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Agricultural shocks and long-term conflict risk: Evidence from desert locust swarms

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Abstract
Can transitory economic shocks affect long-term violent conflict risk? This paper studies this question using data on con ict events and desert locust swarm exposure across 0.25◦ grid cells in Africa and the Arabian peninsula from 1997-2018. A staggered event study approach shows that swarm exposure increases the average annual probability of any violent conflict in a cell by 1.8 percentage points (64%) in subsequent years. Effects are driven by initial agricultural destruction: exposure to swarms in nonagricultural areas or outside the main crop growing season has no impact. Agricultural activity (but not average productivity) falls following swarm exposure, indicating persistent indirect economic effects which may reduce opportunity costs of fighting. The largest effects of swarms on con ict occur with a 7 year lag and there are no effects in the year of exposure, inconsistent with predictions based on changes in opportunity costs of fighting alone. Impacts of past exposure are concentrated in periods of social/political disruptions driven by other proximate causes (e.g., the Arab Spring, civil war). This creates the delay in the largest impacts of swarm exposure, and aligns with models of civil conflict emphasizing the role of grievances in conflict onset. Patterns of long-term impacts on conflict and heterogeneity by local unrest are similar for exposure to droughts, indicating the mechanisms are not specific to locust swarms. These results add motivation for policies mitigating the risk of agricultural shocks and promoting household resilience and recovery.

Keywords
conflict, agriculture, desert locusts, natural disasters, Africa

JEL Classifications
D74, J16, Z12

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1 Introduction

A large economic literature explores the impacts on conflict risk of transitory agricultural shocks which do not permanently affect potential land productivity, such as weather and commodity price fluctuations. This is an important policy concern given the prominence of agricultural livelihoods in many of the areas most affected by civil conflict, the threat to agriculture posed by climate change, and the severe economic and human harms of civil conflict (see e.g., Blattman and Miguel, 2010; Fang et al., 2020). Studies of this relationship focus on short-term impacts, and those that analyze shocks to agricultural production are limited in their ability to identify causal mechanisms.[1] This paper analyzes the dynamic long-term impacts of a severe transitory shock to agricultural production—exposure to a desert locust swarm—on violent conflict, and tests for evidence of income-related mechanisms.

Desert locusts are the world's most dangerous and destructive migratory pest due to their potential to form massive cohesive swarms (Cressman et al., 2016; Lazar et al., 2016), which effectively constitute an agriculture-specific natural disaster. Climate change is creating conditions more conducive to swarm formation (Qiu, 2009), potentially undoing progress from increased international monitoring and control efforts in recent decades. The arrival of a locust swarm often leads to complete destruction of agricultural production and other vegetation (Symmons and Cressman, 2001; Thomson and Miers, 2002). Swarm flight patterns create quasi-random variation in the areas exposed to agricultural destruction in a swarm's migratory path, and their migratory nature means that exposure to a swarm does not increase future risk from locusts. Outbreaks of desert locust swarms in a given region are also relatively infrequent over the last several decades, permitting analysis of long-term effects. These characteristics make locust swarms a useful natural experiment for analyzing how transitory agricultural production shocks affect the long-term risk of conflict.

Using data on the location and timing of desert locust swarm observations from the Food and Agricultural Organization of the United Nations (FAO) and of conflict events from the Armed Conflict Location & Event Data Project (ACLED) and Uppsala Conflict Data Program (UCDP), I estimate a model of conflict at the annual level for 0.25° (around 28×28km) grid cells between 1997-2018 across Africa and the Arabian peninsula.[2] As severe transitory economic shocks may have persistent indirect effects which could affect conflict risk, beyond the initial direct impacts, I define exposure to a locust swarm as an absorbing treatment: cells are considered treated in all periods following exposure. I estimate average impacts of swarm exposure as well as dynamic

[1]Several studies find that shocks to agricultural prices increase conflict incidence (e.g., Dube and Vargas, 2013; Fjelde, 2015; McGuirk and Burke, 2020; Ubilava et al., 2022). Impacts on agricultural productivity are speculated to explain the widely-studied relationship between climate or weather shocks and conflict risk (see Burke et al. (2015), Carleton et al. (2016), Dell et al. (2014), Hsiang and Burke (2013), Koubi (2019), and Mach et al. (2019) for reviews), though weather may affect conflict through mechanisms other than agriculture and some studies find results that are not consistent with effects through agricultural productivity (e.g., Bollfrass and Shaver, 2015; Sarsons, 2015) and many reviews point to a need for more evidence on mechanisms.

[2]I include all countries where at least 10 locust swarms are reported during the sample period. Torngren Wartin (2018) estimates short-term impacts of desert locusts on conflict in Africa using similar data. That paper focuses on potential measurement issues which I discuss in Sections 4.2 and 6.2, and does not consider long-term impacts of locust swarms or mechanisms that are the main contributions of this paper.

event study impacts using the Borusyak et al. (2024) estimator to account for staggered treatment timing and potential heterogeneous effects, and test the sensitivity of the results to using other difference-in-differences estimators.

Locust swarms increase the annual probability of any violent conflict event occurring in a 0.25° grid cell by 1.8 percentage points (64%) on average in years after exposure to the swarm, compared to unaffected areas in locust migration paths in the same country. I find no significant impacts of locust swarms on violent conflict in the year of exposure or the following year but significant and large increases in subsequent years up to 14 years after exposure. Impacts are entirely driven by cells with crop or pasture land and by swarms arriving in crop cells during the main growing or harvest season in particular, indicating that effects are driven by the initial agricultural destruction. I find limited evidence of conflict spillovers outside of exposed cells, and the results are robust to a variety of alternative specifications.

I interpret the results and evaluate income-related mechanisms through the lens of a commonly-used model of individual occupation choice between different livelihood activities and engaging in armed conflict (Chassang and Padró i Miquel, 2009; Dal Bó and Dal Bó, 2011; McGuirk and Burke, 2020). In the model, transitory agricultural shocks affect the short-term risk of conflict by changing both the returns to engaging in agricultural production—the *opportunity cost* mechanism—and the returns to fighting over agricultural output—the *rapacity* mechanism. I extend the model to allow past agricultural production shocks to indirectly affect conflict risk through a wealth or *permanent income* mechanism triggered by costly consumption smoothing strategies. This wealth effect can decrease long-run productivity, leading to persistent reductions in the opportunity cost of fighting. Drawing on models of grievance and conflict (Collier and Hoeffler, 2004) and building on selected studies of short-term impacts of agricultural shocks in African countries (Berman and Couttenier, 2015; Buhaug et al., 2021), I allow for time-varying social or political tensions to affect the cost of and the returns to fighting and propose a *grievance* mechanism affecting when a prior shock is more likely to increase conflict incidence.

The results do not align with predictions under an opportunity cost mechanism alone. Locust swarms have no significant effect on violent conflict in the year of exposure or the following year despite this being the period when the opportunity cost effect from direct reductions in agricultural productivity should be strongest. This is true for measures of both output conflict and conflict over territory or factors of production, indicating that the null immediate impact is not due to offsetting effects of the opportunity cost and rapacity mechanisms.

Long-term conflict risk also does not increase uniformly, with the largest effects on violent conflict risk coming 7-10 years after swarm exposure. This lag aligns with the gap between the main locust exposure event in 2003-2005 and the onset of various conflicts in the sample countries caused by the Arab Spring, multiple civil wars, and the spread of terrorist organizations. In line with this and models of grievance and conflict, I find that long-term impacts of past swarm exposure on violent conflict are concentrated in periods of greater civil conflict and insecurity. This heterogeneity can rationalize the null short-term impact on conflict, which shows that severe agricultural shocks need not cause the onset of new conflict. Fighting is inherently a group activity motivated by some particular goal or grievance, and individuals in areas exposed to locust swarms may have lower opportunity costs and therefore be more likely to mobilize around proximate drivers of violent conflict.

I directly test for evidence of persistent effects of swarm exposure on measures of economic activity that could affect the long-term opportunity cost of fighting, in line with a permanent income mechanism. I find no significant long-term effects on the Normalized Difference Vegetation Index (NDVI) or on measures of local crop yields using remote sensing (Cao et al., 2025) or Demographic and Health Survey (DHS) data (IFPRI 2020). This indicates no permanent decrease in agricultural productivity, though analyses at the level of 0.25° cells may struggle to capture such effects when the median locust swarm would affect just 6% of cell area. I do observe significant long-term decreases in crop area cultivated and suggestive evidence of increases in out-migration, indicating transitions away from agricultural work. Together with evidence from other studies using survey data to show persistent adverse effects of locust swarms on agricultural production and measures of human capital, these results suggest that a long-term decrease in the opportunity cost of fighting is a plausible mechanism but more evidence from household-level analyses is needed to test it further.

Finally, to analyze whether the dynamic effects on conflict risk I estimate are specific to locust swarms I test impacts of exposure to drought, measured using monthly Standardized Precipitation and Evapotranspiration Index (SPEI) data. I find large time-varying increases in conflict risk that are also driven by locations experiencing civil conflict in surrounding areas, implying that the same mechanisms underly the effects of both types of agricultural shocks. The long-term increases in conflict risk indicate that analyses defining shock 'treatment' as transitory and estimating short-term impacts using two-way fixed effects (the main method in studies of climate or agricultural shocks and conflict) are misspecified for shocks with non-zero average long-term effects. I show that such specifications result in downward-biased estimates of the short-term impacts of both locust swarms and drought on violent conflict, affecting the policy implications.

This paper makes several contributions to the literature. First, I add to our understanding of the drivers of conflict (Bazzi and Blattman, 2014; Blattman and Miguel, 2010; Buhaug et al., 2021; Collier and Hoeffler, 1998, 2004; Grossman, 1999; Miguel et al., 2004), and the roles of climate (Burke et al., 2024; Mach et al., 2020) and shocks to agricultural production (Crost et al., 2018; Harari and La Ferrara, 2018; McGuirk and Nunn, 2025; Von Uexkull et al., 2016) in particular. While a relationship between climate and conflict has been repeatedly demonstrated, the mechanisms driving this impact are not fully understood. Weather shocks affect a variety of economic and social outcomes in addition to reducing agricultural labor productivity and agricultural output (Dell et al., 2012, 2014; Mellon, 2022), but much of the literature has emphasized an opportunity cost mechanism to explain increases in conflict risk.[3] The results of this paper indicate that the opportunity cost of

[3]Little attention is given to the rapacity mechanism in the climate-conflict literature. Studies showing evidence of opportunity cost and rapacity mechanisms in agriculture have primarily explored impacts on conflict risk of changes in global prices of agricultural goods (e.g., Dube and Vargas, 2013; Fjelde, 2015; McGuirk and Burke, 2020) rather than shocks to local agricultural production. McGuirk and Nunn (2025) is an exception, analyzing impacts of drought

fighting mechanism alone cannot explain dynamic impacts of locust swarms and drought on conflict risk over time. Similar to Buhaug et al. (2021)'s analysis of short-term effects of drought on conflict but considering longer-term dynamics, I highlight the importance of grievances and insecurity in determining when adverse effects of past shock exposure may lead to violent conflict.

Second, I contribute to a broader literature on the dynamic long-term impacts of environmental shocks and natural disasters. Many papers have explored how adverse environmental shocks can have persistent effects on poverty and well-being (Baseler and Hennig, 2023; Carter and Barrett, 2006; Carter et al., 2007; Lybbert et al., 2004). Studies of the impacts of agricultural production shocks on conflict have focused on the short-term, with the exception of Narciso and Severgnini (2023) who show that individuals in families more affected by the Great Irish Famine were more likely to participate in the Irish Revolution decades later.[4] More generally, the evidence on long-term impacts of disasters such as hurricanes and droughts is limited, inconclusive, and focused on a small number of outcomes (see Botzen et al. (2019) and Klomp and Valckx (2014) for reviews). I study dynamic impacts of desert locusts swarms—an extreme shock to agricultural production akin to a natural disaster—on conflict risk and test whether patterns are consistent with an opportunity cost mechanism, and find similar results when considering exposure to drought. The dynamic long-term impacts on conflict risk imply that studies estimating transitory short-term impacts of severe economic shocks may be biased, even if the direct effects of those shocks are temporary.

Third, this paper adds a new dimension to studies on the economic impacts of agricultural pests (Oerke, 2006), including desert locusts (see e.g., Thomson and Miers, 2002), and builds on a small literature on the long-term impacts of pest shocks (Ager et al., 2017; R. Baker et al., 2020; Banerjee et al., 2010). The range of many agricultural pests is expanding due to climate change and globalization, and—though locust outbreaks have become less frequent in recent decades due to increased monitoring—desert locusts are ideally situated to benefit from climate change (Qiu, 2009). A small recent body of research links locust data with survey data and finds that locust swarm exposure adversely affects long-term education (Asare et al., 2023; De Vreyer et al., 2015), health (Conte et al., 2023; Gantois et al., 2024; Le and Nguyen, 2022; Linnros, 2017), and agricultural production (Marending and Tripodi, 2022) outcomes, but I am the first to study long-term impacts of a pest shock on conflict and explore potential mechanisms. The impacts of locust swarms on long-term economic activity and conflict risk should be considered in determining policy around desert locust prevention and control.

on conflict between pastoralists and farmers.

[4]To my knowledge, no other study has explored long-term impacts on conflict risk of a transitory negative shock to agricultural production. Crost et al. (2018) and Harari and La Ferrara (2018) estimate effects of weather shocks on conflict over 2 and 5 years. Iyigun et al. (2017) consider long-run effects on conflict risk of a *positive and permanent* agricultural productivity shock from the introduction of the potato to the Eastern Hemisphere.

2 Background: Desert locusts

Desert locusts (*Schistocerca gregaria*) are a species of grasshopper always present in small numbers in desert 'recession' areas from Mauritania to India.[5] They usually pose little threat to livelihoods but favorable climate conditions in breeding areas—periods of repeated rainfall and vegetation growth overlapping with the breeding cycle—can lead to exponential population growth. Unique among grasshopper species, after reaching a particular population density locusts undergo a process of 'gregarization' wherein they mature physically and begin to move as a cohesive unit (Symmons and Cressman, 2001), with adult winged locusts forming large mobile swarms. When swarms migrate away from breeding areas and affect multiple countries, this is referred to as an outbreak or upsurge. Climate change is expected to increase the risk of desert locust swarm formation and upsurges, as these locusts can easily withstand elevated temperatures and the increased frequency of heavy rainfall events can create conditions conducive to population growth (McCabe, 2021; Qiu, 2009; Youngblood et al., 2023).

Desert locust swarms vary in density and extent, but the average swarm covers tens of square kilometers and includes billions of locusts (Symmons and Cressman, 2001). About half of swarms exceed 50 km^2 in size (FAO and WMO 2016). The size of swarms is what makes them so destructive. A small swarm covering one square kilometer consumes as much food in one day as 35,000 people and the median swarm consumes 8 million kg of vegetation per day (FAO, 2023a), without preference for different types of crops (Lecoq, 2003). The arrival of a swarm can lead to the total destruction of local vegetation (Symmons and Cressman, 2001; Thomson and Miers, 2002). For example, during the 2003-2005 locust upsurge in North and West Africa, 100, 90, and 85% losses on cereals, legumes, and pastures respectively were recorded, affecting more than 8 million people and leading to 13 million hectares being treated with pesticides (Showler, 2019).

Extensive locust monitoring and control operations are conducted in countries at regular risk from locust swarms. These are insufficient to prevent all upsurges but can help limit their spread and damages. These activities and knowledge of desert locust breeding patterns and swarm flight characteristics also inform efforts to predict locust swarm formation and movements, but forecasts remain highly imprecise (Latchininsky, 2013). Even given such information, farmers have no proven effective recourse when faced with the arrival of a locust swarm; activities such as setting fires or placing nets on crops or to capture locusts may slow damage but have little effect on locust population (Dobson, 2001; Hardeweg, 2001; Thomson and Miers, 2002). The only current viable method of swarm control is direct spraying with pesticides, which can take days to have effects as well as being slow and costly to organize and requiring robust locust control infrastructure (Cressman and Ferrand, 2021). Farmers in affected areas report viewing locust swarms as an unpredictable natural disaster that is the government's responsibility to address (Thomson and Miers, 2002).

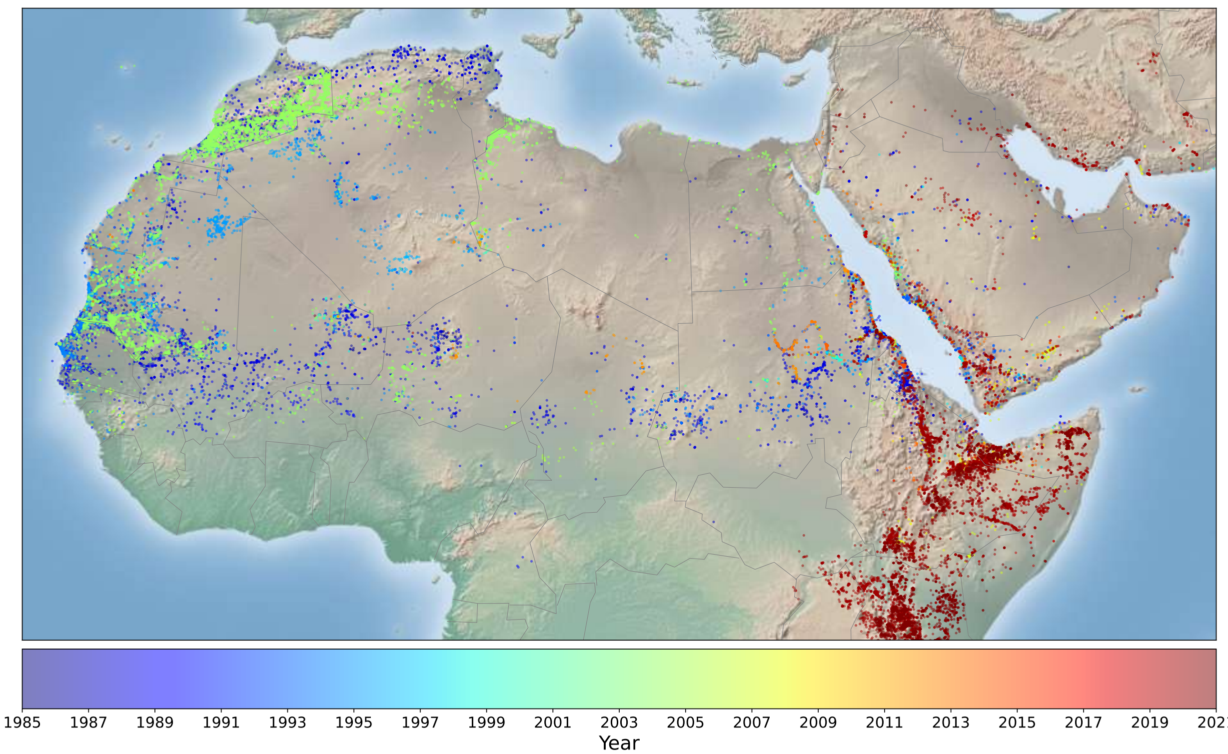
Households exposed to locust swarms use a variety of measures to cope with the adverse food security and livelihood effects. Locusts may be eaten if they have not been sprayed with pesticides,

[5] Additional detail on desert locusts is included in Appendix C. Any time I use 'locusts' in this paper I am referring exclusively to desert locusts.

but in most cases this is unlikely to substitute for lost production. In addition to seeking help from social networks and food aid, households commonly report selling animals and other assets, consuming less food, sending household members away, taking loans and cutting expenses, and consuming seed stocks as coping strategies (Thomson and Miers, 2002). A swarm exposure shock therefore represents a shock to income and household wealth as well as a shock to agricultural productivity in the year of exposure.

Figure 1 displays the locations of desert locust swarm observations recorded in the FAO Locust Watch database from 1985 (the first year they were recorded) to 2021, for the area of interest for this study.[6] As illustrated by the figure, nearly all locust swarms are observed during periods of major upsurges.

Figure 1: FAO Locust Watch swarm observations by year



Note: Map created by author using locust swarm records in the FAO Locust Watch database. Each dot represents the location of a single locust swarm observation, and the color indicates the year of the observation.

The characteristics of desert locust swarms make them a useful natural experiment for analyzing the long-term impacts of agricultural production shocks on the risk of conflict and testing income-related mechanisms. First, the timing of upsurges and patterns of swarm flight create quasi-random temporal and local variation in swarm exposure. Locusts swarms are migratory and fly 9-10 hours per day, generally downwind, easily moving 100 km or more per day even with minimal wind (FAO and WMO 2016). Conditional on being in the migratory path during an upsurge, swarm flight

[6]Desert locust swarms also affect other countries in the Middle East and South Asia, but not during the time period of this study.

patterns create quasi-random variation in exposure as some areas in the flight path are flown over and spared any damages.[7] The arrival of a swarm also does not change future risk (Figure C3 shows this empirically), so the direct shock to agricultural production is transitory even if indirect effects may persist.

Second, the arrival of a swarm is effectively a locally and temporally concentrated natural disaster affecting crops, pasture, and other vegetation (Hardeweg, 2001), but not directly affecting other aspects of the economy. This contrasts with temperature and precipitation shocks which may affect infrastructure or physiology as well as agricultural production. Third, the level of damage to agriculture from swarms and lack of tools for farmers to prevent damages imply severe reductions in agricultural production. This increases the potential for persistent adverse effects on well-being due to efforts to cope with the shock, and therefore for persistent effects on conflict risk.

3 Conceptual framework

Weather affects the economy and society through multiple channels (Dell et al., 2012, 2014; Mellon, 2022), which is part of why recent reviews highlight the need for more research into the mechanisms driving the relationship between weather and conflict (Burke et al., 2024; Mach et al., 2020). One key mechanism is effects on agricultural production, but other studies have pointed to physiological, psychological, and infrastructural effects of weather shocks as also helping to explain impacts on conflict (Baysan et al., 2019; Burke et al., 2024; Carleton et al., 2016; Chemin et al., 2013; Dell et al., 2014; Hsiang and Burke, 2013; Sarsons, 2015; Witsenburg and Adano, 2009). These channels may be important in determining the effect of agricultural shocks on conflict risk (Burke et al., 2024), but for the purpose of this paper I focus on testing mechanisms operating through effects on agricultural production and income. This seems appropriate for the case of desert locust swarms which do not have direct effects on infrastructure or human physiology, though effects on institutions and human psychology may be important and a subject for future study.

An important set of models in the economics literature on agricultural shocks and intergroup conflict uses an occupational choice framework (as in French and Taber, 2011; Heckman and Honore, 1990; Roy, 1951) where actors allocate their labor between productive activities and fighting.[8] Chassang and Padró i Miquel (2009) develop a bargaining model of conflict where groups allocate labor to crop production or fighting over land, and Dal Bó and Dal Bó (2011) model individuals choosing between a labor-intensive sector, a capital-intensive sector, and an 'appropriation' sector fighting over output. McGuirk and Burke (2020) develop this latter model to allow both factor and output conflict and incorporate consumers who may also engage in fighting. These models emphasize how a shock to labor productivity in a given sector affects both the opportunity cost of engaging in conflict for individuals in that sector, as well as the returns to capturing output or production factors in that sector. These are the opportunity cost and rapacity mechanisms,

[7] Figure C4 illustrates the local and temporal variation in exposure to swarms for the area around Mali.

[8] These models follow an early application by Becker (1968), who uses a similar setup to model interpersonal conflict such as theft.

respectively.

In this section I present a simple streamlined model of occupational choice drawing on these models but allowing for the possibility of dynamic long-term effects of a transitory productivity shock. I follow Dal Bó and Dal Bó (2011) and others in treating fighting as an individual/group decision, rather than Chassang and Padró i Miquel (2009) and others who present conflict as a failure of a bargaining process between groups. This decision simplifies the model and provides a useful framework for presenting income-related mechanisms I use the model to build intuition and generate testable hypotheses about the effects of agricultural shocks on conflict. The objective of these tests is to evaluate whether empirical effects are consistent with the opportunity cost mechanism promoted in much of the literature on agricultural shocks and conflict, or whether other mechanisms are needed to explain dynamic effects over time.

3.1 Model

In the model, individuals in each time period allocate one unit of labor L to either agricultural production, non-agricultural work, or violent conflict to maximize total net income I. Violent conflict is therefore treated as a potential occupation with appropriable returns. I set aside other types of objectives for engaging in intergroup conflict, though these could be considered as also generating indirect economic returns. Returns to all activities are affected by individual and location characteristics X such as land quality, level of education, fighting ability, and local economic development.

Net returns to agricultural production $F^A(L^A, S, W, X)$ are affected by adverse agricultural shocks S, with $\frac{\partial F^A}{\partial S} < 0$ and $\frac{\partial^2 F^A}{\partial S^2} < 0$. A larger S, such as a drop in international crop prices or a severe drought, therefore reduces the returns to agricultural labor—the *opportunity cost mechanism*: producers have less to lose by engaging in conflict. At the same time, lower agricultural prices or output reduce the returns to predatory attacks: bandits or looters have less to gain from fighting. Dube and Vargas (2013) refer to this as the *rapacity mechanism*. These mechanisms are not unique to agricultural shocks, as they are also discussed in earlier work on the economic drivers of conflict more generally (Collier and Hoeffler, 1998, 2004; Grossman, 1999).

Prior research models transitory agricultural shocks—which only directly affect returns to labor in the period they occur—as having only temporary effects on conflict, as there is no direct persistent effect on agricultural productivity. The immediate shock income could have persistent effects, however, as most agricultural households in developing countries lack insurance and have constrained access to credit. Strategies to smooth consumption following an income shock, such as selling animals and other assets, taking loans, reducing food, health, and education spending, and sending members away reduce household physical and human capital (e.g., de Janvry et al., 2006; Dercon and Hoddinott, 2004; Dinkelman, 2017). The resulting reductions in wealth mean transitory shocks can have persistent impacts on productivity (Dercon, 2004; Donovan, 2021; Hallegatte et al., 2020; Hoddinott, 2006; Karim and Noy, 2016). I therefore propose a wealth or *permanent income mechanism* whereby a transitory negative shock indirectly but persistently reduces productivity through direct effects on productive assets, and affects conflict risk by persistently reducing the opportunity cost of fighting.

I incorporate this into the model by making agricultural production depend on wealth W with $\frac{\partial F^A}{\partial W} > 0$, where wealth broadly includes human, physical, and financial capital. Wealth in period t is weakly increasing in income I from activities in period $t-1$. As agricultural shocks decrease income, this creates an indirect relationship between past agricultural shocks S_{t-s} and agricultural production in period t, where $s \in [1, \tau]$ for some τ. We can write $F_t^A = F^A(L_t^A, S_t, W_t(\{S_{t-s}\}_{s=1}^{\tau}), X_t))$, with $\frac{\partial F_t^A}{\partial S_{t-s}} < 0$ capturing the possibility of long-term effects of a transitory agricultural shock.

