

Does the Felony-Murder Rule Deter? Evidence from FBI Crime Data

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Conviction for murder, with one exception, requires proof of intent. A person is liable for murder only if she specifically intends the death that she causes, intends to inflict serious bodily harm on the victim, or acts with reckless disregard for human life. The exception is called the felony-murder rule. In a state with the felony-murder rule, a person can be convicted of murder without proof of any intent to cause death if the death at issue occurred during the commission of a felony. The overwhelming majority of legal commentary addressing the rule criticizes this exception to the general intent requirement for a murder conviction.¹ England and other common law countries have long since abolished the rule.² Yet, forty-three American states still retain it.

Proponents of the rule argue that it encourages criminals to reduce the number of felonies they commit and to take greater care to avoid causing death while committing a felony. Opponents argue that criminals are unaware of the felony-murder rule and, more importantly, that it is impossible to deter criminals from committing unintentional acts. Critics also argue that the felony-murder rule distorts marginal deterrence incentives—once a felon has accidentally caused one death, there is less to deter him from intentionally killing other witnesses to the crime.

Although nearly 20 percent of all murders annually are felony murders, no one has empirically validated the effects of the rule. This paper attempts to fill this hole using

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¹ See, e.g., See Kevin Cole, *Killings During Crime: Toward a Discriminating Theory of Strict Liability*, 28 Am Crim L Rev 73, 73-74 (1991) (concluding that despite the uniform hostility of commentators, the felony murder rule remains “quite durable”).

² Homicide Act of 1956, 5 & 6 Eliz 2, ch 11, § 1 (England); Indian Penal Code §§ 299-300 (India). The rule is unknown in civil law countries such as France and Germany. American Law Institute (“ALI”), 201.2 *Model Penal Code* (“MPC”) 40 (ALI, Tentative Draft No 8, May 9, 1958).

state-level data on felonies and felony homicides from 1970-98. My results suggest, first and foremost, that the felony-murder rule has a relatively small effect on criminal behavior: it does not substantially affect either the overall felony or felony-murder rate. Second, the effects of the rule vary by type of felony. While it has the positive effect of reducing deaths during burglaries, larcenies and auto thefts, it may have the perverse effect of increasing the number of robbery homicides. The rule has little effect on rapes. This paper traces out the chain of causation that produces these disparate results and demonstrates that neither proponents' nor opponents' theories alone explain the effects of the rule.

Part I explains the felony-murder rule in greater detail and reviews the debate over its practical impact. Part II describes the research design and data used in this paper. Part III presents my results. Readers seeking a non-technical exposition are encouraged to jump from Part I to Part III.C.

I. THE FELONY-MURDER RULE

A. Scope of the Rule

According to the felony-murder rule, a person is guilty of murder if someone dies while the person is committing a felony. It does not matter whether the death was intended or the product of a reckless disregard for the risk to human life or criminally negligent behavior. Even completely unintended or accidental death may trigger murder liability for felons.³ In short, the rule holds criminals strictly liable for all deaths during a felony. Table 1 compares how death caused by a defendant is classified during and in the absence of a felony, given the mental state of the defendant and other surrounding circumstances.

This description of the felony-murder rule is general. The precise contours of the rule vary across states. Most of the variation occurs along four dimensions. First, the specific felonies covered by the rule differ from state to state.⁴ All states that have the rule apply

³ For example, if defendant D, while committing a robbery, points a gun at victim V and V dies of a heart attack induced by fright, D is liable for murder even though D never pulled the trigger and the gun was not loaded. *People v Stamp*, 2 Cal App 3d 203, 82 Cal Rptr 598 (1969).

⁴ Most states provide detailed lists of felonies during which a killing is punished as first-degree murder. *See, e.g.*, Cal

it to robbery, burglary, rape and arson.⁵ Other common felonies covered by the rule include kidnapping, drug crimes and sex offenses such as child molestation.⁶ One constant across all states, however, is that the rule is limited to those felonies that are independent of the homicide, i.e., felonies whose sole purpose is not physically to harm the victim.⁷ For instance, no state applies the rule to the felony of manslaughter, for that would simply convert every manslaughter into murder.⁸ Most states do not even permit felony-murder prosecutions for deaths due to assault.⁹

A second source of variation is the defined period of the felony. At common law, the felony-murder rule only applied if death occurred during commission of a felony.¹⁰ Most states, however, expand the duration of a felony to include the period of attempt before commission of the felony.¹¹ Some states even extend it to include subsequent flight from the felony.¹² The third source of variation is the scope of vicarious liability. Most states hold a felon liable for the deaths of non-felons caused by co-felons.¹³ Some states, however, go further and hold a felon liable for the deaths of non-felons caused by anyone during the felony.¹⁴ A small minority of states does not stop there. Instead of limiting liability to those deaths “in furtherance of the felony,” these states hold a felon liable even for the deaths of co-felons.¹⁵ The final source of variation is the strength of causation required

Penal Code § 189 (West 1998). Killings during other felonies or during felonies specified in a separate list are punished as second-degree murder or manslaughter. Regardless of whether a state chooses to so enumerate the felonies that are covered by its felony-murder rule, most states’ courts limit the scope of murder liability to homicides during “inherently dangerous felonies.” *People v Phillips*, 64 Cal 2d 574, 414 P2d 353 (1966). Some states, cognizant of this judicial limitation on the scope of the felony-murder rule, have simply noted in their homicide statutes that the rule only applies to “inherently dangerous felonies” or to “forcible felonies.” See, e.g., Ala Code § 13A-6-2(a)(3) (1998); ILCS ch 720, § 5/9-1(a)(3) (1998).

⁵ See, e.g., Wis Stat Ann § 940.03 (West 1998).

⁶ See, e.g., 18 Pa Cons Stat Ann § 2502 (Purdon 1998).

⁷ Joshua Dressler, 31.07 *Understanding Criminal Law* at 469-70 (Matthew Bender 1987). This rule does not, however, exclude rape from the purview of the felony-murder rule. This exception makes sense if a rapist is thought to derive satisfaction from the rape independent of the harm it causes the victim.

⁸ *Id* at 468-69.

⁹ See, e.g., *People v Ireland*, 70 Cal 2d 522, 538-39, 450 P2d 580, 589 (1969).

¹⁰ See also, e.g., Mississippi (Miss Code § 97-3-19(1)(c) (1998)).

¹¹ See, e.g., DC Code 1981 § 22-2401 (1998).

¹² See, e.g., 17-A Me Rev Stat Ann § 202(1) (1998).

¹³ See, e.g., Ark Stat Ann § 5-10-101(a)(1) (1998); Conn Gen Stat Ann § 53a-54c (West 1998).

¹⁴ See, e.g., Alaska Stat § 11.41.110(a)(3) (1998); NJ Stat Ann § 2C:11-3(a)(3) (West 1998). For example, if a police officer aims at the felon but accidentally shoots a fellow officer, the felon may be charged for the murder of the fallen officer. *Commonwealth v Almeida*, 362 Pa 596, 68 A2d 595 (1949).

¹⁵ See, e.g., Ariz Rev Stat Ann § 13-1105(A)(2) (West 1998). Some states seem to take an intermediate position, allowing liability for the death of the co-felon at the hands of the felon, but not at the hands of any non-felon. See, e.g., ND Cent Code § 12.1-16-01(1)(c) (1998).

between felonious conduct and death. All states require, at the very least, that the felony be a but-for cause of the death at issue.¹⁶ Most states go further and require that the felony be a proximate or immediate cause of the death that triggers liability.¹⁷

B. Distribution of the Rule

While these four dimensions—felonies covered, period covered, extent of vicarious liability, and causation required—best explain the variation in state felony-murder rules, a more convenient way to compare states is to examine how they punish completely unintended deaths that occur during a felony. Thirty-eight states and the District of Columbia¹⁸ currently punish any death caused during robbery, burglary, rape or arson as either first-degree murder (or ordinary murder if they do not classify murder into degrees). Six states punish such death as second-degree murder and two—Arizona (since 1976) and Wisconsin (since 1956)—punish it as manslaughter. The remaining four—Delaware (since 1974), Hawaii (since 1973), Kentucky (since 1974), and Michigan (since 1980)—have abolished the felony-murder rule altogether. These groupings and their evolution over time (see Table 2) are the lynchpin for the empirical analysis in Part III.

¹⁶ For example, if a felon rapes a victim and she, out of shame, commits suicide, the felon is liable for murder. *Stephenson v. State*, 205 Ind 141, 179 NE 633 (1932). See also, e.g., *People v Arzon*, 92 Misc2d 739, 401 NYS2d 156 (1978).

¹⁷ For example, if a felon and her co-felon are piloting a plane with illegal drugs and, due to fog, the plane crashes, killing the co-felon, the felon is not liable for the death of the co-felon. The reason is that, assuming that the felons did not seek to fly into the fog to avoid detection, the felony did not uniquely increase the risk of death due to a plane crash. *King v Commonwealth*, 6 Va App 351, 368 SE2d 704 (1988).

¹⁸ Part III presents regression results without including the District of Columbia in the sample. However, these results are robust to including the District in the sample.

C. Predictions Regarding the Likely Effects of the Rule

Advocates of the felony-murder rule advance three practical justifications.¹⁹ First, the rule induces felons to take greater care to avoid accidental deaths when committing felonies, for example, by using less deadly weapons or by planning crimes with greater precision.²⁰ Holding the number of felonies constant, the rule decreases the number of accidental deaths during felonies. Second, like any strict-liability regime, the felony-murder rule induces felons not only to take care while committing crimes, but also optimally to adjust the level of their overall criminal activity.²¹ Thus, the rule is likely to reduce the total number of felonies in any given year. Third, strict liability for deaths during a felony deters intentional killings.²² There may be felons who kill intentionally, thinking they can claim under oath to have acted accidentally. Under the felony-murder rule felons cannot escape punishment for intentional killings by perjuring themselves in this manner because they are held strictly liable for all deaths during felonies.²³

¹⁹ There are three basic moral justifications for the felony-murder rule. First, under a theory of transferred intent, the intent to commit a felony translates into the intent to cause the death of the victim. William L. Clark and William L. Marshall, *A Treatise on the Law of Crimes* 656 (Martin Q. Barnes, ed) (7th ed 1967). Because the latter warrants a murder conviction, so too does the former. *State v O'Blasney*, 297 NW2d 797, 798 (SD 1980). Second, because society judges an accidental death during a robbery as "more closely akin to murder than to robbery," retribution requires that deaths during felonies result in harsher punishments. David Crump and Susan W. Crump, *In Defense of the Felony Murder Doctrine*, 8 Harv J of L & Pub Pol 359, 363 (1985). Finally, the rule is a form of expressive punishment that demonstrates either reverence for human life or solidarity with the victim. The rule may even facilitate expiation by allowing the felon to "repay his debt to society." *Id* at 367-68.

On the basis of morality, critics of the rule contend that it is unjust because it disconnects punishment from moral culpability as measured by the defendant's mental state during the crime. Under the felony-murder rule, a felon who accidentally kills while committing a felony is treated the same as the ruthless killer who carefully plots the death of his victim. James J. Tomkovicz, *The Endurance of the Felony-Murder Rule: A Study of the Forces that Shape Our Criminal Law*, 51 Wash & Lee L Rev 1429, 1438-1441 (1994); ALI, 201.2 MPC at 36-37 (cited in note 2); Nelson E. Roth and Scott E. Sundby, *The Felony-Murder Rule: A Doctrine at Constitutional Crossroads*, 70 Cornell L Rev 446, 459 (1985).

