EXTERNAL SHOCKS, INTERNAL SHOTS: THE GEOGRAPHY OF CIVIL CONFLICTS

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Abstract—We use georeferenced information on the location of violent events in sub-Saharan African countries and provide evidence that external income shocks are important determinants of the intensity and geography of civil conflicts. More precisely, we find that (a) the incidence, intensity, and onset of conflicts are generally negatively and significantly correlated with income variations at the local level; (b) this relationship is significantly weaker for the most remote locations; and (c) at the country level, these shocks have an insignificant impact on the overall probability of conflict outbreak but do affect the probability that conflicts start in the most opened regions.

I. Introduction

THE role of income shocks as a determinant of civil conflicts has been at the core of intense debates among economists and political scientists over the past decade. Particular attention has been given to the effect of commodity price variations, taken as a proxy for exogenous external income shocks (Besley & Persson, 2008; Bruckner & Ciccone, 2010; Fearon, 2005). At the country level, the results are mixed at the very least.[1] Recently, Bazzi and Blattman (2014) challenged most of the findings of the literature, arguing that a significant relationship between commodity prices and conflict incidence can only be detected using very specific samples, definitions of civil conflicts, or estimators. The few results that are available at the microlevel point to a more robust causal relationship (Dube & Vargas, 2013). However, even when income shocks are found to significantly affect conflict probability, the identification of the precise transmission channel remains problematic.

This paper uses detailed information on the date and location of conflicts events in sub-Saharan African (SSA) countries to study the effect of external income shocks on the likelihood of violence. We work with a full grid of SSA countries divided in subnational units of 0.5×0.5 degrees latitude and longitude; our unit of observation is the cell-year. We have two main objectives. The first is to use the different dimensions of our data to study the effect of external shocks both within and across countries and to try to reconcile the results found by micro- and macrolevel studies. The second is to discuss the plausibility of various channels through which external income shocks might affect conflict outbreak and intensity.

Our paper makes several contributions to the literature. First, existing papers have generally studied the impact of income shocks on conflict at the country-level, with the exception of Dube and Vargas (2013), who use geographically disaggregated data but for a single country (Colombia). We use finely grained disaggregated data for the entire set of SSA countries, which significantly improves the external validity of the results. Second, the literature has almost exclusively used commodity price changes as a proxy for exogenous income variations. We propose a number of alternative ways to identify exogenous income shocks through international trade patterns. We improve the usual measures of commodity shocks by constructing region-specific measures of agricultural specialization. More precisely, we consider changes in the world demand for the agricultural commodities produced by the regions within the countries, removing the usual assumption that specialization is similar across cells. Moreover, we go further than the existing literature by also considering a longer-lasting external demand shock: the number of banking crises in the country's trading partners (weighted by the share of each partner in the country's total exports). Third, we combine these shocks with cell-specific information reflecting their "natural" level of trade openness, proxied by the distance to the nearest major seaport. Our study therefore differs from the existing literature in its level of analysis (both across and within countries) and scope (types of shocks). From an identification perspective, combining temporary and long-lasting external shocks with cell-specific information also ensures that we are capturing different aspects of exogenous changes in income. Moreover, our methodology allows us to study how external shocks affect the geography and intensity of conflict within countries.

At the microlevel, we find that the incidence of conflicts is generally negatively and significantly correlated with income shocks within cells. Put differently, positive external income shocks reduce the probability to observe a conflict within a given cell. Second and important, the relationship between external income shocks and conflict is significantly weaker in naturally less open cells (i.e., when one moves away from the seaports).[2] This clearly suggests that we are identifying

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[1] Among the most recent contributions, Besley and Persson (2008) find a positive relationship between income shocks and civil war incidence, while Bruckner and Ciccone (2010) find the opposite.

[2] Therefore, our methodology identifies the effect of foreign demand shocks on conflict in relatively open regions. In that sense, our results represent a local average treatment effect. However, we show that the conflicts triggered by our shocks are not, on average, different from the conflicts observed in the sample in general.

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the effect of exogenous shocks related to international trade, which are less likely to affect the most remote regions. This result holds for all our considered shocks and is not sensitive to the use of several alternative measures of local agricultural specialization. Our findings also apply to conflict onset, ending, and intensity and remain remarkably robust to the use of various conflict data sources and samples, estimation techniques, as well as to the inclusion of additional country- and cell-specific controls, among which are the cell's GDP and its distance to the capital city, international borders, or natural resource fields. Quantitatively, both the average effect and its heterogeneity are nonnegligible. In the most open cells, a standard deviation increase in the world demand for the agricultural commodities produced by the cell increases conflict probability by 1 to 3 percentage points.[3] This effect is two to three times larger when we restrict the sample to cells in which at least one event occurs over the period. On the other hand, no significant effect can be detected in the most remote cells.

The fact that external demand variations affect the likelihood of conflict on average within cells, especially for the most open ones, implies that these shocks affect the intensity and geography of civil conflicts at the country level. In that sense, income shocks act as threat multipliers, just like the sharp rise in food prices accelerated and intensified protests during the Arab Spring. The next step is to study the effect of our shocks on conflict outbreak at the country level. When doing so, we fail to find any significant effect, a result consistent with Bazzi and Blattman (2014). However, this is partly due to the fact that these trade-related shocks affect regions heterogeneously. Moving back to the local level, we find that both types of shocks significantly affect the probability that a country-level conflict starts in the most opened locations (the effect being slightly more robust in the case of our long-lasting shock: foreign financial crises). This illustrates the advantage of using geographically disaggregated data to study the determinants of violence, as country-level data ignore by definition local heterogeneity.

Our findings yield at least two important conclusions. The first pertains to the predictions of workhorse models of conflict. These are a priori ambiguous: On the one hand, a larger income might decrease the risk of conflict by reducing individuals' opportunity cost of insurrection or increasing the capacity of the state to prevent rebellion (see, e.g., Fearon & Laitin, 2003); on the other hand, positive income shocks might raise the likelihood of conflict by increasing the value of resources to fight over (the "state-as-prize" mechanism). Our result that income variations decrease conflict probability within cell clearly points to the first group of predictions. Between the opportunity cost and the state capacity mechanisms, we favor the opportunity cost interpretation, for the following reasons. First, the state capacity mechanism should be more prevalent in cells close to the political center of the country, that is, the capital city (Buhaug, 2010), but we do not find that our income shocks have a larger effect in cells located closer to the country's capital city. Second, our shocks indeed have a significant effect on local-level GDP per capita. Third, our shocks do not increase military spending and do not have a larger effect in countries in which revenue mobilization is more efficient, contrary to what we would expect if the state capacity mechanism were driving our findings.

The second implication of our results is that external income shocks are probably more important to understand the geography and intensity of ongoing conflicts than the outbreak of wars at the country level. Our findings suggest that if the opportunity cost story is relevant, it is mainly through the escalation and spatial evolution of ongoing conflicts rather than through the outbreak of new ones. More generally, our results contribute to the literature on the impact of international trade on civil conflicts (Barbieri & Reuveny, 2005; Jha, 2008; Martin, Mayer, & Thoening, 2008). In particular, we show that trade openness might influence importantly the geography of conflicts within countries.

Our paper is related to the literature documenting the effect of income shocks at the microlevel. The limitations of cross-country studies, as well as the availability of more geographically detailed data, have recently pushed researchers to move toward a more disaggregated approach. Buhaug et al. (2011) find that within countries, conflicts are more likely to erupt in the poorest regions. Buhaug (2010) argues that civil wars originate farther away from the capital in more powerful political regimes.[4] Hidalgo et al. (2010) use data on Brazilian municipalities and find that favorable economic shocks, instrumented by rainfall, negatively affect the number of land invasions within municipalities. This is also the case for Bohlken and Sergenti (2010) in the case of Hindu-Muslim riots in India. These results provide support to the view according to which decreases in income incite individuals to enroll in rebellions by lowering the opportunity cost of such activities. While this idea has received important anecdotal support,[5] only a few research papers have dealt with the determinants of participation in civil war. Humphrey and Weinstein (2008) find that monetary incentives played a significant role in explaining individuals' enrollment to the Revolutionary United Front in Sierra Leone in the early 1990s. Enlistment has also been shown to be correlated with negative individual income variations or local economic downturns in Rwanda (Friedman, 2010), Nigeria (Guichaoua, 2010), or Burundi (Nillesen & Verwimp, 2009). Similarly, negative

[3] The unconditional probability of a conflict occurring in a given cell is between 2% and 4%, depending on the sample.

[4] These two papers use UCDP/PRIO data on the location of the first reported violent event of conflicts for a number of countries. They do not consider income shocks or the geography of conflicts afterward.

[5] NGOs have reported that the wages or payments paid or promised by armed groups were a primary motive for enrollment (Human Rights Watch, 2003a–2003c; Dube & Vargas, 2013). The important drop in coffee prices in the late 1990s has been proposed as one of the explanations of the occurrence of civil wars in Burundi, Rwanda, and Uganda, three countries that depend heavily on coffee revenues (Bruckner & Ciccone, 2010). A similar link can be made between the 40% drop in coffee price in the late 1980s and the civil wars in Uganda and Rwanda in the early 1990s).

shocks to agricultural production and crops prices have been found to be positively correlated with conflict by Dube and Vargas (2013), in the case of coffee prices in Colombia,[6] and Jia (2011), who finds that droughts increased the probability of (sweet-potatoes-producing) peasants' revolts in China using historical data over the 1470–1990 period. By focusing on a specific country, this strand of research is able to identify precisely the effect of income shocks on conflicts through individuals' behavior. The generalization of these results is, however, made difficult by the external validity concerns inherent in any country-specific study. Our paper complements their findings and constitutes a first attempt to make a link between macro, cross-country studies and micro, country-specific ones, through the consideration of both within- and between-countries variations.

In the next section, we describe the data and our methodology to identify income shocks. Section III presents the econometric methodology. Sections IV and V present our main results on the effect of external income shocks on conflict within and across countries. We discuss the interpretation and relation of our results with the existing literature in section VI. The last section concludes.

