HIV Treatment and Labor Supply in Rural South Africa *

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Abstract

Antiretroviral therapy (ART) reduces HIV-related morbidity and mortality and has been shown to improve the productivity of HIV-positive workers. However, little is known about the impact of HIV illness and recovery (with ART) on labor supply of HIV patients and their households in Southern Africa, the epicenter of the HIV pandemic and a setting with slack labor markets. This paper assesses the impact of HIV treatment on labor supply in an area of rural South Africa with very high HIV prevalence (28% among adults). Twelve years of longitudinal population-based surveillance data on nearly 50,000 working age adults and their households were linked to clinical records from the government ART program that serves this population. To construct a plausible counterfactual, HIV patients were matched to controls on employment and other characteristics 3-5 years before ART initiation and the matched sample was followed up over time. Three-to-six years after ART initiation, employment among HIV patients had recovered to 68% of levels observed in matched controls and 85% among survivors. Conditional on losing work, jobless spells were long; however, HIV patients on ART were not significantly disadvantaged in regaining employment relative to matched controls. Further welfare gains for people with HIV could be attained from recruitment into care prior to job-threatening illness. Employment declined for female household members of HIV patients in the

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last two years before ART and then recovered after treatment initiation, consistent with reduced care-giving burdens. **JEL Classifications: I10, J22, J24, 015**

1 Introduction

Since 2004, South Africa has provided antiretroviral therapy (ART) for HIV through public sector clinics and hospitals, with the goal of universal treatment access [1]. Six million South Africans are infected with HIV [2], and over three million are projected to be on ART by 2020 [3]. Access to ART has transformed HIV from a death sentence to a managable chronic disease. People with HIV who initiate ART early in their illness have a life expectancy approaching that of people who are not HIV-infected [4]. Whereas clinical recovery of HIV patients on antiretroviral therapy (ART) is well documented [5, 6], less is understood about the economic impact of ART on HIV patients and their households [7].

A sexually-transmitted disease, HIV predominantly affects working age adults. Advanced HIV-related illness is associated with sharply reduced energy levels, wasting, and fatigue [8]. Workers who suffer HIV-related illness have lower productivity [9], lower capacity for manual labor [10], higher rates of absenteeism [11, 12, 13, 10], and many stop working altogether [12, 14, 15]. In addition to its health benefits, ART may reverse these productivity declines and shield HIV-infected workers and their households from economic losses associated with lost employment income [16, 17], medical expenditures [18], caregiving burdens [19], and funeral costs [20, 21]. ART may also raise survival expectations, which could lead to greater labor supply [22].

This paper investigates the effect of ART on labor supply of HIV patients and their households using data from a large population surveillance system in rural KwaZulu-Natal, South Africa, an area where 28% of adults are HIV-positive [23]. The extent of labor supply recovery for HIV patients on ART has not been established for patients in low-employment contexts typical of southern Africa, the region with the world's highest HIV rates [2]. This paper contributes to the literature on labor market returns to investments in "health capital" [24, 25, 26, 27]. It also relates to the literature on labor supply and life events such as childbirth [28], job loss [29, 30], disability [31], or the illness of a household member [32]. HIV illness and ART initiation are commonplace events in HIV-endemic regions. Understanding the impact of ART on labor supply has practical implications for prospective patients, clinicians, and policy makers seeking to maximize the benefits of ART for intended beneficiaries, as well as for the integration of ART programs with social protection and active labor market programs.

Several firm-based studies have documented recovery in productivity following ART initiation among HIV-positive workers in Cote d'Ivoire [33], Kenya [13, 10], and Botswana [11]. Gains in productivity have typically occurred within the first year after initiation and have largely tracked improvements in patients' immunological health. These samples, however, are highly selected, consisting of strongly-attached workers at firms with workplace HIV treatment programs, and may not reflect the experience of the average HIV patient in the general population. Less is known about the recovery of productivity for HIV patients receiving care in public sector treatment programs, where problems of adherence and retention may be more pronounced and where there are no financial incentives to monitor productivity.

Even if productivity gains are anticipated, recovery of labor supply and employment may not necessarily follow. In a basic labor supply model, individuals choose to work if they receive wage offers in excess of their reservation wage. Wage offers are received at a rate that depends on search effort, and individuals look for work if the expected payoffs of search exceed its costs [34]. Whereas wage offers are a function of productivity, other factors associated with HIV illness or ART may raise an individual's reservation wage or the costs of search and thereby reduce labor supply. For example, treatment side effects or lingering illness may raise the utility costs of work even if they do not affect productivity [35]. Patients may view clinical HIV care and the social support of their households as local amenities which make labor migration less attractive [36]. The time required for monthly clinic visits may present barriers to full time work [37]. And although it is illegal in South Africa, people with HIV may fear discrimination, particularly from former employers who may have witnessed their declining health.

In addition to factors associated with HIV and ART, the extent to which productivity gains translate into labor supply for HIV patients on ART may depend critically on labor market context. South Africa's unemployment rate is among the highest in the world, at 26.7% of the labor force; just 41.9% of working age adults are employed [38]. One contributing factor to low employment rates is the geographic segregation of many black South Africans in rural areas far from employment opportunities, a legacy of Apartheid-era policies. A poorly educated workforce, skill-biased technical change, and wage rigidities have also contributed to a mismatch between labor supply and demand.

Previous clinical studies have found significant gains in employment among patients following ART initiation in Kenya [39], India [40], and elsewhere in South Africa [8, 41]. However, since these studies enrolled patients only after they sought medical care, it is difficult to interpret these estimates. First, the time costs of care-seeking imply that this decision may be endogenous to employment status. The result is an identification problem akin to the Ashenfelter Dip in the job training literature [42], in which treatment effects measured from the date of program enrollment cannot be disentangled from the natural churning of the workforce (mean reversion). Second, the depth of any HIV-related employment shock prior to ART initiation depends on how sick a person is before she seeks care. As ART programs expand, the marginal patient will initiate treatment in better health, and the "gains" in employment following initiation would likely decline (even though the effect of ART had not changed). In addition to these issues, only one clinical study - and none in southern Africa - has controlled for secular trends in employment opportunities in the larger community [39]. To date, no study has rigorously evaluated the impact of ART on employment and labor force participation of HIV patients in the slack labor market context that prevails in southern Africa.[1]

Whereas existing evidence on ART and labor supply derives from clinical cohorts [39, 40, 8, 41] and firm-based studies [11, 13, 10, 33], this study analyzes the recovery of labor supply among ART initiators in a population-based surveillance system. Twelve years of data on employment and reasons for non-employment (2001-2012) were collected for all adults in a 432 km^2 community in rural KwaZulu-Natal. Data were collected by the Africa Centre for Health and Population Studies (Africa Centre), a research center affiliated with the University of KwaZulu-Natal and funded by the Wellcome Trust. Population surveillance data were linked with clinical data on those cohort members that sought care in the public sector ART program [43].

These data have several strengths. They include HIV patients with varying attachment to the labor force; they enable adjustment for community-level trends in labor market opportunities; and they are robust to clinical attrition, job loss, and seasonal migration. Most importantly, with baseline data observed years before any job-threatening HIV illness and the decision to seek clinical care, I am able to estimate a meaningful counterfactual: levels of labor supply that would have existed for HIV patients receiving ART had they never acquired HIV in the first place. This counterfactual is an upper bound on what one might expect from the broad class of clinical therapies that seek to remove a health limitation and restore a person as close as possible to their former health status (e.g., as opposed to performance enhancing drugs).

Labor supply decisions of HIV patients may be made in the context of household wide decisions regarding labor supply. The effect of HIV illness on labor supply of other household members is theoretically ambiguous. Market labor supply of other household members may increase to compensate for employment loss of persons with HIV illness or for medical costs of opportunistic infections (income effect). Market labor supply may also increase due to a cross-substitution effect if persons with HIV take on home production tasks that were previously conducted by other household members [44]. In contrast, market labor supply of household members could decline if the added care giving burden at home raises reservation wages for market employment, or if income losses make job search relatively more costly [45]. How households adapt to HIV illness and how they reoptimize as the health of an HIV-positive member improves with treatment will have implications for the welfare effects of ART. If households are able to smooth the economic shock associated with HIV illness, then the economic benefits of ART may be lower; conversely if HIV illness imposes substantial care giving costs, then

[1]An earlier version of this study was published as Bor J, Tanser F, Newell M, Bärnighausen T. (2012). In a study of a population cohort in South Africa, HIV patients on antiretroviral therapy had nearly full recovery of employment. *Health Affairs, 31(7),* 1459-69. This paper has now been revised to include two additional years of data, additional outcomes, and evidence on household spillover effects.

the economic benefits of ART for households may exceed the direct benefits experienced through greater earnings of people with HIV. As a caveat, this analysis does not estimate community-level spillover or general equilibrium effects, although this is an important area for research [46].

To forecast the results, I find that by 3-6 years after initiation, employment recovered to 68% of levels for matched controls, and to 85% in analysis limited to survivors. HIV patients who lost work prior to initiation faced long jobless spells; however, these jobless spells were not significantly longer than those of matched controls, suggesting that the scarcity of job opportunities rather than discrimination, area amenities, lingering health problems, or government grants explains the delayed recovery of employment. Further welfare gains could be obtained from recruiting people into care prior to HIV-related job loss. Among other household members, employment levels of female household members declined in the two years prior to ART initiation, and then recovered, slowly, but completely, a pattern consistent with the care-giving hypothesis.

This paper proceeds in 7 parts. Section 2 provides a description of the data and study context. Section 3 describes the empirical approach. Section 4 describes trends in labor supply for HIV patients and presents causal estimates of long-run recovery. Section 5 investigates the duration of non-employment spells for ART patients. Section 6 analyzes the effect of HIV and ART on the labor supply of other household members. Section 7 concludes.

2 Data description and study context

2.1 Africa Centre Demographic Cohort

Since 2000, the Africa Centre has collected longitudinal demographic data on over 100,000 people living in a 432 km^2 demographic surveillance area (DSA) in uMkhanyakude District, in northern KwaZulu-Natal (Figure 1). The DSA includes both rural areas that were a designated Zulu "homeland" area during Apartheid and urban/periurban areas that formerly constituted a black-only township. The legacy of such segregation can be seen in the population's demographics: population of the DSA is nearly exclusively black and isiZulu-speaking. The population of the DSA is quite poor [43], with high rates of unemployment and government assistance [45]. In 2011, 28% of adults were HIV positive [23].

Data are collected by Africa Centre field teams every six months on basic household demographics, place of residence, and vital status of members, with household response rates of $> 99\%$ [43]. Surveillance began on 1 January 2000 and continues to the present; end of follow-up for this study was 1 July 2012. Every 1-2 years, a socioeconomic survey is added, with questions on employment status, type of employment, and reasons for unemployment. Socioeconomic data were collected in 2001, 2003/4, 2005, and annually from 2007-2012. HIV biomarkers were collected from 2003-2011, although refusal rates were high.

To capture the complexities of household living arrangements in South Africa, the Africa Centre distinguishes between place of residence and household membership. Many individuals reside outside the DSA, but continue to be members of a household in the DSA, returning on weekends or holidays and often sending remittances. At any given point in time, about one third of adult cohort members resided outside of the surveillance area. Socioeconomic data are reported by household proxy, typically by the head of household. The ability to track the employment status and other socioeconomic characteristics of non-resident household members is a major strength of these data, as they are relatively robust to the high rates of circular migration that exist in rural South Africa.

[Figure 1 about here.]

2.2 Hlabisa HIV Treatment and Care Programme

In 2004, the Africa Centre collaborated with the South African Department of Health to launch an HIV care and treatment treatment program at government facilities in Hlabisa sub-district. The Hlabisa HIV Treatment and Care Programme has received financial support from the United States Government, via the Presidential Emergency Programme for AIDS Relief (PEPFAR). The first patients were initiated in July 2004 at the referral hospital in Hlabisa; by 2010 the program had been rolled out to all 17 public sector primary health care clinics in the subdistrict [47]. To date, over 13,500 patients have been initiated on ART in the Hlabisa Care and Treatment Programme.

The Africa Centre DSA lies in the southern third of the Hlabisa health services catchment area (Figure 2), and approximately 40% of patients in the Hlabisa HIV Treatment and Care Programme reside in the DSA [47]. All households residing in the Africa Centre DSA reside within the catchment area of the program. With ART available in the public sector free of charge, private sector provision of ART is very rare in the DSA (personal communication, Dr. Kevi Naidu, Clinical Programme Leader, Hlabisa HIV Treatment and Care Programme, 12 April 2011).

[Figure 2 about here.]