These morality-based arguments are difficult to compare or reconcile. Many scholars have tried, and readers should refer to their writings for insight into the moral debate over the felony-murder rule. *See, e.g.*, Roth and Sundby, 70 Cornell L Rev at 446; Tomkovicz, 51 Wash & Lee L Rev at 1429; Crump and Crump, 8 Harv J of L & Pub Pol at 359; George P. Fletcher, *Reflections on Felony-Murder*, 12 Southwestern U L Rev 413 (1981); ALI, 201.2 MPC at 36-39. The contribution of this paper is simply an empirical analysis of the practical effects of the rule.

²⁰ Crump and Crump, 8 Harv J L & Pub Pol at 369-70; Cole, 28 Am Crim L Rev at 97. *See also People v Washington*, 62 Cal 2d 777, 781, 402 P2d 130,133, 44 Cal Rptr 442, 445 (1965).

²¹ Richard A. Posner, *An Economic Theory of the Criminal Law*, 85 Colum L Rev 1193, 1222 (1985). *See also People v Washington*, 62 Cal 2d at 790, 402 P2d at 139, 44 Cal Rptr at 451 (Burke dissenting).

²² A related argument is that the felony-murder rule saves prosecutorial resources. If a person who kills during the course of a dangerous felony is almost certainly guilty of intentional or extremely reckless homicide—that is, if homicide during the commission of a felony is probably murder independent of the felony-murder rule, then the state can reduce the costs of prosecution by simply eliminating the burden of proving intent. Richard A. Posner, *Economic Analysis of Law* 257 (Aspen 5th ed 1998); ALI, 201.2 MPC at 36-37. This argument has grave weaknesses. Most notably, concern for prosecutorial resources justifies creating a rebuttable presumption that the defendant had specific intent to kill, not an irrebuttable presumption. The argument is also very hard to test because data on prosecutorial resources expended on felony-murder trials are difficult to gather.

²³ Crump and Crump, 8 Harv J of Law & Pub Pol at 375-76; Cole, 28 Am Crim L Rev at 96; Malcolm Budd and Andrian

Opponents of the felony-murder rule primarily argue that the rule does not reduce crime rates. They claim that felons are probably not aware of the rule.²⁴ Even if they are, criminals are irrational and do not respond to the incentives the rule provides. Even if felons are rational, crime scenes are rather chaotic and criminals cannot do much to reduce the risks of accidental deaths.²⁵ Finally, even if criminals could control crime scenes, the felony-murder rule does not substantially increase the criminal penalty for killing during a felony. Without the rule, the state can probably prove that a felon was reckless and therefore guilty of murder.²⁶ The number of cases where the criminal is not liable for murder without the felony-murder rule is so small that the rule may not deter many accidental homicides.

An argument can also be made that the felony-murder rule actually increases crime rates. The punishment for homicide ordinarily rises as the criminal's mental state moves from negligence to specific intent. The felony-murder rule eliminates such grading for deaths during felonies. Thus the rule diminishes marginal deterrence against reckless or intentional killing after the incidence of accidental death during a felony. For example, without the felony-murder rule, if a robbery victim died accidentally, the robber has a strong incentive to avoid intentionally causing additional deaths because his punishment would increase from a robbery sentence to a murder sentence. With the rule, after causing an accidental death, the disincentive to intentionally taking a life is diminished because an intentional killing after an accidental death only increases a robber's punishment from one murder sentence to two murder sentences. The latter increment in punishment is smaller than the former.²⁷

Lynch, *Voluntariness, Causation, and Strict Liability*, 1978 Crim L Rev 74, 76 n 6 (1978) (observing that strict liability for crimes generally eliminates any benefits from perjury). The weakness of this argument is that killing of any sort during a felony increases the police resources devoted to catching the felon so intentional killing is not costless to the felon if the alternative is not killing at all.

²⁴ Roth and Sundby, 70 Cornell L Rev at 451-52 (cited in note 19).

²⁵ *Id* at 451.

²⁶ ALI, 210.2 *MPC* at 37; Professor Albert Alschuler has suggested this argument to the author.

²⁷ Of course this reduction in marginal punishment is offset by any increase in police attention attracted by a second death during a felony. Moreover, in many states a second homicide is an aggravating factor that may be used to justify a death sentence. See, e.g., Ky. Stat. § 532.025(2)(a)(3) (requiring also the use of a weapon that is likely to lead to multiple deaths).

D. Previous Empirical Work

No one yet has tested empirically these predictions about the effect of the felony-murder rule on crime rates. Prior studies have only noted that the incidence of death during felonies is exceedingly low. The drafters of the Model Penal Code observed, for example, that deaths occur in less than 0.5 percent of all robberies.²⁸

Critics of the rule have used these studies to argue that the rule is unnecessary. The fact that felony deaths are low probability events, however, proves little. First, the relevant issue is not whether the probability of death during felonies is low. After all, the probability of death during an airplane flight is low, but few support abolishing airline safety regulations. The better question to ask, as Justice Holmes suggested, is whether “deaths happen disproportionately often in connection with other felonies.”²⁹ Looking at data on robberies from 1973-1998, the probability of homicide during a robbery (0.38 percent) is over 49 times greater than the probability of homicide absent a robbery (0.0077 percent).³⁰ Second, the studies cited in the Model Penal Code collected data from jurisdictions that have a felony-murder rule. As such, the findings (that there are few deaths during felonies in these jurisdictions) are consistent with the hypothesis that the felony-murder rule substantially reduces felony murders.³¹ Finally, although the number of felony deaths is small compared to the number of felonies, it is rather large when compared to the overall number of murders and nonnegligent manslaughters. Between 1970 and 1998, there were on average 19,921 murders and nonnegligent homicides annually. Each year nearly 19 percent of these, or 3812, occurred during crimes confirmed to be felonies.³²

²⁸ ALI, 201.2 *MPC* at 38 n 96 (cited in note 2). The death rate for other felonies is even lower.

²⁹ Oliver Wendell Holmes, *The Common Law* 59 (Little, Brown 1951).

³⁰ The probability of homicide during a robbery is calculated by dividing robbery homicides by robberies. The probability of homicide absent a robbery is calculated by dividing total murders not due to robbery by population. The probabilities reported are weighted averages of the probabilities in each state between 1973-1998, with state populations as weights.

To be precise about calculating the relative odds of death during a robbery, one would also have to hold units of time constant. One would want to compare the probability of death during a one-hour robbery to the probability of death during an average hour during which a person was not being robbed. This would substantially increase the relative odds of death during a felony calculated in the text because that number compared the probability of death during a (short lived) robbery to the probability of death during all the other hours in a day. If the average robbery were one hour long and each person were robbed once a year on average, a more accurate estimate of the relative odds of death during a robbery would be $0.38/(0.0077/8759) = 429,191$, where 8759 is the number of hours in a year minus a one hour robbery.

³¹ Even statistics on felony-murder probabilities based on national data, such as those in Philip J. Cook, *Robbery Violence*, 78 *J of Crim L & Criminology* 357, 358 (1987), are misleading because most states retain the felony-murder rule.

³² These felony numbers exclude the felonies of murder, manslaughter, and assault.

II. DATA

A. Research Design.

The goal of this paper is not merely to estimate the overall effect of the felony-murder rule on felony murders, but to determine the chain of causation that leads to this final result. Such an analysis would not only enable one to test the specific predictions by proponents and opponents of the rule, but would also shed light on criminal behavior in a manner that may be useful for predicting the effects of other criminal sanctions. The basic approach is to decompose the total number of deaths during a given type of felony into its constituent parts and to estimate the effects of the felony-murder rule on each. Selected properly, these constituent parts would shed light on how the felony-murder rule operates.

For example, the number of felony-murder incidents, F_D , in a state may be decomposed into the number of felonies, F , times the share of felonies that end in death, F_D/F —that is, $F_D \equiv F \cdot [F_D/F]$. Taking the log of this identity and then differentiating each side with respect to changes in the felony-murder rule (R),³³ one gets

$$\partial \log[F_D]/\partial R = \partial \log[F]/\partial R + \partial \log[F_D/F]/\partial R \quad [1]$$

If, in addition to simply regressing $\log[F_D]$ on an indicator for the felony-murder rule and explanatory variables, one regresses $\log[F]$ and $\log[F_D/F]$ separately on the indicator and other variables, one can determine whether the rule, say, reduces felony deaths because it makes felons take more caution during crimes, a lower F_D/F , or because it reduces the crime rate, a lower F .³⁴

Another useful decomposition exploits the fact that the number of felony deaths (D) also equals the number of felonies that end in death times the number of deaths per felony

³³ Of course R is a discrete variable so technically one cannot take derivatives of the continuous variables D and F with respect to R . It would be more appropriate to use “ Δ ” instead of “ ∂ ,” but for ease of exposition, I will use “ ∂ ” and speak of derivatives instead of changes.

³⁴ It might appear that a more direct estimate of the effect of the rule on deaths per felony (D/F) might be to add felonies (F) as an independent variable to the regression of felony deaths (D) on the rule (R). As it turns out, this will simply produce a coefficient on the rule that is identical to the coefficient on the rule in a regression with deaths per felony (D/F) as the dependent variable. To see this, note that the proposed regression equation for D is $\log[D] = \log[F] + \beta X + \varepsilon$, where X represents the explanatory variables, while a combination of the decomposition $\log[D] \equiv \log[F] + \log[D/F]$ and the regression equation for D/F , $\log[D/F] = \gamma X + \nu$, yields the regression equation $\log[D] = \log[F] + \gamma X + \nu$. A contradiction results unless $\beta X + \varepsilon = \gamma X + \nu$, or after taking expectations, $\beta = \gamma$.

that ends in death (D/F_D), i.e., $D \equiv F_D \cdot [D/F_D]$. D/F_D measures whether, because felons take more care during their crimes, fewer felonies involve multiple deaths.

A third, more direct decomposition uses the fact that crime data disaggregate felony murders by weapon used. This permits estimation of the effect of the felony-murder rule on gun use. Presumably, more careful felons are less likely to use guns.

A final decomposition, of felonies (F) into felonies with death (F_D) and without death (F_{ND}), is a check on spurious correlations. Neither F_D nor F_{ND} cleanly test the intensive (care) or extensive (activity levels) margins of criminal response to the felony-murder rule. F_D may fall either because criminals reduce activity levels (F) without increasing care (F_D/F) or vice versa. F_{ND} may either fall because activity levels (F) fall, or rise because felons take more care (F_D/F) without reducing activity levels (F). However, if, for example, F and F_D/F fall, but F_D does not, one might suspect that the relationship between the rule and F and F_D/F cannot be trusted. Likewise, if F_D/F alone falls, but F_{ND} does not rise, then one might suspect that the effect of the rule on F and F_D/F is spurious. This is an important check because F and F_D/F are constructed variables, while F_D and F_{ND} are directly observed. F is constructed by adding F_D and F_{ND} , while F_D/F is created by taking the ratio of F_D and F ; F_D and F_{ND} come directly from FBI data.³⁵

The empirical strategy used to estimate the marginal effect of the felony-murder rule on crime rates is that typically employed in the economics of crime and criminology literatures. The data set is a panel of annual, state-level observations covering 29 years, 1970-98. Logged crime rates are regressed on an indicator for the rule; on explanatory variables that control for the probability of arrest and likely sentence, for economic conditions in the state, and for demographics; and on state and year fixed effects that control for cross-sectional and time-series omitted variables, respectively. Crime rates and most explanatory variables are logged. The remaining variables are dummies or expressed in percentages (0-100). The next section describes the data in detail. Observations are

³⁵ Another advantage regressions on F_{ND} have is that there are fewer missing observations on this variable than F . F_{ND} is available for every state and year from 1970-98.

weighted by annual state population, the regression error terms are assumed heteroskedastic across states, and robust (White) coefficient standard errors are calculated.

