Health, Human Capital and Domestic Violence[†]

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Abstract: We study the impact of an HIV treatment breakthrough (HAART) on domestic violence and illicit drug use among low-income women infected with HIV. To estimate causal effects, we examine HIV-positive women before and after the introduction of HAART and exploit differences in these women's health prior to the breakthrough. We find that HAART introduction reduced domestic violence and illicit drug use. To explain our results, we argue that health is a form of human capital and that positive health shocks reduce incentives for risky behaviors, such as drug use, and also improve options outside of violent relationships.

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1 Introduction

Domestic violence is tragic, rampant, and costly. In the U.S., there are about 4.5 million instances of domestic abuse each year, and about 22% of women will be physically assaulted by an intimate partner at least once in their lives (Tjaden and Thoennes, 2000). The annual cost of intimate partner (domestic) violence — including direct medical expenditures and losses to productivity — is estimated at $5.8 billion.[1]

Counting productivity losses along with direct healthcare expenditures highlights two important relationships. The first is the well-established relationship between domestic violence and poor labor market outcomes. This relationship reflects how factors such as low education or drug abuse can increase the likelihood of violence and simultaneously discourage successful employment. Previous literature has shown that it also reflects causality in both directions. Abuse can deter human capital accumulation or undermine a woman's success at work. In the other direction, women with few resources, poor labor market prospects or low earnings have fewer options outside violent partnerships (Browne et al., 1999; Swanberg and Macke, 2006; Aizer, 2010). Less understood is the relationship between health and domestic violence. Poor health and chronic illness have been shown to be associated with abuse, once again reflecting how underlying factors (e.g., lack of education and drug abuse) contribute to both (Black et al., 2011). Mechanically, this relationship is also causal, at least in one direction: violence, by its nature, potentially damages health. Scant attention has been paid to the causal effect of health shocks on a woman's likelihood of suffering abuse.

This paper studies the impact of the introduction of a break through medical innovation, Highly Active Anti-Retroviral Therapy (HAART), on domestic violence and illicit drug use among a sample of women who are infected with HIV (henceforth *HIV-positive* or HIV+).[2] Estimating the causal impact of health on domestic violence and drug use is difficult due to possible reverse causality or omitted third factors. To identify causal effects, we therefore examine a plausibly exogenous variation in women's current and expected health trajectories following the introduction of HAART. The sample is from a longitudinal study providing rich information on domestic violence, health, illicit drug use (such as crack cocaine and

[1] Further costs accrue through spillover effects in classrooms (Carrell and Hoekstra, 2010), intergenerational persistence (Pollak, 2004), emotional duress and compromised quality of life. The above estimate also does not include costs to the justice system or social services and so annual expenditures probably amount to a gross under-estimation of the true economic costs of domestic violence.

[2] HIV stands for Human Immunodeficiency Virus. Without treatment, a newly infected HIV+ individual lives an average of 11 years. There is no vaccine or cure for HIV, but HAART is the current standard treatment. In general, 1996 is marked as the year when two crucial clinical guidelines that comprise HAART came to be commonly acknowledged. First, protease inhibitors (made widely available towards the end of 1995) would be an effective HIV treatment. Second, several anti-retroviral drugs taken simultaneously could indefinitely delay the onset of immune system decline among HIV+ patients.

heroin) and sociodemographic variables. The women in our sample are predominantly black and report lower income and educational attainment than average U.S. women. This is an appropriate sample for our study since U.S. women with these characteristics are disproportionately affected by HIV (Pellowski et al., 2013).[3] Moreover, two key features of HIV make it an appropriate setting for our study. First, the introduction of HAART was unanticipated, providing a quasi-experiment that allows us to identify a causal effect of positive shocks to health. Second, the severity of HIV infection coupled with the effectiveness of HAART resulted in effect sizes large enough to detect the nuanced causal relationships between medical innovation, drug use and violence. Untreated HIV leads to immune system deterioration (known as AIDS) after which fairly routine infections cause grave symptoms, illness and death.[4] HAART effectively transformed HIV infection from a virtual death sentence into a manageable, chronic condition, reducing mortality rates by over 80% within two years of its introduction (Bhaskaran et al., 2008).[5] The introduction of HAART can therefore be helpful for understanding the impact of large positive health shocks, in this case on illicit drug use and domestic violence. Importantly, we observe an objective, continuous measure of immune system health (known as CD4 count and described in greater detail below), which allows us to distinguish women based on their time-varying, pre-treatment immune system health. Moreover, women in the study were told of their own health status, which could affect their behavior and long-run investments. Their knowledge of their health status is key to our identification strategy, as we explain below.[6]

Identifying causal effects by exploiting the introduction of HAART requires that we construct treatment and control groups. We distinguish women based on their immune system health prior to HAART introduction. Our treatment group consists of HIV+ women who, prior to HAART introduction, exhibited a CD4 count low enough that medical guidelines suggest they commence treatment, but not low enough that they already experienced the symptoms of compromised immune systems. Rather, they were just beginning to experience immune system deterioration, which means an effective treatment for HIV was particularly salient and urgent since they faced relatively imminent declines in physical health and longevity. Because women in the treatment group had not yet experienced the debilitat-

[3] In 2014, there were almost one million individuals with HIV in the United States, and about 230,000 were women (CDC, 2015).

[4] AIDS stands for Acquired Immunodeficiency Syndrome.

[5] Because of the effectiveness of HAART and the severity of HIV, the take-up of HAART was quite fast. For our sample of HIV+ women, over 55% had taken HAART within two years of the introduction and over 70% had within three years.

[6] Oster et al. (2013) provide a leading example of individuals responding to information about their health status. We appeal to similar logic by relating HAART introduction to women's behavior, including women whose contemporaneous symptoms may not be affected by HIV or HAART even though their underlying health is.

ing physical health effects or low survival rates associated with full-blown AIDS, they had not yet experienced symptoms and are thus comparable to HIV+ women with higher CD4 counts, whom we use as a control group. Any responses to HAART introduction would thus be comparatively large for the treatment group, which is part of our strategy to identify causal effects.[7] Importantly, our treatment and control groups are defined based solely on pre-HAART characteristics. More specifically, we identify the causal impact of HAART using a difference-in-differences approach, which allows us to estimate the differential effect of HAART on the treatment group relative to HIV+ women with high CD4 counts.[8] Identification of a causal effect relies on pre-HAART trends in outcomes being the same across our treatment and control groups, and we perform several analyses to show that we cannot reject this hypothesis. A caveat to our identification strategy is that the distinction between treatment and control groups does not rule out the possibility that relatively healthy HIV+ women who constitute our control group also benefited from HAART in expectation, which means we may under-estimate causal effects. We return to this point when discussing our identification strategy.

This study is the first to establish that health improvements can reduce domestic violence. We show that HAART led to reductions in domestic violence of roughly 6% for the treatment group relative to the control group. To explain this finding, we appeal to two broad ideas. First, health is a form of human capital (Grossman, 1972; Becker, 2007). Second, higher levels of human capital enable women to leave abusive partners, because they face better options outside of violent partnerships. Moreover, women with more human capital face stronger incentives to avoid risky behaviors with negative future consequences, such as illicit drug use. Thus, we also assess the impact HAART on the use of illicit drug use, such as crack cocaine and heroin. We show that the medical breakthrough we study led to decreases in illicit drug use of about 10%. Our results are robust when considering domestic violence and the use of heroin. Results on stimulants, however, are weaker and sensitive to the specification and therefore must be interpreted with caution.

Having provided evidence that HAART introduction substantially reduced violence and illicit drug use, we turn to exploring some possible mechanisms. First, we address whether HAART affected both violence and drug use independently or affected one of these solely through its impact on the other. Though it is difficult to say definitively with the data

[7] We provide a formal model to make this point in Appendix A.1. The model is discussed in Section 3.2 when we discuss identification.

[8] An alternative approach would be to focus solely on women who actually use HAART, though medication choice is endogenous. In results available from the corresponding author, we show that HAART usage reduces violence if we use HAART introduction as an instrumental variable for HAART usage. One benefit of our approach is that we do not focus exclusively on users, so we can capture how introduction of HAART affected non-users through, for example, changes in expectations over future health induced by HAART introduction.

we have, we provide some evidence that HAART affected both outcomes even after we control for the correlation between drug use and violence via joint estimation. Second, we examine whether our results are explained by contemporaneous changes in mental health (measured as depressive symptoms) or physical symptoms (measured as ailments associated with AIDS). While we do show relative improvements to underlying health (CD4 count) for women in our treatment group after HAART, we find no evidence of relative reductions in mental or physical symptoms. This is because women in the treatment group, though they had experienced relatively large drops in CD4 counts prior to HAART, had not yet become sick enough to experience symptoms associated with AIDS. Thus, HAART-induced shifts in violence and drug use are attributed to better underlying health, measured as CD4 count, which improved future health, well-being and longevity, rather than to reductions in symptoms. Indeed, the fact that women in the treatment group did not see appreciable reductions in symptoms after HAART relative to the control group underscores that they are comparable. Third, we explore whether the impact of HAART on violence and drug use can be explained by changes in labor market outcomes. We show evidence of increases in employment among women in the treatment group relative to the control group after HAART. We do not claim to have isolated the precise mechanisms linking employment to HAART, drug use or violence. However, improvements to labor market outcomes are consistent with the view that HAART amounted to an exogenous upward shift to women's human capital, which improved women's outcomes on a variety of dimensions, including drug use, violence and employment.

Our findings suggest that interventions that improve women's health or otherwise augment their human capital could reduce domestic violence and illicit drug use. The potential policy relevance of our findings is amplified by the fact that it is not always clear which policies most effectively reduce these behaviors. In the case of domestic violence, for example, there have been large declines over time, which are not yet fully understood (Black et al., 2011). Earlier work has suggested that increases in women's earnings relative to men's have contributed to this decline, which suggests a role for women's labor market human capital (Aizer, 2010). Other recent work, however, has shown evidence that increased compulsory education actually increased psychological violence in Turkey (Erten and Keskin, 2018).[9] In light of existing findings, these relationships require further study. The medical innovation we examine provides a unique opportunity to test whether a particular type of exogenous positive shift in health human capital could also play a role in reducing violence along with

[9]While puzzling, this result is consistent with Anderberg and Rainer (2013), who provide theory and evidence of a non-monotonic relationship between the wage gap and intra-partnership violence if an abusive man attempts to sabotage his partner's efforts to achieve labor market success when market conditions improve for women.

other costly social problems, such as illicit drug use. Nonetheless, we are cautious in generalizing our results to other types of health shocks since HIV is a specific chronic condition, the introduction of HAART was an unusually large and abrupt pharmaceutical innovation, and the resulting shift in human capital was also of a particular type.

The remainder of this paper is organized as follows: Section 2 introduces the data set used in this project and presents a preliminary data analysis. Section 3 discusses how we link health to illicit drug use and domestic violence, first conceptually and then empirically. Section 4 presents our main econometric results concerning the effect of HAART on violence and drug use. Section 5 examines some possible mechanisms explaining why better health reduces drug use and violence. Section 6 speculates on the broader implications of our results and concludes.

2 Data

In this section, we introduce the data set used in our analysis, the Women's Intra-Agency HIV Study. We include a discussion of data on health status in the context of HIV. We also discuss construction of our analytic sample.

2.1 The Women's Intra-Agency HIV Study

We employ a unique data set from the Women's Interagency HIV Study (WIHS). The study was initiated to investigate the impact of HIV on women in the United States, and the sample was selected to include both HIV+ and uninfected or HIV-negative (henceforth: HIV−) women.[10] Women in the WIHS study are predominately black and low-income and exhibit low levels of education. This reflects efforts to create a sample of women who are representative of women with HIV. Participants were recruited from a variety of places, including: HIV primary care clinics, hospital-based programs, research programs, community outreach sites, women's support groups, drug rehabilitation programs, HIV testing sites and referrals from enrolled participants (Barkan et al., 1998). The study began in 1994, and a second cohort was added to the sample in 2001-2002. Each woman in the sample was enrolled in one of six clinical consortia, located in: Bronx/Manhattan, New York; Washington, DC; San Francisco/Bay Area; Los Angeles/Southern California/Hawaii; Chicago, IL; and Brooklyn, New York. Semi-annual interviews are ongoing. Women were compensated for participation with monetary remuneration, gift packs, bathing and laundry facilities, meals, transporta-

[10]Though not included in most of our analyses, HIV− women are examined for a test of validity.

tion and access to dental care at some sites. In addition, services such as HIV counseling, health assessments, health education and referral to clinical trials, primary care and social services were provided. For more information on the WIHS, see Barkan et al. (1998).[11]

The WIHS data set is well-suited for use in assessing the causal effect of medical innovation on domestic violence and illicit drug use for several reasons. First, the panel structure of the data allows us to follow women over time. Importantly, the WIHS started interviewing women in October 1994, before HAART became widely available in late 1996. This means we observe women before and after the unanticipated medical innovation and can compare women based upon their pre-treatment characteristics. For women in our main analysis, there were about four visits before the introduction of HAART. Second, there was an additional cohort added in 2001-2002, after the introduction of HAART. Although not included in our main sample, we use this second cohort in a series of robustness checks to assess the potential effects of participation in the study. Simply participating in WIHS can be beneficial to the participants, and we use the second cohort to separate the experimental effect from the effect of medical innovation.[12] Third, the data includes a rich set of behavioral, socio-demographic and health variables. Information is elicited on employment, income, housing status, relationship and marital status, sexual behaviors, illicit drug use, and medication use.[13]

