government income occurs simultaneously.

Taken together, my findings are consistent with the argument that the opportunity cost and state capacity mechanisms are not mutually exclusive. Instead, economic shocks lead to civil conflict both by decreasing the opportunity cost of fighting and by decreasing state capacity. However, I find little support for my argument that the opportunity cost and state capacity mechanisms are mutually reinforcing.

8.2 Discussion of robustness checks

My main results should be interpreted with some skepticism in light of my robustness tests, which find that my models do not consistently measure the relationship between economic shocks and civil conflict onset. The relationships between my two explanatory variables and civil conflict onset lose statistical significance when tested using linear probability models with country and year fixed effects. While some of this lost significance can be attributed to the use of a linear probability model with a binary dependent variable, including fixed effects causes an additional loss of significance. This suggests that my main findings are partially driven by characteristics of specific countries or years in the sample.

My robustness tests also show that my models do not consistently measure the relationship between economic shocks and related types of internal conflict. Evaluating the relationship between economic shocks and civil *war* onset, I find that GDP growth is negatively and significantly associated with civil war onset, but aid growth is not significantly associated

with civil war onset. I find no evidence of an interactive effect between GDP growth and aid growth in predicting civil war onset. These results indicate that shocks to citizens' incomes explain the onset of both small- and large-scale internal conflict, while shocks to government income are only relevant to the onset of small-scale internal conflict. Evaluating the relationship between economic shocks and civil conflict *incidence*, I find that while GDP growth is negatively and significantly associated with civil war incidence, aid growth is not. Taken together, these two results suggest that economic shocks of both kinds only explain the breakout of internal strife rather than its continuation.

Overall, my robustness tests suggest that shocks to citizens' incomes are much stronger drivers of the shock-conflict relationship than are shocks to government income, which only predict the onset of civil conflict with any significance.

8.3 Discussion of research design

While my main findings are substantively meaningful and statistically significant, my robustness tests show that these results are inconsistent. My findings are far from definitively explaining the shock-conflict association and evaluating the opportunity cost and state capacity theories. So in this section, I will reevaluate my research design to derive some suggestions for how future empirical analysis of the shock-conflict association should be conducted.

The overarching limitation affecting my research design was the need for empirically feasible measures. I began my search for variables with the question "which measures of income shocks are the most plausibly exogenous?", rather than "which measures of income shocks most closely correspond to the concepts of the theory I aim to test?" In retrospect, I should have instead looked for the most theoretically-grounded measures, and then picked the most feasible of those. This is a difficulty encountered in much of the literature on conflict and development as well. For example, in their review of the literature on conflict and development,

Ray and Esteban (2017) relate how the study of ethnicity and conflict was hindered by using the readily-available ethnic fractionalization variable, rather than the more theoretically-grounded ethnic polarization variable.

When studying the shock-conflict association in particular, choosing measures of income shocks involves a tradeoff between exogeneity and theoretical grounding. This is true both for this paper and for the shock-conflict literature more generally. On one hand, plausibly exogenous measures of income shocks are often identified in a roundabout fashion, rather than being derived from theory. On the other hand, more theoretically-grounded measures of income shocks often lack exogeneity. The clearest example of this tradeoff in my paper is how I measured shocks to state capacity.

Testing my second hypothesis required a measure of shocks to government income that would decrease the state's capacity to suppress rebellion. It is easy to identify various measures that fit this requirement, such as changes in military expenditure per capita. But the need for an exogenous measure forced me to forego these theoretically-grounded measures in favor of an exogenous measure. To satisfy this exogeneity requirement, I chose aid growth instrumented by Security Council membership. Though this measure is arguably exogenous, it lacks a strong theoretical foundation. Although the argument that aid growth corresponds to changes in state capacity is based on the well-documented finding that aid is fungible, I do not consider the time period over which this premise may be valid. If aid is only fungible over the long run, then aid flows should not affect the government's short-term ability to suppress rebellions. Second, I assume that governments use aid funds primarily for security purposes. Yet governments vary in how they use aid funds. Some governments might use aid to fund public goods, while others might use aid to fund private goods (Bueno De Mesquita and Smith, 2010). The connection between aid and the government's ability to suppress rebellion may only be present in certain contexts.