B. Overview of the Data Set

Tables 3 and 4 set out summary statistics for all crime rate and non-crime rate variables, respectively. Crime figures are expressed in rates per 100,000 people. Probabilities are expressed in percentage terms and run from 0 to 100. All monetary values are expressed in 1967 dollars.

Data on state crime rates are based on the number of crimes reported to police over the course of a year, as compiled annually by the Federal Bureau of Investigation (FBI) in its *Uniform Crime Reports* (UCR) and *Supplemental Homicide Reports* (SHR).³⁶ Felony rates are based on data for five Index I crime categories: robbery, forcible rape, burglary, motor vehicle theft, and larceny.³⁷ Homicide rates for specific felonies are based on homicides reported in the SHR as occurring under circumstances of that particular felony. Difficulties with using UCR and SHR data are discussed in the Appendix.

Arrest data are available from the UCR,³⁸ while data on persons sentenced to death row, prisoners executed, and prison populations are gathered by the Bureau of Justice Statistics (BJS).³⁹ Arrest rates for specific felonies are calculated by dividing, at the state-

³⁶ The UCR data for this paper were obtained from Thomas B. Marvell and Carlisle E. Moody, *The Lethal Effects of Three-Strikes Laws*, 30 J Legal Stud 89 (2001) (data available at <<http://faculty.wm.edu/cemood/research.html>>). This data can also be found at the Bureau of Justice Statistics (BJS) website, <<http://www.ojp.usdoj.gov/bjs/datast.htm>>. The SHR data for 1970-1975 and 1976-1998 are drawn from Marc Riedel and Margaret Zahn, *Trends In American Homicide, 1968-1978: Victim-Level Supplementary Homicide Reports (ICPSR 8676) [Computer file]*, compiled by Center for the Study of Crime, Delinquency, and Corrections, Southern Illinois University, Carbondale, ICPSR ed. (Inter-university Consortium for Political and Social Research [producer and distributor] 1994), and James Alan Fox, *Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976-1998 (ICPSR 3000) [Computer file]*, ICPSR ed. (Northeastern University, College of Criminal Justice [producer] 2000) (ICPSR [distributor] 2000), respectively.

³⁷ Precise definitions of these crimes can be found in numerous articles, see, e.g., Steven D. Levitt, *The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation*, 111 Q J Econ 319, 348-49 (1996), or directly from the FBI, see *Crime in the United States: 1993* 380-81 (FBI 1994). The distinctions between FBI definitions of robbery, burglary and larceny are as follows. Robbery is the taking of property from the control of a person with force, while larceny is the taking of property without force. Burglary is merely the entry of a structure to commit a felony. The FBI definitions of these crimes may differ from the state definitions applicable in felony-murder statutes. See, for an example of this problem from a different context, *Taylor v United States*, 495 U.S. 575, 599-602 (1990) (reconciling definition of burglary in the Armed Career Criminal Statute and those in various state statutes). For simplicity and because the differences are likely to have negligible effects, I ignore them.

³⁸ UCR arrest data for 1970-77 and 1978-98 were obtained from Thomas F. Pogue, *Deterrent Effects of Arrests and Imprisonment in the United States, 1960-1977 (ICPSR 7973) [Computer file]*, 1st ICPSR ed. (Inter-university Consortium for Political and Social Research [producer and distributor], 1997), and David Mustard, respectively.

³⁹ BJS capital sentencing data were drawn from U.S. Department of Justice, BJS, *Capital Punishment in the United States, 1973-1998 (ICPSR 2977) [Computer file]*, compiled by the U.S. Dept. of Commerce, Bureau of the Census, ICPSR

level, arrests by offenses.⁴⁰ Capital sentencing and execution rates are calculated by dividing sentences and executions by the number of murders plus non-negligent homicides.⁴¹ Gaps are filled in via linear interpolation. Prison population is measured per 100 thousand population. Arrest rate data capture the probability of apprehension, while capital sentencing and execution rates capture the severity of punishment upon apprehension.⁴² Prison population rates capture a little of both. Together these variables measure expected punishment.

Whether a state has a felony-murder statute in any given year is captured by an indicator variable that registers as either one or zero. In the regression analysis I use two versions of this variable. The first takes a value of one if the state, in its homicide statute, punishes accidental homicide during a felony as first-degree murder, and zero otherwise. The second takes a value of one if the state punishes accidental homicide during a felony as first or second degree murder, and zero if it punishes such death as manslaughter or not at all.⁴³ Looking at a state's homicide statute is an easy way to estimate a state's felony-murder rule. This approach, however, is subject to some degree of measurement error because state courts often modify their state's felony-murder statute either by interpreting it in a non-literal way, constructively filling in gaps in the statutory language, or by limiting the statute to accord with state constitutional law or general criminal jurisprudence. Nevertheless, such modifications often take place at the margins of a state's felony-

version (Inter-university Consortium for Political and Social Research [producer and distributor], 2000). BJS executions data for 1968-76 and 1977-98 were obtained from Thomas B. Marvell and Carlisle Moody, *The Impact of Out-of-state Prison Population on State Homicide Rates: Displacement and Free-Rider Effects*, 36 *Criminology* 513 (1998) (data available at <<http://faculty.wm.edu/cemood/research.html>>) and <<http://www.ojp.usdoj.gov/bjs/cp.htm>>, respectively. BJS prison population data are also from Marvell and Moody, 30 *J Legal Stud* 89 (data available at <<http://faculty.wm.edu/cemood/research.html>>) (cited in note 36).

⁴⁰ Arrest rates greater than one are capped at one.

⁴¹ While capital sentences and executions can be broken down by specific types of felony murder, disaggregated rates are not possible for many states for many years because those states have zero relevant felony murders in those years.

⁴² Data on state felony prosecutions and sentences are not included because they are only available for a small subset of the 29 years covered in my data set. See, e.g., the National Center for State Courts and BJS websites, at <<http://www.ncsc.dni.us/ncsc.htm>> and <<http://www.ojp.usdoj.gov/bjs/sent.htm>>, respectively.

⁴³ Regressions were run with more precise indicators of each state's felony-murder rule, that is, with separate indicators for first-degree murder, second degree murder, and manslaughter. The signs of the coefficients on the detailed rule indicators are roughly consistent with the findings in Tables 7 and 8, although few of these coefficients are statistically significant. With so few states mitigating or abolishing the felony-murder rule, breaking down the general indicator for how a state punishes accidental felony homicides into indicators for the precise classification of such deaths lowers the variance in the indicator variables, and hence reduces the precision of any subsequent estimates of the coefficient on the indicators.

murder rule—they rarely alter, for example, the classification of accidental death during a felony as murder or manslaughter.⁴⁴ Since the felony-murder indicator variable only measures this classification, looking to state statutes instead of state case law should not introduce serious measurement error.

The remaining variables⁴⁵ in the data set are those typically used in state-level studies of crime. Demographic variables are the percentage of population ages 15-19, 20-24, and 25-29, i.e., the groups with the highest arrest rates; the percentage of the population that is African American; and the percentage of population that resides in metropolitan areas. Economic variables are the unemployment rate, the employment rate, real personal income per capita and the poverty rate.

C. Felonies Examined

The analysis in this paper focuses on robberies, rapes, burglaries, larcenies and auto theft. Although rapes (36 per 100 thousand population) and robberies (220 per 100 thousand population) are among the least common of these felonies, they are among the more common circumstances for felony murder at 0.06 and 0.74 per 100 thousand population, respectively. Auto theft and larceny are the opposite, very common felonies (524 and 2971 per 100 thousand population, respectively) and very uncommon circumstances for felony murder (0.01 per 100 thousand population each). Unlike the others, burglaries are both very common felonies (1249 per 100 thousand population) and relatively very common circumstances for felony murder (0.08 per 100 thousand).

Burglaries, rapes and larcenies tend to be underreported, but such underreporting should not bias the regression results so long as the underreporting is not correlated with any of the explanatory variables in my model. It is safe to assume that burglaries, rapes

⁴⁴ Michigan is the only state whose courts changed its classification of accidental death during a felony such that the actual status of the felony-murder rule in that state is not as its homicide statute suggests. Michigan's Supreme Court struck down its felony-murder statute in 1980. As a result, accidental death during a felony triggers no special punishment in Michigan. *People v Aaron*, 409 Mich 703, 728, 299 NW2d 304, 314 (1980).

⁴⁵ These are drawn from Marvell and Moody, 30 J Legal Stud 89 (data available at <<http://faculty.wm.edu/cemood/research.html>>) (cited in note 36). The demographic data was originally gathered by the Bureau of the Census, and the economic variables are from the Bureau of Labor Statistics (unemployment) and the Bureau of Economic Analysis (employment, personal income).

and larcenies that result in death are no less likely to be reported in states that have a felony-murder rule than in those that do not because deaths related to felonies are unlikely to be immediately dismissed as completely unintentional. It is plausible that local police might systematically fail to report purely accidental deaths that occur during a felony if their state has not felony-murder, but also very unlikely given the special attention police pay to homicides in all states.

State felony-murder statutes do not always cover larcenies and auto thefts.⁴⁶ However, most burglaries are committed for the purpose of, and thus followed by, larceny.⁴⁷ So, if a statute covers burglary-deaths, it also implicitly covers larceny deaths because death during a larceny will usually be attributable by prosecutors to a preceding burglary. Similarly, auto thefts that result in death often qualify as either robbery murders, because the death results while taking the car from the possession of the victim, or carjacking, which is often covered in felony-murder statutes.⁴⁸

This paper does not explore deaths during drug crimes, arson and kidnapping. All of these crimes are covered by the typical felony-murder statute. Moreover, after robbery murder, drug crime murders are the most common (0.41 per 100 thousand population) felony murders. However, data on deaths during drug crimes, arson and kidnapping are not available for many years. And, in the case of drug crimes, deaths during the felony are covered by the federal felony-murder statute.⁴⁹ State rules probably do not add much in the way of marginal deterrence and thus the variation in state felony-murder rules would likely have little effect on drug crime rates.

⁴⁶ It should be noted that it is not altogether clear that police reports of the felonies accompanying homicides follow the same hierarchical reporting system as the police reports of the primary offense. So, for example, while a burglary and larceny accompanied by a homicide are reported in the UCR as a murder, it is unclear whether they are reported in the SHR as a burglary or a larceny.

⁴⁷ Of all the observations in the sample, only Tennessee and Michigan from 1977-1979 had statutes that punished all homicides during larcenies as murder. No other states that enumerate those felonies during which homicide liability is strict list larceny as such a felony. The remaining states' courts generally rule out felony-murder liability during a larceny by applying the "inherently dangerous felony" limitation on the scope of the rule. See text accompanying note 4.

⁴⁸ See, e.g., Fla Stat Ann § 782.04(1)(a)(2)(l) (West 1998).

⁴⁹ 18 USC § 1111 (1970).

D. An Initial Comparison of Data from States With and Without the Rule

As a first step in determining the effect of the felony-murder rule, Table 5 compares the difference in average crime rates in states and years with and without the rule (defined as states that do and do not punish felony murder as first-degree murder, respectively) during the 1968-98 period. The last column indicates whether the differences in means across the two types of states are statistically significant. This simple, predominantly cross-sectional comparison of means does not reveal any clear pattern of differences between felony-murder rule and non-felony-murder rule jurisdictions. Moreover, most differences are not statistically significant. Table 6 may be more informative. It presents the change in average felony-murder rates in the states that changed the felony-murder rule (defined as states that at some point punished felony murder as first-degree murder). This longitudinal comparison of means better controls for unobserved differences between states. Note, however, that roughly half the changes yielded an increase and half the changes a decrease in the average felony-murder rate, with only one of the changes statistically significant.

These ambivalent results are obviously derived without controlling for important factors, such as the probability of apprehension or expected sentence and the economic circumstances of the population in a jurisdiction. Fortunately, regression analysis permits a more refined analysis. The next section demonstrates, however, that additional rigor merely confirms the initial findings from a simple comparison of means.

