Consumption Smoothing and HIV/AIDS: The Case of Two Communities in South Africa*

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Abstract

HIV/AIDS threatens to overstretch the already frail informal safety nets in countries heavily affected by the epidemic. Worn-down safety nets make it difficult for non-affected households as well as for those with an HIV-infected member to keep up appropriate consumption levels when experiencing shocks. Affected households may in addition face exclusion from informal insurance networks due to wide-spread stigmatization, thus depriving them of their main coping mechanism in a time of increased health expenditure and income loss. Surprisingly, the resilience of informal networks to HIV-induced shocks and their uncertain worth for afflicted households in particular have not been empirically tested to date. Using three years of a novel panel data set from two poor South African communities experiencing HIV-related illnesses and deaths, I investigate the ability of households to insure their consumption. Based on the finding that affected as well as non-affected households adjust their food expenditure in reaction to income changes, the benchmark hypothesis of full insurance is rejected. A supplementary negative effect is observed for HIV-affected households who smooth food consumption at the expense of regular spending, thereby undermining their future capacity to cope with shocks. Furthermore, our results suggest that HIV/AIDS has a negative economic impact even before death, the focus in the current literature. These novel findings contribute towards our understanding of the complex economic effects of the epidemic, adding to the much-needed empirical foundation for the proper design of support mechanisms for households affected by HIV/AIDS.

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I. Introduction

HIV/AIDS exacerbates the risk environment of households in regions heavily hit by the epidemic through repeated and prolonged illness periods, related loss of income and increased health care expenditure. The economic impact of such shocks depends on the extent to which households have access to either formal insurance institutions or private risk-sharing networks such as non-contractual transfers between members of the family or within a village. In many developing countries, this latter type of mechanism is of particular relevance given weak formal financial markets (Asfaw and von Braun 2004; Grimard 1997). The epidemic has predominantly struck people in countries that were already distressed by shocks such as natural disasters, diseases, or the loss of livestock, even before the onset of the epidemic. Now, these households are in addition exposed to a myriad of shocks brought about by HIV/AIDS as discussed in Barnett and Whiteside (2002).

To date, most studies investigating the microeconomic effects of the epidemic focus on direct effects of HIV/AIDS, in particular premature adult mortality[1]. The main drawback of this literature relying on prime age mortality to identify households as affected is that HIV-affected households[2] may experience detrimental economic consequences long before a member of the household dies. The shocks experienced by such households are indirect in nature, cumulative, and not easily detected by looking at isolated shock episodes. As a result, the current literature provides only limited guidance for identifying vulnerable households in need of support. In countries with a pandemic HIV/AIDS crisis, the covariant nature of the illness and mortality shocks puts a heavy strain on informal insurance networks that function best for coping with idiosyncratic risk. Informal insurance mechanisms may then offer only limited protection from shocks brought about by the epidemic. Households with a HIV-

infected member are in addition threatened by the exclusion from these informal safety nets, for example due to wide-spread stigmatization associated with HIV/AIDS. Also, the permanence of illness brought about by the epidemic may render directly-affected households undesirable members of insurance networks.

In the developing world, large fractions of the population gain their living in the informal sector, and also spend their money predominantly in this sector. Although the size of the informal economy is difficult to estimate, some statistics underline its importance: in sub-Saharan Africa, it is estimated that the non-agricultural employment share of the informal workforce is 78%, and that the contribution of the informal sector to the GDP in this region is about 41% (ILO 2002). In South Africa, the contribution of the informal sector to the GDP has been estimated at 28.4% (Schneider 2002). Similarly, access to formal saving and credit in South Africa is very limited, in particular in former homelands and current townships, where people are typically poor and infrastructure is lacking. In addition, many people do not have the necessary identification documents for opening a bank account. In the current dataset, less than 11% of households on average have formal savings, and if they do, the average amount is less than 2% of household income. Just slightly over 4% of households in the sample obtained credit from a bank. These statistics underline the importance of informal insurance networks in coping with income or expenditure shocks. For example, almost 15% of the households under consideration owe money to family or friends, and a full third of households owe money to store owners, one of the main sources of informal credit. For these reasons, it is important to evaluate the impact of HIV/AIDS on the effectiveness of informal networks in coping with the economic effects of the epidemic.

This paper investigates whether households in two communities in South Africa heavily affected by the epidemic manage to effectively smooth consumption, and whether households with HIV-infected members enjoy the same access to insurance as non-affected households. The analysis is based on the benchmark theory of full insurance, which builds on the premise that changes in consumption should be independent of changes in income for fully insured households. Focusing on the outcome variable of consumption smoothing rather than the effects of specific shocks holds the potential to uncover the effects of long-term cumulative shocks an affected household may experience over time. In contrast, the current literature typically evaluates the economic consequences of long-term shocks through direct observation, an approach that requires a data series of equal length as the shock. Most data series used for such analysis are however of much shorter duration. Also, given the frequency of illness episodes brought about by HIV/AIDS, it may be more informative to assess the capacity of a household to maintain appropriate consumption levels after repeated shocks rather than evaluating the costs of single sickness episodes. Investigating households' capacity to effectively employ informal coping mechanisms then offers more information for the design of support mechanisms than investigating isolated episodes of illness or mortality. The approach also offers the advantage of addressing the impact of HIV/AIDS on so-called "non-affected" households, i.e. those without a known sero-positive member that the literature generally overlooks. Such households may face stress by caring for sick relatives, the sending of remittances, or through overstretched informal insurance mechanisms.

