Methodological issues in the study of the socioeconomic consequences of HIV/AIDS

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Rigorous empirical research on the socioeconomic effects of AIDS is important for developing appropriate strategies to mitigate impacts and ultimately improve living standards. This paper provides a broad overview of the challenges in studying the impact of the epidemic on individuals, households and economies, drawing examples from existing studies. We start with a discussion of macro-economic studies and argue that they reach vastly different conclusions about the impact of AIDS, depending on what parameter assumptions they make. Whereas microstudies could provide insights into some of these parameters and effects, there are many technical hurdles to overcome. We discuss the use of comparator groups, spillover effects, longitudinal datasets and the time horizons of studies. Under scrutiny of these technical requirements, the existing empirical evidence of the impoverishing effects of AIDS deaths on African households seems unexpectedly limited. After many years of study, large gaps remain in the empirical literature with regard to our understanding of the magnitude and heterogeneity of these impacts. We conclude that the literature thus far has not convincingly shown that AIDS is the main contributor to low levels and high inequities of socioeconomic outcomes in Africa. Demand for research on the causal impact of HIV/AIDS on poverty is only increasing with the scaling up of antiretroviral treatment.

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Introduction

Rigorous empirical research on the socioeconomic effects of AIDS is important for developing appropriate strategies to mitigate impacts and ultimately improve living standards. The socioeconomic consequences of the epidemic, however, have proved difficult to assess. Economic impacts are probably easier to define (poverty, income, wealth/assets/savings), whereas social impacts can range widely from physical and mental health to schooling and social networks (including social stigma) to demographic outcomes such as marriage, fertility and migration. There are many routes through which AIDS could affect socioeconomic outcomes. Obviously AIDS-related morbidity and mortality affect the health and life expectancy of an infected person, and these effects can spread to the caregivers or survivors in the household, the extended family, the community and the nation. Families incur direct costs of illness and death (medical expenditures, funeral costs) and indirect costs (loss of labor and income) as well as declines in psychosocial well-being [1]. National economies and governments can also be affected. For example, if civil servants and specialized laborers in certain key sectors of the economy are not easily replaceable, the disease can undermine the economy and reduce the government's service delivery capacity. These effects can be intergenerational. If schooling and health investments in children shrink at present, the economy could be affected for decades.

Much has been written to describe such potential routes of socioeconomic impact [2,3]. These writings are largely theoretical, speculative and anecdotal. After many years of study, there are still large gaps in the empirical literature on the magnitude and heterogeneity of these impacts. These gaps persist partly because of the demanding methodological and data requirements to investigate satisfactorily any of the routes of impact. This paper is not, per se, an overview of existing studies, many of which we cite below; rather, we focus on methodological issues relevant to such studies. Even with this narrow perspective, we cannot exhaustively cover all methods used in the

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vast literature on the socioeconomic impact of AIDS, or do justice to studies of the effects on agriculture, business enterprises, government service delivery and so forth. Rather, we focus on methods relevant to the impact on individual productivity, household assets and income, investments in children and economic growth.

Estimating the macroeconomic impact of AIDS

Macroeconomic projections of the impact of AIDS, by definition, take an economy-wide view of the consequences of the epidemic. They usually focus on implications of the disease for gross income per capita, whereas micro-impact studies inform upon other outcomes such as poverty, child schooling or food (in)security. Neoclassical growth models and computable general equilibrium models developed to study the economic impact of HIV are based on a wide set of assumptions about the impact of AIDS on labor supply by skill or education level, labor productivity, saving and investment rates, and demographic outcomes (morbidity, mortality and fertility patterns). Cross-country regressions to study the impact of HIV/AIDS on economic growth allow researchers to avoid developing large-scale macroeconomic models. They are, however, 'beset with serious problems arising from errors in variables', relating to both outcomes (gross domestic product) and national-level indicators of HIV [4], and results from cross-country growth regressions are potentially sensitive to slight alterations [5].

These studies on the impact of AIDS on economic growth have been characterized as showing mixed results [4], consensus about the negative and substantive effect [6] and a lack of consensus on the link between AIDS and economic growth [7,8]. Despite this inconclusiveness, one study bluntly states 'A survey of recent writings on the interactions between the AIDS epidemic and livelihoods in Africa leaves one with the impression that development practitioners, academics, and even casual observers of developments in Africa are hell-bent on pinning most of Africa's economic stagnation on the AIDS epidemic' [9]. Many of the studies estimate large declines in gross income (or declining growth in income), which are offset in large part by slower population growth. In addition to this effect of smaller population, the per capita income standing of surviving households could theoretically rise through increases in labor scarcity, which, in turn, increase wage rates. A very similar phenomenon has been documented by economic historians studying Britain's black plague [10–12]. Studies that developed macroeconomic models that include such intergenerational effects on human capital accumulation (such as school attainment of orphans) found large negative effects on national per capita income [13–15]. On the other hand, one can also include this effect and nevertheless find that a reduction in population still dominates, resulting in higher per capita consumption possibilities [16].

