

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

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Abstract

This paper examines the effect of U.S. food aid on conflict in recipient countries. To establish a causal relationship, we exploit time variation in food aid caused by fluctuations in U.S. wheat production together with cross-sectional variation in a country's tendency to receive any food aid from the United States. Our estimates show that an increase in U.S. food aid increases the incidence, onset and duration of civil conflicts in recipient countries. Our results suggest that the effects are larger for smaller scale civil conflicts. No effect is found on interstate warfare.

Keywords: Civil War, Conflict, Food Aid, Humanitarian Assistance.

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“We are unable to determine whether our aid helps or hinders one or more parties to the conflict... However, it is clear that the losses – particularly looted assets – constitutes a serious barrier to the efficient and effective provision of assistance, and can contribute to the war economy. This raises a serious challenge for the humanitarian community: can humanitarians be accused of fueling or prolonging the conflict in these two countries?” — Humanitarian Affairs Advisors for Medecins Sans Frontiers (MSF) Amsterdam writing about Chad and Darfur (Polman and Waters, 2010, p. 105).

1 Introduction

Humanitarian aid in general, and food aid in particular, is one of the key policy tools used by the international community to help alleviate hunger and suffering in the developing world.¹ However, the efficacy of humanitarian aid, and food aid in particular, has received increasing criticism, especially in the context of conflict-prone regions. Aid workers, human rights observers and journalists have accused humanitarian aid of being not only ineffective, but of actually *promoting* conflict (e.g., Anderson, 1999; de Waal, 1997 and Polman and Waters, 2010). LeRiche (2004) argues that “despite being widely known, the utilization of the humanitarian aid system as a logistical support system for war is one of the most overlooked constituent tactics of modern warfare. As such, it has not received adequate research or public attention”.

The qualitative evidence points to *aid stealing* as an important mechanism. Humanitarian aid is particularly easy for armed factions and opposition groups to appropriate since it is physically transported over long distances, often through territories only weakly controlled by the recipient government. Reports indicate that up to eighty percent of aid can be stolen en route (Polman and Waters, 2010, p. 121). Even when aid reaches its intended recipients, it can still be appropriated or “taxed” by armed groups, against whom the recipients are typically powerless. This misappropriated aid is then used to fund conflict. Much to the concern of aid watchers, such accounts are not isolated but have been documented in numerous contexts: e.g., Afghanistan (2001 - present), Western Sahara (1950s), the Democratic Republic of Congo (1971 - present), Ethiopia (1974-91), Eritrea (1972-74, 80-81), Israel (1950s), Iraq (1992), Liberia (1993-2003), Rwanda (1994-2008), Sierra Leone (1991-2002), Somalia (1991-present), Sri Lanka (1983-2009), Sudan (1983-2005) and the former Yugoslavia (1992-95) (Polman and Waters, 2010).

¹Food aid is the main component of humanitarian aid and is also an important part of economic aid generally. For example, according to data from USAID, among the countries and years in our sample (non-OECD countries between 1972 and 2006), 29.5 percent of U.S. economic aid was food aid.

Although the grave nature of these accounts warrant the immediate attention of aid donors, it is difficult to redesign aid policy without systematic evidence of the impact of existing policies. In particular, a question of first-order importance is whether the qualitative accounts are representative of the *average* effect of humanitarian aid or whether they capture the effect within a set of extreme cases. Unfortunately, our current understanding remains limited (Pillai, 2000, p. 197). Our study attempts to fill this gap by providing a rigorous estimate of the causal impact of food aid, the most important component of humanitarian aid, on conflicts in recipient countries. It aims to answer several important questions: What is the average effect of food aid on conflict? What types of conflicts are affected? Are the effects more or less prominent in certain contexts?

The main difficulty in empirically estimating the impact of food aid on conflict arises from reverse causality and joint determination. Furthermore, the direction of the bias is difficult to predict *ex ante*. On the one hand, the fact that food aid and conflict are more likely to be present during times of political and economic crises suggests that the OLS estimates of the impact of food aid on conflict may be biased upwards. On the other hand, the possibility that donor countries condition food aid on characteristics correlated with low levels of conflict may cause OLS estimates to be biased downwards.²

To overcome these issues, we develop a novel identification strategy for estimating the impact of U.S. food aid on conflict. Our analysis exploits two sources of variation. First, we exploit plausibly exogenous time-variation in U.S. wheat production, which is mainly driven by weather shocks.³ Due to U.S. price support policies for American wheat producers, the U.S. government accumulates reserves during high production years. In the following year, much of the surplus is then shipped to developing countries as food aid. In the data, we observe that U.S. wheat production is positively correlated with U.S. food aid shipments in the following year. Second, we exploit cross-sectional variation in a country's likelihood of being a U.S. food aid recipient, which we measure as the proportion of years that a country receives a positive amount of U.S. food aid during the 35 years of our study, 1972-2006. By also using this cross-sectional variation, we are able to control for time-varying factors with the inclusion of region-year fixed effects. Thus, we instrument for the amount of food aid received by a country in a year with the interaction of last year's U.S. wheat production and the likelihood that a country was a U.S. food aid recipient. Our baseline estimates

²In addition, it is possible that conflict and food aid receipts are both outcomes of a third omitted factor such as the strategic objectives of the aid donor. For example, during the 1960s and 1970s, the U.S. government supported South Vietnam's war against North Vietnam by giving the South Vietnamese government enormous amounts of food aid, which could be monetized and used to fund the war (Kodras, 1993). 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.

³In a previous version of the paper, we use weather conditions in wheat producing regions of the U.S. directly to instrument for U.S. food aid. In this version, we use U.S. production as an instrument because it is easier to interpret the first stage and reduced form coefficients. The resulting 2SLS estimates for the two methods are very similar. For brevity, we do not report the earlier estimates in this version of the paper. They are available upon request.

also include country fixed effects, which control for all time-invariant differences between countries, including the main effect of the likelihood that a country was a U.S. food aid recipient; as well as region-year fixed effects, which control for changes over time that affect countries within each region similarly. Our identification strategy relies on the interaction term being exogenous conditional on these fixed effects (and the set of additional baseline controls that we describe below).

Our strategy follows the same logic as a *difference-in-differences* estimator. For example, the reduced form estimate compares the difference in conflict between years following high U.S. wheat production and years following low U.S. wheat production between countries that regularly receive U.S. food aid and countries that rarely receive U.S. food aid.

There are a few potential concerns related to the excludability of our instrument. First, the main driving force of the time variation in U.S. wheat production, U.S. weather conditions, may be correlated with weather conditions in aid-recipient countries, and thereby influence conflict through channels other than U.S. food aid. To address this, we directly control for the weather conditions of recipient countries. Second, U.S. production shocks are potentially correlated with global wheat prices, which may affect conflict in recipient countries. In practice, this is not a serious problem because U.S. government price stabilization policies cause U.S. wheat prices to be very stable over time. The data show that global wheat prices are essentially uncorrelated with U.S. wheat production in the time-series. Nevertheless, we cautiously control for region-specific time fixed effects to capture region-specific changes in wheat prices over time. We also control for the possibility that changes in global wheat prices may affect recipient countries differently depending on the extent to which they are producers or importers of cereals.

Our main outcomes of interest are measures of the incidence of conflict with more than 25 combat deaths in a country and year. We separately examine the incidence of all conflicts, civil conflicts and inter-state conflicts. The analysis uses an annual panel of non-OECD countries between 1972 and 2006. The OLS estimates of the impact of U.S. food aid on conflict are negative, small in magnitude and statistically insignificant for all forms of conflict. By contrast, the 2SLS estimates identify a large, positive and statistically significant impact of U.S. food aid provision on the incidence of civil conflict, but no effect on the incidence of inter-state conflict. The estimates imply that increasing U.S. food aid by 1,000 metric tons (MT) increases the incidence of civil conflict by 0.38 percentage-points. For a hypothetical country that receives the sample mean of U.S. food aid - approximately 27,600 MT - and experiences the mean incidence of conflict - 17.6 percent, the estimates imply that increasing food aid by ten percent increases the incidence of conflict by approximately 1.14 percentage-points. This increase is equal to six percent of the mean of conflict.

To better understand how food aid can affect conflict, we undertake two additional tests. First, we examine which types of conflict are most impacted by U.S. food aid by also estimating the impact of food aid on the incidence of large-scale armed conflicts, defined as

conflicts involving 1,000 or more combat deaths. We find that the effect of U.S. food aid is much smaller for large-scale conflicts than for the baseline measure, which includes all forms of civil conflict, both small and large. This finding is consistent with descriptive accounts of humanitarian aid being appropriated by small-scale rebel groups or refugee warriors to fund their military activities. Second, we decompose the incidence estimates by separately examining civil conflict onset and duration. We find that U.S. food aid increases the incidence of civil conflicts by increasing both the probability of their onset as well as their duration.

A potential caveat for interpretation comes from the possibility that U.S. food aid may crowd out food aid from other countries or other forms of aid. Hence, U.S. food aid could increase conflict due to a *reduction* in these other forms of aid, rather than an increase in U.S. food aid. We explore this alternative interpretation, which has drastically different policy implications, by directly examining the effects of U.S. food aid on: (i) other forms of aid from the United States, (ii) food aid from other countries, and (iii) total official development assistance (ODA) from all countries. We find no evidence that these other types of foreign aid respond to U.S. food aid and therefore conclude that it is unlikely that our results are due to aid crowd-out.

Having provided estimates of the average impact of U.S. food aid on conflict, we then turn to an examination of whether the impacts are more or less prominent in certain contexts. We find suggestive evidence that food aid causes fewer conflicts in countries with low levels of ethnic fractionalization and with well-developed transportation infrastructure, measured by road density. The latter result is particularly interesting because it is consistent with numerous accounts of food aid being stolen by armed factions during transit. The results are consistent with it being easier for aid deliveries to circumvent road blocks and other attempts at theft when there are more alternative routes for reaching the targeted population.

Our findings contribute to several literatures. First, they add to the debate about the general impacts of foreign aid.⁴ Studies in this literature face similar identification challenges as our study.⁵ Our use of donor-country shocks to instrument for aid provision follows a similar logic as Werker, Ahmed and Cohen (2009) and Ahmed (2010), who exploit oil price shocks and the fact that wealthy oil-rich donors tended to favor muslim nations to estimate

⁴See for example, Burnside and Dollar (2000), Easterly (2003), and Svensson (1999). For studies focusing specifically on the impacts of food aid, see Lavy (1992), Pedersen (1996), Kirwan and McMillan (2007), Levinsohn and McMillan (2007), Quisumbing (2003) and Yamano, Alderman and Christiaensen (2005).

⁵The benefits of foreign aid for recipient countries is a much studied and controversial subject. Prominent economists, such as Jeffrey Sachs (2006), present many case studies of the success of aid, and Stern (1974) argues that rich countries are morally obligated to provide aid to assist in the development of poor ones. However, critics such as Bauer (1975) argue that foreign aid not only does not help, but hurts development. Recent academic studies are more nuanced and focus on the question of whether foreign aid can be effective in good policy environments. On one hand, studies such as Burnside and Dollar (2000) and Svensson (1999) find that aid can be effective in good policy environments. On the other hand, others, such as Easterly (2003), argue that there is no evidence for the effectiveness of foreign aid. Other studies that question the effectiveness of aid include Boone (1996) and Easterly, Levine and Roodman (2004). The main challenge for these studies has been in estimating a causal impact of aid.

the impact of foreign aid on various macro-economic outcomes. Although they do not examine conflict as an outcome, our finding that aid can have adverse effects is broadly consistent with their finding that aid has no effect on economic growth (Werker, Ahmed and Cohen, 2009) or that aid reduces institutional quality (Ahmed, 2010). Our finding that aid is partly determined by changes in U.S. domestic production links our study to existing studies that find that aid is often determined by the strategic or economic needs of donor countries (e.g., Ball and Johnson, 1996; Alesina and Dollar, 2000; Kuziemko and Werker, 2006; and Nunn and Qian, 2010).

A small number of papers examine the impact of foreign aid receipts on civil conflict. Our finding that aid can increase conflict is consistent with the recent study by Besley and Persson (2011), which finds that increased official development assistance (ODA) associated with UN Security Council membership during the Cold War increases the incidence of civil wars in recipient countries. It is also consistent with Crost, Felter and Johnston (2011), which finds that across municipalities within the Philipinees, eligibility for a large World Bank funded foreign aid program is positively correlated with conflict causalities. Our study differs from these works in its focus on food aid, which may have different effects on conflict relative to foreign aid generally.⁶ In our focus on a specific form of aid, our study is similar to the recent study by Dube and Naidu (2010), which finds a positive relationship between U.S. military aid and paramilitary violence across regions within Columbia.

Finally, our study is closely related to a large empirical literature examining the determinants of conflict, which is reviewed by Blattman and Miguel (2010). In particular, in establishing causality, our study is similar to recent studies such as Miguel, Satyanath and Sergenti (2004), Dube and Vargas (2009), and Bruckner and Ciccone (2010) that develop clever strategies to identify the causal impact of income shocks on civil conflict. Our results complement these studies by providing additional evidence on the determinants of civil conflict.⁷

The paper is organized as follows. Section 2 discusses U.S. food aid policy and how food aid can affect conflict. Section 3 describes our identification strategy and estimating equations. Section 4 describes the data. Section 5 presents our 2SLS estimates. Section 6 offers concluding remarks.

