

## ABSTRACT

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ESTIMATES OF THE COST OF CRIMINAL  
VICTIMIZATION

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Robust estimates of the price of crime, measured as the costs of crime to victims, inform a wide range of policy analysis. The most commonly cited studies are constrained by limited data and rely on indirect methods to estimate prices. In these studies, health statistics are used to estimate direct losses from crime, jury award data are used to estimate indirect damages from crime, and self-reported crime data are used to weight injury prevalence within broad crime categories. While the relationship between injury and damages can be observed at the individual level in civil court records, individual level data have not previously been available that link crimes and injury. Since both individual and aggregate data are combined in these studies, prior research has not corrected sampling bias, and the estimates of victimization costs have been reported only as point estimates without confidence intervals. Estimates have been developed for only

a few broad categories of crime and these estimates have not been robust to study design.

This study analyzes individual-level data from two sources: jury award and injury data from the RAND Institute of Civil Justice and crime and injury data from the National Incident-Based Reporting System. Propensity score weights are developed to account for heterogeneity in jury awards. Data from the jury awards are interpolated onto the NIBRS data based on the combination of all attributes observable in both data sets. From the combined data, estimates are developed of the price of crime to victims for thirty-one crime categories. Until data become available linking information about criminal incidents to jury award data, the strategy used here is likely to yield the most robust estimates of the costs to crime victims that can be generated from the jury compensation method.

WHAT IS THE PRICE OF CRIME? NEW ESTIMATES OF THE COST OF  
CRIMINAL VICTIMIZATION

By

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# Dedication

To: Mildred and Francis P. Roman

John and Elizabeth Vance

## Acknowledgements

Like any large research project this dissertation required the assistance of many others and the paper was substantially improved by their efforts. First, I would like to thank my committee for guiding me toward a project that was superior to the one I initially envisioned. Foremost, thanks to Peter Reuter for his endless patience, critical insights, and thoughtful prodding. Randi Hjalmarsson was always willing to discuss the project, and provided invaluable guidance on both methods and substance. Bill Sabol met with me on several occasions to talk about my project even before he joined my committee. Phil Cook pressed me to seriously address the limitations of the jury method in measuring victim costs from the perspective of neo-classical economic theory. Ray Paternoster provided a much needed criminological perspective that kept the project grounded.

I wish to thank many others who volunteered their time to help me finish this paper. Carly Knight, who was unfortunate enough to work across the hall from me, offered flawless technical assistance, and provided thoughtful comments in response to my nearly endless stream of methodological, substantive and metaphysical concerns. Eric Grodsky spent a good deal of Iowa and Nebraska discussing the relative merits of covariates versus propensity matching methods, among other esoterica, and for that and all the other ad hoc guidance I am grateful. I benefited greatly from working at the Urban Institute during the course of this research, and many of my colleagues supported this project. In particular, Aaron Chalfin, Bogdan Tereshchenko, Avi Bhati, Adele Harrell and Shelli Rossman all offered critical insights during this process. I am also particularly in debt to Nick Pace at the RAND Institute of Civil Justice, who was kind enough not only

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If you are lucky, a dissertation turns into a labor of love. Either way, dissertation labors put an enormous strain on those you love. Over the last three decades, my father has probably used the phrase “now that would make a great dissertation!” hundreds of times. So, Dad, here it is, and I thank you for your boundless intellectual curiosity, some of which I was fortunate enough to inherit. My mother has always been my role model, and I have tried to emulate her intelligence, persistence, dedication, and earnestness in approaching my work. I thank my brother Pete for being my rock and truly showing me what hard work is.

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## Chapter One: Developing Robust Estimates of the Price of Crime

Crime creates a substantial burden and results in diseconomies through the sub-optimal allocation of scarce resources. As described in Becker's foundational work on the economics of crime (1968), harms from crime are understood to include the direct losses to victims not internalized by offenders, the costs of crime control, and the losses to society from incapacitation of offenders. Since much of the harm from crime results from externalities, and safety is a (quasi-)public good, there is under-investment in private crime prevention (Samuelson, 1954; Tiebout, 1956). Public crime control expenditures are intended to create a more efficient equilibrium, by investing in crime control up to the point that marginal expenditures are set equal to marginal harms (social costs) from crime. Among Becker's costs of crime, external costs to victims are the most difficult to quantify. Optimal allocation of state resources therefore requires accurate estimates of the social costs of crime. The most widely cited estimates of the social costs of crime rely on civil jury award data with serious limitations (Cohen, 1998; Cohen et al., 1994; Cohen and Miller, 1999; Miller, Cohen and Rossman, 1993; Miller, Cohen and Wiersema, 1996; Miller, Fisher and Cohen, 2001; Rajkumar and French, 1997).

Most prior work in the field seeks to estimate costs of crime rather than focusing on the price to victims. Costs of crime include the price and quantity of victimizations and the prices and quantities of subsequent criminal justice response, including crime prevention. Developing estimates of the costs to the criminal justice system is a relatively trivial exercise, since data are available from the Bureau of Justice Statistics to directly estimate these prices and quantities. By contrast, the price of crime to a victim or

potential victim can not be directly observed. As is discussed later in this chapter, victim harms are estimated to be at least as large as the criminal justice system costs (if not larger) and are thus a critical part of the policy calculus. However, prior estimates of the prices of crime have not proven to be particularly robust, as they rely on small samples, and inconsistent (and unreliable) methods. This dissertation will develop new estimates of the price of crime to victims, measured as the willingness to accept crime losses as estimated from administrative data and jury award verdicts.

The award estimates will use jury award data from the RAND Institute of Civil Justice to estimate the average jury award by type of physical injury. These micro-level data will then be linked with data from the National Incident-Based Reporting System (NIBRS) to estimate harms for 31 types of offenses.

There are six stages in the estimation process. First, four variables (with 19 possible values) are identified that are observable in both the RAND and NIBRS data, including age and gender of the victim, type of injury, and region of the case. Second, a mean award is estimated for each of the possible cross-combination of those four variables in the RAND data. For example, a mean award is estimated for a 30-39 male with a major injury in the west, a mean award is estimated for a 30-39 female with the same injury in the same region, etc. The process is repeated until a mean award has been estimated for all possible combinations (there are 1,213 valid combinations in NIBRS, and fewer in RAND, yielding a total of 342 combinations that are available in both datasets). The process is repeated in the NIBRS data to create cross-combinations in that data set. Third, three analytic data sets are created in the RAND dataset, including: all observations in the

RAND dataset (12,918); only those observations that can be affirmatively coded as crimes (895) and a subgroup of a propensity weighted sample (5,415).

Fourth, the estimated mean award in RAND is interpolated onto the NIBRS dataset for all observations in NIBRS that have a cross-combination of attributes that also appears in the RAND data. These estimates are then aggregated within crime types yielding an estimated distribution of expected awards for 10 types of Part I crimes. Fifth, estimates are developed for the prices of crime for victims of 8 serious property crimes. The estimate is the product of the reported property loss from NIBRS and a weight to account for non-economic losses. The non-economic loss weight is calculated as the ratio of economic to noneconomic losses in the RAND data for crimes where the victims suffered no injury or apparent minor injuries. Sixth, the price of crime is estimated for 13 types of minor property crimes. The estimates are developed directly from reported losses in the NIBRS data. Finally, to address the concern that those cases in NIBRS that are linked to a case in RAND (about 1.4 million) are not different from those cases in NIBRS that do not match to RAND (about 400,000), t-tests are used to compare the mean property loss in Part I property crimes (burglary and robbery).

This approach offers substantial advantages over extant estimates. This study had the advantage of access to more comprehensive data than was available in previous research. This minimizes the number and breadth of assumptions that must be made in the analysis. The consistent use of micro-level data avoids problems associated with aggregation and the large datasets improves power. The study narrative describes a transparent method to aid replicability, and complicated (and unnecessary) regression analyses were not included in the analysis. The final product of this research will be the



development of estimates for a full distribution of crime prices, including estimates for dozens of crime that do not currently have price estimates in the literature.

### Why Robust Estimates of the Price of Crime are Important to Policy-Making

While there is general agreement that crime in the United States results in significant costs, there is limited consensus about the magnitude of those costs. The 1967 President's Commission on Law Enforcement yielded the first estimate of the costs of crime to victims, and estimated total harms from crime at \$650 billion (all monetary estimates in this paper are in 2003 dollars). More recent estimates of the crime burden are generally around \$1 trillion: Collins (1994), writing in the popular press, estimated a total cost of \$900 million, and Anderson (1999), writing in *Law and Economics* estimated about \$1.36 trillion in losses, net of transfers<sup>1</sup>. In testimony before the US Senate in 2006, Ludwig estimated the total costs of crime at \$2 trillion. While these estimates reveal a consensus that there is a substantial crime burden, there is substantial variation in the magnitude of crime costs. Annual crime costs are equivalent to between 7% and 17% of GDP, and between \$3,000 and \$6,500 per US resident.

Total costs of crime have several components: direct costs to victims; indirect costs to victims; public crime control expenditures; crime induced production (including private crime prevention, drug trafficking, and costs of substance abuse); white collar crime/corporate fraud; and, opportunity costs to offenders. Direct losses (also referred to as tangible losses) from crime include lost wages and medical care. The most recent

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<sup>1</sup> Anderson estimates more than \$730 billion in transfers, e.g. the value of goods transferred from a victim to an offender. There is substantial debate in the law and economics literature about whether these costs should be included in total costs calculations, as is discussed in Chapter 2.

national estimates of the direct costs of crime to victims from the Bureau of Justice Statistics (Klaus, 1994) estimates that direct costs of crime in 1992 was \$25.6 billion, with a median direct economic loss per crime of \$38.70.

Most of the published literature in this area has focused on both the direct and indirect costs of crime to victims, where indirect costs (also referred to as intangible losses) include fear, pain and suffering, and risk of death. There is substantial variation in the estimates of the total direct and indirect costs associated with crime, including fear of repeated victimization, but the total of both indirect and direct costs are an order of magnitude larger than the BJS estimates of direct losses alone. Using surveys to estimate private citizen's willingness to pay to avoid victimization yields the largest estimates. Using a contingent valuation technique, Cohen (2005) estimates the total direct and indirect costs of crime at \$500 billion (also see Cohen, Rust, Steen and Tidd, 2005)<sup>2</sup>. Ludwig (2006) weights the Cohen estimates to account for the exclusion of less serious crimes and estimates total intangible costs of about \$700 billion.

Estimates derived from willingness to accept approaches yield slightly smaller estimates, ranging from \$75 to \$300 billion annually. McCollister (2004) estimates \$200 to \$300 billion, Miller, Cohen and Wiersema (1996) estimate \$80 to \$135 billion, Rajkumar and French (1997) estimate between \$130 and \$200 billion and Cohen (1988) estimates between \$70 and \$100 billion. Anderson (1999) uses a willingness to accept strategy and estimates an annual cost of \$165 billion to victims who survive crime and

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<sup>2</sup> Author's calculations. Cohen et al.'s cost to victims were multiplied by the number of crimes in each of seven index offenses for each year between 1982 and 2003 and translated into 2003 dollars. The same methodology was applied to the other four extant estimates of costs of crime to victims.

\$540 billion in lost life. Miller et al. (1996) estimate the total costs of crime, including intangible costs, totaled \$585 billion.

These costs of crime estimates have three important applications. First, they inform cost-benefit analysis of new policies and programs by creating a standardized metric with which to evaluate outcomes. Second, because the costs of crime vary between crimes, these estimates can inform the optimal allocation of criminal justice resources in crime control, where criminal justice system stakeholders must choose among a range of anti-crime strategies with heterogeneous impact on types of crime. Third, they provide new information that can be used to evaluate the effectiveness of crime control efforts over time.

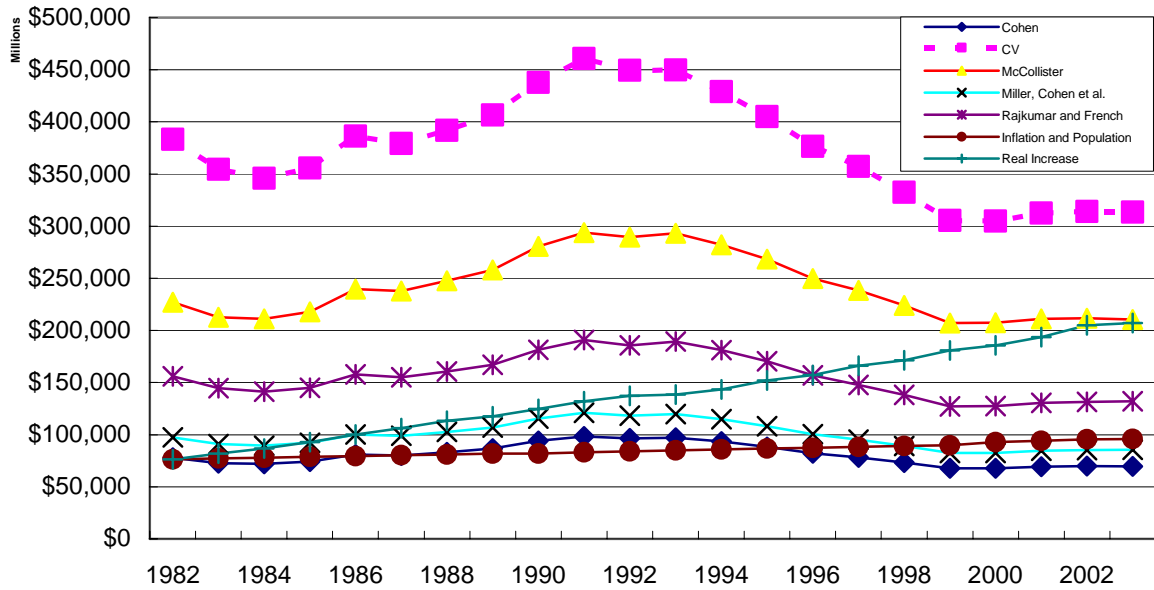
In addition, these estimates can be observed longitudinally to compare these changes to trends in public anti-crime expenditures, and trends in population growth and inflation. If the estimates of the costs of crime to victims are robust, then these data can offer answers to critical policy questions: Are costs to victims increasing or decreasing over time? Do the costs of crime to victims exceed public investment in anti-crime expenditures<sup>3</sup>? Do these results hold when changes in population size and inflation, are accounted for? Figure 1 combines population data from the Census Bureau, inflation data from the Bureau of Labor Statistics Consumer Price Index, public crime expenditure data

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<sup>3</sup> Descriptive data suggest that crime rates have been relatively volatile over the last two decades, but display a general trend where annual increases in the 1980s were followed by declines in the 1990s. The same general pattern holds for both violent crimes and all crimes. By contrast, crime control expenditures have been stable throughout the period. Crime control expenditures increased every year, and increased at a rate greater than inflation, generally between 3 and 6 percent above inflation. If economic theory holds, and crime control expenditures reflect the expected marginal harms from crime, these data suggest that the expected marginal harm from offending is increasing in time (which is also reflected by the increases in estimated costs of crime to victims). This may result from more serious average victimization, or may result from a change in taste for crime. That is, as the American population ages (the average age in the US has increased from about 30 in 1980 to almost 37 in 2007) and grows wealthier (real per capita income increased 70% in this period), the relative price of crime may rise if citizens are risk averse in crime.

from the Bureau of Justice Statistics, and costs to crime victim data from the four most commonly cited studies: Cohen's (1988) and Miller et al. (1996) using jury awards, Rajkumar and French (1997) and McCollister (2004) jury award studies, and the Cohen et al. (2005) contingent valuation study (labeled CV).

Figure 1. Trends in Estimated Social Costs of Crime (1982-2003).



Source: The estimates of the cost of public crime control expenditures are developed from the Bureau of Justice Statistics Justice Expenditure and Employment Extracts Series, using data from the Census Bureau's Annual Government Finance Survey and Annual Survey of Public Employment (1983-2003). Data describing victim costs are developed from two sources: 1) the Federal Bureau of Investigation's Uniform Crime Reports (1983-2003) were used to develop crime trends, and 2) estimates of the monetized value of harms were taken from Cohen (1988), Miller et al. (1996), Rajkumar and French (1997), McCollister (2003) and Cohen et al (2005). Bureau of Labor Statistics Consumer Price Index data were used to calculate changes in real spending.

As shown in Figure 1, there is substantial variation across the cost of crime to victim's estimates, even when dollar values are standardized. This suggests that extant estimates of the price of crime are not robust with respect to the estimation strategy (contingent valuation studies consistently return higher estimates than willingness to accept estimates), nor are they robust within method (as older studies return lower

estimates than more recent studies). Overall, the Cohen et al., (2005) contingent valuation study returns the highest values in each year, followed by the McCollister study. The earlier jury award studies (Cohen, 1988; Miller, et al., 1996) returns the lowest values. Together they tell a provocative story where the price of crime is actually increasing over time. As the average American is both older and wealthier at the end of this series than at the beginning, this may well have some merit. Conversely, this may simply be an artifact of changes in study design. Nevertheless, these changes over time suggest the potential value of robust estimates in informing large-scale policy formulation.

Importantly, these estimates also exclude several types of crime that may have large costs to society. Extant estimates of victim's costs are generally limited to the Federal Bureau of Investigation's (FBI) list of index crime. White collar crime is estimated by Anderson (1999) to cost almost \$700 billion annually, once direct costs to victims of burglary, theft and larceny are excluded. Using the same data as Anderson (1999), Cohen (2005) arrives at a similar estimate of the cost of white collar crime/fraud. Computer crime, which is not included in these estimates of white collar crime, is estimated to add another \$70 billion to total crime costs (FBI, 2005). Anderson (1999) and Ludwig (2006) estimate that lost productivity due to criminal activity totals \$260 billion. Private protection measures total \$64 billion (Anderson, 1999) and costs of avoidance behavior (private precautions revealed by changes in routine activities, see Clotfelter and Seeley, 1979) total \$108 billion. Other unmeasured costs include losses from drug abuse excluding direct crime costs (\$50 billion (Swan, 1998)), private crime prevention purchases total \$20 billion (Clotfelter and Seeley, 1979), and alcohol-related motor vehicle crashes (\$20 billion (Rice, 1993)). Other costs of crime may difficult to

measure, such as the unmeasured costs from crime from urban decay (Taylor, 1995). In addition to the cost of crimes to victims, there are substantial public crime control expenditures to investigate arrest, prosecute and incarcerate offenders. In 2003, the most recent year data were available, total governmental spending on crime control programs was \$205.6 billion (Figure 1). In nominal dollars, total crime control expenditures increased more than five-fold from \$36 billion in 1982 to \$185 billion.

### Research Applying Cost of Crime Estimates

There is a large literature describing the economic foundations of criminal behavior. Economic analyses of crime control policies tend to focus on relationships between social policies and offending that are theoretically predictive of crime. The relationships between the incapacitation and deterrent effects of prison and crime and the value of human capital acquisition have been the topic of many studies (Becker, 1968; Ehrlich, 1973; Ehrlich, 1981; Ehrlich, 1996; Freeman, 1996; Grogger, 1998; Levitt, 1996; 1997; Lochner and Moretti, 2004; Piehl and DiIulio, 1995; Raphael and Winter-Ebner, 2001). While the early literature in this area focused mainly on theory, more recent research have used estimates of the price of crime to victims to more clearly identify the implications of changing the scale of criminal justice institutions. Recently, applied studies of the economics of crime, in the form of cost-benefit modeling of anti-crime interventions, have become more prevalent. These studies tend to link observational data on the local average treatment effect for a discrete sample, and explicitly link those outcomes with price data to estimate economic efficiency (Cartwright, 2000; Cohen, 2000).

The two key challenges in applied studies are 1) the resolution of statistical and identification problems that confound causal inference (Dehejia and Wahba, 2002; Heckman, et al., 1997; 1998; 2001; Imbens and Angrist, 1994; LaLonde, 1986; Manski, 1995; Raphael and Stoll, 2006; Rosenbaum and Rubin, 1983; Rubin, 1974; Smith and Todd, 2005, and 2) the development of robust estimates of prices of criminal victimization in the absence of market data (Cohen, 1998; Cohen et al., 1994; Miller, et al., 1993; Miller, et al., 1996; Miller, et al., 2001; Rajkumar and French, 1997). The scholarly literature on issues of causal inference with observational data is far more extensive than the price literature, and this is reflected in the focus of economic scholarship on theoretical questions with recalcitrant identification problems (Evans and Owens, 2007; Kessler and Levitt, 1997; Knowles, Persico and Todd, 2001; Levitt, 1996; 1997; 1998; Ludwig and Kling, 2007; Raphael and Winter-Ebner, 2001; Waldfogel, 1993). Overall, studies have generally followed one tradition or the other and there are few—if any—studies that treat inference problems and pricing problems with equal weight.

### Limitations of Strategies to Estimate the Costs of Crime to Victims

While most of the extant estimates of the price of crime use jury award data to estimate social costs, more recent work has used contingent valuation methods to develop estimates. In addition, Quality-Adjusted Life Years (QALY's) can be used to estimate prices for crime victims, as can hedonic pricing techniques. However, each approach has some substantial limitations. This section briefly describes the strengths and weaknesses of each approach, with a more thorough discussion in the Chapter 2.

The first cost of crime to victim's studies used hedonic pricing to estimate market prices by observing revealed preferences for the components of a good, usually measured as heterogeneity in real estate prices or wages due to differential risk of crime (Thaler, 1978; Thaler and Rosen, 1975, Clark and Cosgrove, 1990). The hedonic approach is strictly defined as a willingness-to-pay (WTP) method, using ex ante measures of price to estimate equivalent variation. Thaler (1978) used a random sample of single family homes from Rochester, New York to estimate the price of property crime. Clark and Cosgrove (1990) use data on wages, structural features of the home, and individual attributes to estimate the relationship of rental values and safety. The hedonic pricing approach is criticized on the grounds that prices and crime are endogenously determined, and that results are aggregated individual estimates and not equivalent to social costs. With respect to the former issue, Manski (1995:6) calls the use of equilibrium price data to infer demand behavior of consumers and supply behavior of producers "a classic identification problem." On the latter issue, critics note that society's willingness to pay for a policy is different from the sum of individual preferences, due to community-level preferences (Cook and Ludwig, 2000:56-57)).

Recently, studies of the costs of crime to victims have estimated equivalent variation functions, which measure ex ante willingness to pay (WTP) to avoid crime using contingent valuation survey techniques (Cook and Ludwig, 2000; Cohen, et al., 2004). The contingent valuation method uses stated preferences as the proxy for market behavior. The general approach is to use a referendum-based approach following the recommendations of the NOAA Panel on Contingent Valuation to solicit prices respondents would be willing to pay to reduce their (or their communities) risk of



victimization policy (Arrow, et al., 1995). While the Cohen studies cited above adhere closely to the NOAA recommendations, critics of the approach suggest that even those strict standards do not compensate for structural limitations in the method (Carson et al., 1999). Most importantly, the contingent valuation approach elicits stated preferences rather than observing behavior in a binding market transaction. Thus, contingent valuation tends to overestimate willingness to pay (Diamond and Hausman, 1994; Portney, 1994; but see Schelling, 1968; Cook and Ludwig, 2000).

A third approach, QALY's (Quality-Adjusted Life Years) has been used extensively among UK researchers to value the costs of crime to victims (DuBourg and Hamed, 2005) and internationally for the study of health interventions (Grossman, 1972). In health studies, health capital is the product of the sum of discounted health status over time and the value of a year of perfect health (Cutler and Richardson, 1998). In crime studies, the general strategy is to rank-order utility states anchored by high disutility from a criminal event to no disutility from a criminal event. These estimates, drawn from surveys or expert rankings of ex ante expectation of harm from crime, are then used to rank-order the harms from victimization across crime type (Dolan and Peasgood, 2005). Since the intangible costs of crime to be estimated are mainly psychic, a key disadvantage of this approach is that there are no extant estimates of QALY in the presence of heterogeneity of mental health states (Zaric, Barnett and Brandeau, 2000:1104).

The final approach values compensating variations to estimate ex-post willingness to accept (WTA) crime by estimating the amount of compensation necessary to return an individual to their original level of utility (Cohen, 2000). WTA usually combines jury

award data and cost-of-illness data based on ex post estimates of harm from victimization. The general strategy in developing price estimates of victim harms is to observe the distribution of injuries in jury-adjudicated civil cases and use those data to estimate indirect costs of crime. The approach has been widely criticized, on the grounds that estimates are ex post rather than ex ante and that the estimates are aggregated individual losses rather than social cost (Cook and Ludwig, 2000). Most of the studies use a blended cost-of-illness approach and jury compensation strategy (Rajkumar and French, 1997). While the cost-of-illness approach clearly suffers from these limitations, the jury compensation seeks to approximate an ex ante estimation of equivalent variation, and thus is less susceptible to these criticisms.

