Income Shocks and HIV in Africa*

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Abstract

We examine the effects that economic conditions have on the AIDS epidemic in sub-Saharan Africa. Using data from nineteen countries, we find that income shocks lead to substantial increases in HIV prevalence. We match biomarker data on individuals' HIV status from Demographic and Health Surveys (DHS) to data on local rainfall, an exogenous source of variation in income for rural households. Infection rates for women (men) in HIV-endemic rural areas increase significantly by 14% (11%) for every drought event in the previous 10 years. These increases appear driven by an outward shift in the supply of transactional sex. We estimate that income shocks explain up to 20% of the variation in HIV prevalence across African countries.

JEL Codes: I15, O12, O55

Keywords: Income Shocks, HIV/AIDS, sub-Saharan Africa

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1 Introduction

The relationship between income and health has long been of interest to economists, and a lengthy literature documents strong linkages between economic conditions and many important health outcomes (e.g. Currie, 2009). There has been much less progress, however, in understanding the economic foundations of the HIV/AIDS epidemic. This is particularly true in sub-Saharan Africa (SSA), where each year over a million people become newly infected [UNAIDS, 2010].

There is growing evidence, from micro level studies, that economic conditions affect individual sexual decisions. In the context of SSA, economically induced changes in sexual behavior may have implications for the course of the AIDS epidemic. Women may increase their sexual activity in response to economic hardship in order to obtain transfers (both monetary and in-kind) from their male partners. Commonly known as "transactional sex", this type of activity is not limited to sex workers. There is evidence that women of all types increase their sexual activity, in the form of either riskier sex (i.e. sex without a condom) or through increased partnerships, when faced with a negative income shock [Robinson and Yeh, 2011b, Dinkelman et al., 2008, Luke, 2003]. Two recent experiments involving cash transfer programs in Malawi find that women decrease their sexual activity following temporary increases in income due to these transfers [Baird et al., 2010, Kohler and Thornton, 2011]. Men represent the demand side of this market; there is evidence that increases in income for men lead to greater sexual activity [Kohler and Thornton, 2011]. We stress that transactional sex does not imply payments are made at the time of sexual activity. Women may engage in multiple sexual partnerships as a form of insurance; during an income shortfall, these women may expect transfers to be made by their male partners [Swidler and Watkins, 2007, Robinson and Yeh, 2011a].

Three questions remain: 1) Do the findings above generalize to broader populations in SSA?, 2) What is the effect on actual HIV infections?, and 3) Can increases in transactional sex as a response to income shocks help explain the overall AIDS epidemic in SSA? This paper's primary contribution is answering these three questions. In addition, under some stronger assumptions, we make inferences about the underlying market for transactional sex.

We study the relationship between community-level economic shocks and HIV prevalence across a wide swath of rural SSA. To quantify the effect of local economic shocks on HIV outcomes, we employ the latest rounds of Demographic & Health Surveys (DHS) that contain biomarker data on individuals' HIV status, and match these data to location-specific measures of accumulated drought events over the preceding 10 years.[1] We use drought events as proxies for income shocks both because the DHS lack income data, and because local drought events represent plausibly exogenous variation in income for the majority of Africans who depend on rainfed agriculture for their livelihoods.[2] Our empirical strategy thus compares the HIV status of individuals exposed to droughts in recent years to the status of individuals not exposed to droughts over the same period within the same country.

We find large effects of exposure to drought events (which we call "shocks") on HIV infection, particularly for individuals in rural areas where there is a large, generalized HIV/AIDS epidemic (>5% prevalence). For women in these areas, each shock in the past ten years leads to a statistically significant 1.2 percentage point (ppt) increase in the likelihood of HIV infection. This is a large effect: each shock amounts to a 14% increase in HIV risk (given HIV prevalence for this group is 8.3%). For rural men, we find smaller effects in absolute terms, but similar changes in HIV risk; each shock leads to a significant 0.6 ppt increase in HIV rates, which is an 11% increase in HIV risk given a group prevalence of 5.6%. We estimate very small and insignificant point estimates for the effects of shocks on HIV rates for both men and women in urban areas, consistent with the relative insensitivity of urban incomes to weather-related shocks. Our findings are robust to variations in the definition of drought, to alternate sets of controls, and to varying time windows over which droughts can be accumulated.

A concern with using weather as an income shock is that drought events

[1] As described in detail below, we define drought as a year in which growing-season rainfall is at or below the 15th percentile of local historical rainfall realizations.

[2] Other studies using rainfall variation in sub-Saharan Africa as an exogenous shock to income include Miguel [2005], who examines the relationship between income and witch-killings, Miguel et al. [2004], who instrument income with rainfall in a study of the effects of income shocks on civil conflict, and Hoddinott and Kinsey [2001], who examine the relationship between income and child health outcomes.

could lead to other behavioral changes leading us to misinterpret our results. We investigate the three leading alternative explanations of how rainfall shocks could generate our results: 1) income shocks cause young women to leave school and thus become sexually active earlier and increase their risk for HIV, 2) droughts lead men to temporarily migrate to urban areas where they contract HIV and then upon returning to their rural homes infect their partners, and 3) droughts lead to permanent migration of types that are more likely to be HIV-negative generating a selected sample in our study. We present evidence that our findings are inconsistent with these three explanations.

In addition, we explore changes in the underlying market for sex as the mechanism by which weather-induced income shocks affect HIV. Our reduced form results suggest that shocks increase the equilibrium quantity of transactional sex in the market, which could occur through outward shifts in either supply or demand (or both). Using available occupational data in the DHS we find that rainfall shocks disproportionately increase the risk of HIV for women working in agriculture and men working outside of agriculture. This result is consistent with an outward shift in the supply of sex: women whose income is most sensitive to weather shocks are affected the most, as are men (outside of agriculture) whose incomes are presumably least affected. Using an epidemiological model, we also estimate the magnitude of this shift in supply, and estimate that each shock leads to an increase of 1.35 sexual partners; an estimate that is both plausible and consistent with the existing literature.

Finally, we we estimate the effects of country level weather shocks on national HIV prevalence levels in SSA. We find that countries that have experienced more shocks have higher levels of HIV prevalence. In fact, shocks can explain 14-21% of the cross-country variation in HIV prevalence in SSA.

Our findings contribute to several bodies of literature. First, we build directly on recent work that explores how the supply of risky sex responds to economic shocks. Most of the studies in this area focus on individual level income variation and risky sex, and provide evidence that decreases in a female's income lead to increases in risky sexual behavior [Robinson and Yeh, 2011a,b, Dinkelman et al., 2008, Baird et al., 2010, Kohler and Thornton, 2011]. Other studies examine how aggregate level shocks affect the supply of risky sex. Dupas and Robinson [2011] find that women in western Kenya increase their likelihood of engaging in better-compensated unprotected sex after the 2007 election violence in Kenya. Wilson [2011] shows that the copper mining boom in Zambia generated higher levels of economic prosperity in towns near copper mines and this lead to reductions in transactional sex in these same towns.

Our findings help generalize this literature in two ways. First, our study is one of the first to link negative economic shocks to actual disease outcomes rather than to self-reports of sexual behavior.[3] Not only are HIV infections the primary outcome of interest for policy-makers, but biological markers of risky sex are also not subject to the social desirability bias of self-reports on sexual behavior [Padian et al., 2008, Cleland et al., 2004]. Second, we estimate the relationship between economic shocks and HIV from pooled nationally representative surveys, helping us overcome concerns about generalizability from more local, small-sample studies.

We also contribute to the broader literature (within and outside of economics) that seeks to understand why the the AIDS epidemic has disproportionately affected sub-Saharan Africa. Our results provide strong evidence that a primary source of income variation for rural Africans – rainfall-related variation in agricultural productivity – could be an important contributing factor to the epidemic. These results suggest that economic conditions play a significant role in the AIDS epidemic in SSA and are related to previous work that explores the effects of economic growth on the AIDS epidemic (Oster [Forthcoming]).

Finally, we contribute to a broader body of work on the health and livelihood consequences of income shocks. A host of papers show that when saving is difficult and insurance incomplete, negative income shocks can have seriously detrimental effects on longer-run livelihood outcomes. In the context of the weather-related agricultural shocks we study here, past work has highlighted

[3] There are three recent studies which study the effects of *positive* income transfers on risky sexual behavior using biological markers as primary outcomes. Baird et al. [2012] examining the effects of cash transfers on HIV and HSV-2 outcomes for teenage girls in Malawi, de Walque et al. [2012] study the effects of cash transfers conditioned on testing negative for STIs in rural Tanzania, and Duflo et al. [2011] examine the effects of educational interventions on HSV-2 outcomes in Kenya.

the effect of these shocks on early life nutrition and their subsequent effect on long run health and educational outcomes (Alderman et al., 2006, Maccini and Yang, 2009). Similarly, and using data very close to ours, Kudamatsu et al. [2010] show the widespread effect of drought on infant mortality across Africa. Our work highlights a previously unrecognized - and highly consequential - mechanism by which some African households cope with weather-related income variation. A major difference between our work and the existing literature is that the behavioral response we identify is not only detrimental to an individual's health but one which generates large negative health externalities. As such, it adds further impetus to the growing effort aimed at increasing access to risk management tools in the developing world, and could suggest a role for public subsidy if the negative health externalities brought on by incomplete insurance are as large as we estimate.

The rest of the paper is organized as follows. In section 2 we present a simple conceptual framework to motivate our empirical approach. Section 3 presents the data and our empirical methods, and Section 4 discusses our main results, robustness checks, and consideration of alternative explanations. In Section 5 we present evidence that our results are mostly likely explained by an outward shift in the supply of transactional sex by women, and estimate the size of that supply shift. Section 6 explores how these effects scale up to the country level. Section 7 concludes and discusses policy implications.

2 Conceptual Framework

Our empirical approach, described in detail in Section 3, matches data on an individual's HIV status to data on exposure to recent negative income shocks. Because our survey data lack information on either income or expenditure, we proxy negative income shocks with negative deviations from local mean rainfall. Our reduced form approach thus links equilibrium HIV outcomes to recent local rainfall shocks, which are aggregate shocks to the local agrarian economies that characterize our empirical setting.

Why might rainfall shocks affect HIV outcomes? Clearly increases in sexual partnerships or riskiness of sexual encounters increases one's risk of HIV infection. Transactional sex is thought to be one common form of sexual partnering in Sub-Saharan Africa, one which may include prostitution but stretches far beyond it to include token exchanges and partnership in exchange for small gifts within casual relationships (Hunter [2002], Maganja et al. [2007], Leclerc-Madlala [2003], Béné and Merten [2008]). While there are many factors affecting HIV transmission, transactional sex is thought to be a primary pathway within SSA (Alary and Lowndes [2004], Dunkle et al. [2004], Côté et al. [2004]).

Building on both a theoretical and empirical literature that explores the effect of economic conditions on the market for transactional sex (Robinson and Yeh [2011b], Edlund and Korn [2002]), we propose that rainfall-related income shocks could affect both supply and demand in the transactional sex market, which would in turn shape disease outcomes by changing the quantity (and perhaps the riskiness) of sex transacted.

Consider a simple economy in which individuals can allocate a fixed amount of labor (which we normalize to unity) to working for a wage w. Women have the option of allocating a portion of their labor to the supply of transactional sex, X_i, at wage p, with transactional sex better paid than wage labor ($p > w$).[4] For women, engaging in transactional sex entails a utility cost $\phi(X)$, which represents social stigma or the health cost of becoming infected with HIV.

