INCOME SHOCKS AND HIV IN AFRICA*

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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' serostatus to information on local rainfall shocks, a large source of income variation for rural households. We estimate 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 variation in HIV prevalence across African countries, suggesting existing approaches to HIV prevention could be bolstered by helping households manage income risk better.

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 (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 article, 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 trade-offs 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 behaviour (Robinson and Yeh, 2011*b*; Baird *et al.*, 2012; Kohler and Thornton, 2012), 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]

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

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 2,000 individuals across 19 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 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 of work (Skoufias, 2003), or school-aged girls may respond by marrying at an earlier age to increase economic security (Jensen and Thornton, 2003), both behaviour that is associated with an increased risk of 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 (Swidler and Watkins, 2007; Dinkelman *et al.*, 2008; Robinson and Yeh, 2011*b*; LoPiccalo *et al.*, forthcoming). 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 behaviour through cash transfers (Baird *et al.*, 2011; Handa *et al.*, 2012; Kohler and Thornton, 2012; 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 SSA. 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 behavioural responses that are not only detrimental to an individual's or household's well-being 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 article is organised as follows. In Section 1 we present a simple conceptual framework to motivate our empirical approach. Section 2 presents the data and our empirical methods. Section 3 discusses our main results and robustness checks, and Section 4 seeks evidence of behavioural pathways. Section 5 explores how these effects scale up to the country level. Finally, Section 6 discusses policy implications and concludes.

1. Conceptual Framework

The goal of this article is to understand how economic conditions shape HIV risk. Our empirical approach examines how a plausibly exogenous source of income variation – exceptionally low rainfall realisations 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 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 $\partial HIV/\partial S$. 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:

$$\frac{\partial HIV}{\partial S} = \frac{\partial HIV}{\partial p}\frac{\partial p}{\partial z}\frac{\partial z}{\partial S} \tag{1}$$

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

1 $\partial HIV/\partial p$ 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 behaviour. The risk of HIV infection is increasing in risky sexual behaviour such as having multiple concurrent partners or unprotected sex ($\partial HIV/\partial p > 0$) (Stoneburner and Low-Beer, 2004; Epstein, 2007; Halperin and Epstein, 2008; Potts *et al.*, 2008). 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 behaviour and new infections than regions with low prevalence $\partial HIV/\partial p \partial \lambda > 0$.

2 $\partial p/\partial z$ 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 (Robinson and Yeh, 2011*b*; Baird *et al.*, 2012; Kohler and Thornton, 2012). 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 to be infected with HIV. In Section 4, we discuss several ways identified by the literature by which shortfalls in income might alter sexual behaviour, all of which suggest a negative relationship between an income deviation and sexual risk for at least some subset of the population (i.e. $\partial p/\partial z < 0$). Such mechanisms can broadly be considered coping behaviour in response to income shocks, and will be operative for different subsets of the population depending on the coping mechanism in question.

3 Finally, $\partial z/\partial S$ is the relationship between negative rainfall shocks and income shocks. As is frequently recognised in the literature, and as we demonstrate in online 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 ($\partial z_r/\partial S_r < \partial z_u/\partial S_u \leq 0$).

Because there is little disagreement in the literature on the signs of the first and third terms in (1), the overall sign of $\partial HIV/\partial S$ 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 (1).

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 behaviour, the effect of shocks will be amplified in places where there is more HIV to transmit.

2 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 $\partial p/\partial z$ will determine the overall sign of $\partial HIV/\partial S$. 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 (1) indicates that the overall relationship between HIV and shocks for these populations would be positive, $\partial HIV/\partial S > 0$.

2. Empirical Methods

2.1. 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 behaviour.[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 online Appendix Table A1. In all households selected for the men's sample, all surveyed men and women are asked to provide a finger-prick blood smear for 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 behaviour response to income shocks will have different implications

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

[7] Testing success rates for each survey are shown by sex in online Appendix Table A2. Refusal rates are 10%, on average. Mishra *et al.* (2006) examine test refusal rates in DHS testing, which are between 1% and 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 Table A3) so non-response does not induce bias in our results.