⁶Not all studies of the impacts of foreign aid find that aid increases conflict. (Collier and Hoeffler, 2002) find that total ODA has no effect on conflict globally, while (de Ree and Nillesen, 2009) find that within total ODA reduces conflict. The difference in findings across all studies examining foreign aid and conflict is most likely due to different empirical strategies. In addition, the findings in our study may also differ from the findings of (Collier and Hoeffler, 2002) and (de Ree and Nillesen, 2009) because we examine a specific type of aid rather than total ODA.

⁷The literature has thus far tended to focus on determinants 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., Ahmed, 2010; Besley and Persson, 2011), propaganda (e.g., Yanagizawa-Drott, 2010), foreign aid (e.g., de Ree and Nillesen, 2009; Dube and Naidu, 2010), trade (e.g. Martin, Mayer and Thoenig, 2008) and commodity prices (e.g., Collier and Hoeffler, 2004; Ross, 2004; Guidolin and Ferrara, 2007; Bruckner and Ciccone, 2010; Bazzi and Blattman, 2011).

2 Background

2.1 Food Aid and Conflict

U.S. food aid is delivered to recipient countries in the form of food that is typically transported to needy populations through semi-permanent food-aid centers or refugee camps. Aid watchers most frequently point to theft by armed factions on the ground as the primary mechanism through which food aid and other types of humanitarian aid promote conflict.

Food aid is regularly appropriated by militia groups or other armed factions during its distribution. Unlike many other types of foreign aid, food aid and humanitarian aid need to be transported across territories that the recipient country government often has little control over. This makes food aid a particularly attractive target for armed factions, especially in countries where the ruling government has limited control outside of the capital. Armed factions can set up road blocks and “tax” aid agencies for safe passage. For example, accounts from Somalia in the early 1990s indicate that as much as eighty percent of food aid shipments were either looted, stolen or used as protection money. Stolen aid was then traded for arms in neighboring Ethiopia (Perlez, 1992). In Afghanistan, aid organizations in the province of Uruzgan gave over one-third of their food aid and agricultural support to the Taliban. In Sri Lanka, up to 25 percent of the total value of aid was paid to the Tamil Tigers by Dutch aid workers. In the former Yugoslavia, the UNHCR gave thirty percent of the total value of aid to Serbian armed forces, and then more bribes to Croatian forces to pass the respective road blocks in order to reach Bosnia (Polman and Waters, 2010, pp. 96-104).

The amount of theft can even exceed the value of the food, since convoy vehicles and other equipment are also stolen. In 2008, MSF Holland, an international aid organization working in Chad and Darfur, noted the strategic importance of these goods, writing that these “vehicles and communications equipment have a value beyond their monetary worth for armed actors, increasing their capacity to wage war” (Polman and Waters, 2010, p. 105).

One of the most well-established cases of humanitarian aid strengthening rebel groups within a country occurred in Nigeria during the Nigeria-Biafra civil conflict of the late 1960s. The rebel leader Odumegwu Ojukwu only allowed aid to enter the rebel controlled region of Biafra if it was shipped on his planes. He charged aid agencies for the use of his airplanes and filled the remaining space with arms and other military equipment. The shipments of humanitarian aid allowed Ojukwu to circumvent the siege that had been placed on Biafra by the Nigerian government. The food aid also allowed Ojukwu to feed his army, the members of which officially qualified for international humanitarian relief because together with the rest of the population, they were malnourished. Many suggest that the shipment of humanitarian aid resulted in the Biafran civil conflict lasting years longer than it would have otherwise (Polman and Waters, 2010, pp. 115-119).

Aid is not only stolen by rebel militia, but is also appropriated by the government, its

military and government supporters. In Rwanda, in the early 1990s, government stealing of food aid was so problematic that aid shipments were cancelled on several occasions (Uvin, 1998, p. 90). Governments that receive aid often target it to specific populations, excluding opposition groups or populations in potentially rebellious regions. This has been noted to increase hostilities and promote conflict. In Zimbabwe in 2003, the U.S.-based organization, *Human Rights Watch*, released a report documenting examples of residents being forced to display ZANU-PF Party membership cards before being given government food aid (Thurow and Kilman, 2009, p. 206). In eastern Zaire, the leaders of the Hema ethnic group permitted the arrival of international aid organizations only if they agreed to give nothing to their enemies, the Lendu.⁸ Polman and Waters (2010) describe this phenomenon as common, writing that “Aid has become a permanent feature of military strategy. Belligerents see to it that the enemy is given as little as possible while they themselves get hold of as much as they can” (Polman and Waters, 2010, p. 10).

Humanitarian aid workers are aware of the threat of aid theft and have developed a number of strategies for minimizing the amount of theft en route.⁹ However, aid can still fuel conflict even if it is successfully delivered to the intended populations. This commonly occurs because the recipient populations either include members of rebel or militia groups, or the recipients are “taxed” after receiving the aid. The most well-known example of this occurred in the Hutu refugee camps near Goma following the Rwandan Genocide in 1994. Hutu extremist leaders taxed Hutu civilians in the camps, and transferred the appropriated aid to their militia. The aid (and physical protection) provided by refugee camps allowed the Hutu extremists to regroup and rebuild their army. The Hutu militia were then able to carry out raids into Rwanda, which contributed to both the First and Second Congo Wars (Terry, 2002, ch. 5; Lischer, 2005, ch. 4).

For completeness, we discuss two additional channels through which food aid can, in theory, affect conflict. First, because food aid is fungible, it can increase government revenues, which can promote political competition and conflict through the mechanisms of many standard conflict models.¹⁰ In practice, this is unlikely since the value of food aid is very small relative to total government revenues. For example, the average ratio of the value of U.S. food aid relative to total GDP among observations in our sample ranges from 0.0016 to 0.0033, with the difference depending on whether transportation costs are included in the food aid figures.¹¹ Even among the observations with greater than median U.S. food aid recipients, the average ratio only ranges from 0.0011 to 0.0066. Second, one may be

⁸In 2001, six aid workers who gave aid to the Lendu were murdered (Polman, 2009, p. 98).

⁹See Anderson (1999) for a summary of strategies used by aid workers to minimize aid theft and diversion.

¹⁰For example, see the works of Haavelmo (1954), Hirschleifer (1989), Garfinkel (1990), Grossman (1991), Skaperdas (1992), Grossman (1999), Collier and Hoeffler (2002) and Besley and Persson (2011).

¹¹The figure are constructed by calculating for each observation in the sample (i.e., non-OECD countries from 1972 to 2006) the total value of U.S. food aid received divided by total GDP (both measured in nominal U.S. dollars). We then calculate the average among the observations. The lower estimate assumes that 47 percent of the reported value of aid is the actual value of the commodities (see the discussion at the end of Section 2.2). The higher estimate assumes that the full value consists of the value of the commodities.

concerned that food aid causes conflict by increasing the supply of food, therefore reducing the price of food in recipient countries (Pedersen, 1996; Kirwan and McMillan, 2007). The effect this has on conflict is *a priori* unclear. On the one hand, the price change decreases agricultural incomes, which may reduce the opportunity cost of fighting and increase conflict (Miguel, Satyanath and Sergenti, 2004). On the other hand, the decline in prices increases the real income of citizens that are net consumers of these foods, i.e. non-agricultural workers. Through the same mechanism, this may increase their opportunity cost of fighting and decrease the incidence of civil conflict. The net impact of these two contradicting forces is ambiguous and depends on the relative importance and size of both groups, as well as other factors determining whether the decline in agricultural incomes has a more adverse impact than the increase in non-agricultural incomes.

Our discussion has focused on channels through which humanitarian aid can increase conflict. It is important to recognize that there are many channels through which aid can also decrease conflict. The most obvious channel is through increased economic development. Alternatively, if conflict arises because of resource constraints, aid could reduce conflict by reducing those constraints. Our study, which estimates the *average causal effect* of food aid on conflict, captures the net effect of the positive and negative effects of food aid on conflict.

2.2 The Determinants of U.S. Food Aid

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. The United States is the largest donor of food aid in the world, accounting for approximately 58 percent of global food aid in 1990 and 64 percent in 2000 (Barrett and Maxwell, 2005, p. 12).¹²

U.S. food aid flows to poor countries through several mechanisms, the most important being Public Law 480 (PL 480), which was established under the Eisenhower administration in 1954 and was later renamed the *Food for Peace Program* in 1962 by President John F. Kennedy. All forms of food aid are procured by the United States Department of Agriculture (USDA) and administered by either the USDA or the U.S. Agency for International Development (USAID).¹³

¹²It is followed by the European Union countries, which in 2000, together accounted for approximately seventeen percent of food aid flows. The other major donors are Japan (six percent), Australia (three percent) and Canada (three percent) (Barrett and Maxwell, 2005, pp. 10-13).

¹³U.S. food aid falls into four broad categories: Type I, Type II, Type III and other. Type I is administered by the USDA and consists primarily of concessional loans with some grants for commodity exports. Titles II and III programs are administered by USAID. Title II programs provide donations to meet humanitarian and development needs. These are typically channeled through either recipient governments, NGOs or multilateral organizations like the World Food Programme (WFP). Title III aid is sold to developing countries which is then typically monetized to generate funds for development objectives. The final category includes a number of smaller programs including Food for Progress, Section 416(b), Bill Emerson Humanitarian Trust, and International Food for Education and Child Nutrition, all administered by the USDA (Barrett and

Although U.S. food aid is comprised of many different types of food, wheat constitutes the largest proportion of aid. During the period of our study, 1972-2006, 63 percent (measured by weight) of all cereal food aid shipments were wheat, and 58 percent of all food aid shipments (cereals and non-cereals) was wheat. Given the quantitative importance of wheat as a source of U.S. food aid, our study focuses on this crop. The advantage of focusing on a specific crop, rather than examining aggregate aid, is that we are better able to identify the relationship between production shocks and aid shipments.

Food aid is broadly determined by need since fewer developed countries are the primary recipients of aid. However, on a year-to-year basis, food aid is, to a large extent, determined by U.S. production (see e.g., Nunn and Qian, 2010). The USDA accumulates wheat in high production years to stabilize prices for American farmers. This accumulated wheat is stored and then shipped as food aid to poor countries. Given the time lag between harvest, storage, and shipment, wheat harvested in year t , tends to arrive in recipient countries in the next calendar year, $t + 1$. Therefore, in the empirical analysis, we characterize food aid received in year t as a function of U.S. production in year $t - 1$.

The authorization and administration of food aid programs under PL 480, which decides the amount of food aid shipments to countries each year, is the outcome of a complicated set of decisions made by a large number of government agencies (Ball and Johnson, 1996). In the House of Representatives, food aid legislation is determined by the House Agricultural Committee, the Foreign Affairs Committee, and the Select Committee on Hunger. The USDA determines what commodities are available and in what quantities. The Treasury Department sets the terms of loans, the Office of Management and Budget determines if funding is available, the Department of State assesses the political consequences, and USAID implements the programs in the field. Any inter-agency disagreements that may occur are mediated by the Food Aid Subcommittee of the Developing Coordinating Committee (Ruttan, 1993, p. 2). Our empirical analysis assumes that the decision making process results in accumulated wheat reserves being regularly drawn down through increased shipments of food aid that tend to be disproportionately greater for regular food aid recipients than for irregular recipients. We will verify this assumption with the data.

A significant proportion of the reported value of food aid consists of transportation costs. Using data from 1999-2000, Barrett and Maxwell (2005, pp. 166-168) estimate that only 47 percent of the total value of food aid is the actual value of the commodity itself. The other 53 percent is accounted for by transportation costs. Part of the reason for the high shipping costs is that U.S. legislation requires that at least 75 percent of food aid be shipped on U.S. flagged cargo ships that charge inflated rates.¹⁴ Because we are interested in measuring the shipment of food aid to recipient countries exclusive of shipping costs, our analysis uses the

Maxwell, 2005, pp. 20-26). Note that our data on aid receipts only report total U.S. food aid. Therefore, our analysis does not decompose food aid into different categories.

¹⁴Barrett and Maxwell (2005, pp. 166-168) estimate that forty percent of the shipping costs is explained by this shipping premium.

quantity of food aid rather than its reported value. The former does not include the costs of shipment while the latter does.

3 Empirical Strategy

Our main analysis examines the relationship between the incidence of conflict and food aid receipts from the United States, which we characterize with the following equation:

$$C_{irt} = \beta F_{irt} + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_{rt} + \delta_i + \varepsilon_{irt}, \quad (1)$$

where i denotes countries, r denotes geographic regions and t denotes years. The sample is a panel of 134 non-OECD countries for the years 1972-2006. The sample period is limited by the availability of food aid and conflict data.¹⁵ The dependent variable, C_{irt} , is an indicator variable that equals one if there is conflict in country i and year t . The main explanatory variable is the amount of U.S. food aid a country receives, denoted F_{irt} . The specification includes country fixed effects, δ_i , that control for time-invariant differences across countries, and region-year fixed effects, φ_{rt} , that control for changes over time that affect countries within a region similarly. The region classification that we use is taken from the World Bank and consists of the following groups: South Asia, East Asia and Pacific, Europe and Central Asia, Latin America and Caribbean, Middle East and North Africa, and Sub-Saharan Africa. \mathbf{X}_{irt} is a vector of country-year covariates that we motivate and discuss in detail as we present the results.