III. RESULTS

A. Preliminary Matters

Before presenting the primary results of the paper, I address three topics that bear on the interpretation of those results—the extent of variation in the felony-murder rule, the endogeneity of that rule, and the frequent zero values for certain crime rates.

Between 1970-98, only eight states ever punished accidental homicide during a felony less severely than second-degree murder (or murder two). Of these, one (Wisconsin) never imposed a punishment more severe than manslaughter and another (Ohio) imposed a

punishment more severe than manslaughter for only the last year of the sample, further limiting longitudinal variation. An additional complication is that not all crime rates are available for all dates. Some are available from 1970, some from 1973 and the rest from 1976 onward. Because some states changed their felony-murder rules before 1973 or 1976, this data limitation further constrains longitudinal variation.

The situation is not hopeless, however. If states that punish felony death less severely than first-degree murder (or murder one) are compared to the rest, sixteen states fall into the former group. This allows for 16 cross-sectional comparisons after 1970, 11 after 1973, and six after 1976.⁵⁰ While this variation is not ideal, it is acceptable and, more importantly, the most available in the history of the felony-murder rule.

As with the evaluation of any legal change, endogeneity in the variable that measures the law is a potential problem. States with low felony-death rates may be those most willing to weaken their felony-murder rule. Such causation would violate the standard ordinary least squares assumption that explanatory variables are independent of the error term and would introduce bias into the coefficient on the felony-murder rule. Fortunately, there are strong reasons to suppose that endogeneity is not a problem in this paper.

First, most states that mitigated the felony-murder rule did so between 1972 and 1980 as part of a wholesale reform of their criminal statutes. Typically these reforms were triggered by the Supreme Court's 1972 capital punishment ruling, *Furman v. Georgia*.⁵¹ Thus, to the extent that states were motivated to modify their felony-murder statutes by crime data, the relevant statistic was the overall crime rate, not the felony-homicide rate.

The second reason endogeneity is likely not a problem is that state-level statistics on felony-murder rates were not readily available during the period most states mitigated their felony-murder rule. True, the Model Penal Code (MPC) published statistics in 1958 and 1980.⁵² However, the first publication was well before the period analyzed in this pa-

⁵⁰ The corresponding numbers if states that punish felony death as murder one or two are compared to the rest are seven (1970), six (1973), and three (1976).

⁵¹ 409 U.S. 902 (1972). Jo Anne C. Adlerstein, *Felony-Murder in the New Criminal Codes*, 4 Am J Crim L 249, 250 (1975-76).

⁵² ALI, 201.2 MPC 40 (ALI, Tentative Draft No 8, May 9, 1958); ALI, 210.2 MPC Comment 38 n 96 (Official Draft and Revised Comments 1980).

per and the second came after all states that would mitigate their rules had already done so. In any case, the data were geographically limited in scope.⁵³ The FBI published crime data throughout the period during which states were reforming their felony-murder statutes.⁵⁴ However, the data on felony-murder were not broken down by state. The Supreme Court cited robbery-murder statistics in *Enmund v. Florida*,⁵⁵ a 1982 decision proscribing capital punishment for a co-felon who was involved in a homicide but was not the triggerman. Again, the data does not appear to be disaggregated to the state-level. Philip Cook published a study on robbery murder in 1980, but this did not include disaggregated data and was after most states reformed their rules.⁵⁶ Finally, data from the FBI's Supplemental Homicide Reports were not processed for easy consumption until 1994.⁵⁷

Third, to be safe I ran a probit regression⁵⁸ of each state's felony-murder rule on various felony-murder rates, lagged once and twice, and on the other explanatory variables in Table 4, lagged once. These regressions, and variations on them, suggest that felony and felony-death rates had little effect on states' decisions to mitigate their felony-murder rules.⁵⁹ In the rare case where the coefficients on these crime rates were significant, the rule had an effect on par with that of the unemployment rate. Not surprisingly, I found that the number of capital sentences or executions in a state is the best predictor of the felony-murder rule; the coefficient in that variable was consistently significant and large in magnitude. This lends support to the view that the capital punishment debate drove the major overhauls in state criminal statutes; changes in a state's felony-murder rule were probably included on the reform agenda as an afterthought.

A final concern with the data is the prevalence of zero-valued observations on the dependent variable. While murder is a rare incident, felony murder is rarer still. When

⁵³ Cook County, Illinois, 1926-27; Philadelphia, 1948-52; New Jersey, 1975. None of these jurisdictions mitigated their felony-murder rule.

⁵⁴ See, e.g., U.S. Dept. of Justice, FBI, *UCR* 13, 17 (1981).

⁵⁵ 458 U.S. 782, 800 (1982).

⁵⁶ P. Cook, *The Effect of Gun Availability on Robbery and Robbery Murder*, in R. Haveman and B. Zellner, 3 *Policy Studies Review Annual* 743, 747 (1980).

⁵⁷ Riedel and Zahn, *Trends In American Homicide, 1968-1978 [Computer file]* (cited in note 36)

⁵⁸ All regressions in this paper were run using Stata version 6.

⁵⁹ The regression allowed for groupwise heteroskedasticity across states.

broken down by felony, many states report zero incidences for many years. In a log-linear regression model, these observations drop after taking logs, raising the risk of selection bias. The econometric literature discusses many solutions to this problem;⁶⁰ the one I pursue is a compromise between simplicity and rigor. Tables 7 and 8, which include the primary results of the paper, present coefficients on the felony-murder rule from regressions of the log of crime rates on a dummy for the rule and other explanatory variables. In those regressions, if a state reports zero crimes in a given year, it is assumed to have reported 0.1 crimes. This imputation occurs before dividing by the state population or taking logs. In cases where the crime rate is a ratio, the imputation differs. For the share of felonies that end in death, states that report zero felony homicides are imputed 0.1 felony homicides. (No state reports zero felonies in any year.) For deaths per felony resulting in death, the ratio is imputed to be one where the reported ratio is below one or a state reports no felonies resulting in death. This sort of imputation is common in the empirical crime studies literature.⁶¹

⁶⁰ See, e.g., John Mullahy, *Much ado about two: reconsidering retransformation and the two-part model in health econometrics*, 17 *J Health Econ* 247 (1998); Willard G. Manning and John Mullahy, *Estimating Log Models: To Transform or Not to Transform*, NBER Working Paper 0246 (1999).

⁶¹ See, e.g., John R. Lott and David B. Mustard, *Crime, Deterrence, and Right-To-Carry Concealed Handguns*, 26 *J Leg Stud* 1 (1997). It should be noted that imputing 0.1 in place of zeroes typically has a low rate of return in terms of generating additional observations in regressions that control for arrest rates or sentencing. The reason is that states that, for a specific crime, report zero offenses have zero arrests or sentences. I solve this by imputing arrest rates that are missing for this reason via linear interpolation.

To be sure that the 0.1 for 0 imputation does not produce biased results, I compared the coefficients on the felony-murder rule from a number of different estimation strategies designed to control for bias due to the omission of zero-observations on the dependent variable in a log-linear model. I applied each corrective device to regression equations for the four most informative crime rate variables (felony-murder victims (D), incidents of felony murder (F_D), incidents of the felony without death (F_{ND}), and victims per felony-murder incident (D/F_D)) for each of the three most important felonies (robbery, rape and burglary) explored in this paper. I also considered two versions of each regression, first where the felony-murder indicator captures states that punish felony murder as murder one, and second where the indicator captures states that punish felony murder as murder one or two. The four estimation strategies examined are (1) an ordinary log-linear regression, where zero observations, like missing observations, are dropped because crime rates are logged; (2) a log-linear regression where zero observations are adjusted as in Tables 7 and 8; (3) a Poisson regression; and (4) a Heckman two-step estimator designed to adjust for selection bias. In all specifications, explanatory variables are the same as in Tables 7 and 8. Observations are weighted by annual state population. White standard errors are employed.

Specification (1) (as compared to other specifications) reveals the extent of bias and loss of precision in the coefficient on the felony-murder rule introduced by dropping zero crime rate observations. Specification (3) avoids the zero dependent variable problem, intuitively, by estimating the equation $y = \exp(x\beta) + u$ rather than $\log(y) = x\beta + \varepsilon$. The difficulty with comparing the ordinary or adjusted log-linear specifications with the Poisson is that the error structures are different. Specification (4) avoids the zeroes problem by estimating a selection equation that determines whether a state experiences zero or positive crime rates, followed by a second-stage equation of the positive crime rates on explanatory variables, but with a factor, derived from the selection equation, that adjusts for any bias introduced by selecting out zero crime rate observations. See James Heckman, *Sample Selection Bias as a Specification Error*, 47 *Econometrica* 153 (1979). The same covariates are employed in the first and second stage equations. The assumptions of the Heckman model plausibly fit the zero crime rate problem: once expected punishment or economic wealth exceeds a certain level, the return to crime or de-

B. Summary of Results

As mentioned above, Tables 7 and 8 lay out the primary results of the paper. They present the effects of the felony-murder rule on crime rates for five felony categories and all felonies aggregated. The difference between the two tables is that Table 7 compares states that punish felony murder as first-degree murder with the rest, while Table 8 compares states that punish felony murder as either first or second-degree murder with the rest. In each table the rows list the felony categories: robbery, rape, burglary, auto theft, larceny, and all felonies. The columns list the specific crime rates: total felony deaths (D), felony deaths due to gunshot (D_G), total felonies (with or without death) (F), felonies with death (F_D), felonies without death (F_{ND}), share of felonies with death (F_D/F), and deaths per felony with death (D/F_D). The first number in each cell is the coefficient on a felony-murder rule—defined as punishing accidental homicide during a felony as murder one in Table 7 and murder one or two in Table 8—from a regression of the log of the crime rate specified by the row and column headings on an indicator for the rule and the explanatory variables in Table 4.⁶² The number in parentheses below each coefficient is the standard error of that coefficient. The third number is the number of total observations in that regression. The final number is the R^2 from the regression. The number of comparisons varies across regressions because data on different crime rates for different felonies span different periods. Regressions on D and D_G span 1973-98 for specific felonies, and 1970-98 for all felonies aggregated. Regressions on F, F_D , F_D/F , and D/F_D span 1976-98, while regressions on F_{ND} span 1970-98, regardless of felony category.

The first rows of Tables 7 and 8 present the effects of the felony-murder rule on robberies. States that punish felony murder as murder one appear to have a 17 percent (coef-

mand for extralegal employment opportunities vanishes, and further increases in punishment or wealth continue to produce the same zero crime rates. (Of course one must be careful that zero crime rates are not actually the product of underreporting of crime rates. Such underreporting may necessitate the inclusion of different explanatory variables in the selection equation or the rejection of the Heckman estimator if underreporting, for example, produces positive as well as zero crime reports.)

I found that the results reported in Tables 7 and 8 are robust to the specific procedure employed to compensate for the zeroes problem therein and are robust to method of estimation generally. The results from estimation strategies other than the one employed in Tables 7 and 8 are available from the author upon request.

⁶² Each regression includes the arrest rate for murder and the felony at issue. The regression for all felonies includes only the arrest rate for murder. All regressions include year and state fixed effects.

ficient of 0.16)⁶³ higher share of felonies that end in death than states that do not punish felony murder as murder one. Comparing states that punish felony death as murder one or two with the rest produces similar, but more substantial and significant, results. Most notably, the share of robberies that result in death (F_D/F) and even the number of robberies without death (F_{ND}) are higher, by 31 and 12 percent (0.27 and 0.11), respectively. The result is that the number of victims (D) is 31 percent (0.27) higher in states with the harsher punishment.