Patients typically enter the Hlabisa HIV Care and Treatment Programme through voluntary counseling and testing services provided at the clinics. The ART program follows Department of Health guidelines regarding ART eligibility and treatment regimens [48]. Adults are initiated on ART for WHO stage IV HIV disease or a CD4+ lymphocyte count of < 200 cells/μL. Prior to initiation, patients are required to attend treatment literacy and adherence counseling sessions, in which they are encouraged to disclose their status to family members and to maintain adequate nutrition. CD4 counts are monitored semi-annually for both those on treatment and those not yet eligible [47]. Once initiated on ART, maintenance in the program is very high, with only 3.7% of patients lost to follow-up in the first 12 months [49]. Largely as a result of the ART program, AIDS-related mortality has declined precipitously [50]; adult life expectancy increased by 11.3 years between 2003 and 2011 [51].

2.3 Data linkage and human subjects

Under an agreement with the Department of Health, clinical data from the Hlabisa HIV Care and Treatment Programme were regularly entered into the Africa Centre's ART Evaluation and Monitoring System (ARTemis) database [47]. Data were collected on all CD4 test dates and counts, and dates of ART initiation. Data in ARTemis were linked with demographic and socioeconomic surveillance data from the Africa Centre Demographic Information System (ACDIS). Individuals were matched by dedicated Africa Centre personnel according to their unique South African identification number, or by first name, surname, age, and sex. Some patients who resided in the DSA could not be matched (Type-II error), due to data entry errors or use of different names in different settings. Previous analysis (conducted in 2009) found that 26% of patients who reported living within the DSA could not be matched to ACDIS [52]. Strict requirements for a successful match are conservative, but ensure negligible Type-I error.

Patients provided written informed consent to use their clinical records for anonymous research. Ethics approval for data collection, linkage, and use was obtained from the Biomedical Research and Ethic Committee of the University of KwaZulu-Natal, in agreement with the Research Office of the KwaZulu-Natal Department of Health. The current study, which involves the secondary analysis of previously collected, de-identified data was deemed "not human subjects research" by the Harvard School of Public Health Office of Human Research Administration.

2.4 Study population and exclusion criteria

For this analysis, the study population was defined as all working age adults (18-59 years) who resided in the DSA at some point between the start of surveillance and 1 July 2004 and who were under surveillance at some point after 1 July 2004. Individuals were under surveillance so long as they were members of households in the DSA, regardless of their place of residence. Individuals who did not reside in the DSA before the start of the ART program were excluded because non-resident cohort members in need of ART would be unlikely to initiate ART in the community unless they had very poor employment prospects. Previous studies have documented substantial in-migration in response to ART provision [36]. Excluding never-resident members reduces possible selection bias, although any residual bias would be expected to lower my estimates of labor supply recovery. Individuals who exited the cohort prior to July 2004, whether through death or because they ceased to be members of households in the community, were excluded because they would have been very unlikely to initiate ART in the community had they needed ART.

Individuals in the study population were considered "HIV patients initiating ART" if they initiated ART in the Hlabisa HIV Treatment and Care Programme between the start of the program in July 2004 and the end of follow-up. End of follow-up was defined as the last visit date when an individual was observed in the population surveillance or the date when an individual died or ceased to be a member of a household under surveillance.

From 2000 through 2012, a total of 148,914 individuals of all ages were ever under surveillance. Of these, 78,917 resided in the DSA at some point between 1 Jan 2000 and 1 July 2004 and were under surveillance beyond 1 July 2004. Over 99% of these individuals were observed at least once in the socioeconomic survey. Of these 78,358 individuals, 64.9% or 50,821 were ages 18-59 years in at least one survey wave. I further restrict the analysis to the 49,874 individuals (98.1% of those age-eligible) who contributed at least one valid employment status observation. The dataset included 249,699 employment status observations, contributed by 49,874 individuals, of whom 6425 sought clinical care for HIV in the public sector treatment program and 3703 initiated ART while under observation in the population surveillance system.

2.5 Labor market outcomes

This paper focuses on extensive-margin indicators of labor supply: employment, non-employment because of illness, labor force participation, and self-employment vs. wage-employment. The reasons for this emphasis are threefold. First, extensive-margin adjustments are likely to be very important in this labor market context. In rural South Africa, high union density and minimum wage laws maintain formal sector wages above market clearing levels. The result is an employment lottery, which disincentivizes intensive margin adjustments for the employed and renders such adjustments impossible for the non-employed. Surprisingly, informal sector employment is quite rare in South Africa [38], in contrast to many other developing country settings. Second, since data on all household members were collected via proxy report from the head of household or another household respondent, data on extensive-margin labor market outcomes are likely to be much more reliable than continuously measured hours. Finally, information on wages, hours, and occupation were not collected beyond 2006, so I am constrained by available data.

Employment status was derived from two questions. From 2003-2006, proxy respondents were asked: "Does __ do anything to earn money? e.g. having a job, working on a farm, doing domestic work, selling things, casual labor, odd jobs, or any other activity to make money." In 2001, and from 2007-2012, respondents were asked, simply: "Is __ in employment? If so, is it part-time or full-time?" These questions were combined to create a variable, "employed", which took the value 1 if __ received any income or was employed part- or full-time, and 0 otherwise. From 2003-2012, respondents were additionally asked whether __ was self-employed (including informal sector work) or was paid by an employer. The overall employment measure is designed to capture all work to earn money, whether part-time or full-time work, self-employment or wage-employment.

For 2003-2012, respondents were also asked the follow-up question: "If __ is not doing anything to earn money, then what is __ doing?" I created an indicator variable, "unemployed due to illness", taking the value 1 for individuals who were not working because they were sick or injured, and 0 otherwise. I also created an indicator variable for "labor force participation", taking the value 1 if individuals were employed or were reported to be looking for work, and 0 otherwise.

For all socioeconomic survey visits, 2001-2012, I also created an indicator for whether or not __ resided in the DSA on that visit date, using data on continuously-measured residency episodes collected via semiannual demographic surveillance.

2.6 Economic context

Table 1 presents data on the economic activities of the adult population (18-59 years), including resident and non-resident cohort members. Data on occupation and work location of the employed were collected between 2003 and 2006, and only data from these years are included in Table 1. The employment-population ratio was low in this population, with just 41% employed. This is comparable to the employment-population ratio for South Africa as a whole, which was 41.9% of 16 to 64 year olds in 2005 [38]. There was substantial variation by age and sex. Fewer than 30% of women under 35 were employed; in contrast 65% of men 35-59 were employed. Among the employed, occupations were quite gender-segregated, with men most likely to be laborers, security guards, taxi drivers, and artisans, and women most likely to work as teachers, nurses, domestics/cleaners, in sales, or as self-employed vendors. Rates of self-employment were low, given the large proportion of adults without a job. Men were much more likely to work in another province (usually Gauteng), however, labor migration within KwaZulu-Natal was common among both men and women. Across all sex and age groups, the probability of employment was 20-30 percentage points higher among individuals residing outside the community, suggesting a close link between location and labor supply decisions. Individuals who would go on to seek treatment in the ART program were largely similar to the general population, but were more likely to live and work in the surveillance area.

[Table 1 about here.]

It is worth highlighting the vulnerability of households in this community to the loss of employment income. In spite of being a largely rural area, home agricultural production far from universal, and most households purchase a large proportion of their food. Questions on home production and consumption were discontinued after 2006. However, in 2005 and 2006, only 37% of households reported growing crops. In contrast, households spent an average of R504 per month on food shopping, about US$75 in 2006 exchange rates. The average household size in the community is 7.7 members, including both resident and non-resident members, and households are often dependent on a single working income: 58% have zero or one individual who is employed; 80% of households have two or fewer working incomes. Although government grants do play a significant role, only 29% of households receive an Old Age Pension. The loss of employment income due to HIV illness may have devastating effects on households.

2.7 Recovery of immune health on ART

Before turning to the impact of ART on labor supply, I first describe the health impact of ART among patients in the Hlabisa HIV Treatment and Care Programme, as this is the chanel through which we would expect any recovery of productivity and labor supply.

HIV weakens the immune system by infecting and destroying white blood cells with a CD4 receptor (CD4+ lymphocytes), making the person who is infected vulnerable to a wide range of opportunistic infections and tumors. Untreated HIV infection is characterized by a latency period of 5-10 years [53], followed by a period of rapidly declining health and ultimately death [54]. A patient is said to have AIDS once the concentration of CD4+ lymphocyte cells in their blood falls below a certain threshold or they present with characteristic opportunistic infections. Antiretroviral drugs interrupt replication of the HI virus, reducing viral load. Standard treatment regimens combine several (usually three) antiretroviral drugs, in order to prevent the development of drug-resistant mutations. Recovery is often swift, with patients regaining immune function and physical strength within weeks or months. ART dramatically reduces morbidity and mortality rates for HIV patients, with the largest benefits for people who initiate treatment while still relatively healthy [5, 6]. And although patients remain HIV-infected and must continue to take antiretrovirals for the rest of their lives, people with HIV who initiate ART early enough have survival rates that approach HIV negatives [4].

To assess the timing of health recovery in this population, I measured immune function using CD4+ counts, a widely used marker of HIV disease progression and recovery [6]. In contrast to the other outcomes discussed in this paper, which were collected via household survey visits, CD4+ counts were collected via routine clinical monitoring for patients in the ART program shortly before ART initiation and at semi-annual follow-up visits. CD4+ counts much before ART initiation exist only for a highly selected subpopulation of patients.

The analysis of immunological recovery was restricted to those patients who initiated treatment prior to 31 December 2006 and who had a “baseline” CD4+ count in the six months prior to initiation. Nearly all patients (93%) had CD4+ counts taken in the six months prior to initiation. I made the conservative assumption that this was the patient’s CD4+ count on the date of initiation. Thereafter, CD4+ counts were only observed if patients came to the clinic for semiannual follow-up visits. For each patient, I estimated daily CD4+ counts using linear interpolation between consecutive visit dates. Patients who died during follow-up were assumed to have a zero CD4+ count starting at the date of death, as recorded via population surveillance. Patients who were lost to follow-up from the clinic but did not die were assumed to have their last CD4+ count until the end of follow-up. Means and 95% confidence intervals were calculated at six month intervals following ART initiation.

[Figure 3 about here.]

Figure 3 illustrates the trend in CD4+ counts among patients who initiated ART in the Hlabisa HIV Care and Treatment Programme. On average, patients initiated antiretroviral therapy with a CD4+ count of 122 cells per μL, well below the treatment eligibility threshold of 200 cells per μL. Following treatment initiation, immune health recovered rapidly, reaching 235 cells per μL within the first six months. Thereafter, CD4+ counts continued to rise, but at a decreasing rate, with average CD4+ counts over 400 cells per μL at four years. To put these gains in immunological function in perspective, clinical studies have found that patients with CD4+ counts above 200 have dramatically lower risk of adverse health events [55] compared to those with CD4+ counts below that level, and those with CD4+ counts above 500 have survival rates similar to the general population [56].

3 Labor supply of ART patients: methods

3.1 Empirical approach

This paper investigates the extent to which labor supply of HIV patients on ART recovers, relative to levels that would have occurred in the absence of HIV infection. To make this study question more precise: the ideal, though infeasible, experiment would be to assign individuals randomly to an HIV status in the context of guaranteed access and take-up of ART. Such an experiment would clearly be unethical. Moreover, no individual-level instruments for HIV infection exist that would allow quasi-experimental identification of this effect.

I estimate the effect of HIV infection on labor supply, conditional on certain take up of ART. There are two reasons to prefer this approach over a plausible alternative: analyzing the effect of ART on labor supply, conditional on HIV infection. First, ART is already being provided at scale in South Africa and arguments about whether to make treatment available have given way to broad consensus in favor of large scale provision. More relevant policy questions regard the design on clinical programs and integration of ART with social protection and labor market programs. Second, labor supply (and other outcomes) for HIV patients in the absence of ART is well established: following an AIDS diagnosis, health and labor supply decline and death typically occurs within one to two years [54]. Much less is known about the extent to which HIV patients on ART can lead "normal" economic lives.

Establishing the extent of labor supply recovery requires estimating an upper bound to which an HIV treatment program might reasonably strive: the counterfactual level of employment that an HIV patient would have in the absence of HIV. Identifying this counterfactual is the challenge. Underlying risk for HIV infection is unobserved and may be correlated with labor supply. For example, individuals who migrate for work may have higher HIV infection risk than individuals who reside in the community [57]. Cross-sectional comparisons with people from the general population are unlikely to yield valid estimates. However, with longitudinal data, I can difference out fixed characterstics of individuals that may be associated with labor supply, including any time-invariant predictors of HIV risk. Risk of infection per unprotected heterosexual sex act is only about 3 per 1000 [58]; typically, it is long-term behavioral patterns, rather than individual decisions, that determine differences in risk for HIV infection.