Of the four approaches, the jury award approach is most amenable to further research to create more robust estimates of crime, mainly due to the limits of the other approaches. The contingent valuation approach has been used with increasing frequency; however, the limitation of using stated rather than revealed preferences appears to be insurmountable, as is the endogeneity problem in hedonic pricing. Research has yet to show that QALYs produce robust estimates of harms to victims. By contrast, the main limitation of the jury award method (in addition to the problem of ex post estimation) has been the limitations of the data. With new data sources such as NIBRS, a re-estimation of harms to victims may yield more robust estimates.

#### Limitations of the Jury Award Method

Apart from the theoretical criticisms, the main analytic challenge to this approach has been that while micro-level data relating injuries to prices are available, micro-level

data relating injuries to crime were not available in the past. Thus, in past research, the two main sources of information about the harms from criminal victimization cannot be linked at the micro-level. As a result extant price estimates are indirect estimates of harm. One contribution from this dissertation is that the analysis will directly link data at the micro-level.

To address this problem, data from multiple sources are mapped together to describe the relationship between jury awards and crime. Typically, the research first develops an estimate of the association between injury and civil jury award, to estimate psychic losses. Next, health service utilization data is used to estimate the costs associated with crime-related injuries. Next, the prices from the crime-related injuries are weighted according to the distribution of injuries in criminal populations. From these estimates, an award per injury is estimated. Those estimates are then mapped onto data describing the distribution of injuries in all crimes to arrive at estimates of the direct and indirect costs of crime (Cohen, 1988; Miller, Cohen and Wiersma, 1995; Rajkumar and French, 1997; Cohen and Miller, 1999; Cohen, 2000; Cohen and Miller, 2003; McCollister, 2004). Most econometric studies that include an estimate of the social costs of crime rely on WTA jury award data (see Waldfogel, 1993; Levitt, 1996; Lochner and Moretti, 2004). The main empirical criticism of the WTA approach is that it undercounts losses by excluding income taxes, but may overestimate losses if jurors anticipate legal fees (Rajkumar and French, 1997). There is also a concern that civil cases include an unrepresentative sample of criminal victims (Cook and Ludwig, 2000)

Thus, the policy challenge can be summarized as follows. Neoclassical economics theory requires estimates of the price of crime to victims in order to develop models that

predict optimal crime control expenditures. Four strategies are available to develop these estimates, but all face theoretical or empirical objections that have not been resolved in the literature. The fourth approach (WTA) is subject to theoretical critiques on the grounds that estimates are ex post, but estimates developed using this technique are cited more often than estimates from other approaches. However, because past studies have not been able to link micro-level jury award data and micro-level crime data, the results have not been robust to study design. The next section describes prior research using jury awards to estimate crime prices, and the sections that follow propose a strategy for resolving those objections.

#### The Cohen, Miller Literature Using Jury Awards to Estimate Victim Costs

The WTA approach generally uses several data sets to generate estimates of the harms from crime: civil jury award data that describes the relationship between injuries and jury awards for both economic and non-economic damages and data that links injuries to specific types of crime. In response to concerns (especially during the 1990s) that jury awards were increasing in size and were therefore out of proportion with the real harms suffered by victims, a large literature emerged to evaluate the validity of jury awards (this literature is discussed in Chapter 3). The general conclusion of that literature is that juries are credible arbiters of civil claims, and that the factors that cause juries to make awards beyond the specific claims of the plaintiff can generally be observed in jury data. Because jury award data are more readily available than are data for other estimation strategies (such as hedonic pricing), most of the estimates of the price of crime are developed from jury data.

Still, early efforts to assemble civil jury awards in a single dataset yielded limited results. There was—and continues to be—no national reporting data source that aggregates information about cases processed in the civil court system. Data on civil awards is primarily collected by private entities that collect and aggregate award data. Prior to about 1985, the principal jury data aggregators did not use representative sampling strategies. Rather, cases were sporadically collected and data collection was focused on unusual cases (often cases with the largest verdicts). Thus, the samples were therefore a non-representative sample of jury verdict awards, and research using those data faced almost insurmountable identification issues. Since 1985, civil jury award data have been collected using stratified, random sampling. The number of jurisdictions included in the sampling frame has increased as well reducing the severity of identification problems associated with the use of these data<sup>4</sup>.

The main challenge to research using civil jury awards is that few of the awards can be directly linked to a criminal event (for a thorough discussion of this issue, please see Chapter 3). For example, in the first study to use jury award data to estimate intangible costs, Viscusi (1986) was limited to a sample of 159 awards. In two recent studies Miller and Cohen use a national jury award dataset from 1980-1991 was used that included over 100,000 cases. However, for one paper, they can identify only 1,467 intentional injury cases, including 1,106 physical assaults and 361 sexual assaults (Cohen and Miller, 1999) and for the other 514 physical (non-sexual) assaults (Cohen and Miller, 2003). It is also difficult to identify property crime cases. It should be noted, however, that it is likely that many additional civil cases result from criminal behavior, but it is difficult to identify

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<sup>4</sup> This history of the jury award data is developed from a series of conversations with Nick Pace, a long-tenured social scientist at the RAND Institute for Civil Justice.

those claimants in the available data. As a result, most published estimates of the price of crime (or costs to victims) are limited to civil cases involving Part I crimes.

Prior research has been concerned with two issues related to these data. First, that the cases in the sample over-represent the most serious crimes. A second and related issue is that the crimes that can be directly observed may over-represent the most serious crimes within each crime category. The general strategy to work around this problem has been to use the jury award data only to assess indirect losses (such as pain and suffering) and to use other data sources to observe direct losses (such as property loss, lost wages and health care expenses). Typically, some combination of data from a cost-of-illness source is used to value the physical harms from a criminal event, and an additional source (generally the National Crime Victimization Surveys) is used to value the other direct harms from crime (such as property loss and lost wages). This analytic strategy is limited by the lack of case-level data that includes information on both the type of crime committed and the harms from the criminal event. Thus, researchers have had to indirectly observe the relationship between injury and crime.

In a series of widely cited articles over the last two decades, Vanderbilt economist Mark Cohen writing alone (Cohen, 1988; 1990; 2000; 2005) and with Ted Miller (Cohen and Miller, 1994; 2003; Cohen, et al., 1995; Miller, et al., 1996) has used jury award verdicts to predict the cost of crimes to victims. The studies are among the most cited in the law and economics literature. According to Google Scholar, the Miller, Cohen and Wiersema study published in 1996 has been cited slightly more often than a well-regarded article published the same year by Levitt, who estimates the effects of prison on crime rates. In these studies, Miller and Cohen use jury award data to estimate an

expected award for combinations of victim and injury attributes and use prevalence of injuries in the population of crime victims (from the National Crime Victimization Survey) to re-weight the data and calculated the average loss across crime categories.

The approach has several limitations. The approach relies on relatively small samples of data (generally a few hundred observations) and price estimates are generated for only a few, broad categories of crime. The re-weighting is accomplished using a regression analysis that has substantial limitations, in particular, that estimates are derived from non-significant parameters. In addition, because the articles describing the work have mainly appeared in publications for general audiences rather than scholarly journals, the method has not been transparently presented and thus is difficult to replicate.

#### Modifying the Jury Award Method to Develop New Estimate of Costs to Crime Victims

There are now two sets of data available to update the jury award estimates that should allow for more direct estimation of victim costs than were available to MC. First, the RAND Institute of Civil Justice has collected jury award data over a long series (1985-2000) across a large number of jurisdictions, representing about one-quarter of the US population. The data were collected according to a more consistent protocol than was used in the first Cohen studies<sup>5</sup>. Second, data from the National Incident Based Reporting System (NIBRS) allow for estimates to be developed about the generalizability of jury award data, by comparing the prevalence of crimes resulting in jury awards to a broader sample of all crimes. In this paper, the RAND data and the NIBRS data are

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<sup>5</sup> Personal communication with Nick Pace, Principal Investigator of the jury award study at the RAND Institute of Civil Justice. He notes that the data from 1980-1985 were collected haphazardly by jury reporting services, and cautions against using those data for analysis (RAND collected data from this period in Phase II of their study). Data in the study are therefore limited to Phases III-V (1986-2000).

joined to generate direct estimates of the costs of crime to victim. The main advantage of this approach is that the costs of crime can be directly estimated from these two sources, without requiring the use of health data as a middle step to join the data.

#### Jury Data

Data from the RAND Institute for Civil Justice on the size of civil jury verdicts to estimate the monetary harms associated with criminal victimization will be the foundation for developing estimates of the costs of crime to victims. In total, there are 38,141 cases in the combined jury award database. Data are available from 1985 to 1999, and the number of cases increases gradually over time from 1,971 in 1985 to 2,723 in 1999, with a peak of 3,443 in 1997. The data include all jury awards in the states of California (14,803) and New York (10,416), and the cities and surrounding counties of Chicago, IL (4,332), Houston, TX (4,410), Seattle, WA (1,207) and St. Louis, MO (2,973). The data were coded by RAND researchers from trade publications ('jury verdict reporters') that report the outcomes of civil proceedings. Cases in the file include any civil case resulting in a verdict that was adjudicated by a jury in the project period. Cases closed by settlement, hung jury, or cases heard by a judge are not included in the RAND dataset. The plaintiff won in 47.5% of cases, and only these cases are used in this analysis. There is substantial variation in awards in these cases, with a mean of \$1.22 million and a mean of \$147,000. One percent of cases have an award greater than \$10 million, and one percent has an award under \$1,000.

As noted earlier, the jury data are not coded in a way that allows for direct observation of an underlying criminal act. Only about ten percent of the cases involve an intentional tort and can thus be affirmatively coded as a civil case resulting from a



criminal act. However, as is discussed in Chapter 3, there are certainly other criminal cases in the data set, particularly among the negligence and auto liability. However, these cannot be affirmatively coded as having resulted from a criminal incident. Thus, the preferred strategy is to use the co-variation between case attributes and an affirmative coding of a case as having resulted from crime to give more weight to cases that resemble ‘crime’ cases and less to those cases that do not.

### NIBRS Data

NIBRS data is a product developed by the FBI and the Bureau of Justice Statistics to capture detailed characteristics of criminal events. Unlike the FBI Uniform Crime Reports which captured aggregate monthly statistics on the number of offenses, NIBRS data is reported at the incident-level. Data include information about the offense (type of weapon, completed or not), the offender (age, race and gender), the victim (age, race, gender, ethnicity and type of injury), type of property stolen or damage (including value), and arrestee data (age, race, gender, ethnicity, age and type of arrest). As described below, an analogue to some of these measures is contained in the jury award data. Data in NIBRS are available for ten states (Colorado, Idaho, Iowa, Massachusetts, Michigan, North Dakota, South Carolina, Utah, Vermont, Virginia) between 1998 and 2003, where more than half of the population in a given state is covered by NIBRS reporting.

Prior research in this area was performed prior to the development of NIBRS. NIBRS offers some advantages over NCVS, which has been used more often in prior studies to create weights to adjust for the prevalence of crimes in the general population. Most importantly, NIBRS includes far more observations, and thus more observations of the rarest, but most serious crime. Since the small number of serious person crimes

explain most of the total cost of crime, access to these data are critical to the development of price estimates. In addition, some studies have shown that NIBRS data are more reliable on some measures than NCVS. Chilton and Jarvis (1999) find that that some differences are observed between NCVS and NIBRS, especially on victim and defendant attributes. They note that NIBRS data closely resemble data on Supplemental Homicide Reports, suggesting that NIBRS data may be more reliable overall. In addition, NIBRS includes events linked to individuals excluded from NCVS, including military personnel, the homeless, the incarcerated, and itinerant guests, a group that likely includes many recently released ex-prisoners (Maxfield 1999). And, commercial businesses are excluded from NCVS, and thus some victims of “robbery, burglary, arson, larceny-theft, motor vehicle theft and vandalism” may be excluded (Maxfield, 1999:128). In addition, victimless crimes are not included in NCVS but are recorded in NIBRS. Finally, NCVS explicitly excludes victims under 12 years of age, who are included in the NIBRS data.

The NIBRS data for 2000 include 3.8 million observations for which data are not missing for the victim and the type of crime. Almost 97% of crimes in NIBRS were completed. The average direct loss from crime in NIBRS was \$1,432 and the median loss was \$100. Data are also available on recovered items for 325,000 cases, with an average recovery of \$3,075 and a median recovery of \$90. Subtracting recovered losses from all losses does not substantively change the mean or median direct loss. Automobiles (10.3%) are the only type of property lost for more than 10% of cases.

## Methods

The general estimation strategy is to predict the cost of crime to victims for as many different types of crimes as possible. The general model is regress jury awards on crimes, controlling for confounding explanations, according to:

$$\text{JURYAWARD} = \beta(\text{CRIME}_i) + X_i'\theta + \varepsilon \quad (1)$$

where the JURYAWARD is the total award for a case (including general awards, specials and punitive damages), CRIME is a vector of criminal event types, and X is a vector of theoretical attributes associated with non-claimant related jury awards. The parameter on CRIME is the average award for an additional crime of each type, which is interpreted as the cost to a victim of that type of crime. There are three significant issues with this model as specified.

However, (1) cannot be directly estimated since there are few observations within the RAND data set that directly link a jury award to a criminal act. As noted, only about 10% of the observations in the RAND data are intentional tort cases. Some other victims of crime can be identified by the type of injury suffered (sexual assault or gunshot, for instance), but for most cases the underlying criminality of the event that lead to the civil case is not observable. As a result, the RAND jury award data are not sufficient to directly estimate the average costs of crime to victims. This problem can be resolved by specifying a different model that exploits all of the observations in the RAND data set. In the RAND data set, virtually all of the observations include some type of physical harm, and data are available to describe those harms. The RAND data could be used to estimate direct costs of victimization and indirect damages such as the harms from pain and

suffering as a function of different injuries. Since comparable victim injury data are available in both the RAND and NIBRS data sets, the combined dataset would contain information about both crime and damages. NIBRS data categorizes injuries as broken bones, possible internal injury, severe laceration, apparent minor injury, loss of teeth, unconsciousness, other major injury and no injury. RAND data includes much finer injury descriptions that can be aggregated to match the NIBRS coding. Since injury data are available for all cases in the RAND data, using injury data as the variable used to link the RAND data would increase the number of incidents in NIBRS (and the number of CRIME's) that could be matched to a jury award. Once the jury award data associated with different types of injuries (and victim age and gender) are computed, these data can be interpolated onto the NIBRS data to predict awards for criminal acts. These awards can then be interpreted as the cost of crimes to victims.

The analysis will be conducted using three samples from RAND. First, all cases in the RAND data will be used to estimate crime prices. The main advantage of the RAND dataset over alternatives used in past studies is that the data include a much larger sample of jury awards. Prior studies have focused only the crimes that are directly observable in jury data, and this has limited the studies estimates of costs of crime to victims to a small number of crimes. Because the RAND data include more than 12,000 observations, it is possible to identify more cross combinations of attributes, and to estimate the price of crime for a larger number of crimes. Thus, one set of estimates from this study will use all of the observations in RAND to develop price estimates.

Second, prior studies have focused only on those jury cases that can be directly identified as having resulted from a crime. That is, only those cases that can be

affirmatively coded as having resulted from a criminal act are included in the data.

However, as will be discussed in Chapter 3, this means that some criminal cases will be excluded. Nevertheless, it seems prudent to include estimates of the price of crime that are limited to those observations that are known to result from a crime.

Third, a propensity score analysis will be conducted to generate weights that balance across those observations that can be affirmatively coded as crimes and those that cannot. Theoretically, this should not be an issue. Juries are instructed to make awards only as a condition of the actual harms suffered by a victim and not the facts that led to that injury, excluding these studies may unnecessarily limit the power of the analysis. However, when aggregating across types of claim, if the cause of the injury (such as medical malpractice), and not just the characteristics of the injury, were correlated with the size of the jury award than the estimator would be biased. To address this potential selection problem, propensity score models will be used to model the likelihood that the characteristics of an injury reduce bias from selection on observables following the propensity score approach described in Rosenbaum and Rubin (1983). Propensity score matching is a statistical algorithm that matches treated and untreated participants in the presence of multiple determinants of treatment status (Heckman, et al., 1997). While the propensity score approach is preferred *ex ante*, all three models will be reported.

A major concern with this study (and past studies) is that the jury data are not representative of all criminal victims, and thus selection effects may bias the interpretation of crime prices that are estimated from jury awards. Potential sources of bias include heterogeneity in awards by type of civil claim, greater severity in criminal cases that result in civil jury award, and heterogeneity in the prevalence of victim

attributes and injuries. Unfortunately, this study is limited in the extent to which empirical tests can be conducted to determine whether the RAND data are representative of all crimes. A limited number of empirical tests of this question are discussed in Chapter 4. A discussion of the potential implications of this issue is discussed in Appendix A.2.

#### Interpolating the RAND data onto NIBRS

The next step in the analysis is to map the data in the RAND dataset onto the NIBRS dataset. By combining NIBRS and RAND data, a much larger set of observations can be used to estimate the costs of crimes to victims. This approach requires two assumptions. First, that the observed jury award associated with any injury in the RAND data set would be the same as an unobserved jury award for an identical crime observed in the NIBRS data. And second that the observations are comparable, even though the two data sets describe different populations. There is no geographical overlap between the two groups, and the time periods covered by the two data sets only coincide for two years. Finally, since there are no common observations the data cannot simply be merged. The RAND data also cannot be appended to the NIBRS data, since the dependent variable is not observed in the NIBRS data. However, the two data sets do contain data on variables that are consistent across the two groups, and those similarities can be exploited to create a unified data set.

First, common elements are identified between the two sources. These include:

- Victim age (20-29, 30-39, 40-49, 50 and older);
- Victim Gender (Male or Female);

- Type of injury (broken bones, possible internal injury, severe laceration, apparent minor injury, loss of teeth, unconsciousness, other major injury and no injury);
- Urban, rural or suburban;

Next, a variable is created designating each cross combination of attributes and a value is assigned to each observation in both RAND and NIBRS. Within each cross-combination, a distribution of awards is created. For each identical cross combination in NIBRS, the mean of the cross-combination in RAND is interpolated onto all the observations in NIBRS with that cross-combination of attributes. Data in NIBRS are then re-ordered within crime categories, instead of within injury categories, allowing calculations of mean and median victim harms within each crime category. Estimates will therefore be generated for 31 crime categories.

Two adjustments are then made to the above method. First, it should be noted that the support space in RAND and NIBRS do not perfectly overlap. Thus, there are cross-combinations of attributes that are observable in NIBRS that are not observable in RAND. To account for this, prior to running the selection models, only those observations that fall into a cross-combination that is observable in both datasets is selected. In practice, this has the effect of excluding some NIBRS observations (about 29%) from the final analysis.

The analysis described above is not performed for every type of crime in the NIBRS dataset. In the extant literature, serious person crimes are often assigned both an indirect and a direct loss. This study follows that precedent and assigns all jury award values to serious crimes, which are defined as those cases that are likely to have property losses, other direct losses and indirect losses. However, many—if not all—other types of crimes likely have a psychic component as well. To account for this, a regression analysis is

conducted on the logged jury award to predict the ratio of direct to indirect losses in those cases where both values are observable in RAND. This ratio is then used as a multiplier in the NIBRS datasets for crimes that fall within the same strata as crimes that have previously been assigned indirect losses. In a somewhat arbitrary decision this indirect cost multiplier is applied to cases that are likely to have only property losses and no other direct losses. For consistency, the ratio of indirect and direct losses is therefore calculated only for those RAND cases where there were no or minor injuries. For the other ‘minor’ crimes, victim harms are calculated only as the property losses reported to police.

#### Structure of the Dissertation

The dissertation is divided into four chapters. The second chapter describes the foundations of neoclassical theory that informs the development of cost-benefit analysis, and describes the evolution of cost of crime estimates, including the literature on the meaning and interpretation of jury awards that motivates the analysis, with particular attention to the adaptation of this method in the work of Miller and Cohen. The third chapter describes the first stage of the analysis, describes the data, explains the propensity score models and culminates in the estimate of the expected jury award by injury type. The fourth chapter describes the interpolation models used to impute values for crime victim profiles observable in the NIBRS and the RAND data, the results of those models, and the final results of models that use the interpolated and observed jury award values to estimate the costs of crime to victims. The fifth chapter describes the policy and research implications with particular attention to two issues: the opportunity for these estimates to be used to create more efficient use of public safety resources, and, the challenge of



implementing efficient policies where those who pay the costs are different from those who receive the benefits.

## Chapter Two: Review of Prior Research on the Price of Crime

The motivation for studying the costs of crime to victims is straightforward. Most theorists posit that potential offenders that implicitly measure the costs and benefits of committing a crime before acting. Costs of crime include the risk of detection and the severity of punishment. While this supply model of offending is well developed, the demand side of the equation is less clearly articulated. Ehrlich (1996) proposes a demand model where demand for crime is derived from the supply of offenses function. Thus, the potential losses from crime (the analog of the benefits to offenders) determine the amount of public and private crime precaution. It is therefore assumed that the amount of harm experienced by victims of crime can be compared to the costs of detecting and responding to crime to make efficient allocations of scarce anti-crime resources. More intuitively, this model posits that the costs of a program or policy designed to reduce crime should be less than the benefits that are measured as reductions in harm.

This model has been widely applied. Levitt (1996) compares the costs of deterrence from incarceration to the benefits of averted crimes from incarceration in evaluating the efficiency of prison. Lochner and Moretti (2003: 23) estimate “the social savings from crime reduction resulting from a 1% increase in high school graduation rates” using this approach. Lott and Mustard (1997) use this approach to value the savings from increasing gun possession and Ludwig and Cook (1999; Cook and Ludwig, 2000) use this approach to show the savings from decreasing gun ownership. The approach is widely used to evaluate the savings from early interventions with children (Olds, Henderson, Chamberlin, and Tatelbaum, 1992; Schweinhart, Montie, Xiang,

Barnett, Belfield, and Nores, 2005). More recently, a large literature has begun to emerge that measures the costs and benefits of various anti-crime strategies where the benefits are understood in this way.

Controversy surrounds the choice of method to estimate costs of crime to victims. The challenge to developing a robust estimate of harms to victims emerges from the lack of a true market for the exchange of crime, as there is obviously no market place where private actors voluntarily exchange and thus all of the effects of crime are externalities. The various strategies all fall under the general rubric of contingent valuation studies which use secondary markets to price unobservable events. Contingent valuation methods can apply an *ex ante* approach, where stated preferences are used to estimate harms, a revealed preferences approach where choices about job or home selection are parsed to estimate preferences for crime risk, or a reimbursement approach, where neutral parties' estimation of the inflicted harms are applied.

By far the most widely cited method is the third-party valuation approach. In a series of widely cited articles over the last two decades, Vanderbilt economist Mark Cohen writing alone (Cohen, 1988; 1990; 2000; 2005) and with Pacific Institute of Research and Evaluation economist Ted Miller (Cohen and Miller, 1994; 1999; 2003; Cohen, Miller, and Rossman, 1995; Miller, Cohen and Wiersma, 1996) have used jury award verdicts to predict the cost of crimes to victims using a cost of illness model. The logic behind this approach is straightforward. As discussed, crime losses to victims are external to the crime transaction in the standard supply of offenses models. Some victims of criminal acts will seek to have the costs of their harm internalized by the offender (or other liable party), and will use the tort system as the means of recovery. When plaintiffs

win a case, compensatory awards are made in two domains - 1) an award for the recovery of direct costs to victims and 2) an award for the recovery of intangible (fear, pain and suffering) costs. Awards in a third category—punitive—are generally excluded from calculations of the true harms to victims. Reductions in awards due to shared negligence are also excluded from calculations of victim harm on the grounds that harms are a function of loss rather than culpability. Thus, victim losses are measured as the sum of observed harms resulting from a criminal incident.

The cost of illness approach is just one of a class of contingent valuation techniques used to develop estimates of the value of a resource using data from secondary or theoretical markets when there is no market place for the exchange of that good. While the term ‘contingent valuation’ is often used as short-hand to refer to stated preference surveys, contingent valuation methods can be divided into both those approaches that rely on revealed preferences and those that rely on stated preferences. Most recent work has applied willingness to pay methodologies where stated preferences are used to estimate prices and quantities for unobservable markets. This includes estimates to understand the costs of gun violence (Ludwig and Cook, 1999; Cook and Ludwig, 2000) and estimates of the costs of crime across broad categories of harm (Cohen et al., 2004). However, the revealed preference approach to estimate harms dominates the cost of crime literature and most econometric studies, such as the efficiency of deterrence and the effectiveness of improving high school graduation rates, relies on the third party approach. The two strains of contingent valuation can be generalized as follows:

- Willingness-to-pay estimates (WTP) are based on the price an individual would be willing to pay to avoid harms, such as death or disability, resulting from crime.

