Aiding Conflict:
The Unintended Consequences of U.S. Food Aid on Civil War*

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Abstract

This study explores the impact of donor-driven U.S. food aid on civil conflict. We establish the causal effect of food aid by exploiting plausibly exogenous variation in the amount of food aid receipts caused by changes in weather conditions in U.S. wheat-producing regions and the average tendency for a country to receive any U.S. food aid. We find that U.S. food aid significantly increases the incidence of civil war in recipient countries and that these effects are particularly pronounced for countries without civilian governments and those that experience higher levels of conflict on average.

"Food is strength, and food is peace, and food is freedom, and food is a helping hand to people around the world whose good will and friendship we want," – John F. Kennedy when he renamed PL480 to be called the *Food for Peace Program* in 1962.

1 Introduction

During the past thirty years, the number of conflicts in poor countries have increased from approximately fifteen per year (in the early 1970s) to approximately forty per year (by the late 1990s) such that a third of low income countries today are experiencing conflict.[1] Over 98% of all conflict-related deaths in the world have occurred in poor countries and over 75% of these conflicts are civil conflicts(Leitenberg, 2006).[2] It goes without saying that conflicts hinder

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[1]See Hill, Mansoor, and Claudio (2010) for a description of conflict in least developed countries today.

[2]We calculate the proportion of deaths in poor countries from statistics presented in Table 2 of (Leitenberg, 2006).

economic and institutional development (Abadie and Gardeazabal, 2003; Collier, 1998; Murdoch and Sandler, 2004; Rodrik, 1999), and more importantly, they cause enormous humanitarian and social devastations to the populations which fall victim (e.g., Akresh, Bundervoet, and Verwimp, 2010).[3] Despite the numerous studies on the determinants of conflict, and in particular, civil war, there are still many gaps in our understanding of this phenomenon.[4]

One of the main instruments for mitigating the consequences of conflict and for promoting economic development more generally is foreign aid. Since 1970, low income countries have received approximately 2.98 trillion (2008 USD) of aid. One of the most important elements of foreign aid is food aid. During its peak in 1965, food aid accounted for 22% of all aid given to developing countries. However, the effectiveness of foreign aid, and in particular, food aid, has become a subject of controversy and heated debate in recent years.[5] Unfortunately, there is little rigorous empirical evidence for policy makers to consult. Experts on food aid complain that our understanding is "woefully incomplete, excessively focused on univariate analysis and on individual case studies" (Pillai, 2000). Understanding the determinants of conflict and the impact of food aid are both questions of first order importance for development.

This study attempts to address this question by studying the impact of U.S. government-to-government food aid on conflict in recipient countries. Our focus on the United States and government-to-government food aid is mainly driven by practical reasons (see the discussion below). This focus results in little loss in generality since historically, the United States has provided over 60% of total global food aid and over 90% of all food aid has been given government-to-government. Henceforth, for compositional convenience, we use the term food aid to refer to government-to-government food aid.

Since food aid is conceptually similar to other types of foreign aid in that it is fungible and effectively a positive shock to government revenues, it is natural to explore the results from the studies on the relationship between foreign aid and conflict for relevant insights.[6]

[3]Akresh, Bundervoet, and Verwimp (2010) find that exposure to conflict in Burundi decreased the height for age z-scores of children by 0.047 deviations. In a related literature, studies investigate the political consequences of civil war (Blattman, 2008; Buckner and Miguel, 2006, 2008). In 2008, there were an estimated 40.1 million displaced people because of conflict in poor countries (United Nations, 2009). In the post-War era, deaths directly related to conflict exceed 40 million and indirectly related deaths are over three times that number (Leitenberg, 2006). There are studies which find that conflict does not necessarily have negative consequences on all outcomes (e.g., Guidolin and Ferrara, 2007; Miguel and Roland, 2009). See Blattman and Miguel (2010) for an overview of the literature on civil wars.

[4]Blattman and Miguel (2010) provide an overview of this literature.

[5]For example, a recent United Nations Security Council report writes that "... humanitarian resources, notably food aid, have been diverted to military uses. A handful of Somali contractors for aid agencies have formed a cartel and become important power brokers – some of whom channel their profits – or the aid itself – directly to armed opposition groups" (United Nations Security Council, 2010).

[6]Food aid shares the same problems of fungibility as all foreign aid. Pack and Pack (1993) provide evidence on the fungibility of aid against donor preferences using data from the Dominican Republic. In a World Bank survey of aid recipients, Feyzioglu, Swaroop, and Zhu (1998) found that most foreign aid was fungible, making it very difficult to accurately estimate the rate of return on donor-funded projects.

A review of this literature provides mixed evidence. On the one hand, studies such as Collier and Hoeffler (2002) theorize that foreign aid can reduce civil conflict by relaxing government budget constraints, and thereby increase government military spending, which in turn, deters the opposition parties from fighting. This theory is supported by empirical evidence by de Ree and Nillesen (2009). On the other hand, a large number of theoretical studies on the determinants of conflict have developed contest models that predict that the increase in government revenues will increase the returns to controlling the government, which in turn, increases the incidence of civil conflict.[7] In a recent study, Besley and Persson (2010) refines these theories and makes the additional argument that an exogenous increase in government revenues can cause conflict only in the presence of weak institutions or a non-representative government. The empirical evidence for the adverse effects of increases in government revenues on conflict typically comes from the resource curse literature. To the best of our knowledge, there are no studies which find that foreign aid can increase conflict.[8]

There are very few studies that specifically examine food aid. They are typically country-specific case studies, which make it difficult to draw conclusions about the average effect of food aid. For example, researchers studying the effect of food aid in Ethiopia find that it alleviates hunger (Alderman, Christiaensen, and Yamano, 2005; Levinsohn and McMillan, 2007; Quisumbing, 2003). This result has caused some to argue that food aid can be an effective policy for reducing conflict (e.g., Bardhan, 1997). However, critics have pointed to many other contexts where food aid has adverse effects.[9] Based on cross-country evidence, some have even made the more extreme criticism that food aid may not just be misdirected, but that it could have the unintended and perverse effect of *increasing* conflict (e.g., Knack, 2001).

Like all foreign aid, one of the difficulties in evaluating the effectiveness of food aid lies in its fungibility. For example, it is difficult for case studies to know whether a new hospital is funded by food aid revenues or if the new hospital would have been constructed even absent aid.[10] One way to address this difficulty is to create a counterfactual for comparison by conducting a cross-country study. This approach faces the difficulties of reverse causality and joint determination. Does aid cause recipients to do better? Or do countries receive aid because they are doing well? This is especially relevant since foreign aid is often conditional upon a government implementing policies that donors view as beneficial for promoting eco-

[7]For example, see the works of Garfinkel, 1990; Grossman, 1991;Grossman, 1999; Haavelmo, 1954; Hirschleifer, 1989; Skaperdas, 1992 .

[8]See the literature review by Blattman and Miguel (2010).

[9]Often, this is because aid is not targeted or delivered to the intended recipients. See the discussion in the section on Background for some examples of such instances.

[10]See Degnbol-Martinussen and Engberg-Pedersen (2003) for a description of the different types of foreign aid and the associated problems and benefits of each.

nomic or institutional development. In addition, it is possible that conflict and food aid receipts are both outcomes of a third omitted factor such as U.S. strategic objectives. For example, during the 1960s and 1970s, the U.S. government supported South Vietnam's war against North Vietnam by giving the S. Vietnamese government enormous amounts of food aid, which could be monetized and used to fund the war. In this case, the positive correlation between U.S. food aid receipts and conflict in South Vietnam would confound the effect of food aid with the effect of U.S. strategic objectives, which is unobservable.

The principal contribution of our study is to address the difficulties of reverse causality and joint-determination and estimate the causal effect of U.S. food aid receipts on conflict. We do this by exploiting plausibly exogenous variation in U.S. food aid from weather in U.S. wheat-producing regions and a country's tendency to receive any food aid from the United States. Since the 1950s, the U.S. government has provided price support for American farmers by purchasing wheat during high production years. Under PL480, which is also called the *Food for Peace Program*, this wheat is then given as food aid to poor countries. Therefore, U.S. wheat production is positively correlated with food aid receipts in poor countries over time. Poor countries that are more likely to receive U.S. food aid will benefit more from U.S. production booms. Therefore, we can instrument for the amount of food aid received from the United States with the interaction between U.S. wheat production and a country's tendency to receive U.S. food aid. To ensure that we do not confound our estimates by using potentially endogenous variation in U.S. wheat production, we use weather conditions in U.S. wheat-producing regions rather than actual production. We proxy for the tendency to receive U.S. food aid with the average probability that a country receives any U.S. food aid during our study period. Only the interaction terms of weather conditions and the average probability to receive U.S. food aid can be interpreted as exogenous. To address concerns that weather patterns may be correlated between the United States and poor countries or that changes in U.S. policy objectives may affect the impact of food aid, our baseline estimates also controls for factors such as weather conditions in the recipient countries, country fixed effects and region-year fixed effects.

Our empirical strategy assumes that conditional on the baseline controls, the interaction terms between weather conditions in U.S. wheat-producing areas and the average probability for a country to receive U.S. food aid only affects conflict in the recipient country through food aid. We do not take this assumption as given and conduct a large number of placebo and robustness exercises. Section 4 provides a more detailed discussion.

Using a country-level panel for 1976-2004, we find that the correlation between food aid and the incidence of conflict is negative but small in magnitude and statistically insignificant when we use a simple fixed effects strategy. In contrast, the 2SLS estimates of the effect of food aid on conflict show that increasing per capita U.S. wheat aid by 1% increases the incidence of internal conflict by approximately 1.65 percentage-points. In other words, increasing food aid by 61% (100/1.65) will double the incidence of civil conflict. Taken literally, this result, together with the result from Miguel, Satyanath, and Sergenti (2004), implies that a 7% increase in food aid increases the incidence of conflict by the same magnitude as a 5% reduction in GDP growth.[11] We find evidence to suggest that the increase in the incidence of conflict is driven by both an increase in the onset and duration of conflicts. The finding that the 2SLS estimates are larger in magnitude and have the opposite sign as the OLS estimates is consistent with the concern that the latter are negatively biased because the U.S. government conditions aid on factors that are correlated with high levels of conflict. There is no effect on other types of conflicts.

Our results are consistent with the predictions of the contest models of conflict discussed earlier. An important alternative explanation is that food aid increases conflict by reducing the relative prices of food, and thereby cause income inequality between agricultural and non-agricultural workers. However, this relative-price mechanism is unlikely to play a role in our study. We find that our instrument has no effect on the prices of food in recipient countries, which suggests that most of the food aid is being monetized and not delivered as cheap foods to the population.[12]

In addition to the main results, we find that our results are driven by countries that are unlikely to have civilian governments and experience a high level of average conflict incidence. The first result is consistent with the theoretical predictions of Besley and Persson (2010). The second is consistent with the more general notion that commitment problems contribute to conflict if we believe that such problems are more severe in countries that experience higher average levels of conflict.[13]

There are several important points to keep in mind for interpreting these results. First, in terms of the effectiveness of aid, our empirical strategy limits our study to one particular type of food aid – donor-driven government-to-government aid. This does not mean that all aid is bad. In particular, our results do not contradict evidence that emergency relief aid can be effective (Lavy, 1992). Second, we only examine one dimension of outcomes. Therefore, our results cannot reveal the overall welfare impact of food aid. Hence, for policy makers evaluating the benefits and pitfalls of food aid, it is important to take our results in

[11] Our results show that increasing food aid by approximately 7% (12/1.65) increases conflict by twelve percentage-points. Miguel, Satyanath, and Sergenti (2004) finds that a 5% reduction in GDP growth increases the incidence of civil war by twelve percentage-points.

[12] Moreover, since our empirical strategy compares conflict in countries when food aid levels are high to when they are low, the relative price effect will be differenced-out unless if it is asymmetric – i.e., inequality from an increase in agricultural incomes has different effects on conflict from inequality from a reduction in agricultural incomes.

[13] See section 3 for more discussion and references.

consideration of other outcomes.

This study contributes to our understanding of the economic determinants of conflict, the causes and consequences of foreign aid, and the small number of studies examining food aid.[14] To the best of our knowledge, we are the first to empirically study the effect of food aid on conflict. In addition to the main results, we make a significant step towards establishing the likely mechanisms. Our work is closely related to three recent empirical studies. Miguel, Satyanath, and Sergenti (2004) and Dube and Vargas (2009) develop clever identification strategies to study the causal impact of income shocks induced by rainfall shocks or commodity price shocks on civil conflict. Our study differs conceptually from these in that we are examining the effect of an increase in government income rather than individual income.

In using donor country shocks to instrument for aid, our work is similar to two recent studies by Werker, Ahmed, and Cohen (2009) and Ahmed (2010), which exploit variation in oil prices to examine the effect of foreign aid from OPEC countries to other Muslim countries. Although they do not examine conflict as an outcome, our finding that aid has adverse effects is broadly consistent with their conclusions.[15] In finding that aid is partly determined by changes in U.S. domestic production, our study is also related to previous works that find that aid is often determined by the strategic or economic needs of the domestic policies of donor countries (e.g., Alesina and Dollar, 2000; Kuziemko and Werker, 2006).

This paper is organized as follows. Section 2 discusses the background of food aid. Section 3 discusses how food aid can induce conflict. Section 4 presents the empirical strategy. Section 5 describes the data. Section 6 presents the results. Section 7 offers concluding remarks.

