THE EXTENSIVE AND INTENSIVE IMPACTS OF CRIMINAL LAW ON THE INCIDENCE OF CRIME: EVIDENCE FROM STATUTORY-RAPE LAW EXPANSIONS AND TEENAGE PREGNANCIES*

MICHAEL D. FRAKES AND MATTHEW C. HARDING

We explore the deterrent impacts of statutory rape laws, drawing on substantial within-state variations in criminal statutes over time. Our analysis facilitates an evaluation of the impact of initially criminalizing previously permissible activities—a largely unexplored margin in the literature to date—and the subsequent impacts of sentence-severity and enforcement enhancements. Largely using live birth rates to proxy for the targeted behavior and employing a novel methodological framework designed to codify multi-faceted legal structures, we estimate behavioral responses to statutory-rape-law expansions that are consistent with a deterrence story. However, our results suggest that any such deterrence may arise only through the initial-criminalization and enforcement-enhancement channels. *JEL* Codes: K14, K42, I18. Word Count: 11,615

I. INTRODUCTION

Criminal law is generally justified under theories of retribution, deterrence, and incapacitation, the latter two of which endeavor to reduce the incidence of crime prospectively. That is, by criminalizing particular activities and threatening incarceration or other forms of punishment, the law may discourage actors from partaking in harmful activities in the first place—i.e., deterrence. Likewise, by incarcerating those who have committed crimes, the law may further curb the incidence of undesirable activities by precluding the ability of those with criminal proclivities from repeating their past infractions—i.e., incapacitation. To determine whether the law is achieving its stated goals and thus to illuminate our understanding of optimal criminal policy, it is critical that we test for the existence and degree of any such deterrent and incapacitative effects.

In this paper, we approach this empirical task through an evaluation of both the extensive and intensive margins of criminal law. That is, we first ask whether the behaviors targeted by the law decline upon the *initial* criminalization of such activities. This extensive margin to criminality has been virtually unexplored by the empirical literature to date. Next, with respect to the intensive impacts of criminality, we explore whether those behaviors fall even more upon a subsequent enhancement in (1) the severity of punishment implicated by such activities or (2) the degree to which criminal infractions are enforced. We explore these questions in the context of statutory rape laws—i.e., laws prohibiting sexual encounters with

partners below particular ages. Over the years, states have sought to enhance the public health benefits of statutory rape laws by expanding the law along each of these three margins. As such, though focusing on a specific criminal context, this statutory rape analysis provides a unique opportunity to explore the full range of channels by which criminal law may impact behaviors. Our empirical analysis suggests that the sexual behaviors of interest may only fall when states initially criminalize a new range of sexual activity and when they ratchet up enforcement efforts.   Potential offenders do not appear sensitive to sentence-severity enhancements.

This research sheds novel light on a substantial body of literature in economics and criminology that has likewise explored the fundamental questions posed above. Scholars to date have almost exclusively focused on the intensive margins of criminality, with the first major strand of this literature estimating the outcomes of sentence enhancements and other expansions in the severity of punishment associated with particular activities, taking as given the fundamental criminality of those activities in the first instance.[1] Such studies have presented a mixed set of findings, inciting a spirited debate regarding the role of punishment scope in tempering criminal behavior. On conceptual grounds, at least with respect to the

---

[1] For example, scholars have evaluated the impacts of punishment enhancements such as capital punishment (Ehrlich 1975; Ehrlich 1977), add-on gun laws (Abrams 2012), three-strikes laws (Helland and Tabarrok 2007), and discontinuous changes in prison length upon the age of majority (Levitt 1998; Lee and McCrary 2009), among many other approaches. For recent surveys on these studies, see Durlauf and Nagin (2010) and Rupp (2008). These surveys generally contend that enhancements in the certainty of punishment are more effective at deterring crime than enhancements in the severity of punishment.

deterrence component, criminologists have acknowledged that individuals may be aware of and sensitive to the fact of the overall criminalization of a particular activity, but they may not perceive greater personal costs in association with stronger potential punishment. Rather, they propose that individuals may simply perceive a fixed cost of being convicted in and of itself, perhaps attributable to the stigmatization of criminalization and incarceration (Nagin 1988).

The second major branch of this literature has focused on evaluating the second intensive dimension to the law—i.e., observing criminal responsiveness to variations in the certainty by which punishment is imposed. Such variations are often captured through fluctuations in either incarceration rates (Levitt 1996) or police manpower rates (Levitt 1997; McCrary 2002).[2] Ultimately, economists and criminologists appear more confident that deterrent forces may be associated with an increase in the probability of detection (Durlauf and Nagin 2010).

In order to understand the full deterrent and incapacitative impacts of our criminal justice system, one may naturally wish to evaluate a counterfactual of *no criminalization*—i.e., a situation in which there is no system of criminal law in place to discourage the undertaking of certain activities. To be sure, zero enforcement of the law may be akin to no relevant criminal system at all. To our knowledge, however, the variations embraced by the certainty-focused studies

---

[2] Both studies and surveys bearing on punishment certainty enhancements are too numerous to list here. For an example of two recent surveys, see Durlauf and Nagin (2010) and Rupp (2008).

(e.g., police force fluctuations) have largely not drawn upon this extreme. Extrapolating from the marginal variations that we do observe along this certainty dimension may lead us to *understate* the full impact of criminal law, especially in the deterrent context. Analogous to the fixed stigmatization discussion above and consistent with behavioral theories of the "possibility effect" (Kahneman 2012), what may register most with individuals is the knowledge of *some possibility* of incarceration, even if slight, more so than subsequent fluctuations in the incarceration probabilities away from zero.[3] To gain insights into broader systemic impacts that cannot be fully gleaned from these marginal certainty-enhancement studies—i.e., to come closer to constructing a no-criminalization counterfactual—one might consider instead estimating changes in the frequency of an activity upon the criminalization of that activity in the first instance.

It is of course relatively rare to find legal variation along this extensive margin.[4] Murder will always be criminalized after all, as will robbery and many other types

---

[3] Supporting this possibility is research suggesting that individuals may overestimate low-probability events and underestimate high-probability events. Kahneman (2012) label this a "possibility effect," whereby individuals are more responsive to probability changes close to zero than they are at higher probabilities. This effect may be consistent with predictions from prospect theory, in which the value function that people place on losses is convex and steep (Kahneman and Tversky 1979).

[4] Those studies that have explored this dimension to the law have done so in arguably less serious contexts (and in contexts that are at least in colloquial terms less "criminal"). Many representative studies have focused on alcohol consumption—e.g., alcohol consumption and prohibition (Dills and Miron 2004; Dills, Jacobson and Miron 2005), alcohol consumption and the minimum legal drinking age (Carpenter and Dobkin 2009), and alcohol consumption and Sunday blue laws (Lovenheim and Steefel 2011). See Cook and Moore (2002) for additional evidence on alcohol-control measures and its consequences.

of crime.[5] The age-specific nature of statutory rape laws, however, affords us the ability to draw upon legal variations of this nature and thus to explore the impact of criminalizing activities that were previously permissible. While jurisdictions do not vary over time in the overall incidence of statutory rape legislation, a large number of jurisdictions have altered the age-related triggers of their statutory rape laws at various points in time—e.g., to expand the age of consent from 14 years old to 16 years old (as Georgia did in 1996).

Using those age-groups that are not impacted by a state's particular reform as a within-state control-group, we estimate triple-differences specifications bearing on the relationship between the scope and severity of statutory rape laws and the incidence of teenage sexual activity. To proxy for such activity, we use live birth rates for teenage mothers (with an appropriate backdating of ages and years to reflect gestation length). On the one hand, this proxy allows us to explore the more general link between such laws and sexual behaviors among the targeted age groups.[6] On the other hand, this specific relationship is of direct policy relevance itself, as teenage pregnancies have generally received considerable attention by the public health community.[7] Moreover, with congressional encouragement

[5] Further complicating the ability to study expansions in the scope of criminality of this nature are data limitations. Data on the incidence of particular activities that become criminalized at a particular point of time may only be collected and thus readily available during times following the criminalization of the activity itself.

[6] To shed richer light on the mechanism behind a sexual-activity response to statutory rape laws, we also consider the impact of such laws on abortion rates, as discussed further below.

[7] Whether or not teen pregnancy constitutes a "public health problem" is a question that has been much debated by the epidemiological and public health community. On the one side are those who question whether the age of teenage mothers

following the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), states have even been explicit at times in expanding statutory rape policies in order to reduce births by teenage mothers and thus reduce their associated welfare rolls (Sutherland 2003; Donovan 1996).[8]

Consistent with the criminologists' predictions indicated above, our results provide robust evidence suggesting that live birth rates within a particular age group decline upon an increase in the degree to which sexual activity among that age group is initially criminalized; however, such birth rates do not appear to fall further upon the subsequent increase in the degree of punishment. Moreover, we find that the estimated impact of expansions in the reach of statutory rape laws into previously permissible activities is larger in instances when we would predict that such laws would be more heavily enforced—e.g., for the youngest age groups—and when we observe markers actually suggesting stronger associated enforcement. Considering the magnitude of the findings relative to the degree of enforcement of statutory rape laws and considering the fact that the reduction in live birth rates is only associated with movements into criminality as opposed to increases in punishment lengths, it is arguable that the estimated findings are more reflective of a deterrent rather than an incapacitative impact.

