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Yuya KUDO*

March 2022

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JEL classification: H51, I12, I18

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Abstract

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1 Introduction

Several notable infectious diseases have been provided by histories, such as acquired immune deficiency syndrome (AIDS), cholera, ebola, leprosy, tuberculosis, and COVID-19. The significant economic consequences (e.g., Malani and Laxminarayan, 2011) have led policymakers and researchers to debate whether or not to criminalize the behavior that increases the spread of these infections (e.g., Burris, 2006; WHO, 2015). Unfortunately, such legislation not only increases the fear of legal punishment but also strengthens discrimination and prejudice toward those infected or perceived to be infected (e.g., Valdiserri, 2002). Therefore, it possibly discourages the disclosure of infection status, demand for care, and self-protection as well as increases incentives to pursue harmful behavior in secret, consequently increasing societal overall health burden (e.g., Chandrashekhar, 2020). However, there is limited rigorous empirical research to understand the impact of such legislation.

This study analyzes legislation introduced in Mali to lower the occurrence of new human immunodeficiency virus (HIV) infections and evaluates its impact on voluntary testing. More than half of the sub-Saharan African countries have recently adopted HIV-specific laws, a single legislative document covering several HIV-related issues, similar to that in Mali (Eba, 2015). These laws aim to protect alienated people, such as people living with HIV (PLH), men having sex with men (MSM), and sex workers by including various protective provisions. However, these laws include restrictive and punitive provisions, such as criminalizing HIV non-disclosure, exposure, and transmission. Therefore, public health experts and advocacy groups have argued that HIV-specific legislation reinforces discrimination against the already marginalized groups and, thus, discourages serostatus disclosure and the practice of appropriate HIV-protective behaviors (e.g., condom use, sexual abstinence, and voluntary testing) (e.g., Bernard and Cameron, 2016; Eba and Lim, 2017).

To identify the impact of the law, I analyzed data collected from female respondents to the 2006 Standard Demographic and Health Survey (DHS), where informed, anonymous, and voluntary testing was offered to them. On June 29, while this nationally representative household survey was being conducted (from April to December in 2006), Mali enacted its HIV-specific law. Respondents interviewed just before and after the legislation were presumed to share similar background characteristics. Additionally, the interview timing was exogenous to respondents. Therefore, this setting enabled me to use a regression discontinuity (RD) design with the interview day as a running variable.

Empirical analyses revealed that on the day of legislation, the rate of women's HIV test uptake declined by 2—4% from the day before the legislation. Offering the test also reduced the likelihood of the identification of HIV-positive females. These findings held true in parametric estimations varying in a range of bandwidths and orders of polynomials, nonparametric estimations, and statistical inference using a permutation test. Notably, it takes about three weeks for HIV antibodies to be reliably detected (i.e., a so-called window period) after new infections. Also, the deaths attributed to AIDS would exhibit no discontinuous change during the short time interval analyzed here. Therefore, neither the respondents' behavioral changes (that lead to an actual decrease in new infections) nor the selected survival of HIV-negative individuals would explain the instant decline in infection probability.

Additionally, these findings were prominent among women with a radio (or who frequently listened to it) and who lived in rural communities closer to the capital of Mali, Bamako, which suggests that respondents' access to legal information, strength of social ties, and reliable law enforcement may amplify the law's impact. Furthermore, respondents' self-reported discrimination against PLH increased on the legislation day since the law would have publicly increased the social acceptability of discriminatory attitudes toward PLH.

Summarily, the law would have increased HIV stigma and/or fear of legal punishment, thereby discouraging test uptake among HIV-positive females. I derive this interpretation by developing a theoretical model of HIV testing modifying that in Bursztyn et al. (2019), where taking an HIV test signals positive serostatus that is stigmatized by peers.

This study contributes to three strands of the extant literature. First, several quantitative and qualitative studies outside economics have explored the association of HIV-specific legislation with perceptions of and attitudes toward the law (particularly its punitive provisions), HIV disclosure and testing, safer sex practices, access to HIV treatment, and HIV stigma (see O'Byrne et al., 2013 and Harsono et al., 2017 for literature reviews). Closely related to this study, Lee (2015) showed that the self-reported HIV test experience of at-risk individuals in the United States (US) was negatively associated with media coverage of HIV criminalization but not with the existence of HIV-specific statutes in residential jurisdictions. To the best of my knowledge, the present study is the first to complement these commendable achievements by evaluating the causal impact of HIV-specific legislation. Additionally, it differed from most studies conducted in the US with the focus on PLH and/or at-risk individuals (e.g., drug users, heterosexuals, immigrants, MSM, and sex workers), as it investigates the representative populations focusing on Africa, where HIV is similarly (or arguably more) problematic. Moreover, analyzing the biomarker-based HIV serostatus in this context is unique, as it enhances the objectivity of the findings.

Second, this study relates to the literature on HIV prevention programs, in particular that which focuses on HIV stigma, aiming to encourage voluntary testing and/or decrease risky sexual behavior. As assessed in survey articles (e.g., Andersson et al., 2020; Sengupta et al., 2011), only a few empirical studies have rigorously explored the impacts of these programs on HIV stigma. An experimental study by Arimoto et al. (2016) found limited overall effects of multiple interventions in reducing employees' fear of discrimination after HIV testing on their test uptake in a large firm in South Africa. The impacts were less effective for those at higher risk, including Africans and Colored. In Thornton (2008)'s study in rural Malawi, random provision of monetary incentives to those who accepted an HIV test to learn serostatus increased the demand for learning serostatus since the incentives possibly provided an excuse for visiting a voluntary counseling and testing center. In the same study setting, Godlonton and Thornton (2012) highlighted the importance of peers in increasing the demand for learning serostatus. The present study indicates that HIV stigma is an obstacle to test uptake, similar to these previous studies.

According to the findings of this study, HIV-specific legislation may send at-risk populations into hiding, inadvertently causing a long-term increase in HIV prevalence, despite its a priori complex and unclear impacts.[1] Therefore, in a broader perspective, the findings may align with those of some prior non-financial (see McCoy et al., 2010 for the literature review) and financial interventions (e.g., Kohler and Thornton, 2011), which did not significantly reduce HIV incidence, although other financial interventions showed some promise (e.g., Baird et al., 2012; Björkman Nyqvist et al., 2018; de Walque et al., 2012). Additionally, HIV-specific legislation is similar to HIV/AIDS information campaigns (e.g., de Walque, 2007) since its effectiveness relies on public access to legal information. Also, the legislation aims to change the behavior on demand and supply of health services (e.g., de Walque et al., 2015).

Third, several implications of HIV-specific legislation are similar to those involved in regulating a sex market since sex workers are particularly vulnerable (e.g., Cameron et al., 2021; Cunningham and Shah, 2018). Gertler and Shah (2011)'s study of sex markets in Ecuador showed that enforcing licensing regulations in the brothel sector caused some of the sex workers to migrate to the riskier street sector, increasing the incidence of sexually transmitted infections (STIs) in both sectors. In contrast, according to the calibration study in Italy by Immordino and Russo (2015), regulation decreased the prevalence of HIV infection among all sex workers when HIV was seriously stigmatized.[2][3]

The remainder of this paper is organized as follows. Section 2 describes Mali's HIV-specific law. Section 3 presents the theoretical framework explaining the mode of influence of HIV-specific legislation on voluntary testing

[1]The coexistence of the protective and punitive provisions of the legislation may either encourage or discourage HIV testing. The test results, which affect risky sexual behavior either positively or negatively (e.g., Baird et al., 2014; Gong, 2015; Thornton, 2008, 2012), cause ambiguity of the resultant influence on HIV prevalence. Also, altered HIV prevalence may affect the ordinary citizens' health behaviors (e.g., partner change) and, once again, HIV prevalence (e.g., Kremer, 1996).

[2]Similarly, registered sex workers in Senegal had better health outcomes than unregistered ones (Ito et al., 2018).

[3]HIV-specific laws, in effect, may criminalize prostitution and/or increase HIV stigma. Therefore, the trafficking of sex workers may increase since the laws decrease voluntary prostitution and, thus, increase prostitution prices, benefiting the traffickers (Lee and Persson, forthcoming).

while discussing whether and how HIV testing protocols, as adopted by the DHS, help in addressing this study's question. Sections 4 and 5 explain the data overview and empirical strategy, respectively, with the empirical findings reported in Section 6. Section 7 concludes.

2 Institutional background

On June 29, 2006, Mali passed its HIV-specific law (Loi n° 06-028) based on the N'Djamena model law. This model law was developed and promoted by Action for West Africa Region-HIV/AIDS, a non-governmental organization funded by the United States Agency for International Development to standardize legal framework and best practices for addressing STIs and HIV/AIDS in western and central Africa. The model law is named after N'Djamena, Chad, where it was finalized in 2004 at a meeting of parliamentarians from the region's 18 countries. The N'Djamena model law was originally assumed to be tailored to each country's specific legal, social, political, and cultural contexts. However, it was solely and rapidly "sold" as a package to these countries shortly after the meeting (e.g., Grace, 2015). Therefore, most of the countries have adopted the laws closely derived from the N'Djamena model law to date (e.g., Kazatchkine and Kra, 2020).

The HIV-specific law in Mali is omnibus legislation similar to the model law. Its original intention was to protect marginalized people, such as PLH, MSM, and sex workers. Therefore, it includes protective provisions on HIV-related education, equal access to health care and support, informed consent as a condition of HIV testing, the confidentiality of test results, pre-and post-test counseling, and the prohibition of discrimination against PLH.

However, the law criminalizes HIV non-disclosure, exposure, and transmission. For example, PLH must disclose their serostatus to their spouse or sexual partners almost immediately or within six weeks after diagnosis (Article 27). The penal code sentences anyone who violates this article, including six months to two years imprisonment and/or a fine ranging from CFA francs 50,000 to 500,000 (Article 29).[4] The law also passes a sentence of 5—20 years on PLH who intentionally expose others to HIV infection (Article 37). This article was introduced to deter the intentional transmission of HIV. However, proving lack of intent to transmit HIV is generally difficult. It may also be used to define non-malicious HIV exposure and transmission, even a mother-to-child transmission, as a criminal offense since the law is excessively ambiguous (e.g., Csete et al., 2009). Additionally, in some contexts (Article 18 and Article 25), the law permits compulsory HIV testing (e.g., for sexual offenders and pregnant women, and for resolving marital conflict), involuntary notification of HIV status by health care professionals to the patients'

[4] This amount equals about 85 to 850 US dollars based on the exchange rate in March 2022. This fine is considerable in monetary terms since the gross national income per capita in Mali (by the World Bank Atlas method) was only 830 US dollars in 2020 (`https://data.worldbank.org/indicator/NY.GNP.PCAP.CD`).

sexual partners (either directly or indirectly by public health officers), or (as the law is incomplete) discrimination based on another person's HIV status (e.g., partners' and/or parents').

Supposedly these articles discourage the uptake of voluntary testing among (likely, HIV-positive) at-risk individuals, who believe they would not be criminally liable for non-disclosure and intentional transmission unless they are aware of their serostatus. Additionally, these restrictive and punitive provisions strengthen discrimination directed toward PLH, leading to their reluctance to disclose their serostatus, practice HIV-protective behaviors (e.g., condom use, sexual abstinence, and voluntary testing), and seek appropriate health services (e.g., Eba and Lim, 2017; Pearshouse, 2007).

3 Conceptual framework

3.1 A model of HIV testing with fear of legal punishment and stigma

HIV stigma, which means negative attitudes and beliefs about PLH, has been regarded as a barrier to HIV prevention and treatment (e.g., Valdiserri, 2002). Therefore, the following theoretical model modifying that in Bursztyn et al. (2019) explains the law's impact while being explicit about its influence.

Consider a community where PLH are discriminated against. The size of the population is normalized to one. In this community, two types of agents, who have different beliefs about their HIV serostatus based on their past sexual behavior, either positive (type H) or negative (type L), decide whether or not to take an HIV test. The (exogenous) share of the type-H agents is $q \in (0, 1)$. For simplicity, their beliefs are assumed to represent their actual serostatus, although they are unsure until they take the test. This assumption would not be unlikely; for example, in Arimoto et al. (2016)'s experimental study in a large firm in South Africa, factory workers' subjective probability about their serostatus predicted their actual HIV serostatus.[5]

Undergoing testing would benefit the agents since their recent serostatus information would help them update their sexual behavior (Boozer and Philipson, 2000). That is, they change their behavior toward protecting themselves and, if unselfish, their sexual partners. This (expected) benefit is assumed to be symmetrical in both types such that $b_H = b_L = b > 0$, as introducing asymmetry yields no new insight here.

Type-H agents are more likely to fear learning their serostatus than type-L ones. Additionally, the type-H agents may strongly fear taking the test if HIV non-disclosure, exposure, and transmission are criminalized. Furthermore, compared to the type L ones, type-H agents may strongly suffer after being tested because it might

[5] In Anglewicz and Kohler (2009), rural Malawians overestimated their likelihood of HIV infection. However, this finding also implies that HIV positives were unlikely to misperceive their actual serostatus.

undermine their self-image (this psychological burden is seen as one cost of HIV stigma). Consequently, the total psychic cost of HIV testing is assumed to be larger for the type-H agents than for the type-L ones, namely, $c_H > c_L \geq 0$, even with the constant opportunity cost of time traveling and waiting to be tested at a clinic across both types.

Those tested are assumed as (for simplicity) surely and publicly known. Therefore, an agent i (either H or L) who takes an HIV test incurs another cost of HIV stigma if he/she is perceived as HIV positive by peers (e.g., boy- or girl friends, colleagues, neighbors, and spouses).[6] I denote the incremental cost of being seen as PLH and an indicator for agents who take an HIV test as $\rho > 0$ and a_i, respectively. Then, this cost of HIV stigma is written as $\rho\ \text{Prob}(c_i = c_H \mid a_i = 1)$, where $\text{Prob}(c_i = c_H \mid a_i = 1)$ is the peers' (endogenously determined) belief that an agent i tested is HIV positive. Consequently, the agents decide to test if they receive more benefits, namely,

$$\theta_i > \rho\ \text{Prob}(c_i = c_H \mid a_i = 1), \tag{1}$$

where $\theta_i \equiv b - c_i$ (and, thus, $\theta_L > \theta_H$).

