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Essays in Development and Labor Economics

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Jennifer Elizabeth Muz

Dissertation Committee:
Professor David Neumark, Chair
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2016

DEDICATION

To
my loved ones,

for your love
and
for sharing in the struggle.

"We are faltering,
but we must not
let it make us afraid
and perhaps surrender
the new things
we have gained."

Friedrich Nietzsche
Human, All Too Human

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ABSTRACT OF THE DISSERTATION

Essays in Development and Labor Economics

By

Jennifer Elizabeth Muz

Doctor of Philosophy in Economics

University of California, Irvine, 2016

Professor David Neumark, Chair

This thesis uses the tools of applied econometrics to study the impact of economic incentives on household welfare and decision-making and health and risk behaviors in the U.S. and in developing countries.

The first chapter studies the impact of increasing access to credit among low-income households in Mexico. Banco Azteca opened 815 branches simultaneously in a popular retail chain store, Grupo Elektra, in October 2002. Although access to credit increased, affected households experience negative or null impacts on consumption expenditures and asset holdings. I argue that this because the bank encourages individual borrowers to use loans for consumption use at Grupo Elektra. This research demonstrates that the package within which the loan is offered is as important as the loan itself.

The second chapter focuses on the impact of job loss to dual-earner married couples on household fertility decisions, drawing upon the recent experience of job loss during the Great Recession. I build two datasets, covering the years 2003-2011 that match job losses due to mass layoff events to fertility rates among married couples at the county-year and state-quarter level. I find that job losses have a negative impact on fertility. However, areas with more dual-earner households experience lesser declines in fertility rates in response to job losses, suggesting that dual-earner households are more likely to substitute toward

child-rearing in response to job loss compared to single-earner households.

The third chapter, joint with Lisa Cameron and Manisha Shah, exploits the criminalization of sex work in a district in East Java, Indonesia, and utilizes a unique dataset comprised of the first panel data on female sex workers and the first data on clients to estimate the impact of criminalizing sex work on health and risk behaviors. Criminalization increased STI rates among female sex workers by 58 percent. The main mechanism driving this increase is decreased access to condoms and increased non-condom use during commercial sex transactions. We rule out other mechanisms, such as increased transactions or clients per sex worker. This research presents new evidence that criminalizing sex work can put an already vulnerable population in a more precarious situation.

Chapter 1

The Impact of Microcredit on Household Welfare: The Case of Banco Azteca in Mexico

1.1 Introduction

Providing microcredit to the very poor has been one of the most celebrated innovations in poverty alleviation in the past 30 years, beginning with Grameen Bank in Bangladesh. Microcredit has been credited with reducing poverty (e.g., Burgess and Pande (2005)), providing capital funding for entrepreneurs, helping households smooth consumption (e.g., Banerjee et al. (2015) and Karlan and Zinman (2010*b*)), and increasing efficiency in labor markets by empowering females through targeted borrowing programs (e.g., Pitt and Khandker (1998); de Mel, McKenzie and Woodruff (2008); de Mel, McKenzie and Woodruff (2009); Banerjee et al. (2015)). However, recent evidence suggests that providing microcredit does not always achieve these goals. Several recent randomized controlled trials of the impact of microcredit

on household welfare find more ambiguous impacts (Banerjee et al., 2015; Angelucci, Karlan and Zinman, 2015; Crépon et al., 2015).[1] This paper proposes that the conflicting research on the impact of microcredit is due to differences in the characteristics of loans offered by microcredit organizations.

This study exploits the rollout of a Mexican bank, Banco Azteca, to provide evidence of a case in which increasing access to credit among low income households does not lead to improved household welfare. In October 2002, Grupo Elektra, a popular retail chain in Mexico, simultaneously opened 815 branches of Banco Azteca in all of their pre-existing stores, which are located in a subset of municipalities throughout Mexico. Employing a difference-in-differences strategy to compare municipalities that receive Banco Azteca branches to those that did not before and after the rollout of Banco Azteca, I analyze the impact of this rollout using the first two waves of the Mexican Family Life Survey (MxFLS), collected in 2002 and 2005, and branch location data from the National Banking and Securities Commission (CNBV in Spanish).[2] I find that, although Banco Azteca did increase access to credit among households in Mexico, this did not have a positive impact on household welfare. In fact, across several outcomes, Banco Azteca appears to have had a generally negative impact on household consumption and asset holdings. I discuss these findings in the context of several recent randomized controlled trials that also estimate the impacts of microcredit on household welfare and argue that the impact of microcredit expansion depends on the features of the loans offered by microcredit organizations.

Early microcredit organizations required that borrowers use loans for business purposes (i.e., productive loans), advertising possible future returns from successful business investment.

[1] Banerjee et al. (2015) study the introduction of Spandana, which offers group loans to women for both productive and consumption loans, and finds more negative impacts. Angelucci, Karlan and Zinman (2015) study Compartamos Banco, which offers group loans to women for productive purposes and find generally positive impacts on business expansion, household bargaining, borrower happiness and trust, school and medical expenses, and they find that households are less likely to go hungry or sell an asset for extra income. Crépon et al. (2015) also work with an MFI that offers group loans and emphasizes small business investment and finds positive impacts on business expansion, income, health, and some food expenditures.

[2] Comisión Nacional Bancario y de Valores

Loans were offered to peer groups in which members of a group guaranteed payment of everyone elses loans. This incentivized borrowers to moderate their borrowing behavior and make their regularly scheduled payments (Armendariz and Morduch, 2005). More recently, private, for-profit organizations have become common, offering individual liability loans. This second generation of privatized microcredit does not impose limits on loan use, is not as involved in the social and economic development of borrowers, and tends to charge higher interest rates than their predecessors (Cull, Demirguc-Kunt and Morduch, 2007). Because the second generation of microcredit has become similar to private banks that offer individual liability loans to earn a profit, households may be more likely use the loans in ways that result in worse long-term outcomes, which could explain the recent ambiguous welfare impacts.

The loan products offered by Banco Azteca have many characteristics similar to those offered by second generation microcredit organizations. Banco Azteca offers low- and middle-income households small, individual liability loans with flexible collateral requirements, which can be used for any purpose. However, because the branches are located within the Grupo Elektra stores, the loans are more likely to be used for consumption goods.[3] In fact, Grupo Elektra stores advertise the availability of loans through Banco Azteca to encourage large in-store purchases. Therefore, the impact of Banco Azteca on household welfare represents the impact of an increase in the availability of second generation microcredit loans (e.g., individual liability, no restrictions on loan use, less focus on entrepreneurship) when they are presented in their extreme forms.

There are several advantages of studying the rollout of Banco Azteca over increasing access to a microcredit institution via a randomized controlled trial (RCT), as is commonly seen in the recent literature. First, the locations of the Banco Azteca branches are widely distributed across the country, allowing my analysis to include a variety of communities and households

[3]Families primarily use the loans to purchase household luxury items such as a surround sound speaker system, a bicycle, a television, or bedroom furniture (Epstein, 2007). This was also conveyed to me through conversations with the Director of Investor Relations at Banco Azteca.

across Mexico. In contrast, RCTs typically cover smaller geographic regions, such as specific communities or cities. Thus, the estimated impacts of Banco Azteca are more generalizable to the Mexican population than would be the impacts from an RCT. Second, due to the structure of the data and the timing of the bank introduction, I am able to look at the impacts of the bank rollout over a relatively long time frame of three years, compared to one to two year windows seen in RCTs due to cost and time constraints (Angelucci, Karlan and Zinman, 2015; Banerjee et al., 2015; Crépon et al., 2015).

Finally, the microcredit organizations studied in RCTs commonly offer productive loans or a more balanced combination of both consumption and productive loans. Therefore, it is difficult to disentangle the welfare impacts of consumption loans from the productive loans offered by the same institution. Banco Azteca primarily offers consumption loans in a package similar to loans from microcredit organizations and, hence, provides evidence of the impact of increasing lines of credit for non-productive loans. This provides a base for disentangling the impacts of microcredit institutions that offer a combination of consumption and productive loans.

There are three other papers that also study the introduction of Banco Azteca using a similar estimation strategy. Two papers by Love and Bruhn (2011; 2014) study the impact of Banco Azteca on income and business activities and find positive impacts. Ruiz (2011) focuses on the impact of Banco Azteca on consumption smoothing behavior by informal households and finds that borrowing from banks increased among informal households. The current paper differs from those studies by focusing on the impacts on household consumption expenditures and asset holdings among all affected populations in Mexico. Changes in consumption expenditures and asset holdings are a more comprehensive measure of the impact of credit on household welfare than changes in income because consumption expenditures reflect the share of income that is disposable after making loan payments and paying off other bills. Moreover, changes in asset holdings reflect changes in the way households manage and store

wealth, which is not captured by changes in employment and labor income.

1.2 Banco Azteca as microcredit

1.2.1 The rollout of Banco Azteca

In August 2001, Grupo Elektra, one of Mexicos largest retailers for electronics and household goods, requested a bank license from the Ministry of Finance and Public Credit (SHCP in Spanish).[4] On May 23, 2002, Banco Azteca was approved as a Multiple Banking Institution, and on October 26, 2002, its doors were opened to the public. Grupo Elektra simultaneously opened 815 branches of Banco Azteca in all pre-existing Grupo Elektra stores.[5] By December of 2002 there were a total of 824 branches open across Mexico in every Grupo Elektra store. The opening of Banco Azteca had a non-trivial impact on the supply of credit to low income households across Mexico, representing a 15 percent increase in the supply of bank branches.[6]

Because the bank branches were located in Grupo Elektra stores, there was an incentive for store owners to encourage Grupo Elektra customers to become Banco Azteca borrowers to take out loans to make in store purchases. This aspect makes it likely that loans taken out from Banco Azteca were primarily used for consumption purposes. However, the loans from Banco Azteca were not exclusively tied to in store merchandise and could be used for any purpose. Additionally, many of the goods sold at Grupo Elektra could be used as capital in a

[4]Secretaría de Hacienda y Crédito Público (Ministry of Finance and Public Credit).

[5]In discussions with a Director at Banco Azteca, pinpointing the exact locations and lengths of tenure of the Grupo Elektra Stores is difficult, as the stores often change location within a community as different rental spaces become available and stores change ownership.

[6]As noted in Bruhn and Love (2011), Banco Aztecas loan portfolio was large relative to credit disbursed by other comparable microcredit institutions in Mexico at the time, highlighting its importance as a source of credit to low income households. Banco Aztecas loan portfolio grew quickly, increasing from around USD$196 million when it opened in 2002 to about USD$889 million in the last quarter of 2004. In comparison, the combined portfolio for the largest microfinance institutions in MexicoADMIC, Compartamos, FINCOMUN, and Pro Mujerwas USD$444.5 million in the fourth quarter of 2004 (Bruhn and Love, 2011). Further discussion of the details of Banco Azteca is available in Bruhn and Love (2011; 2014).

small business, such as electronics (e.g., televisions, radios, and telephones), large appliances (e.g., washing machines, refrigerators, and ovens/stoves), and furniture for the home and office. Therefore, it is possible that purchases made with the loans in the Grupo Elektra stores are used for business purposes as well as personal consumption.

1.2.2 Loan Characteristics

The primary interest of this paper is to examine the impact of the Banco Azteca expansion on household welfare in order to learn more about the welfare effects of second generation microcredit expansion. Therefore, it is prudent to discuss why Banco Azteca can be viewed through the same lens one would view a microcredit organization.[7] From the beginning, Banco Azteca catered to low- and middle-income households that are not traditionally serviced by banks. Households in the target population are characterized by individuals that have monthly incomes below USD$200 and who have a maximum of secondary education level, which comprised 65 percent of the Mexican population in 2003.

Banco Azteca does not require proof of income and has low collateral requirements. The basic process to acquire a loan requires the client to fill out a form, sign a contract, and provide official identification, a recent payroll statement or income tax form, and proof of property ownership (such as a tax form). However, if the individual does not have any proof of employment or land ownership, this does not disqualify him or her from loan approval. In this case, Banco Azteca requires an endorsing individual or collateral. According to MicroCaptial.org, almost half of Banco Aztecas clients cannot produce proof of income. Similar to microcredit institutions, Banco Azteca services small loans of less than USD$900 to be repaid in weekly installments. There are three repayment terms to choose from: 13 weeks (chosen by 1 percent of clients), 26 weeks (chosen by 8.29 percent of clients), or 39 weeks (chosen by 90.7 percent of clients) (Elektra, 2003). The loans carry an average annual

[7]Note that the bank does not exclude individuals that have higher incomes or education levels.

interest rate of around 55 percent with an effective APR of 110 percent.[8]

The interest rate is high, but prior to Banco Azteca, many of the households serviced by Banco Azteca were restricted to borrowing from pawn shops and moneylenders, which charged interest rates upwards of 220 percent over the same period (Ruiz, 2011). Therefore, the new interest rates would represent a welfare gain over previously available rates.[9] Additionally, the rates charged by Banco Azteca are comparable to other Microcredit institutions in Mexico. Average annual interest rate charged to customers in Mexico by microcredit institutions is 80 percent, and the interest rates reported by Banco Azteca are the same as those reported by Compartamos Angelucci, Karlan and Zinman (2015).[10]

Overall, Banco Azteca exhibits many attributes that characterize second generation microcredit organizations, which offer individual liability loans that can be used for both productive and non-productive activities. However, given Banco Aztecas location within retail stores, it is likely that a higher share of loans serviced by Banco Azteca is used for non-productive, consumption activities than the typical microcredit organization.

1.3 Data

To evaluate the impact of banking services on Mexican households, I use the first two waves of the MxFLS (2002 and 2005), a longitudinal survey of individuals and households, along with data on branch locations from the CNBV from the fourth quarter of 2002 until the fourth quarter of 2005. In addition, I use data from the Human Development Index of Mexican Municipalities (HDIMM), available in 2000 and 2005, to compare characteristics of

[8]The interest rate mentioned is based on conversations with officials at Banco Azteca. This is also the estimate commonly used in newspaper articles about the bank.

[9]Karlan and Zinman (2010*a*) find that an expansion of a credit institution that offered primarily consumer loans at 200 percent APR in South Africa resulted in net benefits to the affected population.

[10]These interest rates are high in comparison with the worldwide average annual interest rates of 28 percent and median annual interest rates of 26 percent for MFIs in 2006.

municipalities that received a Banco Azteca Branch and those that did not.

1.3.1 Household data

I construct a household level dataset from the MxFLS to estimate the impact of Banco Azteca on borrowing behavior and consumption outcomes. The MxFLS collects information on household assets, consumption expenditures, labor decisions, family business and agriculture activities, individual time use, borrowing history, and household decision-making, among other topics. In addition to data collected on households and individuals, there are community surveys that contain information on community infrastructure, health facilities, local schools, credit institutions, etc. The questions about borrowing behavior are targeted to individuals, so I combine the borrowing behavior and reported knowledge of individuals in the same household to create two indicators for the household as a whole. The first wave of the MxFLS (MxFLS-1) was conducted in the first half of 2002 and covers 8,441 households and 35,000 individuals across 150 Mexican communities; the second wave (MxFLS-2) was conducted in late 2005 to early 2006. Therefore, the MxFLS-1 was collected a minimum of two months prior to the opening of Banco Azteca, and MxFLS-2 collects follow-up data three years later (Rubalcava and Teruel, 2006*b*,*a*).

The sample is restricted to households that have location identifiers associated with their record and that have a head of household who is present in the first wave of the MxFLS, who is between the ages of 18 and 65, and who has marital status and educational attainment information in the data. I restrict the sample to households that have at least one member who responded to questions about borrowing behavior. After implementing the sample restrictions, the analysis includes 12,448 households4,616 households from non-Banco Azteca Municipalities over the two years, and 7,832 from Banco Azteca Municipalities. The distribution of households across the municipalities in the MxFLS correctly reflects the higher

average populations in the Banco Azteca municipalities. Table 1.3 presents summary statistics for the household sample. About 20 percent of household heads are female at baseline, with an average age of 42. About 70 percent of the household heads are married.

1.3.2 Bank data

I use data from the CNBV to identify the municipalities in which Banco Azteca opened a branch. These data are available quarterly at the level of the locality, which is similar in concept to a township in the United States; however, the MxFLS scrambles the locality identifiers so that they cannot be matched to outside data sources. Therefore, I aggregate the CNBV data to the municipality level to report the number of branches in each municipality in Mexico and match these municipalities to those sampled in the MxFLS.[11] The final analysis sample includes 136 municipalities in 16 states.

Table 1.1 shows the number of municipalities in the MxFLS sample that receive at least one Banco Azteca branch, as well as the number of branches based on the data from the CNBV. Of the 136 municipalities in my sample, 63 municipalities received a Banco Azteca Branch in 2002, and by 2005 this number had increased to 67. Sixty-nine municipalities received Banco Azteca Branches between 2002 and 2005, with branches opening and closing over the time period. The average number of branches per municipality in the fourth quarter of 2002 was 6.4; by the fourth quarter of 2005, this number had increased to 7.5, suggesting that the treatment of receiving a bank branch may have intensified over time in some municipalities.

[11] A municipality in Mexico is akin to a county in the United States. There are 32 states and, as of December 2005, there were 2,454 municipalities in Mexico. The average number of municipalities in a state is 76. The state with the most municipalities is Oaxaca with 570 municipalities and the state with the least municipalities is Baja California Sur with 5. The average population of a municipality was 45,758. The most populous municipality has a population of 1,815,786 and the least populous municipality has a population of 93. Therefore, there is wide variation in the size of municipalities.

1.4 Empirical Strategy

The main specification employs a difference-in-difference (DD) strategy, given by

$$Y_{imt} = \alpha + \delta(Azteca_m \cdot T2005_t) + \theta T2005_t + X'_{imt}\gamma + \mu_m + \epsilon_{imt}, \tag{1.1}$$

where Y_{ist} is the outcome of interest for FSW or client i at worksite s in time t (e.g., probability of having an STI, condom use, access to health exams, sex work activities, etc.); $Crim_s$ equals 1 if worksite s is a site where sex work is criminalized, 0 otherwise; $Endline_t$ equals 1 for the the period after the criminalization (i.e. data from endline surveys); and ϵ_{ist} is an error term for individual i at worksite s in time t. In all analyses, β_1 is the DD parameter estimate of interest and is reported in the tables.

where Y_{ist} is the outcome of interest for household i in municipality m at time t. The variable $Azteca_m$ is an indicator for having received a Banco Azteca branch by 2005 and is time invariant for municipalities in the MxFLS, $T2005_t$ is equal to one in 2005, and $Azteca_m \cdot T2005_t s$ an indicator equal to one for municipalities that received at least one Banco Azteca branch by 2005. I include a full set of municipality fixed effects, μ_m, which control for any time invariant differences between municipalities that may be correlated with Banco Azteca branch locations. I control for the gender, age, marital status and education of the household head, contained in the vector X_{imt}.[12] For all specifications, I cluster the standard

[12]In an earlier version of the paper I show a specification with additional municipality controls, including population, per capita income, infant mortality rate, the literacy rate, the rate of school attendance. The inclusion of these variables does not qualitatively change the point estimates. The reason to include these additional controls would be to account for potential trends in municipality characteristics that impact household welfare but would not be due to the entrance of Banco Azteca, which would bias the estimates. However, there is also reason to be concerned that one or more of these controls may be impacted by the entrance of Banco Azteca in the second period. For example, average per capita income in the municipality may have increased from 2002 to 2005 because of the new bank. If this is the case, then the causal estimate of the impact of Banco Azteca on consumption would also suffer from bias (Rosebaum 1984). Therefore, the specifications without these municipality controls are preferred. These results are not shown in the paper, but are available upon request.

errors at the municipality level, the level of treatment Bertrand, Duflo and Mullainathan (2004). I use the household weights constructed for MxFLS-1 in all specifications.

I classify a municipality as a Banco Azteca municipality if a branch entered the municipality at any time between 2002 and 2005; however, it is possible that this could count municipalities as having a Banco Azteca branch for which the branch closed in the middle. MxFLS survey participants are only asked about borrowing in the last 12 months, so any households in a municipality that received a Banco Azteca branch in 2002 or later that closed more than 12 months before MxFLS-2 would be considered treated, even though the household could not have borrowed from the bank in the time frame asked in the survey. This is the case for two municipalities. Nevertheless, the fact that the municipality had a Banco Azteca branch at one point may have impacted borrowing behavior and subsequent consumption outcomes, so I count those municipalities as treated in my estimation strategy.[13] This means that the estimated impact of the bank entrance will be combining effects in municipalities that received full and partial treatment, which may cause downward bias in the treatment effect, making the estimated effect a lower bound.

The span of time between surveys means that I will be primarily estimating long term impacts of increased credit access on household welfare. Looking at long term impacts allows time for the banks to become established in the communities and for any potential benefits from loans to manifest themselves. For example, it is likely that it takes one to two years to fully realize the returns to a small business investment Banerjee et al. (2015). At the same time, there are likely dynamic effects on consumption. When the bank first enters, there may be an increase in consumption in the short term, but if this consumption increase is unsustainable borrowing against future income, then in the long term overall consumption may decrease Banerjee et al. (2015). Because of the longer time span between the opening of

[13]Of the 69 municipalities that received at least one Banco Azteca Branch in the sample, 63 municipalities had at least one branch for the entire time period from December 2002 to December 2005. Two municipalities received Azteca branches in 2002 that had closed by 2005, and four municipalities received Azteca branches after 2002 and still had branches in 2005.

the banks and the follow-up questionnaire, if this pattern of consumption is the case, we will likely only see the decrease in consumption. Even for households that do not take out loans, the longer presence of the bank at follow-up could give households time to become aware of the bank and the services it provides and to incorporate the increased loan availability into household expenditure decisions.

After presenting the main results, I will discuss possible selection and the DD assumptions. Then, in addition to the main specification given in equation (1), I also estimate a specification in which I separately identify the treatment effect in municipalities which received more than the median number of branches because it is likely that Banco Azteca had more of an effect in areas that were relatively more saturated with branches. Later, I exploit fact that some municipalities received Banco Azteca Branches later than others to provide some evidence of the dynamic impacts of increased credit.

1.5 Main Results

1.5.1 Impact of Banco Azteca on borrowing

Table 1.3, Panel 2, presents information on borrowing behavior in Banco Azteca and non-Banco Azteca municipalities at the household level.[14] In the analysis, I look at both knowledge of lenders and actual borrowing behavior. Karlan and Zinman (2007) find that there is a stigma associated with borrowing from high interest lenders; as a result, nearly 50 percent of borrowers do not report their borrowing behavior in comparisons of administrative and survey data from a for-profit credit institution in South Africa. Based on conversations I had with a microcredit practitioner in Mexico, there is a similar stigma against borrowing from private institutions in Mexico as well. Thus, there could be substantial underreporting of

[14]The impact of the entrance of Banco Azteca on borrowing has also been documented in Ruiz (2011).

borrowing behavior, particularly borrowing from banks. Even if there is not underreporting of borrowing from banks in the past 12 months, as of the second MxFLS survey Banco Azteca branches had been in some municipalities for up to three years. In these cases, individuals that know they can borrow from a bank but do not report doing so may have borrowed and settled the loan prior to the previous 12 months. Looking at changes in knowledge, which is less likely to be underreported and can also reflect previous borrowing behavior, gives us another view of how the entrance of Banco Azteca impacted borrowing.

Overall knowledge of lenders is similar around 50 percent in both non-Azteca and Azteca municipalities in 2002. A lender encompasses all potential sources of borrowing (e.g., banks, cooperatives, moneylenders, friends, relatives, work, pawn shops, credit programs, government programs, and other sources). Borrowing rates from all sources are actually slightly higher in non-Azteca municipalities in 2002, with 23 percent of households reporting at least one member borrowed versus 19 percent of Azteca households. In particular, households in Azteca municipalities were about 4 percentage points more likely to know of a bank than households in non-Azteca municipalities in 2002. Likewise, households in Azteca municipalities are three times more likely to borrow from a bank in 2002 than households in non-Azteca municipalities. Notably, although the rate of borrowing from all sources decreases between 2002 and 2005, the rate of bank borrowing actually increases in both Azteca and non-Azteca municipalities.[15]

Table 1.4 presents estimates from a linear probability model, using equation (1), of the impact of the entrance of Banco Azteca on knowledge of lenders, knowledge of banks, and on borrowing behavior from lenders and banks.[16] Each column of the table represents a

[15] One striking feature of the data is that less than one percent of the sample borrows from banks in contrast to borrowing rates of about 20 percent overall (see Table 1.4). While it is not surprising that not everyone is borrowing from banks, the size of the disparity is large. Panels 1 and 2 of Table A.1 show the breakdown of borrowing activity across different potential sources of loans for the subsample of borrowers in the MxFLS sample. Panel 2 shows that, by far, the most common sources of loans are relatives. Moneylenders and cooperatives are also more common sources for loans than banks.

[16] I also ran these specifications using a probit model to check robustness of the marginal effects and a version of the linear probability specification with household fixed effects. The point estimates from these

separate outcome. The entrance of Banco Azteca increases the knowledge of lenders by 14 percentage points (column 1)an increase of 26 percent from 2002 knowledge in Banco Azteca municipalities, given at the bottom of Table 1.4. A majority of this increased knowledge of lenders is due to an increase in knowledge of a bank by about 10 percentage points, shown in column 2, or 68 percent compared baseline knowledge. Therefore, the entrance of Banco Azteca increased knowledge about the availability of loans among households in Banco Azteca municipalities. The third column of Table 1.4 shows that Banco Azteca municipalities had an increase of about 6.5 percentage points in the probability of taking out a loan from any source. In column 4, bank borrowing increased by about 1 percentage point due to the entrance of Banco Azteca.[17] While this seems like a small increase, this represents 15 percent of the overall increase in borrowing and a sizeable increase of 167 percent increase from 2002 levels of bank borrowing.

1.5.2 Impact of Banco Azteca on Consumption

Theory predicts that borrowing leads to increases in consumption by loosening credit constraints, either through increased access to loans or through lower interest rate loans Banerjee et al. (2010). In addition, if households invest the loans in businesses, while there may be a short term reduction in non-durable consumption, consumption could increase in the long run as the household earns returns from these investments (de Mel, McKenzie and Woodruff, 2008; Banerjee et al., 2010). On the other hand, Stango and Zinman (2011) find that households underestimate or do not understand the true cost of the interest rates, causing them to systematically over-borrow in the short run; this leads to short term increases in consump-

additional exercises remain stable, though the estimates lose precision for the impact of Banco Azteca on borrowing from any lender and borrowing from a bank in particular. Tables are available upon request.

[17]It is common to have low treatment take-up in studies looking at the impact of credit expansion on borrowing behaviorwith the exception of papers that focus on randomized loan approval from a pool of applicants (see Karlan and Zinman (2010*a*)). In a setting more similar to the current one, Pitt and Khandker (1998) examine the impact of pre-existing MFIs on consumption outcomes. Due to low rates of borrowing in the population, Pitt and Khandker oversample households that were eligible for loans from the MFIs to ensure they would have enough borrowers in the sample to consistently estimate impacts of borrowing.

tion but decreases in consumption in the long run. Under the assumption of a buffer-stock model, even if a household does not borrow from a new credit source but expects to be able to borrow in the future, household members may reduce their holdings of savings or assets in the present period (Deaton, 1991; Fulford, 2013; Rosenqweig and Wolpin, N.d.). This would also result in a short term increase in consumption, but, over the long run, consumption expenditures may decrease because households are not saving or earning returns on as many valuable assets.