---

is causally related to the negative health outcomes widely associated with teenage pregnancy – e.g., low birthweight, prematurity, infant and perinatal mortality, and cognitive deficiencies – or whether these outcomes are themselves more directly the result of other related factors – e.g., socioeconomic status (Lawlor and Shaw 2002). Others acknowledge the potential limitations of any causal medical story but nonetheless emphasize the greater economic and educational consequences that may arise from teenage pregnancies (Scally 2002).

[8] Cook et. al. (1999) provide evidence on state policies can affect pregnancy outcomes even in the short run.

Finally, we discuss and explore the mechanisms underlying the documented decline in live births following expansions in statutory rape laws. A question arises as to whether such laws truly deter the underlying sexual encounters or whether they compel other activities—e.g., additional abortions—that may explain the observed decline in births. While existing data limit our ability to explore all such alternative mechanisms (e.g., greater use of contraception), we do rule out that such findings can be explained by an increase in abortions.

Despite offering the ability to explore novel margins of criminality, an evaluation of statutory rape law expansions presents various methodological difficulties. Such laws are multi-faceted along several dimensions, confounding the ability to codify the relevant laws and draw comparable information from the various expansions in statutory rape policies undertaken by different states. To confront this task, we develop a simulation approach inspired by Frakes and Harding (2009), in which we use micro-level sexual-encounter data to generate various measures of the propensity of given states to criminalize sexual activity among given age groups. This simulation approach allows us to place the idiosyncracies of the state-year-age laws and reforms of relevance into a universal, and empirically-informed, framework, facilitating comparability of the various reforms.

A final contribution of our analysis concerns both the nature of the outcome measure embraced—live birth rates (and abortion rates)—and the data source

underlying this measure. In addition to illuminating certain public-health effects of criminal law (an area of increasing interest to public health scholars), this measure is necessarily robust to certain concerns that generally confront criminal deterrence studies—that is, crime-rate outcomes that suffer from underreporting[9] or jurisdiction manipulation of crime reports—e.g., through downgrading of some offenses to lesser crimes to boost appearances (Eterno and Silverman 2012). In the present study, the use of Natality records provides us with an activity rate of interest that is necessarily comprehensive and arguably non-manipulable.

The paper proceeds as follows. In Section II, we provide a background on statutory rape laws and on their hypothesized impact on teenage sexual activity. In Section III, we discuss the data and the methodology employed to evaluate expansions in both the reach and the severity of statutory rape laws. In Section IV, we present the results of this analysis. Finally, In Section V, we conclude.

II. BACKGROUND

Generally speaking, statutory rape laws implicate criminal liability for the elder member of particular types of sexual encounters depending on the age combinations of the relevant parties. Age restrictions are generally divided into two branches: (1) specified ages of consent and (2) accompanying age-gap

---

[9] Underreporting of crime rates, for instance, may lead to a negative bias in many criminal deterrence contexts, insofar as the error often appears on both the left-hand side and right-hand sides (i.e., appearing in the arrest-rate regressor of interest) of the specification (Levitt 1998).

triggers. For example, criminal liability may be triggered for an offender (i.e., generally, the elder member) to an encounter if the victim is under 16 and the offender is at least 4 years older. Moreover, different types of sexual encounters will trigger different degrees of potential punishment. For instance, an encounter between a 14 year-old and an 18 year-old may trigger a Class C felony statute of a particular jurisdiction while an encounter between a 14 year-old and a 22 year-old may trigger a Class B felony statute of the same jurisdiction.

The threat of criminal liability posed by the presence of a statutory rape law (and by the extent to which it is enforced) may deter potential offenders from deciding to engage in sexual intercourse with partners of prohibited ages. Of course, any such deterrent effect presumes that the elder individual is aware of the law and is responsive to this threat.[10] Even if such deterrent forces are strong enough to influence the decisions of the potential offenders, the net effect on sexual activity among the teenage population is ambiguous, itself depending upon the nature of the matching process between sexual partners and thus on the victim's decision-making process (Henry and Cunningham 2010). For instance, consider a given 15-year-old female whose desired match is a 20-year-old male. Assume that the threat of liability to the male, however, prevents this match from occurring. If the female nonetheless continues searching until she finds another

---

[10] Deterrent forces aside, the law may nonetheless have an impact on the distribution of sexual activity through an incapacitation effect to the extent that the imprisonment of offenders renders them unable to perform similar acts in future periods.

match (e.g., with a younger male), she will nonetheless continue to engage in sexual activity. Of course, to the extent that she does not desire the alternative matches to a degree sufficient to expose herself to the added risks, the deterred match with the 20-year old may indeed lead to a reduction in her sexual activity and thus in the likelihood of a negative health outcome. Moreover, given the possibility that older partners are more persuasive in encouraging sexual relations, it may be less likely that the 15 year-old will find an alternative match.

Certain studies have offered insights into the potential impact of statutory rape laws on teenage sexual behavior by exploring the distribution of age differences commonly observed between sexual partners. In one such study, Elo, King and Furstenberg (1999) use data on characteristics of live births to estimate a mean age difference between teenage mothers and associated fathers of over 3 years. Further, turning to data on first-time sexual encounters, they find that roughly half of those who reported initiating sexual activity under the age of 15 did so with a partner who was at least 4 years older. Such studies end the inquiry too soon, however, failing to capture the impact of the law in practice and failing to account for the possibility of blunted deterrence channels.

Attempting to overcome these deficiencies, two additional studies have estimated the observed impact of variations in state statutory rape laws on teenage pregnancy rates and sexual activity. First, Jepsen and Jepsen (2006) estimate a negative association between the presence of a statutory rape law and birthrates

for white females using an individual sample of teenage females from the June fertility supplements to the Current Population Survey. The authors codify state laws by assigning dummy variables to each individual female in the sample to the extent that they are protected by a rape statute if they engage in sexual intercourse with a hypothetical offender of a given adult age (e.g., 21 years old). They impose alternative hypothetical offender ages across separate specifications.

While this multiple-specification approach attempts to account for variations in age-gap structures over time and across jurisdictions, any given specification fails to capture the full and true extent of the variation in state laws, limiting the power of this exercise. For example, assume a state expands the reach of its statutes to implicate all sexual activity among 14 year-olds, where it previously allowed sexual activity among this age group if the offender was within 4 years of age. In this instance—which is characteristic of many reforms—an empirical specification that assigns binary statutory rape law indicators according to a hypothetical offender age of 20 years old would register no variation in the law despite a potentially impactful change.

Henry and Cunningham (2010) similarly estimate that an expansion of statutory rape laws is associated with an increased delay in sexual debuts. The authors define state laws by assigning each teen respondent an index variable equal to the span between the respondent's age and the age of a potential partner over which sexual intercourse is permitted—e.g., an index value of 9 is assigned if a 14 year

old is permitted to have intercourse with partners up to 23 years old. Henry and Cunningham's specification builds on Jepsen and Jepsen's analysis by providing for variations in the possible formulations of state laws in a single specification. However, it does so in a relatively parametric and linear fashion. In other words, it implicitly treats the following reforms as having a potentially equal impact on sexual behavior: (1) an expansion to preclude a 22 year-old from having sex with a 14 year-old where the law previously did not trigger criminality until the older party reached 23 years of age and (2) an expansion to preclude an 18 year-old from having sex with a 14-year-old where the law previously did not trigger criminality until the older party reached 19 years of age. However, considering the far greater likelihood that 14 year-olds are engaging in sexual encounters with 18 and 19 year-olds than with 22 and 23 year- olds, the former reform is likely to represent a larger effective expansion in the scope of the law. The methodology considered below expands on this framework by specifying a more natural legal variable that captures the breadth of sexual activity permitted or not permitted for particular ages—i.e., the scope of the law—in a manner consistent with the empirical relevancy of the possible offender-victim age combinations.

Our analysis builds on both Jepsen and Jepsen (2006) and Henry and Cunningham (2010) in several additional ways. To begin, our simulation approach facilitates the subsequent evaluation of the impacts of severity enhancements, which were omitted from their analyses and which are difficult to

13

codify through conventional methods (as discussed below). Further, we build on

Jepsen and Jepsen by capturing a far greater time horizon of legal variables and

by accounting for state fixed effects. We likewise draw upon a greater range of

legal variation than Henry and Cunningham[11] and expand upon their analysis by

estimating triple-differences specifications that take advantage of the age-specific

expansions of the law to account for the possibility of contemporaneous state-year

initiatives that likewise target teenage sexual activity.[12]

III. DATA AND METHODOLOGY

III.A. Data

Live Birth Rates—In order to explore whether sexual activity within particular

age groups begins to decline as we criminalize a greater range of sexual activity

involving those ages, we require tracking age-specific annual changes in sexual

activity over a long period of time (to capture reforms from the early 1980s to the

early 2000s) and across all states. Unfortunately, there is no comprehensive data-

---

[11] Henry and Cunningham use the 1979 and 1997 arms of the National Longitudinal Survey of Youth (NLSY), which ask for information on first-time sexual encounters. These data sources effectively prohibit them from taking advantage of the variation in the law in the 1980's. Moreover, their ability to evaluate the rich legal expansions in the mid-1990s is limited. The NLSY in 1997 surveyed a group of 12 to 17 year olds. As such, in evaluating the sexual behavior of youths in 1992 (i.e., querying whether they had commenced sexual relations yet), they are only able to observe the behaviors of those 12 and under at that time (representing responses provided by the 12-17 year olds in 1997). This limits the ability to (A) form within state control groups based on older ages at that time and (B) capture variation in the law at that time that targeted older groups. Similarly, in the years following 1997, they are hamstrung in their ability to collect sexual activity data on the younger age groups. For instance, in 1999, they would be unable to collect information on anyone under 14 years of age, considering that the youngest within the longitudinal survey are 14 years old at that time. Ultimately, their approach does not afford rich information on sexual practices at a consistent age-specific level over our time periods of interest.

