Herder-Related Violence, Agricultural Work, and the Informal Sector as a Safety Net*

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September 15, 2022

Abstract

Violent conflict between nomadic herders and settled—mostly agricultural—communities in Nigeria has risen dramatically over the last decade. These 'farmer-herder conflicts' occur as both groups clash over land and resources, in part, due to a changing climate. In response to rising tensions, a number of Nigerian states have passed open grazing prohibition laws in 2016 and 2017. Farmer-herder conflicts subsequently erupted in these states in 2018. We use panel data from 2010 through 2019 to study the response of households, with a focus on those engaged in agriculture, in states with an open grazing ban to exposure to herder-related violence during the planting season. In the contemporaneous planting season, exposure leads to an increase in informal work and a decrease in agricultural work at both the extensive and intensive margins. In the subsequent harvest season, individuals in exposed households reduce informal work and increase agricultural work at the extensive margin. These findings align with the view that the informal sector can be a 'safety net' for agricultural households in the presence of adverse shocks.

Keywords: Conflict, Violence, Informality, Agriculture, Farmer-herder, Safety nets.
JEL Codes: E26, E29, I31, Q12

*Authors are listed in alphabetical order. The authors thank members of the World Bank's LSMS team for assisting in the use of restricted-use unmasked GPS coordinates. The findings and conclusions in this manuscript are ours and should not be construed to represent any official World Bank determination or policy. All errors are our own.

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1 Introduction

Throughout the developing world, informal work prevails. In many countries, the informal sector is large, persistent, and seems to react counter-cyclically to macroeconomic trends and immediate external shocks (Fiess *et al.*, 2010; Loayza and Rigolini, 2011). Although existing research includes analyses of the informal sector's response to economic contractions, financial crises, trade opening, and environmental shocks (Gunther and Launov, 2012; Epstein and Shapiro, 2017; Adhvaryu *et al.*, 2019; Colombo *et al.*, 2019), the degree to which the informal sector acts as a 'safety net' in response to conflict and violent unrest remains an open question. Despite being increasingly salient in many areas around the world, little is known about how households—and in particular agricultural households—use the informal sector as a way to cope with increased risk of conflict and violence.

Within Nigeria, the last decade has seen a rise in violence propagated by Boko Haram in the northeast region and escalating inter-group conflict between farmers and Fulani pastoralists in the north-central region. Severe violence led to states of emergency in both 2011 and 2013,[1] and despite Boko Haram dominating the news, conflicts between Fulani pastoralists and settled agricultural communities are more deadly.[2] Moreover, emerging evidence suggests that increased temperatures driven by climate change will likely lead to increased conflict between farmers and pastoralists across sub-Saharan Africa and within Nigeria specifically (McGuirk and Nunn, 2020; Eberle *et al.*, 2020).

We study a specific series of shocks—namely, violence between Fulani nomadic herders and sedentary (largely agricultural) communities in Nigeria—and their effect on economic activities. These violent incidents in the so-called 'herder-farmer conflict' led to over 3,600 deaths in a period of heightened violence between 2016 and 2018.[3] In addition to the loss of life, these conflicts also led to widespread destruction of property, displacement, and civil unrest. These conflicts are, in part, related to increasing competition for scarce land and water resources used by both farming and herding communities (George *et al.*, 2021a). Over time, nomadic herders have moved farther away from traditional routes and have remained in areas for longer periods of time, due to prolonged dry seasons driven by climate change and displacement from the country's ongoing conflict with Boko Haram in the northeast. Simultaneously, settled communities have expanded, and dry farming techniques have lengthened their land and water use throughout the year. In response to these tensions, three Nigerian states (Benue, Ekiti, and Taraba) passed outright bans on open grazing in 2016 and 2017. These bans exacerbated previous tensions and directly contributed to a peak of violence in the first half of 2018. We focus, in particular, on the effects of this recent wave of violence.

We combine detailed panel data of households and individuals from Nigeria's General Household Survey (GHS) with data on violent events from the Armed Conflict Location and Event Data (ACLED)

[1] BBC News. December 31, 2011. "Nigeria's president declares state a emergency." available here. Last accessed: November 12, 2020.

[2] The Washington Post. July 26, 2018. "This little-known conflict in Nigeria is now deadlier than Boko Haram." available here. Last accessed: November 12: 2020.

[3] Amnesty International. December 17, 2018. "Harvest of Death: Three Years of Blood Clashes Between Farmers and Herders in Nigeria." available here. Last accessed: February 3, 2021.

project (Raleigh *et al.*, 2010). The GHS data contain four survey rounds, each including two seasonal visits. This means that individuals can be observed in eight separate periods, occurring before, during, and after the 2018 spike in herder-related violence. This violence typically follows a seasonal pattern, with events worsening as herders remain to graze their cattle in areas past May, when they historically moved north.[4] With these data, we leverage variation across time and space using the presence of herder-related violent incidents within a given radius around households and within a given time frame as an indicator of exposure to violence during the spike in violence in 2018.[5] The granularity of these data allows us to include fixed effects at the level of a comparatively small geographic area—by enumeration area for the GHS—which combined with time and individual-level fixed effects, allows us to estimate changes in economic activities associated with herder-related violence while ruling out confounding variation between narrowly defined locations over time. Therefore, our identification strategy relies upon the fact that exposure to these violent events varies meaningfully, even within narrowly defined geographic areas. Consequently, we restrict comparison households to those who are not exposed (defined as a violent event within a given distance window) to herder-related violence after the spike in violence in 2018 but are within 50 km. of a herder-related violent event. This helps ensure that we are comparing households with similar agro-ecological and economic conditions.

This research design could present a challenge in our study context. Recent work demonstrates that two-way fixed effect regressions, with staggered treatment timing, can be biased in the context of heterogeneous treatment effects (De Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon, 2021). In particular, bias can intensify when units treated at the end of the panel use units at the beginning of the panel as a counterfactual (Goodman-Bacon, 2021). Our focus on the particular spike in herder-related violence in 2018 buffers against concern of the staggered timing of treatment because all treated households are treated in one period (Baker *et al.*, 2022), between the 2015-2016 and the 2018-2019 survey rounds. In addition, we are also careful to control for prior exposure to violent events to address issues arising from accumulating effects of previous violence. This supports our use of a two-way fixed effect approach over other estimators (De Chaisemartin and D'Haultfœuille, 2020a,b; Callaway and Sant'Anna, 2020), as these alternative estimators do not apply well to our data and require similar, if not more restrictive, assumptions when employed in our empirical setting.

We find that in Nigerian states that implemented open grazing bans, where violence was most intense, households exposed to herder-related violence in the planting season are more likely to engage in informal work and less likely to engage in agricultural work in the contemporaneous planting season. We find that these results hold at the extensive (i.e., using binary measures of informal or agricultural work) and intensive (i.e., using measures of the number of hours worked in informal or agricultural activities) margins. In terms of magnitude, we find that exposed agricultural households living in states that

[4]This pattern contrasts with more general patterns of conflict following agricultural harvest seasons (Guardado and Pennings, 2020; Ubilava and Atalay, 2021).

[5]Our preferred specification defines our indicator of exposure to herder-farmer conflict in a highly localized way with a radius around households of 10 km. and a time frame of the previous one month. We also report a variety of estimates that use less a less localized definition of 20 and 30 km. radii and a three month time frame.

implemented an open grazing ban increased the likelihood of informal work and decreased the likelihood of agricultural work by roughly 30 percentage points, respectively. In terms of number of hours worked, our estimates imply that individuals in exposed households living in states that implemented an open grazing ban tripled the number of hours worked in an informal enterprise and decreased the number of hours worked in agriculture by 60 percent. In the subsequent harvest season, agricultural households exposed to herder-related violence in the previous planting season and living in states that implemented an open grazing ban did not change their informal work or agricultural work on the extensive margin, but did on the intensive margin. We find that exposure to herder-related violence in the planting season lead agricultural households living in states implementing an open grazing ban to reduce the number of hours worked in an informal enterprise by 41 percent and increase the number of hours worked in agriculture by 54 percent. These results are consistent with the view that work in the informal sector can act as an important 'safety net' for agricultural households exposed to herder-related violence in Nigeria.

Our paper is closely related to two sets of existing literature. The first set investigates the role of the informal sector as a 'safety net' in response to price shocks (Gunther and Launov, 2012; Epstein and Shapiro, 2017; Adhvaryu *et al.*, 2019; Colombo *et al.*, 2019). For example, Adhvaryu *et al.* (2019) study the effect of price shocks in the global coffee market on informal work in Tanzania. The authors document that households cope with declines in the global price for coffee by increasing informal household enterprise ownership. Our paper differs in that rather than focus on price shocks we study how agricultural households respond to exposure to herder-related violence. The second set of studies examine how exposure to conflict in Nigeria specifically, influences agricultural output (Adelaja and George, 2019), food security, (George *et al.*, 2020), and farm labor supply (Odozi and Oyelere, 2021). Our paper differs from these existing studies in that although we use a similar research design with similar data, we take advantage of the highly-granular nature of the geo-location data to define exposure to conflict within a very narrow window (as close as 10 km.), where earlier studies using a similar identification strategy have relied on exposure to violence at the level of the local governance area (LGA). Our approach allows us to narrowly define a set of comparison households and to test for sensitivity to the definition of treatment. Additionally, we provide more specific results that take into account the seasonality of herder-related violence and characterize how agricultural households respond to exposure throughout the agricultural season. Lastly, our results complement these studies by focusing on an additional dimension, informal work, which has previously received little attention.

This study makes three core contributions. First, we add to the literature on coping mechanisms in low- and middle-income countries. In the absence of adequately functioning markets for savings, credit, and insurance (Burgess and Pande, 2005; Cole *et al.*, 2013; Dupas and Robinson, 2013; Karlan *et al.*, 2014), informal coping mechanisms in the form of, for example, intra-household transfers (Townsend, 1994), temporary migration (Bryan *et al.*, 2014; Morten, 2019), and selling assets such as livestock (Lange and Reimers, 2020) persist. Specifically, we add to a subset of this literature on coping mechanisms for households exposed violence and conflict (Verpoorten, 2009), and show that work in the informal sector can act as a 'safety net' for agricultural households exposed to herder-related violence.

Second, we add to the literature on the role of the informal sector in the process of economic development. This literature finds that the informal sector plays an important counter-cyclical role amid macroeconomic dynamics (Fiess *et al.*, 2010; Loayza and Rigolini, 2011). While recent research documents the informal sector acting as a 'safety net' in response to price shocks (Gunther and Launov, 2012; Epstein and Shapiro, 2017; Adhvaryu *et al.*, 2019; Colombo *et al.*, 2019), our study highlights that the informal sector can act as a 'safety net' for those exposed to violence and conflict. Notably, both price shocks and exposure to violence and conflict can reduce the expected earning potential of agricultural production. Since we are able to look at the effects of such violence in both planting and harvest periods, we are also able to show that, even temporary, shifts to informal work can also be accompanied by lagged effects in the economic activity of households several months after exposure to violence has occurred. Such results are important since climate change will likely continue to exacerbate tensions between farmers and pastoralists both across Sub-Saharan African and specifically in Nigeria (McGuirk and Nunn, 2020; Eberle *et al.*, 2020).

Finally, we add to the literature on the consequences of exposure to violence and conflict. This literature documents adverse health effects (Camacho, 2008; Akresh *et al.*, 2012a,b; Grimard and Laszlo, 2014; Minoiu and Shemyakina, 2014; Weldeegzie, 2017), lower levels of educational attainment (Chamarbagwala and Moran, 2011; Singh and Shemyakina, 2016; Brown and Velasquez, 2017; Weldeegzie, 2017), a diminished preference for risk (Bundervoet, 2010; Voors *et al.*, 2012; Moya, 2018; Jakiela and Ozier, 2019), and reduced psychological well-being (Alloush and Bloem, 2022). Specifically, we add to a subset of this literature studying the consequences of exposure to conflict and violence on agricultural outcomes (Singh, 2013; Adelaja and George, 2019; Kaila and Azad, 2019; George *et al.*, 2020; Avuwadah, 2020; George *et al.*, 2021b). We find that agricultural households reduce agricultural work but increase informal work when exposed to violence. Although the informal sector can act as a 'safety net' by partially shielding households from the consequences of increased production costs and decreased farm profits, this diversification in economic activities can come at a cost and, in turn, amplify the economic consequences of violence and conflict (Colombo *et al.*, 2019). In particular, our results offer additional context to existing research on the relationship between conflict and agriculture in Nigeria specifically, which finds that exposure to conflict reduces agricultural output (Adelaja and George, 2019) and food security (George *et al.*, 2020).

The remainder of this paper is organized as follows: In the next section we discuss background details about herder-related violence and informal work in Nigeria. We also discuss the passage of open grazing prohibition laws in three Nigerian states. In Section 3, we present our empirical framework by discussing the data we use in our analysis, reporting descriptive statistics, and summarizing our identification strategy. In Section 4, we present our results, which also include labor response in the contemporaneous planting season and the subsequent harvest season. We also report results on the use of agricultural harvest, agricultural marketing, and enterprise sales. Finally, Section 5 concludes.

2 Background

Amnesty International estimates that between 2016 and 2018, there were more than 3,600 deaths recorded due to clashes and escalating violence between nomadic herders and agricultural-centered communities—the bulk of these attacks occurring in the early part of 2018.[6] Figure 1 shows the number of herder-related violent incidents from the first half of 2010 until the second half of 2019.[7] This semesterly data defines the first half of a year (h1) as starting in December the previous year until May; the second half (h2) is defined as June through November. Though seasonal patterns vary throughout the country, this roughly follows the Middle Belt's dry season (which includes harvest) in the early part of the year and the rainy (planting) season in the latter half.[8]

While some accounts have attributed this violence to ethno-religious tensions (e.g., based on the observation that nomadic herding communities, including those such as the Fulani, are predominantly Muslim, and farmers located in the southern regions of the countries are Christian), the tensions are largely driven by conflicts over scarce land and water resources (McGuirk and Nunn, 2020; George *et al.*, 2021a). Specifically, competition for these resources has been regarded as a violation of a long-standing understanding in and near the Middle Belt zone, which is comprised of the country's central states and has been a historical locus of herding activity, mostly in the first half of the year. This unwritten rule traditionally permitted nomadic herders the open use of lands during the dry season, as this is a low period of agricultural activity. Farmers often welcomed this use as the wandering cattle naturally fertilized the region's agricultural plots. At the end of the dry season, herders historically vacated areas in and around the Middle Belt around May to return the following January. Extended dry seasons, however, and the Boko Haram conflict in the north has increasingly pushed herders south for longer periods of time. Simultaneously, farming communities have expanded their land use and extended their agricultural season through the adoption of dry farming techniques.

The outbreak of violence is often referred to as the herder-farmer conflict, however, these incidents are not exclusively between herding and farming communities. Herding communities have also engaged in agriculture in these areas. Likewise, reprisal attacks have been perpetuated against parties who were neither herders nor farmers: Violent events include attacks on civilians, kidnappings, and the destruction of crops and property.[9] Armed militias (some identified with herding tribes) clashed occasionally, many times as reprisal for earlier violence. Whole communities protested and rioted following the movement of herders into an area—this unrest often simmered over into more violence, including attacks targeted at herding tribes themselves.

[6] Amnesty International. December 17, 2018. "Harvest of Death: Three Years of Blood Clashes Between Farmers and Herders in Nigeria." available here. Last accessed: February 3, 2021.

[7] Additional details on definitions are included in the Data section and Data Appendix.

[8] The Middle Belt refers to North-central Nigeria and includes Abuja, Benue, Plateau, Kogi, Nasarawa, Niger and Kwara.

[9] This observation is based on the authors' reading of events recorded in ACLED.

