THREE ESSAYS IN DEVELOPMENT ECONOMICS

by

PRACHI JHAMB

(Under the Direction of Susana Ferreira)

ABSTRACT

This dissertation examines how environmental shocks influence household behavior and health in data-poor settings across the Global South. The first essay evaluates the validity of a novel harmonized nightlights dataset as a proxy for local economic activity across 34 sub-Saharan African countries. The second essay uses quarterly household data, combined with high-resolution climate indices, to identify the causal effect of droughts and floods on rural coping strategies in 24 countries across Africa, Asia and Latin America. The third essay investigates whether deforestation driven by large-scale land acquisitions affects child health, integrating geocoded DHS surveys with annual forest loss data across nine sub-Saharan African countries and applying the extended two way fixed effects estimator to address staggered treatment timing.

INDEX WORDS: Harmonized Nightlights, Demographic and Health Surveys (DHS), Bushmeat hunting, Rainfall shocks, SPEI index, Environmental risk, Deforestation, Large-Scale Land Acquisitions (LSLAs), Malaria, Africa

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CHAPTER 1

INTRODUCTION

Across much of the Global South, households face recurrent environmental shocks that threaten their livelihoods and health. These shocks may influence how people work, earn, and survive. Yet our understanding of how environmental shocks influence household behavior and health remains limited, particularly in data poor contexts where reliable information is scarce and causal identification is difficult.

This dissertation examines how environmental shocks influence household outcomes and adaptation strategies in developing countries. A critical step in studying these impacts is the ability to measure socioeconomic outcomes at fine spatial scales. In many low-income countries, however, reliable subnational and traditional sources of data (such as household surveys) are missing. Without accurate measures of local living standards, it is difficult to assess vulnerability to environmental shocks or to design targeted policies.

To address this gap, Chapter 2, evaluates the validity of a novel harmonized nightlights (NTL) dataset (developed by bridging the older DMSP and newer VIIRS satellite sensors) as a proxy for local economic activity and human development across 34 sub-Saharan African countries. We examine how well NTL explain variation in living standards beyond what can be attributed to population density. The findings reveal that NTL are a strong predictor of household wealth, explaining nearly half of its variation at subnational level, even after controlling for population density. Importantly, nightlights perform better in urban areas but still have modest explanatory power in rural contexts.

While Chapter 2 focuses on filling measurement gaps, in Chapter 3 and 4, we shift our attention to causal identification in the context of environmental shocks.

In Chapter 3, we ask: How do rural households (in the Global South) cope with environmental shocks when their livelihoods are disrupted? The question is particularly important in low income regions (such as in our setting) where formal insurance and credit markets are limited, forcing households to rely on informal coping mechanisms. Under this broad question, the chapter investigates several specific aspects: i) Do rural households turn to bushmeat hunting as a coping strategy when rainfall shocks disrupt agriculture?; ii) Do they adapt differently to droughts versus floods?; iii) Does the intensity or timing of a shock, shape their response?; and iv) Do differences in income or access to resources amplify inequality in adaptation? We construct a panel dataset using the Poverty and Environment Network (PEN) survey, covering nearly 8000 households covering 24 countries, and combine it with high-resolution climate index (SPEI) to provide causal evidence on how rainfall variability affects income from, and reliance on, bushmeat hunting.

Causal identification is a central contribution in this chapter. Earlier studies have relied on either self-reported shocks or cross-sectional data, making it difficult to isolate the true effects from unobserved heterogeneity. By exploiting within household variation over time and using an objective measure of rainfall shocks, we isolate the short-term causal effect of droughts and floods on household coping behavior.

The findings show that droughts sharply increase income from, and reliance on, bushmeat hunting, especially during the growing season, while wet conditions have the opposite effect. The results suggest that households reallocate labor across productive activities in response to environmental stress, consistent with agricultural household models under incomplete markets. We also find that wealthier households are better able to scale up hunting during droughts, highlighting unequal adaptive capacity across income groups.

Chapter 4 also focuses on identifying causal effects of environmental shocks, but through a different angle. Specifically, the chapter studies whether forest loss induced by large scale land acquisitions (LSLAs) has unintended consequences for household health across nine sub-Saharan African countries. Although LSLAs are promoted as tools for agricultural modernization and rural development, they can generate negative externalities, such as ecological changes that influence the spatial distribution of diseases such as malaria.

Since LSLAs roll out in a staggered manner across space and time, meaning different localities are treated in different years, and many locations are already treated when others begin treatment, conventional two-way fixed effects estimator can provide misleading insights (Goodman-Bacon, 2021). To address this concern, we apply Wooldridge's extended two way fixed effects (ETWFE) estimator (Wooldridge, 2021) which fully saturates the model with cohort and calendar year interactions and avoids forbidden comparisons with already treated units.

We combine geocoded child health data from the DHS with annual deforestation data from Hansen et al. 2013 and detailed spatial and temporal information on LSLAs from the Land Matrix Initiative in a multi-period spatial difference-in-differences framework. Specifically, we compare changes in malaria incidence among children (under age five) living within 10 kms of an LSLA to those living 20 to 90 kms away. Results show that proximity to an LSLA significantly increases the probability of a child experiencing malaria. Further analysis indicates that these effects are driven by deforestation rather than other alternative channels such as migration or economic prosperity.

Across chapters, this dissertation integrates remote sensing data (nightlights, rainfall indices and deforestation maps) with household survey data (DHS, PEN) in data poor-settings of the Global South, to generate new evidence on how environmental shocks shape household behavior and outcomes.

CHAPTER 2

SHEDDING LIGHT ON DEVELOPMENT:LEVERAGING THE NEW NIGHTLIGHTS DATA TO MEASURE ECONOMIC PROGRESS

2.1. Introduction

National-level indicators of economic and social progress may provide a bird's eye view of a country's performance, but they often mask the heterogeneity that exists within countries. Subnational variation in resource endowments and development outcomes can have a significant impact on people's well-being, but their analysis is frequently overlooked due to the lack of reliable disaggregated administrative data (Avendano et al. 2018).

The need for subnational data in developing countries is particularly critical in the context of infectious disease spillover and climate change. The spatial heterogeneity in socio-economic conditions, disease transmission patterns, and healthcare infrastructure within a country can have a significant impact on the effectiveness of public health response measures to control disease outbreaks (Anbarci et al. 2012; Mee et al. 2022). Unfortunately, such data are frequently inaccessible and available only in countries that undertake comprehensive census surveys (Noy et al. 2019). Similarly, in the context of climate change, whose impacts can be highly localized and vary across regions within the same country due to factors such as topography, climate variability, or land use, subnational data can help in identifying areas that are particularly vulnerable to climate impacts and inform resilience strategies (Hallegatte et al. 2017). Without subnational data, researchers in the past have relied on national level development indicators which is problematic since development indicators can conceal significant intra-national variation, particularly in large countries (Kummu et al. 2018).

The average interval between nationally representative economic surveys in half African nations is above 6.5 years, compared to a sub-annual frequency in most wealthy countries (Burke et al. 2021). Household surveys, such as those from the Demographic and Health Surveys (DHS) program have limited repeated observations of the same location (and even the same country) over time (Weidmann and Theunissen et al. 2021; World Bank, 2021). It is estimated that a given African household would appear in a household survey once in 1,000 years, making it difficult to measure changes in well-being over time at a local level (Yeh et al. 2020). For this reason, researchers have turned to alternative and non-traditional sources of data such as nightlights (NTL), daytime satellite imagery or mobile phone call detail records (Blumenstock et al. 2015; Blumenstock, 2016; Steele et al. 2017; Steele et al. 2021).

NTL have been used as a proxy for economic activity in various studies based on the assumption that the amount of light at night is closely tied to the level of economic activity (Chen and Nordhaus, 2011; Henderson et al. 2012; Keola et al. 2015; Hodler et al. 2014). The reliance on NTL intensity extends beyond measuring economic activity, with researchers employing it in various applications such as studying inequality and assessing the accuracy of official statistics in different political contexts (Bundervoet et al. 2015). However, concerns about the reliability of NTL data have emerged, particularly due to the extensive use of outdated and inaccurate data from the now-discontinued Defense Meteorological Satellite Program (DMSP), which ceased production in 2013 (Gibson, 2021).

DMSP data suffer from several limitations, including blurred images, geo-location errors, and top-coding, which result in misattributed light sources and an inability to distinguish between areas of low and high light intensity. These issues undermine the accuracy of analyses, as DMSP data often aggregate light intensities, masking important details (Gibson et al. 2020). This has highlighted the need for a transition to newer and more precise data sources, such as the Visible Infrared Imaging Radiometer Suite (VIIRS).

VIIRS data, available since 2012, offers substantial improvements over DMSP, including 45 times higher spatial resolution and the elimination of blurring and geo-location errors, making it a more reliable alternative for economic analyses (Elvidge et al. 2013; Gibson, 2021). Despite these advantages, the adoption of VIIRS in the economics literature has been limited. Two primary factors contribute to this: Firstly, DMSP data still offers a longer time series spanning from 1992 to 2013, which is advantageous for time series analysis. Secondly, comparing DMSP and VIIRS data directly presents challenges due to differences in their temporal coverage and spatial resolutions (Yong et al. 2022). While DMSP data is available annually, ending in 2013, VIIRS offers annual data only for 2015 and 2016. Monthly VIIRS data is available from 2012 onwards, but comparing monthly VIIRS data with annual DMSP data introduces potential inaccuracies due to differing data processing methodologies (Yong et al. 2022).

To address these challenges, this paper utilizes a novel harmonized NTL dataset developed to bridge the gap between DMSP and VIIRS datasets (Li et al. 2020). This dataset creates a continuous and consistent NTL time series from 1992 to 2021 with high spatial resolution (Appendix A), enabling direct comparisons across global regions.

Our first contribution lies in testing the accuracy of the harmonized NTL dataset in measuring economic activity at a small spatial scale for countries in sub-Saharan Africa. Through this, we aim to provide insights into the effectiveness of the harmonized NTL dataset for economic analyses at fine spatial resolutions in developing regions. Previously, researchers tested the dataset's accuracy for predicting regional GDP in 2012 for Colombia (Pérez-Sindín et al. 2021). We build on this work by focusing on subnational socio-economic activity in sub-Saharan Africa. We rely on household surveys conducted by the DHS as our primary source of information on subnational socio-economic activity. These surveys assess living conditions and household possessions and are available for multiple years and countries. It is worth noting that most previous studies evaluating the accuracy of satellite data compare them against the wealth index derived from DHS data (Yeh et al. 2020; Blumenstock et al. 2015; Jean et al. 2016; Bruederle and Hodler, 2018).

Our second contribution is that we control for population density. Criticisms of using NTL as a proxy for economic activity highlight the likelihood of high correlation between the density of light and population density (Weidmann and Theunissen, 2021). However, few studies so far have evaluated the extent to which NTL vary independently of population density.

Our third contribution is an exploration into whether NTL can also serve as a proxy for human development outcomes, an area that has been largely underexplored in existing literature. While there are notable exceptions, such as (Bruederle and Hodler, 2018; Head et al. 2017), these studies did not utilize the new harmonized NTL dataset. Moreover, their analysis of HDI relied on data constructed using indicators from the DHS itself, such as the wealth index, education, and health. In contrast, our analysis employs alternative datasets, which provide annual gridded data for HDI and GDP per capita, derived entirely from macroeconomic indicators rather than satellite imagery or household surveys. Specifically, we utilize datasets that provide annual gridded data for GDP per capita and the Human Development Index (HDI) (encompassing health, education, and standard of living) (Kummu et al. 2018). Importantly, our analysis benefits from the fact that the subnational indicators of economic activity, namely HDI and GDP per capita, are derived from macro-economic indicators and are not computed using satellite imagery or household surveys.

The efficacy of NTL as a proxy for economic activity, particularly in rural areas, has been a subject of debate in the literature. While some studies found NTL to be unreliable proxies for GDP in rural settings in Indonesia (Gibson et al. 2021), others argue for their reliability in rural areas of Colombia (Pérez-Sindín et al. 2021). Finally, our study contributes to this ongoing debate by investigating the heterogeneity in the variation captured by the new harmonized NTL separately in urban versus rural areas and examining the usefulness of NTL as a proxy for economic activities at subnational levels in rural areas.

By quantifying the degree to which the newly available harmonized NTL data correlate with several indicators of economic activity at the subnational level (i.e. household wealth index derived from the DHS, and gridded HDI and GDP per capita) across 34 countries in sub-Saharan Africa, we aim to provide insights into the effectiveness of NTL as a proxy for economic activity. We also consider the degree to which NTL captures additional information beyond simple population density. Table 2.1 summarizes the datasets utilized in this study, along with their sources and temporal coverage.

Table 2. 1. Data sources and Coverage

Variable	Source	Spatial Coverage and Resolution	Temporal Coverage
Household wealth index	DHS	Over 90 countries, GPS coordinates, 10 km	Since 1980s; patchy
Nighttime lights data	Li et al. (2020)	Global, 1 km	1992 to 2021; annual
Population density	GPWv4	Global, 5 km	2000 to 2020; quinquennial
Human development index	Kummu et al. (2018)	39 countries, 10 km	1990 to 2015; annual
Gross domestic product per capita	Kummu et al. (2018)	82 countries, 10 km	1990 to 2015; annual

Our focus on sub-Saharan Africa is motivated by its unique challenges in obtaining accurate subnational socioeconomic data and its inherent significance as a region experiencing rapid population growth and ongoing economic changes (Akintunde and Oladeji, 2013), along with a high risk of emerging human diseases (Jones et al. 2008).

2.2 Materials and Methods

2.2.1 Data

Demographic and Health Surveys (DHS) Wealth Index

The primary dependent variable in our study is derived from the DHS, which are a series of nationally representative and standardized household surveys conducted in low-income countries in Africa and other regions since the 1980s to monitor and evaluate population, health, and nutrition programs. Our sample includes DHS surveys that have geo-coordinates to match them to NTL data. For a comprehensive list of the countries included in our sample, along with the corresponding years of data collection, please refer to Table A1.

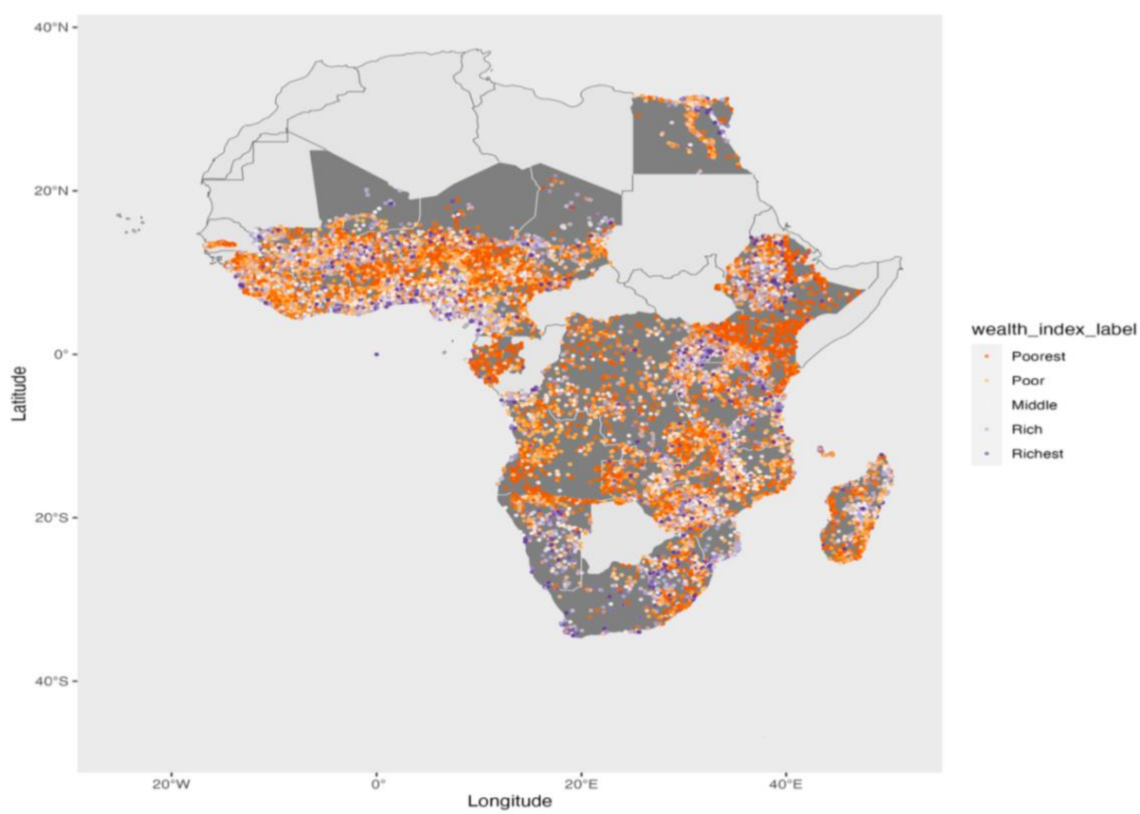
The DHS data are repeated cross-sections rather than a panel survey since a new sample of clusters is drawn for each round for the same country. The geographic information is available at the level of survey clusters or Primary Sampling Units (PSUs) rather than at the level of households. The clusters are categorized into urban and rural groups, and the cluster location is reported with latitude/longitude coordinates (Weidmann and Theunissen, 2021). Most cluster locations are measured using GPS while only some are measured by information from gazetteers (Weidmann and Schutte, 2017). However, to ensure anonymity, cluster location points are randomly displaced by up to 2 kms for urban clusters and by 5 kms for rural clusters, with less than 1 percent of rural clusters displaced by up to 10 km (Bruederle and Hodler, 2018).

Our primary variable of interest is the DHS wealth index, which is derived from data on households' ownership of a selected set of assets such as a phone, radio, car, TV, or motorbike; dwelling characteristics like the number of rooms occupied in a home, flooring material, access to electricity, type of drinking water source, as well as other characteristics related to the wealth status. Specifically, we use the Household Recode (HR) survey data for each country, which contains all household attributes, alongside the corresponding GPS dataset for the same year and phase (for details on construction of DHS wealth index, see Rutstein and Johnson, 2004).

The DHS wealth index is the most widely used variable to capture poverty in studies analyzing predictors of economic well-being (Jean et al. 2016; Yeh et al. 2020). This is because, given the limited availability of comprehensive socioeconomic indicators with high spatial resolution across a wide range of developing countries, the DHS wealth index is considered the most suitable option (Weidmann and Schutte, 2017). However, due to reasons listed in the previous section, its coverage is patchy. Table A1 shows that in our sample of 34 countries between 2004 and 2019, Rwanda was surveyed in most years (5), followed by Ethiopia (4), with most countries surveyed only once.

The household-level wealth index factor score is calculated by analyzing the asset ownership of each individual through principal component analysis. A categorical household wealth index variable ranging from 1 to 5 is derived from the household-level wealth factor score where 1 represents the lowest asset levels or "poorest" households and 5 represents the highest asset levels or “richest” households. However, as stated above, the DHS doesn’t provide household locations but rather the average location of a group of households which is referred to as a household cluster. Therefore, we extract the wealth index across all surveys in our sample and create average wealth index across all households within each cluster.

Figure 2.1 displays the geographical distribution of DHS clusters included in our sample. Each point on the map represents a cluster, and its color indicates the wealth category of the households surveyed. The countries shaded in gray represent those included in our sample. It is important to note that the DHS wealth index is a relative measure of wealth. It is constructed using country-specific methodologies, which limits its applicability for cross-country comparisons. Therefore, interpretations of the wealth index should be confined to within-country comparisons.



Base map data from the spData package in R, derived from Natural Earth (public domain). The figure depicts geographical distribution of DHS clusters in the sample, with points colored by household wealth category (Poorest to Richest). Countries shaded in gray represent those included in the study. The figure was created by the author.

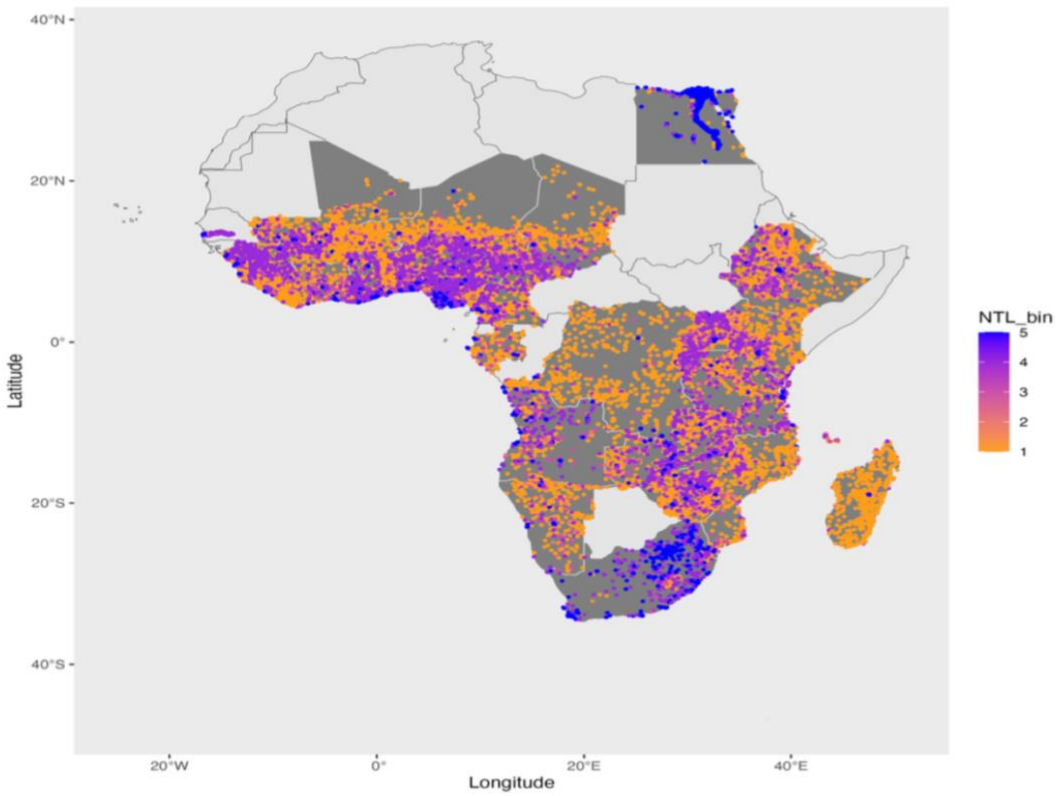
Figure 2. 1. Geographic Distribution of DHS survey clusters in Africa.

Nightlights data

We use a harmonized NTL dataset generated by Li et al. (2020). This allows us to expand the time-period for our study from 2004 to 2019. The dataset is available yearly, downloadable as rasters at a spatial resolution of 30 arc-seconds (~1 km). Unlike the raw data from the two primary sources, DMSP (1992–2013) and VIIRS (2013–2019), which are not directly comparable due to limited temporal overlap and differing spatial resolutions, this harmonized dataset bridges these gaps by simulating DMSP-like data from VIIRS. For details on the harmonization process and data sources, please refer to Appendix A.

By using this dataset, we not only extend the temporal coverage but also enhance the sensitivity of our nighttime light indicator. Their approach, particularly the simulation of DMSP-like data using VIIRS, improves the capture of medium-to-low light emissions, making it valuable for investigating local development impacts and economic output in Africa (Schon and Koren, 2022).

We aggregate these rasters at a coarser resolution of 10 km^2 to match them with DHS household wealth data using the geographic coordinates at the cluster level in the DHS surveys. To provide additional context, we overlay country boundaries on the NTL data in Figure 2.2. This overlay allows us to visualize the within-country variation in NTL and compare it with the within-country variation of the Wealth Index as well as HDI and GDP per capita.



Base map data from the spData package in R, derived from Natural Earth (public domain). The figure depicts geolocated DHS clusters across Africa, overlaid with harmonized nighttime light (NTL) data. Clusters are colored by NTL intensity (low to high), with darker shades representing higher light emissions. Countries included in the study are shaded in gray. The figure was created by the author.

Figure 2. 2. Geolocated DHS clusters in Africa, colored by NTL.

Population density data

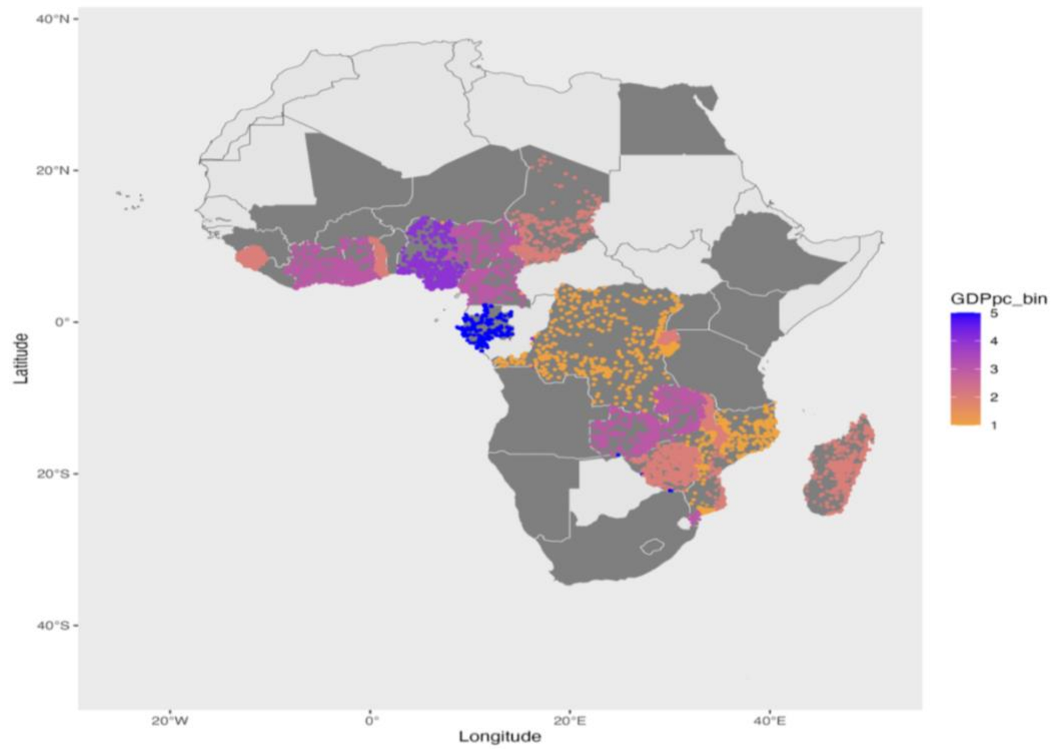
Population density data were sourced from the CIESIN Gridded Population of the World (GPWv4) (2016) at a spatial resolution of 5 km, approximately 2.5 arc minutes, covering intervals of 5 years. For the years between these intervals, we implemented linear interpolation to estimate population density following the methods from previous research (Schmidt et al. 2017). Similar to NTL, these were then aggregated to 10 km^2 and combined with DHS data for the years 2004 to 2019.

Alternative dependent variables: HDI and GDP per capita

Along with the DHS wealth index, we use two alternative dependent variables: GDP per capita and the HDI. GDP per capita stands as a core metric for gauging economic performance and is widely employed as an indicator of average living standards or economic prosperity. HDI is often utilized to categorize countries based on their human development levels (health, educational attainment and standard of living) (UNDP, 2024) and is a more holistic measure compared to just income or wealth alone. However, the annual release of official global HDI estimates by the Human Development Report Office of the United Nations Development Programme (UNDP) is limited to highly aggregated national level estimates, hindering its application in scenarios requiring sub-national details. Despite being considered a more meaningful metric than income alone, HDI has not replaced income measures for assessing development progress within countries due to lack of availability at sub-national levels. Additionally, the reliance on slow, infrequent, and costly global-scale ground-based data collection for all current HDI estimates severely limits their practical usability beyond cross-national rankings (Sherman, et al. 2023).