Note that following several studies on conflict, such as Miguel, Satyanath and Sergenti (2004), our main estimates do not control for a lagged dependent variable to avoid the Nickell (1981) bias. However, as we show in Section 5.4, we obtain qualitatively similar estimates when we control for a one-year lag of the dependent variable.

Assuming casual identification, β is the effect of an additional unit of U.S. food aid on the incidence of conflict. A positive coefficient, $\hat{\beta} > 0$, indicates that, on average, an increase in the provision of U.S. food aid increases the incidence of conflict in the recipient country.

Interpreting the OLS estimates of the effect of U.S. food aid on conflict faces the difficulties of reverse causality and joint determination, discussed in the introduction of the paper. To address these difficulties, we exploit two sources of variation. First, we use time variation in U.S. food aid shipments arising from changes in U.S. wheat production. When U.S. production is high, USDA price support policies generate an accumulation of reserves, which increases the amount of food aid shipped to recipient countries in the subsequent year. We argue that this source of variation is exogenous to factors that influence conflict in recipient countries beyond U.S. food aid (conditional on the baseline controls that are described below). We also exploit a second source of cross-sectional variation from a coun-

¹⁵The panel is not balanced as the number of countries in the world increase over time. The results are similar if we restrict the sample to a balanced panel.

try’s tendency to receive food aid from the U.S., measured by the fraction of years between 1972 and 2006 that a country is a recipient of U.S. food aid. This is motivated by the fact, documented in Section 4, that regular aid recipients experienced greater increases in food aid shipments following U.S. production booms.

The instrument for U.S. food aid is therefore the interaction between lagged U.S. production and the tendency for a country to receive any U.S. food aid. All of our estimates control for country fixed effects and region-year fixed effects. Our instrument, which is constructed by interacting an arguably exogenous term (lagged U.S. wheat production) with one that is potentially endogenous (the likelihood of a country to be a U.S. food aid recipient), can be interpreted as exogenous since we directly control for the main effect of the endogenous variable (i.e., it is absorbed by the region-year fixed effects).¹⁶

An alternative strategy is to use (uninteracted) lagged U.S. wheat production to instrument for U.S. wheat aid. The drawback of this strategy is that we are then unable to also control for region-year fixed effects (or even year fixed effects). Given the potential importance of shifting U.S. foreign policies across regions, *ex ante* this is a significant drawback. *Ex post*, we find that it makes little difference whether we instrument using only time variation from U.S. wheat production (and control for country-specific linear time trends) or using the interacted instrument (and control for region-year fixed effects). For the sake of rigor, our main results use the interacted instrument. The estimate with the uninteracted instrument is reported in section 5.4.

The first stage (and reduced form) of our 2SLS strategy is similar in spirit to a *difference-in-differences* (DD) estimation strategy, where we compare U.S. food aid receipts (and conflict) between countries that frequently receive U.S. food aid to countries that rarely receive U.S. food aid, in years after the U.S. experiences high levels of wheat production relative to years following lower production levels. The main difference between our strategy and a DD strategy is that the treatment in our study is continuous, allowing us to use all of the variation in the treatment variable for our estimates.

Our first-stage equation is as follows:

$$F_{irt} = \alpha (P_{t-1} \times \overline{D}_{ir}) + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_{rt} + \delta_i + \varepsilon_{irt}, \quad (2)$$

where the amount of U.S. food aid received by country i , in region r , during year t is denoted as F_{irt} . P_{t-1} is the amount of U.S. wheat production from the previous year. The variable $\overline{D}_{ir} = \frac{1}{35} \sum_{t=1972}^{2006} D_{irt}$, where D_{irt} is an indicator variable that takes a value of one if country i receives any U.S. food aid in year t . Thus, the amount of U.S. food aid receipt is a function of the interaction of lag U.S wheat production and the tendency for a country to be a U.S. food aid recipient during the period of our study, as well as all of the control

¹⁶The identifying assumption is that the “endogenous” variable and the outcome of interest are jointly independent of the “exogenous” variable. For a more technical discussion, see section 2.3.4 of Angrist and Krueger (1999).

variables from the second-stage equation (1).

Recall that the causal interpretation of the 2SLS estimates assumes that conditional on the controls, the interaction between lagged U.S. wheat production and a country's tendency to receive U.S. food aid only affects conflict through the provision of U.S. food aid. In principle, the excludability of our instrument could be violated if U.S. wheat production impacts foreign conflict by affecting the world price of wheat or other crops that substitute for or are complements to wheat.¹⁷

In practice, this is not a serious problem for our estimates for several reasons. First, the region-year fixed effects in our baseline equation flexibly control for all year-to-year region-specific changes and therefore account for any global or even regions-specific price changes. For U.S. production-induced world price changes to violate the exclusion restriction, they would need to have systematically different within-region effects on the outcome of interest in a manner that is correlated with a country's tendency to be a U.S. food aid recipient. Nevertheless, to be cautious, our analysis addresses this possibility with additional controls that capture differential responses of countries to global price changes. We discuss these controls in detail in Section 5.

Second, during the period of our analysis, the data suggest that U.S. price stabilization policies, which include the government's accumulation of reserves, were quite effective in breaking the link between U.S. production shocks and price changes. Examining the relationship between total production and average wheat prices measured in real U.S. dollars annually between 1972 and 2006, one finds a negative and marginally significant relationship (the correlation coefficient is -0.30 with a p -value of 0.08).¹⁸ However, further examination reveals that this is completely driven by two outlying observations, 1973 and 1974, two years that experienced low wheat production and happened to coincide with the initial OPEC oil shock (October 1973 to March 1974) that drastically increased oil and commodity prices. Excluding 1973 and 1974, we find no correlation between U.S. wheat production and wheat prices. The correlation coefficient is -0.07 with a p -value of 0.70. The lack of a relationship between U.S. production and global wheat prices is also partly explained by the fact that even though the U.S. is among the largest producers of wheat globally, it by no means dominates global supply. For example, in 2000, the U.S. was the third largest producer of wheat, after China and India, and it accounted for 10.3% of global production.

Another concern is that the estimated relationship between food aid and conflict may be confounded by region-specific shifts in U.S. foreign policy during the time horizon of our study.¹⁹ Most of these policy shifts should be absorbed by the region-year fixed effects.

¹⁷For recent evidence on the relationship between commodity prices and civil conflict see Angrist and Kugler (2008), Dube and Vargas (2009), Bruckner and Ciccone (2010) and Bazzi and Blattman (2011).

¹⁸Data on U.S. wheat prices are from the FAO PriceSTAT (1991-2006) and FAO Price Archive (1973-1990). The figures are the producer price per ton, measure in nominal U.S. dollars. The nominal prices were converted to real prices using the U.S. CPI.

¹⁹For example, during the 1960s and 70s, food aid was primarily used to support South Vietnam (Saylor, 1977). During the Carter administration (1977-81), the focus shifted to alleviating hunger worldwide, causing

However, to be cautious, we also include additional controls that are described in Section 3.

4 Descriptive Statistics

We now provide an overview of the data and their sources. Our primary outcome of interest, the incidence of conflict, is constructed using data from the *UCDP/PRIO Armed Conflict Dataset*, where a conflict is defined as the use of armed force between two parties that results in at least 25 battle deaths in a year. We examine the occurrence of intra-state conflicts (i.e. civil conflicts), inter-state conflicts and conflicts of all types. An intra-state conflict is defined as a conflict between a government and one or more internal opposition groups, without intervention from other states. An inter-state conflict is defined as a conflict occurring between two or more states. The measure of all conflicts includes intra- and inter-state conflicts, and also a small number of conflicts labelled by UCDP/PRIO as “extra-systemic” or “internationalized” conflicts.²⁰

Our measure of U.S. food aid is the amount of wheat aid, measured in thousands of metric tons (MT), shipped to a recipient country in a year from the United States. The data are from the Food and Agriculture Organization’s (FAO) *FAOSTAT* database. By measuring aid in terms of volume, we avoid the difficulty in aid valuation described in section 2.2. Data on U.S. wheat production, which is used to construct our instrument, is reported by the U.S. Department of Agriculture (USDA). Production is also measured in thousands of metric tons.

Table 1 presents descriptive statistics for the main variables used in our analysis. There are many conflicts in our sample, which includes all non-OECD countries and all years from 1972 to 2006. Approximately 23 percent of observations, which are at the country and year level, experience some type of conflict. Most of these are civil conflicts, and most periods of conflict are periods of continued conflict (i.e., there is conflict in the preceding year). Only eighteen percent of conflict incidences are new conflicts (i.e., there is no conflict in the preceding year).

Although U.S. wheat aid is a small part of total U.S. wheat production (5.9% on average over the sample period), it can be large from the recipient’s point of view. The average ratio

food aid to be much more broadly disseminated than in the previous decade (O’Loughlin and Grant, 1990). 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 again became more widespread. In particular, Eastern European countries which used to be within the Soviet sphere of influence began to receive 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.

²⁰Extra-systemic conflicts are conflicts between a state and non-state group that occurs outside of the government’s territory. Internationalized conflicts are conflicts between a state and a non-state group with intervention from another state. There are very few incidences of these two types of conflicts. Our estimates are qualitatively identical if we exclude these conflicts from our measure of the incidence of any conflict.

of wheat aid received from the United States relative to domestic wheat production among observations in the sample is 2.05 and the average ratio of U.S. wheat aid to domestic cereal production is 0.93.

The average country in our sample receives some food aid from the United States in 35 percent of the years between 1972 and 2006. Countries range from having never received any food aid from the United States, such as Argentina, Venezuela and South Africa, to countries that received some food aid from the United States every year, such as Honduras, Haiti and Bangladesh.

Our instrumental variables strategy exploits the relationship between wheat production in the U.S. and subsequent supplies of wheat aid to foreign countries. To illustrate this, we report the bivariate relationships between wheat production, accumulated wheat reserves and wheat aid shipments in Figures 1 and 2. Figure 1 shows a strong positive relationship over time between the total production of wheat within the United States and the stock of wheat reserves held by the government at the end of the same year (i.e., at beginning of the following year). Higher wheat production is followed by higher reserves at the end of the year. Figure 2 shows the relationship between the beginning-of-year wheat reserves and the amount of wheat shipped as food aid in that year. We observe a remarkably strong positive relationship. When there is a greater store of reserves at the beginning of the year, more wheat is subsequently shipped as food aid. Together, Figures 1 and 2 show that more production leads to greater reserves, which leads to more food aid being shipped overseas.

Note that we do not use U.S. wheat reserves to construct our instrument because reserves are potentially endogenous to expectations of future aid shipments and to U.S. foreign policy. Instead, we use U.S. wheat production, which we assume to be determined by exogenous weather conditions in wheat producing regions of the United States, and is, therefore, the exogenous component of wheat reserves that determines food aid.²¹

Our identification strategy recognizes that the impact of increased U.S. wheat production on food aid shipments differs across recipient countries. Specifically, we allow for the fact that countries that are frequent U.S. food aid recipients receive a disproportionate amount of the surplus wheat following an increase in U.S. wheat production. To visualize this fact, we divide the countries in our sample into two groups according to whether the frequency with which a country receives any U.S. food aid during the sample period is greater or less than the median value: $\overline{D}_{ir} \leq 0.29$. The group of countries below the sample median comprises the “irregular” aid recipients and the group above the sample median comprises the “regular” recipients. For the two groups, we calculate, for each year, the average quantity of U.S. wheat aid received by each country. We then plot the relationship between this variable and the one-year lag of total U.S. wheat production over time. Figures 3 and 4

²¹We validated this assumption by directly using U.S. weather conditions as instruments for U.S. wheat aid. The 2SLS results are very similar to those presented in this paper, but the first stage and reduced form coefficients are more difficult to interpret. For brevity, they are not reported in this paper, but are available upon request.

show that, over time, there is no correlation between lagged U.S. wheat production and food aid shipments among the irregular recipients. In contrast, Figure 5 shows that there is a strong positive relationship over time for regular recipients. To compare the magnitudes of the slopes between the two groups, the y-axes in Figures 3 and 5 are constructed to have the same scale. In Figure 4, we display the relationship for irregular recipients with a y-axis that covers a smaller range than in Figure 5. The fact that we see no relationship with the magnified y-axis means that the smaller slope shown for the irregular recipients is not an artifact of the lower average quantity of aid received by infrequent recipients.

The patterns shown in Figures 3-5 are consistent with our conjecture that when the U.S. government allocates surplus wheat production across recipients, it ships a disproportionate amount of the surplus to its regular aid recipients.²² The figures also help understand the variation driving our first stage estimates. Conceptually, the estimated coefficient of the instrument, the interaction between lagged U.S. wheat production and a country's tendency to receive any U.S. food aid, $P_{t-1} \times \bar{D}_{ir}$, is similar to the difference between the slopes of the lines shown in Figures 3 and 4 relative to Figure 5. Thus, one can easily see that the first stage estimate of the effect of the instrument on U.S. wheat aid will likely be positive, i.e. $\hat{\alpha}$ from equation (2) is greater than zero.

