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IFPRI Discussion Paper 01146

December 2011

Income Shocks and HIV in Sub-Saharan Africa

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PARTNERS AND CONTRIBUTORS

IFPRI gratefully acknowledges the generous unrestricted funding from Australia, Canada, China, Denmark, Finland, France, Germany, India, Ireland, Italy, Japan, the Netherlands, Norway, the Philippines, South Africa, Sweden, Switzerland, the United Kingdom, the United States, and the World Bank.

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ABSTRACT

Poverty is commonly cited as a key driver of the HIV/AIDS epidemic, yet little causal evidence exists linking economic conditions to actual disease outcomes. Using data on more than 200,000 individuals across 19 Sub-Saharan African countries, we present evidence that negative income shocks can lead to substantial increases in HIV prevalence, particularly for women in rural areas. Building on recent work showing that income shortfalls can induce some women to engage in higher-risk sex, we match data on individuals' HIV status from the Demographic and Health Surveys to data on recent variation in local rainfall, a primary (and exogenous) source of variation in income for rural households in Africa. We find that infection rates for women (men) in HIV-endemic rural areas increase significantly by 14 percent (11 percent) for every drought event experienced in the previous 10 years. Further analysis suggests that women most affected by the shocks (that is, those engaged in agriculture) are driving the women's results; these women are partnering with men least affected (those employed outside agriculture). Our findings suggest a role for formal insurance and social safety nets in tackling the HIV/AIDS epidemic.

Keywords: income shocks, HIV/AIDS, Sub-Saharan Africa

ACKNOWLEDGMENTS

We thank Jeremy Magruder, Ted Miguel, Betty Sadoulet, and seminar participants at the University of California, Berkeley, and Middlebury College for useful comments. All errors remain our own.

1. INTRODUCTION

Poverty has long been thought a key contributor to the HIV/AIDS epidemic in Sub-Saharan Africa (SSA), but untangling the causal relationship between economic conditions and HIV/AIDS has proved challenging (Beegle and de Walque 2009). Cross-sectional evidence suggests that the relationship between wealth and HIV infection in SSA differs widely by country and is often nonmonotonic within countries (see, for example, Fortson 2008). Recent work on negative income shocks and HIV-related sexual behavior is perhaps more suggestive, with Robinson and Yeh (2011b) finding that women in western Kenya who engage in transactional sex increase their supply of riskier, better-compensated sex when faced with negative health shocks, and Dinkelman, Lam, and Leibbrandt (2008) showing similar declines in the condom use of girls living in South African households exposed to negative economic shocks. Understanding whether such behavioral responses are representative of how broader populations might respond to changes in economic conditions and how such responses might affect actual disease outcomes remain important and unanswered questions.

In this paper, we explore the relationship between community-level economic shocks and HIV prevalence across a wide swath of rural SSA. We model the influence of such shocks on risky sexual behavior and HIV prevalence, building on existing evidence that behavioral adjustment on either the extensive margin (more partners) or intensive margin (riskiness or frequency of acts) could increase HIV infection (see, for example, Stoneburner and Low-Beer 2004). To quantify the effect of local shocks on HIV outcomes, we employ the latest rounds of Demographic and Health Surveys (DHS), which contain data on actual HIV status for individuals, and we match these data to location-specific measures of accumulated drought events over the preceding 10 years.[1] We use drought events as proxies for income shocks both because the DHS lack income or expenditure data and because local drought events represent plausible exogenous variation in economic outcomes for the majority of Africans who depend on rainfed agriculture for their livelihoods.[2] Our empirical strategy thus compares the HIV status of individuals exposed to an anomalously high number of droughts in recent years to the status of individuals randomly exposed to fewer droughts over the same period within the same country.

We find large effects of exposure to drought events (shocks) on HIV infection, particularly for individuals in rural areas, where there is a large, generalized HIV/AIDS epidemic (greater than 5 percent prevalence). For women in these areas, each shock in the past 10 years has led to a statistically significant 1.2 percentage point (ppt) increase in the likelihood of infection. This is a large effect: With HIV prevalence for this group of 8.3 percent, each rainfall shock amounts to a 14 percent increase in HIV risk. For rural men, we find smaller effects in absolute percentages, though similar changes in HIV risk; each shock leads to a significant 0.6 ppt increase in HIV rates, which is an 11 percent increase in HIV risk given a group prevalence of 5.6 percent. We estimate very small and insignificant point estimates for the effects of shocks on HIV rates for both men and women in urban areas, consistent with the relative insensitivity of urban incomes to weather-related shocks. Our findings are robust to variations in the definition of drought, to alternate sets of controls, and to varying time windows over which droughts can be accumulated. Reassuringly, placebo tests using future shocks to explain current HIV prevalence show no effect.

We explore changes in the underlying market for risky sexual behavior as a response to these weather-driven income shocks. Our main results suggest that shocks increase the equilibrium quantity of risky sex in the market, which could have occurred through outward shifts in either supply or demand (or both). For instance, women may increase their supply of transactional sex during a shock, or men may increase their demand for risky sex as the opportunity cost of time declines during a local economic downturn. We use available occupational data in the DHS to attempt to distinguish these two stories. We

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

[2] Other studies using rainfall variation in SSA as an exogenous shock to income include Miguel (2005), who examined the relationship between income and witch killings, and Hoddinott and Kinsey (2001), who examined the relationship between income and child health outcomes.

find that shocks disproportionately affect the HIV status of women working in agriculture and men working outside of agriculture, which is most consistent with an outward shift in female labor supply: The women whose income is most sensitive to these shocks are affected the most, whereas the men whose opportunity cost of time should be most affected show little change in their HIV status. Instead, the men whose incomes are least affected by drought show the largest HIV response, suggesting a movement along the demand curve for those men with enough income to participate in the market.

Finally, we show that data on self-reported sexual behavior further support the female labor supply story, and the data are inconsistent with a number of alternative interpretations of our main results—that is, that income shocks cause young women to leave school and become sexually active earlier, that drought-related migration might have led to a selected survey sample, and that temporary migration in response to negative shocks is driving the HIV increases we observe.

Our findings contribute to a number of bodies of literature. First, we build directly on recent work that explored how the supply of risky sex responds to individual and aggregate-level economic shocks. In an innovative set of studies, Robinson and Yeh (2011a, 2011b) monitored the sexual behavior of unmarried women in Busia, Kenya, and found that these women engage in transactional sex as a response to current and future shocks, such as an illness in their household. Dinkelman, Lam, and Leibbrandt (2008) found that girls in South African households experiencing an income shock are less likely to use condoms. Women may also respond to positive income shocks; Kohler and Thornton (2010) presented evidence that women in Malawi in the week after being rewarded a conditional cash transfer decreased their supply of risky sex. Other studies have examined how aggregate-level shocks affect the supply of risky sex. Using the Busia, Kenya, sample described above, Dupas and Robinson (2009) found that women increased their likelihood of engaging in better-compensated unprotected sex after the disruptions of the 2007 elections in Kenya. Wilson (2011) showed that the copper mining boom in Zambia generated higher levels of economic prosperity in towns near the copper mines and provided evidence that this positive income shock led to reductions in transactional sex in those same towns.