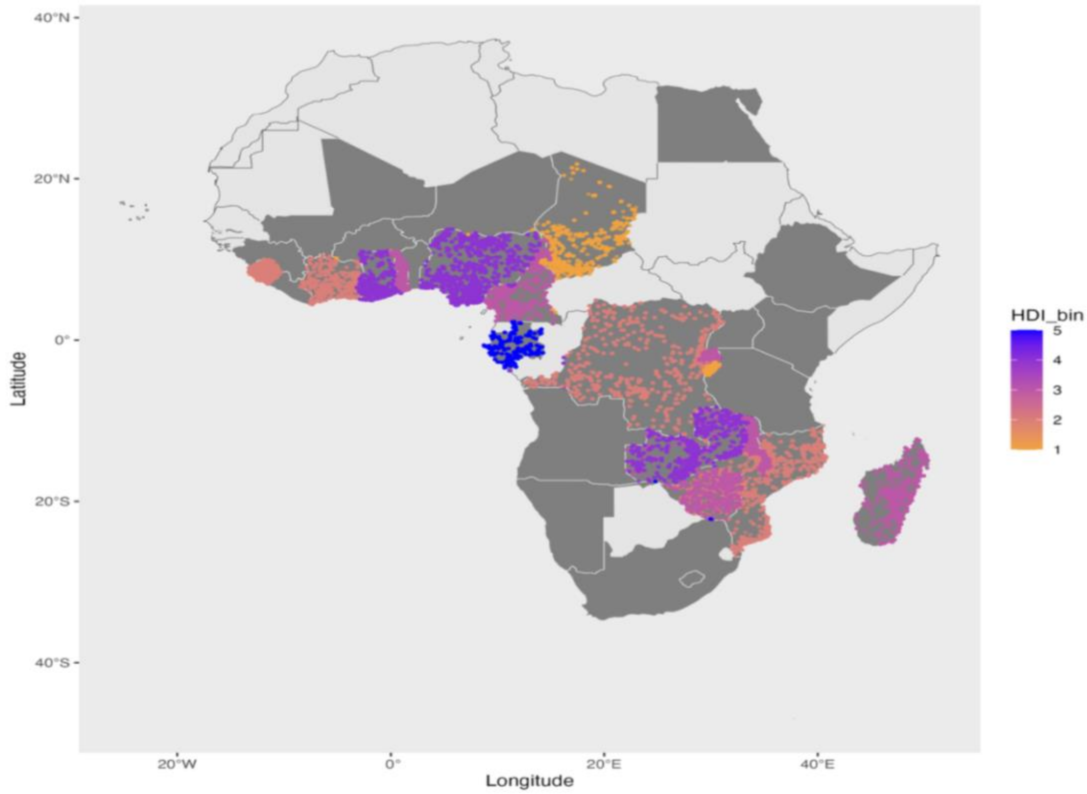
Recognizing this limitation, subnational annual gridded datasets for GDP per capita and HDI have been produced for the whole world at a spatial resolution of 5 arc-min level (10 km) (Kummu et al. 2018). For the GDP per capita, these datasets combine both sub-national (based on Gennaioli et al. 2013) and national datasets (from the World Bank dataset and CIA's World Factbook). Priority is given to reported sub-national data, followed by the utilization of interpolated and extrapolated sub-national data, in conjunction with national averages. For HDI, scaling factors are devised to integrate information from both sub-national and national sources, using national-level data (from UNDP) and subnational-level (from UNDP and census reports where available).

We extracted both these datasets (from Kummu et al. 2018) for the years for which they were available for our sample (2004 to 2015). Below, we plot two maps showing the variation in GDP per capita (Figure 2.3) and HDI data (Figure 2.4) for the DHS cluster locations. We divide the data into 5 bins of equal intervals (similar to 5 bins of the wealth index) to plot them. Note that there is very little variation within countries for both datasets. This goes against the motivation of creating these datasets to explain sub-national variation.



Base map data from the spData package in R, derived from Natural Earth (public domain). The figure depicts geolocated DHS clusters across Africa, categorized by GDP per capita (GDPpc) quintiles. Darker colors indicate higher income levels. Countries shaded in gray represent those included in the study. The figure was created by the author.

Figure 2. 3. Geolocated DHS clusters in Africa, colored by GDP per capita.



Base map data from the spData package in R, derived from Natural Earth (public domain). The figure depicts geolocated DHS clusters in Africa, categorized by Human Development Index (HDI) quintiles. Darker colors indicate higher levels of human development. Countries shaded in gray represent those included in the analysis. The figure was created by the author.

Figure 2. 4. Geolocated DHS clusters in Africa, colored by HDI.

2.2.2 Descriptive Statistics

Table 2.2 presents summary statistics for the full sample in panel (a), for the urban sample in panel (b), and for the rural sample in panel (c). On average, urban areas emit approximately five times more light (23) than rural areas (5.7). The standard deviation of 13 indicates that there is a lower degree of variability in the level of NTL across different rural areas compared to 21 in urban areas. Similarly, urban areas are, on average, over nine times more densely populated (3,250) than rural areas (363). The standard deviation of 984 in rural areas is also smaller than that in urban areas.

Table 2. 2. Summary Statistics

(a) Full Sample

Variable	Obs.	Mean	Std. Dev.	Min.	Max.	Median
Mean Wealth Index	39072	3	1.2	1	5	2.8
Nightlights	39072	12	18	0	63	2.7
Population Density	39072	1418	4262	0	45196	195
Human Development Index	30880	0.5	0.095	0.29	0.7	0.49
GDP per capita	30880	3551	3418	365	17595	1876

(b) Urban Sample

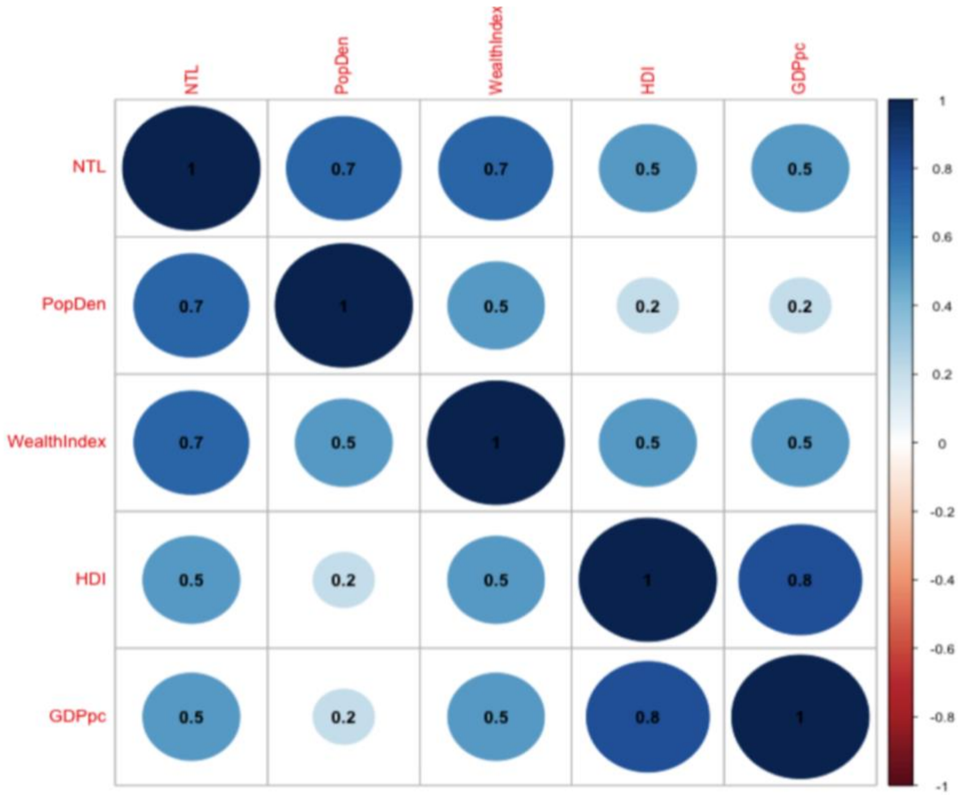
Variable	Obs.	Mean	Std. Dev.	Min	Max	Median
Mean Wealth Index	14278	4.1	0.86	1	5	4.4
Nightlights	14278	23	21	0	63	14
Population Density	14278	3250	6538	0	45196	692
Human Development Index	11160	0.51	0.1	0.31	0.69	0.5
GDP per capita	11160	4346	3769	365	17595	2614

(c) Rural Sample

Variable	Obs.	Mean	Std. Dev.	Min	Max	Median
Mean Wealth Index	24794	2.4	0.81	1	5	2.3
Nightlights	24794	5.7	13	0	63	0
Population Density	24794	363	984	0	42424	109
Human Development Index	19720	0.49	0.091	0.29	0.7	0.48
GDP per capita	19720	3101	3113	365	17595	1649

Notes: NTL are unitless, measured in Digital Number ranging from 0 to 63. These values represent the intensity of the lights, with higher values indicating greater intensity. Population density is measured as the number of people per square kilometer. Wealth Index is an index between 1 to 5. HDI is an index between 0 and 1, and GDP per capita is expressed in constant 2011 US dollars (constant 2011). Household Wealth Index data is derived from DHS. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, Human Development Index (HDI) and Gross Domestic Product per capita data are both from Kummu et al. (2018). For more details, see Table 2.1.

In Figure 2.5, we present the Spearman correlation coefficient matrix for our variables. As anticipated, there is a strong positive correlation between NTL and population density (0.7). Similarly, we find a high correlation between the HDI and GDP per capita (0.8). When examining the wealth index, NTL display a stronger correlation (0.7) compared to population density (0.5). Additionally, both HDI and GDP per capita show a positive correlation of 0.5 with the wealth index. Notably, while NTL exhibit a relatively high correlation (0.5) with HDI and GDP per capita, population density shows a lower correlation of 0.2 with both indicators.



Author's calculations for pairwise correlation pairwise correlations among key variables: Nighttime Light (NTL), Population Density (PopDen), Wealth Index, Human Development Index (HDI), and GDP per capita (GDPpc). The size and color intensity of the circles indicate the strength and direction of the correlations, with darker blue representing stronger positive correlations and lighter shades or red (if present) indicating weaker or negative correlations.

Figure 2. 5. Correlation Plot.

2.2.3. Methods

Our objective is to evaluate the extent to which harmonized NTL data serve as a proxy for socio-economic outcomes at small spatial levels. To achieve this objective, we employ repeated *k*-fold cross-validation to assess the predictive accuracy of our models. This method involves randomly splitting the data into k = 10 equally sized folds, and the process is repeated 10 times.

The models are trained on k-1 folds and tested on the remaining fold for each iteration of the cross-validation process. Predictions are then made on the test data, and performance metrics, such as the R-squared, are computed. This iterative process ensures that each fold is used exactly once as the test dataset. Finally, the out-of-sample R-squared is calculated as the average performance metric across all iterations of the cross-validation process. This metric represents the proportion of the variation in the response variable that is explained by the model when applied to unseen data, thereby serving as an estimate of the model's predictive accuracy on new observations.

We begin by estimating a relationship using Ordinary Least Squares regression, which enables us to examine key associations, including the relationship between NTL with socio-economic outcomes for the pooled sample. Socio-economic outcomes are defined using the three dependent variables measured in this paper: the DHS wealth index, HDI, and GDP per capita. We estimate the models with and without controlling for population density to evaluate the contribution of NTL independently of population density.

Additionally, we examine these relationships within two distinct subsamples: urban and rural. However, we only present results for urban versus rural subsamples in the case of the DHS wealth index, as we found very little difference in the variation captured by NTL for HDI and GDP per capita between urban and rural areas.

Furthermore, to account for the unique characteristics specific to each country and year over the study period, we incorporate country and time fixed effects in all regressions, particularly in those where the DHS wealth index serves as the dependent variable. This is essential due to the lack of direct comparability of the wealth index across different countries or even repeated rounds of survey for the same country. For example, DHS surveys often employ different methodologies over time, making results from different survey rounds potentially inconsistent. For a detailed list of the survey rounds used for each country, refer to Table A1 in the supporting information.

We estimate the following relationship:

$$Y_{cjt} = \beta_0 + \beta_1 \, NTL_{cjt} + \beta_2 \, populationdensity_{cjt} + \Gamma_j + \Gamma_t + e_{cjt} \qquad \text{Equation (1)}$$

where Y_{cjt} represents the socio-economic outcomes in cluster c, country j, and year t, which is measured by three dependent variables - the DHS wealth index, HDI, and GDP per capita. The right hand side variables include: NTL_{cjt} which is night light intensity; $populationdensity_{cjt}$ which is population density; Γ_j and Γ_t are country and year fixed effects, respectively, and e_{cjt} is the error term. Adding country and year fixed effects allows us to explore the relationship of NTL with socio-economic outcomes within specific country and year groups, effectively examining variations within a single survey. Standard errors are clustered at the same level of fixed effects (country and year) which will adjust for potential correlations between error terms within the same country or year.

In the main analysis, all regressions with HDI and GDP per capita as the dependent variables include both country and year fixed effects. However, for robustness, we also present results in the supporting information (Appendix A) for models estimated under alternative specifications: (i) without fixed effects; (ii) with only country fixed effects; and (iii) with only year fixed effects.

We observe that the NTL, population density, and GDP per capita data exhibit right-skewed distributions. To address this skewness, we initially consider using the logarithm of their values as a smoothing technique. However, due to a considerable proportion of zero observations, dropping these observations from our samples is not feasible. To address this challenge while still accommodating zero-valued observations, we opt for the Inverse Hyperbolic Sine (IHS) transformation. This preserves the properties of the log transformation while allowing us to retain zero-valued observations (Bellemare et al. 2011).

2.3. Results

Table 2.3 displays the outcomes of regressing the DHS wealth index on harmonized NTL, both without and with population density (Columns 1 and 3 respectively). Additionally, Column (2) shows the results of regressing the DHS wealth index only on population density. Since NTL and population density have been transformed using the IHS transformation, we can interpret them as percentage changes.

The findings in Table 2.3 reveal that variations in NTL alone significantly explain variations in the wealth index, yielding an Out-of-Sample R-Squared (OOS-R Squared) of 48% (Column 1). When population density is included as a control variable (Column 3), the OOS-R Squared increases slightly to 49%, suggesting that models incorporating both NTL and population density marginally outperform those without population density.

Table 2. 3. OLS regression results with country and year fixed effects.

	Wealth Index		
	(1)	**(2)**	**(3)**
Nightlights	0.573***		0.475***
	(0.002)		(0.005)
Population Density		0.385***	0.104***
		(0.003)	(0.005)
Fixed effects (Country and year)	Yes	Yes	Yes
OOS R^2	0.480	0.352	0.491
Adjusted R^2	0.479	0.351	0.490
Residual Std. Error	0.839	0.937	0.830

Notes: Dependent variable is the DHS mean wealth index. Nightlights and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the country and year level. All models have country and year fixed effects. *p<0.1; **p<0.05; ***p<0.01. Household Wealth Index data is derived from DHS. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4. For more details, see Table 2.1.

It is important to note that the relationships estimated do not imply causation; the findings only indicate that higher levels of NTL are associated with higher wealth, holding all other factors constant. All specifications in the analysis include country and year fixed effects, allowing for within-country comparisons. This is essential because the wealth index is not comparable across countries.

To address potential concerns about spatial autocorrelation in the residuals, we conducted supplementary analyses using spatial fixed effects models. Due to computational constraints, spatial models were estimated on a subset of the data. Importantly, the results from these spatial models (see Table A8) were consistent with the primary analysis presented in Table 2.3, further reinforcing the robustness of our findings.

It is crucial to conduct separate analyses for urban and rural areas due to differences in their economic activity types, their population density, and lighting characteristics. Previous research (Gibson et al. 2021) observed that satellite-detected NTL mainly represent urban economic activity, consisting of concentrated street lamps and industrial facilities typical of urban settings, while such lights are rarely found in rural villages. To address this, we divided our sample into urban and rural regions and presented the results separately in Table 2.4. The table includes the effects of NTL, population density, and their combined influence on wealth in urban versus rural areas. While previous research (Gibson et al. 2021) only used VIIRS data for two years and focused exclusively on Indonesia, our analysis utilizes the harmonized NTL dataset (DMSP + VIIRS) for 34 countries, spanning 2004 to 2019. This harmonized dataset enables us to study long-term changes in nighttime light intensity, addressing inconsistencies between the two sources.

Our findings (from Table 2.4) show that the share of variation in the wealth index explained by NTL in urban areas (43%) (Column 3) is over two times that in rural areas (21%) (Column 6). Similarly, the explanatory power in models with population density is over two times for urban areas (36%) (Column 4) as compared to rural (16%) (Column 7). Furthermore, examining the direction and relative strength of correlations reveals that both NTL and population density exhibit positive correlations with the wealth index, with the NTL consistently demonstrating a significantly stronger correlation across all models.

Table 2. 4. OLS Regression for Wealth Index in Urban v/s Rural Areas.

	Mean Wealth Index							
	Overall		**Urban**			**Rural**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Nightlights	0.572***		0.317***		0.263***	0.352***		0.280***
	(0.003)		(0.005)		(0.009)	(0.007)		(0.008)
Population Density		0.384***		0.181***	0.049***		0.192***	0.096***
		(0.004)		(0.004)	(0.006)		(0.006)	(0.006)
Fixed effects (Country and year)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
OOS R^2	0.481	0.352	0.433	0.365	0.439	0.212	0.171	0.226
Adjusted R^2	0.480	0.351	0.430	0.362	0.436	0.210	0.168	0.224
Residual Std. Error	0.856	0.936	0.649	0.686	0.646	0.719	0.729	0.707

Notes: Dependent variable is the DHS mean wealth index. Nightlights and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the country and year level. All models have country and year fixed effects. *p<0.1; **p<0.05; ***p<0.01. Household Wealth Index data is derived from DHS. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4. For more details, see Table 2.1.

In both urban and rural settings, the inclusion of population density as a control variable leads to a reduction in the magnitude of the coefficient on NTL, but it remains significant. This observation is likely attributable to the fact that although part of the variation in NTL is absorbed by variation in population density within the DHS clusters, NTL still retains substantial information about the wealth index that surpasses the influence of population density. The estimates presented in Table 2.4 are consistent with the notion that lights serve as a useful proxy for urban economic activity. Additionally, variation in lights explains a modest proportion (21%) of the variation in the wealth index in rural areas. Overall, our findings using the harmonized NTL data are broadly consistent with previous research (see Gibson et al. 2021) for both urban and rural areas. For instance, in their study for Indonesia (Gibson et al. 2021), they found that for the urban sector, the relationship between NTL and economic activity remains positive regardless of the type of NTL data used. However, in rural areas, economic activity was negatively related to DMSP NTL, while it was positively but imprecisely related to VIIRS NTL. They reported very small R-squared values for rural areas, 3% using DMSP and 1% using VIIRS, with the results for VIIRS being statistically insignificant. In contrast, they found much higher R-squared values for urban areas, 36% using DMSP and 68% using VIIRS, suggesting that NTL is a better proxy for urban than rural economic activity.

Our results outperform theirs for both DMSP and VIIRS, likely due to the use of harmonized NTL data, which addresses inconsistencies between the two sources. However, it is important to note that the dependent variable in earlier research (Gibson et al. 2021) is regional GDP, whereas our analysis focuses on the DHS wealth index. Thus, the results are not directly comparable.

Next, we estimated Equation 1 using the GDP per capita and Human Development Index (HDI) as the dependent variables in Tables 2.5 and 2.6 respectively. In Table 2.5, we employ the new Harmonized Nighttime Lights (NTL) dataset alongside population density to analyze the variation in GDP per capita within a specific country and year group, incorporating country and year fixed effects.

Table 2. 5. OLS regression for GDP per capita with country and year fixed effects.

	Gross Domestic Product per capita		
	(1)	**(2)**	**(3)**
Nightlights	0.035***		0.036***
	(0.001)		(0.002)
Population Density		0.020***	-0.002
		(0.001)	(0.001)
Fixed effects (Country and year)	Yes	Yes	Yes
OOS R^2	0.935	0.933	0.935
Adjusted R^2	0.935	0.934	0.935
Residual Std. Error	0.220	0.222	0.220

Notes: Dependent variable is the Gross Domestic Product per capita. Gross Domestic Product per capita, Nightlights, and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the country and year level. All models have country and year fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Gross Domestic Product per capita data from Kummu et al. (2018). For more details, see Table 2.1.

Given the transformation of NTL, population density, and GDP per capita using the Inverse Hyperbolic Sine (IHS) transformation, the coefficient estimates are interpreted as elasticities. The estimation of the model in column 1, yields a precisely estimated elasticity of NTL of 0.035, accompanied by a high OOS R-square of 93 percent. However, it is important to note that these results should not be interpreted as causal. Instead, they suggest that a 1% increase in NTL is associated with a 3.4% higher GDP per capita, all else being equal. Similarly, a 1% increase in population density is associated with a 2% higher GDP per capita.

The observed variation in GDP per capita is primarily driven by between-country differences rather than within-country disparities. Specifically, controlling for baseline differences between countries through country fixed effects alone explains 91% of the observed variation, as detailed in Table A2 in the supporting information. Furthermore, the consistent OOS R-square across models, regardless of the inclusion of population density, indicates that NTL possesses significant predictive power on its own, independent of population density. In fact, when both NTL and population density are included in the regression (Column 3), population density is not statistically significant and even exhibits a sign inconsistent with expectations.

We use an alternative indicator of sub-national economic development. The HDI is often considered a more meaningful metric than income or wealth alone. However, there are few studies (Bruederle and Hodler, 2018; Head et al. 2017) that have tried to explain how well NTLs predict HDI. Using the new sub-national global HDI dataset (Kummu et al. 2018), we study the variation in HDI explained by NTL and population density within a specific country and year group, employing country and year fixed effects (Table 2.6).

Table 2. 6. OLS regression for HDI with country and year fixed effects.

	Human Development Index		
	(1)	**(2)**	**(3)**
Nightlights	0.0002***		0.001***
	(0.0001)		(0.0001)
Population Density		-0.0002*	-0.0004***
		(0.00004)	(0.0001)
Fixed effects (Country and year)	Yes	Yes	Yes
OOS R^2	0.984	0.984	0.985
Adjusted R^2	0.984	0.984	0.984
Residual Std. Error	0.011	0.011	0.011

Notes: Dependent variable is the Human Development Index. Nightlights and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the country and year level. All models have country and year fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Human Development Index (HDI) from Kummu et al. (2018). For more details, see Table 2.1.

The model estimated in column 1 explains a substantial proportion of the variation in HDI, with a high OOS R-square of 98%. This suggests that nightlights serve as a suitable proxy for HDI, given significant correlations across all specifications. As with GDP per capita in Table 2.5, after including population density in the regression (column 3), NTL continues being positively and significantly correlated with HDI, while population density exhibits a wrong sign (and now is statistically significant).

Bruederle and Hodler (2018) have examined potential channels through which a positive association between NTL and HDI may have taken effect, highlighting improved schooling outcomes, lower infant mortality rates, and increased local economic activity in areas with higher NTL. In our models, the inclusion of only country fixed effects accounts for up to 92% of the variation in HDI, as detailed in Table A3 in the supporting information. This highlights that the minimal within-country variation in HDI is primarily driven by geographical heterogeneity.

Between-country differences, rather than within-country disparities, explain a substantial portion of the observed variation in HDI.

2.4. Conclusion

Access to consistent and accurate data over long time periods is crucial for understanding trends and formulating informed policy decisions at regional or local levels. For example, analyzing local economic growth patterns or assessing the effects of climate change on regional ecosystems necessitates spatially disaggregated, historical data. NTL data can serve as valuable proxies for measuring development at local levels where alternative indicators may be lacking. The DMSP (discontinued in 2013) and the VIIRS are two widely utilized series of NTL data. While VIIRS data offer improvements over DMSP, including higher precision and updated technology, DMSP is still widely used in economics literature due to its longer time series spanning from 1992 to 2013. Newly available harmonized NTL data are therefore essential to bridging the gap between DMSP and VIIRS datasets.

We utilized a harmonized dataset developed by Li et al. (2020), which integrates DMSP and VIIRS data to offer a continuous NTL time series from 1992 to 2021. This dataset, characterized by its relatively new processing technology and higher precision (Sono et al. 2022), provides a consistent framework for analyzing economic trends with high spatial resolution. To the best of our knowledge, our research is the first to test the accuracy of the harmonized NTL dataset in measuring economic activity using household wealth index from the DHS, and alternatively through other indicators of subnational economic development; namely, gridded HDI and GDP per-capita.

Through our analysis, we make four key contributions to the literature. First, we test the accuracy of the new harmonized NTL dataset in measuring economic activity at a small spatial scale in developing country municipalities. Second, we assess the extent to which NTL vary independently of population density. Third, we explore whether NTL can serve as a proxy for human development (HDI) beyond wealth or income, an area largely unexplored in previous research. Lastly, our study adds to the ongoing debate on the utility of NTL as a proxy for economic activity at subnational levels, specifically focusing on urban versus rural areas.

In evaluating predictive performance of our models on unseen data (new observations), we use k-fold repeated cross validation to calculate out-of-sample R-squared. The results demonstrate that the harmonized NTL data can serve as valuable indicators for studying wealth index at local levels, with strong explanatory power. Moreover, models that control for population density slightly increase the variation explained by NTL in wealth index. Our analysis of the relationship between NTL and wealth index across urban and rural settings reveal that the share of variation explained by NTL in wealth index is two times higher in urban areas as compared to rural areas. This finding is in line with the literature that finds NTL to be a reliable proxy only for urban areas. Furthermore, using a unique global gridded dataset for both GDP per capita and HDI, our study demonstrates that NTL serve as reliable proxies for both GDP per capita and the HDI at subnational levels. Importantly, both estimated coefficients and predictive power of NTL remains significant even after controlling for population density. Additionally, we note that the observed high variation explained in our models explaining both GDP per capita and HDI is primarily driven by between-country differences rather than within-country differences.

The implications of this research extend beyond the field of economics to various disciplines requiring sub-national data on economic prosperity. For instance, in the context of climate change, extreme weather events have highly localized impacts that are often obscured when using aggregated GDP data. Using the new harmonized NTL data, which allows studying long time-periods and captures localized economic activity, can help researchers gain a better understanding of the effects of climate change at sub-national levels. Overall, the insights provided in this study can guide researchers in selecting appropriate indicators for their specific contexts and research objectives. This, in turn, contributes to more informed decision-making and policy formulation.

CHAPTER 3

WEATHERING HARD TIMES WITH BUSHMEAT: RURAL HOUSEHOLD COPING UNDER RAINFALL SHOCKS

3.1. Introduction

Bushmeat, defined as meat from wild animals, primarily mammals, reptiles (such as snakes, crocodiles, lizards, and tortoises), and birds (Fa et al. 2003), serves as an important source of protein for roughly 150 million households in the Global South (Nielsen et al. 2018). Its consumption is associated with a reduced risk of anemia, malnutrition, and stunting in children (Fa et al. 2015; Golden et al. 2011), improved household food security (Friant et al. 2020), dietary quality as well as cognitive function (Neumann et al. 2003). In addition to its dietary role, bushmeat serves as a flexible source of income, particularly during low agricultural seasons (Kümpel et al. 2010; Nasi et al. 2008) and in times of crisis, such as those induced by climate shocks (e.g., droughts) (de Merode et al. 2004; van Vliet et al. 2008). Despite these benefits, harvesting and trade of bushmeat also creates a policy dilemma. Unsustainable harvesting depletes wildlife populations and threatens biodiversity, while bushmeat consumption may increase exposure to zoonotic diseases (Brashares et al. 2011; Pruvot et al. 2019), with links to epidemics such as Ebola, and pandemics such as HIV/AIDS and SARS (Coad et al. 2021; Taylor et al. 2001; Milbank & Vira 2022).

Climate change may only amplify these trade-offs. As extreme weather events grow more frequent and severe (IPCC 2023), rural households may increasingly turn to bushmeat hunting to buffer income and consumption shocks (Paumgarten et al. 2018). While existing research (Appendix B) documents how climate shocks affect household welfare (Carpena, 2019), agricultural productivity (Gornall et al. 2010; Damania et al. 2017), and child health (Hanna & Oliva 2016; Nsabimana & Mensah 2020), their impact on bushmeat hunting (a potential coping mechanism), remains poorly understood. Existing studies on the effect of shocks on bushmeat hunting yield mixed insights. While some suggest bushmeat acts as a drought-induced safety net (Brashares et al. 2011; Mendonça et al. 2016; Paumgarten et al. 2018), others find no evidence to support this claim (Wunder et al. 2014; Nielsen et al. 2015; Nielsen et al. 2017; Nielsen et al. 2018). Similarly, prior research offers mixed evidence on whether bushmeat is primarily a safety net for the poor (Nielsen 2006; Hickey et al. 2016; Torres et al. 2021) or an accessible option only for those with sufficient assets (De Merode et al. 2004; Wilkie et al. 2005; Coad et al. 2010; Brashares et al. 2011). Consequently, it remains unclear whether wealth alleviates or intensifies pressure on wildlife.