Net returns to non-agricultural work $F^N(L^N, X, W)$ are based on the most productive activity available outside of own agricultural production, including migrating to work. The highest returns available depends on individual and location characteristics X and wealth W. As a simplifying assumption, I suppress the direct dependence of non-agricultural returns on S. Returns to non-agricultural work thus set a lower bound on how far the opportunity cost of fighting may fall following a negative agricultural shock. With $\frac{\partial F^N}{\partial W} > 0$, F^N will be weakly smaller for individuals primarily engaged in agriculture that experienced a past agricultural shock due to the permanent income mechanism.

Finally, an individual i can also decide to join in armed conflict against targets J which may include individuals, enterprises, or organizations of different types including the government. These targets may be within the same broadly-defined location as i or in neighboring locations, and may experience the same or different agricultural shocks. The aim of the violent conflict may include both capture of outputs (rapacity) or attempts to capture and control factors of production, such as land or territory. I acknowledge that violent conflict may have other direct objectives but to simplify the model assume that all of these objectives can be represented as permitting more control over resource flows from production outputs or factors.

The potential net returns to fighting are $F^C(L_i^C, X_i, W_i\{I_j, W_j, X_j\}_{j\in J})$. The costs of fighting depend on i's resources W_i and local context X_i, while the probability of successfully capturing resources depends on these factors as well as characteristics of targets X_j. Costs of fighting are incurred with certainty and include economic, social, and emotional costs as well as risk of injury or death. These costs make fighting sub-optimal for most individuals in most time periods. An important variable in $X_{i,t}$ is whether there is preexisting mobilization against outside targets. In practice, individuals are unlikely to engage in violent conflict alone, as such fighting generally involves organized armed groups which recruit members and pay them a wage or share of the returns from victory (Collier and Hoeffler, 2004; Grossman, 1999). Prior mobilization of groups that could engage in armed conflict will reduce the costs to the individual of fighting, in multiple dimensions (financial, social, psychological). Mobilized groups will likely also have or perceive a greater likelihood of engaging in violent conflict successfully.

Heterogeneity in the presence of mobilized groups is likely to be important in determining effects of an agricultural shock. Buhaug et al. (2021) note that opportunity cost models can explain economic motives for engaging in conflict but not when these motives actually translate into action.

Building on Collier and Hoeffler (2004), they propose a model of civil conflict that predicts an income shock to increase violence primarily in a context of collective grievances. In line with this model, they find that drought shocks do not in general increase the risk of rebellion of affected ethnic groups, but do increase this risk among marginalized ethnic groups more dependent on agriculture. The assumption is that high levels of local grievances foster mobilization of groups against the object of the grievances, and that these groups can at times resort to violent conflict to achieve their objectives. An agricultural shock itself may contribute to grievances by for example increasing inequality, but many other factors unrelated to the shock also contribute to grievances, and I abstract away from these underlying causes. Following this logic, I propose a *grievance mechanism* creating variation in when an agricultural shock may increase the risk of conflict by lowering the threshold to which the opportunity cost of fighting must fall to make switching to fighting optimal.

The potential returns to fighting depend on the incomes (production output), wealth (factors of production), and characteristics of the individuals in J. With $\frac{\partial F_t^C}{\partial S_{j,t}} < 0$, agricultural shocks S_j for individuals $j \in J$ decreasing the income available to capture and therefore reduce conflict risk through the rapacity mechanism. With $\frac{\partial F_t^C}{\partial S_{j,t-s}} < 0$ (the permanent income mechanism), past agricultural shocks to individuals $j \in J$ will also reduce conflict risk by decreasing both the output and the factors that i can capture.

The individual's problem in period t can be presented as choosing their labor allocation $L_{i,t}$ to maximize income $I_{i,t}$ given some current and past shock realizations S_i, S_j. For simplicity and intuition I ignore uncertainty in returns and suppose that decisions are made (or equivalently, updated) after the agricultural shocks in the period are realized.

$$\begin{aligned}
\max_{L_{i,t}^A, L_{i,t}^N, L_{i,t}^C} I_{i,t} =& F^A(L_{i,t}^A, S_{i,t}, W_{i,t}(\{S_{i,t-s}\}_{s=1}^{\tau}), X_{i,t}) + F^N(L_{i,t}^N, W_{i,t}(\{S_{i,t-s}\}_{s=1}^{\tau}), X_{i,t}) \\
&+ F^C(L_{i,t}^C, X_{i,t}, W_{i,t}, \{I_{j,t}, W_{j,t}(\{S_{j,t-s}\}_{s=1}^{\tau}), X_{j,t}\}_{j \in J}) \\
\text{subject to } & L_{i,t}^O \in \{0,1\}, \quad F^O(0,.) = 0, \quad \text{and } \sum_O L_{i,t}^O = 1 \text{ for } O \in \{A, N, C\} \\
& \frac{\partial F^A}{\partial S_{i,t}} < 0; \quad \frac{\partial F^A}{\partial S_{i,t-s}} < 0; \quad \frac{\partial F^C}{\partial S_{j,t}} < 0; \quad \frac{\partial F^C}{\partial S_{j,t-s}} < 0
\end{aligned}$$

This yields

$$\begin{aligned}
L_{i,t}^C = 1 \text{ iff } & F^C(1, X_{i,t}, W_{i,t}, \{I_{j,t}, W_{j,t}(\{S_{j,t-s}\}_{s=1}^{\tau}), X_{j,t}\}_{j \in J}) \\
& \geq \max(F^A(1, S_{i,t}, W_{i,t}(\{S_{i,t-s}\}_{s=1}^{\tau}), X_{i,t}), F^N(1, W_{i,t}(\{S_{i,t-s}\}_{s=1}^{\tau}), X_{i,t}))
\end{aligned}$$

In words: individual i chooses to engage in violent conflict if the net returns from fighting exceed their opportunity cost—the highest net returns they could receive from choosing another occupation.

3.2 Testable hypotheses

In general, the effect of a negative agricultural shock on the decision to fight is ambiguous, particularly if there is a strong positive correlation between shocks over space as in most agricultural shocks. At the same time as agricultural producers' opportunity cost of fighting is reduced, the decrease in local agricultural production makes conflict over output (i.e., banditry) less attractive. For transitory agricultural shocks which do not have a permanent direct effect on local agricultural productivity, the value of factors of production—land in particular—should be less affected.[9] In line with this, the literature generally finds that the opportunity cost mechanism dominates for shocks that temporarily reduce agricultural returns, increasing conflict risk, though the rapacity cost mechanism is sometimes found to dominate for shocks that increase agricultural returns.

These considerations imply two testable predictions:

1. If the opportunity cost mechanism dominates, the local risk of violent conflict should increase immediately after shock exposure.
2. If the rapacity mechanism offsets the opportunity cost mechanism, this should attenuate short-term effects on measures of violent conflict, with relatively more attenuation for immediate conflict over output than for conflict over factors.

The long-term effects of past agricultural shocks $S_{i,t-s}$ on the decision to engage in conflict in period t also involve offsetting mechanisms. The permanent income effect persistently decreases production, reducing both the long-term opportunity cost of fighting and the returns to predatory attacks. Results from literature on short-term effects of adverse agricultural production shocks showing the opportunity cost mechanism typically dominates suggests that on net the permanent income mechanism should increase long-term conflict risk through its effects on the opportunity cost of fighting. Dynamic effects will depend on whether affected areas are able to recover over time. Assuming the direct production shock is more severe than subsequent production decreases under the permanent income mechanism, immediate effects should be larger than long-term effects, all else equal.

Persistent effects through a permanent income mechanism should be more likely for more severe shocks. Several papers have documented persistent effects of locust swarms on outcomes which could influence productivity and therefore the opportunity cost of fighting. Studies using the Demographic and Health Surveys (DHS) show that young children exposed to locust swarms are more likely to drop out of school (Asare et al., 2023) and achieve lower educational attainment (De Vreyer et al., 2015), and also have lower height-for-age (Conte et al., 2023; Gantois et al., 2024; Le and Nguyen, 2022; Linnros, 2017) when they are older. Such human capital effects of swarm exposure could decrease permanent labor productivity. More directly, Marending and Tripodi (2022) find that agricultural profits of households in parts of Ethiopia exposed to locust swarms in 2014 are 20-48% lower two harvest seasons after swarm arrival. This indicates a persistent decrease in agricultural productivity despite the fact the swarms have migrated and are no longer directly affecting productivity. Long-term effects on productivity and wealth should be directly observable if this is the mechanism through which a past shock affects current conflict.

[9]Transitory shocks may have some effect on the returns to factors if they affect individuals' ability to productively utilize factors or if they affect expectations about future productivity. Shocks that have direct permanent productivity effects, for example through soil erosion or other land degradation, would have larger effects on the returns to factors.

The model therefore suggests three more testable predictions for long-term impacts of a transitory agricultural shock on conflict risk:

3. If long-term opportunity cost effects under the permanent income mechanism dominate, we should observe persistent increases in conflict risk.
4. Assuming the initial direct production shock is more severe than the subsequent indirect effects on production, increases in conflict risk should be largest in the periods immediately after the shock.
5. If the permanent income mechanism drives long-term effects on conflict risk, we should observe long-term average reductions in measures of productivity following the initial shock.

Since violent conflict is not the norm in most locations and periods, it implies the returns are generally low. While there is evidence that agricultural shocks cause the onset of new violent conflict immediately following the shock, it is not clear when a negative productivity shock will lead an individual to switch from another activity to fighting. The importance of local conditions in determining the costs and returns of fighting mean that we should expect heterogeneity in the effects of an agricultural shock on conflict risk. A reduction in the opportunity cost of fighting is more likely to increase the risk of violent conflict when the costs of forming or joining armed groups are lower or the returns to such engagement are higher. This implies that dynamic impacts of an agricultural shock on conflict risk should be greater in periods of heightened local grievances and insecurity when groups are already mobilized around particular causes. Long-term effects under this mechanism require persistent effects of the shock on measures of productivity or well-being.

This consideration motivates a final testable prediction:

6. If grievance is an important mechanism, the dynamic impacts of a transitory shock on violent conflict should be concentrated in periods of heightened grievance. As grievances can be hard to measure, I consider measures of popular unrest or civil conflict which indicate grievances that have escalated to a high level.

4 Data

The Locust Watch database (FAO 2023) catalogs observations of desert locust swarms as well as smaller concentrations of locusts from 1985 to 2021.[10] I consider only data on locust swarms, which pose the greatest threat to agriculture and whose flight patterns create local variation in exposure. The Locust Watch data include latitude, longitude, and date of swarm observations. Locust observations are recorded by national locust control and monitoring officers on the ground, but incorporate reports from agricultural extension agents, government officials, and other sources. Local farmer scouts are also often trained in locust monitoring and reporting (Thomson and Miers, 2002).

[10] I last retrieved data from the Locust Watch database in 2023. As of Spring 2025, the data on desert locust presence appear to no longer be publicly available.

Data on conflict events come from the Armed Conflict Location & Event Data Project (ACLED) database (Raleigh et al., 2010). The database records the location, date, actors, and nature of conflict events globally starting from 1997 by compiling and validating reports from traditional media at different levels, from institutions and organizations, from local partners in each country, and from verified new media sources. The database includes many types of events including protests, riots, and various conflict-related strategic developments, but I focus on events categorized by ACLED as "violent conflict": battles, explosions, and violence against civilians.[11] As an alternative conflict type, I follow McGuirk and Burke (2020) in constructing a measure of output conflict (i.e., banditry) using ACLED records of violence against civilians, rioting, and looting. Such smaller-scale output conflict aligns well with the modeling of violent conflict as an occupational choice, and motivates using ACLED as the main source of conflict data.

For robustness, I also consider conflict data from the Uppsala Conflict Data Program (UCDP; Sundberg and Melander, 2013). The UCDP database uses similar sources as ACLED but goes back to 1989 and only records conflicts involving at least one "organized actor" and resulting in at least 25 battle-related deaths in a calendar year. The ACLED database has no organized actor or minimum death threshold requirements. The UCDP database has the advantage of being less subject to underreporting bias: some research has shown that events with no fatalities—including many of the types of ACLED violent conflict events I consider—suffer from more underreporting (Croicu and Eck, 2022; Eck, 2012). While UCDP captures a different type of violent conflict from ACLED, it therefore presents a useful check of the results.

I collapse these point event data to a raster grid with annual observations for cells with a $0.25°$ resolution (15 arcminutes, approximately 28×28km). Analyzing impacts at this spatial level reduces potential measurement error about the specific areas affected by swarm and conflict events and allows me to leverage local variation in swarm presence created by their flight patterns. The median swarm covers around 50 km^2, so nearly all swarms will be contained within $0.25°$ cells ($\sim$784 km^2), except those near cell boundaries. I test for robustness to analyzing data at the level of $0.5°$ and $1°$ cells, which will also capture potential spillovers from swarm exposure. In each cell and year I measure whether any locust swarm and conflict event is recorded. I do not account for variation in the counts of swarm or conflict events as the individual events are not themselves of consistent magnitudes. To test for spatial spillovers, I also measure whether any swarms are observed within 100 km outside of each cell-year.

I determine the country and highest sub-national administrative unit in which each cell centroid lies using country boundaries from the Global Administrative Areas (2021) database v3.6. I use sub-national administrative boundaries to create a set of 285 regions, all of which include at least 32 individual grid cells except for small countries with fewer than 32 cells. These regions are either

[11] I provide examples of each from 2012 in Mali. Battle: On August 22 in Ansongo, MUJAO fighters and Military forces from Niger clashed close to the town of Tessit, with four MUJAO fighters killed. Explosion: On August 15 in Ansongo, anti-personnel mines planted by MUJAO exploded and killed two people. Violence against civilians: On July 26 in Timbuktu, a shepherd was killed after resisting an attempt by Ansar Dine fighters to steal one of his animals.

existing sub-national administrative units or combinations of adjacent units within the same country. I cluster standard errors at the level of these regions.

Given the role of weather in desert locust biology, its importance in determining agricultural production, and the well-documented relationship between weather shocks and conflict, all analyses control for local weather to isolate the impact of locust swarm exposure. I measure total annual precipitation (in mm) and maximum temperature (in °C) using high-resolution monthly data from WorldClim available through 2018.[12] I measure drought exposure using monthly Standardized Precipitation and Evapotranspiration Index (SPEI) data from the Global Drought Monitor (Begueria et al., 2014). My primary definition of drought at the annual level is at least 4 consecutive months in a year where the SPEI in a cell is below -1.5, indicating significantly drier conditions relative to local historical norms (values from -1 to 1 indicate near-normal within-cell conditions).

I also incorporate raster population data for every 5 years from CIESIN, 2018, linearly interpolating within cells between years where the population is estimated, and raster data on land cover in 2000 from CIESIN, giving the share of land cover in each cell that is cropland and pasture (Ramankutty et al., 2010). I combine the land cover data with cropland mapping of Africa from 2003-2014 (Xiong et al., 2017) to identify cells with any cropland during the study period. I include additional cell characteristics, such as the start and end month of the main agricultural season, from the PRIO-GRID dataset (Tollefsen et al., 2012), assigning all 0.25° cells the values for the 0.5° PRIO-GRID cell in which they are located.

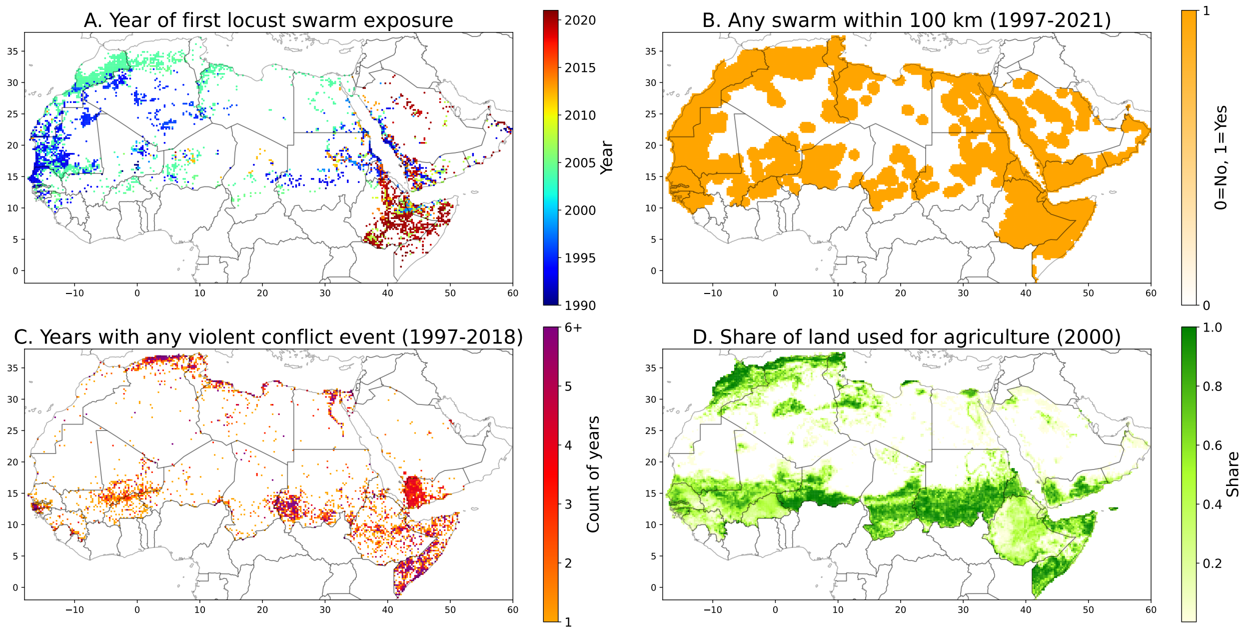
For the analysis of mechanisms, I incorporate data on agricultural production, economic activity, and net migration. Household-level estimates of agricultural production come from the Advancing Research on Nutrition and Agriculture (AReNA) Demographic and Health Surveys (DHS) GIS database (IFPRI 2020), which includes geolocated data at the level of household survey clusters for 40 surveys from 9 countries in the study sample conducted between 1992 and 2018.I incorporate two satellite-based measures of agricultural productivity: the Normalized Difference Vegetation Index (NDVI) and estimates of major crop yields. I calculate the NDVI—a commonly-used satellite-based measure of vegetation greenness at a given point and time—using 16-day 1km satellite imagery from MODIS (Didan, 2015) for the period 2000-2019, taking the maximum of monthly means in each grid cell to construct an annual value. In crop land, NDVI can be considered a rough proxy for agricultural production. Global annual yield data for four major crops—maize, rice, wheat, and soybean—at the 5 arcminute resolution for 1982-2015 come from Cao et al. (2025). Most cells only include data for one of the four crops, but for cells with multiple crops in the dataset I define the 'main' crop as the crop with the highest yield. Finally, net migration at the 5 arcminute resolution for 2000-2019 come from Niva et al. (2023), who estimate net migration based on subnational annual data on population, births, and deaths.

[12] CRU-TS 4.03 (Harris et al., 2014) downscaled with WorldClim 2.1 (Fick and Hijmans, 2017). I test sensitivity to measuring rainfall using CHIRPS (Funk et al., 2015) and temperature using ERA5 (Hersbach et al., 2019) to account for satellite-based weather measurement error (Josephson et al., 2024).

4.1 Sample and summary statistics

Since ACLED records conflicts beginning in 1997 and the main weather data are available until 2018, the analysis sample includes observations from 1997 to 2018. I restrict the analysis to countries with at least 10 locust swarm observations in this period. The resulting analysis sample covers 22 years across 25,435 cells, for a total of 557,018 observations with data on all main estimation variables. Figure 2 visualizes swarm exposure, violent conflict incidence, and agricultural land cover for the sample countries. Summary stats are included in Table A1.

Figure 2: Swarm exposure, violent conflict incidence, and land cover in sample countries



Note: Panel A presents the first year after 1989 in which a locust swarm is observed in a given cell, using FAO Locust Watch data. This defines the swarm exposure year. Panel B shows all cells where any locust swarm was ever observed within 100 km of the cell centroid from 1996-2021. Panel C shows the count of years for each cell in which any violent conflict event was recorded in the ACLED database, from 1997-2018. Panel D presents 'baseline' land cover data for the year 2000 from CIESIN. Land used for agriculture includes crop land and pasture land. T

Locust swarms are relatively rare events, with swarms reported in less than one percent of cell-years (Table A1 Panel A). But at least one locust swarm is recorded in 9% of cells in the study period of 1997-2018 and 55% are within 100 km of any locust swarm report (Figure 2 Panel B). To account for the possibility of persistent effects of swarm exposure, I identify for each cell the first year after 1989 in which a locust swarm is recorded (Figure 2 Panel A), and define a cell as exposed to a locust swarm in each following year and not exposed in all other years or if no locust swarm is ever observed. Cells first exposed to a swarm before 1997 (in dark blue in Figure 2 Panel A) are considered treated during the entire sample period and therefore do not inform the analyses, while cells first exposed to a swarm after 2018 (in red) are considered not treated during the sample period. Just over seven percent of cells are first exposed to a swarm during the sample period, including 5.3% exposed during the 2003-2005 upsurge (in teal).

Violent conflict is also uncommon, with events reported in two percent of cell-years (Table A1 Panel A). Conflict is temporally correlated (Figure 2 Panel C), with 13% of cells experiencing at least one violent conflict event during the study period, in 3.4 different years on average (Table A1 Panel B). The risk of any violent conflict is fairly low from 1997-2010 before increasing significantly over the remainder of the sample period (Figure 5 Panel A). The increase corresponds with the timing of the Arab Spring movements, the spread of Islamic militant groups, and multiple civil wars and separatist movements in the sample countries.

Over half the cells (57%) in the sample have agricultural land: 56% have pasture land while 31% have crop land. Across all cells, the mean share of land allocated to agriculture is 23% (Figure 2 Panel D, Table A1 Panel B), with 18% pasture land and 5% crop land. Given that locust swarms should affect outcomes through agricultural destruction, I test for heterogeneity in impacts by land cover.

4.2 Locust swarm monitoring

The Locust Watch database likely does not include all locations of swarm exposure events over time due to monitoring capacity limitations. Randomly missing swarm events—classical measurement error—would attenuate estimated effects, but swarm monitoring is likely correlated with characteristics that might also be correlated with conflict risk, such as agricultural activity and population levels. For example, Gantois et al. (2024) find heterogeneity in locust reporting across country borders, indicating differences in country monitoring capacities. Unreported swarms are an important challenge for studies using household survey data that must define exposure at the level of specific community coordinates, and studies such as Gantois et al. (2024) and Marending and Tripodi (2022) take different approaches to deal with this concern. An advantage of defining swarm exposure as an annual dummy variable at the level of grid cells is that only one swarm needs to be reported in a relatively large area to define the cell as exposed. But differences in cell-level monitoring effort may still lead to biased estimates.

The main empirical specification accounts for this in two ways. First, I restrict the sample to only cells where a locust swarm was ever reported within 100 km. This drops cells with no real risk of swarm exposure as well as cells far from any monitoring activity. Second, in all regressions I control for population and weather variables which are likely to be strongly correlated with both conflict risk and monitoring intensity. Cell fixed effects also control for fixed characteristics which may affect monitoring and the risk of locust exposure, such as elevation, distance from breeding areas, and being within typical swarm migratory paths. Country-by-year fixed affects control for average differences in state monitoring capacity.

In addition, I conduct several types of robustness checks to test whether issues in locust monitoring may affect the estimated effects on conflict risk. First, I estimate the propensity for a cell to have been exposed to a swarm during the study period, which accounts for differences in both swarm risk and monitoring, and test the sensitivity of results to controlling for propensity-by-year fixed effects. Second, I aggregate the analysis to the level of larger cells, which reduces the risk that individual unreported swarms may affect the analysis as such swarms are more likely to be co-located with other swarms that are reported in larger cells. Third, I systematically exclude different regions from the sample to check whether results are driven by areas with particular conflict and locust monitoring conditions. Fourth, I conduct simulations randomly imputing 'missing' locust swarms across all cells near the locations of reported swarms, to see how different levels of potential swarm underreporting would affect the results.

A specific concern might be that locust reporting is correlated with violent conflict. This concern is the focus of Torngren Wartin (2018)'s analysis of the impact of locusts on conflict, which uses similar data but focuses on the short-term, modeling locusts as temporary shocks. Showler and Lecoq (2021)—which I refer to as 'SL2021' in analyses below—review how insecurity has affected national and international desert locust control operations from 1985-2020 across countries where locusts are active. They mention Chad, Mali, Somalia, Sudan, Western Sahara, and Yemen as countries with areas where insecurity has constrained locust control operations in certain periods since 1997.

Insecurity is likely less of a constraint for locust monitoring than for control operations. FAO locust monitoring guidelines discuss conducting aerial surveys and using reports from local scouts, agricultural extension agents, security forces, and other sources (Cressman, 2001), which would allow reporting even in insecure areas. Gantois et al. (2024) find that contemporaneous conflict reduces the probability of any locust monitoring by 11.7%, but generally no significant effect on locust swarm reporting. This may reflect greater importance or resources for swarm monitoring, or better-established methods for collecting reports of swarms from disparate sources. The Locust Watch data includes observations of locust swarms even in countries and periods where SL2021 indicate control operations have not been possible. The share of cells within 50 km of a locust swarm observation in a given year that have reports of a locust swarm within the cell is around 25% in both the set of countries SL2021 indicate pose challenges for locust control and all other countries, and these swarm reports are much more likely to coincide with violent conflict events in the SL2021 countries suggesting this violence is not unduly deterring locust monitoring.

Missing swarm observations in high-conflict areas would bias my estimates downward by including in the control group areas exposed to locusts with likely higher future levels of conflict, as conflict risk is serially correlated. I test the sensitivity of the results to excluding the countries listed in SL2021 as potential locations of locust swarm under-reporting, and to systematically excluding different regions from the sample to check whether results are driven by areas with particular conflict and locust monitoring conditions. In addition, I conduct simulations randomly imputing 'missing' locust swarms particularly in cells experiencing conflict near the locations of reported swarms to test effects of potential underreporting in these areas.

5 Empirical approach

I use a difference-in-differences approach to estimate the causal impacts of locust swarm exposure on violent conflict. I consider swarm exposure to be an *absorbing treatment* to allow for long-term effects of this transitory agricultural shock. Cells are defined as exposed to a locust swarm in all years starting from the first year a swarm is observed in a cell (starting in 1990), and not exposed before or if no locust swarm is ever observed.