II. Data

Our main objective is to study how income shocks affect the probability of conflict both within and across countries. We therefore need data on (a) the location of conflict events within countries, (b) external shocks potentially affecting conflict through income, and (c) location-specific characteristics influencing the way in which each location might respond to these external income shocks. The online appendix contains further details on the data used throughout the paper.

A. *Conflict Data*

Data description. We use three data sets containing the geolocation of conflict events in sub-Saharan Africa: two versions of the Armed Conflict Location and Event data set[7] (ACLED), and the recently released UCDP-Georeferenced Event data set (UCDP-GED). These data sets cover different countries and time periods. The first ACLED data set (ACLED I hereafter) contains only twelve sub-Saharan African countries, all of which have known large civil war episodes over the period of study, but it covers a long time period (1960–2005). The second ACLED data set[8] (ACLED II hereafter) covers all African countries, plus a small number of non-African countries, but the data start only in 1997. Finally, the UCDP-GED data set[9] covers African countries and the period 1989 to 2010. General characteristics, the complete lists of countries covered by each data set, more information, and discussion of the specificities of each data source appear in the online appendix (sections A.1 and A.2).

In all data sets, the unit of observation is the event. We have information about the date (precise day most of the time), longitude, and latitude of conflict events within each country. These events are obtained from various sources, including press accounts from regional and local news, humanitarian agencies, or research publications. The three data sets mainly differ in the rules they apply for the inclusion of events. ACLED I and UCDP-GED consider only events pertaining to conflicts reaching at least 25 battle-related deaths per year, which makes them comparable to the country-level data commonly used in the literature.[10] Note that UCDP-GED includes all events related to a given conflict, defined by a dyad of actors, even if during a specific year, this conflict did not cause more than 25 deaths. All the events related to a given conflict are included as soon as this conflict caused 25 deaths or more in any given year of the sample period. ACLED II records all political violence, including violence against civilians and rioting and protesting within and outside a civil conflict, without specifying a battle-related deaths threshold.[11] The broader definition of conflict makes the comparison with the country-level literature more difficult. Despite these different rules of inclusion, we show that our results are remarkably similar across samples.

The latitude and longitude associated with each event define a geographical location. The three data sets contain information on the precision of the georeferencing of the events. In all data sets, the geoprecision is at least the municipality level in at least 80% of the cases (more than 95% in ACLED's data sets), and is even finer (village) for more than 65% of the observations (more than 80% in ACLEDs). The geoprecision is generally at the level of the province for the rest of the events. We drop the observations in the UCDP-GED data set where the event cannot be localized at a finer level than the country (less than 2% of the observations).

For each data source, we aggregate the data by year and 0.5×0.5 degree cell.[12] Our unit of observation is therefore a

[6] Dube and Vargas (2013) find evidence in favor of both the opportunity costs and state-as-prize theories. More precisely, they show that positive commodity price shocks decrease the likelihood of conflicts in the case of coffee (a labor-intensive commodity) but raise the probability of conflict for oil (a capital-intensive commodity).

[7] See Michalopoulos and Papaioannous (2011), Harari and La Ferrara (2012), and Besley and Reynal-Querol (2014) for recent contributions using ACLED data.

[8] Raleigh et al. (2010).

[9] See Sundberg, Lindgren, and Padskocimaite (2010) and Melander and Sundberg (2011) for more details.

[10] UCDP/PRIO defines an armed conflict (civil conflict) as "a contested incompatibility that concerns government or territory or both where the use of armed force between two parties results in at least 25 battle-related deaths" (Gleditsch et al., 2002; 618–619).

[11] In the case of ACLED II, we concentrate on violent events to be consistent with the other datasets.

[12] In most cases, we have information on the temporal precision of the event. For most events, the precise day it took place is known, but in a few cases only the week, the month, or even the year is known. ACLEDs do not consider events for which the precision is lower than a month, but UCDP-GED include some events for which we know only the year. Given that we aggregate the information over time, at the yearly frequency, this has no impact on our results.

Table 1.—Basic Statistics on Each Sample

Sample	UCDP-GED	ACLED I	ACLED II
Number of countries	48	12	44
Period	1989–2006	1980–2005	1997–2006
Number of grid cells	8,378	2,700	8,367
Total Number of events	16,364	4,139	15,561

cell-year in the rest of the paper; we study how income shocks affect the probability that a conflict event occurs in a given cell during a given year. Using this level of aggregation ensures that our definition of a location is not endogenous to conflict events.[13] It also mitigates concerns of potential measurement error in the geolocation of the events. Our level of geographical aggregation is the same as the one used in PRIO-GRID, which allows us to merge our conflict data with information contained in this data set, including distances to capital city, national borders, and socioeconomic information.

The structure of our final data set is therefore a full grid of Africa divided in subnational units of 0.5×0.5 degrees latitude and longitude (which means around 55×55 kilometers at the equator). For each conflict data source, we construct a dummy variable that equals 1 if at least one conflict happened in the cell during the year, which we interpret as cell-specific conflict incidence. This is our main dependent variable in the rest of the paper, although we also systematically consider for robustness cell-specific conflict onset, ending, and intensity.

While the geocoding of the events is cross-checked in all three data sets, they are not immune from potential biases. We cannot rule out the possibility that each of these data sets is biased toward certain types of countries, regions, or events. However, as they have been constructed by different institutions and according to different rules of inclusion, these biases are likely to differ across sources. In fact, the correlation between our conflict variables and location-specific variables (such as distances to ports, capital city, border or natural resources or population and GDP) differs across data sets (see section A.3 in the online appendix), even when considering only the set of overlapping countries and years. Obtaining so similar results across samples is therefore reassuring. Our empirical methodology, in particular through the inclusion of cell and country-year fixed effects, also makes unlikely the possibility that our results arise because of systematic biases in the reporting of events.

Descriptive statistics. We concentrate on sub-Saharan African countries as this is the zone covered by the three data sets. Our final sample contains between 12 and 48 countries depending on the conflict data we use (table 1). We also show robustness checks considering non-African countries covered by ACLED as well, including some MENA, Asian, and European countries. Finally, we concentrate on the 1980–2006 period due to data availability for the computation of income shocks and to the fact that the post-2007 period was characterized by a global financial crisis that had unprecedented and still not fully understood effects on international trade and commodity prices. The list of countries, descriptive statistics about the conflict data, and maps showing the geographical distribution of events appear in section A.2 of the online appendix.

Several elements are worth mentioning. First, the unconditional probability of observing at least one conflict in a given cell in a given year is low in all three samples: between 2% and 4% depending on the data set (table 2). The ACLED II data set contains more events per country than the two others, which was expected because it uses a broader definition of conflict events. Conditioning on observing a conflict during the year, the average number of events by cell is between three and four depending on the data set. In the vast majority of cells, no event occurs over the entire period. Note that we run robustness checks using only the cells in which at least one event occurs over the period—high-conflict-risk cells—and show that the quantitative effects of our shocks are much larger in this case.

Second, countries are highly heterogeneous in how they are affected by conflicts in terms of both number of events and their geographical coverage (see tables A.3 to A.6 in the online appendix). Some countries do not display any event over the period (Botswana and Equatorial Guinea in the UCDP-GED data set, for instance), while countries like the Democratic Republic of Congo, Sierra Leone, and Uganda experience a large number of events in all three data sets. Some countries, like Sudan, experienced a large number of conflict events, but these cover only a small share of the total area of the country (given by the total number of grid cells). On the other hand, conflict events cover almost the entire area of some small countries like Burundi and Rwanda.

B. *Income Shocks*

Our identification strategy rests on the use of both country-wide income shocks and cell-specific characteristics. Our first objective is to study the effects of external (i.e., foreign) shocks on the incidence, onset, ending, or intensity of conflict in a given cell within a given country. All of these shocks are based on variations in the foreign demand for the goods produced by the country or region to which the cell belongs. We focus on two different types of foreign shocks. While they are all supposed to capture exogenous variations in foreign demand for the goods exported by the cell, they are different in their scope and nature. In particular, while the first type of shock (based on the world demand for agricultural commodities) can arguably be considered as temporary and limited in scope, the second (based on financial crises) is larger and longer lasting. Considering different shocks allows us to check the robustness of the results and discuss the way in which income shocks affect the incidence of conflicts. Descriptive statistics on each of the income shocks variables

[13] See Harari and La Ferrara (2012), Michalopoulos (2012), or Besley and Reynal-Querol (2014) for papers using a similar methodology.

