The Impact of Legalized Abortion on Crime over the Last Two Decades

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Donohue and Levitt (2001) presented evidence that the legalization of abortion in the early 1970s played an important role in the crime drop of the 1990s. That paper concluded with a strong out-of-sample prediction regarding the next two decades: "When a steady state is reached roughly twenty years from now, the impact of abortion will be roughly twice as great as the impact felt so far. Our results suggest that all else equal, legalized abortion will account for persistent declines of 1% a year in crime over the next two decades." Estimating parallel specifications to the original paper, but using the seventeen years of data generated after that paper was written, we find strong support for the prediction and the broad hypothesis, while illuminating some previously unrecognized patterns of crime and arrests. We estimate that overall crime fell 17.5% from 1998 to 2014 due to legalized abortion—a decline of 1% per year. From 1991 to 2014, the violent and property crime rates each fell by 50%. Legalized abortion is estimated to have reduced violent crime by 47% and property crime by 33% over this period, and thus can explain most of the observed crime decline. (*JEL*: K42, I38, J13)

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The authors would like to thank Jacob Dorn, Bryan Ho, Ammar Inayatali, Mira Korb, Haksoo Lee, Nicolas Peña-Tenjo, Henrik Sachs, and Sidharth Sah for excellent research assistance, and Richard Boylan, Phil Cook, Peter Siegelman, Martin Wells, and conference participants at the Conference on Empirical Legal Studies and the American Law and Economics Association meetings for their helpful comments.

American Law and Economics Review
doi:10.1093/aler/ahaa008

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1. Introduction

Donohue and Levitt (2001) proposed a link between the legalization of abortion and future crime. The theory motivating that analysis is simple: decades of social scientific research have demonstrated that unwanted children are at an elevated risk for less favorable life outcomes on multiple dimensions including criminal involvement,[1] and the legalization of abortion appears to have dramatically reduced the number of unwanted births.[2] As a consequence, cohorts exposed to legalized abortion would be expected to exhibit less criminal behavior than would have been the case absent the legalization of abortion. Using a range of empirical identification strategies, Donohue and Levitt (2001) argued that legalized abortion might well be the single most important factor in reducing crime in the 1990s—perhaps accounting for as much as half of the drop in crime observed in the United States between 1991 and 1997, the endpoint of their data. The claims in Donohue and Levitt (2001) proved to be highly controversial. The research triggered numerous critical academic comments[3] and subsequent replies (Dills and Miron 2006; Joyce 2006; Lott and Whitley 2007;

1. See citations provided on pages 387–389 in Donohue and Levitt (2001).

2. There is also a more direct and mechanical link between abortion and crime. If legalized abortion causes birth rates to decline, then the affected cohorts will be smaller upon reaching peak crime ages. Levine (2007) ("In the United States, the process of abortion legalization in the early 1970s represented the biggest change in policy; studies of the impact of this policy-change suggest that birth rates fell considerably in response."); Levine et al. (1999) and Bitler and Zavodny (2003). It does appear, though, that most of the initial abortion-driven declines in birth rates reflects delaying of fertility (presumably to a more propitious time) rather than a reduction in overall lifetime fertility. Thus, while we show evidence in Table 7 of some crime-reducing impact that operates through this mechanical channel, this effect was short-lived. The specifications of Donohue and Levitt (2001) will capture both the unwantedness and cohort-size effects.

3. For critical academic comments, see Cook and Laub (2002), Zimring (2007), Kahane et al. (2008), Dills et al. (2008), Anderson and Wells (2008), and Joyce (2009a,b). For supportive academic comments, see Berk et al. (2003), Listokin (2003), Pop-Eleches (2006), Charles and Melvin Stephens (2006), Ananat et al. (2009), Hunt (2006), and Cornwell and Cunningham (2013). Quantifying the impact of abortion legalization in reducing unintended births, Lin and Pantano (2015) concludes: "We find that being unintended causes negative outcomes (higher crime, lower schooling, lower earnings) over the life cycle." Examining fertility and crime patterns in Germany after 1989, Chevalier and Marie (2019) conclude: "Our results confirm the Donohue and Levitt (2001) hypothesis that fertility decisions can have a large effect on the subsequent criminal activity of children." Using Bayesian tree ensembles in a recent analysis of our original 1985–97 data, Woody et al. (2020) state: "We find that there is strong support for the existence

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Chamlin et al. 2008; Foote and Goetz 2008), as well as numerous extensions consistent with the original findings (Hay and Evans 2006; Sen 2007; François et al. 2014; Shoesmith 2015).[4] To this day, there remains a great diversity of views on the merits of the hypothesis among academics.

There are a number of reasons why Donohue and Levitt (2001) provoked such a strong academic response.[5] First, the magnitude of the results was both large and surprising. At the time, a voluminous academic literature had developed to address the question of understanding fluctuations in crime, including the reasons for the dramatic crime reduction observed during the 1990s. Prior to Donohue and Levitt (2001), there was no mention in this literature of a link between abortion and crime. For a previously unrecognized mechanism to account for possibly half of the largest crime reduction in American history posed a fundamental challenge to the existing scholarship on crime. Second, the evidence presented in Donohue and Levitt (2001) was suggestive, but not definitive. The identification of the estimates was derived neither from a randomized experiment nor even from a credibly exogenous natural experiment (with the possible exception of the 1973 Supreme Court decision in *Roe v. Wade*, 410 U.S. 113 (1973)). Instead, Donohue and Levitt (2001) presented evidence from a collage of different sources of variation, each of which had its weaknesses.[6]

Third, the timing of the crack epidemic—which coincided with the peak-crime ages of the first cohort exposed to legalized abortion—increased the difficulty of teasing out the causal impact of legalized abortion. Fourth, at the time, it was rare for economists to posit theories with such long lags between a stimulus (in this case, abortion) and an outcome (in this case, crime roughly two or more decades later).[7] All of the existing explanations

of a negative causal effect of abortion on murder, violent crime, and property crime, consistent with the findings of Donohue and Levitt (2001)."

4. We have received more than 200 requests for our original data and code for the purposes of replication.

5. There was also a strong response to the hypothesis in the popular press throughout the world, including front-page headlines (Brandon 1999), network news coverage, and op-ed reactions in many major newspapers (Goode 1999; Samuelson 1999; Stille 2001; Stossel and Varney 2006; Lott 2008). Much of that coverage was negative, or at the least, laced with skepticism.

6. We discuss these different sources of variation at length below.

7. Over the last two decades, such theories have become much more common in economics. See, for example, a number of papers on fetal origins: Almond (2006) and

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for fluctuations in crime focused on more proximate causes, e.g., the number of police, expected punishment, or the state of the labor market. Finally, the results of Donohue and Levitt (2001) were based on a short time window of abortion exposure. The original paper could only rely on crime data through 1997 and arrest data through 1996. At that time, the first nationwide cohort of individuals exposed to legalized abortion was only in their early twenties.

In the face of these inherent challenges, reasonable people might disagree as to the persuasiveness of the evidence presented in Donohue and Levitt (2001). But the Donohue–Levitt theory makes a strong out-of-sample prediction, which was advanced almost two decades before the full of impact of abortion on crime would be felt. In the conclusion to their paper, Donohue and Levitt wrote:

> "Roughly half of the crimes committed in the United States are done by individuals born prior to the legalization of abortion. As these older cohorts age out of criminality and are replaced by younger offenders born after abortion became legal, we would predict that crime rates will continue to fall. When a steady state is reached roughly twenty years from now, the impact of abortion will be roughly twice as great as the impact felt so far. Our results suggest that all else equal, legalized abortion will account for persistent declines of 1% a year in crime over the next two decades."

In this article, we analyze the extent to which the nearly 20 years of crime data generated after our analysis was completed support or refute the hypothesized link between abortion and crime. Our methodology is straightforward: we reproduce the primary tables presented in Donohue and Levitt (2001), while extending the data set to cover the period from 1998 to 2014. The choice of specification in the original paper provides a strong degree of discipline on the exercise we carry out. In contrast to the typical empirical economics paper, where the researchers run many specifications and only report a few of those, we constrain ourselves by the choices made in the original paper.[8] In addition, we report updated results

Almond (2011). Acemoglu et al. (2001) explore whether the colonial origins of a nation can influence its long-term economic development.

8. The major exception we make is to use a better measure of abortion, furnished to us by the Alan Guttmacher Institute (AGI) after our original paper was published, which tracks abortion by state of residence as opposed to state of occurrence. Since we

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using specifications suggested by the subsequent exchange between Foote and Goetz (2008) and Donohue and Levitt (2008).

The results obtained provide strong support for the hypothesized link between abortion and crime. For most of the specifications reported in the original paper, the point estimates are larger in the out-of-sample 1998–2014 period than in the original publication. This finding is particularly striking because the tables use very different sources of identification (e.g. the natural experiment associated with early legalization, cross-state differences in abortion rates after legalization, within-state differences in crime rates for those born just before or after legalized abortion, etc.). Consequently, it appears that the predictions made in Donohue and Levitt (2001) for the next two decades were borne out.

The remainder of the article is as follows. Section 2 provides background on the key legal and institutional factors relating to abortion in the United States and also describes the data used in this analysis. Sections 3 and 4 replicate and extend the results in Donohue and Levitt (2001) to cover the additional years of data from 1998 to 2014. Section 3 uses crime data at the state-year level to estimate the impact of legalized abortion on crime and illustrates the different paths in crime and abortion over our data period for the high- and low-abortion rate states. Section 4 then analyzes age-specific arrest data to link abortion rates of birth cohorts with their arrest rates when aged 15–24. Section 5 shows how the relatively weaker effects of abortion on property crime in our second period is explained by the divergence that emerged over the last 15 years of our data as property crime fell by almost 30% (and perhaps more according to the National Crime Victimization Survey), while property arrests remained at roughly the same level in 2000 and 2014. The section ends with a discussion of, the selective underreporting of crime in low-abortion states identified by Boylan (2019). Section 6 discusses the effect of lead reductions on crime, and Section 7 concludes, placing these findings into the broader context

are interested in how abortion legalization influences the criminality of ensuing birth cohorts, the residence of those having abortions is the relevant measure, as we explained in Donohue and Levitt (2004) and Donohue and Levitt (2008). We continue to rely on the AGI residence data as our core abortion measure, and fully explain all of our data sources and choices in our data appendix, Appendix G.

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of abortion-related research that has been conducted following our 2001 article.

2. Background and Data

For centuries, English common law (and thus early American law) permitted abortion prior to "quickening" (usually in the fourth month of pregnancy). During the 19th century American states began to outlaw the practice, but what ultimately became a uniform nation-wide prohibition started to break down in the United States when then-Governor Ronald Reagan signed the first major liberalizing abortion law in 1967. In late 1969 and 1970, five states (California, New York, Alaska, Hawaii, and Washington) fully legalized abortion. In 1973, the Supreme Court decision in *Roe v. Wade* made abortion legal nationwide, with the court emphasizing that denying the ability to seek an abortion could impose "a distressful life and future" for the mother and "the distress, for all concerned, associated with [bringing an] unwanted child ... into a family already unable, psychologically and otherwise, to care for it."[9]

Roughly 18 years after *Roe v. Wade*, crime unexpectedly began to fall—even as some of the most prominent criminologists of the day were predicting crime was about to explode.[10] From 1991 to 1997, the last year of data used in Donohue and Levitt (2001), violent crime fell 20%, property crime fell 16%, and homicide fell 30%.[11]

It took almost a decade after legalization for the number of abortions performed to reach a steady state, due to a lack of available providers as well as evolving norms. As the rate of abortion rose and cohorts born subsequent

9. *Roe v. Wade*, 410 U.S. 110, 153 (1973). For more institutional details regarding the legalization of abortion, see Donohue and Levitt (2001).

10. DiJulio (1996) and Fox (1996). For example, the Council on Crime in America (co-chaired by Bush "Drug Czar" William Bennett and Carter Attorney General Griffin Bell) released a report in November 1995, which stated: "America is a ticking violent crime bomb, and there is little time remaining to prepare for the blast." The imagery came from Council member John DiIulio, then a professor at Princeton, who later was appointed by President George W. Bush as the first director of the White House Office of Faith-Based and Community Initiatives.

11. https://ucr.fbi.gov/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10tbl01.xls.

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to legalization moved through their high-crime years, crime continued to decline. Between 1997 and 2014 (the last year of data included in our analysis), violent and property crime per capita fell by 40% and homicide declined by 35%, according to the Uniform Crime Reports.

The path to legalization did not create particularly compelling quasi-randomized variation in abortion exposure. Although the early legalization of abortion in five states might appear to serve as a natural experiment, these five states are clear outliers. Even after steady state abortion rates are reached in the 1980s, the effective abortion rate for violent crime in the early-legalizing states are almost double those in the rest of the nation. Indeed, one striking feature of the data is the enormous heterogeneity in abortion usage across states. Dividing states into thirds according to the number of abortions per live birth, the 17 states with the lowest abortion rates have a steady-state effective abortion rate for violent crime that is roughly one-half that of the middle 17 states and one-third that of the 17 states with the highest abortion rates.

As a consequence, Donohue and Levitt (2001) relied on a collage of individually imperfect sources of variation in an effort to discern the causal impact of abortion on crime. These consisted of a comparison of early-legalizing states to the rest of the country, a comparison of states with high- and low-abortion rates after abortion became legal everywhere, differences in crime patterns within states for cohorts born before and after legalization, and differences in arrest rates within states by single year of age.

Because the impact of abortion on crime is not expected to be immediate, but rather is only felt when cohorts exposed to a legalized abortion regime in utero reach an age at which crimes are committed, the specification of the abortion measure is more complex than that of other variables in a typical panel data model of crime.[12] At least initially, the expected impact of abortion on crime increases gradually as more and more of the crime-age cohorts have been exposed to legalized abortion and as these cohorts transition from the relatively low-crime ages of the early teenage years to the

12. A benefit of this time lag is that it helps to distinguish the abortion-crime hypothesis from other competing stories. Most other factors explaining crime (e.g. policing strategies, prisons, crack cocaine) have a contemporaneous impact. In contrast, the abortion-crime hypothesis makes a strong prediction: abortion rates should have no impact on crime until 15–20 years later.

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peak-crime ages in the late teens and early twenties. Thus, the hypothesized impact of abortion on crime emerges only incrementally; the full impact is not felt for many decades.

To capture the extent to which legalized abortion would be expected to influence crime in a given state and year, Donohue and Levitt (2001) developed a metric they named the "effective abortion rate" per 1,000 live births. The "effective abortion rate" is the weighted average of the abortion rates of the birth cohorts in a state, with the weights determined by the 1985 share of total arrests nationally for a particular crime category of individuals of that age. More formally,

$$Effective\ abortion_t = \sum_a Abortion_{t-a} \cdot \left(\frac{Arrests_a}{Arrests_{total}} \right), \tag{1}$$

where t indexes years and a indexes the age of a cohort. Abortion is the number of abortions per 1,000 live births, and the 1985 ratio of arrests inside the parentheses is the fraction of arrests for a given crime involving individuals with age a.[13]

In a steady state with all cohorts subjected to the same abortion rate, the effective abortion rate is equal to the actual abortion rate. For many years following the introduction of legalized abortion, the effective abortion rate will be below the actual abortion rate since many active criminal cohorts are too old to have been affected by legalized abortion. For instance, following *Roe v. Wade*, the actual abortion rate (per 1,000 live births) rose to a steady state of about 368 by the early 1980s. Yet, we estimate that the effective abortion rate in 1991 was only about 24 for homicide, 51 for violent crime, and 110 for property crime. Because property crime is disproportionately done by the young, the effect of abortion legalization is felt earlier for that crime category. The effective rates grew steadily, rising to 132, 170, and 247, respectively, by 1997. In 2014, the effective abortion rates for these three crime categories had risen to 330, 341, and 338, respectively. If legalized abortion reduces crime, then crime should continue to fall (all else equal) as long as the effective abortion rate is rising.

13. This effective abortion rate includes legal abortion exposure prior to 1973 in the five states that legalized in 1969 or 1970.

