The Economics of Rape Reporting: Will Victims Pay for Police Involvement?*

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Abstract

The growing role of the private sector in the provision of public safety has made understanding the nature of individual demand for criminal justice protection of pressing importance. Being the victim of sexual assault is expensive; each incident imposes a cost of over $100k on the victim. However, because the benefits of successfully apprehending and prosecuting an offender are not captured entirely by victims, from a social welfare standpoint rape is likely to be underreported in the free market. Consistent with this, in 2006, approximately 59% of sexual assaults were *not* reported to police. In spite of the centrality of victim reporting in the functioning of the criminal justice system, to date there is very little systematic evidence on the role of incentives in demand for ex post law enforcement. We estimate the sensitivity of victims to the expected cost of reporting in an Alaskan city between 1993 and 2006, during which time the chief of police publicly supported a policy of charging victims of sexual assault for medical procedures required to collect evidence against their attackers. Using a triple differences approach that compares trends in reported sexual assaults to other index crimes over time and across Alaskan cities, we estimate that the combined monetary and psychic costs reduced the number of reported rapes by between 50 and 80%. This large response highlights the importance of public policies that reduce the private cost of reporting crime.

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1. Introduction

The criminal justice system in the United States has historically been a publicly provided good. In recent years, however, public safety is being privatized. After 2001, reliance on private companies to provide public safety has become increasingly commonplace. The Bureau of Justice Statistics currently estimates that there are roughly two private security personnel for every sworn law enforcement officer,[1] and the Bureau of Labor Statistics predicts that employment in private law enforcement will grow by 17 to 30% between 2006 and 2016.[2] Federal, state, and local governments are increasingly turning to private companies to provide deterrence, apprehension, investigation, and punishment, primarily as a cost saving measure. The Bureau of Economic Analysis estimated that private security firms earned roughly $47 billion in 2007.[3] Privately provided law enforcement is big business, and as individuals become more and more able to choose the level of law enforcement that they consume rather than a fixed amount provided for them by the government, there is an increasing need to understand the nature of demand for criminal justice.

A large literature in economics has shown that individuals are willing to spend large amounts of money to avoid becoming victims of crime [Thaler (1978); Lynch and Rassmussen (2001); Linden and Rockoff (2006); Pope (2008)], and that willingness-to-pay for policies that reduce the probability that an individual is victimized is quite high [Cohen et al. (2004)]. Compared to the literature on demand for criminal justice ex ante, almost nothing is known about ex post demand for criminal justice system involvement. This is an unfortunate omission, because the benefits of apprehending and prosecuting a criminal fall almost entirely on society, rather than the victim who in most cases is responsible for initiating the legal intervention. Because the benefits of ex-post criminal

[1] http://www.cops.usdoj.gov/Default.asp?Item=2034
[2] http://www.bls.gov/oco/ocos159.htm
[3] http://bea.gov/industry/gpotables/gpo_list.cfm?anon=81259&registered=0

justice protection do not accrue to the individual who makes the choice, economic theory suggests that victims are likely to report crimes and assist prosecutors at rates that are less than socially optimal.

While intuition might suggest that a victim's demand for criminal justice is inelastic, research on mandatory arrest laws and domestic violence suggests that victims are sensitive to changes in the total cost of reporting [Iyengar (2009)]. Recognizing this inefficiency, many jurisdictions have adopted "no drop" policies, which essentially forbid victims from declining to demand prosecution for certain offenses [Aizer and Del Bo (2009)], and the Federal government has created subsidy programs like the National Victims Compensation Fund to encourage greater levels of victim participation in the criminal justice system. Despite these policies, it is likely that crime victims continue to "under consume" criminal justice protection. Indeed, only 48.8% of violent crimes were even reported to the police in 2006 [Sourcebook of Criminal Justice Statistics (2006)]. One reason underreporting persists may be that we simply do not know what types of policies are successful at correcting this market failure. This is in a large part due to data limitations that prevent researchers from linking reporting behavior to government policies.

In this paper we take advantage of an unusual policy "experiment" to provide evidence on this question in the context of rape reporting. The case of rape is particularly relevant for several reasons. The external costs of rape are very high; at $144 thousand per victimization, rape is second only to arson in terms of the cost per victimization [Miller, Cohen, and Wiersema (1996)]. The total social cost of rape, which includes the costs to non-victims, is potentially even higher; estimates using housing price data

suggests that each rape costs society $1.3 million [Linden and Rockoff (2008)]. Despite this high degree of harm, only 41% of victims even report being raped to police [Sourcebook of Criminal Justice Statistics (2006)], one of the lowest reporting rates among all personal crimes. Since rape offenders are very unlikely to be prosecuted without victim cooperation, understanding the determinants of whether victims report being raped to the police is a key step in learning how to reduce the social harm due to rape.

We study how rape reporting responded to a policy shift initiated by the police department in a small Alaskan town. In 2000, the police chief of Wasilla, a town with a population of approximately 6,000 people, publicly verified that between 1997 and 2000 the department had billed victims or their insurance companies for the forensic procedure used to establish that a rape occurred and potentially identify the attacker. While perhaps not salient until the public announcement in 2000, this policy imposed a personal financial cost of reporting a rape to the police of between $500 and $1,200, as much as 6.6% of annual income of the average Alaskan woman. Using the trends in reporting rates in other Alaskan cities and for other crimes as counterfactual evidence, we find that the public announcement itself resulted in a substantial reduction in the rape reporting rate. While we are limited in our ability to quantify the full increase in both financial and psychic price of reporting rape, our point estimates suggest that demand for criminal justice involvement ex post is likely to be highly price elastic.

The timing and magnitude of the observed reduction is consistent with multiple theories on individual responses to government action. First, we find that the rape reporting rate fell gradually after 1997, the earliest point at which victims could have

been charged, and fell sharply after the practice was reported in the local newspaper in 2000. This is consistent with recent research in public finance on consumer under-response to non-salient taxes [Chetty et al. (2008)], particularly if individuals learn about the cost of emergency room medical procedures after they are preformed. In addition, we find that even after the Alaskan state government technically outlawed the cost shifting a few months after the public announcement, the reduction in reporting propensity persisted. Institutional economists have noted that the informal institutions persist even after discrete law changes [Acemoglu and Robinson (2006)]. We argue that the nature of the 2000 public announcement could plausibly have led victims to believe that while the local government was technically forbidden to charge them for forensic analysis, the police department may have been less than sensitive to victims of sexual assault, which would impose some hedonic cost on victims. After 2000, the *de jure* law required local governments to bear the cost of reporting while the *de facto* police practice imposed, at minimum, high psychic costs on rape victims. Consistent with Acemoglu and Robinson (2006), we show that *de facto* institutions dominate *de jure* laws in terms of affecting behavior.

The paper proceeds as follows. In section two, we review the existing literature on unreported crime and the determinants of ex post demand for criminal justice. Section three outlines the evolution of policies regarding sexual assault in Alaska and our empirical framework. In section four, we describe our method of measuring reporting rates and describe our data set. In section five, we present our results, and finally offer concluding remarks in section six.

2. Official Statistics and Unreported Crime

The FBI's Uniform Crime Reports (UCR) is currently the only national data source that identifies where and when a crime occurred.[4] A survey of 682 articles on the economics of crime published in leading economic journals since 1990 reveals that 72% use data from either the UCR or functionally similar but non-national police reports.[5] While police reports generally, and the UCR specifically, are the workhorses of research in crime and economics, only a particular set of crimes are included in these data-offenses that are "known to police." In almost all cases, police learn about crimes because the victim decides to notify them. The National Crime Victimization Survey (NCVS) suggests that only a fraction of victimizations are reported to police; in 2006, for example, only 48.8% of victims of violent crimes notified authorities.

Such selective reporting would not pose a problem for researchers if the decision to report was conditionally uncorrelated with crime rates and the key independent variables. This assumption is unlikely to hold. Using the NCVS, researchers have found that propensity to notify police varies with the type of crime [Spelman and Brown (1981)], the age and socio-economic status of the victim [Baumer (2002)], and the relationship between the offender and the victim [Block (1974)]. Unfortunately, the NCVS currently does not contain any geographic identifiers that would allow researchers to link individual reports to local government policies or economic conditions.[6] As a

[4] The National Incident Based Reporting System (NIBRS), which at this point is not nationally representative, is subject to exactly the same criticisms as the UCR with respect to its reliance on police reports.

[5] The surveyed journals, selected based on their general ranking and publication of articles on crime are The American Economic Review, The Journal of Political Economy, The Quarterly Journal of Economics, the Review of Economics and Statistics, the Journal of Law and Economics, and the Journal of Public Economics.

[6] A geographically coded NCVS was available between 1998 and 2002, but those data are no longer available. It is possible to identify residents of large MSAs in the NCVS, but the survey is not representative at the MSA level [NRC (2008)]. In addition, because of a change in the survey design, measures of sexual violence in the NCVS are generally not comparable before and after 1992.

result, variation in the "dark shadow" of unreported crime is a relatively unexplained phenomenon.