Methods of estimating willingness to pay include:

- Required compensation, which estimates the price that would have to be paid to an individual in order for that individual to be indifferent about exposure to a dangerous event;
  - Hedonic pricing, where differences in the risk of criminal victimization are observed as a component of property values or wages from which estimates of the amount individuals would pay to avoid being victimized can be inferred; and,
  - Quality-of-life (often expressed as Quality of Life Years or QALYs), which estimates costs according to degrees of disability.
- Victim-compensation or willingness to accept (WTA), is the converse of willingness to pay, and is the aggregated amount that would have to be paid to a victim to make them whole after criminal victimization. Methods of estimating willingness to accept include:

- Jury compensation, which values victim costs including health care, lost productivity, and intangible costs such as pain and suffering at the rate juries compensate victims of crime;
- Discounted future earnings estimates, which are based solely on the costs (or averted costs) of lost productivity due to an incident; and,
- Cost of illness, which uses survey data to aggregate the tangible cost of crime (including health and productivity).

Both strategies have limitations. The main criticism of Willingness to Accept models is that estimates of the cost of victimization are estimated ex post rather than ex ante. Thus, WTA violates a fundamental assumption from price theory that choices about the use of scarce are based on expectations of utility and thus precede the transaction. WTA is in effect an instrument estimating how an individual would have behaved in the market had they been presented with a choice. Since there is no way to measure the correlation between how a pre-transaction choice to the necessary compensation to make a victim whole, the strength of this instrument cannot be evaluated. In addition, WTA relies heavily on an individual's current context to determine appropriate compensation.

If an individual is very young, elderly, or not employed, their losses may be underestimated. When these estimates are extended to include a longer time frame (e.g. discounted future losses are included), these issues will be magnified.

The WTP model generally uses a required compensation approach to survey individuals about their preferences. The model has been well studied, and most research closely follows the methodological recommendations of the NOAA Panel on Contingent Valuation (Arrow, Solow, Portney, Leamer, Radner and Schuman, 1993). In crime research, WTP generally involves asking people what they are willing to pay to avoid being the victim of crime. Thus the model solves the problem of ex ante decision-making. However, the WTP model has two other serious problems. First, as a general rule, it overstates revealed preferences, since people will often express a willingness to pay for goods that exceed what they would actually be willing to pay if a real payment was required. Thus, research suggests that the dollar value developed in these models represents at minimum an upper bound of people's willingness to pay. Second, it is difficult for respondents to accurately estimate their willingness to pay in the presence of uncertainty about risk. Most serious crimes are rare events as on average Americans have a less than 1% chance of experiencing even the most common Part I crime of burglary (FBI, 2008). Risk of victimization is heterogeneous with respect to age, wealth and many socio-economic factors (Tonry and Farrington, 2002), and risk aversion with respect to crime is similarly heterogeneous (Becker, 1968; Ehrlich, 1973).

This chapter is ordered as follows. The chapter begins with a brief review of the neo-classical economics literature on supply and demand of offending focusing on the development of market-based models that incorporate a demand function. Next, the

literature comparing the strengths and weaknesses of the WTA and WTP models is reviewed with particular attention to the three main estimation strategies: hedonic pricing, stated preference surveys and third-party compensation. Next, the Miller and Cohen studies that have applied jury compensation and cost of illness methods to estimate the costs of crime are reviewed. This is followed by a discussion of prior research on jury awards, and brief discussions of the issues related to estimating risk of death.

### **Economic Models of Crime**

Development of accurate estimates of the value of harms experienced by crime victims plays a central role in the development of policy models to optimize the allocation of crime resources. Incorporation of these values into theoretical models of offending is particularly evident in the economics literature. In general, there are two strains of economic theory about crime. The first, and by far the most dominant, is based on the pioneering work of Becker. Becker describes a model where offenders are suppliers of crime. The model specifies two functions: a supply of offenses function and a production function, specifically a social loss function. Offenders maximize their utility by comparing the expected gains from offending compared to costs measured as the certainty and severity of punishment. If a potential offender perceives that the benefits from crime, both pecuniary and psychological, outweigh the expected costs of crime (risk of being caught and incarcerated) then the motivated offender will commit a crime. The simple static models of Becker were extended by many economists, notably Shavell, Polinsky, Witte and Garoupa to accommodate the complex choices inherent in a criminal event. Most of the first generation of the economics of crime literature (Posner, 2005) is

concerned with the appropriate sanction to dissuade offenders from offending. If financial penalties, and penalties in the form of a loss of liberty, could be made sufficiently large, offenders would not commit crimes. The market described in this literature is a partial equilibrium model describing the changes in the supply of offenses from a mix of exogenous (public demand) and endogenous (private demand) factors.

The other strand of economic theory is much less well populated, centered on the work of Isaac Ehrlich. A contemporary of Becker's, Ehrlich extends Becker's model to include a third function – that of derived demand for offenses. The model is solved using simultaneous equations to derive optimal levels of punishment to minimize social harm from offending. Most theorists who follow Becker and Ehrlich, however, focus their scholarship on extending the Becker model, to include uncertainty, variance in punishment strategies, time, and other complexities. Most of this literature neither explicitly includes a demand function, nor an accounting for the endogenous features of the supply and production functions. Regardless, the result has been an extremely flexible theory that can be used to evaluate the effectiveness of various punishment regimes on crime production.

With the exception of Ehrlich (1981), economic theorists consider the crime problem from the perspective of the offender, what Ben-Sharar and Harel call a “perpetrator-centered perspective” (1996: 304). In these models, the focus is on changing incentives of offenders who supply crime, with only the briefest attention to potential victims, who are assumed to have a demand for victimization. The oddness of victims demanding crime is acknowledged by its authors: Cook (1986:19) calls it a “strange-sounding term,” Shavell accepts that it is “unintuitive,” and Ehrlich notes that it is “seem[s]



paradoxical” (1981:309) and caveats that potential victims do not necessarily demand crime, but rather “tolerate” it (1996: 49). In general, economic markets are predicated on the idea that goods are exchanged voluntarily. When a market results in costs being imposed on an unwilling party, those costs are described as an externality, and are counted as a consequence of the exchange of a good, but are not included within the private market equilibrium. The analogy is akin to the problem of pollution, where costs of production of a good do not include the costs to society from the release of pollutants. The solution in the environmental literature is not to consider those costs as external to the market, but rather to internalize pollution costs within the production of the pollution causing good. Similarly, economic theory of crime considers crime to be an externality, and the goal of law enforcement is to cause criminals to internalize those costs.

Serious investigation by economists on the impacts of crime began only in the late 1960’s and early 1970’s. Becker (1968) proposed a supply model of crime to explain an individual actor’s decision to commit crime. In Becker’s model, a potential offender’s decision to commit an offense can be understood using the standard economic choice principal of expected utility. That is, an individual will decide to commit a crime if the expected benefits (minus expected costs) exceed the benefits from alternative activities. In this model, the number of crimes an individual commits is a function of the perceived risk of offending (e.g., the risk of apprehension (certainty), and if apprehended, the risk of punishment (severity)) compared to the perceived rewards/benefits (e.g. income/utility received as a result of the crime). Changes in policy that affect the probability of conviction and the probability of punishment deter offenders by increasing the expected certainty and severity of punishment. In calculating the total cost of punishment, Becker

includes both costs to offenders (including earnings foregone while incarcerated) as well as the cost and/or gain to other members of society.

Becker extends the model to evaluate optimal penalties for offending. Applying the economic theory of choice to the supply model of offenses, the equilibrium price of punishment is in equilibrium when the monetary value of the penalties received equals the marginal external harm caused by the crimes (1968). That is, optimal policy setting will equate crime prevention expenditures and the amount needed to compensate or minimize the loss to society that results from crime (although it is not necessary that the victim is directly compensated). The losses from crime include the sum of social damages--including victim losses--as well as the costs of detection, conviction, and punishment (Becker, 1968: 207).

Becker does focus only on incapacitation, and argues instead that fines have advantages over other forms of punishments. According to Becker, fines use fewer social resources, can be used to compensate victims and society while punishing offenders (which is not the case when prison is used), and the adoption of fines simplifies the determination of the optimal punishment price. Accordingly, the optimal amount of a fine should be determined based on the total harm caused by the offender. While both fines and prison cause offenders to internalize the costs of offending, neither have a direct benefit to the victim. Prison and probation sentences are set equal to the amount of harm caused by an offender, but do not directly compensate victims. While fines could be used as transfer payments to compensate victims, in practice any transfer occurs in the civil rather than criminal justice system.

Becker applies this model to the development of optimal crime policy, asserting that the effectiveness of public policies is dependent on the costs of detection (certainty) and punishment (severity), and the elasticity's of offenses to changes in the probabilities of apprehension and punishment (Becker, 1968: 205). Thus, anti-crime investment is optimized when the costs of apprehension and conviction are minimized and the reduction in offending caused by a change in the probability of apprehension or punishment is maximized. Becker suggests that elasticity's may vary by type of offense with expressive crimes or crimes committed by youth being less responsive to changes in the probabilities of detection and punishment than instrumental crimes. Becker notes that Ehrlich (1967 in Becker, 1968: 205) did not find the elasticity's of murder, rape and auto theft to be significantly smaller than those for robbery, burglary, and larceny.

Becker's supply model of crime explicitly includes private expenditures and actions aimed at reducing the number of crimes, such as hiring security guards, operating security alarms, or avoiding certain areas at night. Becker develops a model for the optimal allocation of private anti-crime resources, where private expenditures are a function of optimal expected individual loss in income in much the same manner as the optimal public policy function described above. Thus, optimal private expenditures and actions taken to prevent victimization would equal the expected harm or loss to individuals from crime. When the private and public models of optimal crime allocation are considered jointly, there is evidence of endogeneity where higher public anti-crime expenditures will result in reduced private expenditures.

Ehrlich (1973) extends Becker's supply model of offending using a model explaining offending as a function of a choice between legal and illegal actions. Similar

to Becker, the choice to commit crime is based on risk tolerance, and legal and illegal opportunities. As a result of differences in risk tolerance, potential offenders will differ in the degree to which the threat of detection and punishment will deter their future offending. Ehrlich then extends his model of individual choice to an aggregated supply of offenses, which is the sum of all offenders' choices about whether to commit crimes given variation in perceived benefits and varied risk preferences. Those who are risk averse will not commit crimes even when penalties are small and potential gains are large, and those who are risk tolerant will enter the crime market even if risks are high and returns are small. Ehrlich predicts a number of factors will influence this decision including the level of assets in the community, as well as the individual's educational attainment and legitimate opportunities.

In this aggregated formulation, Ehrlich identifies the relationship between certainty and severity of punishment and the resulting incidence and prevalence of offending. Thus, for the first time in the economics of crime literature, victims cost of crimes become an explicit variable in the offending model. Ehrlich develops a production function of law enforcement where the productivity of increases in law enforcement expenditures on law enforcement (i.e. police and courts) is related to the level of crime. So, if law enforcement expenditures are constant higher crime rates will result in smaller probabilities of apprehension and punishment. Ehrlich also notes that population size and population density are also expected to be negatively related to the probability of apprehension and punishment.

The public demand for law-enforcement activity is described as, "a negative demand for crime or a positive demand for defense against crime" (Ehrlich, 1968: 541).

Although Ehrlich recognizes that potential victims of crime may wish to engage in both private and collective self-protection activities, Ehrlich focuses on self-protection in the form of law enforcement activity. Law enforcement activity that reduces the crime rate through increases in the probability of apprehension and severity of punishment benefit the public by reducing losses to victims. Thus, Ehrlich introduces the idea that of a trade-off between the costs of adding police and prison resources and the benefits of reduced victim losses.

In later analyses, Ehrlich (1981) argues that the benefits of incarceration may be limited, as criminogenic forces may offset the deterrent effects of incarceration. That is, imprisonment may result in a decrease in “legitimate knowledge and skills” and consequently cause an increase in recidivism rates of offenders after they are released. Additionally, a “replacement effect” in which new offenders decide to enter the crime market or active offenders decide to increase their participation in criminal activity may also offset the incapacitation effect of punishment. Ehrlich (1981: 316, 1996: 62) suggests that the benefits of incapacitation should thus be attributed to general rather than specific deterrence. The larger the elasticity of offending with respect to the certainty and severity of punishment the larger the general deterrent effect will be, and the smaller the specific or incapacitative effect.

Shavell (1987) develops a model in which it is optimal to incarcerate an offender if the offender’s dangerousness (harm done each period if not imprisoned) exceeds the cost of imprisonment. In this model, a prisoner is incarcerated only as long as his expected harm to crime victims exceeds the cost of imprisonment. Thus, any decrease in expected offending should be considered in assessing the efficiency of continued

incarceration. The Shavell model therefore requires explicit enumeration of the expected pattern of offending (commonly referred to as  $\lambda$  in the criminology literature) conditioned on such predictors as age and receipt of rehabilitative services.

More recently, Ehrlich (1996) extends the economic analysis of crime into a market model of crime, which explains the phenomenon of crime through a fully specified equilibrium model. A key assumption of the model is that some level of crime is “socially optimal” or tolerable (Ehrlich, 1996: 51). The supply of offenses and the demand for private and public protections from crime are jointly modeled to predict the optimal level of crime. Using this framework, Ehrlich (1981, 1996: 48) expands upon Becker’s private expenditures model to derive a “demand for crime or tolerance for crime” function, which is the inverse function of the demand for private protection. When aggregated with the supply of offenses model, higher crime rates result in increased self-protection efforts by potential victims, which result in reduced gains to offenders per offense. Reduced net gains are a result of the increased time and effort required to commit an offense when self-protection rates increase. In the market model, offenders, law enforcement, and potential victims are all assumed to behave in accordance with the concept of optimal utility, with the distribution of preferences for crime and for safety assumed to be fairly stable in the short-term. With crime defined as an external diseconomy, or a direct negative effect on welfare, and law enforcement being a “non-exclusionary public good,” it follows that the main objective of law enforcement is the “maximization of social welfare” (Ehrlich, 1996: 45). Again, the model requires a specific enumeration of victim harms to estimate the equilibrium.

According to Ehrlich, the market model of crime will reach equilibrium when 1) the public has made its optimal investment in crime control, 2) offenders have made optimal decisions on how many offenses to commit based on perceived risks and rewards, and 3) individuals have made their optimal private investment in self-protection from crime (1996:51). The equilibrium level of crime is dependent on the shapes of the supply of offenses and the derived demand for crime curves. Changes in sentencing guidelines and court holdings, which impact probabilities of arrest, conviction and punishment, will result in long-term shifts in the equilibrium of the crime market. According to this model, effective rehabilitation and incapacitation policies should result in “a leftward shift in the aggregate supply-of-offenses,” or a decrease in the number of offenses (Ehrlich, 1981: 312).

The market model therefore allows for the evaluation of the relative efficiency of competing policies. For instance, Ehrlich suggests that the effect of rehabilitation programs on crime is more complex than typically presented. Although effective rehabilitation programs may reduce offender supply of crime, rehabilitation also offers “an implicit subsidy” to potential offenders in the form of “training and employment benefits at public expense” (Ehrlich, 1981: 315). Ehrlich argues that the rehabilitation benefits experienced by offenders post program result in a “counter-deterrent effect on potential offenders,” which could potentially increase the crime rate.

Ehrlich (1996: 53) also suggests that policies that create “general incentives” to desist from crime must be jointly modeled with policies that create “specific incentives” to desist from crime. “General incentives” are policies—such as the levels of fines for offending—or broader market forces such as the availability of legitimate wages that

impact all citizens including offenders and non-offenders. By contrast, “specific incentives” such as incapacitation and rehabilitation largely impact convicted offenders alone. Thus, Ehrlich (1996: 53) suggests that it is not sufficient to evaluate the effectiveness of specific incentives (i.e. rehabilitation programs) at the individual offender level, since he finds that in a broader analysis of the entire crime market these programs have a limited impact on the aggregate flow of offenses. By contrast, “general incentives” which likely have a weaker impact at the individual-level, may be more effective at the market-level, since they influence the “entry and exit of marginal offenders” in the crime market (Ehrlich, 1996: 54).

The models of crime developed by Becker and Ehrlich have been criticized along several dimensions. Although extended by Ehrlich to include the derived demand for crime, the model is focused largely on offender decision-making. The model assumes that crime can be deterred, and thus is open to the criticism (originating with Becker) that the model is most applicable to crimes involving monetary gains to offenders, and is less able to explain non-economic crimes. Other critics note that the Becker and Ehrlich models focus primarily on “crime’s pecuniary returns and financial costs associated with state sanctions,” without directly addressing psychic returns which are more important in understanding non-instrumental offending (McCarthy, 2002: 422). It has also been suggested that “the economic benefits of crime are so meager that they cannot be realistically viewed as incentives,” and that the social costs associated with crimes not measured in the model, greatly exceed the costs of state sanctions and lost wages (McCarthy, 2002: 426-427; Nagin, 1998).



Additionally, the theoretical variables of the market model are difficult to measure, as Ehrlich notes (1996). A major challenge is isolating the causal effect of law enforcement activities on crime rates, while ensuring that the estimated relationship does not reverse the true effect (since the crime rate and law enforcement activities are endogenous). The solution is to include exogenous variables in the demand or production functions of law enforcement that are excluded from the supply model of crime (Ehrlich, 1996). Identifying data that captures such exogenous variables has proved to be extremely difficult. Ehrlich (1996: 60) suggests a promising approach is to, “quantify political or institutional factors that affect either law enforcement budgets or ‘rules of the game’.” Applying this strategy, Levitt (1996) uses the instrumental variable of the status of state prison overcrowding litigation to estimate the effect of prison populations on crime. Levitt (1996: 348) finds that, “incarcerating one additional prisoner reduces the number of crimes by approximately fifteen per year.” Determining whether the social benefits of incarceration are worth the costs is thus entirely dependent upon the price of crime of each incident where a criminal victimization was prevented, either due to specific deterrence (the incapacitation of expected recidivist) or general deterrence.

#### Applying the Law and Economics Literature to the Study of Crime

Clearly, it is necessary to develop accurate estimates of the costs to victims of crime to estimate the efficiency of competing crime control policies and programs. The optimal model for measuring crime victim’s harms would observe the ex ante amount of precaution taken by private citizens, the amount of public anti-crime resources, and the resulting risk of victimization at the equilibrium crime rate. The amount of resources

dedicated to crime prevention would then be differenced by the risk of victimization yielding the expected cost per type of expected victimization. These values could then be applied to estimates of the various crime reduction benefits of competing policies and programs to identify more effective approaches. Or, ideally, the estimates could be factored into a general equilibrium or simultaneous equations model such as proposed by Ehrlich, to identify optimal investment in law enforcement.

There are several challenges to this model. First and foremost, there is no true private market for the exchange of crime. While individuals may enter a private market to exchange crime prevention goods, such as security alarms, the criminal event itself is not really a market exchange, since the victim is an involuntary participant. Second, the market is a mix of private demand from crime prevention and public crime prevention which is a quasi-public good. Thus, as will be discussed later, the public investment is likely to be sub-optimal, since there is an incentive for participants to free ride on the investments of others. And third, as a result of the first two points, it is therefore very difficult to develop a true ex ante measure of the amount a potential victim is willing to pay to avoid victimization, or conversely, the amount the victim is ex ante willing to accept to be made whole after being victimized.

#### Properties of a Good Price Estimator

As a result of these challenges, it is difficult to develop a single approach that robustly measures price. Before discussing the approaches that are commonly used to estimate prices that are not directly observable, it is worthwhile to consider the properties of a

good price estimator. The key conceptual properties of a good estimator include a preference for:

- Ex ante to ex post;
- Observed behavior in real markets to behavior in shadow markets;
- Revealed preferences over stated preferences; and,
- Estimates observed at the societal level, rather than aggregated individual preferences.

Conceptually, the first three issues are discussed at length in the neoclassical economics literature. The fourth issue is discussed in the section that follows.

Two approaches are commonly used in neoclassical (welfare) economics to value a policy change (Hicks, 1939). Compensating variation (willingness to pay, or WTP) is the “the maximum amount of income that could be taken from someone who gains from a particular change while still leaving him no worse off than before the change.”

Equivalent variation (willingness to accept, or WTA) is the “the minimum amount that someone who gains from a particular change would be willing to accept to forego the change” (Pearce, 1992: (78), in Leung and Sproule, 2007). Thus, both of the approaches meet all three of the criteria for a good estimator described above: revealed preferences are observed ex ante in a functioning marketplace. Notably, the equivalent variation (WTA) approach estimates the amount necessary to make an individual prior to a policy change. The jury compensation strategy used in this analysis employs the same logic as equivalent variation, but the jury award is rendered post hoc by a third party rather than ex ante by the affected individual. Thus, the jury compensation award is a proxy for ex ante WTA.

In the economics literature, use of WTP is far more prevalent than WTA. For example, Cook and Graham (1977) note that:

The individual probability of loss in many cases is influenced by public activities such as law enforcement, highway design, and medical research. Investments in such areas produce a good (reductions in the probability of loss for each of a number of individuals) that, from an efficiency point of view, should be valued at an amount equal to the sum of the resulting benefits accruing to individuals. E. Mishan points out that the appropriate individual benefit measure in such cases is the "compensating variation" in wealth: the reduction in the individual's wealth, which, when coupled with a reduction in the probability of loss, leaves him at the same (expected) utility level.

Practically, most market transactions are assumed to reflect a WTP strategy. In a choice between two otherwise identical homes, different prices that reflect different expected safety from crime reflect differences in willingness to pay to avoid victimization rather than differences in willingness to accept being victimized.

While WTP is more prevalent in the literature, historically economists have assumed little consequence between the choice between equivalent and compensating variation. Brookshire, Randall and Stoll (1980) note that with a change in quantity, WTP represents the maximum price a buyer is willing to pay and WTA is the reservation price of the seller, or the minimum price the seller is willing to accept for a good. Under perfect competition, the price should be equivalent. In a recent paper challenging the conventional wisdom in using WTP in Slutsky equations, Leung and Sproule (2007:4) test whether WTA and WTP result in different valuations, and find that "[t]he dominance of one vantage point over the other is arbitrary and... potentially misleading." However, recent empirical studies have found that WTA tends to produce larger estimates in applied research (although the theoretical explanation for the difference is unclear). This difference in the size of estimates is a criticism of the jury compensation method which

tends to produce larger estimates than a WTP estimate would (though this may be due to the ex post nature of the jury award rather than the WTA approach) (Cook and Ludwig, 2000).

### Costs to Society

A key goal of any willingness to pay strategy is to generate an estimate that can be used to measure changes in net social welfare. Ultimately, the goal of policy analysis is to determine if the net social welfare change from one policy exceeds the net social welfare change from a competing approach. In a market with perfect competition, those estimates can be derived in a straightforward manner. Since each individual actor in the market maximizes their utility, the sum of the costs and benefits to each individual in response to a change in price is equivalent to the change in net social welfare. Under those market conditions, the assumption that the jury compensation is equivalent to the equivalent variation is relatively modest. Hanneman (1991) finds that CV and EV are equal when the good in question has a perfect market substitute (Cook and Graham (1977) however suggest safety is an irreplaceable commodity and thus there is no substitute). If it can be shown that juries are instructed to make an individual whole after they experience some loss, then a case can be made that the jury award is a relatively strong instrument for the equivalent variation.

A review of a sample of state guidance on instructions to juries about how damage's should be calculated suggests that the jury is told to make an award that is

sufficient to make a defendant whole after a loss. The following is an example of guidance to judge's in charging a jury drawn from Connecticut<sup>6</sup>:

The rule of damages is as follows. Insofar as money can do it, the plaintiff is to receive fair, just and reasonable compensation for all injuries and losses, past and future, which are proximately caused by the defendant's proven negligence. Under this rule, the purpose of an award of damages is not to punish or penalize the defendant for (his/her) negligence, but to compensate the plaintiff for (his/her) resulting injuries and losses. You must attempt to put the plaintiff in the same position, as far as money can do it, that (he/she) would have been in had the defendant not been negligent.

Thus, it can be argued that jury awards are a strong proxy for an estimate of equivalent variation, though somewhat weaker than an ex ante estimation. In a market with perfect competition, the assumption that the aggregated individual damages are equivalent to social cost is relatively weak.

In the case of a price change where there is substantial governmental involvement in the market, e.g. where the good in question is a quasi-public good, that assumption is stronger. In the case of crime prevention, the government plays a substantial role in setting the price. Police, prosecution and corrections activities that affect the supply of crime are public goods. These goods are non-rival (use by one does not preclude use by another) and non-exclusive (no individual can be excluded from their use).

