Fertility and the Abortion-Crime Debate*

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Abstract

Recently some scholars have asserted that abortion legalization during the 1970s resulted in lower crime 15-20 years later. While economists have both substantiated and challenged these findings, sociologists and demographers have been mute on the topic. In this paper, we show that the supposed link between abortion and crime is actually the result of omitted variables bias and difficulties in distinguishing between age-period-cohort effects. We correct these problems and use quasi-experimental methods to retest the causal argument for homicide, property, and violent crime. Using a unique data set compiled from multiple sources, we find that abortion legalization did not have any measurable effect on crime 15-20 years later once appropriate controls are included. Our findings indicate that any drop in crime is the result of a mixture of unmeasured period and cohort effects and not abortion.

1 INTRODUCTION

"If you wanted to reduce crime, you could – if that were your sole purpose – you could abort every black baby in this country and your crime rate would go down. That would be an impossibly ridiculous and morally reprehensible thing to do, but your crime rate would go down."

—William Bennett, former U.S. Secretary of Education (Kirkpatrick 2005)

During the early 1990s, the United States experienced a sudden and persistent decline in the crime rate. Increased incarceration, stiffer penalties for repeat offenders, and disparities in sentencing were the usual explanations for the decline (Donohue and Levitt 2001; Zimring 2006). Recently a more novel and controversial argument has surfaced: abortion legalization in the early 1970s caused the crime decline through time-lagged effects (Donohue and Levitt 2001; 2004).[1] In this paper we bring a new perspective to the abortion-crime decline debate by providing a historical framework with which to understand how omitted variables directly impact Donohue and Levitt's overall findings. We also raise concerns about their estimation

[1]By time-lagged effects, we mean that there is a temporal delay in any "benefits" that are the result of women having abortions. Donohue and Levitt argue that this positive social externality occurs 15-20 years from the time of abortion legalization when fetuses that were aborted would have reached the ages at which criminal offending begins to rise.

methods in both papers, and we use a demographic and econometric approach to show how the observed relationship between abortion and crime hinges on distinguishing between age-period-cohort effects. Once age-period, age-cohort, and period-cohort distinctions are resolved and omitted variables bias is reduced, we do not find statistically significant ($p < .10$) evidence for an abortion-crime decline relationship.

The structure of this paper is as follows: Section II recapitulates the abortion-crime decline argument and debate, paying particular attention to the assumptions and methodologies employed in order to tease out the causal and non-causal associations between abortion legalization and crime rates. Section III outlines and explicates our criticisms of this explanation for the crime decline from a purely demographic standpoint. Section IV highlights our identification and methodological strategies for testing how robust the abortion-crime decline findings are given our criticisms. Section V provides evidence that abortion did not affect crime through time-lagged effects. Section VI presents concluding remarks about our findings, this strain of research, and the sociological impact such research has in the public domain.

The general tenor of the paper is positive but critical, for we consider the abortion-crime thesis to be a novel and illuminating alternative to existing theories of the crime decline. However, our purpose here is to ensure that the argument, data, and findings reflect the historical, demographic, and socio-legal realities of the periods under study.

2 THE ABORTION-CRIME DECLINE ARGUMENT AND DEBATE

Donohue and Levitt (2001) contend that abortion legalization in the early 1970s lowered crime in the 1990s through two processes: 1) reduction in cohort size and/or 2) cohort compositional changes due to the abortion of the marginal child (children born into unfavorable circumstances like poverty, single parent households, etc.) who are statistically more likely to

commit crimes. They focus their analysis on the latter explanation. More specifically, they argue that because abortion legalization allowed women to better time their fertility, potential children who would have been most at risk of becoming future criminals were aborted, thus enabling the United States to enjoy a sharp and persistent decline in crime 15-20 years later. They attribute as much as 50% of the decline in crime to abortion legalization.

Since Donohue and Levitt's 2001 paper, research on this topic has flourished. While some researchers have replicated their findings (Joyce 2004a; Sen 2005) and validated the abortion-crime relationship (Berk, Sorenson, Wiebe, and Upchurch 2003; Sorenson, Wiebe, and Berk 2002; Sen 2005), others have raised important criticisms and limitations as to their research design and statistical tests (Ananat, Gruber, and Levine 2004; Joyce 2004a;b; Sen 2005; Foote and Goetz 2005; Zimring 2006). In particular, the main critique of the original abortion-crime thesis is that the authors fail to account for the effect of the crack-cocaine epidemic of the late 1980s (Fox 2000; Joyce 2004a;b).[2] Donohue and Levitt (2004) assert that accounting for the crack-cocaine epidemic bolsters their original abortion-crime findings, with over 60% of the decline in crime being due to abortion legalization. However, when Sen (2005) employs an instrumental variables approach using Canadian data, the effect of abortion legalization on crime reduction in is lowered to about 10% because there was no crack-cocaine epidemic.[3] Foote and Goetz (2005) argue that Donohue and Levitt find an abortion-crime link because, despite being written in the text of the paper, Donohue and Levitt omitted state-year fixed effects in their computer code, which would have taken into account state-specific omitted variables that vary by year (like the severity of the crack wave). Moreover, Foote and Goetz (2005) contend that failure to use arrest rates, instead of arrest counts, additionally explain why the relationship between abortion and crime disappears.

[2]Crack-cocaine use during the mid to late 1980s resulted in a sharp rise in crime rates because of related gang violence and the visible distributional methods associated with the dissemination of the drug. For a further discussion of the crack-cocaine epidemic, see Fryer et al. (2005) and Zimring (2006).

[3]Canadian and American crime rates waxed and waned around the same time. The only notable difference between the two countries is that the U.S. experienced a severe crack epidemic in the 1980s. Studying the relationship between abortion and crime in Canada removes problems of omitted variables bias due to the surge in crack use and its dissemination.

Other scholars have debated the role of race and sex in the abortion-crime findings. Joyce (2004b) does not find any evidence for the abortion-crime relationship after controlling for race and sex of victims. Yet other researchers, using uninterrupted, national time series models, argue that even when race and sex are controlled, the abortion crime link remains for teenage, adult, and infant homicide rates (Sorenson, Wiebe, and Berk 2002; Berk, Sorenson, Wiebe, and Upchurch 2003). Despite these supportive findings, even Levitt (2001) has denounced using national time series methods to ascertain causality in crime studies.

Donohue and Levitt contend that teenage and single parenthood, in addition to poverty, are the socioeconomic conditions that place youth and young adults at risk of criminal offending. Yet, if the causal argument relies on the notion that future criminals are aborted based on current socioeconomic conditions, then to test the argument's validity, one only needs to examine the trend in births by socioeconomic status. If abortion legalization produced a decline in the number of marginal children (who would later become criminals), then one should expect to see a decline in the proportion of births to women from the socioeconomic groups most at risk. In the event that these risk groups do not experience a break from the general trend, or if the trend is in the opposite direction after abortion legalization, then the abortion-crime thesis is seriously flawed. Zimring (2006) shows that the proximate determinants of the very risk factors Donohue and Levitt purport to be the underlying cause for the qualitative changes in cohorts *do not* decline subsequent to abortion legalization. As a matter of fact, the percentage of births to single mothers age 15-19, and to single mothers in general, increased post *Roe v. Wade* for blacks and whites, while the overall percentage of births to black women remained unchanged (Zimring 2006). We will address these issues in greater detail later in the paper.

3 A DEMOGRAPHIC CRITIQUE

The intensity of the crack-cocaine epidemic is not the only omitted variable that abortion-crime researchers have failed to account for in their models. During the 1965-1980 period, other important historical events occurred that raise serious concerns about the robustness of previous abortion-crime research. In an effort to illuminate problems with this argument—in the context of methods employed and the findings explicated—we raise several issues that researchers have either fully neglected or only partially explored. First, we reframe the discussion around other historical events that influenced fertility and abortion rates during this period, namely contraceptive use, age at first sex, and divorce rates. In doing so we revisit the social terrain of the 1960s and 1970s to highlight important advancements in contraceptive methods and attitudinal research on abortion by race, sex, age, and educational attainment. Second, we present a discussion of crime focused on the demographic characteristics of offenders and their victims. Third, we investigate the timing and accuracy of the abortion-crime thesis. Lastly, we discuss the problem with age-period and age-cohort analyses on this topic.

3.1 BRINGING HISTORY BACK IN: MECHANISMS THAT AFFECT FERTILITY

The 1960s and 1970s ushered in a series of broad and rapid social changes in the United States. While Donohue and Levitt mention the effect abortion had on limiting and timing womens' fertility, some economists have begun to reframe how abortion legalization affected life-cycle fertility. Specifically, Ananat et al. (2004) find that abortion legalization not only enabled better control of fertility timing, but that it also had a lasting impact by reducing completed fertility and increasing the number of childless women. This research suggests the need to determine which women were using, and affected by, abortion. Other researchers have focused on how the introduction of and improvements in contraceptives, namely the

pill, profoundly affected women's educational, marital, employment, and fertility options (Akerlof, Yellen, and Katz 1996; Goldin and Katz 2002; Joyce 2004b; Sen 2005).

Previous research also shows that among unmarried women, whites were more likely to have abortions than were blacks, whereas for married couples, black women were more likely to abort than whites in 1980 (Trent and Powell-Griner 1991). Trent and Powell-Griner (1991) also find that among women with a high school education or less, pregnancies were more likely to result in live births if the women did not previously have children, and that college educated women without children were more likely to abort their pregnancies than were women with less education.

Similarly, research on abortion attitudes by race indicate that blacks were less likely to approve of abortion relative to whites during the periods Donohue and Levitt study (Combs and Welch 1982; Hall and Ferree 1986). In a study of mostly low-income, young black women in Baltimore in 1970, Furstenburg (1972) found that these women opposed abortion as a fertility limiting technique, up to the point at which their desired fertility levels had been achieved. Zelnik and Kantner (1975) show that teenagers were more conservative in their attitudes toward abortion than adults, with teens supporting only the "hard" reasons for abortion (e.g., if the pregnancy endangered the mother's health or if the woman had been raped). While beliefs may not definitively translate into actual behavior—a woman may be morally opposed to abortion but may have an abortion if faced with an untimely, unwanted, or dangerous pregnancy—it is important to remember that beliefs and behavior are aligned for some non-zero proportion of all women.

The aforementioned review of abortion attitudes raises a very interesting issue: if abortion legalization enabled whites to better time their fertility, while enabling black women to limit their fertility at higher parities, then one should expect to find a downward shift in the hazard of first births for whites post *Roe v. Wade* and a steady or increasing trend in the hazard of first births for blacks for any particular age group, assuming all other factors remain constant. More specifically, if attitudinal research is correct, then one would expect the hazard of first

birth to rise for black teenage women because they are much less supportive of abortion as a contraceptive method. This would suggest that any drop in crime for black victims and offenders would not be due to a decline in black teenage fertility levels during periods of legal abortion. This would be evidence against qualitative changes in cohorts because black unwanted births during teenage years would not decline relative to their white counterparts. Unfortunately we are unable to test this alternative theory because first birth probabilities by race and birth cohort were not collected until the late 1970s.