In the economics literature, the existence of unreported crime is generally ignored, or treated as one of many sources of upwards bias in estimates of the effect of government policies on crime [Evans and Owens (2007); Levitt (1998)]. However, because of dramatic changes in the way police investigate and report rape since 1970, using the UCR to study trends in sexual assault rates within and between areas is extremely problematic [Schneider and Wiersema (1990); Maltz (1999)]. The difference between the number of rape victimizations and reported rapes is large enough to produce opposite signed time trends in rape in the National Crime Victimization survey and UCR (figure 1). This difference in trends is universally believed to be because an increase in reporting rates over time [Blumstein et al 1992].

< figure 1 about here >

Closer examination of the aggregate trends in rape victimization and rape reporting suggest that economic factors may play an important role in reporting. Over 89% of rape victims are women. While opposite in the 1980s, trends in rape reporting and victimization are similar to each other after the early 1990s, a pattern that is strikingly similar to the changes in the male/female wage gap [Blau and Kahn 2000, 2006]. We therefore interpret figure 1 as suggestive evidence that rape reporting is sensitive to financial considerations. This result may generalize to other crimes as well; anecdotal evidence suggests that call volume on police "tip lines" are counter-cyclical.[7]

[7] Dewan, S. and Goodman, B. "As Prices Rise, Crime Tipsters Work Overtime" *The New York Times* May 18, 2008

While not conclusive, this does suggest that demand for criminal justice system after a crime has occurred is price elastic.

The idea that ex-post demand for government protection is sensitive to changes in price is reflected in the criminology literature. LaFree (1989) found that the decision to pursue police involvement is particularly sensitive to the victim's expectations of her subsequent interactions with law enforcement and potential negative repercussions of her decision to report the incident; victims "project forward," explicitly considering the costs and benefits of reporting the rape. Recent case studies have also found that training nurse practitioners and promoting the privacy and comfort level of the victim can increase the likelihood that a victim will notify the police [Jones et al. (2008); Campbell et al. (2005); Crandall and Helitzer (2003); Feldhaus et al. (2000)]. These studies explain the behavior of victims who seek medical attention, which may systematically vary from that of victims who do not go to the emergency room but do wish to notify police. To the best of our knowledge, this study presents the first empirical evidence on the extent to which the ex-post demand for police protection is sensitive to changes in the financial and psychic costs of reporting.

3. Government Actions and Rape Reporting

The notion that society, not the individual victim, is the primary beneficiary of criminal apprehension and prosecution is one of the basic tenets of law and economics. For this reason, most western societies have separate criminal courts where the State (often referred to as "the people") is the plaintiff, in contrast with the civil court systems where individuals can file suits [Cooter and Ulen (2007)]. While victims likely gain some benefit from knowing their attacker is punished, entire communities may benefit

from the incapacitation and deterrence of repeat offenders. In fact, legal scholars have long argued that the benefit to the victim of prosecuting the crime can essentially be thought of as zero; as Blackstone argued over two centuries ago, "the private wrong is swallowed up by the public: we seldom hear any mention made of satisfaction to the individual: the satisfaction to the community being so great." At the same time, without the victim making the initial decision to notify the police and press charges, the government is not able to provide punishment. Because the victim, who does not enjoy the full benefits of criminal justice involvement, makes the decision whether or not to "consume" legal intervention, private costs to the victim that are out of proportion to their private benefit (relatively close to zero) will lead to under provision of criminal justice.

3.1 Reporting sexual assault

Since the 1970s, police departments across the United States have used a standardized medical procedure to establish that sexual contact occurred and to collect semen or other biological material left by the perpetrator on the victim's body and clothing. The results of genetic tests can then be used to verify or contradict the identity of a suspect. Obtaining conclusive medical evidence against a defendant is a primary determinant of a prosecutor's decision to pursue a rape charge in court [Horney and Spohn (1996); Kerstetter and Van Winkle (1990)]. Specifically relevant for our study, failure to provide evidence from this test is cause for a police department to "unfound" (i.e., not report) a rape [LaFree (1989)]. The medical tools and materials required for this procedure (ICD9 V71.5 or ICD10 – Z04.4) are commonly termed a "rape kit."

The medical evidence collected in procedure Z04.4 is not used for therapeutic purposes and does not provide any health benefits to the victim. The procedure is only

used to assist the State with its investigation.[8] Having procedure Z04.4 performed is valuable even if the victim declines to cooperate further with the State in this specific instance, as the evidence collected can be used against a repeat offender in a later case. Consistent with this, in order for victims to be eligible for financial assistance, the National Association of Crime Victim Compensation Boards requires that the victim undergo the procedure even if she declines to report her rape to the police.[9] It follows that the socially efficient use of this procedure will occur when the State, as society at large is the primary beneficiary of the procedure, also bears the primary burden of the procedure.

< figure 2 about here >

Figure 2 graphically demonstrates this economic principle. In this simple model, victims of sexual assault choose to report some fraction of assaults to police. Each reported rape imposes some cost on the victim as well as the state, which must devote legal resources to prosecute the crime and potentially punish the offender. The total cost of each reported rape, including collecting evidence, apprehending the offender, and prosecuting the case, is given by the supply curve C, which is increasing in the fraction of rapes reported. The benefit to society of prosecuting rapes is given by the curve B^S. Note that the benefit to society is larger than the benefit to the actual victim, B^V, because it incorporates the additional benefit gained by society as a whole when sexual offenders are apprehended and convicted.

[8] This point is highlighted by Martin (2005), who documents a belief by emergency room doctors that performing the procedure does not reduce any "health threat" and therefore should not be preformed in an emergency room.
[9] http://www.nacvcb.org/articles/VOCA%20ESSENTIAL.FINAL.htm

In the Figure the social benefit of reporting rape is decreasing in the percent of rapes reported, corresponding to the notion that the value to society of the "worst" rapist being prosecuted and convicted is highest, with each additional rapist posing slightly less of a danger to society. The optimal number of rapes reported is R*, which corresponds with a total cost of P_S for the last rape prosecuted. If the victim bore the full cost of reporting and prosecuting rape, the equilibrium point would shift from X to Y, reducing the percent of rapes reported.

The State can induce victims to increase the number if reported rapes by subsidizing the cost to them, from C to C^v, meaning that per rape, the victim bears cost P_V, and the State pays (P_S-P_V). Any attempt to reduce the reporting subsidy will shift the victim's cost curve to C'^v reducing the number of reported rapes from R* to R'. This will decrease the reporting rate by (R*- R')/R where R is the actual number of rapes. While the price paid by the State in this situation is lower ($(P'_S-P'_V) < (P_S-P_V)$), the distortion away from the social equilibrium creates a deadweight loss represented by the shaded area.

The size of this loss depends on the slope of the victim's benefit curve. If victims report rape regardless of cost to them (an inelastic ex-post demand for criminal justice), then the total deadweight loss is zero and shifting the cost from the state to the victim will not affect the number of reported rapes. If victims are particularly cost sensitive, meaning the benefit curve is relatively flat, any attempt to shift the cost will result in a large reduction in rapes reported. Finally, note that this simple model assumes that the fraction of reported rapes R is independent of the number of actual rapes. If sexual offenders are rational, a reduction in reported rapes from R* to R' will decrease the cost

of committing a rape (by reducing the probability of apprehension), increasing the number of rape victimizations.

3.2 Evolution of perceived cost of rape reporting in Alaska, 1993-2006

The political events in one town in Alaska in the late 1990s provide a unique opportunity to test the importance of government actions on rape reporting rates. In this paper we focus on the actions taken by the city police department chief, an important government actor [Sherman (1998)]. City police chiefs serve at the pleasure of the mayor or city planning board. In early 1997, the newly elected mayor of Wasilla, AK dismissed the original police chief, citing differences in policy approaches,[10] appointing D. Charlie Fannon in his place. The chief is the public face of any police department, but because the Wasilla police department is relatively small, with a total of 14 sworn officers in 1997, Chief Fannon's influence was likely to be large relative to other departments.

In 2000, representatives in the Alaska state government became concerned that some local governments (including those in Wasilla) were charging rape victims for the cost of procedure Z04.4.[11] As a result, the state government passed house bill 270, stating that victims of sexual assault may not be required to pay, either directly or indirectly (through their medical insurance), for examinations required to determine whether an assault had occurred or to collect medical evidence. While this law should have decreased the perceived cost to victims of reporting a rape to the police, this was unlikely to be the case in Wasilla. Chief Fannon publicly expressed his negative opinion

[10] Komarnitsky, S. J. (February 1, 1997) "Wasilla keeps librarian, but police Chief is out" Anchorage Daily News. http://www.adn.com/sarahpalin/story/510219.html

[11] The Alaska House committee minutes from March 23, 2000 include comments suggesting that victims were charged in the Mat-Su Valley (where Wasilla is located), the Kenai Peninsula, and in Southeast Alaska. Discussion of house bill 270 on April 10, 2000 also included statements of there being "some difficulty" in the treatment of victims in Mat-Su, Kenai, Anchorage, and Bethel, Alaska.

of the law in May of 2000,[12] stating that he had attempted to shift the cost of procedure Z04.4 away from the city in the past, and would like to continue to do so. In November of 2001, Fannon was promoted to a higher profile position as the city's first emergency dispatch coordinator. His replacement, Don Savage, was appointed by the same mayor. We found no evidence that Chief Savage ever made any public statements regarding procedure Z04.4

Moving the incidence of cost from the State to the individual should reduce the number of these procedures performed. If it is the case that undergoing procedure Z04.4 is a necessary component of including the rape in official police statistics (meaning the rape is "confirmed") the size of the deadweight loss due to this policy change is proportional to the increase in rapes not reported to police.

