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VALUING DISASTER PREVENTION:
DESERT LOCUST MONITORING AND CONTROL

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Valuing Disaster Prevention: Desert Locust Monitoring and Control

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ABSTRACT

Monitoring systems for disaster prevention are costly, and measuring benefits is difficult when monitoring effort is endogenous. We provide the first causal estimate of one such system's impact using three decades of desert locust monitoring data. We document conflict-induced interruptions to monitoring in remote breeding areas, reconstruct how infestations spread to populated areas, and show that exposure to locust swarms around birth decreases child height-for-age, increasing stunting risk by over 7 percentage points. Eliminating the locust monitoring system would induce annual losses of US$25 billion, implying a benefit-cost ratio between 160:1 and 680:1 from child nutrition benefits alone.

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An online appendix is available at http://www.nber.org/data-appendix/w35215

1 Introduction

A substantial share of the harm caused by disasters could potentially be avoided by preventative action. Effective preventative action depends, in turn, on monitoring to detect imminent disasters in time to target a response. Monitoring can sometimes be achieved by a passive system of *in situ* or remote instruments, but in other cases, persistent active surveillance must be maintained. Examples of the latter include monitoring systems for human, plant, and animal pests and diseases, which operate in most countries at a global annual cost easily exceeding half a billion dollars.[1] Deciding how much to spend on these systems depends on measuring their value, but to do so, researchers must overcome two significant empirical challenges. First, the most effective monitoring systems entirely prevent outbreaks, so researchers cannot observe the counterfactual in which monitoring did not take place, often blinding policymakers and the general public to the benefits of monitoring. Second, the level of monitoring is endogenous—a challenge that also applies to impact evaluation in many other contexts, ranging from policing, to social programs, public health, and environmental regulation.[2] Monitoring effort varies widely, such that recorded events may reflect monitoring capacity rather than actual events. The most severe outbreaks may be missing from the data entirely because monitoring failure both precludes preventative action, allowing the outbreak to proceed unimpeded, and implies that the outbreak goes unrecorded.

This paper measures the economic value of one of the earliest and longest-running disaster monitoring systems—specifically, for the desert locust, one of the world's most destructive agricultural pests. Ranging across Central and North Africa, the Middle East, and South Asia (Zhang et al. 2019), the desert locust lives primarily at low population density in remote deserts far from human activity, but periodically enters a frenzied phase of reproduction and swarm development. These swarms may then 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; they eventually develop into plagues affecting entire countries or spanning a continent (FAO 2001). Even a medium-sized swarm can contain 500 million locusts and cover 10 km^2—roughly three times the area of Central Park in New York—consuming the same amount of crops each day as 625,000 people (Latchininsky 2013; Roussi 2020; Spinage 2012).[3] This life cycle creates the opportunity to monitor for swarm development in breeding areas and undertake preventative action before outbreaks spread to populated areas. Established in the early 20th century, the locust monitoring system is an early example of international scientific cooperation extending to the Global South (Uvarov 1943; Worboys 2022), which significantly influenced subsequent cross-boundary early warning systems

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[1] A few examples: the World Health Organization spends over $300 million annually to prevent epidemics and pandemics (WHO 2023); the Global Polio Eradication Initiative's annual surveillance costs exceed $79 million (Duintjer Tebbens et al. 2019); and governments pledged $418 million in 2006 to strengthen avian influenza early warning systems (Roberts 2006).

[2] See, e.g., Di Tella and Schargrodsky (2004), Kreider et al. (2012), Nguimkeu et al. (2019), Dave et al. (2022), Zou (2021), and Assunção et al. (2023).

[3] An individual locust consumes about 2 grams of vegetation per day (Davey 1954). We assume that a person eats about 1.6 kg of food per day (FAO 2025; Bruinsma 2017). Further details are in Section 2.

for droughts, famine, and cyclones.[4] Suggestive evidence for the effectiveness of the monitoring and control system—hereafter, the locust monitoring system—comes from the declining frequency of plague events: Between 1861 and 1960 there were seven major locust plagues, some lasting decades. Between 1961 and 2000, there were only two, neither lasting longer than 4 years.

We use three decades of locust monitoring data to document how monitoring operations in breeding areas affect swarm outbreaks, how these swarms spread, and how they affect child health in populated areas. To overcome the first empirical challenge laid out above—the problem of establishing the counterfactual to effective monitoring—we study conflict events in breeding areas as shocks to monitoring systems, combined with exogenous variation in the suitability of weather conditions for swarm formation. To overcome the second empirical challenge—variance in monitoring capacity and effort that is potentially correlated with both underlying risk and other factors that affect child health—we exploit the fact that locusts follow stable migration patterns from distant breeding areas to populated areas. We reconstruct these patterns and use them to predict locust exposure in populated areas as a function of breeding area activity, creating a highly relevant proxy for exposure that is unaffected by potentially endogenous local monitoring effort.

We first establish that after a year of conflict in locust breeding areas, the likelihood of locust monitoring in a given month falls by 9.2 percentage points (from an average of 28 percent). Conflict directly impedes fieldworkers' monitoring and control efforts, and further reduces control indirectly through the lack of information about emerging swarm outbreaks. Despite the suppression of monitoring effort, conflict over the same period increases the likelihood of swarm detection, by 2.9 percentage points (from an average of 3.6 percent). When conflict occurs during a period of rainfall that favors breeding, this effect intensifies substantially.

Next, in order to infer locust presence in populated areas without relying on local monitoring data, we predict locust presence outside of breeding areas along migration pathways. We use regularized regression (the least absolute shrinkage and selection operator, or LASSO) to detect the stable spatiotemporal connections between locust breeding areas and the affected regions we study, and we use the resulting models to predict locust exposure outside breeding areas.

The third stage of our analysis is to estimate the effects of exposure to locust swarms on child health. We combine our data on observed and predicted locust presence with survey data from 51 Demographic and Health Surveys in 16 locust-affected countries over 27 years to yield a dataset of 409,632 child observations. We estimate effects 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). Using observed locust presence, we estimate that children who are exposed to swarms in the nine months before birth are 0.4 standard deviations shorter and 7 percentage points more likely to suffer from stunting (an 18 percent increase). However, endoge-

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[4] In the Global South, the first regional drought monitoring network was AGRHYMET, established in 1974 (Traore et al. 2014); the famine early warning system (FEWS) was established in 1984-1985 to monitor food security crises across the Sahel (Funk and Verdin 2009); and the first regional early warning network for tropical cyclones was the Panel on Tropical Cyclones (PTC) for South Asia, founded by the WMO/ESCAP in 1972 (Liu et al. 2018).

nous measurement error could create bias of uncertain sign in these estimates. We therefore use predicted locust exposure as a proxy for observed locust exposure—we show that the exclusion restriction required for an instrumental variables approach does not hold. This approach yields effects that are, on a normalized basis, about 45 percent larger on average. The estimated effects on stunting are comparable to the difference between rural and urban stunting rates in our sample (11 percentage points), and to the declines seen during acute food emergencies or famines. These adverse shocks to income and nutrition during infancy and early childhood can have far-reaching consequences later in life (Almond and Currie 2011; De Vreyer et al. 2015; Baker et al. 2020).

We combine the above estimates to quantify the value of the overall monitoring system for the desert locust, in terms of avoided swarm outbreaks and subsequent stunting impacts. We calculate that in the absence of the current monitoring and control system, the countries in our sample would experience increased stunting of an additional 2 million children per birth cohort, which would result in a future decrease of 0.35 percent of GDP, valued at US$25 billion per year. With the current level of locust monitoring and control estimated to cost between $37-77 million per year, this implies a benefit-cost ratio of between 160:1 and 680:1, considering *only* the benefits of averted child stunting. This benefit-cost ratio may be underestimated, because we extrapolate from the marginal effects of reductions in monitoring and do not account for compounding or network effects of the complete removal of the system. It is also likely to increase, as climate change makes the environmental conditions that trigger damaging locust outbreaks more frequent (Salih et al. 2020). A more proximate benchmark comes from realized monitoring interruptions during the recent conflict in Yemen. Applying the same valuation architecture, we estimate that these interruptions, which coincided with favorable precipitation conditions for locust breeding, caused around 445 thousand additional cases of stunting across our study area.

Our primary contribution is to provide the first causal estimate of the value of monitoring for disaster prevention. In doing so, we complement a growing 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., 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), disaster control (Baylis, Boomhower, et al. 2023), or 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 effort is 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 modeling exercises (Roberts et al. 2009; Herida et al. 2016).

Our study also relates to a recent literature on the social and economic consequences of plant pests (see, e.g. Banerjee et al. 2010), and specifically, locusts. While locust biology and control methods have received considerable attention in entomology and agricultural science, the socioeconomic effects of locusts were until very recently not a major topic of research, despite the significant threat to the food supply of millions of people, and in contrast to other drivers of food insecurity and famines.[5] A few studies evaluate the impacts of locusts on child health or infant mortality (Conte et al. 2023; Linnros 2017; Le and Nguyen 2022; He and Lam 2024). Importantly, none of these precedents address the problem of the endogenous measurement of locust prevalence. We show that addressing this problem results in health impacts that are both meaningfully larger—by 45 percent, on average—and statistically distinguishable from estimates that rely only on observed data. Our results complement those of Marending and Tripodi (2023), who use wind direction to predict locust migration in Ethiopia, confirming that locusts affect farm productivity and profits.

Lastly, we contribute to the literature on measurement error. In our setting, it is likely that measurement error is systematically related to the underlying determinants of the outcomes of interest, placing it in the class of non-classical, and specifically, endogenous measurement error. Theoretical analyses show that endogenous measurement error renders OLS estimators inconsistent (Nguimkeu et al. 2019). Most previous strategies to deal with this problem rely on the presence of alternative sources of data on the mis-measured variable (e.g., Kreider et al. 2012), but such alternatives may not be available, as in our case. The strategy we propose is necessarily specific to our context, and other contexts will require similarly tailored approaches. However, our approach of using information about underlying biological and physical processes to propagate a signal from distant source areas to affected areas—thereby circumventing endogenous monitoring in those affected areas—may be conceptually replicable in other contexts.

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 documents the relationship between monitoring and control activities and the likelihood of locust outbreaks. Section 5 describes how locust outbreaks propagate from breeding areas to populated areas. Section 6 analyzes the impact of locust swarms on human well-being. Section 7 provides a calculation of the value of the locust monitoring system. Section 8 concludes.

2 The Cycles of Locust Infestations

Locusts are short-horned grasshoppers of the insect order Orthoptera. What distinguishes the dozen-or-so locust species from the thousands of other grasshopper species is that they exhibit density-dependent changes in behavior, physiology, and morphology. At low population densities, they are *solitarious* and behave as individuals that avoid each other. 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. Breeding occurs in bare sandy soil when it 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

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[5] 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).

types of vegetation, including all agricultural crops (Latchininsky 2013).

Rainfall over 25 mm in two consecutive months is usually sufficient for locust breeding and development (Cressman and Stefanski 2016). When the right conditions arise—typically, sufficient rain for intense breeding, followed by dry conditions that suppress vegetation—locusts find themselves in high numbers, and increasingly crowded around isolated clumps of vegetation. This increased proximity triggers their transition from a solitarious phase to a gregarious phase: individual locusts undergo a behavioral change, where they start to aggregate rather than avoid each other, followed by striking phenotypic changes in color and morphology (Sword et al. 2010). Ultimately, wingless hoppers form marching bands, which are particularly voracious and, upon reaching the adult stage, convert to flying swarms that migrate at a rapid pace. Importantly, phase changes can be transmitted epigenetically across locust generations (Pener and Simpson 2009).

Swarms can reach massive size, with swarm clouds of up to 40 billion individuals (Latchininsky 2013) that extend over 1,000 km^2 in flight (Spinage 2012), exceeding entire city areas. When settled, swarms condense slightly to an average density of about 50 million locusts per km^2 (Symmons and Cressman 2001b). Swarms can also reach great speeds that allow them to fly across a country in a single day. Migration is weather dependent: 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 hundreds of kilometers during a single day (2001b). 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 winds that take them to North Africa (Cressman and Stefanski 2016). Overall, while predicting the precise location of a swarm requires detailed information on wind, weather, and soil moisture, broad seasonal migration patterns are relatively predictable.

Locust monitoring and control aims to prevent local swarm infestations called *outbreaks* from growing into regional *upsurges* that can eventually turn into *plagues*—infestations that affect multiple regions over a prolonged period. On the ground, monitoring and control involves surveying potential breeding areas for locusts (Cressman 2001) and spraying pesticides when deemed necessary (Dobson 2001). This is a costly endeavor: field staff have to travel to remote desert areas and search for locusts, including solitary individuals, across vast areas, and have pesticides and spraying devices available. Because the desert locust is highly migratory, effective monitoring also relies on international coordination and early warning provision. Monitoring and control of locust outbreaks is currently carried out by national offices, regional organizations, and the Food and Agriculture Organization (FAO). In particular, the FAO supports coordination by maintaining a regional observation network, providing forecasts, and spearheading donor fund campaigns to request international assistance and build capacity when conditions deteriorate (Cressman 2001).

Efforts in locust monitoring and control have varied over time, 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. Two important milestones occurred a few decades later: the advent of modern pesticides and the use of planes for airborne spraying (MacCuaig 1958; Gunn 1960), dramatically reducing plague prevalence and frequency. The current monitoring system has benefited from recent advances in understanding locust biology and ecology, improvements in forecasting, and research on biopesticides (Zhang et al. 2019). However, it is significantly constrained by funding cycles that fluctuate with the severity of locust conditions, such that low levels of resources during recession years, when locusts are present in low to intermediate numbers, make it difficult to control new outbreaks early (Zhang et al. 2019). Due to waning international effort (Roussi 2020) and changing weather patterns (Salih et al. 2020), devastating upsurges and plagues have continued to occur roughly once per decade, including a major event in 2019-2022.

The damaging effects of locust swarms on agriculture have been documented since biblical times. An adult locust can consume around its own weight in fresh food every day, i.e. about 2 grams (Davey 1954). This means that even a medium-sized locust swarm of roughly 500 million individuals, covering about 10 km^2, consumes as much food per day as approximately 625,000 people.[6] The sheer extent of damages that swarms cause can be detectable at the scale of an entire economy. For instance, wheat production in India is around 20 percent lower in years of locust upsurges compared to non-upsurge years (Chatterjee 2022). Evidence from a particularly destructive infestation in Syria in 1915 suggests locusts devoured at least 536,000 tons of food over 6-7 months, amounting to a 10-15 percent reduction in the winter harvest and a catastrophic 60-100 percent drop in the summer-fall harvest. Foster (2015) argues that the famine that hit Syria during World War I and claimed half a million lives (out of 3-4 million inhabitants) was in part caused by this locust infestation. Even if total crop losses are negligible at the scale of the economy, they may still be individually disastrous for the affected farmers or cooperatives (Latchininsky 2013). After the 2020 plague in Ethiopia, close to 60 percent of households reported observing locust swarms on their farms, of which 18 percent reported complete crop destruction (Ilukor and Gourlay 2021). Overall, however, precise evidence on the agricultural damage caused by locusts has been limited, partly because of the lack of consistently measured crop yield data across time and space.