Critically, previous literature suffers from three limitations: First, prior studies often rely on small-scale case studies or broadly examine wild foods or non-timber forest products (Wunder et al. 2014; Paumgarten et al. 2018), rather than specifically focusing on bushmeat. Second, existing research often aggregates diverse shocks into broad categories (Wunder et al. 2014), potentially obscuring distinct effects of droughts and floods on hunting engagement. Additionally, these studies employ self-reported shock data that can be prone to endogeneity, whereby the households who successfully rely on bushmeat hunting as a coping strategy could also report lower shock intensity. Third, most studies employ cross-sectional research designs, which are prone to omitted variable bias and cannot fully capture households' adaptive responses over time.

Here we investigate how rainfall shocks influence rural households' income from and reliance on hunting advancing the literature in four ways. First, we leverage the Poverty and Environment Network (PEN) dataset (Angelsen et al. 2011), where almost 8,000 rural households across 24 countries were interviewed four times within one year, allowing us to build a panel to capture short-term (quarterly) responses to shocks. Second, to measure shocks we employ an objective, high-resolution drought index, the Standardized Precipitation Evapotranspiration Index (SPEI) (Vicente-Serrano et al. 2010), to mitigate biases inherent in self-reported shock measures. Third, we differentiate between drought and wet conditions, quantify their intensity, and map their occurrence to planting, growing, and harvesting seasons. Finally, we explore heterogeneity by wealth, testing whether adaptive capacity influences households' ability to exploit bushmeat during crises. Specifically, we address the following questions:

1. Do droughts and wet conditions have (different) impacts on households' income from and reliance on hunting?
2. How does the intensity and timing of these shocks within the agricultural calendar affect income from and reliance on hunting?
3. Does households' adaptive capacity, measured by wealth, moderate these effects?

Our results reveal three key findings. First, droughts consistently increase both income from and reliance on bushmeat hunting, with an extreme drought increasing hunting income by 39% and reliance on hunting income by 1.8 percentage points. The effects are highest during the growing season, when crop income is most vulnerable. Second, wet conditions reduce income from and reliance on bushmeat, but only at moderate intensities, suggesting that improved agricultural output lowers hunting engagement. Third, wealthier households exhibit greater capacity to increase hunting income during droughts, reflecting inequalities in adaptive strategies. Our findings are consistent with prior case studies showing that bushmeat hunting tends to rise during the late dry season, when wildlife is easier to locate near limited water sources (Lindsey et al. 2011; Pukazhenthi, 2004). Hunting also tends to intensify in periods following low crop yields (Lindsey et al. 2011) but typically subsides during the height of the agricultural season, when labor shifts back to farming (Brashares et al. 2011; Lindsey et al. 2013).

This paper contributes to the literature on household adaptation to climate shocks by examining hunting as a productive response available to rural households in the global south. Our findings suggest that, in the face of drought-induced agricultural income losses, households reallocate labor towards hunting (often a labor-intensive activity that is less sensitive to short run climate shocks). This is in line with the development literature on agricultural household models with incomplete markets (Janvry et al. 1991; Taylor & Adelman 2003; Aragón et al. 2021), where households intensify the use of self-provided inputs such as land or labor to stabilize consumption, when faced with negative income shocks. Hunting, in this context, emerges as a safety net, functioning like other ex-post adaptation strategies, such as sale of livestock, shifting labor to non-farm sources of income generation or migration (Blakeslee et al. 2020; Aragón et al. 2021).

The remainder of the paper is organized as follows. Section 2 presents the theoretical framework. Section 3 discusses the data and methods. Section 4 details the empirical strategy. Section 5 discusses the results and robustness checks, and Section 6 concludes.

3.2. Theoretical Framework

To understand how rural households cope in response to climate shocks, we develop a stylized household model in which labor is allocated between two productive activities: farming and hunting of bushmeat, each of them exhibiting diminishing returns to labor. We assume that households face incomplete markets.

Figure 3.1 offers a simple illustration of the allocation of effort between the two activities. The fixed supply of labor is represented as the distance between OF and OB on the horizontal axis. Labor in the farming sector is measured to the right starting from O^F while labor used for bushmeat hunting is measured to the left starting from O^B. In the absence of complete markets for labor, insurance, and credit, households allocate labor to equalize the value of the marginal product of labor (VMPL) in both activities (point O in Figure 3.1). Climate shocks alter the marginal productivity of farming, which in turn drives changes in labor allocation.

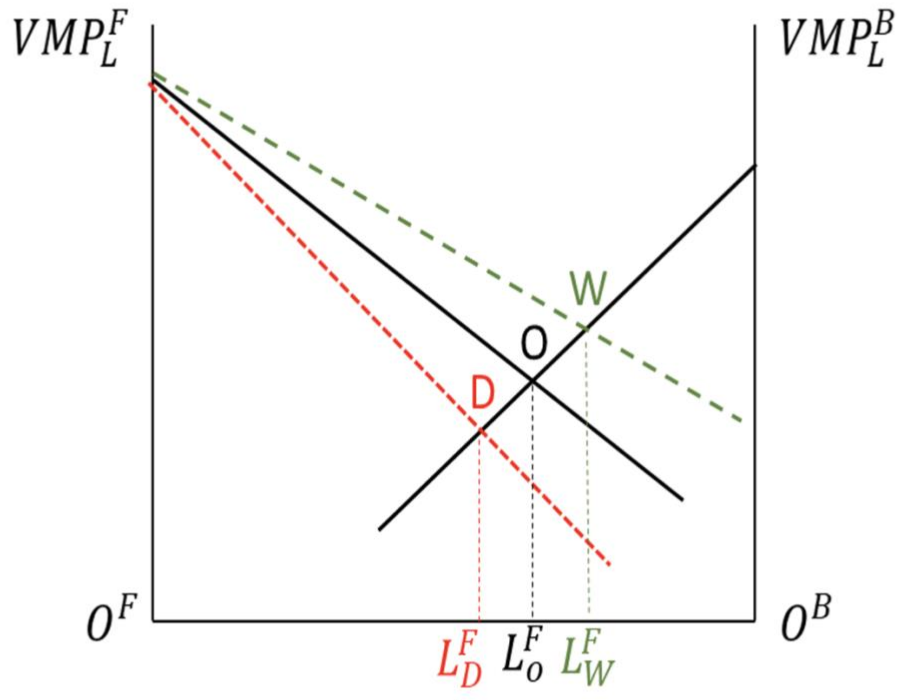


Figure 3. 1: Conceptual illustration of labor allocation response to climate shocks. The x-axis shows the share of labor allocated to farming. The left y-axis shows the Value of the Marginal Product (VMP) of farming and bushmeat hunting, under normal conditions (black lines). Drought conditions (red dashed line), and wet conditions (red dash-dot line) and the right y-axis shows the MP of hunting (green line), which increases as less labor is used in hunting.

Droughts lower the marginal product of farming (red line). Households shift away from farming to hunting until the VMP of both activities equalizes at point D. In contrast, a favorable wet shock (green line) can increase the VMP of farming, leading households to reduce hunting and allocate more labor to farming.

3.3. Data and Methods

3.3.1. Income

Our data originates from the Poverty Environment Network (PEN) dataset (Angelsen et al. 2011), hosted by the Center for International Forestry Research. Considered the most extensive globally comparable survey on rural households and their livelihoods (Nielsen et al. 2018), the PEN survey covers 8,301 households[1] from 333 villages in 58 sites from 24 countries in Sub-Saharan Africa, Asia and Latin America between 2005 and 2010. The data is geo-referenced at the village level. One of the key features of the PEN project is its quarterly recording of household income from all sources (environment, agriculture, livestock, wages, business and remittances. Income from these sources encompasses both monetary (cash) income and subsistence income (value of harvested products used directly by the household rather than sold for cash). Income from harvesting bushmeat is part of environmental income which is derived from both forest and non-cultivated environments. Each household in the dataset was surveyed four times over the course of a year, using short recall periods of 1 to 3 months[2] (Wunder et al. 2014). Importantly, survey sites were not selected based on the level of bushmeat hunting in an area. Instead, the sites chosen are broadly representative of non-coastal tropical and subtropical landscapes with some level of forest access in the study countries (Wunder et al. 2014; Nielsen et al. 2018).

[1] Out of which 7,978 households provided responses to the final survey, which included questions on self-reported shocks over the preceding 12 months.
[2] 1 month for bushmeat income.

In this paper, bushmeat income refers to any income, cash or subsistence, obtained from hunting of wild animals[3] for personal consumption, gifting, trading, or medicinal use (Nielsen et al. 2017). Households reporting bushmeat income, whether cash or subsistence, at any point during the survey period are classified as hunters. Household reliance on bushmeat is expressed as the share of total household income coming from bushmeat, effectively measuring bushmeat's contribution in the overall household income. All income figures are presented in terms of PPP-adjusted US dollars per adult equivalent units,[4] facilitating cross-household comparisons.

According to Nielsen et al. (2018), approximately 39% of surveyed households in the PEN data engage in bushmeat hunting and bushmeat income contributes around 2% to total household income of which 89% is consumed in the household (i.e. subsistence income). Descriptive statistics (Table B1), indicate bushmeat hunting practices vary significantly across countries, with reliance on bushmeat income ranging from 10.88% in the sites in Cameroon to 0% (or no reliance) in the sites in India and Ecuador.

3.3.2. Shocks

We derive our shock variables using the Standardized Precipitation and Evapotranspiration Index (SPEI), introduced by Vicente-Serrano et al. (2010). Because the effect of rainfall on crop yields also depends on the soil's ability to retain water (Harari & Ferrara 2018), SPEI tracks the difference

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[3] We exclude insects, crustaceans, grubs, mollusks, and fish. Our focus is on terrestrial mammals, birds, and reptiles.

[4] To make comparisons between households, we use adult equivalent units (AEU) to standardize the comparisons across different household sizes (such that individuals who are below 15 years of age and adults who are above 65 years of age are assigned a weighting factor of 0.5, whereas all other household members within the age range of 15 to 65 years are assigned a weighting factor of 1). Additionally, for the purpose of comparing income values across different countries, we use purchasing power parity (PPP) rates as specified by the IMF in 2007. These methods are in line with those used by previous studies such as Angelsen et al. (2014); Nielsen et al. (2018) and Noack et al. (2019).

between precipitation and temperature driven evapotranspiration, to better reflect how soil moisture influences plant growth, and more effectively capture wet and dry conditions compared to indices such as the Standardized Precipitation Index (SPI) or the Palmer Drought Severity Index (PDSI) (Vicente-Serrano et al. 2010).

SPEI values can be calculated monthly at various timescales (1 to 48 months) to account for cumulative precipitation deficits or excesses. For instance, the 6-month SPEI can be generated by summing up water balance values from five months before and the present month. In general, SPEI values calculated over 3-month, or 6-month timescale are known to effectively capture shocks affecting agriculture, while longer time-scales (12 or 24-month) are more appropriate for tracking hydrological shocks (Vicente-Serrano et al. 2010; Homdee et al. 2016; Potop et al. 2014; Tirivarombo et al. 2018; Wang et al. 2021). Accordingly, in this paper we focus on SPEI series calculated at a 6-month timescale (SPEI-6). However, we also verify our baseline results using shorter SPEI at a 3-month timescale (SPEI-3).

Since SPEI values are normalized with a mean of zero and a standard deviation of one, values below minus one (SPEI<-1) (or one standard deviation below the long-term (60-year) mean), represent drought conditions. Conversely, SPEI>1 represents wet conditions. Following convention, we categorize continuous SPEI values into binary variables for drought and wet events and use the thresholds of -2 for droughts and 2 for wet conditions for further classification into severe events (McKee et al. 1993; Paulo et al. 2012; Wang et al. 2014). Figure B.2 displays the distribution of SPEI in our sample[5]. We extracted SPEI values for the PEN village center

[5] Please note that SPEI values exceeding the value of 1 are denoted as wet events rather than "floods" due to the complexity of flood development, which cannot be solely attributed to high precipitation levels. Factors such as soil saturation and river discharge also play crucial roles in flood occurrence. However, heavy rainfall is the primary contributor to flood formation (Stephens et al. 2015; Bischiniotis et al. 2018).

coordinates and matched these values with household survey data using interview dates. Hence, SPEI values may differ among households within the same village due to variations in interview timing. Table B2 presents the annual prevalence rates of weather shocks in our sample, as measured by the SPEI.[6]

3.3.3. Seasons

To assess how weather fluctuations across seasons affect bushmeat hunting income, we adopt a methodological framework similar to Noack et al. (2019). We first align our sample villages with relevant geographical units from Sacks et al. (2010), which stands as the most extensive and current repository of crop planting and harvesting dates available (Challinor et al. 2015). Specifically, Sacks et al. (2010) provides georeferenced information about when each crop is planted and harvested. We use these data to classify each day of the year into one of three seasons: planting, growing, or harvesting, for every village in our sample. Finally, we measure the proportion of each quarter that falls in each of the three seasons.

3.4. Empirical Strategy

We begin by examining how droughts and wet events affect the key measures of household's engagement in hunting. To do this, we estimate a two-way fixed effects model:

$$\mathrm{Y_{hjt}} = \beta_0 + \beta_1 \mathrm{Drought_{hjt}} + \beta_2 \mathrm{Wet_{hjt}} + \beta_3 \mathrm{X_{hjt}} + \epsilon_h + \eta_t + \mu_{hjt} \qquad \text{(Equation. 2)}$$

where *h, j, t* index household village and, quarter, respectively. The outcome variable Y is either (i) bushmeat income (transformed using the Inverse Hyperbolic Sine (IHS) to accommodate zeros), or (ii) reliance on bushmeat (already measured as a percentage). The binary variables - Drought and Wet, indicate whether the household experiences a drought (SPEI < -1) or a wet event (SPEI > 1). Although X includes household and village characteristics, such as household size,

[6] Comparing SPEI-measured dry and wet events (Table E.2) with those self-reported by the village officials (Table E.3) suggests a tendency to overreport weather shocks (Table E.4).

household head's gender, age, and education, total land ownership, distance to roads/cities, elevation, and number of electrified households, these factors do not vary over time in our dataset. Consequently, they are absorbed by the inclusion of household fixed effects (ε_{hj}). Household fixed effects will also eliminate any unobservable time-invariant differences across households such as fixed geographical attributes (e.g. soil quality, proximity to forests), long-standing cultural attitudes and preferences towards hunting. This ensures that our estimates account for within-household variation, isolating the effect of exposure to shocks on hunting income and reliance. We also incorporate year fixed effects (η_t) to eliminate factors affecting all households each year (e.g. global macroeconomic conditions). Standard errors are clustered at the village level (the lowest possible cluster), to address cross-sectional correlation among households within the same geographic unit (villages).

Our main identification assumption is that households are generally unable to predict the exact timing and location of drought or wet events in the short run, making the explanatory variable (weather shock) plausibly exogenous (Musungu et al. 2023). Hence, β_1 and β_2 can be interpreted as the causal effect of a drought and wet event on bushmeat income and reliance, conditional on fixed effects.

3.4.1. Intensity of Droughts and Wet Events

Next, we differentiate between moderate/severe shocks (SPEI values between -1 and -2 for drought, +1 and +2 for wet) and extreme shocks (SPEI below -2 or above +2). Therefore, we extend Eq. 1 by splitting the binary indicators for Drought and Wet into separate dummies for moderate vs. extreme conditions. The estimation approach allows us to identify whether households' income from and reliance on bushmeat income varies with the severity of the drought and wet conditions.

3.4.2. Seasonal Effects of Weather Shocks

To evaluate whether the impact of a shock depends on the season in which it occurs, we link each household's quarterly data to detailed planting, growing, and harvesting calendars (Sacks et al. 2010). We then estimate (Eq. 2):

$$\mathrm{Y_{hjt}} = \sum_{i=1}^{3} \alpha_i \,\mathrm{Season_{ihjt}} + \sum_{k=1}^{2} \beta_k \,\mathrm{Shock_{kjht}} + \sum_{i=1}^{3}\sum_{k=1}^{2} \delta_{ik} \left(\mathrm{Season_{ihjt}} \times \mathrm{Shock_{khjt}}\right) + \epsilon_h + \eta_t + \mu_{hjt} \quad \text{(Equation. 3)}$$

where Season represents the proportion of a quarter falling into one of three agricultural phases (*i* = planting, growing, harvesting), and Shock indicates the presence of a drought or a wet event.

If, for example, a drought strikes during the critical growing season, households may experience crop losses and strategically shift labor toward hunting to offset foregone crop income (Figure 3.1). Because crop is typically the most important income source (28.7% of total household income) in our dataset (Angelsen et al. 2014), comparing changes in bushmeat income to changes in crop income across agricultural phases sheds light on as a compensatory strategy or as a supplement to agricultural income even when crops are not heavily impacted. Accordingly, the outcome variable in Equation. 3 is either i) bushmeat income/reliance, or ii) crop income/reliance. Interaction terms between shock and season (Season × Shock) reveal whether households alter their hunting or farming income based on when a shock occurs in the agricultural cycle.

3.4.3. Heterogeneous Effects by Household Income

Finally, we explore whether the shock-hunting relationship varies by households' capacity to adapt. We measure this capacity using total household income excluding bushmeat income.[7] We rank households into quartiles based on this non-bushmeat income measure and estimate:

$$\mathrm{Y_{hjt}} = \beta_0 + \beta_1 \mathrm{Shock_{hjt}} + \beta_2 \mathrm{Shock_{hjt}} \times \mathrm{Income_{hjt}} + \epsilon_h + \eta_t + \mu_{hjt} \quad \text{(Equation. 4)}$$

[7] We exclude bushmeat income from the calculation to avoid endogeneity in the income measure.

where Y represents bushmeat income. A statistically significant interaction term (Shock × Income) would suggest that households with greater incomes, which are arguably better able to cope with or even capitalize on a climate shock, respond differently in terms of bushmeat engagement. The results will help us to better understand the role of economic standing in adaptive behaviors to climate shocks and inform the debate on how wealth relates to reliance on wildlife.

3.5. Results

3.5.1. Impact of drought and wet events on bushmeat income and reliance on hunting

Figure 3.2 displays our baseline estimates (Equation 2) using two different timescales for measuring rainfall shocks (SPEI measured over the previous 3 or 6 months). The left panel illustrates results for the full sample, whereas the right panel focuses on the subset of villages where at least one household engages in hunting ("hunting" sample). Bushmeat income (depicted in blue) is scaled by 100 so that the coefficients can be interpreted as percentage changes, while reliance on bushmeat (shown in red) is measured in percentage points (ppt). Confidence intervals are at the 95 percent level. Full regression results for this specification are provided in Table B5.

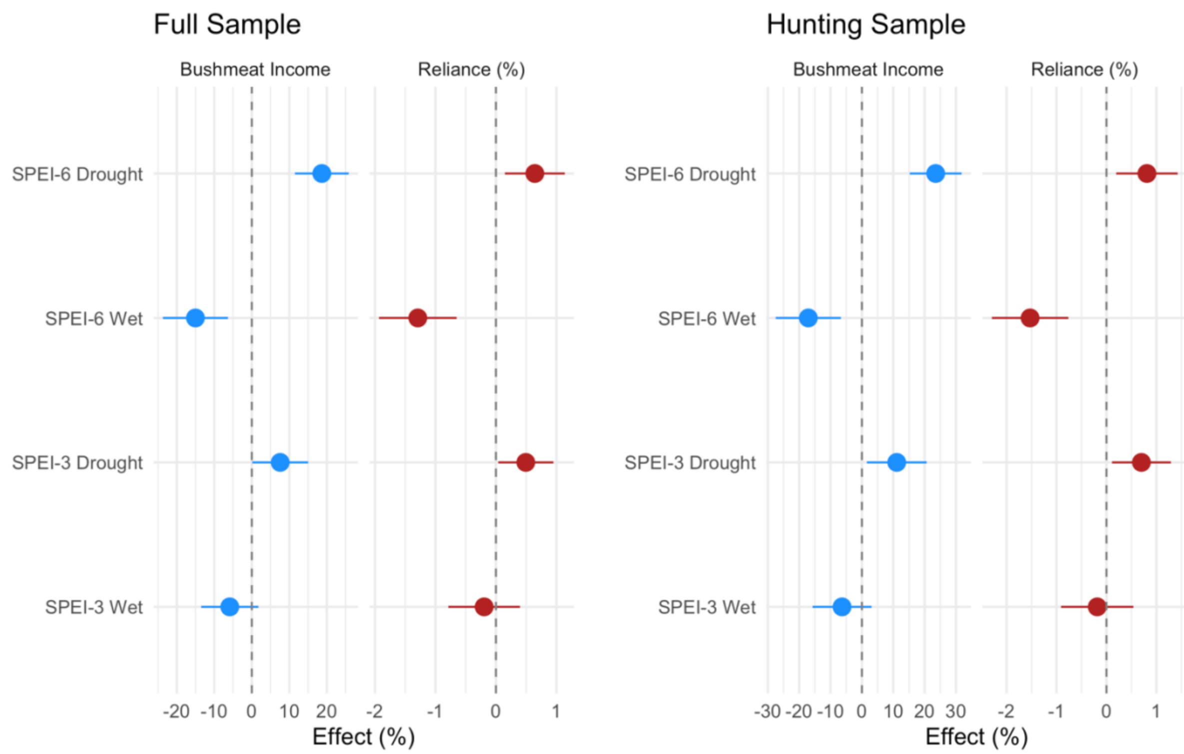


Figure 3. 2: Effect of Drought and Wet Shocks on Bushmeat Income (blue) and Reliance (red). The left panel presents results for the full sample and the right panel presents results for households in villages that report hunting activity (hunting sample). Bushmeat income coefficients are scaled by 100 and can be interpreted as percentage changes. Reliance on bushmeat income is measured in percentage points. All specifications include household and year fixed effects. Robust standard errors are clustered at the village level. SPEI index is calculated at a 3-month and 6-month time-scale. Drought and Wet dummies are created using the intensity scale of SPEI index, with values below -1 indicating drought conditions and values above +1 indicating wet conditions.

The results indicate that droughts significantly increase both income from and reliance on hunting. In the full sample, a drought measured over a 6-month timescale increases bushmeat income by 18% and bushmeat reliance by 0.64 ppt.[8] Among households in hunting villages, the impacts are even larger, with a drought increasing bushmeat income by 23% and bushmeat reliance by 0.81 ppt. This finding is consistent with evidence from previous studies suggesting that drought-

[8] It is important to note that because bushmeat constitutes only about 2% of total household income in the PEN data (Nielsen et al. 2018), even a large absolute increase in bushmeat income translates into a relatively small ppt increase in the reliance.

driven food scarcity concentrates large mammals around water sources, making them easier targets for hunters (Lindsey et al. 2011; Fragoso et al. 2022). By contrast, wet conditions have a negative (though often smaller) effect on engagement in hunting. A wet event over a 6-month timescale reduces bushmeat income by 15% (full sample) to 17% (hunting-only sample), along with a corresponding decrease in bushmeat reliance by 1.29 ppt and 1.52 ppt, respectively. Conforming to intuition, results using a shorter timescale of 3 months yield smaller effects, that in the case of wet events are no longer statistically significant at conventional levels.[9]

These findings suggest that households turn to bushmeat hunting as an adaptive strategy during droughts, presumably to mitigate agricultural losses. In contrast, wet events, which may enhance agricultural productivity, decrease income from hunting and dependence on hunting. Our findings confirm the predictions of our stylized household model outlined in Section 2, where climate-induced shifts in marginal productivity trigger labor reallocation across activities.

Our results align with broader evidence (*Appendix B*) suggesting that while droughts consistently harm agricultural output, the impact of positive rainfall shocks is mixed, potentially benefiting or harming productivity depending on the intensity of rainfall (Damania et al. 2017). For instance, Murken et al. (2024) argue that although extreme wet conditions can lead to waterlogging and crop damage, farmers may still benefit from wet shocks because the relationship between crop yields and climatic conditions is not linear; instead, the optimal yield often occurs with precipitation levels slightly above the average.

In addition to our main results based on objective climate data (SPEI indices), we also compare these findings to estimates using self-reported shocks from the PEN survey, presented in Figure

[9] Previous research (Vicente-Serrano et al. 2012 and Kubik & Maurel 2016) also found results using shorter SPEI timescales to be smaller, in-significant or less precise. Wald tests for statistical differences between shock coefficients are presented in Table E.6 (SM).

B.3. Interestingly, while the objective SPEI measures show that droughts increase bushmeat income, the self-reported shock data suggest no significant or even opposite effects. This divergence highlights one of our key methodological contributions, relying on objective, spatially consistent climate measures can yield very different insights than self-reported data, which may be affected by endogeneity (*Appendix B*).

3.5.2. Impact of shock intensity on bushmeat income and reliance on hunting

In this subsection, we explore how households respond to moderate/severe versus extreme droughts and wet events. Figure 3.3 illustrates these results for both the full sample (top panels) and the subset of “hunting” villages (bottom panels). The left panels show changes in bushmeat income, scaled by 100 for percentage interpretation, while the right panels show changes in bushmeat reliance (measured in percentage points), with 95 percent confidence intervals. In subsequent model estimations, we only present results for SPEI-6 as it provided more consistent results for both drought and wet conditions in our previous estimation. Full regression results for this specification are provided in Tables B7 (bushmeat income) and B8 (reliance) (SM).

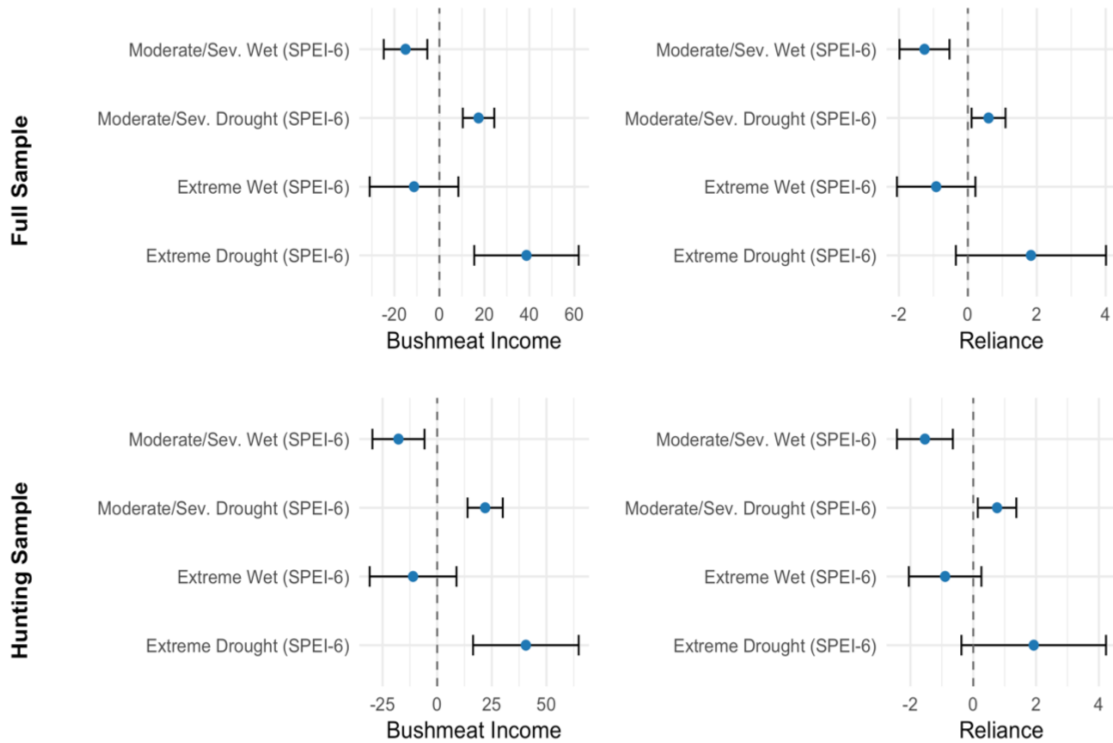


Figure 3. 3: Effect of Extreme versus Moderate/Severe shocks on Bushmeat Income (left panel) and Reliance (right panel). Top row presents results for the full sample and the bottom row presents results for the hunting sample. Bushmeat income coefficients have been multiplied by 100 and can be interpreted as percentage changes. Reliance on bushmeat income is measured in percentage points. All specifications include household and year fixed effects. Robust standard errors are clustered at the village level. Shock dummies are calculated using SPEI index at 6-month timescale, with values below -2, indicating Extreme Drought, values between -1 and -2 indicating Moderate/Severe Drought, values above +2 indicating Extreme Wet and values between +1 and +2 indicating Moderate/Severe Wet conditions.