To quantify health, we use a common measure of immune system functionality, CD4 count, defined as the number of white blood cells per mm^3 of blood. CD4 count is measured using plasma samples, which are collected by medical professionals. This ensures that the

[11]Data in this manuscript were collected by the Women's Interagency HIV Study (WIHS). The contents of this publication are solely the responsibility of the authors and do not represent the official views of the National Institutes of Health (NIH). WIHS (Principal Investigators): UAB-MS WIHS (Michael Saag, Mirjam-Colette Kempf, and Deborah Konkle-Parker), U01-AI-103401; Atlanta WIHS (Ighovwerha Ofotokun and Gina Wingood), U01-AI-103408; Bronx WIHS (Kathryn Anastos), U01-AI-035004; Brooklyn WIHS (Howard Minkoff and Deborah Gustafson), U01-AI-031834; Chicago WIHS (Mardge Cohen), U01-AI-034993; Metropolitan Washington WIHS (Mary Young), U01-AI-034994; Miami WIHS (Margaret Fischl and Lisa Metsch), U01-AI-103397; UNC WIHS (Adaora Adimora), U01-AI-103390; Connie Wofsy Women's HIV Study, Northern California (Ruth Greenblatt, Bradley Aouizerat, and Phyllis Tien), U01-AI-034989; WIHS Data Management and Analysis Center (Stephen Gange and Elizabeth Golub), U01-AI-042590; Southern California WIHS (Alexandra Levine and Marek Nowicki), U01-HD-032632 (WIHS I - WIHS IV). The WIHS is funded primarily by the National Institute of Allergy and Infectious Diseases (NIAID), with additional co-funding from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD), the National Cancer Institute (NCI), the National Institute on Drug Abuse (NIDA), and the National Institute on Mental Health (NIMH). Targeted supplemental funding for specific projects is also provided by the National Institute of Dental and Craniofacial Research (NIDCR), the National Institute on Alcohol Abuse and Alcoholism (NIAAA), the National Institute on Deafness and other Communication Disorders (NIDCD), and the NIH Office of Research on Women's Health. WIHS data collection is also supported by UL1-TR000004 (UCSF CTSA) and UL1-TR000454 (Atlanta CTSA).

[12]Results of this robustness check are discussed further in Section 4.3.

[13]Although we observe if the participants are in a relationship, we do not observe the length of the relationship.

health measure that we use is objective rather than self-reported. Importantly, after each measure was taken, study participants were informed of their CD4 count. For healthy HIV− individuals, average CD4 counts range between 500 and 1,500. For HIV+ individuals, lower counts indicate that immune deterioration has commenced, with counts below 200 signaling high susceptibility to common illnesses (a condition known as AIDS). Guidelines recommend starting HAART as CD4 counts decrease, generally once the CD4 count reaches 350 (Mocroft and Lundgren, 2004; AIDSinfo, 2014). Monitoring CD4 cells allows individuals to track their immune system health, with lower CD4 reflecting a weaker immune system, sometimes known as immunosuppression. For example, a woman who learns her CD4 count is 400 is unlikely to experience symptoms of immunosuppression, but is aware that her immune system health has begun to decline and that her chances of long-run survival are therefore lower than a women with that is still within the 500-1500 range.

Our measure of domestic violence indicates whether women reported experiencing any of three forms of violence in the six months prior to their interview: physical abuse, sexual abuse, or coercion by an intimate partner or spouse. These data are thus uniquely rich in including several forms of violence and not just the most extreme cases (e.g., assaults). We classify the woman as having experienced coercion if a partner threatened to hurt or kill her or prevented her from leaving or entering her home, seeing friends, making telephone calls, getting or keeping a job, continuing her education, or seeking medical attention. Moreover, we do not require that women report being in a relationship in order to report domestic violence. Indeed, many women report not being in a relationship at visits t and $t+1$ and also report violence between the same two visits. This might occur if a woman has a short-term intimate partner who abuses her. Because we do not condition experiencing domestic violence on being in a relationship, we bypass problems that arise if HAART affected selection into a long-term partnership such as marriage.

2.2 Construction of the Analytic Sample

The main analytic sample includes all women from the first WIHS cohort who were HIV+ and answered questions about outcomes including domestic violence, employment, and illicit drug use, as well as all of the controls that we include.[14] The first cohort of the WIHS data set includes 2,071 HIV+ women who participated in the study for up to 33 visits. This amounts to 47,149 person-visits. However, questions about domestic violence were not asked at every visit. Starting in the 10th visit, questions about domestic violence were only asked every other visit. Once we account for the change in timing of domestic violence questions,

[14]See Section 4 for a list of controls.

we are left with 2,065 individuals and 30,135 person-visits.[15] Second, we drop from our analytic sample 53 women who were in the study for just one visit before their death. Additionally, for women who died during the study period, the last "visit" is a record of this, and once we drop these, we are left with 2,012 individuals and 29,492 person-visits. Third, we trim observations that are missing basic information such as date of visit, CD4 count before the introduction of HAART, or age, leaving us with 23,215 observations from 1,995 individuals.[16] Last, we trim observations that are missing information about domestic violence, employment, income, drug use, or relationship status and find that there are 13,948 person observations from 1,055 individuals.[17] Although we use an unbalanced panel, 73% of our sample stayed in the study for all 33 visits.[18] These 33 visits occurred between October, 1994 and April of 2010.

A legitimate concern is the large number of missing observations. Reassuringly, we do not find evidence that observations are missing differentially for treatment versus control groups. To evaluate whether individuals are non-randomly missing from our sample, we perform two main tests. First, we show that demographics, being in the treatment group and experiencing violence pre-HAART are not related to the likelihood of leaving the sample or the number of visits that one stays in the sample. We construct an indicator variable for ever leaving the sample and estimate logit regressions where the outcome of interest is leaving sample for any reason and explanatory variables are being in the treatment group, race, age, site of visit, and experiencing violence pre-HAART. No controls are significantly correlated with leaving the study. As a complementary test, we also regress the number of visits that the woman stayed in the study and find no evidence that any control variable is correlated with this outcome. Results from these estimates can be found in Table A1.

In our second test, we also regress an indicator variable for missing each outcome (domestic violence, crack or cocaine use, heroin use, or employment) on race indicators, age, age squared, site indicators, logged CD4 count, and an interaction between the treatment group and logged CD4 count.[19] The idea is that the coefficient of the interaction term between

[15]For outcomes that were asked every visit, we follow the same steps for trimming our data, but also include the odd numbered visits greater than ten.

[16]This large drop happens mainly because the date of the visit is missing. Date of visit is necessary because it indicates if HAART was available.

[17]When we impute missing variables, our results change very little. Thus, we choose to simply drop observations that are missing information.

[18]We include in our sample all women who are not missing information, regardless if they die during the study. In a conservative robustness check, discussed further in Section 4.3, we re-run our analyses excluding all women who die early in the study and find that our results do not change for most specifications. Given these results, we do not believe that survival bias is driving our main empirical results. Excluding women who die is effectively non-randomly removing individuals from our sample, i.e., we may be removing women who are more likely to suffer domestic violence.

[19]To keep our sample size consistent, when CD4 count is missing, we impute the missing value and include

the treatment group and logged CD4 count will tell us if women from the treatment group exhibit patterns of "missingness" that differ from the other women in the study. Women are included in this regression if they made the first three trims of the data as described above. We focus on this subsample because they are the women for whom we have basic information about sociodemographics. While we do find that women who are less healthy in terms of a lower CD4 count are more likely to be missing observations, as shown in Table A2, the actual changes in the probability of missing data for these outcomes are quite small. A 10% increase in CD4 count increases the probability of nonresponse by roughly 0.2 percentage points. Further, and more importantly, there is no difference between the treatment and control groups in terms of how CD4 count affects the probability of having a missing outcome. Thus, we find that, while health may affect the probability of an individual having a missing outcome, it does not do so differentially across our treatment and control groups.

3 Conceptual Framework and Identification

In this section, we outline how we relate health to domestic violence and drug use. Section 3.1 provides a conceptual framework. Section 3.2 discusses empirical challenges and our identification strategy, which exploits an exogenous medical innovation and a difference-in-differences approach. Section 3.3 presents summary statistics for our analytic sample, compares the treatment to the control group, and discusses the external validity of our sample. Finally, Section 3.4 provides evidence of the validity of our identification strategy.

3.1 Conceptual Framework

To conceptually link health to drug use and violence, we draw on two key ideas from economics. First, health is a form of human capital (Grossman, 1972). Second, individuals with higher levels of human capital face different costs associated with drug use and violence. The reasoning is that human capital extends life, improves well-being and raises labor market productivity. As a result, incentives to avoid drug use and violent partners become stronger since women not only have more to live for, but also have more options outside of violent partnerships. In the context of HIV, HAART extended the life and improved the health of HIV+ women who were taking the drug. We hypothesize that HAART would therefore lead to lower illicit drug use and domestic violence among HIV+ women who were first affected by its introduction.

an indicator in the regressions for imputation.

A potential problem with this conceptual framework is that it presumes that women have some control over both violence and drug use. In the case of illicit drug use, addiction may mean that women are unable to change their behavior even in the face of a strong shift in incentives, such as a positive health shock. Rooted in the idea of rational addiction (Becker and Murphy, 1988), we assume that women make rational choices regarding their drug use, weighing the benefits or consequences of continued use. This assumption is supported by clinical evidence showing that illicit drug use is responsive to shifts in incentives (Hart et al., 2000).[20] Finally, it is consistent with the idea, discussed in sociology, of *desisting*, which posits that individuals choose to avoid (or desist from) risky behaviors like drug use after important life events, such as having children, that give them more to live for (Laub and Sampson, 2001). We hypothesize that HAART incentivized desisting from illicit drug use by endowing women with longer and healthier lives, thus making illicit drug use a relatively more costly option.

The application of our conceptual framework to domestic violence is more delicate. The concern is that the assumption that women can "choose" abuse perpetrated by a violent partner can erroneously be perceived as "blaming the victim" for her own abuse. When relating domestic violence to human capital, we draw upon the resource theory of domestic violence. Often attributed to Gelles (1976), the idea is that women with more resources have better options outside of abusive partnerships and are therefore more likely to leave violent partners. For example, if a woman's outside option is homelessness, she may find it safer to remain with a potentially violent partner. The resource theory of domestic violence helps to explain why women with higher education or more income are more likely to avoid violence. The theory has been used to motivate bargaining theories of domestic violence. In bargaining models, women with better outside options have higher threat points. Because of this, they can credibly threaten to leave partners and therefore essentially bargain for less violence. Resource and bargaining theories of domestic violence have been used to explain why unilateral non-fault divorce has reduced domestic violence (Stevenson and Wolfers, 2006), why cash transfers to poor women can reduce abuse (Bobonis et al., 2013; Angelucci, 2008; Pronyk et al., 2006), and why abuse is associated with poor labor market outcomes (Bowlus and Seitz, 2006; Anderberg and Rainer, 2013) and a larger gender wage gap (Aizer,

[20]For example, Hart et al. (2000) report that regular crack smokers asked, in an experimental setting, to choose between crack and payments to be made several weeks later (and who have no other access to the drug until the following day) regularly opt for the delayed payment, especially when the amount of crack they forgo is fairly small. For earlier evidence against the hopelessness of addiction, see Robins (1993), who studies rapid recovery from heroin addiction among Vietnam veterans upon their return home. One interpretation of her findings consistent with our conceptual framework is that these veterans faced stronger incentives to avoid heroin following a positive shock to their lifespan.

2010).[21] In our case, we do not take a position on intra-partnership bargaining. Rather, we argue simply that HAART improved health, which effectively boosted women's human capital. Therefore, women had more resources to leave violent partners and better options outside of violent partnerships.

Our conceptual framework does not presume a relationship between violence and drug use even though they could be causally linked.[22] One possibility is that better health induces lower illicit drug use, which in turn reduces violence. This would make sense if drug use encourages violence, which has some support in medical literature, especially in the case of stimulants, such as crack cocaine (Phil and Peterson, 1993; Volavka, 2008). Alternatively, less violence can lead to lower drug use, for example, if women who no longer suffer abuse are less prone to drug use as a coping mechanism (El-Bassel et al., 2005). It is also possible that health independently drives reductions in both drug use and violence. Though it is difficult to empirically disentangle these possibilities, we revisit them in Section 5, which provides a discussion of possible mechanisms underlying our key empirical findings. For now, we hypothesize that better health would reduce violence and illicit drug use. We now turn to a discussion of how we test these hypotheses.

3.2 Identifying the Impact of Health on Violence and Drug Use

In relating health to drug use and violence using observational data, we face possible selection problems since health is potentially endogenous. Women in our sample who are in relatively poor health are likely to exhibit a number of other qualities, many of which are unobserved to the econometrician, and which are likely to affect illicit drug use or violence. Thus, correlations between CD4 counts, domestic violence, and drug use cannot be interpreted as causal. For example, there may be reverse causality and simultaneity if drug use or violence affects health. There could also be additional factors that affect all three, such as poverty and mental health problems. More concerning are unobserved and negative transitory shocks that act as stressors that could undermine health and also make violence more likely. Examples include unanticipated job losses, legal problems, financial losses, evictions, or deaths in the family.

