Taylor & Francis
Taylor & Francis Group

Check for updates

The Impact of Food Prices on Conflict Revisited

Jasmien De Winne and Gert Peersman

Department of Economics, Ghent University, Gent, Belgium

ABSTRACT
Studies that examine the impact of food prices on conflict usually assume that (all) changes in international food prices are exogenous shocks for individual countries or local areas. By isolating strictly exogenous shifts in global food commodity prices, we show that this assumption could seriously distort estimations of the impact on conflict in African regions. Specifically, we show that increases in food prices that are caused by harvest shocks outside Africa raise conflict significantly, whereas a "naive" regression of conflict on international food prices uncovers an inverse relationship. We also find that higher food prices lead to more conflict in regions with more agricultural production. Again, we document that failing to account for exogenous price changes exhibits a considerable bias in the impact. In addition, we show that the conventional approach to evaluate such effects; that is, estimations that include time fixed effects, ignores an important positive baseline effect that is common for all regions. Supplementary materials for this article are available online.

1. Introduction

Throughout history, various violent events happened in times of high food prices. The surges in food commodity prices in 2006–2008 and 2010–2011 spurred interest to formally investigate this relationship. While theory provides arguments for both a positive and negative relationship, also the empirical evidence has so far been mixed. For example, Brückner and Ciccone (2010) and Berman and Couttenier (2015) find that higher food prices decrease the risk of violent events. In contrast, Bellemare (2015) and Raleigh, Choi, and Kniveton (2015) concluded that higher prices induce more conflict, while Bazzi and Blattman (2014) found no robust relationship.

Existing empirical studies examining the causal effect of food prices on conflict face several challenges and have some shortcomings. A first issue is endogeneity of food prices. A potential source of endogeneity is reverse causality; that is, conflicts could also influence food prices. To address this problem, most studies use global food commodity prices in the estimations and assume that local conflicts do not affect global prices (e.g., Hendrix and Haggard 2015). Although this assumption is plausible, it still ignores another—and probably more important—source of endogeneity: local conflicts and global food prices may both be determined by a third variable such as global economic activity or oil prices. A worldwide expansion can, for example, result in higher food prices. At the same time, it can affect conflict incidence through (nonfood) trade, income, remittances, or aid flows, which could bias inference and causal interpretations. Such endogenous food price increases are clearly different from price shocks that are unrelated to economic development, for example, caused by failed harvests.

Several studies explicitly or implicitly address the endogeneity problem by including time fixed effects in the estimations, which could, for example, capture the global business cycle. Since international food prices only vary over time, these studies multiply changes in global prices by a local volume indicator to achieve identification. For example, Besley and Persson (2008) and Bazzi and Blattman (2014) weighed food commodity prices by food export shares of countries, and Dube and Vargas (2013) and McGuirk and Burke (2017) by food production at more disaggregated levels. On one hand, this approach still does not solve potential endogeneity problems. For example, it is possible (and likely) that food producers and exporters are systematically more exposed to the global business cycle or changes in oil prices that trigger food price shifts. Again, this could distort inference. On the other hand, by using time fixed effects, these studies essentially estimate relative effects; that is, the estimated coefficients measure to what extent producers or exporters experience more/less conflict when food prices increase compared to other areas or countries. This approach does hence not measure the incidence of conflict that is common for all areas when international food prices rise, which is absorbed by the time fixed effects. Such common effects, which we label as "baseline effects," may be nonnegligible and could offset or even reverse the measured supplementary effects for producers or exporters. The use of time fixed effects can be compared with a difference-in-difference analysis. At the end of the day, it does not inform us about the total effects of changes in food prices on conflict in a given area.

In this article, we investigate whether these issues matter for estimations of the effects of changes in food commodity prices

🔵 Supplementary materials for this article are available online. Please go to *www.tandfonline.com/UBES*.

on local conflict in Africa. To do this, we use two instrumental variables that represent exogenous fluctuations in global food commodity markets as proposed in De Winne and Peersman (2016). Both instruments reflect harvest shocks that are unrelated to economic developments and occurred outside Africa, thereby avoiding possible reverse causality and endogeneity effects. The first instrument is a generic (quarterly) series of unanticipated global harvest shocks. The shocks are prediction errors of a composite production index that aggregates the global harvests of the four most important staple food commodities (corn, wheat, rice, and soybeans). We exclude the harvests of African countries when constructing the index. The second instrument is a (quarterly) dummy variable indicator of four news shocks about global (non-African) harvest volumes that have been identified based on FAO reports, newspaper articles, and several other sources.

We first compare the effects of international food commodity price changes on conflict in Africa that are estimated with the instrumental variables; that is, price changes unambiguously caused by exogenous supply shocks, with a "naive" regression of conflict on international food prices. Since our main contribution is methodological, we try to follow the existing studies as much as possible. In line with recent advances in the literature (e.g., McGuirk and Burke 2017), we use geo-referenced, subnational conflict data and consider two types of conflicts: factor conflict (large-scale battles for territory) and output conflict (smaller-scale conflict such as riots and protests). The violent events are converted to a panel of 10,678 African equally sized cells. However, in contrast to the existing studies we aggregate the data at a quarterly instead of annual frequency, and we use local projection methods to estimate the effects. The advantage of this approach is that it allows us to examine the dynamics of the food-conflict nexus in more detail, which is also a contribution of our study.

The estimations reveal that a rise in international food commodity prices caused by a truly exogenous harvest shock in other regions of the world increases both types of conflict. This finding contrasts with the results from the naive regression of conflict on food prices, which uncovers an inverse relationship. Accordingly, the isolation of exogenous changes in food prices turns out to be very important for the results. Furthermore, for both conflict types, the bulk of the rise in conflict takes place more than four quarters after the initial price increase, while the effect is larger and more persistent over time for output conflict.

In a next step, we evaluate whether the impact on conflict is larger or smaller in regions with more agricultural production. Again, we compare a naive estimation, which now includes time fixed effects, with an instrumental variables estimation. However, since our instruments can be considered as strictly exogenous, we also estimate a third specification without time fixed effects to measure both the baseline impact and the additional effect for food producing regions. The results show that an exogenous price increase leads to more conflict in regions with more agriculture. Once more, we show that failing to account for exogenous price changes exhibits a substantial bias in the estimated effects. For factor conflict incidence, there is even a sign switch; that is, according to the naive estimation with time fixed effects, conflict decreases in regions with more food production when food prices increase. In fact, a negative

relationship for food producers in Africa is what most existing studies find, but this appears to be misleading. Finally, we find a significant positive baseline effect of food price increases in all regions. For regions with an average amount of agriculture, the baseline effect turns out to be larger than the influence of their production share. Put differently, the use of time fixed effects results in an underestimation of the total effect of food price changes on conflict in these regions by more than 50%.

In sum, our results show that it is not sufficient to use international food prices to avoid endogeneity problems when examining the relationship between food prices and conflict. It is crucial to isolate strictly exogenous changes in (international) food prices. Earlier studies in the same spirit are Dube and Vargas (2013) and Bellemare (2015). Dube and Vargas (2013) instrument coffee prices with export volumes of the three major exporters to estimate the impact on armed conflict in Colombian municipalities, while Bellemare (2015) used the amount of global natural disasters as an instrument to estimate the causal effects of food price changes on worldwide social unrest. Although we believe this approach is the way forward, we discuss some relevant drawbacks of both instruments (see Section 2)—basically that there are still potential endogeneity problems—and we argue that it is better to use unanticipated global harvest shocks as an instrumental variable.

Overall, we find a robust strong positive impact of food price increases on conflict, which is greater in food producing regions. This finding is consistent with the conclusion of Smith (2014), Bellemare (2015), Hendrix and Haggard (2015), and Raleigh, Choi, and Kniveton (2015) based on average price shocks, as well as Besley and Persson (2008) and Arezki and Brückner (2014), who documented a positive relationship for countries with higher (net) export shares of food commodities. From a theoretical point of view, our results are in line with so-called predation and deprivation effects, and at odds with opportunity cost effects in food producing areas.

Section 2 provides an overview of the main theories and empirical findings in the literature. We also discuss the caveats of existing empirical studies. Section 3 describes the construction of the instrumental variables, Section 4 the conflict data that are used for the estimations, while Section 5 presents the results for the impact of food prices on conflict. Section 6 examines whether the impact is different in regions with more agriculture. Finally, Section 7 concludes.

2. Existing Literature

We first discuss the main explanations why higher food prices could affect conflict. We then provide an overview of the existing empirical studies and highlight the caveats of this literature. Notice that we limit the review solely to the link between food prices and conflict, which fits within a broader literature analyzing the causes of civil war and other forms of conflict, in particular the role of economic conditions and income shocks. Specifically, several studies have used changes in food commodity prices as an instrument for income shocks to study the relationship of economic developments with conflict. For an overview of the broader literature, we refer to Blattman and Miguel (2010).

