







THE VALUE OF MONITORING FOR DISASTER PREVENTION: THE DESERT LOCUST*

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Abstract

Monitoring systems for diseases and pests are active in most countries, detecting early signs of potentially disastrous outbreaks in time for preventative action. These monitoring systems are costly, however, and identifying their economic value requires estimating damages from outbreaks in empirical settings where monitoring is neither uniform nor exogenous. We estimate the value of monitoring systems for the desert locust—known to devour entire agricultural fields—and their impact on human well-being. Our analysis uses data from 1985 to 2020 across Sub-Saharan Africa, the Middle East and South Asia, including locust monitoring records and data from the Demographic and Health Surveys. We leverage conflict and weather events in locust breeding areas to detect the effects of monitoring interruptions on locust swarm outbreaks. We then reconstruct the spatial patterns of locust migrations to propagate these effects on locust swarm outbreaks beyond breeding areas. Finally, we show that *in-utero* exposure to a locust swarm reduces height-for-age by 0.36 standard deviations and increases the probability of stunting by 7 percentage points (a 16% increase). Taken together, these estimates allow us to quantify the effects of a change in monitoring effort on subsequent locust swarms and on human health. We calculate that in the absence of effective locust monitoring, an additional 238 thousand children per birth cohort in the affected countries would experience stunting, creating economic losses of US\$11.9 billion per year. This implies a benefit-cost ratio of over 300 for locust monitoring budgets.

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*“O locusts, if you
Are seeking a place to winter,
You can find shelter in my heart.”*

—Hsu Chao c.1200

1 Introduction

Disaster prevention relies on two essential components: protective investments to mitigate or adapt to disaster impacts, and the ability to monitor conditions in order to target those investments. For some disasters, monitoring may be achieved by a passive system of *in situ* instruments, while for others, persistent and costly surveillance efforts must be maintained. Examples of the latter are monitoring systems for human, plant and animal diseases and pests, which are active in most countries. These systems aim to detect potentially disastrous outbreaks early, allowing for preventative action on containment. These monitoring systems are costly, however. For example, the Global Polio Eradication Initiative’s surveillance costs exceed \$79 million per year (Duintjer Tebbens et al. 2019), and governments pledged \$418 million in 2006 to strengthen early warning systems for avian influenza (Roberts 2006). Measuring the economic value of these investments in monitoring requires estimating the damage from outbreaks when monitoring fails, in empirical settings where monitoring is neither uniform nor exogenous, particularly in developing countries. This problem is even more difficult in the case where monitoring succeeds, since the challenge is to identify the costs of the counterfactual outbreak that never occurred.

This paper focuses on the desert locust, one of the most destructive agricultural pests across Central and North Africa, the Middle East, and South Asia (Zhang et al. 2019). Locusts live primarily in low population density groups in remote desert areas, far from human activity. They periodically enter a frenzied stage of reproduction and feeding in gigantic swarms, through a process called “gregarization.” Their regional footprint is characterized by swings between “recession” years—during which they live in their solitary phase in the desert—and “plague” years. During “plague” years, traveling swarms leave breeding areas and migrate across vast regions, consuming almost all vegetation in their path, decimating crops, and posing a major threat to food security. A typical swarm could number as many as five billion individual locusts, cover an area of 100 km² (roughly 70% larger than Manhattan), and consume the same amount of crops each day as seven

to eight million people (Latchininsky 2013; Roussi 2020; Spinage 2012).¹

Efforts in locust surveillance and control have varied, as have the frequency of plagues. In the middle of the 20th century, the Anti-Locust Research Centre was created (Uvarov 1943; Waloff and Popov 1990; Yates 2019), beginning a systematic effort to observe, understand, record, and eventually control locust plagues, thus protecting the food supply of millions of people in the historical plague areas. Two important milestones occurred in the middle of the century—the advent of modern pesticides and the use of planes for airborne spraying (MacCuaig 1958; Gunn 1960). Preventative measures, occurring *before* locusts form swarms or move to areas populated by humans, had a dramatic effect on the prevalence and frequency of locust plagues. Between 1861 and 1960 there were seven major locust plagues, some lasting decades, with only 22 recession years during the 100 year period. Between 1961 and 2000, there were only two plagues and six upsurges, none lasting longer than 4 years with 31 recession years out of the 40 year period. Due to a combination of pesticide resistance, waning international effort (Roussi 2020), and changing weather patterns due to climate variability and climate change (Salih et al. 2020), a number of devastating plagues and upsurges have continued to occur, roughly once per decade, including a major upsurge in 2019-2021. Management and monitoring of locust outbreaks ultimately passed to the Food and Agriculture Organization (FAO), which maintains a regional observational network. Despite the vast threat to the food supply of millions of people, the socioeconomic effects of locusts, as opposed to their biology and control methods, have not been a major topic of research, in contrast to other drivers of food insecurity and famines.²

In this paper, we document the value of locust monitoring systems, which combine surveillance and preventative control actions, using 36 years of locust monitoring data. This context poses three key challenges: (1) locust monitoring is endogenous to locust conditions and monitoring capacity, (2) locust monitoring and impacts are connected over large distances, as the activities of locusts in core breeding areas, far removed from centers of agricultural activity, have consequences in distant locations through the development of swarms and their migration out of breeding areas, and (3) reporting of monitoring efforts is likely more incomplete in impact areas relative to breeding areas.

¹ Estimates suggest that in a 100 km² area, a locust swarm would consume approximately 10,000 tons of vegetation per day, on average. We assume that a person eats about 1.3 kg of food per day.

² In addition to the seminal work by Sen (1982), more recent work has examined the Great Famine in China (Meng et al. 2015), the potato famine in Ireland (O’Rourke 1994), the era of famines in India (Burgess and Donaldson 2010), and how famines in general are becoming less frequent (Gráda 2007).

We address these challenges as follows. First, we exploit conflict and weather as shifters of locust monitoring effort in breeding areas, to document the impact of changes in monitoring effort on locust prevalence. Second, we document how these monitoring failures might lead to locust swarm outbreaks that spread outside of breeding areas by identifying locust migration patterns over space. Third, we estimate the effect of resulting swarm outbreaks on human health, using reported swarm dynamics in breeding areas to overcome non-classical measurement error in the locust data. Overall, we document how monitoring operations affect swarm outbreaks, how locust swarms spread over space, and how swarm outbreaks affect human health in populated areas.

We begin by documenting a discontinuity in monitoring effort across country borders, demonstrating that locust monitoring is incomplete and that there are systematic patterns underlying variation in monitoring data. We then show that the onset of conflict in locust breeding areas reduces the likelihood of locust monitoring by 3 percentage points (from an average of 28% state-month observations benefiting from monitoring). Despite this suppression of monitoring, to which locust data recording is tied, conflict that occurs during a year of good rainfall results in a 6.4 percentage point higher occurrence of recorded swarms (over a baseline of 3.8% of state-month observations documented as having a locust swarm present). This conflict-induced monitoring interruption also leads to subsequent increases in swarm likelihood in other countries (specifically, areas in other countries but within the same breeding areas experience a 0.3 percentage point increase in swarm likelihood, over the 3.8% baseline).

Next, we use a LASSO to detect the stable spatiotemporal connections between locust breeding areas and the affected regions we study. This allows us to predict locust presence outside of breeding areas along migration pathways, which allows us to infer locust presence without relying on imperfect monitoring data. For the next stage of our analysis, this gets around two empirical issues that would result from simply estimating the association between monitoring, locusts, and human health in a given location. First, monitoring effort itself is likely endogenous to weather and social processes that affect human health, resulting in nonrandom missing observations. Secondly, given that locust presence is partly determined by local weather, the association between locusts and human health is confounded by multiple other channels (including infectious disease) through which weather can drive changes in human health.

We combine these locust presence data with survey data from 50 Demographic and Health

Surveys in 16 locust-affected countries over 27 years to yield a dataset of 416,890 child observations. We estimate outcomes of locust plagues on children’s height-for-age Z-scores (HAZ), the most reliable measure of longer-term nutritional deficits that can be measured through household surveys (Onis and Branca 2016). Children exposed to swarms while in-utero are 0.36 standard deviations shorter and 16% more likely to suffer from stunting. The large magnitude of these effects is consistent with the declines seen during acute food emergencies or famines.

Taken together, our estimates allow us to quantify the effects of a change in monitoring effort on subsequent locust swarms and on human health. We calculate that in the absence of effective locust monitoring, the affected countries would experience an increased stunting rate of 0.6 percentage points (an additional 238 thousand children per birth cohort in 2020) and a decrease of 0.05% of GDP (US\$11.9 billion per year). This implies a benefit-cost ratio of over 300 for the FAO’s locust monitoring budget.

Our primary contribution is to the literature on the economics of disaster prevention, mitigation, and adaptation. Most studies in this literature have focused on estimating the economic or social costs of disasters (e.g., Anttila-Hughes and Hsiang 2013; Hsiang and Jina 2014; Rosales-Rueda 2018) or understanding to what extent insurance can mitigate losses from disasters (e.g., Carter et al. 2014). Most closely related to this study are a small number of papers that estimate the value of preventative actions (Davlasheridze et al. 2017) or disaster control (Baylis, Boomhower, et al. 2023), or the value of weather or hurricane forecasts (Molina and Rudik 2022; Burlig et al. 2024; Shrader et al. 2023). We contribute to this literature by studying a problem where monitoring is costly and endogenous. As we note above, a wide class of problems share this feature, but previous evidence on the value of such systems is limited to modelling exercises (Roberts et al. 2009; Herida et al. 2016). We provide the first causal estimate of the value of monitoring for disaster prevention.

Our study also relates to a small but growing literature on the social and economic consequences of plant pests, and specifically, locusts. A few studies evaluate the impacts of locusts on child health, either for single countries or epidemics (Conte et al. 2023; Linnros 2017) or, most similar to this study, for all countries with georeferenced anthropometric survey data (Le and Nguyen 2022), or focusing specifically on infant mortality (He and Lam 2024). These studies largely corroborate the effects on child health we discuss in the final stage of our analysis. Related, Banerjee et al. (2010) find similar results in a historical context from the Grape phylloxera, a plant pest that destroyed

grapevines in late 19th century France. These adverse shocks to income and nutrition during infancy and early childhood (Almond and Currie 2011) can also have far-reaching consequences later in life (De Vreyer et al. 2015; Baker et al. 2020). Our results complement those of Marending and Tripodi (2023), who rely on wind direction to predict the migration paths of locusts in Ethiopia, confirming that locusts affect agricultural outcomes and, specifically, farmer’s profits. Other studies also document the negative correlation between conflict and records of locust swarms in the FAO data (Torngren Wartin 2018; Biscaye 2023).

The rest of the paper proceeds as follows. Section 2 discusses the biology of locusts and the history of monitoring and control. Section 3 details the locust data and other data sources used in this paper. Section 4 describes how the locust data suffers from systematic measurement error in the cross-section. Section 5 documents the relationship between surveillance and control activities and the likelihood of locust outbreaks. Section 6 describes how locust outbreaks propagate across space. Section 7 describes two approaches to analyse the impact of locust swarms on human well-being, and presents the results from these analyses. Section 8 provides a calculation of the value of locust monitoring activities. Section 9 concludes.

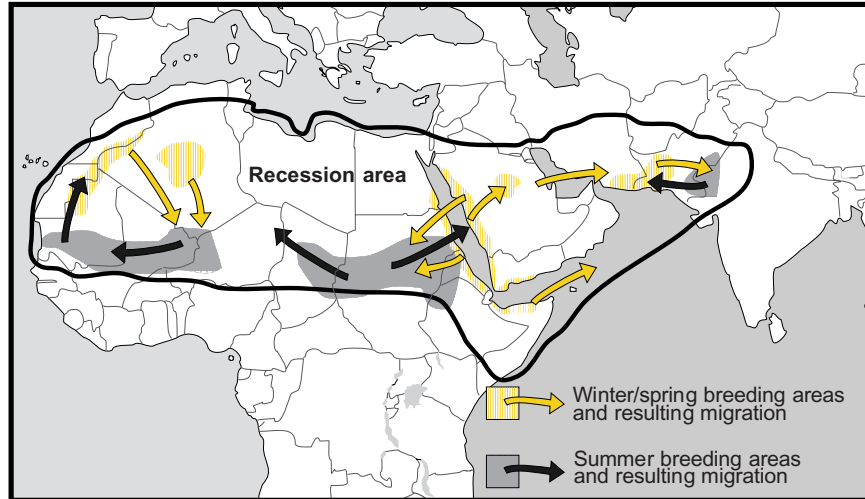
2 The Cycles of Locusts Infestations

Locusts are short-horned grasshoppers of the insect order Orthoptera. There are about a dozen species of locusts, and what distinguishes them from the thousands of other grasshopper species, is that they exhibit density-dependent changes in behavior and morphology. At low population densities, they adopt a solitary and sedentary behavior. At high population densities, they become gregarious, and form voracious swarms that can travel fast and far (Latchininsky 2013).

The journey of the desert locust, *Schistocerca Gregaria*, starts in arid and semi-arid areas contained in a narrow band north of the Equator, going from the Atlantic Ocean to Northern India (see Figure 1). Breeding occurs in bare sandy soil that is sufficiently wet. Once eggs hatch, desert locusts pass through a series of stages, from wingless young called hoppers, to flying adults that eventually mature and breed (Symmons and Cressman 2001a). They are polyphagous i.e. can consume many types of vegetation, including all agricultural crops (Latchininsky 2013).

When the right conditions form—typically an amount of rain that allows for intense breeding,

Figure 1: Locust breeding areas during recession years



Notes: Map shows the desert locust's summer (grey) and winter/spring (yellow) breeding areas. Arrows indicate corresponding migration patterns. Thick black line outlines the recession area (typical habitat). Source: FAO. Reproduced from Symmons and Cressman (2001a).

followed by a decrease in rain that suppresses vegetation—locusts find themselves to be in high numbers, and increasingly crowding around isolated vegetation clumps.³ This increased proximity triggers their transition from a solitary phase to a gregarious phase (Sword et al. 2010): hoppers form marching bands, which are particularly voracious and, upon reaching the adult stage, convert to flying swarms that migrate at a rapid pace.

Swarms vary in size and density: the average density of a settled swarm is about 50 million locusts per km², but this figure can vary between 20 million and 150 million (Symmons and Cressman 2001b); in flight, swarming locusts tend to spread out, forming a cloud that can extend over 1,000 km² (Spinage 2012). Once a swarm forms, it travels every day during the daytime, under reasonable temperature and wind conditions. With a flight speed ranging between 16 and 19 km/hour, and a flight duration of 9-20 hours, a swarm can easily move 100 km or more during a single day. Locust swarms usually travel downwind, at an average speed that is slightly lower than the actual wind speed (Cressman and Stefanski 2016). This downwind displacement tends to bring them into zones of convergence where rain is generated, which provides favorable conditions for feeding and breeding. Locusts do not always follow the prevailing winds: for example, migration after summer breeding in Western Africa follows the rarer and warmer southerly winds³ that take them to North

³ Rainfall over 25 mm in two consecutive months is usually assumed to be enough for locust breeding and development (Cressman and Stefanski 2016).

Africa (Cressman and Stefanski 2016). Overall, broad seasonal migration patterns are relatively predictable, while predicting the precise location of a swarm requires more detailed information on wind, weather, and soil moisture conditions.

The devastation swarms have brought about has been documented for millennia, but looking back on more recent plagues—a plague is “a period of one or more years of widespread and heavy locust infestation” (FAO 2001)—enables to better grasp the extent of the damage locust bands and swarms can cause. For instance, Foster (2015) argues that the famine that hit Syria during World War I and claimed the lives of half a million people (out of 3-4 million inhabitants) was in part caused by a particularly destructive locust infestation. Over the course of 6-7 months, it is argued, locusts devoured at least 536,000 tons of food, which amounted for the winter 1915 harvest to a 10-15% reduction in production, and for the summer-fall 1915 harvest to a catastrophic 60-100% drop. Indeed, current FAO estimates suggest an adult locust can consume 2 grams of fresh food (about its own weight) per day, which for a 40 million locust-strong swarm (about 1 km² wide) amounts to 80 metric tons of food per day, i.e., as much as the daily consumption of 35,000 people (FAO 2015). The extent of damage from a locust plague is scale-dependent: total crop losses may be negligible at the scale of the economy, but the impact is usually disastrous for individual farmers or cooperatives (Latchininsky 2013).

Until the 1960s, plagues were frequent and long. Based on FAO data spanning 1910-2006, Joyce I. Magor et al. (2008) observe that plague conditions were affecting some part of the desert locust range on average four years out of five, with each plague lasting at least 8 years. After 1965 and the deployment of locust preventive control strategies by the newly created FAO, plagues would be shorter (two years at most), leaving more years of locust recession (“period without widespread and heavy infestations by swarms,” Cressman and Dobson (2001)) or swarm-free between consecutive plagues. They also note that the spatial expanse of plagues has been much reduced since the 1960s.

Desert locust swarms are known to have immense destructive capacity, but research on their impact on agricultural outcomes has been limited partly because of the lack of consistently measured crop yield data across time and space. In a survey that took place after the 2020 plague in Ethiopia, close to 60% of the households reported observing desert locust swarms on their farm, of which 18% reported complete crop destruction by desert locusts (Ilukor and Gourlay 2021). Another recent study by Chatterjee (2022) documents declines in wheat production in India following locust

swarms. A few papers have successfully been able to detect locust crop damage (for the oriental locust) using measures of greenness such as NDVI (normalized difference vegetation index) and LAI (leaf area index) (Tian et al. 2008; Ma et al. 2005).

3 Data

The primary goals of the analysis are to study how monitoring and control operations affect swarm outbreaks, and study how swarm outbreaks affect the well-being of exposed human populations. In particular, we use armed conflict as a shifter of monitoring and control operations to overcome their endogeneity, and we develop a proxy for the presence of locust swarms in populated areas to overcome non-classical measurement error in locust reporting. To achieve this, we collect data on locust reports, weather, armed conflict incidence, and repeated cross-sectional surveys of the exposed populations. In what follows, we summarize each data source.

