External shocks, internal shots: the geography of civil conflicts*

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February 6, 2013

Abstract

This paper uses detailed information on the latitude and longitude of conflict events within a set of Sub-Saharan African countries to study the impact of external income shocks on the likelihood of violence. We consider a number of external demand shocks faced by the country or the regions within countries - temporary shocks such as changes in the world demand of agricultural commodities, or longer-lasting events such as financial crises in the partner countries or permanent changes in foreign trade policy - and combine these with information reflecting the natural level of trade openness of the location. We find that (i) the incidence, intensity and onset of conflicts are generally negatively and significantly correlated with income shocks within locations; (ii) this relationship is significantly weaker for the most remote locations, i.e those located away from the main seaports, (iii) at the country-level, only large, long-lasting shocks - financial crises, trade policy - have an effect on the outbreak and location of new conflicts. All together, our results therefore suggest that external income shocks are important determinants of the intensity and geography of conflicts within countries, and provide support in favor of the opportunity cost theories of conflicts.

JEL classification: D74, F15, O13, Q17
Keywords: conflict, income shock, civil war

*We are especially grateful to Dominic Rohner for his numerous comments. We also thank Jean-Louis Arcand, Richard Baldwin, Emmanuel Milet, Frédéric Robert-Nicoud, Mathias Thoenig, Vincent Vicard, seminar participants at Paris School of Economics, University of Namur, WTO, the 8th Annual HiCN Workshop, and the CEPR Workshop on Causal Analysis in International Trade for very useful discussions. Mathieu Couttenier acknowledges financial support from the ERC Starting Grant GRIEVANCES-313327

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1 Introduction

The effect of income shocks on the probability of civil conflict has been at the core of intense debates among economists and political scientists over the last decade. A particular attention has been given to the effect of commodity price variations, taken as a proxy for exogenous external income shocks (Besley and Persson, 2008, Brückner and Ciccone, 2010, Fearon, 2005). At the country-level, the results are mixed at the very least.[1] Recently, Bazzi and Blattman (2011) have challenged most of the findings found by 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. On the other hand, the few results available at the micro-level points to a more robust causal relationship (Dube and Vargas, 2012). However, even when income shocks are found to significantly affect conflict probability, the identification of the precise transmission channel remains problematic.

This paper uses PRIO/ACLED data containing detailed information on the location of conflicts within a set of Sub-Saharan African (SSA) countries to study the effect of external income shocks on the likelihood of violence. Our data contain several countries, and the precise latitude and longitude of violent events within each of them. 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 macro-level studies. The other 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 (2012), who used geographically disaggregated data but for a single country (Colombia). We use fine-grained disaggregated data for several 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 (temporary) commodity shocks by constructing a region-specific measure 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 locations. Moreover, we go further than the existing literature by also considering a number of longer-lasting external demand shocks. We consider two additional shocks: (i) the number of banking crises

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

in the country's trading partners (weighted by the share of each partner in the country's total exports); (ii) changes in foreign trade policy through the entry into force of the African Growth Opportunity Act (AGOA), which provided free access to the US market to a number of SSA countries after 2000 for a wide range of products. Third, we combine these shocks with location-specific information reflecting their "natural" level of trade openness, proxied by the relative 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 (i.e. types of shocks). From an identification perspective, combining temporary and long-lasting external shocks with location-specific information also ensures that we are capturing different aspects of exogenous changes in income. Moreover, the various dimensions of our data allow us to study how external shocks affect the geography and intensity of conflict within countries.

At the micro-level, we find that the incidence, intensity and onset of conflicts are generally negatively and significantly correlated with income shocks within locations. More precisely, a negative external income shock increases the probability and intensity of conflict on average within locations. Second and importantly, the relationship between external income shocks and conflict is significantly weaker in naturally less open locations, i.e when one moves away from the seaports. This clearly suggests that we are identifying the effect of exogenous shocks related to international trade, which are less likely to affect the most remote regions. Importantly, these results are observed for our three external shocks, and remain remarkably robust to the use of various estimation techniques, samples, to the inclusion of additional country-specific controls or location-specific controls, among which the location's GDP, distance to the capital city, to international border or to natural resource fields.

The fact that external demand variations affect the likelihood of conflict on average within locations, especially for the most opened ones, implies that these shocks impact the intensity and geography of civil conflicts at the country-level. In that sense, income shocks act as threat multipliers, just like the boom in food prices accelerated and intensified the protests during the recent Arab Spring. However, our results on conflict outbreak at the country-level are more mixed: only large, persistent shocks - financial crises, permanent changes in trade policy - have a significant impact on conflict onset, while changes in commodities demand or prices have no effect. While more research is needed on this, our findings therefore suggest that the mixed results found by the literature using commodity prices variations (e.g. Bazzi and Blattman, 2011) might also be a direct consequence of the type of shock considered.

Our findings yield at least two important conclusions. First in terms of the predictions of the workhorse models of conflict, which are a priori ambiguous: on the one hand, a larger income might decrease the risk of conflict, either by reducing the individuals' opportunity cost of insurrection or by increasing the capacity of the state to prevent rebelion (e.g. Fearon and Laitin, 2003); on the other hand, positive income shocks might impact positively the likelihood of conflict by increasing the value of resources to fight over. Our results clearly point to the first group of predictions, and more specifically to the opportunity cost mechanism. The state capacity effect should indeed be more prevalent in the regions located close to the political center, i.e. the capital city (Buhaug, 2010). We do not find support for this hypothesis. Second, our results suggest that external income shocks are more important to understand the geography and intensity of ongoing conflicts than the outbreak of a conflict at the country-level. If the opportunity cost story is relevant, it is therefore 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 and Reuveny, 2005, Jha, 2008, Martin *et al.*, 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 micro-level. The limitations of the cross-country studies, as well as the availability of more geographically detailed data, has recently pushed the 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 locate further away from the capital in more powerful political regimes.[2] The impact of income or more generally economic shocks on the incidence of conflict or riots within states have been studied by a number of recent country-specific studies. Using data on Colombian municipalities, Dube and Vargas (2012) find evidence in favor of both the opportunity costs and state as prize theories. More precisely, they show that positive commodity price shocks decreases the likelihood of conflicts in the case of coffee (a labor-intensive commodity) but raises the probability of conflict for oil (a capital intensive commodity). Following Miguel *et al.* (2004), Hidalgo *et al.* (2010) use data on Brazilian municipalities and find that favorable economic shocks, instrumented by rainfall[3], affect negatively 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. Nillesen and Verwimp (2009) find an effect of rainfall, but no significant effect of commodity prices on rebel recruitment in Burundi using village-level data. Finally, Jia (2011) finds that droughts increased the probability of peasant 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 very precisely the effect of income shocks on conflicts through individuals' behavior. The generalization of these results is however made difficult by the potential

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

[3]A large controversy on the robustness of this instrumentation exists since the seminal paper of Miguel *et al.* (2004) - see Couttenier and Soubeyran (2010) for a literature review.

selection bias inherent to any country-case study. Our paper is a first attempt to make a link between macro, cross-country studies and micro, country-case ones, through the consideration of both within and between countries variations.

In the next section, we discuss the theoretical mechanisms that generate predictions about the impact of income on conflict. Section III describes the data and the computation of income shocks. Section IV presents the empirical methodology. Section V and VI 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 VII. The last section concludes.

2 Income shocks and conflicts

Theoretical literature on the roots of civil conflicts generally makes a distinction between "capacity" and "opportunity" related causes of conflicts. Various theoretical mechanisms predict an effect of economic shocks on conflicts. This section presents a short survey of the literature which aims at guiding our empirical strategy in the next sections.[4]

The theoretically ambiguous effect of economic shocks on conflict 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, as shown by Grossman (1991)[5], individual participation depends also on the opportunity cost of fighting, which is itself a positive function of income. The higher the income, the lower the incentives to fight. Similarly, a negative income shock therefore increase the individuals' incentive to fight by reducing this opportunity cost.

However, as in these models the winning party appropriate the resources of the opponents, the effect of an income shock is ambiguous, as shown formally by Fearon (2006)[6]: in the case of a negative shock, the probability of conflict might as well decrease, as the "prize" (the resources that can be appropriated by exerting violence) is lower.[7]

A way to distinguish between these two effects has been proposed by Dal Bó and Dal Bó (2011).