2.1. Theories

According to theory, the impact of food prices on conflict is ambiguous. There are two main reasons why higher food prices can reduce conflict. First, if food prices increase, there is a higher opportunity cost of insurrection for farmers since higher wages and revenues make it less appealing to abandon work. This is the so-called opportunity cost effect (Dube and Vargas 2013; Bazzi and Blattman 2014). Second, even though this channel is probably less important for food than for more easily taxable commodities such as minerals and oil, higher commodity prices can increase state revenues and hence also the capacity of the state to prevent, curb or resolve conflict (Besley and Persson 2010).

On the other hand, there are two major explanations why higher food prices can result in more violent events. First, higher prices increase the value of the appropriable surplus, which could lead to more conflicts. This is the so-called predation effect (Besley and Persson 2008) or rapacity effect (Dube and Vargas 2013). Second, consumers can feel relatively deprived, which can arise from comparisons over time or comparisons with other individuals. According to the relative deprivation hypothesis, unfulfilled material expectations and food insecurity cause anger, ultimately leading to public unrest (Hendrix and Haggard 2015). This hypothesis is not often mentioned in the literature, but it is in line with a long list of episodes of high food prices coinciding with public unrest, ranging from ancient Rome, when "bread and circuses" were needed to appease the people, to the French Revolution, the flour riots in the 19th century in the United States and food riots in 2007–2008 in Africa (see, e.g., Bellemare 2015).

2.2. Empirical Studies

The existing empirical evidence is clearly inconclusive. The online appendix of this article provides an overview of 13 recent studies that have examined the relationship between food prices and conflict. While some of these studies also focus on other (nonfood) commodities or variables, all of them estimate a model with a measure of violent events as the dependent variable and food prices as one of the explanatory variables. Four studies find that higher food prices lead to less conflict, while six studies show that higher food prices cause more conflict. Two studies find mixed evidence and one article finds no significant link.

These seemingly opposing findings make more sense when grouped in a particular way. On one hand, most studies that focus on food producers to achieve identification; that is, international food prices weighted by production or export shares, conclude that higher food prices reduce conflict (Brückner and Ciccone 2010; Dube and Vargas 2013; Berman and Couttenier 2015; Fjelde 2015; Janus and Riera-Crichton 2015). However, this finding is not entirely robust: Besley and Persson (2008) and Arezki and Brückner (2014) found that even when focusing on (net) export-weighted prices, higher food prices cause more conflict. McGuirk and Burke (2017) showed that higher prices result in more output conflict but less factor conflict in food-producing cells. Bazzi and Blattman (2014) found no link between food producer prices and conflict incidence.

On the other hand, the studies that weigh food prices by its import or consumption share always find that higher food prices result in more conflict (Besley and Persson 2008; Janus and Riera-Crichton 2015; McGuirk and Burke 2017). This also applies to studies that do not distinguish between producers and consumers. To measure food price changes, these studies use, respectively, food prices instrumented by natural disasters (Bellemare 2015), food prices instrumented by both international prices and (local) weather variables (Smith 2014; Raleigh, Choi, and Kniveton 2015) or international food prices directly (Hendrix and Haggard 2015).

2.3. Caveats of Existing Studies

There are two important caveats that apply to most existing studies: endogeneity and/or the absence of a baseline effect when the impact of changes in food prices on conflict is estimated for producers or consumers. Specifically, there may be two sources of endogeneity. First, there could be reverse causality running from conflict to food prices. For example, conflicts could destroy crops, resulting in higher food prices. The cause and effect of the food price-conflict relationship are then no longer well defined. For this reason, most studies use international food commodity prices in the estimations (directly or as an instrument for local food prices) and assume that there is no causal effect of local conflicts on global food prices. Especially for African countries, which are typically small food producers, this assumption is appealing (e.g., Smith 2014; Hendrix and Haggard 2015; Raleigh, Choi, and Kniveton 2015). Several country studies also perform a robustness check in which they exclude countries with levels of production above a certain threshold (e.g., Arezki and Brückner 2014; Bazzi and Blattman 2014).

The use of international food commodity prices as an instrument for local price changes is indeed a plausible approach to avoid reverse causality problems. However, a second endogeneity problem arises if both international food prices and conflict are determined by a third variable such as global economic activity or changes in oil prices. Shocks to the global business cycle could, for example, simultaneously trigger food price changes and affect conflict through trade, remittances, or aid flows. The consequences of higher food prices for consumers could then, for example, be (partly) compensated by a rise in income due to the global expansion. More generally, the effects of such endogenous increases in food prices may be very different from the repercussions of higher prices that are triggered by exogenous supply shocks such as failed harvests. If this is the case, inference and causal interpretations are distorted.

Several studies include time fixed effects in the estimations to control for changes in conflict incidence that are related to the global business cycle or other common shocks (e.g., Besley and Persson 2008; Brückner and Ciccone 2010; Dube and Vargas 2013; Arezki and Brückner 2014; Bazzi and Blattman 2014; Berman and Couttenier 2015; Fjelde 2015; Janus and Riera-Crichton 2015; McGuirk and Burke 2017). The use of time fixed effects essentially wipes out any effect that is common for all countries. Since this also applies to shifts in (common) international food prices, these studies resort to a difference-in-difference strategy. In particular, they multiply international

food prices with time-invariant local food production, export or import shares and explore the cross-section variation of food price changes to assess the impact on conflict. This approach does, however, not fully solve endogeneity problems. For example, when countries or regions with higher production, export or import shares are systematically also more exposed to the common shocks that trigger food price shifts, which is plausible, causal interpretations may still be biased.

Another drawback of time fixed effects is that the estimated coefficients represent the additional effect of higher food prices in areas with more production, export or import. However, this additional effect does not tell us anything about the overall effect of food prices on conflict in an area. For example, if an increase in global food commodity prices causes more conflict in all African countries, but slightly less so in food-producing countries, the difference-in-difference estimator *only* provides the latter piece of information. These common effects, which we label as a baseline effect, can be triggered directly, but also indirectly via spillovers across regions or countries. We will examine the relevance of baseline effects explicitly in Section 6.

Overall, the use of international food commodity prices to estimate causal effects of food price changes on conflict suffers from potential endogeneity problems. The only way to address this is the isolation of food price shifts that are strictly exogenous. In this regard, Dube and Vargas (2013) used the (annual) export volumes of the three major coffee exporters as an instrument for (annual) changes in coffee prices, to estimate the impact on armed conflict in Colombian municipalities. Notice, however, that export volumes could also be influenced by global business cycle fluctuations or other common shocks. Moreover, food production could endogenously respond to changes in economic conditions within the one-year horizon (De Winne and Peersman 2016). As an alternative, Bellemare (2015) used the amount of global natural disasters as an instrument to estimate the impact of food price changes on worldwide social unrest. Natural disasters are indeed exogenous events that could cause changes in food prices. However, natural disasters typically also have direct effects on the economy and conflict (e.g., migration flows) that are unrelated to the food price shift, which could again distort inference. In fact, there is a growing literature that finds an impact of climate on conflict (e.g., Miguel, Satyanath, and Sergenti 2004; Burke, Hsiang, and Miguel 2015).

3. Instrumental Variables for Exogenous Food Price Changes

We propose two instruments to isolate exogenous changes in international food commodity prices that are unrelated to the economy: a series of unexpected global harvest shocks and a set of narratively identified news shocks about global food supply.

3.1. Unanticipated Harvest Shocks

The first instrumental variable is a quarterly series of unexpected harvest shocks that occurred outside the African continent. The underlying idea is that unexpected variations in harvests that are sufficiently large to affect global supply of food likely trigger significant shifts in international food commodity

prices, which fulfills the instrument relevance condition. On the other hand, harvest volumes can in principle not (endogenously) respond to changes in the state of the economy within one quarter, which fulfills the exogeneity condition for estimations based on quarterly data. Specifically, for the staple food commodities that we consider, there is a time lag of at least one quarter between the planting and harvesting seasons (De Winne and Peersman 2016). If farmers alter their planting volumes in response to changing economic conditions, this could only have an impact on the harvest volumes at longer quarterly horizons. In any case, the possible influence of food producers on the volumes during the quarter of the harvest itself is plausibly meager relative to variation induced by other factors such as weather conditions, pests or diseases affecting crops. For example, it is not realistic that farmers increase food production significantly by raising fertilization activity during the harvesting quarter in response to an improvement of economic conditions. In fact, several studies have shown that in-season fertilization strategies are inefficient and often even counterproductive for the staples that we consider (see De Winne and Peersman 2016, for a more elaborate discussion). Finally, by only considering harvest shocks outside the African continent, we avoid reverse-causality and the possibility that the instrument captures for example direct effects of weather variation on conflict in African regions.