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

Table 1

DHS Survey Information

| | Country | Year | Individuals | Prevalence | | | |
				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	

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

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.

2.2. *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

[9] This categorisation follows UNAIDS (2010). Figure A2 shows that with the exception of Cameroon, the prevalence classifications for each country remains stable for the 10 years preceding the survey year. Our main results are unchanged when Cameroon is removed from our analysis.

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 livelihood (Davis *et al.*, 2010). As we show below, particularly negative rainfall realisations substantially depress agricultural productivity across the region.

Our weather data are derived from the 'UDel' (University of Delaware) data set, a 0.5 × 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 latitude/longitude 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 1,701 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 12 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 (Paxson, 1992; Fafchamps *et al.*, 1998; Rose, 1999; Jayachandran, 2006; Tiwari *et al.*, 2013), in percentage (Dercon, 2004), or in standard deviation units (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 1,701 grid cells, we fit the history of crop-year rainfall realisations 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

[10] 0.5 degrees is roughly 50 kilometres at the equator. The UDel data are popular in economic applications; recent papers include Dell *et al.* (2008); Jones and Olken (2010); 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 (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 (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 SD 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 shocks of interest, but short enough to capture relatively recent averages if long run means are changing (e.g. with climate change).

realisation 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 not available.[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 realisations (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 realisations 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 realisation below the 15% quantile of the local rainfall distribution – and show that our results are robust to other threshold choices in the neighbourhood 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 SSA, if
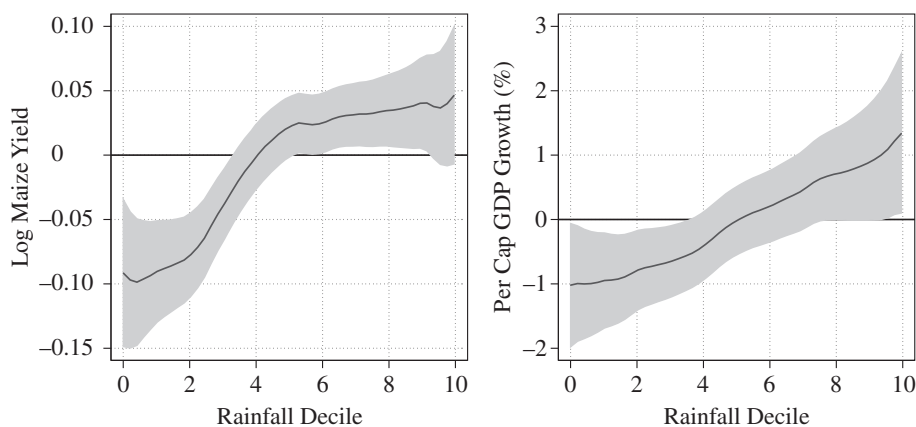

Fig. 1. *Effect of Rainfall Shocks on African Maize Yields (left panel) and Per Capita GDP Growth (right panel)*

Notes. 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.

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[14] Others in the literature have constructed binary shocks using thresholds such as 75% or less of the local mean (Shah and Steinberg, 2013) or 1–2 SD below the local mean (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 realisations for each country; we fit a separate gamma distribution to each country's time series; and within each country each year is assigned its corresponding percentile in its gamma distribution. Crop yield data are from FAO (2011), and data on real per capita economic growth are from the Penn World Tables 7.0 (Heston *et al.*, 2011).

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 realisations in that individual's region between 1997 and 2006 that fell below the 15% cut-off in the local rainfall distribution. We sum the shocks because acquiring HIV is irreversible – if a shock led to an HIV infection seven 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 of a very bad year and a very good year would be similar to the sum of two normal years. The mean and SD 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 cut-off 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

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	8,110	1,701

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

online 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 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 realisations 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.

