Conflict and Risky Health Behavior: Evidence from Mexico's Drug War

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Abstract

Risky health behaviors contribute to a large share of disease in developing countries, yet few papers have studied the effect of conflict on these behaviors. The canonical health capital model predicts that conflict should increase risky health behaviors: as the likelihood of survival falls, incentives to invest in preventive measures also fall, increasing risk-taking. However, recent findings from various violent contexts, including the drug war in Mexico, suggest the behavioral response to conflict may reduce risk-taking. In this paper, I identify the effect of insecurity on sexual risk-taking using unique panel data on female sex workers in Ciudad Juarez, Mexico. I show that more intense conflict generates a large reduction in risky sex transactions. I rule out several alternate explanations, including compositional changes in sex markets and changes in drug use. The results suggest that the behavioral response to insecurity can mitigate the negative effects of conflict on health.

Keywords: Sex work; Risky health behavior; Conflict; HIV

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An estimated two billion people live in countries affected by conflict or violence severe enough to affect development outcomes (World Bank, 2017). Several papers have now documented reduced-form impacts of exposure to conflict on health, with the bulk of the literature focusing on nutritional outcomes.[1] Risky health behaviors account for an increasing share of the burden of disease in developing countries, yet few papers have studied how health behaviors respond to insecurity (de Walque, 2014). In this paper, I identify the effect of insecurity on sexual risk-taking using a unique panel dataset on female sex workers in Mexico.

There are two potential mechanisms for how violent conflict may affect sexual risk-taking, and each generates a different prediction. On one hand, we have the canonical model of health demand: Grossman's (1972) health capital model. This model suggests that insecurity can affect health outcomes in two ways. First, the disruption of economic activity will affect the demand for health inputs (health care, preventive health products, food, etc.) through both price and income effects. However, conflict can also generate a direct behavioral response due to the increased risk of dying: as the likelihood of survival falls, the incentive to invest in preventive measures also falls, and risky health behaviors should increase. Thus, the health capital model predicts a behavioral response that worsens the effects of economic disruption. However, this result assumes constant risk preferences. On the other hand, we have a behavioral literature that largely finds that exposure to violence increases risk aversion, including in response to the drug war in Mexico (Callen et al., 2014; Brown et al., 2019; Jakiela and Ozier, 2019; Nasir, Rockmore and Tan, 2017; Moya, 2018).[2] This literature on conflict and risk preferences would suggests that conflict might increase preventive behaviors.

In this paper, I present evidence consistent with the latter mechanism: I show that the behavioral response to insecurity can be protective. I study this question in the context of a drug-related conflict in Ciudad Juarez, Mexico. 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

[1] Several papers have shown both short- and long-run impacts of childhood exposure to conflict on stunting or adult height (Bundervoet, Verwimp and Akresh, 2009; Akresh et al., 2012; Akresh, Lucchetti and Thirumurthy, 2012; Akbulut-Yuksel, 2014; Minoiu and Shemyakina, 2014). There is also substantial evidence that children exposed to conflict in utero have lower birthweights, consistent with effects of both malnutrition and stress in pregnant women (Camacho, 2008; Domingues, 2011; León, 2012; Nwokolo, 2015). The literature has also documented long-run effects of conflict exposure on other health outcomes, including mental health (Blattman and Annan, 2010; Singhal, 2018) and obesity (Akbulut-Yuksel, 2017).

[2] A notable exception is Voors et al. (2012), who studied the effect of conflict exposure in Burundi on risk preferences measured 6-16 years after the exposure. They find that conflict-exposed individuals exhibit more risk-seeking behavior in incentivized experimental games.

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, 2011).[3]

I exploit a unique data structure that permits identification off of daily variation in conflict intensity, measured as homicides recorded by the coroner's office.[4] This allows me to isolate the immediate effects of changes in the level of insecurity. Month fixed effects control for any channels that respond slowly or discontinuously to violence, including macroeconomic effects of the conflict. In addition, a panel structure allows me to use within-individual variation by including individual fixed effects, which is relatively rare in the micro-conflict literature.

The data on sexual behavior come from a behavioral intervention trial conducted during the conflict.[5] The trial recruited participants on a rolling basis, then scheduled subsequent surveys quarterly after baseline. 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 individual each time they are observed.

I show that conflict leads to a *reduction* in risky sex transactions. When the violence is more intense, sex workers are less likely to exchange unprotected sex. 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 risky sex with clients by 13.6 percentage points.[6] This result is robust to numerous checks and alternate specifications. I find similar results when I use an alternate measure of conflict intensity based on newspaper reports of drug-related homicides. Results are also similar if I define risky sex transactions using either a direct question about how often sex workers used condoms with clients, or indirectly by combining questions to construct the unprotected fraction of total sex acts. My preferred specification also passes a placebo test based on future homicides.

[3]Some of the existing economics literature on these events has argued that they represent a surge in violent crime, and that this is qualitatively different from a civil conflict (Nasir, Rockmore and Tan, 2017). However, homicide rates in Ciudad Juarez during this period were comparable to violent death rates in traditional armed conflict zones (Gilgen, 2011). The violence in Ciudad Juarez also led to large deployments of military and federal police. Finally, violent crime is a major contributor to insecurity in Latin America more generally—in 2014, Latin America recorded homicide rates almost 4 times higher than the global average, accounting for 30 percent of the world's homicides (Vilalta, Torres and Castillo, 2016).

[4]Cartels primarily engaged in homicides, as opposed to other violent activities, during this period (Astorga and Shirk, 2010).

[5]The trial began after the onset of conflict, so there is no pre- or post-conflict data. Trial participants were female sex workers who inject drugs (FSW-IDUs). See Strathdee et al. (2013) for a detailed description of the trial.

[6]To be precise, the main outcome of interest is a binary indicator of inconsistent condom use with clients over the past month, where inconsistent condom use is defined as in previous literature (Muñoz et al., 2010).

I also address external validity concerns related to the fact that the female sex workers in the sample participated in a short behavioral intervention at baseline focused on safer sexual behavior. If the behavioral intervention changed the way female sex workers responded to the violence, this might limit the generalizability of the findings. To address this, I show that the result is robust to focusing on later study visits, when study participants had not received any intervention for at least eight months. Additionally, I show that attrition, social desirability bias, and recall bias in reporting are not driving the result.

