Income Shocks and HIV in Africa*

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Abstract

We examine how variation in local economic conditions has shaped the AIDS epidemic in Africa. Using data from over 200,000 individuals across 19 countries, we match biomarker data on individuals' HIV status to information on local rainfall shocks, a large source of variation in income for rural households. We estimate that infection rates in HIV-endemic rural areas increase by 11% for every recent drought, an effect that is statistically and economically significant. Income shocks explain up to 20% of the variation in HIV prevalence across African countries, suggesting that existing approaches to HIV prevention could be bolstered by efforts to help poor households better manage income risk.

JEL Codes: I15, O12, O55

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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, one of the most important global health challenges. Such an understanding might yield particular dividends in sub-Saharan Africa (SSA), where over a million people continue to become newly infected with the disease each year (UNAIDS, 2010).

In this paper we explore the role of negative income shocks in shaping the evolution of the HIV/AIDS epidemic in Africa. Such shocks represent a well-documented challenge to poor households around the world. Lacking access to formal savings and insurance, income shortfalls often force poor households to make difficult tradeoffs between short-run consumption and longer-run earnings and human capital accumulation (Rosenzweig and Wolpin, 1993; Ferreira and Schady, 2009; Maccini and Yang, 2009). Recent indirect evidence suggests that variation in income could also affect important disease outcomes, either by altering individual sexual behavior (Baird et al., 2012; Kohler and Thornton, 2011; Robinson and Yeh, 2011b), or by affecting other phenomena such as migration or marriage timing that play a documented role in disease transmission (Lurie et al., 2003; Clark, 2004; Oster, 2012). Were income variation to play a role in HIV outcomes through any of these mechanisms, it would suggest that addressing income risk could play an important role in comprehensive HIV prevention strategies.[1]

Using one of the most widespread sources of income variation in the developing world – rainfall-related shocks to agriculture – we directly assess the effect of negative income shocks on HIV outcomes across the African continent. We use the exogenous timing of rainfall events to develop an annual measure of shocks that is orthogonal to time-invariant determinants of disease outcomes. Our definition of a shock is annual rainfall below the 15th percentile of the historical distribution of rainfall for a local area. Using data on roughly two-hundred thousand individuals across nineteen African countries, we compare the HIV status of individuals randomly exposed to a higher number of recent shocks (past 10 years) to the status of nearby individuals exposed to fewer recent shocks.

We find that exposure to recent negative rainfall shocks substantially increases HIV infection rates in rural areas with high baseline HIV prevalence. Exposure to a single additional shock leads to a significant 11% increase in overall HIV infection. These results are robust

[1] Economic interventions such as formal insurance could compliment existing biomedical interventions such as male circumcision and ARV treatment as prevention.

to a variety of ways of constructing the shock measure, to a variety of controls, and to a set of placebo tests. Consistent with expectations, we find little effect of shocks in urban areas (where incomes should be less sensitive to rainfall) and in low-prevalence regions (where there exists less HIV to be transmitted).

We show that these individual-level results are mirrored in the broader cross-country patterns of HIV prevalence observed in SSA. Using country-level data from UNAIDS, we show that exposure to shocks at the country level is also associated with significantly higher levels of HIV infection, and that our shock measure explains 14-21% of the cross-country variation in HIV prevalence across SSA. This provides somewhat independent evidence on the role of shocks in shaping HIV outcomes, and implies that meteorological bad luck earlier on in the AIDS epidemic could have played a substantial role in shaping how the epidemic progressed over the following decades.

While these reduced form results provide direct causal evidence that negative shocks substantially increase equilibrium HIV infection rates, they provide limited insight into the many channels through which shocks might shape HIV risk. For instance, adults may respond to shocks by temporarily migrating in search for work (Skoufias, 2003), or school-aged girls may respond by marrying at an earlier age to increase economic security (Jensen and Thornton, 2003), behaviors that are both associated with an increased risk for HIV (Lurie et al., 2003; Clark, 2004). Alternatively, 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 (LoPiccalo et al., 2012; Swidler and Watkins, 2007; Robinson and Yeh, 2011b; Dinkelman et al., 2008). This "transactional sex" has been documented among women who are not commercial sex workers in numerous African countries and is believed to be a key driver in the AIDS epidemic (UNAIDS, 2010), a fact that has motivated numerous recent attempts to address the link between income and sexual behavior through cash transfers (Baird et al., 2011; Handa et al., 2012; Kohler and Thornton, 2011; de Walque et al., 2012).

While we are unable to definitively isolate the mechanism by which shocks increase HIV, we show that our data are largely inconsistent with either a migration or an early-sexual-debut explanation. In particular, we show that shocks do not induce earlier marriage or increased time away from one's village. Furthermore, we show that the effects of shocks on HIV are larger for men working outside of agriculture (whose purchasing power would have declined the least), evidence that is broadly consistent with an outward shift in the supply of transactional sex.

This work contributes to the literature within and outside of economics that seeks to understand why 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 using macro-level data to explore the effects of economic growth on the AIDS epidemic (Oster, 2012).

We also 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 contrast to existing work, we identify behavioral responses that are not only detrimental to an individual's or household's wellbeing but that also generate large negative health externalities for the community. As such, our results add 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. Section 4 discusses our main results and robustness checks, and section 5 seeks evidence of behavioral pathways. Section 6 explores how these effects scale up to the country level. Finally, section 7 discusses policy implications and concludes.

2 Conceptual Framework

The goal of this paper is to understand how economic conditions shape HIV risk. Our empirical approach examines how a plausibly exogenous source of income variation – exceptionally low rainfall realizations at a given location relative to long-term averages ("shocks") – affects local HIV outcomes. Our primary result establishes a strong positive relationship between these shocks and local HIV prevalence. We argue that this is a causal relationship because our shock measure is, by construction, uncorrelated with other time-invariant factors that might also affect disease outcomes (see further discussion in section 3.2). Here we discuss why rainfall-related shocks might matter for HIV, and use this discussion to generate predictions of where and for whom the reduced form relationship between drought and HIV should be largest.

Our empirical analysis begins by examining the reduced-form relationship between drought-related shocks (S) and HIV infection, or $\frac{\partial HIV}{\partial S}$. Define p as a measure of sexual risk, and z as income. The reduced form relationship between drought shocks and HIV can then be written as:

$$\frac{\partial HIV}{\partial S} = \frac{\partial HIV}{\partial p}\frac{\partial p}{\partial z}\frac{\partial z}{\partial S} \quad (1)$$

The three terms on the right hand side are the following:

- $\frac{\partial HIV}{\partial p}$ represents the relationship between HIV infection and sexual risk. In the sub-Saharan African setting, heterosexual sex is the primary driver of the epidemic (UNAIDS, 2010), and so deviations in the path of the epidemic are driven largely by changes in sexual behavior. The risk of HIV infection is increasing in risky sexual behavior such as having multiple concurrent partners or unprotected sex $\left(\frac{\partial HIV}{\partial p} > 0\right)$ (Halperin and Epstein, 2008; Potts et al., 2008; Stoneburner and Low-Beer, 2004; Epstein, 2007). Importantly, this relationship also depends on the prevalence of HIV in an area (λ). Regions with higher HIV prevalence will have a stronger relationship between sexual behavior and new infections than regions with low prevalence $\frac{\partial HIV}{\partial p \partial \lambda} > 0$.

- $\frac{\partial p}{\partial z}$ represents the impact of a deviation in income on sexual risk (p). A growing literature documents the importance of economic factors in shaping sexual risk in Africa (Baird et al., 2012; Kohler and Thornton, 2011; Robinson and Yeh, 2011b). Sexual risk can be measured as the number of partners and/or number of unprotected sexual acts, but can also be measured by how likely a partner is infected with HIV. In section 5 we discuss several ways identified by the literature by which shortfalls in income might alter sexual behavior, all of which suggest a negative relationship between an income deviation and sexual risk for at least some subset of the population (i.e. $\frac{\partial p}{\partial z} < 0$). Such mechanisms can broadly be considered coping behaviors in response to income shocks, and will be operative for different subsets of the population depending on the coping mechanism in question.

- Finally, $\frac{\partial z}{\partial S}$ is the relationship between negative rainfall shocks and income shocks. As is frequently recognized in the literature, and as we demonstrate in appendix B, variation in rainfall generates substantial variation in both agricultural productivity and broader income measures in Africa. We expect that in rural areas (r), where most income is generated from rain-fed agriculture, rainfall shocks will have a larger (negative) effect on income than in urban areas where agriculture is less important for the local economy $\left(\frac{\partial z_r}{\partial S_r} < \frac{\partial z_u}{\partial S_u} \leq 0\right)$.

Because there is little disagreement in the literature on the signs of the first and third terms in Equation 1, the overall sign of $\frac{\partial HIV}{\partial S}$ will depend on how sexual risk responds to variation in income. If we assume that this term is non-zero, then two immediate predictions are generated from Equation 1.

- The effect of shocks on HIV will be larger (in absolute value) where baseline prevalence λ is higher. Intuitively, if shocks increase HIV through changes in sexual behavior, the effect of shocks will be amplified in places where there is more HIV to transmit.

- The effect of shocks on HIV will be larger (in absolute value) in rural areas where income is more dependent on agriculture (and therefore on rainfall).

The sign of $\frac{\partial p}{\partial z}$ will determine the overall sign of $\frac{\partial HIV}{\partial S}$. If some segment of the population copes with negative income shocks in a way that increases sexual risk, as is suggested by the literature, then Equation 1 indicates that the overall relationship between HIV and shocks for these populations would be positive, $\frac{\partial HIV}{\partial S} > 0$.

3 Empirical Methods

3.1 Individual HIV-status data

Our individual-level data are taken from 21 Demographic and Health Surveys (DHS) conducted in 19 different Sub-Saharan countries.[2] Of the existing DHS surveys available in early 2011, we employ all those that include results from individual-level HIV-tests as well as longitude and latitude information on the individual's location, allowing us to map households to data on shocks.[3] 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.[4]

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.[5] A men's sample is composed of all men within a specified age range within households selected for the men's sample.[6] 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 A.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

[2] A map of these countries can be found in Appendix A.

[3] 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.

[4] As a robustness check, we also estimate using only the most recent survey from each country and the results are unaffected.

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

[6] The age range for men is 15 to either 49, 54, 59 or 64, depending on the survey. See appendix A for details.

HIV-testing.[7] By employing cluster-specific inverse-probability sampling weights, the HIV prevalence rates estimated with this data are representative at the national level.[8]

Table 1 gives the list of included surveys along with basic survey information. The compiled data contain 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.[9]

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.

3.2 Weather data and construction of shocks

To understand how economic shocks shape HIV outcomes, we seek a shock measure that satisfies three criteria: derived shocks are economically meaningful, they are orthogonal to other factors that might also shape disease outcomes, and they capture the potential disjoint between when HIV is acquired and when the individual is observed in the DHS. Because we do not directly observe variation in economic performance at a disaggregated level, and because such variation is likely endogenous to disease outcomes, we adopt an approach that is common in the literature and use variation in weather as a proxy for variation in economic productivity. For the largely agrarian societies of Africa, variation in weather directly shapes the economic productivity of the majority of the population that continues to depend on agriculture for their livelihoods (Davis et al., 2010). As we show below, particularly negative rainfall realizations substantially depress agricultural productivity across the region.

[7] Testing success rates for each survey are shown by sex in Appendix table A.2. Refusal rates are 10%, on average. Mishra et al. (2006) examine test refusal rates in DHS testing, which are between 1% to 22%, depending on the country. They conclude that although those refusing are more likely to be positive, the DHS testing accurately represents national prevalence. In this study, individuals exposed to shocks do not differentially refuse a test (see Appendix Table A.3) so non-response does not induce bias in our results.

[8] For details regarding construction of the weights, see Appendix A.

[9] This categorization follows UNAIDS (2010). Appendix Figure A.2 shows that with the exception of Cameroon, the prevalence classifications for each country remains stable for the ten years preceding the survey year. Our main results are unchanged when Cameroon is removed from our analysis.

Our weather data are derived from the "UDel" (University of Delaware) data set, a 0.5 x 0.5 degree gridded monthly temperature and precipitation data set (Matsuura and Willmott, 2009). These gridded data are based on interpolated weather station data and have global coverage over land areas from 1900-2008.[10] Using the latitude/longitude data in the DHS, we match each DHS cluster to the weather grid cell in which it falls. Because lat/lon 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 grid-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.[11] Annual crop year rainfall estimates are generated by summing monthly rainfall across these twelve "crop year" months at a given location.

To capture shocks to economic productivity that are both meaningful and orthogonal to potential confounders, one must identify years in which accumulated rainfall was unusually low *relative to what is normally experienced in a particular location.* The most common way this has been done is by using the deviation from the local mean in a year or season, either in levels (as in Paxson, 1992; Fafchamps et al., 1998; Rose, 1999; Jayachandran, 2006; Tiwari et al., 2013), in percentage (as in Dercon, 2004), or in standard deviation units (as in Hidalgo et al., 2010). Unfortunately none of these methods is useful for summing shocks over a number of years, as the high years would offset the low years.[12] To avoid this offsetting, we require a binary rather than continuous indicator for whether a year constitutes a shock or not. We define shocks as rainfall below a threshold that is determined by the local rainfall distribution. In particular, for each of our 1701 grid cells, we fit the history of crop-year rainfall realizations to a grid-specific gamma distribution and assign each grid-year to its corresponding percentile in that distribution.[13] A "shock" is then defined as a realization

[10] 0.5 degrees is roughly 50 kilometers at the equator. 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 data sets are available, but none were sufficient for our needs, lacking either sufficient temporal coverage or spatial resolution.