In practice, the crime prevention market has both a quasi-public good element and a competitive market element. Society makes an investment in crime prevention in the form of police, courts and corrections. Individuals then evaluate whether to make additional investments in crime prevention, in the form of private police, and other

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<sup>6</sup> Civil Jury Instructions, State of Connecticut Judicial Branch. Retrieved March 14, 2009 from <http://www.jud.ct.gov/JI/civil/part3/3.4-1.htm>.

private crime precautions. The two markets are endogenously determined – an increase in public investment is likely to lead to a decrease in private precaution.

Conceptualizing crime prevention as a quasi-public good has important implications for the estimation of prices to victims. That is, in addition to the other properties of a good price estimator, it must also be the case that the price estimate (the sum of the product of prices and prevalence) must be equal to society's willingness to pay for those goods. Willingness-to-Pay approaches can employ either a micro- or a macro- approach to valuing costs to society (as is discussed later, hedonic pricing approaches tend to estimate social costs by aggregating micro-observations). Crime control studies applying neoclassical economic theory prefer a macro approach (Cook and Ludwig, 2000; Cohen, Rust, Steen and Tidd, 2004) where costs to society are valued at the societal level. Cook and Ludwig estimate the social costs of gun violence using a WTP approach, where survey respondents are asked (103):

Suppose that you were asked to vote for or against a new program in your state to reduce gun thefts and illegal gun dealers. This program would make it more difficult for criminals and delinquents to obtain guns. It would reduce gun injuries by about 30% but taxes would have to be increased to pay for it. If it would cost you an extra [\$50/\$100/\$200] in annual taxes would you vote for or against this new program?"

The approach is 'macro' in nature since the reduction in crime (and social costs) is observed at the societal, rather than individual level. Thus, the approach measures public safety, rather than aggregated individual harm.

By contrast, the cost-of-illness and jury award approaches estimate social cost from aggregated individual-level data, and thus the approach can be described as a micro-level study. Calculation of crime prices from jury awards applies a relatively straightforward algorithm, where average treatment effects are transformed into a

monetized value. Thus, if one year of prison deters 15 future offenses, then the sum of the price of each of those 15 offenses is the estimate of the total benefit from prison (Levitt, 1996). Jury awards are used to estimate the price of each of those prevented crimes, and the total cost is the sum of these individual-level prices.

But, since prison is a quasi-public good, summing micro-level benefits may yield a total social benefit that is not equal to society's valuation of those benefits. In practice, it is likely that 'society' puts a relatively higher value on that reduction in harm. Since the expected victim of a crime cannot be known in advance, and since the average citizen does not experience any felony victimizations in a given year, it is reasonable to presume that the typical citizen will place a higher value on the benefit of that reduction in victimization than would be developed from an ex post estimate of victim losses. The main reasoning behind this assumption is that in addition to a willingness to pay to prevent their own losses, the average citizen would also seek to limit the losses of others including friends and neighbors. Thus, a willingness to pay survey is likely to elicit a higher valuation of the benefits of reducing crime than would be derived from a WTA estimate using the jury award method.

Thus, two problems emerge. The first is the problem related to the endogeneity. Since the macro-approach tends to lead to smaller investments in crime prevention through the mechanisms described above, those individuals who are more risk averse in crime may choose to increase their investment in crime prevention. However, as noted above, it may well be that the individuals at greatest risk of crime may have the fewest means to privately protect, and thus the change in private crime prevention may well be smaller than a utility maximizing model may predict.



A more serious problem is that of free riders and a resulting under-investment in the quasi-public good. A well known phenomenon in welfare economics, the free rider problem emerges because individuals choose to sub-optimally invest in public goods in expectation that others will make up the difference. Thus, the macro-WTP faces this threat—if survey respondents observe that they can free ride, they will respond to the survey with a lower willingness to pay than if the good was competitively exchanged. This problem is addressed in contingent valuation that use a referendum method, but remains in studies that do not. Finally, the criticism remains that individuals may state preferences that are larger than their revealed preferences.

The jury compensation method thus has some attributes that make it relatively attractive in evaluating prices of the crime market. Since the micro-WTP tends to overestimate harms, and the macro-WTP underestimates due to free riders, a third party ex ante estimate may fall somewhere in the middle. There is little empirical research to support or refute this claim. However, jury awards are almost always less than the specials (damages) claimed by civil plaintiffs which is at least suggestive that the awards would be less than an ex ante solicitation of compensating variation. A similar analysis cannot be undertaken comparing jury awards to macro-WTP estimates, but given that there is no free rider problem in jury awards, it seems reasonable to conclude that the jury awards will be somewhat greater than the macro-WTP estimates.

There is a substantial research literature that describes estimates of crime-related goods using all three of the methods described above – hedonic pricing, contingent valuation and jury compensation (cost of illness). The section that follows reviews those

studies, with particular attention to jury compensation approaches to estimating the price of crime.

### **Hedonic Pricing**

Economic theory posits that the price of any good or service that is exchanged in a competitive market is a function of the attributes of that product. A consumer in the market to purchase a television will likely choose a product based on a series of attributes related to price. Some are easily observed: what is the size of the monitor? Is it analog, digital, high definition or plasma? What is the depth of the screen? What is the wattage of the speakers? How long is the warranty? Others are harder to quantify: what color of casing does the consumer prefer? Is the supplier perceived as having a high quality brand name? The consumers' willingness to pay the market price will be based on both sets of preferences, and many others.

In the study of crime, it has been observed that perceived risk of crime is included in this calculation - the hedonic price function - for many goods and services (Clark and Cosgrove, 1990; Thaler and Rosen, 1975; Thaler, 1978). Two in particular have received significant research: differences in property values due to risk of crime and compensating differentials in wages. Both approaches are used to develop estimates of the social costs of crime.

The most common application of hedonic pricing is the study of differences in property values. The general strategy is to compare the property values of two comparable neighborhoods or houses, and control for all observable differences: the size of houses, number of bathrooms, type of structure, proximity to urban areas, etc. The remaining differences in property values can be at least partially attributed to

unobservable attributes, including crime rates, on the assumption that *ceteris paribus* higher crime rates are associated with lower property values. The alternative approach, compensating wage differentials, applies the same concept, but compares differences in wages where the same job is being performed in high and low crime areas.

The approach satisfies three of the four attributes of a good price estimator. Estimates are *ex ante*, expressed as revealed preferences, and the good is exchanged in a private marketplace. If successfully implemented, price estimates will approximate revealed preferences making them preferable to willingness-to-pay surveys (Clark and Cosgrove, 1990). Generally, the estimates for social cost are estimated as the sum of individual choices rather than as society-level valuations, but as previously discussed; the exchange of the good in a competitive market reduces the importance of this assumption. Another benefit to the hedonic market approach include is that all of the attributes of criminal victimization as the method “incorporates psychic costs associated with actual and potential victimization” (Clark and Cosgrove, 1990: 106). While Becker (1968) and Ehrlich (1973) described a framework for understanding hedonic crime and crime control markets, few hedonic market studies have been undertaken. A small number of studies have observed changes in housing prices or wages to infer the demand for public safety (Thaler, 1978; Thaler and Rosen, 1976, Clark and Cosgrove, 1990).

Using the hedonic pricing approach, Thaler (1978) seeks to estimate consumers’ willingness to pay for lower crime rates by estimating the effect of crimes on property values, and then inferring homeowners’ preferences from these estimates. Thaler (1978) used property crime data from Rochester, New York in 1971 on a random sample of 398 single-family homes sold in the City of Rochester during 1971 (from Maser, S., Riker,

W. & R. Rosett, 1997) in combination with a census geo-coding program to calculate the per capita crime by census tract. Thaler finds that “an increase of one standard deviation in C (property crimes per capita – probability of an individual being victimized by a property crime) decreases the price per acre by \$3,847 or roughly \$430 per house,” which represents, “about 3% of the average price per home in the sample” (Thaler, 1978: 142). Thaler estimates the cost of an average property crime to be about \$500 (about \$1,600 in 2008 dollars), with the caveat that the estimates are based on a small sample from a single jurisdiction.

Clark and Cosgrove (1990) estimate household demand for public safety using a two-stage intercity hedonic model that extends the Roback (1982) and Hoehn, Berger, and Blomquist (1987) models that estimate an inverse demand function for public safety. Data in the study are developed from the Public Use Microdata Sample (PUMS) of the 1990 Census of Population and Housing and include wages, structural features of homes, and individual characteristics. Clark and Cosgrove (1990) estimate the level of public safety as, “the inverse of the predicted murder rate at each location,” and argue that individuals can choose, “any level of public safety by choosing the appropriate location within a given city or by moving between cities within the same region.” They specify a hedonic-rent equation to estimate the implicit price of public safety and estimate a crime-rent elasticity of -0.13.

In the second stage of the model, an ex-ante willingness-to-pay function is derived from the first-stage hedonic function in order to estimate revealed preferences for public safety. Clark and Cosgrove find that demand for public safety is relatively price inelastic, and that the demand for public safety with respect to income approaches one.

Other significant findings include that, 1) married couples' willingness to pay is significantly higher than that of single individuals, 2) the effect of education is positive and significant, 3) residents in central cities are less willing to pay than their suburban neighbors, and an interesting finding that 4) "willingness to pay is minimized at a city size of 1.7 million persons" (120).

There have been few attempts to use the hedonic pricing method to estimate the prices of specific crime types. The primary application of hedonic pricing is to estimate the value society places on saving a 'statistical life'. Rather than housing data, this literature relies on labor market data on wage rate differentials for heterogeneously risky jobs (Viscusi, 1993). Individual attitudes toward risk (willingness to accept risk and the marginal willingness to pay for greater safety) are inferred from observed wage-risk tradeoffs made by a worker's choice of employment. Reasonable estimates of the 'statistical value' of a human life range from \$3 million to \$7 million (Viscusi, 1993). Some have chosen to interpret the meaning of a statistical life as the monetary amount that society is willing to pay in order to change the "fatal risks faced by a group of people," rather than "saving the life of an identified person" (Kenkel, 1998: 797, see Shelling, 1968). However, since the approach uses aggregated individual data, it appears that existing estimates are better interpreted as the latter meaning.

The primary limitation of the use of the hedonic market pricing approach in the evaluation of crime control interventions is that these studies have a high noise-to-signal ratio. The Thaler study, for example, estimates that crime accounts for about 3% of the value of a home. If there is unobserved heterogeneity in housing prices (such as different tastes for aesthetics) that explains more than a small fraction of the value of a home, as it

almost certainly does, then that statistical noise will make recovery of the small crime signal difficult. And, assigning values to crime rather than some other attribute (such as clear air or clean water) is often difficult to empirically support.

As a result of data limitations, hedonic market property value studies have been unable to isolate the costs of any individual crime type (Cohen, 2000). Additional limitations to property value studies are their reliance on assumptions about housing market competitiveness and consumers' knowledge of neighborhood crime rates (Cohen, 2000). Although a few studies have investigated the impact of crime on both property values and wages (see Clark and Cosgrove, 1990; Hoehn, Berger, & Blomquist, 1987), it does not appear that any studies have gone so far as to incorporate the two models in order to explain the interaction between housing prices and wages.

### Contingent Valuation

Contingent valuation methods are generally applied to policy questions in situations where there are no competitive market data. Contingent valuation estimates are generally conducted as surveys of consumers of a good to elicit estimates of the amount they would be willing to pay for public good being studied. The CV methodology was developed in the environmental economics literature, and has been used to measure non-market goods such as clean air and saving endangered species (Cohen et al., 2004). The CV methodology is also known as "hypothetical valuation" since survey respondents are asked how much they would be willing to pay for some good in a hypothetical situation (Boardman, 2001: 358). Although CV has been frequently used in other fields, particularly environmental

economics, this methodology has not been commonly used in criminal justice research (Cohen et al., 2004).

The CV approach has been extensively studied. In 1993, the NOAA Panel on Contingent Valuation released a report that included recommendations for future CV studies. The report (5) also succinctly describes the strengths and weaknesses of CV:

The CV technique is the subject of great controversy. Its detractors argue that respondents give answers that are inconsistent with the tenets of rational choice, that these respondents do not understand what it is they are being asked to value (and, thus, that stated values reflect more than that which they are being asked to value), that respondents fail to take CV questions seriously because the results of the surveys are not binding, and raise other objections as well. Proponents of the CV technique acknowledge that its early (and even some current) applications suffered from many of the problems critics have noted, but believe that more recent and comprehensive studies have already or soon will be able to deal with these objections.

The CV methodology, as applied to crime control research, directly surveys the public to estimate ex ante willingness to pay for crime reduction (Cohen, 2000: 286.) Ludwig and Cook (1999) conducted a nationally representative survey of 1,204 adults in 1998 to estimate the demand for crime reduction. Because they interpret the CV survey responses as representing household willingness to pay, Cook & Ludwig (1999:12) include household-level covariates in their maximum-likelihood estimate model. They find that the mean WTP per household for a 30 percent reduction in handgun injuries equals \$239. At the aggregate level, “a 30 percent reduction in gun violence is worth \$23.8 billion to the American public in 1998 dollars, around \$750,000 per injury” (Ludwig & Cook, 1999: 15). Similar to the wage-risk tradeoff estimates of a statistical life by Viscusi (1993), the estimates of Ludwig & Cook (1999: 15) indicate a range between \$4.05 and \$6.25 million value per statistical life.

Zarkin, Cates and Bala (2000) conducted a study using the CV survey methodology to estimate the social benefits of drug treatment programs. Using a mall-intercept approach, 393 respondents in Triad, North Carolina and Brooklyn, NY were surveyed about their willingness to pay for expansions of existing drug abuse treatment programs. The survey also contained questions on attitudes toward drug abuse treatment and familiarity/exposure to the substance abuse problem. Respondents in each community were asked about their willingness to pay for expansion of a treatment program that targets all drug users as well as one that treats only women drug users. Two versions of the survey were developed in order to estimate the effect of size increase on WTP estimates, with one version estimating the treatment of 100 additional individuals and the other version estimating the treatment of 500 additional individuals. The two WTP questions were each preceded by a two-paragraph description of the drug abuse problem and the benefits of successful drug treatment. A “payment card” CV question format was used where the respondent was given a series of dollar amounts and asked to choose the amount that represents his maximum willingness to pay. Zarkin et al. found a mean WTP for a substance abuse treatment program that successfully treats 500 drug users of \$30.90. The mean WTP for a substance abuse treatment program that treated 500 women drug users was \$41. However the difference between this estimate and the estimate for the program targeting all drug users was not statistically significant (Zarkin, Cates & Bala (2000). While the Zarkin study retains many of the advantages of WTP, the use of a number of program recipients rather than a percentage, means the study is micro-WTP and subject to the criticisms about the use of the results to generate social costs.



Cohen et al. (2004) used the CV methodology to estimate the public's willingness to pay for crime control programs. Telephone interviews were conducted with a nationally representative sample of 1,300 individuals. Respondents were asked, "if they would be willing to vote for a proposal requiring each household in their community to pay a certain amount to be used to prevent one in ten crimes in their community." Respondents were asked about three of five crimes, including 1) burglary, 2) serious assault, 3) armed robbery, 4) rape or sexual assault, and 5) murder, and the amounts of money they were asked if they would be willing to pay were randomized in \$25 dollar increments between \$25 and \$225. Results of the study indicated that, "the typical household would be willing to pay between \$100 and \$150 per year for crime prevention programs that reduced specific crimes by 10 percent in their communities, with the amount increasing with crime seriousness" (105).

Aggregate estimates from the survey, "imply a marginal willingness-to pay to prevent crime of about \$25,000 per burglary, \$70,000 per serious assault, \$232,000 per armed robbery, \$237,000 per rape and sexual assault, and \$9.7 million per murder" (ibid). Interestingly, while jury awards are hypothesized to be greater than estimates from WTP, the Cohen et al, estimates are 1.5 to 10 times higher than prior estimates of victim costs from jury awards by Miller, Cohen and Wiersema (1996). Cohen et al. note that their estimates are consistent with those of Cook and Ludwig (2000) who found an average household willingness-to-pay to reduce handgun violence of about \$200 per year, with an aggregate estimate of about \$1 million per gunshot injury.

Although Cohen (2000) suggests that properly conducted CV surveys have the potential to be "useful policy tools," he also cautions that "any method that asks the

public their willingness to pay for reduced crime inherently must confront the fact that the public might be misinformed about the risk and severity of crime.” Finally, Zerbe (1998:439) suggests that this technique may be subject to aggregation bias, “which arises because the value reported as the WTP by respondents varies inversely with the number of items presented for valuation.”

### Jury Awards

As noted in Chapter 1, the costs of crime to victims literature is dominated by the work of Cohen and Miller, although there are a handful of other papers by other authors (notably Rajkumar and French, 1997) The logic behind the jury award approach is straightforward. Some victims of criminal acts will seek to have the costs of their harm internalized by the offender (or other liable party), and will use the tort system as the means of recovery. When plaintiffs win a case, compensatory awards are made in two domains: 1) an award for the recovery of direct costs to victims and 2) an award for the recovery of intangible (pain and suffering) costs. While data are available about both direct and indirect harms to crime victims, the Miller and Cohen literature generally uses the indirect harms as a way to weight direct costs of crime, which they tend to draw from other secondary, macro-level data.

The first study of the costs of crime to victims was conducted by Cohen (1988). In the study, Cohen did not directly acquire jury award data, but rather used the parameter on the ratio of direct to indirect costs to use as a weight in the analysis of other secondary data. The data used in this study was drawn from data provided by Jury Verdict Research (JVR, which is the source of jury data for the other Miller, Cohen studies). JVR collected

data on more than one hundred thousand civil cases involving a personal injury, where most of the plaintiffs were not injured in the commission of a crime. Since the JVR data did not directly link awards to crime types, Cohen used the relationship between direct and indirect costs as a means of weighting data from multiple other sources to account for pain and suffering. Figure 2 describes the approach used by Cohen.

Cohen assumes that the functional relationship between direct and indirect costs for all crime victims follows the functional relationship from the JVR parameter. Cohen uses the JVR parameter to estimate the intangible award for any case with an injury, and combines these data with the injury rates associated with different crime types to estimate direct costs. Cohen then estimates direct costs for other types of crime from the National Crime Survey (NCS), and calculates a risk of death statistic from FBI data as an added weight to the direct and indirect costs. These data are then combined to yield a total cost estimate for a variety of crimes.

INSERT FIGURE 2 ABOUT HERE

Figure 2. Conceptual Map of Cohen (1988)

There are several important limitations of the Cohen (1988) approach. First, as with later studies, limitations in the data prevent Cohen from directly estimating the direct or indirect costs of crime for all crimes. Second, he assumes—but does not test the assumption—that awards for non-criminal torts are equivalent to criminal torts. Third, Cohen cannot directly observe indirect costs of crime, including fear of crime and risk of death from the jury award or personal injury data. Instead, he must rely on several additional and unrelated data to make these estimates. As shown graphically in Figure 2, this meant linking data from many sources, at different levels of aggregation, often with small sizes. Thus, a substantial number of assumptions were required to link these data. Fourth, since Cohen uses a single parameter to estimate the relationship between direct and indirect harms from crime, he implicitly assumes that individuals all have the same relationship of direct to indirect harm. Thus, while this paper pioneered a new approach to estimating the value of harms from crime, the paper itself was empirically limited, and much of the MC work of the next decade in this area was devoted to addressing those limitations.

#### *Prior Research Modeling Jury Awards*

Miller and Cohen (writing with Wiersema) took a different approach to estimating the price of crime for a series of papers culminating with a National Institute of Justice report that has been widely cited (1996). The 1996 paper is written for a general audience, and thus contains only a limited description of the method used to obtain the results. A later (unpublished) study describes the results of a study to estimate harms to victims of sexual assault and assault, and the paper notes that “[t]his methodology was

used for the purpose of estimating the cost of various types of crimes and injuries in Miller Cohen and Wiersema (1996)” (1999:15).

The method described in that paper is as follows. First, MC note that they are missing past losses for about 40 percent of their sample in assault cases. Since they would like to include these data in their model predicting jury award, they impute the values for missing data by regressing past losses on injury attributes (they note that when transformed from logs to dollars, the imputed values were much lower than observed values). They repeat the process for sexual assault cases, but are less certain about their imputed estimates, as past losses were missing in 65 percent of cases and thus there was limited support space for their imputations.

Having recovered past losses for their sample, MC then test whether their regression model explains variance in jury award (the model is described in Chapter 1 as equation (1)). They report that, “the regression equation does an extremely good job of predicting jury awards around the mean level of the independent variables” (14). As evidence to support this assertion, they note that the when the mean jury award (the dependent variable) from their model is transformed from a logged value back to a monetized value it is very close to the unadjusted mean of the entire sample.

This analysis does not appear to support their conclusion that the regression models are a good fit for their data. Basically, MC regress total jury award for each individual on the part of the jury award for direct compensation, and a set of covariates describing particulars of the case. The regressions are run separately for each crime type, so type of crime is not included in the model. Thus, MC do not predict jury award by crime type, but rather predict how components of a crime affect the award (although it is

not clear why they would include the award for direct costs on both sides of the equation). The usual interpretation of this type of model would be that  $\alpha$  is the expected award for any award of this type,  $\beta_1$  is the marginal award for past and future tangible losses, and some other coefficient can be interpreted as the marginal award for intangible costs, *ceteris paribus*. They do not, however, include a term for intangible losses. The total expected award is estimated as the sum of these marginal awards and the intercept.

Miller and Cohen state that the goal of their model is to describe “the functional relationship between jury awards and independent explanatory variables” (14). They assume that each of the parameters in the regression is a component of the jury award, and that the average jury award can be estimated by summing all of the parameters. As they note, “[b]y multiplying each coefficient in Table 6... by its corresponding mean value, one can estimate the predicted level of compensation” (14). In fact, they do not appear to do this. Instead they note that the total award (once the data are transformed from logarithms back to dollars) is “\$47,908, of which \$3,229 is past and future losses” (14). However, \$3,229 is not the product of the coefficient and the mean values of LOSSES, but simply the transformed value of the mean of LOSSES. It should be noted that the \$47,908 “predicted” cost of sexual assault/rape is not an estimated value either – it is also more or less just the uncorrected average of all observed jury awards (more on this below). In fact, none of the cost of crime estimates presented in MC (1999) are regression parameters – all of them are simply raw average values that have been transformed into logs and then transformed back into dollars.

However, even if they had taken the product of the coefficient and the mean value for each variable, it is not clear what to make of their interpretation. In essence, MC has

estimated that the average cost of each crime as the sum of the product of the coefficients and means of all RHS variables. If the model is correctly specified for an OLS regression then the mean of the error term will be 0 and thus, by construction, the average dependent variables will be equal to the sum of the estimated RHS variables. MC notes that this is in fact the case for both the models they specify. As a result, their regression results do not contribute new information about the dependent variable – the estimated costs to victims of crime are simply the average values of the jury awards.

The goal of this analysis for MC is to establish a functional relationship between jury awards and attributes. Once this is established, estimates can then be generated for various subgroups, where subgroups are defined by age, gender and injury. MC asserts that these subgroups do not appear in their data in the same proportion as the subgroups appear in the population of crime victims. Thus, once an estimate has been generated of the average harms to each subgroup, the prevalence of that population can be re-weighted to be nationally representative. Once re-weighted, the representative proportions can be summed across subgroups to yield separate estimates for assault and sexual assault (see Figure 3 for a graphical representation of the approach).



INSERT FIGURE 3 HERE

Figure 3. Conceptual Map of Miller and Cohen (1999)

Unfortunately, this strategy appears to create more problems than it solves. The intuition for the approach is to demonstrate that the regressors explain jury award. But once that is established, using the same model to generate estimates to allow for prevalence weighting is problematic. First, of the nine independent variables used in the physical assault model to create expected sub-group awards, five are non-significant. In the sexual assault model, *all* nine independent variables are not significant. The appropriate interpretation of those variables is therefore that those variables have a value of zero, rather than the MC approach which is to ignore the non-significance and multiply the X (set to 1) by the coefficient.

Second, there are problems with the inclusion of claimed losses on the right-hand side of the model. MC includes claimed losses presumably because it was an important predictor of variation in harms. But claimed losses co-vary with injuries and other attributes, which MC claim they do. Thus, re-weighting based on only injury and age and not claimed losses is questionable. In the case of the rape victim described above, MC use age and injury to re-weight the sample to be nationally representative. Since claimed losses are included as an independent variable, the effect is to re-weight for the true prevalence of victim's under 12 with internal injuries at the mean of claimed past and future losses. The intuition for including claimed losses in the model is that it is a mediator that clarifies the relationship between age, injury and damages. In this case, clarifying that relationship will obscure the true effect of re-weighting. Since only age and injury are observable in NCVS, there is no re-weighting that accounts for the co-variation between age/injury and claimed losses. If the variables are positively correlated (where more serious injuries result in larger claimed losses), then the effect of the re-

weighting will correct only for the undercounting of the age and injury attributes, and not for the undercounting in claimed losses. Thus, a more prudent strategy would have excluded claimed losses in the regression model.