I extensively explore mechanisms that may be driving the result and conclude that the evidence is most consistent with an individual-level behavioral response. I rule out several alternate explanations for this result, including many potential compositional changes on both the supply side and the demand side of the sex market, and a mechanism related to drug use.

I focus on the quantity of risky sex exchanged because behavioral responses to conflict could be present among both sex workers and clients. In both cases, the health capital model predicts an increase in risky sex transactions. As both sex workers and clients face increased mortality risk due to more intense conflict, the health capital model predicts that they will reduce their willingness to engage in preventive health behaviors. Clients will be more likely to demand unprotected sex, and sex workers will be more likely to accept the transaction. In contrast, a risk preference mechanism would increase willingness to engage in preventive health behaviors among both sex workers and clients. This would generate a reduction in risky sex transactions. However, since there is a compensating wage differential for non-condom use in sex transactions, the predictions for the price of risky sex are ambiguous (Rao et al., 2003; Gertler, Shah and Bertozzi, 2005; Arunachalam and Shah, 2013). Theoretically, increased risk aversion among sex workers increases the price of risky sex: sex workers require more compensation in order to supply risky sex. In contrast, increased risk aversion among clients decreases the price of risky sex: clients reduce their willingness to pay for risky transactions. I find no robust evidence for price effects, suggesting that the behavioral response may be present among both sex workers and clients.

The primary contribution of this paper is to show that the behavioral response to insecurity can mitigate the negative effects of conflict on health. The paper is closely related to Dupas and Robinson (2012), which also studied the effect of conflict on risky sex transactions. In contrast to my finding, they find that sex workers report more risky sex transactions during and after a civil

conflict in Kenya. An important distinction, however, is that Dupas and Robinson (2012) study a region where there were few human casualties, but conflict caused major economic shocks. Thus, as they note, the effect they document is primarily an income effect; they study the response to *both* an environment of insecurity and related disruptions in markets.[7] My paper complements Dupas and Robinson (2012) by documenting how sexual behavior responds to increasing insecurity after controlling for income effects. My findings suggest that the behavioral response can lessen the negative effects of reduced income on health.

My results are consistent with the bulk of the literature finding that exposure to violent conflict increases risk aversion. Brown et al. (2019) and Nasir, Rockmore and Tan (2017) study the same drug-related violence across all of Mexico, using a nationally representative longitudinal survey. The survey measured respondents' risk aversion using hypothetical choices over monetary lotteries. Both papers find that risk aversion increased after the spike in violence. An implication of these previous studies is that the behavioral response to insecurity can mitigate the negative effects of conflict on health. If individuals become more risk averse during times of conflict, this may increase demand for preventive health investments that reduce health risks. My results support that conclusion. My results are also consistent with a related literature on the effect of natural disasters on risk preferences, suggesting that fear and uncertainty may be an important driver of the results. For example, van den Berg, Fort and Burger (2009), Cassar, Healy and von Kessler (2017), Cameron and Shah (2015), and Said, Afzal and Turner (2015) find greater risk aversion among individuals affected by natural disasters in Latin America, Thailand, Indonesia, and Pakistan respectively.[8]

My results push the literature forward by documenting effects of violence on "real-world" risky behaviors. With few exceptions, these papers elicit risk preferences by asking respondents to make hypothetical or real choices over monetary lotteries, and are unable to make claims about risk-taking

[7]Their findings are consistent with the income smoothing model, which 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, Shah and Bertozzi, 2005; Arunachalam and Shah, 2013), for FSWs, this translates into expanded supply of unprotected sex (Robinson and Yeh, 2011). It would be challenging to study income shocks in my setting. The identification strategy for my main results relies on daily or weekly variation in conflict intensity. However, it is difficult to find income shocks that systematically vary on a daily or weekly basis, particularly ones that are important in an urban setting. Future research could explore the effects of income shocks using changes in commodity prices, which influence food prices in urban areas. Indeed, previous literature has used agricultural export commodity prices to show that commodity price shocks may affect drug-related violence in rural areas (Dube, García-Ponce and Thom, 2016).

[8]Eckel and Grossman (2001) and Hanaoka, Shigeoka and Watanabe (2018) find that individuals make more risk loving choices in response to Hurricane Katrina in the United States and the Great East Japan Earthquake; it is perhaps notable that these are both developed countries.

beyond the laboratory setting.[9]

From a policy perspective, my results contribute to a large literature, recently reviewed by Verwimp, Justino and Brück (2018), on the wide-ranging effects of conflict on the well-being of both combatants and civilians. Many papers have studied other types of behavioral response to conflict. For example, previous work has demonstrated effects of conflict on entrepreneurial behaviors (Brück, Naudé and Verwimp, 2013), financial decision-making (Blumenstock, Callen and Ghani, 2014), and labor market participation (Ben Yishay and Pearlman, 2013; Velasquez, 2019).

I also contribute to an interdisciplinary literature on the relationship between violent conflict and HIV risk specifically.[10] 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; Becker, Theodosis and Kulkarni, 2008; de Waal, 2010; Iqbal and Zorn, 2010; Bennett et al., 2015). 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, since individuals at high risk for HIV acquisition and transmission may respond to conflict by reducing their risky behaviors. The results also have implications for how conflict may interact with HIV prevention programming. In this case, over 75 percent of respondents reported access to free condoms during the study period, but disruptions of HIV prevention services may be more significant in other settings. If more intense conflict increases condom use among female sex workers and their clients, this implies that 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. And more generally, the large literature on Mexico's health programs for the poor, including the celebrated *Progresa/Opportunidades* conditional cash transfer program and the *Seguro Popular* health insur-

[9]Cameron and Shah (2015) show that the risk aversion measure is associated with outcomes such as entrepreneurship in their setting. Hanaoka, Shigeoka and Watanabe (2018) find that men exposed to the earthquake engage in more gambling and drinking after exposure, but find no effect for women. None of these papers can distinguish between the effects of individual decision-marking and changes in risk perceptions due to economic disruption.

[10]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.

ance program, has not addressed how escalating violence in Mexico may have influenced program effectiveness.[11] My results are encouraging in suggesting that conflict may not undermine, and may even complement, the effectiveness of these programs.

The remainder of this paper is organized as follows. Section 1 provides more detailed qualitative background on the drug war in Mexico. Section 2 describes the dataset and variables used in the analysis. Section 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 4 reports the main results on inconsistent condom use, while robustness checks are shown in Section 5. Section 6 rules out alternate explanations for a reduction in risky sexual behavior due to conflict, and Section 7 provides a discussion of the results and conclusions.