Each woman i solves the following maximization problem:

$$\begin{aligned} \max_{C,S} \quad & U\left(C_i\right) - \phi(X_i) \\ \text{s.t.} \quad & C_i \leq (1 - X_i)w + pX_i \end{aligned} \tag{1}$$

where C_i is individual i's aggregate consumption, and p is the market price for sex. We assume that $U(.)$ is increasing and concave in C and that women derive no direct utility from transactional sex.[5] We assume that $\phi(.)$ is increasing and

[4] X_i could be considered either an index of riskiness of the average encounter or the number of encounters. Gertler et al. [2005] show that riskier sex (e.g. without a condom) brings a price premium in the commercial sex market, suggesting that riskiness itself is a marketable good.

[5] This does not assume that women do not derive utility from sex, only from transactional

convex in X.[6] Simple comparative statics show that women decrease their supply of transactional sex as prices decrease $\left(\frac{dX_i}{dp} > 0\right)$, and increase their supply as wages decrease $\left(\frac{dX_i}{dw} < 0\right)$ (see Appendix A for comparative statics).

Men choose to spend their wage income on either consumption or transactional sex. The costs of transactional sex are reflected in both the payment for it pX as well as in the health function $\phi(X)$. Male i solves:

$$\begin{aligned} \max_{C,S} \quad & U(C_i, X_i) - \phi(X_i) \\ \text{s.t.} \quad & C_i + pX_i \leq w \end{aligned}$$

Simple comparative statics show that a drop in wages would reduce the demand for transactional sex $\left(\frac{\partial X}{\partial w} \geq 0\right)$ (see appendix A).

Equilibrium effects Equilibrium in the transactional sex market requires that

$$\begin{aligned} Q_S &= Q_D \\ S(p, w) &= D(p, w) \end{aligned}$$

where S is aggregate supply and D is aggregate demand. HIV prevalence is a weakly increasing function of the equilibrium level of transactional sex, that is, $\frac{\partial HIV}{\partial Q} \geq 0$. Suppose that a community level negative shock causes wages to fall for both men and women. The model described here predicts both an increased supply and decreased demand for transactional sex, and as such equilibrium prices will fall (shown formally in appendix A). However, the impacts on HIV infection depend on the change in equilibrium quantity of sex traded, and without further assumptions on the relative price- and income-elasticities of supply and demand, the equilibrium change in quantity is ambiguous.

sex.

[6] The convexity of ϕ with respect to transactional sex X reflects the increasing risk of HIV per encounter when an individual is infected with other STIs. For example, genital ulcer disease increases the transmission rate of HIV [Powers et al., 2008]. Other STIs such as gonorrhea, chlamydia, and syphilis may also increase HIV transmission rates [Chesson and Pinkerton, 2000]. Convexity is not an onerous requirement: the second order conditions require only that ϕ not be concave in X.

The equilibrium effect of aggregate economic shocks on HIV prevalence is thus an empirical question, and identification will depend on finding an exogenous shifter of w. Let θ be an indicator for whether a negative rainfall shock occurred, which we can think of as an instance in which a random draw from an underlying rainfall distribution falls below a predetermined (low) threshold. As we demonstrate empirically, productivity in rain-fed agriculture is heavily dependent on realized rainfall. For individuals whose primary source of income is agriculture, wages will be a decreasing function of these shocks (i.e. $w'(\theta) \leq 0$). Because a higher proportion of the rural labor force is in agriculture, so long as income and price elasticities for sex are roughly similar in rural and urban areas, a simple prediction of the model is that a given θ will have a larger effect on equilibrium levels of transactional sex in rural areas as compared to urban areas.

Finally, the effect of shocks on HIV comes through the change in equilibrium levels of transactional sex $\left(\frac{dHIV}{d\theta} = \frac{dHIV}{dQ}\frac{dQ}{d\theta}\right)$. As our estimates will show, we find that ${}^{dHIV}/_{d\theta} > 0$, suggesting that shocks act to increase the equilibrium quantity of transactional sex. Under the reasonable assumptions of our model, this increase in equilibrium levels of transactional sex is a result of an increase in aggregate supply.[7]

Role of existing prevalence As mentioned, HIV is only weakly increasing in Q: if existing prevalence in the population is very low, increases in transactional sex will have a limited effect on overall HIV prevalence. Let λ represent HIV prevalence. At the extreme,

$$\lambda = 0 \implies \frac{dHIV}{dQ} = 0$$

Intuitively, if no one has HIV, then transactional sex cannot lead to increased prevalence. Further, the risk response is increasing in λ so that

$$\frac{dHIV}{dQd\lambda} > 0$$

[7]Note that, absent an increase in demand, an increase in supply is a necessary but not sufficient condition for an increase in equilibrium quantity.

Empirically, this implies that we should observe a stronger effect of rainfall shocks on HIV in regions with high HIV prevalence.

Predictions To summarize, this simple framework suggests the following:

- An aggregate income shock will increase aggregate supply and decrease aggregate demand for transactional sex, with a theoretically ambiguous effect on equilibrium quantity.
- Any observed net impact should have larger magnitude in rural areas.
- Any observed net impact should have larger magnitude in countries with higher HIV prevalence.

3 Data and empirical approach

3.1 Demographic and Health Surveys

The data on individuals are taken from 21 Demographic and Health Surveys (DHS) conducted in 19 different Sub-Saharan countries.[8] Of the existing DHS surveys available in early 2011, we employ all those that (i) include results from individual-level HIV-tests, and (ii) contain longitude and latitude information, allowing us to map households to data on shocks.[9] For two countries (Kenya and Tanzania), two survey rounds matched these criteria; however, these are separate cross-sections and creation of panel data at the individual or cluster level is not possible. Nonetheless, for each country both rounds are included in the analysis as entirely separate surveys.

Each of these surveys randomly samples clusters of households from stratified regions and then randomly samples households within each cluster. In each sampled household, every woman aged 15-49 is asked questions regarding health, fertility, and sexual behavior.[10] A men's sample is composed of all men

[8] A map of these countries can be found in appendix B.

[9] The one exception is the Mali 2001 survey. We must exclude this survey as it is not possible to link the HIV results to individuals in the GIS-marked clusters.

[10] Mozambique 2009 samples women up to age 64.

within a specified age range within households selected for the men's sample.[11] Depending on the survey, this is either all sampled households, or a random half (or third) of households within each cluster. Details regarding survey-specific sampling are presented in Appendix Table B.1. In all households selected for the men's sample, all surveyed men and women are asked to provide a finger-prick blood smear for serotesting.[12] By employing cluster-specific inverse-probability sampling weights, the HIV prevalence rates estimated with this data are representative at the national level.[13]

Table 1 gives the list of included surveys along with basic survey information. The compiled data contains over 8,000 clusters. On average, there are 25 surveyed individuals per cluster, and 90% of clusters contain between 10 and 50 surveyed individuals. In total, there are over 200,000 individuals in the pooled data. Table 1 also shows HIV prevalence rates for each survey. Overall, women's prevalence is 9.2% and men's is 6.2%. However, these numbers mask a range that varies widely from over 30% prevalence for women in Swaziland to less than 1% prevalence in Senegal. Given that the sexual behavior response to income shocks will have different implications depending on HIV prevalence, we classify countries into two groups: low prevalence countries with less than 5%; and high prevalence countries with over 5% prevalence.[14]

Since the DHS surveys in each country were conducted in different years, we include survey fixed effects in all of our analysis. This controls for any effects that national policies might have on the HIV/AIDS epidemic as well as any time trends of the epidemic. Our analysis is thus focused on making comparisons within country in a given year.

[11] The age range for men is 15 to either 49, 54, 59 or 64, depending on the survey.

[12] Testing success rates for each survey are shown by sex in Appendix table B.2. Refusal rates are 10%, on average. Mishra et al. [2006] examine test refusal rates in DHS testing, which are between 1% and 20%, depending on the country. They conclude that those refusing are more likely to be positive, however the DHS testing accurately represents national prevalence. In this study, individuals exposed to shocks are no less likely to refuse a test.

[13] For details regarding construction of the weights, see Appendix Section B

[14] This categorization follows UNAIDS [2010]. Appendix Figure B.2 shows that with the exception of Cameroon, the prevalence classifications for each country remains stable for the ten years preceding the survey year.

3.2 Weather Data

Weather data are from the "UDel" (University of Delaware) dataset, a 0.5 x 0.5 degree gridded monthly temperature and precipitation dataset [Matsuura and Willmott, 2009].[15] These gridded data are based on interpolated weather station data and have global coverage over land areas from 1900-2008.[16] Using the latitude/longitude data in the DHS, we match each DHS cluster to the weather grid cell in which it falls. Because GIS data in the DHS are recorded at the cluster level, all individuals within a given cluster are assigned the same weather. Our DHS data match to 1701 distinct grid cells in the UDel data.

To capture the seasonality of agriculture, we construct cluster-level estimates of "crop year" rainfall, where the crop year is defined as the twelve months following planting for the main growing season in a region.[17] Annual crop year estimates are generated by summing monthly rainfall across these twelve "crop year" months at a given location.

3.2.1 Measuring weather shocks

We wish to capture occurrences when rainfall accumulations are *unusually low relative to what is normally experienced in a particular location.* That is, given the underlying distribution of rainfall for that location, when is a particular realization in the left tail of this distribution? To estimate the historical distribution of rainfall, we construct estimates of crop-year rainfall back to 1970 at the cell level and use the resulting 38 observations (1970-2008) to estimate a gamma distribution of rainfall for each cell, recovering the shape and scale

[15] 0.5 degrees is roughly 50 kilometers at the equator.

[16] The UDel data are popular in economic applications (recent papers include Jones and Olken [2010], Dell et al. [2008], Bruckner and Ciccone [2011]). Other rainfall datasets are available, but none were sufficient for our needs, lacking either sufficient temporal coverage or spatial resolution.

[17] Estimates of planting dates are derived from Sacks et al. [2010]; planting of staple cereal crops for the primary growing season typically occurs in the boreal (northern hemisphere) spring across most of West and Central Africa, and in the boreal autumn across most of Southern Africa.

parameters for each of our 1701 grid cells.[18][19]

In the absence of definitive estimates of how far into the left tail of the distribution rainfall realization needs to be to constitute a severe "shock", we begin by defining a rainfall shock as a crop-year rainfall realization below the 15% quantile of the local rainfall distribution. These individual rainfall shocks have large effects on agricultural productivity. As shown in Appendix C, panel regressions using country-level data suggest that rainfall shocks cause significant and substantial declines in the productivity of maize, Africa's main staple crop.

Our main independent variable is the number of these shocks that have occurred over the 10 years prior to the survey year at a given location. For instance, if an individual was surveyed in the DHS in 2007, the shock variable takes on a value of between 0 and 10 corresponding to the number of crop-year rainfall realizations in that individual's region between 1997-2006 that fell below the 15% cutoff in the local rainfall distribution. We sum the shocks because acquiring HIV is irreversible – if a shock led to an HIV infection 7 years ago, and that individual is still alive, they will be HIV-positive today – and thus past shocks should have a demonstrable effect on current HIV infection. Note that using a more continuous measure of rainfall - e.g. deviations from average rainfall in levels - would tend to obscure past shocks: the sum of a very bad year and a very good year would be similar to the sum of two normal years. Finally, we choose to sum over the previous 10 years because the median survival time at infection with HIV in sub-Saharan Africa, if untreated, is 9.8 years [Morgan et al., 2002].