Third, MC uses the mean values of JVR data to re-weight their estimates within strata. Thus, if women over 18 with major injuries are X percent of their sample, they multiply the expected damage by X. They then sum across each similarly weighted strata. However, this approach yields only an estimate of the mean value for each crime. No information can be retained about the distribution of harms using this strategy. This would be less of a problem if the data were normally distributed. However, since the mean is three to ten times the size of the median in the unadjusted data the choice of a strategy that only yields means skews toward higher results. Given that the main criticism in the literature is that the cases that progress to civil court are more serious than those that do not, it is critical that some effort be used to address the distributional properties of the victim harms.

Finally, while MC re-weights the data to account for the fact that their jury data may over- or under-represent some sub-groups, they do not account for the possibility that their jury data excludes some populations that are observable in the national population. Almost certainly, the small MC sample sizes (270 sexual assaults and 956 physical assaults) exclude some subgroups that exist in the population of crime victims. Thus, if there are subgroups that are not included in the JVR data, then MC are assuming that the mean value of the awards that are excluded is identical to the mean value of the awards that are included. Given the criticism of these data that jury cases comprise a non-representative sample of all cases, this assumption is likely incorrect. Thus, a better

strategy would be to not use a regression model to re-weight the data to create nationally representative estimates.

### Are Juries Objective Arbiters of Victim Losses?

Even if adjustments to civil jury awards can make those estimates nationally representative the question remains whether jurors are competent, objective arbiters of victim losses, who fairly weigh the evidence in each case—and only the evidence in the case—before making an award. If, for instance, jurors are not capable of understanding issues in complex, or even typical, cases, than their verdicts are not an appropriate source of victim costs. In addition, if jury's award compensation to victims that is unrelated to their physical or emotional injuries, then again, those damages are not a fair source of data to estimate the price of crime. In response to broader policy concerns that juries do a poor job in determining damages—and in particular that jury awards are escalating unfairly—a large body of research about jury behavior has emerged in the last two decades.

The general consensus of this literature is that juries do almost as well as judges in evaluating complex claims and that jury awards are based on the attributes of the case they evaluate. However there are some potential sources of error in jury awards that must be addressed to use jury verdicts to estimate crime prices. If juries consider factors in rendering damages that are unrelated to the actual harms of a victim, than those awards will not be representative of the true harms to crime victims. As the review below of extant literature on jury behavior suggests, juries may alter an award in the presence of certain case attributes that are unrelated to victim harm. In particular, the jury behavior

literature identifies cases where a business is a defendant, where the jury does not have the opportunity to award punitive damages, when the victim is female, and when the defendant is clearly culpable and/or is unsympathetic as cases where awards may not reflect victim losses alone. In addition, prior research has shown that awards vary by region and have tended to increase over time. Thus, the analysis predicting jury award from victim injury should also account for these extra-legal awards. In addition, in many cases the victim shares some liability with the defendant, and this shared culpability must be accounted for as well.

### Prior Research

As discussed in the review of extant literature on the costs of harms to victim in Chapter 2, the most common method for valuing harms to crime victims is to use civil jury awards to approximate true harms (Cohen, 1988; Miller, Cohen and Wiersema, 1996; Cohen and Miller, 2003). As discussed in Chapter 2, jury valuations of real harms to victims have several attributes that makes this an appealing approach to valuing harms to crime victims. In particular, juries act as independent arbiters of the true harms suffered by a victim. In addition, as ex post observers of harm, juries have more complete information about crime losses than ex ante estimates of victim's willingness to pay to avoid harm. Finally, while ex ante estimates are preferred when estimating market prices, ex post estimates may be more reliable when the market is ill-defined, such as is the case for the demand for crime.

Civil jury awards compensate victims in three ways. First, the jury may award damages for economic losses, including property loss, medical costs and lost wages. Second, juries may award damages for noneconomic losses to compensate victim for

indirect or intangible costs, including fear, lost or diminished quality of life, and pain and suffering and lost quality of life<sup>7</sup>. Some prior research has suggested that non-economic costs are the largest component of the cost of most crimes (Miller, Cohen and Wiersema, 1996: 15; Viscusi, 1988). In addition, juries may also award punitive damages, which generally serve as specific or general deterrents to similar future action. In order to estimate harms to victims, only economic and noneconomic losses matter, as punitive awards do expressly relate to the actual harms suffered in the tortuous event.

In order for civil jury award data to provide reliable estimates of the true harms to victims, it must first be demonstrated that juries are capable of consistent and unbiased estimates of harms in all types of civil cases. That is, if jury awards vary by the type of claim, the attributes of the victim, or the attributes of defendant, then the use of these data to estimate crime harms is questionable. And, since intentional torts (criminal acts) are a relatively small proportion of all civil claims, it must be the case that juries make awards based only on the harms suffered and not the attributes of the event, whether it is an intentional tort, malpractice or negligence. If the type of claim matters, then small sample sizes will greatly limit any analysis of harms from crime.

The consistency of civil jury awards has been the subject of significant prior research. In particular, researchers have tested hypotheses that juries are not equipped to objectively evaluate civil claims, and that an increasing number of claims (and more specious claims) have contributed to an inefficient system becoming increasingly ineffective (Danzon and Lillard, 1983). The most pointed critiques allege that civil

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<sup>7</sup> In the Miller and Cohen literature, economic damages are referred to as 'specials' and noneconomic damages are referred to as 'general damages'. In the RAND data, those terms are used to describe claimed damages, not actual award. To avoid confusion, this paper follows the conventions in the RAND data.

juries are incompetent and inherently unable to effectively determine liability. Critics assert that not only are juries unable to evaluate claims in complex cases, but that they are not competent to evaluate claims in relatively simple cases as well (Sugarman, 1985; Vidmar, 1989).

The reliance of a jury of extra-legal factors in making an award is generally referred to as hypothesized 'jury incompetence' which may manifest in two ways. First, it may be that jury incompetence results in random awards. That is, because juries are unable to differentiate a true claim from a false claim (and great harm from small harm), jury awards may not be causally related to the facts of the case. Thus, jury awards could not be used to draw inferences about the association between harm and awards. Second, jury incompetence is hypothesized to result in systematic use of inappropriate criteria to make awards (Daniels, 1989: 280-1). These inappropriate criteria are extra-legal factors that may cause juries to make awards that are not compensation for actual harms, but rather awards made based on the attributes of the plaintiff and defendant that are not related to the actual claim. These may include the perceived wealth of the defendant (which is hypothesized to lead to awards in excess of actual losses), awards for female plaintiffs that do not fully compensate them for their losses, higher awards (relative to actual losses) in certain time periods or in certain regions of the country, and awards in excess of actual losses in cases where the defendant is particularly reprehensible. As Vidmar notes, if researchers who use these awards "ignore or underestimate the conceptual and methodological problems in these data sets" they will "build on a questionable foundation" (1993:265).

The section that follows reports on studies that tests these hypotheses about jury behavior. These perceived deficiencies include the general competency of juries to evaluate civil claims, an evaluation of whether jury verdicts are random, and an assessment of the sources of extra-legal jury awards. If either of the first two critiques (competency and randomness) is affirmed, then jury awards cannot be used to estimate harms from crime. With respect to jury awards based on extra-legal criteria, if there is some evidence that juries use extra-legal criteria, then the data can still be used to estimate harms, if exogenous variables can be identified to control for those criteria.

#### The Competency of the Civil Jury

If jury's are competent, objective arbiters of the facts of a case, then their awards will be causally related to the facts of the case (MC implicitly test for this in the 1999 study discussed in Chapter 2). On the other hand, if jury's are not competent then civil juries are capricious and the civil jury award system is a random "bizarre lottery" of sorts (Daniels, 1989: 280). If true, then the absence of a causal relationship between case facts and case disposition would invalidate jury award data as a tool for estimating the cost of crime to victims. Despite the persistence of this assertion from members of the business community, insurance companies and national trade associations, (Daniels, 1989) and the media (Bailis and MacCoun, 1996), the overwhelming consensus in the scholarly literature is that civil juries are competent and allegations of award randomness are unfounded.

Evaluating jury competency is not a straightforward enterprise. The idea of competency is that jury's make correct decisions, and that the 'correct' decision is observable. Thus, researchers assume that there is an objective level of competency and



that there exists an instrument by which that competency can be measured, weighed and gauged. Multiple issues are involved in assessing jury competence and multiple standards of competency can be applied when studying these issues (Vidmar, 1989: 2). For example, competency could be measured by whether the jury gets the question of causality correct, or by the jury's ability to assess negligence and scientific evidence and /or correctly assess damages. Competency could be compared to an absolute standard or to the conclusions of experts, such decisions by judges or some other tribunal, or an expert jury created to test the claim of competency. Absent a generally accepted legal framework for approaching these issues (Viscusi, 1988: 204), researchers have compared jury verdicts to the verdicts judges would have rendered (Broeder, 1959; Kalven, 1964; Kalven and Zeisel, 1966), and compared jury verdicts to verdicts rendered by a panel of experts (Vidmar, 1992).

The University of Chicago Jury Project (Broeder, 1959) analyzed jury verdicts from 1,500 criminal cases and 1,500 personal injury cases in the U.S. After hearing a case, but before a verdict was rendered, judges were asked the verdict they would have rendered had there been no jury trial. For criminal cases, judge-jury agreement was 81 percent and judges were more likely to convict. For personal injury cases, judge-jury agreement was 83 percent. However, in terms of awarding damages, the study found that juries awarded two to thirty percent higher awards, without controlling for other factors.

A subsequent summary of the Project's findings (Kalven, 1964) updated Broeder's (1959) preliminary results. Judge and jury agreement was 80 percent in criminal cases. For personal liability cases, agreement was 79 percent and jury awards average 20 percent higher than those of the judge. Judges were also asked to rate each

case as “difficult to understand” or “easy” to test the hypothesis that judge-jury agreement increases with the ease of a case (1066). Kalven (1964) found no association between case difficulty and case outcomes, and concluded judge-jury disagreement results from the application of different values in deciding the case, rather than competency.

More recent research has compared civil jury verdicts to verdicts rendered by ‘expert’ juries. Vidmar and Rice (1992) (summarized in Vidmar, 1992) present the same malpractice case to a cohort of twenty-one senior lawyers and judges and a cohort of 89 jurors and compare the resulting damage awards. They found no significant differences in awards size, although the median expert award (\$57,000) was greater than the median civilian jury award (\$47,850) by about 20 percent (Vidmar, 1992: 123). Although the jurors deliberated as individuals rather than as a panel, the study indicates jurors performed well compared to allegedly more qualified individuals.

#### Jury Competency in Highly Complex Cases

While the evidence appears to support the notion that juries are competent to assess damages in the average case, civil and criminal cases are becoming increasingly complex in the presence of advanced technologies, new sources of evidence emerge, and lawyers presenting increasingly sophisticated cases (Vidmar, 1989:4). These developments have motivated concerns about the limits of the civil jury’s ability to evaluate statistical evidence (Thompson, 1989), and to evaluate the claims in highly technical cases, such as toxic torts (Drazan, 1989; Foster, Bernstein and Huber, 1993). Complex cases, which are generally thought to comprise a small minority of total civil cases, have created sufficient concerns in the court system that procedural innovations

have been implemented to assist jury's in evaluating evidence (Horowitz and Bordens, 1990).

There is limited evidence about civil jury performance in complex civil cases. Most research on the topic of jury behavior in complex cases has studied complex criminal cases and mass torts, particularly toxic torts. Thompson (1989) examined the ability of the criminal jury to comprehend statistical evidence, particularly base rates and error rates, presented in criminal trials. In particular, Thompson tested whether juries over-estimated the value of statistical and forensic evidence. He concludes that jurors generally do not overvalue statistical evidence. However, when jurors fail to "take into account the unreliability and partial redundancy of forensic evidence" overvaluation of statistical evidence does take place (41). Drazen (1989) argues the complex scientific and medical evidence of toxic tort litigation is beyond the grasp of jurors and should instead be decided by special juries of experts. Toxic tort cases are particularly challenging because "the tortuous behavior and resulting injury are never contemporaneous" (294) as in the case of cancers that manifest years after exposure to a carcinogen. However, while juries may struggle with cases involving complicated scientific evidence, judges may fare no better (Drazen, 1989: 296). Other scholars (Foster, Bernstein and Huber, 1993) argue for improved standards of scientific evidence presented in court and for improved guidelines for judges to enhance civil jury outcomes.

#### Are Jury Award's for Damages Commensurate with the Facts of a Case?

As noted, some researchers have hypothesized that civil jury awards may not be associated with the real economic and non-economic loss in cases they adjudicate. However, a large body of research (e.g. Bovbjerg, Sloan and Blumstein, 1989; Peterson,

1984; Viscusi, 1988; Wissler et al., 1997; Sloan and Hsieh, 1990; Cohen and Miller, 2003) documents the association between awards and economic and noneconomic loss. A landmark study by the RAND Corporation (Peterson, 1984) analyzed civil jury verdicts in Cook County (Chicago), Illinois using multiple regression analysis.<sup>8</sup> Petersen found that variation in awards was predicted by the plaintiff's medical expenses and lost income, severity of injuries, and disability. Petersen found that the number and type of injuries alone explained nearly 40 percent of the variation in award size. An empirical study of 859 product liability cases (Rodgers, 1993) supports these results, concluding "pain and suffering awards are systematically related to economic losses and to the type and severity of injury" (252).

Viscusi (1988) analyzed pain and suffering awards in a sample of over 10,000 product liability cases and concluded that "pain and suffering compensation varies by injury class, with the nature of the effects being broadly consistent with general perceptions regarding injury severity" (217). Wissler et al. (1997) used mock jurors in two controlled experiments and find "half to three-fourths of the variance in log awards for pain and suffering was accounted for by perceptions of the amount and duration of mental suffering, disability, pain and disfigurement associated with the injuries" (202-3).

Similar to the concerns about juror's ability to accurately assess guilt and liability in complex cases, some researchers have hypothesized that jury's are not able to accurately assess the victim's true harms in complex cases (Ostrom, Rottman and Goerd, 1996). Again, most extant research finds that civil jury awards are explained primarily by the plaintiff's economic and non-economic loss, despite some variation. Rodgers

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<sup>8</sup> The data set used by Petersen is an earlier iteration of the data used in this analysis.

(1993) finds that most heterogeneous component of awards—pain and suffering—are “nonetheless a function of variables we can specify” (Rodgers, 1993: 261). Some of the unexplained variation may be due to differing values plaintiffs place on their own lives as evidenced by riskier behavior and partial responsibility in the plaintiffs own victimization (Cohen and Miller, 2003, 174). Ultimately, “since society has chosen the civil court system as a means of redressing victims, jury awards are a logical way to approximate society’s assessment of the pain and suffering incurred by victims” (Cohen, 1988:541).

#### Extra-Legal Factors in Jury Awards

The preponderance of prior research suggests that civil juries are competent to understand the facts of civil cases and civil jury awards are causally related to the facts of a case.

However, there is also a substantial literature that finds use of extra-legal factors by civil juries to evaluate civil claims. Thus, awards are biased when compared to true damages.

Five key extra-legal factors are indicated by prior research: 1) case type, 2) geographical and temporal factors, 3) defendant wealth effects, 4) plaintiff gender, and 5) defendant culpability and availability of punitive awards.

#### Extra-Legal Considerations by Case Type

In theory, the value of a specific injury should not vary by the type of civil case. That is, if a plaintiff loses the use of their hand, the value of that loss should be the same regardless of how that loss occurred. The damage award should be identical regardless of whether the case was an automobile accident, product liability, malpractice or a tort.

Unfortunately, while there is some evidence that juror awards vary based on the type of case, it is very difficult to causally show bias in jury behavior. Because juries make decisions in secret, and because no independent, objective counterfactual can be

observed, extant research in this area is marked by serious identification problems (MacCoun, 1993). While MacCoun (1993) notes that mock juror experiments, where independent variables are systematically varied across research subjects, control for the effect of extra-legal factors. By contrast, the research on the association between case type and damages mainly uses archival data, with limited covariates. So, while several studies (Goodman, Greene and Loftus, 1989; Daniels and Martin, 1986; Peterson and Priest, 1982) find variation in award amounts across different case types, the studies fail to account for alternative explanations, namely injury severity. Nevertheless, extant research does offer some guidance on the importance of case type, as described below.

Goodman, Green and Loftus (1989: 295) find that the highest mean and median awards are for liability cases, followed by automobile negligence cases and medical malpractice cases, respectively. Daniels and Martin (1986) find product liability cases and medical malpractice cases have higher awards than vehicular accident cases, street hazard cases and premises liability cases. A study of Cook County jury verdicts (Peterson and Priest, 1982), pre-dating Peterson (1984), found substantial variation in award amounts across different types of lawsuits but did not control for injury type or economic damages. Not surprisingly, the types of lawsuits that brought the largest awards involved more seriously injured plaintiffs (Peterson, 1984: 32).

Peterson's (1984) multiple regression analysis of Cook County civil jury awards empirically controls for injury severity and case type to test whether case type or injury severity most explains higher awards. Most differences in awards resulted from injury severity rather than case type—two-thirds of the differences in awards for work injury and automobile accident claims are explained by injury severity. Still, plaintiffs in work

injury, malpractice and product liability cases receive awards that are two to four times greater than for similarly injured plaintiffs in other types of cases—street hazard, injury on property, dramshop, intentional tort, common carrier and automobile accident cases (35-7). Thus, there appears to be a premium for work-related injury, malpractice and product liability plaintiffs compared to injuries in other types of cases.

While the results are relatively consistent across these extant studies, it is difficult to judge how robust the findings are. Few studies examining award variation by case type have empirically controlled for confounding variables, such as injury severity and plaintiff income. The extant research suggests awards with similar fact patterns vary by case type, but the impact of extra-legal factors is not known because confounding explanations are not modeled. Peterson (1984) is the prominent exception, but even there the study controls are limited to case type, injury severity and large lost income. Despite these limitations, case type appears to be an important predictor of damages because plaintiff success rates vary by case type (Daniels and Martin, 1986), and compositions of case types vary across geographical jurisdictions (Peterson, 1987: 39-41).

#### Geographical and Temporal Factors

Many studies hypothesize that similar jury awards may vary over time and space. Research suggests geographical heterogeneity in awards, and, temporal heterogeneity associated with changes in wealth and legal standards. Unfortunately, most archival studies of civil jury awards are confined to only one or two jurisdictions (Hans and Lofquist, 1992; Peterson, 1984; Chin and Peterson, 1985; Hammit, Carroll and Relles, 1985). Jurisdictional differences in “legal cultures” raise problems of generalizability and hinder cross-county comparisons (Vidmar, 1993: 229). Moreover, state legislative

reforms to the civil tort system introduced in the 1970s varied in number, stringency, and timing (Danzon, 1984: 140). Thus, an archival study of civil jury awards that accounts for temporal and spatial attributes of cases would be more informative than one that did not.

Danzon and Lillard (1983) conduct an empirical analysis of medical malpractice claims, controlling for the possibility that California is particularly pro-plaintiff relative to other states. They find no evidence that awards in California, where one-third of the claims in their sample originate, are different from the frequency-weighted mean for other states (361). Peterson's (1987) analysis of civil jury verdicts in Cook County (Illinois) and San Francisco (California) indicates the two jurisdictions shared nearly identical trends in the per capita number of trials throughout the 1960s and 1970s. However, the trends diverged in the 1980s due to changes in settlement practices (i.e. an increasing tendency to settle) or in changes in legal procedures that reduced the number of jury trials in San Francisco and increased the number of trials in Cook County (8-9). These changes caused the average award to fall in Cook County and to climb in San Francisco (Saks, 1992: 1249). Daniels and Martin's (1986) survey of jury verdicts and awards in 43 counties in ten states finds considerable variation in plaintiff success rates and the size of awards, perhaps attributable to differences in state law (Daniels and Martin, 1986: 329) or local legal culture (Daniels and Martin, 1986: 332-3; Saks, 1992: 1252). Tabarrok and Helland (1999) find that in states with an elected judge, awards are larger for out of state defendants and larger in general than in states that do not elect judges.



The finding that awards vary across states and over time has two important implications. First, this variation may reduce the generalizability of a jurisdiction-specific study to other jurisdictions or to other time periods. Second, these findings suggest caution should be taken interpreting results from multi-state data analyses that do not control for jurisdiction-level variation (a common problem). Legal and procedural variations might mean, *ceteris paribus*, a leg is worth twice as much in Florida as it is in Michigan. Notably, Cohen and Miller (2003) include legal and institutional control variables (to control, for example, damage award caps in some states) in their most recent estimates of the implied value of life from jury awards (175).

#### Defendant Wealth Effects and the “Deep Pockets” Hypothesis

MacCoun (1996) summarizes one of the most widely held beliefs about civil litigation, noting “there is a widespread perception that America's tort system is biased against so-called deep-pocket defendants—defendants with extensive financial resources like corporations, governments, and wealthy individuals” (123). This view suggests that when juries perceive that a defendant has the ability to pay a larger damage award, “juries are more likely to find liability, and award more money” (*ibid.*). Such a bias would be a departure from legal standards since the law dictates that compensatory damage awards “be independent of the defendant’s wealth, moral culpability, and other attributes” (Hammit, Carroll and Relles, 1985: 754). Much research has addressed the deep-pockets hypothesis.

A report by the RAND Corporation (Peterson, 1984), discussed above, finds evidence that plaintiffs in work injury, malpractice and product liability cases receive higher awards than plaintiffs with similar injuries and economic loss in other types of

cases. The authors posit this may be because “juries may tend to sympathize more with certain plaintiffs more than defendants” (37). A follow-up report (Chin and Peterson, 1985), the first serious empirical inquiry into defendant wealth effects, analyzes the same dataset of over 9,000 civil jury trials in Cook County (Chicago), Illinois between 1959 and 1979. Building on two previous studies examining civil jury trials in Cook County, the authors find verdicts are “generally less favorable” for deep-pocket defendants, even after accounting for injuries and case types (58). While the differences between individual and corporate defendants and the differences between individual and government defendants were “not as large as one might have expected”, the analyses reveal much greater differences for trials involving severely injured plaintiffs (60). Specifically, “corporate defendants paid awards to severely injured plaintiffs that were over four times as much as awards against individuals and three times as much as awards against government agencies” (60). Unfortunately, the analyses exclude information about liability issues (i.e. perceived defendant culpability) that could contribute to award divergences.

Hammit, Carroll and Relles (1985) analyze the same dataset of civil jury verdicts used by Chin and Peterson (1985), and add a dataset of civil jury verdicts from San Francisco. The results of the multivariate regression analysis lead the authors to conclude, “the type of defendant appears to influence jury awards. Defendants who can be presumed to be wealthy, heavily insured, or able to distribute the costs of awards among their customers or shareholders are forced to pay larger awards” (754). Corporate defendants pay 34 percent larger awards than individuals, after controlling for plaintiffs’ injuries and type of legal case. Government defendants also pay substantially more than

individuals, especially when plaintiffs' injuries are severe. Schmit, Pritchett and Fields (1988), in their regression analysis of a sample of cases in which punitive damages were assessed against corporations, find punitive damages reflect the total assets of the defendant, but total assets are a poor predictor of the amount awarded (464).

As noted above, the datasets commonly used in these archival studies had substantial limitations, not the least of which was that civil cases were not randomly sampled for inclusion in the dataset—rather, the database tended to be populated with novel cases. Alternatively, quantitative studies analyzing different datasets (Daniels and Martin, 1986; Sloan and Hsieh, 1990) and qualitative studies employing mock juror or *post hoc* interview methodologies (Hans and Ermann, 1989; MacCoun, 1996; Vidmar, 1993; Hans and Lofquist, 1992) find evidence contradicting the deep-pockets hypothesis.

Daniels and Martin (1986), collected data from local jury verdict reporters, survey state court jury verdicts and awards in 43 counties in ten states. The study only reports summary statistics that compare award amounts by case type. Nevertheless, Daniels and Martin dispute the deep-pockets hypothesis, concluding, “awards for some kinds of cases, such as product liability and medical malpractice, may be high in comparison to those involving other causes of action, but these types of cases comprise a small proportion of all verdicts and plaintiffs are less likely to win in these lawsuits” (347-8). Sloan and Hsieh (1990) analyze a dataset comprised of closed medical malpractice claims in Florida and samples of jury cases from jury verdict reporters in California, Illinois, Florida, Missouri and Kansas (using RAND data on the malpractice cases for California and Chicago Peterson, 1984; Chin and Peterson, 1985). Sloan and Hsieh's multiple regression analyses also contradict the deep-pockets hypothesis and the findings of Chin

and Peterson (1985). Even though hospitals, the most common deep-pocket defendant, have greater financial assets, the plaintiff received a lower award when a hospital was the defendant (1027).