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Throughout this article, we attempt to mirror the specifications of Donohue and Levitt (2001) as closely as possible, in order to tie our hands with respect to ex post facto model selection. We follow one major upgrade in our core abortion data that we initiated in 2004. In our original 2001 paper, we used abortion data that reflected the state in which an abortion was performed. This was less than ideal for our purposes because a substantial number of women travel across state lines to have an abortion. A much more natural metric for constructing an abortion rate would use the mother's state of residence.[14] This latter measure only became available from the Alan Guttmacher Institute after our initial research was published. We have consistently used this abortion by state of residence measure since it became available (see Donohue and Levitt 2004; Donohue and Levitt 2008; Donohue et al. 2009) and continue to do so in this article.[15]

3. Results Revealing the Abortion-Crime Link

3.1. Crime Fell Earlier and Further for the Five Early-Legalizing States

We begin by looking at the patterns of crime in the five states (Alaska, California, Hawaii, New York, and Washington) that legalized or quasi-legalized abortion around 1970 relative to crime patterns in the rest of the nation where abortion did not become legal until the Supreme Court decision in *Roe v. Wade* of January 1973. Table 1 provides an updated version of Table I in Donohue and Levitt (2001). For each crime category (violent, property, and two measures of murder), we present percent changes in crime between 1976 and 1982, between 1982 and 1997, and between 1997 and 2014 for early-legalizers and the rest of the country. We then show the difference in these percent changes in crime between early-legalizing states and the rest of the nation. The first two columns correspond to data available in Donohue and Levitt (2001). The third and fourth columns, which report how crime

14. For example, after New York legalized its previously very strict abortion laws in 1970, hundreds of thousands of women came from other states for abortions until the 1973 *Roe* decision legalized abortion nationally (Jacobs 2018).

15. The different abortion data between this paper and our 2001 paper explains the discrepancy in our references to particular effective abortion rates. Our data appendix, Appendix G, discusses the sources and details of all our data.

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Table 1. Crime Trends for States Legalizing Abortion Early vs. the Rest of the US Natural Log of Differences in Crime Rates over Various Periods

Legalization Group	1976–82	1982–97	1997–2014 (New)	Cumulative (1982–2014)
Violent crime				
Early legalizers	15.8	−12.9	−61.7	−74.7
Rest of U.S.	20.9	14.5	−41.7	−27.1
Difference	−5.1	−27.5	−20.1	−47.5
SE	5.1	7.3	8.6	11.8
P-value	0.3	0.0	0.0	0.0
Property crime				
Early legalizers	0.8	−44.3	−54.4	−98.6
Rest of U.S.	5.2	−9.5	−52.3	−61.8
Difference	−4.3	−34.7	−2.1	−36.8
SE	2.7	5.7	4.8	8.8
P-value	0.1	0.0	0.7	0.0
Murder (UCR)				
Early legalizers	5.4	−40.8	−62.4	−103.3
Rest of U.S.	0.2	−24.7	−33.1	−57.7
Difference	5.3	−16.2	−29.3	−45.5
SE	7.3	10.7	6.9	11.4
P-value	0.5	0.1	0.0	0.0
Murder (VS)				
Early legalizers	8.4	−38.3	−58.3	−96.7
Rest of U.S.	4.2	−24.6	−27.3	−51.9
Difference	4.2	−13.7	−31.1	−44.8
SE	6.1	9.9	6.1	10.4
P-value	0.5	0.2	0.0	0.0
Effective abortion rate at end of period				
Early legalizers	1.6	281.0	514.4	514.4
Rest of U.S.	0.1	139.4	294.6	294.6
Difference	1.5	141.6	219.8	219.8

Early legalizing states are Alaska, California, Hawaii, New York, and Washington. These five states legalized abortion in late 1969 or 1970. In the remaining states, abortion became legal in 1973 after *Roe v. Wade*. Percent change in crime rate is calculated by subtracting the fixed 1985 population-weighted average of the natural log of crime rate at the beginning of the period from the fixed 1985 population-weighted average of the natural log of crime rate at the end of the period. The rows labeled "Difference" are the difference between early-legalizers and the rest of the United States. The bottom panel of the table presents the effective abortion rate for violent crime, as calculated using equation (1), based on the observed age distribution of national arrests for violent crime in 1985. Entries in the table are fixed 1985 population-weighted averages of the states. Abortion data are from the Alan Guttmacher Institute (by mother's state of residence); crime data are from the Uniform Crime Reports or the National Vital Statistics System.

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changed in the early-legalizers versus the rest of the country in the period 1997–2014 and cumulatively between 1982 and 2014, are new.

As noted above, these five early-legalizing states not only legalized abortion early, but continued to have higher abortion rates throughout the period. The bottom panel of the table presents the effective abortion rate for violent crime for the two sets of states at the end of each time period, calculated using equation (1). The gap in the effective abortion rate between the early-legalizers and the rest of the country has continued to grow over the entire time period, albeit more slowly in the later period. In 1997, the difference in the effective abortion rate between these two sets of states was 141.6; by 2014 the difference had increased to 219.8. Our theory predicts no difference in crime patterns across early-legalizers and the rest of the country prior to 1982 (just before the first abortion-exposed cohort in the early-legalizing states first reaches a crime-committing age), but greater decreases in crime for all periods since then.

The results in Table 1 confirm that prediction. Prior to 1982, there are no statistically different crime trends across early-legalizing and all other states. Property and violent crime were increasing at a slower rate in early-legalizing states between 1976 and 1982, whereas murder was rising faster in early-legalizing states, whether measured by UCR or Vital Statistics data.[16]

Between 1982 and 1997, violent crime fell by 27.5% (se = 7.3) in early-legalizing states relative to the rest of the country. The parallel numbers for property crime and homicide are −34.7% (se = 5.7) and −16.2% (se = 10.7) with UCR data and −13.7% (se = 9.9) using Vital Statistics data. Of greatest interest, however, are the new results presented in column 3. Violent crime fell by an additional 20.1% (se = 8.6) in early-legalizing states relative to the rest of the nation between 1997 and 2014. While the difference in property crime was not statistically significant over the recent time period (−2.1%; se = 4.8); the gap in homicide was large (roughly −30% with

16. We were strongly advised by Phil Cook to rely on the Vital Statistics (VS) homicide counts as a more reliable measure than the Uniform Crime Reports (UCR) figures. Indeed, when we explored this difference we found that the voluntarily reported UCR murder figures showed some troubling under counts of murder, particularly in low-abortion rate states. We present results using UCR murder to maintain consistency with our initial paper and for VS homicide in light of their apparent greater accuracy. Note that in Table 4, using the VS data uniformly strengthens the estimated effect of legalized abortion on murder.

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either measure) and highly significant. The cumulative differences across the entire time period are enormous and highly statistically significant for all four crime measures, −47.5% (se = 11.8) and −36.8% (se = 8.8) for violent crime and property crime, respectively, and −45.5% (se = 11.4) and −44.8% (se = 10.4) for our two measures of homicide.

3.2. Crime Fell More in High-Abortion States Than in Low-Abortion States

A second source of variation for identifying a link between abortion and crime is a comparison of crime patterns across states with differing levels of abortion usage post-legalization. Following Donohue and Levitt (2001), we rank order states by their effective abortion rates for violent crime in 1997 and partition the states into three categories with equal numbers of states in each category: low, medium, and high.[17] The three top panels of Table 2 report the percent changes in high, medium, and low-abortion states for violent crime, property crime, and homicide respectively, for the periods 1973−85, 1985–97, and 1997–2014. The bottom panel of the table reports the mean effective abortion rate at the end of the relevant period for the three groups of states.

There should be little or no impact of abortion on crime prior to 1985, because effective abortion rates are extremely low in 1985, even in high-abortion states. The results in column 1 for 1973–85 are consistent with that conjecture. Violent crime rate patterns are very similar across low, medium, and high-abortion states. Property crime rises less in high-abortion states than low-abortion states, but the opposite pattern is true for homicide, where crime declines are smallest in the high-abortion states.

The crime changes between 1985 and 1997 reveal a very different pattern. For each crime category, high-abortion states experience more favorable crime trends than medium abortion states, with low-abortion states faring the worst. For all three crime categories, the difference between high-abortion

17. The District of Columbia is included here and elsewhere in the paper, giving us 51 states, which allows 17 states per category. The second column in the bottom panel of Table 2 shows the effective abortion rates that were used for the partitioning into the three categories.

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Table 2. Crime Changes 1985–2014 in Low-, Medium-, and High-Abortion Rate States

Abortion Frequency (1997)	1973–85	1985–97	1997–2014 (New)	Cumulative (1985–2014)
Violent crime				
Lowest	32.9	26.3	−23.3	3.1
Medium	28.5	20.6	−36.2	−15.6
Highest	28.6	−1.5	−60.7	−62.2
Property crime				
Lowest	33.6	10.8	−42.1	−31.2
Medium	27.4	2.9	−45.7	−42.8
Highest	13.2	−22.2	−61.5	−83.7
Murder (UCR)				
Lowest	−23.5	7.4	−32.3	−24.9
Medium	−20.8	−12.7	−32.4	−45.2
Highest	−11.9	−25.3	−46.7	−71.9
Murder (VS)				
Lowest	−17.0	13.7	−27.0	−13.3
Medium	−22.8	−12.3	−25.5	−37.7
Highest	−7.4	−23.6	−42.3	−65.9
Effective abortion per 1,000 at end of period				
Lowest	0.8	77.0	179.8	179.8
Medium	1.4	125.6	265.6	265.6
Highest	5.4	232.6	450.8	450.8

States are ranked by effective abortion rates for violent crime in 1997, with the 17 states with lowest abortion rates classified as "lowest," the next 17 states classified as "medium," and the highest 17 states (including District of Columbia) classified as "highest." The effective abortion rate is the weighted average abortion rate per 1,000 live births (number of abortions per state according to mother's state of residence), as calculated using equation (1), weighted using the observed age distribution of national arrests for violent crime in 1985. All values in the table are weighted averages using 1985 state populations as weights. Percent change in crime rate is calculated by subtracting the fixed 1985 population-weighted average of the natural log of crime rate at the beginning of the period from the fixed 1985 population-weighted average of the natural log of crime rate at the end of the period. Because crime rates are extremely low until the mid-teenage years, legalized abortion is not predicted to have had a substantial impact on crime in the period 1973–85, but would be predicted to affect crime in the period 1985–2014. Abortion data are from the Alan Guttmacher Institute; crime data are from Uniform Crime Reports or the National Vital Statistics System.

states and low-abortion states is ∼30 percentage points, and somewhat larger for VS murder.

Column 3 of Table 2 presents results for the time period that post-dates the publication of the original paper. Across all three crime categories, the

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decline in crime is considerably greater for the high-abortion states than for the low-abortion states, as theory would predict. The magnitude of the differences are substantial: violent crime has fallen an additional 35 percentage points since 1997 in high-abortion states relative to low-abortion states. For property crime that difference is over 19 percentage points, and for homicide it is ∼15 percentage points. Aggregating over the entire time period 1985 to 2014 (Column 4), high-abortion states have experienced a reduction in crime relative to low-abortion states of −65.3, −52.5, and −47.0 percentage points for UCR violent crime, property crime, and homicide, respectively. With Vital Statistics data, the homicide differential is −52.6 percentage points.

3.3. Abortion is Highly Significant in Explaining Crime Reductions in Panel Data

A third source of variation comes from panel data analysis that allows us to control for other factors, in addition to abortion rates, that influence crime. The specification estimated takes the form:

$$ln(CRIME_{st}) = \beta_1 ABORT_{st} + X_{st}\Theta + \gamma_s + \lambda_t + \epsilon_{st}. \tag{2}$$

The dependent variable is the respective logged per capita crime rate in state s at time t. Our main independent variable of interest is the effective abortion rate for a given state, year, and crime category.[18] X is a vector of state-level controls, including prisoners and police per capita, a set of variables capturing state economic conditions, lagged state welfare generosity, an indicator for the presence of concealed handgun laws, and per capita beer consumption. Both state and year fixed effects are included, represented by γ_s and λ_t, respectively. All regressions are weighted by state population and adjusted for serial correlation using the method outlined by Bhargava et al. (1982).

18. Table 4 estimates this abortion effect for both the 1985–97 and 1998–2014 periods (and we provide the comparable single estimate for the full 1985–2014 period in Appendix B.)

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Table 3. Summary Statistics, 1985–2014

Variable	Mean	Standard Deviation (Overall)	Standard Deviation (Within State)
Violent crime per 100,000 residents	540.93	238.43	156.97
Property crime per 100,000 residents	3,882.96	1,215.86	968.50
Murder per 100,000 residents (UCR)	6.59	3.60	2.33
Murder per 100,000 residents (VS)	7.03	3.53	2.25
EAR: Violent crime	203.64	152.38	128.47
EAR: Property crime	240.93	151.27	117.39
EAR: Murder	179.33	148.15	128.97
Prisoners per 1,000 residents ($t-1$)	3.83	1.61	1.05
Police per 1,000 residents ($t-1$)	3.08	0.71	0.37
Real state personal income per capita	17,045.93	2,914.40	1,942.68
Real AFDC generosity per recipient family/1,000 ($t-15$)	3.76	1.73	1.05
State unemployment rate (%)	6.20	1.92	1.73
Beer consumption per capita (Gallons of ethanol)	1.22	0.19	0.09
Poverty rate	13.47	3.24	1.77

All values reported are means of annual, state-level observations for the period 1985–2014 with the following exceptions. In 1996, there was a transition from the annual program AFDC to TANF. From 1998 onwards, the AFDC variable reflects TANF assistance. It is lagged by 15 years, measured in thousands of dollars and indexed at 1982–84 values. The police and prisoners data are both logged and once-lagged, so correspond to the years 1984–2013. The values reported in the table are population-weighted averages. The effective abortion rate is a weighted average of the abortion rate for each cohort born in a state, with weights determined by the percentage of arrests by age for a given crime category in the United States in 1985 as shown by equation (1).

Summary statistics for the full estimating sample are provided in Table 3.[19] We present both overall standard deviations and within-state standard deviations, which is the more relevant measure since state-fixed effects are included in all specifications. The effective abortion rates differ across crime categories because the age distribution of arrests differs across crimes.

Regression results are shown in Table 4. The dependent variable in columns 1 and 2 is (logged) violent crime. Columns 3 and 4 present results

19. Appendix A also shows this same set of summary statistics, dividing the nation into high- and low-abortion states. The means of the control variables for the two groups are generally roughly comparable. The ratio of these means (high-abortion/low-abortion states) are all within 15% of each other, except for state per capita income (ratio of 1.16) and AFDC generosity (ratio of 1.39), which are higher in the high-abortion states.

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Table 4. Panel-data Estimates of the Relationship between Abortion Rates and Crime, 1985–2014

	Dependent variable: Log per capita value of...							
	Violent crime		Property crime		UCR murder		VS murder	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effective abortion rate '85–'97	−0.184**	−0.178**	−0.138**	−0.152**	−0.087*	−0.100*	−0.098**	−0.116**
	(0.022)	(0.022)	(0.017)	(0.016)	(0.038)	(0.040)	(0.034)	(0.036)
Effective abortion rate '98–'14	−0.192**	−0.189**	−0.149**	−0.168**	−0.131**	−0.152**	−0.141**	−0.164**
	(0.019)	(0.019)	(0.016)	(0.015)	(0.017)	(0.021)	(0.016)	(0.019)
ln(lagged prisoners per capita)		0.007		−0.111**		−0.121*		−0.133**
		(0.037)		(0.034)		(0.056)		(0.051)
ln(lagged police per capita)		−0.015		−0.027		−0.137*		−0.186**
		(0.015)		(0.014)		(0.053)		(0.049)
Unemployment rate		−0.027		0.536		1.212		1.084
		(0.356)		(0.314)		(0.716)		(0.657)
Ln(Real per capita income)		0.003		−0.076		0.329		0.172
1982–84 $		(0.129)		(0.114)		(0.224)		(0.203)
Poverty rate		−0.002		−0.001		−0.003		−0.001
		(0.001)		(0.001)		(0.003)		(0.003)
Real AFDC generosity		0.004		−0.001		−0.008		−0.003
1982-1984 $		(0.003)		(0.003)		(0.008)		(0.007)
Shall-issue concealed weapons		0.014		0.019		−0.042		−0.023
law		(0.015)		(0.011)		(0.023)		(0.022)
Beer consumption per capita		0.077		0.026		0.286**		0.286**
		(0.050)		(0.044)		(0.106)		(0.096)
Year FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,530	1,517	1,530	1,517	1,530	1,517	1,512	1,499
R^2	0.815	0.844	0.974	0.980	0.853	0.865	0.877	0.887
Adjusted R^2	0.805	0.834	0.973	0.978	0.845	0.857	0.870	0.880

Note: $^*P < 0.05$;$^{**}P < 0.01$.
The dependent variable is the log in the per capita crime rate named at the top of each pair of columns. The first column in each pair presents results from the specifications in which the only additional covariates are state- and year-fixed effects. The second column presents results using the full specification. The data set is comprised of annual state-level observations (including the District of Columbia) for the period 1985–2014. State- and year-fixed effects are included in all specifications. The prison and police variables are once-lagged to minimize endogeneity. Real AFDC generosity per recipient family is lagged by 15 years, measured in thousands of dollars and indexed at 1982–84 values. Estimation is performed using a two-step procedure. In the first step, weighted least squares estimates are obtained, with weights determined by state population. In the second step, a panel data generalization of the Prais–Winsten correction for serial correlation developed by Bhargava et al. (1982) is implemented. Standard errors are in parentheses.

for (logged) property crime, while columns 5 and 6 reflect (logged) UCR homicide and columns 7 and 8 reflect (logged) VS homicide. For each of the four crime measures, two different specifications are reported. The odd-numbered columns present results without control variables (other than the state- and year-fixed effects); the even columns add the full set of controls. The top two rows present the two effective abortion rate measures, one corresponding to the time period included in our earlier study and the other capturing the period since that time. In Appendix B, we also report

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a modified version of Table 4 that estimates a single abortion variable for the entire 1985–2014 time period, instead of estimates for the two separate time periods.