The second rows of the two tables examine the effect the felony-murder rule on rape crime rates. The rule appears to have little effect on rape statistics. States that punish felony death as murder one or two appear to have 30 percent (0.26) more deaths from rape-murder that are attributable to gunshot (D_G) and a 14 percent (0.13) higher share of felonies that result in death (F_D/F), but these relationships are significant only at the 85 percent level. That the rule does not affect the number of rape-murder victims or incidents may not be surprising. With the exception of serial rapists, individuals that rape probably do not view rape as a career in the way robbers and burglars view their felonies as careers; thus they have less incentive to monitor their state's felony-murder rule.

The third rows of Tables 7 and 8 concern burglaries. Whereas the felony-murder rule potentially has perverse effects on robberies, it has more beneficial effects on burglaries. States that punish felony homicide as first-degree murder experience eight percent (0.08) fewer victims per burglary-homicide incident (D/F_D). Although the number of victims in multiple-victim burglary-murders is lower in these states, it is unclear whether the number of burglary-murder incidents is also lower. The coefficient in Table 7 from the regression involving the share of burglaries with a fatality (F_D/F) is negative, but not significant. Similarly, the number of victims of burglary-murder (D) is lower, but not significant. Examining states that punish felony homicide as either murder one or two produces similar results. The coefficient on total victims is positive, but very small and insignificant.

⁶³ Interpretation of the coefficients in Tables 7-9 requires care. If the coefficient on the dummy for the rule is positive (negative) and X , then the effect of the rule is to increase (reduce) crime rates by e^X percent. This is because the dependent variable is logged while the variable representing the rule is simply a 0-1 indicator variable. For low values of X , say below 0.25 in absolute value, X is, however, a good approximation for e^X .

The fourth rows examine auto theft. Comparing states that punish felony death as murder one with the rest, the states with the rule appear to have 9 percent (-0.09) fewer auto thefts (F), a 50 percent (-0.7) lower share of auto thefts that prove fatal (F_D/F). These states do not have a statistically significantly lower number of deaths due to auto theft-murder (D); perhaps this is because so few auto thefts result in death (0.003 percent of all auto thefts, with 523 auto thefts reported per 100 thousand population). Comparing states that punish felony death as murder one or two with the rest, however, reveals more dramatic results. As before, states with a harsher rule experience 9 percent (-0.09) fewer auto thefts (F). However, their share of their auto thefts that end in death (F_D/F) is 63 percent (-0.99) lower. Moreover, these state have 61 percent (-0.95) fewer victims of auto-theft murder (D).

The fifth rows address larceny. States that punish larceny-related death as murder one have three percent (-0.03) fewer deaths per larceny murder (D/F_D). These states also appear to have a lower share of larcenies that result in death (F_D/F), but that relationship is far from statistically significant. States that punish larceny-related death as murder one or two have 21 percent (-0.24) lower share of larcenies that result in death (F_D/F) and four percent (-0.04) fewer victims per larceny-murder incident (D/F_D). On the other hand, the number of these victims that die from gunshot wounds (D_G) is 25 percent (0.22) higher.

The final rows of Table 7 and 8 document the relationship between the felony-murder rule and aggregate felony statistics, i.e., the sum of all felonies. For all the regressions in this row, total felonies means all the felonies in the first five rows of the table. For some of the regressions in this row (D, D_G , F_D , and D/F_D), total felonies also includes all other crimes classified as felonies except murder and assault. Two features of the total felonies regressions stand out. First, the number of victims in multiple-victim felony-murder incidents (D/F_D) falls by six to seven percent (0.06 to 0.07). Second, the effect on the total number of deaths from felony murder (D) is very small and insignificant. This non-result should not be surprising: the rule has nearly opposite effects on robberies, on the one hand, and burglaries, auto thefts, and larcenies, on the other. While the rule has a benefi-

cial effect on the latter set of felonies, robbery murders account for a much higher share of all felony murders (50 percent) than the others (0.5 to 5 percent).

The results in Tables 7 and 8 are fairly robust. The results survive, in sign, magnitude and statistical significance, variations in the set of explanatory variables, such as omission of execution numbers, capital punishment sentence rates, arrest rates, etc. The signs and magnitudes of coefficients on the rule are substantially similar to those in regressions on only a subsample of observations from states in the Midwest (including Kentucky and Minnesota, which are adjacent to that region). The reason for focusing on the Midwest is that five of the eight states that have punished felony death as manslaughter or not at all—Michigan, Wisconsin, Ohio, Kentucky, and Minnesota—are in or adjacent to the Midwest. Looking just at significant coefficients, the results in Tables 7 and 8 generally survive a specification where crime rates are regressed on explanatory variables, including the felony-murder rule dummy, lagged once. Finally, in nearly all cases, the signs of coefficients on the felony-murder rule indicator in Table 7 match a specification where no variables are logged.⁶⁴

C. Interpretation of Results

1. Comparing Predictions and Results

Table 9 compares the results from Tables 7 and 8 with the predictions of proponents and opponents of the felony-murder rule. Again, the columns list the specific crime rates: total felony deaths (D), felony deaths due to gunshot (D_G), total felonies (with or without death) (F), felonies with death (F_D), felonies without death (F_{ND}), share of felonies that end in death (F_D/F), and deaths per felony with death (D/F_D). The first two rows list the sign of each crime rate as predicted by advocates and detractors of the rule. The remaining six rows reproduce in part Tables 7 and 8, listing the coefficients on the felony-murder rule dummy variable from Tables 7 and then 8 from regressions on the crime rates listed in the columns for the felonies listed in the rows. For example, the second term in the sixth row,

⁶⁴ A table with these regression results is available from the author upon request.

fourth column is the coefficient on the rule in a regression of auto theft-murder incidents (F_D) on the rule in states that punish felony murder as murder one or two, other explanatory variables, and year and state fixed effects. Asterisks indicate whether the coefficients are significant; for ease of reading, standard errors, number of observations, and number of longitudinal comparisons are omitted.

Proponents of the felony-murder rule predict that it will reduce the number of felonies and increase the care offenders take during felonies. The first effect should lower total felonies (F). The second effect should reduce the share of felonies that result in death (F_D/F), and the number of deaths per felony resulting in death (D/F_D).⁶⁵ F_D/F tracks the margin between felonies that do not result in death and those that do, while D/F_D tracks the margin between felonies that result in one death and those that result in multiple deaths. Both margins are affected by care taken to avoid fatal violence. Such care presumably involves avoiding the use of guns, and hence should also lower felony deaths due to gunshot (D_G). The effects on total felony deaths (D) should be negative.⁶⁶

Opponents of the rule predict that, with one exception, it will have zero effect. Criminals may not know about the rule, they may be irrational, or they may be unable to control the crime scene. Even if all this is untrue, the rule may actually have the perverse effect of increasing the number of victims per felony-murder incident (D/F_D). The reason is that the rule reduces the marginal punishment for intentional homicide after one accidental death. Assuming that police do not disproportionately focus their resources on multiple-victim felony murders, offenders can reduce their expected punishment by intentionally killing all witnesses after one accidental death.

⁶⁵ Proponents also predict that F_D should fall, but as a result of either reduced activity levels or greater care. F_D may fall as F falls, but F_D/F remains constant, or vice versa. The effects of a harsher rule on F_{ND} is ambiguous. It may rise if F is constant but F_D/F falls, or it may fall if F falls and F_D/F is either constant or also falls.

⁶⁶ Proponents of the rule also predict that it will reduce intentional homicide during felonies because it prevents felons from escaping punishment by recasting their intentional homicide as an accident. This effect is difficult to separate from reduced activity levels and greater care given the activity level. Eliminating the ability to evade punishment for intentional homicide during a felony may both reduce the number of intentional homicides during a felony and reduce the number of felonies. The former is expected if offenders draw independent benefit from the felony (without the homicide), while the latter is expected if offenders do not.

Looking mainly at the magnitudes of coefficients, the results for robbery are inconsistent with either the predictions of proponents or opponents of the felony-murder rule. States that punish robbery death more severely have higher share of robberies that prove fatal (F_D/F). States that punish robbery death as either murder one or two even have higher numbers of robbery deaths due to guns (D_G). However one measures the felony-murder rule, states with stiffer penalties for this crime report greater numbers of victims of robbery-murder. It appears that robbers, on average, take less care in jurisdictions with a harsh felony-murder rule.

Given that the felony-murder rule does not significantly increase the number of deaths per robbery with death, as opponents predict it would, the perverse effects of the rule are hard to explain.⁶⁷ Endogeneity bias has already been ruled out, see Part III.A. Omitted variable bias is minimized by including state and year fixed effects. One might suspect measurement errors in the UCR or SHR. For reasons discussed in Appendix B, however, this is an unlikely explanation for the odd results. Misreporting in FBI data is likely to affect robbery and homicide rates less than other felony rates. The FBI takes great efforts to correct or prevent many common reporting errors. Moreover, there is no obvious reason to suspect that crime rate measurement errors are correlated with any of the explanatory variables, and thus no reason to fear bias in the coefficient on the felony-murder rule. In any case, the fact that the results are robust to different specifications, samples, and estimation techniques should instill confidence that measurement errors cannot explain away the unexpected results.

Looking at burglary, auto theft and larceny, and just at signs, the results provide some support for proponents' theories. The share of felonies resulting in death (F_D/F) fall

⁶⁷ Perhaps a more robust theory of how criminals respond to penalties is necessary. For instance, suppose there are two types of robbers, one highly skilled and able to operate without causing death, the other low skilled, more careless and prone to accidents. If highly skilled robbers have better job opportunities outside robbery—in particular, if the wage from their next best job is closer to their wage from robbery than is the case for low skilled robbers, then an increase in the punishment for felony-related death may drive the highly skilled, but not the low skilled, robbers away from robbery. Not only might the average deaths per robbery rise, but with fewer highly skilled robbers competing for jobs, the number of robberies for low skilled robbers might rise, so that the total number of felonies do not fall. The result might well be an increase in total deaths from robbery. There are two problems with this theory. First, it is fairly complicated. It relies on rather strong assumptions that need to be empirically validated. For example, it assumes that opportunities for robbery are fixed and highly and low skilled robbers compete for the same jobs. Second, the theory is inconsistent, at least nominally, with the results for other property crimes. The rule tends to be associated with lower burglary, auto theft and larceny rates.

for all three crimes, and victims per incident (D/F_D) fall for burglaries and larcenies, suggesting greater care by felons. The number of felonies (F) also falls for auto thefts.

When one considers only statistically significant results, however, the proponents appear successful only at predicting changes in auto theft crime rates. Most burglary coefficients are not statistically significant, and those that are have small magnitudes: victims per felony-murder incident (D/F_D) fall eight to 11 percent, a small response further minimized by the fact that only 0.0063 percent of burglaries result in death. Some larceny coefficients are a bit more substantial (larceny-murder incidents (F_D) may fall up to 20 percent), but others are either small (victims per larceny murder (D/F_D) fall two to three percent) or statistically insignificant.

Stepping back and looking at the results as a whole, two features of the Table 9—the generally tiny magnitude of coefficients and the paucity of statistically significant results—lend support to opponents' arguments that criminals are not responsive to the felony-murder rule. The lack of significance can perhaps be attributed to poor quality data or insufficient variation in the felony-murder rule. I have tried to minimize these criticisms by compiling a large sample that includes virtually all major variations in states' felony-murder rules and a wide variety of control variables. To the extent that any identification of the effects of the felony-murder rule is possible, this paper is likely to achieve it.

2. Practical Effects of the Rule

Table 10 uses the results from Table 7⁶⁸ to present two counterfactuals, the change in crime rates if the states that did not punish felony death as first-degree murder in 1998 had done so, and vice versa. Because the number of states that punished felony death as murder one is far greater than the number of those that did not, the predicted changes are more dramatic for the counterfactual where the felony-murder rule is softened.