3.1 Locust Monitoring Data

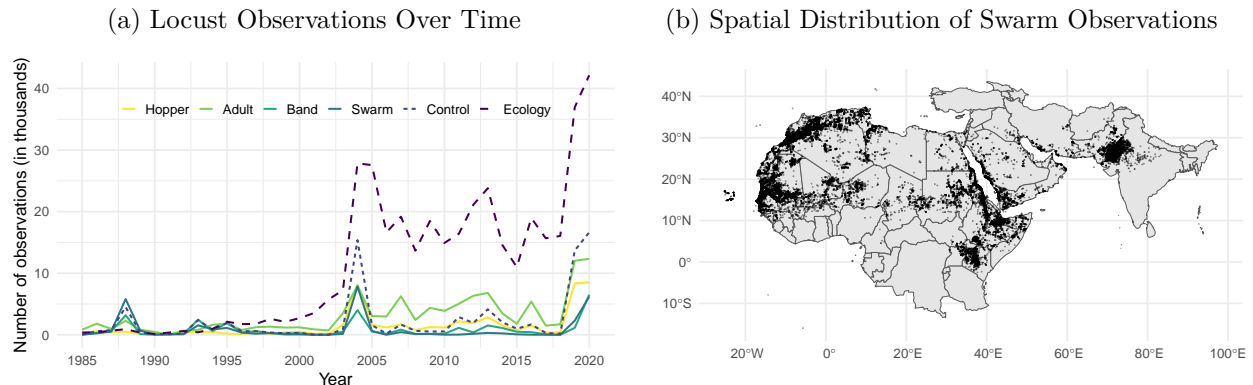
Locust monitoring data comes from the FAO Locust Hub website. This dataset records sightings of four categories of locusts (hoppers, bands, adults, and swarms), as well as locust control operations, and ecology observations on vegetation and soil moisture conditions.⁴ The data are collected by experienced locust officers performing survey and control operations, additional field staff, and can also come from local self-reporting. It is collected in real-time via GPS-connected devices, or in near real-time, especially for earlier records entered manually and later shared via radio, fax, or email. Records are shared with national locust centers, and then centralized and validated by the FAO’s Desert Locust Information Service.

Locust monitoring data started in 1985, and we exploit records made until 2020. Figure 2a plots the annual count of locust reporting events, by record type, for the duration of the sample (1985-2020). Overall, the most frequent type of record is ecology observations: out of all reporting events (unique date and location when at least one record is made), 88% include observations on ecology. This is followed by adult locust observations (25%) and control operations (18%), while

⁴Hoppers are “Desert Locust solitarious hoppers (wingless nymphs)”, bands are “Desert Locust hoppers (wingless nymphs) that form a band”, adults are “any type or maturity of Desert Locust adults that do not form a group or concentration”, and swarms are “any maturity of Desert Locust adults that form a swarm” (FAO).

swarm observations are made 7% of the time. Figure 2b shows the spatial extent of swarm reporting events. Out of all reporting events that include swarm observations, 62% also include a control observation. In general, swarm observations tend to be more frequently associated with control observations over breeding areas.

Figure 2: Spatiotemporal Coverage of Data in the FAO Locust Hub



Notes: (a) Locust observations by year, and by record type. (b) Each dot represents a single locust swarm observation. Source: Food and Agriculture Organization Locust Hub.

3.2 Locust Breeding Areas

At times of low densities (recession years), locust breeding is restricted to “breeding areas”, extending from Mauritania and southern Morocco eastwards through Arabia to the Rajasthan Desert in India (J. Magor et al. 2008, also see Figure 1). These areas are typically where swarm outbreaks first originate. Accordingly, breeding areas have been the focus of much preventive surveillance and control efforts, in an attempt to limit locust population increases early on in the gregarization process (Piou et al. 2017).

Some of our analyses exploit locust and monitoring dynamics in breeding areas, which requires outlining their boundaries. In order to identify breeding areas at high resolution and in a way that reflects locust ecology in our study time period, we use a statistical approach that relies on the locust monitoring data, and confirm that the spatial pattern we obtain matches with coarser historical maps like that provided by the FAO (Figure 1). Specifically, we run a spatial cluster analysis on breeding locust observations made in recession years—that is, the 29 years in the 1985-2020 time period that are not major outbreak years.⁵ This helps exclude breeding along swarm

⁵ Excluded years are 1987, 1988, 2003, 2004, 2018, 2019, and 2020.

migration routes, which is likely to be more intensively observed and reported in major outbreak years. Breeding areas are seasonal, meaning that one area might be active in the summer, while another is active in the spring. Following Symmons and Cressman (2001a), we split the year into three breeding seasons: spring (February-May), summer (June-September), and winter (October-January), and apply spatial clustering to each season separately.

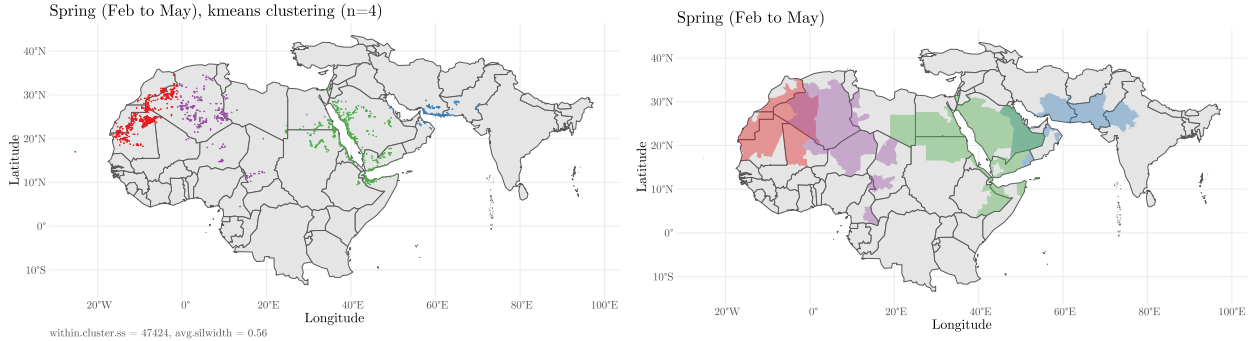
We start by evaluating breeding intensity by location and season. To overcome spatio-temporal variability in reporting, we split our study region into a 0.25 degree grid, and classify each grid cell as a “breeding” location in a particular season and year if any observation of breeding locusts falls within that grid cell and season-year. We then calculate, for each each grid cell and breeding season, the proportion of recession years when breeding occurs. This gives us a measure of breeding intensity that varies by grid cell and season. We run multiple clustering algorithms (kmeans, dbSCAN, hdbSCAN, and hclust), which cluster grid cells based on latitude, longitude, and breeding intensity, and vary their parameter values across realistic ranges. For each season, we choose the clustering result that minimizes within-cluster distance and maximizes distance between clusters, and we visually check that the clusters (i.e. breeding areas) obtained match patterns documented by the FAO. For example, Figure 3a shows spring season breeding areas, obtained by using the kmeans algorithm with four clusters. The best summer clusters are obtained with the kmeans or dbSCAN algorithms and winter clusters with the hdbSCAN algorithm (Figure A1). We sometimes need to restrict analyses to breeding areas using administrative boundaries. Figure 3b shows a spatial envelope of the spring breeding areas obtained using admin 1 boundaries (see Figure A1 for summer and winter versions).

3.3 Weather Data

Weather data comes from ERA5, a reanalysis for global climate and weather from European Centre for Medium-Range Weather Forecasts (ECWMF)—the fifth generation reanalysis data product of the global climate. This dataset is considered to be one of the most accurate reanalysis weather datasets available, which is of particular importance in our geographic setting, where *in situ* weather monitoring is relatively rare. We use the hourly estimates on precipitation, maximum, and minimum of two meters temperature. We calculate the mean of two meters temperature from the maximum and minimum values. We transform the hourly data on precipitation and mean of two

Figure 3: Outlining Locust Spring Breeding Areas

(a) Best Clustering Output for the Spring Season (b) Admin-1 Envelope Applied to Spring Clusters



Notes: See main text for details.

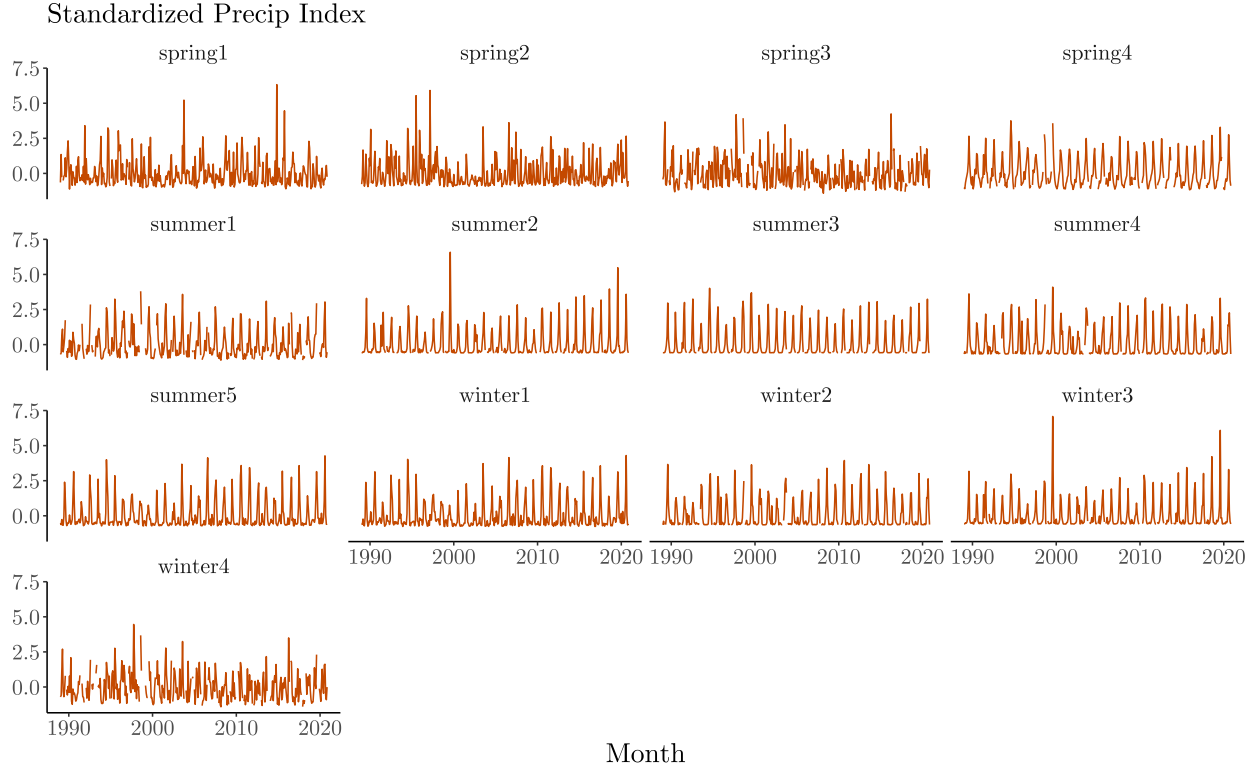
meters temperature into daily weather data. We use these data to create a local weather time series for each DHS cluster (see Appendix B for more details). In Figure 4, we plot the standardized precipitation values for the monthly time series in each breeding area. There is very strong seasonality in rainfall patterns, with long dry periods and bursts of rain, as well as variability across years—with some years experiencing very sharp precipitation shocks.

3.4 Conflict Data

Our conflict data comes from the Uppsala Conflict Data Program (UCDP). The UCDP provides a global geocoded dataset that contains information about conflict events from 1989 to the present. Each observation is an event where there was use of force, either by an organized actor against another organized actor or against civilians, that resulted in at least one death. The UCDP data is more consistently coded and has more accurate geographical information compared to other data sources on conflict with global coverage (Eck 2012). In Figure A2, we map the total incidence of conflict events across 5-year time intervals. There is variation in conflict incidence and intensity over time and space in our sample. There is also overlap between conflict locations and breeding areas at the first administrative unit level.

We aggregate the UCDP data to create a monthly panel at the subnational administrative unit level. An outstanding issue is that multiple events in the data pertain to the same ongoing conflict, meaning that we need some definition of the start, end, and duration of a conflict. We follow previous work by Miguel et al. (2004) and Ge et al. (2022) and define the *onset* of a conflict as any

Figure 4: Variation in Precipitation Across Locust Breeding Areas



Notes: We plot the monthly time series of standardized precipitation value for each of the breeding areas. We standardize each precipitation time series separately.

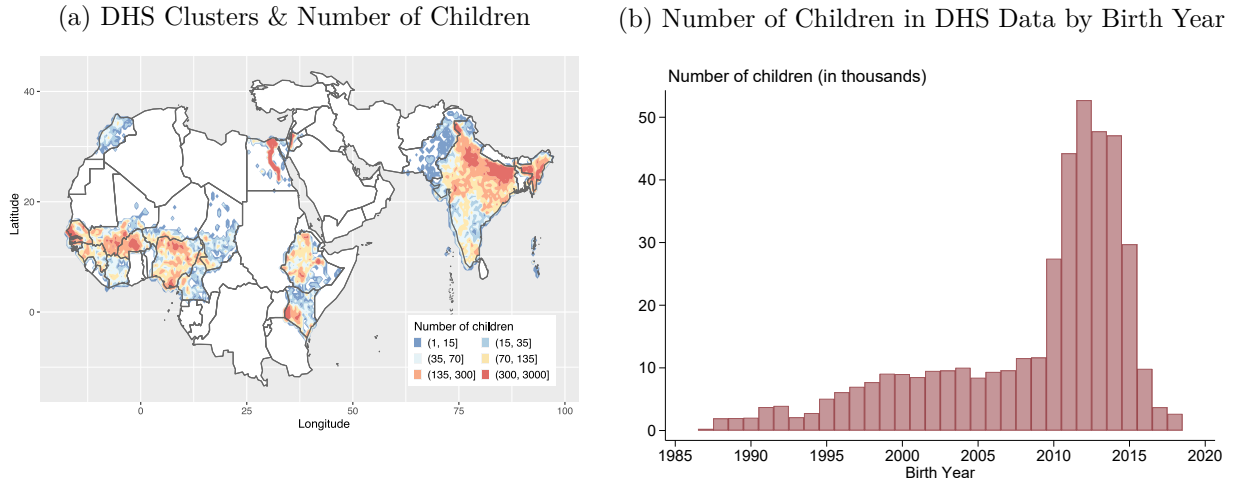
time period in a subnational region where a conflict event occurs after 12 months of no recorded conflict event. We define a conflict ending, the *offset*, as a time period in a subnational region that had at least one conflict event in the past 12 months, but where there is no event in the 12 following months. We also construct a dummy variable that captures ongoing conflict, that equals one if a conflict started in the subnational region at least 12 months ago that has still not ended.

3.5 Household Survey Data

Microdata on children’s health are from the Demographic and Health Surveys (DHS). We use all standard DHS surveys from countries affected by locust for which children’s anthropometric data are available, generating a sample of 416,890 child-level observations in 50 surveys from 16 countries between 1992 and 2018. We calculate each child’s anthropometric status using their height and weight following the NCHS/CDC/WHO International Reference Standard (De Haen et al. 2011) intended to provide a single measure of child nutritional outcomes comparable across ages and

sex-at-birth. We then exclude from the analysis a small number of observations (2.6%) that are flagged for having improbable height-for-age z-scores values.⁶

Figure 5: Spatiotemporal Coverage of Data in the DHS



Notes: (a) Map of the location of the DHS clusters, with colors denoting the number of children surveyed in each cluster. (b) The number of children surveyed across all the DHS surveys in the sample by birth year.

Source: Demographic Health Surveys.

4 Heterogeneity in Monitoring Across Country Borders

Like many non-automated and costly surveillance systems, locust monitoring is incomplete: this means that the absence of a locust observation in a particular place and time can either reflect low surveillance or reporting effort or the actual absence of locust activity. In this section, we provide suggestive evidence of endogenous variation in the locust monitoring data.

The nature of the locust monitoring system suggests that monitoring is likely endogenous to surveillance willingness (itself likely tied to suspected locust conditions) and capacity. While the FAO provides training on locust control and assists countries with equipment, resources, and technical support during outbreaks and plagues, locust surveillance is primarily the responsibility of national governments. Much of the locust monitoring data collection process thus depends on the willingness and capability of governments to carry out monitoring activities. In particular, countries of varying state capacity likely exert different levels of monitoring.

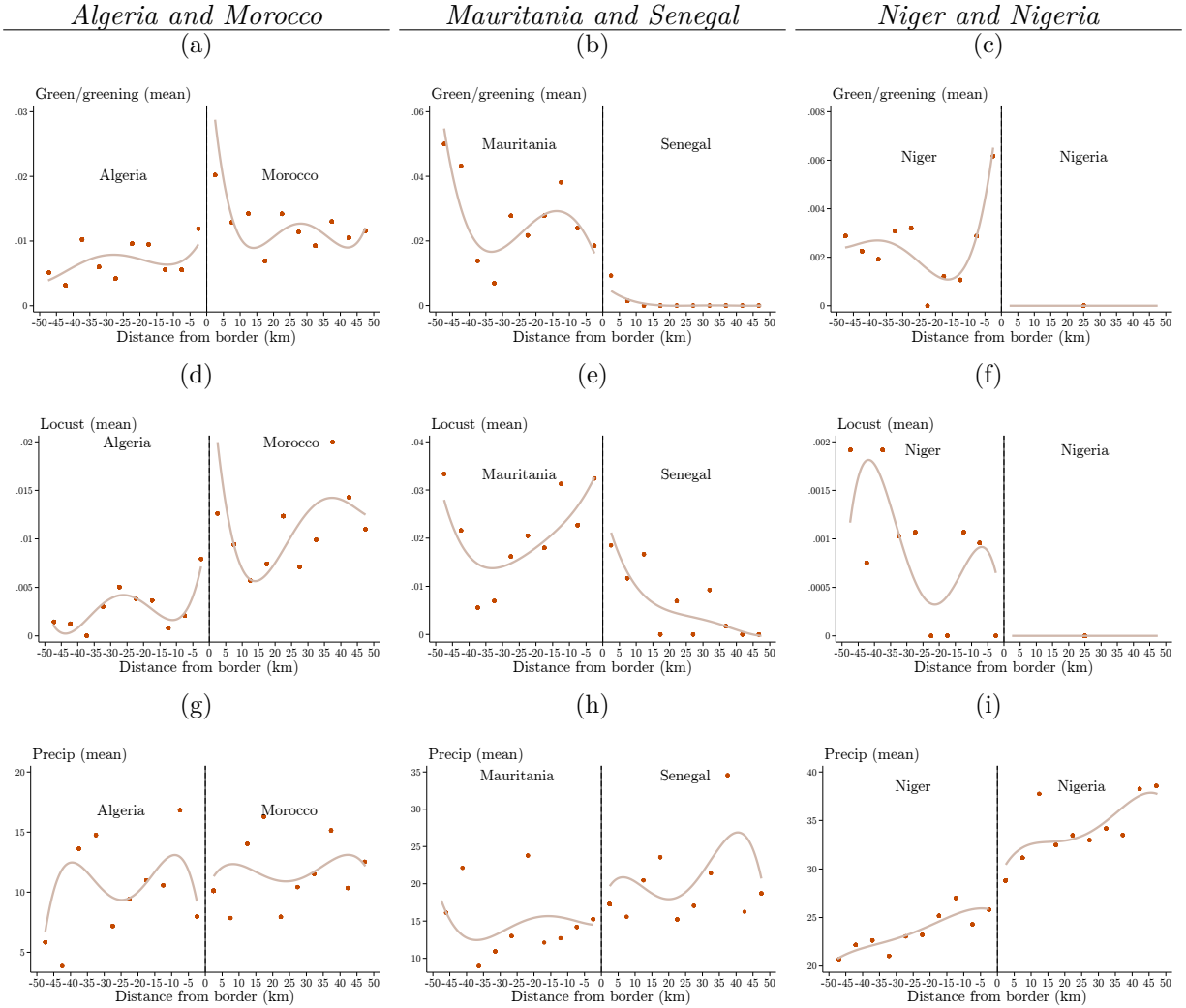
We first illustrate the likely endogeneity of monitoring data to state capacity, by documenting

⁶ As per DHS guidelines, height-for-age z-score with an absolute value larger than 6 are considered invalid.

how monitoring effort and locust detection can change sharply across national borders. We use a spatial regression discontinuity design approach on three outcomes: green or greening observations, which proxy for monitoring effort, locust observations, and precipitation. In Figure 6, we show graphically how each outcome changes across three pairs of country borders in western, central and eastern Africa. Given that environmental conditions are unlikely to change abruptly at country borders, and that locust swarms transcend national boundaries, we would expect the number of locust observations to vary smoothly across the boundary—especially over small distances where locust conditions are likely to remain stable. Despite very large confidence intervals, the discontinuous jumps in the reporting of green or greening conditions, and the count of locusts close to national borders, combined with the absence of discontinuous jumps for precipitation, point to differences in monitoring efforts across countries. The border between Niger and Nigeria captures an extreme case where there is effectively no reporting around the border inside Nigeria.