2 Food Aid

2.1 Food Aid Policy

Food aid has historically been a major element of development assistance geared to support longer-term development and it has been the primary response to countries and people in

[14]These studies have thus far focused on factors such as ethnic divisions (e.g., Alberto Alesina and Easterly, 1999; Fearon and Laitin, 2003; Montalvo and Reynal-Querol, 2005), income (e.g., Dube and Vargas, 2009; Miguel, Satyanath, and Sergenti, 2004), institutions (e.g., Gonzalez and Neary, 2008), propaganda (e.g., Yanagizawa-Drott, 2010), foreign aid (e.g., de Ree and Nillesen, 2009), trade (e.g. Martin, Mayer, and Thoenig, 2008) and commodity prices (e.g., Collier and Hoeffler, 2004; Guidolin and Ferrara, 2007; Ross, 2004). Studies such as Collier and Hoeffler (2000) have provided correlational evidence on aid and conflict. Collier and Hoeffler (2000) find that aid is negatively correlated with conflict duration. This is consistent with the negative correlation between food aid and conflict that we observe in the OLS estimates. For well-known studies of foreign aid, see those such as Burnside and Dollar (2000); Easterly (2003); Svensson (2003a). For studies of food aid, see studies such as Lavy (1992); Pedersen (1996); Kirwan and McMillan (2007); Levinsohn and McMillan (2007); Quisumbing (2003); Alderman, Christiaensen, and Yamano (2005) .

[15]Werker, Ahmed, and Cohen (2009) finds that aid has no beneficial effects on GDP growth but increases government consumption. Ahmed (2010) finds that aid decreases institutional quality. Our study also differs from their study in the outcomes we examine and the geographic scope, which extends beyond poor Muslim countries to include all poor countries.

crisis. It's relevance today is shown by the fact that it was featured prominently in the Millennium Development Goals. International institutional arrangements for food aid were first established during the 1950s. By the 1970s, food aid represented approximately a quarter of Official Development Assistance (ODA). The main goal was to convert "surplus" food production from rich countries into a useful resource in poor countries. However, criticism in recent years that this conversion has not been successfully made has caused food aid to decline to under 4% of total ODA by the 1990s. That said, bilateral food aid remains an important source of food for developing countries, especially for countries in Sub-Saharan Africa and Latin America and the Caribbean. The United States is the largest donor of bilateral food aid.

What we commonly refer to as food aid is comprised of three categories. First, there is program aid. These are subsidized deliveries of food to a central government that subsequently sells the food and uses the proceeds for whatever purpose (not necessarily food assistance). Program food aid provides budgetary and balance of payments relief for recipient governments. While this, in principle, increases flexibility for recipient country governments by allowing them to finance programs with the highest social returns, it also creates an opportunity for corruption. The government needs not spend the money on programs that would benefit the population. Second, is project food aid. It provides support to field-based projects in areas of chronic need through deliveries of food (usually free) to a government or NGO that either uses it directly (e.g., Food for Work, school feeding) or monetizes it, using the proceeds for project activities. The third category, emergency and humanitarian relief, consists of free deliveries of food to government and non-government agencies responding to crisis due to natural disaster or conflict. While the final category of food aid receives the most press coverage, it is by far the smallest in terms of quantity. The first two categories of aid, which is commonly referred to as donor-driven, makes up over 90% of global food aid.

This United States is the largest donor of food aid in the world, accounting for approximately 57% of global food aid in 1990 and 64% in 2000.[16] U.S. food aid flows to poor countries through several mechanisms. In this paper, we focus on PL480, which has historically been the most important, being responsible for over 70% of total U.S. food aid in the post-war era. It is also the most relevant to our paper because it administers the food aid that is driven by U.S. domestic needs.[17]

PL480 was established under the Eisenhower administration in 1954. President Kennedy

[16] It is followed by the EU countries, which together account for less than 20%. The other major donors are Japan, Australia and Canada, each contributing less than 3%. The United Nation's World Food Programme (WFP) accounts for the rest of food aid.

[17] Food aid is also administered by the McGovern-Dole International Food for Education and Child Nutritional Program, which was established in 2002 and replaced the Global Food for Education Initiative that was established in 2000. These programs are significantly smaller than PL480 and do not play an important role in our study.

renamed it to be called the *Food for Peace Program* in 1962. It comprises of three aid categories: Titles I, II and III. Title I is administered by the U.S. Department of Agriculture (USDA) and provides low interest loans to developing and transition countries for the purchases of U.S. agricultural commodities. Historically, it has been the most important component of food aid. It is typically administered government-to-government. Title II are gifts of food from the U.S. government for meeting emergency and non-emergency food needs. In recent years, emergency food needs have received much more resources than non-emergency food needs. It is often administered by NGOs. Title III provides government-to-government grants to support long-term growth in low income countries and makes up a very small part of PL480 food aid (Kodras, 1993).[18] The results of this study, which focuses on donor-driven aid and not emergency relief aid, will mostly reflect the effects of Title I and III aid.

Food aid policies require that at least 50% of food aid be in the form of whole grain commodities bagged in the United States. In practice, this means that almost all U.S. food aid is wheat. Food aid stocks such as those stored by the Bill Emerson Humanitarian Trust (BEHT) which holds four million metric tons of grain reserves to help fulfill PL480 food aid commitments in case of short domestic supplies in the U.S. has only held wheat since 1980. The Commodity Credit Corporation (CCC), a government cooperative that purchases up to thirty billion USD of grains for food aid, typically only purchases wheat and wheat flour. Therefore, we will only examine U.S. wheat production and U.S. wheat aid.

The main recipients of food aid are countries in Latin America, Africa and Asia. During the Cold War, food aid was sometimes used explicitly to fund U.S. government political or military objectives. For example, during the 1960s and 70s, food aid was used to support South Vietnam. In 1973, fully two-thirds of total Title I assistance was given to South Vietnam and Cambodia, more than twice as much as the aid given to all African nations combined (Saylor, 1977). In 1976, Congress reacted to this by legislating that at least 70% of U.S. food aid go to the poorest and most needy countries (Shaughnessy, 1976). We begin our study in 1976, after the end of the Vietnam War, to minimize the confounding influences that U.S. foreign policy would have on our estimates.

This study focuses on the effects of bilateral donor-driven government-to-government food aid from the United States. It is important to keep in mind for the interpretation of our results that most of this aid is given government-to-government. Our data does not allow us to separate out aid that is given to NGOs. However, as this is typically a small percentage of aid, our analysis will mostly capture government-to-government food aid.

For our sample period, donor-based aid is broadly based on need in the sense that rich countries do not receive aid. But on a year-to-year basis, it is largely unrelated to changes in

[18]PL480 Title I aid is administered by the USDA whereas Titles II and III are administered by USAID.

the needs of the recipient country. In fact, it is typically driven by domestic U.S. wheat production. Historically, the U.S. government purchased surplus production as a form of price support for its agricultural sector. This is held by the CCC through Section 416(b) of the PL480 and then transferred to overseas countries either in bilateral agreements or by agencies that coordinate multilateral food support (Hoddinott, Cohen, and Bos, 2004; Barrett and Maxwell, 2005; Diven, 2001). In 1996, the *FAIR Act Frames Farm Policy*, commonly referred to as *The 1996 U.S. Farm Bill*, added income support as an additional method of subsidizing domestic farmers in case of price drops but did not stop the policy of price support for wheat (United States Department of Agriculture Economic Research Service, 1996).[19] In addition to price support for U.S. farmers, U.S. food aid in terms of volume is positively correlated with U.S. food production for another reason. Aid is allocated in dollar terms. This means that during a year of high production and low prices, the same aid dollar buys more wheat. In our empirical analysis, we examine the effect of an additional metric ton of wheat rather than the effect of an additional dollar of wheat.

For the purposes of our study, it is important to note several facts. First, virtually all U.S. food aid is wheat. Therefore, our empirical analysis will focus on wheat production in the United States. Second, over 80% of U.S. wheat is harvested in the spring and most non-emergency aid is allocated at the beginning of a new Congressional session in autumn and shipped out by the end of the fiscal year in the following spring. Hence, in the empirical analysis, we will characterize food aid received in year t as a function of U.S. production from the previous year, $t-1$. Third, food purchased from U.S. farmers can be stored before given as food aid. Hence, even if all the wheat the government purchases are given out as food aid, we will not expect to observe a perfect correlation between U.S. production and aid. However, as long as there are positive storage costs or if the government cannot perfectly predict future production shocks, then U.S. production will be positively correlated with aid (i.e., the U.S. government cannot smooth aid donations perfectly over time).

Fourth, Congress requires that food aid be conditioned upon the human rights practices of recipient countries as reported by Freedom House indices. Since human rights practices are typically correlated with conflict, this means that aid is often cut when conflict levels are very high so that interpreting the correlation between conflict and aid can be very misleading with respect to the causal impact of aid on conflict. A negative correlation may simply be an artifact of the conditions of aid. This is important for the motivation of our empirical strategy.

Fifth, U.S. legislation requires that at least 75% of food aid be shipped on U.S. flagged cargo ships. It is estimated that as much as 90% of the budget allocated for food aid is spent in the U.S. (Barrett and Maxwell, 2005). This means that the dollar value of food aid is

[19]Price support for other commodities such as milk began to be phased out after 1996 (United States Department of Agriculture Economic Research Service, 1996).

not particularly meaningful for our study which wants to understand the effect of aid on the recipient countries. Therefore, for our empirical analysis, we will measure aid in metric tons of food. Related to this point is the fact that the market for wheat is very segmented such that the world price for wheat does not accurately reflect the price at which recipient governments can monetize the wheat aid receipts for. This segmentation is presumed by U.S. food aid policy which stipulates that the recipient government has much discretion over the monetization of the aid with the only constraint being that the prices faced by U.S. farmers must not be affected. Therefore, measuring aid in terms of volume in the empirical analysis allows us to side-step potential difficulties in interpreting the valuation of food across countries.

Finally, during the period of our study, 1976-2004, there were several region-specific shifts in food aid policy. For example, during the Carter administration (1976-80), the focus was on alleviating hunger worldwide. This caused a wider dissemination of food aid across countries than the previous decade, which had mostly focused food aid in South Asia for political reasons (O'Loughlin and Grant, 1990). However, at the same time, political needs in the Middle East caused the administration to shift large amounts of food aid to countries in that region (Cathie, 1989). The Reagan Administration (1981-89) sought to confront the Soviet Union in the Third World during what is often called the "second Cold War" (Halliday, 1986). As a result, massive amounts of military assistance and food aid were shifted to Central America and the Horn of Africa (Coneth-Morgan, 1990). After the Cold War ended, food aid became again more widespread. In particular, Eastern European countries which used to be within the Soviet sphere of influence began to received U.S. food assistance. More recently, since 2000, countries in the Middle East have received more U.S. food assistance presumably because of U.S. political and military involvement in that region. Being aware of these patterns in food policy is important for our study because it means that our empirical analysis will need to control for region-specific changes over time as well as the provision of total foreign aid.

2.2 Perceived Problems of Food Aid

The problems associated with food aid can be divided into three categories. The first problem is one that faces all foreign aid. Food aid can be a significant source of revenues for some recipient countries. In our sample, the 75th percentile observation in terms of the value of U.S. food aid received as a fraction of average GDP during 1976-2004 receives food aid from the United States that is approximately 0.5% of its GDP; the 95th percentile country receives food aid that is approximately 1.3% if its GDP. This increase in resources could increase the returns to controlling government revenues and political competition, which can lead to increased conflicts within the recipient countries.

A second commonly cited problem is that food aid increases the amount of cheap foods in recipient countries, and thus decreases the price of agricultural production and the income of farmers in those countries (Pedersen, 1996; Kirwan and McMillan, 2007). This not only decreases agricultural incomes but also increases income inequality between urban and rural workers. In her study of Somalia, Perlez (1992) writes that "donated food commodities were depressing the market for locally grown agricultural products [and] was not addressing longer-term development needs".

Finally, governments of poor countries often have little political incentive to deliver aid appropriately, i.e. to the most needy. For example, in his study of food aid in Rwanda during the early 1990s, Uvin (1998) found that aid was misused by the government and allocated to a few elites, which spurned discontent and promoted conflict. He said "... the development enterprise [aid] directly and actively contributes to inequality and humiliation. The material advantages accorded to a small group of people... living in Rwanda contribute to greater economic inequality and the devaluation of life of the majority" (Uvin, 1998, p. 142). Similarly, in Zimbabwe, the government would only provide food aid to known political supporters. "The U.S.-based Human Rights Watch had released a report documenting examples of residents being forced to display a ZANU-PF Party membership card before being given some government grain. Those who didn't went hungry, the group reported" (Thurow and Kilman, 2009). In Somalia, food aid was often not used to alleviate the hunger of any population. During the early 1990s, many observed food aid being traded for arms or stolen and then sold for money which was pocketed by the government (e.g., Perlez, 1992). Similarly, in Rwanda during the early 1990s, government stealing of food aid was so problematic that aid was canceled on several occasions (Uvin, 1998, p. 90).

3 Conceptual Framework

In this section, we discuss the two most obvious channels through which food aid can affect conflict and how our empirical strategy captures their effects. We will refer to the first channel as the *government revenues* mechanism. The food aid studied in this paper is like other types of foreign aid in that it is given government-to-government and fungible. Therefore, an increase in food aid is similar to any exogenous increase in government revenues. The

Note that most theoretical studies presume that political competition ubiquitously results in conflict. In a recent study, Besley and Persson (2010) emphasizes the importance of commitment and argue that an exogenous increase in government revenues will only increase conflict in the presence of weak institutions or a non-representative government, when the government and opposing factions cannot commit to make transfers to other factions.[21]

On the other hand, studies have argued that aid flows can reduce conflict. For example, increasing aid revenues can relax government budget constraints, which can increase military spending and deter opposing groups from engaging in conflict. This theory was formalized by Collier and Hoeffler (2002).

Our analysis is a reduced form estimate of the effect of an increase in aid revenues, which captures the net effect of the opposing forces. In addition to our main estimates, we will explore the prediction from Besley and Persson (2010) that an exogenous shock in government revenues is more likely to cause conflict in the presence of non-representative governments and weak institutions.