[21] Results reported in Alvira-Hammond et al. (2014) suggest that the relationship between labor market prospects and lower domestic violence extend to adolescents. Also related, but in a different medical context, Johnson and Pieters (2016) examine violence among women diagnosed with cancer. In our framework, a cancer diagnosis could be seen as a negative health shock, which could affect the likelihood that women experience violence. The authors focus on a different relationship, arguing that violence can affect health of women with cancer diagnoses by influencing how they seek treatment.

[22] Though they do not take a position on causality, Cohen et al. (2000) use the same data set we use to show that poorer women who use drugs are more likely to be abused. They describe this correlation as a "continuum of risk".

In fact, when we look at simple correlations using regression analysis, we find that the relationships between health and violence or illicit drug use are complex and ambiguous. Table 1 shows a naive reduced form model for the full sample of HIV+ women.[23] Some relationships accord with our priors. HIV+ women with higher CD4 counts are less likely to use stimulants and are more likely to be employed. However, other relationships are not in accordance with our priors. For example, there is a no significant correlation between CD4 and domestic violence in the full sample. Further, there may be non-linearities in the relationship between health and violence, which complicates estimation of causal effects. To explore this further, we also focus on women whose CD4 counts were observed to be below 200 at least once and compare them before and after the first time they are observed with AIDS-level CD4 counts. We see that for almost every type of abuse, they are less likely to report abuse when their CD4 counts are actually below 200, as shown in Table 2. In particular, 12% of women reported experiencing domestic violence on their last visit pre-AIDS diagnosis, compared to just 9.5% post-AIDS diagnosis. This could mean that their illness has become so acute that they are not as likely to experience domestic violence. Further, women are also less likely to engage in illicit drug use after they are diagnosed with AIDS. Heroin use falls from 20 to 15% while stimulant use falls from 9.5 to 6.6%. Given these non-monotonic patterns, all of which threaten the validity of comparing low-CD4 count HIV+ women to healthier women, our analysis does not use low-CD4 count women as a treatment group.[24]

In light of these difficulties, we now turn to a discussion of our identification strategy. First, we note that the aim is to identify how a marginal change in health affects domestic violence and drug use. To illustrate, denote the propensity to suffer domestic violence as V and health as H. We aim to estimate:

$$\frac{dV}{dH}. \tag{1}$$

To achieve identification, our approach leverages variation in health status at the time of HAART introduction along with the fact that HAART was an unanticipated innovation. The passage of time from the pre- to the post-HAART era affects our outcomes (domestic violence, employment, and illicit drug use) through the impact of HAART availability on health (H) both directly and indirectly. For violence, this can be written as follows:

$$\Delta V^g = \left[\Delta H^g \times \frac{\partial V}{\partial H}^g\right] + \left[\Delta X^g \times \frac{\partial V}{\partial X}^g\right] \tag{2}$$

[23] We also show that better health is associated with higher rates of employment, which is consistent with the idea that health is a form of human capital. We explore this point further in Section 5.

[24] In results available from the corresponding author, we use this group as a robustness check to show that our results are not driven by interaction with the medical community.

where Δ is the change in a variable from the pre-HAART to the post-HAART eras and g indexes groups distinguished by pre-HAART health status (treatment and control groups, for example). The first expression on the right-hand-side of equation (2) is the effect of HAART introduction on health (the health shock) multiplied by the effect of health on violence. The second expression captures other avenues through which the passage of time between the pre- and post-HAART eras affected violence, including secular trends and omitted factors (together denoted X). For example, domestic violence was trending downward over this time period for women in the United States (Catalano, 2012), and this trend is captured in X. To identify the causal effect of HAART, we compute the difference-in-differences, relying on variation in how women respond to exogenous shifts in medical technology depending on their health status at the time of the innovation.

Identification of causal effects requires appropriate selection of treatment and control groups. Our treatment group consists of women who were beginning to exhibit HIV-induced immune system deterioration, which typically precedes full-blown AIDS, but who had not yet exhibited AIDS-level CD4 counts. These are women whose minimum CD4 count prior to HAART was between 300 and 399. As discussed above, the most basic current guidelines recommend beginning HAART when the CD4 count reaches 350, and our treatment group encompasses this number. However, since women in the treatment group have yet to reach CD4 counts where they would experience physical illness due to AIDS they are more comparable to healthier women, whom we use as controls. The control group consist of women in relatively good health: HIV+ women with CD4 counts that never dipped below 400 prior to HAART introduction. Of the sample eligible to be included in our analysis, 166 individuals, consisting of 2,477 person-visits, are in the treatment group and 269 women, consisting of 4,192 person-visits, are high CD4 count HIV+ women.

We base our choice of control group on the idea that women who have higher CD4 counts were affected by the introduction of HAART less than women who have lower CD4 counts because HAART was more urgently needed by those with lower CD4 counts.[25] Relatively healthy HIV+ women are a natural control group since, by virtue of a CD4-count cutoff, they were not in imminent danger of becoming gravely ill at the time of HAART introduction. Our strategy identifies the effect of HAART by comparing the differential responses to HAART of women above and below the CD4 cutoff. However, because healthier women were likely to experience CD4 declines soon, they were also potential beneficiaries from HAART and may have also changed their behavior in response to the innovation. To the extent that healthier HIV+ women also reacted to the introduction of HAART, using them as a control group

[25]If we repeat the analysis from Table 1 using the treatment group, we do find negative relationships between health and violence.

may tend to under-estimate the impact of HAART.

The reasoning underlying our identification strategy is that sicker women should respond more strongly to HAART compared to healthier women. To formalize our reasoning, we develop a very simple model to characterize the relationship between survival probabilities, medical technology and human capital investments (see Appendix A.1). In the simple two period model, survival probabilities govern whether the individual survives through the second period. Lower survival probabilities dis-incentivize costly human capital investments made in the first period because there is a lower probability of enjoying the full returns to such investments.[26] Moreover, the model predicts that relatively large increases in survival probability lead to large shifts in human capital investments. Further assumptions on the model rest on the specifics of the context of HIV/AIDS. In response to HAART, women in the treatment group should experience larger increases in survival probability relative to women in the control group. There are two reasons. First, medical guidelines regarding commencement of HAART usage mean that women in the control group are less likely to use it compared to women in the treatment group. Second, women in the treatment group are likely to experience increases in their survival probabilities sooner than those in the control group. Note that HAART does not raise their survival probabilities above those of women in the control group, but simply raises them to the same level. Starting lower initially and then ending at the same level, the women in the treatment group experience a larger change in survival probabilities. This implies that women in the treatment group face larger HAART-induced shifts in incentives to make costly investments in their human capital compared to women in the control group. Thus, if HAART lowers domestic violence and drug use, the model predicts that these shifts would be larger for women in the treatment group.

Using HAART introduction as the treatment, we compare how domestic violence and drug use evolved after the introduction of HAART in the treatment group and the control group. In this context, $\Delta H^g = 0$ for the control group, while $\Delta X \times \frac{\partial V}{\partial X}$ is nearly the same for the treatment group and the control group. Indeed, as we discuss below, we find that CD4 counts increased for the treatment group relative to women in the control group after the introduction of HAART.[27] This design effectively treats CD4 dipping below 400 just prior to HAART introduction (and thus being in the treatment group) as independent of domestic violence and drug use.[28]

[26] A key result of human capital theory is that higher life expectancy incentivize further investments in labor market human capital (see e.g., Ben-Porath (1967)). This theory has broad empirical support (Black et al., 2007; Jayachandran and Lleras-Muney, 2009; Oster et al., 2013; Yi et al., 2015).

[27] We present these results in Section 5, when discussing potential mechanisms our main findings.

[28] The treatment and control groups are defined based on their pre-HAART health and therefore do not change after the introduction of HAART.

3.3 Descriptive Statistics

Before assessing the validity of our approach, we present summary statistics for the treatment and control groups. We also discuss how women in our sample compare to other women in the U.S., including HIV+ and other women, which is important when assessing the external validity of our results. Table 3 provides summary statistics for our treatment group and control group in Columns 1 and 2. In Column 3, we test that the means are equal across the treatment and control groups.

3.3.1 Treatment vs Control Group

According to Table 3, treatment and control groups are quite similar in terms of demographics, including race and education, and pre-HAART characteristics such as risky behaviors, symptoms, and experience with violence. About 67% of the treatment group is black, 20% is Hispanic, and 12% is non-Hispanic white (henceforth simply white). This is roughly equivalent to the control group, where these percents are 64, 22, and 12 respectively. Our samples are also similar in terms of education: 30% of each group graduated high school, 22-23% attended some college, and 10% of the treatment group and 7% of the control group graduated college. Pre-HAART incomes are also comparable across groups. While the high CD4 count women were slightly more likely to have been employed (43% vs 38%) and less likely to have been married prior to the introduction of HAART (32% vs 25%) we cannot reject the null hypothesis that the means are equal.

Turning to risky behaviors and violence, we find that, prior to the introduction of HAART, 28% of the treatment group had used stimulants (defined as crack or cocaine), compared to 31% of the high CD4 count women. Heroin use prior to HAART was very similar between the treatment and control groups: 18% for the treatment group and 16% for the high CD4 count women. Overall rates of violence pre-HAART are similar across the treatment and control groups. In particular, 27% of the treatment group experienced domestic violence before the introduction of HAART, while 34% of the high CD4 count women did. It should be noted that the high CD4 count women were more likely to suffer sexual abuse, physical abuse, and coercion than our treatment subsample, but that the only significantly different is sexual assault. However, mean differences in outcomes between the groups do not threaten the validity of using difference-in-differences to estimate causal effects as long as the trends in domestic violence and other outcomes are similar. We discuss the parallel trends of our main outcomes in the following subsection.

3.3.2 External Validity

Compared to national statistics, the women in our sample are less educated and more likely to report violence and drug use.[29] Nevertheless, it is an appropriate starting point for studying links between health, domestic violence and illicit drug use because low-income, under-employed, and non-white women — socio-demographics shared by the majority of women in our sample — are more likely to experience domestic violence and use illicit drugs during their lifetime than the average American woman.

Further, our sample is quite similar to the statistics that the CDC reports about the HIV+ population of women living in the United States. For example, it is estimated that of the total number of women living with diagnosed HIV, 61% are black (CDC, 2015), compared to about 65% for our sample. Additionally, in a sample of individuals living in high poverty areas, the CDC also found that there was a gradient of the likelihood of being HIV+ by completed education and income bin (Denning and DiNenno, 2010), which we also observe in our sample. Drug statistics for our sample are also more similar to HIV+ women than national reports. The National Survey on Drug Use and Health found that about 16% of individuals who had been diagnosed with HIV reported using an intravenous drug in their lifetime, which is extremely close to the 17% reported by our sample.

Finally, in our main analyses, we present results separately for black women. Nationally, black women are more likely to report domestic violence. Lifetime prevalences of rape, physical violence, and/or stalking are estimated to be 43.7% for black women and 34.6% for white women (Black et al., 2011). Black women also suffer domestic violence at higher rates than the white women in our sample: 30% of black women reported experiencing domestic violence between one year prior to the start of the survey and the introduction of HAART, compared to 23% of white women. Patterns of drug use by black women in our sample compared to other women are more nuanced. For example, 30% of black women reported having used stimulants prior to the introduction of HAART, compared to 24% of white women. However, 13% of the black women in the sample reported having used heroin during this time period, compared to 18% of the white women in our sample. The black women in our sample have, on average, less education than the white women. They also come from less well-off households: 49% had maximal pre-HAART incomes below $12,000 compared to 25% of white women.

[29] For example, high school completion rates are about 80% for females in the United States (Heckman and LaFontaine, 2010), compared to about 60% for our full sample.

3.4 Assessing the Validity of the Research Design

In this section, we discuss the the validity of our empirical approach to estimating causal effects.[30] Identification using the difference-in-differences approach relies on the assumption that the path of the outcome variables for the treatment group of HIV+ women and the control group would not be systematically different in the absence HAART introduction. Specifically, this means that the introduction of HAART should be the only factor that drove the treatment group to experience a change in an outcome variable, such as a relative reduction in domestic violence. To confirm this, we first argue that HAART was an unexpected medical breakthrough. Second, we study pre-HAART trends in our outcome variables and show that they are not different for our treatment group and our control group.

The validity of our research design relies on HAART being an unanticipated innovation. For evidence of this within our sample, we turn to questions used to compute the CES-D scale, which is used to asses whether women are likely to be depressed.[31] One question asks women whether they were hopeful about the future in the week leading up to their interview. We consider the probability that women in the sample answered "most or all of the time" to this question, and plot this before and after the introduction of HAART in Figure 1. There are two reasons why this figure suggests that HAART was not anticipated. First, HIV+ women experienced a jump in hopefulness right at the introduction of HAART. Before this, the percentage of HIV+ women who reported being hopeful was relatively flat. If they had anticipated HAART, they might have reported more hopefulness earlier. Second, HIV− women did not experience such a jump. If some other factor drove the increase in hopefulness, then this would be reflected by a jump in the hopefulness of HIV− women.[32]

Next, we discuss pre-HAART trends among our treatment and control group in Figure 2. In particular, we plot the pre-HAART trends in domestic violence (Panel 2a), stimulant use (Panel 2b), and heroin use (Panel 2c) for the treatment group and the control group. The plots show that trends for the treatment group and the control group were comparable prior to the introduction of HAART, which suggests that HAART is the driving force in the difference in outcomes. We also exploit the fact that we have multiple periods prior to the introduction of HAART to conduct a formal test of whether there are differences in trends between the treatment and control groups. For each outcome, we estimate the following

[30] In Section 3.3, we presented summary statistics and compared women in our sample to women in the U.S. and HIV+ women, which addresses the issue of external validity of our results. The discussion we present here addresses the question of internal validity.