TABLE 2.—DESCRIPTIVE STATISTICS

	Observations	Mean	SD	First Quartile	Median	Third Quartile
Sample I: UCDP-GED						
Pr(conflict)	150,804	0.03	0.17	0.00	0.00	0.00
# conflicts	144,522	0.11	1.50	0.00	0.00	0.00
# conflicts (if > 0)	4,384	3.73	7.76	1.00	1.00	3.00
Distance to closest port (km)	150,804	768.96	436.91	402.37	742.71	1,111.65
Distance to border (km)	146,430	152.88	127.75	51.00	118.00	221.00
Distance to capital (km)	150,804	615.69	394.32	305.00	520.00	882.00
Distance to natural resources (km)	150,804	295.77	213.66	126.41	249.64	410.59
Rel. distance to closest port[a]	150,804	0.59	0.24	0.41	0.62	0.78
Rel. distance to border[a]	146,430	0.35	0.25	0.14	0.30	0.53
Rel. distance to capital city[a]	150,804	0.47	0.24	0.27	0.46	0.66
Rel. distance to nat. res.[a]	150,804	0.45	0.25	0.24	0.43	0.65
ln agr. com. shock	130,500	10.05	0.93	9.61	10.13	10.59
Exposure to crises	148,842	0.14	0.18	0.01	0.06	0.21
Sample II: ACLED I						
Pr(conflict)	70,200	0.02	0.14	0.00	0.00	0.00
# conflicts	70,200	0.06	0.75	0.00	0.00	0.00
# conflicts (if > 0)	1,436	2.88	4.43	1.00	2.00	3.00
Distance to closest port (km)	70,200	908.99	476.38	505.02	956.56	1,296.77
Distance to border (km)	70,200	179.37	149.06	56.00	137.00	275.00
Distance to capital (km)	70,200	709.30	415.99	359.00	665.00	1,002.00
Distance to natural resources (km)	70,200	289.95	244.73	106.45	210.48	394.02
Rel. distance to closest port[a]	70,200	0.58	0.24	0.40	0.62	0.76
Rel. distance to border[a]	70,200	0.37	0.26	0.15	0.32	0.56
Rel. distance to capital city[a]	70,200	0.50	0.23	0.32	0.51	0.69
Rel. distance to nat. res.[a]	70,200	0.41	0.25	0.20	0.36	0.60
ln agr. com. shock	41,055	9.95	0.96	9.57	10.03	10.38
Exposure to crises	70,200	0.19	0.19	0.03	0.10	0.26
Sample III: ACLED II						
Pr(conflict)	83,670	0.04	0.20	0.00	0.00	0.00
# conflicts	83,670	0.19	2.36	0.00	0.00	0.00
# conflicts (if > 0)	3,550	4.38	10.64	1.00	2.00	4.00
Distance to closest port (km)	83,670	769.87	436.47	403.71	743.93	1,112.38
Distance to border (km)	81,350	152.39	127.28	51.00	118.00	221.00
Distance to capital (km)	83,670	611.40	393.55	303.00	514.00	875.00
Distance to natural resources (km)	83,670	295.07	212.69	126.19	249.10	410.12
Rel. distance to closest port[a]	83,670	0.59	0.24	0.41	0.62	0.78
Rel. distance to border[a]	81,350	0.35	0.25	0.14	0.30	0.53
Rel. distance to capital city[a]	83,670	0.47	0.24	0.27	0.45	0.65
Rel. distance to nat. res.[a]	83,670	0.45	0.25	0.24	0.43	0.65
ln agr. com. shock	72,450	10.23	0.91	9.86	10.33	10.76
Exposure to crises	82,630	0.07	0.12	0.00	0.02	0.06

From ACLED, UCDP-GED, PRIO and authors' computations.
[a] Relative to maximum distance, computed by country.

are provided in table 2, and the online appendix contains more details about the construction of these variables.

Temporary shock: Agricultural commodities. A number of papers have tried to identify the effect of commodity shocks on the likelihood of conflict across countries. Little work has been done within country (with the notable exception of Dube & Vargas, 2013, focusing on Colombia). In the following, c denotes a cell, p an agricultural commodity (product), i the country to which the cell belongs, and t the year. Our objective is to compute a time-varying cell-specific measure of external demand for the commodities produced by the cell of the form

$$\mathrm{WD}_{ct} = \sum_{c} \alpha_{pc} \times M^W_{ipt} \text{ where } M^W_{ipt} = \sum_{j \neq i} M^W_{jpt}, \qquad (1)$$

where α_{pc} is the share of agricultural commodity p in cell c and M^W_{ipt} is the world import value of commodity p in year t minus the imports of country i.[14] Using the world value of imports instead of world prices allows us to consider a wider range of commodities, including commodities that do not have a world price.[15] Data on M^W_{ipt} are provided by UN-Comtrade. To measure α_{pc}, we use three alternative sources.

Baseline shock: FAO Agromaps. First, we use FAO Agromaps information to obtain a region-specific measure of agricultural specialization. The FAO agromaps data

[14] When multiple years of data are available, we use the average share but perform a number of robustness checks with alternative shares. See the discussion that follows.

[15] Earlier versions of our paper also checked that our results are robust to the use of commodity price variations using the data from Bazzi and Blattman (2014) and use of the quantity component of M^W_{ipt} only.

contain information on the volume of production of different agricultural commodities at the sub-national level for a number of years. Agromaps use the second administrative level boundaries (SALB) defined by the UN based on national administrative units. These administrative units appear in light gray on maps A.1 to A.6 in the online appendix. When a cell contains multiple regions, we sum the shock variable across regions and weight by the share of the cell's area occupied by each region. For each commodity, we obtain the value of production by multiplying the volume provided by the FAO by unit values computed from UN-Comtrade data. We consider here seventy commodities, such as bananas, cocoa, coffee, or tomatoes, and focus on the post-1989 period to be able to match the product classification with HS trade data from UN-COMTRADE.[16]

The FAO agromaps data cover the period 1982 to 2011, but the data are generally available only for a small number of years within this time period for each country. In our baseline estimations, we use the average share of each commodity in the total agricultural production value of the region over the available period for the computation of α_{pc}. However, we show that the results are similar when using alternative shares, including shares computed over the 1982–1993 period (in which case we run the estimations on the post-1993 period) or binary shares that equal 1 if region r has produced the commodity c at least one year over the period. Finally, another potential issue is that country-wide conflicts might affect M_{ipt}^{W} if the country is a large exporter or importer of the commodity. We show that our results are robust to the exclusion of the commodities for which the country exports or imports represent more than 1% of world trade value.

Alternative measures of agricultural specialization: M3 crops and suitability. The FAO agromaps data contain actual production for a long time period and cover most sub-Saharan African countries. However, they also contain many missing values and are available at a higher level of aggregation than our level of observation, which might cause measurement error. The fact that it focuses on actual rather than potential production might also be a source of endogeneity. To check the robustness of our results, we rely on two additional sources. These are based on GIS raster data and therefore contain more geographically disaggregated information, which allows us to compute two alternative versions of α_{pc} at the level of the cell. More details are provided in the online appendix.

First, we use the M3-CROPS data from Monfreda, Ramankutty, and Foley (2008), which contain information on the harvested area in hectares for 137 crops at a resolution of 5 arc minutes $\times$5 arc minutes for the year 2000 (also used in Harari & La Ferara, 2012). This data set has a different approach from the FAO agromap data. It focuses on the land use and does not provide information on the production. It has the advantage of being more finely grained and includes more crops than FAO agromaps (Monfreda et al., 2008). However, it is available only for the year 2000.

Second, we consider the suitability of a cell for cultivating 45 crops from the FAO's global agroecological zones (GAEZ).[17] These data are constructed from models that use location characteristics such as climate information (rainfall and temperature, for instance) and soil characteristics. This information is combined with crops' characteristics (in terms of growing requirements) to generate a global GIS raster of the suitability of a grid cell for cultivating each crop. Suitability is then defined as the percentage of the maximum yield that can be attained in each grid cell. Following Nunn and Qian (2011) and Alesina, Giuliano, and Nunn (2011), we define a cell as suitable for a crop if it can achieve at least 40% of the maximum yield. The main advantage of these data is that crop suitability is exogenous to conflicts because it is not based on actual production.

Note that we interpret an increase of WD_{ct} as a positive income shock for region, despite the fact that we do not know whether production is actually exported or sold domestically. Indeed, even if the product is not exported, our shock might have an effect, albeit lower, on income, as world demand might affect the domestic prices of the commodities produced by a given region. Moreover, as we explain in more detail in the next section, we interact our shocks with measures of trade openness computed at the level of the cell. For a given level of production, the most opened regions are more likely to be net exporters of the commodity. Finally, we compute an alternative version of WD_{ct} that concentrates on the commodities that are exported at some point by the countries over the sample period and show that our results are unchanged.[18]

Changes in the demand for agricultural commodities are generally modest and can be considered temporary.[19] Our second type of external demand shocks is based on large foreign events—financial crises—that might affect domestic income more importantly and more durably.

Long-lasting shock: Banking crises. Our next measure of income shock is the exposure of the country to financial

[16] The data section of the online appendix contains the complete list of commodities, as well as the years for which the production data are available for each country. It also discusses extensively potential sources of measurement error in the FAO agromaps data and their consequences.

[17] See Nunn and Qian (2011) for an excellent discussion of the FAO-GAEZ data.

[18] Still, some regions in principle could be net importers of the commodities they produce (this would, however, be difficult to reconcile with our results), which would complicate the interpretation of our variable. This would be the case for populous regions with little production capacity that are heavily biased toward certain commodities. The fact that some regions might be net importers of the goods they produce would tend to bias downward our coefficients (of both the shock and its interaction with distance if the most open regions are also net importers). We control for cell population and GDP per capita in our estimations. Moreover, the use of GAEZ data ensures that we are not not capturing consumption patterns.

[19] Section A.14 in the online appendix confirms this assertion in our sample. We regress the log change of our baseline agricultural commodity shock, based on agromaps data, on its first, second, and third lags, controlling for year dummies and four-digit product fixed effects. We fail to find evidence of persistence.

crises in the rest of the world.[20] Financial crises destroy trade and are arguably exogenous to trading partners' economic or political situation (especially if the trading partner is a small African economy). Importantly, they typically last several years (on average 4.3 years in our sample) and have persistent effects on the real economy (Cerra & Saxena, 2008) and on imports (Abiad, Topalova, & Mishra, 2011), especially when the origin country is in sub-Saharan Africa (Berman & Martin, 2012).

For each country i, we compute the following time-varying indicator,

$$\text{Crisis exposure}_{it} = \sum_j \omega_{ij} \times C_{jt}, \tag{2}$$

where j is the destination country and t is the year. ω_{ij} is the average share of destination j in country i's total exports over the period, and C_{jt} is a dummy that equals 1 if destination j experienced a banking crisis during year t. The trade data come from the IMF Direction of Trade Statistics (DOTS) and the crisis data from Reinhart and Rogoff (2011).[21] The *Crisis exposure*$_{it}$ variable therefore represents the number of banking crises in the destinations served by country i, weighted by the average share of each destination in its total exports. It represents a global demand shock on all the goods exported by the country.[22]

As this variable is based on trade shares, we interpret it as a real shock on demand for the country's produced goods, despite the fact that we are looking at a financial event. We consider as unlikely the possibility that the shock affects conflict through the country's financial system. Although the geographical distribution of international financial linkages is closely related to trade in goods (see, e.g., Aviat & Coeurdacier, 2007), sub-Saharan countries' financial systems are arguably too small and closed to generate such an effect.

Note that we have checked that financial crises in the partner countries do affect exports of the countries included in our sample. The results appear in section A.4 in the online appendix. We find that banking crises are associated with an 8% to 11% drop in bilateral imports, a result consistent with Abiad et al. (2011) and Berman and Martin (2012), among others.