To illustrate a case of interest which explains the subsequent empirical findings, I assume $\theta_i > 0$ and $\theta_L > \rho > \theta_H > 0$; namely, HIV testing benefits agents if discrimination by peers is absent, and the discrimination is primarily a significant concern only for type-H agents. With these assumptions, all the type-L agents would take the test, resulting in $\text{Prob}(c_i = c_H \mid a_i = 1) = \frac{q\text{Prob}(a_i=1 \mid c_i=c_H)}{q\text{Prob}(a_i=1 \mid c_i=c_H)+1-q}$ by Bayes' rule. Therefore, if an interior equilibrium where only a fraction of type-H agents take the test is assumed, they must be indifferent about testing and not testing, namely, $\theta_H = \frac{\rho q\text{Prob}(a_i=1 \mid c_i=c_H)}{q\text{Prob}(a_i=1 \mid c_i=c_H)+1-q}$. Solving this yields

$$\text{Prob}(a_i = 1 \mid c_i = c_H) = \frac{1-q}{\frac{q\rho}{\theta_H} - q} > 0, \tag{2}$$

$$\text{Prob}(a_i = 1) = \frac{1-q}{1 - \frac{\theta_H}{\rho}} > 0, \tag{3}$$

where $\text{Prob}(a_i = 1) \in (1-q, 1)$, namely, $q\rho > \theta_H$. Thus, if HIV-specific legislation, particularly coercive provisions, increases the incremental cost of being seen as PLH (i.e., ρ), it will reduce the test uptake overall and among HIV-positive individuals. Similar consequences will also arise when fear of legal punishment and test-induced damage to self-esteem, both of which constitute c_i, increase due to the legislation such that $\theta_L > \rho > 0 > \theta_H$.

[6] Therefore, with the abovementioned damage of self-image, I consider "perceived" (or internalized) stigma within the field of psychiatry, in contrast to "enacted" stigma that refers to the actual experience of unfair treatment by others (e.g., Gray, 2002).

3.2 HIV testing protocol in the DHS

The DHS program conducted HIV testing as follows. First, following the testing protocol approved by Mali's National Ethics Committee, field workers, specially trained to conduct HIV testing, obtained a signed consent form from the respondents, stating their understanding of the testing procedures and willingness to be tested. They then collected dried blood spot samples on a special filter paper from a finger prick adhering to adequate hygiene and safety precautions. Next, the collected samples were transported to a laboratory for examining serostatus using enzyme-linked immunosorbent assay (ELISA).

HIV testing and the DHS interview were individually conducted without a third person at the respondents' home and, thus, it is in principle impossible for others (even family members) to know their agreement (or disagreement) to the blood collection. Additionally, even respondents were "not" allowed to view test results. Instead, regardless of whether they consented to testing or not, all respondents were given a list of nearby health facilities, which provided voluntary counseling and testing, with a free voucher for use at those facilities. Therefore, individuals who wished to know their serostatus could go for HIV testing later (for details of the testing protocol, see Macro International, 2007).

In principle, the confidentiality of the test results even to the test takers yields no benefit to those who agreed to be tested or eliminates the cost of test uptake. Therefore, this confidentiality makes this setting inadequate in addressing this study's question at first sight; however, this would be a false conjecture. First, high rates of HIV test uptake in the DHS indicate that the respondents enjoyed positive (e.g., a warm-glow) utility by being tested, for example, helping field workers and/or improving public health. This utility plays a role similar to $b > 0$, as considered in the model above. Second, refusal bias is of substantial interest in prior studies that attempted to estimate the national HIV prevalence in Africa using DHS data (e.g., Adegboye et al., 2020; García-Calleja et al., 2006). According to these studies, people who refused testing were more likely to be HIV positive than those tested (e.g., Marston et al., 2008; Mishra et al., 2008; Reniers and Eaton, 2009). In Mishra et al. (2008), for example, HIV prevalence among females who refused HIV testing was estimated to be 3.71% compared to the observed prevalence of 1.83% among those tested using the 2003 DHS data in Burkina Faso, a neighboring country of Mali (there is no similar research using Mali's DHS data according to my literature review). Notably, whether or not they were infected and/or knew their serostatus, the respondents equally obtained no additional information from the DHS-offered HIV testing. Nevertheless, this refusal bias was present. Therefore, the psychic cost of HIV testing would vary by serostatus (i.e., $c_H > c_L \geq 0$) even if the results are strictly anonymous and confidential. Alternatively, according to (particularly, HIV-positive) respondents' expectations, the confidentiality of HIV testing would not be complete in a household, an extended family, and/or a community, where people have tight-knit connection to each other; therefore, the testing signals serostatus that is stigmatized (i.e., ρ Prob$(c_i = c_H \mid a_i = 1) > 0$). Summarily, the HIV test setting in the DHS still helps to address this study's question.

4 Data

I used cross-sectional data drawn from the 2006 Standard DHS in Mali, which provides nationally representative information by design in population, health, and nutrition. This survey used a two-stage sampling protocol, characterized by the first-stage selection of communities (clusters) from the 1998 General Census of Population, followed by a second-stage selection of households from the respective communities. All women aged between 15 and 49 years in each selected household and men aged between 15 and 59 years in one-third of the selected households were interviewed for this survey. Men and women in one-third of the selected households were also eligible for anonymous and voluntary HIV testing, as described in subsection 3.2 (hereafter, I refer to these respondents as the HIV test sample).

Importantly for the RD design, the DHS was conducted from April to December 2006, exactly when Mali enacted its HIV-specific law. Therefore, I analyzed the HIV test sample who completed their interviews within a 40-day symmetric time window around the legislation day.[7] This restriction would avoid potential bias arising from the extrapolation of data in a quadratic polynomial regression, as used in this study.

Additionally, the female respondents were the main study sample since there are concerned about the validity of an RD design for analyzing male respondents, as detailed with the briefly reported findings in Section S.2 in the supplementary material. The main study sample were the 1255 women residing in 113 communities constituting approximately 25% of the total (female) HIV test sample.

It is noteworthy that the coercive provisions of HIV-specific legislation presumably discriminate against women more compared to men since women are usually the first to know their positive serostatus using antenatal and other health services, which facilitates their family members to charge them with exposing partners to HIV (e.g., WHO, 2015, p. 22). Therefore, the possibility that women hesitate to seek adequate health services (for example, during pregnancy to prevent mother-to-child transmission) to avoid the prosecution and suffering physical and emotional abuse is mentioned (e.g., Ahmed, 2011; Weait, 2011). Responding to this concern, Kenya's High Court

[7] Females interviewed before this time window comprised only 13.7% of the total (female) HIV test sample, and their day of the interview is not continuously distributed.

declared HIV criminalization (Section 24 of the HIV-specific law, as introduced in 2006) unconstitutional in 2015 (e.g., Migiro, 2015).

Two major outcomes exist in this study: an indicator equal to one for respondents who tested for HIV (precisely, they agreed to blood collection for HIV testing) and an indicator for respondents who tested seropositive (this indicator equals zero for those who tested seronegative and those who were not tested). Using these indicators, 98.17% of women belonging to the main study sample accepted the HIV test, with 2.36% of them testing seropositive. For these outcomes, Figure 1 plots the values averaged within three-day bins (scatter plots) with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on quadratic polynomial regression and standard errors clustered at the community and day-of-interview levels. On the legislation day, negative jumps of about 2—4 percentage points for women's HIV test uptake and positive serostatus were observed, which is prima facie evidence that HIV-specific legislation discouraged test uptake among HIV-positive females.

[Here, Figure 1]

5 Empirical strategy

To identify the impact of the HIV-specific legislation, I considered the following specification for respondent i:

$$\begin{aligned} y_i &= \alpha_1 + \alpha_2 S_i + \alpha_3 f(d_i - z) + \epsilon_i, \\ \forall \quad & d_i \in [z-h, z+h], \end{aligned} \tag{4}$$

where y_i is an outcome of interest; d_i is the day of the respondent's DHS interview; z is the cutoff day, namely, June 29, 2006; h refers to the bandwidth (as measured by day) before and after the legislation, respectively; $S_i = \mathbb{1}(d_i \geq z)$ is a treatment indicator; and ϵ_i represents the stochastic error. The function $f(d_i - z)$ represents an RD polynomial of the time distance, $d_i - z$, and coefficients of the polynomial terms are allowed to be different for each side of the cutoff. I primarily estimated equation (4) with the second-order polynomial with $h = 20$ (hereafter, I refer to this specification as the main empirical model) by ordinary least squares (OLS) while occasionally reporting the estimation results of the first-order polynomial model. Referring to Gelman and Imbens (2019), I did not control for the third or higher-degree polynomials.

The influence of HIV stigma on test uptake would vary across communities. Additionally, the model error would be correlated within the interview day due to an inevitable specification error in the RD model with the discrete running variable (Lee and Card, 2008). Therefore, I used standard errors clustered at the community and day-of-interview levels (113 and 41 groups in the main empirical model, respectively).

There are two particular concerns about the validity of the RD design. First, the DHS data excluded those who actively or passively (i.e., absence) refused to be interviewed.[8] Since these people are likely to refuse HIV testing, the relevant estimates would be biased if HIV-specific legislation prompted their nonrandom selection into the interview and HIV testing. Related to this concern, the possibility that survey respondents left their homes to avoid being approached for an HIV test, which was termed a refusal of HIV testing, was mentioned in a study by Angotti et al. (2009) in Malawi. Second, the survey enumerator may have rescheduled the interviews of respondents, who had certain (health) characteristics, for some reason unidentified by researchers (e.g., anticipating the influence of the bill of HIV-specific legislation under consideration on the respondents' agreement to blood collection) or by chance (e.g., failing to contact those who were away from home at the first visit and, therefore, making callback visits).[9] For example, the interview date for the HIV negatives to be interviewed just before the legislation may have been postponed to a date just after the legislation and instead, the date of interview for the HIV positives to be interviewed just after the legislation may have been moved to the date just before the legislation.

To mitigate these concerns, I conducted two exercises. First, and particularly for the first concern, I checked selective sorting across the treatment threshold. In the left panel of Figure 2, I presented a histogram showing the number of female respondents interviewed each day. Additionally, the right panel showed a similar figure to Figure 1 for that number, with both panels revealing no evident sorting across the cutoff. Formally, I performed a density test proposed by Cattaneo et al. (2020), a less restrictive test than McCrary (2008)'s seminal test. Admittedly, such a nonparametric density test is theoretically less plausible due to the discreteness of the running variable (Lee and Card, 2008). However, it is practically useful provided the distribution of the discrete variable is not too coarse (Lee and Lemieux, 2010, p. 336). The null hypothesis of no discontinuity was not rejected, as seen from the corresponding p-value reported at the bottom of the left panel.

Second, Figure 3 demonstrates similar figures to Figure 1 for the 25 (likely) pre-determined variables not affected by the law (see also Table 1 for summary statistics). Among these, individual-level covariates are women's age; religion (Muslim); ethnicity (Bambara, the largest ethnic group in Mali with its corresponding language spoken as this country's lingua franca); highest levels of completed education; height; weight; HIV-related knowledge score (0—10), as detailed in Section S.1 in the supplementary material; marital status; and indicators of whether they

[8]According to the survey manuals, the replacement of respondents is not allowed in the DHS.

[9]During the DHS, field workers usually make three callback visits either at different times of the initial interview day or on different days; see the survey manuals.

had heard of circumcision, whether they were circumcised (this indicator equaled zero for uncut women and those who had never heard of circumcision), and whether they had contracted sexually transmitted diseases (STDs) in the last 12 months. At the household level, the covariates are household size and whether they had a radio, a TV, a bicycle, or electricity. The community-level covariates are an urban indicator, population density, precipitation, temperature, altitude, slope, and distances from Bamako and to the nearest national border (see Section S.1 for the details).[10] For each of them, I estimated the main empirical model and reported the p-value of the estimated α_2 at the bottom of each panel. The examined variables varied smoothly at the cutoff, except for communities' temperatures. Admittedly, there is a concern about the accuracy of self-reported sexual behavior (e.g., Corno and Áureo de Paula, 2019). Nevertheless, an indicator of STDs, a significant risk factor for HIV infection, and HIV-related knowledge scores exhibited no discontinuous changes. The subsequent empirical findings are also robust in controlling for these covariates.

These findings are contextually plausible because (largely, Muslim) women usually have limited freedom of movement for various reasons (e.g., security, division of labor, and religious and gender norms), thereby making it difficult for them to leave their homes to avoid being interviewed (e.g., van den Engle and Hesseling, 2001). Consistent with this view, the survey response rate was 97% for women according to the summary report of the 2006 DHS (Samaké et al., 2007, pp. 9—10).[11] Also, approximately 78% of all female respondents answered that they needed permission from their husband, partner, or someone else before visiting their relatives. Limited mobility would also reduce the possibility of rescheduling the interview. This situation would be different from that of the men who were absent at the time of the interview for many reasons, such as attendance at village meetings, communal tasks (e.g., road maintenance), jobs (including labor migration), and trips and/or avoided the interview (and, thus, HIV testing) easily for these (and other) reasons.[12]

[Here, Figure 2, Figure 3, and Table 1]

[10]The distance to the national border is relevant because labor migration to the neighboring counties (particularly, Côte d'Ivoire) is common, and migrant workers in Africa often engage in risky sexual intercourse (Oster, 2012).

[11]I cannot verify this response rate in the public data, which excluded the respondents who refused to be interviewed.

[12]The rate of survey response for men was 91% in the 2006 DHS (Samaké et al., 2007, pp. 9—10). A lower response rate from men than women was also typical in the DHS performed elsewhere (e.g., Mishra et al., 2008).

6 Empirical findings

6.1 Main results

For an indicator for the respondents who tested for HIV, I estimated the main empirical model in column (a) of Table 2 using the main study sample; in column (b), all the abovementioned covariates and region-fixed effects (seven groups) were included in regressors; and in columns (c) and (d), I also estimated a linear polynomial model of equation (4) with $h = 10$ without and with these controls, respectively (the number of communities in this subsample declined to 68). Additionally, although less desirable in the RD model with the discrete running variable (Lee and Card, 2008), I also estimated equation (4) nonparametrically, selecting the optimal bandwidth based on the methods developed by either Calonico et al. (2014) [CCT, column (e)] or Imbens and Kalyanaraman (2012) [IK, column (f)]. According to the results, the HIV-specific law decreased HIV test uptake by about 2 to 4 percentage points, namely, a 2—4% decline because the rate of test uptake a day before the legislation was 100%.

While the settings are indirectly comparable, the effect size of the immediate 2—4% decline may be assessed using the estimates reported in previous studies. In Rwanda, de Walque et al. (2015) found that providing health facilities with performance-based monetary incentives of 0.92 US dollars per individual tested for HIV and 4.59 US dollars per couple/partner jointly tested increased the self-reported test uptake of married individuals by about 14.5% over 18 months from the baseline uptake rate of 70%. In Thornton (2008)'s experimental study in Malawi (after controlling for all given incentives), providing one US dollar (approximately the local daily wage) for those who accepted an HIV test increased their likelihood of learning their serostatus by about 26% from the control likelihood mean of about 34%.