3.3 An analytic framework for measuring perceived costs and rape reporting rates

There are three distinct treatment periods created by political changes in Wasilla. First, between 1993 and 1997, Irl Stambaugh was chief of police. Stambaugh made no public statements regarding payment for procedure Z04.4. Based on Chief Fannon's statements, at some point between 1997 and 2000, the Wasilla government shifted the costs of Z04.4 to victims. We refer to this period (1997-2000) as having *de jure* cost shifting. Cost shifting should only impact individual behavior if the new cost is salient to victims [Chetty et al. (2008)]. As there was no public announcement of the cost shifting in the local Wasilla paper, it is reasonable to think that victims would not be aware that they were responsible for the cost of the rape kit until they had reported the crime. Following Chetty et al. (2008), we would expect the behavioral response of victims to a

[12] Goode, J. C. (May 23, 2000) "Knowles signs sexual assault bill" Mat-Su Valley Frontiersman http://www.frontiersman.com/articles/2000/05/23/news.txt

shift in cost to grow over time, as more sexual assaults were reported to the police and knowledge of the city's stance spread.

In 2000, the state government attempted to change this perception through formal state law, which Fannon publicly opposed and his replacement never supported. We refer to this later period as a time of *de facto* cost shifting. While technically forbidden to impose any cost on rape victims, it is not clear from the police chief's statements whether or not victims should expect the law to be followed, in the same way that black and female Americans may still expect to face *de facto* discrimination in the housing and labor markets, even though the *de jure* law has prohibited this practice since 1964 [Neumark et al. (1996); Holzer and Neumark (2000); Ross and Turner (2005)]. It is therefore unclear what impact this law change would have had on a victim's expectations of the cost of reporting. On one hand, state law now forbade local agencies from cost shifting. However, the statements by Chief Fannon were very salient and could have both increased the psychic costs of reporting by making the police seem unsympathetic to rape victims, as well as given victims the impression that they would in fact be charged for the rape kits. To wit, while Fannon does not explicitly say that he intended to continue charging victims, his reticence and displeasure with the new law (**"**I just don't want to see any more burden put on the taxpayer") stands in stark contrast to the sentiment expressed by another police chief quoted in the article, who stated, **"**I'm prepared to pay every dime in an investigation. As long as I am chief, I would never bill a victim."

We therefore examine how rape reporting rates vary in three conditions: police and victim perceptions roughly equivalent to statewide perceptions (1993 to 1997), a shift

in formal (*de jure)* city police policy (1997-2000) in which the expected cost of reporting a rape grows over time as it became salient to victims, and then contradicting informal *(de facto)* city attitude and official policy (2000-2006). The relative magnitude of the *de jure* and *de facto* changes is theoretically unclear, although Acemoglu and Robinson (2006) argue that *de facto* policies are at least as important as *de jure* institutions.

One way to evaluate the effect of these policies on rape reporting is a simple difference in difference approach, which would compare changes in rape reporting over time in Wasilla to variation in other police departments in Alaska. This is analogous to estimating the parameters of equation one:

$$(1)\quad Report_{tp} = \alpha_p + \delta_t + \theta Clr_{(t-1)p} + \beta_{97}\left(Att_t^{1997-2000} \times Was_p\right) + \beta_{01}\left(Att_t^{2001-2006} \times Was_p\right) + \varepsilon_{tp}$$

where $Report_{tp}$ is the fraction of rapes reported in year t by police department p, $Att_t^{a\text{-}b}$ is a dummy variable that equals one in the years a through b, and Was_p is a dummy that equals one if the police department is in Wasilla. The ability of the police to solve a rape case is likely to be correlated with the probability that the victim reports. We therefore include a control for the rape clearance rate, defined as the fraction of rapes for which the police made an arrest, in jurisdiction p in year t-1, designated as $Clr_{(t\text{-}1)p}$.[13] We define this to be zero if no rapes were reported. We further allow for time-invariant differences in reporting rates across department (α_p) and arbitrary shocks across departments in each year (δ_t). Under certain assumptions, β_{97} and β_{01} can be interpreted as the impact of *de jure* and *de facto* cost shifting on rape reporting in Wasilla.

[13] While conceptually appropriate, in practice this clearance rate may be correlated with the dependent variable, as the number of reported rapes in year t is the numerator of the dependent variable, and the denominator of the clearance rate is the number of rapes in year *t-1*. Our estimates of β, available on request, are robust to the exclusion of this control.

In order for this interpretation to be correct, it must be true that in the absence of the *de jure* and *de facto* cost shifting, there would have been no difference in the changes in rape reporting in Wasilla relative to the comparison cities in Alaska. This is a strong assumption, especially given that Wasilla was growing rapidly in the late 1990s.
A decrease in the number of police per capita following this population expansion would increase the cost of reporting all crimes, and bias our DID estimates of β_{97} and β_{01} downwards. Variation in the number of tourists in a city, per capita income, and other demographic changes across Alaska would also limit the interpretability of DID estimates. We therefore propose a third round of differencing to account for city-specific changes over time in the propensity of local citizens to report crime to the police. We identify the price sensitivity of demand for ex post police protection as the difference between rape reporting rates in Wasilla and other cities over time *minus* the change over time in the difference between reporting rates for other crimes in Wasilla and elsewhere. This can be expressed mathematically as

$$(2)\ \left(\Delta_t RapeReport_{Was} - \Delta_t RapeReport_{\sim Was}\right) - \left(\Delta_t OthReport_{Was} - \Delta_t OthReport_{\sim Was}\right)$$

Assuming that there is no city- and crime-specific variation in the probability that victims report a crime to the police that changes over time, comparing these means would be satisfactory. Because some police may be better at solving some crimes than others, we instead estimate equation (3), which allows us to control for city-crime-year specific variation in clearance rates.

$$(3)\quad \begin{aligned} Report_{ctp} = {} & \alpha_{cp} + \delta_{ct} + \nu_{tp} + \theta Clr_{c(t-1)p} + \beta_{97}\left(Att_{tp}^{1997-2000} \times Rape_c\right) \\ & + \beta_{01}\left(Att_{tp}^{2001-2006} \times Rape_c\right) + u_{ctp} \end{aligned}$$

In this model, we allow for constant differences in reporting rates across cities and crimes (α_{cp}), as well as arbitrary statewide variation over time in each crime (δ_{ct}) and a police jurisdiction - year fixed effect to allow for the reporting rate for all crimes to be different in each city in each year (υ_{tp}). We identify the impact of formal (*de jure)* and informal *(de facto)* cost shifting on rape reporting as the estimated coefficients on β_{97} and β_{01}. In our fixed effects model, these estimates are identified off of variation in rape reporting in Wasilla that cannot be explained by statewide trends in rape reporting, changes in general police activity or economic conditions in Wasilla, or time invariant differences in the propensity of Wasilla residents to report any other crime relative to the rest of the state. Finally, we allow for arbitrary correlation in the unobserved component of reporting rates over time within police jurisdiction.

If it is the case that official attitudes regarding the cost of rape testing did reduce the propensity of victims to report rapes, we expect that the estimated values of β_{97} and β_{01} to be less than zero, indicating a reduction in reported rape when it was not obvious to victims who would bear the cost of a full police investigation and criminal prosecution. If it is the case that the passage of house bill 270 reassured victims that they would not lawfully be held financially responsible for undergoing procedure Z04.4, then we expect to see that $\beta_{97} < \beta_{01}$. However, if the expression of the police chief's opposition to the bill created the expectation of *de facto* cost shifting, as well as an increase in psychic cost of seeking criminal justice involvement, consistent with Acemoglu and Robinson (2006), we would find that $\beta_{97} \geq \beta_{01}$.

4. Measuring Reporting Rates

Estimating equations (1) (2) and (3) is complicated by a lack of data on reporting rates at the local level. We measure reporting rates in Alaska over time by exploiting the data generation process in the Uniform Crime Reports - Offenses Known and Clearances by Arrest from 1993 to 2006 (UCR).[14] What is reported as the number of sexual assaults in the UCR is actually a composite measure consisting of the product of three variables: the number of rapes (*R*), the percent of victims who report rapes to police (*V*) and the percent of those rapes included by the police in their official statistics (*C*).[15] The ratio of rapes included in the UCR to actual rapes, which in our terminology is equal to *VC*, is commonly referred to as the "reporting rate," and *(1- VC)* the "reporting bias."