To estimate a plausible counterfactual, I matched people with HIV who initiated ART to controls from the surrounding community (excluding known HIV-positives), and followed both longitudinally. Controls were exact-matched to treated on survey year, age, sex, education, baseline employment, place of residence, and other characteristics observed three to five years before ART initiation. This baseline period was chosen as a time when those future HIV patients would not yet have experienced serious illness, would not have sought clinical care, and likely would not even have known their HIV status (in the period I study). The ability to observe people with HIV before they experience illness and seek care is a key strength of this study.

Trends in labor supply outcomes for HIV patients relative to controls were estimated in difference-in-difference models. Since the matched sample is balanced on matching covariates, when I follow this sample longitudinally, these models adjust for secular and age effects as experienced by similar persons in the community. These difference-in-difference models provide covariate-adjusted *descriptive* estimates of the evolution of labor supply for people with HIV who initiate ART.

Can these descriptive estimates be interpreted as causal treatment effects? As mentioned above, short run changes in labor supply around the time of ART initiation are likely confounded by endogenous care-seeking. Some of the employment loss correlated with ART initiation may have nothing to do with HIV illness, but rather reflect a person's choice to seek care only after he lost work. If employment loss is correlated with the decision to seek care, but not causally linked to HIV illness, we would expect a bounce in labor supply following treatment initiation due purely to mean reversion, similar to enrollment in job training programs [42]. Although short run effects may be biased, long-run difference-in-differences estimates yield plausible treatment effect estimates if the following conditions are met. First, the baseline must occur before any change in labor supply correlated with HIV illness or ART initiation. The absence of any pre-baseline trends in employment (discussed below) provides confidence that this is the case. Second, follow-up must occur after the full resolution of any mean reversion. While this is not testable, any lingering effect of endogenous job loss could only bias our estimates of recovery downward. Third, trends in labor supply for controls must be a valid counterfactual for trends that would be observed for people with HIV who initiate ART - had they never acquired HIV. This requries that after matching, there are no unobserved (and unbalanced) factors which are correlated with trends in labor supply. This third assumption is not testable, but it is plausible, and I explore possible violations through sensitivity analyses.

Regression models were used to estimate trends in the following labor market outcomes: employment status, non-employment due to illness, labor force participation, self-employment, wage-employment, and an indicator for whether the individual resided in the DSA. I estimated linear probability models, of the form:

$$Pr(Y_{it} = 1) = \alpha + \beta TSIwindows_{it} + \gamma ART_i + \delta ART_i * TSIwindows_{it} + \epsilon_{it} \qquad (1)$$

for person i observed at time t, where

$$TSIwindows_{it} = \begin{pmatrix} 1[& -8y & \leq & TSI_{it} & < & -5y] \\ & \multicolumn{5}{c}{(reference\ group)} \\ 1[& -3y & \leq & TSI_{it} & < & -2y] \\ 1[& -2y & \leq & TSI_{it} & < & -1y] \\ ... & & & & & \\ 1[& 3y & \leq & TSI_{it} & < & 6y] \end{pmatrix}$$

In this model, TSIwindows is a vector of indicator variables for intervals of "Time since ART initiation" (pseudo-times since initiation were constructed for controls, as described below). Observations occurring more than eight years prior to initiation or more than six years after initiation were excluded due to small sample sizes. The period three to five years prior to initiation was the reference group, and an indicator for five to eight years pre-ART was included to test the hypothesis that the reference group was observed prior to HIV illness. Standard errors were clustered at the individual level to account for intra-individual correlation in the errors [59]. Regressions were weighted such that the baseline distribution of covariates in the controls matched that of the treated (described below). In alternate specifications, I estimated this model as a Probit.

This regression model nests a series of difference-in-differences models, in which the difference between treated and controls is measured at different follow-up times and compared to the difference at baseline. δ is the difference-in-differences estimator. I interpret short run changes in labor supply descriptively, but interpret long run changes as a treatment effect on the treated: the long-run effect of HIV on labor supply, conditional on certain take-up of ART. I calculate long-run recovery of labor supply as the ratio of predicted labor supply for people with HIV receiving ART to the predicted labor supply of matched controls in the period 3-6 years after ART initiation plus any baseline differences.

$$Percent\ recovery = \frac{\alpha + \beta_{3-6\ yrs\ post} + \gamma + \delta_{3-6\ yrs\ post}}{\alpha + \beta_{3-6\ yrs\ post} + \gamma} \tag{2}$$

These estimates should be interpreted as local to people who would take up ART if they were infected with HIV. The results may not be generalizable to individuals who face barriers to accessing ART and would not take up ART if infected with HIV.

3.2 Matching

Prior to matching, two additional restrictions were made on the sample of ART patients, to ensure that the estimated trends are not driven by cohort effects, but instead reflect changes in labor supply over time in the same group of patients. First, ART patients were only included if they were observed in the socioeconomic survey in the baseline period, three to five years before initiation; this reduced the sample to 2951 patients. Second, since the primary endpoint was labor supply at least three years after ART initiation (and six years after the baseline reference period), we excluded all patients whose baseline occurred in 2007 or later, since their primary endpoint could not have been observed by the end of follow-up; this exclusion further limited the sample of ART patients to 1413 people, all of whom initiated ART before July 2010.

Controls were matched as follows. First, a baseline visit date was identified for all ART patients as the latest visit date occurring in the baseline reference period, 3-5 years before initiation, $max(VisitDate| -5\ years < TSI < -3\ years)$. Second, survey observations for controls were matched to ART patients' baseline observations using coarsened exact matching (CEM), in which baseline characteristics were coarsened into categories and observations were matched within unique covariate cells [60, 61]. Control observations were eligible to be matched if the individual was still under surveillance three years later. I excluded from the controls all individuals who were known to be HIV-positive, including: people who sought care for HIV but had not initiated ART by the end of follow-up; all people who were identified as HIV-positive in the Africa Centre's HIV surveillance (2003-2011); and people who died due to HIV, as identified in the population surveillance by verbal autopsy. Observations were matched on survey round, age, sex, education, baseline employment, place of residence, household size, household assets, and the number of incomes in the household. All controls were matched to all treated within strata, and weights were constructed such that the controls matched the baseline covariate distribution of the treated. Finally, the matched baseline observations for HIV patients and controls were merged with the full longitudinal data for each individual.

In order to analyze these data longitudinally on a "Time since ART initiation" scale, controls were assigned pseudo-dates of initiation equal to their baseline visit date plus the average time from baseline to initiation among the treated in that matching stratum (j), $RefVisitDate_{i\in ctrl,j} + avg(DateOfInitiation_{i\in ART,j} - RefVisitDate_{i\in ART,j})$. "Time since ART initiation" was defined at all visit dates, for both HIV patients and matched controls.

The resulting dataset consisted of 1407 ART patients (six were not matched) and 33,378 matched controls (contributed by 19,050 unique individuals). Since ART patients and controls were matched on baseline survey round, controls could contribute to multiple matching strata. Table 2 presents summary statistics for controls, HIV patients, and matched controls. At baseline, ART patients were much more likely to be female, to live in rural areas, and somewhat more likely to be employed than controls. Coarsened exact matching achieved near perfect balance on observables. The proportion of treated and matched controls employed at baseline was 39.4%. Figure 4 displays the distribution of propensity scores (the probability of being an HIV patient conditional on matching strata) for HIV patients and matched controls, with and without weights.

ART patients and matched controls had nearly identical employment rates in the period prior to baseline (34.7% vs. 34.8%, respectively), even though they were not matched on lag-employment. The absence of pre-baseline differences in employment trends between HIV patients and matched controls suggests that the baseline period in fact occurred prior to any job-threatening HIV illness or job-loss correlated with the decision to seek care.

[Table 2 about here.]

[Figure 4 about here.]

[Figure 5 about here.]

3.3 Mortality, attrition, and non-response

Mortality among HIV patients - even on treatment - is likely to be higher than among matched controls. Figure 5 shows the Kaplan-Meier survival curve for HIV patients following ART initiation. About 10% of ART initiators died in the first year after treatment, and about 80% were still alive six years after initiation. Mortality after treatment initiation was much lower for women than for men (hazard ratio = 0.51, z-stat=6.5, as estimated in Cox model).

Attrition and non-response may also be correlated with labor supply and may differ for HIV patients and controls. Although the population surveillance data used in this analysis are robust to clinical attrition, job loss, and temporary migration, individuals may still exit the surveillance if they cease to be members of households in the DSA. Others who were not previously under surveillance may join households in the DSA. Even among individuals under surveillance, employment status observations may be missing because the proxy household respondent did not know, refused to answer, or because of data collection errors.

To assess patterns of missingness due to mortality, attrition, and non-response, I used the following approach. I formed a quasi-balanced panel, imputing missing visit dates for all survey rounds in which a cohort member was age-eligible, but not observed, even if that person had already died or was not a member of a household under surveillance. I then calculated the proportion of "potential observations" that were missing due to attrition from the cohort, mortality, late entry into the cohort, and other sources of missingness.

Table 3 displays patterns of missingness for ART patients and matched controls. Rates of attrition were very low, with only 7.1% (3.4%) of potential observations missing for controls (ART patients) because the respondent had left the cohort. There were about four times as many potential obesrvations lost to mortality among ART patients (6.6%) as there were among controls (1.5%). Just 1% of potential observations were missing because a cohort member had not yet come under surveillance. Finally, about 12% of observations were missing because information on employment was not known, refused, or otherwise missing.

I estimated my primary regression models using only observed data and limiting the sample to survivors. However, due to the potential importance of mortality, attrition, and other forms of missingness, I also estimated a range of employment regression specifications that addressed these possible sources of bias. I imputed employed = 0 for pseudo-observations following an individual's date of death. For missing person-wave observations of surviving individuals (including all non-response and attrition), I imputed using multiple imputation with ten datasets and variance estimates pooled using the Rubin method [62]. The imputation model included the CEM strata, treatment status, time since ART initiation windows, place of residence, age, survey round, and whether the person died within two years.

[Table 3 about here.]

3.4 Potential violations

The identifying assumption in my panel data analysis is that there are parallel trends in outcomes for HIV patients and matched controls. This assumption would be violated if there was some unobserved quality associated with being at high risk for acquiring HIV which is correlated with differential trends in labor supply. Examples might include differential returns to experience or changes in labor demand that are biased towards (or against) high-risk types. As a robustness check, I matched ART patients to other people infected with HIV, as identified through the Africa Centre surveillance. I excluded those who died from HIV from the controls, in order to reduce bias from declining health among HIV-positives.

Another potential source of bias is the selective in-migration of HIV patients described previously. I do not observe cohort members initiating ART outside the Hlabisa catchment area, and migrants who return home to seek care are likely to have worse job prospects than those that initiate ART in the location to which they have migrated. As a robustness check, to further reduce the impact of such selection bias, I included only those cohort members who resided in the DSA for the full six months prior to the July 2004 public sector treatment rollout. In doing so, I excluded cohort members who resided in the DSA at some point between 2000 and 2004, but had weak residential attachment to the DSA.

[Figure 6 about here.]

[Figure 7 about here.]

4 Labor supply of ART patients: results

Before turning to the regression models, I first present graphical results on the evolution of labor supply among ART initiators and matched controls. I estimate local linear regressions, with a bandwidth of four months. For treated observations, I estimate separate regressions before and after the (pseudo) date of initiation, in order to avoid smoothing over effects occurring around the time of treatment initiation.

Figure 6 displays trends in employment for survivors. Outcomes were similar (by construction) for ART initiators and controls at baseline. Of interest, outcomes prior to the baseline period were also quite similar, even though these were not included in matching, suggesting that a) any decline in employment associated with HIV illness or ART initiation had not yet started by the reference period and b) baseline trends were similar for ART patients and controls, giving some confidence in the validity of this counterfactual. Figure 7 displays trends in non-employment due to illness for survivors.

In the year prior to ART initiation, employment declined by about 15% points and there was a sharp rise in non-employment due to illness of about 10% points, from a baseline of 4%. Taking these survey responses at face value, about two-thirds of the employment shock prior to ART could be explained by HIV-related illness. The remaining component of the observed employment shock may reflect the endogeneity of care-seeking to labor market experiences; for example, some patients may have sought care in the Hlabisa ART program only after losing a job as a migrant worker somewhere else.