The level of fertility is also principally important in this discussion. While Donohue and Levitt acknowledge that fertility was falling prior to the onset of abortion legalization, they do not acknowledge the impact of this trend on their argument, even though they control for birth rates in their models.[4] Figure 1 displays the total fertility rate (TFR) from 1960 to 1990 for the United States.[5] Because the white TFR drives the overall TFR, we also disaggregate the TFR by race. We have drawn three vertical lines to indicate important events that preceded and succeeded when states legalized abortion. The first vertical line marks 1964 when Lyndon B. Johnson announced support for legislation aimed at enabling the poor to access birth control. A year later, *Griswold v. Connecticut* reached the U.S. Supreme Court, and the Court struck down Connecticut's birth control prohibition, thereby giving married couples the right to contraception (Luker 1996) and effectively eradicating lingering Comstock Laws.[6] The second vertical line is for 1970, when Alaska, Washington, California, Hawaii, and New York legalized abortion (henceforth referred to as repeal states), becoming the first states to do so. The third line marks the 1973 *Roe v. Wade* U.S. Supreme

[4]Donohue and Levitt define the birth rate as the number of births per 1000 state residents instead of per 1000 women of reproductive age, as commonly done by demographers (i.e., they use the crude birth rate, which is distorted by age structure and the inclusion of males). We use the latter measurement in our analysis.

[5]We focus on the total fertility rate, instead of age specific fertility rates, because abortions had by all women across the age distribution constitute the counterfactual cohort of fetuses at risk of becoming future criminals regardless of the woman's parity and age.

[6]The Comstock Laws were restrictive laws that prevented the acquisition and dissemination of birth control. The Court's decision in *Griswold v. Connecticut* protected a married couple's right to privacy under the First, Fourth, and Ninth Amendments of the Bill of Rights, which helped pave the way for *Roe v. Wade* in 1973.

Court decision that legalized abortion in all 50 states and D.C. Figure 1 shows that the TFR declines from 1960 through 1968, rises a bit—probably due to men returning from the Vietnam War—and then continues on a persistent decline. The main purpose of this graph is to examine the rate of change in the total fertility rate for the 1965-69 and 1970-74 periods by race. Although, Donohue and Levitt partially attribute the decline in fertility after 1970 to the crime decline of the 1990s, they do not take into account the average rate of change in fertility during the periods before and after abortion legalization. We care about the rate of change because fertility was declining very quickly before and after abortion. This figure is important because it shows that the average rate of change in the TFR for blacks during the 1965-69 and 1970-74 periods are approximately the same, with the former falling by 0.158 births per year and the latter by 0.160.[7] For whites, the decline was just as pronounced, but the average rate of change was 0.084 and 0.128 for the 1965-69 and 1970-74 periods, respectively. These rates indicate that white fertility changed more between the periods than did black fertility. If the black rates of change are approximately equivalent in the periods before and after *any* state legalized abortion, and the only difference is in the levels of fertility between the two periods, then there is little reason to associate the abortion legalization period with a crime decline more so than the pre-abortion legalization period.

Building on the work of Zimring (2006), we further inspect the proximate determinants of the risk groups. If the abortion-crime thesis is correct, then we should find a downturn in the births to women in the risk categories (teenagers and blacks). Examining the teen TFR shows that there is evidence of a decline in terms of absolute rates (see Panel A of Figure 2), but an inspection of the proportionality of the change tells a different story about the composition of the birth cohorts. Panel B of Figure 2 illustrates the proportion of the race-specific TFR due to women age 15-19. This graph tracks the proportional change in fertility levels, allowing us to see how much of the race-specific TFR is due to women in the risk categories. From this graph, we see that the proportion of births to black women age

[7] An easy way to identify the average rate of change for the two birth cohorts is to notice the parallelism between the 1965-69 and 1970-74 lines.

15-19, out of all births to black women, rose from 17 to 25% starting in 1960 through 1975, fell slightly during 1976 and 1977, and remained stagnant at 22-23% from 1978 to 1990. This figure raises serious questions about the validity of the abortion-crime thesis: the constant decline in the black TFR during 1965-69 and the increase in the proportion of the black TFR due to teenagers suggests that the marginal children who would later go on to commit crime were not actually aborted, at least not at a rate likely to be sufficient to account for the 1990s crime drop. Moreover, given that the proportion of births to teenage mothers actually increased and remained steady from 1973-1990, it seems unreasonable to attribute the 1990s decline in crime to the marginal child being aborted.

3.1.1 OMITTED VARIABLES THAT AFFECT FERTILITY

While the timing and levels of fertility in the 1970s are an integral part of the abortion-crime thesis, little attention has been paid to the variables that *influenced* fertility during these time periods. In particular, researchers studying the abortion-crime relationship have not directly estimated the effect of contraceptive use and divorce rates on fertility and subsequent abortions. These omitted variables are important for several reasons.

First, the pill enabled women to effectively limit and time their fertility. Oral contraceptives first became available in 1960 when the FDA approved Searle's Enovid as a birth control pill, and by 1965, more than 6.5 million women were users of the pill (PBS Online). Furthermore, Akerlof et al. (1996) and Goldin and Katz (2002) present evidence that married women began using the pill in the early 1960s, with unmarried women obtaining access to oral contraceptives by the late 1960s. Luker (1996) states that the pill was the most popular form of contraception among married women around 1965, and that in 1964, the Office of Economic Opportunity (OEO) began funding birth control for poor women. Moreover, a 1966 bipartisan congressional committee decided that women on welfare—if older than 15—would have access to publicly funded birth control regardless of her martial status (Luker 1996). In 1972, Congress mandated that state welfare departments offer birth control

services to sexually active minors, and in 1974 and 1975, Congress further emphasized that any federally funded family planning clinic had to offer services to adolescents regardless of her martial status (Luker 1996). Considering that the introduction of oral contraceptives and its diffusion from married to unmarried women preceded abortion legalization, it is very important that researchers take into account rates of contraceptive use in order to prevent spurious findings between abortion and crime rates.

Second, abortion-crime researchers have completely ignored the impact divorce has on fertility rates. Divorce matters for two reasons: one, it can suppress fertility, and two, it can lead to a change in an individual's chance to experience crime. During the 1960s and 1970s, most states enacted unilateral divorce laws—"divorce that does not require the explicit consent of both partners" (Gruber 2004). By the end of the 1970s, "nearly every state [had] enacted some form of non-fault divorce" (Simmons 1998). Becker (1993) and Becker et al. (1977) argue that divorce can lower fertility if couples do not invest in or accumulate marriage specific capital (e.g., children) because such capital is intrinsically valuable only to the current marriage and is less valuable after dissolution. Put another way, divorce can alter fertility, *ex ante*, if couples foresee their marriages ending and take extra precautions to prevent having more children. Divorce can also reduce fertility, *post ante*, if divorced men and women do not have sexual partners. Both of these factors would reduce the risk of having an abortion.

Furthermore, if poverty is the underlying mechanism behind criminal offending, proponents of the abortion-crime relationship need to show that divorced women with children did not experience any negative economic effects from unilateral divorce. For example, if children of divorced parents reside with one parent in a household experiencing economic hardship over a long period of time, then those children are now at risk of criminal offending because their socioeconomic conditions have shifted from favorable to unfavorable circumstances. Gruber (2004) finds that children exposed to unilateral divorce obtained less education and lower family income as adults. Research also shows that unilateral di-

vorce has a strong, negative financial impact on women and children for two reasons: first, women's earnings are usually a fraction of their husbands; and second, women are usually awarded primary custody of the children, and divorced wives often do not collect support from their former husbands (Bianchi and Spain 1986; Spain and Bianchi 1996; Duncan and Hoffman 1985). Because larger proportions of female-only-headed households are associated with crime, unilateral divorce helped give rise to an increasing fraction of female-headed households in poverty—often referred to as the *feminization of poverty* (Bianchi and Spain 1986; Spain and Bianchi 1996). Remarriage, however, enables women and children to escape poverty, with 55% of white divorced women getting remarried within five years, compared to 42% of black divorced women (Duncan and Hoffman 1985). There are two implications that follow from this review: divorce can lower period fertility, and it is possible that divorce has placed children of certain cohorts more at risk of future offending, on par with children born to never-married single mothers.

3.2 THE DEMOGRAPHY OF CRIME

During the 1980s and 1990s, it was common to hear racialized colloquialisms about crime—like "black-on-black violence"—in the public domain. Even William Bennett's statement at the opening of this paper implies a relationship between race and crime. Yet, Donohue and Levitt do not control for either race or sex in their models. Accounting for race and sex matters because, in the case of homicide, research shows that victims and offenders share similar characteristics (Fox 2000). Lack of consistent and available data on abortions by race may be the cause of not performing a race-based abortion-crime analysis, but the consequence is that aggregate level models of abortion and crime can give misleading results, particularly in the case of homicide.

Existing research indicates that there are sex differences in criminal activities, arrests, and violence rates (Steffensmeier 1978; 1980; 1983; Steffensmeier and Cobb 1981; O'Brien 1999). Moreover, other studies highlight the effects of racial segregation and concentrated

disadvantage on homicide and violence rates for different racial groups (Harer and Steffensmeier 1992; Peterson and Krivo 1993; 1999; Krivo and Peterson 1996; 2000). Black males experienced the largest drop in the homicide rate beginning in the early-to-mid 1990s (Fox 2000), indicating the importance of controlling for race in multivariate models.[8] Figures 3 and 4 illustrate the difference in homicide rates between blacks and whites by state and year, for men and women age 15-19 respectively. These figures show the difference between black and white homicide rates by sex. For states that have large black, urban centers, the difference in homicide rates is positive, indicating that blacks experience homicide at higher rates than whites. Any analysis that ignores the role of race, sex, and urban centers will miss much of this variability.[9] If abortion has an effect on crime, it is important to know the race and sex of victims and offenders because abortion could reduce the number of victims or offenders, thereby changing the social relationship of crime based on race, if there are racial differences in abortion rates. Obtaining the race of offenders is problematic when studying property crime because victims may not know who vandalized or stole their property. However, by focusing on homicide, researchers are able to test whether race-sex specific abortion rates lead to a corresponding drop in race-sex specific victimization rates. This strategy is justified by a review of the literature.

Another aspect of the demography of crime relates to cohort size. Donohue and Levitt (2001) posit that smaller cohort sizes are one pathway through which abortion may have lowered crime (i.e., the Easterlin (1978) hypothesis). However, Steffensmeier et al. (1987) and Steffensmeier et al. (1992) do not find any evidence for the Easterlin (1978) hypothesis in the case of crime. Moreover, Sen (2005) does not find any evidence that cohort size matters for Canadian crime rates. If cohort size mattered, in the context of U.S. crime rates, then crime should have declined around 1975 because the TFR began to decline in the early 1960s. Yet crime rose between 1975 and 1980. The dynamic process whereby every

[8]The homicide rate is defined as the rate at which people are killed per population exposed to risk, and not the rate at which people kill others per population of killers.