There is a large literature based on the theory of full insurance investigating the vulnerability of households to various shocks in developing countries. This body of literature indicates that the coping ability of households may be specific to the environment and the type of shocks faced[3]. Harrower and Hoddinott (2005) investigate consumption changes for a sample of 275

households in Mali and find idiosyncratic shocks including illness to be of little significance, while covariant shocks exert a considerable negative effect on consumption smoothing. The findings in Dercon, Hoddinott, and Woldehanna (2005) indicate that illness shocks considerably reduce consumption per capita in 15 Ethiopian villages. Using panel data on Indonesian households, Gertler and Gruber (2002) observe that the ability to cope with illness shocks depends on the magnitude of the shocks. Illnesses that do not affect the physical functioning of individuals can be fully insured against, whereas less than 40 percent of major illness episodes can be covered by the household. As mitigation mechanisms are not necessarily shared at the level of the village, Dercon and DeWeerdt (2005) test the extent of risk-sharing for overlapping insurance networks within a village. The authors reject the hypothesis that consumption is fully insured for a sample of 120 households in Tanzania, yet do not detect persistent effects of health shocks.

The above papers analyze illness shocks not related to HIV /AIDS. The epidemic may bring about shocks of a different nature as it is characterized by a long asymptomatic period, followed by several years during which frequent morbidity periods are experienced by the person carrying the virus. In South Africa, large parts of the population are infected, opening the possibility that the disease represents a covariant shock[4]. In the Free State province, the HIV prevalence rate was estimated at 14.9% by the only nationally representative study to date HSRC (2002). Surveys based on data from antenatal clinics have put the HIV prevalence rate amongst 15-49 year olds in 2001 at 20.1% (UNAIDS, 2002). As the morbidity periods caused by the epidemic are typically repeated, they may become expected, and may have permanent effects[5].

It is an open question whether affected households differ from non-affected households in important dimensions determining consumption behavior. In South Africa, it has been argued that in the early phases of the epidemic, it were mainly better-off people who got infected such as teachers of truck-drivers before the disease spread to the general (and poorer) population (Barnett and Whiteside 2002). I will make the case that in the communities under consideration that in general are fairly homogeneous in their socioeconomic variables, the two types of households were similar along key dimensions before the onset of the epidemic. I construct a long-term wealth index using principal component analysis composed of assets that are not readily sold off in the presence of shocks, and compare their long-run wealth. By doing so, I am trying to address the issue of causality between poverty and HIV-infection for the sample of households under consideration. In order to deal with anticipation, instrumental variable regression is employed.

The data underlying the analysis in the paper is particularly apt to uncover the economic effects of HIV/AIDS as households are separated into affected and non-affected households *a priori*. In contrast, the previous literature typically labels households as affected when they experience prime-age mortality during the observation period. Analyses based on such *ex-post* classification of affected households potentially underestimate the detrimental economic consequences of HIV/AIDS as households with HIV-infected members are labeled as non-affected if they do not experience death. The current paper stresses the possibility neglected by the current literature that households with HIV-infected members may experience detrimental economic consequences long before the infected member dies.

The rest of the paper is organized as follows: in Section II the data setting is presented, followed by the theoretical basis in Section III. Section IV discusses why households affected

by HIV/AIDS might experience exclusion from informal inter-household risk-sharing mechanisms. In Section V the empirical specification is discussed, in Section VI the results are presented. Section VII presents some robustness checks, in particular regarding the types of households compared. Section VIII concludes.

II. The Data[6]

The household panel data were collected during six waves starting in May/June of 2001 and repeated approximately every six months until November 2003 in two communities in South Africa's Free State province. The Free State is one of the nine provinces that emerged after the end of the Apartheid regime in South Africa and hosts about 7% of the country's population. The province is predominantly black (88% of the population), and its GDP per capita stood at 5185 US$ (PPP) in 1996, putting it slightly below the national average of 5916 US$ in that year. The aggregate HIV prevalence rate in the Free State was estimated at 14.9% in 2002, the highest rate in any of South Africa's provinces (HRSC 2002). According to the same study, more than one third of the population in that year did not know about the possibilities of testing for HIV, indicating limited awareness of the epidemic.

The first study site, Welkom, is a large urban settlement in the Eastern Free State that includes some wealthy, middle income, and poor residential areas. The other site, Qwaqwa, is the smallest of the former homelands, has very limited resource endowments, poor land quality, and is still governed by traditional leadership. The latter region is hence classified as rural, even though agricultural activity in the sample is for the most part negligible (for example, only about 2.5% of food expenditure is accounted for by home-grown consumption of fruits,

vegetables, and poultry). The median real monthly household income in the sample is 1114 Rand (2000) in Welkom and 604 Rand in Qwaqwa, and is a result of the sampling frame that comprised mainly low income African households. This corresponds to a yearly household income of about 1766 USD and 958 USD respectively using the IFS 2000 exchange rate.

Individuals infected with HIV are sampled from networks and organizations involved in counseling, home-based care and public health care services. With informed consent, the household of the HIV-positive patient is interviewed. In order to retain confidentiality the households are not informed about the serostatus of the member. The interview is presented to the households as a study on the effects of morbidity and mortality in general, HIV/AIDS is not mentioned to avoid refusal due to the stigmatization associated with the epidemic in South Africa. The person interviewed in the household is the person responsible for the organization of the household finances.

The control group consists of households in the immediate physical vicinity of the affected household. This type of household was screened for the presence of HIV-related diseases such as tuberculosis or pneumonia in the month before the interview. If a household showed such an illness, its neighbor was approached until a household without such a disease was found that agreed to participate in the study. As no biomarkers were used for the identification of non-affected households, the control group may be contaminated. During the six waves, 31 households initially classified as non-affected had to be reclassified due to their showing HIV/AIDS-related diseases such as tuberculosis (see below for a discussion of these households). The sample size, though not large, is comparable to that of other studies in the literature on the effect of shocks in developing countries. There are few prior studies of the impact of HIV/AIDS at the household level that have the wealth of information, particularly

concerning the morbidity shocks, available, and, most importantly, the classification of households into affected and unaffected before the start of the data collection. The questionnaire consisted of questions on demographic, health, and economic characteristics of the household. The detailed section on the economic characteristics covered employment (11 questions), income (22 questions), expenditure (17 items), savings (7 items), debt (26 items), assets (16 items), and borrowing (5 items). The questionnaires were constructed in English and then translated into the two most common languages in the two communities, Xhosa and Sesotho.