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

[5] See also Besley and Persson (2011).

[6] Fearon uses a contest model. See also Chassang and Padro-i Miquel (2009) for a similar result using a bargaining approach.

[7] For empirical evidence, see Cotet and Tsui (2013), Lei and Michaels (2011) or Ross (2006).

In their two-sector model, income shocks have an opposite effect depending on whether they affect the capital- or the labor-intensive sectors. In the first case, a larger income (due for instance to a rise in the world price of the good) increases conflict probability, as it increases the value of controlling the state but has a negligible effect on wages. The opposite is true for the labor-intensive sector: the opportunity cost effect dominates, and conflict becomes less likely. Evidence in favor of these predictions have been found by Dube and Vargas (2012) using Colombian data and world changes in the commodity price of coffee (labor intensive) versus oil (capital intensive).

Using a dynamic bargaining model, Chassang and Padro-i Miquel (2009) show that the probability of conflict is more likely to increase when the economy is hit by *transitory* negative income shocks. The intuition is that individuals do not fight for current returns, but rather for the discounted present value of victory. Transitory negative income shocks decrease the opportunity cost of fighting but leave this discounted value unchanged, therefore increasing the likelihood of conflict.

Importantly, a negative correlation between (positive) income shocks and conflict incidence does not provide a definitive test of the opportunity cost mechanism. Conflict probability might as well decrease when the country experiences "good" shocks because they provide the state with the financial means to strengthen the control of opponents or buy off opposition (see Fearon and Laitin, 2003). Intuitively, this "state capacity" effect should be more prevalent in regions located close to the political center of the country, i.e. the capital city. 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.

As emphasized by Cederman *et al.* (2011), inequality is another potential determinant of civil conflict. A positive income shock might therefore have an ambiguous effect, depending on how it affects income inequality. This argument is particularly relevant to the case of foreign shocks, which may affect heterogeneously income across regions within countries. We indeed find some evidence that large positive external shocks *increase* conflict intensity in the most remote regions.

3 Data

Our main objective is to study how income shocks affect the probability conflict both within and across countries. We therefore need data on (i) the location on conflict events within countries; (ii) external shocks potentially affecting conflict through income; (iii) location-specific characteristics influencing the way in which each location might respond to these external income shocks.

3.1 Conflict data

We use the Armed Conflict and Event dataset (ACLED) constructed by UCDP/PRIO[8], which contains detailed conflict data by location, for a subset of African countries. A "location" is defined by a given latitude and longitude. The unit of observation is the event. The data contains the date (precise day most of the time), longitude and latitude of conflicts events within each country. It also includes some information on the type of event (violent, non violent) and issue (rebel or government gains territory) of the conflict. We aggregate the data by year and location, therefore ending up with three main variables that we use as dependent variables in our empirical analysis: a dummy which equals one if at least one conflict happened in the location during the year (which we interpret as location-specific *conflict incidence*), the number of conflicts that did occur in the location during the year (*conflict intensity*) and the outbreak of civil conflict (*conflict onset*) which is coded 1 for the first year of the civil conflict in the location, 0 for each year of peace and set to missing from the second year to the last year of the civil conflict.

The raw dataset contains 13 countries and covers a long time period (1960-2005). We drop the countries with less than ten observations to end up with 8 countries: Angola, Burundi, Democratic Republic of Congo, Congo, Liberia, Sudan, Sierra Leone, and Uganda. We concentrate on the 1980-2005 period due to data availability for the computation of income shocks. All these countries have known civil war episodes over the period of study. Our final sample includes 1054 locations, and an average of 130 locations by country (and a median of 100).[9] Our sample contains only conflicts reaching at least 25 battle-related deaths per year, and is therefore comparable with the country-level data commonly used in the literature.[10] Figure 1 shows the number of locations with at least one event, by year.[11] Table 8 in the appendix contains some descriptive statistics. The sample mean for the conflict incidence variable is low at 7%. Conditioning on observing a conflict during the year, the average number of conflict events is only slightly above 1 (1.39). Note that the maximum number of conflict events observed in a given location during a given year is 16.

We use also an alternative dataset developed by ACLED[12] to check the robustness of our results. It contains most African countries, but unfortunately is limited to the 1997-2010 period. For consistency,

[8]This dataset can be found on the following website: http://www.prio.no/CSCW/Datasets/Armed-Conflict/Armed-Conflict-Location-and-Event-Data/

[9]We run estimations at the location-level, therefore including only the locations which endures a conflict event at some point during our period of study. As shown later, our results are however robust to aggregating our location-specific data by $1\times$ 1 degrees cells.

[10]UCDP/PRIO defines an armed 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]Figures A.1 and A.2 in the online appendix show the locations of conflict events, conflict outbreaks and of the major seaports for each country of our sample.

[12]http://www.acleddata.com/

we concentrate on the countries in which at least one civil conflict occurred over the period according to the UCDP/PRIO definition.[13] This dataset records all political violence including violence against civilians, rioting and protesting within and outside a civil war, without specifying a battle-related deaths threshold. The broader definition of civil conflict makes the comparison with the country-level literature more difficult. We will however show that our micro-level results are qualitatively similar using either dataset.

3.2 Income shocks

Our identification strategy rests upon the use of both country-wide income shocks and location-specific characteristics. Our first objective is to study the effects of external (i.e. foreign) shocks on the incidence, onset or intensity of conflict in a given location within a given country. All these shocks are based on variations in the foreign demand for the goods produced by the country or region to which the location belongs. We focus on three different types of foreign shocks. While they are all supposed to capture exogenous variations in foreign demand for the goods exported by the country / location, they are different in their scope and nature. In particular, while the first shock (based on the world demand for agricultural commodities) can arguably be considered as temporary and limited in scope, the two others (based on financial crises and permanent changes in trade policy) are larger and longer-lasting. Therefore, considering different shocks allows to check the robustness of the results, but also to discuss the way in which income shocks affect the incidence of conflicts. Descriptive statistics on each of the income shocks variables are provided in Table 8 in the appendix.

Temporary Shock: Agricultural commodities. As mentioned earlier, a number of papers have tried to identify the effect of commodity shocks on the likelihood of conflict across countries (e.g. Fearon, 2005, Bazzi and Blattman, 2011, Brückner and Ciccone, 2010, Besley and Persson, 2008). Little work has been done within country (with the notable exception of Dube and Vargas, 2012). We combine our conflict data with FAO Agro-maps information to obtain a region-specific measure of agricultural specialization. The FAO Agro-maps data contains information on the volume of production of different agricultural commodities at the sub-national level, for a number of years.[14] 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 70 commodities such as bananas, cocoa, coffee or tomatoes (see Table A.1 of the online appendix for an exhaustive list). Then we compute the

[13] These are the same countries as our benchmark dataset, plus Rwanda, Chad and Somalia. We drop Somalia as its recorded international trade flows are close to zero over the period.

[14] Agro-maps uses the Second Administrative Level Boundaries (SALB) defined by the UN based on national administrative units. Here we focus on the years 1989-2005 to be able to match the product classification with HS6 trade data from UN-COMTRADE.

average share of each commodity in the total agricultural production value of the region.[15] Finally, we combine this data with the world total imports of each commodity from UN-Comtrade, to end up with a time-varying, region-specific measure of external demand for the commodities produced by the region[16]:

$$\text{WD}_{rt} = \sum_c \alpha_{cr} \times M^W_{ct} \tag{1}$$

where α_{cr} is the average share of agricultural commodity c in region r, and M^W_{ct} are the world imports of commodity c in year t.

Changes in the demand for agricultural commodities are generally modest, and can be considered as temporary. Our second type of external demand shocks is based on large foreign events, such as financial crises or significant changes in trade policy, which might affect domestic income more importantly, and more durably.

Long-lasting shock # I: Banking crises. Our next measure of income shock is the exposure of the country to financial crises in the rest of the world. Financial crises destroy trade[17], 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 in general several years and have persistent effects on imports (Abiad *et al.*, 2011), especially when the origin country is in Sub-Saharan Africa (Berman and 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 1980-2009 period, and C_{jt} is a dummy which equals 1 if destination j experienced a banking crisis during year t. The trade data comes from the IMF Direction of Trade Statistics (DOTS), and the crisis data from Reinhart and Rogoff (2011)[18]. 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

[15]We also tried to replace these shares by a dummy which equals 1 if the region r has produced the commodity c at least one year over the period. The results, available upon request, remained largely unchanged.