Table 1.5 shows the results from estimating equation (1), using per capita expenditures on consumption categories as outcomes of interest. I use the household per capita expenditures on the specified consumption category in Mexican Pesos, and I exclude households with expenditures above the 99th percentile of expenditures in each category.[18] Household non-durable consumption expenditure outcomes include expenditures on cereals in the past 7 days (e.g., pasta, rice, crackers, legumes, flour, corn flour, etc.), tobacco products in the past 7 days, meals eaten outside the home in the past 7 days, and transportation/ festivals in the past year (e.g., funerals, vacations, parties, insurance, and moving or other transportation services). These consumption items can be grouped into two over-arching categories: necessity goods (cereals) and temptation goods (tobacco, meals outside home, and transportation/ festivals). In addition to these four non-durable consumption outcomes, I also examine impacts on expenditures in past year on two types of durable consumption purchases (i.e., assets): electronics (e.g., TV radios, cameras, etc.) and furniture. Across all outcomes, households in Banco Azteca municipalities have higher baseline expenditures on average than households in non-Azteca municipalities in 2002.

[18] I have estimated specifications using the full sample households and using log transformations of the outcome; both exercises produce qualitatively similar results. In addition, as with the regressions looking at borrowing, I have run versions of these models using household fixed effects. Again, results were qualitatively similar. All tables are available upon request.

1.5.3 Non-durable consumption

Columns 1 through 4 of Table 1.5, Panel 1, show the impact of Banco Azteca on the extensive margin of expenditures on the non-durable consumption goods. The first column of Table 1.5, Panel 1, shows there is no change along the extensive margin of expenditures on cereals, a common staple good in Mexican households. This is what one would expect, given that cereals comprise an important part of the Mexican diet. Across all non-durable consumption categories, the estimated impact of Banco Azteca on the extensive margin is negative, though only the estimate for transportation/ festivals is marginally significant.

Panel 2.1 of Table 1.5 shows the impact of Banco Azteca on the level of expenditures on these consumption items, and Panel 2.2 shows the estimates from a specification for which I have split the treatment effect into an overall effect and an effect for households in municipalities that received higher than the median number of branches per municipality.[19] The estimate on the main treatment variable $Azteca_m \cdot T2005_t s$ in Panel 2.1 is negative across all non-durable consumption categories, though only the estimate for cereal expenditures is statistically significant at the 10 percent level.

More interesting, however, is that when the impact in highly treated municipalities is identified separately in Panel 2.2, the impact on consumption expenditures is more strongly negative in highly treated municipalities across all non-durable consumption outcomes, and is statistically significant for cereals, eating meals outside the home, and transportation/ festivals.[20] Further, for these consumption outcomes, it appears that the negative impacts shown in Panel 2.1 are driven entirely by negative changes in expenditures in the most heavily impacted municipalities. These impacts are quite sizeable, representing a 13 percent

[19]Note that the sample size changes because, although I know that Banco Azteca opened branches in the Distrito Federal (D.F.) (a.k.a., Mexico City), I do not know the number of branches that opened, so I have excluded households in D.F. from this analysis.

[20]I also estimated specifications for the consumption outcomes which identified the main treatment as the number of branches per 10,000 residents by municipality, and the results are consistent with what is shown in the reported specifications. Results are available upon request.

decrease in cereal consumption, 38 percent decrease in money spent on eating meals outside the home, and a 56 percent decrease in transportation/ festival spending from 2002. While the decreased expenditures on temptation goods could have a positive interpretation since households are avoiding potentially undesirable expenditures, the fact that it is paired with decreases in expenditures on necessity goods suggests that the households in Banco Azteca municipalities have lower disposable income due to the entrance of Banco Azteca.

1.5.4 Durable consumption

Columns 5 and 6 of Table 1.5 present the estimates for the impact of Banco Azteca on per capita, household expenditures on electronics and furniture. Electronics are the main item type sold at the Grupo Elektra stores, so expenditures on electronics may be especially sensitive to the entrance of Banco Azteca, which made electronics relatively easier to obtain compared to prior to Banco Aztecas entrance. Furniture includes large items in the household that hold value that are not available at Grupo Elektra stores and, therefore, provide a measure of how households potentially change the way they store wealth after the entrance of Banco Azteca. The MxFLS asks both about household expenditures on durable goods in the past 12 months and about the value of current asset holdings within the household. While the latter tells us what has happened to assets over the whole time period, the former gives us insight into the timing of increases or decreases in asset holdings. In the tables in the main text, I show only the results for the impact on expenditures on electronics and furniture, but results on the value of the holdings of these goods can be found in appendix Table A.3.[21]

Similar to consumption expenditures, in 2002, households in Banco Azteca municipalities are more likely to have positive expenditures on assets on the extensive margin and to have spent

[21] Appendix Table A.3 shows impacts of Banco Azteca on expenditures on another type of household asset, domestic appliances. These results are not presented in the main text tables because the estimated impacts are imprecise.

more on these goods in the past year than households in non-Azteca municipalities. Likewise households in Azteca municipalities are more likely to own these durable goods in 2002, and the goods that they own have higher value than households in non-Azteca municipalities. Panel 1 of Table 1.5 shows evidence of a decrease in the likelihood of purchasing assets among households in Banco Azteca municipalities, but these estimates are not significantly different from zero.

Because Banco Azteca made electronics relatively easy to obtain by providing financing through loans, one would expect increased expenditures on these items. However, column 5 of Table 1.5, Panel 2, shows that expenditures on electronics in the past 12 months have actually decreased among households in Banco Azteca municipalities, though the estimates are not statistically significant. Panel 2.2 shows that expenditures on electronics do appear to have decreased by less among households in more heavily impacted municipalities, but there is no evidence of an increase in expenditures on electronics. Appendix Table A.3 shows the impacts on electronics holdings. Although the estimates are not statistically significant, there appears to be an increase in the value of electronics holdings in households in Banco Azteca municipalities, and this increase is concentrated among households in municipalities that received more Banco Azteca branches. These results are not inconsistent with a decrease in expenditures on electronics in the past 12 months. The increase in the value of electronic holdings could suggest that households bought new electronics when the branches first entered the municipalities but have not made purchases recently.

Column 6 shows that household expenditures on furniture in the last 12 months also decreased among households in Banco Azteca municipalities by 64.3 MXN due to the entrance of Banco Azteca. This estimate is statistically significant and represents a 40 percent decrease in expenditures on furniture from 2002 levels. Panel 2.2 of Table 1.5 shows that expenditures on furniture were impacted more negatively among households in municipal-

ities with relatively more Banco Azteca branches.[22] Actual furniture holdings (Appendix Table A.3, column 3) in the household also appear to be decreasing.[23] The decrease in furniture expenditures in the past year is consistent with the findings of decreased expenditures among all other consumption outcomes, strengthening the argument that households in impacted communities may be generally poorer. In addition, the lower value of wealth held in assets as furniture could indicate a general shift in the way that households store wealth and prepare for economic hardship. To explore whether this is the case, it is necessary to explore dynamic impacts of Banco Azteca on asset holdings and consumption.

1.6 Threats to Validity

Before exploring the dynamic impacts of Banco Azteca on household welfare, I will test whether the key identifying assumptions of DD hold in this context. There are two assumptions for DD estimates to provide causal estimates of a treatment effect: (1) no difference in trends of the outcomes of interest across the control and treatment groups absent the treatment; (2) no compositional change in the treatment and control groups.

[22]Panel 2 of Appendix Table A.3 presents the estimates from the specification that separately estimates the treatment effect for households in municipalities that received a relatively large number of branches for the other asset outcomes. The overall positive impacts on domestic appliance and electronics holdings in municipalities where Banco Azteca opened a branch (shown in Panel 1) are concentrated in the municipalities that got the most branches. This makes sense if we think that more branches in a municipality means that the population has relatively better access to the banks and can take better advantage of them to make domestic appliance and electronics purchases, which are the items sold at the Grupo Elektra stores, in which the Banco Azteca branches are located.

[23]However, these estimates are not statistically significant. While the question about durable goods expenditures separates furniture from large appliances and groups large appliances (e.g. stoves, refrigerators) with smaller domestic appliances, the asset holdings questions groups furniture with large appliances, leaving domestic appliances to cover only small appliances. Because there is evidence of a positive impact on domestic appliance expenditures in the last 12 months (Table A.3), grouping them with furniture in the asset holdings measure may decreasing the precision of the estimated decline in furniture holdings.

1.6.1 Selection into treatment and common trends

The first identifying assumption necessary for DD to provide a causal estimate of the impact of Banco Azteca on household borrowing and consumption behavior is that outcome trends in treated (Azteca) and untreated (non-Azteca) municipalities would have been identical after 2002 if Banco Azteca had not entered any of the municipalities. This requires that treatment municipalities were not chosen because of particular patterns in the trajectory of consumption expenditures. Note that it does not require that that the treatment and control groups have identical levels of the outcomes prior to treatment, rather that the trajectory of the changes in outcomes would have been the same had the treatment not occurred.

A priori, municipalities that received Banco Azteca branches (Azteca municipalities) tend to be better off and to have had better access to banks than those municipalities that did not receive a Banco Azteca branch (non-Azteca municipalities). Table 1.2, Panel 1, presents the availability of banking services across Azteca and non-Azteca municipalities using data from the MxFLS community surveys and Panel 2 presents data on school attendance, infant mortality, and per capita income across both municipality types, using data from the HDIMM. Column 3 of Table 1.2 shows that, in 2002, a higher share of communities in Azteca municipalities had access to banks and that Azteca municipalities had higher school attendance rates, lower infant mortality rates, and higher income per capita. Likewise, Panel 1 of Table 1.3 shows that households in Azteca municipalities also tend to be better off than those in non-Azteca municipalities: household heads in Azteca municipalities are 1 percentage point more likely to be female, one year younger on average, 6 percentage points less likely to be married, and 8 percentage points more likely to both hold a high school degree or college degree. Consequently, there may be concern that selection of Banco Azteca municipalities may bias our estimates.

However, the selection mechanism for branch location was not based on differences in munic-

ipalities that may make them better or worse for a new bank; rather Banco Azteca branches were opened in all pre-existing Grupo Elektra stores, which had located prior to the conception of Banco Azteca. There was no selection of which retail stores received Banco Azteca branches. All Grupo Elektra stores that were open in October 2002 received a Banco Azteca branch, and all Grupo Elektra stores that opened subsequently, opened with associated branches. Therefore, Azteca municipalities were not selected because of economic conditions or pre-existing borrowing opportunities, which is the main concern in the context of this study.

In order to empirically test whether selection on observables occurred, I also estimate the main specification using a propensity score matching approach. Propensity score matching controls for selection on observables, such as differences in wealth, by matching treated municipalities to the control municipalities that were most similar a priori. In order to match municipalities, I used a logit to predict which municipalities were most likely to be treated based on pre-Banco Azteca municipality characteristics and calculated a propensity score, p , that measures the probability that a given municipality is selected to receive a Banco Azteca Branch. I imposed common support based on the propensity scores for the treatment and control groups and ran the main specification, equation (1), weighting households in the control municipalities by $\frac{\hat{p}}{1-\hat{p}}$ and the treated municipalities by 1. The estimated impacts are largely consistent with the results presented in this paper.[24]

Moreover, the changes in municipality characteristics indicate that the difference in economic conditions between the two municipality types did not change significantly over the study period, with the exception of presence of banks in Azteca communities, which increased significantly as would be expected in response to the entrance of Banco Azteca. Column 7 of Table 1.2 shows the simple difference-in-difference estimate of the means in columns 1

[24]The propensity score matching approach assumes that selection is based on observables. I incorporate survey weights provided by MxFLS, following recommendations by Zanutto (2006). Results are available upon request.

through 4.[25] Besides the statistically significant increases in the likelihood of banks being present in Azteca communities by more than 20 percentage points, other differences between Azteca and non-Azteca municipalities do not appear to be changing differentially over time (e.g., school attendance rates, infant mortality, and per capita income).[26] Overall, Table 1.2 shows that the entrance of Banco Azteca does not seem to have affected outcomes we would not expect to be affected, alleviating some concern that selection occurred to choose locations that seemed to have favorable trajectories.

To further argue that trajectories of treatment and control municipalities would have been similar, it is common to look at trends in the outcomes pre-treatment. Unfortunately, the MxFLS only provides data from 2002 and 2005, making it impossible to use this data to look at trends in borrowing or consumption prior to 2002. To show that pre-trends in consumption expenditures are similar, I use the Mexican National Survey of Income and Expenditures (ENIGH)(de Estadística y Geografía, 1992-2004).[27] The ENIGH is a random sample of households drawn to create a nationally representative sample; therefore, while there is some overlap in the municipalities included in the ENIGH and the MxFLS, the overlap is not perfect. In addition, the ENIGH survey is a repeated cross section, so the municipalities that overlap with the MxFLS municipalities vary from year to year. Therefore, I examine pre-trends using both the whole sample of municipalities available in the ENIGH each year and using only the municipalities that are in both the MxFLS and ENIGH samples to compare pre-trends in the municipalities that do and do not receive a Banco Azteca branch.

Figure 1.1, panels A and B show the trends in mean per capita household expenditures

[25]Column 7 contains the DD estimate from the following equation, $DD = (\bar{Y}_{2005}^{Azteca} - \bar{Y}_{2005}^{NoAzteca}) - (\bar{Y}_{2000}^{Azteca} - \bar{Y}_{2000}^{NoAzteca})$, where $\bar{Y}_t^m$ is the sample mean of the variable of defined in the left-most column in year t and municipality type m. In this context the standard DD estimator would be given by $DD = (\bar{Y}_{2005}^{Azteca} - \bar{Y}_{2000}^{Azteca}) - (\bar{Y}_{2005}^{NoAzteca} - \bar{Y}_{2000}^{NoAzteca})$. However, in order to emphasize that the difference between the Banco Azteca and non-Banco Azteca municipalities in 2002 and 2005 do not change differentially over that period, I have rearranged the terms, as shown in columns 5 and 6.

[26]Female per capita income is the only dimension along which there appears to be a change, though only at the 10 percent significance level; however, this could be a reflection of increased access to credit for females as a result of Banco Azteca.

[27]ENIGH stands for Encuesta Nacional de Ingresos y Gastos de los Hogares.

(top) and mean per capita food expenditures (bottom) in 2005 Mexican Pesos for the whole ENIGH sample and the sample of municipalities that are in the MxFLS data only, respectively. Figure 1.1 shows that Banco Azteca municipalities are richer on average; however, trends prior to 2002 are similar. In addition to the figure, appendix Table A.2 shows the results from an event study regression with year and municipality dummies of per capita expenditures and per capita food expenditures on the pre-trend differences across Azteca and non-Azteca municipalities. Although there is evidence of differing trends across treated and untreated municipalities from 1992 to 1994, pre-trend differences from 1994 to 2002 are jointly insignificant. Therefore, trends in consumption expenditures over the 8 years prior to the opening of Banco Azteca are not statistically different between Azteca and non-Azteca municipalities. Moreover, Bruhn and Love (2014), establish that pre-trends in real GDP per capita and small business ownership are similar across Azteca and non-Azteca municipalities in separate work.

1.6.2 No change in composition of treatment and control groups

The second DD assumption requires that households do not move from the control to the treatment group to take advantage of the treatment (or vice versa). Movement of households to benefit from Banco Azteca credit availability is unlikely as the presence of Banco Azteca will be most visible to households that already shop at the Grupo Elektra stores in which the banks are located and to households that are located near to store branches and receive advertising. Because of the panel structure of the MxFLS data, I can directly check for composition change across Banco Azteca and non-Azteca municipalities. Of the 7,571 households that are present in both waves of the MxFLS, only 28 households move to new municipalities, and, among these households, there is no systematic pattern of moving to or from Banco Azteca Municipalities.

1.7 Dynamic Impacts

Now that I have provided evidence that the key identifying assumptions of DD hold in this context, I will present results for the dynamic impacts of Banco Azteca. Estimates suggest that household expenditures on both non-durable and durable consumption decreased among households in municipalities where Banco Azteca opened branches. This section will focus on two potential hypotheses that explain these results, both hinging on particular behavioral reactions of households because of the availability of a new source of credit. The first hypothesis proposes that households used loans from Banco Azteca to make electronic purchases when the branches first opened. The second hypothesis proposes that a new loan source may fundamentally change the way that households hold assets to store wealth.

The first hypothesis for decreased consumption in 2005 is that households borrowed from Banco Azteca when the banks first opened in 2002 and made purchases of electronics, which are sold at Grupo Elektra stores. Taking out the loan would result in a short run increase in consumption as households bought new electronics or increased consumption of other goods. However, households may have underestimated the cost of the loan or overestimated the return on an investment Banerjee et al. (2010). Thus, in the long run, borrowers dedicate a large share of their income toward loan and interest payments. If households fall behind on their loans, they may also dedicate income to any resulting extra fees. As a result, households would have less disposable income to spend on other consumption goods in 2005, decreasing expenditures.

The results in the previous section are consistent with this explanation. The increased stock of electronics in households located in Banco Azteca municipalities indicates there may have been an increase in electronics purchases between 2002 and 2004, possibly financed by loans. The decreased household expenditures in 2005 indicate lower disposable income, which could be due to a share of household income being dedicated ongoing loan payments. This

explanation relies on an argument that households saw short run increases in the purchase of electronics and other consumption goods.

The second hypothesis relies on households responding to knowledge about Banco Azteca and behaving according to a buffer-stock economy model. The entrance of Banco Azteca significantly increased knowledge of banks, presumably of Banco Azteca. Even if not all of these households borrow from the bank, under a buffer-stock model, knowing that a line of credit is available to them can result in households selling off assets to free up income in the present period (Deaton, 1991; Rosenqweig and Wolpin, N.d.). Specifically, Fulford (2013) shows that when a new line of credit becomes available to a community of buffer-stock consumers, households sell off assets that they were holding to store wealth in case of a negative income shock, replacing these assets with this new line of credit as their buffer to negative shocks. In the short run, this behavior loosens liquidity constraints and initially results in an increase in consumption. However, in the long term, because households do not hold as much wealth and are, therefore, no longer earning returns on the value of this wealth, overall income and consumption in the community decrease. The decreased expenditures on and holdings of furniture assets is consistent with the buffer-stock model, as is the resulting long-run decrease in expenditures on other consumption categories. Once again, this argument relies on dynamic patterns in the timing of selling off assets and changes in consumption patterns.

For both hypotheses proposed, we would expect short run increases in consumption outcomes as households borrow (hypothesis 1) or sell off assets (hypothesis 2). There are two characteristics of the data that make investigating short run impacts difficult: (1) the MxFLS panel only has data for 2002 and 2005; and (2) most of the Banco Azteca branches opened in the first year. Sixty-three municipalities in the sample received a Banco Azteca branch in October of 2002; meanwhile three municipalities received branches in 2003 and three municipalities received branches in 2005. Despite these limitations, I exploit this variation in

two ways. First, I create a measure that is equal to the number of cumulative quarters that a Banco Azteca branch was present in a municipality. Among treated municipalities, 91 percent had a Banco Azteca branch for the entire period (3.25 years). Second, I interact the DD variable with receiving a Banco Azteca branch late, where receiving a branch late is defined as receiving a branch during 2005.

Table 1.6 shows the results of this exercise. Panel 1 shows the impact of the number of quarters households had access to Banco Azteca on consumption and assets. Across consumption outcomes, the estimate is negative, suggesting that the longer the bank has been in a municipality the lower consumption expenditures are in 2005. Likewise, the estimated impact on furniture expenditures and holdings is negative. This supports the hypothesis that households decrease consumption and asset holdings as the branch is in the municipality longer, however this does not indicate whether there were short-run increases in consumption.[28]

Panel 2 shows the estimates when the main DD interaction is additionally interacted with the municipality having received a branch late. The pattern of the signs of the coefficients on the DD term $Azteca_m \cdot T2005_t s$ and the new interaction term $Azteca_m \cdot T2005_t s \cdot Late$ presents evidence that households in municipalities that received Banco Azteca branches had short run increases in nondurable consumption expenditures with long term declines. The estimated impact of receiving a branch late (i.e., the short run impact of receiving a Banco Azteca branch) is positive for tobacco, meals eaten outside the home, and transportation/ festivals. For meals eaten outside the home and transportation/ festivals, the estimated impacts are positive and large enough so that the overall impact of Banco Azteca on these consumption items are actually positive in municipalities that received a branch late, while the estimated impact on Banco Azteca municipalities is negative overall.

[28] Appendix Table A.4 shows the results of these specifications for the other asset categories. There is evidence of a positive impact on domestic appliance and electronics holdings, though statistically insignificant, which could also support the story that households initially took out loans to purchase these items when the bank entered.

This indicates that households in municipalities where Banco Azteca has been present a relatively short period of time are actually experiencing increases in non-durable consumption from 2002 levels while households in municipalities where Banco Azteca has been present for three years are experiencing decreases in non-durable consumption. Short run increases in non-durable consumption among households in Banco Azteca municipalities is a necessary condition for the validity of both explanations proposed in the previous sections. However, this result does not indicate which of the two proposed hypotheses is more likely. I now turn to the dynamic impacts on electronics and furniture expenditures in order to determine whether evidence is more consistent with hypothesis 1 or with hypothesis 2.

For the first hypothesis, we would expect an increase in expenditures on electronics in the short run if households are using loans from Banco Azteca to make purchases of electronics. However, column 5 of Table 1.6 shows larger negative impacts on electronics expenditures among households in municipalities that do not receive branches until 2005, which is counter to the first potential explanation. However, this is not enough evidence to contradict the hypothesis either. Note that we are comparing 2002 expenditures to 2005 expenditures. The fact that electronic expenditures are lower in 2005 than in 2002 does not mean that expenditures are lower than they were in 2004, immediately prior to Banco Aztecas entrance into these municipalities.

On the other hand, Table 1.6 does provide evidence in favor of the second explanation. Column 6 shows that, while municipalities that received a bank branch late have relatively higher furniture expenditures than the municipalities that received a branch in 2002, the overall impact on furniture expenditures is negative for both types of municipalities. This suggests that as households become aware that a Banco Azteca branch has opened in their area, they are less likely to store wealth in these types of assets, knowing that a new source of credit is available to them if they need extra money during a negative income shock. Because of decreased asset purchases, households increase consumption in the short run, shown in

columns 2 through 4. However, this behavior results in lower consumption in the long run due to the loss of returns on the assets.

Overall, Table 1.6 does not provide conclusive evidence in favor or against the hypothesis that households are unsustainably borrowing, but does suggest that one mechanism for decreased consumption expenditures in 2005 among households in Banco Azteca municipalities may be that households are storing less wealth in assets to use during emergencies. Instead, these households may be planning to rely on the newly available credit through Banco Azteca. Taken together with the main results from Table 1.5, these findings suggest that households in Banco Azteca municipalities experienced decreased welfare, as measured by per capita, household consumption expenditures due, in part, to changes in behavior surrounding purchases and holdings of assets.

1.8 Conclusion

This paper studies the rollout of Banco Azteca, a for-profit bank that opened 815 branches overnight in October 2002 in Mexico. Banco Azteca caters to low income populations by offering small loans with flexible collateral requirements. As is becoming increasingly common among microcredit organizations, Banco Azteca does not place restrictions on loan use. However, Banco Azteca takes this freedom in loan use a step further, by explicitly encouraging purchases of goods sold within the Grupo Elektra stores, within which Banco Azteca branches are located. While Banco Azteca focuses more on consumption loans than is typical of microcredit organizations, its target population, loan size, collateral requirements, and interest rates are all consistent with those of the typical microcredit organization in Mexico. Therefore, this paper presents a unique, large scale study of the impact on household welfare of a microcredit organization that focuses particularly on consumption loans.

I find that overall borrowing increased, suggesting that borrowers are not substituting away from other forms of borrowing (such as from friends, relatives, or moneylenders) toward bank borrowing and that new borrowers are entering the credit market. However, there is evidence of negative impacts on expenditures on non-durable consumption, measured by consumption of cereals, and on temptation or luxury goods, such as eating meals outside of the home. In addition, there is evidence of decreased expenditures on assets, measured by electronics and furniture. These negative expenditure effects are concentrated among people in municipalities in which relatively more branches opened.

The negative impacts of Banco Azteca on household consumption expenditures in Mexico can serve as a cautionary example that demonstrates that providing credit to the poor does not automatically make them better off. The package within which the loan is offered is equally important. In this case, Banco Azteca offered individual liability loans and encouraged use of the loans for consumption purchases. These features may have reduced discipline in paying back loans and promoted over-borrowing among households, which may not have properly planned for loan terms. Further, because Banco Azteca does not encourage use of loans for small business development, households are not pairing these loans with an additional source of income, nor are they likely to earn returns on items purchased. Both of these conditions make consumption loans relatively costlier than small business loans. Finally, as is suggested by this study, the availability of a new source of credit may have changed the way that households store assets in the municipalities where Banco Azteca opened. This is because households can now borrow from Banco Azteca in case of a negative income shock and no longer need to store wealth in assets for this purpose.

The results presented in this paper are largely consistent with findings from studies of the impact of microcredit in other contexts. Previous literature has found that individual liability loans have less favorable impacts on household welfare than group loans Attansio et al. (2015); Augsburg et al. (2015). These studies suggest that qualities associated with group

lending contracts may encourage more responsible borrowing that generally results in more positive welfare impacts for households. These findings are intuitive. Loans targeted for small business development or for investment in assets may have more positive impacts because they loosen liquidity constraints in the short term, while also providing future revenue streams through business profits or returns on new assets purchased with the loan. On the other hand, loans that are targeted primarily for consumption or that are not paired with an emphasis on productive investment may be harmful to households in the long run due to over-borrowing or borrowing with no guaranteed future income streams. Households using loans for consumption or fiscal discipline (e.g. shift expenditures from temptation goods), therefore, may be better served by savings accounts.

More generally, the findings of this study, taken together with prior literature, suggest we should be cautious in judging this second generation of microcredit organizations, which are more likely to offer individual liability loans without restriction on loan use, in the same light as the first generation of microcredit organizations, which offer group loans and focus on small business development. Modifications in these fundamental features could be undermining the qualities of microcredit that made it attractive in the first place.

1.9 Tables

Table 1.1: Presence of Banco Azteca in sample municipalities

	(1)	(2)	(3)	(4)	(5)
Worksite Type	**Total**	**Mean**	**Standard Worksites**	**Minimum**	**Maximum**
Panel 1. December 2002					
Number of branches	471	6.4	6.5	1	32
Number of municipalities	63				
Panel 2. December 2005					
Number of branches	541	7.5	8.9	1	42
Number of municipalities	67				

Means are calculated excluding the municipalities in the Distrito Federal because branch information was not available by municipality there. For the Distrito Federal as a whole, there were 95 branches with 149,443 savings accounts in December 2002; in December 2005 there were 72 branches and 539,803 savings accounts.