The other common method for evaluating the ‘deep pockets’ hypothesis is to use a mock jury, where civilians and/or experts are asked to evaluate claims in a simulated case. Hans and Ermann (1989) apply a juror simulation methodology, soliciting responses from 202 university students for two hypothetical cases. In the first case, the defendant was an individual and, in the second, a corporation. When the corporation was a defendant, the relationship between financial resources and judgments of wrongdoing was negative, contradicting the deep-pockets hypothesis (160-1). When the defendant was an individual, perceived financial resources explained only four percent of award variation (160). The authors suggest that respondents varied award and liability assessments based primarily on perceived “recklessness within the specific context of individual or corporate misbehavior” (161).

Also using the mock jury method, MacCoun (1996) experimentally controls for confounding variables, namely defendant wealth. He finds jurors’ verdicts were “insensitive to reliable differences in perceived defendant wealth”, although “corporations were indeed treated differently” (140). That is, MacCoun finds evidence that it is the ‘corporate identity’ of the victim that matters, not their wealth. Vidmar (1993) uses a sample of 147 citizens waiting to be called for jury selection in the Raleigh, North Carolina state court. The experiment, “yielded no support for the deep pockets hypothesis or the psychological dynamics that are posited to be behind it” (225). Alternatively, Hans and Lofquist (1992) use interview methods to investigate *post hoc*

the decision-making process of tort jurors in business cases. They find that, contradictory to the deep-pockets hypothesis, most jurors held the position that a corporation's assets "should not be and were not relevant" to award decisions (106). Moreover, "at the level of the case, jurors rarely demonstrated the scrutiny or expressed the negativity toward corporations that they showed toward individual plaintiffs" (110).

While early studies analyzing civil jury verdicts find some evidence of the deep-pockets hypothesis, "we cannot say, one way or the other, whether it is wrong on the basis of available data" (Vidmar, 1993: 240). However, most research indicates juries treat corporations differently, though not necessarily because they have deeper pockets. Hans notes that "juries may also be respond to other differences between corporations and individuals besides their financial resources" (1989: 195). MacCoun (1996: 140-1) posits juries may treat corporations less well because of: 1) a general distrust of commercial actors, 2) the impersonal nature of corporations and 3) the expectation of higher standards for corporations. Nonetheless, recent work analyzing civil jury verdicts (Cohen and Miller, 2003) controls for deep-pocket effects. Thus, the literature suggests that identification on corporate identity is more effective at reducing bias than controlling for defendant wealth.

### Plaintiff Gender

Several studies have hypothesized a link between plaintiff gender and civil jury awards. In his analysis of compensatory awards using Jury Verdict Research data, Rodgers (1993) controls for injury severity and finds that gender is not a significant predictor of award. In their analysis of Jury Verdict Research data for Cleveland, Ohio, Nagel and Weitzman (1972) find plaintiff gender does not significantly affect the determination of liability.

However, mostly male jury's award more to male plaintiffs than to female plaintiffs, and mostly female juries favor women, though to a lesser extent than male dominated juries favor men. The authors posit greater severity of injury to males in the sample and the differential earning power of men and women may explain these differences (109).

Goodman, Greene and Loftus (1989: 290) correctly note that Nagel and Weitzman (1972) fail to sufficiently account for strength of evidence or type of injury. Applying a mock juror methodology to wrongful death cases, Goodman, Green and Loftus (1989) experimentally control for liability and test whether the gender of the decedent affects award level. They find statistically significant increases in awards for male plaintiffs in product liability cases and automobile negligence cases, but not in medical malpractice cases (295-6). Mock jurors were also dramatically less likely to consider exponential factors that would escalate awards, such as expected salary increases, when the decedent was female (300). A later study of wrongful death cases using a similar mock jury methodology (Goodman, Loftus, Miller and Greene, 1991) produced similar results. Fewer jurors said they considered the financial loss to the surviving spouse (57 percent vs. 75 percent) when the surviving spouse was male than when she was female; twice as many jurors considered exponential factors when the decedent was male (281). The authors posit that much of the gender effect may result from the emphasis on lost wages in determining awards. Since the value of a female homemaker's productive activities are not assigned a market value, female plaintiffs are most likely to be disadvantaged (282).

### Defendant Culpability/Reprehensibility

Awards are intended to compensate the plaintiff for his or her loss independent of the defendant's moral culpability (Greene, 1989: 232). However, limited research suggests civil juries may take into account defendant culpability or reprehensibility when awarding compensatory damages (Kalven, 1958; Broeder, 1959; Hans and Ermann, 1989; Horowitz and Bordens, 1990; Wissler, Rector and Saks, 2001). Such extralegal considerations would increase awards for defendants perceived as particularly reprehensible, all else being equal.

Kalven's (1958) descriptive study of the civil jury posits that, other factors held constant, juries will vary awards on the basis of varying degrees of defendant liability. Broeder (1959), discussed above, experimentally tests this notion using a mock jury model with two treatments—doubtful defendant liability and very clear defendant liability. The average award of all verdicts with clear liability was \$41,000, compared to \$34,000 where liability was ambiguous (754). MacCoun (1996) finds similar results—mock jurors assess higher compensatory awards when defendant liability is clear. Hans and Ermann (1989), also discussed above, provide an alternative explanation for the 'deep pockets' hypothesis. The authors hypothesize that, if juries hold corporations to higher standards of responsibility, corporations may be perceived more negatively than individuals committing the same actions (154). Applying a mock juror methodology, the authors find mock jurors deem corporations more reckless, morally wrong and deserving of punishment as compared to an individual, despite identical actions (158). They posit perceptions of greater culpability motivate mock jurors to assess higher total awards (161).

Wissler, Rector and Saks (2001) address a similar question of liability spillover into damages decisions. They find jurors assess higher awards when defendant liability was clear. Special instructions not to inflate awards to punish careless defendants reduced the effects of the defendants' conduct on awards (136). Another study using a mock jury methodology by Anderson and MacCoun (1999) investigates the ability of jurors to effectively compartmentalize liability verdicts from compensatory damage judgments. Responses from 91 participants indicated jurors inflate compensatory awards when they do not have the option to award punitive damages. Specifically, jurors inflate the most flexible and variable component of awards—the pain and suffering component.

Horowitz and Borden's (1990) tangentially related study of variations on trial structures (unitary, bifurcated or trifurcated) and order of decisions (liability or causation first) using 768 mock jurors finds similar results. The average compensatory award was lower when liability was presented first than if general causation was presented first (278). The authors conjecture that in trials where juries first assessed causation, jurors may have developed a pro-plaintiff bias, leading them to assess higher awards against the defendant (283).

On the other hand, Cather, Greene and Durham (1996) studied 80 jurors who had just completed jury service and find no evidence to support the hypothesis that the defendant's reprehensibility affected the damage award. Employing a mock jury method to determine the effect of defendant reprehensibility on compensatory awards, the authors assigned participants to low reprehensibility and high reprehensibility conditions (reflected in the trial summaries). They find the defendant's conduct did not affect the awards assessed by jurors.



### Race and Socio-Economic Status

In the criminal law literature, there is substantial evidence that the defendant's race, and socio-economic indicators affect jury decision-making (see Sommers and Ellsworth, 2000 for a thorough review). In particular, it has been shown that cases with white victims and black defendants are more likely to result in death sentences in capital cases (Paternoster and Brame, 2008). The civil jury literature is more ambivalent about the affect of race and socio-economic status on outcomes. Hastie, Schkade and Payne (1998) find that jurors race and education only weakly associate with case outcomes. There appears to be little literature on the effect of the defendant's or plaintiff's race and ethnicity on civil jury awards, although Bornstein and Rajki (1994: 129) note that "although little research has been done on SES and civil verdicts, there appears to be little systematic relationship."

It is not surprising that there has been little research on this topic, given that many defendants in civil cases, particularly in civil cases that result from a criminal act, are not the same individual who committed the tortuous act. Thus, much of the effect of race on outcomes, if there is any, is mediated by the presence of a business as defendant. The effect of socio-economic status is much more difficult to assess. On one hand, there is little scholarly evidence to suggest that there is a relationship between the SES of the jury and awards, or the defendant or plaintiff race and awards. On the other hand, this may be due in part because defendants with relatively low SES status are unlikely to appear as defendants in civil cases. The potential implication of this issue is addressed in the limitations section in Chapter 4.

### Counting Property Losses

The extant literature is ambiguous on the issue of whether property losses should be counted in the estimation of the price of crime. The foundational work in the field draws largely from the University of Chicago Program on Law and Economics who advocate not counting property losses in price of crime calculations (Becker, 1968; Polinsky and Shavell, 1979; Shavell, 1987). As Rajkumar and French note, “In the case of stolen property, unless it is damaged or destroyed, it is typically not counted as a social loss because it is transferred to another member of society, namely, the criminal (Becker, 1968; in Rajkumar and French, 1997: 294). The choice of whether to include property losses has been framed by Cohen (2000) as a choice between adopting a social cost approach or an external cost approach. Social costs, described in detail in Chapter 2, are the sum of society’s welfare. Thus, since crime is merely an (involuntary) transfer of ownership, property that is transferred to an offender but not damaged or destroyed should not be counted as a component of the price of crime. Cohen describes external costs, by contrast, as costs involuntarily imposed by one member of society on another (271-273).

While the Law and Economics tradition advocates not counting property losses, the tradition in the price of victimization literature is to include these costs. The issue is one of standing, that is, determining whose preferences should be counted, particularly in cost-benefit analysis (Whittington and MacRae, 1986). Trumbull (1990: 202) asks, “How should the reduction in theft be treated - as a social benefit or a mere transfer?” Becker (1968:170) describes crime as an economically important activity and advocates counting criminal gains from crime as well as lost criminal opportunity from law enforcement.

Stigler (1970) disagrees, arguing the society has explicitly labeled criminal activity as illicit and thus assigns crime no positive social value. Others argue that the decision about whether to include criminal gains as part of the cost-benefit equation should be left to the analyst to determine whether society gains from a criminal transaction (Whittington and MacRae, 1986; Shavell, 1985). Shavell (1985: 1234; in Trumbull, 1990) states that, “[a]llowing for a divergence between social and private benefits gives the analyst greater freedom to describe society's values.”

Trumbull (1990: 211) disagrees, arguing that, “allowing the analyst the freedom to describe society's values is not consistent with the notion that the evaluation be based solely on economic criteria given the physical and social constraints that exist.” Trumbull distinguishes between tolerant institutions (like markets) and absolute institutions (like criminal laws). An absolute institution “fixes the level of an activity which the individual may not exceed, or fall below as the case may be. . .” [Roberts, 1973: 392]” in (Trumbull, 1990: 211). Trumbull (212) criticizes those who fail to understand this distinction, stating, “[m]issing from these models is the realization that society has a purpose when it labels certain acts criminal; the label communicates that these acts will not be tolerated or counted in the social weal.”

The approach in this paper is to include property losses as a component of the price of harms. As been discussed in Chapter 2, crime is an involuntary transaction. Due to its involuntary nature, criminal activity violates a fundamental attribute of a competitive market. Thus, it seems reasonable to consider the question of standing from a broader perspective (rather than a market-based perspective which would clearly require that these losses are excluded). Broader social institutions, including criminal law,

advocate that participation in criminal activity directly leads to a general loss of standing. It would therefore be incongruous to give criminals standing in estimating the magnitude of harms associated with their criminal activity when that activity otherwise leads to a complete loss in standing in every other aspect of their social status.

### Risk of Death

Prior research on the cost of crime to victims has taken two different approaches to dealing with cases that result in the death of the victim. In the earliest literature, the cost of a homicide was valued according to extant estimates of the value of a life. In subsequent studies, homicides costs were not directly estimated. Rather, the risk of death was associated with the underlying offense that led to a homicide (assault, robbery, rape, etc.). Generally, this was necessitated by the use of aggregated data as researchers could not directly observe whether a particular robbery was part of a homicide.

In this study, homicide is observable in the data, although it is a rare event. Thus, it is fair to ask whether a risk of death calculation should be included in the analysis. Traditionally, researchers count the probability that a crime will become a homicide as an additional component of the price of harms to victims. Hypothetically, if a burglary has a 1 in 1,000 chance of becoming a homicide (that is, a homicide occurs in every 1,000 burglaries) and the value of a statistical life is \$7,000,000 then the product of that probability (0.1 percent) and that value means that there is an additional \$7,000 in costs associated with a burglary, in addition to direct and indirect losses.

The approach was originally used to overcome data limitations, where researchers were unable to directly observe homicides in jury data (Phillips and Votey, 1981; Cohen, 1988; 2005). More recent work has also incorporated a risk of death component into

price estimates (Miller, Cohen and Wiersma, 1995) Cohen and Miller, 1999; Rajkumar and French, 1997). It is fair, however, to question whether this tradition has some intrinsic value in cases where data are available (as well as in cases where data are too limited to directly observe homicide).

There are two main objections to using risk of death measures. First, including risk of death appears to be double-counting in many instances. In any study that relies on ex post measures of changes in criminal offending, the number of homicides (and other criminal events) is known. Thus, if a policy or program reduces burglary and homicide, counting a reduction in risk of death as a social benefit for the burglary cases is double-counting. Even in cases where there is an ex ante estimate of crime reduction including risk of death is double-counting. Consider the finding from Levitt (1996) that each year of incarceration prevents 15 felonies. Levitt calculates the benefit from those prevented crimes by assuming a distribution of offending and assigning a value to each offense. Again, the number of each type of crime is known (albeit via assumption). Counting the value of reductions in homicide and the reductions of risk of death in burglaries that did not become homicides is double-counting.

Second, it is fair to ask why death is special. Every criminal event carries with it a risk that it will escalate into a more serious criminal event, or, expand to include other less serious criminal events. It is obvious that felony person and property crimes carry a risk that they will become a homicide, but it should also be obvious that other less serious crimes do as well. A weapons offense, for example, clearly has a risk of escalating into a homicide. Similarly, driving under the influence could become a homicide. But it also may be the case that drunk drivers are more likely to illegally take a weapon in a vehicle

than sober drivers, and thus each drunken driving incident has a risk of becoming a weapons event. Similarly, each burglary carries with it the risk that the burglar will vandalize the property, and thus a risk of vandalism price component should be added to all burglaries. Obviously, this approach adds enormous computational costs to any cost-benefit analysis that includes the price of harms to victims.<sup>9</sup>

But the entire approach appears to be unnecessary. If it is known whether a burglary also had less serious crimes associated with it, and that it *did not* lead to more serious crimes, than including the risk of other events is double-counting. Even in cases where the prices are applied to the study of future events, the distribution of offending is assumed from some known distribution of prior offending, as is the case in the Levitt (1996) example. Further, this issue identifies a practical advantage of this approach over contingent valuation. Since each incident carries with it a probability of escalation or expansion, it must be assumed that respondents to contingent valuation surveys are aware of those probabilities and include them in their calculus. Thus, when Cohen et al (2004) query respondents about their willingness to pay for a reduction in burglaries, it must be assumed that they include in their calculation the risks of both homicide and vandalism.

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<sup>9</sup> A third very weak criticism can be leveled against the risk of death calculation. Risk of death is at its core an ex ante measure. The intuition is that when a burglary is about to transpire there is some risk that someone may be in the residence and the crime will escalate to homicide. Thus, a priori, it cannot be known whether the criminal event that is about to take place will be a burglary or a homicide. Therefore, it is appropriate to include that risk in the calculation. However, this ex ante approach is inconsistent when incorporated into jury studies (where measures are not ex ante) or cost-of-illness studies (where measures are explicitly ex post). While some may view this as strengthening those methods, it is nevertheless an inconsistency that affects the interpretability of the study, and perhaps the credibility: if ex ante measures are the best way to measure this component of price, especially in cost of illness studies, should they not be used to measure other components?

Since homicides can be directly observed in the NIBRS data, estimates for homicide are created as a separate crime category<sup>10</sup>. As a result, the cost estimates here are not directly comparable to extant estimates. For instance, while past estimates of robbery include the chance that a robbery may become a homicide, here, robberies that become homicides are valued as homicides. The interpretation of the estimate of on robbery in this study is the harm to the victim of a robbery that did not escalate to a more serious crime.

### Summary of Prior Research

The available evidence suggests that civil jury outcomes are not random lotteries. Although they are subject to substantial variation, awards “are nonetheless a function of variables we can specify” (Rodgers, 1993: 261). The overwhelming consensus in the literature contends awards vary in a manner consistent with expectations of economic and non-economic loss (Viscusi, 1988). However, there are several potential extra-legal factors that may lead a jury to make an award based on factors that are not directly related to actual plaintiff harms. Studies of award variation across case type, though econometrically limited, suggest work injury, malpractice and product liability awards are higher than awards in other types of cases. There is also some evidence that geographical variations in legal culture and legal procedure account for differences in awards that are not directly related to victim harm. Defendant wealth does not appear to be associated with higher awards, although the presence of a corporation as the defendant

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<sup>10</sup> MC report that in their data homicides occurring in the commission of a robbery were reported to the FBI as a robbery. In NIBRS data, the top three charges associated with a single event are reported. As discussed later, the NIBRS data were ordered with homicide as the most serious crime, and thus this was not an issue in this analysis.

may. Male defendants appear to receive higher awards than women. And finally, the evidence indicates the jury's perception of defendant culpability or reprehensibility may inflate compensatory awards, particularly when the jury does not have the option to assess punitive awards.



## Chapter Three: Damages Awarded by Jury in Civil Cases

The goal of this analysis is to estimate the average harms to crime victims by crime type. A model with full information would directly predict harms from crime, by type of crime,  $Y = \alpha + X_1\beta + X_2\beta + \varepsilon$  (3) where  $Y$  is the estimated total harms from crime,  $X_1$  is a vector of victim, offender and case attributes, and  $X_2$  is a vector of crime types. The coefficient on  $X_2$  is the mean marginal harm by crime type and total harm for each crime type is  $Y|X$ . Since harms vary by case attributes, the analysis requires incident-level crime data and incident-level data on the value of the losses to the victim.

As discussed in Chapter 2, in past studies of the cost of crimes to victims, incident-level data were not available that described both criminal incidents and associated harms to victims. Beginning in 1998, a national data source for incident-level crime data—the National Incident-Based Reporting System (NIBRS)—became available. NIBRS remains the only incident-level crime data currently available that includes data from multiple states (data on a representative sample of crime victims is available from NCVS and the relative advantages of NIBRS over NCVS is discussed later in this chapter). However, the NIBRS data does not record all data necessary to populate model (1). While NIBRS does report case attributes (including victim attributes and injuries) and property losses, NIBRS records data on only one component of losses from crime (property loss). The other components of harms from crime—direct economic losses from crime (such as medical bills and lost wages) and indirect economic losses (including fear and pain and suffering)—are not collected. Thus,  $Y$  cannot be modeled in (3) using only data from NIBRS.

To overcome this limitation, several prior research studies have modeled the costs of crime to victims using case-level civil jury award data to associate the harms from offending to specific types of victimization. The intuition is straightforward. The identifying assumption is that jury awards yield an unbiased estimate of harms from criminal victimization. Juries are assumed to be independent arbiters of liability. Thus, the total jury award is a reasonable proxy for the total harm resulting from a criminal event. While the harms are not valued *ex ante*, they approximate the true value of losses. A substantial literature has emerged (and is described below) that assesses this assumption. The general conclusion from this research is that juries render appropriate judgments based on case facts, albeit with some bias in the size of awards—particularly for non-economic losses—in the presence of certain case attributes.

Therefore, jury award data could be used to estimate harms to victims from crime, conditional on case attributes and crime type. It should be noted that there are two limitations to using these data to estimate harms to victims. First, it may be that the crimes that result in a civil award are not representative of all crimes. If civil cases are, on average, more serious than the average criminal case, then the estimates from these data will be upwardly biased. Second, it is difficult to clearly identify which civil cases result from a criminal event in the data. Two types of cases—physical assault and sexual assault—are relatively easy to pick out, and thus have been the focus of prior work in the field. However, only a small number of these cases result in a civil jury award which creates analytic problems (as described in Chapter 2). Therefore, adjustments have to be made to account for both the potential non-representativeness of jury data and the limited

number of clearly identifiable criminal cases in order to create estimates for a more comprehensive list of all criminal cases.

Case-level jury award data are publicly available from only one source, the RAND Institute of Civil Justice. RAND collects data on jury awards using a stratified random sample of verdicts from a non-representative sample of U.S. counties between 1985 and 1999. As in past studies, it is difficult to easily distinguish those claims which result from a criminal act from those claims that do not. In a database of more than 40,000 records there are less than 1,500 cases that could reasonably be coded as being the result of a crime-related incident. As with prior studies, the observable criminal cases are limited to intentional torts, rape and a few property crimes. However, it is important to note that there are certainly more than 1,500 crime-related civil cases in the RAND data. For example, auto liability cases are the modal case type in the RAND data, and many of these cases could have resulted from a criminal event, such as an inebriated or reckless driver, a stolen vehicle or an escape. However, these underlying criminal events cannot be discerned in the data.

In prior studies, the general solution to this problem has been to model expected award (including non-economic damages) for each combination of injuries, identify the prevalence of injury types within broad crime categories and estimate the expected total harm for these crime categories. Thus, this approach first predicts model (3) to estimate the expected damages to the victim conditional on socio-demographic attributes and injury severity for each combination of injuries. The expected harm per injury type is subsequently linked to data which associates injuries to crime type to observe the prevalence of injury types in specific crime categories, and the average of the jury awards

for each crime type, conditional on the distribution of injuries within each crime type, is calculated. In some prior research, estimates are generated only for non-economic awards with economic damages being developed from other secondary data.

While the results of these studies are widely cited, there is a disconcerting amount of variation in the estimates across the main studies in the field, as shown in Figure 1. One source of the variation is the number of strong assumptions that have been made in these studies to overcome limitations in the data. In particular, because prior research did not have access to micro-level data on both jury awards and micro-level crime data, the studies have reported a single point estimate for each broad crime category. Since nothing is known about the distribution of harms within crime categories, it is impossible to determine if all prior studies are reporting estimates within the same confidence interval, or are in fact generating widely divergent estimates.

With case-level crime data in NIBRS becoming available beginning in 1998, more direct estimation of the harms from crime is possible. The approach has two substantial advantages over earlier studies. First, it avoids reliance on secondary non-crime data to estimate the prevalence of injuries within crime data, since injuries are directly observable in NIBRS. Given that some of the samples used to relate injuries and crime type are quite small (less than 10 for rape in Cohen, 1988 for example), using NIBRS data with more than two million records per year to estimate the relationship should yield more robust estimates. Second, it avoids errors due aggregation bias. That is, since prior studies rely on aggregate data relating injuries to crimes, they calculate average harm per crime type. They do not, however, estimate the expected harm per victim. If, for example, robbery victims are likely to experience either very serious

physical injuries or no physical injury at all, the average harm within the crime type will not reflect the actual experience of any victim. Linking individual data on injury and crime will therefore identify these strata, but will also allow for the creation of confidence intervals to reflect the reliability of estimates.

These data can also, at least partially, overcome the two challenges defined at the beginning of this chapter: potential non-representativeness in civil data, and small sample sizes. The first challenge can be overcome by linking expected jury award to subgroups comprised of combinations of victim attributes and injuries (as has been done in previous research) but then only estimating harms for subgroups that are observable in both datasets. This obviates the need for the strong assumption made in past research that subgroups that exist in the general population but that *are not* observable in the jury data have the same distribution of harms as the subgroups that are observable in both. The second challenge can be made by making the assumption that juries award based on the attributes of a case, and not the type of case. Thus, all cases in RAND, regardless of their origin, can be used in the analysis. To weaken this assumption, propensity scores can be used to balance crime and non-crime civil cases across observable attributes and thus minimize differences between the two groups.

This research does require the relatively strong assumption that harms are similarly distributed within subgroups across the jury and crime data. If harms are distributed differently within subgroups in the two data sets, particularly if serious harms are more common in the subgroups in the jury data than in the criminal data, then harms will be overestimated. This issue is discussed again in the limitations section. Still, it is reasonable to believe that this is the less serious of the two challenges.

### Analysis Discussed in Chapter Three

Chapter 3 mainly describes the preparation of the data for interpolation onto the NIBRS data set. Results of that interpolation are described in Chapter 4. The general approach is straightforward. The rest of the chapter proceeds as follows. First, the RAND data is described with particular attention to the construction of the dependent variable and the identification of cases as crime or not crime cases. The section that follows describes analysis, including the propensity score analysis, to estimate mean harms per subgroup. Additional analyses are reported in Chapter 4.

### Data

Data from the RAND Institute of Civil Justice were collected in two datasets, an extract of data from 1985-1994 (series 1), and an extract of data from 1995-2000 (series 2). Series 1 contained a total of 24,180 observations and 680 variables. Series 2 contained 16,005 observations and 1,013 variables. There were substantial differences in the types of data that were contained in the two data sets and how information was coded describing each case. A single research database was created by creating consistent variables across the two databases. Decisions about which records to exclude are described below.

