The Economics of Women’s Health Risks

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Abstract

This dissertation studies the health and well-being of women with a specific focus on reproductive health and domestic violence. In Chapter 1, “Safer Sex? The Effect of AIDS Risk on Birth Rates,” I study the effect of increases in the risk of sexually transmitted infection (STI) and resulting STI avoidance behaviors on birth rates. The emergence of AIDS in the 1980s dramatically increased the cost of contracting a sexually transmitted infection (STI). Prior research shows that people responded to the AIDS epidemic by switching to sexual behaviors and contraceptive methods with lower likelihood of AIDS transmission. These behavioral adjustments also affect the likelihood of pregnancy and the incidence of other STIs. This paper provides the first evidence that the AIDS epidemic in the United States increased the birth rate and the abortion rate, and decreased the gonorrhea rate. I show that births among adult women increased on average by 0.5 births per 1,000 women per year, for a total of 330,000 additional births between 1981 and 2001 due to AIDS avoidance behaviors. My analysis suggests that the overall estimates are driven by women who avoid AIDS by shifting to monogamous relationships.

The second chapter, joint with Amalia Miller and Carmit Segal, looks at the effect of the COVID-19 pandemic on domestic violence in Los Angeles. Around the world, policymakers and news reports have warned that domestic violence (DV) could increase as a result of the COVID-19 pandemic and the attendant restrictions on individual mobility and commercial activity. However, both anecdotal accounts and academic research have found inconsistent effects of the pandemic on DV across measures and cities. We use high-frequency, real-time data from Los Angeles on 911 calls, crime incidents, arrests, and calls to a DV hotline to study the effects of COVID-19 shutdowns on DV. We find conflicting effects within that single city and even across measures from the same source. We also find varying effects between the initial shutdown period and the one following the initial re-opening. DV calls to police and to the hotline increased during the initial shutdown, but DV crimes decreased. The period following re-opening showed a continued decrease in DV crimes, as well as decreases in arrests for those crimes and calls to the police and to the hotline. Our results highlight the heterogeneous effects of the pandemic across DV measures and caution against relying on a single data type or source.

The third chapter is joint with Rebecca Brough and studies the effect of abortion counseling laws on birth rates. Nearly two-thirds of states have abortion counseling laws requiring that women receive and acknowledge state-mandated information prior to giving their informed consent for abortion. The mandated information varies widely across states, and may include scientifically inaccurate statements regarding the risks of abortion, illustrations of fetuses, or ultrasound requirements. Despite the ubiquity of these laws, little is known about the effect of counseling laws on women’s abortion decision and birth rates. Using a novel dataset of state-level abortion counseling laws, we find that counseling laws that contain ultrasound requirements increase the birth rate to white women by as much as 1 birth per 1,000 women.

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Chapter 1: Safer Sex?
The Effect of AIDS Risk on Birth Rates

Melissa K. Spencer*

Abstract

The emergence of AIDS in the 1980s dramatically increased the cost of contracting a sexually transmitted infection (STI). Prior research shows that people responded to the AIDS epidemic by switching to sexual behaviors and contraceptive methods with lower likelihood of AIDS transmission. These behavioral adjustments also affect the likelihood of pregnancy and the incidence of other STIs. This paper provides the first evidence that the AIDS epidemic in the United States increased the birth rate and the abortion rate, and decreased the gonorrhea rate. I show that births among adult women increased on average by 0.5 births per 1,000 women per year, for a total of 330,000 additional births between 1981 and 2001 due to AIDS avoidance behaviors. My analysis suggests that the overall estimates are driven by women who avoid AIDS by shifting to monogamous relationships.

Keywords: HIV/AIDS, STI, Fertility, Birth Rate, Epidemic, Infectious Disease, Family Structure

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I. Introduction

Women face two potential health risks from sexual activity: acquiring a sexually transmitted infection (STI) and becoming pregnant. They can mitigate these risks through safer choices regarding sexual behavior and contraceptive use. A key feature of these choices is that risk mitigation strategies entail trade-offs. While some choices, such as abstinence, reduce both STI and pregnancy risk, others decrease one risk but increase or leave the other unchanged. In particular, condoms and monogamy both reduce STI risk, but can potentially increase the likelihood of pregnancy. This is especially true if condoms are used to substitute for more reliable forms of contraception, such as oral contraceptives, or if women with only one sexual partner have unprotected sex more frequently.

I estimate the effect of increases in STI risk and resulting STI avoidance behaviors on birth rates. Because of the trade-offs between STI and pregnancy prevention strategies, the effects of increases in STI risk are theoretically ambiguous. Economic theory predicts that rational individuals will shift to "safer" sexual behaviors in response to increases in the cost of fertility (Becker and Lewis 1973; Willis 1973) and increases in the risk of STI (Posner 1993). When responding to STI risk, those safer choices could increase or decrease birth rates. Empirical studies have validated the importance of economic cost considerations in determining sexual behavior, contraceptive choices, and fertility outcomes (Michael and Willis 1973; Goldin and Katz 2002; Francis 2008; Kearney and Levine 2009; Bailey 2010; Shah 2013; Durrance 2013; Mulligan 2016), but the effect of increased STI risk on fertility in developed countries has not previously been examined.

Understanding the effects of increased STI risk is particularly important given current trends in public health. The rapid rise of drug-resistant gonorrhea in the United States threatens an STI-driven epidemic similar to the AIDS epidemic in the 1980s (US DHHS 2017; Bodie et al. 2019). The COVID pandemic of 2020 highlights both the costs and challenges associated with containing infectious disease. The relationship between STI risk, sexual behaviors, and birth rates is a critical determinant of public health responses to drug-resistant STIs. My research provides insights into how women might adjust their behavior in response to drug-resistant gonorrhea, and can inform public health interventions to combat the spread of disease.

I empirically examine the effect of STI avoidance on birth rates by exploiting variation in the spread of AIDS across U.S. cities in the 1980s and 1990s. The AIDS epidemic created a large and plausibly exogenous increase in the cost of contracting an STI. During this period, the spread of AIDS was largely driven by male same-sex contact and the average time between HIV infection and AIDS diagnosis was 10 years. Thus, within a city, the timing of AIDS arrival and the extent of the epidemic was unrelated to women's sexual behavior. I analyze the effect of local AIDS incidence on birth rates using a fixed effects specification with controls for city and year.

I find that local AIDS incidence has a positive and statistically significant effect on both birth rates and abortion rates. At the height of the AIDS epidemic in 1994, the birth rate for adult women increased by 1.5 percent due to AIDS avoidance behaviors. I find that the corresponding increase in pregnancy rates was even larger, but approximately 44 percent of pregnancies resulting from AIDS avoidance behaviors were aborted. Results are robust to the inclusion of city-specific year trends and controls for drug use, prostitution, poverty, income, education, and sex ratios. The magnitude of estimated effects is in line with the literature: I estimate that births increased on average by 0.5 births per 1,000 women per year. Similar in magnitude, Kearney and Levine (2009) find that expanded family planning coverage decreased the birth rate by 1.5 births per 1,000 women.

I test whether the increase in birth rates is due to an increase in risky sex among women. I evaluate this alternative hypothesis using data on gonorrhea incidence in women. If women are having more unprotected sex, we would expect increases in both birth rates and gonorrhea. In contrast, I find that AIDS risk leads to decreases in gonorrhea incidence. This result shows that women are successfully adopting behaviors that decrease their likelihood of contracting AIDS and other STIs, but at the expense of heightened pregnancy likelihood.

There are two AIDS avoidance behaviors that could result in a decrease in gonorrhea but an increase in births: Women could switch from effective prescription contraceptives to condoms or limit their number of sexual partners.

I provide evidence that the increase in births is due to women entering monogamous partnerships to avoid AIDS. Using birth certificate information, I find that the increase in births is driven by an increase in births for unmarried, cohabiting women. There is no effect of AIDS on births for single women. I supplement these results with survey data to argue that the increase in births is driven by women choosing to have only one sexual partner to protect themselves from AIDS. Survey data shows that 16 percent of unmarried women decided to stop having sex with more than one man (Mosher and Pratt 1993). I estimate that if 16 percent of women switched from multiple partners to one partner, the birth rate would increase by 2 births per 1,000 women. This is higher than my estimate of a 0.5-birth increase, but is consistent with heterogeneous responses in the population as well as women who previously had multiple partners taking more steps to prevent pregnancy even in monogamous partnerships.

I examine whether the effect of AIDS risk on birth rates varies across demographic groups, and find that the positive effect of AIDS risk on births is driven by women aged 30-44 years and by white women. I find no effect of AIDS risk on births to Black women or women aged 20-29 years. I provide a number of possible explanations for this heterogeneity, including differences in abortion rates, AIDS prevention knowledge, and the availability of potential partners.

By documenting an increase in birth rates, this paper contributes to the economics literature on both the effects of AIDS and STIs and the determinants of fertility. There are multiple studies examining the effects of the AIDS epidemic on fertility in countries in sub-Saharan Africa, with mixed results (Fortson 2009; Magadi and Agwanda 2010; Kalemli-Ozcan and Turan 2011; Karlsson and Pichler 2015; Chin and Wilson 2018). Within the STI literature, this work is closest to studies such as Ahituv, Hotz, and Philipson (1996), Lakdawalla, Sood, and Goldman (2006), Francis (2008), Thirumurthy et al. (2012), Shah (2013), Fortin (2015), and Greenwood et al. (2019), which examine behavioral changes in response to STI risk. In the fertility literature, this paper is closest to studies that analyze the effects of changes in the costs of pregnancy prevention on fertility outcomes (Levine 2000; Kearney and Levine 2009; Bailey, Hershbein, and Miller 2012; Bailey 2013).

Similar to this work are studies that examine the spillovers between fertility and STI risk, for example, Sen (2003), Klick and Stratmann (2008), Durrance (2013), Mulligan (2016), Buckles and Hungerman (2018), Mallatt (2019), and Willage (2020). In comparison with this paper, those studies focus on the reverse relationship by studying the effect of changes in the cost of pregnancy prevention on STI rates. I find that women adopt STI avoidance behaviors at the cost of increased pregnancy likelihood, and that as a result, STI risk has a positive effect on birth rates.

II. The AIDS Epidemic in the United States

The first cases of what would come to be known as Acquired Immunodeficiency Syndrome (AIDS) were identified in the United States in June of 1981. During the first year of the epidemic, the Centers for Disease Control and Prevention (CDC) tracked cases of rare pneumonia, cancer, and other opportunistic infections that occurred predominately in young men with same-sex partners in California and New York (Shilts 2007). By July of 1982, the CDC had confirmed cases in hemophiliacs, intravenous (IV) drug users, and infants with the disease. Shortly thereafter, there were at least two documented cases of women who were exposed to the disease via opposite-sex contact (Heywood and Curran 1988).

AIDS is the result of advanced infection with human immunodeficiency virus (HIV), which is found in semen, blood, vaginal and anal fluids, and breast milk. The virus can be transmitted via sexual contact or shared needles, and from mother to child during pregnancy, childbirth, and breastfeeding (US DHHS 2020). Despite early misconceptions among the general public that AIDS was confined to men who have sex with men, scientists had identified the exposure categories and risk factors for AIDS infection as early as 1983 and were concerned that AIDS would spread quickly via opposite-sex contact. Indeed, the early concern that AIDS would reach epidemic levels in ostensibly low-risk groups influenced public health efforts and knowledge of the disease. Early responses to AIDS emphasized the idea of "universal vulnerability," or that everyone is at risk of contracting AIDS (De Cock, Jaffe, and Curran 2011).

Evidence suggests that public health fears of AIDS spreading rapidly via opposite-sex contact influenced knowledge and behavior regarding AIDS. In 1987, *The New York Times* ran an article describing fears of AIDS infection among women. At one clinic in New York, over 40 percent of those requesting HIV tests were women considered to be low risk (i.e., no history of drug use, sex with drug users, or sex with men who have sex with men), none of whom were found to have AIDS (Sullivan 1987). Further contributing to fear of AIDS was the volatile nature of the disease's spread across the United States (Mann 1992). The sudden and unstable spread of AIDS led to a large number of epidemiological studies seeking to track and predict the prevalence of the disease (Taylor 1989; Lam, Fan, and Liu 1996; Steinberg and Fleming 2000). The epidemiology literature identifies the following pattern: Initially, AIDS spread from city to city, with cases concentrated among men with same-sex partners and IV drug users. As AIDS became more prevalent in a city, it began to spread outward from urban areas. Further contributing to the unpredictable nature of the epidemic was the lengthy incubation period between HIV infection and the presentation of AIDS symptoms. In the 1980s and 1990s, the average time between infection with HIV and an AIDS diagnosis was 10 years (Osmond 1998). As a result, outbreaks in cities were not being driven by current behaviors, but behaviors from as much as 10 years prior.

The AIDS epidemic is ideal for studying the relationship between STI risk and birth rates for two reasons. First, by focusing on cities, I can exploit the spread of AIDS, which was plausibly exogenous with respect to birth rates; As noted above, AIDS spread from city to city via same-sex male contact that had occurred years prior. Second, the emergence of AIDS created an economically large increase in the cost of contracting an STI. Results from the 1990 National Survey of Family Growth (NSFG) show that 22 percent of women reported changing their sexual behavior or using condoms to avoid AIDS (Mosher and Pratt 1993). Given that a substantial share of women changed their behavior, it is reasonable to expect that birth rates were affected by the AIDS epidemic.

III. Expected Effect of AIDS Risk on Births

It is unclear how women would respond to an increase in AIDS risk given the concurrent risk of unintended pregnancy. Condoms are the only method of contraception that can protect from AIDS transmission, but are not very effective in preventing pregnancy.[1] Condoms have a typical use failure rate of 18 percent, meaning that 18 out of 100 women will experience unintended pregnancy within the first year of using condoms as their primary contraceptive method. Prescription contraceptive methods such as the Pill and the IUD are more effective in preventing pregnancy: The Pill has a typical use failure rate of 9 percent and the IUD has a typical use failure rate as low as 0.2 percent (Trussell 2004). If many women mitigated the risk of AIDS by switching from more effective methods to condoms, then the birth rate in the population would increase. If women switched from not using contraceptives to using condoms, or adopted condoms in addition to their current method, the birth rate would decrease.

It is also possible that women responded to AIDS risk by adjusting their sexual behavior. At the extensive margin, women can choose to abstain from any sexual activity. At the intensive margin, they can limit their number of sexual partners or choose low-risk partners (i.e., those who do not use IV drugs and do not have concurrent partners). In this case the birth rate would likely increase, since there is less incentive to use consistent contraception with a low-risk partner and the frequency of sexual activity may increase with one partner.

1. In recent years, pre-exposure prophylaxis (PrEP) has offered another method of protection from AIDS transmission. However, in the 1980s and 1990s condoms were the only technological option.

Furthermore, women may be more likely to continue a pregnancy with a low-risk partner or a sole, committed partner.

I model sexual decision-making from the woman's perspective. This is a simplification, since consensual sexual contact requires bilateral decision-making. There are three reasons to focus on the woman's decisions. First, the choice set of overlapping AIDS avoidance behaviors and pregnancy behaviors differs for men. Specifically, prescription contraceptives such as the Pill and the IUD are in the woman's choice set, but not directly in the man's choice set. Similarly, we might think that the costs and benefits associated with male condoms differ for men and women. Second, the probability of biological HIV transmission via heterosexual contact differs for men and women: Women are much more likely than men to contract HIV from heterosexual contact (Nicolosi et al. 1994). Third, it is easier to draw inferences about individual behavior from birth rates when focusing on women; one birth closely approximates one mother. The same is not true for men, who could have multiple children in the same time period with different women.

We expect that there are heterogeneous responses to the AIDS epidemic in the population: Women have different preferences over pregnancy, sexual behavior, and contraception. Using data on birth rates and AIDS risk at the city level, it is only possible to determine the average effect of AIDS risk on birth rates. The average effect depends on both which risk mitigation strategy is most prevalent in the population and how much each strategy affects pregnancy and birth probabilities. For example, 5 percent of women choosing to abstain from sexual activity would have a much larger effect on birth rates than 5 percent of women choosing to add condoms to their current method of contraception.

An ideal data set for this analysis would include the sexual behavior and contraceptive choices of individual women over time. Unfortunately, such a data set does not exist for this time period.[2] However, we can still gain important insights by estimating the average effect.

2. The NLSY79 would not work for this analysis for several reasons: (1) it lacks information on number of sexual partners, (2) responses to questions about sex and contraception are subject to substantial refusals and reporting bias, and (3) the sample size of women who consistently respond to sexual behavior questions is very small.

For example, a positive average effect indicates that some women are switching to condoms or limiting the number of sexual partners. A negative average effect indicates that some women are abstaining or adopting condoms in addition to other contraceptive methods.[3] Additional analyses can then be used to disentangle possible mechanisms and to infer the share of women who adjusted their behavior accordingly.

IV. Data Sources and Sample Construction

I estimate the theorized relationship between birth rates and AIDS risk using data from the CDC. The AIDS Public Information Data Set (APIDS) contains information on the timing of AIDS diagnosis and demographics of the patient (US DHHS 2005). All data are aggregated within Metropolitan Statistical Areas (MSAs) and are available to the public via CDC WONDER for MSAs with more than 500,000 people.

Since an individual's risk of contracting AIDS is unobservable, I use AIDS incidence in the MSA of residence as a proxy for average AIDS risk in the population. AIDS incidence is defined as the number of new AIDS diagnoses per year per 100,000 people. I further define *AIDS risk* for women of childbearing age as the AIDS incidence in women aged 20-44 in the previous year. This definition approximates both risk at the time of conception—as opposed to the time of birth—and the incidence of AIDS being spread by means other than same-sex contact.[4] To demonstrate that this definition is a reasonable proxy for AIDS risk, I use data from National Health Interview Surveys and ABC News polls that asked respondents to rate their chances of getting AIDS (US DHHS 1997, ABC News 1997). I regress respondents' perceived risk of getting AIDS on local AIDS incidence. Results are

3. One important caveat in using data on birth rates and STI incidence for this analysis is the problem of ecological inference that arises when using group averages rather than individual level data (King, Tanner, and Rosen 2004). I discuss this problem in more detail as it relates to this paper in Appendix C: The Ecological Inference Problem.

4. An alternative definition could use the previous year's AIDS incidence in men identifying as heterosexual. However, disclosing sexual identity is subject to reporting bias that may result in measurement error. As such, I choose to use AIDS incidence among women. See Table 3 for further discussion of different measures.

presented in Appendix Tables A1 and A2. I find that local AIDS incidence is predictive of perceived risk. For women respondents, only AIDS incidence in women (my definition of AIDS risk) is predictive of perceived risk.

Using APIDS data, I create a panel of AIDS risk across 102 MSAs from 1981 to 2001.[5] I merge this data with birth records obtained from the CDC's restricted-access Natality Detail file (US DHHS, 2002), which includes information on birth date and mother's county of residence, race, age, and marital status. I aggregate data by MSA-year to create a panel of birth rates from 1969 to 2001. Due to the very low incidence of AIDS in adolescent women during this period, I limit my sample to the population of adult women aged 20-44.[6] Birth rates are calculated as births per 1,000 women aged 20-44. The resulting panel of AIDS risk and birth rates contains 2,142 MSA-year observations across the years 1981 to 2001. Descriptive statistics for the panel are presented in Table 1. Figure 1 plots the difference in AIDS risk from 1981 to 2001 versus the difference in birth rates over the same period. This figure indicates a positive relationship between birth rates and AIDS risk. I empirically examine this relationship by exploiting within-MSA, over-time variation in AIDS risk.

V. Empirical Approach

The primary results presented in this paper come from the following fixed effects specification:

$$y_{m,t} = \beta_0 + \beta_1 Z_{m,t} + \gamma_m + \delta_t + \epsilon_{m,t}, \tag{1}$$

where $y_{m,t}$ is the birth rate in MSA m in year t, $Z_{m,t}$ is AIDS risk in the MSA, and γ_m and δ_t are MSA and year fixed effects, respectively.[7] β_1 is the coefficient of interest. In order to interpret β_1 as the causal effect of AIDS risk on birth rates, it must be that AIDS risk is independent of the error term conditional on MSA and year fixed effects. In other words,

5. APIDS data are available through 2002. However, I stop my panel at 2001 in accordance with the availability of a key control variable: The crack cocaine index. See the data appendix for further information.

6. For more information, see Appendix D: Births and AIDS Risk in Adolescent Women.

7. Note that in all empirical analyses, *AIDS risk* uses the definition given in Section 4 as the AIDS incidence in women aged 20-44 in the previous year.

MSA fixed effects must be sufficient to capture any systematic differences in the cross-section that could be driving trends in both AIDS risk and birth rates. I evaluate this identifying assumption using a series of empirical tests described below.[8]

I test the robustness of estimates from equation (1) to a number of alternative empirical specifications. First, I compare results from an unweighted and population-weighted version of equation (1). Differing coefficients between an unweighted and a weighted version could be evidence of model misspecification or heterogeneous effects (Solon, Haider, and Wooldridge 2015). Second, I add a variety of controls that may plausibly affect both AIDS risk and birth rates, including drug use, poverty, income, incarceration rates, prostitution, employment, sex ratios, and educational attainment. Third, I add MSA-specific linear trends to address concerns about within-MSA trends that could be affecting both AIDS risk and birth rates.

Additional robustness tests exploit characteristics specific to the setting of the AIDS epidemic. I show that the relationship between AIDS risk and birth rates is driven by AIDS diagnoses in women and heterosexual men, and that birth rates are unaffected by AIDS diagnoses in homosexual and bisexual men. I analyze the effect of AIDS risk on gonorrhea incidence to show that estimates are not driven by unobserved sexual behavior. I also analyze the effect of AIDS risk on abortion rates to show that changes in birth rates are not driven by changes in abortion likelihood. Finally, I exploit the 10-year average latency period between HIV infection and AIDS diagnosis to test for a spurious relationship.

VI. Results

A. Effect of AIDS Risk on Birth Rates

Empirical results for the fixed effects specification from equation (1) are presented in Table 2, column (1). For this specification and all subsequent regressions, I estimate robust standard errors that are clustered at the MSA level. I estimate that every additional AIDS

8. Recent advances in econometrics highlight potential problems with the two-way fixed effects approach. I address these concerns and present related robustness checks in Appendix E.

diagnosis in women 20-44 per 100,000 women led to an increase in births of 0.057 per 1,000 women. Table 2, column (2) presents the results from a weighted version of equation (2). I weight regressions by the number of women aged 20-44 in the MSA each year. The estimated coefficient does not change when using a weighted specification.

Despite the inclusion of fixed effects, we might be concerned that there is some omitted factor that varies within MSAs over time and is driving results. For example, the crack cocaine epidemic of the 1980s and 1990s may have resulted in risky behaviors that increased both AIDS risk and birth rates. Other potentially confounding factors include changes to state Medicaid rules or differential trends in poverty rates or prostitution—all of which could plausibly influence both AIDS incidence and birth rates. I address these concerns by adding a set of control variables to equation (1).

I control for the crack cocaine epidemic using a crack index developed by Fryer et al. (2005). The index is calculated at the city and state level and proxies the spatial and temporal patterns in the crack epidemic using a variety of measures that include arrests, emergency room visits, overdose deaths, and news coverage. To further control for use of IV drugs, I calculate MSA-level arrest rates for possession and sale of heroin and crack using data from Uniform Crime Reports (UCR) (Kaplan 2019). I also use UCR data to impute MSA-level arrest rates for prostitution and total drug arrests.

During the 1980s and 1990s, many states changed their Medicaid rules and expanded the population of Medicaid-eligible women (Dave et al. 2015). An ideal specification would capture these changes by adding a state-year fixed effect, but this is not possible due to sample size restrictions. On average, I observe only 2 MSAs per state, and to the extent that AIDS incidence is correlated across MSAs in the same state, including state-year fixed effects would remove the majority of the variation in AIDS incidence. Instead, I use data from the Current Population Survey Annual Social and Economic Supplement (ASEC) to calculate the share of women 20-44 who are covered by Medicaid (Flood et al. 2020). I also use the ASEC to calculate weighted averages for the share of women living below the poverty line, the share of women who are married, the share of women with a high school degree, the share of women with a college degree, the share of women unemployed, the female labor force participation rate, the share of men who are both employed and unmarried, and women's median income.[9] Lastly, I control for variation in the supply of sex partners using the female population share and Bureau of Justice Statistics data on state-level male and female incarceration rates (US BJS 2020). Further information on control variables, including descriptive statistics (Table A3), can be found in the appendix.

Results with controls included are presented in Table 2, column (3). The estimated effect declines when adding controls; however, I still find a positive and statistically significant effect of AIDS risk on birth rates. Estimated coefficients for the control variables can be found in Appendix Table A4, column (1).[10]

As a final test of within-MSA trends that could be affecting both AIDS risk and birth rates, I add an MSA-year linear trend to equation (1). Results are presented in Table 2, column (4). For this regression, I use birth data from 1969 to 2001, which results in a sample size of 3,366. I again find a positive and statistically significant relationship between births and AIDS risk.[11]

To illustrate the relative magnitude of estimated effects, I compare actual birth rates to predicted birth rates under a counterfactual setting in which AIDS risk is zero in every year. I use the specification with controls (Table 2, column (3)) for this analysis. Results

9. To obtain representative averages, I drop MSAs for which I observe less than 10 women in the relevant demographic group. I fill in the missing MSAs using weighted averages among metropolitan areas by the Census division. See the data appendix for further information.

10. The effect of AIDS risk on birth rates may differ before and after the introduction of effective treatment for HIV. Highly Active Antiretroviral Therapy (HAART) is introduced in 1996. When allowing for a trend break at 1997, I find that there is a large, positive effect of AIDS risk on birth rates pre-HAART and no effect post-HAART. This is consistent with the risk of AIDS declining once treatment is developed. However, there is major welfare reform at the same time HAART is approved and it is well-documented in the literature that welfare programs affect fertility (Moffitt 1998). Further research is needed in this area to identify the causal effect of HAART on AIDS avoidance behaviors.

11. Results are robust to using only AIDS cases diagnosed under the 1987 definition of AIDS to create a measure of AIDS Risk. Results are also robust to including a second order polynomial in AIDS Risk. The second order term is negative and statistically significant, indicating a diminishing marginal effect of AIDS risk on birth rates. However, I only find a negative overall effect of AIDS risk on birth rates at very high levels of AIDS risk (i.e., greater than the 99th percentile) and only for 1 percent of observations in my sample. See Appendix Tables A5 and A6, respectively, for further information.

are plotted in Figure 2. On average, I estimate that there were 0.5 additional births per 1,000 women per year due to AIDS avoidance behaviors. In total, I estimate that there were 330,000 additional births between 1981 and 2001. Figure 3 also presents these results as a share of the actual birth rate. At the height of the AIDS epidemic, the birth rate increased by 1.5 percent due to AIDS avoidance behaviors.

VII. Robustness Tests

A. Effect of AIDS by Sexuality

Despite the inclusion of MSA-specific linear trends and controls, we might still be concerned that unobservable changes in sexual behavior within MSAs are violating the identifying assumption by shifting both AIDS risk and birth rates. Specifically, we might worry that people are engaging in more sexual activity, which is increasing both AIDS incidence and birth rates. I address this concern by exploiting characteristics specific to the AIDS epidemic.

An alternative hypothesis that could explain the results displayed in Table 2 is that AIDS risk is higher in areas that are "sexually liberal" (i.e., greater social acceptance of unprotected sex, multiple sex partners, same-sex partners, etc.), and, as a result, also have a higher birth rate. If this were the case, we would also expect to find a positive relationship between birth rates and AIDS incidence in homosexual men. In contrast, if women are adjusting their sexual behavior in response to their true risk of infection, we would expect most of the effect to come from incidence of diagnoses in women or heterosexual men.[12] To test this alternative hypothesis, I analyze the effect of AIDS incidence among people of different sexual identities on birth rates.