[9]It is important to remember that the drop in non-gun-related homicides among Blacks had been falling at about the same rate since the 1970s.

subsequent cohort is smaller than the preceding one raises a logical conundrum: if relative cohort size matters in reducing future crime rates, then researchers should find a persistent decline in crime rates proportional to the decline in fertility, *ceteris paribus*. Figure 5 shows that crime rates cycle independent of fertility rates, recalling that Figure 1 illustrated a continual decline in fertility from 1960 through the late 1970s.[10] Therefore, there is very little evidence that relative cohort size has any impact on U.S. crime rates.

3.2.1 The Decline in Crime

Donohue and Levitt (2004) further contend that repeal states (California, New York, Washington, Alaska, and Hawaii) experienced drops in crime earlier and larger than non-repeal states. Yet the authors do not provide any summary statistics regarding this claim.

$$\Delta\overline{C_t} = \underbrace{\frac{(C_{xt} - C_{xt-1})}{(C_{xt-1})}}_{\overline{X_t}} - \underbrace{\frac{(C_{yt} - C_{yt-1})}{(C_{yt-1})}}_{\overline{Y_t}} \tag{1}$$

Equation 1 represents the overall mean difference in the relative rate of change ($\Delta\overline{C}$) in crime, between repeal ($\overline{X}$) and non-repeal ($\overline{Y}$) states, given the crime rate (C) for a repeal (x) or non-repeal (y) state in year (t). Put another way, this equation captures the within state variation—relative to the year before—and the between group (repeal or non-repeal) variability, evaluated at the mean. If the abortion-crime thesis is correct, these estimates should be negative, and we can calculate how often the difference in the crime rate is in the predicted direction. The real issue, however, is not only whether the differences in the annual relative rates of change are negative, but also whether these differences are so negative that they could not due to chance alone.[11] Because early abortion-legalizing states

[10] We do not disaggregate the rates by race because we want to show that the argument does not hold at the aggregate level. Our later analyses are disaggregated by race.

[11] To test the difference between two groups, the t-statistic is calculated as $t = \frac{\overline{X_t} - \overline{Y_t}}{\sqrt{\frac{s_1^2}{m} + \frac{s_2^2}{n}}}$ with degrees of freedom v calculated as $v = \frac{\left(\frac{s_1^2}{m} + \frac{s_2^2}{n}\right)^2}{\frac{(s_1^2/m)^2}{m-1} + \frac{(s_2^2/n)^2}{n-1}}$ (Devore 2000), where $\overline{X_t}$ and $\overline{Y_t}$ represent the means of the

began providing services in 1970, 1985 marks the first year in which one would expect to find a negative estimate (i.e., an "abortion" effect) if the positive social externalities from induced abortion occur 15-20 years later, as Donohue and Levitt claim. We test the null hypothesis—that there is no difference in the relative rates of change between repeal and non-repeal states—against the prevailing theory that these differences are negative (i.e., $H_a < 0$). Table 1 presents non-parametric estimates of the average difference in the relative rate of change between repeal and non-repeal states. Donohue and Levitt are correct: Between

Table 1: Non-parametric Estimates of the Average Difference in the Relative Rate of Change Between Repeal and Non-Repeal States with t-Statistics by Crime Type and Year, 1985-2002

Year	Violence	t-stat	df	Homicide	t-stat	df	Property	t-stat	df
1985	-0.033	**-1.55**	6.52	-0.035	-0.45	5.87	-0.035	**-1.55**	4.96
1986	0.027	0.65	5.01	-0.055	-0.85	8.33	0.014	0.93	5.66
1987	0.006	0.10	4.21	0.068	1.31	9.21	-0.041	-1.28	4.29
1988	0.011	0.43	5.95	-0.139	-1.40	4.80	0.001	0.03	4.48
1989	-0.007	-0.31	8.12	0.056	0.51	5.08	-0.023	-1.04	4.87
1990	-0.036	**-2.06**	12.07	-0.051	-0.75	5.94	-0.037	-1.44	4.56
1991	-0.009	-0.15	4.34	-0.070	**-1.46**	14.97	0.008	0.35	5.07
1992	0.020	0.73	6.25	0.016	0.25	8.92	0.011	0.65	7.93
1993	-0.002	-0.04	4.62	-0.084	-0.78	48.79	0.002	0.15	6.19
1994	-0.013	-0.51	5.61	-0.025	-0.30	5.84	-0.003	-0.09	4.69
1995	-0.013	-0.30	4.54	-0.039	-0.27	9.37	-0.017	-0.54	4.38
1996	-0.041	**-2.22**	8.06	-0.153	**-2.98**	24.56	-0.053	**-4.13**	8.63
1997	-0.016	-0.76	6.86	0.078	0.95	4.64	-0.031	**-1.79**	6.81
1998	-0.043	**-1.76**	13.99	-0.224	**-2.95**	5.12	-0.026	-1.27	5.19
1999	-0.018	-0.89	13.80	0.189	0.97	4.24	-0.030	**-3.21**	13.69
2000	-0.039	-1.16	12.60	-0.027	-0.24	4.65	0.025	1.07	4.82
2001	0.001	0.04	5.76	0.007	0.06	5.56	0.003	0.16	5.04
2002	-0.011	-0.63	7.27	-0.109	**-1.53**	8.18	0.035	1.23	4.34
85-02	72%	22%		67%	22%		56%	22%	
95-02	88%	25%		63%	38%		63%	38%	

Data source: Uniform Crime Reports, 1960-2002

Bold t-statistics represent statistically significant differences at $p < .1$ or below for a one-sided two sample t-test.

Authors' Calculations.

relative rates of change for repeal and non-repeal states, respectively. In order for a statistically significant difference at $p < .1$ or below, $t \geq t_{2\alpha,v}$ or $t \leq -t_{\alpha/2,v}$.

1985 and 2002 crime did drop earlier in repeal states, relative to non-repeal states, but the differences in the relative rates of change are generally less than 5% in most years. For instance, repeal states experienced a statistically significant 3.3% greater decline in violence in 1985 compared to non-repeal states. In general, differences in violent relative rates of change are negative in 13 out of 18 years during the 1985-2002 period. However, the probability that these differences are not due to chance alone (at $p < .1$) is much lower. In the case of violence, 4 out of 18 years (22%) have negative differences large enough not to be due to chance alone, thereby providing very little support for an abortion-crime relationship. This dearth of evidence could be due to two reasons. First, crime rose and fell ubiquitously across the nation, thereby not enabling repeal states to enjoy a substantial drop in crime relative to non-repeal states. Second, it is possible that Donohue and Levitt's 15-20 year lag is insufficient to address issues of older cohorts cycling out of crime as the cohorts exposed to legal abortion come of age and cycle into their the peak ages (15-25) of criminal activity over the periods under examination. Using a 25 year lag as the initial starting point makes it possible to examine the pure effect of abortion legalization over the age range in which criminologists document the rise in crime. Examining repeal and non-repeal states starting in 1995—where the 1970 abortion cohort is now age 25 and the 1980 cohort is 15 years in age—allows researchers to track the pure cycling of abortion cohorts during their peak ages of crime. Analysis of the 1995-2002 period shows that Donohue and Levitt's predictions improve, depending on the crime. The probability that these negative differences are large enough not to be due to chance alone is also greater during the 1995-2002 period, with homicide and property crime having measurable differences in 3 out of the 8 years (or 38%). In general, we find that the probability that these differences are due to chance alone is much lower than the observed negative differences in the relative rates of change.

Another way of questioning the validity of the abortion-crime thesis is to perform a quasi-experiment; we compare the relative rates of change during the 1970-84 period to the relative rates of change in the 1985-99 period for each crime. This is essentially a two part quasi-

experiment where the first experiment tests whether the difference in the relative rates of change—between repeal and non-repeal states for the 1985-99 period—is negative (as shown in Table 1), and then we use the 1970-84 estimates as a comparison group (i.e., a baseline of change) for the 1985-99 estimates. The 1970-84 period serves as a control group because no counterfactual cohort of legally aborted fetuses has an effect on the crime rate during these years. The 1985-99 period serves as the treatment group because synthetic cohorts of legally aborted fetuses would begin transitioning into their peak ages of criminal activity starting in 1985. Figure 6 plots the probability (p-value) that the difference in the relative rates of change—between repeal and non-repeal states—is due to chance alone, as a function of time for each crime. There are three horizontal lines marking the 10, 5, and 1% probability levels. The white area represents negative differences in the relative rates of change and the shaded area indicates positive differences in the relative rates of change. The vertical line at 1985 marks the first year the 1970 cohort of aborted fetuses would have had an impact on crime rates. If abortion legalization had an impact on crime in 1985-99, then we should find very different trends in both the sign of the difference (positive or negative) and how frequently these differences are due to chance alone. For homicide and property crime, we do not find any large differences between the 1970-84 and 1985-99 periods, with the differences in the relative rates of change being just as likely to be positive or negative in either period.[12] In the case of violence, there is a large decline in the number of positive relative rates of change post 1985; however, there is not much change in how often these differences are due to chance between the test and control periods at the 5% probability level.

To understand why so many of the differences in the relative rates of change—between repeal and non-repeal states—are due to chance alone (i.e. $p > .1$), we examine the relative rates of change for repeal states only. A closer inspection of the repeal states reveals tremendous variation in the three crime categories for the periods under examination. Figure 7 displays the considerable variability in the relative rates of change by state, year, and crime.

[12] Another way of visually recognizing this is that the 1985-99 trend is almost a mirror image of the 1970-84 trend.

The left column is for California, New York and Washington, while the right column includes Alaska and Hawaii. Each row is for a different crime. During the 1985-90 period, the negative effects reported in Table 1 are largely driven by sharp and intense declines in Alaskan crime rates, with California contributing minimally in 1987. Beginning in the early 1990s, California and New York exhibit long, negative relative rates of change; however, the extreme volatility experienced in Alaska and Hawaii—in the case of violence and homicide—offsets the slow and gradual declines in California, New York and Washington, thereby reducing the average relative rate of change for repeal states after 1990. These figures provide evidence that comparing repeal and non-repeal states alone may be insufficient to ascertain whether or not abortion legalization had an effect on the crime decline because not all repeal states experience clear and consistent trends in their crime declines. A better methodological approach would be to match repeal and non-repeal states based on state characteristics and calculate the difference in the crime rates between matched repeal and non-repeal states.