Since year-to-year changes in rainfall in a given location are typically assumed to be "as good as randomly assigned," our shock measure is plausibly exogenous. One concern could be that if rainfall shocks are correlated with

[18]The gamma distribution was selected for its considerable flexibility in both shape and scale. Our results do not depend on the choice of gamma, or the estimation of the distribution more generally. Similar findings result from defining shocks as the lowest 5 observations (15%) of the grid's 38 observations, or defining shocks as 1.5SD below the grid mean, as shown in Appendix D.

[19]The period 1970-2008 was chosen to be a long enough period to be relatively insensitive to the recent shocks of interest, but short enough to capture relatively recent averages if long run means are changing (e.g. with climate change). Further, weather data from SSA prior to 1970 are likely less reliable.

the moments of the local rainfall distribution, then our analysis might be subject to omitted variable bias. For example, if recent shocks are correlated with mean rainfall, then it is possible that shocks are also correlated with unobserved factors that affect HIV.

To help confirm that our measure of rainfall shocks is plausibly exogenous and not correlated with other moments of the rainfall distribution, we regress the number of rainfall shocks in the past 10 years on the mean, variance, and skewness of each cluster's rainfall distribution. Table 2 presents the results. While the mean and variance of the rainfall distribution are significantly correlated with the number of rainfall shocks in the simple bivariate estimations (columns 1 & 3), including survey fixed effects weakens this relationship considerably. In all specifications with survey fixed effects, these correlations are not significant. In other words, when we estimate across clusters *within a given survey,* rainfall shocks are orthogonal to all three moments of the distribution. For this reason we include survey fixed-effects in our main specifications to ensure that the accumulation of rainfall shocks is effectively random.

3.3 Estimation

To estimate the effects of negative income shocks on individual HIV rates, we use the following estimating equation:

$$HIV_{ijk} = \alpha + \beta_1 S_j^t + C_j'\zeta + X_i'\delta + \omega_k + \varepsilon_{ijk} \tag{2}$$

where HIV_{ijk} is an indicator that individual i in cluster j tested HIV-positive in survey k. S_j^t is the number of rainfall shocks that cluster j has experienced in the t years before the survey. The default indicator for S_j^t is the number of crop-years with rainfall at or below the 15% quantile for each grid. As discussed above, the default for t is the 10 years preceding the survey. Both S and t are varied over a range to test the robustness of results.

The vector C_j contains characteristics of the cluster j such as location type (rural or urban) and historical average rainfall. The vector X_i contains characteristics of individual i, including gender and age which are not affected by shocks.[20] The survey fixed effect is ω_k and ε_{ijk} is a mean-zero error term. We estimate linear probability models, allowing for correlation of error terms across individuals in the same weather grid by clustering standard errors at the grid level. Survey specific sampling weights are used to make the results representative of individuals living in these 19 countries in Sub-Saharan Africa (see appendix B) .

4 Results

4.1 Main results

Table 3 shows estimations of equation 2 for the full sample and the rural and urban samples. The overall effect of shocks on HIV rates using the full sample (column 1) is positive (.003) and statistically significant. We find strong effects of rainfall shocks on HIV rates in rural areas (column 2), but not in urban areas (column 3), consistent with the prediction from Section 2. In rural areas, we estimate that each shock leads to a 4 ppt increase in HIV prevalence, which is 9.8% increase in HIV rates given a mean of 4.6%.

As characterized in section 2, increases in risky behavior would yield little change in HIV infection rates if existing HIV prevalence is very low. There needs to be both an increase in sexual activity or risk and a pool of potential partners who are HIV-positive for HIV rates to increase. To capture these potential differential effects by prevalence, we estimate the effect of shocks on rural HIV rates in low and high prevalence countries separately (columns 4 and 5). Countries with low prevalence have an approximately zero effect (column 4). In countries with high prevalence (column 5), there is a large and statistically significant effect of nearly one full percentage point, where given that prevalence is 7%, amounts to a 13% increase in HIV infection.

[20] In certain specifications (which are noted), education is used as an individual characteristic. It is categorized as none, some primary, completed primary or beyond primary. To address the concern that income shocks can affect educational attainment, we restrict the samples where education is used to individuals over age 30. Since our analysis relies on drought events in the preceding ten years of each survey, education for this group will be unlikely to have been affected by any shocks that we use for our analysis.

Disaggregating the effects in rural, high prevalence countries by gender, we find that shocks increase the probability of infection by 1.2 ppt for women and 0.6 ppt for men (columns 6 and 7). These changes are large and represent a 14% increase in HIV risk for women and 11% increase for men based on mean prevalence levels.

Overall, our main results are consistent with the following: (i) rainfall shocks increase the equilibrium quantity of transactional sex, suggesting that women increase their supply in response to income shocks; (ii) this effect is most salient in the rural areas where rainfall shocks are most linked to income; (iii) the effect is entirely contained in high-prevalence countries, as increases in sexual behavior have little effect on HIV in areas of very low prevalence.

4.2 Robustness Checks

Our main results are robust to both how we measure shocks as well as the time window we use when counting shocks. We both vary the definition of a shock (i.e. crop years where rainfall is below 1.5 standard deviations) and the time window from which we measure shocks, and find stable and significant results (see appendix D). One concern in our analysis is that shocks could be proxying for other time-invariant cluster characteristics, which are also associated with HIV risk, causing us to conflate the effect of shocks with some other unobservable.[21] We test for this potential confounder using rainfall shocks that occur *after* the survey year of each sample.[22] Future shocks exhibit no correlation with HIV infection.

Figure 1 presents the results when we vary the time window in which we measure shocks. We measure the number of shocks in 3-year bins (e.g. number of shocks between 1-3 years before survey year, number of shocks 4-6 years before, etc) and include these binned variables as regressors in the baseline regression. Figure 1 plots the point estimates of these regressors. The time profile of the effect of shocks on HIV is very much as we would expect: point estimates

[21] Note that by construction, this is presumably not the case: the number of shocks a given location experienced over the last 10 years should be random.

[22] Given that the DHS surveys were conducted between 2003-2009 and our weather data ends in 2008, we are not able to use a 10 year time window, as in our main analysis.

of the effect of shocks peak within the 10-year window, shocks occurring more than 12 years before the survey year become insignificant (but remain positive), and the point estimate for shocks occurring 1-3 years after the survey year is close to zero and not significant.[23]

In addition, our main results are not sensitive to variations in specification, including: varying the age group and sets of controls, excluding sampling weights, and replacing the survey fixed effects with sub-national region and year fixed effects. Results are also not driven by hyper-endemic countries (HIV prevalence +20%).[24]

4.3 Evidence of the Pathway

Our reduced form results suggest that HIV infections are increasing in rainfall shocks. As laid out in section 2, there is reason to believe that this relationship is driven by an increase in the equilibrium quantity of transactional sex. We provide further evidence of this pathway by examining self-reported sexual behavior in our sample. There are a number of limitations when analyzing this data, but overall we find that recent rainfall shocks lead to increases in self reported risky sexual activity. We detail the concerns we have using this data as well as the results in appendix F.

Clearly, a major concern when using weather data, is that drought induced income shocks could be affecting other types of behavior that might explain our results. In this section, we present evidence against the two leading alternative explanations of our results: 1) earlier sexual activity by teenage girls and 2) migration. [25]

[23]We note that binning the shocks across a more narrow time window reduces the precision of our estimates

[24]Two countries in our sample have HIV prevalence over 20%: Lesotho and Swaziland; excluding both of these countries does not affect our results.

[25]Rainfall shocks that lead to lower economic activity may also induce civil conflict (see Miguel et al. [2004]). If civil conflict affects HIV-outcomes, perhaps due to conflict-related sexual violence, then our findings may be reflecting this channel. Recent studies however find no clear link between conflict/violence and HIV (Spiegel et al. [2007];Anema et al. [2008]).

Early sexual activity One possible channel is that income shocks may cause a household to withdraw girls from school (either to earn income or because school fees become unaffordable), and dropping out of school is a strong predictor of age of onset of sexual activity [Baird et al., 2010].[26] It is possible that this earlier onset of sexual activity is generating the results we observe. If this is the pathway, and not increases in transactional sex, we would expect the effects of shocks to be concentrated among the women who were of schooling age when the shocks occurred. In Table 4, we divide the sample into four categories based on age at the time of survey and re-estimate the main equation for each. Women aged 15-20 at the survey ranged in age from 5 to 19 over the preceding ten years – prime schooling age (column 1). In contrast, women aged 32-49 at the survey were aged 22 or older when any of the shocks occurred, an age past which these women are unlikely to be in school. We find that women well past schooling age at the time of the shocks (columns 3 and 4) exhibit statistically significant results similar to our primary results. As a final check, we regress self-reported age of first sexual activity (sexual "debut") and age at first marrage on our shock variable, and find no significant relationships (columns 5 and 6).

Temporary Migration Rainfall shocks could induce rural individuals – particularly males – to migrate temporarily to cities. It maybe that these individuals become infected with HIV in urban areas, and return to the countryside and infect their spouses. Since we do not have data on migration patterns, we propose a test. Individuals are more likely migrate to urban areas if they reside nearby one. If temporary migration were driving our results, then we would expect shocks to have a smaller effect on HIV in areas more distant from urban areas.[27]

Our measures of distance from the nearest urban area, as well as the popu-

[26]Alternatively, income shocks could affect dynamics in the marriage market, with households marrying off their daughters in response to a negative income shock in order to receive the bride payment (in regions where such payment is customary, e.g. see Hoogeveen et al. [2011]).

[27]The difficulty of course is that distance-to-city is likely correlated with a number of other things that also affect HIV prevalence, so we are hesitant to assign a causal interpretation to any of the distance variables.

lation size of settled areas, are derived from the Global Rural-Urban Mapping Project [CIESIN, 2010]. We classify households within 100 kilometers of an urban area to be "nearby", and we vary the population threshold that qualifies a settlement as being "urban" (100,000 inhabitants, 250,000 inhabitants, or 500,000 inhabitants). [28]

Results are shown in the top panel of Table 5. If temporary urban migration were driving our results, then the interaction between our shock measure and the indicator for "near to urban area" should be positive. The coefficient on this interaction is very small and statistically insignificant for both men and women, except for one case - where we find the opposite sign than the migration story would suggest. Overall, there is little evidence that temporary migration is driving our results.

Permanent Migration Another potential explanation is a selection story, in which certain types of individuals respond to shocks by permanently migrating. If these types are more likely to be HIV negative, then the remaining population would be disproportionately HIV positive, again causing an association between shocks and HIV absent any sexual behavior response. In other words, our findings might be a result of attrition in communities experiencing shocks.

In order to test whether selective migration can account for the results we find, we conduct a bounding exercise motivated by Lee [2009]. We estimate that a community loses 3% of it's population per shock (see appendix G for more details) and that each of these individuals is HIV-negative. We put these individuals back into our sample and reestimate our main results. This in effect stacks the cards against us finding a result; communities that experience shocks now have more HIV-negative individuals. The lower panel of Table 5 first reproduces our primary result: in rural areas of high-prevalence countries, probability of infection increases by 0.9 percentage points per shock. The second column assumes 3% migration (attrition) per shock, while the third column uses 5% migration *per shock*. We find that even accounting for massive out-migration (up

[28]Population data are from the year 2000, helping to mitigate concerns that urban population size could have itself responded to the shocks of interest (for most of our sample, the shocks of interest are post-2000).

to 30% in some clusters) where we assume that all migrants are HIV-negative, our results are still significant. It appears that sample selection/attrition due to permanent migration cannot explain our results.