1 Background: Mexico's Drug War

The drug war in Mexico has been a high-fatality conflict that has affected large swaths of the country. Summary statistics show that homicide victims were concentrated among Mexican men with less than secondary education (Table 1). However, the homicides were public, graphic, and widely discussed, so they had far-ranging impacts. Table 1 shows that 62 percent of homicides occurred in a public street. Decapitations, gruesome mutilation of victims, and public displays of victims' bodies were common (Vulliamy, 2009; Associated Press, 2008). Civilians were targeted, with widely publicized killings in supermarkets and outdoor parties (BBC News, 2010). Nationally representative surveys show that rising homicides in Mexico increased average citizens' fear of being assaulted both during the day and at night, and reduced their willingness to go out at night (Velasquez, 2019). Increasing evidence suggests that the effects of the violence have been widespread, affecting outcomes as varied as risk aversion and trust, elementary school achievement, teenage pregnancy, use of prenatal care, and adult labor market participation (Brown et al., 2019; Brown and Velásquez, 2017; Caudillo and Torche, 2014; Nasir, Rockmore and Tan, 2017; Tsaneva and Gunes, 2018; Torche and Villarreal, 2014; Ben Yishay and Pearlman, 2013; Velasquez, 2019).

The role of fear for one's life may have been exacerbated among the female sex workers in my sample. Since the sex workers in the sample are primarily street workers (Table 2), they were

[11] See Parker and Todd (2017) for a review of the literature on *Progresa/Opportunidades* and Knox (2018) on *Seguro Popular*.

more likely to have individual exposure to homicide risk. Qualitative reports at the time suggest that vulnerable populations were particularly targeted. In a newspaper article, Vulliamy (2009) quoted an ombudsman working in Ciudad Juárez for the Chihuahua human rights commission, who described how the cartels viewed their victims: "The majority of those killed or kidnapped are *malandros* [scoundrels]: down-and-outs, urchins, petty criminals and addicts... desperate, below poverty, whose death has no explanation, except as part of *limpia social* [social cleansing], the extermination of the lowest of the low."

Drug users and drug rehabilitation centers were also targeted in the violence (Torres and Caldwell, 2009; The Associated Press, 2017; Justice in Mexico Project, 2009). In one of the deadliest single attacks during the drug-related violence, in September 2009, 18 people were killed in a machine gun attack on a drug rehabilitation center, which was the third attack on a rehab center that year (Torres and Caldwell, 2009). This is likely to have been highly salient for the sample; by construction, all participants are injection drug users, and at baseline, 38 percent of the sample had previously sought treatment for their drug use.

Lastly, in Ciudad Juarez in particular, rising drug-related homicides interacted with a local history of gruesome sexual assault, mutilation, and homicide targeting vulnerable young women, including female sex workers (Kraul, 2003; Komisar, 2018). The Feminicide Database at the Colegio de la Frontera Norte documented 494 murders of young women in Ciudad Juarez between 1993 and 2007 (prior to the exacerbation of the drug-related violence), and many more women disappeared without resolution (Monárrez Fragoso, 2008; Swenson, 2017). These murders prompted activism in the city that brought international press to the problem of femicide in Ciudad Juarez and Mexico more broadly. Because of this history, reports of rising homicides may have induced more fear among women than the summary statistics would suggest.

2 Data and Sampling

The primary dataset comes from a randomized intervention trial to promote safe sex and safe injection practices in female sex workers who inject drugs.[12] The trial was conducted between November 2008 and August 2011 in Ciudad Juarez, Mexico. Participants were recruited on a

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

Table 1: Demographics of homicide victims during study period

	Mean	S.D.	Min.	Max.	Obs.
Female	0.088	0.28	0	1	7895
Age	31.4	11.2	0	99	7841
Mexican national	0.98	0.15	0	1	7902
Primary school or less	0.29	0.45	0	1	7902
Secondary school or less	0.50	0.50	0	1	7902
Died in public street	0.62	0.48	0	1	5587

rolling basis via targeted sampling. In collaboration with local 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, a short intervention, and three follow-up surveys.[13]

Participants attended four visits over the course of the study. During the first visit, they completed a baseline survey followed by a one-time, one-hour intervention on safer sex and safer injection behavior. All participants completed an intervention, but they were randomized to either a "didactic" or "interactive" intervention. The didactic sessions were lectures based on materials available at local free clinics. The interactive sessions were grounded in psychological theories of behavior change.[14] All interventions (both didactic and interactive) were completed within the one-hour session at the first visit. Participants received no further intervention after the first visit. The second, third, and fourth visits, which were scheduled 4, 8 and 12 months after the first visit, were dedicated entirely to completing surveys.

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

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

[14]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 injection 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.

(iv) shared injection paraphernalia. Though this high risk profile is not necessarily representative of the wider female sex worker population in Mexico, 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*a*). In Russia, estimates of injection drug use among FSWs range from 25 to 80 percent (Lowndes, Alary and Platt, 2003). And, 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*a*).

Risky sexual behavior is self-reported in the surveys.[15] The primary outcome 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 explore alternate definitions of this variable in robustness checks.