2.1 Open Grazing Prohibition Laws

In response to tensions and outbreaks of violence between herders and farming communities, in 2016 and 2017, three Nigerian states (Benue, Ekiti, and Taraba) passed bans on open grazing.[10] Each of these states, particularly Benue and Taraba, was a traditional location of herding and farming activity, particularly prior to the violent incidents. In 2016, Ekiti passed the first open-grazing prohibition, which banned grazing activities in some areas of the state following the killing of two residents. The law also forbade the carrying of firearms by herders; any herder found carrying a firearm could be declared a 'terrorist' under the statute.[11] Two other states, Benue and Taraba followed suit in 2017, both passing stricter open grazing prohibitions.[12] The Open Grazing Prohibition & Ranches Establishment Law in Benue state banned open grazing outright—requiring the establishment of permitted ranches—and allowed for the confiscation of cattle by authorities and imposed a daily 2,000 Naira fine for each cow held by authorities.[13] The Open Grazing Prohibition and Ranches Establishment Bill in Taraba instituted similar bans on open grazing.

Following the enactment of the Benue and Taraba anti-grazing laws, particularly, violence between herders and farming communities intensified. These bans required herders to establish formal ranches, but plots and permits were scarce, adding to a sentiment that the laws targeted and hindered herders' livelihood.[14] The laws were enforced sporadically, or not at all, in remote areas, where authorities' reach was limited.[15] In these areas there were reports that unofficial civilian groups seized herders' cattle under the guise of enforcing the grazing prohibition.[16] Violence quickly escalated. During the first week of January 2018, six villages in Benue were raided by alleged herder groups; the attacks killed more than 80 people.[17] These incidents were followed by several dozen more, killing between 200 and 300 people in the following months. In January alone, nearly 170 people were killed as Amnesty International warned that events were spiraling out of control.[18] Following the escalation, the Nigerian government deployed Army forces in Benue, Taraba, and Nasarawa; civilian militia groups were mobilized, notably in Benue.[19] Several local government areas (LGAs) instituted nighttime curfews, severely limiting mobility.[20]

[10] These laws were, Benue: Open Grazing Prohibition & Ranches Establishment Law in Benue state (enacted in November 2017). Taraba: Open Grazing Prohibition and Ranches Establishment Bill (enacted in January 2018). Ekiti: "Prohibition of Cattle and Other Ruminants Grazing" (enacted in September 2017). A fourth state, Edo, also passed a limited 90-day ban on night grazing, but not an outright ban and is not considered.

[11] The Punch. September 8, 2016. "Force and limits of Ekiti State's anti-grazing law," available here. Last accessed: March 31, 2020.

[12] Though passed in 2017, Taraba's bill was enacted in January-February of 2018.

[13] Reuters. June 26, 2019. "Deadly clashes over cattle continue in Nigeria despite grazing ban," available here. Last accessed: March 31, 2020.

[14] Washington Post. July 26, 2018. "This little-known conflict in Nigeria is now deadlier than Boko Haram". Last accessed via Factiva: March 31, 2020.

[15] New York Times. September 22, 2018. "Nigerian Herders Face Threat from Farmers Competing for Land". Last accessed via Factiva: March 31, 2020.

[16] International Crisis Group. July 26, 2018. "Stopping Nigeria's Spiralling Farmer-Herder Violence," available here. Last accessed: March 31, 2020.

[17] International Crisis Group. July 26, 2018. "Stopping Nigeria's Spiralling Farmer-Herder Violence," available here. Last accessed: March 31, 2020.

[18] Agence France Presse. January 31, 2018. "Herder-farmer violence kills 14 in Nigeria". Last accessed via Factiva: March 31, 2020.

[19] Agence France Presse. January 11, 2018. "Nigeria: mass burial for farmers killed in herder clashes". Last accessed via Factiva: March 31, 2020.

[20] Agence France Presse. February 7, 2018. "Nigeria grapples with mob justice in farmer-herder clashes". Last accessed via Factiva: March 31, 2020.

In response to rising violence levels and anti-grazing statutes, several herders fled into bordering states, including Nasarawa and Cross River states. Two evident consequences of these movements emerged: first, the migrating herders were met with resistance in these bordering states, resulting in attacks by civilian militias and herder-related groups, including protests, riots, and reprisals. Second, violent events occurred near state border areas. Several accounts alleged that herder-related groups would cross into grazing-prohibition states—where they would destroy property, burn fields, or attack individuals under nightfall—only to return across those borders by daylight.[21]

2.2 Informal Work in Nigeria

As in many other low- or middle-income countries, informal work represents a large share of Nigeria's economy (La Porta and Shleifer, 2014). In 2019, the IMF estimated that over 60 percent of Nigeria's GDP belonged to the informal sector. Despite the relative share of the informal sector in terms of GDP, agriculture remains an important sector for employment. Figure 2 shows the sample mean of those working in farming and own-account/household enterprise work (which we define as informal work) over the four GHS rounds with two observations per round: post-planting and post-harvest. The trends for the post-planting (rainy season) and post-harvest (dry season) rounds are separated for agricultural, (e.g., farming) activity. While there is no notable trend for the post-harvest/dry season, there is a distinctly upward trend for the post-planting/rainy season. This is consistent with either a larger proportion of workers engaged in agricultural activity or, also, a greater number of engagements later and into the dry season, as cited in several accounts of the herder-farmer conflict. We also observe a notable uptick in self-employment between 2016 and 2018, which corresponds to the intensification of herder-involved conflict events documented in Figure 1.

The role of informal work, in relation to economic development in general, and more specifically as a means to a 'safety net' in response to adverse shocks remains an open debate among researchers and policy-makers. Specifically, while some argue that the informal sector is the result of competitive market forces, others contend that informal work is the result of market segmentation, and more recently, some argue that the informal sector offers either attractive employment opportunities or a coping strategy of last resort (Gunther and Launov, 2012). Although the extent to which informal work acts as a 'safety net' is an empirical question, existing work demonstrates the potential of informal work to buffer individuals from the consequences of adverse shocks (Loayza and Rigolini, 2011), such as exposure to conflict.

3 Empirical Framework

This section includes four sub-sections. First, we introduce the two primary sets of data we use to construct an individual-level panel data set of exposure to violent conflict events and employment outcomes

[21] International Crisis Group. July 26, 2018. "Stopping Nigeria's Spiralling Farmer-Herder Violence," available here. Last accessed: March 31, 2020.

in Nigeria. Second, we discuss some descriptive statistics which characterize our study sample. Third, we specify our core identification strategy and discuss the interpretation of our preferred estimates. Finally, we discuss the potential consequences of sample attrition and discuss how we address these concerns.

3.1 Data

We combine two detailed panel datasets. The first set of data is from the Nigeria General Household Survey (GHS), which is a product of the Nigeria Bureau of Statistics and the World Bank's Living Standard Measurement Study (LSMS).[22] The GHS was conducted over four rounds in 2010–2011, 2012–2013, 2015–2016, and 2018–2019, and each survey round includes two data points for each household: one in the post-planting (rainy season) period and one in the post-harvest (dry season) period, regardless of whether the household engages in agricultural activities. Data collection in the post-planting period generally occurs in the later fall months, while fieldwork in the post-harvest season occurs within the first few months of the next year.

The GHS is designed to include a nationally (and zonally) representative set of enumeration areas, which act as the primary sampling units from which households are selected.[23] Individuals within households are enumerated in a full roster; individuals are tracked if they leave a household, though this does result in some individual-level attrition. Starting in 2010, 5,000 households in 500 enumeration areas were selected for the panel dataset; however, due to attrition, by the fourth round (2018–2019), a refresh sample of 360 enumeration areas was required.

Due to this sampling design, two points merit some attention. First, non-random attrition poses a challenge to identification. If, for example, attriting individuals are more or less likely to be those that engage in a particular type of economic activity, estimates will be accordingly biased. A related but distinct concern is that attrition itself is related to violent events, which would make treatment endogenous to presence in the sample. Second, the sampling procedure for the fourth round of the GHS included new enumeration areas. Since the main treatment (e.g., exposure to violent events around the farmer-herder conflict) is determined by location, a panel including those new enumeration areas would be inappropriate and therefore, the refreshment enumeration areas are excluded from all analysis of panel data, where individuals (households) appear over multiple rounds of the GHS. We discuss the relative risk to bias from attrition and our approach to account for non-random attrition below.

The second data source is a panel of violent events taken from the Armed Conflict Location and Event Data (ACLED) project (Raleigh *et al.*, 2010). The ACLED project provides several pieces of detailed information, including the approximate date and geo-coded location of an incident, as well as reported information on primary and associated actors in each event. The recorded events are based on accounts from media, NGOs, international organizations, partner reports, and new media (e.g., social media such as Twitter or Facebook). A single account can include several events (as would be the case of one news

[22] These data (and all documentation) were accessed via the World Bank's microdata catalog. Data were downloaded on November 8, 2019.

[23] Nigeria includes 6 geopolitical zones, which are one administrative level up from states.

article describing a series of violent attacks), however, the ACLED database includes one observation per event-location.

We use the detailed information provided by the ACLED project to code individual events. Since the ACLED project also provides information on events such as treaties and peaceful protests, events are classified as i) violent and/or ii) herder-related. Violent events are defined as those with an ACLED coding with a clearly defined violent event type or sub-type. These events include attacks, murders, and kidnappings, but also civil unrest (e.g., mob violence), which may emanate from reprisal or public reaction. The violent destruction of property (such as arson or burning of fields) is also included, given that such events were frequent around the farmer-herder conflict. All fatal events are considered as violent.

We code all incidents as herder-related if they include the terms "pastoralist," "herder," or "herdsmen" as either an actor or associated actor.[24] As noted above, the incident data largely rely on public accounts, and so one risk is that the narrative framing of events could affect the terms used to record each event in the ACLED database. This would occur if terms like "herder" are used when these groups were considered perpetrators, but if another term is used when such groups were the victims of, say, reprisal violence. To mitigate this, we also code incidents involving the term "Fulani" (e.g., the tribe most identified with the nomadic herding) as herder-related. Thus, these data include violent events that occur between herders and civilians, clashing militias, and/or reprisals or actions taken against herdsmen or Fulani tribes. Due to this, we prefer to use the general term herder-related violence. In total, we code 1,564 incidents between 2009 and 2019 as both violent and herder-related.

We combine the GHS and ACLED datasets using GPS coordinates, which are available in both datasets. To construct our main treatment variable of exposure to a herder-related violent event, we create a series of binary variables that take a value of one if a household was within a given distance from any HRV in the month prior to the start of the GHS interview. The exposure measures are calculated for distance windows of 10, 20, and 30 kilometers (km.). We also show an alternative set of results that use a three-month time frame. We are able to calculate exposure measures that are as precise as 10 km. since we were able to use the restricted-use unmasked GPS coordinates from the GHS data.[25] Our preferred specifications use the 10 km. exposure radius as it is the most precise available in our measures, while allowing for potential mis-measurement of the exact location of violent events in the ACLED data. To account for potentially unobservable confounders, we opt for a narrow definition of our treatment and comparison groups. Specifically, we limit our sample to the set of individuals (households) that were within 50 km. of an herder-related violent event (for a given time window), where treatment is defined as exposure within a narrower radius.

[24] Or variants of these terms, including e.g. "Herder."

[25] We are indebted to the World Bank's LSMS team for producing these measures using the unmasked GPS coordinates. Note that this process involved, first, calculating exposure variables using publicly available GPS data; this process was replicated by the LSMS team using the unmasked data. To assure household anonymity, we never gained access to linked data between individual households and specific violent events.

3.2 Descriptive Statistics

Table 1 shows basic descriptive statistics for our set of key, labor-related outcomes at the individual level. We focus on six labor outcomes. The first three are binary values that take a value of one if an individual reports to have worked in own-account or household-enterprise work, agricultural work, or work outside the home, respectively, in the last week. These variables are available in all four rounds of the GHS. Starting in the 2015-2016 round, the GHS began recording the number of hours individuals worked in each of these activities. We report the inverse hyperbolic sine (IHS) values of these variables to account for their non-normal distribution and also so that the transformed values evaluate when a value of zero hours is reported. The latter concern is particularly important in cases where an individual has zero hours reported in one time period but non-zero values in other periods, allowing us to account for changes in work at the extensive margin.

We report these descriptive statistics in six panels, which roughly align to the identification strategy we describe in the next section. All panels are limited to individuals that appear in the data in the 2018-2019 round and in at least one prior GHS round, meaning the sample roughly corresponds to panel specifications with the inclusion of an individual-level fixed effect. The GHS documentation notes that there was a change in the covered enumeration areas with the addition of a refresher set of uneration areas in the 2018-2019 round. By limiting this sample to individuals who appear in the 2018-2019 round and at least one prior GHS round, any sample attrition originates from individuals who dropped from the sample for reasons other than changes in the areas enumerated.[26] For completeness, we also include the descriptive statistics of our main, considered co-variates in Appendix Table A1.

Panel A shows the descriptive statistics for the full sample, while panel B limits the sample to individuals in households that were located within 50 km. of a herder-related violent event. The two panels show virtually indistinguishable means for the key outcome variables: roughly one in five individuals has recently taken on informal work (own-account or in a household enterprise), a third have worked in agriculture, while approximately seven percent have taken on work outside of the home. The near identical means, in turn, give us some confidence that our measure of violence exposures—falling within a 50 km. window—is plausibly as-good-as random.

However, when we split the within-50-km. sample into states with anti-grazing bans, compared to other states, notable differences are evident. Ban states (panel C) show markedly lower means of informal work, while more than half of the individuals in our sample report recent agricultural work. By contrast, in other states one in four individuals took on recent informal work, and only twenty-six percent of individuals worked recently in agriculture. This difference suggests that, in addition to the conditions underlying the herder-farmer tensions in the agriculture-intensive states that instituted anti open-grazing bans, the work patterns of individuals in those states are also notably different. These differences help motivate our subsequent analysis that separates treatment in ban states compared to the other non-ban

[26] Individuals that moved and were tracked are included.

states in Nigeria.

Lastly, as mentioned above, each round of the GHS contains two points of data collection: one in the planting period and one in the harvest period. Since the GHS rounds are conducted at somewhat irregular time periods (i.e., 2010-2011, 2012-2013, 2015-2016, and 2018-2019), we choose to consider separate samples for the planting and harvest periods. In other words, our analysis will compare planting periods to other planting periods as well as harvest to harvest. Panels E and F show the descriptive statistics for the six labor outcomes, divided into planting and harvest periods, respectively.

3.3 Estimation Specification

Our primary specification is a two-way fixed effects, difference-in-difference regression, formally defined as follows:

$$y_{iet} = \delta V_{et'} + \alpha_i + \gamma_{st} + (\boldsymbol{X'\beta}) \times r + \epsilon_{it} \tag{1}$$

Where y_{iet} is one of the six labor-related outcome variables shown in Table 1: three dummy variables if there was informal, agricultural, or out-of-the-home work reported for individual i in the last seven days, and three IHS-transformed variables for the hours worked in each respective type of work over the same period.[27] Enumeration areas are indexed by e and the month-year period of the interview (our measure of time) is indicated by t; α_i is an individual-level fixed effect and γ_t is a time (month-year) fixed effect. The main treatment $V_{et'}$ is a dummy variable that takes a value of one if a herder-related violent event occurred within an indicated distance window during the sharp rise of herder-farmer violence in 2018. We include a vector of controls, $\boldsymbol{X'}$, which we regard as time-invariant and, thus, interact each element with a non-linear time dummy for each survey round-season (e.g., 2019-harvest), indicated by r.

We make several decisions in order to very narrowly define our comparison and treatment groups to estimate equation (1). First, we limit our consideration of treatment to the deadly peak of violence that occurred in the first half of 2018. We have good reason to believe that this spike in violence was unique: this outbreak of incidents was more violent, lethal, and—according to the reports noted above—somewhat more indiscriminate, in part as a response to the opposition to open-grazing bans implemented in certain states. This choice also restricts treatment to one period, which avoids recently discussed problems with two-way fixed effects specifications under staggered implementation (Baker *et al.*, 2022).