2.3. Estimation

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

$$HIV_{ijk} = \alpha + \beta_1 S_j^t + \mathbf{x}_i' \boldsymbol{\delta} + \gamma r_j + \boldsymbol{\omega}_k + \varepsilon_{ijk}, \tag{2}$$

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, no one cluster is any more shock prone than another, i.e. $E(S_m^t) = E(S_n^t) \; \forall j = m, n$. 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 $\mathbf{x}_i$ contains characteristics of individual i that are not affected by shocks, specifically, gender and age. r_j indicates that cluster j is rural. The vector of survey fixed effects is $\boldsymbol{\omega}_k$ 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 SSA (see online Appendix A).

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[17] There are many reasons for including survey fixed-effects. Innumerable differences across countries exist that we cannot observe, including social norms of sexual behaviour, 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.

3. Results

3.1. *Main Results*

Table 3 shows estimates of (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 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 the hypothesis 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 = 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 behaviour 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 HIV (column 4; linear combination), and we reject equality across low and high prevalence countries with 95% confidence (column 4; shocks × 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 the hypothesis that the effect size is the same across genders (column 6; shock × male).

The magnitude of these effects is 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 10-year period. We also can attempt to estimate roughly an income elasticity with respect to HIV risk.[18] We estimate that each drought shock results in a 7–10% loss in annual income (see online Appendix B), which leads to an 11% increase in HIV infection risk. This result is similar to results from Robinson and Yeh (2011*b*) which show that a 3% loss in income leads to about an 8% increase in HIV risk.[19] Both results suggest that

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[18] In order to generate an actual income elasticity with respect to HIV infection risk, we would need: the percentage of income derived from agriculture for all individuals in our sample; individual level crop yields; and crop prices by DHS cluster. These data are required for each year of the past 10 years for everyone in our sample. Unfortunately, these data are not available.

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

Table 3
Effect of Shocks on HIV

	All	All	Rural	Rural	Rural high prevalence	Rural high prevalence
	(1)	(2)	(3)	(4)	(5)	(6)
No. of shocks past 10 years	0.003*	0.004**	0.003**	0.008**	0.008**	0.009**
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
Shocks × urban		−0.004				
		(0.002)				
Shocks × low prevalence countries				−0.008**		
				(0.003)		
Shocks × male						−0.003
						(0.003)
Interaction p-value		0.104		0.016		0.243
Linear combination		−0.000		−0.000		0.006**
		(0.002)		(0.001)		(0.003)
Observations	202,216	202,216	134,874	134,874	77,760	77,760
R^2	0.053	0.053	0.046	0.046	0.030	0.030
Mean of dependent variable	0.050	0.050	0.041	0.041	0.070	0.070

Notes. Column headers indicate sample employed. Specifications include controls for gender and age, rural/urban designation (where applicable), and survey fixed effects. Estimators 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 × urban (column 2), shocks × low prevalence countries (column 4), and shocks × 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 (No. of shocks past 10 years) + (shocks × urban), column (4) is (no. of shocks past 10 years) + (shocks × low prevalence Co.), and column (6) is (no. of shocks past 10 years) + (shocks × male). Significance levels: **5%, *10%.

better means of consumption smoothing can have implications for the HIV/AIDS epidemic.

3.2. *Robustness of Results*

In this sub 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, and omitted variables.

3.2.1. *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 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 into drought afflicted areas and, if these types are more likely to be HIV+, then 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 be likely to have the greatest response in terms of sexual behaviour and we do find a result that is slightly increased over our main specification.

3.2.2. *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 realisation below the 15th percentile of local realisations, the choice of the 15th percentile is somewhat arbitrary. We vary the cut-off for shock definition in increments of 1% 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 neighbourhood between the 10th and 20th percentile generate similar results, although they become less precisely estimated the further they are from the 15th percentile (see Table C1).[20]