The results indicate that even relatively less intense droughts prompt households to increase their engagement in hunting. For instance, a moderate/severe drought increases bushmeat income by 17.4% in the full sample and 21.9% in hunting villages, with a corresponding increase in bushmeat reliance by 0.6 ppt and 0.7 ppt, respectively. As droughts intensify further, the effect becomes notably larger (as confirmed by Wald Tests in Table B9). An extreme drought increases bushmeat income by nearly 39% in the full sample and 40.6% in hunting villages, with a corresponding increase in reliance by 1.8 ppt and 1.9 ppt. These results indicate that households increasingly turn to bushmeat as drought intensity escalates, likely to offset the severe losses in agricultural productivity. In contrast, moderate/severe wet conditions reduce bushmeat income by around 15% (full sample) to 18% (hunting villages) and bushmeat reliance by roughly 1.2 ppt and 1.5 ppt respectively. This suggests that since moderate wet episodes replenish soils and enhance productivity for crop production (Banerjee 2010), they may thereby diminish the need for bushmeat hunting. Extreme wet conditions do not yield statistically significant results.

3.5.3. Impact of shock timing on bushmeat income versus crop income

In this section we examine how the timing of climate shocks within the agricultural cycle affects bushmeat income. Our key hypothesis is that droughts during periods critical for crop growth and yield will prompt households to reallocate labor to hunting to offset anticipated agricultural losses. By contrast, droughts during less critical stages of crop growth will have weaker or no impact on labor reallocation from farming to hunting, as households might anticipate smaller effects on their agricultural yield.

To empirically assess this hypothesis, we estimate *Equation. 3* using either bushmeat income or crop income as the outcome variable. We present the results from estimating *Equation. 3*, through average partial effects (marginal effects) of drought and wet episodes on bushmeat and crop income, depicted in Figure 3.4 with 95 percent confidence intervals. Full regression results for this specification are provided in Table B10 (SM). Estimates of the same specification using bushmeat reliance and crop reliance as dependent variables are presented in Table B11 (SM).

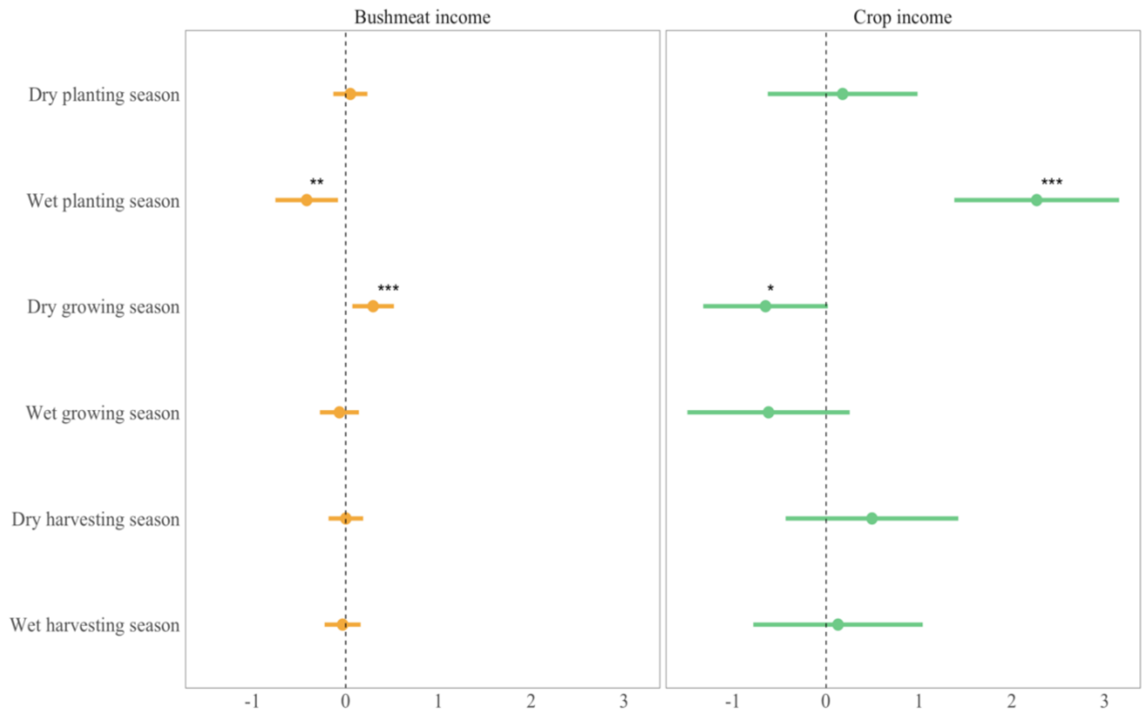


Figure 3. 4: Marginal Effects of Timing of Shocks on Bushmeat and Crop Incomes. The figure displays estimated coefficients at 95% confidence intervals from regressions on bushmeat income (left panel) and crop income (right panel), both transformed using IHS function. Coefficients reflect change in income due to experiencing a shock in each season. Multiplying the coefficients by 100 will approximate the percentage change in income. All regressions include household and year fixed effects, and standard errors are clustered at the village level. *p<0.1; **p<0.05; ***p<0.01

As expected, a drought substantially reduces crop income in the growing season (-65% drop). At the same time, a drought increases bushmeat income by around 29% during the growing season (much higher compared to 5% in the planting season and 0.2% in the harvesting season). This supports our hypothesis that when a drought occurs in a period critical for crop growth, households experiencing (and likely anticipating) sharp agricultural losses in the significant future turn to bushmeat hunting to partially offset those losses.

During the planting season, the effects are more pronounced for wet episodes. A wet event during the planting phases can improve reflecting soil moisture and increase crop income. The same wet event reduces bushmeat income by 41%, suggesting that households allocate less labor to hunting when agriculture is more profitable.

Overall, our results confirm our hypothesis that the timing of climate shocks significantly influence household labor allocation decisions between farming and hunting. These findings align with Noack et al. (2019) who note that livelihood diversification tends to intensify in periods when agricultural income is affected the most and with Costa et al. (2023) who suggest that farmers facing a drought will adapt by adjusting their input mix such as land or labor.

3.5.4. Effects of Income Heterogeneity

In this subsection we analyze how the impacts of droughts on bushmeat income and reliance vary across income quartiles. Throughout the earlier sections of our results, we consistently find that droughts have a significant and robust influence on both bushmeat income and reliance on hunting. In contrast, the effects of wet conditions are less consistent and often statistically insignificant. The effects are also stronger for households in villages where hunting is prevalent (i.e. hunting sample). Consequently, this section focuses exclusively on droughts and on the hunting sample.

Figure 3.5 (and Table B12) reports the marginal effects of a drought on bushmeat income across four income quartiles, calculated using total household income excluding bushmeat income. We find that the marginal impact of a drought increases significantly with income level, peaking in the wealthiest quartile (Q4). Pairwise Wald tests (Table B13) confirm that these differences are statistically significant between the lowest and middle quartiles (Q1 vs. Q2 and Q1 vs. Q3) [10].

[10] The size of bushmeat income varies widely for households in Q4 (Table B14).

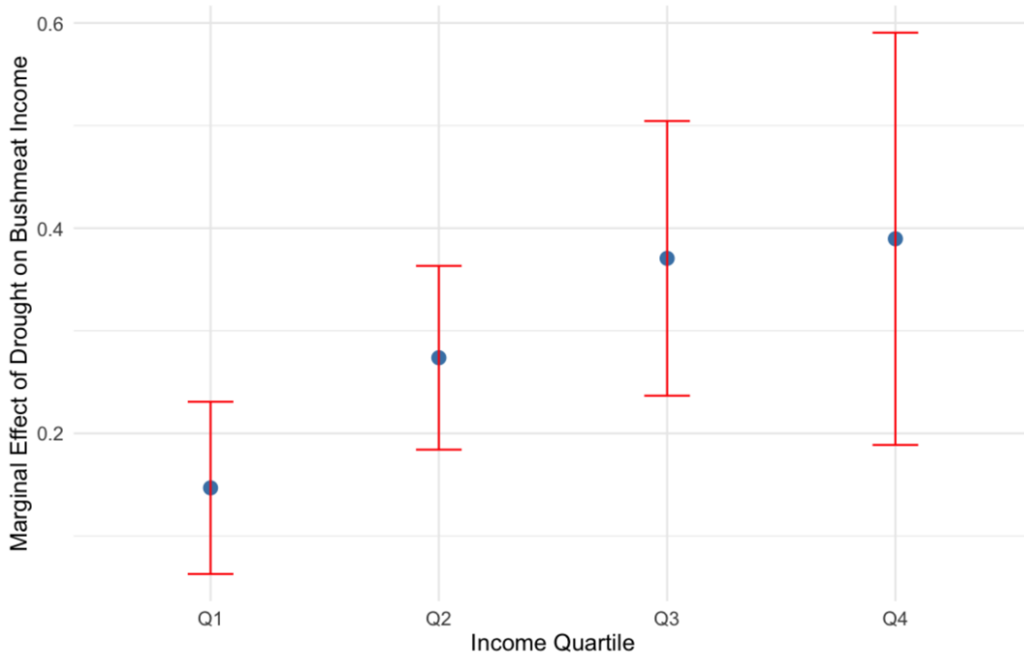


Figure 3. 5: Marginal Effect of Drought on Bushmeat Income by Income Quartiles. The figure displays estimated effect of drought on IHS transformed Bushmeat Income across four income quartiles (Q1 to Q4), with 95% confidence intervals. Quartiles are based on total household income excluding bushmeat income. The sample is limited to households in hunting prevalent villages. Multiplying the coefficients by 100 will approximate the percentage change in income. All regressions control for household and year fixed effects, with standard errors clustered at the village level.

These findings can be explained using insights from our exploratory analysis (SM). Although low-income households face droughts more often (Figure B.4, SM) and rely more on bushmeat when they face a drought than high income households (Figure B.6, SM), they lack the capital or physical resources to scale up hunting when drought hits. High-income households, in contrast, own larger tracts of land, and often reside in areas with greater forest cover, and higher humidity (Figures B.7 a, b). Furthermore, high-income hunting households are also more likely to be male headed, which can relax certain social constraints on hunting[11] (Noss, 1997; Schulte-Herbrüggen et al. 2013).

Our findings align with the broader findings in the literature on forest resource use (Coad, 2008; Arnold & Pérez 2001; Angelsen & Wunder 2003; Heubach et al. 2011) that suggest wealthier households typically derive greater value from forest resources, both in terms of quantity and cash returns from non-timber forest products (*Appendix B*). Our results also support previous literature indicating that economic barriers significantly shape access to wildlife resources among different economic groups (Coad et al. 2010; De Merode et al. 2004; Dercon 1998; Ambrose-Odj 2003). Moreover, previous research suggests that hunting is a risky activity, sometimes involving corruption or bribe payments (Nielsen et al. 2014; Lindsey et al. 2013; Bernard & Stammer 2024). There is evidence that hunters who are caught frequently avoid formal penalties by paying bribes (Nielsen et al. 2014), which can be prohibitively expensive for low-income households. Because high-income hunting households can more readily absorb the costs of fines or bribes, they face a comparatively lower risk if caught, whereas low-income hunting households may forgo hunting altogether.

3.5.5.Robustness Tests

Climate variables tend to be correlated across space, so the standard errors may be biased (Auffhammer et al. 2013). To guard against this, we assess robustness of our baseline results using Conley's (1999) spatial heteroskedasticity and autocorrelation-consistent (spatial HAC) estimator. Table B15 shows the sensitivity of our results using Conley-corrected standard errors, confirming that our main findings remain robust.

[11] Schulte-Herbrüggen et al. (2013), report that women are generally restricted from setting traps in forested areas where more bushmeat was obtained. Instead, their role was more akin to that of assistants, helping men in group hunts by directing animals towards nets set up by men.

To ensure our results aren't driven solely by changes at the intensive margin, we also estimate the probability that a household reports any bushmeat income. Table B16 reports the probability that a household engages in hunting i.e. Pr(bushmeat hunting = 1) using the same two-way fixed effects setup as in our baseline. The results mirror our findings at the intensive margin. A drought (SPEI-6) significantly increases the likelihood of hunting by 8.7 ppt and a wet event reduces the probability of hunting by 4.5 ppt.

Finally, to verify that results from heterogeneity by income are not influenced by focusing solely on hunting-prevalent villages, we re-estimate the same specification on the full sample and results remain robust (Table B17).

3.6. Conclusion

This paper combines high-resolution gridded climate data with quarterly income records from the Poverty and Environment Network across 24 countries to provide causal evidence on how rainfall shocks affect rural households' engagement with bushmeat hunting.

First, our results consistently show that droughts have a significant and positive effect on income from and reliance on bushmeat. A moderate drought can increase bushmeat income by 18%, and up to 39% under extreme drought conditions. By contrast, a moderate wet event can reduce bushmeat income by 15%, likely because improved crop yields reduce the need for alternative income, although extreme wet conditions do not produce a significant effect. Second, by exploiting data on the seasonal calendar, we show that droughts during the growing season led to the largest increase in bushmeat income (roughly 29% increase), highlighting a strategic reallocation of labor when crops incomes are most at risk. Finally, heterogeneity analysis reveals that, when faced with a drought, wealthier households are better positioned to extract higher bushmeat income, even though poorer households rely more heavily on bushmeat as a proportion of their income. In other words, the coping strategy of bushmeat hunting is available to both, but the wealthier can exploit it more effectively, due to better equipment, access, and perhaps greater ability to handle the costs or risks of hunting. Nonetheless, our findings confirm that bushmeat remains a critical fallback option during droughts, corroborating prior evidence that hunting serves as a safety net during periods when agricultural income or other sources of income are disrupted (Jambiya et al. 2007; Ordaz-Németh et al. 2017; Schulte-Herbrüggen et al. 2013; Enns et al. 2023).

Taken together, our paper contributes to the literature in two ways. Methodologically, we show the value of integrating objective measures of climate shocks with household income surveys, to overcome the biases of self-reported shock data. Empirically, we disentangle the effect of droughts versus wet events, that prior studies reliant on coarse shock measures have obscured. Our findings carry implications for policymakers addressing rural resilience, conservation, and zoonotic disease risks. By identifying when and for whom bushmeat serves as a crisis buffer, targeted interventions, such as weather index insurance (Barnett & Mahul 2007; Jansen & Carter 2019), could reduce hunting pressure while safeguarding vulnerable households.

CHAPTER 4

"CUTTING DOWN FORESTS, BUILDING UP DISEASE?": CAUSAL EVIDENCE ON DEFORESTATION'S IMPACT ON MALARIA

4.1. Introduction

Over the past two decades, large-scale land acquisitions (LSLAs), have expanded rapidly across the developing world, particularly in the wake of the 2007-2008 global food price crisis. This surge has been driven by perceptions of abundant "unused" land, and weak land governance structures (Anseeuw et al. 2012; Deininger and Byerlee, 2012). Governments across the developing world have leased millions of hectares to foreign and domestic investors, positioning LSLAs as catalysts for agricultural and rural development (Collier and Dercon, 2014; Ali et al. 2019). Africa has emerged as the primary global target, accounting for nearly two-thirds of the estimated 12.1 million people potentially affected by such deals (Brüntrup, 2011; Deininger and Byerlee, 2012; Osabuohien, 2013; Davis et al. 2014). Advocates argue that smallholder farming alone cannot meet Africa's growing food demand, emphasizing the potential of large scale investments to increase productivity, transfer technology, and generate employment (Hufe and Heuermann, 2017). Yet mounting evidence points to unintended socio-economic and environmental consequences, ranging from food insecurity and displacement to deforestation and water scarcity, leading many observers to characterize these acquisitions as "land grabs" (Rulli and D'Odorico, 2013; Davis et al. 2015, 2020; Johansson et al. 2016). While a growing body of research has examined welfare, agricultural productivity, and labor market outcomes (Jiao et al. 2015; Anti, 2021) of LSLAs, much less is known about potential unintended effects; their implications for human health, especially in the African context. This paper addresses that gap by investigating whether LSLA-driven deforestation affects malaria prevalence across nine countries in sub-Saharan Africa using a staggered difference in difference design.

Many African governments, particularly in the wake of the 2007-2008 global food price crisis, sought to revitalize their agricultural sectors by contracting out large tracts of land to commercial investors through LSLAs. Estimates suggest that over 80% of these LSLAs were directed toward agricultural production (Osabuohien et al. 2019; Shete and Rutten, 2015). The policy rationale behind these deals was to transform subsistence farming into commercially viable enterprises, generate employment, stimulate rural development, and expand access to social amenities (Bunte et al. 2018). Rising demand for bio-fuels, such as those derived from sugarcane and oil palm, also led to significant land investments across the region (Anseeuw et al. 2012; Hufe and Heuermann, 2017). A defining feature of land markets in Africa is that governments typically own or control most of the land available for acquisition, making outright purchases rare. Instead, LSLAs often take the form of long-term leases, particularly because many African countries have constitutional or legal restrictions that ban the outright sale of land to foreigners (Emenyonu et al. 2017). Governments often justify these leases as a means to increase agricultural efficiency, attract foreign capital, and modernize rural economies (Collier and Dercon, 2014). Yet public health concerns are rarely included in the official evaluation of projects, despite the potential for far-reaching effects on local populations (Anti, 2021).

While LSLA programs aim to stimulate agricultural output, they may also reshape landscapes through rapid deforestation and land use change. In sub-Saharan Africa, agricultural expansion is the dominant driver of forest loss, responsible for about 90 percent of deforestation (Curtis et al. 2018). Growing evidence suggests that such environmental degradation has significant impacts on human health, particularly in developing countries where children disproportionately bear the burden (Prüss-Üstün and Corvalán 2006; Berazneva and Byker, 2017). The direction and magnitude of such impacts, however, are theoretically ambiguous. On the one hand, clearing of forests for agricultural or extractive projects typically coincides with infrastructure expansion, employment opportunities, and economic activity around the acquired lands, factors that may lead to improvements in living standards and access to health services that enable households to invest in prevention and treatment of diseases (for instance better housing, bed nets, or improved access to healthcare). On the other hand, the conversion of forests through burning and land clearing releases particulate matter and greenhouse gases, degrading air quality and increasing respiratory illness and mortality. At the same time, deforestation can reduce biodiversity and alter vector habitats in ways that favor malaria transmission (Vittor et al. 2006; Busch and Ferretti-Gallon, 2017; Ijumba and Lindsay, 2001; Bauhoff and Busch, 2020; Chakrabarti, 2021). The interaction of these distinct and often opposing processes during forest loss makes it challenging to anticipate, in advance, the overall impact of LSLA-driven deforestation on malaria. Thus, the net effect of LSLA-driven deforestation on malaria represents an empirical question. It will depend not only on the magnitude of land clearing and habitat change, but also on the extent to which households can translate economic gains into effective malaria prevention.

Empirically, isolating the causal impact of deforestation on disease dynamics remains difficult. Deforestation is rarely random and much of the existing work relies on panel data methods[12] to mitigate bias. For example, Berazneva and Byker (2017) report that deforestation increased

[12] Such as fixed effects approach.

malaria risk among Nigerian children under five, while Garg (2019) find similar effects in Indonesia, attributing them to altered mosquito habitats and human-vector interactions.

Moreover, the external validity of these studies remains limited. This is because the ecological and socio-economic context of sub-Saharan Africa differs substantially from that of Southeast Asia. For example, the dominant malaria vectors in Africa (parasite- *P. falciparum*) - *An. gambiae*, *An. arabiensis*, and *An. funestus*, differ in their habitat preferences and breeding ecology from *An. dirus* (parasite- *P.knowlesi*), which is more common in Southeast Asia. These differences imply that the health impacts of deforestation may vary substantially across regions. A recent review by Yasuoka and Levins (2007) highlight this heterogeneity, showing that the impact of deforestation depends on the type of land conversion, the local dominant vector species, and the specific environmental changes induced.

Unsurprisingly, the broader literature reveals mixed findings. Several studies link deforestation to higher malaria (Austin et al. 2017; Berazneva and Byker, 2017; Fornace et al. 2016; Garg, 2019; Pattanayak et al. 2010), while others report null or even negative effects (e.g. Bauhoff and Busch, 2018; Hahn et al. 2014; Valle and Clark, 2013; Santos and Almeida, 2018)[13].

Several factors could be responsible for such mixed results (Cheng and Miteva, 2019). First, the effects of deforestation on malaria are likely spatially non-uniform. Previous studies have relied on aggregated village level, municipality level or district level data which may obscure

[13] Valle and Clark (2013) show that total deforestation may significantly reduce disease incidence due to the destruction of mosquito habitats. Similarly, Santos and Almeida (2018) find that the relationship between deforestation and malaria follows a quadratic form. At lower levels, deforestation promotes contact between humans and mosquito vectors, raising infection risk. However, beyond a critical threshold of deforested area, the environment becomes too degraded to sustain vector breeding sites, leading to a decline in malaria cases.

localized effects (Hahn et al. 2014)[14]. Second, most empirical work has been country-specific, limiting generalizability across diverse ecological and institutional settings in sub-Saharan Africa. Third, the relationship between forest and malaria is species-specific: vector preferences for sun exposure, breeding habits, and forest types vary widely across regions. For instance, while *An. darlingi* in the Amazon may avoid sunlit areas (Yasuoka and Levins, 2007), other species found in sub-Saharan Africa, such as *An. arabiensis*, *An. gambiae*, and *An. funestus*, may thrive in fragmented forests and sunlit puddles (Sinka et al. 2012). Fourth, empirical studies systematically investigating the impact of deforestation on malaria in Sub-Saharan Africa remain limited. Much of the empirical literature on deforestation and malaria has focused on Brazil and Southeast Asia (Garg, 2019; Fornace et al. 2016; Pattanayak et al. 2010), understanding of this relationship in sub-Saharan Africa remains limited (Berazneva and Byker, 2017 studying Nigeria and, Cheng and Miteva, 2019 studying Liberia are exceptions). Yet the region bears the heaviest global burden of malaria (WHO, 2017). Moreover, the region has experienced rapid forest loss and forest fragmentation in recent decades, driven in part by LSLAs and agricultural expansion.

Given that Sub-Saharan Africa is also experiencing rapid forest conversion due to LSLAs and commercial agriculture, understanding the consequences of deforestation for malaria transmission is of urgent public health and policy relevance. This paper seeks to fill this gap by examining how LSLA-induced deforestation affects malaria risk among children across nine sub-Saharan African countries.

We use the timing and location of LSLA implementation to measure deforestation. The timing of LSLAs is plausibly exogeneous because LSLA contracts, negotiations and staggered

[14] Some studies such as Bauhoff and Busch, 2018 and Berazneva and Byker, 2017 are exceptions because they use individual level data from the DHS.

implementations across sites are driven by national policy windows, investor logistics, not by short term fluctuations in local malaria. However, while LSLA placement is unlikely to be exogeneous because investors are typically drawn to locations with favorable characteristics such as fertile land, proximity to markets, suitable infrastructure, and particular geography, such features are likely time-invariant at the temporal scale of our analysis. This implies that while LSLA locations may differ systematically from non-LSLA locations, the differences are time-invariant and will be taken care of by our difference in difference setup.

To estimate the causal effect of LSLA-induced deforestation on malaria incidence, we then combine the spatially and temporally explicit LSLA data with geocoded household health surveys and annual forest loss data. We employ a staggered, multi-period spatial DiD framework using Wooldridge, (2021) extended two way fixed effect estimator. Identification comes from variation in exposure within 10 kms and beyond 20 kms of LSLA sites (but within 90 kms), before and after implementation. This study offers the first cross-country causal assessment of the health spillovers of LSLAs in Africa and makes two contributions to the literature: 1) it adds to the limited body of causal evidence on deforestation and malaria in Africa; 2) it leverages the unique spatial and temporal variation introduced by LSLAs to identify the public health implications of commercial land use expansion.

We find that the exposure to an LSLA increases the probability of a child experiencing fever by about 5.7 percentage points, which is roughly a 21% increase relative to the control mean. This effect is statistically significant and remains robust across multiple robustness checks. Our results contribute to ongoing debates on the design of land policies and call for integrating health considerations into broader discussions on sustainable development.

The remainder of this paper is organized as follows. Section 2 provides background on LSLA policy and deforestation trends in sub-Saharan Africa, as well as on malaria ecology in the region. Section 3 describes the data sources. Section 4 outlines the identification strategy. Section 5 presents the results and mechanisms. Section 6 concludes.

4.2. Background

4.2.1. Large-Scale land acquisitions (LSLAs) and Deforestation

Although the global rate of forest loss has declined in recent decades, deforestation remains a pressing concern, particularly in tropical regions where agricultural expansion is the leading driver, followed by mining, infrastructure development, and urban growth (Runyan & Stehm, 2020). Chaves et al. (2020) link a detailed multi-regional input–output database to geospatial deforestation and malaria records and estimate that about one-fifth of the tree loss occurring in malaria-sensitive zones is traceable to overseas demand for a handful of commodities, notably wood products, cocoa, tobacco and cotton.

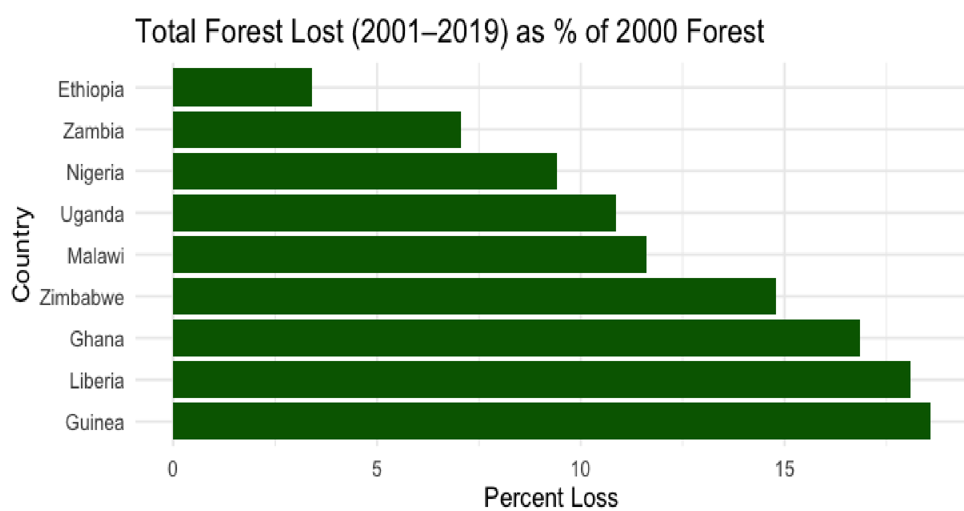


Figure 4. 1: Total Forest Loss as % of 2000 forest cover

Figure 4.1 illustrates the extent of forest loss in the countries analyzed in this study between 2001 and 2019, expressed as a percentage of each country's 2000 forest cover. Rates of deforestation vary substantially across the sample: while Ethiopia experienced relatively limited forest loss (<5%), countries like Guinea, Liberia, and Ghana lost over 15% of their 2000 forest cover.

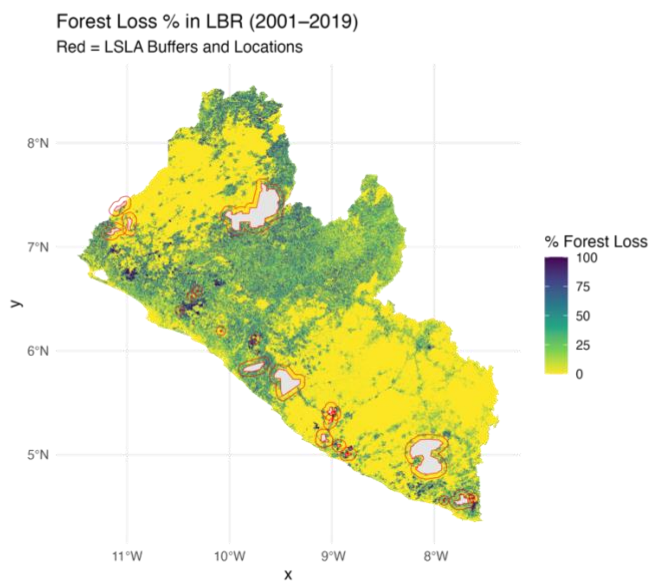


Figure 4. 2: Forest loss and LSLA locations in Liberia (2001-2019)

Figure 4.2 presents a map where loss of forest cover between 2001-2019 in Liberia, is overlaid with the boundaries of LSLAs (implemented over the same time period) shown in red. The dark green areas indicate intense forest cover loss, while yellow areas represent low forest loss.