Before any other data preparation, independent variables were identified that were consistently reported across both the RAND and the NIBRS datasets. Since fewer variables were recorded in NIBRS overall, data from NIBRS was examined to select independent variables that would be available for the final analysis across the two datasets (and are used to construct the profiles). The variables included:

- Plaintiff (offender) attributes: (age, gender);

- Plaintiff injuries (none, minor injury, broken bones, internal injury, other major injury, loss of teeth, severe laceration, unconsciousness, death));
- Jurisdictional attributes (county population).

Variables describing these attributes were then constructed from the two RAND series.

Additional variables were constructed for the RAND data analysis to adjust for theoretical sources of bias.

### Definitions

In the RAND data, there are six different terms to describe damages. The definitions of terms are described below. It should be noted that these definitions following the definitions used in the RAND Codebook, and may be slightly different from the definitions that appear in other public literature on this topic.

Specials. In many jurisdictions, economic damages claimed by a plaintiff, especially in personal injury cases, are described as ‘special’, although the terms ‘damages,’ ‘general damages’ or ‘special damages’ may be used on occasion. Specials are defined here as direct economic losses, including medical costs, including disability, and other costs, including lost wages, lost time, property damage, expenses (for example, transportation) and any other damage not specifically classified as medical (RAND 2000: 84)<sup>11</sup>.

General Awards. General damages include all awards for unobserved economic losses, what are usually referred to as intangible losses in the cost-benefit literature. These include pain and suffering and emotional distress (85).

Punitive Awards. Any award that is made for the purpose of punishing or deterring future tortuous activity. This is sometimes referred to as an exemplary award (85).

### RAND Data Reduction

Cases were dropped from the analysis for several reasons. First, cases were dropped where the plaintiff lost. The logic for excluding these cases is similarly

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<sup>11</sup> This coding scheme follows the instructions given by RAND to those coding the data contained in the jury victim reports. RAND Corporation – Institute for Civil Justice (2000). Jury Verdict Update Project Coding Handbook.

straightforward—plaintiffs may lose a case because there was no harm from the tort, or, because the defendant in the case did not commit those harms. If the reason why the plaintiff lost was observable, it would be instructive to keep cases where a plaintiff claimed harm but the jury did not accept that claim. One criticism of the NIBRS data (and the National Crime Victimization Survey data) is that it may over-estimate the true number of crimes, since reporting crimes is relatively costless. If these data could be used to identify those events where a crime was reported, but in reality no crime was committed, this would allow for corrections for over-reporting. However, in the RAND data, it is not possible to observe why the plaintiff lost, so cases where the jury believed no tort occurred can be distinguished from cases where a tort did occur, but was not due to the actions of the defendant in the case. Thus, cases where the defendant was not found liable contribute no additional information and were deleted.

Next, cases where the plaintiff was not an individual (alive or decedent) were dropped, including cases with a plaintiff who was a business, a government, another institution or not reported/missing. The logic of excluding these cases is simply that harms suffered by institutions are not analogous to injuries suffered by individuals because these crimes are likely to include white collar crimes. In Series 1, 1,898 (7.9 percent) cases were dropped, including: business (1,744), government (37), another institution (34) or not reported/missing (85)<sup>12</sup> with a total of 22,290 cases remaining. In Series 2, 1,103 (6.89 percent) cases were dropped, including: business (788), government

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<sup>12</sup> Allison (2001) suggests that if fewer than 5 percent of data are missing for an item, than the data are ignorable and that threshold is used throughout this paper. In Series 1, missing totaled 0.35 percent of the sample.



(65), another institution (247) or not reported/missing (3) with a total of 14, 902 cases remaining.

In Series 1 covering the period 1985 through 1994, there were a total of 22,290 cases. Of these, there were 11,120 cases where an adjusted award was observed, where adjusted award was the total of economic, non-economic and punitive awards, and this total was adjusted for comparative negligence. In addition, there were 744 cases where liability was observed but the aggregated adjusted award was missing. Total award is coded in the RAND data in several ways, so other variables describing awards were investigated to determine total award. In 344 of these cases, the adjusted award was calculated by summing observed damages awarded to the defendant if available, or adjusted plaintiff awards if available (there were no cases where these two items conflicted). In 400 cases (3.69 percent of the remaining cases in Series 1), no award was observed on any indicator, and these cases were dropped. As a check, there were no cases where an award was observed but no liability was observed in Series 1. Finally, two cases were identified as class action lawsuits and were deleted, and six appeared to have gaps in defendant data and were also deleted. Thus, 11,456 cases remained in Series 1.

Series 2 data were coded in a slightly different manner and thus a different set of adjustments were used to obtain the final dataset. In Series 2 covering the period 1995 through 1999 there were a total of 14,902 cases. Of these cases, an adjusted award greater than 0 dollars was observed in 6,589 cases. This total includes 66 cases where an adjusted award was observed, but liability was not recoded. These cases were classified as a plaintiff win and included in the dataset. In addition there were 8,313 cases with a \$0 award. In 277 of these cases at least one defendant was observed to have liability with an

adjusted award value of \$0. However, in some cases defendants may have been coded with a \$0 value when there was no individual-level \$0 award on the plaintiff sides. To differentiate these cases, cases were coded as having a \$0 award when any of the defendants were coded as receiving \$0 (122 cases or 1.79 percent). In 155 cases (1.79 percent) there was an indication of defendant liability, but no indication of a finding of liability or an award on the plaintiff side. These cases were thus determined to have ambiguous outcomes and were dropped. Thus, a total of 6,711 cases remained.

Once the data were combined, cases were dropped if they were missing the independent variables that will be used to link the RAND and NIBRS data. Almost 3,000 records were missing age of the plaintiff and were dropped. Values for gender were missing in about 50 cases, and values were missing for injury (or were coded as ‘unknown’) and about 1,500 additional cases were dropped. As is described in the section below, some 170 cases were dropped because missing data on the distribution of economic and non-economic awards could not be calculated or imputed. Eleven other cases were dropped because the sum of economic, non-economic, punitive and shared negligence was not equal to total award, and these discrepancies could not be resolved.

Thus, the final dataset contains 12,918 observations and all contained a valid award for damages. These cases are a stratified sample of all civil jury awards in the sampled jurisdictions between 1985 and 1999. Treatment of the weights in this analysis is discussed below. (Some additional coding was required to fix missing values for economic and non-economic damages (described in Appendix A.1)).

### Adjustments to the Dependent Variable

Several adjustments to this variable were necessary before the data were analyzed.

The total jury award has four components:

- An award for economic loss, also referred to as direct losses, including lost property, lost wages, and health related costs;
- An award for non-economic losses, also referred to as indirect losses, which include fear, lost or diminished quality of life, and pain and suffering;
- Punitive awards, and
- Adjustments for shared negligence.

As is discussed below, valid data must be available for punitive awards in order to adjust those awards for each observation (the punitive award is removed before analyses are conducted). A ratio of economic to non-economic awards is required to compute a weight to adjust damages in cases with both property losses (but no other direct losses) and indirect losses. With respect to shared negligence, these reductions were ultimately not included in the final analysis. However, it is worth noting the potential impact these reductions may have on the final estimates. Finally, the stratified sampling procedures used by RAND researchers to identify cases require that weights are applied to all subsequent analysis.

Table 3.1 describes the number of cases in each state in each year, and the percentage of all cases that appear in a given state-year. In general, there is a trend toward more cases in the later years. Cases from Illinois and California make up a larger percentage of the sample in the later years as compared to the beginning of the series.

Table 3.1. Variation in the number of cases by state/region and year

Year	State/Region						Total
	California	Illinois	Washington	Texas	Missouri	New York	
1985	640 (1.81%)	86 (0.24%)	90 (0.25%)	202 (0.57%)	232 (0.66%)	815 (2.30%)	2,065 (5.1%)
1986	914 (2.58)	253 (0.71%)	91 (0.26)	234 (0.66)	195 (0.55)	559 (1.58)	2,246 (5.6)
1987	1,005 (2.84)	297 (0.84)	86 (0.24)	273 (0.77)	225 (0.64)	408 (1.15)	2,294 (5.7)
1988	831 (2.35)	232 (0.66)	110 (0.31)	239 (0.68)	237 (0.67)	442 (1.25)	2,091 (5.2)
1989	906 (2.56)	272 (0.77)	129 (0.36)	309 (0.87)	206 (0.58)	511 (1.44)	2,333 (5.8)
1990	1,074 (3.03)	279 (0.79)	124 (0.35)	285 (0.81)	187 (0.53)	639 (1.81)	2,558 (6.4)
1991	1,124 (3.18)	378 (1.07)	71 (0.20)	310 (0.88)	206 (0.58)	735 (2.08)	2,824 (7.0)
1992	1,191 (3.36)	263 (0.74)	80 (0.23)	311 (0.88)	241 (0.68)	554 (1.57)	2,640 (6.6)
1993	1,135 (3.21)	286 (0.81)	79 (0.22)	352 (0.99)	208 (0.59)	702 (1.98)	2,762 (6.9)
1994	962 (2.72)	245 (0.69)	63 (0.18)	286 (0.81)	141 (0.40)	648 (1.83)	2,345 (5.8)
1995	1,066 (3.01)	421 (1.19)	57 (0.16)	462 (1.31)	329 (0.93)	748 (2.11)	3,083 (7.7)
1996	1,051 (2.97)	379 (1.07)	56 (0.16)	470 (1.33)	272 (0.77)	811 (2.29)	3,039 (7.6)
1997	1,518 (4.29)	380 (1.07)	70 (0.20)	435 (1.23)	300 (0.85)	967 (2.73)	3,670 (9.1)
1998	1,302 (3.68)	371 (1.05)	57 (0.16)	324 (0.92)	179 (0.51)	1,036 (2.93)	3,269 (8.1)
1999	1,132 (3.20)	393 (1.11)	46 (0.13)	307 (0.87)	76 (0.21)	990 (2.80)	2,944 (7.3)
Total	15,850	4,535	1,209	4,799	3,234	10,564	40,191

Source: Data provided by the RAND Institute of Civil Justice. Two data sets were extracted, covering the periods 1985-1994 and 1995-1999. Percentages of all observations in each state-year combination are shown in parenthesis.

Very few cases can be affirmatively coded as having resulted from a criminal incident. By category, there are 162 homicides, 168 sexual assaults, 971 physical assaults, 260 property (burglary) cases, and 194 theft (most of the rest of the intentional torts, 1,740, involve civil rights issues). In the few cases where the type of crime can clearly be distinguished in the data, there is even greater variability in award size, for instance, the mean award is about \$1 million for a plaintiff who was assaulted, and the median is \$6,520.

Table 3.2. Variation in the probability of an award for the plaintiff by state/region and year

Year	State/Region						Total (YR) (n=40,191)
	California (n=15,850)	Illinois (n=4,535)	Washington (n=1,209)	Texas (n=4,799)	Missouri (n=3,234)	New York (n=10,564)	
1985	52.7%	58.1%	60.0%	48.5%	64.7%	51.4	53.7
1986	48.8	60.9	57.1	44.9	52.8	49.2	50.5
1987	52.0	54.6	50.0	46.2	59.6	51.4	52.2
1988	51.6	60.8	60.9	39.3	57.4	54.5	53.0
1989	48.8	53.3	52.7	42.1	61.1	53.2	50.7
1990	51.2	55.6	58.1	41.4	61.0	49.6	51.2
1991	47.7	50.8	56.3	40.3	61.7	48.6	48.8
1992	48.8	56.7	62.5	46.0	57.7	47.3	50.2
1993	50.0	57.3	44.3	40.3	55.8	48.6	49.4
1994	50.6	55.1	57.1	45.1	56.7	45.5	49.5
1995	50.4	50.1	54.4	40.7	51.4	45.4	47.9
1996	41.5	47.2	55.4	42.3	52.2	48.8	45.5
1997	40.7	42.1	52.9	43.7	54.3	46.0	44.0
1998	43.6	47.4	56.1	41.7	58.7	45.4	45.5
1999	42.8	52.9	50.0	45.9	52.6	44.4	45.4
Total (State)	47.6	52.5	55.5	43.0	57.2	48.1	48.7

Source: Data provided by the RAND Institute of Civil Justice. Two data sets were extracted, covering the periods 1985-1994 and 1995-1999.

The probability that a plaintiff won their case follows the opposite pattern of the number of cases. While Table 3.1 shows that the number of cases tends to increase over time, the probability of a plaintiff victory declines by the end of the series. While the overall probability of a plaintiff victory is 48.7%, the first year in the series (1985) had the highest probability of a plaintiff win (53.7%) and the final year in the series had the

second lowest (45.4%). The same pattern generally appears within each state's data as well.

The data appear to include all major categories of crime – intentional torts include assault and battery; conversion/theft include property crimes; dram shop include alcohol related crimes; and other economic crimes, such as fraud, are included as well. However, while the data sets appears to include a broad spectrum of criminal related activity, there are few variables that can be used to directly link a crime to a civil case. Data include awards for actual damages (including property loss, lost wages, and health care costs), general damages (pain and suffering) and punitive damages (for specific and general deterrence). The data also include substantial information about both the plaintiff and the defendant (age and gender), the facts surrounding the criminal event and harms (injuries) inflicted on the victim.

#### Distribution of the Dependent Variable

Table 3.3 describes the variation in jury award across the states for each of the 15 years included in the series. There is variation in awards across time and across states, however the general pattern of awards is relatively stable. Overall, awards increase over time (even after adjusting for inflation). This result is not surprising given the data in Table 3.1 that shows increasing numbers of cases and Table 3.2 showing reduced likelihood of a plaintiff win suggests that the increase in award in Table 3.3 incentivizes more plaintiffs, with less convincing cases to pursue a civil award.

Table 3.3. Variation in the mean jury award by state and year (unadjusted)

Year	State						Total
	California	Illinois	Washington	Texas	Missouri	New York	
1985	1,189,060 (20,827,980)	284,944 (1,029,157)	114,005 (607,268)	52,545,599 (740,864,568)	99,011 (555,053)	266,067 (894,931)	5,641,545 (232,003,373)
1986	259,688 (1,318,840)	432,836 (2,758,696)	49,425 (99,235)	177,118 (870,300)	35,957 (118,686)	498,820 (2,974,706)	302,163 (1,965,685)
1987	423,881 (2,816,097)	347,310 (2,200,070)	312,418 (2,107,399)	220,080 (1,191,519)	135,039 (552,293)	835,799 (4,904,843)	430,467 (2,962,405)
1988	591,671 (7,047,241)	635,114 (2,012,277)	83,332 (215,743)	575,284 (3,592,206)	75,527 (230,289)	668,510 (1,937,040)	525,618 (4,741,406)
1989	432,283 (1,740,771)	505,904 (1,693,006)	116,127 (295,684)	881,201 (5,175,058)	160,264 (48,5101)	906,401 (3,753,288)	562,671 (2,867,782)
1990	1,078,588 (8,822,193)	726,691 (4,911,512)	147,941 (808,560)	479,190 (3,050,155)	287,667 (1,907,185)	796,643 (3,776,765)	803,790 (6,307,862)
1991	619,975 (5,465,797)	913,125 (6,909,660)	77,878 (275,796)	458,616 (2,809,136)	818,689 (6,449,117)	662,914 (3,198,370)	653,543 (4,982,966)
1992	498,166 (3,240,493)	490,205 (1,733,136)	369,929 (1,623,820)	1,648,809 (15,669,879)	231,041 (1,573,171)	656,571 (3,185,678)	637,893 (6,037,930)
1993	973,905 (13,04,2202)	780,468 (3,942,395)	103,015 (333,058)	1,868,211 (17,568,457)	528,115 (5,486,344)	855,372 (4,891,884)	979,242 (10,917,295)
1994	1,073,912 (8,800,958)	788,593 (4,043,786)	225,221 (875,244)	861,120 (4,431,620)	201,546 (645,521)	716,705 (3,298,416)	844,090 (6,240,147)
1995	667,763 (3,478,944)	510,872 (1,713,240)	212,326 (574,975)	712,459 (3,965,125)	488,205 (5,030,231)	693,700 (3,247,989)	631,748 (3,492,789)
1996	893,818 (8,671,075)	963,280 (6,720,538)	135,233 (450,770)	1,377,547 (13,526,266)	297,106 (1,927,602)	987,690 (4,408,671)	934,958 (8,090,017)
1997	636,140 (3,893,296)	383,633 (1,872,903)	356,019 (1,426,745)	458,579 (2,722,369)	356,586 (2,482,507)	807,120 (3,229,372)	605,806 (3,288,999)
1998	877,769 (8,430,958)	606,417 (2,798,733)	188,106 (594,357)	1,195,341 (10,052,785)	241,839 (808,374)	1,052,249 (6,961,223)	886,898 (7,390,533)
1999	5,181,604 (146,554,937)	980,717 (4,514,093)	225,312 (849,900)	959,297 (6,456,571)	474,787 (2,393,368)	938,005 (4,681,190)	2,555,093 (90,971,803)
Total	1,042,208 (39,913,405)	650,874 (3,923,673)	171,425 (915,195)	3,058,724 (152,231,682)	295,440 (2,948,880)	770,398 (4,084,302)	1,081,106 (58,329,291)

Source: Data provided by the RAND Institute of Civil Justice. Two data sets were extracted, covering the periods 1985-1994 and 1995-1999. Standard deviation is in parenthesis.

The pattern of awards across states is relatively stable over time. If the outlier award is dropped from Texas, then the mean award in Texas is about \$913,000. Thus, there are two states with relatively high average awards around \$1M (Texas and California), two states with moderate awards around \$700,000 (Illinois and New York) and two states

with relatively small awards (Washington and Missouri). In 9 of the 13 years after 1985, New York or Texas has the highest average award. Washington or Missouri have the lowest award in every year.

The mean values reflect the skew generated by the presence of a few very large awards. The data in Table 3.3 is consistent with the extant literature which predicts that jury awards are significantly skewed with a large number of relatively small awards, and a few very large awards. Ostrom, Rottman and Goerdts (1996) provide descriptive measures from the Civil Trial Court Network Project, which maintained data on eight key indicators of the civil jury in 45 urban state courts in the 1990s. Again, few cases actually go to trial, from a low of 1.9% in automobile tort cases to a high of 8.2% in medical malpractice. Moreover, plaintiff win rates vary by type of case. These outcomes are important because they may define the bargaining range for out-of-court settlements (234). In Ostrom et al., the median jury award is \$52,000, but the arithmetic mean award is \$455,000. However, a 5% trimmed mean (on both sides) reveals an average award of only \$159,000 in 1992 dollars (237).

As was found in virtually every other study that measured jury awards, the data in this study are highly skewed. The mean award (\$1,328,677) is almost six times as large as the median award (\$229,535). The problem has substantial bearing on outcomes. Past studies (including the MC series) have reported the costs of crime to victims at the mean, thus assigning the value to the average victim, rather than the typical victim which would be reported at the median. In prior studies, researchers have tried to account for these differences in mean and median by transforming the dependent variable into a log. However, as discussed in Chapter 3, the most prudent approach to estimating the price of



crime to crime victims is to report key moments rather than to specify a regression model, which appears to cause more problems in this analysis than it resolves.

The alternative approach is to remove outliers that are contributing to the skew. On one hand, removing outliers is problematic, since these are actual awards. However, a review of the largest awards suggests that these are the cases that appear least to conform to the notion that jury's only use the facts of the case to render a verdict. In these cases, the implied value of a life is in the tens or hundreds of millions, far outside the typical valuation (Viscusi, 1986). Thus, the approach taken here is to drop those cases that are more than three standard deviations from the mean award, or about one percent of cases. Given the skew in the data, all of the outliers that are removed from the analysis are at the upper end of the award distribution.

Table 3.4. Distribution of Total Award (Unweighted)

Category	Total Award	Punitive	Total, Excluding Punitive	Total, Excluding Punitive and Outliers
All (N=12,918)	\$1,376,341	\$133,036	\$1,243,305	\$895,091 (N=12,756)
Crime Indicator				
Crime (N=895)	1,372,276	257,570	1,114,705	737,251 (N=886)
No-Crime (12,023)	1,376,643	123,765	1,252,878	906,872 (N=11,870)
Issue				
Crime (N=890)	1,372,278	257,570	1,114,705	737,251 (N=886)
Malpractice (N=2,219)	2,256,547	67,562	2,188,985	1,631,082 (N=2,194)
Liability (N=8,500)	1,150,014	69,992	1,080,022	771,039 (N=8,397)
Other (N=1,304)	1,356,582	569,920	786,662	555,114 (N=1,279)

Note: All records include a non-missing value for total award (n=12,918). All values are adjusted for inflation and presented in 2003 dollars. The 'Total Award' column includes all awards (before any reduction for shared negligence). The 'Total, Excluding Punitive' column is the total award without punitive award (before any reduction in shared negligence). Total, excluding punitive and outliers, also removes outliers. Sample sizes are in parentheses.

Before discussing the effect of removing outliers in Table 3.1, the other key adjustment in this table is the removal of punitive awards. As noted in Chapter 2, the usual approach in this literature is to drop punitive awards from the calculation of costs of crime to victims. The logic is straightforward. Punitive awards are designed as a deterrent to future negligence, and are not related to the harms experienced by the plaintiff in the case at hand. Thus, the punitive award contributes no information about the victim's losses and is differenced out of the award.

In total, 162 cases were dropped from the analysis in cases where the total award (excluding punitive damages) was more than three standard deviations above the global mean. Once outliers were excluded, a total of 9 'crime' cases (0.6%) and 153 'no-crime' cases (1.3%) were dropped from the sample. Within the no-crime category, 25 malpractice cases (1.1%) were dropped, 103 liability cases (1.2%) and 25 'other' cases (1.9%). Dropping these few cases had a substantial impact on the means. The overall average (after removing punitive awards) was reduced from about \$1.243M to \$895,091 a reduction of 27.9 percent. For cases coded as crime, the reduction was 33.9 percent while for cases that could not be coded as crime were reduced 27.6 percent (a discussion of the approach to coding crime versus non-crime follows in the next section). Notably, only nine of the cases that could be coded as having originated from a criminal act were dropped as outliers. The effect was similarly variable across the types of claims. The mean for 'Other' cases was reduced by 29.4 percent and the mean for liability cases was reduced by 28.6 percent. The reduction in malpractice awards was the smallest, with a 25.4 percent reduction in average award.

Table 3.5. Mean of Total Awards with and without Outliers (Unweighted).

Category	<u>All Cases</u>		<u>Crime Cases Only</u>	
	Total	No Outliers	Total	No Outliers
Sample Size	12,918	12,762	895	886
All Observations (Median)	\$1,243,305 (\$202,615)	\$895,091 (\$194,479)	\$1,114,705 (\$111,628)	\$737,251 (\$110,827)

Note: All records include a non-missing value for total award. There are 12,918 in the full sample and 11,718 in the sample with the outliers removed. All values are adjusted for inflation and presented in 2003 dollars. The means and median values are after punitive awards have been removed, but do not include stratification weights. Median values are in parentheses.

Table 3.5 describes the effect of removing outliers. For all cases, the ratio of mean damage award to median damage award declines from 6.1:1 to 4.6:1. Within those cases coded as being the result of a crime, the mean/median ration declines from 10.0:1 to 6.3:1. Not surprisingly, there is little effect of removing outliers on the means. Thus, it appears that removal of outlier's more than three standard deviations from the mean reduces some of the effect of these observations on the average case, but does little to affect estimates for the typical case.

#### Identifying Crime and Non-Crime Cases

Before making adjustments to the dependent variable, a variable is created that identifies whether cases can be coded as resulting from a crime, or not coded as resulting from a crime. This variable can be helpful in identifying the impact of adjustments for comparative negligence and punitive damages. The RAND civil jury verdict data that will be used to estimate harms to victims of crimes includes many non-crime related civil cases. However, the RAND data does not include variables that allow crime-related cases to be clearly distinguished from non-crime related cases. Using a combination of

variables, 886 out of 12,756 were identified as resulting from a crime. These cases were identified by the type of claim or by the type of plaintiff injury. For example, cases with a gunshot wound (N= 95) were assigned to the crime category, as were rapes (N= 26). Cases in specific categories (conversion/theft (N= 44), RICO (N= 0) and assault (N= 731)) were also coded as crimes.

The indicator variable ‘crime’ denotes whether a case could be validly coded as 1 if the case can clearly be identified as crime-related or 0 if the case cannot be affirmatively coded as resulting from a crime. This definition is substantively different from coding the case as crime or not-crime, which cannot be directly observed. While the crime indicator is a dummy variable, it is important to note that there are not two categories of ‘crime’ cases in the RAND data, but three. In addition to the cases that can be affirmatively labeled as crime case, the cases that cannot be affirmatively labeled as crime are of two types:

- Cases that can validly be labeled as not resulting from a criminal act, and
- Cases that are the result of a crime, but the underlying criminality of those cases cannot be distinguished from the data.