We construct our measure of reporting rates in the spirit of Levitt (1998), who points out that for certain crimes *VC* is likely to be very close to one. This observation, combined with information about crime trends, can be used to measure the extent to which citizens and police are altering their reporting behavior over time. Scaling the officially reported rate of crime A by the officially reported crime rate of a "high *VC*" crime B yields $\frac{R^A}{R^B} VC^A$. In almost all circumstances, the ratio of R^A and R^B will be different from one, as different crimes occur at different rates. However, if this ratio is stable over time (a strong but, as we will show, reasonable assumption), any changes in the ratio of officially reported A to officially reported B should be driven by movement in VC^A.

[14] U.S. Dept. of Justice, Federal Bureau of Investigation. UNIFORM CRIME REPORTING PROGRAM DATA [UNITED STATES]: OFFENSES KNOWN AND CLEARANCES BY ARREST, 1993-2006 [Computer file]. Compiled by the U.S. Dept. of Justice, Federal Bureau of Investigation. ICPSR22400-v1. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor]

[15] In other words,

$$Rapes_{UCR} = Rapes_{Actual} \times P(Victim\ Report\ to\ Police) \times P(Police\ Report\ to\ UCR) = R \times V \times C$$

In this paper, we are particularly interested in determining the role of local government decisions on VC^{rape}. There are three requirements for an appropriate "B" crime: (1) it must have a VC close to one, (2) the crime must be common enough to have meaningful variation in official reports over time, and (3) temporal changes in actual crimes must be correlated with rape (ie: $\frac{R^{rape}}{R^{B}}$ is consistently defined and stable over time). With rare exceptions, murder is always reported to police, and changes in murder rates over time are highly correlated with violent crime as a whole. For these reasons, murder is used by Levitt (1998) as his reference crime. The drawback of using murder is a reference crime is that murder is a relatively infrequent event. Specifically, for the purposes of our analysis, murder rates in Wasilla are essentially zero (four murders occur during our sample period). Murder therefore fails to meet condition (2).

The second plausible candidate for crime B is car theft. The 85% percent of Alaskan car owners with insurance have a strong incentive to report the theft of their vehicle to the police in order to cancel or suspend their insurance policy.[16] More generally, the costs of reporting a car theft are probably low relative to the expected benefit of getting a car back. Consistent with this logic, Levitt (1998) estimates that the elasticity of car theft reporting rates with respect to police force size is essentially zero. Unlike murder, car theft is a common crime in Alaska. There are reported car thefts in Wasilla in ever year, with an average of 25 car thefts per 5,000 residents annually. Car

[16] Individuals found to be without sufficient liability insurance in Alaska will have their driver's licenses suspended, except in some sparsely populated areas where vehicle registration is not required. http://www.ircweb.org/news/20060628.pdf

theft therefore meets conditions (1) and (2). The final condition for "rapes per car theft" to be a valid proxy for VC^{rape} is that $\frac{R^{rape}}{R^{carthef t}}$ is stable over time.

It is obviously inappropriate to use officially reported crime rates to assess the validity of the stability assumption. Therefore, we verify this final condition using national trends in the National Crime Victimization Survey (NCVS). As implied by the title, the NCVS is based on a nationally representative survey of US households, and is generally believed to be the "best" measure of actual crime rates. The national rate actual car thefts are highly correlated ($\rho>0.9$) with rapes in the NCVS, and even taking out a linear trend over time, variation in these crimes are strongly related to each other ($\rho>0.4$). This is consistent with existing research [Jensen and Karpos (1993)]; crime either rises or falls- it is highly unusual for burglary to increase while assault rates decline.[17]

While the ratio of rapes to car thefts is reasonably stable over time at the national level, we cannot eliminate the possibility that this is not true at the city level due to, for example, variation in the number of police per capita that would reduce the cost of reporting all crimes. In order to mitigate this measurement error, we compare changes in the ratio of rapes to car thefts over time with the ratios of burglaries, robberies, assaults, and larcenies to car thefts in each city. We identify variation in rape reporting as variation in the officially reported rapes per car theft that cannot be explained by variation in other index crimes per car theft over time.

There are 46 Alaskan law enforcement agencies that report to the UCR during this time period, 37 of which report in every sample year. Forty agencies report some

[17] See the Bureau of Justice Statistics website for trends in crime rates over time. For reasons of space we do not replicate these figures in this paper. http://www.ojp.usdoj.gov/bjs/gvc.htm#Violence

positive population in each year,[18] and of those agencies with a defined jurisdiction, half of them patrol populations with fewer than 2,500 people on average. Only four agencies, the Juneau Police, the Anchorage Police, the Fairbanks Police, and the State Police Department cover more than 10,000 citizens. We exclude from our analysis the Spenard Special District police, who never report any crime in any year, and the Alcohol Beverage Control, the Fairbanks and Anchorage Airports, and the police departments in Kake, Nenana, Anvik, Emmonak, and Houston Police departments, which report fewer than 10 years of data. Even though they report no population, we include the campus police in Anchorage and Fairbanks, which report as many as 8 rapes per year. In our final sample of 37 agencies, an average of 12.6 rapes is reported per agency each year. The distribution of reported rape is highly skewed, and excluding the largest three agencies (Anchorage, Fairbanks, and the State Police) reduces this number to 2.1 rapes per year. In per capita terms, with an average of 54.4 rapes per 100,000 people in 2006, Alaska has a relatively high rate of rape; the national average is 30.9.[19]

< Table 1 about here >

Table 1 contains mean values of the number of index crimes reported to police each year, average population, and percent of crimes "cleared" by arrest for cities in our sample. The clearance rate is calculated as the number of crimes officers clear by arrest

[18] The agencies reporting zero population the University of Alaska – Fairbanks campus police, the University of Alaska – Anchorage campus police, the Anchorage and Fairbanks airport police, the Alcoholic Beverage Control, and the Spenard Special District police.

[19] In order to verify the validity of UCR data for Alaska, we cross checked the *annual* reported crime counts with local police websites and newspaper reports, and excluded observation that we could not confirm. There are roughly 40 instances in Alaska which *monthly* crime counts are negative 1 or negative 2. We treat these as positive crime counts, although Maltz and Weiss (2006) argue that negative counts that are larger than -24 should be treated as real adjustments to the past months reported crimes. Re-estimating our analysis with the Maltz and Weis (2006) treatment of negative crimes yields qualitatively identical results that are actually slightly larger in magnitude than those presented in this paper, and are available on request.

relative to the total number they confirm, and can be interpreted as a rough measure of the effectiveness of a police force at solving a given crime. We include summary statistics for all Alaskan cities except Wasilla (column 1), agencies that report rape in at least one year (column 2), cities with similar rapes per car theft to Wasilla prior to 1997 (column 3), and Wasilla specifically (column 4). Wasilla police clear roughly 41% of the total number of crimes by arrest each year, and across crimes their clearance rate and crime rates are generally consistent with other "comparable" cities, where comparable cities are defined as ever reporting a rape (sample 1) or reporting a similar rape rate to Wasilla prior to 1997 (sample 2) (about a 47% clearance rate).

While we selected "comparable" cities based on rapes per car theft, which is our outcome of interest, it is possible that larger Alaskan cities (some of which are included) are inherently a poor control group. In the final column, we present summary statistics for all other Alaskan cities with fewer than 10,000 residents, 90% of which we also classify as "comparable" cities. Not surprisingly, the total number of crimes reported in these cities, which are roughly half the size of Wasilla, is about half the number reported in Wasilla. The one exception is rape, for which the smaller cities report more rapes per year, and in fact clear a higher fraction of those cases by arrest each year (44% versus 33%). In all areas except rape, crime in Wasilla is unremarkable relative to other cities in our sample.

While we do not use crimes rates per se in our analysis, as they capture changes in both reporting behavior (the focus of our paper) and actual crime, trends in crimes per capita over time provide some evidence that rape may have been underreported after 1997. Prior to 1997, the Wasilla police reported an average of 30.9 rapes per 100,000

residents per year, only marginally lower than the mean of 36.6 per 100,000 residents elsewhere in the state (p=0.83). After 1997, the number of reported rapes in Wasilla fell to 28.8 per 100,000 residents per year, while in the rest of the state rape rates rose to 63.8 per 100,000 residents per year (p=0.41). Neither difference is larger than could be explained by chance, but this relative reduction in rapes runs counter to other crime trends. By comparison, Wasilla had a higher rate of assault per capita than the rest of the state after 1997 (p=0.02), and a higher, but not statistically significantly higher, rate prior to that (p=0.18). Wasilla also had higher burglary rates (p=0.017) and robbery rates (p=0.003) after 1997, but not before 1997 (p=0.88 and p=0.71, respectively).[20] After 1997, Wasilla had a high crime rate relative to the rest of the state on all dimensions except for rape.