Although attrition typically poses serious problems for data collection efforts in developing countries, Alderman et al. (2001) note that attrition in four studies investigated do not pose obstacles to data analysis. In the current study attrition amounts to 13.42%, mainly because the households could not be relocated. Households leaving the sample tend to be somewhat younger, have fewer assets, and show a higher number of unemployed as well as ill persons. In order to take account of the different socioeconomic characteristics in the analysis, we will present key regressions when including this type of households, and when leaving it out of the regression sample. In the study, households leaving the sample were replaced in the fourth wave with child-headed ones to take account of changing realities on the ground. As these households display very different socioeconomic and demographic characteristics, they were dropped from the analysis. Table A.1 gives descriptive statistics of key variables relating to socioeconomic status of affected and non-affected households.

There were 31 households initially classified as non-affected that were in later periods reclassified as affected because of their showing HIV-related morbidity such as TB, pneumonia, or acknowledged HIV. Of these 31 households, five left the sample during the

observation period. The households changing their affected-status tend to have older and less educated household heads, and be significantly poorer than non-affected households (in the case of the illiquid wealth index, they were also significantly poorer than households initially classified as affected). These households who are reclassified during the observation period are included in the affected sample for two main reasons: first, these households show socioeconomic characteristics that are very similar to those of affected households. Second these households almost certainly already had an HIV-positive member at the beginning of the observation period: due to the typically long period between infection with the virus and first outbreak of opportunistic diseases that would not have been observed had the household become affected after the start of the first wave of the study. Therefore, the households, although undetected by the econometrician, should have been already classified as affected at the beginning had better information been available.

III. The Full Insurance Model and Illness

In the absence of formal insurance mechanisms, households in developing countries rely to a large degree, and sometimes predominantly, on informal sources of insurance (Levine, Gertler, and Moretti 2004; Grimard 1997). These coping mechanisms are based on the spatial spreading of idiosyncratic risk of individuals or households, i.e. across other individuals or households participating in the network (see Townsend 1995 for an overview). Therefore, this type of insurance mechanism works best for shocks that can easily be diversified, i.e. concern only a subset of the network. Studies analyzing the effectiveness of informal insurance mechanisms typically use the benchmark theory of full insurance, which investigates whether income shocks translate into consumption changes. Given a preference by consumers and

households for stable consumption streams, the main proposition stemming from this theory, which can be viewed as the cross-sectional counterpart to the permanent income hypothesis, is that individuals' consumption should not respond to idiosyncratic shocks (Cochrane 1991). Although the theory of full consumption insurance uses the assumption of competitive markets in explicit contingent claims in its derivation, these are not necessary as long as other formal or informal institutions such as gifts or loans from friends or relatives provide insurance (Townsend 1994; Cochrane 1991).

The first order condition stemming from a planning problem maximizing the sum of individual households' utilities weighted by their respective Pareto-weights can be expressed as (for detailed expositions see Mace 1991; Cochrane 1991):

$$(\rho^j)^t \lambda_j u_c(c_t^j, \delta_t^j) = \mu_t \tag{1}$$

where ρ^j stands for the rate of time preference of household j, λ_j is the household's Pareto weight, u_c denotes the first derivative of the utility function with respect to consumption c, δ_t^j signifies preference shifters at time t, and μ_t is associated with the Lagrange multiplier on the budget constraint. As this multiplier is constant across households, individual endowments do not enter into the determination of household's consumption allocations given aggregate consumption and the constant Pareto weights. Using observations at two points in time to remove this time-invariant effect leads to a testable implication of the theory:

$$\rho^j \frac{u_c(c_{t+1}^j, \delta_{t+1}^j)}{u_c(c_t^j, \delta_t^j)} = \frac{\mu_{t+1}}{\mu_t} \tag{2}$$

The discounted marginal utility of individuals depends only on the growth in aggregate consumption and the taste shifters δ, but not on individual endowments or any other idiosyncratic shocks. In order to arrive at an empirical specification, several forms of the utility function can be chosen, for details see Cochrane (1991). When using a utility function with constant relative risk aversion, we arrive at a frequently used empirical specification where the change in the logarithm of income is added as a shock variable on the right hand side[7]:

$$\Delta \ln c_{hvt} = \sum_{vt} \delta_{vt}(D_{vt}) + \beta \Delta \ln y_{hvt} + \delta X_{hvt} + \Delta \varepsilon_{hvt} \,. \tag{3}$$

We regress the change in the log of income (i.e. fixed effects within estimator) on a constant term, the change in log total income, a vector of household characteristics X, as well as time-location dummies D in order to control for covariant shocks that affect all households in the sample similarly. Then, the propensities to spend out of income changes for different groups of households within a sample can be tested, an approach suggested in several papers described in Skoufias and Quisumbing (2005). The sample at hand classifies households *a priori* as affected or non-affected, allowing us to split the sample accordingly:

$$\Delta \ln c_{hvt} = \sum_{vt} \delta_{vt}(D_{vt}) + \beta \Delta \ln y_{hvt} + \gamma(affected * \Delta \ln y_{hvt}) + \delta X_{hvt} + \Delta \varepsilon_{hvt} \,. \tag{4}$$

This equation will serve as the basis for the estimation procedures described in Section VI.

IV. Stigmatization due to HIV/AIDS and Participation in Informal Insurance Networks

The main question of interest in this paper is whether households with an HIV-positive member show the same relative propensity to spend out of income changes as non-affected households. Stated differently, are affected households in a position to benefit from informal insurance against income shocks to the same degree as their non-affected peers? One determinant for participation in such networks that HIV/AIDS may have an impact on is the frequency of interaction with people in the network (Fafchamps and Lund 2003). HIV/AIDS may lead to reduced interaction of individuals in a network due to stigmatization. Discrimination against a person suffering from an illness is particularly likely when the individual can be blamed for its occurrence, if the illness has potentially serious consequences for others, if there are outward signs of the disease, and if it results in a decreased level of competence (Weitz 1991). Diseases brought about by HIV/AIDS display all of these characteristics (Bennett 1987).