Table 1.2: Bank presence and municipality characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Non-Azteca		**Banco Azteca**		**Differences**		
	2002	2005	2002	2005	(3)-(1)	(4)-(2)	(6)-(5)
Panel 1. Bank Presence							
Bank access	0.254	0.418	0.551	0.826	0.297***	0.408***	0.111
					(0.081)	(0.076)	(0.102)
Banks in community	0.075	0.119	0.435	0.681	0.360***	0.562***	0.202**
					(0.068)	(0.069)	(0.078)
Banks that offer loans	0.06	0.075	0.246	0.609	0.187***	0.534***	0.347***
					(0.060)	(0.068)	(0.082)
Banks that offer savings	0.03	0.09	0.348	0.623	0.318***	0.534***	0.216***
					(0.061)	(0.069)	(0.075)
Panel 2. Municipality Characteristics							
School attendance rate (%)	59.9	64.4	63.6	67.3	3.69***	2.97***	-0.695
					(0.676)	(0.741)	(1.07)
Infant mortality	28.7	22.7	22.7	14.9	-5.95***	-7.79***	-1.85
					(0.797)	(1.070)	(1.34)
Per capita income	5,241	6,561	8,873	10,717	3,633***	4,156***	524
					(438)	(539.5)	(724)
Per capita income, male	8,328	9,984	13,218	14,972	4,890***	4,987***	97.9
					(679)	(706)	(979)
Per capita income, female	2,226	3,259	4,733	6,678	2,507***	3,419***	912*
					(316)	(405)	(514)
Observations	67		69		136	136	272

*p<0.10, **p<0.05, ***p<0.01. Data from the MxFLS Community Characteristics Survey. 'Bank Access is a variable that is equal to one if at least one locality within the municipality has access to a bank. Likewise, 'Bank in community is a variable that is equal to one if at least one locality in the municipality has a bank located within its boundaries. Standard errors are clustered at the municipality level, shown in parentheses.

Table 1.3: Summary Statistics

	(1)	(2)	(3)	(4)
	Non-Azteca		**Banco Azteca**	
	2002	2005	2002	2005
Panel 1. Household Head Characteristics				
Female	0.180	0.192	0.190	0.120
Age	43.3	45.9	42.0	44.8
Married	0.731	0.726	0.670	0.682
Less than primary school	0.166	0.179	0.065	0.077
Primary school	0.527	0.521	0.393	0.407
Secondary school	0.178	0.178	0.249	0.244
High school	0.053	0.045	0.121	0.112
Normal basic/college	0.076	0.074	0.158	0.150
Graduate +	0.002	0.003	0.015	0.010
Panel 2. Household knowledge and borrowing				
Knowledge of lender	0.570	0.512	0.551	0.636
Borrowed from lender	0.231	0.159	0.190	0.182
Knowledge of bank	0.102	0.152	0.148	0.297
Borrowed from bank	0.002	0.007	0.006	0.021
Observations	2,449	2,167	4,391	3,541

Data are from the MxFLS waves one and two. Means are calculated using sampling weights constructed for MxFLS-1. Unit of observation is the household.

Table 1.4: Impact of Banco Azteca on household borrowing

	(1)	(2)	(3)	(4)
	Know of place from which to borrow	**Know of bank from which to borrow**	**Borrowed from any source**	**Borrowed from bank**
$(Azteca \cdot T2005)$	0.144***	0.101***	0.065**	0.010***
	(0.036)	(0.034)	(0.031)	(0.004)
$T2005$	-0.065**	0.042**	-0.072***	0.005**
	(0.031)	(0.020)	(0.022)	(0.002)
Observations	12,548	12,548	12,548	12,548
Outcome mean in 2002	0.551	0.148	0.190	0.006

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the municipality level, shown in parentheses. The controls include gender, age, marital status, and education level of the household head, as well as municipality fixed effects. The unit of observation is the household and the outcomes of interest are indicators that the household know of or took out a loan from the specified source. The sample is restricted to households whose head is aged 18 to 65. The r-squared reported is the adjusted r-squared. Household sampling weights constructed from the MxFLS-1 are used to correct for oversampling of rural areas in all regressions.

Table 1.5: Impact of Banco Azteca on consumption outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Necessity	**Temptation Goods**			**Assets**	
	Cereals	**Tobacco**	**Meals outside home**	**Transport./ Festivals**	**Electronics**	**Furniture**
Panel 1. Extensive Margin						
$(Azteca \cdot T2005)$	-0.012	-0.017	-0.033	-0.048*	-0.031	-0.022
	(0.018)	(0.017)	(0.026)	(0.026)	(0.024)	(0.018)
Observations	12,340	12,340	12,340	12,208	12,229	12,219
Outcome mean in 2002	0.919	0.210	0.322	0.225	0.225	0.222
Panel 2. Expenditures						
Panel 2.1. Main Specification						
$(Azteca \cdot T2005)$	-1.23*	-0.467	-2.40	-34.6	-17.3	-64.3***
	(0.643)	(0.321)	(1.61)	(42.4)	(26.3)	(19.4)
Observations	12,159	12,059	12,037	12,212	12,104	12,087
Panel 2.2. Separately identifying the impact in highly treated municipalities						
$(Azteca \cdot T2005)$	-0.804	-0.458	1.65	17.5	-17.5	-26.2
	(0.602)	(0.355)	(1.42)	(45.6)	(29.9)	(29.4)
$(Azteca \cdot T2005 \cdot High)$	-1.47**	-0.104	-5.37**	-124.2**	-12.8	-57.3
	(0.742)	(0.493)	(2.54)	(61.4)	(32.9)	(34.9)
Observations	11,849	11,753	11,734	11,792	11,790	11,778
Outcome mean in 2002	11.5	3.04	14.2	221	214	159

* p<0.10, ** p<0.05, *** p<0.01. This table presents DD estimates from the specification in equation (1). Standard errors are clustered at the municipality level, shown in parentheses. The controls include gender, age, marital status, and education level of the household head, as well as municipality fixed effects. The unit of observation is the household. The measures of the extensive margin are indicators for the household having had positive expenditures on the specified consumption good. The expenditures are measured as expenditures per capita. The sample is restricted to households whose head is aged 18 to 65. The r-squared reported is the adjusted r-squared. Household sampling weights constructed from the MxFLS-1 are used to correct for oversampling of rural areas in all regressions.

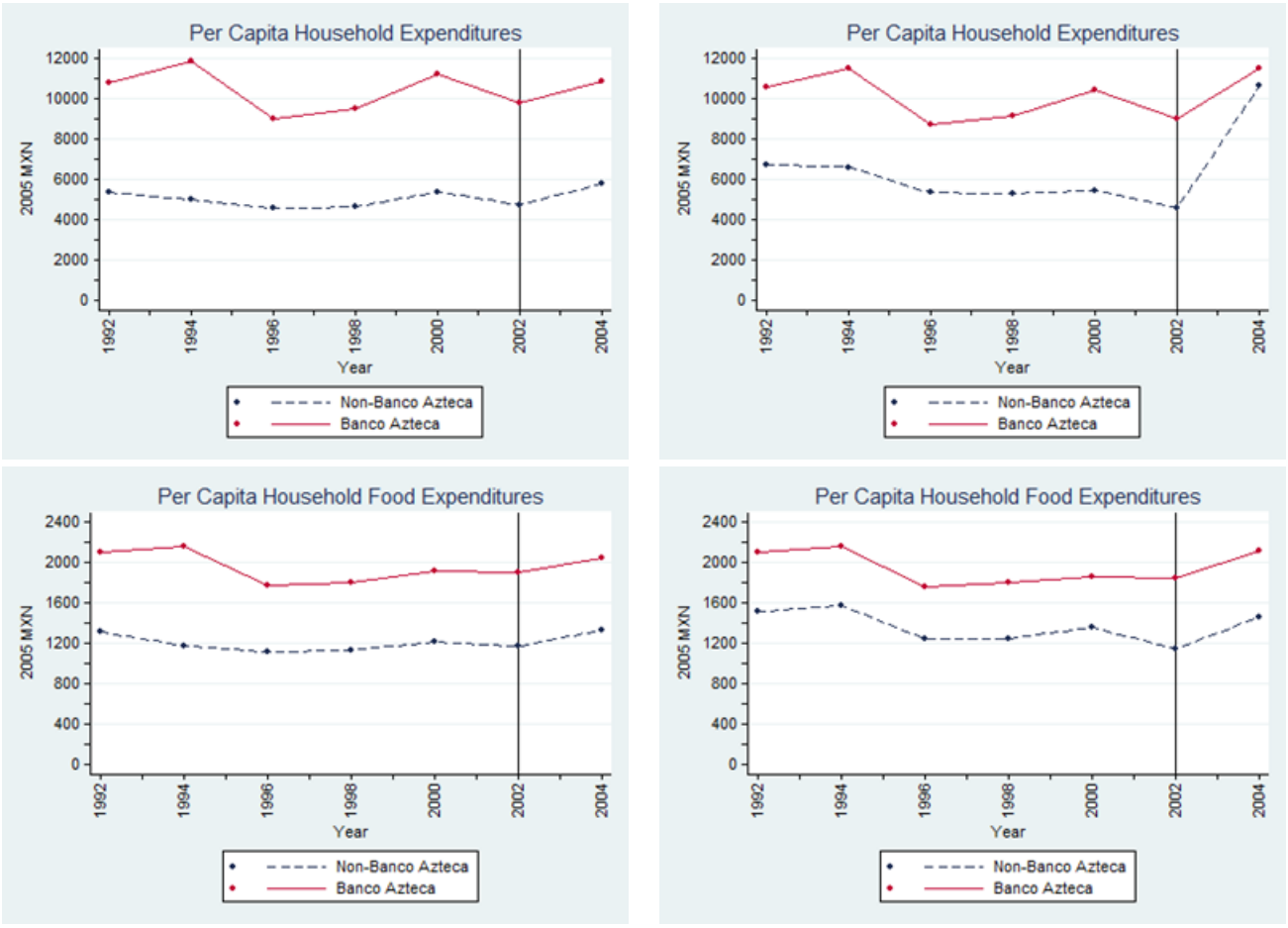
Table 1.6: Impact of Banco Azteca on consumption outcomes, exploiting variation in timing of bank branch entrance

	(1)	(2)	(3)	(4)	(5)	(6)
	Necessity	**Temptation Goods**			**Assets**	
	Cereals	**Tobacco**	**Meals outside home**	**Transport./ Festivals**	**Electronics**	**Furniture**
Panel 1. Treatment measured as cumulative quarters						
Cumulative Quarters	-0.310	-0.144	-0.782	-10.9	-5.61	-20.6***
	(0.209)	(0.101)	(0.525)	(13.8)	(8.26)	(6.23)
Panel 2. Interacting $(Azteca \cdot T2005)$ **with receiving a Banco Azteca branch late**						
$(Azteca \cdot T2005)$	-1.22*	-0.479	-2.52	-37.8	-11.5	-66.3***
	(0.660)	(0.330)	(1.66)	(43.6)	(26.5)	(19.9)
$(Azteca \cdot T2005 \cdot Late)$	-0.385	0.308	3.02*	78.4	-147*	50.1**
	(1.12)	(0.314)	(1.75)	(49.0)	(78.2)	(23.7)
Observations	12,159	12,059	12,037	12,097	12,104	12,087
Outcome mean in 2002	11.5	3.04	14.2	221	214	159

* p<0.10, ** p<0.05, *** p<0.01. See notes for Tables 5. This table presents the estimates of δ from (1) $y_{imt} = \alpha + \delta(CumulativeQtrs_{mt}) + \theta T2005_t + Z_m^{'}\beta + X_{it}^{'}\gamma + \epsilon_{imt}$ in Panel 1 and the estimates of δ_1 and δ_2 from (2) $y_{imt} = \alpha + \delta_1(Azteca_m \cdot T2005_t) + \delta_2(Azteca_m \cdot T2005_t \cdot Late_m) + \theta T2005_t + Z_m^{'}\beta + X_{it}^{'}\gamma + \epsilon_{imt}$ in Panel 2. In both specifications, $Z_m^{'}\beta$ includes only a full set of municipality fixed effects.

Figure 1.1: Trends in mean per capita expenditures and mean per captial food expenditures by presence of Banco Azteca

Panel A. Whole Sample | **Panel B. MxFLS Sample**



Data are from the Mexican National Survey of Income and Expenditures. Means are calculated using household weights. All money values are in 2005 Mexican Pesos (MXN). All expenditures are presented as per capita within the household.

Chapter 2

The Impact of Job Loss on Fertility Decisions among Dual-Earner Households

2.1 Introduction

A framework for the economic analysis of household fertility decisions was first formalized by Becker (1960). Becker treats children as normal goods, and a primary prediction of his model is that an increase in income or a decrease in household expenses will result in an increase in household fertility. Traditionally, researchers using this framework to study household fertility decisions among married couples have focused on shocks to male earnings and employment, treating women as secondary earners or non-labor market participants (Becker, 1960; Lindo, 2010; Jones and Tertilt, 2006; Amialchuk, 2013). While this characterized the household structure at the time Becker developed his analytical framework, the role of married women in the labor market has expanded substantially in the last 50 years. The labor

force participation rate among married women has increased from 35 percent in 1966 to 60.2 percent in 2011 (Winkler, 1988; BLS, 2013).[1] Further, the proportion of dual-earner married couples in the United States increased from 39 percent of all married couples in 1970 to 69 percent in 2009.

As a result, in the late 1970s and 1980s, researchers turned focus toward the relationship between higher wages and economic opportunity for women and decreases or delays in fertility (Bloom and Trussell, 1984; Schultz, 1985). Butz and Ward (1979) proposed that the relationship between fertility and the business cycle would become countercyclical as families moved activities towards things that use relatively less of womens time, which has become more valuable. More recently, researchers have turned their focus to the differential effects of male and female employment and job loss and have found that male employment shocks have stronger impacts on fertility than female employment shocks (Schaller, 2016; Ananat, Gibson-Davis and Gassman-Pines, 2012; Huttunen and Kellokumpu, 2012; Bono, Weber and Winter-Ebmer, 2012). However, these papers have focused on the impact of male and female employment on fertility outcomes separately without concentrating on the interactions of male and female employment shocks within the household.

This paper updates the current literature on the impact of employment shocks on fertility decisions to take into account the increased role of dual-earner married couples in the labor force. I exploit the widespread job loss during the Great Recession that took place from December 2007 to June 2009 (BLS, 2012; EPI, 2012) to estimate the causal impact of exogenous job loss on fertility decisions among dual-earner couples.[2] While unemployment is a common characteristic of recessions, the employment decline during the Great Recession was greater and more widespread across industries than that of any recession since the 1970s. As is typical, job losses were concentrated in the construction and manufacturing sector; how-

[1] Over the same period, labor force participation rates among women overall increased from 39.8 to 58.3 percent and labor force participation rates among men decreased from 80.5 to 70.5 percent (BLS, 2015*a*,*b*).

[2] Throughout the paper, I interchangeably refer to dual-earner married couples, dual-earner couples, and dual-earner households.

ever, the financial, retail, and business services sectors were also significantly impacted, to a greater degree than they have been in past recessions (BLS, 2012; EPI, 2012). Therefore, to a greater extent than past recessions, the Great Recession provides an opportunity to understand the impacts of unexpected job loss to households with labor market participants across a broad range of industries.

Historically, recessions are associated with declines in fertility (see Figure 1). This association was demonstrated during the Great Recession, during which time the total fertility rate fell from 2.1 to 1.9, while the unemployment rate increased from 6 percent to 10 percent. A job loss incurs an income effect and a substitution effect. The income effect, due to lost wages and uncertainty about future earnings (Jacobson, Lalonde and Sullivan, 1993; Eliason and Storrie, 2006), would have a predicted negative impact on fertility (Becker, 1960; Bongaarts and Feeney, 1998), while the substitution effect, due to the decrease in the opportunity cost of time spent child-rearing relative to employment, would have a predicted positive impact on fertility (Becker, 1960; Hotz, Klerman and Willis, 1997).

Because a job loss to a single earner household is equivalent to a complete loss of household income, we would expect the income effect to be stronger relative to a dual-earner household. Among dual-earner couples, fertility decisions may be less responsive to a job loss in the household because they have another income on which they can rely. In fact, there may be an incentive to increase fertility just after a job loss among dual-earner couples. Because child-rearing is relatively time intensive, the price of children is higher for high productivity couples (e.g., dual-earner couples) (Jones and Tertilt, 2006). A shift in the relative price of child-rearing due the job loss to one earner, while the other earner continues to bring home an income, provides a natural career break to accommodate child-rearing. Therefore, due to the relative strength of the substitution effect among dual-earner couples, the increased role of dual-earner couples in the US economy may contribute to a weaker relationship between economic recessions and fertility.

This possibility is reflected in evidence that the fertility rate has become less responsive to unemployment rates in recent recessions. During the two recessions in the early 1970s, the elasticities of fertility with respect to the unemployment rate were -0.119 (1960-70) and -0.361 (1973-75). However, during the recessions of the early 1990s and the early 2000s, these elasticities actually become positive, at 0.037 and 0024, respectively. While the elasticity of fertility became negative once again during the Great Recession, -0.074, the magnitude of the elasticity is still smaller than that of the elasticities in the 1970s. Given that the job loss associated with the Great Recession was the most severe of any recession since World War II (EPI, 2012), one would expect a stronger negative relationship between unemployment and fertility were the same degree of job loss to have occurred during the recessions in the 1970s.[3]

To evaluate the impact of job loss on fertility decisions among dual-earner married couples, I build two longitudinal datasets, covering the years 2003-2011: (1) a county-year dataset that matches yearly job losses due to mass layoff events to fertility rates in the following year at the county level; and (2) a state-quarter dataset that matches quarterly job losses due to extended mass layoff events to fertility rates four quarters in the future at the state level. The levels of aggregation of the datasets are due to the level of observation at which I can obtain data on job losses due to mass layoff events, which are reported by the Bureau of Labor Statistics Mass Layoff Statistics (MLS) program. While the measurement of timing in the state-quarter dataset is preferable for matching the timing of births with the timing of job loss, the geographic proximity of the mass layoff job losses in the county-year data provides a more precise estimate of the localized impact of job loss on fertility. I combine the MLS data with birth counts from the National Center for Health Statistics: National Vital Statistics Natality Data, population counts from the Surveillance, Epidemiology, and End Results Program (SEER) and population characteristics form the Current Population

[3]Authors Calculations using unemployment data from the Bureau of labor Statistics and total fertility rates from the World Bank.

Survey (CPS) Annual Social and Economic Supplement (ASEC).

I present results for two fixed effect, panel models at the county-year and state-quarter level. First I estimate the impact of job losses on fertility rates to replicate the negative relationship between job loss and fertility that is found in previous studies. For the main specification, I add an interaction term between job losses and the share of females in a dual-earner married couple to evaluate whether job losses have a different impact on fertility among dual-earner couples. In this case, the estimated coefficient on the interaction between job losses and the share of females in dual-earner couples is positive, indicating that counties and states with larger shares of females in dual-earner families experience a lesser decline, and potentially an increase, in fertility rates in response to job losses resulting from mass layoff events. Further, the positive impact of the interaction term on fertility rates is strongest when female job losses due to mass layoff events are used as the measure of job loss and when the outcome is restricted to fertility rates among women aged 20-34. Estimates indicate that a one standard deviation increase in the female separation rate due to mass layoff events increases fertility rates in a state when the share of females in dual-earner couples exceeds about 25 percent. The results are robust to more flexible specifications of the interaction between job losses due to mass layoff events and the share of females in a dual-earner married couple (quadratic, quintiles) and alternative time trends. Therefore, I find evidence in support of the hypothesis that females in dual-earner households are more likely to substitute toward child-rearing in response to job loss when compared to otherwise similar females who are not in dual-earner households.

In addition to contributing to the literature estimating the impact of employment shocks on household decisions, this paper builds on research analyzing the relationship between fertility and the business cycle and fertility and income. Research on the relationship between the business cycle and fertility has been mixed, with some research estimating a procyclical relationship (Silver, 1965; Orsal and Goldstein, 2010) and some research estimating a

countercyclical relationship (Butz and Ward, 1979; Mocan, 1990). Furthermore, in recent research on the impact of the Great Recession on fertility in Europe, Goldstein et al. (2013) find that fertility across Europe declined and conclude that fertility is procyclical. Corroborating findings of procyclical fertility, research on the relationship between income and fertility finds economic booms that increase husbands income leads to higher fertility (Black et al., 2013). In this paper, I find that the main effect of job loss on fertility is negative, supporting research finding procyclical fertility; however, I find evidence that fertility is potentially countercyclical among dual-earner couples. Finally, this paper contributes to a literature that finds evidence of changes in parental characteristics during economic downturns that contribute to healthier babies (Dehejia and Lleras-Muney, 2004). My research suggests that dual-earner couples, which are relatively better off households, may be more likely to conceive during economic downturns, contributing to the health improvements found in this literature.

The rest of the paper proceeds as follows. Section 2 discusses the ideal experiment for estimating the impact of job loss to dual-earner married couples and the approach taken, Section 3 describes the data and the construction of the main variables, Section 4 presents the empirical strategy and the main results, and Section 5 concludes.

2.2 Hypothetical Framework

To understand whether job loss to a dual-earner couple has a different impact on household fertility decisions than job loss to a single-earner couple, I would ideally obtain microdata on married couples, some of which are dual-earner married couples and some of which are single-earner married couples. As has traditionally been the case, the sample of single-earner married couples would be couples for which the husband is the sole earner. In addition, because my focus is on the impact of job loss on fertility decisions, I would restrict my

sample to heterosexual married couples.[4]

2.2.1 Ideal Experiment

The ideal experiment would be to randomly allocate job losses to the single-earner couples and randomly allocate job losses to either the male earner or female earner in the dual-earner couples. I would record subsequent fertility outcomes.

To estimate the impact of job loss on dual-earner households relative to single-earner households I would estimate the following specification:

$$Fert_{i(t+k)} = \beta_1 JobLoss_{it} + \beta_2 DualEarner_{i(t-1)} + \beta_3 JobLoss_{it} \cdot DualEarner_{i(t-1)} + X_{it}\xi + \epsilon_{it}, \tag{2.1}$$

where $Fert_{i(t+k)}$ is the fertility in household i, k periods after the job loss in time t; $JobLoss_{it}$ is an indicator for the household suffering a job loss at time t; $DualEarner_{i(t-1)}$ is an indicator for the household being a dual-earner household in the period prior to the job loss; X_{it} is a vector of household controls, including location and time fixed effects; and ϵ_{it} is an error term.

The coefficient of interest is β_3. To compare the impact of a job loss to a dual-earner couple compared to a single-earner couple, I would use the whole sample of dual-earner and single-earner couples, making no distinction between male and female job loss initially. To compare the impact of male job loss to dual-earner couples compared to single-earner couples, I would restrict the sample to single-earner households and dual-earner households

[4]While a period of unemployment for a dual-earner same-sex couple would also lower the opportunity cost of child-rearing, these couples are limited to adoption or surrogacy if they would like to have children. Because these options are very expensive, it is unlikely that the substitution effect due to the lower opportunity cost of time would dominate the income effect.

for which the male lost his job or no job was lost. If the substitution effect is more likely to dominate for dual-earner couples, then I would expect β_3 to be positive in both cases.

To compare the impact of a male job loss to the impact of a female job loss in a dual-earner couple, I would restrict the sample to dual-earner couples and estimate the following specification:

$$Fert_{i(t+k)} = \beta_1 MaleJobLoss_{it} + \beta_2 FemaleJobLoss_{it} + X_{it}\xi + \epsilon_{it}, \tag{2.2}$$

where $MaleJobLoss_{it}$ is an indicator for the male earner in the household suffering a job loss at time period t; $FemaleJobLoss_{it}$ is an indicator for the female earner in the household suffering a job loss at time period t; and $Fert_{i(t+k)}$, X_{it}, and ϵ_{it} are defined as in equation 2.1.

In this case, the analysis involves comparing the estimates of β_1 nd β_2. If the income effect dominates generally, we would expect that $\beta_1 + \beta_2 < 0$. If the substitution effect dominates generally, we would expect $\beta_1 + \beta_2 > 0$. Depending on the relative strength of the income and substitution effect β_1 and β_2 could both be positive, both be negative, or one could be positive while the other is negative. If both coefficients are negative and the substitution effect is relatively stronger for female job losses, then we would expect $|\beta_2| < |\beta_1|$. If both coefficients are positive, then $\beta_2 > \beta_1$ would indicate that the substitution effect is stronger when the couple suffers a female job loss. Alternatively, the coefficients could have opposing signs, indicating the income effect dominates for one type of job loss and the substitution effect dominates for the other.

2.2.2 Next Best Case

Unfortunately, it would be difficult to find a funder who would be willing to fund research which randomly allocates job losses to unsuspecting households. The next best case would be to obtain microdata on households that had detailed information on marital status, labor force participation and job histories, and fertility histories. Using this data, I would follow the strategy outlined in the previous section to estimate the impact of job losses to single and dual-earner households, paying attention to differences in the impacts of male and female job losses. Again, there are practical difficulties to taking this approach. In practice, datasets that provide both detailed employment and fertility histories are sparse. For the publicly available datasets that do exist, sample sizes are not generally large enough to have meaningful variation in the share of dual-earner couples who lose a job and then make a decision about fertility some period in the relatively near future (1 to 3 months after the job loss resulting in a birth 9 to 12 months later). Moreover, at the individual level, the innate endogeneity between labor force participation decisions and fertility decisions would be difficult to untangle. It is likely that there would be unobserved characteristics of individuals who suffer a job loss (particularly someone who was fired) that would also be correlated with their fertility preferences. Even for job losses that are reported to be for economic reasons, it would be difficult to be confident that there was not some innate quality about the individual that caused the employer to select that individual to layoff.[5]

2.2.3 Chosen Approach

For these reasons, I opt for a third option: construct a dataset that contains information on job losses due to mass layoff events for a geographic area and fertility rates among married couples in those same areas. As opposed to the treatment of interest being job losses to

[5]See Appendix B.2 for a discussion of potential individual level dataset and associated concerns.

individual households and the outcome being individual household fertility decisions, the treatment of interest is rate of job loss to dual-earner households due to mass layoff events in a set geographic area and the outcome is the fertility rate among married couples in that area. The ideal dataset of this form would record information on job status and fertility frequently, such as monthly, for small geographic areas, such as a community or county. With the aim to construct such a dataset, I use a combination of several different data sources that provide information on fertility rates, job losses due to mass layoff events, and marital and dual-earner status of the population to create two longitudinal datasets. I will discuss the data in more detail in the following section.

2.3 Data

As mentioned in the previous section, I combine data from several different data sources to estimate the impact of job loss due to mass layoff events to dual-earner married couples. To construct measures of job loss, dual-earner household composition, and fertility rates, I utilize data from the Bureau of Labor Statistics Mass Layoff program (MLS), the Current Population Survey Annual Social and Economic Supplement (CPS ASEC), the National Vital Statistics Natality Data (Natality Data), and the Surveillance, Epidemiology, and End Results Program (SEER). In order to exploit the period of mass layoffs as a result of the economic down-turn during the Great Recession, I construct datasets covering the years 2003-2011. In addition, because of the level at which the MLS data on separations is recorded, I construct one dataset with the unit of observation at the county-year level and one dataset with the unit of observation at the state-quarter level.

I calculate the measure of job loss for the whole population, for males only, and for females only. For the measures of dual-earner households and fertility rates, I calculate measures for the population of females aged 15 to 44 and females aged 20 to 34 separately. In the

following sub-sections, I describe the method for constructing the measures of job loss to dual-earner married couples and the measure of fertility rates among married women[6], and I discuss the summary statistics associated with these measures.

2.3.1 Measure of Job Loss to Dual-Earner Couples

Measure of Job Loss due to Mass Layoff Events

To get a measure of job losses that are more plausibly exogenous to worker characteristics, I use data on job separations associated with a mass layoff event from the MLS. A mass layoff event is recorded when an establishment has at least 50 initial claims filed against them during a consecutive 5 week period; this data is collected monthly. The employer is then contacted by the state agency to determine whether these separations lasted 31 days or longer, and, if so, other information concerning the layoff is collected, such as reason for layoff and worker demographics. Layoffs lasting more than one month are known as extended mass layoffs and are recorded quarterly. The MLS program provides data on the number of initial claimants associated with the mass layoff event, the number of mass layoff events, and the total number of separations associated with the mass layoff event.