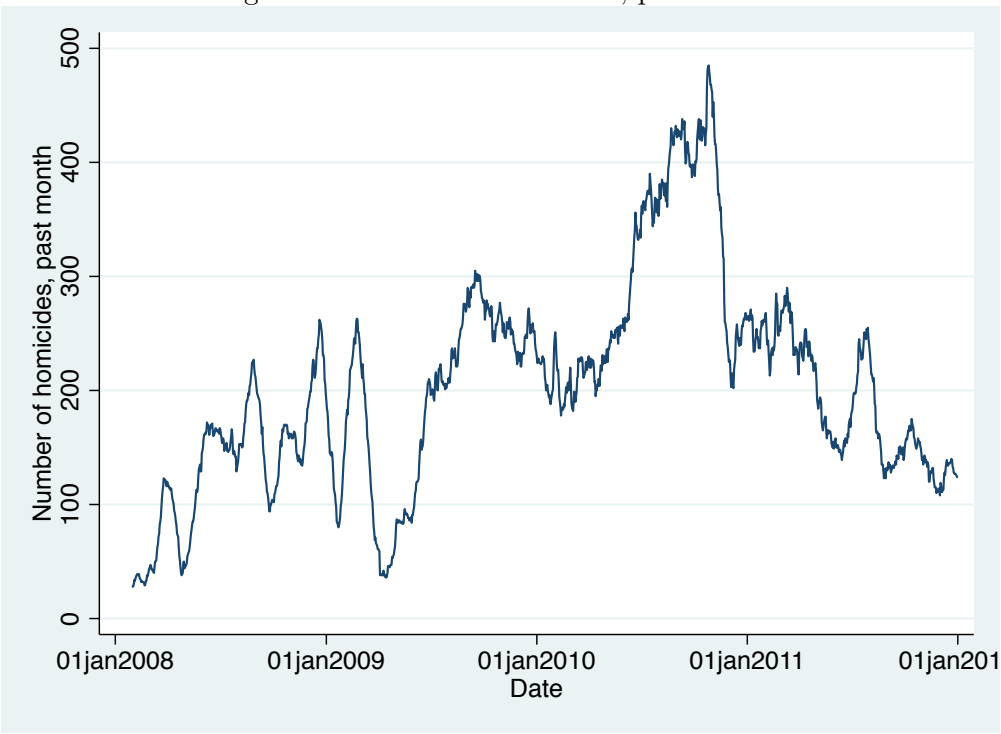
I use the total number of homicides in Ciudad Juarez, based on coroner's reports, as a measure of conflict intensity (Sistema Nacional de Información en Salud, 2014).[16] Homicides, which were the primary violent activity of the cartels, are almost universally used in the literature as a measure of conflict intensity in Mexico (Brown et al., 2019; Brown and Velásquez, 2017; Nasir, Rockmore and Tan, 2017; Tsaneva and Gunes, 2018; Torche and Villarreal, 2014; Ben Yishay and Pearlman, 2013; Velasquez, 2019). Homicides are favored because they are believed to suffer from less reporting bias than other violent crimes.[17] In addition, data on other types of violent crime are reported by the Executive Secretariat of the National Public Security System, and crimes only appear in the dataset if law enforcement established a formal criminal investigation. In contrast, the data I use

[15]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, the analysis is restricted to variables with a recall period of one month.

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

[17]Overall, Molzahn, RodriguezFerreira and Shirk (2013) estimate that fewer than 25% of crimes in Mexico are reported. They argue that homicides are more difficult to conceal because of the presence of a corpse, and thus have a higher rate of reporting.

Figure 1: Number of Homicides, past month



are based on coroner's reports and do not rely on determinations by the police or military, who were themselves important actors in the drug-related conflict (Molzahn, RodriguezFerreira and Shirk, 2013). Homicides are observed daily in the data from the coroner's office.

In the analysis, I construct a rolling sum of 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. 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.Participants could reschedule interviews, but the mean delay between scheduled and completed visits is just 9 days (the median delay is 2 days).

The baseline sample is 300 participants. Of these, 210 have a complete set of four observations

on the primary outcome.[18] Appendix Table A1 compares 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 2. Participants are demographically similar to the wider female sex worker population in Ciudad Juarez and elsewhere (Gertler, Shah and Bertozzi, 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 drugs more than 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*a*), 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 (Figueroa, 2014). 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).

[18] 79 participants completed fewer than four visits. An additional 11 had missing data for the 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.

Table 2: 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.

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 A1 and Appendix Table A2).

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 α_i. To allow for arbitrary correlation between α_i and the regressors, I follow the standard approach of using individual fixed effects.

Second, given that the safer sex intervention directly targeted the outcome of interest, treatment status could be an important omitted variable. Table 3 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. In addition, in Appendix Table A3, I show that the treatments had no detectable short-term or long-term effects on inconsistent condom use. Nevertheless, I conservatively control for 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).

Table 3: 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.

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 Δu_{iv} and Δu_{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 α_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:

$$ICU_{iv,t} = \beta_0 + \beta_1 H_t + \sum_{v=2}^{4} \lambda_v + \sum_{v=2}^{4}\sum_{j=2}^{4} \lambda_v \tau_i^j + g(t) + \alpha_i + u_{iv,t} \quad (1)$$

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.

4 Main Results

Table 4 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 on the outcomes; lagged homicides control for this bias.

Columns 3-5 control more flexibly for time by including quarter fixed effects and month fixed effects. Columns 4 and 5 omit 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 5 as the preferred specification.

In the second row from the bottom of Table 4, I report p-values on the null hypothesis of serially uncorrelated errors, as described in Section 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 A4 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 4, 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 4 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

Table 4: Effect of Homicides on Unprotected Sex

Dependent Variable:	Inconsistent Condom Use				
	(1)	(2)	(3)	(4)	(5)
Homicides during recall month	-0.0918***	-0.109***	-0.133***	-0.112*	-0.155**
	(0.0331)	(0.0338)	(0.0467)	(0.0648)	(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***	0.516*	0.627***	0.770**
	(0.142)	(0.151)	(0.278)	(0.186)	(0.347)
Indiv. FE	X	X	X	X	X
Time trend	X	X			
Month-of-Year FE	X	X	X		
Quarter FE			X		
Month FE				X	X
Observations	1070	1070	1070	1070	1070
Number of id	300	300	300	300	300
P-value: Serially correlated errors	0.730	0.779	0.507	0.484	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. All specifications are linear probability models with individual fixed effects. Visit dummies and their interactions with treatment group indicators, as described in Section 3, are included in all specifications.

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.

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 5.1, I show that the results are robust to using an alternate measure of conflict intensity based on newspaper reports of drug-related homicides.

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 D.2, 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 5.2, 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. Results are robust to controlling for these factors. A full discussion of this analysis and results are shown in Appendix Section D.4.

Third, a behavioral intervention that promotes safer sex could change the way participants respond to violence, raising concerns about the generalizability of the results. The results in Section 5.2 also address this external validity concern. It is unlikely that a one-time, hour-long behavioral intervention would change the behavioral response to violence 8 to 12 months later. Thus, the finding that the effect is larger in magnitude during later visits supports the external validity of the results.

In the Appendix, I present additional robustness checks, including further evidence against social desirability bias (Section D.5), different specifications of the dependent variable (Section D.6), and different assumptions on attrition (Section D.8). I also explore whether the conflict affects other types of risky behavior among the female sex workers in my sample (Section D.7). I find no evidence that conflict intensity affects alcohol use or risky drug use. There are two possible reasons for this. First, since the sex workers in the sample have a very high frequency of substance use, it may have been more difficult for them to adjust their substance use behavior than their sexual behavior. Second, the main results may be driven primarily by clients.