[16]Considering world demand instead of world prices allows us to consider a wider range of commodities, including commodities which do not have a world price. We will however check that our results are robust to the use of commodity price variations using the data from Bazzi and Blattman (2011) - see Table 9.

[17]See for instance Abiad *et al.* (2011) for a long-term perspective, and the literature on the recent trade collapse summarized in Baldwin (2009).

[18]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."

shock on all the goods exported by the country.

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 indeed as unlikely the possibility that the shock affects conflict through the country's financial system: even though the geographical distribution of international financial linkages is closely related to trade in goods (see for instance Aviat and Coeurdacier, 2007), Sub-Saharan countries' financial systems are arguably too small and closed to generate such an effect.

Long-lasting shock # II: African Growth Opportunity Act. Starting in the early 2000s, the US granted free access to its market to a number of African countries, for a large range of products. The year in which these preferences were granted depends on the country.[19] As shown by Frazer and Biesebroeck (2010), the AGOA had a positive and significant effect on these countries' exports. We use a dummy (A_{it}) which equals 1 if the country belongs to the AGOA in year t. This variable is possibly less exogenous that the previous ones. A country becomes eligible to the AGOA only when it meets certain conditions, among which political stability may play a role (although it does not appear explicitly in the list of criteria defined by the agreement). To ensure that we are focusing on a shock that is exogenous, we refine the variable. First, as not all products are eligible to the AGOA, countries should be affected heterogeneously depending on their exposure to AGOA-eligible products, and depending on how much they trade with the US. We define the "exposure to AGOA" as follows:

$$\text{Exp}_{it}^{AGOA,1} = \beta_{ip}^{US} \times A_{it} \tag{3}$$

where β_{ip} is the average share of total exports of country i in AGOA-eligible products to the US before the AGOA enters into force (from 1995 to the year in which the preferences are granted to the country). This variable does not only reflect the fact that a country entered the AGOA, but also the extent to which it is likely to be affected *ex-post* due to its *ex-ante* specialization. This variable is more exogenous to political conditions. We also interact A_{it} with the distance between the country's main seaport (see below for a discussion of the seaport data) and the US (New York City):

$$\text{Exp}_{it}^{AGOA,2} = \text{distance}_i^{US} \times A_{it} \tag{4}$$

We expect a country to be less affected by the AGOA if is it located further away from the US.

[19] For the list of eligible countries, products and dates in which the preferences were granted, see: http://www.agoa.gov/AGOAEligibility/index.asp. We do not consider other unilateral liberalization initiatives such as Everything but Arms in the EU, as these are generally granting free market access for the entire range of products, which limits the scope for identification. The countries included in our estimations entered the AGOA in 2000 (Chad, Congo, Rwanda, Uganda), 2002 (Sierra Leone), 2003 (Democratic Republic of Congo), 2004 (Angola) and 2006 (Burundi, Liberia).

This ensures again that we are identifying an exogenous shock: if a country's eligibility to the AGOA can plausibly be affected by political conditions, there is a priori no reason to believe that this bias is differently distributed according to the distance of the country to the US.

3.3 Natural openness

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

We therefore construct measures of relative 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 to create heterogeneity and to study how external income shocks affect the *geography* of conflicts within each country, which to our knowledge has not been done so far. This identification strategy also help us to reconcile the divergent results found by the cross-country and within-country literatures.

For each location, we compute the distance (in kilometers) to the closest major seaport.[20] 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 locations closer to a foreign port.[21] As we are using a cross-country dataset, a potential issue with using distance in level that it will be systematically higher in larger or landlocked countries. If conflict probability is different in these countries for other (unobserved) reasons, this might bias our results. In our baseline specifications, we therefore take the ratio between this distance and the largest distance observed by country. However we show that our results are similar when using the simple distance between each location and the nearest seaport. Finally, we check our results to an alternative measure of remoteness: the average "roughness" of the terrain around the location as an alternative measure of openness. This variable, which takes values between 0 to 1, is taken from the G-Econ dataset.[22]

[20] The data on major seaports comes from http://www.e-ships.net/ports.php and Couttenier and Vicard (2012).

[21] Restricting our analysis to port located in the same country (therefore excluding landlocked countries) does not alter our results. Similarly, our findings are robust to considering seaports with a maximum draft larger or equal to 12.5 meters or 15 meters. Results are available upon request.

[22] As explained below, the G-econ data contains geographical and economic variables at a geographically disaggregated level, i.e. by 1 × 1 degree grid cells. For a precise description of the computation of the roughness measure, see http://gecon.yale.edu/sites/default/files/envdat.pdf.

Our remoteness variables might be correlated with other location-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 to the capital city, regional GDP or distance to natural resources fields. Descriptive statistics about these various measures are provided in Table 8 in the appendix.

3.4 Other data

We also control for a number of location-specific geographical or economic characteristics, such as distance to natural resource fields or GDP, which we now describe in details.

G-Econ. We add to our dataset information at a slightly more aggregated level from G-Econ. G-Econ data, developed by Nordhaus *et al.* (2006), contains geographical information (roughness, elevation, vegetation, etc.) as well as economic indicators (GDP, population - available every five years from 1990 to 2005) for most countries in the world, divided by 1×1 degree grid cells. We assign each location to the grid cell to which it belongs. Among others, this allows us to control for the GDP of the area.

Natural resources. We compute the distance from each location to the closest diamond and oil fields. The latitudes and longitudes of these fields of natural resources come from PRIO.[23]

4 Empirical methodology

4.1 Baseline specification: Micro Level

Our objective is to study the way in which foreign demand shocks affects the likelihood and intensity of conflict within countries. Let us denote by l a specific location (e.g. a city, or a village), i a country and t a year. In general, we estimate a specification of the form:

$$\text{Conflict}_{l,t} = \beta \text{shock}_{i,t} + \gamma \text{shock}_{i,t} \times \text{remoteness}_l + \eta_t + \mu_l + \varepsilon_{l,t} \quad (5)$$

where $\text{Conflict}_{l,t}$ is a variable that captures the incidence, onset or intensity of a conflict in a given location, 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 location l: alternatively (i) the world demand for agricultural commodities produced by the region (equation (1) - in this case the variable is location-specific, i.e. $\text{shock}_{l,t}$); (ii) the exposure to banking crises (equation (2)), (iii) or the exposure to the AGOA

[23]http://www.prio.no/CSCW/Datasets/Geographical-and-Resource/Diamond-Resources/

(equations (3) and (4)). Finally, remoteness_l represents our inverse measure of the "natural trade openness" of the location. In our baseline estimations, this variable is the distance of location l from the closest seaport divided by the maximum distance of a location to the closest seaport, computed by country.

Finally, in all estimations we control for time dummies η_t and location-specific characteristics μ_l. The latter capture time-invariant characteristics that may affect the average likelihood of conflict in a given location, e.g. the distance to the closest port, to the capital, to natural resources, or the region's roughness. 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 location-specific characteristics.

The sign of β is theoretically ambiguous, as mentioned in section 2. 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; 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, as underlined in section 2, 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. We provide two tests suggesting that the opportunity cost is a more relevant explanation: (i) the size of our effect does not depend on the distance to the capital city; (ii) our shock variables are indeed correlated with changes in GDP per capita at the regional level, the less so as remoteness increases.

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

Finally, while our baseline estimations are carried out at the location level, we check that our results are robust to different level of spatial aggregation. More precisely, we aggregate our data by cells of either 0.5×0.5 degrees or 0.1×0.1 degrees (i.e. respectively a five hundreds or one hundred square kilometers approximatively) and show that our results are very similar.

4.2 Econometric issues

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

$$\Pr(\text{Conflict}_{l,t} > 0) = \beta_1 \text{shock}_{i,t} + \gamma_1 \text{shock}_{i,t} \times \text{remoteness}_l + \eta_t + \mu_l + \varepsilon_{l,t} \tag{6}$$

where the dependent variable is conflict *incidence*, i.e. a dummy taking the value 1 if location l experienced a conflict during year t. We study also the *onset* of a civil conflict by estimating equation (6) conditional on $\text{Conflict}_{l,t-1} = 0$.[24]

The cleaner way to estimate this specification is through a conditional logit estimator that accounts for all location-specific time-invariant unobserved characteristics. This is our preferred estimator. However, due to the difficulty to interpret the size of the estimated coefficients in this case, we systematically show the results obtained with a linear (OLS) estimator with location fixed effects.