Results are presented in Table 3. I find that AIDS diagnoses in both adult women and

12. Analyzing data separately by sexual identity is also important due to differential trends across these groups. As the APIDS manual notes, "Because men who have sex with men comprise such a large proportion of the total number of AIDS cases, trends in this subgroup will overshadow those in other groups unless the data are examined separately. Analysis of data, without regard to specific subgroups, may conceal information or lead to misinterpretation of the data."

heterosexual men result in an increase in births. In comparison, I find there is no effect of AIDS diagnoses in homosexual and bisexual men on births. Thus, instead of adjusting behavior in response to the AIDS epidemic as a whole, women are adjusting their behavior in response to their specific risk of infection, as proxied by the incidence of AIDS in potential sexual partners in their area.

B. Effect of AIDS on Other STIs

Another alternative hypothesis argues that women are engaging in more unprotected sex, which increases both AIDS incidence and birth rates. If the increase in AIDS incidence and birth rates are both driven by an increase in sexual activity, specifically sex without a condom, then the incidence of other STIs would also increase. I analyze the effect of AIDS incidence on other STIs using state-level data on gonorrhea incidence from 1984 to 2001. These data are publicly available via CDC WONDER (US DHHS 2015). Unfortunately, MSA-level gonorrhea data are only available after 1995. Data on syphilis are also available at the state level during the time period. I use gonorrhea as a measure of other STIs because it is much more common than syphilis and less concentrated geographically (Chesson, Harrison, and Kassler 2000).[13]

Using a fixed effects specification, I analyze the effect of AIDS risk on gonorrhea incidence in women. Results are presented in Table 4. I find that gonorrhea incidence decreases in response to AIDS incidence. Columns (2) and (3) repeat this analysis with a weighted specification and a regression with controls included, respectively. Across all specifications, AIDS risk has a negative and statistically significant effect on gonorrhea incidence. This result contradicts the hypothesis that higher births and AIDS incidence are both due to unobserved increases in sexual activity among women. If births and AIDS are increasing due to increases in sexual activity, we would also expect gonorrhea to increase. In contrast, I find that AIDS risk leads to a decrease in gonorrhea in women.

13. Chlamydia data is also included in the CDC WONDER STD Morbidity Database. However, chlamydia diagnoses were not required to be reported to the CDC until 1988 (Worboys 2019).

C. Effect of AIDS on Abortion Rates

A final alternative hypothesis argues that the birth rate is increasing because the abortion rate is decreasing. In other words, holding the rate of pregnancy fixed, AIDS risk results in more women choosing to terminate unintended pregnancies. This could be true if, as argued by Fortin (2015), the AIDS epidemic created a cultural shock that led women to hold more conservative values towards marriage and family. To test this hypothesis, I use data from the Guttmacher Institute on the number of abortions by state each year per 1,000 women aged 15-44 (Jones and Kooistra 2011).[14]

Results showing the effect of AIDS risk on abortion rates are presented in Table 5. I find that AIDS risk has a positive and statistically significant effect on abortion rates. This effect is robust to weighting by female population and the inclusion of control variables described in Appendix Table A3. As with birth rates, I find that this effect is driven by incidence of AIDS among women, with no relationship between abortion rates and AIDS incidence among homosexual and bisexual men (see Appendix Table A7).[15] This result rejects the alternative hypothesis that the birth rate is increasing because the abortion rate is decreasing. In contrast, I find that the overall increase in pregnancies is larger than the increase in births. I estimate that there were 0.066 additional pregnancies per 1,000 women in response to each additional AIDS case, and approximately 44 percent of these pregnancies resulted in abortion.[16] This share is on par with the recent literature estimating that 42 percent of unintended pregnancies result in abortion (Finer and Zolna 2016). The positive effect of AIDS risk on abortion rates also indicates that the additional pregnancies that result from AIDS avoidance behaviors are unintended pregnancies (i.e., unwanted or mistimed).

14. Abortion rates are calculated by state of occurrence, as data on abortion by state of residence is only available for four years in my panel. MSA-level data on abortions and annual data on abortions by age group are unavailable. As such, I use state-level abortion rates for women 15-44, calculating weighted population shares of state-level rates for MSAs that cross state boundaries. Data on abortion rates are only available for 12 years between 1981 and 2001, as a result the sample size for this analysis drops to 1224 MSA-year observations. See the data appendix for more information.

15. This result is also robust to the inclusion of a second order polynomial in AIDS risk. See Appendix Table A8.

16. Using the specification with controls in Tables 2 and 5, $0.029 + 0.037 = 0.066$ and $0.029/0.066 = 0.439$.

As a final robustness check, I exploit the incubation period of HIV infection. In the 1980s and 1990s, the average time between infection with HIV and an AIDS diagnosis was 10 years (Osmond 1998). Thus, AIDS incidence in a given year is not driven by current sexual behaviors but by sexual behaviors from 10 years prior. Therefore, I add a variable for birth rates 10 years prior to the regressions. This inclusion captures the characteristics of sexual behaviors at the time of HIV infection. To the extent that sexual behavior 10 years prior is correlated with current sexual behavior, controlling for prior birth rates removes the unobservable relationship between sexual behavior, AIDS, and births. Results are presented in Appendix Table A9. I find that AIDS risk has a positive and statistically significant effect on current birth rates, even when controlling for birth rates 10 years prior.

The analyses presented in this section argue that there is a causal effect of AIDS risk on birth rates and gonorrhea incidence. The AIDS epidemic led women to adjust their behavior to mitigate the risk of AIDS exposure. My results show that an unintended consequence of these AIDS avoidance behaviors was an increase in the birth rate, an increase in the abortion rate, and a decline in gonorrhea incidence.

VIII. Underlying Mechanisms

What types of behavior changes could decrease the likelihood of AIDS infection but increase the likelihood of pregnancy and birth? One possibility is that women adopt condoms in favor of more effective contraceptives such as the Pill. Switching from effective contraception to condoms would decrease infection likelihood but increase pregnancy likelihood. Another possibility is that women respond to AIDS risk by limiting the number of sexual partners or choosing less risky partners (i.e., based on drug use, number of other partners). The likelihood of a birth may increase in this case if women with only one partner have sex more frequently, are less likely to use contraception, or are more likely to continue a pregnancy. Both mechanisms are consistent with the decrease in gonorrhea incidence: More condom use or fewer sexual partners would both limit the spread of STI.

To evaluate which mechanism is driving results, I make use of information from birth certificate records on mother's marital status and father's age.[17] I analyze the effect of AIDS risk on births for married and unmarried mothers per 1,000 women. For each of these groups, I split the analysis by those with and without information on the father's age. I also split the sample of women by age group due to large age differences in the rate of extramarital childbearing. As with my main analysis, I use a fixed effects specification and control for the full set of age specific control variables described in Appendix Table A3.

For both mechanisms (switch to condoms and limit number of partners), we expect that the birth rate for unmarried women would increase. However, the two mechanisms predict different results on availability of the father's information. Assume that the father's information is more likely to be recorded on a birth record if he was the mother's only partner. Then, if many women are switching to only one partner, the number of births with the father's age recorded would increase. In contrast, we would expect no change in records with the father's age if women are maintaining the current number of partners but switching to condoms.

Results showing the effect of AIDS by the mother's marital status are presented in Table 6.[18] I find that AIDS risk increased the birth rate of unmarried women aged 30-44, but only for the population of birth records with information on the father's age. There is limited evidence of an increase in births for married women, but the effect goes away when including control variables. I find no effect of AIDS on births that lack information on the father. This result further contributes to the causal interpretation of results. If the increase in births and AIDS incidence was driven by IV drug users or more unprotected sex, we would expect an increase in births that lack the father's information. I interpret the increase in births for unmarried mothers with present fathers as evidence that a large share of women are responding to the risk of AIDS by having only one sexual partner.

17. Beginning in 1980, 41 states directly asked for mother's marital status. For the remaining 9 states and the District of Columbia, the CDC inferred marital status by comparing surnames across the mother, father, and child.

18. Note that AIDS risk and all control variables are now defined using age group specific data.

Survey data confirms that women limited the number of sexual partners in response to AIDS risk. According to the 1988 National Survey of Family Growth (NSFG), 5.3 percent of married women and 37.5 percent of unmarried women reported changing their behavior to avoid AIDS. Furthermore, 16 percent of sexually active, unmarried women specifically reported they "stopped having sex with more than one man" to avoid AIDS. Ceasing to have multiple partners was the most common way women reported adjusting their behavior to avoid AIDS (Mosher and Pratt 1993).

IX. Magnitude of Estimated Effects

The main results presented in Section 6 suggest that the birth rate for adult women increased on average by 0.5 births per 1,000 women per year as a result of the AIDS epidemic. I argue that this result is largely driven by women who would have otherwise had multiple partners choosing to have only one partner, at which point the likelihood of pregnancy increases. However, it is difficult to interpret the relative magnitude of this effect due to heterogeneity in both behavioral changes and women's probability of pregnancy. While the results point to many women limiting their number of partners, other women may switch to condoms or even abstain from sexual activity. Though I am not able to precisely estimate what share of women changed their behavior and how, I can evaluate the extent to which estimates appear reasonable in magnitude.

I compare my estimates to those in the existing literature regarding changes in birth rates. For example, we would expect that the advent of the Pill had a much larger effect on birth rates than the AIDS epidemic. Bailey (2010) estimates that the legalization of the Pill in the 1960s decreased the rate of marital childbearing by 13 births per 1,000 women. As expected, the effect of the Pill on birth rates is much larger than the estimated effect of AIDS risk.

Kearney and Levine (2009) analyze the effect of expanded Medicaid family planning on births. They find that expanded family planning coverage decreased the birth rate by

1.5 births per year per 1,000 women. It seems implausible that the risk of AIDS infection would have as large an effect on birth rates as Medicaid coverage. However, Medicaid family planning waivers only increased the share of covered women by 5.3 percentage points. When we consider that potentially all women were "treated" by the heightened risk of AIDS infection, the estimated magnitudes seem more reasonable.

If we assume that all women are treated by the increased risk of AIDS, what share of women would need to change their behavior to justify the estimated effects (i.e., what share of women were "compliers" (Imbens and Angrist 1994))? According to the NSFG, 16 percent of sexually active, unmarried women reported they "stopped having sex with more than one man" to avoid AIDS. Is this share large enough to justify the estimated effect?

To answer this question, I use data from the National Longitudinal Surveys of Youth 1997 (NLSY97) for the years 1998 – 2011. Though this time period is later than the height of the AIDS epidemic, the NLSY97 is an ideal data set because it specifically asks about number of sexual partners. I create an indicator variable that describes whether a sexually active woman reports an additional biological child in the next survey round, and then calculate the weighted mean for multiple demographics.

Across all 89,573 individual-year observations, the probability that a woman aged 20-30 will have an additional child by the next survey round is 0.0668, resulting in 66.8 births per 1000 women.[19] This estimate very closely approximates the actual population fertility rate during the period. I then divide the sample into women with 1 partner and women with 2-5 partners. The birth rate among these two groups are 107.8 births per 1,000 women and 63.5 births per 1,000 women, respectively. The birth rate is 44.3 births higher among women with only one sexual partner. Thus, to increase the birth rate by 1 birth, 22.6 women per 1,000 women (or 2.2 percent) must switch from multiple partners to 1 partner (1/44.3*1,000=22.6).

I repeat this exercise for unmarried women. Among unmarried women, 7.5 percent would need to switch from 2-5 partners to 1 partner in order to increase the birth rate by 1 birth.

19. Due to the age of participants observed in the NLSY97, I can only conduct this analysis using women aged 20-30.

These numbers are similar to the NSFG survey responses. If 16 percent of unmarried women stopped having more than one partner—as reported in the NSFG—the birth rate would increase by a little more than 2 births per 1,000 women. Due to sample selection, this is likely an overestimate of the true effect. Women who ex ante prefer multiple partners may be more likely to take steps to prevent pregnancy and birth, even in monogamous partnership. This is especially true if these women are more likely to get an abortion in response to unintended pregnancy. My results predict that the birth rate increased on average by 0.5 births per 1,000 women per year due to AIDS avoidance behaviors. My lower estimate is consistent with the aforementioned upward bias in predictions from the NLSY97, as well as heterogenous responses in the population, i.e., in addition to the women who decrease the number of partners, there are women who abstain or adopt condoms in addition to other contraceptives, which pulls down the average effect on birth rates.

The available evidence suggests that the magnitude of the estimated effect is reasonable and consistent with approximately 16 percent of unmarried women opting to have only one sexual partner to decrease their chances of AIDS exposure.

X. Heterogeneity in Age and Race

For the final analysis presented in this paper, I evaluate whether there are heterogeneous effects of AIDS risk on birth rates across different demographics. Results presented in Table 6 show that there is heterogeneity across age groups. I also evaluate heterogeneity across race. The effect of AIDS risk by race and mother's marital status for women aged 30-44 is presented in Table 7. I use race-specific and age-specific data to calculate AIDS risk and to create control variables.[20] Results for women aged 20-29 are in Appendix Table A12. I find no effect of AIDS risk on births for women ages 20-29.

For women aged 30-44 years, I find varying effects across white women and Black women. Among white women, I find that births increase for unmarried women with present fathers.

20. Descriptive statistics for race- and age-specific variables are presented in Appendix Tables A10 and A11

This is consistent with white women switching to monogamous partnerships to avoid AIDS. Among Black women, I find a positive and statistically significant effect of AIDS risk on births for married women, but no effect for unmarried women.[21] We might think that the increase in births for married women is due to marriages that happen as a result of pregnancy and birth; however, I find no effect of AIDS risk on marriage rates for any demographic group (see Appendix Table A12).

There are several possible explanations for the heterogeneous effects found in age and race. One possibility is that—even within the more narrowly defined demographic groups—heterogeneous behavioral responses are moving birth rates in opposite directions and resulting in an overall null effect. For example, if women aged 20-29 are equally likely to adopt condoms or switch to monogamy in response to AIDS risk, overall birth rates might remain unchanged.

It is also possible that preferences and costs associated with sexual activity differ for women in their 20s versus women in their 30s. For example, the implicit costs of entering into monogamous partnerships may be higher for women in their 20s, or perhaps there are less men of the same age willing to be monogamous. Costs and preferences over childbirth may also differ. We only expect women to adjust their behavior at the expense of pregnancy risk if the perceived cost of AIDS infection outweighs the perceived cost of a birth. If the cost of a birth is higher for younger women there might not be any behavioral change. This is consistent with the literature showing that the wage penalties associated with motherhood are greater for younger women (Miller 2011). It could be that women in their 20s respond to pregnancies that result from AIDS avoidance behaviors by getting abortions. In this case, though pregnancies are increasing, the birth rate is unaffected. Given that the large majority of abortions are for women aged 20-29, it is likely that the positive effect of AIDS risk on abortion rates is driven by women in their 20s (Sedgh et al. 2013). Thus, women of all ages experience similar increases in pregnancy rates, but women in their 30s continue

21. I am unable to calculate births to Hispanic women, as most states did not ask about Hispanic ethnicity on birth certificates during the 1980s.

the pregnancy while women in their 20s abort the pregnancy. Unfortunately, high frequency data on abortion rates by age group is not available to further test this relationship.[22]

The heterogeneity in results across white women and Black women are even more puzzling. AIDS incidence among Black women was more than 10 times as large as that of white women during the 1980s and 1990s. Yet, I find no effect of AIDS risk among unmarried Black women, and only a small effect of AIDS risk on births for married Black women. I propose two possible explanations for these results.

First, though AIDS risk was much higher among Black women, it is possible that knowledge about AIDS prevention was lower. Table 8 presents results from multiple rounds of the National Health Interview Survey AIDS Supplement in the 1980s and 1990s. Respondents were asked to rate the effectiveness of both condoms and monogamy in preventing AIDS transmission during sexual contact. Among women aged 20-44, Black women were less likely than white women to rate these methods as very effective and more likely to report they were unsure of the effectiveness of these methods.

Second, given that white women responded to AIDS by entering into monogamous partnerships, it is also possible that the racial disparity in estimated effects is due to differences in the number of men available for monogamous partnership. White unmarried women largely responded to the risk of AIDS by choosing to have sex with only one man. A necessary condition for this behavioral response is that there are men available for monogamous partnership. Given that the majority of sexual relationships occur between people of the same race or ethnic group, it is possible that the high incarceration rate of Black men prevented this condition (Laumann et al. 2000). It is well-documented that the high incarceration rate of Black men affects partnerships for Black women (Cohen and Pepin 2018; Charles and Luoh 2010; Cornwell and Cunningham 2008). Furthermore, Johnson and Raphael (2009) find that the high male incarceration rate among Black men explains the majority of the differences in AIDS incidence across Black and white women.

22. For this period, the Guttmacher Institute provides data on abortions by age group for only four years: 1988, 1990, 1996, and 2000 (Maddow-Zimet, Kost, and Finn 2020).

I investigate whether differences in male incarceration rates can explain the racial gap in estimated effects. One way to study this problem empirically is to consider treatment effect heterogeneity. Specifically, does the effect of AIDS risk on birth rates depend on the number of men available for a monogamous partnership? To answer this question, I estimate the marginal treatment effect of AIDS risk on birth rates across values of the male incarceration rate.

I use Gaussian kernel reweighting to more flexibly estimate the marginal treatment effect (Hainmueller, Mummolo, and Xu 2019). Results are presented in Figure 4. There is a positive effect of AIDS risk on birth rates for white women across the range of male incarceration rates, but no effect for Black women. But, as Figure 4 illustrates, the distribution of male incarceration rates among Black people is drastically different from that among white people.

The results presented in Figure 4 indicate that the choice set available to Black women was different than that of white women. Entering into a monogamous partnership is not an option if there are no men available for such a partnership. The differences in the choice set provide one explanation for finding a positive effect for white unmarried women and a null effect for Black unmarried women. It is possible that AIDS risk would have had a positive effect on births for Black unmarried women had the male incarceration rate among Black people been different. However, the racial differences in these two factors are so drastic that it is impossible to estimate the counterfactual effect of AIDS risk, had Black women faced white women's choice set, without using out-of-sample extrapolation.

It is worth highlighting that both of the possible explanations presented here indicate that social inequality is an important contributing factor to the racial disparities in AIDS avoidance behaviors. Both differences in knowledge about AIDS and differences in incarceration rates are directly related to institutional design and structural racism. These factors are highlighted in a 2012 report on HIV/AIDS inequality stating that, "The racial HIV gap and the racial health gap in general, is strongly correlated with the racial wealth gap, which in turn is the direct outcome of both historical and contemporary processes of segregation in housing, education, employment, and health care as well as racially skewed mass incarceration. In this way, race—as it intersects with poverty, gender, and sexuality among other factors—becomes the embodiment of a multifaceted social exclusion and the rationalization for massive health inequities" (Robinson and Moodie-Mills 2012, 2).

Further research is needed in this area to better understand racial disparities in AIDS avoidance behaviors among women, as well as the Black-white gap in AIDS incidence in women. In the meantime, policies should reflect the constraints that many women, especially Black women, face in avoiding HIV/AIDS. For example, policies that promote comprehensive sex education in public schools and polices that ensure modern HIV preventatives, such as PrEP, are accessible and affordable so that people can protect themselves from HIV regardless of partnership constraints.

XI. Conclusion and Policy Implications

I show that the risk of AIDS led to an increase in birth rates for adult women. While prior research has shown that individual AIDS avoidance behaviors can affect AIDS rates in the population, this is the first paper in the US context to relate AIDS avoidance behaviors and birth rates. My results are consistent with two possible behavioral changes: Adopting condoms in place of more effective contraception or decreasing the number of sexual partners. My analysis shows that the latter behavioral change is driving results. The majority of women who change their behavior opt to have only one sexual partner to protect from AIDS. I find that women mitigate the risk of AIDS at the expense of higher pregnancy likelihood. As a result, an unintended consequence of AIDS avoidance behaviors is an increase in birth rates.

The trade-offs between pregnancy prevention and STI protection are of particular importance for the development of screening guidelines for healthcare providers. Current screening guidelines promote a unidimensional idea of "safe sex" (i.e., condoms) and fail to address the variety of margins along which women adjust their sexual behavior to avoid STIs and pregnancy. For example, when health providers screen patients for risk factors, having only one sexual partner is viewed as an indicator for low risk of STI (Lee et al. 2016). However, my results suggest that these patients are at higher risk for unintended pregnancy. Health providers may want to target these patients for discussions of contraceptive options that are effective in preventing pregnancy with a high frequency of sexual activity. Similarly, health providers who prescribe highly effective methods of contraception for a patient may want to emphasize the importance of combining the method with condoms for continued STI protection. These two interventions could be particularly important given current trends in reproductive health. The rapid increase in drug-resistant gonorrhea suggests that more people may undertake STI avoidance behaviors such as decreasing the number of partners. On the other hand, the increased uptake of effective contraceptive methods like IUDs could result in lower condom use. An approach to reproductive health which takes into account the trade-offs between STI protection and pregnancy prevention is able to address both of these potential spillover effects.

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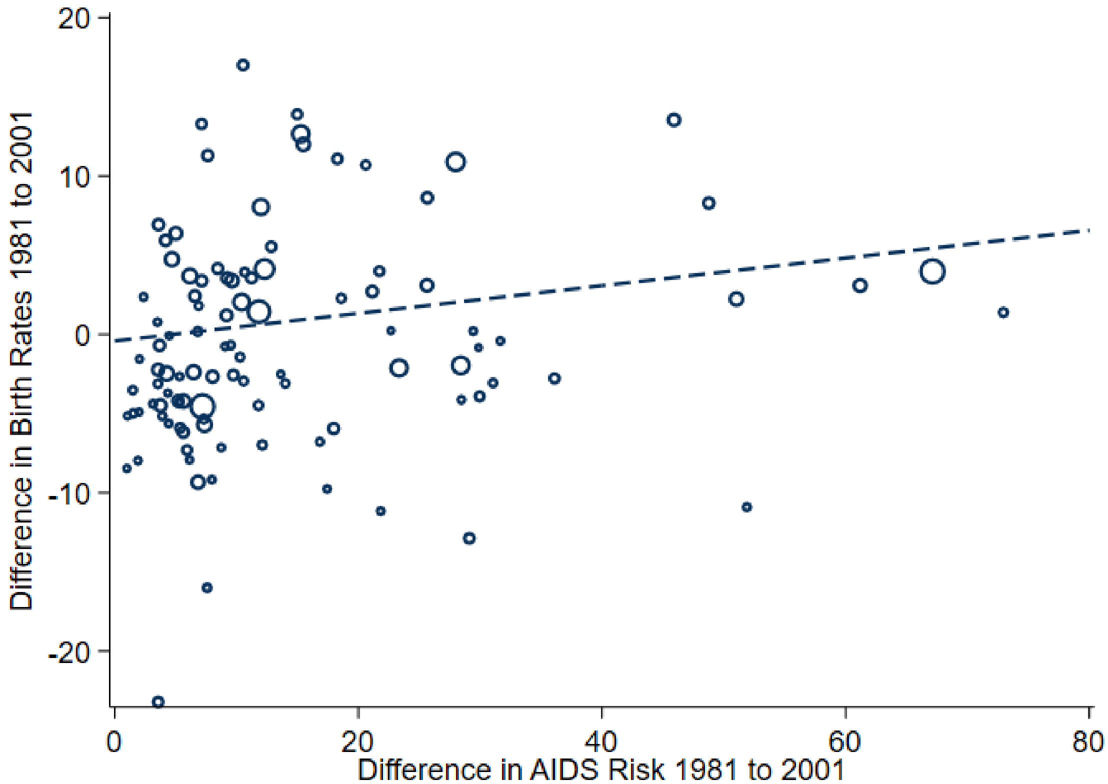

FIGURE 1: CHANGE IN BIRTH RATES VERSUS CHANGE IN AIDS RISK

Notes: This figure plots MSA level changes in birth rates from 1981 to 2001 versus change in AIDS risk over the same period. AIDS risk is defined as the previous year's AIDS incidence among women. Birth rates are calculated as live births per 1,000 women aged 20-44. The size of markers denotes relative female population size within an MSA in 2001. Raw trends indicate a positive relationship between birth rates and AIDS risk.

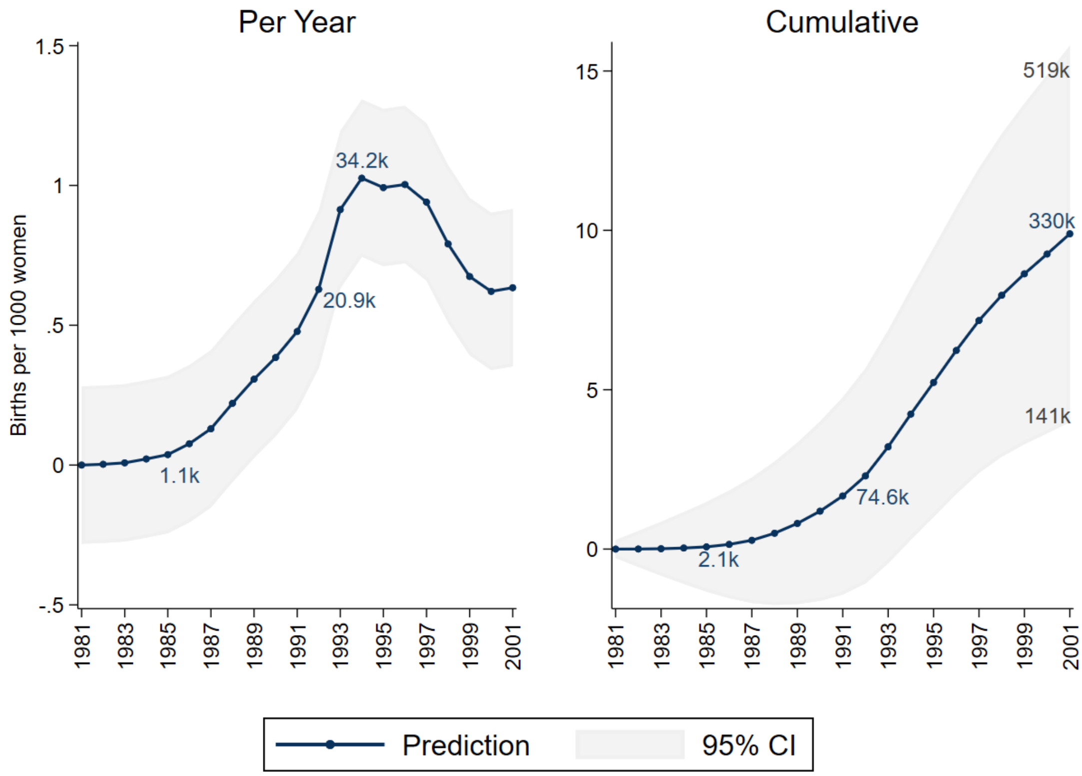

FIGURE 2: BIRTHS RESULTING FROM AIDS AVOIDANCE BEHAVIORS

Notes: This figure shows the additional births that resulted from AIDS avoidance behaviors, based on the effect estimated in Table 2, Column 3. Using the estimated coefficient, I predict the number of births under a counterfactual setting where AIDS risk is zero in ever year and compare this number to the actual number of births. On average, I find that the birth rate increases by 0.5 births per 1,000 women per year due to AIDS avoidance behaviors. In total, I estimate that there were 330,000 additional births between 1981 and 2001 due to the AIDS epidemic.