The MLS program offers data in two main formats: (i) monthly data on the number of initial claimants and the number of mass layoff events, disaggregated by industry; (ii) quarterly extended mass layoff data on initial claimants, layoff events, the total number of separations associated with mass layoff events, and more information about the characteristics of workers who were separated. I use the Quarterly Extended Mass Layoff series because it allows me to identify separations by sex, which is essential for understanding the different impacts that male job loss and female job loss may have on household fertility decisions. While these data

[6]Job loss is the treatment, dual-earner married couples are the treated group, and fertility rates among married women is the outcome of interest.

have the advantage of focusing on job losses that are associated with mass layoff events and are therefore more plausibly exogenous to worker characteristics than workplace job losses would be, one disadvantage that the data are only available at the state-level, so I cannot examine outcomes in smaller geographic regions, which may more accurately capture local labor market effects.

In addition to these consistently updated data series, the MLS program constructed a series on yearly initial claimants by demographic group at the county-level.[7] This dataset allows me to estimate impacts of job losses on fertility rates within a county but has the disadvantage of only being available at the yearly level. Because the data is only available yearly, I will be estimating the impact of job losses on fertility rates in the following year, making it more difficult to precisely estimate the timing of the impact of job loss on fertility rates.

For both the MLS State-Quarterly data and the MLS County-Yearly series, I calculate a measure of the job loss rate as the number of separations associated with a mass layoff event over the working age population by group:

$$JobLoss_{gtc} = (Separations_{gtc}/Population_{gtc}) \cdot 100,$$

where g identifies the subsample for which the job loss rate is calculated (e.g., all, male, female); t indicates the time period for which the job loss rate is calculated, and c indicates the geographic area that the job loss rate is calculated (e.g., the county or the state).

The population estimate in the denominator comes from the SEER population data. The SEER population estimates represent a modification of the annual time series of July 1 county population estimates to account for births and deaths. The population estimates are available at the county level according to age, sex, race, and Hispanic origin. I use these

[7]This dataset was constructed for the years 1995 through 2012 and was provided for the first time through the BLS website in June 2012. No future updates to this file are expected to be made. The county recorded is the county of residence for the initial claimant.

estimates to calculate the working age population for the corresponding subsample, time period, and geographic area.

The measure job loss captures separations related to a mass layoff event for all men and women in the given county and quarter. However, I am primarily interested in job losses specifically to dual-earner married couples, or couples in which both spouses participate in the labor market. Therefore, I construct an additional measure of the share of dual-earners in the geographic area corresponding to the measure of job loss.

Measure of Dual-Earner Married Couples

I use the CPS ASEC data to measure the share of females that are in a dual-earner married couple in the corresponding geographic area. I match females to their spouses in the CPS ASEC data, and identify females who are in the labor force and who have a husband who is also in the labor force. I sum up the number of females in a dual-earner married couple identified in this way for the geographic area. To get a measure of the share of females in a dual-earner married couple, I obtain a population estimate of the total number of working age females in the corresponding geographic area. The measure of $DualEarner_{ct}$ in geographic area c and time t, therefore, is given by:

$$DualEarner_{act} = FemalePopulationinDualEarnerCouple_{act}/FemalePopulation_{act},$$

where a identifies the age group for which the share is calculated for (e.g., females aged 15 to 44 and females aged 20 to 34). For the MLS State-Quarter series, this share is calculated for the state. For the MLS County-Year series, this share is ideally calculated for the county. However, not all counties are identified in the CPS ASEC data. Therefore, I calculate the measure of dual-earners at the county level, for the counties that are identified in the CPS.

For those counties not identified in the CPS, I use the state shares.[8]

Measuring Job Loss to Dual-Earner Married Couples

To measure the rate of job loss to dual-earner married couples, I interact the calculated job loss rate in time t with the measure of the share of females in a dual-earner married couple in the geographic area from the previous year. I use the previous year's measure of dual-earners as the measure of dual-earners in the geographic area prior to the mass layoff events.[9] By using this interaction as the measure of job losses to dual-earner couples, I am implicitly assuming that the job losses are impacting the dual-earner households in the county or state. To test this assumption, I present the joint distribution of females in dual-earner households and the measure of job loss to demonstrate that there are dual-earner households distributed throughout areas with a relatively high number of separations occurring. In addition, I show that the unemployment rate among married couples in which both partners are in the labor force is systematically related to the separation rate. That is, I argue that the job losses are, in fact occurring to dual-earner households. As mentioned previously, one complication is that the measure of the share of dual-earner households comes from the CPS ASEC, which does not identify all counties. Therefore, I will look at the statewide shares to avoid throwing away counties that are not identified in the CPS, as well as the county shares.

First, I show the joint distribution of separations and dual-earner households in Table 2.1. Panel A shows the distribution of female job losses by state against the share of the state female population that is in a dual-earner couple for the MLS State-Quarterly series. Panels B1 and B2 show the distribution of female job losses by county against the share of the state female population that is married and in a dual-earner household by state and by

[8] I use the individual supplemental weights provided by the CPS to weight the observations in the calculation of all statistics generated from the CPS data.

[9] Using the measure of dual-earners from the same year as the mass layoff event would be endogenous to the job losses.

county, respectively for the MLS County-Yearly series. If county separation rates were evenly distributed across states according to the presence of dual-earner couples, then each cell in the separate panels of Table 2.1 would be equal to 5 percent. In general, the joint distribution of the share of females in dual-earner households and female separations are fairly equally distributed across quintile combinations for all three panels. Therefore, I can infer that an increase in separations in a county likely results in an increase in the separations for members of dual-earner couples as well.

To show more directly that the separations are affecting dual-earner couples, Table 2.2 shows the joint distribution of the unemployment rate among dual-earner couples and job losses for the state-quarter dataset. A chi-square test rejects the null hypothesis that the two measures are independently distributed, suggesting that unemployment rates among dual-earner couples are higher where the separation rate is higher. Thus, the increased separations are affecting dual-earner couples, the couples of interest, and I can feel confident that the interaction between the job loss rate for the geographic area and the share of dual-earner couples is measuring, in part, the rate of job loss to dual-earner couples.

2.3.2 Measure of Fertility Rates

I use Natality Data on birth counts and population data from the SEER and CPS ASEC to calculate fertility rates from 2003 to 2011. The natality data provide information on birth counts occurring within the U.S. to both residents and non-residents and provides birth counts for specific demographic groups of mothers, (given by age, race, marital status, and education), as well as birth characteristics, such as gestational age, health status at birth, and others, and are available monthly at the county level. For the purposes of this project, I retrieve birth counts at the county-month level by marital status and age to calculate the married fertility rates for mothers aged 15 to 44 and for mothers aged 20 to 34. I do the

analysis on the restricted age of 20 to 34 because this the age window during which most first births occur, which is the birth that is most likely to be impacted by job loss, as evidenced in previous studies (Bulatao, 1981; Morgan, 2003; Neels, Theunynck and Wood, 2013; Heckman and Walker, 1990).[10] While the natality data is available at the county level, counties that have populations smaller than 100,000 people are grouped together into a single geographic entity that combines several counties.[11]

To get birth rates, I use population data from SEER and the CPS ASEC.[12] The proper denominator to calculate fertility rates among married women is the number of married women in the state or county for the specified age range. Unfortunately the SEER population data is not provided by marital status. Therefore, I obtain estimates of the married population by sex and age and by state from CPS ASEC data. Ideally, I would use the CPS ASEC data to estimate the population of married women by county as well, but the CPS ASEC does not identify all counties so we would only be able to estimate the impacts on a subset of the counties.[13] Therefore, instead of using estimates of county populations of married couples from the CPS, I combine the estimates of the state population of married women from the CPS ASEC with estimates of the state female population and the county female population from the SEER data. I then estimate the county level married populations using the following expression:

$$CountyFemalePopulation_{married,a} =$$

$$(CountyFemalePopulation_a / StateFemalePopulation_a) \cdot StateFemalePopulation_{married,a}$$

[10] I make this age restriction rather than restricting to first births to minimize the suppression of data in small cells, which becomes more frequent as you restrict births according to specific characteristics.

[11] I have applied for access to the restricted use data which provides birth counts at counties with populations smaller than 100,000 and am awaiting approval.

[12] See additional information on the SEER data earlier in this section.

[13] If I restricted the analysis to only those counties that are identified in the CPS ASEC data, we would lose about 60 percent of the counties identified in the natality data.

where *married* indicates that the counted female is married and a indicates age group (e.g., 15-44 or 20-34).

In using the rations of county to state female populations to scale the state female married populations to the relevant county-level populations, I make the assumption that the female married population is distributed across counties in the same way that the female population as a whole is distributed across counties within the state. I test the validity of this assumption using the counties that are available in the CPS to compare whether the married population and the general population are distributed across counties equivalently. For the subset of counties that are identified in the CPS, the ratio of the married female population in the county to the married female population in the state is similar to the ratio of the female population in the county to the female population in the state. This comparison is shown in Table B.1.[14]

2.3.3 Summary Statistics

Panels A and B of Table 2.3 show the summary statistics for the MLS County-Yearly series and the MLS State-Quarterly series, respectively. One thing to note in these tables is that the fertility rates in Panel A are yearly fertility rates by county, while the fertility rates in Panel B are quarterly fertility rates in the state, which is why they differ so greatly in their means.[15] The average yearly married fertility rate is 94.1 births per 1000 married women between the ages of 15 and 44, whereas the average yearly unmarried fertility rate is about half that, at 44.3 births per 1000 unmarried women between the ages of 15 and 44. Job losses is the main treatment variable. Again because of the difference in the geographic and time dimensions in the two datasets, the average job losses measure in the County-Yearly series

[14]There are 4 counties for which the shares do not match up well due to low sampling in the CPS. If I drop those four counties, (out of 2,439 counties), then I cannot reject the hypotheses that $(countypop_g)/(statepop_g) = countypop/statepop$ for $g = married, unmarried$.

[15]Note, however, that if you multiple the fertility rates in Panel B by four, you get roughly similar rates to those in Panel A.

is larger than in the State-Quarterly series. In the County-Yearly series the percent of the working age population that suffers a job loss in a county ranges from 0.004 percent to 12.6 percent. In the State-Quarterly series the percent of the working age population suffering job losses ranges from 0.011 percent to 1.8 percent. Finally, the share of females in dual-earner married couples varies across the states and counties, adding an additional source of variation. The share of females in a dual-earner couple ranges from about 10 percent to 40 percent.

2.4 Empirical Framework and Results

For the main analysis, I have two specifications. First I estimate the impact of job losses on fertility rates. Then, for the main analysis, I add the interaction with the share of females in dual-earner households. I estimate the impact of job losses on fertility rates four quarters later in the State-Quarterly series and job losses on fertility rates in the following year in the County-Year series. The following equation presents the specification of interest:

$$Fert_{c(t+k)} = \beta_1 JobLoss_{ct} + \beta_2 DualEarner_{c(t-j)} + \beta_3 JobLoss_{ct} \cdot DualEarner_{c(t-j)} + X_{ct}\xi + \epsilon_{ct}, \tag{2.3}$$

where $Fert_{c(t+k)}$ is the fertility rate in geographic area c, k periods after the job loss in time t;[16] $JobLoss_{ct}$ is rate of job loss due to mass layoff events in geographic area c at t; $DualEarner_{c(t-j)}$ is the share of females in a dual-earner married couple in geographic area c in j periods prior to the measured job loss rate in time t;[17] X_{ct} is a vector of location and time fixed effects; and ϵ_{ct} is an error term.[18] In equation 2.3, β_3 is the coefficient of interest.

[16] $k = 1$ for the county-year analysis and $k = 4$ for the state-quarter analysis

[17] $j = 1$ for the county-year analysis, and $j = 4$ for the quarter-year analysis

[18] Regressions are weighted by state or county population, and robust standard errors are used.

The location and time fixed effects will control for all time-invariant location effects and location-invariant time effects.[19],[20] I estimate six versions of each specification: measuring separations for the whole population, for males, and for females separately, and for variables defined for individuals aged 15-44 and aged 20-34 separately.[21]

I perform the main analysis using both the county-year dataset and the state-quarter dataset. There are tradeoffs to using each of these two datasets. To estimate the impact of job loss on subsequent behavior, geographic proximity of the job loss to the population of interest is important. In addition, to estimate the impact of some treatment on household fertility decisions, timing is important, so that we know that a treatment occurring at a particular point in time is affecting the birth in question. While the county-year dataset, provides the geographic proximity that we would prefer to measure the impact of job loss on household fertility decisions, the fact that the data is only available by year, means that our estimate of the timing of the impact of the job loss on fertility is more coarse. On the other hand, the state-quarter dataset has better measurement of the timing of the job loss relative to the fertility outcome, but the location of the job loss relative to the dual-earner households is less precise. Using both datasets to perform the analysis allows me to take advantage of each dataset's relative strengths and confirm that the results are consistent across both datasets. In this setting, geographic proximity is likely relatively more important to ensure that the job losses are impacting the dual-earner couples who are making the subsequent fertility decisions. On the other hand, it is unclear what is the exact window within which we would expect a household to make a fertility decision after suffering a job loss. A coarser measure of time may actually do a better job of capturing the impacted household fertility

[19]The results are robust to more saturated interactions of time and location fixed effects.

[20]One potential threat to identification is that job loss is also associated with divorce (Lichter, McLaughlin and Ribar, 2002; Charles and Melvin Stephens, 2004; South and Lloyd, 1992). Such behavior would create a bias against finding impacts of increased fertility among married, dual-earner households. I have run specifications controlling for marriage and divorce rates in the state or county, in $X_c t$, and got qualitatively similar results. See appendix tables A2 and A3.

[21]Specifications have been run with leads of the birth rate (looking further than one year out), and there is no effect of separations.

decisions than the more precise measure of timing at the quarter level. For these reasons, I first present the results from the county-year data and then the results from the state-quarter data.

2.4.1 County-Year Analysis

Panel A of Table 2.4 presents the coefficient estimate from a regression of the fertility rate among married women in time t+1 on the rate of job loss due to mass layoff events in time t. That is, I am estimating the impact of job losses on fertility rates in the following year. Columns (1) through (3) present estimates for the impact of job loss on married fertility rates among women aged 15 to 44, and columns (4) through (6) present the results for fertility rates of women aged 20 to 34. Columns (1) and (3) present the estimate for the impact of all separations on married fertility rates, columns (2) and (4) present the estimate for the impact of female separations on married fertility rates, and columns (3) and (6) present the estimate for the impact of male separations on married fertility rates. The signs of the coefficients are negative across all regressions, suggesting that job losses have a negative impact on fertility rates, but none of the coefficients are statistically significant. The negative sign of the estimates indicates that the income effect dominates in fertility responses to a job loss in the household. However, the magnitude of these coefficients are small, and standard errors do not rule out the possibility of a positive impact on fertility. Interpreting the magnitude of the coefficients, a one percent increase in the working age population suffering a job loss is expected to decrease married fertility rates among 15 to 44 year olds by 0.4 percent.

Panel B of Table 2.4 presents estimates of the coefficients in equation 2.3. As expected, the impact of job losses, female job losses, and male job losses shown in row 1 all have a negative impact on fertility rates, suggesting that the income effect generally dominates in determining the impact of job loss on household fertility decisions. The coefficient of interest

is on $JobLoss \cdot DualShare$, presented in row 3. If we think that individuals facing job loss in a dual-earner household will be less likely to decrease fertility than those who are not in a dual-earner household (i.e., the substitution effect is more likely to dominate for dual-earner households), then we would expect the coefficient to be positive. This is the case for each column of Panel B in Table 2.4. The signs of the coefficients are positive across all models, and marginally statistically significant for the regressions estimating the impact of job loss on fertility among women aged 20 to 34. This suggests that females in dual-earner households aged 20 to 34 are more likely to increase fertility in response to job loss than males in those same households.

The main treatment variable, $JobLoss \cdot DualShare$, is the interaction between the rate of job loss due to mass layoff events interacted with the share of females in a dual-earner household. Because this interaction is between two variables that can take on a number of values, the interpretation of the impact of a separation for a given level of dual-earners must take into account not only the interaction term, but the job loss term as well. Therefore, to interpret the impact of job loss to dual-earner households on fertility rates, I need to calculate the sum of $\beta_1 JobLoss + \beta_3 DualEarn \cdot JobLoss$, holding the share of dual-earners in the county constant. I do this for the regression estimates that show the strongest results, the impact of female job losses to dual-earner households on fertility rates shown in columns (2) and (4).

Figure 2.2 shows the effect of a one standard deviation increase in rate of female job loss as you increase the share of dual-earners on county fertility rates of married women aged 15 to 44 in Panel A and women aged 20 to 34 in Panel B. In both panels, as the share of dual-earner households increases, the impact of a one standard deviation change in the rate of female job losses becomes less negative and eventually positive. In this case, in a county with 26 percent of women aged 15 to 44 in a dual-earner household or a county with 23 percent of women aged 20 to 34 in a dual-earner household, a one standard deviation

increase in the percent of working age females suffering from job loss is expected to increase fertility. The results in Figure 2.2 suggest that at the 10th percentile of the distribution of the share of females in a dual-earner household, a one standard deviation increase in the rate of job loss to females decreases fertility rates by 1.2 percent. At the 50th percentile, a one standard deviation increase leads to a 0.4 percent increase in fertility rates among married women.[22] These results support the hypothesis that a female job loss is less likely to decrease fertility in a dual-earner household than in a non-dual-earner household or than if the male suffered the job loss.

2.4.2 State-Quarter Analysis

Table 2.5 replicates the estimates in Table 2.4 for the state-quarter level dataset. In this case, the outcome is fertility rates among married couples in time t+4, where the job loss occurs in time t. In other words, I am measuring fertility rates four quarters after the mass layoff event. Panel A of Table 2.5 shows that male job losses are strongly associated with decreases in fertility, as has been found in previous research. For an increase in male job losses of 1 percent of the male working age population, I expect fertility rates of married women aged 15 to 44 to decrease by about 0.9 births per 1000 women, or by about 3 percent. This is comparable to findings by Ananat, Gibson-Davis and Gassman-Pines (2012), who find that a 1 percent increase in job loss to the working age population in North Carolina decreases birth rates by around 2 percent. The effect is of a similar magnitude for the fertility of women aged 20 to 34 in column (6) (a 1 percent increase in job losses to the working age population is expected to decrease fertility by about 4 percent). The impact of female job losses on the fertility rates of married women aged 15 to 44 is positive and statistically significant, but for the fertility of married women aged 20 to 34, the positive effect is not statistically significant. This is also consistent with previous research that has found that female job losses have a

[22]The share of females in a dual-earner couple at the 10th percentile of the distribution is 19 percent and at the 50th percentile is 24 percent.

more ambiguous impact on fertility rates.

Panel B of Table 2.5 presents the regression results from estimating equation 2.3 for fertility rates of married women at the state-quarter level. Again, the impact of job losses, female job losses, and male job losses all have a negative impact on fertility rates, and the coefficient of interest is that on $JobLoss \cdot DualShare$ in row three. Although none of the estimates of the coefficient on the interaction term are statistically significant, the signs of all coefficients are positive, corroborating evidence that the substitution effect is more likely to dominate among dual-earner households in determining the fertility response to job loss. Again, the effect is most strongly positive for columns (2) and, especially, (5) which measures the impact of job losses on fertility of married women aged 20 to 34. This supports the hypothesis that when females in dual-earner households are faced with job loss, they may be more likely to substitute toward child-rearing.

As I did for the analysis using the county-year data, in Figure 2.3 I show the effect of a one standard deviation increase in female separations on fertility rates as the share of dual-earners in the state increases. In both Panel (A), which shows the impact on fertility rates for married women aged 15 to 44, and in Panel (B), which shows the impact on fertility rates for married women aged 20 to 34, as the share of females in dual-earner households increases along the horizontal access, the impact of a one standard deviation increase in the percent of the female working age population that suffers a job loss has a less negative, and eventually positive, impact on fertility rates among married women. In Panel (A), the impact of a one standard deviation increase in the percent of the female working age population suffering a job loss becomes positive when the share of females in dual-earner households increases above about 23 percent. In Panel (B), the share of females in dual-earner households needs only to be about 20 percent for the impact of a one standard deviation increase in female separations to have a positive impact on fertility among married women aged 20 to 34. Although the estimates in the regressions using the state-quarter data are not statistically significant, thy

are still suggestive that the impact of job loss has a lesser negative, and potentially positive, impact on fertility rates among dual-earner married couples.

2.5 Conclusion

Using quarterly data from the Extended MLS at the state level and a special yearly series of county-level MLS separations, I find evidence that states and counties with larger shares of females in dual-earner couples see a lesser decline in fertility rates in the face of separations due to mass layoffs. This result is particularly strong for female separations. Therefore, I find evidence in support of the hypothesis that females in dual-earner households that suffer job losses are more likely to substitute toward child-rearing than otherwise similar women that are not in dual-earner households. Prior research has found that male job losses tend to have larger negative impacts on fertility than female job losses. Moreover, female job losses have been found to have ambiguous impacts on fertility rates and that these impacts vary across mother characteristics, such as race and educational background. This paper focused on one hypothesis for why the impact of female job loss is empirically more ambiguous on fertility rates and argues that when a female member of a dual-earner family suffers a job loss, she is more likely to substitute toward child-rearing. This is because the negative income shock from a female job loss is likely weaker than for a male job loss and there is a stronger tradeoff between female time spent working and child-rearing relative to a male that suffers a job loss. In addition, there has been some recent evidence that females suffer less from gaps in their resumes than men do, presumably because employers assume that any gap in employment is more likely due to child-rearing activities. This could imply that involuntary job separations provide relatively more convenient opportunities for women to substitute toward child-rearing in the short run.

2.6 Tables

Table 2.1: Joint Distribution of Female Separations and Share of Female Population in a Dual-Earner Married Couples by State, 2003-2011

	(1)	(2)	(3)	(4)	(5)	(6)
Share	**Female Separations/Working Age Female Population**					
Dual-Earner	**Quintile 1**	**Quintile 2**	**Quintile 3**	**Quintile 4**	**Quintile 5**	**TOTAL**
Panel A. MLS Quarterly, Females aged 15-54						
Quintile 1	3.9%	3.7	5.0	4.8	4.6	21.9
Quintile 2	4.8	3.7	4.0	4.1	4.9	21.4
Quintile 3	3.6	4.8	4.4	5.2	3.4	21.4
Quintile 4	5.1	4.7	3.7	2.8	3.5	19.7
Quintile 5	3.6	3.3	2.9	3.2	2.7	15.6
TOTAL	21.0	20.1	19.9	20.1	19.0	100.0
Pearson chi-squared(16) = 31.3						
Panel B1. MLS Yearly, Females aged 15-54						
Quintile 1	2.9%	3.5	4.7	4.5	4.6	20.1
Quintile 2	3.4	4.5	3.8	4.1	4.2	20.0
Quintile 3	3.4	4.5	3.8	4.1	4.2	20.0
Quintile 4	3.4	4.5	3.8	4.1	4.2	20.0
Quintile 5	5.7	4.0	3.6	3.5	3.0	19.8
TOTAL	20.1	20.0	20.0	20.0	19.8	100.0
Pearson chi-squared(16) = 52.7						
Panel B2. MLS Yearly, Females aged 15-54, CPS ASEC counties						
Quintile 1	2.4%	3.5	4.5	4.8	6.3	21.5
Quintile 2	4.3	3.9	3.3	3.8	4.4	19.7
Quintile 3	3.9	4.0	4.7	5.2	3.9	21.6
Quintile 4	3.9	3.8	3.9	3.7	3.1	18.3
Quintile 5	5.7	4.9	3.7	2.5	2.2	18.9
TOTAL	20.1	20.0	20.0	20.0	19.9	100.0
Pearson chi-squared(16) = 255.5						

See notes from Table 2.3. Value in each cell is the percent of the population that falls in the intersection of the specified percentiles.

Table 2.2: Joint Distribution of Separations and Unemployment Rate among Dual-Earner Households by State, 2003-2011

	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment Rate	**Female Separations/Working Age Female Population**					
	Quintile 1	**Quintile 2**	**Quintile 3**	**Quintile 4**	**Quintile 5**	**TOTAL**
Quintile 1	4.7%	3.9	3.1	1.9	1.6	15.2
Quintile 2	4.9	4.5	3.3	3.9	3.1	19.5
Quintile 3	4.5	4.0	4.5	4.0	3.3	20.4
Quintile 4	2.7	3.9	4.7	5.2	5.3	21.8
Quintile 5	3.2	3.7	4.4	5.0	6.73	23.1
TOTAL	20.0	20.0	20.0	20.0	20.0	100.0
	Pearson chi-squared(16) = 89.6					

See notes from Table 2.1. Value in each cell is the percent of the population that falls in the intersection of the specified percentiles.

Table 2.3: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
		Mean	**S.D.**	**Min**	**Max**	**Obs.**
Panel A. County-Year Data						
Fertility Rate	15 to 44	65.3	10.7	27.6	119.9	4492
Married Fertility Rate	15 to 44	94.1	19.1	13.0	226.6	4492
Fertility Rate	20 to 34	104.3	18.0	37.9	174.9	4492
Married Fertility Rate	20 to 34	164.1	38.4	22.6	412.9	4492
Separations/Working Age Population	×100	0.729	0.739	0.004	12.6	4492
Female Separations/ Working Age Females	×100	0.568	0.550	0.002	6.5	4492
Male Separations / Working Age Males	×100	0.886	1.0	0.004	20.0	4492
Share Dual-Earner	15 to 44	0.272	0.036	0.127	0.414	4492
Share Dual-Earner	20 to 34	0.251	0.042	0.097	0.414	4492
Panel B. Quarter-Year Data						
Fertility Rate	15 to 44	16.6	2.0	11.6	25.1	1632
Married Fertility Rate	15 to 44	23.9	3.4	15.5	41.4	1632
Fertility Rate	20 to 34	26.2	3.4	14.7	38.6	1632
Married Fertility Rate	20 to 34	40.2	5.2	26.9	64.4	1632
Separations/Working Age Population	×100	0.179	0.166	0.011	1.779	1501
Female Separations/ Working Age Females	×100	0.142	0.138	0.005	2.078	1501
Male Separations / Working Age Males	×100	0.212	0.217	0.008	2.053	1501
Share Dual-Earner	15 to 44	0.283	0.045	0.127	0.414	1632
Share Dual-Earner	20 to 34	0.263	0.052	0.097	0.414	1632

Married fertility rates are defined as the number of births to women aged 15 to 44 (or 20 to34) per 1000 married women aged 15 to 44 (20 to 34). Data is from the CDC NVSB, the SEER population estimates, and the CPS ASEC. MLS Quarterly State Separations are defined as the number of separations attributed to extended mass layoff events in all private industries to males (females) over the working age population in the state. Data is from the Extended Mass Layoff series and SEER population estimates. Both fertility rates and separation rates are available quarterly at the state level. The MLS Yearly County Separations are defined as the number of initial claimants associated with a mass layoff event over the working age population in the county. The share of women aged 15 to 44 (or 20 to 34) in dual-earner couples is defined as the number of females that are married, in the labor force, and whose spouse is also in the labor force over the population of females aged 15 to 44 (or 20 to 34). The share of single women aged 15 to 44 (or 20 to 34) who are working is defined as the number of females that are single and report employment over the population of females aged 15 to 44 (or 20 to 34). The share of divorced females is the number of females that are divorced over the population of females. This data is from the CPS ASEC, is available yearly at the state level and for a subset of counties, and I used the CPS provided population weights to calculate the shares.