C. Natural Openness

All the shocks described are based on variations in the foreign demand for the goods produced by the country or region to which the cell belongs or by the cell itself. As these are income shocks based on international trade, we expect them to have a lower impact on the cells that are naturally less open (i.e., the cells for which trade costs are higher). Income in these cells might be primarily driven by self-consumption and disconnected from the world market.

We therefore construct measures of natural trade openness, which we then interact with our external income shocks. This has first an identification purpose: to ensure that we are identifying the effect of (exogenous) external foreign demand shocks and not of some other (e.g., internal) shocks that may be correlated with them. Beyond that, it allows us to study how external income shocks affect the geography of conflicts and show that these shocks have heterogenous effects within countries, which to our knowledge has not been done so far. This identification strategy also helps us to reconcile the divergent results found by the cross-country and within-country literatures: the fact that only certain regions, the most opened ones, are affected has implications for the effect of these shocks on country-level conflict outbreaks.

For each cell, we compute the distance (in kilometers) between the cell's centroid and the closest major seaport. We retain the main ports of each country with a maximum draft of at least 10 meters. Note that the closest seaport is not necessarily located in the same country, as some countries are landlocked or some cells closer to a foreign port.[23]

As we are using a cross-country data set, a potential issue with using distance in levels is that it will be on average higher in larger countries. If conflict probability is different in these countries for other (unobserved) reasons, this might bias our results. As a robustness, we systematically verify that our results are unchanged when taking the ratio between this distance and the largest distance observed by country.

D. Other Cell-Specific Data

Our remoteness variables might be correlated with other cell-specific characteristics such as economic activity or closeness to natural resources. To ensure that we are indeed identifying the effect of trade openness, we include in our robustness checks measures of distance between the cell's centroid and the capital city, the closest international border, and natural resource fields. The first two come from PRIO-GRID. The last is computed using information on the latitudes and longitudes of diamond and oil fields from PRIO. Finally, we control for economic activity and size by using data from PRIO-GRID, which itself relies on the G-Econ data set developed by Nordhaus et al. (2006), on the population

[20] As robustness, we also use the African Growth Opportunity Act (AGOA) as an alternative long-lasting shock. See the online appendix, section A.12 for more details.

[21] Reinhart and Rogoff (2011) define a crisis as (1) "bank runs that lead to the closure, merging, or takeover by the public sector of one or more financial institutions; and (2) if there are no runs, the closure, merging, takeover, or large-scale government assistance of an important financial institution (or group of institutions), that marks the start of a string of similar outcomes for other financial institutions."

[22] Again, if a grid cell contains several countries, we use the sum of *Crisis exposure*$_{it}$ weighted by the share of each country in the cell's total area.

[23] The location of seaports can be seen in maps A.1 to A.6 in the online appendix. We show that our main findings are robust to considering seaports with a maximum draft larger than or equal to 12 meters, the threshold used internationally to consider a port as a deepwater one. These ports are defined as deepwater because they can accommodate loaded "Panamax" ships, whose dimensions are determined by the ones allowed by the Panama Canal's lock chambers. We have checked that all our results are unchanged when using this alternative size threshold for seaports.

and GDP of the region.[24] G-econ data contain information about these indicators every five years between 1990 to 2005 for most countries in the world, divided by 1×1 degree grid cells. We assign each 0.5×0.5 degree cell to the 1×1 degree cell to which it belongs. Descriptive statistics about these various measures are provided in table 2.

III. Empirical Methodology

A. Baseline Specification: Microlevel

Our objective is to study the way in which foreign demand shocks affect the likelihood and intensity of conflict within countries. We denote by c a specific grid cell, i a country, and t a year. In general, we estimate a specification of the form

$$\text{Conflict}_{c,t} = \beta\text{shock}_{i,t} + \gamma\text{shock}_{i,t} \times \text{remoteness}_c + \eta_t + \mu_c + \varepsilon_{c,t}, \tag{3}$$

where $\text{Conflict}_{c,t}$ is a variable that captures the incidence, onset, or intensity of a conflict in a given cell during a given year. The variable $\text{shock}_{i,t}$ denotes a shock affecting the external demand for the goods produced by country i or cell c: alternatively (a) the world demand for agricultural commodities produced by the region (equation (1), in which case the variable is cell or region specific, that is, $\text{shock}_{c,t}$) or (b) the exposure to banking crises, equation (2). Finally, remoteness_c represents our inverse measure of the natural trade openness of the cell. In our baseline estimations, this variable is the log of the distance between cell c and the nearest seaport.

In all estimations, we control for time dummies η_t and cell-specific characteristics μ_c. The latter capture time-invariant characteristics that may affect the average likelihood of conflict in a given cell (e.g., the distance to the closest port, to the capital, natural resources, or the region's roughness). Cell fixed effects also capture potential systematic difference in terms of press coverage (and therefore reporting of events) across regions. In a second step, we show that our results are robust to the inclusion of additional interactions terms between $\text{shock}_{i,t}$ and other cell-specific characteristics.

The sign of β is theoretically ambiguous, as explained in more detail in section V. Assume that an increase of $\text{shock}_{i,t}$ represents an exogenous increase in country i's income (e.g., higher demand for the country's products). According to the state-as-prize theory, this larger income should increase the likelihood of conflict by increasing the value of the state that can be captured through rebellion; β should be positive in this case. On the contrary, the opportunity cost theory predicts that this larger income should increase the opportunity cost of fighting, therefore reducing the risk of conflict; β should be negative. But a negative estimate of β can be also interpreted as evidence in favor of the state capacity channel. The increase in country i's income provides the state with the financial means to strengthen the control of opponents or buy off opposition. Section V presents a number of tests that incite us to favor the opportunity cost mechanism.

We expect β and γ to be of opposite signs: the most remote cells face larger trade costs, are more inward oriented, and should be relatively less affected by foreign income shocks. These shocks should therefore influence the geography of conflicts.

Our results represent a local average treatment effect in the sense that they reflect the impact of our shocks on relatively open regions. Does it mean that we capture only specific types of conflicts? Put differently, are our income shocks triggering only certain conflicts? It would be the case, for instance, if open regions were systematically located away from international borders; our methodology would be less likely to identify separatists events. This of course matters for the interpretation of our results and their external validity. The online appendix (section A.5) contains a general discussion of this issue. We argue that the type of conflicts occurring in the cells that we identify as being open are not, on average, different from the conflicts observed in the sample in general. We also show that our results hold within specific conflicts, that is, within a given dyad of actors.

B. Econometric Issues

Conflict incidence. We assess the effect of external shocks on the incidence of conflict. We first estimate a probabilistic model of the form

$$\Pr(\mathit{Conflict}_{c,t} > 0) = \beta_1 \mathit{shock}_{i,t} + \gamma_1 \mathit{shock}_{i,t} \times \mathit{remoteness}_c + \eta_t + \mu_c + \varepsilon_{c,t}, \tag{4}$$

where the dependent variable is conflict incidence, a dummy taking the value 1 if cell c experienced a conflict during year t. The cleaner way to estimate this specification is through a conditional logit estimator that accounts for all cell-specific time-invariant unobserved characteristics. This is our preferred estimator, but it has two drawbacks. First, it drops all the cells for which the outcome of interest does not vary over the entire period, that is, all cells in which conflicts always or never occur. Second, it makes the size of the coefficients difficult to interpret. Therefore, we systematically report the results obtained with a linear estimator (LPM) with cell fixed effects.

Conflict onset, ending, and intensity. A potential issue with using conflict incidence as a dependent variable has recently been raised by the macrolevel literature. Conflict being a persistent variable, one should estimate a dynamic model with the lagged conflict variable included on the right-hand side or, equivalently, model onset and ending separately (Beck & Katz, 2011; Bazzi & Blattman, 2014). Note that the problem is less clear in our case, as local conflict incidence is much less persistent than country-specific incidence. At the cell level, the vast majority of events, around 75%, do not last more than two years.

[24] See the online appendix for more details about the variables described in this section.

We systematically investigate the robustness of our results to using conflict onset or ending as dependent variables. We define conflict onset as the occurrence of a conflict in cell c, year t, conditional on $Conflict_{c,t-1} = 0$ (the variable is coded as "missing" for ongoing conflicts). Conflict ending is defined as $Conflict_{ct} = 0$ conditional on $Conflict_{c,t-1} = 1$. We also consider conflict intensity, defined as the number of conflict events observed in cell c during year t.

Country-level conflict outbreak. The above specification provides information on the effect of external income shocks on the likelihood of conflicts within a given cell in general, that is, not conditioning on whether a conflict is already taking place elsewhere in the country. It might be the case, however, that income shocks have an effect on the way in which conflicts evolve within countries over time, without being necessarily at the source of the outbreak of the event. In order to better understand whether external income shocks influence the outbreak of a civil conflict, we estimate a variant of equation (4) where we condition on conflict, onset at the country level,

$$\Pr(Conflict_{c,t} > 0 | Conflict_{i,t-1} = 0) = \beta_1 shock_{i,t} + \gamma_1 shock_{i,t} \times remoteness_c + \eta_t + \mu_c + \varepsilon_{c,t} \quad (5)$$

where $Conflict_{i,t-1}$ equals 1 if at least one violent event is recorded in country i during year $t-1$. This specification allows us to study whether external income shocks affect the location of conflicts when a civil conflict starts and, in general, whether these shocks are significant determinants of conflicts outbreak at the country level.

Standard errors. In all estimations, we use robust standard errors, clustered at the regional level, where a region is defined at the SALB-ADM1 level, which is the level of geographical aggregation of our baseline agricultural commodities shock. We also check that our results are robust to a nonparametric estimation of the standard errors allowing for both cross-sectional spatial correlation and cell-specific serial correlation (Conley, 1999; Hsiang, Meng, & Cane, 2011)[25] or, alternatively, to clustering at country-year level.[26]

[25] We have also tried to include spatial covariates in the estimations: the average agricultural commodity shock or the number of conflicts within a 100 kilometer radius around the cell, in the spirit of Harrari and La Ferrara (2012), to control for the spatial correlation and diffusion of shocks and violence. Our results stay similar.