To derive the instrument, we first construct a quarterly index of global food production that is based on four crop types: wheat, maize, rice and soybeans. To do so, we elaborate on De Winne and Peersman (2016, 2018). More precisely, the Food and Agriculture Organization (FAO) publishes annual harvest data for each of the four major staples for 192 countries since the 1960s. These four crops, which are storable and traded in integrated global markets, account for 75% of worldwide calorie production and characterize developments in global food markets reasonably well (Roberts and Schlenker 2013). De Winne and Peersman (2016) combine the annual harvest data of each individual country with that country's planting and harvesting calendars for each of the four crops, to allocate the harvest volumes to a specific quarter. Harvests are only allocated if the planting season was at least one quarter earlier. Since most countries have only one relatively short harvest season for each crop (i.e., a few months) and the delay between planting and harvesting varies between 3 and 10 months, it is possible to assign two-thirds of world harvests to a specific quarter. The harvests are then aggregated across crops and countries using calorie weights into one global quarterly index. In this article, we follow the same approach, but we exclude harvests of African countries from the production index to ensure that the shock occurred elsewhere.

In the next step, we use this index to estimate unexpected changes in global food production (ε_t). In essence, the shocks are prediction errors of the harvest volumes conditional on past harvests and a set of relevant information variables that may influence harvests. Specifically, we estimate the following equation:

$$FQ_t = \beta_0 + \beta_1(L) X_{t-1} + \varepsilon_t. \qquad (1)$$

FQ_t is the seasonally adjusted quarterly index of global food production excluding Africa. X_{t-1} is a vector of control variables that may affect global harvest volumes with a lag of one or more
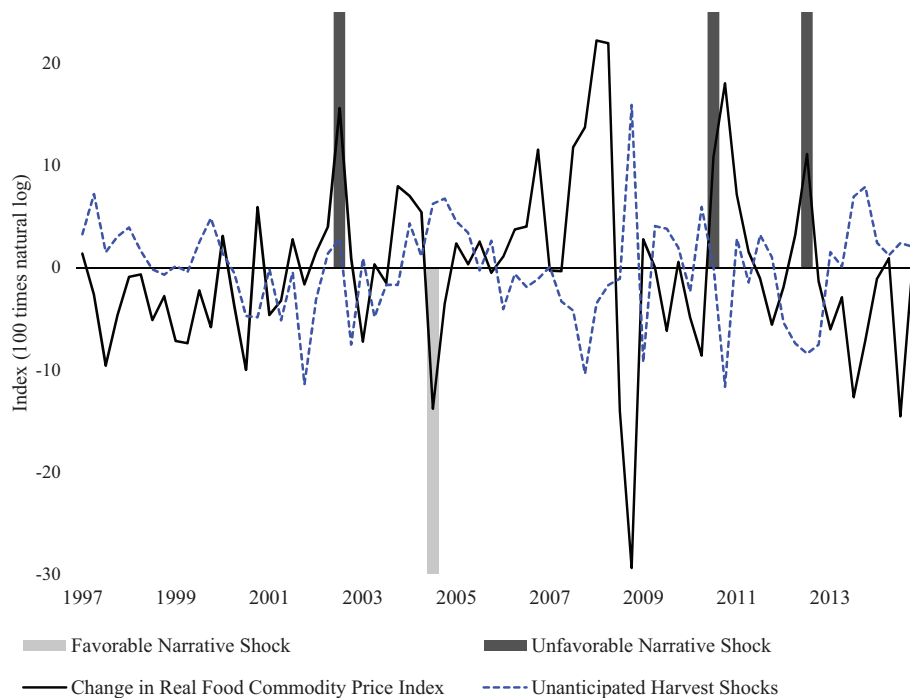
Figure 1. Real food prices, unanticipated harvest shocks, and narrative food supply news shocks. NOTES: The real food commodity price index is a (trend) production-weighted aggregate of the price series of corn, wheat, rice and soybeans made available by the IMF, deflated with U.S. consumer prices. The construction of the unanticipated harvest shocks and the narrative food supply news shocks is explained in Section 3.

quarters: an index of real food commodity prices (based on the same four crops), real oil prices, world industrial production and lags of the food production index. We include five lags of the control variables ($L = 5$). These variables should capture possible influences of economic conditions on food production. Oil prices are included because food commodities can be considered as a substitute for crude oil to produce refined energy products, while oil is used in the production and distribution of food commodities. All variables enter in log-levels to allow for possible cointegration relationships between the variables, although the results are robust when we use first differences. A detailed description of the data can be found in the online appendix. Equation (1) is estimated for the largest available sample period at the time we collected the data (1962Q2–2014Q4). If we assume that the information sets of local farmers are no greater than Equation (1), the residuals (ε_t) of this equation can be considered as unanticipated harvest shocks that occurred outside Africa. Figure 1 displays the shocks, which is our first instrument for changes in real global food commodity prices.

In sum, by excluding African harvests from the index, the issue of reverse causality is addressed. The second endogeneity issue is also addressed since unexpected changes in the index are shocks that are unrelated to economic developments due to the time lag between planting and harvesting of at least one quarter. Note that this also applies to *expected* economic activity since an arbitrage condition ensures that changes in expected future prices also shift spot prices of storable commodities (Pindyck 1993), which are included in Equation (1). It is also worth mentioning that, in the online appendix, we report several robustness checks for alternative specifications to obtain the instrumental variable. In particular, a worry could be that the harvest shocks are systematically correlated with

global weather phenomena such as El Niño, while these weather phenomena could also affect conflict directly. If so, we may be measuring the effect of local weather on conflict instead of food price changes on conflict. We therefore include different local weather variables as control variables in a robustness check, but this does not affect the results. Another concern could be that conflict elsewhere in the world has an effect on food production and, at the same time, influences conflict in Africa. The results, however, remain unchanged when we include measures of international conflict (excluding Africa) in Equation (1). Finally, we document that the results are robust when we restrict the sample to estimate the instrument to 1997Q1–2014Q4, which corresponds more directly to the sample that is used in the second stage.

3.2. Narrative Food Supply News Shocks

The second instrument, which is also borrowed from De Winne and Peersman (2016), is constructed using a narrative identification strategy in the spirit of Hamilton (1983) and Ramey and Shapiro (1998). Based on newspaper articles, FAO reports and disaster databases, De Winne and Peersman (2016) identified a number of historical episodes that can be considered as important news shocks about food supply. Each episode is a major change in food commodity prices that is predominantly caused by an exogenous food market disturbance, and not by another macroeconomic event such as oil or business cycle shocks. It is also unlikely that the shocks had a direct impact on African countries beyond global food prices.

De Winne and Peersman (2016) identified four such shocks for our sample period: three unfavorable shocks in 2002Q3,

2010Q3, and 2012Q3, respectively, and one favorable shock in 2004Q3. In the summer of 2002, droughts in major wheat and coarse grain producing countries (especially Russia and Australia), led to large drops in production. As a consequence, real food commodity prices rose by 9.4%. In 2004Q3, favorable weather conditions resulted in better-than-expected cereal harvests in Europe, China, Brazil, and the United States. Real food prices declined by 6.9% in response to this news. In the summer of 2010, droughts in Russia and Eastern Europe led to a surge in real food prices of 8.6% and 13.5% in the subsequent quarter. Finally, in 2012Q3, droughts in Russia, Eastern Europe, Asia, and the United States caused a decline in global cereal production of 2.4%. Real food commodity prices increased by 7.9% in that quarter. A more detailed description is included in the online appendix. For excerpts from the newspaper articles and reports, we refer to De Winne and Peersman (2016).

The instrument that we construct based on these events is a dummy variable equal to one for unfavorable food market disturbances and minus one for the favorable shock. The narrative shocks are also displayed in Figure 1. The correlation with the harvest shocks is 0.18. The advantage of the narrative method is that we can incorporate a large amount of information. For example, we can ensure that these shocks are not the result of conflict in Africa or anywhere else in the world. The downside is that it requires judgment from the researcher. In the benchmark analysis, we will use both instruments simultaneously. As a robustness check, we will also study the effect of each instrument separately.

4. Conflict Data

To measure conflict, we rely on two highly disaggregated databases, listing individual events that can (almost always) be allocated to a specific day and a specific geographical location (down to the level of individual villages). These two databases have often been used in the literature. They are constructed based on information from various sources: local and international media sources, reports from NGOs and international organizations, research articles, etc. McGuirk and Burke (2017) find an opposite impact of food price changes on what they label as large-scale "factor conflict" and smaller-scale "output conflict." Hence, we also use this labeling and distinguish between both types of conflict in the estimations.

As in McGuirk and Burke (2017), we use the Uppsala Conflict Data Program (UCDP) Georeferenced Event Dataset version 4 (Sundberg and Melander 2013) to measure factor conflict. Since the events are restricted to incidents of lethal violence committed by an organized actor, the scope of this database is rather narrow. Additionally, only those dyads (pair of conflicting parties) are included if the conflict resulted in at least 25 battle deaths. Given that this variable measures larger conflicts, McGuirk and Burke (2017) argued that it is deemed appropriate to capture conflicts associated with the permanent control of land.