Our primary contribution is that our findings help generalize the results described above to much broader patterns of sexual behavior across highly endemic countries in SSA. We provide evidence that suggests that income variation due to weather-related shocks in agricultural productivity may be a contributing factor to the HIV/AIDS epidemic in SSA. In addition, our study is one of the first to quantify the effect of economic shocks on actual HIV outcomes, rather than on self-reports of sexual behavior. Not only are HIV infections the primary outcome of interest for policymakers, but also biological markers of risky sex are not subject to the social desirability bias of self-reports on sexual behavior (Padian et al. 2008; Cleland et al. 2004). In addition, self-reported measures of sexual behavior may not detect changes on the intensive margin, which may be a primary driver of new HIV infections (Beegle and de Walque 2009). Given that heterosexual sex is the primary mode of HIV transmission in this context, we believe our work to be a powerful confirmation of the existing microliterature on this topic.[3]

In addition, we contribute to the literature that applies economic reasoning to issues surrounding the HIV/AIDS epidemic in SSA. Work by Oster (forthcoming) has found a relationship between export levels and increases in HIV incidence. Such findings are most likely explained by an increase in the movement of high-risk individuals—namely, truckers, who are key players in an export-driven economy. Fortson (2009), Kalemli-Ozcan and Turan (2010), and Young (2005) considered how fertility decisions respond to existing HIV prevalence. Dupas (2011) found that teenage girls in Kenya respond to information about the age gradient of HIV by changing their sexual behavior on the intensive margin. Oster (2007) suggested that the muted response of sexual behavior in response to high HIV prevalence may be a result of significant competing mortality risks. In contrast, our work considers behavior as a driver of the epidemic rather than a response to it.

[3] Other means of HIV transmission are using needles infected with HIV (for example, intravenous drug use or vaccines) and transfusion from contaminated blood supplies. Although we are unable to rule out these channels, it appears unlikely that economic shocks would lead to increases in intravenous drug use or contaminated blood transfusions. Further, in most of our study areas, both intravenous drug use and blood transfusions are extremely rare.

Finally, we contribute to a broader body of work on the consequences of shocks on health and livelihood outcomes. A host of papers show that when saving is difficult and insurance is incomplete, negative shocks can have seriously detrimental effects on longer-run livelihood outcomes. In the context of the weather-related agricultural shocks we study here, past work has highlighted the effect of these shocks on early life nutrition, as well as their subsequent effect on long-run health and educational outcomes (Alderman, Hoddinott, and Kinsey 2006; Maccini and Yang 2009). Similarly, and using data very close to ours, recent work by Kudamatsu, Persson, and Stromberg (2010) showed the widespread effect of drought on infant mortality across Africa. Our work highlights a previously unrecognized—and highly consequential—mechanism by which some African households cope with uninsured shocks. As such, it adds further impetus to the growing effort aimed at increasing access to risk-management tools in the developing world. Our results suggest that the return to such efforts could be much higher than previously thought.

The rest of the paper is organized as follows. In Section 2, we present a conceptual framework that predicts the effects to be greatest in rural areas where HIV prevalence is high. The data are presented in Section 3, and the main empirical findings, in Section 4. In Section 4, we also show our main result is robust to variations in the definition of drought, to alternate sets of controls, and to varying time windows over which droughts can be accumulated. In Section 5, we present evidence that the most likely channel for our results is a shift out of the supply curve for risky sex, and we rule out alternative explanations. Section 6 concludes and discusses policy implications.

2. CONCEPTUAL FRAMEWORK

Why might rainfall shocks affect HIV outcomes? To fix ideas, consider the following relationship:

$$\frac{\partial HIV}{\partial s} = \frac{\partial HIV}{\partial p} \frac{\partial p}{\partial y} \frac{\partial y}{\partial s} \tag{1}$$

where an individual's probability of HIV infection (HIV) is related to the intensity of rainfall shocks (ss) via deviations from normal income (y) and risky sexual behavior (s). We discuss each of these relationships in turn.

The relationship $\partial HIV/_{\partial p}$ describes the response of HIV infection to risky sexual behavior (p). We can think of p as measuring either the intensive or extensive margin of risky sex, such as lack of condom use or number of sexual partners. There is substantial evidence suggesting that this relationship is positive—that is, one's risk of HIV infection increases in the number of partners (Halperin and Epstein 2008; Potts et al. 2008; Stoneburner and Low-Beer 2004; Epstein 2007). This relationship will also depend on the prevalence of HIV in an area (λ). Regions with higher HIV prevalence will have a stronger relationship between risky behaviors and new infections than regions with low prevalence $\left(\partial HIV/_{\partial p \partial \lambda} > 0\right)$. In situations where HIV prevalence is near zero, the relationship between p and HIV will be much more muted.

The relation $\partial y/_{\partial s}$ describes how rainfall shocks translate into income shocks. In rural areas, where most income is generated from rainfed agriculture, we expect $\partial y/_{\partial s} > 0$, or greater deviations from average rainfall lead to lower crop yields and reduced income. In urban areas, where agriculture plays a smaller role in the local economy, we expect rainfall to have little or no effect on income.

The relationship $\partial p/_{\partial y}$ describes the impact of a deviation from mean income on an individual's level (or amount) of risky sexual behavior. Existing evidence from sociology, anthropology, and economics suggests that $\partial p/_{\partial y} > 0$ for women. In other words, women increase their supply of risky sex in response to income shocks. Kohler and Thornton (2010) offered evidence that men's demand for transactional sex is generally increasing in income, which may suggest that it would decrease in the presence of a negative income shock, meaning $\partial p/_{\partial y} < 0$ for men. However, there is no evidence on this behavior of which we are aware. In aggregate, the sign of $\partial p/_{\partial y}$ is ambiguous and, as such, is an empirical question.

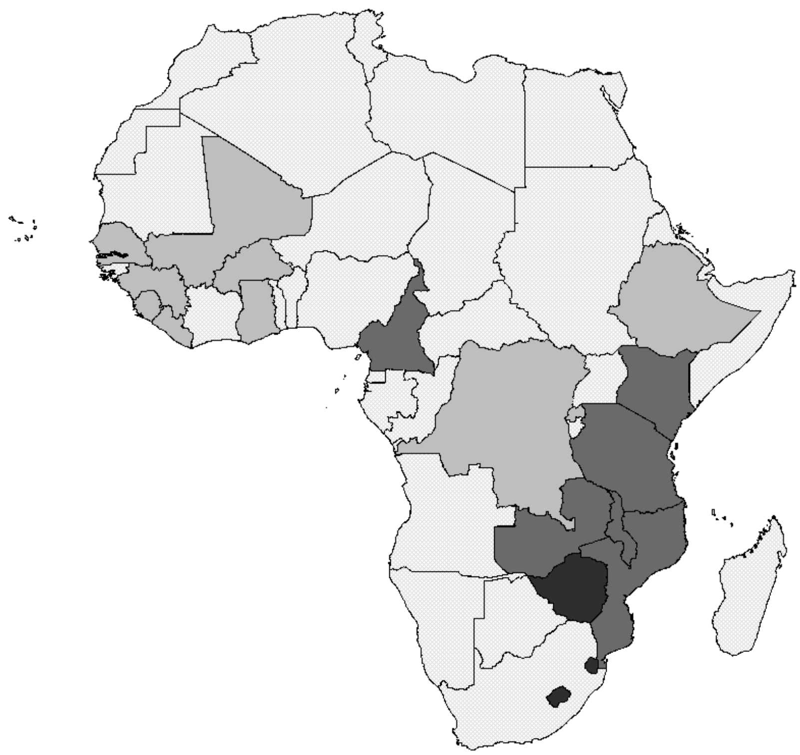
Given that $\partial HIV/_{\partial p}$ and $\partial y/_{\partial s}$ are both positive, the relationship we estimate, $\partial HIV/_{\partial s}$, will have the same sign as the aggregate $\partial p/_{\partial y}$. In Section 5, we discuss whether the aggregate response is dominated by men's or women's behavior.