Lastly, in Appendix Section D.9, I study whether other types of violent crimes, which could be drug-related, induce the same response as homicides. These violent crime data are available only at a monthly frequency and at the state level, and because they are collected from police reports, they are believed to suffer from more measurement error than the coroner's reports. Nevertheless, while there is no detectable reduction in risky behavior in response to kidnappings and assaults, I do find suggestive evidence of a similar response to violent robberies.

5.1 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 and began classifying more deaths as homicides (Kolb, 2012).[19] 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

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

in Ciudad Juarez (Office of the Mexico Attorney-General, 2011). 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 were compiled by the Trans Border Institute (TBI) based on a tally of drug-related homicides kept by the national newspaper *Reforma*. Three major newspapers in Mexico reported weekly drug-related homicides; TBI selected the *Reforma* tallies because they 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,[20] and homicides are observed at the weekly level rather than the daily level,[21] generating greater measurement error.

Table 5 replicates the main results (Table 4) 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 two specifications (Column 3 and 4). The point estimates are smaller in magnitude than in Table 4, but this is consistent with greater measurement error due to reporting at the state and week level. The coefficient in the preferred specification (Column 5) 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.

5.2 Social desirability bias and intervention effects

This robustness check specifically addresses potential effects of the intervention, in which both control and treatment individuals received information about the importance of condom use. The intervention could change the way that participants respond to violence, or it could change the degree of social desirability bias in their responses. Since individual fixed effects already control for

[20] 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, 2011)

[21] "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 5: Robustness Check: Alternate measure of conflict intensity

Dependent Variable:	Inconsistent Condom Use				
	(1)	(2)	(3)	(4)	(5)
Homicides during recall month (TBI)	-0.0655**	-0.0688*	-0.0574	-0.107	-0.177**
	(0.0317)	(0.0358)	(0.0534)	(0.0797)	(0.0895)
Homicides -3m (TBI)		-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	0.627***	0.764
	(0.143)	(0.145)	(0.295)	(0.209)	(0.475)
Indiv. FE	X	X	X	X	X
Time trend	X	X			
Month-of-Year FE	X	X	X		
Quarter FE			X		
Month FE				X	X
Observations	1070	1070	1070	1070	1070
Number of id	300	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 3, are included in all specifications.

Table 6: Effect over time

Dependent Variable:	Inconsistent Condom Use				
	(1)	(2)	(3)	(4)	(5)
Homicides during recall	-0.0655*	-0.0673*	-0.105*	-0.0691	-0.112
	(0.0390)	(0.0385)	(0.0571)	(0.0721)	(0.0791)
Homicides during recall X Later Visit	-0.0549	-0.107**	-0.0505	-0.0848	-0.0834
	(0.0414)	(0.0479)	(0.0665)	(0.0767)	(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**	0.532**	0.578***	0.745**
	(0.157)	(0.184)	(0.265)	(0.191)	(0.347)
Indiv. FE	X	X	X	X	X
Time trend	X	X			
Month-of-Year FE	X	X	X		
Quarter FE			X		
Month FE				X	X
Observations	1070	1070	1070	1070	1070
Number of id	300	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 3, are included in all specifications.

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 6 shows the differential effect of homicides during later visits (visits 3 and 4), 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.

6 Mechanisms

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 consistent with increased risk aversion among sex workers or clients due to violence, and if it represents an individual-level response, it is inconsistent with the health capital model. Therefore, it is important to understand whether an individual-level response is the likely mechanism. In this section, I review alternate mechanisms that could generate the observed reductions in risky sex during violent periods. I consider (i) compositional 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 preferences are an important part of the story.

The main results are consistent with a behavioral response by sex workers, clients, or both. I also use the price analyses in this section to evaluate whether there is evidence that either sex workers or clients are driving the result. The results are consistent with a behavioral response among both sex workers and clients.

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 the quantity exchanged of both protected and unprotected sex, but it could generate a reduction in risky sex due solely to income effects if there is substitution away from unprotected sex toward lower-priced protected sex. Table 7 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

Table 7: 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)
Indiv. FE	X	X	X	X
Month FE	X	X	X	X
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.

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 7 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.[22] 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.

[22]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.

There is some evidence for lag effects. Table 7, Column 3 suggests that homicides may decrease the price of unprotected sex one month later, and Column 4 indicates a corresponding effect on the premium. However, in the second-to-last row of Table 7, I show results for the test of serially correlated errors described in Section 3. The errors in Column 3 are serially correlated, so this result should be seen as suggestive. Similarly, the result in Column 4 is not robust to different specifications of the time trend; in Table A13 in the Appendix, I show that the negative effect of lagged homicides on the price premium is no longer significant when controlling for quarterly rather than monthly fixed effects, and the effect becomes positive when controlling for a linear time trend. In contrast, the main results in Table 4 are quite robust across specifications of the time trend. Taken together, the results indicate that changes in overall demand cannot explain the main result.

These results are also helpful for distinguishing between a supply-side mechanism (i.e., sex worker risk preferences) and a demand-side mechanism (i.e., client risk preferences). If sex workers become more risk averse, their willingness to engage in unprotected sex will decline. On the other hand, if clients become more risk averse, their willingness to pay for unprotected sex will decline. In both cases, fewer unprotected sex transactions will occur, but the implications for prices will be different. If sex workers' risk preferences alone are driving the result, we will observe higher prices for unprotected sex relative to condom use: sex workers will require higher compensation to engage in this health risk. Alternatively, if clients' risk preferences alone are driving the result, we will observe lower prices for unprotected sex relative to condom use: clients will have lower willingness to pay for the riskier option. The absence of robust effects on either the price of unprotected sex, or the price premium for unprotected sex, is consistent with a behavioral response among *both* sex workers and clients.

6.2 Demand-side compositional effects

This section evaluates whether 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

Table 8: Client Composition

Panel A: Effect of Conflict on Client Types				
	(1)	(2)		
	Reg. Clts	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)		
Indiv. FE	X	X		
Month FE	X	X		
Panel B: Role of Composition in Main Results				
	(1)	(2)	(3)	(4)
	ICU	ICU	ICU	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)
Indiv. FE	X	X	X	X
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 3.