Table 2.4: Main Results using the County-Year Panel Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Married Fertility Rates$(t+1)$					
Panel A. Impact of Job Loss due to a Mass Layoff Event on Married Fertility Rates						
Job Loss	-0.419	-0.218	-0.374	-0.812	-0.205	-0.811
	(0.101)	(1.432)	(0.710)	(2.69)	(4.21)	(1.79)
Panel B. Impact of Job Losses due to a Mass Layoff Event to Dual-Earner Married Couples on Married Fertility Rates						
Job Loss	-5.31	-7.73	-3.38	-17.6	-23.8*	-13.1
	(4.84)	(5.98)	(3.95)	(10.8)	(13.4)	(8.65)
Dual-Earner	-34.5	-36.8	-32.6	-27.8	-33.0	-22.4
	(28.42)	(28.8)	(28.0)	(42.1)	(43.3)	(41.5)
Job Loss× Dual-Earner	19.6	30.5	13.7	73.1*	104.4*	52.8
	(17.2)	(21.8)	(13.9)	(41.6)	(52.1)	(33.1)
Sample Size	4492	4492	4492	4492	4492	4492
Ages	15 to 44	15 to 44	15 to 44	20 to 34	20 to 34	20 to 34
Job Loss Sub-Population	All	Female	Male	All	Female	Male

See notes from Table 2.3. Outcome is the fertility rate (births per 1000 women) for married women aged 15 to 44 in columns (1)-(3) and for women aged 20 to 34 in columns (4)-(6). Job separations are measured as the percent of the working age population that suffers a job loss in the given quarter. All specifications include county and year fixed effects. Regressions are weighted using county population of the demographic group for which separations are defined. Robust standard errors are used. * p<0.10, ** p<0.05, *** p<0.01.

Table 2.5: Main Results using the State-Quarter Panel Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Married Fertility Rates$(t+4)$					
Panel A. Impact of Job Loss due to a Mass Layoff Event on Married Fertility Rates						
Job Loss	-0.556	0.691	-1.008**	-1.083	1.523	-2.065*
	(0.591)	(0.568)	(0.407)	(1.923)	(2.162)	(1.222)
Panel B. Impact of Job Losses due to a Mass Layoff Event to Dual-Earner Married Couples on Married Fertility Rates						
Job Loss	-2.34	-5.44	-1.74	-8.23	-8.98	-7.51
	(3.77)	(4.50)	(2.61)	(8.97)	(10.69)	(6.28)
Dual-Earner	-6.23	-8.12	-5.43	-1.40	-2.77	-0.372
	(6.01)	(6.36)	(5.66)	(10.86)	(11.31)	(10.17)
Job Loss× Dual-Earner	6.86	23.6	2.83	29.5	44.2	22.0
	(13.1)	(16.6)	(8.66)	(32.65)	(40.6)	(22.5)
Sample Size	1632	1632	1632	1632	1632	1632
Ages	15 to 44	15 to 44	15 to 44	20 to 34	20 to 34	20 to 34
Job Loss Sub-Population	All	Female	Male	All	Female	Male

See notes from Table 2.3. Outcome is the fertility rate (births per 1000 women) for married women aged 15 to 44 in columns (1)-(3) and for women aged 20 to 34 in columns (4)-(6). Job separations are measured as the percent of the working age population that suffers a job loss in the given quarter. All specifications include state, year, and quarter fixed effects. Regressions are weighted using state population of the demographic group for which separations are defined. Standard errors are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

2.7 Figures

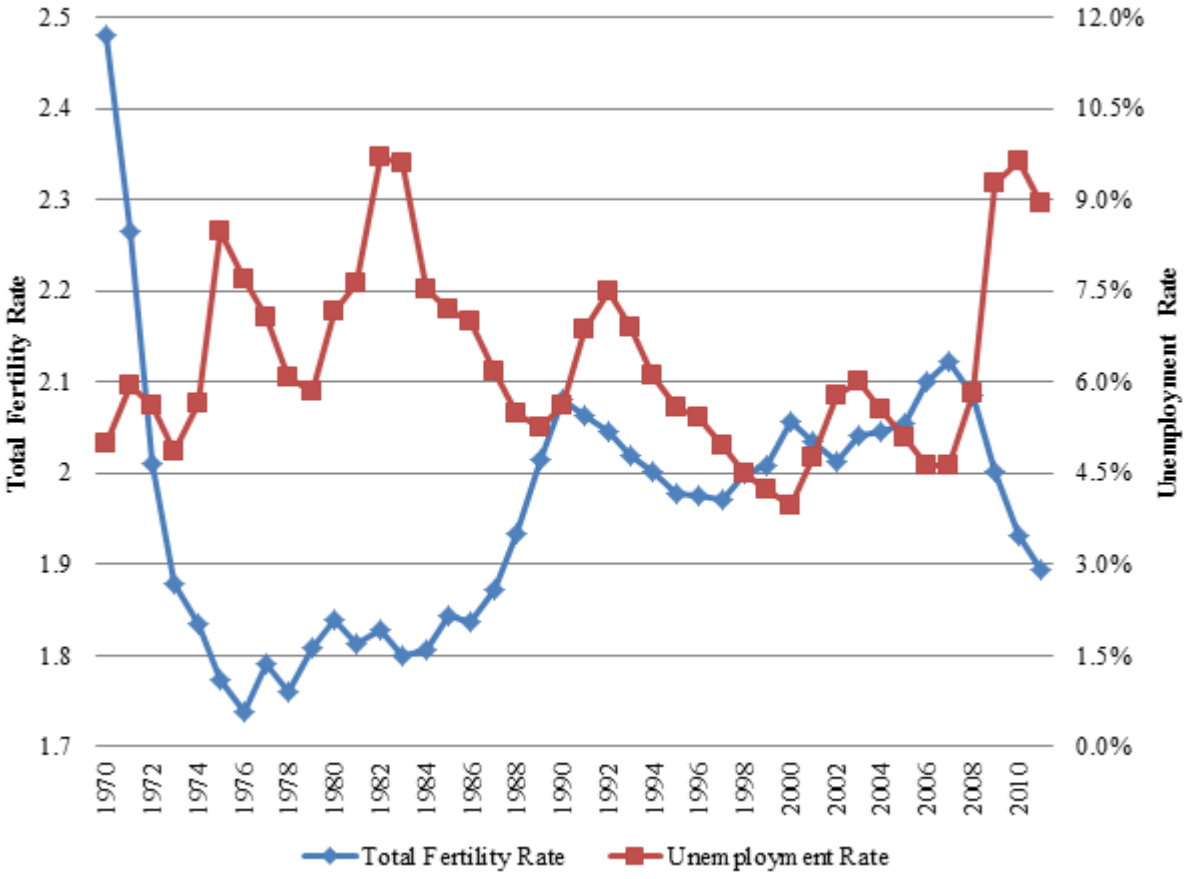
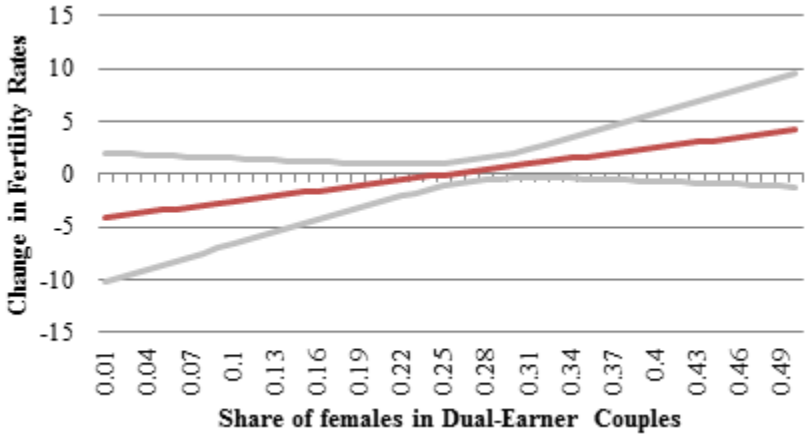


Figure 2.1: U.S. Total fertility Rate vs. Unemployment Rate, 1970 to 2011

Panel A. Ages 15-44



Panel B. Ages 20-34

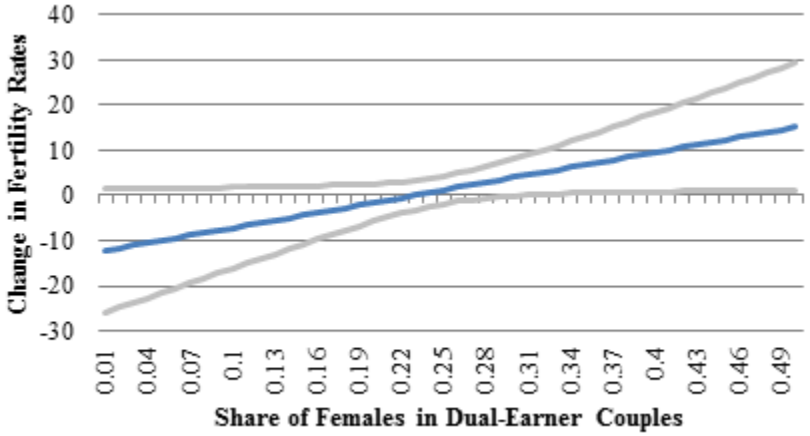
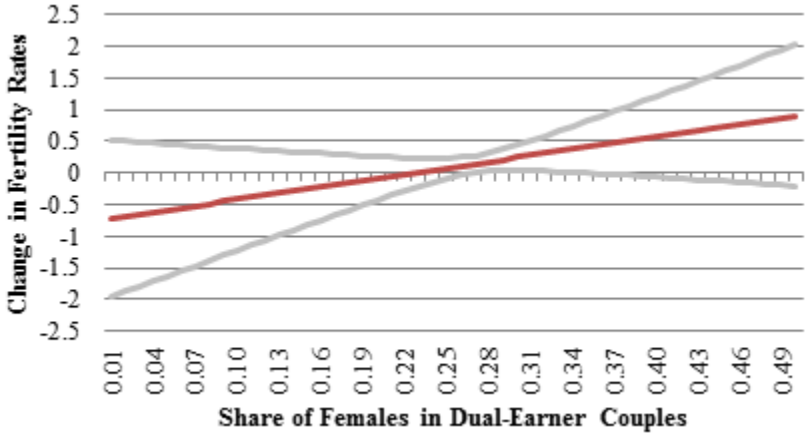


Figure 2.2: Mass Layoff Statistics, County-Year: Interpreting the impact of female separations on fertility as the share of females in dual-earner couples increases, $\beta_1 JobLoss + \beta_3 DualEarn \cdot JobLoss$

Panel A. Ages 15-44



Panel B. Ages 20-34

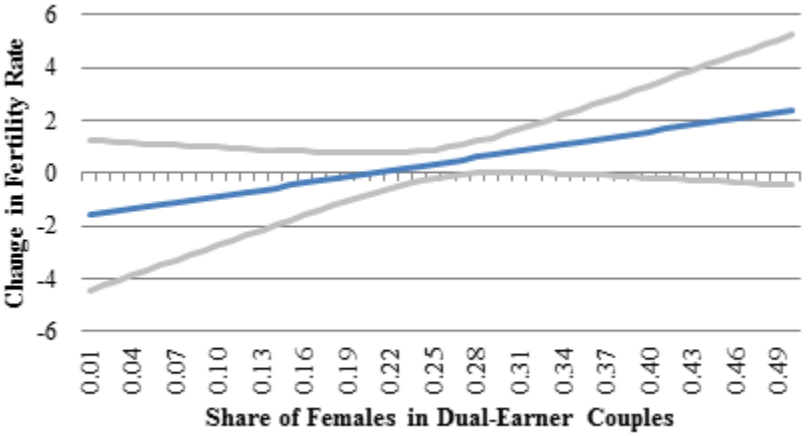


Figure 2.3: Mass Layoff Statistics, State-Quarter: Interpreting the impact of female separations on fertility as the share of females in dual-earner couples increases, $\beta_1 JobLoss + \beta_3 DualEarn \cdot JobLoss$

Chapter 3

Crimes against Morality: Unintended Consequences of Criminalizing Sex Work

3.1 Introduction

The regulation of sex work is a hotly debated issue in both developed and developing countries, and persists with varying degrees of legality around the world (Farmer and Horowitz, 2013). Those who favor the prohibition of sex work take a largely moral stance (Weitzer, 2007), arguing that sex work is associated with high victimization rates of female sex workers (Brewer et al., 2007; Farley and Barkan, 1998; Posner and Silbaugh, 1996), human trafficking inflows (Cho, Dreher and Neumayer, 2013), and contributes to the spread of sexually transmitted infections (STIs) (Willcox, 1962; Wren, 1967; Dunlop, Lamb and King, 1971; Posner and Silbaugh, 1996; Potterat, Rothenberg and Bross, 1979). Those who favor decriminalization and regulation of sex markets argue that decriminalizing sex work increases sex worker

bargaining power with potential clients (Aizer, 2010; Stevenson and Wolfers, 2006), reduces victimization of sex workers by clients and the police (Brents and Hausbeck, 2005; Ehrlich, 1973), and makes sex workers feel safer (Brents, Jackson and Hausbeck, 2009). Recently, Amnesty International shone a spotlight on this debate by passing a resolution calling for the decriminalization of sex work. They argue that decriminalizing sex work is the best way to defend sex workers' human rights against violations such as exclusion from health care (*Global Movement Votes to Adopt Policy to Protect Human Rights of Sex Workers*, 2015).

This paper presents new evidence on the impact of criminalizing sex work on female sex worker (FSW) health, risk behaviors, and access to health services. We exploit a natural experiment in which commercial sex work was criminalized in one district in East Java, Indonesia, while it remained non-criminalized in the neighboring districts. We improve upon data used in previous research on sex work by constructing a unique dataset from data we collected on FSWs in East Java at both criminalized and non-criminalized worksites, before and after the criminaliztion occurs. This dataset comprises the population of FSWs in both the criminalized and non-criminalized districts at baseline and is the first panel data on FSWs in any context. Additionally, we collected data on a sample of clients at all study locations before and after criminalization, allowing us to construct the first representative and quantitative dataset on clients of sex workers. We estimate the causal impact of criminalization on sex worker health outcomes and risk behaviors, employing a difference-in-differences (DD) framework, and we corroborate these findings using the data on clients. We find that criminalizing sex work increases STI rates among FSWs by 27.3 percentage points, or 58 percent. The main mechanisms driving this increase in STIs is decreased access to condoms at the criminalized worksites, resulting in decreased condom use during commercial sex transactions.

There is need for better objective evidence on the impacts that varying regulations have on the operation of commercial sex work and its market participants. The regulation of sex

work affects a non-trivial share of the population. Across the developing world, 2 percent of females engage in sex work, but the share of the population engaged in sex work is as high as 14 percent in areas of Madagascar (Vandepitte et al., 2006). In addition to directly affecting the women engaged in sex work, the commercial sex industry is an important contributor to the spread of STIs and HIV. On average, HIV rates among FSWs are 14 times higher than the general population, and this is true even in countries with generalized HIV epidemics (Kerrigan et al., 2013). Indonesia is an important case study for understanding the impact of regulating sex on the spread of HIV and STIs, as HIV rates are 38 times higher among FSWs than the general female population, with HIV prevalence particularly high among female sex workers in East Java (Kendall and Razli, 2010). As the primary transmission channel of HIV has transitioned from intravenous drugs to heterosexual sex since 2007, controlling HIV among the FSW population is critical for controlling the spread of HIV among the general population (*Integrated Biological and Behavioral Survey*, 2011). Moreover, rates of STIs (e.g., gonorrhea, chlamydia, and syphillis) in Indonesia are reported to be the highest among Asian countries (Kendall and Razli, 2010; Magnani et al., 2010).

This paper contributes to a growing literature on the regulation of sex work and its impacts on sex worker health. While there is some evidence of the impact of the decriminalization of sex work on sex worker and general population health in the U.S. context, there is little evidence of the opposite phenomenon, where sex work becomes criminalized after having operated "legally", and there is no evidence on the criminalization or decriminalization of sex work in a developing country context. Cunningham and Shah (2014) and Brents and Hausbeck (2005) find that decriminalizing sex work decreases population STI rates and decreases violence against female sex workers. This paper also contributes to research that, while not focusing on the legal status of commercial sex work directly, compares female sex workers who work indoors to those who work on the streets. This literature finds that indoor workers are more likely to use condoms and are less likely to have STIs (Gertler and Shah, 2011; Jeal and Salisbury, 2007; Seib et al., 2009; Seib, Fischer and Najman, 2009).

These findings suggest that criminalizing sex work, which is likely to push commercial sex workers into the street, would decrease condom use and increase STI rates. Finally, there is qualitative research that finds that repressive measures against FSWs undermine supportive professional networks and increase female sex worker vulnerability (Choi, 2011). Our paper adds to this literature by providing a quantitative study of the impact of criminalizing sex work on the operations of formal brothel complexes in East Java, which are run by central committees organized by the FSWs and have partnerships with the local health ministry.

We begin the paper by describing the context of the study worksites and the practical implications of the local government's decision to criminalize sex work at some locations. We then describe our data collection process and our sample of worksites, female sex workers, and clients. We establish that criminalizing sex work decreased the number of sex workers and clients at the criminalized worksites, but that sex work continued to take place at all locations. We show that, at the worksites where sex work was criminalized, the incidence of sexually transmitted infections (STIs) among FSWs increases, as measured by self-reports and verified by biological samples taken during medical exams. We argue that the increase in STIs is due to a decrease in access to condoms and health care. At the same time, the number of transactions per worker and types of transactions occurring at the criminalized sites did not change. Therefore, criminalizing sex work can put an already vulnerable population in a more precarious situation.

3.2 Context of the Worksites

Prostitution is not directly addressed in Indonesian national law, making it a legal grey area. However some officials commonly interpret the section of law titled "Crimes Against Morals" to apply to prostitution. As a result, prostitution is widespread and tolerated throughout Indonesia, with well-known red light districts in Jakarta and Surabaya and different districts

tolerating varying degrees of formality. Our study location is in East Java, where commercial sex work has historically been tolerated. The study includes 17 worksites in total across Malang District, Pasuruan City and District, and Batu City. These locations were selected in early 2014 for a related study in partnership with a local community service organization, which had an ongoing relationship with FSWs in these districts.

As is common throughout Indonesia, our study areas include both formal worksites, locally known as *lokalisasis*, and informal worksites (i.e., street sites). The formal worksites are recognized centers where prostitution takes place and are organized by a Pokja, an organizational committee typically run by a man who lives at the worksite with his family and maintains the sites. The FSWs at the formal worksites live in rental units at the sites and pay regular localization fees to cover the cost of electricity, water, and other worksite maintenance, such as security. In addition, all women at the formal worksites are required to have monthly health and STI checks, which are administered by the local ministry of health. If the FSWs fail to receive testing for more than one month in a row, they are at risk of being barred from the worksite. The informal worksites are located at local markets or in neighborhoods near the homes of the informal FSWs. While the women at these informal locations are still somewhat organized, in that the women regularly gather at the same locations, there is no centralized organizational committee, the women do not live at the informal sites, and there are no health check requirements.

Column (1) of Table 3.1 shows the number of formal and informal worksites surveyed in each of the locations. Out of the 17 worksites included in the study, nine of the worksites are in Malang (six of which are formal), six are in Pasuruan (four of which are formal), and two are in Batu. In general, the informal worksites tend to be much smaller than formal worksites, with an average of seven FSWs working regularly at each site, compared to an average size of 55 FSWs at the formal worksites.

On July, 11 2014, Malang District Secretary Abdul Malik announced that all formal worksite

locations within Malang District would be closed on the 28th of November, 2014. The timing of the closure of the worksites lined up with anniversary celebrations in Malang District, with Malik calling the closure a "birthday present" to Malang (Sukarelawati, 2014). The intention of the local government was to end all sexual activity at the worksites and to reclassify the worksites as centers of different legalized activity. For example, one worksite was to be transformed into a family karaoke center and another was to be transformed into a local market for Gunung Kawi sweet potatoes, a local specialty (Sukarelawati, 2014). Leading up to the closures, the local government planned to conduct local meetings to prepare the FSWs for the impending closures and transition of commercial activity into new sectors.

The closure of the worksites was not instigated or enforced via a change in the local ordinances, and there was no specific budget allocated to the transition of the worksites away from prostitution (*November, Pemkab tutup Tujuh Lokalisasi*, 2014). The closure seems to have been driven by the announced transition of Dolly, the largest operating red-light district in Java, away from prostitution and had religious motivations (*November, Pemkab tutup Tujuh Lokalisasi*, 2014; Assifa, 2014). During his announcement of the closures, Malik said that he hoped the women at the localizations would obtain a job that was "more pleasing to God" (Assifa, 2014). Enforcement of the worksite closures relied on cooperation of the local pimps, who were asked not to accept any new FSWs to the worksites after Eid al-Fitr[1] (July 28, 2014), and raids by the local police after the closing date of November 28.

As will be discussed in more detail later, the "closure" did have a real impact on operations at the worksites, however prostitution activities did not completely cease at the affected worksites. Most of the disruption to the worksites started just after the official closure of the sites at the end of November. Field work conducted in January and February revealed that frequent raids were occurring at the formal sites in Malang. In addition, a several of the formal worksites in Malang were transformed into karaoke centers, and FSWs located

[1]Eid al-Fitr is the also called the Feast of Breaking the Fast, which occurs at the end of the Islamic holy month of fasting, Ramadan

at those sites began referring to themselves as music guides. Women provide sex service if it was requested, but such activities now occur outside of the boundaries of the formal worksite, clandestinely. The situation calmed over the ensuing months. However, since the closure of the worksites, the local health ministry is no longer conducting regular health exams and the Pokjas are less able to provide condoms, putting the FSWs at greater risk of infection with STIs.[2]

3.3 Data Collection

3.3.1 Survey Data Collection

Data for this project was collected by the authors in partnership with a local survey firm.[3] Baseline data was originally collected in relation to a field experiment designed to study the relationship between liquidity constraints and risk behaviors among female sex workers by offering a subsample of women savings accounts. The baseline survey questionnaire was designed to collect information on sex worker demographics, employment and income, past savings behavior, characteristics of clients and commercial sex transactions, HIV/STI knowledge, risk and time preferences, personality type, and cognitive ability.

Baseline field work was conducted during February and March of 2014. We worked with a local community service organization (CSO) to identify 17 worksites in the neighboring areas of Malang District, Pasuruan City and District, and Batu City in East Java, Indonesia.[4] Each of the identified worksites had at least one sex worker who had a previous relationship with our partner CSO; however, all FSWs working at the site were surveyed regardless

[2]See *Bupati Merasa Ditelikung Pengelola Eks Lokalisasi* (2014) for an example of a news article discussing the continued commercial sex activities and surprise raids of worksites in Malang.

[3]SurveyMETER, located at Jl. Jenengan Raya No.109, Maguwoharjo, Depok, Sleman Yogyakarta 55282

[4]Moving forward, I will refer to these three locations only as Malang, Pasuruan, and Batu.

of whether they had a prior relationship with our partner CSO. In addition to surveying FSWs, we also surveyed a sample of clients and collected basic information on each of the 17 worksites, including the total number of FSWs working at the location and information about the worksite structures.

In total, we surveyed 505 FSWs across the 17 worksites, which comprises the population of FSWs at these locations, and 300 clients. The *Malang Post* estimated that the localizations set for closure in Malang consisted of as many as 327 FSWs (*November, Pemkab tutup Tujuh Lokalisasi*, 2014). We surveyed a greater number of women at each worksite for which the article provided estimates. Table 3.1 shows the baseline sample sizes of FSWs (column (2)) and clients (column (3)) at each worksite type. The majority of our sample is at the formal worksites in Malang.

In July of 2014, after baseline data was collected, it was announced that the local government would be closing the worksites on November 28, 2014. Due to the disruption of criminalizing the worksites, we were unable to move forward with the original field experiment studying the impact of offering savings accounts on FSW risk behaviors. However, seeing this as an opportunity to study the effects of criminalizing sex work on the structure of the sex market and FSW risk behaviors and health, we shifted focus to develop a strategy to follow-up with all FSWs and clients at the worksites included in our baseline data sample to understand the effects of the worksite criminalization on FSW risk behaviors and well-being, client risk behaviors and demand for transactional sex, and the structure of the commercial sex Market in Malang overall. To this end, we designed an endline survey for FSWs and clients on topics covered in baseline and on the effects of the worksite closure. We also designed a short worksite questionnaire that asked a worksite contact about worksite operations and changes that occurred after criminalization.

Endline data collection took place during May and June of 2015. During endline, an effort was made to recontact FSWs from our baseline survey, as well as any additional women who

were now working at the study locations, to conduct a full endline survey. The respondents to this endline survey comprise our endline analysis sample. If a baseline respondent was unable to be reached in person at the site, we also conducted telephone surveys and "informant" surveys, in which we asked women who knew the respondent about the original respondent's current whereabouts and occupation.[5] Therefore, we have panel data on a sample of FSWs over time. We perform all analyses using both the full baseline and endline samples of FSWs and the sample of panel FSWs. Because we contacted clients at the worksites and home addresses were not recorded, we did not aim to follow-up with clients from baseline, but conducted an additional cross-sectional endline client survey.[6]

3.3.2 Biological Data Collection

In addition to baseline and endline surveys, we also collected biological test results for a sample of the FSWs from our baseline sample in September of 2014, prior to the worksite closures in Malang, and in September of 2015, after criminalization and after our endline fieldwork was complete.[7]

While the baseline and endline surveys ask FSWs about sex practices and STI symptoms, there is some doubt about the reliability of self-reported behavior and health data. For example, the sex worker might report condom use because she knows from prior interactions with non-governmental organizations or health practitioners that she should use a condom. Therefore, results from the biological tests can provide a more reliable measure of STI preva-

[5]We use information from the telephone and informant surveys to understand what FSWs choose to do after worksite closures, but do not use this data in our main analyses in this paper.

[6]Table C.1, Panels A and B, show the baseline and endline data samples at each worksite type for the FSWs and clients, respectively. In addition, Panel A of Table C.1 provides information on the size of the panel sample.

[7]Our baseline sample of FSWs comprised a universe of the FSWs at the worksites at the time that we conducted baseline field work. However, sex workers are a transient population, with many women originating from outside of our study cities in other areas of Java. Therefore, we were not able to obtain biological samples for some of women interviewed at baseline. In addition, some women who we did not interview at baseline were taken to get biological testing. Table C.1 presents the sample sizes for the Biological Test Sample in columns (4)-(6).

lence, which could also inform us about the reliability of the self-reported condom use. Most reliable studies supplement self-reports of STI symptoms and condom use with biological testing (Baird et al., 2012; Hong et al., 2011).

Biological test results were collected with assistance from the local health ministry in Malang, and with the assistance of our partner CSO via mobile health clinics at the localizations in Pasuruan and Batu. Although the health ministry was no longer coordinating with the criminalized localizations in Malang at endline, we were able to work with them to conduct health exams on a sample of FSWs on a one-time basis. The tests included a standard diagnostic test using microscopy to analyse biological swabs for the presence of Gram-negative intracellular diplococci in polymorphonuclear leukocytes (PMNL) for the diagnosis of gonorrhea.[8] In addition, a Whiff Test is performed to identify the presence of bacterial vaginosis, which, while not a sexually transmitted infection, is an indicator of unsafe sexual practices and vulnerability to contracting STIs.