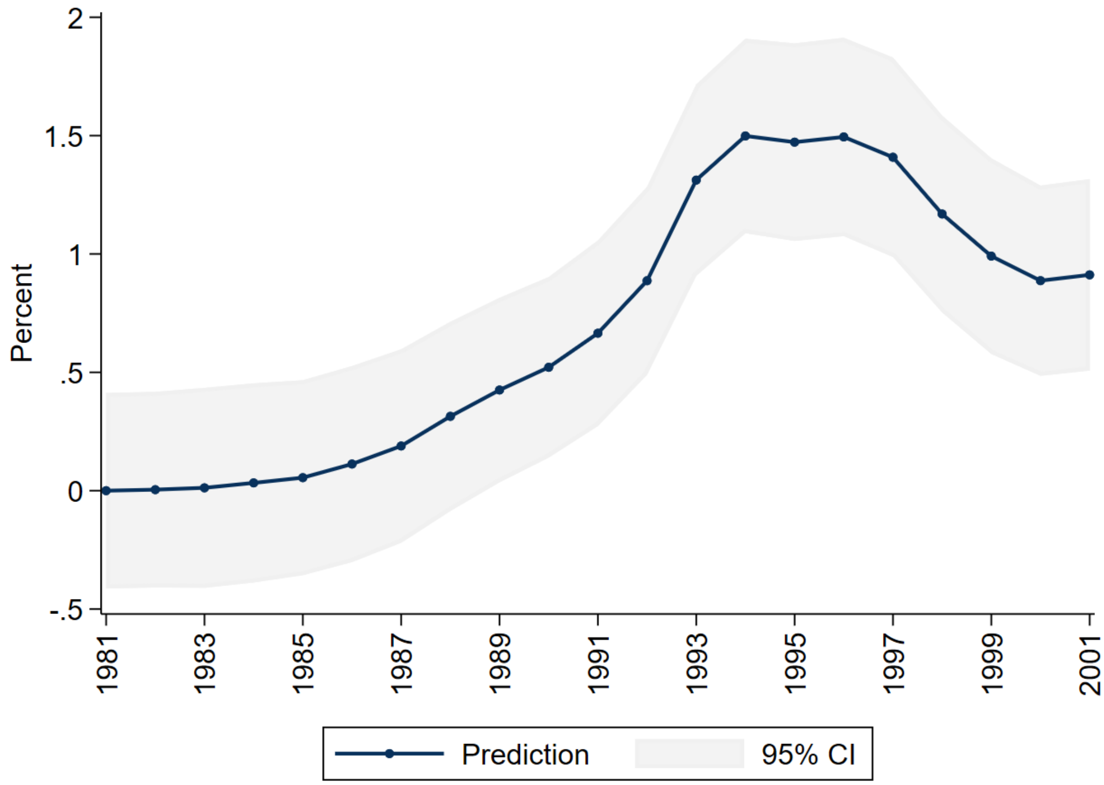

FIGURE 3: SHARE OF BIRTHS RESULTING FROM AIDS AVOIDANCE BEHAVIORS

Notes: This figure shows the additional births that result from AIDS avoidance behaviors as a percent of the total number of births in each year, based on the effect estimated in Table 2, Column 3. Using the estimated coefficient, I predict the number of births under a counterfactual setting where AIDS risk is zero in ever year and compare this number to the actual number of births. At the height of the AIDS epidemic from 1994 to 1996, I find that the birth rate increased by 1.5 percent due to AIDS avoidance behaviors.

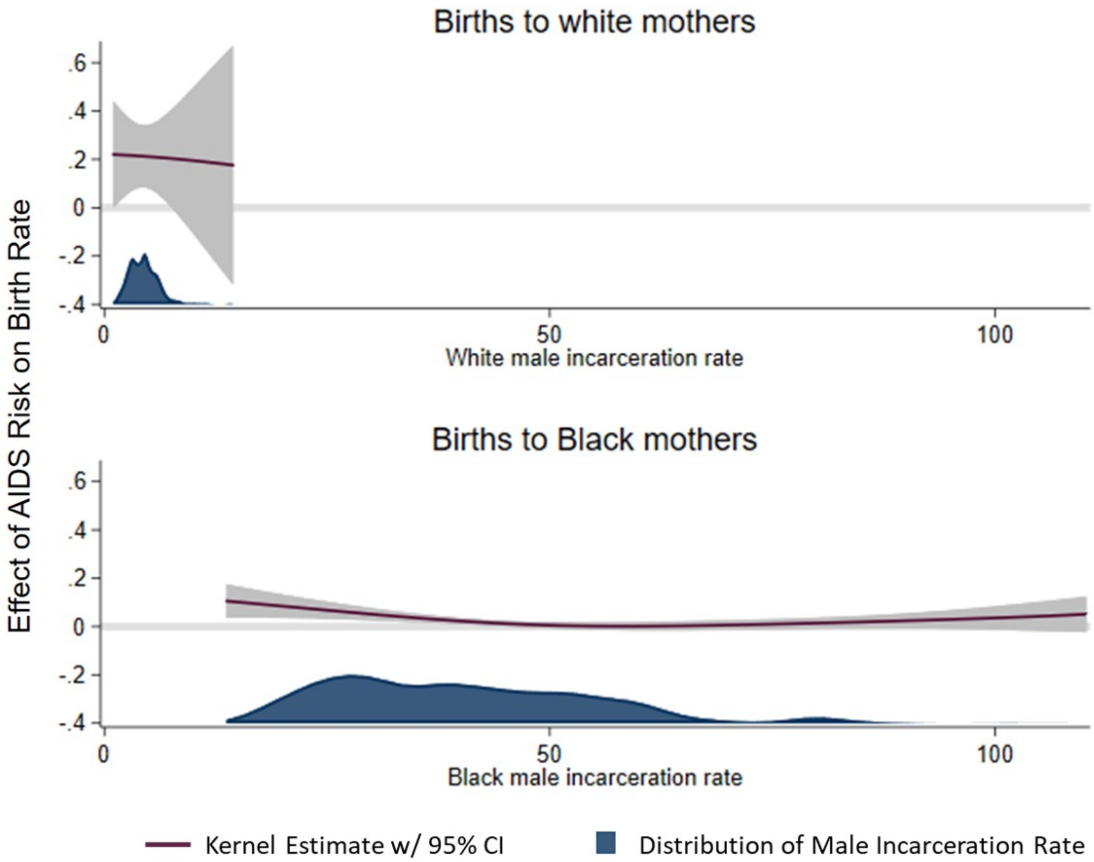

FIGURE 4: RACE DIFFERENCES AND MALE INCARCERATION RATE

Notes: This graph shows: (1) the effect of AIDS risk on birth rates by race across different levels of male incarceration rates, and (2) the distribution of male incarceration rates across white and Black men. For white women, AIDS risk has a positive and statistically significant effect on birth rates. I find that there is no effect of AIDS risk on births to Black women. However, Black women and white women faced very different constraints. Specifically, because male incarceration rates are drastically different across races, it is not possible to estimate how AIDS risk would have effected the birth rate among Black women had Black women faced male incarceration rates less than 20 (the range for white women) without making out of sample predictions.

Table 1: Descriptive Statistics

	Mean	Std. Dev.	Median	Min	Max
Births per 1000 women aged 20-44	68.51	9.88	66.73	48.78	125.85
AIDS Incidence in previous year...					
Total	14.65	9.60	18.98	0	203.11
Women 20-44	10.17	17.81	4.07	0	159.25
Homosexual/Bisexual Men 20-44	32.91	41.45	23.21	0	575.99
Heterosexual Men 20-44	15.25	24.96	6.95	0	218.06
N	2142				

Notes: This table presents descriptive statistics for the primary dependent and explanatory variables. The unit of observation is at the MSA-year level. Birth rates are calculated as live births per 1000 women. AIDS incidence is defined as number of new AIDS diagnoses per year per 100,000 people. I calculate the previous year's AIDS incidence among women aged 20-44 to create a measure of female AIDS risk.

Table 2: Effect of AIDS Risk on Birth Rates

	Primary Specification (1981-2001)			MSA Trends (1969-2001)
	(1)	(2)	(3)	(4)
AIDS risk	0.057^{***}	0.056^{***}	0.037^{**}	0.054^{***}
	(0.015)	(0.010)	(0.014)	(0.017)
N	2142	2142	2142	3366
MSA and year FEs	X	X	X	X
Weighted		X		
Controls included			X	
MSA-specific linear trends				X

Notes: This table shows the effect of AIDS risk on birth rates. AIDS risk is defined as AIDS incidence in the previous year among women aged 20-44. There is a positive and statistically significant effect of AIDS risk on birth rates. Results are robust to a population-weighted specification (Column 2), controls for drug use, incarceration, prostitution, poverty, income, Medicaid coverage, sex ratio, and educational attainment (Column 3), and the inclusion of MSA-specific year trends (Column 4). When including MSA-specific year trends, the sample size increases because I include more pre-AIDS years. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of AIDS Incidence by Sexuality on Birth Rates

	Birth Rate among Women Aged 20-44					
	(1)	(2)	(3)	(4)	(5)	(6)
AIDS incidence in previous year among...						
Women	0.057***	0.037**				
	(0.015)	(0.014)				
Heterosexual Men			0.047***	0.029***		
			(0.010)	(0.010)		
Homo/Bisexual Men					0.007	0.003
					(0.007)	(0.006)
N	2142	2142	2142	2142	2142	2142
MSA and year FEs	X	X	X	X	X	X
Controls included		X		X		X

Notes: This table presents the effect of AIDS incidence among people of different genders and sexual identities on birth rates. AIDS incidence among women and heterosexual men leads to an increase in birth rates among women. There is no effect of AIDS incidence among homosexual and bisexual men on birth rates. This result is consistent with women adjusting their behavior in response to their true risk of AIDS, as opposed to unobservable factors such as attitudes towards sexual behavior. Regressions in columns (2), (4), and (6) include the full set controls as described in Appendix Table A3, as well as MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of AIDS Risk on Gonorrhea Incidence in Women

	Gonorrhea Incidence (1984-2001)		
	(1)	(2)	(3)
AIDS risk	-0.815**	-0.629*	-1.015***
	(0.315)	(0.335)	(0.293)
N	1835	1835	1835
MSA and year FEs	X	X	X
Weighted		X	
Controls Included			X

Notes: This table shows the effect of AIDS risk (previous year's female 20-44 AIDS incidence) on gonorrhea incidence in women. I find that AIDS risk has a negative and statistically significant effect on gonorrhea. This result rejects the alternative hypothesis that both AIDS risk and birth rates are increasing because women are having more unprotected sex, which would also result in higher gonorrhea incidence. Results are robust to weighting by female population size (column 2) and to the inclusion of controls described in Appendix Table A3 (column 3). All regressions include as MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of AIDS Risk on Abortion Rates

	Abortions per 1,000 Women 15-44		
	(1)	(2)	(3)
AIDS Risk	0.040***	0.032***	0.029***
	(0.010)	(0.012)	(0.011)
Mean Abortion Rate	26.32		
N	1224	1224	1224
MSA and year FEs	X	X	X
Weighted		X	
Controls Included			X

Notes: This table shows the effect of AIDS risk (previous year's female 20-44 AIDS incidence) on the number of abortions per 1,000 women aged 15-44. I find that AIDS risk has a positive and statistically significant effect on the abortion rate. This result rejects the alternative hypothesis that the increase in births is driven by a decrease in abortions. All regressions include as MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of AIDS Risk on Birth Rates by Mother's Marital Status

	Married		Unmarried w/ Father		Unmarried no Father	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Birth Rate Women Aged 30-44						
AIDS Risk	0.032**	0.019	0.011***	0.008***	0.004	0.004
	(0.015)	(0.015)	(0.004)	(0.003)	(0.003)	(0.002)
Mean of dependent variable	24.04	24.04	1.77	1.77	1.60	1.60
Effect as percent of mean	0.13%		0.62%	0.45%		
Panel B: Birth Rate Women Aged 20-29						
AIDS Risk	0.031	0.029	-0.003	-0.007	-0.011	-0.007
	(0.025)	(0.023)	(0.022)	(0.019)	(0.018)	(0.017)
Mean of dependent variable	53.71	53.71	10.17	10.17	10.02	10.02
N	2142	2142	2142	2142	2142	2142
MSA and year FEs	X	X	X	X	X	X
Controls included		X		X		X

Notes: Each cell in this table presents the results of a regression of birth rates by marital status and age group on age-specific AIDS risk. AIDS risk increases the birth rate to unmarried mothers with the father present, a proxy for cohabiting parents. This results confirms the mechanisms that women are responding to AIDS risk by entering into monogamous partnerships. The positive effect of AIDS risk on birth rates is limited to women aged 30-44 years old. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of AIDS Risk on Birth Rates by Race and Marital Status

	Married		Unmarried w/ Father		Unmarried no Father	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Birth Rate White Women Aged 30-44						
AIDS Risk	0.107*	0.062	0.038***	0.026***	0.008	0.007
	(0.057)	(0.051)	(0.011)	(0.009)	(0.008)	(0.007)
Mean of dependent variable	25.19	25.19	1.51	1.51	1.01	1.01
Effect as percent of mean	0.42%		2.52%	1.66%		
Panel B: Birth Rate Black Women Aged 30-44						
AIDS Risk	0.011***	0.010***	0.002	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Mean of dependent variable	14.05	14.05	3.91	3.91	4.97	4.97
Effect as percent of mean	0.078%	0.071%				
N	2142	2142	2142	2142	2142	2142
MSA and year FEs	X	X	X	X	X	X
Controls included		X		X		X

Notes: Each cell in this table presents the results of a regression of birth rates among women aged 30-44 years by race and marital status on race and age specific AIDS risk. AIDS risk increases the birth rate for unmarried mothers with the father present at birth, a proxy for cohabiting parents, but only among white women. Among Black women, there is a positive effect for married women. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Knowledge of AIDS Prevention among Women by Race

	Effectiveness of Condoms			Effectiveness of Monogamy		
	White	Black	Difference	White	Black	Difference
Very effective	0.290	0.280	0.010**	0.762	0.671	0.091***
	(0.454)	(0.449)	(2.27)	(0.426)	(0.470)	(14.19)
Somewhat effective	0.576	0.504	0.073***	0.182	0.212	-0.030***
	(0.494)	(0.500)	(15.34)	(0.386)	(0.408)	(-5.15)
Not at all effective	0.048	0.077	-0.029***	0.0226	0.0498	-0.027***
	(0.213)	(0.266)	(-13.49)	(0.149)	(0.218)	(-11.27)
DK how effective	0.067	0.110	-0.043***	0.0193	0.0423	-0.023***
	(0.251)	(0.313)	(-16.88)	(0.138)	(0.201)	(-10.27)
N	25,392	5,419		25,392	5,419	

Notes: This table shows NHIS respondents' weighted average perceptions of how effective condoms and monogamy are in preventing the transmission of AIDS. The sample is limited to women aged 20-44 years. White women are more likely than Black women to rate condoms and monogamy as very effective. Black women are more likely to rate condoms and monogamy as not effective, and are more likely to report they don't know how effective these methods are in preventing the transmission of AIDS. Statistical significance for a t-test on the comparison of means is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A: Additional Tables

Table A1: Relationship between AIDS Incidence and Perceived Risk

	Full Sample		Unmarried Women Sample			
	Total AIDS		Female AIDS		Total AIDS	
	(1)	(2)	(3)	(4)	(5)	(6)
High	0.180*	0.204**	0.417**	0.424**	0.385	0.394
	(0.0991)	(0.101)	(0.198)	(0.199)	(0.242)	(0.243)
Medium	0.0379	0.0523	-0.0396	-0.0474	0.0387	0.0331
	(0.0457)	(0.0467)	(0.0954)	(0.0955)	(0.121)	(0.121)
Low	0.0298	0.0255	-0.0449	-0.0445	-0.0171	-0.0178
	(0.0182)	(0.0192)	(0.0414)	(0.0422)	(0.0537)	(0.0547)
None	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
N	241,826	241,826	23,874	23,874	23,874	23,874
Region and year FEs	X	X	X	X	X	X
Controls		X		X		X
Share High	0.0059		0.01			

Notes: This table presents the effect of AIDS incidence on perception of AIDS risk using data from NHIS supplements. From 1987 - 1995, respondents were asked to rate their own chance of getting AIDS as high, medium, low, or none. This table presents the effects of a multinomial logit regression of responses on AIDS incidence. The unit of observation is the individual. AIDS incidence is calculated at the regional level. Regional AIDS incidence has a positive and statistically significant effect on the probability of rating own AIDS risk as high. Among unmarried women, only AIDS incidence among females is predictive of perceived risk. All regressions include region and year fixed effects. Columns (2), (4), and (6) include controls for educational level, race, and poverty status. Column (2) includes additional controls for sex and marital status. Standard errors are shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Relationship between AIDS Incidence and Perceived Risk

	Afraid AIDS for self ('85, '87, '90, '91)		AIDS risk for Public ('85, '87, '90)	
	linear (1)	logit (2)	linear (3)	logit (4)
AIDS incidence	0.004*	0.022*	0.004*	0.026*
	(0.002)	(0.012)	(0.002)	(0.014)
Mean of dependent variable	0.25		0.83	
N	5206	5206	3946	3946
State and year FEs	X	X	X	X

Notes: This table presents the effect of AIDS incidence on perception of AIDS risk using data from ABC News polls. In certain years, polls asked the following yes or no questions: "Are you afraid that you may pick up the AIDS virus yourself?" and "Do you think AIDS is a threat to the general public in the United States?" This table presents the effect of AIDS incidence in the respondent's census division on answers to AIDS questions. Local AIDS incidence has a positive and statistically significant effect on perception of AIDS risk, both for one's self and for the general public. All regressions include state and year fixed effects. Robust standard errors are clustered at the state level and shown in parentheses. Columns (1) and (3) present results from a linear regression and columns (2) and (4) present results from a logit regression. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Descriptive Statistics - Control Variables

	Mean	Std. Dev.	Min	Max	Obs
Gonorrhea diagnoses per 1000 women	195.47	115.61	6.46	664.71	1835
Abortions per 1000 women	26.32	10.43	6.2	55.86	1224
Crack index	1.21	1.3	-1.01	8	2142
Female incarceration rate per 1000 women	0.46	0.31	0.03	1.89	2142
Male incarceration rate per 1000 men	8.29	3.8	1.5	22.45	2142
Prostitution arrests per 100k people	142.56	284.15	6.91	4037.85	2142
Heroin/Coke possession arrests per 100k people	193.31	244.01	3.56	3501.54	2142
Heroin/Coke sale arrests per 100k people	120.52	129.15	3.56	2693.6	2142
Total drug arrests per 100k people	638.52	1013.01	70.85	23042.59	2142
Among women 20-44 years old...					
Female population share	0.5	0.01	0.46	0.54	2142
Share below poverty level	0.13	0.06	0	0.52	2142
Share married	0.58	0.07	0.21	0.92	2142
Median income	9033	4231	0	27000	2142
Share on Medicaid	0.09	0.05	0	0.38	2142
Labor force participation rate	0.74	0.07	0.44	0.96	2142
Unemployment rate	0.06	0.04	0	0.37	2142
Share with high school degree	0.66	0.21	0.17	1	2142
Share with college degree	0.15	0.11	0	0.53	2142
Share of men employed and unmarried	0.35	0.07	0.06	0.65	2142
Among women 30-44 years old...					
Female population share	0.51	0.01	0.47	0.54	2142
Share below poverty level	0.11	0.06	0	0.55	2142
Share married	0.68	0.08	0.35	1	2142
Median income	10371	5248	0	30000	2142
Share on Medicaid	0.07	0.05	0	0.43	2142
Labor force participation rate	0.74	0.08	0.44	1	2142
Unemployment rate	0.05	0.04	0	0.3	2142
Share with high school degree	0.66	0.23	0.06	1	2142
Share with college degree	0.17	0.11	0	0.62	2142
Share of men employed and unmarried	0.25	0.08	0	0.62	2142
Among women 20-29 years old...					
Female population share	0.5	0.02	0.44	0.54	2142
Share below poverty level	0.16	0.08	0	0.63	2142
Share married	0.42	0.11	0	0.9	2142
Median income	7532	3748	0	24000	2142
Share on Medicaid	0.11	0.07	0	0.73	2142
Labor force participation rate	0.73	0.09	0.12	1	2142
Unemployment rate	0.08	0.06	0	1	2142
Share with high school degree	0.67	0.21	0.05	1	2142
Share with college degree	0.12	0.11	0	0.71	2142
Share of men employed and unmarried	0.49	0.1	0	0.88	2142
N	2142				

Notes: This table presents descriptive statistics for control variables used in Tables 2-6. See the data appendix for further information on data sources and sample creation.

Table A4: Control Coefficients

	Birth Rate Women 20-44 (1)	Gonorrhea Rate Women (2)	Abortion Rate Women 15-44 (3)	Birth Rate 30-44 Unmarried, w/ father (4)
AIDS Risk	0.0365**	-1.015***	0.0292***	0.00834***
	(0.0141)	(0.291)	(0.0106)	(0.00273)
Female population share	-365.3***	404.6	-46.75	-61.79***
	(71.18)	(709.9)	(39.36)	(15.86)
Crack index	0.625***	12.72***	0.275	0.0957**
	(0.200)	(2.952)	(0.188)	(0.0402)
Share below poverty level	-0.0561	18.95	0.0851	0.427
	(3.069)	(37.22)	(2.213)	(0.703)
Share married	1.011	-17.22	-1.375	0.461
	(1.776)	(23.42)	(1.601)	(0.439)
Median income	0.000143***	-0.000111	-0.0000177	0.00000349
	(0.0000450)	(0.000602)	(0.0000504)	(0.0000102)
Share on Medicaid	-5.183	-88.34*	4.392*	0.783
	(3.999)	(49.67)	(2.238)	(0.703)
Labor force participation rate	-4.777**	51.03**	-1.108	0.138
	(2.110)	(24.94)	(2.275)	(0.436)
Unemployment rate	-1.581	24.81	-2.894	-0.974**
	(3.229)	(31.33)	(1.879)	(0.484)
Share with high school degree	-3.782*	33.57	9.590***	-0.120
	(1.987)	(25.48)	(2.289)	(0.506)
Share with college degree	5.622**	-0.929	1.559	-0.981*
	(2.408)	(30.94)	(2.858)	(0.541)
Share of men employed and unmarried	0.513	-5.423	1.843	0.267
	(1.413)	(19.65)	(1.605)	(0.376)
Female incarceration rate	2.423	47.18*	0.927	0.686
	(2.413)	(27.85)	(1.373)	(0.503)
Male incarceration rate	-0.185	-2.184	-0.378**	0.0191
	(0.271)	(3.150)	(0.163)	(0.0510)
Prostitution arrests	-0.00120***	-0.00161	-0.000885**	0.0000617
	(0.000432)	(0.0215)	(0.000396)	(0.000273)
Heroin/Coke possession arrests	0.000480	-0.0127	0.000966*	0.0000264
	(0.000406)	(0.0104)	(0.000531)	(0.000105)
Heroin/Coke sale arrests	-0.000191	0.0495***	0.00157	0.000571**
	(0.00105)	(0.0129)	(0.000992)	(0.000246)
Total drug arrests	0.0000704	-0.000769	0.000104	-0.0000687**
	(0.0000558)	(0.00390)	(0.000207)	(0.0000304)
N	2142	1835	1224	2142

Notes: This table presents coefficients on control variables for regressions presented in the main analysis. See Appendix B for further information on control variables. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of AIDS Risk on Birth Rates

	Primary Specification (1981-2001)			MSA Trends (1969-2001)
	(1)	(2)	(3)	(4)
AIDS risk (1987 definition)	0.108***	0.108***	0.074***	0.101***
	(0.021)	(0.023)	(0.018)	(0.018)
N	2142	2142	2142	3366
MSA and year FEs	X	X	X	X
Weighted		X		
Controls included			X	
MSA-specific linear trends				X

Notes: This table shows the effect of AIDS risk on birth rates using the 1987 definition of an AIDS diagnosis. There is a positive and statistically significant effect of AIDS risk on birth rates. Results are robust to a population-weighted specification (Column 2), controls for drug use, incarceration, prostitution, poverty, income, Medicaid coverage, sex ratio, and educational attainment (Column 3), and the inclusion of MSA-specific year trends (Column 4). When including MSA-specific year trends, the sample size increases because I include more pre-AIDS years. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect of AIDS Risk on Birth Rates

	Primary Specification (1981-2001)			MSA Trends (1969-2001)
	(1)	(2)	(3)	(4)
AIDS Risk	0.1030***	0.1124***	0.0899***	0.1194***
	(0.0332)	(0.0315)	(0.0275)	(0.0379)
AIDS Risk*AIDS Risk	-0.0004*	-0.0004**	-0.0005***	-0.0005**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
AIDS Risk at max. effect	123.0182	127.1780	92.5101	111.3588
N	2142	2142	2142	3366
MSA and year FEs	X	X	X	X
Weighted		X		
Controls included			X	
MSA-specific linear trends				X

Notes: This table shows the effect of AIDS risk on birth rates including a second order polynomial in AIDS risk. The quadratic term is negative and statistically significant, suggesting a diminishing marginal effect of AIDS risk on birth rates. However, the effect of AIDS risk on birth rates is positive overall except at very high levels of AIDS Risk (more than the 99th percentile of the AIDS risk distribution in the panel or less than 1 percent of observations). Results are robust to a population-weighted specification (Column 2), controls for drug use, incarceration, prostitution, poverty, median income, Medicaid coverage, sex ratio, and educational attainment (Column 3), and the inclusion of MSA-specific year trends (Column 4). When including MSA-specific year trends, the sample size increases because I include more pre-AIDS years. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effect of AIDS Incidence by Sexuality on Abortion Rates

	Abortions per 1,000 Women 15-44					
	(1)	(2)	(3)	(4)	(5)	(6)
AIDS incidence in previous year among...						
Women	0.040***	0.029***				
	(0.010)	(0.011)				
Heterosexual Men			0.016**	0.006		
			(0.007)	(0.008)		
Homo/Bisexual Men					-0.0001	-0.004
					(0.003)	(0.004)
N	1224	1224	1224	1224	1224	1224
MSA and year FEs	X	X	X	X	X	X
Controls included		X		X		X

Notes: This table presents the effect of AIDS incidence among people of different genders and sexual identities on abortion rates. AIDS incidence among women leads to an increase in abortion rates among women. There is no effect of AIDS incidence among homosexual and bisexual men on abortion rates. This result is consistent with women adjusting their behavior in response to their true risk of AIDS, as opposed to unobservable factors such as attitudes towards sexual behavior. Regressions in columns (2), (4), and (6) include the full set controls as described in Appendix Table A3, as well as MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Effect of AIDS Risk on Abortion Rates

	Abortions per 1,000 Women 15-44		
	(1)	(2)	(3)
AIDS Risk	0.0515**	0.0476	0.0438*
	(0.0258)	(0.0413)	(0.0233)
AIDS Risk*AIDS Risk	-0.0001	-0.0001	-0.0001
	(0.0002)	(0.0003)	(0.0002)
AIDS Risk at max. effect	217.0505	190.4084	150.6218
N	1224	1224	1224
MSA and year FEs	X	X	X
Weighted		X	
Controls included			X

Notes: This table shows the effect of AIDS risk on abortion rates including a second order polynomial in AIDS risk. The quadratic term is negative and statistically significant, suggesting a diminishing marginal effect of AIDS risk on abortion rates. However, the effect of AIDS risk on abortion rates is positive overall except at very high levels of AIDS risk (only 2 observations in the full panel have an AIDS risk greater than 150). Results are robust to a population-weighted specification (Column 2), and controls for drug use, incarceration, prostitution, poverty, median income, Medicaid coverage, sex ratio, and educational attainment (Column 3). All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Robustness Test: 10 year lag between HIV infection and AIDS diagnosis

	Birth Rate among Women Aged 20-44			
	(1)	(2)	(3)	(4)
Birth Rate 10 years prior	-0.083**	-0.089**	-0.101***	-0.103**
	(0.035)	(0.043)	(0.037)	(0.042)
AIDS Risk			0.062***	0.042***
			(0.015)	(0.014)
N	2142	2142	2142	2142
MSA and year FEs	X	X	X	X
Controls included		X		X

Notes: This table presents the results of an additional robustness check. I evaluate whether the positive relationship between AIDS risk and birth rates is driven by unobserved increases in sexual behavior by exploiting the 10 year incubation period of AIDS infection. I control for sexual behavior at the time of HIV exposure using birth rates ten years prior and find that the relationship between AIDS risk and birth rates among women remains positive. All regressions include the full set of race-specific controls as described in Appendix Table A3, as well as MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Descriptive Statistics

	Mean	Std. Dev.	Median	Min	Max
AIDS Incidence in previous year...					
All women 20-44	10.17	17.81	4.07	0	159.25
All women 20-44 (1987 definition)	5.59	10.33	2.18	0	100.31
All women 30-44	11.79	21.67	4.31	0	220.89
All women 20-29	7.45	12.7	2.93	0	130.02
White women 20-44	3.06	4.51	1.56	0	61.75
White women 30-44	3.45	5.56	1.64	0	86.91
White women 20-29	2.37	3.63	1.01	0	30.06
Black women 20-44	41.02	66.49	15.51	0	595.95
Black women 30-44	51.43	85.49	17.43	0	833.33
Black women 20-29	25.93	46.91	7.00	0	554.17
N	2142				

Notes: This table presents descriptive statistics for additional explanatory variables. The unit of observation is at the MSA-year level. AIDS incidence is defined as number of new AIDS diagnoses per year per 100,000 people.