3.3 A LEXIS MODEL APPROACH: AGE-PERIOD-COHORT DISTINCTIONS

Donohue and Levitt's (2001) findings rely on the use of state-specific time series methods to estimate the overall impact of abortion legalization on crime. Although they use year and state fixed-effect models to examine within-state change in crime rates due to abortion legalization, many critics complained that such methods did not account for the crack-cocaine epidemic of the 1980s. In their more recent paper (2004), they compare the 1965-70 and 1970-75 birth cohorts over six year periods in order to tease out the causal effects of abortion legalization on crime. Methodologically, they use differences-in difference-in difference (DDD) estimation to obtain these effects; however, there are two problems with both papers. First, in the 2001 edition, they employ an age-period approach to estimating the overall abortion effect (i.e., time-series methods). The problem with this approach is that different age groups have different period-specific life experiences for any given age.

Interrupted time series models that control for age but not period events tend to mix period-cohort effects across the age distribution, and it becomes very difficult to tease out period and/or cohort effects for any given age. This makes it extremely difficult to compare 20 year-olds in 1985 to 20 year-olds in 1990. As seen in Panel A of Figure 8, a Lexis model is the clearest way to illustrate this problem.[13] The three diagonal lines highlight the birth cohorts of 1965-69, 1970-74, and 1975-79 (A, B, and C respectively) as they age 30 years. We focus on these cohorts at ages 15-19, 20-24, and 25-29 (1, 2, and 3, respectively). Time series models that control for age look like C1, A2, and older cohorts in 1995 (see the vertical line). Because 15 to 30 year-olds in each of these three cohorts will experience period events at different ages (e.g., some will experience crack-cocaine at younger ages than others), it is important to control for these events to the best of one's abilities. Examining the time period alone insufficiently "solves" this problem. In studying the abortion-crime relationship, researchers need to explicitly control for states that experienced the crack-cocaine epidemic first in the same way that they identify states that legalized abortion first. Wilmoth (1990) and Wilmoth et al. (1990) discuss age-period-cohort effects, in the context of mortality, in such a manner.

The second problem is that in Donohue and Levitt's (2004) attempt to correct the aforementioned problem, the birth cohorts they intend to compare do not age properly. They define two birth cohorts (1965-70 and 1970-75) that age five years over a six year period. This is either an editing oversight or there is a serious problem with the way the authors are dealing with the last birth year in their defined cohorts.[14] Moreover, they only capture half of the cohort age range. Consider, for example, Table 2 of their 2004 paper. In this table they examine the 1970-75 cohort exposed to abortion legalization during the 1980-85 period. They report that this cohort should be 10-14 years of age for that defined period. However,

[13] A Lexis model is "a complete description of a population over age and time, including detailed estimates of population size (and perhaps characteristics), as well as the transition rates (and events) associated with changes in population size (and characteristics), as a function of both age and time," Wilmoth (2005).

[14] By this we mean that 1970 should either be included or excluded in the first birth cohort, and if excluded, it should be included in the second cohort only if 1975 is excluded from the second cohort, assuming Donohue and Levitt really want the cohorts to age by 5 years.

the 1970-74 birth cohort *should be* age 5-14 for the period 1980-84 *and not* 10-14.[15] As a result, the authors only capture five-sixths of the top triangle of the period-cohorts they intend to study. To present this more clearly, consider Panel B of Figure 8. Here we have shifted the life-lines of our three cohorts such that we track the same cohort across periods. Imagine a horizontal line drawn at age 10. If you compare DL1 in Panel A to DL2 in Panel B, the age misappropriation is evident: according to Donohue and Levitt, the 1970-74 birth cohort is [10,15) years in age (see DL1 in Panel A); however, this birth cohort is actually composed of [5, 15) year-olds (see DL2 in Panel B). In this heuristic, it is easy to see that the authors are only capturing the top triangle of the period-cohort, which represents members who were born in the first half of 1970-74 cohort.[16] In order to ensure that members born in the latter half of the birth period are properly included in the analysis, one must also examine 10-14 year olds in the 1985-89 period because they are also members of the 1970-74 birth cohort. At older ages in later periods, this problem of not capturing the other half of the birth cohort leads to biased crime estimates by age, period, and cohort.

Using the Lexis framework, period-cohorts will experience different period events at the same age, and this will facilitate cohort comparisons more clearly. In other words, researchers can compare A1 to B1, B1 to C1, A2 to B2, and so on (see Panel B) to consistently and accurately tease out abortion-related effects.

[15]For clarity purposes, we use standard demographic nomenclature for denoting age and period ranges. For example, for the 1980-84 period, the birth cohort of 1965-69 versus 1970-74 would be between 10-19 and 5-14 years of age, respectively. In Donohue and Levitt's analysis, the 1970-75 birth cohort should be 4-15 for the 1980-85 period (i.e., their period-cohort should have an age range of 12 years.

[16]Wilmoth et al. (2005) have a more elaborate and detailed discussion and illustration of this in the Methods Protocol of the Human Mortality Database.

4 DATA, MEASURES, METHODS, AND MODELS

4.1 DATA

Because of the inherent complexity in studying the abortion-crime relationship, we created a unique and rich dataset to properly address our criticisms of previous research on this topic. The data we use come from many different sources. We obtained race-specific abortion counts from the Center for Disease Control (CDC) Summary Surveillance Reports for 1970-1980, and we got the total number of abortions from the Alan Guttmacher Institute (AGI) by way of Donohue and Levitt's data. We use Census data from 1960-2000 to obtain population counts for subpopulations for the 50 states and D.C.[17] Also, we use data from the 1988 National Survey of Family Growth (NSFG) Cycle IV to reconstruct the sexual histories of women. Based on where women grew up, we obtain state-level estimates of contraceptive use, formal sexual education, and age at first sex. We do not include any abortion estimates from the NSFG due to known underreporting (Zavodny 2001). Data on divorce rates come from the National Center for Health Statistics for the 1968-1978 period. We also make use of information on homicide from the Supplementary Homicide Reports (SHR) for the 1976-2000 period. The SHR provides detailed victim and offender information (e.g., race, sex, age, and relationship to offender). We focus solely on the victimization part of the data because official crime statistics are based on victimization. Donohue and Levitt's original covariates are also included in our dataset. Lastly, we include state-specific estimates of the intensity of the crack-cocaine epidemic from Fryer et al. (2005) in order to control for the effects of crack diffusion on crime.

4.2 MEASURES

One of Joyce's (2004a) criticisms centered on Donohue and Levitt's (2001) use of abortion ratios (i.e., the number of abortions per 1000 live births). Yet Donohue and Levitt (2004)

[17]We interpolated population counts for intercensusal years.

contend that even if they use abortion rates (the number of abortions per 10,000 women age 15-44), they continue to find an abortion-crime link. Given Donohue and Levitt's contention that it does not matter whether one uses abortion rates or ratios, we use AGI, CDC, and Census data to obtain race-specific abortion rates. In Equation 2, we calculate the race-specific abortion rate (ABR) as

$$ABR_{rst} = ({}^{AGI}A_{st}) * \frac{{}^{CDC}A_{rst}}{{}^{CDC}A_{st}} * \frac{100000}{W_{rst}} \tag{2}$$

where ${}^{AGI}A_{st}$ is the number of abortions in state s during year t from the Alan Guttmacher Institute; ${}^{CDC}A_{rst}$ is the number of abortions of race r in state s during year t from the Center for Disease Control; and W_{rst} is the period person years lived by women age 15-44.[18] Therefore, ABR_{rst} is the race-specific abortion rate per 100,000 women of reproductive age.

We calculate the divorce rate as a true rate (the hazard of divorce) and not the number of divorces per adult population. Our contraceptive rates are the result of aggregating female responses to the state level, and calculating the probability that women used any particular fertility-limiting method while residing in a state during a particular point in time. The homicide rate is the number of people killed per population exposed to risk. The covariate we use to describe the crack epidemic is based on the work of Fryer et al. (2005) and measures the severity of the epidemic by year and state, accounting for the racial composition of the state. See Table 2 for a detailed description of our variables and their data sources.

[18]It is worth noting that the first part of our abortion rate—$({}^{AGI}A_{st}) * \frac{{}^{CDC}A_{rst}}{{}^{CDC}A_{st}}$—is commonly used to estimate the underreporting of abortions in the National Survey of Family Growth. For more on this, see Zavodny (2001).

Table 2: Variable Descriptions and Sources

Variable	*Description*	*Source*
abortionCDC	Race-specific Abortion Counts	CDC Summary Surveillance Reports, 1970-1980
abortionAGI	Total Number of Abortions	Donohue & Levitt (2001) / AGI
ABRwhite	Abortion rate per 100,000 white women in state pop	Sykes et al. (2005)
ABRblk	Abortion rate per 100,000 black women in state pop	Sykes et al. (2005)
pop	population counts	U.S. Census Bureau 1960-2000
divorce	the probability of divorce	NCHS, 1968-1978
homicide	homicide rate per 100,000 population	SHR & U.S. Census Bureau, 1976-2000
crack	intensity of crack-cocaine epidemic	Fryer et al. (2005)
violence	# of violent crimes per 1000 ppl	Donohue & Levitt (2001)
property	# of property crimes per 1000 ppl	Donohue & Levitt (2001)
murder	# of murders per 1000 ppl	Donohue & Levitt (2001)
prison	ln(state # of prisons per capita), lagged one year	Donohue & Levitt (2001)
police	ln(state # of police per capita), lagged one year	Donohue & Levitt (2001)
unemployment	% state pop unemployed	Donohue & Levitt (2001)
income	ln(state income per capita, 97 dollars), BEA.gov	Donohue & Levitt (2001)
poverty	% of state pop below poverty line	Donohue & Levitt (2001)
afdc	welfare generosity per family	Donohue & Levitt (2001)
beer	beer consumption per capita	Donohue & Levitt (2001)
fb	% of foreign-born individuals living in the state per 1000 residents	Donohue & Levitt (2001)
gunlaw	shall-issue concealed weapons law, dummy	Donohue & Levitt (2001)
fertility	birth rate per 1000 women age 15-44 in state pop	Donohue & Levitt (2001) and Census Bureau
pill	fraction of women in state population that used the pill	NSFG Cycle IV
condom	fraction of women in state population that used condoms	NSFG Cycle IV

CDC is the Center for Disease Control, 1970-1980

AGI is the Alan Guttmacher Institute, 1970-1980

NSFG is the National Survey of Family Growth, 1988

NCHS is the National Center for Health Statistics, 1968-1978

SHR is the Supplementary Homicide Reports, 1976-2000

4.3 METHODS

Abortion legalization could have two competing effects on fertility: one, abortion legalization could lower fertility if the pregnancy is unwanted or untimed; or two, abortion legalization could raise fertility because the costs of sexual intercourse are much lower and women who become pregnant may decide to keep the unplanned pregnancy if it conflicts with her moral or religious values. We use two stage least squares to test if abortion rates decreases fertility and to estimate the effect of (predicted) fertility on crime. Because the costs of unwanted pregnancy is much lower after legalization of abortion, it is plausible that legalization decreased the fertility rate only marginally. This would be evidence that legalized abortion increased the prevalence of unwanted childbearing. Given that we have detailed state-level information on contraceptive methods, fertility, abortion, and divorce hazards, we are able to estimate the relationship between abortion and crime in two stages. First, we estimate the effect of race-specific abortion rates on fertility rates, controlling for other plausibly influental variables. With the estimated coefficients, we predict fertility rates and use them in the second stage equation to test the relationship between fertility rates and crime.