Second, we narrow our comparison group to those individuals located in households that were within 50 km. of a violent event during this outbreak of violence. Along with the use of fixed effects at the level of enumeration areas, this geographical restriction helps ensure that no other local, unobserved shocks explain the observed variation in labor outcomes. Our choice of a 50 km. window is somewhat arbitrary, but we believe it is wide enough to allow the effects of exposure to violence to plausibly dissipate, while narrow enough to form a reasonable comparison group. Still, we face a choice when deciding on the distance windows we use to define treatment (Butts, 2021). We vary this distance window to the extent

[27] The outcomes for hours worked are only available in the 2015-2016 and 2018-2019 data.

that our data allow: over 10, 20, and 30 km. windows.[28] In doing so, each larger radius includes all treated individuals within smaller radii: the 20 km. window is *inclusive* of those treated in the 10 km. window, and the 30 km. window is inclusive of both the 20 and the 10 km. windows. Surely, any noted effects will not completely dissipate at these sharp cutoffs; effects are likely to be fuzzy, and so there may be some spillover effects in our comparison groups. To address this issue, we also include a 'donut' specification, which considers individuals as treated if they are within 10 km. of an event but omits the 11 to 20 km. ring, using the 21 to 50 km. group as the comparison group. Appendix A1.5 gives an illustration of the varying distance windows, including of the donut specification.

We are also careful to address issues that may arise due to the timing of the survey rounds. All specifications include a month-year fixed effect, which adjusts for common temporal shocks. As noted above, the intermittent timing of the GHS also gives us reason to treat the planting and harvest data separately. We estimate equation (1) separately for both periods, more directly comparing patterns in like seasons (planting to planting and harvest to harvest); these separate specifications can also reveal some additional patterns in the effects of exposure to herder-related violence in different seasons, just as the demands of individuals' work may change from season to season. However, this decision causes us to define the time window for treatment differently—denoted by t' in equation (1). In the planting season, our main specifications use a one-month time window. In later results, we also include a three-month time window for robustness. For the post-harvest season, however, since we want to consider the potential lasting effects of violence exposure, we define treatment as exposure to herder-related violence at any time in the planting season.

Lastly, our attempts to narrow the differences between the comparison and treatment groups also drives our selection of the elements of $\boldsymbol{X'}$. The accounts of the herder-farmer conflict noted that incidents tended to occur in heavily agricultural areas, and reports of the 2018 outbreak of violence cited nighttime raids, often in remote areas. We include dummy variables for three additional, binary location co-variates: if a household was located within 10 km. of a population center with at least 20,000 inhabitants, the state-level administrative center, and a market, respectively. Critically, the likelihood that effects from previous exposure to herder-related violence could affect individuals' reactions to this violence and, thus, may bias our results.[29] We, therefore, include a dummy variable indicating if a household had previously been exposed to herder-related violence in the two-year period before 2018. Likewise, in the harvest season specifications, since our main treatment is exposure herder-related violence in the planting season, we include an additional control if there has been another violent event in the interim. We treat all elements of $\boldsymbol{X'}$ as time invariant, fixing the location variables to their value as of the 2015-2016 GHS round, and thus, we also interact each element with a non-linear time trend in the form of time dummies for each round-season in the data (r).

As noted above, three states passed outright prohibitions on open grazing in 2016 and 2017. These

[28]For more information on how these measures were constructed, see A1.3.

[29]A concern similar to the one motivating much recent DiD literature, e.g., De Chaisemartin and D'Haultfœuille (2020b).

bans followed ongoing tensions but also resulted in large numbers of reprisal attacks and displacement. Many herders also reported that they were forced to move out of grazing-ban states, where their cattle could be confiscated, into neighboring states. There, they were also met with hostility, resulting in further conflict. The different levels and intensity of this violence in the ban states, in particular, motivates us to also consider those states separately from the other states in Nigeria. This pattern is clearly illustrated in Figure 3, which shows the six-month running average of herder-related violent incidents from the start of GHS data collection (August 2010) until the end of data collection. The vertical black line represents the month prior to the start of Round 4 (June 2018).

To test the differential effect in these states, we also use the following specification as an estimation specification which is akin to a triple difference-in-difference:

$$y_{iet} = \delta V_{et'} + \varphi V_{et'ban} + \alpha_i + \gamma_{st} + (\boldsymbol{X'\beta}) * r + \epsilon_{it} \tag{2}$$

Equation (2), in turn, gives the additional co-efficient of interest φ—indexed by a ban being in place *ban*—which indicates if the effect of herder-farmer violence differed in those ban states, which were also the locus of some of the most intense and deadly outbreaks of violence.

In sum, our identifying assumptions are that, within the narrowly defined comparison groups that we have outlined, exposure to a herder-related violent event was plausibly random. This plausibility rests upon several assumptions, which may be justifiable: various reports of the rash of herder-farmer violence noted that the patterns and location of the violence were shifting, largely as seasonal patterns continued to change. We likely do not observe any households or individuals *directly* involved in these events. And, so, our construction of our exposure variable can be considered plausibly random as this exposure likely precludes any one household being directly targeted, for example. Lastly, our use of individual fixed effects also, then, means our identifying assumption only needs to hold so far as the post-treatment *difference from* previous individual outcomes are non-parallel.

3.4 Sample Attrition

Endogenous sample attrition represents a further threat to credible identification. This could well be the case if prior violent events or unobserved shocks affect the likelihood that an individual remains in the panel sample. Indeed, in our study context this is a sizeable threat. There are 12,918 individuals in the GHS data who were first interviewed in the 2010 and who are also located in enumeration areas that were part of the coverage of all four rounds. Of these, 3,732 (or 29 percent) are observed all the way through wave 4 of the GHS, including those who moved and were tracked. This gives a back-of-the-envelope, cumulative attrition rate of approximately 16 percent for each period. While this is not worrisome for each individual period, it does present problems cumulatively. Although a relatively long household-level panel dataset provides the ability to track individuals and households over time and account for time-invariant individual or household characteristics, they also inherently lead to a greater risk of attrition bias. To address non-ignorable attrition, we use the following probit estimation to estimate attrition-corrected sampling weights:

$$pr(attrit = 0) = \Phi[\theta V_{et'} + \gamma_{st} + (\boldsymbol{X'\beta})t + \epsilon_{it}] \tag{3}$$

Equation (3) includes the enumeration area herder-related violent events, a state-time fixed effect and the full list of time-interacted co-variates as described above. From equation (3) we calculate a predicted probability of survival (attrit=0) for each individual and time period. We then average these probabilities and invert them to produce inverse probability weights (IPWs). We use these weights throughout our analysis of panel-level data, which provide a correction for potential attrition bias by up-weighting those individuals with a higher probability of attriting but remain in the sample. The mean value for our IPW is 3.2, with a standard deviation of 1.8, a minimum value of 1.3, and a maximum value 12.8. This correction relies on the assumption that attrition is random, conditional on the factors included in equation (3). We include individual-level fixed effects in all specifications to adjust for any unobserved time-invariant factors that could affect selection into exposure to herder-related violence.

4 Results

We present three sets of results. First, we examine the contemporaneous labor response in the planting season to exposure to herder-related violence in the same planting season. Second, we investigate the labor response in the subsequent harvest season to exposure to herder-related violence in the planting season. Finally, we examine agricultural and enterprise outcomes using a cross-sectional subset of our panel data to understand the welfare effects of labor responses after an exposure to herder-related violence.

4.1 Labor Response

We first investigate the labor response after exposure to herder-related violence. We do this in two ways. First, we estimate contemporaneous effects; the labor response in the planting season to exposure to herder-related violence in the planting season. Next, we estimate lagged effects; the labor response in the harvest season to exposure to herder-related violence in the planting season. Taken together, these results allow us to characterize how agricultural households respond to exposure to herder-related violence throughout the agricultural season.

Planting Season Results—We first estimate the contemporaneous labor response in the planting season to exposure to herder-related violence during the same planting season. Table 2 reports results from variations on equations (1) and (2) using a binary herder-related violence indicator variable that defines exposure to HRV within one month with varying distances (i.e., 10 km., 20 km., and 30 km.). The estimates reported in column (1) provide a baseline set of estimates, which we compare to more specific estimates in states implementing an open grazing ban reported in column (2). The estimates reported in column (1), across all distances defining herder-related violence exposure, imply that the likelihood of own-account work or in a household enterprise (i.e., our measure of informal work) does not respond to exposure to herder-related violence. Although this result may perhaps run counter to the view that the informal sector can serve as a 'safety net' in the literature, it is important to note that these estimates are relatively noisy and may mask some heterogeneous effects. We now turn to a key source of heterogeneity: effects in the states implementing open-grazing bans, which were followed by intense outbreaks of herder-related violence.

Column (2) in Table 2 reports results which differentiate effects by states that implemented open grazing prohibitions. Within states implementing an open grazing ban, we find that exposure to herder-related violence increases the likelihood that an individual will engage in own-account or household-enterprise work. The estimates, which are qualitatively consistent across all distances defining herder-related violence exposure, imply that exposure increases the likelihood of own-account or household-enterprise work in states implementing an open grazing ban by a range of 25 to 31 percentage points. These estimates are statistically significant at conventional levels and large in magnitude. Relative to the sample mean of 20 percent, these estimates imply an over 100 percent increase in informal work due to a change from no local herder-related violence to any herder-related violence.

So far we find that exposure to herder-related violence increases informal sector work on the intensive margin for households that live in a state implementing an open grazing ban. What explains this result? We first investigate this by estimating the effect of exposure to herder-related violence on other employment outcomes: (i) agricultural work and (ii) any work outside the home, both measured within the previous week. These results investigate if the increase in informal work pulls employment away from existing work in agriculture or pulls employment from any work not occurring at home.

Columns (3) and (4) in Table 2 report results using a binary outcome variable indicating any household farm work in the last week. The estimates reported in column (3), across all distances defining herder-related violence exposure, suggest that the likelihood of working in agricultural activities does not respond to exposure to herder-related violence. Again, these results may hide important heterogeneous effects. Column (4) reports results which differentiate estimates by states implementing an open grazing ban. Within states implementing an open grazing ban, we find that exposure to herder-related violence reduces the likelihood that an individual will engage in agricultural work. The estimates, which are qualitatively consistent across all distances defining herder-related violence exposure, imply that exposure reduces the likelihood of agricultural work by a range of 29 to 31 percentage points in states implementing an open grazing ban. These estimates are statistically significant at conventional levels and large in magnitude. Relative to the sample mean of 31 percent, these estimates imply roughly a 100 percent increase in agricultural work due to a change from no local herder-related violence to any herder-related violence.

Finally, columns (5) and (6) in Table 2 report results using a binary outcome variable indicating any work outside the home in the last week. The estimates reported in column (5), across all distances defining herder-related violence exposure, show that the likelihood of working outside of the home does not respond to exposure to herder-related violence. In column (6), when we differentiate by whether a state implemented an open grazing ban, we find a preponderance of evidence suggesting that exposure to herder-related violence is associated with only small changes in whether an individual worked outside of the home even in states implementing an open grazing ban. The one possible exception is in panel B, where we find a statistically significant and possibly economically meaningful increase in the likelihood of working outside the home. This result, however, does not persist in our preferred specification and is not robust to other distances defining herder-related violence exposure.

In addition to reporting results using different distances to define herder-related violence exposure, we conduct several additional robustness and sensitivity checks on the results discussed in this section so far. First, we investigate the possibility of spillover effects biasing our results. It may be the case that households who are just outside of the distance used to define exposure to herder-related violence may in fact behave as if they are exposed. Therefore, to test for bias driven by spillover effects, we estimate a 'donut' specification where we purposefully exclude households who are 11 and 20 km. away from a recorded herder-related violence event and therefore restrict our comparison between households within 10 km and households further than 20 km away but within 50 km. We report these results in panel D of Table 2 and find qualitatively similar effect estimates to what we find in panel A. That is, even when estimated with our 'donut' specification, exposure to herder-related violence increases the likelihood an individual engages in own-account or household-enterprise work, reduces the likelihood of agricultural work, and does not meaningfully change the likelihood of working outside the home for households within states implementing an open grazing ban.

We visualize these results in a series of figures which help provide additional context. The left panel of figure 4 illustrates estimates on the likelihood of own-account or household-enterprise work associated with exposure to herder-related violence. Across all distances defining herder-related violence exposure and using exposure within the previous month, we find that within all states exposure does not change the likelihood of own-account or household-enterprise work but we find consistent and diverging estimates based on whether the household is in a state implementing an open grazing ban. The left panel of Figure 5 shows estimates on the likelihood of agricultural work. Similarly, across all distances defining herder-related violence exposure we find that within all states exposure does not change the likelihood of agricultural work, increases the likelihood in non-ban states, and consistently decreases the likelihood in ban states. Finally, the left panel of Figure 6 reports estimates on the likelihood of work outside the home. Although the estimates are relatively noisy, we find that, within all states, exposure does not change the likelihood of work outside the home. Similarly, some estimates are not statistically significant, we find small decreases in the likelihood of work outside the home in non-ban states and small increases in the likelihood of work outside the home in ban states. Although, it should be noted that these estimates are much smaller in magnitude than the estimates on own-account or household-enterprise work and agricultural work.

In the right panel of Figures 4, 5, and 6 we also report results using an alternative time window to define exposure to herder-related violence. These results hold the distance used to define exposure fixed at 10 km., but show how estimates change when we use a three month time window verses a one month time window. These results all show the diminishing influences of exposure to herder-related violence over time. In the right panel of Figure 4, we find that when we use a three month time window the estimated change in the likelihood of own-account or household-enterprise work falls in magnitude and becomes statistically insignificant. Similar results hold for the estimates reported in the right panel of Figures 5 and 6.[30] Taken together, these results highlight that the contemporaneous effect of exposure to herder-related violence in the planting season is a relatively short-term phenomena. However, as we will soon discuss, we do find important lagged effects of exposure to herder-related violence in the planting season on labor outcomes in the subsequent harvest season.

The results discussed so far only consider an extensive margin labor response to exposure to herder-related violence. In Table 3 we report results using a continues measure of the number of hours worked in the previous week within our three labor categories. Each of these variables are transformed using the inverse hyperbolic sine transformation which is a log-like function that can compute zero-valued observations. With the caveat that these hours worked variables are only available in a sub-set of our full panel data set (i.e., in the 2015-2016 survey wave and onward), we find meaningful intensive margin labor responses in the planting season to exposure to herder-related violence in the planting season. Columns (1) and (2) report results on the number of hours worked in an own-account or household-enterprise in the last week. In panel A, which represents our preferred specification, we find in column (2) that exposure to herder-related violence during the planting season leads households living in states that implemented an open grazing ban to over triple the number of hours worked in an own-account household enterprise in the last week.[31] These results are qualitatively consistent in subsequent panels using different distance windows to define exposure to herder-related violence and in our 'donut' specification. Columns (3) and (4) report results on the number of hours worked in on farm agricultural work in the last week. In panel A, column (4) shows that exposure to herder-related violence during the planting season leads to a 60 percent decrease in the number of hours worked in on farm agricultural work in the last week for households who live in states that implemented an open grazing ban. Again, it is important to note that these results are qualitatively consistent in subsequent panels using different distance windows to define exposure to herder-related violence and in our 'donut' specification. Finally, columns (5) and (6) report results on the number of hours work on any work outside the home. In column (6), we find that exposure to herder-related violence for households living in states implementing an open grazing ban leads to a 38 percent increase in the number of hours worked outside the home.

Harvest Season Results—We now turn to estimating the labor response in the harvest season to exposure to herder-related violence during the planting season. Table 4 reports results from a regression

[30]We also show these results in tabular form in Table A2 in the Supplemental Appendix.

[31]With an outcome variable transformed by the inverse hyperbolic sine transformation, the semi-elasticity is approximately equal to 100 × exp($\hat{\beta}$) - 1, with $\hat{\beta}$ representing the coefficient of interest (Bellemare and Wichman, 2019).

specification that uses a binary indicator of herder-related violence during the plating season that defines exposure with varying distances (e.g., 10 km., 20 km., and 30 km.). We also report results from a 'donut' specification that aims to address concerns related to spillover effects.

