UNIVERSITY OF CALIFORNIA, SAN DIEGO

# Essays in Health and Development Economics

A dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Economics

by

Shanthi Manian

Committee in charge:

Professor Joshua Graff Zivin, Chair
Professor Eli Berman, Co-Chair
Professor Prashant Bharadwaj
Professor Craig McIntosh
Professor Steffanie Strathdee

2017

The dissertation of Shanthi Manian is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

_____

_____

_____

_____
Co-Chair

_____
Chair

University of California, San Diego

2017

DEDICATION

To my husband,

*Samba Sow*,

who has been a true partner in this challenging, exhilarating journey.

To my son,

*Karim Sow*,

who brings me immeasurable joy and lights up every room with his smile.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

I would like to thank my dissertation committee for their invaluable support throughout my graduate studies: Joshua Graff Zivin, Eli Berman, Prashant Bharadwaj, Craig McIntosh, and Steffanie Strathdee. I would also like to thank Thomas Patterson and Hugo Staines, who, along with Steffanie Strathdee, generously shared the data from their behavioral intervention trial for use in Chapter 1. I am eternally grateful to Prof. Souleymane Mboup, Marieme Soumare, and Dr. Ibrahima Traore, without whom Chapter 2 would not have been possible. I thank Saly Amos Diatta, Fatou Gaye, and Ellen Kim for excellent research assistance, three peer educators, and staff at *Association AWA* and *Réseau Africain de Recherche sur le Sida*, especially Ndeye Astou Fall Cissé and Bintou Thiam Ndiaye.

My most sincere gratitude to the following people for extensive comments and moral support: Maya Duru, Erin Giffin, Elizabeth Hastings, Sarojini Hirshleifer, Veena Jeevanandam Blume, Adeline Lo, Héctor Pifarré i Arolas, Mauricio Romero, Brigitte Roth Tran, Erin Troland, Ketki Sheth, and Erin Wolcott. I would also like to thank Stefan Baral, Richard Carson, Julie Cullen, Gordon Dahl, Roger Gordon, Michelle White and seminar participants at UCSD, NEUDC 2015, ACLEC 2016, FISP Symposium 2016, PacDev 2017 and MWIEDC 2017 for additional comments on this research.

Finally, I would like to thank my husband, my parents, my brother, my mother-in-law and my sisters-in-law for supporting me at home so I could succeed at work. Special thanks also to Mariah Grooms and staff at Little Sprouts Academy in San Diego for excellent, caring, and enriching childcare.

I gratefully acknowledge funding provided by the National Institute on Drug Abuse (NIDA) grant 3T32DA023356-07S1; the Frontiers of Innovation Scholarship Program, the Policy Design and Evaluation Lab, the Economics department, and

the Dean of Social Sciences, all at UC San Diego; and Captricity.org. Chapter 1 uses data from a behavioral intervention trial funded by NIDA grant RO1DA023877.

Chapter 1, in full, is currently being prepared for submission for publication of the material. Manian, Shanthi. The dissertation author is the sole author of this material.

Chapter 2, in full, is currently being prepared for submission for publication of the material. Manian, Shanthi. The dissertation author is the sole author of this material.

Chapter 3, in full, is currently being prepared for submission for publication of the material. Manian, Shanthi; Shah, Manisha. The dissertation author was the primary investigator and author of this material.

## VITA

| | |
|---|---|
| 2006 | Bachelor of Science in Foreign Service *magna cum laude*, Georgetown University |
| 2014 | Master of Arts in Economics, University of California, San Diego |
| 2017 | Doctor of Philosophy in Economics, University of California, San Diego |

## PUBLICATIONS

Padian N, Manian S. (2011). Invited Comment: The concurrency debate: time to put it to rest. *The Lancet*.

Padian N, McCoy S, Manian S et al. (2011). Evaluation of Complex, Combination HIV Prevention Programs: Essential Issues. *Journal of Acquired Immune Deficiency Syndromes.*

ABSTRACT OF THE DISSERTATION

**Essays in Health and Development Economics**

by

Shanthi Manian

Doctor of Philosophy in Economics

University of California, San Diego, 2017

Professor Joshua Graff Zivin, Chair
Professor Eli Berman, Co-Chair

This dissertation studies responses to uncertainty and incomplete information in the context of female sex markets. Chapter 1 studies the impact of violent conflict on risky sexual behavior and considers implications of the results for expected utility models. Chapter 2 considers the theoretical model underlying sex work "legalization and regulation", a common program that provides government certification of sex workers' health, and shows that key assumptions of the model are not satisfied. Finally, using data from Ecuador, chapter 3 studies whether and how information about sex workers' health is transmitted in sex markets.

# Chapter 1

# Risk Vulnerability and Health Behavior: Evidence from Mexico's Drug War

Theory predicts that individuals facing riskier environments become increasingly averse to new, independent risks (Gollier and Pratt, 1996). This property, known as risk vulnerability, is a feature of most workhorse expected utility models and has been documented empirically in the financial and monetary domains (Heaton and Lucas, 2000; Harrison et al., 2007; Guiso and Paiella, 2008; Lusk and Coble, 2008; Dimmock, 2012). However, research testing vulnerability to non-financial risk is limited. This paper considers the effect of rapid and dramatic changes in background mortality risk, generated by a drug-related conflict in Ciudad Juarez, Mexico, on risky health behavior. Previous research on violence and risk preferences relies on hypothetical or laboratory experiments, with mixed results (Voors et al., 2012; Callen et al., 2014; Brown et al., 2015; Jakiela and Ozier, 2015).[1] This paper is the first to show that changes in background mortality risk can affect risk-taking outside a laboratory setting.

[1] A related literature studies the effect of changes in background mortality risk due to natural disasters. Evidence on risk vulnerability is mixed. van den Berg et al. (2009), Cassar et al. (2011), and Cameron and Shah (2013) find behavior consistent with risk vulnerability among individuals affected by natural disasters in Latin America, Thailand, and Indonesia, respectively. In contrast, Eckel and Grossman (2001) show that individuals affected by Hurricane Katrina in the United States make more risk loving choices.

From 2008 to 2011, Ciudad Juarez became known as the "murder capital" of the world. Following the outbreak of a gory turf war between two drug cartels at the end of 2007, violence escalated rapidly. By 2008, drug-related homicides had increased nearly 10-fold. Between 2008 and 2011, more than 7,500 people were killed in the conflict (Office of the Mexico Attorney-General, 2006). This paper documents the effect of this violence on sexual risk-taking in a population at high risk for HIV infection: female sex workers who inject drugs (FSW-IDUs) and their clients. Due to their participation in two major pathways of HIV transmission (commercial sex and injection drug use), FSW-IDUs play an important role in the epidemiology of HIV (Aral, 2000). HIV prevalence in this population was estimated at 12% in 2008, and rates continue to rise (Strathdee et al., 2008).

Violent conflict is a multidimensional treatment, with many potential effects that can impact behavior. To isolate the immediate effects of the change in background risk, I exploit a unique data structure that permits identification off of daily variation in conflict intensity. Month fixed effects control for any channels that respond slowly or discontinuously to violence (e.g., macroeconomic effects of the conflict). In addition, a panel structure allows me to use within-individual variation by including individual fixed effects.

The data come from a behavioral intervention trial conducted during the conflict.[2] I measure conflict intensity by number of homicides.[3] The trial recruited participants on a rolling basis, then scheduled subsequent surveys quarterly after the baseline interview. Thus, individuals are observed at different, exogenously determined points in time. Although there is no geographic variation in the sample, there is plausibly exogenous variation in the level of violence experienced by a given

[2]The trial began after the onset of conflict, so there is no pre- or post-conflict data.

[3] Homicides were the primary violent activity of the cartels during this period (Astorga and Shirk, 2010).

individual at the time of each interview.

I show that more intense violence leads to a reduction in risky sex transactions, consistent with risk vulnerability among FSW-IDUs or their clients. The preferred specification implies that a one-standard-deviation increase in the number of homicides in a month ($\sigma = 87.8$) reduces the probability of inconsistent condom use with clients by 13.6 percentage points. The result is robust to two measures of conflict intensity, two measures of inconsistent condom use, and controls for bias in self-reports. I rule out several alternate explanations for this result, including demand effects, many potential compositional changes in the commercial sex market, and a mechanism related to drug use. I conclude that the evidence is most consistent with a change in risk attitudes, and I present suggestive evidence that client risk attitudes are the primary driver of the effect.

The results provide new evidence on two models relevant to conflict: the income smoothing model and the human capital model. Conflict is a massive economic shock (Blattman and Miguel, 2010). The income smoothing model predicts that in the absence of credit or insurance, income shocks induce an expansion in labor supply (Kochar, 1999; Jayachandran, 2006). Since there is a compensating wage differential for non-condom use (Rao et al., 2003; Gertler et al., 2005; Arunachalam and Shah, 2013), for FSWs, this translates into expanded supply of unprotected commercial sex (Robinson and Yeh, 2011). Dupas and Robinson (2012) find support for this model during a civil conflict in Kenya, but the population was not directly affected by violence. In contrast, my paper offers evidence in the context of a large change in mortality risk. The human capital model also predicts that conflict will increase unprotected sex. In this model, reduced life expectancy decreases investment in preventive health activities (such as condom use) and other human capital investments (Grossman, 2000). Several authors have

shown the empirical relevance of the human capital model (Jayachandran and Lleras-Muney, 2009; Fortson, 2011; Oster and Thornton, 2011; Oster et al., 2013). My results suggest that ignoring risk attitudes masks important aspects of the behavioral response to conflict.

My results also contribute to a public health literature on the relationship between violent conflict and HIV risk.[4] The belief that violent conflict favors the spread of HIV is prevalent among policymakers and program implementers in the HIV response, but existing empirical evidence is mixed (Spiegel et al., 2007; de Waal, 2010; Iqbal and Zorn, 2010). This paper contributes evidence on one potential mechanism, risky sexual behavior, that has not been rigorously studied in a violent context. The results demonstrate that the relationship between conflict and HIV risk is more complex than suggested by many advocates.

The remainder of this paper is organized as follows. Section 1.1 provides a conceptual framework for the relationship between risk vulnerability and sexual risk-taking. Section 1.2 describes the dataset and variables used in the analysis. Section 1.3 discusses the exogeneity assumption for homicides, addresses issues associated with the use of time series variation for identification, and presents the empirical strategy. Section 1.4 reports the main results on inconsistent condom use, while robustness checks are shown in Section 1.5. Section 1.6 rules out alternate explanations for a reduction in risky sexual behavior due to conflict, and Section 1.7 explores whether the result is driven primarily by sex workers or clients. Section 1.8 provides a discussion of the results and conclusions.

[4]Mock et al. (2004) and Spiegel (2004) review this literature. Numerous potential mechanisms have been described, including fatalism, reduced access to HIV prevention services, and increased mixing due to population movements and military mobilizations. The literature has also described a prediction similar to the income smoothing model, arguing that the interruption of normal economic activity and the loss of male partners to either conscription or death will shift women into transactional sex. Other factors that could affect risky sexual behavior include the disruption of community structures and norms and increased drug use.

## 1.1 Conflict and Risk Vulnerability

Gollier and Pratt (1996) and Eeckhoudt et al. (1996) have shown that for a large class of expected utility models, changes in background risk affect choices about an independent lottery. In a standard expected utility model with Von Neumann-Morgenstern utility, background risk refers to the probability distribution of wealth. If a risk with a nonpositive expectation is added to wealth, the theory predicts that many expected utility maximizers will make more risk averse choices when evaluating an independent lottery. Utility functions exhibiting this property are *risk vulnerable.*[5] The property has intuitive appeal in the financial domain. For example, if the variance of stock market returns increases, it seems natural that a decision-maker holding her wealth in stocks will make a more risk averse choice for her next investment.

Note that the theory does not imply that *preferences* change in response to background risk; instead, individuals make more risk averse *choices* when faced with background risk. This is analogous to the concept of decreasing absolute risk aversion. Preference parameters do not change, but individuals become more or less risk averse as their wealth level changes deterministically.

The intuition extends to health if we consider health and life as having consumption value (Grossman, 2000). Assume that decision-makers derive utility directly from health, that their utility over health is increasing and concave, and that they are expected utility maximizers.[6] Then under weak conditions, the theory

[5]Necessary and sufficient conditions for risk vulnerability are described in Gollier and Pratt (1996) and Eeckhoudt et al. (1996). All utility functions with hyperbolic absolute risk aversion (HARA) are risk vulnerable. This class includes constant and decreasing absolute risk aversion (CARA and DARA), linear utility, quadratic utility, log utility, and the constant relative risk aversion (CRRA) specification commonly used in experimental studies of risk aversion.

[6]If we do not assume expected utility, alternate predictions are possible. For example, Quiggin (2003) shows that rank dependent preferences generate "diminishing sensitivity" to risk, so that independent background risks make individuals more risk loving.

of risk vulnerability implies that when health risk is added to the status quo, the decision-maker will make a more risk averse choice the next time she faces an independent health-related lottery.[7]

Violent conflict adds risk to background health. Noncombatant individuals in conflict zones can become collateral damage, suffering severe injury or death. The key insight of risk vulnerability is that even if the risk has a mean close to zero (i.e. the actual probability of injury or death in the conflict is small), the increased variance will generate more risk averse behavior.

Female sex workers and their clients face an independent health-related lottery every time they negotiate condom use. During the study period, HIV prevalence was estimated at 12 percent among FSW-IDUs in Ciudad Juarez (Strathdee et al., 2008), and 75 percent of the study population had a sexually transmitted infection at baseline. Data are not available for clients in Ciudad Juarez, but a recent study estimated HIV prevalence among clients in Tijuana at 4 percent, and STI prevalence at 14 percent (Patterson et al., 2009). Risk vulnerability implies that when conflict is more severe, FSWs and/or clients will be less willing to accept the risk of unprotected sex, so condom use will increase.

## 1.2 Data and Sampling

The primary dataset comes from a randomized intervention trial to promote safe sex and safe injection practices in FSW-IDUs.[8] The trial was conducted between November 2008 and August 2011 in Ciudad Juarez, Mexico. Participants were recruited on rolling basis via targeted sampling. In collaboration with local

[7] In related work,Dardanoni and Wagstaff (1990) show that for expected utility maximizers, an increase in background health risk increases the demand for inputs into the health production function (e.g., preventive health behavior or medical care).

[8] See Vera et al. (2012) for a detailed description of the intervention and trial design.

NGOs, study staff identified FSW-IDUs in locations they are known to frequent, such as bars, hotels, street corners, shooting galleries, and brothels. Participants completed a baseline survey (visit 1), a short intervention, and three follow-up surveys (visits 2, 3, and 4).[9]

The inclusion criteria for participation in the study highlight the high baseline prevalence of HIV risk behaviors in the sample. Study participants are adult females who had engaged in all of the following behaviors in the month prior to recruitment: (i) exchanged sex for money, drugs, or material goods; (ii) injected drugs at least once; (iii) had unprotected sex with a male client; and (iv) shared injection paraphernalia.[10]

After completing the baseline survey, all participants received a short information session addressing safer sex and safer injection. The control sessions were didactic lectures based on materials available at local free clinics. The intervention sessions, in contrast, were interactive and grounded in psychological theories of behavior change.[11] There was no further intervention after this initial session.

Risky sexual behavior is self-reported in the surveys.[12] The primary outcome

[9]Women who were intoxicated or high, and therefore unable to provide informed consent, were not excluded, but rather rescheduled to a later date.

[10]This high risk profile is not representative of the wider female sex worker population in Mexico, but FSW-IDUs constitute an important and sizable risk group in their own right. In a 2005 study of 450 female sex workers in Ciudad Juarez, 14 percent reported ever injecting drugs, and 9 percent reported injecting in the past month (Strathdee et al., 2008). In Russia, estimates of injection drug use among FSWs range from 25 to 80 percent (Lowndes et al., 2003).

[11]The trial used a 2X2 factorial design. The information session had two 30-minute components addressing safer sex and safer injection respectively. Participants received either two control sessions, one control and one intervention session, or two intervention sessions. The safer injective intervention emphasized the "risk ladder" of injection behaviors and negotiating safer practices with injection partners, including clients. The safer sex intervention focused on negotiating condom use with commercial clients, especially in the context of substance use by the sex worker or the client. The safer injection intervention alone had no treatment effect, but the safer sex intervention significantly reduced combined HIV/sexually transmitted infection incidence—the number of new infections with HIV or any other sexually transmitted infection (Strathdee et al., 2013).

[12]To reduce social desirability bias, all questions relating to participants' own behavior were administered via computer. Evidence suggests that computer-based interviewing improves disclosure of sexual behavior (e.g. Richens et al., 2010; Perlis et al., 2004). To reduce recall bias,

measure is inconsistent condom use (ICU) with clients over the past month. This is a binary indicator constructed from the response to the questions: "In the past month, how often did you use condoms for vaginal/anal sex with your male clients?" The variable is equal to one if the response for either vaginal or anal sex was "Sometimes" or "Never", and equal to zero if the response for both vaginal and anal sex was "Often" or "Always", or if the respondent did not exchange sex during the past month.

I use the total number of homicides in Ciudad Juarez, based on coroner's reports, as a measure of conflict intensity (Sistema Nacional de Información en Salud, 2014).[13] Homicides, which were the primary violent activity of the cartels, are observed daily. However, in the analysis, I aggregate homicides roughly at the month level to match the survey recall period. Risky sexual behavior is measured over the past month. Then, "homicides during the recall period" is calculated as the sum of homicides in the 30 days prior to the interview. I also use lagged homicides in some specifications. The first lag prior to recall corresponds to the number of homicides from 31 to 60 days prior to the interview, the second lag to homicides from 61 to 90 days prior to the interview, etc.

Identification comes from observing individuals at different, predetermined points along the time series shown in Figure 1.1. After the baseline survey, participants were scheduled for follow-ups in 4, 8 and 12 months. The interview dates were determined in advance by study staff.[14]

The baseline sample is 300 participants. Of these, 210 have a complete set of four observations on the primary outcome.[15] Appendix Table 2.13 compares

the analysis is restricted to variables with a recall period of one month.

[13]Note that since all participants live and work in Ciudad Juarez, there is no geographic variation in this measure.

[14]Participants could reschedule interviews, but the mean delay between scheduled and completed visits is just 9 days (the median delay is 2 days).

[15]79 participants completed fewer than four visits. An additional 11 had missing data for the

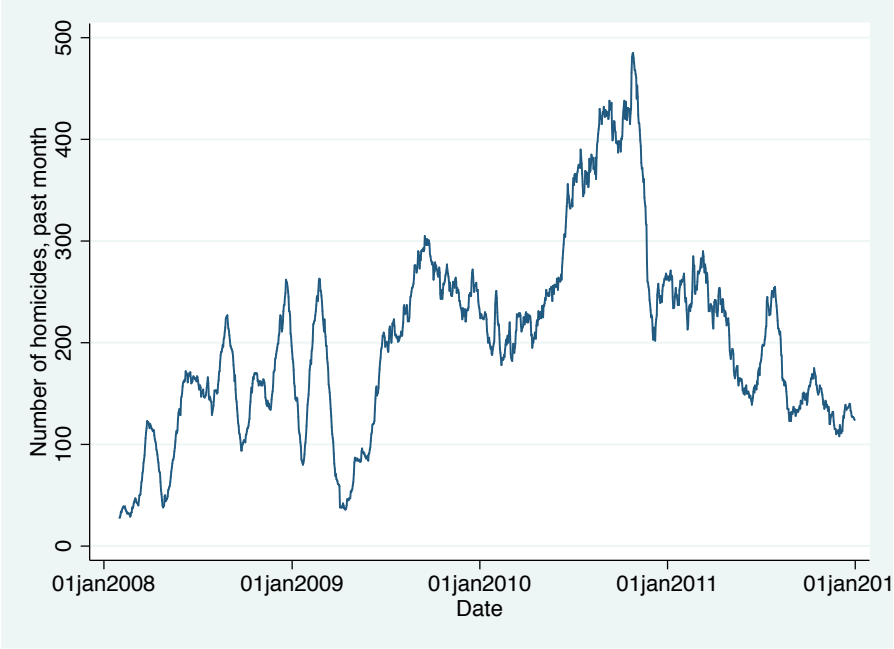


**Figure 1.1**: Homicides over Time

respondents who never have missing data (Non-attritors) and those who have at least one missing observation for inconsistent condom use (Attritors). There are no significant differences in ICU or other measures of sexual risk at baseline. Therefore, I ignore attrition for the main analysis and estimate on the unbalanced panel, consisting of 1070 observations. I consider weaker assumptions on attrition in robustness checks.

Summary statistics are presented in Table 1.1. Participants are demographically similar to the wider female sex worker population in Ciudad Juarez and elsewhere (Gertler et al., 2005; Patterson et al., 2008; Robinson and Yeh, 2011; Arunachalam and Shah, 2013): they are relatively young, with a mean age of 33, and have below primary education. Consistent with the inclusion criteria, over three quarters of participants are primarily street workers and inject more than

primary outcome due to nonresponse. There are no significant differences between attritors and non-attritors on the primary outcome or other measures of sexual risk at baseline, but attritors do exhibit a slightly lower injection frequency.

once a day. However, it is worth noting that direct participation in the drug trade, such as selling drugs or acting as a lookout, is limited.

Ciudad Juarez saw a mean 234 homicides per month during the study period; homicides peaked at 480 in a single month. More importantly for my analysis, there is substantial variation over time: the standard deviation is 87.8 homicides per month. Participants reported inconsistent condom use about half the time, and exchanged sex roughly 65 times per month, or slightly more than two times per day on average. Prices are lower than in previous work in Ciudad Juarez (de la Torre et al., 2010). This partly reflects sample selection (Strathdee et al., 2008), but there is also a downward trend in prices over the study period. The average price premium for unprotected sex, equivalent to about $4, is similar to previous estimates in percentage terms.

The surveys collected data from sex workers about the characteristics of their clients over the past moth. The vast majority of clients are non-regular (i.e., not repeat customers). Although Ciudad Juarez lies just across the border from El Paso, Texas, fewer than half of sex workers report clients from the United States. This is consistent with anecdotal reports of a steep drop in tourism due to the conflict.[16] Client drug use is extremely common: respondents reported clients who use drugs 71 percent of the time. While there is substantial overlap in demand for drugs and commercial sex in northern Mexico, this is much higher than previous estimates of client drug use (Patterson et al., 2009).

[16]**Figueroa, Lorena.** 2014. Office in El Paso to promote Juarez tourism, attractions. *El Paso Times*, February 27. http://www.elpasotimes.com/news/ci_25082301/office-el-paso-promote-tourism-attractions.

**Table 1.1**: Summary Statistics

| | Mean | S.D. | Min. | Max. | Obs. |
|---|---|---|---|---|---|
| *Demographics:* | | | | | |
| Age[1] | 33.1 | 8.67 | 18 | 58 | 299 |
| Years of education[1] | 5.77 | 2.94 | 0 | 15 | 299 |
| Years since first exchanged sex[1] | 13.1 | 8.49 | 0 | 38 | 299 |
| Mainly worked in street | 0.76 | 0.43 | 0 | 1 | 995 |
| Injected more than once a day | 0.79 | 0.41 | 0 | 1 | 1068 |
| Participated in drug trade[2] | 0.054 | 0.23 | 0 | 1 | 1054 |
| *Homicides:* | | | | | |
| Homicides during recall month (hundreds) | 2.34 | 0.88 | 0 | 5 | 1070 |
| *Outcome and Controls:* | | | | | |
| Sometimes or Never used Condom[3] | 0.51 | 0.50 | 0 | 1 | 1070 |
| Number of sex acts exchanged | 65.0 | 74.5 | 0 | 1360 | 1068 |
| Price of protected vaginal sex (pesos)[4] | 129.4 | 93.0 | 0 | 2100 | 990 |
| Price of unprotected vaginal sex (pesos)[4] | 177.1 | 96.2 | 0 | 1000 | 915 |
| Number of clients | 52.6 | 54.0 | 0 | 600 | 1069 |
| Number of regular clients | 5.67 | 12.1 | 0 | 200 | 1069 |
| Had U.S. client | 0.41 | 0.49 | 0 | 1 | 992 |
| Had client who uses drugs | 0.71 | 0.46 | 0 | 1 | 916 |

There were 1083 surveys conducted with 300 individuals; summary statistics are shown for 1070 observations with non-missing condom use data. Baseline interviews were conducted on a rolling basis from November 2008 to August 2010 with subsequent surveys 4, 8, and 12 months after baseline. Missing observations represent inapplicable questions or responses of "Don't know," "Refuse to Answer," or "Not applicable." All variables were measured over a recall period of one month, except where noted.

1. At baseline.

2. Recall period of 6 months baseline and 4 months in follow-ups.

3. Observations with no commercial client in the past month are coded as 0.

4. Response to the question "On average, how much money do you earn each time you perform (sex act)?". Prices reported in US dollars were converted to pesos at the average exchange rate during the study period, 1 USD = 12.4 MXN.

## 1.3 Empirical Strategy

Identification relies on the structure of the data, in which interviews are dispersed over time. For each individual $i$, I observe each visit $v \in \{1,2,3,4\}$. Visits take place on day $t \in \{1,2,...,1003\}$. Thus, I denote inconsistent condom use by $ICU_{iv,t}$. Let $H_t$ denote homicides during the month prior to day $t$—i.e., the period over which outcome $ICU_{iv,t}$ is reported by the sex worker. For causal identification of the average effect of homicides on the outcome of interest, I need conditional exogeneity of $H_t$. The data structure raises several possible threats to identification.

First, the data are not a simple random sample. One concern is that violence affected the targeted sampling procedure. There is no evidence for this type of effect in the data. There is no statistically or economically significant relationship between the number of visits on a given day and homicides during that day, and interviews on any given day never represent more than 4 percent of the sample (Appendix Figure 1.2 and Appendix Table 1.12).

Even if violence did not affect visit dates on average, this does not rule out selection: that participants sampled during violent periods somehow differ systematically from those sampled during periods with fewer homicides. However, any such omitted variable is likely to be time invariant. Denote the time invariant unobserved effect by $\alpha_i$. To allow for arbitrary correlation between $\alpha_i$ and the regressors, I follow the standard approach of using fixed effects.

Second, given that the safer sex intervention directly targeted the outcome of interest, treatment status could be an important omitted variable. Table 1.2 reports a regression of treatment status on $H_t$. As expected, given random assignment of treatment status, there is no significant difference in average number of homicides experienced across treatment groups. Nevertheless, I conservatively control for

**Table 1.2**: Treatment Status and Homicides

| | (1) |
|---|---|
| Safer Injection | -0.0331 |
| | (0.0762) |
| Safer Sex | -0.0627 |
| | (0.0763) |
| Safer Sex & Safer Injection | -0.0618 |
| | (0.0764) |
| Control | 2.382*** |
| | (0.0544) |
| Observations | 1070 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The dependent variable is number of homicides during recall month. This table demonstrates that treatment status is uncorrelated with the independent variable of interest.

intervention effects by including visit number fixed effects interacted with treatment group indicators.

Finally and most critically, identification comes from time series variation in homicides. Homicides are nonstationary and serially correlated, raising the specter of spurious regression. I address the risk of common trends by directly controlling for time, first with a linear trend and then more flexibly. In addition, spurious regression can be detected *ex post* by serial correlation in the residuals. Since the panel is unbalanced, I conduct a regression-based test, from Wooldridge (2002). The data are defined as a panel indexed by individual $i$ and visit number $v$. Denote the error term by $u_{iv,t}$. The procedure estimates the panel using first differences, to obtain estimated residuals $\Delta\hat{u}_{iv} = \hat{u}_{iv,t} - \hat{u}_{iv-1,\tilde{t}}$, where $\tilde{t}$ represents the date of visit $v-1$. If the error term is serially uncorrelated, the correlation of $\Delta u_{iv}$ and $\Delta u_{iv-1}$ is identically -0.5. The test regresses the estimated residuals on their lags and performs a Wald test on the null that the coefficient is -0.5, using panel-corrected

standard errors. Note that this test is robust to correlation between $\alpha_i$ and the regressors. In addition, I conduct a placebo test using homicides in future periods.

Accounting for these different threats to identification yields the following estimating equation:

$$ICU_{iv,t} = \beta_0 + \beta_1 H_t + \sum_{v=2}^{4} \lambda_v + \sum_{v=2}^{4}\sum_{j=2}^{4} \lambda_v \tau_i^j + g(t) + \alpha_i + u_{iv,t} \quad (1.1)$$

As previously defined, $ICU_{iv,t}$ denotes inconsistent condom use over the past month for individual $i$ at visit $v$, on day $t$; and $H_t$ is the number of homicides in the month prior to day $t$. The sum terms are visit fixed effects and their interactions with treatment group indicators (nine separate terms), to control for intervention effects. The $\{\lambda_v\}_{v=2}^4$ represent visit fixed effects, and the $\{\tau_i^j\}_{j=2}^4$ represent indicators equal to one if individual $i$ was in treatment group $j$. Finally, I include time controls $g(t)$. In the base specification, $g(t)$ includes month-of-year indicators to control for seasonality, and a linear trend $t$. All specifications are estimated using the linear fixed effects estimator, with robust standard errors clustered at the individual level.

## 1.4 Main Results

Table 1.3 presents the main results. Across all specifications, the contemporaneous effect of violence on inconsistent condom use (ICU) is negative, implying that more intense violence leads to a reduction in risk-taking. Column 1 shows the contemporaneous effect of homicides on ICU during the past month, after controlling for a linear time trend and seasonality (month-of-year fixed effects). Column 2 includes controls for lagged homicides. Serial correlation in homicides could generate omitted variable bias if homicides have a delayed or persistent effect

**Table 1.3**: Effect of Homicides on Unprotected Sex

| *Dependent Variable:* | Inconsistent Condom Use | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Homicides during recall month | -0.0918*** | -0.109*** | -0.133*** | -0.155** |
| | (0.0331) | (0.0338) | (0.0467) | (0.0726) |
| Homicides -1m | | 0.0161 | -0.0335 | -0.0914 |
| | | (0.0361) | (0.0565) | (0.0817) |
| Homicides -2m | | 0.0341 | 0.0114 | 0.0476 |
| | | (0.0370) | (0.0503) | (0.0841) |
| Homicides -3m | | 0.0246 | 0.0678 | 0.0121 |
| | | (0.0435) | (0.0591) | (0.0904) |
| Constant | 0.608*** | 0.558*** | 1.750*** | 0.770** |
| | (0.142) | (0.151) | (0.491) | (0.347) |
| Time trend | X | X | | |
| Month-of-Year FE | X | X | X | |
| Quarter FE | | | X | |
| Month FE | | | | X |
| Observations | 1070 | 1070 | 1070 | 1070 |
| Number of id | 300 | 300 | 300 | 300 |
| P-value: Serially correlated errors | 0.730 | 0.779 | 0.507 | 0.428 |
| P-value: Long-term effect = 0 | . | 0.538 | 0.504 | 0.329 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. The dependent variable is inconsistent condom use (ICU). All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

on the outcomes; lagged homicides control for this bias.