To measure output conflict we use the Armed Conflict Location and Event Data Project (ACLED) database version 6 (Raleigh and Dowd 2016). The scope of the database is wide, including various subtypes of conflict. In line with McGuirk and Burke (2017), we retain only two event types: riots and protests, and violence against civilians. These events are more transitory and more likely to capture appropriation of surplus. The UCDP database covers the globe between 1989 and 2014 and the ACLED database covers only African countries between 1997 and 2015. To make useful comparisons between the two types of conflict, we look at the overlapping sample for our benchmark analysis: Africa between 1997 and 2014.

Following Berman and Couttenier (2015), Fjelde (2015), and McGuirk and Burke (2017), we consider subnational units of analysis, defined by a standardized grid structure covering all 54 African countries. The grid has a spatial resolution of 0.5 decimal degrees latitude/longitude (approximately 55 × 55 km at the equator), dividing the continent into 10,678 equally sized cells (Tollefsen, Strand, and Buhaug 2012). However, in contrast to these studies, we consider a quarterly frequency. Since there is large inter-annual variability in food prices and conflict incidence, a quarterly frequency could provide relevant insights on the dynamics of the impact of food prices on conflict. Thus, the cell-quarter is our unit of analysis. The events are transformed into a set of dummy variables indicating whether or not an event took place in a given cell-quarter (conflict incidence). This approach is commonly used in the literature (e.g., Besley and Persson 2008; Smith 2014; Berman and Couttenier 2015; Fjelde 2015; McGuirk and Burke 2017). A caveat of this binary approach is that it discards potentially valuable information concerning the intensity of the conflict. We therefore also report results for the number of total events in a cell in a given quarter (conflict intensity), which is an alternative approach that has been used in the literature (e.g., Dube and Vargas 2013). In the sample period, ACLED output conflict has been twice as common as UCDP factor conflict; that is, the unconditional probability of any event taking place in a given quarter has been 2.36% for output conflict versus 1.18% for factor conflict. For additional descriptive statistics and the evolution of the variables over time, we refer to the online appendix.

5. The Impact of Food Prices on Conflict: New Evidence

5.1. Methodology

For the estimations, we use local projections methods proposed by Jordà (2005). This method has become increasingly popular to study the dynamic effects of economic shocks, such as the effects of government spending (Owyang, Ramey, and Zubairy 2013), banking crises (Teulings and Zubanov 2014), or household debt (Mian, Sufi, and Verner 2017) on the business cycle. Bazzi and Blattman (2014) have raised the issue that a binary indicator of conflict incidence constrains shocks to have the same effect on conflict onset, continuation or ending. In addition, since conflict is a persistent variable, ignoring dynamics could bias the estimations. A solution for these problems is the use of a dynamic model. Another attractive feature of local projections is that, in combination with the higher frequency of our dataset, it allows a detailed examination of the dynamic pattern of conflict over time in response to changes in food prices.

Essentially, a local projection directly estimates the impact of a shock in an exogenous regressor x on the dependent variable y at horizon h after the shock, controlling for some other variables that may influence the dependent variable. In our case, this local projections method entails estimating the following panel model for different horizons h:

$$C_{it+h} = \alpha_{ih} + \beta_h \mathrm{FP}_t + \lambda_h(L) \, C_{it-1} + \psi_h(L) \, \mathrm{FP}_{t-1} + \delta_{ch} \\ \times y_{ct-1} + \gamma_{ih} \times \mathrm{trend}_t + \mu_{it+h}, \qquad (2)$$

where C_{it+h} is either the dummy variable indicating whether conflict took place in cell i in a given quarter (conflict incidence) or the variable counting the number of events (conflict intensity). α_{ih} are cell fixed effects. In the benchmark analysis, we use a (weighted) index of international prices of four crop types (FP_t) as the key exogenous regressor. The four crops (wheat, maize, rice, and soybeans) correspond directly with the instruments (see Section 3.1). The crop prices, made available by the IMF, are representative for the global market and determined by the largest exporter of each commodity. In the robustness section, we show that the results are robust to using a broader index of food commodity prices. As control variables we include five lags of the conflict variable and five lags of the price variable ($\lambda_h(L)$ and $\psi_h(L)$ are polynomials in the lag operator, with $L = 5$). We also allow for a country-specific impact of the natural logarithm of annual national GDP ($\delta_{ch} \times y_{ct}$) lagged by one year. Finally, we include cell-specific trends ($\gamma_{ih} \times \mathrm{trend}_t$) to capture possible time trends of conflict in each cell i. A description of the data can be found in the online appendix.

We estimate Equation (2) in two ways: a "naive" estimation using ordinary least squares (OLS), which assumes that all changes in international food commodity prices are exogenous, and an estimation where we use the harvest shocks and the narrative dummy variable as instruments for FP_t. For the IV estimations we also include five lags of the harvest shocks (ε_t) as additional control variables to satisfy the lead/lag exogeneity condition of the instrumental variables (Stock and Watson 2017). Specifically, even though the presence of serial correlation can be rejected for the full sample period of the harvest shocks (i.e., 1962Q2–2014Q4), Ljung–Box tests suggest that the harvest shocks are weakly (at 10% level) serially correlated up to lag five for the shorter sample (i.e., 1997Q1–2014Q4). We evaluate the strength of the instrument set with the cluster-robust Kleibergen–Paap F-statistic.

Equation (2) is estimated for horizons $h = 0, 1, \ldots, 12$. The coefficient β_h thus measures the effect of a change in food prices at time t on conflict at each horizon h. When the dependent variable is the binary indicator of conflict, the estimations correspond to a linear probability model, which is most commonly used in the literature. By using a cell fixed effects model, we study the variation of conflict within cells. We assume that the Nickell (1981) bias is small in our set-up given that $T = 72$. Notice that, since the international food commodity price index (FP_t) does not vary across cells, we cannot use time fixed effects. Because the errors may exhibit time effects, meaning that the errors may have arbitrary correlation across cells at a moment in time, we use Driscoll–Kraay standard errors to assess statistical significance. These standard errors also correct for persistent common shocks, with the degree of persistence increasing with

horizon h. Note also that, by not including time fixed effects, the estimates could be biased if there are omitted persistent common factors. However, in the robustness section we show that an IV estimator with demeaned variables to capture these unobserved common factors yields very similar results.

5.2. Results

It is common practice in the conflict literature to show the results in a table. Since we study the dynamic effects on conflict over time, however, it is more useful to show the results as impulse response functions, which is typically done for local projections. In particular, Figure 2 shows β_h of Equation (2) for horizons $h = 0, 1, \ldots, 12$ for the different conflict variables. The impulse responses hence represent the evolution over time—until 12 quarters after the initial shock—of the effect of a 1% increase in real food commodity prices at horizon $h = 0$. The figures include the estimated parameters together with one and two standard error bands. The left column shows the results for the naive OLS estimations, while the right column depicts the results for the IV estimations. The results of the first-stage regressions of the IV estimations are reported in the online appendix (Table A4). Both instruments turn out to be highly significant (p-values <0.01). The Kleibergen-Paap F-statistics are larger than 26, which is safely above the Stock and Yogo (2005) critical values for possible weak instruments. Also the Hansen J-statistics indicate that the instrument set is appropriate.

For the OLS estimations, Figure 2 reveals that an increase in international food prices only has a significant impact on conflict incidence and intensity at a few horizons. Moreover, if significant (at the 5% level), this effect is negative. In contrast, for the IV estimations, an increase in food prices has a significant positive effect on conflict. We can thus conclude that it matters to use instrumental variables and isolate changes in food commodity prices that are strictly exogenous. A potential explanation could be that average price changes are at least partly caused by fluctuations in global economic activity, which affect conflict beyond food price increases. The positive impact on conflict is consistent with Besley and Persson (2008), Arezki and Brückner (2014), Smith (2014), Bellemare (2015), Hendrix and Haggard (2015), and Raleigh, Choi, and Kniveton (2015). From these studies, Smith (2014) and Raleigh, Choi, and Kniveton (2015) are also for Africa. A possible reason why both studies deviate from our naive results is that their estimates are based on local prices, while, besides international food commodity prices, they use local weather variables as instruments.