[31] CES-D is a depression screening test and stands for the Center for Epidemiological Studies Depression scale.

[32] See Ostrow et al. (1989) or Detels et al. (2001) for use of the CES-D scale score data in the context of an HIV study.

models:

$$O_{it+1} = X_{it}\beta + Treatment_i\delta + D_t\alpha_t + \sum_{t=-3}^{t=28} Treatment_i \times D_t\gamma_t + \epsilon_{it}, \quad (3)$$

where O_{it+1} is the outcome of interest (violence, stimulant use, or heroin use), D_t is an indicator for the date of visit bin such that D_0 is the last period before HAART was introduced. Each bin is a six month period, and HAART is not available in the bins -3 through 0. We also include controls for age at visit, age squared, indicators for race, and indicators for site of visit. For each model, we test for pairwise parallel pre-HAART trends. Specifically, we test the null hypothesis that coefficients γ_{-3} through γ_0 are equal. This essentially tests that the trends in outcomes prior to HAART are parallel. In Table 4, we show that we cannot reject the null hypothesis that the coefficients are equal for both domestic violence and heroin use, suggesting that there is no difference in pre-HAART trends for these outcomes. For stimulant use, we estimate a p-value of 0.09, which is a borderline significant value. This suggests possible mean reversion, which could lead to an over-estimation of causal effects, so we must interpret estimates with caution. Additionally, we test that the pre-treatment trends in domestic violence, stimulant use, and heroin use are not jointly significant, and fail to reject the null hypothesis that there is no difference in trends (p=0.12). As we explain below, the p-value is 0.19 when we account for differences in observables using inverse probability weighting.

In a related exercise, we plot the residuals from a probit model that regresses domestic violence on age, age squared, age cubed, race dummies, and site dummies. As shown in Figure 3, there is a clear break between the treatment group and the control group after the introduction of HAART. This suggests that the introduction of HAART affected the two groups differently.

We also conduct an event study to show that pre-HAART trends are not driving our results. Figure A1 in Appendix A shows the coefficient of the interaction for the periods leading up to HAART and the periods after the introduction of HAART. Because there are so many more periods after HAART was introduced than before, we combine the post-HAART periods in this exercise. Periods prior to HAART introduction are one year in length, and periods after HAART was introduced are longer. For each outcome, we expect that coefficients on dummies for periods -2 and -1 (the times prior to HAART) should not be significant and negative, because if this was the case, declines in abuse or drug use for the treatment group would have begun prior to the introduction of HAART. Indeed, we find that this is the case. For domestic violence and heroin use, there is no difference in pre-HAART

trends between the treatment group and the control group. For stimulant use, we find that the treatment group did exhibit a rising trend (relative to the control group) in use prior to HAART. However, this rise is opposite to the direction we observe after HAART, which is reassuring because it suggests that post-HAART changes are not driven by trends beginning prior to HAART.

Finally, we note two additional concerns that might threaten the validity of our research design. First, one might be worried that another shift (e.g., a government program or policy change) had an impact on the treatment group, but not on the comparison group (or vise-versa). An obvious candidate is the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which reformed welfare and was signed into law in August of 1996, right at the time HAART was introduced. However, given that the comparison groups are similar among socio-demographic characteristics, including income and education, it is unlikely that welfare reform affected the control group differently than the treatment group.[33] A second concern might be that domestic violence is drastically under-reported. By some measures, 50% of violent episodes go un-reported (Greenfeld et al., 1998). However, this would affect our results only if there were a shift in the magnitude of mis-reporting that only affected the treatment group and, moreover, this shift coincided with the introduction of HAART. Though we cannot rule out this possibility, we believe that it is unlikely.

4 Main Results

In this section, we present our main findings. We show that the treatment group experienced reductions in domestic violence and illicit drug use that the control group did not. We also perform several robustness checks, including showing that our results are not driven by survival bias and are robust to estimation using propensity score matching.

4.1 Health and Domestic Violence

In order to test if the treatment group experienced a reduction in domestic violence after the introduction of HAART, we use a difference-in-differences approach. We estimate probit models where the dependent variable is an indicator of whether a woman experienced domestic violence since her last visit using the following specification:

$$V_{it+1} = X_{it}\beta + HAART_t\alpha + Treatment_i\delta + HAART_t \times Treatment_i\gamma + \epsilon_{it} \qquad (4)$$

[33]Unfortunately, we cannot test this assumption more directly since our data only contain information about welfare participation after the introduction of HAART.

where V_{it+1} indicates if the woman reported violence at $t+1$, which she experienced between periods t and $t+1$. X_{it} is a vector of individual i's characteristics at time t, $HAART_t$ is an indicator variable for HAART being available at time t.[34] $Treatment_i$ is a dummy variable indicating if the woman is in the treatment group. X_{it} includes basic controls: age, age squared, and age cubed at time t, as well as indicator variables for race and site of study.[35] The coefficient of interest is γ, which indicates if the treatment group responded differently to the introduction of HAART than the control group. To control for serial correlation, all specifications are clustered at the individual level (Bertrand et al., 2004).

We find that the treatment group experienced a reduction in domestic violence that the control group did not. We show our findings in two tables. Table 5 presents the marginal effects of the interaction term. We follow Puhani (2012) in calculating marginal effects of the interaction term.[36] Table A3, found in the appendix, shows the coefficients obtained from estimating the probit models. In each table, findings for domestic violence are shown in the first two columns. We show two specifications for both our main sample and the sample consisting of only black women. Column 1 includes the interaction but no other controls, and Column 2 includes the basic controls described above. Our finding that the treatment group experienced a decrease in domestic violence that otherwise similar women did not is robust to this change. It is also interesting that HAART availability is associated with a decline in domestic violence, as shown in the first row of each panel of Table A3. This is consistent with secular declines in domestic violence during this time period (Catalano, 2012).

Turning to the marginal effects, we find that the treatment group experienced a rather large decrease in violence due to the introduction of HAART. Specifically, the decline for the full sample was between 1.5-1.7 percentage points, depending on the specification. Although at first glance this effect appears small, this amounts to a decrease in domestic violence of about 6%. When we restrict the sample to black women, we find similar results. Black women in the treatment group experienced a decline in domestic violence between 2.1-2.4

[34]We use lagged HAART availability to account for the fact that domestic violence and other outcomes are measured since the last visit. For example, consider visits that occurred in September of 1996, the same time as HAART was introduced. At this visit, women were asked about violence that they had experienced in the last six months, roughly the time since their previous visit. However, HAART was not available to them during this time period and therefore they would not have experienced any benefits of this medical innovation.

[35]Although we could also control for other demographic and behavioral controls, such as relationship status, income, and drug use, we acknowledge that these might also change with the introduction of HAART and so we leave them out of our specifications. In results available from the corresponding author, we show that main results are robust to the inclusion of these additional variables

[36]Following Puhani (2012), Karaca-Mandic et al. (2012) also show how to calculate standard errors in the same manner.

percentage points as compared to black women in the high CD4 HIV+ group. This is equivalent to a decrease in violence of between 7 and 8%.

In assessing the magnitude of these declines, it is difficult to find direct comparisons since our study is novel in linking medical innovation and domestic violence. Instead, we compare our findings to research on how policy interventions, shifts of women's resources or other natural experiments affect violence. Heaton (2012) finds that Sunday liquor laws have no effect on domestic violence, while Iyengar (2009) reports that mandatory domestic violence arrest laws actually lead to an increase in intimate partner homicides. More closely related to our study are changes to women's earnings, both absolutely and relative to men's. A key example is Aizer (2010), who shows reductions in violence of about 9% that is explained by a 20-year decline in the male-to-female wage gap. In a recent paper, Cesur and Sabia (2016) show that combat veterans are between three and six percentage points more likely to be violent than veterans who were not assigned to combat zones.

Alternatively, one can look at experimentally assigned conditional cash transfers. Among poor Mexican women, Angelucci (2008) shows relatively modest declines in domestic violence, some of which can be explained by lower alcohol usage among transfer recipients. On the high end in terms of the magnitude of causal effects, Bobonis et al. (2013) report that recipiency of a conditional cash transfer targeting women is associated with a decrease in domestic violence of about 40%. The authors consider data from a program in Mexico called *Oportunidades*, which offers substantial cash transfers to families whose children are in school that amount to about 10% of their average monthly expenditures. We should note that our results are somewhat difficult to compare since they also find an increase in intimidation and threats, which may be evidence of substitution among different forms of abuse.[37]

As discussed in Section 2, black women suffered domestic violence at higher rates than other women in the sample. For ease of exposition, we run separate regressions for black women in our sample. However, in a separate analysis, we consider heterogeneous effects and re-estimate equation (4) including an interaction between black, HAART availability, and an indicator for being in the treatment group. Although the coefficient on the triple interaction is negative, it is not statistically significant, as shown in Table A4. Thus, we cannot rule out that declines for non-black women in our sample were of the same magnitude as declines for non-black women.[38]

[37]In contrast, we find evidence that all forms of abuse, including coercion, decreased. These results are available upon request from the corresponding author.

[38]We show probit coefficients here because of the difficulty calculating appropriate marginal effects of a triple interaction.

4.2 Drug Use

Next, we turn to the study of how the introduction of HAART affected use of stimulants and heroin. Similar to our study of domestic violence, we estimate models of the following form:

$$B_{it+1} = X_{it}\beta^B + HAART_t\alpha^B + Treatment_i\delta^B + HAART_t \times Treatment_i\gamma^B + \epsilon^B_{it}, \quad (5)$$

where B_{it+1} refers to individual i's behavior (e.g., drug use) reported at time $t+1$. Again, $HAART_t$ is an indicator for HAART availability at time t and $Treatment_i$ indicates if individual i is in the treatment group. X_{it} includes the same basic controls discussed above: age, age squared, age cubed, race and site indicators.

We find limited evidence that the treatment group decreased their use of stimulants compared to HIV+ high CD4 count women. Similar to our findings concerning domestic violence, in Table A3 (Columns 3 and 4), we show the results from the probit models and in Table 5, we show marginal effects from the interaction in Equation (5). The results for the full sample are only statistically significant under the most basic specification. In this specification, we find a decrease in stimulant use of 2.9 percentage points, or about 10%. Although γ^B is always negative when we restrict the sample to black women, we cannot rule out the possibility that there is no effect.

Next, we consider heroin use. We find that the treatment group also decreased their use of heroin compared to the high CD4 count HIV+ women, as shown in Table 5 (Columns 5 and 6). The results are robust and the average effects are always significant at at least the 5% level. We find that heroin use decreased by 1.9-2.2 percentage points, or 10-12%. However, when we restrict the sample to only include black women, we find that the decrease is only significant in the most basic specification.

Contextualizing our results is again challenging. Part of this is due to the lack of findings on how policy affects drug use. The WIHS is somewhat unique in that it asks about illicit drug use over time. One related study, Corman et al. (2013), examines the effect of welfare reform on the drug use of women who are at risk of being on welfare. They find that self-reported illicit drug use in the past year (excluding marijuana) fell by about 18% after welfare reform, which changed work incentives for women.

4.3 Robustness Checks

In this section, we discuss results from two main robustness checks. First, we test that survival bias is not driving our results. To do this, we restrict our sample to women who were in the study for at least 15 visits, which is about 7.5 years. As shown in Appendix A, Table A5, which shows marginal effects of our main findings with a restricted sample, our results are not driven by survival bias. In fact, if anything, it appears that our findings on drug use are stronger. Even when we exclude women who died or left the study early, our main findings remain. About 95% of our sample stayed in the study for at least this period of time.[39]

The second important robustness check that we perform is to employ a propensity score analysis. We follow Imbens (2015) and construct normalized differences of our covariates in order to show that there is overlap between our treatment and control group. To test that baseline characteristics are similar between groups, Imbens (2015) suggests a rule of thumb that normalized differences be below 0.25. The majority of our coefficients are below 0.1, as shown in Table A6, found in Appendix A, which provides some evidence that the treatment and control group are comparable. Figure A2 shows that the propensity scores for the treatment group and the HIV+ high CD4 group have good overlap.[40] Looking at the figure, the average of the estimated propensity score is lower for the treatment group, which indicates that the two groups are not comparable, except after appropriate reweighing of the observations. We thus repeat our main analyses using inverse probability weights. Similar to our main specification checks, we jointly test that the pre-HAART trends in domestic violence, stimulant use, and heroin use are different and fail to reject the null hypothesis when using the inverse probability weights ($p = 0.19$). Thus, even when considering weighting by the inverse probability of being in the treatment group, there is little evidence that pre-HAART trends differed between the treatment and healthy HIV+ groups. Turning to the main findings, we find that there are no differences between the treatment group and control group pre-HAART, as shown in Table A7. However, after the introduction of HAART, violence and heroin use fell for the treatment group compared to the control group. These findings are quite similar to those from our main difference-in-differences specifications.