5 The Supply Response to Shocks

Evidence for a positive supply response

We propose in this work that rainfall affects HIV through income shocks, via the market for transactional sex. That is, when women face unexpectedly low income, they increase their supply of transactional sex. If this prosposed pathway is valid, a few logical predictions follow: (i) individuals whose income depends on rainfall should experience more significant income shocks from drought than those whose income does not; and (ii) individuals experiencing larger income shocks should have a stronger behavioral response, that is, women will increase supply more and men will decrease demand more.

While we do not observe wages, we do observe occupation; in particular, whether or not an individual's primary income source is from agriculture. Based on the simple assumption that incomes of agricultural workers are more sensitive to drought than others, we would expect to see that, for women, the largest effects of drought on HIV are among those employed in agriculture. These women experience a larger income shock, and thus increase their supply the most. For men, those employed in agriculture would have the greatest shock-induced reduction in demand, leaving men employed outside agriculture to consume the excess supply at the new, lower price. Therefore we would expect that, for men, the largest effects of drought on HIV would be among those employed outside agriculture.

A practical concern in using our occupational data is that we are able to classify individuals by their employment type at the time of the survey but not at the time of the shock. Our analysis thus makes the assumption that occupation is fairly persistent: individuals in agriculture at the time of survey are *more likely* to have been in agriculture at the time of the shock, and thus our occupational categories are meaningful.[29] [30]

The first four columns of Table 6 show differential effects of shocks on HIV by gender and occupation, for individuals in high prevalence countries employed in the past year. For a woman working in agriculture, each rainfall shock increases her probability of infection by 1.3 ppt, or 17%, while the effect for women working outside agriculture is not statistically different from zero. We cannot reject the null that the effects of shocks on HIV for the two groups are the same (p-value = 0.17). The results for men suggest larger impacts for non-agricultural men, with a single shock resulting in a significant 1ppt increase in HIV for men in this group. Our point estimate for the impacts on agricultural men are less than half as large, but as with the women, we cannot reject the null that impacts for agricultural and non-agricultural men are identical (the p-value = 0.25).

What does this imply? These results are consistent with a model where women experiencing income shocks increase their supply of transactional sex as a risk coping mechanism. This outward shift in the supply curve lowers prices in this market. Men working outside agriculture, whose incomes are relatively less affected by weather shocks, are those who consume more transactional sex as a response to the decrease in prices. These results offer evidence of increases in the supply of transactional sex as a valid mechanism by which rainfall may affect HIV.

As further evidence on the role of supply shifts, we explore whether women who are likely to have alternative risk coping mechanisms respond differentially to shocks. In particular, we might expect responses to shocks to vary by educational levels, with more educated women having other income generating opportunities and thus being less likely to engage in transactional sex in response to a shock.

[29] We include only those employed in the last year, as the unemployed do not report an occupation. As such, it is difficult to assume whether the currently unemployed previously worked in agriculture or not.

[30] An additional concern is that occupational category may be endogenous to shocks. We examine the predictive effect of number of shocks in the past 10 years on current employment in rural areas, to check its potential to induce bias. Shocks have no predictive effect for employment in agriculture (results available upon request).

To test for this, we include an indicator for whether an individual completed primary school in our main specification for women, and interact it with the measure of shocks. Because finishing primary school is negatively correlated with working in agriculture, and to explore differential effects of education by occupation type, we run separate regressions by occupation. We find that shocks have a significantly different effect on women who are primary school graduates (Table 6; Column 5).[31] Agricultural women without primary school education are 1.8 percentage points more likely to be infected with HIV per shock, while shocks have no statistically significant effect on women in agriculture who are primary school graduates.[32] Since educational decisions may be endogenous to shocks, we also limit the sample to women age +30 at the time of the survey, who would likely have completed their education before our shocks of interest occur. We find nearly identical significant effects for this sample (column 7), despite the reduction in sample size.

Estimating the size of the supply response

In this section, we introduce a bit more structure to further investigate changes in the market for transactional sex resulting from local income shocks. Since our shocks are on a community level, we expect that they exert both income and price effects on individual behavior. Our goal in this section is to distinguish changes resulting from price vs. income effects in order to estimate the size of the supply response to changes in income. Estimating the impact of income effects on supply allows us to: 1) compare our results to the previous work we have cited to see if our results are plausible, and 2) understand the magnitude of the income effect. The latter is important for policy; a large income effect on the supply side would warrant further research on the role that risk coping mechanisms such as insurance and savings could have on the AIDS epidemic.

We note, given our stylized context, there are three factors that affect an female's risk of HIV infection: 1) income shocks that lead to a shift out in the

[31] We also include controls for household assets in these specifications to differentiate the effects of education vs. wealth.

[32] The linear combination of Shocks+(Shocks X Primary School) has p=0.46

supply of transacitonal sex, 2) the price of transacitonal sex, and 3) community prevalence. These first two factors go in opposite directions, that is as the supply curve shifts out, it lowers prices in the market, resulting in a decrease in the willingess to supply transactional sex. To help clarify these two concepts, we use notation from section 2:

$$\begin{aligned} A: \frac{dHIV_i}{dw_i} &= \frac{\partial HIV_i}{\partial X_i}\frac{\partial X_i}{\partial w_i} \leq 0 \\ B: \frac{\partial HIV}{\partial p} &= \frac{\partial HIV_i}{\partial X_i}\frac{\partial X_i}{\partial p} \geq 0 \end{aligned}$$

where the income effect is A, or as wages in the labor market decrease (w_i), we expect women to supply more transactional sex (X_i), which leads to an increase in their HIV risk, and the price effect is B, or as the price of transctional sex decreases, women supply less transacitonal sex (X_i), and HIV risk decreases. We also note, that community HIV prevalence weakly affects a female's HIV risk, as it increases the likelihood a woman matches with an HIV-positive partner:[33] $C: \frac{\partial HIV}{\partial \lambda} \geq 0$

We make the following assumptions:

1. Women working outside agriculture have wages unaffected by rainfall and thus are immune from the income effect (A).

2. All women are subject to the change in the market price, and price elasticities are the same across sectors. That is, the supply of sex for women in agriculture and non-agriculture responds similarly to changes in price. Therefore, the price effects (B) are the same for women in both sectors.

3. Women in both sectors randomly match with male partners and face the same likelihood of matching with an HIV-positive partner. The effects of community prevalence on HIV rates (C) are therefore the same for women in both sectors.[34]

[33] We stress that this is a weak inequality because women in long term mongmous relationships may face no increased risk of HIV in the event that community prevalence increases.

[34] We can relax this assumption and generate similar results (see appendix E).

In other words, we are assuming that women in both sectors have similar sexual behavior, and the only difference is that women in the agricultural sector are expericncing an income shock that induces a shift out in their supply of transactional sex. Given this characterization, we can estimate income effects by using the results disaggregated by occupation. For women working in agriculture, the .013 increase in Pr(HIV) represents the sum of the income, price and prevalence effects (A, B, and C). For women working outside agriculture, the .006 increase represents the sum of the price and prevalence effects (B and C). Therefore, the income effect on the increase in supply (A) is estimated by the difference, $.013 - .006 = .07$. However, this difference is given in the likelihood of an individual's risk of HIV infection; to estimate the actual change in underlying sexual behavior, we use an epidemological model. In other words, we estimate the change in sexual partnerships that would result in a .07 ppt increase in HIV infection.

Following the methodology developed by Gong [2011], we use a simple epidemiological model to estimate the change in the number of sexual partners for a given a change in HIV infection rates. The model takes as parameters the HIV transmission rate, condom usage, and the likelihood of matching with an HIV-infected partner. The full model with parameter values and assumptions is found in Appendix E. As with all modeling exercises that involve sexual behavior and HIV infections, the estimates generated are sensitive to the parameter values. In this exercise, we rely on parameter values from the health and epidemiological peer-reviewed literature, and we are explicit on the assumptions that we make.

For women, we employ the 0.07 change in HIV calculated above, and the model suggests an additional 1.35 partners per shock. This represents the increase in supply as a response to an income shock. In this sample, women report on average 2.11 lifetime partners. Based on clinical trials using prostate-specific antigen, which detects sexual activity in the past 48 hours, Minnis et al. [2009] showed that women in Zimbabwe underreport sexual behavior by about 50%. This suggests an average of 4 lifetime partners per woman. Annualizing based on the average woman in the sample (age 28 with sexual debut at 16), this averages to one partner every three years. Given this, an increase of about one additional partner in the event of a drought shock seems reasonable.

How do these estimated changes in sexual behavior compare to other studies? The only study of which we are aware that provides an estimate of the supply of transactional sex as a response to income shocks is Robinson and Yeh [2011a]. However, this is a difficult comparison case, as their study is based on a sample of 192 women who identify as sex workers and average more than 1.5 partners *per day.* Given the significant difference between the samples, it is difficult to say whether one would expect the supply responses to be similar. Nonetheless, we report their results as they are the nearest comparison. Robinson and Yeh find that an individual level health shock that results in total income loss for one day leads women to increase their number of sexual partners the following day by 0.3, an 18% increase. We find that this is comparable to our findings that a year-long income shock increases a woman's lifetime partnerships by about 34%.

6 Macro level implications

Our results so far, in addition to the previously cited micro level work on shocks and sexual behavior, suggest that economic conditions play an important role in an individual's risk of HIV infection. An important question is, "Do our results generalize to a country-level?" In other words, can income shocks account for the variation in HIV prevalence across countries in sub-Saharan Africa? It is not immediately obvious that increases in an individual's risk of HIV leads to increased HIV prevalence in equilibrium. Individual's who are HIV-negative may abstain sexually as a response to higher risks of infection. To address this question, we apply our basic approach of using drought events to proxy for income shocks to country-level estimates of HIV prevalence provided by UNAIDS.

UNAIDS estimates of country level HIV prevalence uses data from the DHS, but also incorporates data from many other sources (e.g. antenatal surveys), and provide estimates of prevalence distinct from that used above. We use the same gridded climate data to derive a time series of annual average rainfall for each country, where the observation for a given country-year is a weighted average of all the grid cells in that country, using percent of each cell covered by cropland as weights.[35] Similar to above, we calculate these annual rainfall totals for each country back to 1970, fit a separate gamma distribution to each country's time series, and define a shock as a year in which country-average rainfall fell below the 15th percentile in that country's rainfall distribution. We then seek to explain the cross-sectional prevalence in HIV in a given year as a function of accumulated shocks over the previous decade.

Figure 2 plots these relationships for the two decades for which UNAIDS reports data. Countries with a higher number of shocks are more likely to have higher levels of HIV-prevalence; this is true both in the 1990s (left plot) when the epidemic was growing rapidly, as well as in the 2000s, when the epidemic has plateaued or started to decline in many countries. These simple cross sectional relationships are statistically significant and explain 14-21% of the cross-sectional variation in HIV prevalence across the continent (see Appendix H for regression results).