For all crimes but rape, between 1994 and 2007, Wasilla appeared to move from a relatively average risk city to a relatively high risk city. Because these are official crime statistics, this change in RVC is due either to a decrease in rapes accompanied by increases in all other crimes (a change in values of R), or a change in reporting behavior of either victims or the police (a change in VC). We distinguish between these two sources of variation by dividing each value of RVC through by the annual number of car thefts in each city, assuming that $R/R^{cartheft}$ is roughly constant.

< Figures 3 – 7 about here >

Figures 3 through 7 show the time pattern of reporting rates (offenses per car theft) in Wasilla and cities that fall into both samples 1 and 2. Consistent with our

[20] Prior to 1997, the mean number of assaults, burglaries, and robberies per 10,000 people in Wasilla was 112, 84.9, and 6.1 respectively. In the rest of Alaska, the means were 68.8, 80.5, and 4.7, respectively. After 1997, the mean assault, burglary, and robbery rates were 116, 83.5, and 9.18 in Wasilla and 61, 56, and 3.6 in the rest of Alaska.

assumptions, the reporting rates in Wasilla for robbery (figure 3), larceny (figure 4), burglary (figure 5), and assault (figure 6) appear to track those in other cities reasonably well, and remain relatively stable over time. However, the pattern for rape looks very different. It is quite apparent that after 1999, Wasilla appears to fall behind the rest of Alaska in terms of rapes per car theft (figure 7). Specifically, the rape reporting rate appears to be increasing over time in the rest of the state, but not in Wasilla.[21]

5. Results

5.1 DID results

Even though Alaska has the highest rate of sexual assault in the country, it is possible that the pattern in figure 7 is driven by changes in police effectiveness, or some other unobserved change in city environment. We present estimates of equation (1), which allow for variation in clearance rates as well as city and year fixed effects in table 2. In the first column of table 2, we focus on sample 1, which includes cities that report at least one rape between 1993 and 2006. If we allow for only one change in reporting rates we estimate that rape reporting fell by 63% in Wasilla after 1997.

< *Table 2 about here* >

It is conceptually appropriate to allow for arbitrary correlation in rape reporting within police departments, but because there are only 19 cities in this sample, the estimated standard error of 0.316 is unlikely to be a consistent estimate of the true standard error. We therefore supplement our analysis by performing a Fisher permutation test, in which we compare the observed point estimate on rape in Wasilla (-

[21] Recall that in nation overall, the largest increases in rape reporting occurred in the 1980s. This pattern suggests that this changed occurred more slowly Alaska.

0.626), with the point estimates obtained by performing the same DID analysis for rape reporting in every other city in the sample, as well as burglary, larceny, and robbery in all cities (including Wasilla). In effect, we estimate the probability that we would have observed the change in reporting in Wasilla at random by comparing the observed point estimate for Wasilla (already given in parenthesis above) to the distribution of point estimates obtained through the series of replications in which we "turn on" the dummy variable Att_t^{a-b} for every other city (and later city-crime combination for the DIDID analysis) besides Wasilla. The student's t-statistics are displayed in brackets- there is essentially zero probability that the null hypothesis of no change in rape reporting in Wasilla is true.

In columns (2) and (3) we test the relative importance of *de jure* and *de facto* changes in the burden of payment by looking for changes in rape reporting that occurred between 1997-2000 and then after 2000. We also test whether the impact of *de facto* cost shifting attenuated over time, perhaps because women learned that they would not in fact be charged for the rape kits. It does not appear to be the case that the propensity of a woman to report a rape to the police changed between 1997 and 2000, which is both consistent with the relative importance of *de jure* shifting in Acemoglu and Robinson (2009) as well as the predictions of Chetty et al. (2009), since it is not clear that the cost shifting was made public. At the same time, the null finding is sensitive to the comparison sample that we choose; when we examine cities with similar pre-treatment levels of rape reporting prior to 1997 in columns 4-5, we do estimate a smaller and precisely estimated reduction. While our *de jure* results are mixed, our DID results clearly suggest that after Fannon made his public statement, rape reporting by Wasilla

women fell. The impact of this public statement on the propensity of women to take the first step in pursuing legal charges against their attackers does not seem to have deteriorated over time. If anything, we find that this reduced propensity to report grew larger over time, as we also estimate a negative coefficient during the 2003-2006 time period that is statistically improbable under the null hypothesis.

< Table 3 about here >

One concern with our approach is that we are excluding observations in which there are no reported rapes (since they are dropped using the log transformation), resulting in a truncated sample that could create bias. In Table 3, we present results in which we replace null observations with 0.001 rapes per car theft. We include on the right hand side a dummy variable equal to one if the true value of the dependent variable is zero. Our results are generally robust to this adjustment, and also to the choice of comparison group. When we look across all cities, it appears that after 1997, rape reporting fell by roughly half. This was primarily driven by the period in which *de facto* cost shifting occured, implying that even if it is technically unlawful for the city government to shift the cost of procedure Z04.4 to victims, the public statement that the police chief would prefer to do so significantly reduced demand for ex post criminal justice involvement. As expected, when we include cities with no rape reporting, our estimates from sample 2 fall in magnitude, but in sample 1 they actually increase.

5.2 DIDID results

Our difference in difference results strongly suggest that rape reporting changed in Wasilla after Fannon's public statements. However, we cannot rule out that some other factor reduced the propensity of all Wasilla residents to report crimes, or that there

was an increase in the rate of Wasilla car thefts. Our DIDID analyses, in which we subtract out any temporal variation in reporting of other crimes, are presented in tables 4 and 5.

< *Table 4 about here* >

Looking across samples, we estimate that between 1997 and 2006, the ratio of reported rapes to car thefts fell by 50% in Wasilla relative to the rest of Alaska, over and above any changes in the propensity of Alaskans to report other crimes. There are two possible explanations for this large reduction- a decline roughly equal to 0.2 fewer rapes per car theft. First, it is possible that there was a real decrease in sexual assault in Wasilla. However, the cause of this decline must have affected *only* sexual assault and not aggravated assault, burglary, robbery, larceny, or car theft, and must have *only* happened in Wasilla. Since burglary, assault, and robbery are thought to be particularly correlated with rape rates in victimization data, this seems unlikely. The second possible explanation is that rape reporting rates fell under Fannon and Savage. When we divide our treatment period into the pre-2000 and post-2000 periods, we find that the reduction in reporting is driven by a 71% reduction (se=0.2) in the later *de facto* period.

If we restrict our comparison group to only cities that had similar levels of rapes per car theft prior to 1997, the impact of Fannon's public statement increases. Once we subtract out differential reporting of other crimes, we also find some evidence that the propensity of women to report rape rebounded in Wasilla relative to cities with similar rapes per car theft (samples 2 and 3); after 2003 we estimate a 10% increase in reporting that our Fisher tests suggest is unlikely to happen at random.

< *Table 5 about here* >

The inclusion of "no crime" observations in table 5 alters our conclusions in two ways. First, in the full sample, columns 1 and 3, the average impact of the perceived cost shift was an 80% (se=0.1) reduction in reported rapes, although this increase is not statistically distinguishable from the 50% reduction observed overall. We still observe that this reduction was concentrated immediately after Fannon's public statement and deteriorated over time. Our qualitative conclusions are robust to variations in the comparison group; they are driven by something in Wasilla that did not occur elsewhere in Alaska.

< Figures 8 – 13 about here >

To demonstrate the change in rape reporting in Wasilla graphically, in figures 8-13 we present the density of observed coefficient estimates from Fisher tests for columns 7 and 8 from tables 4 and 5. As expected, the distribution of coefficient estimates for all permutations is centered on zero, with a relatively symmetric distribution around that mass point. With the exception of the unadjusted *de jure* effect (prior to 2000), the point estimate on rape in Wasilla is consistently located on the far left tail of the distribution.

Our estimates of the change in rape reporting induced by a perceived shift in the burden of cost are potentially biased in the presence of temporal changes in the reporting of other crimes in Wasilla. For example, victims of sexual assault could have chosen to pursue criminal charges against their attacker by reporting that they were victims of assault, which would not require undergoing procedure Z04.4. This shift in crime reporting would lead us to overstate the impact of cost shifting on rape reporting, as reporting would simply be displaced from one type of crime to another.

< Table 6 about here >

In order to parse out the effect of any particular crime, in table 6 we replicate our estimates excluding one control crime at a time. For reasons of space we do not present t-statistics from a Fisher test for these estimates, and limit our analysis to sample 2 and the unadjusted dependent variable.[22] These results confirm that our previous estimates are driven by changes in rape reporting, not by variation in any other crime. The size of our coefficient estimates are stable across excluded categories, although our estimate is slightly less precise when we exclude robbery from our control group.

Allowing for heterogeneous *de jure* and *de facto* effects yields qualitatively identical conclusions about the magnitude of the externality created by cost shifting. The *de facto* cost shifting appears to have dramatically reduced rape reporting by at least 60% (with some point estimates being over 100%). The *de jure* cost shifting also reduced rape reporting, with the average reduction on the order of 20%, but the point estimates range from no change to a 40% reduction. An alternate and equally plausible interpretation is that women were not aware of the cost shifting prior to media publicity surrounding the passage of the state law and the police chief's public response.