3. DATA

Demographic and Health Surveys

The data on individuals are taken from 21 Demographic and Health Surveys (DHS) conducted in 19 different Sub-Saharan countries (Figure 3.1). Of the existing DHS surveys available in early 2011, we employ all those that (1) include results from individual-level HIV tests and (2) include longitude and latitude information, allowing us to map households to data on shocks.[4] For two countries (Kenya and Tanzania), two survey rounds matched these criteria; however, these surveys are separate cross-sections, and creation of panel data at the individual or cluster level is not possible. Nonetheless, for Kenya and Tanzania, both rounds are included in the analysis as entirely separate surveys.

Figure 3.1—Countries included in the study



Source: Authors' creation.
Note: Included countries are shown in gray. Shades correspond to HIV prevalence as measured in the DHS, with darker gray corresponding to higher prevalence.

Each survey 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, households

[4] 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 Geographic Information System–marked clusters.

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

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

selected for the men's survey are either all sampled households or a random half (or third) of households within each cluster. In all households selected for the men's sample, all surveyed men and women are asked to provide a finger-prick blood smear for serotesting.[7] By employing cluster-specific inverse-probability sampling weights, the HIV prevalence rates estimated with these data are representative at the national level.

Table 3.1 gives the list of included surveys, along with basic survey information. The compiled data contain more than 8,000 clusters. On average, there are 25 surveyed individuals per cluster, and 90 percent of clusters contain between 10 and 50 surveyed individuals. In total, more than 200,000 individuals are in the pooled data. Table 3.1 also shows HIV prevalence rates for each survey. Overall, women's prevalence is 9.2 percent, and men's is 6.2 percent. However, these numbers mask a range that varies widely, from over 30 percent prevalence for women in Swaziland to less than 1 percent prevalence in Senegal. Given that the sexual behavior response to economic shocks may have different implications depending on HIV risk, we classify countries into two HIV prevalence groups: low-prevalence countries, with less than 5 percent prevalence, and high-prevalence countries, with more than 5 percent prevalence.

Table 3.1—DHS survey information

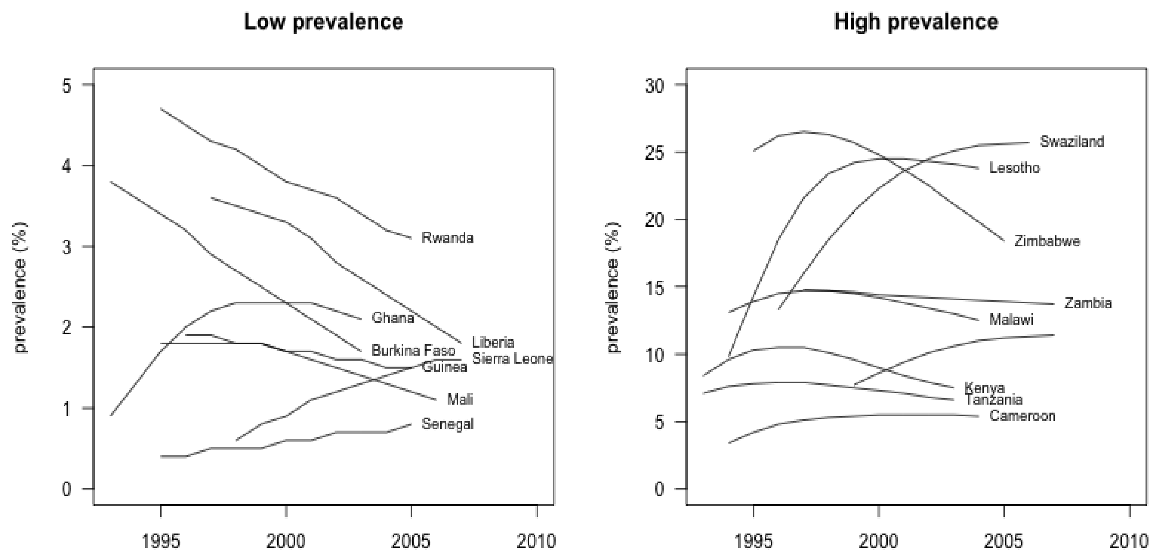
					HIV Prevalence			
	Country	Year	Clusters	Individuals	Female	Male	Overall	Category
1	Swaziland	2007	271	8,186	31.10%	19.70%	25.90%	High
2	Lesotho	2004	381	5,254	26.40%	18.90%	23.20%	High
3	Zambia	2007	398	26,098	21.10%	14.80%	18.10%	High
4	Zimbabwe	2006	319	10,874	16.10%	12.30%	14.20%	High
5	Malawi	2004	521	5,268	13.30%	10.20%	11.80%	High
6	Mozambique	2009	270	10,305	12.70%	9.00%	11.10%	High
7	Tanzania	2008	345	10,743	7.70%	6.30%	7.00%	High
8	Kenya	2003	399	6,188	8.70%	4.60%	6.70%	High
9	Kenya	2009	397	6,906	8.00%	4.60%	6.40%	High
10	Tanzania	2004	466	15,044	6.60%	4.60%	5.70%	High
11	Cameroon	2004	466	10,195	6.60%	3.90%	5.30%	High
12	Rwanda	2005	460	10,391	3.60%	2.20%	3.00%	Low
13	Ghana	2003	412	9,554	2.70%	1.60%	2.20%	Low
14	Burkina Faso	2003	399	7,530	1.80%	1.90%	1.90%	Low
15	Liberia	2007	291	11,688	1.90%	1.20%	1.60%	Low
16	Guinea	2005	291	6,767	1.90%	1.10%	1.50%	Low
17	Sierra Leone	2008	350	6,475	1.70%	1.20%	1.50%	Low
18	Ethiopia	2005	529	11,049	1.90%	0.90%	1.40%	Low
19	Mali	2006	405	8,629	1.50%	1.10%	1.30%	Low
20	Congo DR	2007	293	8,936	1.60%	0.90%	1.30%	Low
21	Senegal	2005	368	7,716	0.90%	0.40%	0.70%	Low
	Total		8031	203,796	9.20%	6.20%	7.80%	

Source: Authors' calculations based on DHS data.
Note: Prevalence estimates are weighted to be representative at the national level.

[7] Testing success rates for each survey are shown by sex in Table A.2.

Historical trends in HIV prevalence for the countries in our study are shown in Figure 3.2. For each country, we take the 10 years preceding the survey year and plot yearly estimates of HIV prevalence from UNAIDS (2010).[8] For a majority of countries, HIV prevalence had been declining over the 10 years prior to the DHS survey. With the exception of Cameroon, the high- and low-prevalence classifications for each country had remained stable for the 10 years preceding the survey year.

Figure 3.2—Pre-survey 10-year HIV trends for low- and high-prevalence countries



Source: UNAIDS (2010).

The DHS data also provide information on individual characteristics, which we employ as controls in our analysis. Level of education is categorized as “none,” “some primary,” “completed primary,” or “beyond primary.” For nearly all individuals over age 25, this level will have been determined prior to the period included in our analysis.

Weather Data

Weather data are from the University of Delaware (UDel) dataset, a 0.5 x 0.5–degree gridded monthly temperature and precipitation dataset (Matsuura and Willmott 2009).[9] These gridded data are based on interpolated weather station data and have global coverage over land areas from 1900–2008. Using the latitude and longitude data in the DHS, we match each DHS cluster to the nearest cell in the gridded weather data. Because Geographic Information System (GIS) data in the DHS are recorded at the cluster level, all individuals within a given cluster are assigned the same weather.[10] Our DHS data match to 1,701 distinct grid cells in the UDel data.

To capture the seasonality of agriculture, we construct cluster-level estimates of “crop year” rainfall, where *crop year* is defined as the 12months following planting for the main growing season in a region. Estimates of planting dates are derived from Sacks et al. (2010); planting of staple cereal crops for the primary growing season typically occurs in the boreal (northern hemisphere) spring across most of western and central Africa, and in the boreal autumn across most of southern Africa. Annual crop year estimates are generated by summing monthly rainfall across these 12 crop year months at a given location.