[12] Likewise, these prior studies fail to distinguish between legislative variations representing expansions of the law from those representing contractions, a distinction that may bear on possible attenuation of the results, as discussed below.

14

source of this nature providing information on the full breadth of sexual activity. However, comprehensive data is available on the incidence of pregnancies among the relevant populations, a measure which may be used to proxy for general sexual activity. Of course, considering that statutory rape laws are often designed with the purpose of curbing teenage pregnancies, such rates are more than mere proxies. They can also be seen as our outcome of interest.

Pregnancies will generally end in one of miscarriage, abortion, still-birth or live birth. Data is unavailable at the required level for all such possibilities; however, rich data is indeed available on live births. As such, to proxy for pregnancy rates more generally, we collect data on the number of live births associated with each state-year-age cell from the Natality Detail files of the National Vital Statistics System.[13] Geographic identifiers are not available with the Natality Detail records following 2004 and thus we end our sample period in that year. In calculating such rates, we count the number of live births born to mothers in each age group between 12 and 17 years old and in each state and year and subsequently divide these counts by the corresponding population.[14] Of course, our interest is in exploring how statutory rape laws impact the fact of pregnancy itself. As such, the relevant time period for this analysis is the time of conception.

---

[13] We have collected some limited data at the state-year-age level on the number of abortions reported. This data was collected by the Centers for Disease Control. In the Online Appendix, we present results of specifications using these alternative measures.

[14] State-year-age population data are from the U.S. Census Bureau and were collected from the National Cancer Institute, a division of the U.S. National Institutes of Health, who compiled these figures for use in calculating cancer and mortality rates.

Accordingly, in allocating live births to state-year-age cells, we use information provided in the Natality records bearing on the length of gestation to estimate the year of conception and the age of the mother at the time of conception.[15]

Statutory Rape Laws—We track the evolution of each state's statutory rape laws from 1982 to the present, taking care to observe whether criminal liability is triggered for each potential victim-age / offender-age match-up.[16] We also track any variation over time in the nature of the associated punishment provisions. This research documents a substantial amount of within-state variation over this time period. With respect to the initial criminalization dimension, roughly 27 states experienced some change in their laws that altered the distribution of sexual activity (for at least one age) that triggers a felony in the first instance. The nature of the variation in 17 of these states represented an expansion of criminality into previously permissible activities.[17] For instance, in 1996, Georgia elevated its age of consent from 14 years old to 16 years old. North Carolina increased its age of consent from 13 to 16 in 1995. Numerous other examples abound. The Online

[15] After correcting, as stated above, for the estimated time of conception, the average state-year cell in the sample contains roughly 26 live births born to mothers age 12 years old, 123 born to mothers 13 years old, 396 born to mothers 14 years old, 895 born to mothers 15 years old, 1569 born to mothers 16 years old and 2342 born to mothers 17 years old. Live births are extremely rare in the case of mothers 11 years of age and younger. As such, we focus our empirical analysis on the 12-17 year-old age range. However, nearly unchanged results are generated by specifications that nonetheless include 11 year old mothers (available upon request).

[16] To track these changes, we observed various sources including current statutory provisions, superceded statute archives, and session law archives. To verify our understanding of the results of such searches, we frequently reviewed case law from the relevant time periods referencing the relevant laws. While our initial goal was to form a 20-year sample and go back to 1985, we observed a number of states that expanded the scope of their laws in 1985 and thus sought to push the sample back a few more years to take advantage of these variations. Our ability to comprehensively and reliably track the evolution of the relevant statutes became exceedingly more difficult the further back in time our research took us, given certain limitations in the legal data archives. As such, we halted our efforts in the early 1980s.

[17] The remaining 10 states reduced the scope of their laws such that some activities became permissible which had previously triggered felonies.

Appendix provides more information on the nature of these statutory variations. When also incorporating the punishment severity dimension, the vast majority of states experienced some change in their relevant laws over the sample period.

Statutory rape laws take on different forms in different jurisdictions, complicating the ability to place all relevant information in comparable terms and to estimate single specifications that draw upon the experiences of multiple treatment states. However, such differences at least collapse to a manageable number of dimensions: (1) minimum ages of consent, (2) the extent of any gap between the offender and the victim before a violation is triggered, (3) the number of degrees of statutory rape that a state imposes on different offender-victim age combinations and (4) the severity of punishment associated with each statutory degree. The commonalities that nonetheless remain in the structure of statutory rape policies provide us with enough traction to be able to codify and parameterize these laws while still accounting for structural differences.

Felony/Initial Criminalization Specifications—To codify and define the relevant statutory rape laws, we first aim to simulate a measure that effectively captures that portion of the distribution of sexual activity among specific age groups that is triggered by a statutory rape law. The higher the percentage of sexual activity for a particular age group that implicates criminal liability for the relevant offender, the more expansive is the specified statutory rape law variable and thus the more likely it may be that the law will curb the incidence of

pregnancy. Relative to an approach that simply specifies a binary variable capturing the existence of a statutory rape provision, this specification allows us to take advantage of more variation in the law—e.g., that arising from a reduction in the specified age gap. It also allows us to better codify the variation we do observe—e.g., to appropriately capture the difference between a state that previously permitted all sexual activity with a 15 year old and that now criminalizes all such activity and a state that likewise newly criminalizes sexual activity with 15 year olds but that nonetheless retains a zone of permissibility if the offender is within three years of age. Presumably, the former state's expansion represents a larger expansion of the law than the latter's.

This approach uses the age distribution of actual sexual encounters to simulate the scope of the laws, thereby assigning different values to the alternative formulations of the law in accordance with their empirical relevancy. For instance, if sex between a 15- and 24-year old is very rare, then the law prohibiting activity only with offenders in the 24-plus range will be given accordingly little weight in a specification that attempts to explain pregnancy rates among all 15 year-olds.

To obtain a joint offender-victim age distribution for a set of sexual encounters with minors, as required by this simulation approach, we draw on data from the National Survey of Family Growth (NSFG). The NSFG surveys a female population on information regarding their first-time sexual encounters, providing

details on the ages of both the teen/minor female and her partner in the encounter. While some respondents may be recounting information from a first sexual encounter that occurred years prior, we take an approach similar to that of Elo, King and Furstenberg (1999) and simply view this data as a time-invariant sample of first-time sexual encounters themselves. Despite the possibility that the distribution of the ages of partners of subsequent encounters differ from those of this initial encounter, we can treat this data as a universal proxy for teenage sexual activity—a proxy that is neither year- nor state-specific. With a general sense of what teenage sexual matches look like—e.g., with a sense of how many 15 year olds are engaging in sexual activity with 19 year olds—it is possible to simulate the effective reach of the laws.

More specifically, to form the simulated measures, we begin by taking each observation among the relevant age group in the NSFG and flagging whether the encounter triggers a felony under the laws of the given state and year. We then collapse this information such that we are left with a set of simulated measures of the propensity of the given state and year to extend felony criminal liability to sexual activity within certain age classes—i.e., the percentage of sexual activity among the relevant age group that triggers a felony. This percentage measure serves as the relevant legal measure of interest in the primary specifications.[18]

---

[18] This methodology was recently employed by the authors in an evaluation of the deterrent effect of capital punishment eligibility (Frakes and Harding 2009). Representing one of the pioneering applications of this approach, Currie

To simulate an arguably exogenous legal measure, it is preferable to use data on sexual encounters that abstract away from state-specific unobservable factors and that are thus unaffected by the state's laws (Currie and Gruber 1996). For these reasons, we use the national, non-state-specific NSFG records as the baseline for the simulation. As such, the exercise does not necessarily codify the scope of the law by asking what percentage of sexual encounters actually observed within the state trigger a felony within that state. Rather, to focus on capturing the *pure* structure of law itself, we determine the extent to which the relevant state-year-age law criminalizes sexual encounters within a more universal setting.

Punishment Severity Specifications—States often vary their laws not just by moving certain activities into and out of felony status, but also by expanding the potential severity of punishment associated with a given infraction, either by increasing the relevant minimum or the maximum punishment level. The above simulation framework also provides a manageable way to account for variations along the punishment severity dimension. The need for an approach of this nature is perhaps more paramount here. Consider for instance a state that modifies its law by doubling its minimum punishment level from 1 to 2 years for a statutory rape infraction across the board. Now compare this state with another that modifies its law by doubling its minimum punishment level from 1 to 2 years only

and Gruber (1996) draw on within-state changes in Medicaid eligibility rules over time to estimate the effect of Medicaid eligibility on healthcare utilization and outcomes. To abstract from individual- and state-specific factors that may be correlated with both utilization and eligibility propensities, they instrument individual eligibility with simulated measures of the percentage of children in national samples (within age groups) that are eligible for Medicaid based on the prevailing eligibility rules for the relevant state-year-age group.

in the case of a second degree statutory-rape violation, which is triggered only in the relatively rare instance when a person under 14 has sexual relations with someone 24 years of age and older.[19] The former reform is likely to have a broader impact on sexual activity.   By using a sample of sexual encounters to simulate the expected levels of punishment generally implicated by sexual activity among certain age groups, the empirical framework embraced below facilitates comparisons of "apples-to-apples" in capturing the severity of the law.