4.2.2. Malaria in sub-Saharan Africa

Malaria is only spread through the female *Anopheles* mosquito and transmission occurs when the mosquito was previously infected through a blood meal taken from an infected person. The typical lifespan of the female *Anopheles* is 2 weeks, and they can travel distances as far as 2 km. Despite global progress in reducing malaria incidence, the disease continues to be a major public health burden, especially for children under five and pregnant women in sub-Saharan Africa, which accounted for 90% of the estimated 216 million global malaria cases in 2016 (WHO, 2017; WHO, 2018b). Recent studies also highlight the devastating indirect consequences of malaria, such as maternal anemia, low birth weight, perinatal deaths, and long term impairments in children (Menendez, Fleming, and Alonso 2000; Guyatt and Snow 2004).

4.2.3. Deforestation and Malaria Transmission

Deforestation influences malaria transmission through a combination of ecological and economic pathways (Pattanayak & Pfaff, 2009; Garg, 2019). First, numerous studies have demonstrated that forest clearing modifies environmental conditions, such as increased sunlight exposure, elevated air and water temperatures, and changes in surface hydrology, that create favorable breeding habitats for *Anopheles* mosquitoes (Austin, 2013; Burkett-Cadena and Vittor, 2018; Tucker Lima et al. 2017). These conditions accelerate larval development and enhance adult survivorship, thereby increasing vectorial capacity (Afrane et al. 2012; Kilpatrick and Randolph, 2012; Kweka et al. 2016). For instance, field evidence from western Kenya shows that *An. gambiae* productivity is substantially higher in agricultural puddles than in natural forest or wetland environments (Ndenga et al. 2011). Correspondingly, deforested zones have shown elevated densities and biting rates of vectors such as *An. gambiae* and *An. funestus*, both major malaria vectors in sub-Saharan Africa (Janko et al. 2018; Burkett-Cadena and Vittor, 2018).

Second, forest loss is often a byproduct of agricultural expansion. Certain agricultural practices, such as cultivation of crops that require standing water, can create habitats for malaria vectors (Garg, 2019). Third, deforestation leads to increased ground temperatures, conditions that are associated with increased mosquito biting frequency and faster mosquito maturation rates. Fourth, land use changes following deforestation, can facilitate greater human-vector contact. Fifth, biodiversity loss due to deforestation can reduce populations of natural mosquito predators (Pattanayak & Pfaff, 2009). Finally, socio-economic processes accompanying deforestation further reinforce these mechanisms. In tropical countries, forest degradation is frequently intertwined with poverty, as rural households and migrants settle in forest frontiers in pursuit of new agricultural opportunities (Sunderlin et al. 2005; Wunder, 2001). Immigrants to such places may have also limited acquired immunity to local malaria strains and reduced access to healthcare (Baeza et al. 2017; Hahn et al. 2014; Mitchell et al. 2022). This phenomenon, termed “frontier malaria” (de Castro et al. 2006), reflects how socio-ecological transformation exposes vulnerable populations to increased malaria risk. As noted by Wesolowski et al. (2012), this influx can intensify malaria transmission dynamics, not only among migrants but also among long-settled populations in adjacent endemic zones.

However, the mechanism is not-straightforward. Because deforestation intensity doesn’t just proxy for environmental degradation. It can also reflect economic activity on the LSLA, due to clearing land to farm, building infrastructure and employment of workers. So, the coefficient on deforestation combines conceptually opposite channels, ecological and economic. More forest loss may increase breeding sites and therefore higher malaria transmission rates. At the same time, more forest loss may also increase jobs/income, leading to better housing, nets and better healthcare access, potentially reducing malaria incidence. Our coefficient will capture the net of both effects.

A meta-analysis covering 87 mosquito species in 12 countries found that over half become more abundant in deforested settings (Burkett-Cadena and Vittor 2018). These ecological changes disproportionately affect vulnerable populations. Risk is heaviest for groups that lack acquired immunity or face dual exposure, such as very young children and pregnant women, leading to prematurity, low birth-weight, and elevated neonatal deaths (Desai et al. 2007). Beyond short-term health outcomes, early life malaria exposure can have lasting economic consequences, including lower educational attainment, reduced productivity and altered fertility outcomes (Cutler et al. 2010; Lucas 2013). Recent micro-level evidence from rural Nigeria illustrates these dynamics: a one-standard-deviation reduction in local tree cover increases child malaria incidence by more than 5 percent in the following year (Berazneva & Byker, 2024).

4.3. Data

Our pooled dataset spans from 2001 to 2020 and includes all children under five living within 90 kms of an LSLA (LSLA's boundary for polygons and centroid for points). We construct this by combining data from child health records the Demographic and Health Surveys (DHS) with geo-referenced information on LSLAs from the Land Matrix Initiative. Below we describe each data source and the procedure used to spatially match DHS clusters to nearby LSLAs.

4.3.1. Large-Scale land acquisitions

We draw data on the location, timing, and characteristics of land acquisitions from the Land Matrix Initiative (https://landmatrix.org/), an independent global land monitoring project that compiles information on large-scale land deals in developing countries. This is the largest and most comprehensive database on the global land rush and aggregates deal-level information from a wide range of publicly available sources, including government publications, company disclosures, NGO and media reports, and academic studies (Castet, 2024). It provides geo-referenced information on land transactions, including contract size, intended land use, investor origin, and implementation status (e.g. intended, concluded, implemented). Where available, deals are used as polygons; in cases of limited spatial precision, point-level centroids are used to approximate deal locations. The dataset also provides the year in which land rights were formally transferred from local users to investors (year of contract signing). However, in cases where it was not available, we use the year in which the LSLA project became operational (year of implementation). Previous studies have documented that negotiation processes before implementation can itself displace or disadvantage locals (Castet, 2024, Engström, 2018, Borras et al. 2022; Broegaard et al. 2022)

For the purposes of this study, we restrict our attention to concluded deals in order to capture realized impacts of LSLA-induced land use change. We exclude projects that were either abandoned or remained in "intended" status, as well as those lacking sufficient spatial information to locate them reliably. Our sample further focuses on deals that:

i) Cover at least 200 hectares[15], consistent with Land Matrix definitions of large-scale acquisitions; and

ii) Involve a foreign investor.

Applying these criteria, we compile all concluded LSLA deals across nine countries in sub-Saharan Africa: Ethiopia, Ghana, Guinea, Liberia, Malawi, Nigeria, Uganda, Zambia, and Zimbabwe. This dataset serves as the basis for linking land acquisitions to both deforestation outcomes and health outcomes, particularly malaria prevalence, in the empirical analysis that follows.

Figure 4.3 shows the evolution of large-scale land acquisitions (LSLAs) in a sample country (Liberia) across four time periods: 2000, 2005, 2010, and 2015. Red triangles denote LSLA points while polygons indicate LSLA locations where exact polygons were available. The maps illustrate a clear spatial and temporal expansion of LSLA activity, particularly during the mid-2000s to early 2010s, a period often described as the "global land rush".

[15] According to the Economic Commission for Africa, an LSLA refers to an acquisition or use of at least 200 hectares of land (Union, 2014).

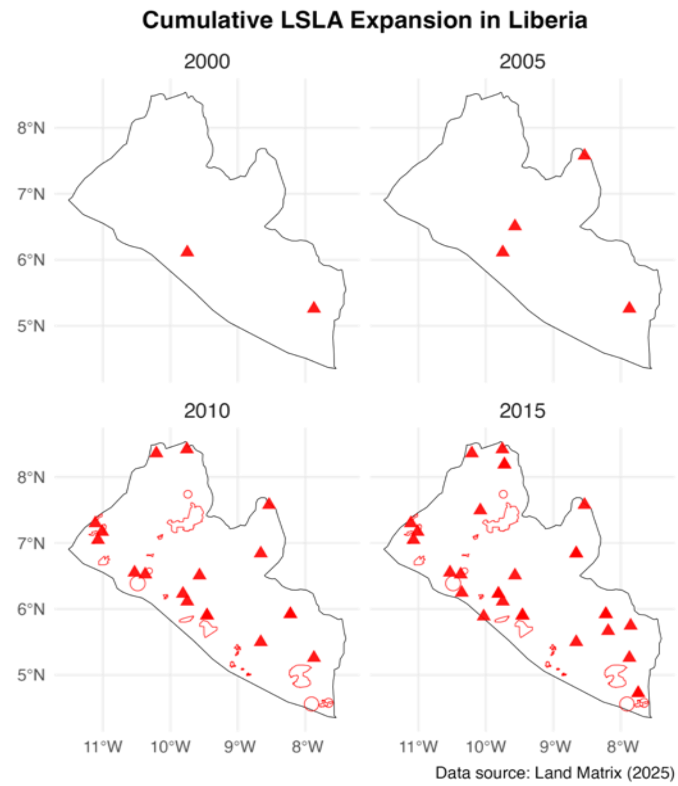


Figure 4. 3: Spatial Expansion of LSLA Sites in Liberia, 2000-2015

4.3.2. Demographic and Health Survey

We use child health records from the Demographic and Health Surveys which are nationally representative repeated cross sectional household surveys conducted at irregular intervals across low- and middle-income countries. The DHS employs a stratified two-stage sampling design, selecting clusters (typically based on enumeration areas) with probability proportional to population size, and then sampling households within clusters.

The DHS contain detailed information on individual- and household-level characteristics, including demographics, education, wealth, and health variables critical to understanding disease outcomes and socioeconomic patterns. For this study, we focus on children under age five and extract data on fever incidence[16] (a commonly used proxy for malaria), cough and diarrhea incidence, as well as individual- and household level covariates such as demographics (age and sex of the child and household head, size of the household), maternal education, urban or rural status, migration, and household wealth. To protect respondent anonymity, the DHS randomly displaces cluster coordinates by five to ten kilometers from their actual locations. This displacement prevents us from reliably identifying whether the same clusters (or villages) are resampled over time, making it impossible to construct a cluster-level pseudo panel dataset. We use survey rounds conducted between 2001 and 2020 that contain geo-coded cluster coordinates. Table C.1 lists all countries and survey rounds used in the analysis.

We spatially link each DHS cluster to the nearest LSLA recorded in the Land Matrix dataset. For clusters near LSLAs with incomplete establishment dates or unclear operational status, we follow prior studies and exclude those observations within a 20 km radius, reducing potential contamination from ambiguous exposure (Anti, 2021). For each remaining cluster, we calculate the geodesic distance to the nearest LSLA polygon boundary, or to the centroid when polygon data are unavailable. This spatial merging allows us to analyze how proximity to LSLA areas shapes child-level health outcomes, across the 9 country sample.

4.3.3. Deforestation data

To quantify environmental changes associated with large-scale land acquisitions (LSLAs), we construct a deforestation exposure variable that measures how much forest loss has occurred on or around LSLA lands since the onset of each project.

Forest loss is derived from the Hansen Global Forest Change dataset (Hansen et al. 2013), which provides annual, high-resolution (30 m) global maps of forest loss from 2001 onward. Each

[16] Fever incidence is recorded in the DHS based on responses to whether the child under the age of five experienced a fever in the two weeks preceding the survey.

pixel therefore contains the year in which forest loss occurred, enabling us to construct annual and cumulative deforestation patterns.

For each LSLA, we calculate the total area of forest loss occurring within its spatial footprint or within its immediate surroundings. When the LSLA is represented as a polygon, forest loss is measured directly within its boundary; when only a point location is available, a 10-kilometer radius buffer is constructed around the LSLA's reported coordinates.

To focus on deforestation that is plausibly related to LSLA activity, we consider only forest loss occurring after the year in which the LSLA became active. Cumulative forest loss is then computed since the year of LSLA implantation to the year of survey. This cumulative measure is subsequently linked to households in the DHS based on spatial proximity, such that, each DHS cluster is assigned the deforestation exposure of its nearest LSLA.

4.4. Identification Strategy

4.4.1. Defining Treatment and Control Groups

To identify the causal effect of LSLAs on local malaria prevalence, we construct a spatially and temporally explicit treatment variable that links each DHS survey cluster to nearby LSLAs based on geodesic distance and implementation timing.

Each DHS cluster is treated as a geographic point representing the centroid of the households surveyed in that cluster. From each cluster, we compute the distance to all implemented LSLAs located in the same country, using both polygon and point geometries from the Land Matrix dataset. When the LSLA is represented as a polygon, the distance is measured from the cluster point to the closest boundary of the LSLA; if the cluster falls inside the polygon, the distance is recorded as zero. For point-based LSLAs, the distance corresponds to the straight-line distance between the two coordinates.

We define the 10-km treatment band as our main exposure window. The assumption is that LSLA impacts attenuate with distance, households located closer to LSLA areas would experience stronger effects of land-use and environmental change. A DHS cluster is classified as treated if at least one LSLA was implemented within 10 kilometers of its location. In practice, this means that the shortest distance from the cluster point to any LSLA boundary (or LSLA point) is less than or equal to 10 km. Among all LSLAs meeting this criterion, the cluster is assigned the earliest implementation year as its treatment year. This year marks the first point in time when the cluster became exposed to treatment. For instance, if a cluster lies 8 km from an LSLA implemented in 2005 and 5 km from another implemented in 2010, its treatment year will be 2005 (the earlier LSLA within 10kms). Finally, we restrict the analysis sample to clusters located within 90 kms of any LSLA to ensure comparability across treated and untreated areas.

Clusters located 20 km or more from the nearest LSLA are controls. Clusters situated between 10 and 20 km from an LSLA are excluded from the main specification to maintain a clear spatial separation between treated and control areas and to minimize contamination effects at the boundary.

Figure 4.4 displays the spatial overlap between the DHS cluster locations (blue points) and LSLA sites (red outlines and triangles) in a sample country (Liberia).

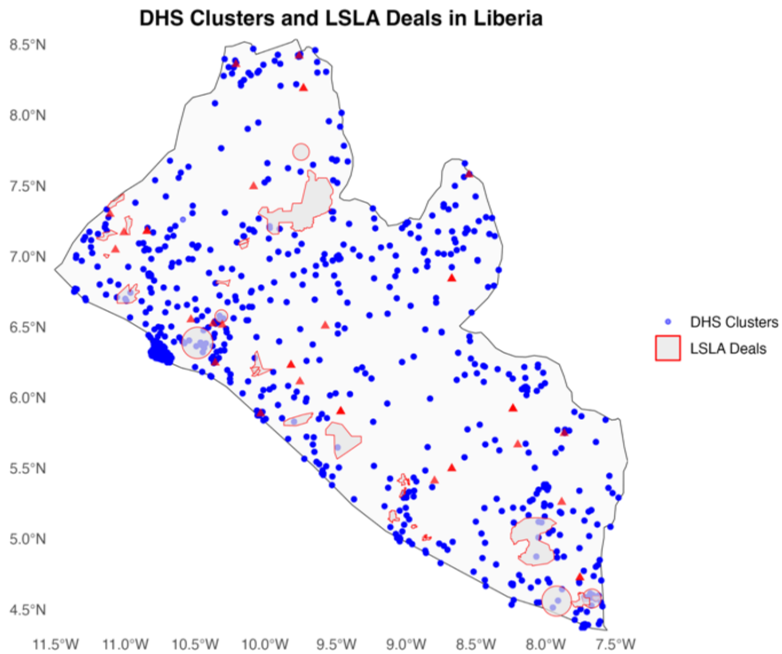


Figure 4. 4: Spatial Distribution of DHS clusters (blue) and LSLA sites (red) in Liberia

4.4.2. Estimation

We have a multi-period staggered rollout design. While standard fixed effect estimators have been used extensively in settings with staggered treatment, recent literature has highlighted that such designs can yield biased estimates when treatment occurs in multiple units in different time periods (Goodman-Bacon 2021). In our setting, different units start treatment in different years. And TWFE would most likely mix comparisons across cohorts and can assign negative weights (Goodman-Bacon 2021). To address this concern, we implement the "extended two-way fixed effects" (ETWFE) framework introduced by Wooldridge (2021). This estimator is well suited for settings with staggered treatment timing and is presented as an alternative to Callaway and Sant' Anna (2021).

The ETWFE model provides a solution to the heterogeneous treatment effects problem by fully saturating the model with treatment-cohort-year fixed effects. We follow the methodology adopted by Anti and Zhang (2023), and estimate the following model using ETWFE:

$$\mathrm{Y_{itc}} = \beta_0 + \beta_1 \mathrm{LSLA_c} + \Sigma_j \Sigma_k \beta_{jk} \mathrm{LSLA_c} \times \mathrm{After_{ct}} \times \mathrm{Cohort_j} \times \mathrm{Year_k} + \mathrm{X_{it}}$$
$$+ \mathrm{FE_t} + \mathrm{FE_{month}} + \mathrm{FE_{district}} + \mathrm{FE_{region * year * month}}$$
$$+ \varepsilon_{itc} \qquad \text{Equation (5)}$$

Where $\mathrm{Y_{itc}}$ is the binary health outcome for child i in cluster c and time t (year of survey). The model focuses on three outcome variables drawn from the DHS, including i) child fever incidence as a proxy for malaria incidence (measured by whether the child has reported fever in the two weeks prior to the survey); and ii) child cough incidence and diarrhea incidence used as placebo outcome variables. $\mathrm{LSLA_c}$ is a binary variable equal to one if the cluster in which the child is located is in the treated group, i.e. within 10 kms of atleast one LSLA between 2001 and 2019. $\mathrm{After_{ct}}$ is a binary variable equal to one if the cluster was surveyed after the implementation of LSLA. The variable $\mathrm{X_{it}}$ includes a set of individual and household level covariates such as demographics (age and sex of the child and household head, size of the household), maternal education, urban or rural status.

Let j be the treatment cohort and k as the year of survey, then the ETWFE model then estimates the coefficient of interest, β_{jk}, allowing for differential effects across both dimensions. Aggregated treatment effects on the treated (ATTs) are derived as a weighted average of all the cohort-year effects. We also include district fixed effects[17] to account for any unobserved time-invariant differences at the district level; month fixed effects to account for differences due to seasonality;

[17] These districts correspond to administrative Level-2 units from the Global Administrative Areas (GADM) database.

survey year fixed effects to account for year common specific shocks affecting all households and region-month-year interaction[18] fixed effects to account for unobserved regional level seasonal and annual variation that may affect the incidence of fever and other diseases. Standard errors are clustered at the DHS cluster level.

Further, to establish deforestation as the mechanism through with LSLA affects fever incidence, we extend Equation 5, by adding an interaction term capturing deforestation intensity on or around each LSLA. Specifically, we estimate the following:

$$
\begin{aligned}
\mathrm{Y_{itc}} = \beta_0 + \beta_1\,\mathrm{LSLA_c} \\
&+ \Sigma_j \Sigma_k\, \beta_{jk}\, \mathrm{LSLA_c} \times \mathrm{After_{ct}} \times \mathrm{Deforestation_Intensity_{ct}} \times \mathrm{Cohort_j} \times \mathrm{Year_k} \\
&+ \mathrm{X_{it}} + \mathrm{FE_t} + \mathrm{FE_{month}} + \mathrm{FE_{district}} + \mathrm{FE_{region*year*month}} \\
&+ \varepsilon_{itc} \qquad \text{Equation (6)}
\end{aligned}
$$

The deforestation intensity variable is calculated as the cumulative deforestation in hectares since the year of LSLA implementation up until the year of survey. This is because only the deforestation after LSLA should be attributed to it. This measure is then attached to the DHS cluster nearest to the LSLA.

4.4.2 Parallel Trends Assumption

The validity of our estimation lies on the satisfaction of the parallel trends assumption, which requires that, in the absence of LSLAs, malaria outcomes in treated and control groups would have evolved similarly over time. This would imply that any post-treatment divergence in fever prevalence can be attributed to LSLA implementation rather than pre-existing trends.

To test this assumption, we first present trends in the raw data using locally weighted (Lowess) smoothing (Figure 4.5), following the approach used by Benshaul-Tolonen (2024). This helps us

[18] Here regions correspond to Level-1 administrative units downloaded from GADM.

visualize pre-treatment trends in fever prevalence across treated and control groups. Visual inspection of the patterns in fever prevalence supports that both groups followed comparable trends prior to LSLA implementation. A notable divergence appears slightly before year 0, which may be marked by preparatory processes before LSLA implementation. To address potential contamination from these early effects, we omit observations from one year immediately prior to LSLA implementation.

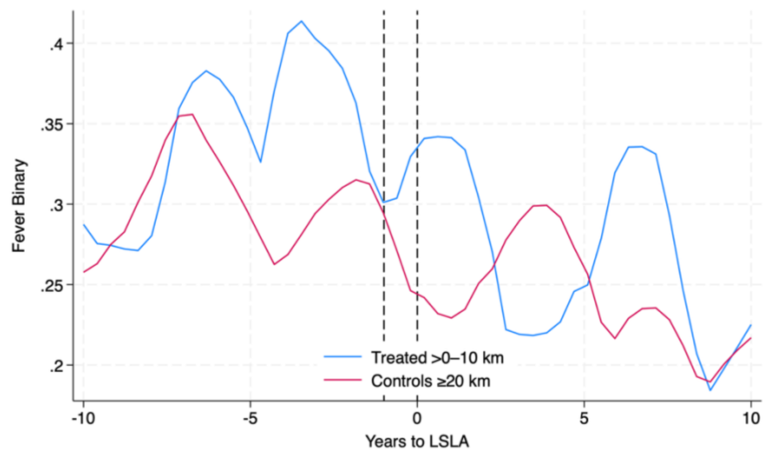


Figure 4. 5: Nonparametric trend illustration in Fever incidence using the Lowess smoother. Time to treatment for control groups is defined by the first year the nearest LSLA became operational beyond 20 kms.

Second, to further assess potential pre-trends, we extend Equation 5 to estimate an event study specification with leads and lags relative to the year of LSLA implementation (Figure 4.6). Each coefficient represents the difference between treated and control groups at a given time relative to the LSLA. The coefficients for years preceding the LSLA are statistically insignificant, indicating no significant divergence before treatment. The estimated post-LSLA coefficients show an immediate positive increase in fever prevalence in the first year after implementation, which attenuates in later years, suggesting only a short term disruption caused by LSLAs.

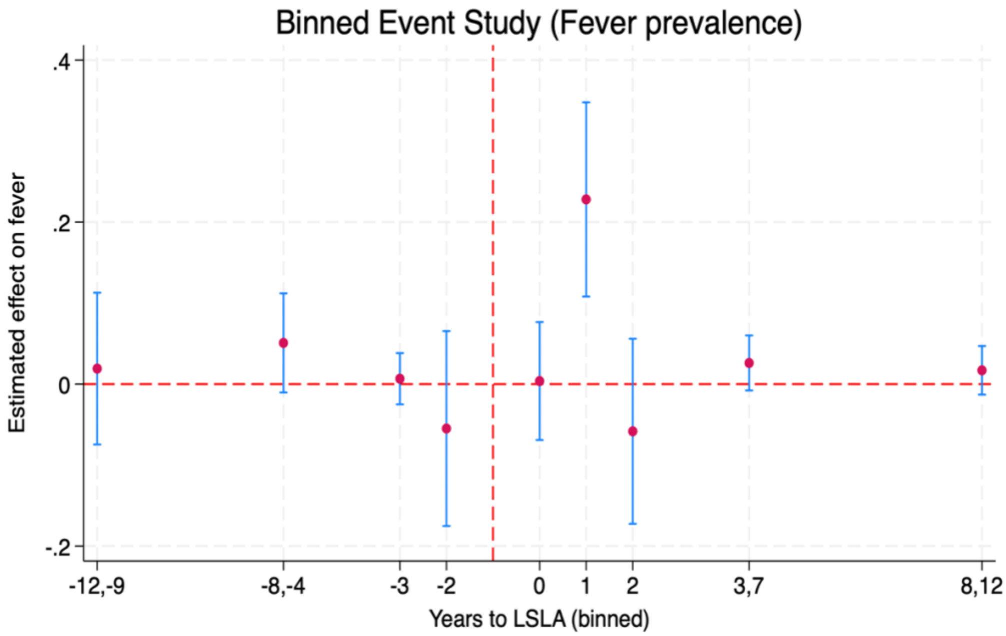


Figure 4. 6: Event Study (Fever Prevalence)

Finally, following Benshaul-Tolonen (2024), and Anti and Zhang (2023) we examine the new harmonized nightlights intensity (from Li et al. 2020) as an indicator of local economic activity or infrastructure development. Parallel trends in nightlights (Figure 4.7) between the treated and control groups prior to LSLAs onset provide additional confidence that both groups were evolving similarly in terms of economic development, and that LSLA placement was not systematically driven by pre-existing economic differences. Collectively, these checks support the validity of the parallel trends assumption in this setting.

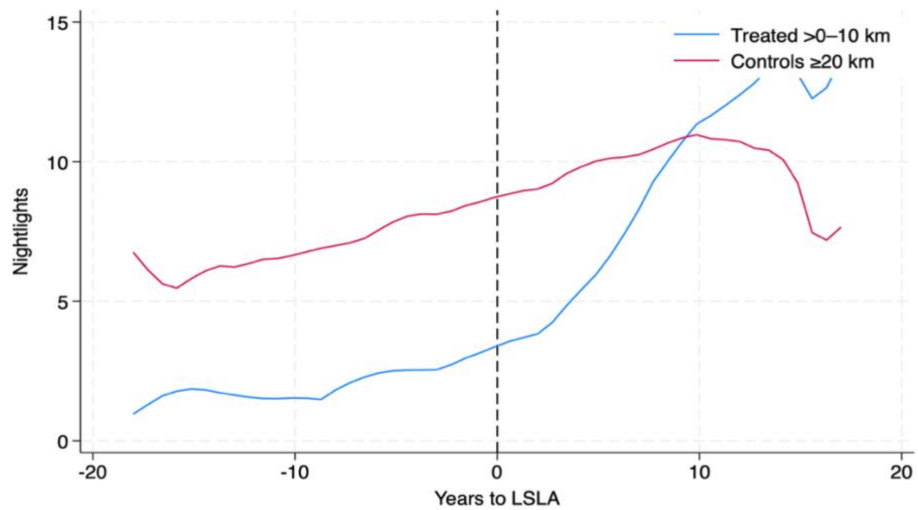


Figure 4. 7: Nonparametric trend illustration in Nightlights using the Lowess smoother. Time to treatment for control groups is defined by the first year the nearest LSLA became operational beyond 20 kms.

4.5. Results

Table 4.1 presents the estimated average treatment effects of LSLA exposure on three child health outcomes: fever, cough and diarrhea, based on equation 5. The results indicate a statistically significant increase in fever incidence for children living close to LSLA. However, we find no significant effect of LSLA on the other two disease outcomes.

Specifically, living within 10 kms of an LSLA increases the probability that a child had a fever in the last two by 5.7 percentage points (p.p.). This represents an increase of roughly 21 percent relative to the control group mean of 0.273. In contrast, the estimated effects for cough and diarrhea are small and statistically insignificant. This suggests that the observed health impacts of malaria operate primarily through malaria transmission rather than through general morbidity.

Table 4. 1: ATT Estimates on the Effect of LSLA on child symptoms

	(1) FEVER	(2) Cough	(3) Diarrhea
ATT (simple)	0.057	0.007	0.016
SE	0.020	0.023	0.022
p-value	0.005	0.760	0.476
CI low	0.018	-0.038	-0.027
CI high	0.097	0.052	0.058
N	109303	109068	108968
Clusters	6539	6539	6539
Control mean	0.273	0.281	0.172

FE: District; Month; Year; Region×Year×Month. Standard errors clustered at cluster level.

However, there could also be several potential mechanisms through with LSLA may increase malaria. One potential mechanism is that LSLAs may alter local environmental conditions through land clearing (deforestation) which may increase mosquito breeding habitats close to LSLAs. To examine whether LSLA induced deforestation explains the rise in malaria incidence for the treated group, we use data from Hansen et al. (2013) to calculate cumulative forest loss - defined as "how much forest loss occurred in or around the LSLA land since the LSLA became active" and estimate Equation 6. We categorize deforestation exposure intensity into three bins ($0 < \text{dose} \leq 0.25$ (i.e. clusters near LSLAs with low deforestation intensity); $0.25 < \text{dose} \leq 0.50$ (medium); and $\text{dose} > 0.50$ (high)). Results presented in Table 4.2 are consistent with results from ecology literature (Alroy 2017; Brock et al. 2019). Our results are statistically significant and positive across all bins, but the magnitude declines with higher deforestation intensity. Moderate or partial deforestation tends to increase malaria risk more heavily by expanding forest edges and creating habitats ideal for *Anopheles* mosquito breeding, while extensive forest clearing may eventually destroy these habitats or reduce vector abundance thereby having a lower increase in malaria risk.