All of the coefficients on abortion in both Table 4 and Appendix B are negative, implying that higher abortion rates are associated with lower crime. These estimated effects of abortion are in all cases highly statistically significant—more so than any other variable included in the analysis. Eleven of the twelve estimates based on specifications with a full set of controls in these two tables are significant at the 0.01 level (with the first-period effect on UCR murder significant at the 0.05 level). Notably, the coefficients on abortion in the time period that post-dates our initial study are larger in magnitude in all eight Table 4 specifications, implying that the out of sample results are even stronger than those in the original paper.[20] The real-world magnitude implied by the coefficients on abortion is substantial. An increase in the effective abortion rate of 100 per 1,000 live births (the mean effective abortion rate in 2014 for violent crime is 340.95 with a standard deviation of 131.37 across states) is associated with a reduction of roughly 10–20% in crime. Looking at the estimates with controls for the entire time period (Appendix B), we see that the estimated drop in crime from an increase of 100 in the effective abortion rates ranges from a low of 15.8% for UCR murder to 18.9% for violent crime.

The consistency of the strong negative relationship between abortion and crime shown in Table 4 is striking. Note that adding controls to the panel data models (the even columns) has little effect on the estimated abortion effect for violent crime, and it increases the effect for property crime and the two measures of murder. We also experimented with different specifications and additional controls and the Table 4 findings remained remarkably robust. For example, when we used the controls from Donohue et al. (2019) instead of those from our original (and current) Table 4, the results are essentially

20. As a robustness check we re-ran the Table 4 regressions following Zeoli et al. (2018) and Webster et al. (2020) in excluding Florida, Kansas, Kentucky, Nebraska, and Montana because of "systemic Uniform Crime Reports (UCR) reporting issues over multiple years." Excluding these five states yielded the identical pattern of uniform statistical significance on the abortion variables, with the estimated second-period effects stronger in all cases than the first-period effects.

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the same (see Appendix C).[21] Specifically, the negative effect of abortion on crime becomes modestly stronger for violent and property crime, and overall for five of the eight estimates, while modestly weaker in three. Every estimated abortion effect using the Table 4 or the modified Appendix C set of controls is statistically significant at at least the 0.01 level for violent crime, property crime and Vital Statistics murder.[22]

3.4. Illustrating the Greater Drops in Crime in High-Abortion States

To illustrate how tightly the relative increases in abortions in high-abortions states correspond to the relative drops in crime in these states relative to low-abortion rate states, we divided the states into two groups of roughly equal population in 1985 according to the number of abortions per 1,000 live births based on state of residence.[23] Both groups had roughly 119 million residents in 1985, although their growth rates differed over time. The low-abortion states had a somewhat higher population in 1977 but had almost 18 million fewer residents than the high-population states by 2014, as shown in the table below:

	1977	1985	2014
Low-abortion states	112.1	118.9	150.5
High-abortion states	107.6	119.0	168.4

Notes: Population in millions.

21. The most important changes in this alternative specification are the inclusion of a variable reflecting the percentage of state population living in MSA's and six different age-race variables for whites, blacks, and other races for ages 15–19 and 20–39. In other words, including a robust set of demographic controls did not alter our Table 4 findings, as shown in Appendix C.

22. The UCR murder estimate for the entire period is also significant at the 0.01 level (Appendix B), but for the first period in Table 4 it is significant at the 0.05 level and only has a t-statistic of 1.8 in the modified Appendix C specification. But as we discuss in Section 5.2.2, Boylan (2019) has found substantial undercounts of UCR crime in ways that suppresses the estimated impact of abortion. Accordingly, we repose greater confidence in the Vital Statistics murder results, which are uniformly statistically significant and are always greater in magnitude than the corresponding UCR murder estimates. That is true for Table 4, Appendix B, Appendix C, and the specification described in footnote 20 (dropping five states with poor UCR data).

23. The 32 states in the low-abortion rate group and the 19 high-abortion rate states are listed in Appendix A.

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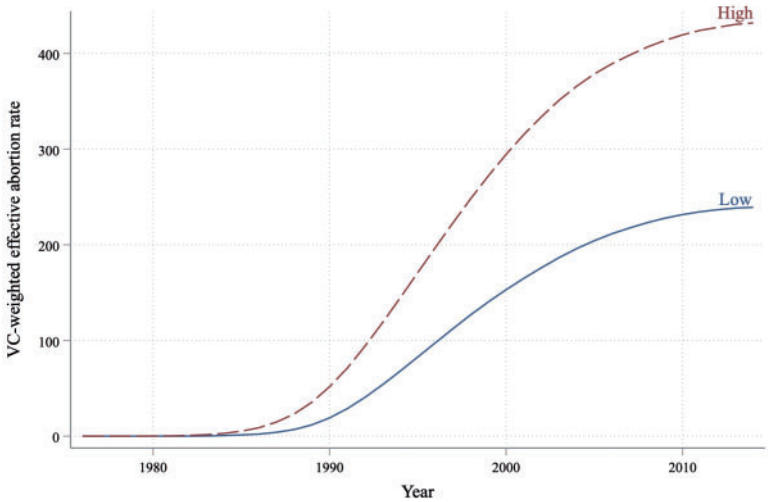

Figure 1. Effective Abortion Rates in High- and Low-Abortion States (Weighted by Violent Crime).

Note that our crime rates for the following figures are crimes per 100,000 population. Figure 1 shows the difference in effective abortion rates between our high- and low-abortion states, weighted to reflect when legalized abortion would be expected to influence violent crime. The impact of legalized abortion starts slowly but rises substantially in both sets of states after 1990, although obviously much more rapidly in the high-abortion states. Our thesis predicts that while both sets of states would experience downward pressure on crime by virtue of the growing effective abortion rates, the impact on crime would be substantially greater in the high-abortion states.

Figure 2 illustrates the pattern of violent crime over the period from 1977 to 2014 in these two sets of states. The first aspect to note in this figure is the striking parallel trends in the pattern of violent crime in these two sets of states in the roughly 15 years prior to the impact of legalized abortion began to take effect. While both sets were initially battered by sharply rising violent crime, the pattern reversed in the early 1990s, and the high-abortion states experienced much more dramatic drops in crime for the remainder of our data period. While in the late 1970s and up to the early 1990s, violent

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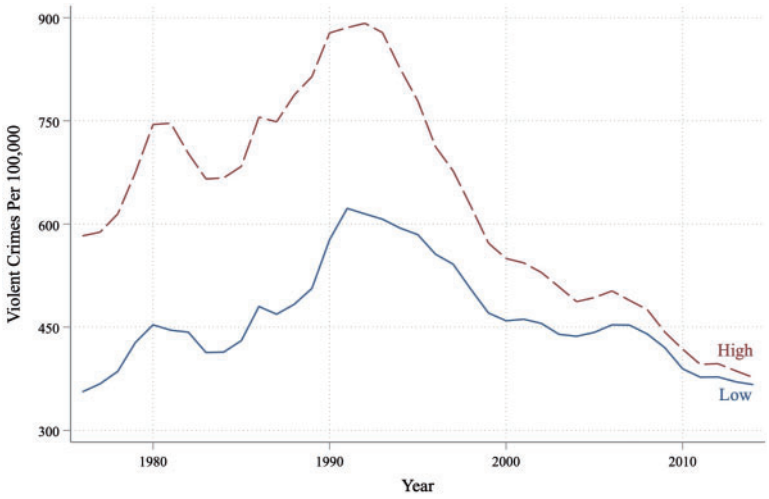

Figure 2. Violent Crime Rates in High- and Low-Abortion States, 1977–2014.

crime was substantially higher in high-abortion states, the persistent faster decline over the next quarter-century largely eliminated that gap.

The interesting connection between the greater increase in abortion and the greater drops in violent crime experienced in the high-abortion states can be seen by plotting the difference in violent crime rates on the same graph as the difference in the abortion rates in these two sets of states, as shown in Figure 3. The violent crime rate vacillated between 250 and 300 more crimes per 100,000 population in the high-abortion states in the decade prior to 1990. But just as the greater increases in abortion in the high-abortion states took hold, continuing for the remainder of our data period, the more sharply declining violent crime rate in the high-abortion states virtually eliminated the crime differential by 2014.

The same underlying patterns in violent crime shown in Figures 1–3 for high- and low-abortion states also exist for murder and property crimes. Figures 4 and 5 replicate the Figure 3 juxtaposition showing the pronounced opposing movements of the rising gap in effective abortion rates between the two groups of states with the simultaneously declining gap in murder and property crime, respectively. Indeed, as Figure 4 reveals, the relatively greater drop in murder in the high-abortion states was so substantial that by

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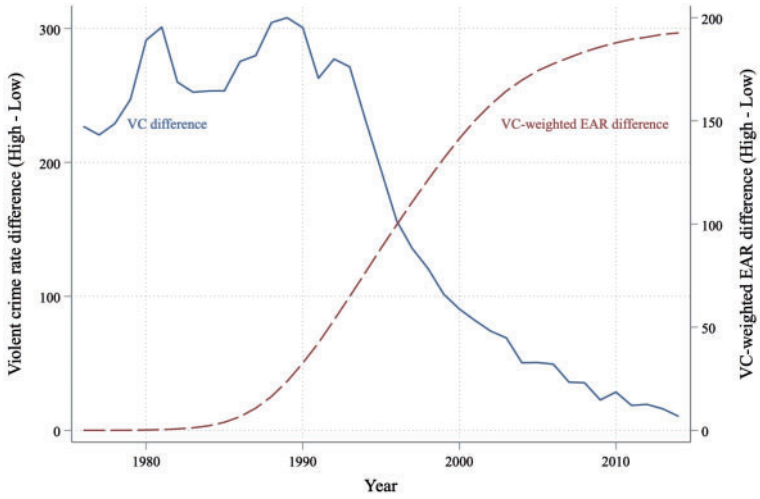

Figure 3. The Growing Abortion Disparity Corresponds to a Relative Decline in the Violent Crime Rate, 1977–2014.

the end of our data period the higher murder rates of the high-abortion states had not only been eliminated but had actually been reversed. By 2014, the high-abortion states had a VS murder rate that was 0.709 per 100,000 *lower* than the murder rate in low-abortion states.[24]

Visually, the same pattern of a higher initial crime rate in the high-abortion states that declines and was ultimately reversed at the same time that the abortion differential grew is seen in Figure 5 for property crime. Note that the murder rate gap was eliminated in roughly 2008 and turned in favor of high-abortion states thereafter, while in the case of property crime this contemporaneous decline in the higher initial crime was eliminated almost a decade earlier. Interestingly, the higher property crime and murder rate differentials of the high-abortion rate states were both eliminated at about the time that their respective effective abortion rate differentials (over the low-abortion rate states) reached 175 per 1,000 births. Since property

24. The 2014 VS murder rate was 5.36 in the low-abortion states and 4.65 in the high-abortion states.

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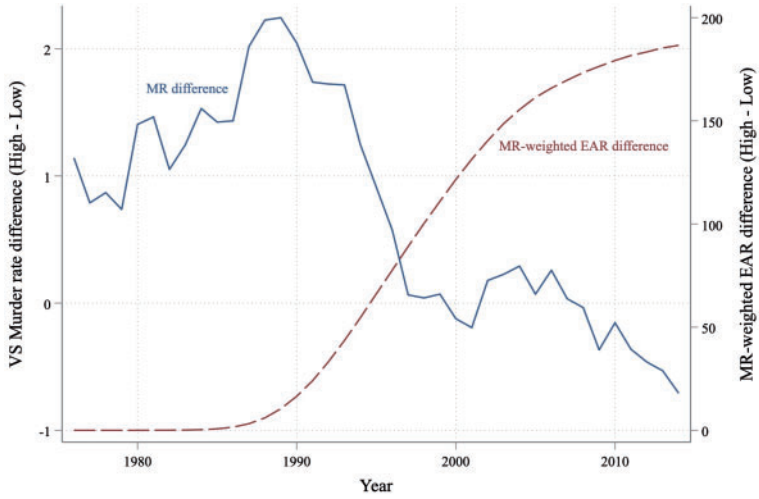

Figure 4. The Growing Abortion Disparity Corresponds to a Relative Decline in the VS Murder Rate, 1977–2014.

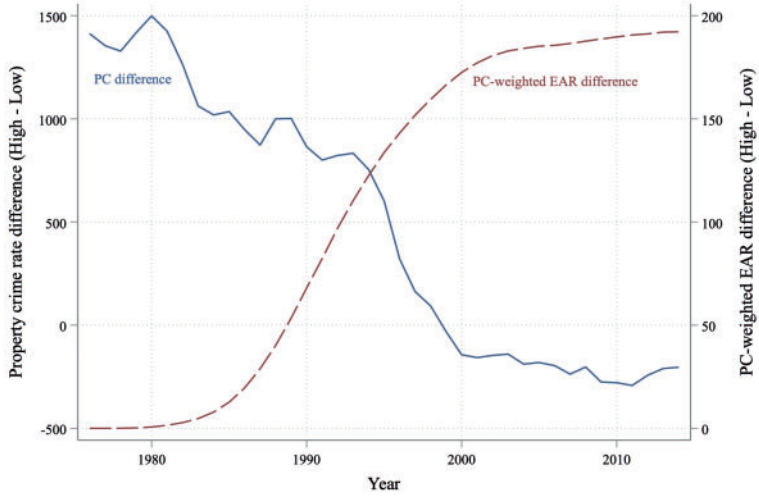

Figure 5. The Growing Abortion Disparity Corresponds to a Relative Decline in the Property Crime Rate, 1977–2014.

crime tends to be committed by younger criminals, the property crime effective abortion rate differential reached 175 roughly 10 years earlier than the murder effective abortion rate.

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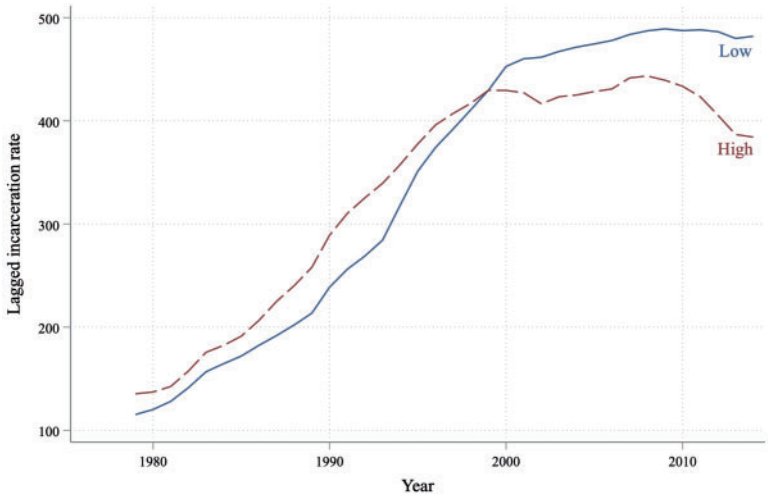

Figure 6. Incarceration Rate Trends in High- and Low-Abortion States.
Note: The incarceration rate is state and federal prisoners per 100,000 population.