We will call the second channel through which food aid can affect conflict the *relative price* mechanism. While food aid can be monetized, it can of course, also be delivered to the population of the recipient country as cheap foods, which can affect the relative prices of food in the recipient country (e.g., Pedersen, 1996; Kirwan and McMillan, 2007). If food prices and therefore, agricultural incomes, in recipient countries are lowered, then the opportunity cost relative to the returns to fighting will be lowered for agricultural workers. However, this mechanism is unlikely to be captured by our empirical strategy, which controls for country fixed effects because it compares the incidence of conflict in years when a country receives more aid to years when it receives less aid. Therefore, the effect of changes in relative prices are differenced out unless if they are asymmetric.

To illustrate this point, consider a two-good economy where an individual works in producing either food or a non-food good. Assume that food aid levels can be high or low for exogenous reasons and that all food aid is delivered to the population as free food. In the high state, the influx of free food will suppress agricultural prices and incomes, which could decrease the opportunity cost for agricultural workers to fight relative to their expected returns from

in government revenues caused by factors such as an increase in world commodity prices because food aid is less likely to produce potentially offsetting wealth effects. For example, an increase in commodity prices simultaneously increases the returns to fighting and the opportunity cost to fighting because the government risks losing tax revenues from production when there are high levels of conflict (e.g., Fearon and Laitin, 2007). An increase in foreign aid revenues does not produce similar offsetting effects.

[21]Incomplete contracting and information asymmetry were ascribed as the causes for compromise breakdown by Fearon (1995). Since then, there have been studies on the role of information asymmetry such as Baliga and Sostrom (2004), Chassang and Padro-i Miquel (2008, 2009), Dal-Bo and Powell (2009), Powell (2002), Esteban and Ray (2008). For studies on the role of incomplete contracting, see for example those by Besley and Persson (2010), Fearon and Laitin (2003), La Ferrara and Bates (2001), McBride and Skaperdas (2007), Powell (2006) and Garfinkel and Skaperdas (2000a).

fighting.[22] In the second state, a symmetric situation arises. The lack of free food increases agricultural prices and incomes, which decreases the relative incomes of non-agricultural workers. Therefore, the non-agricultural workers will want to fight. In other words, even if food aid affects the relative prices of food and this causes income inequality and conflict, our empirical strategy will most likely not capture it. Our strategy mechanically differences out relative price effects unless if food is innately different from the other good in such that decreasing relative food prices is more likely to cause conflict that decreasing the relative price of the other good. This is an important point to keep in mind for interpreting our results.

4 Empirical Strategy

We begin with the following estimating equation that postulates a relationship between the incidence of conflict and food aid receipts.

$$Conflict_{irt} = \beta \ln FoodAidPC_{irt} + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_{rt} + \delta_i + \varepsilon_{irt} \quad (1)$$

In the equation, the incidence of conflict in a recipient country i from region r during year t, $Conflict_{irt}$, is a function of: the natural logarithm of the per capita quantity of food aid received from the U.S. by country i in year t, $\ln FoodAidPC_{irt}$; a vector of country-year controls, $\mathbf{X}_{irt}$; region-year fixed effects, φ_{rt}; and country fixed effects, δ_i.

Country fixed effects control for all time-invariant differences across countries. Region-year fixed effects control for all secular changes over time that affect countries within a given region similarly.[23] These control for factors that vary by region and time which may affect aid and conflict over time. For example, during the 1980s countries in Central and South America were strategically important to the U.S., when they received a disproportionate amount of food aid and experienced frequent conflict.

Interpreting OLS estimates of β as causal, is dangerous given problems of reverse causality and omitted variable bias. For example, recall from the discussion in section 2that U.S. food aid is conditioned on there not being excessive levels of human rights abuses, a characteristic that is correlated with the incidence of conflict. Alternatively, aid may be targeted to countries that are experiencing high levels of malnutrition, which could be a direct outcome of conflict.

To address these difficulties, we exploit two sources of variation in U.S. food aid receipts. First, we exploit time variation from changes in weather conditions in U.S. wheat-producing regions. Because of price support policies for U.S. wheat producers, good weather in U.S.

[22]The opportunity cost mechanism has been featured in many theoretical studies on the effect of economic shocks on conflict. It was particularly emphasized in Grossman (1991) and Collier and Hoeffler (2004).

[23]Following convention, we divide our sample into five regions: Sub-Saharan Africa (SSA), Latin America and Caribbean Countries (LAC), East Asia and the Pacific (EAP), Europe and Central Asia (ECA) and other countries (OTH).

wheat-producing regions (and thereby, high levels of wheat production) causes higher levels of food aid to be given by the U.S. government. Second, we exploit cross-sectional variation in countries' tendencies to receive U.S. food aid, which we proxy for with the average probability that a country receives any U.S. food aid during our study period. Only the combination of the two sources of the variation can be interpreted as plausibly exogenous. Therefore, we instrument for food aid received with the interaction terms of weather conditions in U.S. wheat-producing regions and the average probability for a country to receive any U.S. food aid. Note that because weather conditions only vary over time and a country's tendency to receive U.S. food aid only vary across countries, the main effects of these variables are absorbed by the region-year and country fixed effects.

In the first stage of our instrumental variables (IV) strategy can be written as the following equation:

$$\begin{aligned} \ln FoodAidPC_{irt} = & \sum_{m=1}^{12} \mu_m \, Temp_{t-1}^m \cdot AidRecip_i \\ & + \sum_{m=1}^{12} \nu_m \, Precip_{t-1}^m \cdot AidRecip_i \\ & + \gamma Conflict_{it-1} + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_{rt} + \delta_i + \varepsilon_{irt}. \end{aligned} \quad (2)$$

The log of U.S. per capita food aid received by country i in region r during year t, $\ln FoodAidPC_{irt}$, is a function of: twelve interaction terms of the average temperature in U.S. wheat growing areas in each month m of the previous year $(t-1)$, $Temp_{t-1}^m$, and the proportion of years in which country i receives food aid during the sample period, $AidRecip_i$; twelve interaction terms of the average precipitation in wheat growing areas of the U.S. for every month, $Precip_{t-1}^m$, and $AidRecip_i$; the lagged second stage dependent variable (incidence of conflict), $Conflict_{it-1}$; a vector of time and country-varying covariates such as weather conditions in the arable parts of the recipient country, $\mathbf{X}_{irt}$; region-year fixed effects, φ_{rt}; and country fixed effects, δ_i. All standard errors are clustered at the country level.

Since our estimation strategy controls for country-specific and year-specific factors using fixed effects, it is similar in spirit to differences-in-differences (DD) estimation strategy, where one compares conflict between aid recipients and non-aid recipients in years following good wheat-producing weather in the United States to years following bad wheat-producing weather in the United States. The main difference is that we use continuous measures such that we capture all of the variation in the data.

Our empirical strategy requires the assumption that conditional on the baseline controls, the interaction between U.S. weather conditions and the average probability of receiving U.S. food aid only affects conflict through U.S. food aid. With the exception of price pass-throughs, it seems highly unlikely that this assumption would be violated, especially since we always control for weather conditions in arable regions of recipient countries to control for the possible geographic correlations in global weather patterns. We consider price pass-throughs below.

U.S. production can affect recipient-country food prices through two channels. First, it can affect prices through aid. Higher U.S. production can lower food prices in recipient countries by increasing food aid quantities and therefore the supply of cheap foods. For the reasons we discussed earlier, it is unlikely that our strategy can capture relative-price effects. However, the presence of such effects do not invalidate our strategy because it is simply one of the mechanisms captured by our reduced form estimates. The second way in which U.S. production can affect food prices in recipient countries is more troublesome. Since the United States is one of the world's main food exporters, an increase in U.S. production could not only increase U.S. food aid, but also decrease world food prices, which can potentially affect conflict in any country that imports or exports food through channels that are not related to food aid. This will confound our empirical strategy if the extent of the pass-through differs according to a country's average tendency for receiving U.S. food aid.

Our prior is that this is very unlikely because the market for food is very segmented across regions. In fact, such segmentation is explicitly assumed by U.S. food aid policy, which allows recipient governments to sell and monetize the food aid as long as it does not affect the prices of American farmers. The assumption that there is little price pass-through between the U.S. grain market and food prices in poor countries is born out by the data. The FAO reports food commodity prices from approximately 54 countries of our sample during 1991-2008 in terms of current USD. Using this data, we find that there is no correlation between U.S. annual grain production and wheat prices in aid-recipient countries. The correlation coefficient is very small in magnitude, 0.07, and statistically insignificant. There is no difference in this relationship according to the average tendency to receive any food aid from the United States. This supports the belief that there is little price pass-through between the United States and aid-recipient countries.

Second, our strategy faces the difficulty of weak instruments in using the 24 interacted weather instruments. These instruments can be weak for two reasons. Average weather conditions in the U.S. may be imprecise predictors of total wheat production since the weather conditions and seasonal patterns for producing wheat varies across the United States. Second, interacting weather conditions with the average probability of whether a country receives any food aid can further weaken the instruments.[24] Recall that we need to exploit variation

[24]The most well-known example of this latter problem is Angrist and Krueger (1991), where quarter-of-birth were by themselves strong instruments for educational attainment, but when interacted with the state of birth dummies became weak instruments.

from interaction terms because this allows us to control for country fixed effects, which is necessary for avoiding omitted variable bias caused by time-invariant country-specific factors. Weak instruments can bias the estimated coefficients and standard errors of 2SLS. We address this problem by using the conditional likelihood ratio (CLR) test developed by (Moreira, 2003). CLR overcomes the distortions of standard tests by adjusting the critical values for hypothesis tests from sample to sample so that, for given data, the critical values used yield a correct significance level because they are conditioned on that data.[25] This method has come to be the test of choice, particularly in over-identified systems with a single endogenous variable (e.g., Andrews, Moreira, and Stock, 2005; Andrews, Moreira, and Stock, 2006). We will also estimate the effects of food aid using the LIML estimator, which more robust to weak instruments than 2SLS.

Third, one could be concerned over the interpretation of our estimates as the *average* effect of food aid. The identification relies on the differences in the effect of changes in U.S. production across countries with different average tendencies to receive any U.S. food aid. If the elasticity of conflict with respect to food aid differs with the average tendency to receive any U.S. food aid, then our estimates will not capture the average effect. For example, if countries that have a higher average tendency to receive U.S. food aid are also more volatile in that a small amount of change in food or government revenues can lead to very large changes in conflict incidence, then our strategy will overestimate the true average effect. This does not challenge the internal validity of our estimates. Rather, it is a question over the external validity of our estimates that is innate to any instrumental variables estimation, which estimates the *Local Average Treatment Effect* (LATE)(Imbens and Angrist, 1994). To address this issue, we will examine whether the effect of food aid differs for countries that are very likely to receive some U.S. food aid on average to those that are less likely.

Another concern for our strategy is the potential correlation of the error terms. One may be concerned that there is auto-correlation across shocks that affect conflict within countries. To address this, the main results will present standard errors that are clustered at the recipient-country level. One could also be concerned that there is correlation in shocks that affect agricultural production across countries within a year, in which case we should cluster the standard errors at the year level. See the section on robustness for more discussion.

To help address some of the issues raised above, our baseline estimates will control for a large set of country-year factors – e.g., the vector $\mathbf{X}_{irt}$ from the first and second stage equations, equations (1) and (2).

- Monthly precipitation and temperature conditions of the arable regions in recipient countries. These control for the possibility that weather patterns may be negatively corre-

[25] Alternative tests in the case of weak instruments and one endogenous regressor are Anderson and Rubin (1949) and Kleibergen (2002).

lated between the United States and recipient countries, and cause a spurious correlation between U.S. production and recipient conflict levels because recipient country weather shocks cause low production and therefore conflict (e.g., Miguel, Satyanath, and Sergenti, 2004).

- Lagged conflict. This controls for potential hysteresis in conflict overtime. If conflicts persist for reasons spurious to food aid, then the occurrence of conflict last year will mechanically increase the likelihood of conflict this year regardless of food aid. In our estimation, we are careful to address the potential bias caused by controlling for the lagged dependent by also estimating the effects without the control.
- The interaction terms of the average logarithm of log per capita cereal imports into a country and the full set of year dummy variables and the interactions terms of the average log per capita cereal production in the recipient country and the full set of year dummy variables. As discussed earlier, there is no reason to believe that U.S. wheat production affects the food prices in recipient countries. These controls are extra precautions because if there is any pass-through, they address the possibility that U.S. production affects world food prices, which has differential effects for countries of different domestic production levels and import levels. Controlling for the two sets of interaction terms – average cereals imports and average cereal production, each interacted with the full set of year dummy variables – controls for this possibility. We use the averages for each country to avoid the potential endogeneity from the fact that imports and production in a given year can be an outcome of food aid.[26]

In addition to the exercises described in this section, we conduct many placebo and robustness tests, which we motivate and describe later in the paper in Section 6.2.

5 Data

We construct a country level panel using data from several public sources. Our sample begins in 1976, after the Vietnam War ends, and finishes in 2004, the most recent year for which we have conflict data. It is comprised of a panel of 113 countries over 29 years. These are all the countries that are classified as non-High Income Countries by the World Bank for which we have production, food aid and conflict data. The panel is not balanced because many countries, mostly from the former U.S.S.R., only have data for the post 1991 period. However, this does not have a big effect on our results. They are very similar when we used a balanced panel where such countries are excluded. Therefore, we use the full sample in

[26]Alternatively, we can use a more flexible functional form by estimating the residuals between recipient country food import and food production and food aid, and controlling for these residuals. This does not affect our estimates. Therefore, for the sake of brevity, they are not reported in the paper. They are available upon request.

this paper.[27]

The main outcome measure is the incidence of conflict, which we construct from the publicly available conflict data reported by the UCDP/PRIO Armed Conflict Dataset. We examine three types of conflict reported in this dataset:

1. Interstate armed conflict occurs between two or more states.
2. Internal armed conflict occurs between the government of a state and one or more internal opposition group(s) without intervention from other states. These are often referred to as "civil wars".
3. Internationalized internal armed conflict occurs between the government of a state and one or more internal opposition group(s) with intervention from other states (secondary parties) on one or both sides.[28]

Based on the UCDP data, we construct three dummy variables to indicate if there was a conflict of each type. We then generate a fourth dummy variable that equals one if there is any conflict of any type and a fifth dummy variable that equals one if there was any conflict that incurred more than 1,000 battle deaths. See the Data Appendix for details on how we constructed these variables.