Conflict intensity. As a measure of conflict intensity, we use the number of conflict events[25] in location l during the calendar year t, $N^c_{l,t}$, as a dependent variable and estimate:

$$N^c_{l,t} = \beta_1 \text{shock}_{i,t} + \gamma_1 \text{shock}_{i,t} \times \text{remoteness}_l + \eta_t + \mu_l + \varepsilon_{l,t} \tag{7}$$

As $N^c_{l,t}$ is a count variable, the use of fixed-effects Pseudo-Maximum Likelihood Poisson (PPML) estimator is appropriate. Again, we replicate the results using a linear estimator.

In all estimations, we use robust standard errors, clustered at the location level. We check that our results are robust to a non-parametric estimation of the standard errors allowing for both cross-sectional spatial correlation and location-specific serial correlation (Conley, 1999; Hsiang *et al.*, 2011).[26] Finally, to control for temporal trends in the causes of conflicts (Miguel *et al.*, 2004, Bazzi and Blattman, 2011) we systematically check that the our results are robust to the inclusion of country-specific time trends (for both conflict probability and intensity).

Country-level conflict outbreak. The above specifications provide information on the effect of external income shocks on the likelihood or intensity of conflicts within a given location in general, i.e. 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 (6) where we condition on conflict onset at the country level, i.e.:

$$\Pr(\text{Conflict}_{l,t} > 0 | \text{Conflict}_{i,t-1} = 0) = \beta_1 \text{shock}_{i,t} + \gamma_1 \text{shock}_{i,t} \times \text{remoteness}_l + \eta_t + \mu_l + \varepsilon_{l,t} \tag{8}$$

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

[25] We do not have information on the number of death by event, contrary to the country-level literature.

[26] We have also tried to include spatial covariates in the estimations: the average agricultural commodity shock or the number of conflicts within a 100km radius around the location, in the spirit of Harrari and La Ferrara (2013), to control for the spatial correlation and diffusion of shocks and violence. Our results are similar.

where $\text{Conflict}_{i,t-1}$ 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*.

4.3 Relation with the cross-country literature: Macro Level

As we are using location fixed effects, our results should be interpreted as the effect of external shocks within a given location, over time. By studying how the probability of conflict varies for each location, we are implicitly studying the intensity of conflict at the country-level: an increase in the probability of conflict on average across locations implies a magnification of conflict intensity at the country-level. To ease the comparison between our results and those of the existing literature (e.g. Bazzi and Blattman, 2011), 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, i.e. estimate a specification of the form:

$$\text{Conflict}_{i,t} = \beta \text{shock}_{i,t} + \eta_t + \mu_i + \varepsilon_{i,t} \tag{9}$$

where $\text{Conflict}_{i,t}$ denotes conflict incidence (a dummy which equals 1 if at least one violent event was recorded during year t in country i), onset (a dummy which 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] or intensity (number of locations 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, country-specific time trends, and we control for country-specific unobservable characteristics through the inclusion of country fixed effects μ_i.

5 Micro-level results

5.1 Temporary shocks: demand for agricultural commodities

Baseline results. We first consider external commodity shocks. As mentioned earlier, we use an indicator of income shock based on the agricultural specialization of the region to which the location belongs, i.e. the foreign demand for the region agricultural products as defined by equation (1). We consider the impact of changes in foreign demand on the probability and intensity of conflict within a given location. We further interact this variable with the remoteness of the location (the relative

[27] As before, this variable is coded as "missing" for ongoing conflicts.

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.

Table 1: Agricultural commodities shocks and conflicts: baseline results

Dep. Var.	Conflict incidence			Conflict onset			# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE logit	FE-LPM	PPML	PPML	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Obs.	11815	11815	14025	11472	11472	13698	11815	11815	14025
PANEL A:									
ln agr. shock	-1.408^a	-1.480^a	-0.085^a	-1.368^a	-1.512^a	-0.070^a	-1.113^b	-1.149^a	-0.169^a
	(0.368)	(0.410)	(0.018)	(0.362)	(0.398)	(0.014)	(0.514)	(0.405)	(0.061)
PANEL B									
ln agr. shock	-4.290^a	-3.342^a	-0.200^a	-3.708^a	-3.006^a	-0.156^a	-4.334^a	-3.612^a	-0.476^a
	(0.551)	(0.577)	(0.032)	(0.487)	(0.535)	(0.025)	(0.841)	(0.569)	(0.102)
ln agr. shock × remoteness[1]	4.174^a	2.933^a	0.165^a	3.345^a	2.372^a	0.124^a	4.642^a	3.614^a	0.442^a
	(0.503)	(0.589)	(0.037)	(0.410)	(0.518)	(0.029)	(0.547)	(0.616)	(0.126)

Robust standard errors, clustered by location in parentheses (see Table 10 in the appendix for robustness allowing for spatial serial correlation). All estimations include year dummies and location fixed effects. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. [1] distance to closest seaport relative to maximum distance computed by country.

Our baseline results are shown in Table 1. 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 relative distance to the closest seaport. An increase in world demand of the region's agricultural commodities significantly decreases the probability of conflict incidence (columns (1) to (3)), onset (columns (4) to (6)) and intensity (columns (7) to (9)) within locations in all estimations. The effect is also quantitatively relevant: a 10% increase in foreign demand decreases the probability of conflict by almost 1 percentage point (column (3)).

The effect is however heterogeneous across locations. In Panel B, we find the coefficient on our interaction between remoteness and our shock variable is always positive and significant, i.e. the probability or intensity of conflict in the least opened locations is significantly less affected by changes in the world demand for the commodity produced by the region. The quantitative interpretation of our results is straightforward: in column (3) we see that for the least most remote locations, a 10 percent increase in foreign demand leads to a 2 percentage points decrease in the conflict probability. On the contrary, the effect is almost zero for the most remote locations.

Alternative remoteness measures and additional regressors. Table 2 contains a number of robustness checks. In Panel A and B we first replicate the results of the Table 1 using two alternative indicator of trade openness: the log of distance to the nearest seaport and the roughness of the area.

These are a more imperfect proxies: the first can capture the country-specific geographical location, while the second only captures the topology of the region, and not the overall remoteness of the location. Our results are however very similar: in the less opened regions, conflict incidence, onset and intensity are found to be less affected by external changes in demand for the commodities produced by the region.

In panel C, we include a number of additional interactions terms in our estimations. Indeed, our remoteness measure might be correlated with a number of location-specific characteristics affecting the way in which the location responds to external shocks. In particular, remoteness may be correlated with the location's economic size. In the same way, the regions located far from the seaports tend to be also located far from the country's economic and political center (capital city). The correlation between the relative distance to the main seaport and relative distance the capital city is indeed positive and statistically significant, around 0.40. It could therefore be the case that we are identifying the effect of economic activity or political influence, not specifically the effect of trade openness.

We therefore include in our estimations additional interaction terms between our shock variable and (i) distance to the capital city (relative to the maximum distance computed by country); (ii) the log of GDP of the area in 2000; (iii) a dummy representing proximity to an international border, which equals 1 if the location is within a 1× 1 degree cell belonging to two different countries; (iv) a dummy representing the proximity to natural resources (oil and diamond) fields. Natural resources variables are supposed to capture the level of political instability of the region. Two results are worth mentioning. First, the effect of our shock variable, as well as its interaction with the relative distance to seaports, is very robust to the inclusion of these variables. The interaction terms between the shock variables and the relative distance to seaports remain significant at the 1% or 5% levels in all specifications, and the estimated coefficients are quantitatively close to our baseline estimates. Second and importantly, the coefficients of the interaction terms with distance to the capital city and the GDP of the area are much less robust. 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.

Additional robustness. Our results are also robust to: (i) the use of a commodity price shock variable from Bazzi and Blattman (2011) (see Table 9 in the appendix)[28]; ; (ii) allowing for cross-sectional spatial correlation and location-specific serial correlation (columns (1) to (3) of Table 10 in the appendix) (Hsiang *et al.*, 2011); (iii) aggregating our data by 0.1×0.1 degrees (Table 11 in the appendix) or 0.5×0.5 degrees (Table A.8 in the online appendix); (iv) the use of our alternative sample

[28]We are most grateful to Samuel Bazzi and Chris Blattman for making their data available.