[8] 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 10 years.

[9] 0.5 degrees is roughly 50 kilometers at the equator.

[10] Similarly, clusters within the same grid also have the same observations of weather.

Measuring Weather Shocks

A drought is generally defined as a prolonged period with below-average rainfall. How far below average rainfall must be in order to affect crop yields is not clearly defined, however. Furthermore, a simple deviation-from-average measure might be problematic if the distribution of potential rainfall realizations differs across regions. For instance, if the total amount of precipitation in a village in a given crop year is a random variable drawn from an underlying distribution, then there is no *a priori* reason that the distribution should be normal.[11] If, instead, the amount exhibits a long, positive tail, an occurrence of rainfall that is below average—that is, less than the mean—could be quite common and therefore not result in an income shock at all.

We wish to capture occurrences of *unusually* low rainfall—that is, given the underlying distribution of rainfall for a village, what level occurs with only 15 percent probability? We estimate a gamma distribution of historical rainfall for each grid, based on annual rainfall observations from 1970 to 2008.[12] Recovering the shape and scale parameters for each of the 1,701 distributions, we calculate the 15 percent quantile of each. Crop year rainfall realizations below this cut-off are designated negative shocks, and our main independent variable is the number of these shocks that 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 between 1997 and 2006 that fell below the 15 percent cut-off. We choose 10 years because the median survival time at infection with HIV in Sub-Saharan Africa, if untreated, is 9.8 years (Morgan et al. 2002). Sensitivity tests repeat the same exercise for the 5, 10, and 20 percent quantiles of the gamma distribution, as well as for various periods over which the shocks are allowed to accumulate.

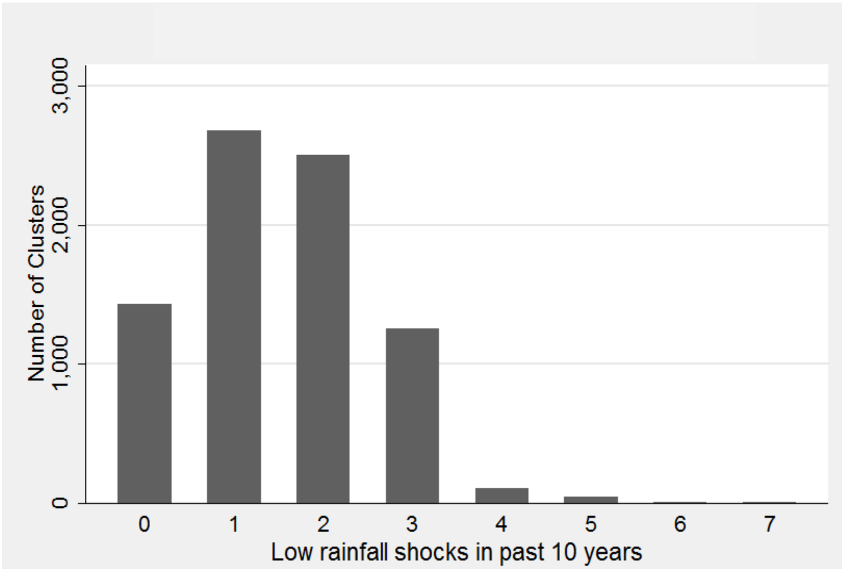
Figure 3.3 shows the distribution of accumulated negative rainfall shocks across our DHS clusters over the 10 years preceding the survey year for that cluster. Although we cannot directly show the importance of these shocks for household income (as noted above, the DHS do not include income or consumption measures), aggregate data suggest that these shocks are economically important. Table 3.2 shows the impact of rainfall dropping below the 10th or 15th percentile on (log) country-level maize yields across Sub-Saharan African countries, based on panel regressions using country and year fixed effects. Maize is the most widely grown crop in Africa, and annual maize yields are strongly affected by precipitation—for instance, yields are about 12 percent lower in a year with rainfall at or below the 15th percentile and 16 percent lower in a year with rainfall below the 10th percentile. Results are robust to including temperature shocks in the regression (available on request). With 60–80 percent of rural African incomes derived directly from agriculture, these productivity impacts likely represent significant shocks to household incomes (Davis et al. 2010).[13]

[11] In all cases, it will be censored at zero.

[12] The gamma distribution is selected for its considerable flexibility in both shape and scale. The period 1970–2008 was chosen to be long enough to be relatively insensitive to the recent shocks of interest, but short enough to capture relatively recent averages if long-run means are changing (for example, with climate change).

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

Figure 3.3—Distribution of clusters by rainfall shocks



Source: Authors' calculations.
Note: Low rainfall is defined as occurring with less than 15 percent probability, based on cluster-specific historical rainfall distribution.

Table 3.2—Impact of precipitation shocks on yields

	(1) Log maize yield	(2) Log maize yield
10th percentile shock	–0.161***	
	(0.025)	
15th percentile shock		–0.122***
		(0.023)
Constant	–0.067	–0.066
	(0.052)	(0.052)
Observations	1888	1888
R squared	0.317	0.316
Probability of shock	0.083	0.146

Sources: Yield data from FAO (2010); weather data from Matsuura and Willmott (2009).
Notes: Standard errors are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
The dependent variable is the log of country-level maize yield. Regressions cover years 1961–2008, include country fixed effects, year fixed effects, and a constant; and are weighted by country average maize area.

Because year-to-year changes in rainfall in a given location are typically assumed to be as good as randomly assigned, our definition of *shocks* should help us avoid many of the omitted variables problems that generally plague cross-sectional studies of the relationship between income and HIV. To help confirm that our measure of rainfall shocks is plausibly exogenous and not correlated with other moments of the rainfall distribution, we regress the number of rainfall shocks in the past 10 years on the mean, variance, and skewness of each cluster's rainfall distribution. The worry is that if rainfall shocks are somehow correlated with mean rainfall, then it is possible that rainfall shocks are also correlated with unobserved factors that also affect HIV (for example, if wetter areas are somehow wealthier). Table 3.3 presents the results. Although the mean and variance of the rainfall distribution are significantly correlated with the number of rainfall shocks in the simple bivariate estimations (columns 1 and 3), including survey fixed effects weakens this relationship considerably. In all specifications with survey fixed effects, the correlations between the number of rainfall shocks and the mean, variance, and skewness of the rainfall distribution are not significant. In other words, when we estimate across clusters *within a given survey*, we find evidence that rainfall shocks are orthogonal to all three moments of the distribution. For this reason, we include survey fixed effects in our main specifications to ensure that the accumulation of rainfall shocks is effectively random.[14]

Table 3.3—Rainfall shocks and overall variability

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

Source: Authors' calculations based on DHS and UDel data.
Notes: Robust standard errors are shown in parentheses and clustered on the grid level. The dependent variable is the number of 15 percent rainfall shocks in the past 10 years. SurveyFE = survey fixed effect.

[14] There are a host of other reasons for including survey fixed effects. Innumerable differences across countries exist that we cannot observe, including social norms on 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 times within the same country, thus motivating a within-survey estimation.

4. EMPIRICAL ANALYSIS

Estimation

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

$$HIV_{ijk} = \alpha + \beta_1 S_j^t + C'^{\zeta}_j + X'^{\delta}_i + \omega_k + \varepsilon_{ijk}, \quad (2)$$

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

The vector C_j contains characteristics of the cluster jj, such as location type (rural or urban) and historical average rainfall. The vector X_i contains characteristics of individual ii, including gender and age. The survey fixed effect is ω_k, and ε_{ijk} is a mean-zero error term. We estimate linear probability models, allowing for a correlation of error terms across individuals in the same weather grid by clustering standard errors at the grid level. Survey-specific sampling weights are used to make the results representative of the population of interest, which are rural individuals living in Sub-Saharan Africa (see Appendix A).