Conflicts are coded by the UCDP as "missing" if they cannot be confident that a conflict has occurred, or if they are unsure of the nature actors and intensity of the conflict. This can potentially result in their under-reporting conflicts.[29] To check that our results are not an artifact of this coding peculiarity, we directly examining whether the occurrence of a missing value is correlated with U.S. wheat aid. We create a sixth outcome dummy variable that equals one if the observation has a missing conflict value.

Our main measures of food production and food aid are from data reported by the *Food and Agriculture Organization* (FAO) of the United Nations. Our study focuses on U.S. wheat aid and wheat production which are reported in dollar values and in terms of volume. We will use the latter to avoid measurement issues related to the pricing of aid that we described earlier in Section 2. Our main explanatory variable is per capita U.S. wheat aid. To calculate this, we divide the volume of U.S. wheat aid a country receives in a given year by its total population as reported by World Development Indicators (WDI).

U.S. food aid under PL480 is published annually by the USAID Greenbook. This breaks down food aid into Titles I, II and III. We can use these to check our assumption that U.S. production shocks mostly affect Titles I and III aid and not Title II aid, which is mostly

[27]Results from the alternative balanced panel sample are not reported in the paper for the sake of brevity. They are available upon request.

[28]The UCDP reports a fourth type of conflict, extra-systemic armed conflict, which occurs between a state and a non-state group outside its own territory. In the data, there are very few cases of extra-systemic conflict. Therefore, it will not be included in the empirical analysis.

[29]In our sample, there are 150 missing values.

used for emergency aid. However, because the USAID Greenbook only reports aid in dollar terms, we will not use its aid measures as explanatory variables for the main analysis.

The weather data we use are from the *Terrestrial Air Temperature and Precipitation: 1900-2006 Gridded Monthly Time Series, Version 1.10*, which has recently been used by Dell, Jones, and Olken (2008). The data reports, for each month, daily mean temperature (measured in degrees Celsius) and daily mean precipitation (measured in millimeters) with 0.5 degree by 0.5 degree (approximately 56 km by 56 km) grid-cells globally from 1900 to 2006. For documentation see Matsuura and Willmott (2007). We calculate the yearly monthly mean temperature and precipitation for wheat-producing regions in the United States, and for regions that are suitable for the production of any staple crops in recipient countries. We control for weather in all regions that produce any staple crops in recipient countries to address the fact that many recipient countries do not produce wheat but consumers may substitute wheat aid for other staple crops. Therefore, if we are concerned that the effect of wheat aid partly rests on the amount of substitutable foods produced in recipient countries, we will need to control for weather conditions in regions that produce any staple crops. To determine the suitability of each geographic cell for the production of wheat we use suitability information from the Food and Agricultural Organizations's (FAO) GAEZ 2002 Database. The FAO provides data on the suitability of 0.5 degree by 0.5 degree global grid-cells for producing a variety of different crops. See Nunn and Qian (2010d) for a detailed description of the data and our specific method of calculation. Our constructed variables are the monthly averages of daily mean temperature (in degrees Celsius) and the monthly average of daily precipitation (millimeters) for wheat-producing regions in the United States and for all staple crop producing regions in food-aid recipient countries. We code a grid-cell as being suitable for the production of the relevant crop or crops if the grid-cell is identified as being moderately suitable or better for growing the crop or crops in question.

Finally, the FAO also reports food commodity prices. For our sample, we are able to obtain food prices for 54 countries from 1991 onwards. They are reported in current USD. In our analysis, we examined the prices of wheat and all other cereal and staple foods. As the results are very similar, we only report the estimates for wheat in the paper. There are many problems with this data. The main issue is that the reported prices are typically collected from urban markets and do not reflect rural farmgate prices. However, this does not affect our interpretation in our use of this data to investigate the extent to which there is price pass-through between U.S. and recipient country foods prices. Since most food imports are delivered to urban markets, price pass-throughs between U.S. and urban markets should be at least as high, if not higher, than between the United States and rural farmgate prices.

Table 1 reports the means. The data shows that the most common form of conflict is internal conflict. Approximately 20% of the country-year observations experience some level of internal conflict. 26% of the sample experience any level of conflict. Almost 17% of the sample have missing values for conflict. Only 10% of the sample experience conflict with more than 1,000 battle deaths. On average, a country will receive food aid from the United States 38% of the time. The average amount of total food aid (all cereals and coarse grains) that a country receives from the United States is approximately 35,538 MT. Of this 31,696 MT, is made up of wheat. Therefore, our study of the effects of U.S. wheat aid is effectively a study of the effects of all U.S. food aid. In per capita terms, a country receives three kilograms of wheat annually per person on average from the United States. Recall from our earlier discussion that historically, the majority of food aid will comprise of PL480 food aid, which is reported by type from the USAID Greenbook. From this data we see that most of PL480 food aid are Titles I and II aid.

The production data for cereals shows that the U.S. produces over eleven times the amount of cereals as food aid recipient countries. The data shows that food aid from the U.S. is significant relative to domestic production. On average, wheat aid from the United States is less than 1% of domestic cereals production (in terms of volume). However, amongst country-year observations that receive some aid, aid is on average 2% of domestic production for the median observation, 8% for the observation on the 75th-percentile and 27% for the observation on the 90th-percentile of the distribution of the quantity of aid received. The countries in our sample do not import significant amounts of cereals. The median observation imports no cereals. However, the value of cereals imports as a share of GDP is large for observations on the upper quantiles of the distribution of cereals import. For example, the 75th percentile and the 90th percentile observations import cereals that are 5% and 25% of GDP. Relative to U.S. food aid, food imports are relatively unimportant as they are only 7.7% of the value of food aid on average. It is interesting to note that food aid and cereals imports are not correlated. The correlation coefficient is only 0.07 and statistically insignificant.

On average, recipient countries have per capita GDP of 1,924 USD (Constant 2006). They have polity2 indices of near zero, which is typically the cutoff political scientists use to define a country that has an autocratic executive (e.g., < 0) versus one that has a democratically accountable executive (e.g., > 0). Approximately 42% of the populations of recipient countries live in urban areas. Appendix Table A1 presents the average monthly temperature and precipitation in wheat-producing regions in the United States.

Before we present the estimation results, we can illustrate the first stage and reduced form relationships in the time-series. Figure 1 plots lagged log U.S. wheat production and average log per capita U.S. wheat aid received over time. It shows a strong positive correlation. Figure 2 plots lagged log U.S. wheat production and the average probability of any conflict over time. It shows that with the exception of the height of tensions towards the end of the Cold War (1987-89), there is a strong positive correlation between U.S. wheat production and the probability of a conflict.

6 Results

6.1 Main Results

Table 2 columns (1)-(6) presents the OLS estimates of the relationship between per capita U.S. wheat aid and the incidence of any conflict. We introduce the baseline controls gradually to illustrate their effect until we have all of them together and estimate equation (1). The estimates are small in magnitude, mostly negative in sign and statistically insignificant. In Table 3 Panel A, we show the estimated effects for all of the conflict outcomes with all the baseline controls. They are all small in magnitude and statistically insignificant. The sample used for the main results in columns (1)-(5) only contain observations for which the UCDP did not report a missing value for conflict. Those observations are included in the sample used in column (6), where we estimate the effect of aid on the probability that there are missing values.

Before estimating the first stage equation with the 24 interacted weather instruments from equation (2), we illustrate the intuition behind the first stage with an alternative instrument: the interaction term of lagged U.S. wheat production and the proportion of years a country received food aid from the United States during the sample period. While this instrument is not as endogeneity-proof as the main weather instruments, it has the advantage that it is easy to interpret and allow us to clearly verify the channels through which U.S. wheat production affect food aid. Therefore, we present them to complement the main first stage estimates using the weather variables. The first stage estimates using the alternative instrument are presented in Table 4. Panel A column (1) shows that for countries that always receive some U.S. food aid, the elasticity between lagged U.S. wheat production and per capita wheat aid received from the United States is 0.07. The estimate is statistically significant at the 1% level. The estimates in columns (2)-(4) show that U.S. wheat production is positively correlated with the logarithm of per capita Title I and III food aid, but not with Title II food aid. This is consistent with the fact that Title II aid is mostly used for emergency relief and therefore should not respond to U.S. domestic production as much. Note that aid in column (1) is measured in terms of volume while it is measured in terms of value (constant USD) in columns (2)-(4).

In Panel B of Table 4, we examine the effect of U.S. wheat production on a set of placebo outcomes. Columns (1)-(4) show that the interaction term between lagged log U.S. wheat production and the tendency to receive U.S. food aid is uncorrelated with other foreign aid:

the log of per capita aid received from the World Food Program, the log of non-U.S. per capita bilateral aid and the log of per capita total Official Development Assistance.

Next, we examine the relationship between the interaction term and recipient country food production and prices. Columns (4)-(5) show that it is uncorrelated with per capita cereal production and the price of wheat in the recipient country.[30] The fact that we find no effect of recipient wheat prices is important because it suggests that food aid, on average, does not affect the relative prices of foods in recipient countries. This would occur if recipient governments monetized the food aid and did not deliver it as cheap food to the population. We cautiously interpret this evidence as only suggestive because of the issues with the price data that we discussed earlier.

Columns (6)-(8) show that it is uncorrelated with per capita U.S. wheat aid lagged one, two or three years. These estimates support the identification assumption that an increase in weather-driven U.S. wheat production increases wheat aid in the following year but does not affect our outcome variables through other channels such as other types of aid.[31]

The estimates shown in Table 4 Panel B are important in establishing the validity of our instrument. They show that the interaction of lagged U.S. wheat production and the average tendency to receive U.S. food aid is correlated with government-to-government food aid in the current year, but not in previous year; is not correlated to any other types of aid; and is not correlated with recipient country food production or prices.

Table 5 shows the first stage estimates from equation (2). Column (4) shows the baseline estimates with all of the baseline controls: region-year fixed effects, monthly mean temperature and precipitation in the arable regions of the recipient countries and country fixed effects. The coefficients of the monthly interactions are difficult to interpret. At the bottom of the table, we present the F-statistic for the test of joint significance. It shows that the instruments in the baseline equation are jointly different from zero and significant at the 1% level. The F-statistic does not pass the Stock-Yogo test for weak instruments. To address this, we estimate (Moreira, 2003) CLR confidence intervals and LIML estimates along with the 2SLS estimates.

In Table 2, columns (7)-(12), we report our 2SLS estimates of the effect of food aid on the incidence of any conflict in the recipient country. As with the OLS estimates, we introduce

[30]Similarly, we find no correlation between the interaction term between lagged log U.S. wheat production and the tendency to receive U.S. food aid and the log of U.S. economic aid or the log of U.S. military aid. We can also measure production of coarse grains or meat in recipient countries, as well as the prices of other food commodities. We find no correlation between the interaction term between lagged log U.S. wheat production and the tendency to receive U.S. food aid and any of these outcomes. These results are not reported for the sake of brevity. They are available upon request.

[31]The reduced form estimates of the effect of the weather instruments are presented in Appendix Table A2. For interpretational ease and consistency, we also present the reduced form estimates with the alternative instrument – lagged U.S. wheat production interacted with the average probability of receiving food aid – in Appendix Table A3.

the controls gradually until we estimate the full baseline specification in column (12). The estimates show that the controls do not greatly affect the magnitude or precision of the estimates until we add the control for the lagged dependent variable. Adding this control reduces the magnitude of the estimate by half. This is consistent with the notion that conflicts may persist for reasons that are spurious to food aid. It also shows that our control of the lagged dependent variable does not bias our baseline estimates upwards. The estimate in column (12) shows that a 1% increase in per capita food aid from the United States increases the incidence of any conflict by 0.0172 or 1.72 percentage-points. This estimate is statistically significant at the 5% level.

In Table 3 Panel B, we present the 2SLS estimates with all of the baseline controls for all of the conflict outcomes. The 2SLS estimate in column (2) shows that increasing food aid by 1% increases the incidence of civil conflict by 0.0165 or 1.65 percentage-points. This estimate is statistically significant at the 1% level. The estimate in column (5) suggests that food aid also increases the likelihood of a large conflict that incurs more than 1,000 battle deaths. However, we will show in the next section that this estimate is not robust. The estimates in columns (3)-(4) and (6) show that food aid has no effect on the incidences of interstate conflicts, international conflicts or the occurrence of a missing value in the UCDP dataset. The estimates are small in magnitude and statistically insignificant. The latter estimate in column (6) is reassures us that our main estimates are not biased by the coding peculiarities of the UCDP. Recall that the sample size in columns (6) is larger than those from columns (1)-(5) because it is not restricted to observations for which the UCDP assigned non-missing values to the conflict variable.

Below the regression results, we present 95% CLR confidence intervals, LIML coefficient estimates and their p-values. Consistent with the presence of weak instruments, the CLR interval is larger than what the estimated standard errors from using 2SLS would imply. However, the intervals in columns (1) and (2) for the estimated effect of aid on the incidence of any conflict and the incidence of civil conflict do not contain zero. The p-values show that these two estimated effects are statistically significant at 1% significance levels. Therefore, we can reject the null that food aid has no affect on internal conflict or any conflict. Unlike columns (1) and (2), the CLR intervals in columns (3)-(4) and (6) contain zero. Therefore, we cannot reject the null that food aid has no effect on the incidences of interstate, international conflicts and the UCDP coding an observation as missing.

Note that in all cases, the estimated effects implied by the CLR intervals and the LIML estimates are larger in magnitude than but not statistically different from the 2SLS estimates. This is consistent with the fact that weak instruments bias 2SLS towards the bias of OLS, which we believe to be downwards since U.S. policy stipulates that food aid not be given to countries with conditions that are likely to be positively correlated with conflict.