4.4 MODELS

We use two stage least squares to resolve the endogeneity between abortion and fertility. If abortion has an effect on future crime rates, these effects would act through reductions in fertility. We model the first stage as equation 3

$$\begin{aligned} fertility_{st-15} &= \beta_0 + \beta_1 ABRblk_{st-15} + \beta_2 ABRwhite_{st-15} + \beta_3 unemployment_{st-15} \\ &+ \beta_4 income_{st-15} + \beta_5 afdc_{st-15} + \beta_6 fb_{st-15} + \beta_7 pill_{st-15} \\ &+ \beta_8 condom_{st-15} + \beta_9 divorce_{st-15} + \lambda_s + \theta_t + \varepsilon \end{aligned}$$

$$where \quad \varepsilon \sim \mathbf{N}(0, \sigma^2) \tag{3}$$

where we control for the effects of state characteristics, abortion rates by race, contraceptive use by type, and the divorce probability on the birth rate (fertility) for each state (s) and year (t). We also include year and state fixed effects. The second stage, as shown in equation 4, models the effects of the fertility rate and state-level characteristics on murder, violent, and property crime rates, with year and state fixed effects. The predicted fertility rate is the weighted average of the number of fertility-lags in any year, where each fertility-lag is given a weight proportional to its overall effect in that year.[19]

$$\begin{aligned} Crime_{st} &= \beta_0 + \beta_1 \widehat{fertility}_{st-[15,\bar{1}9)} + \beta_2 prison_{st} + \beta_3 police_{st} + \beta_4 unemployment_{st} \\ &+ \beta_5 income_{st} + \beta_6 gunlaw_{st} + \beta_7 beer_{st} + \beta_8 crack_{st} + \beta_9 fb + \lambda_s + \theta_t + \varepsilon \end{aligned}$$

$$where \quad \varepsilon \sim \mathbf{N}(0, \sigma^2) \tag{4}$$

5 FINDINGS

5.1 Two Stage Least Squares Results

Table 3 presents our first stage estimates of the effect of race-specific abortion rates, contraception, and divorce on fertility rates for all states during the 1970-1980 period.

[19]Because Donohue and Levitt contend that abortion legalization lowered crime 15-19 years later, the abortion effects could have as many as five lags in any one calendar year. We construct a weighted average of the influence of those lags for that year. For instance, if abortion has an effect on crime (via fertility) in 1987, the first legal abortion cohorts are now ages 15, 16, and 17. The overall effect of abortion on fertility for each of these cohorts is not equal, so we weight the three predicted fertility-lags (15, 16, and 17) proportional to their overall contribution in order to obtain one weighted estimate of predicted fertility from 1970-72 for our models of period crime rates in 1987.

Table 3: First Stage Estimates of the Effect of Race-Specific Abortion Rates, Contraceptive Use, and the Hazard of Divorce on Birth Rates, 1970-1980

	Model 1		Model 2		Model 3	
	coef.	se	coef.	se	coef.	se
ABRblk/1000	.017	(.005)	.050	(.024)	.073	(.022)
ABRwhite/1000	.018	(.187)	–.217	(.137)	.006	(.234)
unemp	–78.562	(17.832)	–57.565	(20.241)	–89.550	(29.092)
income	–.118	(5.722)	2.279	(12.226)	–12.889	(7.993)
afdc1000	.676	(.241)	.137	(.252)	.438	(.296)
fb/1000	.002	(.069)	.048	(.074)	–.219	(.091)
pill			–1.732	(.675)	–.525	(1.071)
condom					.186	(1.338)
divorce					–1.902	(3.626)
constant	95.828	(53.962)	75.560	(115.314)	160.797	(100.869)
Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
R^2	.982		.988		.994	
N	459		281		171	

Note: All models are adjusted for Huber-White robust standard errors.
The state birth rate (the number of births per 1000 women age 15-44) is the dependent variable in all three models.
Authors' Calculations

As you can see, race-specific abortion rates have no effect on birth rates in all three models. Pill use, however, has a statistically significant and negative effect on fertility (model 2). Reductions in our sample size may explain why divorce rates and condom use (model 3) do not have statistically significant effects on fertility (i.e., state level divorce data are limited during this period). We find that the pill (and not abortion) had a large effect on reducing birth rates in the 1970s. This could be due to the diffusion and ubiquity of pill use before and after any state had legal abortion. This finding would explain why Figure 1 shows parallel slopes when fertility began to decline during the mid-to-late 1960s.

If abortion reduces fertility, then these fertility reductions should lead to less crime if the abortion-crime thesis is correct. Although our first stage estimates do not support this

hypothesis, we test the effect of fertility on future crime rates. Table 4 shows the effect of abortion on crime rates via predicted fertility without controlling for pill use (Model 1 from Table 3). The coefficient on our predicted fertility rate in the second stage is positive and not significant in any of the models. This implies that abortion has no statistically significant effect on crime rates via birth rates. The crack-cocaine epidemic, however, has a positive and statistically significant effect on increasing murder, violent, and property crime rates.

Table 4: Second Stage Estimates of the Effect of Abortion on Crime Rates via Predicted Fertility, with Controls for the Crack-Cocaine Epidemic, 1985-1999

	Violence		Property		Murder	
	coef.	se	coef.	se	coef.	se
$\widehat{\texttt{fertility}}$	3.333	(4.468)	33.991	(25.395)	.071	(.093)
prison	–50.691	(27.775)	–913.316	(184.193)	–1.672	(.571)
police	16.616	(42.052)	–265.910	(226.115)	–1.092	(.870)
unemp	–356.971	(365.529)	4935.017	(1789.579)	–11.008	(7.820)
income	–119.520	(144.476)	947.990	(753.961)	–4.893	(2.584)
gunlaw	23.939	(10.606)	253.733	(61.444)	.305	(.224)
beer	1.364	(2.281)	43.911	(11.849)	.049	(.042)
crack	14.820	(3.675)	49.024	(26.313)	.209	(.074)
fb/1000	–1.374	(.514)	–8.636	(3.250)	–.006	(.009)
constant	989.810	(1598.318)	–9635.652	(8349.219)	41.377	(28.992)
Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
R^2	.961		.935		.925	
N	550		550		550	

Note: All models are adjusted for Huber-White robust standard errors.
Authors' Calculations

In model 2 of Table 3 we found that increased pill use (and not abortion) led to reductions in state level birth rates. We model the effect of this predicted fertility rate (controlling for pill use) on murder, violent, and property crime rates in Table 5. We find that a rise in the birth rate actually leads to lower property crime, providing evidence that pill use could actually lower crime via fertility depending on who used oral contraceptives. Specifically,

the pill was used by cohorts of young women to delay their fertility until a desired time. If the initial cohorts of pill users delayed their fertility to older ages, thereby increasing period birth rates in later years, then our findings for property crime would be consistent with this story. With regard to murder and violent crime, there is no evidence that reductions in fertility actually lower future crime rates due to abortion and pill use. The crack-cocaine epidemic has a strong, positive effect on crime rates. In general, our second stage estimates further confirm that the abortion-crime thesis is invalid.

Table 5: Second Stage Estimates of the Effect of Abortion and Pill Use on Crime Rates Via Predicted Fertility, with Controls for the Crack-Cocaine Epidemic, 1985-1999

	Violence		Property		Murder	
	coef.	se	coef.	se	coef.	se
$\widehat{\texttt{fertility}}$	1.440	(3.136)	–34.208	(16.206)	.015	(.064)
prison	–11.308	(28.988)	–827.950	(191.593)	–1.485	(.600)
police	27.057	(41.681)	–258.863	(244.871)	–1.474	(.908)
unemp	–160.022	(385.826)	6308.592	(2027.027)	–8.487	(8.353)
income	88.292	(157.523)	1798.344	(748.796)	–3.411	(2.933)
gunlaw	16.492	(10.047)	265.214	(59.999)	.326	(.238)
beer	3.985	(3.630)	47.278	(15.189)	.075	(.055)
crack	11.903	(3.695)	49.306	(28.690)	.157	(.072)
fb/1000	–1.755	(.667)	–8.499	(3.247)	–.009	(.012)
constant	253.015	(1580.805)	–9009.616	(7468.783)	48.597	(29.051)
Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
R^2	.968		.942		.932	
N	481		481		481	

Note: All models are adjusted for Huber-White robust standard errors.
Authors' Calculations

5.2 Race-Based Homicide Results

Absent in the abortion-crime debate is the complex social relationships between victims and offenders. Because crime rates can be reduced through fewer victims or fewer offenders to harm the non-offending population, we test whether race-specific abortion rates directly

affect homicide rates given the race (r) and gender (g) of the offenders (O) and victims (V) in state (s) at time (t). Equation 5 formally displays our model.

$$\begin{aligned} V_{rgst}O_{rgst} &= \beta_0 + \beta_1 ABRblack_{st-[15,\bar{1}9)} + \beta_2 ABRwhite_{st-[15,\bar{1}9)} \\ &+ \beta_3 prison_{st} + \beta_4 police_{st} + \beta_5 unemployment_{st} \\ &+ \beta_6 income_{st} + \beta_7 gunlaw_{st} + \beta_8 beer_{st} + \beta_9 crack_{st} \\ &+ \beta_{10} fb_{st} + \beta_{11} fertility_{st-15} + \lambda_s + \theta_t + \varepsilon \end{aligned}$$

$$where \quad \varepsilon \sim \mathbf{N}(0, \sigma^2) \tag{5}$$

Table 6 presents our race based results. All standard errors are corrected in these Prais-Winsten models. Model 1 shows that, independent of the victim and offender's race and gender, race-specific abortion rates have no impact on aggregate homicide rates. All other covariates are in the expected direction. Similarly, estimates from Model 2 provides evidence that black abortion rates had no statistically significant, negative effects on male black-on-black homicide rates, and the same is true for white male homicides where the perpetrators were white men (Model 3). These findings indicate that the race and sex of victims and offenders are crucially important in investigating the plausibility of the abortion-crime thesis.

Table 6: Estimating the Effect of Race-Specific Abortion Rates on Victim-Offender Race-Sex Homicide Rates, 1988-1999

	All Murder		BM-BM		WM-WM	
	coef.	se	coef.	se	coef.	se
ABRblack	.00001	(.0001)	.012	(.006)		
ABRwhite	–.0002	(.0001)			–.006	(.007)
prison	–1.778	(.550)	–50.441	(34.283)	–43.935	(24.424)
police	–1.301	(.942)	13.637	(50.744)	13.932	(36.700)
unemp	–14.578	(7.438)	–505.423	(442.348)	135.454	(303.087)
income	–8.342	(2.729)	–195.280	(133.469)	–189.757	(110.289)
gunlaw	.479	(.219)	.289	(10.901)	2.530	(7.940)
beer	.012	(.036)	3.342	(1.723)	1.562	(1.069)
crack	.201	(.071)	7.477	(4.016)	.929	(2.481)
fbrate	–.009	(.010)	–1.799	(.614)	–.445	(.491)
fertility	.007	(.013)	–1.505	(.627)	–.914	(.578)
constant	100.731	(27.941)	–2097.044	(1344.651)	1915.855	(1062.805)
Year FE	Yes		Yes		Yes	
State FE	Yes		Yes		Yes	
R^2	.931		.918		.972	
N	483		483		483	

Note: All models are corrected for heteroskedastic standard errors using Prais-Winsten.
Authors' Calculations using SHR, CDC, NSFG, Census, Donohue & Levitt, & Fryer et al. data.