Results

Table 4.1 shows estimations of equation (2) for the full sample and the rural and urban samples. The overall effect of rainfall shocks on HIV rates using the full sample (column 1) is positive (.003) and statistically significant. We find strong effects of rainfall shocks on HIV rates in rural areas (column 2) but not in urban areas (column 3). This result is expected, because rainfall shocks affect agricultural income, which is concentrated in rural areas. In rural areas, we estimate that each shock leads to an increase in HIV prevalence that is 9.8 percent of the mean.

Table 4.1—Effect of shocks on HIV

	(1) All	(2) Rural	(3) Urban
15 PCT shock (10 years)	.003**	.004**	–.001
	(.001)	(.002)	(.002)
Male	–.023***	–.016***	–.037***
	(.002)	(.002)	(.003)
Age	.002***	.001***	.003***
	(.000)	(.000)	(.000)
Observations	202216	134874	67342
R^2	.053	.046	.063
Mean dependent variable	.050	.041	.070

Source: Authors’ calculations based on DHS and UDel data.
Note: All specifications include controls for gender, age, mean rainfall, rural/urban designation, 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.

As described in Section 2, increases in risky behavior would yield little change in HIV infection rates if existing HIV prevalence were very low. There needs to be both an increase in risky sex and a pool of potential partners who are HIV positive for HIV rates to increase. To capture these potential differential effects by prevalence, we split our sample of individuals into those living in low- and high-prevalence countries (see Table 3.1) and estimate the effect of shocks on HIV rates (Table 4.2). Countries with low prevalence have an approximately zero effect (Table 4.2, column 1). In countries with high prevalence, there is a large effect of nearly one full percentage point, where overall prevalence is 7 percent (column 2). This effect is different from zero at the 99 percent confidence level. We disaggregate this effect by gender and find that shocks increase the probability of infection by 1.2 percentage points (ppt) for women and 0.6 ppt for men (columns 3 and 4). For women, this represents a 14 percent increase in HIV risk, given an underlying prevalence of 8.3 percent. For men, existing prevalence is 5.7 percent, so the effect of shock on men represents an increase of 11 percent.

Table 4.2—Effect of shocks on HIV in rural areas by country prevalence

			High Prevalence	
	Low Prevalence	**High Prevalence**	**Women**	**Men**
	(1)	**(2)**	**(3)**	**(4)**
15 PCT shock (10 years)	–.000	.009***	.012***	.006**
	(.001)	(.003)	(.004)	(.003)
Observations	57114	77760	43147	34613
R2	.003	.031	.029	.032
Mean dependent variable	.010	.070	.083	.057

Source: Authors' calculations based on DHS and UDel data.
Notes: Rural sample. All specifications include controls for gender, age, mean rainfall, and survey fixed effects. Estimations are weighed to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Overall, our main results are consistent with the following: (1) rainfall shocks primarily affect the incomes of those living in rural areas, (2) females respond to income shocks by engaging in transactional sex, (3) this behavioral response leads to higher HIV infection rates for both females and males, and (4) this shock-related rise in infection rates is absent in countries with very low prevalence, where increased amounts of risky sex would not be expected to significantly affect infection rates.

Robustness Checks

We conduct a variety of robustness checks on our main results for rural areas in high-prevalence countries. We first vary the estimating equation by changing the individual and cluster-level controls. We also vary both the time window in which rain shocks occur and the definition of a rainfall shock. Overall, our results are robust to a variety of alternative specifications.

We first show that our main results are not sensitive to variations in respondent age across different surveys (see Table A.1). Employing a consistent age group across all surveys (age 15 to 49) changes our estimated effect in rural areas of high-prevalence countries from .009 to .010 and does not affect precision (Table 4.3, column 1). We also estimate the effects of rainfall shocks on HIV without individual and cluster-level controls (column 2), without sampling weights (column 3), and replacing the survey fixed effects with country and year fixed effects (column 4); our results remain consistent throughout these alternative specifications. Finally, we remove two hyperendemic countries (an HIV prevalence of greater than 20 percent), Lesotho and Swaziland, and find very similar results.

Table 4.3—Robustness checks

	(1) Age 15–49	(2) No Controls	(3) No Weights	(4) CoYrFE	(5) No Hyperendemic	(6) w/Temp
15 PCT shock (10 years)	.010***	.008**	.005*	.009***	.009***	.010***
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
15 PCT temp. shock						-.008***
						(.003)
Observations	75570	77760	77760	77760	68287	77760
R^2	.034	.020	.068	.031	.026	.032
Mean dependent variable	.070	.070	.110	.070	.068	.070

Source: Authors' calculations based on DHS and UDel data.
Notes: Rural sample from high-prevalence countries. All specifications include controls for gender, age, mean rainfall, and survey fixed effects, except as noted. Estimations are weighed to be representative of the 19 countries, except as noted. Robust standard errors are shown in parentheses clustered at the grid level. CoYrFE = country and year fixed effects.

We also show that our results do not depend on using a 10-year window for the timing of the rainfall shocks. We vary this window to include shocks from the preceding 5, 7, or 13 years, rather than 10 (Table 4.4; columns 1–3). In each instance, the point estimate remains stable and statistically significant.

Table 4.4—Robustness to length of shock window and placebo test

	(1)	(2)	(3)	(4)	(5)
15 PCT shock past 5 years	.007*				
	(.004)				
15 PCT shock past 7 years		.009***			
		(.004)			
15 PCT shock past 13 years			.009***		
			(.003)		
15 PCT shock 2 years ahead				.003	
				(.007)	
15 PCT shock 3 years ahead					.001
					(.006)
Observations	77760	77760	77760	67436	49523
R^2	.030	.031	.031	.039	.045

Source: Authors' calculations based on DHS and UDel data.
Notes: Rural sample from high-prevalence countries. All specifications include controls for gender, age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Another concern is that shocks could somehow be proxying for other time-invariant cluster characteristics that are also associated with HIV risk, causing us to conflate the effect of shocks with some other unobservable.[15] We test for potential confounders using rainfall shocks that occur *after* the survey year of each sample. Given that the DHS surveys were conducted between 2003 and 2009 and our weather data end in 2008, we are not able to use similar time windows (that is, 10 years) that we use for our main

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

analysis. Instead, we create two time windows: (1) all shocks two years after the survey year, and (2) all shocks three years after the survey year (Table 4.4; columns 4–5). This placebo test suggests that our presurvey shocks measure is unlikely to be proxying for other factors that also affect HIV risk.

We also check that our estimates are not sensitive to the definition of a shock. Defining shocks as annual rainfall with a probability of less than 5, 10, or 20 percent produces estimates consistent with the 15 percent definition. Employing a simpler definition of shocks as rainfall that is 1.5 standard deviations or more below the grid's long-term mean provides similar estimates as well (results shown in Table B.2)

5. DISCUSSION: PATHWAYS

The results shown here clearly suggest that HIV infections are increasing in rainfall shocks. Based on the framework presented in Section 2, this implies that the response of risky sexual behavior to negative income shocks is positive as well ($^{\partial p}/_{\partial y} > 0$). In this section, we present evidence that increasing risky sex to offset income shortfalls is indeed the pathway by which rainfall shocks affect HIV infections.

We first check for supporting evidence that shocks do affect sexual behavior by examining self-reported data. Next, we endeavor to determine whether the hypothesized increases in risky or transactional sex result from an increase in supply by women or an increase in men's demand. Secondary analysis suggests that the effects result solely from an increase in women's supply. Finally, we consider possible links between rainfall shocks and HIV other than increased risky sex that could be explaining our results. Rejecting the ability of each of these to generate the results shown here, we conclude that indeed the pathway connecting rainfall and HIV is as proposed.