For the South African economy in particular, several studies estimated the impact of the epidemic on the economy and reached vastly different conclusions [13,17–19]. Why? The underpinnings of the macroeconomic studies are the behaviors and outcomes for individuals affected by AIDS and people with socioeconomic ties to infected persons. These macroeconomic studies thus depend critically on a large set of parameters that describe the microsocioeconomic behaviors of agents in the economy. Here we have, at best, a mixed set of results for some indicators (medical costs and productivity of persons living with AIDS) and very little evidence for others (such as labor supply, savings, investment and fertility of other household members). When evidence from micro-impact studies is lacking, each macroeconomic study must make its own assumptions about key parameters that then influence results. Unfortunately, macroeconomic studies use vastly different methods, making it difficult to understand different findings when the underlying assumptions can also differ. For example, studies may assume that savings are either constant or zero for AIDS-affected households [16,17] or that the productivity loss of AIDS morbidity is zero or otherwise [14,19]. They may assume some or no total factor productivity effect [16,17], as well as a differing magnitude of fertility response to the epidemic [13,16].

The methodology of micro-impact studies of AIDS

Compared with empirical studies of national income, and despite facing challenges in practice, micro-impact studies have appeal because in theory they offer more direct evidence of the impact of AIDS. These studies typically use survey data on infected individuals or 'affected' households to quantify the impact of the disease in terms of various outcome indicators and they can serve several purposes. First, they can inform parameters that underlie projections of economic growth. They can validate the predictions in macroeconomic models. Finally, they inform upon socioeconomic outcomes that the macro models do not normally address, such as human development outcomes, income inequality and poverty. With this brief typology, we highlight the features of an impact study that are critical to assessing its rigor, in particular three distinctions in methodology: definition of the comparator group; cross-sectional or longitudinal design; and time horizon.

Comparator group

A study of the disease's impact ideally asks what would have happened in its absence. This counterfactual is often

captured by defining a comparator group (e.g. households with no prime-aged adult mortality or morbidity) and comparing it with a treatment group (e.g. households with the condition). As very few impact studies are able to determine the cause of the illness and/or death, they are unable to distinguish AIDS-related mortality and morbidity from others. Instead, they focus on age-specific illnesses and deaths, and sometimes illness and deaths with a specific set of reported symptoms. A review of 36 impact studies found that only five had a comparator group for direct comparisons with affected households or persons [20]. This situation may reflect the fact that many impact studies recruit respondents from ongoing health programmes and not from a population-based sample, which would include a comparator group. Studies with no comparator group then must either assume the counterfactual or use an affected household's self-reported counterfactual. Direct costs of illness as measured by medical expenditures and indirect costs such as loss of earnings based on total number of days sick could overstate the impact of an HIV/AIDS illness, because it assumes that the affected person would otherwise have suffered no illness. Household self-reports that a child was taken out of school as a response to an AIDS death assume that the child would have been in school otherwise.

Although the use of comparator groups may avoid these suppositions, it is difficult to identify such groups appropriately. By designating households as affected and non-affected, a study design assumes that the epidemic has no effect on the comparator group. Although extended families and communities are commonly perceived to bear much of the economic burden of the disease, in practice, impact studies using comparator groups cannot measure this 'spillover' effect. They measure economic effects as differences between comparator and treatment groups but in doing so they do not necessarily capture the full impact on the treatment group, because the comparator group was not unaffected. Most impact studies will only measure any effect beyond the spillover effect. Meanwhile, the seemingly unaffected comparator group households may be affected if they help pay medical and funeral costs or work on others' farms. Elderly parents can suffer income loss in the event of the death of an adult child who had not lived in the household but had been offering income support. There can be community or economy-wide effects, such as the depletion of the supply of teachers or government expenditures redirected away from basic services towards the delivery of antiretroviral therapy. If the comparator group is negatively affected then the study will underestimate the impact. If the comparator group is positively affected (e.g. with higher wages, or the ability to purchase cheap land and assets sold under stress) then the effect could be overestimated. Perhaps more worrying is that whereas the use of the comparator group is a methodological imperative, it also seems to exclude any rigorous analysis of family, community and economy-wide effects.

Among those studies of the impact of AIDS using a comparator groups, many refer to HIV-negative individuals or unaffected households and individuals [9,21–23]. Some studies recruit comparator observations from broad catchment areas [23,24] or from neighboring households [25,26]. For example, several studies examined observed differences between orphans and non-orphans in demographic and health surveys, which may misleadingly attribute cause to a parent's death [27,28]. If poorer parents face higher mortality risks, however, then lower relative schooling observed for orphans may capture an education gap that would otherwise exist whether or not they had become orphans. Or a lack of observed differences between orphans and non-orphans may reflect higher HIV prevalence among adults of higher socioeconomic status and then mask an actual negative impact of the loss of a parent. Perhaps contrary to common perception, several studies found such a positive correlation in Africa [29–31].