[11] Estimates of planting dates are derived from gridded maps in 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.

[12] Other previously used continuous methods, which are also not useful for us, include the total level (or log of the level) in a season or year (as in Bruckner and Ciccone, 2011; Bruckner, 2012; Cole et al., 2012), the timing of the onset of Monsoon or rainy season, days of rain in rainy season, and length of longest dry spell in rainy season (as in Jacoby and Skoufias, 1998; Macours et al., 2012). Also, Miguel et al. (2004) employ year-over-year rainfall growth, which, as pointed out by Ciccone (2011), is potentially a poor measure of shocks due to mean reversion.

[13] 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 1.5 standard deviations below the grid mean. We use the history of rainfall over the period 1970-2008, which was chosen to be a long enough period to be relatively insensitive to the recent

below a pre-determined percentile in the location-specific distribution. The literature does not provide definitive estimates of the percentile below which a shock becomes meaningful, and unfortunately disaggregated (e.g. grid) measures of economic productivity over time are unavailable.[14] To make progress, we construct an analogous measure of rainfall shocks at the country level and assess how country-level agricultural productivity and GDP growth respond to these shocks.[15] Resulting estimates from panel regressions of country level maize yields or GDP growth on percentile rainfall realizations (purged of country- and time-fixed effects) are shown in Figure 1. Maize is the continent's primary staple crop, the crop grown by the majority of smallholder farmers, and thus perhaps the best direct measure of rural incomes. Point estimates from these panel regressions suggest that realizations below about the 15th percentile are the most harmful to maize yields (Figure 1, left panel). A similar pattern is found in GDP growth (right panel). We thus adopt this 15% threshold as our initial measure of a "shock" - i.e. we define a shock as a crop-year rainfall realization below the 15% quantile of the local rainfall distribution - and show that our results are robust to other threshold choices in the neighborhood of 15%, as well as to other plausible methods of constructing binary shocks.

Finally, because the DHS only observes the disease status of a particular individual at one point in time, and an HIV+ individual could have become infected at any time over the previous decade or longer (median survival time at infection with HIV in sub-Saharan Africa, if untreated, is 9.8 years (Morgan et al., 2002)), 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. We again 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

shocks of interest, but short enough to capture relatively recent averages if long run means are changing (e.g. with climate change).

[14]Others in the literature have constructed binary shocks using thresholds such as 75% or less of the local mean (as in Shah and Steinberg, 2013) or 1 to 2 standard deviations below the local mean (as in Bobonis, 2009; Skoufias et al., 2012).

[15]That is, we aggregate crop year rainfall over all cells in a given country (weighting by crop area) to get a time-series of rainfall realizations for each country; we fit a separate gamma distribution to each country's time series; and within each country each year is assigned it's corresponding percentile in its gamma distribution. Crop yield data are from FAO (2011), and data on real per capita economic growth is from the Penn World Tables 7.0 (Heston et al., 2011).

of a very bad year and a very good year would be similar to the sum of two normal years. The mean and standard deviation of shocks by cluster are shown in Table 2.

By construction, this shock measure should be orthogonal to other confounding variables. Because shocks at a given location are defined relative to that location's historical rainfall distribution, and the same *percentile* cutoff is used in each location to define a shock (instead of the same absolute cutoff), all locations have the same expected number of shocks over any given 10 year period: each year any location has a 15% chance of experiencing a shock. But because rainfall in a given location varies over time, some 10-year time windows will accumulate more shocks than other windows, and it is this plausibly random variation that we exploit.[16] We confirm in Appendix B that accumulated rainfall shocks are orthogonal to the first three moments of the rainfall distribution, providing additional confidence that our shock measure is uncorrelated with other time-invariant unobservables that might also affect HIV outcomes.

This definition of shocks assumes that relative (rather than absolute) deviations in rainfall are what matter for income and HIV outcomes. This construction is necessary for identification – using an absolute threshold for a shock would mean that areas with lower or more variable rainfall would expect more shocks, and these areas could differ in other unobserved ways that matter for HIV – but it is also plausibly captures what is important in our setting. Farmers choose crops that are adapted to the conditions under which they are grown, with farmers in drought-prone regions in Africa sowing crops (such as millet and sorghum) that can withstand low rainfall realizations, and farmers in areas with higher average rainfall sowing crops that are generally higher yielding but less tolerate of drought (e.g. maize). The results in Figure 1, which are constructed using this relative shock measure, confirm that relative deviations matter for both agricultural outcomes and broader economic performance.

3.3 Estimation

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

$$HIV_{ijk} = \alpha + \beta_1 S_j^t + X_i'\delta + \gamma r_j + \omega_k + \varepsilon_{ijk} \qquad (2)$$

where HIV_{ijk} is an indicator for whether 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 in the last 10 years for a given cluster. Note again that by construction,

[16]In Appendix C we discuss why mean reversion is not a concern for our shock measure.

no one cluster is any more shock prone than another, i.e. $E(S^t_m) = E(S^t_n) \quad \forall j = m, n$. All clusters expect the same total number of shocks over the 38 years in our rainfall data, and our identifying variation comes from the random timing of these shocks: some clusters happen to receive more of their shocks in the decade immediately before we observe them, and others receive fewer. Both t and the definition of S are varied over a range to test the robustness of results.

The vector X_i contains characteristics of individual i that are not affected by shocks, specifically, gender and age. r_j indicates that cluster j is rural. The survey fixed effect is ω_k and ε_{ijk} is a mean-zero error term.[17] We estimate linear probability models, allowing for correlation of error terms across individuals in the same weather grid. Survey specific sampling weights are used to make the results representative of individuals living in these 19 countries in Sub-Saharan Africa (see Appendix A).

4 Results

4.1 Main results

Table 3 shows estimations of Equation 2, employing various samples and interaction terms. The overall effect of shocks on HIV rates using the full sample is 0.3 percentage points (ppt) and is statistically significant at the 10% level (Column 1). Our simple conceptual framework predicts differential effects depending on whether an individual lives in an urban or rural area, and in line with this prediction we find that the effects are concentrated in rural areas. We cannot reject that urban effects are zero (Column 2; Linear combination), and the difference between estimates for rural and urban areas is borderline significant at conventional levels ($p - value = 0.104$). Focusing our analysis on rural areas (Column 3), we find that shocks have a meaningful effect: we estimate that each shock leads to a 0.3 ppt increase in HIV prevalence, an effect that is significant at the 5% level and that corresponds to a 7.3% increase in HIV rates given a mean of 4.1%.

The second prediction from our framework is that increases in risky behavior as a result of an income shock would result in little change in HIV infection rates if existing HIV prevalence is very low. To capture differential effects by baseline prevalence, we focus on the rural sample and include an interaction between shocks and an indicator for low-prevalence countries. In countries with low prevalence (less than 5%), shocks have an approximately zero effect on

[17] There are a host of reasons for including survey fixed-effects. Innumerable differences across countries exist that we cannot observe, including social norms of sexual behavior, male circumcision rates, access to health services, and the national response to the AIDS epidemic. Such unobservable differences may also apply to different time periods within the same country, thus motivating a within-survey estimation.

HIV (Column 4; Linear combination), and we reject equality across low and high prevalence countries with 95% confidence (Column 4; Shocks x Low Prevalence). Column 5 presents the estimation for the rural sample in high prevalence countries only. In these areas, each shock increases HIV by 0.8 ppt, an 11% increase based on overall prevalence of 7%.

Finally, column 6 disaggregates the impact by gender. We find that shocks increase the probability of infection by 0.9 ppt for women and 0.6 ppt for men, both of which are statistically significant at the 5% level. Given that HIV prevalence is 8.3% for women and 5.6% for men in high prevalence rural areas, these estimates represent large effect sizes of 11% increases in HIV per shock for both women and men. We cannot reject that the effect size is the same across genders (Column 6; Shock x Male).

The magnitude of these effects are meaningful. In our entire sample, the mean number of shocks is 1.5, which, combined with our primary results, suggests that drought-induced income shocks lead to a 17% increase in HIV risk over a ten-year period. We also can attempt to roughly estimate an income elasticity with respect to HIV risk.[18] We estimate that each drought shock results in a 7% to 10% loss in annual income (see Appendix B), which leads to an 11% increase in HIV infection risk. This result is similar to results from Robinson & Yeh (2011b) which show that a 3% loss in income leads to about an 8% increase in HIV risk.[19] Both results suggest that better means of consumption smoothing can have implications for the HIV/AIDS epidemic.

4.2 Robustness of results

In this section, we examine whether our primary result – the large response of HIV to shocks in rural, high prevalence areas shown in Table 3, column 5 – is robust to various issues of specification, variable definition, sample selection, or omitted variables.

Specification

We first examine whether our results are sensitive to the specification or sample used. We sequentially remove individual level controls, remove population weights, and replace survey-year-fixed effects with country- and year-fixed effects and our results remain stable (Table 4; Columns 1-3). We also vary the sample used, removing hyper-endemic countries such as Swaziland and Lesotho where HIV-prevalence exceeds 20%, and our results remain stable (Column 4). Finally, within each DHS cluster (i.e. village), we remove all visitors

[18]In order to generate an actual income elasticity with respect to HIV infection risk, we would need: 1) percent of income derived from agriculture for all individuals in our sample, 2) individual level crop yields, and 3) crop prices by DHS cluster. This data is required for each year of the past ten years for everyone in our sample. Unfortunately this data is not available.

[19]It is important to note that the sample used by Robinson & Yeh (2011b) consists of female sex workers in Western Kenya, while the sample in this paper is representative of the rural population in 19 countries.

from the sample, defined as those who have lived in the area for less than a year at the time of the survey. We do this for two reasons. First, we want to identify the effect of shocks on HIV for those who were actually living in the area at the time of the shock and removing visitors helps us establish this. Second, it may be that rainfall shocks are inducing NGO and government workers to migrate in drought afflicted areas, and if these types are more likely to be HIV+, than this could potentially explain our results. Removal of these visitors from the sample does not change our results (Column 5). We also present an estimate that employs only the most recent survey from each country, excluding the KE 2003 and TZ 2004 surveys, which produces similar results (Column 6). Finally, we provide results only for individuals who were between the ages of 15 and 50 when the shocks occurred (Column 7). These individuals would likely have the greatest response in terms of sexual behavior, and we do find a result that is slightly increased over our main specification.

Shock definition

We also examine the sensitivity of our results to the definition of a shock. While our primary specification defines a shock as a crop-year rainfall realization below the 15th percentile of local realizations, the choice of the 15th percentile is somewhat arbitrary. We vary the cut-off for shock definition in increments of 1 percent between the 5th and 40th percentile. The estimated coefficients for each percentile are presented in Figure 2. Overall, the point estimate is relatively stable around our default 15th percentile shock measure, and as the definition of a shock becomes less (more) severe the point estimates generally decrease (increase). Shocks in the neighborhood between between the 10th percentile to 20th percentile generate similar results, although they become less precisely estimated the further they are from the 15th percentile (see appendix Table C.1).[20]

For rainfall at or above the 40th percentile, point estimates suggest that there is no effect on HIV. This corresponds to the estimated relationships between rainfall and maize yields, and rainfall and GDP growth, shown in Figure 1. Both maize yields and GDP growth are unaffected by rainfall realizations above the 40th percentile, and consistent with this we find that HIV becomes similarly unaffected by rainfall around this threshold.

We also vary the period of time over which shocks are summed, for comparison with our default definition of shocks summed over the past ten years. We sum shocks in 5-year bins (e.g. number of shocks 1-5 years before the survey, number of shocks 6-10 years before, etc.) and employ each of these binned variables as the regressor in our main specification. Figure 2 plots the point estimates of these regressors. As we show in Appendix E, this

[20]Shocks that approach the 20th percentile may not be severe enough to effect behavior, while shocks that approach the 10th percentile may have stronger effects on behavior, but their relative rarity reduces the statistical power of hypothesis tests.

time profile of the effect of shocks on HIV is very much as we would expect, with point estimates for the effect of shocks peaking early within the 10-year window. Intuitively, an earlier shock has more time to reverberate through the population and generate additional infections compared to a more recent shock, but effects are attenuated over time as the earliest infected die. Given the observed infection rate and the observed timing of mortality following infection, we show via simulation in Appendix E that the effect of a shock will peak 6-10 years later.