In panel (B) of Table 2, I conducted similar exercises for an indicator for the respondents who were tested and HIV positive, namely, $\text{Prob}(a_i = 1 \cap c_i = c_H)$ using the notation of the theoretical model in subsection 3.1. This indicator itself is of interest, as it captures the degree to which offering HIV testing in residential homes resulted in identifying PLH. However, these estimation results are also useful for understanding the influence of the law on the uptake of HIV-positive individuals, namely, $\text{Prob}(a_i = 1 \mid c_i = c_H)$ since (1) $\text{Prob}(c_i = c_H)$ remains constant around the cutoff (namely, individual serosatus would not change immediately after the legislation) and (2) given that,

$$\Delta\text{Prob}(a_i = 1 \mid c_i = c_H) = \frac{\Delta\text{Prob}(a_i = 1 \cap c_i = c_H)}{\text{Prob}(c_i = c_H)}, \tag{5}$$

where Δ refers to the law-induced changes in the corresponding probabilities. Indeed, this impact may attract significant interest from policymakers. According to the results of the parametric (or nonparametric) regressions, the probability of finding HIV-positive females by offering the test declined by about four (or eight) percentage points. In the data, all respondents were tested one day before the legislation, with 7.41% of them being seropositive. Thus, with the assumption of Prob$(c_i = c_H) = 0.07$, the law would have discouraged HIV-positive females from taking the test by at least about 55% ($= \frac{0.04}{0.07}$) provided all of those infected had been tested before the legislation.

[Here, Table 2]

6.2 Why did the uptake decline?

The decline in test uptake among HIV-positive females may have resulted from the fear of legal sanction, the increased cost of HIV stigma, or both, which I will examine in this subsection. The actual and immediate legal sanction is unlikely because the law does not criminalize pre-legislation behavior. However, the fear of legal punishment may have discouraged test uptake on the legislation day if people incorrectly understood the details of the law and its enforcement modality. Additionally, according to Castle (2003, 2004)'s research conducted just before the law was introduced, Malians were afraid that PLH would be highly stigmatized in the community, thereby discouraging their use of voluntary testing and counseling services.

6.2.1 Fear of legal sanction

The expected enforcement of the law may decay with distance from the capital (e.g., Michalopoulos and Papaioannou, 2014). Therefore, if HIV-positive women's willingness to avoid legal consequences explains the decline of test uptake, the law's impact may be stronger in communities closer to Bamako. To explore this impact heterogeneity, I separated the sample by the mean distance (350 km) from the DHS community to Bamako and (to keep the sample size tenable for this analysis) estimated the quadratic polynomial model in the first four columns of Table 3. Overall, the results support the above conjecture. Notably, HIV-positive women may also have disliked the law because the legal consequences, if (likely) known by peers, may have resulted in them and their family members suffering from discrimination in their community due to criminalization and/or positive serostatus. Therefore, concerns about HIV stigma, as examined below, may also explain the law's stronger impacts on residents of communities near the capital. However, controlling for covariates reduced the magnitude and/or statistical significance of the estimates in columns (d) and (l). The empirical model that fits too many covariates (namely, 25 covariates and region fixed effects), along with the decreased sample size, might solely have made the estimates rather imprecise because of the reduced degrees of freedom, possible multicollinearity, and overfitting. Nevertheless, the findings should therefore be interpreted cautiously. Finally, splitting the sample by the median distance to Bamako also yielded a similar implication with a somewhat declined statistical significance, as seen in Table S.1 in the supplementary material.

[Here, Table 3]

6.2.2 HIV stigma

I performed two exercises to examine the influence of HIV stigma. First, the law-induced cost of HIV-related discrimination would be higher in a rural community, as rural people often develop tightly knit connections with each other and provide mutual livelihood support (e.g., informal insurance). Closer social relationships may also increase the probability that the test uptake is known by peers, thereby increasing the expected cost of being seen as PLH. Consistently, the law's impact was stronger in rural communities, as confirmed in the last four columns of Table 3 reporting the impact heterogeneity.

Second, I explored the law's impact on the cost of women's perceived stigma (i.e., c_i and ρ), precisely an indicator for the respondents who agreed to both the following views: "PLH should be ashamed of themselves" and "PLH should be blamed for bringing the disease to the community." The results of both parametric and nonparametric estimations similar to those performed in Table 2 are reported in Table 4.

While measuring the stigma is complex and challenging, this indicator still aligns with a negative self-image and concern with public attitudes toward PLH, as conceptualized in Berger et al. (2001). In the DHS, however, only those who (reported that they) had heard of AIDS were eligible to provide these pieces of information, about 8% of whom responded "don't know or no opinion" to the survey. Therefore, for those who responded "don't know or no opinion," this indicator had a value of zero in panel (A) but was valued as one in panel (B); therefore, these measures may reflect the lower and upper bounds of perceived discrimination, respectively.

In the nonparametric estimations and the odd columns of parametric estimations, I estimated the empirical model using only data from those who had heard of AIDS. In the remaining columns, I included those who had never heard of AIDS in the estimated sample after setting the outcome indicators to a value of zero for these respondents (i.e., assuming that they did not discriminate against PLH as they had never heard of AIDS) and controlling for an indicator that was set to zero for these respondents, all abovementioned covariates, and region fixed effects.[13]

According to the results, the law increased self-reported HIV-related discrimination; the lower- and upper-bound

[13] Therefore, coefficients on the indicator for respondents who had heard of AIDS were significantly positive by construction.

stigma indicators increased by about 58% [column (a)] and 74% [column (g)], respectively, as one day before the legislation, about 20% (lower bound) and 25% (upper bound) of the HIV test sample (excluding those who had never heard of AIDS) agreed to the abovementioned two views of HIV-related discrimination.

The effect size is noteworthy, as most prior experimental interventions aimed at reducing HIV stigma were ineffective, as summarized in Andersson et al. (2020) and Sengupta et al. (2011). For example, among few effective ones, Klepp et al. (1997) offered a local HIV/AIDS education program to 1063 sixth-grade students (at baseline) in public primary schools in northeast Tanzania and found that positive attitudes toward PLH, as measured by a summary score (1—20), increased by about 40% from the baseline control mean score of 6.9 after 12 months. Likewise, in Nyamathi et al. (2013)'s study of 68 rural Indian females living with HIV, compared to those who received a usual care program, those who received a special care program delivered by HIV-trained village women revealed a decrease in the internalized stigma score (1—4) by 56% from the baseline control mean score of 3.55 after six months.

Two interpretations of these findings are possible, which indicate that HIV stigma perceived by HIV positives increased due to legislation. First, the law strengthened discrimination against PLH. However, personal attitudes toward PLH may not have changed immediately in a discontinuous manner. Second, and more likely, the law might have made it more comfortable for those who had already discriminated against PLH to express those views publicly, as it sends a message that the state supports such views.

Admittedly, these findings should be treated with caution. First, as the sample size is already modest around the cutoff, splitting the sample raises a concern about extrapolation bias (Table 3). Second, respondents may not report their attitudes toward PLH truthfully due to social desirability bias (Tables 4); notably, given the RD design, the resulting measurement error in the dependent variable would be classical and, thus, maintain the consistency of the estimates. However, it might still have made the visual jumps on the stigma indicators somewhat noisier, as seen in Figure S.1 in the supplementary material, where I performed similar graphical analyses for these indicators to those in Figure 1 using only data from those who had heard of AIDS. Nevertheless, all of the above findings are still persuasive.

[Here, Table 4]

6.2.3 Alternative interpretations

Two alternative interpretations may explain the decline of the infection probability on the legislation day: the selected survival of HIV-negative women and the actual decrease in new infections (that results from changes in sexual behavior following the legislation). However, I reject the former because AIDS-related deaths would have remained constant around the day of legislation. The latter is also unlikely as the window period between the occurrence of new infections and when the ELISA can reliably detect antibodies against the virus is about three weeks (e.g., Kucirka et al., 2011); therefore, even if sexual behavior changes on the day of legislation, identifying the resulting immediate decline of the infection probability would be difficult.[14]

To further support the interpretation of this study (i.e., PLH's avoidance of HIV testing), in Table S.2 in the supplementary material, I estimated two outcomes, precisely one indicator for the respondents who agreed to blood collection for anemia testing [panel (A)] and another indicator for the respondents who were anemic [panel (B)], defined as those having hemoglobin levels $\leq$ 12 g/dl, which is the threshold set by the World Health Organization for females (the latter indicator is set to zero those having hemoglobin levels $>$ 12 g/dl and those untested).

In the DHS, anemia testing was offered to the respondents separately from HIV testing, enabling them to take either test. However, about 99% of the respondents either took or rejected both tests. Notably, HIV disease progression is presumed to cause blood disorders, such as anemia (e.g., Harding et al., 2020). Therefore, the probability of identifying females with anemia would have also declined after the legislation if the law discouraged PLH from testing for HIV (and anemia). This conjecture is indeed confirmed, although the statistical significance is somewhat weak (probably, because not all the PLH were anemic and even HIV positives in the study sample were relatively few); see also Figure S.2 in the supplementary material for the graphical analysis.

6.3 Were Malians aware of the law?

The interpretation of this study requires one crucial assumption, which I will discuss in this subsection: Malians were aware of the law when it was introduced.

HIV criminalization has long been a concern among researchers, community leaders, activists, and lawyers in western and central Africa (Claivaz-Loranger and Kazatchkine, 2017). Among the francophone counties that introduced the HIV-specific laws derived from the N'Djamena model law, for example, the introduction of the 2008 HIV-specific legislation in Burkina Faso had already been open to the public through local media by 2006 (Sannon et al., 2009). Although PLH generally do not want to make their serostatus public, two prosecutions for intentional HIV transmission had also been brought under this law by 2009 (Sannon et al., 2009). In Togo, with the passage of the first HIV-specific law in 2005 and the criminalization of willful transmission in 2010, four

[14] One may argue that people anticipated the legislation and changed relevant behaviors in advance, resulting in an actual decline of new infections. However, such behavioral changes would not yield a negative jump in infection probability at the cutoff. Additionally, gradual behavioral changes would yield the "decreasing" trend of the infection probability toward the cutoff. However, this decreasing tread was not observed in Figure 1.

criminal prosecutions and two convictions were indicated in Kazatchkine (2010).[15] Additionally, most subjects disagreed with the criminalization of HIV according to Kpanake et al. (2013) who surveyed 199 individuals in a small town of northern Togo. In Mauritania, the adoption of the 2007 HIV-specific legislation was also released to the public through the government's official newspaper in that year (Journal Officiel de la Republique Islamique de Mauritanie, 2007).

In contrast, the availability of literature on such awareness (and even the literature on factors associated with HIV-positive status disclosure) in Mali is limited (e.g., Ballayira et al., 2021). To the best of my knowledge, Cissé et al. (2015), who performed a community-based cross-sectional survey on 300 adult PLH in Bamako, Kati, and Koulikoro, is the only study that suggested the influence of the 2006 HIV-specific legislation. According to them, HIV stigma discouraged PLH from disclosing their serostatus, and the law is regarded as "adding even more complexity to the issue of disclosure."

However, in Mali, the control of HIV/AIDS was formally integrated into both the national and local development policies in 2004. The period from 2004 to 2011, which recorded a significant increase in available international funds, was the heyday in the fight against HIV/AIDS (Castro, 2013).[16] As described in the national strategic framework at that time (Haut Conseil National de Lutte Contre le VIH/SIDA, 2006), over 60% of Malian women listened to the radio at least once a week, and about 31% of the active female population had access to TV news, thereby encouraging the government to actively work with media to combat HIV/AIDS during 2006—2010. In the 2006 DHS, about 63.7% and 34.8% of the main study sample also listened to the radio and watched TV at least once a week, respectively.

Indeed, the government seems to have implemented such a policy. As suggested from Cissoko et al. (2014), for example, the most significant source of information about HIV was the radio and TV for high school students in northern Mali in 2008. Additionally, using data drawn from the DHS in 27 countries in sub-Saharan Africa (including the 2006 DHS in Mali), Westoff et al. (2011) also showed that radio and TV exposure positively correlated with HIV/AIDS knowledge and preventive behavior.

Therefore, people with electronic equipment, such as a radio and a TV, would have been more familiar with HIV-specific legislation. Then, Mali's law would have more strongly discouraged the test uptake among those who possessed such property (mainly HIV positives). Similarly, those people would have also reported stronger

[15]Similar prosecutions and several cases of the threat have also been reported elsewhere in western and central Africa (Claivaz-Loranger and Kazatchkine, 2017).

[16]In 2004, the government created a comprehensive governance system for the control of HIV/AIDS by adopting a common national framework, establishing a national coordination body, and developing a monitoring and evaluation system (Castro, 2013). In this year, Mali also declared free access to antiretroviral therapy (e.g., Tamí-Maury et al., 2012; Teisseire et al., 2008).

discrimination against PLH after the legislation. In this study, I particularly focused on the role of radio as its broadcasting network would have been most widespread (particularly in rural areas). The estimation results in the first four columns of Table 5, where I examined this impact heterogeneity, support these conjectures, although the statistical significance in column (2a) is only 10.9%. This finding is consistent with Lee (2015) who found a negative association between the self-reported HIV test experience of at-risk individuals and media coverage of HIV criminalization in the US. In the last four columns, I separated the sample into those who listened to the radio at least once a week and the remaining and conducted the same exercises, which yielded a similar implication.

To gain further insight into people's familiarity with the law, I also used Wayback Machine (`https://archive.org`), a famous digital library of Websites. Precisely, I accessed past news that was reported in MaliWeb.net (`https://www.maliweb.net`), an online news distributor in French, using it. Since the MaliWeb.net selectively distributed part of news sourced from Mali's multiple newspapers, such as L'Essor (Mali's state-owned national daily newspaper), there must have been other news not on this Website (and/or not archived in the Wayback Machine). Nevertheless, I examined all of the news headlines and contents reported in the MaliWeb.net during 2005—2007, which amounted to 112,803 words in total.[17] In this web scraping exercise, I found no news concerning Mali's HIV-specific law. Therefore, by year and month, I reported the frequency of the occurrence of the words "VIH" (HIV in English) or "SIDA" (AIDS in English) on the then news in the MaliWeb.net, in the top-left panel of Figure S.3 in the supplementary material. For comparison, I also reported similar numbers of appearance for other words, in the remaining panels, namely, "éducation" (education) or "école" (school), "football," and "gouvernement" (government).

The words "VIH" or "SIDA" were not frequently used in local newspapers similar to the remaining words. At first sight, this finding undermines the assumption that people were informed of the HIV-specific law. However, it may solely confirm that local newspapers were not the main source of information about HIV/AIDS, as indicated in the abovementioned studies of Cissoko et al. (2014) and Westoff et al. (2011). This interpretation is plausible because, in the 2006 DHS, about 86.6% of the main study sample were unable to read a simple sentence at all during the survey, and about 93.2% read no newspaper and magazine in a week.[18]

Summarily, there is difficulty proving or disproving the assumption's plausibility due to a lack of direct information about people's awareness of the law. However, this study jointly tests this assumption with PLH's avoidance of HIV testing, with all yielded findings collectively supporting its validity; otherwise, I could not have shown any

[17] I avoided extracting news (through the Wayback Machine) directly from the Websites of Mali's newspapers because the Wayback Machine stored much less news from those sites for 2005—2007 than news from the MaliWeb.net.