Columns (1) and (2) in Table 4 report results using a binary outcome variable indicating working in an own-account or household-enterprise in the last week. Across all distances defining herder-related violence exposure and our 'donut' specification, we find that exposure during the planting season has a small and statistically insignificant effect on the likelihood of engaging in own-account household enterprise work in the harvest season. Columns (3) and (4) of Table 4 report results on the likelihood of agricultural work in the last week. Similar to the results in the previous two columns, across all distances defining herder-related violence exposure and our 'donut' specification, we find that exposure during the planting season has a small and statistically insignificant effect on the likelihood of engaging in agricultural work in the harvest season. Finally, columns (5) and (6) report results on the likelihood of any work outside the home. Again, across all distances defining herder-related violence exposure and our 'donut' specification, we find that exposure during the planting season has a small and statistically insignificant effect on the likelihood of any work outside the home in the harvest season. These results all represent the lagged extensive margin labor response to exposure to herder-related violence during the planting season, and broadly show little change across each of our labor outcomes.

These extensive margin outcomes may, however, hide an important intensive margin response. In Table 5 we report results using a continuous measure of the number of hours worked in the previous week within our three labor categories. Again, each of these variables are transformed using the inverse hyperbolic sine transformation. We find meaningful intensive margin labor responses in the harvest season to exposure to herder-related violence in the planting season. Columns (1) and (2) report results on the number of hours worked in an own-account or household-enterprise in the last week. In panel A, which represents our specification with the most narrow geographic distance boundary, we find in column (1) that exposure to herder-related violence during the planting season leads to a 62 percent increase in the number of hours worked in an own-account or household-enterprise in the last week. In column (2), when we differentiate effects by states that implemented an open grazing ban, we find that the sign of the coefficient changes and the magnitude remains large. Within states implementing an open grazing ban, we find that exposure to herder-related violence reduces the number of hours worked in an own-account household enterprise by 41 percent. The difference in these estimated effects between states implementing an open grazing ban and other states highlights the importance of this policy-relevant heterogeneity in our analysis. It should be noted, however, that while these results are robust when we use the 'donut' specification in panel D, they are generally not robust to wider geographic distance boundaries, in panels B and C.

Columns (3) and (4) report results on the number of hours worked in on farm agricultural work in the last week. In panel A, column (3) shows that exposure to herder-related violence during the planting season leads to a 37 percent decrease in the number of hours worked in on farm agricultural work in the last week. When we differentiate effects by states that implemented an open grazing ban, in column (4), we find that the sign of the coefficient changes and the magnitude remains large. Within states implementing an open grazing ban, we find that exposure to herder-related violence increase the number of hours worked in on farm agricultural work by 54 percent. Similar to the results in the previous two columns, the difference in these estimated effects between states implementing an open grazing ban and other states highlights important heterogeneity in our analysis. Again, it is important to note that while the results are generally not robust to wider geographic distance boundaries, in panels B and C, they are robust when we use the 'donut' specification in panel D.

Finally, columns (5) and (6) in Table 5 report results on the number of hours work on any work outside the home. The estimates reported in column (5), across all distances defining herder-related violence exposure, imply that exposure does not have a statistically significant effect on the number of hours worked outside of the home. In column (6), when we differentiate the effect by whether a state implemented an open grazing ban, again find no evidence that exposure to herder related violence during the planting season has an effect on the number of hours worked outside the home during the harvest season.

One way to interpret these results is that agricultural households in states that implemented a open grazing ban and were exposed to herder-related violence during the planting season feel the need to make up for lost time working in agricultural work. Exposure to herder-related violence during the planting season increases the likelihood they engage in own-account household enterprise work and reduces the likelihood that they engage in agricultural work during the planting season. During the harvest season, these households reduce the number of hours worked in own-account or household-enterprise work and increase the number of hours worked in agricultural work. In the next section we will examine the extent to which these contemporaneous and lagged labor responses to exposure to herder-related violence influence revenue earned from agricultural or enterprise work.

4.2 Harvest Use, Agricultural Marketing, and Enterprise Sales

We now investigate the consequences of exposure to herder-related violence in the planting season on outcomes, such as: harvest use, agricultural marketing, and enterprise sales. These outcomes explore possible mechanisms that might explain why previous research finds that exposure to conflict can lead to diminished food security (George *et al.*, 2020). Our analysis in this section requires that we restrict our data to the 2018-2019 round of data, where the variables measuring harvest use, agricultural marketing, and enterprise sales are consistently and accurately recorded and available. The use of these cross-sectional data requires a similar but slightly more restrictive identifying assumption compared to the use our panel data in previous sections. In particular, our analysis in this section requires we assume that exposure to herder-related violence is exogenous to our outcome variables of interest within the 2018-2019 cross section of our data, and does not allow us to account for time-invariant household-level characteristics.

Use of Harvest—We first estimate how exposure to herder-related violence during the planting season influences how agricultural households use their harvest. Table 6 reports results on various ways households can use their harvest. Columns (1) and (2) examine the total amount (measured in kilograms transformed by the inverse hyperbolic sine transformation) and do not find evidence of robust findings across each regression specification using different distances to define exposure to herder-related violence exposure. In panel A, we estimate an overall increase in the amount of crops harvested associated with exposure to herder-related violence and a relatively large but statistically insignificant decrease when we differentiate this effect within states that implemented an open grazing ban. These findings, however, do not hold in panels B, C, or D suggesting that these results are relatively sensitive to how we define exposure to herder-related violence. In columns (3) and (4) we estimate the effect of exposure to herder-related violence in the planting season on the amount of crops saved for future use. We find evidence of a reduction in the amount of crops saved the future for households living in states that implemented an open grazing ban. In terms of magnitude, we find that exposure to herder-related violence in the planting season is associated with a 48 to 70 percent reduction in the amount of crops saved for future use. The only exception to this finding is in Panel C, where we use a relatively wide distance window to define exposure to conflict. Similarly, in columns (5) and (6) we find that exposure to herder-related violence is associated with a reduction in household consumption of an agricultural household's own harvested crops. Although these results are only statistically significant when using a relatively narrow distance window to define exposure, in panels A and D, the sign is robust across all specifications. In particular, we find that exposure to herder-related violence in the planting season is associated with a 53 to 78 percent reduction in the amount of a household's own crops that they keep for their own consumption.

The remainder of the columns in Table 6 investigate the amount of a household's harvest crop that is either paid or given away, the amount of crop that is lost, and the amount of crop that is sold. For each of these outcomes, and across each of the distances defining herder-related violence exposure, we find inconsistent results. We do observe a relatively large and statistically significant reduction in the amount of harvest paid or given, in panel C of column (8), but this result does not hold in other panels using more conservative distance windows to define exposure. Similarly, we also find a relatively large and statistically significant reduction in the amount of a household's crop that is lost, in panel C of column (10), but this result is not robust to the use of alternative more conservative distance windows to define exposure to herder-related violence. Finally, in columns (11) and (12) we do not find any statistically significant changes in the amount of crop sold associated with exposure to herder-related violence.

Agricultural Marketing—We now turn to a more specific investigation of how agricultural sales of harvested crops respond to an agricultural household's exposure to herder-related violence in the planting season in Table 7. Columns (1) and (2) show results using a binary outcome variable indicating if the household sold crops. In panels A and D of column (1), which use a relatively narrow distance window to define exposure, we find that exposure to herder-related violence is generally associated with a 12 percentage point reduction in the likelihood an agricultural household will sell crops. In column (2), we find no difference for households within states that implemented an open grazing ban. The next six columns show results on the amount of unprocessed or processed crops sold conditional on if a household sold crops. In panel A of column (4), we observe that exposure to herder-related violence in the planting season is associated with a reduction in the amount of unprocessed crops sold for households in states that implemented an open grazing ban. Although the statistical significance of this result is not robust when using wider distance windows to define exposure, it is robust to the use of our 'donut' specification in panel D. To the contrary, in panel A of column (6), we find that exposure to herder-related violence in the planting season is associated with an increase in the amount of processed crops sold for households in states that implemented an open grazing ban. This finding is the strongest when we use relatively narrow windows defining exposure, and declines in magnitude and statistical significance when using a wider distance window to define exposure and when using our 'donut' specification. Finally, columns (7) and (8) confirm the previous results by an outcome variable representing the share of the amount of crops sold unprocessed over the total amount of crops sold. The findings in column (8) confirm that agricultural households in states that implemented an open grazing ban increase the amount of unprocessed crops sold when exposed to herder-related violence.

Non-farm Enterprise Sales—Finally, we examine changes in non-farm enterprise sales for households exposed to herder-related violence. Our previously discussed labor response results show that agricultural households in states that implemented an open grazing ban and exposed to herder-related violence during the planting season initially were much more likely to work on their own-account or household-enterprise. Subsequently, in the harvest season, these households dedicated fewer hours to informal work. So, the question remains: how much, if at all, do non-farm household enterprise sales change for households exposed to herder related violence? Moreover, does in a state that implemented an open grazing ban influence the associated changes?

Table 8 reports results estimating changes in indicators of non-farm enterprise sales associated with exposure to herder-related violence. In columns (1) and (2) we report estimates using a binary outcome variable if the household had any non-farm enterprise sales. With the exception of the widest distance window defining exposure, in panel C, we find that although households exposed to herder-related violence do not increase the likelihood of having non-farm enterprise sales in general, households in states that implemented an open grazing ban do increase the likelihood of having non-farm enterprise sales. In particular, agricultural households in states that implemented an open grazing ban and were exposed to herder-related violence are between 13 and 22 percentage points more likely to have non-farm enterprise sales. Columns (3) and (4) report results with a specification using the amount of non-farm enterprise sales, transformed using the inverse hyperbolic sine function. Although the estimated changes associated with exposure to herder-related violence is only statistically significant in panel B, the estimates are of meaningful magnitude in the specifications the most narrow distance windows defining exposure and using the 'donut' specification. In particular, we find that households living in states that implemented an open grazing ban and were exposed to herder-related violence experienced an increase in non-farm enterprise sales of between 42 and 135 percent. These result aligns with the previously discussed labor response results and also support the view that the informal sector can serve as a 'safety net' found in the existing literature.

5 Conclusion

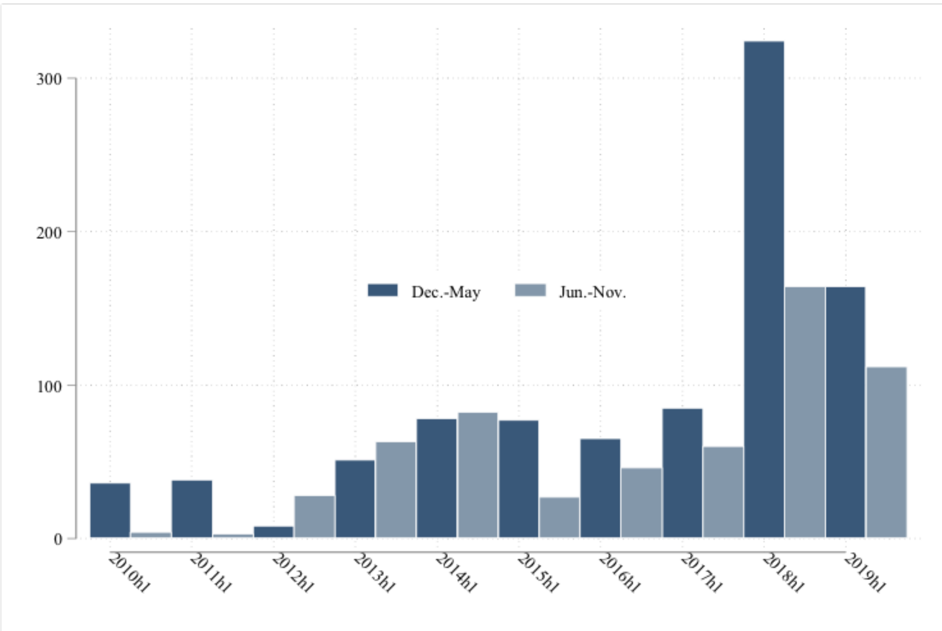
We study how households cope with the consequences associated with exposure to conflicts. In the absence of conflict, the benefit of agricultural work is worth the risk. When exposed to conflict, however, the risk associated with agricultural work may outweigh the benefit and, therefore, agricultural households must seek strategies to cope with this risk. To do this we investigate how agricultural households in Nigeria respond to a specific type of conflict. Herder-related violent conflict events describe clashes between nomadic herders and sedentary agricultural communities in Nigeria, which are partly motivated by increasing competition for scarce land and water resources (McGuirk and Nunn, 2020; George *et al.*, 2021a). As agricultural seasons have shifted associated with climate change, nomadic herders have changed the timing of their migratory patterns and agricultural households have lengthened their growing seasons. Rising tensions motivated several Nigerian states to implement open grazing bans in 2016 and 2017. These bans are associated with a sharp escalation of violence in the first half of 2018, which represents the core source of variation in herder-related violence in our study.

We construct panel data by combining information from Nigeria's General Household Survey and the ACLED Project from 2010 through 2019 and pay particular attention to both the seasonality in herder-related conflict and how agricultural households respond to exposure throughout the agricultural season. We find that in states that implemented open grazing bans households exposed to herder-related violence in the planting season are more likely to engage in informal work and less likely to engage in agricultural work in the contemporaneous planting season at both the extensive and intensive margins. In the subsequent harvest season, agricultural households exposed to herder-related violence in the previous planting season and living in states that implemented an open grazing ban did not change their informal work or agricultural work on the extensive margin, but did on the intensive margin. These results are consistent with the view that work in the informal sector can act as an important 'safety net' for agricultural households exposed to herder-related violence in Nigeria.

What are the consequences of this labor response to exposure to herder-related violence? We find that households living in states that implemented an open grazing ban and were exposed to herder-related violence in the planting season are less likely to save their harvested crops for future use and or their own consumption. Instead, these households are less likely to sell unprocessed crops and more likely to sell processed crops. Furthermore, although households exposed to herder-related violence are more likely to work with and earn sales from a non-farm enterprise on the extensive margin, we do not find robust evidence that these households earn more sales on the extensive margin. Taken together, these results suggest that households exposed to herder-related violence within states that implemented an open grazing ban are (i) less likely to store their crop and be able to take advantage of output price fluctuations, (ii) less likely to use their crop for their own food consumption and are at risk of increased challenges associated with achieving food security (George *et al.*, 2020), and (iii) despite working more in an informal enterprise are not more likely to report additional non-farm enterprise sales.

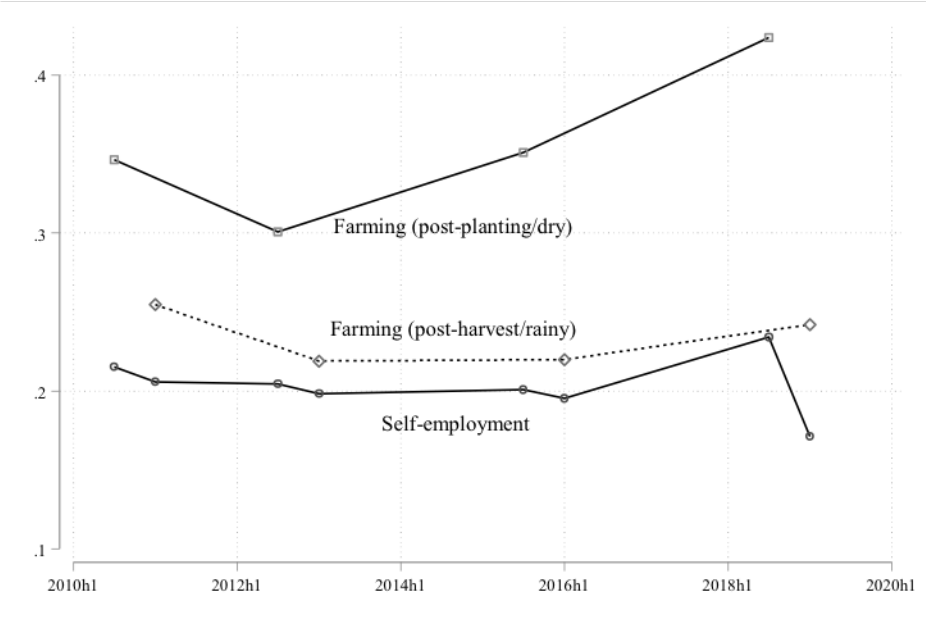
Despite our finding that agricultural households exposed to herder-related violence in Nigeria might use the informal sector as a way to cope with the increased risk associated with agricultural production, this 'safety net' does not seem to effectively guard against these households experiencing adverse economic consequences. Therefore, herder-related violence can have wide-reaching indirect costs beyond the direct costs associated with the loss of human life and the destruction of property. In the absence of the elimination of violence and conflict in the future, future policy-relevant research could focus on understanding how to most effectively provide formal economic support for households exposed to violence and conflict to supplement informal coping mechanisms.