In our initial approach to codifying severity enhancements, we designate two generic tiers of criminal punishment: (1) the first tier representing the triggering of a basic felony for the associated encounter (i.e., incarceration of greater than one year, with a minimum of less than 10 years) and (2) the second tier implicating a heightened felony (i.e., a minimum incarceration of at least 10 years).   For each state-year-age cell, we then simulate the percentage of sexual activity among the relevant age groups that triggers liability within each of the above tiers according to the laws of the relevant state and year.   In yet other specifications, we capture severity expansions by simulating the expected minimum or mean incarceration levels associated with a given sexual encounter among the various age groups. Moreover, in the Online Appendix, we break the non-parametric approach down into three and four different punishment tiers.

---

[19] Limited sentence enhancements of this nature are common in the documented statutory rape law variations.

Descriptive Statistics—In Table I, we provide summary statistics for each of the key variables used in the empirical analysis below, both overall and by age. We document that the typical teenage sexual encounter will trigger a statutory rape felony a significant proportion of the time—47 percent of the time to be precise. Keeping in mind the general structure of many statutory rape statutes, this 47 percent finding is consistent with Elo, King and Furstenberg's (1999) own finding of a 3-4 year mean age difference between teenage mothers and the associated fathers and between the age of a girl during her first sexual encounter and the age of her male partner. In other words, given the distribution of ages between sexual partners actually observed, the law at least has the potential to affect a significant number of sexual encounters. As presented in Table I, this simulated felony incidence percentage falls monotonically from an age-specific high of 82 percent for 12 year olds to 12 percent for 17 year olds.

This 47-percent figure varies considerably over the sample, both across states and within-states over time. Thirty-two percent of the felony-incidence variation is of the latter variety, reflecting a large amount of inter-temporal legislative changes within states. Take Mississippi for example. The average encounter among 12—17 year olds triggered a felony 12 percent of the time at the beginning of the sample period, a figure that rose to over 50 percent by the end of the sample period as a result of legislative expansions in the mid-1980s and late 1990s.

The incidence of heightened felonies emerges only in the lower age groups, with 12 percent of sexual encounters involving 12 year olds triggering a felony carrying a minimum sentence of 10 years. A given sexual encounter involving 12—17 year-old girls will expect to trigger a crime carrying a minimum of 1.3 years of imprisonment (ranging from 3.7 years for encounters with 12 year olds to 0.1 years for encounters with 17 year olds) and a mean (over the possible incarceration range) of 5.7 years of imprisonment (ranging from 13.5 years for encounters with 12 year olds to 0.95 years for encounters with 17 year olds). As such, a given encounter among the youngest age groups is likely to carry more significant consequences. Moreover, in an arena characterized by significant prosecutorial and police discretion, such younger encounters are also more likely to prompt enforcement activity. With this latter idea in mind, we test below for heterogeneous impacts of the law by age, predicting that expansions in the reach of the law will likely be more impactful at younger ages.

III.B. Empirical Specifications

To estimate the relationship between teenage pregnancy rates and expansions in the scope and severity of statutory rape laws, we embrace a difference-in-difference-in-difference (DDD or triple-differences) methodology (Gruber 1994). States modified their laws in a staggered fashion over the course of the sample. As a first layer of control, we observe how pregnancy rates change in connection

with expansions in statutory rape laws, comparing these changes with the associated changes over the corresponding time period within non-modifying states. However, we also take advantage of the age distinctions in the law. When amending their statutory rape laws at a particular point in time, states will generally do so with respect to only one age group, or a subset of age groups. This affords us the opportunity to create another dimension of differentiation through the use of within-state control groups.[20] Nonetheless, we begin the analysis by estimating more basic difference-in-difference (DD) specifications that do not include within-state controls.

By facilitating the inclusion of state-by-year fixed effects, the DDD specification allows us to account for unobservable shocks in particular state-year cells. Among other things, this allows us to control for the influence of unobservable policies enacted by states over the sample period that likewise have a bearing on teenage pregnancy rates—e.g., sexual education reforms, sex offender registration and notification laws, etc. Under this DDD approach, the relationship between pregnancy rates and statutory rape laws is identified under an assumption that there are no unobservable state-year-age factors (e.g., teenage pregnancy-related policies that target specific ages) that are also correlated with

---

[20] For instance, if a state amends its treatment of intercourse with 13 and 14 year-olds but not its treatment of the remaining age groups, then the 12-, 15-, 16- and 17-year old age groups within that state may also be employed as control groups for the empirical analysis.

the state-year-age statutory rape laws and associated pregnancy rates and that may otherwise be responsible for the estimated findings.

More specifically, we estimate the following specification:

$$(1) \qquad Ln(Birth\_Rate_{sta}) = \alpha + \gamma_s + \lambda_t + \mu_a + \tau_{st} + \delta_{sa} + \rho_{ta} +$$

$$\beta_1 Law_{sta} + \beta_2 RACE_{sta} + \varepsilon_{sta}$$

where *Birth_Rates$_{sta}$* are the state-year-age specific live birth rates, calculated as above.[21] State fixed effects, γ_s, year fixed effects, λ_t, and age fixed effects, μ_a, control for fixed differences across states, years and specific age groups, respectively. The inclusion of τ_{st}, δ_{sa}, $\rho_{t,a}$ provides for fixed differences across different state-year groups, state-age groups and year-age groups respectively. The percentage of the state-year-age female population that is black is captured by *RACE$_{sta}$*. The various alternative specifications of the statutory rape law variable discussed above are captured by **Law$_{sta}$**. The coefficient of interest is represented by β_1, reflecting the relationship between live birth rates (which proxy for pregnancy rates and for sexual activity more generally) and expansions in the scope or severity of statutory rape laws. Negative values of this coefficient are suggestive of a deterrent and/or incapacitative effect of statutory rape provisions.

---

[21] Using the natural log of the birthrate as the dependent variable would be problematic in the presence of a significant number of zeroes in live birth counts at the state-year-age level. However, out of nearly 7000 cells, only 23 contained zeroes. The results presented below are virtually identical when simply dropping those zero-valued cells through the log transformation of the birth rate or when imposing one live birth in each such cell (the approach taken in the analysis below). Moreover, the general results presented below are robust to the alternative estimation of Poisson specifications that deal with zero values more directly, as discussed below and as discussed more thoroughly in the Online Appendix.

This DDD specification implicitly expects age-specific responses to certain statutory rape law modifications. With respect to the incapacitation channel, if repeat offenders of statutory rape violations are inclined to target the same age groups again, then an age-specific reform could indeed be expected to result in an age-specific decline in sexual activity. With respect to the deterrence channel, the DDD specification requires certain assumptions regarding potential offenders' awareness of the changing legal environment. For instance, consider a reform that targets only one age group. To result in an age-specific deterrent response, potential offenders must arguably be made aware that the laws regarding the targeted age group were altered while those regarding the other ages were not. Of course, it could be the case that some potential offenders only target partners of particular ages, in which case all that may be required to observe a deterrent response is that those offenders become aware of changing legal expectations for the types of sexual encounters they are otherwise interested in pursuing.

Law awareness questions of this nature arise frequently in deterrence contexts. At least in comparison with other types of serious crime (e.g., murder, robbery, etc.), statutory rape laws implicate a general activity in which a substantial percentage of the population partakes: the matching process of finding a sexual partner. It is perhaps plausible to believe that at least a portion of this population may be aware of relevant changes in this legal landscape—e.g., an increase in the age of consent. Sutherland (2003) supports this general law-awareness

expectation in the statutory rape context through a number of observations including the long-term maintenance of the slang-term "jailbait." In any event, the results below are nearly identical when estimating more basic difference-in-difference specifications, which require weaker knowledge-related assumptions.

IV. RESULTS

IV.A. *Felony / Initial Criminalization Specifications*

In Table II, we present results of the felony-incidence specifications, which bear on the impacts of initially criminalizing a greater portion of sexual encounters. More specifically, the specifications estimated in Table II codify an expansion in statutory rape laws according to the percentage of sexual activity among girls of each age that trigger a felony according to the laws associated with the relevant state-year-age cell.[22] In our primary specifications reported in Panel A, we focus on variations in the law of an expansionary nature only—that is, we only draw upon instances in which states expand upon the breadth of sexual activity that triggers a felony (e.g., an increase in the age of consent or a decrease in the relevant age gap), as opposed to instances in which states also cut back on this breadth. The primary concern with drawing upon a contraction in the law is that

---

[22] Each observation in the specifications estimated throughout Section IV is weighted by the state-year-age population count used to form the denominator in the birthrate variable. Moreover, the coefficients estimated in each table are multiplied by 100, facilitating an interpretation of the findings as a percentage change in the relevant live birth rate. Standard errors are clustered at the state level to allow for arbitrary within-state correlations of the error structure—e.g., within-state auto-correlation in age-specific residuals and/or within-state auto-correlation in cohort-specific residuals.

the legislature may have simply codified a pre-existing disposition on the part of the state not to prosecute the previously implicated activities. For instance, if prosecutors had previously elected not to pursue offenders within several years of age of the victim, then the imposition of a 3-year age gap provision by the legislature may not be expected to alter the deterrence landscape, potentially attenuating the results towards zero.[23] Nonetheless, we likewise estimate specifications that embrace both expansions and contractions in the law.

We begin in Column 1 of Table II with a naïve difference-in-difference specification that includes only state, age and year fixed effects. We estimate an 11.5 percent reduction in live birth rates associated with an increase from 0 to 100 percent in the degree to which sexual activity among a given age group triggers a felony for the elder party to the encounter. As demonstrated by Column 2, this estimate changes only slightly to -10.7 with the inclusion of various state-year controls bearing on the prevailing economic and social environment.[24] Controlling flexibly for a greater range of time-varying state-specific factors and taking advantage of the within-state control groups afforded by the age-specific

---

[23] For this reason, the expansion-only specification was overwhelmingly the preferred specification by criminal law scholars that we spoke with concerning this analysis.