Exclusion from an insurance network because of HIV/AIDS-related stigmatization can happen for two reasons: first, individuals learning about their serostatus might withdraw themselves from society because they feel self-depreciation and shame (Fife and Wright, 2000). Potentially more important for the exclusion from insurance networks is the concept of "enacted stigma" that happens when people are actually discriminated against because they have or are thought to have HIV (Bond, Chase, and Aggleton 2002).

There is ample evidence for stigmatization against HIV-positive individuals: Nelkin (1986) reports that people with AIDS suffer attempts of their exclusion them from the workplace, and children with AIDS are excluded from the school room. Lawless, Kippax, and Crawford

(1996) find that HIV-infected women experience discrimination when accessing health care facilities in Australia. In a study on Zambia, people who are suspected to be infected with HIV frequently experience that other people in the community put physical distance between them or talk badly about them (Bond, Chase, and Aggleton 2002). In a study involving four sites in the KwaZulu-Natal province of South Africa it was found that HIV/AIDS can lead to land tenure vulnerability of widows and orphans (Drimie 2002).

Another determinant of access to informal insurance schemes found in the empirical literature is the wealth level of the household, with poor households often being discriminated against (Skoufias and Quisumbing 2005). HIV/AIDS may contribute to increased poverty due to increased medical expenses and transport costs for going to a clinic. Affected households may experience loss of income due to illness periods or when caring for sick household members, forcing them to sell off assets. Health care costs in the sample account for more than 6% of total expenditure for both types of households, and there is evidence that households affected by HIV/AIDS were forced to sell off relatively liquid assets in order to cope with the disease: when comparing an index for illiquid wealth constructed using principal component analysis, affected and non-affected households differ only slightly. This wealth index was constructed based on assets that are not easily sold off or changed, and include the quality of the roof, the quality of the dwelling, whether the dwelling is owned by the household, and the number of rooms in the dwelling. Land is not included in the construction of this index as there are very few households owning property. The illiquid wealth index therefore gives an impression of the wealth situation of the household over a relatively long time period, ideally even before HIV/AIDS started to have an impact on the household.

When looking at a similar index constructed from more liquid assets such as radios, bikes, mopeds, fridges, and cell-phones, affected households display a significantly lower value as shown in Table A.2. It is noteworthy that the absolute difference in the value for the liquid asset is more than three times the size as the gap for the illiquid asset index, indicating that we observe a genuine difference in liquid wealth levels. Distressed households have been observed to sell off their most liquid assets first (Sauerborn, Adams and Hien 1996; Frankenberg, Smith, and Thomas 2002), inviting the interpretation of the observed levels for the two types of asset indices that affected and non-affected households were relatively similar in their wealth levels before the onset of the epidemic. The wealth indices then suggest that affected and non-affected households potentially enjoyed relatively similar levels of economic well-being before the onset of the epidemic that subsequently contributed to the relative impoverishment of households with an HIV-positive member. The coping mechanism of the selling of liquid assets can potentially have negative consequences for subsequent access to informal insurance networks.

The probability of observing affected and non-affected households that were alike in important socioeconomic characteristics before the osnet of the HIV/AIDS epidemic is increased by the selection method used for control households: this latter group was chosen in the immediate physical neighborhood of the affected household. Given the homogeneous nature of the socioeconomic status of households within the two communities, geographical vicinity is likely to be a good indicator socioeconomic status, unless impoverished households had to move from a relatively wealthier neighborhood.

V. The Empirical Specification and Variable Discussion

The literature cited above employs an ex-post classification of affected households as households that experience a prime age adult death between two observation periods. By definition, all affected households experience a *change* in their status from non-affected to affected when using this classification method. The main drawback of this literature is that it does not take into account effects that happen to households with HIV-infected members before these seropositive people pass away, despite the long time between infection and death that is one of the main characteristics of the disease. The set-up of these studies may actually contribute to the relatively low effect of HIV/AIDS found by them: as the control group of non-affected households in fact includes households with HIV-positive members that experience morbidity but not mortality, these households likely experience detrimental economic effects from the disease that are wrongly attributed to households classified as non-affected.

The current dataset, in contrast, does not rely on prime-age adult mortality for identifying affected households. Affected households are defined as households having at least one seropositive member who knows about his status due to prior testing. Based on the informed consent of these HIV-positive people to interview the household they belong to, we are able to investigate the economic shocks experienced by these households even in the absence of morbidity or mortality through this *a priori* classification method. For households classified originally as affected as well as for those reclassified after the beginning of the survey, the classification is based on an individual in the household that has contracted the disease before the beginning of the study. Potential behavioral responses due to learning of the household's affected-status are taken into account by the fixed-effects estimation used in this study. For

example, if a health shock is expected, consumption in the period prior to the illness may be reduced in order to equalize the marginal utility in the two periods (Dercon and DeWeerdt 2005). If affected households reduce their overall level of consumption when learning of their affected-status in expectation of (repeated) illness periods whose timing cannot be precisely predicted, this effect is taken into account by the fixed effect. The assumption made is therefore that the affected households in the sample have already incorporated any behavioral changes due to their change in status before the onset of the survey. For this reason, we also include in the affected group households that were reclassified from non-affected to affected status because of their showing HIV-related morbidity, as these households have had an HIV-positive member for a relatively long time due to a time of between 7 and 10 years from infection to death in the absence of ARV treatment. Another reason for including them in the affected group is that their socioeconomic characteristics are similar to those of households originally classified as affected as can be seen in Table A.2 in the appendix. As we do not know whether the infected individuals in the household communicate their serostatus to the other household members, we cannot test this assumption, however. In order to be able to study behavioral impacts, a set-up would be required in which there is full information about the serostatus of the household members and the information about the serostatus in the household.