In the years following ART initiation, employment for survivors increased steadily, recovering completely relative to baseline levels observed 3-5 years before initiation. Relative to the matched controls - the estimated counterfactual - employment recovery was substantial but incomplete, as controls experienced rising employment due to aging and secular trends throughout the study period. After imputing zeros for pseudo-observations after death, the recovery in employment was less pronounced. However, even in these estimates, employment of ART patients recovered to about two-thirds of levels in the matched controls and did not decline further below levels observed at initiation.

Following ART initiation, illness as a cause of non-employment declined rapidly, consistent with the large, clinically important gains in immunological functioning in the first year of treatment reported above. In contrast to other contexts, in which labor supply recovery followed closely on health improvements [11, 39], increases in employment lagged behind observed health gains for HIV patients in this study.

[Table 4 about here.]

[Table 5 about here.]

Table 4 presents regression estimates for trends in employment and unemployment due to illness for survivors. Columns 1 and 3 show crude trends in ART initiators. Columns 2 and 4 adjust for trends in matched controls in difference-in-differences specifications. Coefficients of interest are in the bottom third of the table, which show the difference in employment (or non-employment due to illness) between ART initiators in a particular interval of time since initiation and counterfactual employment rates in that same interval, estimated for matched controls. From a baseline of 39 percent, 3-5 years before treatment initiation, employment fell by 15 percentage points. Three to six years after ART initiation, employment among surviving ART intiators was just 6.7 percentage points lower than employment among matched controls. Non-employment due to illness recovered to baseline within a year after ART initiation.

Regressions for other labor market outcomes are presented in Table 5. As before, these regressions are limited to observed data on survivors. For ART patients, changes in labor force participation after treatment initiation largely coincided with changes in employment. Following ART initiation, recovery in self-employment occurred more rapidly than recovery in wage-employment, consistent with the greater flexibility of the self-employed to adjust working hours given available earning opportunities and constraints, and with the difficulties of finding formal sector work in this low-employment setting. Finally, labor supply decisions are known to be strongly related to location decisions in this context. In the final column of Table 5, I model the probability that an individual resides in the DSA. In the years following ART initiation, patients were more likely to reside in the population surveillance area indicating lower levels migration.

[Table 6 about here.]

Thus far, I have presented regression results based on observed data and for survivors only. Table 6 presents alternative difference-in-difference specifications using approaches that account for mortality and possible biases induced through non-response and attrition. Although I observe all living individuals regardless of whether they are actively enrolled in clinical care, attrition occurs if an individual residing outside the DSA stops being a member of a household or if the entire household out-migrates. Further, although the Africa Centre asks about the employment status of all adult household members, proxy respondents are more likely to report "Don't Know/Missing" if the individual is non-resident. Finally, those who exit the sample through death would likely have been too sick to work had they remained in the sample.

The first column of Table 6 shows trends in the probability that a person had died. By the period 3-6 years after ART intiation, HIV patients were 4.5 times as likely to have died as matched controls, with excess mortality of 12.6% points. Column (2) presents a "data as is" model of employment, including both survivors and respondents who died during follow-up, yet censoring data at death or loss to follow-up. Results are very similar to the survivors-only model. Column (3) imputes zeros for all employment status pseudo-observations occurring after death. Column (4) imputes zeros for deaths and uses multiple imputation to impute for all other missing data. Column (5) imputes for missing data, but restricts the sample to survivors. Accounting for missingness and attrition lowers the differences-in-differences estimator for long run employment (3-6 years post-ART) from -6.7% to -9.8% in the survivor-only models. Just imputing zeros for deaths lowers the coefficient to -12.9%. Accounting for both deaths and missingness (column 4) lowers the coefficient to -14.5%, a decline of about eight percentage points.

[Figure 8 about here.]

[Table 7 about here.]

Figure 8 and Table 7 display employment trends for ART patients and matched controls by sex. Although previous studies have found differential effects in productivity [10] and labor supply by sex [39], employment trends for HIV patients in this population were qualitatively similar for men and women.

Table 8 summarizes the long-run difference-in-difference estimates and calculates "percent recovery", the ratio of employment levels in ART patients to the counterfactual estimated using the matched controls. Panel A summarizes results for models with all respondents. Panel B summarizes results for the survivors sample and subjects this model to additional specification checks. Model (6) presents marginal effects from Probit estimates, which are nearly identical to the results from the linear probability model. Models (7) and (8) assess the robustness of the results to potential violations of the identifying assumption. Model (7) displays results in which HIV patients on ART were matched not to HIV-negatives, but instead to other people with HIV. Model (8) excludes individuals who did not reside in the surveillance area for the first six months of 2004, prior to the start of the ART program. Results across these specification checks are all quite similar to the baseline model for survivors.

In summary, three-to-six years after ART initiation, employment levels among people with HIV recovered to about two-thirds (68%) of levels predicted for these individuals had they never contracted HIV (Model 3, Table 8). Among survivors, employment recovered to 85% of predicted counterfactual levels and to 79% after imputing for missing data (Models 4,5).

[Table 8 about here.]

5 Duration analysis: returning to work after job loss

The panel models identified a sharp decline in employment, followed by substantial but slow and incomplete recovery, which lagged behind observed health gains. This lag is consistent with several explanations, including: incomplete health recovery, side effects of treatment, discrimination from employers, time spent seeking care, dependence on government grants, and difficulties finding work for returning migrants. An alternative explanation is that the lag in employment recovery is simply a product of high background unemployment rates. This section investigates an important, and as-of-yet unstudied question: how do jobless spells experienced by people on ART compare with jobless spells in the general population? Using tools from duration analysis, I investigate whether, conditional on job loss, the time to re-employment is longer for individuals who left work in the three years prior to ART initiation, compared to matched controls. Note that I am unable to determine what proportion of ART initiators in this sample stopped working due to HIV illness vis-a-vis other reasons. Thus, these results should not be interpreted as the expected unemployment duration for someone who loses work due to illness, but rather as the average experience of ART initiators who lost work in the three years prior to initiation.

5.1 Empirical approach

Due to the panel nature of the data, I do not observe exact transition dates into and out of employment. Rather, I observe an interval in which an individual lost his or her job. I then follow these individuals through subsequent survey rounds occurring every 1-2 years, until they are observed to be employed again, or until the last survey round in which they are observed (end of follow-up). Data in which the start and failure times are not observed exactly, but known to fall within an interval, are referred to as "doubly interval-censored" data in the biostatistics literature [63].

I included as "treated" all HIV patients who were observed to be employed in the period 0 to 3 years prior to antiretroviral treatment initiation, and who were subsequently observed to be not employed at some point prior to six months after initiation. I matched these "treated" with controls from the population cohort, who also lost work at a similar time. Controls were exact-matched on the following covariates: sex, education (<12 years, 12+ years), two-year birth cohort, survey year for both the left and right boundaries on the interval containing the transition out of employment, and place of residence when last employed (in vs. outside the surveillance area). Controls were sampled with replacement, with a 5:1 ratio of controls to treated. This approach yielded 401 treated and 2005 control observations, contributed by 1508 unique individuals, with duplicates due to sampling with replacement in small cells. To account for such duplicates, all statistical inference was adjusted by clustering standard errors at the level of the individual.

Tools for the analysis of doubly interval-censored data are not available in routine statistical software. However, by making assumptions about the distribution of origin and failure times within the intervals, the problem simplifies. I estimated the hazard of job loss for the employed and the hazard of job gain for the not employed in the pooled sample. I then sampled employment transition dates assuming a constant hazard in the intervals. Having imputed both origin times and failure times, the problem simplifies to standard survival analysis, in which durations are known exactly or right censored. I present Cox proportional hazard and exponential models based on the doubly imputed data. I also estimated alternate specifications in which jobless spells were not censored at death, akin to imputing zeros for employment after death.

To account for the variability introduced by imputing employment transition dates, I constructed confidence intervals and p-values by simulation. I replicated the following procedure 1000 times: I randomly drew dates of job loss and re-employment; I estimated exponential and Cox models. To incorporate both the sampling and estimation variability, for each model I drew a single point estimate from a normal distribution defined by the regression coefficients and their standard errors from each simulation. I then calculated the average point estimate, identified the 2.5th and 97.5th percentiles, and calculated the probability that the null-hypothesis is true given the observed data (an inverted p-value). The mean point estimates were then exponentiated to obtain the reported hazard ratios.

To visualize the data, I pooled the 1000 simulated samples (with both origin and failure times imputed) and plotted the Kaplan-Meier failure curves for treated and controls. This figure is presented as Figure 9. Beginning from the date of job loss, these curves can be interpreted as the cumulative probability that an individual has regained employment. Note that the low rate of re-employment in the first year after job loss is an artifact of the panel data collection. On average, respondents' employment status is not observed for a full year after job loss. I additionally caution that due to the snapshot nature of data collection, it is possible that there were temporary transitions into and out of employment that occurred between survey rounds and were not observed. However, it is unlikely that this is the case for more than a few percent of workers and the bias on observed jobless spells is likely to be minimal.

[Figure 9 about here.]

[Table 9 about here.]

5.2 Results of duration analysis

Figure 9 presents Kaplan-Meier failure curves for individuals who lost their job within the three years prior to treatment vs. matched controls. This figure includes posthumous person time to account for the non-random censoring of jobless spells by mortality. Conditional on job loss, durations of time without work were very long in this population, with the median time out of work at 3.3 years among matched controls. Surprisingly, ART initiators did not fare much worse than controls in finding employment after job loss. The median length of jobless spells was 3.9 years, only marginally longer than controls. For the typical HIV patient who lost work prior to ART, almost 90 percent of the time out of work could be explained by factors faced by the general population.

Regression results for the exponential and Cox models are presented in Table 9. Censoring individuals at death, people with HIV who lose work within 3 years prior to ART initiation had a nearly identical (HR=.98) hazard of finding work, compared to controls. Including posthumous person-time, ART intiators had an 11% lower hazard of finding work, a result that is marginally statistically significant, but still very small. Cox and exponential models yielded similar results.

This simple duration analysis demonstrates that people who lose work prior to ART initiation were not substantially disadvantaged in returning to work, compared to a matched control population. Further, their slight disadvantage - such that it existed - was driven by higher mortality rates. Although we cannot assume that those who died would have had similar hazards of re-employment to surviving HIV patients, they could only have improved their chances of finding work had they survived. This comparison suggests that targeted active labor market programs to hasten the return to work for people on ART may have limited impact, without broader macroeconomic or fiscal policies to reduce structural unemployment. In contrast, given the length of non-employment spells, earlier recruitment into care and treatment prior to any HIV-related job loss would have large welfare benefits for people with HIV.

6 Labor supply of other household members

This paper has assessed the impact of ART on labor supply of HIV patients. I now turn to an analysis of the effects of HIV illness and ART on labor supply of other household members. Employment loss of an HIV-positive household member is only one aspect of the economic burden placed on households by HIV illness. Households face medical treatment costs for opportunistic infections [18], added caregiving burdens [19], and the prospect of very large funeral costs [20]. ART may enable households to avoid these losses. However, even with ART, households may face significant out of pocket spending on health care [37] and the extent to which earnings recover for HIV patients is uncertain.

In this final section, I assess how households reallocate labor in the face of HIV illness and subsequent recovery on ART. As described in the introduction, other household members may *increase* labor supply during another member's HIV illness due to a negative income effect on leisure, or because of a cross-substitution effect, in which the person with HIV takes on home production tasks (e.g. caring for young children), freeing other household members to supply market labor. On the other hand, other household members may *decrease* market labor supply during HIV illness if the shadow wage of home production increases due to caregiving needs or because the household can no longer afford to finance job search of their members.

To assess the effect of HIV illness and ART on market labor supply of other household members, I constructed an individual level dataset in which working age adults were matched to HIV treatment initiators who were members of the same household. Shared membership was defined at the point in time two years before ART initiation, and individuals were considered to share household membership with this person thereafter. This period two years before initiation was chosen because my analysis of own labor supply found little evidence of significant changes in labor supply or non-employment due to illness prior to one year before ART initiation. By defining shared household memberships with future ART patients at this point in time, my analysis of household labor supply is robust to endogenous recombination of households during the period of acute HIV illness and the years following ART initiation.

I estimated linear probability fixed effects models of employment on time since ART initiation (of a household member), controlling for month and day of survey visit, survey wave, and education-specific age profiles. Analysis was restricted to an individual's first exposure to ART through shared household membership. All ART initiators were excluded from the analysis. I estimated models separately by sex, due to very different roles of men and women in home production in this setting. Standard errors were clustered at the household level in all cases.