Figures 6 and 7 illustrate the variation underlying our reduced-form estimates of the effect of lagged wheat production on conflict for regular and irregular U.S. aid recipients. Again, there is a stark difference between the two groups. Figure 6 shows that there is no relationship between lagged U.S. wheat production and conflict among infrequent aid recipients, whereas Figure 7 shows that the relationship is positive for regular recipients. The reduced form estimate of the effect of the instrument on conflict is the difference in the slopes shown in these two figures. Thus, Figures 6-7 show that the reduced-form estimate will likely be positive.

Conceptually, the 2SLS estimates are the quotient of the reduced-form and first-stage estimates. Therefore, Figures 3-7 show that the instrumented effect of U.S food aid on conflict will most likely be positive. In the next section, we examine whether this is the case in a regression framework where we introduce our baseline set of controls, which also allows us to examine the statistical precision of the estimates.²³

²²We also observe similar relationships when we measure aid receipts and lagged U.S. production in logs. This is also illustrated later in the paper when we test the robustness of our estimates to alternative specifications and measure U.S. production and wheat aid receipts in logs. This means that the same percentage increase in lagged U.S. wheat production results in a larger percentage increase in wheat aid receipts for regular aid recipients relative to irregular recipients.

²³Note that the estimated coefficient in the regression analysis also differs from the illustrated difference in slopes in that it uses all of the variation in \bar{D}_{ir} instead of the difference between the two arbitrarily defined groups of countries.

5 Results

5.1 OLS Estimates

We begin the analysis by first reporting the OLS estimates of equation (1), which are presented in panel A of Table 2. Column (1) reports estimates of the correlation between U.S. food aid and the incidence of any conflict for a specification that only includes recipient-country fixed effects and region-year fixed effects. The OLS estimate of the impact of U.S. food aid on conflict is very close to zero and statistically insignificant. In the remaining columns of the table, we include additional covariates to control for factors that may be correlated with conflict, food aid shipments or U.S. wheat production. Since our 2SLS strategy relies on weather-induced year-to-year variation in U.S. production, a potential concern is that weather conditions in the wheat growing regions of the United States may be correlated with weather conditions in recipient countries, which can have a direct effect on conflict.²⁴ To address this, in column (2), we control for twelve variables that measure the average temperature in each month of year t and twelve variables that measure total precipitation in each month of the same year. By controlling separately for weather in different months, we account for the fact that different parts of the world have different crops with different growing seasons, and hence, different sensitivities to temperature and precipitation.²⁵

A second concern is that variation in U.S. wheat production can affect overseas conflict through a channel other than food aid if U.S. production affects international wheat prices, which may, in turn, affect conflict. The weak times-series relationship between U.S. production and world prices discussed in Section 3, and the inclusion of region-year fixed effects in our estimates suggest that this concern is unwarranted. Nevertheless, to be as cautious as possible, we also control for the possibility that year-to-year price changes may have differential effects on countries within regions. For example, a country's sensitivity to changes in world prices may depend on the extent to which it imports, exports and/or produces wheat or other cereals. Thus, we control for a country's: (i) per capita net imports of cereals and (ii) per capita production of cereals for each country, each interacted with year fixed effects.²⁶ To address the possibility that cereal import and production can be outcomes of aid, we do not control for time-varying measures of each variable. Instead, we calculate country averages for each variable and control for the interaction of the country-

²⁴This is a particular concern given that past studies have found that weather shocks can affect conflict (e.g., Miguel, Satyanath and Sergenti, 2004).

²⁵The measures are constructed using country boundaries and monthly weather data measured across grid-cells from the *Terrestrial Air Temperature and Precipitation: 1900-2006 Gridded Monthly Time Series, Version 1.10*. The data reports 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 for each month from 1900 to 2006. For documentation see Matsuura and Willmott (2007) and Dell, Jones and Olken (2008) for a recent application.

²⁶Cereal production and cereal imports and exports are from the FAO's ProdSTAT and TradeSTAT databases. Both are measured in thousands of metric tons. Population data are from the World Bank's World Development Indicators.

specific measure with year fixed effects.²⁷ These controls allow the impact of global wheat prices, (which are absorbed by the year fixed effects), to differ across countries depending on the extent to which they produce or import cereals. Estimates after including the two additional controls are reported in column (3).

An additional concern is that regular recipients of U.S. food aid (i.e., countries with a high value of \overline{D}_{ir}) may be systematically different from irregular recipients (with a low value of \overline{D}_{ir}) in other ways that influence conflict. For example, regular recipients may also be more likely to also receive U.S. military aid or other forms of U.S. economic aid (besides food aid). If these differences vary systematically over time and across countries within a region, then they will not be captured by our country and region-year fixed effects. We address this possibility by controlling for year fixed effects interacted with: (i) the average annual amount of per capita U.S. military aid received by a country during the sample period and (ii) the average annual per capita amount of other forms of U.S. economic aid (net of food aid).²⁸ Column (4) reports estimates that also include these additional controls.

It is also possible that the regularity in which a country receives U.S. food aid, \overline{D}_{ir} , affects the extent to which weather shocks experience by a country affects the incidence of conflict within the country. Specifically, it is possible that the impact of adverse weather shocks on conflict are weaker if a country regularly receives U.S. food aid. To control for this possibility, we include the interaction of each of our twelve monthly temperature variables and twelve monthly precipitation variables with the frequency in which a country to receive food aid from the United States, \overline{D}_{ir} . The results with these additional controls are reported in column (5) of the table.

The estimates of columns (2)-(5) show that the OLS correlation between U.S. food aid and the incidence of conflict are always negative in sign, small in magnitude and statistically insignificant. They are unaffected by the addition of controls. In columns (6) and (7), we separately investigate the effects on the incidence of civil and international conflicts. We find similarly small and statistically insignificant estimates.

5.2 First-Stage and Reduced-Form Estimates

The first-stage estimates of equation (2) are shown in panel D of Table 2. These estimates show that there is a strong positive correlation between the instrument and food aid shipments. The first stage F -statistics for the excluded instrument range from 13.8 to 20.1. Thus, it is unlikely that our estimates are biased by weak instruments. In terms of magnitude, the estimated coefficient in column (5) suggests that for a country that receives some amount of food aid from the U.S. in every year of the sample period (i.e., $\overline{D}_{ir} = 1$), a 1,000

²⁷Estimates from using contemporaneous or one-year lagged time-varying measures of production and imports, each interacted with year fixed effects are virtually identical to the estimates reported in the paper. They are available upon request.

²⁸Aid data are from the USAID and population data are from the World Bank's World Development Indicators. The figures are measured in 2007 U.S. dollars per person.

MT increase in total U.S. wheat production increases the amount of food aid received in the following year by 2.44 MT. As reported in Table 1, the average value of \bar{D}_{ir} in our sample is 0.35. Therefore, evaluated at the sample mean, a 1,000 MT increase in U.S. wheat production is predicted to increase U.S. food aid shipments by $0.35 \times 2.44 = 0.85$ MT. Multiplying this by the number of countries, 134, gives 114.4 MT, which is an approximate measure of the predicted increase in total U.S. food aid shipments to the world that result from a 1,000 MT increase in U.S. wheat production.

To check that our first-stage estimates are not confounded by spurious positive trends between U.S. wheat production and food aid shipments to U.S. food aid recipients, we conduct a falsification exercise and estimate alternative first-stage equations where the instrument is used to predict past food aid rather than future food aid. Columns (2) and (3) of Table 3 report estimates of two alternative specifications where the dependent variable is wheat aid shipments one and two years *before* the year of the production shock. We find no relationship between our instrument and past U.S. food aid. The relationship is statistically insignificant, negative, and very small in magnitude. These results support our identification assumptions.

As a check that our instrument does not violate the exclusion restriction by also affecting recipient-country cereal production, we also examine the relationship between the instrument and a recipient country's per capita cereal production. The estimate, reported in column (4) of Table 3, shows that the instrument is uncorrelated with foreign-country cereal production. This finding is consistent with the existing empirical evidence, which generally fails to find a link between food aid and production (FAO, 2006, pp. 40-41, Abdulai, Barrett and Hoddinott, 2005).

We report the reduced-form effects of our instrument on the outcome variables of interest in panel B of Table 2. For these regressions, we have multiplied the dependent variable by 1,000 for presentation purposes. The impact of the instrument on the incidence of all conflicts and intra-state conflicts are positive and statistically significant at the one percent level. However, there is no impact on inter-state conflict. In addition, the estimates are also very stable across the specifications reported in columns (1)-(5).

5.3 2SLS Estimates

Table 2 panel C reports 2SLS estimates of equation (1). Like the reduced form, the 2SLS estimates remain stable as we introduce the baseline controls in columns (1)-(5). According to the estimates using the full set of baseline controls reported in column (5), a 1,000 MT increase in U.S. wheat aid increases the incidence of conflict by 0.47 percentage-points, an effect that is statistically significant at the one percent level. Columns (6) and (7) show that the impact on overall conflict is driven by an increase in intra-state conflicts and not by inter-state conflicts.

The finding that food aid only affects intra-state conflicts is consistent with the qualita-

tive accounts that emphasize the impact of food aid on fueling local conflicts between rebel groups and the government. Given the focus of the descriptive literature and our findings, the remainder of our analysis focuses on intra-state conflicts.

To assess the magnitude of the implied effect of aid on civil conflict, consider that the sample mean of the incidence of civil conflict is 17.6 percentage-points (i.e., 0.176) and of U.S. wheat aid is 27.6 thousand MT. Therefore, for a country at the mean level of U.S. wheat aid, the estimate from column (6) implies that a ten percent increase (i.e., 2.76 thousand MT) in U.S. food aid is associated with a 1.11 percentage-point increase in the incidence of civil conflict, which is approximately six percent of the mean. Expressed in terms of standard deviations, our estimates imply that a one-standard-deviation increase in food aid will increase the incidence of civil conflict by 1.25 standard deviations.

Note that in addition to the 2SLS estimates, panel C also reports the *Limited Information Maximum Likelihood* (LIML) estimates and their corresponding p -values, as well as *Conditional Likelihood Ratio* (CLR) 95% confidence intervals. The fact that the LIML estimates and the mid-point of the CLR intervals are similar or higher than the 2SLS estimates is consistent with our instruments being strong and the 2SLS estimates not suffering from weak instruments bias.²⁹

5.4 Robustness

We now check the robustness and sensitivity of the estimated impacts of U.S. food aid on civil conflict. First, we examine the robustness of the findings to the inclusion of additional controls that may affect conflict and be correlated with food aid. These are shown in Table 4. In column (1), we first reproduce our baseline estimates, but with the smaller sample size that results from the inclusion of additional control variables. The smaller sample produces an estimate that is qualitatively identical to the estimate with the larger sample.³⁰ In columns (2) and (3), we control for GDP per capita, which has been found to be an important determinant of civil conflict (Miguel, Satyanath and Sergenti, 2004). Since contemporaneous GDP is potentially endogenous to U.S. food aid, we either control for lagged per capita GDP (reported in column (2)) or control for a country’s average real per capita GDP over the sample period interacted with year fixed effects (in column (3)).³¹

In column (4), we address the possibility that a country’s level of democracy may affect both the extent to which it receives U.S. food aid and civil conflict by controlling for the interaction of a country’s average “polity2” score (from the Polity IV database) during the sample period and with year fixed effects.

²⁹See Moreira (2003) for a discussion of how LIML estimates are less vulnerable to weak instruments bias relative to 2SLS estimates and of how CLR intervals address the weak instruments problem in the case with one endogenous regressor.

³⁰All specifications reported in Table 4 use the same sample for which data for all control variables are available. Allowing the number of observations to vary across the specifications yields qualitatively identical results.

³¹The results are also robust to controlling for contemporaneous per capita GDP.

In column (5), we consider the fact that the Cold War ended during the middle of our sample and how this can influence our estimates. The region-specific year fixed effects absorb the impacts of the Cold War and its conclusion if the impact of these changes were similar across countries within a region. However, there remains the concern that the events may have had differential effects across countries within regions. Specifically, there is concern that Soviet Cold War allies, which tend to be irregular recipients of U.S. food aid (e.g. a low value of \bar{D}_{ir}), may have experienced a greater decline in conflict after the end of the Cold War. In this case, a spurious decline of U.S. wheat production in the post-Cold War era would bias our estimates towards finding that greater U.S. production increases conflicts among regular U.S. food aid recipients relative to irregular recipients. This seems unlikely because the data show no decline in U.S. production after the end of the Cold War. However, to be as careful as possible, we address this concern explicitly by controlling for the interaction of a post-Cold War indicator variable and \bar{D}_{ir} .

Finally, in column (6), we include all of the robustness controls simultaneously. The estimated impact of U.S. food aid on civil conflict remains robust across all specifications: the coefficients for U.S. food aid remain positive, statistically significant, and roughly the same magnitude as the baseline estimate.

Next, we check the robustness of our findings to the use of alternative estimating equations. We begin by estimating an alternative specification taken from Miguel, Satyanath and Sergenti (2004). The specification is the same as equation (1), but with country-specific time trends rather than region-year fixed effects:

$$C_{irt} = \beta F_{irt} + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_i Year_t + \delta_i + \varepsilon_{irt}. \quad (3)$$

The vector of controls, \mathbf{X}_{irt} , now includes the time invariant country controls (i.e., average cereal production, cereal imports, U.S. military aid and U.S. economic aid), each interacted with a time trend rather than time-period fixed effects. Column (1) of Table 5 reproduces the baseline estimate for comparison, while columns (2) and (3) report estimates of equation (3). Column (2) controls for a time trend that is the same for all countries, while column (3) controls for country-specific time trends. In both cases, the estimated impact of U.S. food aid on conflict remains positive, highly significant, and roughly the same magnitude as our baseline estimate.