Table 2: Agricultural commodities shocks and conflicts: robustness

Dep. Var.	Conflict incidence			Conflict onset			# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE logit	FE-LPM	PPML	PPML	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Obs.	11815	11815	14025	11472	11472	13698	11815	11815	14025
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PANEL A									
ln agr. shock	-7.654[a]	-4.884[a]	-0.291[a]	-6.750[a]	-4.660[a]	-0.237[a]	-6.400[a]	-4.402[a]	-0.733[b]
	(1.126)	(1.006)	(0.071)	(0.917)	(0.909)	(0.055)	(0.909)	(0.791)	(0.299)
ln agr. shock × remoteness (2)	1.009[a]	0.578[a]	0.032[a]	0.864[a]	0.536[a]	0.026[a]	0.838[a]	0.525[a]	0.088[c]
	(0.161)	(0.143)	(0.010)	(0.128)	(0.129)	(0.008)	(0.094)	(0.104)	(0.045)
PANEL B									
ln agr. shock	-2.137[a]	-1.969[a]	-0.128[a]	-1.820[a]	-1.813[a]	-0.098[a]	-1.971[a]	-1.956[a]	-0.283[a]
	(0.462)	(0.424)	(0.019)	(0.442)	(0.416)	(0.015)	(0.711)	(0.383)	(0.063)
ln agr. shock × remoteness (3)	2.914[a]	2.464[b]	0.181[a]	1.714[b]	1.470	0.121[a]	3.452[a]	4.137[a]	0.485[a]
	(0.913)	(1.177)	(0.040)	(0.793)	(0.981)	(0.029)	(1.070)	(1.270)	(0.108)
PANEL C									
ln agr. shock	-3.909[a]	-3.468[a]	-0.205[a]	-3.336[a]	-2.927[a]	-0.154[a]	-3.987[a]	-3.624[a]	-0.472[a]
	(0.647)	(0.649)	(0.033)	(0.588)	(0.602)	(0.026)	(0.975)	(0.608)	(0.100)
ln agr. shock × remoteness (1)	2.434[a]	1.871[b]	0.108[a]	2.009[a]	1.511[b]	0.080[b]	2.400[a]	2.322[a]	0.320[b]
	(0.709)	(0.749)	(0.041)	(0.624)	(0.679)	(0.032)	(0.768)	(0.781)	(0.128)
ln agr. shock × ln dist. to cap.	1.906[a]	0.889	0.050[b]	1.796[a]	0.913	0.049[a]	1.886[a]	0.102	0.029
	(0.610)	(0.618)	(0.025)	(0.559)	(0.589)	(0.019)	(0.563)	(0.628)	(0.100)
ln agr. shock × ln GDP area	-0.024[b]	-0.026[b]	-0.002[c]	-0.024[b]	-0.025[b]	-0.002	-0.031[c]	-0.027[b]	-0.004[b]
	(0.012)	(0.010)	(0.001)	(0.011)	(0.010)	(0.001)	(0.016)	(0.013)	(0.002)
ln agr. shock × border	0.351	0.765[c]	0.041[a]	-0.017	0.324	0.017	0.862[c]	1.349[a]	0.132[a]
	(0.401)	(0.435)	(0.015)	(0.375)	(0.403)	(0.012)	(0.455)	(0.502)	(0.045)
Exp. to crises × nat. ress.	-1.816[a]	-0.751	-0.018	-1.674[a]	-1.023[b]	-0.038	-1.610[a]	-0.718	-0.063
	(0.460)	(0.523)	(0.033)	(0.400)	(0.480)	(0.026)	(0.480)	(0.510)	(0.072)

Robust standard errors, clustered by location in parentheses. All estimations include year dummies and location fixed effects. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. (1) distance to closest seaport relative to maximum distance computed by country. (2) ln distance to closest seaport. (3) roughness of terrain from G-econ; rel. dist. to cap.: distance to capital city relative to maximum distance computed by country; ln GDP area: GDP of the area in 2000, from G-econ; nat. ress.: dummy which equals 1 if the location is within 100km of an oil or or diamond field.

(Table A.1 in the online appendix); (v) controlling for past instability through the inclusion of the cumulated number of years in which a conflict was observed in the location before year t (panel A of Table A.6 in the online appendix); (vi) controlling for time-varying country specific controls (such as GDP or institutions) or country-year dummies (panel A of Table A.7 in the online appendix); (vii) dropping each country separately from the estimations (results available upon request).

5.2 Long-lasting shocks: financial crises and trade policy

Long-lasting Shock # I: Exposure to financial crises. The first type of long-lasting income shocks we consider is the exposure of the country to financial crises in its trading partners. This variable has a negative impact on the country's income through lower exports. On the other hand, this impact on income should be lower on regions located further away from the main sea ports. Table 3 contains the results of our estimations. Panel A contains the results of estimations where only the crisis exposure variable is included. In Panel B, we add the interaction term between exposure and the distance from the location to the closest sea port. Again, we consider both conflict probability (estimations (1) to (3) of each panel), onset (estimations (4) and (5)) or intensity (estimations (6) to (8)).

Table 3: Exposure to crises and conflicts: baseline results

Dep. Var.	Conflict incidence			Conflict onset		# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE-LPM	PPML	PPML	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	No	Yes	Yes
Observations	27378	27378	27404	26817	26869	27378	27378	27404
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A								
Exposure to crises	0.047	-0.166	-0.031	0.477	0.010	0.429	0.385	-0.037
	(0.537)	(0.544)	(0.041)	(0.578)	(0.037)	(0.635)	(0.649)	(0.104)
PANEL B								
Exposure to crises	4.040^a	4.980^a	0.312^a	4.196^a	0.257^a	3.621^a	4.249^a	0.591^a
	(0.706)	(0.715)	(0.059)	(0.762)	(0.051)	(0.979)	(0.993)	(0.190)
Exp. to crises × remoteness[1]	-7.495^a	-9.663^a	-0.626^a	-6.865^a	-0.450^a	-6.322^a	-7.740^a	-1.143^a
	(1.043)	(1.092)	(0.082)	(1.098)	(0.068)	(1.395)	(1.484)	(0.232)

Robust standard errors, clustered by location in parentheses (see Table 10 in the appendix for robustness allowing for spatial serial correlation). All estimations include year dummies and location fixed effects. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. (1) distance to closest seaport relative to maximum distance computed by country.

On average across locations, the effect of exposure to financial crises in partner countries is statistically insignificant (Table 3, Panel A). This is however due to the fact that the impact is heterogeneous across regions. Introducing the interaction term between exposure to crises and remoteness confirms this heterogeneity (Panel B). For the least remote locations, the effect of financial crises in partner countries is positive and significant. The interaction term is negative and significant, i.e. distance to seaports dampens the effect of negative income shocks on conflict probability, onset and intensity. This is the case both when using non linear (FE Logit and Poisson) or linear (OLS) estimators, as well as when including country-specific time trends. The effect is also quantitatively relevant: for the seaport itself (relative distance equal to 0), a standard deviation increase in the exposure to crises variable raises conflict probability by almost 4 percentage points (based on column (3)). Note that we find in Table 3 that for the most remote locations, the effect of exposure of foreign financial crises on conflict probability is actually *negative* and significant (summing the coefficients of Panel B, columns (3), (5) or (8)). This result is however not robust to the inclusion of additional interaction terms between the shocks and location-specific characteristics , or to the use of our alternative sample (see subsection "robustness" below).

Long-lasting Shock # II: African Growth Opportunity Act. The last shock we consider is the entry of the country into the African Growth Act Opportunity. Starting in 2000, the US granted free access to a number of products to most Sub-Saharan African countries. As explained before, the scope and timing of this gain in market access differed according to the country. As shown by Frazer and Biesebroeck (2010), the AGOA had a positive and significant effect on these countries' total exports. We study the impact of this positive income shock on conflict probability, onset and intensity within African countries.