Table A11: Descriptive Statistics - Race-specific Control Variables

	White Women				Black Women			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Female incarceration rate per 1000 women	0.24	0.17	0.02	1.3	2.2	1.54	0.31	9.34
Male incarceration rate per 1000 men	4.27	1.85	0.99	14.45	38.7	15.68	13.7	110.24
Prostitution arrests per 100k people	78.7	122.2	0	2129.58	60.88	176.5	0	3062.09
Heroin/Coke possession arrests per 100k people	101.82	106.08	0	1370.42	89.93	181.87	0	2919.24
Heroin/Coke sale arrests per 100k people	55.23	59.7	0	1363.59	64.44	93.36	0	1321.29
Total drug arrests per 100k people	360.59	323.45	14.13	4514.84	269.88	764.33	0	18468.49
Among women 30-44 years old...								
Female population share	0.5	0.01	0.46	0.54	0.51	0.05	0.26	0.58
Share below poverty level	0.08	0.06	0	0.59	0.33	0.11	0	0.79
Share married	0.72	0.08	0.36	1	0.23	0.1	0	0.75
Median income	10429	5610	0	35500	11240	4876	0	40000
Share on Medicaid	0.05	0.04	0	0.35	0.29	0.11	0	0.73
Labor force participation rate	0.74	0.08	0.38	1	0.68	0.1	0.18	1
Unemployment rate	0.04	0.04	0	0.63	0.17	0.1	0	0.73
Share with high school degree	0.67	0.22	0	1	0.6	0.23	0.07	1
Share with college degree	0.18	0.12	0	0.68	0.06	0.06	0	0.55
Share of men employed and unmarried	0.24	0.08	0	0.62	0.46	0.1	0	0.93
Among women 20-29 years old...								
Female population share	0.49	0.01	0.43	0.54	0.52	0.05	0.22	0.58
Share below poverty level	0.13	0.07	0	0.59	0.26	0.1	0	0.7
Share married	0.46	0.11	0	0.91	0.42	0.1	0	0.87
Median income	8332	3938	0	27000	4642	3419	0	20820
Share on Medicaid	0.08	0.06	0	0.62	0.2	0.09	0	0.6
Labor force participation rate	0.74	0.09	0.26	1	0.76	0.09	0.35	1
Unemployment rate	0.06	0.05	0	0.56	0.09	0.06	0	0.45
Share with high school degree	0.68	0.21	0.06	1	0.6	0.26	0	1
Share with college degree	0.13	0.12	0	0.69	0.1	0.08	0	0.72
Share of men employed and unmarried	0.49	0.11	0	0.91	0.33	0.1	0	0.85

Notes: This table presents descriptive statistics for race and age specific control variables used for regressions in Tables 7 and A12.

Table A12: Effect of AIDS Risk on Birth Rates by Race and Marital Status

	Married		Unmarried w/ Father		Unmarried no Father	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Birth Rate White Women Aged 20-29						
AIDS Risk	0.046	-0.002	-0.024	-0.057*	0.025	0.019
	(0.063)	(0.060)	(0.035)	(0.033)	(0.028)	(0.025)
Mean of dependent variable	57.53	57.53	8.55	8.55	6.12	6.12
Effect as percent of mean				-0.67%		
N	2142	2142	2142	2142	2142	2142
Panel B: Birth Rate Black Women Aged 20-29						
AIDS Risk	0.015*	0.003	-0.017	-0.019	-0.009	-0.007
	(0.008)	(0.008)	(0.012)	(0.012)	(0.010)	(0.010)
Mean of dependent variable	31.40	31.40	21.60	21.60	29.71	29.71
Effect as percent of mean	0.048%					
N	2142	2107	2142	2107	2142	2107
MSA and year FEs	X	X	X	X	X	X
Controls included		X		X		X

Notes: Each cell in this table presents the results of a regression of birth rates among women aged 20-29 years old by race and marital status on AIDS risk. I find no effect of AIDS risk on births to women aged 20-29 across any demographic group. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Effect of AIDS Risk on Marriage Rates

	Share of Women Married					
	All		White		Black	
	30-44	20-29	30-44	20-29	30-44	20-29
	(1)	(2)	(3)	(4)	(5)	(6)
AIDS Risk	-0.00006	0.00012	-0.00047	0.00090	-0.00001	-0.00005
	(0.00020)	(0.00035)	(0.00052)	(0.00075)	(0.00005)	(0.00006)
AIDS Risk lagged	-0.00003	-0.00037	0.00014	-0.00071	0.00005	0.00005
	(0.00019)	(0.00026)	(0.00047)	(0.00088)	(0.00005)	(0.00009)
N	2142	2142	2142	2142	2142	2142
MSA and year FEs	X	X	X	X	X	X

Notes: This table shows the effect of AIDS risk on marriage rates using regressions of birth rates on current year's AIDS risk and previous year's AIDS risk. I find no effect of AIDS risk on marriage rates. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B: Data Sources and Sample Construction

AIDS Public Information Data Set

The AIDS Public Information Data Set (APIDS) contains MSA-level annual data from 1981 to 2002 and is publicly available on CDC Wonder. I use APIDS for counts of AIDS diagnoses by year diagnosed among women aged 20-44, as well as heterosexual men aged 20-44, and homosexual and bisexual men aged 20-44. I also use APIDS for counts of AIDS diagnoses among women by age group (20-29 and 30-44) and race (white and Black). As the AIDS epidemic developed, the CDC expanded the criteria for an AIDS diagnosis. I use AIDS cases diagnoses under any criteria for my main analysis. As a robustness check, I also limit counts to AIDS cases diagnosed under the 1985 and 1987 criteria and find similar results.

Natality Detail File

I use restricted geographic data from the National Vital Statistics System to create annual counts of births by MSA. I also make use of information on birth certificates to create counts of births by mother's age, mother's race, and mother's marital status.

Population Data

To create measures of AIDS incidence and birth rates, I use population data from the Survey of Epidemiology and End Results (SEER) as made available online by the National Bureau of Economic Research. Specifically, I use adjusted, county-level data disaggregated by 19 age groups and white, Black, or other races.

Gonorrhea Surveillance Data

Information on gonorrhea incidence among women is publicly available via CDC WONDER's Sexually Transmitted Disease Morbidity Data. These data are available at the state level for the years 1984 - 2014. I merge this data with AIDS incidence data. For MSAs that cross state boundaries, I calculate the share of the MSA population in each state using SEER data, and the calculate the corresponding weighted average of gonorrhea incidence in each MSA.

Crack Cocaine Index

I use the crack cocaine index developed by Fryer et al. 2005. The crack index is calculated at the city and state level and proxies the spatial and temporal patterns in the crack epidemic using a variety of measures including arrests, emergency room visits, overdose deaths, and news coverage. For each MSA, I use the value for the largest city in that MSA. For

MSAs that lack city-level crack index data, I use state values. This data is available from 1980-2000. Since I lag all control variables by one year to account for the gestation period, including this control variable restricts the years in my final sample to 1981 to 2001.

Uniform Crime Reports Arrest Data

I use data from the Uniform Crime Reporting Program on arrests by race to calculate MSA-level arrest rates for drug-related offenses in every year. I take into account the number of months each agency reported to the UCR to calculate annual counts of arrests. In cases where I do not observe any agencies in a given MSA that report to the UCR, I use census division arrest rates to fill in missing observations. Arrest rates are calculated per 100,000 people.

Annual Social and Economic Supplement

I use the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) to create multiple control variables. The ASEC is an annual household survey that asks respondents questions regarding employment, poverty, and program participation. I use survey data from 1980 - 2000 on respondents' marital status, poverty status, employment status, educational attainment, wage income, and Medicaid enrollment. I use sampling weights to calculate weighted averages of responses in each MSA in every year among women aged 20-44. In cases where the sample size in a given MSA-year is less than 10 people, I use the corresponding average among metropolitan areas in the MSA's census division. I repeat this exercise to create control variables for all race and age specific sub-samples.

National Prison Statistics

The National Prison Statistics (NPS) data, available on ICPSR, details counts of persons incarcerated in state and federal prisons in each year by state, race, and sex. I merge this data with MSA data on birth rates and AIDS risk. For MSAs that cross state boundaries, I calculate the share of the MSA population in each state using SEER data, and the calculate the corresponding weighted average of incarceration rates in each MSA. Incarceration rates are calculated per 1,000 people.

ABC News AIDS Public Opinion Poll

ABC News Opinion Polls are publicly available on ICPSR and contain demographic information and geographic information at the census division level. In 1985, 1987, 1990, and 1991, these polls asked respondents if they were afraid of contracting the AIDS virus and if they thought AIDS was a threat to the general public.

National Health Interview Survey AIDS Supplement

The National Health Interview (NHIS) AIDS Supplement was conducted every year between 1987 and 1995 and asks respondents about their own perceived risk of getting AIDS, as well as their AIDS knowledge. The publicly available data includes demographic information as well as geographic information at the census region level.

Abortion Rate

Data on abortions per 1,000 women aged 15-44 is from the Guttmacher Institute's Data Center. Data are state level (based on state of occurrence) and available for the years 1981, 1982, 1984, 1985, 1987, 1988, 1991, 1992, 1995, 1996, 1999, 2000. For MSAs that cross state boundaries, I calculate the share of the MSA population in each state using SEER data, and the calculate the corresponding weighted average of abortion rates in each MSA.

Table B1: Data Sources

Data	DOI / URL
AIDS Public Information Dataset	https://wonder.cdc.gov/aidspublic.html
SEER U.S. County Population Data 1969-	https://data.nber.org/seer-pop/uswbo19agesadj.dta.zip
Sexually Transmitted Disease Morbidity Data	https://wonder.cdc.gov/std.html
IPUMS Current Population Survey	https://cps.ipums.org/cps/
National Prison Statstics, [United States], 1978-2018	https://doi.org/10.3886/ICPSR37639.v1
Jacob Kaplan's Concatenated Files: Uniform Crime Reporting (UCR) Program Data: Arrests by Age, Sex, and Race, 1974-2018.	https://doi.org/10.3886/E102263V10
Crack Cocaine Index	https://scholar.harvard.edu/fryer /publications/measuring-crack-cocaine-and-its-impact
National Health Interview Survey, 1987: AIDS Supplement.	http://doi.org/10.3886/ICPSR09271.v1
National Health Interview Survey, 1988: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR09411.v1
National Health Interview Survey, 1989: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR09708.v1
National Health Interview Survey, 1990: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR09909.v1
National Health Interview Survey, 1991: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR06050.v1
National Health Interview Survey, 1992: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR06347.v1
National Health Interview Survey, 1993: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR06529.v1
National Health Interview Survey, 1994: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR06871.v1
National Health Interview Survey, 1995: AIDS Knowledge and Attitudes Supplement.	http://doi.org/10.3886/ICPSR02531.v1
ABC NEWS/WASHINGTON POST POLL, SEPTEMBER 1985.	http://doi.org/10.3886/ICPSR08589.v1
ABC NEWS/WASHINGTON POST POLL, MARCH 1987.	http://doi.org/10.3886/ICPSR08845.v1
ABC NEWS AIDS POLL, JUNE 1990.	http://doi.org/10.3886/ICPSR09460.v1
ABC NEWS POLL, JULY 1991.	http://doi.org/10.3886/ICPSR09758.v1
Guttmacher Institute Data Center: State-year estimates	https://data.guttmacher.org/states/

Notes: This table presents DOIs or URLs for all publicly available data sets used in this paper.

Appendix C: The Ecological Inference Problem

One challenge in conducting analyses with data on birth rates and STI incidence is problems associated with using group averages rather than individual level data, the ecological inference problem (King, Tanner, and Rosen 2004). For example, in this setting, we might be concerned that AIDS risk is affecting the size of the female population. Since female population is the denominator used to calculate birth rates, the primary dependent variable for this analysis, unobservable correlations between AIDS risk and female population could bias results. Thus, the effect of AIDS risk on birth rates could be positive if AIDS risk is resulting in a decrease in the female population, either due to death or migration.

There are two ways I try to address this concern about ecological inference. First, I use known information to bound effects. For example, consider that prior to the development of effective treatment in 1996, most women who contract AIDS die within just a few years. Thus, we know that AIDS risk does decrease the female population. We can check whether this decrease is large enough to explain the increase in births that I find in my analysis. In the extreme case, if all women who contract AIDS die within one year, each additional AIDS case per 100,000 women would increase the birth rate by no more than 0.01 births per 1,000 women.[23] We can think of 0.01 as a lower bound below which we cannot reject the possibility that deaths due to AIDS are driving results. I estimate that every additional AIDS case per 100,000 women results in 0.05 more births per 1000 women, much larger than the lower bound of 0.01.

Second, because I separately observe information on birth counts and population levels, I can directly test whether there is a relationship between AIDS risk and female population. Using data at the MSA-year level and a fixed effects specification, I regress female population on AIDS risk by demographic group. Results are presented in Table C1. I find no effect of AIDS risk on female population. These results suggest that the positive effect of AIDS risk on birth rates is driven by an increase in births, and not a decrease in population.

These two tests address one specific form of the ecological inference problem (i.e., that changes in the denominator used to calculate group averages might bias results). It is also possible that there are other factors associated with AIDS incidence in the population that could bias results. We can think of this as an extreme form of omitted variable bias. While it is not possible to directly test for all possible sources of omitted factors, I refer readers to robustness checks in the main analysis that argue for a causal interpretation of results.

23. If one woman per 100,000 women dies, that means that 0.01 woman per 1,000 women die, so the birth rate would increase by 0.01.

Table C1: Effect of AIDS Risk on Female Population

	Female Population					
	All		White		Black	
	30-44	20-29	30-44	20-29	30-44	20-29
	(1)	(2)	(3)	(4)	(5)	(6)
AIDS Risk	213.688	-51.981	-101.818	35.987	12.442	4.471
	(168.316)	(94.416)	(168.940)	(146.020)	(8.617)	(3.095)
N	2142	2142	2142	2142	2142	2142
MSA and year FEs	X	X	X	X	X	X

Notes: This table shows the effect of race and age specific AIDS risk on female population. I find no evidence that AIDS risk affected female population size for any demographic group. This result rejects the hypothesis that women are migrating away from cities with high AIDS incidence. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D: Births and AIDS Risk among Adolescent Women

This appendix presents results on the effect of AIDS risk on births for adolescent women aged 15-19. For the majority of my sample, AIDS incidence among women aged 13-19 years old is zero (see Table D1). Repeating my primary analysis on the sample of adolescent women, I find limited evidence of a decline in birth rates due to AIDS risk (see Table D2). However, this effect disappears when controlling for MSA-specific trends. Thus, the effect of AIDS risk on births to adolescent mothers is not robust to the overall downward trend in teen births during this time period.

Table D1: Descriptive Statistics

	Mean	Std. Dev.	Median	Min	Max
Births per 1000 women aged 15-19	52.44	15.4	52.72	15.57	109.22
AIDS Incidence in previous year...					
Women 13-19	1.07	2.71	0	0	34.45
N	2142				

Notes: This table presents descriptive statistics for the birth rate and AIDS incidence among adolescent women. The unit of observation is at the MSA-year level. Birth rates are calculated as live births per 1000 women. AIDS incidence is defined as number of new AIDS diagnoses per year per 100,000 people.

Table D2: Effect of AIDS Risk on Birth Rates to Adolescents Aged 15-19

	Primary Specification (1981-2001)			MSA Trends (1969-2001)
	(1)	(2)	(3)	(4)
AIDS risk	-0.126*	-0.100*	-0.153**	0.046
	-0.064	-0.057	-0.063	-0.043
N	2142	2142	2142	3366
MSA and year FEs	X	X	X	X
Weighted		X		
Controls included			X	
MSA-specific linear trends				X

Notes: This table shows the effect of AIDS risk on birth rates to adolescents aged 15-19. Columns (1)-(3) suggest that births to adolescent mothers declined in response to AIDS risk, However, this effect disappears when controlling for MSA-specific linear trends. The overall decline in births to adolescent mothers during this time period appears to be driving results. Furthermore, the median AIDS risk for adolescent women is the sample is 0 and the mean is only 1.07, suggesting that adolescents are not at risk of AIDS during this time period. When including MSA-specific year trends, the sample size increases because I include more pre-AIDS years. All regressions include MSA and year fixed effects. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix E: Robustness of the Two-Way Fixed Effects Approach

Recent advancements in the econometric literature highlight potential problems with the two-way fixed effects approach used in this paper. In the presence of heterogeneous treatment effects, the two-way fixed effects estimator is a weighted sum of all average treatment effects across groups and time (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2018). To my knowledge, there does not exist a solution to this problem in the case of continuous treatment variable. As such, it is necessary to discretize AIDS risk in order to test the robustness of results to concerns about the fixed effects approach.

In the continuous treatment case, the relevant variation is the timing of AIDS spread within MSAs. To approximate this variation, I discretize AIDS risk by the timing at which AIDS risk reaches certain levels in each MSA. Figure E1 illustrates this variation. First, I define an indicator variable that equals one as soon as AIDS incidence in an MSA reaches or exceeds 1 case per 100,000 women in the previous year (my definition of AIDS risk). The timing of this event ranges between 1983 and 1992 across the sample of MSAs. Second, I define an indicator that equals one as soon as AIDS risks reaches 2. The timing of this event ranges between 1983 and 1994. Using these discretized definitions of AIDS spread, I conduct a difference-in-difference analysis to estimate the effect of being in the "post" period on birth rates. Results are presented in Table E1. The positive effect of AIDS risk on birth rates is robust to the difference-in-difference specification.

However, the difference-in-difference specification does not fully address the concerns with two-way fixed effects. As noted by Goodman-Bacon (2018), even with the discrete treatment case, the variation in treatment timing results in a general estimator that is a weighted average of multiple treatment effects. Unfortunately, the balance test proposed by Goodman-Bacon (2018) to estimate these weights only works in cases where some groups are "never treated." In this setting, all MSAs are eventually "treated" by AIDS risk. Instead, I check for further heterogeneity in treatment effects by estimating an event study around the timing of AIDS spread. Results are presented in Figure E2 and confirm the positive effect of AIDS risk on birth rates. In addition, the event study results show little evidence of pre-trends, further contributing to a causal interpretation of the results presented in the main analysis.

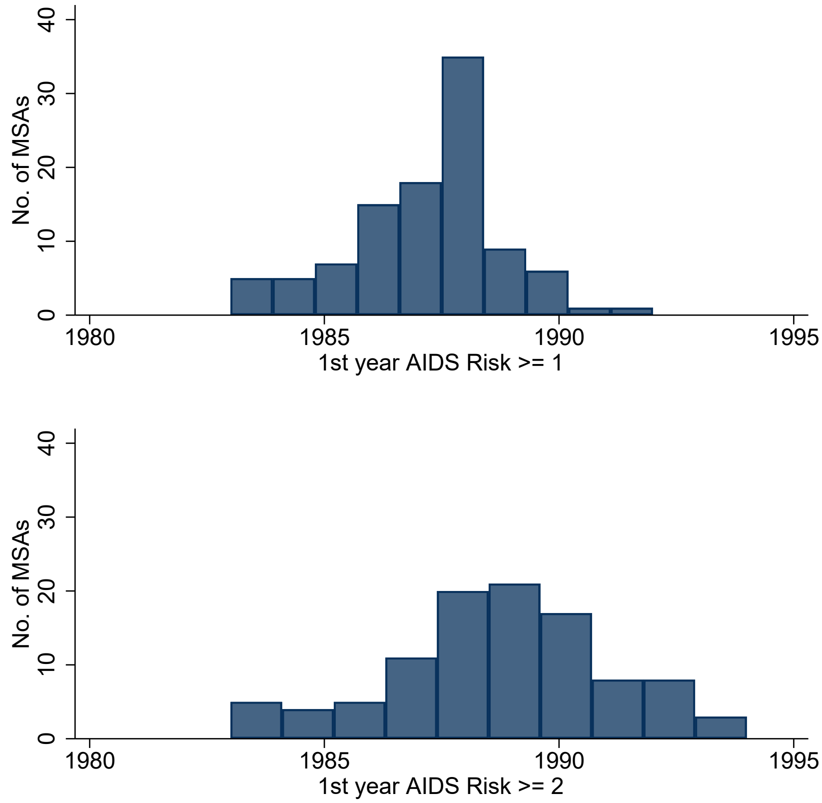

FIGURE E1: DISCRETE VARIATION IN THE TIMING OF AIDS SPREAD

Notes: This figure shows the year that AIDS risk reached 1 case per 100,000 women and 2 case per 100,000 women, respectively, for the 102 MSAs in the sample. Not that AIDS risk is defined as the number of new AIDS diagnoses per 100,000 women aged 20-44 in the previous year.

Table E1: Effect of AIDS Risk on Birth Rates by Timing of AIDS Spread

	Difference-in-Difference Estimates					
	(1)	(2)	(3)	(4)	(5)	(6)
Post AIDS Risk >= 1	0.795*	0.785**	1.572***			
	(0.458)	(0.350)	(0.529)			
Post AIDS Risk >= 2				1.197***	0.896***	1.940***
				(0.400)	(0.326)	(0.481)
N	2142	2142	3366	2142	2142	3366
MSA and year FEs	X	X	X	X	X	X
Controls included		X			X	
MSA-specific linear trends			X			X

Notes: This table presents difference-in-difference estimates of the effect of AIDS risk on birth rates by the timing in which AIDS risk reaches 1 and 2, respectively, in each MSA. The positive effect of AIDS risk on birth rates is robust to this alternative specification. Regressions in columns (2) and (5) include the full set of controls as described in Appendix Table A3, as well as MSA and year fixed effects. When including MSA-specific fixed effects, the sample size increases because I include more pre-AIDS years. Robust standard errors are clustered at the MSA level and shown in parentheses. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

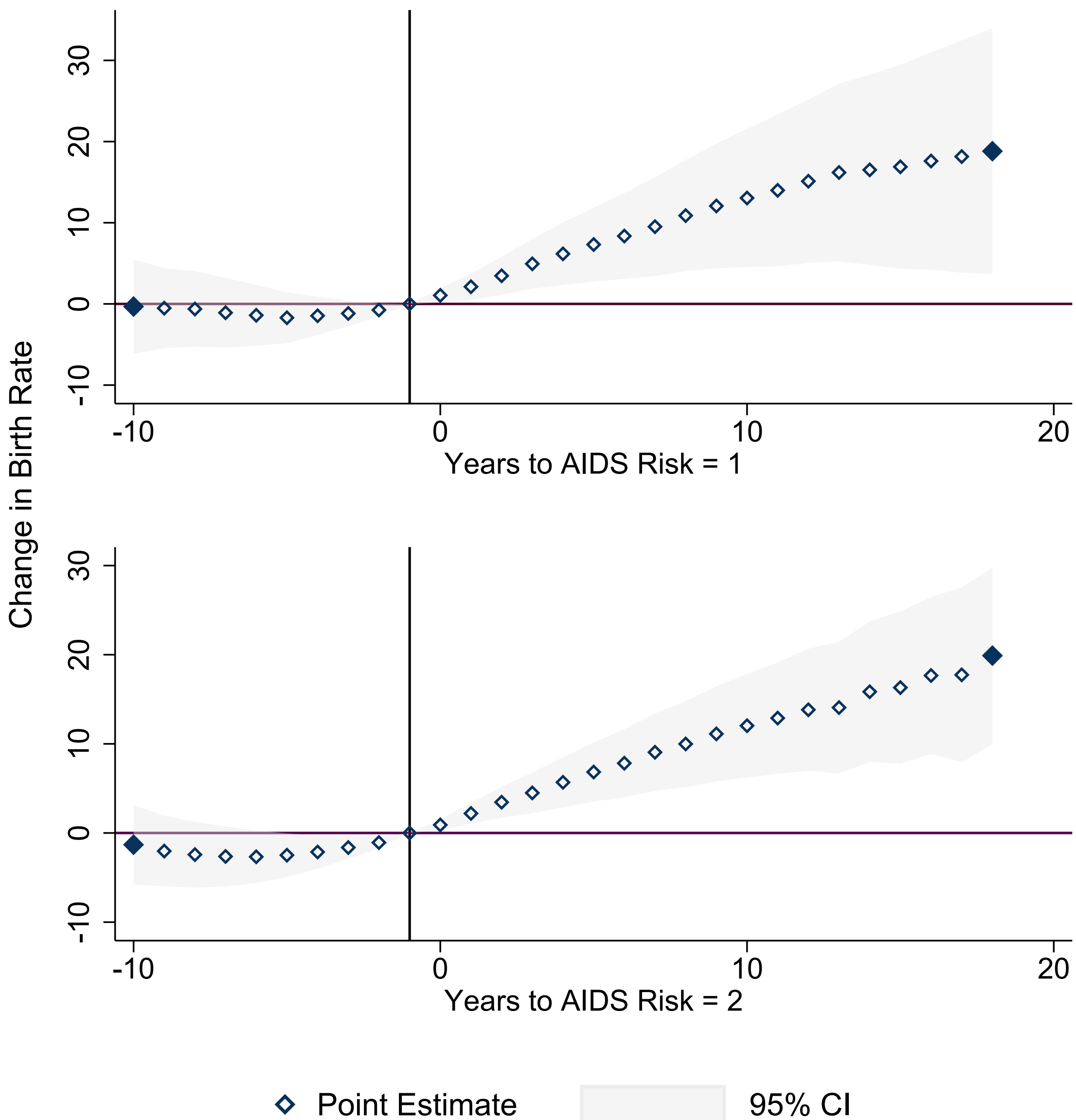

FIGURE E2: EVENT STUDY ANALYSIS

Notes: This figure shows the results from an event study analysis from the year that AIDS risk reached 1 and 2 per 100,000 women, respectively, in each MSA. Results show little evidence of pre-trends and confirm a positive effect of AIDS risk on birth rates. Estimates come from regressions which include MSA and year fixed effects with robust standard errors clustered at the MSA level.

Chapter 2: Effects of the COVID-19 Pandemic on Domestic Violence in Los Angeles

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Abstract

Around the world, policymakers and news reports have warned that domestic violence (DV) could increase as a result of the COVID-19 pandemic and the attendant restrictions on individual mobility and commercial activity. However, both anecdotal accounts and academic research have found inconsistent effects of the pandemic on DV across measures and cities. We use high-frequency, real-time data from Los Angeles on 911 calls, crime incidents, arrests, and calls to a DV hotline to study the effects of COVID-19 shutdowns on DV. We find conflicting effects within that single city and even across measures from the same source. We also find varying effects between the initial shutdown period and the one following the initial re-opening. DV calls to police and to the hotline increased during the initial shutdown, but DV crimes decreased. The period following re-opening showed a continued decrease in DV crimes, as well as decreases in arrests for those crimes and calls to the police and to the hotline. Our results highlight the heterogeneous effects of the pandemic across DV measures and caution against relying on a single data type or source.