[18] The corresponding figures among the male sample are 65.7% (along with an additional 12.5% that could read only parts of the sentence) and 85.3%, respectively.

meaningful results so far. Therefore, these results should be considered in the first order of importance, although not entirely free from this concern.[19]

[Here, Table 5]

6.4 Robustness checks

I conducted five robustness checks for the main estimation results that do not exploit any controls. First, the above findings were robust to other choices of bandwidth and the orders of the polynomial, as examined in Figure 4 that reported the estimated α_2 with 95% confidence intervals for the test uptake, serostatus, and (both lower- and upper-bound) stigma indicators. Second, I confirmed no discontinuities at other dates of the interview in Figure S.4 in the supplementary material that reported the estimated α_2 with 95% confidence intervals, where the estimates corresponding to the value M (that varies from -5 to 5) on the horizontal axis stem from the regression of equation (4) using "June 29, 2006, + M days" as the cutoff day. For comparison, the original estimates, as reported in Tables 2 and 4, are shown at $M = 0$. Third, I checked the sensitivity of the findings to the response of units near the cutoff in Figure S.5 in the supplementary material. In this figure, for each value of W (that varies from 0 to 5) on the horizontal axis, I estimated equation (4) using data excluding the observations with $|d_i - z| < W$ and reported the estimated α_2 with 95% confidence intervals, which is a standard donut-hole exercise; the original estimates are reported at $W = 0$. Since the original sample size is modest, excluding observations makes the estimates imprecise, as seen in the confidence intervals that increase as the value of W increases. However, for all examined outcomes, the estimates are robust to excluding the interviewed respondents on the legislation day ($W = 1$). Additionally, the sign of the estimates remains unchanged, except for the estimations of the serostatus using the quadratic polynomial model with the larger value of W. These findings further support the absence of selective sorting at the treatment threshold.

Fourth, I performed a variant of a permutation test in Figure 5 that reports the distribution of 1000 placebo estimates. Specifically, I obtained each placebo estimate by permuting the date of the interview randomly across individuals and estimating equation (4) using a "fake" treatment indicator based on the "fake" running variable. In this figure, the dashed lines represent the values of the 5th and 95th percentiles in the respective distribution of the placebo estimates, and the solid lines report the original estimates, as reported in Tables 2 and 4. Fifth, for the test uptake, serostatus, and (to be more conservative) lower-bound stigma indicator, in Table S.3 in the supplementary

[19] In general, African people are also cautious of laws regulating socially sensitive issues. For example, due to the legal prohibition of female genital cutting, parents in rural Burkina Faso have taken their daughters to its neighboring countries for this practice, where no similar laws exist or its enforcement is not so strict (Sayagues, 2009).

material, I reported p-values that adjusted the family-wise error rate and sharpened q-values that controlled the false discovery rate. The overall implications of this study remain unchanged after using these alternative methods of statistical inference.

[Here, Figure 4 and Figure 5]

7 Conclusion

To better understand the public health impact of criminalizing harmful behavior that increases the spread of infectious diseases, I examined the impact of Mali's HIV-specific law on women's voluntary testing. Toward this end, an RD design was applied to a unique setting where Mali enacted an HIV-specific law while performing a nationally representative household survey. Therefore, I exploited the most promising setting including the (e.g., biomarker-based) data available to date for the causal identification of this interest.

According to empirical analyses, the law decreased women's HIV test uptake by approximately 2—4% compared to a day before the legislation. The probability of identifying HIV-positive females by offering the test also declined on the legislation day. The impact was arguably stronger when respondents had access to media (i.e., a radio) and resided in rural communities near the capital. Therefore, the law's impact appeared to be more significant when people were aware of the law, had close social relationships with others, and believed that the law would be enforced. Additionally, female respondents reported stronger discrimination against PLH after the legislation, since the law may have increased their willingness to express discriminatory attitudes toward PLH by signaling the state's support for such attitudes. Altogether, with the implication of the developed model of HIV testing, Mali's HIV-specific legislation reinforced HIV stigma and/or fear of being prosecuted, thereby reducing test uptake among HIV-positive females.

Although obtained from only one country, these findings could be a reference point when considering relevant impacts on other contexts. On the one hand, during the DHS, HIV testing was offered to respondents in a setting where the monetary, opportunity, and psychological costs were minimized (Mishra et al., 2006); respondents could take the test using clean and safe equipment free of charge without visiting health facilities and with no language barrier. Additionally, the strict anonymity and confidentiality of the test results in line with internationally accepted medical standards would have decreased their fear of legal punishment and/or concern about HIV stigma (at least, in principle). Therefore, the law may strongly discourage the uptake of HIV testing if traveling to local medical facilities is needed for voluntary counseling and testing. Also, such an unfavorable effect may be more significant than that shown in this study, since the DHS failed to reach non-household population groups, such as at-risk individuals living on the street or in institutions (e.g., brothels) (Mishra et al., 2008).

On the other hand, the overall HIV prevalence in Mali is not significantly high. Therefore, compared with epidemic countries (e.g., eastern and southern Africa), this "minorityness" of PLH, yet with the overly perceived risk of HIV infection in general (e.g., Godlonton and Thornton, 2013), may have made HIV stigma more widespread in this country, meaning that at-risk individuals would be more selective in their decision to take an HIV test. Additionally, in contrast to the N'Djamena model law adopted in many countries in western and central Africa that also have similar HIV prevalence to Mali, HIV-specific laws introduced in epidemic countries in eastern and southern Africa are less salient on the issue of HIV criminalization and are more inclusive of marginalized people (e.g., Csete et al., 2009, p. 158). Therefore, HIV-specific legislation in the latter region may not necessarily induce HIV-positive individuals' avoidance of HIV testing.

Admittedly, I identified only the total and immediate impact of the law's protective and punitive provisions on HIV testing.[20] Therefore, an urgent need to explore the law's longer-term impact on this and other outcomes, such as sexual behavior and HIV prevalence remains, including the relative significance of both provisions. Nevertheless, the reported findings still offer an important lesson for those who might use the law to control infectious diseases.

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[20] However, even if the law's test-discouraging effect, as found in this study, was only temporary, it is true that it failed to encourage HIV testing. Additionally, if such an unfavorable effect has long persisted, it would be much worse.

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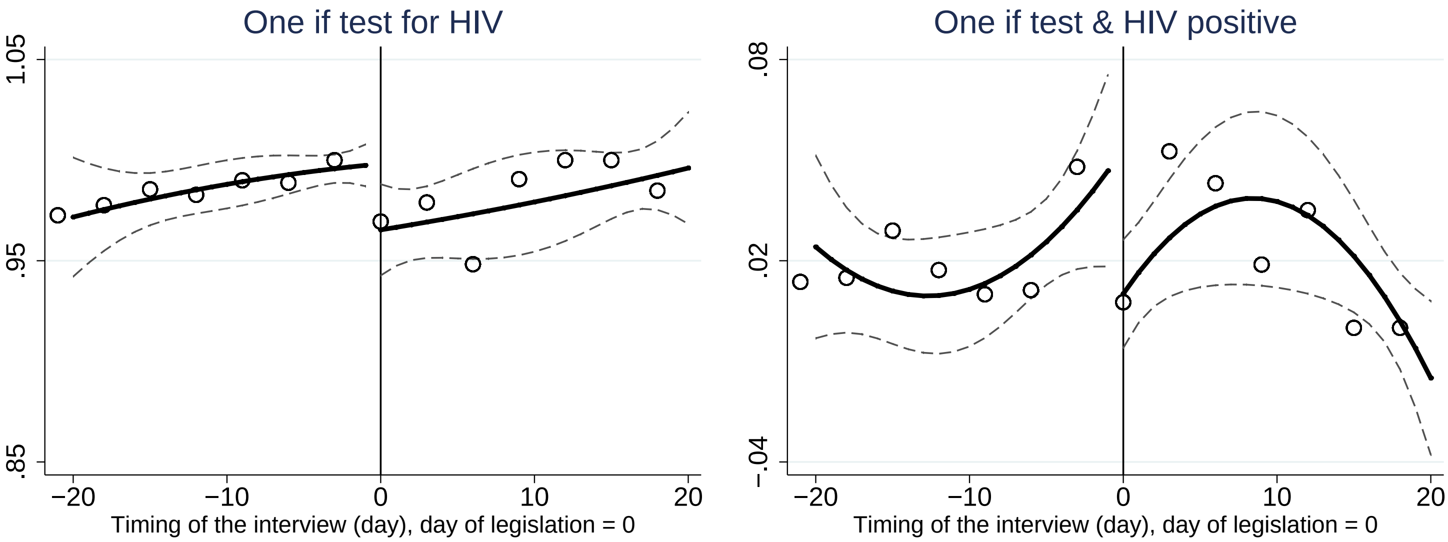

Figure 1: Uptake and serostatus

Note: This figure plots the value of the analyzed variables averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a quadratic polynomial regression of equation (4). Standard errors are clustered at the community and day-of-interview levels.

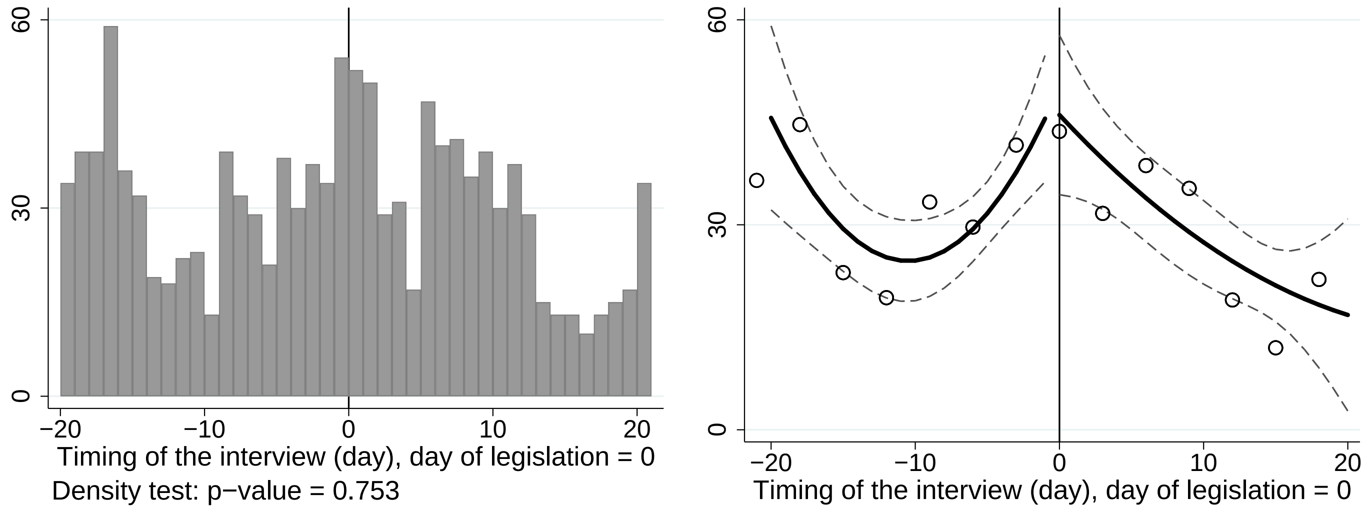

Figure 2: The number of respondents

Notes: (1) The left panel shows the number of respondents interviewed within a 40-day symmetric time window around the day of legislation. (2) The right panel plots the number of respondents averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a global quadratic polynomial regression of equation (4). Unlike standard errors adjusted for two-way clustering in Figure 1, I used robust standard errors because the analytical unit in this panel is the day of the interview.

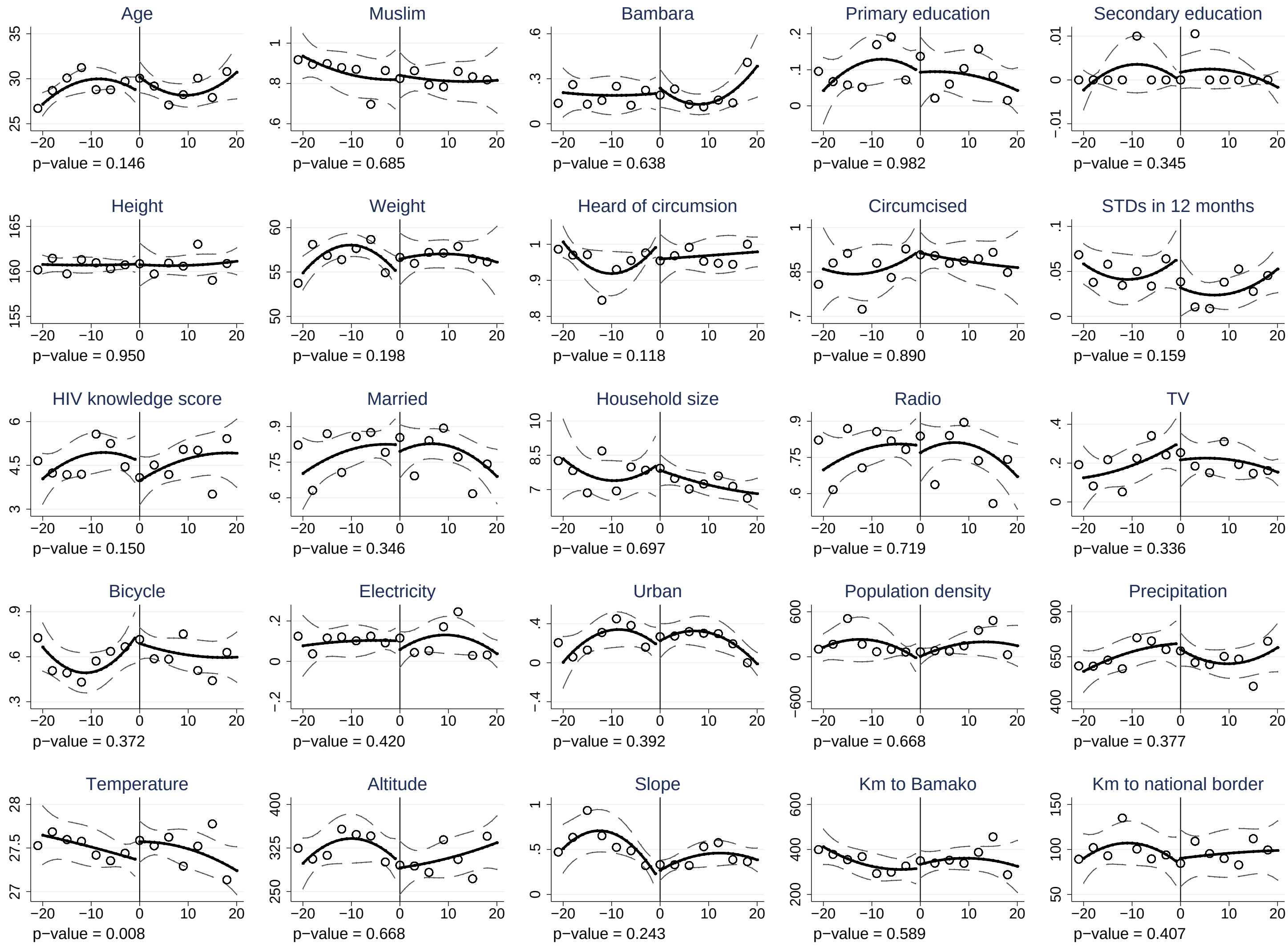

Figure 3: Balanced covariate checks

Notes: (1) This figure plots the value of the analyzed variables averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a quadratic polynomial regression of equation (4). Standard errors are clustered at the community and day-of-interview levels. (2) The p-value of the estimated α_2 is reported at the bottom of each panel after estimating a quadratic polynomial model of equation (4).