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

Table 4

Robustness Checks

	No controls (1)	No weights (2)	Country-year fixed effects (3)	No hyper-endemic (4)	No visitors (5)	One survey per country (6)	Aged 15–50 at shock (7)	Alternative shock definitions	
								(8)	(9)
Panel (*a*): robustness to specifications and sample									
No. of shocks past 10 years	0.008**	0.005*	0.008**	0.008**	0.011***	0.009**	0.010**		
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)		
Shocks, defined by 1970–95 weather								0.006**	
								(0.003)	
1.5 SD shocks, past 10 years									0.015**
									(0.007)
Observations	77,760	77,760	77,760	68,287	53,596	65,326	36,812	77,760	77,760
R^2	0.020	0.068	0.030	0.025	0.035	0.035	0.033	0.030	0.030
Mean of dependent variable	0.070	0.110	0.070	0.068	0.063	0.078	0.158	0.070	0.070

	Mean (10)	Variance (11)	Skew (12)	All Moments (13)
Panel (*b*): robustness to controlling for moments of rainfall distribution				
No. of shocks past 10 years	0.009***	0.008**	0.008**	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	77,760	77,760	77,760	77,760
R^2	0.031	0.030	0.030	0.031
Mean of dependent variable	0.070	0.070	0.070	0.070

Notes. Rural sample from high-prevalence countries. All specifications include controls for gender, age and survey fixed effects, except as noted. Column 1 does not include the standard controls. Column (2) does not include weights. Column (3) employs country-year fixed effects, rather than survey fixed effects. Column (4) excludes the hyper-endemic countries (Swaziland and Lesotho). Column (5) excludes individuals who have lived in their current village for less than one year. Column (6) employs only the most recent survey from each country (excludes KE 2003 and TZ 2004). Column (7) includes only individuals who are aged 15+ at the time of the shocks. Columns (8)–(9) employ alternative definitions of a shock: column (8) employs only the years 1970–95 to create the historical distribution from which the 15th percentile is the shock definition, and column (9) defines a shock as 1.5 SD below the local mean. Columns (10) - (13) include additional controls as shown in column headers. Estimators are weighted to be representative of the 19 countries, except as noted. Robust standard errors are shown in parentheses clustered at the grid level. Significance levels: ***1%, **5%, *10%.
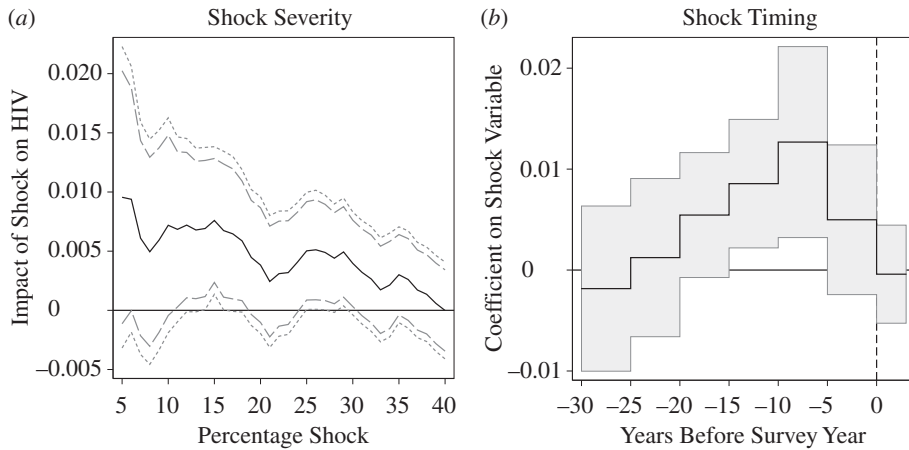
Fig. 2. *Effect of Rainfall Shocks on HIV, by (a) Severity and (b) Timing*
Notes. The black line represents the coefficient point estimates of the impact of a particular rainfall shock on HIV for the rural sample from high-prevalence countries, using (*a*) various definitions of 'shock' accumulated over the previous 10 years and (*b*) 15% shocks accumulated over different time periods, including placebo future shocks up to three 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.

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 realisations 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 10 years. We sum shocks in five-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 online Appendix E, this 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 online Appendix E that the effect of a shock will peak 6–10 years later.

To address concerns that shocks from the mid-1990s onwards (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 SD 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.

3.2.3. *Sample selection*

Droughts can also effect other types of behaviour 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 online 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 out-migration of 2% per shock, the estimated coefficient (0.7 percentage points) is nearly identical to our original estimate and still significant.