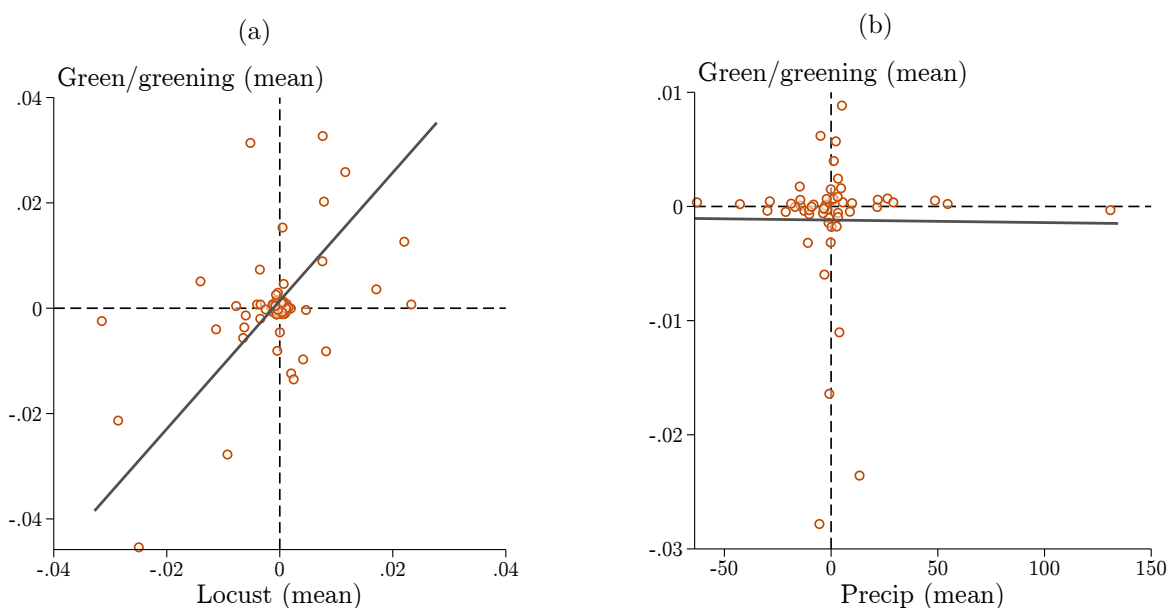
Following the descriptive evidence on sharp changes at the borders between countries, we estimate the magnitude of these border discontinuities for all country pairs in our study region. In Figure 7, we plot regression discontinuity coefficients corresponding to green/greening observations for each country pair, either against regression discontinuity coefficients corresponding to locust observations, or precipitation. This general exercise recovers the same notion as the descriptive plots above: there are sharp discontinuities in green/greening observations at many national borders, denoting sharp changes in monitoring effort across countries. In addition, countries that make fewer reports about environmental conditions are also those that report locust less frequently, which highlights how endogenous monitoring affects locust observations. In contrast, with respect to precipitation, there is no similar relationship. Any differences in precipitation across country border fail to predict reported differences in green/greening conditions. Perhaps more importantly, for cases where there are virtually no differences in precipitation (values around zero on the x-axis in Figure 7b), there is a wide spread in the differences in reporting of green/greening conditions—capturing variation in effort even when environmental conditions are highly similar. Overall, this section documents evidence of endogeneity in reporting effort and locust observations. In the next section, we document that in addition to this cross-sectional variation in monitoring effort, we also observe monitoring suppression following armed conflict events.

Figure 6: Descriptive Evidence for Sharp Changes in Monitoring Effort Across Country Borders



Notes: We first calculate whether there is any green/greening observation, and any locust observation in every year and month across a 0.05° grid. Similarly, we calculate monthly total precipitation across a 0.25° grid. If a grid cell resides in multiple countries, we run a separate calculation on every country fraction and weigh the result by the fraction size. We then use the share of year-months when a green/greening report is made as the RDD outcome (first row), the share of year-months when any locust report is made (second row), or the average of monthly total precipitation (third row). The distance between each grid cell and the border is calculated using the grid cell centroid. The regression discontinuity design plots above use 5 km bins and 4th-degree polynomial curves.

Figure 7: RDD Coefficients Across All Country Pairs



Notes: Each dot shows an RDD coefficient estimated at a border of two countries for the green or greening reports (y-axis) versus for any locust report (x-axis). A three-percent jitter is applied to all points to make it easier to observe the mass of points around (0,0). In panel 7a, one outlier at (0.028, 0.138) is excluded for visual clarity. We see that locust reporting declines sharply across country border when the reporting on environmental conditions—which proxies for reporting effort—drops sharply as well. In panel 7b, sharp changes in environmental conditions do not correlate with actual environmental discontinuities, as shown by precipitation on the x-axis.

5 Local Relationship Between Surveillance, Control & Swarm Outbreaks

In this section, we document the relationship between monitoring activities and the likelihood of locust outbreaks. The key challenge in estimating how monitoring operations affect locusts is that the surveillance part of monitoring is likely endogenous to the expected level of locusts, which would increase following heavy rains that make conditions more favorable to swarm outbreaks. Similarly, control operations likely respond to observed locust conditions, but observing realized locust levels relies on surveillance efforts. To evaluate the effectiveness of locust surveillance and control efforts requires a quasi-experimental shifter. In what follows, we use the term “monitoring” to refer to surveillance operations (excluding control), and thus refer thereafter to the impact of conflict on “monitoring and control”.

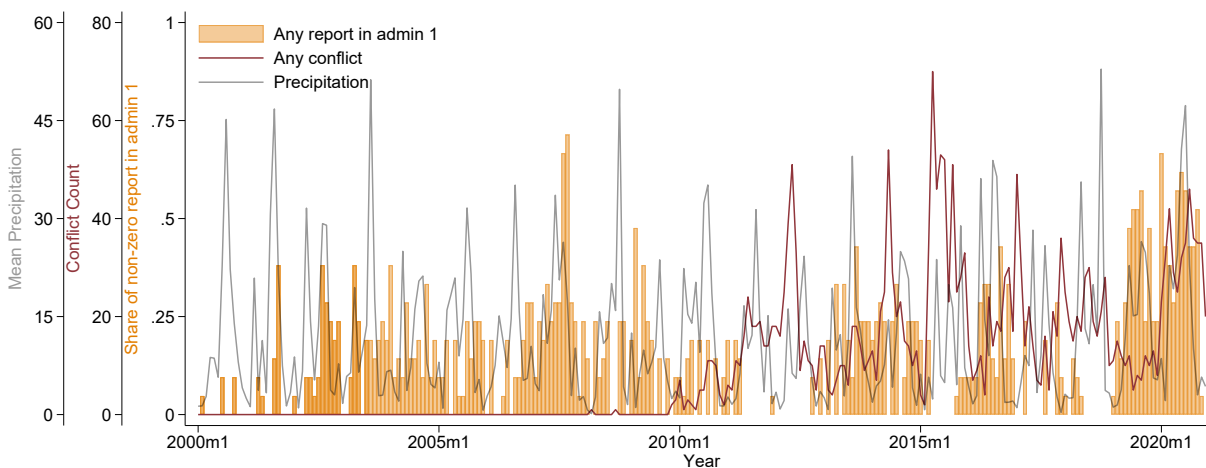
5.1 Descriptive Statistics Tying Armed Conflict to Locust Monitoring

As we document below, armed violent conflict suppresses monitoring and control operations. Our empirical approach uses the fact that: (i) conflict interrupts monitoring and control; and (ii) interruption of monitoring and control only matters when weather conditions are right for locust swarms to form. We then show that recent conflict events that occur when precipitation levels are high result in excess swarms. We focus on the interaction of the conflict and precipitation shocks, while controlling separately for the direct effects of precipitation and any direct effects of conflict on monitoring or (perhaps less plausible) on locusts themselves.

We motivate this approach by documenting the most recent outbreak of locusts in 2019, which has been attributed to monitoring failures caused by the onset of civil war in Yemen in 2014 (Showler and Lecoq 2021)—although conflict erupted even before the official onset of 2014 (Gros et al. 2015). In Figure 8, we plot the national-level time series for precipitation, conflict, and locust monitoring. Two key insights emerge from this descriptive figure. First, before 2010, when conflict levels were effectively at zero, there were no long lulls in reporting of locust activity, yet after 2010, we notice three periods in which *none* of the admin-1 units reported any monitoring activity. The second takeaway from this figure is that these three periods of suppressed reporting are correlated with large spikes in conflict counts. The monitoring lulls do not appear to be explained by drought

conditions, which could have explained why no monitoring occurred, as precipitation levels appear to follow similar seasonality as before. In what follows, we examine whether this pattern is more widely observed throughout our sample.

Figure 8: Conflict and Monitoring in Yemen Following the Onset of Civil War



Notes: Summarizing precipitation, conflict, and locust reporting across admin-1 units in Yemen around the onset of the civil war. The three national-level time series show that while high precipitation levels did occur after conflict counts increased, locust monitoring declined sharply during periods in which conflict activity spiked—consistent with the recent literature that attributes the 2019 locust outbreak to the monitoring failures linked to the civil war in Yemen (Showler and Lecoq 2021).

5.2 Regressing Locust Monitoring & Control Operations on Local Conflict

Our analysis begins with quantifying the interruptions to monitoring and control activities caused by conflict. Armed conflicts pose a major challenge for consistent monitoring and control operations in breeding areas. For the time period of 1986 to 2002, Showler (2003) lists 14 salient examples of conflicts that directly or indirectly interrupted locust survey and control and claims that such interruptions were a chief reason for the upsurges and plagues observed during the same time period. Thus, episodes of conflict may be seen as a shifter in locust monitoring. However, lapses in monitoring and control should only lead to excess locust activity when the weather conditions are favorable to locusts.

To analyze the effect of conflict on monitoring, we create a monthly panel of locust outcomes for each admin-1 unit that also overlaps with a breeding area, and combine this data with our monthly panel data for conflict events. We estimate the following specification:

$$Y_{at} = \mathbf{Conflict}_{at}\Theta + \lambda_a + \delta_{st} + \eta_{bm} + \varepsilon_{at} \quad (1)$$

Where Y_{at} is the outcome for admin-1 unit a , in month t . Our main focus is how monitoring changes during times of armed conflict and after. We measure monitoring effort as any ecology or locust report, regardless of life stage and gregarization status. Alternatively, we focus on the ecology reports as an outcome that should capture monitoring efforts even in the absence of locusts. For each outcome, we construct two variables: a dummy variable that is equal to one whenever there is a non-zero count of reports, and the number of days in the time period t in which at least one report was made. The dummy variable focuses on the extensive margin of monitoring, while the days variable helps us study monitoring effort intensity. We construct the same two variables for control operations.

We use the conflict data to construct a variety of variables, $\mathbf{Conflict}_{at}$, that capture the duration of the conflict. The simplest form of this uses a dummy variable that is equal to one if there is a reported armed conflict event in the admin-1 a at time t . An alternative univariate approach that captures previous conflict conditions is to calculate the mean number of months in the past 12 months that have had a conflict event. When no conflict has occurred for the past year, this variable is equal to zero, while if conflict conditions happened in 10 out of the 12 recent months, it will be equal to 0.83. Accounting for how short-lived conflicts (under a year) might have different effects than long-lived conflicts (over a year), we define two variables: Onset_{at} is a dummy variable that equals one if a conflict started at any period from t to $t - 12$ in admin-1 a , and Ongoing_{at} is a dummy variable that equals one if a conflict had started more than $t - 12$ periods ago, and has still not ended. To define an onset event, we use the first reported conflict event after a period of at least 12 months where no conflict was reported. Similarly, we define an offset event, Offset_{at} , as a dummy variable that indicates that a conflict ended in period t in admin-1 a if no conflict was reported from $t + 1$ to $t + 12$. In the most saturated version of this regression, when we include three dummy variables for conflict onset, ongoing conflict, and conflict offset, the omitted category pools the time periods without conflict—that are at least one year after a conflict.

To account for different baseline reporting propensities and locust suitability, we include admin-1 fixed effects, λ_a . To flexibly capture temporal trends and seasonality, we control for breeding area

season-by-year-by-month fixed effects δ_{st} , as well as specific breeding area-by-month fixed effects, η_{bm} . The former allows for pooled shocks at the monthly level that can vary by the three seasons that define breeding areas (spring, summer, and winter). The latter controls for the seasonality of a specific breeding area (e.g. the summer breeding area in West Africa). Because an admin-1 can overlap with several breeding areas, there is many-to-one mapping between admin-1 units and breeding areas, such that admin-1 units are used in estimating more than one fixed effect.

5.3 Conflict Suppresses Locust Monitoring Efforts

Armed conflict conditions sharply reduce locust monitoring in meaningful magnitudes that are precisely estimated. In Table 1, we report coefficients from estimating Equation (1). In Panel A, we report results for any monitoring, broadly defined (see previous subsection). Whether we use the dummy for contemporaneous conflict (columns 1 and 5), the share of months in the recent year that had conflict events (columns 2 and 6), the onset and ongoing dummies (columns 3 and 7), or the inclusion of the offset dummy (columns 4 and 8), we recover the same fundamental result across both measures of monitoring: Monitoring declines during times of conflict.

A month with contemporaneous conflict lowers the likelihood of any monitoring occurring by 0.032 percentage points (column 1), reflecting a decline of 11.7% relative to the mean level of non-zero monitoring incidence. This effect triples in magnitude if all the previous 12 months had non-zero conflict events (column 2). If we separate short- and long-lived conflicts, we find that a conflict event in the recent year lowers any monitoring likelihood by 0.03 percentage points, and conflicts that have been ongoing for over a year, further suppress any monitoring by 0.055 percentage points. These effects are all precisely estimated, allowing us to reject the null hypothesis of conflict having a zero effect on any monitoring at significance levels well below one percent. When we account for the offset of a conflict (conflict conditions end for at least one year following an onset), we find that, on average, monitoring returns to baseline levels.

The observed declines in the any monitoring dummy are capturing a complete cessation of monitoring efforts. However, monitoring activity could fall from high or medium levels to non-zero levels following a conflict shock. We document this intensive margin of monitoring effort in columns 5 to 8 of Panel A in Table 1. These results demonstrate that even if monitoring continues during conflict, the number of days on which reports occur declines.

Table 1.
Estimating the Disruption & Suppression of Monitoring due to Conflict

Panel A. Any Monitoring								
	Dummy				Days			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conflict dummy	-0.032** (0.012)				-0.433*** (0.125)			
$\overline{\text{Conflict dummy}}_{[0,-12]}$		-0.090** (0.031)				-1.248*** (0.335)		
Onset [0, -12]			-0.030** (0.009)	-0.028** (0.009)			-0.213 (0.122)	-0.134 (0.125)
Offset [0, -12]				-0.004 (0.009)				-0.151 (0.151)
Ongoing (≥ 13 mos.)			-0.055** (0.018)	-0.055** (0.018)			-0.794** (0.249)	-0.806** (0.248)
Observations	48,858	47,230	47,287	47,287	48,858	47,230	47,287	47,287
R^2	0.364	0.370	0.370	0.370	0.394	0.398	0.398	0.398
Mean Dep. Var.	0.273	0.276	0.276	0.276	1.955	2.004	2.001	2.001
Panel B. Any Ecology								
	Dummy				Days			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conflict dummy	-0.031** (0.011)				-0.385** (0.145)			
$\overline{\text{Conflict dummy}}_{[0,-12]}$		-0.087** (0.031)				-1.244** (0.372)		
Onset [0, -12]			-0.027** (0.009)	-0.023** (0.009)			-0.123 (0.116)	-0.064 (0.117)
Offset [0, -12]				-0.007 (0.009)				-0.113 (0.150)
Ongoing (≥ 13 mos.)			-0.055** (0.017)	-0.055** (0.017)			-0.846** (0.263)	-0.855** (0.261)
Observations	48,858	47,230	47,287	47,287	48,858	47,230	47,287	47,287
R^2	0.374	0.377	0.378	0.378	0.387	0.391	0.391	0.391
Mean Dep. Var.	0.240	0.246	0.245	0.245	1.748	1.803	1.801	1.801

Notes: Each column reports the coefficients from one regression, capturing the effects of conflict incidence (Conflict dummy), the share of previous 12 months with conflict, any conflict onset in previous 12 months, any conflict offset in previous 12 months, and ongoing conflict. Each regression includes admin-1 fixed effects, as well as breeding area season-by-year-by-month, and specific breeding area-by-month fixed effects. Standard error is clustered by admin-1.