I estimate both static average impacts and dynamic impacts over time using the Borusyak et al. (2024) imputation estimator (BJS). I also consider alternative estimators including Callaway and Sant'Anna (2021), Cengiz et al. (2019), De Chaisemartin and d'Haultfoeuille (2024), and Sun and Abraham (2021) and a standard two-way fixed effects (TWFE) specification, and find similar results. The estimators mainly differ in how they estimate pre-trends. BJS impute counterfactual untreated outcomes for all units and make comparisons against the average over all pre-treatment periods, leading to smoother pre-treatment dynamics (Roth et al., 2023). This imputation approach deals with concerns with TWFE estimators when there is heterogeneity in treatment effects by time since treatment or across treatment cohorts, which can lead to 'forbidden' comparisons between late- and early-treated groups and negative weighting of effects for certain treatment groups or periods (Goodman-Bacon, 2021).

I present a standard TWFE estimation model to build intuition on the setup and the source of identifying variation in the analysis, as this is the same with the BJS estimator. The static average impact TWFE linear probability model takes the form:

$$Y_{ict} = \alpha + \beta Exposed_{ict} + \delta X_{ict} + \gamma_{ct} + \mu_i + \epsilon_{ict} \tag{1}$$

where i indexes cells, c countries, and t years. The outcome Y in the primary specifications is a dummy variable for observing any violent conflict event in the cell during a given year in the ACLED database. Analyzing conflict as a binary variable at an annual level reduces potential measurement error and is the main approach in the climate and conflict literature. I consider effects on other outcomes in tests of the impact mechanisms. *Exposed* is an absorbing dummy variable for having been exposed to a locust swarm, . switching from 0 to 1 in the year of exposure and for all subsequent years. Cells that are never exposed in the sample period have a value of 0 for all years, and cells first exposed beforehand have a value of 1 for all years. The dynamic version of the analysis includes 10 years of both lags and leads of the indicator for the onset of treatment. I choose 10 years to account for the fact that the analysis sample focuses on the period 1997-2018 and just under three-quarters of swarm exposure in this period takes place in 2004-2005. I test the robustness to considering different numbers of lags and leads.

Cell fixed effects μ_i control for time-invariant cell characteristics (over the sample period) which might affect both the likelihood of swarm exposure and of experiencing violent conflict. Country-by-year fixed effects γ_{ct} flexibly control for country-level variation over time in the intensity of locust swarms and violent conflict. X_{ict} is a vector of time-varying controls at the cell level to account for locally-varying conditions which may affect both swarm exposure and the risk of conflict. The main specifications include annual cell total precipitation, maximum temperature, and population. Impacts of swarm exposure are therefore estimated using differences within cells in variation over time in violent conflict, between cells in the same country and with the same precipitation, temperature, and population but with different exposure to locust swarms. Standard errors (SEs) are clustered at the sub-national region level (285 clusters) to allow for correlation in the errors within nearby areas over time.[13]

The BJS estimator follows a similar setup and includes the same fixed effects and controls, so leverages the same identifying variation. The process of the estimation is different, however, proceeding in three general steps (Borusyak et al., 2024, p. 3255). First, the estimator fits $\hat{\alpha}$, $\hat{\mu_i}$, $\hat{\gamma_{ct}}$, and $\hat{\delta}$ using only untreated observations. Next, these fitted values are used to impute untreated potential outcomes for treated cells post-exposure. These imputed outcomes are subtracted from actual outcomes to estimate treatment effects. Then, the estimator takes an average of treatment effect, either on average for the static impact estimate or by period relative to treatment for the dynamic estimate. Because the time fixed effects are estimated using only data from untreated observations, the $\hat{\gamma_{ct}}$ are implicitly weighted toward periods with more untreated units, but otherwise the averages are calculated using equal weighting across treatment cohorts (by year of exposure). In this study the untreated groups are quite large, so the implicit weighting should be fairly uniform.

The identification strategy relies on quasi-random variation in which areas in the migratory path of locust swarms during a given outbreak are actually exposed to locust destruction, due to swarm flight patterns. The key identifying assumption of the econometric design is that trends in conflict risk would be parallel over time in exposed and unexposed areas within the same country in the absence of locust swarm exposure, after controlling for effects of weather and population.

The key threat to the assumption is if locust swarms are more likely to affect areas with different propensity to experience conflict. The literature on locust biology suggests that—conditional on swarms forming in breeding areas and then beginning to migrate—where swarms land is driven by wind direction and time of day rather than land cover or particular types of vegetation. This implies quasi-randomness in exposure within swarm migratory paths. But selection in locust reporting, discussed in subsection 4.2, could lead to a violation of the parallel trends assumption by leading to differences in where swarms are observed.

While the parallel trends assumption is not possible to test directly, I explore its plausibility in two main ways: testing for balance in baseline or fixed cell characteristics and testing for parallel trends in outcomes prior to exposure. I discuss balance in cell characteristics here and discuss pre-trends when presenting the main event study results. Cells exposed to a locust swarm during the sample period have different baseline characteristics than unexposed cells which are largely consistent with desert locusts rarely being observed in the interior of the Sahara desert (as shown in Figure 2). Exposed cells have larger populations, are closer to capital cities, have a greater share of pasture land and smaller share of barren land, and have lower maximum temperatures (Table A2). These differences are smaller and lose some statistical significance when restricting the sample to cells within 100 km of any locust swarm from 1997-2021, a proxy for being in potential swarm

[13] Patterns of statistical significance are largely unchanged when using two-way clustered errors at the year and region level and using Conley (1999) Heteroskedasticity and Autocorrelation-Consistent (HAC) SEs allowing for spatial correlation over 100 and 500 km and serial correlation over 0 or 10 time periods, following Hsiang (2010)'s approach (Figure B3). Clustering at the sub-national region level consistently leads to SEs at least as large as Conley SEs allowing for spatial correlation within 500 km and serial correlation over 10 years, implying the main SEs I report are conservative and may understate statistical significance of certain estimates.

migratory paths (a joint test of differences in cell characteristics yields $F = 2.38$ and $p < 0.01$). These patterns appear more consistent with selection in locust swarm reporting than vegetation-driven selection in where swarms land, particularly as there is no difference in the share of cell crop land by exposure status.

One approach to account for imbalance in covariates is to weight observations by the conditional probability of being treated (Abadie, 2005; A. Baker et al., 2025; Stuart et al., 2014). I estimate the propensity of any locust swarm exposure during the study period as a function of fixed cell characteristics including land cover and population in 2000, distances to the capital and to a national boundary, mean weather realizations over the study period, and country fixed effects. I first use a lasso regression to select parameters and then estimate exposure propensity using a logit regression. I use the results to construct inverse propensity weights as $\frac{1}{p}$ for cells that were exposed and $\frac{1}{1-p}$ for cells that were not, where p is the estimated probability of swarm exposure. I assign cells with estimated probabilities outside the range of common support a weight of 0. Restricting to cells in likely migratory paths and including these weights largely eliminates differences in baseline characteristics ($F = 0.64$ and $p = 0.824$ for the joint test of differences; Table A2). The Borusyak and Hull (2023) approach to dealing with non-random exposure to exogenous shocks involves adjusting for average treatment across shock counterfactuals—this has been used in the case of locust swarms by Marending and Tripodi (2022). I draw on this by testing the robustness of the results to controlling for swarm exposure propensity-by-year fixed effects, which would account for potential different trends in areas with high exposure propensities, treating this as a proxy for average treatment under different shock realizations.

Baseline differences by swarm exposure status are not a concern if they do not affect conflict risk or only affect levels of conflict, but it is plausible that some of the differences would affect conflict trends. However, the controls in the empirical specifications should absorb most of these differences. Cell fixed effects control for time invariant cell characteristics that might affect the risk of conflict such as distance to major cities or country boundaries, topography, and agricultural suitability, but also distance from locust breeding areas. Country-by-year fixed effects flexibly control for factors varying over time at the country level that might affect conflict risk, such as food price shocks, weather patterns, the policy environment and national economic and social conditions. Importantly, they control for trends in violent conflict incidence, which increases over the sample period. I also directly include in the regressions time-varying characteristics that differ between exposed and unexposed cells and may affect locust reporting and conflict risk—population and temperature–as well as annual precipitation. I control for baseline differences in distance to the capital by defining all cells as either below or above the median distance to the capital within each country, and including capital distance by year fixed effects in the regressions to account for potential differential trends in conflict risk over time by proximity to country capitals. Finally, the main analyses restrict the sample to cells within 100 km of any locust swarm from 1997-2021 (illustrated in Figure 2 Panel B) and within the range of common support of the estimated propensity of swarm exposure across exposed and unexposed cells. This effectively restricts the sample to areas within swarm migratory paths during recent outbreaks, where exposure is most likely to be quasi-random.

Another identification assumption relevant to event study designs with staggered treatment timing is the no anticipation assumption: knowledge of future treatment timing does not affect current outcomes (Roth et al., 2023). Populations may expect a higher probability of swarm exposure in years of major upsurges but cannot perfectly anticipate timing of exposure. For example, the FAO Desert Locust Watch publishes monthly forecasts of areas predicted to be at risk of locust swarm exposure but the predictions include a great deal of uncertainty due to unpredictable minor variations in swarm flight patterns. Consequently, areas forecast to be at risk are generally quite large, the majority of which end up not being affected by locusts.[14] Anticipation may also have limited effects as there are no effective methods of defending vegetation against locust swarms, and farmers in at-risk areas typically describe locust prevention and control as out of their hands and the responsibility of governments (Thomson and Miers, 2002).

Finally, the empirical specification assumes no effects of locust swarm exposure outside of the exposed cells. In the robustness checks, I first test the sensitivity of the results to conducting the analysis in larger grid cells, to increase the likelihood that any effects are contained within exposed cells. I then relax this assumption by including an absorbing treatment indicator for being within 100 km of a locust swarm during the study period, defined similarly as the within-cell exposure variable.

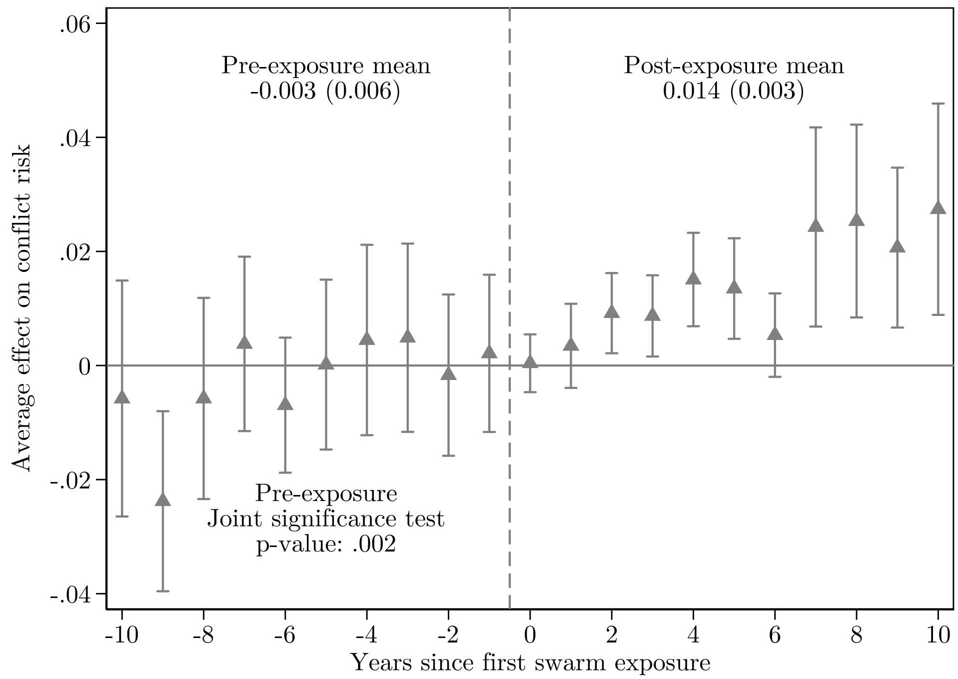
6 Results

Figure 3 presents event study estimates of the impact of desert locust swarm exposure on violent conflict at the cell-year level using the Borusyak et al. (2024) imputation estimator (BJS). The average pre-exposure difference is -0.003 but is not significantly different from 0, with 5 positive coefficients and 5 negative coefficients. Only one pre-exposure coefficient is larger than 0.007 in magnitude or statistically significant: on average violent conflict is 2.4 percentage points (pp) less likely in areas exposed to locust swarms compared to unexposed areas 9 years before exposure. Because of this one highly significant difference I reject that pre-exposure differences are jointly equal to 0 ($p = 0.002$), but I fail to reject that the 9 other pre-exposure coefficients are jointly equal to 0 ($p = 0.230$). I find similar patterns and also fail to reject that pre-exposure coefficients are jointly equal to 0 if I include only 6 pre-exposure periods (Figure B2 panel A). Together, these results indicate similar probability of violent conflict by swarm exposure in the pre-exposure periods. Though this does not preclude the possibility that trends would differ in the years after swarm exposure for reasons unrelated to agricultural destruction, it is an encouraging sign that the parallel trends assumption likely holds.

In contrast with the pre-exposure coefficients, 8 of 11 treatment coefficients are statistically significant at a 95% confidence level. The average effect is a 1.4pp increase in the annual risk of any violent conflict event. All coefficients are positive and all but the three non-significant coefficients

[14] I find that monthly forecasts of at-risk areas during the major upsurge in 2004 covered on average 40.6% of 0.25° cells in sample countries, but nearly one-quarter of swarms in this period were recorded outside of these areas.

Figure 3: Impacts of exposure to locust swarms on violent conflict risk over time



Note: The dependent variable is a dummy for any violent conflict event in a cell-year. Estimated impacts in each time period are weighted averages across effects for swarm exposure in particular years, calculated using the BJS estimator. Time period 0 is the year of first swarm exposure. Brackets represent 95% confidence intervals using SEs clustered at the sub-national region level. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. Observations are grid cells approximately 28×28km by year. Coefficient estimates are shown in Table A3 column 1. At the top left and right I show averages of pre- and post-exposure coefficients. On the bottom left I show the results of a joint test that the pre-exposure coefficients equal 0. A joint test omitting the period 9 years before exposure yields $p = 0.230$.

are larger than 0.007 in magnitude, the largest pre-exposure magnitude outside of the period 9 years before exposure.

Despite the large average long-term effects, the point estimate for the effect on violent conflict risk in the year of exposure is a fairly precise 0. This contrasts with much of the literature on climate and conflict which focuses on short-term effects. These studies generally find significant concurrent increases in conflict risk, though Crost et al. (2018) and Harari and La Ferrara (2018) also find that growing season weather shocks have delayed effects on conflict. Estimated treatment effects are positive but not significant in period 1 after exposure but then significant and generally increasing until another null effect 6 years after exposure. The most striking result is that the largest effects of exposure on violent conflict risk are realized starting 7 years post-exposure: coefficients for periods 7-10 are all larger than 0.021. While the standard errors for these coefficients are larger they are all significant at a 99% confidence level. I explore reasons for this pattern of dynamic impacts in the Mechanisms section.

I estimate event study effects using several alternative staggered difference-in-differences estimators (Callaway and Sant'Anna, 2021; Cengiz et al., 2019; De Chaisemartin and d'Haultfoeuille, 2024; Sun and Abraham, 2021) and find nearly identical dynamic treatment effects (Figure B1) and average long-term effects (Table B1). There is some variation in estimated pre-exposure differences, which is expected as a key difference across these estimators is how pre-exposure effects are calculated. Pre-exposure standard errors are generally larger in the main Borusyak et al. (2024) method because comparisons are made against averages over the full pre-treatment period and there are not 10 years of pre-exposure data for most treated cells. The Callaway and Sant'Anna (2021) finds more significant pre-exposure differences, but these alternate between positive as negative likely because this method makes comparisons between adjacent periods, suggesting no clear pre-exposure pattern in violent conflict risk. The other estimators each find limited significant (negative) pre-exposure differences as in the BJS method.

6.1 Average impacts of swarm exposure on violent conflict

Figure 3 shows that on average across 11 exposure and post-exposure periods, swarm exposure increases the annual risk of any violent conflict event by 1.4pp. Table 1 column 1 shows estimated average long-term impacts across all exposure and post-exposure periods. On average cells exposed to locust swarms are 1.8pp more likely to experience any violent conflict in a given year in the period after swarm exposure than cells not exposed. This represents a 64% increase over the mean for non-exposed cells after 2004, the main year of swarm exposure. The larger average long-term impact in Table 1 column 1 than in Figure 3 indicates that the larger magnitude effects on violent conflict persist more than 10 years after swarm exposure, and I show this when including 14 years of post-exposure effects in Figure B2 Panel B.

The average long-term impact of swarm exposure is large compared to the same-year effects of weather fluctuations. A 1 SD increase in annual precipitation increases the probability of any violent conflict in the same year by 0.4pp (14%) compared to 1.2pp (43%) for a 1 SD increase in the maximum annual temperature. The effect of temperature is in the upper end of the distribution of estimates of the impacts of climate on intergroup conflict in Burke et al. (2024)'s meta-analysis, potentially because of the use of maximum temperature and the time period studied. Cell population is also positively associated with conflict risk, with an increase of 10,000 people associated with a 1.0pp (36%) increase in the probability of any violent conflict event in a year.

If estimated effects are capturing causal effects of swarm exposure we should expect heterogeneity by land cover since swarm exposure should primarily impact outcomes through agricultural destruction (Table 1 columns 2 and 3). Swarm exposure in non-agricultural cells (23% of the analysis sample) has no significant effect while in agricultural cells annual violent conflict risk increases by 1.9pp, though I cannot clearly reject that the effects are the same ($p = 0.109$). Locust swarms increase annual violent conflict risk by 2.3pp in crop cells (57% of the sample) compared to no significant effect in non-crop cells (46% of which have pasture land), and here the difference is statistically significant ($p = 0.025$). These differences are consistent with locust swarms affecting conflict risk through initial agricultural destruction.

I find similar average long-term effects of swarm exposure estimated using TWFE as presented in Equation 1 (Table B2). The average long-term impact with this estimator is a 2.0pp increase, slightly larger than the main BJS estimate. This suggests limited bias in the TWFE estimates from

Table 1: Average impacts of exposure to locust swarms on violent conflict risk by land cover

Outcome: Any violent conflict event	(1) All land	(2) Land = Any crop or pasture land	(3) Land = Any crop land
Average effect of swarm exposure	0.018***		
	(0.004)		
Total annual precipitation (SDs)	0.004**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)
Max annual temperature (SDs)	0.012**	0.012**	0.012**
	(0.005)	(0.005)	(0.005)
Population (10,000s)	0.010***	0.010***	0.010***
	(0.002)	(0.002)	(0.002)
Effect of exposure, Land=0		0.006	0.007
		(0.006)	(0.004)
Effect of exposure, Land=1		0.019***	0.023***
		(0.005)	(0.006)
Observations	301664	301664	301664
p-value, equality of effect by land cover		.109	.025
Outcome mean post-2004, no exposure	0.028	0.028	0.028
Country-Year FE	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: The table presents results from three separate regressions of average long-term impacts of locust swarm exposure on the probability of any violent conflict event in a cell-year using the BJS estimator. In columns 2 and 3 I estimate heterogeneous effects by land cover in 2000, considering whether there is any crop or pasture land and any crop land, respectively. The 'Land=0' row shows the estimated average long-term effects in cells with no such land cover while the 'Land=1' row shows the impact in cells with such land cover. At the bottom of the table I include p-values for the test of equality of these two coefficients. The outcome mean for control cells is shown for post-2004 for comparison with exposure impacts in the period after the majority of swarm exposure occurred. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

staggered timing of swarm exposure, potentially because close to three-quarters of exposure occurred in the same period in 2003-2005. I find slightly larger differences in average long-term effects of swarm exposure by land cover, and the difference by any agricultural land becomes statistically significant at a 95% confidence level. The TWFE regressions also allow me to test heterogeneity in effects of precipitation, temperature, and population (Table B2 columns 2 and 3), which is not possible with the Stata package implementing the BJS estimator. Effects of precipitation are marginally significantly larger in cells with any crop land but remain significant in non-agricultural cells. Effects of temperature do not vary by land cover. These results echo previous work questioning whether agricultural mechanisms explain the relationship between climate and conflict (Bollfrass and Shaver, 2015; Sarsons, 2015). The association between population and conflict risk does not vary significantly with land cover. The estimated effect in non-agricultural cells is very large but there is little identifying variation driving this estimate.

To further test that impacts of locust swarms are driven by initial impacts on agricultural production, I consider heterogeneity in impacts by both land cover and by timing of swarm exposure relative to local crop calendars. I categorize swarms as arriving during particular stages of the crop production cycle by matching the month in which a swarm is observed to crop calendar information from the PRIO-GRID dataset, filling in missing data with country-level crop calendars from The United States Department of Agriculture (USDA) (2022).The off season—between harvesting and planting—lasts between 3 and 6 months in most of the sample cells, with an average of slightly over 4 months. I distinguish between swarms arriving during the off season and planting season (first two months of the main agricultural season) when they are unlikely to significantly damage crop production, and swarms arriving in the growing and harvesting season when potential damages should be greatest. Figure A1 presents the timing of locust swarms by region across the sample period. Swarms in cells with crop land are more frequently observed during the growing/harvest season, likely because locust breeding primarily occurs during wetter months which tend to overlap with the planting period and swarm migration out of breeding areas follows in subsequent months.

As with the main analysis, I use the first year a cell was exposed to a locust swarm during a particular season to construct an absorbing treatment variable. I then estimate Equation 1 with the two seasonal swarm exposure treatments using TWFE and fully interact all variables with dummies for any agricultural or crop land cover. Table 2 column 1 shows that effects on violent conflict risk are driven by exposure to locusts swarms during the growing or harvest season in cells with any agricultural land, where exposure increases average annual risk of any violent conflict event by 2.6pp. There are no significant effects of swarm exposure in other seasons or non-agricultural cells. The patterns are similar when considered differences by any crop land in particular (column 2).

Table 2: Impacts of swarm exposure by land cover and swarm timing

Outcome: Any violent conflict event	(1) Land = Any crop or pasture land	(2) Land = Any crop land
Off/planting season, Land=0	-0.001	0.001
	(0.005)	(0.006)
Off/planting season, Land=1	0.007	0.009
	(0.006)	(0.007)
Grow/harvest season, Land=0	0.007	0.012
	(0.011)	(0.007)
Grow/harvest season, Land=1	0.026***	0.028***
	(0.007)	(0.008)
Observations	301664	301664
Outcome mean post-2004, no exposure	0.028	0.028
p, off/plant season non-crop=crop effect	0.321	0.288
p, grow/harvest season non-crop=crop effect	0.161	0.127
p, non-crop off/plant=grow/harvest effect	0.536	0.254
p, crop off/plant=grow/harvest effect	0.059	0.087
Country-Year FE	Yes	Yes
Cell FE	Yes	Yes
Controls	Yes	Yes

Note: The table presents results from a single regression interacting two seasonal swarm exposure treatment variables with a dummy for crop land cover with the same fixed effects and controls as in Equation 1. The coefficients and standard errors are calculated using Stata's *xlincom* command based on the sums of the coefficients for the non-crop seasonal effects and the crop interaction terms. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

For both land cover types (any agricultural land and any crop land specifically), I can reject equality of effects of swarm exposure in cells with such land cover by whether the exposure occurred in the off/planting seasons compared to the growing/harvest seasons ($p = 0.059$ and $p = 0.087$, respectively for the two land cover dummies). Positive point estimates for effects of off/planting season swarms in such cells indicates that in some cases there are impacts of swarm exposure through destruction of vegetation even outside of peak crop production periods. This could include destruction of pasture, of perennial crops such as tree crops, and of early growth of annual crops. What we might consider 'placebo' swarms in non-agriculture or non-crop cells during the off or planting seasons have an estimated effect close to 0, in line with expectations. Point estimates of the effect of growing or harvest season swarms in non-agriculture and non-crop cells are positive and close to being marginally significant in the case of non-crop cells ($p = 0.131$). I cannot reject equality of effects by land cover of growing/harvest season exposure ($p = 0.161$ and $p = 0.127$, respectively for the two land cover dummies), suggesting effects through destruction of pasture or other vegetation (pasture is present in nearly half of non-crop cells).

This finding adds to other studies showing that the impact of weather shocks on conflict risk varies depending on whether the timing of the shock is such that it is likely to decrease agricultural productivity (Caruso et al., 2016; Crost et al., 2018; Harari and La Ferrara, 2018). The results in Table 2 also increase confidence that the estimated effects represent true causal impacts of swarm exposure and not bias due to selection in where swarms are reported.

6.2 Robustness

Event study results are similar when varying the fixed effects and controls included in the estimation (Figure B4). Estimates are nearly unchanged when dropping the population, weather, and distance to capital by year controls and when adding estimated swarm exposure propensity-by-year fixed effects. Replacing country-by-year fixed effects with year or sub-national region-by-year fixed effects leads to generally more positive (but still not statistically significant) pre-exposure period coefficients and very similar post-exposure dynamic treatment effects.

In line with these results, average long-term impacts are also similar when varying fixed effects and controls (Figure B5). In addition to the specifications shown in Figure B4, I also vary which of the main controls are included, add weather lags, use alternative measures of precipitation and temperature from CHIRPS (Funk et al., 2015) and ERA5 (Hersbach et al., 2019) respectively, and add agricultural land-by-year fixed effects. The smallest estimates are with the same controls but year rather than country-by-year fixed effects (a 1.3pp increase in violent conflict risk) and when adding exposure propensity-by-year fixed effects (a 1.4pp increase). I find a 1.9pp increase in violent conflict risk when using sub-national region-by-year fixed effects to identify impacts off of more local variation in swarm exposure. In all cases the estimated long-term average impact of swarm exposure on violent conflict remains large and statistically significant, and in no case can I reject that the estimate under the alternative specification is the same as the main specification.

The main specification includes all years from 1997-2018 and excludes cells more than 100 km from any swarm report and outside the range of common support of estimated swarm exposure probability. Dropping individual years when locust swarm exposure events occurred does not affect the estimates, though the estimate is much noisier when dropping the main 2004 exposure event (Figure B6 Panel B). This indicates that the overall estimates are not driven by any particular swarm exposure events. The results are similar when including the full sample of cells and when dropping various geographic regions (Figure B6 Panel A). This addresses concerns that the long-term impacts on violent conflict may be spurious and due to swarm exposure during the sample period being correlated with factors driving later conflict emergence. For example, dropping North Africa ensures that results are not driven by the Arab Spring and dropping Arabia ensures results are not driven by the civil war in Yemen.