Columns 3 and 4 control more flexibly for time by including quarter fixed effects and month fixed effects, respectively. Column 4 omits month-of-year fixed effects, which are collinear with month fixed effects. Despite much larger standard errors due to a substantial power loss, the relationship remains statistically significant. Therefore, I adopt Column 4 as the preferred specification.

In the second row from the bottom of Table 1.3, I report $p$-values on the null hypothesis of serially uncorrelated errors, as described in Section 1.3. I am unable to reject the null hypothesis in any specification, suggesting that spurious

regression is not a concern. As an additional check, I show in Appendix Table 1.13 that homicides in future periods have no effect on inconsistent condom use.

There is no evidence for lag effects. I investigate the persistence of the main effect using joint tests. In the last row of Table 1.3, I report $p$-values for a Wald test of the hypothesis that the sum of the lagged and contemporaneous effects is equal to zero. The test fails to reject the null hypothesis across all specifications, confirming that the effect of violence on condom use with clients is immediate and temporary.

The results in Table 1.3 indicate that violence leads to a statistically and economically significant reduction in risky sexual behavior among FSW-IDUs. The standard deviation of homicides during recall month is about 88; the estimate in Column 4 implies that a one-standard-deviation increase in homicides reduces the probability of reporting inconsistent condom use by 13.6 percentage points. This is a considerable effect, representing more than one quarter of a standard deviation.

## 1.5 Robustness

This section presents robustness checks for the causal effect of homicides on inconsistent condom use with clients. First, the coroner's reports could be subject to measurement error. In Section 1.5, I show that the results are robust to using an alternate measure of conflict intensity based on newspaper reports.

Second, self-reported data could be subject to bias. It would be ideal to validate the condom use results with biological measures of incident (i.e., newly acquired) sexually transmitted infections (STIs) in the past month. The trial conducted STI testing and treatment at each visit, which allows me to measure STI infections acquired since the last visit, and I can then regress incident STI infections at visits 2, 3, and 4 on violence during the past four months. However, this is an

imperfect test. I may not have sufficient power to detect results on this measure. We should expect effects to be smaller on STIs, since every unprotected sex act does not result in an infection. At the same time, since I cannot calculate incident infections at baseline, the sample size for this analysis is smaller. Finally, summing over violence during the past four months reduces variation in the independent variable. In Appendix Section 1.9.3, I find that violence indeed does not have a detectable effect on STI incidence in the sample.

Therefore, I directly address possible sources of bias in self-reports. The primary risk is social desirability bias: after receiving an intervention that promotes safer sex, participants are more likely to overreport condom use. However, social desirability bias generated by the intervention should attenuate over time. In Section 1.5, I show that, if anything, the effect is larger in magnitude during later visits, when participants have had no intervention for roughly 8 months. Another risk is that recall or reporting bias is systematically related to violence. In other words, perhaps FSW-IDUs are not actually using condoms more consistently during violent times, but they perceive their usage to be more frequent when the conflict is more intense. The survey measures several factors that might be affected by violence and could bias perceptions, including alcohol and drug use. Results are robust to controlling for these factors. A full discussion of this analysis and results are shown in Appendix Section 1.9.3.

In the Appendix, I present additional robustness checks, including further evidence against social desirability bias (Section 1.9.3), different specifications of the dependent variable (Section 1.9.3), and different assumptions on attrition (Section 1.9.3).

**Alternate measure of conflict intensity**

The use of coroner's reports to measure conflict intensity raises two concerns related to measurement error. First, there is evidence that the coroner's office was overwhelmed by the number of bodies at the peak of the violence,[17] and began classifying more deaths as homicides.[18] The coroner's office may also have been under pressure from the federal government to report homicides in a way that suited their aims, implying that reported homicides could be endogenous to government, and possibly military, activity. Second, yearly government data indicate that from 2008 to 2011, drug-related homicides represented between 54 and 93 percent of total homicides in Ciudad Juarez (Office of the Mexico Attorney-General, 2006). Thus, there is systematic measurement error in the data, and its variance changes over time. Although all estimates are robust to heteroskedasticity, to understand the impacts of perceived changes in mortality risk due to the conflict, it would be preferable to focus directly on drug-related homicides.

Therefore, I present results using an alternate measure of conflict intensity from independent newspaper reports. These data, compiled by the Trans Border Institute (TBI), are based on a tally of drug-related homicides kept by the national newspaper *Reforma*. Three major newspapers in Mexico report weekly drug-related homicides; the *Reforma* tallies are the most conservative. Drug-related homicides documented in newspapers may be a better measure of exposure to violence in the study population that official reports. However, the newspaper reports are aggregated at the state level rather than the municipal level,[19] and homicides

[17] **Kolb, Joseph.** 2012. "Juarez Medical Examiner Catches His Breath, Enjoys Lull in Violence." *Fox News Latino,* October 23. http://latino.foxnews.com/latino/news/2012/10/23/juarez-medical-examiner-catches-his-breath-enjoys-lull-in-violence/.

[18] In the data, homicides are negatively correlated with non-homicide mortality during the study period.

[19] Between 2008 to 2011, drug-related homicides in Ciudad Juarez represented between 53 and 67 percent of total drug-related homicides in Chihuahua (Office of the Mexico Attorney-General,

are observed at the weekly level rather than the daily level,[20] generating greater measurement error.

Table 1.4 replicates the main results (Table 1.3) using this alternate measure of homicides. The effect remains negative across specifications, and it is statistically significant at the 10 percent level in all but one specification (Column 3). The point estimates are smaller in magnitude than in Table 1.3, but this is consistent with greater measurement error due to reporting at the state and week level. The coefficient in the preferred specification (Column 4) implies that a one-standard-deviation increase in drug-related homicides reduces ICU by 10.9 percentage points, relative to the main estimate of 13.6 percentage points. The results support the conclusion that more intense conflict generates a reduction in risky sexual behavior.

**Social desirability bias**

This robustness check specifically addresses desirability bias generated by the intervention, in which both control and treatment individuals received information about the importance of condom use. Since individual fixed effects already control for a given individual's propensity to misreport risky sexual behavior, time varying desirability bias is the concern. The intervention, delivered between baseline and the first follow-up, is the most obvious source of variation over time in desirability bias.

No further intervention was delivered after the baseline visit. Therefore, if the result is generated by intervention effects, it should attenuate over time. Table 1.5 shows the differential effect of homicides during later visits (visits 3 and 4),

2006)

[20] "Homicides during the recall period" is calculated as the sum of homicides in the 4 weeks prior to the week of the interview. I also use lagged homicides in some specifications. The first lag prior to recall corresponds to the number of homicides from 5 weeks to 8 weeks prior to the interview, the second lag to homicides from weeks 9 to 12 prior to the interview, etc.

**Table 1.4**: Alternate Measure of Conflict Intensity

| *Dependent Variable:* | Inconsistent Condom Use | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Homicides during recall month (TBI, hundreds) | -0.0655** | -0.0688* | -0.0574 | -0.177** |
| | (0.0317) | (0.0358) | (0.0534) | (0.0895) |
| Homicides -1m (TBI, hundreds) | | -0.0219 | -0.0181 | -0.148 |
| | | (0.0387) | (0.0546) | (0.0989) |
| Homicides -2m (TBI, hundreds) | | -0.00168 | -0.0161 | 0.0479 |
| | | (0.0381) | (0.0541) | (0.0988) |
| Homicides -3m (TBI, hundreds) | | 0.0343 | 0.0594 | 0.0634 |
| | | (0.0407) | (0.0593) | (0.0968) |
| Constant | 0.582*** | 0.567*** | 0.316 | 0.764 |
| | (0.143) | (0.145) | (0.295) | (0.475) |
| Time trend | X | X | | |
| Month-of-Year FE | X | X | X | |
| Quarter FE | | | X | |
| Month FE | | | | X |
| Observations | 1070 | 1070 | 1070 | 1070 |
| Number of id | 300 | 300 | 300 | 300 |
| P-value: Long-term effect = 0 | . | 0.229 | 0.814 | 0.427 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

when respondents have received no intervention for at least 8 months. While I have limited power to detect this differential effect, the point estimate is negative across specifications, and statistically significant in one specification. The effect is either constant or getting stronger as intervention effects fade. This is inconsistent with the idea that results are generated by intervention effects.

## 1.6 Alternate Explanations

The preceding sections demonstrate that more intense conflict leads to a reduction in unprotected sex between sex workers and their clients, a measure of risk-taking. This result is inconsistent with models that abstract away from risk attitudes, including the human capital model and the income smoothing model, and consistent with a model of risk vulnerability.

In this section, I review alternate mechanisms that could generate the observed reductions in risky sex during violent periods. I consider (i) changes on the demand side, (ii) compositional changes on the supply side, and (iii) concurrent changes in the drug market. Then I explore evidence for these effects in the data. While I cannot rule out every possible mechanism, I find no evidence that any of these alternate explanations is driving the result, suggesting that risk vulnerability is an important part of the story.

### 1.6.1 Demand-side Effects

I begin by considering the effect of violence on the overall level of demand. Conflict may represent an income shock for clients, which could reduce demand for paid sex. This would likely generate reductions in both protected and unprotected sex exchanged, but it could generate a reduction in risky sex if there is substitution

**Table 1.5**: Effect over Time

| *Dependent Variable:* | Inconsistent Condom Use | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Homicides during recall | -0.0655* | -0.0673* | -0.105* | -0.112 |
| | (0.0390) | (0.0385) | (0.0571) | (0.0791) |
| Homicides during recall X Later Visit | -0.0549 | -0.107** | -0.0505 | -0.0834 |
| | (0.0414) | (0.0479) | (0.0665) | (0.0777) |
| Homicides -1m | | 0.0245 | -0.0259 | -0.0959 |
| | | (0.0360) | (0.0566) | (0.0819) |
| Homicides -2m | | 0.0478 | 0.0109 | 0.0361 |
| | | (0.0378) | (0.0505) | (0.0855) |
| Homicides -3m | | 0.0646 | 0.0695 | 0.0101 |
| | | (0.0467) | (0.0590) | (0.0905) |
| Constant | 0.538*** | 0.364** | 1.352*** | 0.745** |
| | (0.157) | (0.184) | (0.495) | (0.347) |
| Time trend | X | X | | |
| Month-of-Year FE | X | X | X | |
| Quarter FE | | | X | |
| Month FE | | | | X |
| Observations | 1070 | 1070 | 1070 | 1070 |
| Number of id | 300 | 300 | 300 | 300 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

away from unprotected sex toward protected sex. Table 1.6 quantifies the effect of conflict on overall demand. In Column 1, I present results from regressing the total number of clients on homicides, lagged homicides and controls. Neither contemporaneous nor lagged homicides have a statistically significant effect on the number of clients. Since homicides are serially correlated, in the last row, I also report $p$-values for the joint hypothesis that all coefficients on homicides are zero. There is no evidence for a statistically significant effect. Measurement error in the dependent variable could generate large standard errors, but the point estimate is small: it implies that a one-standard-deviation increase in homicides leads to 2.5 more clients on average, which is a standardized effect size of 0.05.

I do not observe the entire market for sex work, so demand could fall without changing the number of clients seen by study participants. However, this would affect prices. Therefore, Columns 2-4 of Table 1.6 investigate the impact of violence on the average price of protected vaginal sex, unprotected vaginal sex, and the premium for unprotected vaginal sex (the difference of the first two variables) during the past month[21]. Because there is a downward trend in prices over the study period, all regressions include month fixed effects. There is no evidence for contemporaneous effects of homicides on prices for any of these outcomes. None of the coefficients is statistically or economically significant. The largest point estimate, in Column 4, indicates that a one-standard-deviation increase in homicides increases the premium for unprotected sex by 7.9 pesos (approximately 64 cents). This is a standardized effect size of 0.07.

There is some evidence for lag effects. Table 1.6, Column 3 suggests that homicides may decrease the price of unprotected sex one month later, and Column

[21]The survey collected also data on the prices of protected and unprotected anal sex. However, 591 observations report that they did not exchange anal sex in the past month, so prices are observed for less than half the sample. This would make any analysis difficult to interpret.

**Table 1.6**: Overall Demand

| | (1) Num. Clts | (2) Prot. Price | (3) Unprot. Price | (4) Prem. |
|---|---|---|---|---|
| Homicides during recall month | 3.140 | -5.009 | 0.0304 | 9.017 |
| | (7.785) | (9.667) | (14.27) | (13.43) |
| Homicides -1m | -0.219 | -16.27 | -40.88** | -30.49* |
| | (8.640) | (12.47) | (16.73) | (16.30) |
| Homicides -2m | 6.967 | -13.44 | 1.177 | 10.56 |
| | (7.386) | (14.06) | (16.69) | (14.23) |
| Homicides -3m | -11.13 | -15.11 | 25.19 | 35.85* |
| | (8.504) | (15.53) | (17.32) | (20.97) |
| Constant | 94.62*** | 257.4*** | 286.0*** | 66.58 |
| | (30.10) | (56.84) | (61.84) | (61.91) |
| Observations | 1070 | 990 | 915 | 910 |
| Number of id | 300 | 300 | 300 | 300 |
| P-value: Serially correlated errors | 0.00672 | 0.520 | 0.000337 | 0.375 |
| P-value: Joint F test | 0.301 | 0.484 | 0.160 | 0.124 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications include individual fixed effects, month fixed effects, visit dummies, and their interactions with treatment group indicators.

4 indicates a corresponding effect on the premium. However, in the second-to-last row of Table 1.6, I show results for the test of serially correlated errors described in Section 1.3. The errors in Column 3 are serially correlated, so this result should be seen as suggestive. Taken together, the results indicate that changes in overall demand cannot explain the main result.

Even if the level of overall demand is constant, conflict could affect the composition of the client pool in ways that generate a reduction in risky sex. First, the drug war induced major population movements in Ciudad Juarez. More than 200,000 people, out of a population of 1.3 million, are estimated to have left the city during the violence (Instituto Nacional de Estadística y Geografía, 2010; Internal Displacement Monitoring Centre, 2012). Large population movements disrupt sexual networks, including the regular business of FSWs. Previous studies, including qualitative work with FSWs in Ciudad Juarez, have found that sex workers are more likely to accept unprotected sex with regular clients, due to greater trust and intimacy (Vuylsteke and Jana, 2001; Robertson et al., 2013). Thus, the disappearance of regular clients and a shift toward new clients could result in increased condom use.

I consider evidence for a shift away from regular clients in Column 1 of Table 1.7. In Panel A, I study the effect of conflict on the number of regular clients, measured over the past month. Controlling for the total number of clients, the effect of contemporaneous homicides on the number of regular clients is negative and statistically significant at the 10% level. The point estimate is of moderate magnitude: a one-standard-deviation increase in homicides reduces the number of regular clients by 2.2, off a mean of 5.6. Thus, the direction and magnitude of the effect are consistent with a compositional shift that could reduce risky behavior. In Panel B, Column 1, I explore the impact of this compositional shift on the main

**Table 1.7**: Client Composition

| *Panel A: Effect of Conflict on Client Types* | | |
|---|---|---|
| | (1) Reg. Clts | (2) U.S. Clts |
| Homicides during recall month | -2.499* | -0.0289 |
| | (1.475) | (0.095) |
| Homicides -1m | -1.931 | -0.104 |
| | (1.683) | (0.092) |
| Homicides -2m | 0.769 | -0.0730 |
| | (1.511) | (0.111) |
| Homicides -3m | 3.570* | -0.0303 |
| | (1.993) | (0.116) |
| Number of clients | 0.0716** | 0.00102** |
| | (0.030) | (0.000) |
| Constant | -5.609 | 1.159** |
| | (7.821) | (0.496) |
| P-value: Joint F test | 0.171 | 0.853 |

| *Panel B: Role of Composition in Main Results* | | | | |
|---|---|---|---|---|
| | (1) ICU | (2) ICU | (3) ICU | (4) ICU |
| Homicides during recall month | -0.147** | -0.155** | -0.141*** | -0.139*** |
| | (0.073) | (0.073) | (0.047) | (0.048) |
| Homicides -1m | -0.0872 | -0.0906 | -0.0451 | -0.0486 |
| | (0.080) | (0.081) | (0.058) | (0.059) |
| Homicides -2m | 0.0345 | 0.0398 | 0.0136 | 0.0162 |
| | (0.083) | (0.084) | (0.051) | (0.050) |
| Homicides -3m | 0.00306 | 0.0170 | 0.0730 | 0.0324 |
| | (0.090) | (0.091) | (0.060) | (0.061) |
| Number of clients | 0.000264 | 0.000488 | 0.000557* | |
| | (0.000) | (0.000) | (0.000) | |
| Number of regular clients | 0.00362** | | | |
| | (0.001) | | | |
| U.S. clients (ordinal scale) | | 0.0349 | | |
| | | (0.025) | | |
| Border volume (thousands) | | | 0.000243 | |
| | | | (0.000) | |
| March 2009 | | | | 0.260 |
| | | | | (0.217) |
| January 2010 | | | | 0.140 |
| | | | | (0.164) |
| Constant | 0.761** | 0.700** | 1.215 | 1.911*** |
| | (0.351) | (0.353) | (0.810) | (0.493) |
| Month-of-Year FE | | | X | X |
| Quarter FE | | | X | X |
| Month FE | X | X | | |
| Observations | 1067 | 1067 | 1067 | 1070 |
| Number of id | 300 | 300 | 300 | 300 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications include individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

result. I estimate Equation 1.1 with controls for the total number of clients and the number of regular clients. As expected, the number of regular clients is positively correlated with risky sexual behavior.

If a compositional shift toward nonregular clients is driving the main result, we should see the coefficient on contemporaneous homicides attenuate toward zero when we control for regular clients. Relative to the main result (Table 1.3, Column 4), the coefficient falls slightly in magnitude, but the change is not statistically significant. More importantly, contemporaneous homicides still have a significant, independent impact on inconsistent condom use, indicating that this compositional shift explains, at most, only a small part of the effect.

A second alternate explanation relates to tourist versus local clients. Ciudad Juarez lies just across the border from El Paso, Texas, and in a previous study conducted prior to the conflict, nearly 60 percent of FSWs reported clients from United States (Patterson et al., 2008). However, anecdotal reports suggest a steep drop in tourism and substantial fortification of border security due to the violence (Felbab-Brown, 2011).[22] Thus, more intense conflict could shift the composition of the client pool away from U.S. clients and toward Mexican clients. In previous work in Tijuana and Ciudad Juarez, FSWs who had clients from the U.S. exchanged more unprotected sex (Strathdee et al., 2008), suggesting that a shift toward nonregular Mexican clients could explain reductions in risky sexual behavior.

The surveys provide an ordinal measure of the proportion of U.S. clients.[23]

[22] **Figueroa, Lorena.** 2014. "Office in El Paso to promote Juarez tourism, attractions." *El Paso Times,* February 27. http://www.elpasotimes.com/news/ci_25082301/office-el-paso-promote-tourism-attractions.

**Rice, Andrew.** 2011. "Life on the Line Between El Paso and Juarez." *The New York Times Magazine,* July 31. http://www.nytimes.com/2011/07/31/magazine/life-on-the-line-between-el-paso-and-juarez.html.

[23] This variable is the response to the question "In the past month, about how many of your clients lived in the U.S.?" Five ordered responses were possible, ranging from "None" to "All." The response is coded as "None" for those who had no clients in the past month.

Table 1.7, Column 2, reports results related to this variable. In Panel A, I show results from a linear regression of the share of U.S. clients on homicides, lagged homicides and controls. Neither contemporaneous nor lagged homicides have a statistically significant effect, but the direction of the point estimate is consistent with a risk-reducing compositional shift.[24]

In Panel B, Column 2, I investigate whether a change in U.S. v. Mexican client composition explains the relationship between conflict and risky behavior, by controlling for the share of U.S. clients. The measure of U.S. client share is not associated with risky sex, suggesting that this compositional shift is less relevant for the study population. Moreover, controlling for U.S. clients has no statistical or meaningful impact on the main result.

One concern is that the ordinal measure of the share of U.S. clients does not adequately capture true variation in client composition. Therefore, I supplement the analysis with northbound border crossing volumes at El Paso, from the Bureau of Transportation Statistics. Border crossing volumes are a measure of cross-border activity, including both tourism and migration. Since the data are collected only at the monthly level, it is not possible to estimate the preferred specification with month fixed effects. Instead, I show the next most conservative specification in Panel B, Column 3. Results are similar for this measure of tourism: cross-border activity is not associated with risky sexual activity, and controlling for border crossing volumes has no impact on the main result.

Finally, the increasing violence led to large military and police mobilizations. Over 10,000 members of the military and federal police were deployed to Ciudad Juarez between 2008 and 2010, including 5,000 troops and 1,000 federal police

[24]Since the measure is ordinal, the magnitude of the point estimate is not interpretable.

in March 2009 and 2,000 federal police in January 2010.[25,26] There is a strong historical association between the military and consumption of commercial sex (Gaydos et al., 2000; Lajous et al., 2005), but the impact of these clients on the prevalence of *risky* commercial sex is not clear. On one hand, military and police clients are better educated than the average client, which may be associated with a preference for condom use (Lagarde et al., 2001). On the other hand, with a steady income, the population has greater ability to pay for unprotected sex.

Unfortunately, data on military and police clients are not available. However, if military and police clients have an impact on risky commercial sex exchanged, we would expect to observe a shock to ICU in months when new forces were deployed. Column 4 of Table 1.7 shows the change in unprotected sex in March 2009 and January 2010, relative to the quarterly average. There is no significant change in unprotected sex in either month.

As a whole, there is no evidence in Table 1.7 that variation in conflict intensity affects overall demand or the composition of the client pool. This result may seem surprising given reports of large population movements and economics impacts as a result of the violence. However, by including month fixed effects, my estimates control for medium- and long-term demand effects. Identification relies on high-frequency variation in conflict intensity. For demand-side effects to drive the observed result, we would require that clients heterogeneously move in and out of the market in response to short-term variation in conflict intensity. In general, it is more likely that clients respond discretely to violence above some threshold (e.g., by leaving the city or canceling planned visits).

[25]More than 2,000 federal troops and police were already in the city at the start of the study period. They were deployed in March 2008.

[26]**Ellingwood, Ken.** 2009. "Mexico sending more forces to Ciudad Juarez." *Los Angeles Times*, March 3. http://www.latimes.com/world/la-fg-mexico-juarez-police3-2009mar03-story.html.

"SSP enviara 2 mil federales mas a Ciudad Juarez." 2010. *El Universal*, January 13. http://www.eluniversal.com.mx/notas/651437.html.

### 1.6.2 Supply-side Effects

A reduction in risky sex could also result from compositional changes in the sex worker population. If sex workers who exchange more risky sex are also more likely to exit the commercial sex market during violent periods, we could observe a reduction in risky sex that is unrelated to risk vulnerability and risk preferences. For example, perhaps these "riskier" FSWs are more likely to operate in higher conflict areas, have a higher expected cost of practicing sex work during violent periods, and therefore exit the market.

For supply-side compositional effects to generate the result, we require that (i) those who exit the market are different from those who do not; and (ii) market exit is correlated with violence. Denote "safe" types as those *would have* practiced consistent condom use if they had been in the market, and "risky" types as those who would have practiced inconsistent condom use. Note that since our primary dependent variable codes both consistent condom use and market exit the same way, market exit by safe types does not affect the results.

I begin by comparing those who ever exited the market during the study period to the rest of the population at baseline. I define market exit as reporting "did not exchange sex" or having 0 clients in the past month. In Column 1 of Table 1.8, I show results from regressing "ever exited" on inconsistent condom use at baseline. There is no statistical difference between those who exit and those who do not.

In Column 2, I show that contemporaneous homicides *reduce* the probability of market exit.[27] It is difficult to see how this compositional change could generate

[27]This is consistent with the income smoothing model if other income-generating activities are disrupted by the violence. However, there is marginal evidence of serial correlation in the error term (last row of Table 1.8), suggesting that the result could be spurious. A detailed discussion of this result is beyond the scope of this paper.

**Table 1.8**: Sex Worker Composition

| | (1) ICU | (2) Exit | (3) ICU(Exit=1) |
|---|---|---|---|
| Ever Exited | 0.0209 | | |
| | (0.0623) | | |
| Homicides during recall month | | -0.0995** | -0.252*** |
| | | (0.0429) | (0.0744) |
| Homicides -1m | | -0.0644 | -0.164* |
| | | (0.0498) | (0.0866) |
| Homicides -2m | | -0.0125 | 0.0361 |
| | | (0.0439) | (0.0882) |
| Homicides -3m | | 0.0678 | 0.0834 |
| | | (0.0525) | (0.0944) |
| Constant | 0.778*** | 0.283 | 1.069*** |
| | (0.0919) | (0.186) | (0.365) |
| Time trend | X | | |
| Month-of-Year FE | X | | |
| Month FE | | X | X |
| Observations | 299 | 1070 | 1070 |
| Number of id | | 300 | 300 |
| P-value: Serially correlated errors | . | 0.0788 | . |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column 1: Robust standard errors in parentheses, Columns 2-3 : Standard errors in parentheses, clustered at sex worker level. All specifications include individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in Columns 2-3.

a reduction in risky sex. If risky types are less likely to exit during violent periods, this implies the opposite result of what we find: it implies that conflict increases risky sexual behavior.

Column 3 of Table 1.8 illustrates this effect. Suppose all those who exited are risky types (i.e. their counterfactual outcome, had they not exited, is ICU). I can simulate the effect without exit by coding market exit as 1 (risky behavior). Column 3 shows that in the simulated scenario, the effect of conflict on inconsistent condom use is larger in magnitude (-0.252 v. -0.188). This implies that if supply-side compositional changes have any effect, they bias against finding a reduction in risky sexual behavior due to conflict.

### 1.6.3 Drug Market

Ciudad Juarez is a major drug trafficking route, and as a result there is a flourishing local market in drugs that do not successfully cross the border into the United States. There is very little existing evidence documenting the norms of the retail drug market in Ciudad Juarez or how the market responded to the violence. In theory, however, violence has ambiguous effects on the supply of drugs in Ciudad Juarez. Fighting is likely to disrupt cartel operations, diverting resources away from the core drug business. On one hand, this could increase the cost of selling drugs (locally or abroad), reducing supply. On the other hand, heightened border security could impede drug trafficking across the border, leading to an expansion of supply in Mexico.

Changes in drug availability could have a direct impact on substance use by both sex workers and clients. While there is no causal evidence linking substance use to unprotected sex, previous work has documented a strong association between the two behaviors (Strathdee et al., 2008). Thus, a reduction in drug use (e.g.,

due to reduced drug supply) could explain the observed reduction in risky sexual behavior.[28]

I investigate both client side and sex worker drug use using the survey data. The survey measures respondent injection frequency on an ordinal scale from "Never" to "More than once a day" and drug use right before or during sex with clients on an ordinal scale from "Never" to "Always". The survey also includes an indicator of having a client who uses drugs. Several observations are missing for both sex worker drug use and client drug use, due to nonresponse; the analyses in this section are conducted on the subsample excluding those observations.

Table 1.9 shows results from controlling for drug use. Column 1 verifies the main result on the subsample with no missing data for FSW drug use. Column 2 controls for FSW injection frequency, and Column 3 for drug use just before or during sex with clients. As expected, more frequent drug use, both in general and during sex with clients, is strongly and significantly associated with ICU. However, the coefficient on contemporaneous homicides is robust. The point estimate slightly increases in magnitude relative to the main specification (although the difference is not significant). Column 4 verifies the main result on the subsample with data for client drug use, and Column 5 controls for client drug use. Again, the main result is statistically unchanged, with the point estimate increasing slightly. There is no evidence that changes in client or sex worker drug use are driving the effect.

## 1.7 Risk Vulnerability among Sex Workers or Clients?

The previous section suggests that the observed reduction in risky sex exchanged is not due to income effects or compositional effects, but rather represents

[28]Of course, conflict could also increase drug use. For example FSWs and clients might use drugs to cope with the violent situation. However, increased drug use as a result of conflict would most likely increase risky sexual behavior, contrary to my finding.

**Table 1.9**: Drug Use by FSWs and Clients

| *Dependent Variable:* | Inconsistent Condom Use | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Homicides during recall month | -0.160** | -0.183** | -0.175** | -0.141* | -0.175** |
| | (0.0743) | (0.0730) | (0.0742) | (0.0761) | (0.0768) |
| Homicides -1m | -0.101 | -0.107 | -0.0976 | -0.0585 | -0.0654 |
| | (0.0858) | (0.0844) | (0.0847) | (0.0847) | (0.0834) |
| Homicides -2m | 0.0769 | 0.109 | 0.0694 | 0.00249 | 0.0213 |
| | (0.0892) | (0.0874) | (0.0872) | (0.0893) | (0.0874) |
| Homicides -3m | 0.00576 | -0.00516 | 0.0346 | -0.0207 | 0.00733 |
| | (0.0938) | (0.0938) | (0.0939) | (0.0972) | (0.0995) |
| Injection frequency | | 0.0499*** | | | |
| | | (0.00823) | | | |
| Drugs before or during sex | | | 0.0953*** | | |
| | | | (0.0170) | | |
| Had client who uses drugs | | | | | 0.180*** |
| | | | | | (0.0410) |
| Constant | 0.812** | 0.652* | 0.565 | 0.903** | 0.737* |
| | (0.369) | (0.377) | (0.368) | (0.371) | (0.378) |
| Observations | 1022 | 1022 | 1022 | 991 | 991 |
| Number of id | 300 | 300 | 300 | 298 | 298 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

a change in a given individual's decision to bear health risk. However, a sex transaction is an outcome of a bargaining game with two players. Thus, the results may be driven by either FSW-IDUs or clients. This section explores this question.