* 0.10 ** 0.05 *** 0.01

One immediate concern is that reporting is lower because of a true decline in locust activity that is somehow systematically correlated with conflict. We rule this out by focusing on a set of locust reports that fall under the ecology reporting group. These reports focus on the environmental conditions that might be favorable to locust, and occur regularly even if no locusts are observed. In Panel B of Table 1, we report effectively the same pattern as in Panel A—confirming that the sharp declines in any monitoring are predominately driven by *lower levels of monitoring* and not lower levels of locusts.

5.4 Excess Locust Activity Following Favorable Weather Conditions & Monitoring Failures

Lapses in monitoring and control are concerning in general, but potentially even more severe if they happen when environmental conditions are favorable for solitary locust to become gregarious. In other words, monitoring and control failures should only lead to excess locust activity when the weather conditions are right. To test this hypothesis, we interact recent conflict with recent precipitation and show that the interaction of conflict with precipitation predicts additional locusts, after controlling for the direct effects of recent precipitation and the monitoring suppression effects of conflict. Explicitly, we estimate a modified version of Equation (1):

$$\begin{aligned}
 Y_{at} = & \sum_{k \in \{0,5,10\}} \beta_{1,k} \overline{\text{Conflict} \times \text{Precip. Shock}}_{at}^{t=0-k, \dots, -4-k} + \sum_{k \in \{0,5,10\}} \beta_{2,k} \overline{\text{Conflict}}_{at}^{t=0-k, \dots, -4-k} + \\
 & \sum_{k \in \{0,5,10\}} \beta_{3,k} \overline{\text{Precip. Shock}}_{at}^{t=0-k, \dots, -4-k} + \lambda_a + \delta_{st} + \eta_{bm} + \varepsilon_{at}
 \end{aligned} \tag{2}$$

Where the outcomes are defined as they were before, though we add control and swarm report outcomes to the monitoring effort outcome. We now define three new explanatory variables, for three relative time periods. Starting from the precipitation shock, we define $\overline{\text{Precip. Shock}}_{at}$ as the share of recent months that have had a positive precipitation shock. Our baseline definition uses rainfall events either above the median level or the 75th percentile of precipitation, defined using the *local* climatology at the admin-1 level. We construct this variable for the contemporaneous month up to the fourth lagged month ($t = 0, \dots, -4$), as well as for two additional lagged periods

($t = -5, \dots, -9$ and $t = -10, \dots, -14$). This allows us to pool the dynamic effects of precipitation to three periods of interest. We choose five months for each period based on results we obtain by regressing an indicator for reported swarms on lagged precipitation shocks, while excluding periods of armed conflict and no reported monitoring of locust to ensure the estimation is not attenuated by suppressed reporting (see Online Appendix).

We define the conflict variable, $\overline{\text{Conflict}}_{at}$ in the same way such that it captures the share of recent months that have had conflict events. Finally, the interaction variable, $\overline{\text{Conflict} \times \text{Precip. Shock}}_{at}$, is the share of months that have had simultaneous conflict events *and* precipitation shocks—the exact combination of conditions that suppress monitoring, and give rise to favorable environmental conditions for locusts. We report the estimation results from this specification in Table 2, for monitoring, control, and swarms measured as a dummy variable for non-zero levels of reporting.

Table 2: Estimating Conflict and Weather Impacts on Swarm Outbreaks

	Any Monitoring				Control				Swarm			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\overline{C} \times P_{50[0,-4]}$	-0.005 (0.036)	0.035 (0.030)			0.028 (0.021)	0.029 (0.024)			0.025 (0.017)	0.039** (0.020)		
$\overline{C} \times P_{50[-5,-9]}$		0.096** (0.039)				0.019 (0.024)				0.040* (0.023)		
$\overline{C} \times P_{50[-10,-14]}$		0.043 (0.037)				0.012 (0.018)				0.033 (0.022)		
$\overline{C} \times P_{75[0,-4]}$			0.043 (0.038)	0.078** (0.031)			0.049** (0.021)	0.059** (0.024)			0.044*** (0.017)	0.064*** (0.019)
$\overline{C} \times P_{75[-5,-9]}$				0.112*** (0.039)				0.045** (0.022)				0.059*** (0.022)
$\overline{C} \times P_{75[-10,-14]}$				0.080** (0.036)				0.036** (0.018)				0.049** (0.022)
$\overline{C}_{[0,-4]}$	-0.051** (0.023)	-0.063*** (0.021)	-0.075*** (0.023)	-0.088*** (0.021)	-0.014 (0.012)	-0.009 (0.014)	-0.025** (0.012)	-0.027* (0.014)	-0.001 (0.008)	-0.008 (0.011)	-0.011 (0.008)	-0.022** (0.011)
$\overline{C}_{[-5,-9]}$		-0.028 (0.025)		-0.032 (0.025)		-0.017 (0.016)		-0.028* (0.016)		-0.022 (0.015)		-0.030** (0.014)
$\overline{C}_{[-10,-14]}$		-0.050** (0.023)		-0.070*** (0.022)		-0.002 (0.012)		-0.014 (0.011)		-0.008 (0.013)		-0.016 (0.013)
$\overline{P}_{50[0,-4]}$	0.176*** (0.017)	0.170*** (0.016)			0.091*** (0.010)	0.105*** (0.011)			0.063*** (0.009)	0.071*** (0.009)		
$\overline{P}_{50[-5,-9]}$		0.005 (0.018)				0.047*** (0.010)				0.028*** (0.009)		
$\overline{P}_{50[-10,-14]}$		0.074*** (0.017)				0.036*** (0.008)				0.028*** (0.008)		
$\overline{P}_{75[0,-4]}$			0.210*** (0.020)	0.213*** (0.020)			0.118*** (0.016)	0.132*** (0.017)			0.067*** (0.010)	0.074*** (0.011)
$\overline{P}_{75[-5,-9]}$				0.005 (0.022)				0.045*** (0.012)				0.014 (0.010)
$\overline{P}_{75[-10,-14]}$				0.062*** (0.020)				0.021** (0.009)				0.018** (0.009)
Observations	46,901	42,865	46,901	42,865	46,901	42,865	46,901	42,865	46,901	42,865	46,901	42,865
R^2	0.371	0.375	0.371	0.375	0.210	0.220	0.212	0.221	0.178	0.186	0.177	0.185
Mean Dep. Var.	0.279	0.283	0.279	0.283	0.053	0.055	0.053	0.055	0.037	0.038	0.037	0.038

Notes: Each column reports the coefficients from one regression, capturing the effects of the share of recent months with both conflict and precipitation shocks ($\overline{C} \times \overline{P}$), share of recent months with conflict (\overline{C}), and share of recent months with precipitation shocks (\overline{P}). Each regression includes admin-1 fixed effects, as well as breeding area season-by-year-by-month, and specific breeding area-by-month fixed effects. Standard error is clustered by admin-1.

* 0.10 ** 0.05 *** 0.01

We continue to observe that conflict suppresses monitoring, as well as control operations (Table 2, columns 1 to 8). As we would expect, precipitation shocks lead to higher levels of monitoring and control as local FAO offices realize that environmental conditions for locusts are favorable, and when they monitor the breeding areas, on average, they detect more locusts. When conflict and precipitation shocks occur simultaneously, captured by the interaction term, we observe that monitoring increases, especially following lagged periods of potentially consequential periods of conflict and high precipitation (columns 2 and 4). For example, if in the previous five to nine months, the admin-1 experienced conflict and high levels of rainfall (above median or above the 75th percentile), then monitoring would be 0.096 or 0.112 higher, reflecting an increase of 34% or 40% relative to a mean level of 0.28. For control operations, we see a similar spike following precipitation shocks, and a somewhat more muted response during times of conflict, albeit a negative effect. Following recent incidence of conflict and high rainfall (from $t = 0$ to $t = -4$), we see an increase in control operations, but it is only precisely estimated when we define the precipitation shock as being above the 75th percentile level (columns 7 and 8).

The propensity to report non-zero swarms increases after periods of conflict and precipitation shocks (columns 9 to 12). This increase is larger, relative to the mean likelihood of a swarm, than the relative increase in monitoring alone. In other words, while we see monitoring recovering following the interaction of conflict and high precipitation, swarm detection increases even more once we take into account that swarms are a rarer event in the data than monitoring. Relative to the mean level of non-zero swarm incidence in the data, periods of conflict and high precipitation result in more than a doubling of the propensity to report a swarm. In addition, the likelihood of observing any swarms strongly increases with high precipitation, and weakly decreases with conflict. This confirms the previous results regarding monitoring, conflict, and precipitation. Conflict suppresses monitoring, which in turn leads to fewer swarm observations. High precipitation induces monitoring—because conditions are favorable for swarm formation—and results in the detection of more swarms.

5.5 Suggestive Evidence for Negative Externalities Following Monitoring Failures

The above results help to establish the *local* effects of conflict and precipitation shocks on monitoring, control, and swarm activity. Here we broaden the spatial scope and test whether conflict *across*

countries that happens *within* the same breeding area results in locust spillovers. The intuition here is that if conflict in one country suppresses its monitoring, then a neighboring country that shares a locust breeding area might not receive an early warning signal. Instead, the neighboring country might experience a migration of a swarm from the breeding area in the country that is experiencing conflict, necessitating a stronger control response.

We test for this spatial spillover channel by estimating a regression where we focus on a focal admin-1 unit, and how either local conflict within that unit, conflict in the country in which the unit resides in, or conflict outside of that country—but that is linked to the same breeding areas as the focal admin-1—affect monitoring, locust presence, and control operations. In other words, we are estimating how remote conflict affects local locust activity while controlling for local conflict conditions. To do so, we define a triplet of dummy variables—conflict onset, ongoing conflict, and conflict offset—as we have defined them for Equation (1) for each of the three spatial bands: the focal admin-1 (FA), outside the admin-1 but inside the breeding area-country (BA-C), and outside of the country of the focal admin-1 but overlapping with the same breeding area (BA). Due to the computational intensity of the specification and the data, we simplify the fixed effects such that we control for admin-1 and breeding area season fixed effects, as well as year and month-of-year fixed effects to account for pooled shocks and seasonality. This leads to the following regression specification in which we estimate nine coefficients of interest:

$$\begin{aligned}
Y_{at} = & \beta_1 \text{Onset}_{at}^{FA} + \beta_2 \text{Offset}_{at}^{FA} + \beta_3 \text{Ongoing}_{at}^{FA} + \\
& \beta_4 \text{Onset}_{at}^{BA-C} + \beta_5 \text{Offset}_{at}^{BA-C} + \beta_6 \text{Ongoing}_{at}^{BA-C} + \\
& \beta_7 \text{Onset}_{at}^{BA} + \beta_8 \text{Offset}_{at}^{BA} + \beta_9 \text{Ongoing}_{at}^{BA} + \\
& \lambda_a + \delta_s + \gamma_y + \eta_m + \varepsilon_{at}
\end{aligned} \tag{3}$$

Conflicts in more remote areas, yet within connected breeding areas, result in more reports of locust activity once the conflict ends and monitoring returns to its previous baseline level. In Table 3, we report the estimation results for Equation (3). We observe, as before, that conflict suppresses monitoring. Both the first year of conflict onset and conflicts that continue beyond a year reduce any monitoring or any ecology reports (focal admin-1 and BA-country band coefficients, columns

1 and 2). This monitoring suppression occurs both from conflicts within the focal admin-1 and within the country of the admin-1. In fact, we observe that the onset of a conflict in the country of the focal admin-1 suppresses reports of monitoring, ecology, all types of locust, as well as control operations (BA-country band coefficients, columns 1 to 7).

After a conflict ends in the country that neighbors that of the focal admin-1, we see that the focal admin-1 meaningfully increases its reporting of locust activity by more than 3% across a variety of outcomes (BA band offset coefficient, columns 3 to 6). These increases are precisely estimated, and they align with an increase of control operations by 9.3% relative to their mean level (column 7). We interpret this as suggestive evidence for spatial spillovers that result from monitoring failures—one country that fails to monitor locust conditions because local conflict suppresses monitoring leads to the neighboring country facing increased locust activity. The last row of coefficients in Table 3 suggests that when a conflict in the neighboring country is severe, proxied by its duration, the focal admin-1 “lowers its guard” and locust reports decline sharply across all outcomes. This potentially reflects the notion that in the absence of an early warning from the neighboring country, the focal admin-1 misses a timely signal about developing locust conditions.

Table 3.
Spatial Spillovers Across Countries Within Overlapping Breeding Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any Monitoring	Any Ecology	Any Locust	Swarm	Swarm or Band	Any Gregarious	Control
<i>Focal admin-1</i>							
Onset	-0.024** (0.010)	-0.016* (0.010)	-0.008 (0.010)	0.003 (0.005)	0.006 (0.007)	0.008 (0.007)	0.003 (0.006)
Offset	-0.002 (0.009)	-0.003 (0.009)	-0.002 (0.009)	-0.002 (0.005)	-0.005 (0.007)	-0.005 (0.007)	-0.005 (0.006)
Ongoing	-0.043** (0.018)	-0.036* (0.021)	-0.020 (0.017)	0.010 (0.008)	0.015 (0.010)	0.013 (0.011)	0.002 (0.009)
<i>BA-country band</i>							
Onset	-0.015*** (0.004)	-0.012*** (0.004)	-0.011*** (0.004)	-0.007*** (0.002)	-0.012*** (0.002)	-0.013*** (0.003)	-0.005** (0.002)
Offset	-0.000 (0.004)	-0.001 (0.004)	0.003 (0.003)	-0.003 (0.002)	-0.000 (0.003)	0.001 (0.003)	-0.000 (0.002)
Ongoing	-0.027* (0.014)	-0.041** (0.016)	-0.001 (0.011)	0.014** (0.006)	0.014** (0.007)	0.011 (0.008)	0.001 (0.007)
<i>BA band</i>							
Onset	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.001)	-0.004** (0.001)	-0.005*** (0.002)	-0.001 (0.002)
Offset	-0.000 (0.002)	0.001 (0.002)	0.008*** (0.002)	0.003** (0.001)	0.004** (0.001)	0.005*** (0.002)	0.005*** (0.002)
Ongoing	-0.068*** (0.011)	-0.059*** (0.014)	-0.052*** (0.009)	-0.035*** (0.005)	-0.041*** (0.006)	-0.050*** (0.007)	-0.036*** (0.006)
Observations	44,662	44,662	44,662	44,662	44,662	44,662	44,662
R^2	0.263	0.272	0.188	0.091	0.101	0.114	0.111
Mean Dep. Var.	0.285	0.255	0.163	0.038	0.052	0.060	0.054

Notes: Estimation results for the specification in Equation (3). Each column reports the coefficients from one regression, capturing the effects of conflict onset, ongoing conflict, and conflict offset within the admin-1 (focal admin-1), outside of the admin-1 but within the same country (BA-country band), and outside of the country but within the same breeding area (BA band). Each regression includes admin-1 and breeding area season fixed effects, as well as year and month-of-year fixed effects. Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

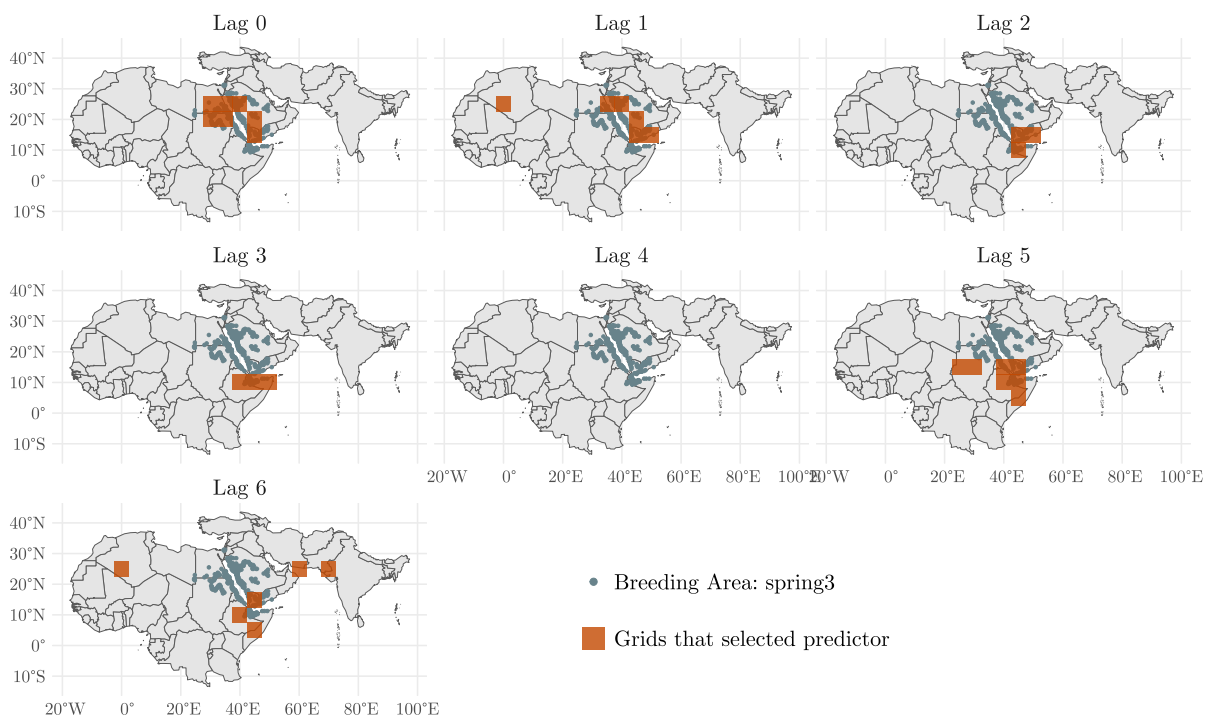
6 Propagation of Locust Activity Across Space

In the previous section, we documented that conflict in breeding areas interrupts monitoring and that when the conflict episode is concurrent with a precipitation shock, this leads to an increase in the probability that a locust swarm is observed *within* the same breeding area. However, swarm outbreaks affect human well-being *beyond* breeding areas, once they escape and reach locations where populations live and agricultural activities take place. From locust ecology expertise and the literature, we know that swarms move from remote breeding areas to more densely populated areas along relatively stable migration routes, determined to a large extent by continent-scale prevailing wind patterns (Cressman and Stefanski 2016, also see Figure 1). In this section, we seek to reconstruct these teleconnections between swarm outbreaks in breeding areas and exposure to swarms beyond breeding areas. This will allow us to (1) propagate swarms across space, to link monitoring and control impacts on swarm outbreaks in breeding areas, with health impacts of swarm outbreaks outside of breeding areas; and (2) create a proxy for the presence of locust swarms outside breeding areas, based on breeding area dynamics and teleconnections, which overcomes non-classical measurement error in locust reporting (see section 7).