The regression framework based on fixed effects estimation has the advantage that, while potentially causing a bias towards zero of the coefficients, it addresses measurement errors that are person-specific and constant over short periods of time (Asfaw and von Braun 2004). However, time-variant effects such as preference shifters in the form of household demographic variables have to be controlled for. If shocks affect the household composition or other variables associated with a shift in preferences, a bias would arise. The problem does

not come up if the change in the household composition takes place in response to the status of being an affected household, as this adjustment would already have taken place before the start of the data collection. It is not clear that the household size would systematically change in either direction as it could increase due to inmigration of people caring for the sick member, while outmigration of members with little productivity such as children could also be observed (Gertler and Gruber 2002).

In what follows, we assume consumption and health to be separable. If this condition is not fulfilled, the health status changes the marginal utility of consumption, leading to biased coefficient estimates (Gruber and Gertler 2002)[8]. Two factors support the separability hypothesis in the current database: on the one hand, marginal utility from consumption may decrease for ill people, leading to a reduction in food expenditure. A countervailing factor is that in response to observing the illness, higher quality food such as meat may be purchased with the intention of providing the sick person with higher-value nutrients. The higher prices associated with higher quality food would run counter to the first effect. In the absence of more detailed data on food expenditure, this issue cannot be pursued further in this paper. Second, as the variables in the regression analysis are measured at the household and not the individual level, the demand-driven change in consumption due to lower quantity and possible higher quality nutrition of a single individual may not significantly influence total household consumption, in contrast to studies investigating food consumption at the individual level.

A further step taken to make consumption state-independent of health shocks is that expenditures related to illness and death associated with HIV/AIDS are excluded from the measure of regular expenditure, in particular health spending itself as well as transport

expenditure that is often a non-trivial expense category for households in developing countries[9]. Testing is done separately for total expenditure, consumption expenditure and regular expenditure as previous studies suggest that different expenditure categories may be associated with different degrees of priority (Jalan and Ravallion 1999; Morduch 2002; Skoufias and Quisumbing 2005). Households may place the highest importance on smoothing basic needs such as food consumption while assigning a lower priority to keeping the marginal utility from the expenditure for luxury goods constant.

Total expenditure includes expenses for food, education, health care, household maintenance, transport, clothing, rent, personal expenses, durable expenses, once-off expenses, and home-grown production. The relative importance of these expenditure categories is presented in Table A.3. As this is a very broad expenditure category, households are likely to trade off certain expenditures within this category in the face of a shock to income or expenditure following a morbidity or mortality episode. Regular expenditure as used in the regression analysis includes expenditure for education, household maintenance, clothing, rent, personal expenses, and durable expenses.

Dummy variables for location interacted with observation wave control for shocks experienced in both communities. Dercon and DeWeerdt (2005) discuss the appropriate unit among which informal insurance may take place. There may be other networks (defined by ethnicity, family ties, or other characteristics) that may be more appropriate units of analysis, but data limitations prevent further investigation of this issue in the present study. In this study, community-level consumption as a measure of covariate income shocks are not used as community-level dummies are more flexible in what they can cover (Morduch 1995; Deaton 1997), and because for some villages we have few observations(the average number of

households per residential area in this study is relatively low as the individuals were sampled at health clinics that attract patients from a multitude of residential areas in the near and further neighborhood of the clinic). Such dummies control for shocks to the community economies such as price shocks or weather, shocks that can bring about changes in health (Gruber and Gertler 2002), or other aggregate resources that are not reflected in individual incomes and assets (Hoogeveen 2002).

VI. Results

In Table 1, we present the results of a regression of the change in total consumption on wave dummies interacted with location, as well as income changes, and preferences shifters such as changes in the composition of the household. Total expenditure encompasses several expenditure categories for which different degrees of adjustment might take place in response to a shock. For example, a household experiencing a morbidity episode of one of its members might incur transport cost to a clinic and subsequent health care cost, and might react by scaling down household maintenance or some other expenses that is not perceived as equally urgent. The magnitude and direction of the overall change is therefore not clear a priori. In Table 2, the results for a regression with regular expenditure excluding health care and transport costs are represented. This category by construction should be less dependent on health shocks and reflects to a certain extent the capacity of the household to maintain expenditure that contributes to its medium-term economic position through the investment in items such as education, maintenance of the household, clothing. Table 3 represents the same type of regression for food expenditure. If households do not manage to keep up this type of

expenditure they are in a position of immediate risk, as malnutrition can enhance the chance of falling sick.

In column 1 of the regression tables below, ordinary least-squares (OLS) regression results are presented using standard errors that are corrected for potential heteroskedasticity using the Huber/White method. In column 2 quantile regression results that are less susceptible to data outliers than OLS regression are displayed. Unlike OLS estimation, this technique does not impose the restriction that all households in the sample behave similar to the mean household (Buchinsky 1998). The third column presents the results arrived at using instrumental variable regression as the OLS estimates above may be subject to bias due to measurement error or endogeneity of income changes (Cochrane 1991). Measurement error does not likely pose a serious problem for the consumption variables employed in this study as they are not imputed, which is mainly a problem for agricultural households that typically consume part of their own produce. However, recall bias may lead to this source of error (Deaton 1997).

Behavioral changes in anticipation of periods of reduced income or increased expenditure can lead to biased coefficient estimates. For the study at hand endogeneity of income generating activities as an a priori mechanism to deal with shocks is likely not an important problem due to a high rate of unemployment (about 75% of working-age adults in the two communities are unemployed) in the sample. In a similar vein, the fraction of people participating in agriculture is negligible, eliminating crop choice as a risk reduction tool. Several studies such as Dercon and Krishnan (2000) use health shocks as instruments for income changes. If illness is repeated and/or has a delayed impact, the total impact will not be reflected when using the current shock indicator as an instrument only. Lagged values of the shock variable will have to be included in such a case as suggested in Dercon and DeWeerdt (2005). Other

instruments for income changes used in the literature are the wealth level and income of the household in the first period observed (Asfaw and von Braun 2004). The current study uses both types of variables as instruments for income changes.