To address concerns that shocks from the mid-1990's onward (our main shocks of interest, given our HIV data are from 2003 to 2009) may be endogenous to how shocks are defined, we also employ a shock definition that is based on the 15th quantile of the historical distribution derived from rainfall data only up through 1995. The cluster-specific definition of shocks then does not depend on anything that happened after 1995. We find that the results do not differ significantly using this alternate measure (Table 4; Column 8). Finally, as an alternative to the quantile-based definition, we also define shocks as rainfall that is 1.5 standard deviations or more below the historical mean for the area. The primary estimation employing this definition of shock is shown in column 9 of Table 4, where the estimated coefficient is similar, though slightly larger, and remains statistically significant.

Sample selection

Droughts can also effect other types of behaviors that might explain our results. If shocks induce permanent out-migration and the migrants are disproportionately HIV negative, this could yield a spurious correlation between observed shocks and higher HIV prevalence among the remaining population. In order to test whether selective migration can account for our results we conduct a bounding exercise suggested by Lee (2009). Using national rural and total population figures by country, we estimate that rural areas lose approximately 2% of population per shock (see Appendix D for more details) and conservatively assume that *each one* of these individuals is HIV-negative.[21] We replace these individuals in our sample and re-estimate our main results. This in effect stacks the deck against finding a result: communities that experience shocks now have more HIV-negative individuals. We note however that the assumptions we make about migration rates are strong, and therefore some caution is warranted when interpreting the results.

Table 5 first reproduces our primary result based on the rural sample of high-prevalence countries: the probability of infection increases by 0.8 percentage points per shock. We then vary the assumed percentage who migrate when a shock occurs, starting with our estimate of 2% and increasing in increments of 1%. We find that when accounting for estimated

[21] In Section 5 we find no evidence of differential migration rates (due to shocks) between clusters close and far from urban centers.

out-migration of 2% per shock, the estimated coefficient (0.7 ppt) is nearly identical to our original estimate, and still significant.

Note that if *all* of rural to urban migration were caused *only* by shocks, then a more accurate estimate would be that 4% of the population migrates when a shock occurs (again, see Appendix D for details). Thus, the assumption of 4% loss per shock is an extreme upper bound. When we replace a 4% population loss per shock, our effect remains positive (0.4 ppt) and significant at the 10% level. Though 4% is the upper bound, we nonetheless report estimations under the assumptions of 5% and 6% loss per shock to show that the estimate does not lose significance until we assume 6% loss per shock – three times our best estimation of 2% loss per shock. This suggests that sample selection due to permanent migration is unlikely to explain our results.

Omitted variables

A final concern is that our results might be driven by omitted variables. For example, some aspects of local weather might be correlated with other unobservables (wealth, education, etc) that also affect HIV rates. While this is unlikely to be true for our measure of rainfall shocks – by construction all areas expect the same total number of shocks over time – we confirm that our estimates are robust to controlling for characteristics of the underlying distribution. In Table 4, Panel B, we sequentially control for the first three moments of the rainfall distribution (mean, variance, skew) in our main specification (Columns 10-12), and also include all three moments (Column 13). Our estimate remains stable throughout these various specifications.

We can further test for these potential confounders with a "placebo" test - we check whether shocks in the future can predict present HIV rates or other observable present characteristics. Given that the DHS surveys were conducted between 2003 and 2009, and our weather data ends in crop-years 2007-2008, we are only able to examine shocks up to four years in the future.[22] We find no relationship between HIV rates and shocks 1 to 4 years in the future (Table 6; Columns 1-4).[23] We also find no relationship between current wealth quintile and future shocks (Columns 5-7), nor any relationship between an individual's years of education and future shocks (Columns 8-10).

[22] The only 2003 survey which has individual HIV infections (Kenya), does not have data on wealth and education. Therefore correlations with these characteristics can only be estimated using data in years 2004+, so these can only be observed up to three years in the future.

[23] We note that the estimates for shocks one year into the future may have measurement error. Because each DHS survey takes many months to complete, and because our data on which months are in the "crop-year" typically do not vary sub-nationally, the timing of a particular survey in a particular cluster may mean that some months of that cluster's "future" crop-year could occur in the past. In the vast majority of our specifications, these problems "around the edges" are minimized by summing shocks over a ten year period. However, when looking just at shocks in the future 1 year, the rainfall measure in certain clusters might not perfectly capture rainfall 1 year ahead, making this particular estimate somewhat noisier.

Finally, during the 2000's, there was increasing access to antiretrovirals (ARVs) for HIV-positive individuals, which may bias our results if access was in any way correlated with shocks. We show that during most of our study time frame, ARV access was relatively low (less than 30% for all but one country) and that there is no evidence that suggests ARV access is correlated with our shock measure (see Appendix F). Taken together these tests provide additional evidence that shocks are picking up meaningful variation in economic conditions prior to the survey year, and that this variation is uncorrelated with other factors that might also explain disease outcomes.

5 Exploring Pathways

5.1 Behavioral pathways

How might changes in income induce behavioral changes that increase HIV infection? As HIV is overwhelmingly transmitted by heterosexual sex in this context, we first examine whether risky sexual behaviors increase in response to recent shocks, using self-reported sexual behavior. We then consider three separate coping behaviors that could lead to increased sexual risk.

Risky sexual behavior

The use of self-reported sexual behavior is subject to a few caveats. There is a large body of evidence that suggests self-reported sexual behavior suffers from social desirability bias (Cleland et al., 2004) and that women significantly under-report their sexual activity (Minnis et al., 2009).[24] In addition, we only have measures of sexual behavior during the 12 months prior to the survey. It is not immediately clear which time window of shocks should be considered to impact sexual behavior in the past 12 months. Certainly shocks in the current and previous year should, however, given the potential lag between lack of rainfall and lack of income, perhaps droughts two years ago should have a similar impact. Further, more distant shocks that induced the creation of new sexual relationships may have continuing impacts on current behavior if those relationships (or behaviors) are persistent.[25] For this reason,

[24] Additional caveats are that 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. In addition, the questions about sexual behavior are not present in all the employed DHS surveys, and therefore the analysis is performed on a sub sample of our data.

[25] Swidler and Watkins (2007) cite multiple works documenting long-term extramarital unions in exchange for transfers. In addition, the sex-workers in Robinson and Yeh's (2011b) study started as sex-workers on average 9.7 years prior to the study.

we present the impact on recent sexual behavior of shocks within the past 10 years, shocks within the past 5 years, and having a shock that affected income over the past 12 months. Given these caveats, we interpret results on self-reported sexual behavior with caution.

The outcome variables we examine are whether in the past 12 months the respondent has (i) been sexually active, (ii) had multiple partners, or (iii) had non-spouse partner(s).[26] Table 7 shows results of estimations of Equation 2, separately by gender, with these self-reported sexual behaviors as the dependent variables regressed separately on three categories of independent variables as noted. A strong and consistent finding is that both men and women are significantly more likely to have engaged with a non-spouse partner if exposed to a shock in any of the three time periods considered. For both men and women, shocks affecting the past 12 months increase non-spouse partnership rates by about 10-20%. Shocks in nearly all of the periods also increase the likelihood of engaging with multiple concurrent partners by 10-15%, though the estimates are not precise in all periods. Point estimates for the impact of shocks on being sexually active at all are positive for men, but not significantly different from zero, and for women are not consistent across the periods considered.

Overall, these self-reports of sexual behavior indicate that individuals who have experienced recent shocks are more likely to report risky sexual activity. Keeping the caveats discussed earlier in mind, these findings suggest that shocks are indeed changing sexual behavior – and in particular leading to riskier sexual behavior – and that these behavioral changes are what likely link rainfall shocks to HIV. In the remainder of this section, we seek evidence for which coping behaviors may be primarily responsible for this relationship.

Temporary Migration

One response to drought-induced income shocks is to migrate from rural to urban areas in search of employment (Skoufias, 2003; Ellis, 2000). Migration is associated with greater levels of risky sexual activity and higher rates of HIV (Lurie et al., 2003; Brockerhoff and Biddlecom, 1999). Individuals may temporarily migrate to urban areas in response to droughts, acquire HIV due to additional partnerships or high-risk partners, and then infect others when returning to their rural communities.[27] If income shocks induce temporary migration, then $\frac{\partial p}{\partial z} < 0$ for both men and women, as both the migrant and his/her partner in the rural village would face increased risk.

As a check for this pathway, we use information on the number of times individuals have been away from home in the past 12 months, and whether any time away has lasted more

[26] In this data, a monogamous cohabiting union is considered a spousal partner, irrespective of formal marital status. Also, single, sexually active individuals are included in those having non-spouse partners.

[27] Note that, if the migration is of a permanent nature, this should not affect HIV in the rural area, though it may affect *our estimation* of rural HIV, due to sample selection. We directly address this in section 4.2.

than one month. If temporary migration is a primary coping behavior in this setting, we would expect that a shock in the past year would significantly increase both indicators. These outcomes are available for men in 17 (and for women in 9) of our 21 surveys, and estimation results are presented in Table 8. For comparison, the main estimation from Table 3, col. 5 is presented in cols. 1 and 2, for men and women respectively, for these sub-samples.

Cols. 3-6 of Table 8 show that for both men and women, shocks affecting the past 12 months have a correlation with the number of times away from home and being gone for more than one month in the past year that is either negative or indistinguishable from zero. We have disaggregated this effect for individuals who live near to an urban area versus those in more remote areas.[28] Neither of these sub-samples exhibit more frequent temporary migration when exposed to a shock.[29] This suggests that in our rural sample, droughts are not inducing significant temporary migration.

Dropping-out and Early marriage

A second set of coping behaviors that may affect sexual risk are changes in schooling and marriage behavior. In SSA, a common response to a negative income shock is to withdraw children from school (Ferreira and Schady, 2009), which appears particularly true for girls (Bjorkman, 2006). Once a girl has withdrawn from school she is much more likely to be sexually active and to marry (Osili and Long, 2008; Duflo et al., 2011; Ozier, 2010), both of which are risk factors for HIV (Clark, 2004; Baird et al., 2011). Furthermore, households may marry-off daughters earlier in response to a shock, especially in regions where bride payment is customary (Hoogeveen et al., 2011; Jensen and Thornton, 2003). If income shocks induce early drop-out and early marriage, which result in earlier sexual activity, then $\frac{\partial p}{\partial z} < 0$. While this could apply to both men and women, young women would be most affected through this channel. If early marriage is the pathway, either as a direct response to shocks or as a result of withdrawing from school, we would expect droughts to be associated with a younger age at marriage, and increased probability of marriage at the time of the survey. Further, if shocks are inducing drop-out, we would expect shocks to be associated with fewer years of schooling and expect shocks to have the strongest effects on HIV for women who were school-aged at the time of the shock.

The first two columns of Table 9 show estimates of the impact of shocks occurring when a woman was potentially subject to early marriage on whether she has ever married by the

[28]Near to urban is defined as being within 100km of an urban center with population 250,000 or more. Urban populations are from the Global Rural-Urban Mapping Project.

[29]These results look very similar when employing shocks during the past 2 or 3 years, rather than 12 months; results not shown.

time of the survey.[30] As mean age at marriage for women in this sample is 18, women are considered at risk for early marriage when aged 13 to 18 (col. 1) or aged 15 to 20 (col. 2). In neither case do shocks yield a significant increase in the likelihood of marriage at or before the time of the survey.

The second two columns estimate the impact of shocks during the same periods of life on the resulting age at marriage for those who have ever married. The coefficients reflect an effective zero change in age at marriage when exposed to a shock at these critical ages. In short, it seems that shocks do not induce earlier marriage for women in this sample.

Even if youth are not marrying earlier, households may respond to income shocks by withdrawing children from school, especially girls. Girls that drop out early are at higher risk for early sexual activity and HIV transmission (Baird et al., 2010). If this is a contributing factor in the link between rainfall and HIV, we would expect to find two telltale results. First, shocks should reduce total schooling for women who were school-aged when the shock occurred; second, the link between rainfall and HIV should be restricted to women who had not yet completed their schooling when the shock occurred. Columns 5 and 6 of Table 9 estimate the effect of shocks when aged 13 to 18 (and 15 to 20) on years of education. Both estimates produce a negative coefficient, however, both reflect effect sizes of less than 1% and are not statistically different from zero. We do not find evidence that rainfall shocks induce significant dropping out of girls.[31] Finally, columns 7 and 8 replicate our primary estimation, excluding women who were school aged during the past 10 years. We find that the results are robust to this exclusion, suggesting that women who were school-aged at the time of the shock are not driving the results. In sum, we find no evidence that early marriage and dropping out are the primary coping behavior linking rainfall to HIV.

Transactional Sex

A third coping mechanism is engaging in transactional sex. Transactional sex is thought to be common in sub-Saharan Africa, and is broadly defined to include both prostitution as well as transfers within casual relationships and long-term partnerships (Luke, 2006; Swidler and Watkins, 2007; Béné and Merten, 2008; Hunter, 2002; Maganja et al., 2007; Leclerc-Madlala, 2002).[32] Women may respond to income shocks either by taking on additional

[30] Only shocks occurring during the HIV epidemic are considered (1980 or later), as only these could be driving the results found.

[31] These findings are consistent with work by Shah and Steinberg (2013), showing that children in India actually attend school less when rains are plentiful as there is more work to be done outside school.