Keywords: Domestic violence, COVID-19, pandemic, crime reporting, police data

JEL Codes: I18, J12, J16, K14, K42, R28

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1. INTRODUCTION

This paper uses high-frequency, real-time data from Los Angeles (LA), California, to study how domestic violence (DV) has been affected by the COVID-19 pandemic. We focus on LA because it is unique among major US cities in providing data on multiple DV measures from both police and non-police sources. We address two main aims. The first is an empirical determination of what impacts the pandemic and pandemic-related shutdown polices had on DV in LA. The second is epistemic, about the value and limitations of using readily available real-time administrative data to learn about the ongoing pandemic.

The study is motivated by the widespread concern that the COVID-19 pandemic, and especially that government-mandated restrictions on economic activity and personal mobility, would increase DV and trap victims with their abusers. The concern has featured prominently in news coverage of the pandemic going back to the first lockdown in China (Graham-Harrison, Athens, and Ford 2020; Vanderklippe 2020; Allen-Ebrahimian 2020; Taub 2020; Townsend 2020) and in policy responses to the pandemic by international organizations (UN Women 2020; FIFA, EC and WHO 2020) and governments around the world (Kottasová and Di Donato 2020). In the US, the Coronavirus Aid, Relief, and Economic Security (CARES) Act, passed on March 27, 2020, included $47 million of supplemental funding to support public policy responses to DV under the 1984 Family Violence Prevention and Services Act (FVPSA; Title III of P.L. 98-457) Program, an increase of 24% relative to appropriations from FY2020 and FY2019 (Fernandes-Alcantara and Sacco 2020).[1] The expectation that shutdowns in particular will increase DV has also been cited as a reason against imposing them (Friedman 2020; Lomborg 2020).

This attention to DV within the scope of COVID-19 analysis and policy is natural given the significant economic and social costs of DV (Fearon and Hoeffler 2014; Garcia-Moreno and Watts 2011; Max et al. 2004) and the risks that the pandemic could increase violence. The COVID-19 pandemic in the US has already been shown to affect a range of behaviors and outcomes that could affect DV, including mortality risk (Weinberger et al. 2020), unemployment and economic distress (Bitler, Hoynes, and Schanzenbach 2020), and gender equality (Alon et al. 2020). It is

[1] The FVPSA allocation entailed $45 million to formula grants for shelters and support services for DV survivors (26% increase) and $2 million to the National Domestic Violence Hotline (17% increase), but no funding for the Domestic Violence Prevention Enhancement and Leadership Through Alliances (DELTA) program. The CARES Act also included a moratorium on evictions of certain tenants, including some covered under the 1994 Violence Against Women Act (VAWA; Title IV of P.L. 103-322), though it did not provide funding for VAWA programs or for crime victim support under the 1984 Victims of Crime Act (VOCA; P.L. 98-473).

predicted to have severe mental health consequences as well (Pfefferbaum and North 2020; Galea, Merchant, and Lurie 2020). Prior economics research on DV suggests that these factors might increase DV incidence (Card and Dahl 2011; Berg and Tertilt 2012).

A further concern is that the pandemic increased the costs to victims of reporting crimes to authorities or leaving the household and made it more difficult for victims to access support services. Lower reporting rates could exacerbate the risk of abuse by reducing the expected punishment (Becker 1968) and make it difficult for authorities to detect and respond to an increase in violence. If DV increases but reporting rates are significantly depressed by the pandemic, it is unclear if we should expect official reports of DV to increase or decrease, and how we should interpret either finding. As a result, both increases and decreases in reported DV rates could be troubling: the increase in cases is taken as a sign of increased prevalence while the decrease is a sign of depressed reporting (e.g., Stone, Mallin, and Gutman, 2020, Li and Schwartzapfel 2020).

This difficulty is not limited to the pandemic. Rather, under-reporting is a persistent challenge for studying and mitigating DV. When possible, researchers have employed two main strategies to address the measurement challenge. They have either focused on fatal outcomes, such as homicide and suicide, that are reported nearly universally (Stevenson and Wolfers 2006; Miller and Segal 2019; Iyengar 2009; Aizer and Dal Bó 2009) or they have relied on data from other sources, primarily medical records (Aizer 2010; Miller and Segal 2019) and victimization surveys (Stevenson and Wolfers 2006; Miller and Segal 2019). These sources tend to be distributed only with substantial time delays (on the order of a year or two), so are not feasible for guiding current policy.[2] Studies relying on police reports have typically assumed no effects on either reporting (Card and Dahl 2011) or on incidence (Iyer et al. 2012; Muchow and Amuedo-Dorantes 2020) to interpret results. Unfortunately, neither assumption is plausible in this setting.

The interpretation of data on reported crimes is further complicated by the fact that it is not necessary that the pandemic and associated policy responses will increase DV or that they will lower reporting rates. For example, shutdowns may have lowered violence among ex-partners and couples who are not cohabiting, by reducing their time spent together. The pandemic may have

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[2] These sources also have important limitations when they are available. Fatal outcomes are relatively rare and may miss substantial variation in non-fatal outcomes. Even larger surveys tend to have relatively few observations of crime and injury outcomes, preventing any conclusive analysis of smaller metropolitan or non-metropolitan areas. There may also be concerns that the COVID-19 pandemic is altering survey response rates or inducing additional recall bias for past crimes. The COVID-19 could also induce changes in rates at which DV victims seek medical care, which would bias data from hospital or medical records.

also slowed the rate of formation of new relationships, which could also lower (or delay) violence. There could be a deterrence effect if the expected cost to abusers increased, either because of the risk of contracting the virus in jail if arrested or because of higher costs to them from having the relationship end. Reporting rates could have also increased because of increased public attention to the issue of DV in news articles, such as the ones cited above, and also advertisements and the dissemination of information about DV risks and support services in official federal, state and local public health resources related to the pandemic. Reporting by neighbors or other witnesses may have also increased if they became more aware of, or concerned about, continuing violence during the shutdown. The potential presence of these offsetting effects implies that the overall impact of the pandemic in any location will be an average across individuals in that location for whom the effects may have opposite signs.

This theoretical ambiguity is also reflected in the inconsistency in the observed effects of the pandemic on DV. Some cities experienced increases in DV measures, while others saw decreases (Taub 2020). Emerging academic studies of the pandemic and DV using different outcomes, data sources, and locations tend to report increased rates of DV during the pandemic: Leslie and Wilson (2020) and Sanga and McCrary (2020) show increased domestic calls to police in pooled samples of US cities; Agüero (2021) and Perez-Vincent et al. (2020) show increased calls to DV hotlines in Peru and Argentina, respectively; Ravindran and Shah (2020) finds increased DV complaints in India; and Piquero et al. (2020) shows increased domestic incidents in Dallas. However, the findings have not been universal and studies have reported decreases in DV crime rates in Chicago (Bullinger, Carr, and Packham 2020) and in Austin, Chicago, Nashville, and San Francisco (Abrams 2020); Silverio-Murillo, Balmori de la Miyar, and Hoehn-Velasco (2020) finds significant drops in both hotline calls for DV legal aid and in DV police reports across the 16 municipalities in Mexico City.

This paper is the first to use multiple measures of DV, including police and non-police sources, from a single major US city to determine whether variation in findings is coming from differences across measures or just differences across cities. Our police measures are DV calls for service, crime incidents, and arrests; our non-police measure is calls to the county DV hotline. We depict daily variation these outcomes between January 1 and August 24 in the years 2018, 2019 and 2020 using figures that show the three key time periods of pandemic shutdown policy. We estimate the effects of the shutdown using difference-in-differences comparing average changes in 2020 between the pre-shutdown period, the initial shutdown (March 19 – May 28), and the period following initial re-opening (May 29 – August 24), to changes in prior years, controlling for variation across months and by day of the week. We focus on shutdowns for our explanatory variables because they have been the primary state and local policy responses to the pandemic that are predicted to affect DV.[3]

We find significant effects of the initial shutdown on DV, but the direction differs across the outcomes: DV calls to police and the hotline both increased, but DV crimes decreased. This divergence within a single city suggests that extreme caution is warranted before extrapolating from a single outcome measure. Bullinger et al. (2020) examines two DV measures from police data in Chicago (calls and crimes) and also finds conflicting effects between the two outcomes. Outside the US, Ivandic et al. (2020) finds increases in both DV calls and crimes in London police data, but a decrease in crimes involving ex-partners (consistent with expectations). Perez-Vincent et al. (2020) studies calls to a Buenos Aires DV hotline and finds an increase in calls from DV victims, but a decrease in calls from police.

To our knowledge, this paper is also the first to separately measure the effects of the two initial phases of COVID-19 pandemic policy: shutdown and re-opening. If the economy recovered somewhat and individual stress levels decreased, we should expect the initial effects of the shutdown to diminish after restrictions are lifted. To the extent that reporting was suppressed during the shutdown, there may be a short-term burst of delayed reports after re-opening. Although we find significant changes between the initial shutdown and the period after initial re-opening in LA, they are all reductions, which does not support increased reporting. The previous growth in calls to police and to the hotline recedes during reopening, while the decline in DV crimes further deepens.

In addition to studying overall effects, we also examine different categories of police calls and crimes to examine variation in DV severity. We find that the increase in police calls is

[3] Shutdowns limited mobility, business and social activity, and coincided with school closures. Each of these could affect DV and DV reporting. As public health guidance shifted on the value of face coverings, mask mandates were also introduced in several states, mainly in summer of 2020. By October 1, 2020, 34 states and DC had implemented statewide mask mandates and only one of those (in Mississippi, on September 30) had been removed. California imposed a mask mandate on June 18, 2020 for indoor settings outside the home. We do not study mask policies because they are unlikely to directly affect DV. They may contribute indirectly by reducing transmission rates for a given level of economic activity, and therefore by enabling relaxation of other restrictions. As a result, our examination of mechanisms in Section 4.4 may capture some effects of mask mandates by including measures of infection rates and economic activity.

primarily driven by calls for less severe crimes. The estimated drop in crimes is larger in absolute terms for less serious crimes, but not in proportion to their baseline rates in the prior years.

Finally, we examine mechanisms related to the pandemic and policy responses that contribute to the total effects by adding contextual variables to our regression models that measure key factors associated with the pandemic that could have contributed to the overall effects on DV. We find that school closures significantly increase all of our measures of DV in LA. The effects of recent COVID-19 cases are mixed, increasing 911 calls and crimes, but not hotline calls, and lowering arrests (per population and per crime), consistent with policing intensity dropping in response to disease risk, but not to shutdown policy itself. Political protests also have mixed effects, increasing crimes but decreasing hotline calls, with insignificant effects on 911 calls and arrests. The mobility drop that preceded the shutdown by 5 days is negative and significant for DV crimes, showing that the decline started before the shutdown. Higher unemployment increases hotline calls, but not the police measures.

Although each mechanism is operative on some outcomes, accounting for them leaves most of the effects of the shutdown unexplained. In some cases (crimes and calls in the post-shutdown period), the mechanisms go against the direction of the overall effect, so accounting for them increases the size of the effect, making the unexplained impact larger than the total. Several studies have estimated heterogeneous effects, by location characteristics or timing of reporting, to investigate potential subgroups that are more or less affected by the shutdown (Bullinger, Carr, and Packham 2020; Ivandic, Kirchmaier, and Linton 2020). However, we are not aware of prior studies that have used contextual variables to decompose the effects of pandemic shutdowns.

2. DATA DESCRIPTION

We focus on LA because of its importance and the depth of publicly available real-time data on measures of DV. Nearly 4 million people live in the city and an additional 6 million are in the surrounding county. Relative to the rest of the country, LA experienced early exposure to the COVID-19 pandemic and responded quickly with strict restrictions. One of the earliest confirmed COVID-19 cases in the US was in LA in late January (Fox11 News 2020). On March 19th, California implemented a stay-at-home order and closed all non-essential businesses (California 2020). The city of LA also publishes real-time data from the LA Police Department (LAPD) on individual 911 calls, crime incidents and arrests that allows researchers to distinguish between domestic and non-domestic cases, and to categorize both DV calls and crimes by severity. In addition to the police data, we also obtained a measure of DV in LA that we were not able to obtain for other cities: calls to a DV hotline. This measure captures DV cases that are not necessarily reported to police.

2.1 *Police Dispatches*

Our data source for LAPD dispatches (also referred to as 911 calls or calls for service) is the Los Angeles Open Data Portal.[4] The data are updated weekly and include dispatch-level information on call type, dispatch date and time, reporting district, and area of occurrence. We use call type codes and textual information on call type descriptions to identify domestic-related dispatches. Specifically, we define a domestic-related call as any call for which the description contains the phrases “Dom Viol” or “Family.” Within the set of domestic-related calls, we are able to further identify the nature of calls using call type codes. Domestic-related calls with codes 245 indicate aggravated assault, 242 indicates simple assault, and 620 indicates dispute. We use this information to create four variables: all domestic-related 911 calls, domestic and family dispute calls, domestic aggravated assault calls, and domestic simple assault calls. Call counts are aggregated at the daily level and presented per 100,000 people within the LAPD jurisdiction. Data on population served are from the Uniform Crime Report’s 2018 Law Enforcement Officers Killed and Assaulted (LEOKA).

2.2 *Crime Incidents and Arrests*

Our data source for LAPD crime incidents and arrests is also the Los Angeles Open Data Portal.[5] The data are at the incident-level and are updated weekly. For each incident, we observe up to four different crime codes, the date the incident was reported, and modus operandi (MO) codes. We make use of all available information to determine if a crime is domestic (i.e., using all four crime codes plus the MO code). We categorize the severity of incidents by type of crime based on the most severe crime reported for each incident.

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[4] The calls for service data used in this analysis is publicly available at <https://data.lacity.org/A-Safe-City/LAPD-Calls-for-Service-2020/84iq-i2r6>. Data presented here were downloaded on September 14, 2020.

[5] The crime incident data used in this analysis is publicly available at <https://data.lacity.org/A-Safe-City/Crime-Data-from-2010-to-2019/63jg-8b9z> and <https://data.lacity.org/A-Safe-City/Crime-Data-from-2020-to-Present/2nrs-mtv8>. Data presented here were downloaded on September 14, 2020. To the extent that arrest information is added with some delay, arrest outcomes at the end of our sample period may not reflect ultimate outcomes for those incidents.

Based on conversations with the LAPD, we have determined that there are two ways that domestic incidents will appear in the data. The first is with a DV-specific crime code: DV aggravated assaults are code 236 and DV simple assaults are code 626. The second is using the MO code of 2000. An MO code of 2000 accompanied by a crime code of 230 indicates DV aggravated assault, while an MO code of 2000 and a crime code of 624 or 625 indicates DV simple assault. We also observe non-assault crimes with the MO code 2000. We split these crimes into two groups: crimes that are more severe than assault and crimes that are less severe than assault. The more severe crimes are homicide, rape, robbery, and kidnapping. The less severe category includes thefts, vandalism, threats, and other misdemeanor crimes. We group these crimes into a category of crimes less severe than assault.

We use this information to create six variables: all DV crimes, DV assault crimes, DV aggravated assault, DV simple assault, more severe DV crimes, and less severe DV crimes. Crime incident counts are aggregated at the daily level and presented per 100,000 people within the LAPD jurisdiction. We also track whether crimes resulted in an arrest and examine arrest rates per population, as well as per incident, for various types of DV crimes. Finally, we compare DV assaults to non-DV assaults. The non-DV assaults are defined as crimes with code 230, 624, or 625 that do not have an MO code of 2000. We divide DV assault crimes by the total number of assault crimes to obtain the share of assaults that are domestic.

2.3 *DV Hotline Calls*

The LA County Domestic Violence Hotline, housed within the LA County Department of Public Health, acts as a switchboard to connect domestic violence victims with local agencies (i.e., shelters, legal aid, etc.). Callers to the hotline are prompted to enter their zip code. This information is then used to transfer their call to a designated DV agency in their area. The hotline is completely computer operated; callers only speak directly with a person after they are connected with a local agency. We have data from the hotline on hourly call counts going back to January 2018.[6] Because the hotline serves the entire county, we use the county population to compute our measure of calls per 100,000 population.[7]

[6] We are missing hotline data from April 1-7 and July 1-14 in 2018.
[7] Note that 911 call and crime incident data is for the city of Los Angeles, while the Hotline serves both the city and the county of Los Angeles.

Descriptive statistics for our outcome variables are presented in Table 1. We provide means and standard deviations for outcome variables in 2018 and 2019 to show the pre-pandemic rates.

2.4 *Explanatory and Contextual Variables*

Our primary explanatory variables for this analysis are the dates of initial shutdown (March 9) and re-opening (May 29). We pool together the period from May 29 to August 24 as following the initial shutdown, though we note that there was another shutdown on July 13, followed by re-opening on September 2, right after our sample period. The additional contextual variables used to study mechanisms are from several sources. We use data from the *New York Times* on daily county-level counts of new COVID-19 infections to create a measure of new infections in the prior 14 days in LA County.[8] Our data on school closures are from the Los Angeles Unified School District instructional calendars. We measure MSA-level unemployment for non-institutionalized civilians aged 16 and older from the CPS monthly files (Flood et al. 2020). Because the reference week for the CPS is generally the calendar week that contains the 12th day of the month, we match the first 18 days of the month with the prior month's CPS and the rest of the days with the current month. We control for the change in mobility, following the coding in Sanga and McCrary (2020), with an indicator for the date of the major initial national decline in mobility on March 14, 2020. Finally, we control for political protests and violence using data from the US Crisis Monitor data compiled by the Armed Conflict Location & Event Data Project (ACLED).[9] Our measure captures the total number of demonstrations (protests and riots) in the county, over the prior 14 days, scaled to county population.

3. EMPIRICAL APPROACH

Our main analysis centers on the measuring variation in two discrete time periods in the course of pandemic shutdown policy. The first period is the initial shutdown, when substantial restrictions to business and personal activity were imposed, which was on March 19, 2020 in LA. The second period we define is the one following the easing of the restrictions associated with the initial shutdown. We call the period from May 29 to August 24, 2020 the post (initial) shutdown period.

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[8] <https://github.com/nytimes/covid-19-data>

[9] Accessed at <https://acleddata.com/special-projects/us-crisis-monitor/> on October 14, 2020.

The pandemic itself could affect economic, social and psychological outcomes, and that could influence DV incidence or reporting. COVID-19 illness and mortality in a household could certainly affect DV outcomes, as could heightened fear and anxiety or behavioral responses to the risk. While it is possible to control for the measured disease burden and implied risk in a local area using data on new cases (and we do this in our exploration of mechanisms in Section 4.4), it is difficult to measure the subjective perceptions that individuals hold about those risks. Although California was among the first states to experience local COVID-19 transmission and deaths, the cumulative reported case numbers were still below 2.5% of the county population by the end of our sample period (August 24, 2020). This means that the direct effect of cases may be less likely to affect DV outcomes than the responses to increased disease risk. Because of that, and the fact that shutdown policies could shift perceptions about risk and seriousness and increase the salience of the pandemic in the population, even without changes in cases, it is empirically difficult to isolate the impact of shutdowns from changes in risk. Rather than attempting to do that, we focus on estimating the impact of shutdown policy variation, with the understanding that the mandates can have both direct effects by proscribing certain activities as well as indirect effects related to shifting perceptions.

Our basic empirical model takes a day as the unit of analysis and regresses various DV outcomes, scaled to population, on indicators for days that follow the start of the initial shutdown (*$InitialShutdown_t$*) or that follow the initial re-opening (*$PostInitialShutdown_t$*). The estimation equation is:

$$(1)\ \mathrm{DV}_t = \beta_1 InitialShutdown_t + \beta_2 PostInitialShutdown_t + \boldsymbol{y_t} + \boldsymbol{m_t} + \boldsymbol{d_t} + \varepsilon_t$$

The β_1 coefficient is a difference-in-differences estimate of the average change in outcomes between the initial shutdown in 2020 and the earlier part of the same year, compared to the average seasonal variation between those periods in the two prior years. The β_2 coefficient is the difference-in-differences estimate for the change in the post-shutdown period, relative to the initial shutdown period, between 2020 and the two prior years. We include a vector of year fixed effects $\boldsymbol{y_t}$ and account for seasonal and within-week variation with month ($\boldsymbol{m_t}$) and day of week ($\boldsymbol{d_t}$) fixed effects.

Our basic model considers each shutdown period as a whole, notwithstanding the variation in restrictions during the initial period and following the initial re-opening. We therefore supplement our regressions with figures that plot daily variation in outcomes and to depict the variability within each of the three main time periods. The figures show smoothed (7-day moving average) measures of each of our outcomes of interest over the period from January 1 to August 24. The bold red line in each figure is for 2020. Data from two prior years (2019 and 2018) are shown (in black and grey) to provide a benchmark for seasonal variation the figures. Vertical lines depict the start and end of the initial shutdown period.

After presenting the overall effects of the pandemic shutdowns, using figures and regressions, we then expand our regression analysis to explore the mechanisms underlying the overall effects. We do this using data related to potential pathways for the COVID-19 pandemic policy to affect DV outcomes and then assessing their contributions to the overall impacts we find. In particular, we estimate an expanded version of equation 1:

$$(2)\ \mathrm{DV}_t = \beta_1 InitialShutdown_t + \beta_2 PostInitialShutdown_t + \boldsymbol{\beta_3 X_t} + \boldsymbol{y_t} + \boldsymbol{m_t} + \boldsymbol{d_t} + \varepsilon_t$$

The second model is the same as the first, with the addition of a vector of controls $\boldsymbol{X_t}$ containing these elements: indicator for school closure, including weekends and holidays; MSA-level monthly unemployment rate; indicator for dates after the initial national mobility drop on March 14, 2020; number of new COVID-19 cases in the county over the prior 14 days, scaled to population; and the number of political protests and riots in the county over the prior 14 days, per 100,000 people. The interpretation of the β_1 and β_2 coefficients in this model is shifted from the overall effect of the pandemic policies (in equation 1) to the *unexplained* portion of the effect that is not attributable to variables in the included $\boldsymbol{X_t}$ controls.

4. ESTIMATED EFFECTS OF THE COVID-19 PANDEMIC ON DV IN LA

4.1 *Overall Effects by DV Outcome*

This section presents estimates for the total effects on of the shutdown policies on measures of each of the four types of DV outcomes we observe for LA. These are: (1) 911 calls, (2) crime incidents and (3) arrests from LAPD data, and (4) calls to the DV hotline. This section contains the high-level examination of all calls and crimes related to DV; we later examine calls and crimes separately by severity. For arrests, we first consider total daily DV arrests per population and then examine arrest propensities at the level of individual DV crime incidents.

We start with DV-related calls for service. Similar to prior papers, we find an increase in calls to police related to DV following the initial shutdown in LA. The volume of DV-related service calls was initially lower in 2020 than in the two earlier years, but there was a clear relative increase following the initial shutdown (Figure 1, Panel A). However, the initial increase in calls was followed by a larger decrease in the period immediately following the initial shutdown.

This pattern is also present in the regression results from equation (1), using the number of DV-related 911 calls to the LAPD, scaled to population 100,000 people served, reported in column 1 of Table 2. Calls increased by 0.54 per day (s.e. 0.08; a 13% increase relative to the 2018-2019 mean of 4.06 in Table 1) during the initial shutdown period and then declined by 0.69 (s.e. 0.08) from that relative peak. Comparing the post-shutdown period to the period before the initial shutdown, we find a significant ($p = 0.05$, reported in the final row of Table 2) decrease in call volume of 0.15, corresponding to 4% of the prior years' mean. This result highlights the importance of examining evolving public policy and of measuring effects beyond the immediate shock. The initial impact of the pandemic on DV police calls in LA was not reflective of the long-term or overall effects.

The contrast between the estimates for 911 calls and the next results for crime rates highlights the importance of studying multiple types of data. We find conflicting effects even within a single city, over the same time periods, and from the same data source. DV calls to police increased during the initial shutdown period (Figure 1, Panel A), but DV crimes decreased (Panel B). Furthermore, Panel A shows a reversal of the effect in the post-shutdown period of the effect on calls, while Panel B shows a continuation of the effect for crimes. Crime rates were initially similar across the three years (2018-2020), but there was a significant relative decline in 2020 starting in late March that persisted over the summer. The regression estimates in Table 2 (column 2) show a drop of 0.12 (s.e. 0.02) in the initial shutdown period, with a further incremental drop of 0.08 (s.e. 0.03) afterward. The total change in the post-shutdown period (relative to the pre-shutdown period) is a drop of 0.21 crimes ($p < 0.001$), corresponding to a 15% reduction in DV crimes compared to the 2018-2019 average of 1.34 in Table 1.

The effects on DV arrests, scaled to population, echo those on DV crimes. Panel A of Figure 2 shows a relative decline in 2020 during the initial shutdown that increased in size during the post-shutdown period. The regression estimates in Table 3, column 1, show a small and statistically insignificant decline of 0.01 (s.e. 0.02) in DV arrests during the initial shutdown, followed by a significant drop of 0.09 (s.e. 0.02) in the post-shutdown period.

The pattern for calls to the hotline is closer to the pattern for police calls than for crime incidents. Figure 3 shows a substantial increase in hotline call volume during the initial shutdown period with no counterpart in the prior years. The call level continued to grow during the shutdown period and then decreased sharply around the time of the initial re-opening. In contrast to the pattern in Figure 1 for police calls, however, the level of hotline calls remained well above average through the month of August. Regression estimates in Table 2 (column 3) confirm the significant increase of 0.22 calls (s.e. 0.02), the significant drop of 0.08 calls (s.e. 0.03) and the persistence of the elevated calls level in the post-shutdown period relative to the pre-shutdown period (0.22-0.08 = 0.14, significant at $p < 0.000$). The size of the initial increase corresponds to a 152% increase relative to the mean of .15 calls in 2018-2019 (Table 1). Even the smaller 0.14 increase in the post-shutdown period reflects 98% more daily calls than the average in the prior two years.

The estimated effects of the COVID-19 shutdowns in LA differ dramatically across the outcomes we considered. When comparing calls to police or to shelters, the magnitudes of estimates are quite different, particularly relative to the average rates of each call type, but the general directions are the same: first an increase and then a decrease. For crimes, however, the initial shutdown estimate has the opposite sign and the post-shutdown period shows an amplification of the initial effect rather than a reversal of it.

These stark differences, within a single city, highlight the limitation of relying on a single type or even a single source of data to measure the total impact of the COVID-19 shutdowns on DV. Nevertheless, these may still be consistent with more subtle impact of the shutdowns on DV. In the next sections, we use detailed information on DV calls and crimes available from the LAPD to examine variation within severity categories as we explore the forces driving the divergence between the two outcomes.

4.2 *Analysis of Severity of DV Calls and Crimes*

One way to reconcile the initial increase in DV calls and with no corresponding increase in DV crimes is that increase in calls came from disturbances or conflicts that were not actually criminal incidents. This could happen, for example, if there was increased reporting of DV to police by third parties (such as neighbors) who have limited information about the events inside the home (e.g., as found Ivandic, Kirchmaier, and Linton 2020 in London). They might have been more likely to call police during the shutdown because they are spending more time at home or because of exposure to informational campaigns about the danger of increased DV during the pandemic. Another possibility is that increased publicity around the issue of DV (or heightened concern about being shut-in with a potential abuser) increased the propensity of first-time (or low severity) victims to report incidents to police before they escalated to criminal incidents, consistent with heterogeneous effects found in Leslie and Wilson (2020) and Sanga and McCrary (2020). In that case, effective police intervention could lower rates of future crimes.

Both of these stories would result from more 911 calls coming from less severe incidents. Because we are unable to observe data on the underlying incident when a criminal incident report is not filed, we first examine severity information contained in the initial service call. The information could be erroneous for various reasons,[10] but it should also be systematically related to the underlying severity of the incident. We therefore split 911 calls, based on LAPD classifications, into the broad categories of domestic disputes and DV assaults and examine each outcome separately with figures and regressions.