We use a data-driven approach to reconstruct migration-driven teleconnections in swarm dynamics. Specifically, we use locust monitoring data to identify, for any location, the breeding areas that “feed” locusts to this location, and the temporal lag structure that characterizes each connection. We start by splitting our study region into a 5 degree grid to allow for subregion-specific teleconnection models. For each grid cell, we run a separate LASSO regression that selects from a pool of contemporaneous and lagged locust activity in breeding areas—we refer to this as the “LASSO stage.” The dependent variable in this regression is a monthly time series of locust swarm presence dummies in a given grid cell. The set of predictors that LASSO selects from are monthly time series of locust swarm presence dummies across all spring, summer, and winter breeding areas; both the contemporaneous time series, and up to six monthly lags are included. There is no further structure imposed, so that each grid cell can be linked to more than one breeding area, and to more than one lag for each breeding area. This LASSO stage provides us with a set of lagged dummies from different breeding areas, which are most predictive of locust swarm activity in a given grid cell.

To visualize the reconstructed propagation of swarms across space, we reverse the outcome of the LASSO regressions. In Figure 9, we show one spring breeding area (blue dots) and all the grid cells it “sends locusts to” across a monthly timeline. From contemporaneous time (lag 0) to a 6-month lag, it shows the recipient grid cells whose LASSO model chose the dummy predictor from that breeding area with the corresponding lag. Some propagation-like patterns are visible, with recipient grid cells moving away from the focal breeding area with increasing lags, though spatial patterns are noisy, which could be due in part to correlations among all possible LASSO predictors.

Figure 9: Inferring Locust Swarm Teleconnections using LASSO

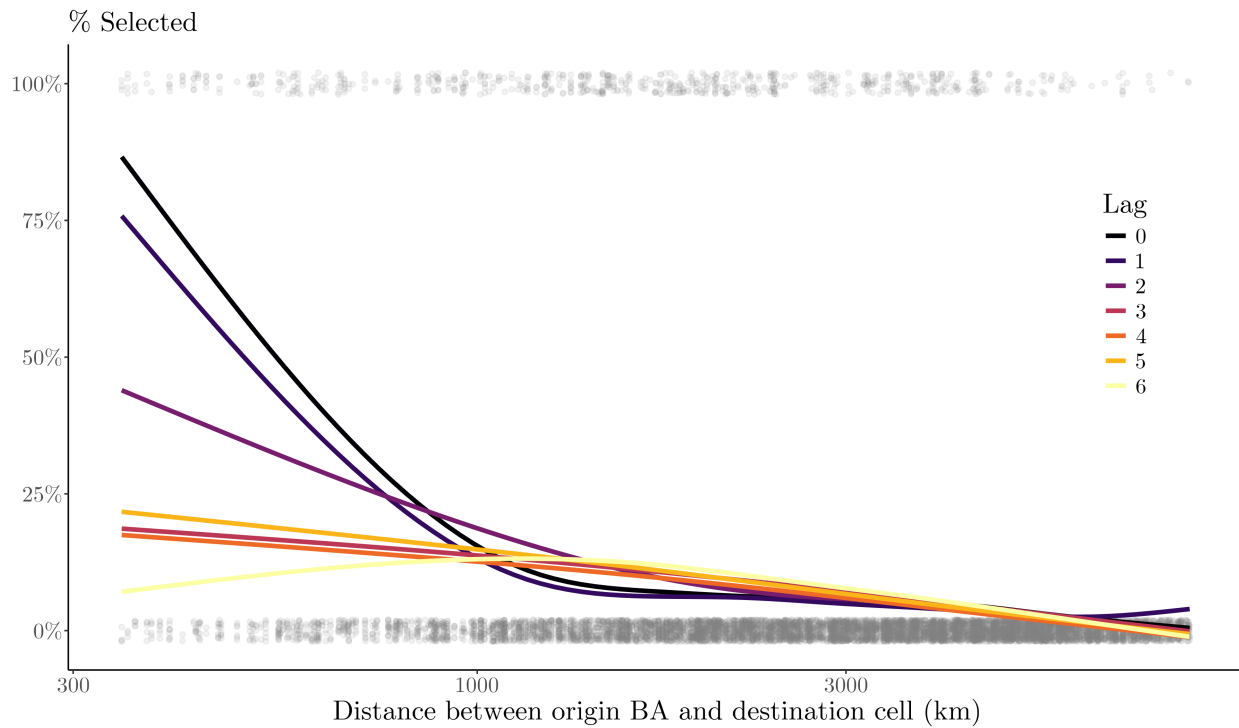


Notes: Set of 5 degree grid cells (brown squares) “teleconnected” to a spring breeding area (blue dots, corresponding to spring breeding cluster “3”, see Figure 3) at different monthly lags from lag 0 to lag 6.

To evaluate more systematically the credibility of the reconstructed propagation patterns, we show the selection probability of breeding area predictors as a function of the distance between origin breeding areas and destination grid cells, separately for each lag level. We expect shorter lags to be selected with higher probability than longer lags at short distances and for that ordering to reverse as distance increases. This pattern is visible in Figure 10, where the selection probability is higher for shorter lags when the distance is small, and the ordering flips as the distance between origin breeding area and destination grid cell increases. In addition, selection probability decreases

with distance overall.

Figure 10: LASSO predictor selection by grid cell distance from breeding areas.



Notes: Each grey dot corresponds to a (breeding area, grid cell, lag) triplet, placed at either 100% (breeding area predictor selected for that grid cell at that lag level), or 0% (predictor not selected). Each colored line is a locally smoothed curve correspond to a specific lag level. The (breeding area, grid cell) pairs that are closer than 300 km have been excluded from this graph due to sparsity.

Overall, predicted monthly swarm time series at the grid cell level are well aligned with observed monthly swarm time series (Table 4 and Figure C1). We come back to these teleconnections in section 7 when constructing a proxy for human exposure to locusts, and in section 8 when evaluating the back-of-the-envelope value of early warning systems.

Table 4: Observed Swarms vs. Predicted Swarms

	Observed Swarm Dummy	
	(1)	(2)
Pred. Swarm Values	0.996*** (0.002)	0.995*** (0.002)
<i>Fixed Effects</i>		
Grid cell	✓	✓
Year	✓	
Month	✓	
Year×Month		✓

Notes: Each column reports the coefficients from one regression, regressing observed swarm dummy values at the grid-year-month level on predicted swarm values generated by the lasso model. Fixed effects vary by column. Standard errors are clustered by grid cell.

* 0.10 ** 0.05 *** 0.01

7 The Impact of Locusts Outbreaks on Human Well-Being

We have demonstrated that monitoring and control activities in breeding areas are interrupted by conflict and if this coincides with weather conditions that are favorable for locust breeding, it is more likely that a locust swarm will form. Next, we showed that locust activity in remote breeding areas predicts swarms outside of breeding areas. In this section, we analyze what this means for human well-being.

The main goal of this section is to estimate the causal short-term and long-term implications of locust swarm exposure for human well-being. The purpose of this step is to document the human costs of swarms that escape breeding areas into more populated areas and is essential for our understanding of the value of locust surveillance and control. Our main outcome is children’s height-for-age, a marker of a child’s early nutritional environment that is linked to cognitive and economic outcomes later in life (Case and Paxson 2008a; 2008b).⁷ As such, height-for-age is one way of measuring the potentially far-reaching human impacts of exposure to desert locust swarms. Our main analysis focuses on children living in rural areas.

7.1 Fixed Effects Estimation

Our first approach uses the plausibly exogenous combination of the required environmental conditions that lead to locust infestations. Each locust infestation event provides us with a natural experiment where certain locations are exposed to the locust shock. Using the history of locust records that spans over four decades, we construct the location-specific time series of locust exposure.

To determine if an individual in our DHS data was exposed to a desert locust swarm outbreak, we first need to establish the spatial extent of a locust swarm. Specifically, we need to answer how large of an area is affected when we observe a locust swarm in one location. This is important because monitoring of locusts is resource-constrained, and unlike a hurricane, for example, there is neither precise tracking of their trajectory over time nor an underlying physical process that lends itself naturally to modeling. We turn to a data-driven approach and base the spatial extent of a locust swarm on the spatial clustering of swarms in across space using Ripley’s K-function, giving

⁷ In Appendix C, we provide support for the idea that locust swarms affect food security by establishing a link between locust outbreaks and food emergency declarations.

us a cutoff at 150 km. This procedure is described in detail in Online Appendix B.

Second, we need to decide how to account for the different timing of exposure. This is particularly important when considering the effects on children, as *in-utero* exposure might have a larger impact than post-birth exposure (Almond and Currie 2011). Our treatment assignment rule flags a child surveyed at time t as exposed while *in-utero* if any swarm was observed within 150km from the DHS cluster during the nine months before birth, and as exposed after birth if the swarm is observed in the nine months post-birth. In robustness checks, we also evaluate exposure in 9 to 18, and 18 to 27 months after birth.⁸

Our assignment of treatment relies on the presence of a swarm within a 150 km radius of a DHS cluster. We focus on the presence of a swarm because we consider the extensive margin of swarm presence to be measured with higher accuracy relative to the intensive margin of the density of swarms. In addition, the evidence for spatial clustering of swarms supports the notion that if we observe one swarm in the region, there are likely more swarms that could be unobserved in the data. However, the choice of assigning treatment based on a distance threshold presents a risk of violating the stable unit treatment value assumption (SUTVA). Children in DHS clusters that have swarms detected further than 150 km might still be affected by the locust outbreak. This is especially true if an observed presence of a locust swarm 200 km from the DHS cluster is highly indicative of an unobserved swarm closer to the DHS cluster. In robustness tests, we extend the 150 km threshold, run “donut” versions of the analysis, and allow the treatment effect to vary by distance.

Our interest is to study how locust exposure affects the outcome of child i , of birth cohort b , born in month m , observed in DHS cluster d , in administrative level a , in country c , in region r , surveyed in year t , and in month n . We estimate how the outcome y_{ibmd} responds to a locust swarm shock by using the following regression specification:

$$y_{ibmd} = \beta_1 \text{Locust}_{bmd}^{\text{pre-9-months}} + \beta_2 \text{Locust}_{bmd}^{\text{post-9-months}} + f(\text{weather}_{bmd}) + \lambda_a + \delta_{rb} + \eta_{rm} + \omega_{rt} + \theta_{rn} + \varepsilon_{ibmd} \quad (4)$$

⁸ Exposure before conception might still have an impact during pregnancy and after birth.

We account for any time-invariant cross-sectional variation in anthropometric scores, human capital accumulation, and other demographic outcomes by including fixed effects at the first administrative unit-level (admin 1), λ_a .⁹ We flexibly control for time-trends by including year of birth fixed effects, δ_{rb} , as well as year of DHS survey fixed effects, ω_{rt} , which we allow to vary by WHO region. To capture seasonality, we include month of birth fixed effects (η_{rm}), as well as month of survey fixed effects (θ_{rn}), which we also allow to vary by WHO region. We also account for local weather conditions by including $f(weather_{bmd})$ that flexibly controls for temperature and precipitation.¹⁰ Any unobserved heterogeneity is captured by the error term, ε_{ibmd} . We cluster our standard errors at the admin-1 level.

There are two main concerns with the fixed effects identification strategy: non-random exposure to locust swarms within a locality, and non-classical measurement error.

First, while the panel fixed effects regression in Equation (4) accounts for local weather conditions, time-invariant factors, and pooled seasonality and time trends at the WHO region level, it does not account for non-random exposure to locusts within admin-1 regions. While the sequence of events that trigger a locust swarm outbreak might be considered “as-good-as random,” the locations that end up exposed are not entirely random, meaning that there may be a cross-sectional component to the likelihood of ever experiencing a locust swarm. However, we argue that the uncertainty around the timing of a locust outbreak provides a valid natural experiment: neither the affected population nor the FAO can fully predict when an outbreak will occur. Moreover, the sequence of contagion across space is not deterministic. Under these assumptions, untreated children in locations that periodically experience a locust outbreak can approximate the counterfactual outcomes for treated children in those locations. One limitation of the fixed effects approach is that outcomes in locations that never experience a locust outbreak might not approximate non-exposure counterfactuals for the treated population.

Our second concern is that in this setting, measurement error almost certainly results in false negatives where we fail to observe a locust swarm in the data. A swarm observation in the data almost surely reflects a true detection of a swarm, however, not all swarms are necessarily observed,

⁹This is the first level below the country level, meaning the most coarse sub-national level.

¹⁰We define child year-of-birth precipitation quartiles globally and temperature-day bins as follows (deg. Celsius): <11, 11-14, 14-17, 17-20, 20-23, 23-26, 26-29, 29-32, 32-35, >35, using 17-20 as the reference bin. We include 1-period lags and leads of precipitation quartiles and temperature-day bins.

and swarms that are observed are not necessarily observed each time they land in a new location. This issue of false negatives in the data is further complicated by a potential correlation between swarm intensity and likelihood of measurement. It is plausible that the monitoring and recording capacity of locust presence declines more in areas that are more severely affected during a locust infestation. One way in which such a correlation might arise is because shocks to monitoring capacity likely also affect locust control, resulting in more intense exposure to treatment. This means there might be a positive correlation between worse outcomes and the likelihood of false negatives. We would effectively assign these most severely hit locations as part of the control group, attenuating our estimates, or even reversing the sign of the effect. In what follows, we describe how we can use what we documented in section 6, namely that desert locust activity in breeding areas predicts swarms outside of breeding areas, to address the concerns covered above.

We use a proxy for locust swarm exposure in order to address these two potential threats to the baseline identification strategy.

In addition to the non-random exposure and non-classical measurement error issues we discuss above, there are three other threats to our identification strategy that we consider. First, there might be unobserved local responses to locust outbreaks that affect our outcomes of interest. For example, local outbreaks could lead to large insecticide spraying campaigns that could have their own detrimental impacts on health. To rule out that we are attributing the presence of a locust swarm to insecticide exposure we also run regressions where we include dummies for swarm presence as well as insecticide spraying. Other responses that could affect local agricultural productivity could occur if countries cease cloud seeding flights in order to reduce soil humidity levels—critical for the next generation of locust eggs to hatch—yet we did not find any reports of changes to cloud seeding in response to swarm outbreaks. Second, for our admin-1 level fixed effect to capture, among other factors, the mean relationship between agricultural productivity and the outcomes of interest, we are also assuming that we observe locations in our sample in their post-adaptation equilibrium, such that they do not engage in crop switching or adopt new varieties. We consider this a plausible assumption because these societies have been exposed to locust for centuries, and no new locust-resilient crop varieties have been developed during our sample period. Finally, our empirical approach also relies on the effect of a locust swarm to fade out within a few years, such that conditions are back to their baseline level by the time a new locust outbreak occurs.

7.2 Using Locust Activity in the Teleconnected Breeding Areas as a Proxy for Local Swarm Exposure

As described above, our identifying assumption might be violated due to residual cross-sectional differences, endogenous responses, or non-classical measurement error. To address these concerns, we use the fact that variation in locust activity outside of breeding areas is driven by locust activity in remote breeding areas via long-distance migration patterns (see section 6) to construct a proxy for local locust swarm exposure.

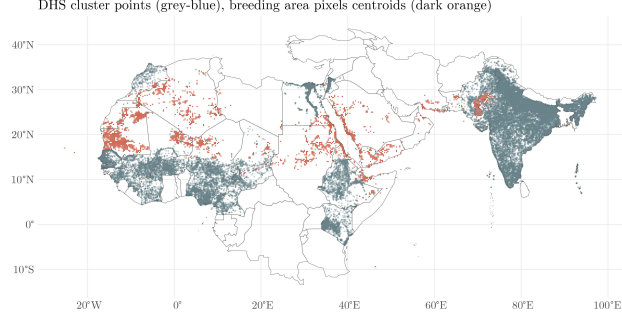
In section 6, we generated an individual LASSO model of locust swarm presence in every 5-degree grid cell, based on lags of locust swarm activity across teleconnected breeding areas. Following methods in Belloni et al. 2012; Chen et al. 2023; Lennon et al. 2022, we use each LASSO model to generate a monthly time series of predicted swarms in every grid cell, and use these predicted swarm values as a proxy for observed swarms in all the DHS clusters that are located in that grid cell. The exclusion restriction is that locust swarms that occur in remote, yet teleconnected, breeding areas are only correlated with the outcomes of interest through local locust swarm presence, conditional on local weather. In Figure 11, we show evidence of spatial separation between breeding areas and the DHS clusters where we measure impacts. We further check that the impact exposure zones (150 km radius around DHS cluster centroids) do not completely overlap with LASSO “recipient” grid cells that contain breeding area pixels (this is a conservative check since several of these red grid cells only overlap with the edge of a breeding area and predicted locust incidence in these cells is determined by locust incidence further away).