6 CONCLUSIONS

We have presented non-parametric and parametric evidence that abortion legalization did not have any measurable effect on future crime rates. First, we showed that there were not qualitative changes in birth cohorts immediately after abortion legalization because births to black, teenage mothers did not decline significantly (Panel B of Figure 2). As a matter of fact, births to black teenagers rose between 1970 and 1974, with the proportion of births remaining stagnant between 1975-1990 at late 1960 fertility levels.

Second, findings from our non-parametric, quasi-experimental analysis (Table 1 and Figure 6) suggest that the timing of abortion legalization did not translate into significantly

different trends in the relative rates of change in crime rates for repeal and non-repeal states. Using the 1970-84 period as a control group for the 1985-99 treatment group (i.e., cohorts exposed to abortion legalization), we do not find any measurable differences between the relative rates of change in the crime rates.

Third, evidence from our parametric analysis (Table 3) highlights the effects contraceptive methods, divorce, and race-specific abortion rates have on fertility. Our research indicates that abortion rates affected fertility only marginally. This is also evident in Figure 1 because the race-specific TFRs show that the average rate of decline *before* abortion legalization was the same as the average rate of decline *after* abortion legalization. Because the pill was widely used by both married and unmarried women before legalized abortion, it is possible that the abortion-crime finding is the result of omitting this very important variable from the analysis.

Furthermore, Tables 4 and 5 underscore the fact that abortion did not have any effect on crime via fertility once period crack effects and pill use are controlled. Although previous research makes use of fixed effects models that account for omitted variables, we explicitly control for the most important omitted factors (the intensity of the crack-cocaine epidemic, contraceptive use, divorce rates, and race specific abortion rates) in our fixed effects models, which provide a more precise picture of the relationship between abortion and crime. Until now, abortion-crime researchers have been unable to model precise social relationships between race-sex specific abortion and crime rates because the Alan Guttmacher Institute (AGI) data are believed to have accurate numbers of abortions, while the CDC is believed to have correct race-specific abortion proportions. Combining information from the CDC and AGI allows researchers to track partial correlations between abortion and crime given the observed social context wherein intragroup victimization and offending is known to occur (as exemplified in homicide data).

The apparent abortion-crime relationship is the result of a misspecification between abortion and crime by race and sex. Previous research assumes that the racial distribution of

abortions and crimes are similar, which is false. Homicide is an intragroup phenomenon. Therefore, if whites used abortion to time their fertility more than blacks, and blacks disproportionately experience crime (see Figures 3 and 4), then the drop in black crime rates would not be due to white aborted fetuses. This race-specific abortion-crime mismatch should be thought of in the context of residential segregation and spatial isolation. If the individuals most prone to experience certain crimes are socially isolated by race from others who are least likely to experience the phenomena, then a race analysis is further warranted. The impact of other important omitted variables—contraceptive methods, the crack-cocaine epidemic, divorce, racial differences in victimization and offending, for example—also need to be included within this framework. Furthermore, by not including the entire birth cohort exposed to risk (see the period-cohort discussion), previous estimates are biased because the period-cohorts do not age properly.

Lastly, it is important to remember that not all people who are "at risk" of being criminals actually experience crime, either as victims or offenders. By treating an entire synthetic cohort of unborn children as victims or offenders gives very extreme and distorted views about poverty, single parenthood, and crime, for there are plenty of poor children from single-parent households that never commit crimes. A more precise measure would net out the fraction of the cohort that never experiences crime during year $t+15$ versus the remaining fraction that is composed of either victims or offenders. This is necessary because crime can be reduced through fewer victims or through fewer offenders that would harm members of the non-offending population. All of these issues paint a very broad picture of why the link between abortion legalization and the crime decline is the result of not accounting for other important factors that influenced fertility around the time of abortion legalization.

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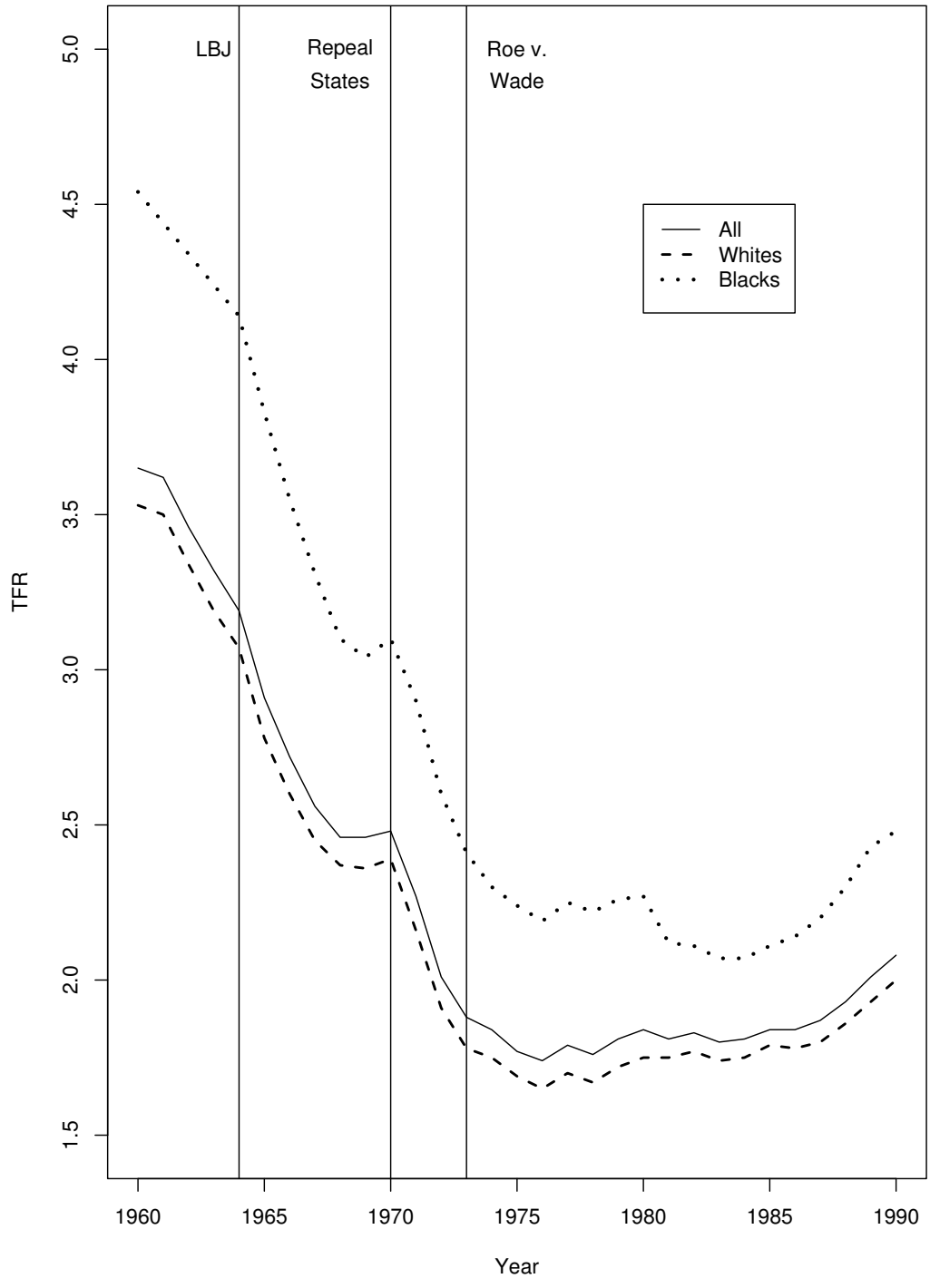
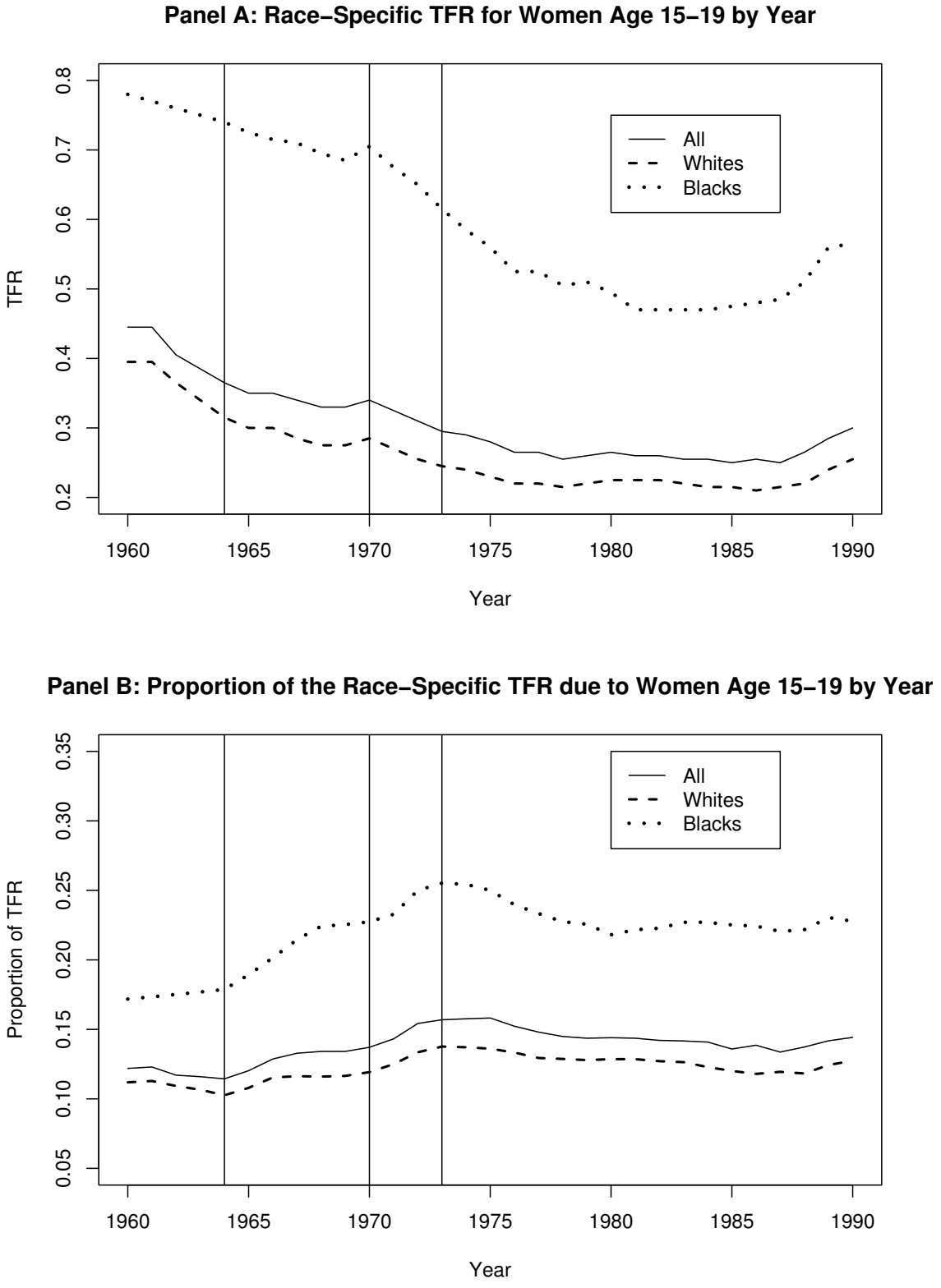
Figure 1: Total Fertility Rate (TFR) by Race and Year, U.S. 1960-1990