The results are provided in Table 4. Beyond the AGOA dummy, we include a number of interaction terms to check that we are indeed identifying the effect of this trade policy changes. In estimations (1) to (8) of panel A, we interact the AGOA dummy with the country's pre-AGOA share of total exports in the product eligible to the AGOA. Entering the AGOA significantly decreases both the probability of conflict and the intensity. As expected, the effect is more pronounced for the countries specialized in the products eligible to the AGOA. Second, we interact the AGOA dummy with the distance between the country's main seaport and the US (New York City) in estimations (1) to (8) of panel B of Table 4. Distance to the US limits the effect of the AGOA on exports, and in turn income and conflict likelihood: the interaction term is positive and significant in all estimations.

Finally, consistent with our previous estimates, the effect is heterogeneous within country. In panel C, we find that the most remote regions relative to the main seaports are roughly unaffected by the AGOA, while conflict probability decreases by 18 percentage points for the seaport itself. This is consistent with our previous results on financial crises and agricultural commodities.

Robustness. As for our agricultural commodity shock, the results presented in Tables 4 and 3 are remarkably robust to a number of sensitivity tests, including: (i) the use of alternative remoteness indicators and the inclusion of additional location-specific controls (Tables A.4 and A.5 in the online appendix); (ii) the use of our alternative sample (Tables A.2 and A.3 in the online appendix); (iii) aggregating the data into 0.1×0.1 or 0.5×0.5 degree cells (see Tables 12 and 13 in the appendix

Table 4: AGOA and conflicts: baseline results

Dep. Var.	Conflict incidence			Conflict onset		# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE-LPM	FE Poisson	FE Poisson	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	No	Yes	Yes
Obs.	27378	27378	27404	26817	26869	27378	27378	27404
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A								
AGOA dummy	-0.645^a	-1.020^a	-0.033^a	-0.569^a	-0.025^a	-0.948^a	-1.582^a	-0.160^a
	(0.127)	(0.250)	(0.011)	(0.119)	(0.009)	(0.204)	(0.584)	(0.056)
AGOA × Exposure	-2.615^a	-10.728^a	-0.109^a	-2.370^a	-0.092^a	-4.306^a	-13.351^b	-0.273^b
	(0.745)	(2.469)	(0.031)	(0.645)	(0.024)	(1.383)	(5.935)	(0.127)
PANEL B								
AGOA dummy	-1.235^a	-1.660^a	-0.036^a	-1.144^a	-0.028^a	-1.584^a	-1.995^a	-0.167^a
	(0.166)	(0.239)	(0.011)	(0.158)	(0.009)	(0.228)	(0.234)	(0.056)
AGOA × ln dist. to US	7.495^a	13.484^a	0.402^a	7.264^a	0.311^a	8.671^a	12.747^a	0.872^a
	(0.930)	(1.431)	(0.033)	(0.851)	(0.021)	(1.347)	(1.658)	(0.181)
PANEL C								
AGOA dummy	-3.290^a	-5.206^a	-0.180^a	-2.848^a	-0.131^a	-4.419^a	-6.552^a	-0.504^a
	(0.504)	(0.673)	(0.018)	(0.438)	(0.013)	(0.675)	(0.922)	(0.079)
AGOA × remoteness[1]	3.814^a	6.686^a	0.227^a	3.302^a	0.163^a	5.027^a	7.947^a	0.533^a
	(0.638)	(0.859)	(0.024)	(0.549)	(0.017)	(0.833)	(1.114)	(0.095)

Robust standard errors, clustered by location in parentheses (see Table 10 in the appendix for robustness allowing for spatial serial correlation). All estimations include year dummies and location fixed effects. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. (1) distance to closest seaport relative to maximum distance computed by country.

and A.9 and A.10 in the online appendix); (iv) allowing for cross-sectional spatial correlation and location-specific serial correlation (columns (4) to (9) of Table 10 in the appendix) (Hsiang *et al.*, 2011); (v) controlling for past instability through the inclusion of the cumulated number of years in which a conflict was observed in the location before year t (panels B and C of Table A.6 in the online appendix); (vi) controlling for time-varying country specific controls (such as GDP or institutions) or country-year dummies (panels B and C of Table A.7 in the online appendix)[29]; (vii) dropping each country separately from the estimations (results available upon request).

5.3 Discussion

Our main finding so far is that external income shocks affect significantly the incidence, onset, and intensity of conflict within locations in the considered set of Sub-Saharan African countries. Impor-

[29]In some cases, 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. These specifications are however very demanding. Given that we focus on relatively rare events (crisis, entry into the AGOA) in these estimations, these results should probably be interpreted with caution.

tantly, this result survives to the inclusion of various location-specific characteristics, such as distance to the country's political center (its capital city). This is particularly important as our effect of income shocks on conflict probability could as well be interpreted as evidence of the state capacity hypothesis, according to which positive income shock provide the state with the (financial) means to prevent conflicts. Indeed, controlling for distance to the major seaport, this state capacity effect should be more prevalent in regions located close to the political center, where the state influence is stronger (Buhaug, 2010). We do not find support for this assumption. This allows us to interpret our results as evidence in favor of the opportunity cost effect of economic shocks.

Table 5: External shocks and regional GDP per capita

Dep. Var.	ln GDP per cap.		ln GDP per cap.		ln GDP per cap.	
Shock	Commodities		Crises		AGOA	
Spec.	(1)	(2)	(3)	(4)	(5)	(6)
[1em] Shock	0.327[a]	0.295[a]	-0.235[a]	-0.152[a]	0.269[a]	0.218[a]
	(0.052)	(0.063)	(0.041)	(0.049)	(0.035)	(0.039)
Shock × remoteness (1)	-0.467[a]	-0.506[a]	0.361[a]	0.466[a]	-0.418[a]	-0.558[a]
	(0.060)	(0.064)	(0.049)	(0.064)	(0.050)	(0.063)
Shock × ln dist. to capital (2)		0.098		-0.200[a]		0.266[a]
		(0.096)		(0.068)		(0.076)
Observations	796	796	896	896	896	896

Estimations run at the regional level, where a region is defined by the grid square computed in G-econ data. Standardized coefficients. Robust standard errors, clustered by region in parentheses. All estimations include year dummies and region fixed effects. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. (1) distance to closest seaport relative to maximum distance computed by country. (2) distance to capital city relative to maximum distance computed by country.

Further evidence in favor of the opportunity cost interpretation can be found in Table 5, in which we show the correlation between the GDP per capita of the region and our different income shock variables. These are based on G-econ data (see section 3.4) which contains for each country the GDP and population of different geographical regions defined by square grids of 100 kilometers. The data is available only for four years (1990, 1995, 2000 and 2005). We regress the log of GDP per capita of the G-econ area on our three shock variables and their interaction with remoteness (columns (1), (3), (5)). The coefficients have the expected sign: a positive external income shock raises GDP per capita, the less so the region is located far away from the seaports. In columns (2), (4) and (6) we further include interaction terms between the shock variables and distance to the capital city in the estimations. The effect of remoteness remains significant at the 1% level.

6 Country-level results

The results presented in the previous section suggest that external income shocks affect the probability of conflict within locations, and therefore 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. In this subsection, we consider the effect of external demand shocks on conflict at country-level (equation (9)). The dependent variable is a dummy which equals 1 if at least one location in the country experienced a violent event during the year. Note that the results are very similar when we use country-specific UCDP/PRIO data on conflict incidence or onset. Our estimations include year dummies and country-specific time trends, and country fixed effects.

Table 6: Agricultural commodities shocks and conflicts: country-level results

Aggregation	Country level				Location level		
Dep. var.	Incidence	Onset	Intensity*	Intensity**	Onset (if $\text{Conflict}_{i,t-1} = 0$)		
Estimator	FE-LPM		PPML		FE-LPM		
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ln agr. com. shock	-0.557	8.269	-5.754^a	-5.405^b	-0.015	-0.006	-0.008
	(0.537)	(5.910)	(2.075)	(2.660)	(0.012)	(0.028)	(0.018)
ln agr. com. shock × remoteness (1)						-0.002	
						(0.004)	
ln agr. com. shock × remoteness (2)							-0.015
							(0.031)
Observations	119	37	119	119	3234	3234	3234

Robust standard errors, clustered by country in columns (1) to (4) and by location in column (5) to (7) in parentheses. [1] ln distance to closest seaport; * number of locations with violent events during year t; ** total number of violent events during year t. Estimations (1) to (4) include country fixed effects, estimations (5) and (6) include location fixed effects. All estimations include year dummies and country-specific time trends. [c] significant at 10%; [b] significant at 5%; [a] significant at 1%. (1) ln distance to closest seaport. (2) distance to closest seaport relative to maximum distance computed by country.