[32] One could argue that early marriage as a response to an income shock may also be considered transactional sex in some form. We argue that these are conceptually distinct as early marriage would be an increase in sexual activity at the extensive, rather than the intensive margin. Further, these are distinct from a policy perspective.

partnerships or engaging in more frequent or riskier sexual activity (i.e. unprotected sex) to increase transfers. Both types of behaviors have been documented throughout sub-Saharan Africa, with women in rural Malawi engaging in multiple partnerships in response to income insecurity (Swidler and Watkins, 2007), and women in South Africa and Western Kenya more likely to engage in unprotected sex as a response to negative income shocks (Dinkelman et al., 2008; Robinson and Yeh, 2011b; Dupas and Robinson, 2011). While there are many factors affecting the HIV/AIDS epidemic, transactional sex is thought to be a major driver within SSA (Alary and Lowndes, 2004; Dunkle et al., 2004; Côté et al., 2004), and a growing empirical literature suggests that economic conditions affect risky sexual behavior and the market for transactional sex (Baird et al., 2010; Kohler and Thornton, 2011; Robinson and Yeh, 2011a).

We cannot directly examine changes in this behavior, as we lack data on transactions.[33] To make progress, we make a few assumptions on the transactional sex market. First, we follow the literature in assuming that women supply and men demand transactional sex (Edlund and Korn, 2002). Second, in keeping with a recent micro literature (Baird et al., 2012; Kohler and Thornton, 2011; Robinson and Yeh, 2011b), we assume that women increase their supply of transactional sex if other sources of income decrease; and that when supply increases, prices fall. Finally, we assume that individuals experiencing larger income shocks should have a stronger behavioral response – that is, supply is increasing and demand is decreasing in shock exposure.

While we do not observe individual changes in income, we do observe occupation – in particular, whether or not an individual's primary income source is from agriculture.[34] We assume that incomes of individuals working in agriculture are more sensitive to drought than those working outside agriculture. In the market for transactional sex, we would expect that men working outside agriculture would increase their quantity demanded in the face of an

[33] Whether a man has paid for sex in the past year is only queried in four surveys from high prevalence countries. This likely only captures explicit prostitution, rather than all forms of transactional sex, as the reporting is low (3%). Women are not queried regarding payment for sex in any of our surveys. In addition, examining whether women are entering the transactional sex market, or are simply make changes on the intensive margin as a response to shock would be very interesting, however, given these data limitations, we are unable to say anything about this topic.

[34] 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. 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. A concern with using occupational category is that it 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.

aggregate shock, based on the reduced price. Further, men working in agriculture would reduce their quantity demanded. However, as men working in agriculture will also face increased network risk, the effect of shocks on their HIV status should be dampened but not necessarily reversed, relative to men working outside agriculture. Before turning to the results, we stress that given the assumptions we make, our findings in this section warrant caution. While our results are consistent with transactional sex being the channel linking shocks to HIV, we cannot definitively claim this. Future research with comprehensive data on shocks, sexual behavior, and transfers will shed more light onto this channel.

Table 10 presents the primary estimation for both men and women, with interactions by occupation. For women, the effects of shocks appear concentrated on agricultural women (Column 4; Row 1), while women in the non-agricultural sector appear relatively unaffected by shocks (Column 4; Row 5).[35] These results makes sense as income of agricultural women is most affected by a drought; these results are also consistent with various channels. To sharpen our analysis, we examine the effects of shocks on men separated by occupation. We find the impact on non-agricultural men's HIV risk is large and significant at the 10% level (Column 2; Row 5), while the effects of shocks for agricultural men is nearly zero (Column 2; Row 1). While we cannot reject the null that shocks have the same effect for men in and outside of agriculture (p-value = .167), these estimates are consistent with transactional sex being the channel linking shocks to greater HIV rates.[36] If shocks are inducing women to supply more sex, then men whose incomes are least affected by droughts (i.e. men employed outside of agriculture) should increase their demand. While men in agriculture would face lower prices in the market for transactional sex, their income will also be affected by drought, dampening any price effects.

Finally we note that our shock measure is an aggregate level shock that would presumably effect the incomes of all men and women in an area (regardless of occupation). However, it maybe the case that men are better insured against shocks than women (see Dercon and Krishnan, 2000) which may lead women to be more responsive to aggregate shocks than men. Our findings are consistent with this view as well as previous work that finds the supply side responding more to aggregate shocks than the demand side (Dupas and Robinson, 2011; Wilson, 2011).

To then summarize the results from this section, our main finding is that individuals

[35] We cannot reject the null that shocks have the same effect on women in and outside of agriculture (p-value = .252).

[36] We also find that the magnitude of increases in HIV are consistent with increases in transactional sex. Robinson and Yeh (2011a) find that an individual level health shock that results in total income loss for one day leads a woman to increase her number of sexual partners the following day by 0.3, an 18% increase in their sample. We find that this is comparable to our findings that a year-long income shock increases a woman's lifetime partnerships by about 33%. See simulation in appendix G.

exposed to recent drought events are more likely to be infected with HIV $\left(\frac{\partial HIV}{\partial S} > 0\right)$. Given the strong evidence of both the relationship between droughts and income $\left(\frac{\partial z}{\partial S} > 0\right)$ and risky sexual behavior and HIV $\left(\frac{\partial HIV}{\partial p} > 0\right)$, this suggests that the underlying mechanism connecting droughts and HIV is a behavioral response to income shocks that is leading to increased sexual risk $\left(\frac{\partial p}{\partial z} < 0\right)$. We find no evidence that temporary migration or dropping out /early marriage are the key drivers of this relationship. This section provides evidence that is broadly consistent with transactional sex as a pathway. However, we cannot conclusively establish the primary behavior driving this result, nor can we rule out any single behavior as a contributing factor.

5.2 Non-behavioral pathways

Each type of behavior discussed above - early sexual activity, migration, transactional sex - has a well-documented connection to HIV risk and a plausible link to community-level income shocks. However, droughts also have documented effects on other important factors in rural areas, such as nutrition and civil conflict. We argue that the evidence linking these factors to HIV outcomes is, at best, inconclusive, and that they are unlikely to be pathways that link shocks to HIV.

For HIV infected individuals, malnutrition is associated with higher mortality rates and higher viral loads (John et al., 1997; Weiser et al., 2009). Thus the effect that malnourished HIV-positive individuals will have on the epidemic is ambiguous; higher mortality rates would lead to fewer HIV-positive individuals but higher viral loads would make them more infectious.[37] For HIV-negative individuals, little is known about the relationship between malnutrition and susceptibility to HIV infection (Mock et al., 2004). Though malnutrition may lead to a compromised immune system which could play a role in susceptibility (Schaible and Stefan, 2007), to the best of our knowledge there is no work that demonstrates an increase susceptibility to HIV infection for malnourished HIV-negative individuals. While we cannot rule out that this is a contributing pathway, given the existing evidence it does not appear to play a primary role in the HIV/AIDS epidemic.

Some recent evidence suggests that negative rainfall deviations are associated with higher incidence of civil conflict in Africa (Miguel et al., 2004; Hsiang et al., 2013). This could indicate another pathway between rainfall and HIV if civil conflict has a direct effect on disease outcomes, for instance due to conflict-related sexual violence. While we again cannot directly rule out this possibility in our data, recent studies find no clear link between conflict and HIV in either the observational data from Africa (Spiegel et al., 2007), or using epidemiolog-

[37] We note, however, that high viral loads may make individuals too sick to be sexually active (see Thirumurthy et al., 2012).

ical models that attempt to explain observed HIV prevalence with reported rates of sexual violence (Anema et al., 2008). We have thus focused our empirical exploration of pathways on the the three coping behaviors described above.

6 Macro level implications

Our results suggest that community-level economic conditions play an important role in an individual's risk of HIV infection. A natural question is the extent to which our results inform broader observed patterns of HIV prevalence on the continent. In other words, can income shocks help explain the striking country-level variation in HIV prevalence across sub-Saharan Africa? Given that our estimation strategy above uses only within-country variation, and that we only have individual-level HIV data for about half of the countries in the Sub-Saharan region spread out over different years, it's not obvious that our estimates should inform these broader patterns.

To address this question, we apply our basic approach to country-level estimates of HIV prevalence provided by UNAIDS. UNAIDS estimates of country level HIV prevalence over time build heavily on HIV surveillance data distinct from what is in the DHS (e.g. data from antenatal testing at designated clinics), and thus provide prevalence estimates that are somewhat independent from the DHS biomarker data we focus on 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.[38] 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. This regression uses a different source of variation from our individual specifications (cross-country rather than within-country), uses data that are related but distinct, and includes many countries not in our individual-level data. It thus provides a test of the relationship between shocks and HIV that is substantially distinct from the results presented above.

Figure 3 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-

[38]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).

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).[39] Again, as with our individual-level results this estimate is not picking up differences in underlying propensity to experience shocks (which could be correlated with other factors affecting HIV), but relies instead on the random timing of recent shock exposure.

We draw three implications from these results. 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 economic conditions exert significant influence on HIV outcomes. Second, our results suggest that bad luck with the weather might have played a surprising role in shaping observed patterns of the AIDS epidemic across the African continent: countries that were hit with large negative shocks during the early years of the epidemic have much higher infection rates many years later. Finally, and somewhat more speculatively, given that many areas in sub Saharan Africa lack social safety nets and depend heavily on rainfed agriculture, recurring droughts may play an important and prominent role in explaining why the AIDS epidemic has disproportionately affected sub-Saharan Africa.

7 Conclusion

Ultimately any halt to the AIDS epidemic will require a medical intervention, such as a vaccine or methods approximating one (e.g. the aggressive use of ARVs). However, our results suggest that economic factors, and in particular the ways in which individuals respond to changes in their economic environment, also play an important role in shaping outcomes in the epidemic. As such, our findings unite two widely-held notions among researchers in the HIV/AIDS community: that heterosexual sex is a primary driver of the AIDS epidemic in sub-Saharan Africa, and that economic conditions play some role in sexual behavior in these countries.

Our paper provides compelling evidence that a deterioration in economic conditions, in the form of rainfall-related income shocks, contributes significantly to both village- and country-level rates of HIV infection in sub-Saharan Africa. While there are several possible pathways linking shocks to HIV, the available evidence is inconsistent with all the poten-

[39] We also explore whether shocks can explain the time-path of the epidemic by looking at cross-country decadal *changes* in HIV prevalence as a function of accumulated shocks. Effect sizes are again large but not always quite significant at conventional levels (p=0.12 on the shock variable for 1990s changes), and we explain somewhat less of the cross-country variance in decadal trends than we do in levels. Nevertheless, results are broadly consistent with cross-sectional results.

tial pathways discussed here, except transactional sex. Nonetheless, we have no conclusive evidence that transactional sex is indeed the pathway, and we cannot fully rule out that the other risk coping mechanisms discussed, such as early marriage, school drop-out, or migration, are also contributing factors.

Regardless of the pathway, the policy implications of these findings are substantial. If income shocks lead households to smooth income in ways that contribute to the epidemic, policies that prevent the need for these coping mechanisms would appear to 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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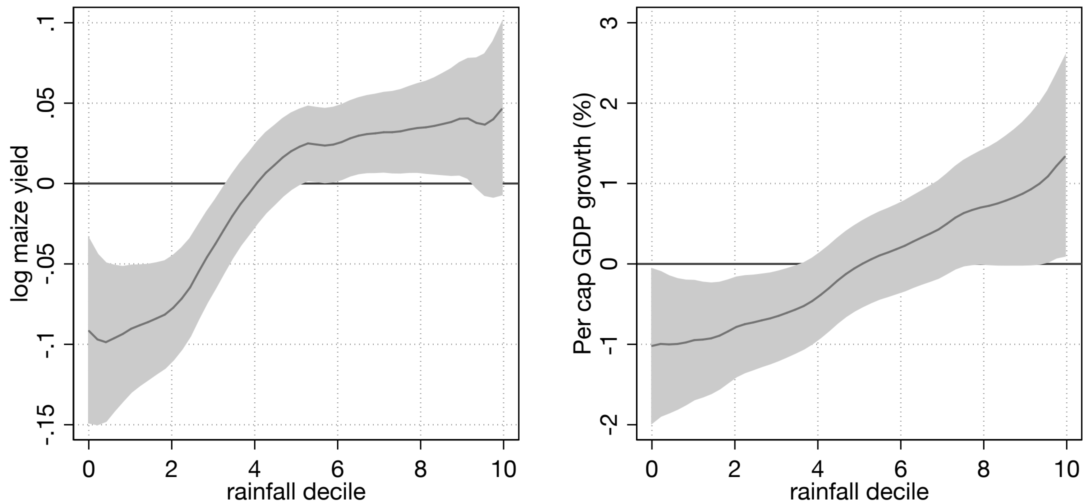
Figures

Figure 1: Effect of rainfall shocks on African maize yields (left panel) and per capita GDP growth (right panel)