We conclude that our result that aid causes conflict is not undermined by weak instruments and that our 2SLS estimates are robust to correcting for weak instruments. Henceforth, we will interpret the more conservative 2SLS estimates as our main results.[32]

6.2 Additional Robustness Checks

Confounding Factors First, we check the robustness of our results to other types of foreign aid, and institutional and economic factors. The sample for this exercise is smaller than the sample used for the main estimates because we do not have some of the control variables for all observations. In Table 6 Panel A, we focus on internal conflict, which is the conflict outcome most responsive to food aid.[33] For consistency, we re-estimate the baseline 2SLS equation on this reduced sample and present the estimates in column (1). The estimate is very similar to the baseline estimate using the full sample. It is also significant at the 1% level. The LIML estimate, CLR 95% confidence intervals and p-values are reported at the bottom of the table.

In column (2), we control for the natural log of per capita ODA (total World Development Assistance). The estimates are all similar in magnitude to the baseline and statistically significant at the 1% level. In column (3), we control for lagged per capita GDP to control for the possibility that aid and conflict are both outcomes of low income. To the extent that income is agricultural, this should already be accounted for in our baseline estimates which control for agricultural conditions in the recipient country. Adding the control for the log of GDP per capita also accounts for income that is not associated with weather conditions. We control for the lagged GDP because contemporaneous GDP per capita can be an outcome of aid. Column (3) shows that adding this control does not affect our estimates. In column (4), we alternatively control for the interaction of average log GDP per capita and the full set of year dummies. This controls for the possibility that countries of different income levels experienced different trends in conflict over time. The interaction with year dummy variables allow the effect of average income to vary fully flexibly over time. This set of controls does not affect our estimates.

In column (5), we control for the political institutions of a country measured by the polity2 index, which reflects the autocracy of the executive. We cannot control for a contemporaneous measure as it can be an outcome of food aid. Therefore, we control for the interaction of

[32]For consistency, we present the 2SLS estimates from using the alternative one-instrument strategy in Appendix Table A3 Panel B. The estimates are larger in magnitude but not statistically different from the main 2SLS estimates in Table 5. They produce the same pattern in that food aid increases the incidence of any and civil conflict but have no effect on other types of conflict.

[33]The other estimates are not reported for the sake of brevity but are available upon request.

average polity2 scores and year fixed effects. The estimates are similar in magnitude to the baseline and statistically significant at the 1% level. We also used other measures of political institutions such as average democratic accountability calculated from Freedom House Indices and the probability of having a civilian government calculated from variables provided by the Banks data set. Our results are robust to controlling for these measures interacted with the full set of time dummy variables. We do not report them in the paper for brevity. They are available upon request.

Finally, in column (6), we control for all of the controls together. The estimate is similar in magnitude relative to the baseline estimate and is statistically significant at the 1% level. We conclude from these estimates that our main estimates are very robust to additional controls.

In Panel B, we repeat these robustness checks for our estimated effect on the occurrence of a big war. We find that these estimates are not robust to either the sample change or the additional controls. Therefore, we cannot conclude that food aid increases the incidence of large wars. Henceforth, we conduct robustness checks only on internal conflict.

In addition to these robustness checks, we also check that our estimates are not sensitive to our choice of lags. We re-estimated the second stage equation alternatively using U.S. weather lagged two years and the moving average of U.S. weather over the past two years as instruments. The estimates are very similar in magnitude and significance. For brevity, we do not report these additional results in the paper. They are available upon request.

Correlated Shocks For reasons that were described in section 4, we check that our results are robust to alternative methods of adjusting for standard errors. Our main estimation clusters the standard errors at the country level. These are re-stated in columns (1) and (5) of Table 7. Then, we show Newey-West standard errors that corrects for heteroskedasticity in small samples. Third, we show the estimated standard errors from when we cluster at the year level. This corrects for spatially correlated shocks within a year. The standard errors are smaller than the robust standard errors or those that are clustered at the country level, which most likely reflects the fact that there are only 29 years in our sample and clustering at the year level induces small sample bias. Finally, we cluster the standard errors at the year and country levels. The estimated standard errors are very similar to the main estimates where we only cluster at the country level. These results show that our main results which cluster the standard errors at the country level are the most conservative. We conclude that the significance of our 2SLS estimates are not sensitive to different methods of adjusting the standard errors.

Outliers and Influential Observations Finally, we check for the presence of outliers in our estimates and investigate which observations are the most influential in driving our results. First, we calculate the studentized residuals from the first stage and reduced form estimates. Observations with large residuals are those whose dependent-variable value is unusual given its values on the predictor variables and may indicate a sample peculiarity or may indicate a data error. Therefore, we need to check that our estimates are robust to their exclusion. In Table 8 column (2), we estimate the 2SLS estimate on a sample where we exclude all observations whose absolute values of residuals are greater than the conventional threshold of 2.5. The estimate is very similar to the baseline, which is re-stated in column (1). It is also statistically significant at the 1% level.

Next, we calculate the leverage ratios from the first stage and reduced form estimates. Leverage ratios measure how far an independent variable deviates from its mean. Observations with high leverage ratios can affect the estimate of regression coefficients. Therefore, we want to check that our estimates our robust to their exclusion. In column (3), we estimate the 2SLS estimate for a sample for where the observations with leverage ratios greater than the conventional threshold of $(2k + 2)/n$ are omitted. The estimate is again very similar to the baseline estimate shown in column (1). It is also statistically significant at the 1% level.

We also examine which observations have the most overall influence by calculating the DFITS from the first stage and reduce form estimates. They are listed in Appendix Table A4. The five countries that provide the highest numbers of influential observations according to this measure are Jordan, Guyana, Egypt, Cambodia and Suriname. In Table 8 column (4), we re-estimate the baseline 2SLS using a sample where we exclude all observations for which the first stage or reduced form DFITS is greater than $2 \times \sqrt{k/n}$, the conventional threshold for an "influential" observation. The estimated effect is very similar in magnitude to that from the full sample in column (1) and statistically significant at the 5% level.

In column (5), we estimate the baseline 2SLS on a sample where we simultaneously exclude all of the observations excluded in columns (2)-(4). The estimate is very similar in magnitude to the baseline shown in column (1) and statistically significant at the 1% level. We conclude that our baseline estimates are very robust to the exclusion of outliers and influential observations.

In addition, we also check that our results are not driven by the difference between countries that never receive U.S. food aid from other countries. In column (6), we re-estimate our results on a sample where we exclude the twelve countries that never receive U.S. food aid during our sample period. The estimates are very similar.

Heterogenous Weather Shocks and Wheat Crops within the U.S. A concern with our IV strategy is the use of average monthly weather conditions across all wheat-producing regions of the U.S. to predict aggregate wheat production in the U.S. in a given year. This is because locations produce either winter wheat or durum (spring) wheat, with each type of wheat having different planting, growing, and harvesting months. As a result, it is likely that there is heterogeneity across locations in the effects that weather shocks in a given month have on wheat production, and that this heterogeneity depends on the type of wheat grown in the location.

Motivated by this concern, we undertake an alternative 2SLS strategy. We estimate the relationship between monthly weather shocks (rainfall and temperature) and wheat production at the wheat type (durum/spring and winter), county and year levels.[34] This allows us to estimate a relationship between weather and production that differs dependent on the type of wheat grown. We then construct estimates of production by wheat type, county and year that is predicted by monthly weather shock. We then aggregate over counties and wheat types to obtain a measure of total wheat production each year, that is predicted by local weather conditions in wheat-producing regions.

The first stage, reduced form and IV estimates are shown in Appendix Table A3 Panels C and D. The IV estimates are very similar to our main estimates. The point estimates using this alternative strategy are similar in magnitude to the main LIML estimates and always within the CLR intervals. However, they are less precise with larger standard errors.

6.3 Comparison with Previous Studies

Our baseline estimate, which is restated in Table 9 column 1, includes a lag of the dependent variables. In their well-known examination of the effect of income on civil conflict by Miguel, Satyanath, and Sergenti (2004), the authors estimate an equation similar to ours, where the dependent variable is incidence of civil war, but do not control for the existence of conflict in the previous period. Recall from Table 3 that excluding the control for the lagged dependent variable causes our estimates on the incidence of any conflict to be larger in magnitude. Table 9 column (2) shows that, not surprisingly, the same is true for our estimates on the incidence of civil conflict. Therefore, our results on civil war are not biased upwards by our inclusion of the lagged dependent variable. In column (3), we follow (Miguel, Satyanath, and Sergenti, 2004) and control for country specific time trends. In column (4), we relax the linearity assumption by controlling for linear country-time trends and quadratic country-time trends. Neither of these sets fo controls affect our estimate.

Thus far, our results show that food aid increases the incidence of civil conflict. This can

[34]The annual county-level wheat production data are from the USDA.

occur if food aid increases the probability that civil occurs (e.g., onset), and also if food aid prolongs the duration of existing conflict. We attempt to quantify the contribution of these two mechanisms. In a well-known study, (Collier and Hoeffler, 2004) devised a strategy to disentangle the conflict onset from conflict incidence. They created a dependent variable that is an indicator that equals one for the first year of a conflict in a given country and then omitted all observations for which there is conflict but for which it is not the first year from the sample. For our sample, this is equivalent to if we simply dropped all observations that experience continuing conflict. We repeat our baseline 2SLS estimation on this restricted sample. The estimates are reported in column (5) of Table 8. The estimate is positive in magnitude but statistically insignificant.

Since the lack of precision can be partly due to the reduction in sample size, we use an alternative strategy borrowed from Fearon and Laitin (2003). Using the full sample, we estimate our baseline 2SLS specification with onset (e.g., a dummy for the first year of a conflict) as the dependent variable and an additional control for whether there was a conflict in a country in the past year. The estimate is reported in column (6). It is positive but only significant at the 15% level. It suggests that increasing per capita food aid by 1% will increase the probability that a civil conflict begins by approximately 0.7 percentage-points.

Next, we attempt to examine the effect of food aid on conflict duration, an outcome that has received relatively less attention the conflict literature. However, in our context, it is very interesting. One can just as easily imagine that an increase in government resource can prolong conflict just as much as it can cause the start of a conflict. We create a dependent dummy variable that equals one if there is an ongoing war (i.e., if there is a conflict but it is not the first year of the conflict). The regression in column (7) does not control for a lagged internal conflict incidence and the estimated effect is positive and statistically significant at the 1% level. However, when we control for lagged conflict to control for the fact that the occurence of conflict in the previous period can mechanically affect whether a continued conflict can be observed in the current period, the estimate is reduced in magnitude and only significant at the 20% level.

To summarize, the finding that food aid increases the incidence of internal conflict is not specific to our choice of specifications. The estimated effects are actually larger in magnitude when we use specifications from Miguel, Satyanath, and Sergenti (2004). The results on conflict onset and duration suggest that food aid increases the incidence of conflict through both channels. However, we do not have enough statistical power to make conclusive statements on this issue.

6.4 Heterogeneous Effects

In this section, we explore the existence of heterogeneous effects of aid. We focus on the discussion on the incidence of internal conflict. The estimates for the onset and duration of internal conflict are also shown in Table 10.

First, we examine whether our main estimate for the effect is driven by countries that are more likely to receive some U.S. food aid on average. Recall from the discussion in section 4 that this relevant for interpreting the average effect of aid from our baseline estimates, which are LATE estimates that are identified off of the differential response to food aid between countries that have a higher average tendency to receive some U.S. food aid to countries that have lower tendencies. If conflict in countries that have higher average tendencies for receiving some U.S. food aid are more responsible to food aid, then our estimates will overstate the true average effect of food aid on conflict. One way to explore this is to see if the effect of food aid on conflict is much larger for countries that have higher average tendencies for receiving U.S. food aid. We divide the sample into countries that are above and below the sample median for this tendency (e.g. the median country in our sample receives some U.S. food aid 30% of the sample period), and estimate the interaction effect of being above the sample median and wheat aid. The interaction effect is instrumented by the triple interaction terms of the dummy for being above the sample median, the 24 monthly U.S. temperature and rainfall measures and the average tendency to receive any food aid from the United States. To avoid having the below median group estimates be driven by countries that never receive U.S. food aid, we omit those countries from the sample. Recall from Table 8 that this exclusion does not affect the average effect. In column (2), we find that the interaction effect is positive and statistically significant but very small in magnitude. It implies that for a country that is below the median in average tendency to receive U.S. food aid (but does receive it in at least one of the years in our study period), a 1% increase in food aid increases the incidence of conflict by 0.93 percentage-points. The estimate is not significant at conventional levels. For a country that is above the sample median, a 1% increase in aid increases the incidence of conflict by 0.96 $(0.93 + 0.029)$ percentage-points. The estimate for the interaction effect is significant at the 1% level. The difference is very small, which suggests that our main estimates are not driven by a higher elasticity of conflict with respect to food aid in countries that have a higher average tendency to receive U.S. food aid. We find similarly small estimates for the interaction effects on conflict onset and duration in Panels B and C.

Next, we examine the theory from Besley and Persson (2010) that an exogenous shock on government revenues can only increases conflict in the presence of weak institutions or a non-representative government. We proxy for the presence of the latter with the average tendency for a country to have a civilian government as reported by the Banks Dataset.

This is a yearly index for the extent to which a government is civilian run. There are four categories: civilian, civilian-military, military and other. We create a dummy variable for if a government is entirely civilian and then compute the average of that dummy for each country for the years that the Banks Dataset is available and use this to proxy for the average tendency a country will have a civilian government for our 29 year sample period. The value of zero for this measure means that a country never has a wholly civilian government, and the value of one reflects a country always having a wholly civilian government. According to the predictions of Besley and Persson (2010), the aid should only increase conflict for countries that have a low tendency for having non-civilian governments. Column (3) provides strong evidence to support their theory. The estimated main effect of food aid is positive and the estimated interaction effect is negative. Both are large in magnitude and statistically significant at the 1% levels. The estimates show that for a country that never has a civilian government, a 1% increase in U.S. food aid increases the incidence of internal conflict by 6.6 percentage-points. In contrast, for a country that always has a civilian government, a 1% increase in food aid decreases the incidence of civil conflict by 1.2 percentage-points. If one interprets civilian governments as having higher "quality" political institutions, then this result is consistent with a number of studies which find that aid is more effective when institutions are better (e.g., Burnside and Dollar, 2000;Svensson, 2003). The estimated interaction effects for conflict onset and duration show similar patterns.