3 Locusts Outbreaks & Anthropometric Data

We assemble data on locust reports, weather, armed conflict incidence, and repeated representative cross-sectional surveys of populations exposed to locust swarms. We summarize each data source here and report summary statistics by country in Table A1.

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[6] Note that FAO calculations yield a lower number, as they assume that a person eats about 2.3 kg of food per day, following a USDA estimate (FAO 2025). Given that per capita food consumption is higher in industrial countries than in Sub-Saharan Africa (3440 vs. 2360 kcal/person/day respectively, in 2015; Bruinsma 2017), we adjust the human food consumption parameter proportionally, down to 1.6 kg of food per day, for greater regional relevance.

3.1 Locust Monitoring Data

Locust monitoring data come from the FAO Locust Hub. The dataset records sightings of four categories of locusts, defined by maturity stage and behavioral phase—wingless solitarious hoppers, wingless gregarious bands, winged solitarious adults, and winged gregarious swarms—and whether the observed locusts are breeding. Hereafter, we use "swarms" to denote locusts that have entered a winged gregarious swarm phase. Field visits do not always result in locust sightings. The FAO data include two other types of record. The first describes the "ecology" in a location—the conditions of vegetation and soil moisture. The second is a record of "control" activity for any type of locust control undertaken, including pesticide spraying. While control observations are almost always accompanied by locust sightings, not all locust sightings result in control.

Data are collected by experienced locust officers and other field staff who travel long distances to target areas to perform survey and control operations. Data can also be self-reported locally. Observations are recorded in real-time via GPS-connected devices, or in near real-time, especially for earlier manual records later shared via radio, fax, or email. Records are shared with national locust centers, and centralized and validated by the FAO's Desert Locust Information Service.

Locust monitoring began in 1985, and we use records made until 2020. Figure 1a plots the annual count of records made, by record type. We define a reporting event as a set of one or more records that share a unique date and location. Ecology observations are most frequent, as 88 percent of all reporting events include ecology observations. This is followed in frequency by adult locust observations (25 percent) and control operations (18 percent), while swarms are observed 7 percent of the time. Out of all reporting events that include swarm observations, 62 percent also include a control operation. Figure 1b maps swarm reporting events.

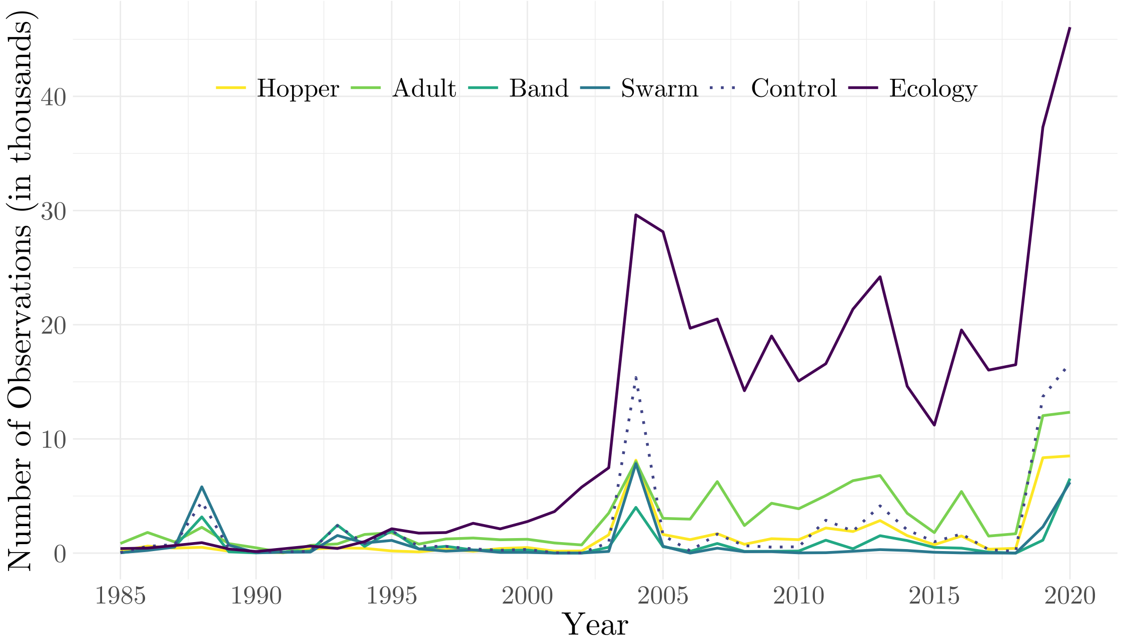
3.2 Locust Breeding Areas

During recession years—periods of low density— locust breeding is restricted to "breeding areas", which extend from Mauritania and southern Morocco eastward through Arabia to the Thar Desert in Rajasthan, India (Magor et al. 2008). These areas are typically where swarm outbreaks originate. Accordingly, breeding areas have been the focus of much preventive monitoring and control efforts, in an attempt to limit locust population increases early in the gregarization process and thus curtail damaging outbreaks (Piou et al. 2017). Indeed, swarm observations are more frequently accompanied by control operations in breeding areas.

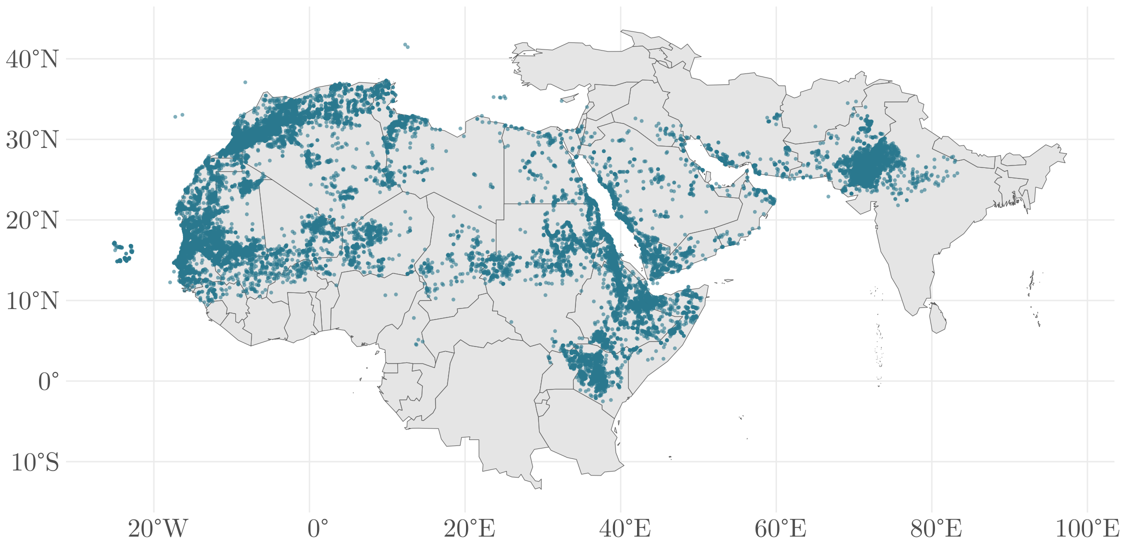
The first step in our analysis is to spatially delineate separate breeding areas. To identify breeding areas at high resolution and in a way that reflects locust ecology during our study period, we use a statistical approach that relies on the locust monitoring data from breeding areas, confirming that the spatial pattern we obtain matches with coarser maps like that provided by the FAO (Symmons and Cressman 2001a). Specifically, we run a spatial cluster analysis on breeding locust observations made in recession years—that is, the 29 years between 1985 and 2020 that are not major outbreak

Figure 1: Spatiotemporal Coverage of Data in the FAO Locust Hub, 1985-2020

(a) Locust Observations Over Time



(b) Spatial Distribution of Swarm Observations



Notes: (a) Locust observations by year, and by record type: *Hopper, Adult, Band,* and *Swarm* refer to different combinations of maturity and behavioral phase; *Control* refers to control efforts undertaken; and *Ecology* refers to the local vegetation and soil moisture conditions. (b) Each dot represents a single locust swarm observation. Source: Food and Agriculture Organization Locust Hub.

years.[7] This helps exclude breeding along swarm migration routes, which can happen in years when locust infestations spill beyond their usual range. Breeding areas are seasonal, meaning that one area might be active in summer, while another is active in spring. Following Symmons and Cressman (2001a), we divide the year into three breeding seasons—spring (February-May), summer (June-September), and winter (October-January)—and apply spatial clustering separately in each.

We construct spatial clusters as follows. We start by evaluating breeding intensity by location and season. To overcome spatiotemporal variability in reporting, we classify each cell of a 0.25-degree grid as a "breeding" location if any observation of breeding locusts falls within that cell and season-year. We then calculate the proportion of recession years when breeding occurs in each grid cell and breeding season, as a measure of breeding intensity. We then cluster grid cells based on latitude, longitude, and breeding intensity, running multiple clustering algorithms (kmeans, dbscan, hdbscan, and hclust) and varying parameter values across realistic ranges. For each season, we select the clustering result that minimizes within-cluster distance and maximizes distance between clusters, and best matches patterns documented by the FAO in the case of similar clustering metrics.

The breeding areas obtained are shown in Figure 2a for spring. In some analyses, the unit of analysis is defined by administrative boundaries. Figure 2b shows a spatial envelope of the spring breeding areas obtained using first-level administrative unit boundaries ("admin-1"). Figure A1 shows similar figures for summer and winter, with further details in Appendix A2.

3.3 Weather Data

Weather data come from ERA5, the fifth generation reanalysis data product for the global climate and weather produced by the European Centre for Medium-Range Weather Forecasts (Hersbach et al. 2020). This data product is considered one of the most accurate reanalysis weather datasets available (Soci et al. 2024; Ibebuchi et al. 2024), which is of particular importance in our setting where *in situ* weather monitoring is relatively sparse (Gleixner et al. 2020). The data comprise hourly estimates of multiple weather variables at 0.25 degree resolution. We use hourly precipitation data to calculate monthly and annual precipitation totals. We use hourly maximum and minimum surface air temperature to calculate, for different temperature bins, the number of days per month or year when daily average temperature falls into that bin. We aggregate both precipitation and temperature variables across space according to the spatial unit of analysis, which differs across analyses (see Appendix B1 for more details). Figure 3 plots total monthly precipitation over time for each breeding area. In the desert climates that characterize locust breeding areas, there is very strong seasonality in rainfall—with long dry periods and bursts of rain—as well as variability across years, with some years experiencing very sharp precipitation shocks.

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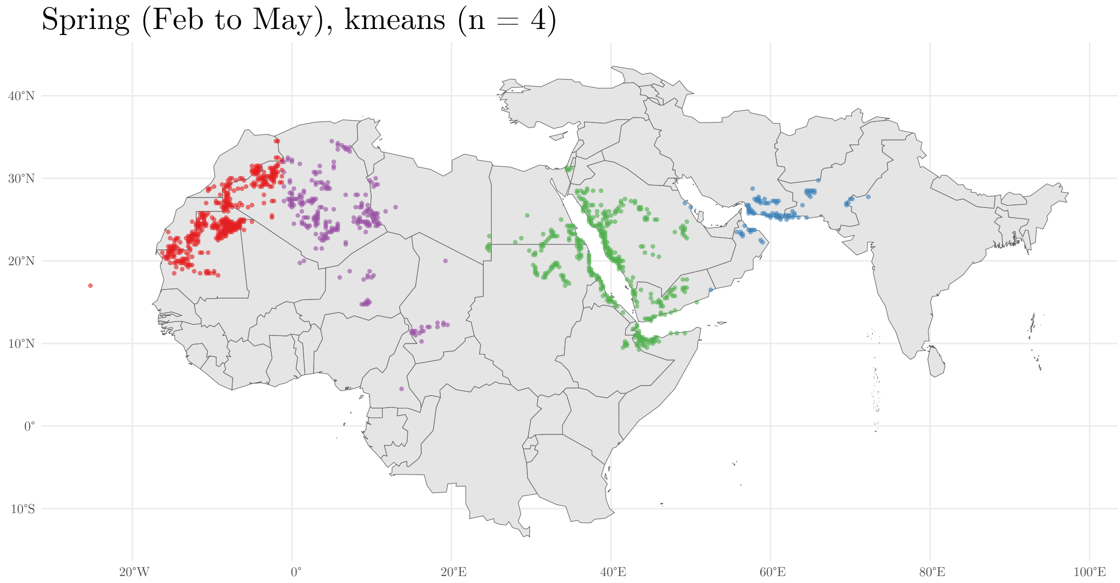
[7] Excluded years are 1987, 1988, 2003, 2004, 2018, 2019, and 2020.