As described in Section 1.9.3, the survey collected data on attempts to negotiate condom use.[29] It is not necessarily clear how to interpret this measure. If client demand for unprotected sex is fixed, attempts to negotiate condom use represent sex workers' willingness to exchange unprotected sex. However, sex workers do not have to negotiate condom use if it is proposed by the client. Therefore, if the sex workers' willingness to exchange unprotected sex is fixed, but client demand for unprotected sex is changing, the measure will also shift. The measure then captures the sum of these two effects. Consistent with this, attempts to negotiate condom use are associated with an *increase* in risky sex exchanged in the sample, even after controlling for the total number of clients (Table 1.15). Thus, it appears that the effect of changing client demand dominates in general.

We can use attempts to negotiate condom use to help distinguish between client and sex worker risk vulnerability as the primary driver of the observed effect. If violence increases attempts to negotiate condom use, this suggests that sex workers' willingness to exchange unprotected sex is falling. However, if violence decreases attempts to negotiate condom use, this implies that client demand for unprotected sex is falling (since we know that the amount of unprotected sex exchanged also decreases). In Column 1 of Table 1.10, I find that the coefficient on homicides is negative and statistically significant, consistent with the latter effect. The results suggest that client risk vulnerability plays an important role in the observed reduction in risky sex exchanged.

I collect further evidence for this by examining other risky behaviors. If the

[29]This variable is the response to the question "In the past month, what was the total number of times you tried to talk regular/nonregular male clients into using a condom for sex?"

result is generated by risk vulnerability, we would expect decision makers to reduce their risk on several margins. That is, if the result is driven by sex workers, we would expect them to reduce many types of risky behaviors.[30] We have already seen that market exit, which is risk reducing, is less likely during violent periods (Table 1.8), contrary to the prediction of risk vulnerability. However, it is possible that it is too costly to reduce risk on this margin. The surveys provide data on a number of risky behaviors related to drug use, including frequency of injection drug use, needle sharing, use of sterile needles, cleaning of needles, and sharing of other injection equipment. As previously mentioned in Section 1.9.3, the survey also measured drug and alcohol right before or during sex transactions. In total, the survey contains 12 measures related to risky substance use in the past month.

To assess the impact of violence on risky substance use, I construct a summary index of risky substance use, following Anderson (2008a). This method takes an average of the (standardized) outcomes, weighted by their inverse pairwise covariance to account for correlation among the outcomes. It also ignores missing data, allowing me to use almost all of the main sample for this analysis. Columns 2-4 of Table 1.10 report the effect of violence the summary index of risky drug use, with varying controls for time. The summary index is coded so that higher numbers are riskier. Homicides do not have a statistically significant effect on risky drug use in any specification, independently or jointly. The results lend further support to the notion that clients drive the reduction in risky sex exchanged.

## 1.8 Discussion and Conclusion

This paper demonstrates that more intense conflict reduces risky sexual behavior among among FSW-IDUs and their clients in Ciudad Juarez. There is

[30] Unfortunately, I do not have data for other risky behaviors practiced by clients.

**Table 1.10**: Risk Vulnerability among Sex Workers v. Clients

| | (1) Negot. Cond. | (2) Drg Use | (3) Drg Use | (4) Drg Use |
|---|---|---|---|---|
| Homicides during recall month | -10.34*** | 0.0196 | 0.0207 | 0.0606 |
| | (3.922) | (0.0195) | (0.0225) | (0.0451) |
| Homicides -1m | -1.536 | | 0.0254 | 0.0634 |
| | (3.712) | | (0.0240) | (0.0528) |
| Homicides -2m | 6.784* | | -0.0234 | 0.00874 |
| | (3.608) | | (0.0243) | (0.0599) |
| Homicides -3m | -3.569 | | -0.00851 | 0.00441 |
| | (5.411) | | (0.0249) | (0.0571) |
| Constant | 51.72** | -0.0113 | -0.00185 | -0.357* |
| | (20.50) | (0.0899) | (0.0921) | (0.216) |
| Observations | 1064 | 1068 | 1068 | 1068 |
| Number of id | 300 | 300 | 300 | 300 |
| P-value: Long-term effect = 0 | 0.307 | . | 0.642 | 0.215 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications include individual fixed effects, month fixed effects, visit dummies, and their interactions with treatment group indicators. *Negot. Cond.* is the number of times the sex worker attempted to negotiate condom use. *Drg Use* is a summary index of risky substance use.

no evidence that this effect is generated by compositional shifts in the commercial sex market or changes in drug use by clients or sex workers. While conflict can affect several aspects of this decision, acting as both an income shock and a shock to life expectancy, in standard models these effects generate an increase in sexual risk-taking, contrary to my results.

The results are most consistent with risk vulnerability: conflict increases background mortality risk, which induces more risk averse choices when facing a series of independent, health-related lotteries in the form of sex transactions. There is suggestive evidence that risk vulnerability among clients largely drives the result. My findings suggest that risk attitudes play an important role in individuals' response to changes in mortality risk, providing empirical support for the argument that human capital models should account for uncertainty (Grossman, 2000).

The paper provides new evidence on the effect of conflict on risk attitudes. Since 1960, the majority of the world's countries have experienced violent conflict (Blattman and Miguel, 2010). Understanding individual responses to conflict is therefore of great importance. Callen et al. (2014) show that exposure to conflict induces more risk averse choices over monetary lotteries. My paper suggests that this result extends to choices over other types of risk. While risk averse choices generally have positive externalities in the health domain, they may have adverse consequences in other contexts, especially in incomplete insurance environments (e.g., reducing entrepreneurship (Kihlstrom and Laffont, 1979) or risky investments (Rosenzweig and Binswanger, 1993)).

The paper also has implications for HIV prevention in conflict-affected areas. Over 75 percent of respondents reported access to free condoms during the study period. If more intense conflict increases condom use among FSWs and their clients, the returns to condom provision are higher during conflict periods. Sustaining

HIV prevention services during times of conflict, especially those that empower individuals to make healthier choices, is therefore critical.

Finally, this paper suggests that risk attitudes respond temporarily to variation in conflict intensity, consistent with risk vulnerability. If post-conflict environments are less risky than conflict environments, sexual risk-taking may revert to prior levels or even increase after the conflict. Understanding the behavioral responses of female sex workers to post-conflict conditions would be a fruitful area for further research.

## Acknowledgment

Chapter 1, in full, is currently being prepared for submission for publication of the material. Manian, Shanthi. The dissertation author is the sole author of this material.

## 1.9 Appendix

### 1.9.1 Attrition and Missing Data

Table 2.13 compares respondents who have no missing data (Non-attritors) and those who have at least one missing observation for inconsistent condom use (Attritors). There are no significant differences in ICU or other measures of sexual risk at baseline.

### 1.9.2 Number of Visits and Violence

Figure 1.2 shows the distribution of visits by day. Interviews on any given day never represent more than 4 percent of the sample. Table 1.12 reports the

**Table 1.11**: Baseline Means by Attrition Status

| | Non-attrit. | Attrit. | Diff. | p-val. |
|---|---|---|---|---|
| Sometimes or Never used Condom | 0.738 | 0.764 | -0.0259 | 0.635 |
| Unprotected fraction of total sex acts | 0.543 | 0.528 | 0.0144 | 0.706 |
| Number of sex acts exchanged | 93.78 | 98.48 | -4.703 | 0.777 |
| Number of clients | 75.42 | 72.47 | 2.952 | 0.721 |
| Number of regular clients | 7.276 | 7.522 | -0.246 | 0.923 |
| Had U.S. client | 0.462 | 0.489 | -0.0274 | 0.666 |
| Had client who uses drugs | 0.801 | 0.838 | -0.0364 | 0.477 |
| Injection frequency | 6.738 | 6.500 | 0.238 | 0.095 |
| Observations | 210 | 90 | | |

**Table 1.12**: Correlation between Violence and Number of Visits

| | (1)<br>Visits |
|---|---|
| Homicides (hundreds) | -1.633 |
| | (2.051) |
| Constant | 3.213*** |
| | (0.196) |
| Observations | 351 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Robust standard errors in parentheses.

results of a regression of number of visits on a given day on homicides that day. The coefficient is statistically indistinguishable from zero, indicating that visit dates were uncorrelated with violence.

### 1.9.3 Additional Robustness Checks

#### Placebo test

Table 1.13 shows the effect of future homicides on violence. "Homicides +1m" refers to homicides 1 to 30 days after the recall period, "Homicides +2m" 31 to 60 days after the recall period, etc. Future homicides have no relationship with inconsistent condom use in the preferred specification.

**Table 1.13**: Placebo Test: Effect of Future Homicides

| *Dependent Variable:* | Inconsistent Condom Use | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Homicides during recall month | -0.156** | -0.151** | -0.136* |
| | (0.0744) | (0.0749) | (0.0778) |
| Homicides -1m | -0.0908 | -0.0932 | -0.0715 |
| | (0.0830) | (0.0832) | (0.0853) |
| Homicides -2m | 0.0475 | 0.0390 | 0.0746 |
| | (0.0841) | (0.0836) | (0.0863) |
| Homicides -3m | 0.0119 | 0.0480 | 0.0653 |
| | (0.0904) | (0.102) | (0.104) |
| Homicides +1m (hundreds) | -0.00451 | 0.0275 | 0.0216 |
| | (0.0888) | (0.0932) | (0.0940) |
| Homicides +2m (hundreds) | | 0.0923 | 0.138 |
| | | (0.0885) | (0.0978) |
| Homicides +3m (hundreds) | | | 0.0909 |
| | | | (0.0823) |
| Constant | 0.783* | 0.539 | 0.159 |
| | (0.408) | (0.503) | (0.609) |
| Observations | 1070 | 1070 | 1070 |
| Number of id | 300 | 300 | 300 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

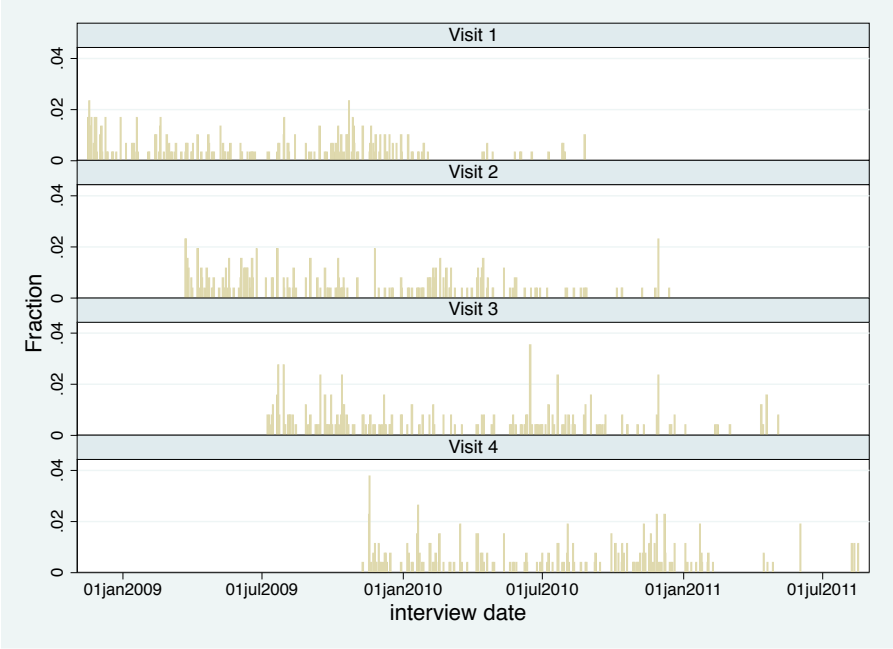


**Figure 1.2**: Distribution of Visits by Day

### Sexually transmitted infection incidence

Table 1.14 shows the effect of homicides during the past 120 days on incident sexually transmitted infections at visits 2, 3, and 4. The dependent variable is an indicator for having acquired HIV, syphilis, chlamydia, gonorrhea, or trichomonas since the last visit. Regardless of control for time, homicides have no detectable effect on combined HIV/STI incidence.

### Recall bias

In this section, I address recall bias by controlling for three factors that could affect perceived frequency of condom use: the total number of commercial transactions, the number of clients, and attempts to negotiate condom use. If the total number of sex acts decreases during violent periods, respondents may incorrectly perceive that they are having unprotected sex less often because the

**Table 1.14**: Effect of Violence on Combined HIV/STI incidence

| *Dependent Variable:* | Combined HIV/STI incidence | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Homicides, past 4 months (hundreds) | -0.00854 | 0.0287 | 0.00713 |
| | (0.0178) | (0.0437) | (0.0598) |
| Constant | -0.114 | -0.318 | -0.0764 |
| | (0.222) | (0.411) | (0.447) |
| Time trend | X | | |
| Month-of-Year FE | X | X | |
| Quarter FE | | X | |
| Month FE | | | X |
| Observations | 783 | 783 | 783 |
| Number of id | 290 | 290 | 290 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications include individual fixed effects and visit dummies.

absolute number of unprotected acts has decreased. Changes in the number of clients could have a similar effect if the perception is based on the number of clients who demand unprotected sex. For attempts to negotiate condom use, the direction of bias is unclear. On one hand, it may be easier to remember unprotected sex acts that occur after a failed attempt to negotiate condom use. On the other hand, more attempts to negotiate condom use, even if they fail, may fuel the perception that overall condom use has increased.

The measure of total number of commercial sex acts is constructed as the sum of vaginal and anal sex acts exchanged with regular and nonregular clients. The number of clients is directly elicited. Attempts to negotiate condom use are measured as "the total number of times you tried to talk regular/nonregular male clients into using a condom for sex." All variables are measured over the past month.

Table 1.15 shows that the results are robust to the inclusion of these controls.

Column 1 verifies that the main result holds on the subsample with no missing data for any of the controls.[31] Column 2 controls for the total number of sex acts exchanged, and Column 3 introduces the total number of clients. Neither has a statistically significant or meaningful impact on the estimated coefficients. Column 4 reports results when controlling for attempts to negotiate condom use. Interestingly, attempts to talk a client into condom use are associated with an increase in reported risky behavior. Nevertheless, the main result is robust. The coefficient on contemporaneous homicides falls slightly in magnitude, but remains statistically significant and negative. These results suggest that recall bias is not driving the observed effect of violence on risky sexual behavior.

**Alternate measure of risky sexual behavior**

Using the responses to multiple survey questions, it is possible to construct an alternate measure of inconsistent condom use: the unprotected fraction of total commercial sex acts. This measure is constructed by summing across eight different survey questions, so it is more difficult to manipulate than the direct, ordinal measure used in the main analysis.[32] Nevertheless, the unprotected fraction is highly correlated with the ordinal measure within individual.[33]

Table 1.16 presents the effect of violence on this alternate measure of sexual risk-taking. The coefficient on contemporaneous homicides is negative in all

[31] Two observations are missing for total sex acts and six for negotiating condom use, due to nonresponse. I verify that the main results hold on the remaining sample, then ignore the missing observations.

[32] The denominator is the total number of commercial sex acts, described in Section 1.9.3. The numerator is the number of unprotected commercial sex acts. I first calculate the number of times the respondent used a condom with clients, which is the sum of the number of times the respondent used a condom for vaginal and anal sex acts, with regular and nonregular clients. Then, the number of protected sex acts is subtracted from the total to obtain the number of unprotected sex acts.

[33] A fixed effects regression of fraction unprotected on the ordinal measure yields a regression coefficient of 0.85.

**Table 1.15**: Controls for Recall Bias

| *Dependent Variable:* | Inconsistent Condom Use | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Homicides during recall month | -0.157** | -0.158** | -0.159** | -0.147** |
| | (0.0727) | (0.0726) | (0.0723) | (0.0732) |
| Homicides -1m | -0.0863 | -0.0853 | -0.0897 | -0.0869 |
| | (0.0819) | (0.0815) | (0.0816) | (0.0811) |
| Homicides -2m | 0.0647 | 0.0635 | 0.0607 | 0.0535 |
| | (0.0835) | (0.0832) | (0.0835) | (0.0832) |
| Homicides -3m | 0.0116 | 0.0137 | 0.0180 | 0.0212 |
| | (0.0906) | (0.0905) | (0.0908) | (0.0902) |
| Number of sex acts exchanged | | 0.000152 | -0.000556 | -0.000391 |
| | | (0.000240) | (0.000347) | (0.000357) |
| Number of clients | | | 0.00116** | 0.000900 |
| | | | (0.000564) | (0.000580) |
| Number of times negotiated condom use | | | | 0.00115* |
| | | | | (0.000657) |
| Constant | 0.767** | 0.746** | 0.728** | 0.672* |
| | (0.350) | (0.353) | (0.353) | (0.356) |
| Observations | 1064 | 1064 | 1064 | 1064 |
| Number of id | 300 | 300 | 300 | 300 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

**Table 1.16**: Alternate Measure of Risky Sexual Behavior

| *Dependent Variable:* | Unprotected fraction of acts | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Homicides during recall month | -0.0304 | -0.0377* | -0.0601** | -0.0575 |
| | (0.0195) | (0.0215) | (0.0264) | (0.0388) |
| Homicides -1m | | 0.0152 | -0.0323 | -0.0698 |
| | | (0.0240) | (0.0329) | (0.0496) |
| Homicides -2m | | 0.0132 | -0.00213 | -0.0162 |
| | | (0.0221) | (0.0301) | (0.0479) |
| Homicides -3m | | -0.0144 | 0.0194 | -0.0186 |
| | | (0.0255) | (0.0368) | (0.0499) |
| Constant | 0.612*** | 0.617*** | 0.965*** | 0.661*** |
| | (0.0779) | (0.0830) | (0.282) | (0.180) |
| Time trend | X | X | | |
| Month-of-Year FE | X | X | X | |
| Quarter FE | | | X | |
| Month FE | | | | X |
| Observations | 991 | 991 | 991 | 991 |
| Number of id | 300 | 300 | 300 | 300 |
| P-value: Long-term effect = 0 | . | 0.451 | 0.305 | 0.0976 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications.

specifications and statistically significant in two specification. The lack of statistical significance is likely due to measurement error, which is exacerbated by summing across survey questions. These results concur with the conclusion that violence reduces risky sexual behavior with clients.

### Different specifications of dependent variable

Table 1.17 presents robustness checks that vary how the primary outcome is defined. In the main results, the primary outcome is equal to 0 if the respondent did not exchange sex. This combines the intensive and extensive margin of safe behavior. I verify that the results are not sensitive to this decision by recoding

these observations as missing, so that the variable reflects only the intensive margin of condom use. Columns 1 of Table 1.17 shows results with a linear time trend, and Column 2 shows results with month fixed effects. The effect is slightly larger when restricting analysis to the intensive margin. This is consistent with the results in Table 1.8, which show that the effect goes in the opposite direction on the extensive margin.

Another concern may be that the results rely on the specific cutoff chosen to define inconsistent condom use. I therefore show results from an ordinary least squares regression on the full ordered response variable[34]. The linear model is not the most efficient estimator for an ordinal dependent variable, and the magnitudes of the coefficients are not interpretable. However, unlike nonlinear models such as ordered probit, OLS allows for the use of individual fixed effects. For this reason, it provides most appropriate test of whether the cutoff matters. Column 3 of Table 1.17 shows results on the full dependent variable with a linear time trend, and Column 4 shows results with month fixed effects. The result is robust with the linear trend; with month fixed effects, it just missed significance with a p-value of 0.123. The results are consistent with the interpretation that violence reduces risky behavior.

**Attrition**

To explore the impact of attrition, I begin by studying heterogenous effects by attrition status. I define *Attritor* as an indicator equal to one if the respondent has fewer than 4 observations for the dependent variable, due to either a missed visit or non-response. In Columns 1 and 2 of Table 1.18, I interact this variable with contemporaneous homicides. In the presence of individual fixed effects, this

[34]Observations that always used condoms and those that did not exchange sex are both coded as 0.

**Table 1.17**: Different Specifications of Inconsistent Condom Use

| | (1) ICU-Int. Marg. | (2) ICU-Int. Marg. | (3) ICU-Ord. Resp. | (4) ICU-Ord. Resp. |
|---|---|---|---|---|
| Homicides during recall month | -0.134*** | -0.183** | -0.142** | -0.199 |
| | (0.0373) | (0.0798) | (0.0619) | (0.128) |
| Homicides -1m | -0.00873 | -0.0948 | 0.140** | 0.0726 |
| | (0.0380) | (0.0903) | (0.0678) | (0.149) |
| Homicides -2m | 0.0580 | 0.0558 | 0.00914 | 0.0883 |
| | (0.0391) | (0.0896) | (0.0636) | (0.140) |
| Homicides -3m | 0.0338 | 0.0341 | -0.00325 | -0.0632 |
| | (0.0466) | (0.0961) | (0.0770) | (0.152) |
| Constant | 0.649*** | 0.831** | 1.339*** | 1.486*** |
| | (0.157) | (0.375) | (0.231) | (0.531) |
| Time trend | X | | X | |
| Month-of-Year FE | X | | X | |
| Month FE | | X | | X |
| Observations | 995 | 995 | 1070 | 1070 |
| Number of id | 300 | 300 | 300 | 300 |
| P-value: Serially correlated errors | 0.688 | 0.986 | 0.350 | 0.103 |
| P-value: Long-term effect = 0 | 0.423 | 0.369 | 0.966 | 0.736 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. The dependent variable in columns 1 and 2 is the intensive margin of inconsistent condom use, where observations that did not exchange sex are coded as missing instead of 0. The dependent variable in columns 3 and 4 is the full ordered response variable. All specifications are are estimated using OLS with individual fixed effects.

interaction is identified for attritors with two or three observations; 11 of 90 attritors have only one observation.

In Column 1, I use the base specification to maximize power. In Column 2, I show the differential effect in the preferred specification including month fixed effects. In both specifications, the coefficient on contemporaneous homicides, which represents the effect for non-attritors, remains negative and statistically significant.

The differential effect for attritors is negative and statistically significant, indicating that the effect is stronger (larger in magnitude) for this population. If we assume that this effect is a good proxy for the missing responses of attritors, then their periodic absence from the sample biases my estimate of the average effect *toward* zero, meaning that the estimated effect is a lower bound.

We can see this in Columns 3 to 5 of Table 1.18, in which I drop all attritors, leaving a sample of 210 individuals and 840 observations. I estimate the base specification, then control for lagged homicides and month fixed effects. In the last row of the table, I report $p$-values for a test of statistical difference between the main effect on the full sample (Table 1.3) and on the restricted sample. The effect remains negative and statistically significant in Columns 3 and 4, but the point estimate is statistically smaller in magnitude than in the full sample. After controlling for month fixed effects, the coefficient remains negative, although it is no longer statistically significant or significantly different from the full sample, due to reduced power. The results in Table 1.18 suggest that, if I had a full set of observations, the estimated effect would be even more negative.

**Table 1.18**: Attrition

| *Dependent Variable:* | Inconsistent Condom Use | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Homicides during recall month | -0.0762** | -0.142* | -0.0490 | -0.0627* | -0.0772 |
| | (0.0344) | (0.0735) | (0.0362) | (0.0368) | (0.0824) |
| Homicides during recall X Attritor | -0.0989** | -0.0863* | | | |
| | (0.0487) | (0.0500) | | | |
| Homicides -1m | | -0.0878 | | 0.00134 | -0.0415 |
| | | (0.0821) | | (0.0393) | (0.0870) |
| Homicides -2m | | 0.0493 | | 0.0250 | 0.0947 |
| | | (0.0831) | | (0.0386) | (0.0933) |
| Homicides -3m | | 0.0130 | | 0.0606 | 0.0740 |
| | | (0.0894) | | (0.0464) | (0.0983) |
| Constant | 0.598*** | 0.768** | 0.540*** | 0.450*** | 0.371 |
| | (0.140) | (0.345) | (0.144) | (0.159) | (0.384) |
| Time trend | X | | X | X | |
| Month-of-Year FE | X | | X | X | |
| Month FE | | X | | | X |
| Observations | 1070 | 1070 | 840 | 840 | 840 |
| Number of id | 300 | 300 | 210 | 210 | 210 |
| P-value: equal to coefficient on full sample | . | . | 0.383 | 0.352 | 0.481 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 1.3, are included in all specifications. Columns 1 and 2 examine differential effects for attritors having more than one observation. Columns 3 to 5 estimate effects on the subsample of individuals with a full set of four observations.

# Chapter 2

# Health Certification in the Market for Sex Work: A Field Experiment in Dakar, Senegal

## 2.1 Introduction

Female sex workers (FSWs) are disproportionately affected by sexually transmitted infections (STIs). In developing countries, estimates of STI prevalence among FSWs have ranged from 28 to 84 percent (Cwikel et al., 2008). Many of these infections are curable, but female sex workers often do not seek health care even when they have symptoms. Because female sex workers are core population in the epidemiology of STIs, high STI prevalence in the population helps maintain the wider STI epidemic (Aral, 2000). A common solution aimed at STI control is a health certification program known as "legalization with regulation." Under this policy, which is used in over 20 countries, FSWs can avoid some criminal penalties if they obtain certification by registering with a government clinic or agency, undergoing regular gynecological check-ups, and treating any diagnosed STIs.[1]

[1]Some legalization programs use laboratory testing for STIs, but many others, including the one described in this paper, diagnose STIs based on clinical symptoms. The medical literature has shown that this approach is effective in significantly reducing STI prevalence among sex workers (Ndoye et al., 1998; Ghys et al., 2001; Steen and Dallabetta, 2003).

On first examination, legalization and regulation appears to be well grounded in economic theory. We would expect sex markets to suffer from the asymmetric information problem famously described by Akerlof (1970). Under this assumption, sex workers know more about their STI status than their clients. Even if clients are willing to pay more to transact with a sex worker who does not have an STI, they have no way of verifying whether a given sex worker is STI-free. Sex workers then have limited incentives to invest in remaining STI-free, and there are more STIs in the market than under the Pareto optimum. Certification mitigates this problem by providing a credible mechanism for the sex worker to disclose her health status. An information disclosure model adapted from Leland (1979) and Spence (1973) predicts that when sex workers' STI status is unobservable, certified providers should earn higher prices, and when certification costs are low enough, all suppliers should choose to get certified.

Despite the theoretical informational benefits of certification, participation in legalization programs remains low in most settings (Sirotin et al., 2010; Gertler and Shah, 2011). Senegal has had a legalization and regulation policy, operating through registration at public STI clinics, since 1969.[2] Yet local estimates suggest that certified sex workers represent just 20 percent of the sex worker population (Tucker, 2012). As with any social program (Moffitt, 1983), there are three possible explanations for low take-up: (i) eligible participants do not know about certification; (ii) the costs associated with participation are high; or (iii) the benefits of participation are low.

I conducted a novel randomized experiment that addressed the first two constraints. In a sample of 291 uncertified sex workers, I provided information about

[2]Under the program, certified sex workers cannot be prosecuted for prostitution. However, public soliciting, deriving financial gain from prostitution, and other activities related to sex work remain illegal (Mgbako and Smith, 2009).

certification and offered a one-time, monetary certification incentive to a randomly selected treatment group. Take-up of this incentive, which was approximately equal to a day's wage for the median sex worker, was surprisingly low: only 7 percent of the treatment group got certified, relative to 2 percent of the control group. This low participation rate does not appear to be driven by the time and transportation costs of certification: the incentive amount was equal to the reimbursement given for participation in surveys, which carried equivalent time and transportation costs. In contrast to certification rates, 93 percent of individuals randomized completed the endline survey.

Given this result, I investigate the benefits of certification, and find no evidence that the market values certification.[3] The price premium to certified sex workers appears to be statistically and economically insignificant in both causal and descriptive analysis. Using detailed transaction-level panel data I collected, I show that the experimental intention-to-treat effect is near zero. Then, I study differential, within-individual price changes over time for participants who got certified versus those who did not. While this analysis is descriptive, I control for a large part of the selection bias using individual fixed effects. Certification is not significantly associated with higher prices in any specification. With reasonable controls, the point estimate is negative. Clients asked about certification in just 11 percent of transactions, providing further support that certification is not valued in the market.

The lack of a large certification premium can be explained, in part, by evidence that sex workers' STI status is partially observable. I present new evidence that prices respond to an individual sex workers' STI status even in the absence of

[3]I also show that, in practice, the legal benefits of participation are also small due to limited enforcement of prostitution laws. At baseline, only 3.4 percent of the sample reported recent detention or arrest, and fewer than 1 percent did jail time. Moreover, recent arrest and jail time do not predict take-up.

certification. Controlling for individual fixed effects and types of sex acts exchanged, sex workers with visible STI symptoms receive a 19 percent discount on transaction prices. This effect appears to be driven by client willingness-to-pay: in contrast to visible STI episodes, STI episodes that are not visible to the client have no significant effect on prices. This result may be surprising given that the most important unobservable STI is HIV. However, clients may plausibly believe that the risk of acquiring HIV through exchanging sex in Senegal is very low. This conclusion follows from the combination of low general population HIV prevalence in Senegal (0.5 percent in 2015)(UNAIDS, 2015) and longstanding, free access to antiretroviral therapy, which reduces the likelihood of HIV transmission (Cohen et al., 2011). Therefore, it is reasonable to believe that STIs that produce visible symptoms are a greater concern for clients in Senegal.

The absence of a certification premium alone can explain low take-up of Senegal's certification program in general; however, it does not explain the low response to the moderate financial incentive offered in this experiment. To complete the analysis of low take-up, I analyze the non-monetary costs of certification. While the monetary costs of the program are low, I find that stigma costs associated with registering are extremely high for a large proportion of FSWs. In a stated willingness-to-accept elicitation, 44 percent of respondents said there was no incentive amount that could convince them to register. Self-stigma (i.e, unwillingness to accept a "sex worker" identity) is highly predictive of refusal to register.

Finally, I consider whether certification may be helpful in limiting the infectious disease externality of sex work. Given low take-up, the effectiveness of the program for STI control depends largely on targeting. The program must reach those at highest risk of acquisition and transmission of STIs. However, I present evidence that FSWs who self-select into the program are at relatively low

risk for acquiring and transmitting STIs. While experiencing recent STI symptoms at baseline is predictive of take-up, it is only predictive among health-seeking types: women who were already engaged in numerous health-protective behaviors that reduce their risk of both acquiring and transmitting STIs.