These results generate two conclusions. First, the fact that we can replicate our basic micro level results using different sources of variation on both the left- and right-hand side gives us additional confidence that we have identified a potential pathway (transactional sex) linking economic conditions to HIV prevalence. Secondly, given that many areas in sub-Saharan Africa lack social safety nets and depend heavily on rainfed agriculture, weather fluctuations that generate conditions such as droughts may play an important and prominent role in explaining why the AIDS epidemic has disproportionately affected sub-Saharan Africa.

[35]This provides country-level rainfall estimates that are relevant for agriculture but that are also effectively weighted by rural population density, since areas that are farmed more intensively in rural Africa tend to be areas with higher population density (given very small average farm plot size).

7 Conclusion

Ultimately any halt to the AIDS epidemic will require a medical intervention, such as a vaccine or methods approximating one, such as the aggressive use of ARVs. However, much progress can be made if we think about how economic factors play a role in the epidemic. Two notions are widely accepted by researchers in the HIV/AIDS community: (i) heterosexual sex is a primary driver of the AIDS epidemic in sub-Saharan Africa, and (ii) economic conditions play some role in the sexual behavior of women in these countries. Our paper provides compelling evidence that economic conditions, in the form of income shocks, are a significant driver of the AIDS epidemic in sub-Saharan Africa.

The policy implications of these findings are striking. When women experience a negative income shock, they significantly increase their number of sexual partners, by about one partner per shock. It is clear that this behavior is a risk-coping mechanism, as women that have completed primary education, who assumedly have other means to earn, have a significantly muted effect. This income-smoothing behavior increases not only their own risk of HIV, but that of their partners, and in fact, everyone in their community. This behavior, assumedly undesirable to the woman herself, also exerts significant negative externalities from both public health and economic perspectives.

Any policies that prevent the need for this coping mechanism would yield large positive returns. Comprehensive social safety nets may unfortunately be an unrealistic short-run goal for many revenue and capacity-constrained governments on the continent. However, more targeted interventions such as access to credit and savings, weather-indexed crop insurance or the development of drought-resistant crop varieties could have an indirect affect on the spread of HIV by reducing the sensitivity of incomes to rainfall shocks. Our results suggest that the social returns to investments in these and related interventions could be much larger than previously thought, particularly in countries where HIV prevalence remains high.

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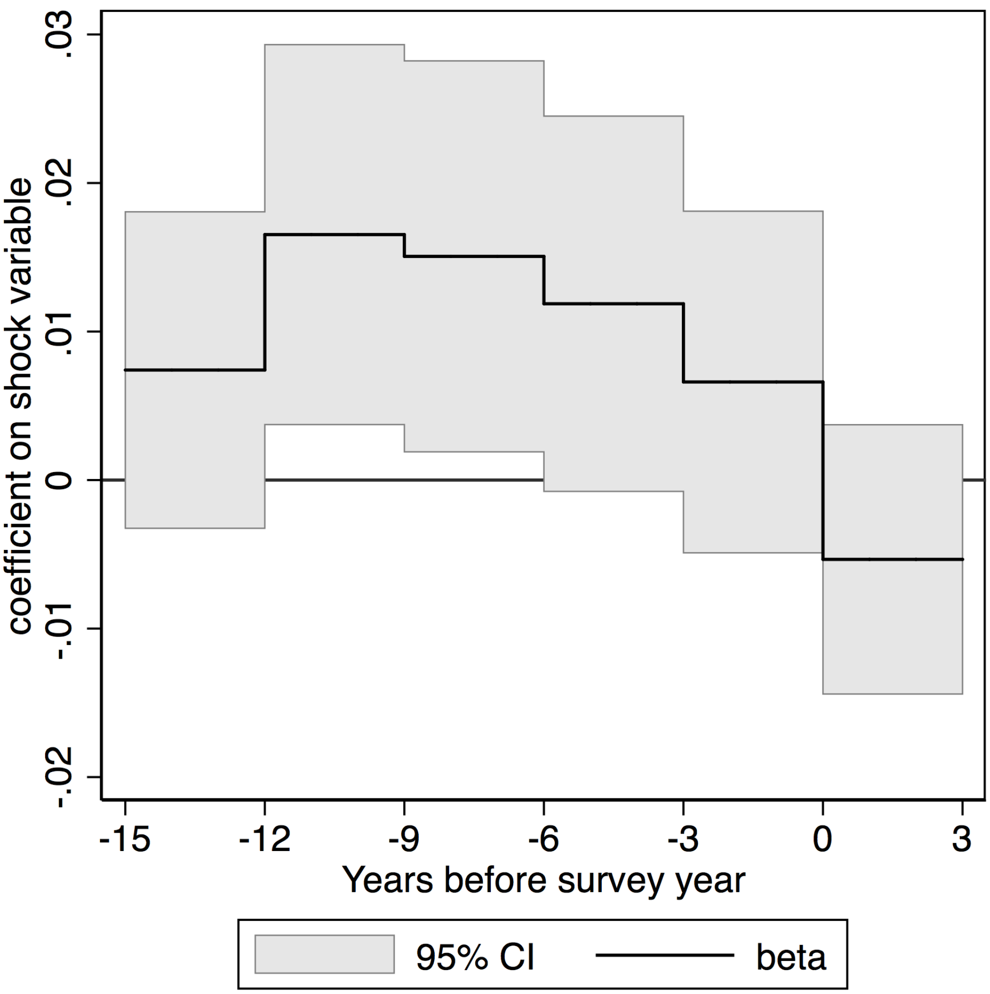
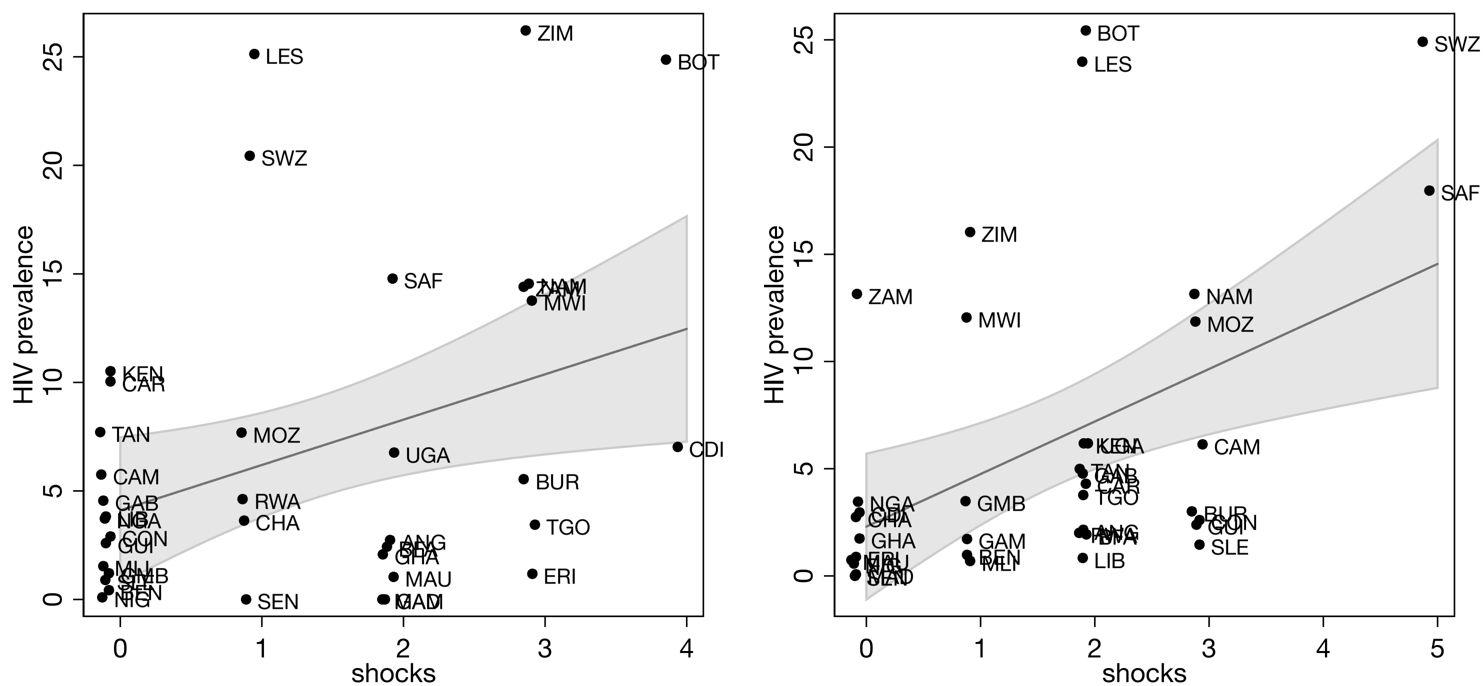
Figures

Figure 1: Effect of rainfall shocks on HIV, by shock timing



Note: Black lines represent coefficient point estimates from running the main specification with 3-year binned shocks replacing the single 10-year shock measure. Grey areas represent 95% confidence intervals around each point estimate.

Figure 2: Country-level HIV prevalence & Shocks



Note: The left panel presents results for HIV prevalence in 1999 (y-axis) and accumulated shocks over the previous decade (x-axis). The right panel presents results for HIV prevalence in 2008 and accumulated shocks since 2000. HIV data are from UNAIDS (2010). Dark lines are linear least squares fits, with gray areas representing the 95% confidence interval. Data are jittered to make country labels more legible.

Tables

Table 1: DHS Survey Information

					Prevalence			
	Country	Year	Clusters	Individuals	Female	Male	Overall	Category
1	Swaziland	2007	271	8,186	31.1%	19.7%	25.9%	High
2	Lesotho	2004	381	5,254	26.4%	18.9%	23.2%	High
3	Zambia	2007	398	26,098	21.1%	14.8%	18.1%	High
4	Zimbabwe	2006	319	10,874	16.1%	12.3%	14.2%	High
5	Malawi	2004	521	5,268	13.3%	10.2%	11.8%	High
6	Mozambique	2009	270	10,305	12.7%	9.0%	11.1%	High
7	Tanzania	2008	345	10,743	7.7%	6.3%	7.0%	High
8	Kenya	2003	399	6,188	8.7%	4.6%	6.7%	High
9	Kenya	2009	397	6,906	8.0%	4.6%	6.4%	High
10	Tanzania	2004	466	15,044	6.6%	4.6%	5.7%	High
11	Cameroon	2004	466	10,195	6.6%	3.9%	5.3%	High
12	Rwanda	2005	460	10,391	3.6%	2.2%	3.0%	Low
13	Ghana	2003	412	9,554	2.7%	1.6%	2.2%	Low
14	Burkina Faso	2003	399	7,530	1.8%	1.9%	1.9%	Low
15	Liberia	2007	291	11,688	1.9%	1.2%	1.6%	Low
16	Guinea	2005	291	6,767	1.9%	1.1%	1.5%	Low
17	Sierra Leone	2008	350	6,475	1.7%	1.2%	1.5%	Low
18	Ethiopia	2005	529	11,049	1.9%	0.9%	1.4%	Low
19	Mali	2006	405	8,629	1.5%	1.1%	1.3%	Low
20	Congo DR	2007	293	8,936	1.6%	0.9%	1.3%	Low
21	Senegal	2005	368	7,716	0.9%	0.4%	0.7%	Low
	Total		8031	203,796	9.2%	6.2%	7.8%	

Prevalence estimates are weighted to be representative at the national level.