[26] When standard errors are clustered at some administrative level (region or country), we face the issue that a cell can contain several administrative units. In this case, we assign a main country or region to the cell, as defined as the country or region with the highest share of the cell's total area. Note that we consider administrative units that are defined at the end of the period and fixed over time; we do not consider changes in international or regional borders as these are potentially endogenous to conflict. Note, however, that distance to capital and to international borders, which are taken from PRIO-GRID, are time varying; they take into account changes in international borders, which occurred in Erithrea (1993), Ethiopia (1993), Namibia (1990), and South Africa (1990) during our period of study.

C. *Relation with the Cross-Country Literature: Macrolevel*

Because we are using cell fixed effects, our results should be interpreted as the effect of external shocks within a given cell, over time. By studying how the probability of conflict varies for each cell, we are implicitly studying the intensity of conflict at the country level: an increase in the probability of conflict on average across cells implies a magnification of conflict intensity at the country level. To ease the comparison between our results and those of the existing literature (Bazzi & Blattman, 2013), we perform a number of additional estimations at the country level. More precisely, we study the effect of our various income shocks on conflict onset, incidence, or intensity at the country level, that is, estimate a specification of the form

$$Conflict_{i,t} = \beta shock_{i,t} + \eta_t + \mu_i + \varepsilon_{i,t}, \quad (6)$$

where $Conflict_{i,t}$ denotes conflict incidence (a dummy that equals 1 if at least one violent event was recorded during year t in country i), onset (a dummy that equals 1 if at least one violent event was recorded during year t in country i, but no violent event was recorded in $t-1$),[27] ending (a dummy that equals 1 if no violent event was recorded in year t but a least 1 was recorded in $t-1$), or intensity (number of cells with violent events, or total number of violent events observed in country i during year t). Finally, in all estimations, we include time dummies η_t and control for country-specific time-invariant unobservable characteristics through the inclusion of country fixed effects μ_i.

IV. Microlevel Results

A. *Temporary Shocks: Demand for Agricultural Commodities*

Baseline results. We first consider agricultural commodity shocks. As mentioned earlier, we use an indicator of income shocks based on the agricultural specialization of the region to which the cell belongs, that is, the foreign demand for the region agricultural products as defined by equation (1). Our baseline estimations are based on FAO agromaps data. We consider the impact of changes in foreign demand on the probability of conflict within a given cell. We further interact this variable with the remoteness of the cell, proxied by the distance to the nearest seaport. Changes in foreign demand are expected to affect less the most remote locations, for which trade costs are higher—and therefore trade openness is naturally lower.

Our baseline results are shown in table 3. Panel A contains estimations in which the effect is assumed to be the same across regions. Panel B includes the additional interaction term between our shock variable and distance to the closest seaport. Columns 1 and 2 use UCDP-GED conflict data, columns 3 and 4 ACLED I, and columns 5 and 6 ACLED II data. Finally, odd-numbered columns contain FE-logit estimations, and even-numbered ones show LPM results. Most

[27] This variable is coded as "missing" for ongoing conflicts.

TABLE 3.—AGRICULTURAL COMMODITIES DEMAND AND CONFLICT

Dependent Variable:	Conflict Incidence		Conflict Incidence		Conflict Incidence	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)
Panel A						
ln agr. shock	−3.211***	−0.069***	−2.800***	−0.008	−2.599***	−0.058**
	(0.666)	(0.018)	(0.898)	(0.015)	(0.937)	(0.023)
Panel B						
ln agr. shock	−5.474***	−0.250***	−6.922***	−0.113**	−6.371***	−0.341***
	(1.131)	(0.065)	(1.924)	(0.045)	(1.780)	(0.088)
ln agr. shock × remoteness[a]	0.458***	0.031***	0.794***	0.018***	0.667***	0.047***
	(0.153)	(0.009)	(0.260)	(0.007)	(0.258)	(0.013)
Panel C						
ln agr. shock	−4.051***	−0.121***	−4.483***	−0.044***	−3.787***	−0.114***
	(0.633)	(0.027)	(1.105)	(0.016)	(1.118)	(0.034)
ln agr. shock × remoteness[b]	2.550***	0.104***	2.996***	0.071***	2.571**	0.112***
	(0.515)	(0.027)	(0.854)	(0.018)	(1.007)	(0.040)
Sample	UCDP-GED		ACLED 1		ACLED 2	
Years	1989–2006	1989–2006	1989–2005	1989–2005	1997–2006	1997–2006
Number of countries	39	45	12	12	41	44
Observations	26,208	130,500	6,545	41,055	13,900	72,450

Significant at *10%, **5%, ***1%. Robust standard errors are clustered by administrative region in parentheses (see section A.15 in the online appendix for robustness allowing for spatial serial correlation and other types of clustering). All estimations include year dummies and cell fixed effects.
[a]ln distance to closest seaport.
[b]Distance to closest seaport relative to maximum distance, computed by country.

of the other tables in this paper are organized in the same way.

An increase in the world demand for the region's agricultural commodities generally decreases the probability of conflict incidence within cells. This result is robust across conflict data sets, except in column 4 (panel A). However, not all cells are equally opened to trade and therefore equally likely to be affected by foreign demand. In panel B, we find that the effect is heterogeneous across cells. The coefficient on the interaction between remoteness and our shock variable is always positive and significant; that is, the probability of conflict in the least open locations is significantly less affected by changes in the world demand for the commodities produced by the cell. This result is extremely robust across data sets. Quantitatively, the effect is not negligible. For the seaport itself, a standard deviation increase in foreign demand decreases the conflict probability by 1 (column 4) to 3 (column 6) percentage points (to be compared with an unconditional probability of conflict of between 2% and 4% depending on the sample). Around 1,000 kilometers from the seaport, however, no statistically significant effect can be detected in any of the estimations.[28]

In panel C of table 3, we test the robustness of our results using an alternative indicator of trade openness: distance to the nearest seaport relative to the maximum distance computed by country. This prevents the value of the variable from being systematically higher in large countries, the case for the level measure used in the baseline estimations. On the other hand, this ratio is, by construction, bounded between 0 and 1, and it tends to underestimate the effect of large within-country distances. Qualitatively, our results are very similar. In the least open cells, conflict incidence is found to be significantly less affected by external changes in agricultural commodities demand. Note that the quantitative interpretation of our results is in this case straightforward. For instance, a standard deviation increase in foreign demand leads to a 4 to 10 percentage point decrease in conflict probability depending on the cells. On the contrary, summing the coefficients in columns 2, 4, or 6, we see that the effect is always statistically insignificant for the most remote locations.

Additional regressors. Our remoteness measures might be correlated with a number of characteristics of the cells affecting the way in which they react to external shocks. These include, for instance, economic size or the distance to the countries' political center. The correlation between the distance to seaports and distance to the capital city is indeed positive and statistically significant (around 0.45). One can argue that we might be identifying the effect of economic activity or political influence rather than the effect of trade openness.

In table 4, we add to our baseline estimations interaction terms between our shock variable and (a) the log of distance to the capital city,[29] (b) the log of the distance to the closest international border, (c) the log of distance to the closest natural resources field (oil, gas, and diamond), (d) the log of GDP of the area in 2000, and (e) the log of the population of the area in 2000. Two results are worth mentioning. First, the effect of our agricultural commodity shock, as well as its interaction with the distance to seaports, is robust to the inclusion of these variables. The interaction terms between the shock variables and the distance to seaports remain significant in all

[28] Section A.16 of the online appendix provides an illustration of these results using specific examples of commodities and countries.

[29] Section A.7 in the online appendix reports very similar results using distance measures computed as ratios as in table 3, panel C.

TABLE 4.—AGRICULTURAL COMMODITIES DEMAND AND CONFLICT: ROBUSTNESS

Dependent Variable:	Conflict Incidence		Conflict Incidence		Conflict Incidence	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)
ln agr. shock	−6.658**	−0.205**	−7.226	−0.247***	−14.284***	−0.335**
	(2.971)	(0.095)	(4.511)	(0.075)	(3.903)	(0.156)
ln agr. shock × remoteness[a]	0.306**	0.032***	0.445	0.022***	0.562**	0.054***
	(0.147)	(0.011)	(0.367)	(0.008)	(0.262)	(0.015)
ln agr. shock × ln dist. to capital	−0.164	−0.009	0.313	0.004	0.360	0.015
	(0.187)	(0.009)	(0.299)	(0.010)	(0.285)	(0.015)
ln agr. shock × ln dist. to border	−0.282**	−0.010***	−0.257	−0.012***	−0.424**	−0.013
	(0.120)	(0.003)	(0.193)	(0.004)	(0.183)	(0.008)
ln agr. shock × ln dist. to nat. res.	0.310**	0.014**	0.457**	0.018***	0.760***	0.040***
	(0.144)	(0.006)	(0.220)	(0.004)	(0.227)	(0.012)
ln agr. shock × ln GDP area	−0.231	−0.000	0.175	0.013**	0.095	0.023***
	(0.168)	(0.006)	(0.234)	(0.006)	(0.246)	(0.007)
ln agr. shock × ln pop. area	0.211	−0.003	−0.004	0.007	0.417	−0.023***
	(0.166)	(0.005)	(0.317)	(0.005)	(0.257)	(0.006)
Sample	UCDP-GED		ACLED 1		ACLED 2	
Years	1989–2006	1989–2006	1989–2005	1989–2005	1997–2006	1997–2006
Number of countries	38	43	12	12	40	43
Observations	25,902	125,101	6,460	40,800	13,720	69,500

Significant at *10%, **5%, ***1%. Dist. to nat. res.: distance to nearest natural resource field (oil, gas or diamond). ln GDP and pop. area: PPP GDP and population of the area in 1990, from G-Econ. Robust standard errors, clustered by administrative region in parentheses. All estimations include year dummies and cell fixed effects.
[a] ln distance to closest seaport.

specifications but column 3, and the estimated coefficients are quantitatively very close to our baseline estimates. Second, and important, apart from distance to natural resources, none of the additional interaction terms have a robust effect across estimations. This is in particular the case for the interactions with distance to the capital city and with the GDP of the area. This clearly suggests that we are capturing an income effect of external shocks on conflict that channels through international trade rather than an effect related to the economic size or the political instability of the location.