Figure 2: Outlining Locust Spring Breeding Areas

(a) Best Clustering Output for the Spring Season



(b) Admin-1 Envelope Applied to Spring Clusters

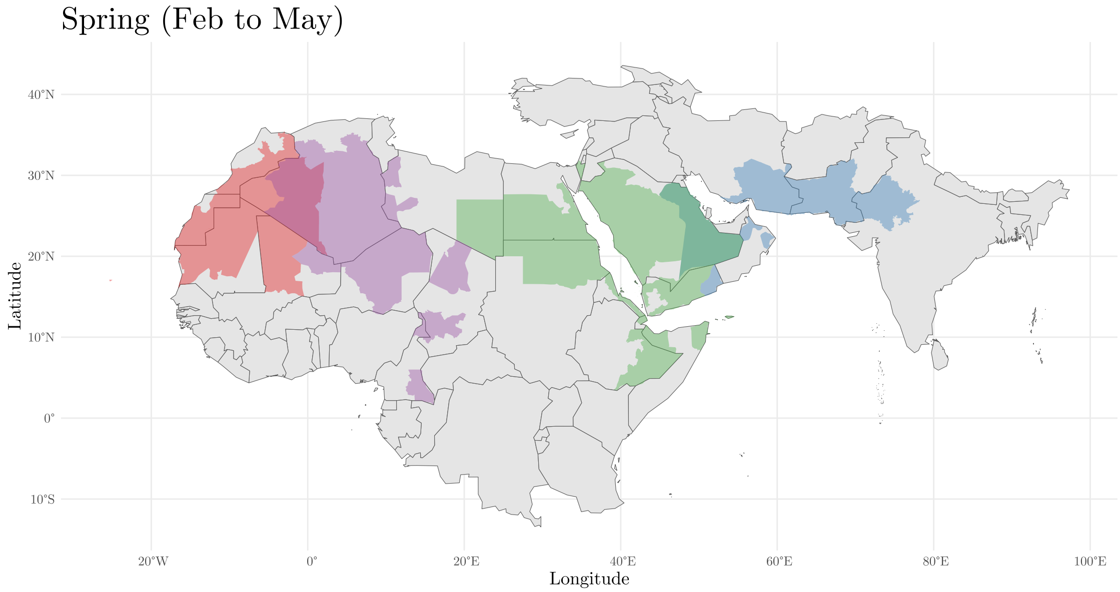
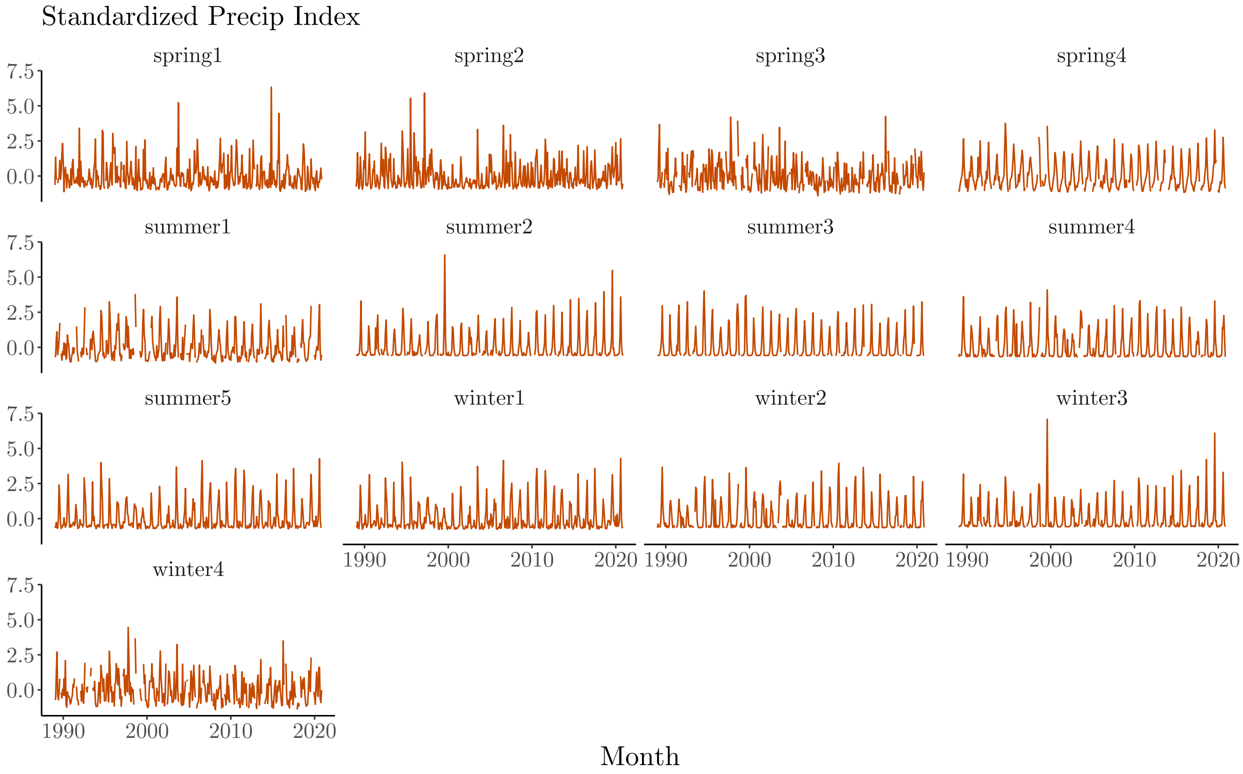


Figure 3: Variation in Precipitation Over Time In Locust Breeding Areas



Notes: We standardize total monthly precipitation separately for each breeding area cluster. "spring1" refers to the first of four clusters in the spring season, "spring2" refers to the second cluster, etc.

3.4 Conflict Data

Conflict data come from the Uppsala Conflict Data Program (UCDP) (Pettersson and Wallensteen 2015). The UCDP provides a global geocoded dataset that contains information about conflict events from 1989 to the present. Each event documents a use of force by an organized actor, either against another organized actor or against civilians, that resulted in at least one death. The UCDP data are more consistently coded and have more accurate geographical information than other global conflict datasets (Eck 2012). Figure A3 maps the incidence of conflict events across 5-year time intervals, showing variation in conflict incidence and intensity over time and space in our sample. Despite their remote location and low population density, the administrative units surrounding breeding areas are nonetheless affected by conflict relatively frequently. We aggregate the UCDP data to create a monthly, subnational panel at the admin-1 level

We aggregate the UCDP data to create a monthly, subnational panel at the admin-1 level. A complicating issue is that multiple events in the data pertain to the same ongoing conflict, which implies that we need to define 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 month in a subnational region where a conflict event occurs after 12 months of no recorded conflict event. We define the *offset* of a conflict as a month in a subnational region that had at least one conflict event in the past 12 months, but no event in the following 12 months. We also construct a dummy variable that captures ongoing conflict, which 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 child health are from the Demographic and Health Surveys (DHS). DHS surveys are sampled to be representative of the urban and rural populations, with the cluster as primary sampling unit and the household as secondary sampling unit. Within a household, anthropometric data are recorded for all children under 5. We use all standard surveys—regular, comprehensive, and large-sample—with anthropometric data from countries affected by locusts, generating a sample of 409,632 child-level observations in 51 surveys from 16 countries between 1992 and 2018. Specifically, we exclude countries if no child recorded in a DHS survey is exposed to a locust swarm, based on the definition of exposure we develop in section 6. We also exclude observations from the Indian islands of Andaman and Nicobar, given their distance from continental India and the absence of locust observations there. Each child's height-for-age Z-score (HAZ) follows the NCHS/CDC/WHO International Reference Standard (De Haen et al. 2011), providing a single measure of child nutritional outcomes comparable across ages and sex-at-birth. We exclude from the analysis a small number of observations (2.6 percent) that are flagged as having improbable HAZ values.[8]

4 Local Relationship Between Monitoring & Swarm Outbreaks

In this section, we document the relationship between monitoring and the likelihood of locust outbreaks. Estimating this relationship is challenging for several reasons. First, monitoring can affect both the level of locust activity and whether it is recorded. When early signs of swarm activity are detected, control efforts can suppress future swarm activity, while when swarms go undetected, they may develop unchecked. Second, because monitoring is costly and requires active effort, monitoring is incomplete, and monitoring intensity varies widely. High levels of monitoring activity can reflect capacity and resources rather than actual locust risk, as monitoring effort is correlated with state capacity (Appendix C1). Interruptions to monitoring systems are also frequent (Figure C2). Together, these problems could create severe measurement error: places appearing free of locusts could include both those with total monitoring failure and severe outbreaks, and those with the most effective monitoring, where outbreaks are completely prevented.

To evaluate the consequences of locust monitoring and control, we therefore need shocks that interrupt these efforts. We turn to a systematically documented reason for interruptions to monitoring: armed conflicts, which pose a major challenge for consistent monitoring and control operations. Between 1986 and 2002, Showler (2003) identifies 14 case studies of conflicts that directly or indirectly interrupted locust surveys or control, positing that such interruptions were a chief reason for the upsurges and plagues observed during the same period. Outside breeding areas, there is

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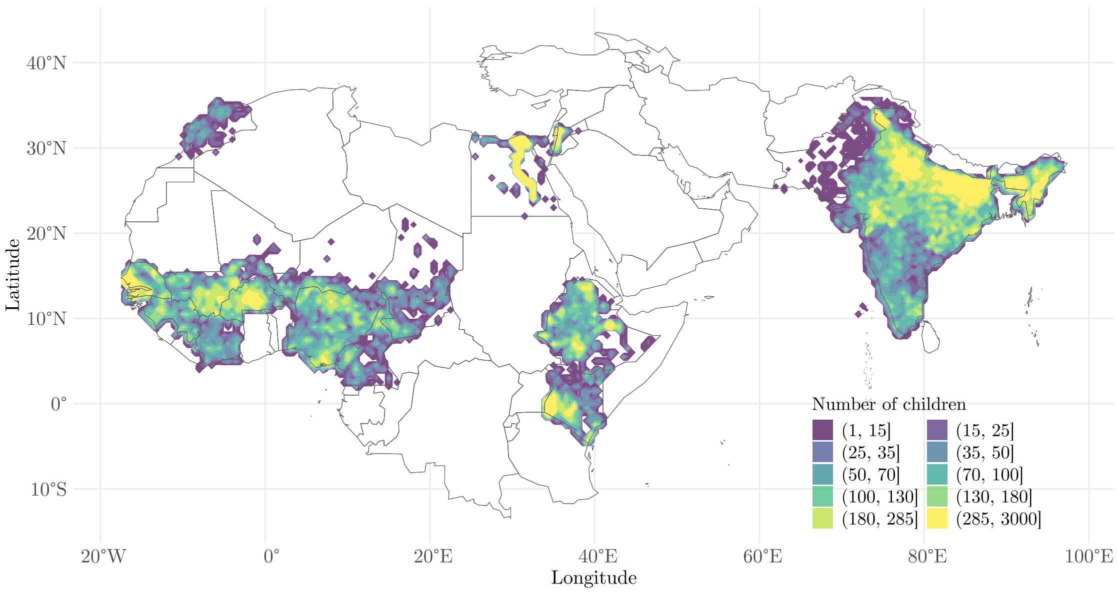
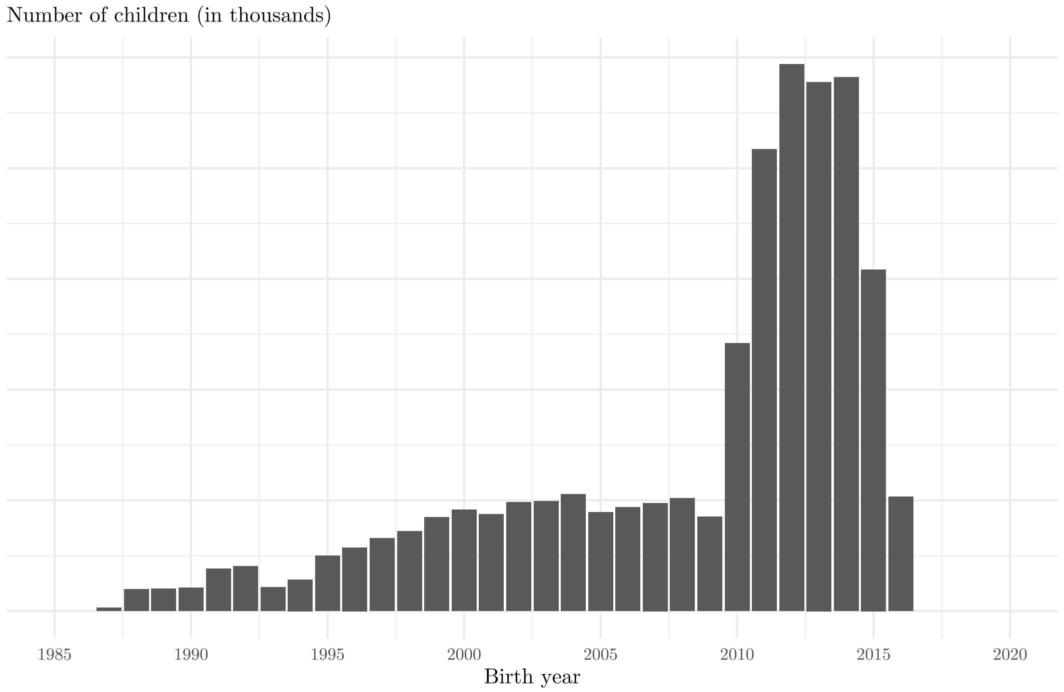
[8] As per DHS guidelines, HAZ with an absolute value larger than 6 are considered invalid.

Figure 4: Spatiotemporal Coverage of Data in the DHS

(a) DHS Clusters & Number of Children



(b) Number of Children in DHS Data by Birth Year

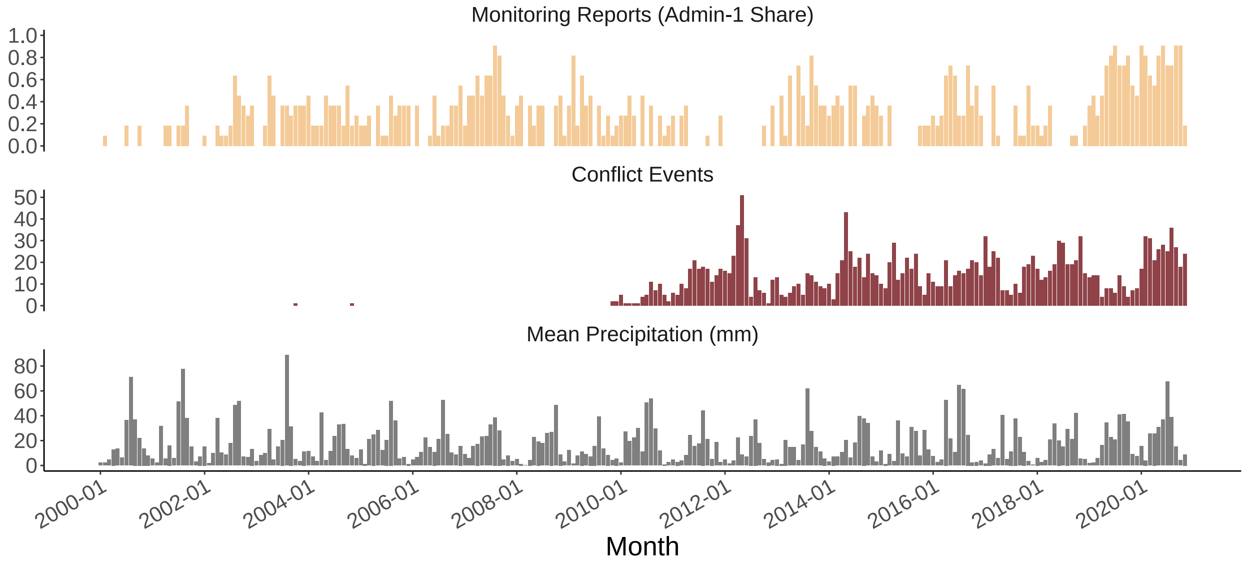


Notes: (a) Location of DHS clusters, with colors denoting the number of children surveyed in each cluster. (b) The number of children surveyed across all the DHS surveys in the sample by birth year.
Source: Demographic Health Surveys.

a documented negative correlation between locust activity and contemporaneous conflict, most likely a product of interruptions to locust monitoring when conditions are unsafe for field workers (Torngren Wartin 2018; Biscaye 2025). Focusing specifically on breeding areas, we test whether (i) violent conflict interrupts monitoring; and (ii) precipitation events that favor locust breeding result in excess swarms when they occur during conflict. When we estimate effects on swarms, we focus on the interaction of the conflict and precipitation shocks, allowing us to separately control for the direct effects of precipitation and any direct effects of conflict on swarms, whether through reduced monitoring and control or (perhaps less plausibly) if conflict affects locusts independently.