What can we learn about the dynamics of conflict? The IV estimations reveal that the impact of rising food prices on conflict is relatively modest and often not significant during the first year after the price shock, to become significantly positive after approximately one year. For factor conflict, the effects peak around six quarters after the price increase and dissipate after roughly two years. The impact on output conflict is much more persistent; that is, there is still a significant, though diminishing, effect in the third year after the shock. The graphs for conflict intensity show that an increase in food prices has a qualitative similar effect on the total number of events. To get a better understanding of the dynamics, we also re-estimate Equation (2)

(A) Conflict Incidence
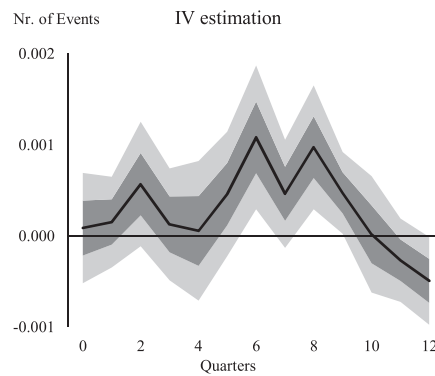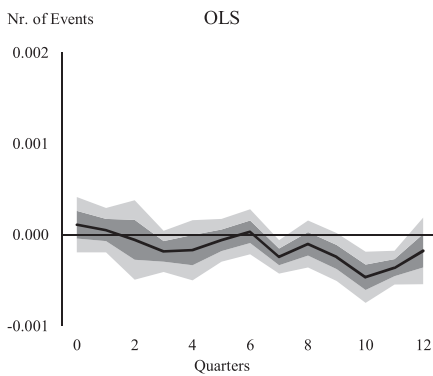
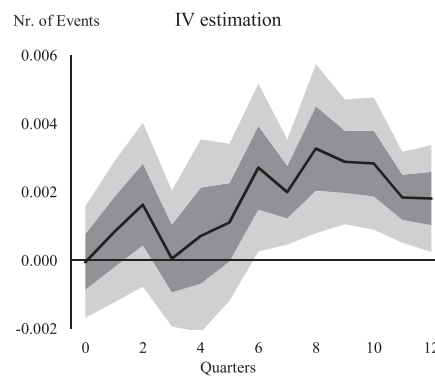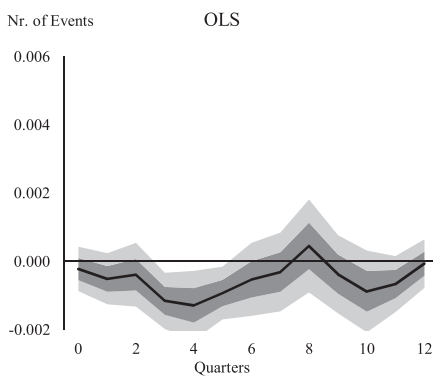*(1) UCDP Factor Conflict*
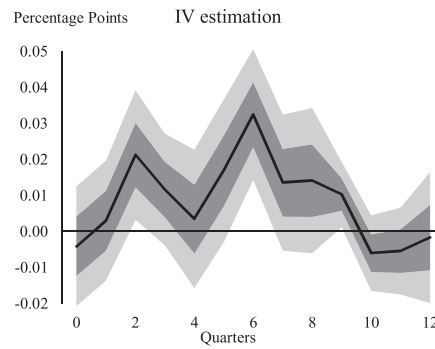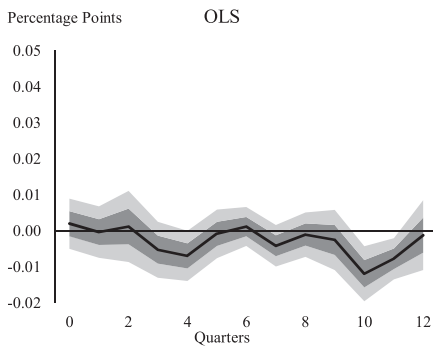
(2) ACLED Output Conflict
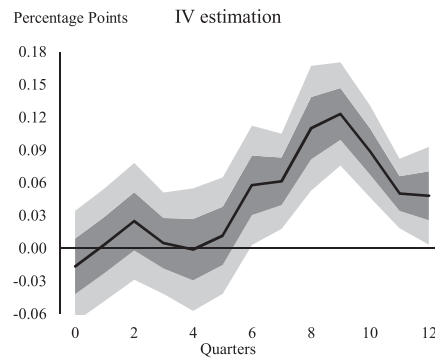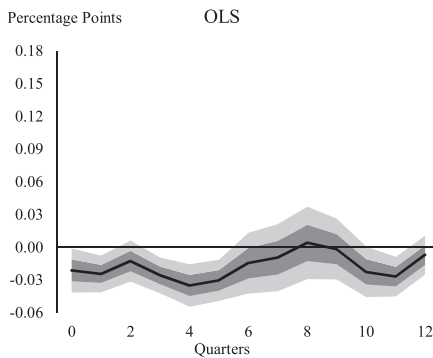
(B) Conflict Intensity

(1) UCDP Factor Conflict

(2) ACLED Output Conflict

Figure 2. Effects of a 1% increase in food prices on conflict. NOTES: Cell fixed effects, cell-specific time trends, 5 lags of the conflict variable, 5 lags of food prices, 5 lags of the harvest shock, and country-specific annual lags of national GDP are always included. Instrumental variables: harvest shocks and narrative food supply news shocks. One and two standard error confidence bands based on Driscoll–Kraay standard errors.

Percentage Points
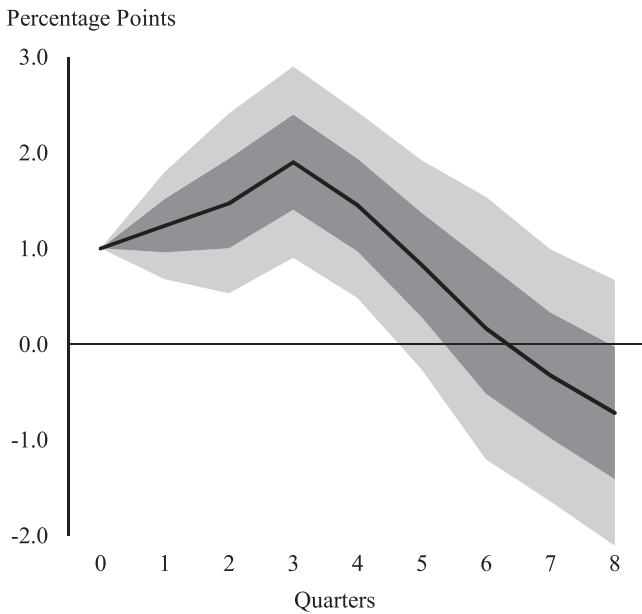
Figure 3. Evolution of food prices after an exogenous food price shock. NOTES: 5 lags of food prices, 5 lags of the harvest shock, and country-specific annual lags of national GDP are included. Instrumental variables: harvest shocks and narrative food supply news shocks. One and two standard error confidence bands based on Driscoll–Kraay standard errors.

with the food commodity price index as the dependent variable. This allows us to examine the dynamic effects of harvest (news) shocks on food prices themselves. The result is shown in Figure 3. As can be observed, food prices remain elevated for about three quarters, after which there is a gradual decline back to the baseline in the second year after the shock. It appears that people do not immediately engage in conflict. Only when prices have been higher for a while, there is a rise in the amount of riots, protests and large-scale (factor) conflicts.

How large are the effects? A 1% rise in food prices augments factor conflict incidence by 0.03 percentage points after six quarters. The unconditional probability of such a large-scale event taking place in a given cell-quarter is 1.18%. Accordingly, a 10% rise in food prices leads to a relative increase in factor conflict probability of 25%. On the other hand, output conflict incidence rises by 0.12 percentage points after nine quarters. The unconditional probability of such a smaller-scale event taking place in a given cell-quarter is 2.36%. Hence, a 10% exogenous increase in food prices leads to an increase in factor conflict probability of 52%. This finding is in line with Arezki and Brückner (2014), who showed that rises in food prices have a stronger positive effect on the incidence of demonstrations and riots than on civil conflict. For conflict intensity, we find that a 1% increase in real food commodity prices leads to a peak rise in the number of events by 0.001 and 0.003 for factor and output conflict, respectively.

5.3. Robustness

In the online appendix, we present and discuss various robustness checks. First, we show that the results do not depend on one of the instruments; that is, the estimates from using the two instruments separately are similar to the baseline results with both instruments. We also show that the results reported in Figure 2 are robust to the inclusion of local weather variables as additional control variables in the estimations, the use of a broader index of food commodity prices (which, e.g., also includes meat and seafood), the use of precisely measured events only, and when we limit the sample to estimate the harvest shocks to 1997–2014. Furthermore, a relevant concern is that we assume homogeneous slope coefficients for all cells. In the next section, we will examine whether the coefficients are different for cells with more agriculture, but various other sources of heterogeneity may exist. Figure A2i in the appendix, however, shows that also a mean group estimator yields very similar results. Finally, we show that results are similar when we account for a possible bias in the estimates due to persistent omitted common factors. Overall, we can conclude that the rise in conflict in response to exogenous food price increases is a very robust finding.

6. Food Prices and Conflict in Food-Producing Regions

Most existing studies use time fixed effects to control for changes in conflict incidence that are related to common shocks, such as the global business cycle. Since changes in international food prices are also common shocks for all regions (countries), these studies multiply changes in global prices with food production (export) and/or consumption (import) shares. In essence, these studies resort to a difference-in-differences strategy to achieve identification. However, as we have argued, the use of time fixed effects does not necessarily avoid endogeneity problems. Specifically, regions that produce or consume more food than other regions may also be systematically more exposed to common (nonfood) shocks. In this section, we assess whether endogeneity is still a problem for the food-conflict relationship in African regions when we include time fixed effects in the estimations. To allow for variation across cells, we weigh food prices with food production in each region.