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 8. 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 result. I estimate Equation 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 4, 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.[23]

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; Figueroa, 2014; Rice, 2011). Thus, more intense conflict could shift the composition of the client pool away from U.S. clients and toward Mexican clients.

[23]This analysis follows the mediation analysis approach developed in psychology (MacKinnon, Fairchild and Fritz, 2007) and frequently used in economics (Angrist and Pischke, 2009). However, because the number of regular clients is affected by the violence, it is a "bad control" in the sense of Angrist and Pischke (2009). That is, once the number of regular clients is included in a control, the main coefficient on homicides is, in effect, estimated on a selected sample, and it therefore no longer a good causal estimate. Nevertheless, this analysis, and subsequent similar analyses, are helpful in understanding the extent to which compositional changes can explain the causal effect in Table 4. In general, compositional changes appear to explain very little of the causal effect.

In previous work in Tijuana and Ciudad Juarez, FSWs who had clients from the U.S. exchanged more unprotected sex (Strathdee et al., 2008*b*), 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.[24] Table 8, 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.[25]

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 in March 2009 and 2,000 federal police in January 2010 (Ellingwood, 2009; El Universal, 2010).[26] There is a strong historical association between the military and consumption of commercial sex (Gaydos, Quinn and Gaydos, 2000; Lajous et al.,

[24]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.

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

[26]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.

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 8 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 8 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 economic impacts 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).

6.3 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. 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. 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 9, I show results from regressing "ever exited" on inconsistent condom use at baseline. There is no statistical difference between those who exit and

Table 9: Supply-side Compositional Effects: Multinomial logit

	OLS		Multinomial logit	
	(1)	(2)	(3)	(4)
	ICU	Market Exit	Market Exit	Cons. Cond. Use
Ever Exited	0.0318			
	(0.0640)			
Homicides during recall month (hundreds)		-0.0995**	-0.138**	0.323***
		(0.0429)	(0.0644)	(0.0800)
Homicides -1m (hundreds)		-0.0644	-0.0405	0.0327
		(0.0498)	(0.0529)	(0.0966)
Homicides -2m (hundreds)		-0.0125	0.0184	0.0625
		(0.0439)	(0.0615)	(0.103)
Homicides -3m (hundreds)		0.0678	0.0927	0.0856
		(0.0525)	(0.0725)	(0.105)
Indiv. FE		X		
Month FE	X	X	X	X
Observations	299	1070	771	771
Number of id		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 2-4 : Standard errors in parentheses, clustered at sex worker level. Visit dummies and their interactions with treatment group indicators, as described in Section 3, are included in Columns 2-4. Columns 3 and 4 shows marginal effects from a pooled multinomial logit. Columns 3 and 4 specifications include within-individual averages of homicides, month fixed effects, visit dummies, and their interactions with treatment group indicators.

those who do not.

However, in Column 2, we see that contemporaneous homicides do reduce the probability of market exit.[27] To understand the impact of this effect, I use a multinomial logit to separately study three possible outcomes: inconsistent condom use (the referent group), consistent condom use, and market exit. Columns 3 and 4 of Table 9 show marginal effects from this analysis. Violence significantly reduces the likelihood of market exit relative to inconsistent condom use, but it also significantly increases the likelihood of consistent condom use relative to inconsistent condom use. The results indicate that supply-side compositional effects cannot fully explain the main results.

[27]However, there is marginal evidence of serial correlation in the error term (last row of Table 9), suggesting that the result could be spurious. A detailed discussion of this result is beyond the scope of this paper.

6.4 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*a*). 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 10 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

[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 10: Drug use by FSWs and clients

Dependent Variable:	Inconsistent Condom Use				
	(1)	(2)	(3)	(4)	(5)
Homicides during recall month (hundreds)	-0.160**	-0.183**	-0.175**	-0.141*	-0.175**
	(0.0743)	(0.0730)	(0.0742)	(0.0761)	(0.0768)
Homicides -1m (hundreds)	-0.101	-0.107	-0.0976	-0.0585	-0.0654
	(0.0858)	(0.0844)	(0.0847)	(0.0847)	(0.0834)
Homicides -2m (hundreds)	0.0769	0.109	0.0694	0.00249	0.0213
	(0.0892)	(0.0874)	(0.0872)	(0.0893)	(0.0874)
Homicides -3m (hundreds)	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 include individual fixed effects, month fixed effects, visit dummies, and their interactions with treatment group indicators, as described in Section 3.

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.

7 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 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. The results are most consistent with an individual-level behavioral response among sex workers and clients.

Conflict can affect several aspects of the decision to exchange unprotected sex, acting as both an income shock and a shock to life expectancy. In the standard health capital model, these effects generate an increase in sexual risk-taking, contrary to my results (Grossman, 2000). My findings suggest that risk attitudes may play an important role in individuals' health behavioral response to insecurity.

The paper thus 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)).

Finally, this paper suggests that risk attitudes respond temporarily to variation in conflict intensity. 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.

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Appendix

A Attrition and missing data

Table A1 compares 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.

Table A1: Baseline Means by Attrition Status

	Non-attritor	Attritor	Difference	p-value
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		

B Number of Visits and Violence

Figure A1 shows the distribution of visits by day. Interviews on any given day never represent more than 4 percent of the sample. Table A2 reports the 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.