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)
No. of shocks past 10 years	0.008*** (0.003)	0.007** (0.003)	0.006** (0.003)	0.005* (0.003)	0.004* (0.003)	0.004 (0.002)
Observations	77,760	81,792	84,191	86,523	88,775	91,330
R^2	0.030	0.022	0.023	0.023	0.024	0.024

Notes. 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 online Appendix D). All specifications include controls for gender, age and survey fixed effects. Estimates are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level. Significance levels: ***1%, **5%, *10%.

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[21] In Section 4 we find no evidence of differential migration rates (due to shocks) between clusters close and far from urban centres.

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 online Appendix D for details). Thus, the assumption of a 4% loss per shock is an extreme upper bound. When we replace a 4% population loss per shock, our effect remains positive (0.4 percentage points) and significant at the 10% level. Though 4% is the upper bound, we nonetheless report estimates 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.

3.2.4. *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 end in crop-years 2007–8, we are only able to examine shocks up to four years in the future.[22] We find no relationship between HIV rates and shocks one to four 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).

Finally, during the 2000s, there was increasing access to 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 online 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.

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[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 onwards, 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 minimised by summing shocks over a 10 year period. However, when looking just at shocks in the future one year, the rainfall measure in certain clusters might not perfectly capture rainfall one year ahead, making this particular estimate somewhat noisier.

Table 6
Placebo Tests

Dependent variable	HIV				Wealth quintile			Years of education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
No. of shocks in future 1 year	0.006 (0.006)				0.005 (0.110)			−0.152 (0.232)		
No. of shocks in future 2 years		−0.002 (0.007)				0.113 (0.095)			−0.134 (0.194)	
No. of shocks in future 3 years			−0.004 (0.007)				0.104 (0.096)			−0.146 (0.194)
No. of shocks in future 4 years				−0.006 (0.006)						
Observations	49,523	43,881	26,059	12,434	49,523	43,881	26,059	49,489	43,861	26,039
R^2	0.044	0.040	0.025	0.010	0.031	0.033	0.029	0.119	0.118	0.102

Notes. 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 end in 2007–8 crop years. All specifications include controls for gender, age and survey fixed effects. Estimators are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

4. Exploring Pathways

4.1. Behavioural Pathways

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

4.1.1. Risky sexual behaviour

The use of self-reported sexual behaviour is subject to a few caveats. There is a large body of evidence that suggests self-reported sexual behaviour 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 behaviour during the 12 months prior to the survey. It is not immediately clear which time window of shocks should be considered to impact sexual behaviour 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 behaviour if those relationships (or behaviour) are persistent.[25] For this reason, we present the impact on recent sexual behaviour of shocks within the past 10 years, shocks within the past five years and having a shock that affected income over the past 12 months. Given these caveats, we interpret results on self-reported sexual behaviour with caution.

The outcome variables we examine are whether in the past 12 months the respondent:

    (*i*) has been sexually active;
    (*ii*) had multiple partners; or
    (*iii*) had non-spouse partner(s).[26]

Table 7 shows results of estimation of (2), separately by gender, with these self-reported types of sexual behaviour 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

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[24] Additional caveats are that data that are available for sexual behaviour do not capture all aspects of risky behaviour 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 behaviour 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 (2011*b*) study started as sex-workers on average 9.7 years prior to the study.

[26] In these 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.

Table 7

Exploring Behaviour: Increasing Risky Sexual Behaviour

	Women			Men		
	Sexually active (1)	Multiple partners (2)	Non-spouse partner (3)	Sexually active (4)	Multiple partners (5)	Non-spouse partner (6)
No. of shocks past 10 years	0.012**	0.003*	0.007*	0.008	0.018***	0.015**
	(0.005)	(0.002)	(0.004)	(0.006)	(0.004)	(0.006)
R^2	0.060	0.011	0.018	0.223	0.034	0.051
No. of shocks past 5 years	0.021***	0.004*	0.013**	0.008	0.016**	0.021**
	(0.007)	(0.002)	(0.005)	(0.009)	(0.006)	(0.009)
R^2	0.060	0.011	0.018	0.223	0.033	0.050
Y/N shock affecting past 12 months	−0.027**	0.003	0.023**	0.013	−0.007	0.035**
	(0.012)	(0.004)	(0.010)	(0.012)	(0.012)	(0.017)
R^2	0.059	0.011	0.018	0.222	0.032	0.051
Observations	43,145	43,119	43,147	34,607	34,563	34,613
Mean of dependent variable	0.759	0.024	0.120	0.738	0.154	0.269