This paper contributes to the literature on the economics of sex work, in particular understanding how client demand responds to STI risk. This paper is the first to show a given sex worker's prices respond to her own STI status, conditional on individual fixed effects. By distinguishing between visible and invisible STI symptoms, I present convincing evidence that this effect is driven primarily by client demand. Previous work has focused largely on sex workers' preferences and incentives, showing that clients who demand unprotected or anal sex compensate sex workers for the increased riskiness of the transaction (e.g., Gertler et al. (2005); Arunachalam and Shah (2013)). Robinson and Yeh (2011) show that female sex workers in Kenya are less likely to exchange sex on days when they have STI symptoms, but it is not clear whether this is a demand effect or a productivity effect (i.e., supplying sex is painful when the sex worker has an STI). Arunachalam and Shah (2013) show that sex workers earn lower prices where the local STI prevalence is higher, which is consistent with the notion that clients are willing to pay for a lower risk of acquiring an STI.

This paper also contributes to a large literature on information disclosure mechanisms that aim to alleviate asymmetric information problems (see Dranove and Jin (2010) for a review). It is a general prediction of this literature that certified providers should earn higher prices and, as long as certification costs are negligible, almost all suppliers choose to obtain certification. I present a new example of the failure of this prediction: in the understudied market for sex work, I find no evidence for a certification price premium and very low take-up of certification,

despite zero monetary certification costs.[4]

In addition, this paper contributes to a growing subset of the certification literature exploring how markets may differentially respond to different types and sources of information. Several studies have found evidence for the intuitive idea that certification has a greater impact on the market when the *added* informational value is higher. For example, Elfenbein et al. (2015) show that seller quality certification on the online marketplace eBay affects demand more among sellers with less established reputations, as measured by the customer feedback score. In other words, certification and seller reputation are substitutes. Jin and Leslie (2009) and Xiao (2010) find similar results analyzing mandatory restaurant hygiene grade cards and accreditation of childcare operators, respectively. Dranove and Sfekas (2008) identify the informational channel for this effect, using a structural model to show that hospital report cards had an impact only when the results were a "surprise" relative to consumers' prior beliefs about hospital quality. In a similar vein, I show that when a seller attribute is partially observable, consumers may place limited informational value on third-party certification, and as a result the certification has no impact on the market. I find that clients are willing to pay more to transact with a sex worker who has no visible STI symptoms, but there is no evidence for a certification premium even among sex workers with no symptoms.

From a policy perspective, this study makes several contributions to the literature on sex worker registration programs. Senegal's program is similar to regimes prevalent in Latin America, Europe and Asia (Sirotin et al., 2010; Gertler and Shah, 2011; Liu and So, 1996; Harcourt et al., 2010), and as the only program of its kind in Africa, it has been discussed as a model for other programs on the

[4]Logan and Shah (2013) analyze informal solutions to asymmetric information problems in the online male sex work market in the United States, and highlight the importance of sex worker photos as a signalling mechanism. However, there is no third-party certification in the market they analyze.

continent. This study provides the first rigorous evidence on barriers to take-up of registration programs. I show that many sex workers are unwilling to register even when provided information about the program and offered a moderate incentive to do so. This result rules out lack of information or minor hassle costs as the primary barriers to registration for a large proportion of uncertified sex workers. I present evidence that resistance to registration is related to stigma toward female sex workers, a widespread phenomenon. The results suggest that take-up of registration will continue to be low in most settings.

Second, I demonstrate the importance of selection bias in understanding simple comparisons between certified and uncertified sex workers. Previous studies relying on simple comparisons have found that certified sex workers earn significantly higher prices (e.g. Sirotin et al. (2010) in Tijuana, Mexico).[5] In contrast to previous work, my data include panel variation in registration status, allowing for the use of individual fixed effects to analyze prices, and I find no evidence for a price premium for registration.[6] In previous studies, registration was also associated with improvements in risk behaviors (Liu and So, 1996; Kerrigan et al., 2006; Harcourt et al., 2010; Sirotin et al., 2010; Gaines et al., 2013). For example, in early studies in the Philippines, certified sex workers reported more consistent condom use despite comparable levels of HIV-related knowledge, and in Mexico, certified sex workers

[5]Gertler and Shah (2011) is an exception to this: they exploit geographic variation in law enforcement prosecution of uncertified sex workers in Ecuador. They focus on the causal effect of registration on STI rates, and find lower STI rates in regions where enforcement is greater in the riskier street sector, but higher STI rates where police conduct raids on brothels and nightclubs. However, they also study prices and quantities exchanged, and find that greater enforcement led to increased prices and lower quantities exchanged for both protected and unprotected sex. Taken together, the results are more consistent with a direct effect of police activities on the supply of sex rather than an effect of registration per se.

[6]In addition to selection bias, the difference may stem from contextual factors: it is possible that there is a certification premium in Tijuana, but not in Senegal. However, HIV prevalence among female sex workers in Tijuana is substantially lower than in Senegal, so to the extent that HIV is the most important unobservable STI, we would expect the premium to be even lower in Tijuana.

were less likely to have an STI (Liu and So, 1996; Amadora-Nolasco et al., 2001; Sirotin et al., 2010).[7] However, I show that sex workers who select into registration are "health-seeking types" who were engaged in preventive behaviors even before registering. Thus, while the biomedical literature has shown that regular screening and testing of STIs reduces the STI burden (World Health Organization, 2012), the impacts of registration programs on risk behaviors remain to be tested.

Finally, my results on low take-up and targeting have wider implications for public health policy. In particular, if FSWs at the highest risk of acquiring and transmitting STIs are less likely to participate in registration programs, alternate services for screening and treatment of STIs are needed to achieve STI control. It appears that Senegal has made significant progress on this front: at baseline, nearly 30 percent of the sample reported a well visit (i.e, seeing a health care provider for gynecological care even when they did not have symptoms) in the past month. Nevertheless, in order to reach World Health Organization recommendations for prevention and treatment of STIs among FSWs (World Health Organization, 2012), further research is needed to understand how best to encourage female sex workers to participate in either registration or alternate health care.

The remainder of the paper proceeds as follows. In Section 2.2, I lay out a conceptual model to provide a framework for the empirical results. In Section 2.3, I provide background on sex work in Senegal, the registration program, and the experimental design. Section 2.4 describes the data and estimation strategy. Section 2.5 presents the experimental results; results on the certification premium, client willingness to pay for sex workers' health, and the existence of asymmetric information in the market; and evidence on non-monetary certification costs. Section 2.6 discusses policy implications of the study, including results related to targeting.

[7]The result in Mexico was largely explained by demographic and behavioral characteristics, which is also suggestive of selection bias.

Section 2.7 concludes.

## 2.2 Model

In this section, I develop a conceptual model to guide the empirical results. First, I show that a standard signaling model based on Leland (1979) and Spence (1973) generates two key theoretical predictions, which I test in this paper: (i) the unobservable nature of sexually transmitted infections generates a price premium for certified female sex workers; and (ii) if certification costs are low, most sex workers should choose to obtain certification.

Then, I analyze a market with partially observable information. In particular, I consider symptoms that reveal a sex workers' STI to the client with some probability. I show that the price premium and take-up of certification vary with the probability of symptoms conditional on having an STI. As the probability of symptoms increases and the degree of asymmetric information falls, take-up of certification falls and the price premium for certified sex workers disappears.

### 2.2.1 Unobservable Health Status

#### Model Set-up

Each sex worker is either healthy or infected with an STI. Let $\theta \in \{\theta_H, \theta_S\}$ denote her health status, which is unobservable. She has some probability of being healthy $q(\rho) = Pr(\theta_H|\rho)$, where $\rho \in [0, \bar{\rho}]$ is a level of health investment and $q(\rho)$ is continuous and strictly increasing in $\rho$. I assume that regardless of health investments, there is uncertainty around health status: $q(\rho = 0) > 0$ and $q(\bar{\rho}) < 1$. Sex workers who make the minimal health investment may still be healthy, and sex workers who make the maximal health investment can still be infected with STIs.

For ease of exposition, I will assume that the level of health investment $\rho$ is exogenously given and cannot be changed. This simplification does not generate substantively different results from a model where health investments are optimally determined according to sex workers' preferences. For this reason, I call individuals with higher $\rho$ "health seekers". I assume that there is at least one agent who chooses $\bar{\rho}$. I also assume that there are agents who choose $\rho < \bar{\rho}$. That is, if $f(\cdot)$ denotes the density of $\rho$, $f(\bar{\rho}) < 1$.

Health investment carries a cost. The cost of supplying one unit of sex at a given investment level is $c(\rho)$. I assume $c(0) = 0$ and $c(\bar{\rho}) > 0$, and $c$ continuous in $[0, \bar{\rho}]$. I also assume the cost is weakly increasing: $c'(\rho) \geq 0$. Both the health production function $q(\rho)$ and the cost function $c(\rho)$ are homogenous across agents. Therefore, the only source of heterogeneity across agents is the level of health investment $\rho$, which is driven by sex workers' individual preferences.

On the demand side, clients value health according to some value function $v(\theta)$. I assume that the value of a transaction with a healthy sex worker is strictly greater than the value of a transaction with a sex worker who has an STI: $v(\theta_H) > v(\theta_S)$. Both $q$ and $\rho$ are unobservable to clients; therefore, the value of a transaction depends on clients' beliefs about the probability that the sex worker is healthy, denoted by $\mu$. The subjective expected value of a transaction is then $\mathrm{E}(v(\theta)) = \mu v(\theta_H) + (1-\mu)v(\theta_S)$. I normalize $v(\theta_S) = 0$ and $v(\theta_H) = 1$ to obtain demand $p^D \leq \mu$.

In addition to making private investments $\rho$, sex workers can choose to participate in a health program. Clients observe the participation decision, denoted by $h \in \{h_0, h_1\}$, and form beliefs about the probability of health conditional on $h$, $\mu(h)$. FSWs who participate in the program are "certified." Participation in the program sets the probability of health to that under the maximal $\rho$: $q(h_1) = q(\bar{\rho}) := \bar{q}$.

It also carries a health care cost $\gamma$. Health care is not subsidized through the program, so $\gamma = c(\bar{\rho})$. Recall the previous assumption that at least one agent chooses $\bar{\rho}$. Thus, there is at least one agent for whom the health care cost associated with participation in the program is equal to her health investment in the absence of the program. In Section 2.4.2, I show that this assumption is reasonable in the Senegalese context.

**Equilibrium**

Since normalized demand is just the client's belief about health $\mu$, in any perfect Bayesian equilibrium, clients will pay $\mu^*(h_1)$ to certified sex workers and $\mu^*(h_0)$ to uncertified sex workers, where beliefs $\mu^*$ are derived from equilibrium strategies using Bayes' rule where possible. Now, the health program ensures that the probability of health is $\bar{q} = q(\bar{\rho})$. Therefore, $\mu^*(h_1) = \bar{q}$ and any equilibrium must satisfy:

$$p = \begin{cases} \bar{q} & \text{if } h_1 \\ \mu^*(h_0) & \text{if } h_0 \end{cases}$$

for some $\mu^*(h_0)$. Given client demand, sex workers' utility from a transaction is:

$$U(h) = \begin{cases} \bar{q} - \gamma & \text{if } h_1 \\ \mu^*(h_0) - c(\rho) & \text{if } h_0 \end{cases}$$

**Certification take-up**

Sex workers choose to participate when $U(h_1) > U(h_0)$, or when the marginal benefit of participating is greater than the marginal cost of participating:

$$\bar{q} - \mu^*(h_0) > \gamma - c(\rho)$$

I do not model the benefits of legalization (i.e. protection from prosecution for certified sex workers). In Section 2.4.2, I show that in the Senegalese context, the benefits of legalization appear to be nominal, since enforcement of laws against prostitution is limited.

**Separating Equilibrium**

As specified above, for all certified sex workers in equilibrium, it must be the case that the marginal benefit of participating in the health program is greater than $\gamma - c(\rho)$. Since $\gamma$ is constant and $c(\rho)$ is monotonically increasing, this implies that health-seeking types are more likely to participate: there is some $\hat{\rho}$ such that $\rho \leq \hat{\rho}$ for all uncertified sex workers. Bayes' rule then requires that $\mu^*(h_0) = Pr(\theta_H | h_0, \rho \leq \hat{\rho})$. There is an equilibrium where $\hat{\rho}$ satisfies:

$$\bar{q} - Pr(\theta_H | h_0, \rho \leq \hat{\rho}) = \gamma - c(\hat{\rho}) \tag{2.1}$$

and the marginal benefit curve intersects the marginal cost curve from below:

$$\frac{\partial}{\partial \hat{\rho}}(Pr(\theta_H | h_0, \rho \leq \hat{\rho})) < c'(\hat{\rho}) \tag{2.2}$$

Figure 2.1 illustrates an example of a separating equilibrium. The curvature of marginal benefit and marginal costs curves is for illustrative purposes only; I

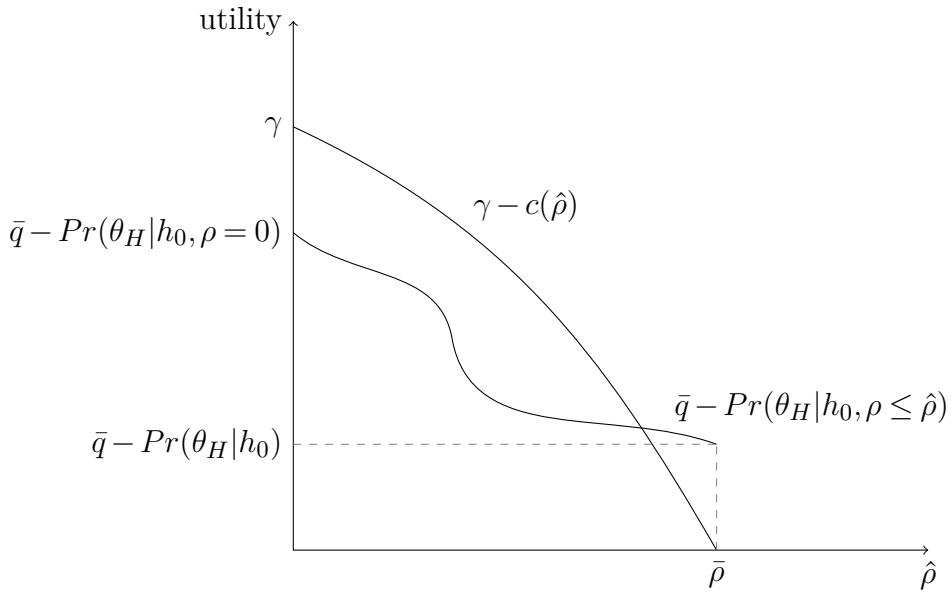


**Figure 2.1**: Example: Separating Equilibrium.

have not made any assumptions on the shape of these curves, only that they are weakly decreasing.[8]

**Result 1.** *In any separating equilibrium, $\bar{q} > \mu^*(h_0)$. That is, there is a price premium for certified sex workers.*

Since $\rho \in [0, \bar{\rho}]$, $\bar{q} = q(\bar{\rho})$ is the maximum achievable probability of health. Therefore we have $\bar{q} \geq \mu^*(h_0)$ for any rational belief $\mu^*(h_0)$. In a separating equilibrium, from the characterization in (2.1), it must be the case that this inequality is strict:

$$\mu^*(h_0) = Pr(\theta_H|h_0, \rho \leq \hat{\rho}) < q(\bar{\rho})$$

The lefthand term decreases with $\hat{\rho}$. At the maximal $\hat{\rho} = \bar{\rho}$, this term is just the share of sex workers in the market with $\theta_H$ if no sex workers register, $Pr(\theta_H|h_0)$.

[8] In this example, the separating equilibrium is unique; however, this need not be the case. The following assumptions are sufficient for uniqueness: weak convexity of $c(\rho)$; the distribution of $\rho$ is such that $F(\hat{\rho}) = Pr(\theta_H|h = 0, \rho \leq \hat{\rho})$ is either weakly concave or weakly convex (i.e., no inflection points); and $\bar{q} - Pr(\theta_H|h_0, \rho = 0) < \gamma$.

This must be less than $q(\bar{\rho})$ because (i) I assumed that there are agents with $\rho < \bar{\rho}$ (i.e., $f(\bar{\rho}) < 1$); and (ii) $q(\rho)$ is strictly increasing.

**Pooling equilibrium**

There is no pooling equilibrium in which all sex workers remain uncertified. Consider an outcome in which all sex workers choose $h^* = 0$. Then we must have $\mu^*(h_0) = Pr(\theta_H|h_0)$. It is profitable for sex workers to deviate to $h_1$ if:

$$\bar{q} - Pr(\theta_H|h_0) > \gamma - c(\rho)$$

But, as shown above, $\bar{q} - Pr(\theta_H|h_0) > 0$. And, by assumption $\gamma - c(\bar{\rho}) = 0$. Therefore deviation to $h_1$ is profitable for all sex workers with $\rho = \bar{\rho}$. Thus, this result follows from the assumption that there is at least one agent with $\rho = \bar{\rho}$. This result is easy to see in Figure 2.1: the marginal cost curve intersects the x-axis at $\bar{\rho}$, while the marginal benefit curve remains strictly above the x-axis. Thus, the two curves cannot intersect at $\bar{\rho}$.

**Result 2.** *If the cost of obtaining health certification is small, there is a pooling equilibrium in which all sex workers get certified.*

Suppose all sex workers are certified. FSWs will deviate to $h_0$ if

$$\bar{q} - \mu^*(h_0) < \gamma - c(\rho)$$

Let $\mu^*(h_0) = Pr(\theta_H|h_0, \rho = 0)$. This belief makes sense: for given any $\mu^*(h_0)$, the sex worker most likely to deviate is the sex worker with the most to gain, i.e. the maximal $\gamma - c(\rho)$. This occurs at $c(\rho) = 0$, and $c(\rho = 0) = 0$. Given $\mu^*(h_0)$, FSWs then have no incentive to deviate if:

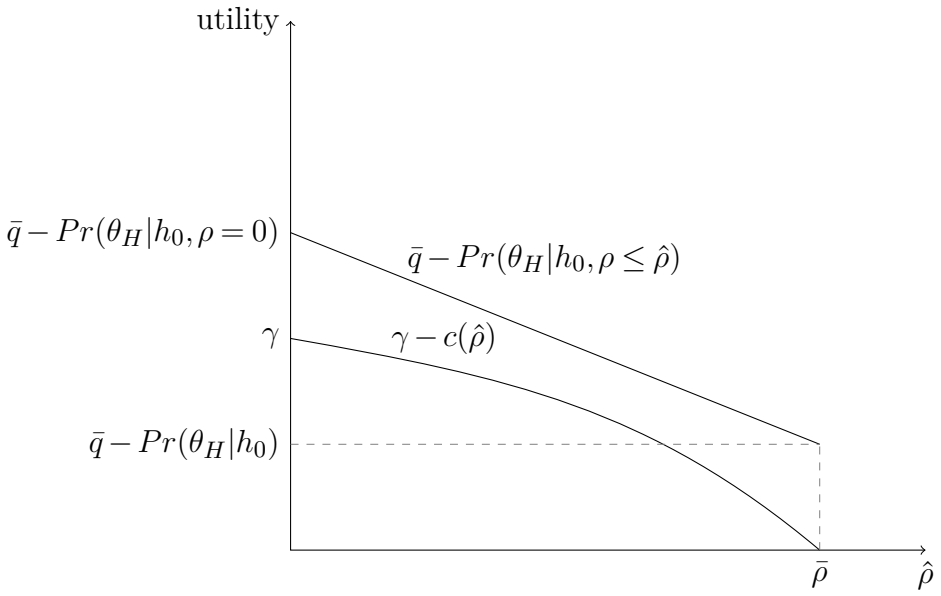


**Figure 2.2**: Example: Unique Pooling Equilibrium

$$\bar{q} - Pr(\theta_H|h_0, \rho = 0) > \gamma$$

The lefthand side of the above expression is the difference between the probability of health given maximal health investment, and the probability of health given minimal health investment. If this difference is larger than the cost of the health program, all FSWs are better off remaining certified. This condition might be satisfied when the baseline probability of STIs is high, and the health program is extremely effective. It might also be satisfied when the participation cost $\gamma$ is very low.

A sufficient condition for uniqueness of the pooling equilibrium is that a separating equilibrium does not exist. This occurs when the marginal benefit of certification is everywhere greater than the marginal cost (for example, when the marginal cost is very low). An example of a unique pooling equilibrium is shown in Figure 2.2.

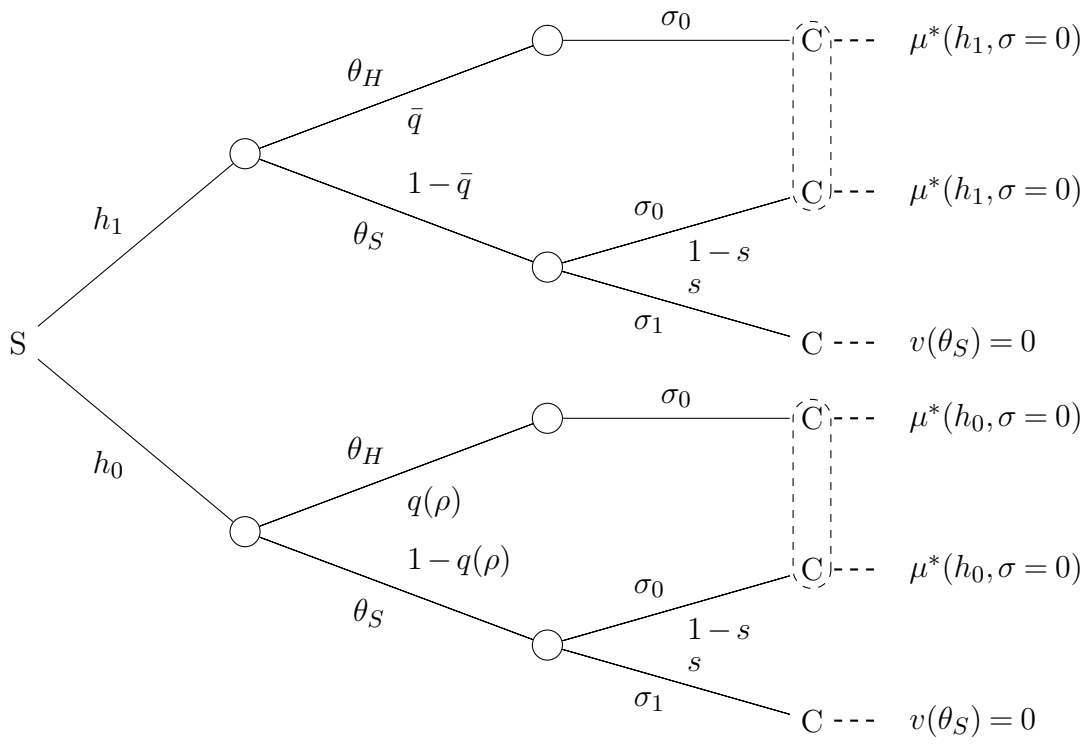


**Figure 2.3**: Certification Decision under Partially Observable Information

### 2.2.2 Partially Observable Health Status

Suppose now that there is a signal $\sigma \in \{\sigma_0, \sigma_1\}$ that fully reveals STI infection. That is, $Pr(\theta_S|\sigma_1) = 1$. The signal is never sent when the sex worker is healthy, but when the sex worker is infected with an STI, the signal is sent with some probability $s$:

$$Pr(\sigma_1) = \begin{cases} 0 & \text{if } \theta_H \\ s & \text{if } \theta_S \end{cases}$$

We can think of $\sigma = 1$ as visible STI symptoms. Figure 2.3 depicts a decision tree for the certification decision under partially observable information.

**Equilibrium**

In equilibrium, clients will pay $\mu^*(h,\sigma)$, where, as before, beliefs $\mu^*$ are derived from equilibrium strategies using Bayes' rule where possible. For $\sigma_1$, clients know with certainty that the sex worker is infected with an STI, so $\mu^*(h,\sigma_1) = 0 \; \forall \, h$.

For $\sigma_0$, demand depends on beliefs as before. Clients will pay $\mu^*(h_1,\sigma_0)$ to certified sex workers who do not have symptoms, and $\mu^*(h_0,\sigma_0)$ to uncertified sex workers who do not have symptoms. From Figure 2.3, we can see that the belief for certified sex workers must be:

$$\mu^*(h_1,\sigma_0) = \frac{\bar{q}}{\bar{q} + (1-\bar{q})(1-s)}$$

Hence, equilibrium demand is:

$$p = \begin{cases} \dfrac{\bar{q}}{\bar{q} + (1-\bar{q})(1-s)} & \text{if } h_1, \sigma_0 \\ \mu^*(h_0,\sigma_0) & \text{if } h_0, \sigma_0 \\ 0 & \text{if } \sigma_1, \forall \, h \end{cases}$$

for some $\mu^*(h_0,\sigma_0)$. This demand function generates the following result.

**Result 3.** *In any separating equilibrium, sex workers who have visible symptoms earn lower prices, regardless of certification status.*

This result is immediate for certified sex workers: $\bar{q}/[\bar{q} + (1-\bar{q})(1-s)] > 0$ since $\bar{q} > 0$. For uncertified sex workers, the result follows from the assumptions that those with the minimal health investment still have a non-zero probability of health: $q(\rho = 0) > 0$. Thus, even if only the lowest type remains uncertified, $\mu^*(h_0,\sigma_0) > 0$.

On the supply side, sex workers' utility now includes the possibility that the

STI infection will be revealed, and if this occurs they will earn the minimal price ($p = 0$). From Figure 2.3, the expected utility from certification is:

$$\begin{aligned} \mathrm{E}[U(h_1)] &= [\bar{q} + (1-\bar{q})(1-s)]\mu^*(h_1, \sigma_0) - \gamma \\ &= \bar{q} - \gamma \end{aligned}$$

Note that the welfare of certified sex workers is, in expectation, equal to their welfare in the case of fully asymmetric information.[9] The expected utility for uncertified sex workers is:

$$\mathrm{E}[U(h_0)] = [q(\rho) + (1-q(\rho))(1-s)]\mu^*(h_0, \sigma_0) - c(\rho)$$

**Certification take-up**

Sex workers will get certified when the expected utility of participation $\mathrm{E}[U(h_1)]$ exceeds $\mathrm{E}[U(h_0)]$, the expected utility of remaining uncertified :

$$\bar{q} - \gamma > [q(\rho) + (1-q(\rho))(1-s)]\mu^*(h_0, \sigma_0) - c(\rho)$$

or, as before, the marginal benefit of obtaining certification exceeds the marginal cost:

$$\bar{q} - [q(\rho) + (1-q(\rho))(1-s)]\mu^*(h_0, \sigma_0) > \gamma - c(\rho)$$

**Separating Equilibrium**

In the case of unobservable health status, there was some $\hat{\rho}$ such that all individuals with $\rho > \hat{\rho}$ chose $h_1$. This followed from the fact that the marginal benefit of certification was the same for all $\rho$, while the marginal cost of certification

[9] This result holds only when utility is linear in money.

was decreasing in $\rho$. Now in the partially observable case, the marginal cost of certification is still decreasing in $\rho$. However, the marginal benefit is also decreasing in $\rho$: health-seeking types benefit from a lower likelihood of experiencing symptoms $\sigma_1$ even when uncertified. Therefore, the added value of certification falls as $\rho$ increases.

Nevertheless, there exists a similarly defined $\hat{\hat{\rho}}$ such that all individuals with $\rho > \hat{\hat{\rho}}$ will get certified, if the marginal cost of obtaining certification increases faster in $\rho$ than the marginal benefit:

$$-sq'(\rho)\mu^*(h_0, \sigma_0) > -c'(\rho)$$

This condition simplifies to:

$$s\mu^*(h_0, \sigma_0) < \frac{c'(\rho)}{q'(\rho)}$$

Then Bayes' rule requires $\mu^*(h_0) = Pr(\theta_H | h_0, \sigma_0, \rho \leq \hat{\hat{\rho}})$ and there is an equilibrium where $\hat{\hat{\rho}}$ satisfies:

$$\bar{q} - [q(\hat{\hat{\rho}}) + (1 - q(\hat{\hat{\rho}}))(1 - s)] Pr(\theta_H | h_0, \sigma_0, \rho \leq \hat{\hat{\rho}}) = \gamma - c(\hat{\hat{\rho}})$$

or equivalently,

$$\bar{q} - [q(\hat{\hat{\rho}}) + (1 - q(\hat{\hat{\rho}}))(1 - s)] \frac{Pr(\theta_H | h_0, \rho \leq \hat{\hat{\rho}})}{Pr(\sigma_0 | h_0, \rho \leq \hat{\hat{\rho}})} = \gamma - c(\hat{\hat{\rho}}) \tag{2.3}$$

**Result 4.** *In any separating equilibrium, as the probability of symptoms increases, certification take-up falls: as* $s \to 1$, $\hat{\hat{\rho}} \to \bar{\rho}$.

To see this result, we will first consider how the separating equilibrium changes when we move from $s = 0$ to an arbitrary $s \in (0, 1)$. The case of $s = 0$

corresponds to unobservable health status. There is some $\hat{\rho}$, defined by equation (2.1), such that all individuals with $\rho \leq \hat{\rho}$ remain uncertified, while those with $\rho > \hat{\rho}$ obtain certification.

Now consider the sex worker with $\hat{\rho}$ when $s = 0$ moves to $s \in (0,1)$. For the sex worker with $\hat{\rho}$, the marginal benefit of obtaining certification is now :

$$\bar{q} - [q(\hat{\rho}) + (1 - q(\hat{\rho}))(1 - s)] \frac{Pr(\theta_H | h_0, \rho \leq \hat{\rho})}{Pr(\sigma_0 | h_0, \rho \leq \hat{\rho})}$$

or equivalently,

$$\bar{q} - \frac{Pr(\sigma_0 | h_0, \hat{\rho})}{Pr(\sigma_0 | h_0, \rho \leq \hat{\rho})} Pr(\theta_H | h_0, \rho \leq \hat{\rho})$$

Since a higher probability of health reduces the unconditional probability of symptoms, we have $Pr(\sigma_0 | h_0, \hat{\rho}) > Pr(\sigma_0 | h_0, \rho \leq \hat{\rho})$, which implies that the marginal benefit of certifying under partially observable health status is lower for the sex worker with $\hat{\rho}$ than it was under unobservable health status:

$$\bar{q} - \frac{Pr(\sigma_0 | h_0, \hat{\rho})}{Pr(\sigma_0 | h_0, \rho \leq \hat{\rho})} Pr(\theta_H | h_0, \rho \leq \hat{\rho}) < \bar{q} - Pr(\theta_H | h_0, \rho \leq \hat{\rho})$$

However, in both cases the marginal cost of certifying is $\gamma - c(\rho)$. Thus, when we move from unobservable health status to partially observable status, the sex worker with $\hat{\rho}$ will strictly prefer to remain uncertified. Therefore, it must be the case that $\hat{\rho} < \hat{\hat{\rho}}$. The threshold above which certification is valuable is higher when health status is partially observable, leading to lower take-up of certification.