⁶⁸ These numbers are very similar to predictions from a two-part model, with a first stage probit giving the probability of a positive observation, which then is used to discount the positive-valued prediction from a second stand log-linear regression. See W. G. Manning, N. Duan, and W.H. Rogers, *Monte-Carlo evidence on the choice between sample selection and two-part models*, 35 *J Econometrics* 59 (1987) (suggesting that two-part models are better for prediction of actual responses).

Looking at deaths (D) from felony murder, if all states had punished felony death as murder one in 1998, my results suggest that 33 more individuals would have died. If no states punished felony death as murder one, 78 lives might have been saved. This effect is predominantly due to the higher level of robbery homicides in states with a harsh felony-murder rule. The relative magnitude of the effect, however, should not be exaggerated. There were 2882 felony deaths and 15,343 murders in 1998.

Turning to felonies (F), the picture is more clouded. Although there may have been 2599 and 9376 more robberies and larcenies, respectively, if all states punished felony death as murder one, there would have been 188 fewer rapes, 2144 fewer burglaries, and 24,035 fewer auto thefts. These figures appear large, but just as with the number of lives possibly affected by the rule, the number of felonies perpetrated or avoided is a small fraction of the total number of felonies in 1998. For example, there were over 401 thousand robberies and 1.1 million auto thefts in 1998.

CONCLUSION

Policymakers should draw one conclusion from this paper: the felony-murder rule does not substantially improve crime rates. If the main reason a state retains the rule is to reduce crime, it should reconsider the rule.⁶⁹ The rule seems to increase the number of felony deaths in a state. Although the rule reduces the rate of some felonies, this effect is small and can be easily replicated by increasing the penalty for these felonies. This conclusion emerges from among the best data available for analyzing the effects of the rule.

Legal scholars, criminologists and economists should consider the results presented in this paper further. The data suggest, first, that the felony-murder rule may have different effects on different felonies. It increases robbery crime rates and reduces burglary, auto theft, and larceny crime rates. The varied impacts of the rule suggest not only that penalties should be more narrowly targeted to specific classes of criminal activity, but also that different individuals engage in different sorts of crimes. Even robbers and burglars can-

⁶⁹ The trend in courts to limit the rule suggests that courts resolve the moral debate in favor of the rule's opponents. Tomkovicz, 51 Wash & Lee L Rev at 1433, n 16-17, 1465 n 146 (cited in note 19). This paper is even more relevant if, as some scholars claim, the felony-murder rule arose on accident. *Id* at 1443 n 60, 1444 n 64.

not be treated the same. Second, the data suggest that the felony-murder rule may produce surprising changes in criminal behavior. In particular, it is associated with higher numbers of robbery-murder incidents. This effect is hard to explain with existing theories of criminal response and warrants further exploration.

Finally, this paper demonstrates that empirical validation is an essential complement to blind theorizing about the likely consequences of criminal rules. Scholars have long debated the practical merits of the felony-murder rule without checking whether any of their theories could explain actual criminal behavior. This paper shows that their theories cannot. As crime data becomes more and more accessible, empirical testing of theories concerning the impacts of other controversial criminal policies should be more readily achievable. Such empirical testing should be the goal of conscientious policymakers.

APPENDIX: UCR AND SHR DATA

Both the SHR and UCR suffer from underreporting.⁷⁰ Police agencies on average do not submit SHR reports to the FBI for 2.4 months out of each year.⁷¹ In some cases this reflects the fact that there were no homicides during non-reported months. In other cases it reflects either a lack of care or an attempt to underreport homicides. While many methods of correction have been proposed,⁷² no method of correction clearly solves the un-

⁷⁰ Sometimes states fail to report SHR data for entire years. In particular, Florida failed to report in 1988-1991, Iowa in 1991, Kentucky in 1988, Maine in 1991-1992, Montana in 1987, 1993-1994, and North Dakota in 1989. In addition, two observations are lost because there were no data on how often Vermont agencies reported to the SHR in 1978-1979.

⁷¹ This is estimated by taking a weighted average of the number of months per year each local police agency submitted SHR reports during the period from 1977-1992, where the weights are the size of the population in each agency.

⁷² First, one can simply multiply SHR estimates by the ratio 12/(number of months reported). This is a linear upward adjustment treating nonreported months as the same as reported months. This will overestimate homicides if agencies tend not to report data in months with zero or few homicides.

Second, one could use UCR homicide numbers as an adjustment: multiply all state-level SHR numbers by the state-level ratio of total UCR homicides to total SHR homicides. Vickie Brewer, *Issues in the Use of Supplemental Homicide Reports*, in Carolyn Rebecca Block and Richard A. Block, *Questions and Answers in Lethal and Non-Lethal Violence 1993*, Proceedings of the Second Annual Workshop of the Homicide Research Working Group, June 13-17 1993, National Institute of Justice Research Report, (DOJ 1993), <<http://www.icpsr.umich.edu/NACJD/HRWG/proceed.html>> (visited Nov 11, 1999). The average ratio of UCR to SHR homicide numbers during 1976-1994 was 1.08, but this discrepancy is disappearing. Marc Reidel, *Sources of Homicide Data: A Review and Comparison*, in M. Dwayne Smith and Margaret A. Zahn, eds, *Homicide: A Sourcebook of Social Research* 75-95, 88 (Sage 1999).

Third, one could multiply all state-level SHR estimates upwards by the ratio of total state-level homicides estimated by the National Center for Health Statistics (NCHS) to total state-level homicides estimated by the SHR. NCHS data is based on death certificates and the results of autopsies by medical examiners rather than on police reports. It is estimated that by 1985 this program captured 99 percent of all births and deaths. Reidel, *Homicide: A Sourcebook* 83. The ratio of NCHS numbers to SHR numbers has been 1.17 on average since 1976. Moreover, there is substantial interstate variation in this ratio. Some states underreport by more than 50 percent. W.M. Rokaw, J. A. Mercy, and J.C. Smith, *Comparing Death Certificate Data with FBI Crime Reporting Statistics on U.S. Homicides*, 105 Public Health Reports 447-455 (1990). Geor-

derreporting problem.⁷³ This paper calculates state felony-murder rates by multiplying state-level SHR figures by the ratio of state-level UCR and SHR numbers.

However severe the underreporting problem, because crime rates are dependent variables in the analysis, measurement error from underreporting will not lead to bias in the parameter estimates unless the error is correlated with the explanatory variables. It is generally difficult to test for such bias. The typical method is to use the Hausman specification test. The problem with this method is that it requires at least one estimate of crime rates with measurement errors that are not correlated with the right-hand side variables. Of course if one had such a variable there would be no reason to use or test any other. This prevents me from testing whether the UCR data creates measurement error biases.⁷⁴ The SHR data, however, is another story. If one assumes that at least one of the various adjustments criminologists have suggested to correct for underreporting in SHR eliminates measurement errors, one could use the Hausman test to check every other ad-

gia (1.51), Montana (6.18), New Mexico (7.26), North Dakota (1.69), South Dakota (2.73) and Vermont (2.46) had the highest reported NCHS homicide to SHR homicides ratios. Rokaw et al, 105 Public Health Reports 447-455. Note that in none of these states is the felony-murder rule mitigated. This suggests that the deterrence effect of felony-murder rules may be overstated unless the SHR is somehow adjusted upwards. However, the overestimate is not likely to be large since, other than Georgia, none of these states have a high population.

A fourth method of adjustment is suggested by James Alan Fox, the principal investigator for the main data set used in this paper. Fox notes that when large agencies fail to report, it is probably not because of a lack of homicides. Therefore, data on the number of months reported should only be used to adjust estimates up (according to the first method) when the agency is large. Fox, *Uniform Crime Reports Codebook* at 1 (cited in note 36). Of course this creates problems regarding how to define large.

⁷³ The importance of these adjustments should not be exaggerated. One should not assume the NCHS data is perfectly accurate: one study found that NCHS underestimated the actual number of deaths in Washington state from 1981-1986 by 23 percent and the number of UCR homicides by around 20 percent. Robert D. Keppel, Joseph G. Weis and Robert LaMoria, *Improving the Investigation of Murder: The Homicide Information and Tracking System (HITS)*, Final Report for National Institute for Justice Grant #87-IJ-CX-0026 at 7 (1990). Michael Rand argues that Keppel's data may even overestimate the difference between UCR numbers and actual numbers (though not NCHS numbers). Michael R. Rand, *The Study of Homicide Caseflow: Creating a Comprehensive Homicide Dataset*, in Block and Block, eds, *Questions and Answers in Lethal and Non-Lethal Violence*, Proceedings of the Second Annual Workshop of the Homicide Research Working Group, June 13-17, 1993, NIJ Research Report 103-118, 105 (DOJ 1993), <<http://www.icpsr.umich.edu/NACJD/HRWG/proceed.html>> (visited Nov 11, 1999). A number of studies that have compared SHR data and independent city police information directly suggest that the underreporting is not severe. The ratio of the number of murders reported by police departments and the SHR in 1978 was between 0.97 and 1.07 for seven cities compared in a study by Reidel. Marc Reidel, *Nationwide Homicide Data Sets: An Evaluation of Uniform Crime Reports and the National Center for Health Statistics Data*, in D. MacKenzie Layton, P.J. Baunach, and R.R. Roberg, eds, *Measuring Crime: Large-Scale, Long-Range Efforts* 175-205, 181 (SUNY 1990). Reidel also reports that the discrepancy between SHR murders and UCR murders is much smaller in the years before 1978. *Id.* at 205. Finally, at least longitudinally, the NCHS, UCR data and the SHR homicide data follow similar trends, even though their level may differ. Reidel, *Source of Homicide Data* at 88. Therefore, most studies conclude that the SHR presents reliable estimates. Rand, *Questions and Answers* at 104.

⁷⁴ Instead, papers using UCR data simply attempt to mitigate such bias using transformations of the variables that would seem to eliminate the correlation between explanatory variables and measurements error. See Steven D. Levitt, *The Effects of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation*, 111 Quarterly J of Econ 319, 328 (1996) (using growth rates). No such transformations are attempted here. The predictions this paper tests concern level effects, not growth rates or other functions of crime data.

justment for bias. This paper performed such a test on regressions with total robbery deaths and the probability of death during a robbery as dependent variables. The three adjustments compared were the adjustment reported in the previous paragraph, an adjustment based on the number of months state agencies reported homicides to the SHR,⁷⁵ and an adjustment that fills in nonreported months with averages from reported months for those agencies with populations above 100,000. The Hausman test could not reject the hypothesis that the differences in coefficients across the adjustments, in either of the two regressions, were not systematic. This result lends support to the claim that the adjustment used in this paper creates no more measurement error bias than a number of others suggested in the homicide research literature.

Beyond underreporting, one might suppose that the SHR suffers from systematic misreporting.⁷⁶ When a death occurs during a felony, police have three choices: not report the death, report it as negligent manslaughter or report it as a non-negligent manslaughter/murder (where nonnegligent means worse than negligent). It is plausible that police in a felony-murder jurisdiction code an accidental or negligent death during a robbery as a nonnegligent manslaughter/murder, while in a non-felony-murder jurisdiction police code that same death as negligent manslaughter or, if the death was accidental, do not report that death at all. This bias has not been systematically investigated with regard to any criminal laws, let alone the felony-murder rule.⁷⁷ Yet such bias could pose major problems for the analysis here. Suppose that states with the rule reported accidental deaths during a robbery as murder but states without the rule do not record such deaths. Then a regression of accidental felony deaths on the felony-murder rule would suggest that states without the felony-murder rule have lower accidental felony-death rates. It would be incor-

⁷⁵ See note 72 for a description of this adjustment.