Note that our shock has a larger effect in cells located close to a natural resource field (this is also the case when we consider exposure to crises). This suggests that income shocks play a more important role in more unstable cells. The online appendix (section A.6) contains a number of estimations consistent with this idea. We restrict the sample to high-risk cells (those in which at least a conflict happens over the period) or include interaction terms between our shocks and the level of past instability through the inclusion of the cumulated number of years in which a conflict was observed in the cell before year t. Qualitatively, our results are unchanged. But interestingly, we find that the effect of our shock is much larger in these politically unstable cells.

Alternative measures of agricultural shocks. Both the FAO agromaps data and the way in which we compute the shock have potential drawbacks, as we discussed in section IIB. We perform two additional types of checks: the first uses modified versions of our agricultural commodity shocks but still focuses on the agromaps data; the second uses different data sources.

Our baseline estimates use the average share of each commodity in the total agricultural production value of the region over the available period. Using weights computed at the beginning of the sample period would result in an important loss of observations due to missing production data for most regions for early years. Missing production data is also a problem, as it can create measurement error. We compute alternative versions of our shock variables (table A.1 in the table appendix). In panel A, we use binary weights—weights that equal 1 if the commodity is produced by the region at some point over the period, 0 otherwise. In panel B, we use weights computed on the pre-1993 period. In this case, we run the estimations on the post-1993 period only. The sample size is drastically reduced in panel B, but the results are very robust and stable, if anything, they are slightly strengthened.

A second issue with our variable is that it might be endogenous to local conflicts if the cell is a large enough exporter or importer of the commodity to influence the world demand. Panel C of table A.1 shows that our results are robust to the exclusion of all commodities countries that exports or imports represent more than 1% of world trade value. Finally, we also provide estimations based on a version of the shock that concentrates only on the commodities that are exported at some point by the countries over the sample period (panel D). This drops 5% to 10% of the observations depending on the sample but leaves the point estimates unchanged.

All of these estimations are based on FAO agromaps data, whose main advantages are to contain actual production data and cover a long time period. But they again have many missing values, are quite geographically aggregated, and actual production might be to some extent endogenous. Tables A.2 and A.3 in the table appendix replicate our baseline results using two alternative data sources to measure the agricultural specialization of the cell. Table A.2 uses M3 crop data, which contain more finely grained data and are quasi-exhaustive in terms of geographical coverage but are available only for the year 2000. Table A.3 shows the results using FAO-GAEZ

TABLE 5.—EXPOSURE TO CRISES AND CONFLICTS

Dependent Variable:	Conflict Incidence		Conflict Incidence		Conflict Incidence	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)
Panel A						
Exposure to crises	−0.434	−0.008	−0.477	−0.029***	1.851	0.045
	(0.513)	(0.011)	(0.901)	(0.010)	(1.271)	(0.035)
Panel B						
Exposure to crises	5.694**	0.242***	10.700***	0.076	16.624***	0.720**
	(2.212)	(0.080)	(2.570)	(0.053)	(5.596)	(0.289)
Exp. to crises × remoteness[a]	−0.968**	−0.038***	−1.844***	−0.016**	−2.162**	−0.100**
	(0.379)	(0.013)	(0.473)	(0.008)	(0.852)	(0.042)
Panel C						
Exposure to crises	1.888***	0.056***	1.579*	−0.020	7.785***	0.183**
	(0.699)	(0.019)	(0.938)	(0.015)	(1.899)	(0.084)
Exp. to crises × remoteness[b]	−4.314***	−0.114***	−4.354**	−0.017	−9.337***	−0.241*
	(1.531)	(0.041)	(1.989)	(0.021)	(3.150)	(0.125)
Sample	UCDP-GED		ACLED 1		ACLED 2	
Years	1989–2006	1989–2006	1989–2005	1989–2005	1997–2006	1997–2006
Number of countries	40	46	12	12	42	44
Observations	28,566	148,842	11,336	70,200	15,250	82,630

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by administrative region in parentheses (see section A.15 in the online appendix for robustness allowing for spatial serial correlation and other types of clustering). All estimations include year dummies and cell fixed effects.
[a]ln distance to closest seaport.
[b]Distance to closest seaport relative to maximum distance, computed by country.

data, which contain information on the suitability of the cell for producing each crop instead of actual yield or production. Again, our results remain robust and quantitatively similar to our baseline estimations.

B. *Long-Lasting Shock: Financial Crises*

We now consider the exposure of the country to financial crises in its trading partners as an alternative, longer-lasting income shock. This variable has a negative impact on the country's income through lower exports (section A.4 in the online appendix). On the other hand, this impact on income should again affect regions heterogeneously; that is, it should be lower in regions located further away from the main seaports. Table 5 contains the baseline results. Again, we consider conflict incidence with the UCDP-GED data set (estimations 1 and 2 of each panel), ACLED I data set (estimations 3 and 4), and ACLED II (estimations 5 and 6). Panel A uses only the crisis variable, while we add interaction terms between exposure to crises and to the closest seaport, either in logarithm or as a ratio (panels B and C).

On average across cells, the effect of exposure to financial crises in partner countries is generally statistically insignificant (table 5, panel A), which can be due to the fact that the impact is heterogeneous across regions. Introducing the interaction terms between exposure to crises and remoteness confirms this heterogeneity (panel B). For the least remote cells, exposure to financial crises in partner countries increases conflict probability. The interaction term is negative and significant; distance to seaports dampens the effect of negative income shocks on conflict incidence. This is the case when using both nonlinear (FE logit) and linear (OLS) estimators. Note that in some cases, we find that for the most remote locations, being exposed to foreign financial crises actually has a negative and significant effect on conflict probability in some cases (adding up the coefficients in columns 2 and 6, panel C). This result is, however, not robust, in particular to the inclusion of additional interaction terms between the shocks and cell-specific characteristics.

C. *Conflict Onset and Ending*

In this section we model separately conflict onset and ending. Our coefficients might be biased when using conflict incidence if the latter is highly persistent as we do not include lags of the dependent variable (Beck & Katz, 2011) as regressors. At the local level, conflict is much more transitory, which lessens the problem. Still, the processes underlying outbreaks and endings might differ, and using conflict incidence implicitly constrains them to be the same.

We relax this constraint by considering alternatively onset ($Conflict_{ct} = 1|Conflict_{ct-1} = 0$) and ending ($Conflict_{ct} = 0|Conflict_{ct-1} = 1$) as dependent variables. The results are shown in table 6. We consider both agricultural commodities shocks and exposure to financial crises and both logit and LPM estimations. Table 6 considers UCDP-GED data; the complete results using the other conflict data sets (ACLED I and ACLED II) and all of our shock variables are shown in the online appendix, section A.8, which also considers conflict intensity (defined as the number of events in the cell during the year) as an alternative dependent variable.

Our results on conflict onset are extremely robust (columns 1 to 4).[30] In the case of conflict ending, the coefficients are

[30] The fact that conflict is not persistent is apparent when one looks at the number of observations in conflict-onset estimations, which is extremely close to our baseline estimates on conflict incidence.

TABLE 6.—CONFLICT ONSET, ENDING, AND INTENSITY

Dependent Variable:	Onset				Ending			
Shock:	Agricultural Commodities		Crises		Agricultural Commodities		Crises	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)	FE Logit (7)	FE-LPM (8)
Panel A								
Shock	−3.541***	−0.040***	−0.306	−0.005	1.224***	0.137***	0.680*	0.027
	(0.540)	(0.007)	(0.571)	(0.006)	(0.385)	(0.032)	(0.404)	(0.029)
Panel B								
Shock	−6.125***	−0.110***	5.613**	0.078*	3.232***	0.300***	−0.342	−0.228
	(0.929)	(0.019)	(2.445)	(0.045)	(1.004)	(0.069)	(1.881)	(0.223)
Shock × Remoteness[a]	0.502***	0.012***	−0.899**	−0.013*	−0.390**	−0.031***	0.157	0.042
	(0.130)	(0.003)	(0.397)	(0.007)	(0.171)	(0.011)	(0.279)	(0.034)
Observations	22,688	128,899	24,817	147,099	7,177	13,155	7,900	14,789

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by administrative region in parentheses. All estimations include year dummies and cell fixed effects. Conflict events data from UCDP-GED.
[a] ln distance to closest seaport.

statistically significant for agricultural commodities, not for financial crises, even though quantitatively they are similar to our baseline estimates using conflict incidence (comparing column 8 of table 6 to column 2 of table 5). The fact that the estimates for conflict ending are not as precise as the onset ones is not surprising given the much smaller sample size.

D. *Additional Robustness*

We proceed to a battery of additional robustness checks, through which we make sure that our baseline results from tables 3 and 5 are not sensitive to the use of alternative estimation techniques, samples, or controls variables.

These include, in particular, (a) controlling for past instability through the inclusion of the cumulated number of years during which a conflict was observed in the cell before year *t* (section A.6 in the online appendix); (b) including additional cell-specific controls in the estimations using exposure to crises as a shock (section A.7); (c) dropping potential outliers, that is, countries or cells at the top or bottom of the distribution in terms of number of conflict events (section A.9); (d) adding country-specific time trends or country-year dummies to control for country-specific temporal trends in the causes of conflict (section A.10);[31] (e) dropping each country separately from the estimations (results available on request); (f) considering only deepwater seaports (section A.11); (g) adding a number of non-African countries contained in ACLED (section A.13); (h) the use of the African Growth Opportunity Act as an alternative income shock (section A.12); and (i) allowing for cross-sectional spatial correlation and cell-specific serial correlation (Hsiang et al., 2011), or alternatively for different levels of clustering of the standard errors (section A.15).