An intuitive motivation for our approach comes from the most recent outbreak of locusts in 2019. Observers have attributed this outbreak to monitoring failures caused by conflict associated with the civil war in Yemen (Showler and Lecoq 2021; Gros et al. 2015). Figure 5 plots locust monitoring, conflict, and precipitation over time in Yemen. Before 2010, when conflict levels were effectively zero, there were no prolonged interruptions in locust records, yet after 2010, we observe several periods in which *no* admin-1 unit reported any monitoring activity. Further, the three periods when reporting is most suppressed coincide with large spikes in conflict counts. There are no obvious alternative explanations for this relationship: while drought could cause monitoring interruptions, precipitation patterns during the conflict follow similar seasonal patterns as before.

Figure 5: Conflict and Monitoring in Yemen



Notes: Locust reporting, conflict, and precipitation 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 monitoring failures linked to the civil war in Yemen (Showler and Lecoq 2021).

4.1 Armed Conflict Suppresses Locust Monitoring Efforts

We now quantify whether and to what extent we observe interruptions to monitoring and control during periods of conflict in breeding areas in our full dataset. We assemble a monthly panel of locust outcomes for each admin-1 unit that intersects a breeding area. Combining these data with monthly panel data for conflict events, we estimate the following specification:

$$Y_{at} = \theta \text{Conflict}_{at} + \lambda_a + \delta_{st} + \eta_{bm} + \varepsilon_{at} \tag{1}$$

where Y_{at} is an outcome in admin-1 unit a in month-by-year t, and Conflict_{at} is a measure of conflict in a at time t. The regression includes fixed effects λ_a for each admin-1 to account for time-invariant differences across space, including baseline reporting propensities and locust suitability. To flexibly capture aggregate temporal trends shared by each seasonal cluster of breeding areas (s, denoting whether breeding occurs in an area in the spring, summer, or winter season), we control for breeding season-by-year-by-month fixed effects δ_{st}. This detrends the data separately by breeding season cluster and allowing for pooled shocks active in a particular season. To further capture differences in seasonality across specific breeding areas (b), we include breeding area-by-month fixed effects η_{bm}, thus additionally controlling for the seasonality of each specific breeding area (e.g., the summer breeding area in West Africa). Because an admin-1 can overlap with several breeding areas, there is a many-to-one mapping between admin-1 units and breeding areas.

Our main focus is on how monitoring changes during periods of armed conflict. We measure monitoring effort as any report in the data: locusts of any life stage and gregarization status, control, or ecology. The rationale for aggregating all observation types is that field staff likely submit at least one type of record when they visit the field to monitor locust activity. We construct a dummy variable that is equal to one when there is at least one report and zero otherwise.

We construct multiple versions of Conflict_{at}. The simplest approach uses a dummy variable equal to one if there is a reported armed conflict event in admin-1 a in month-year t. An alternative approach that captures recent conflict conditions is to calculate the mean share of the current and preceding four or 12 months that have had a conflict event. When there is no conflict in the current month and no conflict has occurred during the preceding months, this variable is equal to zero. If, for example, an admin-1 is currently experiencing conflict and conflict conditions also happened in 10 of the 12 preceding months, the conflict share variable for 12 lags will equal $^{11}/_{13}$. We focus here on these univariate measures of conflict, but we obtain similar results if we more explicitly model conflict dynamics (Appendix C2, see also Miguel et al. 2004; Buhaug et al. 2021).

Armed conflict conditions meaningfully reduce locust monitoring. In Table 1, we report coefficients from estimating Equation (1). In columns 1–3, we report results for any monitoring, broadly defined. Whether we use the dummy for contemporaneous conflict (column 1), or the share of recent months with conflict events (columns 2 and 3), we recover the same fundamental result: monitoring declines during periods of conflict. A month with contemporaneous conflict lowers the likelihood of any monitoring occurring by 3.1 percentage points (column 1), reflecting a decline of

11 percent relative to the average. This effect more than doubles or triples in magnitude if the current month and the preceding four or 12 months all had non-zero conflict events (columns 2 and 3). These effects are all precisely estimated, allowing us to reject the null hypothesis that monitoring is unaffected by conflict at significance levels below five percent.

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

	Any Monitoring			Any Swarm		
	(1)	(2)	(3)	(4)	(5)	(6)
Conflict Dummy	-0.031**			0.012**		
	(0.012)			(0.006)		
Conflict Share[0,-4]		-0.072***			0.022*	
		(0.025)			(0.011)	
Conflict Share[0,-12]			-0.092***			0.029*
			(0.032)			(0.015)
Observations	47,230	47,230	47,230	47,230	47,230	47,230
R^2	0.369	0.370	0.370	0.173	0.173	0.173
Mean Dep. Var.	0.276	0.276	0.276	0.036	0.036	0.036

Notes: Each column reports the coefficients from one regression, capturing the effects of conflict incidence (Conflict Dummy), or the share of previous 4 or 12 months with conflict dummy variable outcomes for either any monitoring or any locust swarm detection. 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 errors are clustered at the admin-1 level.
* 0.10 ** 0.05 *** 0.01

Estimates that use an alternative outcome variable definition that captures intensive margin effects as well as extensive margin effects—the count of days in month-year t on which at least one report was made—yield similar results (Table C2, panel B columns 1–3). If anything, effects on the count variable are larger in proportion to the sample average than are the dummy variable estimates, suggesting that monitoring effort declines even when some level of monitoring is maintained.

In columns 4–6 of Table 1, we report the estimated effects of conflict on the detection of locust swarms. Conflict potentially affects the recording of swarms in breeding areas via two opposing effects. Reduced monitoring decreases the likelihood that any given swarm is recorded, which in turn decreases the likelihood that preventative action can be taken to prevent further swarms. The net estimated effects on swarms are positive across conflict definitions, and are precisely estimated at the five percent significance level, a pattern that suggests that the effect on swarm prevalence outweighs the effect on swarm reporting. A month with contemporaneous conflict raises the likelihood of a swarm report by 1.2 percentage points (column 4), reflecting an increase of 33 percent relative to the mean level of swarm report incidence. This average effect could also obscure heterogeneous impacts across environmental conditions, to which we turn in the following subsection.

Further evidence that the effects on monitoring are driven specifically by changes in monitoring capacity or effort comes from reports of ecological conditions that might be favorable to locust breeding. These reports are submitted by field staff regardless of whether locusts are observed (see Figure 1a). We observe essentially the same pattern for ecology reporting (Table C2, columns 7–9) as for any monitoring (Table 1, columns 1–3), confirming that the sharp declines in any monitoring are predominantly driven by *lower levels of monitoring* rather than lower levels of locusts. Alongside the positive effects on swarm detection, this assuages any concern that lower monitoring reflects a decline in locust activity that is somehow systematically correlated with conflict instead of a change in monitoring capacity.[9]

Ex-ante, the effect on control operations is also ambiguous. Conflict may decrease control operations, both directly and indirectly, through reduced swarm detection. At the same time, higher swarm prevalence increases the need for control operations. The net effect is not distinguishable from zero (Table C2, columns 10–12).

4.2 Monitoring Failures During Favorable Weather Increase Locust Swarms

Lapses in monitoring are concerning in general, but their consequences are potentially more severe when they coincide with environmental conditions that are favorable for solitarious locusts to become gregarious. This raises the question of whether the effects of conflict on locust activity differ systematically between periods with and without environmental conditions conducive to swarm formation. To address this question, we expand our model to interact recent conflict with recent precipitation, allowing us to control separately for the direct effects of conflict and precipitation. We first confirm that weather conditions affect locust prevalence and then show that when conflict and favorable precipitation coincide, swarm prevalence increases significantly despite lower monitoring effort. Specifically, we estimate a modified version of Equation (1):

$$\begin{aligned} Y_{at} = &\beta_1 \text{Conflict} \times \text{Precip Share}[t, ..., t-4]_{at} + \beta_2 \text{Conflict Share}[t, ..., t-4]_{at} \\ &+ \beta_3 \text{Precip Share}[t, ..., t-4]_{at} + \lambda_a + \delta_{st} + \eta_{bm} + \varepsilon_{at} \end{aligned} \tag{2}$$

where the outcomes are defined as previously. We define Precip Share$[t, ..., t-4]_{at}$ as the share of recent months that have had sufficient rainfall for gregarization. Our baseline definition uses monthly rainfall above 25 mm, consistent with the required conditions for locust egg development (Cressman and Stefanski 2016). We aggregate this information over the current and preceding four months to capture the dynamic effects of precipitation on swarm development, based on estimates from a distributed lag model, which suggests that rainfall more than four months previously is less predictive of locust activity (see Figure A4). We similarly define the conflict variable, Conflict Share$[t, ..., t-4]_{at}$ such that it captures the share of recent months that have had conflict events. Finally, the interaction variable, Conflict × Precip Share$[t, ..., t-4]_{at}$, is the share of recent

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[9] For example, Biscaye (2025) argues that income shocks following locust swarm events increase conflict risk, though any such effect only materializes after seven years.

months that have had both conflict events *and* above-threshold precipitation—the exact combination of conditions that suppress monitoring and control activity and favour locust breeding. Table 2 reports the results from this specification for monitoring, control, and swarms.

Table 2: Estimating Conflict and Weather Impacts on Swarm Outbreaks

	Any Monitoring	Control	Swarm
	(1)	(2)	(3)
Conflict × Precip Share[0,-4]	0.032	0.020	0.034*
	(0.045)	(0.018)	(0.018)
Conflict Share[0,-4]	-0.068***	-0.010	-0.005
	(0.025)	(0.012)	(0.010)
Precip Share[0,-4]	0.143***	0.087***	0.054***
	(0.022)	(0.013)	(0.009)
Observations	48,512	48,512	48,512
R^2	0.370	0.206	0.175
Mean Dep. Var.	0.274	0.052	0.036

Notes: Each column reports the coefficients from one regression, capturing the effects of the share of recent months with both conflict and precipitation shocks (Conflict × Precip Share[0,-4]), share of recent months with conflict (Conflict Share[0,-4]), and share of recent months with precipitation shocks (Precip Share[0,-4] with Precip defined as $\mathbb{1}\{> 25\text{mm}_g\}$, where g is any 0.25° ERA5 grid cell within the admin-1). 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 errors are clustered at the admin-1 level.
* 0.10 ** 0.05 *** 0.01

Favorable precipitation conditions significantly increase the likelihood of both monitoring and control activities, as well as swarm reports (Table 2). In the absence of conflict, favorable precipitation throughout the recent window is associated with a 14.3 percentage point increase in the probability of any monitoring, as locust officers anticipate a higher likelihood of gregarization (column 1), an 8.7 percentage point increase in the probability of any control (column 2), and a 5.4 percentage point increase in the probability of recorded swarms (column 3). These correspond to effects of 52%, 167%, and 150% respectively, relative to the mean levels of each outcome.

Conflict lowers monitoring activity in both favorable and unfavorable precipitation conditions. In the absence of favorable precipitation throughout the recent window, monitoring is 6.8 percentage points lower when there is also conflict over the same period. With favorable precipitation throughout the reference window, monitoring is 3.6 percentage points lower—reflecting the combined effect of conflict's direct suppression of monitoring (-6.8 percentage points) and the partial attenuation of this suppression during simultaneous periods of favorable precipitation (+3.2 percentage points)—possibly because the importance of monitoring during favorable precipitation conditions partially offsets the conflict-driven suppression.

In contrast, and again despite these reductions in monitoring, conflict either barely changes swarm prevalence or substantially increases it, depending on precipitation conditions. In the absence of favorable precipitation, conflict has essentially no impact on swarms—a barely perceptible and statistically insignificant 0.5 percentage point decrease. That observed swarms barely decrease is noteworthy in itself, given the significant reduction in monitoring in these circumstances. When precipitation is favorable, however, conflict increases swarm prevalence by 2.9 percentage points. This effect is very substantial relative to the mean incidence of 3.6 percent. Put another way, the effect of favorable precipitation conditions on swarms is 63% larger when conflict is present throughout the reference period than when it is absent: an 8.8 percentage point increase compared to a 5.4 percentage point increase. Taken together, these results confirm that monitoring and swarm prevalence covary with precipitation and conflict in ways that are consistent with monitoring failure increasing swarm prevalence, and the consequence of monitoring failure being more pronounced during periods where environmental conditions favour locust reproduction.

For control operations, the estimated effects of conflict—both on its own and interacted with precipitation—are comparatively small and statistically imprecise (column 2). Relative to the pronounced response of swarm reporting, the more muted response of control activity suggests that, during periods of conflict that coincide with favorable precipitation, detected swarm events are less likely to translate into control action.

We test the sensitivity of these results to variations in the lag structure and how we measure precipitation shocks. First, we extend the lag period to 12 months, as correlations between swarm reports and precipitation are significant up to the 12-month lag (Figure A4), suggesting that environmental conditions continue to influence swarm prevalence over this timescale. Monitoring recovers more completely, resulting in an implied effect of conflict on monitoring after favorable rainfall conditions that is no longer negative on net, but the proportional effect on swarms is even larger than with the 4-month lag (Table C4). Second, we define precipitation shocks relative to the local climate, based on the median or the 75th percentile of precipitation at the admin-1 level. The same patterns hold (Tables C5 and C6).

5 Propagation of Locust Activity Across Space

The previous section describes the effects of conflict and weather on swarm activity in breeding areas. However, swarm outbreaks affect human well-being across regions far from the remote desert breeding areas, once swarms reach locations where people live and land is cultivated. From the literature on locust ecology, we know that swarms move along relatively stable migration routes, determined to a large extent by continent-scale prevailing wind patterns (Cressman and Stefanski 2016). We refer to these links between breeding areas and their associated recipient areas as "teleconnections." In this section, we use a data-driven approach to reconstruct these teleconnections.

We use locust monitoring data to identify, for any location where households in our data live, the breeding areas that supply locusts to this location and the temporal lag structure of each connection.

We divide our study region into a 5°x5° grid and run a separate linear LASSO regression for each destination cell, selecting selects from predictors measuring contemporaneous and lagged locust activity in breeding areas. The dependent variable in each regression is a monthly dummy for locust swarm presence in the destination cell. Predictors are monthly measures of contemporaneous and up to six lags of locust swarm presence in each breeding area, represented by a single dummy for whether any swarm was recorded in that breeding area and month. To cleanly separate dependent variable and predictors, we use swarm observations tagged as "breeding" in breeding areas to predict swarm observations tagged as "non-breeding" in destination cells. There is no further structure imposed, so each grid cell can be linked to more than one breeding area and to more than one lag for each breeding area. To reduce the risk of overfitting, we set the penalty parameter by 10-fold cross-validation to minimize out-of-sample prediction error. The output from each model is the set of breeding-area dummies that are most predictive of locust swarm activity in that destination cell.