As shown in Panel A of Figure 4, there is a clear increase in calls for domestic disputes during the shutdown, followed by a dramatic decline after re-opening. This is consistent with the changes in domestic calls coming from less severe incidents. However, Panel B of Figure 4 also shows a relative increase in DV calls classified as assaults during the initial shutdown period. The trendline for DV assaults in 2020 started at a much lower baseline than the lines for the prior two years. During the shutdown, the 2020 line increased to a level more similar to the prior two years, and then it declined again following re-opening. If we assume that the earlier baseline would have persisted absent the pandemic, then there is also a clear increase in calls classified as assaults.

The estimates for these categories of DV calls are in Table 4. The first column repeats the overall estimate from Table 2, while the next two columns decompose the estimate into contributions from domestic disputes (column 2) and assaults (column 3). Both show significant increases during the initial shutdown, but the magnitude of the increase in disputes is larger than the one for assault in both absolute terms (0.41 versus 0.13) and relative to the 2018-2019 baseline mean for that variable (in Table 1; 16% versus 8.4%).

When we further subdivide calls classified as assaults into simple and aggravated DV assaults, we find that the increase in assaults is coming from an increase in simple assaults. Column

[10] For example, violence may escalate between the 911 call and time that police arrive at the scene, or third-party callers may misunderstand the situation.

4 of Table 4 shows an increase in simple assaults of 0.13 (s.e. 0.04) during the initial shutdown (that is reversed afterward), while column 5 shows an increase in aggravated of only 0.003 (s.e. 0.017). Although calls for simple DV assaults are significantly more common than those for aggravated DV assaults (Table 1), the estimated increases during the shutdown are proportionally much larger for simple (10%) than for aggravated (0.8%). This pattern is similarly reflected in Figure 6, where simple DV assaults are in Panel A and aggravated DV assaults are in Panel B.

We also examine the severity of crimes recorded by the LAPD and find declines in both assaults (Panel A of Figure 5) and non-assault DV crimes (Panel B of Figure 5). The point estimates are negative and significant in both periods for both overall DV assaults and simple assaults. The point estimates for DV aggravated assaults (-0.020 and -0.018) are smaller than those for DV simple assaults (-0.055 and -0.059), and not statistically significant. This is reflected in Figure 6, where Panel A shows a clear decline in DV simple assaults and Panel B for aggravated DV assaults is much noisier. Despite their lack of significance, the estimated effects for aggravated assaults are proportionally larger. Relative to average crime levels in 2018-2019, DV aggravated assaults initially declined by 9.5% and then by a further 8.6%, while simple assaults declined by 6.5% and then 6.9%. In columns 5 and 6 of Table 5, we split non-assault DV crimes into those less severe than assault and more severe than assault. We find significant declines in those outcomes during the initial shutdown period with no additional change in re-opening. The point estimates are larger for less severe crimes, but the effects are proportionally larger for more severe (24.2%) than for less severe (13.8%) crimes. Overall, these results show reductions in both more and less severe crimes, with generally larger proportional effects in the more severe categories.

4.3 *Interpretation of Overall Effects on DV Outcomes in LA*

The results in the previous sub-section indicate that the increase in calls is coming primarily, but not exclusively, from less severe DV. This could either happen if less severe crimes are more responsive to the pandemic or if reporting rate increased more for those crimes. Evidence from victimization surveys shows that reporting rates are higher when the incident is more severe (e.g., Miller and Segal 2019, Appendix Table 2C). If the pandemic only lowered the severity threshold for reporting, it should shift the distribution of reported incidents toward less severe cases. To the extent that increase in calls is from cases that are less severe than the threshold for a crime, it is possible to reconcile increased calls with no corresponding increase in crimes.

But why are reported crime rates falling? One possibility is that crime incidence is declining. This could happen as a result of increased reporting (or even the expectation of a higher reporting rate on the part of potential abusers), even of non-crime DV incidents, if police interventions are effective at deterring escalation or if police assist victims in accessing supportive social and legal resources in the community.[11]

Community advocacy resources can also be accessed directly by calls to DV shelters through the hotline we study. In addition to the increase in general publicity around the issue of DV during the pandemic, there was special attention to the issue of emergency shelter housing for victims and a well-publicized initiative in the city to provide support services and hotel rooms as needed to supplement emergency shelter beds. The program, called Project Safe Haven, was announced by the LA mayor Eric Garcetti in his daily briefing on April 29, 2020 and funded in part by a $4.2 million donation from the singer Rihanna and Twitter CEO Jack Dorsey.[12] To the extent that the increased hotline calls in our data also reflect greater use of non-police community resources, there may be a natural relationship between hotline calls and lower crime. Under this interpretation of the data in LA, the policy implication is that there was capacity to improve DV outcomes in the city, through initiatives that increase reporting to police and non-police public services. The pandemic may have been an impetus in LA and other cities to pay more attention to DV victims and crime incidence may have decreased.

In contrast to this optimistic interpretation, it is also possible to explain the pattern in calls and overall crimes as coming from a decrease rather than an increase in reporting rates, coupled with an even larger increase in the incidence of violence. If reporting rates were depressed more for more serious crimes, perhaps among victims with fewer resources to exit the relationship and access police or other services, we might see an increase in calls for less serious incidents but a decline for more serious cases. It is impossible to use the publicly available data from LA to determine how the distribution of calls by severity maps into crime incidents by severity or DV coding and the extent to which this mapping is affected by the pandemic. It would be possible if

[11] This could happen, for example, through the Domestic Abuse Response Team (DART) partnership program between the LAPD and local victim advocacy organizations.
[12] See local news coverage at <https://losangeles.cbslocal.com/2020/04/09/rihanna-twitter-ceo-donate-4-2m-to-shelter-domestic-violence-victims-amid-coronavirus-pandemic/>.

LA provided a common identifier with which to merge the calls and crimes data, but they declined our request for this information.[13]

Another troubling possibility is that crime rates increased, and that is what drove the increase in reporting, but that police were less responsive and less likely to record domestic incidents as crimes in official data. Here again, data that tracks DV calls into various outcomes could shed light on the relevance of the story, but the data are not available to us. One possibility that we can explore is the effect on arrests.

One reason that police might avoid recording DV incidents as crimes is that they are reluctant to arrest abusers. This could be to protect offenders from the increased risk of COVID-19 infections in jails (Hawks, Woolhandler, and McCormick 2020) or from a desire to minimize their own exposure to potentially infected individuals. DV arrests per population declined somewhat during the initial shutdown (in Figure 3, Panel A and Table 3, column 2), consistent with less policing, but this decline could come from a decrease in crimes. We therefore examine policing intensity using incident level data on arrests to test if the pandemic depressed arrest rates, conditional on crime incidents. The incident-level regression estimates for all DV crimes are in column 2 of Table 3; Figure 2, Panel B shows similar information with the daily (7-day moving average) share of DV crimes leading to arrests. Neither shows any evidence of lower arrest rates during the initial shutdown. To examine if the decline is being masked by a shift in the mix of cases, we also estimate the incident-level arrest model for each of our sub-categories of DV crimes. The estimates are in the remaining columns of Table 3: assaults in column 3, simple assaults in column 4, and non-assault crimes in column 5. We see no decline in arrests for any of these outcomes and the coefficients are generally positive (and insignificant). The initial decrease in DV crimes is therefore unlikely to be explained by less intensive policing of DV overall. However, we do find negative estimates for arrest rates in the post-shutdown period, which suggests that less intensive policing might contribute to the persistence of the decrease in crime rates in the second period. We return to this in the next section on mechanism.

Before turning to mechanism, we first place the estimated decline in DV crimes in the broader context of violent crimes in the city. To increase comparability, we focus on assaults. Column 2 of Table 6 reports estimated effects of the pandemic on assaults that are not coded as

[13] We also requested records from internal reports maintained by the department on domestic incidents that are not considered crimes, but that request was also denied.

DV and column 1 repeats the estimates for DV assaults (from column 2 of Table 5). The decrease in DV assaults is also present for non-DV assaults in the initial shutdown period, and the effect is substantially larger in magnitude.[14] As a result, the share of assaults attributable to DV actually increased during the initial shutdown, despite the drop in DV crimes, as shown in column 3. In the period after the shutdown, DV assaults continued to decrease, while non-DV assaults increased back toward their pre-shutdown levels (but still lower by 0.17, $p = 0.003$). During that period, the DV share among assaults declined and was not significantly different from the pre-pandemic level. Figure 7 shows this pattern clearly in the smoothed data.

4.4 *Examination of Mechanisms*

As described above in Section 4.2, and shown in Panel A of Table 2, estimation of the model in equation 1 revealed significant overall effects of the pandemic on DV calls to police, crimes and hotline calls. The initial shutdown is associated with increases in DV calls to both the LAPD and the county hotline but with decreases in DV crimes. This section discusses results from estimation of equation 2 that includes measures of different components of the pandemic and associated policy response.

The estimates for total domestic 911 calls, DV crimes and hotline calls from the expanded model are presented in columns 4-6 of Table 2.[15] Panel B of Tables 3, 4, and 5 show the estimates for arrests and outcomes measuring subsets of DV calls or crimes. Columns 4-6 in Table 6 show non-DV assaults and the DV share among assaults.

School closure has a significant positive effect on the four total measures of DV: calls to police or the hotline, crimes, and arrests. It also increases DV calls of all types except for aggravated assault (where there was no overall effect of the pandemic) and DV assault crimes overall and simple assault. School closure has no estimated impact on non-DV assaults (column 5 of Table 6). This suggests that stress or other factors associated with having children at home lead to increased DV incidence or reporting. Because the initial shutdown increased the frequency of

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[14] Abrams (2020) studies a wide range of crimes in 25 cities (4 of which contain information on DV, mentioned above) and finds decreases in crimes following COVID-19 shutdowns; these are most pronounced for drug crimes, theft, residential burglary, and similar to our finding in LA, for violent crimes, including assaults.

[15] We also estimated an expanded version of these models with an indicator for observations after the initial re-opening ended and second shutdown started, on July 13. Because that indicator was not itself significant and did not alter the estimated effects of the other shutdown or contextual variables for any of the outcomes, we focus on the more parsimonious model in the tables.

school closures, this channel contributes to the increased 911 and hotline calls but does not explain the decrease in DV crimes.

The other variables have less consistent effects on the DV outcomes. Increased unemployment in the metropolitan area is not statistically related to any of the 3 main police outcomes, but it is positively associated with more calls to the DV hotline (and with more non-assault DV crimes). The mobility drop on March 14 is not significantly related to DV calls to police or the hotline, but it has a negative and significant point estimate for DV crimes (and for aggravated DV assaults). This indicates that some of the decline in reported DV crimes preceded the formal shutdown. COVID-19 cases (per population; new cases in the prior 2 weeks) in the county are significantly associated with increased DV 911 calls and crimes (as well as with increased non-DV assaults), but the coefficient on for hotline calls is negative and insignificant.

Political protests in the county (per 100,000 population) are associated with more DV crimes (overall, assaults and other DV crimes), but not with non-DV assaults or with DV police calls or DV arrests (per population or per incident), except for non-assault DV crimes. The estimate for hotline calls is also significant, but negative. Because protests increased primarily in the post-shutdown period, the negative effect for hotlines helps explain the decrease in calls in that period. For crimes, however, the protest effect goes against the continued decrease in crimes in the post shutdown period.

Accounting for the various mechanisms reduces the size of the initial increase in 911 DV calls by 41% (a 0.22 decline from a base of 0.54) and in hotline calls by 45% (a 0.10 drop from a base of 0.22), leaving more than half of the overall effect unexplained. For DV crimes, the mechanisms tend to go against the direction of the overall effect, so accounting for them produces a larger residual effect of the shutdown than otherwise: the estimate increases by 17% (0.02 from a base of 0.12). The post-shutdown declines in DV police calls and crimes grow larger in magnitude after accounting for the mechanisms, but the estimate for hotline calls is cut in half.

5. CONCLUSIONS

We find large effects of the COVID-19 pandemic on DV outcomes in LA, using high-frequency, real-time data from the LAPD and from the county's DV hotline. These effects vary over time, with the phases of the public policy response, and they vary across outcome measures. The initial shutdown increased DV calls to police and to the hotline, but decreased DV crimes and arrests for those crimes. The re-opening period showed a continued decrease in DV crimes and arrests, as well as decreases in calls to police and to the hotline. However, while the increase in DV calls to police from the shutdown period was entirely reversed in the next period, calls to the hotline remained elevated.

These types of variation, over time and across outcome measures, within a single city highlights the challenge that researchers face in attempting to measure the impact of an ongoing pandemic, or other emergency, on DV rates. Because of the heterogeneous effects within LA, we caution against using our results to predict effects in other cities.

Instead, we believe that our analysis highlights the need for greater transparency and distribution of police data in US cities. We focused on LA largely because of the relatively rich data provided by the authorities. Other cities that provide public data often provide less detailed information about calls (e.g., not distinguishing DV assaults from domestic disputes) and crimes (e.g., only reporting the most severe crime in an incident, only identifying assaults as domestic crimes) and many other departments provide no real-time public data. Without better data, researchers and policymakers have a weak evidentiary foundation on which to base their policy choices and resource allocations in response to the current ongoing pandemic or to future crises.

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FIGURES

Panel A: Domestic Dispatches | Panel B: Domestic Crimes

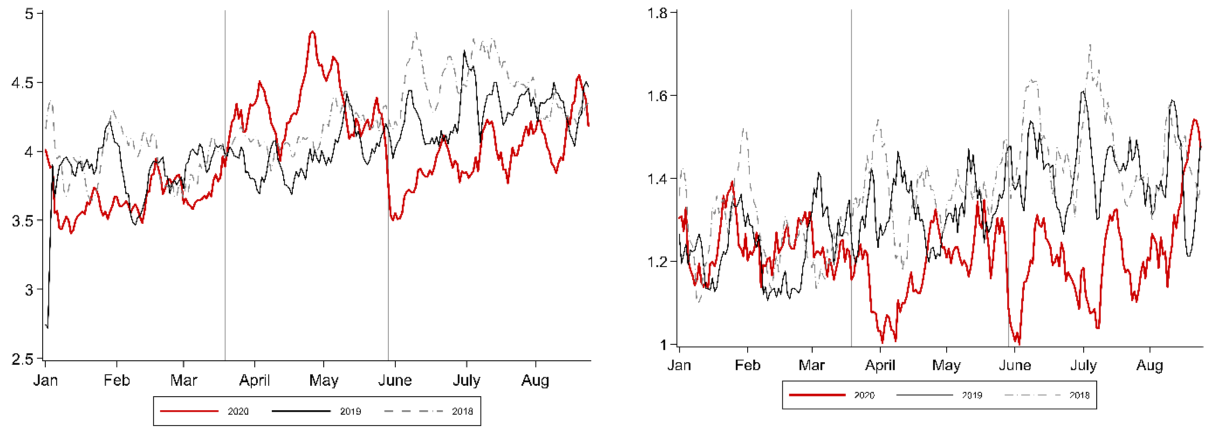

Figure 1: LAPD Domestic Dispatches and Domestic Crimes

Notes: Panel A shows LAPD dispatches for domestic-related 911 calls between January 1 and August 24 in 2020, 2019, and 2018. Panel B shows domestic crime incidents recorded by the LAPD over the same period. Calls and crimes are presented as 7-day moving averages per 100,000 population served by the LAPD. Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

Panel A: Total Domestic Arrests | Panel B: Share of Domestic Crimes with an Arrest

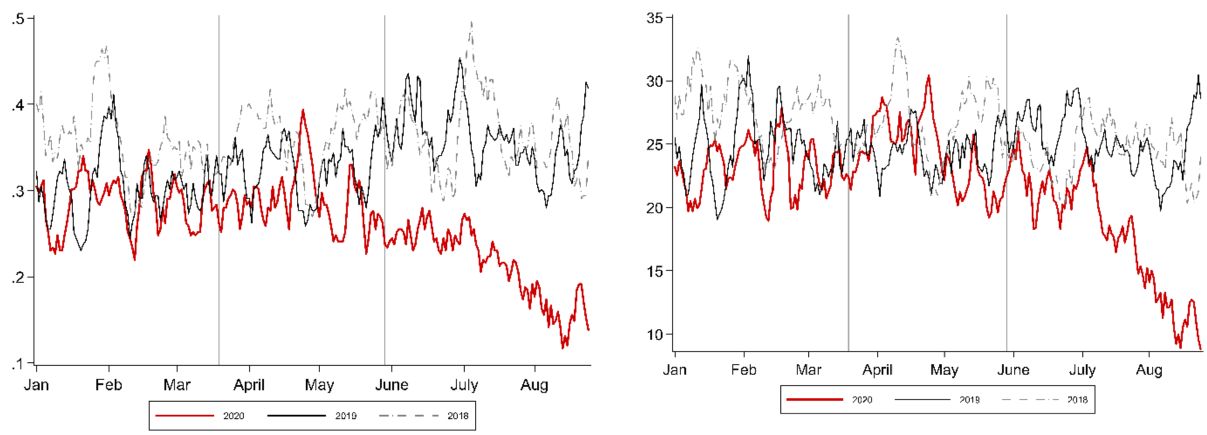

Figure 2: LAPD Arrests for Domestic Crimes

Notes: Panel A shows total LAPD arrests for domestic crimes between January 1 and August 24 in 2020, 2019, and 2018. Panel B shows the domestic crimes over the same period that resulted in an arrest. Arrests in Panel A are presented as 7-day moving averages per 100,000 population served by the LAPD. Shares in Panel B are also 7-day moving averages. Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

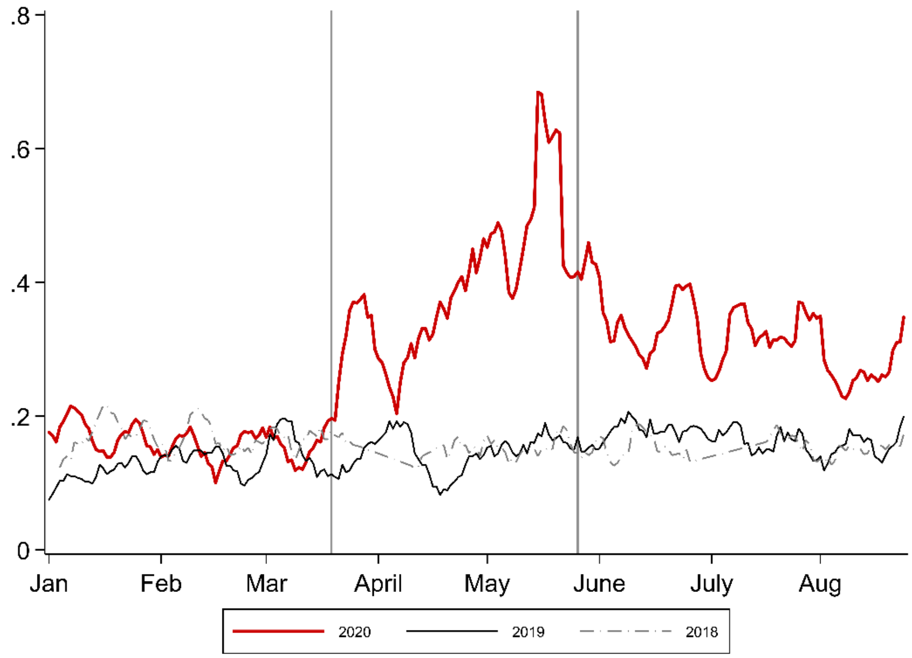

Figure 3: LA County Domestic Violence Hotline Calls

Notes: The figure shows daily calls to the LA County Domestic Violence Hotline between January 1 and August 24 in 2020, 2019, and 2018. Calls are presented as 7-day moving averages per 100,000 population in Los Angeles County Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

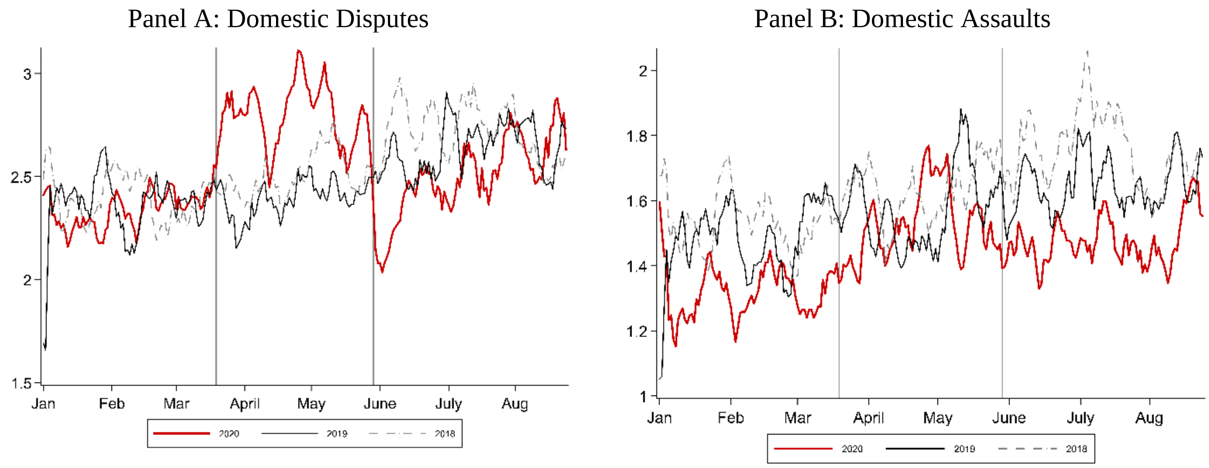

Figure 4: LAPD Dispatches for Domestic Disputes and Domestic Assaults

Notes: Panel A shows LAPD dispatches for domestic dispute and family fight calls between January 1 and August 24 in 2020, 2019, and 2018. Panel B shows LAPD dispatches for domestic assault calls over the same period. Calls are presented as 7-day moving averages per 100,000 population served by the LAPD. Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

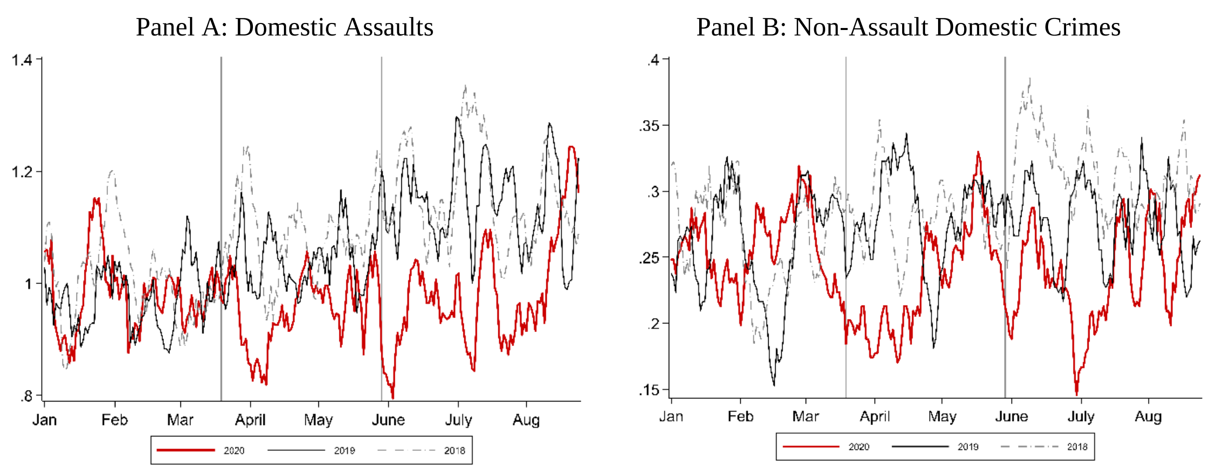

Figure 5: LAPD Domestic Assault and Non-Assault Crimes

Notes: Panel A shows domestic assault crime incidents recorded by the LAPD between January 1 and August 24 in 2020, 2019, and 2018. Panel B shows non-assault domestic crime incidents over the same period. Crimes are presented as 7-day moving averages per 100,000 population served by the LAPD. Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

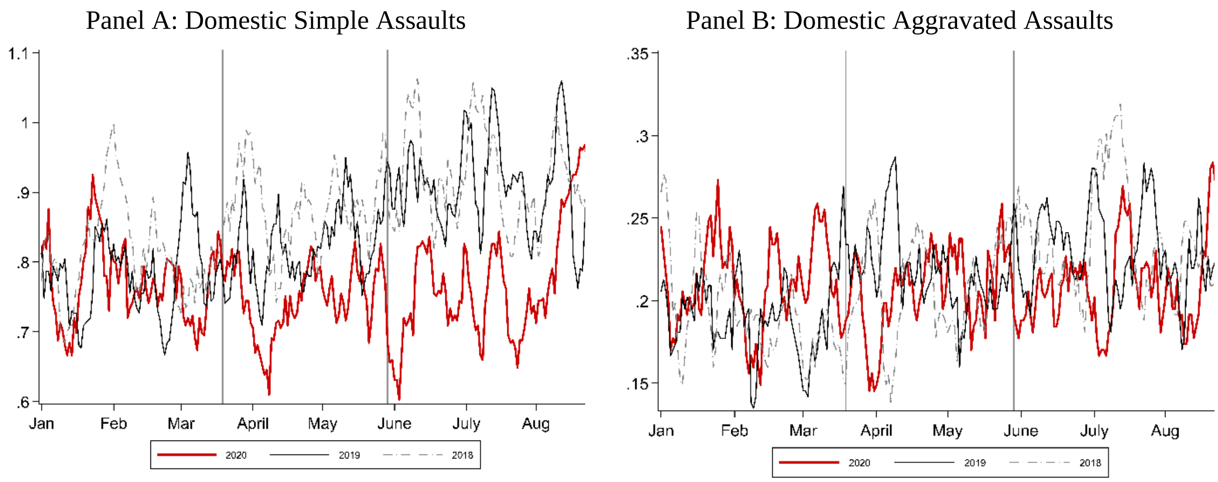

Figure 6: LAPD Domestic Simple and Aggravated Assault Crimes

Notes: Panel A shows domestic simple assault crime incidents recorded by the LAPD between January 1 and August 24 in 2020, 2019, and 2018. Panel B shows domestic aggravated assault crime incidents over the same period. Crimes are presented as 7-day moving averages per 100,000 population served by the LAPD. Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

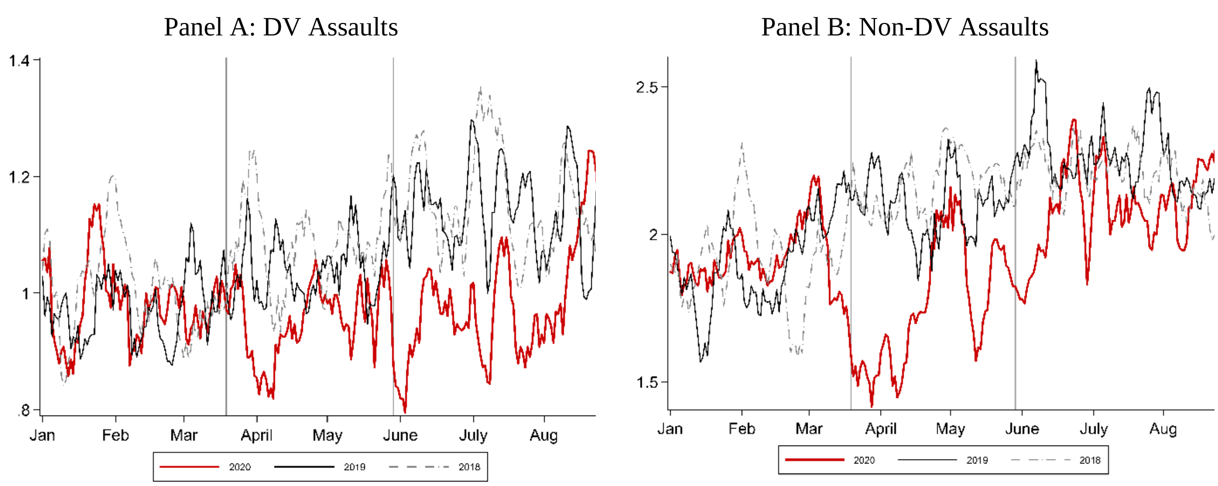

Figure 7: LAPD Domestic Assault and Non-Domestic Assault Crimes

Notes: Panel A shows domestic assault crime incidents recorded by the LAPD between January 1 and August 24 in 2020, 2019, and 2018. Panel B shows non-domestic assault crime incidents over the same period. Crimes are presented as 7-day moving averages per 100,000 population served by the LAPD. Vertical lines indicate the timing of the initial shutdown: beginning March 19th and ending May 28th.