Self-Reported Sexual Behavior

To lend further credence to a behavior response to income shocks, we examine self-reports of sexual behavior. There are several caveats to this analysis. First, a large body of evidence suggests that self-reported sexual behavior is biased (Cleland et al. 2004) due to social desirability bias. Second, the data that are available for sexual behavior do not capture all aspects of risky behavior that could lead to HIV infection. For example, the type of sexual partner you have (for example, a commercial sex worker or an individual with multiple partners) will affect the likelihood of HIV infection, and these data are not available.[16] Finally, several of the measures we have of sexual behavior are for recent behavior (12 months prior to the survey). Thus, unless sexual behavior is persistent, income shocks in the past may not affect more recent sexual behavior.[17] To deal with this, we differentiate between the number of recent shocks (that is, within the past five years) and the number of earlier shocks (5–10 years ago). Given these caveats, we proceed to examine whether shocks affect self-reported sexual behavior.

Table 5.1 shows results of estimating equation (2) with alternate self-reported sexual behaviors as the dependent variable. We first look at the number of lifetime sexual partners an individual reports (column 1) and find that each shock in the past 10 years increases the average number of partners per person by 0.21. Although this is only a 3 percent increase, the effect is significant at the 1 percent level, despite the reduced sample size, as this question was not asked in all surveys. This serves as a strong indicator that shocks increase sexual partnerships.

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

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

Table 5.1—Effect on shocks on self-reported behavior

	(1) Long-term Partners	(2) Sexually Active	(3) Multiple Partners	(4) Condom Use
15 PCT shock (10 years)	.209***			
	(.077)			
Recent 15 PCT shocks		.020*	.023***	–.027
		(.011)	(.006)	(.035)
Earlier 15 PCT shocks		.015	017***	.005
		(.009)	(.005)	(.030)
Observations	50298	77753	56352	8525
R^2	.144	.128	.098	.060
Mean dependent var	6.000	.738	.208	.482

Source: Authors' calculations based on DHS and UDel data.
Notes: Rural sample from high-prevalence countries. All specifications include controls for gender, age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

In columns 2–4 of Table 5.1, we examine indicators of sexual behavior in the 12 months prior to the survey. We find that recent shocks lead to a small increase (0.02) in the probability of being sexually active. Further, conditional on being sexually active, shocks lead to a higher likelihood of engaging in multiple partnerships within the year (column 3). For each shock in the past five years, the chance of having multiple partners this year is increased by more than 10 percent (significant at the 1 percent level). Interestingly, shocks more than five years ago seem to have persistent effects, increasing the chance of multiple partnerships each year by 8 percent. We find no significant impact of shocks on condom use with a nonspouse partner, conditional on having a nonspouse partner. This seems to suggest that any increases in p, as defined in Section 2, arise from increased partnerships rather than from increased risk per encounter.

Increased Supply or Increased Demand?

Existing literature suggests that some African women respond to income shortfalls by increasing their supply of transactional sex, either for income smoothing or simply for survival. The clear increase in HIV infections resulting from rainfall shocks implies that shocks are associated with an increase in the quantity of risky sex occurring.

To formalize ideas, consider a market for transactional sex where the quantity traded (SS) represents time spent engaged in transactions.[18] Both women and men have a limited amount of time available, which we normalize to unity. Women divide their time among working for a wage w_1, leisure time ll, and supplying SS in order to maximize their utility function

$$\max_{C,H} U^W = U\left(C, H(S), l\right)$$

$$s.t.\ C \leq w_1(1 - l - S) + p_s S,$$

where $H(S)$ is health status as a function of risky sexual behavior, C is aggregate consumption, and p_s is the market price for sex.[19] We assume that U^W is increasing in $C, H,$ and ll at a decreasing rate, and that HH is decreasing in SS at a decreasing rate.

[18] This could be extended so that S may also represent an index of riskiness of the average encounter. Gertler, Shah, and Bertozzi (2005) showed that riskier sex (for example, without a condom) brings a price premium in the commercial sex market, suggesting that riskiness itself is a marketable good.
[19] We set the price of C as the numeraire.

Men divide their time among working for wage w_2, engaging in sex, S, and all other leisure time, l. Men will demand S to maximize their utility function

$$\max_{C,S} U^M = U(C,S,l)$$

$$s.t.\ C + p_s S \leq w_2(1 - l - S),$$

where men derive utility from general consumption (C), from sex, and from other leisure.

Market equilibrium requires that

$$Q_s = S^*_W = S^*_M = Q_D.$$

Our empirical results suggest that the equilibrium quantity is increasing in rainfall shocks, suggesting that shocks cause an outward shift in the supply curve, an outward shift in demand, or both.

We note that

$$\frac{\partial Q_S}{\partial w_1}.$$

In other words, women will increase supply whenever their wages decrease. However, for men, the demand for sex ambiguously responds to changes in wage.[20] Decreased men's wages will lower the budget constraint, reducing the quantity of S demanded at a given price. However, decreased wages also reduce the opportunity cost of time spent in leisure and engaging in sex. If men substitute time away from working and toward leisure activities, this could potentially increase demand. Whether the income effect or substitution effect will dominate is essentially an empirical question, which we consider below.

Although we do not observe wages in our data, we do observe occupation, and we would expect different occupations to be differentially affected by negative rainfall shocks. In particular, it seems likely that individuals employed in agriculture would differentially suffer reduced wages as a result of rainfall shocks. If increased transactional sex is being driven by an increase in supply, we would expect women's results to differ across occupational sector (greater for agricultural women), and we would expect results to be consistent across both sectors for men, reflecting a reduced price for sex. On the other hand, if the change is driven by an increase in demand, we would expect men's effects to be greatest among agriculturalists, and women's effects to be consistent across both sectors, reflecting an increase in price. We therefore examine the effects of shocks disaggregated by sex and occupation.

A practical concern in our data is that we are able to classify individuals by their employment type at the time of the survey but not at the time of the shock.[21] We therefore assume that employment type is roughly persistent—individuals in agriculture at the time of the survey are more likely to have been in agriculture at the time of the shock. In order to avoid bias, we check whether the experience of shocks affects occupational choice and find that it does not (results available on request).[22] Nevertheless, we interpret the following results with caution.

Table 5.2 shows differential effects of shocks on HIV by gender and occupation. We find results consistent with a small decrease in demand that is dominated by a significantly increased supply, yielding an increased equilibrium quantity. Women's effects are strongly concentrated among agriculturalists; the effect for other women is not statistically different from zero. This result supports increased supply and rejects increased demand.

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[20] A specific functional form of utility may have unambiguous predictions for this response. However, we are hesitant to impose such strong assumptions.

[21] An advantage of classification by employment type is that we are able to differentiate women in urban areas who are employed in agriculture versus nonagriculture. For this analysis, we therefore do not condition on the rural sample.

[22] As an additional robustness check, we limit the sample to those who were over 30 years of age at the time of the survey, as these types might have been less likely to have switched their type of employment, and our results are similar.

Table 5.2—Effect shocks by employment type

	Women		Men	
	In Agriculture	Nonagriculture	In Agriculture	Nonagriculture
	(1)	(2)	(3)	(4)
15 PCT shock (10 years)	.013***	.006	.005	.016**
	(.004)	(.006)	(.003)	(.007)
Observations	20586	16901	17145	8652
R^2	.020	.036	.021	.044
Mean dependent variable	.077	.148	.056	.096

Source: Authors' calculations based on DHS and UDel data.
Notes: Sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Given an increase in supply, we assume that the price of sex decreases and that the *quantity* demanded will increase for all men, even if demand does not shift out (that is, a move along the demand curve). However, what we find is that quantity demanded (as proxied by HIV infections) significantly increases only for men working *outside* agriculture. The increase for these men nearly mirrors that of agricultural women, suggesting that women seeking to compensate for income shortfalls partnered with the men least affected by the shock. We cannot statistically distinguish the effect for agricultural men from zero, suggesting that, given lower prices, the demand curve must have shifted inward in response to decreases in wage.