The heterogeneity in effects of swarm exposure by timing and land cover indicate that the estimated impacts are not driven by potential bias in where locust swarms are reported. But measurement error in locust swarms may still create bias, and other studies of impacts of swarm exposure have pointed to concerns raised by Showler and Lecoq (2021)—SL2021—that swarms may be particularly underreported in insecure areas (Gantois et al., 2024; Torngren Wartin, 2018). I find that average long-term effects of swarm exposure are slightly smaller in magnitude (a 1.5pp increase in violent conflict risk) when dropping countries where SL2021 indicate insecurity has limited locust control operations, and when dropping cells that experienced violent conflict during the 2003-2005 locust upsurge which might have prevented swarm reporting (Figure B6 Panel A). If violent conflict were driving important levels of swarm non-reporting we might have expected larger effects of swarm exposure when dropping these countries and cells by reducing the share of cells incorrectly classified as not exposed to any swarm. Instead, the smaller effects reflect lower average change in violent conflict in the rest of the sample relative to these more insecure areas.

The similar results in these samples imply that bias due to conflict-driven underreporting of locust swarm exposure during the study period is unlikely to be a meaningful factor in the analysis. I further test the potential for missing swarm observations to affect the estimates by simulating how the results change as I increase the share of cell-years where potential 'missing' locust swarms are imputed within 100 km of a locust swarm report. I first impute hypothetical 'missing' swarms in the cells most likely to have been exposed. For this, I impute swarms in all cells with a high estimated propensity of locust exposure in any year that another locust swarm observed within 100 km. The event study results are very similar even if I assign all cells with an estimated propensity of exposure above 0.25 to have been exposed in the first year a swarm is reported nearby (Figure B7).

Next, I impute swarms in areas experiencing violent conflict near existing swarm observations, where SL2021 suggest they may be likely to be unreported. This type of measurement error should bias my estimates downward as conflict is serially correlated, and I confirm that estimated effects of swarm exposure increase as I impute more 'missing' swarms in such areas (Figure B8 Panel A).

I then randomly impute hypothetical 'missing' swarms across all cell-years with nearby swarms reported. Estimated average long-term effects of swarm exposure on violent conflict risk fall as I impute swarms in a larger share of cell-years around where swarms are reported (Figure B8 Panel B). This could indicate that swarms are more likely to be reported in locations with higher long-term conflict risk, but is also consistent with attenuation from random error in the treatment definition. Although I cannot distinguish these explanations, the results are useful in bounding the potential effect of locust swarm exposure. Even if I randomly assign 20% of cell-years near a swarm report to be exposed to locusts, the estimated effect of swarm exposure remains economically and statistically significant, with a mean 0.6pp increase in violent conflict risk that is significant at the 95% level in

69% of simulations (Figure B8 Panel C). These results strongly imply that the estimated effects of locust swarm exposure are not driven by selection in where swarms are reported.

Next, I consider differences in estimated effects when aggregating the sample to larger grid cells. Using larger grid cells addresses several potential measurement issues. First, it minimizes the possibility that the area exposed to a locust swarm recorded in a cell exceeds the boundaries of the cell. Second, it reduces concerns about nearby areas that might have been affected by unreported swarms since the entire cell is considered exposed if any swarm is reported within it. Third, it limits the potential for conflict spillovers outside the cell. Downsides to analyzing impacts in a more coarse grid are dilution of treatment intensity (as the share of the cell affected by swarms weakly decreases with cell size) and the loss of quasi-random local variation in swarm exposure which is central to the identification approach. The latter weakness implies that results at more aggregated cell sizes should be interpreted with caution.

I observe similar patterns in dynamic impacts of swarm exposure over time when collapsing the data to 0.5° or 1° cells (Figure B9). Impacts in post-exposure periods follow a similar patterns as in Figure 3 but are larger in magnitude. Pre-exposure coefficients are more positive and generally larger in magnitude and some are marginally statistically significant. Looking at average long-term impacts, absolute effects are larger when collapsing the data to the level of 0.5 or 1° cells, but effects relative to the probability of any violent conflict in unexposed cells are larger in 0.25° cells (Table B3). This is consistent with higher probabilities of any violent conflict as cell size increases, and with less dilution of the swarm exposure treatment in smaller cells.

Similar estimated magnitudes of swarm exposure impacts when using larger grid cells despite dilution of treatment intensity indicate potential spillovers of violent conflict outside exposed cells. Such spillovers are predicted by a model with spatially correlated agricultural shocks, as the returns to fighting over output will be larger in areas not affected by the shock. For example, McGuirk and Nunn (2025) find that drought in pastoral areas leads to conflict spillovers in nearby agricultural areas. Agricultural destruction due to desert locust swarms is both particularly severe and less spatially correlated than other agricultural shocks, due to swarm flight patterns. This implies both a sharp decrease in the opportunity cost of fighting for agricultural producers in affected areas and potentially more localized spillovers of this conflict. The main analysis considers $\sim 28 \times 28$km grid cells where the median locust swarm would only affect around 6% of cell area. Conflict incited by locust destruction may be more likely to be realized within a grid cell, as the opportunity costs and rapacity mechanisms are less likely to offset each other, but large absolute effects in larger grid cells suggests some conflict spillovers may yet occur.

To test for this directly I define a spillover exposure treatment as being within 100 km of a swarm outside of the cell and estimate effects of this treatment alongside direct exposure using the TWFE estimator. I find a fairly precise null average effect of this spillover treatment when controlling for direct swarm exposure, though spillovers from the 2003-2005 upsurge are marginally significant and indicate a 1.1pp average long-term increase in conflict risk for cells within 100 km of a swarm during that upsurge (Table B4). Focusing on impacts within 0.25° cells may therefore understate the full effect of swarm exposure on violent conflict risk around affected areas, but the results indicate that most effects are contained within cells.

7 Mechanisms

Swarm exposure may affect future violent conflict risk through a variety of channels. A focus on the long-term average increase in violent conflict might suggest hysteresis as an explanation as conflict is highly autocorrelated over time. But the dynamic patterns rule this out as primary driver of the impacts of swarm exposure, as there are no immediate effects that could lead to persistent long-term insecurity.

In this section I focus on testing effects through agricultural productivity or permanent income presented in section 3 An exploration of all potential mechanisms, such as increases in local inequality, changed perceptions of land productivity and agricultural risk, or effects on trust in the state, in neighbors, or in religion, is outside the scope of the present study. The focus on testing productivity- or income-related mechanisms does not rule out a potential role for such alternative mechanisms.

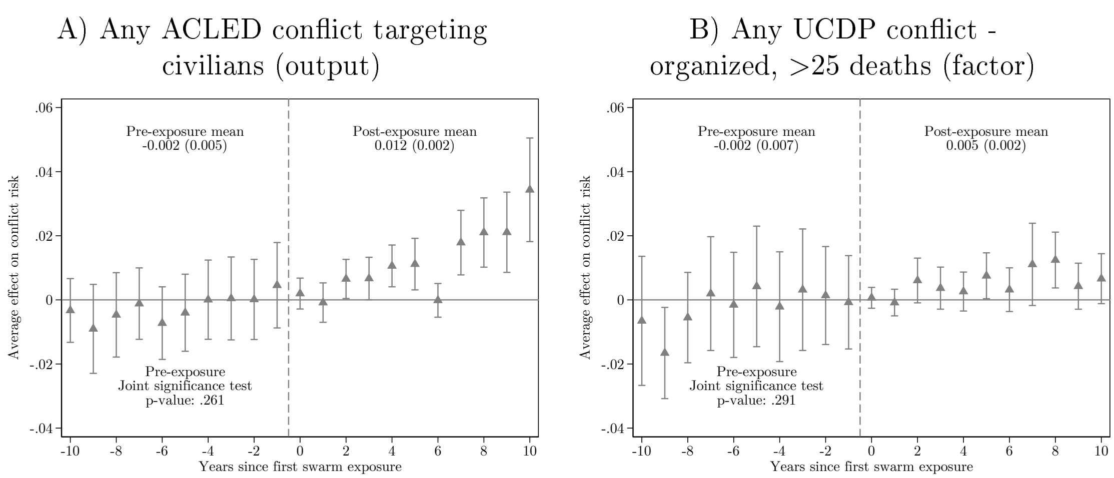
7.1 Opportunity cost vs. rapacity in explaining null immediate effects

An opportunity cost mechanism alone, acting through immediate direct effects of swarm exposure on agricultural productivity, would suggest immediate impacts of swarm exposure on violent conflict (prediction 1 from Section 3). Severe negative effects of locust swarms on agricultural output are well-documented (Green, 2022; Newsom et al., 2021; Showler, 2019; Symmons and Cressman, 2001; Thomson and Miers, 2002). Agricultural producers whose productivity falls should be immediately more likely to switch to engaging in violent conflict, if they do not have a stronger outside livelihood option. This prediction is rejected by the results: there are null effects of locust swarms on violent conflict in the year of exposure and the following year. This implies that decreases in opportunity costs of fighting are either not sufficient to motivate a switch in occupation to fighting, that affected populations primarily turn to other livelihood alternatives, or that another mechanism offsets the opportunity cost mechanism.

One possibility is that the rapacity mechanism offsets the opportunity cost mechanism in the short term. Other studies of agricultural shocks have shown instances where the rapacity mechanism outweighs the opportunity cost mechanism (Berman et al., 2017; Koren, 2018; McGuirk and Burke, 2020; Ubilava, 2024; Ubilava et al., 2022). If these mechanisms are offsetting, we would expect smaller short-term effects for conflict over output—reduced by the agricultural production shock and therefore decreasing returns to predatory attacks—than over factors of production, whose returns are not directly affected by the transitory shock (prediction 2). I follow McGuirk and Burke (2020) in defining reports of violence against civilians, riots, and looting from ACLED as more likely to represent conflict over output and violent conflict events reported in the UCDP database as more likely to represent conflict over factors of production. Violence against civilians and rioting is unlikely to target capture of land or other factors of production and regularly involves banditry and theft though it may also have objectives other than capture of output. Violent conflict events recorded in the UCDP database must involve two organized actors and at least 25 battle-related deaths; these conditions are unlikely to occur outside of situations where groups are contesting control over territory and therefore of its factors of production.

Figure 4 presents event studies for the effects of swarm exposure on these measures of output and factor conflict. I find null effects on swarms on conflict risk in the year of exposure and the following year for both conflict types, with point estimates close to 0. Both conflict types show no evidence of significant pre-exposure trends, though the coefficient on the period 9 years before exposure is negative and significant for UCDP conflict. Effects of swarm exposure on UCDP conflict are less consistently statistically significant. Besides the periods 0, 1, and 6 years after exposure, all estimated effects of swarm exposure on ACLED output conflict are statistically significant at a 95% confidence level or greater. This level of confidence is only reached for 2 of 11 estimates of post-exposure effects of swarms on UCDP conflict, with another 3 coefficients significant at a 90% confidence level (Table A3 columns 2 and 3).

Figure 4: Impacts of swarm exposure on conflict risk over time, by conflict type



Note: The dependent variables are dummies for any conflict event being observed in a cell in a year, with the conflict type specified in the panel title. Each panel replicates Figure 3 for a different conflict outcome. See the figure note for Figure 3 for more detail.

Contrary to prediction 2, if anything the point estimates for periods 0 and 1 immediately following swarm exposure are slightly larger (though statistically indistinguishable) for output compared to factor conflict. Decreased returns to predatory attacks therefore do not appear to explain the null short-term effects of swarm exposure on violent conflict.

Larger long-term effects on a measure of output conflict than one of factor conflict (Table A4) implies much of the increase in violent conflict following swarm exposure stems from banditry, looting, terrorism, and other attacks on civilians rather than civil conflict over control of territory. This type of violence provides the livelihood for many armed groups engaged in civil conflict. Locust swarm exposure does not increase the returns to such rapacity, so persistent increases in output conflict after the immediate period of exposure must be driven by other factors.

7.2 Effects of swarm exposure on agricultural and economic activity

Long-term average increases in conflict risk could be explained by persistently reduced opportunity costs of fighting through a permanent income mechanism (prediction 3), even though null immediate effects are not consistent with an opportunity cost mechanism alone. Effects of swarm exposure on the opportunity cost of fighting through the immediate productivity shock or through a permanent income mechanism following efforts to cope with the initial shock should be observable on measures of productivity in affected areas (prediction 5). Severe negative effects of locust swarms on agricultural output are well-documented (Green, 2022; Newsom et al., 2021; Showler, 2019; Symmons and Cressman, 2001; Thomson and Miers, 2002). While an immediate decrease in agricultural productivity and thus in the opportunity cost of fighting in affected areas seems incontrovertible, there is less evidence on the longer-term effects of desert locusts on productivity.

I present results of tests of average long-term effects of swarm exposure on various measures related to local economic activity in Table 3. I first test for effects on measures of agricultural productivity, considering the annual maximum of the cell-level average monthly Normalized Difference Vegetation Index (NDVI), the maximum annual cell-level yield across major crops estimated via remote sensing (Cao et al., 2025), and mean crop yield across DHS survey locations in each cell (IFPRI 2020).

Table 3: Average impacts of locust swarm exposure on indicators of economic activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Max cell annual NDVI	Mean cell estimated main crop yield (kg/ha)	Cluster avg crop yield (kg/ha)	Cluster total crop prodn area (ha)	Cluster total crop prodn (metric tons)	Cluster tropical livestock units per km^2	Estimated net migration (per 1000 ppl)
Average effect of swarm exposure	-0.000	35.9	54.9	-116.7***	-316.7	-0.897	-4.691
	(0.002)	(37.7)	(78.7)	(44.1)	(218.4)	(2.304)	(3.391)
Observations	258346	50144	3912	4075	4075	4075	249645
Outcome control mean	0.242	2694.7	3567.8	2308.6	12751.5	41.352	-0.494
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents results from separate regressions of swarm exposure on different outcomes, following Equation 1. Differences in sample sizes are due to differences in data availability across outcomes. Crop-specific yield estimates are only available in cells where that crop is produced. Data on DHS survey clusters are only available in cell-years with any completed DHS surveys within the cell. NDVI is calculated from MODIS satellite imagery (Didan, 2015) as the maximum of monthly average NDVI values in each cell for 2000-2018. Crop-specific yield data are from Cao et al. (2025)) for 1997-2015, where the 'main' crop in cells with multiple crops is defined as the highest-yield crop in the cell. DHS cluster data are from the DHS AReNA database for 1997-2018 (IFPRI 2020) and represent average values within DHS clusters at the time surveys were conducted. Annual net cell migration for 2000-2018 is from Niva et al. (2023). All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

On average, locust swarm exposure has no significant effect on the maximum of average monthly cell NDVI in subsequent years and the point estimate is a fairly precise 0, indicating peak greenness is not changing at the cell level. While I find significant decreases in NDVI in the years in which locust swarms are reported and the following year (Figure A2 Panel A), these are similar in magnitude to differences in the years before exposure and the effects do not persist. Unexpectedly, I also find significant *increases* in maximum NDVI in two post-exposure periods, though no other post-exposure estimates are positive.

I also find no average long-term effects of swarm exposure on measures of crop yield estimated by Cao et al. (2025) via machine learning combining administrative statistics and remotely sensed data, or in the periods and locations where DHS surveys have been conducted (IFPRI 2020). Estimated differences in remotely-sensed crop yield in the periods before swarm exposure are positive but very noisy and are also positive but uniformly smaller post-exposure, and no estimated effects are statistically significant (Figure A2 Panel B). These null long-term effects on measures of agricultural productivity are consistent with locust swarms—a migratory pest—being a transitory shock that does not affect agricultural productivity fundamentals. They also indicate that to the extent swarm exposure may affect later labor productivity in affected areas this is not reflected in measures of NDVI or crop yields at the cell level.

While swarm exposure has no long-term effect on measures of agricultural productivity, it does significantly decrease total crop area planted in DHS survey clusters. Cluster crop production area falls by 117 ha (5.0%) on average in the years following swarm exposure. While there is no significant effect on total cluster crop production quantity, the point estimate is large and negative, in line with the finding of no significant effect of swarm exposure on mean survey cluster crop yield (Table 3 columns 3-5). The decrease in crop production could indicate a transition away from agriculture in exposed areas. I do not find any impact of locust swarms on density of livestock ownership (measured in Tropical Livestock Units), but the DHS AReNA database includes limited information on other measures of household wealth which could be used to test the hypothesis that coping with swarm exposure persistently decreases wealth.

Finally, I consider effects on migration. Leaving to search for work is a common response to locust crop destruction (Thomson and Miers, 2002) and over 8 million people were displaced across East Africa as a result of the 2019-2021 locust outbreak (The World Bank, 2020). Ghorpade (2024) finds that locust swarm exposure increases stated willingness to migrate of rural individuals in Yemen by 12 percentage points, driven by agricultural households. Using data from Niva et al. (2023), I find that swarm exposure does not significantly affect net annual migration in subsequent years. The point estimate is large, indicating 4.7 more people per 1000 population migrating out of exposed areas per year on average, suggesting that areas directly affected by locust swarms may indeed respond with more out-migration despite null average cell-level effects. There are no significant pre-treatment differences in estimated net migration (though standard errors are large) and no immediate effects of swarm exposure, but 8 of 11 treatment effects are negative and 3 of these are marginally statistically significant (Figure A2 Panel C). Persistent out-migration from affected areas could be consistent with lower labor productivity.

Taken together, the results on the effects of swarms exposure on economic outcomes at the grid cell level in Table 3 do provide limited evidence of a possible permanent income mechanism. NDVI and crop yields do not decrease significantly, indicating agricultural productivity is not reduced in years following a locust swarm on average. Livestock ownership is also not significantly lower, which does not align with a permanent income mechanism based on an initial income shock depleting household assets. On the other hand, significant decreases in crop production area and suggestive increases in out-migration suggest some households leave agriculture in favor of migrating or other activities, in line with reductions in agricultural productivity prompting a shift in occupation. If migration is an attractive alternative to agricultural production for households in areas exposed to locust swarms it may be the case that engaging in violent conflict would also be attractive in some circumstances, particularly in periods of heightened grievance.

One possible reason for the non-significant average effects of locust exposure on most outcomes in Table 3 is that these intent-to-treat estimators include too small a share of treated areas. The median locust swarm covers around 50 km^2, or 6% of the area of a 0.25 degree cell. Most DHS clusters in cells exposed to locust swarms will likely not have experienced any agricultural destruction, so the intent to treat effects I estimate will be attenuated toward 0. Taking average NDVI or remotely-sensed crop yield values or estimated net migration over the entire cell will also attenuate any impacts in areas actually affected. Analyzing impacts of swarm exposure on violent conflict at the grid cell level reduces measurement error from uncertainty in the exact areas exposed to locust swarms and in where conflict events occur, and reduces concerns about spillover conflict realized in the area surrounding affected populations rather than in their particular location. This grid cell-level approach is less likely to capture economic impacts of swarm exposure which are likely more concentrated in directly affected areas. More targeted intent to treat analyses focusing on economic impacts of locust swarms only in the close vicinity of the swarm reports would be more likely to detect effects, though raise difficulties in determining how to define exposed areas.

A growing body of evidence uses different approaches to define community-level swarm exposure and link these to survey data, and finds persistent effects of swarm exposure on outcomes that could imply reduced labor productivity. Most directly, Marending and Tripodi (2022) find that agricultural profits of households in parts of Ethiopia exposed to locust swarms in 2014 are 20-48% lower two harvest seasons after swarm arrival, driven by a large drop in farm revenues. This indicates that impacts on agricultural productivity are not limited to the year of swarm exposure. Indirectly, several studies show that young children exposed to locust swarms achieve lower educational attainment (Asare et al., 2023; De Vreyer et al., 2015) and have lower height-for-age (Conte et al., 2023; Gantois et al., 2024; Le and Nguyen, 2022; Linnros, 2017) when they are older. Such human capital effects of swarm exposure could decrease permanent labor productivity. Additional work is needed to further test predictions of the permanent income and opportunity cost mechanisms but conducting such analyses is beyond the scope of this paper.

Exposure to locust swarms may increase vulnerability to future grievances through channels other than the permanent income mechanism presented in Section 3. Desert locust swarms are localized natural disasters with concentrated effects on agricultural production in only part of each 0.25° cell, increasing within-cell inequality which may create discontent and cause conflict (Gurr, 2015). Swarm exposure could also have psychological effects on affected populations, as documented in other studies of the climate-conflict relationship (see Burke et al. (2024) for a review). Locust control is seen by many households as the responsibility of the state (Thomson and Miers, 2002). Exposure to a locust swarm may therefore decrease trust in the state and foster or add to a sense of grievance, particularly as many countries affected by locust upsurges had limited capacity to offer relief to affected areas. Effects of locust swarms on religiosity may also be important. The dominant religion in the sample countries is Islam, where locusts are mentioned as a punishment from Allah and as a metaphor for Judgment Day, and they have similar connotations in Christianity.[15] If locust swarms increase religiosity in exposed areas this may affect the perceived returns to fighting by increasing social, emotional, and supernatural costs, suppressing immediate violent conflict. Increased religiosity could also help explain higher conflict incidence in exposed areas following the onset of various civil conflicts (religion played an important role in the protests and conflicts during and following the Arab Spring) and Islamic terror movements in the sample countries. Such mechanisms could be tested empirically by analyzing differences in impacts of swarm exposure on conflict by measures of proximity to governing groups and religious identity across cells, and by considering how swarm exposure affects measures of government trust or support and of religiosity over time.

7.3 Explaining dynamic impacts of swarm exposure on conflict

The pattern of dynamic long-term impacts of locust swarm exposure on violent conflict shown in Figure 3 is not intuitive. Why are the largest impacts delayed, particularly given the null immediate effects? The opportunity cost mechanism alone would suggest immediate impacts of swarm exposure on violent conflict (prediction 1 from section 3), and that if they persist through persistent effect on permanent income impacts should either fall over time as affected areas recover or be fairly stable if households reach a new productivity equilibrium (prediction 4). Instead, these predictions are rejected by the results, indicating some mechanism creating heterogeneity in dynamic impacts of swarm exposure.

One potential source of heterogeneity could be exposure to subsequent economic shocks. These could further lower the opportunity cost of fighting in swarm-exposed areas and cause violent conflict if the effects from the initial locust shock was not sufficiently severe. I test for this possibility by estimating heterogeneity in impacts of swarm exposure by whether a country is experiencing a famine and whether a cell is experiencing a severe drought in a given year (Table 4). Swarm exposure has a

[15] Verse 7:133 of the Quran says "So We plagued them with floods, locusts, lice, frogs, and blood—all as clear signs, but they persisted in arrogance and were a wicked people" in describing the punishment of Ancient Egypt. The same punishments are described in Exodus 10 of the Bible, where verses 14-15 say "They invaded all Egypt and settled down in every area of the country in great numbers. Never before had there been such a plague of locusts, nor will there ever be again. They covered all the ground until it was black. They devoured all that was left after the hail-everything growing in the fields and the fruit on the trees. Nothing green remained on tree or plant in all the land of Egypt." Verse 54:7 of the Quran says "They will emerge from the graves as if they were scattered locusts with their eyes cast down" in describing the resurrection of the dead on the Day of Judgment. In the Bible, Revelations 9:1-10 describes the coming of a plague of locusts to torture "only those people who did not have the sign of God on their foreheads" during the period of Judgment Day.

significantly larger impact on violent conflict in countries experiencing famine, but the average effect outside these contexts is similar to the overall average effect (column 1). On the other hand, there is no significant difference in the effect of swarm exposure by whether a cell experienced drought either in the current or previous year (columns 2 and 3). I cannot test heterogeneity in impacts by subsequent exposure to locust swarms as control cells by definition have no exposure during the sample period, but I find no heterogeneity in impacts by exposure prior to 1990 which could have suggested either adaptation to or compounding effects of repeated swarm exposure (column 4). These results indicate that subsequent economic shocks cannot explain the patterns of dynamic impacts of swarm exposure.

Table 4: Heterogeneity in impacts of exposure to locust swarms on violent conflict risk by exposure to other shocks

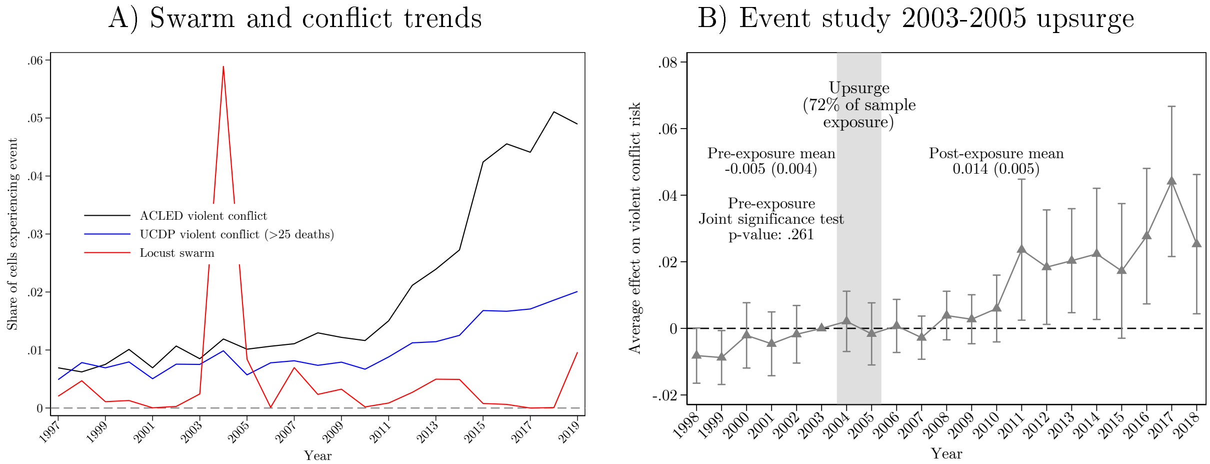
Outcome: Any violent conflict event	(1) Any active famine in country	(2) Any active drought in cell	(3) Any drought prev. year in cell	(4) Any swarm before 1990 in cell
Effect of exposure, No shock	0.016***	0.011***	0.018***	0.017***
	(0.004)	(0.003)	(0.004)	(0.004)
Effect of exposure, Any shock	0.060***	0.015**	0.023***	0.019***
	(0.017)	(0.007)	(0.008)	(0.006)
Observations	301664	294353	295564	301664
p-value, test of equality of coefficients	0.010	0.511	0.527	0.626
Country-Year FE	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: The table presents results from separate regressions where I estimate heterogeneous average long-term impacts of locust swarm exposure on the probability of any violent conflict event in a cell-year by indicators of other shocks using the BJS estimator and built-in heterogeneity option in the Stata implementation of the estimator. The famine indicator in column 1 is defined at the country-year level by whether any famine was declared. The drought indicators in columns 2-3 are cell-year level variables, defined by whether there are at least 4 consecutive months where the SPEI is below -1.5. Swarm exposure prior to 1990 in column 4 considers exposure in the period from 1985-1989 where there were several major locust upsurges not considered in the definition of swarm exposure in the study sample period. At the bottom of the table I include p-values for the test of equality of the coefficients representing effects by the presence of civil conflict. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

An important observation is that the gap between swarm exposure and the largest impacts on violent conflict risks corresponds to the gap between the timing of the main swarm exposure event in the sample period—the 2003-2005 upsurge—and the years when the general risk of conflict increased across the sample countries due to the Arab Spring, the spread of Islamic terrorist groups, and multiple civil wars, as shown in Figure 5 Panel A. Panel B shows that exposure to this upsurge did not significantly increase the risk of violent conflict until 2011, the year of several uprisings related to the Arab Spring, but that effects remain large and statistically significant in subsequent years. I find similar patterns when looking at effects of the upsurge in different countries with different events precipitating the spread of violent conflict (Figure B10).