Regardless of the cities to which we compare Wasilla, how we treat years in which no rapes are reported, or how we control for reporting rates of other crimes, we consistently find that a *de facto* shift in the cost of procedure Z04.4 lead to large reductions in the propensity of a rape victim to notify police. This shifting of the burden of cost was technically illegal, and over time the demand for ex post legal action increased, but by 2006 the propensity of sexual assault victims to notify the police had not yet returned to pre-1997 levels. This leads us to conclude that rape reporting in

[22] A replication of this robustness test for additional samples and adjusted dependent variables is available on request.

Wasilla changed in an unusual manner that is consistent with either a unique and dramatic reduction in sexual assault unaccompanied by any other changes in crime that occurred in no other Alaskan city, or with a reduced propensity of victims to report to police. Based on our knowledge of events and the existing research on the behavior of rape victims, we conclude that the latter explanation is more plausible.

6. Conclusion

Rape and sexual assault are relatively infrequent crimes that impose large costs on society. Attempts to reduce the incidence of sexual assault are complicated by the limitations of existing data. Specifically, victims of sexual assault are less likely to report the offense to a police than the victims of any other violent crime, meaning that policy changes aimed at reducing rape are particularly difficult to evaluate. In order to better understand what determines demand for criminal justice system intervention ex post, we analyze the impact of a change in how an Alaskan police department treated alleged rape victims. The shift in police attitudes can be divided into two periods; between 1997 and 2000, the official position of the police department may or may not have been to bill victims, or their insurance, for procedure Z04.4. During this time period, we classify the potential shift in cost to be *de jure;* i.e., the official practice. In 2000, the Alaskan state legislature specifically mandated that city governments bear the cost of the procedure, eliminating the possibility of *de jure* cost shifting. However, because the police chief publicly voiced his disapproval of the law (as well as noting the procedure's cost, which victims may not have been aware of prior to 2000) we argue that a *de facto* informal shifting of the expected cost burden was still in place. Because these *de facto* costs also

include the psychic or stigma cost of contacting a police department that was perhaps hostile towards rape victims, this later effect could plausibly be larger.

From a theoretical standpoint, this government policy created inefficiency in rape reporting- society was benefiting from a procedure without paying for it. This would cause rape reporting to fall below its optimal level after this change. The magnitude of the efficiency loss generated by this policy depends on the sensitivity of the victim's propensity to report rape to an increased monetary burden- if victims are not sensitive to changes in the cost of reporting, we would not see that reported rapes fell relative to any other reported crimes.

By examining the trends in reported rapes relative to all other crimes in all other cities, we find evidence that rape reporting falls when the cost of processing a rape is borne by the victim. This is consistent with research on domestic violence, which frequently involves sexual assault. Aizer and Dal Bo (2009) and Iyengar (2009) find that in the wake of domestic violence, individuals are sensitive to the price of criminal justice involvement; changes in the cost of police involvement are negatively correlated with the probability of victim reporting. While current estimates suggest that potential victims are willing to pay high amounts of money for criminal justice system involvement ex ante, we find that increasing the private costs associated with reporting rape reduced the propensity of victims to report sexual assault by as much as 88%. We also find that the impact of *de facto* cost shifting was approximately twice the size as the impact of *de jure* cost shifting.

While the magnitudes of our point estimates are large, even without taking into account the stigma costs, the size of monetary cost shifting was substantial. In 2000, the

median income of all Alaskan women was $18,107 (or $32,624 for full time, year round workers).[23] Based on media reports about the cost of procedure Z04.4, between $500 to $1,200 of the cost of reporting a rape was shifted from the state to the victim between 1997 and 2000. The magnitude of the total monetary cost borne by rape victims is unknown, but this cost was roughly 6% of the median income of all working Alaska females.

Taken together, our results imply that the demand for rape reporting is quite responsive to its price. The idea that the police chief supported levying a fee of roughly 7% of pre-tax income on rape victims reduces the probability that sexual assaults are reported by as much as 80%. Because of the magnitude of this response, we conclude that the efficiency loss created by the perception of a shift in the cost of procedure Z04.4 to victims almost certainly outweighed any cost savings by the local government. While this expenditure reduction may have resulted in a tax break for local constituents,[24] the failure by victims to report rape to police has multiple dimensions of social cost. Failure to report rape hamstrings efforts to target effective social services at needy populations. Perhaps most importantly, an unreported rape is an unsolved rape, meaning that the offender will bear no cost of his actions. Reducing the expected cost of committing sexual violence may increase the incidence of sexual assault [Becker (1968)]. It is outside the scope of this paper to estimate the number of additional sexual assaults generated by this policy shift, but at a cost of between $114 thousand and $1.3 million

[23] http://factfinder.census.gov/servlet/QTTable?-geo_id=04000US02&-qr_name=DEC_2000_SF3_U_QTP33&-ds_name=DEC_2000_SF3_U

[24] This conjecture follows from public finance literature on intergovernmental grants [Bradford and Oates (1971)], which argues that the propensity for governments to consume out of tax revenue is quite low.

per victimization, the expense associated with even a small increase in the number of rapes would far outweighs the fiscal savings of $1,200 per investigation.

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Figures:

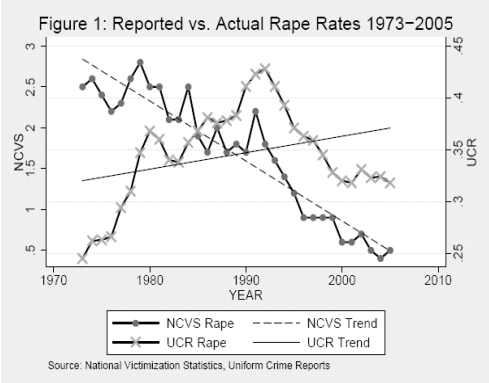
Figure 1: Reported vs. Actual Rape Rates 1973–2005
NCVS
UCR
YEAR
NCVS Rape
UCR Rape
NCVS Trend
UCR Trend
Source: National Victimization Statistics, Uniform Crime Reports

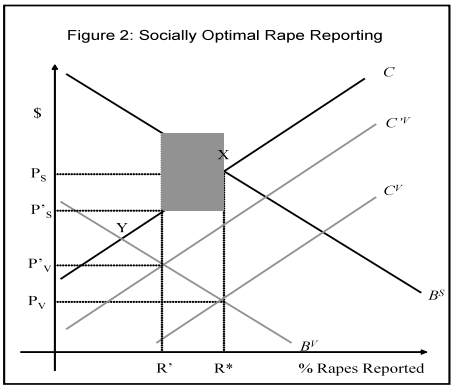
Figure 2: Socially Optimal Rape Reporting
$
C
C^V
X
P_S
P'_S
C^V
Y
P'_V
P_V
B^S
B^V
R'
R*
% Rapes Reported

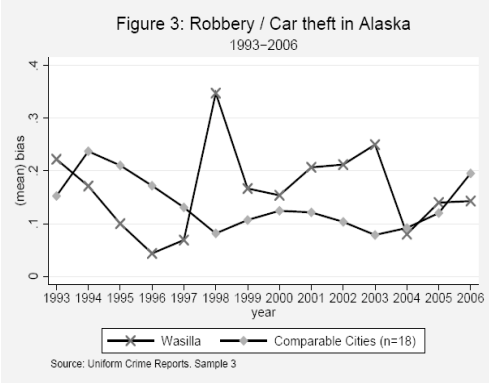
Figure 3: Robbery / Car theft in Alaska
1993–2006
(mean) bias
year
Wasilla
Comparable Cities (n=18)
Source: Uniform Crime Reports. Sample 3

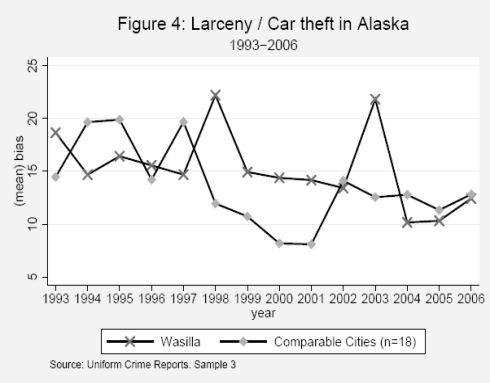
Figure 4: Larceny / Car theft in Alaska
1993–2006
(mean) bias
year
Wasilla
Comparable Cities (n=18)
Source: Uniform Crime Reports. Sample 3


Figure 5: Burglary / Car theft in Alaska
1993–2006
(mean) bias
year
Wasilla
Comparable Cities (n=18)
Source: Uniform Crime Reports. Sample 3