We next check the robustness of our estimates to the use of an alternative instrument. Since the exogenous source of the variation we exploit comes from time variation in U.S. wheat production, we check that we find similar estimates using only this source of variation, i.e. using P_{t-1} instead of $P_{t-1} \times \bar{D}_{ir}$ as an instrument. Equation (3) allows us to do this since it imposes a functional form on unobserved time-varying factors rather than allowing them to vary flexibly over time. The 2SLS estimate of equation (3), using lagged U.S. wheat production (only) as an instrument, is reported in column (4) of Table 5. The point estimate is positive, statistically significant and similar in magnitude to the baseline

estimate. This finding shows that our main results are not biased by interacting lagged U.S. wheat production with the regularity that a country receives U.S. food aid.³²

In column (5), we alter our baseline equation (1) by including a one-year lag of the dependent variable as a control to account for the potential persistence of conflict. This estimate should be interpreted cautiously since it suffers from the Nickell (1981) bias. Nevertheless, the fact that the estimate is robustly positive and statistically significant is reassuring.

In columns (6) and (7), we show that we obtain qualitatively identical results if we normalize U.S. food aid shipments by the recipient’s population or if we measure U.S. food aid and U.S. production in natural logs rather than raw values. In both cases, the results remain robust, and the magnitudes of the estimated impact of food aid, assessed by comparing standardized beta coefficients, are similar. Thus, our results are not specific to our choice of functional form.

5.5 Mechanisms

Thus far, our analysis has focused on estimating a causal relationship between U.S. food aid and the incidence of civil conflict. We next explore potential mechanisms underlying these results.

5.5.1 Onset and Duration

Our main outcome of interest, the incidence of civil conflict, reflects both the onset of new conflicts and the continuation of existing conflicts. Anecdotally, there are many accounts of food aid affecting both onset and duration. For example, it has been argued that humanitarian aid during the Nigeria-Biafran civil conflict (1967-1970) strengthened the rebel leader Odumegwu Ojukwu, causing the conflict to last twelve to sixteen months longer than it otherwise would have (Polman and Waters, 2010, pp. 114-122). More recently, observers have argued that the aid given to Hutu extremists in refugee camps allowed Hutu leaders to regroup, regain resources, and launch raids and attacks into Rwanda, leading to the First and Second Congo Wars (Polman and Waters, 2010, pp. 13-34). To investigate the individual contributions of onset and duration to our findings, we separately estimate the impact

³²One may also be concerned that our measure of a country’s average tendency to receive food aid includes whether a country received any U.S. food aid in year t , which is related to the dependent variable in the first stage, the amount of U.S. food aid a country receives. We obtain virtually identical results if we use alternative instruments where we interact lagged U.S. production with different lagged moving averages of the tendency to receive U.S. food aid, or with the average tendency of receiving U.S. food aid for all years except for year t . In addition, we have also checked that our estimates are robust to using the interaction of the tendency for a country to receive U.S. food aid and the lag of predicted U.S. wheat production, constructed using county level data on U.S. wheat production and grid-cell level data on monthly temperature and precipitation. Alternatively, we also instrumented for U.S. wheat aid directly with U.S. weather shocks. Since, a priori, the probability that U.S. producers base production decisions on factors that affect conflict in poor aid-recipient countries is very low, it is not surprising that estimates when using the instruments based on predicted production are very similar to our baseline estimates. For brevity, we do not describe those estimates in detail or report them in the paper. They are available upon request.

of food aid on the two outcomes.

To examine the impact on civil conflict onset, we rely on specifications from existing studies that examine the determinants of the onset of civil conflict, defined as the first year of a conflict episode or, equivalently, a conflict that occurs in a year following a year with no conflict. We examine the determinants of the onset of civil conflict by first using the methodology from Collier and Hoeffler (2004), which removes observations that are periods of continued conflict. Therefore, the sample only includes periods of no conflict and periods of conflict onset.³³ The 2SLS estimate of the impact of U.S. food aid on the onset of civil conflict is reported in column (1) of Table 6. We find a positive, but statistically insignificant impact of U.S. food aid on civil conflict onset.

Column (2) reports estimates using an alternative specification from Fearon and Laitin (2003). Rather than excluding periods of continued conflict from the sample, the authors include all observations and control for the incidence of civil conflict in the previous period, which captures the mechanical relationship between the onset of civil conflict and the presence of conflict in the previous period. This alternative estimation strategy generates a point estimate that is nearly identical to the estimate reported in column (1), but is much more precisely estimated and statistically significant. We interpret the findings from columns (1) and (2) as evidence that U.S. food aid has a positive effect on the onset of civil conflict.

Next, we examine the effect of U.S. food aid on the duration of civil conflict. This follows existing studies examining the duration of civil conflict, including Collier et al. (2004) and Fearon (2004). The event of interest is the end of civil conflict.³⁴ Let t index time, i index civil conflicts and $T_i \geq 0$ denote the length, in years, of the civil conflict (i.e., the duration). The sample includes all country-years that are “at risk” for transition out of conflict, i.e. all of the observations for which there was a civil conflict in the previous period. The estimation uses the discrete hazard $h_{it} = \Pr(T_i = t \mid T_i \geq t)$, where it is assumed that h_{it} follows a logistic distribution.³⁵

Estimates of the impact of U.S. food aid on a country’s transition out of civil conflict are reported in columns (3)-(5) of Table 6. Column (3) reports estimates only controlling for the duration of the conflict up until period $t - 1$. We allow the impact of duration on the hazard rate to vary in a flexible manner by including a third degree polynomial of duration. In column (4), we also control for the time-invariant country characteristics from our set of

³³Our specification includes all baseline control variables.

³⁴In this context, what one commonly refers to as “survival” in hazard models is continued conflict.

³⁵In practice, the estimation relies on the insight from Allison (1984) and Jenkins (1995) regarding the equality of the log likelihood function of discrete time hazard models and the standard likelihood function for a binary regression model in which y_{it} (an indicator that equals one if the country transitions out of conflict at time t) is the dependent variable. The data are structured so that there is an observation for each period that the country is at risk of transitioning out of the current state. The insight that the logit of the discrete-time hazard model can be estimated using a logistic regression model is particularly useful since our independent variable of interest, U.S. food aid, is instrumented with the interaction term using lagged U.S. wheat production. We are thus able to estimate the impact of U.S. food aid on the hazard rate by applying a control function approach that uses the two-step approach from Rivers and Vuong (1988). The two-step approach is implemented by controlling for the first-stage residuals in the second-stage logit regression.

baseline control variables: a country’s average receipt of U.S. military aid over the period, its receipt of U.S. economic aid net of food aid, its average import of cereals and its average production of cereals. Column (5) reports estimates from a specification that also controls for region fixed effects. The estimates show that U.S. food aid increases the duration of civil conflict, i.e. it reduces the probability that a civil conflict ends. In all three specifications, the coefficients for U.S. food aid are significant at the one-percent level. The magnitude of the estimates (marginal effects evaluated at means) suggests that a 1,000 MT increase in food aid shipments decreases the probability of the civil conflict ending in a year by between 0.48 and 0.61 percentage-points, a large effect given that the sample mean for the probability that a civil conflict ends is 0.188.

Overall, the results reported in Table 6 show that food aid increases both the onset and the duration of civil conflicts. These findings are consistent with accounts of food aid providing resources that can be used to both start civil conflicts and to prolong existing conflicts.³⁶

5.5.2 The Scale of Conflict

Descriptive accounts of humanitarian aid being an important source of funds for small-scale rebel groups and “refugee warriors” suggest that aid may have a particularly large impact on the incidence of smaller-scale conflicts. Our main conflict measures, which define conflict incidence to be the presence of an armed conflict that causes 25 or more combat deaths within a year, include both small and large conflicts. To investigate the extent to which our main results are driven by small-scale conflict, we compare our main estimates to estimates when using data from the *Correlates of War* (COW) Database, which only includes conflicts that incur a higher threshold of 1,000 or more combat-related deaths in a year.

For comparison, columns (1)-(3) of Table 7 restate the baseline estimates for all conflicts, intra-state conflicts and inter-state conflicts using the UDCP/PRIO. Columns (4)-(6) presents estimates of the same specifications, but using the higher threshold COW data. We continue to find a positive impact of food aid on all conflicts and intra-state conflicts, and no impact on interstate conflicts. The magnitudes of the positive impacts of food aid on all conflicts and intra-state conflicts are significantly smaller than the baseline estimates using the lower conflict threshold UDCP/PRIO data.

³⁶We have also examined the lag structure of our estimates. Our baseline specification estimates the effect of contemporaneous U.S. food aid on conflict. However, wheat aid in the previous years may also affect conflict. This could occur if food aid is stored and then fought over by the opposing factions within a country in the following year. Alternatively, food aid expropriated by rebel groups in a year may strengthen them and increase their ability to fight in the following year. We have investigated this by estimating the effect of one-year and two-year lagged U.S. food aid on the incidence of civil conflict. We find that, conditioning on food aid in the current period, food aid from previous years has no effect on conflict. The coefficients are small in magnitude and are statistically insignificant. This suggests that the impact of food aid on conflict is immediate, a result that is consistent with accounts of food aid generating resources for militias that allow them to continue fighting. For the sake of brevity, these results are not reported in the paper. They are available upon request.

Part of the difference between the coefficients in columns (1)-(2) versus (4)-(5) is a mechanical result of the fact that larger conflicts are less prevalent than small conflicts. (See the descriptive statistics in Table 1.) However, even if we compare the estimated coefficients to the sample means, the impact of food aid on conflict continues to be smaller for larger-scale conflicts. Similarly, if one also compares standardized beta coefficients, which are reported in Table 7 below the estimated coefficients, the same conclusion is reached. For example, while a one-standard-deviation increase in U.S. food aid increases the incidence of small-scale intra-state conflicts by 1.25 standard deviations (column 2), it increases the incidence of large-scale conflicts by only 0.74 standard deviations (column 5). Overall, these findings are consistent with descriptive accounts that emphasize the theft of humanitarian aid by small-scale militants and other similar armed groups.

5.5.3 Crowding-Out of Other Aid

Our interpretation of the main results is that U.S. food aid has a direct causal impact on conflict in recipient countries. However, a possible alternative explanation is that food aid affects conflict indirectly by crowding out other types of aid. For example, other donor countries or multilateral agencies may respond to an increase in U.S. food aid by reducing their own aid provisions. If these other forms of aid *reduce* conflict, then this form of “crowd-out” can explain why U.S. food aid increases conflict. Similarly, if the reduction in food aid is large enough, then an increase in U.S. food aid could actually cause total foreign aid to decline. Our results could, again, be explained by aid crowd-out if total foreign aid *reduces* conflict. It is important to keep in mind that this does not undermine the validity of our estimates of the causal effect of U.S. food aid on conflict, but the mechanism of crowd-out is very different from the ones that motivated our study, and they have very different policy implications.

We explore this important alternative explanation by re-estimating equation (1) but with other forms of aid provision as dependent variables. We first examine the effect of U.S. wheat aid on total wheat aid provision (from all countries). If U.S. aid is crowding out wheat aid from other countries, then we would expect to find that a one-unit (i.e., 1,000 MT) increase in U.S. wheat aid increases total food aid by less than 1,000 MT. Table 8 column (1) reports this point estimate, which is 1.13 and statistically significant. The point estimate, which is very close to one, suggests that U.S. aid does not crowd out the provision of wheat aid from other countries. Column (2) estimates the same regression but with cereals aid from all countries, rather than wheat aid as the dependent variable. The point estimate again shows that U.S. wheat aid does not crowd out food aid from other countries. The finding that there is no crowding out of either wheat or cereals aid is also confirmed by the estimates reported in columns (3) and (4), which show that U.S. wheat aid has no impact on the provision of wheat aid and cereal aid from non-U.S. donor countries.

We next turn to the possibility that U.S. food aid crowds out the provision of other

types of aid by the U.S., such as economic aid or military aid. Columns (5) and (6) report estimates of the effect of U.S. wheat aid on U.S. military aid and U.S. economic aid (exclusive of food aid). We find that food aid has no effect on these other types of aid and that there is no evidence for this form of crowd out. To help assess the magnitude of coefficients, the mean of each dependent variable is reported in the first row of the table. The estimated coefficients are small in magnitude (relative to the means), and statistically insignificant. Columns (7) and (8) test whether U.S. food aid crowds out total foreign aid provision by other countries. The columns report estimates of the impact of U.S. food aid on two measures of total net Official Development Assistance (ODA) from non-U.S. donors, both taken from the Roodman’s (2007) Net Aid Transfers Dataset. The measure of ODA used in column (7) includes loans and grants net of principal and interest payment on existing loans. The measure used in column (8) is also net of cancelled “Other Official Finance” (OOF) loans, which are typically included as ODA (see Roodman, 2007 for further details). We find no evidence of aid crowd-out using either measure. The coefficients in both specifications are small in magnitude and not statistically different from zero.