As we discussed in section 3, one of the key underlying reasons for why strong or representative governments matter is that they increase the ability for the government to commit to redistribute wealth to opposition groups. Another proxy for the ability to commit is the average incidence of internal conflict. Governments in countries with a higher incidence of civil wars may find it more difficult to redistribute, and both the government and the opposition may find it more difficult to commit to stop fighting. Column (4) shows that the average incidence of fighting is indeed an important factor. The adverse effect of aid on conflict greatly increases in magnitude with average conflict incidence level. For a country on the 10th percentile of the distribution of average incidence of conflict, which experiences no conflict on average, aid has no effect on conflict. However, for a country on the 90th percentile of the distribution, which experiences conflict 94% of the sample period, a 1% increase in U.S. food aid increases the incidence of conflict by 2.6 $(0.47 + 2.60 \times 0.94)$ percentage-points. The estimated interaction effect is statistically significant at the 1% level.[35] Interestingly,

[35]Ethnic fractionalization and polarization may also be correlated with commitment difficulties as higher fractionalization creates more free-riding problems (e.g., Alberto Alesina and Easterly, 1999). Using the measures of fractionalization and polarization from (Alesina and Dollar, 2000) and (Montalvo and Reynal-Querol, 2005), we estimate the interaction effects food aid and each measure in two separate regressions. We find that the interaction terms have positive signs and are large in magnitude. But they are not statistically significant. Therefore, we cannot reject the null that food aid has similar effects across

the estimates in Panels B and C show that the interaction effect of average conflict incidence operates by prolonging conflict. The interaction term has no effect on conflict onset.

For interest, we also examine whether the effect of food aid is different for countries that produce more food domestically and have more agricultural workers, whom are more likely to be affected by changes in the farmgate prices of food caused by food aid. As we discussed in section 3, it is not clear how such changes in relative prices and incomes of agricultural workers would affect conflict in the context of our estimation strategy. Nevertheless, it is interesting to examine. We find no evidence that either the value of domestic agricultural production or the fraction of rural population matters to our estimates. In column (5), we estimate the interaction effect of the percentage of population that is rural and food aid. The estimated interaction effect is small in magnitude and statistically insignificant. Next, we calculate the average GDP from agricultural production as a percentage of total GDP, as reported by the WDI, for each country. Column (6) shows that the interaction effect of this variable and food aid is negative, small in magnitude and statistically insignificant. These results should be interpreted very cautiously because the percentage of rural population and the percentage of agricultural GDP do not directly map into relative agricultural prices. Even if they did and these estimates provide conclusive evidence that changes in relative prices do not affect our estimates, we would not be able to rule out that changes in relative price increases conflict. Rather, our empirical strategy, which compares years in which the relative price of food is high to year when the relative price of food is low, differences out the symmetric effects of increases and decreases ineeai pric(fo)-1(nric(fo-356(conprices.]TJ -11.703 -17.92

under-five child mortality for each country and divide the sample into countries above and below the sample median. Our estimates show that there is no interaction effect between average child mortality and food aid. These estimates are not reported for the sake of brevity. They are available upon request.[37]

7 Conclusion

This study shows that donor-driven government-to-government food aid has the unintended consequence of increasing the incidence of civil wars. Although our analysis uses a reduced form estimation strategy, the results are able to shed light on the mechanisms driving these results. Our results are perfectly in line with theoretical studies which predict that the exogenously increasing government revenues can political competition, which increases the incidence of conflict if a peaceful resolution such as redistribution cannot be found. The additional finding that the adverse effect on increasing conflict is driven by countries that are unlikely to have a civilian government is consistent with the predictions of Besley and Persson (2010) that an exogenous increase in government revenues cause conflict only in the presence of a non-representative government. Similarly, the finding that food aid causes more conflict in countries that have a higher level of average conflict is consistent with the notion that commitment plays an important role in determining conflict if one believes that it is more difficult to commit to a peaceful resolution in a country where there is a history of conflict.

The main competing hypothesis to how food aid can cause conflict is through its effects on the relative prices of food, which can change the relative opportunity costs of agricultural workers for fighting, and thereby affect the incidence of conflict. However, it is very unlikely that our results are driven by this mechanism for several reasons. First, our empirical strategy will mechanically difference out the effects of changes in relative food prices unless if food is innately different from the other goods such that decreasing relative food prices is more likely to cause conflict that decreasing the relative price of other goods. Second, the data show that our instrument has no effect on recipient country food prices, which suggests that most food aid is monetized and does not translate into cheap foods for populations in the recipient government.

Therefore, the most likely explanation for our findings is that the exogenous increase in food aid exogenously increases government revenues, which increases the returns to controlling government revenues. In countries where the government cannot commit to redistributing revenues to the opposition, such as those with non-representative governments or

[37] We also estimated the effect of U.S. wheat aid on the incidence of conflict for each region separately. The estimates are large and positive for countries in Sub-Saharan Africa, Latin America and Caribbean and East Asia and Pacific. The estimates are near zero for countries in Europe and Central Asian and elsewhere. However, all of the estimates are very imprecise. Therefore we do not present them in the paper for brevity's sake. They are available upon request.

a high level conflict historically, this increase in revenues results in increased incidences of civil conflict.

The results of this study confirm some of the most extreme concerns of policy makers over the unintended negative consequences of foreign aid. We show that one of the largest humanitarian disasters to have befallen poor countries in the post-War era – i.e., civil conflict – is partly caused by a policy that is explicitly meant for humanitarian purposes. That said, the fact that the adverse results are driven by countries with high levels of historic conflict or undemocratic political institutions can be seen as an upside because it means that aid is not bad *per se*. Rather, it is the interaction of aid and institutions that cause the negative effects we observe. Therefore, our results can provide some guidance to policy makers as they suggest that one way to avoid the unintended consequences on conflict is to target aid towards countries with civilian governments.

Our study suggests several interesting avenues for future research. One future topic is to develop an empirical strategy to study the effect of relative price and income changes (i.e., income inequality) on conflict. As we have discussed in our study, this is very difficult to do with within-country comparisons. Another interesting and important subject is to estimate the overall welfare impact of foreign aid or food aid. Note that this is beyond the scope of our study since we only examine one outcome. To assess overall welfare, one would need to know the effect of aid on other outcomes such as human capital and institutional development.

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Figure 1: Lagged U.S. Wheat Production and Food Aid

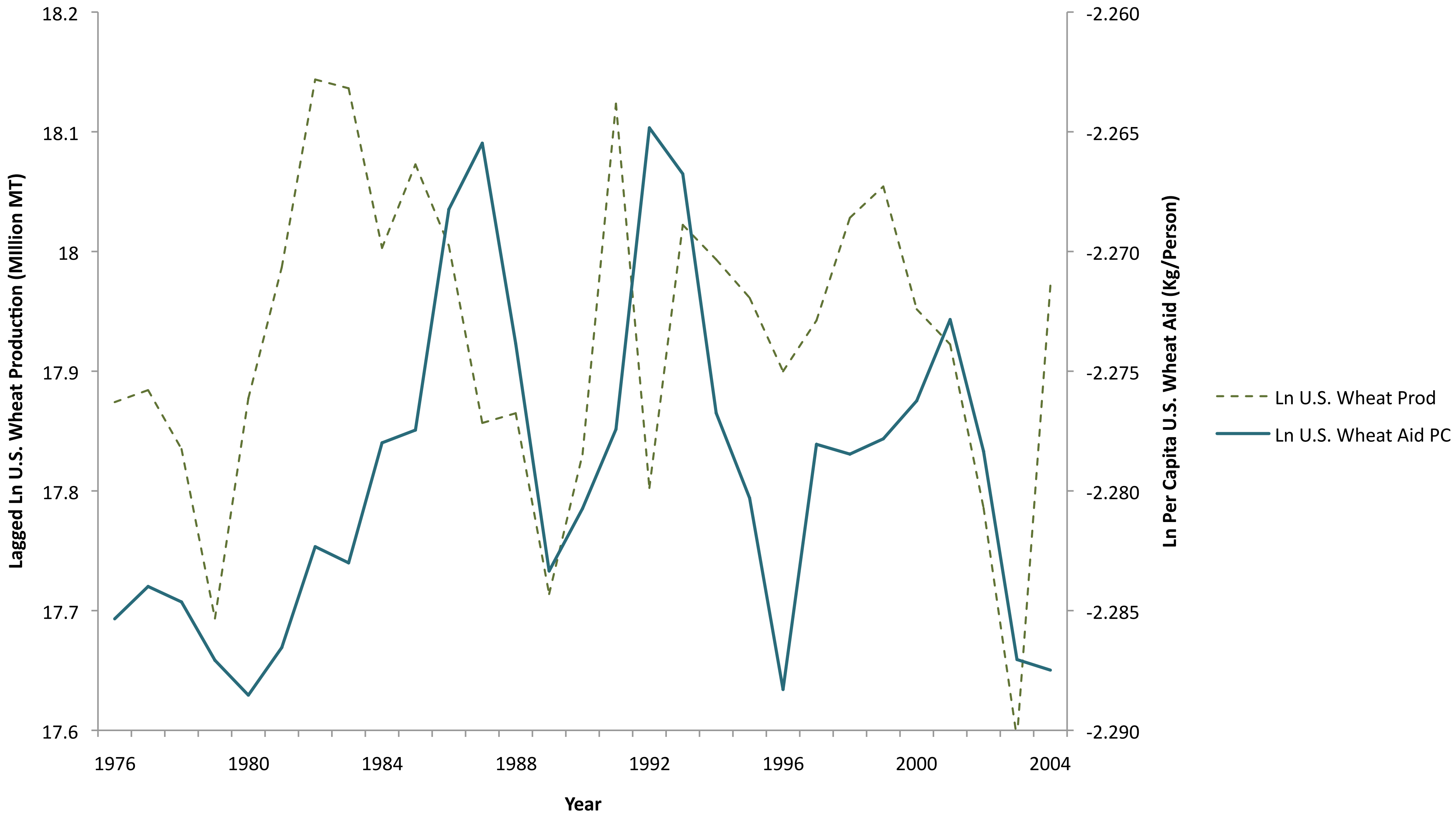

Table 1: Descriptive Statistics

	Obs	Mean	Std. Dev.
Internal Conflict	2922	0.1961	(0.397)
Interstate Conflict	2922	0.0258	(0.159)
International Conflict	2922	0.0723	(0.259)
Any Conflict	2922	0.2640	(0.441)
Missing Values	3477	0.1661	(0.372)
Big War (Battle Deaths >1000)	2922	0.1005	(0.301)
Average Probability of Receiving U.S. Food Aid	2922	0.3788	(0.302)
U.S. Food Aid (MT)	2922	35489	(128261)
U.S. Wheat Aid (MT)	2922	32149	(125007)
U.S. Per Capita Wheat Aid (MT)	2922	0.0026	(0.008)
PC U.S. Food Aid (USD)	2922	1.3211	(2.896)
Title I	2922	0.4790	(1.690)
Title II	2922	0.6410	(2.010)
Title III	2922	0.0394	(0.380)
U.S. Cereal Production (MT)	2922	68900000	(7888874)
Recipient Country Cereal Production (MT)	2922	5921771	(28300000)
GDP PC (USD)	2949	1924	(3392)
Polity 2	2922	-0.3394	(6.885)
Urban Population (%)	2947	41.7903	(20.795)

Notes: Each observation is at the country-year level.

Table 2: OLS and 2SLS Estimates of the Effect of Food Aid on the Incidence of Any Conflict

	Dependent Variable: The Incidence of Any Conflict											
	OLS						2SLS					
	(1)	(2)	(3)	(4)	(5)	(6) Baseline	(7)	(8)	(9)	(10)	(11)	(12) Baseline
Ln PC U.S. Wheat Aid	-0.0192	-0.0124	-0.0356	-0.0412	-0.0640	-0.0718	3.312	4.246	4.290	4.249	4.206	1.718
	(0.220)	(0.219)	(0.224)	(0.232)	(0.228)	(0.106)	(1.478)	(1.999)	(2.031)	(2.006)	(2.049)	(0.894)
Controls												
Year FE	Y	N	N	N	N	N	Y	N	N	N	N	N
Country FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region-Year FE	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Recipient Weather	N	N	Y	Y	Y	Y	N	N	Y	Y	Y	Y
Avg Ln Recipient PC Cereal Prod x Year FE	N	N	N	Y	Y	Y	N	N	N	Y	Y	Y
Avg Ln Recipient PC Cereal Import x Year FE	N	N	N	N	Y	Y	N	N	N	N	Y	Y
Lagged Dependent Variables	N	N	N	N	N	Y	N	N	N	N	N	Y
Observations	2922	2922	2922	2922	2922	2922	2922	2922	2922	2922	2922	2922
R-squared	0.499	0.542	0.547	0.551	0.553	0.694	0.346	0.315	0.316	0.329	0.336	0.656

Standard errors are clustered at the recipient country level. In columns (7)-(12), ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing areas and the average probability that a country receives U.S. food aid during 1976-2004.