[39] In another test, we show that our results are not driven by simply participating in the WIHS study. We accomplish this by comparing violence and drug use trajectories for women in our analytic sample versus women in the second cohort who entered the sample after HAART was introduced. Results from this test are available from the corresponding author.

[40] We do not present figures from other outcomes, as they are very similar. However, these are available upon request from the corresponding author.

5 Mechanisms

In this section, we further explore possible mechanisms explaining why health improvements lowered drug use and violence. Section 5.1 examines the relationship between illicit drug use and violence. Section 5.2 considers the roles of both physical and mental health improvements. Section 5.3 studies potential HAART-induced improvements in labor market outcomes and explores whether they play a role in explaining or main estimates.

5.1 Relating Drug Use and Violence

We return to the question discussed in Section 3, in which we introduced our conceptual framework. In theory, the finding that HAART reduced both drug use and violence is consistent with several possible mechanisms. One possibility is that HAART affected both independently. Another possibility is that HAART only affected violence through its impact on drug use. The idea is that by lowering drug use, violence mechanically declined. Alternatively, HAART may have reduced violence, which led women to avoid drugs, perhaps experiencing less need for drugs to cope with violence. It would be difficult to distinguish among these possibilities given available data. However, we believe that we can make some progress on the question.

In one set of results, we allow drug use and violence to be jointly determined. In effect, doing so controls for the correlation between drug use and violence. We show that our basic results are qualitatively similar even when we control for this correlation, as shown in Tables A8 (stimulant use) and A9 (heroin use). These results therefore suggest that, even when we control for the correlation between the two outcomes, HAART appears to have an independent effect on both violence and illicit drug use. In other words, one is not simply the by-product of the other.[41]

Second, we note that different drugs appear to have different relationships with abuse. In Table 6, we present results from a regression of violence on drug use, income, employment, and our usual set of controls. We instrument heroin and stimulant use with previous period drug use in order to bypass the possibility of simultaneity. This avoids capturing responses to domestic violence (e.g., using drugs as a coping mechanism). Importantly, we separate heroin use from stimulant use. We find that, whereas use of stimulants (such as crack cocaine) is associated with more violence, use of heroin is in fact associated with less violence. At first

[41] It is worth mentioning that results on joint estimation are not robustness tests of main results. Rather, they serve to examine whether HAART had effects on two different outcomes once we have controlled for correlation among the two outcomes (versus, for example, having a direct effect on one outcome that affects the other).

glance, this result may be unexpected. However, it has some basis in medical literature that studies the impact of drug use on violence, which highlights how heroin has a pacifying or sedating effect on users.[42] The negative coefficient on heroin use helps to bolster the argument that HAART had independent effects on both violence and drug use. The reasoning is as follows. Suppose HAART only affected heroin use and had no impact on violence except through its correlation with drug use. Then, we might expect violence to rise if heroin use went down. Instead, we see both decline. Though this evidence is speculative, these empirical patterns are consistent with the idea that HAART had an independent impact on both violence and illicit drug use.

5.2 Mental and Physical Health Improvements

We now consider two alternative mechanisms which could help to explain our findings: mental and physical health. Starting with mental health, it might be the case that a positive shock in expected health due to the arrival of a new medical technology leads women to exhibit better mental health. If so, we might expect women to perceive better options outside of violent partnerships and to refrain from illicit drug use.[43] In Table 8 (Columns 1 and 2), we use the same difference-in-differences approach as in our main analysis to study depression as the outcome variable. To measure depression, we use the CES-D Score, which is a widely-used measure of depression (with higher numbers being associated with a higher likelihood of depression). We find little evidence that mental health can explain the links between HAART introduction, domestic violence, and illicit drug use. Further, in results available upon request from the corresponding author, we show that controlling for mental health does not change our main results.[44]

Another possible mechanism involves physical symptoms. We begin by documenting that women in the treatment group experienced large increases in their immune system health (CD4 count) in comparison to our control group. In particular, in Table 7, we return to our differences-in-differences framework to show relative increases in CD4 count among women in the treatment group after HAART. This improvement in underlying health may have

[42]See Boles and Miotto (2003) or Volavka (2008) on pharmacologically-induced violence. For the effects of heroin, see, in particular, Jaffe and Jaffe (1999). An underlying and important assumption in relating illicit drug use to domestic violence is that intimate partners are likely to use the same drugs. This assumption has broad empirical support from a variety of fields. See, for example, Vanyukov et al. (1996) for a review of this literature.

[43]A correlation between depressive symptoms and exposure to violence is reported in Johnson et al. (2014), albeit among adolescents. A related view is that lower illicit drug use can improve mental health, which previous research has shown can lead to declines in violence (Devries et al., 2013).

[44]In a related exercise, in results available from the corresponding author, we show that our main findings are also not driven by increased interaction with the medical community after HAART was introduced.

translated to improvements in how women felt after HAART, which could presumably lead to declines in domestic violence or illicit drug use. In Table 8 (Columns 3 and 4), we assess whether women in the treatment group exhibit shifts relative to control group in the probability of experiencing at least one symptom, where symptoms we consider include: fever, memory problems, numbness, weight loss, mental confusion and night sweats. We again return to the original difference-in-differences empirical design.[45] We find little evidence of post-HAART relative declines in reporting at least one of these symptoms for women in the treatment group after HAART.[46] This lack of changes in symptoms is not surprising since, at the time it was introduced, HAART had severe side effects, and so side effects may have simply replaced symptoms, meaning that women would feel about the same even if their underlying health had improved.[47]

In other words, women simply feeling better, measured by a lack of physical ailments, does not appear to be an important mechanism generating our main results.[48] Again, our findings here are perhaps counterintuitive since we argue that a medical innovation affected behavior without affecting symptoms. Recall, however, that women in the treatment group, despite lower CD4 counts at the time of HAART introduction, had yet to experience the symptoms of AIDS. This means that HAART-induced reductions in violence and illicit drug use do not appear to be driven by contemporaneous improvements in how they feel physically as measured by symptoms. Instead, our findings on symptoms suggest that much of the role of HAART in affecting drug use and violence operated through its impact on underlying health, which affected longevity and expected future well-being.

5.3 Employment and Income

Until now, we have shown evidence that HAART reduced both illicit drug use and domestic violence. Moreover, though intuitively appealing, the idea that direct physical and mental health improvements (as measured by symptoms and the CESD score, respectively) led to declines in violence and drug use is not borne out by our data. In this section, we consider changes to labor market outcomes induced by HAART. The idea is that improved labor

[45]For consistency with the CESD Score, we use OLS specifications. However, in results available upon request, we also tried the probit specifications and found no difference.

[46]We also tested individual symptoms along with the total the number of symptoms using a Poisson count and found no difference between the treatment and control group.

[47]In related work, Papageorge (2016) shows that HAART side effects among HIV+ men replaced some of the symptoms of HIV, at least among the sickest patients, so that average reports of physical ailments remained about the same even if underlying health improved dramatically.

[48]However, it is worth reiterating the idea that these results on symptoms bolster the argument that women in the treatment group are comparable to the control group in that, despite having lower CD4 counts, they are not physically ill prior to the introduction to HAART.

market prospects might help to explain why women face stronger incentives to desist from drug use or perceive better options outside of violent partnerships. Further, our conceptual framework uses the idea that health is a form of human capital and that women with more human capital are less likely to use illicit drugs and suffer abuse. Showing evidence of HAART-induced improvements in labor market outcomes would help to support idea that health is a form of human capital.

To assess HAART-induced differences in labor market outcomes, we return to the difference-in-differences specification shown earlier. We consider employment at the time of visit as the labor market outcome of interest. The marginal effects are presented in Table 9. Estimates show that the treatment group became relatively more likely to be employed after HAART. Black women in the treatment group became much more likely to be employed relative to black women in the HIV+ high CD4 count group for all of our specifications. This amounts to a increase in the probability of employment for the treatment group of about 4.8-5.5 percentage points, or about 12-14%, for the full sample and 7.7-8.1 percentage points, or 20%, for the black women subsample.

Our findings on employment are broadly consistent with those in Goldman and Bao (2004), who also study HAART and employment. They show that HAART use increased the probability that HIV+ individuals kept working by 37%. Our results are smaller for at least two reasons. First, we do not condition on HAART use as they do and only rely on HAART introduction (similar to an intent-to-treat analysis). Second, their finding conditions on working at the time of HAART introduction while ours does not. Indeed, individuals in our sample are not highly educated and do not exhibit strong ties to the labor market before the introduction of HAART. Perhaps more comparable to our setting, Goldman and Bao (2004) find no effect of HAART on the likelihood of returning to work, conditional on not working prior to HAART introduction. Compared to their estimates for non-workers, our findings on employment are relatively large.[49]

Though our results on labor market outcomes provide support for the idea that HAART boosted women's labor market human capital, the precise mechanism connecting our findings is difficult to identify. We are able to verify that women with better labor market prospects are better able to avoid abusive partnerships. Among women in the study, we use individual level fixed effects and find that income and employment are associated with lower rates of

[49]Our results on employment are broadly consistent with literature considering how HAART affected labor market outcomes in developing countries, specifically in the continent of Africa. For example, Thirumurthy et al. (2008) find that antiretroviral therapy is associated with an increased attachment to the labor force, in terms of both participation and hours, for patients in western Kenya. Habyarimana et al. (2010) document patterns of absenteeism in Botswana and provide evidence that an increase in CD4 count decreases illness-related absence from work.

violence. We show this using a linear probability model in which we regress domestic violence on employment in the same period, income, various sets of sociodemographic measures, and individual fixed effects. In the final specification with the largest set of controls, including health, socio-demographics and risky behaviors, we find that being employed lowers the likelihood of being abused by 0.8 percentage points, as shown in Table 10.

We are also able to verify that drug use and violence are negatively correlated with employment among women in our sample.[50] However, there are several conceptual frameworks that could explain why HAART would lower drug use and violence and also have an impact on employment and income. For example, reductions in drug use and violence would presumably make working easier. Alternatively, it could be that health improvements improve labor market outcomes directly and women with better labor market prospects see higher costs of drug use and have more resources to leave partners. Even further, if HAART improves labor market prospects, it could incentivize further investments in human capital. In the case of poor women with HIV, avoiding drug use could therefore be seen as an investment in future earnings (Becker and Murphy, 1988). Given the data we have, we are unable to distinguish among these mechanisms; indeed, it is possible that all of them play a role. However, our findings on labor market outcomes help to bolster the conceptual framework we have proposed to understand our empirical findings. HAART improved women's lives on a number of dimensions, including domestic violence, illicit drug use and, as our final estimates suggest, labor market prospects. The broad underlying mechanism is that a new medical technology, by improving health trajectories, boosted women's human capital.

6 Conclusion

We have presented evidence showing that HAART lowered domestic violence by 6% and reduced drug use by 10% among a group of low-income, predominantly black, HIV+ women. The fact that HAART marked a massive improvement over previous HIV treatments, together with the seriousness of HIV, enable us to detect subtle, indirect behavioral effects of this pharmaceutical innovation on drug use and domestic violence. Our results provide evidence consistent with the hypothesis that HAART functioned as a positive health shock that increased women's human capital with possible benefits in several dimensions. Policies surrounding both domestic violence and drug use often utilize criminal sanctions and attempt to change attitudes; policies directed specifically at women also attempt to provide support for those who have been abused. Our findings suggest a complementary approach

[50] These results are shown in Appendix A, Tables A10 and A11.

focusing on interventions that increase women's human capital.

How far our results generalize to other groups, to *negative* health shocks, to other chronic illnesses and to behaviors other than domestic violence and drug use are, of course, open questions. We have studied a particular population and a particular medical condition that is accompanied by stigma, depression and physical deterioration in ways that other chronic illnesses are not. For example, it would be difficult to argue that our results extend to high-income women with stable home lives and strong ties to the labor market. Nevertheless, we cannot resist speculating on the broader implications of our results.

Our results suggest the potentially far-reaching implications of behavioral responses to medical innovations. In this respect, our results are analogous to those of Goldin and Katz (2002) which documents the effect of the birth control pill on women's education, career choices, employment, marriage, and childbearing. Our findings suggest that policies that provide access to better health care and enhance human capital can alleviate persistent and intractable social problems. Finally, by isolating an under-recognized source of value in pharmaceutical innovation, our findings have implications for the optimal allocation of biomedical research investment: ignoring behavioral responses to improvements in medical technology can lead to under investment in and misallocation of biomedical research dollars.