All three regression techniques indicate that neither type of household manages to fully insure food consumption as the community-wave interaction dummies are jointly significant. Covariate shocks such as prices, wages, or interest rates appear to be important in explaining fluctuations in consumption. The coefficient on income changes for affected households is expressed as an extra effect observed over the coefficient on the same variable for non-affected households. A coefficient o zero, for example, would suggest that the relative propensities to consume out of income changes for the both types of households do not differ.

In Table 1, the results for the regressions for total expenditure are presented. The first result is that the benchmark theory of full insurance is rejected as evidenced by the non-zero coefficients on the time/location dummies that measure covariate shocks that are not diversified away. We also observe that both types of household show a non-zero propensity to consume out of income changes for this expenditure category, while the relative propensities between non-affected and affected households do not differ as shown by a coefficient of affected households that is not significantly different from zero. Changes in the household size lead to an increase in the propensity to consume out of income changes, while changes in the dependency ratio lead to its decrease. When including households subject to attrition in the above regression, the results do not change for the propensities to consume out of income changes, while the changes in the household size and the dependency ratio cease to have a statistically significant impact[10]. The results are almost exactly the same when employing the method of quantile regression that is more robust to outliers. When instrumenting the income

changes using morbidity and mortality shocks as well as initial wealth levels, the propensity to consume out of income changes decreases for non-affected households while increasing for affected households, but neither coefficient is statistically significant, potentially because of a very low explanatory power of the first stage regressions.

As can be seen in Table 2, affected households display a higher marginal propensity for consuming food out of income changes that is however not statistically significant. Only the results arrived at using instrumental variable regression differ in that the coefficients for non-affected households and affected households are almost opposite in magnitude compared to the other regression techniques. Neither coefficient is statistically significant, however, possibly due to the results of the first stage regression that show relatively low F-statistics with values of between 4.10 and 4.93. Bound, Jaeger, and Baker (1995) suggest that this statistic is an indicator of the quality of the instruments used. Most of the variables controlling for preference shifters except for changes in the household composition entered in the regression are not statistically significant, and the coefficients on the change in log income are robust to their exclusion, a finding in line with previous studies such as Asfaw and von Braun (2004).

In Table 3, we repeat the above estimations for regular consumption expenditure as the dependent variable. This type of expenditure includes education expenditure, rent, and household maintenance but excludes food consumption, health expenditure and transport costs that may be correlated with health shocks. For expenses of this type we observe a propensity to consume out of income changes that is significantly higher for affected households than for non-affected ones. Again, the control variables are largely insignificant and the results are robust to their exclusion. The results therefore suggest that affected and

non-affected households manage to keep food consumption constant while affected households seem to be unable to smooth regular expenditure in the face of income shocks. However, when including households leaving the sample before the end of the survey, the extra propensity for affected households to consume out of income changes is somewhat smaller in size and becomes statistically significant only at the 17% level.

VII. Robustness Checks and Extensions

Several studies suggest that household size may be an endogenous variable (Asfaw and von Braun 2004; Gertler and Gruber 2002; Frankenberg, Smith, and Thomas 2002). In order to address this potential specification problem, the log of adult equivalent consumption is used as the dependent variable while at the same time excluding the variable related to household size from the right hand side of the equation. The coefficient estimates for the two types of households are almost identical for total expenditure and food consumption and are only slightly higher for regular expenditure for affected households as can be seen in column 4 of Tables 1, 2 and 3. Changes in the household size through the in- or outmigration of household members therefore do not seem to be used as a coping mechanism in the sample.

Next, an informal test is performed to indicate whether mortality, the focus in the current empirical literature on HIV/AIDS, is driving the results or whether households display an additional propensity to consume out of income changes even in the absence of prime age adult death. In column1 of Table 4, we use the approach used in the literature in identifying affected households as those households experiencing a mortality episode in the six months before the survey wave. We see that there is no difference in the propensity to consume out of

income changes between so-called "affected" households and the non-affected ones when classified as in the literature (which, evidently, include about 50% of households with HIV-positive members as about 50% of affected households as classified in the current study observe one or more death over the six observation periods).

Second, we test whether we observe a different relative propensity to consume out of income changes between the two types of households when we exclude households experiencing an adult death in a survey wave. By excluding households with mortality episodes we informally test whether there are either long-term effects of mortality episodes (as we exclude households only immediately after they experience the death), or whether there are also economic effects of morbidity. We cannot disentangle the effects that are due to mortality and those due to morbidity as we do not know which households had a mortality episode sometime before the beginning of the survey or just following it. We observe in column 2 of Table 4 that affected households still have a higher propensity to consume out of income changes, indicating that it is not mortality that is exclusively driving the results. The coefficient that is weakly non-significant in the OLS specification turns significant at the 8% level when we employ the more robust method of quantile regression.

Summing up, we find some evidence that a) when using the approach used in the literature to classify households into affected and non-affected no significant effect of being an affected household is detected and b) that even when we households experiencing mortality episodes during one of the survey periods we find evidence that households undergo economic shocks due to HIV-related morbidity or longer-term effects of mortality the literature fails to detect.

When stratifying the sample into poor households that have a negative value for the illiquid asset index in the first wave of the data and those with a positive value, we find that it is mainly poor affected households that show an extra propensity to consume out of income changes when compared to non-affected households in column 3 of Table 4. What is disconcerting is that the fact that this result holds not only for regular expenditure but also for food expenses (the result is not statistically significant, though, with a p-value of 0.18), indicating that households that are both poor and affected may experience difficulty even managing their nutritional situation in the face of shocks[13]. A similar result is found for households harboring orphans: affected households with this characteristic show a propensity to consume out of income changes that is significantly higher for regular expenditure than the corresponding coefficient for non-affected households as evidenced in column 4 of Table 4.