[24] Such controls include unemployment rate, percentage black, percentage living in urban areas, median household income, and the prevailing violent-crime offense rate. State-year unemployment rates are from the U.S. Bureau of Labor Statistics. Demographic measures and percent urbanization are from decennial Census files (1969-1999) and American Community Surveys (ACS) (2000-2004). Criminal offense data is from the Federal Bureau of Investigation's Uniform Crime Reports.

expansions in the law, we next find that this estimated coefficient remains nearly unchanged at -12.5 with the inclusion of state-year fixed effects (Column 3).

This latter finding suggests that omitted state-year initiatives likewise bearing on teenage pregnancy rates may have not necessarily confounded the basic difference-in-difference findings and thus suggests that state-year expansions in statutory rape laws are perhaps close to random in nature. Nonetheless, to also control flexibly for omitted age-year, and state-age effects, we estimate the full triple-differences specification (Column 4), finding a 13 percent reduction in live birth rates in association with an increase from 0 to 100 percent in the simulated felony incidence rate. While few covariates are available at the state-year-age level over this time period, we do confirm that these estimates remain virtually unchanged with the inclusion of a control for the percentage of the state-year-age population that is black (Column 5).[25] Moreover, as discussed in greater detail in the Online Appendix, we confirm that the statistical significance of these findings likewise holds under an alternative randomization inference approach.

The basic identifying assumption of the triple-differences specification is that there are no unobserved factors specific to given state-year-age groups that are

---

[25] When systematically dropping each state one-by-one from the estimation sample, the primary triple-differences coefficient from Column 3 (with our baseline of comparison equal to -13) remains in roughly the same range, from a low (in terms of an implied deterrence impact) of -8.9 when dropping Pennsylvania to a high of −16.0 when dropping Nevada. While the felony incidence variable benefits from an empirically-informed depiction of the potential reach of the law, it nonetheless operates in a relatively parametric fashion in the above specification. In Table A2 of the Online Appendix, we present results from a more non-parametric approach, where, rather than treating the simulated felony incidence variable linearly, we divide the felony incidence variable into four quartiles and estimate the association between live birth rates and the incidence of moving into a higher quartile.

correlated with the state-year-age-specific expansions in statutory rape laws. As an additional specification check, we challenge this identifying assumption by imposing state-age-specific linear time trends (e.g., a unique time trend associated with 12 year olds in Iowa), allowing us to control for slowly-moving unobservable factors that may be correlated with both state-age-specific pregnancy rates and state-age-specific laws. This inclusion suggests a slight intensification of the estimated association of interest (Column 6).

To rule out concerns that these results are merely reflective of pre-existing differential trends between the treatment and controls groups, as opposed to being reflective of a true policy response, we also estimate specifications that include 2- and 4-year leads of the simulated felony-incidence variable (Column 7).[26] The estimated coefficients of these lead variables are nearly 0 in magnitude and statistically indistinguishable from 0 (moreover, while the F-statistics are not provided, they are likewise jointly indistinguishable from 0). In Column 8, we estimate an even more dynamic specification by including 2- and 4- year lag variables,[27] allowing us to test for any delayed responses to the change in the

---

[26] These variables indicate at time *t* a state-age group's simulated felony percentage at times *t+2* and *t+4*, respectively. As such, if a state-age group's simulated felony percentage increases from 0.25 to 0.5 in 1995, the 2-year lead of this variable will register this switch in 1993. Pre-period trends could signal such things as mean reversion, legislative endogeneity or other omitted factors of concern.

[27] These variables indicate at time *t* a state-age group's simulated felony percentage at times *t-2* and *t-4*, respectively. As such, if a state-age group's simulated felony percentage increases from 0.25 to 0.5 in 1995, the 2-year lag of this variable will register this switch in 1997.

criminal law environment. The negative association of interest appears to occur predominantly in the 2-year period following the enactment of the law change.

Finally, in Panel B of Table II, we include variations in state statutory rape laws that incorporate both expansions and contractions in the law. Consistent with our expectations that contractions in the law may merely reflect previous dispositions of the state not to prosecute the now officially permissible behavior, this inclusion appears to attenuate the results slightly towards zero. The estimated coefficient of the felony-incidence variable falls (in absolute value) to -10.6, though nonetheless remains statistically significant.

In this primary approach to exploring the relationship between teenage pregnancies and expansions in the scope of statutory rape laws, we estimate least squares (OLS) regression specifications and use live birth rates (per population) as the dependent variable. In alternative specifications, we explore this relationship using Poisson regressions (with an exposure offset based on the relevant population). In the Online Appendix, we discuss and demonstrate the general robustness of the OLS results to this alternative specification.[28]

---

[28] The analysis is aggregated enough in time and geography—i.e., year and state—that the rare-event rationale for a count model is perhaps less relevant. With an average of roughly 890 live births in each state-year-age cell, there is little general concern of censoring at zero that could lead to a skewed error distribution at this lower bound. Moreover, with only 23 instances of 0-valued cells out of nearly 7000 state-year-age cells, there is little interference with the ability to log transform the birthrate dependent variable to deal with general skewness in the error distribution and better achieve normality. Moreover, with an average female population size per state-year-age cell of roughly 37,000, the estimated birth rates need not be viewed as so discrete in nature and may be sufficiently continuous to warrant least squares estimation (Osgood 2000). This is arguably true even in the case of the younger ages within the sample, where live birth counts per cell are more modest but where female population counts—i.e., the denominator—are nonetheless sufficiently large to generate continuity in the calculated birthrates. Ultimately, for these reasons, we elect to estimate OLS specifications as our primary approach. Nonetheless, as a robustness exercise, the Online Appendix estimates Poisson specifications.

IV.B. Felony Incidence and Sentence Enhancement Specifications

Having addressed the deterrent impacts associated with initially criminalizing a broader range of sexual activity, we now explore the subsequent impacts of sentence severity enhancements. We begin in Table III by estimating specifications that include measures simulating both the percentage of sexual encounters triggering a basic felony statute and the percentage of such encounters triggering a heightened felony (as specified above). Each column of Table III follows the associated specification from Table II. For instance, in Column 4, we estimate the full triple-differences specification, finding a similarly-sized reduction in live birth rates of roughly 15 percent in connection with an increase from 0 to 100 percent in the basic simulated felony incidence (the basic-felony coefficients from the remaining specifications closely follow those estimated in Table II). However, we estimate only a 2 percent subsequent reduction (statistically insignificant) in live birth rates in connection with an increase from 0 to 100 percent in the incidence of a heightened felony (Column 4).

Of course, these basic/heightened-felony specifications fail to capture much of the fine-grained variation in the severity dimension of the law. In Table A4 of the Online Appendix, we estimate specifications that include three and four punishment bins. The estimated coefficients of the basic felony incidence variable in these alternative specifications remain nearly identical to those

presented in Tables II and III, while the coefficients of the variables capturing the percentage of sexual activity falling into the heightened punishment tiers are closer to zero in magnitude and statistically indistinguishable from zero. Overall, the results continue to demonstrate that the documented association between live birth rates and expansions in the scope in statutory rape laws appear to be concentrated in the initial movements into criminalization, with little, if any, subsequent impact documented from severity enhancements.

The specifications estimated in Tables III and A4 benefit from a relatively non-parametric treatment of incarceration length associated with infractions of statutory rape laws. However, the coarse classifications still do not capture the full extent of the documented variations. For instance, they do not capture increases in minimum sentence lengths from 1 to 2 years or increases in the potential range of permissible incarcerations due, for instance, to an extension in the maximum punishment. In Table IV, we attempt to fill these gaps and codify punishment severity by simulating the expected minimum (Columns 1-3) or mean (Columns 4-6) incarceration length associated with a given sexual encounter among the relevant age group (where the mean is based on the *potential* punishment range). In Columns 2-3 and 5-6, we likewise include a measure of the simulated felony incidence, allowing us to simultaneously estimate the extensive and intensive dimensions of the law. The estimated coefficients of the simulated felony-incidence variables are consistent in magnitude and precision

with those estimated in Tables II and III. Moreover, the estimated coefficients of both the expected minimum and mean incarceration variables are consistent with the heightened-felony incidence variables from Tables III and A4 in suggesting that sentence enhancements may not be associated with further reductions in live birth rates. For instance, as presented in Column 2, we estimate that an increase of 1 year in the expected minimum punishment levels—representing a roughly 69% increase over a mean of 1.45 years—is associated with a modest and statistically-insignificant 0.3 percent *increase* in live birth rates.

IV.C. Enforcement; Rationality Exercises

Enforcement effects—One important aspect of the law not emphasized in the above analysis bears on its enforcement. A technical violation of the law may only lead to actual consequences for the actor to the extent that the police force and prosecutors pursue the offending party. One may be concerned that expansions in the statutory scope or severity of the law as it applies to teenagers as a whole are accompanied by increases in the degree to which the state generally enforces its statutory rape provisions (both with respect to its existing provisions and these new provisions), jeopardizing identification. However, to the extent that enforcement initiatives are not enacted at times in an age-specific manner within states, the triple-differences specification is designed to rule out concerns of this nature and better target the expanded scope of the law. To the

extent that variations in enforcement efforts within states occur on a victim-age-specific basis, this targeting becomes more difficult given the lack of enforcement-related data over the sample period at the state-year-age level.[29]

Nonetheless, to shed some light on the isolated effects of enforcement initiatives with the data that is available, we estimate difference-in-difference specifications building on Column 1 of Table II that include state-year arrest rates for non-forcible rape sex offenses (which include statutory rape offenses). We present the results of this exercise in Table V. In Column 1, we find that a 1-standard-deviation increase in the arrest rate of 18-30 year old males is associated with a roughly 3.3 percent reduction in live birth rates.[30] The estimated coefficient of the felony-incidence variable in this specification is nearly identical to that estimated in Table II, suggesting little concern that our main difference-in-difference findings is merely reflective of correlated enforcement initiatives.