We start by considering agricultural commodities demand in Table 6. Consistent with our micro-level results, our commodity demand shock has a significant impact on conflict intensity (columns 3-4). However, we cannot detect any significant effect on conflict incidence or onset, a result consistent with Bazzi and Blattman (2011) (columns 1-2). Note that as the number of observations is logically much smaller than in our previous estimations, this lack of significance could be the result of a much less efficient estimation. In columns (5) to (7) of Table 6, we run our estimations at the location level, but under the condition that no other location experiences a civil conflict in the same country the year before (as in equation (8)). We are therefore considering the outbreak of new conflicts at the country level, but at a geographically disaggregated level, which improves the efficiency of the estimations. Despite the fact that the number of observations increases significantly, we still cannot detect any effect of our agricultural commodities shock on the probability conflict onset (column 5), even in the

Table 7: Long-lasting shocks and and conflicts: country-level results

Aggregation	Country level				Location level		
Dep. var.	Incidence	Onset	Intensity*	Intensity**	Onset (if $\text{Conflict}_{i,t-1} = 0$)		
Estimator	FE-LPM		PPML		FE-LPM		
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PANEL A							
Exp. to crises	-0.320	0.882	-0.112	0.385	0.286^a	0.553^a	0.330^a
	(0.349)	(0.929)	(1.067)	(1.428)	(0.039)	(0.133)	(0.067)
Exp. to crises × remoteness (1)						-0.065^b	
						(0.028)	
Exp. to crises × remoteness (2)							-0.111
							(0.127)
Observations	208	100	208	208	10390	10390	10390
PANEL B							
AGOA dummy	-0.197	0.026	-0.300	-0.678	-0.025^a	-0.097^a	-0.026^a
	(0.263)	(0.267)	(0.497)	(0.550)	(0.006)	(0.019)	(0.007)
AGOA × remoteness (1)						0.014^a	
						(0.003)	
AGOA × remoteness (2)							0.001
							(0.011)
Observations	208	100	208	208	10390	10390	10390

Robust standard errors, clustered by country in columns (1) to (4) and by location in column (5) to (7) in parentheses. [1] ln distance to closest seaport; * number of locations with violent events during year t; ** total number of violent events during year t. Estimations (1) to (4) include country fixed effects, estimations (5) and (6) include location fixed effects. All estimations include year dummies and country-specific time trends. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. (1) ln distance to closest seaport. (2) distance to closest seaport relative to maximum distance computed by country.

most opened destinations (columns 6-7).

Are these results relevant for any type of (foreign) income shocks? We now consider both the effect of exposure to crisis and AGOA on conflict incidence, onset and intensity at country-level (equation (9)). The results are presented in Table 7. Panel A considers exposure to crises, while Panel B studies the effect of the AGOA. Again, whatever the considered shock, the effect is not statistically significant on conflict incidence or onset (columns 1-2). However, running the estimations at the location level conditional on no conflict being observed in the same country the year before shows a contrasting picture: as shown in columns (5) to (7) both the exposure to financial crises and the entry into the AGOA are found to have a significant effect on the probability of conflict outbreak (columns (5)), especially so for the most opened locations (column (5)).

How can we interpret these findings? First, the type of income shock matters. While variations in commodity demand or prices are not sufficient to lead to the outbreak of new conflicts at the country-level, larger, longer-lasting shocks such are financial crises and permanent changes in trade policy *do* have a significant impact on conflict onset, as well as on its location within countries. The mixed results found by the literature so far might therefore be a direct consequence of the type of shock considered, rather than a lack of relevance of the opportunity cost mechanism.

Second, as civil conflicts are rare events, running estimations at the country-level might be misleading, as the identification is made on a small number of switches of the dependent variable, which leads to an important loss of efficiency.[30] The use of geographically disaggregated data allows to circumvent this issue.

Last but not least, even shocks that are limited in scope, such as foreign commodity demand, do have a significant effect on conflict intensity geography, i.e. on the number and on the location of violent events after the start of the conflict. Therefore, while 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 like the boom in food prices accelerated and intensified the protests during the recent Arab Spring. 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.

7 Conclusion

In this paper, we have used detailed information on the location of conflicts within Sub-Saharan African countries to study the effect of external shocks both within and across countries. In order to reconcile the seemingly contradictory results found by micro- and macro-level studies, we propose a number of alternative ways to identify exogenous income shocks through international trade patterns. First, we improve the usual measure of temporary commodity shock using a region-specific measure of agricultural specialization. We go also further by considering two long-lasting shocks with the number of banking crises in the country's partners and changes in foreign trade policy through the entry into force of the African Growth Opportunity Act. Second, we combine these shocks with location-specific information reflecting their "natural" level of trade openness.

[30] Indeed, to detect an effect of commodity price shocks on conflict incidence at the country level, we need commodity prices shocks to affect conflict onset or ending, as with country fixed effects, the identification of an effect is only possible when the dependent variable switches from zero to one or inversely.

Our results are manifold. First, at the micro-level, we show 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, as these are precisely the location in which income is less affected by foreign demand. These results are robust to the use of various estimation techniques, measures of income shocks, samples or to the inclusion of a number of location-specific additional controls. Second, we argue that our results can be interpreted as evidence in favor of the opportunity cost mechanism, rather than of the state capacity, as the distance to the country's political center does not influence the effect of income shocks. Finally, at the country-level, we find that only large, long-lasting shocks do have an impact on conflict onset, while temporary changes in commodities demand do not, which is consistent with Bazzi and Blattman (2011).

In nutshell, our results suggest 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 however 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 primarily cause of the recent Arab spring, but many analysts emphasized its role in accelerating and magnifying the protests. Likewise, income shocks may act as a "threat multiplier", and certainly explain an important part of the timing, geography and intensity of conflicts around the world.

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8 Appendix

8.1 Additional Tables

Table 8: Descriptive statistics

	Mean	S.D.	1st Quartile	Median	3rd Quartile
Pr(conflict)	0.07	0.26	0.00	0.00	0.00
# conflicts	0.10	0.67	0.00	0.00	0.00
# conflicts (if > 0)	1.39	2.06	1.00	1.00	1.00
Distance to capital (km)	445.95	486.79	113.02	251.17	563.50
Distance to closest port (km)	788.85	472.03	226.02	1016.57	1138.33
Rel. distance to closest port[1]	0.66	0.27	0.50	0.74	0.87
Rel. distance to capital city[1]	0.53	0.27	0.32	0.57	0.76
Roughness	0.15	0.14	0.05	0.09	0.19
GDP (1990, PPP adj.)	0.30	0.41	0.06	0.18	0.42
Elevation	893.86	479.69	457.46	953.87	1238.04
ln agr. com. shock	14.55	0.56	14.1	14.54	14.94
Exp. to crises	0.08	0.10	0.02	0.05	0.11
AGOA	0.14	0.34	0.00	0.00	0.00

Note: Source: ACLED, G-econ and authors' computations. The number of observations is 27,404 except for ln agr. com. shock (14,042) . [1] relative to maximum distance, computed by country.

Table 9: Agricultural commodities shocks and conflicts: Bazzi and Blattman (2011) data

Dep. Var.	Conflict incidence			Conflict onset		# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE-LPM	PPML	PPML	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	No	Yes	Yes
Obs.	27378	27378	27378	26817	26843	27378	27378	27378
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A:								
ln price shock	-0.514^a	-0.462^a	-0.022^a	-0.473^a	-0.017^a	-0.398^a	-0.331^a	-0.038^b
	(0.101)	(0.098)	(0.006)	(0.098)	(0.005)	(0.114)	(0.103)	(0.017)
PANEL B								
ln price shock	-0.901^a	-0.640^a	-0.039^a	-0.892^a	-0.032^a	-1.160^a	-0.881^a	-0.126^a
	(0.175)	(0.165)	(0.010)	(0.174)	(0.009)	(0.189)	(0.164)	(0.026)
ln price shock × remoteness[1]	0.911^a	0.407	0.039^b	0.972^a	0.033^b	1.766^a	1.245^a	0.195^a
	(0.324)	(0.282)	(0.018)	(0.317)	(0.014)	(0.456)	(0.368)	(0.048)

Robust standard errors, clustered by location in parentheses. All estimations include year dummies and location fixed effects. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. Price shock is the change, averaged over the current and two previous years, in the commodity price index as computed by Bazzi and Blattman (2011). [1] distance to closest seaport relative to maximum distance computed by country. The number of observations is 27,378 in conflict incidence and intensity estimations, 26,817 in conflict onset estimations.