Table 2: Rainfall Shocks and Overall Variability

Dependent Variable: Number of 15% rainfall shocks in past 10 years.

	(1) Pooled	(2) SurveyFE	(3) Pooled	(4) SurveyFE	(5) Pooled	(6) SurveyFE
Log Mean Rainfall	.362***	.039				
	(.087)	(.083)				
Log Var Rainfall			.262***	-.086		
			(.059)	(.061)		
Skew Rainfall					-.207	-.382
					(.320)	(.244)
Observations	8031	8031	8031	8031	8031	8031
R^2	.031	.359	.040	.360	.001	.360

Robust standard errors are shown in parentheses, clustered on the grid level.

Table 3: Effect of Shocks on HIV

				Rural		Rural, Hi Prev	
	All	Rural	Urban	Low Prev	Hi Prev	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
15% Shocks (10 Yrs)	.003**	.004**	-.001	-.000	.009***	.012***	.006**
	(.001)	(.002)	(.002)	(.001)	(.003)	(.004)	(.003)
Observations	202216	134874	67342	57114	77760	43147	34613
R^2	.053	.046	.063	.003	.031	.029	.032
Mean Dependant Var	.050	.041	.070	.010	.070	.083	.057

All specifications include controls for gender, age, mean rainfall, rural/urban designation, and survey fixed effects. Columns 4-6 employ sub-samples of countries by overall prevalence (Hi >5%). Estimations are weighted to be representative of the included countries. Robust standard errors are shown in parentheses clustered at the grid level.

Table 4: Alternative Pathway: Early School Leaving or Early Sexual Debut

	Dep. Var: HIV				Dep. Var:	
	Age15_20	Age21_31	Age32_41	Age42+	DebutAge	MarryAge
	(1)	(2)	(3)	(4)	(5)	(6)
15% Shock (10 yrs)	0.002	0.017***	0.012**	0.015**	-0.030	0.077*
	(0.003)	(0.005)	(0.006)	(0.006)	(0.047)	(0.045)
Observations	11440	16156	9604	5850	36997	33063
R^2	0.020	0.036	0.042	0.027	0.036	0.050
Mean Dependent Var	0.033	0.102	0.110	0.077	16.694	18.009

Female, rural sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Table 5: Alternative Pathway: Migration

	Women			Men		
TEMPORARY	(1)	(2)	(3)	(4)	(5)	(6)
	100K	250K	500K	100K	250K	500K
15% Shock (10 Years)	0.011***	0.014***	0.012***	0.006*	0.007**	0.006**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Near urban	0.010	0.048***	0.019	0.012	0.022	0.004
	(0.011)	(0.017)	(0.024)	(0.009)	(0.014)	(0.019)
Shock * Near urban	0.003	-0.024***	-0.003	0.001	-0.007	0.008
	(0.007)	(0.008)	(0.016)	(0.005)	(0.006)	(0.012)
Observations	43147	43147	43147	34613	34613	34613
R^2	0.030	0.030	0.029	0.033	0.033	0.032

PERMANENT	(1)	(2)	(3)
	Observed	ThreePct	FivePct
15% Shock (10 Years)	.009***	.006**	.005*
	(.003)	(.003)	(.003)
Observations	77760	84259	88890
R^2	.021	.023	.023

Rural sample from high-prevalence countries. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level. **Upper Panel:** Specifications include controls for age, mean rainfall, and survey fixed effects. The "Near urban" variable indicates whether a given cluster is within 100km of an urban area (defined as the population size in the column header). Locale populations from the Global Rural-Urban Mapping Project. **Lower Panel:** Specifications include controls for mean rainfall and survey fixed effects. Columns (2) and (3) include additional observations to account for out-migration (see text).

Table 6: Effect of Shocks By Occupation

	Men		Women				Women age 30+	
	Agric	NonAg	Agric	NonAg	Agric	NonAg	Agric	NonAg
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
15% Shock (10 Years)	.004	.010**	.013***	.006	.018***	.013	.019***	.010
	(.003)	(.005)	(.004)	(.006)	(.004)	(.009)	(.006)	(.012)
Primary Sch					.024**	.005	.038**	-.009
					(.011)	(.018)	(.015)	(.027)
Shock X Primary Sch					-.014**	-.012	-.016**	-.015
					(.006)	(.009)	(.008)	(.014)
Observations	18845	18740	20586	16901	20585	16901	10657	8217
R^2	.022	.044	.020	.036	.024	.039	.028	.055
Mean Dependent Var	.056	.096	.077	.148	.077	.148	.088	.178

Employed sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

For Online Publication: Appendices

A Comparative Statics

This section details the comparative statics for the men's and women's utility maximization problems presented in the conceptual framework.

Women The comparative statics for the women's maximization problems is as follows (where to simplify notation we assume that there is just one wage w). Assuming that the constraint binds, the woman maximizes:

$$U\left((1-X_i)w + pX_i\right) - \phi(X_i)$$

The FOC is:

$$U_X(p-w) - \phi_X = 0$$

The second order condition is:

$$U_{XX}(p-w)^2 - \phi_{XX} < 0$$

Assuming U(.) is concave and $p - w > 0$, the SOC shows that we need ϕ to be linear or convex in X, which does not appear an unreasonable condition. Totally differentiating the FOC gives us the following expression for how supply of transactional sex changes with the wage:

$$\frac{dX}{dw} = \frac{U_X - (1-X)(p-w)U_{XX}}{(p-w)U_{XX} - \phi_{XX}}$$

If U is increasing and concave, the numerator is always positive, and as long as ϕ is linear or convex and $p > w$, the denominator is negative, which gives

$dX/dw < 0$. Comparative statics with respect to price give us:

$$\frac{dX}{dp} = \frac{-U_X + X(p-w)U_{XX}}{(p-w)^2 U_{XX} - \phi_{XX}}$$

which is positive under the same assumptions (both the numerator and denominator are negative).

Men

$$\begin{aligned} \max_{C,X} \quad & U(C_i, X_i) - \phi(X_i) \\ \text{s.t.} \quad & C_i + pX_i \leq w \end{aligned}$$

The FOCS:

$$\begin{aligned} U_C - \lambda &= 0 \\ U_X - \phi_X - \lambda p &= 0 \\ w - C - pX &= 0 \end{aligned}$$

Substituting the first equation into the second generates two linear equations

$$\begin{aligned} U_X - \phi_X - pU_C &= 0 \\ w - C - pX &= 0 \end{aligned}$$

Total differentiation leads to:

$$\begin{aligned} (U_{XC} - pU_{CC})\,dC + (U_{XX} - \phi_{XX} - pU_{CX})\,dX + (-U_C)\,dp &= 0 \\ -dC - pdX + dw - Xdp &= 0 \end{aligned}$$

Using Cramer's Rule we find $\frac{\partial X}{\partial w} = \frac{|C|}{|D|}$ where:

$$\begin{aligned} |C| &= \begin{vmatrix} (U_{SC} - pU_{CC}) & 0 \\ -1 & 1 \end{vmatrix} \\ &= (U_{SC} - pU_{CC}) \end{aligned}$$

$$\begin{aligned} |D| &= \begin{vmatrix} (U_{SC} - pU_{CC}) & (U_{SS} - \phi_{SS} - pU_{CS}) \\ -1 & -p \end{vmatrix} \\ &= \underbrace{[(U_{SC} - pU_{CC})(-p)]}_{d_1} - \underbrace{[(-1)(U_{SS} - \phi_{SS} - pU_{CS})]}_{d_2} \end{aligned}$$

We assume for simplifying reasons that the cross partial (U_{SC}) is zero, or that levels of consumption have no effect on the utility derived from transactional sex. This implies that $|C| < 0$. For $|D|$, $d_1 < 0$ and $d_2 > 0$ (given the convexity of ϕ) which implies that $|D| < 0$, and thus $\frac{\partial S}{\partial w} \geq 0$ or that as wages increase for men, the demand for transactional sex S increases.

Equilibrium changes We know that in equilibrium

$$Q_S = Q_D$$

The question is how do prices and quantity of sex change after a shock? Shocks will lower wages causing an outward shift in supply and this will also effect prices. On the demand side, shocks will also lower wages causing a inward shift in demand and effect prices. We therefore can write both Q_S and Q_D as a function of θ:

$$\begin{aligned} Q_S &= S(p(\theta), w(\theta)) \\ Q_D &= D(p(\theta), w(\theta)) \end{aligned}$$

Using the equilibrium condition

$$D(p,w) = S(p,w)$$

and totally differentiating

$$\frac{\partial D}{\partial p}\frac{dp}{d\theta} + \frac{\partial D}{\partial w}\frac{dw}{d\theta} = \frac{\partial S}{\partial p}\frac{dp}{d\theta} + \frac{\partial S}{\partial w}\frac{dw}{d\theta}$$

$$\begin{aligned}\left(\frac{\partial D}{\partial p} - \frac{\partial S}{\partial p}\right)\frac{dp}{d\theta} &= \left(\frac{\partial S}{\partial w} - \frac{\partial D}{\partial w}\right)\frac{dw}{d\theta} \\ \frac{dp}{d\theta} &= \frac{\left(\frac{\partial S}{\partial w} - \frac{\partial D}{\partial w}\right)\frac{dw}{d\theta}}{\left(\frac{\partial D}{\partial p} - \frac{\partial S}{\partial p}\right)}\end{aligned}$$

we know the denominator is negative since $\frac{\partial D}{\partial p} \leq 0$ and $\frac{\partial S}{\partial p} \geq 0$. For the numerator, given that $\frac{dw}{d\theta} < 0$ and that supply decreases $\frac{\partial S}{\partial w} \leq 0$ while demand increases $\frac{\partial D}{\partial w} \geq 0$ as a response to higher income (wages), the numerator is positive. Thus $\frac{dp}{d\theta} \leq 0$ or that as shocks increases, the price of transactional sex decreases.

From the supply side (women) we get the same result:

$$Q = S(p(\theta), w(\theta))$$

$$\frac{dQ}{d\theta} = \frac{\partial S}{\partial p}\frac{dp}{d\theta} + \frac{\partial S}{\partial w}\frac{dw}{d\theta}$$

since $\frac{dp}{d\theta} \leq 0$ and $\frac{\partial S}{\partial p} \geq 0$ then $\frac{\partial S}{\partial p}\frac{dp}{d\theta} \leq 0$ and given that $\frac{dw}{d\theta} \leq 0$ and $\frac{\partial S}{\partial w} \leq 0$ then $\frac{\partial S}{\partial w}\frac{dw}{d\theta} \geq 0$ and again the change in equilibrium quantity is ambiguous.

B DHS Data

Weighting

Sampling weights are used in this paper so that estimated effects represent the average effect of the population of interest (the population of 19 sub-Saharan African countries). The sampling weights are constructed as follows.

- Each individual is assigned an inflation factor that is $\rho = {}^{N_c}/_{n_c}$ where n_c is the sample size for survey in which he appears, and N_c is the population of his country in the year of that survey.

- Further, each individual has a survey-specific inflation factor h that is provided in the DHS data. h is the inverse probability of his HIV test results being present in the data. MEASURE DHS calculates h based on an individual's probability of being sampled for HIV testing (based on stratification of the survey) and his probability of providing a blood sample if requested, based on observable characteristics.