Notes. Rural sample from high-prevalence countries. Dependent variables are types of sexual behaviour 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. Estimators are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level. Significance levels: ***1%, **5%, *10%.

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 behaviour 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 behaviour – and, in particular, leading to riskier sexual behaviour – and that these behavioural changes are what likely link rainfall shocks to HIV. In the remainder of this section, we seek evidence for which coping behaviour may be primarily responsible for this relationship.

4.1.2. Temporary migration

One response to drought-induced income shocks is to migrate from rural to urban areas in search of employment (Ellis, 2000; Skoufias, 2003). Migration is associated with greater levels of risky sexual activity and higher rates of HIV (Brockerhoff and Biddlecom, 1999; Lurie *et al.*, 2003). 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

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[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 2.

Table 8

Exploring Behaviour: Temporary Migration

	Main HIV result for this sub-sample		Away for month + in past year		Total times away in past year	
	Men (1)	Women (2)	Men (3)	Women (4)	Men (5)	Women (6)
No. of shocks past 10 years	0.007* (0.003)	0.015*** (0.004)				
Y/N shock in past year			−0.010 (0.024)	0.018 (0.018)	−0.421*** (0.149)	−0.229** (0.115)
Near urban × shock in past year			−0.018 (0.031)	−0.008 (0.027)	−0.419 (0.282)	−0.569*** (0.188)
Near urban			−0.008 (0.011)	−0.009 (0.012)	−0.148 (0.199)	0.162* (0.084)
Observations	26,096	26,299	26,133	26,300	23,802	22,990
R^2	0.038	0.041	0.004	0.016	0.025	0.117
Mean of dependent variable	0.064	0.094	0.151	0.130	2.064	0.990

Notes. Rural sample from high-prevalence countries. The 'Near urban' variable indicates whether a given cluster is within 100 kilometres 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 is given in columns (1) and (2). All specifications include controls for age and survey fixed effects. Estimators are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level. Significance levels: ***1%, **5%, *10%.

shocks induce temporary migration, then $\partial p/\partial z < 0$ 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 than one month. If temporary migration is a primary coping behaviour 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, column (5) is presented in columns 1 and 2, for men and women respectively, for these sub-samples.

Columns (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.

[28] Near to urban is defined as being within 100 kilometres of an urban centre 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 two or three years, rather than 12 months; results not shown.

4.1.3. *Dropping-out and early marriage*

A second set of coping behaviour that may affect sexual risk is changes in schooling and marriage behaviour. 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, 2013). 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, 2011), 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 (Jensen and Thornton, 2003; Hoogeveen *et al.*, 2011). If income shocks induce early drop-out and early marriage, which result in earlier sexual activity, then $\partial p / \partial z < 0$. 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 time of the survey.[30] As mean age at marriage for women in this sample

Table 9

Exploring Behaviour: Early School Drop-out and Marriage

| Dependent variable | Ever married | | Age at marriage | | Years of education | | HIV Status | |
| | | | | | | | Aged 25+ | Aged 30+ |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of shocks, aged 13–18	−0.000 (0.005)		0.000 (0.046)		−0.010 (0.056)			
No. of shocks, aged 15–20		−0.003 (0.005)		0.006 (0.057)		−0.005 (0.054)		
No. of shocks past 10 years							0.011* (0.006)	0.016** (0.006)
Observations	24,679	22,679	23,770	23,005	27,429	25,242	12,280	3,845
R^2	0.125	0.065	0.033	0.023	0.223	0.222	0.031	0.022
Mean dependent variable	0.881	0.923	18.1	18.3	5.5	5.4	0.091	0.067

Notes. 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. Estimators are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level. Significance levels: **5%, *10%.