Figure 1: **Total Herder-Involved Violent Events, 2010-2019**



Source: Authors' calculations based on ACLED data.

Figure 2: **GHS Sample Means: Farming and Self-Employment**



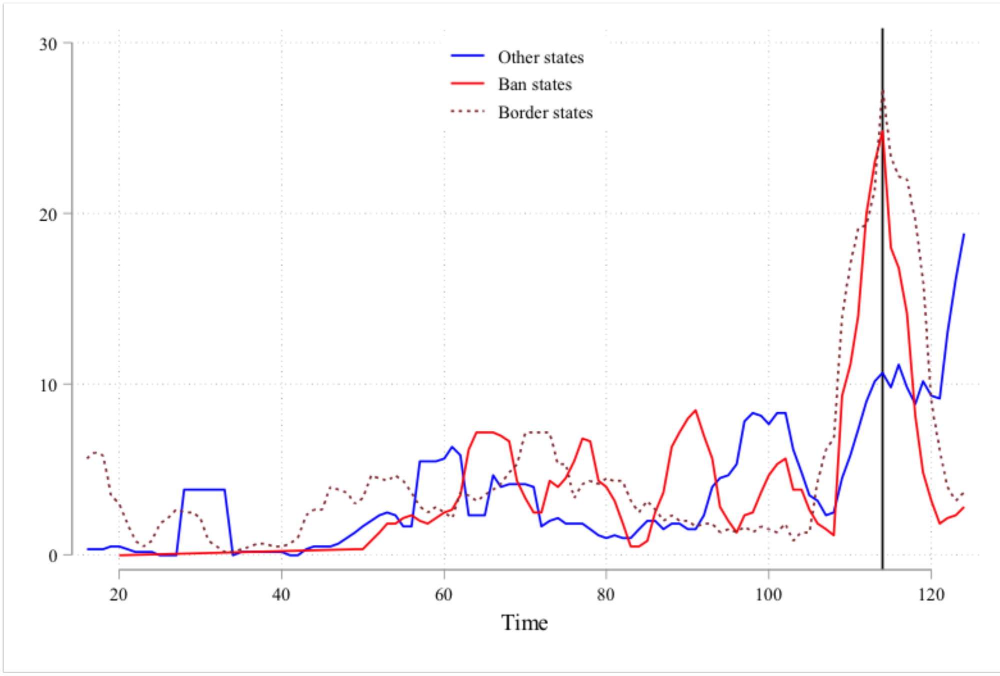
Source: Authors' calculations based on GHS data.

Table 1: Descriptive Statistics

(A) Sample	Mean	SD	Min.	Max.	n
Own-account HH-enterprise work (Y=1)	0.20	0.40	0	1	48,331
Agricultural work (Y=1)	0.31	0.46	0	1	48,346
Work outside the home (Y=1)	0.07	0.25	0	1	48,339
Hrs. Own-account HH-enterprise work (IHS)	0.78	1.61	0.00	5.35	26,706
Hrs. Agricultural work (IHS)	1.14	1.75	0.00	5.12	26,716
Hrs. Work outside the home (IHS)	0.26	1.04	0.00	5.26	26,719
(B) HRV within 50 km.	Mean	SD	Min.	Max.	n
Own-account HH-enterprise work (Y=1)	0.20	0.40	0	1	8,325
Agricultural work (Y=1)	0.33	0.47	0	1	8,326
Work outside the home (Y=1)	0.07	0.25	0	1	8,319
Hrs. Own-account HH-enterprise work (IHS)	0.76	1.60	0.00	5.35	4,611
Hrs. Agricultural work (IHS)	1.29	1.82	0.00	5.12	4,611
Hrs. Work outside the home (IHS)	0.28	1.05	0.00	5.10	4,611
(C) HRV within 50 km., Ban States	Mean	SD	Min.	Max.	n
Own-account HH-enterprise work (Y=1)	0.06	0.25	0	1	2,346
Agricultural work (Y=1)	0.52	0.50	0	1	2,347
Work outside the home (Y=1)	0.05	0.21	0	1	2,342
Hrs. Own-account HH-enterprise work (IHS)	0.28	0.99	0.00	4.94	1,299
Hrs. Agricultural work (IHS)	1.92	1.96	0.00	4.97	1,299
Hrs. Work outside the home (IHS)	0.18	0.86	0.00	4.72	1,299
(D) HRV within 50 km., Other States	Mean	SD	Min.	Max.	n
Own-account HH-enterprise work (Y=1)	0.25	0.43	0	1	5,979
Agricultural work (Y=1)	0.26	0.44	0	1	5,979
Work outside the home (Y=1)	0.08	0.26	0	1	5,977
Hrs. Own-account HH-enterprise work (IHS)	0.95	1.75	0.00	5.35	3,312
Hrs. Agricultural work (IHS)	1.04	1.70	0.00	5.12	3,312
Hrs. Work outside the home (IHS)	0.31	1.12	0.00	5.10	3,312
(E) HRV within 50 km., Post-planting Season	Mean	SD	Min.	Max.	n
Own-account HH-enterprise Work (Y=1)	0.20	0.40	0	1	4,211
Agricultural Work (Y=1)	0.39	0.49	0	1	4,212
Work outside the home (Y=1)	0.07	0.25	0	1	4,205
Hrs. Own-account HH-enterprise Work (IHS)	0.79	1.64	0.00	5.30	2,369
Hrs. Agricultural Work (IHS)	1.57	1.92	0.00	4.97	2,369
Hrs. Work outside the home (IHS)	0.27	1.04	0.00	5.10	2,369
(F) HRV within 50 km., Post-harvest Season	Mean	SD	Min.	Max.	n
Own-account HH-enterprise Work (Y=1)	0.19	0.39	0	1	4,114
Agricultural Work (Y=1)	0.28	0.45	0	1	4,114
Work outside the home (Y=1)	0.07	0.25	0	1	4,114
Hrs. Own-account HH-enterprise Work (IHS)	0.72	1.57	0.00	5.35	2,242
Hrs. Agricultural Work (IHS)	0.99	1.67	0.00	5.12	2,242
Hrs. Work outside the home (IHS)	0.29	1.07	0.00	4.97	2,242

Source: Authors' tabulations of individual-level GHS data. All panels include observations that are observed in the 2018-2019 GHS round and also at least one previous round. Borno state is excluded. IHS - indicates that the variable has been transformed using the inverse hyperbolic sine.

Figure 3: **Six-Month Running Average of Herder-Related Violence**



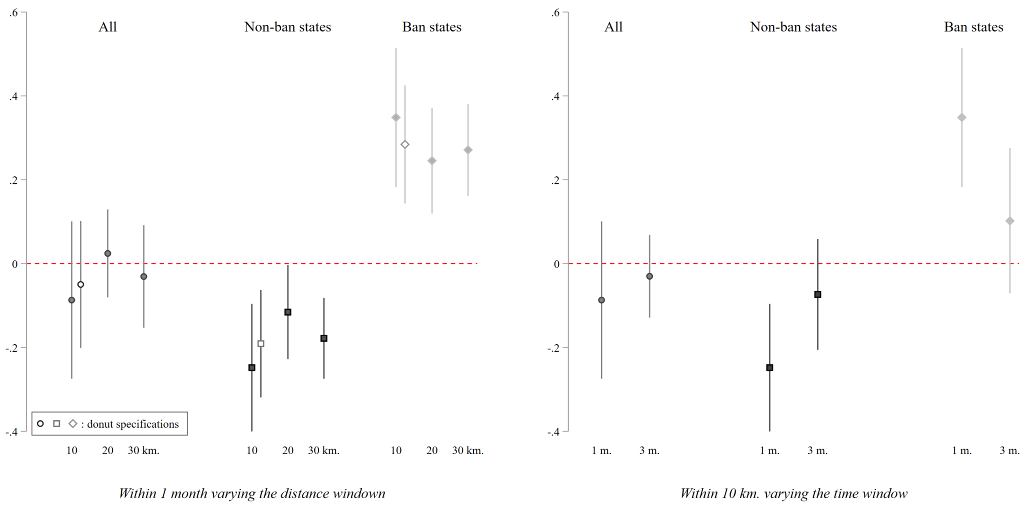
Source: Authors' calculations based on ACLED data. Each line shows the six-month running average of all herder-related incidents by state group. BAN states are Benne, Ekiti, and Taraba. The time-frame runs from August 2010 (t=20) through March 2019 (t=123). The vertical line identifies the month prior to the start of Round 4.

Table 2: Planting Season Labor Response to HRV Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Own-account HH-enterprise		Agricultural work		Work outside the home	
Panel A: HRV exposure = 1 month, 10 km						
HRV in planting season	-0.0869	-0.248***	0.0785	0.227*	-0.0313	-0.0567**
	(0.0928)	(0.0753)	(0.108)	(0.114)	(0.0189)	(0.0230)
HRV*Ban state		0.349***		-0.322**		0.0548*
		(0.0820)		(0.127)		(0.0319)
R-squared	0.697	0.699	0.702	0.703	0.730	0.730
Observations	4,053	4,053	4,063	4,063	4,057	4,057
Panel B: HRV exposure = 1 month, 20 km						
HRV in planting season	0.0243	-0.116**	0.0352	0.190*	-0.0178	-0.0673***
	(0.0519)	(0.0556)	(0.0780)	(0.0961)	(0.0196)	(0.0217)
HRV*Ban state		0.245***		-0.270**		0.0868***
		(0.0624)		(0.104)		(0.0280)
R-squared	0.699	0.700	0.702	0.703	0.729	0.730
Observations	4,053	4,053	4,063	4,063	4,057	4,057
Panel C: HRV exposure = 1 month, 30 km						
HRV in planting season	-0.0308	-0.178***	-0.0527	0.0403	0.0156	-0.00331
	(0.0604)	(0.0477)	(0.0608)	(0.0857)	(0.0206)	(0.0296)
HRV*Ban state		0.271***		-0.171**		0.0348
		(0.0541)		(0.0759)		(0.0244)
R-squared	0.697	0.700	0.705	0.706	0.729	0.729
Observations	4,053	4,053	4,063	4,063	4,057	4,057
Panel D: Donut specification						
HRV in planting season	-0.0497	-0.191***	0.0251	0.174	-0.0231	-0.0496
	(0.0747)	(0.0633)	(0.1000)	(0.118)	(0.0212)	(0.0325)
HRV*Ban state		0.284***		-0.299**		0.0534
		(0.0695)		(0.130)		(0.0394)
R-squared	0.707	0.708	0.721	0.722	0.729	0.729
Observations	3,756	3,756	3,756	3,756	3,756	3,756

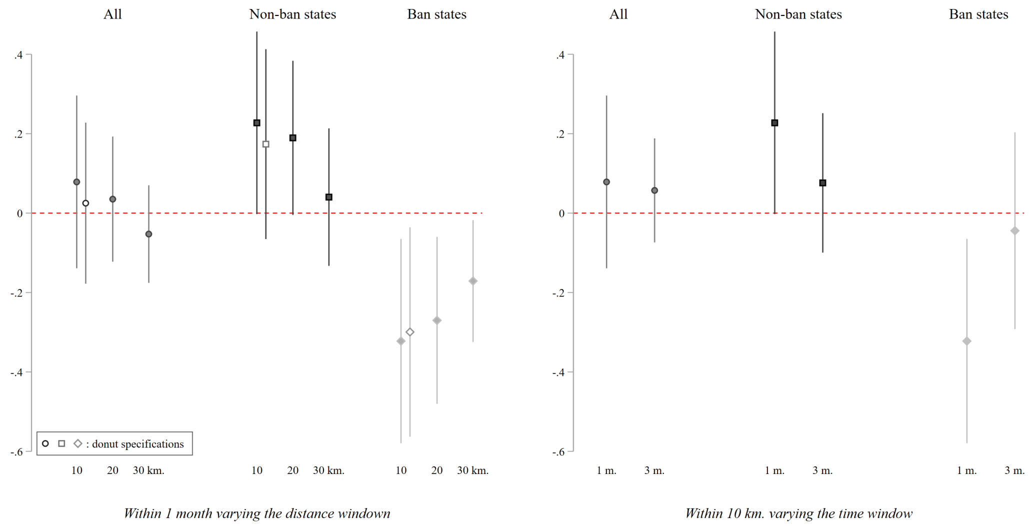
*** p<0.01, ** p<0.05, * p<0.1. Individual-level panel data, over four post-planting rounds of the GHS. HRV: herder-related violent event in the planting season. Borno state is excluded. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance and time windows, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include individual-level, year-month (time), and EA-level fixed effects. X' is a vector of controls, each interacted with a non-parametric time trend and includes a confrol exposure to a violent event in the 2 years prior to the 2018 planting season and additioanl controls, including dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

Figure 4: **Own-account HH-enterprise work in the last week (planting season)**



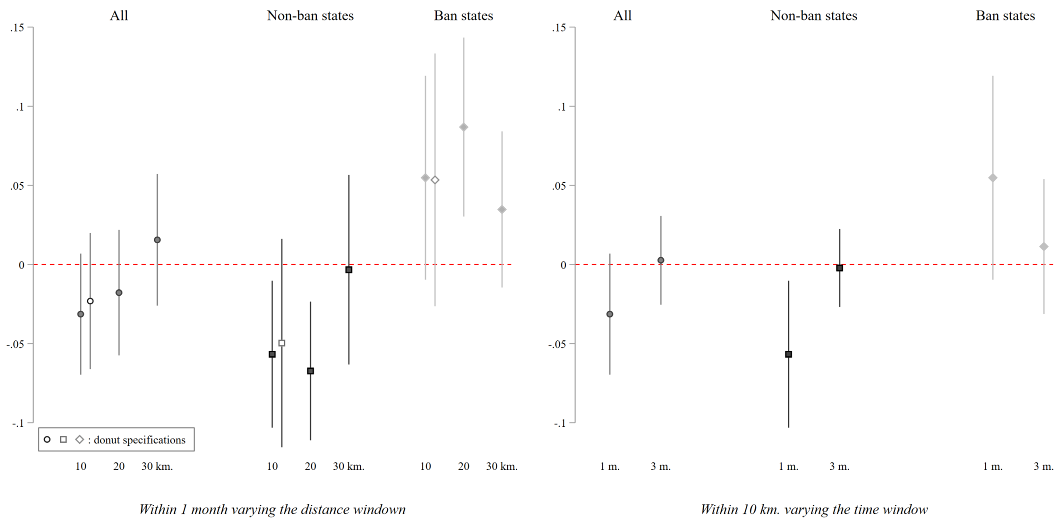
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals in the left panel correspond to estimates in Table 2. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (1). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (2); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The left panel varies the distance window as shown in the four panels (A–D) of the table. Similarly, the right panel shows the main specifications for the 10-km. and 1-month windows (Panel A), but it also includes coefficients from an additional specification where the time window is extended to three months (results not shown in the table).