When health status is fully *observable* (i.e, $s = 1$), the separating equilibrium condition (2.3) reduces to:

$$\bar{q} - q(\hat{\hat{\rho}}) = \gamma - c(\hat{\hat{\rho}})$$

This condition is satisfied only at the maximal health investment $\bar{\rho}$. All sex workers with $\rho < \hat{\hat{\rho}}$ will choose to remain uncertified, while sex workers with $\rho = \hat{\hat{\rho}}$ are indifferent between remaining uncertified and obtaining certification.

**Result 5.** *As the probability of symptoms increases, the certification premium for those without symptoms falls to 0.*

Given the characterization of $\mu^*(h_0, \sigma_0)$ in (2.3) above, the certification premium for sex workers with no symptoms is:

$$\frac{\bar{q}}{\bar{q} + (1-\bar{q})(1-s)} - \frac{Pr(\theta_H | h_0, \rho \leq \hat{\hat{\rho}})}{Pr(\theta_H | h_0, \rho \leq \hat{\hat{\rho}}) + (1 - Pr(\theta_H | h_0, \rho \leq \hat{\hat{\rho}}))(1-s)}$$

As $s \to 1$, $(1-s) \to 0$, and both terms of the above expression go to 1, and the difference goes to 0.

### 2.2.3 Summary of Model Predictions

The preceding sections first establish that a standard signaling model, with very few assumptions, generates two key theoretical predictions:

1. In any separating equilibrium, there is a price premium for certified sex workers.

2. If the cost of obtaining health certification is small, there is a pooling equilibrium in which all sex workers get certified.

Then, I showed that a model where sex workers' health status is partially observable due to visible STI symptoms generates three results:

3. In any separating equilibrium, sex workers who have visible symptoms earn lower prices, regardless of certification status.

4. In any separating equilibrium, as the probability of symptoms increases, certification take-up falls.

5. As the probability of symptoms increases, the certification premium for those without symptoms falls to 0.

In the remainder of the paper, I show that the evidence is more consistent with a world where health status is partially observable, and clients believe the probability of symptoms $s$ is high. I present experimental evidence that certification take-up is low, which contradicts Result 2 and is consistent with Result 4. I show in both causal and descriptive analyses that there is no evidence for the certification premium described in Result 1; rather, the evidence is consistent with Result 5. And, I show that prices respond to visible STI symptoms, consistent with Result 3.

## 2.3 Study Design

This paper uses data from a randomized field experiment I conducted in Dakar, Senegal, from October 2015 to June 2016. The final sample (after attrition) consists of 291 female sex workers who were uncertified at baseline. Participants were individually randomized via public lottery into treatment or control, and members of the treatment group were offered an encouragement to get certified consisting of (i) a persuasive informational session and (ii) a cash incentive to get certified.

### 2.3.1 Context

The sex worker registration program in Senegal has been in place since 1969. Sex workers can get certified at any public STI clinic in Senegal. In Dakar, the primary STI clinic is housed at the Institut d'Hygiène Sociale (IHS), a public hospital. Any woman can receive treatment at STI clinics, but FSWs must be over the age of 21 to get certified. There are no registration fees; however, each clinic visit carries a fee of 1,000 FCFA ($1.70). Administrative requirements for registration include a one-time interview with a social worker, informed consent, a copy of the national identity card or passport, and 3 passport photos. The registration is valid for one month; thus, FSWs must attend monthly visits to keep their registration current.

The clinic has implemented syndromic case management of STIs, an algorithmic approach based on symptoms observable during a gynecological exam. This approach is considered effective in reducing STI prevalence in female sex workers, and is recommended by the World Health Organization when laboratory testing is not possible (Meda et al., 1999; Shahmanesh et al., 2008; World Health Organization, 2014).[10] If a clinic health care provider diagnoses a curable STI and recommends treatment, certification is suspended until the FSW can show that she has obtained any prescribed medications. STI treatment is not free: FSWs must pay for these treatments in order to keep their registration up to date.

HIV testing is performed annually. After an HIV diagnosis, FSWs are referred to the national HIV treatment program, which provides free medical care and antiretroviral therapy (ART). HIV-positive FSWs may stay certified as long as

[10]Syndromic management is somewhat less effective than laboratory testing, since many STIs are asymptomatic (World Health Organization, 2012; Bekker et al., 2014). However, there are no low-cost laboratory tests for most STIs, with the exception of HIV(World Health Organization, 2011)

they respect the monthly clinic visits. The monthly visits emphasize adherence to HIV treatment; women receiving ART have a very low risk of transmitting HIV to male sexual partners (Cohen et al., 2011).

### 2.3.2 Sampling and Surveys

I implemented the study in partnership with a community-based organization, *Association AWA*, that provides services to certified and uncertified sex workers. From October 2015 to February 2016, participants were recruited using snowball sampling, a peer-referral recruitment method that is frequently used for hidden populations such as sex workers. Thirty one "seeds" were purposively selected among FSWs currently participating in HIV prevention projects with *AWA*. Of these, 21 were certified. These seeds were then asked to recruit three FSWs meeting eligibility criteria for the study. Those recruited were, in turn, be asked to recruit three more FSWs. Recruitment continued through this peer referral until the target sample size was obtained. Following standard practice, participants were compensated 5,000 FCFA ($8.50) for participation in the study and 2,500 FCFA ($4.25) for each recruited participant who was eligible and agreed to enroll in the study. In order to be eligible to participate, sex workers had to be uncertified and eligible for certification: 21 years of age or older and possessing a valid form of identification.[11]

Snowball sampling does not generate a representative sample. However, I consider this the policy-relevant sample for two reasons. Peer outreach is the primary method that service providers use to reach sex workers, and it is recommended by

[11]Other eligibility criteria were: (i) Born female, (ii) Report exchanging sex for money, gifts or goods within the past 6 months, (iii) Declare an intention to stay in Dakar for a period of 3 months, (iv) Mentally sound and capable of giving consent, (v) Speak French, Wolof or both, (vi) Present to the study with a valid recruitment coupon, (vii) Provide informed consent to participate in the study.

the World Health Organization (World Health Organization, 2011). Second, since severe stigma limits mass media promotion of policies affecting FSWs, this sample reflects the population most likely to be affected by policy changes. Consistent with this, in my sample, among those who had previously heard of the registration program at baseline, 86 percent heard about it from another sex worker.

All study activities were conducted at the offices of *Association AWA*.[12] The site is in a discreet location close to a busy market, ensuring that participants could come and go without attracting attention. Notably, the site is also a short walking distance (500m) from the primary registration clinic in Dakar, *Institut d'Hygiene Sociale* (IHS), so time and travel costs associated with study participation are roughly equivalent to time and travel costs associated with certification.

Following standard procedure for snowball sampling, participants completed informed consent and a baseline survey during their initial study visit. They returned two to four weeks later for randomization, and finally they completed an endline survey, generally one to three months after randomization.[13] Female sex workers are a highly mobile population that is difficult to track. We therefore allowed participants to complete missed visits whenever possible; as a result, there is some variation in time between visits. In total, from October 2015 to February 2016, we recruited and conducted baseline surveys for 400 female sex workers. Of these, 314 participants were eligible and randomized.[14] Between December 2015 and June 2016, 291 participants completed endline surveys, representing a 93 percent retention rate from randomization to follow-up.

[12]The use of an established community-based organization follows World Health Organization recommendations for network-based sampling(World Health Organization, 2011)

[13]A small number of participants completed their endline surveys 4+ months after randomization after missing their initial appointments.

[14]85 participants were lost to follow-up. One participant was determined to have been ineligible (under age 21) during data analysis.

### 2.3.3 Randomization

There are two groups in the study, treatment and control. Randomization was implemented via public lottery in a series of randomization sessions. After completing the baseline survey, participants received a randomization appointment two to four weeks later, which allowed them time to recruit other participants before randomization. We scheduled randomization sessions in groups of at least 10; however, in practice, due to missed appointments, randomization occurred in groups ranging from 3 to 25 participants.[15]

There were 33 randomization sessions over the course of approximately 4 months. The sessions were run by one member of the study staff and two trained peer educators. Then, participants drew their group assignment by drawing a colored ball from a sack. In total, 164 participants were randomized to treatment and 151 to control.[16] In all analyses, I account for varying treatment probabilities in each session (due to odd numbers of participants) using weighted regression (Gerber and Green, 2012). Each observation $i$ in session $j$ is weighted by

$$w_{ij} = \frac{T_i}{p_j} + \frac{1 - T_i}{1 - p_j} \tag{2.4}$$

where $T_i$ is the treatment assignment and $p_j$ is the proportion of subjects assigned to treatment in session $j$.

As specified in a pre-analysis plan, I conduct all analyses on the sample that completed the entire study (n=291). Appendix 2.8.1 shows that there is no

[15]The final participant to be randomized was randomized alone. She drew her group assignment in the presence of study staff and peer educators to preserve the concept of a public lottery.

[16]In general, we included an even number of balls in the sack to ensure that participants had a 50 percent probability of being selected for treatment. Thus, despite the varying sizes of the randomization sessions, the ex ante assignment probability was constant. There is one exception to this: due to a miscommunication with the implementer, on December 14, approximately 75 percent of participants (14 of 18) were randomized to treatment instead of 50 percent.

differential attrition across treatment and control groups. Appendix 2.8.2 shows balance across treatment and control groups. I evaluate balance in the analysis sample on 27 variables specified in a pre-analysis plan, using weighted regressions. There is a significant difference between treatment and control on one variable: whether any transactions were completed in a public place. Since the randomization was not stratified, this is consistent with chance, and I control for the unbalanced variable in evaluating treatment effects.

### 2.3.4 Encouragement

An encouragement intervention was delivered at the randomization session to members of the treatment group. The encouragement intervention involved two parts. First, the treatment group participated in a 30-minute persuasive discussion with the peer educators, who were certified sex workers. They shared their experience with the registration program, discussed the benefits of certification, and answered questions raised by participants. Those who indicated during the randomization session that they wished to register were given an appointment the following day for a peer educator to accompany them to the registration clinic and answer any questions about the process.

The second part of the encouragement was a cash incentive to register. We offered a total of 6,000 FCFA($10.20) to anyone who got certified within 15 days. This amount was designed to provide reimbursement of the 1,000 FCFA ($1.70) clinic visit fee and 5,000 FCFA($10.20) to the sex worker. In order to claim the incentive, participants had to return to the study offices and show their registration card to study staff. The study offices were open late and were a short walking distance (500m) from the primary registration clinic, so claiming the incentive did not require additional travel.

The randomization sessions proceeded as follows. First, prior to drawing their treatment assignment, all participants received basic, neutral information about the registration program, their legal rights in Senegal, and a brief education module on sexually transmitted infections. Following the public lottery, participants were divided by treatment, and trained peer educators delivered the encouragement intervention to the treatment group. Control group participants were reminded about compensation for the follow-up survey, and then allowed to leave.

## 2.4 Data and Estimation Strategy

### 2.4.1 Data

The baseline and endline surveys collected individual-level data and transaction-level data. The individual-level data covered demographics, income and labor supply, sex work history including number of clients and transactions, legal history and knowledge of the registration program, self-reported STI symptoms and health-seeking behavior, and alcohol and drug use. In addition, I collected detailed data on the five most recent sex transactions, including type of sex acts exchanged, price, numerous client characteristics, risk expectations, substance use by the sex worker and the client, whether violence was perpetrated, how the transaction was arranged, and where the transaction took place. Two versions of the endline survey were used. The first version of the survey was very similar to the baseline survey (except time invariant questions). We completed 54 endline surveys using this version. After it became clear that take-up of the incentive would almost certainly be very low, I added 21 questions to the survey to help understand the mechanisms for low take-up of the incentive. The modified survey was implemented beginning in February 2016 and was used for 238 participants. Since the large majority of

participants are illiterate and given the sensitive nature of the data collected, surveys were administered by female enumerators with significant experience working with female sex workers in Senegal. All surveys were conducted in a private room with a closed door, and enumerators used a low tone of voice to maximize privacy.

### 2.4.2 Baseline Characteristics

Table 2.1 shows summary statistics for the sample. Although the sampling procedure was not designed to generate a representative sample, participants in the study were similar to other samples of female sex workers in Sub-Saharan Africa, which are also typically convenience samples (Scorgie et al., 2012). Mean age is 37, mean education is 3.14 years, the majority of women are divorced, and they have on average 2.11 children under 18, reflecting the circumstances that typically precipitate sex work in sub-Saharan Africa. The participants appear to be well established in sex work, with a mean 8.36 years in sex work. 91 percent consider sex work their "main job" over the past 6 months. Moreover, 83 percent were aware of the registration program at baseline, suggesting that lack of knowledge about the program is unlikely to be a significant barrier.

At the median, income from sex work accounts for 83 percent of total monthly income. Median monthly income is 90,000 FCFA ($153.06). The 5,000 FCFA incentive I offered for registration is thus approximately 1/18th of median monthly income, or just over a day's wage. The 1,000 FCFA clinic visit fee associated with registration is just over 1/5 of a day's wage.

Finally, given that one of the stated benefits of the registration program is protection from prosecution, it is worth considering the implications of remaining uncertified for interactions with the legal system. It appears that even uncertified sex workers are largely able to operate without interference from police. At baseline,

**Table 2.1**: Sample Demographics and Sex Work History

| | Mean | S.D. | Med. | Min. | Max. | Obs. |
|---|---|---|---|---|---|---|
| Age | 37.4 | 8.89 | 37 | 21 | 59 | 290 |
| Divorced | 0.63 | 0.48 | 1 | 0 | 1 | 291 |
| Senegalese | 1.00 | 0.059 | 1 | 0 | 1 | 291 |
| Muslim | 0.98 | 0.14 | 1 | 0 | 1 | 291 |
| Years of education | 3.11 | 3.55 | 2 | 0 | 14 | 291 |
| Children under 18 | 2.12 | 1.72 | 2 | 0 | 10 | 288 |
| Years in sex work | 8.37 | 7.09 | 6 | 0 | 32 | 289 |
| Main job is sex work | 0.91 | 0.28 | 1 | 0 | 1 | 289 |
| Ever heard of registration program | 0.83 | 0.38 | 1 | 0 | 1 | 290 |
| Hours in sex work, past week | 6.92 | 8.77 | 5 | 0 | 84 | 291 |
| Number of clients, past month | 13.2 | 16.6 | 8 | 0 | 144 | 290 |
| Income from sex work, past month (FCFA, thousands) | 95.4 | 91.6 | 75 | 0 | 700 | 289 |
| Total income, past month (FCFA, thousands) | 109.9 | 93.0 | 90 | 0 | 700 | 289 |
| Arrested, past month | 0.034 | 0.18 | 0 | 0 | 1 | 290 |
| Jail, past month | 0.0069 | 0.083 | 0 | 0 | 1 | 290 |

This table presents summary statistics at baseline for 291 female sex workers comprising the analysis sample (participants who satisfied eligiblity criteria and completed the baseline survey, randomization, and endline survey). Responses of *Don't know* and refusals to respond are coded as missing. FCFA is the currency of Senegal. During the study period, the exchange rate was approximately 588 FCFA = $1.

only 3.4 percent of the sample reported a recent arrest or detention, and less than 1 percent (two participants) reported recent jail time. Among those who were arrested or in jail, only 1 participant got certified, suggesting that the legal benefits of the program are nominal.

Table 2.2 shows summary statistics for the transaction-level data. The market for sex work in Senegal can be divided into two broad sectors: a local market and a sex tourism market. In the latter market, FSWs typically solicit in expensive hotels, bars and clubs, and report earnings of more than 50,000 FCFA ($85.03) per day (Homaifar and Wasik, 2005; Agence Nationale de la Statistique et de la Demographie, 2013). In the local market, in contrast, earnings are lower, and transactions are typically arranged through street soliciting, illegal brothels, or mobile phones (Do Espirito Santo and Etheredge, 2004; Homaifar, 2006).

There is qualitative evidence that women participating in the local market are less likely to be certified (Homaifar and Wasik, 2005), and indeed my sample appears to operate almost exclusively in the local market. The median transaction price is 5,000 FCFA, and 72 percent of transactions reported were carried out with regular clients, while fewer than 1 percent of transactions were completed with foreign clients. Clients asked about registration in just 11 percent of transactions. Some features of the transactions suggest a need for safer working conditions. Although anal sex is uncommon in this context, occurring in 6.7 percent of transactions, unprotected sex was reported in 17 percent of transactions. Participants report violence, which included violent threats, physical violence, sexual violence, and forced unprotected sex, in 5.1 percent of transactions.

Finally, Table 2.3 reports summary statistics on STIs and health care use at baseline. The women in the sample appear to have a high level of access to health care outside the registration program. In the sample as a whole, 21 percent

**Table 2.2**: Transaction Characteristics

| | Mean | S.D. | Med. | Min. | Max. | Obs. |
|---|---|---|---|---|---|---|
| Transaction price (FCFA, thousands) | 10.0 | 15.6 | 5 | 0 | 400 | 1453 |
| Regular client | 0.72 | 0.45 | 1 | 0 | 1 | 1440 |
| Foreign client | 0.0083 | 0.091 | 0 | 0 | 1 | 1438 |
| Unprotected sex | 0.17 | 0.37 | 0 | 0 | 1 | 1455 |
| Anal sex | 0.067 | 0.25 | 0 | 0 | 1 | 1455 |

This table presents summary statistics for transaction-level data collected during the baseline survey from 291 female sex workers comprising the analysis sample (participants who satisfied eligiblity criteria and completed the baseline survey, randomization, and endline survey). Each sex worker answered a series of questions about each of her last five sex transactions, generating a sample of 1,455 transactions. Responses of *Don't know* and refusals to respond are coded as missing. FCFA is the currency of Senegal. During the study period, the exchange rate was approximately 588 FCFA = $1. Client violence is equal to 1 if sex workers reported any of the following: threat of physical violence, physical violence, sexual violence, forced unprotected sex, threatened to report the sex worker to police.

**Table 2.3**: STIs and Health Care Use

| | Mean | S.D. | Min. | Max. | Obs. |
|---|---|---|---|---|---|
| Saw a doctor for STI symptoms, past month | 0.21 | 0.41 | 0 | 1 | 291 |
| If had STI symptoms, saw a doctor | 0.81 | 0.40 | 0 | 1 | 77 |
| Saw a doctor for a routine exam | 0.29 | 0.46 | 0 | 1 | 290 |
| Saw a doctor for any reason, past month | 0.44 | 0.50 | 0 | 1 | 291 |

This table presents summary statistics at baseline for 291 female sex workers comprising the analysis sample (participants who satisfied eligiblity criteria and completed the baseline survey, randomization, and endline survey). Responses of *Don't know* and refusals to respond are coded as missing.

saw a doctor for recent STI symptoms, which represents 80 percent of those who had recent STI symptoms. 29.2 percent reported a well visit in the past month at baseline. Overall, 44 percent of the sample reported a doctor visit in the past month. Registration does not act as a gateway to treatment. These data support the assumption in Section 2.2 that for a large proportion of the sample, the health care costs associated with registration are similar to health care costs they were incurring anyway, adding to the puzzle of low take-up.

### 2.4.3 Estimation Strategy and Identification

I begin by estimating the intention-to-treat (ITT) effect of the encouragement (persuasive information and incentive) on take-up of registration, using the following estimating equation:

$$D_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + v_i \tag{2.5}$$

where $D_i$ is registration since baseline, $T_i$ is the treatment assignment, and $X_i$ is a vector of variables with baseline imbalances (see Section 2.3.3). In all analyses of take-up, all observations are weighted by $w_{ij}$, defined in (2.4), to account for varying treatment assignment probabilities in different randomization sessions.

I then examine evidence for certification effects by studying differential price changes over time for individuals who got certified:

$$p_{tir} = \alpha_i + \beta_1 Certified_{tir} + \lambda_r + \lambda_r \times T + \epsilon_{tir} \tag{2.6}$$

where $Certified_{tir}$ is the registration status of individual $i$ during transaction $t$ and round $r$. In this analysis, I am pooling individuals who got certified in the treatment and control groups in order to improve power. As a result, $\beta_1$ is not

causally identified. Nevertheless, I analyze whether the results are consistent with the existence of a certification premium. In Section 2.5.2, I argue that $\beta_1$ is likely an upper bound.

To understand the results on the certification premium, I estimate the effect of recent STI symptoms on prices using the following estimating equation:

$$p_{tir} = \alpha_i + \beta_1 STI_{tir} + \lambda_r + \lambda_r \times T + \epsilon_{tir} \tag{2.7}$$

where $p_{tir}$ is the price of transaction $t$, individual $i$, in round $r \in (1,2)$ and $STI_{ir}$ is an indicator for having an STI episode during the past month in round $r$. I include individual fixed effects $\alpha_i$, round fixed effects $\lambda_r$, and differential trends for treatment status ($\lambda_r \times T$) in all specifications. The sample is restricted to transactions conducted in the past month to match the observation period for STIs.

## 2.5 Results

### 2.5.1 Impact of Certification Incentive

The encouragement had a statistically significant, but small, effect on take-up (Table 2.4). When controlling for baseline covariates, the incentive increased take-up by only 4.8 percentage points, relative to a 2.84% take-up rate in the control group (Column 3). This effect was much smaller than expected: based on preparatory discussions with the implementing partner and members of the sex worker community, I expected take-up above 50%.

The effect size is small relative to similarly sized incentives for health programs in other contexts. For example, Thornton (2008) offered incentives for individuals who had undergone HIV testing in Malawi to go pick up their HIV test

**Table 2.4**: Impact of Certification Incentive

| *Dependent Variable:* | Registration Take-up | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treatment | 0.0450* | 0.0458* | 0.0478* |
| | (0.0256) | (0.0262) | (0.0266) |
| Constant | 0.0284** | 0.0282* | 0.0286** |
| | (0.0140) | (0.0144) | (0.0145) |
| Baseline controls | No | No | Yes |
| Weighted | No | Yes | Yes |
| Observations | 291 | 291 | 291 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The dependent variable is an indicator for obtaining certification between baseline and follow-up. *Treatment* is an indicator for assignment to the treatment group, which received (i) an informational intervention designed to be persuasive and (ii) an incentive to obtain certification. *Baseline controls* indicates inclusion of variables that were imbalanced at baseline due to chance. *Weighted* indicates inclusion of regression weights to account for randomization strata, as defined in equation (2.4).

results. In that study, an incentive equal to one-tenth of a day's wage increased take-up by approximately 30 percentage points, while a day's wage increased take-up by nearly 50 percentage points. In India, in-kind incentives with a value roughly equal to the opportunity cost of time associated with participating in a series of immunization camps (roughly two day's wages) increased immunization coverage by nearly 20 percentage points (Banerjee et al., 2010). I can rule out effects of this magnitude: the upper end of the confidence interval on the coefficient in Table 2.4, Column 3 is 10 percentage points.

Notably, the incentive amount of 5,000 FCFA was equal to the reimbursement given for participation in surveys. Transportation costs to the study site and registration site are essentially equivalent: the study site is located only 500m from the registration clinic. In practice, the time costs of surveys and registration were also similar. Registration takes about half a day. And while the actual survey took

less than an hour, most participants arrived at the study site in groups and waited while everyone in the group completed their study activities, spending at least half a day at the study offices. Thus, for those who returned for the endline survey, the incentive must have been adequate to compensate for time and transportation costs associated with registration. This implies that the incentive successfully reduced the monetary costs of registration to zero (if not negative).

### 2.5.2 Certification Premium

Extremely low take-up in the experiment, despite low monetary costs of certification, contradicts the theoretical prediction of the standard signaling model in Section 2.2 (Result 2). That prediction followed from result 1, that there is a price premium for certified sex workers. I test this prediction in this section.

In Table 2.5, I investigate evidence for a certification premium among the sex workers in my study who did register. First, in Column 1, I show the causally identified intention-to-treat (ITT) effect, comparing prices in the treatment and control groups at follow-up. This difference is negative and very small, indicating no causal impact of the treatment on prices. However, this is unsurprising: 93 percent of the treatment group is not certified.[17] Therefore, I turn to a descriptive analysis of differential, within-individual price changes over time for those who got certified, at the transaction level. Again, there is no significant difference in prices

[17]Clearly, the causal parameter of interest is the local average treatment effect (LATE) of certification on those who were induced to register by the encouragement. In principle, this object can be estimated through two-stage least squares, where treament assignment is used as an instrument for registration status. However, this estimate suffers from a weak instrument problem (Bound et al., 1995), because the compliance differential between treatment and control groups is just 4.5 percentage points, and it is significant only at the 10 percent level. The "first-stage" F-statistic is just 2.79, where a sufficiently strong instrument should have an F-statistic above 10 (Stock and Yogo, 2005). Because of the weak instrument, even very small direct effects that the encouragement may have had on prices, such as income effects, are amplified (Bound et al., 1995), and the estimate of the LATE is inconsistent. Therefore, I do not present LATE estimates.

**Table 2.5**: Certification Premium

| *Dependent Variable:* | Transaction Price | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | ITT | FE | FE | FE |
| Treatment | 20.34 | | | |
| | (919.6) | | | |
| Actively registered | | 985.4 | 175.3 | -428.9 |
| | | (840.2) | (1042.7) | (1383.7) |
| Constant | 9315.1*** | 9841.9*** | 11066.4*** | 9449.2*** |
| | (606.6) | (291.8) | (807.6) | (1780.4) |
| Round FE | No | Yes | Yes | Yes |
| Treatment controls | No | Yes | Yes | Yes |
| Month fixed effects | No | No | Yes | Yes |
| Sex act controls | No | No | No | Yes |
| Individual FE | No | Yes | Yes | Yes |
| Observations | 1224 | 2588 | 2588 | 2583 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at individual level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey to maintain a comparable sample with Table 2.6. *Treatment* is an indicator for assignment to the treatment group, which received (i) an informational intervention designed to be persuasive and (ii) an incentive to obtain certification. *Actively registered* is an indicator for the sex worker having a valid registration when the transaction was completed. FE indicates individual fixed effects.

for actively certified sex workers (Column 2), regardless of controls for time (Column 3). The raw differential change in prices for certified FSWs is positive, but small in magnitude. Given a mean price of 10,342 FCFA ($17.59) in the sample, the point estimate in Column 2 indicates that actively certified sex workers earned 9 percent more than uncertified sex workers. When I control for month fixed effects, the estimate falls in magnitude (Column 3). In the preferred specification controlling for type of sex exchanged and month fixed effects, the estimate is actually negative (Column 4). Registration is associated with a *discount* of approximately 3 percent. As previously noted, these estimates are descriptive. However, we should expect these estimates to represent an upper bound on the true certification premium. I show in Section 2.6 below that the women who got certified are "health-seeking" types who experienced a low-probability STI shock prior to certification. Therefore, reversion to the mean suggests that we should expect a differential increase in prices for these women. Combined with the fact that clients asked about registration in only 11 percent of transactions (Table 2.1), these results are most consistent with the conclusion that there is no certification premium in this market.

If we consider the model in Section 2.2, the price premium for certified sex workers follows from the assumption that health status is unobservable. If, however, health status is partially observable, the model implies that we may observe a low certification premium and low take-up. To obtain support for this interpretation of the results, I test the key prediction of the a model with partially observable health status: that sex workers with observable STI symptoms will earn lower prices.

Table 2.6 shows the association of prices with STI symptoms in the past month. First, recent STI symptoms are negatively but insignificantly associated with prices (Column 1). In the remainder of the table, I disaggregate by recent STI episodes that included visible symptoms (i.e., observable by the client), and those

**Table 2.6**: Prices and STIs

| *Dependent Variable:* | Transaction Price | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Any STI | -1101.2 | | | | | |
| | (1404.8) | | | | | |
| Visible STI | | -2149.1** | -2048.6** | -1828.5* | -2131.9** | -2148.6** |
| | | (974.7) | (1007.2) | (1023.2) | (979.4) | (985.7) |
| Invisible STI | | | | 2276.1 | | |
| | | | | (3991.1) | | |
| Actively registered | | | | | 442.2 | 328.3 |
| | | | | | (829.7) | (777.5) |
| Actively registered × Visible STI | | | | | | 561.8 |
| | | | | | | (1990.7) |
| Constant | 11350.8*** | 10291.8*** | 10164.3*** | 10083.4*** | 10288.3*** | 10291.8*** |
| | (869.3) | (391.5) | (1763.7) | (1876.3) | (389.3) | (393.9) |
| Month fixed effects | Yes | No | Yes | Yes | No | No |
| Sex act controls | No | No | Yes | Yes | No | No |
| Observations | 2577 | 2588 | 2583 | 2583 | 2588 | 2588 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at individual level. Analysis is conducted at the transaction level using data collected at baseline and endline. The sample is restricted to transactions in the month prior to the survey. At baseline and endline, sex workers reported up to three recent STI episodes. *Any STI* is an indicator for reporting an episode of STI symptoms in the past month. STI episodes are classified as either visible or invisible. *Visible STI* is an indicator for an episode with visible symptoms, and *Invisible STI* is an indicator for an episode with no visible symtpoms. *Actively registered* is an indicator for the sex worker having a valid registration when the transaction was completed. Individual and round fixed effects and controls for treatment group are included in all regressions.

that included *only* invisible symptoms.[18] Recent *visible* episodes are significantly associated with a reduction in prices (Column 2). After controlling for type of sex act exchanged (Column 3), women earned, on average, 1918.8 FCFA ($3.26) less when they had a visible STI episode than when they did not. This corresponds to a 19 percent discount relative to the mean price. Column 4 shows that invisible episodes are not significantly associated with prices. Their inclusion substantially increases the standard errors; however, the point estimate on invisible episodes is actually positive. In Column 5, I compare the effect of a visible STI episode to the effect of registration. When controlling for visible STI, the estimate on registration falls even further relative to Column 3 of Table 2.5, suggesting a roughly 5 percent discount for certified FSWs. The coefficient on visible STI episodes remains significant, and is nearly 4 times the magnitude of the coefficient on registration.