There are several ways in which future shock-conflict literature could avoid the trade-off be-

tween exogeneity and theoretical groundedness. First, measures of economic shocks should be more closely derived from theory, and chosen first and foremost for their theoretical groundedness rather than their exogeneity. Second, future research of the shock-conflict relationship should focus on smaller-scale analysis of natural experiments, in which exogeneity can be observed rather than assumed using roundabout instruments. Third, future research should move away from the country-year unit of analysis toward using finer-grained data, with which cause and effect can be more easily identified.

9 Conclusion

Let us review our train of thought so far. The most consistent finding of the civil conflict literature is that civil conflict is driven by economic conditions. Low income and low growth in particular are the two most robust predictors of civil conflict. Two interpretations of this income-conflict association predominate in the literature. The opportunity cost explanation argues that low income corresponds to a low opportunity cost of fighting faced by citizens. The state capacity explanation argues that low income corresponds to low state capacity to suppress rebellion. Adjudicating between these explanations is difficult, because they are observationally equivalent. While the literature rarely argues outright that the opportunity cost and state capacity explanations are mutually exclusive, it tends to treat them as such, choosing one explanation over the other. In this paper, I consider the possibility that these explanations need not be mutually exclusive, and might even be mutually reinforcing. I build upon Chassang and Padro i Miquel's (2009) model of economic shocks and civil conflict to propose how the opportunity cost and state capacity mechanisms might function in parallel, and how their underlying causal mechanisms are related. After using this extended model to derive three hypotheses, I then test my argument by using instrumental variables to identify exogenous variation in the incomes of citizens and governments.

I find mixed evidence in support of my argument. Shocks to citizens' incomes and shocks to government income are strongly associated with higher probabilities of civil conflict onset, suggesting that the opportunity cost and state capacity mechanisms are not mutually exclusive. I also find that only large shocks are significantly associated with civil conflict onset, suggesting that the shock-conflict association is driven by extreme downturns rather than regular economic fluctuations. I find no evidence that shocks to citizens' and government incomes are mutually reinforcing. In the end, however, I propose viewing all these results with a bit of skepticism in light of my robustness tests, which indicate that many of my results are not robust to the inclusion of fixed effects or related measures of conflict. They also suggest that shocks to citizens' incomes are much stronger drivers of the shock-conflict association than are shocks to government income, and that economic shocks only explain the onset of civil conflict rather than its continuation.

I then reevaluate my research design in light of my results and derive a few suggestions for future empirical research into the relationship between income and conflict. I argue that my paper, and the larger income-conflict literature, face a trade-off between measures of income shocks that are theoretically-grounded and measures that are exogenous. To remedy this problem, I suggest that future literature search for natural experiments, where exogenous income variation can be identified without using instruments of questionable validity. I also suggest a move away from large-N, cross-country research using the country-year unit of analysis, and toward the use of finer-grained data with which cause and effect can be identified with greater precision.

Explaining the relationship between economic shocks and civil conflict has substantial policy significance as well, especially for the question of how international actors should try to prevent civil conflict. The answer to this question hinges on the mechanism by which economic shocks lead to civil conflict. If civil conflict is primarily driven by citizens' income shocks, then outside actors should try to promote economic development which makes citizens' in-

comes less volatile, such as agricultural insurance. If civil conflict is primarily driven by government income shocks, then outside actors should focus on strengthening governments fiscal and military capacity. Both these theories linking economic shocks to civil conflict suggest that outside actors should focus on promoting short-term economic stability in the least-developed countries, perhaps even at the expense of rapid, but more volatile, economic growth.

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