We begin by confirming that our proxy for locust presence captures meaningful variation in exposure to locusts, focusing on the time period around birth. We regress a dummy variable that indicates the presence of a locust swarm within 150 km from the DHS cluster during the time period of interest – either nine months before or nine months after birth – on the mean predicted values in the grid cell for the two time periods. We include the same fixed effects and control variables as in Equation (4), giving us the following specification:

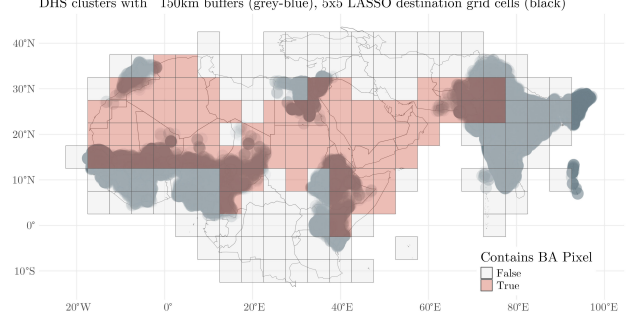
$$\text{Locust}_{ibmd}^{k\text{-months}} = \beta_1 (\text{Mean Predicted Values})_{bmd}^{\text{pre-9-months}} + \beta_2 (\text{Mean Predicted Values})_{bmd}^{\text{post-9-months}} + f(\text{weather}_{bmd}) + \lambda_a + \delta_{rb} + \eta_{rm} + \omega_{rt} + \theta_{rn} + \varepsilon_{ibmd} \quad (5)$$

Figure 11: Overlaying Locust Breeding Areas and DHS Clusters

(a) Breeding Area Centroids and DHS Cluster Points



(b) 5 degree grid cells and 150 km Buffers Around DHS Cluster Points



Notes: (a) Breeding area pixels include spring, summer, and winter breeding locations (see 3.2 for details on methodology). (b) The 5 degree grid cells are the same ones used to derive teleconnections between origin breeding areas and destination grid cells. The cells are highlighted in red when they overlap with at least one breeding area pixel.

where the main regressors are now the predicted values we obtained from the grid cell regressions described in Section 6. If we were to use our proxy in as an instrumental variable, we would then proceed to run a second stage regression of the outcome on the first stage predicted values. However, because of the non-classical measurement error in this setting, the coefficient we obtain from the second stage is potentially inflated. This occurs because the first stage coefficient becomes more attenuated with more false negatives—the exact type of non-classical measurement error we are concerned about. The second stage effectively scales the reduced form coefficients by the first stage coefficients, which means our denominator is attenuated, relative to the case of zero measurement error. For any given level of the reduced form estimate, and non-zero rate of false negatives, our second stage estimate will get inflated. We verify this through a simulation where we increase the rate of false negatives by turning some locust swarms dummy values from one to zero. As a result, we instead use the predicted values as a proxy for local swarm activity, and estimate the following reduced form specification:

$$y_{ibmd} = \beta_1 (\text{Mean Predicted Values})_{bmd}^{pre-9-months} + \beta_2 (\text{Mean Predicted Values})_{bmd}^{post-9-months} + f(\text{weather}_{bmd}) + \lambda_a + \delta_{rb} + \eta_{rm} + \omega_{rt} + \theta_{rn} + \varepsilon_{ibmd} \quad (6)$$

Our proxy that relies on locust swarm observations in breeding areas that are teleconnected to

DHS clusters results in a strong first stage. In Table 5, we report the coefficients from Equation (5) for both the nine months pre- and post-birth locust exposure. The results for both coefficients show a very precisely estimated positive effect of the mean predicted values on the local locust swarm dummy. In particular, the mean predicted values in the nine months pre-birth are predictive of the local swarm presence dummy pre-birth, and the same holds for the nine months post-birth. The mean predicted values in the nine months pre-birth are also predictive of the local swarm presence dummy post-birth, but the reverse does not hold. Both regressions have an F-stat above 50. We report regressions with either admin-1 fixed effects, as we do throughout the analysis (column 2 and 3), as well as grid fixed effects because we use the grid cells to generate the predicted values.

Table 5.
First Stage Results for Locust Exposure Around Birth

	Adm1 Fixed Effects		Grid Cell Fixed Effects	
	9m pre-birth	9m post-birth	9m pre-birth	9m post-birth
$\widehat{9m\ pre-birth}$	0.717*** (0.113)	0.255*** (0.060)	0.715*** (0.311)	0.296*** (0.066)
$\widehat{9m\ post-birth}$	-0.038 (0.063)	0.800*** (0.131)	-0.038 (0.182)	0.841*** (0.134)
Observations	341,862	341,862	341,862	341,862
SW F Stat.	55.162	55.162	61.315	61.315
SW F Stat.	133.317	133.317	145.119	145.119

Notes: Estimation results from Equations (5). Columns report first stage results. Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

7.3 Exposure to Locust Outbreaks Lowers Standardized Height in Exposed Children

In this section, we describe the main estimation results for the impact of exposure to locust swarms on height-for-age, using the fixed effects and reduced form approaches. Children who were exposed to a locust swarm around the time of their birth experience a negative shock to their growth and development, as reflected in their standardized height-for-age. In Table 6, we report the estimation

results using the fixed effects (FE) specification in Equation (4), as well as the reduced form (RF) specification in Equation (6). Experiencing a locust swarm exposure while *in-utero* lowers height-for-age by 0.36 standard deviations (column 1). Exposure in the nine months after birth lowers height-for-age by 0.22 standard deviations. These are meaningful reductions in growth as reflected in the higher likelihood for stunting—being two standard deviations below the mean height-for-age—in a population with a mean height-for-age of -1.6. Swarm exposure either *in-utero* or nine months after birth increases the likelihood of stunting by 0.7 percentage points, reflecting an increase of 16.3% relative to the mean 43% prevalence of stunting in the sample (column 3).

The predicted locust swarm reduced form results largely agree with the fixed effects results. In columns 2 and 4 of Table 6, we find precisely estimated effects on height-for-age and stunting in the same directions as in columns 1 and 3 for both *in-utero* exposure, and in the nine months after birth. The coefficients in columns 2 and 4 are much larger, yet they cannot be directly interpreted without considering the distribution of the predicted values. For *in-utero* exposure, moving from the 25th to the 75th percentile value in our sample, 0.06 and 0.31, results in a reduction in height-for-age by 0.27 standard deviations. Repeating this calculation for the stunting dummy results in a 6 percentage point increase for *in-utero* exposure.

To benchmark our results, we compare how height-for-age and stunting are affected following a food emergency declaration. In Table C2 in Appendix C, we report the correlation between a food emergency declaration, and the outcomes from Table 6. We find that a food emergency declaration leads to a 0.3 standard deviation reduction in height-for-age, and an 8.5 percentage points increase in the likelihood of stunting. These estimates are very close to our estimated effects of locust swarm exposure, which supports the idea that locust outbreaks lead to local food insecurity, as we report in Table C1. To put our estimates into an even broader perspective, Dell (2010) finds increased childhood stunting by 6 percentage points in locations formerly subjected to the *mita* (a forced labor system) in Peru; Filmer et al. (2023) find that shocks in food prices, especially those of perishable nutritious goods (eggs), increase child stunting by 11 percentage points (Philippines); finally, in Rwanda, Akresh et al. (2011) determine that *in-utero* exposure to the civil war reduced height-for-age z-scores by 1.05 standard deviations, whereas *in-utero* exposure to crop failure decreased that of *girls* by 0.86 standard deviations.

In the Online Appendix, we report additional results that support our interpretation of the

Table 6
Locust Exposure Around Birth & Children’s Height

	Standardized Height		Stunting Dummy	
	(1)	(2)	(3)	(4)
	FE	RF	FE	RF
Swarm Exposure, 9m pre-birth	-0.359*** (0.083)		0.067*** (0.016)	
Swarm Exposure, 9m post-birth	-0.218*** (0.046)		0.070*** (0.014)	
Mean Pr. Val. 9m pre-birth		-1.084*** (0.223)		0.239*** (0.059)
Mean Pr. Val. 9m post-birth		-0.668*** (0.176)		0.175*** (0.053)
Observations	341,862	341,862	341,862	341,862
R^2	0.104	0.104	0.070	0.070

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

results. In Figure C2 and Table C3, we document the scope of SUTVA violations and how those might attenuate our main estimates. In Table C4, we verify our results are robust to excluding India, and focusing on African countries only. In Table C6, we show that our main results are not sensitive to the inclusion of more granular unit fixed effects such as admin-2, DHS cluster, and mother fixed effects. In Tables C7 and C8, we demonstrate that as expected, the negative impacts of swarm exposure are concentrated in rural DHS clusters. In Table C9, we rule out the potential concern that we are picking up the effects of control operations in the form of insecticide spraying. Finally, in Table C10, we report heterogeneity of the results by sex-at-birth, and find that male fetuses experience a larger penalty relative to female ones—suggesting that there are different scarring and selection effects on male and female fetuses.

7.4 Examining Selection & Scarring Effects

Our analysis thus far has focused on growth and development, proxied by height-for-age. The finding on a negative shock to height-for-age from exposure to a locust swarm around the timing

of birth suggests a strong scarring effect, either *in-utero*, or in early infancy that does not fully recover even years after the exposure. The magnitude of the effects we find also raises the question of whether in the presence of such severe shocks do children and infants experience higher mortality.

Mortality can occur both after or before birth. If mortality *in-utero* (fetal death) occurs at a high rate, and there is selection against those who might have had lower height-for-age, this can result in a reversal of the sign—swarm outbreaks leading to positive effect on height-for-age (Deaton 2007). Here we examine how the effects on height-for-age compare to the effect on infant mortality, under-five mortality, and the number of births per woman.

We fail to find a precisely estimated effect on infant mortality or under-five mortality in the full sample (Table 7, Panel A. columns 2 and 3). However, we do find a drop of 0.07 births per-mother, reflecting a decline of 4.1% relative to the mean in the sample (Table 7, Panel A. column 4). This lower birth rate effect increases by an order of magnitude to a drop of 0.216, a 12.8% change, when we focus on the earlier part of the sample of 1987 to 2000 (Table 7, Panel B. column 4). In that time period, the effects on height-for-age are much more muted in their magnitude and precision (Table 7, Panel B. column 1). Similarly, we fail to detect any change in infant or under-five mortality (Table 7, Panel B. columns 2 and 3).

When we focus on the latter part of the sample, 2001 to 2018, we find larger effects on height-for-age (Table 7, Panel C. column 1). We estimate that the likelihood of infant mortality increase by 0.8 percentage points if the child was exposed to a locust swarm *in-utero*, reflecting a 13.3% increase relative to the mean prevalence of infant mortality (Table 7, Panel C. column 2). We do not find evidence of a change to under-five mortality or the number of births per-woman.

Combined, these effects suggest that earlier in the sample, selection effects dominated scarring effects. The number of births per-woman were substantially lower in exposed admin-1 areas, consistent with a higher fetal death rate (selection). In the presence of higher selection, we would not expect to see a strong effect on height-for-age, if weaker fetuses who would have obtain a lower height-for-age were negatively selected. In the latter period, when we no longer observe suggestive evidence for selection, we see more evidence of scarring in the form of more severe impacts on height-for-age, and infant mortality.

Table 7.
Examining Selection Versus Scarring Channels

	H4A (1)	IM (2)	U5M (3)	BR (4)
Panel A. All Birth Years				
Swarm Exposure, 9m pre-birth	-0.359*** (0.083)	0.004 (0.003)	0.006 (0.004)	-0.070** (0.034)
Swarm Exposure, 9m post-birth	-0.218*** (0.045)	0.001 (0.003)	0.005 (0.005)	0.005 (0.029)
Observations	341,862	501,557	501,557	4,581
Mean Dep. Var.	-1.605	0.060	0.072	1.693
R^2	0.104	0.013	0.030	0.189
Panel B. Birth Years 1987-2000				
Swarm Exposure, 9m pre-birth	-0.172* (0.103)	-0.003 (0.005)	0.004 (0.008)	-0.216*** (0.075)
Swarm Exposure, 9m post-birth	0.067 (0.085)	0.001 (0.007)	0.008 (0.009)	0.036 (0.056)
Observations	61,913	94,933	94,933	1,283
R^2	0.198	0.021	0.051	0.286
Panel C. Birth Years 2001-2018				
Swarm Exposure, 9m pre-birth	-0.407*** (0.063)	0.008* (0.004)	0.007 (0.004)	-0.001 (0.039)
Swarm Exposure, 9m post-birth	-0.259*** (0.068)	0.001 (0.003)	0.004 (0.004)	0.008 (0.035)
Observations	279,948	406,624	406,624	3,283
R^2	0.097	0.008	0.015	0.216

Notes: Estimation results from Equation (4). Column (1) includes results for height-for-age, column (2) for infant mortality dummy, column (3) for under-5 children mortality dummy, and column (4) for the number of births divided by the number of women at the admin-1 level. Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample consists of all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

8 The Value of Early Warning Systems

In this section, we combine our estimates, along with findings on the economic damages of stunting, to conduct a back-of-the-envelope calculation regarding the value of monitoring locusts. We calculate that in the absence of effective locust monitoring, the affected countries would experience an increased stunting rate of 0.6 percentage points and a decrease of 0.05% of their GDP.

Conflict onset decreases monitoring by 2.4 percentage points during the month, and during the month of offset we find an increase in swarms of 0.3 percentage points (in other parts of the breeding area). We convert these to 9-month likelihoods of exposure to make them comparable to the effects of swarms in-utero on subsequent child stunting. Since the monthly sample mean of swarm presence is 3.8%, the baseline likelihood of swarm exposure in-utero is 29.4%. If monitoring were reduced from its current mean of 28.5% of months to zero, monthly swarm frequency would rise from the 3.8% baseline to 7.4% ($0.285 * (0.003/0.024)$), meaning the likelihood of in-utero exposure would rise to 49.8%. This is a 20.3% rise in a child's risk of exposure to a swarm in-utero.

Since exposure to a swarm during one month in-utero increases stunting likelihood by 2.9 ($0.263/9$) percentage points, then an elimination of monitoring would increase stunting likelihood for all in-utero children by 0.6 percentage points.

In 2020, 40 million children age 0-1 lived in areas exposed to locusts in our data. Our estimates imply that the absence of monitoring would result in an additional 238 thousand children stunted, from that cohort alone. We then appeal to Galasso and Wagstaff (2019) which uses a development accounting framework to estimate the effects of stunting in the workforce on GDP, through channels of decreased educational attainment, decreased height, and decreased cognitive skills. Their preferred specification suggests that a 1% increase in the stunting rate of the workforce reduces GDP by 0.155%.

In 2020, the countries that experienced some amount of locusts in our data had a total population aged 0-1 of 69.6 million. The increased number of stunted children in our calculation above implies a change in the stunting rate (with all children in the sample countries in the denominator) of 0.34 percentage points, which would decrease GDP by 0.05%. Since the combined GDP of the sample countries is 22.4 trillion (US\$PPP), the decrease in GDP would be US\$11.9 billion. This would be the yearly global GDP loss of eliminating all monitoring efforts. Since the average

yearly budget for monitoring and control is around \$39m per year, this implies a benefit-cost ratio of around 300 for monitoring and control investment, when considering only the benefits due to reduced stunting.¹¹

There is a temporal lag between policy action (monitoring and control to prevent locust swarms) and the benefits we model (increased productivity in the workforce through avoided stunting). To adjust the estimated benefit-cost ratio to account for discounting, we calculate the discount factor that should be applied to the benefits to account for the fact that they materialize over the working lives of children born at the time of the policy action. At an extremely conservative discount rate of 7%, benefits should be discounted with a factor 0.12. Even this very conservative adjustment leaves a cost-benefit ratio of approximately 36:1. A discount rate of 2%, more typical of social benefit calculations, gives a discount factor of 0.49 and a cost-benefit ration of 147:1.

9 Discussion

With climate change affecting conditions in breeding areas and as well as the wind patterns governing where the locust migrates, outbreaks may become more frequent and widespread (Salih et al. 2020). Despite significant progress made in recent decades in preventing outbreaks, the 2020-2022 locust infestation demonstrated that the desert locust remains a threat to agricultural production in low-income countries.

This paper provides a systematic evaluation of the value of early warning systems put in place to monitor these outbreaks, an exercise consisting of three steps. First, we demonstrate how exogenous breaks in monitoring and control activities results in locust outbreaks. We connect outbreaks in breeding areas by tracing out the spatial and temporal patterns of locust swarms. Finally, we show that pre and postnatal exposure to locust swarms adversely affects human well-being, as measured by anthropometric status.

¹¹ \$39 million is the average yearly outlay from 2002-2022 from all bilateral and multilateral donors to all developing countries for “Plant and post-harvest protection and pest control.” While this likely includes activities beyond locust monitoring and control, it provides a conservative estimate for the benefit-cost ratio.

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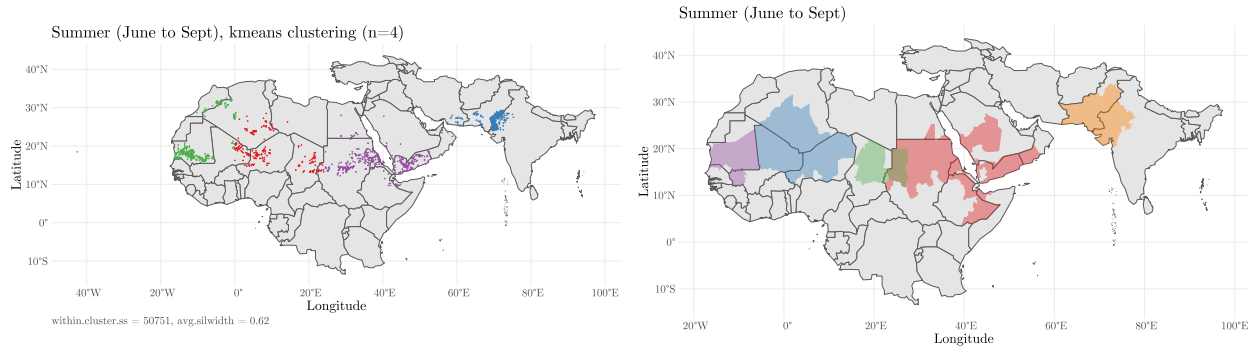
Appendix

A Data Appendix

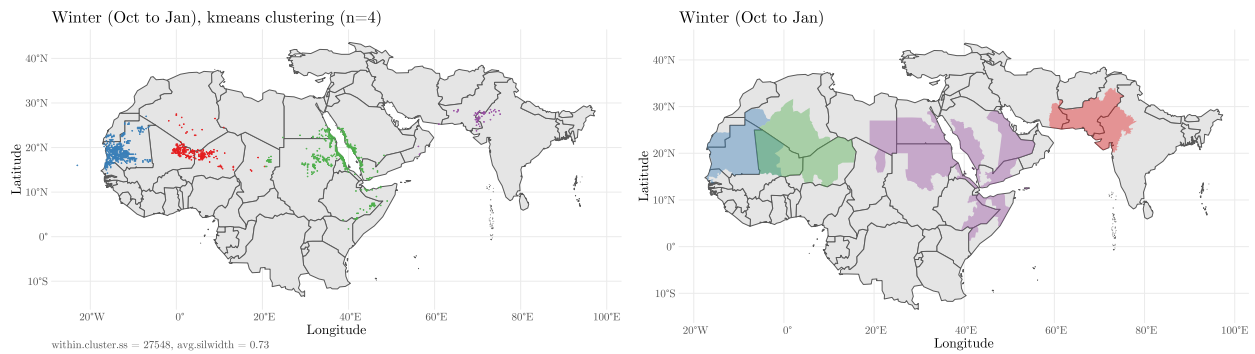
A.1 Locust breeding areas

Figure A1: Outlining Locust Summer and Winter Breeding Areas

- (a) Best Clustering Output for the Summer Season (b) Admin-1 Envelope Applied to Summer Clusters