Figure 5: **Agricultural work in the last week (planting season)**



Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals in the left panel correspond to estimates in Table 2. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (3). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (4); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The left panel varies the distance window as shown in the four panels (A–D) of the table. Similarly, the right panel shows the main specifications for the 10-km. and 1-month windows (Panel A), but it also includes coefficients from an additional specification where the time window is extended to three months (results not shown in the table).

Figure 6: **Work outside the home in the last week (planting season)**



Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals in the left panel correspond to estimates in Table 2. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (5). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (6); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The left panel varies the distance window as shown in the four panels (A–D) of the table. Similarly, the right panel shows the main specifications for the 10-km. and 1-month windows (Panel A), but it also includes coefficients from an additional specification where the time window is extended to three months (results not shown in the table).

Table 3: Planting Season Labor Response to HRV Exposure, IHS Transformed Hours Worked

	(1)	(2)	(3)	(4)	(5)	(6)
	Own-account HH-enterprise		Agricultural work		Work outside the home	
Panel A: HRV exposure = 1 month, 10 km						
HRV in planting season	-0.309	-1.054***	-0.0479	0.388**	-0.0855	-0.239**
	(0.381)	(0.296)	(0.241)	(0.167)	(0.0937)	(0.112)
HRV*Ban state		1.553***		-0.909***		0.321**
		(0.361)		(0.286)		(0.142)
R-squared	0.783	0.786	0.853	0.854	0.843	0.843
Observations	1,586	1,586	1,586	1,586	1,586	1,586
Panel B: HRV exposure = 1 month, 20 km						
HRV in planting season	0.143	-0.406	0.213	0.430**	-0.0705	-0.332***
	(0.219)	(0.242)	(0.338)	(0.207)	(0.0927)	(0.114)
HRV*Ban state		1.004***		-0.396		0.478***
		(0.311)		(0.446)		(0.130)
R-squared	0.787	0.790	0.853	0.853	0.842	0.843
Observations	1,586	1,586	1,586	1,586	1,586	1,586
Panel C: HRV exposure = 1 month, 30 km						
HRV in planting season	-0.0607	-0.613**	-0.0632	0.341	0.0351	-0.125
	(0.216)	(0.259)	(0.292)	(0.206)	(0.0874)	(0.102)
HRV*Ban state		1.030***		-0.753*		0.299***
		(0.309)		(0.388)		(0.0884)
R-squared	0.782	0.787	0.852	0.854	0.842	0.842
Observations	1,586	1,586	1,586	1,586	1,586	1,586
Panel D: Donut specification						
HRV in planting season	-0.270	-0.937***	-0.0753	0.433**	-0.101	-0.251**
	(0.349)	(0.300)	(0.256)	(0.180)	(0.0915)	(0.110)
HRV*Ban state		1.373***		-1.045***		0.310**
		(0.377)		(0.303)		(0.144)
R-squared	0.788	0.790	0.861	0.863	0.850	0.851
Observations	1,470	1,470	1,470	1,470	1,470	1,470

*** p<0.01, ** p<0.05, * p<0.1. Individual-level panel data, over four post-planting rounds of the GHS. HRV: herder-related violent event in the planting season. Borno state is excluded. IHS: inverse hyperbolic sine. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance and time windows, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include individual-level, year-month (time), and EA-level fixed effects. X' is a vector of controls, each interacted with a non-parametric time trend and includes a confrol exposure to a violent event in the 2 years prior to the 2018 planting season and additioanl controls, including dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

Table 4: Harvest Season Labor Response to HRV Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Own-account HH-enterprise		Agricultural work		Work outside the home	
Panel A: HRV exposure = 1 month, 10 km						
HRV in planting season	0.0690	0.0696	0.0234	0.0238	-0.00603	-0.00607
	(0.0703)	(0.0709)	(0.0832)	(0.0837)	(0.0120)	(0.0121)
HRV*Ban state		-0.0810		-0.0645		0.00465
		(0.0734)		(0.0846)		(0.0155)
R-squared	0.740	0.740	0.693	0.693	0.743	0.743
Observations	8,138	8,138	8,137	8,137	8,137	8,137
Panel B: HRV exposure = 1 month, 20 km						
HRV in planting season	0.0553*	0.0503	-0.0346	-0.0286	0.00131	0.000275
	(0.0309)	(0.0358)	(0.0495)	(0.0568)	(0.0071)	(0.0075)
HRV*Ban state		0.0317		-0.038		0.00658
		(0.0452)		(0.0709)		(0.0152)
R-squared	0.740	0.740	0.693	0.693	0.743	0.743
Observations	8,138	8,138	8,137	8,137	8,137	8,137
Panel C: HRV exposure = 1 month, 30 km						
HRV in planting season	0.0425*	0.0332	-0.0164	-0.0162	-0.00363	-0.00780
	(0.0218)	(0.0255)	(0.0340)	(0.0405)	(0.00746)	(0.00778)
HRV*Ban state		0.0500		-0.000920		0.0225
		(0.0614)		(0.0853)		(0.0234)
R-squared	0.739	0.739	0.693	0.693	0.743	0.743
Observations	8,138	8,138	8,137	8,137	8,137	8,137
Panel D: Donut specification						
HRV in planting season	0.0683	0.0689	0.0234	0.0239	-0.00614	-0.00618
	(0.0699)	(0.0705)	(0.0832)	(0.0837)	(0.0120)	(0.0121)
HRV*Ban state		-0.0810		-0.0646		0.00457
		(0.0731)		(0.0846)		(0.0155)
R-squared	0.741	0.741	0.691	0.691	0.735	0.735
Observations	8,122	8,122	8,121	8,121	8,121	8,121

*** p<0.01, ** p<0.05, * p<0.1. Individual-level panel data, over four post-harvest rounds of the GHS. HRV: herder-related violent event in the planting season. Borno state is excluded. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance window, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include individual-level, year-month (time), and EA-level fixed effects. X' is a vector of controls, each interacted with a non-parametric time trend and includes a confrol exposure to a violent event in the 2 years prior to the 2018 planting season and additioanl controls, including dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

Table 5: Harvest Season Labor Response to HRV Exposure, IHS Transformed Hours Worked

	(1)	(2)	(3)	(4)	(5)	(6)
	Own-account HH-enterprise		Agricultural work		Work outside the home	
Panel A: HRV exposure = 1 month, 10 km						
HRV in planting season	0.488***	0.503***	-0.457***	-0.469***	-0.00823	-0.00873
	(0.0926)	(0.0892)	(0.158)	(0.155)	(0.0387)	(0.0392)
HRV*BAN state		-0.529***		0.433***		0.0177
		(0.0224)		(0.0187)		(0.0226)
R-squared	0.798	0.798	0.757	0.757	0.860	0.860
Observations	3,138	3,138	3,138	3,138	3,138	3,138
Panel B: HRV exposure = 1 month, 20 km						
HRV in planting season	0.489***	0.381**	-0.204	-0.247	0.0312	0.00738
	(0.1490)	(0.1850)	(0.2850)	(0.3410)	(0.0451)	(0.0600)
HRV*BAN state		0.362		0.145		0.0803
		(0.2950)		(0.4040)		(0.1060)
R-squared	0.798	0.798	0.758	0.758	0.860	0.860
Observations	3,138	3,138	3,138	3,138	3,138	3,138
Panel C: HRV exposure = 1 month, 30 km						
HRV in planting season	0.158	0.156	-0.136	-0.315	0.00412	-0.0156
	(0.150)	(0.191)	(0.207)	(0.198)	(0.0582)	(0.0645)
HRV*BAN state		0.00761		0.895***		0.0983
		(0.340)		(0.294)		(0.143)
R-squared	0.797	0.797	0.761	0.762	0.860	0.860
Observations	3,138	3,138	3,138	3,138	3,138	3,138
Panel D: Donut specification						
HRV in planting season	0.484***	0.499***	-0.459***	-0.471***	-0.0147	-0.0152
	(0.0936)	(0.0901)	(0.159)	(0.155)	(0.0404)	(0.0409)
HRV*BAN state		-0.528***		0.434***		0.0196
		(0.0237)		(0.0186)		(0.0253)
R-squared	0.798	0.798	0.756	0.756	0.860	0.860
Observations	3,130	3,130	3,130	3,130	3,130	3,130

*** p<0.01, ** p<0.05, * p<0.1. Individual-level panel data, over four post-harvest rounds of the GHS. HRV: herder-related violent event in the planting season. Borno state is excluded. IHS: Inverse hyperbolic sine. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance window, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include individual-level, year-month (time), and EA-level fixed effects. X' is a vector of controls, each interacted with a non-parametric time trend and includes a confrol exposure to a violent event in the 2 years prior to the 2018 planting season and additioanl controls, including dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

Table 6: Use of Harvest (IHS Kilograms) in Response to HRV Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total harvested		Future use		HH consumption		Paid or given		Losses		Sold	
Panel A: HRV exposure = 1 month, 10 km												
HRV in planting season	1.570*	1.842*	0.200	0.376	-0.637	-0.291	-0.490	-0.395	-0.0816	-0.0716	-0.0243	0.0213
	(0.830)	(1.078)	(0.241)	(0.279)	(0.392)	(0.290)	(0.341)	(0.399)	(0.0630)	(0.0756)	(0.443)	(0.570)
HRV*Ban state		-1.016		-0.656**		-1.291*		-0.355		-0.0373		-0.170
		(1.240)		(0.331)		(0.715)		(0.623)		(0.113)		(0.775)
R-squared	0.262	0.262	0.645	0.645	0.611	0.612	0.328	0.328	0.115	0.115	0.526	0.526
Observations	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576
Panel B: HRV exposure = 1 month, 20 km												
HRV in planting season	0.694	0.773	0.355*	0.519**	-0.658***	-0.526**	0.0314	0.0426	-0.0679	-0.0693	0.250	0.230
	(0.480)	(0.561)	(0.189)	(0.201)	(0.248)	(0.248)	(0.328)	(0.375)	(0.0422)	(0.0449)	(0.388)	(0.453)
HRV*Ban state		-0.464		-0.965***		-0.775		-0.0654		0.00824		0.117
		(0.933)		(0.304)		(0.677)		(0.617)		(0.114)		(0.739)
R-squared	0.259	0.259	0.644	0.645	0.612	0.612	0.328	0.328	0.116	0.116	0.524	0.524
Observations	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576
Panel C: HRV exposure = 1 month, 30 km												
HRV in planting season	0.384	0.251	0.160	-0.0483	-0.516***	-0.299	-0.206	0.107	-0.154***	-0.0454	0.0934	0.103
	(0.336)	(0.397)	(0.172)	(0.201)	(0.184)	(0.182)	(0.230)	(0.255)	(0.0570)	(0.0407)	(0.330)	(0.394)
HRV*Ban state		0.484		0.756**		-0.788		-1.137**		-0.395***		-0.0335
		(0.741)		(0.297)		(0.564)		(0.558)		(0.134)		(0.500)
R-squared	0.252	0.252	0.644	0.645	0.613	0.614	0.328	0.332	0.116	0.118	0.524	0.524
Observations	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576	1,576
Panel D: Donut specification												
HRV in planting season	1.606*	1.881*	0.244	0.440	-0.622	-0.306	-0.332	-0.330	-0.0731	-0.0621	0.0519	0.0796
	(0.868)	(1.111)	(0.243)	(0.269)	(0.387)	(0.329)	(0.350)	(0.416)	(0.0675)	(0.0775)	(0.449)	(0.578)
HRV*Ban state		-1.053		-0.749**		-1.208		-0.00787		-0.0423		-0.106
		(1.334)		(0.336)		(0.741)		(0.616)		(0.129)		(0.794)
R-squared	0.271	0.271	0.637	0.637	0.595	0.596	0.320	0.320	0.119	0.119	0.525	0.525
Observations	1,352	1,352	1,352	1,352	1,352	1,352	1,352	1,352	1,352	1,352	1,352	1,352
Adjust for total KGs harvested?	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Household-level data from the 2019 post-harvest GHS data. HRV: herder-related violent event in the planting season. Borno state is excluded. IHS: inverse hyperbolic sine. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance window, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include year-month (time) and state-level fixed effects, as well as controls for exposure to a violent event in the 2 years prior to the 2018 planting season. An additional control is included if a household is exposed to a violent event in the harvest season. X' is included in all columns and consists of a vector of dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

Table 7: Agricultural Marketing in Response to HRV Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sold crops		Unprocessed sold		Processed sold		Share unprocessed	
Panel A: HRV exposure = 1 month, 10 km								
HRV in planting season	-0.116***	-0.145***	0.526	1.130***	0.210	-0.282	0.0305	0.103**
	(0.0304)	(0.0384)	(0.465)	(0.325)	(0.587)	(0.665)	(0.0567)	(0.0437)
HRV*Ban state		0.106		-1.979***		1.612**		-0.237***
		(0.0823)		(0.529)		(0.770)		(0.0606)
R-squared	0.451	0.451	0.399	0.403	0.176	0.178	0.167	0.171
Observations	1,576	1,576	904	904	904	904	896	896
Panel B: HRV exposure = 1 month, 20 km								
HRV in planting season	-0.0191	-0.0330	0.325	0.524	0.0990	-0.109	0.0182	0.0466
	(0.0471)	(0.0548)	(0.370)	(0.412)	(0.371)	(0.414)	(0.0418)	(0.0454)
HRV*Ban state		0.0815		-1.002		1.048*		-0.143**
		(0.106)		(0.663)		(0.626)		(0.0706)
R-squared	0.449	0.449	0.399	0.400	0.172	0.174	0.166	0.168
Observations	1,576	1,576	904	904	904	904	896	896
Panel C: HRV exposure = 1 month, 30 km								
HRV in planting season	-0.0179	-0.0351	0.448*	0.570*	-0.395	-0.312	0.0689**	0.0696*
	(0.0405)	(0.0469)	(0.231)	(0.309)	(0.313)	(0.290)	(0.0338)	(0.0359)
HRV*Ban state		0.0627		-0.362		-0.243		-0.00218
		(0.0603)		(0.581)		(0.721)		(0.0782)
R-squared	0.451	0.451	0.402	0.402	0.176	0.176	0.171	0.171
Observations	1,576	1,576	904	904	904	904	896	896
Panel D: Donut specification								
HRV in planting season	-0.0986**	-0.142**	0.00699	0.927***	1.449***	1.784**	-0.0741	-0.000491
	(0.0466)	(0.0580)	(0.557)	(0.298)	(0.418)	(0.709)	(0.0493)	(0.0512)
HRV*Ban state		0.0963		-1.596***		-0.581		-0.128*
		(0.0936)		(0.521)		(0.816)		(0.0668)
R-squared	0.468	0.468	0.391	0.393	0.183	0.183	0.161	0.162
Observations	1,220	1,220	706	706	706	706	702	702

*** p<0.01, ** p<0.05, * p<0.1. Household-level data from the 2019 post-harvest GHS data. HRV: herder-related violent event in the planting season. Borno state is excluded. IHS: inverse hyperbolic sine. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance window, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include year-month (time) and state-level fixed effects, as well as controls for exposure to a violent event in the 2 years prior to the 2018 planting season. An additional control is included if a household is exposed to a violent event in the harvest season. X' is included in all columns and consists of a vector of dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively. All columns also control for the total number of KGs harvested (IHS).