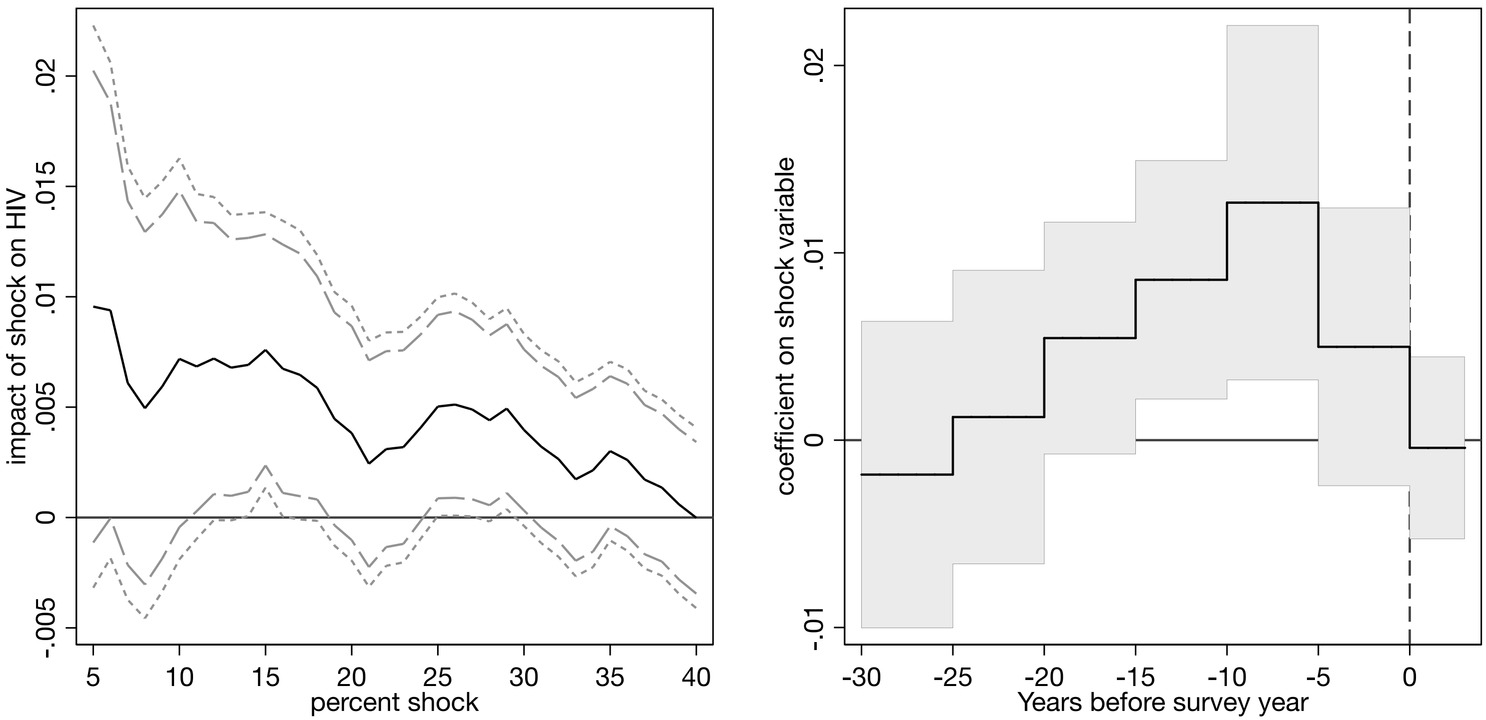


Data are at the country level over the period 1970-2008, and include all sub-Saharan African countries. Dark lines display point estimates from kernel-weighted local polynomial regressions of the outcome on rainfall percentiles, after removing country and year fixed effects. Grey areas represent 95% confidence intervals. Data sources are given in the text.

Figure 2: Effect of rainfall shocks on HIV, by severity and timing

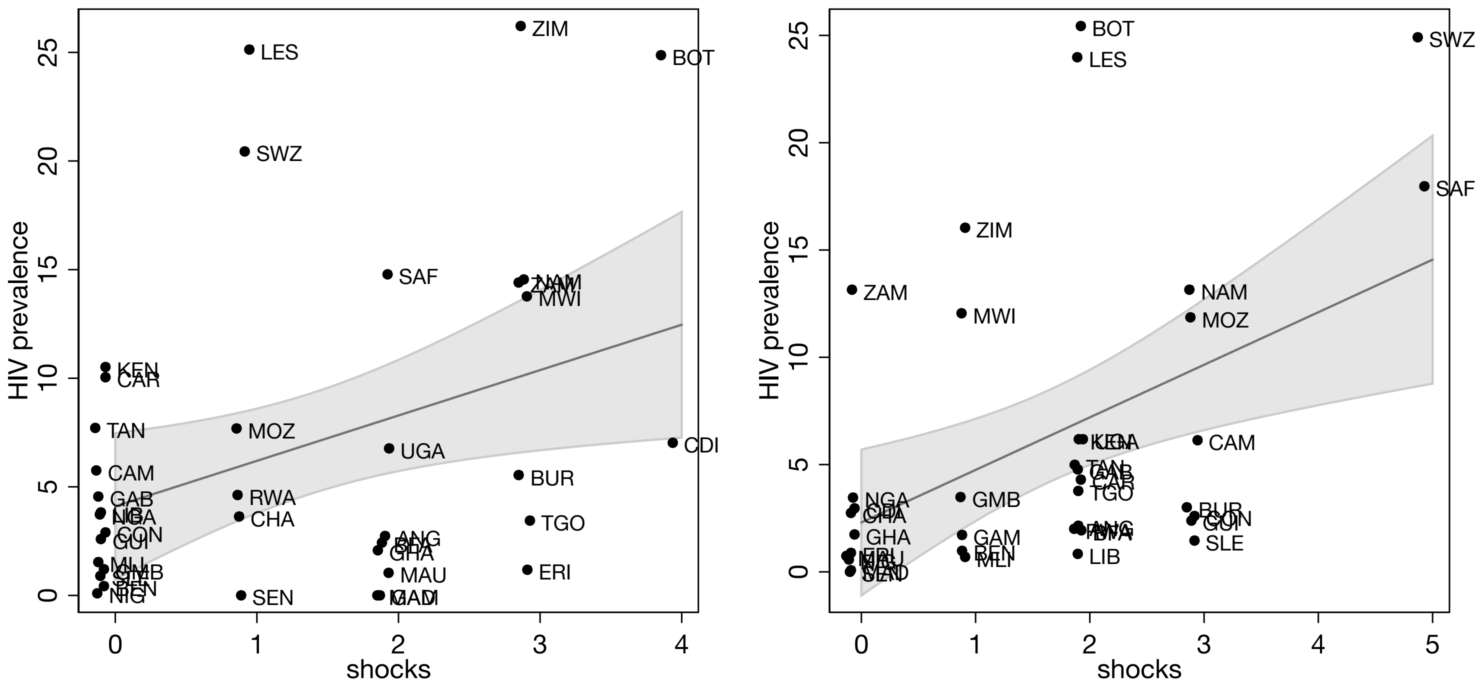
Figure 2.A: Shock severity

Figure 2.B: Shock timing



The black line represents the coefficient point estimates of the impact of a particular rainfall shock on HIV for the rural sample from high-prevalence countries, using (A) various definitions of "shock" accumulated over the previous 10 years and (B) 15% shocks accumulated over different time periods, including placebo future shocks up to 3 years past the survey date. Plot A: dotted lines represent the 95% confidence intervals; dashed lines represent the 90% confidence intervals. Plot B: shaded area represents 95% confidence interval.

Figure 3: Country-level HIV prevalence & Shocks



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

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

Table 2: Shock Prevalence by Country

Prevalence Rank	Country	Survey Year	Mean Shocks	SD Shocks	Number of Clusters	Weather Grids
1	Swaziland	2007	2.90	0.46	275	13
2	Lesotho	2004	1.89	0.44	405	18
3	Zambia	2007	0.84	0.75	319	146
4	Zimbabwe	2006	1.28	0.76	398	122
5	Malawi	2004	1.04	0.75	521	53
6	Mozambique	2009	2.54	1.51	270	115
7	Tanzania	2008	0.77	0.82	345	167
8	Kenya	2003	1.17	0.62	400	81
9	Kenya	2009	1.22	0.78	398	93
10	Tanzania	2004	1.92	0.93	475	178
11	Cameroon	2004	1.59	1.06	466	112
12	Rwanda	2005	2.37	0.61	462	14
13	Ghana	2003	1.31	0.80	412	71
14	Burkina Faso	2003	1.28	0.90	400	88
15	Liberia	2007	1.35	1.05	298	37
16	Guinea	2005	1.34	0.75	295	72
17	Sierra Leone	2008	3.00	0.00	353	27
18	Ethiopia	2005	1.12	1.12	535	167
19	Mali	2006	1.00	0.71	407	149
20	Congo DR	2007	1.89	1.06	300	168
21	Senegal	2005	0.70	0.69	376	61
	Total		1.51	1.04	8110	1701

Table 3: Effect of Shocks on HIV

	All	All	Rural	Rural	Rural Hi Prevalence	Rural Hi Prevalence
	(1)	(2)	(3)	(4)	(5)	(6)
Num. shocks past 10 yrs.	.003*	.004**	.003**	.008**	.008**	.009**
	(.001)	(.002)	(.002)	(.003)	(.003)	(.004)
Shocks * Urban		-.004				
		(.002)				
Shocks * Low Prevalence Co.				-.008**		
				(.003)		
Shocks * Male						-.003
						(.003)
Interaction p-value		.104		.016		.243
Linear combination		-.000		-.000		.006**
		(.002)		(.001)		(.003)
Observations	202216	202216	134874	134874	77760	77760
R^2	.053	.053	.046	.046	.030	.030
Mean Dependent Var	.050	.050	.041	.041	.070	.070

Column headers indicate sample employed. Specifications include controls for gender and age, rural/urban designation (where applicable), 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. "Interaction p-value" is the p-value for Shocks X Urban (column 2), Shocks X Low Prevalence Co. (column 4), and Shocks X Male (column 6). "Linear combination" is the sum of coefficients on the number of shocks and the interaction term in each specification. For column 2, the linear combination is (Num shocks past 10 years) + (Shocks X Urban), column 4 is (Num shocks past 10 years) + (Shocks X Low Pow Prevalence Co.), and column 6 is (Num shocks past 10 years) + (Shocks X Male).

Table 4: Robustness Checks

Panel A: Robustness to specifications and sample

	No Controls	No Wgts	Co-YR FxEfx	No Hyper-Endemic	No Visitors	One svy per cntry	Aged 15-50 at shock	Alternative shock definitions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Num. shocks past 10 yrs.	.008**	.005*	.008**	.008**	.011***	.009**	.010**		
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.004)		
Shocks, defined by 1970-95 weather								.006**	
								(.003)	
1.5 SD shocks, past 10yr									.015**
									(.007)
Observations	77760	77760	77760	68287	53596	65326	36812	77760	77760
R^2	.020	.068	.030	.025	.035	.035	.033	.030	.030
Mean Dependent Var	.070	.110	.070	.068	.063	.078	.158	.070	.070

Panel B: Robustness to controlling for moments of rainfall distribution

	Mean	Variance	Skew	All Moments
	(10)	(11)	(12)	(13)
Num. shocks past 10 yrs	.009***	.008**	.008**	.008***
	(.003)	(.003)	(.003)	(.003)
Observations	77760	77760	77760	77760
R^2	.031	.030	.030	.031
Mean Dependent Var	.070	.070	.070	.070

Rural sample from high-prevalence countries. All specifications include controls for gender, age and survey fixed effects, except as noted. Col. 1 does not include the standard controls. Col. 2 does not include weights. Col. 3 employs country-year fixed effects, rather than survey fixed effects. Col. 4 excludes the hyper-endemic countries (Swaziland and Lesotho). Col. 5 excludes individuals who have lived in their current village for less than one year. Col. 6 employs only the most recent survey from each country (excludes KE 2003 and TZ 2004). Col. 7 includes only individuals who are aged 15+ at the time of the shocks. Cols. 8-9 employ alternative definitions of a shock: col. 8 employs only the years 1970-1995 to create the historical distribution from which the 15th percentile is the shock definition, and col. 9 defines a shock as 1.5 SD below the local mean. 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.