Figure A1: Distribution of Visits by Day

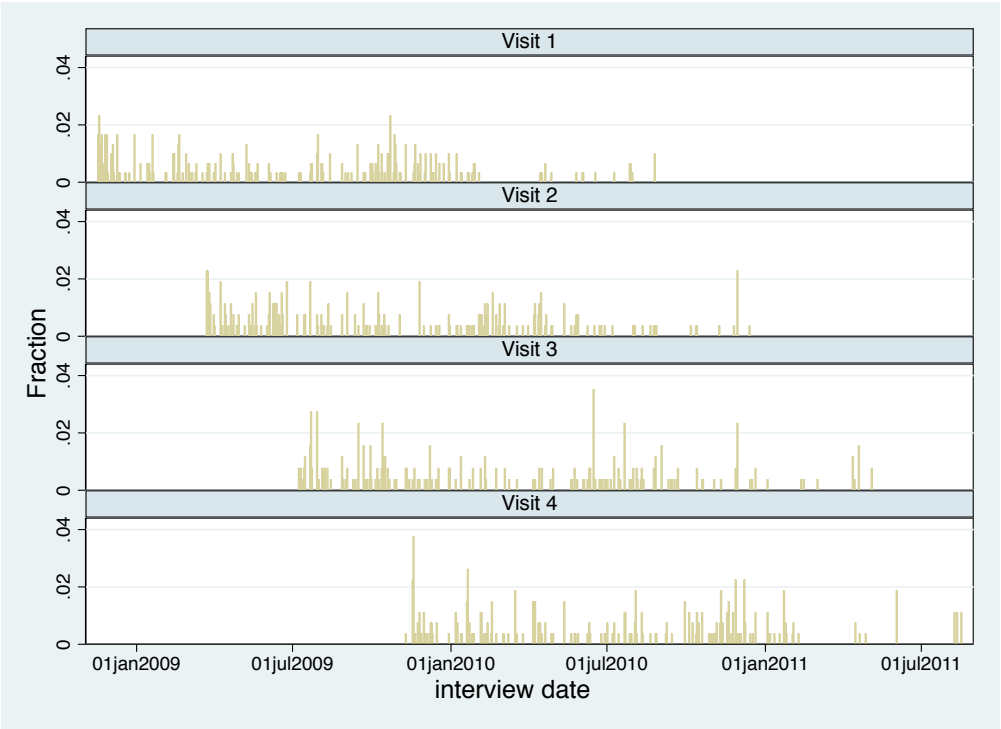

Table A2: Correlation between Violence and Number of Visits

	(1)
	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.

C Behavioral Intervention Treatment Effects

In this paper, I conduct a secondary analysis of data from a behavioral intervention trial. This section shows that the behavioral interventions had no short-term or long-term effects on inconsistent condom use. Inconsistent condom use was not the primary outcome in the original trial; the trial focused instead on combined HIV/sexually transmitted infection incidence—the number of new infections with HIV or any other sexually transmitted infection. The trial was also conducted in two cities—- Ciudad Juarez and Tijuana—and analyzed both sites jointly. The safer injection intervention alone had no treatment effect, but the safer sex intervention significantly reduced combined HIV/STI incidence across all sites (Strathdee et al., 2013).

Table A3: Behavioral Intervention Effects on Inconsistent Condom Use

Dependent Variable:	Inconsistent Condom Use					
	(1)	(2)	(3)	(4)	(5)	(6)
	Visit 2	Visit 2	Visit 2	Visit 4	Visit 4	Visit 4
Safer Sex=1	-0.0443		-0.116	-0.0605		-0.0325
	(0.0605)		(0.0859)	(0.0607)		(0.0872)
Safer Injection=1		-0.0587	-0.128		-0.0406	-0.0124
		(0.0603)	(0.0851)		(0.0607)	(0.0870)
Safer Sex=1 × Safer Injection=1			0.142			-0.0562
			(0.121)			(0.122)
Constant	0.278***	0.283***	0.337***	0.330***	0.319***	0.338***
	(0.0636)	(0.0615)	(0.0762)	(0.0644)	(0.0649)	(0.0762)
Baseline ICU	X	X	X	X	X	X
Observations	259	259	259	261	261	261

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. All specifications are linear probability models and include controls for the baseline value of inconsistent condom use to improve precision. Columns 1-3 study short-term treatment effects (at visit 2, after 4 months) and Columns 4-6 study long-term treatment effects (at Visit 4, after 1 year).

D Additional Robustness Checks

D.1 Placebo test

Table A4 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.

Table A4: 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 3, are included in all specifications.

Future homicides have no relationship with inconsistent condom use in the preferred specification.

D.2 Sexually transmitted infection incidence

Table A5 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.

D.3 Interaction of Behavioral Intervention and Violence

The main result is a secondary analysis of data from a randomized behavioral intervention trial. Although the behavioral interventions had no effect on inconsistent condom use (see Appendix

Table A5: 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.

Table A3), there may still be a concern that heterogenous treatment effects are driving the main result. However, Table A6 shows that there is no evidence for heterogeneity by treatment status. The effect is slightly smaller in magnitude in the control group alone, but it is not statistically different from the main effect, which pools all treatment groups. This analysis suggests that the reduction in risky behavior in response to violence is not due to the behavioral intervention.

D.4 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 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

Table A6: Interaction of Behavioral Intervention and Homicides

Dependent Variable:	Inconsistent Condom Use		
	(1)	(2)	(3)
Homicides during recall month (hundreds)	-0.145*	-0.144**	-0.126
	(0.0772)	(0.0722)	(0.0769)
Homicides -1m (hundreds)	-0.0909	-0.0946	-0.0955
	(0.0819)	(0.0828)	(0.0835)
Homicides -2m (hundreds)	0.0486	0.0432	0.0449
	(0.0844)	(0.0842)	(0.0844)
Homicides -3m (hundreds)	0.0126	0.0130	0.0143
	(0.0906)	(0.0905)	(0.0907)
Homicides (recall) X Safer Sex	-0.0177		
	(0.0427)		
Homicides (recall) X Safer Injection		-0.0243	
		(0.0439)	
Homicides (recall) X Injection			-0.0420
			(0.0640)
Homicides (recall) X Sexual			-0.0346
			(0.0566)
Homicides (recall) X Sexual & Injection			-0.0418
			(0.0587)
Constant	0.763**	0.782**	0.775**
	(0.349)	(0.350)	(0.352)
Indiv. FE	X	X	X
Month FE	X	X	X
Observations	1070	1070	1070

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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 3, are included in all specifications.

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 A7 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. Two observations are missing for total sex acts and six for negotiating condom use, due to nonresponse. 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.

D.5 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. The denominator is the total number of commercial sex acts, described in Section D.4. 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. The unprotected fraction is highly correlated with the ordinal measure within individual. A fixed effects regression of fraction unprotected on the ordinal measure yields a regression coefficient of 0.85.

Table A8 presents the effect of violence on this alternate measure of sexual risk-taking. The

Table A7: Robustness Check: 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 3, are included in all specifications.

Table A8: 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 3, are included in all specifications.

coefficient on contemporaneous homicides is negative in all 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.

D.6 Different specifications of dependent variable

Table A9 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 A9 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 9, 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. Observations that always used condoms and those that did not exchange sex are both coded as 0. 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 A9 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.