For a few destination cells, the LASSO does not select any predictors. Since breeding area activity does not predict swarms in those locations, we set predicted swarm values to zero. Observed swarm values in these locations are, in any case, virtually always zero. In practice, none of our conclusions will be sensitive to what value we use for these locations or to focusing only on the subsample of gridcells where the LASSO models select predictors.

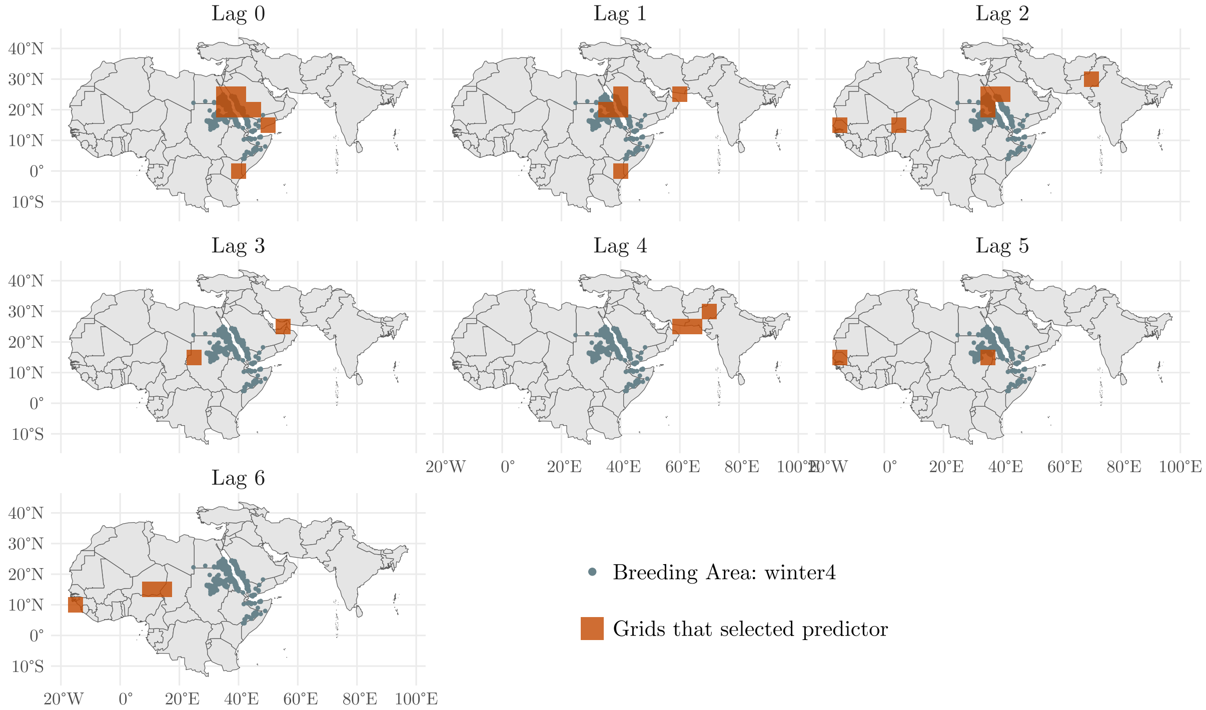
To visualize the reconstructed propagation of swarms across space, Figure 6 shows one winter breeding area in blue and all the grid cells to which it is teleconnected in orange, i.e., the destination grid cells for which the LASSO model chose the dummy predictor from that breeding area. The first panel shows contemporaneous connections, and each subsequent map shows an increasing lagged effect. Although the spatial patterns display some noise, possibly due to correlations among possible LASSO predictors, propagation-like patterns are clearly visible, with teleconnected destination cells tending to be further from the focal breeding area as the lag length increases.

To systematically evaluate the credibility of the reconstructed propagation patterns, we examine the probability of selecting a breeding area as a predictor of destination grid cell swarm activity as a function of the distance between the breeding area and the destination grid cell, separately for each lag. 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 7. In addition, the probability of selection at any lag decreases with distance.

Monthly time series of predicted swarms at grid cell level are well-aligned with observed swarm monthly time series. Predicted swarms explain around a third of the variance in observed swarm exposure, after partialling out variation across space and global trends (Table C7). We show the strong positive relationship between predicted and observed swarms in more detail in Figure C4.

We return to these teleconnections in section 6 and 7. In section 6, we estimate health impacts from swarm exposure. These health impacts implicitly occur outside of breeding areas, where monitoring effort is less systematic and therefore more incomplete and inconsistent, and most problematically, potentially correlated with other local factors that also affect child health. We thus combine information about swarm dynamics within breeding areas with the teleconnections

Figure 6: Inferring Locust Swarm Teleconnections using LASSO



Notes: Set of 5-degree grid cells (brown squares) "teleconnected" to a winter breeding area (blue dots, corresponding to winter breeding cluster #4 (of 4), see Figure 2) at monthly lags from lag 0 to lag 6.

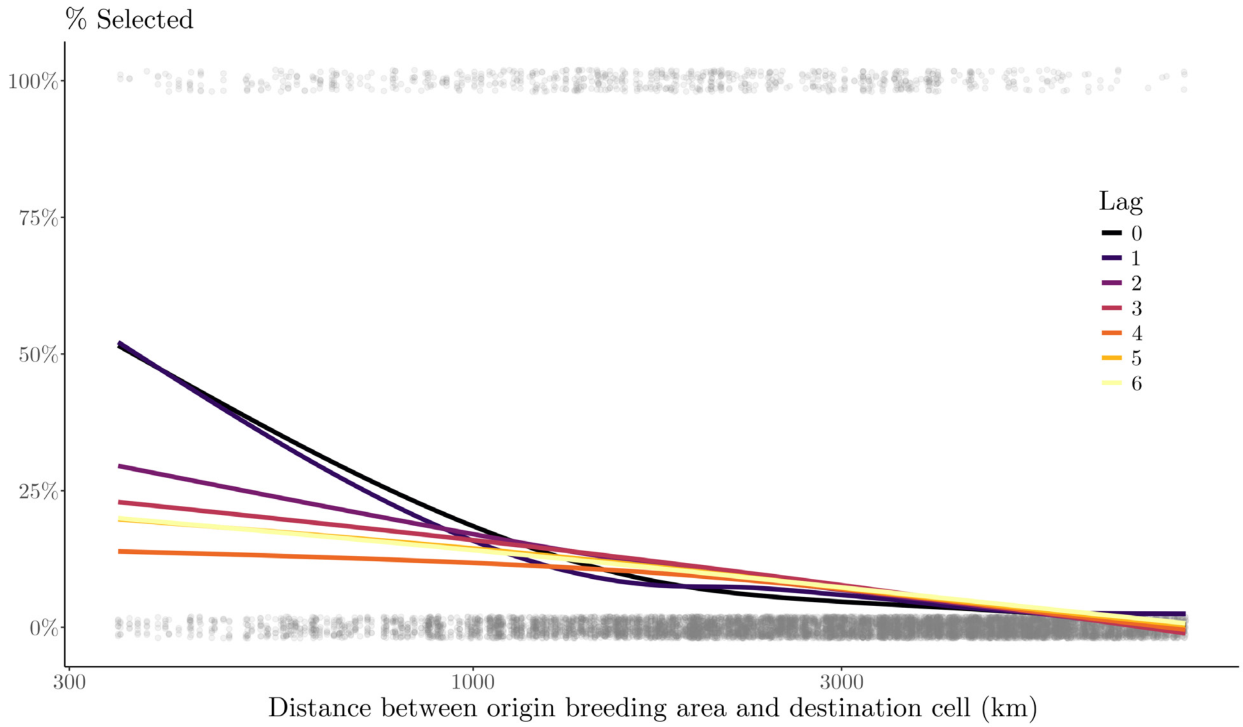
we establish in this section to create a proxy for swarm exposure outside breeding areas that is independent of local monitoring effort. In section 7, we combine our estimates to calculate the value of early warning systems.

6 The Impact of Locusts Outbreaks on Human Well-Being

We now document the human impacts of swarms that escape breeding areas into more populated areas, which will be essential for understanding the value of the locust monitoring system. Specifically, our goal is to estimate the causal effects of locust swarm exposure on human well being.

Swarms decimate vegetation in their path and thus primarily affect humans by destroying crops and pastures, which has direct impacts on food availability. Consistent with this mechanism, locust swarms increase the price of staple foods, suggesting increased scarcity (Table C9). Accordingly, the main outcome on which we focus is children's standardized height-for-age (HAZ) as recorded in DHS surveys. HAZ is a marker of a child's early nutritional environment and linked to cognitive and economic outcomes later in life (Case and Paxson 2008a; 2008b). This outcome captures the potentially broad and persistent human impacts of exposure to desert locust swarms. We focus primarily on children in rural areas, who are most directly affected by the impacts of locust swarms.

Figure 7: LASSO Predictor Selection by Grid Cell Distance from Breeding Areas



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

6.1 Estimation using Observed Locust Exposure

We first estimate effects that simply exploit the spatial and temporal variation in observed exposure to locust infestations. We construct the location-specific time series of locust exposure using the three decades of historical locust records, focusing on the presence of swarms.[10]

To understand whether a child was exposed to a desert locust swarm, we first need to establish the characteristic spatial scale of locust swarm exposure. Monitoring is resource-constrained and, as a consequence, very incomplete, with a large share of swarm exposure events likely unobserved. Unlike a hurricane, for example, there is neither precise tracking of a locust swarm's trajectory over time nor an underlying physical process that lends itself naturally to modeling over time scales of more than a few days. A swarm observed on one day in a given location certainly affected other locations on other days, and where one swarm is observed, others are probably in the vicinity. Since locust swarms can travel hundreds of kilometers in a day, we should expect the spatial scale over which people are exposed when a locust swarm is observed in one location to be large.

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[10] The FAO data does contain an estimate of swarm extent for two-thirds of the swarm observations, but these data are unreliable. Cressman (2001) notes that: "It is nearly impossible to determine with any precision the size of flying swarms from the ground. A very rough estimate can be made by measuring the time that it takes for a swarm to pass overhead, estimating the width of the swarm, and measuring the wind speed [...]. The results should be used with extreme caution."

To estimate this scale, we turn to a data-driven approach based on the spatial clustering of swarm observations. Ripley's K-function compares the observed spatial distribution to a random distribution across space, showing that swarm observations are spatially clustered at a characteristic scale of 150 km. This implies that indeed, where one swarm exposure is recorded, more swarms are likely nearby. In the observed data, when one swarm is recorded, an average of 87 additional swarm reports—which may correspond to the same swarm or to different swarms—are recorded in the same month within a 150 km radius. Other swarms surely went unrecorded. We thus consider any household living within 150 km of a swarm observation to have been exposed. This procedure is described in detail in Appendix B2.

Assigning treatment based on a distance threshold presents a risk of violating the stable unit treatment value assumption (SUTVA). Children with swarms detected further than 150 km away might still be affected by the outbreak. The clusters most likely to be affected are those just outside the 150 km radius used to define treatment. In robustness tests, we extend the 150 km threshold, run "donut" versions of the analysis, and allow the treatment effect to vary by distance.

We also need to account for the timing of exposure. Exposure during the earliest period of life is most likely to be critical for growth and development, and *in-utero* exposure may be particularly important (Almond and Currie 2011). We treat a child surveyed at time t as potentially exposed *in-utero* if any swarm was observed within 150 km 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.

We are interested in how locust exposure affects the outcome y of child i, of birth cohort b, born in month m, observed in DHS cluster d, in country c, in region r, surveyed in month n of year t. Since our data are a repeated cross-section of children born and surveyed at different times and locations, each child i is associated with unique values of $b\,(i)$, $m\,(i)$, $d\,(i)$, etc. We suppress the $.\,(i)$ notation throughout this section for brevity. We estimate how y_i responds to locust swarm exposure using the following regression specification:

$$\begin{aligned} y_i = &\beta_1 \text{Locust}_{bmd}^{pre-9-months} + \beta_2 \text{Locust}_{bmd}^{post-9-months} \\ &+ f(\text{weather}_{bmd}) + \lambda_d + \delta_{rb} + \eta_{rm} + \omega_{rt} + \theta_{rn} + \varepsilon_i \end{aligned} \tag{3}$$

We account for any time-invariant spatial variation in locust exposure or outcomes by including fixed effects at the DHS cluster level, λ_d. 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}, both of which we allow to vary by World Health Organization (WHO) region.[11,12] 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, which might be correlated with locust presence and non-locust drivers of child health, by including flexible controls for local

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[11] In our sample, the WHO classifies Egypt, Jordan, Morocco, and Pakistan in the Eastern Mediterranean region; India in South-East Asia, and all remaining countries in Africa.

[12] We include region-by-DHS survey year fixed effects for completeness. In our main specification, they are redundant, as each DHS cluster is only surveyed in a unique survey year, but this is not the case for robustness tests with alternative spatial fixed effects.

temperature and precipitation in the year of birth, $f(\text{weather}_{bmd})$.[13] Any unmodeled heterogeneity is captured by the error term, ε_i. We cluster our standard errors at the admin-1 level.

We apply weights throughout that adjust for the probability of individual households being surveyed in a given country and year, and for differences in DHS survey sample sizes across countries and year. Specifically, we weight observations by:

$$w_{itc}^{\text{avg. child}} = \left(\frac{\text{DHS weight}_{itc}}{\text{survey size}_{tc}}\right)\left(\frac{\text{population}_{c,2000}}{\text{total surveys}_c}\right)$$

where $DHS\ weight_{itc}$ is the survey weight within country c assigned in the DHS data, which sum to the sample size; $survey\ size_{tc}$ is the DHS sample size in country c in year t; $population_{c,2000}$ is the population of country c in the year 2000; and $total\ surveys_c$ is the number of DHS surveys in country c in our sample. The weights sum to the total population in each country, implying that each country takes weight proportional to its year 2000 population in summary statistics. These weights imply that summary statistics pertain to the average child across the sample countries. While the resulting regression coefficients do not have a straightforward interpretation as average treatment effects if effects are heterogeneous across geographies or time (Solon et al. 2015), weighted and unweighted regression results are very similar, suggesting that in practice the results are not sensitive to the weighting scheme (Table C10).

Whether a child is exposed to a locust swarm is plausibly exogenous, after controlling for variance in expected exposure across space and time, as a consequence of variance in the environmental conditions required for locust infestations. We control for spatial variance in expected exposure at the DHS cluster level—which because DHS clusters are unique to survey rounds also partly captures local time trends—as well as pooled seasonality and time trends at the WHO-region level. The exogeneity of locust swarm exposure arises from the essential unpredictability of the timing and trajectory of a given outbreak. While the sequence of contagion across space follows patterns that are predictable on average, the behaviour of any individual swarm is not deterministic. Neither the affected population nor the FAO can fully predict where and when an outbreak will occur. Thus, if one could perfectly observe swarm exposure, one could plausibly consider outcomes for untreated children in locations that periodically experience a locust outbreak as the counterfactual for treated children in the same locations, and treat exposure as a natural experiment.