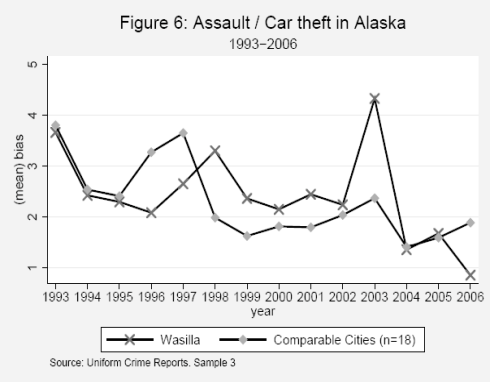
Figure 6: Assault / Car theft in Alaska
1993–2006
(mean) bias
year
Wasilla
Comparable Cities (n=18)
Source: Uniform Crime Reports. Sample 3

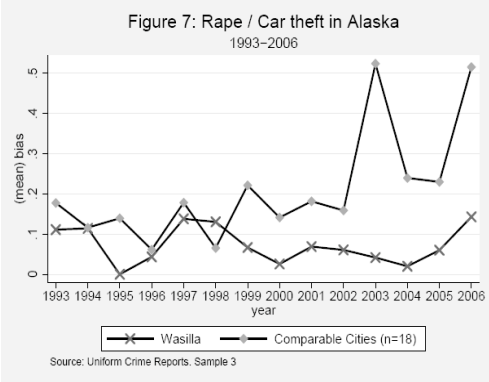
Figure 7: Rape / Car theft in Alaska
1993–2006
(mean) bias
year
Wasilla
Comparable Cities (n=18)
Source: Uniform Crime Reports. Sample 3

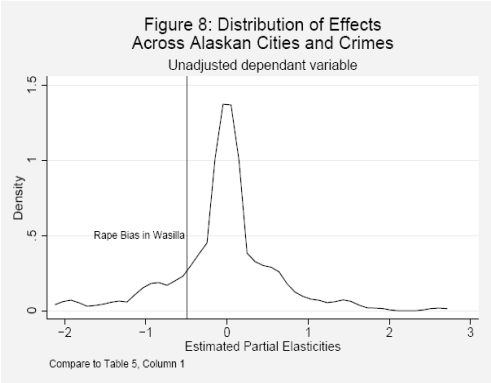
Figure 8: Distribution of Effects
Across Alaskan Cities and Crimes
Unadjusted dependant variable
Density
Rape Bias in Wasilla
Estimated Partial Elasticities
Compare to Table 5, Column 1

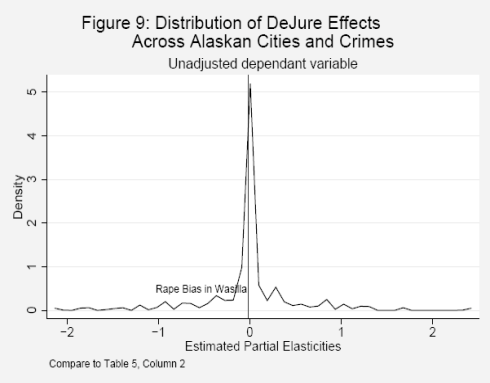
Figure 9: Distribution of DeJure Effects
Across Alaskan Cities and Crimes
Unadjusted dependant variable
Density
Rape Bias in Wasilla
Estimated Partial Elasticities
Compare to Table 5, Column 2

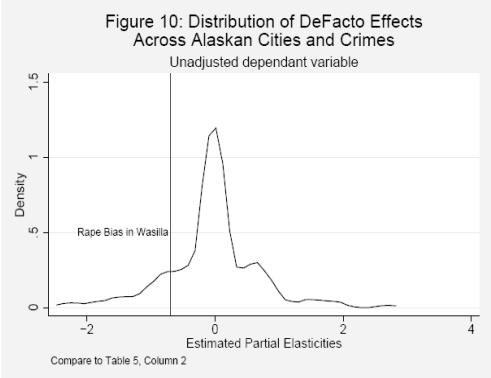
Figure 10: Distribution of DeFacto Effects
Across Alaskan Cities and Crimes
Unadjusted dependant variable
Density
Rape Bias in Wasilla
Estimated Partial Elasticities
Compare to Table 5, Column 2

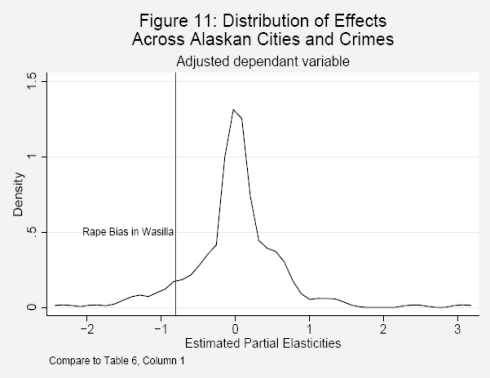
Figure 11: Distribution of Effects
Across Alaskan Cities and Crimes
Adjusted dependant variable
Density
Rape Bias in Wasilla
Estimated Partial Elasticities
Compare to Table 6, Column 1

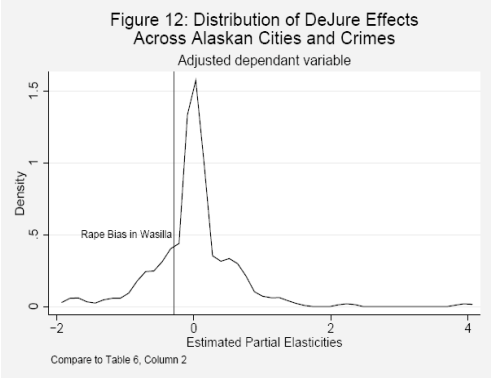
Figure 12: Distribution of DeJure Effects
Across Alaskan Cities and Crimes
Adjusted dependant variable
Density
Rape Bias in Wasilla
Estimated Partial Elasticities
Compare to Table 6, Column 2

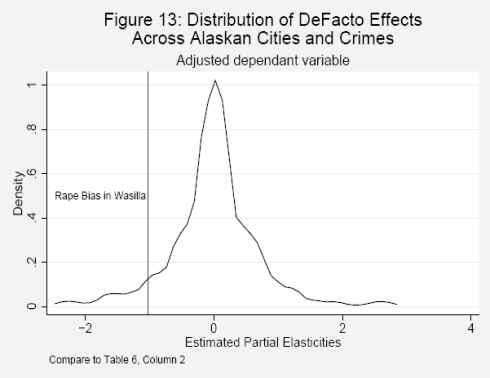
Figure 13: Distribution of DeFacto Effects
Across Alaskan Cities and Crimes
Adjusted dependant variable
Density
Rape Bias in Wasilla
Estimated Partial Elasticities
Compare to Table 6, Column 2

Tables:

Table 1: Official Crime and Clearance Rates in Alaska 1993-2006

	All Agencies	Sample 1	Sample 2	Wasilla	Cities < 10,000 pop
City/years	360	348	293	14	289
Population	21,225	24,536	16,849	5,915	3,554
	(59,693)	(62,486)	(53,489)	(1,162)	(2,305)
Murders	1.43	1.58	1.13	0.29	0.15
	(4.55)	(4.68)	(4.16)	(0.47)	(0.4)
Clearance Rate	0.72	0.70	0.68	0.25	0.66
	(0.43)	(0.44)	(0.40)	(0.50)	(0.47)
Car Thefts	88.2	98.4	85.1	29.4	14.4
	(285)	(294.3)	(301)	(12.4)	(18.5)
Clearance Rate	0.37	0.30	0.38	0.25	0.41
	(0.34)	(0.26)	(0.35)	(0.11)	(0.36)
Rapes	16.6	18.7	13.8	1.86	2.38
	(47.8)	(49.9)	(45.2)	(1.23)	(5.25)
Clearance Rate	0.42	0.42	0.40	0.33	0.44
	(0.38)	(0.43)	(0.38)	(0.45)	(0.42)
Robberies	21.1	23.4	24.5	4.57	1.28
	(92.7)	(95.8)	(102)	(2.38)	(2.18)
Clearance Rate	0.40	0.42	0.37	0.46	0.42
	(0.38)	(0.43)	(0.38)	(0.39)	(0.42)
Burglaries	144	159	114	46.5	22.2
	(421)	(437)	(379)	(21.9)	(20.8)
Clearance Rate	0.32	0.40	0.33	0.21	0.35
	(0.33)	(0.37)	(0.35)	(0.10)	(0.35)
Larcenies	600	675	603	426	122
	(1,804)	(1882)	(1,936)	(138)	(120)
Clearance Rate	0.34	0.27	0.37	0.36	0.37
	(0.32)	(0.20)	(0.33)	(0.12)	(0.34)
Assaults	97.7	109	87.6	67.9	21.3
	(277)	(287)	(275)	(24.4)	(24.5)
Clearance Rate	0.72	0.70	0.71	0.72	0.73
	(0.37)	(0.36)	(0.39)	(0.25)	(0.39)

Standard deviations in brackets. 90.3% of agencies with less than 10,000 residents are in the "comparable" sample

Table 2: OLS estimates of Ln(Reported Rape / Reported Car Thefts); Alaska 1993-2006