Of course, these graphs are only juxtaposing the contemporaneous growth in abortion rates with the greater crime drops of the high-abortion rate states, but we know that the rates of police staffing and incarceration were growing very substantially over this period. One could imagine that the high-abortion rate states simply grew their police forces and incarceration rates faster than low-abortion rate states starting around 1990. In this event, these other policies might explain all or most of the relative crime drop that we have paired with the rising relative abortion rates.

To explore this possibility, we plot the relative changes in our two sets of states for rates of incarceration and police staffing Figures 6 and 7, respectively. The figures clearly document the very substantial expansions in these two crime-fighting technologies but two points underscore why these factors do not undermine the hypothesized link between legalized abortion and crime. First, the steady increases in both incarceration and police cannot explain the sudden and unanticipated decline in the crime rate starting in roughly 1990. Second, we have just seen that the crime drops were substantially greater in high-abortion states, so if the

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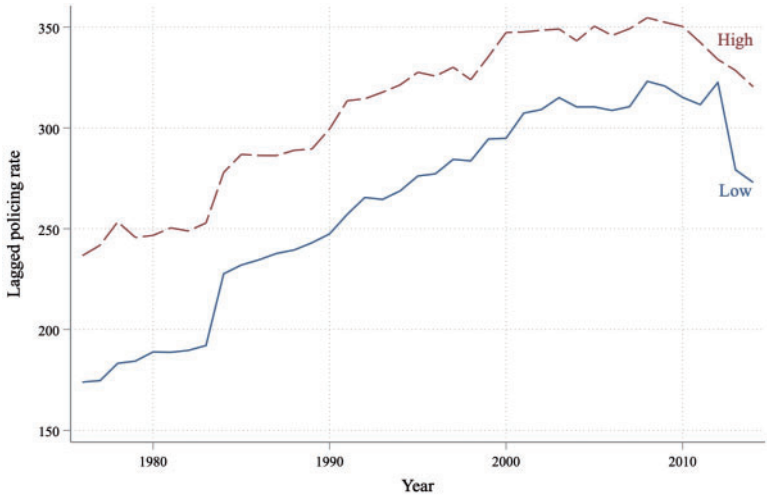

Figure 7. Police Staffing Rate Trends in High- and Low-Abortion States.
Note: The policing rate is law enforcement employees per 100,000 population.

abortion-crime thesis is to be undermined by increasing incarceration or growing police forces we would need to see greater increases in these factors in the high-abortion states. But the figures refute this proposition. Figure 6 shows that incarceration rates rose more sharply and are now substantially higher in the low-abortion states. Thus, the greater crime-reducing increases in incarceration in the low-abortion states would suggest that the relative crime improvements in the high-abortion states depicted in Figures 3–5 that we attribute to increased abortion are, if anything, understated.

Figure 7 reaffirms that both sets of states have substantially increased police staffing over our data period, at least until the financial crisis led to budget cutbacks and both groups experienced dips in the ensuing years. While the high-abortion states have always had higher police staffing rates, at least over the last 15 years the police staffing gap between high- and low-abortion states has narrowed. Again the relatively greater crime-reducing expenditures on police employment in low-abortion states would be the opposite of the pattern needed to explain away the link between higher abortion and lower crime.

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4. Linking Abortion Rates to Arrests By Age

4.1. The Benefits of Shifting From Crime Data to Arrest Rate Data

The analysis up to this point has established the relationship between rising effective abortion rates and declining rates of violent crime, property crime, and murder. Crime rates are the obvious and conceptually appropriate outcome to focus on, but they have the unfortunate limitation that we do not know the crime rate by age of offender because the perpetrator is frequently unknown. Consequently, any analysis using crime rates is restricted to having state-year as the unit of analysis. This level of analysis does not allow us to take advantage of the unusual richness in the predictions of the abortion-crime hypothesis, which argues that crime patterns should differ by cohort, even in a given state and year, depending on the abortion rate when that cohort was in utero.

Since it is impossible to develop accurate crime data by age, we turn to arrest data, which enable us to test the hypothesis with a level of specificity not possible with aggregate crime data. For the subset of crimes in which an arrest is made, the age of the individuals arrested is reported. Thus, we can analyze arrest data at the level of state × year × single year of age. This allows us to include in our panel data analysis of arrests by age dummy variables for state × age, age × year, and state × year. The precise specification estimated is:

$$ln(ARRESTS_{sta}) = \beta_1 ABORT_{sta} + \gamma_{sa} + \lambda_{at} + \Theta_{st} + \epsilon_{sta}, \qquad (3)$$

where s, t, and a index state, year, and age, respectively. The variable *ARRESTS* is the raw number of arrests for a given crime. As our measure of the abortion rate for a particular cohort, we use the abortion rate in the state where the arrest was made, in the calendar year most likely to have preceded the arrestee's birth. State × age, age × year, and state × year dummies absorb variation along those different dimensions. All of the variation in the covariates used in the panel crime regressions estimated above is at the state × year level, so no variation remains in those covariates in these specifications. Data by single year of age are available only for ages 15–24 (for older and younger ages the data are grouped, typically into 5-year age windows) so we limit our sample to that age range.

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Table 5. The Relationship between Abortion Rates and Arrests, by Single Year of Age, 1985–2014

	Dependent variable:					
	ln(Violent arrests)			ln(Property arrests)		
	(1)	(2)	(3)	(4)	(5)	(6)
Abortion rate '85–'97 (×100)	−0.033	−0.056	−0.031	−0.048	−0.032	−0.029
	(0.006)**	(0.008)**	(0.006)**	(0.007)**	(0.006)**	(0.004)**
	[0.012]**	[0.024]*	[0.014]*	[0.017]**	[0.012]**	[0.009]**
Abortion rate '98–'14 (×100)	−0.042	−0.049	−0.057	−0.086	−0.080	−0.044
	(0.006)**	(0.006)**	(0.007)**	(0.005)**	(0.005)**	(0.007)**
	[0.024]	[0.026]	[0.017]**	[0.015]**	[0.015]**	[0.019]*
Abortion rate '85–'14 (×100)	−0.039	−0.051	−0.038	−0.074	−0.067	−0.033
	(0.005)**	(0.006)**	(0.005)**	(0.005)**	(0.005)**	(0.003)**
	[0.019]*	[0.025]*	[0.013]**	[0.012]**	[0.012]**	[0.010]**
Year * Age?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Implied	Implied	Yes	Implied	Implied
State * Age?	No	Yes	Yes	No	Yes	Yes
State * Year?	No	No	Yes	No	No	Yes
Observations	13,765	13,765	13,765	13,770	13,770	13,770

Note: $^{*}P < 0.05$;$^{**}P < 0.01$.
Results in the table are coefficients from estimation of equation (3). The unit of observation in the regression is annual arrests by state by single year of age. The sample covers the period of 1985–2014 for ages 15–24, and the top panel of the table estimates the effect of abortion both for our initial period (1985–97) and for the remainder of our full data period (1998–2014). The bottom panel (the third row) estimates a single abortion variable model over the entire 1985–2014 time period. The abortion rate for a cohort of age a in state s in year y is the number of abortions per 1000 live births in state s in year $y - a - 1$. Note that this is the actual abortion rate, rather than the "effective" abortion rate used in preceding tables. Therefore, the coefficients in this table are not directly comparable to those of earlier tables. If data were available for all states, years, and ages, the total number of observations would be 15,300. Due to missing arrest data and occasional zero values for arrests, the actual number of observations is somewhat smaller. A complete set of year-birth cohort interactions are included in all specifications to capture national changes in the shape of the age-crime profile over time. The table indicates the various fixed effects included in each column. Estimation is weighted least squares, with weights determined by total population by state-year-age. Standard errors clustered by cohort year of birth and state are included in parentheses; this accounts for correlation over time within a given birth cohort in a particular state. Such a correction is necessary because the abortion rate for any given cohort is fixed over time, but multiple observations corresponding to different years of age are included in the regression. Standard errors clustered by state are included in square brackets below the first set of clustered standard errors.

Table 5 presents the results. The dependent variable is the natural log of the number of arrests for the crime category listed at the top of the column. Following the original paper, we present results for violent crime (columns 1–3) and property crime (columns 4–6), but not for homicide, because homicide is rare enough (with arrest often rarer) that many state-year-age cells are empty. The set of covariates included grows moving from left to right for a given crime category, as noted in the bottom portion of

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the table. The top two rows present the coefficient on the abortion rate in the period covered by the original data (top row) and in later years (second row). Only the coefficient on the abortion rate is shown in the table. As we did for our Table 4 crime regression, we also estimated our arrest regression using the single abortion variable specification, which we report in the third row of Table 5.

Across both crime categories and all specifications, Table 5 reports abortion coefficients that are negative and highly statistically significant. The inclusion of additional covariates does not have an obvious impact on the magnitude of the abortion coefficients. Consistent with the Table 4 regression results, the point estimates on the abortion coefficient are larger in magnitude in the later period than in the initial sample period in five of the six columns in Table 5. In some of the specifications, those differences are statistically significant.[25]

The third row of Table 5 shows effects of abortion on crime estimated over the entire 1985–2014 time span. Note that columns 3 and 6, which estimate the effect of abortions on arrests with the full set of fixed effects, show an overall effect of −0.038 for violent crime and −0.033 for property crime. Both are highly statistically significant using either of the two presented standard error estimates: clustered by birth cohort and state (in parentheses) and by state (in square brackets).[26]

4.2. Improving the Precision of Our Abortion Measures

The results presented thus far have directly mimicked the specifications and data definitions of Donohue and Levitt (2001) in order to make the comparison of the new results to the original results as clear as possible. As

25. With the standard errors generated by clustering at the state-year level, the second-period estimates are statistically significantly greater than the first-period estimates for columns 3–5 (and for column 6 the P-value is 0.053). With clustering by state, the P-values for a test of the difference in first- and second-period estimates in columns 3 and 6 rise above 0.15, but they remain lower in columns 4 (P-value of 0.057) and 5 (P-value of 0.005).

26. Appendix D mimics the six regressions in the top panel of Table 5 while also generating for both our time periods the individual abortion estimates by single year of age (which we also presented in Table VII in Donohue and Levitt (2001)). One hundred and eighteen of the 120 individual estimates for specific ages in the two time periods have the predicted negative sign.

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noted, the primary exception to this was to use the superior AGI abortion by state of residence data that was furnished to us after our initial publication and which we have exclusively relied on as our main abortion measure since that time.

In the years since that first paper was published, we have also tried to address the problem of measurement error in the abortion variable in three ways with two improvements to our variable construction to more closely link these variables to what the theory suggests are the appropriate proxies and by using an instrumental variable for our residence abortion measure. First, we constructed an abortion measure that better corresponds to the actual month and year of birth of the individual. Second, we have adjusted our abortion measure to take into account cross-state mobility between birth and adolescence. Third, recognizing the noise in our abortion proxy (based on Alan Guttmacher Institute data), we have used another independently generated estimate of the abortion rate (from the Centers for Disease Control) as an instrumental variable.[27]

Table 6 illustrates the impact of running the same Table 5 regressions while using these two adjustments to our abortion measure and instrumenting to address the impact of measurement error in our abortion proxy. We report our instrumental variables (IV) estimates in two ways (as we did in Table 5): first separating our abortion effect into 1985–1997 and 1998–2014 in the first two rows, and then estimating a single abortion variable for 1985–2014, which is reported in the third row. Comparing Tables 5 and 6, one sees that the 18 estimated effects of abortion on crime are larger using the better abortion measure and the instrumenting—often doubling in magnitude—in every case except the second-period effect on property arrests with the state × year interaction (column 6), which is essentially zero.[28] All the overall period estimates in row 3 are statistically significant for both sets of standard error estimates, and considerably larger than the corresponding Table 5 values.

The move from Table 5 to Table 6 illuminates some aspects of our data and an important social phenomenon: the declining interstate mobility since

27. For additional details, see Donohue and Levitt (2008).

28. Section 5.2.1 discusses that the weaker second-period effect for property crime is the result of the strong divergence that emerges between the continuing fall in property crime (consistent with our theory) and the undiminished level of property crime arrests.

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Table 6. Estimated Effects of Abortion on Crime with Measurement Error Adjustments, 1985–2014

	Dependent Variable:					
	ln(Violent arrests)			ln(Property arrests)		
	(1)	(2)	(3)	(4)	(5)	(6)
IV Abortion effect '85–'97 (×100)	−0.065	−0.114	−0.115	−0.100	−0.074	−0.080
	(0.013)**	(0.018)**	(0.026)**	(0.013)**	(0.014)**	(0.017)**
	[0.017]**	[0.025]**	[0.029]**	[0.026]**	[0.025]**	[0.018]**
IV Abortion effect '98–'14 (×100)	−0.067	−0.068	−0.074	−0.166	−0.154	0.003
	(0.010)**	(0.012)**	(0.021)**	(0.014)**	(0.016)**	(0.022)
	[0.032]*	[0.034]*	[0.034]*	[0.056]**	[0.062]*	[0.028]
IV Abortion effect '85–'14 (×100)	−0.066	−0.087	−0.108	−0.135	−0.121	−0.065
	(0.010)**	(0.013)**	(0.022)**	(0.013)**	(0.015)**	(0.014)**
	[0.021]**	[0.029]**	[0.030]**	[0.041]**	[0.052]*	[0.017]**
Year * Age?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Implied	Implied	Yes	Implied	Implied
State * Age?	No	Yes	Yes	No	Yes	Yes
State * Year?	No	No	Yes	No	No	Yes
Observations	13,765	13,765	13,765	13,770	13,770	13,770

Note: $^{*}P < 0.05$; $^{**}P < 0.01$.
Table 6 is exactly comparable to Table 5, except that it adjusts the abortion measure to better link the timing of abortion with each relevant age cohort and to reflect the inter-state movement from birth state to where individuals live when we are measuring state arrest rates. Our instrumental variables (IV) estimate uses a CDC abortion measure as an instrument for our AGI measure. The top panel of the table estimates the effect of abortion both for our initial period (1985–97) and for the remainder of our full data period (1998–2014). The bottom panel (the third row) estimates a single abortion variable model over the entire 1985–2014 time period.

the early 1970s. All six first-period estimates at least doubled by introducing the Table 6 corrections, while none of the second-period estimates did. Focusing on the violent crime estimates, the three first-period estimates grew by at least 100%, while the second-period increases ranged from 30% to 60%. This is not surprising since our abortion data are clearly less well-measured in the early days of legalization, and therefore we would expect that instrumenting to improve the accuracy of the abortion data would have a bigger effect on our first-period estimates. Similarly, our Table 6 migration adjustments are more consequential in the first period when the amount of inter-state migration was much greater.[29] As a result, our effort to link the abortion rate for a given cohort to the state in which

29. The long-run decline in interstate mobility in the U.S. is well documented. "The interstate migration rate in 2013 was 51% below its 1948–71 average." (Molloy et al. 2014).

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the cohort members ultimately reside improves the quality of our measures, thereby raising the magnitude of the estimated effect of abortion on crime.[30]

Foote and Goetz (2008) argued that our state panel data analysis of crime rates (Table 4 in this paper) and our regressions explaining arrests by age (Tables 5 and 6 in this paper) might not fully establish the abortion-crime link that we posited. The heart of their critique was that the abortion rate may be proxying for some state-specific omitted variable, and that therefore the "crucial" test needed to eliminate the "potential omitted variable bias on the state-year level" would focus on a per capita arrest rate regression that included state-year fixed effects. This regression would control for whatever factors were influencing crime in a given state and a given year and determine whether the abortion rate at the time of any birth cohort would correlate with the arrest rate for that cohort years later as it moved through ages 15–24 (for which we have age-specific arrest rates). Foote and Goetz (2008) also suggested that concerns about residual-independence assumptions that emerged after "[Donohue and Levitt] published their 2001 paper" made it advisable to provide "a second set of standard errors [that would] cluster the standard errors by state."