Table 5: Robustness to sample selection from permanent migration

	Observed	Replacing lost population share ***per shock*** 2%	3%	4%	5%	6%
	(1)	(2)	(3)	(4)	(5)	(6)
Num. shocks past 10 yrs.	.008***	.007**	.006**	.005*	.004*	.004
	(.003)	(.003)	(.003)	(.003)	(.003)	(.002)
Observations	77760	81792	84191	86523	88775	91330
R^2	.030	.022	.023	.023	.024	.024

Rural sample from high-prevalence countries. Column headers denote the population share added to the sample to account for out-migration, assuming all out-migrants are HIV negative. Note that 2% is the most accurate estimate with 4% as the extreme upper bound (see appendix D). All specifications include controls for gender, age 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 6: Placebo Tests

Dependent Variable	HIV				Wealth quintile			Yrs of Education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Num. shocks in future 1 yr	.006				.005			-.152		
	(.006)				(.110)			(.232)		
Num. shocks in future 2 yrs		-.002				.113			-.134	
		(.007)				(.095)			(.194)	
Num. shocks in future 3 yrs			-.004				.104			-.146
			(.007)				(.096)			(.194)
Num.shocks in future 4 yrs				-.006						
				(.006)						
Observations	49523	43881	26059	12434	49523	43881	26059	49489	43861	26039
R^2	.044	.040	.025	.010	.031	.033	.029	.119	.118	.102

Rural sample from high-prevalence countries. Note that the only survey in 2003 (Kenya) does not contain information on wealth and education; therefore, the correlations of these characteristics with shocks can only be calculated up to three years in the future, as weather data ends in 2007-2008 crop years. All specifications include controls for gender, age 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 7: Exploring Behaviors: Increasing risky sexual behavior

	Women			Men		
	Sexually Active	Multiple Partners	Nonspouse Partner	Sexually Active	Multiple Partners	Nonspouse Partner
	(1)	(2)	(3)	(4)	(5)	(6)
Num. shocks past 10 yrs.	.012**	.003*	.007*	.008	.018***	.015**
	(.005)	(.002)	(.004)	(.006)	(.004)	(.006)
Observations	43145	43119	43147	34607	34563	34613
R^2	.060	.011	.018	.223	.034	.051
Num shocks past 5 yrs	.021***	.004*	.013**	.008	.016**	.021**
	(.007)	(.002)	(.005)	(.009)	(.006)	(.009)
Observations	43145	43119	43147	34607	34563	34613
R^2	.060	.011	.018	.223	.033	.050
Y/N shock affecting past 12mo	-.027**	.003	.023**	.013	-.007	.035**
	(.012)	(.004)	(.010)	(.012)	(.012)	(.017)
Observations	43145	43119	43147	34607	34563	34613
R^2	.059	.011	.018	.222	.032	.051
Mean of Dep Var.	.759	.024	.120	.738	.154	.269

Rural sample from high-prevalence countries. Dependent variables are sexual behaviors in the past year. "Non-spouse" indicates sex with a non-spouse partner; this includes all sex for single individuals. All specifications include controls for age 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 8: Exploring Behaviors: Temporary migration

	Main HIV result for this sub-sample		Away for month+ in past year		Total times away in past yr	
	Men	Wmn	Men	Wmn	Men	Wmn
	(1)	(2)	(3)	(4)	(5)	(6)
Num. shocks past 10 yrs.	.007*	.015***				
	(.003)	(.004)				
Y/N shock in past yr			-.010	.018	-.421***	-.229**
			(.024)	(.018)	(.149)	(.115)
Near urban * shock in past yr			-.018	-.008	-.419	-.569***
			(.031)	(.027)	(.282)	(.188)
Near urban			-.008	-.009	-.148	.162*
			(.011)	(.012)	(.199)	(.084)
Observations	26096	26299	26133	26300	23802	22990
R^2	.038	.041	.004	.016	.025	.117
Mean of Dep Var	.064	.094	.151	.130	2.064	.990

Rural sample from high-prevalence countries. The "Near urban" variable indicates whether a given cluster is within 100km of an urban area (defined as population size 250K+); this represents 27% of the rural population in high prevalence countries. Urban populations are from the Global Rural-Urban Mapping Project. Variables on being away are not available for all countries (see text); the main estimation for these sub-samples are given in Cols. 1 and 2. All specifications include controls for age 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 9: Exploring Behaviors: Early school drop-out and marriage

Dependent Variable:	Ever married		Age at marriage		Years of Educ		HIV Status	
							Aged 25+	Aged 30+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Num. shocks, aged 13 to 18	-.000		.000		-.010			
	(.005)		(.046)		(.056)			
Num. shocks, aged 15 to 20		-.003		.006		-.005		
		(.005)		(.057)		(.054)		
Num. shocks past 10 yrs.							.011*	.016**
							(.006)	(.006)
Observations	24679	22679	23770	23005	27429	25242	12280	3845
R^2	.125	.065	.033	.023	.223	.222	.031	.022
Mean Dep Var	.881	.923	18.1	18.3	5.5	5.4	.091	.067

Female, rural sample from high-prevalence countries. The first six columns examine the impacts of shocks that occurred when woman was in the noted age range, since the start of the epidemic (1980). The last two columns examine the impact of shocks in the past 10 years on HIV for women who were above a minimum age during all of the past 10 years. All specifications include controls for age 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 10: Exploring Behaviors: Impact on HIV by exposure to drought-induced income shock

	Men		Women	
	(1)	(2)	(3)	(4)
(1) Num. shocks past 10 yrs.	.004	.002	.009**	.011***
	(.003)	(.003)	(.004)	(.004)
(2) Non-Ag * Shocks		.007		-.007
		(.005)		(.006)
(3) Non-Ag employment		.049***		.116***
		(.016)		(.023)
(4) $p - value$ on interaction		.167		.252
(5) Impact of shocks on NonAg (lin. comb.)		.009*		.004
		(.004)		(.006)
Observations	37585	37585	37487	37487
R^2	.033	.039	.032	.039
Mean Dep Var (Ag)		.056		.077
Mean Dep Var (Non-Ag)		.096		.148

Sample from high-prevalence countries. All specifications include controls for age and urban, and survey x employment type fixed effects to allow the correlation between HIV and occupation to vary by country. Note that NonAg indicator alone is indicative only for the country excluded in fixed effects (Mozambique). Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Appendices (for online publication)

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

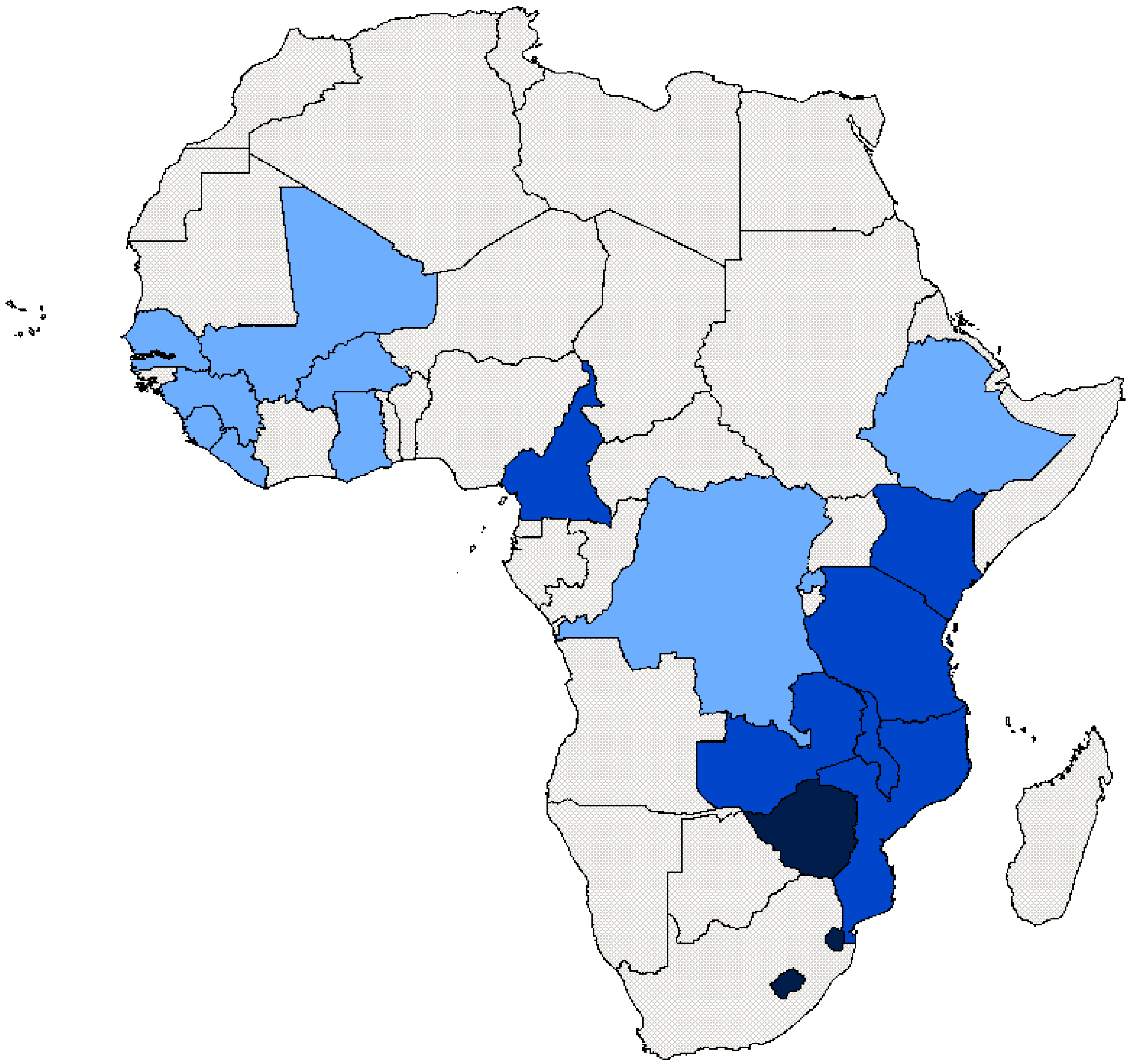
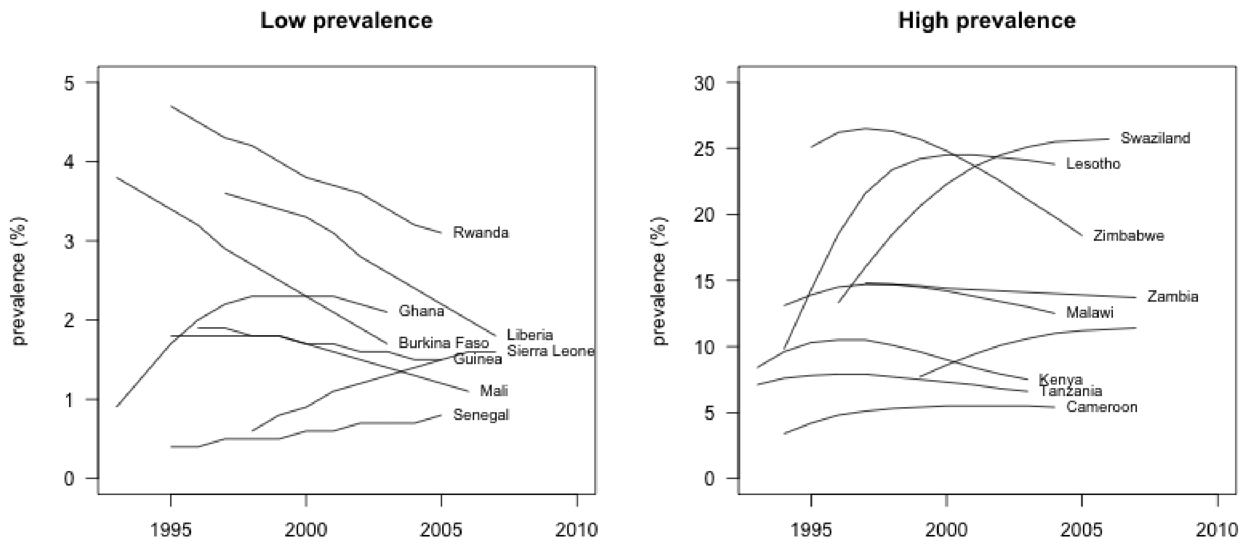


Figure A.2: Pre-survey 10-year HIV trends, Low and High Prevalence Countries.



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 A.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 A.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%

Rates are for the full HIV testing sample, with the exception of Mozambique. Rates for MZ are for the 15-49 sample. Rates are those reported in the DHS final reports for each survey, as the outcome of HIV measuring at the individual level is not included as an indicator in most data sets.

Table A.3: Non-response is not correlated with Shocks

Dependent Variable –>	Refused (1)	Refused (2)	Selected but not tested (3)	Selected but not tested (4)
Num. shocks past 10 yrs.	-.002	-.002	.001	.001
	(.005)	(.004)	(.003)	(.003)
Indiv. controls	No	Yes	No	Yes
Mean of Dep. Var	.100	.100	.118	.118
95% CI for coeff	(-.011, .007)	(-.009, .006)	(-.006, .008)	(-.005, .006)
Observations	70547	70547	190794	190794
R^2	.026	.034	.032	.045

Note: Whether or not a selected individual refused an HIV test is the dependent variable in columns 1 and 2. The outcome of the test request, including refusal and failure to test for other reasons is given only for women in the following surveys: 2005 (ZW), 2006 (ML, SZ), 2007 (DRC, LB, ZM), 2008 (SL, KE), is given for men and women in 2007 TZ, and is not given at all in the remaining 12 surveys. For this reason, columns 3 and 4 employ all surveys and use lack of HIV test result as the dependent variable. Recall that only a sub-sample of households were selected for the men's survey and HIV testing (see section 3.1), and we endeavor to include only individuals from these households in this analysis. Selection into the sub-sample is not indicated in the data, and thus these households are only identifiable by the existence of an interview with, or a test result from, a male in the household. For households without data on a male, we are not able to identify the selected households (some households were selected but had no male present, for example). The sample employed in columns 3 and 4 includes all individuals in households that have data on a male in any of the surveys. These individuals were definitely selected for HIV testing, though they are not *all* of the individuals selected for testing.