3.4 Empirical Framework and Results

We analyze the impact of criminalizing the formal worksites in Malang using a difference-in-differences strategy, comparing FSWs at the criminalized worksites to those at non-criminalized sites, before and after criminalization occurs. For the main analysis, the control group of FSWs is FSWs at non-criminalized, formal worksites. In the appendix, we expand the control group to include FSWs at non-criminalized, informal worksites as well.[9] We supplement the analysis of the impact on FSWs with data on clients, to show that impacts on both the supply and demand side of the market are consistent. The following equation presents the main specification that we use for our analysis of individual level data on FSWs

[8]This test detects 40-60 percent of culture-positive specimens in women. The specificity of the test, 80-95 percent is dependent upon the experience of the microscopist (Unemo et al., 2013).

[9]We will discuss the reasoning for the restricted control group in the following section.

and clients.

$$Y_{ist} = \beta_1 Crim_s \cdot Endline_t + \beta_2 Endline_t + X_{ist}\xi + \alpha_1 S_s + \epsilon_{ist}, \tag{3.1}$$

where Y_{ist} is the outcome of interest for FSW or client i at worksite s in time t (e.g., probability of having an STI, condom use, access to health exams, sex work activities, etc.); $Crim_s$ equals 1 if worksite s is a site where sex work is criminalized, 0 otherwise; $Endline_t$ equals 1 for the the period after the criminalization (i.e. data from endline surveys); and ϵ_{ist} is an error term for individual i at worksite s in time t. In all analyses, β_1 is the DD parameter estimate of interest and is reported in the tables.

For the regressions using the FSW individual level data, X_{st} is a vector of covariates that includes individual controls for marital status, age, years of education, whether the FSW has children, the number of years the FSW has been at the worksite, an estimated discount factor based on a hypothetical scenario to elicit time preferences, and an indicator for risk tolerance based on a risk game with monetary rewards, and S_s is a set of worksite fixed effects. For the regressions using the client individual level data, X_{st} is a vector of covariates that includes individual controls for marital status, age, years of education, an estimated discount factor, and an indicator for risk tolerance, and S_s is a set of city by worksite type (formal, informal) fixed effects.[10] Standard errors are clustered at the worksite level for all analyses.[11]

[10]We do not include worksite fixed effects for the client regressions for two reasons. First, clients were not interviewed at all worksites at endline, so including worksite fixed effects over-controls for variation in the sample. In addition, clients are more likely than sex workers to move around to different worksites. It is likely that clients visit worksites around the city in which they live. Therefore, controlling for similarities of men within cities and worksite types is more appropriate than at individual worksites. However, specifications with worksite fixed effects have been estimated, and results are consistent with those shown.

[11]In total, there are 17 worksites, so there are, at most 17 clusters for the analysis using all non-criminalized worksites as the control group. For our main analysis, which restricts the control group to only FSWs at formal worksites, there are 10 worksites and 10 clusters. Cameron and Miller (2015) suggest that at least 20 clusters is a good rule of thumb to ensure that the OLS model is not over-fitted. To correct for this, we follow Cameron, Gelbach and Miller (2008) and employ the wild cluster bootstrap-t procedure to estimate

In addition to individual level data, we also have data from FSWs and clients that is measured at the transaction level, with multiple observations per individual. For analysis using the transaction level data, we use a similar difference-in-differences specification, but we include a full set of individual level fixed effects instead of worksite fixed effects and cluster standard errors at the individual level. In our analysis, we use this data to measure risk behaviors during commercial sex transactions. In these specifications we control for client characteristics, including whether the client for a particular transaction is a regular client (someone the FSW sees on a regular basis) or a casual client. [12]

3.4.1 Summary Statistics

Female Sex Workers

Panels A and B of Table 3.2 present baseline summary statistics of the FSWs in our study sample, by sample and worksite type. Panel A presents summary statistics for all FSWs surveyed at baseline. Panel B presents summary statistics only for FSWs surveyed at baseline who were also surveyed at endline and indicated that they were still engaged in sex work at endline. These are the two criteria used to define the panel sample in our analysis.[13] Column (1) presents the baseline summary statistics for FSWs at the criminalized worksites. Column (2) presents the baseline summary statistics for FSWs at all non-criminalized sites, including both formal and informal worksites, and column (4) presents the baseline means for FSWs at

appropriate p-values for our main coefficient of interest, β_1. These are included in all results tables.

[12]We have additionally controllled for whether the client is rich, clean, old, handsome, or from outside of the city in which the worksite is located and results are qualitatively similar.

[13]Note that we were able to follow-up with a larger sample of FSWs than is implied in Panel B of Table 3.2. We were able to obtain follow-up information on 348 FSWs from our baseline sample and conduct full interviews with 219 FSWs from baseline. See Table C.6 for additional information. These follow-up rates are quite high when considering that many FSWs are migrant workers, originating from outside of our study areas and often traveling home to visit with family or children. Our overall follow-up sample implies attrition of 31 percent; when restricting the follow-up sample to FSWs for whom we were able to obtain a full endline survey, attrition is 56 percent. This attrition is comparable to that in other surveys of migrant workers. For example, attrition in the Urban Migrant Survey in the Longitudinal Survey on Rural Urban Migration in China was 64 percent between the first and second wave from 2008 to 2009 (for the Study of Labor , IZA).

only the formal, non-criminalized worksites. Columns (3) and (5) present the p-values from a statistical test of the difference between the FSWs at the criminalized and non-criminalized worksites. Panel B of Table 3.2 present summary statistics for the subset of FSWs who are in our panel sample.[14]

Overall, Table 3.2 shows that FSWs at the criminalized and non-criminalized sites are largely similar, and that this similarity is consistent in both the full and panel samples. FSWs in our sample have 5-6 years of education on average, less than 20 percent are married, 90 percent of the women have at least one child, and levels of patience and risk tolerance appear to be consistent across samples. However, women working at the informal worksites tend to be older and to have worked at their current location for longer. This can be seen by comparing columns (2) and (4) relative to column (1). The average age of the sample of FSWs at all non-criminalized sites, including the informal sites, is about two years older than the average age of the FSWs at the criminalized sites and the non-criminalized, formal worksites. Likewise, women at all non-criminalized sites have been working at their locations approximately one year longer than FSWs at only non-criminalized, formal worksites and than FSWs at criminalized, formal worksites.[15]

Because of these differences between the FSWs at formal and informal worksites and because the informal worksites tend to be much smaller than the formal worksites (see Table 3.1), our main results will highlight estimates that use only FSWs at the formal worksites in Pasuruan in the control group. While including all non-criminalized worksites in our analysis allows us to compare FSWs at criminalized sites to non-criminalized sites within Malang, there are differences between the two types of worksites that make FSWs at informal sites less ideal comparisons for FSWs at formal worksites. Nevertheless, we also perform analyses

[14]Table C.2 in the appendix additionally shows the sample means for the FSWs at the informal, non-criminalized worksites only in column (6) and the p-value for a test of difference in means between the FSWs at the criminalized worksites and those at the informal, non-criminalized worksites.

[15]Table C.2 shows the differences between the FSWs at formal, non-criminalized worksites and informal, non-criminalized worksites directly.

combining the samples of FSWs at formal and informal non-criminalized worksites in the control group. These results are available in the appendix.[16]

Clients

Table 3.3 presents baseline summary statistics of the clients in our study sample. Again, column (1) presents summary statistics of clients at the criminalized worksites, while columns (2) and (4) show summary statistics for the control group that comprises clients from both formal and informal criminalized sites and from only formal criminalized sites, respectively. Columns (3) and (5) present p-values from the simple test of the difference in means between columns (1) and (2) and (1) and (4). Table 3.3 shows that clients at the criminalized worksites are more likely to be married, are older, and have lower discount factors, implying clients at criminalized worksites are less patient than those at non-criminalized sites. For the client analysis, we compare clients at the criminalized worksites to clients at all non-criminalized worksites. While FSWs tend to work at only one worksite, clients are more likely to visit several different locations.

At baseline, 42 percent of clients said that they visited worksites in more than one location, and, at endline, 33 percent of clients reported visiting more than one worksite. Therefore, in the difference-in-differences specifications using the client data, we use the clients at the non-criminalized worksites, including formal and informal worksites as the control group for clients at the criminalized sites.[17]

[16] We have also run specifications using triple differences (DDD), using FSWs at the informal worksites in Malang and Pasuruan as a second control group. The DDD estimates are consistent with the findings of the DD specifications. However, due to small samples in some of the subgroups. we do not present these results in the paper. Tables are available upon request.

[17] We also use the whole sample of clients at non-criminalized worksites for practical reasons, as we were unable to survey clients at all worksites included in the baseline sample at endline. Therefore, restricting to formal worksites is unnecessarily restrictive. We have run all client specifications restricting the control group to just those clients at the formal non-criminalized worksites and the results are qualitatively similar. We do not report these results in the text, but tables are available upon request.

At baseline, 64 percent of clients at the criminalized worksites are married compared to 53 percent of clients at non-criminalized worksites. Clients at the criminalized worksites are 3 years older than clients at non-criminalized worksites, on average, and have 1 more year of education. Although clients at criminalized worksites seem to be less patient than clients at the non-criminalized worksites, measures of risk tolerance are consistent across the client samples.

3.4.2 Impact of Criminalization on Worksite Operations

Before going into the main results on the impact of criminalization on FSW health outcomes, we first discuss the impact of the criminalization on the size of the of the sex market in Malang. We would expect that criminalization of the worksites would decrease the number of FSWs and clients engaging in commercial sex transactions at the affected worksite. This is because criminalization increases stigma associated with commercial sex work, effectively increasing the barrier to entry into the market (Guista, Tommaso and Strom, 2009). To assess the impact of criminalization on worksite operations, we utilize information collected from the worksite surveys.

At both baseline and endline, as well as during a midline census, we collected information on the total number of women at each worksite. We use these population counts to construct and event study graph, shown in Figure 3.1. Figure 3.1 presents the change in the population of FSWs at the criminalized worksites, the formal, non-criminalized worksites, and all noncriminalized workistes, with the population measure normalized to the FSWs population at baseline. In the figure, the timing of the population counts are indicated with markets, in March 2014, September 2014, and May 2015. The event study figure shows that there was a decrease in all worksite populations from March 2014 to September 2014. However, from September 2014 to May 2015, the FSW population at the criminalized worksites

decreased dramatically down to 40 percent of its baseline population level, while the populations at the non-criminalized worksites remained stable. This figure highlights that the worksite closures in Malang did decrease the size of the formal worksites in Malang, while leaving the non-criminalized worksites unchanged. In addition, this figure presents evidence that it is not the case that FSWs left the criminalized worksites in Malang in favor of the non-criminalized worksites in the surrounding areas. Overall, the population of sex workers at the criminalized worksites decreased by about 60 percent.

Although we did not ask for the number of clients that visit a worksite at baseline and endline so that we could replicate Figure 3.1 for clients, during our endline field work we asked informants at 10 of the surveyed worksites whether there had been an change in worksite operations since December 1, 2014.[18] Worksite informants were asked to report whether the number of clients visiting the worksites had increased, decreased, or stayed the same. At the criminalized worksites, 100 percent of the informants reported that the number of clients visiting the worksites had decreased since December 2014. At the non-criminalized worksites, 70 percent of informants reported a decrease in the number clients, while 30 percent reported that the number of clients had stayed the same or had increased.

Overall, evidence suggests that criminalizing sex work at the formal worksites decreased the number of FSWs working at the sites, as well as the number of clients visiting the sites. In the following sections, we will explore the impacts of criminalizing sex work on FSW health and behavior among FSWs that continue to engage in sex work post-criminalization.

[18]December 1, 2014 was chosen as a reference date because sex work at the formal worksites was meant to officially end on November 28, 2014. Because informal worksites in Malang and the worksites out side of Malang were not facing criminalization, we selected a salient date, the first of the month, just after the criminalization date, without referring directly to the criminalization of sex work.

3.4.3 Impact of Criminalization on Sex Worker Health

Using the specification presented in equation (1), Columns (1) through (5) of Table 3.4 present estimates of the impact of criminalization on self reported STI symptoms and testing positive for STI symptoms from the biological test, using FSWs at non-criminalized formal worksites as the control group. Each column in the table represents a different regression, with the outcome indicated in the the column heading. Panel A of Table 3.4 presents results using the full baseline and endline sample, and Panel B presents results for the panel sample.[19]

Across all outcomes the criminalization of worksites appears to have had a positive impact on self-reported STI symptoms, as well as an increase in the probability of testing positive for STI symptoms during the biological test. Across the self-reported symptoms, the strongest results come from the estimated impact of criminalization on "discharge". The estimated impact of criminalization on reported discharge averages by 12.1 percentage points on average, representing a large increase of over 300 percent from the baseline rate of 3.5 percent reporting. The estimate of the impact of criminalization on self reported STI rates in column (1) is consistent with this estimated increase n discharge. Column (1) of Table 3.4 reports that criminalization increased the probability of reporting at least two of the symptoms in columns (2) through (4) by 8.9 percentage points, representing a 160 percent increase from the baseline rate of 5.6 percent.

Column (5) of Table 3.4 presents results for the impact of criminalization on the probability of testing positive for STIs during the biological test. The outcome variable is equal to 1 if the FSW tested positive for cervicitis, which is an indicator for gonorrhea, or bacterial vaginosis, an indicator of unsafe sexual practices.[20] Column (5) shows that there was an increase in

[19]FSWs are selected for our panel sample if they were interviewed at baseline and at endline and indicated that they were still engaged in sex work at endline.

[20]These measures are standard in the public health literature (Unemo et al., 2013).

the probability of the presence of positive indicators for STIs at criminalized worksites of about 27 percentage points, representing a 58 percent increase in the presence of positive markers from the baseline mean of 46 percent.[21] Note that individual controls have not been included in the regression in column (5). This is because some health exams at baseline were administered to FSWs who were not included in the baseline survey; therefore, including baseline controls reduces the sample size. We have run specifications of the health exam regressions using only the sample of FSWs for which we have survey data and include controls in the regression. The magnitude and significance of the coefficient on the DD interaction remains qualitatively similar after the sample restriction and inclusion of controls.[22]

Therefore, the results in columns (1) through (5) of Table 3.4 indicate that there was an increase in the prevalence of STIs at the criminalized worksites relative to the non-criminalized sites. One hypothesis for the increased prevalence of STIs among FSWs at the criminalized sites is that criminalization broke down the organizational structure of the formal worksites in Malang. This organizational disintegration decreased the ability of the worksite to organize visits to local health centers for exams. Moreover, because sex work is now occurring clandestinely at the criminalized worksites, there may be new barriers to promoting condom use at the formal worksites. For example, in one formal worksite where sex work was criminalized, signs around the complex that read "Condoms must be used here" are now advertising karaoke activities around the complex. The following section explores the impact of criminalization on access to health exams and condoms.

[21] The baseline rate of 46 percent of FSWs at the criminalized worksites is consistent with other measures of STI rates among the FSW population in Malang. According to the 2011 Integrated Biological and Behavioural Survey in Indonesia, 36.4 percent of FSWs tested positive for Gonorrhea and 34 percent of FSWs tested positive for Chlamydia in Malang City (*Integrated Biological and Behavioral Survey*, 2011)

[22] These tables are available upon request. We additionally run the regressions in columns (1) through (4) of Table 3.4 on the restricted sample for which we have health test results and find qualitatively similar results.

3.4.4 Impact of Criminalization on Access to Health Exams and Condoms

Access to Health Exams

Column (6) in Panel A of Table 3.4 suggests that criminalization may have had a negative impact on the probability of having had a health exam in the past three months. This negative result is qualitatively small, representing a 6 percent decrease from baseline, and reduces to zero when the sample is restricted to the panel sample in our specifications using only FSWs at formal worksites in the control group. In the appendix Table C.3, when FSWs at non-criminalized informal worksites are included in the control group, estimated coefficients on the DD interaction in the regressions in are both negative, suggesting a 13 percent decrease in access to health exams. However, these results remain statistically insignificant.

This evidence points toward a negative impact of criminalization on access to health exams; however, evidence is weak. One explanation for a weak impact on access to health exams may be that women at the formalized worksites are accustomed to receiving regular exams, and continue doing so even after the criminalization. Although local health centers are no longer coordinating exams en masse with FSWs from the formal worksites, they are still happy to perform exams when women visit the center. This possibility is reflected in the fact that we were able to coordinate with the health centers to administer health exams in September 2015, after sex work at the worksites had been criminalized.

Access to Condoms and Condom Use

Columns (7) through (10) of Table 3.4, present evidence on the impact of criminalization on condom access and condom use. Column (7) presents convincing evidence that women at the criminalized worksites had significantly less access to condoms. FSWs at criminalized

worksites are 44.9 percentage points, or 63 percent, less likely to report having easy access to a condom at the worksite in our whole sample, and in the panel sample, FSWs are 40.6 percentage points, or 52 percent less likely to report having easy access to condoms. Additionally, column (8) presents estimates that the average price of condoms increased by over 150 percent in the full sample and by 120 percent in the panel sample. Both of these findings suggest that access to condoms decreases at criminalized worksites, perhaps because local markets no longer sold condoms so as not to draw attention to the possibility that sex work was continuing at the criminalized worksites. If this decreased access to condoms resulted in decreased use of condoms during commercial sex transactions, FSWs at criminalized worksites would have much greater exposure to potential infections, leading to the increased prevalence of STIs.

Column (9) of Table 3.4 shows evidence that reduced access to condoms at criminalized worksites decreased use of condoms during commercial sex transactions, as reported FSWs. The unit of observation in this regression is the transaction, and there are up to three transactions per sex worker. Because there are multiple observations per FSW in each period, individual fixed effects are included in place of individual controls. In addition, we control for whether the client associated with the transaction is a regular client (i.e. someone the FSW sees often) or a casual client. The coefficient estimate indicates that criminalization increases the probability that a condom is not used during a transaction by 13 percentage points, nearly doubling the rate of non-condom use from baseline. [23] Finally, column (10) presents evidence on condom use as reported by clients. Consistent with reports by the FSWs, clients are 14.2 percentage points more likely to report never using a condom during a commercial sex transaction at criminalized worksites.

[23]We have a broader range of client controls, including whether the client was rich, clean, or handsome, among other characteristics. When the full set of client controls is included, the estimated impact of criminalization on non-condom use is qualitatively similar. In addition, we have run similar specifications using reports on condom use during transactions by clients and find consistent results on non-condom use. Tables available upon request.

Overall, FSWs at criminalized worksites are more likely to report and exhibit symptoms consistent with STIs. We show that this is due primarily decreased access to condoms. Column (6) of Table 3.4 suggests that FSWs at criminalized sites had reduced access to health exams, though these results are qualitatively small and not significant. More convincingly, Columns (7) and (8) of Table 3.4 confirm that FSWs at the criminalized sites had reduced access to condoms and columns (9) and (10) confirm that the decreased access to condoms also resulted in decreased condom use. Thus, evidence is consistent with the story that criminalizing the formal worksites led to an increased incidence of STIs due to decreased access and use of condoms and possibly decreased access to health exams.

3.5 Alternative Mechanisms

In the previous section, we showed that the criminalization of the formal worksites in Malang resulted in increased prevalence of STIs. We argue that the increase in STI symptoms is due to a decreased access to condoms which resulted in a decreased use of condoms. In addition, women may have lost access to regular health exams and were unable to treat such symptoms. However, there are other explanations for the increased prevalence of STIs. For example, if women at criminalized worksites are now seeing more clients or are engaging in riskier sex, then we might also see an increase in STIs, independent from any decrease in access to condoms or health care. In the following sections, we will explore other potential mechanisms that could explain the increased prevalence of STIs among FSWs at the criminalized sites.

3.5.1 The Impact of Criminalization on Worksite Operations

Table 3.5 presents results from using equation (1) to estimate the impact of criminalization on the operations of the criminalized worksites. Columns (1) through (4) of table 3.5

present estimates for the impact of criminalization on the number of clients, the number of transactions, the number of hours worked, and the total earnings in the past seven days, as reported by the FSWs. Each column represents the difference-in-differences (DD) estimate from a different regression, with the outcomes in the column headings. Across all outcomes, the estimates are economically and statistically insignificant. Columns (5) through (7) of Table 3.5 presents similar outcomes as columns (1) through (3), reported by clients. Again, across all outcomes, estimated coefficients on the DD interaction term are economically and statistically insignificant.

Overall, none of the impacts on outcomes in Table 3.5 are statistically significant (and in most cases the standard errors are over twice as large as the estimate). Therefore, we interpret these results as indicating that the criminalization of worksites in Malang had no systematic impact on worksite operations in terms of the number of clients seen by FSWs, transactions per week per FSW, or hours worked. Client reporting of sex work utilization are consistent with accounts from FSWs.

3.5.2 Changes in Transaction, Sex Worker, and Client Characteristics

While the previous section explored the impact of criminalization on the volume of activity, in this section we explore whether there was a change in the type of transactions that took place. Our primary argument is that the criminalization of prostitution decreased FSW access to condoms and possibly health exams, thereby making them more susceptible to acquiring an STI. An alternative explanation for the increase in the prevalence of STIs could be that the nature of the transactions changed or that the types of clients who visit the worksites changed.

Columns (1) through (8) of Panels A and B of Table 3.6 presents results from estimating

the impact of criminalization on the type of transactions and types of clients serviced at the criminalized worksites, as reported by the FSWs. Columns (1) through (8) of Panel C estimates the impact of criminalization on the type of transactions and types of FSWs providing services, as reported by the clients. The unit of observation is the transaction.

The first two columns of Table 3.6 shows that there is no change in the type of activity that occurs during a transaction, as measured by the probability of a transaction involving vaginal sex or anal sex. In addition, column (3) indicates that there was no change in price of a typical transaction, as reported by both the clients and FSWs. Therefore, there is no evidence that criminalization changed the types of transactions taking place at the criminalized worksite, outside of the increased incidences of non-condom use.

In columns (5) through (8), we explore whether FSWs reported any changes in client characteristics or clients reported any changes in FSW characteristics. FSWs are not more likely to service casual clients, nor are clients more or less likely to be clean, attractive, or wealthy. However, FSWs at criminalized sites are more likely to report servicing clients originating from outside of Malang. These results are not robust to including FSWs at non-criminalized informal worksites in the control group (see Table C.5). Nevertheless, future analysis will evaluate whether these clients are systematically riskier than other clients in order to determine whether this could be partially contributing to our results. Overall, changes in the prevalence of STI symptoms does not seem to be driven by significant changes in the types of transactions occurring at the criminalized worksites.

While clients report no statistically significant changes in whether they are being serviced by an FSW they visit regularly or in the attractiveness of the FSW, they do report a decrease in the cleanliness of the female sex worker. This could be reflecting that FSWs are more likely to be infected with an STI as a result of the worksite criminalization.

Along with data on characteristics of FSWs and Clients from survey questions about trans-

action characteristics, we also have basic information on FSW and client characteristics from the baseline and endline surveys. Columns (9) through (12) of Table 3.6 present DD estimates of the impact of criminalization on FSW and client marital status, years of education, age, patience, and risk tolerance. In addition, we also present estimates for the impact of criminalization on the probability that an FSW has children.

There are not any statistically significant impacts on age, marital status, or patience for either the clients or the FSWs. However, there is some evidence that FSWs at the criminalized sites have a year more of education after criminalization occurs. These estimates are robust to including individuals at non-criminalized informal worksites into the control group. Future analysis will explore whether these changes in education could be contributing to the observed changes in condom use and STI rates. However, you would expect more highly educated FSWs to engage in safer sex practices and be more likely to use a condom during a commercial sex transaction. In addition, it is comforting that the estimated impacts on STIs and condom use are consistent when the sample is restricted to the panel of FSWs, where education levels are not changing over time, since there is no change in composition of the sex workers in the panel sample.

Column (11) presents evidence that clients at criminalized sites are younger than clients at non-criminalized worksites after criminalization occurs. Analysis of the relationship between risk preferences and age show that older clients are more risky and less likely to use a condom. Therefore, it does not appear to be the case that the change in composition toward younger clients is explaining the decreased condom use and the increased prevalence of STIs among the FSWs.[24]

Overall, it does not seem to be the case that changes in transaction, FSW, or client characteristics are driving the main results. In addition, we are including these controls in all of our regressions at baseline and endline, so that any changes in composition in terms of age

[24]Results available upon request.

or education are controlled for.

3.5.3 Sample Selection at Endline

Although it is comforting that there does not seem to be any large compositional changes in the types of FSWs at the criminalized worksites during endline and that the results are consistent across the cross-section and panel samples, there still may be a concern that certain types of FSWs were less likely to appear in our endline data or stay in sex work at the criminalized worksites than at the non-criminalized worksites. Because we have a panel of a sub-sample of the FSWs that we interviewed at baseline, we are able to estimate the determinants of not appearing in our endline sample or leaving sex work. Columns (1) and (2) of Table C.7 presents coefficient estimates from a regression of an indicator for leaving the analysis sample on individual characteristics, interacted with an indicator for being at a worksite that was criminalized. Column (1) restricts the sample to only those FSWs at formal worksites at baseline, which is our main analysis sample. Column (2) uses the full sample of FSWs at both formal and informal worksites.

Focusing on Column (1) of Table C.7, none of the included characteristics are statistically significant predictors for leaving the sample or leaving sex work. Therefore, there is no evidence that certain types of women were more likely to leave our sample when the sample is restricted to women at the formal worksites. In column (2), there is some evidence that older women are more likely to leave the sample at the criminalized worksites. This confirms our concern that FSWs at the informal worksites should not be included in the control group because of differences in the characteristics and behaviors of formal versus informal sex workers.

In addition to concern about differences in the types of FSWs that are included in our baseline and endline survey samples, there may be concern that a change in the composition

of the FSWs who are tested for STIs in our biological test samples is driving the increase in STI rates. Column (3) of Table C.7 presents estimation results from a regression estimating the probability of not being tested for STIs at endline after being tested at baseline. The sample is restricted to FSWs at formal worksites who were tested in September 2014. This estimation shows that FSWs at criminalized worksites who tested positive at baseline were not more likely to leave the biological test sample than FSWs at non-criminalized worksites. However, there is some evidence that FSWs who tested positive at baseline were more likely to leave the biological test sample in general. Nevertheless, given that the rate of leaving the sample did not vary between criminalized and non-criminalized worksites, this does not bias our estimates of the impact of criminalization on STI rates among FSWs.

Overall, Table C.7 does not provide evidence that the increased STI rates or reduced condom use is being driven by systematic changes in the types of FSWs who appear in the endline sample compared to the baseline sample.

3.6 Discussion and Conclusion

This study provides the first causal estimates of the impact of criminalizing sex work in the context of a developing country on the prevalence of STIs. We exploit a natural experiment in which sex work was criminalized in one district in East Java, Indonesia, while it remained non-criminalized at worksites in a neighboring district. To evaluate the impact of criminalization on FSW health outcomes and behavior, we utilize a unique dataset on FSWs and clients collected by the authors. The data is comprised of the population of FSWs at the study sites and the first panel data on FSWs to date. This is also the first known quantitative and representative dataset on clients of sex workers.

We find that criminalization does decrease the number of FSWs engaging in sex work 6

months after the criminalization; however, at minimum, 40 percent of the FSWs from baseline continue to engage in sex work. Comparing FSWs at criminalized worksites to FSWs at non-criminalized worksites using a difference-in-differences frame work, we find that criminalization increased STI rates by about 60 percent, measured by self reports and biological test results. The main mechanism driving this increase in STI rates is decreased access to condoms at criminalized worksites, which translated into decreased condom use during commercial sex transactions. We rule out possible alternative mechanisms that could be driving increased STI rates, such as changes in the composition of FSWs and clients at the worksites, changes in the type of commercial sex transactions taking place, and changes in number of clients served by FSWs.