Furthermore, we have argued that the estimates reflect the additional effect of higher food prices on conflict in cells with more production when time fixed effects are used. The results are therefore not informative about the total impact on conflict in a cell. However, since we have identified shocks that are strictly exogenous, we do not have to include time fixed effects to control for common shocks. This allows us to estimate both the baseline effect, which is common for cells with and without agriculture, as well as the additional effects for food producing regions. Note that the analysis in this section can easily be extended to food consumption shares. Since our focus is on the methodology and the question whether or not the results are distorted, this extension is out of the scope of this article.

6.1. Methodology

We run three types of estimations. As a starting point, we follow the approach that is typically used in the literature. In particular, we estimate the following specification:

$$C_{it+h} = \alpha_{ih} + \beta_h^p \mathrm{FP}_t \times s_{it} + \lambda_h(L) C_{it-1} + \psi_h^p(L) \mathrm{FP}_{t-1}$$
$$\times s_{it-1} + \gamma_{cth} + \mu_{it+h}, \tag{3}$$

where international food prices are multiplied with a measure of agricultural specialization (s_{it}) to create a so-called "food producer price index." Following McGuirk and Burke (2017), we use cell-specific land-use data to measure agricultural specialization; that is, the share of the area of a cell dedicated to agriculture provided by the PRIO-GRID. Data are available for 1990, 2000, and 2010. We interpolate and extrapolate missing values. The agricultural land share ranges between 0% and 99%, and is 7% on average. Finally, we include country-specific time fixed effects (γ_{cth}) in Equation (3).

Equation (3) is first estimated with OLS, which assumes that all changes in the producer price index are exogenous for individual cells. To assess the relevance of distortions due to endogeneity, we compare the results with an IV estimation of Equation (3). As instruments, we multiply the harvest and narrative shocks with the measure of agricultural specialization. As explained above, both estimations correspond with a difference-in-differences strategy: β_h^p measures the effect of an increase in food prices on conflict for cells with an additional unit of agricultural specialization, but is not informative about the baseline effect that is common for all producing cells, nor about the average effects across cells. As a third option, we therefore also estimate the following equation using instrumental variables, in which we replace the time fixed effects by the food price index:

$$C_{it+h} = \alpha_{ih} + \beta_h^C \text{FP}_t + \beta_h^{\tilde{p}} \text{FP}_t \times s_{it} + \lambda_h(L) C_{it-1}$$
$$+ \psi_h^c(L) \text{FP}_{t-1} + \psi_h^{\tilde{p}}(L) \text{FP}_{t-1} \times s_{it-1} + \tau_h s_{it}$$
$$+ \delta_{ch} \times y_{ct-1} + \gamma_{ih} \times \text{trend}_t + \mu_{it+h}. \quad (4)$$

The coefficient β_h^C measures the baseline effect of a food price increase for all cells (with and without agriculture), while $\beta_h^{\tilde{p}}$ measures how much this effect is different for cells with an additional unit of agricultural specialization. Note that we also include agricultural specialization (s_{it}) as a separate variable in Equation (4) to avoid a possible omitted variables bias when interaction terms are used in the estimations. Furthermore, in all IV estimations, we include five lags of the (harvest) instruments as additional control variables to account for possible distortions due to serial correlation in the harvest shocks. Finally, notice that the Kleibergen-Paap F-statistics range between 46.3 ($h = 0$) and 65.6 ($h = 12$) for Equation (3), and between 14.0 ($h = 0$) and 9.5 ($h = 12$) for Equation (4).

6.2. Results

Figure 4 shows three sets of results: (i) Equation (3) estimated with OLS, (ii) Equation (3) estimated with IV, and (iii) Equation (4) estimated with IV. The impulse responses represent the effect of an increase in food prices on conflict for cells with an additional unit of agricultural specialization. For the estimation of Equation (4), Figure 4 also shows the baseline effects.
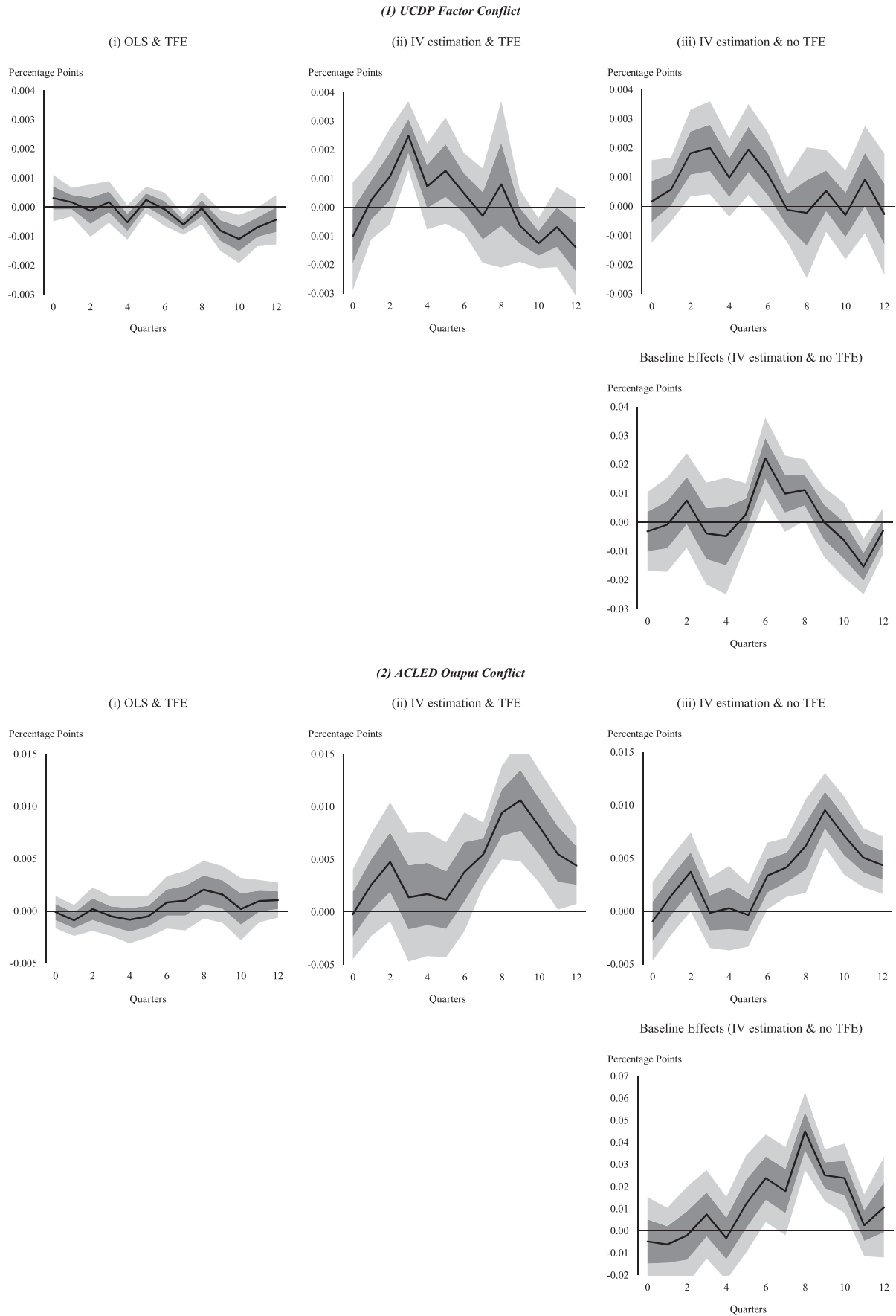
A comparison of columns (i) and (ii) reveals that the use of time fixed effects is not sufficient to address endogeneity problems; that is, there appears to be an important difference between the OLS estimates and the IV estimates with time fixed effects. According to the OLS estimation, there is a decline in factor conflict incidence after a rise in food producer prices, whereas there is a moderate (mostly insignificant) rise in output