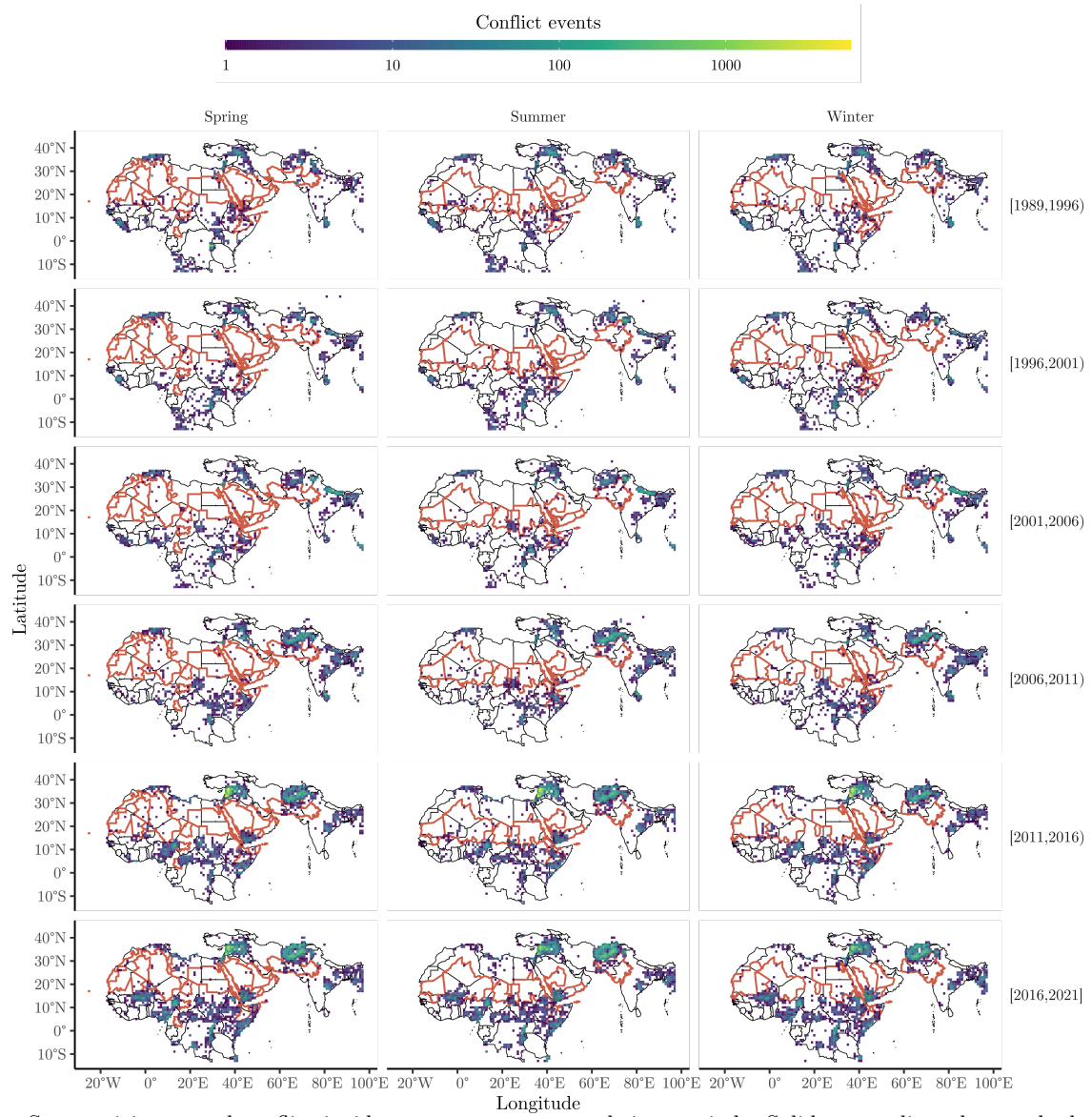
- (c) Best Clustering Output for the Winter Season (d) Admin-1 Envelope Applied to Winter Clusters



Notes: See main text for details.

A.2 Conflict Data

Figure A2: Spatiotemporal Variation in Armed Conflict



Notes: Summarizing armed conflict incidence across seasons and time periods. Solid orange lines denote the boundaries of locust breeding areas.

B Methods Appendix

B.1 Construction of the Weather Data

Weather data comes from ERA5, a reanalysis for global climate and weather from European Centre for Medium-Range Weather Forecasts (ECWMF). We use the hourly estimates on precipitation, maximum, and minimum of two meters temperature. We calculate the mean of two meters temperature from the maximum and minimum values.

We transform the hourly data on precipitation and mean of two meters temperature into daily weather data. We adjust the time from standardized into local timezone to get better measures of the daily values of each variable. With the adjusted local timezone daily data, we calculate the monthly total precipitation and number of days in each temperature bin between 1 to 99 percentile. To aggregate the data at each DHS cluster, we created a polygon buffer of 150 kilometers around each cluster point. We put weights on each intersecting grid cells for the aggregation. We aggregated the monthly data for total precipitation and the number of days in each temperature bin at DHS clusters using the weights.

B.2 Determining the spatial extent of local locust exposure

To determine the spatial extent of locust exposure, we evaluate the spatial clustering of the locust point data by using Ripley's K-function (Goreaud and Pélissier 1999; Haase 1995). The main intuition behind this method is that we are assessing whether a point has a higher density of neighboring points around it—the K value—relative to what we would expect given the mean concentration of all points in the study area if all points were randomly distributed across space.¹² In Figure B1a, we report that the difference between the observed and expected K values increases with distance, and peaks around the cutoff of 150km. When the observed K is higher than the expected K, which is calculated under the assumption of complete spatial randomness, the data are considered to be spatially clustered. In Figure B1b, we estimate the function separately in each year and plot the distribution of the intersection of the observed and expected K values.

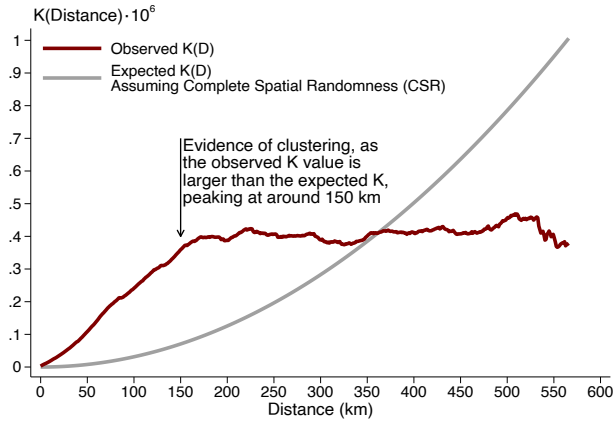
We choose the threshold of 150km as a conservative threshold. Even though the observed K value is higher than the expected one as far out as 350km there is a clear change in the deviation

¹² The following provides a succinct review of the Ripley's K function method: <https://pro.arcgis.com/en/pro-app/3.0/tool-reference/spatial-statistics/h-how-multi-distance-spatial-cluster-analysis-ripl.htm>

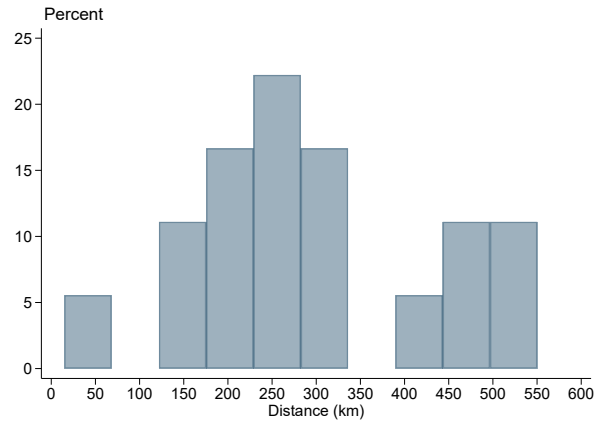
from observed to expected K values at 150km.

Figure B1: Assignment of Treatment Status Using Spatial Clustering

(a) Evidence for Spatial Clustering of Locust Swarms



(b) Distribution of Intersection Between Observed & Expected K Values



Notes: (a) We use all years and countries in the sample to assess the degree of spatial clustering of swarm events using Ripley's K function. There is evidence of spatial clustering that extends as far as 350km, however, we use 150km as the threshold to define exposure because the observed K function peaks at 150km and remains flat thereafter. (b) We repeat the estimation of the Ripley's K function for all countries, year-by-year, and plot the distribution of the point of intersection between the observed and expected K values. Here the distribution peaks at 250km, yet 150km shows up as the first instance with more than 10% of density, while K values below 50 km only account for 5% of the density.

C Results Appendix

C.1 Predicted Swarm Time Series Using Reconstructed Teleconnections

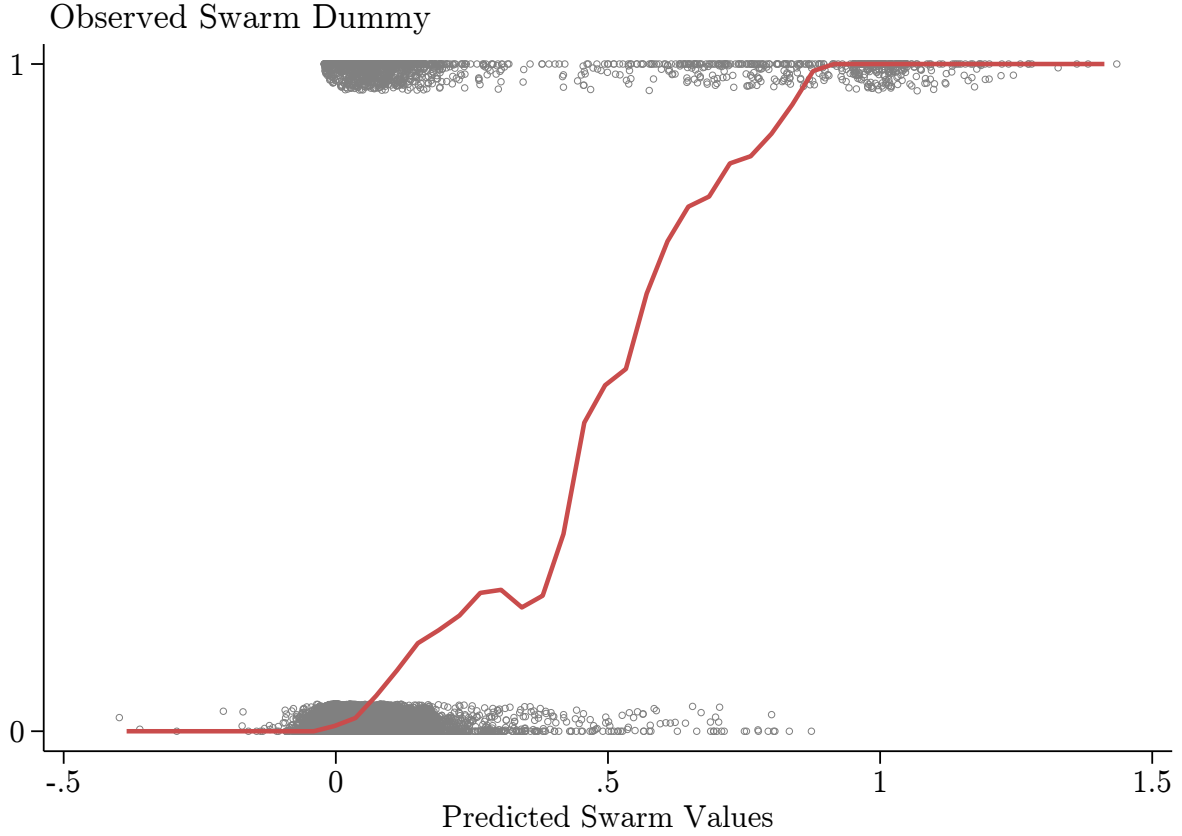
In the main text, we describe how we use the LASSO to construct predicted swarm values that link grid cells with breeding areas. In Figure C1, we provide a simple descriptive result that summarizes the correlation between the observed swarm dummies in each grid cell, at the monthly level, and the predicted values from the LASSO procedure. Three stylized facts arise here. First, the correlation between observed and predicted values is positive, and almost linear (captured by the local polynomial red line fit). Second, very low predicted values can occur even when there are observed swarms. Finally, as the predicted values get close to one, they always correctly predict a truly observed swarm. Even though the correlation is not perfect—some swarms get assigned with low predicted values, and vice-versa—we observe a strong pattern of positive correlation between the two, validating that the predicted values are a useful proxy for swarm presence.

C.2 Locust Outbreaks and Food Security

For locust swarms to have an impact on nutritional status, they must impact access to food. We provide support for this claim by studying the effect of locust outbreaks on food emergency declarations. Food emergencies and famines are often not the result of insufficient aggregate food supplies, but of issues with food distribution systems (Sen 1982; Meng et al. 2015). If agricultural production data were available at a subnational level, we would have been able to test for the relationship between locust outbreaks and food production—unfortunately, such data are not available for most of the countries in our sample. Instead, we use data on food emergency declarations from the Emergency Events Database (EM-DAT). Our goal is to test whether locust outbreaks increase the likelihood of a food emergency declaration. Food emergencies are declared at the national level, or at the admin-1 level. We use both types of declarations in our sample, which spans 1985 to 2020, and include 13 declarations at the national level, and 156 declarations at the admin-1 level. When a declaration is at the national level, we consider each admin-1 as treated.

The following specification uses the dummy variable for a food emergency declaration, at an admin-1 a , country c , WHO region r , and year t , to run a linear probability model as a function of locust swarms:

Figure C1: Observed Swarms Versus Predicted Swarms



Notes: Correlation between the observed swarm dummy and the predicted swarm value using the LASSO procedure described in Section 6.

$$(\text{Food Emergency Declaration})_{act} = \beta(\text{Locust})_{act} + \delta_{rt} + \lambda_a + \varepsilon_{act} \quad (7)$$

We construct the $(\text{Locust})_{act}$ variable to either be a dummy for the presence of a locust swarm in the admin-1 area in a given year, and/or lags of locust swarm presence. Alternatively, we split the non-zero count of locust swarms into terciles. We include WHO-region-by-year fixed effects to account for flexible time trends that absorb pooled shocks such as droughts, and geopolitical instabilities that affects the region.¹³ Any unobserved heterogeneity is absorbed by the error term,

¹³ The WHO divides the countries in our sample to the following regions: Africa (Burkina Faso, Cameroon, Chad, Cote d'Ivoire, Ethiopia, Guinea, Kenya, Mali, Niger, Nigeria, Senegal), Eastern Mediterranean (Egypt, Jordan, Morocco, Pakistan), and South-East Asia (India). The results presented here and in what follows are robust to different definitions of the regions.

ε_{acrt} . We cluster the standard errors at the admin-1 level.

Table C1.

Food Emergency Declaration & Locust Swarms

	(1)	(2)	(3)
Swarm exp. (t)	0.023*** (0.008)	0.026*** (0.008)	
Swarm exp. ($t - 1$)		-0.012 (0.0088)	
Swarm exp. ($t - 2$)		0.016** (0.008)	
Swarm Count (L)			0.016 (0.011)
Swarm Count (M)			0.034*** (0.013)
Swarm Count (H)			0.024** (0.012)
Observations	4,871	4,605	4,881
Mean Dep.Var.	0.072	0.077	0.072
R^2	0.52	0.53	0.52

Notes: Results from the specification in Equation (7). All regression include admin-1 fixed effects, and WHO-region-by-year fixed effects. Standard errors clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

The results in Table C1 show a strong correlation between the presence of a locust swarm and a declaration of a food emergency. We estimate that the presence of a locust swarm in an admin-1 area increases the likelihood of a food emergency declaration for the area by 2.3 percentage points (column 1), reflecting an increase of 32% relative to the mean of 7.2%. Including lagged exposure does not make the effect size disappear (column 2). Finally, when splitting the count of non-zero swarm events into terciles, we find that the positive relationship between locust swarms and food emergency declaration is driven by the second and third terciles (column 3). In other words, we only observe this positive correlation when there is a high number of reported swarms, which would happen during an outbreak event.

Table C2.
Height-for-Age & Food Emergency Declaration

	Standardized Height	Stunting Dummy
	(1)	(2)
Food Emergency Declaration	-0.305** (0.135)	0.085*** (0.032)
Observations	410,072	410,072
R^2	0.102	0.068

Notes: Estimation results from running a regression of height-for-age outcomes on a dummy variable for there being a food emergency declaration in the admin-1 region of the DHS cluster. Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

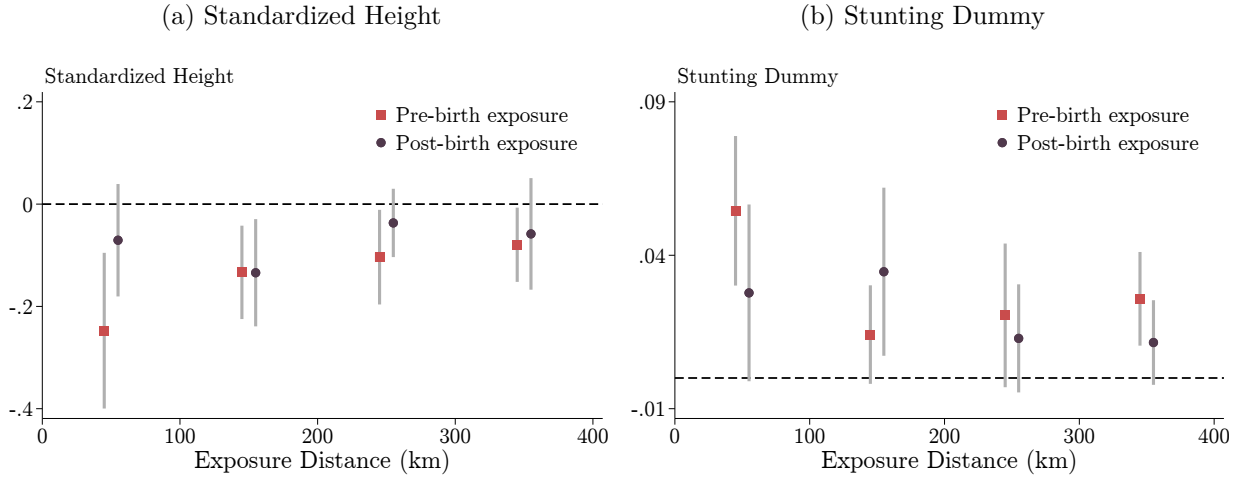
* 0.10 ** 0.05 *** 0.01

C.3 Examining the Scope for SUTVA Violations

In the main text, we discuss how the data-driven choice of the 150 km threshold to define swarm exposure might result in SUTVA violations. In short, it is still plausible that DHS clusters for which we observe a swarm further away than 150 km are still experiencing some negative impacts from that swarm, especially if that remote swarm is indicative of an undetected or unreported swarm closer to the DHS cluster (a false negative). In our assignment of swarm exposure we would treat the DHS cluster as part of the control group, which could potentially attenuate our estimates. Here we demonstrate that we can detect effects even when we account for swarm exposures that extend beyond 150 km.