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7 Figures and Tables

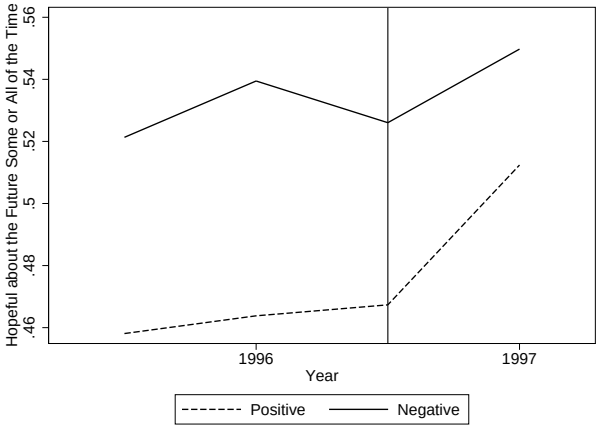
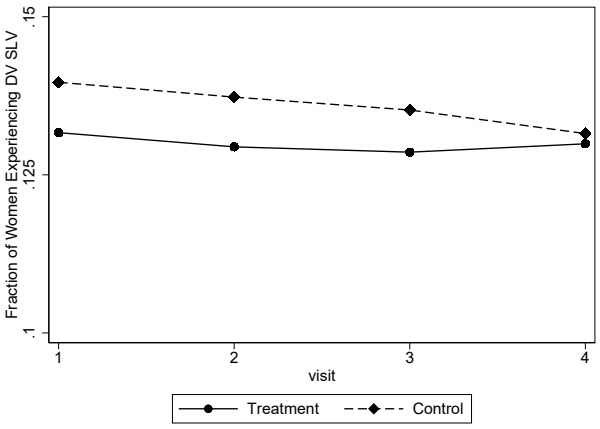
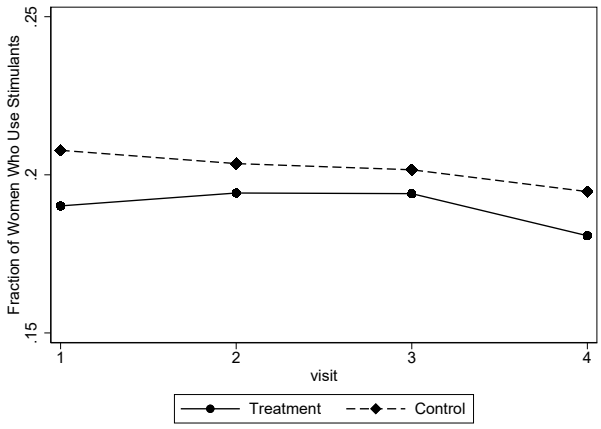


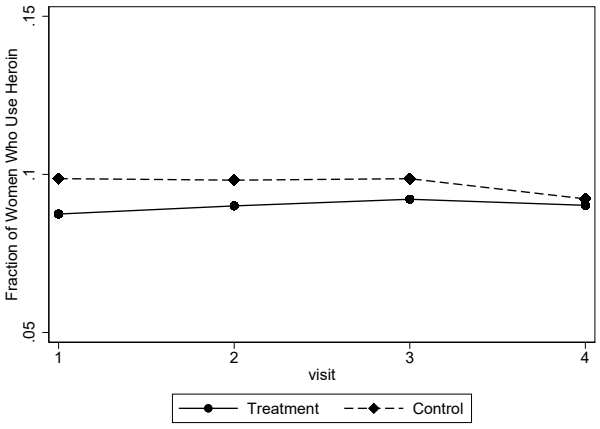
Figure 1: This figure shows the probability of reporting being hopeful about the future sometimes or all of the time the week before the visit for HIV+ and HIV− women.



(a) Domestic Violence



(b) Stimulant Use



(c) Heroin Use

Figure 2: This figure shows pre-HAART trends in outcomes.

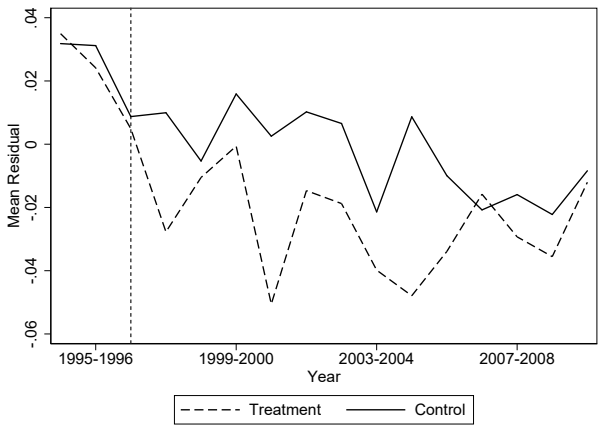


Figure 3: This figure shows residuals for the treatment group and the HIV+ high CD4 count group from a probit model of experiencing domestic violence, controlling for age, age squared, age cubed, race and site dummies.

Table 1: Relationship Between Health, Drug use, and Employment

	Full Sample
Domestic Violence	
CD4 count	.00007
	(.0001)
Obs.	13914
Stimulant Use	
CD4 count	-.0004***
	(.0001)
Obs.	31165
Heroin Use	
CD4 count	-.0001
	(.0001)
Obs.	31159
Employment	
CD4 count	.0005***
	(.00007)
Obs.	31305

This table shows estimates from probit regressions relating health to domestic violence, illicit drug use and employment. Each specification is clustered at the individual level and includes controls for age at visit, age squared, age cubed, race dummies, and site dummies.

Table 2: Domestic Violence Statistics by time of AIDS Diagnosis

	Last visit pre AIDS	First visit with AIDS
Sexual Abuse	1.1	1.4
Physical Abuse	4.7	4.2
Coercion	10.5	8.7
Domestic Violence	11.9	9.5
Stimulant Use	20.3	15.2
Heroin Use	9.5	6.6
Employed	17.0	19.5
Observations	800	2508

This table shows the percent of women who experienced each type of abuse in the six months prior to their last visit before an AIDS diagnosis and the six months prior to their first visit with AIDS. The sample is restricted to HIV+, low CD4 count women from the first cohort before HAART was introduced. AIDS is defined as having a CD4 count less than or equal to 200.

Table 3: Summary Statistics

	(1) Treatment Group	(2) High CD4 HIV+	(3) Equal Means p-value
Average Age	42	41	.23
African American	67	64	.41
Hispanic	20	22	.68
White (Non-Hispanic)	12	12	.95
Other	1	3	.13
Education:			
LT high school	37	41	.51
High school grad	30	30	.94
Some college	23	22	.82
College grad	10	7	.34
Pre-HAART Income:			
$\leq$ 6000	17	17	.97
6000-12000	33	33	.93
12001-18000	13	14	.72
18001-24000	11	10	.64
24001-30000	7	9	.33
$>$ 30000	19	17	.60
Employed pre-HAART	38	43	.25
Married pre-HAART	32	25	.11
Lived with kids at baseline	51	47	.38
Risky Behaviors Pre-HAART:			
Ever smoked	66	70	.32
Abstainer	36	33	.54
Light drinker	32	28	.36
Moderate drinker	20	23	.61
Heavy drinker	12	17	.17
Ever used crack	22	23	.79
Ever used cocaine	17	21	.34
Ever used pot	32	33	.80
Ever used heroin	18	16	.50
Ever used hard drugs	33	33	.97
Ever used stimulants	28	31	.52
Symptoms pre-HAART:			
Memory problems	31	36	.22
Numbness	39	42	.61
Weight loss	33	27	.23
Mental confusion	17	20	.50
Night sweats	35	41	.22
Domestic Violence:			
Experienced sex abuse	5	10	.09
Experienced physical abuse	17	19	.65
Experienced coercion	26	28	.59
Experienced domestic violence	27	34	.14
Observations	166	269	
Person-Visits	2477	4192	

The full sample includes all women from the first cohort who answered questions about domestic violence, employment, and illicit drug use, as well as all controls used. The treatment group is defined as having a minimum pre-HAART CD4 count between 300 and 399. High CD4 refers to minimum pre-HAART CD4 count greater than or equal to 400. Income is measured as yearly household income. Light, moderate and heavy drinking means $<$ 3, 3-13 and $>$ 13 drinks per week, respectively. Hard drugs are defined as crack, cocaine, heroin, or (illicit) methadone. Stimulants are crack or cocaine. Domestic violence is defined as physical or sexual abuse or coercion by an intimate partner or spouse. Coercion indicates that the partner threatened to hurt or kill the subject or prevented her from: leaving or entering her home, seeing friends, making telephone calls, getting or keeping a job, continuing her education, or seeking medical attention. Column (3) shows p-values from the tests of differences in means between the treatment group and the HIV+ high CD4 count group.

Table 4: Test of Equality of Pre-HAART Trends

Outcome	p-value
Domestic Violence	0.237
Stimulant Use	0.090
Heroin Use	0.383
Joint Significance	.124

This table shows the p-values from tests that the pre-HAART trends in outcomes are parallel. For each outcome, we regress the outcome on age, age squared, race indicators, site of visit indicators, six-month time dummies, an indicator for being in the treatment group, and an interaction of the time bins and the treatment group. We then test that the pre-HAART interactions are zero.

Table 5: Health, Violence and Drug Use: Marginal Effects

	Domestic Violence		**Stimulant Use**		**Heroin Use**	
	[1]	[2]	[3]	[4]	[5]	[6]
Full Sample						
Treatment × HAART	-0.015*	-0.017*	-0.029**	-0.022	-0.022***	-0.019**
	(0.008)	(0.009)	(0.013)	(0.014)	(0.007)	(0.008)
	0.068	0.063	0.027	0.109	0.003	0.011
Observations	6669	6669	16265	16265	16261	16261
Black Sample						
Treatment × HAART	-0.021**	-0.024**	-0.034	-0.028	-0.022**	-0.016
	(0.011)	(0.011)	(0.021)	(0.022)	(0.010)	(0.011)
	0.044	0.034	0.103	0.201	0.037	0.151
Observations	4281	4281	9355	9355	9352	9352
Basic Controls	N	Y	N	Y	N	Y

This table shows the marginal effects of the interaction term from the difference in difference probit models. Standard errors are presented in parenthesis, and p-values are found below. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table 6: Drug Use and Violence

	[1]	[2]	[3]
Heroin use	-.143***	-.141***	-.143***
Stimulant use	.144***	.142***	.119***
Age	-.010	-.010	-.009
Age squared	.00004	.00005	.00004
Age cubed	1.62e-07	1.54e-07	1.86e-07
Yearly income 6001-12000	.	-.007	-.007
Yearly income 12001-18000	.	-.009	-.009
Yearly income 18001-24000	.	-.005	-.004
Yearly income 24001-30000	.	-.015	-.015
Yearly income > 30000	.	.0001	.0005
Employed	.	.002	.002
Yearly income 6001-12000, employed	.	-.002	-.002
Yearly income 12001-18000, employed	.	-.002	-.002
Yearly income 18001-24000, employed	.	-.019	-.020
Yearly income 24001-30000, employed	.	.004	.004
Yearly income > 30000, employed	.	-.006	-.006
Married	.	.014*	.014*
Not married, lives with prtnr	.	.020***	.020***
Widowed	.	-.012	-.012
Divorced/Annuled	.	.005	.005
Separated	.	.009	.008
Other Marital Status	.	.003	.003
Used marijuana SLV	.	.	.018***
Never smoker	.	.	.031
Current smoker	.	.	.011
Light (lt 3 drinks/wk)	.	.	-.003
Moderate (3-13 drinks/wk)	.	.	.005
Heavier (gt 13 drinks/wk)	.	.	.026**
No. male sex prtnr SLV	.	.	.00005
Obs.	17906	17906	17906

This table shows estimates from regressions of violence on illicit drug use. The sample is restricted to women from the first cohort who answered questions about domestic violence, which is defined in Table 3. Each specification uses individual level fixed effects.

Table 7: Difference-in-differences: CD4 Count

	[1]	[2]
Panel A: Full Sample		
HAART available	-79.940***	-73.840***
	(12.824)	(13.338)
Treatment Group	-252.501***	-245.119***
	(14.061)	(14.442)
Treatment × HAART	137.740***	136.016***
	(21.814)	(21.613)
Obs.	6524	6524
Panel B: Black Sample		
HAART available	-90.840***	-85.041***
	(17.268)	(17.701)
Treatment Group	-267.766***	-265.419***
	(17.346)	(18.838)
Treatment × HAART	142.861***	139.667***
	(25.966)	(25.727)
Obs.	4189	4189
Basic Controls	N	Y

This table shows estimates from OLS difference-in-differences models where the outcome variable is CD4 count. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table 8: Depression Score and Symptoms

	CESD Score		Any Symptoms	
	[1]	[2]	[3]	[4]
Full Sample				
HAART available	-2.734***	-3.328***	-.017	-.080***
	(.441)	(.507)	(.016)	(.018)
Treatment Group	-2.406***	-2.507***	-.016	-.022
	(.871)	(.872)	(.030)	(.029)
Treatment × HAART	.501	.803	.011	.028
	(.720)	(.718)	(.027)	(.026)
Obs.	14324	14324	16765	16765
Black Sample				
HAART available	-3.478***	-4.223***	-.004	-.068***
	(.611)	(.666)	(.021)	(.023)
Treatment Group	-3.398***	-3.074***	.023	.026
	(1.067)	(1.056)	(.040)	(.038)
Treatment × HAART	1.486	1.702*	-.045	-.026
	(.940)	(.936)	(.035)	(.034)
Obs.	8205	8205	9608	9608
Basic Controls	N	Y	N	Y

This table shows estimates from OLS difference-in-differences models. The first outcome variable is CES-D Scale Score, where higher values mean depression is more likely. The second outcome is having any symptom(fever, memory problem, numbness, weight loss, mental confusion, or night sweats). Results for having any symptom are robust to a probit specification, and available upon request. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table 9: Employment, Marginal Effects

	[1]	[2]
Full Sample		
Treatment × HAART	0.055*	0.048*
	(0.030)	(0.029)
	0.065	0.099
Observations	16348	16348
Black Sample		
Treatment × HAART	0.081*	0.077*
	(0.041)	(0.040)
	0.051	0.054
Observations	9407	9407
Basic Controls	N	Y

This table shows the marginal effects of the interaction term from the difference in difference probit models. The outcome of interest is being employed at the time of visit. Standard errors are presented in parenthesis, and p-values are found below. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table 10: Domestic Violence and Resources

	[1]	[2]	[3]	[4]	[5]
Employed	-.012***	.	-.011**	-.010**	-.008*
Yearly income 6001-12000	.	-.009*	-.008*	-.008*	-.007
Yearly income 12001-18000	.	-.016***	-.015**	-.015***	-.015***
Yearly income 18001-24000	.	-.011*	-.010	-.011	-.011
Yearly income 24001-30000	.	-.020***	-.018**	-.019**	-.020***
Yearly income > 30000	.	-.003	-.0006	-.002	-.004
Age	-.025***	-.025***	-.024***	-.025***	-.025***
Age squared	.0003	.0003	.0003	.0003	.0003*
Age cubed	-1.12e-06	-8.32e-07	-8.53e-07	-9.14e-07	-9.60e-07
Obs.	24896	25526	25526	25526	25526
Basic Controls	Y	Y	Y	Y	Y
Demographic Controls	N	N	N	Y	Y
Risky Behaviors	N	N	N	N	Y

This table shows results from an OLS model with individual level fixed effects estimates where the outcome variable is experiencing domestic violence since the last visit, which is defined in Table 3. Basic controls include age, age squared, and age cubed. Demographic controls include indicator variables for marital status (never married omitted). Risky behaviors include indicator variables for drug use, cigarette smoking, alcohol use, and the number of male sex partners.