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

is 18, women are considered at risk for early marriage when aged 13–18 (column 1) or aged 15–20 (column 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 youths 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 13–18 (and 15–20) 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 behaviour linking rainfall to HIV.

4.1.4. *Transactional sex*

A third coping mechanism is engaging in transactional sex. Transactional sex is thought to be common in SSA and is broadly defined to include both prostitution as well as transfers within casual relationships and long-term partnerships (Hunter, 2002; Leclerc-Madlala, 2002; Luke, 2006; Maganja *et al.*, 2007; Swidler and Watkins, 2007; Béné and Merten, 2008).[32] Women may respond to income shocks either by taking on additional partnerships or engaging in more frequent or riskier sexual activity (i.e. unprotected sex) to increase transfers. Both types of behaviour have been documented throughout SSA, 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, 2011*b*; Dupas and Robinson, 2012). 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; Côté *et al.*, 2004; Dunkle *et al.*, 2004) and a growing empirical literature suggests that economic conditions affect risky

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[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.

sexual behaviour and the market for transactional sex (Baird *et al.*, 2010; Robinson and Yeh, 2011*a*; Kohler and Thornton, 2012).

We cannot directly examine changes in this behaviour, 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 (Robinson and Yeh, 2011*b*; Baird *et al.*, 2012; Kohler and Thornton, 2012), 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 behavioural 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 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 behaviour 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, while women in the non-agricultural sector appear relatively unaffected by shocks.[35] These results make 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

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[33] Whether a man has paid for sex in the past year is only queried in four surveys from high prevalence countries. This probably 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 making 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.

[35] We cannot reject the null that shocks have the same effect on women in and outside of agriculture (p = 0.252).

Table 10

Exploring Behaviour: Impact on HIV by Exposure to Drought-induced Income Shock

	Men		Women	
	(1)	(2)	(3)	(4)
No. of shocks past 10 years	0.004	0.002	0.009**	0.011***
	(0.003)	(0.003)	(0.004)	(0.004)
Non-Agriculture × shocks		0.007		−0.007
		(0.005)		(0.006)
Non-Agriculture employment		0.049***		0.116***
		(0.016)		(0.023)
p-value on interaction		0.167		0.252
Impact of shocks on Non-Agriculture (linear combination)		0.009*		0.004
		(0.004)		(0.006)
Observations	37,585	37,585	37,487	37,487
R^2	0.033	0.039	0.032	0.039
Mean of dependent variable (Ag)		0.056		0.077
Mean of dependent variable (Non-Agriculture)		0.096		0.148

Notes. Sample from high-prevalence countries. All specifications include controls for age and urban, and survey × employment type fixed effects to allow the correlation between HIV and occupation to vary by country. Note that Non-Agriculture indicator alone is indicative only for the country excluded in fixed effects (Mozambique). Estimators are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level. Significance levels: ***1%, **5%, *10%.

significant at the 10% level, while the effects of shocks for agricultural men is nearly zero. While we cannot reject the null that shocks have the same effect for men in and outside of agriculture (p = 0.167), 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 quantity demanded. 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 affect 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 (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 that the supply side responds more to aggregate shocks than the demand side does (Wilson, 2011; Dupas and Robinson, 2012).

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[36] We also find that the magnitude of increases in HIV is consistent with increases in transactional sex. Robinson and Yeh (2011*a*) 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 online Appendix G.

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

4.2. Non-behavioural Pathways

Each type of behaviour 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 increased 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 epidemiological 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 three coping behaviour described above.

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

5. 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 SSA? 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 is 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 percentage 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-prevalence; this is true both in the 1990s (upper plot) when the epidemic was growing rapidly, as well as in the 2000s, when the epidemic had 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 online 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.

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[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).