The increased rates of STIs among the FSW population not only has negative impacts on FSW health and risk for contracting more serious diseases, such as HIV, but also could have implications for population-wide STI rates. This is because the probability that clients match with an infected FSW increases, increasing the likelihood that the client becomes infected and spreads the infection among his other sexual partners. At the same time, the decreased size of the FSW population and unchanged work volume per sex worker implies that the size of the sex market is reduced, which could lead to an overall decrease in population STI rates. Future research will explore these trade-offs in more depth to understand the impact of criminalizing sex work on general population STI rates in addition to the increase in FSW STI rates.

Overall, this research presents new evidence that criminalizing sex work, which was intended to end commercial sex activities in Malang, puts FSWs in a more vulnerable situation, while not stopping the sale of commercial sex.

3.7 Tables

Table 3.1: Worksite Types and Baseline Data Samples

	(1)	(2)	(3)	(4)
Worksite Type	**No. of Worksites**	**No. of FSWs**	**No. of Clients**	**Criminalized**
Malang				
Formal	6	373	220	Y
Informal	3	34	23	N
Pasuruan and Batu				
Formal	4	80	44	N
Informal	4	18	13	N
TOTAL	17	505	300	

This table reports the number of worksites in each city by worksite type. as well as the number of FSWs and clients surveyed at each worksite type at baseline. The sample of FSWs interviewed at baseline represents the population of FSWs at the sample worksites at the time of the baseline survey. The sample size of 300 clients was targeted, with the distribution of clients across worksites set to be proportional to the size of the worksite relative to others. The main analysis is performed using only the FSWs at the formal worksites and the clients from all worksites.

Table 3.2: Baseline Characteristics: Female Sex Workers

	(1)	(2)	(3)	(4)	(5)
Variable	**Crim.**	**Non-Crim.**	**Diff.**	**Non-Crim.**	**Diff.**
		All	**P-Value**	**Formal Only**	**P-Value**
Panel A. Full Sample					
Married	0.180	0.197		0.138	
	(0.020)	(0.035)	0.659	(0.039)	0.366
Divorced or Widowed	0.777	0.727		0.738	
	(0.022)	(0.039)	0.243	(0.050)	0.442
Never Married	0.043	0.076		0.125	
	(0.011)	(0.023)	0.143	(0.037)	0.0041
Years of Education	5.82	5.72		5.86	
	(0.154)	(0.296)	0.766	(0.402)	0.912
Age	34.5	35.8		33.5	
	(0.381)	(0.691)	0.098	(0.817)	0.257
Children	0.906	0.864		0.850	
	(0.015)	(0.030)	0.171	(0.040)	0.136
Years at Location	2.09	3.19		2.42	
	(0.125)	(0.370)	0.0003	(0.410)	0.312
Discount Factor	0.388	0.373		0.371	
	(0.024)	(0.040)	0.758	(0.051)	0.764
Risk Tolerance	0.380	0.323		0.354	
	(0.025)	(0.041)	0.247	(0.054)	0.670
Sample Size	373	132		80	
Panel B. Panel Sample					
Married	0.285	0.167		0.125	
	(0.050)	(0.049)	0.099	(0.059)	0.072
Divorced or Widowed	0.655	0.767		0.750	
	(0.052)	(0.055)	0.150	(0.078)	0.340
Never Married	0.059	0.067		0.125	
	(0.026)	(0.032)	0.862	(0.059)	0.242
Years of Education	6.08	4.72		4.88	
	(0.320)	(0.434)	0.015	(0.673)	0.071
Age	33.8	38.4		35.3	
	(0.792)	(1.01)	0.0004	(1.21)	0.315
Children	0.929	0.917		0.906	
	(0.028)	(0.036)	0.793	(0.052)	0.691
Years at Location	1.99	3.23		2.69	
	(0.242)	(0.434)	0.008	(0.610)	0.200
Discount Factor	0.338	0.374		0.381	
	(0.049)	(0.060)	0.642	(0.083)	0.650
Risk Tolerance	0.429	0.339		0.355	
	(0.054)	(0.062)	0.283	(0.087)	0.480
Sample Size	84	60		32	

This table reports baseline means for each indicated sample. "Married", "Divorced or Widowed", and "Never Married" are indicators for the corresponding marital status; "Years of Education" is the number of years of education completed; "Age" is age in years, "Children" is an indicator for having at least one child, "Years at location" is the number of years the FSW reports being at the current worksite location, "Discount Factor" is a variable that ranges from 0 to 1, where 1 indicates zero discounting on the future, and 0 indicates 100 percent discounting on the future; "Risk Tolerance" is an indicator for selecting the riskiest option during a risk game played during the baseline and endline survey. Standard errors are reported in parentheses below the means. The reported p-values are for a simple t-test of the difference in means between the FSWs at the criminalized worksites compared to the FSWs(clients) at the non-criminalized worksites and the non-criminalized formal worksites, reported separately. The main analysis compares FSWs at the criminalized worksites to the FSWs at the non-criminalized, formal worksites. In the appendix, the population of FSWs at all non-criminalized worksites is used as the control group.

Table 3.3: Baseline Characteristics: Clients

	(1)	(2)	(3)
Variable	**Crim.**	**Non-Crim.**	**Diff.**
		All	**P-Value**
Married	0.641	0.525	
	(0.032)	(0.056)	0.069
Divorced or Widowed	0.641	0.525	
	(0.032)	(0.056)	0.069
Never Married	0.305	0.300	
	(0.027)	(0.052)	0.083
Years of Education	8.78	7.90	
	(0.218)	(0.420)	0.048
Age	39.3	36.5	
	(0.747)	(1.45)	0.074
Discount Factor	0.225	0.340	
	(0.027)	(0.051)	0.033
Risk Tolerance	0.523	0.500	
	(0.034)	(0.056)	0.729
Sample Size	220	80	

See notes for Table 3.2.

Table 3.4: Impact of criminalization on self-reports on FSW health and condom use, formal worksites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Female Sex Worker Data					**Client Data**
	Self Reports				**Test**	**Health**	**Condom**	**ln(Condom**	**No**	**Never**
	STI	**Disch**	**Sore**	**Swell**	**Positive**	**Exam**	**Access**	**Price)**	**Condom**	**Condom**
Panel A. Whole Sample										
Crim×Endline	0.089*	0.121***	0.034	0.037	0.273***	-0.063	-0.449*	1.68**	0.137	0.146
	(0.043)	(0.033)	(0.070)	(0.041)	(0.032)	(0.076)	(0.218)	(0.590)	(0.086)	(0.085)
Conventional p-value	0.067	0.005	0.635	0.392	0.000	0.426	0.069	0.019	0.112	0.103
Wild cluster bootstrap-t p-value	0.109	0.029	0.649	0.461	0.024	0.367	0.163	0.015	0.072	0.168
Endline	-0.089**	-0.104***	-0.145**	-0.024	-0.055**	-0.008	0.069*	-1.45**	-0.157**	0.036
	(0.032)	(0.027)	(0.063)	(0.020)	(0.021)	(0.053)	(0.036)	(0.515)	(0.078)	(0.080)
Sample Size	611	611	611	611	446	611	611	611	1698	593
Baseline Mean	0.056	0.035	0.220	0.054	0.464	0.916	0.714	930 IDR	0.146	0.209
Panel B. Panel Sample										
Crim×Endline	0.137**	0.112**	0.037	0.066*	0.208***	0.030	-0.406*	1.19	0.134*	
	(0.047)	(0.037)	(0.058)	(0.034)	(0.053)	(0.087)	(0.213)	(1.03)	(0.080)	
Conventional p-value	0.024	0.019	0.542	0.096	0.008	0.737	0.098	0.285	0.095	
Wild cluster bootstrap-t p-value	0.082	0.018	0.514	0.082	0.015	0.806	0.182	0.436	0.074	
Endline	-0.114*	-0.095***	-0.099***	-0.037	0.000	-0.140***	-0.022	-0.932	-0.155**	
	(0.052)	(0.016)	(0.024)	(0.024)	(0.000)	(0.037)	(0.056)	(0.892)	(0.072)	
Sample Size	232	232	232	232	148	232	232	232	643	
Baseline Means	0.054	0.032	0.204	0.054	0.340	0.935	0.774	885 IDR	0.170	
Worksite Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	N	N
City-Worksite Type Fixed Effects	N	N	N	N	N	N	N	N	N	Y
Individual Fixed Effects	N	N	N	N	N	N	N	N	Y	N
Unit of Observation	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Transaction	Individual

The reported estimate is the estimate of β_1 from equation (1). The data in the columns (1)-(8) come from the FSW surveys and the unit of observation is the individual. The data in column (9) is from the FSW surveys and the unit of observation is the transaction. There are up to three transactions per FSW. The data in column (10) is from the client surveys and the unit of observation is the individual. Note that we do not have a panel sample of clients, so there is no reported estimate in Panel B for the client sample in column (5). Clustered standard errors are presented in parentheses below the estimated coefficient. Standard errors are clustered at the worksite level in columns (1)-(8) and (10), and at the individual level in column (9). Because we are clustering at the worksite level, and there are, at most, 17 worksites, we are concerned that the standard errors may be underestimates due to few clusters. Following Cameron, Gelbach, and Miller (2008), we employ the wild cluster bootstrap-t procedure to estimate appropriate p-values for causal inference. For regressions using the FSW data, the control group is comprised of FSWs at non-criminalized formal worksites in Pasuruan. For regressions using the client data, the control group is comprised of clients interviewed at all non-criminalized worksites, including formal and informal worksites in Pasuruan and Batu. For regressions using FSW individual data (columns (1)-(8)), excluding column (5), controls for marital status, years of education, age, whether the FSW has children, the number years the FSW has been at the worksite, an estimated discount factor, and an indicator for risk tolerance are included along with worksite fixed effects. In column (9), a control for the client being a regular client is included along with individual fixed effects. In regressions using the Client individual data (column (10)), controls for client marital status, years of education, age, an estimated discount factor, and an indicator for risk tolerance are included along with city by worksite type fixed effects. Outcomes presented in columns (1)-(5) are indicator variables for reporting a symptom or presenting a positive result for the biological test. The outcome "STI" in the self-reports is equal to 1 if the respondent reports at least two of the three symptoms, discharge ("disch"), soreness ("sore"), or swelling ("swell"). "Health Exam" is an indicator variable equal to 1 if the FSW reported having a medical exam with a speculum in the past 3 months. "Condoms Access" is an indicator variable equal to 1 if the FSW reports that it is easy to obtain a condom within the worksite. "ln(Condom price)" is the natural log of the reported price of condoms plus one at the location. "No Condom" is an indicator variable equal to 1 if the FSW reports that she did not use a condom at all during a single transaction. "Never Use Condom" is an indicator variable equal to 1 if the client reports never using a condom. * p<0.10, ** p<0.05, *** p<0.01

Table 3.5: Impact of Criminalization on Worksite Operations, formal worksites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female Sex Worker Data				**Client Data**		
	No. of Clients	**No. of Trans.**	**No. of Hrs Worked**	**ln(Wkly Earnings)**	**No. of FSWs**	**No. of Trans.**	**ln(Wkly FSW Expenditures)**
Panel A. Whole Sample							
Crim×Endline	3.95	4.28	-4.38	0.667	0.665	-0.536	-0.271
	(2.76)	(2.52)	(11.8)	(0.900)	(0.925)	(0.640)	(1.27)
Conventional p-value	0.186	0.124	0.720	0.477	0.483	0.414	0.834
Wild cluster bootstrap-t p-value	0.345	0.229	0.773	0.707	0.822	0.520	0.854
Endline	-4.64*	-4.68*	-6.54	-0.058	-0.760	0.237	-0.412
	(2.28)	(2.25)	(10.4)	(0.806)	(0.920)	(0.623)	(1.09)
Sample Size	611	611	611	611	593	593	593
Baseline Mean	8.30	8.34	57.2	735,040 IDR	1.48	1.43	144,123 IDR
Panel B. Panel Sample							
Crim×Endline	1.56	2.13	-12.9	-1.11			
	(1.54)	(1.18)	(13.8)	(1.04)			
Conventional p-value	0.345	0.114	0.381	0.321			
Wild cluster bootstrap-t p-value	0.452	0.362	0.466	0.370			
Endline	-3.78**	-3.97**	-2.85	1.19			
	(1.18)	(1.17)	(13.2)	(0.95)			
Sample Size	232	232	232	232			
Baseline Mean	10.1	10.2	62.1	960,435 IDR			
Worksite Fixed Effects	Y	Y	Y	Y	N	N	N
City-Worksite Type Fixed Effects	N	N	N	N	Y	Y	Y

See notes for Table 3.4. Columns (1)-(4) use data from the FSW surveys. Columns (5)-(7) use data from the Client surveys. Outcomes of interest are indicated in the column headers. For the outcomes of the regressions using the FSW data, "No. of Clients" is the number of paying clients the respondent serviced in the seven days prior to the survey date, "No. of Trans." is the number of transactions in the past seven days, "No. of Hrs Worked" is the reported number of hours worked in the past seven days, "ln(Wkly Earnings)" is the natural log of the reported earnings by the FSWs in the past seven days plus one. For the outcomes of the regressions using the Client data, "No. of FSWs" is the number of FSWs that the client reports visiting in a week, "No. of Trans." is the number of times the client reports having sex with an FSW in the past 7 days, and "ln(Wkly FSW Expenditures)" is the natural log of the expenditures reported by the client for the past 7 days plus one. * p<0.10, ** p<0.05, *** p<0.01

Table 3.6: Impact of Criminalization on Transaction, Sex Worker, and Client Characteristics, formal worksites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Transaction Characteristics			**Client Characteristics**					**FSW Characteristics**					
	Vaginal	**Anal**	**ln(Price)**	**Reg**	**Clean**	**Attract**	**Outsider**	**Rich**	**Married**	**Educ**	**Age**	**Patience**	**Risk**	**Child**
Panel A. Reported by Female Sex Workers														
Crim×Endline	-0.004	0.000	-0.064	-0.082	-0.057	0.069	0.263**	0.076	-0.006	1.15**	-1.67	-0.025	0.064	-0.013
	(0.017)	(0.000)	(0.105)	(0.108)	(0.104)	(0.087)	(0.120)	(0.056)	(0.112)	(0.455)	(1.05)	(0.051)	(0.149)	(0.041)
Conventional p-value									0.961	0.032	0.145	0.627	0.679	0.753
Wild Cluster bootstrap-t p-value									0.993	0.035	0.331	0.697	0.655	0.749
Endline	0.013	0.000	0.269***	0.088	0.050	-0.045	-0.214*	-0.088**	-0.047	-0.731	1.984*	-0.012	0.015	0.043
	(0.014)	(0.000)	(0.085)	(0.092)	(0.088)	(0.079)	(0.111)	(0.040)	(0.064)	(0.442)	(1.026)	(0.049)	(0.141)	(0.034)
Sample Size	1721	1721	1721	1721	1721	1721	1721	1721	611	611	611	611	606	611
Baseline Mean	0.984	0.001	106,54	0.386	0.733	0.200	0.089	0.127	0.180	5.82	34.5	0.388	0.380	0.906
Panel B. Reported by Female Sex Workers, Panel Sample														
Crim×Endline	-0.004		-0.064	-0.082	-0.057	0.069	0.263**	0.076	-0.131	-0.012	0.074	0.0417	0.052	0.012
	(0.016)		(0.098)	(0.102)	(0.097)	(0.081)	(0.112)	(0.052)	(0.088)	(0.146)	(0.336)	(0.099)	(0.237)	(0.025)
Conventional p-value									0.180	0.937	0.831	0.686	0.833	0.653
Wild Cluster bootstrap-t p-value									0.290	0.884	0.870	0.648	0.914	0.600
Endline	0.013		0.269***	0.088	0.050	-0.045	-0.214**	-0.088**	0.000	0.250	1.188***	0.001	-0.000	0.000
	(0.014)		(0.080)	(0.086)	(0.082)	(0.074)	(0.104)	(0.038)	(0.083)	(0.134)	(0.313)	(0.092)	(0.217)	(0.000)
Sample Size	648		648	648	648	648	648	648	232	232	232	232	229	232
Baseline Mean	0.989		107,154	0.360	0.712	0.176	0.101	0.157	0.258	5.99	34.0	0.336	0.434	0.921
Worksite Fixed Effects	N	N	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y
Individual Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N	N	N
	Transaction Characteristics			**FSW Characteristics**					**Client Characteristics**					
	Vaginal	**Anal**	**ln(Price)**	**Reg**	**Clean**	**Attract**	**Outsider**	**Rich**	**Married**	**Educ**	**Age**	**Patience**	**Risk**	**Child**
Panel C. Reported by Clients														
Crim×Endline	0.192	0.000	0.060	0.426	-0.487*	0.439			0.003	-0.425	-5.26**	-0.030	0.015	
	(0.242)	(0.000)	(0.500)	(0.443)	(0.295)	(0.358)			(0.073)	(0.460)	(1.95)	(0.079)	(0.128)	
Conventional p-value									0.968	0.369	0.016	0.710	0.909	
Wild Cluster bootstrap-t p-value									0.930	0.364	0.042	0.812	0.95	
Endline	-0.000	0.000	0.103	-0.272	0.295*	0.023			-0.100*	0.264	3.65**	0.050	-0.082	
	(0.000)	(0.000)	(0.484)	(0.411)	(0.168)	(0.223)			(0.050)	(0.358)	(1.524)	(0.060)	(0.121)	
Sample Size	1253	1253	1233	1253	1253	1253			593	593	593	593	593	
Baseline Mean	0.971	0.013	97,380	0.510	0.839	0.482			0.641	8.78	39.3	0.225	0.523	
City-Worksite Type Fixed Effects	N	N	N	N	N	N			Y	Y	Y	Y	Y	
Individual Fixed Effects	Y	Y	Y	Y	Y	Y			N	N	N	N	N	

See notes for Table 3.4. Results in Panel A and Panel B are based on the FSW data. Results in Panel C are based on the client data. Columns (1)-(8) present estimates using transaction level data on transaction and commercial sex partner characteristics. Standard errors are clustered at the individual level and individual fixed effects are included for both the FSW and the client data. Outcomes are indicated in the column headers. "Vaginal" is an indicator equal to 1 if the transaction involved vaginal sex, "Anal" is an indicator equal to 1 if the transaction involved anal sex. In columns (4)-(8), the client (FSW) characteristics "reg", "clean", "attract", "outsider", and "rich" are indicators for whether the FSW (client) reported the client (FSW) was a regular, clean, attractive, from outside Malang, or rich, respectively. Note that it is not necessary to calculate the wild cluster boostrap-t p-value for columns (1)-(8) because standard error are clustered and the individual level, and therefore there is no issue with few clusters. Columns (9)-(14) use individual-level data. FSW (client) characteristics reported in these columns are as reported by the individual themselves. All regressions control for the characteristics included in columns (9)-(14). See Table 3.2 for variable definitions. Standard errors are clustered at the worksite level. * p<0.10, ** p<0.05, *** p<0.01

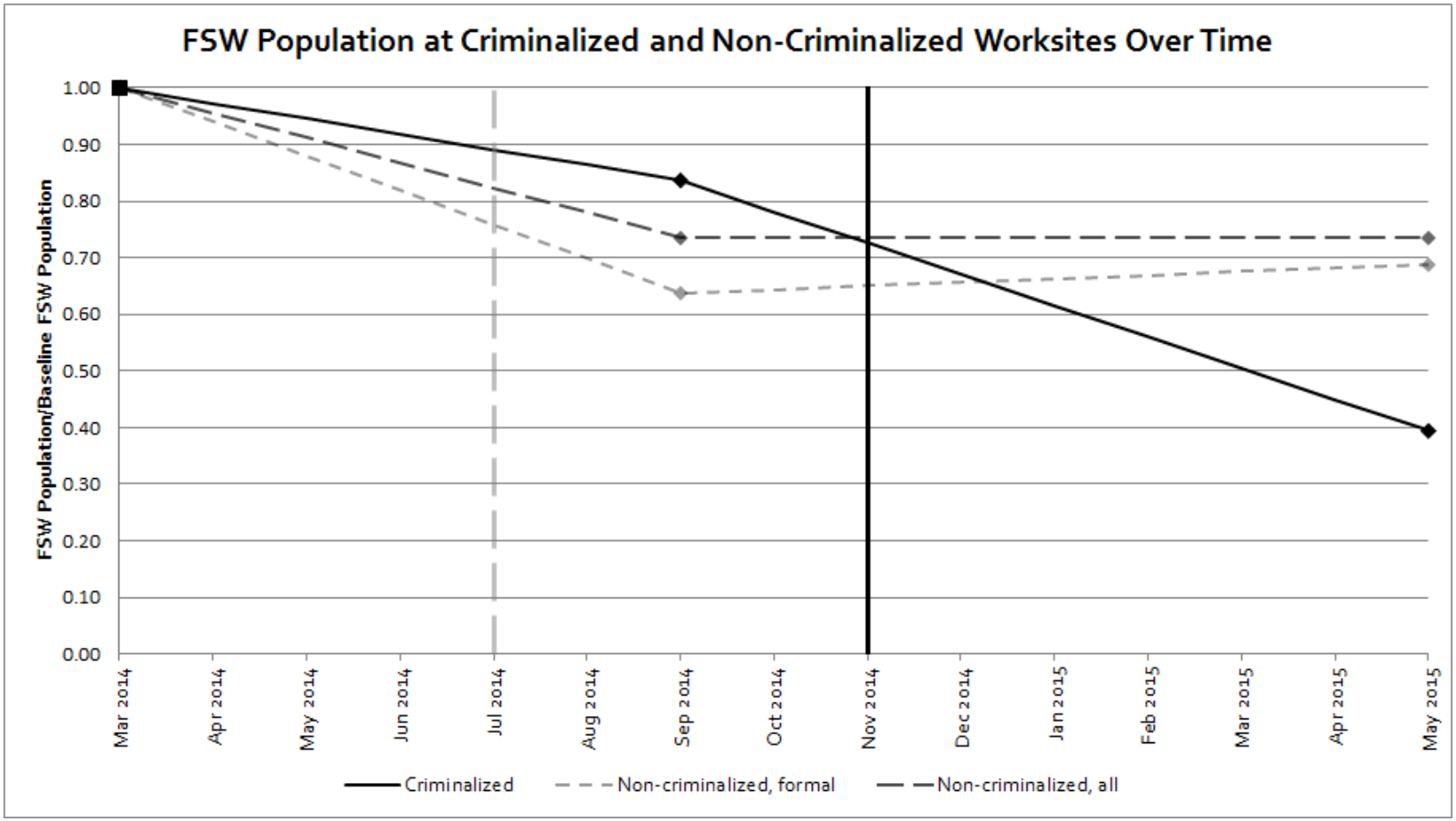


Figure 3.1: Impact of Criminalization on the FSW Population

This figure present the change in the population at the study worksites over time. The FSW population is measured at three points in time: March 2014, September 2014, and May 2015. These points are indicated with markers in the figure. The population of the FSWs at the worksites is scaled relative to the baseline populations at the worksites. The first point, March 2014, represents the populations at the worksites during the baseline survey. There were 373 FSWs at the criminalized worksites, 132 FSWs at all non-criminalized worksites, and 80 FSWs at the formal non-criminalized worksites. The second point is based on the population of women found at the worksites during a census of the worksites taken in September 2014, after the worksite closures were announced but before the worksites were closed. The final point, in May 2015, is the population of FSWs at the worksites during the endline survey. The announcement of the worksite closures occurred in July 2014 and is indicated by a light grey vertical line. The worksite closures occurred in November 2014, which is indicated with a solid, dark vertical line.

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Appendix A

Chapter 1 Appendix

Table A.1: Sample Borrowing Behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-Azteca		**Banco Azteca**		**Non-Azteca**		**Banco Azteca**	
	2002	2005	2002	2005	2002	2005	2002	2005
	1. Know of a [...] from which to borrow				**2. Borrowed from a [...]**			
Bank	0.050	0.121	0.095	0.237	0.011	0.046	0.035	0.106
Cooperative	0.112	0.234	0.184	0.262	0.094	0.188	0.167	0.154
Money lender	0.165	0.190	0.094	0.136	0.139	0.115	0.052	0.086
Relative	0.300	0.260	0.329	0.281	0.294	0.212	0.309	0.229
Friend	0.430	0.293	0.282	0.251	0.359	0.245	0.209	0.180
Work	0.084	0.088	0.160	0.090	0.073	0.091	0.150	0.067
Pawn shop	0.044	0.065	0.123	0.141	0.027	0.034	0.064	0.063
Credit program	0.006	0.006	0.014	0.027	0.004	0.000	0.008	0.021
Government loan	0.011	0.016	0.030	0.018	0.012	0.026	0.035	0.020
Other	0.007	0.059	0.014	0.112	0.020	0.072	0.016	0.143
Observations	659	387	1,207	676	659	387	1,207	676

Data are from the MxFLS surveys. All means are weighted using sample weights calculated for MxFLS-1. The Sample is restricted to individuals that indicated that they borrowed in the respective year.

Table A.2: Event Study of the introduction of Banco Azteca

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Whole Sample				**MxFLS Sample**			
	All Consumption		Food		All Consumption		Food	
	Per Capita Expenditures							
$(Azteca \cdot T1994)$	1,297**		184*		1,327		16.6	
	(563)		(100)		(1,305)		(406)	
$(Azteca \cdot T1996)$	-1,480*		-178*		-206		-230	
	(791)		(100)		(1,485)		(241)	
$(Azteca \cdot T1998)$	-440	939	-114	86.4	310	335	-143	77.3
	(741)	(560)	(99.1)	(76.4)	(1,162)	(957)	(254)	(101)
$(Azteca \cdot T2000)$	188	1,630	-101	109	895	866	-282	-44.5
	(1,363)	(1,170)	(111)	(84.7)	(1,650)	(1,199)	(264)	(113)
$(Azteca \cdot T2002)$	-238	1,189*	-119	78.2	-77.4	-288	-120	87.9
	(809)	(661)	(114)	(92.0)	(1,153)	(1,146)	(258)	(129)
$(Azteca \cdot T2004)$	1,027	511	-91.0	120	-6,831	-7,351	-183	52.9
	(1,558)	(1,783)	(112)	(106)	(4,371)	(5,112)	(276)	(124)
Observations	98,209	74,864	98,209	74,864	27,204	21,248	27,204	21,248
Sample years	1992-2004	1996-2004	1992-2004	1996-2004	1992-2004	1996-2004	1992-2004	1996-2004
F-test of hypothesis that pre-trend interactions are jointly insignificant								
F-statistic	3.38	1.58	2.72	0.630	1.08	0.470	0.550	0.440
P-value	0.005	0.192	0.019	0.599	0.376	0.701	0.737	0.728

* p<0.10, ** p<0.05, *** p<0.01. These coefficients are reported from estimation of the following specification: $y_{imt} = \alpha + \Sigma_{(t=1992)}^{2004} \delta_t (Azteca_m \cdot \theta_t) + \theta_t + \mu_m + \epsilon_{imt}$, where $t = 1992, 1994, 1996, 1998, 2000, 2002, 2004$, and $(Azteca_m \cdot \theta_t)$ is the interaction between the municipality receiving a Banco Azteca branch and a year dummy and θ_t and μ_m are a full set of year and municipality fixed effects. Unit of observation is the household. Household weights constructed for the ENIGH are used in all regressions, and standard errors are clustered by municipality.