Table 3: OLS, 2SLS, LIML and CLR Estimates of the Effect of Food Aid on All Types of Conflict

	Dependent Variable: Incidence of Conflict					
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Conflict	Internal Conflict	Interstate Conflict	International Conflict	Big War	Missing Values
	A. OLS Estimates					
Ln PC U.S. Wheat Aid	-0.0812	-0.113	0.0212	-0.0422	-0.0534	0.00142
	(0.114)	(0.105)	(0.0318)	(0.0550)	(0.110)	(0.0265)
Observations	2922	2922	2922	2922	2922	3477
R-squared	0.696	0.715	0.451	0.612	0.585	0.974
	B. 2SLS Estimates					
Ln PC U.S. Wheat Aid	1.718	1.651	-0.0996	-0.242	1.037	-0.0864
	(0.894)	(0.749)	(0.337)	(0.410)	(0.597)	(0.0641)
LIML	**2.308**	**2.314**	**-0.159**	**-0.297**	**1.822**	**-0.106**
p-value	**0.004**	**0.001**	**0.685**	**0.591**	**0.020**	**0.666**
CLR Intervals	**[.7157695, 4.408811]**	**[.8830873, 4.284202]**	**[-1.08391, .7170105]**	**[-1.464178, .8135777]**	**[.2865599, 3.882701]**	**[-.6916942, .4382381]**
Observations	2922	2922	2922	2922	2922	3477
R-squared	0.656	0.669	0.441	0.609	0.559	0.974

Notes: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. In Panel B, ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing areas and the average probability that a country receives U.S. food aid during 1976-2004. 95% CLR confidence intervals (Moreira, 2003) are presented in square brackets. LIML estimates are presented beneath.

Table 4: Illustrative First Stage Estimates of the Effect of Lagged U.S. Wheat Production on U.S. Wheat Aid

	Dependent Variables							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. U.S. Food Aid							
	Ln PC U.S. Wheat Aid	Ln PC Title I	Ln PC Title II	Ln PC Title III				
Ln Lag U.S. Wheat Production x Avg. U.S. Food Aid Prob	0.0686	0.740	-0.0389	1.034				
	(0.0236)	(0.416)	(0.336)	(0.270)				
Observations	2922	2922	2922	2922				
R-squared	0.525	0.479	0.633	0.306				
	B. Placebo Outcomes							
	Ln PC WFP	Ln PC Non-U.S. Bilateral Aid	Ln PC ODA	Ln Recipient Country Cereal Productio n	Ln Wheat Price in Recipient Country (USD)	Ln PC U.S. Wheat Aid 1 Year Ago	Ln PC U.S. Wheat Aid 2 Years Ago	Ln PC U.S. Wheat Aid 3 Years Ago
Ln Lag U.S. Wheat Production x Avg. U.S. Food Aid Prob	0.167	-0.176	-0.0163	-0.00684	0.227	0.0283	-0.0244	-0.0301
	(0.513)	(0.151)	(0.0112)	(0.0481)	(0.372)	(0.0311)	(0.0354)	(0.0362)
Observations	2819	2797	2789	2922	703	2921	2833	2744
R-squared	0.468	0.802	0.775	0.950	0.674	0.513	0.511	0.514

Notes: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level.

Table 5: The First Stage Estimates of the Effect of Lagged U.S. Weather Conditions on U.S. Wheat Aid

	Dependent Variable: Ln PC U.S. Wheat Aid			
	(1)	(2)	(3)	(4)
Lagged U.S. Wheat Ln Rain Jan x Avg. U.S. Food Aid Prob	-0.000342	-0.00227	-0.00427	-0.00431
	(0.0201)	(0.0207)	(0.0197)	(0.0197)
Lagged U.S. Wheat Ln Rain Feb x Avg. U.S. Food Aid Prob	0.0716	0.0643	0.0667	0.0678
	(0.0177)	(0.0202)	(0.0206)	(0.0217)
Lagged U.S. Wheat Ln Rain Mar x Avg. U.S. Food Aid Prob	0.0598	0.0578	0.0616	0.0634
	(0.0367)	(0.0368)	(0.0369)	(0.0375)
Lagged U.S. Wheat Ln Rain Apr x Avg. U.S. Food Aid Prob	-0.116	-0.0747	-0.0691	-0.0698
	(0.0610)	(0.0542)	(0.0544)	(0.0549)
Lagged U.S. Wheat Ln Rain May x Avg. U.S. Food Aid Prob	0.120	0.113	0.105	0.105
	(0.0818)	(0.0834)	(0.0846)	(0.0848)
Lagged U.S. Wheat Ln Rain Jun x Avg. U.S. Food Aid Prob	-0.0880	-0.0980	-0.0921	-0.0933
	(0.0418)	(0.0441)	(0.0431)	(0.0441)
Lagged U.S. Wheat Ln Rain Jul x Avg. U.S. Food Aid Prob	0.148	0.154	0.151	0.154
	(0.0887)	(0.0826)	(0.0818)	(0.0834)
Lagged U.S. Wheat Ln Rain Aug x Avg. U.S. Food Aid Prob	-0.209	-0.172	-0.170	-0.173
	(0.0627)	(0.0553)	(0.0546)	(0.0564)
Lagged U.S. Wheat Ln Rain Sep x Avg. U.S. Food Aid Prob	-0.0483	-0.0579	-0.0597	-0.0613
	(0.0443)	(0.0432)	(0.0453)	(0.0459)
Lagged U.S. Wheat Ln Rain Oct x Avg. U.S. Food Aid Prob	0.0255	0.0232	0.0211	0.0210
	(0.0214)	(0.0193)	(0.0199)	(0.0199)
Lagged U.S. Wheat Ln Rain Nov x Avg. U.S. Food Aid Prob	0.115	0.0907	0.0887	0.0899
	(0.0432)	(0.0393)	(0.0390)	(0.0396)
Lagged U.S. Wheat Ln Rain Dec x Avg. U.S. Food Aid Prob	-0.0415	-0.0397	-0.0356	-0.0352
	(0.0460)	(0.0450)	(0.0463)	(0.0460)
Lagged U.S. Wheat Ln Temp Jan x Avg. U.S. Food Aid Prob	-0.00897	-0.0152	-0.0146	-0.0145
	(0.0448)	(0.0469)	(0.0473)	(0.0472)
Lagged U.S. Wheat Ln Temp Feb x Avg. U.S. Food Aid Prob	-0.000148	0.00858	0.00879	0.00794
	(0.0499)	(0.0457)	(0.0463)	(0.0460)
Lagged U.S. Wheat Ln Temp Mar x Avg. U.S. Food Aid Prob	-0.0607	-0.0846	-0.0827	-0.0836
	(0.0995)	(0.0957)	(0.0992)	(0.0994)
Lagged U.S. Wheat Ln Temp Apr x Avg. U.S. Food Aid Prob	0.0625	0.00360	0.0236	0.0276
	(0.176)	(0.182)	(0.181)	(0.177)
Lagged U.S. Wheat Ln Temp May x Avg. U.S. Food Aid Prob	0.156	0.236	0.224	0.225
	(0.213)	(0.211)	(0.205)	(0.205)
Lagged U.S. Wheat Ln Temp Jun x Avg. U.S. Food Aid Prob	0.342	0.263	0.240	0.242
	(0.272)	(0.246)	(0.231)	(0.232)
Lagged U.S. Wheat Ln Temp Jul x Avg. U.S. Food Aid Prob	2.188	2.016	2.013	2.046
	(0.884)	(0.832)	(0.838)	(0.866)
Lagged U.S. Wheat Ln Temp Aug x Avg. U.S. Food Aid Prob	-0.919	-1.020	-1.009	-1.017
	(0.437)	(0.472)	(0.475)	(0.481)
Lagged U.S. Wheat Ln Temp Sep x Avg. U.S. Food Aid Prob	-0.270	-0.209	-0.222	-0.229
	(0.153)	(0.130)	(0.132)	(0.131)
Lagged U.S. Wheat Ln Temp Oct x Avg. U.S. Food Aid Prob	0.229	0.150	0.146	0.149
	(0.243)	(0.207)	(0.206)	(0.205)
Lagged U.S. Wheat Ln Temp Nov x Avg. U.S. Food Aid Prob	0.0309	0.0392	0.0222	0.0202
	(0.101)	(0.0980)	(0.0996)	(0.0982)
Lagged U.S. Wheat Ln Temp Dec x Avg. U.S. Food Aid Prob	-0.0421	-0.0651	-0.0635	-0.0642
	(0.0313)	(0.0355)	(0.0353)	(0.0358)
Controls				
Region-Year FE	N	Y	Y	Y
Recipient Country Weather	N	N	Y	Y
Lagged Probability of Any Conflict	N	N	N	Y
Observations	2922	2922	2922	2922
R-squared	0.471	0.531	0.537	0.537
F-Statistic for Joint Significance	3.475	2.616	2.244	2.180

Notes: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level.

Table 6: The 2SLS Estimates of the Effect of U.S. Wheat Aid on Conflict -- Robustness

	(1) Baseline	(2)	(3)	(4)	(5)	(6)
	A. Dependent Variable: Incidence of Internal Conflict					
Ln PC U.S. Wheat Aid	1.301	1.258	2.423	2.297	1.508	2.060
	(0.630)	(0.611)	(0.853)	(0.756)	(0.636)	(0.705)
Controls						
Ln ODA PC	N	Y	N	N	N	Y
Lagged Ln GDP PC	N	N	Y	N	N	Y
Avg Ln GDP PC x Year FE	N	N	N	Y	N	Y
Avg Polity2 x Year FE	N	N	N	N	Y	Y
LIML	**2.892**	**2.527**	**2.971**	**3.356**	**2.616**	**3.341**
p-value	**0.010**	**0.013**	**0.010**	**0.000**	**0.004**	**0.002**
CLR Intervals	**[.6822824, 6.843395]**	**[.5308167, 5.701075]**	**[.716362, 7.077662]**	**[1.567619, 6.228388]**	**[.8064974, 5.369848]**	**[1.253307, 6.91386]**
Observations	2452	2452	2452	2452	2452	2452
R-squared	0.705	0.707	0.656	0.662	0.702	0.685
	B. Dependent Variable: Incidence of Big War					
Ln PC U.S. Wheat Aid	0.194	0.166	0.622	0.617	0.412	0.414
	(0.460)	(0.426)	(0.574)	(0.528)	(0.448)	(0.535)
Controls						
Ln ODA PC	N	Y	N	N	N	Y
Lagged Ln GDP PC	N	N	Y	N	N	Y
Avg Ln GDP PC x Year FE	N	N	N	Y	N	Y
Avg Polity2 x Year FE	N	N	N	N	Y	Y
LIML	**0.572**	**0.3959**	**0.725**	**1.706**	**0.9439**	**1.268**
p-value	**0.648**	**0.6908**	**0.5805**	**0.1518**	**0.3612**	**0.4149**
CLR Intervals	**[-2.285296, 3.972045]**	**[-1.919077, 2.918121]**	**[-2.133147, 4.269327]**	**[-.6687481, 5.349792]**	**[-1.179023, 3.543]**	**[-2.183371, 6.637037]**
Observations	2452	2452	2452	2452	2452	2452
R-squared	0.579	0.579	0.580	0.579	0.584	0.594

All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. Ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing areas and the average probability that a country receives U.S. food aid during 1976-2004.

Table 7: The OLS and 2SLS Estimates of the Effect of U.S. Food Aid on Conflict -- Alternative Standard Error Adjustments

	Dependent Variable: Incidence of Civil Conflict							
	A. OLS				B. 2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standard Error Adjustment	Clustered at Country Level	Newey-West	Clustered at Year Level	Clustered at Year and Country Level	Clustered at Country Level	Newey-West	Clustered at Year Level	Clustered at Year and Country Level
Ln PC U.S. Wheat Aid	-0.110	-0.110	-0.110	-0.110	1.651	1.651	1.651	1.650
	(0.100)	(0.0761)	(0.0576)	(0.0805)	(0.749)	(0.572)	(0.519)	(0.658)
Observations	2922	2922	2922	2922	2922	2922	2922	2922
R-squared	0.715	0.715	0.715	0.715	0.669	0.669	0.669	0.669

Notes: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. In Panel B, Ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing areas and the average probability that a country receives U.S. food aid during 1976-2004.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Omit Outliers (Residuals<-2.5 or Residuals>2.5)	Omit Leverage > (2k+2)/n	Omit Influential Observations (DFITS>2√(k/n))	Omit outliers, high leverage and influential observations	Omit Countries that never receive any U.S. food aid
Ln PC U.S. Wheat Aid	1.651	1.579	1.698	1.548	1.759	1.677
	(0.749)	(0.556)	(0.734)	(0.742)	(0.724)	(0.817)
Observations	2922	2694	2847	2691	2547	2610
R-squared	0.669	0.848	0.654	0.811	0.851	0.679

Notes: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country levB [illegible]m PC U.S Whe atid isn istrumienredwitht theienrrauctionnrrms of: laggedannuealU.S., monthly temperature and

Table 9: 2SLS Estimates of the Effect of U.S. Wheat Aid on Conflict -- Alternative Specifications

	Dependent Variable: Internal Conflict							
	Incidence				Onset		Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Miguel and Satyanath (2005)	Miguel and Satyanath (2005)		Collier and Hoefller (2004), Omit if there is a conflict but it is not the first year of conflict	Fearon and Laitin (2003)		
Ln PC U.S. Wheat Aid	1.651	4.282	3.785	3.695	0.300	0.728	4.119	0.923
	(0.749)	(1.878)	(1.670)	(1.622)	(0.551)	(0.474)	(1.860)	(0.539)
Controls								
Lagged Internal Conflict Incidence	Y	N	N	N	N	Y	N	Y
Country FE x Year	N	N	Y	Y	N	N	N	N
Country FE x Year2	N	N	N	Y	N	N	N	N
Observations	2922	2922	2922	2922	2437	2922	2922	2922
R-squared	0.669	0.274	0.523	0.530	0.159	0.101	0.305	0.841

Notes:All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. Ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing areas and the average probability that a country receives U.S. food aid during 1976-2004.