[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.
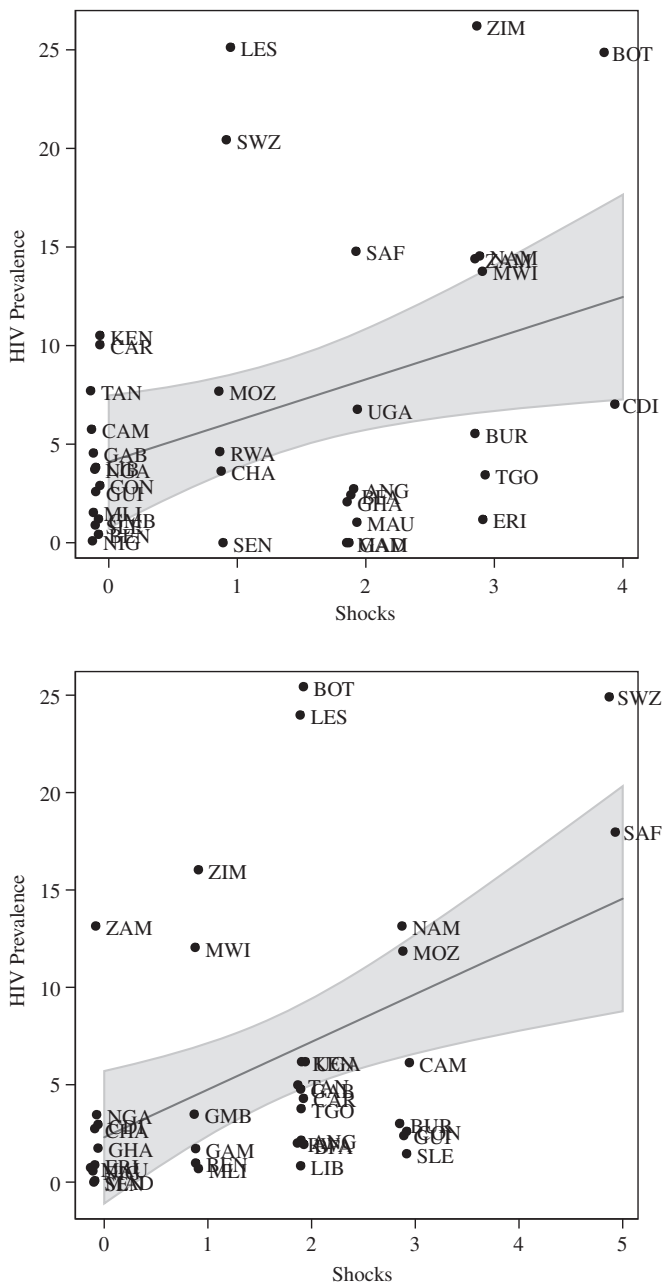
Fig. 3. *Country-level HIV Prevalence & Shocks*
Notes. The top panel presents results for HIV prevalence in 1999 (*y*-axis) and accumulated shocks over the previous decade (*x*-axis). The bottom panel presents results for HIV prevalence in 2008 and accumulated shocks since 2000. HIV data are from UNAIDS (2010). Dark lines are linear least squares fits, with grey areas representing the 95% confidence interval. Data are jittered to make country labels more legible.

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 SSA lack social safety nets and depend heavily on rain-fed agriculture, recurring droughts may play an important and prominent role in explaining why the AIDS epidemic has disproportionately affected SSA.

6. 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 SSA and that economic conditions play some role in sexual behaviour in these countries.

Our article 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 SSA. While there are several possible pathways linking shocks to HIV, the available evidence is inconsistent with all the potential 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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Additional Supporting Information may be found in the online version of this article:

Appendix A. DHS Data.
Appendix B. Weather Data and Impact of Drought on Crop Yields.
Appendix C. Shock Definition.
Appendix D. Estimating Sample Selection Due to Out-migration.
Appendix E. Considering Shock Timing.
Appendix F. The Role of ARV Access.
Appendix G. Estimating Changes in Sexual Behaviour.
Appendix H. Country-level Prevalence.
Data S1.

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