As the 2003-2005 locust upsurge accounts for 72% of swarm exposure in the sample period, its dynamic effects drive the main event study results including all swarm exposure events. Figures 3 and 5 show clearly that swarm exposure does not generally cause the immediate onset of new violent conflicts. Instead, exposed areas appear to be more vulnerable or susceptible to be engaged

Figure 5: Changes in conflict environment and impacts of exposure to 2003-2005 locust upsurge on violent conflict risk



Note: Panel A shows the share of sample cells experiencing any locust swarm or conflict event by year. Panel B shows results for an event study of exposure to the 2003-2005 desert locust upsurge, with 2003 as the reference period, with the same fixed effects and controls specified in Equation 1. The dependent variable is a dummy for any violent conflict event, and treatment is defined as any locust swarm observation from 2003-2005. The bars represent 95% confidence intervals using SEs clustered at the sub-national region level. Observations are grid cells approximately 28×28km by year.

in later violent conflict precipitated by other factors, implying mechanisms related to the returns to engaging in conflict and not just the opportunity cost of fighting.

Prediction 6 of the model provides a potential explanation: variation in local grievances, as reflected in episodes of civil unrest, should create heterogeneity in the dynamic impacts of swarm exposure. Null short term effects may reflect relatively peaceful conditions at the time of the main locust exposure events, limiting the feasibility of fighting and the potential net returns. The largest impacts of swarm exposure are realized in a period characterized by multiple popular uprisings, civil wars, and outbreaks of Islamic militancy. Significant increases 2-6 years after exposure in the main event study compared to null effects over this period for the 2003-2005 upsurge are consistent with later swarm exposure events occurring closer to or during periods when heightened grievances manifest in civil conflict.

I do not measure grievances directly, so instead I rely on situations of observable civil conflict as proxies of contexts or almost certain heightened underlying grievances. To formally test prediction 6 I consider whether there is heterogeneity in average long-term impacts of swarm exposure by various indicators of civil conflict or insecurity. At the cell-year level, I measure whether there are any concurrent violent conflict events in surrounding cells, either in the encompassing 1° cell, in the surrounding sub-national region, or in the rest of the country outside the surrounding sub-national region. These measures rely on the ACLED data and do not take into consideration any particular causes of the conflict.

At the country-year level, I identify a variety of situations which led to increases in civil conflict and insecurity in the following years during the sample period. For each of these, I define a country as being in an insecure situation in all years after the year in which the particular situation began.

I first consider the Arab Spring specifically, which began in late 2010 or early 2011 in a number of sample countries. For example, civil conflict in Libya began to increase in 2011 with the start of the Arab Spring and then continued with the subsequent civil war. Egypt underwent a revolution in 2011 as part of the Arab Spring and experienced a coup d'etat in 2013 and continued higher levels of insecurity. I then look at cases of revolutions and coups d'etat and of civil wars and separatist movements. For example, the 2013 coup d'etat in Egypt falls under the former category, and the civil war in Libya falls under the latter. Finally, I analyze the spread of (often Islamic) terrorist organizations across the sample countries. For example, armed Islamist violence escalated in Burkina Faso starting in 2016, with many attacks by Al Qaeda and IS affiliates that established control over many rural parts of the country. Sudan and Somalia are categorized as subject to Islamic terrorist organization for the entirety of the sample period.

Table 5 shows that average long-term impacts of locust swarm exposure on violent conflict are smaller and in some cases not statistically significant in locations and areas not characterized by some form of civil conflict, a proxy for heightened grievances. Consistent with prediction 6, effects of swarm exposure are significantly larger in all conflict situations, and the magnitudes of these differences are all quite large. The largest differences are by nearby violent conflict at the cell-year level. Past swarm exposure has no effect on violent conflict in locations and periods with no violent conflict elsewhere around the cell, in the sub-national region, or elsewhere in the country. But cells exposed to locust swarms are much more likely to experience violent conflict when it is occurring in surrounding areas. One possibility is that swarm exposure could be causing conflict within the cell that spills over into the rest of the region or country. This seems unlikely given limited evidence of conflict onset or spillovers from swarm exposure (Table B4,Figure 3). I also find similar heterogeneity when considering violent conflict in the country outside the cell's region, which is less likely to reflect spillovers from swarm exposure in the cell. The other interpretation is that violent conflict that emerges elsewhere is more likely to either target or engage areas previously exposed to locust swarms.

Turning to country-level absorbing indicators of civil conflict or insecurity, I find that average effects of swarm exposure are statistically and economically much larger in contexts experiencing a given type of civil conflict. The effect of swarm exposure outside of national civil conflict situations is about half the overall average impact (Table 1), and remains significant because countries not experiencing a given type of insecurity may be experiencing other types. Swarm exposure in countries affected by the Arab Spring protests and uprisings increases the likelihood of any violent conflict by 3.6pp. Exposure increases the likelihood of violent conflict by 2.9pp in countries having experienced a revolution or coup d'etat, by 4.0pp in countries having experienced a civil war or separatist movement, and 2.5pp in countries with active Islamic terror groups engaging in violent conflict.

The heterogeneity in locust swarm impacts by indicators of local civil conflict or insecurity could potentially be due to mechanisms other than grievances. First, insecurity may further decrease local labor productivity, decreasing the opportunity cost of fighting. If effects of the initial swarm exposure

Table 5: Heterogeneity in impacts of exposure to locust swarms on violent conflict risk by indicators of civil conflict

Outcome: Any violent conflict event	(1) Any violent conflict in surrounding 1 deg cell	(2) Any violent conflict in surrounding sub-region	(3) Any violent conflict in country outside sub-region	(4) Post-onset of Arab Spring in country	(5) Post-revolution or coup d'etat in country	(6) Post-civil war or separatist movement in country	(7) Post-onset of Islamic terror groups in country
Effect of exposure,	0.001	0.001	0.001	0.009***	0.008***	0.008***	0.010***
No civil conflict	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)
Effect of exposure, Any	0.098***	0.038***	0.020***	0.036***	0.029***	0.040***	0.025***
Any civil conflict	(0.013)	(0.008)	(0.005)	(0.011)	(0.008)	(0.011)	(0.007)
Observations	301664	301664	301664	301664	301664	301664	301664
p-value, test of equality of coefficients	0.000	0.000	0.000	0.022	0.009	0.004	0.058
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table presents results from separate regressions where I estimate heterogeneous average long-term impacts of locust swarm exposure on the probability of any violent conflict event in a cell-year by indicators of civil conflict using the BJS estimator and built-in heterogeneity option in the Stata implementation of the estimator. The civil conflict indicators in columns 1-3 are cell-year level variables, defined by the presence of any violent conflict event in the ACLED database for the stated areas around but outside a particular cell. The indicators in columns 4-7 are absorbing variables defined at the country level based on the timing that Arab Spring uprisings, revolutions or coups d'etat, civil wars or separatist movements, or Islamic terror attacks began in the country. Countries not exposed to such conflicts are coded as 0 for all years. At the bottom of the table I include p-values for the test of equality of the coefficients representing effects by the presence of civil conflict. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

are not sufficient to motivate affected individuals to choose to fight, an additional negative shock could provide the necessary push. In this case I would also expect impacts of swarm exposure to be larger in periods with other negative agricultural shocks. While effects are indeed larger in years when countries are experiencing a famine as shown in Table 4, there is no significant difference in effects by whether a cell is experiencing a drought.

Second, areas exposed to locust swarms may be less able to defend themselves from attacks and therefore be targeted by armed groups when these become active. The same mechanisms that would make exposed areas more vulnerable—persistent reductions in wealth—would also make them less attractive targets, however, so it is unclear how the expected returns to predation in these areas would change.

The other possibility relates to the returns to engaging in violent conflict, and how grievances may both reduce the costs of and increase expected benefits from fighting. Individuals in areas with lower opportunity costs of fighting following a severe prior agricultural shock may generally not find switching to fighting optimal as violent conflict is a costly and collective activity. Formation of armed groups and recruitment of fighters will be easier in areas of greater grievances reducing the social, emotional, and monetary costs of fighting. While swarm exposure may itself create lasting grievances, the local variation in exposure may prevent this from leading to general mobilization except in situations of broader grievances prompted by other factors or events, when fighting is more accessible or there are additional potential returns to conflict.

This heterogeneity in swarm impacts relates to other studies of heterogeneous effects of agricultural shocks on conflict risk. Buhaug et al. (2021) find that drought only causes the onset of civil conflict among ethnic groups experiencing recent political marginalization, an indicator of likely heightened grievances. Berman and Couttenier (2015) study the short-term effects of exogenous income shocks through international agricultural commodity prices on the risk on violent conflict in areas producing these commodities. They find that external income shocks act as threat multipliers affecting the "geography and intensity of ongoing conflicts" (p. 759), pointing to a likely opportunity cost mechanism Similarly, Bazzi and Blattman (2014) find that commodity price shocks primarily affect conflict incidence and not conflict onset.

The results on locust swarms further indicate that exposure to an agricultural shock can have delayed effects on conflict incidence. This relates to Narciso and Severgnini (2023)'s study of the impact of the Great Irish Famine in 1845-1850 on conflict during the Irish Revolution in 1916-1921. That study shows that individuals in families more affected by the famine were more likely to participate in the revolution and argues that impacts operate through persistent effects on grievances against British rule.

8 Estimating long-term impacts of transitory shocks

Locust swarms are a unique and catastrophic agricultural shock, but the model described in Section 3 is general and predicts similar patterns of impacts on the risk of violent conflict for other severe transitory shocks to agricultural production. In this section I first compare the impacts of locust swarm exposure to the impacts of exposure to drought, a type of shock that has been studied much more extensively, and consider whether the results point to similar mechanisms. I then briefly discuss the implications of the results for different approaches to estimating impacts of transitory economic shocks which may nevertheless have persistent effects.

8.1 Comparing locust swarms and drought

Many studies find short-term impacts of drought exposure on conflict risk,[16] but I am not aware of any considering dynamic impacts over time. To measure drought exposure I follow several of these papers in using the Standardized Precipitation and Evapotranspiration Index (SPEI) which combines both precipitation and the ability of the soil to retain water. The units of the SPEI are standard deviations from the historical average within a grid cell, where deviations within 1 are typically considered near normal conditions. I use monthly data from the SPEI Global Drought Monitor (Begueria et al., 2014) as compiled in the PRIO-GRID database. I define a cell as experiencing a drought shock in a particular year if there are at least 4 consecutive months where the SPEI is below -1.5, where this streak can include the last months of the previous year. This value is chosen to reduce the probability of multiple such drought exposures during the study period. A streak of at least 4 drought months is observed in 3.6% of all cell-years, compared to 7.7% for streaks of at least 3 months and 22.3% for streaks of at least 2 months.[17]

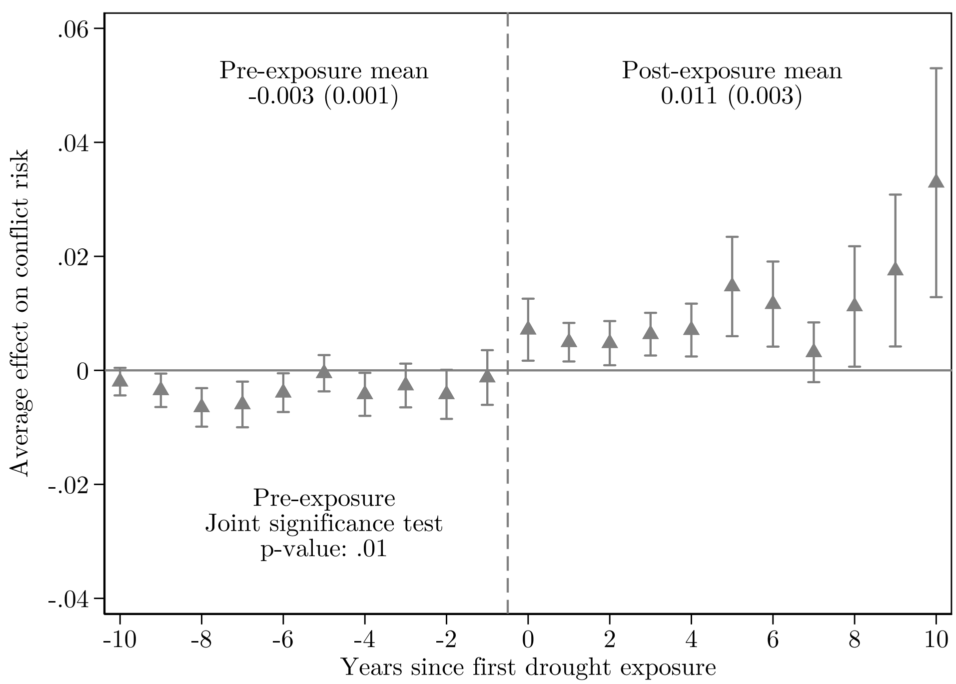
[16] See e.g., Buhaug et al. (2021), Couttenier and Soubeyran (2014), Harari and La Ferrara (2018), Jia (2014), Maystadt and Ecker (2014), McGuirk and Nunn (2025), Von Uexkull (2014), and Von Uexkull et al. (2016)

[17] Patterns of treatment effects are similar but standard errors are much larger if I use a threshold of 5 months but I find no significant effects with a threshold of 6 months (Table B5).

As with locust swarm exposure I identify the first year during the sample period in which a cell experiences a drought and consider cells to be 'affected' in all subsequent years. Across all sample cells, 48.6% experience at least one drought from 1996-2014. Nearly half (48.8%) of all exposure occurs in 2010 when around one-third of the study area was affected by drought, with no other year accounting for more than 8% of exposure.

Figure 6 shows the results from an event study of drought exposure. Pre-exposure coefficients are uniformly negative and small in magnitude but are statistically significant at a 95% confidence level in 6 of 10 pre-exposure periods. Standard errors for pre-exposure periods are smaller than in Figure 3 because nearly all exposed cells are observed for at least 10 years pre-exposure. This result indicates a slightly lower baseline risk of violent conflict in areas ever exposed to drought compared to those not yet or never exposed. But there is no evidence of changes in this difference over time before the first drought exposure in the sample period. Treatment effect estimates are positive for all post-exposure periods and are statistically significant for 10 of 11 periods. The average effect over the 10 years post-exposure is a 1.1pp increase in the annual risk of violent conflict.

Figure 6: Impacts of exposure to drought on violent conflict risk over time



The dependent variable is a dummy for any violent conflict event in a cell-year. The figure replicates Figure 3 but considering drought instead of locust swarm exposure. See the figure note for Figure 3 for more detail. Drought exposure is defined as ≥ 4 consecutive months in the year with SPEI<-1.5.

Violent conflict risk increases by a statistically significant 0.7 percentage points in the year of drought exposure. This significant increase, in contrast to null immediate effects of exposure to a desert locust swarm, suggests droughts cause the onset of some conflict. A delay in the largest impacts of drought exposure on violent conflict mirrors the pattern for locust swarm exposure; in the case of drought the largest effects occur 5 to 10 years after exposure. The main drought exposure event was in 2010, around the time that insecurity and violent conflict in the study area began to increase (Figure 5 Panel A), which may explain why drought causes conflict onset while locust swarms do not. Lags in the largest impacts of drought exposure are consistent with the timing of the largest increases in violent conflict.

Table 6 compares average long-term impacts of swarm and drought exposure. Columns 1-3 reproduce results for impacts of swarm exposure from Table 1 and Table 5. Column 4 shows that on average, drought exposure increases the risk of any violent conflict in a given year by 0.8pp (73%). This estimate is smaller than the average of the event study treatment period effects because treatment effects decrease more than 10 years after drought exposure, and are no long statistically significant starting 13 years afterward (Figure B11). The effect is also smaller than the impact of swarm exposure, potentially reflecting the greater intensity of agricultural destruction from locust swarms compared to drought. Column 5 shows that impacts of drought on violent conflict are concentrated in cells with any crop land, with an even sharper difference than for effects of locust swarms.

Column 6 tests for heterogeneity by activity of armed groups in the sub-region around exposed cells in a given year, an indicator of local civil conflict and proxy for heightened local grievances. I find a precise null effect of drought exposure in cell-years with no violent conflict in surrounding cells in contrast to a 1.8pp increase in the probability of violent conflict in cell-years with such insecurity nearby. This relates to findings from Buhaug et al. (2021) and Michelini (2025) that the short-term impact of drought on conflict is driven by a areas with marginalized ethnic groups (in Africa) and areas with higher predicted conflict risk outside of drought periods (globally), respectively. Buhaug et al. (2021) argue that drought acts like a trigger to transform preexisting grievances into violent conflict. The heterogeneity in impact of past shock exposure by nearby civil conflict is not as large for drought as for locust swarms, likely because the sample for the analysis of swarm exposure includes more high-conflict years from 2015-2018 that are not included in the drought analysis sample.

Table 6: Average impacts of exposure to agricultural shocks on violent conflict risk

Outcome: Any violent conflict event	(1) Swarms	(2) Swarms, Z = Any crop land	(3) Swarms, Z = Any conflict in region outside cell	(4) Drought	(5) Drought, Z = Any crop land	(6) Drought, Z = Any conflict in region outside cell
Average effect of shock exposure	0.018***			0.008***		
	(0.004)			(0.002)		
Effect of exposure, Z=0		0.007	0.001		-0.000	-0.000
		(0.004)	(0.001)		(0.001)	(0.001)
Effect of exposure, Z=1		0.023***	0.038***		0.020***	0.018***
		(0.006)	(0.008)		(0.005)	(0.003)
Observations	301664	301664	301664	452574	452559	452574
p-value, equality of effect by Z		.025	$< .001$		$< .001$	$< .001$
Outcome mean, no exposure	0.028	0.028	0.028	0.011	0.011	0.011
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reproduces results for impacts of swarm exposure from Table 1 and Table 5, and then presents the same analyses considering impacts of drought rather than swarm exposure. See the table notes for Table 1 and Table 5 for more detail. Drought exposure is defined as ≥4 consecutive months in the year with SPEI<-1.5. Observations are grid cells approximately 28×28km by year for 1997-2018 for swarms and 1997-2014 for drought. The sample for impacts of swarm exposure is restricted to cells within 100 km of a swarm observation.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The similar patterns for impacts of exposure to drought and locust swarms on the risk of violent conflict indicate that they may be driven by similar mechanisms. The heterogeneity by local insecurity in particular highlights the importance of these shocks in creating conditions that increase vulnerability to future conflict prompted by more proximate grievances.

The results also increase our confidence that the large long-term impacts of swarm exposure on violent conflict are not driven by bias in where locust swarms are reported. Identifying drought from remotely-sensed data does not depend on any reporting and where such droughts are realized over time and space is plausibly random, given that the drought index is based on within cell variation over time. Therefore, any potential omitted variable that could explain the relationship between reported swarm exposure and conflict is unlikely to also explain the impacts of drought exposure.

8.2 Estimating the effects of economic shocks

These results have implications for research on the impacts of economic shocks. The economic literature on weather or agricultural shocks and conflict has overwhelmingly focused on the short-term and assumes effects of shocks are transitory—lasting only for the period in which the shock occurs—or otherwise persisting for very few periods.This follows from a focus on the direct effects of the shocks, such as decreases in agricultural productivity, which typically are transitory. A common empirical approach is a distributed lag two-way fixed effects model which takes the form:

$$Conflict_{ict} = \alpha + \beta_1 Shock_{ic,t} + \beta_2 Shock_{ic,t-1} + \delta X_{ict} + \gamma_{ct} + \mu_i + \epsilon_{ict} \quad (2)$$

This follows the persistent effects model in Equation 1 with the exception that instead of the *Shock* variable representing an absorbing treatment status over subsequent years, in this transitory effects model the outcome is unaffected in the years following a shock except as captured by the one year lag. This lag allows for limited delays or persistence in impacts of the shock (Burke et al., 2015).

With cell fixed effects the short-term impacts in the transitory effects model are estimated relative to conflict risk in other years in the same cell where a shock is not observed, including years after exposure to a shock. For shocks that cause persistent increases in conflict, this implies that the transitory effects estimate will be biased downward as a result of comparing conflict risk in the year a shock is observed against later years with no shock but higher conflict risk *caused* by the initial shock.

Table 7 shows that this is the case for locust swarms and drought, comparing estimates from regression models assuming transitory (one year) effects or medium-term (five year) effects to the BJS event study estimates which model the shock as a permanent treatment (for 10 post-exposure periods) and accurately capture dynamic treatment effects. For locust swarms, the transitory effects model estimates a highly significant 1.5pp *decrease* in the probability of any violent conflict in the year of exposure relative to unaffected cells. The bias is not reduced by including 5 years of lags in the model, which allows for persistence of effects only for the number of periods included as lags. The year zero estimate in this model is 1.6pp lower than the event study estimates, while the estimates for the next five periods are consistently 1.4-2-0pp lower. I can reject that the transitory and five-year estimates are the same as the event study estimates with high confidence, consistent with downward bias of these models when treatment effects are not only persistent but increasing over a long period.

Table 7: Impacts of agricultural shocks on the conflict risk, treating shocks as temporary vs. persistent

	(1)	(2) Locust swarm	(3)	(4)	(5) Drought	(6)
	Transitory effects	5 year effects	Event study effects	Transitory effects	5 year effects	Event study effects
Any shock during year	-0.015***	-0.019***	-0.003	0.004	0.001	0.007**
	(0.004)	(0.005)	(0.003)	(0.003)	(0.002)	(0.003)
Any shock, 1 year lag		-0.012**	0.005		-0.000	0.005***
		(0.006)	(0.004)		(0.002)	(0.002)
Any shock, 2 year lag		-0.007	0.013***		-0.001	0.005**
		(0.006)	(0.005)		(0.002)	(0.002)
Any shock, 3 year lag		-0.008	0.012***		-0.002	0.006***
		(0.006)	(0.004)		(0.002)	(0.002)
Any shock, 4 year lag		0.002	0.015***		-0.003	0.007***
		(0.007)	(0.005)		(0.003)	(0.002)
Any shock, 5 year lag		-0.001	0.013**		-0.004	0.015***
		(0.007)	(0.005)		(0.004)	(0.004)
Average long-term effect			0.018***			0.008***
			(0.004)			(0.002)
p-value, year 0 equality with event study	0.024	0.006		0.407	0.118	
p-value, year 5 equality with event study		0.090			0.002	
Observations	301664	233104	301664	454113	327746	452574
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Estimation	TWFE	TWFE	BJS	TWFE	TWFE	BJS

Note: The dependent variable is a dummy for any violent conflict event observed in a cell-year. All regressions estimate the effect of shock exposure on violent conflict controlling for total cell population and current year measures of total precipitation and maximum annual temperature along with cell and country-by-year fixed effects and distance to capital-by-year fixed effects. In columns 1 and 4 the shock is assumed to only have an effect in the year it is observed, and effects are estimated using TWFE. Columns 2 and 5 allow for persistent or delayed effects of the shock for up to 5 years after it is observed, and also include 5 lags of precipitation and temperature. Effects are estimated using TWFE. Columns 3 and 6 present a subset of the treatment effect estimates from the BJS event study estimator considering 10 pre-exposure and 10 post-exposure periods. These estimates are the same as those shown in Figure 3 and Figure 6. At the bottom of these columns I also present the estimates of average long-term effects of the shock from Table 6. At the bottom of columns 1, 2, 5, and 5 are p-values for tests of equality between coefficients under the transitory, 5 year, and event study models. Observations are grid cells approximately 28×28km by year for 1997-2018 for swarms and 1997-2014 for drought. The sample for impacts of swarm exposure is restricted to cells within 100 km of a swarm observation. SEs clustered at the sub-national region level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The transitory effects estimate for locust swarms—a 1.5pp decrease in violent conflict the year locusts are observed—is very close to Torngren Wartin (2018)'s estimate of a 1.3pp decrease using a similar method.[18] He interprets the result as suggesting endogenous under-reporting of locust swarm presence correlated with violent conflict. The much larger event study estimate for the impact of swarms on conflict in the same year suggests the large negative estimate in the transitory

[18] Torngren Wartin (2018) employs the same general distributed lag specification with cell and country-by-year fixed effects as in columns 1 and 2 of Table 7 but with some different weather controls and varying lags of locust presence. His analysis is at the level of 0.5° and 0.1° cells and considers locust swarms and bands together while I focus on more destructive swarms alone. He also includes some African countries with very few locust swarm observations over time which I exclude, while excluding Arabian countries with extensive locust activity which I include.

effects regression can instead be attributed to downward bias from ignoring long-term impacts of swarm exposure on conflict risk.

In the case of drought, the transitory effects estimate for the year of exposure is a non-significant 0.4pp increase in violent conflict risk, compared to a statistically significant 0.7pp increase in the event study estimate. While I cannot reject that these estimates are the same (p=0.407), the two approaches would yield different conclusions with different policy implications. As with the analysis with locust swarms, including five lagged shock variables aggravates the bias in the estimated year zero effect, though I still cannot reject equality with the event study estimate (p=0.118). The bias in the transitory effects estimates is generally lower when considering drought relative to locust shocks. This can be explained by the lower average long-term effect: a 0.8pp increase in conflict risk for drought compare to 1.8pp for locust swarms.

These results provide evidence of a potential misspecification of studies analyzing short-term impacts of transitory economic shocks, if these have persistent indirect effects on outcomes of interest. Studies of such shocks using specifications similar to Equation 2 and ignoring possible long-term effects will generate biased short-term impact estimates to the extent the shocks have long-term indirect impacts. This concern is a special case of contamination in estimated effects of treatment leads and lags in settings with dynamic and heterogeneous treatment effects (Sun and Abraham, 2021), which can lead to errors in both magnitude and sign (Roth et al., 2023) as shown here particularly for the impacts of locusts swarms on violent conflict risk. A large literature has studied this limitation of TWFE estimators and proposes a variety of event study estimators to address its limitations including those I use to estimate long-term effects of locust swarm exposure on conflict (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Cengiz et al., 2019; De Chaisemartin and d'Haultfoeuille, 2024; Sun and Abraham, 2021).