We address these issues in our current Table 7, which continues our practice (in Tables 5 and 6) of showing two sets of standard errors in all our arrest rate regressions, including their preferred clustering by state. Every regression in Table 7 also includes the "crucial" state-year fixed effects. Accordingly, Table 7 presents in columns 2 and 4 exactly what Foote and Goetz stated would establish or refute the abortion-crime link. The initial, short answer is that, using the precise per capita arrest rate variable and cluster adjustment that Foote and Goetz advocate, the abortion and crime effect that we identified in 2001 remains strong and statistically significant at the 0.05 level for violent crime (see the value of −0.05 in row 4, column 2) and is negative but not statistically significant for property crime (a value of −0.007 in row 4, column 4) when estimated for the 1985–2014 period.

30. We had noted that all of our Table 4 estimates of abortion on *crime* were larger in the second period, than in the first. When we shifted to an analysis of *arrests by age* in Table 5, this pattern remained true for five out of six estimates. With the greater increase in the first-period estimates in Table 6, the pattern erodes further, with only three of the six estimates larger in the second period.

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Table 7. Distinguishing Between the Channels through Which Abortion Affects Crime, 1985–2014

	Dependent Variable:			
	ln(Viol. arrests) (1)	ln(Viol. arrests pc) (2)	ln(Prop. arrests) (3)	ln(Prop. arrests pc) (4)
IV Abortion effect '85–'97 (×100)	−0.065	−0.041	−0.044	−0.006
	(0.025)*	(0.021)*	(0.017)**	(0.013)
	[0.028]*	[0.029]	[0.019]*	[0.020]
IV Abortion effect '98–'14 (×100)	−0.084	−0.089	−0.004	−0.011
	(0.021)**	(0.021)**	(0.021)	(0.022)
	[0.029]**	[0.029]**	[0.031]	[0.037]
ln(SEER population)	0.680		0.486	
	(0.066)**		(0.053)**	
	[0.125]**		[0.134]**	
IV Abortion effect '85–14 (x100)	−0.069	−0.050	−0.036	−0.007
	(0.021)**	(0.018)**	(0.014)*	(0.012)
	[0.023]**	[0.023]*	[0.019]	[0.020]
ln(SEER population)	0.672		0.503	
	(0.061)**		(0.051)**	
	[0.124]**		[0.137]**	
Year * Age?	Yes	Yes	Yes	Yes
State fixed effects?	Implied	Implied	Implied	Implied
State * Age?	Yes	Yes	Yes	Yes
State * Year?	Yes	Yes	Yes	Yes
Ln Population?	Yes	No	Yes	No
Observations	13,765	13,765	13,770	13,770

Note: $^{*}P < 0.05$; $^{**}P < 0.01$
Table 7 modifies the column 3 and 6 specifications from Table 6 in two ways to remove the cohort-size effect of abortion on arrests by single year of age for ages 15–24. Columns 1 and 3 of Table 7 simply add a control for the population of each state by single year of age. The fact that the estimated values for this population control are substantially below 1 (see rows 3 and 5) illustrates the presence of measurement error in our population variable. Columns 2 and 4 control for population by changing the dependent variable to ln(per capita arrest rate) by single year of age, and these estimates will suffer from ratio bias because of the observed measurement error in the population variable that appears in the denominators of both the dependent variable and the abortion independent variable. Standard errors clustered by cohort year of birth and state are included in parentheses, with standard errors clustered by state in square brackets directly below.

While 17 years of additional data have strengthened the evidence in support of the abortion-crime hypothesis, the current Table 7 finding is essentially the same as what we showed in our 2008 response to Foote and Goetz.[31]

31. The first-period estimates in Table 7 are not identical to those in our reply to Foote and Goetz (the "IV using CDC" row of Table II in that paper) for five reasons:

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But while the data through 2014 clearly meets the "crucial" test that Foote and Goetz articulated for the link between abortion and declining violent crime, it should also be noted that their recommended per capita arrest rate regression understates the impact of legalized abortion on crime in multiple ways. First, legalized abortion impacted crime in the 1990s and therefore contributed to the momentous crime drop in a way that is universally acknowledged: it reduced the size of cohorts moving into their high-crime teenage years beginning in the early 1990s.[32] Table 7 regression will not capture that effect because it only captures the per capita crime rate of each cohort, thereby neglecting any cohort-size effect.

Second, and more critically, the per capita regression that Foote and Goetz advocate is biased against a finding that legalized abortion has a selection effect that leads to a per capita reduction in crime. The reason is straightforward: the denominator of the dependent variable in the per capita regression is the size of the cohort born in year *t*, which is also the identical denominator of the abortion rate independent variable for that same cohort. In other words, we have the same population variable in the denominator of both the dependent variable and the key independent variable of interest. Since Foote and Goetz acknowledge that this population variable "is measured with error," the estimated effect of abortion on crime in columns 2 and 4 will be upward biased, thereby understating or obscuring any dampening effect that legalized abortion had on the per capita arrest rate.[33]

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(1) throughout this paper, we define our first-period results for the same 1985–97 time period used in Table IV of our 2001 paper, while Foote and Goetz used data through 1998; (2) we used updated AGI and CDC abortion data and the precise date of legalization for our early-legalizing states, while the reply to Foote and Goetz simply used 1970 for those states; (3) previously we weighted our arrest regressions with a Census population measure by state-year; we now adopt the conceptually preferable weighting by state-year-age population using SEER data; (4) all UCR crime and arrest data is subject to revision and we used the most updated data in this paper; and (5) while our earlier statistical analysis was done in Stata, we used R version 3.6.3 for this paper, which did have modest impacts on the size of our standard errors. We describe the details of all of our variables in greater detail in Appendix G.

32. As Levine et al. (1999) notes, "States legalizing abortion experienced a 4% decline in fertility relative to states where the legal status of abortion was unchanged. The relative reductions in births to teens, women more than 35 years of age, non-White women, and unmarried women were considerably larger."

33. Long (1980) explains that estimates of the relationship between two ratio variables that share a common denominator can be seriously biased by measurement error and random errors of 5 or 10%—-a "level of error likely to be present in much

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To illustrate the presence of this "ratio bias," we include in Table 7 a regression with the count of arrests by cohort (the dependent variable) regressed on the abortion rate and a control for the size of that cohort. Since the coefficients on the population variable (shown in the third and fifth rows of Table 7) are substantially below 1, being roughly two-thirds for violent crime (column 1) and about one-half for property crime (column 3), we know—as Foote and Goetz acknowledged from the same evidence—that these results reflect measurement error in the population by state and age data. But this measurement error confirms the presence of ratio bias that attenuates the estimated abortion coefficient in the per capita regressions of Table 7 for which Foote and Goetz advocate, with that bias presumably greater for property crime by virtue of the considerably smaller column 3 coefficient on population.[34]

But while the columns 1 and 3 regressions reveal the attenuation in the estimated influence of population on arrests, these regressions are superior to the columns 2 and 4 per capita regressions in that they do not suffer from the ratio bias that understates the true selection effect of abortion on crime in the per capita regressions. First, note that while both of the overall (row 4) estimates of the impact of abortion on violent crime arrests are highly significant using either standard error measure, the column 1 estimate is almost 40% larger in absolute value than the column 2 estimate. The column 1 violent crime arrest estimates are also statistically significant in each of our two time periods, again using either standard error measure. Second, the column 3 regression generates a sizeable negative estimated effect of abortion on property crime arrests for the entire period with a *P*-value of 0.083 when clustering by state (significant at the 0.05 level when clustering

social science data"—can reverse the sign of a true negative relationship. Long concludes "Given the seriousness and generality of these measurement error complications with ratio variables, we should exercise special caution when interpreting ratio relationships in the course of research."

34. Furthermore, just as we have identified how mismeasurement in the age cohort by state population data has led to attenuation bias that understates the relationship between cohort size and cohort arrests, mismeasurement error in the abortion data can lead to a similar attenuation of the impact of abortion rates on cohort arrest counts and rates. As we explained in Donohue and Levitt (2004, 2008): "As more controls are included in the regression [such as state-age, age-year, and state-year effects], the remaining variation in the abortion measure may become dominated by noise. The shrinkage of the abortion coefficient, in this scenario, is due to attenuation bias." (Donohue and Levitt 2008: 428.)

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by birth cohort per state). This column 3 property crime arrest estimate for the entire period is more than five times larger than the column 4 estimate that is marred by ratio bias. Note also that the column 3 first-period estimate of abortion on property crime arrests is negative and statistically significant using either standard error measure.

4.3. Robustness Check of the Abortion-Arrest Rate Link

One might be concerned that our arrest by age results could be driven by an outlier large state like New York that had a particularly dramatic drop in crime or a jurisdiction with an unusually high number of abortions like D.C. To test this possibility, we ran our exact Table 7 analysis on the 26 states for which complete arrest data are available. The results are shown in Table 8, which drops a very eclectic, and non-selected (by us) sample of 25 states, including New York, Florida, D.C., Pennsylvania, and Colorado. Comparing the four abortion estimates for the entire 1985–2014 period depicted in Tables 7 and 8, we see that all are more negative for the states with complete arrest data, and the negative abortion coefficient in the property per capita arrest regression (row 4, column 4) triples in absolute value in Table 8 and is significant at the 0.05 level with clustering by birth-cohort by state. Moreover, the 1985–2014 abortion estimates for both violent and property crime that are not marred by attenuation bias (row 4 for columns 1 and 3) are both statistically significant at the 0.05 level even with the more stringent clustering. In other words, our arrest regressions findings are not only robust, but are strengthened even with the diminished sample size when we limit the sample to the 26 states with the most complete arrest data.[35] We also report our Tables 5 and 6 results with our set of 26 states with complete arrest data in Appendices E and F, respectively.

35. We also re-ran the Table 7 regressions with the same robustness check we used earlier (see footnote 20), which involved dropping the five states (Florida, Kansas, Kentucky, Nebraska, and Montana) that other scholars had identified as having persistent reporting problems in their UCR data. Excluding these five states increased the abortion impact in ten of the 12 estimates, but did not fundamentally change the results.

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Table 8. Distinguishing Between the Channels through Which Abortion Affects Crime 26 States with Complete Arrest Data, 1985–2014

	Dependent Variable:			
	ln(Viol. arrests) (1)	ln(Viol. arrests pc) (2)	ln(Prop. arrests) (3)	ln(Prop. arrests pc) (4)
IV Abortion effect '85–'97 (×100)	−0.060	−0.046	−0.038	−0.011
	(0.020)**	(0.018)**	(0.010)**	(0.009)
	[0.038]	[0.040]	[0.020]	[0.020]
IV Abortion effect '98–'14 (×100)	−0.100	−0.107	−0.042	−0.056
	(0.022)**	(0.023)**	(0.021)	(0.023)*
	[0.036]**	[0.034]**	[0.023]	[0.027]*
ln(SEER population)	0.798		0.602	
	(0.060)**		(0.052)**	
	[0.125]**		[0.173]**	
IV Abortion effect '85–'14 (×100)	−0.071	−0.060	−0.039	−0.021
	(0.017)**	(0.016)**	(0.010)**	(0.010)*
	[0.031]*	[0.032]	[0.016]*	[0.016]
ln(SEER population)	0.767		0.600	
	(0.056)**		(0.053)**	
	[0.129]**		[0.170]**	
Year * Age?	Yes	Yes	Yes	Yes
State fixed effects?	Implied	Implied	Implied	Implied
State * Age?	Yes	Yes	Yes	Yes
State * Year?	Yes	Yes	Yes	Yes
Ln Population?	Yes	No	Yes	No
Observations	7,800	7,800	7,800	7,800

Note: $^{*}P < 0.05$; $^{**}P < 0.01$
Table 8 replicates the Table 7 analysis but only using the 26 states for which complete arrest data is available. The 25 excluded states are listed in the notes to Appendix E. Standard errors clustered by cohort year of birth and state are included in parentheses, with standard errors clustered by state in square brackets directly below.

5. Evaluating Our Overall Regression Results

5.1. Comparing Our Crime and Arrest Rate Results

Taking stock of our two strands of regression analysis, we first showed that there is a continuing drop in crime that is proportional to our abortion measure in each state as cohorts born following the legalization of abortion entered adolescence and then their high-crime years (Table 4). No other explanatory variable in our panel data model of crime comes close to the

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statistical power of abortion, and this result is robust with an alternative set of controls (Appendix C) or dropping 5 states that had consistent problems reporting crime data under the Uniform Crime Reports for multiple years.

We then switched to an analysis of arrest data so that we could directly link arrests of 15 year olds in a given state with the relevant abortion rate for their birth cohort, and so on by single year of age through age 24 (the end of the UCR's arrest data by single year of age). Table 5 established the inverse relationship between the natural log of arrests for a single year of age and the abortion rate for the birth year of that arrest cohort. We showed that this relationship held and was highly statistically significant (using two different standard error measures), even when controlling for state-year fixed effects.

Our findings based on *arrests* dramatically support the earlier findings of our panel data analysis of state-year *crime* data, but we should pause to emphasize how striking the arrest results are and how they rely on a very different source of variation, with a substantially different abortion measure that has a sharply different time trend. As one can see in Figure 1, the basic pattern for the effective abortion rate that we use as the key explanatory variable in our state-year crime regressions is predominantly rising throughout our data period, although of course differently across different states (and with a few states experiencing a downturn in the effective abortion rate for property crime late in our data period).[36] But the pattern for the abortions per 1,000 live births that we use for our arrest rate regressions by single age of arrest peaks far earlier at 432 in 1981 and then declines over the relevant portion of our data (the 1999 abortion rate would be the last year relevant to the arrest of 15 year olds in 2014). Of course, these patterns vary substantially by state, but for some years the youngest cohorts will have the highest abortion rates that we link with their arrests and sometimes it will

36. The EAR for both murder and violent crime never peaked before 2013 or 2014 for any jurisdiction, except for DC (in 2011 and 2010, respectively). The picture is different for the EAR based on property crime, for which 29 states peaked before 2013, although with most states having only modest declines by the end of our data period in 2014. The EAR for property crime peaked in DC in 2005 and declined by 11.2% by 2014, which was the earliest peak and the largest decline for any state.

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be the oldest cohorts that have the highest abortion rates. Given all of the imperfections in abortion and arrest data, the lapse of time between birth and arrests with inter-state mobility, and the fact that we are controlling for the level of crime in the particular state and year, the fact that the negative relationship between abortion and arrests emerges so powerfully is really quite stunning.

We continued this arrest rate analysis by trying (1) to improve the measurement of the appropriate abortion rate for any particular arrest cohort to better reflect the timing of the relevant abortions and the level of arrests years later for the corresponding cohort and (2) to capture the abortion rate in the state of birth for those who had subsequently moved to a different state. In addition, we used an instrumental variable approach to address imperfections in our abortion measure. Again, the results were highly statistically significant (even using the most stringent standard error measures), and the magnitudes of the estimated abortion effects grew substantially with these better measures (Table 6). While the impact of abortion on property crime was large and highly statistically significant when estimated for the entire period from 1985 to 2014 and for the initial period from 1985 to 1997, the one deviation from the uniformly strong abortion effects was a near-zero estimate for property crime arrests from 1998 to 2014 when state-year controls were added—an anomaly that we discuss below.

Finally, we followed Foote and Goetz in estimating per capita arrest rates and showed that what they viewed to be the critical test of the abortion-crime link was met for violent crime arrests—even with the most stringent standard error measures. Although the property crime estimates were negative but not statistically significant with this per capita specification, we showed that this model had the unattractive quality of using the relevant population for each particular age group in the denominators of both the arrest rate (the dependent variable) and the abortion rate (the key independent variable). Since the resulting ratio bias would tend to obscure the abortion-crime link given the documented errors in our population measures, we addressed this concern by estimating an alternative arrest rate regression based on arrest counts that directly controlled for the relevant population. The resulting estimates in columns 1 and 3 of Table 7 further strengthened the already strong violent crime results and generated a highly statistically significant estimated effect of abortions on property crime in the first period (with

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either standard error measure). Again, we see a weak property arrest rate effect in the second period, as in Table 6.[37]

5.2. Our Arrest Rates Results for Violent and Property Crime

What then are we to make of the strong findings that legalized abortion substantially reduced *all crime*, reduced violent crime *arrests* for both periods, and reduced property crime arrests—but only for the first period? One possibility is that the poorer quality of arrest data by age versus state crime data may explain the weaker property crime arrest estimates in the second period. Support for this view comes from re-estimation of Table 7 using only the 26 states that had complete arrest rate data. This approach increases *all* of the violent and property overall period estimates and generates a statistically significant impact of abortion on property arrests, even using the most stringent standard error measure (Table 8, Column 3).