conflict incidence. However, according to the IV estimations with time fixed effects, higher food commodity prices increase both factor and output conflict. In particular, there is a strong positive impact on factor conflict during the first two years, which becomes insignificant during the third year. Only for the 10th quarter after the shock, there is a negative effect. For output conflict, we find a persistent rise in conflict, which is considerably larger than the OLS results and still significant three years after the food price hike. As can be observed in Figure 4(b), except for the sign switch for factor conflict, the results for conflict intensity are similar.
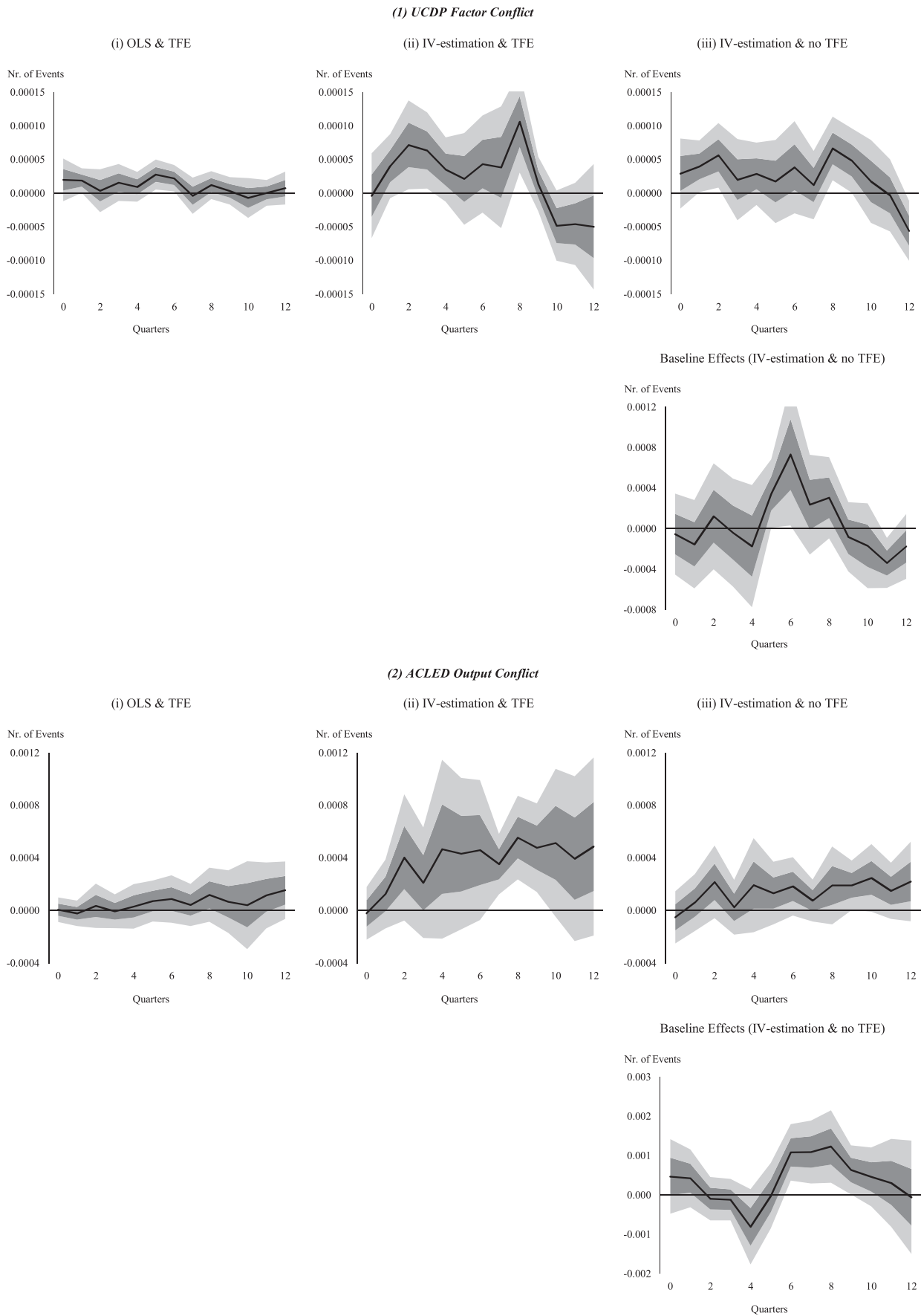
From the comparison between the OLS and IV estimations, we can conclude that failing to account for exogenous price changes leads to a substantial downward bias in the estimated effects on conflict incidence and even the opposite sign for factor conflict incidence. Clearly, the use of time fixed effects does not resolve endogeneity problems when estimating the relationship between food prices and conflict. This observation raises questions about the existing evidence. In particular, in line with our OLS results, an opposite impact of food prices on both types of conflict was also documented by McGuirk and Burke (2017), while the majority of studies that do not distinguish between factor and output conflict typically find that rising producer prices lead to fewer conflict events (e.g., Brückner and Ciccone 2010; Dube and Vargas 2013; Berman and Couttenier 2015; Fjelde 2015; Janus and Riera-Crichton 2015). From the studies discussed in Section 2.2 that use time fixed effects in combination with international food commodity prices, only Besley and Persson (2008) and Arezki and Brückner (2014) find a positive relationship for countries with higher (net) export shares of food commodities. Yet, their magnitudes may also be an underestimation of the actual effects.

Next, we focus on column (iii), which shows the IV estimations without time fixed effects (Equation (4)). The first row shows the additional effect for one percentage point more agricultural land (coefficient $\beta_h^{\tilde{p}}$ in Equation (4)). The additional effect for producers is very similar to the IV results with time fixed effects in column (ii), which is not very surprising since the shocks are strictly exogenous. Most importantly, the second row shows the baseline effects (coefficient β_h^C in Equation (4)), which represent the effects that are common for all cells, regardless their level of agricultural production. For both conflict types, there appears to be a significant positive baseline effect of food price changes on conflict. For example, for output conflict incidence, a 1% rise in real food prices leads to an increase in the absolute probability of conflict of 0.045 percentage points after two years. For a 1% food price increase, this corresponds with a relative increase in the probability of 20%. Each additional percentage point of land devoted to agriculture increases the absolute probability with 0.006 percentage points. For cells with an average share of agricultural land of 7%, a 10% increase in prices augments the relative probability of output conflict additionally with 18%. In total, the probability of conflict occurring in those cells will thus increase by 38%. Put differently, the use of time fixed effects results in an underestimation of the total effect of food price changes on conflict in average cells by more than half.

Overall, rising food prices appear to have an unambiguously positive and significant effect on violence, which further

(1) UCDP Factor Conflict

(i) OLS & TFE

(ii) IV estimation & TFE

(iii) IV estimation & no TFE

Baseline Effects (IV estimation & no TFE)

(2) ACLED Output Conflict

(i) OLS & TFE

(ii) IV estimation & TFE

(iii) IV estimation & no TFE

Baseline Effects (IV estimation & no TFE)

Figure 4(a). Effects of a 1% increase in food "producer" prices on conflict incidence. NOTES: Cell fixed effects, 5 lags of the conflict variable and 5 lags of food "producer" prices are always included. Estimations of Equation (3) include country-specific time fixed effects (TFE). Estimations of Equation (4) also include 5 lags of food prices and the measure of agricultural specialization, 5 lags of the harvest shock, and the country-specific annual lag of national GDP. Instrumental variables: harvest shocks and narrative food supply news shocks. One and two standard error confidence bands based on Driscoll-Kraay standard errors.

(1) UCDP Factor Conflict

(i) OLS & TFE

(ii) IV-estimation & TFE

(iii) IV-estimation & no TFE

Baseline Effects (IV-estimation & no TFE)

(2) ACLED Output Conflict

(i) OLS & TFE

(ii) IV-estimation & TFE

(iii) IV-estimation & no TFE

Baseline Effects (IV-estimation & no TFE)

Figure 4(b). Effects of a 1% increase in food "producer" prices on conflict intensity. NOTES: Cell fixed effects, 5 lags of the conflict variable and 5 lags of food "producer" prices are always included. Estimations of Equation (3) include country-specific time fixed effects (TFE). Estimations of Equation (4) also include 5 lags of food prices and the measure of agricultural specialization, 5 lags of the harvest shock, and the country-specific annual lag of national GDP. Instrumental variables: harvest shocks and narrative food supply news shocks. One and two standard error confidence bands based on Driscoll–Kraay standard errors.

increases in food producing regions. From a theoretical point of view, this suggests that relative deprivation and predation effects more than offset the opportunity cost effects of insurrection in food producing regions. In contrast to McGuirk and Burke (2017), we find that this is the case for both large-scale battles for territory (factor conflict) and smaller-scale conflicts such as riots and protests (output conflict). On one hand, when food prices rise, the income gap between net consumers and net producers typically increases, which can result into anger and protest according to the relative deprivation hypothesis. On the other hand, higher food prices in producing regions raises the gains of appropriation and promotes rapacity over the surplus. Note, however, that the positive baseline effects for both types of conflict also apply to cells with no food production at all. This suggests that declines in real income cause food consuming households to involve in looting and other violent activities. This is again consistent with the existence of relative deprivation effects, but it may also be that poverty lowers the opportunity cost of insurrection in food consuming areas when food prices increase and real wages decrease.

6.3. Robustness

In the online appendix, we present various robustness checks. First, we perform the same checks as in the previous section: controlling for local weather conditions, using both instruments separately, removing the influence of four data points, a shorter sample period to estimate the harvest shocks, a broader index of food commodity prices and precisely measured events only. Again, the results prove to be robust.