As a rationality check on our main findings above, we take further advantage of the availability of these state-year arrest rates for non-forcible rape sex offenses to test the prediction that the triple-differences coefficient from the initial-criminalization analysis is more pronounced when accompanied by a criminal justice system that is inclined to pursue offenders of non-forcible rape sex

---

[29] Limited information may be available under the National Incident-Based Reporting System, however only for a limited number of years and a limited number of states, jeopardizing the ability to simultaneously estimate a triple-differences specification drawing on variations in the scope of statutory rape laws.

[30] These findings remain essentially identical when using 18-40 year old male arrest rates or 18-plus male arrest rates. The specification controls for forcible-rape arrest rates to account for other sex-offense enforcement efforts more broadly.

offenses relative to one that is not so inclined.[31] To test this prediction, we also estimate specifications that modify that of Column 3 of Table II (the main triple-differences specification) to include an interaction between the state-year-age felony-incidence variable and the state-year arrest rate for non-forcible rape sex crimes (see Column 2 of Table V).[32] As predicted, we estimate a negative value of the coefficient of the interaction term suggesting a stronger response (that is, a heightened negative response) to the law when met by an environment that is more inclined to pursue associated violations.

Differential enforcement and heterogeneous treatment effects—One would imagine that a given infraction of a statutory rape law is more likely to be enforced when the victim is on the younger end of the spectrum considered by our analysis—e.g., 12, 13 or 14 years old. As such, one might predict that the impact on teenage pregnancy rates felt from initially criminalizing sexual activity, in light of these likely enforcement differentials, will be more pronounced for younger age groups. As such, in Table VI, we present regression results whereby we test for heterogeneous responses to expansions in the scope of statutory rape laws across the different age groups. Consistent with these expectations, we indeed find that the association between the simulated felony-incidence variable and live

---

[31] A related prediction is that an increase in enforcement proclivities will have a greater impact on the incidence of sexual activities in those jurisdictions that have a broader reach of the law (i.e., a higher simulated felony-incidence value) relative to those with a more limited reach.

[32] Of course, we likewise include the necessary constitutive term—i.e., the state-year arrest rate for non-forcible rape sex offenses. Likewise, to control for forcible rape enforcement efforts, we include forcible rape sex offense rates, along with the interaction between those rates and the felony-incidence variable.

birth rates is the most strongly negative among 12 year olds and becomes less negative (or more positive) as age increases monotonically, suggesting that the deterrent impact of statutory rape laws weakens with age, as expected.

Finally, in the Online Appendix, we also test for heterogeneity in treatment effects across races. Much has been said in the statutory rape law literature about the extent to which the enforcement of the law has been levied in a very highly disproportionate manner against black or Hispanic males (Sutherland 2003), supporting a prediction that the expansion in the scope of statutory rape provisions into new ranges of sexual activity will have a stronger impact on associated lived birth rates among the minority population.[33] We present evidence in the Online Appendix supportive of this prediction, consistent with the above rationality checks suggesting stronger treatment effects in settings in which we either know of stronger enforcement or would predict stronger enforcement.

V. DISCUSSION AND CONCLUSION

The empirical literature in crime has long endeavored to explore the impact of criminal law on the incidence of those activities implicated by criminal statutes. This exercise has faced numerous obstacles, beginning with the difficulties associated with identifying the channel or mechanism behind any observed

---

[33] As discussed further in the Online Appendix, this heightened enforcement targeted at black and Hispanic males does not arise simply from situations in which they have intercourse with white females. Prosecutions also arise frequently from situations in which black or Hispanic females become pregnant as a result of such encounters (Sutherland 2003).

response. That is, are the observed reductions in crime attributable to deterred behavior on the part of would-be offenders or are they attributable to incapacitating previous offenders from repeating their past activities?

The present study is not necessarily designed to confront this first obstacle and rather aims to solidify our understanding of the fundamental impacts of criminality as a whole. That being said, it is perhaps reasonable to believe that a deterrent response is behind much of the roughly 11-13 percent reduction in live birth rates for a given age group that is found to be associated with the full criminalization of sexual encounters (to the offending party at least) involving that age group. To understand this, it is first important to consider how many fewer live births are in fact implicated by the findings. The mean simulated percentage of encounters that trigger a felony over the sample period (for 12-17 year olds) is a substantial 47 percent. As such, by removing this system of laws and in consideration of the above estimates, live birth rates for the implicated age groups may rise by an average of 5-6 percent. Incapacitative forces are unlikely to account for this substantial of a reduction considering that only around 0.2 percent of males aged 18-30 are arrested on average in a given year for non-forcible sex offenses (which includes statutory rape offenses). Moreover, if incapacitative forces had a large impact on live birth rates, one would presumably

expect to observe a response along the severity dimension as well (though the above findings are concentrated on initial movements into criminalization).[34]

Even assuming a deterrent, as opposed to an incapacitative, framework, additional mechanism questions arise. Precisely what behavior is being deterred? Consider the present context. Expansions in statutory rape laws are found to be associated with reductions in live birth rates. However, is this attributable to an actual reduction in sexual activity? Or has the underlying incidence of sexual activity remain unchanged and have we simply seen an increase in measures taken to avoid pregnancies—e.g., greater contraception—or an increase in measures taken to avoid live births—i.e., abortions? Parties may respond to statutory-rape-law expansions in these latter manners in an attempt to circumvent detection of a statutory rape violation (at least to serve the interests of the elder partner). Premised behind this response is the idea that the fact of a late-stage pregnancy or a live birth itself may signal to authority figures or parents that a possible statutory rape violation has occurred.

In the Online Appendix, we present evidence suggesting that the observed reduction in live birth rates cannot likely be explained by a corresponding

---

[34] It is worth mentioning, of course, that low rates of enforcement of this nature may also operate to blunt any deterrent channel. However, diminishment of deterrence need not occur if the consequences associated with statutory rape violations are considerable enough (Becker 1968). In a crime of this nature, more is certainly at stake than simply imprisonment. Relative to some serious crimes, a greater degree of social humiliation may follow incarceration for the commission of a sex offense against a minor. This social stigma channel is perhaps of more significance since the 1990s, following the move across jurisdictions, precipitated by two federal mandates, to require sex offenders to register with, and provide personally identifiable information to, law enforcement agencies and subsequently mandate that this information be made available to the public (Prescott and Rockoff 2011).

increase in abortion rates, as would be predicted by this circumvent-detection mechanism, suggesting that the response observed above may indeed be a reflection of reduced sexual activity itself. With richer data on contraception use across states and time (and ideally across ages), future research might hopefully shed light on a possible contraception circumvention mechanism.[35] Evidence of either reduced underlying sex or increased live-birth-avoidance measures would be consistent with a responsiveness in behavior to the prevailing criminal law environment—i.e., evidence of deterrence of some nature, whether of underlying sexual encounters or of sexual encounters that culminate in live births. However, it may be worthwhile to identify the precise nature of deterrence in order to determine whether the goals of statutory rape law have been satisfied. Of course, those goals themselves are multi-faceted. While most normative theories of statutory rape likely aim at curbing the underlying sexual activity itself, policymakers, as discussed above, have often looked upon such laws as tools to battle live births by teenage mothers as a specific outcome.

Another challenge facing the empirical criminal literature is one that plagues policy evaluation more generally: how to construct multi-state analyses that both manage across-jurisdictions differences in the structure of the relevant policies

---

[35] In future research, we also hope to extend this methodological framework to data sources providing more direct information on underlying sexual practices, thus more directly addressing this concern. For instance, we hope to acquire geographic identifier codes within the NSFG records facilitating the ability to construct similar DD and DDD analyses in questioning the impact of statutory rape expansions on the onset of sexual debuts. The NLSY records considered by Henry and Cunningham (2010) also afford this opportunity. However, for the reasons identified above, the NLSY is likely to be limited in the extent of legislative variations it is able to accommodate.

while not discounting information obtained from such structural variations. To accomplish these sometimes conflicting goals, we use individual characteristics from micro-level data sources to simulate the severity of criminal statutes in light of the manner in which those statutes treat such characteristics. Among other things, this affords us the opportunity to codify sentence enhancements in this statutory rape context given that both the degree of enhancements and the conditions under which they are triggered vary considerably across jurisdictions and within-jurisdictions over time.

The final and arguably most important challenge facing the literature's desire to explore the impact of the law on the incidence of the targeted activities is posed by the fact that most of the experimental settings facilitating this empirical exercise focus on sentence-enhancement policies or certainty-enhancement policies. While such enhancements may increase the perceived threat of criminal sanctions and intensify deterrence, these evaluations may understate the full impact of the law to the extent that potential offenders attach the biggest cost to the simple possibility of incarceration itself, no matter whether of 1-year duration or 5-year duration and no matter whether the chances of being caught are 10 percent or 20 percent. By identifying situations in which the law extends serious criminal liability to previously permissible activities and by drawing on data sources that provide consistent records of the targeted activity in both the pre- and post-criminalization periods, we are able to fill this gap in the literature and shed

light on the extent of this understatement in the impact of the law. Consistent with this fixed-cost theory of criminal behavior, our results indeed suggest that the largest driver of deterrence may indeed come with simply criminalizing the activity in the first instance.