⁷⁶ C. Loftin gives reasons other than those presented here for why SHR estimates of robbery murders are unreliable. He suggests that SHR definitions are poor and police often respond by placing deaths in the "undetermined category." His study is limited, however, because it bases its conclusions on a survey of only one city, Baltimore. Moreover, he does not indicate whether the misreporting is correlated with any of the explanatory variables used here. C. Loftin, *The Validity of Robber-Murder Classifications in Baltimore*, 1 *Violence & Victims* 191 (1986).

⁷⁷ Victoria W. Schneider and Brian Wiersema, *Limits and Use of the Uniform Crime Reports*, in Doris Layton MacKenzie, Phyllis Jo Baunach, and Roy R. Roberg, eds, *Measuring Crime: Large Scale, Long-Range Efforts* 21, 31 (SUNY 1990).

rect, however, to infer from this that the felony-murder rule actually increases accidental felony deaths. The lower accident rates in non-felony-murder rule states would be purely a result of reporting differences.

The UCR system, however, has several features that are likely to mitigate this bias. First, according to the Uniform Crime Reporting Handbook, “any death due to . . . commission of a crime is counted as a [murder or nonnegligent homicide].”⁷⁸ Examples given in the Handbook support this rule.⁷⁹ Second, the Handbook states that categorization of deaths by circumstance “should be based on information known to law enforcement following their investigation, not decisions of a grand jury, coroner’s inquest, or other agency outside law enforcement.”⁸⁰ This suggests to police agencies that they should not look to prosecution decisions to categorize crimes. Third, the problem of misreporting only affects reports of accidental deaths. Under the SHR, police must report even the circumstances of negligent manslaughters. Moreover, the only accidental deaths police need not report are those involving no physical act taken by a felon towards the victim.⁸¹ While these non-reported accidents may be felony murders under the laws of some jurisdictions, they create a statistical problem only if such accidents are correlated with the existence of a felony-murder rule in a state since both states with and without the rule are instructed not to report such accidental deaths in the SHR. Even if nonreported accidents are correlated with the rule, nonreporting will only bias coefficients towards zero, not reverse the sign of correlation.⁸² Fourth, nonreporting of “nonphysical” accidents may not be a huge problem. It seems reasonable to suppose they are a small fraction of all accidental deaths during a crime. Moreover, one might suspect that police officers have an anticrime bias that would

⁷⁸ US Dept of Justice, Federal Bureau of Investigation, *Uniform Crime Reporting Handbook* 6 (FBI 1980).

⁷⁹ The murder and non-negligent homicide section gives death during robbery as an example, while the manslaughter by negligence section states that circumstances suggesting a negligent categorization are hunting accidents, gun cleaning, and children playing with a gun. *Id* at 61.

⁸⁰ *Id* at 60.

⁸¹ For example, if “[a] store owner shoots at a robber, misses, and kills an innocent bystander,” or if “a witness of a crime dies of a heart attack,” the death does not count as a criminal homicide. *FBI – Uniform Crime Reports – Frequently Asked Questions* Q19-Q20, <<http://www.fbi.gov/ucr/faqs.htm>> (visited Nov 19, 1999).

⁸² To see this, suppose that states with and without a felony-murder rule have identical crime rates except that states without the rule have higher numbers of accidental deaths not involving any physical action take by the felon towards the victim. If both types of states do not report such accidental deaths, a regression of such felony deaths on the felony-murder rule will yield a zero rather than negative coefficient. If states without the rule have less such accidents, the coefficient would be zero instead of positive.

encourage them to report even these accidental deaths as homicides committed by a felon. Finally, the FBI regularly conducts audits of SHR reports by police agencies. These audits should correct for many of the errors in the data or deter inaccurate reporting.

TABLE 1

CLASSIFICATION OF DEATHS UNRELATED TO A FELONY AND DURING A FELONY,
BY THE CRIMINAL'S MENTAL STATE AND CIRCUMSTANCE OF THE KILLING

Criminal's mental state	Other circumstances	Classification of death	
		Death unrelated to a felony	Death during a felony
Specific intent or reckless indifference	Premeditation	1st degree murder	1st degree murder
Specific intent or reckless indifference		2nd degree murder	1st degree murder
Specific intent or reckless indifference	Adequate provocation or unknowing recklessness*	Voluntary manslaughter	1st degree murder
Criminal negligence		Involuntary manslaughter	1st degree murder
Unintentional		None	1st degree murder

NOTE.— This table is rough characterization of the law of homicide. There is a great deal of state-level variation, but this table provides the text-book treatment of homicide law in the average state.

* See Joshua Dressler, 31.06(B) *Understanding Criminal Law* at 462 (Matthew Bender 1987).

TABLE 2

CLASSIFICATION OF ACCIDENTAL DEATH DURING A FELONY, BY STATE
AND TIME PERIOD, 1970-1998 (YEARS WITH CHANGES UNDERLINED)

	1st Deg. Murd.	2nd Deg. Murd.	Man.	None		1st Deg. Murd.	2nd Deg. Murd.	Man.	None
AL	70-98				MT	70-98			
AK	70-77	<u>78-98</u>			NE	70-98			
AZ	70-98				NV	70-98			
AR	70-75		<u>76-98</u>		NH	70-73	<u>74-98</u>		
CA	70-98				NJ	70-98			
CO	70-98				NM	70-98			
CT	70-98				NY	70-74	<u>75-98</u>		
DE	70-71	<u>72-74</u>		<u>74-98</u>	NC	70-98			
DC	70-98				ND	70-98			
FL	70-98				OH	<u>98</u>		70-97	
GA	70-98				OK	70-98			
HA	70-72			<u>73-98</u>	OR	70-98			
ID	70-98				PA	70-72	<u>73-98</u>		
IL	70-98				RI	70-98			
IN	70-98				SC	70-98			
IA	70-98				SD	70-98			
KS	70-98				TN	70-98			
KY	70-73			<u>74-98</u>	TX	70-98			
LA	70-72	<u>73-98</u>			UT	70-72	<u>73-90</u>		
ME	70-98					<u>91-98</u>			
MD	70-98				VT	70-98			
MA	70-98				VA	70-98			
MI	70-79			<u>80-98</u>	WA	70-98			
MN	<u>81-98</u>		70-80		WV	70-98			
MS	70-98				WI			70-98	
MO	70-78	<u>79-98</u>			WY	70-98			

NOTE.— Statutory citations are on file with the author.

TABLE 3
 NATIONAL SUMMARY STATISTICS FOR FELONY AND
 FELONY-DEATH RATES, BY TYPE OF FELONY, 1970-1998.

Felony type	Crime Rate									
	Felony deaths (D)	Felony deaths with gun (DG)	Felonies (F)	Felonies with death (FD)	Felonies without death (FND)	Deaths per felony (D/F)	Deaths per felony with death (D/FD)	Felonies with death as a share of all felonies (FD/F)	Felony deaths as a share of all murders†	Specific felony death as a share of all felony deaths
Robbery	0.84 (0.47) 1278	0.57 (0.36) 1278	220.4 (129.7) 1128	0.74 (0.41) 1128	214.6 (129.1) 1449	0.0043 (0.0031) 1128	1.11 (0.19) 1034	0.0038 (0.0024) 1128	0.095 (0.037) 1278	0.502 (0.155) 1231
Rape	0.06 (0.07) 1278	0.01 (0.02) 1278	36.3 (12.2) 1128	0.06 (0.06) 1128	33.9 (12.7) 1449	0.0018 (0.0022) 1128	1.04 (0.13) 741	0.0017 (0.0020) 1128	0.008 (0.011) 1278	0.042 (0.057) 1231
Burglary	0.09 (0.08) 1278	0.04 (0.05) 1278	1249.4 (408.7) 1128	0.08 (0.07) 1128	1256.2 (419.1) 1449	0.0001 (0.0001) 1128	1.10 (0.23) 760	0.000063 (0.000047) 1128	0.011 (0.012) 1278	0.059 (0.060) 1231
Auto theft	0.01 (0.03) 1278	0.01 (0.02) 1278	523.7 (230.2) 1128	0.01 (0.02) 1128	511.6 (227.8) 1449	0.0000 (0.0001) 1128	1.04 (0.20) 325	0.000026 (0.000055) 1128	0.002 (0.006) 1278	0.009 (0.033) 1231
Larceny	0.01 (0.02) 1278	0.01 (0.01) 1278	2970.9 (664.0) 1128	0.01 (0.02) 1128	2854.3 (752.6) 1449	0.0000 (0.0000) 1128	1.03 (0.20) 196	0.000003 (0.000007) 1128	0.001 (0.004) 1278	0.005 (0.017) 1231
All felonies	1.64 (0.86) 1428	1.00 (0.61) 1428	5001.2 (1168.7) 1128	1.42 (0.75) 1128	4870.6 (1269.5) 1449	0.0003 (0.0002) 1128	1.20 (0.25) 1084		0.192 (0.071) 1428	

Note.—Figures are calculated first within states and then by taking population weighted averages over all 50 states. Data are from the FBI's *Uniform Crime Reports* and *Supplemental Homicide Reports*. The first number in each cell is the statistic defined by the row and column headings. The second number is the standard error of the first statistic. The third number is the number of observations used to calculate the first statistic.

† The denominator actually includes murders and non-negligent manslaughters.

TABLE 4

SUMMARY STATISTICS FOR STATE-LEVEL EXPLANATORY VARIABLES, 1970-98.

Variable	Units	Years	Obs	Mean	Std. Dev.
Persons aged 15-19	% of pop.	1970-98	1450	8.2	1.3
Persons aged 20-24	% of pop.	1970-98	1450	8.2	1.1
Persons aged 25-29	% of pop.	1970-98	1450	8.1	1.0
African-American persons	% of pop.	1970-98	1450	11.7	7.5
Persons in metropolitan areas	% of pop.	1970-98	1450	76.8	17.3
Persons employed	% of pop.	1970-98	1450	51.8	5.5
Persons unemployed	% of pop.	1970-98	1450	6.5	2.0
Persons below poverty line	% of pop.	1970-98	1450	13.3	3.7
Income	Real, per capita	1970-98	1450	4462	803
Persons in prison	Per 100,000 pop.	1970-98	1450	216	137
Persons sentenced to death	% of all murders	1970-98	1428	1.13	1.47
Persons executed	% of all murders	1970-98	1428	0.09	0.33
Arrests for murder	% of all murders	1970-98	1450	80.1	19.2
Arrests for robbery	% of all robberies	1970-98	1450	29.3	14.2
Arrests for rape	% of all rapes	1970-98	1450	41.7	17.8
Arrests for burglary	% of all burglaries	1970-98	1420	14.2	5.4
Arrests for auto theft	% of all auto thefts	1970-98	1420	14.2	7.9
Arrests for larceny	% of all larcenies	1970-98	1420	16.3	4.8
Region - Northeast	Dummy	1998	50	0.19	0.40
Region - Midwest	Dummy	1998	50	0.23	0.43
Region - South	Dummy	1998	50	0.35	0.48
Region - West	Dummy	1998	50	0.22	0.42

Note.—Means are calculated by weighting state observations by state populations. Missing arrest rate figures are not imputed via linear interpolation as they are in the regressions reported in Tables 7 and 8.

TABLE 5

CROSS-SECTIONAL COMPARISON OF MEAN FELONY-HOMICIDE RATES
IN STATES THAT DO AND DO NOT PUNISH FELONY HOMICIDE
AS FIRST-DEGREE MURDER, BY FELONY TYPE, 1970-1998.