Table 8: Non-farm Enterpise Sales in Response to HRV Exposure

	(1)	(2)	(3)	(4)
	Any NFE		NFE sales	
Panel A: HRV exposure = 1 month, 10 km				
HRV in planting season	0.0400	-0.00464	0.122	0.0575
	(0.0412)	(0.0401)	(0.182)	(0.206)
HRV*Ban state		0.217**		0.354
		(0.0960)		(0.371)
R-squared	0.089	0.093	0.097	0.097
Observations	1,978	1,978	948	948
Panel B: HRV exposure = 1 month, 20 km				
HRV in planting season	0.00321	-0.0170	-0.0486	-0.132
	(0.0260)	(0.0238)	(0.169)	(0.181)
HRV*Ban state		0.128*		0.855**
		(0.0690)		(0.349)
R-squared	0.086	0.088	0.097	0.099
Observations	1,978	1,978	948	948
Panel C: HRV exposure = 1 month, 30 km				
HRV in planting season	-0.00378	-0.0118	0.102	0.111
	(0.0174)	(0.0176)	(0.152)	(0.175)
HRV*Ban state		0.0320		-0.0601
		(0.0448)		(0.296)
R-squared	0.088	0.088	0.107	0.107
Observations	1,978	1,978	948	948
Panel D: Donut specification				
HRV in planting season	0.0907**	0.0312**	-0.0215	-0.188
	(0.0378)	(0.0152)	(0.309)	(0.348)
HRV*Ban state		0.187**		0.834
		(0.0838)		(0.584)
R-squared	0.092	0.094	0.107	0.108
Observations	1,547	1,547	752	752

*** p<0.01, ** p<0.05, * p<0.1. Household-level data from the 2019 post-harvest GHS data. HRV: herder-related violent event in the planting season. Borno state is excluded. IHS: inverse hyperbolic sine. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance window, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include year-month (time) and state-level fixed effects, as well as controls for exposure to a violent event in the 2 years prior to the 2018 planting season. An additional control is included if a household is exposed to a violent event in the harvest season. X' is included in all columns and consists of a vector of dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

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A1 Data Appendix

A1.1 General Household Survey (GHS), Nigeria Bureau of Statistics

The following discusses the main outcome variables we use from the GHS data.

Own-account work or in a household-enterprise—A binary measure taking the value of 1 if the answer is "yes" to the following:

> During the past 7 days, has [NAME] worked on their own account or in a business enterprise belonging to [NAME] or another household member, for example, as a trader, shop-keeper, barber, dressmaker, carpenter or taxi driver?

In the 2015–16 and 2018–19 GHS rounds, information on the number of hours in this type of work was also recorded:

> During the past 7 days, how many hours has [NAME] worked in the household nonfarm enterprise?

Agricultural work—A binary measure taking the value of 1 if the answer is "yes" to the following:

> During the past 7 days, has [NAME] worked on a farm owned or rented by [NAME] or another member of your household, either in cultivating crops or in other farming tasks, or has [NAME] cared for livestock belonging to [NAME] or another member of your household?

In the 2015–16 and 2018–19 GHS rounds, information on the number of hours in this type of work was also recorded:

> During the past 7 days, how many hours has [NAME] done this agricultural work for the household?

Work outside the household—A binary measure taking the value of 1 if the answer is "yes" to the following:

> During the past 7 days, has [NAME] worked for someone who is not a member of your household, for example, an enterprise, company, the government or any other individual for payment in cash or in-kind?

In the 2015–16 and 2018–19 GHS rounds, information on the number of hours in this type of work was also recorded:

> During the past 7 days, for how many hours in total has [NAME] worked for payment?

A1.2 Conflict Event Data, ACLED Project

The following discusses how we use the data from the ACLED project.

Definition of a Violent Event—The ACLED Project includes coded definitions of event types and sub-types. Values for violent `event type` include:

- Violence against civilians
- Explosions/remote violence
- Battles

Violent `sub event types` values include:

- Abduction/forced disappearance
- Attack
- Looting/property destruction
- Sexual violence
- Armed clash
- Excessive force against protesters
- Mob violence
- Remote explosive/landmine/IED
- Violent demonstration

Definition of Herder-Involved Conflict—All events including the terms "herder," "herdsmen," or "pastoralist" (or variants of those words, e.g., "herder") as a direct or associated actor were coded as herder-involved; as were occurrence of those terms in the detailed event descriptions. Similarly, if the term "Fulani" was included, the event was coded as herder-involved. Specific areas with the term "Fulani," such as the village "Birim Fulani" were excluded if they did not include a term such as "pastoralist." Positively coded events were reviewed for wording such as "likely not Fulani." Such cases were re-coded as not herder involved.

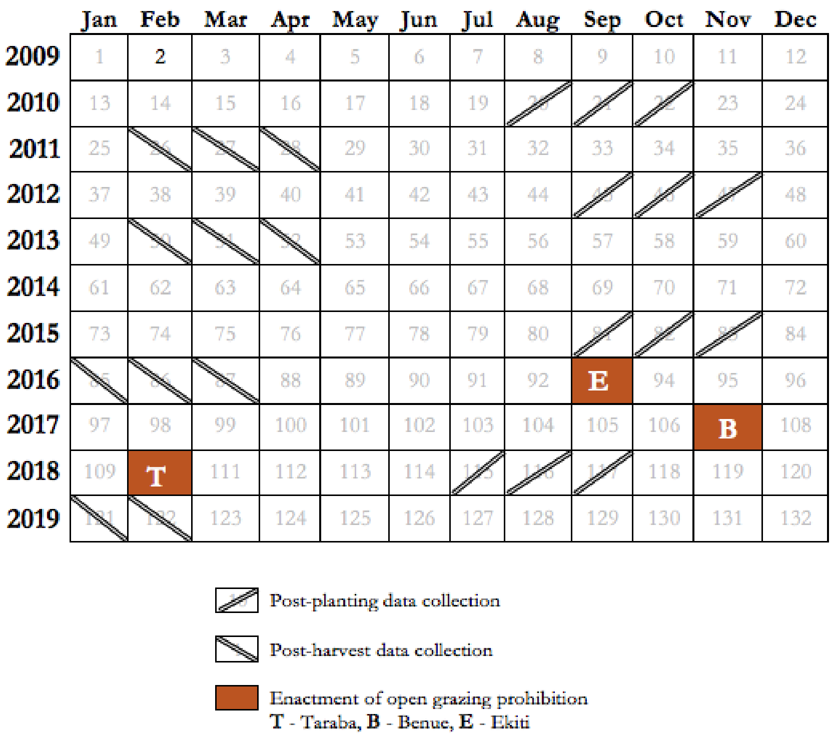
A1.3 Data Combination

Often, the exact location of an event is unknown or imprecise. ACLED's database notes these cases and includes a variable `geo precision`. Events take a value of 1 for *geo precision* if a specific town or locale is noted. In these cases, the corresponding latitude and longitude coordinates for the town/locale are used. In less precise cases, where a part of a region or sub-region is indicated, a representative town/locale is used, and these cases take a value of two for `geo precision`. Cases with a value of three for `geo precision` are those that are reported in a larger area; the GPS coordinates for those cases are those of the closest identified landmark, such as a town, border crossing, or geographic point of interest, such as a lake or road.

Of the herder-involved incidents coded in the ACLED data, 899 (50.4 percent) take a value of one for `geo precision`; 845 (47.3 percent) are coded two; and 41 (2.3 percent) are coded as three.

The Figure A1 below shows the timing of the GHS collection periods and their alignment to the ACLED data and key dates. Each square represents a month in a given year. Squares are numbered sequentially for illustration and begin in January 2009 (1) and end in December 2019 (134). The GHS panel was conducted in four rounds: 2010–11, 2012–13, 2015–16, and 2018–19. Each round consisted of two visits, one in the post-planting period, a second in the post-harvest period. The legend below the figure indicates which data collection periods aligned to the post-planting (dry season) and post-harvest (rainy season) periods.

Figure A1: **GHS Data Collection Timing**



Working with the World Bank's LSMS team, we were able to receive matched GHS data, using unmasked (and precise) GPS coordinates. For confidentiality reasons, we were not granted unrestricted access to the unmasked data and received, at our request, matched data for distance windows of 10, 20, 30, and 50 km. Time windows were available for 1 and 3 months, with 3 months.

Our team is indebted to the LSMS team for this support.

A1.4 Descriptive Statistics of Co-variates

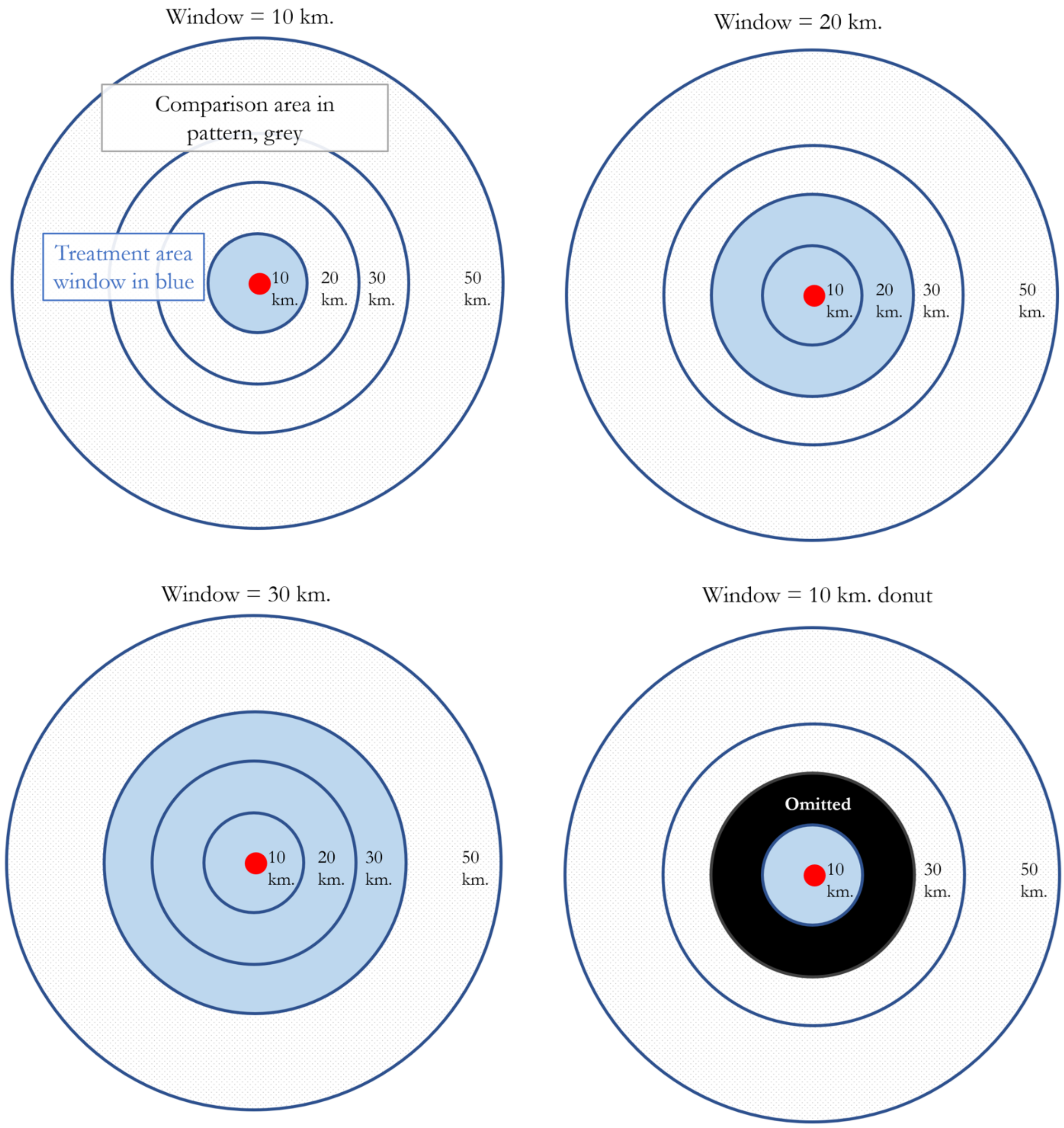
Table A1: Descriptive Statistics

(A) Sample	Mean	SD	Min.	Max.	n
Within 10 km. of state admin. center	0.13	0.34	0	1	70,365
Within 10 km. of a market	0.10	0.30	0	1	70,365
Within 10 km. of a 20k population center	0.25	0.43	0	1	70,365
(B) HRV within 50 km.	Mean	SD	Min.	Max.	n
Within 10 km. of state admin. center	0.13	0.34	0	1	26,691
Within 10 km. of a market	0.09	0.29	0	1	26,691
Within 10 km. of a 20k population center	0.21	0.41	0	1	26,691
(C) HRV within 50 km., Ban States	Mean	SD	Min.	Max.	n
Within 10 km. of state admin. center	0.12	0.33	0	1	2,559
Within 10 km. of a market	0.00	0.00	0	0	2,559
Within 10 km. of a 20k population center	0.13	0.34	0	1	2,559
(D) HRV within 50 km., Other States	Mean	SD	Min.	Max.	n
Within 10 km. of state admin. center	0.13	0.34	0	1	24,132
Within 10 km. of a market	0.10	0.30	0	1	24,132
Within 10 km. of a 20k population center	0.22	0.41	0	1	24,132
(E) HRV within 50 km., Post-planting Season	Mean	SD	Min.	Max.	n
Within 10 km. of state admin. center	0.14	0.35	0	1	11,718
Within 10 km. of a market	0.10	0.29	0	1	11,718
Within 10 km. of a 20k population center	0.22	0.41	0	1	11,718
(F) HRV within 50 km., Post-harvest Season	Mean	SD	Min.	Max.	n
Within 10 km. of state admin. center	0.12	0.33	0	1	13,706
Within 10 km. of a market	0.09	0.28	0	1	13,706
Within 10 km. of a 20k population center	0.20	0.40	0	1	13,706

Source: Authors' tabulations of individual-level GHS data. All panels include observations that are observed in the 2018-19 GHS round and also at least one previous round. Borno state is excluded.

A1.5 Illustration of HRV Exposure

Figure A2: **Illustration of Varying Distance Windows**



A2 Additional Tables and Figures

The following tables and figures provide additional results to supplement the discussion in the main manuscript.

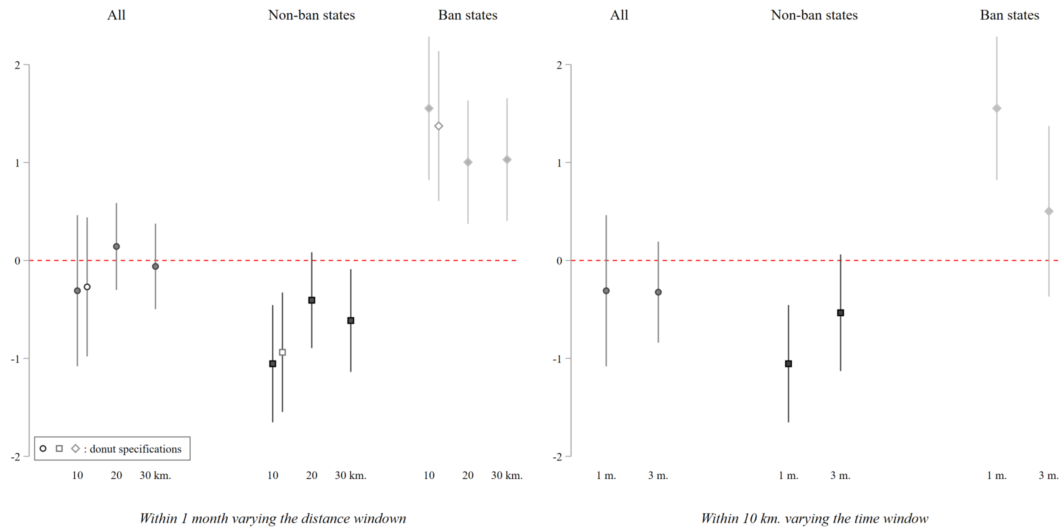
- Table A2 shows results similar to Table 2 but uses a three month window to define exposure to herder-related violence.
- Figure A3 through Figure A5 present coefficient plots using a continuous measure of hours worked of effect estimates in the contemporaneous planting season.
- Figure A6 through Figure A8 present coefficient plots using a binary measure of work of effect estimates in the subsequent harvest season.
- Figure A9 through Figure A11 present coefficient plots using a continuous measure of hours worked of effect estimates in the subsequent harvest season.