VIII. Concluding Remarks

The empirical literature investigating the microeconomic impact of HIV/AIDS focuses almost exclusively on the effects of prime-age adult mortality and largely ignores shocks prior to this event. The above analysis demonstrates that there is reason to believe that the negative economic consequences brought about by the epidemic begin much earlier, and may be felt by affected households even in the absence of mortality shocks. The results indicate that both types of households manage to keep the marginal utility from food consumption constant except for covariate shocks, but potentially at the expense of other regular expenditure. This possibility is particularly apparent for households classified as affected. As regular expenditure includes expenses for education and household maintenance, their reduced spending may undermine the future capacity of affected households to deal with economic

shocks. These novel findings contribute towards an empirical foundation for the design of social support mechanisms for households affected by HIV/AIDS, a much-debated issue in South Africa.

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Table A1: Variable Descriptions and Sample Summary Statistic

		Non-affected			Affected			Reclassified	
Variable name	Variable description	Mean	Std Error	p-value	Mean	Std Error	p-value	Mean	Std Error
$\Delta \ln c$	Change in the log of total food consumption	-.004	.020	.563	.013	.022	.813	-.001	.058
	Change in the log of total regular expenditure	-.010	.034	.320	.037	.033	.314	-.054	.095
	Change in the log of adult-equivalent food consumption	-.006	.020	.420	.018	.018	.922	.024	.057
	Change in the log of adult-equivalent regular expenditure	-.013	.034	.364	.029	.031	.499	-.030	.094
$\Delta \ln y$	Change in the log of total household income	.032	.030	.422	.065	.028	.095	-.064	.081
	Change in the log of adult-equivalent income	-.003	.030	.286	.040	.027	.115	-.079	.080
X	Fraction of households headed by females	.553	.016	.405	.572	.015	.102	.506	.038
	Age of the household head	48.596	.453	.102	49.668	.462	.657	5.236	1.278
	Education of the household head in years	7.764	.141	.000	6.822	.126	.591	6.638	.315
	Change in the dependency ratio	.015	.015	.637	.005	.014	.431	.037	.040
	Change in the log of total household size	.004	.009	.109	-.016	.009	.308	-.040	.022
Wealth Index	Illiquid wealth index	.144	.046	.007	-.043	.049	.002	-.445	.105
	Liquid wealth index	.353	.057	.000	-.254	.051	.972	-.249	.131

Table A.2: Percentage of households experiencing morbidity by affected status

Wave	Affected	Non-affected
1	67.80	18.24
2	51.71	11.95
3	45.54	6.67
4	42.23	5.33
5	42.03	19.59
6	36.18	21.68
Mean	**47.58**	**13.91**

Note: Households reclassified after wave one due to their showing HIV-related illnesses are included in the 'affected'-category.

Table A.3: Relative importance of different expenditure categories for affected and non-affected households

In % of total regular expenditure	Affected households	Non-affected households
Monthly food expenses	40.92	34.24
Monthly education expenses	4.84	7.16
Monthly health care expenses	6.20	6.84
Monthly household maintenance expenses	17.63	15.67
Monthly transport expenses	5.92	8.70
Monthly clothing expenses	4.79	6.06
Monthly rent expenses	1.47	1.24
Monthly personal expenses	5.44	6.56
Monthly durable expenses	11.85	12.49
Consumption of home-grown vegetables/fruit	0.78	1.00
Consumption of own poultry or livestock	0.17	0.06

Table 1: Test of risk sharing for affected and non-affected households: Total expenditure

Variable	(1) OLS	(2) QUANTILE REGRESSION	(3) IV-REG	(4) OLS – ADULT EQUIVALENT
Constant	-.051	.096	-.036	-.018
	(.157)	(.171)	(.167)	(.157)
Change in log income of non-affected households	.333 ***	.303	.182	.345 ***
	(.032)	(.031)	(.888)	(.033)
Change in log income of affected households (additional effect)	.025	.011	.147	.018
	(.047)	(.040)	(.591)	(.049)
Time * Location Dummies (probability value for F-test)	5.87 ***	2.47 **	4.63 ***	5.25 ***
	(.000)	(.043)	(.001)	(.000)
Female headed household	-.016	-.038	-.021	-.010
	(.030)	(.033)	(.031)	(.030)
Age of the head	.002	-.002	.001	.000
	(.006)	(.006)	(.006)	(.006)
Age square of the head	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)
Years of education of the head	-.004	-.007	-.003	-.004
	(.004)	(.004)	(.005)	(.004)
Change in the log of the number of household members	.177 **	.148 **	.155 **	-
	(.070)	(.068)	(.079)	-
Change in the dependency ratio	-.061 *	-.056	-.061 *	-.073
	(.036)	(.040)	(.040)	(.037)
Adjusted R^2	.215	.097	.210	.211
Number of observations	1561	1561	1516	1562

Note. Figures in parentheses are standard errors. Coefficients on community*time effects are not reported. Standard errors given in parentheses under the coefficient estimates.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 2: Test of risk sharing for affected and non-affected households: Food consumption

Variable	(1) OLS	(2) QUANTILE REGRESSION	(3) IV-REG	(4) OLS – ADULT EQUIVALENT
Constant	-.097	-.110	-.046	-.028
	(.148)	(.116)	(.152)	(.041)
Change in log income of non-affected households	.238 ***	.236 ***	.005	.247 ***
	(.028)	(.020)	(.213)	(.027)
Change in log income of affected households (additional effect)	.042	.017	.332	.044
	(.041)	(.027)	(.309)	(.041)
Time * Location Dummies (probability value for F-test)	4.66 ***	4.56 ***	4.20 ***	4.58 ***
	(.000)	(.000)	(.000)	(.000)
Female headed household	- .010	.025	-.017	-.011
	(.029)	(.022)	(.031)	(.029)
Age of the head	.001	.001	-.000	-.000
	(.005)	(.004)	(.005)	(.005)
Age square of the head	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)
Years of education of the head	-.001	-.001	-.001	-.002
	(.004)	(.003)	(.004)	(.004)
Change in the log of the number of household members	.127 *	.116 ***	.152 *	-
	(.068)	(.045)	(.084)	-
Change in the dependency ratio	-.007	-.016	-.001	-.028
	(.040)	(.026)	(.040)	(.041)
Adjusted R^2	.16	.07	.11	.16
Number of observations	1523	1523	1478	1527