We estimate a similar specification to the one in Equation (4), but including mutually exclusive dummies for swarm exposure in 100 km distance bins. Specifically, we include four dummies for swarm exposure in the nine months pre-birth, and four dummies for swarm exposure in the nine months post-birth. Each dummy is equal to one if we observe a swarm in the 0-100, 100-200, 200-300, and 300-400 km ranges in each period. We report the estimation results in Figure C2. We estimate the proximity to observed swarms results in higher impacts on height-for-age and the likelihood of stunting, but the we still recover coefficients that suggest that exposure even up to 400 km might have an negative impact.

Figure C2: Locust Exposure Around Birth & Children’s Height: Exposure Distance Bins



Notes: Estimation results from Equation (4). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes observations from rural households. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

To further address how this might affect attenuation in the main sample and baseline specification, we re-estimate our model while excluding DHS clusters that are within 150 to 300 km or 150 to 400 km. In Table C3, we report results for both the swarm dummies and the predicted swarm values versions of the result. Overall, we recover larger coefficients (in absolute values) relative to the sample where we do not exclude these “donut” DHS clusters. The change in effect size is meaningful, yet we cannot reject that the coefficients are statistically different than one another.

C.4 Sample & Specification Variations

Excluding India In the main text, we pool all the DHS countries with swarm exposure across Africa and Asia. In Asia, the only country for which we have both swarm exposure data and geocoded DHS data are India. India reflect a large number of children observations in the sample, making up 42% of the pooled sample. We test for the robustness of our results when excluding India from the sample. In Table C4, we report results separately for India only, and for countries in Africa only. In short, this estimation demonstrates that our main results are driven by the non-India sample. In Panel A, we fail to detect any impacts of swarm exposure on children’s heights. In Panel B, we estimate results that are very close to those we report in the main text.

Excluding Movers In the main text, we include the data from all the interviews in each rural DHS cluster, even for those who answer they have ever moved and migrated. Because migration is a potential endogenous response to swarm exposure, we report results that exclude any records where the mother has answered they migrated to the surveyed village. Despite shrinking the sample size by 70.1%, from 341,862 to 99,777 records, we estimate similar results to those in the baseline sample. Exposure to swarms lowers height-for-age and increases the stunting likelihood, albeit the coefficients are a bit smaller (in absolute values), yet we cannot reject that they are the same at the 95% confidence level.

Specification Sensitivity In the main text, we include admin-1 fixed effects in our baseline specification. Here we examine how including more granular fixed effects affect our results. In Table C6 we report three variations to the unit fixed effects. We either include admin-2, DHS cluster, or mother fixed effects. Each subsequent choice of fixed effects nests the one above it (admin-2 nest admin-1, DHS clusters nest admin-2, and mother fixed effects nest DHS clusters). We recover nearly identical results across all three fixed effects versions, and those are nearly identical to the results we report in the main text. In short, the choice of unit fixed effects does not appear to meaningfully affect the results. However, including mother fixed effects does reduce the sample size by 20.6%, from 341,862 in the main sample to 271,552, as this specification limits the sample to mothers for which we observe at least two separate births.

Urban DHS Clusters In the main text, we report results for the sample of children in DHS clusters that are classified as rural—as we expect the majority of locust swarm exposure to be concentrate in rural and not urban areas. Here we report results that also use data from the urban DHS sample. In Table C7, we report two sets of results. In Panel A, we estimate the same baseline specification for swarm exposure dummies and predicted swarm values using the urban sample only. We estimate smaller effect sizes, and only for the exposure that occurs during the nine months pre-birth. In Panel B, we combine the rural and urban data, and estimated a specification with a pooled effect as well as an interaction with a rural dummy variable. This specification allows us to easily test whether the effect in rural areas is meaningfully and precisely estimated to be different than in urban areas. We find that rural areas experience a larger negative impact in all cases, using either swarm dummies or predicted swarm values, in either the pre-birth or post-birth

periods. These findings validate our prediction that local food insecurity due to swarm outbreaks should have a larger effect in rural than in urban areas. For completeness, we report the results for the first stage in the urban sample in Table C8.

Accounting for Locust Control Operations In the main text, we discuss how control operations—most likely to be insecticide spraying—might confound our analysis. In other words, the effect we observe could potentially be driven by the exposure to insecticides, and not due to the degraded food security conditions. Here we directly test for the effect of exposure to control operations. Specifically, we define the control exposure dummy in the same way as the swarm exposure dummy—we set it to one if we observe a control operation point in the FAO data within 150 km of the DHS cluster in a given year-month combination. Unlike swarm detection, we expect false negatives to be less of a concern with respect to control operations.

We do not find evidence that support the notion that negative effect we estimate can be attributed to control operations (insecticide spraying). We estimate three specifications and report them in Table C9. First, we only include the dummy variables for control exposure in the months before and after birth (columns 1 and 4). Then we include both control and swarm exposure dummies (columns 2 and 5). Finally, we include control exposure dummies and predicted swarm values (columns 3 and 6). We failed to detect meaningful and precisely estimated negative health impacts from exposure to control operations. More importantly, we do not detect meaningful changes to our estimates for swarm exposure or predicted swarm values when we account for control operations.

Sex-Specific Effects In the main text, we do not report results that examine heterogeneity by sex-at-birth. In Table C10, we report estimation results that pool the effects for males and females, and include an interaction term with a female dummy variable. In each case, whether using swarm exposure dummies or swarm predicted values, we find that there is meaningful heterogeneity by sex-at-birth—male births experience a larger penalty relative to female births. This might suggest that female fetuses are experiencing a larger negative selection, resulting in higher rates of fetal deaths.

Table C3
Locust Exposure Around Birth & Children's Height When Excluding Donut DHS Clusters

	Standardized Height		Stunting Dummy	
	(1)	(2)	(3)	(4)
	FE	RF	FE	RF
Panel A: Excluding 150-300km Exposures				
Swarm Exposure, 9m pre-birth	-0.490*** (0.170)		0.064** (0.030)	
Swarm Exposure, 9m post-birth	-0.302*** (0.079)		0.115*** (0.018)	
Mean Pr. Val. 9m pre-birth		-0.886*** (0.279)		0.161** (0.073)
Mean Pr. Val. 9m post-birth		-1.262*** (0.209)		0.323*** (0.073)
Observations	314,123	314,123	314,123	314,123
R^2	0.103	0.104	0.070	0.070
Panel B: Excluding 150-400km Exposures				
Swarm Exposure, 9m pre-birth	-0.553*** (0.198)		0.070** (0.032)	
Swarm Exposure, 9m post-birth	-0.259*** (0.087)		0.112*** (0.021)	
Mean Pr. Val. 9m pre-birth		-0.635** (0.294)		0.068 (0.084)
Mean Pr. Val. 9m post-birth		-1.269*** (0.257)		0.308*** (0.078)
Observations	303,233	303,233	303,233	303,233
R^2	0.103	0.103	0.069	0.070

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural observations but excludes all observations exposed to a swarm between 150 and 300 km (panel A) and 150 and 400 km (panel B). Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

Table C4
Locust Exposure Around Birth & Children's Height When Excluding India

	Standardized Height		Stunting Dummy	
	(1)	(2)	(3)	(4)
	FE	RF	FE	RF
Panel A: Only India				
Swarm Exposure, 9m pre-birth	-0.126		0.086**	
	(0.135)		(0.032)	
Swarm Exposure, 9m post-birth	0.000		0.000	
	(.)		(.)	
Mean Pr. Val. 9m pre-birth		-0.176		0.040
		(0.380)		(0.161)
Mean Pr. Val. 9m post-birth		-0.031		0.002
		(0.804)		(0.230)
Observations	157,939	157,939	157,939	157,939
R^2	0.072	0.072	0.044	0.044
Panel B: Only Countries in Africa				
Swarm Exposure, 9m pre-birth	-0.347***		0.065***	
	(0.072)		(0.011)	
Swarm Exposure, 9m post-birth	-0.253***		0.082***	
	(0.051)		(0.010)	
Mean Pr. Val. 9m pre-birth		-1.362***		0.307***
		(0.227)		(0.046)
Mean Pr. Val. 9m post-birth		-0.567***		0.160***
		(0.197)		(0.058)
Observations	224,972	224,972	224,972	224,972
R^2	0.143	0.144	0.103	0.104

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes observations from households in Southeast Asia (panel A) and Africa (panel B). Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

Table C5
Locust Exposure Around Birth & Children's Height: Excluding Movers

	Standardized Height		Stunting Dummy	
	(1)	(2)	(3)	(4)
	FE	RF	FE	RF
Swarm Exposure, 9m pre-birth	-0.224*** (0.068)		0.032** (0.014)	
Swarm Exposure, 9m post-birth	-0.116** (0.054)		0.055*** (0.019)	
Mean Pr. Val. 9m pre-birth		-0.918*** (0.221)		0.233*** (0.054)
Mean Pr. Val. 9m post-birth		-1.169*** (0.274)		0.312*** (0.085)
Observations	99,777	99,777	99,777	99,777
R^2	0.153	0.154	0.122	0.124

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes observations from households in which the mother has never moved. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.
* 0.10 ** 0.05 *** 0.01

Table C6

Locust Exposure Around Birth & Children's Height With Various Unit Fixed Effects

	Standardized Height		Stunting Dummy	
	(1) FE	(2) RF	(3) FE	(4) RF
Panel A: Admin-2 Fixed Effects				
Swarm Exposure, 9m pre-birth	-0.349*** (0.077)		0.065*** (0.015)	
Swarm Exposure, 9m post-birth	-0.213*** (0.046)		0.068*** (0.013)	
Mean Pr. Val. 9m pre-birth		-1.167*** (0.274)		0.261*** (0.074)
Mean Pr. Val. 9m post-birth		-0.718*** (0.163)		0.189*** (0.051)
Observations	341,813	341,813	341,813	341,813
R^2	0.125	0.125	0.086	0.087
Panel B: DHS Cluster Fixed Effects				
Swarm Exposure, 9m pre-birth	-0.372*** (0.085)		0.072*** (0.017)	
Swarm Exposure, 9m post-birth	-0.261*** (0.044)		0.079*** (0.013)	
Mean Pr. Val. 9m pre-birth		-1.476*** (0.308)		0.327*** (0.084)
Mean Pr. Val. 9m post-birth		-0.986*** (0.195)		0.245*** (0.059)
Observations	341,525	341,525	341,525	341,525
R^2	0.258	0.258	0.203	0.203
Panel C: Mother Fixed Effects				
Swarm Exposure, 9m pre-birth	-0.325*** (0.083)		0.069*** (0.021)	
Swarm Exposure, 9m post-birth	-0.232*** (0.059)		0.080*** (0.021)	
Mean Pr. Val. 9m pre-birth		-1.295*** (0.330)		0.317*** (0.082)
Mean Pr. Val. 9m post-birth		-0.688*** (0.178)		0.217*** (0.046)
Observations	271,553	271,553	271,553	271,553
R^2	0.480	0.481	0.460	0.460

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-2 (panel A), DHS cluster (panel B), and mother (panel C) fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

Table C7
Locust Exposure Around Birth & Children's Height

	Standardized Height		Stunting Dummy	
	(1) FE	(2) RF	(3) FE	(4) RF
Panel A: Urban Households				
Swarm Exposure, 9m pre-birth	-0.153** (0.067)		0.019 (0.019)	
Swarm Exposure, 9m post-birth	-0.074 (0.095)		0.034 (0.025)	
Mean Pr. Val. 9m pre-birth		-0.848*** (0.270)		0.250*** (0.078)
Mean Pr. Val. 9m post-birth		-0.315 (0.212)		0.071 (0.056)
Observations	135,010	135,010	135,010	135,010
R^2	0.088	0.088	0.052	0.052
Panel B: All Households, Rural Interaction				
Swarm Exp., 9m pre-birth	-0.078 (0.062)		-0.002 (0.016)	
Swarm Exp., 9m pre-birth × Rural	-0.298*** (0.095)		0.074*** (0.026)	
Swarm Exp., 9m post-birth	0.067 (0.082)		0.014 (0.018)	
Swarm Exp., 9m post-birth × Rural	-0.328*** (0.079)		0.062*** (0.021)	
Mean Pr. Val. 9m pre-birth		0.141 (0.334)		-0.062 (0.080)
Mean Pr. Val. 9m pre-birth × Rural		-1.383*** (0.318)		0.355*** (0.087)
Mean Pr. Val. 9m post-birth		0.506** (0.211)		-0.153** (0.066)
Mean Pr. Val. 9m post-birth × Rural		-1.366*** (0.237)		0.376*** (0.067)
Observations	476,872	476,872	476,872	476,872
R^2	0.103	0.103	0.069	0.069

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes observations from rural households (panel A) and all households (panel B). Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

Table C8.
First Stage Results for Locust Exposure Around Birth in Urban Sample

	Adm1 Fixed Effects		Grid Cell Fixed Effects	
	9m pre-birth	9m post-birth	9m pre-birth	9m post-birth
Mean 9m $\widehat{\text{pre-birth}}$	0.703*** (0.100)	0.157*** (0.061)	0.695*** (0.101)	0.174*** (0.057)
Mean 9m $\widehat{\text{post-birth}}$	0.031 (0.041)	1.046*** (0.118)	0.023 (0.039)	1.059*** (0.119)
Observations	135,010	135,010	135,010	135,010
SW F Stat.	41.917	41.917	41.697	41.697
SW F Stat.	83.451	83.451	95.733	95.733
Grid FE			✓	✓
Admin-1 FE	✓	✓		

Notes: Estimation results from Equations (5). Columns 2, 3, 5, and 6 report first stage results. Columns 1 and 4 report reduced form results. Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all urban DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

Table C9
Locust Exposure Around Birth & Children's Height

	Standardized Height			Stunting Dummy		
	(1) FE	(2) FE	(3) RF	(4) FE	(5) FE	(6) RF
Control Exposure, 9m pre-birth	-0.217** (0.097)	-0.055 (0.061)	-0.147* (0.082)	0.032 (0.020)	-0.001 (0.014)	0.016 (0.016)
Control Exposure, 9m post-birth	-0.093 (0.079)	0.023 (0.079)	-0.028 (0.075)	0.023 (0.018)	-0.017 (0.014)	0.006 (0.017)
Swarm Exposure, 9m pre-birth		-0.329*** (0.081)			0.069*** (0.016)	
Swarm Exposure, 9m post-birth		-0.226*** (0.061)			0.077*** (0.015)	
Mean Pr. Val. 9m pre-birth			-1.006*** (0.200)			0.236*** (0.054)
Mean Pr. Val. 9m post-birth			-0.615*** (0.191)			0.155*** (0.056)
Observations	328,425	326,868	326,868	328,425	326,868	326,868
R^2	0.103	0.104	0.104	0.069	0.070	0.070

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.

* 0.10 ** 0.05 *** 0.01

Table C10
Locust Exposure Around Birth & Children's Height

	Standardized Height		Stunting Dummy	
	(1) FE	(2) RF	(3) FE	(4) RF
Swarm Exp., 9m pre-birth	-0.440*** (0.089)		0.091*** (0.017)	
Swarm Exp., 9m pre-birth × Female	0.168*** (0.045)		-0.049*** (0.012)	
Swarm Exp., 9m post-birth	-0.251*** (0.057)		0.082*** (0.014)	
Swarm Exp., 9m post-birth × Female	0.065 (0.050)		-0.025** (0.011)	
Mean Pr. Val. 9m pre-birth		-1.320*** (0.272)		0.321*** (0.070)
Mean Pr. Val. 9m pre-birth × Female		0.493*** (0.156)		-0.170*** (0.036)
Mean Pr. Val. 9m post-birth		-0.786*** (0.217)		0.205*** (0.057)
Mean Pr. Val. 9m post-birth × Female		0.244* (0.133)		-0.063*** (0.024)
Observations	341,862	341,862	341,862	341,862
R^2	0.104	0.104	0.070	0.070

Notes: Estimation results from Equations (4) and (6). Each regression includes admin-1 fixed effects, birth year, interview year, birth month, and interview month fixed effects, which are allowed to differ across WHO regions. Sample includes all rural DHS clusters. Observations are weighted using DHS sample weights (see main text for more details). Standard errors are clustered at the admin-1 level.
* 0.10 ** 0.05 *** 0.01

Table C11
Reported Monitoring Interruptions & Conflict

	Reported Monitoring Interruption ($\bar{Y} = .048$)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conflict Event	0.085*** (0.025)	0.036* (0.021)	0.085*** (0.025)	0.078*** (0.024)	0.077*** (0.024)	0.036* (0.021)	0.022 (0.015)	0.022 (0.015)
Observations	20,592	20,592	20,592	20,592	20,592	20,592	20,592	20,592
R^2	0.016	0.139	0.018	0.071	0.072	0.141	0.195	0.197
<i>Fixed Effects</i>								
Country		✓				✓	✓	✓
Month			✓		✓	✓		✓
Year				✓	✓		✓	✓

Notes: Standard errors are clustered at the country level.
* 0.10 ** 0.05 *** 0.01

Table C12
Swarm Exposure and Food Prices: Only Staple Foods

	Log(price)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Excluding Zimbabwe					
Swarm Exposure	-0.029*	-0.030**	-0.032***	0.023**	0.126***
	(0.016)	(0.013)	(0.011)	(0.011)	(0.016)
Observations	60,349	60,349	60,347	60,347	60,348
R^2	0.957	0.972	0.981	0.982	0.955
<i>Fixed Effects</i>					
Market	✓	✓			
Product Group		✓			
Year	✓	✓			
Month	✓	✓			
Year×Month			✓		
Product×Year				✓	
Product×Month					✓
Market×Product Group			✓		
Panel B: Excluding ≥ 2019					
Swarm Exposure	0.085***	0.089***	0.078***	0.054***	-0.085***
	(0.026)	(0.021)	(0.017)	(0.018)	(0.027)
Observations	50,327	50,327	50,326	50,325	50,326
R^2	0.962	0.975	0.984	0.983	0.959
<i>Fixed Effects</i>					
Market	✓	✓			
Product Group		✓			
Year	✓	✓			
Month	✓	✓			
Year×Month			✓		
Product×Year				✓	
Product×Month					✓
Market×Product Group			✓		

Notes: Standard errors are clustered at the market level. * 0.10 ** 0.05 *** 0.01