The results of these household labor supply models are presented in Table 10. Working age women experienced a significant decline in employment of 1.9 percentage points, from a baseline of 35% employed in the two years prior to ART initiation of an HIV positive household member. Following ART initiation, employment probabilities recovered somewhat for female household members. The timing of this decline and recovery is consistent with the caregiving hypothesis: acute illness raises the shadow wage of home production, pulling some carers out of market employment. For men, there is some evidence of higher employment in all periods after the reference period, however the effect is small not not significant and does not appear to be correlated with the period of acute HIV illness. Any income or cross-substititution effects appear to be either offset by care-giving effects or muted in this context due to slack labor markets. It is also possible that ART substantially insures households against economic costs related to HIV, as many patients initiate treatment before experiencing severe illness.

[Table 10 about here.]

7 Conclusion

This paper has investigated the impact of ART on labor supply for HIV patients and members of their households, in a region of rural South Africa with very high rates of HIV. Among survivors, I find substantial recovery of employment following ART initiation, with employment reaching 85% of estimated counterfactual levels 3-6 years after initiation. Accounting for mortality, employment 3-6 years after initiation was two-thirds of counterfactual levels that would be expected among similar persons who never contracted HIV. In a departure from previous studies in other settings, I find that employment recovery lags several years behind health gains. However, this delay appears to be due to factors faced by the general population, rather than factors associated with HIV illness or ART. In addition to recovery of employment among HIV patients, ART also leads to small but significant gains in employment among female household members.

Antiretroviral treatment does not just prevent death. It enables HIV patients to lead long, productive lives. The extent and timing of employment recovery, however, depends on contextual factors, in particular the availability and location of job opportunities. The high levels of unemployment and labor migration observed in the area of KwaZulu-Natal where this study takes place are common to much of Southern Africa, the region with the world's highest HIV burden. In such settings, further protection against economic losses for people infected with HIV and their households could be attained by initiating patients earlier, prior to any job-threatening illness. In light of recent research on the positive externalities of treatment in reducing HIV transmission, providing information to prospective patients on the private economic benefits of ART could also help achieve socially optimal treatment levels.

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Figure 1: Location of Africa Centre for Health and Population Studies

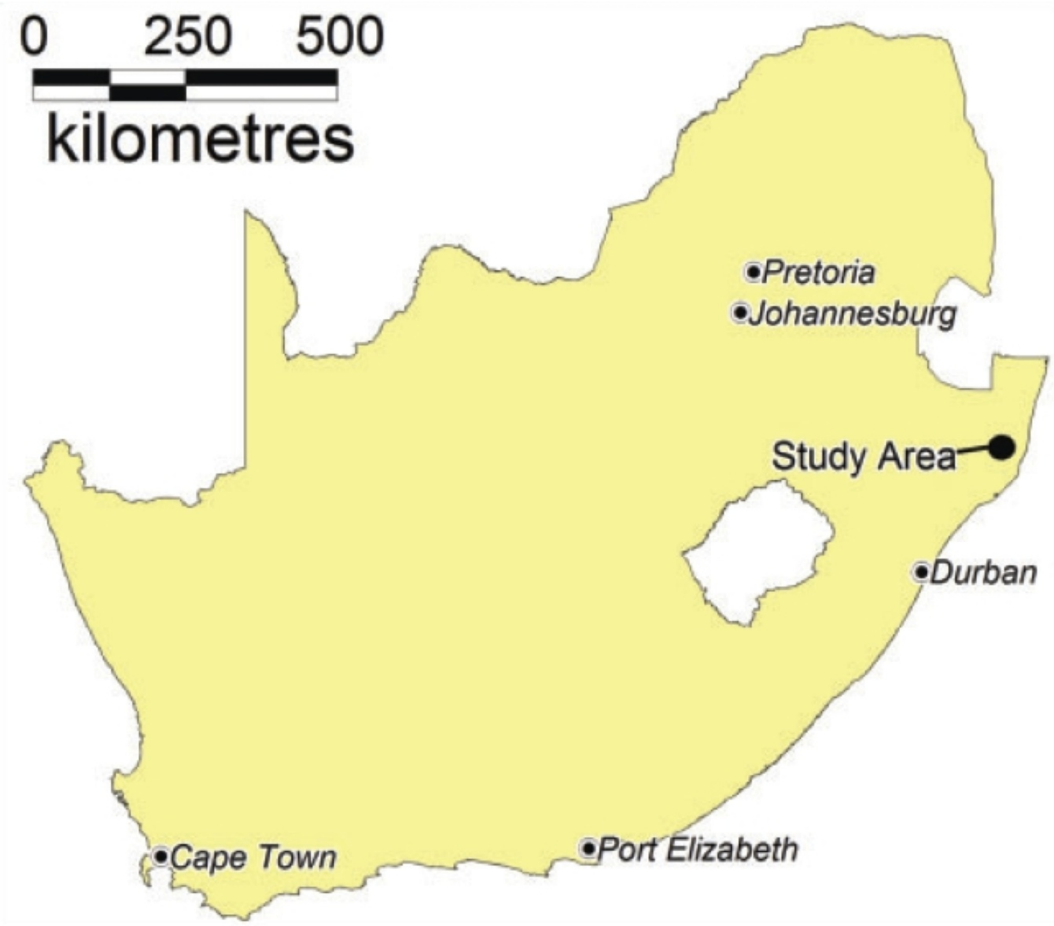


Figure 2: Hlabisa HIV Treatment and Care Programme catchment area

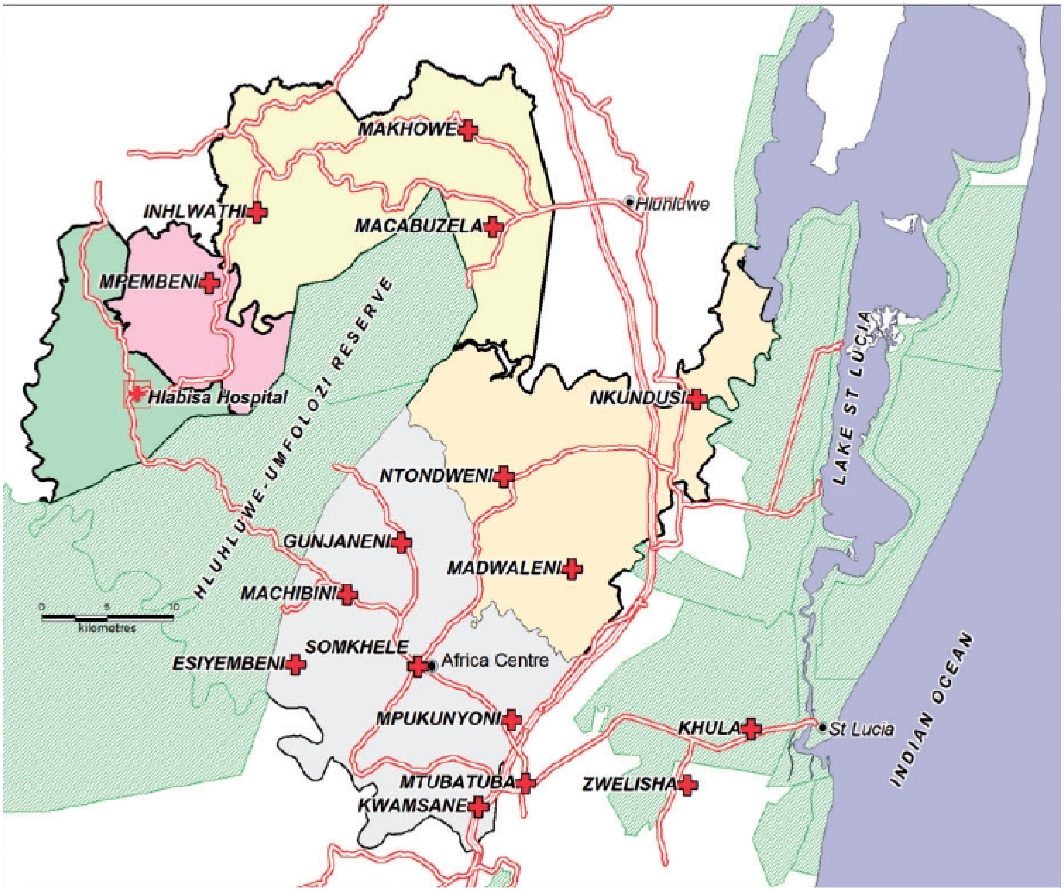
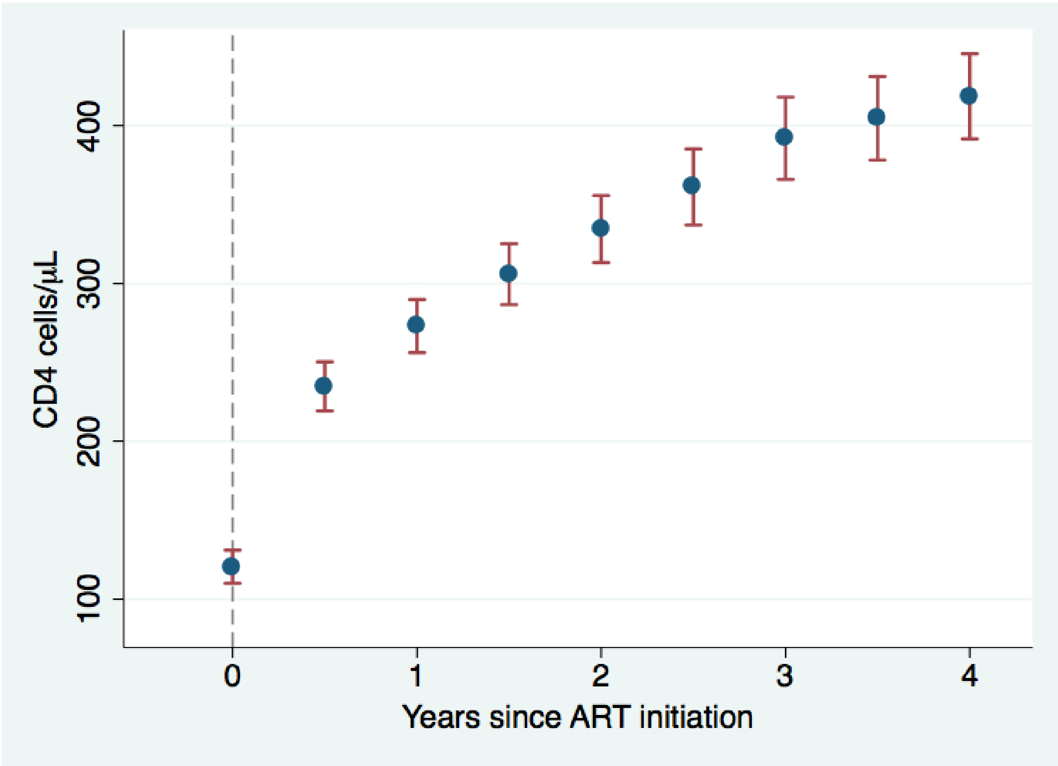


Figure 3: Immunological recovery among HIV patients on ART



Note: Figure displays crude trends in CD4+ lymphocyte counts, an indicator of immune health. The data are restricted to a balanced panel of 372 patients in the Hlabisa HIV Treatment and Care Programme who initiated treatment before December 31, 2006 and who had a CD4+ count within the six months prior to initiation. CD4+ count data were collected via routine clinical monitoring among patients in the HIV treatment program. Daily CD4 counts were estimated via linear interpolation between consecutive clinic visits and averages were calculated at six month intervals. CD4+ lymphocyte counts were imputed as zeros following date of death (as observed in the population-based demographic surveillance) and as last observed CD4+ count for clinical loss to follow-up which did not result in death. Eighty-five percent of the patients were alive after four years of treatment.