A more refined approach to constructing a comparator group is propensity score matching, a technique appropriate under two conditions. First, the researcher needs to have at hand a sufficiently rich set of covariates to predict the likelihood of 'treatment' (being HIV positive or households that experience an adult death), but these covariates are not themselves affected by treatment assignment. Second, the researcher must be able to assume reasonably that treatment assignment is independent of the outcomes of interest (medical expenditures, poverty, etc.), conditional on this set of covariates. For example, HIV status must be random among individuals with the same set of observed and measured traits used to predict treatment (such as age and education). If this assumption is valid, propensity score matching can create the observational equivalent of a randomized experiment by matching treatment and comparison observations on the basis of their estimated likelihood of belonging to the treatment group [32,33]. Using this approach, one study found that the impact of HIV status on medical costs and income loss is smaller compared with simple comparison techniques that do not select a comparator group (HIV-negative persons) matched to the treatment sample (HIV-positive persons) on predetermined characteristics such as education and age [24].

Ordinary least squares estimation requires the same exogeneity assumptions as propensity score matching, but also imposes arbitrary functional form assumptions about how HIV status links to outcomes [34]. Like randomized experiments, propensity score matching avoids these additional assumptions; it will exclude treatment (comparator) observations that do not have sufficiently similar comparator (treatment) observations. But if, in doing so, it excludes a non-random sample of treatment observations, then sampling bias may occur. Unless the researchers intentionally balance the observations, ordinary least squares will typically keep these

observations in the sample, whereas they do not, in fact, provide any meaningful variation.

A necessary condition for applying propensity score matching and ordinary least squares methods with a single round of data is that all variables simultaneously determining outcomes and HIV status (morbidity or mortality) are observed. This is a strong assumption. For example, health attitude is unobserved and yet we can easily hypothesize that it determines both the risk of infection and outcome variables of interest such as medical expenditures, income (which depends on health, particularly in developing countries mainly dependent on agriculture) and children's schooling and health.

Longitudinal data

Longitudinal studies offer an array of statistical methods for addressing these confounding factors that can determine both the likelihood of being affected and the outcome indicator of interest [35]. Using data measuring individuals or households before being affected and after treatment, difference-in-difference methods can control for unobserved characteristics that bias propensity score matching and ordinary least squares estimates, provided they are time invariant and additive [21,36,37]. This time invariance assumption may not be as innocent as it seems. Continuing with the example above, if health attitudes determine income growth (as opposed to income level), then even a double difference would yield biased results.

Panel data are not without problems, with concerns over attrition probably most often cited. In large rural household panels designed to study the effects of adult mortality, household attrition rates span a wide range: 22% over 3 years in Zambia [21]; 6% over 2 years in Kenya [36]; and 7% over 13 years in Tanzania [37]. Individual attrition is typically higher, because most household surveys do not follow individual members who migrate out (although there are exceptions [37]). Attrition is of concern if it is perceived to be non-random, such as attrition caused by migration. Adult mortality can significantly increase the likelihood of migration and household dissolution [38,39]. If investigators do not re-interview these households or individuals, biased conclusions may then result.

Time horizon

The final distinction in methodology we note is the time horizon of micro-impact studies. Whereas the economic growth models can make forecasts far into the future (say, up to 2025), the micro-empirical studies cover much shorter horizons. Cross-sectional studies necessarily rely on data reported retrospectively. The perceived reliability of retrospective reporting may dictate the time horizon of the study. The quality of retrospectively reported information about income before a death (say, 5 years before) may be suspect. Longitudinal data are not a panacea; when longitudinal data are available, they have covered 2–4 years, with very few moving beyond a 5-year time horizon [22,37]. If household (or individual) coping strategies can be maintained only for a short period, then most existing studies may underestimate impacts. On the other hand, effects observed in the short-run might dissipate.

Concluding remarks on methodology

Inherent in micro-empirical work is the trade-off among space, time and size. The demographic and health surveys have formed the basis of several studies of socioeconomic impact using dozens of comparable household datasets from different African countries [27,28]. The power of these multisurvey studies, i.e. their broad geographical coverage and sample sizes, comes at a cost: these surveys are not longitudinal and thus can not satisfactorily address the causality question. On the other hand, multitopic household surveys that are longitudinal often have small sample sizes without national coverage. Many studies have a sample size of fewer than 500 households, which is small in terms of statistical power to study low-frequency events such as death [20].

In considering the advantages and disadvantages of alternative study designs, we conclude that the strength of impact studies will come from a continued array of designs rather than emphasis on one single method.