The result that prices respond to episodes that are observable by the client, but do not respond to episodes that are observable only to the sex worker, strongly suggests a demand-driven explanation for the association between prices and STI symptoms.[19] The results are most consistent with the hypothesis that client willingness-to-pay responds to the presence of STIs, implying that STIs are partially observable.

Returning to our model, these results indicate that visible STI symptoms

[18]Visible symptoms were unusual or foul-smelling vaginal discharge and warts or sores in the genital area. Invisible symptoms were genital irritation, lower abdominal pain, and burning during urination. An episode is classified as visible if it inclued any visible symptoms. It is classified as invisible if it had invisible symptoms only. I collected these data on up to 3 episodes occuring in the past month.

[19]The most obvious alternate story for an association between STI and reduced prices is that an STI is a health shock, and sex workers respond by expanding their labor supply. This is known as the "income smoothing" hypothesis, and empirical support for this has been found in other African contexts (Robinson and Yeh, 2011). In Appendix 2.8.3, I show that total quantity exchanged in a month is lower when sex workers have STI symptoms, which suggests a limited labor supply response. More importantly, in the case of income smoothing, prices should respond to any STI episode, not just visible episodes as we see here.

reveal the health status to be $\theta_S$. This suggests that, in fact, we should not expect any impact of registration on prices for those who have visible symptoms. However, there may be an effect among those who do not. In Column 6 of Table 2.6, I explore this implication by studying the interaction of registration status and visible STIs, with minimal controls to maximize power. The coefficient of interest is the uninteracted term for registration status, which represents the within-individual effect of registration when FSWs did not have visible STIs. The coefficient on registration status remains insignificant, and is smaller in magnitude than the coefficient in Column 1. The coefficient on the interaction term is also not significant, suggesting that registration has no impact on the price response to visible STI symptoms.

The results in Table 2.6 suggest that health status is at least partially observable. In Table 2.7, I perform an additional test to explore whether there is important asymmetric information remaining in the market. That is, what are clients beliefs about the value of $s$, the probability that visible symptoms will appear conditional on STI infection? Using a probit model, I predict STI status based on observable characteristics of the sex worker, such as age, area of residence, or where the transaction was arranged.[20] Clients may use these characteristics as signals of STI risk. Column 1 shows that this prediction is highly correlated with actual STI status.

If clients are concerned about invisible STIs, we would expected the predicted risk to matter even when controlling for visible STI symptoms. In Columns 2-4, I study correlations between predicted STI risk and prices in the pooled sample, without individual fixed effects. Column 2 shows that, although the coefficient is

[20]The full list of variables used in the prediction is: age, area of residence, region of birth, ethnic group, education, religion, whether the FSW has a manager or pimp, where the transaction was arranged, where the transaction took place, sex worker alcohol use, and sex worker drug use.

**Table 2.7**: Prices and Predicted STI Risk

| | (1) Any STI | (2) Price | (3) Price | (4) Price |
|---|---|---|---|---|
| Predicted STI risk | 0.962*** | -2624.0 | -674.4 | -806.0 |
| | (0.118) | (1746.2) | (2147.5) | (2138.7) |
| Visible STI | | | -2455.7*** | -2393.2** |
| | | | (941.1) | (933.3) |
| Invisible STI | | | | 688.9 |
| | | | | (3940.4) |
| Constant | 0.000534 | 10676.3*** | 10772.2*** | 10760.0*** |
| | (0.0240) | (1195.0) | (1065.4) | (1133.5) |
| Month fixed effects | No | Yes | Yes | Yes |
| Observations | 2580 | 2588 | 2588 | 2588 |

Bootstrapped standard errors in parentheses, clustered at individual level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Round fixed effects included in all regressions.

Sample restricted to transactions in the month prior to survey.

not significant, predicted STI risk is associated with a large reduction in prices. However, when I control for visible STI episodes (Column 3), the residual effect of predicted risk is much smaller in magnitude. Column 4 controls for both visible and invisible STI episodes, which does not change the results.

The results in this section suggest that from the client's perspective, the most important information about health status is observable, and as a result, certification is not valued in the market. There is no evidence for a statistically or economically significant price premium for certified sex workers, and there is no evidence that clients use other signals to make inferences about unobservable health status.

### 2.5.3 Discussion: HIV and Asymmetric Information

A key result of this paper is that clients do not appear to value information about unobservable STIs. This may seem surprising, given that the most well-known unobservable STI is HIV. This section gives an overview of the HIV epidemic in Senegal, considers several possible explanations for why the results are reasonable even in the context of HIV, and evaluates the plausibility of these different explanations.

It is important to note that Senegal does not have a generalized HIV epidemic: general population prevalence is 0.5 percent, and has been stable or declining for over a decade (UNAIDS, 2015).[21] New HIV infections among young people are also declining (UNAIDS, 2012). Senegal was the first African country to provide free access to antiretroviral therapy (ART), beginning in 2001 (Desclaux et al., 2003). Nevertheless, female sex workers are a high risk population. HIV prevalence estimates among female sex workers in Senegal range from 3.3 percent to as high as 19.9 percent (Lyons et al., 2017; Baral et al., 2012).[22] While there are no good estimates of prevalence among clients, in one convenience sample of 1,083 clients in Dakar, 4 percent of participants tested positive for HIV. Thus, while overall prevalence is low, it may be difficult to argue that HIV is an insignificant concern for female sex workers and their clients.

Even so, there are several reasons that clients may not value health certifica-

[21] UNAIDS defines an HIV epidemic as "generalized" if 1-5 percent of pregnant women attending antenatal clinics are HIV positive. In generalized epidemics, the majority of new infections occur in the general population, rather than specific high-risk groups, such as sex workers or men who have sex with men(UNAIDS, 2016). HIV prevalence levels in Senegal contrast starkly with those of hyperendemic countries in Eastern and Southern Africa, where HIV prevalence is 7 percent on average, and exceeds 20 percent in some countries.

[22] It is useful to compare these numbers with estimates in hyperendemic countries: for example, HIV prevalence among female sex workers is 45.1 percent in Kenya, 70.7 percent in Malawi, 59.6 percent in South Africa, and 60.1 percent in Zimbabwe (Baral et al., 2012).

tion. First, it is possible that clients do not know that HIV is often unobservable; they may even believe that symptoms of curable STIs are, in fact, symptoms of HIV. While I do not have data to directly measure client beliefs, we might expect some correlation between client beliefs and sex worker beliefs. In my sample, at baseline, 91 percent of sex workers knew that HIV can be asymptomatic.[23] Moreover, sex workers correctly answered this question more often than any of six other HIV knowledge questions asked. Since sex workers are targeted for HIV-related education in Senegal, this is likely an upper bound on client HIV knowledge; nevertheless, given these results, it seems unlikely that clients have massively misunderstood the extent to which HIV is observable.

Another possibility is that because of longstanding ART access in Senegal, clients perceive the cost of HIV infection to be relatively small. de Walque et al. (2012) show that beliefs about the effectiveness of ART predict risky sexual behavior in Mozambique, and studies have documented increases in risky sexual behavior in response to ART access among men who have sex with men in San Francisco and adolescent girls in Kenya (Mechoulan, Mechoulan; Friedman, 2015). These results strongly suggests that ART availability has reduced the perceived cost of HIV infection in several contexts. On the other hand, even in the presence of ART, HIV remains a chronic illness requiring active management. Moreover, the costs of HIV infection are not limited to its health effects; HIV remains a highly stigmatized illness. In Senegal in 2014, 51.7 percent of adults reported discriminatory attitudes towards people living with HIV (UNAIDS, 2015).

Finally, clients may believe that the risk of HIV infection from sex transactions is low. Again, this belief could be related to the longstanding program providing free access to ART. With sufficient adherence, ART reduces the proba-

[23]In particular, they agreed with the statement, "A person may have HIV even if they seem to be in good health."

bility that a person with HIV will transmit the virus by 96 percent (Cohen et al., 2011). Despite the fact that the preventive value of ART was shown in the medical literature only recently, 75.5 percent of the sex workers in my sample knew that HIV treatment can reduce the probability of transmitting the virus.[24] These beliefs may actually predate the confirmation from the medical literature: using data from 2008 and 2010, Baranov et al. (2013) find that, among HIV-negative individuals in Malawi, ART availability reduced the subjective probability of HIV infection. The combination of low general population prevalence and longstanding ART access may create the perception that the risk of HIV infection in the sex market in Dakar is quite low, and the most salient risk is a curable STI.[25]

### 2.5.4 Costs of Certification

The analysis up to this point provides a clear explanation for low take-up of Senegal's sex worker registration program: both the stated advantage of legalization and the theoretical price premium to certified sex workers are limited in practice. Given limited benefits of registration from the sex worker's perspective, it is reasonable to expect that time, travel, and hassle costs would generate low take-up.

However, in my experiment, I offered a financial incentive that should more than cover minor costs associated with certification. Thus, with the incentive, the costs of registration should be zero or even negative. Even if benefits of registration are low, we should expect high take-up of the incentive. Yet, a strikingly large

[24]This is measured by agreement with the statement, "A woman with HIV can take medicine that reduces the risk of transmittng the virus."

[25]It is also worth noting that the reported rate of condom use is quite high: unprotected vaginal or anal sex is reported in only 14.9 percent of transactions, and I observe only one unprotected sex act with a certified sex worker. It may be the case that clients who are most concerned about HIV infection use condoms consistently.

**Table 2.8**: Study Comprehension

| | Mean | S.D. | Min | Max | Obs |
|---|---|---|---|---|---|
| Inf. WTA | 0.44 | 0.50 | 0 | 1 | 228 |
| WTA $\mid < \infty$, FCFA ($) | 34,525 | 82,530 | 0 | 500,000 | 121 |
| | (58.72) | (140.36) | (0.00) | (850.34) | |
| Understood incentive offer | 0.98 | 0.13 | 0 | 1 | 119 |
| Stated incentive amount | 0.53 | 0.50 | 0 | 1 | 118 |

number of women refused registration even when offered this incentive. This suggests that although the monetary costs associated with certification are low, there may be other costs that my model did not account for. Referring back to the model in Section 2.2, I hypothesized that the treatment made the cost of registration $\gamma$ zero or negative. However, low take-up in the experiment suggests that $\gamma$ is higher than hypothesized. This section further explores the costs of registration, documents substantial resistance to registration in my sample, and provides evidence contrary to the hypothesis of low certification costs.

First, one potential reason for low take-up of the incentive to register is mismanagement: perhaps the participants did not understand the incentive offer. I rule this out as the primary explanation using two pieces of evidence: stated willingness-to-accept (WTA), and study comprehension questions included in the endline survey.[26] Table 2.8 presents summary statistics for these questions.

The WTA data indicate very significant resistance to registration in the sample. Nearly half (44 percent) of the sample said there was no incentive amount that would convince them to register (i.e., their WTA is infinite). While the question was not incentivized, there is no clear reason that subjects would have an

[26]The questions were included only in version 2 of the endline survey, as explained in Section 2.4. Stated willingness-to-accept is the answer to the question, "What is the minimum incentive you would accept to register?" The comprehension questions were included only for members of the treatment group. In total, 118 subjects were asked this question; 3 members of the treatment group were not asked because their treatment assignment was mismarked on the survey.

incentive to falsely state an infinite WTA. In addition to this, mean WTA among those with a finite WTA was 34,566 FCFA, which is nearly seven times the incentive I offered, and represents 31 percent of mean monthly income in the sample with WTA data.

The study comprehension questions provide further evidence that the incentive offer was successfully communicated. Nearly all of the treatment participants understood that the study offered them an incentive for registration (Table 2.8, Panel 2). Approximately 52 percent were able to correctly state the incentive amount when asked (Table 2.8, Panel 3). This is a bit lower than anticipated; however, this response is highly correlated with having an infinite WTA (last row of Table 2.8). This result is more consistent with bounded attention than mismanagement: individuals who would never register did not pay attention to the details of the incentive offer.

Therefore, I explore substantive reasons for the resistance to registration in the sample. First, I present summary statistics on participants' stated reasons for remaining uncertified, disaggregated by finite or infinite WTA (Table 2.9).[27] Column 1 shows means and the number of participants stating the reason for finite WTA, while Column 2 shows the same for infinite WTA. The most commonly stated reasons, by far, are related to stigma and fear that their status as a sex worker will be revealed. Regardless of WTA, the top two reasons are fear that someone will find the registration card; and fear of being seen at the clinic, which is known to serve vulnerable populations such as sex workers. The third most commonly cited reason in both groups is related to self-stigma: not wanting to assume a "sex worker" identity. Column 3 shows the difference between Columns 1 and 2, along with p-values from a simple t-test of the difference. Both fear that

[27]The responses were not mutually exclusive: participants could give more than one reason

**Table 2.9**: Reasons for Remaining Uncertified

| | (1) WTA$< \infty$ mean(n) | (2) WTA$= \infty$ mean(n) | (3) Difference (1)-(2) | p-val. |
|---|---|---|---|---|
| Clinic visits are too expensive | 0.055 | 0.040 | 0.014 | 0.62 |
| | (7) | (4) | | |
| STI treatment is too expensive | 0.0078 | 0 | 0.0078 | 0.38 |
| | (1) | (0) | | |
| Clinic is too far away | 0.031 | 0.010 | 0.021 | 0.28 |
| | (4) | (1) | | |
| Clinic visits take too long | 0.016 | 0.010 | 0.0055 | 0.72 |
| | (2) | (1) | | |
| Clinic hours are not convenient | 0.047 | 0.010 | 0.037 | 0.11 |
| | (6) | (1) | | |
| Afraid of HIV test | 0.016 | 0.020 | -0.0046 | 0.79 |
| | (2) | (2) | | |
| Afraid someone will see me at the clinic | 0.45 | 0.48 | -0.040 | 0.55 |
| | (57) | (48) | | |
| Afraid someone will find the health card | 0.55 | 0.74 | -0.19 | 0.00*** |
| | (70) | (73) | | |
| Afraid of police harassment | 0.047 | 0.081 | -0.034 | 0.29 |
| | (6) | (8) | | |
| Clinic personnel do not treat me with respect | 0.062 | 0.10 | -0.039 | 0.29 |
| | (8) | (10) | | |
| Do not know how to register | 0.11 | 0.010 | 0.099 | 0.00*** |
| | (14) | (1) | | |
| Do not want to assume "sex worker" identity | 0.11 | 0.39 | -0.28 | 0.00*** |
| | (14) | (39) | | |

someone will find the card and self stigma are significantly more common among participants who state an infinite WTA.

It is clear from the summary statistics that concerns related to stigma and confidentiality are paramount, but it is also useful to understand the relative importance of different aspects of stigma. I divide stigma concerns into three categories: community stigma, self stigma, and direct confidentiality. Community stigma refers to the cost of community members learning the subject's status as a sex worker. Self stigma, in contrast, refers to the cost of the subject admitting to herself that she has a stigmatized identity. Finally, direct confidentiality concerns are related to the confidentiality of the information provided during registration and the registration list. It may be that FSWs do not trust the government clinic to safeguard registration records.

I find that self stigma is the most important predictor of certification refusal. I construct summary indices of variables related to each type of stigma cost, following Anderson (2008b), and evaluate associations between each index and infinite WTA.[28] Table 2.10 shows regressions of infinite WTA on these indices. Neither confidentiality concerns nor community stigma costs are significantly associated with infinite WTA (Columns 1-2). In contrast, self stigma is strongly and significantly associated with infinite WTA (Column 3). A one-$\sigma$ increase in the self stigma index increase the probability of reporting infinite WTA by 24.1 percentage points.

It is worth noting that the requirement to self-identify as a sex worker is

[28]The confidentiality index includes a direct measure of whether the subject believes information provided during registration is confidential, and two questions about who can access the registration list. The community stigma index includes two reasons for remaining uncertified: "Afraid someone will see me at the clinic" and "Afraid someone will find the health card"; whether the participant has ever been to the registration clinic for gynecological care (so the cost of going to the clinic to register would be low); and whether the participants solicits or completes transactions in public (so registration increases risk of community stigma by a relatively smaller amount). The self stigma index includes "Do not want to assume 'sex worker' identity" and agreement with the statement "Some women exchange sex for money but are not sex workers."

**Table 2.10**: Stigma and Refusal to Register

| | (1) Inf. WTA | (2) Inf. WTA | (3) Inf. WTA | (4) Inf. WTA |
|---|---|---|---|---|
| Confidentiality index | -0.01000 | | | -0.0610 |
| | (0.0581) | | | (0.0540) |
| Community stigma index | | 0.103 | | 0.0746 |
| | | (0.0777) | | (0.0723) |
| Self stigma index | | | 0.250*** | 0.254*** |
| | | | (0.0301) | (0.0297) |
| Constant | 0.435*** | 0.425*** | 0.424*** | 0.416*** |
| | (0.0477) | (0.0486) | (0.0450) | (0.0458) |
| Observations | 228 | 228 | 228 | 228 |

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Controls for treatment and registration status included in all regressions.

fundamental to the concept of a registration program. Reducing this cost on a large scale would require dramatic changes in social norms. This is distinct from community stigma and confidentiality costs, which could in principle be reduced by improved program design (e.g., expanding registration sites beyond STI clinics, or redesigning the registration card). This result suggests that in order to achieve universal health coverage of women who exchange sex for money, legalization and regulations regimes require complementary services to reach underground and occasional sex workers.

## 2.6 Discussion and Policy Implications

The preceding sections establish a clear explanation for low take-up of health certification among sex workers in Senegal: the benefits of certification are limited, and stigma costs of certification are high. Moreover, both low benefits and high costs follow from fundamental features of this market and certification mechanism.

The evidence is consistent with a model where the certification premium is low because STI status is partially observable, through visible STI symptoms; and the stigma costs of certification stem from the fact that it requires self-identification as a sex worker. These results thus suggest that we should continue to expect low take-up of certification programs in Senegal, and in other contexts where sex work is highly stigmatized.

This section therefore considers policy implications of low take-up, through the lens of targeting. If take-up is low, but the registration program reaches those at highest risk of acquiring and transmitting STIs, it may still be effective in controlling STIs, and, thus limiting the infectious disease externality inherent in sex markets. To study targeting, I evaluate baseline predictors of opting in to registration in my sample. Overall, there is some evidence that registration improves attachment to the health system, but the results are inconsistent with effective targeting of high risk FSWs.

First, I show that recent STI episodes predict selection into the program (Table 2.11, Column 1). Those who had STI symptoms in the past month were 9.83 percentage points more likely to register, and this association is significant at the 5% level. Column 2 explores registration among those who had a "well visit" in the past month: they saw a doctor for gynecological care even when they did not have STI symptoms. Those who completed a well visit in the past month were less likely to register, and recent STI symptoms predict take-up only among those who did not complete a well visit (Column 3). Among those who completed a well visit in the past month, the association between recent STI symptoms and registration is statistically indistinguishable from zero. Registration is more likely among participants who had a recent STI and were not already participating in regular medical check-ups at baseline.

**Table 2.11**: Targeting: STI Infection and Take-up

| | (1) Registered | (2) Registered | (3) Registered |
|---|---|---|---|
| Treatment | 0.0498* | 0.0541** | 0.0535** |
| | (0.0265) | (0.0269) | (0.0268) |
| STI symptoms, past month | 0.0958** | 0.0909** | 0.119** |
| | (0.0403) | (0.0396) | (0.0516) |
| Well visit, past month | | -0.0686*** | -0.0423** |
| | | (0.0180) | (0.0165) |
| STI symptoms, past month × Well visit, past month | | | -0.118** |
| | | | (0.0519) |
| Constant | 0.00230 | 0.0213 | 0.0135 |
| | (0.0149) | (0.0162) | (0.0172) |
| Baseline controls | Yes | Yes | Yes |
| Observations | 288 | 287 | 287 |

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All observations weighted by $w_{ij}$ in 2.4

Why are women with recent STIs more likely to register? The registration program is described and marketed as a health program, both in my persuasive intervention and in wider messaging. The program is run by the Ministry of Health (not by the police), and the registration card is called a "health book." Thus, participants likely view registering as a health-promoting action, even if they did not register to get treatment for a specific STI episode (77 percent of those who got certified and had STI symptoms had already seen a doctor for those symptoms). Consistent with this hypothesis, I show that women who register are more likely to be "health-seeking types" (Table 2.12). I construct a summary index (Anderson, 2008a) of baseline health-seeking behavior using 10 different variables, including 3 measures of condom use with clients, number of casual partners, condom use with casual partners, contraceptive use, time since last HIV test, alcohol and drug use, and a question on willingness to take health risks "in general".[29] It is worth noting that many of these measures are unrelated to sex work: they are intended to capture a preference for health.

Health seeking types are more likely to register (Column 1). In fact, recent STIs are most predictive of registration take-up among health-seeking types, and it is this interaction that drives the effect of STIs on take-up (Column 2). The interaction of health-seeker index and recent STIs is large and significant at the 1 percent level, while the coefficient on STIs alone, which corresponds to the effect of STIs at the mean of the health seeker index, is no longer significant. Appendix 2.8.4 shows that these results are not driven by any single component of the index. Additionally, I elicited subjects' subjective probability of getting an STI in the next month. This measure is not associated with take-up of registration in the full sample (Column 3); however, the health seeker index is more predictive in interaction with

[29]The question on willingness to take health risks is drawn from Dohmen et al. (2011), and is the only variable that is measured at endline. Results are robust to excluding it from the index.

**Table 2.12**: Registration Take-up among Health-seeking Types

| | (1) Registered | (2) Registered | (3) Registered | (4) Registered |
|---|---|---|---|---|
| Any STI, past month | 0.0902** | 0.0738** | 0.0954** | 0.0916** |
| | (0.0376) | (0.0325) | (0.0394) | (0.0390) |
| Health seeker index | 0.103*** | 0.0261 | 0.127*** | -0.0288 |
| | (0.0378) | (0.0265) | (0.0452) | (0.0772) |
| Any STI, past month × Health seeker index | | 0.302*** | | |
| | | (0.114) | | |
| Subj. prob. of STI, next month | | | 0.000534 | -0.000195 |
| | | | (0.0135) | (0.0137) |
| Subj. prob. of STI, next month × Health seeker index | | | | 0.0812 |
| | | | | (0.0508) |
| Constant | -0.000325 | 0.00113 | -0.00312 | 0.00275 |
| | (0.0148) | (0.0143) | (0.0248) | (0.0236) |
| Observations | 288 | 288 | 275 | 275 |

Standard errors in parentheses

*Health seeker index* is a summary index (Anderson 2008) of 10 health protective behaviors and attitudes: three measures of condom use with clients, contraceptive use, number of casual partners, condom use with casual partners, time since last HIV test, alcohol use, substance use, and willingness to take health risks. Treatment status controls included in all regressions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

subjective probability of STI (Column 4). Taken together, these results suggest that registration take-up is driven by STI shocks among health-seeking types.

These results are not consistent with effective targeting of high risk FSWs. While the components of the health seeker index are self-reported, the results are similar across a wide range of outcomes. The women who got certified report a high rate of condom use, suggesting that they were at low risk of acquiring and transmitting STIs, but were simply unlucky.[30] Those at the highest risk of acquiring and transmitting STIs remain outside the system.

## 2.7 Conclusion

This paper presents empirical evidence on the theoretical underpinnings of sex work legalization and regulation programs. Even after receiving information about the registration program and a moderate incentive to register, take-up for registration is extremely low. This can be explained, in part, by the fact that there is no evidence that sex workers who register earn a price premium. The absence of a price premium is consistent with a lack of an asymmetric information problem in sex markets, at least from the perspective of clients. Sex workers who experienced recent visible STI symptoms earn lower prices. Invisible STI symptoms, in contrast, have no effect on prices, suggesting that this effect is driven by client demand.

The result that client willingness-to-pay responds to STI risk may help inform new client side interventions. One explanation for the lack of a certification premium is that clients interpret the absence of visible STI symptoms as a clean bill of health. In reality, however, most STIs are asymptomatic (Farley et al., 2003). Among the FSWs who reported an invisible STI episode in this study, none

[30] While condom use significantly reduces the risk of acquiring an STI, it does not eliminate it. WIth typical use, condoms reduce the risk of HIV transmission by about 80 percent, and are believed to have similar effectiveness rates for STIs (Weller and Davis-Beaty, 2002).

reported visible symptoms in the same month. Educating clients about this fact could have impacts on demand for certified sex workers or condom use.

I also show that certification is costly for a large proportion of sex workers. The most significant cost is self stigma: in a context where sex work is highly stigmatized, it is costly for a woman to admit, even confidentially, that she is a sex worker. This cost is fundamental to the structure of registration programs. Thus, this result suggests that while registration programs may be valuable for a subset of sex workers who participate, they are unlikely to achieve full health coverage of female sex workers. In most settings, complementary services to reach uncertified sex workers are necessary to achieve STI control.

Finally, I present evidence that FSWs who self select into the program are at low risk for acquiring and transmitting STIs. While experiencing recent STI symptoms at baseline predicts take-up, it is only predictive among health-seeking types: women who were already engaged in numerous health-protective behaviors, such as condom use and transacting with fewer clients, that reduce their risk of both acquiring and transmitting STIs. The registration program is marketed as a health program, and a recent STI shock increases demand for health care, but it does so differentially among low risk types. This result reinforces the need for complementary services to uncertified populations. Mechanisms for targeting FSWs at the highest risk of STI acquisition and transmission would be a fruitful area for further research.

## Acknowledgment

Chapter 2, in full, is currently being prepared for submission for publication of the material. Manian, Shanthi. The dissertation author is the sole author of this material.

## 2.8 Appendix

### 2.8.1 Attrition

Table 2.13 presents a regression of attrition status on treatment assignment. There is no differential attrition across experimental groups.

**Table 2.13**: Attrition

| | (1) Attrited |
|---|---|
| Treatment | 0.0140 |
| | (0.0294) |
| Constant | 0.0658*** |
| | (0.0212) |
| Observations | 315 |

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

### 2.8.2 Balance

To account for the randomization sessions discussed in section 2.3.3, I test balance using separate, weighted regressions of the following form for each variable $x_i$:

$$x_i = \beta_0 + \beta_1 T_i + \epsilon_i \tag{2.8}$$

where $T_i$ is the treatment assignment and each observation is weighted by $w_i$ defined in equation (2.4).

I show balance in Tables 2.14 and 2.15. Each row represents a separate regression.

**Table 2.14**: Randomization Balance (I)

| | Treatment | Constant | N |
|---|---|---|---|
| Transaction Price | 133.90 | 9840.81 | 1453 |
| | (803.69) | (673.63) | |
| Total income, past month | 2.64 | 107.42 | 289 |
| | (10.95) | (7.11) | |
| Registration knowledge index | -0.01 | 3.06 | 291 |
| | (0.26) | (0.18) | |
| Age | -0.62 | 37.84 | 290 |
| | (1.05) | (0.72) | |
| No. of arrests, past month | 0.08 | 0.03 | 289 |
| | (0.05) | (0.03) | |
| Time in jail, past month | 0.31 | -0.00 | 290 |
| | (0.24) | (0.00) | |
| Any clients met in a public place | -0.04 | 0.27 | 291 |
| | (0.05) | (0.04) | |
| No. of clients met in a public place | -0.07 | 0.62 | 291 |
| | (0.14) | (0.10) | |
| Any transactions completed in a public place | 0.03* | 0.01 | 291 |
| | (0.02) | (0.01) | |
| No. of transactions completed in a public place | 0.09 | 0.03 | 291 |
| | (0.07) | (0.03) | |
| No. of recent STI episodes | 0.05 | 0.29 | 288 |
| | (0.08) | (0.04) | |
| Severity of recent STI episodes | -0.10 | 0.53 | 291 |
| | (0.11) | (0.09) | |
| No. of clients | -3.14 | 14.90 | 290 |
| | (1.98) | (1.49) | |
| No. of unprotected sex acts, past month | -0.65 | 3.40 | 290 |
| | (1.05) | (0.68) | |

Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 2.15**: Randomization Balance (II)

| | Treatment | Constant | N |
|---|---|---|---|
| Frequency of condom use with clients | 0.14 | 3.36 | 291 |
| | (0.13) | (0.10) | |
| Exchanged unprot. sex, any of last 5 trans. | -0.08 | 0.38 | 291 |
| | (0.06) | (0.04) | |
| Exchanged unprot. sex, no. of last 5 trans. | -0.25 | 0.98 | 291 |
| | (0.17) | (0.13) | |
| HIV knowledge index | -0.01 | 1.98 | 291 |
| | (0.11) | (0.08) | |
| Saw a doctor for recent STI symptoms | 0.06 | 1.65 | 291 |
| | (0.07) | (0.05) | |
| No. of preventive medical visits, past month | 0.06 | 0.36 | 290 |
| | (0.08) | (0.06) | |
| Ever had an HIV test | 0.01 | 0.90 | 291 |
| | (0.04) | (0.03) | |
| Time since last HIV test | -0.04 | 5.29 | 287 |
| | (0.38) | (0.28) | |
| Can charge more if registered | 0.07 | 0.61 | 291 |
| | (0.06) | (0.04) | |
| Any of last 5 clients asked if registered | 0.00 | 0.24 | 291 |
| | (0.05) | (0.04) | |
| No. of last 5 clients who asked if registered | 0.04 | 0.52 | 291 |
| | (0.15) | (0.10) | |
| Any of last 5 clients asked to see reg. card | -0.01 | 0.11 | 291 |
| | (0.04) | (0.03) | |
| No. of last 5 clients asked to see reg. card | -0.00 | 0.28 | 291 |
| | (0.12) | (0.08) | |

Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

### 2.8.3 Extensive Margin Effects of STIs

**Table 2.16**: Extensive Margin Effects of STIs

| | (1) Quant. Exch. | (2) Quant. Exch. | (3) Quant. Exch. |
|---|---|---|---|
| STI_any | -8.700* | | |
| | (4.878) | | |
| Visible STI, past month | | -7.721 | -8.576 |
| | | (5.632) | (5.809) |
| Invisible STI, past month | | | -7.939* |
| | | | (4.697) |
| Constant | 29.14*** | 28.30*** | 28.99*** |
| | (2.524) | (2.345) | (2.533) |
| Observations | 577 | 580 | 580 |

Standard errors in parentheses, clustered at individual level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Individual and round fixed effects, treatment controls included in all regressions.