Figure 4: Distribution of propensity scores in matched sample

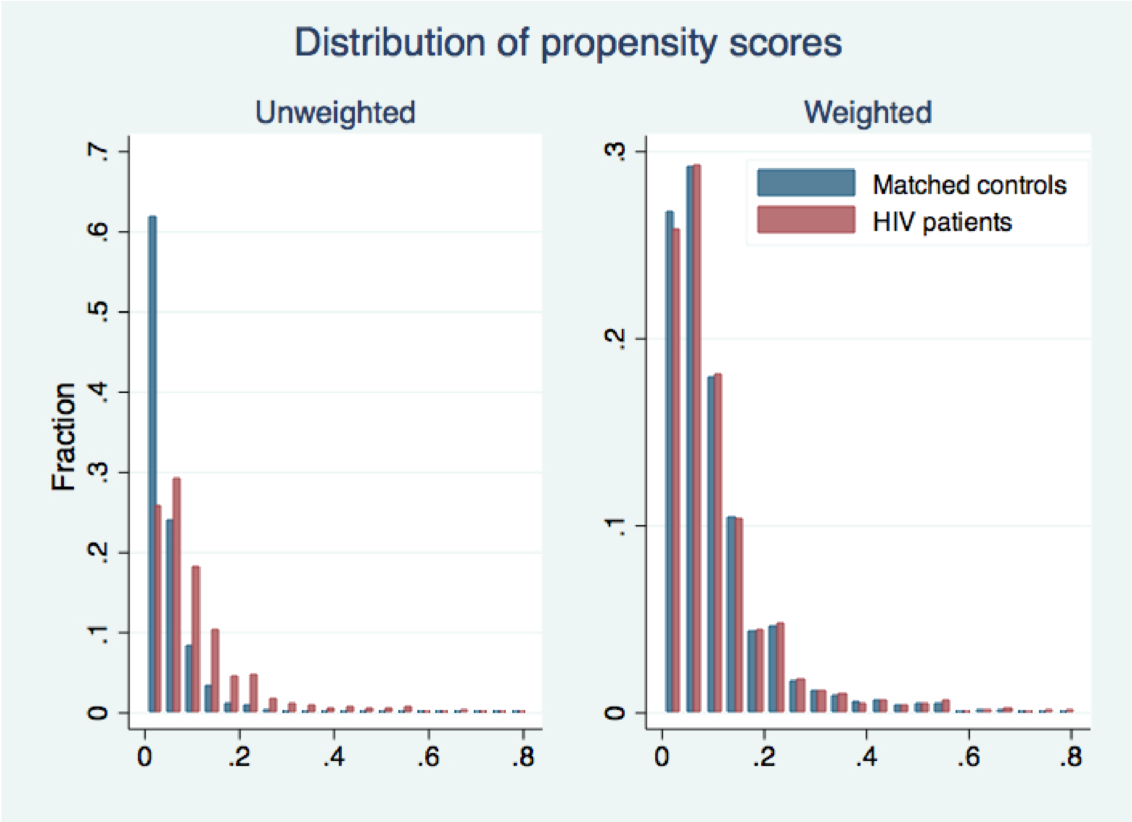


Figure 5: Survival following ART initiation

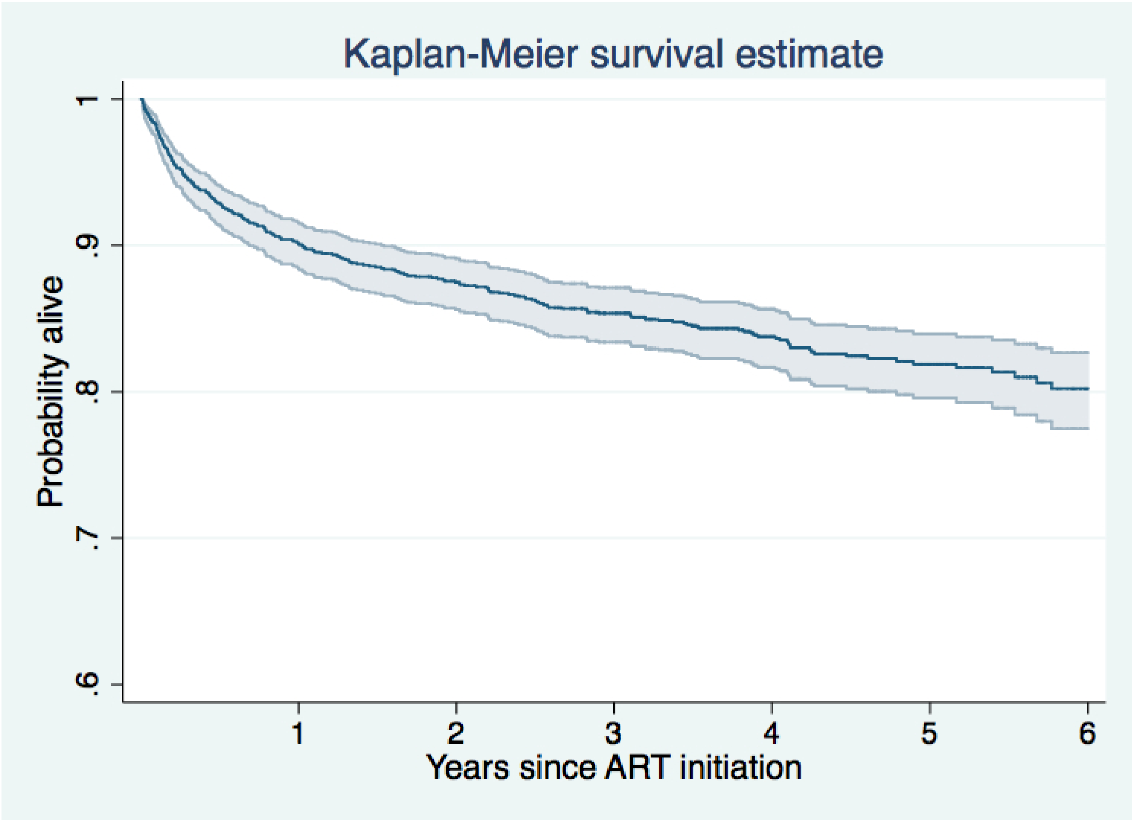


Figure 6: Employment trends among survivors

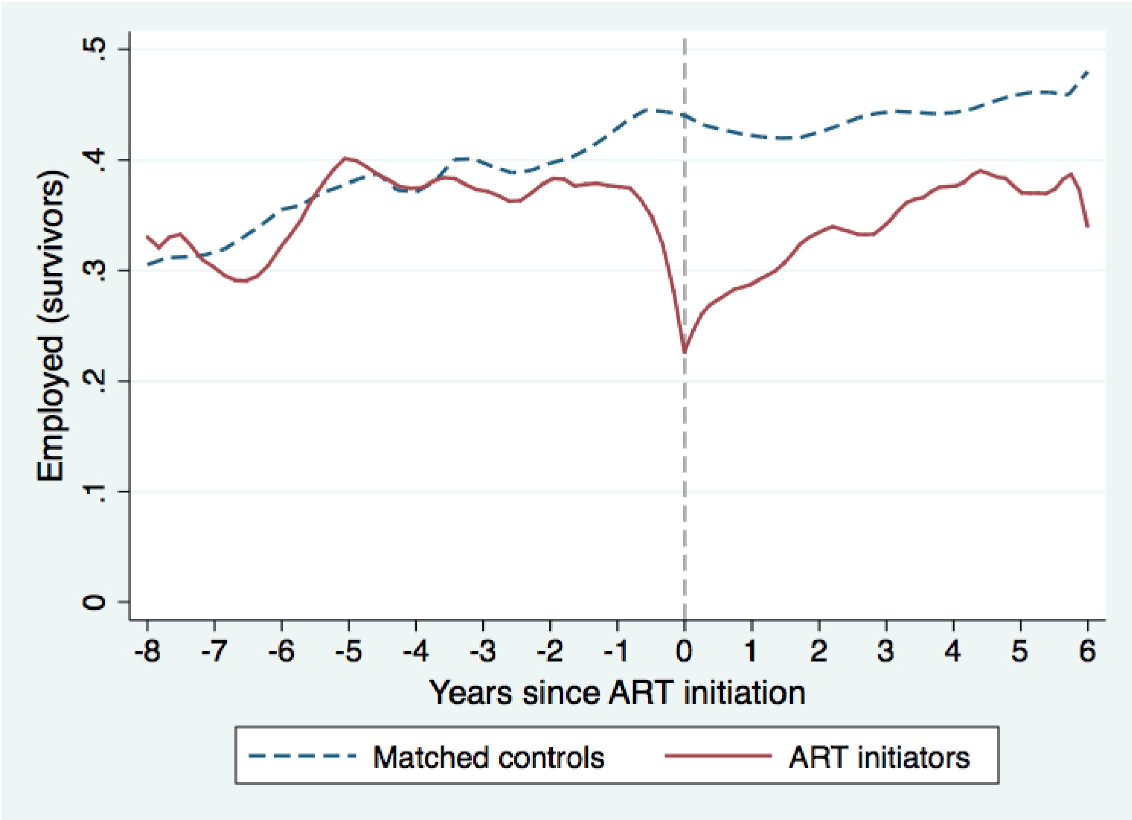


Figure 7: Non-employment due to illness or disability among survivors

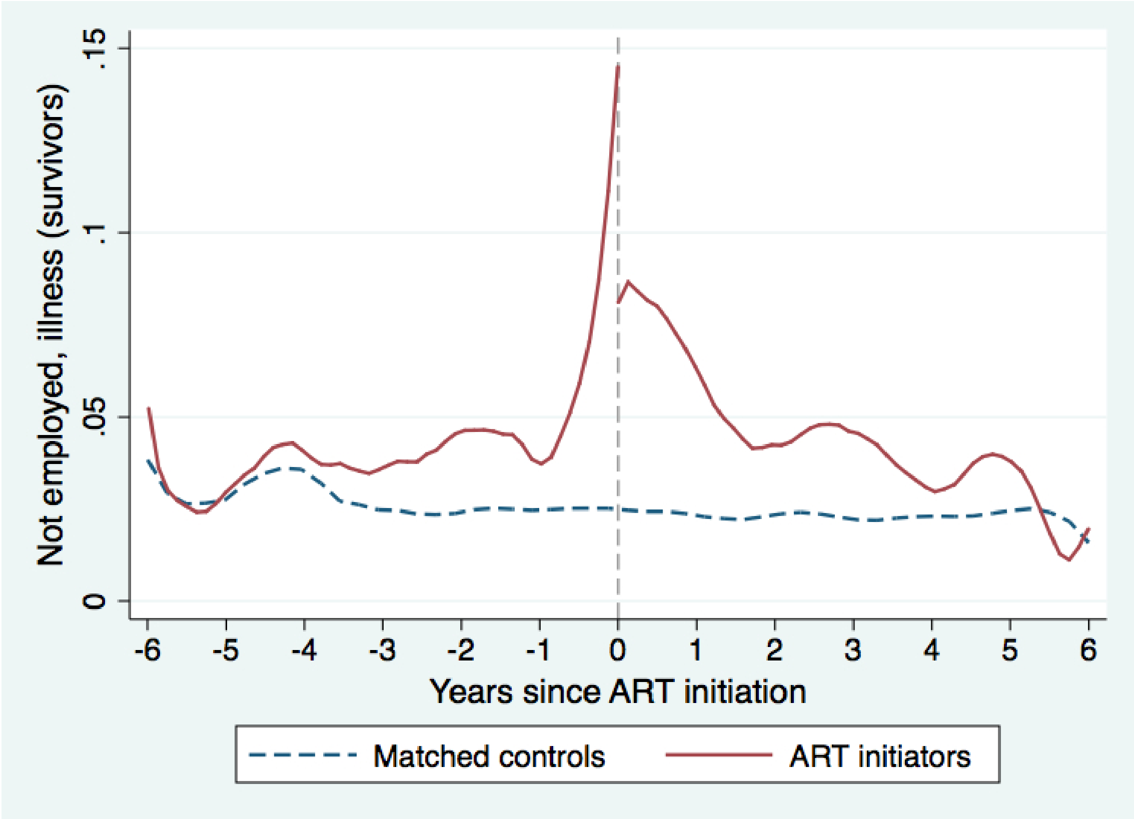


Figure 8: Employment trends among survivors, by sex

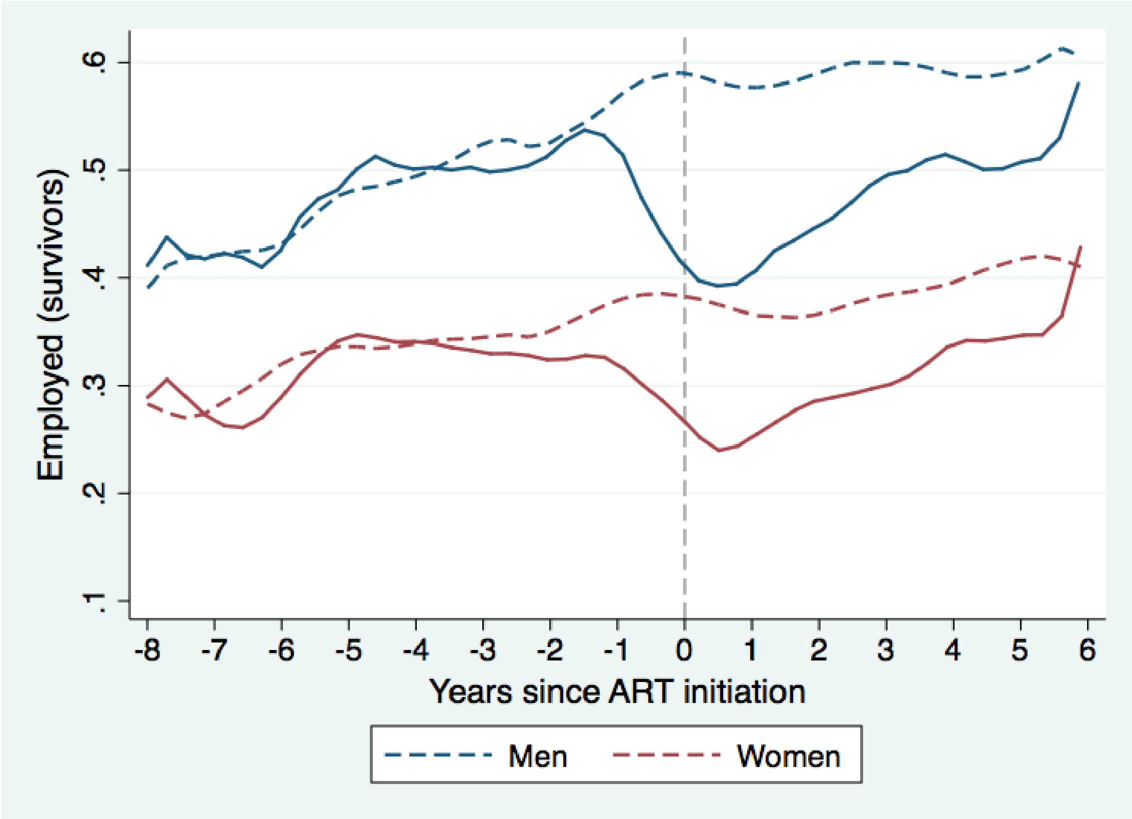


Figure 9: Jobless spells for HIV patients on ART vs. matched controls

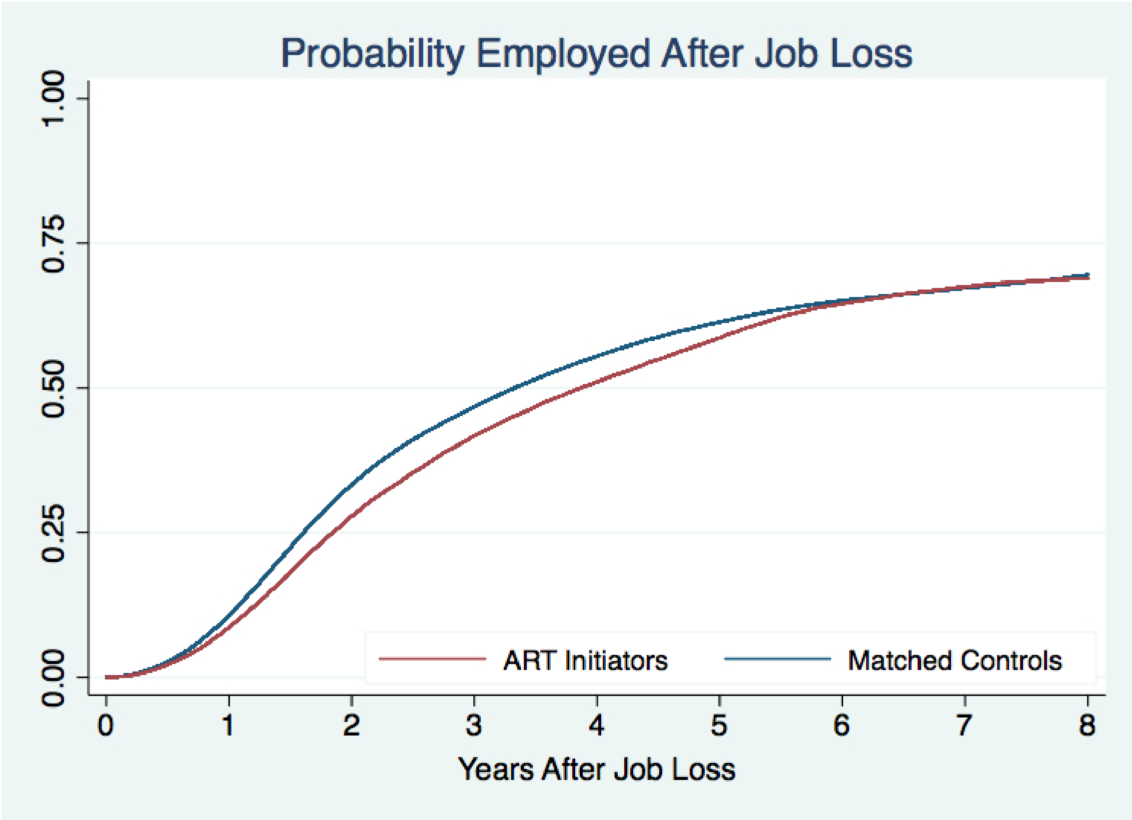


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Table 1: Economic Activities of Population

	M 18-34	**F 18-34**	**M 35-59**	**F 35-59**	**Total**	**ART**
Employment & Migration						
Employed (%)	40	28	65	48	41	38
Resides in DSA	50	59	54	77	59	73
Employed | resides in DSA	29	20	52	38	32	32
Employed | resides outside DSA	56	41	82	67	57	53
If employed, is person self employed?						
Paid Employee	90	85	83	68	82	78
Self Employed	10	15	17	32	18	22
Total	100	100	100	100	100	100
If employed, what is person's occupation?						
Laborer	16	7	14	6	11	10
Security Guard	8	1	5	0	4	3
Artisan	9	1	11	1	6	6
Clerical / Sales	8	17	4	8	9	11
Domestic / Cleaner	3	20	2	20	10	16
Farmworker	3	3	4	6	4	5
Professional	4	10	6	15	8	4
Selling / Making	3	14	4	25	11	14
Transport	9	1	15	1	6	6
Other	19	17	24	14	19	18
Missing	19	9	12	4	12	6
Total	100	100	100	100	100	100
If employed, where does person work?						
Locally	46	56	52	69	55	68
Elsewhere in KZN	34	37	31	26	32	25
Outside KZN	18	6	16	4	11	5
Missing/DK	2	1	1	1	1	2
Total	100	100	100	100	100	100

Data are for 114,797 person-wave observations from 2003-2006. The column denoted "ART" denotes observerations of people in the ART sample, occurring at least 3 yrs before seeking care.

Table 2: Characteristics of population and matched sample

Baseline characteristics	All controls	Matched controls	ART patients
Employed (%)	35.5%	39.4%	39.4%
Lag employed (%)[1]	34.7%	35.0%	34.6%
Age (years)	31.4	33.2	33.0
Female (%)	55.2%	73.0%	73.0%
Education (years)	8.5	7.7	8.0
Non-resident (%)	25.6%	18.7%	18.7%
Peri-urban (%)	50.2%	48.9%	46.4%
Rural (%)	18.7%	27.2%	27.2%
Urban (%)	5.5%	5.3%	7.7%
Household assets (#)[2]	7.8	6.9	6.8
Household size (#)	10.9	10.4	10.2
Household incomes (#)[3]	2.3	2.1	2.1
Pension-eligible in HH (#)[4]	0.45	0.44	0.46
Observations[5]	130,185	42,936	1409

Notes: (1) Lag employment is employment in the survey round prior to baseline. Controls were not matched on lag employment, but it is included here to illustrate similar baseline trends in employment for ART patients and matched controls. (2) Assets were a simple count out of 25 possible assets. (3) Household incomes are a count of major income sources: the number employed, receiving a disability grant, or of pension age. (4) Pension eligibility was 60 years for women and 65 for men (from 2008-2010, men's age of eligibility was gradually lowered to 60 years). (5) For some variables, summary statistics are calculated from fewer observations; during the matching process, missing values were coded as their own category or lumped together with another category if the number missing was small. The 42,936 baseline control observations were contributed by 24,432 individuals.