Table 10: The Effect of Food Aid on Internal Conflict -- Heterogeneous Effects

	Dependent Variables											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A. Incidence of Internal Conflict											
Ln PC U.S. Wheat Aid	1.642	0.927	6.584	0.470	-0.0621	1.651						
	(0.745)	(0.637)	(2.790)	(0.510)	(1.647)	(0.765)						
Ln PC U.S. Wheat Aid x High Avg U.S. Food Aid Prob		0.0286										
		(0.0133)										
Ln PC U.S. Wheat Aid x Avg Prob of Civilian Gov			-7.791									
			(3.640)									
Ln PC U.S. Wheat Aid x Avg Conflict (Any) Incidence				2.595								
				(0.683)								
Ln PC U.S. Wheat Aid x % Rural Population					0.0250							
					(0.0286)							
Ln PC U.S. Wheat Aid x % GDP Agriculture						-0.0254						
						(0.0179)						
Observations	2922	2610	2922	2922	2922	2835						
R-squared	0.667	0.709	0.636	0.696	0.682	0.669						
	B. Conflict Onset						C. Conflict Duration					
Ln PC U.S. Wheat Aid	0.688	0.417	2.728	0.523	0.387	0.917	0.954	0.510	3.856	-0.0536	-0.450	0.734
	(0.462)	(0.410)	(1.420)	(0.426)	(1.169)	(0.473)	(0.539)	(0.509)	(1.968)	(0.387)	(1.023)	(0.484)
Ln PC U.S. Wheat Aid x High Avg U.S. Food Aid Prob		0.0102						0.0184				
		(0.00846)						(0.00941)				
Ln PC U.S. Wheat Aid x Avg Prob of Civilian Gov			-2.899						-4.892			
			(1.789)						(2.595)			
Ln PC U.S. Wheat Aid x Avg Conflict (Any) Incidence				0.145						2.450		
				(0.494)						(0.598)		
Ln PC U.S. Wheat Aid x % Rural Population					0.00602						0.0190	
					(0.0195)						(0.0177)	
Ln PC U.S. Wheat Aid x % GDP Agriculture						-0.0212						-0.00420
						(0.0109)						(0.0140)
Observations	2922	2610	2922	2922	2922	2835	2922	2610	2922	2922	2922	2835
R-squared	0.097	0.125	0.046	0.113	0.094	0.089	0.840	0.857	0.831	0.851	0.849	0.842

All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. Ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing areas and the average probability that a country receives U.S. food aid during 1976-2004. The interaction terms of Ln PC U.S. Wheat Aid are instrumented with the triple interaction terms of lagged annual U.S. monthly temperature and precipitation in wheat producing ares, the average probability that a country receives U.S. food aid and the relevant third variable. In columns (5) and (11), we also control for the main effect of the % of rural population.

Table A1: Mean Monthly Weather Conditions in Wheat Producing Regions of the United States

	Obs.	Mean	Std. Dev.
A. Precipitation (mms)			
January	2922	66.361	21.393
February	2922	62.387	16.218
March	2922	80.876	17.017
April	2922	82.776	18.352
May	2922	98.611	20.497
June	2922	101.071	19.078
July	2922	95.235	17.035
August	2922	85.064	14.254
September	2922	81.202	17.221
October	2922	71.365	21.435
November	2922	74.779	22.231
December	2922	65.981	20.156
B. Temperature (Degrees Celsius)			
January	2922	-0.0004	2.465
February	2922	2.3722	2.421
March	2922	7.1529	1.492
April	2922	12.8374	1.249
May	2922	18.0821	1.216
June	2922	22.4756	0.740
July	2922	24.7333	0.703
August	2922	23.9891	1.009
September	2922	20.1793	0.893
October	2922	13.8398	1.052
November	2922	7.3205	1.676
December	2922	1.9771	2.286

Observations are at the country-year level.

Table A2: The Reduced Form Estimates of the Effects of U.S. Weather Conditions on Conflict

	Dependent variable: Incidence of conflict					
	Any Conflict	Internal Conflict	Interstate Conflict	International Conflict	Big War	Missing Values
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged U.S. Wheat Rain Jan x Food Aid Prob	-0.308	-0.244	-0.105	-0.0173	-0.325	-0.0258
	(0.137)	(0.120)	(0.0540)	(0.0847)	(0.129)	(0.0308)
Lagged U.S. Wheat Rain Feb x Food Aid Prob	-0.170	-0.105	-0.149	-0.105	-0.359	-0.0408
	(0.195)	(0.145)	(0.0785)	(0.0866)	(0.136)	(0.0398)
Lagged U.S. Wheat Rain Mar x Food Aid Prob	0.135	0.0234	0.0618	0.0706	0.368	0.0823
	(0.248)	(0.230)	(0.0587)	(0.123)	(0.178)	(0.0526)
Lagged U.S. Wheat Rain Apr x Food Aid Prob	0.0503	0.154	0.0762	-0.0888	-0.0692	-0.0559
	(0.185)	(0.179)	(0.104)	(0.125)	(0.148)	(0.0496)
Lagged U.S. Wheat Rain May x Food Aid Prob	0.610	0.580	0.0791	-0.128	0.371	-0.00971
	(0.263)	(0.255)	(0.168)	(0.126)	(0.258)	(0.0638)
Lagged U.S. Wheat Rain Jun x Food Aid Prob	-0.346	-0.375	-2.34e-05	0.0389	-0.303	0.00918
	(0.156)	(0.132)	(0.0730)	(0.0844)	(0.185)	(0.0144)
Lagged U.S. Wheat Rain Jul x Food Aid Prob	0.345	0.176	0.0297	0.138	0.483	0.0582
	(0.243)	(0.220)	(0.115)	(0.181)	(0.225)	(0.0425)
Lagged U.S. Wheat Rain Aug x Food Aid Prob	-0.423	-0.227	-0.188	-0.0244	-0.584	-0.0701
	(0.333)	(0.260)	(0.119)	(0.183)	(0.248)	(0.0557)
Lagged U.S. Wheat Rain Sep x Food Aid Prob	-0.0575	-0.162	0.0835	0.0733	-0.0318	0.000424
	(0.180)	(0.166)	(0.0614)	(0.0783)	(0.145)	(0.0456)
Lagged U.S. Wheat Rain Oct x Food Aid Prob	0.188	0.208	0.0672	-0.0617	0.262	0.00752
	(0.103)	(0.109)	(0.0773)	(0.0638)	(0.131)	(0.0244)
Lagged U.S. Wheat Rain Nov x Food Aid Prob	-0.0476	-0.0530	-0.0292	-0.0291	-0.198	-0.00938
	(0.194)	(0.180)	(0.0855)	(0.113)	(0.142)	(0.0381)
Lagged U.S. Wheat Rain Dec x Food Aid Prob	-0.0890	-0.201	0.0151	0.195	0.0981	0.00128
	(0.229)	(0.223)	(0.0964)	(0.107)	(0.187)	(0.0640)
Lagged U.S. Wheat Temp Jan x Food Aid Prob	-0.0275	-0.00965	0.0949	0.0615	0.0682	0.00774
	(0.153)	(0.125)	(0.0643)	(0.0935)	(0.106)	(0.0444)
Lagged U.S. Wheat Temp Feb x Food Aid Prob	-0.0765	-0.0395	-0.139	-0.0712	-0.304	-0.0462
	(0.296)	(0.267)	(0.139)	(0.142)	(0.261)	(0.0621)
Lagged U.S. Wheat Temp Mar x Food Aid Prob	0.511	0.343	0.338	0.279	1.292	0.0931
	(0.510)	(0.422)	(0.252)	(0.272)	(0.441)	(0.106)
Lagged U.S. Wheat Temp Apr x Food Aid Prob	-2.687	-2.883	-0.822	0.904	-2.343	-0.0937
	(0.785)	(0.717)	(0.505)	(0.385)	(0.663)	(0.186)
Lagged U.S. Wheat Temp May x Food Aid Prob	2.359	2.996	0.491	-1.015	2.617	-0.0170
	(1.191)	(1.115)	(0.453)	(0.581)	(0.796)	(0.248)
Lagged U.S. Wheat Temp Jun x Food Aid Prob	2.018	1.046	0.855	-0.271	0.713	-0.0426
	(1.182)	(0.965)	(0.526)	(0.552)	(0.912)	(0.104)
Lagged U.S. Wheat Temp Jul x Food Aid Prob	-0.0205	0.351	-2.428	0.173	-0.304	0.387
	(3.030)	(2.702)	(1.290)	(2.195)	(2.023)	(0.517)
Lagged U.S. Wheat Temp Aug x Food Aid Prob	-3.990	-4.818	-0.850	1.617	-1.377	-0.0573
	(2.117)	(1.728)	(0.784)	(0.927)	(1.415)	(0.396)
Lagged U.S. Wheat Temp Sep x Food Aid Prob	0.0626	0.665	0.352	-0.929	-0.266	0.00192
	(1.082)	(1.070)	(0.524)	(0.608)	(0.967)	(0.238)
Lagged U.S. Wheat Temp Oct x Food Aid Prob	-1.324	-1.756	-0.406	0.945	-0.460	0.152
	(1.175)	(1.244)	(0.447)	(0.715)	(0.711)	(0.349)
Lagged U.S. Wheat Temp Nov x Food Aid Prob	1.246	1.062	0.579	-0.142	1.180	0.0306
	(0.526)	(0.462)	(0.321)	(0.237)	(0.468)	(0.125)
Lagged U.S. Wheat Temp Dec x Food Aid Prob	-0.0839	-0.105	0.0657	0.0593	-0.00618	0.0183
	(0.182)	(0.165)	(0.0553)	(0.0961)	(0.116)	(0.0246)
Observations	2922	2922	2922	2922	2922	3477
R-squared	0.698	0.718	0.448	0.613	0.591	0.975
F-Stat	2.622	2.647	0.794	1.080	2.293	0.725

: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal imports x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. The critical -value for joint significance is 1.76 for the 1% level and 1.52 for the 5% level.

Table A3: The Reduced Form Estimates of the Effect of Lagged U.S. Wheat Production on Conflict

	Dependent variable: The Incidence of Conflict						
	Ln U.S. Wheat Aid PC	Any Conflict	Internal Conflict	Interstate Conflict	International Conflict	Big War	Missing Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Reduced Form Estimates						
Lag Ln U.S. Wheat Prod x Avg Food Aid Prob		0.300	0.344	0.0706	-0.142	0.188	-0.0245
		(0.121)	(0.118)	(0.0666)	(0.0795)	(0.118)	(0.0312)
Observations		2922	2922	2922	2922	2922	3477
	Panel B. 2SLS Estimates -- Instrument with Lagged U.S. Wheat Production x Avg Probability of Receiving U.S. Food Aid						
Ln PC U.S. Wheat Aid		3.967	4.589	0.932	-1.874	2.480	-0.368
		(1.906)	(2.200)	(0.837)	(1.149)	(1.664)	(0.501)
Observations		2922	2922	2922	2922	2922	3477
	Panel C. 1st Stage and RF Estimates						
Lagged Ln U.S. Wheat Production (Predicted) x Avg U.S. Food Aid Prob	0.102	0.310	0.325	-0.0264	-0.0939	0.202	0.00284
	(0.0649)	(0.160)	(0.135)	(0.0586)	(0.0628)	(0.110)	(0.0181)
Observations	2922	2922	2922	2922	2922	2922	3477
R-squared	0.529	0.695	0.716	0.442	0.610	0.585	0.975
	Panel D. 2SLS Estimates -- Instrumente with Lagged (Predicted) U.S. Wheat Prediction x Avg Probability of Receiving U.S. Food Aid						
Ln U.S. Wheat Aid PC		3.035	3.215	-0.261	-0.927	1.956	0.0666
		(2.415)	(2.483)	(0.624)	(0.879)	(1.636)	(0.431)
Observations		2922	2922	2922	2922	2922	3477
R-squared		0.579	0.552	0.435	0.584	0.496	0.974

Notes: All regressions control for baseline controls: lagged conflict, monthly mean temperature and precipitation in arable farming areas, average ln recipient country per capita cereal production x year FE, average ln recipient country per capita cereal import x year FE, region-year fixed effects, and country fixed efffects. Standard errors are clustered at the country level. In Panel B, Ln PC U.S Wheat Aid is instrumented with the interaction terms of lagged annual U.S.wheat production and the average probability that a country receives U.S. food aid during 1976-2004. In Panel D, ln PC U.S Wheat Aid is instrumented with the interaction terms of predicted lag ln U.S. Wheat Production and the average probability that a country gets any food aid from the U.S. U.S. wheat production is predicted by county-season level data on weather conditions and production in the U.S., which is aggregated to the national level for each year.

Table A4: Influential Observations

Country	# Influential Observations	Country	# Influential Observations
Jordan	14	Mongolia	2
Guyana	13	Mexico	2
Egypt	12	Comoros	2
Cambodia	9	Lithuania	2
Suriname	8	Sudan	2
Costa Rica	7	Somalia	2
Morocco	7	Paraguay	1
Afghanistan	7	Panama	1
Thailand	7	Moldova	1
El Salvador	6	Chad	1
Eritrea	6	Malaysia	1
Jamaica	6	Peru	1
Bosnia and Herzegovina	6	Uzbekistan	1
Bolivia	5	Dominican Republic	1
Senegal	4	Croatia	1
Georgia	4	Iraq	1
Zimbabwe	4	Latvia	1
Sri Lanka	4	Yemen	1
Tunisia	4	Trinidad and Tobago	1
Bangladesh	4	Algeria	1
Mozambique	3	Turkmenistan	1
Liberia	3	Burundi	1
Armenia	3	Guatemala	1
Djibouti	3	Kenya	1
Kyrgyzstan	3	Sierra Leone	1
Nepal	3	Burkina Faso	1
Haiti	3	Belize	1
Nicaragua	3	Turkey	1
Lebanon	3	Cote d'Ivoire	1
Azerbaijan	3	Angola	1
Tajikistan	3	Portugal	1
Israel	2	Cape Verde	1
Mali	2	Cameroon	1
Togo	2	Nigeria	1
Cyprus	2	Mauritius	1
Russian Federation	2	Equatorial Guinea	1
Niger	2	Estonia	1
Ghana	2	Indonesia	1
Albania	2	Congo	1

Influential observations are defined as those with DFITS>$2\sqrt{(k/n)}$ from either the baseline first stage or reduced form estimations.