When applied to the imperfectly measured swarm observation data, however, an important concern arises: non-classical and potentially endogenous measurement error. Our measure of swarm exposure is binary, implying that measurement error is non-classical. Measurement error is also asymmetric, almost certainly resulting primarily in false negatives. A swarm observation in the data very likely reflects a true swarm detection, but not all swarms are observed, and swarms that are observed in one location may go unobserved when they land in a new location. This implies that we wrongly assign some treated observations to the control group, creating attenuation bias.

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[13] We define child year-of-birth precipitation quartiles globally and temperature-day bins as follows (°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.

Most importantly however, measurement error is potentially endogenous. For example, if greater monitoring capacity is associated with better institutions, and better institutions also result in improved child health, this will create a downwards bias in the estimated effects of locust exposure on child health (see Appendix B3). Monitoring effort could also be influenced by the underlying risk itself, either because of greater efforted exerted because risk is high or because monitoring capacity is strained or overwhelmed during plague events. Endogenous measurement error creates bias of ambiguous sign in the estimated effects of locust exposure when we rely on observed swarm presence.

An additional, albeit more minor concern relates to the potential for omitted variables bias resulting from local weather conditions. Weather conditions that are favorable to locust swarm outbreaks might also affect other determinants of health, such as agricultural yields or infectious diseases. We flexibly account for local weather conditions, but it remains possible that these do not completely account for potential confounding between locust swarm outbreaks and health outcomes.

To address these potential threats to identification, we use a proxy for locust swarm exposure, which we describe in the next section. The proxy builds from the results in section 5 by using desert locust activity in remote breeding areas to predict swarms outside of breeding areas where human exposure occurs.

There might be unobserved local responses to locust outbreaks that also affect our outcomes of interest. For example, local outbreaks could lead to insecticide spraying campaigns which have their own potentially detrimental impacts on health (see, e.g., Frank 2024). To the extent that such campaigns are the direct consequence of an earlier failure to contain locust outbreaks, they continue to represent the causal effects of the outbreak. However, effects driven by insecticide use would have quite different policy implications to effects driven by food insecurity. To understand whether insecticide use drives the results, we report results that control for insecticide spraying.

Our approach requires two further conditions to hold. First, for the DHS cluster fixed effect to capture, among other factors, the mean relationship between agricultural productivity and the outcomes of interest, we are also assuming that locations in our sample are in their post-adaptation equilibrium, such that they do not engage in crop switching or adopt new varieties as a result of locust risk. We consider this a plausible assumption because these societies have been exposed to locusts for centuries, and no new locust-resilient crop varieties have been developed during our study period. Second, our approach also relies on the effects of a locust swarm fading out within a few years, such that conditions return to their baseline level before a new locust outbreak occurs.

6.2 Using Teleconnected Breeding Areas to Proxy for Local Swarm Exposure

To address the problem of potentially endogenous measurement error and the threats to identification that we describe above, we exploit the fact that locust activity in populated areas is driven by locust activity in remote breeding areas via long-distance migration patterns (see section 5). We leverage these patterns to construct a proxy for locust swarm exposure driven solely by activity in remote breeding areas, which thus does not vary with local monitoring effort.

The output of our analysis in section 5 is a monthly prediction of the probability of the presence of a locust swarm at the 5-degree grid cell level, based on LASSO-selected models of contemporaneous and lagged locust swarm activity across teleconnected breeding areas. Motivated by the literature on constructing optimal instruments (Belloni et al. 2012; Chen et al. 2023; Lennon et al. 2022), which uses machine learning methods to aggregate information across many potential predictors, we use the resulting grid-cell–level predicted swarm values as a proxy for observed swarms at DHS cluster locations.

We begin by confirming that our proxy for locust presence captures meaningful variation in exposure to locusts around birth for children in our dataset. We regress a dummy variable for the presence of an observed locust swarm within 150 km of the DHS cluster during the periods of interest—either nine months before or nine months after birth—on the mean predicted values from the LASSO in the relevant 5-degree grid cell for both periods. Because the LASSO estimates a linear probability model, the underlying predictions are not constrained to lie in the [0,1] interval, although in practice the majority (87 percent) do so. We average predicted values across the nine months of exposure because conditions in every month in the period affect the likelihood of exposure during the overall period. We include both pre- and post-birth predicted exposure variables to confirm that they independently predict the corresponding observed exposure variables. Including the same fixed effects and control variables as in Equation (3) yields the following specification:

$$\begin{aligned}\text{Locust}_i = &\beta_1(\text{Mean Predicted Values})_{bmd}^{pre-9-months} + \beta_2(\text{Mean Predicted Values})_{bmd}^{post-9-months} \\ &+ f(weather_{bmd}) + \lambda_d + \delta_{rb} + \eta_{rm} + \omega_{rt} + \theta_{rn} + \varepsilon_i \end{aligned} \tag{4}$$

where the main regressors are now the predicted values we obtained from the grid cell regressions described in Section 5, averaged over the nine-month exposure periods for each child.

Our proxy variable strongly predicts observed exposure to locust swarms. Table 3 reports the estimated coefficients from Equation (4) for pre- and post-birth observed locust exposure. The mean predicted values in the nine months pre-birth have a positive and precisely-estimated relationship with observed exposure pre-birth, and the same holds for the nine months post-birth. The relationships are not one-to-one. Although the underlying predictions do have a one-to-one mapping to swarm exposure risk at the gridcell-month level (Table C7), this mapping is altered when we aggregate monthly predictions to construct the nine-month predicted swarm exposure measures. Also, the sample of children in our analysis is not evenly distributed within gridcells or across time. The statistically significant but smaller off-diagonal coefficients likely reflect serial correlation in locust exposure across adjacent exposure periods. Results are very similar with either destination grid cell fixed effects—the spatial resolution of predicted locust exposure—or the more conservative DHS cluster fixed effects from our main specification. In both cases, Sanderson and Windmeijer (2016) F-statistics for both observed measures of locust prevalence are above 60.

Table 3: Predicting Locust Exposure Around Birth

	Grid Cell Fixed Effects		DHS Fixed Effects	
	9m pre-birth	9m post-birth	9m pre-birth	9m post-birth
$\overline{\text{9m }\widehat{\text{pre-birth}}}$	0.624***	0.319***	0.616***	0.328***
	(0.112)	(0.068)	(0.122)	(0.073)
$\overline{\text{9m }\widehat{\text{post-birth}}}$	-0.113**	0.692***	-0.152***	0.693***
	(0.056)	(0.115)	(0.059)	(0.117)
Observations	409,618	409,618	409,100	409,100
SW F Stat.	75.0	82.6	85.1	64.8

Notes: Estimation results from Equations (4). Each regression includes grid cell (left) or DHS cluster (right) 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 details). Standard errors are clustered at the admin-1 level.
* 0.10 ** 0.05 *** 0.01

Then, using the predicted values as a proxy for local swarm activity, we estimate the following reduced-form specification:

$$\begin{aligned} y_i = &\beta_1 (\text{Mean Predicted Values})_{bmd}^{pre-9-months} + \beta_2 (\text{Mean Predicted Values})_{bmd}^{post-9-months} \\ &+ f(weather_{bmd}) + \lambda_d + \delta_{rb} + \eta_{rm} + \omega_{rt} + \theta_{rn} + \varepsilon_i \end{aligned} \tag{5}$$

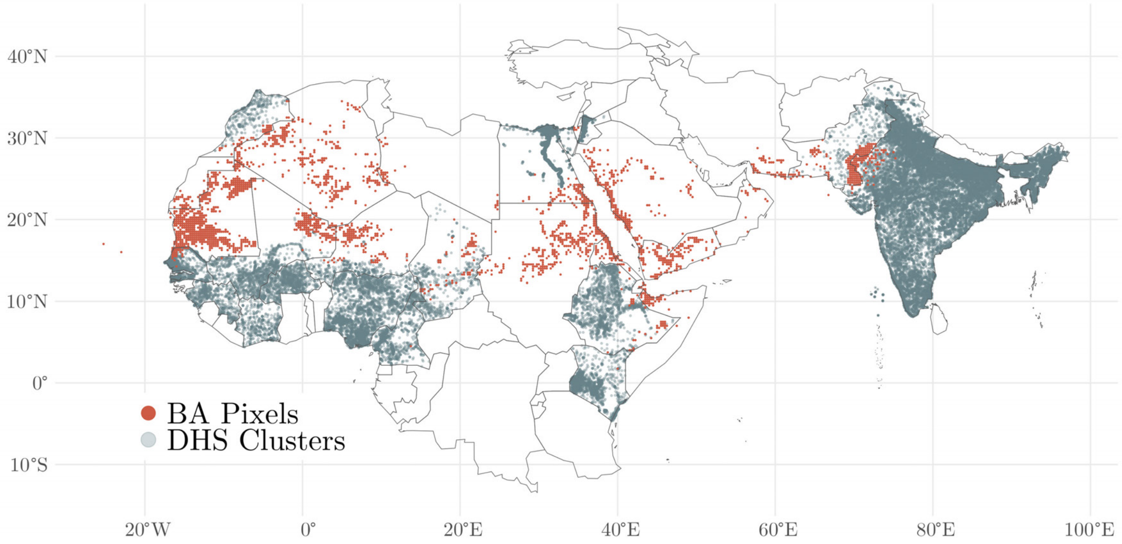
The identifying assumption is that locust swarms originating in remote but teleconnected breeding areas affect child outcomes only through realized local locust presence, conditional on fixed effects and local weather controls. The plausibility of this assumption depends primarily on the distance between breeding areas and the populated locations observed in the DHS. Figure 8 shows that these areas are spatially distinct: even when defining exposure zones as 150-km radii around DHS clusters, only 14 percent of cluster exposure zones overlap with any breeding-area pixel.

A natural concern is that weather shocks are correlated over large regional scales, but all specifications flexibly control for local weather. Another potential concern is confounding from regional conflict, given the relationship we document between conflict and monitoring activity, but results are unchanged when we exclude households exposed to conflict. To bias our estimates, an omitted factor would need not only to operate over large distances but also to vary in a manner that mirrors the precise spatial and temporal pathways of locust migration—a requirement that substantially limits the set of plausible confounders.

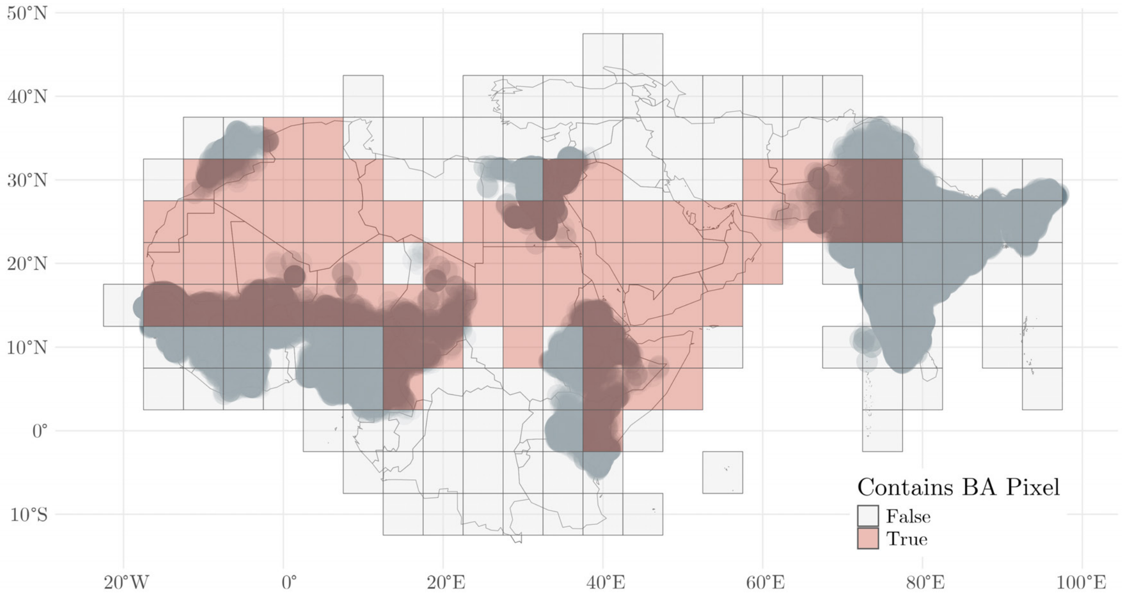
One might be tempted to use our predicted locust risk measure as an instrument for observed swarm exposure in a two-stage least squares framework. However, the standard argument that IV corrects measurement error relies on orthogonality between the instrument and the measurement error in the regressor, as in the canonical case of classical measurement error. We cannot assume

Figure 8: 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.

such orthogonality in our setting. Our proxy captures underlying locust risk, and monitoring effort and reporting capacity plausibly respond to that same risk. Moreover, our observed exposure variable is a misclassified binary indicator. With binary misclassification, even if false positive and false negative rates were independent of underlying locust risk, the realized measurement error will generally be correlated with any instrument that shifts true exposure, because the error is intrinsically linked to the latent exposure itself. As a result, 2SLS need not recover the causal effect of true exposure and instead may identify a rescaled parameter that can be highly unstable. We show this theoretically and using simulations (Appendix B3) and we confirm that 2SLS estimates are indeed substantially inflated relative to OLS estimates (Appendix Table C8).

Under our identifying assumptions, the coefficients we estimate using predicted exposure as a proxy for observed exposure (Equation 5) are unbiased. However, the parameters recovered are implicitly scaled by a factor reflecting how informative predicted locust exposure is about true locust exposure (Appendix B3). Because true locust exposure is unobserved, this factor is unknown. We discuss how to compare effects based on predicted and observed exposure in Section 6.3. The sign and statistical significance of the estimated effects are informative in all cases. To the extent that our predicted measure of locust exposure is itself measured with error, in part because it is trained on observed locust prevalence in the absence of data on true prevalence, this will attenuate our estimates toward zero, rendering our approach conservative.

6.3 Locust Outbreaks Lower Standardized Height in Exposed Children

This section describes the estimated impacts of exposure to locust swarms on height-for-age, comparing results based on observed and predicted exposure. Children who are 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 (HAZ). In Table 4, we report results using observed exposure (Equation (3))and predicted exposure (Equation (5)).

Children who are exposed to locust swarms in the nine months before birth have 0.37 standard deviations lower HAZ (column 1). Children exposed in the nine months after birth have 0.28 standard deviations lower HAZ (column 1). In a population whose mean standardized HAZ is -1.5, these effects increase the likelihood of stunting, defined as being two standard deviations below mean height-for-age. Children exposed to swarms in the nine months before birth have a 7.2 percentage point higher likelihood of stunting, while children exposed in the nine months after birth have an 8.4 percentage point higher risk of stunting (column 3). These changes in risk correspond, respectively, to increases of 18 percent and 21 percent, relative to the sample mean stunting prevalence of 40.2 percent.