Building on this literature and to provide intuition for the results in Table 7, I conduct simulations estimating different regression models under several scenarios of dynamic treatment effects (Figure A3, Table A5). I show that sign errors for TWFE estimators assuming transitory treatment effects are more likely when the true effect in the treatment period is small relative to effects in later periods. The magnitude of the bias in the transitory effects estimator depends on the true immediate effect and on the average of treatment effects in subsequent periods rather than on particular dynamics of those effects. This is because fixed effects estimators compare differences in outcomes in a given period against the average across other periods. Intuitively, there is also less bias in transitory effects estimates if the effects of treatment do not persist for very long. Including lagged treatment terms attenuates this bias under constant and decreasing long-term effects of treatment, but aggravates it under increasing long-term effects.

Attention to these estimation issues in difference-in-differences settings has been rapidly increasing (see A. Baker et al. (2025) for a recent guide for practitioners). Certain difference-in-differences methods can also be applied in settings with repeated treatments, an important consideration for shocks such as droughts or locust swarms which may recur in the same location over time. I abstract away from that in this study—where such recurrence is rare in the sample period—by considering only the first exposure during the study period and defining absorbing treatment variables. Future work could explore dynamic effects of agricultural shocks on conflict while accounting for potential repeated treatments.

9 Conclusion

Violent conflict can have devastating consequences for economic and human development which are the subject of significant study even beyond the economics literature. This paper shows that exposure to severe agricultural production shocks—both desert locust swarms and drought—significantly increases long-term risk of violent conflict.

The results emphasize the limitations of individual-level conflict models focusing primarily on the role of changing opportunity costs of fighting following a productivity shock. Exposure to locust swarms does not cause the immediate onset of violent conflict, as predicted under an opportunity cost mechanism. An analysis of the timing of the main locust swarm exposure event in the sample suggests this may be due to limited popular grievances or unrest in the study area during this period. I find that long-term impacts of both locust swarms and drought are concentrated in periods of civil conflict—an indicator of heightened grievances that have escalated into violence—due to other proximate causes, when the feasibility of fighting and expected returns are likely to be higher and costs are lower. This results echoes a few other studies (Berman and Couttenier, 2015; Buhaug et al., 2021) but has not been emphasized in the economics literature on climate and conflict, and has important implications for considering what areas are most likely to become engaged in civil conflict after it is triggered by some proximate cause.

I propose a permanent income effect from strategies to cope with the initial agricultural destruction reducing later productivity and opportunity costs of fighting as a potential income-related mechanism for long-run effects of locust swarm exposure on conflict risk. Swarm exposure does not persistently affect measures of agricultural productivity at the level of the 0.25° grid cells I analyze, but it reduces agricultural production area and there is suggestive evidence that it increases out-migration. These results add to other work showing long-term adverse production and human capital effects of locust swarm exposure and many studies showing long-term economic impacts of natural disasters. Further research on long-term impacts of transitory economic shocks on household measures of productivity, labor supply, food security, and wealth would help further explore the potential role of a permanent income mechanism.

The findings suggest additional future avenues of research in the literature on climate and conflict. I show that the methods typically used in this literature, which treat transitory climate or weather shocks as only affecting conflict risk in the short-term when the shock directly affects agricultural outcomes, can result in biased estimates of short-term effects when the shocks have long-term indirect impacts. New event study analyses could test the extent and patterns of long-term impacts of other climate or economic shocks on conflict risk. Although not a focus of this paper, the lack of variation in the immediate impacts of precipitation and temperature deviations on violent conflict risk by agricultural land cover cast further doubt on whether effects on agricultural production are the primary mechanism. The association between climate and conflict has been demonstrated in a wide variety of settings but the mechanisms remain unclear (Burke et al., 2024; Mach et al., 2020). A better understanding of the different mechanisms is essential to determining the appropriate policy responses. Analyses of long-term impacts of agricultural shocks on measures of local inequality and on psychological factors could be particularly helpful in understanding both income- and non-income-related mechanisms.

The economic and human costs of increased conflict risk following severe agricultural shocks highlights the importance of policy efforts to respond to such shocks. Burke et al. (2024) find robust evidence across studies that higher living standards reduce sensitivity of conflict risk to climate shocks. Additional research could explore whether policies that can promote resilience to agricultural shocks also reduce conflict risk, building on existing studies looking at cash transfers (Crost et al., 2016), insurance (Sakketa et al., 2025), improved infrastructure (Gatti et al., 2021), and work programs (Fetzer, 2020).

The results also have implications for estimates of the economic and social costs of desert locust outbreaks. Past research on desert locusts has argued that limited impacts of outbreaks on aggregate national measures of agricultural production may mean expensive locust monitoring and control operations have limited net economic benefits (Joffe, 2001; Krall and Herok, 1997), though others have argued that local damages are extensive and motivate continued proactive locust control efforts (Showler, 2019; Zhang et al., 2019). A consideration of the broader long-term economic and social impacts of agricultural destruction by locust swarms—notably increased vulnerability to violent conflict—could motivate greater investment in proactive locust monitoring and control, as well as increased cross-country communication and collaboration in response to threats of locust swarms.

Beyond contributing to our understanding of the relationship between agricultural production shocks and conflict risk, the findings are also relevant for considering multilateral policy around climate change mitigation and adaptation. Climate change is increasing the frequency and severity of agricultural shocks, including by creating conditions suitable for desert locust swarm formation. These shocks impose additional costs on society through their impacts on conflict risk which should be considered when weighing the costs and benefits of potential actions to reduce and respond to risks from agricultural shocks.

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A Additional figures and tables

Table A1: Summary statistics

Panel A: Yearly variables

	Mean	SD	Min	50^{th}	75^{th}	Max	N
Any violent conflict event - ACLED	0.02	0.14	0.0	0.0	0.0	1.0	557018
Any violent conflict event - UCDP	0.01	0.10	0.0	0.0	0.0	1.0	557018
Any swarms in cell	0.00	0.07	0.0	0.0	0.0	1.0	557018
Any swarms within 100km outside cell	0.05	0.21	0.0	0.0	0.0	1.0	557018
Any swarms within 100-250km of cell	0.11	0.32	0.0	0.0	0.0	1.0	557018
Population (10,000s)	1.63	8.92	0.0	0.1	0.9	749.8	557018
Total annual rainfall (100 mm)	2.40	3.79	0.0	0.8	2.8	43.4	557018
Max annual temperature (deg C)	37.55	5.11	11.5	38.2	41.3	49.1	557018

Panel B: Fixed variables

	Mean	SD	Min	50^{th}	75^{th}	Max	N
Any ACLED violent conflict event in cell from 1997-2018	0.13	0.33	0.0	0.0	0.0	1.0	25435
Any UCDP violent conflict event in cell from 1997-2018	0.07	0.26	0.0	0.0	0.0	1.0	25435
Any locust swarm in cell from 1985-2021	0.20	0.40	0.0	0.0	0.0	1.0	25435
Any locust swarm in cell from 1997-2018	0.09	0.29	0.0	0.0	0.0	1.0	25435
First exposed to locust swarm between 1997-2018	0.07	0.26	0.0	0.0	0.0	1.0	25435
First exposed to locust swarm in 2003-2005 upsurge	0.05	0.22	0.0	0.0	0.0	1.0	25435
Any locust swarm within 100 km from 1985-2021	0.78	0.41	0.0	1.0	1.0	1.0	25435
Any locust swarm within 100 km from 1997-2018	0.55	0.50	0.0	1.0	1.0	1.0	25435
Any cropland or pasture in cell	0.57	0.50	0.0	1.0	1.0	1.0	25435
Share of cell allocated to crops or pasture	0.23	0.32	0.0	0.0	0.4	1.0	25435
Any pasture in cell	0.56	0.50	0.0	1.0	1.0	1.0	25435
Share of cell allocated to pasture	0.18	0.27	0.0	0.0	0.3	1.0	25435
Any cropland in cell	0.31	0.46	0.0	0.0	1.0	1.0	25435
Share of cell allocated to crops	0.05	0.13	0.0	0.0	0.0	1.0	25435

Note: Observations are grid cells approximately 28×28km by year. The study period covers 1997-2018. Data on locust swarm observations is available from the FAO Locust Watch for 1985-2021. Data on conflict events are fron the ACLED and UCDP databases. Data on population is from GPW (CIESIN, 2018). Data on precipitation and temperature are from WorldClim (Fick and Hijmans, 2017). Values for land cover are for the year 2000 from Ramankutty et al. (2010).

Table A2: Balance in cell characteristics by exposure to any locust swarm

	All cells No weights		W/in 100km of any swarm No weights		W/in 100km of any swarm Inverse exposure propensity weights	
	Control Mean (SD)	Treat Diff. (SE)	Control Mean (SD)	Treat Diff. (SE)	Control Mean (SD)	Treat Diff. (SE)
Population in 2000 (10,000s)	1.22	1.32***	1.65	0.90***	1.67	0.43
	[6.99]	(0.37)	[8.67]	(0.33)	[8.71]	(0.50)
Mean of cell nightlights	0.05	0.01**	0.05	0.00	0.05	0.00
1996-2012 (0-1)	[0.04]	(0.00)	[0.05]	(0.00)	[0.05]	(0.00)
Distance to national capital	707.37	-182.42***	586.82	-61.87**	586.45	17.59
(km)	[405.76]	(46.07)	[357.73]	(28.29)	[358.35]	(24.98)
Distance to nearest national	200.16	-40.97***	195.63	-36.45***	194.52	8.31
boundary (km)	[151.74]	(13.96)	[147.84]	(11.90)	[147.70]	(11.54)
Percent of cell covered by	4.69	0.83	5.83	-0.31	5.89	0.63
crop land in 2000	[13.12]	(0.78)	[14.48]	(0.69)	[14.55]	(0.80)
Percent of cell covered by	17.43	11.03***	21.59	6.87***	21.79	-0.98
pasture land in 2000	[26.69]	(2.40)	[28.05]	(2.16)	[28.12]	(1.59)
Percent of cell covered by	68.97	-9.98***	61.16	-2.16	60.76	0.98
barren area in 2009	[42.75]	(3.68)	[43.52]	(3.04)	[43.58]	(2.73)
Percent of cell covered by	0.08	0.10	0.10	0.08	0.10	0.01
urban area in 2009	[0.75]	(0.07)	[0.88]	(0.07)	[0.89]	(0.05)
Percent of cell covered by	6.80	-1.09	7.12	-1.42	7.20	-0.07
forest area in 2009	[17.00]	(1.03)	[16.15]	(0.91)	[16.22]	(0.92)
Percent of cell covered by	2.01	1.49**	2.92	0.58	2.94	0.20
water area in 2009	[10.70]	(0.61)	[12.75]	(0.61)	[12.81]	(0.53)
Mean annual max NDVI	0.23	-0.01	0.24	-0.02	0.24	0.01
(1997-2018)	[0.21]	(0.02)	[0.20]	(0.01)	[0.20]	(0.01)
Mean annual rainfall 1997-2018	2.42	0.15	2.68	-0.11	2.71	0.11
(100 mm)	[3.80]	(0.25)	[3.76]	(0.17)	[3.77]	(0.18)
Mean annual max temperature	37.63	-1.61***	36.70	-0.69*	36.67	-0.21
1997-2018 (deg C)	[5.11]	(0.45)	[5.05]	(0.37)	[5.07]	(0.38)
Mean of cell annual share of	0.09	-0.00	0.09	0.00	0.09	0.00
months with drought 1998-2014	[0.03]	(0.00)	[0.04]	(0.00)	[0.04]	(0.00)
		$F = 5.00$		$F = 2.38$		$F = 0.64$
Joint significance		$p < 0.01$		$p < 0.01$		$p = 0.824$

Note: The table shows results from separate bivariate regressions of baseline or mean cell characteristics on a dummy for being exposed to a locust swarm during the study period. The rows indicate which dependent variable is used. The first set of columns includes all cells while the second restricts the sample to cells within 100 km of any locust swarm observation from 1997-2021. The third set of columns includes all cells but weights observations by the inverse of the estimated propensity to have been exposed to a locust swarm during the study period. I include results of joint tests that there is no relationship between any of the characteristics and swam exposure. Observations are grid cells approximately 28×28km by year. SEs clustered at the sub-national region level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Event study estimates of impact of swarm exposure on different conflict outcomes

	(1) Violent conflict (ACLED)	(2) Output conflict (ACLED)	(3) Factor conflict (UCDP)
pre10	-0.006	-0.003	-0.007
	(0.011)	(0.005)	(0.010)
pre9	-0.024***	-0.009	-0.017**
	(0.008)	(0.007)	(0.007)
pre8	-0.006	-0.005	-0.006
	(0.009)	(0.007)	(0.007)
pre7	0.004	-0.001	0.002
	(0.008)	(0.006)	(0.009)
pre6	-0.007	-0.007	-0.002
	(0.006)	(0.006)	(0.008)
pre5	0.000	-0.004	0.004
	(0.008)	(0.006)	(0.010)
pre4	0.004	0.000	-0.002
	(0.009)	(0.006)	(0.009)
pre3	0.005	0.000	0.003
	(0.008)	(0.007)	(0.010)
pre2	-0.002	0.000	0.001
	(0.007)	(0.006)	(0.008)
pre1	0.002	0.005	-0.001
	(0.007)	(0.007)	(0.007)
tau0	0.000	0.002	0.001
	(0.003)	(0.002)	(0.002)
tau1	0.003	-0.001	-0.001
	(0.004)	(0.003)	(0.002)
tau2	0.009**	0.007**	0.006*
	(0.004)	(0.003)	(0.004)
tau3	0.009**	0.007**	0.004
	(0.004)	(0.003)	(0.003)
tau4	0.015***	0.011***	0.003
	(0.004)	(0.003)	(0.003)
tau5	0.013***	0.011***	0.008**
	(0.004)	(0.004)	(0.004)
tau6	0.005	-0.000	0.003
	(0.004)	(0.003)	(0.003)
tau7	0.024***	0.018***	0.011*
	(0.009)	(0.005)	(0.007)
tau8	0.025***	0.021***	0.012***
	(0.009)	(0.006)	(0.004)
tau9	0.021***	0.021***	0.004
	(0.007)	(0.006)	(0.004)
tau10	0.027***	0.034***	0.007*
	(0.009)	(0.008)	(0.004)
Total annual precipitation (SDs)	0.004**	0.002*	-0.000
	(0.002)	(0.001)	(0.001)
Max annual temperature (SDs)	0.012**	0.005	0.005
	(0.005)	(0.004)	(0.004)
Population (10,000s)	0.010***	0.009***	0.003***
	(0.002)	(0.002)	(0.001)
Observations	295348	295348	295348

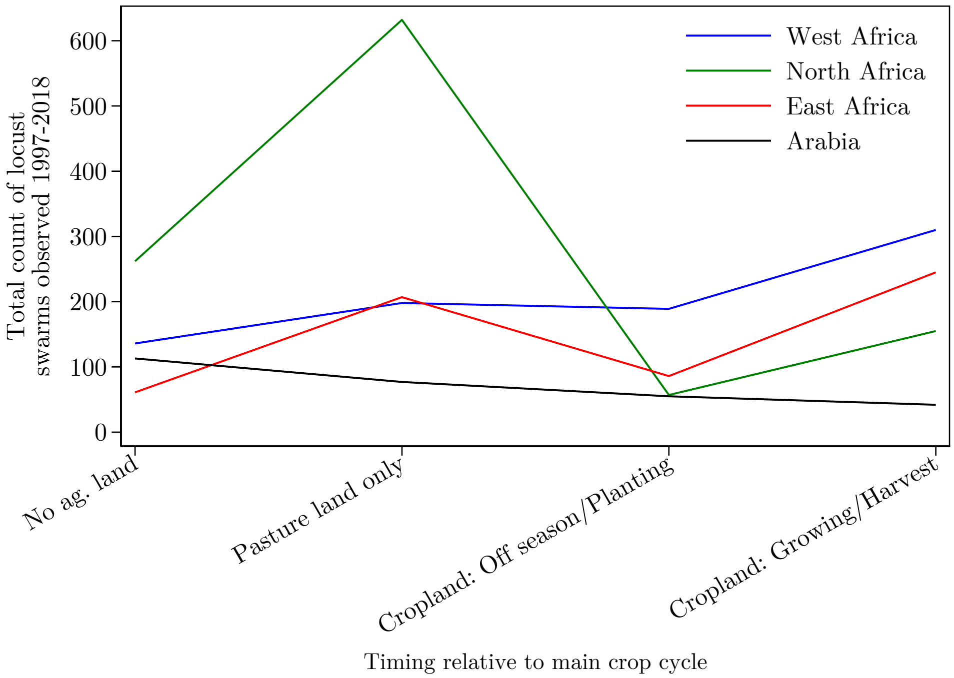
Note: The table shows the results of event study estimates of the impact of locust swarm exposure, with each column considering a different conflict outcome. Estimated impacts in each time period are weighted averages across effects for swarm exposure in particular years, calculated using the BJS estimator. Time period 0 is the year of first swarm exposure. Brackets represent 95% confidence intervals using SEs clustered at the sub-national region level. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital-by-year fixed effects. Observations are grid cells approximately 28×28km by year.

Table A4: Average impacts of locust swarm exposure on different conflict types

	(1) Violent conflict (ACLED)	(2) Output conflict (ACLED)	(3) Factor conflict (UCDP)
Average effect of swarm exposure	0.018***	0.014***	0.006***
	(0.004)	(0.003)	(0.002)
Observations	301664	301664	301664
Outcome mean post-2004, no exposure	0.028	0.019	0.012
Proportional effect of exposure	0.626	0.752	0.510
Country-Year FE	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes
Controls			

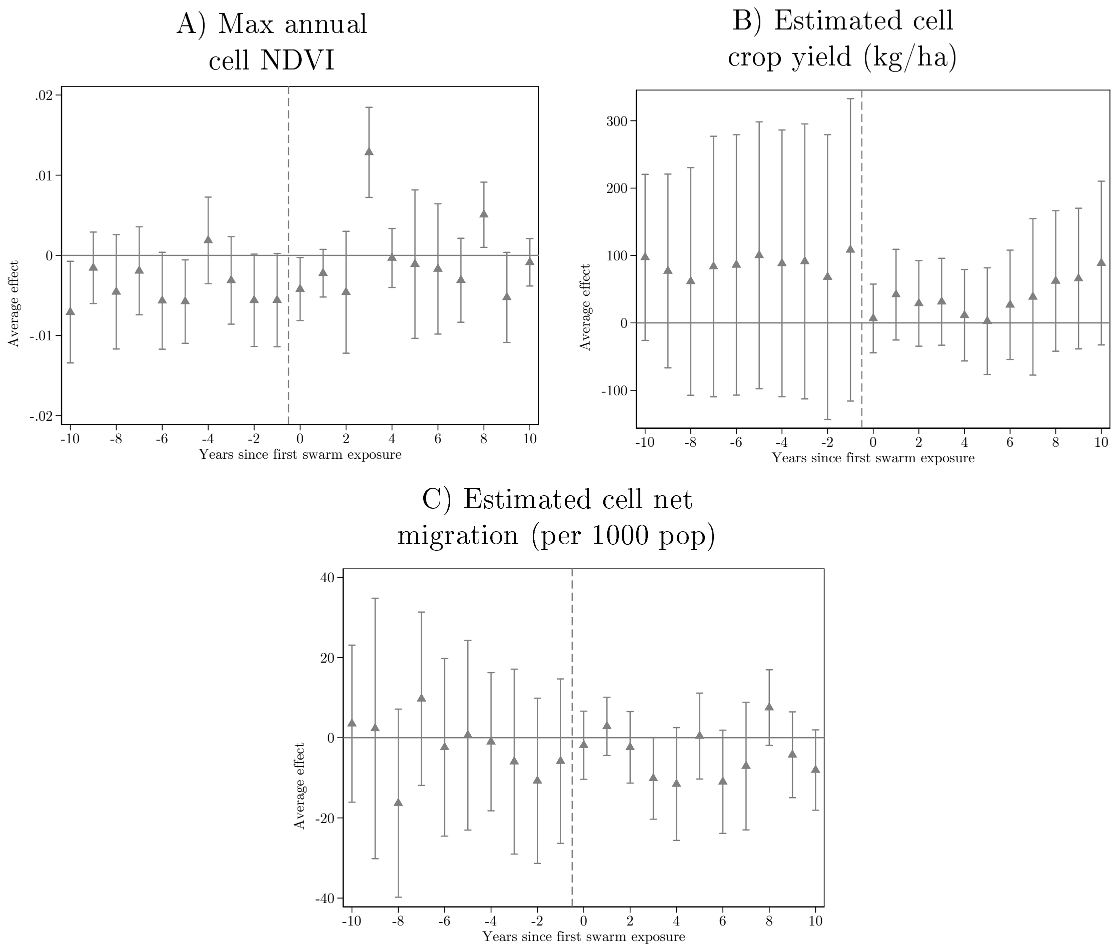
Note: Column 1 is the same as Table 1 column 1, and the other two columns replicate this for different conflict outcomse. The dependent variables are dummies for any conflict event being observed in a cell in a year. See the note for Table 1 for more detail on the estimation. Column 2 defines 'output' conflict as violence against civilians, riots, and looting from ACLED and column 3 defines 'factor' conflict as violent conflict events reported in the UCDP database (involving defined actors and at least 25 fatalities in a year), following McGuirk and Burke (2020).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A1: Variation in swarm timing and location during study period, by region



Note: The figure identifies the timing of swarm observations from 1997-2018 based on information on local crop calendars for major crops. This timing is not considered in cells with no cropland. These seasons are used to define timing of swarm exposure used in Table 2.

Figure A2: Impacts of exposure to locust swarms on economic outcomes over time



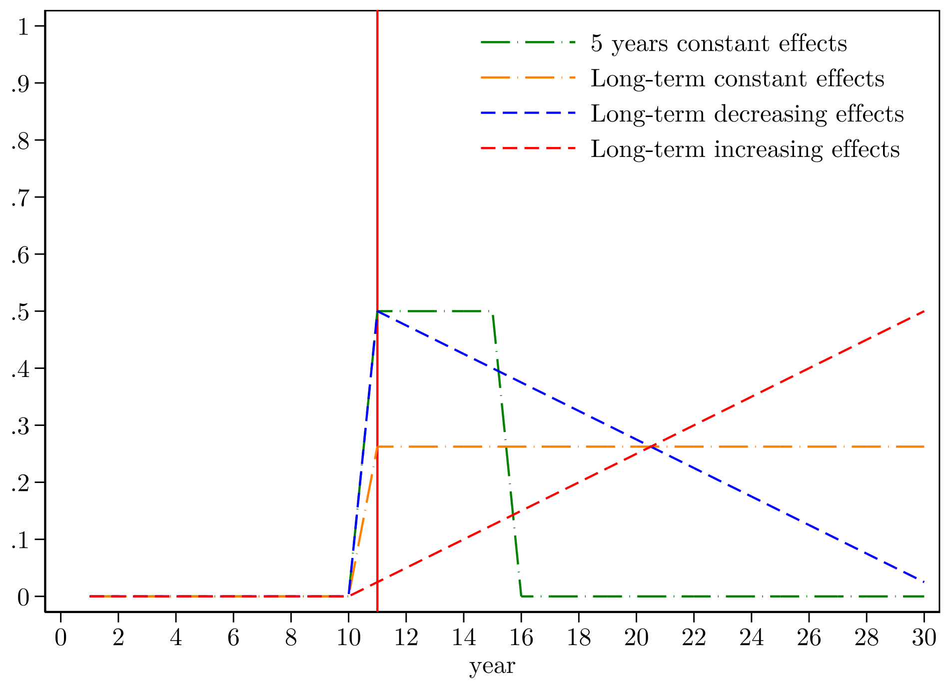
Each panel replicates Figure 3 but considering a different outcome as indicated in the panel title. See the figure note for Figure 3 for more detail.
NDVI is calculated from MODIS satellite imagery from 2000-2018 (Didan, 2015). I calculate average NDVI in each cell-month based on 16-day NDVI observations, and then take the maximum of these values in each cell-year. The event study is restricted to crop cells, where NDVI is a potential proxy for agricultural production. Crop-specific yield data at the cell level are estimated by Cao et al. (2025)) for four main crops globally from 1997-2015. I consider the 'main' crop in cells with multiple crops as the highest-yield crop in the cell. Annual net cell migration for 2000-2018 is estimated by Niva et al. (2023).