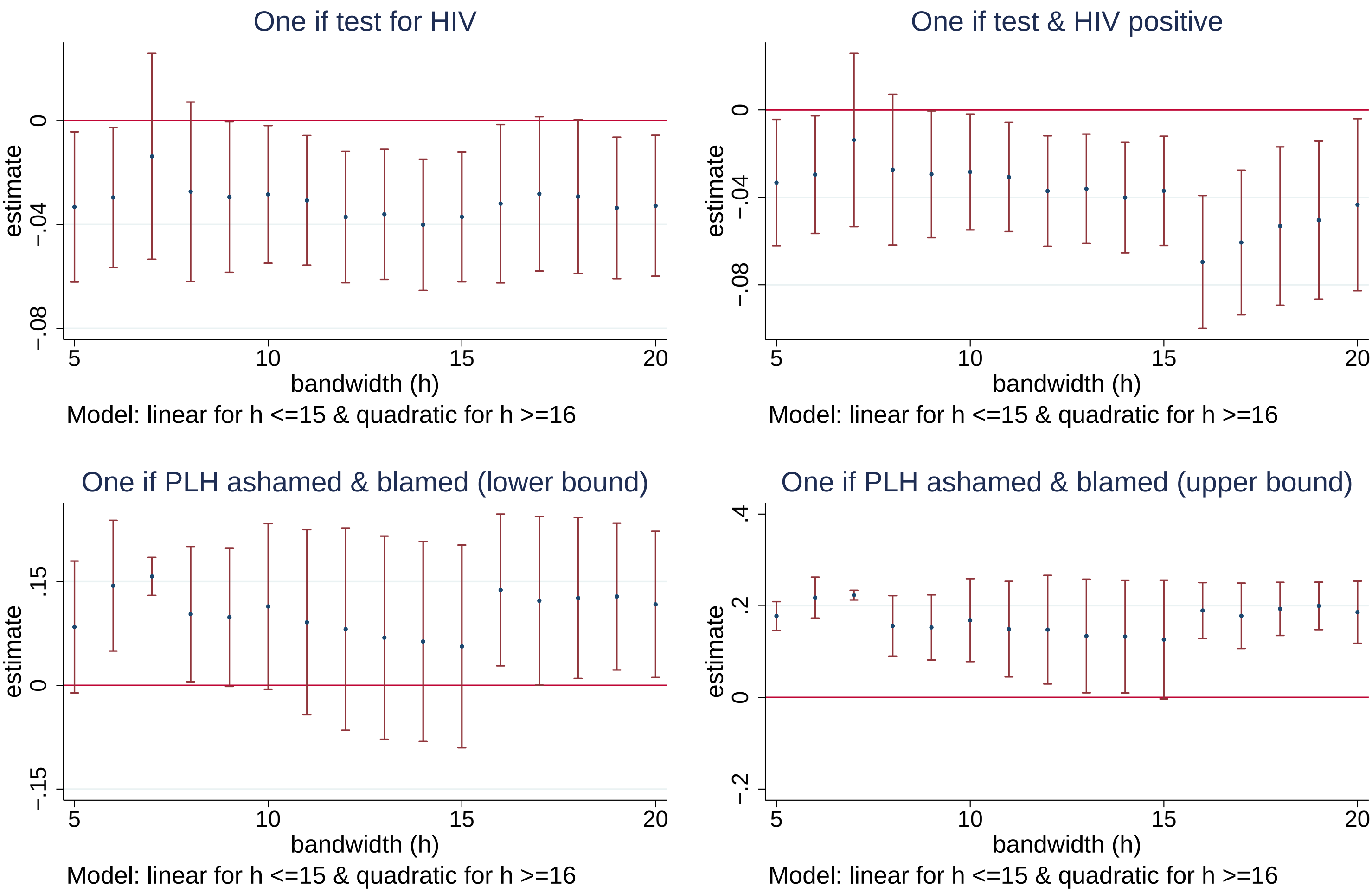

Figure 4: Sensitivity to a range of bandwidths and orders of polynomials (OLS)

Note: This figure reports the estimated α_2 in equation (4) with 95% confidence intervals by varying the choices of bandwidth and the orders of the polynomial. Standard errors are clustered at the community and day-of-interview levels.

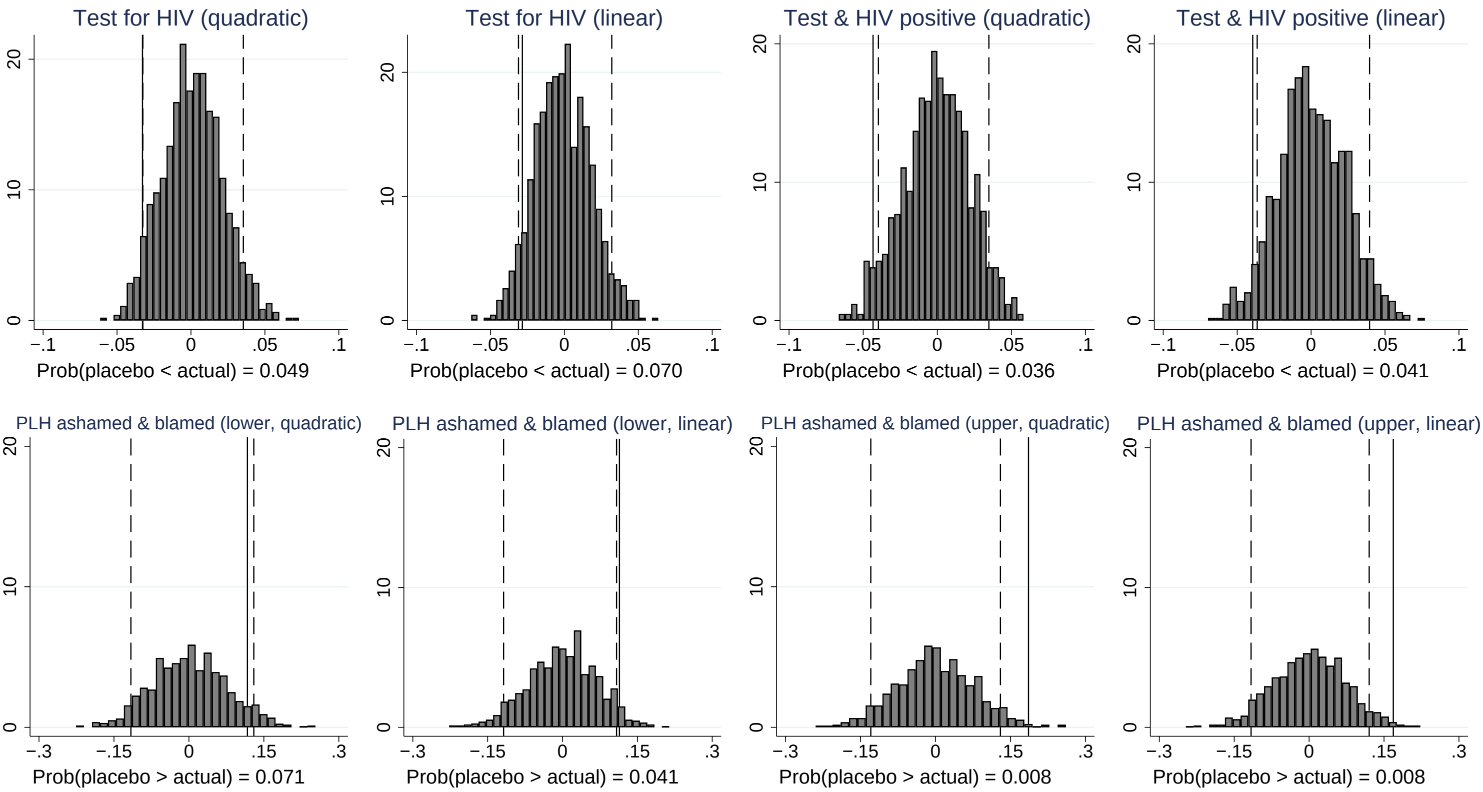

Figure 5: Permutation test: Distribution of placebo estimates (OLS)

Notes: (1) This figure reports a distribution of 1000 placebo estimates (horizontal axis) with the estimates' frequency (vertical axis). Each placebo estimate is obtained by permuting the date of interview at random across individuals and estimating equation (4) using a "fake" treatment indicator based on the "fake" running variable. (2) The dashed lines represent the values of the 5th and 95th percentiles in the respective distribution of the placebo estimates. (3) The solid lines present the original estimates of the parametric regressions (either quadratic OLS with $h = 20$ or linear OLS with $h = 10$), as reported in Table 2 and Table 4.

Table 1: Summary statistics

	Mean	Std.	No. of obs.
Outcomes			
One if test for HIV	0.98	0.13	1255
One if test & HIV positive	0.02	0.15	1255
One if PLH should be ashamed of themselves & blamed for bringing the disease to a community†			
Lower bound	0.29	0.45	1023
Upper bound	0.35	0.47	1023
One if test for anemia	0.97	0.15	1245
One if test & anemic (hemoglobin $\leq$ 12 g/dl)	0.64	0.47	1228
Covariates			
Age (years)	29.02	9.73	1255
Muslim (dummy)	0.84	0.36	1255
Bambara (dummy)	0.19	0.39	1255
Highest level of education (dummy)			
Primary	0.09	0.29	1255
Secondary or above	0.00	0.03	1255
Height (cm)	160.74	6.82	1241
Weight (kg)	56.75	10.83	1225
Heard of circumcision (dummy)	0.96	0.19	1255
Circumcised (dummy)	0.87	0.32	1255
STDs in last 12 months (dummy)	0.04	0.19	1250
HIV knowledge score (0—10)	4.60	3.19	1227
Married (dummy)	0.86	0.34	1255
Household size	7.54	3.96	1255
Household property (dummy)			
Radio	0.77	0.41	1231
Television	0.20	0.40	1231
Bicycle	0.60	0.48	1233
Electricity	0.09	0.29	1231
Urban (dummy)	0.24	0.42	1255
Population density, 2005	142.23	561.41	1251
Precipitation (mm), 2005	652.62	252.14	1251
Temperature (°C), 1970—2000	27.50	0.58	1251
Altitude (m)	315.22	91.35	1255
Slope (degree)	0.47	0.50	1251
Distance to Bamako (km)	346.25	165.30	1251
Distance to the national border (km)	96.87	66.17	1251

Notes: (1) The information is relevant only to respondents who have heard of AIDS for †. (2) The circumcision indicator takes a value of zero for uncut women and those who have never head of circumcision. (3) HIV knowledge score takes a value of zero for those who have never heard of AIDS.

Table 2: Uptake and serostatus

	Quadratic OLS	Quadratic OLS	Linear OLS	Linear OLS	Nonparametric CCT	Nonparametric IK
	(a)	(b)	(c)	(d)	(e)	(f)
(A) One if test for HIV						
Post law (S_i)	-0.033**	-0.029**	-0.028**	-0.021*	-0.043**	-0.038*
	(0.014)	(0.012)	(0.013)	(0.011)	(0.022)	(0.020)
Bandwidth (h)	20	20	10	10	5.70	6.74
R-squared	0.004	0.039	0.008	0.046	-	-
No. of obs.	1255	1162	738	695	419	480
	(g)	(h)	(i)	(j)	(k)	(l)
(B) One if test & HIV positive						
Post law (S_i)	-0.043**	-0.036**	-0.039**	-0.035*	-0.081**	-0.078**
	(0.020)	(0.018)	(0.019)	(0.021)	(0.039)	(0.036)
Bandwidth (h)	20	20	10	10	6.50	7.45
R-squared	0.008	0.042	0.008	0.053	-	-
No. of obs.	1255	1162	738	695	480	550
Controls	NO	YES	NO	YES	NO	NO
Region FE	NO	YES	NO	YES	NO	NO

Notes: (1) Figures ( ) are standard errors. In columns (a)—(d) and columns (g)—(j), they are clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table 1. (3) In nonparametric regressions, the optimal bandwidth is selected based on the methods developed by either Calonico et al. (2014) (CCT) or Imbens and Kalyanaraman (2012) (IK).

Table 3: Law enforcement and social ties (quadratic OLS, $h = 20$)

	Distance to Bamako				Residential area			
	Above mean	Above mean	Below mean	Below mean	Urban	Urban	Rural	Rural
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
(A) One if test for HIV								
Post law (S_i)	-0.002	-0.015	-0.058**	-0.040**	-0.012	-0.062	-0.037**	-0.025
	(0.021)	(0.015)	(0.028)	(0.019)	(0.048)	(0.046)	(0.018)	(0.020)
R-squared	0.006	0.066	0.013	0.062	0.025	0.103	0.010	0.050
No. of obs.	560	507	695	655	303	278	952	884
	(i)	(j)	(k)	(l)	(m)	(n)	(o)	(p)
(B) One if test & HIV positive								
Post law (S_i)	-0.027	-0.033	-0.058*	-0.038	-0.005	0.014	-0.049**	-0.038*
	(0.024)	(0.031)	(0.034)	(0.030)	(0.049)	(0.096)	(0.023)	(0.021)
R-squared	0.005	0.062	0.019	0.059	0.015	0.126	0.013	0.056
No. of obs.	560	507	695	655	303	278	952	884
Controls	NO	YES	NO	YES	NO	YES	NO	YES
Region FE	NO	YES	NO	YES	NO	YES	NO	YES

Notes: (1) Figures ( ) are standard errors clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table 1.