CORNELL LAW SCHOOL; CORNELL GRADUATE ECONOMICS FACULTY

DEPARTMENT OF ECONOMICS, STANFORD UNIVERSITY

REFERENCES

Abrams, David S. "Estimating the Deterrent Effect of Incarceration Using Sentence Enhancements." *American Economic Journal: Applied Economics*, 4, no. 4 (2012), 32-56.

Becker, Gary. "Crime and Punishment: An Economic Approach." The *Journal of Political Economy*, 76 (1968), 169–217.

Carpenter, Christopher, and Carlos Dobkin. 2009. "The Effect of Alcohol Consumption on Mortality: Regression Discontinuity Evidence from the Minimum Drinking Age." *American Economic Journal: Applied Economics*, 1, no. 1 (2009), 164-82.

Cook, Philip, Alan Parnell, Michael Moore, and Deanna Pagnini. "The Effects of Short-Term Variation in Abortion Funding on Pregnancy Outcomes." *Journal of Health Economics,* 18 (1999), 241-258.

Cook, Philip, and Michael Moore. "The Economics of Alcohol Abuse and Alcohol-Control Policies." *Health Affairs*, 21 (2002), 120-133.

Currie, Janet, and Jonathan Gruber. "Health Insurance Eligibility, Utilization of Medical Care, and Child Health." *Quarterly Journal of Economics*, 111 (1996), 431–66.

Dills, Angela K., and Jeffrey A. Miron. "Alcohol Prohibition and Cirrhosis." *American Law and Economics Review*, 6, no. 2 (2004), 285-318.

Dills, Angela K., Mireille Jacobson, and Jeffrey A. Miron. "The Effect of Alcohol Prohibition on Alcohol Consumption: Evidence from Drunkenness Arrests." *Economics Letters*, 86, no. 2 (2005), 279-294.

Donovan, Patricia. "Can Statutory Rape Laws be Effective in Preventing Adolescent Pregnancies?" *Family Planning Perspectives*, 29, no. 1 (1996), 30-40.

Doob, Anthony M., and Cheryl Webster. "Sentence Severity and Crime: Accepting the Null Hypothesis." *Crime and Justice*, 30 (2003), 143-195.

Durlauf, Steven N., and Nagin, Daniel S. "The deterrent effect of imprisonment." In *Controlling Crime: Strategies and Tradeoffs*, NBER Chapters. (Cambridge, MA: National Bureau of Economic Research, Inc., 2010).

Ehrlich, Isaac. "The Deterrent Effect of Capital Punishment: A Question of Life and Death." *American Economic Review*, 65, no. 3 (1975), 397-417.

Ehrlich, Isaac. "Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence." *Journal of Political Economy*, 85, no. 4 (1977), 741-88

Elo, Irma T, Rosalind Berkowitz King, and Frank F. Furstenberg. "Adolescent Females: Their Sexual Partners and the Fathers of their Children." *Journal of Marriage and the Family*, 61, no. 1 (1999), 74-84.

Eterno, John A., and Eli B. Silverman. *The Crime Numbers Game: Management by Manipulation (Advances in Police Theory and Practice)* (CRC Press; 1st edition, 2012).

Frakes, Michael, and Matthew Harding. "The Deterrent Effect of Death Penalty Eligibility: Evidence from the Adoption of Child Murder Eligibility Factors." *American Law and Economics Review*, 11, no. 2 (2009), 451-497.

Gruber, Jonathan. "The Incidence of Mandated Maternity Benefits," *American Economic Review*, 84 (1994), 622-41.

Helland, Eric, and Alexander Tabarrok, "Does Three Strikes Deter?: A Nonparametric Estimation." *Journal of Human Resources*, 42, no. 2 (2007), 309-330

Henry, Matthew, and Scott Cunningham. "Do Statutory Rape Laws Work?" http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1443273, 2010.

Jepsen, Christopher, and Lisa Kay Jepsen. "The Effects of Statutory Rape Laws on Nonmarital Teenage Childbearing." *Contemporary Economic Policy*, 24, no. 1 (2006), 35-51.

Kahneman, Daniel. *Thinking Fast and Slow*. (New York, NY: Farrar, Straus and Giroux, 2012).

Kahneman, Daniel, and Amos Tversky. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica*, 47, no. 2 (1979), 263-292.

Lawlor, Debbie, and Mary Shaw. "Too much too young: Teenage pregnancy is not a public health problem." *International Journal of Epidemiology*, 31 (2002), 552-3.

Lee, David S., and Justin McCrary. "The Deterrence Effect of Prison: Dynamic Theory and Evidence." CEPS Working Paper No. 189, 2009.

Levitt, Steven D. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation." *Quarterly Journal of Economics*, 111, no. 2 (1996), 319-51.

Levitt, Steven D. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime," *American Economic Review*, 87, no. 3 (1997), 270-90.

Levitt, Steven D. "Juvenile Crime and Punishment." *Journal of Political Economy*, 106, no. 6 (1998), 1156-1185.

Levitt, Steven D. "Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?" *Economic Inquiry*, 36, no. 3 (1998), 353-72.

Lovenheim, Michael F., and Daniel P. Steefel. "Do blue laws save lives? The effect of Sunday alcohol sales bans on fatal vehicle accidents." *Journal of Policy Analysis and Management*, 30, no. 4 (2011), 798-820.

Nagin, Daniel. "Criminal Deterrence Research at the Outset of the Twenty-First Century." In *Crime and Justice: A Review of Research*, vol. 23. Michael Tonry, ed. (Chicago: University of Chicago Press, 1998).

Osgood, D. Wayne. "Poisson-Based Regression Analysis of Aggregate Crime Rates." *Journal of Quantitative Criminology*, 16, no. 1 (2000), 21-43.

Prescott, J.J., and Jonah E. Rockoff. "Do Sex Offender Registration and Notification Laws Affect Criminal Behavior?" *Journal of Law and Economics*, 54, no. 1 (2011), 161-206.

Rupp, Thomas. "Meta analysis of crime and deterrence : a comprehensive review of the literature." Dissertation. Publications of Darmstadt Technical University, Institute of Economics (VWL) 30959, Darmstadt Technical University, Department of Business Administration, Economics and Law, Institute of Economics (VWL), 2008.

Scally, Gabriel. "Too much too young? Teenage Pregnancy is a public health problem, not a clinical option." *International Journal of Epidemiology*, 31 (2002), 554-5.

Sutherland, Kate. "From Jailbird to Jailbait: Age of Consent Laws and the Construction of Teenage Sexualities." *William & Mary Journal of Women and the Law*, 9 (2003), 313-49.

TABLE I

SUMMARY STATISTICS

	OVER- ALL	12 YEAR OLDS	13 YEAR OLDS	14 YEAR OLDS	15 YEAR OLDS	16 YEAR OLDS	17 YEAR OLDS
Panel A: State-Year-Age Variables							
Statutory Rape Law Variables							
Simulated Incidence of Any	0.47	0.82	0.70	0.54	0.45	0.18	0.12
Felony	(0.42)	(0.29)	(0.31)	(0.36)	(0.38)	(0.32)	(0.31)
Simulated Basic Felony	0.44	0.70	0.66	(0.52)	0.45	0.18	0.12
Incidence (1-10 years							
minimum)	(0.41)	(0.38)	(0.33)	0.36	(0.38)	(0.32)	(0.31)
Simulated Any Heightened	0.03	0.12	0.04	0.02	0.00	0.00	0.00
Felony Incidence (10+							
years minimum)	(0.16)	(0.29)	(0.18)	(0.13)	(0.03)	(0.00)	(0.00)
Simulated Expected	1.30	3.72	2.00	1.04	0.69	0.23	0.13
Minimum Incarceration							
Level (years)	(2.94)	(5.49)	(2.75)	(1.88)	(0.75)	(0.35)	(0.32)
Simulated Expected Mean	5.66	13.54	8.95	4.97	4.02	1.37	0.95
Incarceration Level							
(years)	(8.31)	(10.08)	(9.37)	(6.67)	(5.79)	(3.77)	(3.72)
Other Variables							
Live Birth Rate (Per 1,000	24.84	0.69	3.48	11.21	25.13	44.09	64.88
females)	(25.91)	(0.46)	(1.82)	(5.02)	(10.03)	(15.87)	(20.81)
Live Birth Counts	840.28	23.59	119.05	380.36	852.65	1463.85	2182.06
(unweighted)	(1573.13)	(30.76)	(151.44)	(485.07)	(1062.12)	(1784.05)	(2566.68)
Panel B: State-Year Variables							
Arrests for Non-Forcible	1.6	-	-	-	-	-	-
Rape Sex Offenses Per							
1,000 Population (18-30	(0.86)						

year-old males)

Arrests for Forcible Rape

Sex Offenses Per 1,000	0.74	-	-	-	-	-	-
Population (18-30 year-	(0.36)						
old males)							

Notes: data in Panel A are from a sample of 6,758 state-year-age cells. Live birth rates are from the Natality Detail Files of the National Vital Statistics Data. State-year-age population data (with percentage black) are from the U.S. Census Bureau and were collected from the National Cancer Institute, a division of the United States National Institutes of Health. The underlying data applied to the statutory rape laws of each state-year-age cell used to form the simulated statutory rape law variables are from the 1996 and 2002 arms of the National Survey of Family Growth. All measures (except for the female population total and the unweighted live birth count) are weighted by the associated female population of the state-year-age cell. Data in Panel B are from the FBI's Uniform Crime Reports.