V. Discussion and Theoretical Interpretation

As we mentioned in section I, the effect of income shocks on conflicts is theoretically ambiguous.[32] Our results can be understood using contest theories, in which the probability of conflict depends on a trade-off between production and expropriation. In these models (Haavelmo, 1954, and Hirshleifer, 1989, among others), appropriation is modeled as a contest success function in which the probability of winning depends on the fighting technology, which is defined broadly and may include, for instance, the geographical conditions. In case of success, the individuals appropriate the opponent's economic production, which represents an opportunity to gain. But individual participation also depends on the opportunity cost of fighting, which is itself a positive function of income (Grossman, 1991; Besley & Persson, 2011). A positive income shock (say, an increase in production) therefore has two opposite effects: on the one hand, it increases the "prize," that is, the resources that can be appropriated by exerting violence;[33] on the other hand, it decreases individuals' incentives to fight by increasing the opportunity cost of insurrection.

Is our result that positive income shocks decrease conflict probability within cell sufficient to argue in favor of the opportunity cost mechanism? It is not; conflict risk might as well decrease when a country experiences good shocks because they provide the state with the financial means to strengthen the control of opponents or buy off opposition (Fearon & Laitin, 2003). In principle, our results could reflect this state capacity effect. This section details the reasons that incite us to favor the opportunity cost interpretation.

The first reason is that distance to the capital city does not seem to play a role in our estimations. Intuitively, the state capacity effect should indeed be more prevalent in regions located close to the political center of the country, where the

[31] In the case of crises, when country-year dummies are included, the coefficients on the interaction term (the effect of the shock alone cannot be identified in this case, as it is country-year specific) display the expected sign but fail to reach significance in some cases, especially with the ACLED I data set. These specifications are, however, very demanding. Given that we focus on relatively rare events in these estimations and only twelve countries, these results should probably be taken with caution.

[32] For more exhaustive surveys on the theories of conflict, see Garfinkel and Skaperdas (2007) or Blattman and Miguel (2010).

[33] See Fearon (2006) for a theoretical contribution using a contest model or Chassang and Padro-i Miquel (2009) who use a bargaining approach. For empirical evidence, see Cotet and Tsui (2013), Lei and Michaels (2011), or Ross (2006).

Table 7.—Channels of Transmission

Dependent Variable:	ln GDP per Capita		Military Spending				Conflict Incidence			
Shock:	Agricultural Commodities	Crises	Agricultural Commodities	Crises	Agricultural Commodities	Crises	Agricultural Commodities		Crises	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock	0.512**	−0.445***	−0.152	−0.447**	0.060	−0.269*	−0.042	−0.161***	0.009	0.240***
	(0.048)	(0.140)	(0.133)	(0.178)	(0.128)	(0.158)	(0.041)	(0.059)	(0.047)	(0.118)
Shock × Remoteness[a]	−0.029*	0.004						0.018***		−0.030**
	(0.004)	(0.013)						(0.006)		(0.015)
Shock × Rev. Mobilization							0.001	0.004	−0.004	−0.014
							(0.010)	(0.009)	(0.013)	(0.013)
Sample estimator	FE-LPM		FE-LPM				UDCP-GED, FE-LPM			
Observations	26,188	29,766	597	626	615	645	114,804	114,804	124,578	124,578

Significant at *10%, **5%, ***1%. Robust standard errors in parentheses, clustered by cell in columns 1 and 2, country-year in columns 3 to 6, and administrative region in columns 7 to 10. All estimations include year dummies and individual fixed effects (cell fixed effects in columns 1, 2, and 7 to 10, and country fixed effects in columns 3 to 6). Estimations 1 and 2 include interactions between the shock variable and distance to the capital city, distance to border, and distance to natural resource fields. GDP per cap.: GDP per capita from G-Econ. Military spending: country-level military spending from SIPRI, in level in columns 2 and 3, as a share of GDP in columns 4 and 5. Rev. mobilization: efficiency of revenue mobilization from QOG.

[a] ln distance to closest seaport.

influence of the state is stronger. This would be consistent with Buhaug (2010), who finds that conflicts are more likely to be located far from the capital in countries with more powerful regimes. However, we have already seen in table 4 that the coefficient on the interaction term between distance to capital city and our shock is not significant. It is also the case when using alternative shocks such as financial crises.

The second argument is that our variables are indeed significantly correlated with local GDP per capita. In table 7, columns 1 and 2, we regress the log of GDP per capita of the cells on our shock variable (agricultural commodities demand and exposure to financial crises) and their interaction with remoteness. These estimations include year dummies, cell fixed effects, and additional interactions between our shocks and distances to capital city, border, and natural resource fields. The data on GDP per capita come from G-Econ, which contains geolocalized economic data by slightly more aggregated cells (1 × 1 degree), for four years in our sample (1990–2005, every five years). Of course, local GDP per capita data are extremely difficult to measure, which is why these results should be interpreted cautiously. However, we find that our two shocks have, respectively, strong positive and negative effects for the least remote locations. A larger distance to seaports dampens these effects, although the coefficient on the interaction term is significant only in the case of the agricultural commodity shock.[34]

Another way to test for the relevance of the state capacity mechanism is to use country-level proxies for state capacity. In the spirit of Cotet and Tsui (2013), we first consider the effect of our shocks on military spending. If the negative effect of income shocks on conflict probability that we observe were due to an improvement of state capacity, we should observe an increase in the level of military spending at the country level. We use data from the Stockholm International Peace Research Institute (SIPRI). In columns 3 and 4, we consider the level of expenditures, while columns 5 and 6 use spending as a share of GDP. The estimated coefficients are either statistically insignificant or negative.

The last test we consider is the following: the state capacity effect should be more prevalent in countries characterized by a more efficient system of revenue mobilization. We proxy the efficiency of revenue mobilization using data from the World Bank's IDA Resource Allocation Index (IRAI), which is itself built from the results of the annual Country Policy and Institutional Assessment. We interact this variable with our income shock proxies. As shown in columns 7 to 10, these interaction terms are systematically insignificant.

All in all, we favor the opportunity cost interpretation in our case because (a) distance to capital city does not matter, (b) local GDP per capita is correlated with our shocks, (c) our shocks do not affect military expenditures, and (d) our shocks do not have a stronger effect in states where revenue mobilization is more efficient. Of course, all these tests are indirect, so that we cannot totally rule out the state capacity mechanism. It might also be the case that it is a prevalent mechanism for other types of income shocks, for instance, in the case of large income changes driven by resource booms, which more directly affect the revenues of the state (Cotet & Tsui, 2013).

VI. Country-Level Results

The results presented in the previous sections suggest that external income shocks affect the probability of conflict within cells and that their effect is heterogeneous across cells. This implies that these shocks affect the geography of conflict and conflict intensity at the country level. However, they do not allow us to determine whether they are significant determinants of conflict outbreak at the country level. We now consider the effect of our external demand shocks on conflict at the country level (see equation [6]). We pursue two alternative methodologies. In the first, we aggregate our geolocalized conflict data and construct time-varying country-specific measures of conflict incidence, outbreak, ending, and intensity (the total number of events observed

[34] The interaction term becomes significant in the case of exposure to crises when we restrict the sample to countries contained in ACLED I.

TABLE 8.—MACROLEVEL RESULTS

Dependent Variable:	Incidence		Onset		Ending		Intensity
Source:	UCDP-GED	PRIO	UCDP-GED	PRIO	UCDP-GED	PRIO	UCDP-GED
Estimator:	FE-LPM		FE-LPM		FE-LPM		FE-LPM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A							
ln agr. com. shock	−0.160	0.098	−0.098	0.042	0.245**	-0.081	−44.577***
	(0.122)	(0.078)	(0.149)	(0.048)	(0.121)	(0.268)	(17.204)
Observations	774	774	443	733	509	122	774
Panel B							
Exposure to crises	−0.115	0.012	0.065	0.039	0.123	0.146	−0.627
	(0.080)	(0.047)	(0.090)	(0.038)	(0.094)	(0.213)	(8.473)
Observations	1,262	1,262	930	1,180	541	182	1,262

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by country-year in parentheses. All estimations include year dummies and country fixed effects.

TABLE 9.—COUNTRY-LEVEL CONFLICT OUTBREAK: MICRORESULTS

Dependent Variable:	Incidence		Intensity		Incidence		Intensity	
Condition:	Country-Level Onset				Country-Level Onset			
Estimator:	FE-LPM		FE-LPM		FE-LPM		FE-LPM	
Shock:	Agricultural Commodity				Crises			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shock	−0.098	−1.888**	−0.264	−18.834	0.508	1.313**	−0.466	1.769
	(0.200)	(0.854)	(0.725)	(13.270)	(0.406)	(0.576)	(2.735)	(2.818)
Shock × Remoteness[a]		0.269**		2.788		−0.128**		−0.355*
		(0.129)		(2.024)		(0.065)		(0.187)
Observations	3,449	3,449	3,449	3,449	3,449	3,449	3,449	3,449

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by administrative region in parentheses. All estimations include year dummies and cell fixed effects. All estimations are based on UCDP-GED data set.
[a] ln distance to closest seaport.

in a country a given year). We use the UCDP-GED data set, which maximizes the number of years and countries. Alternatively, we directly use country-level data on civil conflicts from UCDP/PRIO data. This maximizes the number of countries (all sub-Saharan Africa) and years (from 1980).

We start by considering agricultural commodities shocks (table 8, panel A). Consistent with our microlevel results, commodity demand has a significant impact on conflict intensity (column 7) and ending (column 5). However, we cannot detect any effect on conflict incidence or onset (columns 1 to 4).[35] These results are globally consistent with Bazzi and Blattman (2014). Similarly, exposure to crisis plays no significant role on any of the outcomes considered (panel B). Because the number of observations is logically much smaller than in our previous estimations, however, this lack of significance might also be the result of a less efficient estimation.

In table 9, we run our estimations at the cell level, but under the condition that no other cell experiences a civil conflict in the same country the year before, as in equation (5). In other words, we are considering the outbreak of new conflicts at the country level, but at a geographically disaggregated level, which improves the efficiency of the estimations. We focus only on the UCDP-GED sample because it is the only one containing enough observations on conflict outbreak for this kind of exercise.

On average, our shocks do not have a significant effect on conflict outbreak; they do not seem to trigger new conflicts at the country-level (columns 1, 3, 5 and 7 of table 9). When we interact them with distance to seaports, however, a different picture emerges. Changes in demand for agricultural commodities and exposure to financial crises have a significant effect on conflict outbreak in the most opened locations (columns 2 and 4). In other words, conditional on country-level outbreak, conflicts are more likely to start in the most open locations following negative income shocks. This is true for both shocks, with the result being slightly more robust when looking at exposure to crisis (i.e., a large and longer-lasting shock).