Table A2: Planting Season Labor Response to HRV Exposure, 3 Month Time Window

	(1)	(2)	(3)	(4)	(5)	(6)
	Own-account HH-enterprise		Agricultural work		Work outside the home	
Panel A: HRV exposure = 3 month, 10 km						
HRV in planting season	-0.0415	-0.0892	0.0260	0.0503	0.00741	0.00337
	(0.0496)	(0.0649)	(0.0663)	(0.0881)	(0.0136)	(0.0119)
HRV*Ban state		0.113		-0.0574		0.00936
		(0.0846)		(0.127)		(0.0217)
R-squared	0.671	0.671	0.674	0.674	0.696	0.696
Observations	10,338	10,338	10,349	10,349	10,339	10,339
Panel B: HRV exposure = 3 month, 20 km						
HRV in planting season	0.017	-0.0149	0.0111	0.0494	0.0116	0.00418
	(0.0354)	(0.0449)	(0.0407)	(0.0496)	(0.0139)	(0.0128)
HRV*Ban state		0.0773		-0.093		0.0185
		(0.0623)		(0.0724)		(0.0256)
R-squared	0.671	0.671	0.675	0.675	0.696	0.696
Observations	10,338	10,338	10,349	10,349	10,339	10,339
Panel C: HRV exposure = 3 month, 30 km						
HRV in planting season	0.000570	-0.0174	0.0125	0.0444	0.0243*	0.0217*
	(0.0292)	(0.0319)	(0.0368)	(0.0421)	(0.0127)	(0.0123)
HRV*Ban state		0.0623		-0.112*		0.0101
		(0.0494)		(0.0565)		(0.0188)
R-squared	0.672	0.672	0.675	0.676	0.696	0.696
Observations	10,338	10,338	10,349	10,349	10,339	10,339
Panel D: Donut specification						
HRV in planting season	-0.0420	-0.0868	0.0170	0.0492	0.00863	0.00234
	(0.0488)	(0.0634)	(0.0672)	(0.0883)	(0.0154)	(0.0131)
HRV*Ban state		0.111		-0.0796		0.0151
		(0.0816)		(0.122)		(0.0243)
R-squared	0.668	0.668	0.670	0.670	0.700	0.700
Observations	9,461	9,461	9,472	9,472	9,462	9,462

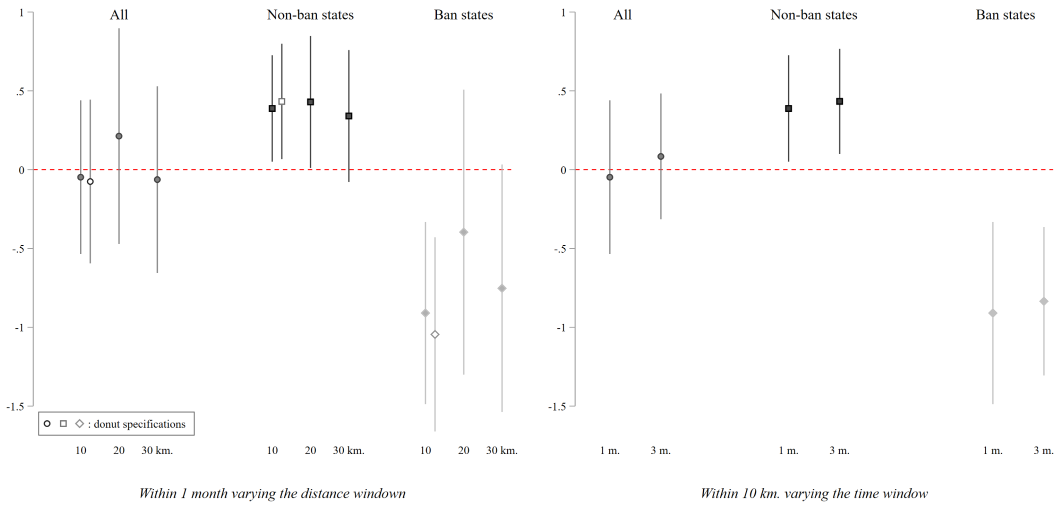
*** p<0.01, ** p<0.05, * p<0.1. Individual-level panel data, over four post-planting rounds of the GHS. HRV: herder-related violent event in the planting season. Borno state is excluded. Robust standard errors allowing for clustering at the level of enumeration area (EA). Observations include all households within 50 km. of an HRV; the treatment group of households are within the given distance and time windows, and the comparison households are outside of the distance window (but still within 50km.). For the 'donut' specification, treatment is exposure within a 10 km. radius, while the comparison group is 21 to 50 km. (those within 11 to 20 km. are omitted). All columns include individual-level, year-month (time), and EA-level fixed effects. X' is a vector of controls, each interacted with a non-parametric time trend and includes a control exposure to a violent event in the 2 years prior to the 2018 planting season and additional controls, including dummy variables (=1) if a household is located within 10 km. of an administrative center, of a market, and a population center with 20,000 inhabitants, respectively.

Figure A3: **IHS own-account hours worked in the last week (planting season)**



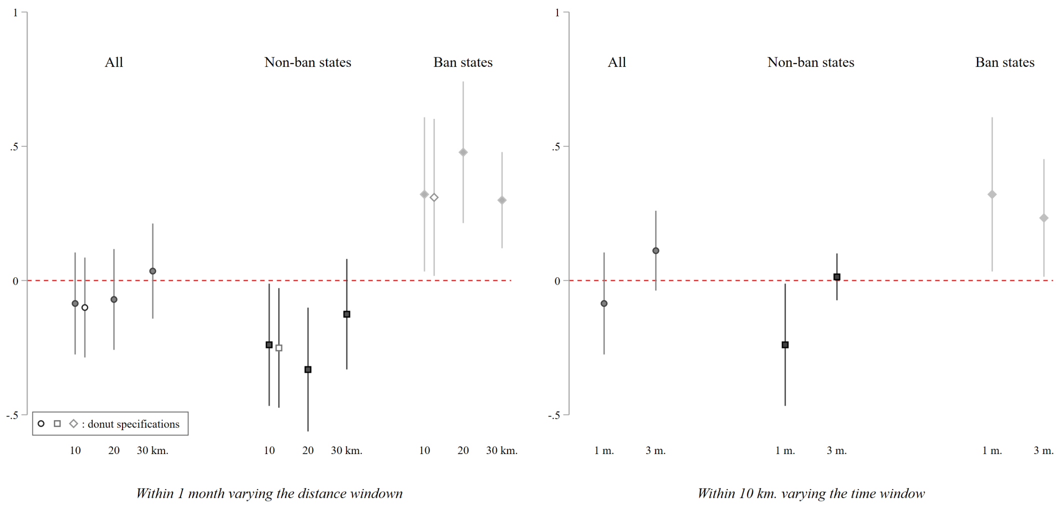
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals in the left panel correspond to estimates in Table 3. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (1). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (2); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The left panel varies the distance window as shown in the four panels (A–D) of the table. Similarly, the right panel shows the main specifications for the 10-km. and 1-month windows (Panel A), but it also includes coefficients from an additional specification where the time window is extended to three months (results not shown in the table).

Figure A4: **IHS agricultural hours worked in the last week (planting season)**



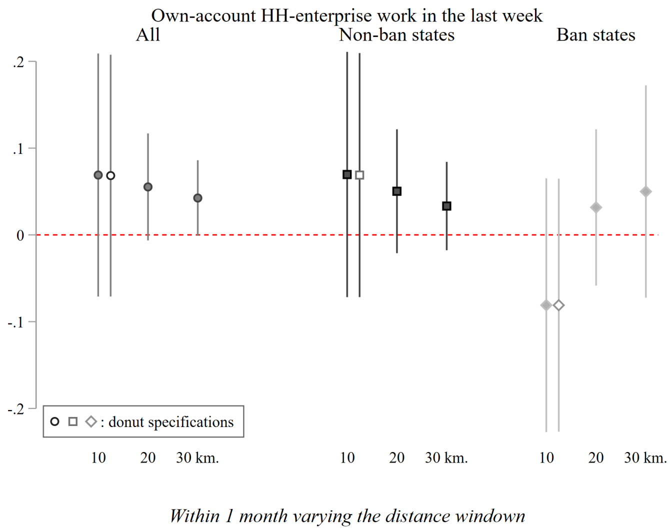
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals in the left panel correspond to estimates in Table 3. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (3). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (4); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The left panel varies the distance window as shown in the four panels (A–D) of the table. Similarly, the right panel shows the main specifications for the 10-km. and 1-month windows (Panel A), but it also includes coefficients from an additional specification where the time window is extended to three months (results not shown in the table).

Figure A5: **IHS outside work hours in the last week (planting season)**



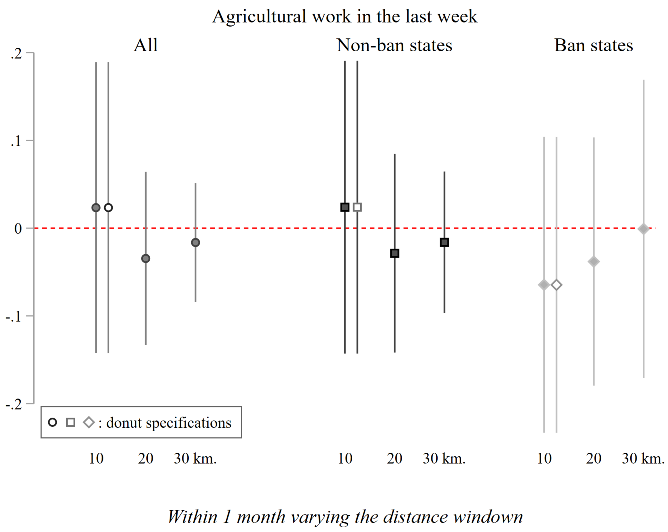
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals in the left panel correspond to estimates in Table 3. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (5). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (6); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The left panel varies the distance window as shown in the four panels (A–D) of the table. Similarly, the right panel shows the main specifications for the 10-km. and 1-month windows (Panel A), but it also includes coefficients from an additional specification where the time window is extended to three months (results not shown in the table).

Figure A6: **Own-account HH-enterprise work in the last week (harvest season)**



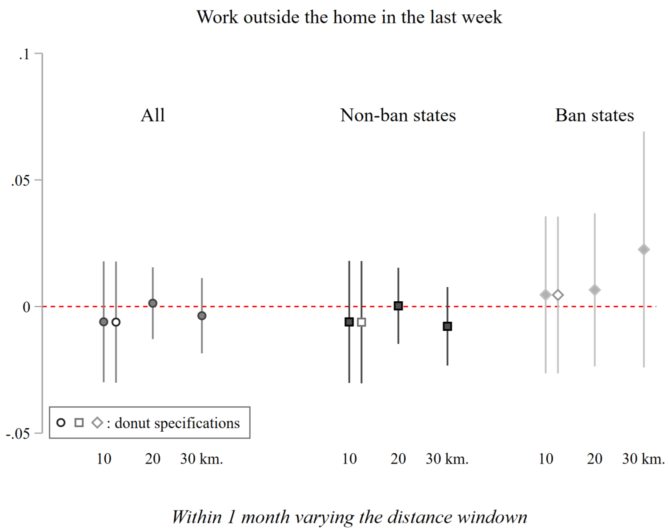
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals correspond to estimates in Table 4. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (1). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (2); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The figure varies the distance window as shown in the four panels (A–D) of the table.

Figure A7: **Agricultural work in the last week (harvest season)**



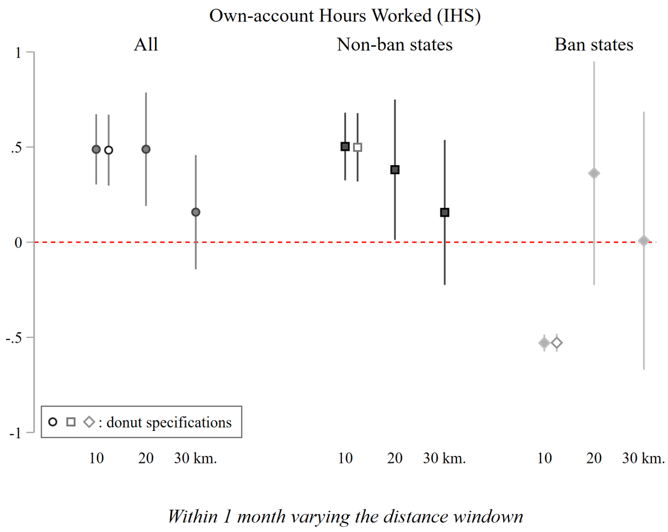
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals correspond to estimates in Table 4. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (3). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (4); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The figure varies the distance window as shown in the four panels (A–D) of the table.

Figure A8: **Work outside the home in the last week (harvest season)**



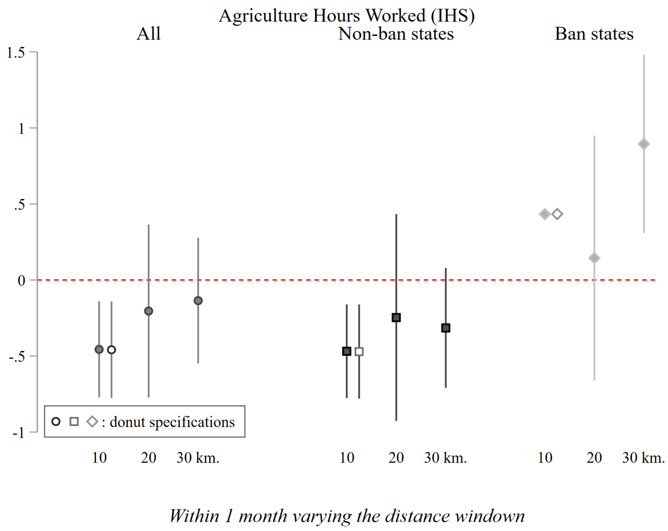
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals correspond to estimates in Table 4. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (5). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (6); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The figure varies the distance window as shown in the four panels (A–D) of the table.

Figure A9: **IHS own-account hours worked in the last week (harvest season)**



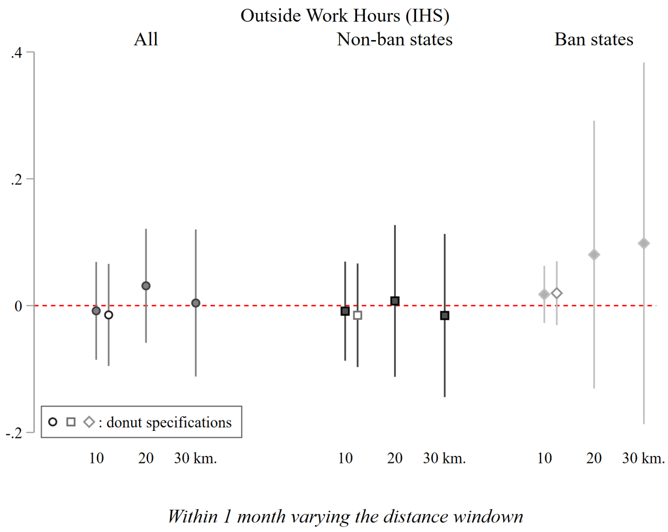
Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals correspond to estimates in Table 5. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (1). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (2); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The figure varies the distance window as shown in the four panels (A–D) of the table.

Figure A10: **IHS agricultural hours worked in the last week (harvest season)**



Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals correspond to estimates in Table 5. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (3). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (4); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The figure varies the distance window as shown in the four panels (A–D) of the table.

Figure A11: **IHS hours worked outside the home in the last week (harvest season)**



Notes: HRV - herder-related violent event. The coefficient point estimates and 95% confidence intervals correspond to estimates in Table 5. Three groupings are shown. "All" states corresponds to the coefficient for "HRV in planting season" in Column (5). The groupings for "Non-ban states" and "Ban states" are from the triple-difference specification in Column (6); they correspond to the coefficients shown in "HRV in planting season" and "HRV *Ban state", respectively. The figure varies the distance window as shown in the four panels (A–D) of the table.