	Punishment for felony homicide			One- and two-sided tests of the statistical significance of the difference in means		
	Not murder one (A)	Murder one (B)	Difference (A) - (B)	Pr{(A) < (B)}	Pr{(A) ≠ (B)}	Pr{(A) > (B)}
All felonies	1.315 (0.051)	1.160 (0.025)	0.156 (0.056)	0.997	0.006	0.003
Robbery	0.619 (0.026)	0.615 (0.016)	0.004 (0.030)	0.5477	0.9046	0.4523
Rape	0.047 (0.003)	0.062 (0.003)	-0.015 (0.004)	0.0006	0.0012	0.9994
Burglary	0.075 (0.005)	0.070 (0.003)	0.005 (0.006)	0.7769	0.4463	0.2231
Auto theft	0.013 (0.003)	0.013 (0.001)	-0.000 (0.004)	0.4728	0.9455	0.5272
Larceny	0.004 (0.001)	0.006 (0.001)	-0.002 (0.001)	0.0536	0.1072	0.9464

Note.—Means are calculated without weighting by state population. (This explains the slight differences between means here and in Table 3). However, unequal variances across subpopulations are allowed. Below each mean is its standard error in parentheses.

TABLE 6

AVERAGE TOTAL FELONY-HOMICIDE RATES BEFORE AND AFTER A CHANGE
IN THE FELONY-MURDER RULE (DEFINED AS STATES THAT PUNISH FELONY HOMICIDE
AS FIRST-DEGREE MURDER), BY STATE, 1968-98.

	Before	After	Difference		Before	After	Difference
AK	0.09 (0.25)	0.40 *** (0.17)	0.31 (1.03)	MO	1.00 *** (0.24)	0.53 *** (0.18)	-0.48 (2.56)
AR	-0.01 (0.28)	0.37 *** (0.17)	0.38 (1.33)	NH	-0.70 *** (0.32)	-0.97 *** (0.16)	-0.27 (0.56)
DE	-0.10 (0.40)	-0.14 (0.15)	-0.04 (0.01)	NY	0.69 *** (0.30)	0.94 *** (0.16)	0.25 (0.54)
HI	-0.30 (0.35)	-0.45 *** (0.16)	-0.15 (0.14)	OH	0.06 (0.79)	0.07 (0.15)	0.01 (0.00)
KY	0.29 (0.32)	-0.05 (0.16)	-0.34 (0.87)	PA	-0.04 (0.35)	0.18 (0.16)	0.22 (0.33)
LA	0.18 (0.35)	1.57 *** (0.16)	1.39 *** (13.00)	UT	-0.62 *** (0.22)	-0.73 *** (0.19)	-0.11 (0.14)
MI	1.13 *** (0.23)	1.06 *** (0.18)	-0.07 (0.06)	Average	0.07	0.15	0.08
MN	-0.62 *** (0.19)	-0.65 *** (0.22)	-0.03 (0.01)				

Note.—Averages calculated within states are not weighted by annual state population. Average calculated across states also is not weighted by state population. Below every mean is its standard error in parentheses. A *** next to a mean indicates it is significant at the 95 percent confidence level, ** at the 90 percent level, and * at the 85 percent level.

TABLE 7

THE EFFECT OF PUNISHING FELONY DEATH AS FIRST-DEGREE MURDER
ON VARIOUS CRIME RATES: RESULTS FROM STATE-LEVEL REGRESSIONS, 1970-98.

Felony	D	D _G	F	F _D	F _{ND}	F _D /F	D/F _D
Robbery	0.11	-0.002	0.02	0.18 **	0.07	0.16 **	-0.01
	(0.09)	(0.13)	(0.06)	(0.10)	(0.06)	(0.09)	(0.03)
	1273	1273	1123	1123	1444	1123	1123
	0.64	0.65	0.96	0.66	0.95	0.5	0.09
Rape	0.06	0.16	-0.01	-0.10	0.00	-0.10	-0.02
	(0.23)	(0.19)	(0.10)	(0.17)	(0.08)	(0.22)	(0.02)
	1273	1273	1123	1123	1444	1123	1123
	0.45	0.43	0.9	0.48	0.89	0.44	0.13
Burglary	-0.24	-0.15	-0.01	-0.07	0.01	-0.07	-0.08 **
	(0.19)	(0.27)	(0.04)	(0.15)	(0.04)	(0.17)	(0.04)
	1275	1275	1125	1125	1441	1125	1125
	0.42	0.38	0.94	0.43	0.92	0.32	0.13
Auto theft	-0.30	-0.16	-0.09 **	-0.79 ***	-0.07	-0.70 ***	-0.002
	(0.36)	(0.25)	(0.05)	(0.35)	(0.06)	(0.35)	(0.01)
	1274	1274	1124	1124	1440	1124	1124
	0.44	0.47	0.91	0.48	0.9	0.48	0.1
Larceny	-0.03	0.23	0.01	-0.09	-0.03	-0.09	-0.03 ***
	(0.31)	(0.24)	(0.03)	(0.26)	(0.03)	(0.26)	(0.01)
	1275	1275	1125	1125	1441	1125	1125
	0.33	0.38	0.9	0.35	0.91	0.32	0.1
All felonies	-0.01	-0.02	-0.02	0.05	-0.02	0.06	-0.07 ***
	(0.07)	(0.10)	(0.03)	(0.09)	(0.03)	(0.09)	(0.02)
	1425	1425	1125	1125	1446	1125	1125
	0.58	0.63	0.91	0.61	0.91	0.57	0.17

NOTE.— This table presents the coefficients on a felony-murder rule dummy variable from a series of regressions. The data is a state panel from 1970-1998. The rows indicate the felony that is the subject of a regression. The columns indicate the form of the dependent variable in the regression. For example, the sixth cell in the third row indicates the coefficient on the rule dummy in a regression with log average number of deaths per burglary as the dependent variable. Each cell provides the coefficient on the rule dummy, the standard error, and the number of observations and the R-squared from that regression. A *** indicates a coefficient is significant at the 95 percent confidence level, ** at the 90 percent level, and * at the 85 percent level. Estimation was via weighted least squares, using state populations as weights. Standard errors are calculated using White's formula and assuming there is state-wise heteroskedasticity. Each regression contains the independent variables in Table 4, with one modification. The arrest rates included were that for murder and the class of felony to which the dependant variable belonged. (The "All felonies" regressions only contained the arrest rate for murder.) Each regression contained year and state fixed effects. Many states for many years have zero observations on the dependent variables. These states were imputed 0.1 of the relevant crime, before taking rates and then logs. Zero observations for arrest rates were imputed via linear interpolation within states before taking logs.

TABLE 8

THE EFFECT OF PUNISHING FELONY DEATH AS FIRST- OR SECOND-DEGREE MURDER
ON VARIOUS CRIME RATES: RESULTS FROM STATE-LEVEL REGRESSIONS, 1970-98.

	D	D _G	F	F _D	F _{ND}	F _D /F	D/F _D
Robbery	0.27 *** (0.08) 1273 0.64	0.23 ** (0.12) 1273 0.65	0.05 (0.05) 1123 0.96	0.27 *** (0.08) 1123 0.66	0.11 * (0.08) 1444 0.95	0.22 *** (0.11) 1123 0.5	0.01 (0.02) 1123 0.09
Rape	-0.10 (0.19) 1273 0.45	0.26 * (0.16) 1273 0.43	-0.08 (0.10) 1123 0.9	0.05 (0.11) 1123 0.48	-0.10 (0.10) 1444 0.9	0.13 * (0.09) 1123 0.44	-0.004 (0.02) 1123 0.13
Burglary	0.01 (0.11) 1275 0.42	0.23 (0.18) 1275 0.38	0.002 (0.04) 1125 0.94	-0.03 (0.12) 1125 0.43	0.01 (0.04) 1441 0.92	-0.04 (0.13) 1125 0.32	-0.12 *** (0.05) 1125 0.13
Auto theft	-0.95 *** (0.24) 1274 0.44	-0.51 *** (0.22) 1274 0.48	-0.09 ** (0.05) 1124 0.91	-1.08 *** (0.32) 1124 0.48	-0.11 (0.08) 1440 0.9	-0.99 *** (0.35) 1124 0.48	-0.004 (0.02) 1124 0.1
Larceny	-0.09 (0.14) 1275 0.33	0.22 * (0.13) 1275 0.38	0.03 (0.04) 1125 0.9	-0.21 * (0.13) 1125 0.35	0.03 (0.03) 1441 0.91	-0.24 * (0.16) 1125 0.32	-0.04 *** (0.01) 1125 0.1
All felonies	0.04 (0.10) 1425 0.58	0.09 (0.11) 1425 0.63	-0.001 (0.04) 1125 0.91	0.03 (0.10) 1125 0.61	0.01 (0.03) 1446 0.91	0.03 (0.11) 1125 0.57	-0.06 *** (0.02) 1125 0.16

NOTE.—See note from Table 7. The one change is that sample sizes are omitted because they are the same as in Table 7.

TABLE 9
 COMPARING PREDICTIONS ABOUT THE LIKELY CONSEQUENCES
 OF THE FELONY-MURDER RULE WITH ESTIMATED

	D	DG	F	FD	FND	F _D /F	D/FD
Predicted relationships							
Proponents	< 0	< 0	< 0	< 0	?	< 0	< 0
Opponents	≥ 0 †	0	0	0	0	0	≥ 0
Estimated coefficients							
Robbery	0.11 0.27 ***	0.02 0.23 **	0.02 0.05	0.18 ** 0.27 ***	0.07 0.11 *	0.16 ** 0.22 ***	-0.01 0.01
Rape	0.06 -0.10	0.16 0.26 *	-0.01 -0.08	-0.10 0.05	0.001 -0.10	-0.10 0.13 *	-0.02 0.00
Burglary	-0.24 0.01	-0.15 0.23	-0.01 0.00	-0.07 -0.03	0.01 0.01	-0.07 -0.04	-0.08 ** -0.12 ***
Auto theft	-0.30 -0.95 ***	-0.16 -0.51 ***	-0.09 ** -0.09 **	-0.79 *** -1.08 ***	-0.07 -0.11	-0.70 *** -0.99 ***	0.002 0.00
Larceny	-0.03 -0.09	0.23 0.22 *	0.01 0.03	-0.09 -0.21 *	-0.03 0.03	-0.09 -0.24 *	-0.03 *** -0.04 ***
All felonies	-0.01 0.04	-0.02 0.09	-0.02 0.00	0.05 0.03	-0.02 0.01	0.06 0.03	-0.07 *** -0.06 ***

NOTE.—The estimated coefficients are from Tables 7 and 8. The first in each cell is from a regression that compares states that punish felony-death as first degree murder with the rest, and the second from a regression that compares states that punish felony-death as first or second degree murder with the rest. No standard errors are reported. However, an * indicates a coefficient is significant at 10 percent; ** at five percent; and *** at one percent.

† The total deaths due to felonies rise only if the average number of deaths per felony resulting in death rises, that is, sign (D) = sign (D/FD).

TABLE 10

PREDICTIONS CONCERNING THE EFFECTS OF THE FELONY-MURDER
RULE, BY FELONY, 1998.

Felony	Crime rate	Year	Predicted change in crime rate assuming that ...	
			states that currently do not punish felony-murder as murder one begin to do so	states that currently punish felony-murder as murder one stop doing so
Robbery	D	1998	36.22	-93.43
	F	1998	2599	-7134
Rape	D	1998	0.48	-2.44
	F	1998	-187	685
Burglary	D	1998	-3.38	14.16
	F	1998	-2143	9016
Auto theft	D	1998	-0.53	3.91
	F	1998	-24035	94573
Larceny	D	1998	-0.05	0.23
	F	1998	9375	-36568
Sum	D	1998	32.74	-77.56

Note.—Predictions are based on coefficients from Table 7. The first number in each cell in the last two columns is the predicted change in deaths during the felony specified in the row label given the reform specified in the column label. The second number is the predicted change in the number of felonies specified in the row label given the reform specified in the column label.