Table A.3: Impact of Banco Azteca durable consumption and asset holding

	(1)	(2)	(3)	(4)
	Domestic appliance expenditures	**Domestic appliance holdings**	**Furniture holdings**	**Electronics holdings**
Panel 1. Main Specification				
$(Azteca \cdot T2005)$	10.3	15.8	-171	56.0
	(18.8)	(32.5)	(146)	(99.3)
Observations	12,107	11,564	11,485	11,502
Panel 2. Separately identifying the impact in highly treated municipalities				
$(Azteca \cdot T2005)$	-0.493	-49.9	-246	-39.6
	(25.1)	(34.9)	(196)	(110)
$(Azteca \cdot T2005 \cdot High)$	3.86	79.3	63.9	53.9
	(22.7)	(53.4)	(209)	(140)
Observations	11,797	11,274	11,201	11,216
Outcome mean in 2002	86.2	402	2,433	2,024

* p<0.10, ** p<0.05, *** p<0.01. See notes for Tables 5. This table presents DD estimates from the specification in equation (1).

Table A.4: Impact of Banco Azteca on consumption outcomes, exploiting variation in timing of bank branch entrance

	(1)	(2)	(3)	(4)
	Domestic appliance expenditures	**Domestic appliance holdings**	**Furniture holdings**	**Electronics holdings**
Panel 1. Treatment measured as cumulative quarters				
Cumulative Quarters	4.04	6.02	-55.7	11.9
	(5.75)	(10.6)	(45.6)	(31.5)
Panel 2. Interacting $(Azteca \cdot T2005)$ **with receiving a Banco Azteca branch late**				
$(Azteca \cdot T2005)$	10.4	23.6	-160	64.2
	(18.9)	(33.0)	(148)	(101)
$(Azteca \cdot T2005 \cdot Late)$	-4.12	-208***	-294	-211
	(55.9)	(44.2)	(186)	(259)
Observations	12,107	11,564	11,485	11,502
Outcome mean in 2002	86.2	402	2,433	2,024

* p<0.10, ** p<0.05, *** p<0.01. See notes for Tables 5. This table presents the estimates of δ from (1) $y_{imt} = \alpha + \delta(CumulativeQtrs_{mt}) + \theta T2005_t + Z_m^{'}\beta + X_{it}^{'}\gamma + \epsilon_{imt}$ in Panel 1 and the estimates of δ_1 and δ_2 from
(2) $y_{imt} = \alpha + \delta_1(Azteca_m \cdot T2005_t) + \delta_2(Azteca_m \cdot T2005_t \cdot Late_m) + \theta T2005_t + Z_m^{'}\beta + X_{it}^{'}\gamma + \epsilon_{imt}$ in Panel 2. In both specifications, $Z_m^{'}\beta$ includes only a full set of municipality fixed effects.

Appendix B

Chapter 2 Appendix

B.1 Appendix Tables

Table B.1: Share of Female Married Population compared to Share of Female Population Across Counties

	(1)	(2)	(3)	(4)
	Mean	**S.D.**	**1st Percentile**	**99th Percentile**
County Fem. Pop. / State Fem. Pop.	0.0892	0.1456	0.0044	0.7488
County Married Fem. Pop. / State Married Fem. Pop.	0.0634	0.1071	0.0029	0.6883
County Unmarried Fem. Pop./ State Unmarried Fem. Pop.	0.0654	0.1103	0.0025	0.7069
Difference between County Share and Married Share	0.0005	0.0195	-0.0457	0.0675
Difference between county Share and Unmarried Share	-0.0015	0.0196	-0.0595	0.0617

Data on County and state female population is from the SEER population estimates. The estimates of population by marital status are derived from authors calculations from the CPS ASEC.

Table B.2: Impact of Job Loss on Divorce Rates, County-Year Panel Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Married Fertility Rates($t+1$)					
Panel A. Impact of Job Loss due to a Mass Layoff Event on Divorce Rates						
Job Loss	0.000	0.001	0.000	0.002	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Panel B. Impact of Job Losses due to a Mass Layoff Event to Dual-Earner Married Couples on Divorce Rates						
Job Loss	0.001	0.001	0.001	0.013**	0.018***	0.009*
	(0.005)	(0.007)	(0.004)	(0.006)	(0.007)	(0.005)
Dual-Earner	-0.058*	-0.060*	-0.058*	-0.025	-0.022	-0.027
	(0.032)	(0.033)	(0.031)	(0.041)	(0.041)	(0.041)
Job Loss× Dual-Earner	-0.003	-0.001	-0.003	-0.051*	-0.071**	-0.038*
	(0.022)	(0.031)	(0.016)	(0.027)	(0.031)	(0.022)
Sample Size	4492	4492	4492	4492	4492	4492
Ages	15 to 44	15 to 44	15 to 44	20 to 34	20 to 34	20 to 34
Job Loss Sub-Population	All	Female	Male	All	Female	Male

See notes from Table 2.4. Outcome is the divorce rate. * p<0.10, ** p<0.05, *** p<0.01.

Table B.3: Impact of Job Loss on Divorce Rates, State-Quarter Panel Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Married Fertility Rates($t+4$)					
Panel A. Impact of Job Loss due to a Mass Layoff Event on Divorce Rates						
Job Loss	0.003*	0.003	0.001	0.006**	0.005	0.003
	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.002)
Panel B. Impact of Job Losses due to a Mass Layoff Event to Dual-Earner Married Couples on Divorce Rates						
Job Loss	-0.001	-0.002	-0.003	0.044***	0.052***	0.030**
	(0.012)	(0.015)	(0.009)	(0.015)	(0.018)	(0.012)
Dual-Earner	-0.075***	-0.074***	-0.075***	-0.034*	-0.032*	-0.039**
	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.018)
Job Loss× Dual-Earner	0.020	0.021	0.017	-0.158***	-0.197***	-0.106**
	(0.044)	(0.056)	(0.032)	(0.060)	(0.073)	(0.046)
Sample Size	1632	1632	1632	1632	1632	1632
Ages	15 to 44	15 to 44	15 to 44	20 to 34	20 to 34	20 to 34
Job Loss Sub-Population	All	Female	Male	All	Female	Male

See notes from Table 2.5. Outcome is the divorce rate. * p<0.10, ** p<0.05, *** p<0.01.

Table B.4: Main Results, controlling for Divorce

	(1)	(2)	(3)	(4)	(5)	(6)
	Married Fertility Rates($t+1$)					
Panel A. County-Year Panel Data, Impact of Job Losses due to a Mass Layoff Event to Dual-Earner Married Couples on Married Fertility Rates						
Job Loss	-5.443	-7.936	-3.935	-17.355	-23.460*	-12.890
	(4.901)	(6.062)	(3.987)	(10.761)	(13.362)	(8.578)
Dual-Earner	-37.208	-39.629	-35.336	-28.837	-33.964	-23.593
	(29.222)	(29.622)	(28.791)	(41.605)	(42.853)	(40.965)
Job Loss× Dual-Earner	20.072	31.322	14.073	71.987*	102.875*	51.931
	(17.457)	(22.206)	(14.057)	(41.539)	(52.292)	(32.867)
Divorce Rate	-34.573	-34.902	-34.859	-32.099	-31.531	-33.449
	(42.958)	(42.778)	(43.025)	(88.256)	(88.207)	(88.569)
Sample Size	4492	4492	4492	4492	4492	4492
Panel B. State-Quarter Panel Data, Impact of Job Losses due to a Mass Layoff Event to Dual-Earner Married Couples on Married Fertility Rates						
	Married Fertility Rates($t+4$)					
Job Loss	-2.097	-5.599	-1.464	-10.78	-12.137	-9.075
	(2.485)	(3.162)	(1.785)	(4.673)	(5.689)	(3.332)
Dual-Earner	-6.219	-8.403	-5.270*	-4.341	-5.754	-2.798
	(2.966)	(3.050)	(2.846)	(5.257)	(5.418)	(5.026)
Job Loss× Dual-Earner	6.285	24.69	1.965	41.205	58.468	29.130
	(8.710)	(11.585)	(6.111)	(16.896)	(22.087)	(11.973)
Divorce Rate	-6.378	-7.055	-6.378	-7.038	-7.338	-7.116
	(4.745)	(4.706)	(4.754)	(8.778)	(8.821)	(8.747)
Sample Size	1632	1632	1632	1632	1632	1632
Ages	15 to 44	15 to 44	15 to 44	20 to 34	20 to 34	20 to 34
Job Loss Sub-Population	All	Female	Male	All	Female	Male

See notes from Tables 2.4 and 2.5 * p<0.10, ** p<0.05, *** p<0.01.

B.2 Data Appendix

I have explored the possibility of using several different datasets that measure marital status, labor force participation rates, information regarding job loss (preferably measures of layoffs due to economic reasons), and fertility decisions in the household.

One option for a microdata set that measures these variables is the American Community Survey Data (ACS). At the micro level, I see births in the last year and current employment status concurrently, therefore it is not possible to look at the impact of an individual's job loss on fertility decisions in the next period. I could use the measure of births at the individual level and use the measure of job loss due to mass layoff events at the county level used in this paper. However, the measure of births in the ACS is "births in the last year" so timing is poor. In addition, the ACS data does not publicly identify the county, only public use microdata areas (PUMAS), which do not directly correspond to counties. Therefore the analysis would be by state-year, which is worse timing and geographic locality than I am able to measure in the current paper.

In terms of longitudinal datasets that would let me measure the impact of job loss on fertility in the next period, I have explored the possibility of using the Panel Study of Income Dynamics (PSID), The National Longitudinal Survey of Youth 1997 panel (NLSY1997), and the Survey of Income and Program Participation (SIPP). The sample sizes of the relevant populations are too small in the PSID (Stevens, 1997) and NLSY for the population of interest. For the SIPP, the four year panels do not line up quite right to campture job loss during the Great Recession (2004, 2008).

One issue with the PSID is that it does not distinguish between whether an individual is fired from a job, which is likely to be endogenous to individual characteristics, and a layoff, and the timing of displacement is imprecise. The questionnaire asks why a respondent's last job ended if the respondent started a new job in the last year or if the respondent is

not currently working. Therefore, if a respondent gets laid off, then gets a new job, then leaves that job for a new one, the survey is not going to capture the layoff. Given that work trajectories ar more unstable after layoff events, this trajectory is likely. This problem with the PSID became more prevalent after 1998, when the survey began to be given every other year, making it more likely to miss intervening job layoffs (Lindo, 2010). Finally, there is evidence that women's displacement is endogenous in this survey. Lindo (2010) finds that there is an increase in women's work just before a displacement occurs. He notes that a fix for this would be to restrict the sample to women with long job tenures, but once this restriction is made, sample sizes ar too small to estimate the impact of job loss on fertility decisions.

Not only are the sample sizes in the NLSY1997 small[1], but also the ages of individuals for whom you could estimate the impact of job loss on fertility decisions is quite narrow. The NLSY1997 cohort was born between 1980 and 1984, so that respondents would be aged 23 to 27 at the beginning of the Great Recession. This is a specific cohort, which would limit how generalizable the estimates of the impact of job loss on fertility would be.

[1]There are 34 married dual-earner couples who experience job loss during 2007, three of whom have a child in a subsequent period

Appendix C

Chapter 3 Appendix

Table C.1: Analysis Data Samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample			Biological Test Sample		
Worksite Type	**Baseline**	**Endline**	**Panel**	**Baseline**	**Endline**	**Panel**
Panel A. Female Sex Worker Data						
Malang						
Formal	373	116	84	269	76	53
Informal	34	30	20	0	11	0
Pasuruan and Batu						
Formal	80	42	32	62	39	21
Informal	18	10	8	2	0	0
Total, formal worksites	453	158	116	331	115	74
Total, all worksites	505	198	144	333	126	74
Panel B. Client Data						
Malang						
Formal	220	193				
Informal	23	54				
Pasuruan and Batu						
Formal	44	35				
Informal	13	11				
TOTAL	300	293				

This table reports sample sizes of FSWs (Panel A) and clients (Panel B) at each worksite at baseline and at endline,the sample size of FSWs used in the panel sample. An FSW is included in the panel sample if she participated in an endline survey and indicated that she was still participating in sex work. See Table C.6 for a full explanation of the FSW follow-up samples. The analysis sample of FSWs at endline includes endline surveys conducted in person and by phone. Note that all baseline surveys were conducted in person. For the FSWs, we also report the sample sizes of the women for which we were able to conduct biological tests at baseline and endline, as well as the sample size of FSWs for whom we were able to obtain a biological test result at baseline and at endline.

Table C.2: Baseline Characteristics, including comparison with informal worksites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable	**Crim.**	**Non-Crim.**	**Diff.**	**Non-Crim.**	**Diff.**	**Non-Crim.**	**Diff.**
		All	**P-Value**	**Formal Only**	**P-Value**	**Informal Only**	**P-Value**
Panel A. Full Sample							
Married	0.180	0.197		0.138		0.288	
	(0.020)	(0.035)	0.659	(0.039)	0.366	(0.063)	0.063
Divorced or Widowed	0.777	0.727		0.738		0.712	
	(0.022)	(0.039)	0.243	(0.050)	0.442	(0.063)	0.291
Never Married	0.043	0.076		0.125		0.00	
	(0.011)	(0.023)	0.143	(0.037)	0.0041	(0.00)	0.129
Years of Education	5.82	5.72		5.86		5.52	
	(0.154)	(0.296)	0.766	(0.402)	0.912	(0.430)	0.497
Age	34.5	35.8		33.5		39.3	
	(0.381)	(0.691)	0.098	(0.817)	0.257	(1.06)	0.000
Children	0.906	0.864		0.850		0.885	
	(0.015)	(0.030)	0.171	(0.040)	0.136	(0.045)	0.623
Years at Location	2.09	3.19		2.42		4.38	
	(0.125)	(0.370)	0.000	(0.410)	0.312	(0.667)	0.00
Discount Factor	0.388	0.373		0.371		0.378	
	(0.024)	(0.040)	0.758	(0.051)	0.764	(0.064)	0.879
Risk Tolerance	0.380	0.323		0.354		0.275	
	(0.025)	(0.041)	0.247	(0.054)	0.670	(0.063)	0.143
Sample Size	373	132		80		52	
Panel B. Panel Sample							
Married	0.285	0.167		0.125		0.214	
	(0.050)	(0.049)	0.099	(0.059)	0.072	(0.079)	0.464
Divorced or Widowed	0.655	0.767		0.750		0.786	
	(0.052)	(0.055)	0.150	(0.078)	0.340	(0.079)	0.199
Never Married	0.059	0.067		0.125		0.00	
	(0.026)	(0.032)	0.862	(0.059)	0.242	(0.00)	0.190
Years of Education	6.08	4.72		4.88		4.54	
	(0.320)	(0.434)	0.015	(0.673)	0.071	(0.536)	0.016
Age	33.8	38.4		35.3		41.9	
	(0.792)	(1.01)	0.0004	(1.21)	0.315	(1.41)	0.000
Children	0.929	0.917		0.906		0.929	
	(0.028)	(0.036)	0.793	(0.052)	0.691	(0.050)	1.00
Years at Location	1.99	3.23		2.69		3.86	
	(0.242)	(0.434)	0.008	(0.610)	0.200	(0.606)	0.001
Discount Factor	0.338	0.374		0.381		0.366	
	(0.049)	(0.060)	0.642	(0.083)	0.650	(0.088)	0.779
Risk Tolerance	0.429	0.339		0.355		0.321	
	(0.054)	(0.062)	0.283	(0.087)	0.480	(0.090)	0.321
Sample Size	84	60		32		28	

See notes for Table 3.2. Column (6) shows the baseline means for for the population of FSWs at the informal worksites only. Column (7) shows the p-value from a test of the difference in means between the FSWs at the criminalized worksites and at the informal non-criminalized worksites.

Table C.3: Impact of criminalization on self-reports on FSW health and condom use, all worksites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Female Sex Worker Data								
		Self Reports			**Test**	**Health**	**Condom**	**ln(Condom**	**No**
	STI	**Disch**	**Sore**	**Swell**	**Positive**	**Exam**	**Access**	**Price)**	**Condom**
Panel A. Whole Sample									
Crim×Endline	0.057	0.087**	0.038	0.033	0.273***	-0.136	-0.370	1.35***	0.125*
	(0.040)	(0.032)	(0.049)	(0.037)	(0.032)	(0.096)	(0.216)	(0.416)	(0.076)
Conventional p-value	0.173	0.016	0.451	0.386	0.000	0.176	0.106	0.006	0.098
Wild cluster bootstrap-t p-value	0.250	0.040	0.490	0.400	0.010	0.202	0.324	0.000	0.048
Endline	-0.058**	-0.075***	-0.153***	-0.022	-0.055**	0.067	-0.006	-1.08**	-0.146**
	(0.023)	(0.024)	(0.036)	(0.013)	(0.021)	(0.082)	(0.048)	(0.372)	(0.066)
Sample Size	703	7103	703	703	459	703	703	703	1928
Baseline Mean	0.056	0.035	0.220	0.054	0.465	0.916	0.714	930 IDR	0.146
Panel B. Panel Sample									
Crim×Endline	0.077	0.068*	0.065	0.050	0.208***	-0.098	-0.385*	0.649	0.125*
	(0.054)	(0.032)	(0.059)	(0.029)	(0.053)	(0.113)	(0.201)	(0.620)	(0.071)
Conventional p-value	0.177	0.061	0.293	0.111	0.008	0.402	0.080	0.316	0.079
Wild cluster bootstrap-t p-value	0.256	0.146	0.218	0.098	0.031	0.432	0.248	0.362	0.052
Endline	-0.080	-0.070**	-0.149***	-0.029	-0.000	-0.004	-0.040	-0.373	-0.145**
	(0.046)	(0.023)	(0.044)	(0.018)	(0.000)	(0.100)	(0.042)	(0.532)	(0.060)
Sample Size	288	288	288	288	148	288	288	288	779
Baseline Means	0.054	0.032	0.204	0.054	0.340	0.935	0.774	885 IDR	0.170
Worksite Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	N
Individual Fixed Effects	N	N	N	N	N	N	N	N	Y
Unit of Observation	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Individual	Transaction

See notes for Table 3.4. The control group is comprised of all FSWs at non-criminalized worksites, including formal and informal worksites in Pasuruan and Batu. * p<0.10, ** p<0.05, *** p<0.01

Table C.4: Impact of Criminalization on Worksite Operations, all worksites

	(1)	(2)	(3)	(4)
	Female Sex Worker Data			
	No. of Clients	**No. of Trans.**	**No. of Hrs Worked**	**ln(Wkly Earnings)**
Panel A. Whole Sample				
Crim×Endline	2.50	2.77	-6.89	0.493
	(2.27)	(2.01)	(8.45)	(0.586)
Conventional p-value	0.287	0.187	0.427	0.412
Wild cluster bootstrap-t p-value	0.378	0.286	0.490	0.468
Endline	-3.31*	-3.33	-3.91	0.148
	(1.68)	(1.67)	(6.21)	(0.482)
Sample Size	703	703	703	703
Baseline Mean	8.30	8.34	57.2	735,040 IDR
Panel B. Panel Sample				
Crim×Endline	1.25	1.68	-12.6	-0.573
	(1.43)	(1.21)	(8.90)	(0.738)
Conventional p-value	0.401	0.190	0.183	0.453
Wild cluster bootstrap-t p-value	0.482	0.276	0.184	0.434
Endline	-3.57**	-3.63**	-2.41	0.576
	(1.20)	(1.27)	(7.69)	(0.583)
Sample Size	288	288	288	288
Baseline Mean	10.1	10.2	62.1	960,435 IDR
Worksite Fixed Effects	Y	Y	Y	Y

See notes for Table 3.5. The control group is comprised of all FSWs at non-criminalized worksites, including formal and informal worksites in Pasuruan and Batu. * p<0.10, ** p<0.05, *** p<0.01

Table C.5: Impact of Criminalization on Transaction, Sex Worker, and Client Characteristics, all worksites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Transaction Characteristics			**Client Characteristics**					**FSW Characteristics**					
	Vaginal	**Anal**	**ln(Price)**	**Reg**	**Clean**	**Attract**	**Outsider**	**Rich**	**Married**	**Educ**	**Age**	**Patience**	**Risk**	**Child**
Panel A. Reported by Female Sex Workers														
Crim×Endline	0.001	-0.000	-0.044	-0.012	-0.014	0.047	0.123	0.006	-0.086	0.755*	-1.34	-0.003	0.058	0.003
	(0.013)	(0.000)	(0.084)	(0.090)	(0.094)	(0.067)	(0.083)	(0.054)	(0.116)	(0.369)	(0.785)	(0.077)	(0.103)	(0.040)
Conventional p-value									0.467	0.058	0.107	0.974	0.582	0.946
Wild cluster bootstrap-t p-value									0.612	0.096	0.254	0.952	0.566	0.998
Endline	0.007	0.000	0.249***	0.018	0.007	-0.023	-0.074	-0.017	0.034	-0.338	1.65**	-0.035	0.020	0.027
	(0.008)	(0.000)	(0.059)	(0.069)	(0.076)	(0.057)	(0.073)	(0.037)	(0.073)	(0.354)	(0.755)	(0.076)	(0.092)	(0.033)
Sample Size	1956	1956	1953	1956	1956	1956	1956	1956	703	703	703	703	695	703
Panel B. Reported by Female Sex Workers, Panel Sample														
Crim×Endline	0.001		-0.044	-0.012	-0.014	0.047	0.123	0.006	-0.198**	-0.095	0.012	0.068	0.031	0.029
	(0.012)		(0.079)	(0.084)	(0.089)	(0.063)	(0.078)	(0.050)	(0.072)	(0.110)	(0.240)	(0.101)	(0.166)	(0.030)
Conventional p-value									0.018	0.403	0.115	0.514	0.854	0.361
Wild cluster bootstrap-t p-value									0.034	0.360	0.928	0.548	0.916	0.624
el	0.007		0.249***	0.018	0.007	-0.023	-0.074	-0.017	0.067	0.333***	1.25***	-0.025	0.021	-0.017
	(0.008)		(0.055)	(0.065)	(0.072)	(0.053)	(0.066)	(0.035)	(0.067)	(0.095)	(0.209)	(0.095)	(0.137)	(0.017)
Sample Size	786		786	786	786	786	786	786	288	288	288	288	284	288
Worksite Fixed Effects	N	N	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y
Individual Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N	N	N

See notes for Table 3.6. The control group is comprised of all FSWs at non-criminalized worksites, including formal and informal worksites in Pasuruan and Batu. * p<0.10, ** p<0.05, *** p<0.01

Table C.6: Following-up with the Female Sex Workers from the Baseline Sample

	(1)	(2)	(3)	(4)
	Criminalized Worksites	**Non-Criminalized Formal Worksites**	**Non-Criminalized Informal Worksites**	**TOTAL**
Panel A. Number of FSWs in Baseline and Follow-up Surveys†				
Baseline Sample	373 (%)	80 (%)	52 (%)	505 (%)
Follow-up Sample	256 (68.6)	57 (71.3)	35 (67.3)	348 (68.9)
Panel B. Occupations of FSWs at time of Follow-up††				
Follow-up Sample	256 (%)	57 (%)	35 (%)	348 (%)
Still in sex work	143 (55.9)	45 (78.9)	30 (85.7)	218 (62.6)
Work in Warung	5 (1.95)	0 (0.00)	0 (0.00)	5 (1.44)
Own small business	16 (6.25)	1 (1.75)	1 (2.86)	18 (5.17)
Work as laborer	26 (10.2)	3 (5.26)	1 (2.86)	30 (8.62)
Other or unspecified	34 (13.2)	1 (1.75)	3 (8.57)	38 (10.9)
Not working	32 (12.5)	7 (12.3)	0 (0.00)	39 (11.2)
Panel C. Locations of FSWs if no longer in Sex Work at Follow-up††				
No longer in sex work	113 (%)	12 (%)	5 (%)	130 (%)
Did not move	7 (6.19)	2 (16.7)	4 (80.0)	13 (10.0)
Moved within same city (not home)	13 (11.5)	0 (0.00)	0 (0.00)	13 (10.0)
Returned home	69 (61.1)	9 (75.0)	0 (0.00)	78 (60.0)
Moved somewhere else	24 (21.2)	1 (8.33)	1 (20.0)	26 (20.0)

Panel A reports the sample sizes of the FSWs from baseline who we were able to recontact at endline (as well as the rate of recontact from baseline) for criminalized worksites, non-criminalized formal worksites, and non-criminalized informal worksites. The "Baseline Sample" is the total number of FSWs that were interviewed at baseline. The baseline sample represents the universe of FSWs at the study worksites at the time of the baseline survey. The "Follow-up Sample" is the total number of FSWs from the baseline sample that we were able to obtain information about at endline either though (1) surveys directly with the respondent or (2) surveys with an informant who was able to answer basic questions about where the original respondent is now and what she is doing. Panel B reports the current occupation of FSWs from baseline (1) who we were able to recontact at endline in person, (2) who we were able to recontact at endline by telephone, or (3) for whom we were able to interview an informant. Panel C. Reports the current location of FSWs from baseline if they report that they are no longer in sex work. † The percentage in parentheses indicates the percent of the baseline survey sample that was re-surveyed in each follow-up subsample. The percentages in this panel do not add up to 100 percent. †† The percentage in parentheses is the percent of the full follow-up sample that is partaking in each occupation in Panel A. and the percent of FSWs who are no longer in sex work and moved. The percentages in Panels B. and C. add up to 100 percent.

Table C.7: Determinants of not appearing in our panel sample

	(1)	(2)	(3)
	Leave sample or leave SW		**Leave Health sample**
Crim×Positive			0.003
			(0.024)
Crim×Married	-0.256	-0.248	
	(0.146)	(0.120)	
Crim×Education	-0.023	-0.024	
	(0.019)	(0.015)	
Crim×Age	0.010	0.009**	
	(0.006)	(0.004)	
Crim×Children	0.035	0.089	
	(0.101)	(0.102)	
Crim×Years	-0.007	-0.020	
	(0.015)	(0.014)	
Crim×Discount	0.103	0.041	
	(0.061)	(0.071)	
Crim×Risk	0.084	0.046	
	(0.134)	(0.101)	
Positive			0.043***
			(0.006)
Married	0.132	0.121	
	(0.107)	(0.071)	
Education	0.022	0.023	
	(0.018)	(0.014)	
Age	-0.006	-0.006**	
	(0.005)	(0.003)	
Children	-0.111	-0.166	
	(0.096)	(0.097)	
Years at Worksite	0.006	0.019*	
	(0.011)	(0.009)	
Discount Factor	-0.060	0.003	
	(0.046)	(0.059)	
Risk Tolerance	-0.110	-0.072	
	(0.127)	(0.092)	
Sample Size	453	505	333
Formal Worksites Only	Y	N	Y
Worksite Fixed Effects	Y	Y	Y

The purpose of this table is to estimate whether FSWs with particular characteristics were less likely to appear in our endline sample, and whether these characteristics varied between the FSWs at the criminalized worksites and the non-criminalized worksites. The sample of FSWs in columns (1) and (2) of this table is all FSWs interviewed at baseline. The outcome is an indicator equal to 1 if the baseline FSW was not re-interviewed in-person at endline or indicated that she was no longer engaged in sex work. Column (1) includes only FSWs at the formal worksites in Malang (where sex work was criminalized) and Pasuruan. Column (2) includes FSWs at all formal and informal worksites in Malang, Pasuruan, and Batu. The outcome for the regression reported in column (3) table is an indicator equal to 1 if an FSW who was tested at baseline was not tested at endline. The sample is all FSWs who were tested at baseline. Standard errors are presented in parentheses and are clustered at the worksite level. Worksite fixed effects are included in all regressions.