Source: National Center for Health Statistics (1990)

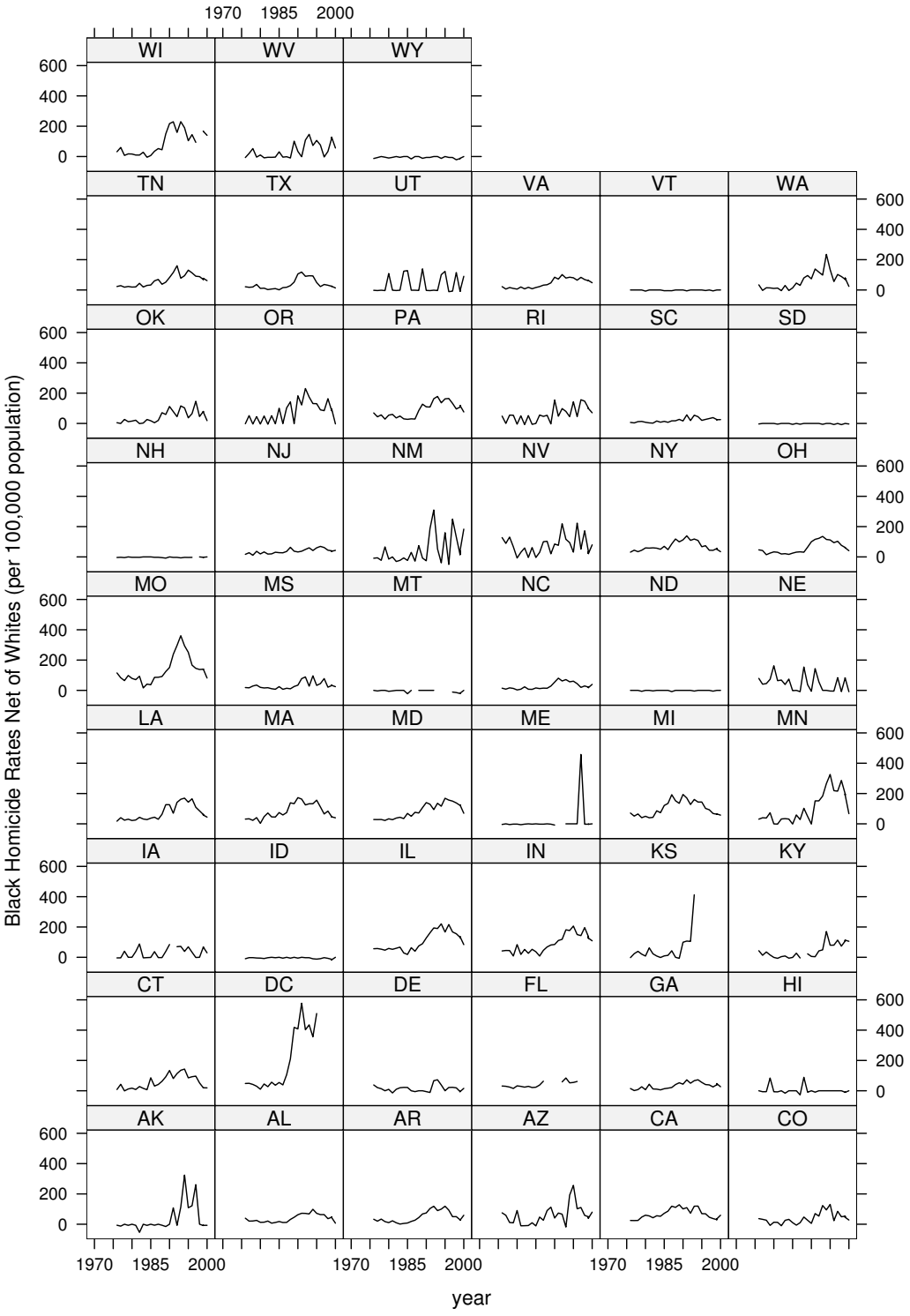
Note: Due to missing data, values for the Black TFR have been linearly interpolated for 1961-1963.

Figure 2: Total Fertility Rate (TFR) for Women Age 15-19 by Race and Year, U.S. 1960-1990



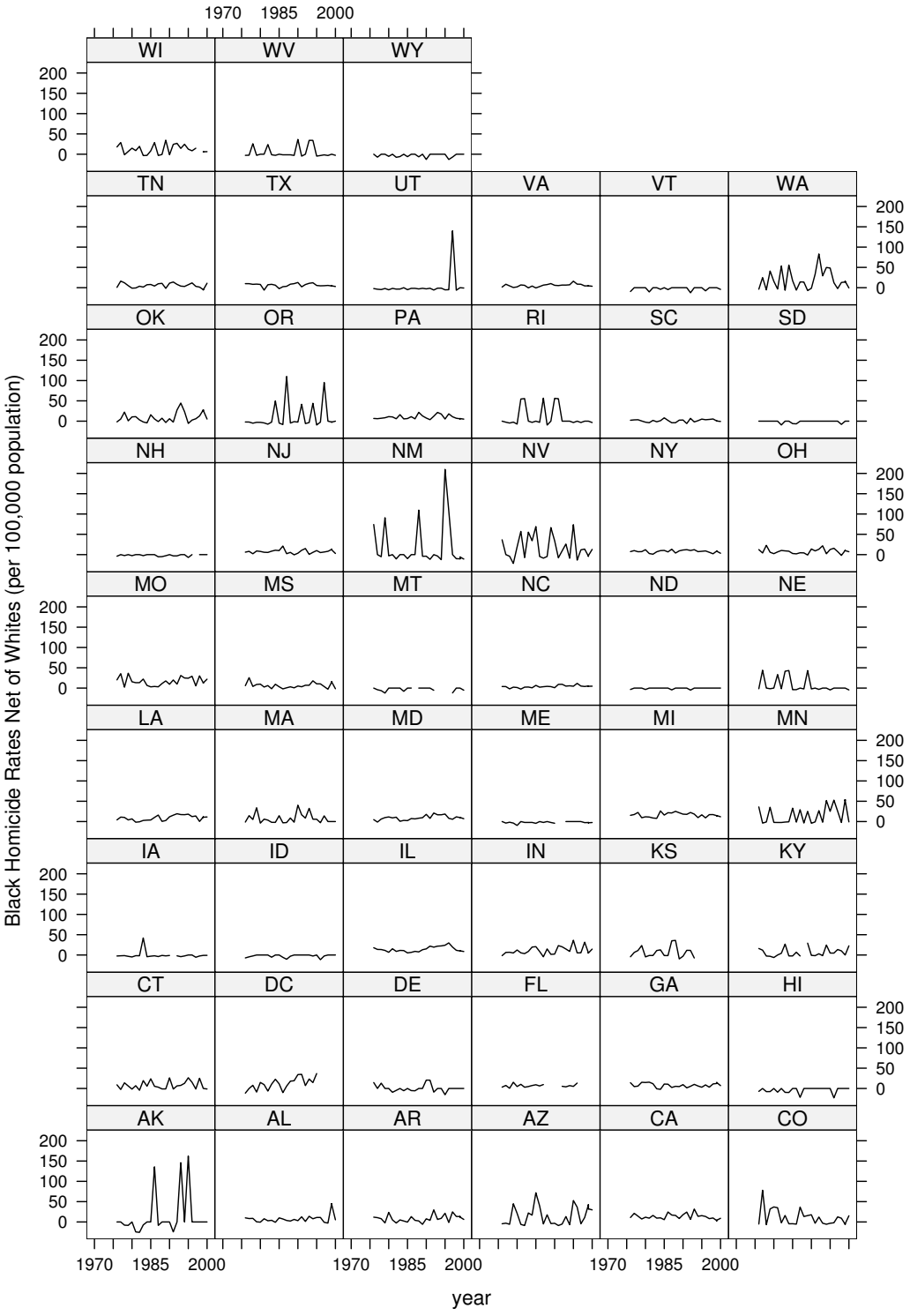
Source: National Center for Health Statistics (1990)

Figure 3: Comparison of Homicide Victimization Rates for Black and White Males Age 15-19 by State & Year, 1976-2000