Table 10: Shocks and conflicts: spatial serial correlation

Dep. Var.	Incidence	Onset	Intens.	Incidence	Onset	Intens.	Incidence	Onset	Intens.
Shock		Agr. com.			Crises			AGOA	
Obs.	14025	13698	14025	27378	26843	27378	27378	26843	27378
Estimator	LPM	LPM	LPM	LPM	LPM	LPM	LPM	LPM	LPM
Observations	11815	11472	11815	27378	26817	27378	27378	26817	27378
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Shock	-0.200	-0.156	-0.476	0.312	0.257	0.591	-0.180	-0.131	-0.504
Spatial: 100km; Time: 2 years	(0.046)	(0.040)	(0.127)	(0.132)	(0.125)	(0.290)	(0.039)	(0.032)	(0.099)
Spatial: 100km; Time: 10 years	(0.045)	(0.040)	(0.124)	(0.132)	(0.125)	(0.288)	(0.039)	(0.032)	(0.097)
Spatial: 1000km; Time: 2 years	(0.075)	(0.062)	(0.186)	(0.151)	(0.141)	(0.316)	(0.048)	(0.041)	(0.117)
Spatial: 1000km; Time: 10 years	(0.075)	(0.062)	(0.184)	(0.151)	(0.141)	(0.313)	(0.048)	(0.041)	(0.116)
Shock × remoteness[1]	0.167	0.123	0.475	-0.626	-0.451	-1.143	0.227	0.164	0.533
Spatial: 100km; Time: 2 years	(0.056)	(0.049)	(0.163)	(0.157)	(0.150)	(0.340)	(0.048)	(0.040)	(0.115)
Spatial: 100km; Time: 10 years	(0.055)	(0.049)	(0.161)	(0.156)	(0.149)	(0.335)	(0.048)	(0.040)	(0.112)
Spatial: 1000km; Time: 2 years	(0.082)	(0.068)	(0.220)	(0.172)	(0.151)	(0.333)	(0.058)	(0.049)	(0.131)
Spatial: 1000km; Time: 10 years	(0.081)	(0.067)	(0.218)	(0.171)	(0.150)	(0.328)	(0.057)	(0.048)	(0.128)

Standard errors, corrected for spatial and serial correlation in parentheses. All estimations include year dummies, location fixed-effects and country-specific time trends. [1] distance to closest seaport relative to maximum distance computed by country.

Table 11: Agricultural commodities shocks and conflicts: 10km × 10km grid cells

Dep. Var.	Conflict incidence			Conflict onset			# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE logit	FE-LPM	PPML	PPML	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Obs.	10608	10608	12512	10247	10247	12167	10608	10608	12512
PANEL A:									
ln agr. shock	-1.559^{a}	-1.592^{a}	-0.088^{a}	-1.447^{a}	-1.593^{a}	-0.073^{a}	-1.250^{a}	-1.000^{b}	-0.158^{a}
	(0.372)	(0.424)	(0.019)	(0.371)	(0.403)	(0.015)	(0.475)	(0.430)	(0.059)
PANEL B									
ln agr. shock	-4.197^{a}	-3.174^{a}	-0.194^{a}	-3.500^{a}	-2.742^{a}	-0.144^{a}	-4.249^{a}	-3.179^{a}	-0.471^{a}
	(0.520)	(0.569)	(0.033)	(0.469)	(0.516)	(0.025)	(0.716)	(0.584)	(0.101)
ln agr. shock × remoteness[1]	3.955^{a}	2.642^{a}	0.156^{a}	3.063^{a}	1.947^{a}	0.105^{a}	4.458^{a}	3.365^{a}	0.462^{a}
	(0.495)	(0.588)	(0.038)	(0.403)	(0.514)	(0.029)	(0.561)	(0.627)	(0.134)

Robust standard errors, clustered by location in parentheses. All estimations include year dummies and location fixed effects. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. [1] distance to closest seaport relative to maximum distance computed by country.

Table 12: Exposure to crises and conflicts: 10km × 10km grid cells

Dep. Var.	Conflict incidence			Conflict onset		# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE-LPM	PPML	PPML	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	No	Yes	Yes
Obs.	24466	24466	24492	23882	23934	24466	24466	24492
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A								
Exposure to crises	-0.181	-0.391	-0.059	0.223	-0.009	0.322	0.267	-0.092
	(0.536)	(0.550)	(0.043)	(0.577)	(0.039)	(0.607)	(0.624)	(0.104)
PANEL B								
Exposure to crises	3.701^{a}	4.801^{a}	0.294^{a}	3.818^{a}	0.243^{a}	3.531^{a}	4.210^{a}	0.601^{a}
	(0.741)	(0.755)	(0.064)	(0.798)	(0.054)	(0.941)	(0.969)	(0.196)
Exp. to crises × remoteness[1]	-7.171^{a}	-9.547^{a}	-0.634^{a}	-6.522^{a}	-0.452^{a}	-6.321^{a}	-7.806^{a}	-1.245^{a}
	(1.100)	(1.157)	(0.089)	(1.160)	(0.072)	(1.386)	(1.480)	(0.255)

Robust standard errors, clustered by location in parentheses. All estimations include year dummies and location fixed effects. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. (1) distance to closest seaport relative to maximum distance computed by country.

Table 13: AGOA and conflicts: 10km × 10km grid cells

Dep. Var.	Conflict incidence			Conflict onset		# Conflicts		
Estimator	FE logit	FE logit	FE-LPM	FE logit	FE-LPM	FE Poisson	FE Poisson	FE-LPM
Country spec. time trend	No	Yes	Yes	No	Yes	No	Yes	Yes
Obs.	24466	24466	24492	23882	23934	24466	24466	24492
Spec.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A								
AGOA dummy	-1.383^a	-4.105^a	-0.046^a	-1.337^a	-0.038^a	-1.973^a	-4.473^a	-0.195^a
	(0.245)	(0.532)	(0.013)	(0.246)	(0.010)	(0.363)	(0.767)	(0.057)
AGOA × Exposure	-5.435^a	-22.519^a	-0.130^a	-5.350^a	-0.110^a	-7.602^a	-22.434^a	-0.264^c
	(1.240)	(2.841)	(0.049)	(1.211)	(0.038)	(1.714)	(3.906)	(0.143)
PANEL B								
AGOA dummy	-1.152^a	-1.559^a	-0.034^a	-1.137^a	-0.028^a	-1.457^a	-1.861^a	-0.173^a
	(0.164)	(0.228)	(0.013)	(0.169)	(0.010)	(0.187)	(0.223)	(0.058)
AGOA × ln dist. to US	7.247^a	12.990^a	0.444^a	7.388^a	0.332^a	7.977^a	11.953^a	1.006^a
	(0.894)	(1.350)	(0.035)	(0.912)	(0.023)	(1.059)	(1.512)	(0.193)
PANEL C								
AGOA dummy	-3.150^a	-5.163^a	-0.183^a	-2.821^a	-0.131^a	-4.187^a	-6.382^a	-0.536^a
	(0.506)	(0.696)	(0.019)	(0.463)	(0.014)	(0.653)	(0.939)	(0.080)
AGOA × remoteness[1]	3.690^a	6.669^a	0.239^a	3.288^a	0.167^a	4.763^a	7.756^a	0.582^a
	(0.642)	(0.889)	(0.027)	(0.582)	(0.019)	(0.807)	(1.132)	(0.098)

Robust standard errors, clustered by location in parentheses. All estimations include year dummies and location fixed effects. ^{c} significant at 10%; ^{b} significant at 5%; ^{a} significant at 1%. (1) distance to closest seaport relative to maximum distance computed by country.

8.2 Additional figures

Figure 1: Number of locations with at least one conflict, by year