Table 4. 2: Estimated Effect on Fever by Deforestation Exposure Intensity

Dose Intensity Bin	Coefficient	Std. Error	95% CI
$0 < \text{dose01} \leq 0.25$	0.144	0.052	[0.042, 0.247]
$0.25 < \text{dose01} \leq 0.50$	0.168	0.064	[0.042, 0.293]
$\text{dose01} > 0.50$	0.081	0.040	[0.003, 0.159]

Notes: Each row reports ATT estimates from `estat simple, asis` after `jwdid`.
Dependent var: Fever (binary). Include: district FE; month FE; region×year×month FE. Standard errors clustered at cluster level.

A second potential mechanism driving the effect of LSLA on malaria could be employment or wealth. This is because the increase in malaria incidence for the treated group after an LSLA could simply be due to decline in income for the nearby communities which reduce access to healthcare (or preventative measures such as bed nets), thereby increasing malaria risk. However, the DHS does not collect information on income. Instead, following Castet (2024), we examine three indirect indicators of household economic conditions as outcome variables: i) employment status of the woman; ii) the household wealth index; and iii) nightlights.

Formally, we run the following three specifications:

$$\begin{aligned}\text{Employment}_{itc} &= \beta_0 + \beta_1 \text{LSLA}_c + \Sigma_j \Sigma_k\, \beta_{jk}\, \text{LSLA}_c \times \text{After}_{ct} \times \text{Cohort}_j \times \text{Year}_k + X_{it} \\ &\quad + \text{FE}_t + \text{FE}_{month} + \text{FE}_{district} + \text{FE}_{region*year*month} \\ &\quad + \varepsilon_{itc} \qquad \text{Equation (7)}\end{aligned}$$

$$\begin{aligned}\text{Wealth}_{itc} &= \beta_0 + \beta_1 \text{LSLA}_c + \Sigma_j \Sigma_k\, \beta_{jk}\, \text{LSLA}_c \times \text{After}_{ct} \times \text{Cohort}_j \times \text{Year}_k + X_{it} + \text{FE}_t \\ &\quad + \text{FE}_{month} + \text{FE}_{district} + \text{FE}_{region*year*month} \\ &\quad + \varepsilon_{itc} \qquad \text{Equation (8)}\end{aligned}$$

$$\begin{aligned}\text{Nightlights}_{tc} &= \beta_0 + \beta_1 \text{LSLA}_c + \Sigma_j \Sigma_k\, \beta_{jk}\, \text{LSLA}_c \times \text{After}_{ct} \times \text{Cohort}_j \times \text{Year}_k + X_{it} \\ &\quad + \text{FE}_t + \text{FE}_{month} + \text{FE}_{district} + \text{FE}_{region*year*month} \\ &\quad + \varepsilon_{tc} \qquad \text{Equation (9)}\end{aligned}$$

where, work status is a binary variable equal to 1 if the woman respondent reports being employed currently. Wealth index is the DHS household wealth index, based on ownership of assets and Nightlights are a proxy for local economic activity in cluster c, at time t.

Table 4. 3: ATT Estimates on the Effect of LSLA on Employment, Wealth and Nightlights

	(1) Employment	(2) WEALTH	(3) Nightlights
ATT (simple)	0.007	-0.139	-0.209
SE	0.034	0.103	0.271
p-value	0.833	0.176	0.440
CI low	-0.060	-0.340	-0.740
CI high	0.075	0.062	0.322
N	109097	109303	101421
Clusters	6539	6539	5905
Control mean	0.647	3.089	0.944

FE: District; Month; Year; Region×Year×Month. SE clustered at cluster level.

Table 4.3 presents the results for employment and wealth/nightlights as outcomes. We find no significant effect of LSLA exposure on either employment status of women or on the household wealth index and nightlights. These patterns are inconsistent with an income-driven mechanism to explain the effect of LSLA on malaria incidence. In Table C.3 we also presents results re-estimating our baseline equation 5 adding Nightlights as a control variable, to investigate if nightlights (as a proxy for local economic activity) mediates the total effect of LSLA on fever. This is because part of the LSLA effect on fever might operate through reduced income (poorer households would be more vulnerable). Essentially we are asking, among the places that have the same level of nightlights, what is the remaining effect of LSLA on fever. Results in Table C.3 are very similar to the ones reported in Table 4.1, suggesting that controlling for nightlights does not attenuate the effect.

Further, Figure C.1, we also test whether the effect of LSLA on fever vary by wealth index. If economic prosperity (as measured by the wealth index) were the mechanism that explained the increase in fever due to LSLA, then the effects should be concentrated among the poorer households. Our results suggest that on average, being within 10 km of an LSLA increased the probability of fever by 7.3 pp among the poorest households and about 7 pp among the richest households, but they are not statistically different from each other.

A third possible driver of our result could be due to migration. It is likely that people migrating close to LSLAs for work opportunities are more vulnerable to the disease due to lack of acquired immunity. To test if migration could be causing the effect, we re-estimate our baseline Equation 5, restricting the sample to non-migrant households only, defined as those who have lived in their current residence for the last 5 years since the year of survey. The estimated coefficients (Table 4.4) are nearly identical to that in the full sample, indicating that the results are not driven by migration patterns.

Altogether, our findings suggest that environmental change (such as deforestation) induced by LSLAs, rather than migration, or income, are the primary channel linking LSLAs to malaria incidence.

Table 4. 4: ATT Estimates on the Effect of LSLA on child symptoms (for Non-migrants)

	(1) FEVER	(2) Cough	(3) Diarrhea
ATT (simple)	0.056	0.014	0.033
SE	0.020	0.024	0.020
p-value	0.004	0.572	0.094
CI low	0.018	-0.033	-0.006
CI high	0.094	0.061	0.071
N	61289	61182	61163
Clusters	5075	5075	5075
Control mean	0.281	0.277	0.167

FE: District; Month; Year; Region×Year×Month. Standard errors clustered at cluster level.

4.6. Conclusion

This paper provides new causal evidence on how LSLAs shape disease risk in sub-Saharan Africa by combining geocoded household surveys, remote sensing forest loss data, and detailed information on the timing and location of land deals. By exploiting the staggered rollout of LSLAs across nine countries and comparing households located within 10 km of an LSLA to those 20 to 90 kms away, we implement a multi-period spatial difference-in-differences design using Wooldridge's (2021) extended two-way fixed effects (ETWFE) framework. ETWFE corrects for the well-known biases that arise in staggered treatment settings under standard TWFE estimation.

We find that exposure to LSLA significantly increases the probability of fever incidence for children under five. The effect is specific to malaria related symptoms: we find no change in cough or diarrhea. We also rule out alternative explanations. We find no effect of LSLAs on women's employment, household wealth, or nightlights, three proxies of economic well-being. The results are also unchanged when restricting the sample to non-migrant households, suggesting that the effects are not driven by temporary workers with lower immunity moving toward LSLA sites.

Taken together, these findings point to environmental change, particularly deforestation, rather than migration or income shocks as the primary channel linking LSLAs to malaria.

This study provides the first multi-country evidence on the unintended public-health spillovers of LSLAs in Africa. The results highlight an important trade-off: policies aimed at promoting agricultural investment and rural development can unintentionally increase disease risk for nearby communities. These findings highlight the need for policymakers to integrate public health considerations into decisions about large-scale agricultural expansion.

CHAPTER 5

CONCLUSIONS

This dissertation examines how environmental shocks shape household behavior and outcomes in the Global South, particularly in data poor-contexts where reliable information is scarce. Environmental change, whether through climate variability or human-induced deforestation, has become a defining feature of development in many low income regions. Yet the ways in which these shocks affect livelihoods, health and adaptation remain insufficiently understood. The chapters in this dissertation integrate remote sensing with micro-level household data to estimate the socioeconomic and health impacts of environmental change.

Chapter 2 addresses the challenge of lack of reliable subnational data. It tests whether newly harmonized nightlights (NTL) data can serve as accurate proxies for subnational economic and human development outcomes in sub-Saharan Africa. The findings confirmed that NTL capture significant within-country variation in household wealth, especially in urban areas.

Chapters 3 and 4 move beyond measurement to identify causal effects of environmental change on household welfare. Chapter 3 asked how rural households respond when rainfall variability threatens their livelihoods. Combining quarterly household-level income records with objective climate measures (SPEI), we construct a unique panel dataset combining nearly 8,000 households across 24 countries. This allowed us to exploit within-household variation over time and isolate the causal effects of rainfall shocks on household adaptation strategies. Our results suggest that droughts raise both the level of bushmeat income and its share in total income, with the largest effects during the growing season when crop revenues are most vulnerable; moderate wet episodes generally reduce hunting. However, wet events have the opposite effect. These findings suggest that bushmeat serves as a safety net, consistent with predictions from agricultural household models under incomplete markets. Because timing matters, interventions aligned with crop calendars (e.g. employment during drought-sensitive windows) are likely to be most effective.

Chapter 4 examines a different but related question: how do large scale environmental shocks, such as deforestation driven by large scale land acquisitions (LSLAs) affect household health? This is because clearing of forests has been linked to increase in malaria incidence (through creation of ecological conditions ideal for malaria mosquito vector). To identify these health impacts, we combine high-resolution forest-loss rasters with geo-referenced DHS and detailed spatial and temporal information on LSLAs. Because the LSLAs are implemented at different times across locations, the empirical design employs a staggered differences in differences framework using Wooldridge's extended two way fixed effects estimator rather than the conventional TWFE approach.

Together, this dissertation provides an understanding of three topics, from measuring economic development where administrative data are absent, to examining how households adapt to climatic shocks that threaten livelihoods, to identifying the health consequences of large-scale land-use change.

APPENDIX A

ESSAY ONE

Supplemental Information

Harmonised Nightlights Data

The DMSP NTL data is a time series dataset with a spatial resolution of 30 arc-seconds. However, the raw DMSP NTL data is not comparable across years. To overcome this limitation, a stepwise calibration approach was used (Li et al. 2020) to generate a temporally consistent NTL dataset, which outperforms traditional approaches in terms of temporal trends and correlation with electricity consumption data. In contrast, the VIIRS Day/Night Band (DNB) data provides radiance records with improved radiometric resolution and a higher spatial resolution of 15 arc-seconds. The monthly VIIRS NTL data were further preprocessed and composited as annual time series data. The dataset spans from 2012 to 2020.

To generate the harmonized series from 1992 to 2020, a three-step framework was employed (Li et al. 2020). Firstly, annual VIIRS NTL data was produced from monthly observations and noise from temporary lights was excluded. Secondly, the relationship between processed VIIRS data and DMSP NTL data in 2013 was quantified using a sigmoid function. Thirdly, the derived relationship was applied globally to obtain DMSP-like data from VIIRS. The consistent NTL dataset was generated by integrating the temporally calibrated DMSP NTL data from 1992 to 2013 and DMSP-like NTL data from VIIRS from 2014 to 2020 (Li et al. 2020).

Literature Review

A few previous studies in economics have assessed the accuracy of NTL data in predicting GDP at various levels, such as national, regional and sub-regional levels (Chen and Nordhaus, 2011; Henderson et al. 2012; Keola et al. 2015; Hodler et al. 2014) or to evaluate the accuracy of official national account statistics (Clark et al. 2020; Pinkovskiy et al. 2016). Some studies have found that NTL can predict wealth indicators (Weidmann and Schutte, 2017) as well as human development outcomes at the local level created from DHS data on education, health and wealth (Bruederle and Hodler, 2018). However, these studies have all used DMSP data. A parallel literature in remote sensing and artificial intelligence uses a combination of these alternative data sources and computer vision models to predict estimates of inequality or wealth.

For example, one study (Jean et al. 2016) trained a convolutional neural network (CNN) with high-resolution satellite imagery to predict local economic output for 5 countries using a transfer-learning approach. They did so by combining information from an image classification dataset trained to predict NTL corresponding to daytime imagery and information from the DHS and Living Standard Measurement Survey (LSMS). Since then, there have been several studies (Head et al. 2017; Blumenstock et al. 2015) in this direction. Another study (Kondmann and Zhu, 2020) adopted a similar approach but tested whether it could be used to measure changes in poverty over time. However, these methods do not come without limitations. According to a survey conducted by the Asian Development Bank (ADB) and the United Nations Economic and Social Commission for Asia and the Pacific, incorporating big data into their programs is a challenge for 7 out of 16 National Statistical Offices in the ADB member countries due to difficulties in accessing these alternative data sources (Hofer et al. 2020). Acquiring high-resolution satellite images is expensive, and when combined with the computer-intensive deep learning methods required in their analysis, widespread adoption becomes challenging. It is especially difficult for development organizations grappling with limited resources. At the same time, these deep learning models are considered “black-boxes” in the sense that it is difficult for policy makers to understand and apply them to solving real world problems (Ledesma et al. 2020; Han et al. 2020). They are often criticized for attempting to only maximize model performance rather than model interpretability which is especially important when developing policies and interventions that have the potential of greatly affecting people’s well-being (Ayush et al. 2020). For instance, a recent review (Hall et al. 2023) of 32 papers on wealth/poverty prediction that used satellite images as one of their inputs and deep neural networks as their method to predict survey data, finds that almost all literature conducted so far does not meet the requirements for interpretability and explainability, things that are important for wider acceptance/adoption of the research in the development community. They establish specific criteria for interpretability and explainability in such studies. These include emphasizing understandable descriptions of the model’s properties, potentially using tools like heat maps for complex models to illustrate what the model reacts to.; and detailed explanations for predictions, addressing why certain areas are classified as poor or not and exploring what changes in the satellite data would be needed for an area to transition from being poor to not so poor. These criteria aim to enhance the acceptance and adoption of research findings, particularly within the development community. They note that almost all research conducted has been by the “technical community” and therefore domain knowledge is mostly missing and should be integrated in future research. Further, model outputs from these studies are rarely made publicly available as rasters, data frames or other data products that would be easy to incorporate into studies of the influence of poverty and household income by non-experts in remote sensing.

Table A.1: List of countries and DHS waves in our sample

Country	DHS survey years
Angola	2015
Benin	2012, 2017
Burkina Faso	2010
Burundi	2010, 2016
Cameroon	2004, 2011, 2018
Chad	2014
Comoros	2012
Congo Democratic Republic	2007, 2013
Cote d'Ivoire	2012
Egypt	2005, 2008, 2014
Eswatini	2006
Ethiopia	2005, 2010, 2016, 2019
Gabon	2012
Gambia	2019
Ghana	2008, 2014
Guinea	2005, 2012, 2018
Kenya	2008, 2014
Lesotho	2004, 2009, 2014
Liberia	2007, 2013, 2019
Madagascar	2008
Malawi	2004, 2010, 2015
Mali	2006, 2012, 2018
Mozambique	2011
Namibia	2006, 2013
Niger	2012
Nigeria	2008, 2013, 2018
Rwanda	2005, 2008, 2010, 2014, 2019
Sierra Leone	2008, 2013, 2019
South Africa	2017
Tanzania	2010, 2015
Togo	2013
Uganda	2006, 2011, 2016
Zambia	2007, 2013, 2018
Zimbabwe	2005, 2010, 2015

Table A.2: OLS Regression for GDP per capita using country fixed effects

	Gross Domestic Product per capita			
	(1)	**(2)**	**(3)**	**(4)**
Nightlights		0.041***		0.048***
		(0.001)		(0.002)
Population Density			0.021***	-0.007***
			(0.001)	(0.001)
Constant	9.538***	9.435***	9.441***	9.452***
	(0.00000)	(0.005)	(0.005)	(0.006)
Fixed effects (Country)	Yes	Yes	Yes	Yes
OOS R^2	0.918	0.923	0.920	0.923
Adjusted R^2	0.918	0.923	0.920	0.923
Residual Std. Error	0.247	0.240	0.245	0.240

Notes: Dependent variable is the Gross Domestic Product per capita. Gross Domestic Product per capita, Nightlights, and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the country level. All models have country fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Gross Domestic Product per capita data from Kummu et al. (2018). For more details, see Table 2.1.

Table A.3: OLS Regression for HDI using country fixed effects

	Human Development Index			
	(1)	**(2)**	**(3)**	**(4)**
Nightlights		0.002***		0.004***
		(0.0001)		(0.0002)
Population Density			0.0003***	-0.002***
			(0.0001)	(0.0001)
Constant	0.533***	0.527***	0.532***	0.533***
	(0.0002)	(0.0005)	(0.0005)	(0.001)
Fixed effects (Country)	Yes	Yes	Yes	Yes

OOS R^2	0.921	0.923	0.921	0.924
Adjusted R^2	0.921	0.923	0.921	0.924
Residual Std. Error	0.026	0.026	0.026	0.026

Notes: Dependent variable is the Human Development Index. Nightlights and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the country level. All models have country fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Human Development Index (HDI) from Kummu et al. (2018). For more details, see Table 2.1.

Table A.4: OLS regression for GDP per capita with year fixed effects

	Gross Domestic Product per capita		
	(1)	**(2)**	**(3)**
Nightlights	0.224***		0.344***
	(0.003)		(0.003)
Population Density		0.062***	-0.145***
		(0.003)	(0.003)
Fixed effects (year)	Yes	Yes	Yes
OOS R^2	0.393	0.224	0.448
Adjusted R^2	0.393	0.224	0.448
Residual Std. Error	0.675	0.765	0.642

Notes: Dependent variable is the Gross Domestic Product per capita. Gross Domestic Product per capita, Nightlights, and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the year level. All models have year fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Gross Domestic Product per capita data from Kummu et al. (2018). For more details, see Table 2.1.

Table A.5: OLS regression for HDI with year fixed effects

	Human Development Index		
	(1)	**(2)**	**(3)**
Nightlights	0.023***		0.032***
	(0.0003)		(0.0004)
Population Density		0.009***	-0.011***
		(0.0003)	(0.0004)
Fixed effects (year)	Yes	Yes	Yes
OOS R^2	0.432	0.294	0.456
Adjusted R^2	0.432	0.294	0.455

Residual Std. Error	0.071	0.080	0.070

Notes: Dependent variable is the Human Development Index. Nightlights and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors (in parentheses) are clustered at the year level. All models have year fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Human Development Index (HDI) from Kummu et al. (2018). For more details, see Table 2.1.

Table A.6: OLS regression for GDP. No fixed effects

	Gross Domestic Product per capita		
	(1)	**(2)**	**(3)**
Nightlights	0.255***		0.379***
	(0.003)		(0.004)
Population Density		0.083***	-0.154***
		(0.003)	(0.003)
Fixed effects	No	No	No
OOS R^2	0.283	0.047	0.341
Adjusted R^2	0.286	0.039	0.352
Residual Std. Error	0.732	0.849	0.697

Notes: Gross Domestic Product per capita, Nightlights, and Population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors are in parentheses. These results don't have fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Gross Domestic Product per capita data from Kummu et al. (2018). For more details, see Table 2.1.

Table A.7: OLS regression for HDI. No fixed effects

	Human Development Index		
	(1)	**(2)**	**(3)**
Nightlights	0.029***		0.038***
	(0.0003)		(0.0004)
Population Density		0.012***	-0.012***
		(0.0003)	(0.0003)
Fixed effects	No	No	No
OOS R^2	0.302	0.071	0.333
Adjusted R^2	0.302	0.071	0.333
Residual Std. Error	0.079	0.091	0.077

Notes: Dependent variable is the Human Development Index. Nightlights and Population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors are in parentheses. These results don't have fixed effects. *p<0.1; **p<0.05; ***p<0.01. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4, and Human Development Index (HDI) from Kummu et al. (2018). For more details, see Table 2.1.

Table A.8: Results from the spatial model with country and year fixed effects.

	Wealth Index		
	(1)	**(2)**	**(3)**
Nightlights	0.574***		0.475***
	(0.005)		(0.008)
Population Density		0.386***	0.105***
		(0.005)	(0.006)
Fixed effects (Country and year)	Yes	Yes	Yes
OOS R^2	0.478	0.351	0.489
Adjusted R^2	0.478	0.351	0.489
Residual Std. Error	0.699	0.872	0.684

Notes: Dependent variable is the DHS mean wealth index. Nightlights and population density have been transformed using the inverse hyperbolic sine (IHS) transformation. Standard errors are in parentheses. All models have country and year fixed effects and account for spatial auto-correlation. *p<0.1; **p<0.05; ***p<0.01. Household Wealth Index data is derived from DHS. Nighttime lights data is derived from Li et al. (2020). Population density is sourced from the GPWv4. For more details, see Table 2.1

APPENDIX B

ESSAY TWO

Literature Review

1. On the differential effect of droughts and floods

In their analysis of the effects of climate variability on land ownership in Uganda, Murken et al. (2024) provide evidence that households that experienced drought conditions during the previous growing season are significantly less likely to express a willingness to acquire full ownership rights for their land. Conversely, exposure to wet conditions during the same timeframe appears to initially increase the willingness of households to invest in land ownership. They posit that while excessive moisture can lead to detrimental outcomes such as water logging and crop damage, conditions slightly wetter than the norm are generally considered favorable. They argue that above-average precipitation levels support crop growth in rain-fed farming systems. Das (2023) explores the effect of climate extremes on migration and finds that while an additional month of dryness significantly raises the likelihood of increased household migration, an increase in wetness correlates with reduced migration, potentially due to increase in agricultural productivity. They argue that excess precipitation leads to improved agricultural conditions and a subsequent decrease in the need for migration.

Nsabimana and Mensah (2020) examine the impact of weather shocks on stunting in children. They find that while dry shocks significantly increased stunting rates, wet shocks had no significant effect on child stunting. They suggest that the minimal impact of wet shocks may be because flood events are not randomly distributed, disproportionately impacting lowlands as opposed to highlands. Households in vulnerable areas may employ strategic measures to mitigate the effects of floods. Carpena (2019) examines the effects of droughts on household food consumption and finds that while droughts significantly reduce food availability, wet shocks had a positive influence on household food security, suggesting that food security issues are only a concern during drought periods. They highlight the critical nature of droughts in exacerbating food insecurity in rural settings. Paumgarten et al. (2018) likewise report that while both droughts and floods cause crop losses, droughts are considered a more severe threat due to slower recovery and higher wildlife-induced losses and food costs.

Damania et al. (2017), investigate the effects of rainfall shocks on agricultural productivity, and report an asymmetric relationship whereby while even small negative shocks (i.e., dry conditions) decrease agricultural productivity, positive shocks (i.e., wet conditions) increase productivity. Randell et al. (2022), investigate the impact of climatic conditions on household food security, finding that increased precipitation was positively correlated with per capita household consumption, crop income, and nutritional outcomes. They highlight that greater rainfall enhances food security and nutritional status primarily through boosting household income.

At a macroeconomic level, previous literature (Loayza et al. 2012; Cuñado and Ferreira 2014, Zaveri et al. 2023) has also shown that while droughts always have severe negative effects on economic growth, floods generally benefit economic growth, though only for moderate events and in developing countries. They argue that by increasing soil fertility, flooding has a positive effect on agricultural production.

2.On hunting as a "safety net"

Several case-studies have highlighted hunting as a "safety net" for rural communities during crises (de Merode et al. 2004; Brashares et al. 2011; Schulte-Herbrüggen et al. 2013), although Paumgarten et al. (2018) offer a nuanced perspective highlighting the potential of creating poverty traps. In contrast, other studies have found no evidence that hunting bushmeat acts as a safety net during shocks (Nielsen et al. 2015; Nielsen et al. 2017; Nielsen et al. 2018). Additionally, the notion that forests serve as seasonal gap fillers has been questioned (Wunder et al. 2014). These findings, however, may partly be attributed to the fact that these studies rely solely on self-reported shocks which may suffer from endogeneity - as in those households who successfully rely on bushmeat hunting as a coping strategy could also report a lower shock intensity. Furthermore, the absence of observations of major catastrophic shocks in the study sample may also skew the results reported by these studies.

3.Contextualizing Wealth and Bushmeat Hunting in the Face of Economic Shocks

Research on the relationship between household wealth and the hunting or consumption of bushmeat provides mixed evidence. On one end, some authors (e.g., Nielsen, 2006; Hickey et al. 2016) suggest that the poor rely more on bushmeat or wild foods harvesting as a safety net, during times of food insecurity. Conversely, others (de Merode et al. 2004; Wilkie et al. 2005; Coad et al. 2010; Brashares et al. 2011; Mgawe et al. 2012) found that bushmeat hunting tends to increase with wealth. Thus, challenging the prevailing notion that bushmeat is predominantly a resource for the poor, arguing that the capacity to exploit these resources is frequently contingent upon the possession of certain assets that poorer households lack. For instance, de Merode et al. (2004) contend that poorer community members use wildlife resources less frequently than wealthier ones due to the need for investments such as guns, nets, and snare wire. Brashares et al. (2011) observe that food insecurity does not necessarily lead to increased hunting activities among the poorest, due to barriers such as the initial cost of hunting equipment. This investment hurdle aligns with Dercon's (1998) observations in Tanzania, where poorer households tended to engage in low-risk, low-return activities due to lack of capital to invest in higher-return assets like livestock. Meanwhile, Torres et al. (2021) challenge this perspective, arguing that wealth does not have a significant impact on bushmeat harvest / consumption patterns.

Further, Ambrose-Odj (2003) found that low-income households faced both economic and social barriers in accessing non-timber forest products (NTFPs). This has been largely attributed to wealthier households having larger land holdings, better means of transporting NTFPs, along with the ability to pay bribes (Arnold and Pérez, 2001; Angelsen and Wunder, 2003; Heubach et al. 2011). Contrary to the common perception that NTFPs primarily benefit the poor, evidence suggests that high-value NTFPs are typically used by individuals who possess greater power, more assets, and stronger connections (Dove, 1993; Coad, 2008), indicating that these resources are dominated by the rich.

This contention of whether wealth creation reduces or increases wildlife consumption is central to understanding whether poverty alleviation strategies can simultaneously serve biodiversity conservation goals (Torres et al. 2021). Economic theory on bushmeat consumption suggests varied outcomes depending on whether bushmeat is considered an inferior, normal, or superior good (Milner-Gulland and Bennett, 2003; Wilkie and Godoy, 2001; Wilkie et al. 2005; Torres et al. 2021). Where bushmeat is seen as an inferior good, its consumption is expected to decline as wealth increases. Conversely, if bushmeat is seen as superior to alternative proteins, then increased wealth could lead to higher bushmeat consumption, potentially exacerbating conservation and public health risks.

4.On the Effect of Climate Shocks on Wildlife Populations

We review the current evidence on bushmeat availability under different climate shocks.

1. Short-Term Impacts of Drought: Bodmer et al. (2018) find that both terrestrial and arboreal mammal populations remain stable during droughts lasting one season, implying that bushmeat supply is relatively unaffected in the immediate aftermath of such climatic events.

2. Impact of Floods: The same study by Bodmer et al. (2018) study consecutive floods over 4 years and find that while arboreal mammal populations are largely unaffected by floods, terrestrial mammals experience significant declines following consecutive intense flood events.

3. Effect of Extended Droughts: Evidence from Bickford et al. (2010) shows that prolonged droughts spanning multiple years can induce reproductive failures, leading to population crashes for many species. Sperry and Weatherhead (2008) similarly highlight cascading effects of drought in central Texas - reduced ground vegetation led to a collapse in small-mammal populations, which then affected predator survival, such as rat snakes. Hillman and Hillman (1977), studying Nairobi National Park during a 16-month drought, document significant mortality for specific species such as wildebeest, zebra, and kongoni, while other populations remained stable. Walker et al. (1987) in a broader study of four African conservation areas, note that herbivore mortality spikes during the second year of a drought when food reserves are depleted.

4. Species-Specific Responses: The literature highlights species-specific vulnerabilities to drought. Ferreira et al. (2019) find that white rhinos, which graze on grasses, exhibit lower birth rates and higher mortality during droughts due to reduced grass biomass. In contrast, black rhinos, which browse on trees, are largely unaffected. They suggest that animal mortality has more to do with lack of food availability than water availability, which disproportionately affects grazers. Similarly, Walker et al. (1987) report that browsers were less impacted by drought compared to

herbivores (grazers), further suggesting that herbivore mortality is primarily linked to food scarcity rather than water access.

5. Reptiles and Flood-Induced Mortality: Flood events can also induce mortality in reptiles, particularly those that lay eggs on land. Bickford et al. (2010) point out that heavy precipitation may lead to increased mortality due to nest flooding and fungal infections on eggs. These effects may reduce the availability of reptilian bushmeat species, such as turtles and lizards, in flooded areas.