But in light of our Table 4 estimates showing the dampening effect of abortion is roughly the same for both violent and property crime, it remains a bit of a puzzle why our estimated abortion effects would be greater for arrests for violent crime than for property crime (Tables 7 and 8). Two factors may be relevant to this issue, which we discuss in turn.

5.2.1. *The Divergence Between Property Arrests and Property Crime in Our Second Period.* Recall that the abortion-crime hypothesis posits that the legalization of abortion will reduce *crime* many years later as post-legalization birth cohorts move through their years of highest criminal

37. The move from Table 6 to Table 7 enabled us to decompose the total impact of abortion on arrests (captured in Table 6) into two components: the impact on the size of the birth cohort as well as the per capita reduction in arrests induced by abortion (with the second effect reflected in Table 7). Since the original legalization of abortion reduced the size of early birth cohorts (thereby diminishing total arrests when those cohorts reached their high-crime years), we would expect that the Table 7 estimates, which removed this effect, would be smaller than the first-period estimates in Table 6. This is exactly what we see. Conversely, in the later birth cohorts, which did not experience the cohort-size shrinkage, our controls for population actually increase the estimated dampening effect of abortion on arrests in our second period.

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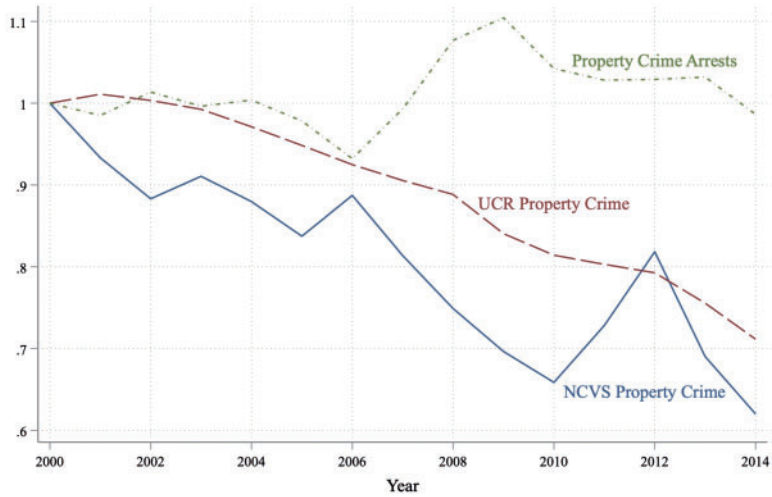

Figure 8. Property Crime and Arrests Rates, 2000–14 (Indexed to 1 in 2000).
Note: Values in 2014: Arrests = 0.986; Crime = 0.711 (UCR), and = 0.620 (NCVS).

activity. In the first portion of the article, we addressed this issue directly looking at crime data by state and year. Our subsequent arrest analysis gave us the advantage of being able to link abortion rates of birth cohorts to specific ages in which the members of that birth cohort were arrested. While this aided our effort to test the abortion-crime hypothesis in a more precise way, it was premised on the assumption that the relationship between arrests and crime would be stable over time. While this assumption is true for the relationship between violent crime and violent arrests, and was true for the first-period of our data analysis for property crime, the arrest/crime ratio for property crime shifted sharply during our second data period.

Figure 8 below has two lines that document the substantial decline in property *crime rates* in the United States from 2000 to 2014: one reflects the police-reported property crime rate as published by the UCR and one reflects the decline documented by the National Crime Victimization Survey (NCVS). As the figure shows, property crime itself fell by roughly 30% or more. The top line in the graph, which measures property crime arrests per capita, shows a very different path. Despite the very substantial drop

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in property crime over this period, there was virtually no drop in property arrests over this period. Indeed, property crime *arrests* rose sharply starting in 2006, even as property *crime* continued to fall substantially.[38]

Since the abortion and crime hypothesis posits that the increased abortion rate will lead to reductions in *crime*, but our arrest rate regressions link abortion rates with arrests, Figure 8 illustrates that our arrest rate regressions should be considerably weakened in our second period. In fact, this is the exact pattern we observe in Table 7: our first-period effect is negative and statistically significant with even the most stringent standard errors, but the second-period effect is dramatically smaller and not statistically significant.

Figure 9 shows the comparable graph for violent crime and arrests from 2000 to 2014. Even though there is some divergence starting in 2006, the trends in UCR violent crime and arrests are virtually identical thereafter. Moreover, the overall disparity of a 19% decline in violent crime arrests versus a 29% drop in violent crime is only about one-third the size of the arrest-crime disparity for property crime over this period.

5.2.2. *Selective Under-Reporting of Crime.* A new paper by Richard Boylan provides additional illumination on the abortion-crime relationship (Boylan 2019). Boylan begins by using two tests to show that police departments are less likely to submit statistics when crime is high. Since our hypothesis is that higher abortion rates reduce crime, it is conceivable that there might be a selection effect operating that could influence our estimates of the impact of abortion on crime. Boylan finds this to be the case, and then goes on to show that studies such as ours that rely on UCR crime data will tend to underestimate crime and thereby tend to understate the impact of policies on crime.

38. We turned to some of the top scholars on crime and none were previously aware of this unusual divergence between property crime and arrest during our second period. Phil Cook provided an interesting conjecture that "technological shifts (more cameras, electronic tracking devices) have made crimes like shoplifting and vehicle theft easier to solve." Richard Boylan suggested that the boom in property values after 2000 swelled governmental budgets and police hiring, that might have led to more property arrests simply because there were more officers in place even as all crime was falling substantially.

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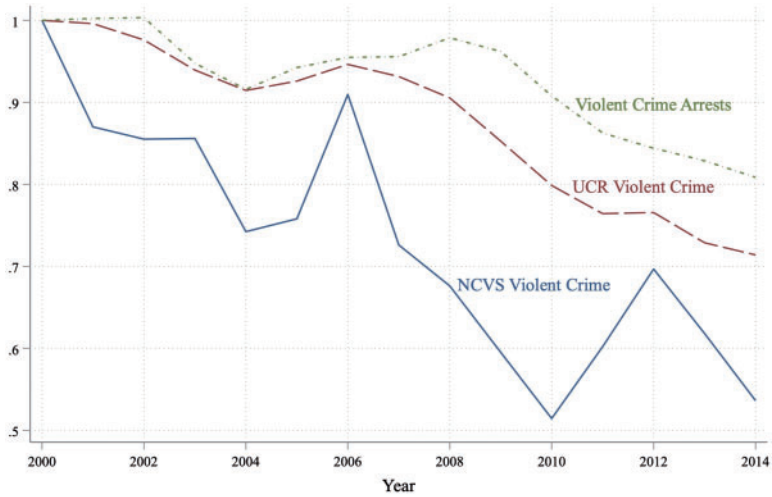

Figure 9. Violent Crime and Arrests Rates, 2000–14 (Indexed to 1 in 2000).
Note: Values in 2014: Arrests = 0.809; Crime = 0.714 (UCR), and = 0.536 (NCVS).

We alluded to this effect in footnote 16 where we noted that the low-abortion states were more likely to under count UCR homicides, which led to a stronger estimated impact of abortion on murder in Table 4 when we used Vital Statistics data, which is not collected by the police and is subject to mandatory reporting requirements. In other words, the police departments that disproportionately under-report to the UCR tend to be in lower-abortion rate states. Boylan describes his findings as follows:

> "Depending on the manner that I account for missing statistics, an increase in the effective abortion rate of 100 per 1000 live births is associated with a reduction in property crimes by 10–11%, while accounting for sample selection leads to a 15% reduction in property crime. Thus, the results show that missing reports are biasing downwards the relation between abortion and crime."

In summary, Boylan provides evidence suggesting that the true crime-reducing impact of increased abortion is greater than our estimates using UCR crime and arrest data (on which we exclusively rely in all of our violent and property crime and arrest models). Indeed, the bias towards zero that Boylan identifies in the relation between abortion and crime is particularly noteworthy for property crime.

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Accordingly, it will require further exploration to determine whether the weaker second-period effects of abortion on property crime observed in Table 7 reflect a shift in policing tactics or clearance rates that alters the relationship between arrests and property crime or defects in the quality of arrest data (as suggested in Table 8) or crime data (as suggested by Boylan).

6. Considering the Impact of Lead on Crime

We have already noted that the conventional explanations for both the enormous drop in crime after 1992 and the wide differences among states in the degree of this decline do not have anywhere near the explanatory power of the abortion effect that we documented in Table 4's state panel data model for the period 1985–2014. Another novel theory posits that the efforts to reduce lead exposure have also contributed importantly to this crime decline. This raises the obvious question: might the abortion effect that we document simply be proxying for the lead effect, which is in fact the true causal force behind the post-1992 crime decline?

Reyes (2007) is one of the most important papers illustrating the impact of childhood lead exposure on crime. Reyes is the first researcher to present a state-level panel data analysis that links lead levels in early childhood to subsequent changes in crime, and she specifically explores whether controlling for the lead effect undermines the abortion and crime link. The short answer is that it does not.

Figure 10 reproduces Table 5 from Reyes (2007), which essentially conforms to the specification of our Table 4 over the period 1980–2002. The first row of the Reyes table estimates the elasticity of crime with respect to lead and the second row introduces the elasticity of crime with respect to abortion to test whether the abortion effect is explained by the lead effect. The highlighted estimates make it clear that this is not the case. The abortion effect on crime is extremely strong and highly statistically significant for violent crime, property crime, and murder.

When one considers the large increase in the number of abortions that occurred after legalization, the Reyes (2007) results showing that a doubling of the abortion rate would lead to almost a 25% drop in both violent crime and murder and a nearly 15% drop in property crime is impressive. Moreover, the property estimate is significant at the 0.01 level and the violent

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TABLE 5 — Panel Data Estimates of the Relationship Between Childhood Lead Exposure and Crime

	Violent Crime			Property Crime			Murder		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lead (grams per gallon)	0.976 *	0.888 **	0.785 *	0.427	-0.046	-0.078	1.084	0.492	0.369
	(0.542)	(0.449)	(0.403)	(0.368)	(0.304)	(0.281)	(0.656)	(0.650)	(0.596)
Abortion			-0.224 **			-0.144 **			-0.232 **
			(0.057)			(0.056)			(0.067)
State unemployment rate		-0.023	0.702		2.329 **	2.878 **		2.086 **	2.845 **
		(1.057)	(0.839)		(0.880)	(0.819)		(1.314)	(1.221)
Log income per capita		-0.547	-0.073		-0.434	-0.171		-0.092	0.440
		(0.350)	(0.371)		(0.277)	(0.285)		(0.387)	(0.491)
Poverty rate		-0.007	-0.003		-0.009 **	-0.008 **		-0.016 **	-0.011 *
		(0.005)	(0.004)		(0.004)	(0.004)		(0.008)	(0.007)
AFDC generosity (15 yr lag)		0.013	0.008		0.005	0.003		0.010	0.005
		(0.013)	(0.011)		(0.012)	(0.012)		(0.024)	(0.021)
Teen pregnancy rate (effective)		2.276	0.263		0.444	-0.511		6.376	3.734
		(3.961)	(3.471)		(2.888)	(2.597)		(5.364)	(5.004)
Log prisoners per capita (1 yr lag)		0.119	0.061		-0.138	-0.150		-0.133	-0.214
		(0.110)	(0.092)		(0.111)	(0.108)		(0.159)	(0.133)
Log police per capita (1 yr lag)		-0.221 **	-0.181 *		-0.214	-0.189		-0.424 **	-0.383 **
		(0.117)	(0.110)		(0.152)	(0.150)		(0.179)	(0.173)
Shall-issue concealed weapons law		0.060 **	0.041 *		0.066 **	0.058 **		-0.020	-0.044
		(0.030)	(0.026)		(0.028)	(0.026)		(0.054)	(0.050)
Beer consumption per capita		0.043 **	0.020 **		0.059 **	0.047 **		0.031 *	0.004
		(0.013)	(0.011)		(0.011)	(0.012)		(0.019)	(0.020)
Share of population age 15 to 29		1.141	-1.285		1.310	-0.121		2.384	-0.303
		(1.855)	(1.737)		(1.291)	(1.151)		(2.389)	(2.287)
R-squared	0.95	0.96	0.96	0.94	0.96	0.96	0.94	0.96	0.96

Notes. The dependent variable is the natural log of the per capita crime rate shown at the top of the column. Lead is effective gasoline lead exposure (grams per gallon) in the first 3 years of life, corrected for migration. To represent elasticities, coefficients and standard errors for effective lead exposure have been multiplied by the mean of the effective lead exposure variable over the sample period (1.27 grams per gallon, c.f Table 1.) A similar adjustment is made for effective abortion exposure. The data include observations for 50 states and the District of Columbia for the years 1985 to 2002 (918 observations). Observations are weighted by state population. State and year fixed effects are included in all columns. Standard errors are Huber-White robust and clustered on state. Significance is indicated by ** for p-values below 0.05 and * for p-values below 0.10.

Figure 10. Reyes (2007) Explores Lead and Abortion Effect, 1985–2002.

crime and murder estimates are significant at well below the 0.0001 level! Moreover, introducing the abortion variable into the panel data model leaves no estimated lead effect on crime to be statistically significant at the 0.05 level, so it is clear that the lead hypothesis does not undermine the link between abortion and crime.

7. Conclusion

It is rare for an economic theory to make predictions for twenty years into the future that are both bold and precise. The abortion-crime hypothesis of Donohue and Levitt (2001), however, did just that. Based on an extrapolation that assumed the same point estimates in the ensuing two decades as were estimated in the original sample, Donohue and Levitt (2001) predicted that crime would fall an additional 20% in the United States. The results in this paper provide strong support for that prediction. Using the same specifications as Donohue and Levitt (2001), but extended to a sample that includes an additional 17 years of data, in almost all cases the point estimates are at least as large as in the original analysis, and in many cases the point estimates are bigger. From 1997 to 2014, the effective abortion rate for

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violent crime rose from roughly 170 to 341 and the effective abortion rate for property crime rose from 247 to 348. Using the preferred specifications in Table 4—the same specifications upon which the original predictions were based—the implied overall crime decline due to legalized abortion over the ensuing 17 years was 17.5%.[39] From the 1991 peak of crime in the United States, the rates of violent and property crime each fell by 50% and VS murder fell by 52% by 2014. Over that same period, we estimate that legalized abortion reduced violent crime by 47%, property crime by 33%, and VS murder by 41%. Thus, while many other factors were operating to stimulate or suppress crime, legalized abortion can explain most of the observed crime decline.[40]

The strong evidence of the impact of legalized abortion on crime in the United States would of course be strengthened by similar evidence from a different continent where the timing of abortion legalization and frequency of abortions varies greatly from ours. In fact, François et al. (2014) provide such evidence with a panel data analysis with country and year fixed effects from 1990–2007 for 16 Countries in Western Europe. The paper "confirm[s] the negative impact of abortion on crime for both homicides and thefts...." While the authors do not compute the impact of their regression coefficients and even speculate that their estimates are smaller than ours, their model

39. This estimate is based on the Table 4 figures showing that from 1998 to 2014 each increase of 100 in the effective abortion rate would lower violent crime by 18.9% and property crime by 16.8%.

40. If abortion legalization had beneficial impacts on crime, which is largely a male pathology, one might expect to see some beneficial improvements for females emerging at roughly the same time. Indeed, we do. As Donohue et al. (2009) noted: "After 41 consecutive years of increase, the out-of-wedlock teen birth rate declined in 1992 and by 2002 was 20% below its peak....We find that the historical abortion rate—that is, the abortion rate in the state and year of a teenager's own birth—is negatively correlated with teen fertility, even after controlling for age-year interactions, state-age interactions, and state-year interactions. State-year interactions are particularly important, since they absorb any environmental factors that are common to a state at a point in time, including the current abortion rate, state laws regarding abortion access, state economic factors, and welfare generosity. We estimate that, 15–19 years later, when these cohorts reach childbearing age, in utero abortion exposure is associated with a 6% reduction in unmarried teen births.... Based on our point estimates, legalized abortion in the 1970s appears to explain about 25% of the observed decline in teen out-of-wedlock childbearing between 1991 and 2002." The identical timing of the sudden, unanticipated benign shifts in male and female outcomes 18 years after the decision in *Roe v. Wade* when the first post-legalization cohort reached age 17 is striking.