How can we interpret these findings? First, they illustrate the need to consider finely grained conflict data. In its search for exogenous changes in income, the conflict literature (including this paper) has focused on foreign shocks, such as commodity price changes. These being related to international trade, their effect naturally depends on trade openness, which varies both across and within countries. Considering geographically disaggregated data shows that these shocks do matter once we allow for spatial heterogeneity. A second—purely statistical—reason that running

[35] Note that these insignificant results could be due to measurement error stemming from missing production data in the computation of the agricultural shocks. However, concentrating on countries with the highest coverage or using alternative sources for agricultural specialization leads to the same conclusion.

estimations at the country level might be misleading is that, civil conflicts being rare events, the identification is made on a small number of switches of the dependent variable, which leads to an important loss of efficiency.[36] Using disaggregated data lessens this problem by increasing sample size.

Overall, our results suggest that external income shocks are not the main determinants of conflict outbreak, but that they have a significant effect on conflict intensity and the geography of conflict, that is, on the number and the location of violent events after the start of the conflict. Therefore, although there are probably other, deeper, underlying causes of conflicts, such as long-term institutional issues, ethnic problems, or inequalities, income shocks (even small ones) might importantly affect the geography and intensity of conflicts. In that sense, they might act as threat multipliers, just as the boom in food prices accelerated and intensified the protests during the recent Arab Spring. At this stage, these interpretations are of course only tentative. An interesting extension of this work, which we leave for future research, would be to determine whether conflict outbreak is affected by the interaction between income shocks and with long-term institutional or ethnic issues.

VII. Conclusion

We used in this paper information on the location of conflicts within SSA countries to study the effect of income shocks both within and across countries. In order to reconcile the seemingly contradictory results found by micro- and macrolevel studies, we have proposed a number of alternative ways to identify exogenous income shocks through international trade patterns. First, we have improved the usual measure of temporary commodity shocks using a region-specific measure of agricultural specialization. We also went further by considering a long-lasting shock with the number of banking crises in the country's partners. Second, we combined these shocks with location-specific information reflecting their natural level of trade openness.

Our results are manifold. At the microlevel, we find that income shocks are generally negatively and significantly correlated with the incidence, intensity, and onset of conflicts within locations. However, the relationship between external shocks and conflict is significantly weaker for locations that are naturally less open because these are precisely the ones in which income is less affected by foreign demand. These results are robust to the use of various conflict data, measures of income shocks, estimation techniques, samples, or the inclusion of a number of location-specific additional controls. We argue that our findings can be interpreted as evidence in favor of the opportunity cost mechanism rather than of the state capacity. This has interesting indirect consequences: the opportunity cost argument is a purely economic one, which means that individuals engaging in rebellions because of external shocks affecting their income are probably different in that they do not enter in the conflict only because of political convictions or agenda. The specificity of this motive for rebellion might be important to understand the evolution and the outcome of conflicts.

In a nutshell, this paper suggests that external income shocks are important to understand the geography and intensity of ongoing conflicts and might affect the outbreak of new country-wide conflicts if they are large and persistent. Further research is needed on this point and more generally on the way in which income shocks may interact with other long-term issues such as inequality or ethnic problems. The boom in food prices was not the primary cause of the recent Arab spring, but many analysts emphasized its role in accelerating and magnifying the protests. Income shocks also may act as a threat multiplier and explain an important part of the timing, geography, and intensity of conflicts around the world.

[36] Indeed, to detect an effect of commodity price shocks on conflict incidence at the country level, we need commodity price shocks to affect conflict onset or ending; as with country fixed effects, the identification of an effect is possible only when the dependent variable switches from 0 to 1 or vice versa.

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Table Appendix

TABLE A.1.—AGRICULTURAL COMMODITIES SHOCKS: FURTHER ROBUSTNESS

Dependent Variable:	Conflict Incidence		Conflict Incidence		Conflict Incidence	
Dataset:	UCDP		ACLED 1		ACLED 2	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)
Panel A: Binary weights						
ln agr. shock	−8.418***	−0.305***	−9.191***	−0.128**	−11.490***	−0.549***
	(1.815)	(0.073)	(2.905)	(0.057)	(2.359)	(0.108)
ln agr. shock × Remoteness[a]	0.588***	0.032***	1.149***	0.017***	0.834***	0.058***
	(0.159)	(0.009)	(0.232)	(0.006)	(0.310)	(0.016)
Observations	26,244	132,066	6,545	41,055	13,910	73,320
Panel B: Weights before 1993						
ln agr. shock	−11.478***	−0.449***	−12.170***	−1.141***	−6.725***	−0.302***
	(1.648)	(0.116)	(2.384)	(0.243)	(1.930)	(0.104)
ln agr. shock × Remoteness[a]	1.697***	0.066***	1.899***	0.155***	1.431***	0.054***
	(0.303)	(0.018)	(0.402)	(0.034)	(0.303)	(0.016)
Observations	6,682	47,008	1,908	7,032	6,150	36,160
Panel C: Dropping large players						
ln agr. shock	−5.589***	−0.263***	−6.534***	−0.100**	−6.088***	−0.334***
	(1.170)	(0.067)	(1.875)	(0.047)	(1.948)	(0.089)
ln agr. shock × Remoteness[a]	0.606***	0.035***	0.800***	0.016**	0.859***	0.052***
	(0.168)	(0.010)	(0.259)	(0.007)	(0.291)	(0.014)
Observations	26,208	130,500	6,545	41,055	13,900	72,450
Panel D: Only exported products						
ln agr. shock	−5.560***	−0.257***	−6.924***	−0.114**	−6.472***	−0.353***
	(1.126)	(0.065)	(1.911)	(0.045)	(1.766)	(0.089)
ln agr. shock × Remoteness[a]	0.443***	0.031***	0.803***	0.019***	0.628**	0.048***
	(0.153)	(0.009)	(0.265)	(0.007)	(0.261)	(0.014)
Observations	26,100	125,172	6,545	41,055	13,870	69,490

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by administrative region in parentheses. All estimations include year dummies and cell fixed effects. Estimations cover only the post-1993 time period in panel D.
[a] ln distance to closest seaport.

TABLE A.2.—AGRICULTURAL COMMODITIES SHOCKS: M3 CROP DATA

Dependent Variable:	Conflict Incidence		Conflict Incidence		Conflict Incidence	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)
Panel A						
ln agr. shock, M3 Crop	−0.265	−0.009	−1.475	0.005	−1.274*	−0.053*
	(0.438)	(0.012)	(0.928)	(0.014)	(0.734)	(0.031)
Panel B						
ln agr. shock, M3 Crop	−3.567***	−0.199***	−6.375***	−0.110**	−4.222**	−0.306***
	(1.255)	(0.061)	(1.728)	(0.054)	(1.948)	(0.103)
ln agr. shock × Remoteness[a]	0.570***	0.032***	0.898***	0.018**	0.546*	0.044***
	(0.169)	(0.009)	(0.232)	(0.007)	(0.316)	(0.017)
Panel C						
ln agr. shock, M3 Crop	−2.109***	−0.078***	−3.536***	−0.050**	−2.113**	−0.111***
	(0.679)	(0.024)	(1.106)	(0.020)	(0.985)	(0.043)
ln agr. shock × Remoteness[b]	3.168***	0.124***	3.393***	0.080***	2.056*	0.116**
	(0.653)	(0.030)	(0.930)	(0.022)	(1.201)	(0.052)
Sample	UCDP-GED		ACLED 1		ACLED 2	
Years	1989–2006	1989–2006	1989–2005	1989–2005	1997–2006	1997–2006
Number of countries	39	43	12	12	42	43
Observations	25,452	106,992	6,222	35,241	13,540	59,440

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by administrative region in parentheses. All estimations include year dummies and cell fixed effects. Agricultural commodities shock computed M3 crop data set.
[a] ln distance to closest seaport.
[b] Distance to closest seaport relative to maximum distance, computed by country.

TABLE A.3.—AGRICULTURAL COMMODITIES SHOCKS: GAEZ SUITABILITY DATA

Dependent Variable:	Conflict Incidence		Conflict Incidence		Conflict Incidence	
Estimator:	FE Logit (1)	FE-LPM (2)	FE Logit (3)	FE-LPM (4)	FE Logit (5)	FE-LPM (6)
Panel A						
ln agr. shock, GAEZ	0.109	0.007	−0.550	0.001	−0.471	−0.003
	(0.327)	(0.010)	(0.543)	(0.009)	(0.441)	(0.018)
Panel B						
ln agr. shock, GAEZ	−3.618***	−0.209***	−4.891***	−0.084**	−3.848**	−0.251***
	(1.194)	(0.059)	(1.316)	(0.037)	(1.514)	(0.085)
ln agr. shock × Remoteness[a]	0.623***	0.034***	0.790***	0.013**	0.555**	0.039***
	(0.180)	(0.009)	(0.172)	(0.005)	(0.228)	(0.013)
Panel C						
ln agr. shock, GAEZ	−1.537***	−0.064***	−1.888**	−0.020	−1.656**	−0.069**
	(0.540)	(0.018)	(0.821)	(0.014)	(0.729)	(0.033)
ln agr. shock × Remoteness[b]	3.235***	0.124***	2.967***	0.037**	2.224**	0.117**
	(0.728)	(0.030)	(0.991)	(0.016)	(1.033)	(0.048)
Sample	UCDP-GED		ACLED 1		ACLED 2	
Years	1989–2006	1989–2006	1989–2005	1989–2005	1997–2006	1997–2006
Number of countries	36	43	12	12	38	42
Observations	17,388	77,238	4,879	29,478	9,510	42,900

Significant at *10%, **5%, ***1%. Robust standard errors, clustered by administrative region in parentheses. All estimations include year dummies and cell fixed effects. Agricultural commodities shock computed FAO-GAEZ data.

[a] ln distance to closest seaport.

[b] Distance to closest seaport relative to maximum distance, computed by country.