TABLES

Table 1: Summary Statistics for Los Angeles

	Mean	Std. Dev.
	2018-2019	
911 calls to police		
All DV calls	4.06	0.61
DV assault calls	1.59	0.33
DV aggravated assault calls	0.32	0.10
DV simple assault calls	1.27	0.28
Domestic dispute calls	2.47	0.38
Non-DV assault calls	6.40	0.93
Non-DV aggravated assault calls	3.26	0.58
Non-DV simple assault calls	3.15	0.48
Crime incidents		
All DV crime	1.34	0.25
DV assaults	1.06	0.22
DV aggravated assaults	0.21	0.079
DV simple assaults	0.85	0.19
Other DV crime	0.27	0.083
DV crime, more severe than assault	0.054	0.037
DV crime, less severe than assault	0.22	0.08
Non-DV assaults	2.10	0.34
DV share of assaults	0.35	0.05
DV arrests	0.34	0.11
DV hotline calls	0.15	0.073
Contextual variables for all years		
Monthly MSA unemployment rate	0.043	0.0044
Public schools closed	0.49	0.50
	2020	
Contextual variables in 2020		
New COVID-19 Cases, Prior 14 Days	0.069	0.092
Political Protests and Violence, Prior 14 Days	0.13	0.22

Notes: Data on 911 calls, crime incidents, and arrests are from the LAPD and computed as daily rates per 100,000 city population. Hotline calls are daily per 100,000 county population and from the LA county public health department. Unemployment rate is MSA-level from the CPS. Public school closure is a daily indicator for public K-12 schools not being in session. COVID-19 case data are at the county-level from the *New York Times* and scaled to county population. Political protest data are a county-level sum over the past 14 days, scaled to 100,000 population, from the Armed Conflict Location & Event Data Project. Sample period is from January 1 to August 24.

Table 2: COVID-19 Shutdown Effects on DV Police Calls, Crimes and Hotline Calls in LA

	(1) 911 Calls	(2) Crimes	(3) Hotline Calls	(4) 911 Calls	(5) Crimes	(6) Hotline Calls
Initial shutdown	0.540***	-0.119***	0.223***	0.319**	-0.139***	0.123***
	[0.0834]	[0.0374]	[0.0247]	[0.149]	[0.0530]	[0.0307]
Post initial shutdown	-0.691***	-0.0876**	-0.0806***	-0.936***	-0.428***	-0.0429
	[0.0829]	[0.0407]	[0.0272]	[0.207]	[0.0974]	[0.0641]
School closed				0.179***	0.0827***	0.0250***
				[0.0550]	[0.0258]	[0.00875]
Unemployment				-0.125	0.230	1.199***
				[0.956]	[0.391]	[0.305]
Mobility drop				0.0923	-0.0914**	-0.00805
				[0.125]	[0.0454]	[0.0197]
COVID-19 recent cases				2.078**	1.715***	-0.152
				[0.817]	[0.394]	[0.201]
Political protests				0.0941	0.357***	-0.121**
				[0.224]	[0.104]	[0.0612]
Observations	709	709	709	709	709	709
R-squared	0.495	0.324	0.588	0.513	0.359	0.619
Pr(shutdown + post) = 0	0.054	0.000	0.000	0.0157	0.000	0.261

Notes: Unit of observation is a day. Outcomes are scaled to 100,000 population in city (columns 1, 2, 4,5) or county (columns 3, 6). Sample includes January 1 to August 24 in years 2018, 2019 and 2020. Initial shutdown date is March 19, 2020. Post-initial shutdown is May 29, 2020. All regression models include fixed effects for month, year and day-of-week. Models in columns 4-6 also include contextual variables for school closure, unemployment, mobility drop (March 14, 2020), and county level COVID-19 cases (scaled to population) and number of political protests (per 100,000 population) in the prior 14 days. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effects of COVID-19 Shutdowns on DV Arrests and Arrest Rates in LA

		Arrest for DV Incidents				
	DV Arrests	All	Assaults	Simple Assaults	Aggravated Assaults	Other DV Crimes
Panel A: Overall Effects, No Contextual Variables						
	(1)	(2)	(3)	(4)	(5)	(6)
Initial shutdown	-0.00835	0.0181	0.0169	-0.000037	0.0228	0.0209
	[0.0171]	[0.0116]	[0.0132]	[0.0315]	[0.0143]	[0.0243]
Post initial shutdown	-0.0923***	-0.0395***	-0.0451***	-0.0357	-0.0460***	-0.0192
	[0.0166]	[0.0109]	[0.0124]	[0.0301]	[0.0133]	[0.0228]
Observations	709	37,279	29,643	6,047	23,596	7,636
R-squared	0.324	0.005	0.006	0.011	0.006	0.005
Panel B: With Contextual Variables						
	(7)	(8)	(9)	(10)	(11)	(12)
Initial shutdown	-0.0202	0.0133	0.0162	-0.0475	0.0293	0.00297
	[0.0338]	[0.0306]	[0.0336]	[0.0863]	[0.0358]	[0.0731]
Post initial shutdown	-0.0444	0.0500	0.0345	-0.0773	0.0610	0.106
	[0.0332]	[0.0399]	[0.0442]	[0.108]	[0.0478]	[0.0930]
School closed	0.0214*	-0.000574	-0.00415	-0.00107	-0.00389	0.00877
	[0.0119]	[0.00710]	[0.00807]	[0.0198]	[0.00868]	[0.0148]
Unemployment	-0.00906	-0.0794	-0.0918	-0.165	-0.0830	0.0158
	[0.188]	[0.128]	[0.144]	[0.338]	[0.157]	[0.278]
Mobility drop	0.00769	0.0255	0.0232	0.0660	0.0176	0.0280
	[0.0330]	[0.0292]	[0.0322]	[0.0830]	[0.0341]	[0.0698]
COVID-19 recent cases	-0.326**	-0.532***	-0.500***	0.0498	-0.642***	-0.625***
	[0.135]	[0.0958]	[0.107]	[0.256]	[0.116]	[0.216]
Political protests	0.0291	-0.0370	-0.0139	-0.0278	-0.0107	-0.126*
	[0.0492]	[0.0334]	[0.0381]	[0.0885]	[0.0420]	[0.0679]
Observations	709	37,279	29,643	6,047	23,596	7,636
R-squared	0.334	0.006	0.007	0.012	0.007	0.006

Notes: In columns 1 and 7, the unit of observation is a day and outcome is DV arrests per 100,000 population. All other columns have a DV crime incident as the unit of observation and the outcome is an indicator for an arrest. Sample includes January 1 to August 24 in years 2018, 2019 and 2020. Initial shutdown date is March 19, 2020. Post-initial shutdown is May 29, 2020. All regression models include fixed effects for month, year and day-of-week. Models in Panel B also include contextual variables for school closure, unemployment, mobility drop (March 14, 2020), and county level COVID-19 cases (scaled to population) and number of political protests (per 100,000 population) in the prior 14 days. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of COVID-19 Shutdowns on DV Police Calls by Type in LA

	All	Disputes	Assaults	Simple Assaults	Aggravated Assaults
Panel A: Overall Effects, No Contextual Variables					
	(1)	(2)	(3)	(4)	(5)
Initial shutdown	0.540***	0.406***	0.133***	0.130***	0.00252
	[0.0834]	[0.0585]	[0.0451]	[0.0401]	[0.0168]
Post initial shutdown	-0.691***	-0.534***	-0.156***	-0.130***	-0.0262
	[0.0829]	[0.0631]	[0.0420]	[0.0378]	[0.0172]
Observations	709	709	709	709	709
R-squared	0.495	0.330	0.463	0.425	0.193
Panel B: With Contextual Variables					
	(6)	(7)	(8)	(9)	(10)
Initial shutdown	0.319**	0.290***	0.0284	0.0812	-0.0529*
	[0.149]	[0.106]	[0.0827]	[0.0828]	[0.0284]
Post initial shutdown	-0.936***	-0.758***	-0.173*	-0.141*	-0.0320
	[0.207]	[0.167]	[0.0941]	[0.0856]	[0.0380]
School closed	0.179***	0.117***	0.0639**	0.0556**	0.00828
	[0.0550]	[0.0377]	[0.0322]	[0.0282]	[0.0114]
Unemployment	-0.125	-0.394	0.249	0.199	0.0497
	[0.956]	[0.706]	[0.470]	[0.433]	[0.177]
Mobility drop	0.0923	0.0479	0.0459	-0.00220	0.0481*
	[0.125]	[0.0873]	[0.0799]	[0.0812]	[0.0263]
COVID-19 recent cases	2.078**	1.790***	0.284	0.151	0.133
	[0.817]	[0.661]	[0.370]	[0.328]	[0.147]
Political protests	0.0941	0.0915	-0.00521	0.0284	-0.0336
	[0.224]	[0.191]	[0.108]	[0.0979]	[0.0489]
Observations	709	709	709	709	709
R-squared	0.513	0.355	0.468	0.430	0.197

Notes: Unit of observation is a day. Outcomes are scaled to 100,000 population. Sample includes January 1 to August 24 in years 2018, 2019 and 2020. Initial shutdown date is March 19, 2020. Post-initial shutdown is May 29, 2020. All regression models include fixed effects for month, year and day-of-week. Models in Panel B also include contextual variables for school closure, unemployment, mobility drop (March 14, 2020), and county level COVID-19 cases (scaled to population) and number of political protests (per 100,000 population) in the prior 14 days. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects of COVID-19 Shutdowns on DV Crimes by Type in LA

	All	Assaults	Simple Assaults	Aggravated Assaults	Less Severe than Assault	More Severe than Assault
Panel A: Overall Effects, No Contextual Variables						
	(1)	(2)	(3)	(4)	(5)	(6)
Initial shutdown	-0.119***	-0.076**	-0.055*	-0.020	-0.0305**	-0.0131*
	[0.037]	[0.034]	[0.029]	[0.014]	[0.0134]	[0.00689]
Post initial shutdown	-0.088**	-0.077**	-0.059*	-0.018	-0.00579	-0.00500
	[0.041]	[0.036]	[0.030]	[0.014]	[0.0137]	[0.00681]
Observations	709	709	709	709	709	709
R-squared	0.324	0.320	0.302	0.101	0.087	0.046
Panel B: With Contextual Variables						
	(7)	(8)	(9)	(10)	(11)	(12)
Initial shutdown	-0.139***	-0.127**	-0.116**	-0.011	-0.0103	-0.00168
	[0.053]	[0.054]	[0.046]	[0.020]	[0.0334]	[0.0113]
Post initial shutdown	-0.428***	-0.277***	-0.217***	-0.060*	-0.122***	-0.0289**
	[0.097]	[0.094]	[0.076]	[0.033]	[0.0286]	[0.0141]
School closed	0.083***	0.088***	0.078***	0.009	-0.00293	-0.00194
	[0.026]	[0.023]	[0.020]	[0.009]	[0.00893]	[0.00430]
Unemployment	0.230	-0.083	-0.193	0.110	0.339***	-0.0260
	[0.391]	[0.357]	[0.303]	[0.149]	[0.124]	[0.0691]
Mobility drop	-0.091**	-0.018	0.013	-0.031*	-0.0612*	-0.0121
	[0.045]	[0.048]	[0.041]	[0.018]	[0.0335]	[0.0115]
COVID-19 cases	1.715***	1.123***	0.897***	0.226*	0.458***	0.134**
	[0.394]	[0.364]	[0.290]	[0.129]	[0.122]	[0.0584]
Political protests	0.357***	0.236**	0.207**	0.028	0.108**	0.0127
	[0.104]	[0.103]	[0.097]	[0.041]	[0.0420]	[0.0214]
Observations	709	709	709	709	709	709
R-squared	0.359	0.348	0.331	0.108	0.117	0.053

Notes: Unit of observation is a day. Outcomes are scaled to 100,000 population. Sample includes January 1 to August 24 in years 2018, 2019 and 2020. Initial shutdown date is March 19, 2020. Post-initial shutdown is May 29, 2020. All regression models include fixed effects for month, year and day-of-week. Models in Panel B also include contextual variables for school closure, unemployment, mobility drop (March 14, 2020), and county level COVID-19 cases (scaled to population) and number of political protests (per 100,000 population) in the prior 14 days. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effects of COVID-19 Shutdowns on DV and Non-DV Assaults in LA

	(1) DV Assaults	(2) Non-DV Assaults	(3) DV Share of Assaults	(4) DV Assaults	(5) Non-DV Assaults	(6) DV Share of Assaults
Initial shutdown	-0.076**	-0.385***	0.030***	-0.127**	-0.438***	0.030**
	[0.034]	[0.058]	[0.009]	[0.054]	[0.066]	[0.012]
Post initial shutdown	-0.077**	0.216***	-0.044***	-0.277***	-0.042	-0.057***
	[0.036]	[0.062]	[0.009]	[0.094]	[0.139]	[0.021]
School closed				0.088***	0.007	0.017***
				[0.023]	[0.036]	[0.005]
Unemployment				-0.083	1.723***	-0.237**
				[0.357]	[0.651]	[0.102]
Mobility drop				-0.018	-0.130**	0.009
				[0.048]	[0.060]	[0.011]
COVID-19 recent cases				1.123***	1.393***	0.078
				[0.364]	[0.538]	[0.080]
Political protests				0.236**	0.013	0.051**
				[0.103]	[0.183]	[0.026]
Observations	709	709	709	709	709	709
R-squared	0.320	0.278	0.115	0.348	0.295	0.138

Notes: Outcomes are scaled to 100,000 population in city. Sample includes January 1 to August 24 in years 2018, 2019 and 2020. Initial shutdown date is March 19, 2020. Post-initial shutdown is May 29, 2020. All regression models include fixed effects for month, year and day-of-week. Models in columns 4-6 also include contextual variables for school closure, unemployment, mobility drop (March 14, 2020), and county level COVID-19 cases (scaled to population) and number of political protests (per 100,000 population) in the prior 14 days. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3: The Effects of Abortion Counseling Laws

Rebecca Brough

Melissa K. Spencer

Abstract

Nearly two-thirds of states have abortion counseling laws requiring that women receive and acknowledge state-mandated information prior to giving their informed consent for abortion. The mandated information varies widely across states, and may include scientifically inaccurate statements regarding the risks of abortion, illustrations of fetuses, or ultrasound requirements. Despite the ubiquity of these laws, little is known about the effect of counseling laws on women's abortion decision and birth rates. Using a novel dataset of state-level abortion counseling laws, we find that counseling laws that contain ultrasound requirements increase the birth rate to white women by as much as 1 birth per 1,000 women.

Keywords: Abortion, Fertility, Birth Rate, Contraception

JEL Codes: J13, I12, K38

The results contained herein were derived in part from data provided by the National Center for Health Statistics, and specifically the natality detail data compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program.

1. INTRODUCTION

There has been a dramatic increase in the number of state-level abortion restrictions over the past decade. Between 2011 and 2015, states enacted 288 pieces of legislation restricting abortion access. Restrictions during this four-year period account for 25 percent of all legislation passed since legalization in 1973 (Nash et al. 2016). While there is a large literature studying the effects of abortion restrictions, recent research primarily focuses on restrictions related to parental involvement laws, mandatory waiting periods, and abortion clinic closure (Myers and Ladd 2020; Lindo and Pineda-Torres 2019; Lindo et al. 2017). In contrast, there is relatively little research analyzing the effects of one of the most common abortion restrictions: abortion counseling laws.

Abortion counseling laws mandate that women seeking abortion be provided specific information about abortion prior to signing an informed consent statement. The mandated information varies widely across states. In some states, the mandated information differs only slightly from a standard medical informed consent: For example, by including an additional statement describing alternatives to abortion such as adoption. In other states, the mandated information includes statements that are not based on scientific fact, such that abortion increases the risk of breast cancer, future stillbirth, or infertility. Some states further require that women view illustrations of fetuses, acknowledge that abortion 'terminates a human life,' or receive an ultrasound prior to giving their informed consent.

As of 2017, thirty states required some form of abortion-specific informed consent. Of these thirty states, twenty-two have abortion counseling laws and seven require an ultrasound prior to abortion. We have compiled what we believe is a comprehensive database of abortion counseling laws between 1975 and 2020. Using our novel data set of abortion counseling laws, we conduct a difference-in-difference analysis of the effect of these laws on county-level birth rates between 2002 and 2017. We control for mandatory waiting periods, which are often combined with abortion counseling laws, as well as county-level socioeconomics characteristics and other state laws related to family planning and abortion.

We find no evidence that abortion counseling laws affect birth rates. However, we find that counseling laws with ultrasound requirements increase the birth rate to white women by as much as 1 birth per 1,000 women. The positive effect of ultrasound requirements on births to white women is robust to the inclusion of many policy and demographic controls, as well a specification check where we drop states with very early and very late enforcement of ultrasound requirements.

An event study analysis of ultrasound requirements further confirms that these laws increase the birth rate to white women.

We conduct additional tests to demonstrate that timing of state ultrasound requirements is exogenous with respect to birth rates. First, we show that trends in birth rates across states with and without ultrasound requirements are parallel prior to the enforcement of these laws. Second, we compare states with enforced ultrasound requirements to those with enjoined ultrasound requirements (i.e., ultrasound requirements that have been passed by the state government but are prevented from being enforced by court order). We find that there is no effect of enjoined ultrasound requirements on birth rates. Third, we conduct an event study and show that there is no statistically significant effect of ultrasound requirements on birth rates prior to the time of enforcement.

There is limited existing research looking at the effects of abortion counseling laws. Existing studies focus on only one state or one year and find no effect of counseling laws on birth rates (Joyce, Henshaw, and Skatrud 1997; Medoff 2012). In creating our database, we are able to analyze the effects of abortion counseling across the United States over 16 years. We also contribute to the broader literature analyzing the effect of abortion restrictions on birth rates. Studies have found that the *Roe v. Wade* Supreme Court decision, which expanded the legality of abortion to all states, decreased the birth rates in states in which abortion was newly legalized (P B Levine et al. 1999). These studies also found a consistent decrease in birth rates among the teen population, ranging from 2 percent to 13 percent (Angrist and Evans 1996; P B Levine et al. 1999). Studies analyzing the effects of parental involvement laws and restrictions on Medicaid funding for abortion have found varying effects on births and abortions across demographic groups (Myers and Ladd 2020; Phillip B Levine 2003; Bitler and Zavodny 2002; 2001; Tomal 1999; Phillip B. Levine, Trainor, and Zimmerman 1996; Kane and Staiger 1996). We contribute to this literature by analyzing one of the most common but understudied abortion restrictions: counseling laws.

2. LEGAL CONTEXT AND COUNSELING DATA

Proponents of abortion counseling laws argue that these laws prevent women from being coerced into abortion by ensuring that they are fully informed about the risks and nature of an abortion procedure. For example, Wisconsin’s legal code states that, "Many women now seek or are encouraged to undergo elective abortions without full knowledge of the medical and psychological risks of abortion, development of the unborn child or of alternatives to abortion. An abortion decision is often made under stressful circumstances. The knowledgeable exercise of a woman's decision to have an elective abortion depends on the extent to which the woman receives sufficient information to make a voluntary and informed choice between 2 alternatives of great consequence: carrying a child to birth or undergoing an abortion."[1]

Indeed, it is precisely this argument that led the Supreme Court to decide abortion-specific informed consent laws are constitutional in *Planned Parenthood v. Casey*, with the deciding opinion stating, "In attempting to ensure that a woman apprehend the full consequences of her decision, the State furthers the legitimate purpose of reducing the risk that a woman may elect an abortion, only to discover later, with devastating psychological consequences, that her decision was not fully informed."[2] Opponents of abortion counseling laws argue the opposite: that these laws create an undue burden on women's right to choose and coerce women into continuing an unwanted pregnancy at substantial cost to mother and child (NARAL 2021).

To analyze the effects of counseling laws on births we have compiled what we believe is a comprehensive database of abortion counseling laws between 1975 and 2020.[3] This database was created in three steps. First, we compiled a list of states that had, at any point in the past 50 years, passed a law related to abortion-specific informed consent. This list was compiled using information from multiple sources, including: current information from the Alan Guttmacher Institute, NARAL Pro-Choice America, and the Kaiser Family Foundation state law and policy databases. Historic information was accessed from NARAL Pro-Choice America's annual *Who Decides* reports.[4]

Beginning with the most recently passed abortion restriction for each state, we then used the LexisNexis legal database and the Hein Online Session Laws Library to track the history of abortion counseling laws in each state. Our full dataset includes the bill number and/or state statute number for every state abortion counseling law passed in the past fifty years. For each law and

[1] 1985 Wisconsin Act 56
[2] Planned Parenthood of Southeastern Pennsylvania v. Casey, 112 U.S. 2791, 2823 (1992).
[3] This dataset is available from the authors upon request.
[4] These reports are available online via the internet archive beginning in 2000. Reports from the years 1989 – 1999 were accessed via library Special Collections at multiple libraries throughout the country.

amendment, we also include the date of passage, the date of enforcement, and the date repealed (where relevant). Additionally, we track all legal proceedings related to abortion counseling laws and include in our dataset the timing at which these laws are enjoined. Enjoined laws have been approved and passed by state government, but enforcement has been prevented by court order. Laws can be enjoined either due to preliminary or permanent injunction. Our dataset includes the start dates and end dates (where relevant) of injunctions.

In compiling this data, we also sought to define a consistent classification system for describing the range of abortion counseling laws. We initially define three categories: *Abortion-specific informed consent*, *Risk-focused counseling, and Fetal-focused counseling. Abortion-specific informed consent* laws resemble standard medical informed consent, but include additional statements specific to the setting of abortion. We define laws as meeting this standard if the mandated information includes statements relating to alternatives to abortion, such as adoption, financial assistance for prenatal care, postnatal care, or child expenses, or information regarding the father's obligation in supporting a child. For example, in 2001 Arkansas passed Women's Right to Know legislation requiring that:

> "(3) Prior to and in no event on the same day as the abortion, the woman is informed, by telephone or in person, by the physician who is to perform the abortion, by a referring physician or by an agent of either physician:
>
> > (A) That medical assistance benefits may be available for prenatal care, childbirth, and neonatal care;
> >
> > (B) That the father is liable to assist in the support of her child, even in instances in which the father has offered to pay for the abortion;"[5]

This law meets our standard for abortion-specific informed consent.

We define *Risk-focused counseling* as laws that require women be given scientifically inaccurate information regarding the risks of an abortion. These laws include statements regarding breast cancer, infertility, and risks to future pregnancies. For example, in 2015 Arkansas amended their abortion informed consent law to require that women be told:

> "(ii) The immediate and long-term medical risks associated with the proposed abortion method, including without limitation the risks of:
>
> > (a) Cervical or uterine perforation;

[5] Arkansas 83rd General Assembly, Regular Session, 2001, Act 353

(b) Danger to subsequent pregnancies;

(c) Hemorrhage; and

(d) Infection;"[6]

This law meets our standard for risk-focused counseling.

Our final category, *Fetal-focused counseling*, describes laws which contain statements or requirements relating to characteristics of a fetus. These laws include requirements that physicians describe to women the anatomical and physiological characteristics of the fetus, show women illustrations of fetuses at various stages of development, assert that the fetus can feel pain during an abortion procedure (a scientifically unproven statement), describe options for fetal burial following an abortion, state that abortion terminates the life of an unborn child, or require a sonogram or ultrasound of the fetus prior to abortion.

For example, in 2009 North Dakota passed a law requiring that women sign a statement acknowledging that, "the abortion will terminate the life of a whole, separate, unique, living human being."[7] In 2011, Indiana passed a law requiring women be informed that, "objective scientific information shows that a fetus can feel pain at or before twenty (20) weeks of postfertilization age."[8] In 2010, Missouri required that,

> "The physician who is to perform or induce the abortion or a qualified professional has presented the woman, in person, printed materials provided by the department, which describe the probable anatomical and physiological characteristics of the unborn child at two- week gestational increments from conception to full term, including color photographs or images of the developing unborn child at two- week gestational increments. Such descriptions shall include information about brain and heart functions, the presence of external members and internal organs during the applicable stages of development and information on when the unborn child is viable."[9]

As of 2017, seven states require an ultrasound or sonogram prior to a woman giving informed consent for an abortion. We consider all of these laws fetal-focused counseling.

For all of the above categories, we only consider a law as meeting our coding standards if it is required that information be given to a woman prior to abortion. In some cases, information

[6] 2015 Ark. ALS 1086
[7] 2009 N.D. HB 1445
[8] Indiana 16-34-2-1.1.
[9] 2010 MO SB 793

describing the fetus is offered to the woman and she may decline. We would not consider these laws fetal-focused counseling. We also note that all laws which contain fetal-focused or risk-focused counseling also contain abortion-specific informed consent.

There are a few drawbacks to our classification system. First, we do not capture within-category changes to counseling laws: Our coding is unaffected if a state that already requires women be shown pictures of fetal development amends their law to additionally require statements about fetal pain. Second, while we make the decision to distinguish between risk-focused and fetal-focused counseling in our database, this distinction may be minor and subject to interpretation, especially since states often pass laws that contain characteristics of both categories. Finally, while we consider ultrasound requirements fetal-focused counseling, it is likely that these requirements are more costly and invasive to women than other types of counseling.

To address these drawbacks, we define a broader category of abortion counseling laws that includes any law which meets our standard for either risk-focused or fetal-focused counseling. Additionally, we add a category specifically for ultrasound and sonogram requirements.

Because abortion counseling laws are often passed in conjunction with other abortion restrictions, we combine our legal database with that used in Myers and Ladd (2020), which includes information on parental involvement laws, mandatory waiting periods for abortion, and whether the mandatory waiting period requires two visits to a provider. Figure 1 describes the number of states that have abortion counseling laws and mandatory waiting periods in each year. Between 2002 and 2017, 11 states add abortion counseling laws and 7 states add ultrasound requirements. An additional 2 states pass ultrasound requirements in this period, but the laws are never enforced.

3. DATA

Our primary outcome variables are created using birth records obtained from the CDC's restricted-access Natality Detail file, which includes information on birth date, mother's county of residence, age and race. We create a panel of county-level birth rates using mother's county of residence. Birth rate is calculated as the number of live births per 1,000 women aged 15-44.

We use years 2002 to 2017 for our analysis. Our resulting panel includes 3,076 counties per year for a total 16 years. Table 1 present descriptive statistics for the counseling law variables, waiting period variables, and outcome variables. In addition to birth data and our counseling database, use the same set of control variables as those used in Myers and Ladd (2020). We define three sets of controls. Our main controls include county-level unemployment rate, poverty rate, and median income. We define a set of additional state policy controls that includes parental involvement laws, length of mandatory waiting periods, Medicaid expansion, maximum welfare benefit for a family of three, family caps on welfare benefits, and mandates that insurance cover contraceptives. Additional county-level demographic controls include the child poverty rate, the percent urbanized, a Gini coefficient, the percent of the population that has a high school degree, and the percent of the population that has a college degree. Summary statistics for control variable are presented in Appendix Table A1.