The estimates suggest a fairly precise zero effect of shocks on test refusal or non-response. Based on the 95% confidence intervals, we can reject that a shock affects testing rates by more than one percentage point.

B Weather data and impact of drought on crop yields

To help confirm that our measure of recent 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 grid's rainfall distribution. Table B.1 presents the results. In all specifications, these correlations are not significant. In other words, when we estimate across grids, recent rainfall shocks are orthogonal to all three moments of the historical distribution.

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. To demonstrate this, we construct a country-level shock measure and estimate it's impact on maize yields and real per capita economic growth (Data are from Matsuura and Willmott, 2009; FAO,2011, and Heston et al. (2011), respectively). Maize is the most widely grown crop in Africa, and we have data for 41 Sub-Saharan African countries for 1961-2008.[40] We similarly have data on real per capita income growth for these same countries across 1961-2008.

The first four columns of Table B.2 show 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. Annual maize yields are strongly affected by precipitation: yields are about 12% lower in a year with rainfall at or below the 15th percentile, and 18% lower in a year with rainfall below the 10th percentile. Results are robust to including temperature shocks in the regression. 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).[41] We repeat the same regressions using growth in real per capita GDP as the outcome variable (Columns 5-8). Negative rainfall shocks again reduce growth rates dramatically, with bigger shocks leading to larger declines in growth rates. We estimate that a 15% shock reduces the economic growth rate in that year by 1.8 percentage points, and a 10% shock by 1.9 percentage points. This demonstrates again that rainfall shocks exert substantial influence on economic productivity in Africa.

[40]The included countries are: Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Central African Republic, Chad, Congo, Côte d'Ivoire, Democratic Republic of the Congo, Eritrea, Ethiopia, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mozambique, Namibia, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, South Africa, Swaziland, Togo, Uganda, United Republic of Tanzania, Zambia, and Zimbabwe.

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

Table B.1: Rainfall Shocks and Overall Variability

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

	(1)	(2)	(3)	(4)
mean	.000			.000
	(.000)			(.000)
variance		-.000		-.000
		(.000)		(.000)
skew			-.181	-.128
			(.143)	(.167)
Observations	1701	1701	1701	1701
R^2	.181	.182	.185	.194

Estimation at the grid level, with country fixed effects. Robust standard errors are shown in parentheses, clustered at the country level.

Table B.2: Impact of precipitation shocks on maize yields and per capita GDP growth.

	(1) yield	(2) yield	(3) yield	(4) yield	(5) GDP	(6) GDP	(7) GDP	(8) GDP
10 PCT shock	-0.180***				-1.880***			
	(0.016)				(0.475)			
15 PCT shock		-0.118***				-1.821***		
		(0.023)				(0.631)		
20 PCT shock			-0.148***				-1.694***	
			(0.025)				(0.600)	
30 PCT shock				-0.055				-1.155
				(0.039)				(0.817)
Constant	0.015	0.003	0.009	-0.005	5.490*	5.505*	5.495*	5.506*
	(0.078)	(0.077)	(0.078)	(0.084)	(2.810)	(2.812)	(2.830)	(2.768)
Observations	1537	1537	1537	1537	1533	1533	1533	1533
R squared	0.270	0.259	0.271	0.249	0.156	0.157	0.157	0.154

Standard errors in parentheses

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

Dependent variable is the log of country-level maize yield (columns 1-4) or real per capita GDP growth (columns 5-8). Regressions cover years 1970-2008 and include country fixed effects, year fixed effects, and a constant, and are weighted by country average maize area (maize regressions) or country population (GDP regressions). Errors are clustered at the country level. Yield data are from FAO (2011), GDP data are from the Penn World Tables (Version 7.0), and weather data are from UDel.

C Shock Definition

Because by construction the number of shocks in each cluster is fixed across the 40 years of rainfall data that we use, one concern is that shocks are therefore mean reverting from decade to decade, and that this somehow might bias our results. In this section, we show that any mean reversion is likely to make our estimates lower bounds on the effect of shocks on HIV.

To see this, we first note that HIV is incurable and is thus fairly persistent across decades: while 10 years is a benchmark life expectancy following infection, in fact only 50% of those infected will die within 10 years (see appendix figure E.1). Given this fact, consider two clusters, A and B. As constructed, the total number of shocks in each cluster is fixed, such that more shocks falling in one decade leads in expectation to fewer shocks in the next. Suppose that cluster B had more shocks between 1988-1997 than cluster A and thus higher HIV rates in 1997 compared to A. Given that there is substantial persistence in HIV prevalence over time - HIV is incurable, and average survival after infection is 10 years, meaning that many HIV+ individuals survive for more than a decade - when we measure HIV in 2007 for cluster B, we'll also be picking up the effects of past shocks (1988-1997). But because our shock measure is (in expectation) negatively correlated across decades, the lower number of shocks in cluster B in 1998-2007 will bias towards zero our estimate of the effect of these shocks on 2007 prevalence: we saw fewer shocks in 1998-2007, but higher prevalence due to the previous decade's shocks. A similar logic holds for Cluster A: the persistence of HIV means that a low number of shocks in 1988-1997 leads, all else equal, to lower prevalence in 2007, but low shocks in 1988-97 also mean a likelihood of higher shocks in 1998-2007. Our estimate of the effect of this higher number of shocks will again be biased towards zero, because 2007 prevalence in A will be lower due to the low number of 1988-97 shocks.

As further evidence of the robustness of our shock measure and chosen thresholds, and in the interest of full transparency, Table C.1 provides results from 40 separate estimations using different thresholds for the various population sub-samples used throughout the text. The analogous figure for our main results is Figure 2A.

Table C.1: Vary Shock Definition: 10 to 20%

	Dependent Variable: HIV Infection			
Pct Shocks			Rural & High Prevalence	
Threshold	All	Rural	All	Women
	(1)	(2)	(3)	(4)
10%	0.002	0.003	0.007	0.009*
	(0.002)	(0.002)	(0.005)	(0.005)
11%	0.002	0.003	0.007*	0.008*
	(0.002)	(0.002)	(0.004)	(0.004)
12%	0.002	0.003*	0.007*	0.009**
	(0.002)	(0.002)	(0.004)	(0.004)
13%	0.002	0.003*	0.007*	0.009**
	(0.002)	(0.002)	(0.004)	(0.004)
14%	0.002*	0.003*	0.007**	0.009**
	(0.001)	(0.002)	(0.003)	(0.004)
15%	0.003*	0.003**	0.008**	0.010***
	(0.001)	(0.002)	(0.003)	(0.004)
16%	0.002	0.003*	0.007**	0.009**
	(0.001)	(0.002)	(0.003)	(0.004)
17%	0.002	0.003*	0.006*	0.008**
	(0.001)	(0.002)	(0.003)	(0.004)
18%	0.002	0.003*	0.006*	0.007**
	(0.001)	(0.002)	(0.003)	(0.004)
19%	0.001	0.002	0.004	0.006*
	(0.001)	(0.002)	(0.003)	(0.003)
20%	0.001	0.002	0.004	0.005
	(0.001)	(0.002)	(0.003)	(0.004)
N	202,216	134,874	77,760	43,147

Each cell represents a separate regression, with the row indicating the threshold at which a shock is generated and the column indicating the sum-sample employed. All specifications include controls for gender, age 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.

D Estimating sample selection due to out-migration

In order to estimate the rate at which shocks affect permanent out-migration rates, we begin with an estimate of rural-to-urban migration. 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. Given the wide range of reasons for migrating to urban areas, migration in response to a shock likely accounts for no more than 20%-30% of this total (van Dijk et al., 2001). Nonetheless, we conservatively assume that low-rainfall shocks account for as much as half of this migration, and therefore induce a rural population loss of approximately 2.9% *over ten years*. Note that this is the accumulated loss from all shocks occurring during a ten year period.

We use these estimates to back-out the share of population that leaves during each shock. The column headers in Table D.1 show several possible assumptions of population loss *per shock* ranging from 1% to 5%. A bit of algebra reveals that if, for example, 3% of the population leaves *during each shock*, a village with three shocks over the past ten years has lost 8.73% 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 D.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 preceding the survey. The bottom row of table D.1 shows these estimates of total population loss over a ten year period.

The second column, which assumes that 2% of the population leaves per shock predicts that the rural sample has lost 2.91% of the population over the ten year period. This prediction aligns the best with the estimate that rural areas lose 2.9% of population to drought-induced migration over a ten year period. Therefore 2% loss per shock is the assumption maintained. Notice that, if we assume that *all* out-migration is shock induced (i.e. 5.8% loss over 10 years), this would suggest 4% population loss per shock. We therefore take 4% loss per shock as our extreme upper bound.

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

Table D.1: Potential Loss in Rural Populations due to Shock-induced Migration

Out-migration Per Shock –>	1%	**2%**	3%	**4%**	5%
Shocks over 10 yrs	10-yr population loss				
0	0.00%	0.00%	0.00%	0.00%	0.00%
1	1.00%	2.00%	3.00%	4.00%	5.00%
2	1.99%	3.96%	5.91%	7.84%	9.75%
3	2.97%	5.88%	8.73%	11.53%	14.26%
4	3.94%	7.76%	11.47%	15.07%	18.55%
5	4.90%	9.61%	14.13%	18.46%	22.62%
6	5.85%	11.42%	16.70%	21.72%	26.49%
7	6.79%	13.19%	19.20%	24.86%	30.17%
Estimate of **10-yr reduction** in population based on number of shocks observed in our data	1.46%	**2.91%**	4.33%	**5.74%**	7.13%

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 10-year loss* from the rural sample as a whole based on the shocks observed in the data. The highlighted columns best match our rural-to-urban migration estimates that (col. 2) rural areas lose approximately 2.9% of population over the course of ten years due to drought-induced migration and (col. 4) villages lose 5.8% of population over the course of ten years due to total (all-cause) migration.

E Considering shock timing

In this appendix, we consider whether shocks occurring within the past ten years differ in their impacts on the HIV epidemic according to whether they occurred relatively early or late during that period. We begin by simulating a model of the epidemic and then observe the impact of simulated shocks at various points in time.

Simulating the epidemic

We simulate an epidemic broadly representative of the high prevalence countries in our sample in the following way. In 1950, one person is infected in a country with a population of 25 million.[43] Each year, the newly infected individuals infect, on average, 1.2 other individuals (Pinkerton, 2008). Those infected more than 1 year ago infect another with an annual probability of 0.1 (Pinkerton, 2008). Each cohort of new infections dies at a rate that is specific to the years since their seroconversion (Fig. E.1).

At the end of each year, the number of infections is given by the number of infections at the end of the previous year, minus the deaths in the current year (from previous cohorts), plus the year's cohort of new infections.

In 1990, when prevention efforts began *en masse*, the annualized probability of transmission drops to 0.6 for individuals with an acute infection, and 0.04 for individuals with a chronic infection (>1 yrs since seroconversion). The "current year" (or year of observation) is 2005, which is the mean and median of our data years. The simulated epidemic is shown from 1970 to 2005 by the black line (labeled "Model") in Fig. E.2. Notice the dashed line at 1995; this serves as visual reference for comparing the post-1995 trend to the graphs of country prevalence in Fig. A.2.

When the simulation ends in 2005, there are 3.8m People Living with HIV (PLWH), yielding a prevalence of 16.5%. Of these, the number that were infected in each of the previous 18 years is calculated (note that none survives year 19 in the model). The CDF in Fig. E.3 shows that for PLWH, more than 80% were infected in the past 10 years. One might consider extending the time period of analysis to 18 years to capture 100% of infections, but notice that for those infected 11-18 years ago, only 20% are currently alive (see Fig. E.1).

[43] The first documented case of HIV in SSA was in 1959, suggesting that the virus mutated between 1910 and 1950 (Zhu et al, 1998; Worobey et al, 2008). Our survey countries' average population is 22 million.

Role of shock timing

New infections in each year are a function of new infections in previous years. Therefore, a shock that increases incidence in 1995 will indirectly increase incidence in each of the following 10 years (when observing in 2005). In contrast, a shock in 2000 will only affect the incidence of the following 5 years until 2005. Shocks further in the past should have greater impacts on current prevalence (conditional on the shock being within the past 10 years).

The red and blue lines in Fig. E.2 depict simulations of shocks to incidence that occur 8 years prior ("early") and 2 years prior ("late") to the end-point of 2005, respectively. In each shock, the annual transmission probabilities increase to 0.7 and 0.04 for acute and chronic infections, respectively, during the year of the shock and the following year. It is clear that the path of prevalence between 1996 and 2005 differs significantly in the two scenarios. Given a shock of the same size and duration, the earlier shock increases 2005 prevalence by 50% more than does the later shock (1.7 percentage points vs. 1.1 ppts).

This suggests that shocks occurring 6 to 10 years ago will exhibit a stronger impact on current prevalence than will shocks that have occurred in the past 5 years.