Table 3: Patterns of attrition, mortality, and missingness

	Matched controls		**ART patients**	
	%	**#**	**%**	**#**
Individuals	---	33,378	---	1407
Observations	78.5	219,251	76.8	9,525
Mortality	1.5	4,296	6.6	822
Attrition	7.1	19,843	3.4	443
Late entry	0.6	1,767	1.0	126
Non-response	12.3	34,322	12.0	1,494
Observations and pseudo-observations[1]	100	279,479	100	12,410

Notes: (1) Observations and pseudo-observations reflect the total number of observations that would exist if individuals were observed in every survey wave for which they were age eligible, regardless of whether they were alive or under surveillance at the time. Pseudo-observations were created in order to assess patterns of missing data and as an intermediate step in imputation. For survey waves when an individual was not observed, a pseudo-visit date was drawn from the distribution of visit dates that actually occurred in that survey wave. Data were missing for one of the following mutually exclusive, collectively exhaustive reasons: the individual had died; the individual was no longer a member of a household under surveillance; the individual had not yet become a member of a household under surveillance; or the individual was alive and under surveillance, but there was no employment status observation. This last category of non-response includes cases in which the household proxy respondent did not know a member's employment status or refused to answer, as well as errors in data collection, entry, and management.

Table 4: Employment and non-employment due to illness regressions

Survivors only	**(1)** **Employed**	**(2)** **Employed**	**(3)** **Not employed due to illness**	**(4)** **Not employed due to illness**
Constant	0.409***	0.389***	0.025***	0.030***
	(0.004)	(0.005)	(0.001)	(0.002)
ART	-0.023	-0.004	0.013**	0.008
	(0.014)	(0.015)	(0.006)	(0.006)
-8 to -5 years pre		-0.053***		-0.004
		(0.006)		(0.003)
-3 to -2 years pre		-0.002		-0.006***
		(0.005)		(0.002)
-2 to -1 years pre		0.018***		-0.005**
		(0.006)		(0.002)
-1 to 0 years pre		0.058***		-0.005**
		(0.006)		(0.002)
0 to 1 years post		0.038***		-0.006**
		(0.006)		(0.002)
1 to 2 years post		0.028***		-0.009***
		(0.007)		(0.002)
2 to 3 years post		0.048***		-0.006**
		(0.007)		(0.003)
3 to 6 years post		0.059***		-0.008***
		(0.007)		(0.002)
ART * -8 to -5	-0.050***	0.002	-0.015	-0.010
years pre	(0.018)	(0.018)	(0.011)	(0.011)
ART * -3 to -2	-0.030*	-0.028	-0.002	0.005
years pre	(0.018)	(0.019)	(0.008)	(0.009)
ART * -2 to -1	-0.009	-0.027	0.008	0.013
years pre	(0.020)	(0.021)	(0.009)	(0.010)
ART * -1 to 0	-0.028	-0.086***	0.021*	0.025**
years pre	(0.020)	(0.021)	(0.010)	(0.011)
ART * 0 to 1	-0.112***	-0.150***	0.044***	0.049***
years post	(0.019)	(0.020)	(0.011)	(0.011)
ART * 1 to 2	-0.082***	-0.110***	0.002	0.011
years post	(0.020)	(0.021)	(0.009)	(0.009)
ART * 2 to 3	-0.053***	-0.100***	0.012	0.018*
years post	(0.020)	(0.021)	(0.009)	(0.010)
ART * 3 to 6	-0.008	-0.067***	-0.004	0.004
years post	(0.019)	(0.020)	(0.008)	(0.008)
Individuals	19,154	19,154	18,818	18,818
Observations	214,816	214,816	189,708	189,708
R-squared	0.001	0.006	0.001	0.001

Linear probability models. Standard errors clustered at individual level, *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Regressions for other labor market outcomes