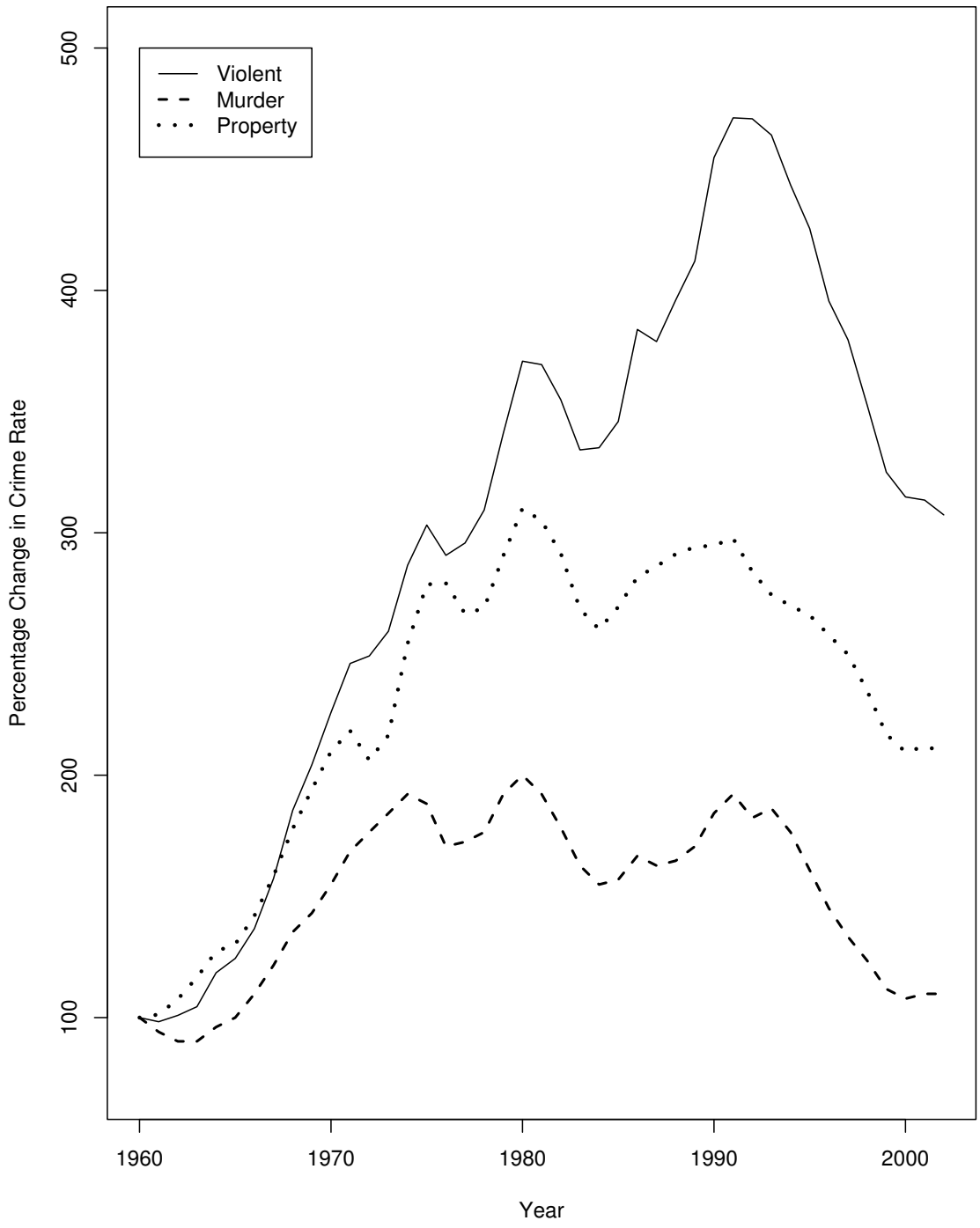
Source: Uniform Crime Reports, Supplementary Homicide Reports, 1976-2000
Authors' Calculations

Figure 4: Comparison of Homicide Victimization Rates for Black and White Females Age 15-19 by State & Year, 1976-2000



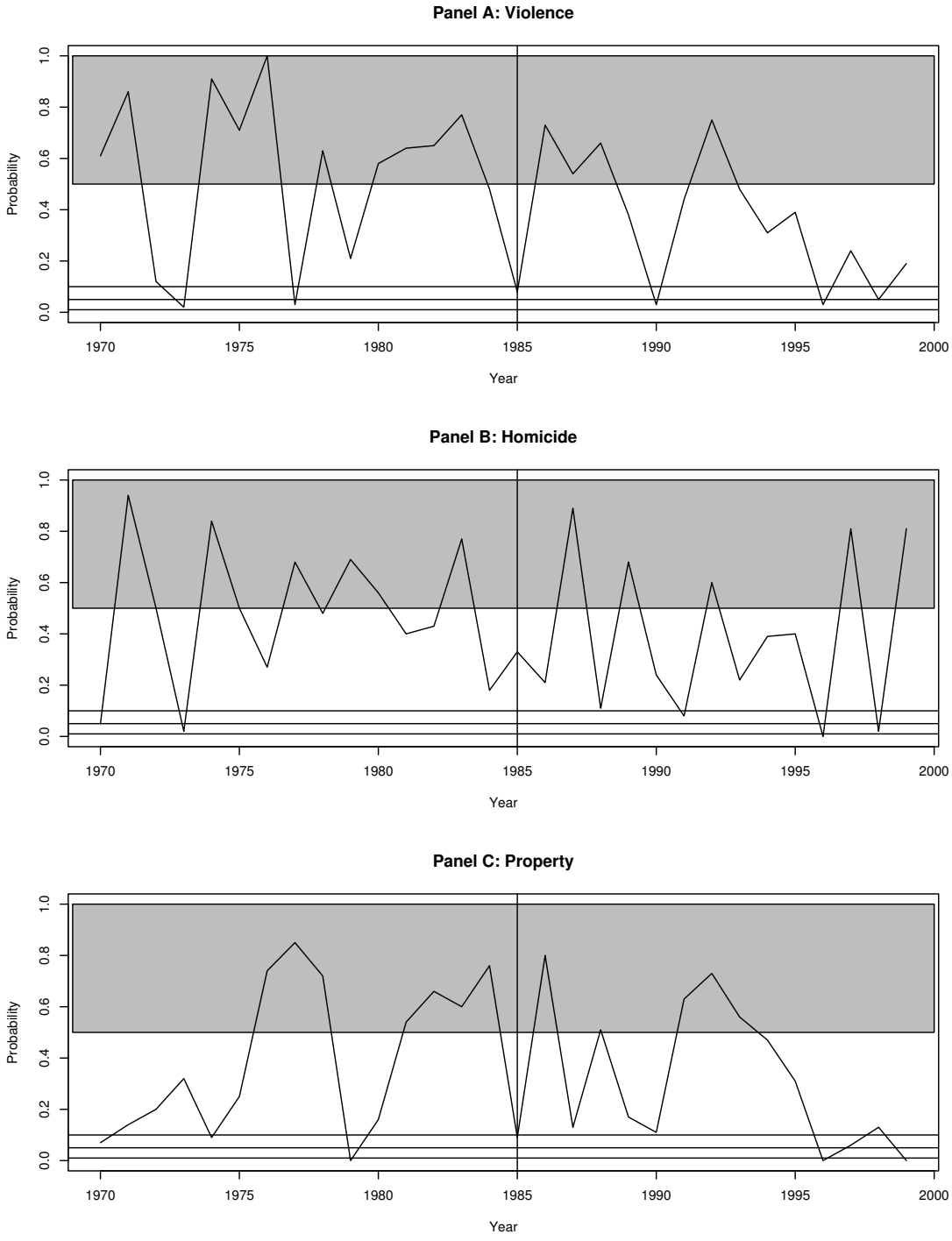
Source: Uniform Crime Reports, Supplementary Homicide Reports, 1976-2000
Authors' Calculations

Figure 5: Change in the Crime Rate Relative to 1960 by Crime and Year (in Percentage Terms), U.S. 1960-1990



Source: Uniform Crime Reports, 1960-2002
Author's Calculations

Figure 6: P-values from Testing Whether the Difference in the Relative Rates of Change Is Statistically Different from Zero by Crime & Year, 1970-1999

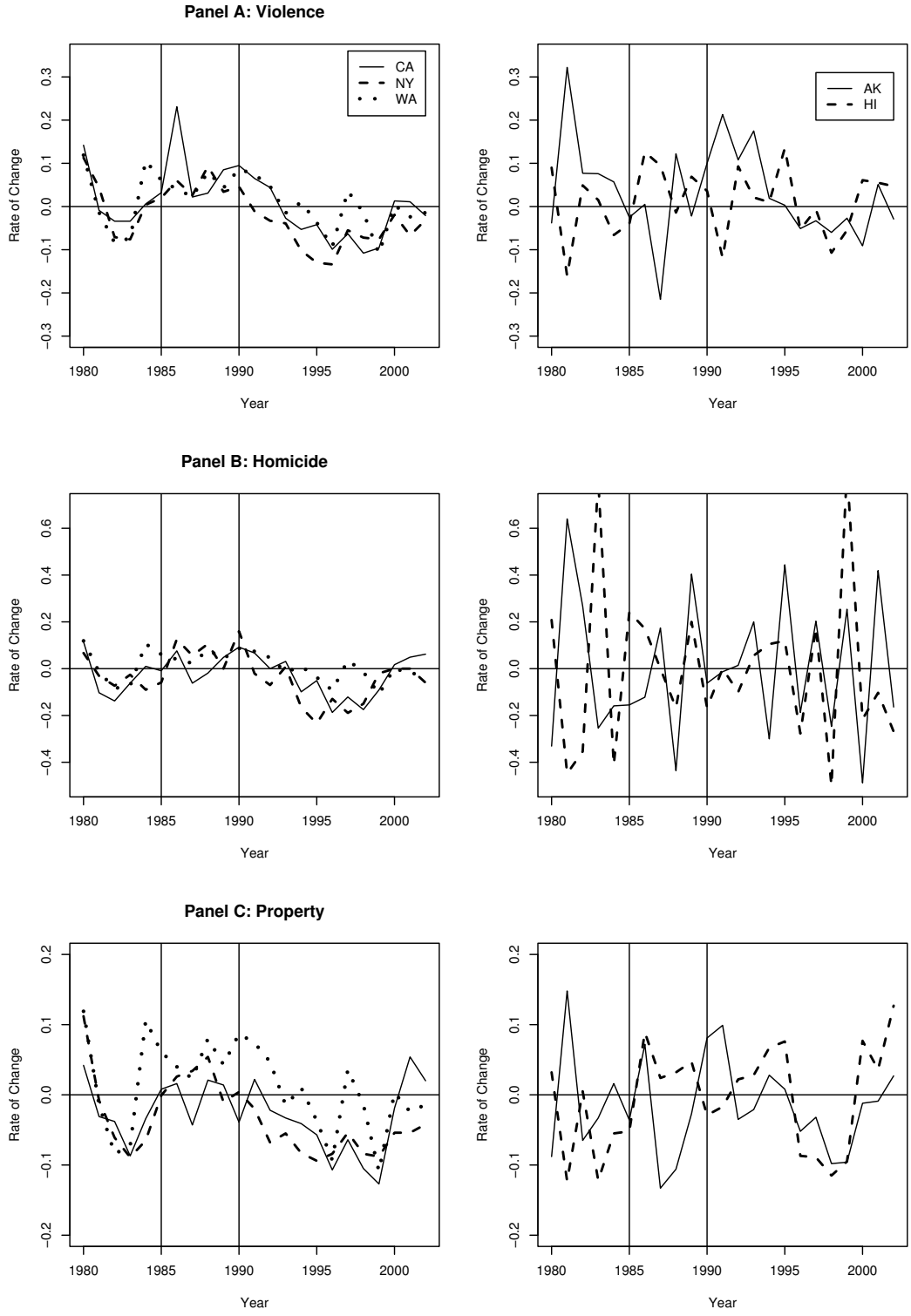


Source: Uniform Crime Reports, 1960-2002

Note: These probabilities were derived from a one-sided two-sample t-test.

Authors' Calculations.

Figure 7: Comparison of Relative Rates of Change for Repeal States by Crime & Year, 1980-2002



Source: Uniform Crime Reports, 1960-2002
Authors' Calculations

Figure 8: Age-Period-Cohort Lexis Regions by Quinquennium & Birth Cohort