- A composite weight that is the product of ρ and h is employed in all specifications. A robustness check shows that the primary results of this work are not dependent on the use of sampling weights.

Figure B.1: Countries included in the study. Darker shades corresponding to higher HIV prevalence.

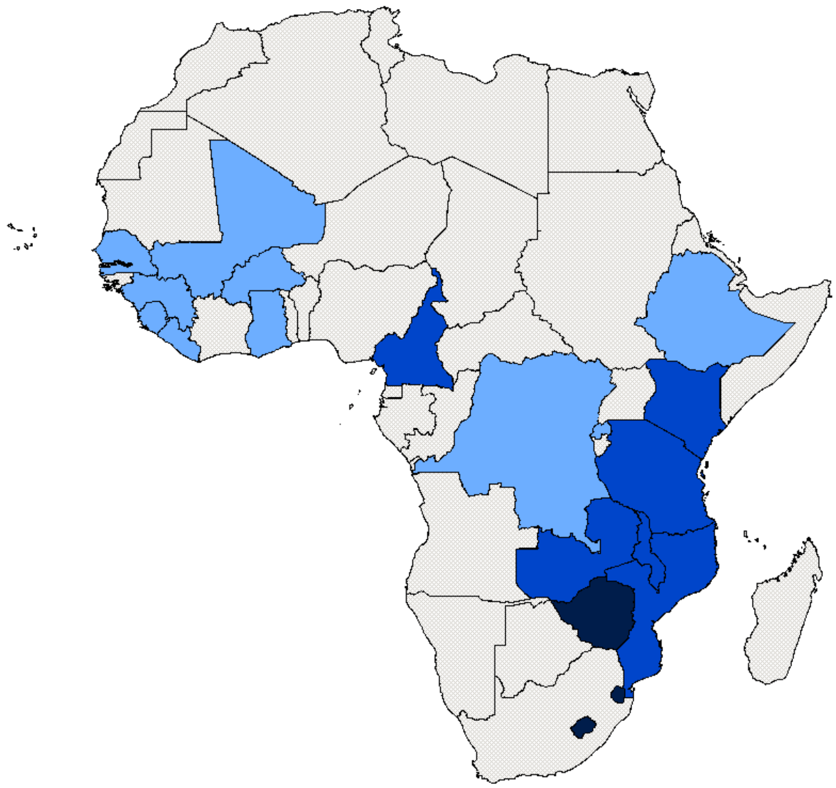
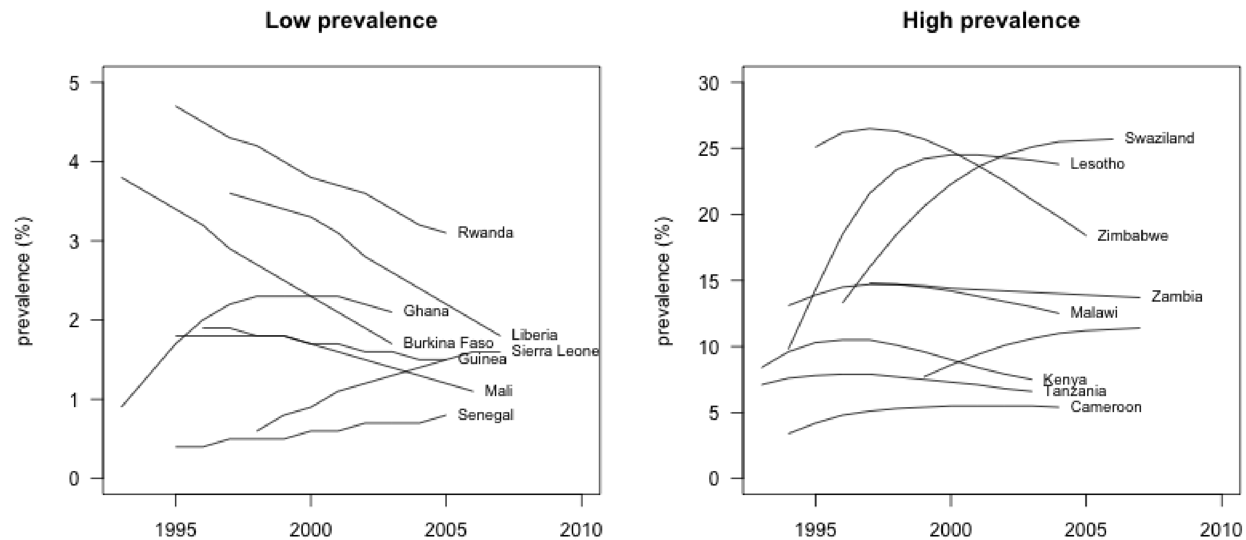


Figure B.2: Pre-survey 10-year HIV trends, Low and High Prevalence Countries.



For each country, we take the ten years preceding the survey year and plot yearly estimates of HIV prevalence from UNAIDS (2010). Ethiopia and Democratic Republic of Congo are not included in the figures as UNAIDS does not have historical estimates of HIV-prevalence for either country. We assume that both countries remained in the low prevalence category over the past ten years.

Table B.1: DHS Sampling for Serostatus Testing

Country	Year	Men Aged	Women Aged
Testing in all sampled households			
Mozambique	2009	12-64	12-64
Swaziland*	2007	15-49	15-49
Tanzania	2004, 2008	15-49	15-49
Liberia	2007	15-49	15-49
Zimbabwe	2006	15-54	15-49
Zambia	2007	15-59	15-49
Ghana	2003	15-59	15-49
Testing in random 50% of sampled households			
Sierra Leone**	2008	6-59	6-59
Kenya	2003, 2009	15-49	15-49
Lesotho	2004	15-59	15-49
Cameroon	2004	15-59	15-49
Congo DR	2007	15-59	15-49
Ethiopia	2005	15-59	15-49
Guinea	2005	15-59	15-49
Rwanda	2005	15-59	15-49
Testing in random 33% of sampled households			
Malawi	2004	15-54	15-49
Burkina Faso	2003	15-59	15-49
Mali	2006	15-59	15-49
Senegal	2005	15-59	15-49

* Swaziland: additional HIV testing for those aged 12-14 and 50+ in a random 50% of sampled households.

** Sierra Leone: Individual questionnaires were administered only to those aged 15-49 (59 for men)

Table B.2: Non-response for Serostatus Testing

		Men		Women	
Country	Year	Tested	Refused	Tested	Refused
Lesotho	2004	68%	16.6%	81%	12.0%
Swaziland	2007	78%	16.6%	87%	9.5%
Zimbabwe	2006	63%	17.4%	76%	13.2%
Malawi	2004	63%	21.9%	70%	22.5%
Mozambique	2009	92%	6.1%	92%	6.1%
Zambia	2007	72%	17.6%	77%	18.4%
Cameroon	2004	90%	5.6%	92%	5.4%
Kenya	2003	70%	13.0%	76%	14.4%
Kenya	2009	79%	7.8%	86%	8.2%
Tanzania	2008	80%	8.0%	90%	6.3%
Tanzania	2004	77%	13.9%	84%	12.3%
Burkina Faso	2003	86%	6.6%	92%	4.4%
Congo DR	2007	86%	5.7%	90%	4.4%
Ethiopia	2005	75%	12.6%	83%	11.2%
Ghana	2003	80%	10.7%	89%	5.7%
Guinea	2005	88%	8.5%	93%	5.0%
Liberia	2007	80%	11.3%	87%	7.3%
Mali	2006	84%	4.8%	92%	3.2%
Rwanda	2005	96%	1.9%	97%	1.1%
Sierra Leone	2008	85%	5.5%	88%	4.7%
Senegal	2005	76%	16.0%	85%	9.9%
Average		79%	11%	86%	9%

Note: Rates are for the full HIV testing sample, with the exception of Mozambique. Rates for MZ are for the 15-49 sample

C Impact of drought on crop yields

While we cannot directly show the importance of rainfall shocks for household income (as noted, the DHS do not include income or consumption measures), aggregate data suggest that these shocks are economically important. Table C.1 shows the impact of rainfall dropping below the 10th or 15th percentile on (log) country-level maize yields across Sub-Saharan African countries, based on panel regressions using country and year fixed effects. Maize is the most widely grown crop in Africa, and annual maize yields are strongly affected by precipitation: for instance, yields are about 12% lower in a year with rainfall at or below the 15th percentile, and 16% lower in a year with rainfall below the 10th percentile. Results are robust to including temperature shocks in the regression (available on request). With 60-80% of rural African incomes derived directly from agriculture, these productivity impacts likely represent significant shocks to household incomes [Davis et al., 2010].[36]

[36] Schlenker and Lobell [2010] demonstrate that these strong negative impacts of weather shocks generalize to other African staples, not just maize.

Table C.1: Impact of precipitation shocks on maize yields.

	(1) log maize yield	(2) log maize yield
10 PCT shock	-0.161***	
	(0.025)	
15 PCT shock		-0.122***
		(0.023)
Constant	-0.067	-0.066
	(0.052)	(0.052)
Observations	1888	1888
R squared	0.317	0.316
Pct. drought	0.083	0.146

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent variable is the log of country-level maize yield. Regressions cover years 1961-2008 and include country fixed effects, year fixed effects, and a constant, and are weighted by country average maize area. Yield data are from FAO (2010), and weather data are from UDel.

D Robustness Checks

Table D.1: Robustness to Definition of Shock

	(1)	(2)	(3)	(4)	(5)
5 PCT Shock Past 10 Years	.011*				
	(.006)				
10 PCT Shock Past 10 Years		.009*			
		(.005)			
15 PCT Shock (10 Years)			.009***		
			(.003)		
20 PCT Shock Past 10 Years				.004	
				(.003)	
1.5 SD Shocks Past 10 Years					.013*
					(.007)
Observations	77760	77760	77760	77760	77760
R^2	.031	.031	.031	.030	.031

Rural sample from high-prevalence countries. All specifications use theinclude controls for age, mean rainfall, rural/urban designation, and survey fixed effects. All specifications are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Table D.2: Robustness to Length of Shock Window & Placebo Test

	(1)	(2)	(3)	(4)	(5)
15 PCT Shock Past 5 Years	.007*				
	(.004)				
15 PCT Shock Past 7 Years		.009***			
		(.004)			
15 PCT Shock Past 13 Years			.009***		
			(.003)		
15 PCT Shock 2 Years Ahead				.003	
				(.007)	
15 PCT Shock 3 Years Ahead					.001
					(.006)
Observations	77760	77760	77760	67436	49523
R^2	.030	.031	.031	.039	.045

Rural sample from high-prevalence countries. All specifications include controls for gender, age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Table D.3: Robustness to Specification

	(1) Age15_49	(2) No Controls	(3) No Wgts	(4) CoYrFE	(5) No Hypr	(6) w/Temp
15 PCT Shock (10 Years)	.010***	.008**	.005*	.009***	.009***	.010***
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
15 PCT Temp. shock						-.008***
						(.003)
Observations	75570	77760	77760	77760	68287	77760
R^2	.034	.020	.068	.031	.026	.032
Mean Dependent Var	.070	.070	.110	.070	.068	.070

Rural sample from high-prevalence countries. All specifications include controls for gender, age, mean rainfall, and survey fixed effects, except as noted. Estimations are weighted to be representative of the 19 countries., except as noted. Robust standard errors are shown in parentheses clustered at the grid level.