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showing the impact on crime 15 years after abortion legalization implies that over the ensuing decade, abortion legalization reduced homicides by 12–40% and reduced theft by 23–43%. These estimates are roughly comparable to and therefore provide significant support for our own estimates on data from the United States.

An enormous literature has developed showing that optimizing the circumstances of pregnancy and early childhood can improve life prospects on everything from cognitive development and physical and mental health to educational success, earnings, and avoidance of crime (Almond, Currie, and Duque 2018). Since legalization of abortion provides a vehicle to delay childbirth until a time when these critical environmental and family circumstances would be relatively more favorable or prevent it if they are particularly dire, this growing literature on improving adult outcomes is supportive of the underlying mechanism of the abortion-crime hypothesis.[41] As we have noted previously, our study has tried to elucidate one previously unidentified factor that can provide insight into the otherwise

41. The case of Clifford Boggess illustrates the nature of the causal mechanism. Boggess' mother suffered from mental illness and alcoholism and, although married, had multiple pregnancies in a relationship with her employer. Believing that she could not care for more children when she became pregnant with twins, but with abortion illegal in Texas in the 1960s, she tried to give herself an at-home abortion, with the result that one fetus died and the other was born with brain damage. Subsequently pregnant with her employer's child again, she reluctantly gave birth in 1965 to Clifford Boggess, who she badly beat and neglected. When he was 11 months old, the State removed all eight children because of the neglect, and when they were later returned, the mother declined to take Clifford back since she could not handle an infant. Boggess was bounced around and rejected from different homes until he ultimately killed two elderly men in separate robberies in 1986 and was executed by the state of Texas in 1998 (Frontline 1998a,b).

A somewhat similar situation was observed for the Norwegian mass murderer Anders Breivik. His mother was a troubled young woman with mental illness who fled an abusive household at age 17. When she later became pregnant with Anders, she described the unborn baby as "a nasty child that wreaked havoc and tormented her." As a result, she sought an abortion but this was denied since she was just past the three-month limit. After Breivik was born, the mother stopped breastfeeding the child because he was "sucking the life out of her." In 1983 and 1984, some of Norway's top child psychologists wanted to forcibly remove the child from his mother's care, noting he "was brutally rejected. She openly stated, in front of other people, that she wished her son was dead." In July 2011, at age 32, Breivik killed 77 (TV2 2016).

A recent study examined the economic consequences on women who sought abortions but were turned away because of a time limit: "We find evidence of a large and persistent increase in financial distress for the women who were denied an abortion that is sustained for the 6 years following the intended abortion" (Miller et al. 2020).

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unexplained drop in crime over the last two decades. All of the estimated crime-reducing effects from legalized abortion could be generated by reducing unwanted pregnancies and births.[42] But as Darroch et al. (2001) noted, "U.S. teenagers [had] the highest rates of pregnancy, childbearing, and abortion" from 1970 to 2000 compared to England, Canada, Sweden, and France primarily because of less contraceptive use. Overall, 18.8% of pregnancies in the United States ended in abortions in 2014 (Jones et al. 2007).[43] Restraining access to abortion without reducing unwanted pregnancies is both personally and socially costly.

42. Indeed, a concerted effort to provide birth control to women in Delaware who expressed a desire not to become pregnant, has led to a very sharp reduction in unwanted pregnancies and abortion in that state. In 2010, Delaware had the highest unintended pregnancy rate in the country, but as a result of the birth-control initiative unintended pregnancies were reduced and the abortion rate declined 37% between 2014 and 2017 (Leonhardt 2019).

43. Worldwide, 288.5 million abortions were performed over the 5-year period from 2010 to 2014 (Ganatra et al. 2017).

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Appendix A. Summary Statistics for High- versus Low-Abortion Rate States, 1985–2014

	High Abortion Rate States			Low Abortion Rate States		
Variable	Mean	Std Dev (Overall)	Std Dev (Within State)	Mean	Std Dev (Overall)	Std Dev (Within State)
Violent crime per 100,000 residents	601.3	259.4	193.2	475.4	193.1	104.2
Property crime per 100,000 residents	3,964.5	1,357.0	1,140.7	3,794.4	1,033.9	737.4
Murder per 100,000 residents (UCR)	6.9	4.0	2.7	6.3	3.1	1.8
Murder per 100,000 residents (VS)	7.3	3.7	2.6	6.8	3.3	1.8
EAR: Violent crime	264.3	169.4	156.5	137.8	94.6	88.5
EAR: Property crime	311.5	160.8	142.0	164.3	91.6	82.8
EAR: Murder	233.2	167.8	157.5	120.8	92.9	88.1
Prisoners per 1000 residents (t-1)	3.8	1.3	0.8	3.9	1.9	1.2
Police per 1000 residents (t-1)	3.3	0.7	0.3	2.9	0.7	0.4
Real state personal income per capita	18,276.5	2,832.4	2,005.0	15,709.8	2,361.3	1,872.7
Real AFDC generosity per recipient family/1,000 ($t - 15$)	4.4	1.7	1.1	3.1	1.6	1.0
State unemployment rate (percent)	6.3	2.1	1.9	6.1	1.7	1.5
Beer consumption per capita (Gallons of ethanol)	1.2	0.2	0.1	1.3	0.2	0.1
Poverty rate	13.1	2.9	1.7	13.9	3.5	1.8

Real AFDC generosity per recipient family is measured in thousands of dollars and indexed at 1982-1984 values. The 32 states in the low-abortion rate group are: Alabama, Alaska, Arkansas, Delaware, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, West Virginia, Wisconsin, and Wyoming. The 19 high-abortion rate states are: Arizona, California, Colorado, Connecticut, District of Columbia, Florida, Georgia, Hawaii, Maryland, Massachusetts, Michigan, Nevada, New Hampshire, New Jersey, New York, North Carolina, Rhode Island, Virginia, and Washington.

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Appendix B. Panel Data Estimates of the Relationship between Abortion Rates and Crime (Single Abortion Variable version of Table 4), 1985–2014

	Dependent Variable: Log Per Capita Value of...							
	Violent crime		Property crime		UCR murder		VS murder	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effective abortion rate '85–'14	−0.192**	−0.189**	−0.146**	−0.165**	−0.136**	−0.158**	−0.146**	−0.170**
	(0.019)	(0.019)	(0.016)	(0.015)	(0.016)	(0.020)	(0.016)	(0.019)
ln(lagged prisoners per capita)		0.008		−0.105**		−0.111*		−0.124*
		(0.037)		(0.034)		(0.056)		(0.051)
ln(lagged police per capita)		−0.015		−0.027		−0.138*		−0.186**
		(0.015)		(0.014)		(0.054)		(0.049)
Unemployment rate		−0.006		0.562		1.413		1.259
		(0.354)		(0.310)		(0.723)		(0.669)
Ln(Real per capita income) 1982–84		−0.0002		−0.086		0.320		0.164
		(0.129)		(0.115)		(0.223)		(0.203)
Poverty rate		−0.002		−0.001		−0.003		−0.0002
		(0.001)		(0.001)		(0.003)		(0.003)
Real AFDC generosity 1982–84		0.004		−0.001		−0.008		−0.003
		(0.003)		(0.003)		(0.008)		(0.007)
Shall-issue concealed weapons law		0.014		0.019		−0.043		−0.023
		(0.015)		(0.011)		(0.023)		(0.022)
Beer consumption per capita		0.078		0.024		0.287**		0.285**
		(0.050)		(0.044)		(0.107)		(0.097)
Year FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,530	1,517	1,530	1,517	1,530	1,517	1,512	1,499
R^2	0.813	0.843	0.972	0.979	0.853	0.864	0.876	0.886
Adjusted R^2	0.803	0.833	0.971	0.977	0.845	0.856	0.869	0.879

Note: $^{*}P<0.05$; $^{**}P<0.01$.
The dependent variable is the log in the per capita crime rate named at the top of each pair of columns. The first column in each pair presents results from the specifications in which the only additional covariates are state- and year-fixed effects. The second column presents results using the full specification. The data set is comprised of annual state-level observations (including the District of Columbia) for the period 1985–2014. State- and year-fixed effects are included in all specifications. The prison and police variables are once-lagged to minimize endogeneity. Real AFDC generosity per recipient family is lagged by 15 years, measured in thousands of dollars and indexed at 1982–84 values. Estimation is performed using a two-step procedure. In the first step, weighted least squares estimates are obtained, with weights determined by state population. In the second step, a panel data generalization of the Prais–Winsten correction for serial correlation developed by Bhargava et al. (1982) is implemented. Standard errors are in parentheses.

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Appendix C. Panel Data Estimates of the Relationship between Abortion Rates and Crime (Modified Specification with Added Demographic Controls), 1985–2014. This table provides a robustness check on our Table 4 results by using the crime model specified in Donohue et al. (2019) (analyzing the impact of right-to-carry gun laws) to provide estimates of the impact of abortion on crime. The results are clearly quite comparable.

	Dependent Variable: Log Per Capita Value of...							
	Violent crime		Property crime		UCR murder		VS murder	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effective abortion rate '85–'97	−0.184**	−0.184**	−0.138**	−0.162**	−0.087*	−0.078	−0.098**	−0.104*
	(0.022)	(0.023)	(0.017)	(0.018)	(0.038)	(0.045)	(0.034)	(0.041)
Effective abortion rate '98–'14	−0.192**	−0.194**	−0.149**	−0.174**	−0.131**	−0.142**	−0.141**	−0.156**
	(0.019)	(0.020)	(0.016)	(0.016)	(0.017)	(0.029)	(0.016)	(0.026)
ln(lagged prisoners per capita)		0.012		−0.108**		−0.132*		−0.157**
		(0.037)		(0.033)		(0.060)		(0.056)
ln(lagged police per capita)		−0.017		−0.028*		−0.098*		−0.143**
		(0.016)		(0.014)		(0.046)		(0.043)
Unemployment rate		−0.019		0.558		1.121		0.973
		(0.358)		(0.314)		(0.691)		(0.624)
Ln(Real per capita income) 1982–84		0.021		−0.088		0.523*		0.356
		(0.132)		(0.114)		(0.228)		(0.205)
Poverty rate		−0.002		−0.001		−0.004		−0.002
		(0.001)		(0.001)		(0.003)		(0.003)
Real AFDC generosity 1982–84 $		0.004		−0.001		−0.008		−0.002
		(0.003)		(0.003)		(0.008)		(0.007)
Shall-issue concealed weapons law		0.013		0.015		−0.023		−0.003
		(0.015)		(0.011)		(0.023)		(0.022)
Beer consumption per capita		0.084		0.027		0.266**		0.267**
		(0.050)		(0.043)		(0.101)		(0.090)

(Continued)

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Appendix C. (Continued)

Percent living in metropolitan statistical areas		0.001		−0.0002		−0.001		−0.003
		(0.001)		(0.001)		(0.002)		(0.002)
Population Black males 15–19		0.182		−0.172*		0.139		0.039
		(0.102)		(0.081)		(0.148)		(0.139)
Population Black males 20–39		−0.046		−0.067		0.123*		0.164**
		(0.050)		(0.036)		(0.059)		(0.055)
Population White males 15–19		−0.054		−0.090**		0.091		0.054
		(0.040)		(0.033)		(0.062)		(0.058)
Population White males 20–39		−0.019		0.007		−0.068**		−0.073**
		(0.017)		(0.009)		(0.020)		(0.019)
Population other race males 15–19		−0.001		−0.368*		−0.253		−0.243
		(0.193)		(0.157)		(0.239)		(0.228)
Population other race males 20–39		−0.006		0.022		−0.062		−0.068
		(0.050)		(0.053)		(0.086)		(0.082)
Year FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,530	1,517	1,530	1,517	1,530	1,517	1,512	1,499
R^2	0.815	0.852	0.974	0.981	0.853	0.865	0.877	0.889
Adjusted R^2	0.805	0.842	0.973	0.980	0.845	0.855	0.870	0.881

Note: $^{*}P<0.05$; $^{**}P<0.01$.

The dependent variable is the log in the per capita crime rate named at the top of each pair of columns. The first column in each pair presents results from the specifications in which the only additional covariates are state- and year-fixed effects. The second column presents results using the full specification. The data set is comprised of annual state-level observations (including the District of Columbia) for the period 1985–2014. State- and year-fixed effects are included in all specifications. The prison and police variables are once-lagged to minimize endogeneity. Real AFDC generosity per recipient family is lagged by 15 years, measured in thousands of dollars and indexed at 1982–84 values. Estimation is performed using a two-step procedure. In the first step, weighted least squares estimates are obtained, with weights determined by state population. In the second step, a panel data generalization of the Prais–Winsten correction for serial correlation developed by Bhargava et al. (1982) is implemented. Standard errors are in parentheses. Compared to Table 4 this model contains the following additional covariates: a variable reflecting the percentage of state population living in MSA's and six different age-race variables for whites, blacks, and other races for ages 15–19 and 20–39.

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Appendix D. The Relationship between Abortion Rates and Arrests, by Single Year of Age, 1985–2014

	Dependent Variable:					
	ln(Violent arrests)			ln(Property arrests)		
	(1)	(2)	(3)	(4)	(5)	(6)
Ab. Rate '85–'97 × Age = 15	0.0003	−0.056	−0.025	−0.042	−0.009	−0.018
	(0.011)	(0.014)**	(0.011)*	(0.010)**	(0.012)	(0.007)*
	[0.025]	[0.028]*	[0.020]	[0.028]	[0.024]	[0.011]
Ab. Rate '85–'97 × Age = 16	−0.013	−0.047	−0.015	−0.053	−0.014	−0.015
	(0.010)	(0.013)**	(0.009)	(0.009)**	(0.011)	(0.006)*
	[0.021]	[0.027]	[0.017]	[0.022]*	[0.022]	[0.010]
Ab. Rate '85–'97 × Age = 17	−0.032	−0.045	−0.007	−0.050	−0.007	0.004
	(0.009)**	(0.014)**	(0.008)	(0.008)**	(0.013)	(0.006)
	[0.016]*	[0.026]	[0.013]	[0.017]**	[0.025]	[0.010]
Ab. Rate '85–'97 × Age = 18	−0.063	−0.056	−0.022	−0.066	−0.017	−0.004
	(0.008)**	(0.011)**	(0.007)**	(0.008)**	(0.011)	(0.005)
	[0.013]**	[0.021]**	[0.008]**	[0.012]**	[0.018]	[0.006]
Ab. Rate '85–'97 × Age = 19	−0.062	−0.058	−0.020	−0.056	−0.025	−0.006
	(0.009)**	(0.012)**	(0.006)**	(0.008)**	(0.010)*	(0.004)
	[0.019]**	[0.023]*	[0.008]*	[0.013]**	[0.016]	[0.005]
Ab. Rate '85–'97 × Age = 20	−0.065	−0.059	−0.022	−0.043	−0.028	−0.007
	(0.012)**	(0.013)**	(0.007)**	(0.011)**	(0.013)*	(0.005)
	[0.028]*	[0.027]*	[0.011]*	[0.016]**	[0.019]	[0.006]
Ab. Rate '85–'97 × Age = 21	−0.055	−0.057	−0.025	−0.024	−0.031	−0.011
	(0.020)**	(0.018)**	(0.010)**	(0.014)	(0.017)	(0.007)
	[0.043]	[0.034]	[0.016]	[0.020]	[0.020]	[0.007]
Ab. Rate '85–'97 × Age = 22	−0.035	−0.043	−0.012	−0.019	−0.043	−0.016
	(0.024)	(0.019)*	(0.011)	(0.014)	(0.015)**	(0.007)*
	[0.053]	[0.037]	[0.019]	[0.018]	[0.015]**	[0.007]*
Ab. Rate '85–'97 × Age = 23	−0.032	−0.052	−0.024	−0.025	−0.063	−0.028
	(0.033)	(0.024)*	(0.014)	(0.014)	(0.014)**	(0.006)**
	[0.065]	[0.042]	[0.024]	[0.019]	[0.015]**	[0.008]**
Ab. Rate '85–'97 × Age = 24	−0.014	−0.040	−0.014	−0.045	−0.089	−0.041
	(0.043)	(0.026)	(0.013)	(0.016)**	(0.016)**	(0.010)**
	[0.076]	[0.044]	[0.027]	[0.022]*	[0.018]**	[0.011]**
Ab. Rate '98–'14 × Age = 15	0.001	−0.032	−0.026	−0.072	−0.040	−0.010
	(0.010)	(0.016)	(0.012)*	(0.010)**	(0.014)**	(0.011)
	[0.030]	[0.038]	[0.024]	[0.027]**	[0.030]	[0.026]
Ab. Rate '98–'14 × Age = 16	−0.015	−0.029	−0.027	−0.091	−0.049	−0.016
	(0.008)	(0.015)*	(0.010)**	(0.009)**	(0.012)**	(0.010)
	[0.025]	[0.033]	[0.020]	[0.023]**	[0.026]	[0.022]
Ab. Rate '98–'14 × Age = 17	−0.030	−0.039	−0.035	−0.087	−0.044	−0.004
	(0.008)**	(0.015)**	(0.009)**	(0.008)**	(0.012)**	(0.010)
	[0.024]	[0.031]	[0.020]	[0.021]**	[0.027]	[0.023]