Although we cannot reject the null that shocks have the same effect for women (men) in agriculture versus non-agriculture, these results provide some evidence that our main findings on HIV are driven by a shift out in the supply curve rather than a shift in demand. When a drought occurs, women in agriculture are most affected and increase their supply of risky sex to compensate for the income shortfall; and it is the men in the nonagricultural sector, whose incomes are relatively less affected by drought, who appear to be partnering with these women.

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

To test for this, we include an indicator for whether an individual completed primary school in our main specifications and interact it with the measure of shocks. We find that shocks have a significantly different effect on women who are primary school graduates (Table 5.3; column 1).[23] Women without primary school are 1.8 percentage points more likely to be infected with HIV per shock, whereas shocks have no statistically significant effect on women who are primary school graduates.[24] Because educational decisions might have been affected by these shocks, we also limit the sample to women age older than 30 at the time of the survey, who would likely have completed their education before our shocks of interest occur. We find nearly identical significant effects for this sample (Table 5.3; column 3), despite the reduction in sample size. We interpret this as further evidence in support of an outward shift in female supply as the driver of these results.

[23] We also include controls for household assets in these specifications to differentiate the effects of education versus wealth.
[24] The linear combination of Shocks + (Shocks x Primary School) has p = 0.46.

Table 5.3—Women's education mitigates the effect of shocks

	All		Age 30+	
	In Agriculture (1)	Nonagriculture (2)	In Agriculture (3)	Nonagriculture (4)
15 PCT shock (10 years)	.018***	.013	.019***	.010
	(.004)	(.009)	(.006)	(.012)
Primary school	.024**	.005	.038**	–.009
	(.011)	(.018)	(.015)	(.027)
Shock X primary school	–.014**	–.012	–.016**	–.015
	(.006)	(.009)	(.008)	(.014)
Observations	20585	16901	10657	8217
R^2	.024	.039	.028	.055
Mean dependent variable	.077	.148	.088	.178

Source: Authors' calculations based on DHS and UDel data.
Notes: Female sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Other Pathways?

In the remainder of this section, we explore other explanations for a link between rainfall shocks and HIV, in particular focusing on the results for women.

Early School Leaving

One possible channel is that income shocks cause rural women to leave school prematurely, which may lead them to be sexually active at an earlier age (Baird et al. 2009). In this case, absent any transactional sex, shocks would still be correlated with HIV infection. If this were occurring, we would expect the effects to be concentrated in the women who were of schooling age when the shocks occurred.

In Table 5.4, we divide the sample into four categories based on age at the time of survey, and we re-estimate the main equation for each. Women age 15–20 at the survey ranged in age from 5 to 19 over the preceding 10 years—prime schooling age (column 1). In contrast, women age 32–49 at the survey were aged 22 or older when any of the shocks occurred, an age past which these women are unlikely to be in school. We find that women well past schooling age at the time of the shocks (columns 3 and 4) exhibit statistically significant results similar to our primary results. The estimated effect on women age 5 to 19 at the time is smaller and is not statistically different from zero. Given these estimates, it is unlikely that school leaving is the primary driver of our results. As a final check, we regress self-reported age of first sexual activity (sexual debut) on our shock variable and find no significant relationship (column 5).

Table 5.4—Are school-age females driving results?

	(1) Age 15–20	(2) Age 21–31	(3) Age 32–41	(4) Age 42+	(5) Debut
15 PCT shock (10 years)	0.002	0.017***	0.012**	0.015**	–0.030
	(0.003)	(0.005)	(0.006)	(0.006)	(0.047)
Observations	11440	16156	9604	5850	36997
R^2	0.020	0.036	0.042	0.027	0.036

Source: Authors' calculations based on DHS and UDel data.
Notes: Female rural sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

Permanent Migration

Another potential alternative explanation is the possibility of selective permanent outmigration from rural areas in the event of droughts. If certain types respond to shocks by permanently migrating, and if these types are more likely to be HIV negative, then the remaining population would be disproportionately HIV positive. This too could cause a correlation between shocks and HIV, absent any sexual behavior response. To test whether selective migration can account for our results, we simulate the replacement of the assumed migrants into the sample.

In adding such *ghost* individuals to our data, two questions arise:

(1) How many people leave as a result of a shock? and

(2) What was the HIV prevalence of those who left?

Several calculations are performed in order to answer question (1), which are detailed in Appendix C. The calculations suggest that an assumption of 3 percent population loss per shock approaches reality, with 5 percent as an extreme upper bound. The second question is to what degree the individuals who migrated were less likely to be HIV positive than those who stayed. In order to be as conservative as possible, we assume that every migrant was HIV negative. We then create enough ghost women to increase the female population in each cluster according to the schedule shown in Table C.1 for the 3 percent and 5 percent assumptions.

Table 5.5 reproduces our primary result: In high-prevalence countries, rural women's probability of infection increases by 1.2 percentage points per shock. The second column shows the same estimation based on data that include the additional ghost migrants under the 3 percent assumption. We see that although the point estimate is mechanically reduced, the phenomenon cannot fully explain the positive and statistically significant results we estimate.

Table 5.5—Accounting for potential permanent migration

	(1) Observed	(2) 3%	(3) 5%
15 PCT shock 10 years	.012***	.009**	.007**
	(.004)	(.003)	(.003)
Observations	43147	46537	49313
R^2	.025	.027	.028

Source: Authors' calculations based on DHS and UDel data.
Notes: Female rural sample from high-prevalence countries. All specifications include controls for age, mean rainfall, and survey fixed effects. Columns (2) and (3) include additional observations to account for outmigration (see text). Estimates are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

In the third column, we repeat the entire exercise under the upper-bound 5 percent assumption and find that, even accounting for massive outmigration (up to 30 percent in some clusters), we can still reject that the effect is zero. It does not appear that sample selection due to outmigration cannot explain the effects we find.

Temporary Migration

Although permanent migration as a result of shocks appears unlikely to explain our main results, it could still be that *temporary* migration explains the observed relationship between shocks and HIV. One story could be that rural individuals—in particular, males—migrate temporarily to cities in the face of negative income shocks, are sexually active and become infected with HIV in the city, and return to the countryside and infect their spouses. Although we do not directly observe shock-related temporary migration in our data, we indirectly explore the possibility by interacting our shock measure with estimates of whether a rural household is near an urban area. The assumption is that individuals are more likely to migrate to urban areas in response to shocks if they reside closer to urban areas. If the shock-related migration story were true, then we would expect shocks to have a smaller effect on HIV in areas more distant from urban areas.

Our measures of distance from the nearest urban area, as well as the population size of settled areas, are derived from the Global Rural-Urban Mapping Project (CIESIN 2010). We assume that households within 100 kilometers of an urban area are near that area, and we vary the population threshold that qualifies a settlement as being urban (100,000 inhabitants, 250,000 inhabitants, or 500,000 inhabitants). Population data are from the year 2000, helping to mitigate concerns that urban population size could have responded to the shocks of interest (for most of our sample, the shocks of interest are post-2000).