From these results, we conclude that our main results are not driven by the crowding out of other types of food aid or foreign aid.

5.5.4 Crowding-Out of Domestic Production

We next turn to the question of whether U.S. food aid crowds out domestic production, which could result in lower incomes and thereby increase conflict. *A priori*, we find it unlikely that our estimates are explained by this mechanism because our estimates are identified from the short-term impacts of year-to-year variation in U.S. food aid provision. For changes in domestic production to explain our findings, production decisions would need to be made in anticipation of U.S. food aid shipments. This seems unlikely to be true in practice since planting decisions are made months before the crops are harvested. Thus, it seems unlikely that rural farmers in poor countries, often far from the national capital, forecast changes in food aid receipts in the following year and then make plans based on these forecasts. Nevertheless, to be cautious, we investigate this possibility in columns (9) and (10) of Table 8, where we report estimates of the impact of U.S. food aid on the recipient’s wheat and cereal production. The estimated impact is small in magnitude and statistically insignificant. There is no evidence that U.S. wheat aid crowds out recipient countries’ domestic food production.

5.5.5 Heterogenous Treatment Effects

Our findings have shown that food aid, on average, increases the incidence, onset and duration of civil conflict. We now explore whether there are certain environments in which food aid does not have such adverse effects by testing for heterogeneous impacts of food aid.

The factors we examine are motivated by descriptive accounts and existing studies on aid or conflict.

We begin by examining a country's level of overall economic development, measured by real GDP per capita. It is possible that because more-developed countries are generally more stable and less prone to conflict, food aid has a weaker impact on conflict within this group. We calculate each country's average annual real per capita income between 1972 and 2006, construct an indicator variable for countries below the median level of average income, and interact this with the measure of U.S. food aid, F_{irt} . This allows the impact of food aid on conflict to differ for countries below and above the median value of average per capita income. This second stage equation can be written as the following.

$$C_{irt} = \beta (F_{irt} \times I_{ir}) + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_{rt} + \delta_i + \varepsilon_{irt}, \quad (4)$$

where I_{ir} is an indicator variable that equals one if country i of region r has an average value of real per capita income between 1972 and 2006 that is below the median value in the sample. The other variables are the same as in equation (1). Since the direct effect of the indicator variable I_{ir} is absorbed by the country fixed effects, the only difference between equations (1) and (4) is the interaction term $F_{irt} \times I_{ir}$.

To establish causality, the main effect of U.S. wheat aid, F_{irt} , and the interaction term, $F_{irt} \times I_{ir}$, are instrumented with the original instrument, $P_{t-1} \times \bar{D}_{ir}$, and the interaction of the original instrument with the indicator variable, $P_{t-1} \times \bar{D}_{ir} \times I_{ir}$. The controls include the baseline covariates and the full set of double interaction terms. The first stage equation for F_{irt} is:

$$F_{irt} = \pi_1 (P_{t-1} \times \bar{D}_{ir} \times I_{ir}) + \pi_2 (P_{t-1} \times \bar{D}_{ir}) + \pi_3 (P_{t-1} \times I_{ir}) + \mathbf{X}_{irt}\mathbf{\Gamma} + \varphi_{rt} + \delta_i + \varepsilon_{irt}. \quad (5)$$

Note that the direct effects of \bar{D}_{ir} and I_{ir} , as well as their double interaction, $\bar{D}_{ir} \times I_{ir}$, are absorbed by the country fixed effects. As before, the direct effect of P_{t-1} is absorbed by the region-year fixed effects. The first-stage equation for the interaction term is identical to equation (5) but with $F_{irt} \times I_{ir}$ as the dependent variable.

The instrumented second-stage results reported in column (2) of Table 9 show that the impact of food aid on conflict is statistically similar for low and high income countries. The estimated coefficient for the interaction term is negative but statistically insignificant.³⁷

We next examine political institutions, dividing countries in our sample according to whether they have an average polity2 score over the period that is above or below the sample median.³⁸ This addresses the hypothesis that more democratic governments (i.e., those with a high polity2 score) are more accountable to citizens and therefore more likely

³⁷We do not report the first stage estimates for brevity. They are available upon request.

³⁸The polity2 measure is taken from the Polity IV Database.

to distribute aid in a way that does not promote conflict.³⁹ For similar reasons, we also distinguish between countries with civilian and non-civilian governments, again using the median of the average measure over the sample period as a threshold.⁴⁰ The results reported in columns (3) and (4) show that the effect of U.S. food aid is statistically similar across countries with different levels of democratization and countries with civilian vs. military governments.

Given that internal conflict is often tied to within-country ethnic differences, we examine whether the impact of food aid differs depending on the ethnic diversity and ethnic polarization of a country. We create interaction terms for countries with below median-levels of ethnic diversity and below-median levels of ethnic polarization.⁴¹ The estimates reported in columns (5) and (6) show that food aid has similar impacts across countries with different levels of ethnic polarization, but has a weaker impact on conflicts in countries with lower levels of ethnic diversity. The latter estimate is negative in sign and large in magnitude, although it is only statistically significant at the 15% level.

Next, we investigate the plausibility of first-hand accounts that armed factions steal aid during transit, often by setting up road blocks, and then use the appropriated aid to feed soldiers and fund the fighting. To do this, we hypothesize that road-blocks erected by armed factions, as well as other similar forms of theft, are more successful in countries where transportation networks are less developed. The effectiveness of roadblocks for armed factions decreases with the number of routes between the original location of aid and the destination where aid is to be delivered. This is based on the logic that it is easier for aid deliveries to circumvent road blocks if there are more alternative routes. Our measure of transportation networks is the annual average kilometers of roads per capita over the sample period.⁴² The estimates reported in column (7) show that food aid has a smaller impact on conflict in countries with a more developed road network. The estimated interaction effect is negative and large in magnitude, although it is only statistically significant at the 15% level.

The last factor we examine is not a country characteristic, but a change in the global political environment that occurred during the sample period. Motivated by studies that find evidence of differing motives behind U.S. aid provision during the Cold War, relative

³⁹See Besley and Persson (2008) for a theoretical discussion.

⁴⁰The classification of whether a country has a civilian government is taken from the “RegimeType” variable from the Banks’ Cross-National Time-Series Data Archive, which reports if a government is wholly civilian, mixed, military, or other. We create a dummy variable that equals one if the government is entirely civilian and then calculate the average of this variable for each country over time. Note that the Banks dataset is available up to 1999. Thus, we assume that the country mean for 1972-1999 is similar to the country mean for the period of our study, 1972-2006.

⁴¹The measure of ethnic diversity is from Alesina et al. (2003) and the measure of polarization is from Montalvo and Reynal-Querol (2005). These measures are only available for one year for each country and the year varies. See Alesina et al. (2003) and Montalvo and Reynal-Querol (2005) for detailed discussions.

⁴²The measure is constructed as the average for the years 1990-2003 using data reported by the World Development Indicators. Our estimates assume that the average for 1990-2003 is broadly similar to the average for the period of our study, 1972-2006.

to after the Cold War (e.g., Meernik, Krueger and Poe, 1998), we examine whether food aid affected conflict differently during the Cold War. The results, reported in column (8), provide no evidence of a differential impact during the Cold War. The estimated coefficient for the Cold War interaction is small in magnitude and statistically insignificant.

Finally, in column (9), we include all interaction terms in the same regression, which addresses the fact that many of the factors we have examined may be correlated. The estimates confirm the previous findings that food aid has similar effects on conflict on countries with different levels of: income, democratization, civilian rule, ethnic polarization; and that it has similar effects during the Cold War and post-Cold War periods.

The differential effect for low ethnic diversity countries remains statistically significant, although its magnitude is half as large as its estimated magnitude without the other interactions also included (reported in column (6)). The estimated interaction effect for road density remains robust. The magnitude of the coefficient is similar to the magnitude reported in column (7), and it is now statistically significant at the 1% level. The sum of the coefficients for U.S. food aid and the interaction of U.S. food aid and the high road density indicator variable (reported at the bottom of Table 9) is small and close to zero, which implies that U.S. food aid has little effect on conflict in countries with high road density and our main results are primarily driven by countries with low road density. This is consistent with the observation that food aid is often stolen in transit and used by armed factions to feed troops and finance rebel activities.

6 Conclusion

Humanitarian aid is one of the key international policy tools for helping needy populations in times of crises or for aiding those suffering from endemic poverty. In recent years, humanitarian aid and, in particular, its main component, food aid, have increasingly come under criticism. Observers argue that humanitarian aid not only fails to achieve its main goal of improving the well-being of the target population, but it can actually harm the intended beneficiaries by promoting conflict. This controversial topic has already sparked much discussion amongst aid watchers. However, to undertake systematic reform of food aid policy, one requires rigorous empirical evidence on the *average* effect of existing policies.

Our findings show that the concerns of critics are very real. By exploiting year-to-year variation in wheat production in the United States, along with cross-country variation in a country's propensity to receive food aid from the United States, we show that, on *average*, food aid *promotes* civil conflict. An increase in U.S. food aid increases the incidence, onset and duration of armed civil conflicts in recipient countries. We show that our results are not a result of U.S. food aid crowding out aid from other countries. We also find that food aid has a more adverse effect on small-scale armed conflicts and in countries with a less developed transportation network. These findings support qualitative accounts of food aid

either being stolen during transport or being taken from target populations by small armed groups that use the resources to fund conflict.

Our study is a small first step towards the larger goal of understanding the tradeoffs that exist for food aid and humanitarian aid policies. Ideally, one would like to examine a broad range of outcomes that capture both the potential benefits as well as the costs of food aid. One set of important outcomes to study are those related to health, such as infant and child mortality. Unfortunately, given existing data constraints, we are unable to do this using our empirical strategy, which relies on year-to-year variation across a broad sample of developing countries. For example, much of the the data currently reported by the *World Health Organization* or the *World Bank* are constructed by interpolating between years for which actual data are available. The real variation in the data generally is not year-to-year and consequently it cannot be used with our empirical strategy.⁴³

One would also like to better understand the specific mechanisms that cause the adverse impact of food aid identified here. We attempt to do this to the extent possible given the available data. But our measures are admittedly coarse, with the variation remaining at the country-year level. We believe that collecting finer-grained (e.g., subnational or project-level) data is essential for future research on understanding the effects of food aid.⁴⁴

In interpreting our results, it is important to note that we only study the effects of one particular type of aid, food aid. This form of aid is very different from other types of foreign aid in that it (and many other forms of humanitarian aid) is delivered in-kind and is therefore particularly vulnerable to being appropriated by armed factions during transport. In contrast, much of foreign aid is transferred as money from the donor government to the recipient government. Whether such types of foreign aid affect conflict is beyond the scope of our study and our results should not be extrapolated as evidence of the impacts of foreign aid in general.

⁴³In the future, one may be able to apply our strategy to a panel of health outcomes constructed from the *Demographic Health Surveys*. The surveys began too recently to allow the construction of a sufficiently long panel for analysis today. Most of the *Demographic Health Surveys* (DHS) began in the mid-1990s. These surveys record the completed fertility history of women age 15-49. Using this data for our analysis faces two challenges. First, there are very few births from the 1970s and 80s, which means that currently the resulting panel is too short for our statistical analysis. This can be addressed in the future when the constructed panels will naturally be longer, assuming that U.S. food aid policy does not change and our empirical strategy remains valid at that time. Second, the DHS samples are conditional on women being alive during the year of the survey. Since conflict causes mortality, this raises the concern that DHS samples are affected by the incidence of past conflict.

⁴⁴Two examples of recent studies taking a more micro-oriented approach (although not examining food aid per se) are Crost, Felter and Johnston (2011) and Dube and Naidu (2010).