Supplementary Tables

Table B1. Summary of sample

Country	Sites (N)	Villages (N)	HHs (N)	Hunting HHs (N)	Hunting HHs (%)	Reliance (% of Total Inc)
Bangladesh	4	23	378	180	47.62	1.07
Belize	1	7	125	66	52.80	1.21
Bolivia	3	21	321	242	75.39	**8.61**
Brazil	3	18	247	139	56.28	4.40
Burkina Faso	3	26	586	89	15.19	0.60
Cambodia	3	15	578	482	83.39	6.20
Cameroon	1	5	111	108	**97.30**	**10.88**
China	3	6	234	4	1.71	0.20
DRC	2	5	193	88	45.60	4.55
Ecuador	1	32	193	0	**0**	**0**
Ethiopia	4	28	576	18	3.12	0.06
Ghana	2	30	609	427	70.11	7.82
Guatemala	1	11	150	10	6.67	0.48
India	2	5	247	0	**0**	**0**
Indonesia	4	8	381	70	18.37	3.60
Malawi	2	28	372	43	11.56	0.53
Mozambique	4	7	843	720	**85.41**	2.78
Nepal	4	7	489	32	6.54	0.04
Nigeria	2	4	229	30	13.10	1.22
Peru	1	14	104	49	47.12	2.92
Senegal	2	5	140	48	34.29	1.46
Uganda	3	18	521	368	70.63	0.96
Vietnam	1	6	155	16	10.32	0.61
Zambia	2	4	196	124	63.27	2.62

Note: In cases where total income is negative, zero, or less than bushmeat income, reliance is assumed to be 1. Source: Author's calculations based on PEN survey data.

Table B1 presents the summary of the distribution of households that engage in hunting across countries in the PEN dataset. Hunting participation is highest in Cameroon where 97.3% of households surveyed report hunting bushmeat at least once in the past month, and Mozambique (85.41%) and lowest in India and Ecuador (0%). Reliance on bushmeat income is also highest in Cameroon where it constitutes a 10.88% of total household income, followed by Bolivia (8.61%) and lowest in India and Ecuador (0%). The majority of bushmeat income in our sample is allocated for subsistence purposes (i.e. bushmeat hunted for self-consumption) instead of sale for cash, indicating that the primary function of hunting is to fulfill household dietary requirements (Figure B1). For instance, in Guatemala, all bushmeat hunted is used for own consumption. This is true for all countries except Nigeria and Vietnam, where the cash share of bushmeat income is higher than subsistence bushmeat income.

Table B2. Percentage (%) of our sample that experienced SPEI measured shocks

Year	Wet (%)	Moderate Wet (%)	Severe Wet (%)	Extreme Wet (%)	Dry (%)	Moderate Dry (%)	Severe Dry (%)	Extreme Dry (%)
2005	32.27	0	0	32.27	21.51	21.51	0	0
2006	9.26	1.33	1.10	6.83	9.00	8.16	0.65	0.19
2007	12.41	3.37	3.72	5.32	26.15	24.20	1.50	0.46
2008	5.91	3.04	0.07	2.80	19.03	19.03	0	0
2009	0	0	0	0	0	0	0	0
2010	0	0	0	0	0	0	0	0

Source: Author's calculations based on SPEI data.

Table B2 presents the annual prevalence of weather shocks in our sample measured by the SPEI. The moderate, severe, and extreme categories are based on standard SPEI classifications (McKee et al. 1993; Paulo et al. 2012; Wang et al. 2014). The highest percentage of households in our sample experiencing SPEI-measure drought conditions did so in 2007 (26.15%), while only

9% encountered such conditions in 2006. Conversely, SPEI-measured wet events peaked in 2005, with 32.27% of households affected, while the lowest incidence occurred in 2008 (5.91%). Notably, no SPEI-measured droughts or wet-events were recorded in 2009 and 2010.

Self-reported shocks

The PEN dataset includes quarterly household surveys that collect income information as well as annual village and household surveys that have modules related to self-reported shocks. The nature of the shocks reported varies between the two types of surveys. In the village survey, village officials are asked about any crises experienced by the village in the past 12 months. They choose from a list of crises including floods, droughts, wildfires, human epidemics, and political or civil unrest, among others. In contrast, the annual household survey asks households if they faced any significant unexpected expenses over the past 12 months. They select from a range of reasons for such expenses, including severe crop failure, substantial livestock loss, major asset loss, among others.

For our analysis, we focus on the self-reported shocks from the village survey, as it is challenging to attribute the shocks to crops, livestock or other assets to droughts or wet events. While the subjective nature of these shocks introduces some methodological challenges (i.e., they may not accurately reflect the intensity of climate events as experienced by the households) - our analysis allows us to compare our findings with previous studies that also relied on self-reported shocks.

Table B3. Percentage (%) of our sample living in villages that self-reported shocks

Year	Floods (%)	Severe Floods (%)	Drought (%)	Severe Drought (%)
2006	22.27	8.80	15.10	8.25
2007	18.64	1.69	10.13	1.51
2008	11.15	1.29	9.82	1.16
2010	5.44	0	6.37	0

Note: The villages in the sample were asked if they faced any crisis in the past 12 months. Source: Author's calculations based on self-reported shock questions in the PEN survey.

Table B3 presents a summary of our sample residing in villages that reported experiencing a drought or a wet event (flood). The summary reveals a consistent trend wherein the share of households reporting exposure to floods surpasses that of droughts in most years, except for 2010. Additionally, floods are consistently depicted as more severe compared to droughts across years. Notably, in contrast to the absence of SPEI-measured droughts or floods in the year 2010, 5% of households self-reported floods, and 6% reported experiencing droughts. It highlights a tendency to overreport weather shocks, suggesting instances where measured weather shocks are not present according to the SPEI but reported by village officials.

To further evaluate the extent of biases in self-reported weather shocks, we conducted an analysis comparing self-reported shocks with SPEI data following the methodology of Nguyen and Nguyen (2020). The agreement between self-reported, and SPEI-defined weather shocks was assessed using Kappa statistics.19 A Kappa statistic of 1 denotes perfect agreement, while -1 indicates perfect disagreement (Table B4). The agreement is highest for droughts at 0.22, and the lowest for floods at -0.02, This reveals a significant lack of agreement between self-reported shocks and SPEI measured shocks. This observation is in line with Baquie and Fuje (2020) who also note a low correlation between self-reported shocks and shocks measured exogenously. Additionally, Figure B3 presents the results from investigating the effect of self-reported shocks on bushmeat income.

[19] Various tests, such as odds ratio, predictive positive value, predictive negative value, and Kappa statistic, can be used to identify this proportion. However, we use the Kappa Statistic due to its recent application in shock literature for measuring agreement between self-reported and objectively measured weather shocks (Nguyen and Nguyen, 2020; Mulungu & Kilimani, 2023).

Table B4. Agreement between self-reported and SPEI measured weather shocks using Kappa Statistics (k)

Event	k-value	z-statistic	p-value	Strength of Agreement
Droughts	0.2233381	2.946032	0.0032188	Fair
Floods	-0.0224063	-0.2955592	0.7675667	None

Note: k-Values <= 0 indicate no agreement. Values between 0.01 to 0.20 indicate slight agreement, 0.21 to 0.40 indicate fair agreement, 0.41 to 0.60 indicate moderate agreement, 0.61 to 0.80 suggest substantial agreement, and values from 0.81 to 1.00 indicate almost perfect agreement (McHugh, 2012). *Source:* Author's calculations from SPEI data and PEN survey.

Our investigation into the relationship between bushmeat income and self-reported shocks at the village level aligns with the findings of Nielsen et al. (2017) and Nielsen et al. (2018). In accordance with their conclusions, our results revealed no significant effects of droughts and floods on bushmeat income (Figure B3). These results lend support to the notion that when relying on self-reported shocks to study bushmeat income, one may find that bushmeat income does not play a role as a safety net in times of shocks. The primary reason for that is that self-reported shocks are often reported as a generic category like "crop failure" rather than a precise measure of a drought or flood. Another reason is that people tend to forget the timing, severity of the shock especially when interviews happen months after the event. If respondents misclassify, or under-report shocks, the measure is likely an unreliable measure of real exposure.

Table B5. Effect of Droughts and Wet Events on Bushmeat Income and Reliance (Baseline)

	Dependent variable:							
	Bushmeat Income (IHS)				Reliance on Hunting (in %)			
	Full Sample		Hunting Sample		Full Sample		Hunting Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SPEI-3 Drought	0.076^{**}		0.111^{**}		0.494^{**}		0.700^{**}	

	(0.038)		(0.048)		(0.232)		(0.299)	
SPEI-3 Wet	-0.059		-0.063		-0.195		-0.185	
	(0.039)		(0.048)		(0.301)		(0.368)	
SPEI-6 Drought		0.187***		0.235***		0.643**		0.810**
		(0.036)		(0.042)		(0.252)		(0.313)
SPEI-6 Wet		-0.150***		-0.171***		-1.292***		-1.529***
		(0.044)		(0.053)		(0.326)		(0.389)
Household FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	30,804	30,804	23,195	23,195	30,804	30,804	23,195	23,195
R^2	0.580	0.583	0.553	0.557	0.394	0.395	0.382	0.384

Note: Dependent variables are i) IHS transformed Bushmeat Income, and ii) Reliance on Bushmeat Income (in percentage). All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI index is calculated at a 6-month timescale. Drought and Wet dummies are created using the intensity scale of SPEI, with values below -1, indicating drought conditions, and values above +1, indicating wet conditions. *p<0.1; **p<0.05; ***p<0.01

In addition to reporting results for the full sample, we also present findings from a restricted subsample, which we refer to as the hunting sample. This subsample includes households located in villages where hunting is prevalent, defined as villages where at least one household reports bushmeat income in any survey quarter. This ensures that the observed effects on bushmeat income and reliance on hunting are representative of households in environments where hunting is a common practice.

We chose to define the hunting sample as households located in villages where hunting is prevalent, rather than limiting to households that hunt because the number of households that hunt in the dataset are relatively small (3349 households out of 7978 unique households), resulting in insignificant estimates. By expanding the sample to include households in villages with at least one hunter (6049 households), we capture a broad range of households who are likely exposed to the practice of hunting and may have easier access to hunting resources or knowledge.

Table B6: Wald Tests for Statistical Difference Between Shock Coefficients

Comparison	χ^2	df	p-value
Drought: SPEI-3 = SPEI-6	**8.9341290**	**1**	**0.0028**
Wet: SPEI-3 = SPEI-6	**5.1881537**	**1**	**0.0227**
Within SPEI-6: dry = wet	**28.3662831**	**1**	**1.00e-07**
Within SPEI-3: dry = wet	0.0183672	1	0.89

Note: Table presents tests for statistical difference between estimates from Tables S5. Wald chi-square tests for equality are conducted using the "linear hypothesis" function in R. All models include household and year fixed effects with standard errors clustered at the village level.

Table B7. Effect of Extreme vs Moderate Droughts and Wet Events on Bushmeat Income

	Dependent variable:							
	Bushmeat Income (IHS)							
	Full Sample				Hunting Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Extreme Drought (SPEI-6)	0.387***				0.406***			
	(0.118)				(0.123)			
Extreme Wet (SPEI-6)	-0.113				-0.110			
	(0.100)				(0.101)			
Extreme Drought (SPEI-3)		1.189***				1.208***		
		(0.077)				(0.086)		
Extreme Wet (SPEI-3)		-0.100				-0.095		
		(0.113)				(0.113)		
Moderate/Sev. Drought (SPEI-6)			0.174***				0.219***	
			(0.036)				(0.041)	
Moderate/Sev. Wet (SPEI-6)			-0.150***				-0.177***	
			(0.049)				(0.060)	
Moderate/Sev. Drought (SPEI-3)				0.078**				0.114**
				(0.037)				(0.048)
Moderate/Sev. Wet (SPEI-3)				-0.044				-0.048
				(0.037)				(0.048)
Household FE	Y	Y	Y	Y	Y	Y	Y	Y

Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	30,804	30,804	30,804	30,804	23,195	23,195	23,195	23,195
R^2	0.580	0.580	0.583	0.580	0.553	0.552	0.556	0.553

Note: Dependent variable is IHS transformed Bushmeat Income. All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI index is calculated at 2 different timescales in months (i.e. 3 months, 6 months respectively), representing short term to medium term drought and wet conditions. Shock dummies are calculated using SPEI index, with values below -2, indicating Extreme Drought, values between -1 and -2 indicating Moderate/Severe Drought, values above +2 indicating Extreme Wet and values between +1 and +2 indicating Moderate/Severe Wet conditions. *$p<0.1$; **$p<0.05$; ***$p<0.01$*

Table B8. Effect of Extreme vs Moderate Droughts and Wet Events on Reliance

	Dependent variable:							
	Reliance on Hunting (%)							
	Full Sample				Hunting Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Extreme Drought (SPEI-6)	1.831*				1.925			
	(1.108)				(1.168)			
Extreme Wet (SPEI-6)	-0.919				-0.896			
	(0.579)				(0.586)			
Extreme Drought (SPEI-3)		12.971***				13.022***		
		(0.408)				(0.468)		
Extreme Wet (SPEI-3)		-0.918				-0.877		
		(0.563)				(0.551)		
Moderate/Sev. Drought (SPEI-6)			0.600**				0.758**	
			(0.250)				(0.311)	
Moderate/Sev. Wet (SPEI-6)			-1.259***				-1.539***	
			(0.369)				(0.451)	
Moderate/Sev. Drought (SPEI-3)				0.525**				0.735**
				(0.232)				(0.299)
Moderate/Sev. Wet (SPEI-3)				-0.009				0.041
				(0.314)				(0.402)

Household FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	30,804	30,804	30,804	30,804	23,195	23,195	23,195	23,195
R^2	0.394	0.393	0.395	0.394	0.382	0.381	0.384	0.382

Note: Dependent variable is Reliance on Bushmeat Income (in %). All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI index is calculated at 2 different timescales in months (i.e. 3 months, 6 months respectively), representing short term to medium term drought and wet conditions. Shock dummies are calculated using SPEI index, with values below -2, indicating Extreme Drought, values between -1 and -2 indicating Moderate/Severe Drought, values above +2 indicating Extreme Wet and values between +1 and +2 indicating Moderate/Severe Wet conditions. **p<0.1; **p<0.05; ***p<0.01.*

Table B9: Wald Tests for Statistical Difference Between Extreme and Moderate/Severe Shocks

Comparison	Chi-square Statistic	p-value
Drought: Extreme vs Moderate/Severe (Full Sample, SPEI-6)	**3.8952**	**0.0484**
Wet: Extreme vs Moderate/Severe (Full Sample, SPEI-6)	0.0169	0.8965
Drought: Extreme vs Moderate/Severe (Hunting Sample, SPEI-6)	**2.9291**	**0.0870**
Wet: Extreme vs Moderate/Severe (Hunting Sample, SPEI-6)	0.1433	0.7050
Drought: Extreme vs Moderate/Severe (Reliance, SPEI-6)	0.8678	0.3516
Wet: Extreme vs Moderate/Severe (Reliance, SPEI-6)	0.0220	0.8822

Note: Table presents tests for statistical difference between estimates from Tables S8 and S9. Wald chi-square tests for equality are conducted using “linear hypothesis” function in R. All models include household and year fixed effects with standard errors clustered at the village level.

Table B10: Effect of Timing of Shocks on Bushmeat Income and Crop Income

	Dependent variable:	
	Bushmeat Income (IHS)	Crop Income (IHS)
	(1)	(2)
Planting	0.133**	-2.236***
	(0.053)	(0.353)
Growing	0.052	-0.037
	(0.066)	(0.291)
Harvesting	-0.074	1.138***
	(0.061)	(0.304)
Drought	0.034	-0.339
	(0.099)	(0.424)
Wet	-0.128	-0.360
	(0.119)	(0.315)
Drought*Planting	0.016	0.516
	(0.105)	(0.423)
Wet*Planting	-0.291**	2.626***
	(0.129)	(0.434)
Drought*Growing	0.261**	-0.314
	(0.105)	(0.500)

Wet*Growing	0.061	-0.260
	(0.154)	(0.523)
Drought*Harvesting	-0.032	0.832**
	(0.111)	(0.414)
Wet*Harvesting	0.096	0.488
	(0.091)	(0.454)
Household FE	Y	Y
Year FE	Y	Y
Obs.	29,047	29,047

Note: Dependent variables are IHS transformed i) Bushmeat Income, and ii) Crop Income. Multiplying the coefficients by 100 will approximate the percentage change in income. All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI are calculated at 6-month timescale, representing drought and wet conditions. SPEI is measured on an intensity scale with values below -1, indicating drought conditions, and values above +1, indicating wet conditions. Drought and Wet are dummies created using these thresholds. *p<0.1; **p<0.05; ***p<0.01

Table B10 shows that crop income is on average 223% below baseline (off-and growing season levels) during the planting season, when farmers face heavy input costs and 113% higher compared to those same baselines in the harvesting season. By contrast, bushmeat income is 13% higher in planting season and % lower in the harvesting season, compared to the off-season and growing season. This highlights that bushmeat may act as a buffer, stepping in when crop revenues are negative.

Table B11: Effect of Timing of Shocks on Bushmeat and Crop Reliance

	Dependent variable:	
	Bushmeat Reliance	Crop Reliance
	(1)	(2)
Planting	0.795**	-7.954***
	(0.374)	(2.867)
Growing	0.308	7.650***
	(0.522)	(2.167)
Harvesting	-0.835*	13.119***
	(0.447)	(2.240)
Drought	0.650	1.707
	(0.738)	(4.198)
Wet	-0.596	1.455
	(0.850)	(3.077)
Drought*Planting	-0.683	-5.872
	(0.705)	(4.421)
Wet*Planting	-3.820***	18.595***
	(1.040)	(4.553)
Drought*Growing	0.223	-4.351
	(0.660)	(3.808)
Wet*Growing	0.753	-11.717*
	(1.207)	(5.983)
Drought*Harvesting	0.115	2.212
	(0.638)	(3.715)
Wet*Harvesting	0.510	6.484
	(0.695)	(3.982)
Household FE	Y	Y
Year FE	Y	Y
Obs.	29,047	20,585

Note: Dependent variables are Reliance on i) Bushmeat Income, and ii) Crop Income (in percentage). Negative crop income was classified as NA when calculating Reliance on Crop Income. All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI are calculated at 2 different timescales in months (i.e. 3 months, 6 months respectively), representing short term to medium term drought and wet conditions. SPEI is measured on an intensity scale with values below -1, indicating drought conditions, and values above +1, indicating wet conditions. Drought and Wet are dummies created using these thresholds. *p<0.1; **p<0.05; ***p<0.01.

Table B12: Effect of Drought on Bushmeat Income by Income Quartiles

	Dependent variable:
	IHS Bushmeat Income
Drought	0.147***
	(0.043)
Drought × Q2	0.127***
	(0.038)
Drought × Q3	0.224***
	(0.060)
Drought × Q4	0.243**
	(0.100)
Household FE	Yes
Year FE	Yes
Obs.	23,195

Note: Table displays the estimated effect of drought on IHS transformed Bushmeat Income across four income quartiles (Q1 to Q4 - lowest to highest), with 95% confidence intervals. The sample is limited to households in hunting prevalent villages. Quartiles are based on total household income excluding bushmeat income. Multiplying the coefficients by 100 will approximate the percentage change in income. All regressions control for household and year fixed effects, with standard errors clustered at the village level. *p<0.1; **p<0.05; ***p<0.01

Table B13: Pairwise Comparisons of Drought Effect Heterogeneity Across Income Quartiles

(Chi Square Tests with Holm Adjustment)

HYPOTHESIS	CHI-SQUARED	RAW P-VALUE	ADJUSTED P-VALUE
Q2 - Q1	11.103	0.001	0.004
Q3 - Q1	13.704	0.000	0.001
Q4 - Q1	5.880	0.015	0.061
Q3 - Q2	2.059	0.151	0.454
Q4 - Q2	1.368	0.242	0.484
Q4 - Q3	0.043	0.836	0.836

Note: The sample is limited to households in hunting prevalent villages. Q1 to Q4 refers to Income quartiles from lowest to highest. Dependent variable is IHS transformed bushmeat income. Wald chi-square tests for equality are conducted using "linear hypothesis" function in R. P-values are corrected using the Holm method (Holm, 1979) for multiple group testing. All models include household and year fixed effects with standard errors clustered at the village level. Holm method is applied to correct for multiple hypothesis testing across six pairwise comparisons.

Table B14: Distribution of Bushmeat Income (PPP adj.USD/AEU) by Income Quartile

Quartile	Obs.	Mean BM Inc	SD of BM Inc	CV (SD/Mean)	Min. BM Inc	10th Perc	Median BM Inc	90th Perc	Max. BM Inc
1	5799	5.11	60.89	11.93	0	0	0	5.68	3402.66
2	5799	7.40	86.18	11.64	0	0	0	9.43	5341.78
3	5799	14.98	223.38	14.91	0	0	0	19.08	15525.66
4	5798	46.02	1086.57	23.61	0	0	0	48.09	81812.77

Note: The sample is limited to households in hunting prevalent villages. Quartiles are based on total household income excluding bushmeat income.

Table B15: Baseline results with spatial HAC standard errors

	Dependent variable:							
	Bushmeat Income (IHS)				Reliance on Hunting (in %)			
	Full Sample		Hunting Only		Full Sample		Hunting Only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SPEI-3 Drought	0.076		0.111^{*}		0.494		0.700^{*}	
	(0.052)		(0.066)		(0.307)		(0.388)	
SPEI-3 Wet	-0.059		-0.063		-0.195		-0.185	
	(0.048)		(0.062)		(0.295)		(0.354)	
SPEI-6 Drought		0.187^{***}		0.235^{***}		0.643^{**}		0.810^{**}
		(0.049)		(0.055)		(0.271)		(0.332)
SPEI-6 Wet		-0.150^{***}		-0.171^{***}		-1.292^{***}		-1.529^{***}
		(0.057)		(0.068)		(0.427)		(0.497)
Household FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.580	0.583	0.553	0.557	0.394	0.395	0.382	0.384

Note: Dependent variables are i) IHS transformed Bushmeat Income, and ii) Reliance on Bushmeat Income (in percentage). All specifications include household and year fixed effects. Standard errors (in parentheses) are calculated using spatial HAC based on Conley (1999). SPEI are calculated at 2 different timescales in months (i.e. 3 months, 6 months respectively), representing short term to medium term drought and wet conditions. SPEI is measured on an intensity scale with values below -1, indicating drought conditions, and values above +1, indicating wet conditions. Drought and Wet are dummies created using these thresholds. *p<0.1; **p<0.05; ***p<0.01

Table B16: Effect of Droughts and Wet Events on Probability of Hunting

	Dependent variable:			
	Probability of Hunting			
	Full Sample		Hunting Only	
	(1)	(2)	(3)	(4)
SPEI-3 Drought	0.045***		0.064***	
	(0.017)		(0.021)	
SPEI-3 Wet	-0.018		-0.020	
	(0.015)		(0.018)	
SPEI-6 Drought		0.087***		0.110***
		(0.013)		(0.014)
SPEI-6 Wet		-0.045***		-0.050***
		(0.013)		(0.015)
Household FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Obs.	30,804	30,804	23,195	23,195
R^2	0.563	0.568	0.520	0.525
Adjusted R^2	0.411	0.417	0.350	0.358

Note: Dependent variable is Probability of engaging in hunting. All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI are calculated at 2 different timescales in months (i.e. 3 months, 6 months respectively), representing short term to medium term drought and wet conditions. SPEI is measured on an intensity scale with values below -1, indicating drought conditions, and values above +1, indicating wet conditions. Drought and Wet are dummies created using these thresholds. *p<0.1; **p<0.05; ***p<0.01

Table B17: Effect of Drought on Bushmeat Income by Income Quartiles (Full Sample)

	Dependent variable:
	IHS Bushmeat Income
Drought	0.118***
	(0.035)
Drought × Q2	0.102***
	(0.031)
Drought × Q3	0.161***
	(0.052)
Drought × Q4	0.185**
	(0.082)
Household FE	Yes
Year FE	Yes
Obs.	30,804

Note: Table displays the estimated effect of drought on IHS transformed Bushmeat Income across four income quartiles (Q1 to Q4 - lowest to highest), with 95% confidence intervals. Results are for the Full sample. Quartiles are based on total household income excluding bushmeat income. Multiplying the coefficients by 100 will approximate the percentage change in income. All regressions control for household and year fixed effects, with standard errors clustered at the village level. *p<0.1; **p<0.05; ***p<0.01

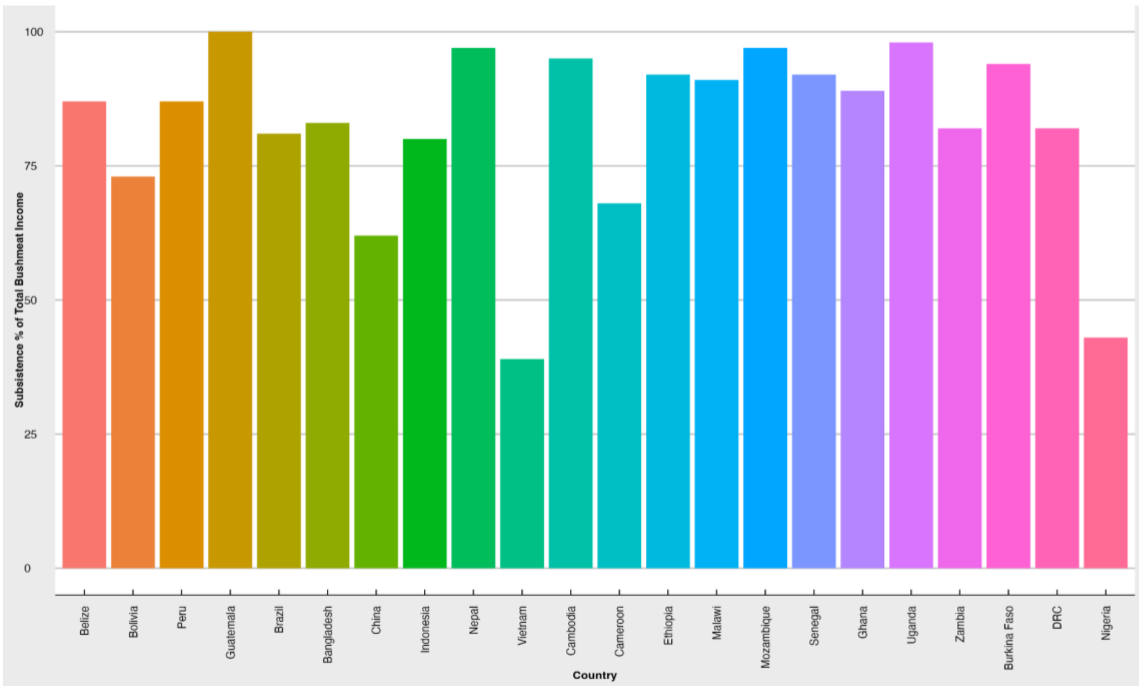
Table B18: Robustness -Effect of Timing of Shocks on Bushmeat Income and Crop Income with Lags

	Dependent variable:	
	Bushmeat Income (IHS)	Crop Income (IHS)
	(1)	(2)
Planting	0.127**	-2.086***
	(0.054)	(0.324)
Growing	0.051	0.004
	(0.067)	(0.290)
Harvesting	-0.089	1.500***
	(0.065)	(0.304)
Drought	0.032	-0.288
	(0.099)	(0.415)
Wet	-0.131	-0.303
	(0.118)	(0.307)
Drought*Planting	0.026	0.292
	(0.106)	(0.431)
Wet*Planting	-0.289**	2.566***
	(0.132)	(0.439)
Drought *Growing	0.263**	-0.367
	(0.105)	(0.516)
Wet*Growing	0.068	-0.404
	(0.155)	(0.517)
Drought*Harvesting	-0.032	0.821**
	(0.111)	(0.415)
Wet*Harvesting	0.094	0.529
	(0.091)	(0.424)
Drought (Lag 6)*Harvesting	0.027	-0.665**
	(0.056)	(0.304)
Wet (Lag 6)* Harvesting	0.037	-0.839***
	(0.062)	(0.199)
Household FE	Y	Y
Year FE	Y	Y
Obs.	29,047	29,047

Note: Dependent variables are IHS transformed i) Bushmeat Income, and ii) Crop Income. Lag 6 terms refer to a drought/wet event 6 month before harvesting season. Multiplying the coefficients by 100 will approximate the percentage change in income. All specifications include household and year fixed effects. Robust standard errors (in parentheses) are clustered at the village level. SPEI are calculated at 6-month timescale, representing drought and wet conditions. SPEI is measured on an intensity scale with values below -1, indicating drought conditions, and values above +1, indicating wet conditions. Drought and Wet are dummies created using these thresholds. *p<0.1; **p<0.05; ***p<0.01