The predicted locust swarm results are consistent in sign with the observed exposure results. In columns 2 and 4 of Table 4, we find statistically significant effects on height-for-age and stunting in the same directions as in columns 1 and 3 for exposure both nine months before and nine months after birth. Because predicted exposure is a proxy variable, the magnitudes of the estimated coefficients in columns 2 and 4 are not directly comparable to those in columns 1 and 3 without further assumptions. One approach is to normalize both exposure measures so that we can compare the effects of a one standard deviation change in observed and predicted exposure. So normalized, the predicted locust swarm measures yield meaningfully larger estimates. Across the two outcome variables—standardized height and the stunting dummy—the predicted measures result in effects that are between 18 and 76 percent larger in magnitude, on average 45 percent larger (Table C11). Estimating the normalized effects of observed and predicted exposure in a stacked regression framework allows us to statistically reject the null hypothesis that the effects of observed and predicted exposure are equal (p=0.024 for a joint test of equality of pre- and post-birth observed and predicted exposure effects for standardized height; p=0.042 for stunting).

A reasonable concern is that these comparisons depend on a specific normalization of the two exposure metrics. An alternative approach that does not depend on normalization is to compare the explanatory power of the two metrics using Akaike's Information Criteria (AIC). For both standardized height and stunting, the predicted measure of exposure results in considerably lower AIC values ($\Delta AIC = 221$ for standardized height, $\Delta AIC = 151$, where values above 10 suggest substantially higher explanatory power, see Burnham and Anderson 2002). This difference is striking because predicted exposure is necessarily only an imperfect proxy for true exposure.

Table 4: Locust Exposure Around Birth & Children's Height

	Standardized Height		Stunting Dummy	
	(1)	(2)	(3)	(4)
Swarm Exposure, 9m pre-birth	-0.369***		0.072***	
	(0.097)		(0.021)	
Swarm Exposure, 9m post-birth	-0.280***		0.084***	
	(0.041)		(0.015)	
Mean Pr. Val. 9m pre-birth		-1.446***		0.315***
		(0.324)		(0.090)
Mean Pr. Val. 9m post-birth		-0.961***		0.266***
		(0.187)		(0.062)
Observations	409,100	409,100	409,100	409,100
R^2	0.266	0.266	0.209	0.210
Mean Dep. Var.	-1.512	-1.512	0.402	0.402

Notes: Estimation results from Equations (3) and (5). Each regression includes DHS cluster 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

To benchmark our results, we compare how height-for-age and stunting respond to a food emergency declaration and famine conditions, captured in the Emergency Events Database (EM-DAT) (Delforge et al. 2025). Food emergency declarations occur when the country declares a state of emergency while food shortages and famine conditions are reported. Famine conditions can be reported even without a formal emergency declaration. A food emergency declaration leads to a 0.33 standard deviation reduction in HAZ, and a 12 percentage point increase in the likelihood of stunting (Table C12, columns 1 and 4). These estimates are similar in magnitude to our estimated effects of locust swarm exposure, which lends further support to the idea that locust outbreaks lead to local food insecurity.[14] Declines in HAZ and increases in stunting in famine conditions are of similar magnitude as the food emergency declaration effects but are less precisely estimated.

Additional analyses support our interpretation of the results. The results are not sensitive to alternative spatial fixed effects specifications, including mother fixed effects (Table C13). Controlling for insecticide spraying does not alter the main result, suggesting that we detect the effects of locust swarms themselves rather than control operations (Table C14). The pre-birth exposure impacts are very similar if we use an alternative LASSO-based proxy that excludes breeding areas that intersect grid cells from the possible predictors of locust activity in that grid cell, although the post-birth effects are more sensitive to this more conservative approach (Tables C15, C16, and C17). Dropping observations in grid cells for which the LASSO models select no predictors, rather than setting these predictions to zero, also does not impact the results (Table C18). We continue to detect effects of observed exposure when we extend the threshold distance for defining treatment, and results from both observed and predicted exposure are very similar if we exclude children at intermediate observed distances from swarms in a "donut" regression (Figure C5 and Table C19).

Because we only observe locations at the time of survey, we could potentially misallocate treatment to children if they have moved between birth and the survey. However, the results are robust to excluding movers (Table C20). One might also be concerned about the potential confounding effects of local conflict, since we show that conflict affects monitoring. In general, local conflict should bias effects based on observed locust presence downwards: conflict reduces the likelihood that swarms are recorded and negatively affects child health (Wagner et al. 2018). In practice, the results remain very similar if we exclude children exposed to nearby conflict either at birth or at the time of survey (Table C21). Results are also robust to limiting the sample to Africa (Table C22). Widening the sample to include urban DHS clusters, where effects are likely diluted through greater access to markets, yields fractionally smaller average effects that remain strongly statistically significant (Table C23). Urban-rural heterogeneity models confirm that effects are consistently smaller in urban areas, albeit not always statistically distinguishable (Tables C24 and C25). We continue to focus on rural areas as the more conservative approach when we later calculate the benefits of locust monitoring systems because the slightly larger effects in the rural-only sample are much more than offset by only considering effects on children born in rural areas. Effects are stronger for male children than female children, particularly pre-birth (Table C26), consistent with other evidence showing that male fetuses are more affected by socioeconomic or biological stressors (Kraemer 2000; Mulmi et al. 2016; Maccini and Yang 2009; Der Goltz et al. 2020).

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[14] Data on food, assets, and income from the DHS surveys cannot be used to further explore contemporaneous mechanisms. These data are measured at the time of the survey and not at the time of locust exposure, and the variance in exposure at the time of the survey is small compared to the variance across birth cohorts.

6.4 Selection Effects Bias Estimates Downwards

Our analysis thus far has focused on growth and development, proxied by height-for-age. Reduction in height-for-age from exposure to locust swarm events around 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 children and infants experience higher mortality in the presence of such severe shocks. Mortality can occur before or after 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 had they survived, this can bias the estimated effects towards zero or even reverse their sign (Deaton 2007).

We find that locust swarms increase under-five mortality, with the likelihood of mortality increasing by a statistically significant 1.1 percentage points if the child was exposed to a locust swarm in the 9 months before birth and by 0.5 percentage points if the child was exposed during the first 9 months of life, although the post-birth effect based on observed exposure is not statistically distinguishable from zero (Table C27). The pre-birth effects correspond to a 16 percent increase relative to the mean prevalence of under-five mortality. About two-thirds of the pre-birth effect arises through infant mortality (column 3). Using predicted locust swarm exposure yields larger mortality impacts, when we compare a one standard deviation change in observed and predicted swarm exposure (Table C28).

We also find a decline of 0.5 percentage points in the probability that a mother gives birth in the year they experience swarm exposure and a decline of 0.9 percentage points in the probability that they give birth in the following year, corresponding respectively to a 2.6 and 4.7 percent decrease relative to the sample mean (Table C29). Comparing effects based on normalized observed and predicted swarm exposure variables, we again find that estimates based on predicted exposure are larger in magnitude, and they are slightly more precisely estimated (Table C30).

Combined, these effects suggest that selection effects—an increase in infant and under-five mortality and a reduction in the birth rate—bias our estimated effects on height-for-age downwards. Stronger selection effects bias the estimated scarring effects downwards if weaker fetuses and infants do not survive, increasing the expected health and stature of surviving children.

7 The Value of Early Warning Systems

In this section, we turn to our overall question of valuing the benefits of monitoring systems. We combine our estimates from the previous sections to quantify the value of monitoring and control for the desert locust. We undertake three separate exercises. We begin by estimating the damages from a realized disruption to the existing monitoring system, by calculating the region-wide additional stunting resulting from monitoring interruptions in Yemen. We find that around 445 thousand additional children experienced stunting across our sample countries due to conflict-induced monitoring failures in Yemen. Second, we extrapolate from our estimates to calculate a lower bound on the benefit-cost ratio of the complete monitoring system, by comparing the economic benefit of avoided stunting with the current level of monitoring and control to the estimated costs of maintaining that level of monitoring and control. We calculate each country's increased stunting rate resulting from the elimination of the existing system and estimate that it would decrease the total GDP of sample countries by 0.35 percent, or $25 billion per year. The estimate is a lower bound because we only consider the productivity effects of avoided stunting as a channel for benefits. Lastly, we provide an alternative benchmark for the value of the system by by calculating how many children would need to receive nutrition interventions to offset the health impacts of eliminating the locust monitoring system and the costs of these hypothetical alternative programs.

7.1 The Stunting Cost of Monitoring Interruptions in Yemen

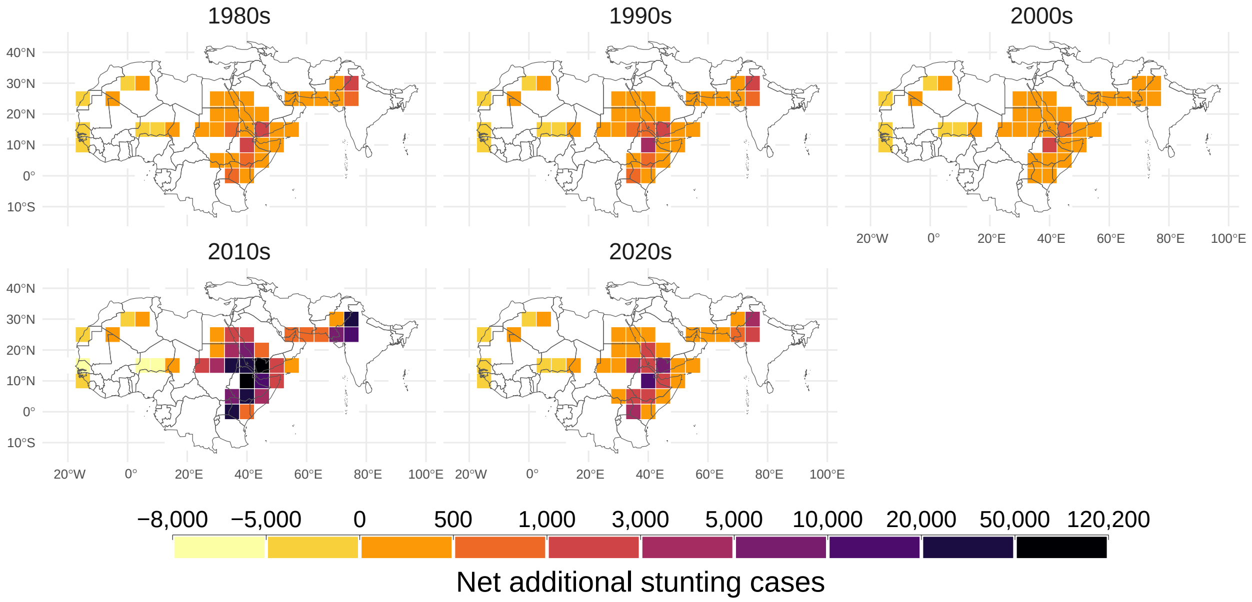
We first use our estimation pipeline to calculate the realized stunting costs of marginal changes to the existing monitoring system. Specifically, we focus on documented interruptions in monitoring in Yemen due to conflict and calculate the resulting changes in swarm prevalence and stunting cases that arose as a consequence.

We first predict marginal changes in the likelihood of local swarm incidence caused by favorable precipitation during periods when monitoring is interrupted by conflict. The prediction is the product of the interaction coefficient from Table 2, column 3, with the share of recent months in which both conflict and high precipitation were observed in admin-1 districts in Yemen, aggregating the additional swarm likelihood predictions to the breeding area level by taking the maximum over all internal admin-1 units. We then propagate these additional swarm likelihoods using the teleconnections from the LASSO models described in Section 5 (see Figure B3). The additional predicted probability of swarm incidence in destination cells is the linear combination of the LASSO coefficients and the additional predicted swarms in the breeding areas. We then construct predicted additional swarm exposure pre- and post-birth by month and grid cell of birth, which we interact with the estimated effect of swarm exposure on stunting to derive additional stunting risk. After averaging the risk factors annually within each destination cell, we multiply by the number of rural births to yield total additional stunting cases (Fig 9). This exercise estimates an additional 445 thousand children experienced stunting, with 89 percent of cases triggered after 2010, when Yemen experienced waves of heavy and widespread conflict (Fig 5). Most of the effects are spillovers beyond Yemen: 83 percent of additional stunting cases occurred outside the country.

7.2 Estimating the Benefits and Costs of the Complete Monitoring System

While the previous exercise estimates the consequence of a realized marginal change in monitoring effort, our overarching goal was to estimate the value of the monitoring and control system in its entirety. In this subsection, we proceed to estimating the stunting avoided by the full current monitoring system. First, we estimate the health benefits of the current system by simulating the elimination of all monitoring effort in breeding areas and calculating the resulting change in stunting cases. We monetize these health benefits using the estimated relationship between stunting rates

Figure 9: Additional Stunting Cases from Yemen Monitoring Failures



Notes: Each panel shows the total additional predicted stunting cases by LASSO destination grid cell and decade, generated from conflict and precipitation-caused monitoring failures in only Yemen. See main text for details.

and GDP (Galasso and Wagstaff 2019). We then gather data on the costs of the current system and combine them with the monetized health benefits to obtain the benefit-cost ratio.

Health Benefits from Monitoring We consider a scenario in which monitoring halts in all breeding areas and map the implied changes in monitoring to changes in swarm prevalence. In breeding areas, 12 months of conflict occurrence decreases the likelihood of monitoring by 9.2 percentage points and increases swarm reports by 2.9 percentage points, implying a ratio of detected to monitoring of about -0.3 (Table 1, columns 3 and 6). This is a conservative approach to estimating this ratio, because while conflict increases true swarm prevalence, it also reduces the likelihood that swarms are detected, meaning that the true ratio is almost certainly larger in magnitude.

We then extrapolate to infer that suppressing monitoring in any admin-1 and month (i.e. changing the monitoring dummy from 1 to 0) increases the probability of swarm prevalence in that admin-1 and month by about 0.3. Extrapolating the marginal effects to the simulated removal of all monitoring effort in breeding areas requires relatively strong assumptions. However, it likely underestimates the true effects, since monitoring failures multiply and compound: uncontrolled swarms then breed further swarms. Further, while returns to monitoring are certainly heterogeneous, our evidence is most consistent with low-return monitoring being more likely to be neglected on the margin than high-return monitoring (see Section 4, where we show that the suppression of monitoring during conflict is stronger in the absence of weather conditions that favor locust breeding, when the returns to monitoring are likely low).

We then use this estimated ratio to convert the panel of observed monitoring activity into a panel of expected changes in swarm prevalence in breeding areas from suppressing monitoring activity. Under the monitoring system removal scenario, we project no change in swarm prevalence in places where no monitoring took place, and a 30 percentage point increase in swarm prevalence where monitoring occurred. We then aggregate this panel by breeding area and month by taking the maximum of expected swarm prevalence changes across all admin-1 units in a breeding area.