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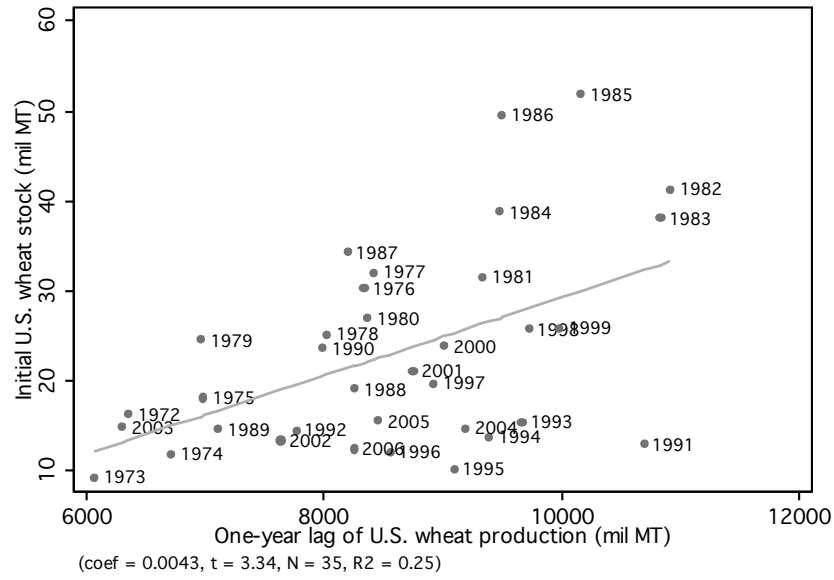


Figure 1: U.S. Wheat Reserves and Lagged U.S. Wheat Production

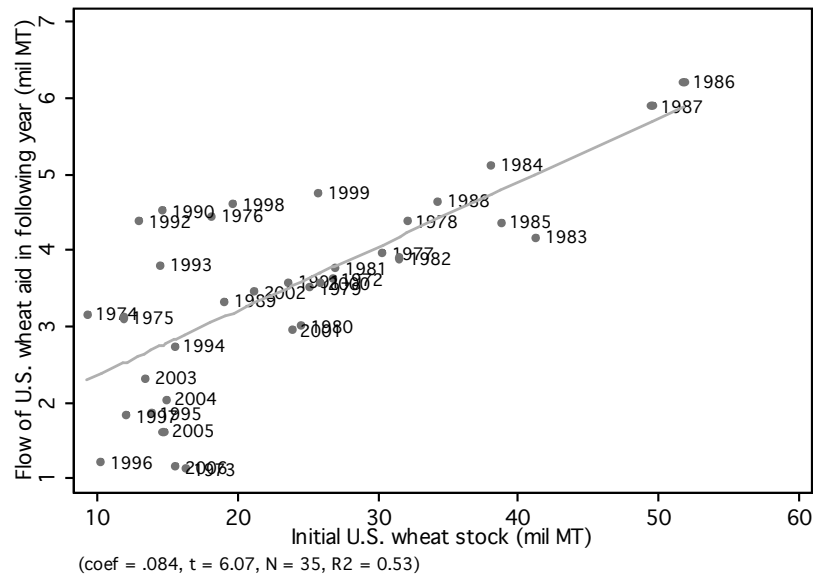


Figure 2: U.S. Wheat Aid and Initial U.S. Wheat Reserves

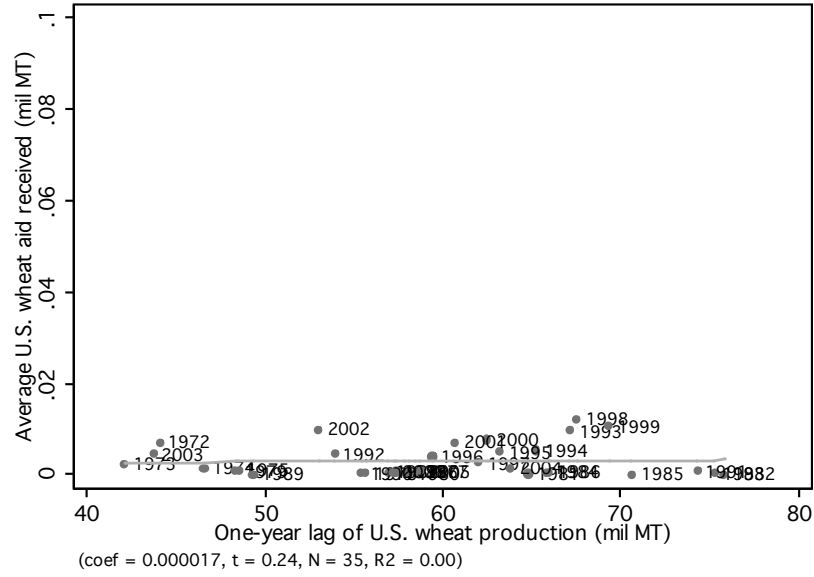


Figure 3: Average U.S. Wheat Aid and Lagged U.S. Wheat Production
– Irregular Recipients: $\bar{D}_{ir} < 0.29$

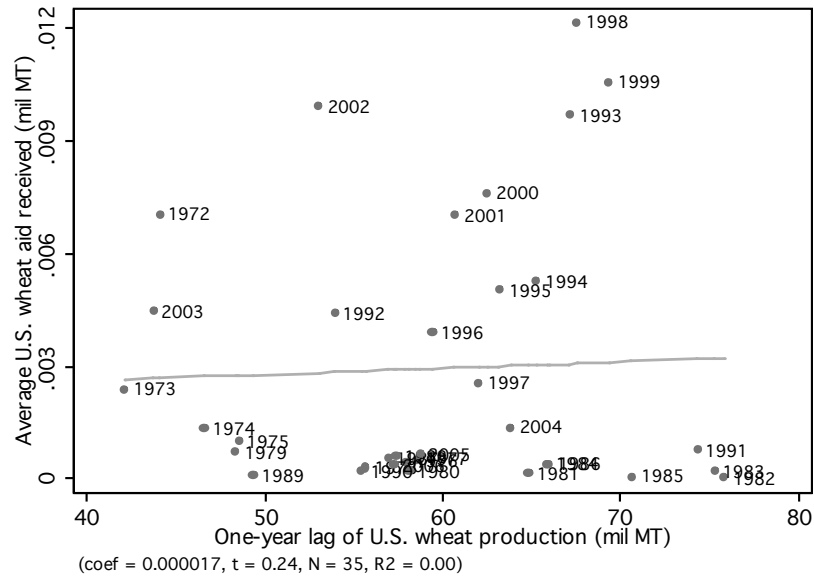


Figure 4: Average U.S. Wheat Aid and Lagged U.S. Wheat Production – Unscaled Y-Axis
– Irregular Recipients: $\bar{D}_{ir} < 0.29$

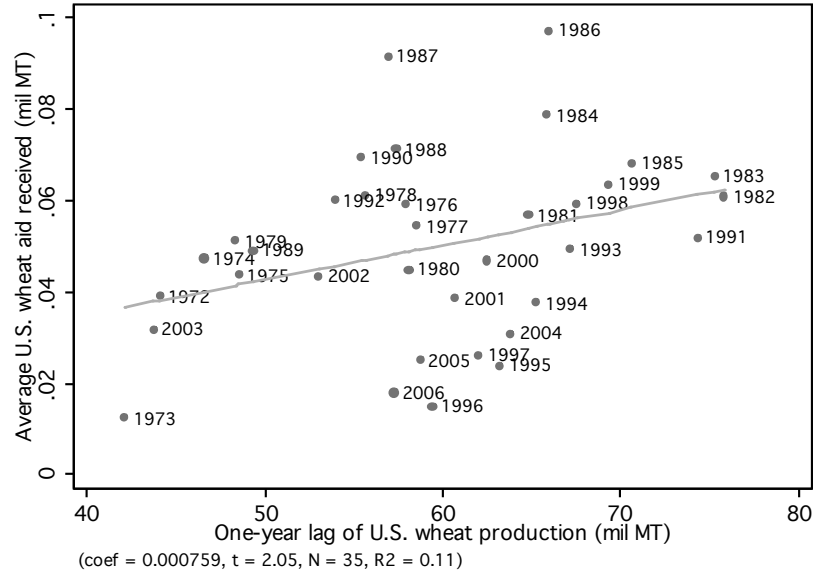


Figure 5: Average U.S. Wheat Aid and Lagged U.S. Wheat Production
– Regular Recipients: $\overline{D}_{ir} \geq 0.29$

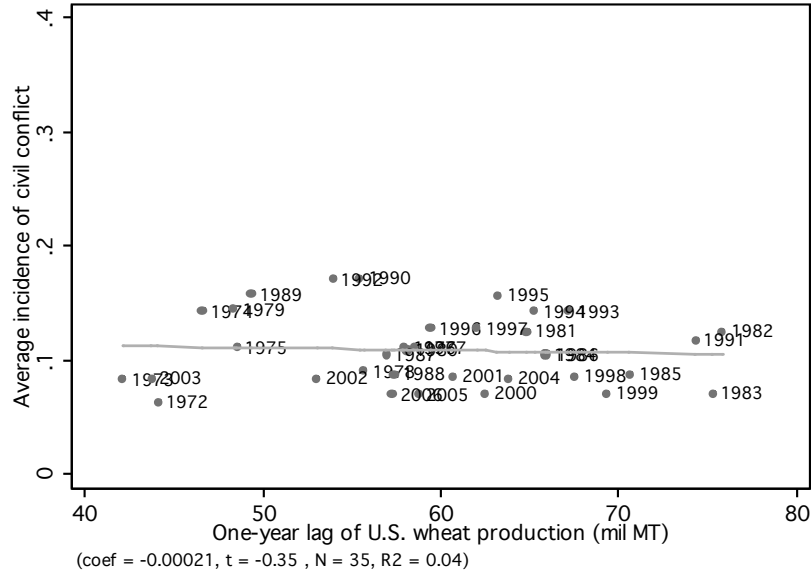


Figure 6: Average Civil Conflict Incidence and Lagged U.S. Wheat Production
– Irregular Recipients: $\bar{D}_{ir} < 0.29$

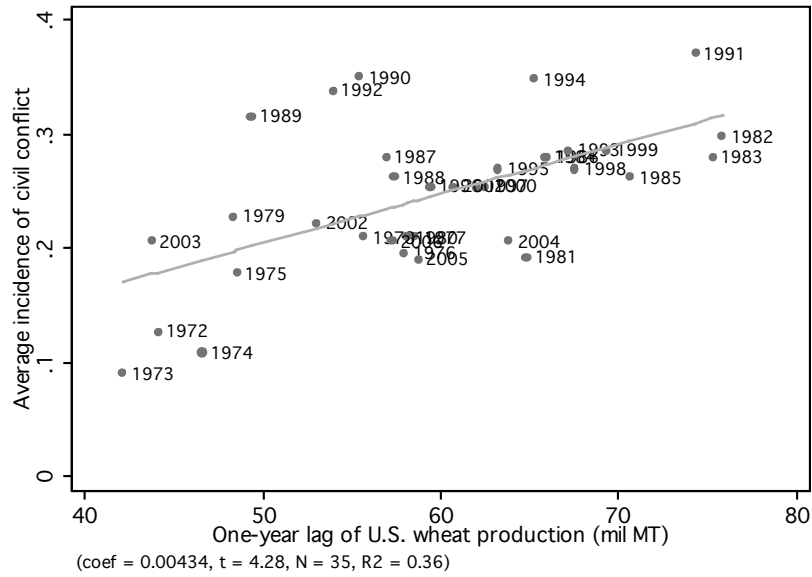


Figure 7: Average Civil Conflict Incidence and Lagged U.S. Wheat Production
– Regular Recipients: $\bar{D}_{ir} \geq 0.29$

Table 1: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.
<u>Conflicts with 25+ battle deaths (from UCDP/PRIO):</u>			
Any Conflict	4240	0.219	0.413
Intra State Conflict	4240	0.176	0.381
Inter State Conflict	4240	0.028	0.165
Onset of Intra State Conflict (all observations)	4210	0.033	0.178
Onset of Intra State Conflict (observations that follow no-conflict only)	3509	0.039	0.195
Offset of Intra State Conflict (observations that follow conflict only)	707	0.188	0.391
<u>Conflicts with 1000+ battle deaths (from COW):</u>			
Any Conflict	4240	0.155	0.362
Intra State Conflict	4240	0.142	0.350
Inter State Conflict	4240	0.022	0.146
U.S. Wheat Aid (1000 MT)	4240	27.63	116.38
Frequency of Receiving Any U.S Food Aid	4240	0.351	0.301
Lagged U.S. Wheat Production (1000 MT)	4240	59650	8610
Recipient Cereal Production (1000 MT)	4240	8500	37577

Notes: An observation is a country and year. The sample includes 134 non-OECD countries for the years 1972-2006.

Table 2: The Effect of Food Aid on Conflict

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable (Panels A, B and C):	Parsimonious Specifications			Baseline Specification			
	Any Conflict	Any Conflict	Any Conflict	Any Conflict	Any Conflict	Intra State	Inter State
A. OLS Estimates							
U.S. Wheat Aid (1000 MT)	-0.00009 (0.00016)	-0.00009 (0.00016)	-0.00013 (0.00016)	-0.00015 (0.00016)	-0.00014 (0.00015)	-0.00005 (0.00015)	-0.00013 (0.00004)
R-squared	0.521	0.523	0.538	0.543	0.549	0.526	0.383
B. Reduced Form Estimates (x 1000)**							
Lag U.S. Wheat Production (1000 MT) x Avg Prob of Any U.S. Food Aid	0.00924 (0.00256)	0.00945 (0.00255)	0.00895 (0.00265)	0.01067 (0.00314)	0.01106 (0.00301)	0.01004 (0.00296)	-0.00079 (0.00087)
R-squared	0.523	0.525	0.540	0.546	0.551	0.529	0.379
C. 2SLS Estimates							
U.S. Wheat Aid (1000 MT)	0.00399 (0.00143)	0.00403 (0.00135)	0.00391 (0.00134)	0.00474 (0.00159)	0.00465 (0.00156)	0.00411 (0.00160)	-0.00032 (0.00036)
LIML point estimate	0.00373	0.00385	0.00364	0.00475	0.00453	0.00411	-0.00032
p-value	0.000	0.001	0.003	0.000	0.000	0.001	0.427
CLR 95% Interval [Lower bound, Upper bound]	[0.00197, 0.00730]	[0.00205, 0.00758]	[0.00187, 0.00731]	[0.00246, 0.01080]	[0.00241, 0.00962]	[0.00216, 0.00871]	[-0.00130, 0.000536]
D. First Stage Estimates							
U.S. Wheat Aid (1000 MT)							
Dependent Variable (Panel D):							
Lag U.S. Wheat Production (1000 MT) x Avg Prob of Any U.S. Food Aid	0.00248 (0.00074)	0.00245 (0.00072)	0.00246 (0.00073)	0.00224 (0.00066)	0.00244 (0.00082)	0.00244 (0.00082)	0.00244 (0.00082)
First Stage F-Statistic	20.11	19.61	13.84	15.94	15.94	15.94	15.94
Controls (for all panels):							
Country FE	Y	Y	Y	Y	Y	Y	Y
Region-Year FE	Y	Y	Y	Y	Y	Y	Y
Monthly Recipient Temperature and Precipitation	N	Y	Y	Y	Y	Y	Y
Avg Recipient Cereal Imports x Year FE	N	N	Y	Y	Y	Y	Y
Avg Recipient Cereal Production x Year FE	N	N	Y	Y	Y	Y	Y
Avg U.S. Military Aid x Year FE	N	N	N	Y	Y	Y	Y
Avg U.S. Economic Aid (Net of Food Aid) x Year FE	N	N	N	Y	Y	Y	Y
Recipient Weather Variables x Avg Prob of Any U.S. Food Aid	N	N	N	N	Y	Y	Y
Observations (for all panels)	4240	4240	4240	4240	4240	4240	4240

Notes: An observation is a country and a year. The sample includes 134 non-OECD countries for the years 1972-2006. The controls included are indicated in the table by Y (yes) or N (no). Standard errors are clustered at the country level. **In panel B, the point estimates and standard errors are multiplied by 1000 for presentation purposes. In Panel C, we also report the LIML estimates, p-values, and Conditional Likelihood Ratio (CLR) 95% confidence intervals. In panel D, we also report first-stage Cragg-Donald F-statistics. The Stock-Yogo critical values (with a 5% significance level) are 8.96 and 16.38 for 15 and 10% maximum bias in size, respectively.