Table A5: Simulation: bias in estimates of short-term effects of shocks under different dynamic treatment effects

	(1) Immediate effect only	(2) Constant 5 year effects	(3) Constant long-term effects	(4) Decreasing long-term effects	(5) Increasing long-term effects
A. True effects:					
Treatment during current year	0.500***	0.500***	0.262***	0.500***	0.025***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 1 year lag	0.000	0.500***	0.262***	0.475***	0.050***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 2 year lag	0.000	0.500***	0.262***	0.450***	0.075***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 3 year lag	0.000	0.500***	0.262***	0.425***	0.100***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 4 year lag	0.000	0.500***	0.262***	0.400***	0.125***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
B. Estimated transitory effects:					
Treatment during current year	0.500***	0.431***	0.091***	0.336***	-0.155***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Difference from actual treatment effect	0.000	0.069	0.171	0.164	0.180
C. Transitory effects w/ 1st differences:					
Treatment during current year	0.500***	0.250***	0.131***	0.263***	0
	(0.000)	(0.000)	(0.000)	(0.000)	(.)
Difference from actual treatment effect	0.000	0.250	0.131	0.237	0.025
D. Estimated lagged effects:					
Treatment during current year	0.500***	0.500***	0.105***	0.380***	-0.170***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 1 year lag	0.000	0.500***	0.105***	0.355***	-0.145***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 2 year lag	0.000	0.500***	0.105***	0.330***	-0.120***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 3 year lag	0.000	0.500***	0.105***	0.305***	-0.095***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment 4 year lag	0.000	0.500***	0.105***	0.280***	-0.070***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Differences from actual treatment effect	0.000	0.000	0.157	0.120	0.195
Observations	300000	300000	300000	300000	300000

Note: I simulate 10,000 observations across 30 periods, and randomly assign 20% to be treated in period 11. I define the outcome as taking a value of 0 for all units before period 11, and then vary the value based on different possible dynamic treatment effects. In column (1) treatment increases the outcome by 0.5 in the initial treatment period only. In column (2) treatment increases the outcome by 0.5 in each of the first 5 treatment periods. In column (3) treatment increases the outcome by 0.26 in all subsequent periods. In column (4) treatment increases the outcome by 0.5 in the first treatment period, but the effect *decreases* linearly over all subsequent periods. In column (5) treatment increases the outcome by 0.025 in the first treatment period, but the effect *increases* linearly over all subsequent periods. The evolution of the outcome over time is shown in Figure A3. Patterns are similar with reversed signs if I simulate negative treatment effects. I estimate three treatment effect models for each scenario which make different assumptions about dynamic treatment effects. Panel A shows the results of event study estimates of the true effects for the first 5 treatment periods. Panel B shows the results of TWFE estimates that assume a transitory treatment that only affects the outcome in the initial treatment period. Panel C is similar to Panel B but includes four lagged treatment indicators, assuming treatment effects only persist for five periods. All regressions include period and unit fixed effects. SEs clustered at the unit level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

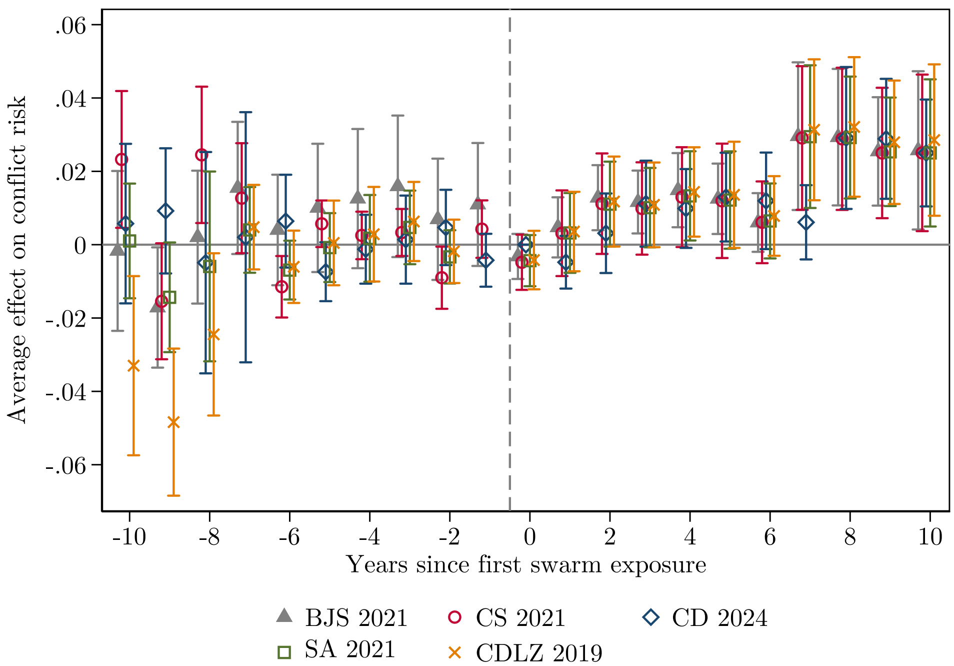
Figure A3: Simulation: evolution of outcome under different dynamic treatment effects



Note: This figure shows the evolution of the simulated outcomes as described in the notes to Table A5.

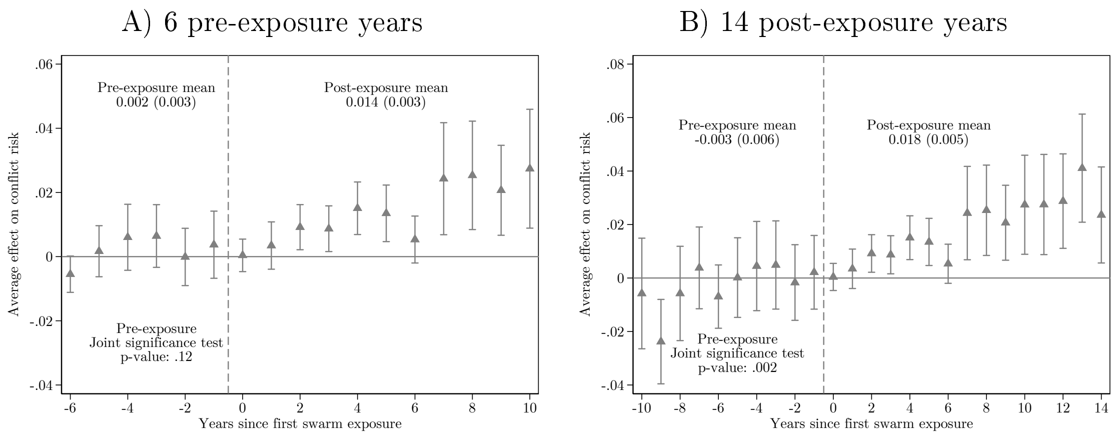
B Robustness

Figure B1: Alternative locust swarm exposure event study estimators



The figure shows event study impacts of locust swarm exposure on the risk of any violent conflict estimated using different methods. 'BJS 2021' refers to the Borusyak et al. (2024) method first introduced in 2021, 'CS 2021' refers to Callaway and Sant'Anna (2021), 'CD 2024' refers to De Chaisemartin and d'Haultfoeuille (2024), 'SA 2021' refers to Sun and Abraham (2021), and 'CDLZ 2019' refers to Cengiz et al. (2019). Time period 0 is the year of first swarm exposure. Brackets represent 95% confidence intervals using SEs clustered at the sub-national region level. Observations are grid cells approximately 28×28km by year. All regression include cell and year fixed effects and no controls, in contrast to the main specification which uses BJS with cell and country-by-year fixed effects and controls, due to constraints in including these additional controls in the Stata packages implementing some of the alternative estimators. I test the sensitivity of the main BJS estimates to different fixed effects and controls in Figure B5.

Figure B2: Impacts of exposure to locust swarms on violent conflict risk over different time horizons



Each panel replicates Figure 3 but changes the number of time periods included as indicated in the panel title. See the figure note for Figure 3 for more detail.

Table B1: Average impacts of exposure to locust swarms on violent conflict risk by estimator

Outcome: Any violent conflict event	(1) BJS 2021	(2) CS 2021	(3) CD 2024	(4) SA 2021	(5) CDLZ 2019	(6) TWFE
Average post-treatment effect	0.014***	0.014***	0.013***	0.015***	0.016***	0.017***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)
Observations	295348	301664	301664	301664	301664	301664

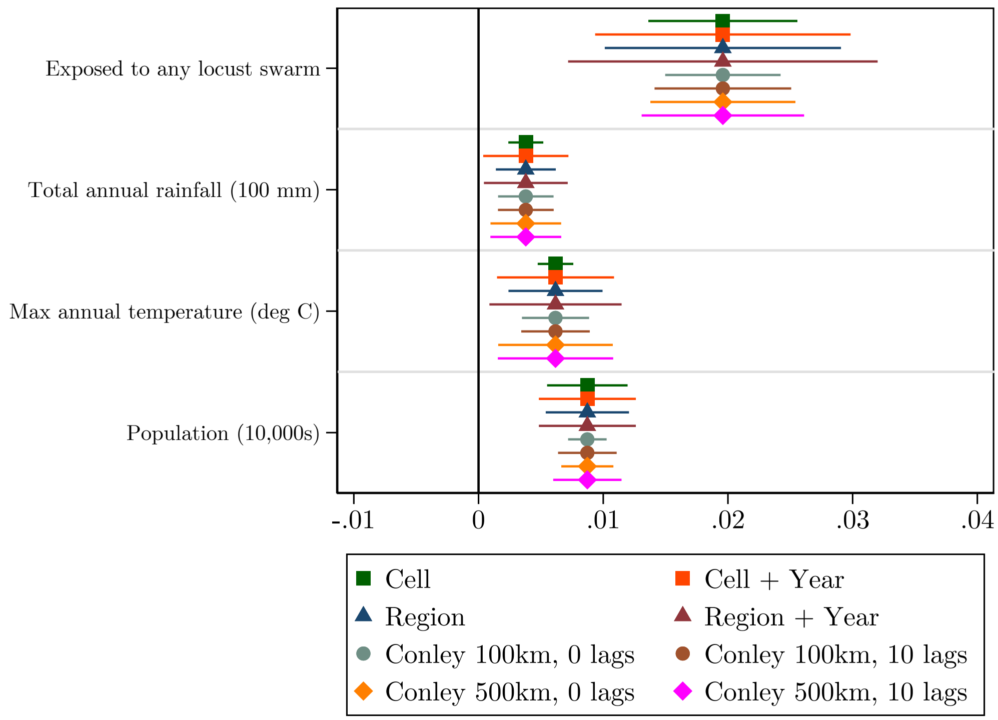
Note: The table shows the results from estimating average long-term impacts of swarm exposure on violent conflict using different estimators. 'BJS 2021' refers to the Borusyak et al. (2024) method first introduced in 2021, 'CS 2021' refers to Callaway and Sant'Anna (2021), 'CD 2024' refers to De Chaisemartin and d'Haultfoeuille (2024), 'SA 2021' refers to Sun and Abraham (2021), 'CDLZ 2019' refers to Cengiz et al. (2019), and 'TWFE' refers to the two-way fixed effects specification shown in Equation 1. As in Figure B1, all regression include cell and year fixed effects and no controls, in contrast to the main specification which uses BJS with cell and country-by-year fixed effects and controls, due to constraints in including these additional controls in the Stata packages implementing some of the alternative estimators. The results in column 1 using BJS therefore differ from those in Table 1 column 1 which follow the main specification. The purpose of the table is to test sensitivity of the estimates to the choice of estimator. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B2: TWFE average impacts of exposure to locust swarms on violent conflict risk by land cover

Outcome: Any violent conflict event	(1) All land	(2) Land = Any crop or pasture land	(3) Land = Any crop land
Exposed to any locust swarm	0.020***	0.005	0.006
	(0.005)	(0.006)	(0.006)
Total annual precipitation (SDs)	0.004**	0.002*	0.002*
	(0.002)	(0.001)	(0.001)
Max annual temperature (SDs)	0.014**	0.012**	0.013**
	(0.005)	(0.006)	(0.006)
Population (10,000s)	0.009***	0.026**	0.007
	(0.002)	(0.011)	(0.006)
Exposed to any locust swarm × Land=1		0.017**	0.021**
		(0.008)	(0.009)
Total annual precipitation (SDs) × Land=1		0.002	0.003*
		(0.002)	(0.002)
Max annual temperature (SDs) × Land=1		0.001	0.000
		(0.004)	(0.005)
Population (10,000s) × Land=1		-0.018	0.002
		(0.011)	(0.006)
Observations	301664	301664	301664
Outcome mean post-2004, no exposure	0.028	0.028	0.028
Country-Year FE	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

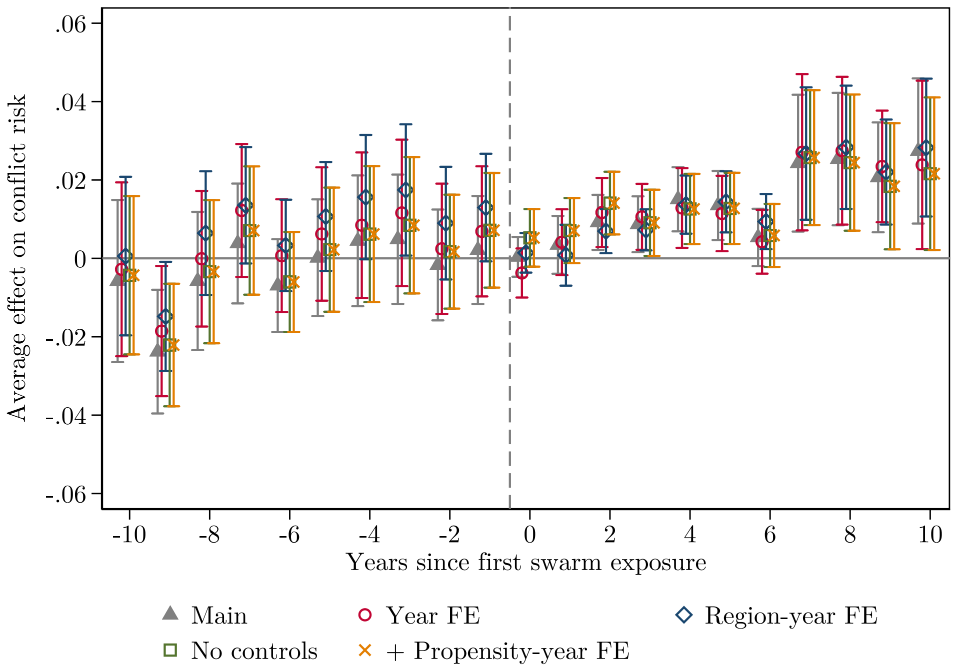
Note: The table replicates the results in Table 1 but using a two-way fixed effects (TWFE) estimator. Column 1 includes no land cover interactions and the other two interact all right-hand side variables (except cell fixed effects) with cell land cover dummies. The 'Land=1' rows show the coefficients for the interaction of right-hand side variables with cell land cover dummies indicated in the column heading. The outcome mean for control cells is shown for post-2004 for comparison with exposure impacts in the period after the majority of swarm exposure occurred. All regressions include country-by-year, cell, and distance to capital by year fixed effects in addition to the controls shown. Observations are grid cells approximately 28×28km by year. SEs are clustered at the sub-national region level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B3: Estimated coefficients from Equation 1 with different SEs



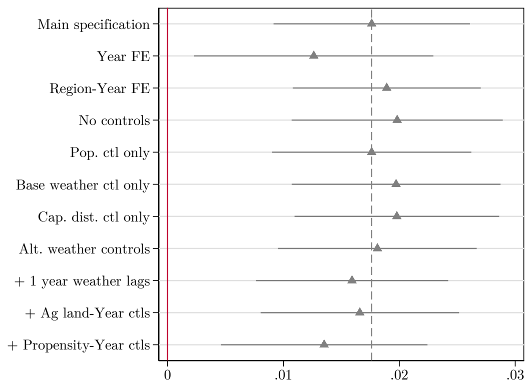
Note: The outcome variable is a dummy for any violent conflict observed. The figure shows 95% confidence intervals for estimates from Table B2 column (1) applying different clustering for the SEs. Regions are clusters of sub-national administrative units constructed so that each one includes at least 32 grid cells. Observations are grid cells approximately 28×28km by year. Regressions also include country-by-year and cell FE.

Figure B4: Sensitivity of locust swarm exposure event study results to different specifications



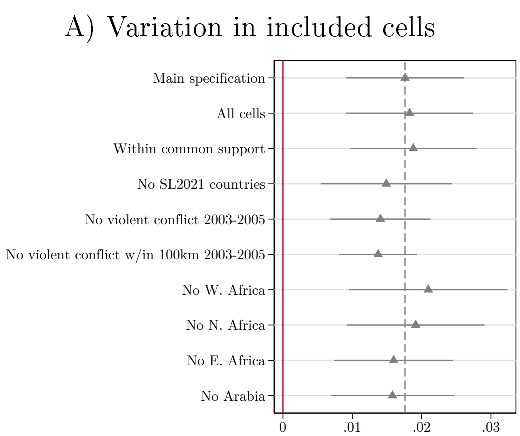
Note: Each event study replicates Figure 3 but changes some aspect of the specification as indicated in the legend. Time period 0 is the year of first swarm exposure. Brackets represent 95% confidence intervals using SEs clustered at the sub-national region level. Observations are grid cells approximately 28×28km by year. All regressions estimate dynamic effects of swarm exposure on violent conflict risk using the BJS estimator, and include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects unless otherwise stated. The year and region-year FE specifications replace country-by-year FE with FE at these levels, where region indicates the same sub-national regions used in clustering the SEs. The propensity-year FE specification adds estimated swarm exposure propensity by year fixed effects; the estimation of these propensities is explained in the Empirical approach section.

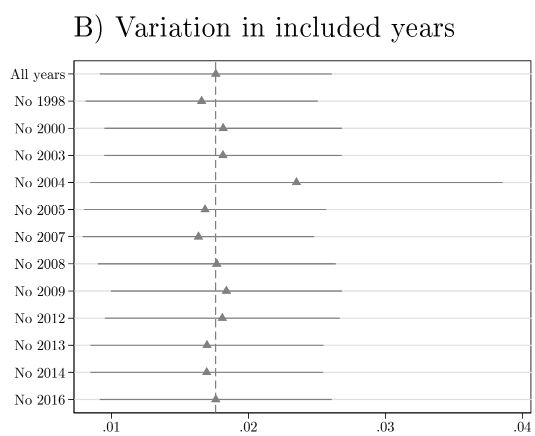
Figure B5: Sensitivity of average impacts of locust swarm exposure on violent conflict risk to alternative specifications



Note: Each estimate and 95% confidence interval is from a separate regression replicating Table 1 column 1 with a given modification. The dashed gray line indicates the main estimate from Table 1 column 1.
The main specification includes controls for precipitation, temperature, and population at the cell-year level and cell, country-by-year, and distance to capital-by-year FE. Each coefficient is associated with a specific change in this specification. 'Alt.' weather controls replace the rainfall and temperature measures from WorldClim with measures from CHIRPS and ERA5, respectively. The bottom three specifications add additional controls: 1 year lags of precipitation and temperature, any agricultural land-by-year FEs, and estimated swarm exposure propensity-by-year FEs.

Figure B6: Sensitivity of average impacts of locust swarm exposure on violent conflict risk to alternative samples

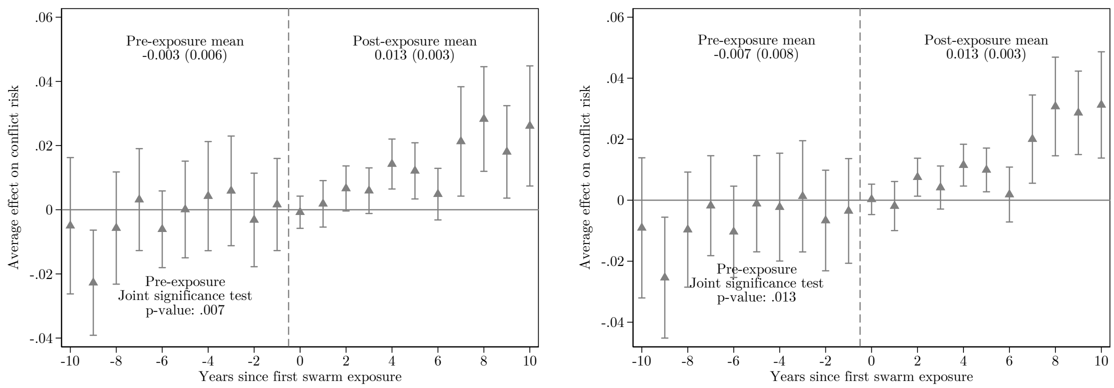




Note: Each estimate and 95% confidence interval is from a separate regression replicating Table 1 column 1 with a given modification. The dashed gray line indicates the main estimate from Table 1 column 1.
The main specification includes all years from 1997-2018 and excludes cells more than 100 km from any swarm report and outside the range of common support of estimated swarm exposure probability. Panel A shows results varying the set of included cells. I first vary whether cells more than 100 km from any swarm report and outside the range of common support of estimated swarm exposure probability are excluded. I then maintain the main sample restrictions but add additional restrictions. I drop countries where Showler and Lecoq (2021) report insecurity prevented some locust control operations during the sample period and cells that experienced violent conflict during the 2003-2005 locust upsurge which might have prevented locust reporting. Finally I drop countries in particular geographic regions.
Panel B shows results from dropping individual years when locust swarm exposure events occurred.

Figure B7: Impacts of exposure to locust swarms on violent conflict risk over time, imputing swarms in high-propensity areas

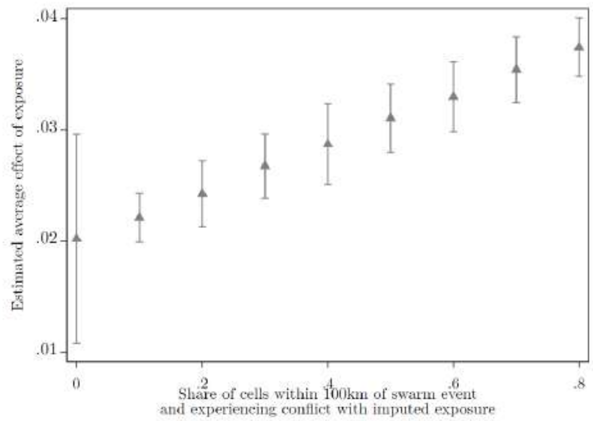
A) Imputed swarm if propensity≥ 0.5 B) Imputed swarm if propensity≥ 0.25



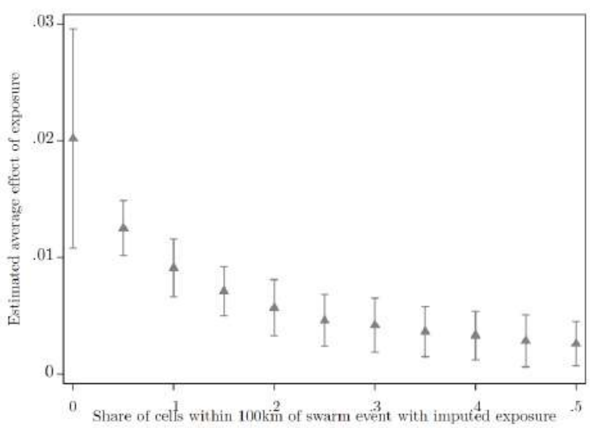
Each panel replicates Figure 3 but changes the imputation of potential 'missing' locust swarms. In each case, I assign a cell as experiencing a locust swarm if its estimated propensity of exposure (over the whole study period) is above a certain level in any year where a locust swarm was reported within 100 km of the cell. I then define exposure based on the first year such a swarm is imputed, and estimate the event study as previously. See the figure note for Figure 3 for more detail on the estimation.

Figure B8: Simulations of missing swarm observations

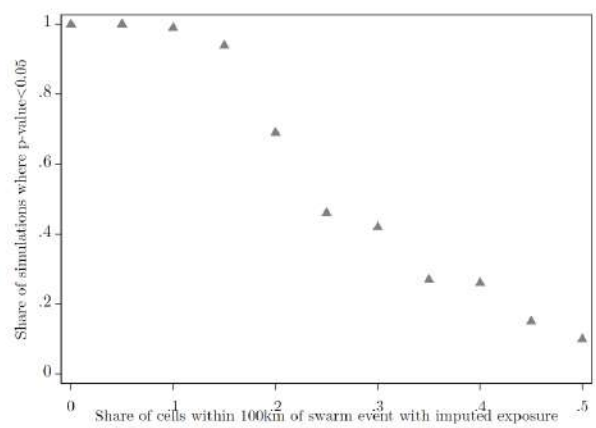
A) Betas, simulated swarms
in areas experiencing conflict
near swarm report



B) Betas, simulated swarms
in any area near swarm report

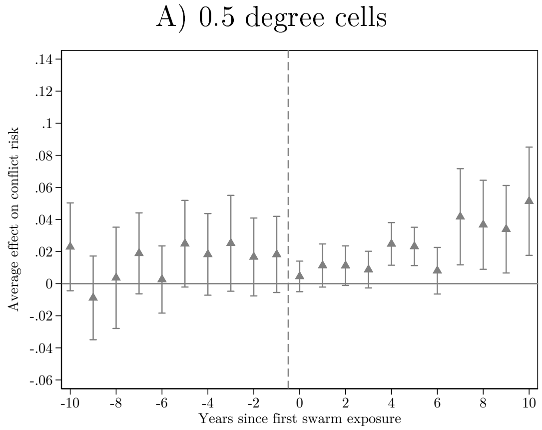


C) Rejection of H_0, simulated swarms
in any area near swarm report



Note: The figures show the results from estimating Equation 1 in simulations imputing the presence of increasing shares of unreported locust swarms in cell-years with a locust swarm reported within 100 km. For each share, I run 100 simulations randomizing which cell-years are imputed with an unreported swarm, recalculating the swarm exposure treatment variable, and estimating the average long-term impact of swarm exposure on violent conflict risk. In Panel A, I only impute swarms in cell-years both experiencing violent conflict and within 100 km of a reported locust swarm, to simulate effects of missing swarm reports in insecure areas. In Panels B and C, I impute swarms across all cell-years within 100 km of a reported locust swarm. Panels A and B report the average estimated effect across all simulations by share of imputed swarms, along with a 95% confidence interval for these estimates. Panel C reports the share of simulations where the p-value for the coefficient on swarm exposure is less than 0.05, by share of imputed swarms.

Figure B9: Dynamic impacts of swarm exposure on violent conflict risk at different scales



Each panel replicates Figure 3 estimating dynamic impacts of locust swarm exposure of the risk of any violent conflict event using the BJS method at different spatial scales. The main analysis uses 0.25° cells. When collapsing to larger cells I take the maximum of the swarm exposure and violent conflict variables and the mean of control variables across smaller cells within the aggregate cell. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. SEs are clustered at the sub-national region level, which is assigned based on the region of the majority of 0.25° component cells.