	Sample 1			Sample 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Wasilla x (1997 - 2006)	-0.626			-0.898		
	[0.316]			[0.243]		
	{8.73}			{12.0}		
Wasilla x (1997-2000) *de jure*		-0.001	0.003		-0.217	-0.211
		[0.279]	[0.281]		[0.177]	[0.178]
		{-0.042}	{-0.10}		{3.07}	{2.98}
Wasilla x (2000-2006) *de facto*		-0.885	-0.728		-1.173	-1.007
		[0.343]	[0.396]		[0.293]	[0.339]
		{10.9}	{9.16}		{14.2}	{12.3}
Wasilla x (2003-2006) *de facto*			-0.271			-0.283
			[0.185]			[0.214]
			{4.04}			{5.34}
R^2	0.59	0.59	0.59	0.59	0.59	0.59
n		185			145	
Cities		25			19	
Mean Rape / Car Theft		0.45			0.34	

Sample 1 contains only agencies which ever report a rape. Sample 2 includes all cities with less than 0.15 rapes per car theft in any year prior to 1996. Each regression includes the lagged crime specific clearance rate with agency and year fixed effects. Standard errors in brackets allow for arbitrary correlation in reporting bias with agency. Student's t-statistics of a Fischer test against all other crimes in all other cities, and all crimes besides rape in Wasilla $\left(H_0 : \overline{\beta} = \beta_{Was,Rape}\right)$ in braces

Table 3: OLS estimates of Ln(Max[Reported Rape / Reported Car Thefts, 0.001]); Alaska 1993-2006

	Full Sample			Sample 1			Sample 2			Sample 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Wasilla x (1997 - 2006)	-0.528			-0.843			-0.533			-0.859		
	[0.299]			[0.318]			[0.286]			[0.296]		
	{5.59}			{6.09}			{5.06}			{6.69}		
Wasilla x (1997-2000)		-0.147	-0.147		-0.451	-0.451		0.210	0.214		-0.053	-0.051
de jure		[0.377]	[0.378]		[0.421]	[0.422]		[0.307]	[0.307]		[0.340]	[0.340]
		{1.30}	{0.52}		{2.70}	{2.04}		{-1.91}	{-1.93}			{-0.04}
Wasilla x (2000-2006)		-0.687	-0.670		-1.007	-1.056		-0.841	-0.636		-1.190	-1.085
de facto		[0.321]	[0.303]		[0.338]	[0.292]		[0.307]	[0.306]		[0.314]	[0.270]
		{5.88}	{6.17}		{7.43}	{9.30}		{7.05}	{5.34}		{8.45}	{8.64}
Wasilla x (2003-2006)			-0.029			0.086			-0.355			-0.181
de facto			[0.324]			[0.362]			[0.188]			[0.133]
			{-0.71}			{-0.88}			{2.56}			{2.95}
R^2	0.77	0.77	0.77	0.77	0.77	0.77	0.82	0.82	0.82	0.83	0.83	0.83
n		374			301			307			242	
Cities		32			25			24			19	
Mean Rape / Car Theft		0.25			0.30			0.18			0.21	

Sample 1 contains only agencies which ever report a rape. Sample 2 includes all cities with less than 0.15 rapes per car theft in any year prior to 1996, and sample 3 contains only cities meeting both selection criteria. Each regression includes the lagged crime specific clearance rate with agency and year fixed effects. Standard errors in brackets allow for arbitrary correlation in reporting bias with agency. Student's t-statistics of a Fischer test against all other crimes in all other cities, and all crimes besides rape in Wasilla $\left(H_0 : \overline{\beta} = \beta_{Was,Rape}\right)$ in braces

Table 4: OLS estimates of Ln(Reported Crimes / Reported Car Thefts); Alaska 1993-2006

	Full Sample			Sample 1			Sample 2			Sample 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Wasilla x (Rape / Car theft) x (1997 - 2006)	-0.497			-0.499			-0.631			-0.629		
	[0.205]			[0.204]			[0.185]			[0.184]		
	{9.41}			{7.61}			{8.62}			{7.37}		
Wasilla x (Rape / Car theft) x (1997-2000) *de jure*		-0.013	-0.013		-0.015	-0.015		-0.116	-0.117		-0.113	-0.114
		[0.173]	[0.173]		[0.171]	[0.171]		[0.173]	[0.173]		[0.171]	[0.171]
		{0.01}	{-0.12}		{0.36}	{0.31}		{2.04}	{2.12}		{1.39}	{1.40}
Wasilla x (Rape / Car theft) x (2000-2006) *de facto*		-0.701	-0.728		-0.703	-0.731		-0.845	-0.906		-0.843	-0.903
		[0.238]	[0.249]		[0.237]	[0.248]		[0.233]	[0.219]		[0.232]	[0.217]
		{12.4}	{11.8}		{9.78}	{9.04}		{10.9}	{11.3}		{9.13)	{8.93}
Wasilla x (Rape / Car theft) x (2003-2006) *de facto*			0.047			0.049			0.105			0.103
			[0.167]			[0.165]			[0.180]			[0.179]
			{-1.72}			{-1.06}			{-2.24}			{-1.66}
R^2	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94
N		1,357			1,212			1,126			990	
Cities		31			25			24			19	
Mean (Rape / Car Theft)		0.45			0.45			0.34			0.34	

Sample 1 contains only agencies which ever report a rape. Sample 2 includes all cities with less than 0.15 rapes per car theft in any year prior to 1996, and sample 3 contains only cities meeting both selection criteria. Each regression includes agency x year, agency x crime, and crime x year fixed effects and lagged crime specific clearance rate. Standard errors in brackets allow for arbitrary correlation in reporting bias with agency. Student's t-statistics of a Fischer test against all other crimes in all other cities, and all crimes besides rape in Wasilla $\left(H_0 : \overline{\beta} = \beta_{Was,Rape}\right)$ in braces

Table 5: OLS estimates of Ln(Max[Reported Crimes / Reported Car Thefts, 0.001]); Alaska 1993-2006

	Full Sample			Sample 1			Sample 2			Sample 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Wasilla x (Rape / Car theft) x (1997 - 2006)	-0.808			-0.701			-0.849			-0.756		
	[0.114]			[0.117]			[0.101]			[0.104]		
	{17.9}			{10.8}			{15.4}			{11.4}		
Wasilla x (Rape / Car theft) x (1997-2000) *de jure*		-0.292	-0.292		-0.266	-0.267		-0.333	-0.333		-0.305	-0.306
		[0.125]	[0.125]		[0.133]	[0.133]		[0.135]	[0.135]		[0.139]	[0.139]
		{6.38}	{6.56}		{4.46}	{4.47}		{7.10}	{7.11}		{5.33}	{5.34}
Wasilla x (Rape / Car theft) x (2000-2006) *de facto*		-1.027	-1.223		-0.885	-1.030		-1.069	-1.262		-0.946	-1.057
		[0.135]	[0.170]		[0.135]	[0.153]		[0.120]	[0.175]		[0.121]	[0.145]
		{20.4}	{22.4}		{12.2}	{13.6}		{18.0}	{18.8}		{12.7}	{12.6}
Wasilla x (Rape / Car theft) x (2003-2006) *de facto*			0.341			0.253			0.337			0.192
			[0.158]			[0.173]			[0.172]			[0.152]
			{-7.58}			{-4.52}			{-6.28}			{-3.35}
R^2	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97
N		1,870			1,505			1,535			1,210	
Cities		32			25			24			19	
Mean Rape / Car Theft		0.25			0.30			0.18			0.21	

Sample 1 contains only agencies which ever report a rape. Sample 2 includes all cities with less than 0.15 rapes per car theft in any year prior to 1996, and sample 3 contains only cities meeting both selection criteria. Each regression includes agency x year, agency x crime, and crime x year fixed effects and lagged crime specific clearance rate. Standard errors in brackets allow for arbitrary correlation in reporting bias with agency. Student's t-statistics of a Fischer test against all other crimes in all other cities, and all crimes besides rape in Wasilla $\left(H_0 : \overline{\beta} = \beta_{Was,Rape}\right)$ in braces

Table 6: OLS estimates of Ln(Reported Crimes / Reported Car Thefts) with restricted control group; Sample 2 1993-2006

excluded crime:	Assault		Larceny		Burglary		Robbery	
Wasilla * (Rape / Car theft) * (1997 - 2006)	-0.7952		-0.569		-0.669		-0.517	
	[0.300]		[0.350]		[0.174]		[0.190]	
Wasilla * (Rape / Car theft) * (1997 - 2000)		-0.219		-0.070		-0.185		-0.030
de jure		[0.247]		[0.195]		[0.218]		[0.156]
Wasilla * (Rape / Car theft) * (2000 - 2006)		-1.03		-0.776		-0.870		-0.719
de facto		[0.348]		[0.220]		[0.220]		[0.234]
R^2	0.97	0.97	0.91	0.91	0.95	0.95	0.93	0.93
N	856	856	849	849	849	849	969	969

The mean value of (Rape / Car theft) is 0.34. Each regression includes agency x year, agency x crime, and crime x year fixed effects and lagged crime specific clearance rate. Standard errors in brackets allow for arbitrary correlation in reporting bias with agency.