### 2.8.4 Robustness: Health-seeking Types

**Table 2.17**: Registration Take-up and Health Seeker Index Components (I)

| | (1) | (2) | (3) |
|---|---|---|---|
| Treatment | 0.0438* | 0.0462* | 0.0429* |
| | (0.0255) | (0.0259) | (0.0258) |
| N. recent STIs | 0.0120 | 0.0998** | 0.112** |
| | (0.0172) | (0.0464) | (0.0537) |
| Always used condoms with clients | 0.0258 | | |
| | (0.0179) | | |
| Always used condoms with clients × N. recent STIs | 0.1000* | | |
| | (0.0585) | | |
| N. unprot. trans., past month | | -0.000539 | |
| | | (0.000442) | |
| N. recent STIs × N. unprot. trans., past month | | -0.00816** | |
| | | (0.00377) | |
| N. unprot. trans., of last 5 | | | -0.00743 |
| | | | (0.00552) |
| N. recent STIs × N. unprot. trans., of last 5 | | | -0.0259** |
| | | | (0.0129) |
| Constant | -0.0120 | 0.00687 | 0.0118 |
| | (0.0114) | (0.0172) | (0.0196) |
| Observations | 288 | 287 | 288 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

**Table 2.18**: Registration Take-up and Health Seeker Index Components (II)

| | (1) | (2) | (3) |
|---|---|---|---|
| Treatment | 0.0379 | 0.0418 | 0.0420 |
| | (0.0254) | (0.0261) | (0.0261) |
| N. recent STIs | -0.0122 | 0.0764** | 0.0106 |
| | (0.0128) | (0.0368) | (0.00767) |
| Contraceptive use | 0.0242 | | |
| | (0.0246) | | |
| Contraceptive use × N. recent STIs | 0.103** | | |
| | (0.0478) | | |
| N. casual partners | | -0.00322* | -0.00196 |
| | | (0.00182) | (0.00129) |
| N. recent STIs × N. casual partners | | -0.0429** | |
| | | (0.0191) | |
| Always used condoms/no casual partner | | | 0.0243* |
| | | | (0.0138) |
| Always used condoms/no casual partner × N. recent STIs | | | 0.0637* |
| | | | (0.0366) |
| Constant | -0.000221 | 0.00883 | -0.0138 |
| | (0.0186) | (0.0157) | (0.0114) |
| Observations | 287 | 286 | 286 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

**Table 2.19**: Registration Take-up and Health Seeker Index Components (III)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Treatment | 0.0372 | 0.0464* | 0.0576** | 0.0262 |
| | (0.0266) | (0.0265) | (0.0255) | (0.0272) |
| N. recent STIs | 0.0794** | 0.0742** | 0.209** | 0.162* |
| | (0.0369) | (0.0356) | (0.0890) | (0.0824) |
| Ever used alcohol | 0.103 | | | |
| | (0.0711) | | | |
| Ever used alcohol × N. recent STIs | -0.0871 | | | |
| | (0.128) | | | |
| Ever used drugs | | -0.0463** | | |
| | | (0.0191) | | |
| Ever used drugs × N. recent STIs | | -0.0812** | | |
| | | (0.0370) | | |
| Time since last HIV test | | | 0.00225 | |
| | | | (0.00382) | |
| N. recent STIs × Time since last HIV test | | | -0.0230* | |
| | | | (0.0117) | |
| Health risk tolerance | | | | -0.00917 |
| | | | | (0.00947) |
| N. recent STIs × Health risk tolerance | | | | -0.0445 |
| | | | | (0.0305) |
| Constant | -0.000800 | 0.00690 | -0.0159 | 0.0207 |
| | (0.0155) | (0.0156) | (0.0301) | (0.0277) |
| Observations | 288 | 288 | 284 | 232 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

# Chapter 3

# Information Disclosure and Health in the Female Sex Market

**with Manisha Shah**

## 3.1 Introduction

The prevalence of sexually transmitted infections (STIs) among female sex workers is extremely high, with estimates in developing countries ranging from 24 to 84 percent (Cwikel et al., 2008). Economic theory suggests that this high prevalence is partly due to asymmetric information in sex markets: sex workers possess more information about their health status than clients do. As in any market (Akerlof, 1970), this asymmetric information reduces market incentives for sex workers to invest in health improvements, which increases STI prevalence. The core assumption underlying this theory is that sex workers' STI status is unobservable, and sex workers have no way of providing credible information about their health status to clients.

Using data from 2,721 female sex workers in Ecuador, this paper evaluates the observability of sex worker's STI status by studying how market outcomes vary with sex workers' STI status. Further, we consider whether the results are

consistent with a theoretical model of voluntary information disclosure. We adopt a theoretical framework that makes two key prediction. First, voluntary information disclosure will occur only when there is some probability that the STI status will be independently revealed. Second, voluntary disclosure requires repeated interactions.

We present a set of results suggesting that sex workers' STI status is partially observable. We find some support for a theory of voluntary information disclosure; evidence is consistent with the first theoretical prediction, that disclosure occurs only when the information could be independently revealed. Our primary empirical specification combines the results of laboratory STI tests with a biomedical literature on the disease-specific prevalence of visible symptoms (Farley et al., 2003; Chandrasekar, 2016) to classify sex workers' STI status as either visible or invisible. We show that sex workers with visible STIs earn lower prices at the transaction-level, relative to both uninfected sex workers and those with invisible STIs. There is no such association between invisible STIs and prices. The results are robust to the inclusion of numerous controls for sex worker demographics, work location, and types of sex act exchanged.

However, we find no evidence that repeated interactions are necessary to produce an association between STI visibility and prices, in contrast to the second prediction of our framework. Given these results, we consider an alternate, supply-side explanation for the observed association between STI visibility and prices: that sex workers with lower willingness-to-accept are more likely to acquire visible STIs. We use data on client and sex worker characteristics to rule out this mechanism. Finally, we present suggestive evidence that information disclosure may be supported by venue-level rather than individual-level reputations.

This paper contributes to the literature on the economics of sex work by providing new insight into how STI risk is priced into sex markets. Previous research

has documented that sex workers earn a price premium in exchange for unprotected sex, an effect driven by STI risk to the sex worker (Gertler et al., 2005; Robinson and Yeh, 2011; Arunachalam and Shah, 2013). Relatively little research has considered how the market responds to STI risk to the client. Using the same dataset as this paper, Arunachalam and Shah (2013) show that prices decline with the local STI rate, which suggests that client willingness-to-pay responds to STI risk. Using data from Senegal, Manian (2016) shows that prices respond to self-reported visible STI episodes, but not invisible episodes. This paper reinforces the previous results by demonstrating similar effects using sex workers' own, biologically confirmed STI status.

The results also have implications for the optimal policy response to STI epidemics. If information about sex workers' STI status is being transmitted in the market in the absence of government intervention, this suggests that interventions should target infectious disease externalities rather than asymmetric information. For example, while occupational licensing programs are a common approach to STI control among female sex workers (Gertler and Shah, 2011; Manian, 2016), simply subsidizing STI testing and treatment may be a more effective strategy.

## 3.2 Theoretical Considerations

Consider an environment in which a sex worker and a client must decide whether to undertake a transaction. There is some probability that the sex worker is infected with an STI. The client is willing to pay more if she does not have an STI, but he cannot observe her health status. The benchmark case is a one-shot interaction. In this case, without further information, the client will evaluate his expected utility from the transaction based on the probability that any given sex worker has an STI. A sex worker who has an STI has no incentive to disclose her

status, since this will just reduce the price that she earns. Moreover, she has an incentive to misrepresent her status and try to obtain a higher price, so messages indicating "no STI" are not credible. Thus, in this case, no information about sex worker STI status is transmitted in the market. This leads to a version of the classic "lemons" problem (Akerlof, 1970): since clients can't observe the sex workers' STI status, sex workers' incentives to invest in their health are reduced, and STI prevalence increases.

However, information transmission about sex workers' STI status can be supported, even in the absence of formal intervention, under two conditions: if there is some way of observing STI status ex post, and if there are repeated interactions (see Mccluskey (2000) for an analogous discussion in the realm of organic foods). Suppose that if the sex worker has an STI, her status may may be revealed during the course of the transaction by observable symptoms. Then with some probability, a sex transaction is an experience good: its quality can be observed after the transaction takes place. With the complementary probability, it is a credence good: the client will never know whether or not the sex worker had an STI.

Consider the following strategies in a repeated game. The first time the sex worker has an STI, she discloses to the client and offers a discount. She discloses all future STIs if the client decides to continue the relationship. Similarly, the client continues the relationship whenever the sex worker discloses an STI, but ends the relationship if he discovers that she had an STI and did not disclose it. As long as the sex worker is sufficiently patient and there is a sufficiently high probability that failure to disclose will be discovered, the value of the future stream of profits will exceed the one-time cost of providing a discount (or forgoing the transaction) when she has an STI, and she will voluntarily disclose her status.[1]

[1]Note that disclosing the presence of an STI is costly, so this is not a cheap talk setting.

This theoretical framework generates three key testable predictions. First, if sex workers' STI status is completely unobservable and no information about STIs is transmitted in the market, an individual sex worker's STI status should have no effect on prices (Manian, 2016). In contrast, if there is voluntary disclosure of information about STIs, it should occur when (1) there is a nonzero probability that the STI status will be revealed and (2) the relationship with the client is repeated. Thus, if there is a price response to STIs, it should be stronger among sex workers with visibly symptomatic STIs, and in repeated interactions. The remainder of the paper explores whether the pattern of associations between transaction price and sex workers' STI status in Ecuador is consistent with these predictions.

## 3.3 Data

This paper uses detailed individual- and transaction-level survey data from female sex workers in eight cities in Ecuador. The data were collected in 2003 to serve as a baseline for the Frontiers Prevention Project (FPP), a cluster-randomized trial of an initiative aimed at populations at risk of seeing an acceleration of their HIV epidemics. Thus, the eight cities were selected primarily for logistical reasons related to the ability to implement the project (Gutiérrez et al., 2013). However, the cities selected include the country's largest city, Guayaquil, and the capital, Quito, and all represent urban environments.[2]

Data were collected from 2,926 sex workers using targeted sampling. In collaboration with local female sex workers, nongovernmental organizations, and community-based organizations, researchers identified sites where female sex workers were likely to be concentrated and sampled from those locations. The data were collected via face-to-face interviews conducted by trained female sex workers.

[2]The remaining cities are Machala, Milagro, Daule, Esmeraldas, Santo Domingo and Quevedo.

Importantly, the survey collected no identifying information. Transaction-level data were collected by asking sex workers to provide the details of their 3 most recent sex transactions, an approach commonly used in research on sex work (e.g., Gertler et al. (2005); Manian (2016)).

As discussed in Section 3.2, our goal is to test whether the association between prices and sex workers' STI status is moderated by the presence of visible symptoms, which increase the probability that the STI status will be revealed during the transaction. Blood samples were collected at the time of data collection and tested for four STIs: syphilis, gonorrhea, chlamydia, and herpes simplex virus (HSV). We exclude HSV because it is incurable, and a positive test is unlikely to reflect recent sexual behavior or symptoms. We classify the remaining infections as either visible or invisible based on the biomedical characterization of each type of infection. The majority of syphilis infections in women are symptomatic and cause visible genital sores soon after infection (Chandrasekar, 2016). In contrast, 68 percent of gonorrhea infections and 75 percent chlamydia infections in women are estimated to be asymptomatic, and typical symptoms include pain and abnormal discharge, which would not be easily detectable by a client (Farley et al., 2003). Thus, syphilis infections are classified as visible, while chlamydia and gonorrhea infections are classified as invisible.

With regard to the second prediction of the model, that voluntary information disclosure will occur only in repeated relationships, we explore heterogeneity in the influence of visible STIs at the transaction level, according to whether respondents classified clients as regular or new. We additionally explore whether heterogeneity in several other individual- and transaction-level dimensions is consistent with voluntary information disclosure.

We conduct the analysis on a sample of 2,721 sex workers with a complete

**Table 3.1**: Sex Worker Summary Statistics

| | Mean | S.D. | Min. | Max. | Obs. |
|---|---|---|---|---|---|
| *Demographics:* | | | | | |
| Age | 27.7 | 7.87 | 13 | 66 | 2721 |
| Married | 0.48 | 0.50 | 0 | 1 | 2721 |
| Any children | 0.86 | 0.35 | 0 | 1 | 2721 |
| Literate | 0.96 | 0.19 | 0 | 1 | 2709 |
| Highest grade completed | 4.16 | 1.76 | 0 | 11 | 2721 |
| *Sex work history:* | | | | | |
| Registered | 0.83 | 0.38 | 0 | 1 | 2721 |
| Years in sex work | 4.16 | 4.96 | 0 | 40 | 2721 |
| *STI prevalence and knowledge:* | | | | | |
| Any STI | 0.079 | 0.27 | 0 | 1 | 2721 |
| Syphilis | 0.027 | 0.16 | 0 | 1 | 2675 |
| Chlamydia | 0.048 | 0.21 | 0 | 1 | 2664 |
| Gonorrhea | 0.011 | 0.10 | 0 | 1 | 2664 |
| Ever heard of STIs | 0.91 | 0.28 | 0 | 1 | 2682 |
| Identified syphilis symptom | 0.18 | 0.39 | 0 | 1 | 2721 |

This table presents summary statistics for 2721 female sex workers comprising the analysis sample (participants with a complete set of observations for all variables used in the analysis). Responses of *Don't know* and refusals to respond are coded as missing.

set of observations for all variables used. Table 3.1 presents summary statistics on the demographic composition, sex work history, and STI status of the sample. As described by Arunachalam and Shah (2013), the sample is demographically similar to other samples of female sex workers in Latin America and East Africa. Nearly 83 percent of the sample participates in Ecuador's sex worker registration program, which requires a gynecological exam every 8-15 days as well as periodic STI testing (Gertler and Shah, 2011). Consistent with the high rate of registration in the sample, STI prevalence is much lower than in other developing country contexts (Cwikel et al., 2008). Just under 8 percent of the sample tests positive for syphilis, chlamydia, or gonorrhea. Chlamydia is the most common STI in the sample, followed by syphilis and gonorrhea.

Given our hypothesis that voluntary information disclosure drives an association between prices and sex worker STIs, it is important to verify that sex workers are familiar with STIs. Reassuringly, over 90 percent of the sample had previously heard of the concept of sexually transmitted infections, and close to one fifth identified genital sores, the primary symptom of syphilis, as an STI symptom without prompting.

The primary outcome of interest is the total price paid for a given sex transaction. While we have three price observations for each individual, there is only a single blood draw for each individual.[3] We do not know exactly when the STI was acquired or when symptoms may have been present. We therefore restrict the analysis to transactions conducted in the past month.

Transaction-level summary statistics are presented in Table 3.2. The mean transaction occurred 3.12 days prior to the survey, implying that STI test results at the time of the survey are a good proxy for STI status during the transaction. The vast majority of transactions include vaginal sex; just 2.3 percent include riskier anal sex, and 8.2 percent include oral sex. Sex workers reported that a condom was not used in 13 percent of transactions, which is on par with rates reported in other samples of female sex workers (e.g., Gertler et al. (2005); Manian (2016)). The bottom panel of Table 2 reports summary statistics for different types of work locations. 60 percent of transactions occurred in brothels. The higher-priced nightclub sector (Arunachalam and Shah, 2013) accounted for 20 percent of transactions, and 10 percent of transactions occurred in the street.

[3]While follow-up data were collected in 2007, because the surveys were anonymous, those data cannot be matched to the data used in this paper (Gutiérrez et al., 2013).

**Table 3.2**: Transaction Summary Statistics

| | Mean | S.D. | Min. | Max. | Obs. |
|---|---|---|---|---|---|
| Days since transaction | 3.12 | 5.06 | 0 | 31 | 7807 |
| Price | 7.04 | 7.36 | 0 | 95 | 7807 |
| Vaginal sex | 0.98 | 0.14 | 0 | 1 | 7804 |
| Anal sex | 0.023 | 0.15 | 0 | 1 | 7807 |
| Oral sex | 0.082 | 0.27 | 0 | 1 | 7807 |
| No condom | 0.13 | 0.34 | 0 | 1 | 7807 |
| *Work location:* | | | | | |
| Brothel | 0.60 | 0.49 | 0 | 1 | 7807 |
| Night club | 0.20 | 0.40 | 0 | 1 | 7807 |
| Street | 0.10 | 0.30 | 0 | 1 | 7807 |

This table presents summary statistics for 7807 sex transactions comprising the analysis sample (transactions occurring in the past month for participants with a complete set of observations for all variables used in the analysis).

## 3.4 Empirical Strategy

The primary empirical strategy involves comparing transaction prices for female sex workers with a visible STI (syphilis) to those for female sex workers with invisible STIs (chlamydia and gonorrhea), conditional on controls. Let $P_{ij}$ denote the price of transaction $i$ for sex worker $j$. Our preferred estimating equation is:

$$P_{ij} = \beta_0 + \beta_1 AnySTI_i + \beta_2 AnySTI_i * Visible_i + \mathrm{W_{ij}}\gamma + \mathrm{X_i}\delta + \eta_i + \epsilon_{ij}$$

where $AnySTI_i$ is an indicator for having any STI, $Visible_i$ is an indicator for having a visible STI, $\mathrm{W_{ij}}$ is a vector of transaction-level controls, and $\mathrm{X_i}$ is a vector of individual-level controls. $\beta_2$ is the coefficient of interest. We use wild cluster bootstrapped standard errors (Cameron et al., 2008) to account for the use of cluster sampling in a small number of cities (eight).

Identification of the effect of visible STIs on prices would require the assump-

tion that conditional on having an STI (and other controls), the type of infection acquired is exogenous. To explore this assumption, in Table 3.3, we assess associations between STI visibility and five fixed demographic characteristics, conditional on registration status, city fixed effects, and work location controls. Column 1 indicates that older women are significantly more likely to have a visible STI: women with visible STIs are 2.3 years older on average than those with invisible STIs. While the remaining demographic characteristics are not significantly associated with visibility (Columns 2-5), these results suggest that we should not assume that STI visibility is conditionally exogenous. Estimates should therefore be taken as descriptive. However, it is worth noting that the sexual risk behaviors that lead to visible and invisible STIs are similar.

## 3.5 Results

### 3.5.1 STI Visibility

Our first empirical test describes the association between transaction price and sex workers' STI status, and determines whether STI visibility moderates this association. Results are shown in Table 3.4. Column 1 shows that, in general, there is no association between STI status and price: the coefficient of interest is both statistically and economically insignificant, representing just 0.05 standard deviations. However, Column 2 shows that when we disaggregate by visible versus invisible STIs, sex workers with visible STIs earn lower prices, while there is no such association for sex workers with invisible STIs. Column 3 represents the preferred approach, comparing sex workers with visible STIs to those with invisible STIs. Conditional on having an STI, visibility predicts a decrease in prices of nearly $2.00, representing 0.27 standard deviations. This result is consistent with a model of

**Table 3.3**: STI Visibility and Demographics

| | (1) Age | (2) Married | (3) Any children | (4) Literate | (5) Highest grade completed |
|---|---|---|---|---|---|
| Any STI × Visible | 2.266* | 0.0763 | 0.00412 | -0.00107 | -0.0258 |
| | (0.075) | (0.295) | (0.900) | (1.000) | (0.945) |
| Any STI | -1.056 | -0.00232 | 0.0387* | -0.0166 | 0.117 |
| | (0.260) | (1.000) | (0.080) | (0.485) | (0.445) |
| Constant | 29.91*** | -0.110*** | 0.523 | 1.022*** | 2.349 |
| | (0.000) | (0.005) | (0.410) | (0.000) | (0.410) |
| Observations | 7807 | 7807 | 7807 | 7798 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values in parentheses, clustered at city level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects, controls for registration status, and work location controls included in all regressions.

**Table 3.4**: Price and STI Visibility

| *Dependent Variable:* | Transaction Price | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Any STI | -0.371 | | 0.321 |
| | (0.458) | | (0.620) |
| Invisible STI | | 0.260 | |
| | | (0.710) | |
| Visible STI | | -1.693*** | |
| | | (0.005) | |
| Any STI × Visible | | | -1.995** |
| | | | (0.015) |
| Observations | 7807 | 7807 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values in parentheses, clustered at city level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

voluntary disclosure in which sex workers offer a discount when there is a chance that their STI status will be revealed during the transaction.

Table 3.5 explores robustness of the preferred specification to various types of controls. Column 1 controls for demographic characteristics that are strongly correlated with prices, such as age, marital status and education. Column 2 includes controls for work location, an important control given previous showing that STI prevalence in a given work location predicts prices in the same dataset (Arunachalam and Shah, 2013). Reassuringly, the effect of visibility remains statistically significant and similar in magnitude as both types of controls are introduced. Finally, Column 3 controls for type of sex act exchanged. Given that there is a price premium for unprotected sex in this setting (Arunachalam and Shah, 2013), one possible explanation for the visibility result is that information disclosure leads clients to substitute toward lower-priced sex with a condom. However, the stability of the

**Table 3.5**: Robustness: Price and STI Visibility

| *Dependent Variable:* | Transaction Price | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Any STI × Visible | -1.596* | -1.706* | -1.754* |
| | (0.080) | (0.055) | (0.090) |
| Any STI | 0.190 | 0.191 | 0.190 |
| | (0.755) | (0.790) | (0.780) |
| Demographic controls | Yes | Yes | Yes |
| Work Location controls | No | Yes | Yes |
| Sex act controls | No | No | Yes |
| Observations | 7807 | 7807 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values in parentheses, clustered at city level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

coefficient from Columns 2 to 3 suggests that this is not the mechanism for the effect.

Interestingly, these results are very similar to results from Manian (2016) establishing that prices respond to visible STI episodes self-reported by female sex workers, but not to invisible episodes. The effect in that paper is identified using individual fixed effects, suggesting that it is not driven by unobservable differences between higher and lower risk types. The similar finding here, using biologically confirmed STI status, reinforces the result.

### 3.5.2 Repeated Interactions

The second prediction following from our theoretical discussion is that voluntary disclosure can only be supported in repeated interactions. We explore the importance of repeated interactions in Table 3.6. In contrast to the results in Section

3.5.1, these results do not support the theory. In each column, the coefficient of interest is the first to appear. In this table, we use traditional, parametric standard errors clustered at the sex worker level to maximize power. Columns 1 and 2 show that there is no differential association between visible STIs and prices among regular clients, regardless of controls. While the point estimate is negative, it is small and not statistically significant. Similarly, when comparing those with visible STIs to those with invisible STIs (Columns 3 and 4), the coefficient on visibility remains statistically significant among new clients, with no differential effect for regular clients, again regardless of controls.

A possible explanation for these null results is that after sex workers disclose their status, regular clients decline to complete the transaction, leading sex workers to substitute toward new clients with lower willingness to pay. In this scenario, we would observe no effect on prices for regular clients, but we should see that sex workers with visible STIs are less likely to report recent transactions with regular clients. To test this hypothesis, Table 3.7 regresses client type on sex workers' STI status. However, contrary to the hypothesis, there is suggestive evidence that sex workers with visible STIs are actually more likely to report recent transactions with regular clients relative to the rest of the sample as well as those with invisible STIs (Columns 1 and 3). However, neither association survives the inclusion of demographic or work location controls (Columns 2 and 4), suggesting that there is likely no differential effect of STI visibility on client type. Taken together, we do not find support for the theory that repeated interactions are necessary to produce a price response to STIs.

**Table 3.6**: Heterogeneity by Client Type

| *Dependent Variable:* | Transaction Price | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Visible STI × Reg. clt. | -0.234 | -0.0664 | | |
| | (0.541) | (0.596) | | |
| Visible STI | -1.569*** | -1.554*** | | |
| | (0.432) | (0.535) | | |
| Invisible STI | 0.268 | 0.181 | | |
| | (0.641) | (0.561) | | |
| Any STI × Visible STI × Reg. clt. | | | -0.0807 | 0.585 |
| | | | (1.162) | (1.121) |
| Any STI × Visible STI | | | -1.944** | -2.002** |
| | | | (0.984) | (0.951) |
| Any STI × Reg. clt. | | | -0.158 | -0.688 |
| | | | (1.076) | (0.992) |
| Any STI | | | 0.395 | 0.474 |
| | | | (0.916) | (0.821) |
| Reg. clt. | 0.125 | 0.495** | 0.134 | 0.533** |
| | (0.202) | (0.215) | (0.207) | (0.223) |
| Sex act controls | No | Yes | No | Yes |
| Demographic controls | No | Yes | No | Yes |
| Work location controls | No | Yes | No | Yes |
| Observations | 7807 | 7807 | 7807 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

**Table 3.7**: Extensive Margin of Client Type

| *Dependent Variable:* | Regular Client | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Visible STI | 0.0888* | 0.0288 | | |
| | (0.0512) | (0.0486) | | |
| Invisible STI | -0.0552 | -0.0259 | | |
| | (0.0360) | (0.0354) | | |
| Any STI × Visible STI | | | 0.148** | 0.0579 |
| | | | (0.0616) | (0.0593) |
| Any STI | | | -0.0622* | -0.0308 |
| | | | (0.0365) | (0.0360) |
| Demographic controls | No | Yes | No | Yes |
| Work location controls | No | Yes | No | Yes |
| Observations | 7807 | 7807 | 7807 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, clustered at sex worker level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

## 3.6 Alternate Explanations

Given that we find only partial support for a model of voluntary information disclosure, in this section we investigate alternate explanations for the association between visible STIs and transaction prices documented in the previous section.

### 3.6.1 Supply-side Explanations

A leading alternate explanation for the results so far is a supply-side effect, rather than a demand-side effect. We hypothesized that visible STIs affect prices because client willingness-to-pay is lower for sex workers with STIs. However, it may instead be the case that sex workers with lower willingness-to-accept, and thus lower prices, are also more likely to acquire visible STIs. Our classification of visible

and invisible STIs also applies to men: 84 percent and 58 percent of chlamydia and gonorrhea cases, respectively, are entirely asymptomatic in men (Farley et al., 2003), while early syphilis infection is characterized by genital sores (Chandrasekar, 2016). Sex workers with lower willingness-to-accept may be more likely to transact with clients who have visible symptoms. If this is the case, we would expect the association between visible STIs and prices to be stronger (more negative) among sex workers who report recent transactions with clients who have visible STIs. In contrast, if demand-side effects drive the association, we would expect a weaker (less negative or positive) effect for clients who already have STIs.

While we do not have biological data for clients, the survey included the sex worker's assessment of the likelihood that each client had an STI, as well as other client characteristics. Table 3.8 explores how the association between STI visibility and prices varies with these client characteristics. Since there are a large number of missing observations for client characteristics, Column 1 verifies that the main result holds on the subsample with no missing data for all client characteristics studied. While the effect is no longer statistically significant due to the smaller sample, the point estimate for the coefficient on STI visibility remains negative and of similar magnitude to the comparable result in Table 3.5. Column 2 shows that the association between visible STIs and prices becomes weaker as the likelihood that a client had an STI increases: the interaction of visibility and client STI likelihood is positive and statistically significant. This is contrary to the prediction of a supply-side effect, and consistent with a demand-side effect. Columns 3 and 4 demonstrate that there is no similar effect for client marital status or client wealth, and Column 5 shows that the interaction of visibility and client STI likelihood remains positive with the inclusion of additional client controls, and is just marginally insignificant with a p-value of .105.

**Table 3.8**: STI Visibility and Client Characteristics

| *Dependent Variable:* | Transaction Price | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Any STI × Vis. | -1.475 | -1.747 | -0.753 | -1.297* | -1.570 |
| | (0.190) | (0.170) | (0.505) | (0.055) | (0.220) |
| Any STI | 0.0606 | 0.206 | -0.00520 | 0.198 | 0.235 |
| | (0.905) | (0.775) | (1.000) | (0.260) | (0.755) |
| Any STI × Vis. × Cl. STI prob. | | 1.130* | | | 1.032 |
| | | (0.090) | | | (0.105) |
| Any STI × Cl. STI prob. | | -0.654** | | | -0.560 |
| | | (0.040) | | | (0.120) |
| Cl. STI prob. | | -0.165 | | | -0.283 |
| | | (0.520) | | | (0.225) |
| Any STI × Vis. × Cl. married | | | -1.285 | | |
| | | | (0.165) | | |
| Any STI × Cl. married | | | 0.105 | | |
| | | | (0.905) | | |
| Client married | | | 0.216* | | 0.103 |
| | | | (0.095) | | (0.515) |
| Any STI × Vis. × Mid. class | | | | 0.0476 | |
| | | | | (0.955) | |
| Any STI × Vis. × Wealthy | | | | -1.420 | |
| | | | | (0.830) | |
| Any STI × Mid. class | | | | -0.156 | |
| | | | | (0.945) | |
| Any STI × Wealthy | | | | -0.121 | |
| | | | | (1.000) | |
| Middle class | | | | 1.182*** | 1.177*** |
| | | | | (0.000) | (0.000) |
| Wealthy | | | | 6.567*** | 6.558*** |
| | | | | (0.000) | (0.000) |
| Sex act controls | Yes | Yes | Yes | Yes | Yes |
| Demographic controls | Yes | Yes | Yes | Yes | Yes |
| Work location controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 5814 | 5814 | 5814 | 5814 | 5814 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values in parentheses, clustered at city level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

As a second test of a supply-side effect, we explore heterogeneity in three demographic characteristics that are associated with prices and might reasonably be predictive of willingness to accept: age, marital status, and education. These results are shown in Table 3.9. Although age is strongly and significantly associated with price (Column 1), STI visibility has no differential association with prices for older women. Marriage is associated with lower prices in general (Column 2). This is suggestive of lower willingness to accept, since a sex worker's marital status should not be observable. However, the coefficient on visible STI indicates the association is *less* pronounced for married women, although the estimate is imprecise. Again, this is inconsistent with a supply-side effect driving the main association between visible STIs and prices. Similarly, while education is predictive of higher prices (Column 3), the association between visible STIs and prices is stronger among more highly educated women. Overall, the results are not consistent with the notion that the association between visible STIs and prices is driven by women with lower willingness-to-accept.