Table 4: HIV stigma

	Quadratic OLS	Quadratic OLS	Linear OLS	Linear OLS	Nonparametric CCT	Nonparametric IK
	(a)	(b)	(c)	(d)	(e)	(f)
(A) One if PLH should be ashamed & blamed (lower bound)						
Post law (S_i)	0.117**	0.093***	0.114*	0.117**	0.130	0.126
	(0.054)	(0.031)	(0.061)	(0.050)	(0.093)	(0.083)
Heard of	-	0.371***	-	0.390***	-	-
AIDS (dummy)		(0.060)		(0.091)		
Bandwidth (h)	20	20	10	10	7.08	8.93
R-squared	0.012	0.203	0.005	0.258	-	-
No. of obs.	1023	1162	606	695	445	504
	(g)	(h)	(i)	(j)	(k)	(l)
(B) One if PLH should be ashamed & blamed (upper bound)						
Post law (S_i)	0.186***	0.157***	0.169***	0.160***	0.215**	0.214**
	(0.035)	(0.022)	(0.046)	(0.037)	(0.093)	(0.094)
Heard of	-	0.526***	-	0.541***	-	-
AIDS (dummy)		(0.061)		(0.089)		
Bandwidth (h)	20	20	10	10	7.67	7.48
R-squared	0.013	0.253	0.009	0.315	-	-
No. of obs.	1023	1162	606	695	445	445
Controls	NO	YES	NO	YES	NO	NO
Region FE	NO	YES	NO	YES	NO	NO

Notes: (1) Figures ( ) are standard errors. In columns (a)—(d) and columns (g)—(j), they are clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table 1. (3) In nonparametric regressions, the optimal bandwidth is selected based on the methods developed by either Calonico et al. (2014) (CCT) or Imbens and Kalyanaraman (2012) (IK). (4) In columns (a), (c), (g) and (i) of parametric estimations and the nonparametric estimations, I used only data from respondents who (reported that they) had heard of AIDS.

Table 5: Awareness of the law (quadratic OLS, $h = 20$)

	Have a radio				Listen to the radio at least once a week			
	Yes	Yes	No	No	Yes	Yes	No	No
	(1a)	(1b)	(1c)	(1d)	(1e)	(1f)	(1g)	(1h)
(A) One if test for HIV								
Post law (S_i)	-0.043***	-0.038***	0.011	0.010	-0.045***	-0.035***	-0.016	-0.016
	(0.015)	(0.014)	(0.016)	(0.016)	(0.013)	(0.011)	(0.017)	(0.022)
R-squared	0.006	0.037	0.023	0.136	0.013	0.045	0.021	0.112
No. of obs.	956	902	275	260	805	737	447	422
	(2a)	(2b)	(2c)	(2d)	(2e)	(2f)	(2g)	(2h)
(B) One if test & HIV positive								
Post law (S_i)	-0.023	-0.032*	-0.055	-0.048	-0.055**	-0.049**	-0.030	-0.032
	(0.015)	(0.019)	(0.057)	(0.054)	(0.024)	(0.024)	(0.033)	(0.040)
R-squared	0.009	0.051	0.036	0.121	0.008	0.063	0.014	0.075
No. of obs.	956	902	275	260	805	737	447	422
	(3a)	(3b)	(3c)	(3d)	(3e)	(3f)	(3g)	(3h)
(C) One if PLH should be ashamed & blamed (lower bound)								
Post law (S_i)	0.116***	0.113***	-0.033	0.059	0.123**	0.120**	-0.010	0.015
	(0.003)	(0.018)	(0.247)	(0.171)	(0.062)	(0.047)	(0.135)	(0.086)
Heard of	-	0.319***	-	0.544***	-	0.340***	-	0.419***
AIDS (dummy)		(0.066)		(0.110)		(0.060)		(0.094)
R-squared	0.014	0.189	0.018	0.357	0.015	0.174	0.022	0.318
No. of obs.	796	902	209	260	704	737	316	422
	(4a)	(4b)	(4c)	(4d)	(4e)	(4f)	(4g)	(4h)
(D) One if PLH should be ashamed & blamed (upper bound)								
Post law (S_i)	0.151***	0.159***	0.193	0.163	0.153***	0.176***	0.183	0.108
	(0.008)	(0.022)	(0.172)	(0.122)	(0.053)	(0.039)	(0.116)	(0.090)
Heard of	-	0.481***	-	0.691***	-	0.486***	-	0.555***
AIDS (dummy)		(0.064)		(0.106)		(0.066)		(0.079)
R-squared	0.010	0.231	0.028	0.407	0.009	0.199	0.025	0.396
No. of obs.	796	902	209	260	704	737	316	422
Controls	NO	YES	NO	YES	NO	YES	NO	YES
Region FE	NO	YES	NO	YES	NO	YES	NO	YES

Notes: (1) Figures ( ) are standard errors clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table 1. (3) In columns (3a), (3c), (3e), (3g), (4a), (4c), (4e), and (4g), I used only data from respondents who (reported that they) had heard of AIDS.

Supplementary material to "Impact of Legislation for Infectious Disease Control: Evidence from HIV Testing in Mali"

S.1 Data, survey manuals, and variable description

The DHS data and relevant documents are publicly available from `https://www.dhsprogram.com/what-we-do/survey/survey-display-276.cfm` with an assessment and approval of the users' request. Access to the locational information of the surveyed communities and HIV datasets requires additional permission because of its sensitive nature. Regarding the survey manuals, see also ICF International (2012) and ICF (2020).

Several covariates, as used in this study, are described below. Among them, community-level geospatial covariates are directly available from the 2006 DHS in Mali; see Mayala et al. (2018) for more details.

HIV-related knowledge score: the total number of correct answers to the following ten questions: (1) whether no sex decreases the chances of getting HIV; (2) whether always using condoms decreases the chances of getting HIV; (3) whether having only one partner with no other partners decreases the chances of getting HIV; (4) whether HIV can be transmitted through mosquito bites; (5) whether HIV can be transmitted through food-sharing with PLH; (6) whether a healthy-looking person can have HIV; (7) whether HIV can be transmitted from mother to child during pregnancy; (8) whether HIV can be transmitted from mother to child during delivery; (9) whether HIV can be transmitted from mother to child through breastfeeding; and (10) whether HIV can be transmitted through witchcraft or supernatural means. This score also takes a zero value for the respondents who reported having never heard of AIDS.

Temperature: average temperature from 1970 to 2000 in degrees Celsius within a 2 km (urban) or 10 km (rural) buffer surrounding each DHS community.

Precipitation: average annual rainfall (mm) in 2005 within a 2 km (urban) or 10 km (rural) buffer surrounding each DHS community.

Slope: 30-arc second terrain roughness (degrees) in 1996 around each DHS community.

Altitude: altitude (m) in each DHS community.

Distance to the national boarder: distance (km) to the nearest international borders in 2014.

United Nations (UN) Population density: average UN-adjusted population density of the area within a 2 km (urban) or 10 km (rural) buffer surrounding each DHS community in 2005.

S.2 Results for males

I analyzed the data from male respondents—1137 men in 114 communities—who completed the individual interview within a 40-day symmetric time window around the legislation day, where 95.07% of them took an HIV test with 0.93% of those tested being seropositive; see also Table S.4 for summary statistics.

Similar to Figure 2, I examined nonrandom sorting in Figure S.6, where the null of no discontinuity in the density at the cutoff was not rejected for the male respondents. However, for the pre-determined covariates mentioned above (excluding height, weight, and circumcision-related variables available only for females), the null of no discontinuity was rejected at the conventional level of statistical significance for seven (out of the 21) items in Figure S.7, where I performed similar exercises to those in Figure 3. Notably, on the cutoff day, there was a positive jump in male respondents' HIV-related knowledge scores and a negative jump in the likelihood of contracting STDs in the last 12 months, with 10% and 5% statistical significance, respectively. These findings may indicate that the fraction of those at a lower risk of HIV infection (i.e., those who were more knowledgeable about HIV and had not contracted STDs) increased immediately after the legislation.

Importantly, callback visits for interviews would be more common for men than women, as the former would usually be absent at the time of the interview and/or have many things to do outside the home, which prevents field workers from completing their interviews. Therefore, the possible change in the order of interviews across respondents might have generated these findings, thereby raising concerns about the validity of the RD design. Indeed, the rate of survey response for men was 91%, which was lower than 97% for women (Samaké et al., 2007, pp. 9—10).

Given this concern, equation (4) was estimated in Table S.5 (see also Figure S.8 for the graphical analysis). The estimated impact of the law on men's HIV test uptake was almost zero in most empirical specifications, and the legislation was negatively associated with those who tested seropositive, although there was no statistical significance. Two interpretations of these findings are possible. First, similar to the consequences for women, HIV-specific legislation discouraged test uptake among HIV-positive men. However, the estimated α_2 was biased upward due to the increased composition of (likely) HIV-negative respondents immediately after the legislation, as described above. Second, the law had a weaker impact on men than women, because it is considered to disproportionately affect the latter, as described in Section 4. The strict interpretation of the findings is left for future research.

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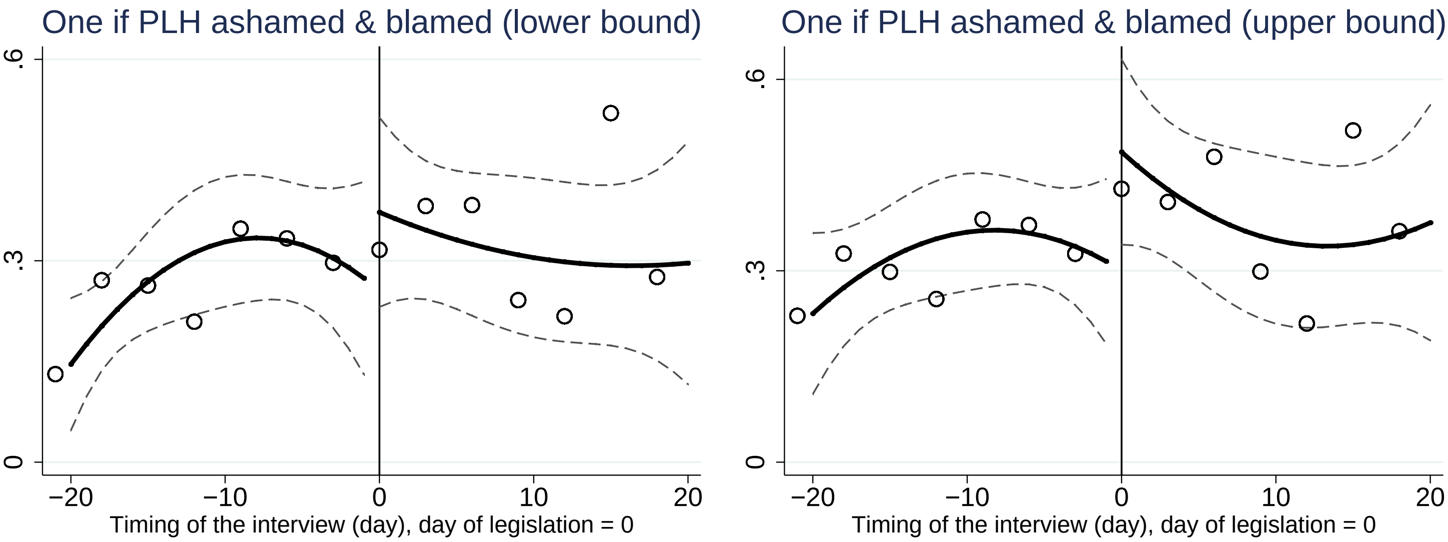

Figure S.1: HIV stigma

Note: This figure plots the value of the analyzed variables averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a quadratic polynomial regression of equation (4). Standard errors are clustered at the community and day-of-interview levels.

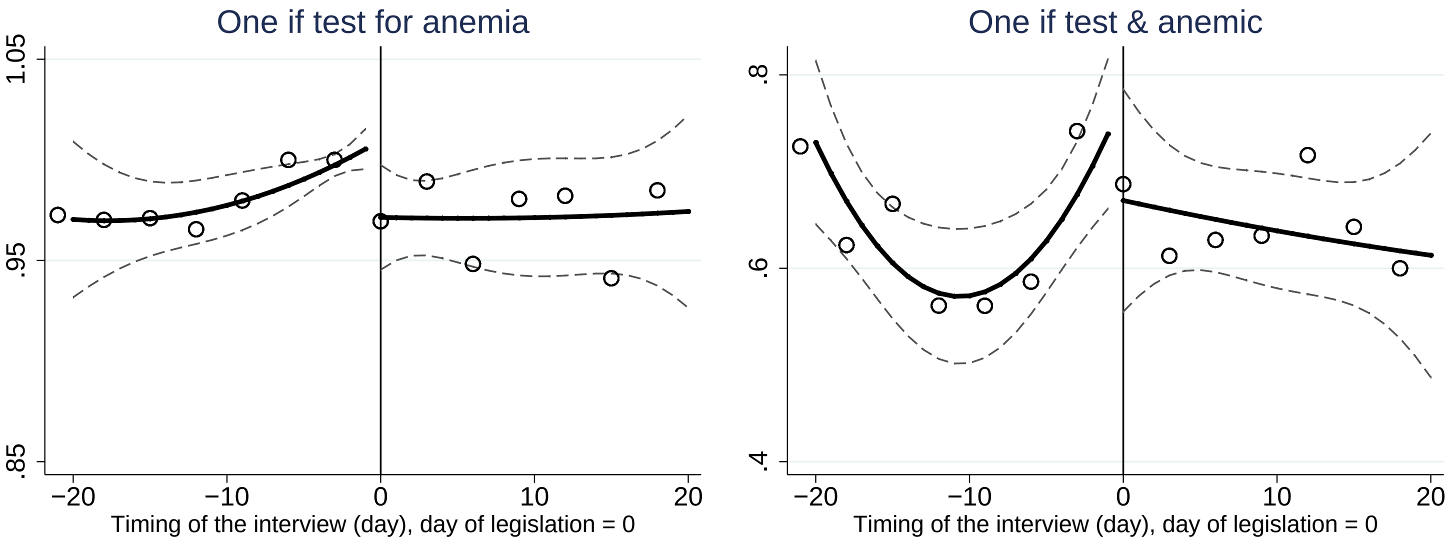

Figure S.2: Anemia: Uptake and status

Note: This figure plots the value of the analyzed variables averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a quadratic polynomial regression of equation (4). Standard errors are clustered at the community and day-of-interview levels.