D.7 Other types of risky behavior

If the result is due to an individual-level behavioral response, we might expect those individuals 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, and similarly for clients. Unfortunately, I do not have data for other risky behaviors practiced by clients, but the surveys do provide data for sex workers on a number of risky behaviors related to drug use, including frequency of alcohol use, frequency of injection drug use, needle sharing, use of sterile needles, cleaning of needles, sharing of other injection equipment, and drug and alcohol use 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 (2008). 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. Table A10 reports the effect of violence on alcohol use, injection frequency, and 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

Table A9: 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.

Table A10: No Impact of Violence on Risky Substance Use

	(1) Alc. Freq.	(2) Alc. Freq.	(3) Inj. Freq.	(4) Inj. Freq.	(5) Drg Use	(6) Drg Use
Homicides during recall month	0.229	0.196	0.224	0.509	0.0390	0.0606
	(0.196)	(0.233)	(0.289)	(0.340)	(0.0371)	(0.0451)
Homicides -1m		0.401		0.182		0.0634
		(0.261)		(0.410)		(0.0528)
Homicides -2m		0.265		-0.672		0.00874
		(0.304)		(0.425)		(0.0599)
Homicides -3m		-0.465		0.198		0.00441
		(0.320)		(0.399)		(0.0571)
Constant	3.556***	3.664***	3.661***	3.194**	-0.197*	-0.357*
	(0.611)	(1.149)	(0.686)	(1.482)	(0.111)	(0.216)
Observations	1049	1049	1068	1068	1068	1068
Number of id	300	300	300	300	300	300
P-value: Long-term effect = 0	.	0.536	.	0.804	.	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. *Drg Use* is a summary index of risky substance use.

any specification.

There are two leading explanations for these results. First, it may be more difficult for heavy substance users to adjust their risk on the margin of substance use than on the margin of sexual behavior. Table 2 shows that nearly 80 percent of women in the sample injected drugs more than once a day. Also consistent with this notion, previous work from this study showed that injection cessation was extremely rare over the course of the study, and women in the sample had almost no access to evidence-based substance abuse treatment (West et al., 2016). Even the intensive injection risk intervention conducted by the RCT had only a small effect on risky behavior related to substance use (Strathdee et al., 2013). An alternate explanation is that the reduction in risky sex is driven primarily by clients (i.e., clients were less likely to request unprotected sex during violent periods).

D.8 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 A11, I interact this variable with contemporaneous homicides. In the presence of individual fixed effects, this 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 A11, 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 4) 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 A11 suggest that, if I had a full set of observations, the estimated effect would be even more negative.

D.9 Other violent crimes

In this section, I study the effect of three additional types of violent crimes on risky sexual behavior: kidnappings, assaults, and violent robberies. While municipal-level data are available for more recent years, only state-level, monthly data are available for the period under study in this paper.

Table A11: Robustness check: 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 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.

These data are compiled by the Executive Secretariat of the National Public Security System (SESNSP) and are based on police reports. Thus, while qualitative work indicates that kidnappings, assaults, and robberies were an important part of the drug-related violence, results in this section should be interpreted with considerable caution. Given that the federal and state police were key actors in the drug-related violence, the data are believed to suffer from substantial under-reporting, particularly for kidnappings and assaults.

Results on these other types of violent crime are shown in Table A12. Because the data are collected monthly, it is not possible to include month fixed effects in these specifications. Therefore, I present results for the next most conservative specification, with quarterly fixed effects and month-of-year fixed effects to control for seasonality. Consistent with measurement error and under-reporting of kidnappings and assaults, there is no detectable effect of these crimes on risky sexual behavior (Column 1 and 2).

There is, however, some evidence that exposure to violent robberies reduces risky sexual behavior (Column 3). This result, which is consistent with the main result for homicides, is statistically significant at the 10 percent level. The estimate is smaller in magnitude, but still significant at the 10 percent level, when controlling for a linear time trend rather than quarter fixed effects (not shown). There is no evidence that the result is driven by spurious regression, as shown in the second row from the bottom. The estimate suggests that a one standard-deviation increase in violent robberies in a month reduces the probability of report inconsistent condom use by 23 percentage points. There is also evidence for a compensating lag effect, so that there is no long-term effect of exposure to robberies on inconsistent condom use, as in the case of homicides.

Table A12: Effect of other violent crimes on risky sexual behavior

Dependent Variable:	Inconsistent Condom Use		
	(1)	(2)	(3)
Kidnappings -1m	0.509		
	(0.615)		
Kidnappings -2m	0.228		
	(0.484)		
Kidnappings -3m	1.204		
	(0.982)		
Kidnappings -4m	-0.826*		
	(0.438)		
Assaults -1m		-0.168	
		(0.280)	
Assaults -2m		-0.0908	
		(0.407)	
Assaults -3m		0.194	
		(0.328)	
Assaults -4m		0.0942	
		(0.372)	
Violent Robberies -1m			-0.169*
			(0.0886)
Violent Robberies -2m			0.0510
			(0.0543)
Violent Robberies -3m			0.0512
			(0.0538)
Violent Robberies -4m			0.224**
			(0.0911)
Constant	1.602***	1.432	0.250
	(0.481)	(2.219)	(0.621)
Indiv. FE	X	X	X
Month-of-Year FE	X	X	X
Quarter FE	X	X	X
Observations	1070	1070	1070
Number of id	300	300	300
P-value: Serially correlated errors	0.448	0.500	0.520
P-value: Long-term effect = 0	0.503	0.982	0.270

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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 3, are included in all specifications.

D.10 Robustness of effects of price premium

Table A13: Effect of homicides on price premium: Robustness

Dependent Variable:	Price Premium		
	(1)	(2)	(3)
Homicides during recall month (hundreds)	7.434	1.409	9.017
	(7.117)	(8.133)	(13.43)
Homicides -1m (hundreds)	4.663	-9.247	-30.49*
	(13.86)	(15.08)	(16.30)
Homicides -2m (hundreds)	0.834	-10.17	10.56
	(9.161)	(12.06)	(14.23)
Homicides -3m (hundreds)	-7.132	-17.47	35.85*
	(16.39)	(17.76)	(20.97)
Constant	3120.3	158.2***	66.58
	(2252.8)	(59.56)	(61.91)
Indiv. FE	X	X	X
Time trend	X		
Month-of-Year FE	X	X	
Quarter FE		X	
Month FE			X
Observations	910	910	910
Number of id	300	300	300
P-value: Serially correlated errors	0.832	0.664	0.375
P-value: Joint F test	0.792	0.435	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.