Appendix A

In this appendix, we discuss the model of human capital investment, as well as additional tables and figures from "Health, Human Capital and Domestic Violence." We discuss these findings in the order that they appear in the main text.

First, we present a simple model of consumption and human capital investment. In this framework, human capital investment is determined by the probability of surviving until the next period. We show that relatively sick women, such as those in the treatment group, will have a larger response to new medical technology, such as HAART, than relatively healthier women, such as those in the control group.

To test that observations are missing at random, we regress missing visits on an indicator for the treatment group, lagged CD4 count, and interaction between lagged CD4 and the treatment group along with the basic controls discussed in Section 4. Table A2 shows the coefficients on the interaction. Although health is a significant predictor of missing a visit, we find that health does not have differential effects on the likelihood of missing a visit in the treatment group versus the control group.

In addition to the marginal effects presented in the main text, we also present the probit coefficients in Table A3. The table is organized similarly to Table 5 in the main text.

To verify that the differences-in-differences approach is valid, we conduct an event study. We regress each outcome of the main outcomes we study (domestic violence, stimulant use and heroin use) on dummies for the periods leading up the the introduction of HAART and the periods after HAART introduction, an indicator for the treatment group, and interactions between the treatment group and the lead/lag periods. We also include the basic controls discussed in Section 4. In Figure A1 we show results from the interactions between the treatment group and the time periods, plotting coefficients. Importantly, we find that the periods leading up to HAART are never significantly negative, implying that our findings are not driven by trends that existed before the introduction of HAART.

To test that black women were affected more by the introduction of HAART, we interact an indicator for the treatment group with HAART availability and being black. Table A4 shows the findings from this triple interaction and is described in Section 4.1.

To test that our results are not driven by survival bias, we estimate models including only women who stayed in the survey for at least 15 visits (about 7.5 years). These results are shown in Table A5. As an additional robustness check, we also conduct our analysis using propensity score weighting. Table A6 shows the normalized differences for observable characteristics of the sample. Given that these differences are all very small, we conclude that

the treatment group and high CD4 count HIV+ women are quite similar prior to HAART. In fact, no outcomes are above the threshold of .25 suggested by Imbens (2015). Table A7 shows the results from our propensity score estimation. We estimate both a linear specification and a quadratic specification. Again, we follow the algorithm proposed by Imbens (2015) in choosing the controls for the propensity score matching. Table A7 shows that prior to HAART, the groups were very similar in terms of violence, stimulant use and heroin use. However, we find that the introduction of HAART had a significant impact on the treatment group in comparison to the control group. Propensity score matching requires that the two groups have good overlap of the score, and we show that this is the case in Figure A2. This figure shows the overlap in propensity score between the treatment group and the HIV+ high CD4 count women for domestic violence. The figures for other outcomes are quite similar and are available upon request from the corresponding author.

Turning to mechanisms, we allow for the fact that drug use and domestic violence may be correlated and jointly estimate the impact of HAART on violence and stimulant use in Tables A8 and heroin use in Tables A9. Finally, Tables A10 and A11 show regressions examining the relationships amongst employment, health, and stimulant use and heroin use, respectively.

Appendix A.1 Model

Suppose there are two women, indexed by $i \in 1, 2$. Both women solve a two-period problem where the probability of survival through the end of the second period is given by $\rho_i \in (0, 1)$. Each woman has period log utility of consumption, where consumption in period 1 for woman i is denoted c_{i1}. At the start of the first period, both women are endowed with 1 unit of consumption goods. Anything the women do not consume in period 1 is invested in their human capital h_i. Human capital raises earnings and thus consumption in period 2, which is denoted c_{i2}. Assume that each unit of human capital generates one unit of earnings e_i, which is then consumed in the second period. Thus, $c_{2i} = e_i = h_i = (1 - c_{1i})$. Given ρ_i, women solve the following optimization problem:

$$\max_{c_{i1}} V_i = \ln(c_{i1}) + \rho_i \ln(1 - c_{i1})$$

Optimal consumption is:

$$c^*_{i1} = \frac{1}{1 + \rho_i}$$

It follows immediately from the above expression that c_{i1} falls when ρ_i increases. The intuition is that investments in human capital, which decrease period 1 consumption and raise period 2 consumption, increase when the likelihood of surviving through the second period increases (Ben-Porath, 1967). Moreover, using the expression, we can show that relatively large increases in ρ_i lead to relatively large investments in human capital. One way to show this is to compute the difference:

$$\frac{1}{1 + \rho_i + \delta} - \frac{1}{1 + \rho_i} \tag{6}$$

where $\delta > 0$ represents the positive change in survival probability and where $(\rho + \delta) \in (0, 1)$, to insure that the new survival probability does not exceed 1. It can be shown that the derivative of the above difference taken with respect to δ is strictly positive. The intuition is that relatively large changes to survival probability incentivize relatively large changes to investments in human capital. The result is driven by the fact that survival probability ρ_i enters the two-period problem linearly.

Until now, the model is not specific to the context we study Now, we consider the implications of assumptions that are specific to HIV/AIDS and the introduction of HAART. Without loss of generality, suppose that woman 1 is in the treatment group and that woman 2 is in the control group. Woman 1 has a lower CD4 count than woman 2 and thus faces a lower

probability of survival through the second period.[51] In the model, this is operationalized by the assumption that $\rho_1 < \rho_2$. Next, we assume that HAART introduction raises survival probability to $\bar{\rho}$, where we assume that:

$$\bar{\rho} \geq \rho_2 > \rho_1 \tag{7}$$

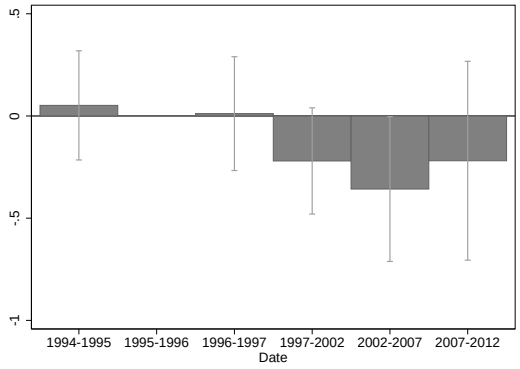
Given model assumptions, woman 1 will experience larger changes in her survival probability than woman 2. To see this, note using the previous expression $\bar{\rho} - \rho_1 > \bar{\rho} - \rho_2$. This is a crucial assumption that arises for two reasons. One, medical guidelines suggest commencement of HAART at CD4 counts around 350, which is nearly the CD4 count of women in the treatment group, but not in the control group, whose CD4 counts are higher. Thus, women in the control group may experience no immediate benefit to HAART. Second, HAART allows sicker women to recover a healthier survival probability, which means that HAART leads woman 1's survival probability to reach that of woman 2, but would leave woman 2's survival probability (which has not yet declined) unchanged. These possibilities are captured by the differences between $\bar{\rho}$ versus ρ_1 and ρ_2. Given the reasoning discussed above, the introduction of HAART will have a larger impact on the survival probability of woman 1, the sicker woman, and thus a larger impact on her investments in human capital.

To fix ideas, consider the following example. Suppose $\rho_1 = 0$ and $\rho_2 = 0.5$. Then, $c_{11} = 1$ and $c_{12} = 2/3$. Woman 1, foreseeing a low probability of survival until the next period makes no investment in her human capital. Woman 2, foreseeing a higher probability of survival until the next period makes a positive investment in her human capital. Next, suppose a technology arrives. It raises ρ_i to 1 for both women. Now, woman 1 and woman 2 will each consume $c_{1i} = 1/2$. Thus, the technology shift has lowered consumption in period 1 from 2/3 to 1/2 for the relatively healthy woman, raising her human capital investment by 1/6 of the total endowment. The technology shift has raised the human capital investment of the sicker woman by 1/2, which is larger than 1/6. In other words, if the technology shifts incentives for human capital investments, we would expect a larger shift for the relatively sick woman, even though both women are affected by it.[52]

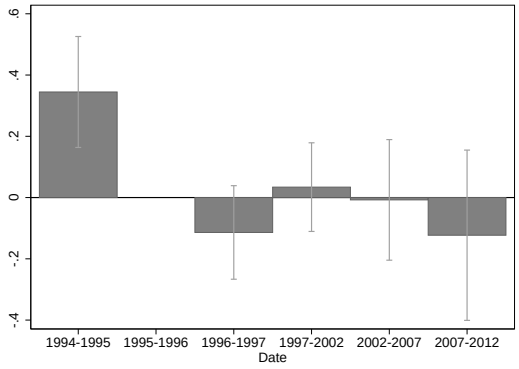
[51] Women in the treatment do not face contemporaneous changes in survival, but expect to sooner than women in the control group, which is captured by different survival rates for the second period. The model could accommodate this more explicitly if, for example, we included a third period with lower survival probability for the healthier women in the third period. We simplify the model by maintaining the assumption of a single future period. One way to view this is that each period encompasses several 6-month periods and that the likelihood of survival through the first period is assured for all women, while the likelihood of surviving the full second set of periods is higher for woman 1.

[52] This model could be extended to account for the possibility that failing to make human capital investments could be very costly for some women, which would overwhelm the differences shown in the current version of the model. For example, women who are symptomatic and exhibit AIDS-level CD4 counts may

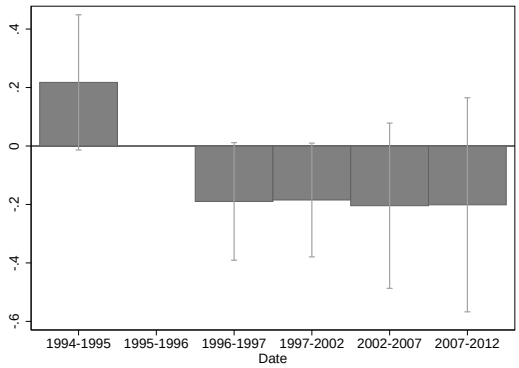
Appendix A.2 Supplemental Tables and Figures



(a) Domestic Violence, HIV+ High CD4



(b) Stimulant Use, HIV+ High CD4



(c) Heroin Use, HIV+ High CD4

Figure A1: This figure shows coefficients of the interaction between the treatment group and the periods leading up to and lagging HAART. Each bar represents the estimated coefficient and the capped, vertical line show the estimated 90% confidence interval.

not use illicit drugs not because they are incentivized to make choices with long-run benefits, but because drug use implies a high utility cost.

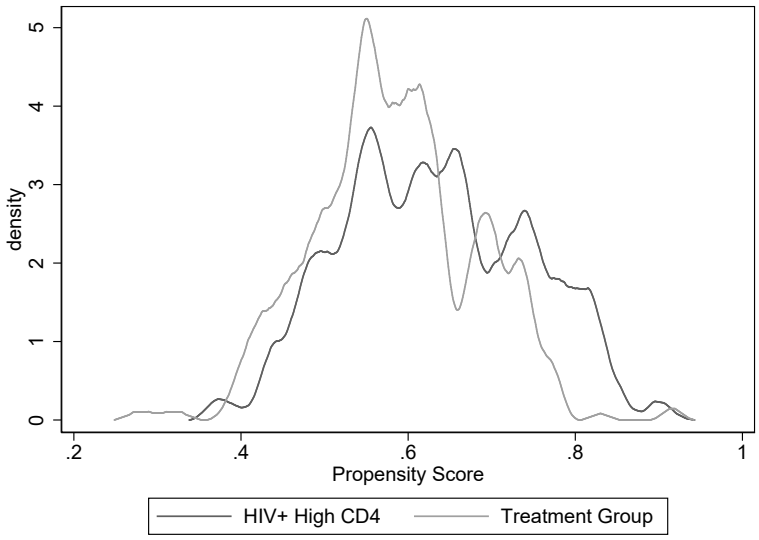


Figure A2: This figure shows overlap of the estimated propensity score using a linear specification for the treatment group and for HIV+ high CD4 count women.