Figure E.1: Survival Following Seroconversion (East African population without ARV)

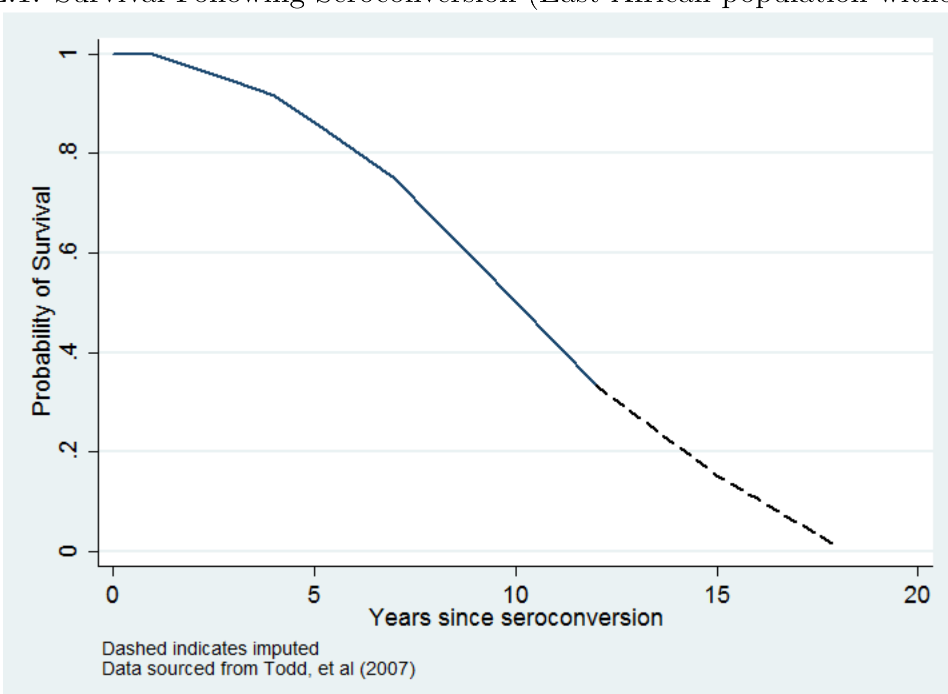


Figure E.2: Epidemic Curve

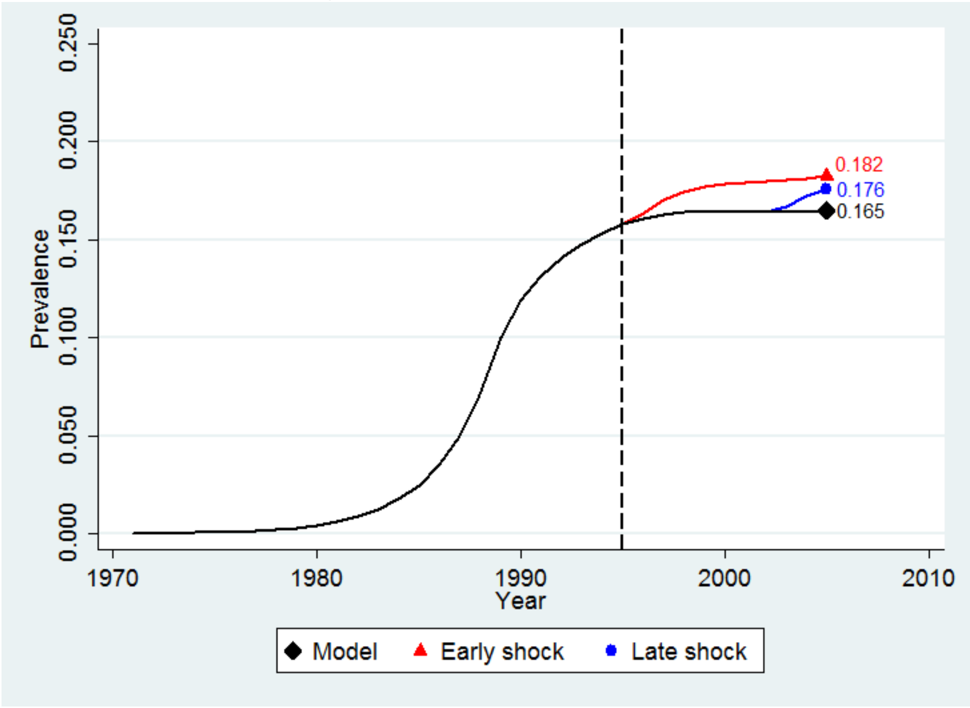
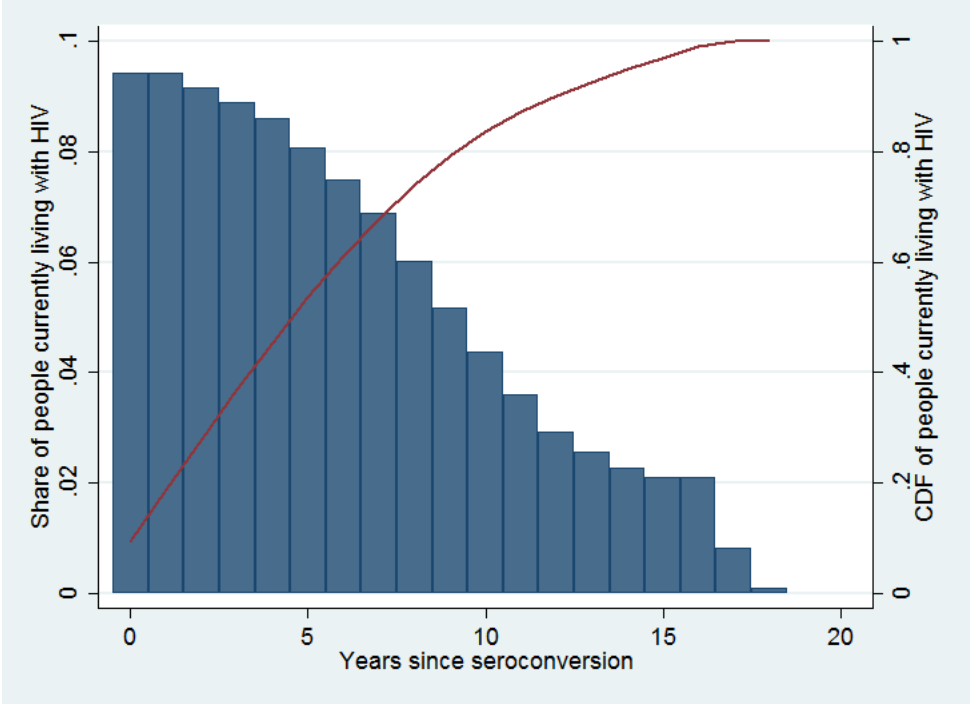


Figure E.3: People currently living with HIV, by year of seroconversion

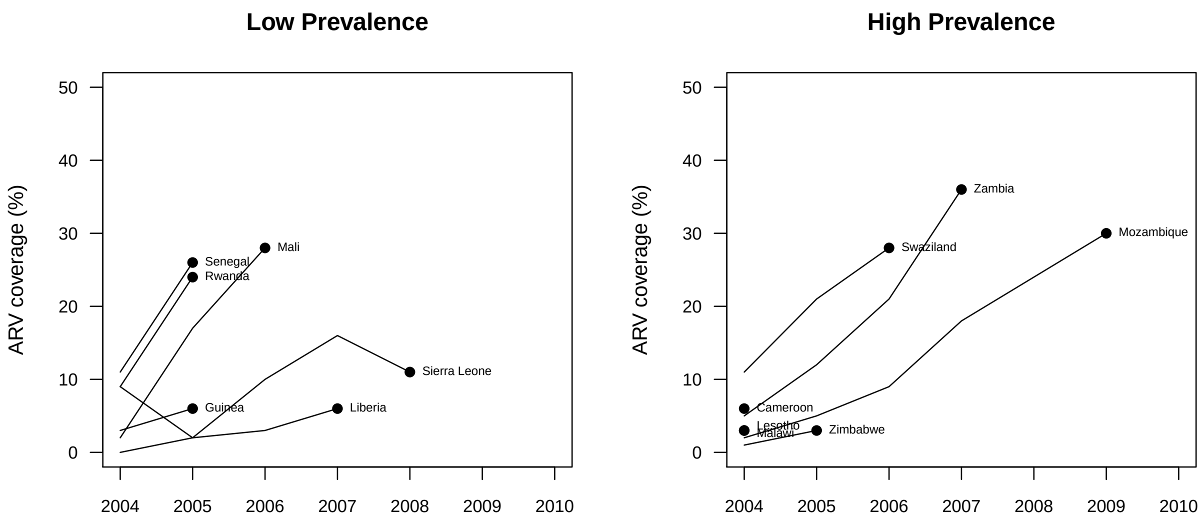


F The role of ARV Access

The number of people in sub-Saharan Africa being treated with ARVs has increased dramatically during the 2000 to 2009 period. This is due to both increased funding by PEPFAR and the Global Fund as well as substantial reductions in procurement costs (WHO, 2006; Friedman, 2012). Figure F.1 presents ARV coverage rates on a country level for when data is first available (2004) up until the year of the DHS survey. ARV coverage rates (the percentage of those receiving ARVs who are in need) are below 40% for all countries in our sample time frame. In rural areas, where we identify the effects that shocks have on HIV rates, ARV coverage rates are even lower. Access to ARVs is more limited for individuals living in rural areas due to both resource constraints and fewer trained medical professionals (van Dijk et al., 2009), as well as the greater distances that rural individuals may have to travel to access ARVs at clinics (Ojikutu, 2007). A number of country-specific studies substantiate this claim. In Kenya, ARVs were first targeted at urban areas and regions with high HIV prevalence (Friedman, 2012), longer travel times make it more difficult to access ARVs in rural areas in Zambia (van Dijk et al., 2009), and overall access to health services (including ARV access) is much more limited in rural than urban Mozambique (Groh et al., 2011).

One concern might be that shocks are correlated with ARV access which could lead to a different interpretation of our main results. For example, if ARVs are more readily available in areas with shocks then our results might be explained by more HIV+ individuals living longer in areas with shocks. Unfortunately sub-national data on ARV availability is not available for our sample. The DHS does ask "Have you heard of drugs to help infected people to live longer?" in select countries, which we use as a crude proxy for ARV accessibility. We find that our shock measure is not correlated with ARV awareness which suggests that ARV access and shocks are not correlated (Table F.1).

Figure F.1: ARV Coverage Rates (2004-2009)



ARV Coverage rates from the World Bank Development Indicators. Black dots indicate the year of the DHS survey used for each country.

Table F.1: ARV Awareness and Shocks

	(1) All	(2) Rural	(3) Rural& High Prevalence
Num. shocks past 10 yrs.	-.002	-.001	.007
	(.006)	(.006)	(.006)
Observations	89208	55336	43082
R^2	.304	.337	.101
Mean of Dep. Var	.658	.633	.819

Sample includes: Congo DR, Liberia, Malawi, Mozambique, Sierra Leone, Swaziland, Tanzania (2008), Zambia, and Zimbabwe. Column headers indicate sample employed. Specifications include controls for gender and age, rural/urban designation (where applicable), 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 Estimating Changes in Sexual Behavior

To investigate the plausibility of coping behaviors as the link between rainfall and HIV, we estimate the changes in underlying sexual behavior that would be needed to generate the observed increases in HIV. To back out the actual change in underlying sexual behavior as a result of shocks, we follow the methodology developed by Gong (2012) to estimate the change in sexual partnerships that would result in the 0.9 and 0.6 ppt increases in HIV infection reported in Table 3, Column 6.[44] As with all modeling exercises that involve sexual behavior and HIV infections, the estimates generated are sensitive to the parameter values. For these, we rely on values from the health and epidemiological peer-reviewed literature.[45]

For men, we estimate that each shock leads to an increase of .65 partners, which is about one-eighth of the mean number of lifetime partners for men; 95% CI (.61,.69) For women, the model suggests an additional 1.42 partners per shock would be needed to generate their .9 ppt change in HIV infection; 95% CI (1.30,1.53). In this sample, women report on average 2.2 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 under-report sexual behavior by about 50%. This suggests an average of 4.4 lifetime partners per woman. Annualizing based on the average woman in the sample (age 28 with sexual debut at 16), this averages to about one partner every three years.

[44] We focus on the change in number of sexual partnerships following Kaplan (1990), Kremer (1996), and Oster (2005).

[45] The model used to estimate changes in sexual behavior uses a simple transformation of the AVERT model (Rehle et al., 1998).

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

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. Further information about parameters employed is available from the authors on request.

H Country-level prevalence

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 function of number of shocks over previous 10 years for the 38 countries in Sub-Saharan Africa with HIV data.[46] Details on the AIDS data are provided in UNAIDS (2010). 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) levels 1990s	(2) levels 2000s	(3) change 1990s	(4) change 2000s
Num. shocks in past 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). Regressions include the 38 Sub-Saharan African countries with data in the UNAIDS database.

[46] The countries included in these regressions are: Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Central African Republic, Chad, Congo, Côte d'Ivoire, Eritrea, Gabon, Gambia, Ghana, Guinea, Guinea-Bissau, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mozambique, Namibia, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, South Africa, Swaziland, Togo, Uganda, United Republic of Tanzania, Zambia, and Zimbabwe.