Note. Figures in parentheses are standard errors. Coefficients on community*time effects are not reported. Standard errors given in parentheses under the coefficient estimates.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 3: Test of risk sharing for affected and non-affected households: Regular expenditure

Variable	(1) OLS	(2) QUANTILE REGRESSION	(3) IV-REG	(4) OLS – ADULT EQUIVALENT
Constant	-.076	.146	-.032	-.070
	(.262)	(.315)	(.265)	(.257)
Change in log income of non-affected households	.242 ***	.240 ***	.379	.247 ***
	(.045)	(.054)	(.380)	(.045)
Change in log income of affected households (additional effect)	.119 *	.137 *	.126	.141 **
	(.064)	(.072)	(.550)	(.064)
Time * Location Dummies	5.95 ***	3.19 ***	6.06 ***	4.29 ***
(probability value for F-test)	(.000)	(.000)	(.000)	(.000)
Female headed household	-.052	.003	-.055	-.033
	(.049)	(.060)	(.051)	(.047)
Age of the head	.005	-.004	.003	.002
	(.009)	(.011)	(.009)	(.009)
Age square of the head	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)
Years of education of the head	-.009	-.014 *	-.008	-.005
	(.006)	(.008)	(.007)	(.006)
Change in the log of the number of household members	.167	.091	.056	-
	(.110)	(.122)	(.121)	-
Change in the dependency ratio	-.113	.011	-.121 *	-.131 **
	(.066)	(.072)	(.069)	(.066)
Adjusted R^2	.12	.06	.10	.12
Number of observations	1461	1461	1419	1464

Note. Figures in parentheses are standard errors. Coefficients on community*time effects are not reported. Standard errors given in parentheses under the coefficient estimates.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 4: Robustness Checks and Extensions applied to regression on regular expenditure

	(1)	(2)	(3)		(4)	
Variable	Mortality	No mortality	Non-poor households	Poor households	Hhs without orphans	Households with orphans
Constant	.030	.042	-.107	.421	.149	-.188
	(.253)	(.267)	(.334)	(.419)	(.293)	(.513)
Change in log income of non-affected households	.299 ***	.254 ***	.249 ***	.229 ***	.258 ***	.195 *
	(.034)	(.044)	(.055)	(.076)	(.049)	(.100)
Change in log income of affected households (additional effect)	.064	.084	.034	.178 *	.049	.281 **
	(.142)	(.066)	(.083)	(.100)	(.072)	(.133)
Female headed household	-.052	-.070	-.118 **	.048	-.050	-.089
	(.048)	(.050)	(.058)	(.087)	(.057)	(.099)
Age of the head	.004	.005	.011	-.011	.002	.007
	(.009)	(.010)	(.013)	(.014)	(.010)	(.019)
Age square of the head	-.000	-.000	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Years of education of the head	-.010	-.010	-.009	-.024 *	-.019 **	.010
	(.006)	(.006)	(.007)	(.014)	(.008)	(.011)
Change in the log of the number of household members	.165	.170	.123	.204	.159	.183
	(.110)	(.115)	(.124)	(.193)	(.127)	(.230)
Change in the dependency ratio	-.106	-.097	-.105	-.132	-.134	-.088
	(.066)	(.068)	(.077)	(.121)	(.087)	(.100)
Adjusted R^2	.098	.097	.090	.129	.097	.139
Number of observations	1461	1368	905	556	983	478

Note. Figures in parentheses are standard errors. Coefficients on community*time effects are not reported. Standard errors given in parentheses under the coefficient estimates.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

[1] See, for example, Jayne and Yamano (2005); Ainsworth, Beegle, and Koda (2005); Jayne and Yamano (2004); or Lundberg, Over, and Mujinja (2000).
[2] Affected households for the purpose of this study constitute households with at least one known HIV-positive member. We call the control group "non-affected households", being aware that these households may a) have a seropositive person who did not show HIV-related symptoms at the beginning of the observation period, leading to a contaminated control group, and b) that these households may suffer indirectly from the effects of the epidemic, such as through care for relatives in an affected household.
[3] There is a discussion in the literature on whether the relative propensity to consume out of income changes between two types of households can be interpreted as relative vulnerability. We follow the strand in the literature that argues that the benchmark of full insurance can only be rejected or not but not interpreted quantitatively. I would like to thank the referee for pointing this discussion out to me.
[4] Community-level dummies would correct for community-level health shocks, providing an informal indication as to the degree to which illness brought about by HIV/AIDS possesses covariate characteristics.
[5] Table A1 in the appendix details the frequency of morbidity episodes of affected and non-affected households. This table also serves to support the success of the classification of the households into the two groups, as affected households show a much higher frequency of morbidity episodes.
[6] For a detailed discussion of the dataset see Bachmann and Booysen (2003).
[7] Other studies employing a similar empirical specification include Skoufias and Quisumbing (2005) and Harrower and Hoddinott (2005).
[8] As is typical in the literature, we also assume consumption and leisure to be separable. For more thorough discussions of this point in relation to health shocks see Gertler and Gruber (2002) or Levine, Gertler, and Moretti (2004).
[9] The variables relating to expenditure, income, and health status of household members are reported by the person responsible for the household finances, for problems associated with self-reported variables see the excellent discussion in Strauss and Thomas (1996).
[10] The latter results are not shown but can be obtained from the author upon request.
[13] Result not shown but available from the author upon request.