Table A1: Test of Non-Random Attrition

	Leaving the Sample			No. of Visits in Study		
Treatment Group	.147	.093	.098	-.695	-.656	-.645
	(.215)	(.229)	(.229)	(.817)	(.799)	(.802)
Age	.	.054	.049	.	.681	.678
		(.509)	(.508)		(1.592)	(1.594)
Age squared	.	.001	.001	.	-.023	-.023
		(.013)	(.013)		(.042)	(.042)
Age cubed	.	-.00002	-.00002	.	.0002	.0002
		(.0001)	(.0001)		(.0004)	(.0004)
Black	.	.117	.100	.	-1.143	-1.177
		(.356)	(.358)		(1.228)	(1.239)
Hispanic	.	-.633	-.641	.	1.673	1.660
		(.438)	(.438)		(1.474)	(1.477)
Other race	.	-.128	-.157	.	-1.990	-2.022
		(.917)	(.924)		(3.037)	(3.044)
Violence pre-HAART	.	.	.101	.	.	.187
			(.242)			(.842)
Obs.	435	435	435	435	435	435

Columns 1-3 show estimated coefficients from a logit model where the outcome is leaving the study at anytime. Columns 4-6 show results from an OLS model where the outcome is the number of visits that the woman stays in the study. In every specification, site of visit is controlled for (Chicago omitted).

Table A2: Missing Outcomes

	Domestic Violence	Stimulant Use	Heroin Use	Employ-ment	Income
Treatment Group	-.031	-.001	-.002	-.008	.018
	(.052)	(.057)	(.057)	(.056)	(.026)
Log CD4	-.015***	-.020***	-.019***	-.019***	-.001
	(.002)	(.003)	(.003)	(.003)	(.001)
Treatment × CD4	.006	.001	.002	.002	-.003
	(.009)	(.010)	(.010)	(.009)	(.004)
Obs.	23215	23215	23215	23215	23215
Basic Controls	Y	Y	Y	Y	Y

This table shows results from an OLS model where the outcome variable is an indicator for missing an observation for the event listed. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table A3: Health, Violence and Drug Use: Probit Coefficients

	Domestic Violence		**Stimulant Use**		**Heroin Use**	
	[1]	[2]	[3]	[4]	[5]	[6]
Full Sample						
HAART available	-.365***	-.235***	-.203***	-.304***	-.163***	-.273***
	(.075)	(.085)	(.052)	(.058)	(.058)	(.068)
Treatment Group	-.061	-.023	.043	.071	.091	.122
	(.116)	(.116)	(.098)	(.099)	(.113)	(.114)
Treatment × HAART	-.202*	-.213*	-.171**	-.129	-.276***	-.242**
	(.117)	(.121)	(.080)	(.084)	(.098)	(.104)
Obs.	6669	6669	16265	16265	16261	16261
Black Sample						
HAART available	-.411***	-.321***	-.246***	-.355***	-.177**	-.365***
	(.092)	(.098)	(.068)	(.072)	(.081)	(.082)
Treatment Group	-.004	.039	.085	.130	.086	.096
	(.140)	(.141)	(.126)	(.128)	(.147)	(.152)
Treatment × HAART	-.273*	-.289*	-.165	-.152	-.267*	-.216
	(.145)	(.148)	(.105)	(.108)	(.139)	(.143)
Obs.	4280	4280	9355	9355	9352	9352
Basic Controls	N	Y	N	Y	N	Y

This table shows difference-in-differences probit model estimates where the outcome variable is having experienced domestic violence since the last visit. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table A4: Heterogeneity in Effects of HAART on Domestic Violence

	[1]	[2]
Panel A: Treatment vs High CD4 HIV+ Women		
HAART available	-.269**	-.123
	(.128)	(.141)
Treatment Group	-.230	-.205
	(.202)	(.204)
Black	.242	.462**
	(.153)	(.180)
Treatment × HAART	-.005	.002
	(.186)	(.197)
AA × HAART	-.142	-.162
	(.157)	(.164)
Treatment × AA	.226	.249
	(.246)	(.247)
Treatment × AA × HAART	-.267	-.296
	(.235)	(.245)
Obs.	6669	6669
Basic Controls	N	Y

This table shows difference-in-differences probit model estimates are presented where the outcome variable is experiencing domestic violence since the last visit. We also include dummy variables and interactions for women who are black to estimate heterogeneity in effects of HAART by race. Basic controls include age at visit, age squared, age cubed and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level.

Table A5: Health, Violence and Drug Use: Marginal Effects of Women Who Did Not Die Within 7.5 Years of Study

	Domestic Violence		**Stimulant Use**		**Heroin Use**	
	[1]	[2]	[3]	[4]	[5]	[6]
Full Sample						
Treatment × HAART	-0.016*	-0.017*	-0.034**	-0.031**	-0.022***	-0.021***
	(0.008)	(0.009)	(0.013)	(0.013)	(0.007)	(0.007)
	0.059	0.064	0.011	0.016	0.003	0.003
Observations	6448	6448	15860	15860	15857	15857
Black Sample						
Treatment × HAART	-0.021**	-0.023**	-0.044**	-0.043**	-0.021**	-0.018*
	(0.011)	(0.011)	(0.020)	(0.021)	(0.010)	(0.010)
	0.049	0.041	0.031	0.035	0.028	0.057
Observations	4097	4097	9047	9047	9044	9044
Basic Controls	N	Y	N	Y	N	Y

This table shows the marginal effects of the interaction term from the difference-in-differences probit models. Standard errors are presented in parenthesis, and p-values are found below. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual level. The sample is restricted to women who participated in the study for at least 15 visits (7.5 years).

Table A6: Normalized Differences

	HIV+, Healthy Sample
African American	0.08
Hispanic	-0.03
White	-0.02
Other race	-0.16
Max income pre-HAART	-0.00
Max inc pre-HAART $<$ 6000	0.00
Max inc pre-HAART 6001-12000	0.02
Max inc pre-HAART 12001-18000	-0.03
Max inc pre-HAART 18001-24000	0.04
Max inc pre-HAART 24001-30000	-0.09
Max inc pre-HAART $>$ 30000	0.04
Age at visit	0.19
Bronx	0.05
Brooklyn	-0.07
DC	0.24
LA	-0.09
Less than HS	-0.08
HS graduate	0.00
Some college	0.08
College graduate	0.09
Married pre-HAART	0.15
Lived with kids at baseline	0.09
Experienced DV pre-HAART	-0.13
Stimulant use pre-HAART	-0.05
Heroin use pre-HAART	0.07
Employed pre-HAART	-0.13
Smokes	-0.09
Drinks	-0.01
Time since 1^{st} + HIV test	0.16
Observations	263

This table shows normalized differences between the treatment group and HIV+ high CD4 count women.

Table A7: Propensity Score Results

	Pre-HAART		Post-HAART	
	Linear	Quadratic	Linear	Quadratic
Panel A: Treatment vs High CD4 HIV+				
Domestic Violence	0.0093	-0.0001	-0.0337***	-0.0388***
	(0.0178)	(0.0190)	(0.0121)	(0.0123)
	1329	1294	1697	1675
Heroin Use	0.0084	0.0087	-0.0177***	-0.0135**
	(0.0112)	(0.0119)	(0.0053)	(0.0053)
	1435	1325	5192	4722
Stimulant Use	0.0149	0.0032	0.0022	-0.0050
	(0.0151)	(0.0158)	(0.0084)	(0.0082)
	1351	1348	4670	4721

This table shows the average treatment effects from propensity score matching. We show findings from both a linear and quadratic specification. Controls following the algorithm proposed by Imbens (2015) are included. For each outcome, we show the estimated coefficient, standard error in parenthesis, and number of observations.

Table A8: Joint Estimation: Domestic Violence and Stimulant Use, Treatment Group vs HIV+ High CD4 Count Women

	[1]	[2]
Panel A: Full Sample		
Domestic Violence		
HAART available	-.374***	-.248***
	(.074)	(.084)
Treatment Group	-.070	-.028
	(.115)	(.116)
Treatment × HAART	-.188	-.201*
	(.117)	(.121)
Obs.	6669	6669
Stimulant Use		
HAART available	-.154**	-.190***
	(.061)	(.068)
Treatment Group	.020	.047
	(.126)	(.125)
Treatment × HAART	-.123	-.121
	(.099)	(.101)
Obs.	6669	6669
Rho	0.331***	0.325***
Panel B: Black Sample		
Domestic Violence		
HAART available	-.418***	-.331***
	(.092)	(.098)
Treatment Group	-.011	.036
	(.140)	(.141)
Treatment × HAART	-.262*	-.279*
	(.145)	(.148)
Obs.	4280	4280
Stimulant Use		
HAART available	-.151**	-.209**
	(.077)	(.085)
Treatment Group	.178	.211
	(.155)	(.159)
Treatment × HAART	-.151	-.141
	(.119)	(.122)
Obs.	4280	4280
Rho	0.319***	0.319***
Basic Controls	N	Y

This table shows difference-in-differences estimates from a bivariate probit model where the outcome variables are domestic violence and stimulant use and the control group consists of relatively healthy HIV+ women. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual.

Table A9: Joint Estimation: Domestic Violence and Heroin Use, Treatment Group vs HIV+ High CD4 Count Women

	[1]	[2]
Panel A: Full Sample		
Domestic Violence		
HAART available	-.365***	-.234***
	(.075)	(.085)
Treatment Group	-.065	-.027
	(.115)	(.116)
Treatment × HAART	-.199*	-.211*
	(.117)	(.121)
Obs.	6669	6669
Heroin Use		
HAART available	-.055	-.125
	(.069)	(.081)
Treatment Group	.133	.153
	(.144)	(.145)
Treatment × HAART	-.318**	-.306**
	(.127)	(.131)
Obs.	6669	6669
Rho	0.233***	0.262***
Panel A: Black Sample		
Domestic Violence		
HAART available	-.409***	-.319***
	(.092)	(.098)
Treatment Group	-.009	.033
	(.140)	(.141)
Treatment × HAART	-.272*	-.290*
	(.146)	(.148)
Obs.	4280	4280
Heroin Use		
HAART available	-.087	-.229***
	(.081)	(.081)
Treatment Group	.161	.170
	(.178)	(.177)
Treatment × HAART	-.298*	-.253
	(.155)	(.159)
Obs.	4280	4280
Rho	0.272***	0.289***
Basic Controls	N	Y

This table shows difference-in-differences estimates from a bivariate probit model where the outcome variables are domestic violence and heroin use and the control group consists of relatively healthy HIV+ women. Basic controls include age at visit, age squared, age cubed, race (Caucasian omitted), and site of visit (Chicago omitted). In all specifications, errors are clustered at the individual.

Table A10: Employment, Stimulant Use and Health

Current Stimulant Use		
Stimulants	-.104***	-.101***
CD4 count	.	.0002***
CD4 squared	.	-9.36e-08***
Age	-.002	.006
Age squared	.0003	.0001
Age cubed	-4.15e-06	-3.09e-06
Obs.	9952	9952
Stimulant Use Last Year		
Stimulant Use Last Year	-.076***	-.074***
CD4 count	.	.0002***
CD4 squared	.	-9.80e-08***
Age	-.0009	.007
Age squared	.0003	.0001
Age cubed	-4.07e-06	-3.01e-06
Obs.	9952	9952
Stimulant Use Two Years Ago		
Stimulant Use Two Years Ago	-.054***	-.055***
CD4 count	.	.0002***
CD4 squared	.	-9.98e-08***
Age	-.0007	.007
Age squared	.0003	.0001
Age cubed	-3.96e-06	-2.86e-06
Obs.	9952	9952

This table shows results from an OLS model with individual level fixed effects where the outcome is employment at the time of visit. Each panel shows results from including current health and stimulant use from a different point in time: current, use last year, and use two years ago.

Table A11: Employment, Heroin Use and Health

Current Heroin Use		
Used heroin SLV	-.092***	-.093***
CD4 count	.	.0002***
CD4 squared	.	-9.75e-08***
Age	-.00008	.007
Age squared	.0003	.0001
Age cubed	-3.96e-06	-2.89e-06
Obs.	9947	9947
Heroin Use Last Year		
Heroin Use Last Year	-.080***	-.083***
CD4 count	.	.0002***
CD4 squared	.	-9.84e-08***
Age	-.00007	.008
Age squared	.0003	.0001
Age cubed	-3.96e-06	-2.88e-06
Obs.	9947	9947
Heroin Use Two Years Ago		
Heroin Use Two Years Ago	-.081***	-.086***
CD4 count	.	.0002***
CD4 squared	.	-1.00e-07***
Age	.0007	.009
Age squared	.0002	.00008
Age cubed	-3.84e-06	-2.74e-06
Obs.	9947	9947

This table shows results from an OLS model with individual level fixed effects where the outcome is employment at the time of visit. Each panel shows results from including current health and heroin use from a different point in time: current, use last year, and use two years ago.