Discussion

Bearing in mind the methodological issues discussed in the previous section, we start this section with a brief overview of evidence from empirical studies of the socioeconomic impact of AIDS in Africa. We focus on studies that are quantitative in nature and attempt to build a comparator group, and again underscore the fact that the studies almost always ignore community cross-household and spillover effects.

Perhaps the empirically most well-established and uncontroversial effect has been found at the individual level and for people at the AIDS stage of the virus. Studies found significant negative effects on the patient's capacity to engage in income-earning activities [23,40].

By contrast, empirical evidence of the impoverishing effects of AIDS deaths on African households is unexpectedly limited. A 2005 review of household studies of the impact of AIDS-related morbidity and mortality on income and expenditure emphasized the gaps in this literature [41]. Several studies have found that affected households are not uniformly poorer than non-affected households [9,21,22]. One study found that effects may be present in the short-run but do not persist in the long-run [37]. In areas with surplus labor, working hours in households might not increase either during illness or after a death [42].

There is an extensive descriptive and speculative literature on orphans, probably largely motivated by strong previous assumptions that the 'orphan' effect would be negative and large; this literature may reflect donor-driven views rather than traditional or local African notions of vulnerability and orphanhood [43]. Perhaps it is surprising, then, that the findings from empirical studies of schooling and health are often mixed [44]. Among the handful of studies utilizing panel data, which allow for inferences of causality, we do find a significant negative impact of parental death on child schooling [45–49]. Those that examine father's and mother's death separately found that maternal deaths are causally linked to decreases in schooling, whereas paternal deaths are not (although they may be correlated with lower schooling). Because of the duration of most studies, they usually assess the impact on the probability of attending school and are unable to measure the impact on completed years of schooling. One study, however, followed children into adulthood and found that maternal orphans lose approximately one year of final schooling [46].

In our reading of the literature on the socioeconomic impact of HIV/AIDS, we did not find overwhelming and conclusive evidence of a large negative impact, nor did we find the opposite. Rather, there is a lack of carefully executed empirical work, partly as a result of a shortage of appropriate datasets. When stringent methodological requirements are applied to existing studies, few survive scrutiny. There are also few studies that explicitly evaluate AIDS interventions. Nevertheless, some patterns emerge. The micro-empirical household survey literature, including the studies cited above, provides evidence of a large effect on infected people's individual productivity, a short-run income effect on the households in which they live and a negative effect on children's schooling after the death of their mother. It must be stressed that these conclusions are, at best, based on a handful of countries and regions. These points reinforce many issues raised by others in the systematic review of existing studies [22].

In conclusion, we offer four points of reflection. First, establishing causality is key to informing policy. Irrespective of any correlation between AIDS and poverty, if there is no causal link, then tackling the epidemic would not be expected to contribute to poverty reduction. In fact, in this scenario they constitute competing budget aims.

Even with evidence of causal effects of AIDS on socioeconomic outcomes, clear policy prescriptions often remain elusive and few studies seem to address the policy relevance of their findings. For example, one of the few robust conclusions from the literature is that maternal deaths causally lower children's school attainment. Yet multiple policy responses exist: offering antiretroviral therapy to AIDS-infected women, scaling up HIV/AIDS prevention campaigns and providing scholarships to maternal orphans.

Third, there is need to study targeting issues more explicitly. Even if some groups face socioeconomic setbacks because of AIDS, a large portion of affected households may remain better-off than unaffected counterparts. For example, in several countries orphans attain higher levels of schooling than non-orphans [27]. Likewise, as noted in studies above, affected households in Africa may not have uniformly lower income than unaffected households. Both findings are consistent with negative impacts of AIDS if affected households otherwise are wealthier or have better educated children. The rationale for targeting these households would then need to be based on efficiency, rather than equity grounds.

Finally, given the empirical evidence that has emerged to date on the socioeconomic impact of AIDS, the epidemic seems unlikely to be a main contributor to extremely low levels and high inequities of socioeconomic outcomes in Africa. In sub-Saharan Africa, approximately 60% of children (65% of boys and 54% of girls) complete primary school, falling far short of the millennium development goal of universal primary education, whereas it is estimated that a far lower 12% of children under 18 years are orphans [50,51]. We cannot attribute the low level of schooling in Africa mainly to the epidemic insofar as it is manifested by this rate of orphanhood. Moreover, poverty alleviation in sub-Saharan Africa should direct substantial efforts towards relatively low-prevalence countries. Of the 18 poorest countries in sub-Saharan Africa (gross national income per capita at or below US$350), 11 (comprising more than 70% of this population of 324 million) have an HIV prevalence rate below 5%, and they account for two-thirds of the population living in poverty (under US$1 per day). Even with successful efforts to eliminate HIV/AIDS in Africa, combatting poverty and poor human development outcomes are likely to remain major challenges.

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