### 3.6.2 Sectoral Effects

Since the evidence does not seem to be consistent with the supply side effect described in the previous section, we explore whether there may be other sources of information transmission in the market. In particular, information disclosure could be supported through venue-level reputations rather than individual-level reputations. In Table 3.10 we explore heterogeneity in the association between STI visibility and prices by whether the transaction was conducted in a venue such as brothel, nightclub, or massage parlor; or outside of a venue, primarily in the street. While brothels and nightclubs might have venue-level reputations, any effects outside of a venue would rely on individual reputations.

**Table 3.9**: STI Visibility and Sex Worker Characteristics

| *Dependent Variable:* | Transaction Price | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Any STI × Vis. × Age | 0.0556 | | |
| | (0.730) | | |
| Any STI × Age | -0.0661 | | |
| | (0.440) | | |
| Any STI × Vis. × Married | | 2.574 | |
| | | (0.150) | |
| Any STI × Married/Partnered | | -1.570 | |
| | | (0.115) | |
| Any STI × Vis. × Highest grade | | | -0.824** |
| | | | (0.050) |
| Any STI × Highest grade completed | | | 0.384* |
| | | | (0.085) |
| Any STI × Vis. | -3.209 | -3.085* | 1.731 |
| | (0.600) | (0.065) | (0.105) |
| Any STI | 1.927 | 0.928 | -1.462* |
| | (0.520) | (0.465) | (0.050) |
| Age | -0.0561*** | -0.0593*** | -0.0579*** |
| | (0.005) | (0.005) | (0.005) |
| Married/Partnered | -0.928* | -0.872* | -0.914** |
| | (0.050) | (0.055) | (0.050) |
| Has children | 0.164 | 0.144 | 0.148 |
| | (0.760) | (0.800) | (0.795) |
| Highest grade completed | 0.135** | 0.133** | 0.131** |
| | (0.020) | (0.020) | (0.020) |
| Work Location controls | Yes | Yes | Yes |
| Sex act controls | Yes | Yes | Yes |
| Observations | 7807 | 7807 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values in parentheses, clustered at city level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

**Table 3.10**: STI Visibility and Work Location

| *Dependent Variable:* | Transaction Price | |
|---|---|---|
| | (1) | (2) |
| Any STI × Visible × Venue | -2.995** | -3.082 |
| | (0.045) | (0.120) |
| Any STI × Venue | 1.799 | 1.865 |
| | (0.185) | (0.105) |
| Venue | -5.010 | -4.341 |
| | (0.450) | (0.315) |
| Any STI | -1.345 | -1.516 |
| | (0.205) | (0.145) |
| Any STI × Visible | 0.489 | 0.772 |
| | (0.460) | (0.395) |
| Sex act controls | No | Yes |
| Demographic controls | No | Yes |
| Work location controls | Yes | Yes |
| Observations | 7807 | 7807 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values in parentheses, clustered at city level. Analysis is conducted at the transaction level. The sample is restricted to transactions in the month prior to survey. Syphilis is classified as a visible STI, while gonorrhea and chlamydia are classified as invisible STIs. City fixed effects and controls for registration status included in all regressions.

The results in Table 3.10 are consistent with the hypothesis that venue reputations could generate the association between STI visibility and prices. The association between visible STIs and prices is most pronounced for transactions in venues, and there is no correlation between visible STIs and prices outside venues (Column 1). While the difference becomes marginally insignificant after including controls (Column 2), the coefficients are similar. The lack of significance appears to stem from larger standard errors after the inclusion of controls. .

## 3.7 Conclusion

This paper documents an association between the visibility of STI symptoms and transaction prices in Ecuador's female sex market. Women with visible STIs earn lower prices relative to both women without STIs and women wtih invisible STIs. There is no evidence that this association is moderated on the intensive or extensive margin by transactions with regular clients. Thus, we find partial support for a model of voluntary information disclosure in sex markets that requires (i) the possibility that a sex workers' STI status will be revealed during the transaction; and (ii) repeated interactions with clients. We are able to rule out a mechanism where sex workers with lower willingness-to-accept are also more likely to acquire visible STIs, and we present suggestive evidence consistent with information transmission supported by venue reputation effects rather than repeated interactions at the individual level.

The results suggest that there is likely some information transmission about sex workers' STI status in the market. However, this transmission is incomplete: there is no price response to the invisible STIs chlamydia and gonorrhea. Thus, there may be scope for information-based policy responses that provide a credible signal of the presence or absence of asymptomatic STIs to improve welfare.

## Acknowledgment

Chapter 3, in full, is currently being prepared for submission for publication of the material. Manian, Shanthi; Shah, Manisha. The dissertation author was the primary investigator and author of this material.

# References


Agence Nationale de la Statistique et de la Demographie (2013). Situtation Economique et Sociale du Senegal en 2011. Technical report.

Akerlof, G. A. (1970, aug). The Market for "Lemons": Quality Uncertainty and the Market Mechanism. *The Quarterly Journal of Economics 84*(3), 488.

Amadora-Nolasco, F., R. Alburo, E. Aguilar, and W. Trevathan (2001). Knowledge, Perception of Risk for HIV, and Condom Use: A Comparison of Registered and Freelance Female Sex Workers in Cebu City, Philippines. *AIDS and Behavior 5*(4), 319–330.

Anderson, M. L. (2008a, dec). Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association 103*(484), 1481–1495.

Anderson, M. L. (2008b, dec). Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association 103*(484), 1481–1495.

Aral, S. O. (2000, jul). Behavioral aspects of sexually transmitted diseases: core groups and bridge populations. *Sexually transmitted diseases 27*(6), 327–8.

Arunachalam, R. and M. Shah (2013). Compensated for Life: Sex Work and Disease Risk. *Journal of Human Resources 48*(2), 345–369.

Astorga, L. and D. A. Shirk (2010). Drug Trafficking Organizations and Counter-Drug Strategies in the U.S.-Mexican Context.

Banerjee, A. V., E. Duflo, R. Glennerster, and D. Kothari (2010, jan). Improving immunisation coverage in rural India: clustered randomised controlled evaluation of immunisation campaigns with and without incentives. *BMJ 340*.

Baral, S., C. Beyrer, K. Muessig, T. Poteat, A. L. Wirtz, M. R. Decker, S. G. Sherman, and D. Kerrigan (2012, jul). Burden of HIV among female sex workers in low-income and middle-income countries: a systematic review and meta-analysis. *The Lancet infectious diseases 12*(7), 538–49.

Baranov, V., D. Bennett, and H.-P. Kohler (2013). The indirect impact of antiretroviral therapy.

Bekker, L.-G., L. Johnson, F. Cowan, C. Overs, D. Besada, S. Hillier, and W. Cates (2014, jul). Combination HIV prevention for female sex workers: what is the evidence? *The Lancet*.

Blattman, C. and E. Miguel (2010, mar). Civil War. *Journal of Economic Literature 48*(1), 3–57.

Bound, J., D. A. Jaeger, and R. M. Baker (1995, jun). Problems with Instrumental Variables Estimation when the Correlation between the Instruments and the Endogenous Explanatory Variable is Weak. *Journal of the American Statistical Association 90*(430), 443–450.

Brown, R., V. Montalva, D. Thomas, and A. Velásquez (2015, mar). Impact of Violent Crime on Risk Aversion : Evidence from the Mexican Drug War.

Callen, M., M. Isaqzadeh, J. D. Long, and C. Sprenger (2014, jan). Violence and Risk Preference: Experimental Evidence from Afghanistan. *American Economic Review 104*(1), 123–148.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008, jul). Bootstrap-Based Improvements for Inference with Clustered Errors. *Review of Economics and Statistics 90*(3), 414–427.

Cameron, L. and M. Shah (2013, oct). Risk-Taking Behavior in the Wake of Natural Disasters.

Cassar, A., A. Healy, and C. V. Kessler (2011). Trust, Risk, And Time Preferences after a Natural Disaster: Experimental Evidence from Thailand.

Chandrasekar, P. H. (2016). Syphilis Clinical Presentation.

Cohen, M. S., Y. Q. Chen, M. McCauley, T. Gamble, M. C. Hosseinipour, N. Kumarasamy, J. G. Hakim, J. Kumwenda, B. Grinsztejn, J. H. S. Pilotto, S. V. Godbole, S. Mehendale, S. Chariyalertsak, B. R. Santos, K. H. Mayer, I. F. Hoffman, S. H. Eshleman, E. Piwowar-Manning, L. Wang, J. Makhema, L. A. Mills, G. de Bruyn, I. Sanne, J. Eron, J. Gallant, D. Havlir, S. Swindells, H. Ribaudo, V. Elharrar, D. Burns, T. E. Taha, K. Nielsen-Saines, D. Celentano, M. Essex, and T. R. Fleming (2011, aug). Prevention of HIV-1 infection with early antiretroviral therapy. *The New England journal of medicine 365*(6), 493–505.

Cwikel, J. G., T. Lazer, F. Press, and S. Lazer (2008, mar). Sexually transmissible infections among female sex workers: an international review with an emphasis on hard-to-access populations. *Sexual health 5*(1), 9–16.

Dardanoni, V. and A. Wagstaff (1990, jun). Uncertainty and the demand for medical care. *Journal of Health Economics 9*(1), 23–38.

de la Torre, A., A. Havenner, K. Adams, and J. Ng (2010, may). Premium sex: Factors influencing the negotiated price of unprotected sex by female sex workers in Mexico. *Journal of Applied Economics 13*(1), 67–90.

de Waal, A. (2010, jan). Reframing governance, security and conflict in the light of HIV/AIDS: a synthesis of findings from the AIDS, Security and Conflict Initiative. *Social science & medicine (1982) 70*(1), 114–20.

de Walque, D., H. Kazianga, and M. Over (2012, oct). Antiretroviral Therapy Perceived Efficacy and Risky Sexual Behaviors: Evidence from Mozambique. *Economic Development and Cultural Change 61*(1), 97–126.

Desclaux, A., M. Ciss, B. Taverne, P. S. Sow, M. Egrot, M. A. Faye, I. Lanièce, O. Sylla, E. Delaporte, and I. Ndoye (2003, jul). Access to antiretroviral drugs and AIDS management in Senegal. *AIDS (London, England) 17 Suppl 3*, S95–101.

Dimmock, S. G. (2012, aug). Background Risk and University Endowment Funds. *Review of Economics and Statistics 94*(3), 789–799.

Do Espirito Santo, M. E. G. and G. D. Etheredge (2004, jan). And then I became a prostitute ... Some aspects of prostitution and brothel prostitutes in Dakar, Senegal. *The Social Science Journal 41*(1), 137–146.

Dohmen, T., A. Falk, D. Huffman, U. Sunde, J. Schupp, and G. G. Wagner (2011, jun). Individual Risk Attitudes: Measurement, Determinants, and Behavioral Consequences. *Journal of the European Economic Association 9*(3), 522–550.

Dranove, D. and G. Z. Jin (2010, dec). Quality Disclosure and Certification: Theory and Practice. *Journal of Economic Literature 48*(4), 935–963.

Dranove, D. and A. Sfekas (2008, sep). Start spreading the news: A structural estimate of the effects of New York hospital report cards. *Journal of Health Economics 27*(5), 1201–1207.

Dupas, P. and J. Robinson (2012, nov). The (hidden) costs of political instability: Evidence from Kenya's 2007 election crisis. *Journal of Development Economics 99*(2), 314–329.

Eckel, C. and P. Grossman (2001, apr). Chivalry and solidarity in ultimatum games. *Economic Inquiry 39*(2), 171–188.

Eeckhoudt, L., C. Gollier, and H. Schlesinger (1996, may). Changes in Background Risk and Risk Taking Behavior. *Econometrica 64*(3), 683.

Elfenbein, D. W., R. Fisman, and B. McManus (2015, nov). Market Structure, Reputation, and the Value of Quality Certification. *American Economic Journal: Microeconomics 7*(4), 83–108.

Farley, T. A., D. A. Cohen, and W. Elkins (2003, apr). Asymptomatic sexually transmitted diseases: the case for screening. *Preventive Medicine 36*(4), 502–509.

Felbab-Brown, V. (2011, sep). Calderon's Caldron: Lessons from Mexico's Battle Against Organized Crime and Drug Trafficking in Tijuana, Ciudad Juarez, and Michoacan. Technical report, Brookings Institution.

Fortson, J. G. (2011, feb). Mortality Risk and Human Capital Investment: The Impact of HIV/AIDS in Sub-Saharan Africa. *Review of Economics and Statistics 93*(1), 1–15.

Friedman, W. (2015). Antiretroviral Drug Access and Behavior Change.

Gaines, T. L., M. L. Rusch, K. C. Brouwer, S. M. Goldenberg, R. Lozada, A. M. Robertson, E. Perkins, S. A. Strathdee, and T. L. Patterson (2013). Venue-level correlates of female sex worker registration status: a multilevel analysis of bars in Tijuana, Mexico. *Global public health 8*(4), 405–416.

Gaydos, C. A., T. C. Quinn, and J. C. Gaydos (2000, apr). The Challenge of Sexually Transmitted Diseases for the Military: What Has Changed? *Clinical Infectious Diseases 30*(4), 719–722.

Gerber, A. S. and D. P. Green (2012). *Field Experiments: Design, Analysis, and Interpretation*. W. W. Norton.

Gertler, P., M. Shah, and S. M. Bertozzi (2005, jun). Risky Business: The Market for Unprotected Commercial Sex. *Journal of Political Economy 113*(3), 518–550.

Gertler, P. J. and M. Shah (2011, nov). Sex Work and Infection: What's Law Enforcement Got to Do with It? *Journal of Law and Economics 54*(4), 811–840.

Ghys, P. D., M. O. Diallo, V. Ettiègne-Traoré, G. A. Satten, C. K. Anoma, C. Maurice, J. C. Kadjo, I. M. Coulibaly, S. Z. Wiktor, A. E. Greenberg, and M. Laga (2001, jul). Effect of interventions to control sexually transmitted disease on the incidence of HIV infection in female sex workers. *AIDS (London, England) 15*(11), 1421–31.

Gollier, C. and J. W. Pratt (1996, sep). Risk Vulnerability and the Tempering Effect of Background Risk. *Econometrica 64*(5), 1109+.

Grossman, M. (2000). Chapter 7 The human capital model. *Handbook of Health Economics 1*, 347–408.

Guiso, L. and M. Paiella (2008, dec). Risk Aversion, Wealth, and Background Risk. *Journal of the European Economic Association 6*(6), 1109–1150.

Gutiérrez, J. P., E. E. Atienzo, S. M. Bertozzi, and S. McPherson (2013). Effects of the Frontiers Prevention Project in Ecuador on sexual behaviours and sexually transmitted infections amongst men who have sex with men and female sex workers: challenges on evaluating complex interventions. *Journal of Development Effectiveness 5*(2), 158–177.

Harcourt, C., J. O'Connor, S. Egger, C. K. Fairley, H. Wand, M. Y. Chen, L. Marshall, J. M. Kaldor, and B. Donovan (2010, oct). The decriminalization of prostitution is associated with better coverage of health promotion programs for sex workers. *Australian and New Zealand journal of public health 34*(5), 482–486.

Harrison, G. W., J. A. List, and C. Towe (2007, mar). Naturally Occurring Preferences and Exogenous Laboratory Experiments: A Case Study of Risk Aversion. *Econometrica 75*(2), 433–458.

Heaton, J. and D. Lucas (2000, jan). Portfolio Choice in the Presence of Background Risk. *The Economic Journal 110*(460), 1–26.

Homaifar, N. (2006). Taking a step towards prevention. *Exchange on HIV and AIDS, Sexuality and Gender*, 14–15.

Homaifar, N. and S. Z. Wasik (2005, feb). Interviews with senegalese commercial sex trade workers and implications for social programming. *Health care for women international 26*(2), 118–33.

Instituto Nacional de Estadística y Geografía (2010). Censo de Población y Vivienda. Technical report.

Internal Displacement Monitoring Centre (2012). Forced displacement linked to transnational organised crime in Mexico. Technical Report May.

Iqbal, Z. and C. Zorn (2010, jan). Violent Conflict and the Spread of HIV/AIDS in Africa. *The Journal of Politics 72*(01), 149.

Jakiela, P. and O. Ozier (2015). The Impact of Violence on Individual Risk Preferences : Evidence from a Natural Experiment.

Jayachandran, S. (2006, jun). Selling Labor Low: Wage Responses to Productivity Shocks in Developing Countries. *Journal of Political Economy 114*(3), 538–575.

Jayachandran, S. and A. Lleras-Muney (2009, feb). Life Expectancy and Human Capital Investments: Evidence from Maternal Mortality Declines. *Quarterly Journal of Economics 124*(1), 349–397.

Jin, G. Z. and P. Leslie (2009, jan). Reputational Incentives for Restaurant Hygiene. *American Economic Journal: Microeconomics 1*(1), 237–267.

Kerrigan, D., L. Moreno, S. Rosario, B. Gomez, H. Jerez, C. Barrington, E. Weiss, and M. Sweat (2006, jan). Environmental-structural interventions to reduce HIV/STI risk among female sex workers in the Dominican Republic. *American journal of public health 96*(1), 120–5.

Kihlstrom, R. E. and J.-J. Laffont (1979, sep). A General Equilibrium Entrepreneurial Theory of Firm Formation Based on Risk Aversion. *Journal of Political Economy 87*(4), 719–748.

Kochar, A. (1999, feb). Smoothing Consumption by Smoothing Income: Hours-of-Work Responses to Idiosyncratic Agricultural Shocks in Rural India. *Review of Economics and Statistics 81*(1), 50–61.

Lagarde, E., M. Caraël, J. R. Glynn, L. Kanhonou, S. C. Abega, M. Kahindo, R. Musonda, B. Auvert, A. Buvé, and Study Group on the Heterogeneity of HIV Epidemics in African Cities (2001, jul). Educational level is associated with condom use within non-spousal partnerships in four cities of sub-Saharan Africa. *AIDS (London, England) 15*(11), 1399–1408.

Lajous, M., N. Mueller, A. Cruz-Valdéz, L. V. Aguilar, S. Franceschi, M. Hernández-Ávila, and E. Lazcano-Ponce (2005, jul). Determinants of Prevalence, Acquisition, and Persistence of Human Papillomavirus in Healthy Mexican Military Men. *Cancer Epidemiology Biomarkers & Prevention 14*(7), 1710–1716.

Leland, H. E. (1979, jan). Quacks, Lemons, and Licensing: A Theory of Minimum Quality Standards. *Journal of Political Economy 87*(6), 1328.

Liu, T. I. and R. So (1996, dec). Knowledge, attitude, and preventive practice survey regarding AIDS comparing registered to freelance commercial sex workers in Iloilo City, Philippines. *The Southeast Asian journal of tropical medicine and public health 27*(4), 696–702.

Logan, T. D. and M. Shah (2013, jan). Face Value: Information and Signaling in an Illegal Market. *Southern Economic Journal 79*(3), 529–564.

Lowndes, C. M., M. Alary, and L. Platt (2003, jan). Injection Drug Use, Commercial Sex Work, and the HIV/STI Epidemic in the Russian Federation. *Sexually Transmitted Diseases 30*(1), 46–48.

Lusk, J. L. and K. H. Coble (2008, aug). Risk aversion in the presence of background risk: Evidence from an economic experiment. In J. C. Cox and G. W. Harrison (Eds.), *Risk Aversion in Experiments*, Volume 12 of *Research in Experimental Economicss*, pp. 315–340. Emerald Group Publishing Limited.

Lyons, C. E., S. Ketende, D. Diouf, F. M. Drame, B. Liestman, K. Coly, C. Ndour, G. Turpin, S. Mboup, K. Diop, C. Toure-Kane, D. Castor, N. Leye-Diouf, and S. Baral (2017, jan). Potential Impact of Integrated Stigma Mitigation Interventions in Improving HIV/AIDS Service Delivery and Uptake for Key Populations in Senegal. *Journal of acquired immune deficiency syndromes (1999) 74 Suppl 1*(Suppl 1), S52–S59.

Manian, S. (2016). Health Certification in the Market for Sex Work: A Field Experiment in Dakar, Senegal. pp. 1–47.

Mccluskey, J. J. (2000). A Game Theoretic Approach to Organic Foods : An Analysis of Asymmetric Information and Policy. *Agricultural and Resource Economics Review 29/1*(April), 1–9.

Mechoulan, S. Risky Sexual Behavior, Testing, and HIV Treatments. *Forum for Health Economics & Policy 10*(2).

Meda, N., I. Ndoye, S. M,Boup, A. Wade, S. Ndiayee, C. Niang, F. Sarr, I. Diop, and M. Caraël (1999, jul). Low and stable HIV infection rates in Senegal: natural course of the epidemic or evidence for success of prevention? *AIDS 13*(11), 1397–1405.

Mgbako, C. and L. Smith (2009). Sex Work and Human Rights in Africa.

Mock, N., S. Duale, L. Brown, E. Mathys, H. O'Maonaigh, N. A. Husn, and S. Elliott (2004, oct). Conflict and HIV: A framework for risk assessment to prevent HIV in conflict-affected settings in Africa. *Emerging Themes in Epidemiology 1*(1), 6+.

Moffitt, R. (1983). An Economic Model of Welfare Stigma. *The American Economic Review 73*(5).

Ndoye, I., S. Mboup, A. De Schryver, E. Van Dyck, J. Moran, N. D. Samb, M. L. Sakho, I. Thior, A. Wade, D. L. Heymann, and A. Meheus (1998, jun). Diagnosis of sexually transmitted infections in female prostitutes in Dakar, Senegal. *Sexually transmitted infections 74 Suppl 1.*

Office of the Mexico Attorney-General (2006). Homicides presumably related to DTO rivalry.

Oster, E., I. Shoulson, and E. R. Dorsey (2013, aug). Limited Life Expectancy, Human Capital and Health Investments. *American Economic Review 103*(5), 1977–2002.

Oster, E. and R. Thornton (2011, jan). Menstruation, Sanitary Products, and School Attendance: Evidence from a Randomized Evaluation. *American Economic Journal: Applied Economics 3*(1), 91–100.

Patterson, T. L., S. Goldenberg, M. Gallardo, R. Lozada, S. J. Semple, P. Orozovich, D. Abramovitz, and S. A. Strathdee (2009, aug). Correlates of HIV, sexually transmitted infections, and associated high-risk behaviors among male clients of female sex workers in Tijuana, Mexico. *AIDS (London, England) 23*(13), 1765–71.

Patterson, T. L., B. Mausbach, R. Lozada, H. Staines-Orozco, S. J. Semple, M. Fraga-Vallejo, P. Orozovich, D. Abramovitz, A. de la Torre, H. Amaro, G. Martinez, C. Magis-Rodríguez, and S. A. Strathdee (2008, nov). Efficacy of a brief behavioral intervention to promote condom use among female sex workers in Tijuana and Ciudad Juarez, Mexico. *American journal of public health 98*(11), 2051–7.

Perlis, T. E., D. C. Des Jarlais, S. R. Friedman, K. Arasteh, and C. F. Turner (2004, jul). Audio-computerized self-interviewing versus face-to-face interviewing for research data collection at drug abuse treatment programs. *Addiction 99*(7), 885–896.

Quiggin, J. (2003, oct). Background risk in generalized expected utility theory. *Economic Theory 22*(3), 607–611.

Rao, V., I. Gupta, M. Lokshin, and S. Jana (2003, aug). Sex workers and the cost of safe sex: the compensating differential for condom use among Calcutta prostitutes. *Journal of Development Economics 71*(2), 585–603.

Richens, J., A. Copas, S. T. Sadiq, P. Kingori, O. McCarthy, V. Jones, P. Hay, K. Miles, R. Gilson, J. Imrie, and M. Pakianathan (2010, aug). A randomised controlled trial of computer-assisted interviewing in sexual health clinics. *Sexually Transmitted Infections 86*(4), 310–314.

Robertson, A. M., J. L. Syvertsen, H. Amaro, G. Martinez, M. G. Rangel, T. L. Patterson, and S. A. Strathdee (2013, may). Can't Buy My Love: A Typology of Female Sex Workers' Commercial Relationships in the Mexico-U.S. Border Region. *Journal of sex research*, 1–10.

Robinson, J. and E. Yeh (2011, jan). Transactional Sex as a Response to Risk in Western Kenya. *American Economic Journal: Applied Economics 3*(1), 35–64.

Rosenzweig, M. R. and H. P. Binswanger (1993, jan). Wealth, Weather Risk and the Composition and Profitability of Agricultural Investments. *The Economic Journal 103*(416), 56.

Scorgie, F., M. F. Chersich, I. Ntaganira, A. Gerbase, F. Lule, and Y.-R. Lo (2012, may). Socio-demographic characteristics and behavioral risk factors of female sex workers in sub-saharan Africa: a systematic review. *AIDS and behavior 16*(4), 920–33.

Shahmanesh, M., V. Patel, D. Mabey, and F. Cowan (2008, may). Effectiveness of interventions for the prevention of HIV and other sexually transmitted infections in female sex workers in resource poor setting: a systematic review. *Tropical medicine & international health : TM & IH 13*(5), 659–79.

Sirotin, N., S. A. Strathdee, R. Lozada, L. Nguyen, M. Gallardo, A. Vera, and T. L. Patterson (2010). A comparison of registered and unregistered female sex workers in Tijuana, Mexico. *Public health reports (Washington, D.C. : 1974) 125 Suppl*, 101–9.

Sistema Nacional de Información en Salud (2014). Defunciones: Bases de Datos en Formato Estándar.

Spence, M. (1973, aug). Job Market Signaling. *The Quarterly Journal of Economics 87*(3), 355.

Spiegel, P. B. (2004, sep). HIV/AIDS among conflict-affected and displaced populations: dispelling myths and taking action. *Disasters 28*(3), 322–39.

Spiegel, P. B., A. R. Bennedsen, J. Claass, L. Bruns, N. Patterson, D. Yiweza, and M. Schilperoord (2007, jun). Prevalence of HIV infection in conflict-affected and displaced people in seven sub-Saharan African countries: a systematic review. *Lancet 369*(9580), 2187–95.

Steen, R. and G. Dallabetta (2003, nov). Sexually transmitted infection control with sex workers: regular screening and presumptive treatment augment efforts to reduce risk and vulnerability. *Reproductive health matters 11*(22), 74–90.

Stock, J. and M. Yogo (2005). Testing for Weak Instruments in Linear IV Regression. In D. W. K. Andrews (Ed.), *Identification and Inference for Econometric Models*, pp. 80–108. New York: Cambridge University Press.

Strathdee, S. A., D. Abramovitz, R. Lozada, G. Martinez, M. G. Rangel, A. Vera, H. Staines, C. Magis-Rodriguez, and T. L. Patterson (2013, jan). Reductions in HIV/STI incidence and sharing of injection equipment among female sex workers who inject drugs: results from a randomized controlled trial. *PloS one 8*(6), e65812.

Strathdee, S. A., R. Lozada, S. J. Semple, P. Orozovich, M. Pu, H. Staines-Orozco, M. Fraga-Vallejo, H. Amaro, A. Delatorre, C. Magis-Rodríguez, and T. L. Patterson (2008, mar). Characteristics of female sex workers with US clients in two Mexico-US border cities. *Sexually transmitted diseases 35*(3), 263–268.

Strathdee, S. A., M. M. Philbin, S. J. Semple, M. Pu, P. Orozovich, G. Martinez, R. Lozada, M. Fraga, A. de la Torre, H. Staines, C. Magis-Rodríguez, and T. L. Patterson (2008, jan). Correlates of injection drug use among female sex workers in two Mexico-U.S. border cities. *Drug and alcohol dependence 92*(1-3), 132–40.

Thornton, R. L. (2008, dec). The Demand for, and Impact of, Learning HIV Status. *The American economic review 98*(5), 1829–1863.

Tucker, G. M. (2012, mar). The Invisible Challenge to HIV/AIDS Prevention: Clandestine Prostitution in Senegal. *Journal of International Women's Studies 13*(1).

UNAIDS (2012). Senegal: a success story of AIDS investments and impact.

UNAIDS (2015). AIDSinfo.

UNAIDS (2016). What are the different epidemiological scenarios?

van den Berg, M., R. Fort, and K. Burger (2009). Natural Hazards And Risk Aversion: Experimental Evidence From Latin America.

Vera, A., D. Abramovitz, R. Lozada, G. Martinez, M. G. Rangel, H. Staines, T. L. Patterson, and S. A. Strathdee (2012, aug). Mujer Mas Segura (Safer Women): a combination prevention intervention to reduce sexual and injection risks among female sex workers who inject drugs. *BMC public health 12*, 653+.

Voors, M. J., E. E. M. Nillesen, P. Verwimp, E. H. Bulte, R. Lensink, and D. P. V. Soest (2012, apr). Violent Conflict and Behavior: A Field Experiment in Burundi. *American Economic Review 102*(2), 941–964.

Vuylsteke, B. and S. Jana (2001). Reducing HIV Risk in Sex Workers, Their Clients and Partners. In P. R. Lamptey and H. D. Gayle (Eds.), *HIV/AIDS Prevention and Care in Resource Constrained Settings*, Chapter 8. Arlington, VA: Family Health International.

Weller, S. C. and K. Davis-Beaty (2002, jan). Condom effectiveness in reducing heterosexual HIV transmission. In S. C. Weller (Ed.), *Cochrane Database of Systematic Reviews*. Chichester, UK: John Wiley & Sons, Ltd.

Wooldridge, J. M. (2002, oct). *Econometric analysis of cross section and panel data* (1 ed.). MIT Press.

World Health Organization (2011). Preventing HIV in sex work settings in sub-Saharan Africa. Technical report, Geneva.

World Health Organization (2012, dec). Prevention and treatment of HIV and other sexually transmitted infections for sex workers in low- and middle-income countries: recommendations for a public health approach. Technical report.

World Health Organization (2014). Consolidated Guidelines on HIV prevention, diagnosis, treatment and care for key populations. Technical Report July, Geneva.

Xiao, M. (2010). Is quality accreditation effective? Evidence from the childcare market. *International Journal of Industrial Organization 28*(6), 708–721.