Table B3: Average impacts of swarm exposure on violent conflict risk at different scales

Outcome: Any violent conflict event	(1)	(2)	(3)	(4)	(5)	(6)
Average effect of swarm exposure	0.018***	0.025***	0.028***	0.131***	0.118***	0.096***
	(0.004)	(0.007)	(0.011)	(0.032)	(0.033)	(0.037)
Observations	301664	93940	30763	301664	93940	30763
Outcome mean post-2004, no exposure	0.028	0.067	0.128	0.028	0.067	0.128
Proportional effect of exposure	0.623	0.368	0.216			
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cell size (degrees)	0.25	0.5	1	0.25	0.5	1
Violent conflict outcome	Any	Any	Any	SDs	SDs	SDs

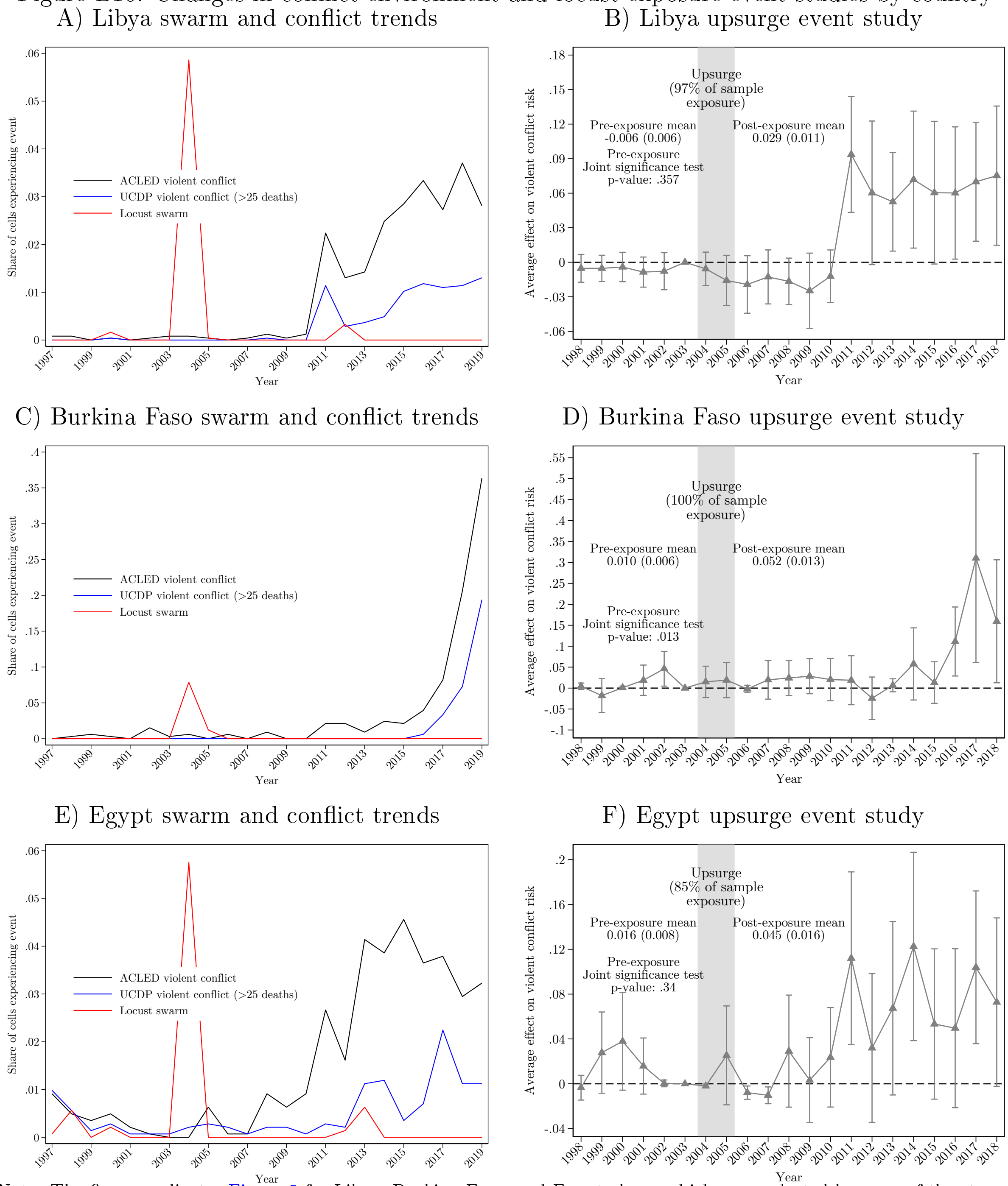
Note: The table replicates the results in Table 1 column 1 at different spatial scales, as presented in the note to Figure B9. Columns 1-3 show absolute effects on a dummy for any violent conflict while columns 4-6 show relative effects on the standard deviation in the probability of any violent conflict. The latter approach maintains comparability in effect sizes as the baseline risk of any conflict in a cell-year increase with cell size.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B4: Average impacts of direct and spillover exposure to locust swarms on violent conflict risk

Outcome: Any violent conflict event	(1) All swarms	(2) 2003-2005 upsurge swarms
Exposed to swarm	0.020***	
	(0.005)	
Exposed to swarm w/in 100km outside cell	0.001	
	(0.003)	
Exposed to swarm		0.018***
		(0.005)
Exposed to swarm w/in 100km outside cell		0.011*
		(0.006)
Observations	301664	301664
Country-Year FE	Yes	Yes
Cell FE	Yes	Yes
Controls	Yes	Yes

Note: The table presents results from estimating Equation 1 but including a spillover swarm exposure variable based on the first year a swarm is outside the cell but within 100 km of a cell centroid. As with within-cell swarm exposure, spillover swarm exposure is an absorbing treatment that takes a value of 1 in all years after the first exposure in the study period. Column 1 considers all swarm exposure events while column 2 focuses on the 2003-2005 upsurge. All regressions include country-by-year and cell fixed effects and controls for annual precipitation, maximum temperature, and population, and distance to capital by year fixed effects. SEs are clustered at the sub-national region level. Observations are grid cells approximately 28×28km by year.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B10: Changes in conflict environment and locust exposure event studies by country



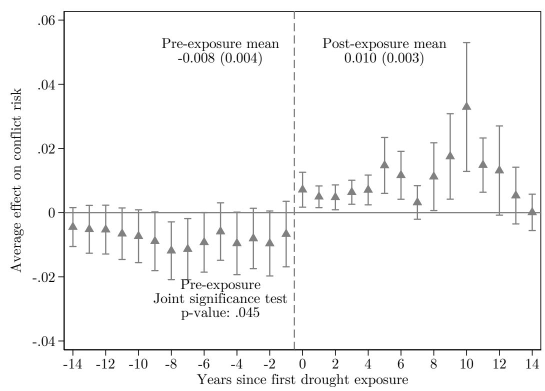
Note: The figure replicates Figure 5 for Libya, Burkina Faso, and Egypt alone, which were selected because of the strong concentration of locust exposure during the 2003-2005 upsurge. Conflict in Libya began to increase in 2011 with the start of the Arab Spring and the subsequent civil war. Armed Islamist violence escalated in Burkina Faso starting in 2016, with many attacks by Al Qaeda and IS affiliates that established control over many rural parts of the country. Egypt underwent a revolution in 2011 as part of the Arab Spring and experienced a coup d'etat in 2013 and continued higher levels of insecurity.

Table B5: Robustness of drought estimates to changing number of consecutive drought months

Outcome: Any violent conflict event	(1) 5 month drought	(2) 5 month, Z = Any crop or pasture land	(3) 5 month, Z = Any conflict in country outside sub-region	(4) 6 month drought	(5) 6 month, Z = Any crop or pasture land	(6) 6 month, Z = Any conflict in country outside sub-region
Average effect of shock exposure	0.004**			0.001		
	(0.002)			(0.002)		
Effect of exposure, Z=0		-0.002	0.000		-0.003*	0.001
		(0.002)	(0.001)		(0.002)	(0.001)
Effect of exposure, Z=1		0.008***	0.005**		0.004	0.001
		(0.003)	(0.003)		(0.003)	(0.002)
Observations	454986	454971	454986	455310	455300	455310
Outcome mean, no exposure	0.012	0.012	0.012	0.012	0.012	0.012
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reproduces columns 4-6 of **??** changing the definition of what constitutes a drought. The main definition uses at least 4 consecutive months of drought (observed in 3.6% of cell-years), and the alternative definitions presented here use thresholds of 5 months (1.8% of cell-years) and 6 months (1.1%).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B11: Event study impacts of drought exposure with 14 years of pre- and post-exposure estimates

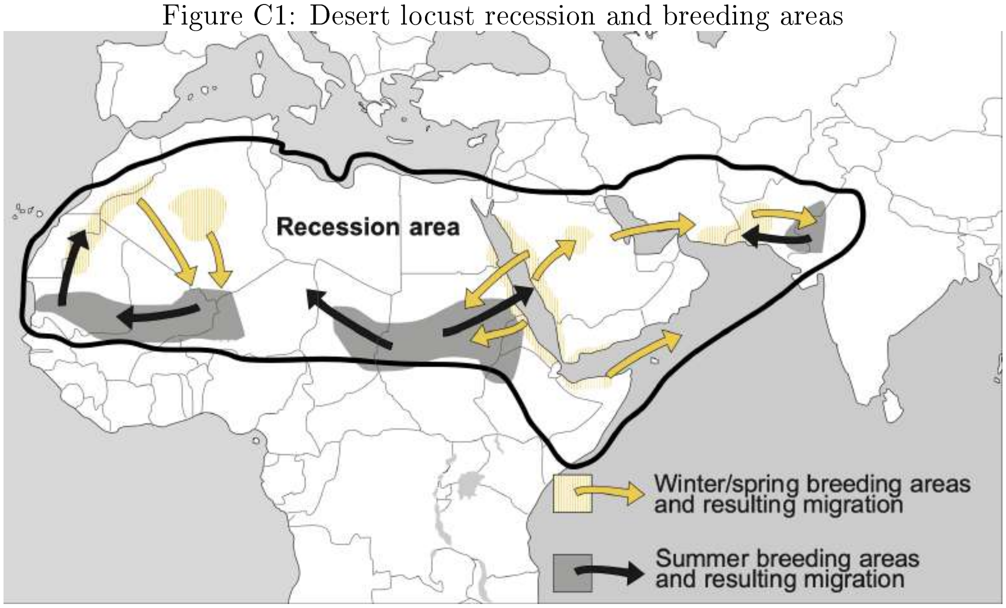


The figures replicates Figure 3 but changes the number of time periods included as indicated in the panel title. See the figure note for Figure 3 for more detail.

C Desert locusts background

Desert locusts (*Schistocerca gregaria*) are a species of grasshopper always present in small numbers in desert 'recession' areas from Mauritania to India (Figure C1). They usually pose little threat to livelihoods but favorable climate conditions in breeding areas—periods of repeated rainfall and vegetation growth overlapping with the breeding cycle—can lead to exponential population growth. For example, the 2019-2021 locust upsurge persisted in large part because of repeated heavy precipitation out of season due to cyclones, prompting explosive reproduction (Cressman and Ferrand, 2021). The 2003-2005 upsurge was initiated by good rainfall over the summer of 2003 across four separate breeding areas. This was followed by two days of unusually heavy rains in October 2003 from Senegal to Morocco, after which environmental conditions were favorable for reproduction over the following 6 months (FAO and WMO 2016).

Figure C1: Desert locust recession and breeding areas



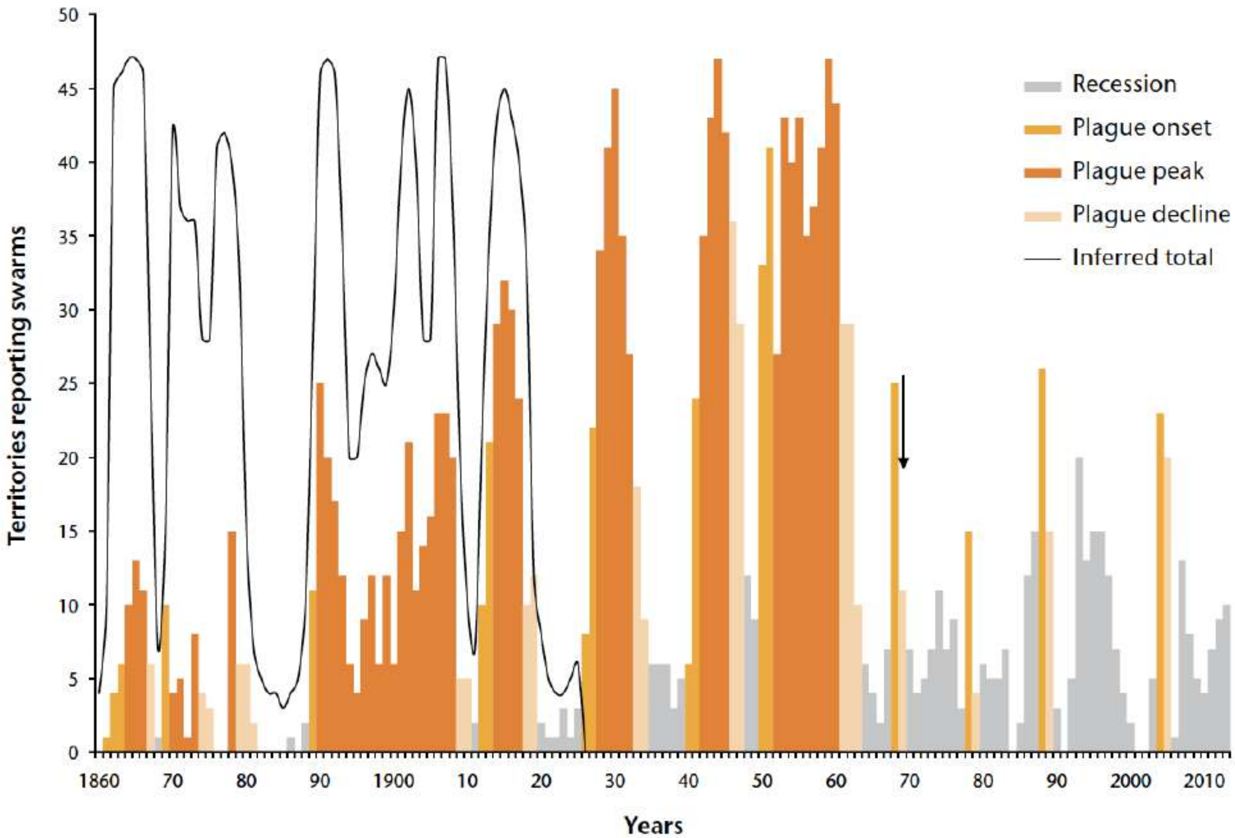
Source: Symmons and Cressman (2001). The recession area is the area in which desert locusts are most commonly found. The arrows indicate typical directions of downwind locust migration from breeding areas at different times of year.

Unique among grasshopper species, after reaching a particular population density desert locusts undergo a process of 'gregarization' wherein they mature physically and form large bands or swarms which move as a cohesive unit (Symmons and Cressman, 2001). Locust bands may extend over several kilometers and alternate between roosting and marching, typically downwind (FAO and WMO 2016). In this paper, I focus my analysis on gregarious swarms of adult desert locusts and set aside observations of locusts in other phases.

Locust swarms form when bands of locusts remain highly concentrated when they reach the adult stage and become able to fly. Swarms vary in their density and extent (Symmons and Cressman, 2001). The average swarm includes around 50 locusts per m^2 with a range from 20-150, and can cover under 1 square kilometers to several hundred (Symmons and Cressman, 2001). About half of swarms exceed 50 km^2 in size (FAO and WMO 2016), meaning swarms typically include over a billion individuals.

The formation of swarms can lead to 'outbreaks,' where locusts spread out from their largely desert initial breeding areas. Locusts in swarms have increased appetites and accelerated reproductive cycles, and are thus particularly threatening to agriculture. The FAO distinguishes different levels of locust swarm activity (Symmons and Cressman, 2001). I use the terms 'outbreak' and 'upsurge' interchangeably to refer to any locust swarm activity. Few locust swarms are observed outside of major outbreaks, as conditions favoring swarm formation tend to produce large swarms which reproduce and spread rapidly and are very difficult to control.

Figure C2: Desert locust observations by year



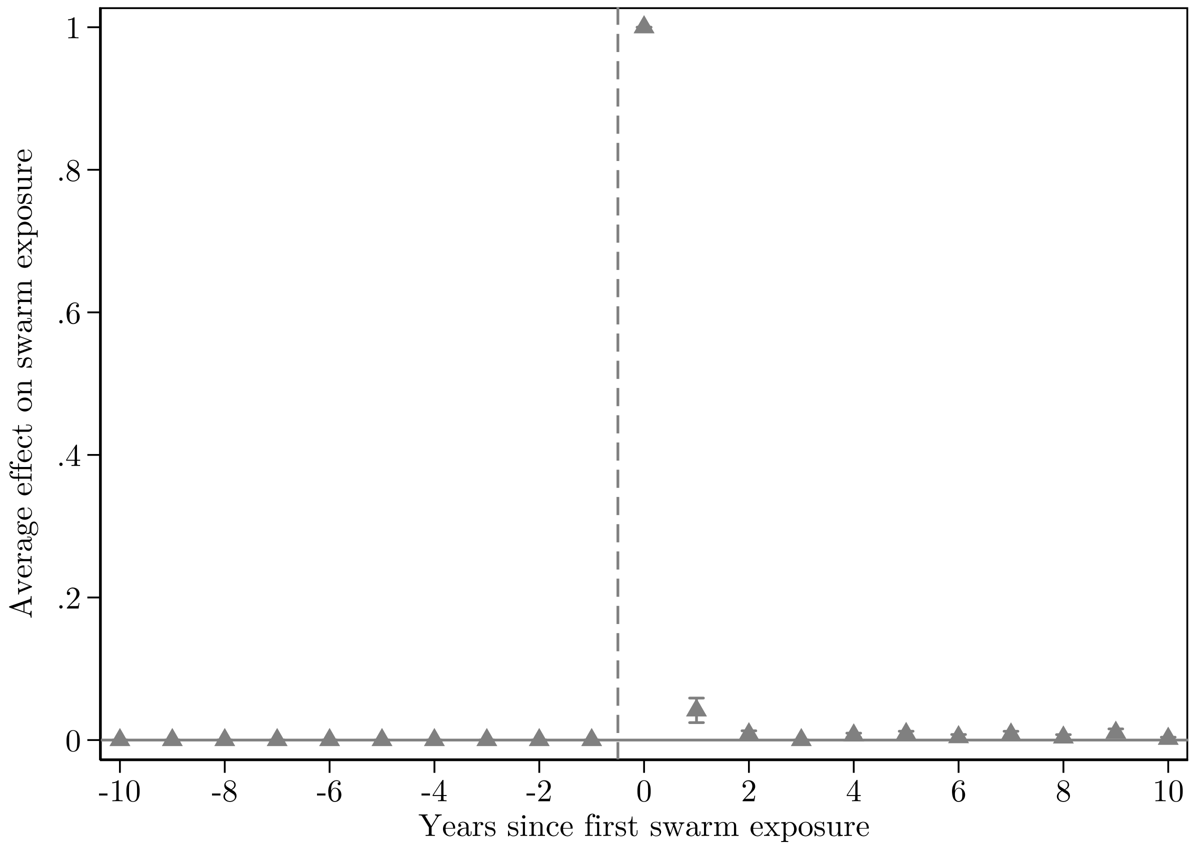
Source: Cressman and Stefanski (2016). Territories are generally individual countries.

The frequency of large-scale outbreaks has fallen since around the 1980s (Figure C2), in large part due to increases in coordinated preventive operations (Cressman and Stefanski, 2016). But climate change is expected to increase the risk of locust swarm formation and upsurges. Desert locusts can easily withstand elevated temperatures and the increased frequency of heavy rainfall events can create conditions conducive to population growth (McCabe, 2021; Qiu, 2009; Youngblood et al., 2023).

As illustrated by Figure 1, locust swarms are not observed with any regularity over time or space. Desert locusts are migratory, moving on after consuming all available vegetation, and outside of outbreak periods are ultimately restricted to desert 'recession' areas. Unlike many other insect species, therefore, the arrival of a desert locust swarm does not signal a permanent change in local agricultural pest risk. Instead, the arrival of a swarm can be considered a locally and temporally concentrated natural disaster where all crops and pastureland are at risk (Hardeweg, 2001). Figure C3 illustrates how exposure to a locust swarm does not significantly affect the risk of exposure over the following years, consistent with exposure being a function of quasi-random variations in wind patterns and flight duration during swarm outbreaks. The only period in which cells exposed to locusts are more likely to experience a later locust swarm is in the period immediately after exposure, which reflects that many locust outbreaks last more than one year.

Locust swarms are migratory and fly downwind from a few hours after sunrise to an hour or so before sunset when they land and feed. Swarms do not always fly with prevailing winds and may

Figure C3: Impact of swarm exposure on future exposure risk



Note: The figure shows an event study of the impact of swarm exposure on the probability of having any locust swarm recorded in a subsequent year for the study period 1997-2018. By construction, none of the exposed areas had any locust swarm recorded in the years preceding their first exposure since 1990, and all had a locust swarm present in the year of exposure.

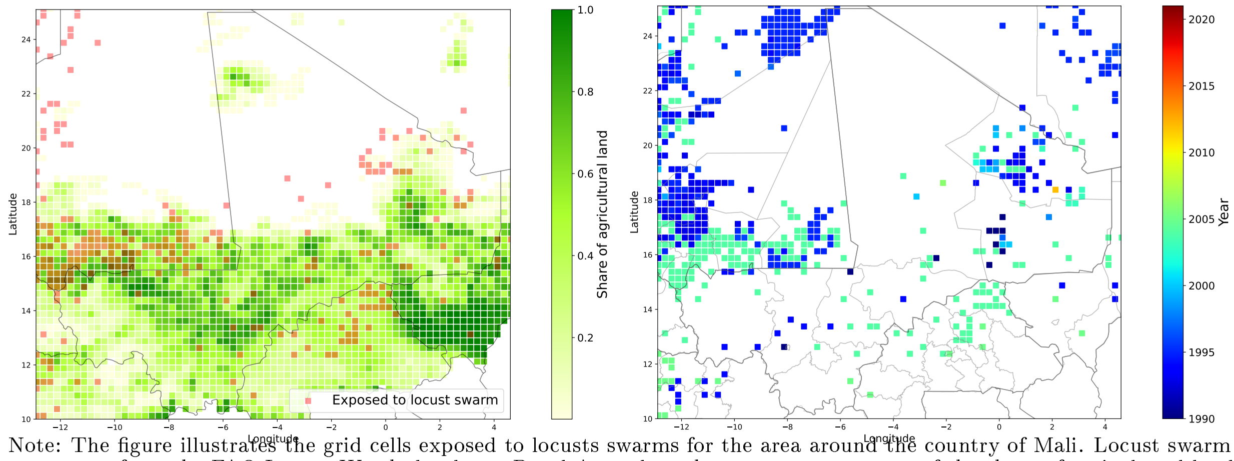
wait for warmer winds. Small deviations in the positions of individual locusts in the swarm can also lead to changes in swarm flight trajectory, making their movements difficult to predict. Seasonal changes in these winds tend to bring locusts to seasonal breeding areas at times when rain and the presence of vegetation is most likely, allowing them to continue breeding (FAO and WMO 2016).

Patterns in swarm movements lead to local variation in locust swarm exposure. After taking off, swarms fly for 9-10 hours rather than landing as soon as they encounter new vegetation. A swarm can easily move 100 km or more in a day even with minimal wind (Symmons and Cressman, 2001). Consequently, the flight path of a locust swarm will include both affected and unaffected areas, with the affected areas determined by largely by patterns of wind direction and speed over time from the initial swarm formation in breeding areas. Figure C4 illustrates the variation in areas affected by locust swarms over space around Mali. Swarm reports are densely clustered in the breeding areas in southern Mauritania where locust swarms reproduced in summer 2004. Outside of this area there is considerable variation in where swarms were reported, with distances between reported swarms over time consistent with typical flight distances.

These movement characteristics inform efforts to predict locust swarm movements, but these remain highly imprecise. The desert locust bulletins produced monthly by the FAO include forecasts of areas at risk of desert locust activity, but the areas described are quite large, often encompassing several countries in periods with increased swarms. While breeding regions and the broad areas at

Figure C4: Reports of locust swarms around Mali

A) Areas exposed to locust swarms since 1997 B) Year of first swarm exposure since 1990



Note: The figure illustrates the grid cells exposed to locusts swarms for the area around the country of Mali. Locust swarm reports are from the FAO Locust Watch database. Panel A overlays these reports on a map of the share of agricultural land area in each cell (Ramankutty et al., 2010), while Panel B illustrates the timing of first exposure to locust swarms.

risk over different time periods can generally be predicted with some accuracy (Latchininsky, 2013; Samil et al., 2020; Zhang et al., 2019), predicting specific local variation in swarm presence remains a challenge due to the multiple factors influencing specific flight patterns (FAO and WMO 2016).

While desert locusts can exhibit some preferences over types of vegetation, avoiding woody plants, dry vegetation, and some toxic plants in particular, swarms of locusts in their gregarious phase are less selective than solitary adults or nymphs (Despland, 2005; Despland and Simpson, 2005). The size of locust swarms and their polyphageous nature has led them to be considered the world's most dangerous and destructive migratory pest (Cressman et al., 2016; Lazar et al., 2016). A small swarm covering one square kilometer consumes as much food in one day as 35,000 people and the median swarm consumes 8 million kg of vegetation per day (FAO, 2023a), without preference for different types of crops (Lecoq, 2003.

The arrival of a locust swarm can therefore lead to the total destruction of local agricultural output (Showler, 2019). During the last locust upsurge in 2003-2005 in North and West Africa, 100, 90, and 85% losses on cereals, legumes, and pastures respectively were recorded, affecting more than 8 million people (Brader et al., 2006; Renier et al., 2015). Damages to crops alone were estimated at $2.5 billion USD and $450 million USD was required to bring an end to the upsurge (ASU, 2020). Over 25 million people in 23 countries were affected during the most recent 2019-2021 upsurge and damages were estimated to reach $1.3 billion (Green, 2022), with control efforts—including treating over 2 million hectares with pesticides—estimated to have prevented over $1 billion in damages (Newsom et al., 2021).

An important result of the local variation in locust swarm damages during outbreaks is that macro level impacts may be muted, since outbreaks occur in periods of positive rainfall shocks which tend to increase agricultural production in unaffected areas. Several studies find that impacts of locust outbreaks on national agricultural output and on prices are minimal, despite devastating losses in affected areas (Joffe, 2001; Krall and Herok, 1997; Showler, 2019; Thomson and Miers, 2002; Zhang et al., 2019). Chatterjee (2022) finds that wheat yields are 12% lower on average in Indian districts typically affected by desert locusts in years of locust outbreaks, in contrast to very large decreases in the specific areas exposed to locust swarms in those years.

Farmers have no proven effective recourse when faced with the arrival of a locust swarm, though activities such as setting fires, placing nets on crops, and making noise are commonly attempted. While these may slow damage they have little effect on locust population or total damages (Dobson, 2001; Hardeweg, 2001; Thomson and Miers, 2002). Desert locusts live 2-6 months and swarms continue breeding and migrating until dying out due to a combination of migration to unfavorable habitats, failure of seasonal rains in breeding areas, and control operations (Symmons and Cressman, 2001). The only current viable method of swarm control is direct air or ground spraying with pesticides (Cressman and Ferrand, 2021). These control operations do not prevent immediate agricultural destruction as they take some time to kill the targeted locusts, but will limit their spread. The 2003-2005 locust upsurge ended due to lack of rain and colder temperatures which slowed down the breeding cycle, combined with intensive ground and aerial spraying operations which treated over 130,000 km^2 at a cost of over US$400 million (FAO and WMO 2016).

Desert locust control is most effective before locust populations surge, and the FAO manages an international network of early monitoring, warning, and prevention systems in support of this goal (Zhang et al., 2019). While improvements in desert locust management have been largely effective in reducing the frequency of outbreaks (as seen in Figure C2), many challenges remain. Desert locust breeding areas are widespread and often in remote or insecure areas. Small breeding groups are easy to miss by monitors, and swarms can migrate quickly. In addition, control operations are slow and costly, resources for monitoring and control are limited outside of upsurges, and the cross-country nature of the thread creates coordination issues. Insecurity may also limit locust control activities (Showler and Lecoq, 2021).