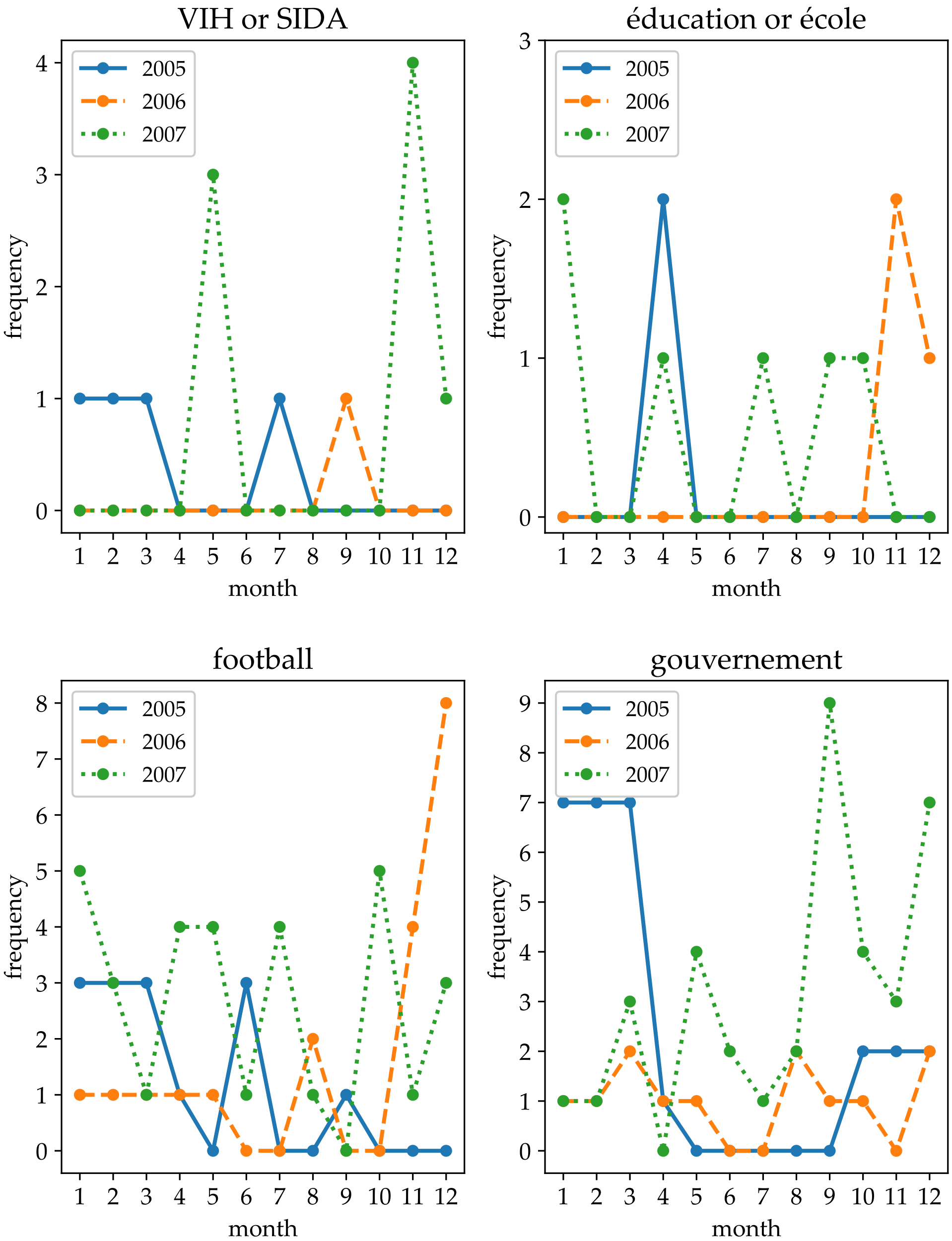

Figure S.3: Web scraping results

Note: This figure reports the frequency of words that appeared in news headlines and contents on MaliWeb.net in 2005—2007.

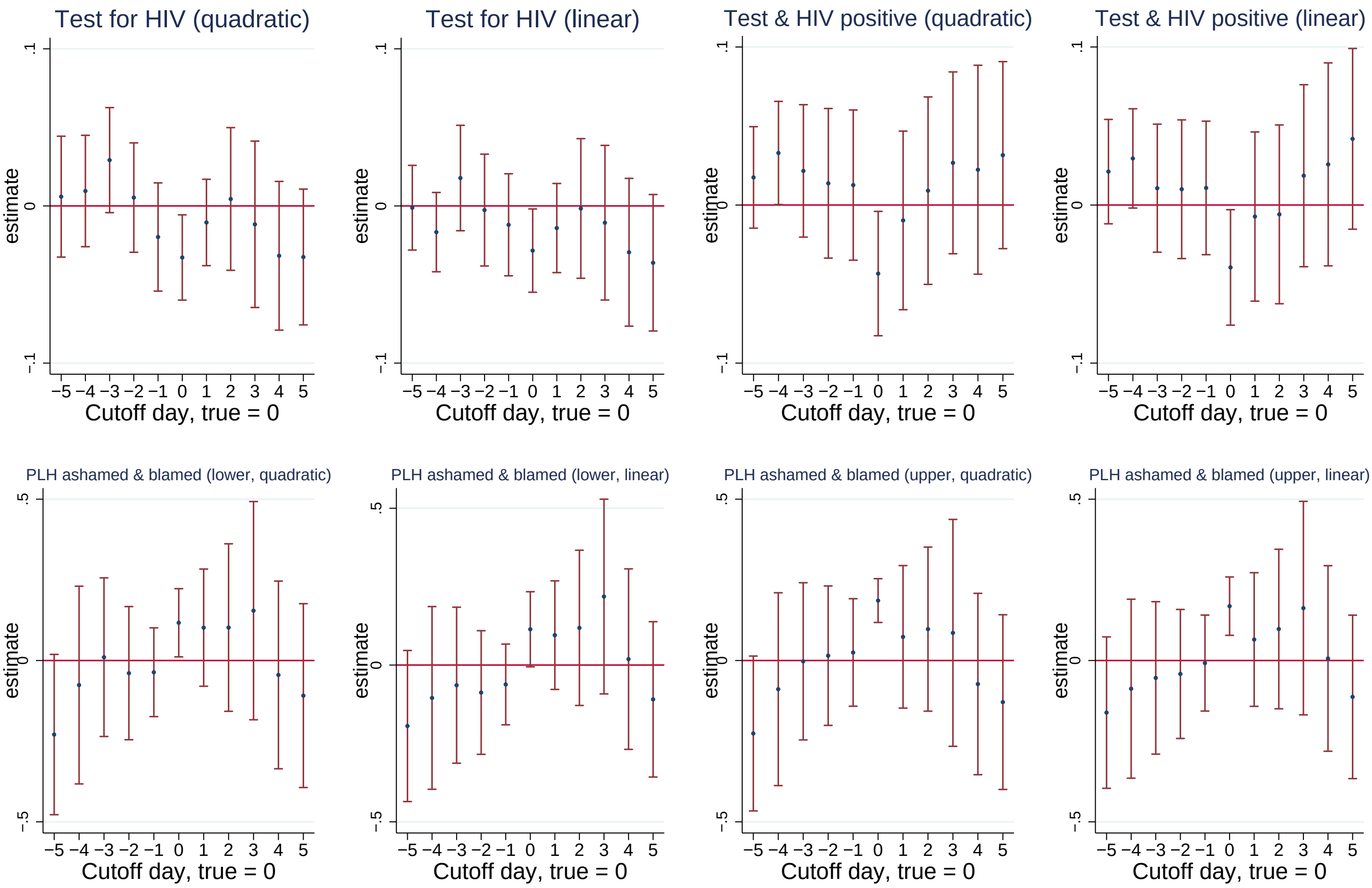

Figure S.4: Checks on the absence of discontinuities at other dates of the interview (OLS)

Notes: (1) This figure reports the estimated α_2 of the parametric regressions (either quadratic OLS with $h = 20$ or linear OLS with $h = 10$) with 95% confidence intervals, wherein the estimates corresponding to the value M (that varies from -5 to 5) on the horizontal axis stem from the regression of equation (4) using "June 29, 2006, + M days" at the cutoff day. For example, the estimated α_2 using June 28, 2006 (or June 30, 2006) at the cutoff day is reported at $M = -1$ (or $M = 1$). The original estimates, as reported in Tables 2 and 4, are shown at $M = 0$. (2) Standard errors are clustered at the community and day-of-interview levels.

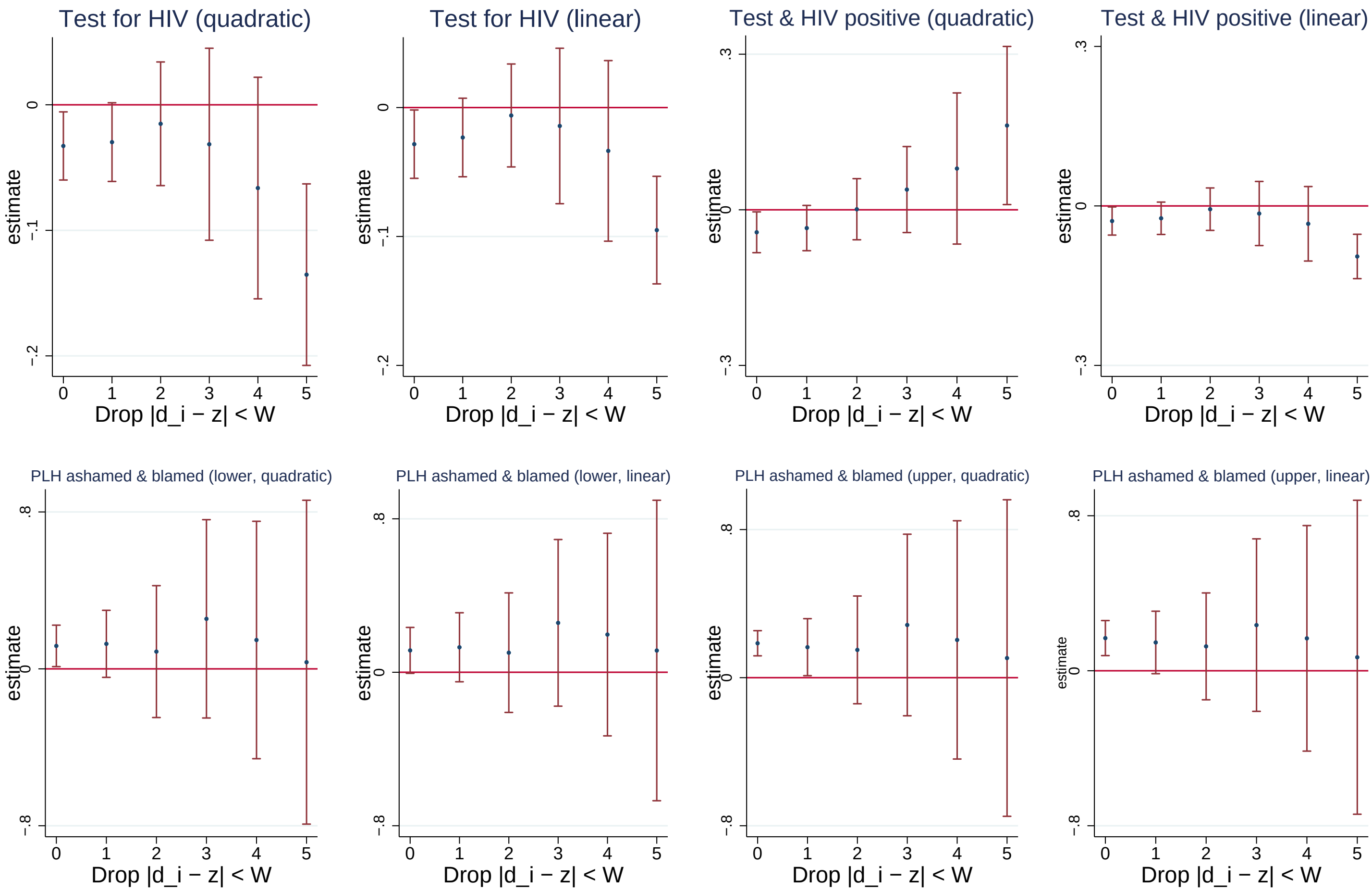

Figure S.5: Sensitivity to observations near the cutoff (OLS)

Notes: (1) This figure reports the estimated α_2 of the parametric regressions (either quadratic OLS with $h = 20$ or linear OLS with $h = 10$) with 95% confidence intervals, wherein the estimates corresponding to the value W (that varies from 0 to 5) on the horizontal axis stem from the regression of equation (4) using data excluding the observations with $|d_i - z| < W$. For example, the estimated α_2 using data excluding from the sample the respondents interviewed on the day of legislation is reported at $W = 1$. The original estimates, as reported in Tables 2 and 4, are shown at $W = 0$. (2) Standard errors are clustered at the community and day-of-interview levels.

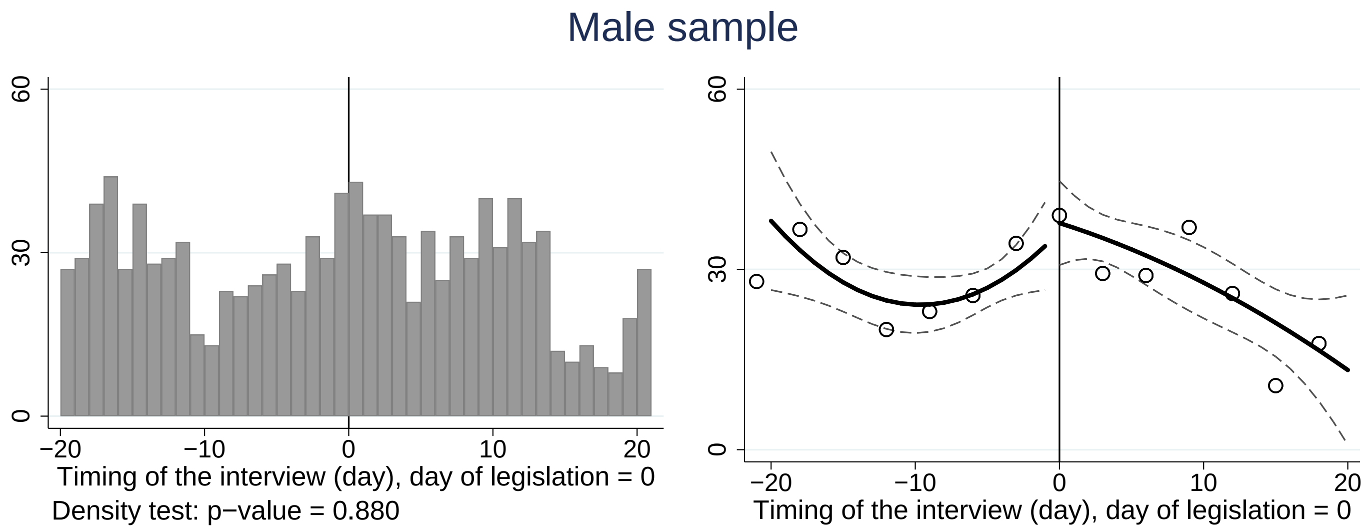

Figure S.6: The number of respondents: Male sample

Notes: (1) The left panel shows the number of respondents interviewed within a 40-day symmetric time window around the day of legislation. (2) The right panel plots the number of respondents averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a global quadratic polynomial regression of equation (4). Unlike standard errors adjusted for two-way clustering in Figure 1, I used robust standard errors because the analytical unit in this panel is the day of the interview.