TABLE II

RELATIONSHIP BETWEEN LIVE BIRTH RATES AND SIMULATED PERCENTAGE OF SEXUAL ACTIVITY

THAT TRIGGERS A FELONY

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Primary Specifications: Expansion-only Variations in Statutory Rape Laws								
Simulated Felony Percentage: 4-Year Lead	-	-	-	-	-	-	0.1 (5.4)	1.54 (5.5)
Simulated Felony Percentage: 2-Year Lead	-	-	-	-	-	-	0.9 (4.6)	-0.3 (5.2)
Simulated Felony Percentage	-11.5** (5.5)	-10.7** (5.3)	-12.5* (6.7)	-13.0** (5.9)	-12.7** (5.8)	-19.1*** (5.7)	-13.8** (6.4)	-24.1** (9.3)
Simulated Felony Percentage: 2-Year Lag	-	-	-	-	-	-	-	2.6 (9.0)
Simulated Felony Percentage: 4-Year Lag	-	-	-	-	-	-	-	8.5 (1.5)
Percentage Black	-	-	-	-	238.6** (95.7)	-	-	-
N	6525	6437	6525	6525	6437	6525	6525	5402
Panel B. Alternative Specifications: Expansions and Contractions in Statutory Rape Laws								
Simulated Felony Percentage: 4-Year Lead	-	-	-	-	-	-	0.8 (5.1)	2.2 (5.2)
Simulated Felony Percentage: 2-Year Lead	-	-	-	-	-	-	2.9 (5.1)	2.0 (5.8)

Simulated Felony Percentage	-4.7	-4.7	-10.7	-10.6**	-9.0**	-5.9	-13.5***	-14.1*
	(5.4)	(5.0)	(6.5)	(4.5)	(4.1)	(5.1)	(4.2)	(8.3)
Simulated Felony Percentage: 2-Year Lag	-	-	-	-	-	-	-	0.1 (6.9)
Simulated Felony Percentage: 4-Year Lag	-	-	-	-	-	-	-	0.5 (4.1)
Percentage Black	-	-	-	-	238.0** (91.4)	-	-	-
N	6759	6668	6759	6759	6668	6759	6759	5601
State-Year Covariates	NO	YES	NO	NO	NO	NO	NO	NO
State-Year Fixed Effects?	NO	NO	YES	YES	YES	YES	YES	YES
Age-Year Effects?	NO	NO	NO	YES	YES	YES	YES	YES
State-Age Effects?	NO	NO	NO	YES	YES	YES	YES	YES
State-Age-Specific Linear Trends	NO	NO	NO	NO	NO	YES	NO	NO

Notes: robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Estimated statistics are multiplied by 100. All regressions include state, age and year fixed effects and are weighted by the female population of the associated state-year-age cell. Live birth rate data is from the Natality Detail Files. .

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE III.

RELATIONSHIP BETWEEN LIVE BIRTH RATES AND SIMULATED PERCENTAGE OF SEXUAL ACTIVITY THAT TRIGGERS A CRIME WITHIN TWO FELONY PUNISHMENT TIERS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Simulated Basic								
Felony Incidence								
4-Year Lead	-	-	-	-	-	-	-0.9	0.4
							(5.6)	(5.3)
2-Year Lead	-	-	-	-	-	-	0.5	0.5
							(4.5)	(5.0)
Contemporaneous	-11.7*	-10.9*	-12.4*	-15.2***	-14.8***	-19.9***	-15.9**	-23.0**
	(5.8)	(5.7)	(7.1)	(5.5)	(5.5)	(5.9)	(6.1)	(9.3)
2-Year Lag	-	-	-	-	-	-	-	0.6
								(9.9)
4-Year Lag	-	-	-	-	-	-	-	6.2
								(6.5)
Simulated Heightened								
Felony Incidence								
4-Year Lead	-	-	-	-	-	-	-5.0	-5.2
							(10.0)	(10.7)
2-Year Lead	-	-	-	-	-	-	6.5	14.3
							(14.5)	(19.6)
Contemporaneous	-10.5	-9.5	-12.7	-2.2	-2.0	-14.6	-5.1	-25.1
	(7.3)	(7.1)	(9.1)	(8.3)	(7.8)	(9.6)	(11.2)	(18.2)
2-Year Lag	-	-	-	-	-	-	-	9.8
								(12.2)
4-Year Lag	-	-	-	-	-	-	-	15.5**
								(7.1)

Percentage Black	-	-	-	-	244.7** (94.7)	-	-	-
N	6521	6433	6521	6521	6433	6521	6521	5326
State-Year Covariates	NO	YES	NO	NO	NO	NO	NO	NO
State-Year Fixed Effects?	NO	NO	YES	YES	YES	YES	YES	YES
Age-Year Effects?	NO	NO	NO	YES	YES	YES	YES	YES
State-Age Effects?	NO	NO	NO	YES	YES	YES	YES	YES
State-Age-Specific Linear Time Trends	NO	NO	NO	NO	NO	YES	NO	NO

Notes: robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Estimated coefficients and standard errors are multiplied by 100. All regressions include state, age and year fixed effects and are weighted by the female population of the associated state-year-age cell. Live birth rate data is from the Natality Detail Files. .

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE IV

RELATIONSHIP BETWEEN LIVE BIRTH RATES AND SIMULATED EXPECTED MINIMUM AND

MEAN INCARCERATION RATES ASSOCIATED WITH SEXUAL ACTIVITY AMONG RELEVANT AGES

	(1)	(2)	(3)	(4)	(5)	(6)
Felony Incidence						
Simulated Percentage: 4-Year Lead	-	-	0.9	-	-	2.0
			(5.9)			(6.9)
Simulated Percentage: 2-Year Lead	-	-	0.3	-	-	-2.0
			(5.6)			(5.8)
Simulated Percentage	-	-15.2**	-15.9**	-	-19.2***	-18.8***
		(5.8)	(6.0)		(5.5)	(5.7)
Expected Minimum Incarceration Length (Years)						
Simulated Expected Minimum: 4-Year Lead	-	-	-0.18	-	-	-
			(0.38)			
Simulated Expected Minimum: 2-Year Lead	-	-	0.20	-	-	-
			(0.77)			
Simulated Expected Minimum	0.1	0.3	0.26	-	-	-
	(0.2)	(0.3)	(0.63)			
Expected Mean Incarceration Length (Years)						
Simulated Expected Mean: 4-Year Lead	-	-	-	-	-	-0.2
						(0.3)
Simulated Expected Mean: 2-Year Lead	-	-	-	-	-	0.3
						(0.4)
Simulated Expected Mean	-	-	-	0.0	0.3	0.2
				(0.2)	(0.2)	(0.4)

| | | N | 6543 | 6519 | 6519 | 6543 | 6519 | 6519 |

Notes: robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Estimated coefficients and standard errors are multiplied by 100. All regressions include state, age and year fixed effects, along with state-year effects, age-year effects and state-age effects. Regressions are weighted by the female population of the associated state-year-age cell. Live birth rate data is from the Natality Detail Files. .

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE V

RELATIONSHIP BETWEEN LIVE BIRTH RATES AND STATE-YEAR SEX-OFFENSE ARREST RATES

	(1)	(2)
Non-Forcible Rape Sex Offense Arrest Rate (Divided by 1 Standard Deviation of Rate)	-3.3*** (1.2)	17.3*** (1.3)
Forcible Rape Sex Offenses Arrest Rate (Divided by 1 Standard Deviation of Rate)	3.4*** (1.1)	-12.5*** (1.0)
Simulated Felony Incidence	-11.7** (5.7\)	-16.1** (7.6)
Simulated Felony Incidence X Non-Forcible Rape Arrest Rate	-	-4.7* (2.6)
Simulated Felony Incidence X Forcible Rape Arrest Rate	-	5.0** (1.9)
N	5982	5982
State-Year Fixed Effects?	NO	YES
Age-Year Fixed Effects?	NO	YES
State-Age Fixed Effects?	NO	YES

Notes: robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Estimated coefficients and standard errors are multiplied by 100. Arrest rates are originally calculated per 1,000 males aged 18-30 years old. To facilitate interpretation of the results, the rates are then divided by their standard deviation, such that an increase in 1 of the indicated variable corresponds to an increase in 1 standard deviation of the respective arrest rate. All regressions include state, age and year fixed effects. Regressions are weighted by the female population of the associated state-year-age cell. Live birth rate data is from the Natality Detail Files. .

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE VI

HETEROGENEITY IN TREATMENT EFFECTS BY AGE

	(1)
(Omitted category: 17 Year Olds)	
Simulated Felony Incidence	51.9**
	(20.6)
Simulated Felony Incidence X 12-Year Old Dummy	-72.6***
	(21.4)
Simulated Felony Incidence X 13-Year Old Dummy	-65.0***
	(23.0)
Simulated Felony Incidence X 14-Year Old Dummy	-60.1***
	(21.1)
Simulated Felony Incidence X 15-Year Old Dummy	-51.4**
	(21.0)
Simulated Felony Incidence X 16-Year Old Dummy	-37.8*
	(20.3)
N	6525

Notes: robust standard errors corrected for within-state correlation in the error term are reported in parentheses. Estimated coefficients and standard errors are multiplied by 100. All regressions include state, age and year fixed effects, along with state-year effects, age-year effects and state-age effects. Regressions are weighted by the female population of the associated state-year-age cell. Live birth rate data is from the Natality Detail Files. .

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.