4. EMPIRICAL APPROACH

The primary results presented in this paper come from the following difference-in-difference specification:

$$y_{ct} = \beta_0 + \boldsymbol{\beta}_1 \boldsymbol{D}_{ct} + \boldsymbol{\beta}_2 \boldsymbol{X}_{ct} + \gamma_c + \delta_t + \epsilon_{c,t}$$

where y_{ct} is the outcome variable of interest in county c in year t, $\boldsymbol{D}_{ct}$ is a vector of abortion counseling laws and mandatory waiting periods, $\boldsymbol{X}_{c,t}$ is the vector of control variables, and γ_c and δ_t are county and year fixed effects, respectively. $\boldsymbol{\beta}_1$ is the coefficient of interest. In order to interpret $\boldsymbol{\beta}_1$ as the causal effect of abortion counseling laws on birth rates and birth characteristics, it must be that that y_{ct} is independent of the error term, conditional on controls and fixed effects. In other words, it must be that the timing at which new abortion counseling laws are signed into law and enforced is exogenous with respect to birth rates, after controlling for observables. This identifying assumption would be violated if there is some unobservable characteristic that is both leading state legislatures to pass abortion counseling laws, and directly affecting birth rates and characteristics. To test this identifying assumption, we compare trends in birth rates across states with and without counseling laws prior to the laws being enforced. We also exploit the timing of judicial injunctions which prevented abortion counseling laws from being enforced, and compare states with enforced laws to those with enjoined laws.

5. RESULTS

Results for the difference-in-difference regression are presented in Table 2. For this specification and all subsequent regressions, we estimate robust standard errors that are clustered at the state level. We also weight regressions by county-year female population. We find no effect of counseling mandates on birth rates. However, we find that ultrasound requirements increase the birth rate to white, non-Hispanic women by 1.026 births per 1,000 women (Column 2). This result is significant at the 5 percent level. We find no effect of ultrasound requirements on birth rates to Black or Hispanic women.

We test whether the positive effect of ultrasound requirements on births to white women is robust to a number of alternative specifications. Results are presented in Table 3. Column (1) shows that we find a similar estimate when dropping controls for unemployment, median income, and poverty rate. Column (2) repeats the result from our main specification in Table 2. In columns (3) and (4) we add the set of policy controls and demographic controls, sequentially.[10] The inclusion of additional controls slightly decreases the estimated coefficient; however, we still find that the effect is positive and statistically significant at the 5 percent level. In column (5), we test the robustness of results to a limited sample where we drop states with very early or very late ultrasound requirements (Alabama in 2003 and Iowa in 2017). In dropping these states, the "treated" group is limited to 5 states that pass ultrasound requirements at nearly the same time, between 2011 and 2013. Once again, we find that ultrasound requirements have a positive and statistically significant effect on birth rates to white women.

5.1 *Robustness Checks*

We might be concerned that some unobservable factor is driving both the increase in birth rates to white women and the passage of ultrasound requirements in treated states. For example, if states pass ultrasound requirements due to increasing levels of anti-abortion sentiment among its citizens, this change in public opinion could also be driving the increase in birth rates. To test this alternative hypothesis, we first compare trends in birth rates across states that ultimately pass ultrasound requirements and those that do not.

[10] Coefficients on control variables for these regressions can be found in Appendix Table A2.

Figure 2 shows the weighted average birth rate to white women between 2002 and 2017 for treated and untreated states.[11] Prior to 2011 when ultrasound requirements are first passed, trends in birth rates appear to be roughly parallel across the two groups. Between 2011 and 2013, when 5 states pass ultrasound requirements, the birth rate increases in treated states and declines in untreated states. After 2013, the birth rate increases across both groups, but at a faster rate in the treated states. This preliminary analysis suggests that changes in public opinion about abortion are not driving results. However, we employ the following event study design to test this more rigorously,

$$y_{ct} = \beta_0 + \sum_{\boldsymbol{j=-5}}^{\boldsymbol{j=7}} (\boldsymbol{\alpha_\tau} * \mathbf{1}(\boldsymbol{\tau} = \boldsymbol{j})) + \boldsymbol{\beta}_1 \boldsymbol{D}_{ct} + \boldsymbol{\beta}_2 \boldsymbol{X}_{ct} + \gamma_c + \delta_t + \epsilon_{c,t}$$

where y_{ct} is the birth rate to white women in county c in year t, $\mathbf{1}(\boldsymbol{\tau} = \boldsymbol{j})$ is an indicator for the time to the ultrasound requirement in each state, $\boldsymbol{D}_{ct}$ is a vector of abortion counseling laws and mandatory waiting periods, $\boldsymbol{X}_{c,t}$is the vector of control variables, and γ_c and δ_t are county and year fixed effects, respectively. We set the omitted period to $\boldsymbol{\tau}$ =-1 and $\boldsymbol{\tau}$=**0** is the first year an ultrasound requirement is ever enforced in a state. The regression is weighted by white female population and standard errors are robust and clustered at the state level.

The results from the event study regression with the full sample and the main set of controls are presented in Figure 3. We find no statistically significant effects on white birth rates prior to the enforcement of ultrasound laws. Following the enforcement of ultrasound laws, we find a positive effect on birth rates to white women. The positive effect is not statistically significant until the 2nd year of enforcement. This is unsurprising given (a) that laws are enforced for only part of the year in year 0, and (b) the 9-month gestational period between conception and birth. Year 2 is the first year in which all births are potentially treated by the ultrasound requirement. Figure 4 presents the results of an event study using the limited sample and the full set of controls. We once again find no evidence of pre-trends and a positive effect on births to white women after full enforcement of ultrasound requirements.[12]

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[11] We exclude states that are treated early and late (Alabama and Iowa), as well as states with enjoined ultrasound requirements (North Carolina and Oklahoma).

[12] The results from the event study analysis also shows that results are robust to concerns about the difference-in-difference design. See for example Goodman-Bacon (2018).

As a final robustness check, we compare the effect of enforced versus enjoined ultrasound laws. If the passage of these laws and the increase in births to white women is driven by changing attitudes, we would expect to find an increase in birth rates among states the pass ultrasound requirements, even if a judge prevents the requirement from being enforced. Table 4 presents the results of a regression including indicators for both enforced and enjoined ultrasound requirements. We find no effect of enjoined ultrasound requirements on birth rates. Further, we find that the positive effect of enforced ultrasound requirements on births rates is robust to the inclusion of an indicator for enjoined laws. This result suggests it is the actual enforcement of ultrasound requirements that is driving results.

6. CONCLUSION

We find that birth rates to white women increase when states pass laws requiring an ultrasound or sonogram prior to an abortion. The positive effect of ultrasound requirements on white birth rates is robust to the inclusion of demographic and policy controls, a limited sample, and an event study analysis. We further provide evidence that there is no effect of enjoined ultrasound laws on birth rates, suggesting that the increase in the white birth rate is due to enforcement of ultrasound requirements and not changing attitudes that lead to the laws being passed.

This research contributes to a large literature studying the effects of abortion restrictions on fertility. We compile a comprehensive database on state abortion counseling laws going back to 1975, and define a classification system for studying these laws. Our preliminary analysis suggests that only abortion counseling laws with ultrasound requirements have an effect on birth rates. However, further research is needed to understand the mechanism underlying this result and heterogeneity across demographic groups and states.

For example, there are multiple mechanisms via which ultrasound requirements could increase birth rates. It’s possible that women, upon receiving an ultrasound, change their mind about having an abortion and choose to continue a pregnancy. However, it’s also possible that cost of an ultrasound prevents women from having an abortion, or that the cost to providers of obtaining an ultrasound machine and technician lead to clinic closures. We intend to study these possible mechanisms in future research.

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FIGURES

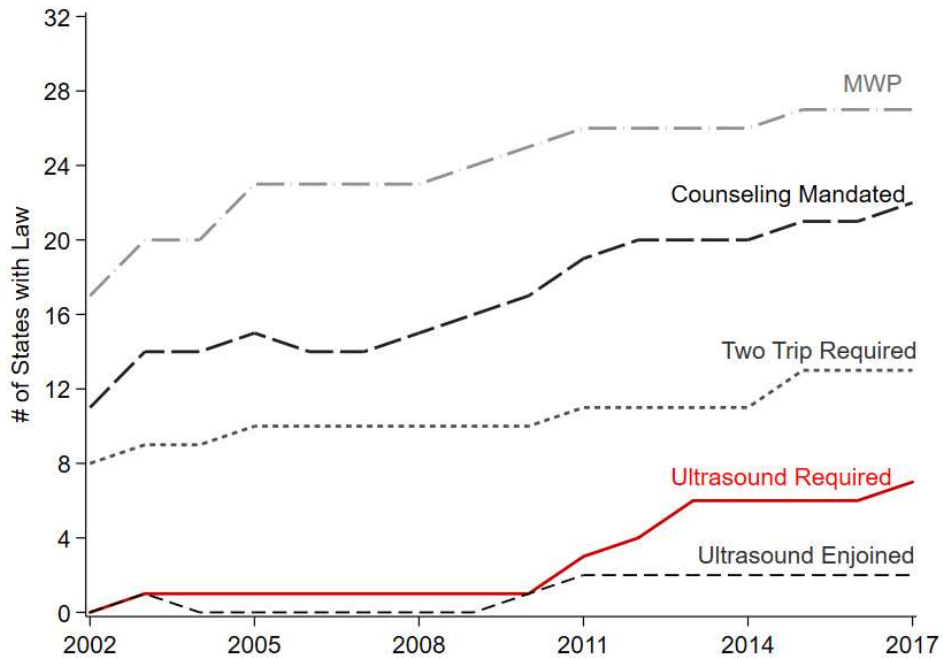

Figure 1: State Abortion Counseling Laws

Notes: This figure plots the number of states in each year that have abortion counseling laws, including mandatory waiting periods, mandated counseling, two trip requirements, enforced ultrasound requirements, and enjoined ultrasound requirements. Five states pass and enforce ultrasound requirements between 2011 and 2013.

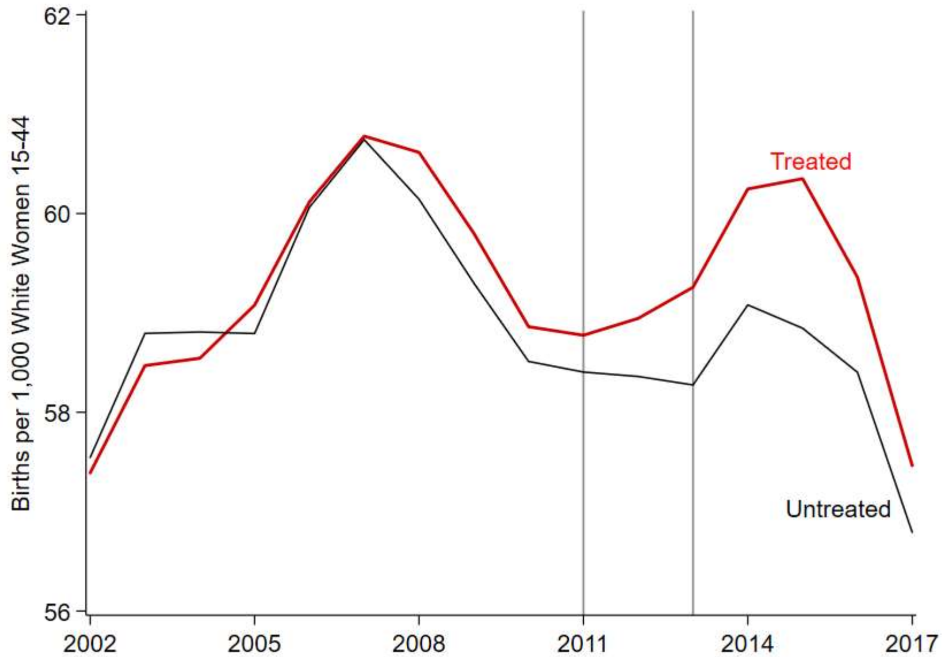

Figure 2: Ultrasound Requirements and Trends in Birth Rates

Notes: This figure plots the birth rate to white women in states that enforce ultrasound requirements between 2011 and 2013 (treated), and states that never pass ultrasound requirements (untreated). Prior to 2011, trends in birth rates across the treated and untreated group are roughly parallel. Beginning in 2011, birth rates in the treated states increase by more than birth rates in the untreated states. The birth rate is calculated as a weighted average of county-level birth rates, where weights are equal to the white female population in each year. States with enjoined ultrasound laws and states with ultrasound laws passed before 2003 or after 2016 are excluded.

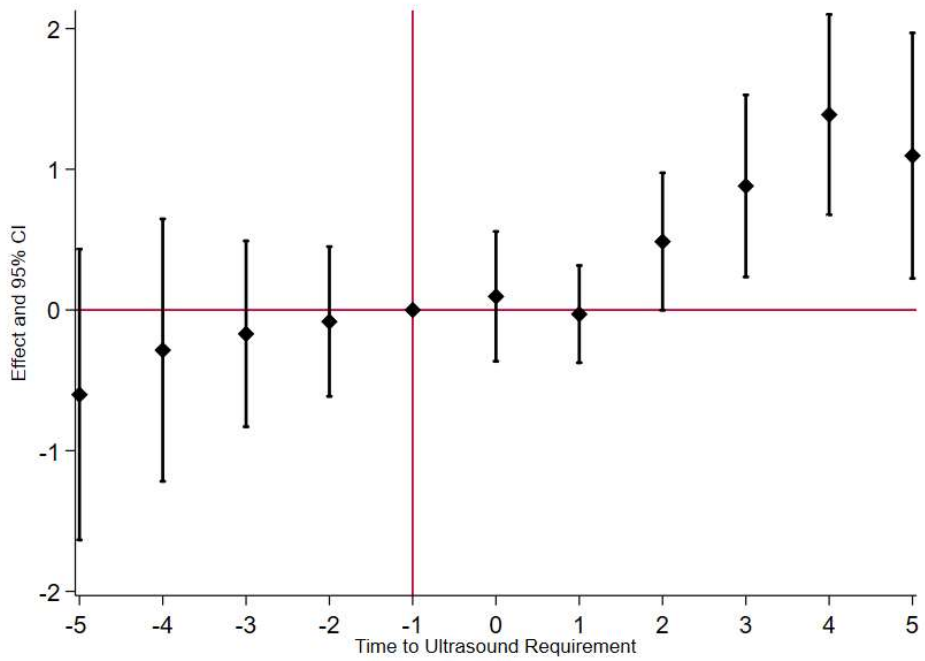

Figure 3: Effect of Ultrasound Requirements on Births to White Women

Notes: This figure shows the results of an event study analysis on the effect of ultrasound requirements on the birth rate to white women. Point estimates and 95% confidence intervals are displayed. Coefficients come from a weighted fixed effects regression with controls for counties and years. Weights are computed using county-year population for the corresponding demographic group. Regressions also include controls for county-year unemployment rate, poverty rate, and log of median income. Standard errors are robust and clustered at the state level. Time 0 indicates the first year (or partial year) that an ultrasound requirement is enforced. Beginning at Time 2, ultrasound requirements have a positive and statistically significant effect on births to white women. Prior to the enforcement of the law, there are no statistically significant effects on birth rates.

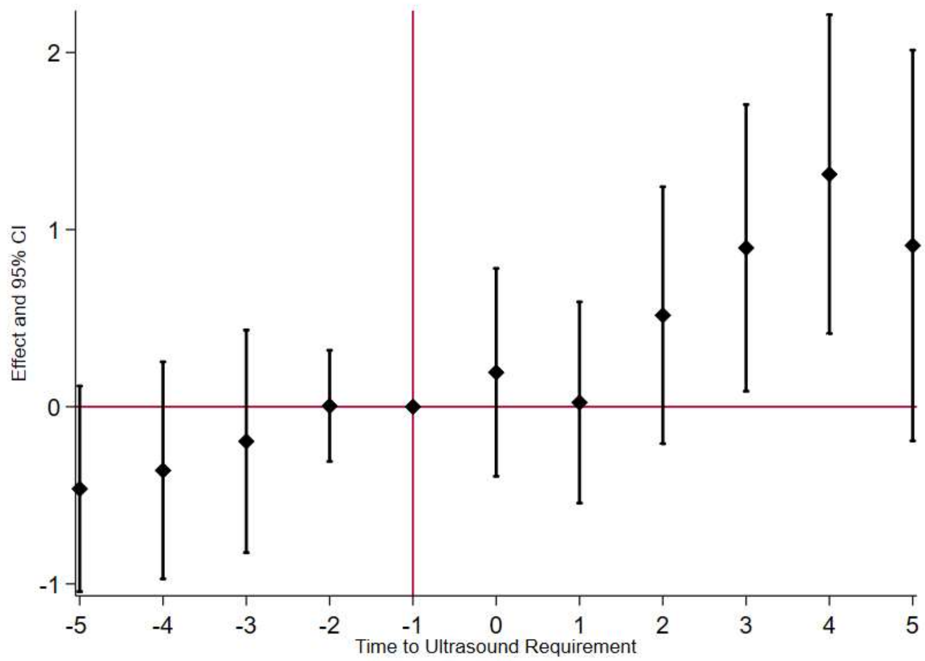

Figure 4: Robustness Check of Effect of Ultrasound Requirements

Notes: This figure shows the results of an event study analysis on the effect of ultrasound requirements on the birth rate to white women. Point estimates and 95% confidence intervals are displayed. Coefficients come from a weighted fixed effects regression with controls for counties and years. Weights are computed using county-year population for the corresponding demographic group. States that pass ultrasound requirement before 2011 or after 2013 are dropped (Alabama in 2003 and Iowa in 2017). Regressions also include the full list of controls described in Table A1. Standard errors are robust and clustered at the state level. Time 0 indicates the first year (or partial year) that an ultrasound requirement is enforced.

TABLES

Table 1: Descriptive Statistics

	Mean	Std.Dev.	N
	(1)	(2)	(3)
Indicator for abortion restrictions…			
Ultrasound requirement	0.1	0.3	49168
Counseling mandated	0.46	0.5	49168
Two visit requirement	0.32	0.47	49168
Mandatory waiting period (MWP)	0.67	0.47	49168
Ultrasound requirement enjoined	0.03	0.16	49168
Birth rate per 1,000 women aged 15-44…			
All	64.59	10.15	49168
White; Non-Hispanic	58.99	9.91	49168
Black; Non-Hispanic	66.31	11.98	47917
Hispanic	82.38	20.84	49077

Notes: This table presents descriptive statistics for abortion restrictions and birth rates. The unit of observation is the county-year level. The sample period includes years 2002 to 2017. The abortion restriction variables take on a value of 0 if never enforced in a year, or 1 if enforced during any part of a year. All states with ulstrasound requirements also have mandated counseling. Birth rates are calculated as number of live births per 1,000 women of the given demographic group. The average birth rates presented here are weighted averages using race-specific female population in each county-year as weights.

Table 2: Effect of Abortion Counsleing Laws on Birth Rates

	Birth Rates per 1,000 Women			
	All Women 15-44	White Women 15-44	Black Women 15-44	Hispanic Women 15-44
	(1)	(2)	(3)	(4)
Ultrasound requirement	-0.388	1.026**	0.805	1.896
	[-0.21]	[2.22]	[1.11]	[0.92]
Counseling mandated	0.0876	-0.326	-1.307	3.285
	[0.06]	[-0.71]	[-1.46]	[1.47]
Two visit requirement	-1.69	0.789	1.163	5.147
	[-0.81]	[0.95]	[1.32]	[1.30]
Mandatory waiting period	-0.99	-0.874	0.273	-16.82***
	[-0.88]	[-1.58]	[0.29]	[-5.27]
Observations	49,168	49,168	47,917	49,077
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Weighted	Yes	Yes	Yes	Yes

Notes: This table shows the effect of abortion ultrasound requirements on county-level birth rates. Coefficients come from a weighted fixed effects regression with controls for counties and years. Weights are computed using county-year population for the corresponding demographic group. Regressions also include controls for county-year unemployment rate, poverty rate, and log of median income. Standard errors are robust and clustered at the state level. Results indicate that ultrasound requirements increase the birth rate to white women.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Effect of Ultrasound Requirements on Birth Rates to White Women

	Birth Rates per 1,000 White Women 15-44				
	Uncontrolled	Baseline	Policy Controls	Demographic Controls	Limited Sample
	(1)	(2)	(3)	(4)	(5)
Ultrasound requirement	1.036**	1.026**	0.951**	0.820**	0.849**
	[2.20]	[2.22]	[2.53]	[2.29]	[2.32]
Counseling mandated	-0.543	-0.326	-0.196	-0.121	-0.0536
	[-1.20]	[-0.71]	[-0.38]	[-0.24]	[-0.11]
Two visit requirement	0.811	0.789	0.818	0.705	0.694
	[0.91]	[0.95]	[0.86]	[0.74]	[0.72]
Mandatory waiting period	-0.925	-0.874	0.568	0.355	0.444
	[-1.55]	[-1.58]	[0.47]	[0.30]	[0.37]
Observations	49,168	49,168	49,168	49,168	46,528
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Main Controls	No	Yes	Yes	Yes	Yes
Policy Controls	No	No	Yes	Yes	Yes
Demographic Controls	No	No	No	Yes	Yes
Drop Alabama & Iowa	No	No	No	No	Yes
Weighted	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of abortion ultrasound requirements on birth rates to white women. Coefficients come from a weighted fixed effects regression with controls for counties and years. Weights are computed using county-year population for the corresponding demographic group. Column (1) includes no additional controls. Column (2) presents the baselines results from Table 2. Column (3) adds additional controls for state-level policies, including: parental involvement laws, length of mandatory waiting period, Medicaid expansion, maximum welfare benefit, family cap on welfare, and contraceptive insurance mandates. Column (4) includes additional county-level demographic controls, including: child poverty rate, percent urbanized, Gini coefficient, percent with high school degree, and percent with college degree. Column (5) drops counties in Alabama (added ultrasound requirement in 2003) and Iowa (added ultrasound requirement in 2017). Standard errors are robust and clustered at the state level. The effect of ultrasound requirements on births to white women is positive and statistically significant across all specifications. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Enforced versus Enjoined Ultrasound Requirements

	Birth Rates per 1,000 White Women 15-44				
	Uncontrolled	Baseline	Policy Controls	Demographic Controls	Limited Sample
	(1)	(2)	(3)	(4)	(5)
Enforced Ultrasound	1.011**	0.990**	0.945**	0.816**	0.838**
	[2.09]	[2.11]	[2.50]	[2.27]	[2.26]
Enjoined Ultrasound	-0.47	-0.647	-0.185	-0.128	-0.304
	[-0.64]	[-0.99]	[-0.20]	[-0.13]	[-0.26]
Counseling mandated	-0.505	-0.27	-0.181	-0.11	-0.0298
	[-1.06]	[-0.57]	[-0.35]	[-0.22]	[-0.06]
Two visit requirement	0.651	0.567	0.753	0.66	0.585
	[0.64]	[0.61]	[0.68]	[0.59]	[0.50]
Mandatory waiting period	-0.765	-0.654	0.611	0.386	0.515
	[-1.03]	[-0.96]	[0.53]	[0.35]	[0.46]
Observations	49,168	49,168	49,168	49,168	46,528
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Main Controls	No	Yes	Yes	Yes	Yes
Policy Controls	No	No	Yes	Yes	Yes
Demographic Controls	No	No	No	Yes	Yes
Drop Alabama & Iowa	No	No	No	No	Yes
Weighted	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of enforced versus enjoined abortion ultrasound requirements on birth rates to white women. Only enforced requirements have an effect on birth rates. Coefficients come from a weighted fixed effects regression with controls for counties and years. Weights are computed using county-year population for the corresponding demographic group. Column (1) includes no additional controls. Column (2) presents the baselines results from Table 2. Column (3) adds additional controls for state-level policies, including: parental involvement laws, length of mandatory waiting period, Medicaid expansion, maximum welfare benefit, family cap on welfare, and contraceptive insurance mandates. Column (4) includes additional county-level demographic controls, including: child poverty rate, percent urbanized, Gini coefficient, percent with high school degree, and percent with college degree. Column (5) drops counties in Alabama (added ultrasound requirement in 2003) and Iowa (added ultrasound requirement in 2017). Standard errors are robust and clustered at the state level. The effect of ultrasound requirements on births to white women is positive and statistically significant across all specifications. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX A: ADDITIONAL TABLES

Table A1: Control Variables Summary Statistics

	Mean	Std.Dev.	Min	Max
	(1)	(2)	(3)	(4)
Poverty rate	14.24	5.40	2.2	62
Unemployment rate	6.42	2.47	1.12	28.86
ln(median income)	10.97	0.25	9.93	11.83
I(Parental involvement law)	0.84	0.36	0	1
Length (hours) of mandatory waiting period	17.76	15.19	0	72
I(Medicaid expanded)	0.61	0.49	0	1
Maximum welfare benefit (family of 3)	422.54	159.18	170	1021
I(Family cap on welfare)	0.4	0.49	0	1
I(Contraceptive insurance mandate)	0.59	0.49	0	1
Child poverty rate	22.12	9.06	2.1	76.7
Percent urbanized	17.87	32.78	0	100
Gini coefficient	0.49	0.03	0.39	0.62
Percent with high school degree	82.81	7.85	34.7	97.55
Percent with college degree	18.83	8.6	3.66	72.79
Observations	49,168			

Notes: This table presents descriptive statistics for control variables.

Table A2: Effect of Ultrasound Requirements on Birth Rates to White Women

	Birth Rates per 1,000 White Women 15-44				
	Uncontrolled	Baseline	Policy Controls	Demographic Controls	Limited Sample
	(1)	(2)	(3)	(4)	(5)
Enforced Ultrasound	1.036**	1.026**	0.951**	0.820**	0.849**
	[2.20]	[2.22]	[2.53]	[2.29]	[2.32]
Counseling mandated	-0.543	-0.326	-0.196	-0.121	-0.0536
	[-1.20]	[-0.71]	[-0.38]	[-0.24]	[-0.11]
Two visit requirement	0.811	0.789	0.818	0.705	0.694
	[0.91]	[0.95]	[0.86]	[0.74]	[0.72]
Mandatory waiting period	-0.925	-0.874	0.568	0.355	0.444
	[-1.55]	[-1.58]	[0.47]	[0.30]	[0.37]
Poverty rate		0.208***	0.194***	-0.0452	-0.0395
		[3.57]	[3.53]	[-0.78]	[-0.65]
Unemployment rate		-0.262***	-0.280***	-0.331***	-0.330***
		[-3.35]	[-3.41]	[-4.07]	[-3.92]
ln(median income)		9.921***	9.445***	9.972***	9.933***
		[4.40]	[3.78]	[4.45]	[4.34]
I(Parental involvement law)			0.349	0.32	0.311
			[1.06]	[0.97]	[0.92]
Length (hours) of MWP			-0.048	-0.0499	-0.0535
			[-1.23]	[-1.31]	[-1.35]
I(Medicaid expanded)			-0.404	-0.544	-0.633*
			[-1.20]	[-1.62]	[-1.86]
Maximum welfare benefit			-0.00127	-0.00308	-0.00283
			[-0.35]	[-0.90]	[-0.82]
I(Family cap on welfare)			-0.556	-0.526	-0.556
			[-1.12]	[-1.08]	[-1.14]
I(Contraceptive insurance mandate)			-0.19	-0.232	-0.228
			[-0.49]	[-0.60]	[-0.57]
Child poverty rate				0.137***	0.129***
				[3.05]	[2.76]
Percent urbanized				-0.0296**	-0.0303**
				[-2.04]	[-2.06]
Gini coefficient				-2.146	-2.251
				[-0.51]	[-0.52]
Percent with high school degree				0.465***	0.480***
				[5.12]	[5.13]
Percent with college degree				-0.613***	-0.647***
				[-8.19]	[-8.86]
Observations	49,168	49,168	49,168	49,168	46,528

Notes: This table shows the effect of abortion ultrasound requirements on birth rates to white women. Coefficients come from a weighted fixed effects regression with controls for counties and years. Weights are computed using county-year population for the corresponding demographic group. Column (5) drops counties in Alabama (added ultrasound requirement in 2003) and Iowa (added ultrasound requirement in 2017). Standard errors are robust and clustered at the state level. The effect of ultrasound requirements on births to white women is positive and statistically significant across all specifications. *** p<0.01, ** p<0.05, * p<0.1