Table 3: The Effect of U.S. Wheat Production on Past Wheat Aid and Recipient Cereal Production

	Dependent Variable:			
	U.S. Wheat Aid (1000 MT) (1)	U.S. Wheat Aid (1000 MT), Period $t-2$ (2)	U.S. Wheat Aid (1000 MT), Period $t-3$ (3)	Recipient Cereal Production (1000 MT) (4)
Lag U.S. Wheat Production (1000 MT) x	0.00244	-0.00036	-0.00057	-0.0610
Avg Prob of Any U.S. Food Aid	(0.00082)	(0.00076)	(0.00095)	(0.0809)
Observations	4240	3972	3838	4240
R-squared	0.528	0.546	0.556	0.955

Notes: OLS estimates are reported. The unit of observation is a country in a year. The sample includes 134 non-OECD countries for the years 1972-2006. All regressions include the full set of baseline controls - see Table 2 columns (5)-(7) for a full list. Coefficients are reported, with standard errors clustered at the country level.

Table 4: The Effect of Food Aid – Robustness to Additional Controls

	Dependent Variable: Incidence of Civil Conflict					
	Baseline (1)	(2)	(3)	(4)	(5)	(6)
U.S. Wheat Aid (1000 MT)	0.00312 (0.00122)	0.00325 (0.00132)	0.00269 (0.00109)	0.00311 (0.00117)	0.00264 (0.00102)	0.00247 (0.00093)
<i>Controls:</i>						
All Baseline controls	Y	Y	Y	Y	Y	Y
Lagged ln real per capita GDP	N	Y	N	N	N	Y
Avg ln real per capita GDP x Year FE	N	N	Y	N	N	Y
Avg Polity Score x Year FE	N	N	N	Y	N	Y
Cold War x Avg U.S. Food Aid	N	N	N	N	Y	Y
LIML point estimate	0.00312	0.00325	0.00269	0.00311	0.00264	0.00247
p-value	0.000	0.000	0.001	0.000	0.000	0.000
CLR 95% Interval [Lower bound, Upper bound]	[0.00139, 0.00692]	[0.00144, 0.00744]	[0.00106, 0.00603]	[0.00140, 0.00679]	[0.00134, 0.00478]	[0.00123, 0.00447]
First Stage F-Statistic	15.92	14.85	16.55	16.44	28.87	30.71
Observations	3482	3482	3482	3482	3482	3482

Notes: 2SLS estimates are reported. The sample includes 113 non-OECD countries for the years 1972-2006. The smaller number of countries (from 134) is due to missing data for the additional control variables. U.S. Wheat Aid in year t is instrumented by U.S. wheat production in year $t-1$ x the frequency of receiving any U.S. food aid during 1972-2006. All regressions include the full set of baseline controls - see Table 2 columns (5)-(7) for the list. Y (yes) or N (no) indicate the inclusion or exclusion of additional controls. Standard errors are clustered at the country level. The table also reports LIML point estimates (and p-values), as well as Conditional Likelihood Ratio 95% confidence intervals. The Cragg-Donald first stage F-statistics are also reported at the bottom of the table. The Stock-Yogo critical values (with a 5% significance level) are 8.96 and 16.38 for 15 and 10% maximum bias in size, respectively.

Table 5: The Effect of Food Aid – Robustness to Alternative Specifications

	Dependent Variable: Incidence of Civil Conflict						
	Baseline IV: using lagged U.S. Wheat Prod x Avg Food Aid Prob	MSS (2004): country- specific time trends	MSS (2004): time trends	IV using lagged U.S. wheat production only (uninteracted)	Controlling for lagged conflict	Normalizing aid by population	Taking natural logs of aid and production
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
U.S. Wheat Aid (1,000 MT)	0.00411 (0.00160)	0.00399 (0.00171)	0.00325 (0.00137)	0.00362 (0.00196)	0.00278 (0.00106)	0.0600 (0.0236)	0.256 (0.0785)
Standardized beta coefficient	1.254	1.058	0.899	1.124	0.848	1.159	1.185
Controls:							
Country FE	Y	Y	Y	Y	Y	Y	Y
Region-Year FE	Y	N	N	N	Y	Y	Y
Lag Dependent Variable	N	N	N	N	Y	N	N
Time Trend	N	Y	N	N	N	N	N
Country-Specific Time Trends	N	N	Y	Y	N	N	N
Observations	4240	4240	4240	4240	4205	4208	4240

Notes: 2SLS estimates are reported. The sample includes 134 non-OECD countries for the years 1972-2006. In columns (1)-(3) and (5)-(7), U.S. Wheat Aid in year t is instrumented by U.S. wheat production in year $t-1$ x the frequency of receiving any U.S. food aid during 1972-2006. In column (4), U.S. Wheat Aid is instrumented by U.S. wheat production in year $t-1$. The regressions in columns (1) and (5)-(7) include the full set of baseline controls. See Table 2, columns (5)-(7) for a list. The regressions in columns (2)-(4) include the same controls, but with each country-level characteristic interacted with a linear time trend instead of year FEs. Y (yes) or N (no) indicate the inclusion or exclusion of the different control variables. Coefficients are reported with standard errors clustered at the country level. The table also reports standardized 'beta coefficients'.

Table 6: The Effect of Food Aid on Conflict Onset and Duration

	Dependent variable:				
	Civil war onset		Civil war offset		
	Collier and Hoefler (2004) (1)	Fearon and Laitin (2003) (2)	Logistic Discrete Time Hazard Model (3) (4) (5)		
U.S. Wheat Aid (1,000 MT)	0.00231 (0.00148)	0.00183 (0.00080)	-0.00483 (0.00199)	-0.00614 (0.00203)	-0.00501 (0.00213)
Controls:					
Lagged civil conflict incidence	N	Y	n/a	n/a	n/a
Third-order poly of duration	n/a	n/a	Y	Y	Y
Time-invariant controls	n/a	n/a	N	Y	Y
Region fixed effects	n/a	n/a	N	N	Y
Observations	3,509	4,210	560	558	707

Notes: In all specifications, U.S. Wheat Aid in year t is instrumented by U.S. wheat production in year $t-1 \times$ the probability of receiving any U.S. food aid between 1972 and 2006. In columns (1) and (2), the dependent variable is an indicator that equals one for the onset of a civil war. Both specifications include the full set of baseline covariates. See columns (5)-(7) of Table 2 for a list of these variables. In column (1), observations that are periods of continued conflict are omitted from the sample. The regression in column (2) includes a one-year lag in the incidence of civil conflict as an additional control variable and uses the full sample. Columns (3)-(5) estimate a discrete time hazard model for the incidence of civil war offset. In this setting, survival is continued conflict. The coefficients reported are marginal effects evaluated at means. The control function approach is used to generate IV estimates for the hazard models.

Table 7: The Effect of Food Aid on Small- and Large-Scale Conflicts

	Dependent Variable: Incidence of Conflict					
	> 25 Combat Deaths (UCDP/PRIO)			> 1,000 Combat Deaths (COW)		
	Any	Intra State	Inter State	Any	Intra State	Inter State
	(1)	(2)	(3)	(4)	(5)	(6)
U.S. Wheat Aid (1,000 MT)	0.00453 (0.00174)	0.00411 (0.00160)	-0.00032 (0.00036)	0.00252 (0.00112)	0.00221 (0.00112)	0.00001 (0.00047)
Standardized beta coefficient	1.275	1.254	-0.228	0.809	0.735	0.009
Observations	4240	4240	4240	4240	4240	4240

Notes: 2SLS estimates are reported. The sample includes 134 non-OECD countries for the years 1972-2006. U.S. Wheat Aid in year t is instrumented by U.S. wheat production in year $t-1$ x the average probability of receiving any U.S. food aid during 1972-2006. All regressions include the full set of baseline controls - see Table 2 columns (5)-(7) for a complete list. Coefficients are reported with standard errors clustered at the country level.

Table 8: The Effect of Food Aid on Other Aid and Recipient Country Wheat Production

	Dependent Variable:									
	U.S.					economic				
	World wheat aid (1000 MT)	World cereal aid (1000 MT)	Non-U.S. wheat aid (1000 MT)	Non-U.S. cereal aid (1000 MT)	U.S. military aid (1000 real USD)	U.S. aid excl. food aid (1000 real USD)	Non-U.S. net ODA (1000 real USD)	Non-U.S. net ODA 2 (1000 real USD)	Recipient wheat production (1000 MT)	Recipient cereals production (1000 MT)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean of Dep. Variable	40.29	60.94	12.66	18.01	39776	59326	430108	408165	2048.3	8608.7
U.S. Wheat Aid (1000 MT)	1.129	1.261	0.129	0.091	152.7	-251.8	752.2	1163	-13.63	-24.98
(Mean = 27.63)	(0.131)	(0.288)	(0.131)	(0.198)	(1258.0)	(672.2)	(1571.0)	(1390)	(13.22)	(33.63)
Observations	4240	4240	4240	4240	4240	4240	4240	4240	4240	4240

Notes: 2SLS estimates are reported. The sample includes 134 non-OECD countries for the years 1972-2006. U.S. Wheat Aid in year t is instrumented by U.S. wheat production in year $t-1$ x the probability of receiving any U.S. food aid during 1972-2006. All regressions control for the full set of baseline controls - see Table 2 columns (5)-(7) for a full list. Coefficients are reported with standard errors clustered at the country level.

Table 9: The Heterogeneous Effects of Food Aid on Conflict

	Dependent Variable: Incidence of Civil Conflict								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
U.S. Wheat Aid (1000 MT)	0.00411 (0.00160)	0.00514 (0.00290)	0.00277 (0.00161)	0.00427 (0.00280)	0.00404 (0.00241)	0.00628 (0.00257)	0.00445 (0.00186)	0.00507 (0.00181)	0.00372 (0.00283)
U.S. Wheat Aid x Indicator for: Low Income		-0.00272 (0.00321)							0.00153 (0.00406)
Democratic (High Polity)			0.00085 (0.00239)						0.00026 (0.00135)
Civilian Government				-0.00140 (0.00323)					-0.00102 (0.00143)
Low Polarization					-0.00056 (0.00321)				-0.00106 (0.00241)
Low Ethnic Fractionalization						-0.00480 (0.00285)			-0.00208 (0.00196)
High Road Density							-0.00322 (0.00234)		-0.00275 (0.00132)
Cold War Years								-0.00220 (0.00155)	-0.00048 (0.00155)
Observations	4240	4085	3664	4240	2917	3358	3437	4240	2499
U.S. Wheat Aid + (U.S. Wheat Aid x Indicator) p-value		0.00242 0.115	0.00362 0.079	0.00287 0.043	0.00348 0.112	0.00148 0.087	0.00123 0.504	0.00287 0.017	0.00097** 0.750

Notes: 2SLS estimates are reported. The baseline sample in column (1) includes 134 non-OECD countries for the years 1972-2006. The sample size in columns (2)-(9) varies according to data availability. U.S. Wheat Aid in year t and the interaction of wheat aid and the indicator variable are instrumented with U.S. wheat production in year $t-1$ x the probability of receiving any U.S. food aid during 1972-2006, and the triple interaction of the indicator x U.S. wheat production in year $t-1$ x the probability of receiving any U.S. food aid during 1972-2006. The regressions also include the relevant double interaction terms. All regressions control for the full set of baseline controls - see Table 2 columns (5)-(7) for the full list. Coefficients are reported with standard errors clustered at the country level. The joint estimate for U.S. wheat aid + U.S. wheat aid x indicator variable, and their p-values, are reported in the final row of the table. **In column (9), the joint estimate refers to the estimate for U.S. wheat aid + U.S. wheat aid x high road density.