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Appendix D. (Continued)

Ab. Rate '98–'14 × Age = 18	−0.058	−0.055	−0.056	−0.118	−0.065	−0.024
	(0.007)**	(0.011)**	(0.009)**	(0.007)**	(0.010)**	(0.008)**
	[0.019]**	[0.025]*	[0.020]**	[0.015]**	[0.017]**	[0.018]
Ab. Rate '98–'14 × Age = 19	−0.057	−0.056	−0.055	−0.102	−0.068	−0.023
	(0.007)**	(0.012)**	(0.008)**	(0.007)**	(0.009)**	(0.008)**
	[0.022]**	[0.026]*	[0.018]**	[0.015]**	[0.015]**	[0.016]
Ab. Rate '98–'14 × Age = 20	−0.053	−0.051	−0.053	−0.088	−0.073	−0.029
	(0.007)**	(0.012)**	(0.008)**	(0.007)**	(0.009)**	(0.008)**
	[0.026]*	[0.029]	[0.019]**	[0.015]**	[0.014]**	[0.017]
Ab. Rate '98–'14 × Age = 21	−0.055	−0.057	−0.063	−0.079	−0.085	−0.041
	(0.008)**	(0.012)**	(0.008)**	(0.008)**	(0.010)**	(0.008)**
	[0.031]	[0.027]*	[0.018]**	[0.016]**	[0.017]**	[0.020]*
Ab. Rate '98–'14 × Age = 22	−0.050	−0.054	−0.062	−0.075	−0.096	−0.050
	(0.008)**	(0.011)**	(0.008)**	(0.008)**	(0.009)**	(0.008)**
	[0.035]	[0.028]	[0.019]**	[0.016]**	[0.019]**	[0.020]*
Ab. Rate '98–'14 × Age = 23	−0.046	−0.053	−0.063	−0.074	−0.107	−0.059
	(0.009)**	(0.011)**	(0.009)**	(0.008)**	(0.009)**	(0.008)**
	[0.037]	[0.028]	[0.020]**	[0.017]**	[0.019]**	[0.019]**
Ab. Rate '98–'14 × Age = 24	−0.044	−0.046	−0.057	−0.076	−0.114	−0.065
	(0.010)**	(0.011)**	(0.009)**	(0.009)**	(0.010)**	(0.008)**
	[0.041]	[0.028]	[0.019]**	[0.017]**	[0.018]**	[0.017]**
Year * Age?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Implied	Implied	Yes	Implied	Implied
State * Age?	No	Yes	Yes	No	Yes	Yes
State * Year?	No	No	Yes	No	No	Yes
Observations	13,765	13,765	13,765	13,770	13,770	13,770
R^2	0.957	0.965	0.994	0.950	0.962	0.994
Adjusted R^2	0.955	0.963	0.992	0.949	0.960	0.993

Note: $^{*}P < 0.05$;$^{**}P < 0.01$.
This mimics the six regressions in the top panel of Table 5 while also generating for both time periods the individual abortion estimates by single year of age.

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Appendix E. The Relationship between Abortion Rates and Arrests, by Single Year of Age; Version of Table 5 Using 26 States with Complete Arrest Data, 1985–2014

	Dependent Variable:					
	ln(Violent arrests)			ln(Property arrests)		
	(1)	(2)	(3)	(4)	(5)	(6)
Abortion rate '85–'97 (×100)	−0.030	−0.040	−0.041	−0.036	−0.032	−0.033
	(0.008)**	(0.008)**	(0.008)**	(0.007)**	(0.007)**	(0.005)**
	[0.013]*	[0.020]*	[0.018]*	[0.013]**	[0.008]**	[0.011]**
Abortion rate '98–'14 (×100)	−0.044	−0.048	−0.055	−0.098	−0.097	−0.062
	(0.007)**	(0.007)**	(0.008)**	(0.006)**	(0.006)**	(0.009)**
	[0.028]	[0.031]	[0.013]**	[0.016]**	[0.016]**	[0.017]**
Abortion rate '85–'14 (×100)	−0.040	−0.046	−0.045	−0.083	−0.083	−0.041
	(0.006)**	(0.007)**	(0.006)**	(0.006)**	(0.006)**	(0.004)**
	[0.023]	[0.028]	[0.015]**	[0.015]**	[0.014]**	[0.010]**
Year * Age?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Implied	Implied	Yes	Implied	Implied
State * Age?	No	Yes	Yes	No	Yes	Yes
State * Year?	No	No	Yes	No	No	Yes
Observations	7,800	7,800	7,800	7,800	7,800	7,800

Note: $^{*}P<0.05$; $^{**}P<0.01$

Results in the table are coefficients from estimation of equation (3). The unit of observation in the regression is annual arrests by state by single year of age. The sample covers the period of 1985–2014 for ages 15–24, and the top panel of the table estimates the effect of abortion both for our initial period (1985–97) and for the remainder of our full data period (1998–2014). The bottom panel (the third row) estimates a single abortion variable model over the entire 1985–2014 time period. The abortion rate for a cohort of age a in state s in year y is the number of abortions per 1000 live births in state s in year $y-a-1$. Note that this is the actual abortion rate, rather than the "effective" abortion rate. A complete set of year-birth cohort interactions are included in all specifications to capture national changes in the shape of the age-crime profile over time. The table indicates the various fixed effects included in each column. Estimation is weighted least squares, with weights determined by total population by state-year-age. Standard errors clustered by cohort year of birth and state are included in parentheses; this accounts for correlation over time within a given birth cohort in a particular state. Such a correction is necessary because the abortion rate for any given cohort is fixed over time, but multiple observations corresponding to different years of age are included in the regression. Standard errors clustered by state are included in square brackets below the first set of clustered standard errors. This table includes only the states with no missing arrest data from 1985–2014, which means that we drop 25 states: AL, AR, CO, DC, DE, FL, HI, IA, IL, KS, KY, ME, MI, MN, MT, NH, NV, NY, OK, PA, RI, SC, SD, VT, and WI.

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Appendix F. Estimated Effects of Abortion on Crime with Measurement Error Adjustments; Version of Table 6 Using 26 States with Complete Arrest Data, 1985–2014

	Dependent Variable:					
	ln(Violent arrests)			ln(Property arrests)		
	(1)	(2)	(3)	(4)	(5)	(6)
IV Abortion effect '85–'97 (×100)	−0.058	−0.070	−0.116	−0.066	−0.070	−0.080
	(0.018)**	(0.019)**	(0.022)**	(0.018)**	(0.020)**	(0.012)**
	[0.025]*	[0.024]**	[0.040]**	[0.030]*	[0.031]*	[0.017]**
IV Abortion effect '98–'14 (×100)	−0.066	−0.057	−0.071	−0.174	−0.181	−0.020
	(0.013)**	(0.013)**	(0.022)**	(0.017)**	(0.020)**	(0.022)
	[0.040]	[0.042]	[0.042]	[0.061]**	[0.073]*	[0.027]
IV Abortion effect '85–'14 (×100)	−0.064	−0.060	−0.105	−0.142	−0.152	−0.066
	(0.013)**	(0.014)**	(0.018)**	(0.018)**	(0.021)**	(0.011)**
	[0.032]*	[0.033]	[0.039]**	[0.058]*	[0.073]*	[0.014]**
Year * Age?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Implied	Implied	Yes	Implied	Implied
State * Age?	No	Yes	Yes	No	Yes	Yes
State * Year?	No	No	Yes	No	No	Yes
Observations	7,800	7,800	7,800	7,800	7,800	7,800

Note: $^{*}P<0.05$; $^{**}P<0.01$
The 25 excluded states are listed in the notes to Appendix E.

Appendix G: Data

The following outlines the sources and data construction used for all variables in our dataset. For further details, please see our replication package.

Crime. Our violent crime, property crime, and murder data are obtained from the FBI Uniform Crime Reporting (UCR) Statistics at the state-year level for 1973–2014. We use the population from the FBI UCR data to generate crimes per 1,000 population for 1973–2014. Our other source of murder data is the National Vital Statistics System (VS). While the UCR crime data are derived from crimes reported to police agencies, VS homicide data are derived from death certificates and subject to mandatory reporting. We combine VS data from CDC WISQARS and CDC WONDER to generate the most complete VS data from 1973 to 2014 (due to missing values that exist in each dataset). We use population from the VS data to generate VS murders per 100,000 population. Note that the VS data exclude legal interventions, which are essentially equivalent to justifiable homicides committed by law

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enforcement officers. It does not exclude or identify justifiable homicides committed by private citizens. This violent crime, property crime, UCR murder, and VS murder rate data is used in Tables 1–4. From 1985–2014, we are not missing any crime rate data.

Abortion. The abortion data from our original 2001 paper was from the Alan Guttmacher Institute (AGI), aggregated by state the abortion occurred in. Following up on improvements to the data made in Donohue and Levitt (2004), our current AGI abortion data are aggregated by mother's state of residence. Note that in this article, we combine AGI abortion rate data from the AGI website and the Johnston Archive to create the most complete AGI abortion rate by residence. We take the AGI website data as our base and for cases where the AGI website data are missing but the Johnston Archive AGI data are non-missing, we use the Johnston Archive data. We use our live births data (see Live Births section below) to generate an abortion rate, which is the number of abortions per 1,000 live births. In Tables 1–5 and Appendices A–D, we exclusively use AGI abortion rates by residence. For Tables 6, 7, 8, Appendices E and F, we use AGI abortion rates by residence as our main abortion variable and CDC abortion rate by occurrence as our instrumental variable.

To generate our CDC abortions by occurrence data, we combine CDC data from the CDC Abortion Surveillance Reports (ASR) and the Johnston Archive. We take the CDC Abortion Surveillance Reports as the base and for cases where the ASR data are missing but the Johnston Archive CDC data are not missing, we use Johnston Archive CDC data. Note that for both our AGI and CDC data, we assume abortion rates to be zero prior to their abortion legalization effective date for early-legalizing states (California: September 5, 1969; Hawaii: March 11, 1970; New York: July 1, 1970; Alaska: July 29, 1970; and Washington: November 3, 1970) and to be zero prior to January 22, 1973 (*Roe v. Wade* decision) for all other states. We use a daily imputation method to impute missing values during the 1970–75 time period, where the AGI residence abortion data has many missing values. This imputation method uses the accurate effective passage dates for early-legalizers and the *Roe v. Wade* decision date for all other states. Following our daily imputation method for the 1970–75 time period, we use linear interpolation on the remainder of the 1976–2014 time period

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to generate the most complete AGI and CDC abortion data possible from 1970 to 2014. In addition, for CDC occurrence data in New Hampshire and California, we are either missing all years of data from 1998 to 2014 (for New Hampshire) or we have unrealistically low values (for California). We use linear extrapolation to fill in the CDC occurrence values from 1998 to 2014 for both these states.

Following all three imputation methods, we have 0 missing values for our AGI by residence abortion data and 16 missing values for our CDC by occurrence abortion data from 1970 to 2014. It is widely recognized that over the relevant data period for our study, estimates of abortions based on AGI's abortion provider survey were more complete than CDC measures. See Darroch et al. (2001), who find that "comparison of officially reported abortions in the United States with an independent survey of all known providers indicates that official statistics underreport abortions by approximately 13%." Moreover, only 23 states required abortion providers to report to the CDC information concerning residence of the abortion patient, which further undermined the quality of CDC abortion counts by state of residence (Saul 1998). Accordingly, we used AGI residence data as our core abortion measure and instrument with CDC abortion counts by state of occurrence.

Live Births. We use live births data from 1970 to 1994 from the NBER Vital Statistics Natality Birth Data, while from 1995 to 2014 we use live births from the CDC Wonder Natality data. We have no missing values for live births from 1970 to 2014.

Police. The total number of law enforcement employees is generated from the FBI UCR Police Employees Masterfile. We have no missing values for our log of lagged law enforcement employees per 1,000 population from 1985–2014. Note that we impute values in 2013 for Illinois, Indiana, Louisiana, New Mexico, Utah, and Ohio as these states have unrealistically large drops in police employee data due to under-reporting.

Incarceration. We use the total number of prisoners under the jurisdiction of state or federal correctional authorities from the National Prisoner Statistics Program of the Bureau of Justice Statistics. We have 13 missing values for our log of lagged prisoners per 1,000 population from 1985 to 2014.

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Population by State-Year. Our population data at the state-year level are derived from the U.S. Census Bureau's Annual Estimates of the Resident Population for the United States, Regions, States, and Puerto Rico: April 1, 2010 to July 1, 2015 (NST-EST2015-01). This population is used to weight the Table 4 regressions. We have no missing values from 1985 to 2014.

Population by State-Year-Age. We use the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program U.S. population by state-year-age, as recommended by Foote and Goetz (2008). This population is used to weight the Tables 5–8 and Appendices D–F regressions. We have no missing values from 1985 to 2014.

Poverty Rate. We use the published Census estimates for the proportion of the population earning less than the poverty line. Note that since we only have the number of individuals living under the poverty line in thousands, we do not calculate this poverty rate using our own population figures. The source data for 1980–2015 is rounded to the tenths place, so we report our data for 1979 to the tenths place as well. We have no missing values from 1985 to 2014.

Unemployment Rate. We use data on the number of employed and unemployed individuals, as well as the number of individuals in the labor force, from the Bureau of Labor Statistics. We generate the unemployment rate by dividing the number of unemployed individuals by the size of the labor force. We have no missing values from 1985-2014.

Income. Per capita personal income is defined as total personal income divided by mid-year population and is derived from the Bureau of Economic Analysis. We have no missing values from 1985 to 2014.

AFDC/TANF generosity. Note that TANF became effective (in place of AFDC) as soon as each state submitted a complete plan implementing TANF, but no later than July 1, 1997. For AFDC data from 1984–96, we use data from the Statistical Abstract of the United States; this gives us our lagged-15 AFDC data from 1999 to 2011. For TANF data from 1997 to 1999, we use TANF assistance and caseload data from the Office of Family Assistance. This gives us the lagged-15 TANF data from 2012 to 2014. By

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combining these sources we are able to construct a panel from 1985 to 2014. We have no missing values from 1985 to 2014.

Right-to-Carry Laws. We generate a dummy variable that is given the value of 0 when a RTC law is not in effect that year, a value of 1 when a RTC law is in effect that entire year, and a value equal to the portion of the year an RTC law is in effect otherwise. For a list of the dates of RTC adoption for each state, see Appendix Table A1 in Donohue et al. (2019). We have no missing values from 1985 to 2014.

Beer. Our measure of beer consumption per capita is generated with data from the National Institute on Alcohol Abuse and Alcoholism. We have no missing values from 1985 to 2014. Beer consumption per capita from this source is defined as the number of gallons of ethanol (from beer) consumed per capita.

Arrests. The numbers of violent and property arrests by state-year-age are generated using the FBI UCR Arrest Master File. The natural log of these counts are used as dependent variables in Tables 5–8 and Appendices D–F. We generate violent and property arrests per capita using the SEER population data; these are used as the dependent variables in Tables 7 and 8. In addition, we use the proportion of arrests by age and crime category from 1985 as our weights for the effective abortion rates seen in Tables 1–4, and Appendices A–C. We have 1,535 missing values at the state-year-age level for violent arrests and 1,530 missing values for property arrests, out of a total of 15,300 observations (for each crime type).

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