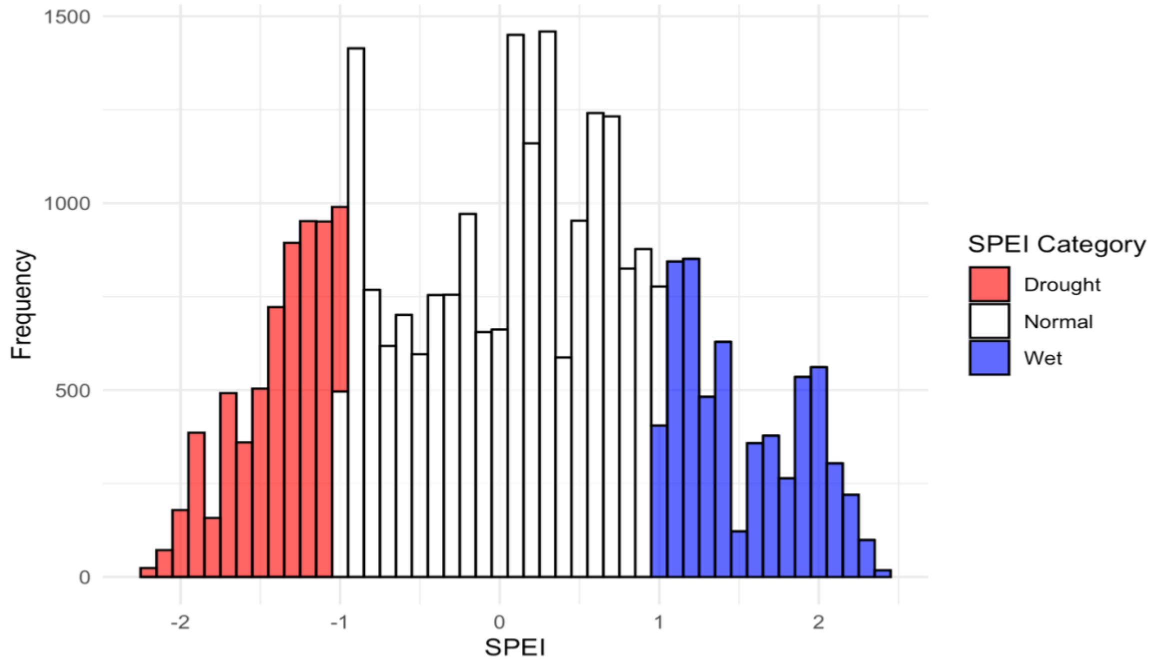
Supplementary Figures

Figure B1: Share of Subsistence Bushmeat Income in Total Bushmeat Income



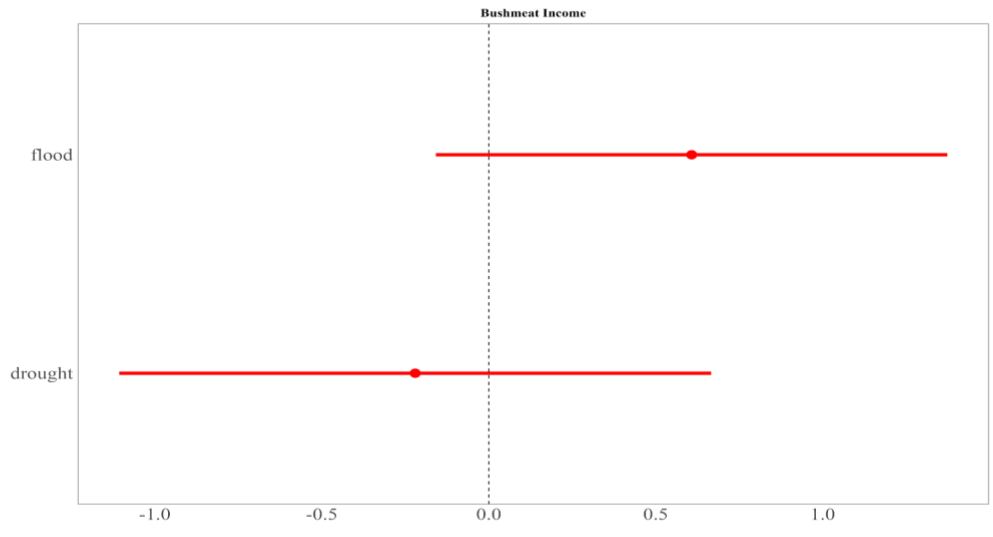
Source: Author's calculations based on PEN survey data.

Figure B2. Distribution of SPEI Values in our sample



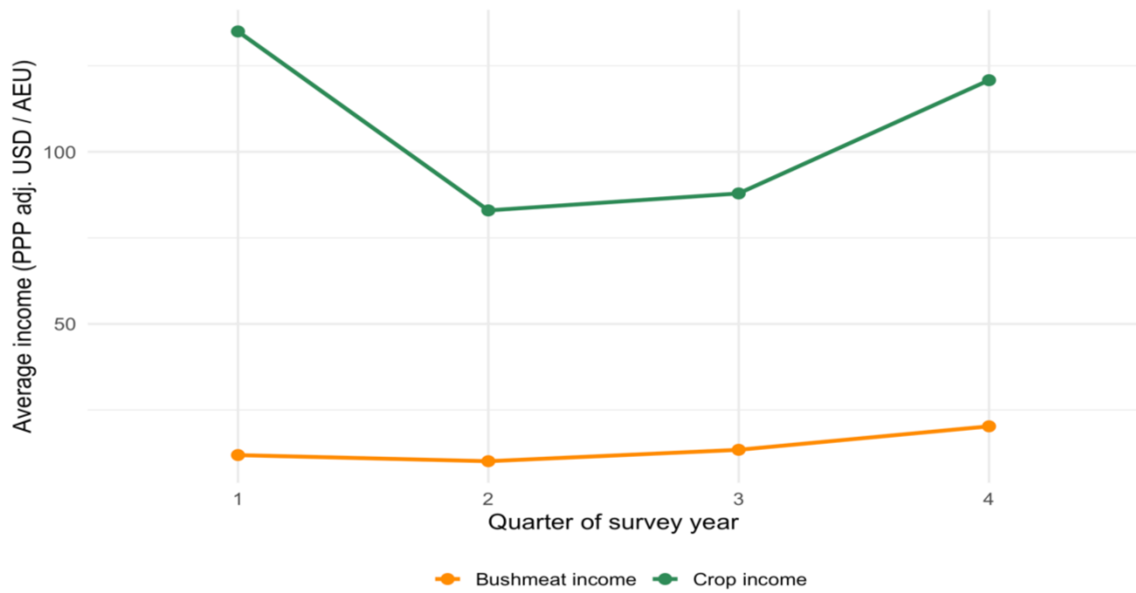
Source: Author's calculations based on SPEI06 data.

Figure B3. Marginal effect of self-reported (PEN survey) drought and wet event (flood) on bushmeat income



Source: Author's calculations from self reported shocks and bushmeat income in the PEN survey.

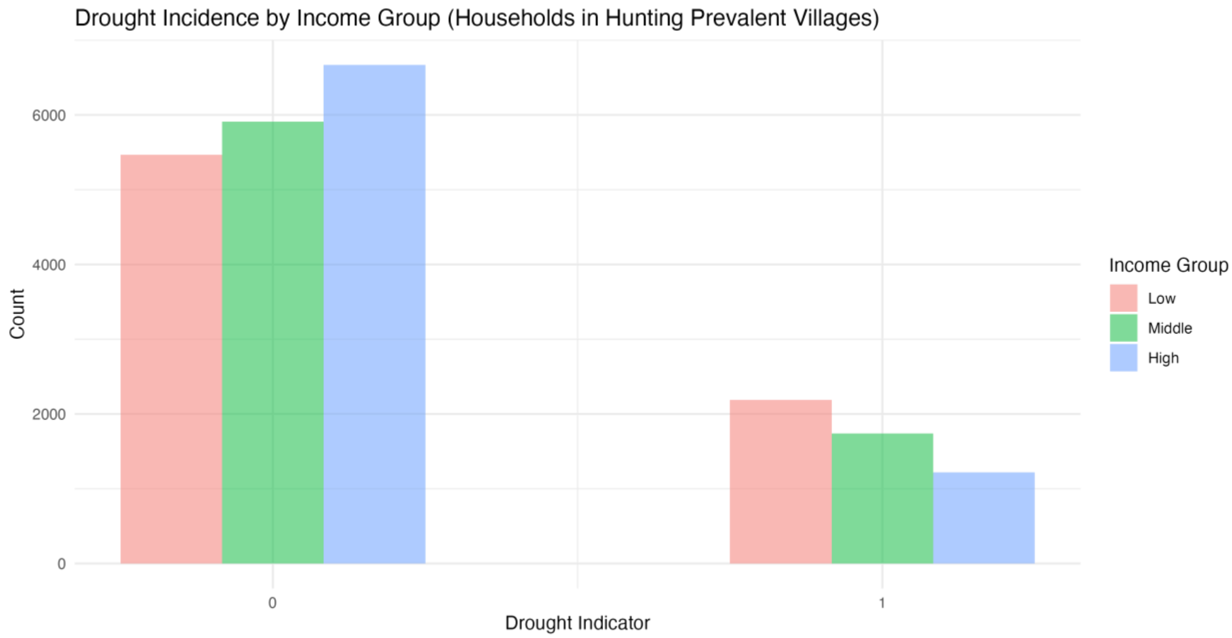
Figure B4. Seasonal Co-movement of crop and bushmeat income



Source: Author's calculations from crop and bushmeat income reported in the PEN survey.

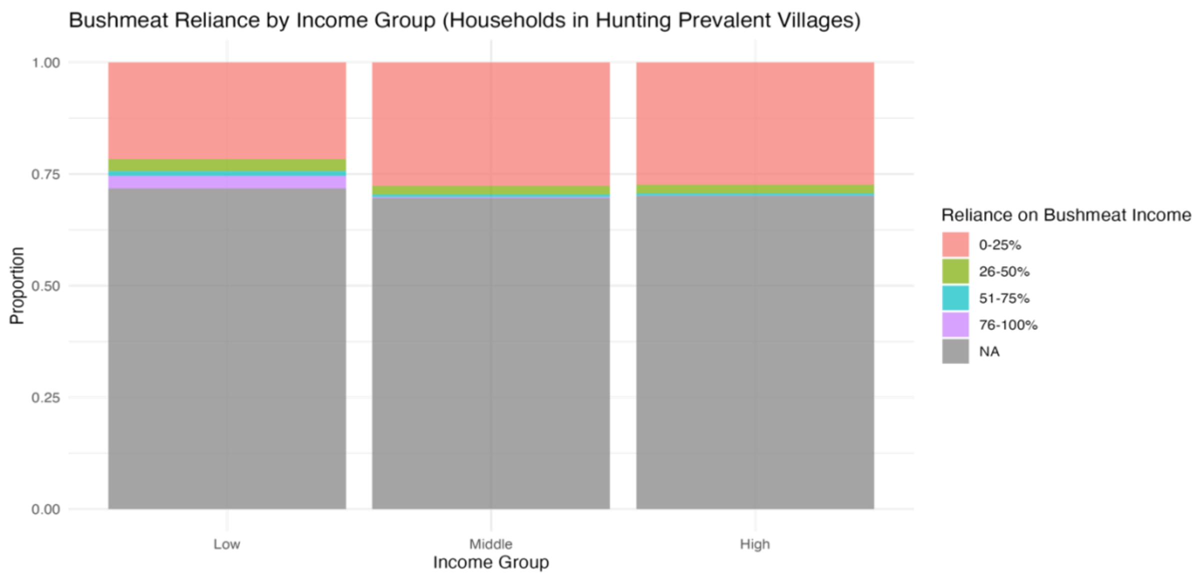
Figure B4 shows how crop and bushmeat incomes move over the four survey quarters. Crop incomes are highest in Quarter 1, when most of the previous season's harvest is sold, then fall by over 40 percent in Quarter 2 as planting and input costs dominate, before gradually recovering in Quarter 3 and rising again in Quarter 4 as stored grain is marketed. By contrast, bushmeat income remains nearly flat all year.

Figure B5. Drought distribution by Income Group (Households in Hunting Prevalent Villages)



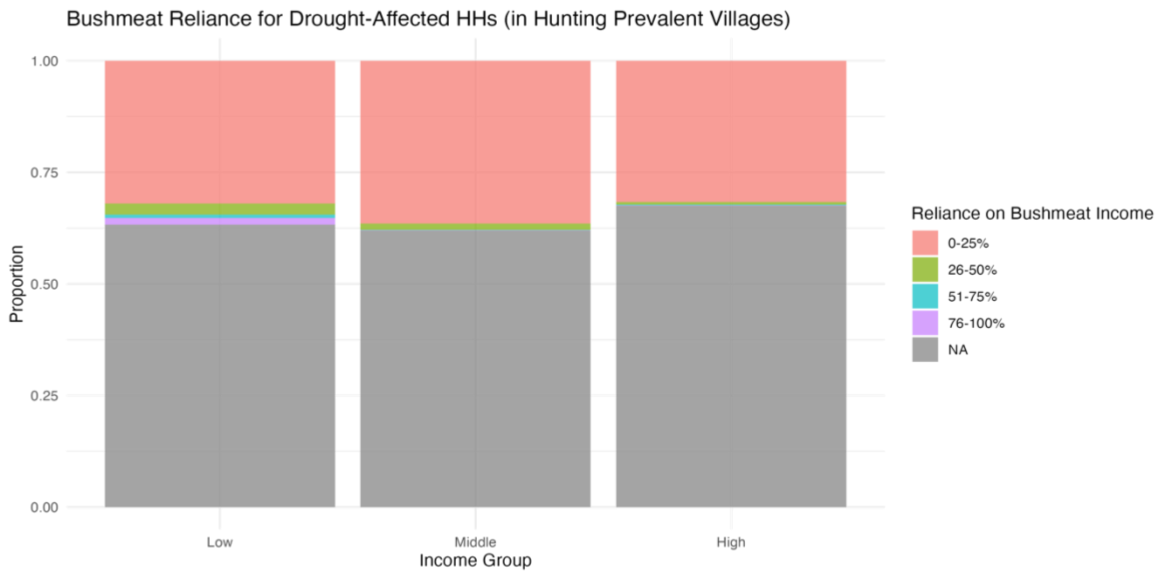
Source: Author’s calculations based on SPEI06 data and PEN survey.

Figure B6. Distribution of Reliance on Bushmeat Income (Households in Hunting Prevalent Villages)



Note: NA refers to households that do not hunt. Source: Author's calculations based on PEN survey.

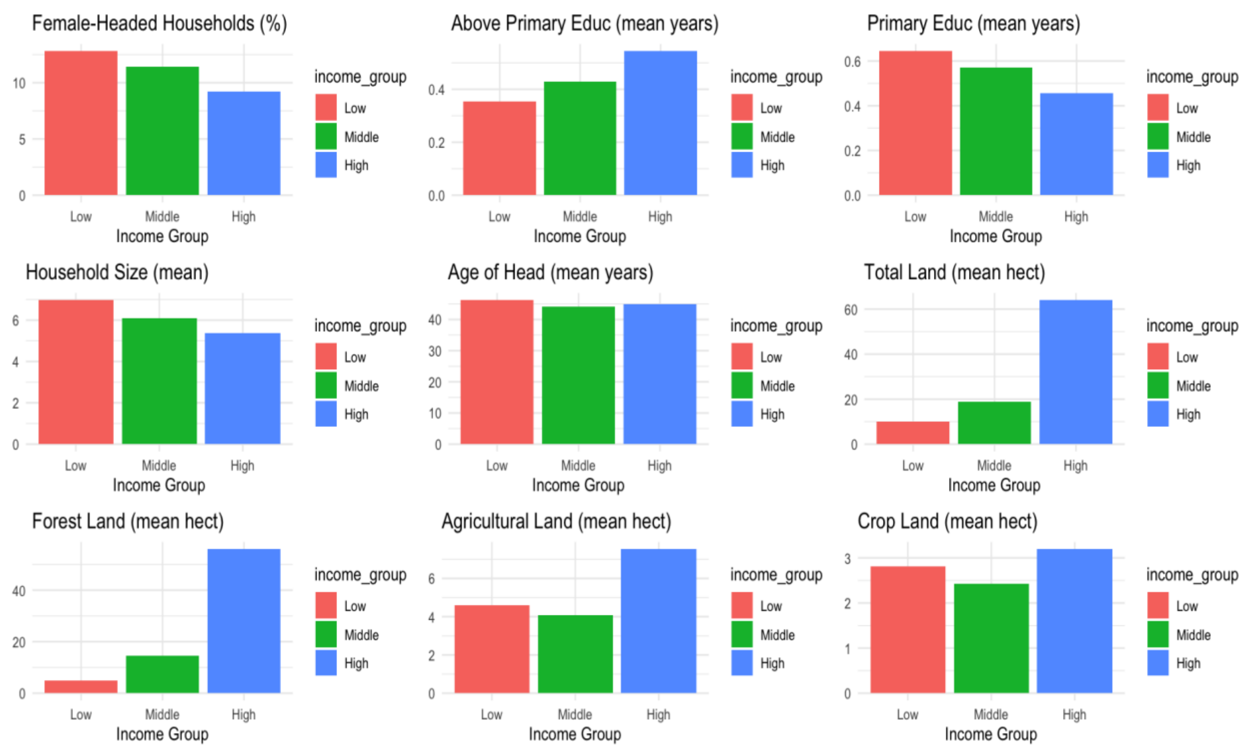
Figure B7. Distribution of Reliance on Bushmeat Income for Drought Affected Households in Hunting Prevalent Villages



Note: NA refers to households that do not hunt. Source: Author's calculations based on SPEI06 data and PEN survey.

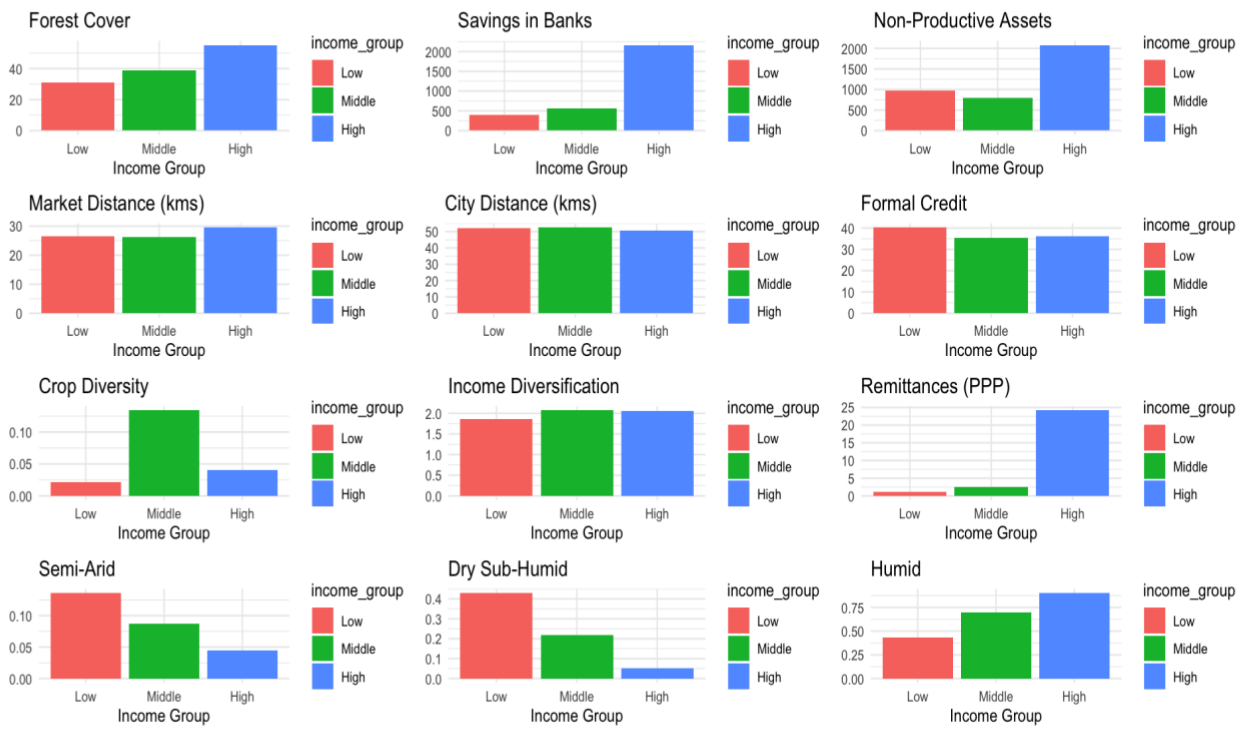
Figure B8a. Differences in household' socio-economic characteristics by Income Group

(Households in Hunting Prevalent Villages)



Source: Author's calculations based on the PEN survey.

Figure B8b. Differences in socio-economic characteristics by Income Group (Households in Hunting Prevalent Villages) (contd.)



Source: Author's calculations based on the PEN survey.

APPENDIX C

ESSAY THREE

Table C.1 : DHS data coverage by country and year.

Country	**Survey Rounds (Years)**
Ethiopia	2005, 2011, 2016, 2019
Ghana	2003, 2008, 2014
Guinea	2005, 2012, 2018
Liberia	2007, 2013
Malawi	2004, 2010, 2016
Nigeria	2003, 2008, 2013, 2018
Uganda	2001, 2006, 2011, 2016
Zambia	2007, 2013, 2018
Zimbabwe	2005, 2010, 2015

Table C.2: Malaria Burden in Selected Countries (Source: WHO)

Country	Malaria Burden summary
Ethiopia	Approximately 68% of the population lives in malarious areas; about 75% of the landmass is considered malaria-prone.
Ghana	Among the top 15 countries globally for malaria burden; approximately 5.2 million cases reported in 2022.
Guinea	Malaria is a leading cause of mortality and morbidity; prevalence among children under 5 was estimated at 17% in 2021.
Liberia	Remains a major public health concern; an estimated 1.9 million cases were reported in 2021.
Nigeria	Highest malaria burden globally; accounts for 27% of the world's malaria cases and deaths. In 2021, recorded approximately 68 million cases and 194,000 deaths.
Malawi	Malaria is endemic in over 95% of the country; responsible for approximately 7 million cases.
Uganda	Among the countries with the highest malaria burden; over 90% of the population is at risk; ~12.6 million cases annually.

Zimbabwe	Malaria is the third most common illness; reported cases fluctuate between 250,000 and 500,000 annually.
Zambia	Among the top 20 countries globally for malaria incidence and mortality; contributes 1.5% of global malaria cases.

Table C.3: ATT Estimates on the Effect of LSLA on child symptoms (with Nightlights as a control)

	(1) FEVER	(2) Cough	(3) Diarrhea
ATT (simple)	0.056	0.007	0.015
SE	0.020	0.023	0.022
p-value	0.006	0.755	0.479
CI low	0.016	-0.038	-0.027
CI high	0.095	0.052	0.058
N	109303	109068	108968
Clusters	6539	6539	6539
Control mean	0.273	0.281	0.172

FE: District; Month; Year; Region×Year×Month. Standard Errors clustered at cluster level.

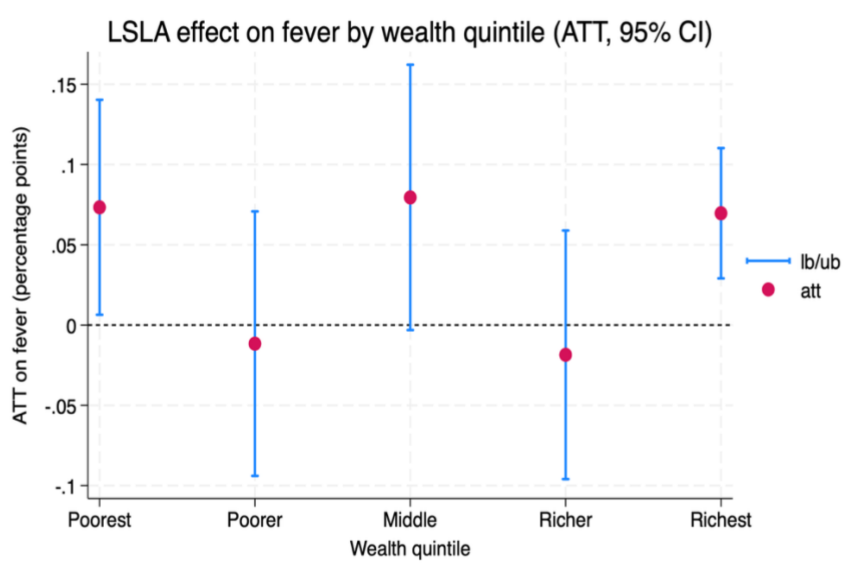


Figure C.1 : Effect of LSLA on fever by wealth quintile

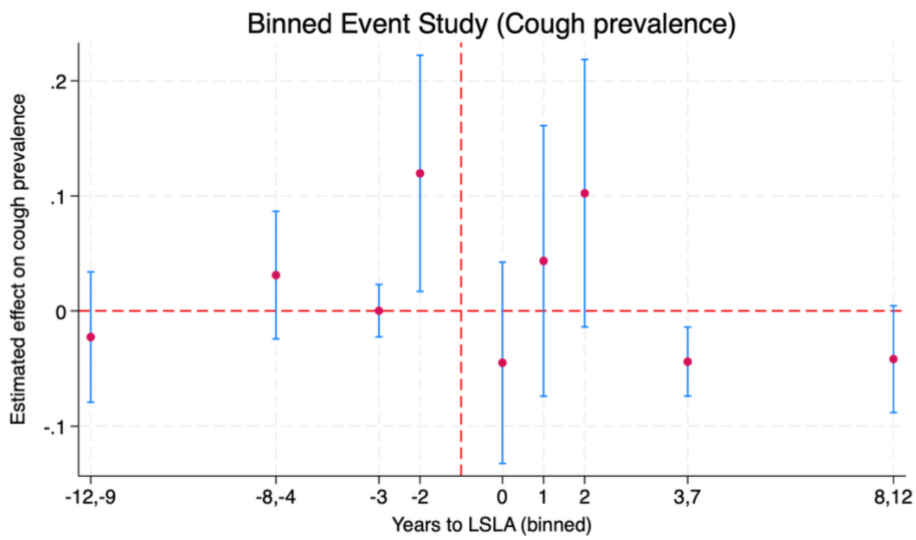


Figure C.2 Event Study (Cough Prevalence)

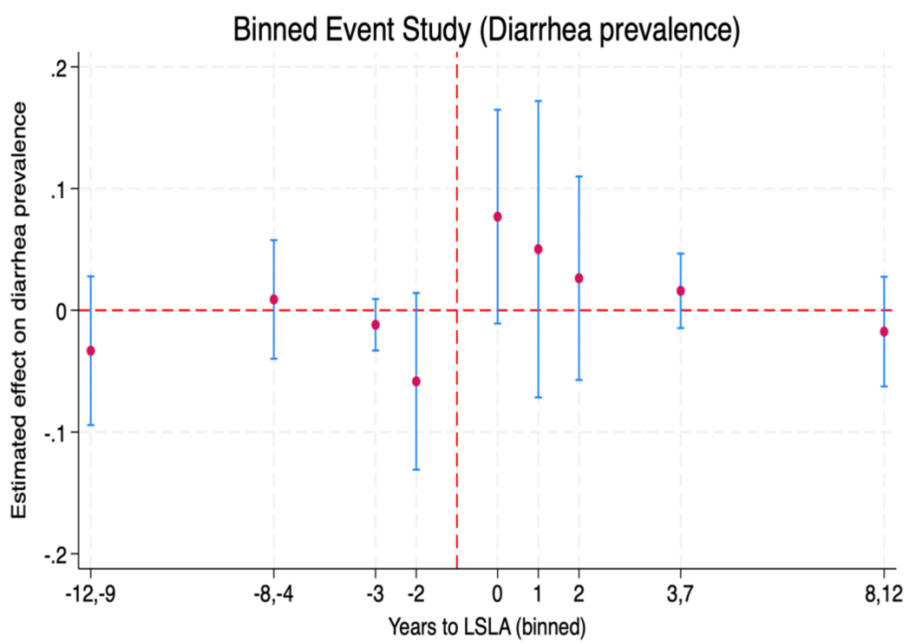


Figure C.3: Event Study (Diarrhea Prevalence)

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