To propagate impacts from breeding areas to teleconnected regions, we convert the expected changes in swarm prevalence in breeding areas into changes in expected swarm prevalence in all 5-degree grid cells and months, using the LASSO models estimated in Section 5.

We convert the resulting changes in expected swarm exposure to changes in stunting risk by first constructing predicted additional swarm exposure 9 months pre- and post-birth for a hypothetical child born in a rural area in every 5-degree grid cell and month, and then using our coefficient estimates from the stunting regression (Table 4, column 4) to calculate the additional stunting risk by grid cell and year of birth, averaged across time. We note that the rescaling factor on our estimated health effects— which results from using predicted locust exposure as a proxy for true locust exposure—implicitly cancels in this calculation. Since we multiply predicted additional swarm exposure by the coefficient on predicted swarms, the units are consistent and the overall effect of true additional exposure is recovered correctly.

To obtain the aggregate health benefits—or avoided stunting damages—from the monitoring system, the final step required is to convert the modeled increases in stunting risk into additional stunting cases in the population. We calculate the additional number of stunted children for one example cohort (the 2020 birth cohort) and then calculate the increase in the stunting rate of that cohort. Since all cohorts would on average be similarly affected if the monitoring system were permanently eliminated, we interpret this cohort's increase in stunting rate as the estimated stunting rate in the entire population in the long run. For the predicted additional stunting cases (the numerator in the stunting rate), we multiply the additional stunting risk by the number of rural births in 2020 residing in a given 5-degree grid cell. We estimate approximately 2 million additional stunted children globally in the 2020 birth cohort of 69 million children born in rural areas, and we conservatively assume that the 15 million children born in urban areas are unaffected. We use these figures to calculate the stunting rate increase by country.

Monetized Health Benefits Following Galasso and Wagstaff (2019), we calculate the impact on GDP Y in country c from higher stunting rates using:

$$\Delta \log(Y_c) = -0.155 \times \Delta \mathrm{SR}_c$$

where ΔSR is the additional stunting rate calculated above. We use the World Bank's annual national GDP time series in constant 2015 USD, which we inflate to constant 2020 USD, using FRED's CPIAUCSL. Our estimated increase in each country's stunting rate translates into a total impact of removing the locust monitoring system of $25 billion in 2020, or 0.35 percent of the total GDP of locust-affected countries. Because we only account for effects on stunting, this estimate excludes, for example, impacts on farmer profits and pastoralist livelihoods. The proportionally largest impact would occur in Senegal, which would experience a loss of 2.6 percent of GDP.

Monitoring and Control Costs We estimate the annual costs of the desert locust monitoring system to range between \$37 and \$77 million across our study period. We include both the costs of monitoring and the costs of control, since the objective of monitoring is to facilitate preventative control measures. Our estimate is a combination of preventive costs incurred in recession years and emergency costs incurred in plague years. For recession years, we rely on publicly available financial documents to track down budget data for all organizations involved in locust monitoring, at the international, regional, and national levels (FAO 2023b; 2023a; DLCO-EA 2023; Agence Nationale de Lutte Antiacridienne 2014). Summing annual budgets across these organizations yields a lower bound of \$11 million per year and an upper bound of \$60 million per year in preventive costs for recession years. The range reflects different assumptions that we make to handle missing data on national level spending on locust monitoring and control. For plague years, we rely on documentation of the costs actually incurred to address the three plague events that fall within our study period (FAO 2023c; Skaf et al. 1990). This yields an average of \$148 million per year in emergency costs in plague years. Averaging these estimates across all recession and plague years yields the final estimate of annual costs ranging between \$37 and \$77 million across our study period. We further describe the accounting process and documentation sources in Appendix C16.

Benefit-Cost Ratio Estimation of costs and benefits imply a lower bound on a benefit-cost ratio ranging between 680:1 and 320:1 for the monitoring system, considering only the benefits due to reduced stunting (Table 5, column 1).

There is a temporal lag between policy action (monitoring and control to prevent childhood exposure to locust swarms) and the benefits we model (increased productivity through avoided stunting, which presumably only accrue once protected individuals join the workforce). A social planner might want to discount future benefits for two reasons: first, because of pure time preference, and second, because they expect future generations to be richer than current generations and thus to obtain a lower marginal utility from an incremental unit of income (see, e.g. Stern 2008). However, many economists argue that discounting the utility of future generations purely because they will exist in the future is unethical and that the pure rate of time preference should be close to zero. We also implicitly already discount future consumption because we do *not* model economic growth when we project future benefits.[15]

One could counter, however, that we must still account for foregone opportunities to invest in other social welfare enhancing projects in the present day. To adjust the estimated benefit-cost ratio to account for discounting, we calculate the discount factor that should be applied to the

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[15] Specifically, Ramsey's social discount rate r is defined as $d + ng$, where d is the pure rate of time preference, n is the elasticity of marginal utility of consumption, and g is the growth rate. Stern (2008) argues that d should be near zero for ethical reasons and that 1 is a plausible value for n. Thus $r \approx g$, implying that discounting would exactly offset growth.

benefits under the additional assumption that the benefits materialize uniformly over the working lives of children born at the time of policy action, such that the benefits in any one year are the results of actions fifteen to sixty years previously.[16] A discount rate of 2 percent, typical of social benefit calculations[17], gives a discount factor of 0.49 and a cost-benefit ratio ranging between 320:1 and 160:1 (Table 5, column 2). Even at an extremely conservative discount rate of 7 percent,[18] implying a discount factor of 0.12, results in a cost-benefit ratio between 80:1 and 40:1.

Table 5: Benefit Cost Ratio Under Alternative Assumptions

		Monetized Benefits from Reduced Stunting	
		No discounting: $25 billion	2% discount rate: $12 billion
Costs	Lower bound: $37 million	680:1	320:1
	Upper bound: $77 million	320:1	160:1

Notes: Annual benefits for reference year 2020. Costs are an annual average over our study period, including recession and plague years. See main text for details.

Considering the removal of the existing monitoring system is not just an academic exercise: several factors threaten the system's continued capacity. Funding continues to fluctuate as a function of perceived outbreak risk, perpetuating an imbalance in funding availability towards crisis response over preventive control; budgets for regional locust organizations have shrunk over time as member state arrears accumulate; and recent budget cuts at the FAO have left critical staffing gaps in locust system expertise and management (FAO 2021). The institutional knowledge and capacity that allow the FAO to serve its central coordinating role depend on a small and dedicated group of individuals, creating vulnerability whenever key staff members retire. The previous Senior Locust Forecasting Officer served for 37 years between 1987 and 2024, with uncertainty over his replacement until close to his retirement date.[19]

While extrapolating from estimated marginal effects to the total removal of the monitoring system implies strong assumptions, the result is likely to underestimate the benefits rather than the reverse, as we argue above. One might reasonably prefer a cost-benefit ratio for marginal changes to the system in specific places and times. Our architecture allows us to estimate the marginal benefits of such changes—as we illustrate for Yemen above—but the paucity of disaggregated cost

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[16] Future benefits are discounted to present value using the mean of annual discount factors over the working lifetime, specifically $\bar{d} = \frac{1}{46}\sum_{t=15}^{60}\frac{1}{(1+r_t)^t}$, assuming that the workforce comprises the population between ages 15 to 60 at any moment in time.

[17] This reflects recent guidance from the U.S. Office of Management and Budget issued during the Biden administration (Office of Management and Budget 2023).

[18] This was the discount rate earlier recommended for public investments, based on the prevailing historical pretax rate of return on private investments (Office of Management and Budget 1992).

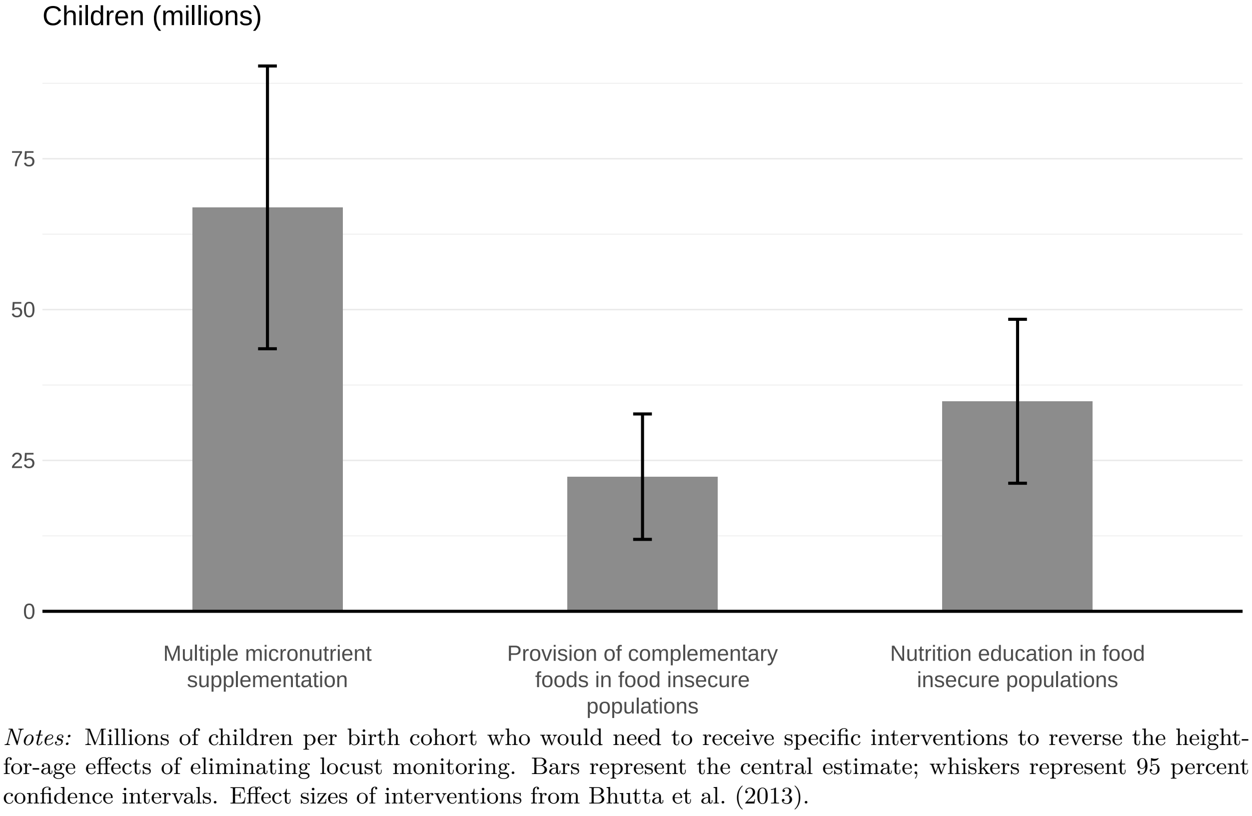
[19] The FAO (2021) report highlighted this concern: "It is worth noting that the DLIS Senior Locust Forecaster will retire in a few years and FAO has yet to take steps to fill this looming gap."

data makes it impossible to estimate the corresponding marginal costs, precluding the estimation of a meaningful cost-benefit ratio for such changes.

7.3 Benchmarking Locust Monitoring Against Other Nutrition Interventions

To benchmark the effects of the locust monitoring system against other health investments, we calculate the scale and cost of nutrition interventions needed to offset the adverse effects of eliminating locust monitoring. This provides us with an alternative valuation of the benefits of the locust monitoring system that does not depend on the sparse cost data. First, we take the changes in swarm exposure resulting from eliminating monitoring activities, estimated above, and convert these to changes in HAZ using our coefficient estimates from the HAZ regression (Table 4, column 2). Using the effect sizes of three nutrition interventions from Bhutta et al. (2013), we project the number of children who would need to receive a nutrition intervention to offset the estimated HAZ decrease resulting from monitoring elimination. For each birth cohort, offsetting these effects would require around 67 million additional children to receive multiple micronutrient supplementation, or 22 million children to receive provision of complementary foods, or 35 million to receive nutrition education (Figure 10), at approximate costs of \$1.1 billion, \$1.05 billion, and \$0.5 billion, respectively (unit cost estimates from Horton et al. 2010, updated to 2020 US\$). These hypothetical beneficiary populations compare to the total rural birth cohort in 2020 of 69 million children.

Figure 10: Interventions Required to Offset Effects of Swarm Exposure



Notes: Millions of children per birth cohort who would need to receive specific interventions to reverse the height-for-age effects of eliminating locust monitoring. Bars represent the central estimate; whiskers represent 95 percent confidence intervals. Effect sizes of interventions from Bhutta et al. (2013).

8 Discussion

Effective disaster prevention requires monitoring to identify and warn people about evolving disaster risks and to allow policymakers to target investments to mitigate disaster impacts or enable recovery. However, the benefits of these monitoring systems are difficult to quantify, primarily because of two major empirical challenges. First, the costs of disasters that are averted by effective monitoring are unobserved and hard to estimate, and second, monitoring effort is endogenous—particularly when active monitoring is required, as for diseases, agricultural pests, or crime.

This paper provides the first causal estimate of the value of an early warning system that addresses these challenges. We examine the specific case of the desert locust, the monitoring system for which is one of the oldest early warning systems in existence. We establish the value of the monitoring system in three steps: First, we demonstrate how shocks to monitoring and control activities resulted in outbreaks in locust breeding areas. Second, we trace out the spatial and temporal patterns of locust migration routes to connect outbreaks in breeding areas to exposure in populated areas. Third, we show that pre- and post-birth exposure to locust swarms adversely affects human well-being, as measured by anthropometric status: exposure around birth increases stunting risk by over 7 percentage points. We combine these results to estimate that spending on locust monitoring and control has a cost-benefit ratio between 160:1 and 680:1.

To contexualize the estimated effects on stunting, we compare our effects to the effects of conflict and other natural disasters. 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 food price shocks increase stunting by 11 percentage points in the Philippines; Anttila-Hughes et al. (2021) find that the 2015 El Niño increased stunting by 1.7 percentage points among children in ENSO-teleconnected countries exposed during the first year of life; finally Akresh et al. (2011) report that *in-utero* exposure to Rwanda's civil war reduced HAZ by about 1 standard deviation and that *in-utero* exposure to crop failure reduced HAZ of girls by about 0.9 standard deviations. The impact of locust swarms, then, appears to be of similar magnitude to other major shocks evaluated in previous literature.

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, and funding for locust monitoring and control remains precarious. The value of monitoring and early warning systems is only likely to increase as the climate changes, affecting both conditions in breeding areas and the wind patterns that govern where the locust migrates, and potentially increasing the frequency and geographic spread of outbreaks (Salih et al. 2020). As climate change increases both the intensity and the variability of many disasters, including pest and disease outbreaks, the challenge of ensuring society appropriately values and invests in early warning systems becomes ever more pressing.

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

—Hsu Chao c.1200

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