E Estimating Changes in Sexual Behavior

The epidemiological model used to estimate changes in sexual behavior uses a similar technique employed by Gong [2011] which does a simple transformation of the AVERT model (Rehle et al., 1998). The model is expressed as:

$$M = \frac{log(1 - \mathbb{P}(HIV\ Infection))}{log(W[1 - R(1 - FE))^N + (1 - W))} \tag{3}$$

where $\mathbb{P}(HIV\, Infection)$ is the likelihood of HIV infection, W =HIV prevalence, R = HIV transmission per unprotected coital act, F =fraction of sexual acts where a condom is used, E = effectiveness of condoms at reducing HIV transmission, N = Number of sex acts per partner, and M= Number of sexual partners. Parameter estimates are in Table E.1.

One of the key parameters is the HIV transmission rate, or the likelihood of HIV infection per unprotected coital act. The major factor in determining this is the stage of HIV infection that the HIV infected partner is in. There are three stages of infection: 1) acute, 2) asymptomatic, and 3) late stage, with the acute stage lasting approximately six months after the initial infection occurs, and the asymptomatic phase lasting approximately 8.5 years, until the onset of AIDS which marks the late stage. The HIV transmission rate in the asymptomatic phase is estimated to be approximated .07% (Boily et al., 2009, Powers et al., 2008), while the acute phase amplifies the transmission rate 26-fold (Hollingsworth et al., 2008). We use a single HIV transmission rate, by taking a weighted average of these HIV transmission rates. We estimate that 8% of those infected with HIV are in the acute phase, while the remainder are in the asymptomatic phase. Those with late stage infections are no longer sexually active (Powers et al. 2011) .

Finally, the condom parameters (F and E) come either from the DHS data or the epidemiological literature, while the number of sexual acts per partner assumes a 6 month duration for each partnership with 5 sexual acts per month (Powers et al. 2011).

We initally assume that there is random matching of women with male partners, and thus all women face the same likelihood of matching with an HIV-positive partner. However, we can relax this assumption. For example, suppose women experincing an income shock are increasing their supply of transacitonal sex and are matching with male partners who are riskier (i.e. more likely to be HIV-positive). Following Sweat et al. [2000], we can increase the likelihood that women in the agricultural sector face a 25% higher likelihood of matching with an HIV-positive male, and thus there overall likelihood of matching is 12% ($.097 \times 1.25$). Using this higher rate of matching for women in agriculture, leads to an estimated change in partnerships of .85 (compared to our inital estimated of 1.35 in section 5).

Table E.1: Parameter Values

Parameter	Value	Source
W (Prevalence Men) in High Prevalence Countries	9.7%	DHS
W (Prevalence Women) in High Prevalence Countries	14.6%	DHS
R (HIV Transmission)	.58%	(Powers et al., 2008, Boily et al., 2009, Hollingsworth et al., 2008, Magruder, 2011)
F (Fraction of Acts Condom Used)	11%	DHS
E (Condom Effectiveness)	80%	(Weller and Davis, 2002)
N (Sex Acts per Partner)	30	(Powers et al., 2011)

F Self-reported sexual behavior

The DHS include a number of questions on self-reported sexual behavior, and we explore whether such behavioral reports respond to negative shocks in a way consistent with our reduced-form HIV results. There are several caveats to this analysis. First, there is a large body of evidence that suggests that self-reported sexual behavior suffers from social desirability bias (Cleland et al. [2004]); one study found that some women in Zimbabwe under-report sexual activity by 50% [Minnis et al., 2009]. Secondly, the data that is available for sexual behavior doesn't capture all aspects of risky behavior that could lead to HIV infection. For example, the type of sexual partner you have (commercial sex worker, individual with multiple partners, etc.) will affect the likelihood of HIV infection, but such data are not available in the DHS.[37] In addition, the questions about sexual behavior are not present in all the employed DHS surveys, and therefore the proceeding analysis is performed on a subsample of our data.[38] Finally, virtually all of the measures we have of sexual behavior are for recent behavior (12 months prior to the survey). Thus, unless sexual behavior is unchanging over time, income shocks in the past may not affect more recent sexual behavior.[39] To deal with this, we differentiate between the number of recent shocks (i.e. within the past 5 years) and earlier shocks (6 to 10 years ago). Given these caveats, we interpret results on self-reported sexual behavior with caution.

The outcome variables of interest are: (i) the lifetime number of sexual partners and (ii) whether the respondent has been sexually active in the past 12 months. For those who are sexually active, we further know whether they had (iii) multiple partners, (iv) nonspouse partners[40], and (v) unprotected sex

[37] See Dupas [2011] for a discussion of the importance of partner selection in HIV risk.

[38] Lifetime partners is available in 7 surveys for women (KE09, MZ09, SZ07, TZ04, TZ08, ZM07, ZW06). The other four indicators for women are available in the same seven surveys, plus an additional four (CM04, KE03, LS04, MW04). For men, the sets are the same, except that CM04 also provides lifetime partners.

[39] An additional concern is that coefficient estimates will be biased if exposure to shocks changes how an individual *reports* her sexual behavior without actually changing her sexual behavior.

[40] In this data, a monogamous cohabiting union is considered a spousal partner, irrespective

with a nonspouse partner.[41]

Table F.1 shows results of estimations of equation 2, separately by gender, with these self-reported sexual behaviors as the dependent variables. Rural women report low numbers of lifetime partners, 2.2 on average, and neither recent nor earlier shocks have an effect (column 1). The remaining behaviors are relevant only for the past 12 months, and as expected, shocks occurring more than 5 years ago have no significant effect. Recent shocks do significantly increase the likelihood that a woman is sexually active, and also significantly increases the likelihood of having a nonspouse partner (columns 2 & 4). Interestingly, while only 16% of women report nonspouse partnering, of these, more than 70% report that the intercourse was unprotected. Rural men report six lifetime partners on average, which is much higher than what women report. Both recent and earlier shocks significantly increase the number of lifetime partners for men (column 6). While recent shocks have no effect on whether a man is sexually active (past 12 months), recent shocks do increase the likelihood of multiple partners, nonspouse partners, and unprotected sex for those with nonspouse partners.

These self-reports of sexual behavior indicate that sexual activity is more common in areas that have experienced recent shocks. Keeping the caveats discussed earlier in mind, these findings are consistent with our interpretation of the main findings: drought-induced income shocks lead to increases in the equilibrium level of sexual activity which is generating higher rates of HIV prevalence.

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of formal marital status. Those having nonspouse partners would include single, sexually active individuals.

[41] While some DHS surveys also query condom use within marriage/cohabiting union, many of the surveys included here only inquire regarding nonspouse partners.

Table F.1: Effect of Shocks on Self-Reported Behavior

WOMEN	(1) Lifetime Partners	(2) Sexually Active	(3) Multiple Partners	(4) Nonspouse Partner	(5) Unprotected Nonspouse
Num. Shocks in last 5 years	-.076	.021***	.003	.013**	-.011
	(.052)	(.007)	(.003)	(.006)	(.014)
Num. Shocks 6 - 10 yrs ago	.035	.003	.001	-.002	-.016
	(.059)	(.007)	(.003)	(.007)	(.019)
Observations	28295	43143	31882	31910	5358
R^2	.032	.060	.014	.044	.041
Surveys Reporting	7	11	11	11	11
Mean Dependent Var	2.163	.759	.032	.158	.702

MEN	(6) Lifetime Partners	(7) Sexually Active	(8) Multiple Partners	9) Nonspouse Partner	(10) Unprotected Nonspouse
Num. Shocks in last 5 years	.542***	.009	.012*	.023***	.030**
	(.188)	(.008)	(.007)	(.008)	(.014)
Num. Shocks 6 - 10 years ago	.393*	.010	.028***	.007	.007
	(.222)	(.008)	(.008)	(.010)	(.015)
Observations	22003	34610	24470	24527	9195
R^2	.068	.226	.036	.256	.072
Surveys Reporting	8	11	11	11	11
Mean Dependent Var	6.004	.738	.208	.365	.571

Rural sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

G Migration

We vary the rate that shocks affect migration rates. The column headers in Table G.1 show several possible assumptions ranging from 1% to 10% *per shock*. A bit of algebra reveals that if, for example, 5% of the population leaves during each shock, a village with three shocks over the past ten years has lost 14.3% of its population in that time. The calculation of lost population by number of shocks and assumption maintained are shown in the body of table G.1. By applying these calculations to the rural clusters in our data according to each cluster's number of shocks, we calculate the total population lost in our rural sample over the ten years before the applicable survey. The bottom row of table G.1 shows these estimates.

For each country in our sample, we calculate the reduction in rural population (as a share of total population) over a recent 10-year period, based on data from the World Bank.[42] On average, the rural share of the populations of these countries is reduced by 5.8% over ten years. Based on the assumption that 3% of a village leaves during each shock, we estimate that our rural sample has lost 4.26% of its population in the past ten years; an assumption of 5% leaving yields a total decline of 7.01%. This suggests that an assumption of 3% population loss per shock approaches reality, with 5% as an extreme upper bound.

[42]Figures from World Bank Development Indicators, 1990-2000.

Table G.1: Potential Reductions in Rural Populations due to Shock-induced Migration

	Share of Population Emmigrating Per Shock				
Shocks / 10 yrs	1%	3%	5%	7%	10%
0	0.00%	0.00%	0.00%	0.00%	0.00%
1	1.00%	3.00%	5.00%	7.00%	10.00%
2	1.99%	5.91%	9.75%	13.51%	19.00%
3	2.97%	8.73%	14.26%	19.56%	27.10%
4	3.94%	11.47%	18.55%	25.19%	34.39%
5	4.90%	14.13%	22.62%	30.43%	40.95%
6	5.85%	16.70%	26.49%	35.30%	46.86%
7	6.79%	19.20%	30.17%	39.83%	52.17%
Estimate of total population reduction based on number of shocks observed in our data	1.44%	4.26%	7.01%	9.69%	13.59%

Each cell represents the ten-year population loss in a cluster that has occurrences of shocks as given by the row, and population loss-per-shock as given by the column. The last row represents the assumed total loss from the rural sample based on the shocks observed in the data, under the various assumptions of population loss-per-shock as given by the column headers.

H Results at the country level

To explore the relevance of shocks for the broader patterns of HIV prevalence across Sub-Saharan Africa, we run simple cross-sectional regressions relating prevalence at the end of each decade to accumulated shocks over the previous decade. Country-level HIV prevalence is estimated as a functions of number of shocks over previous 10 years for the 38 countries in Sub-Saharan Africa with HIV data. Shocks are the sum of rainfall realizations below the 15th percentile, based on annual country-average rainfall (weighted by crop area). Country HIV prevalence data are from UNAIDS.

Table H.1: Shocks predict country-level HIV prevalence

	(1)	(2)	(3)	(4)
	levels 1990s	levels 2000s	change 1990s	change 2000s
15% shocks (10 yrs)	2.089**	2.450***	1.250	0.408*
	(0.887)	(0.788)	(0.798)	(0.234)
Observations	37	37	36	37
R^2	0.140	0.216	0.064	0.068
Mean dep. var.	7.0	6.3	4.6	-0.7

Regressions marked “levels” have HIV prevalence in either 1999 (model 1) or 2008 (model 2) as the dependent variable; Regressions marked “changes” have as the dependent variable the change in HIV prevalence over the previous decade, with the end year either 1999 (model 3) or 2008 (model 4).