Survivors only	(1) Labor force participant	(2) Self-employed	(3) Paid employee	(3) Resides in DSA
Constant	0.538***	0.104***	0.276***	0.813***
	(0.005)	(0.003)	(0.005)	(0.004)
ART	0.036**	0.015	-0.018	-0.003
	(0.016)	(0.010)	(0.015)	(0.012)
-8 to -5 years pre	0.043***	0.001	-0.012	0.041***
	(0.010)	(0.006)	(0.009)	(0.005)
-3 to -2 years pre	-0.033***	-0.012***	0.015***	-0.037***
	(0.006)	(0.003)	(0.005)	(0.004)
-2 to -1 years pre	-0.017***	-0.008**	0.031***	-0.063***
	(0.006)	(0.004)	(0.006)	(0.005)
-1 to 0 years pre	0.019***	-0.008*	0.072***	-0.079***
	(0.007)	(0.004)	(0.006)	(0.006)
0 to 1 years post	0.013**	-0.030***	0.074***	-0.083***
	(0.006)	(0.004)	(0.006)	(0.005)
1 to 2 years post	-0.003	-0.040***	0.074***	-0.085***
	(0.007)	(0.004)	(0.006)	(0.006)
2 to 3 years post	0.009	-0.036***	0.091***	-0.103***
	(0.007)	(0.004)	(0.007)	(0.006)
3 to 6 years post	0.010	-0.036***	0.102***	-0.311***
	(0.007)	(0.004)	(0.007)	(0.006)
ART * -8 to -5	-0.019	-0.034*	0.024	-0.017
years pre	(0.033)	(0.019)	(0.029)	(0.016)
ART * -3 to -2	-0.017	0.006	-0.040**	0.036**
years pre	(0.021)	(0.013)	(0.017)	(0.015)
ART * -2 to -1	-0.045*	-0.007	-0.018	0.059***
years pre	(0.023)	(0.015)	(0.019)	(0.017)
ART * -1 to 0	-0.119***	-0.038***	-0.050**	0.088***
years pre	(0.023)	(0.014)	(0.020)	(0.017)
ART * 0 to 1	-0.164***	-0.028**	-0.123***	0.142***
years post	(0.023)	(0.013)	(0.019)	(0.016)
ART * 1 to 2	-0.104***	-0.006	-0.104***	0.140***
years post	(0.023)	(0.014)	(0.019)	(0.016)
ART * 2 to 3	-0.108***	-0.011	-0.091***	0.106***
years post	(0.023)	(0.013)	(0.019)	(0.018)
ART * 3 to 6	-0.083***	-0.010	-0.057***	0.093***
years post	(0.022)	(0.013)	(0.020)	(0.019)
Individuals	18,818	18,818	18,818	19,154
Observations	189,720	189,720	189,720	214,816

Linear probability models. DSA = demographic surveillance area. All outcomes observed for 2003-2012; resides in DSA additionally observed in 2001. Standard errors clustered at the individual level, *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Employment regressions: imputing for death, attrition, missingness

All respondents	(1) Deceased	(2) Employed	(3) Employed, deaths=0	(4) Employed, deaths=0, MI	(5) Employed, MI, survivors
Constant	-0.000	0.390***	0.390***	0.390***	0.389***
	(.)	(0.005)	(0.005)	(0.005)	(0.005)
ART	0.000	-0.002	-0.002	0.001	-0.001
	(0.000)	(0.014)	(0.014)	(0.014)	(0.015)
-8 to -5 years pre	0.000	-0.053***	-0.053***	-0.044***	-0.044***
	(.)	(0.006)	(0.006)	(0.006)	(0.006)
-3 to -2 years pre	0.000	-0.005	-0.005	0.003	0.006
	(.)	(0.005)	(0.005)	(0.005)	(0.005)
-2 to -1 years pre	0.000**	0.017***	0.016***	0.023***	0.025***
	(0.000)	(0.006)	(0.006)	(0.006)	(0.006)
-1 to 0 years pre	0.004***	0.059***	0.057***	0.065***	0.068***
	(0.000)	(0.006)	(0.006)	(0.006)	(0.007)
0 to 1 years post	0.013***	0.037***	0.030***	0.047***	0.055***
	(0.001)	(0.006)	(0.006)	(0.006)	(0.006)
1 to 2 years post	0.018***	0.028***	0.018***	0.043***	0.052***
	(0.001)	(0.007)	(0.007)	(0.007)	(0.007)
2 to 3 years post	0.024***	0.047***	0.032***	0.058***	0.071***
	(0.002)	(0.007)	(0.007)	(0.007)	(0.007)
3 to 6 years post	0.036***	0.058***	0.032***	0.061***	0.080***
	(0.002)	(0.007)	(0.007)	(0.005)	(0.005)
ART * -8 to -5	-0.000	0.005	0.005	-0.004	-0.008
years pre	(0.000)	(0.017)	(0.017)	(0.017)	(0.019)
ART * -3 to -2	-0.000	-0.026	-0.026	-0.029*	-0.030
years pre	(0.000)	(0.017)	(0.017)	(0.017)	(0.018)
ART * -2 to -1	-0.000	-0.021	-0.021	-0.020	-0.022
years pre	(0.000)	(0.019)	(0.019)	(0.019)	(0.021)
ART * -1 to 0	-0.003**	-0.093***	-0.091***	-0.101***	-0.093***
years pre	(0.001)	(0.019)	(0.019)	(0.018)	(0.021)
ART * 0 to 1	0.049***	-0.162***	-0.174***	-0.187***	-0.165***
years post	(0.007)	(0.019)	(0.018)	(0.019)	(0.020)
ART * 1 to 2	0.092***	-0.114***	-0.144***	-0.160***	-0.129***
years post	(0.010)	(0.020)	(0.019)	(0.018)	(0.020)
ART * 2 to 3	0.111***	-0.106***	-0.147***	-0.158***	-0.118***
years post	(0.010)	(0.020)	(0.019)	(0.020)	(0.021)
ART * 3 to 6	0.126***	-0.070***	-0.129***	-0.145***	-0.098***
years post	(0.011)	(0.020)	(0.018)	(0.017)	(0.018)
Observations	281,422	223,100	227,648	281,422	267,608

Linear probability models. Model (2) presents results for all individuals (not just survivors) as observed, with data censored at time of death or loss to follow-up. Model (3) imputes zeroes for employment status pseudo-observations occurring after a person's date of death. Model (4) imputes zeros for deaths, and imputes for attrition, non-response, and other missing data using multiple imputation. Model (5) imputes for missing data, but limits the sample to survivors. Standard errors are clustered at the individual level, *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Employment regressions: by sex

By sex	(1) Men, survivors	(2) Men, imputed	(3) Women, survivors	(4) Women, imputed
Constant	0.507***	0.505***	0.345***	0.345***
	(0.009)	(0.009)	(0.006)	(0.006)
ART	0.008	-0.003	-0.003	0.003
	(0.030)	(0.027)	(0.017)	(0.016)
-8 to -5 years pre	-0.077***	-0.061***	-0.044***	-0.038***
	(0.012)	(0.011)	(0.006)	(0.006)
-3 to -2 years pre	0.010	0.014	-0.004	0.001
	(0.010)	(0.009)	(0.006)	(0.006)
-2 to -1 years pre	0.027**	0.030**	0.019***	0.025***
	(0.013)	(0.012)	(0.007)	(0.006)
-1 to 0 years pre	0.090***	0.090***	0.049***	0.058***
	(0.012)	(0.011)	(0.007)	(0.008)
0 to 1 years post	0.080***	0.074***	0.027***	0.040***
	(0.012)	(0.012)	(0.007)	(0.007)
1 to 2 years post	0.066***	0.059***	0.013*	0.037***
	(0.013)	(0.011)	(0.008)	(0.008)
2 to 3 years post	0.091***	0.078***	0.035***	0.053***
	(0.013)	(0.012)	(0.008)	(0.007)
3 to 6 years post	0.088***	0.063***	0.055***	0.066***
	(0.012)	(0.011)	(0.008)	(0.006)
ART * -8 to -5	0.002	0.011	0.000	-0.008
years pre	(0.038)	(0.034)	(0.021)	(0.019)
ART * -3 to -2	-0.079**	-0.074**	-0.014	-0.013
years pre	(0.036)	(0.033)	(0.022)	(0.019)
ART * -2 to -1	0.028	0.004	-0.050**	-0.029
years pre	(0.040)	(0.036)	(0.024)	(0.022)
ART * -1 to 0	-0.122***	-0.129***	-0.075***	-0.089***
years pre	(0.042)	(0.039)	(0.024)	(0.022)
ART * 0 to 1	-0.213***	-0.243***	-0.134***	-0.169***
years post	(0.042)	(0.036)	(0.022)	(0.021)
ART * 1 to 2	-0.181***	-0.202***	-0.083***	-0.143***
years post	(0.042)	(0.038)	(0.024)	(0.020)
ART * 2 to 3	-0.141***	-0.209***	-0.089***	-0.140***
years post	(0.044)	(0.037)	(0.023)	(0.022)
ART * 3 to 6	-0.086*	-0.194***	-0.061***	-0.129***
years post	(0.044)	(0.035)	(0.022)	(0.019)
Individuals	7327	7951	11,827	12,506
Observations	70,632	94,774	144,184	186,648

Linear probability models. Models (1) and (3) include only observed data for survivors. Models (2) and (4) impute zeros following death and use multiple imputation for other missing data. Standard errors clustered at the individual level, *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Employment recovery: summary table

Panel A: imputation for mortality, attrition, and missingness

Model	(1) Observed data	(2) Deaths=0	(3) Deaths=0, MI	(4) Survivors, MI
Constant (α)	0.390***	0.390***	0.390***	0.389***
	(0.005)	(0.005)	(0.005)	(0.005)
ART (β)	-0.002	-0.002	0.001	-0.001
	(0.014)	(0.014)	(0.014)	(0.015)
Post (γ)	0.058***	0.032***	0.061***	0.080***
	(0.007)	(0.007)	(0.005)	(0.005)
ART*Post (δ)	-0.070***	-0.129***	-0.145***	-0.098***
	(0.020)	(0.018)	(0.017)	(0.018)
Percent recovery	84.3%	69.3%	68.0%	78.9%
Individuals	20,457	20,457	20,457	19,154
Observations	223,100	227,648	281,422	267,608

Panel B: survivors only, no imputation

Model	(5) Baseline	(6) Probit, dy/dx	(7) HIV+ controls	(8) In DSA 2004
Constant (α)	0.389***	.387***	0.377***	0.369***
	(0.005)	(0.005)	(0.011)	(0.006)
ART (β)	-0.004	-0.004	0.003	-0.001
	(0.015)	(0.015)	(0.018)	(0.017)
Post (γ)	0.059***	0.058***	0.073***	0.047***
	(0.007)	(0.007)	(0.015)	(0.008)
ART*Post (δ)	-0.067***	-0.067***	-0.084***	-0.060**
	(0.020)	(0.020)	(0.024)	(0.024)
Percent recovery	84.9%	84.9%	81.4%	85.6%
Individuals	19,154	19,154	4146	13,716
Observations	214,816	214,816	47,659	157,091

Notes: All models are linear probability difference-in-differences models. The constant (α) represents employment levels among the matched controls in the baseline period, 3-5 years pre-ART. Baseline employment for ART initiators is (α+β). Post is an indicator for observations 3-6 years after ART initiation. Other time since ART initiation indicators are not reported. Percent recovery is the ratio of predicted employment levels in ART patients at follow-up= ($\alpha+\beta+\gamma+\delta$) / ($\alpha+\beta+\gamma$). Standard errors clustered at the individual level, *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Hazard regression models: re-employment after job loss

Model	**(1) Exponential**	**(2) Cox prop. hazards**	**(3) Exponential**	**(4) Cox prop. hazards**
Hazard ratio	0.98	0.98	0.89	0.92
95% CI	(0.85, 1.14)	(0.85, 1.10)	(0.78, 1.00)	(0.79, 1.07)
Pr(H_0 true | data)	0.41	0.39	0.02	0.15
Censored at death?	Y	Y	N	N

Notes: The data for this analysis included 401 jobless spells of ART patients and 2,005 jobless spells contributed by 1508 unique controls, which were matched 5:1 with replacement. Jobless spells of ART patients were included if they were observed to be working in the three years prior to ART initiation, and were subsequently observed to be not working in the three years prior to ART initiation or the first six months after starting treatment. Controls were exact-matched on year of birth, sex, education, year of job loss, and place of residence when last employed. Dates of job loss and re-employment were sampled from the intervals in which individuals were known to have lost (gained) employment, assuming a constant hazard of job loss (gain) in that interval. Confidence intervals and p-values constructed by simulation (s=1001), as described in text. Models 3 and 4 continue to "count" posthumous person-time until 1 July 2012, an approach akin to imputing zeros for observations occurring after date of death.

Table 10: Employment for household members of ART initiators

Employment of other household members	**(1) Women**	**(2) Men**
-8 to -4 years pre-ART (mean)	0.347	0.502
-4 to -2 years pre-ART	0.004	0.015*
	(0.008)	(0.009)
-2 to 0 years pre-ART	-0.019**	0.014
	(0.009)	(0.009)
0 to 2 years post-ART	-0.016*	0.010
	(0.010)	(0.010)
2 to 4 years post-ART	-0.015	0.007
	(0.012)	(0.013)
4 to 6 years post-ART	-0.010	0.017
	(0.018)	(0.018)
Observations	143,099	133,645
Individuals	35,269	33,077
R-squared	0.042	0.094

Notes: Regressions are fixed effects linear probability models, with controls for month, day, and year of survey visit, and for sex-by-education specific age profiles. Standard errors are clustered at the household level. Individuals were linked to ART initiators through shared household membership on the date two years prior to ART initiation. Analysis was restricted to individuals' first "exposure" to ART through shared household membership. *** p<0.01, ** p<0.05, * p<0.1