Results are shown in Table 5.6. If temporary urban migration were driving our results, then the interaction between our shock measure and the indicator for "near urban area" should be positive. Our data do not suggest that to be the case. The coefficient on the interaction is generally very small and statistically insignificant for both men and women; in the one case where the coefficient is significant, it is of the opposite sign than the migration story would suggest. Thus, there is little evidence that temporary migration is driving our results.

Table 5.6—Checking for effects from temporary migration

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

Source: Locale populations from CIESIN (2010).

Notes: Rural sample from high-prevalence countries. The near urban variable indicates whether a given cluster is within 100 kilometers of an urban area (defined as the population size in the column header). All specifications include controls for age, mean rainfall, and survey fixed effects. Estimations are weighted to be representative of the 19 countries. Robust standard errors are shown in parentheses clustered at the grid level.

6. CONCLUSION

Understanding the economic roots of the HIV/AIDS epidemic is of clear interest to both policymakers and academics. Previous work has been largely unable to demonstrate whether economic conditions exert causal influence on HIV outcomes among broad populations of interest. Anecdotal reports and an intriguing new microliterature suggest that in the face of economic hardship, women in SSA supply transactional sex in order to smooth consumption. By women increasing partnerships or increasing the risks taken within existing partnerships, it seems plausible that this shock-induced behavioral response could contribute significantly to increasing the risk of HIV transmission.

We investigate whether such behavioral responses to income shocks yield significant increases in HIV infections across SSA. In 19 African countries, we match an individual's serostatus test results to information on recent fluctuations in local economic conditions. Lacking any direct information on income variation at the individual level, we proxy village-level economic shocks by the number of droughts experienced over the preceding 10 years. In rural African areas, the majority of income is derived from agriculture and nearly all farming is rainfed; therefore, these shocks represent one of the most important sources of rural income variation on the continent.

In countries with severe epidemics (5 percent prevalence or higher), we find that a crop year rainfall realization below the 15th percentile of historical realizations (that is, what we deem a negative rainfall shock) increases the risk of HIV by 14 percent for rural women and 11 percent for rural men. Examination of self-reported data on sexual behavior reveals that those exposed to shocks have more lifetime partners and are more likely to have multiple concurrent partnerships. This supports the theory that increased HIV rates result from increases in risky or transactional sex. Based on a simple model of the transactional sex market, we explore whether an increase in transactional sex is attributable to an outward shift in women's supply curve or to an outward shift in men's demand. We find that the effects for women are concentrated among those most affected by the shocks (those working in agriculture), whereas for men, they are among those unaffected by the shock (that is, those employed in nonagricultural jobs). This rules out an increase in demand and provides evidence for an increase in supply. We find little evidence for alternative pathways in our data: Results are not driven by young women who terminate schooling early as a result of shocks, nor do they appear to be driven by permanent or temporary outmigration from *shocked* villages.

The findings presented here provide strong evidence that changes in sexual behavior in response to economic shocks are an important contributing factor in the AIDS epidemic in Africa. Further, it seems that such behavior is specifically motivated by the vulnerability of rural women. Efforts to protect target groups from income volatility could thus pay large social dividends. Comprehensive social safety nets may be an unrealistic short-run goal for many revenue- and capacity-constrained governments on the continent. However, more targeted interventions, such as crop insurance or the development of drought-resistant crop varieties, could stem the spread of HIV by mitigating the sexual response to negative agricultural 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.

APPENDIX A: FURTHER DATA DETAILS

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

Source: Author's calculations using DHS data.
Notes: * Swaziland: additional HIV testing for those aged 12–14 and 50 and older 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—Nonresponse 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%

Source: Authors' calculations based on DHS data.
Note: Rates are for the full HIV testing sample, with the exception of Mozambique. Rates for Mozambique are for the 15–49 sample.

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 the survey in which he appears, and N_c is the population of his country in the year of that survey.

Each individual has a survey-specific inflation factor h, which 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.

APPENDIX B: ADDITIONAL TABLES

Table B.1—Frequency of rain shocks over 10 years

			Number of Shocks					
Survey		**HIV Rank**	**0**	**1**	**2**	**3**	**4+**	**Total Clusters**
Swaziland	2007	1	0	0	44	211	16	271
Lesotho	2004	2	0	60	302	19	0	381
Zambia	2007	3	114	148	52	5	0	319
Zimbabwe	2006	4	51	209	115	23	0	398
Malawi	2004	5	129	248	137	7	0	521
Mozambique	2009	6	13	68	59	66	64	270
Tanzania	2008	7	6	156	212	58	34	466
Kenya	2003	8	47	237	114	1	0	399
Kenya	2009	9	66	198	112	21	0	397
Tanzania	2004	10	153	132	47	13	0	345
Cameroon	2004	11	68	167	140	78	13	466
Rwanda	2005	12	0	31	229	200	0	460
Ghana	2003	13	45	236	88	43	0	412
Burkina Faso	2003	14	107	85	200	4	3	399
Liberia	2007	15	101	13	151	26	0	291
Guinea	2005	16	49	95	147	0	0	291
Sierra Leone	2008	17	0	0	0	350	0	350
Ethiopia	2005	18	205	149	89	80	6	529
Mali	2006	19	96	221	81	7	0	405
Congo DR	2007	20	30	58	146	37	22	293
Senegal	2005	21	154	172	39	3	0	368
Total			1434	2683	2504	1252	158	8031
Percent of clusters			18%	33%	31%	16%	2%	100%

Source: Authors' calculations based on DHS and UDel data.

Table B.2—Effects of shocks of varying size

	(1) 5 PCT	(2) 10 PCT	(3) 15 PCT	(4) 20 PCT	(5) 1.5 SD
5 PCT shock past 10 years	.012*				
	(.007)				
10 PCT shock past 10 years		.011**			
		(.005)			
15 PCT shock (10 years)			.012***		
			(.004)		
20 PCT shock past 10 years				.006*	
				(.003)	
1.5 SD shocks past 10 years					.017**
					(.008)
Observations	43147	43147	43147	43147	43147
R^2	.028	.028	.029	.028	.029

Source: Authors' calculations based on DHS and UDel data.

Notes: Robust standard errors are shown in parentheses clustered at the grid level. All specifications use the rural female sample from high-prevalence countries and include controls for age, mean rainfall, rural/urban designation, and survey fixed effects. All specifications are weighted to be representative at the national level. Parameter estimates significantly different from zero at 99 (***), 95 (**), and 90 (*) percent confidence.

APPENDIX C: RURAL POPULATION LOSS ARISING FROM SHOCKS

We could make a variety of assumptions regarding the share of a rural village that migrates during a shock. The column headers in Table C.1 show several possible assumptions ranging from 1 to 10 percent *per shock*. A bit of algebra reveals that if, for example, 5 percent of the population were to leave during each shock, a village with three shocks over the past 10 years would lose 14.3 percent of its population in that time. The calculation of lost population by number of shocks and assumption maintained are shown in Table C.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 10 years before the applicable survey. The bottom row of Table C.1 shows these estimates.

Table C.1—Potential reductions in rural populations due to shock-induced migration

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

Source: Authors' calculations based on DHS and UDel data.
Notes: Each cell represents the 10-year population loss in a cluster that has occurrences of shocks (as given by the row) and population loss per shock (as given by the column). The last row represents the assumed total loss from the rural sample based on the shocks observed in the data, under the various assumptions of population loss per shock (as given by the column).

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.[25] On average, the rural share of the populations of these countries is reduced by 5.8 percent over 10 years. Based on the assumption that 3 percent of a village leaves during each shock, we estimate that our rural sample lost 4.26 percent of its population in the past 10 years; an assumption of 5 percent leaving yields a total decline of 7.01 percent. This result suggests that an assumption of 3 percent population loss per shock approaches reality, with 5 percent as an extreme upper bound.

[25] Figures from World Bank Development Indicators, 1990–2000.

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