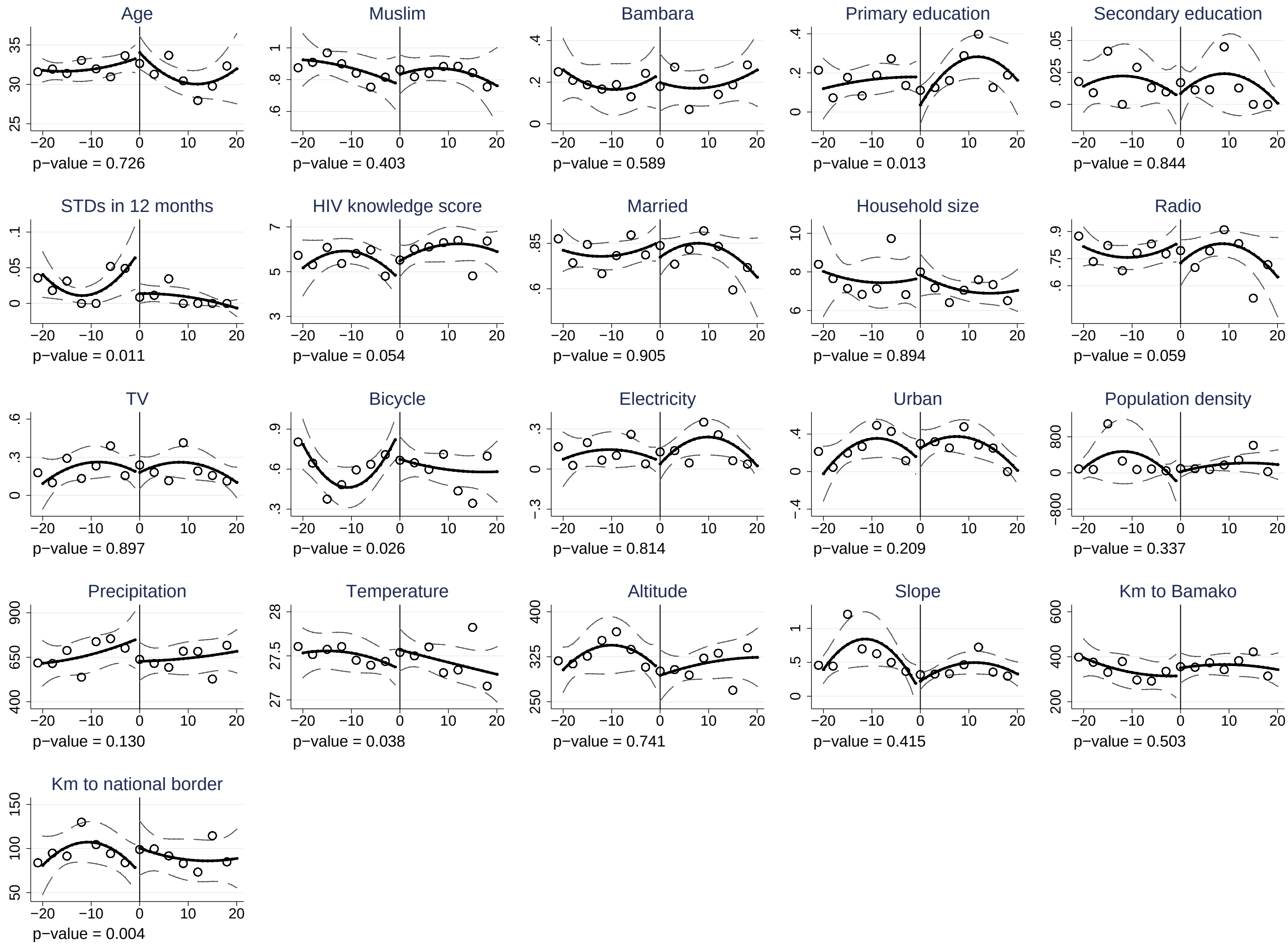

Figure S.7: Balanced covariate checks: Male sample

Notes: (1) This figure plots the value of the analyzed variables averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a quadratic polynomial regression of equation (4). Standard errors are clustered at the community and day-of-interview levels. (2) The p-value of the estimated α_2 is reported at the bottom of each panel after estimating a quadratic polynomial model of equation (4).

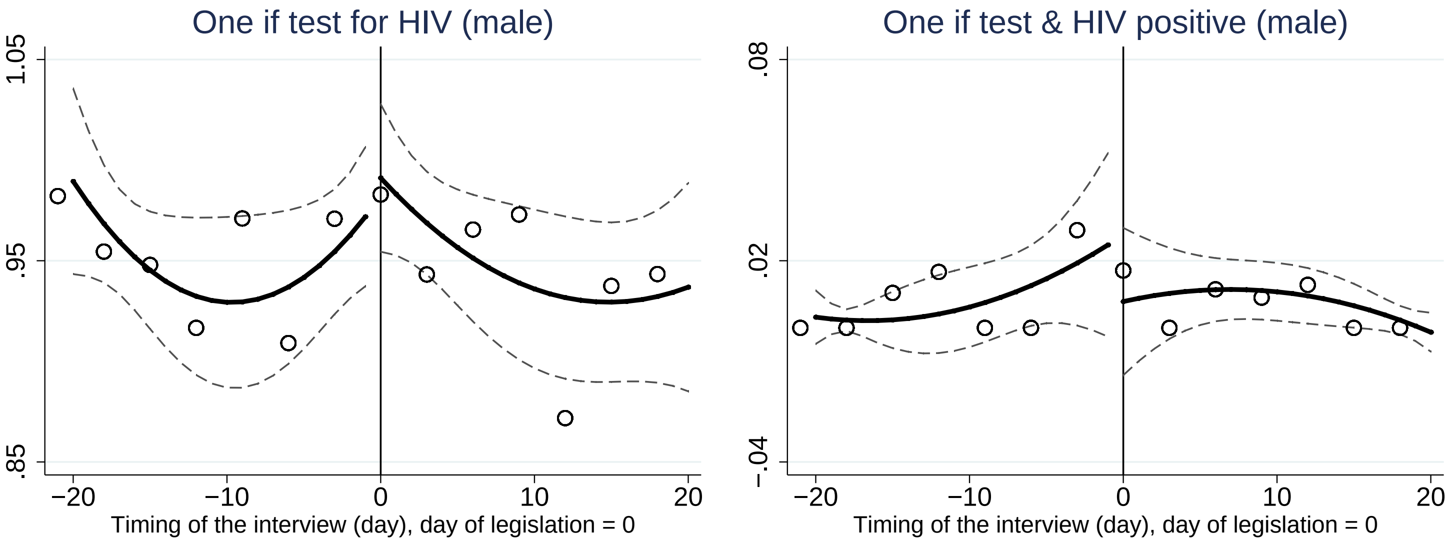

Figure S.8: Uptake and serostatus: Male sample

Note: This figure plots the value of the analyzed variables averaged within three-day bins (scatter plots) together with the predicted outcomes (solid line) and associated 95% confidence intervals (dashed line) based on a quadratic polynomial regression of equation (4). Standard errors are clustered at the community and day-of-interview levels.

Table S.1: Law enforcement: Heterogeneity by the median distance to Bamako (quadratic OLS, $h = 20$)

	Distance to Bamako			
	Above median	Above median	Below median	Below median
	(a)	(b)	(c)	(d)
(A) One if test for HIV				
Post law (S_i)	-0.008	-0.013	-0.055*	-0.027
	(0.020)	(0.016)	(0.029)	(0.021)
R-squared	0.005	0.073	0.014	0.069
No. of obs.	631	578	624	584
	(e)	(f)	(g)	(h)
(B) One if test & HIV positive				
Post law (S_i)	-0.029	-0.038	-0.058*	-0.029
	(0.022)	(0.028)	(0.035)	(0.032)
R-squared	0.005	0.059	0.017	0.073
No. of obs.	631	578	624	584
Controls	NO	YES	NO	YES
Region FE	NO	YES	NO	YES

Notes: (1) Figures ( ) are standard errors clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table 1.

Table S.2: Results on anemia testing

	Quadratic OLS	Quadratic OLS	Linear OLS	Linear OLS	Nonparametric CCT	Nonparametric IK
	(a)	(b)	(c)	(d)	(e)	(f)
(A) One if test for anemia						
Post law (S_i)	-0.038**	-0.026*	-0.029**	-0.014	-0.038*	-0.038*
	(0.015)	(0.015)	(0.014)	(0.013)	(0.020)	(0.021)
Bandwidth (h)	20	20	10	10	6.28	6.07
R-squared	0.005	0.048	0.009	0.061	-	-
No. of obs.	1245	1154	734	691	477	477
	(g)	(h)	(i)	(j)	(k)	(l)
(B) One if test & anemic (hemoglobin $\leq$ 12 g/dl)						
Post law (S_i)	-0.106*	-0.134	-0.072	-0.093	-0.106	-0.095
	(0.060)	(0.083)	(0.059)	(0.077)	(0.085)	(0.078)
Bandwidth (h)	20	20	10	10	6.47	7.76
R-squared	0.007	0.066	0.007	0.095	-	-
No. of obs.	1228	1150	729	688	475	543
Controls	NO	YES	NO	YES	NO	NO
Region FE	NO	YES	NO	YES	NO	NO

Notes: (1) Figures ( ) are standard errors. In columns (a)—(d) and columns (g)—(j), they are clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table 1. (3) In nonparametric regressions, the optimal bandwidth is selected based on the methods developed by either Calonico et al. (2014) (CCT) or Imbens and Kalyanaraman (2012) (IK).

Table S.3: Multiple hypothesis testing

	Table	Col.	Original coefficient	Original p-values	Adjusted p-values: Bonferroni	Adjusted p-values: Holm (1979)	Adjusted p-values: Hochberg (1988)	Sharpened q-values Benjamini et al. (2006)
Dependent variables:			(a)	(b)	(c)	(d)	(e)	(f)
(A) Quadratic OLS, $h = 20$								
Test for HIV	2	(a)	-0.033	(0.018)	(0.053)	(0.053)	(0.030)	(0.032)
Test & HIV positive	2	(g)	-0.043	(0.031)	(0.092)	(0.060)	(0.030)	(0.032)
PLH should be ashamed & blamed								
Lower bound	4	(a)	0.117	(0.030)	(0.090)	(0.060)	(0.030)	(0.032)
(B) Linear OLS, $h = \mathbf{10}$								
Test for HIV	2	(c)	-0.028	(0.036)	(0.107)	(0.103)	(0.061)	(0.057)
Test & HIV positive	2	(i)	-0.039	(0.034)	(0.103)	(0.103)	(0.061)	(0.057)
PLH should be ashamed & blamed								
Lower bound	4	(c)	0.114	(0.062)	(0.185)	(0.103)	(0.061)	(0.057)

Note: In this table, I report p-values that adjust the familywise error rate using (conservative) Bonferroni, Holm (1979), and Hochberg (1988) and sharpened q-values that control the false discovery rate using Benjamini et al. (2006).

Table S.4: Summary statistics: Male sample

	Mean	Std.	No. of obs.
Outcomes			
One if test for HIV	0.95	0.21	1137
One if test & HIV positive	0.00	0.09	1137
One if PLH should be ashamed of themselves & blamed for bringing the disease to a community†			
Lower bound	0.32	0.46	1037
Upper bound	0.39	0.48	1037
Covariates			
Age (years)	31.72	12.98	1137
Muslim (dummy)	0.85	0.34	1137
Bambara (dummy)	0.19	0.39	1137
Highest level of education (dummy)			
Primary	0.18	0.38	1137
Secondary or above	0.01	0.13	1137
STDs in last 12 months (dummy)	0.01	0.13	1134
HIV knowledge score (0—10)	5.77	2.90	1116
Married (dummy)	0.64	0.47	1137
Household size	7.41	4.11	1137
Household property (dummy)			
Radio	0.78	0.41	1135
Television	0.21	0.41	1137
Bicycle	0.60	0.48	1137
Electricity	0.14	0.34	1134
Urban (dummy)	0.26	0.44	1137
Population density, 2005	210.79	817.72	1137
Precipitation (mm), 2005	656.61	259.56	1137
Temperature (°C), 1970—2000	27.47	0.57	1137
Altitude (m)	319.74	90.90	1137
Slope (degree)	0.51	0.57	1137
Distance to Bamako (km)	350.96	167.36	1137
Distance to the national border (km)	93.41	64.38	1137

Notes: (1) The information is relevant only to respondents who have heard of AIDS for †. (2) HIV knowledge score takes a value of zero for those who have never heard of AIDS.

Table S.5: Uptake and serostatus: Male sample

	Quadratic	Quadratic	Linear	Linear	Non-parametric	
	OLS	OLS	OLS	OLS	CCT	IK
	(a)	(b)	(c)	(d)	(e)	(f)
(A) One if test for HIV						
Post law (S_i)	0.009	0.018	0.003	0.011	0.014	-0.001
	(0.016)	(0.017)	(0.018)	(0.017)	(0.038)	(0.032)
Bandwidth (h)	20	20	10	10	6.46	8.85
R-squared	0.008	0.058	0.006	0.082	-	-
No. of obs.	1137	1108	625	609	410	518
	(g)	(h)	(i)	(j)	(k)	(l)
(B) One if test & HIV positive						
Post law (S_i)	-0.020	-0.029	-0.022	-0.030	-0.035	-0.031
	(0.021)	(0.022)	(0.021)	(0.020)	(0.027)	(0.025)
Bandwidth (h)	20	20	10	10	7.75	9.58
R-squared	0.004	0.027	0.008	0.035	-	-
No. of obs.	1137	1108	625	609	467	581
Controls	NO	YES	NO	YES	NO	NO
Region FE	NO	YES	NO	YES	NO	NO

Notes: (1) Figures ( ) are standard errors. In columns (a)—(d) and columns (g)—(j), they are clustered at the community and day-of-interview levels. *** denotes significance at 1%, ** at 5%, and * at 10%. (2) The exploited controls are all covariates, as reported in Table S.4 in the supplementary material. (3) In nonparametric regressions, the optimal bandwidth is selected based on the methods developed by either Calonico et al. (2014) (CCT) or Imbens and Kalyanaraman (2012) (IK).