Furthermore, notice that agricultural specialization could be correlated with other characteristics. For example, there is a positive correlation between the share of agricultural land in a cell and the population of a cell. A higher population density could be a breeding ground for certain types of conflict. Therefore, as another robustness check, we include interaction terms of food prices with other characteristics that could be correlated with agricultural specialization and/or that have been found to play an important role in the literature. Specifically, we consider interactions with population, travel time, the polity2 index for democracy and an ethnic diversity dummy. Overall, for both factor and output conflict incidence, the additional effects for cells with more agricultural production remains significantly positive when including these interactions.

7. Conclusions

According to the ACLED database, political violence in Africa has resulted in more than 600,000 deaths between 1997 and 2015. Numerous studies have examined which factors cause such havoc. A large strand of this literature has analyzed the link between income shocks and conflict. Within this literature, several studies have, in turn, focused on changes in food commodity prices as a source of income shocks. To achieve identification, these studies typically assume that all changes in international food commodity prices are exogenous events for local areas and individual countries. In this article, we have challenged this assumption by estimating the dynamic effects of

food price shocks on conflict in Africa that are strictly exogenous; that is, caused by unexpected variation in harvests in other regions of the world. These results are compared with estimations for average international food price changes, which essentially include endogenous price shifts in response to global economic conditions.

Our findings are the following: First, exogenous food price increases raise conflict incidence and intensity in Africa, while "naive" estimates show the opposite effect. Accordingly, identifying exogenous price changes seems to be very important for the results. Second, the effects are more pronounced for output conflict types such as riots and protests (39% higher probability after a 10% real food price increase), than for battles over land control (25% higher probability). We show that the bulk of the effect only takes place beyond one year after the price increase. Especially for output conflict the effect is persistent; that is, in the third year after the shock there is still a significant effect on output conflict. Third, the rise in conflict is more pronounced in areas with more agricultural land. Again, the use of shocks that are strictly exogenous matters for the results. Finally, we show that the inclusion of time fixed effects, which is commonly done in the literature to evaluate the effect for food producers, wipes out a positive baseline effect for areas with and without agriculture. As a result, the total effect for food producers is much larger than such estimates suggest.

Overall, our results confirm that income shocks are a likely source for violent events. Although most violence does probably not occur because of higher food prices, but are caused by broader economic conditions or political grievances (Bush 2010; Berazneva and Lee 2013), these income shocks can be a trigger to engage in violent events. It is unlikely that the implications of these results will become less important in the future. For example, the demand for cereals in sub-Saharan Africa will approximately triple by 2050 and, unless there is significant local agricultural intensification and massive cropland expansion, will depend much more on imports of cereals than it does today. Consequently, our results support the type of policy recommendations oriented at insuring poor societies against negative income shocks to avoid violent events (Blattman and Miguel 2010). However, our results also suggest that these insurance schemes should not only be targeted toward farmers, but also—and perhaps more importantly—toward the consumers.

Supplementary Materials

Acknowledgments

Funding

References

Arezki, R., and Brückner, M. (2014), "Effects of International Food Price Shocks on Political Institutions in Low-Income Countries: Evidence From an International Food Net-Export Price Index," *World Development*, 61, 142–153. [548,549,553,555,556]

Bazzi, S., and Blattman, C. (2014), "Economic Shocks and Conflict: Evidence From Commodity Prices," *American Economic Journal: Macroeconomics*, 6, 1–38. [547,549,552]

Bellemare, M. (2015), "Rising Food Prices, Food Price Volatility, and Social Unrest," *American Journal of Agricultural Economics*, 97, 1–21. [547,548,549,550,553]

Berazneva, J., and Lee, D. (2013), "Explaining the African Food Riots of 2007–2008: An Empirical Analysis," *Food Policy*, 39, 28–39. [559]

Berman, N., and Couttenier, M. (2015), "External Shocks, Internal Shots: The Geography of Civil Conflicts," *Review of Economics and Statistics*, 97, 758–776. [547,549,552,556]

Besley, T., and Persson, T. (2008), "The Incidence of Civil War: Theory and Evidence," NBER Working Paper Series. [547,548,549,552,553,556]

——— (2010), "State Capacity, Conflict, and Development," *Econometrica*, 78, 1–34. [549]

Blattman, C., and Miguel, E. (2010), "Civil War," *Journal of Economic Literature*, 48, 3–57. [548,559]

Brückner, M., and Ciccone, A. (2010), "International Commodity Prices, Growth and the Outbreak of Civil War in Sub-Saharan Africa," *Economic Journal*, 120, 519–534. [547,549,556]

Burke, M., Hsiang, S., and Miguel, E. (2015), "Climate and Conflict," *Annual Review of Economics*, 7, 577–617. [550]

Bush, R. (2010), "Food Riots: Poverty, Power and Protest," *Journal of Agrarian Change*, 10, 119–129. [559]

De Winne, J., and Peersman, G. (2016), "The Macroeconomic Effects of Disruptions in Global Food Commodity Markets: Evidence for the United States," *Brookings Papers on Economic Activity*, 2016, 183–286. [548,550,551,552]

——— (2018) "Agricultural Price Shocks and Business Cycles: A Global Warning for Advanced Economies," Ghent University Working Paper Series 18/945. [550]

Dube, O., and Vargas, J. (2013), "Commodity Price Shocks and Civil Conflict: Evidence From Colombia," *Review of Economic Studies*, 80, 1384–1421. [547,548,549,550,552,556]

Fjelde, H. (2015), "Farming or Fighting? Agricultural Price Shocks and Civil War in Africa," *World Development*, 67, 525–534. [549,552,556]

Hamilton, J. (1983), "Oil and the Macroeconomy Since World War II," *Journal of Political Economy*, 91, 228–248. [551]

Hendrix, C., and Haggard, S. (2015), "Global Food Prices, Regime Type, and Urban Unrest in the Developing World," *Journal of Peace Research*, 52, 143–157. [547,548,549,553]

Janus, T., and Riera-Crichton, D. (2015), "Economic Shocks, Civil War and Ethnicity," *Journal of Development Economics*, 115, 32–44. [549,556]

Jordà, O. (2005), "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95, 161–182. [552]

McGuirk, E., and Burke, M. (2017), "The Economic Origins of Conflict in Africa," NBER Working Paper Series. [547,548,549,552,556,559]

Mian, A., Sufi, A., and Verner, E. (2017), "Household Debt and Business Cycles Worldwide," *The Quarterly Journal of Economics*, 132, 1755–1817. [552]

Miguel, E., Satyanath, S., and Sergenti, E. (2004), "Economic Shocks and Civil Conflict: An Instrumental Variables Approach," *Journal of Political Economy*, 4, 725–753. [550]

Nickell, S. (1981), "Biases in Dynamic Models With Fixed Effects," *Econometrica*, 49, 1417–1426. [553]

Owyang, M., Ramey, V., and Zubairy, S. (2013), "Are Government Spending Multipliers Greater During Periods of Slack? Evidence From Twentieth-Century Historical Data," *American Economic Review*, 103, 129–134. [552]

Pindyck, R. (1993), "The Present Value Model of Rational Commodity Pricing," *Economic Journal*, 103, 511–530. [551]

Raleigh, C., Choi, H., and Kniveton, D. (2015), "The Devil Is in the Details: An Investigation of the Relationships Between Conflict, Food Price and Climate Across Africa," *Global Environmental Change*, 32, 187–199. [547,548,549,553]

Raleigh, C., and Dowd, C. (2016), *Armed Conflict Location and Event Data Project (ACLED) Codebook*. [552]

Ramey, V., and Shapiro, M. (1998), "Costly Capital Reallocation and the Effects of Government Spending," *Carnegie-Rochester Conference Series on Public Policy*, 48, 145–194. [551]

Roberts, M., and Schlenker, W. (2013), "Identifying Supply and Demand Elasticities of Agricultural Commodities: Implications for the US Ethanol Mandate," *American Economic Review*, 103, 2265–2295. [550]

Smith, T.G. (2014), "Feeding Unrest: Disentangling the Causal Relationship Between Food Price Shocks and Sociopolitical Conflict in Urban Africa," *Journal of Peace Research*, 51, 679–695. [548,549,552,553]

Stock, J. H., and Watson, M. W. (2017), "Identification and Estimation of Dynamic Causal Effects in Macroeconomics," Working Paper. [553]

Stock, J. H., and Yogo, M. (2005), *Testing for Weak Instruments in Linear IV Regression*, New York: Cambridge University Press, pp. 80–108. [553]

Sundberg, R., and Melander, E. (2013), "Introducing the UCDP Georeferenced Event Dataset," *Journal of Peace Research*, 50, 523–532. [552]

Teulings, C., and Zubanov, N. (2014), "Is Economic Recovery a Myth? Robust Estimation of Impulse Responses?," *Journal of Applied Econometrics*, 29, 497–514. [552]

Tollefsen, A., Strand, H., and Buhaug, H. (2012), "PRIO-GRID: A Unified Spatial Data Structure," *Journal of Peace Research*, 49, 363–374. [552]