For example, it is certain that some of the automotive cases were related to criminal acts, such as driving while intoxicated or reckless driving. In addition, it is also certain that some of the negligence cases are criminal negligence cases. Thus, those cases that cannot be affirmatively labeled as ‘crime’ related, cannot simply be labeled as not crime, and dropped from the analysis.

The premise of this research is that injury data and other case attributes are unbiased predictors of award regardless of the type of claim. According to Harcourt, the guiding principle of the criminal justice system should be that, "all similarly situated persons be treated alike" (2004: 1283). Any variance in award is therefore should be a function of the distribution of harms associated with the claim, rather than due to the type of claim. The same harms in each category—negligence, malpractice, liability, crime, and other—are expected to yield the same award. If these estimates are unbiased, then the award for each cross-combination of attributes can reasonably be used to predict awards in the NIBRS data. If this premise does not hold, however, then the resulting NIBRS estimates will be biased.

Returning to the issue of adjustments to the dependent variable, as is described in Table 3.1, valid observations for the total award and the punitive awards were available for all observations. There is no difference in expected total award between the 886 ‘crime’ awards and the 11,870 non-crime awards ( $p=0.43$ ). Punitive damages are awarded as a means of deterring futures libelous behavior, and thus are awards that seek to achieve a goal beyond compensating victims for the harm they suffered. Thus, they are not included in the final estimates of harm, and are removed from the total. Although punitive awards are twice as large for crime cases as for non-crime cases at the mean, across all observations the difference is not significant ( $p=0.32$ ). Once punitive awards have been removed, are significant differences in the expected award between crime and non-crime cases ( $p<0.01$ ).

### Comparative Negligence

As noted in the far right column of Table 3.6, there are many cases where a determination is made that there is shared negligence, that is, where the jury determines that the plaintiff shares some fault for damages. In these cases, a determination must be made whether the full award should be used as the estimate of damages, or whether the reduced award is more appropriate since it accounts for the idea that but for the actions of the victim (plaintiff) there would not have been a claim. The choice is not trivial: the average total award is significantly reduced from \$1.24 million to \$1.04 million if the reduction due to negligence is included.

One possible approach to resolving this issue might be to consider who a policy is directed toward, and calculate the value of harms accordingly. If a policy is intended to reduce the supply of crime, and is therefore directed at potential offenders, the full value of damages would best capture the magnitude of the harms the policy attempts to affect. Alternatively, if a policy is directed at potential victims (e.g., a crime prevention initiative) then developing estimates of harm that include reductions for comparative negligence is most appropriate. Finally, in the case where the policy is aimed at both parties, a mix of the two estimates would seem most appropriate.

Table 3.6. Distribution of Total Award (Unweighted)

Category	Total, Excluding Punitive and Outliers	Percent any Shared Negligence	Mean Percentage of Shared Negligence
All Observations (N=12,756)	\$895,091	25.4%	16.5%
Crime Indicator			
Crime (N=886)	737,251	15.9	10.7
No-Crime (11,870)	906,872	26.1	16.9
Issue			
Crime (N=886)	737,251	15.9	10.7
Malpractice (N=2,194)	1,631,082	11.6	7.7
Liability (N=8,397)	771,039	31.9	20.6
Other (N=1,279)	555,114	13.0	8.7

Note: All records include a non-missing value for total award (n=12,918). All values are adjusted for inflation and presented in 2003 dollars. The ‘Total Award’ column includes all awards (before any reduction for shared negligence). The ‘Total, Excluding Punitive and Outliers’ column is the total award without punitive award and with outliers removed (before any reduction in shared negligence). ‘Percent any Shared Negligence’ is the percentage of all cases (in that category) that have any ‘comparative’ or shared negligence. ‘Mean Percentage of Shared Negligence’ is the average percentage reduction due to ‘comparative’ or shared negligence.

For this paper, I chose to follow Cohen and Miller (2003: 167) who argue that “since juries are told to first determine the full level of compensation” the use of the un-adjusted estimate is most appropriate. That is, since the goal of an estimate of the price of harm is to determine what harms were suffered, inclusion of a normative standard that the plaintiff can only claim those damages that were not their responsibility seems to only cloud the issue. Ultimately, if policy moves in a direction where the victim’s culpability is considered in determining the efficacy of a policy choice, then the damages adjusted for comparative negligence should be used instead.

## Weights

The RAND data is comprised of probability weighted random samples with unequal selection probabilities. RAND researchers observed the prevalence of a type of case in the population of cases resulting in civil jury award. Cases were stratified to conserve coding resources. For the majority of cases (56.9 percent) there was no sampling and thus cases were assigned a weight of 1. In 9 percent of cases, every second case was selected, in 7.7 percent every third case was selected, and in 27.2 percent every fourth case was selected. All analysis includes the sampling weight using the FWEIGHT function in STATA 10.1 or the weight function in SAS 9.0. To avoid having the effective sample size increase to 26,313 with the inclusion of the weights, the weights are normalized to sum to 12,756. The effects of weighting are shown in Table 3.7, which accounts for the stratification weights. As shown in Table 3.5, adding the weights to the analysis produces several slight differences, mainly a small increase in average award.



Table 3.7. Distribution of Total Award (Weighted)

Category	Total, Excluding Punitive and Outliers	Total, Excluding Punitive and Outliers (Weighted)
All Observations (N=12,756)	\$895,091	\$921,015
Crime Indicator		
Crime (N=886)	737,251	768,287
No-Crime (11,870)	906,872	934,364
Issue		
Crime (N=886)	737,251	768,287
Malpractice (N=2,194)	1,631,082	1,590,433
Liability (N=8,398)	771,039	786,639
Other (N=1,279)	555,114	507,412

Note: All records include a non-missing value for total award (n=12,918). All values are adjusted for inflation and presented in 2003 dollars. The ‘Total, Excluding Punitive and Outliers’ column is the total award without punitive award and with outliers removed (before any reduction in shared negligence). The ‘Total, Excluding Punitive and Outliers (Weighted)’ column is the total award without punitive award and with outliers removed (before any reduction in shared negligence) and applies the stratification weights.

### Effect of Adjustments to the Dependent Variable

Thus, the cumulative effect of all of the adjustments in the data is to reduce the award by about 25 percent, while also reducing the standard deviation by about half—the mean award (excluding punitive damages) is \$1.38 million (SD= \$5.9M) in the unadjusted data and \$1.0M (SD=\$3.1) in the data that is weighted and has both the punitive awards and the outliers removed. From Table 3.5 it is clear that the adjustments had larger effects on particular categories. The crime category has a mean that is about 40 percent smaller after the adjustments, and the ‘other’ category, which includes negligence cases, is reduced by more than half. The variance is substantially reduced in all categories.

Table 3.8. Distribution of Total Award (Weighted).

Category	Unadjusted Average Award (N=12,918)	Total, Excluding Punitive and Outliers and Weighted (N=12,756)
All Observations	\$1,376,341 (\$5,893,546)	\$921,015 (\$2,915,095)
Crime Indicator		
Crime	1,372,278 (6,101,927)	768,268 (2,770,531)
No-Crime	1,376,643 (5,878,000)	1,016,258 (2,924,938)
Issue		
Crime	1,372,278 (6,101,927)	768,268 (2,770,531)
Malpractice	2,256,547 (8,967,848)	1,590,433 (4,957,694)
Liability	1,184,525 (4,668,991)	786,639 (2,212,087)
Other	1,356,582 (6,167,062)	507,412 (1,646,160)

Note: All records include a non-missing value for total award (n=12,918). All values are adjusted for inflation and presented in 2003 dollars. The ‘Unadjusted Average Award’ reports the averages before any adjustments. The ‘Total, Excluding Punitive and Outliers (Weighted)’ column is the total award without punitive award and with outliers removed (before any reduction in shared negligence) and applies the stratification weights. Standard deviations are in parentheses.

### Economic and Non-economic Awards

While all 12,756 observations contain valid values for most components of the jury awards, there is substantial missing data on the distribution of economic and non-economic awards within the total award. In order to calculate non-economic damages for property crimes (where the economic damages will be calculated from NIBRS), missing data in economic and non-economic awards must be calculated whenever possible.

Appendix A.1 describes the procedures used to recover economic and non-economic

damages. After these adjustments, a total of 5,539 cases had both economic and non-economic damages and 7,217 did not.

### Selecting the Analytic Samples

A critical question in this analysis is whether the RAND data are representative of all criminal cases. The RAND data might be un-representative due to two issues. First, it may be that claims that end up being settled by a jury award are different from cases that are settled out of court, or not pursued in the civil court system at all. A substantial literature has emerged addressing this issue, and it is reviewed in Appendix A.2. Chapter 4 includes a few empirical tests of this proposition, to the extent the data allow. That is, I test for whether the NIBRS cases that was matched with the RAND data are different from the NIBRS cases that did not match. If the NIBRS dataset as a whole is unrepresentative of all crimes, as some have argued, that bias cannot be tested in this analysis.

This section takes up the issue of whether the RAND data are unrepresentative when all cases and not just crime cases are included in the analytic sample. There are three possible analytic samples to use in this analysis: all cases, only cases affirmatively coded as resulting from a crime, and a sample from both groups that have been empirically balanced to control for differences both in case attributes and in variables theoretically associated with unrepresentative jury awards. Coding of the ‘all case’ sample and the ‘crime’ sample is straightforward. The section that follows describes the propensity score analysis.

### Propensity Score Analysis

The data presented in Table 3.5 show that there are substantial differences between the ‘crime only’ group, the ‘all case’ sample and the propensity score sample. If a larger dataset can be retained the analysis would have substantial advantages over prior research, in terms of the power of the analysis and the number of types of crime that could be valued. In order to do so, it must be demonstrated that the cases that can be coded as resulting from a crime are not different from the cases that cannot be coded as having resulted from a crime across key theoretical predictors. Initially, there are only small differences in total award from crime (\$1.372M) and non-crime cases (\$1.376M) in the raw (but weighted) data in Table 3.4 ( $p=0.98$ ). Once punitive damages are removed, the difference increases, but is still non-significant ( $p=0.43$ ). However, when the outliers are removed, large and significant differences emerge ( $p<0.01$ ) that persist once the stratification weights are added ( $p<0.05$ ).

One means of isolating the effect of crime on jury awards is to control for the confounding influences of other theoretically relevant covariates via multivariate regression (Sampson and Laub, 1990, 1993; Warr, 1998). However, a substantial body of research (Greene, 1981; Heckman, 1977, 1990; Rosenbaum and Rubin, 1983) demonstrates that covariates alone are insufficient to attain unbiased estimates, and the problem is even more serious in this data as is described below. Propensity score models have been proposed as a viable solution to modeling selection bias (Rosenbaum and Rubin, 1983; Heckman Ichimura and Todd, 1997; Dehejia and Wahba, 1999; Caliendo and Kopeinig, 2006). Under certain conditions, propensity score analysis can approximate a randomized controlled trial and generate unbiased estimates (Caliendo and

Kopeinig, 2005; Rosenbaum and Rubin, 1983). Propensity scores are used here to address the possibility that there are variables related to both crime and award. The propensity score is the conditional probability of being in the crime category or the not-crime category,  $\Pr(T_i = 1 | X_i)$ , where  $T_i = 1$  if the case is labeled as a crime and  $X_i$  is a vector of covariates theoretically or empirically linked to the categorization of a case as a 'crime' case.

There is little advice in the extant literature about the ideal functional form that a propensity score model should take (Smith, 1997). As logit and probit models yield similar results for binary measures, I follow Caliendo and Kopeinig (2005) and use a logit specification as the logistic distribution has more density mass in the bounds. Predicted propensity scores can be sensitive to variable selection and omission of important variables in propensity score models can seriously bias the resulting estimates (Heckman Ichimura and Todd, 1997; Dehejia and Wahba, 1999). However, over parameterization of propensity score models can decrease the support space of the propensity scores and increase the variance and the resulting standard errors (Augurky and Schmidt, 2001; Bryson Dorsett and Purdon, 2002). Ultimately the choice of variables should be dictated both by relevant theory and by previous empirical findings (Caliendo and Kopeinig, 2005).

I follow Heckman, Ichimura, Smith and Todd (1997) and begin with a parsimonious model containing several theoretically important predictors of selection. The model includes all case attributes that can reasonably be expected to have an association with whether or not the case was a crime. The first variables added to the model are the variables common to both RAND and NIBRS (the variables that will be

used to create the cross-combination of attributes that will be used predict crime prices in NIBRS). These include age, gender, dummies for nine injury types and region. Next, variables are added that are theoretically linked to biased jury awards, including a business as a defendant (and the type of business), whether there was a punitive award, and whether there was shared negligence.<sup>13</sup> Four variables that co-vary with whether a case was coded as a ‘crime’ or not are also included (population, number of plaintiffs, age-squared and number of issues). Finally, selected interaction terms were added to each model (age by gender, gender by region) to ensure a model whose functional form is properly specified. This process yields a propensity score model containing 28 predictors. Thus, the general model is a logistic regression model of the following form:

$$\text{Log}(Y_j/(1-Y_j)) = \phi T_j + \gamma Z_j + \varepsilon$$

where  $Y_j$  is one if case  $j$  is coded as a ‘crime’ case and 0 if case  $j$  is coded as a non-crime case;  $T_j$  is the vector of independent variables that are observable in both RAND and NIBRS data and  $Z_j$  is the vector of independent variables that are only observable in the RAND data.

Tables 3.9, 3.10 and 3.11 describe the independent variables used in this analysis.

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<sup>13</sup> The values of independent variables are described later in the chapter. The literature review early in this chapter describes five sources of bias in jury awards. These are extralegal factors that may lead juries to award plaintiffs more (or less) than they would receive if the jury’s award was strictly limited to the objective harms they suffered. As the literature review at the beginning of this chapter suggests, certain types of cases—product liability and medical malpractice in particular—tend to have higher awards. In addition, there is substantial variation in awards by state; cases where the defendant is a business may receive higher awards; men receive larger awards than women on average; and cases with particularly reprehensible defendants have higher awards. The purpose of including these variables is to balance the crime and non-crime cases along these attributes. But, to the extent that these factors may lead to an overestimation of the price of crime, they are not controlled for in the analysis. This is discussed in more detail in the next section “Why Propensity Scores are Preferred to Covariates.”

Table 3.9. Distribution of Injuries in RAND

Injury Categories	Independent Variables Predicting Jury Award - Injury		
	<u>All Cases</u> (N=12,756)	<u>Not Crime</u> (N=12,023)	<u>Crime (N=895)</u>
None	7.8%	8.0%	10.1%**
Dead	11.0	11.1	9.7
Broken Bones	23.4	23.5	23.0
Internal Injuries	7.1	7.3	4.5***
Severe Lacerations	6.7	7.0	3.7***
Apparent Minor Injuries	27.5	27.0	30.0
Other Major Injuries	49.0	49.6	42.3***
Loss of Teeth	5.3	4.9	10.3***
Unconsciousness	5.1	5.0	5.6

Source: Analysis of data from the RAND Institute of Civil Justice, 12,918 cases from 1985-1999.

Frequency weights are applied to all analyses. Injuries do not sum to 1 as individuals may report more than one injury. All dollars are calculated as 2003 Dollars. Independent samples t-tests results are reported comparing crime and not crime categories. \*p<0.05, \*\*p<0.01, \*\*\*p<0.001.

Most of the injury categories are significantly different when comparing the crime and the non-crime cases. Rates of internal injuries, severe lacerations and other major injuries are lower in the cases coded as resulting from crime than in the cases not coded as resulting from crime. Rates for loss of teeth and no injury are lower in the crime category. There is no difference in rates of death, broken bones, apparent minor injuries and unconsciousness.

Virtually all of the theoretical predictors of jury award bias differ between the two groups. Cases in the crime group involve fewer plaintiffs, appear a little earlier in the sample and have slightly younger defendants.<sup>14</sup> A significantly smaller percentage of

<sup>14</sup> In the RAND data, a specific age is observed for all plaintiffs, except those under 18, who are categorized as 'under 18'. Since there was no available information to impute those ages, five age categories were created: under 18, 19-29, 30-39, 40-49, and over 50. These categories were used as well in

defendant were businesses in the crime group, and there were far more punitive awards (as noted in Appendix A.3, if there was a punitive award, it was reverse-coded so there was a value=0 for the dummy indicating the opportunity for a punitive award). Females were less common in the crime category and cases were significantly more likely to come from the west and less likely from the northeast.

Table 3.10. Theoretical Predictors of Jury Award Bias

Injury Categories	Theoretical Predictors of Jury Award		
	All Cases (N=12,918)	Not Crime (N=12,023)	Crime (N=895)
Number of Plaintiffs	1.29	1.29	1.24**
Mean Year of Case	1992	1992	1991***
Victim Age	3.58	3.63	3.05***
Defendant is a Business	45.0%	45.5	40.0***
Punitive Award	4.6	3.2	20.1***
Case After 1990	62.2	63.1	52.1***
Gender (Female=1)	42.4	43.5	30.9***
Region (South)	3.7	3.7	3.1
Region (Midwest)	25.4	25.4	25.7
Region (Northeast)	30.0	30.7	21.7***
Region (West)	40.9	40.1	49.5***

Source: Analysis of data from the RAND Institute of Civil Justice, 12,918 cases from 1985-1999. Frequency weights are applied to all analyses. All dollars are calculated as 2003 Dollars. Independent samples t-tests results are reported comparing cases categorized as crime to cases not categorized as crime within each row. \*p<0.05, \*\*p<0.01, \*\*\*p<0.001.

The type of claim<sup>15</sup> also varied significantly across the two groups. In the crime category, there were significantly more ‘other’ malpractice cases, dramshop (alcohol outlet) cases, as well as defamation, civil rights, intentional torts, property, conversion/theft, and other business cases. There were significantly fewer vehicle/auto

the creation of the cross combination of attributes in the RAND data. Thus, a value of 3.05 suggests the average person in the crime category was in their 30’s.

<sup>15</sup> RAND researchers coded more than one type of claim if appropriate.



liability and common carrier liability cases, as well as fewer medical malpractice, product liability and wrongful termination cases. There was no difference in the prevalence of property/premises, insurance/bad faith and other negligence cases.

Table 3.11. Types of Claim in Civil Cases

Injury Categories	Independent Variables Predicting Jury Award – Issue		
	<u>All Cases</u> (N=12,918)	<u>Not Crime</u> (N=12,023)	<u>Crime (N=895)</u>
Vehicle/Auto Liability	37.2%	40.2%	2.7%***
Common Carrier Liability	1.8	1.9	1.0*
Property/Premises Liability	23.8	23.9	22.5
Medical Malpractice	17.9	19.3	1.7***
Other Malpractice	5.3	4.9	10.0***
Product Liability	9.5	10.3	0.9***
Dramshop	1.0	0.6	5.0***
Defamation	0.4	0.3	1.0*
Civil Rights	3.7	2.1	22.6***
Intentional Tort	7.5	0	93.8***
Other Negligence	5.4	5.4	4.5
Insurance/Bad Faith	1.5	1.6	0***
Wrongful Termination	2.8	2.9	1.2**
Property Suit	0.2	0.1	1.2***
Conversion/Theft	0.4	0	4.7***
Other Business	3.0	2.9	4.2*

Source: Analysis of data from the RAND Institute of Civil Justice, 12,918 cases from 1985-1999. Frequency weights are applied to all analyses. There were no RICO cases or shareholder cases. RAND researchers coded cases into more than one category. Independent samples t-tests results comparing cases coded as crime and cases coded as not crime are reported. \*p<0.05, \*\*p<0.01, \*\*\*p<0.001.

To generate the propensity score, a logistic regression was run where the binary dependent variable (crime or not crime) was regressed on the variables in Tables 3.6 and

3.7.<sup>16</sup> In addition, interaction terms describing the interaction between gender and age and region were added to the model, along with a squared age term. In the best fitting model, including the propensity score weight,<sup>17</sup> independent samples t-tests of the differences between crime and non-crime cases find that 19 of the 23 attributes are significantly different at  $p < 0.05$ .

#### Choice of Weighting or Matching

The results of the propensity model can be used to generate several different types of corrections for group imbalance. One common approach is to match observations where the predicted probability is similar, and to delete those observations that are outside the range of common support. However, such an approach requires that the groups are relatively equivalent in terms of the number of observations in each group. In this analysis, the two groups are not equivalent in size – the crime coded group is about one-fifth the size of the non-crime group. While nearest neighbor matching could still be performed using a 5 to 1 match, with replacement, I find little support in the literature for that a high a many to one ratio. Instead, I generate inverse probability of treatment weights, which are a common correction in the propensity literature. Using the fitted values from the propensity score models, I follow Sampson, Laub and Wimer (2006) and construct IPTW weights to account for selection into the crime category. Each case is assigned a weight that is the inverse probability of crime receipt conditional upon  $T_j$  and

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<sup>16</sup> The type of claims in Table 3.11 were not added to the model, since the dependent variable was constructed from those claim categories, or sub-categories.

<sup>17</sup> The propensity weight and the stratification weight were combined into a ‘super-weight’, as is discussed later in the chapter.

$Z_j$ . Thus, the crime cases that resemble non-crime cases along  $T_j$  and  $Z_j$  and the non-crime cases that resemble crime cases along  $X_i$  receive the greatest weight in the analysis.

These weights were then standardized in the same manner as the RAND stratification weights. The product of the two weights creates a ‘super-weight’ for each observation in the analysis. This combined weight jointly accounts for an observation’s probability of stratification and selection, and these weights are used in all models that control for selection. In order to ensure that no single observation had an undue amount of influence on outcomes, weights were winsorized at a value of four. Notably, when crime prices are estimated for crime types that do not account for selection (all non-person crimes); only the stratification weight is used.

#### Reducing the Sample to Achieve Better Balance on Covariates

Thus, the initial propensity score models failed to adequately balance the sample. While there is no completely satisfactory means of testing whether a propensity score model has created a weight that yields a more balanced model, the goal of the modeling is to create groups that are equivalent on attributes that predict assignment. Thus, there should be no more differences in these attributes than is predicted by chance. However, the global test of the hypothesis that all coefficients were equal to zero was rejected at ( $p < 0.001$ ). While this test is less reliable in large samples, the p-value suggests substantial unexplained heterogeneity in the propensity score models.

One likely source of heterogeneity was the unbalanced distribution of the dependent variable (886=1; 11,870=0). The implication of this imbalance is that there were types of cases in the non-crime cohort for which there was no similar match in the smaller crime cohort. One solution to this problem is to only include those cases with an

analogue in the other group. In order to create a more balanced sample, cross-combinations of all the covariates in the crime and no crime group were created. Only those cross-combinations which were observed in both groups were retained.

Selecting only those observations with common attributes would theoretically create a more consistent dataset for analysis. If, for instance, malpractice cases with female defendants were not observed in the original dataset, there would be no way to control for differential jury verdicts between that group in the crime and not-crime conditions. This resulted in a much smaller dataset being available with a total of 5,403 observations. A total of 603 crime cases (weighted by the stratification weights to resemble 585 cases) and 4,800 not-crime cases (weighted to resemble 4,770 not-crime cases) were observed. Next, this sample was used to create the new propensity weights. The propensity score model was re-run using the original specification, and the inverse of the probability condition on crime is calculated.

When the global test of the hypothesis that all coefficients were equal to zero is re-calculated, the hypothesis is again rejected ( $p < 0.001$ ). In small datasets this might indicate a lack of balance. However given the size of this sample, and thus the statistical power, failing to reject the null would require close to perfect balance. To ensure that the weights and properly balancing the sample, weighted t-tests were run on the sample of 5,403 jury cases using the RAND stratification weight alone, and the super-weight.

These results suggest a substantial improvement in the balance across the two samples. Table 3.13 describes t-test results comparing the 'Crime' sub-group to the 'Not crime' subgroup on the independent variables (excluding interactions). The statistic reported is the difference between the two groups, first adjusting only for the

stratification weights in RAND, and then adjusting for both the stratification and IPTW (the ‘superweight’). Except for age, the variables are mean percentages, and thus the statistic reported is the percentage difference between crime and not crime.

Table 3.12. Propensity Score Diagnostics (N=5,403)

Independent Variables	Difference Between ‘Crime’ Mean and ‘Not Crime’ Mean	
	No Propensity Weight	w/Propensity Weight
Age (1=‘Crime’)	0.232***	-0.003
Gender	0.035+	0.023
Region (Midwest)	-0.048**	0.024
Region (West)	0.002	-0.060**
Region (South)	0.006	0.068
Defendant a Business	0.026	0.029
Shared Negligence	0.018*	-0.015
Punitive Award	-0.004***	-0.004
Defendant (Government)	-0.125***	-0.06***
Defendant (Hospital)	0.08***	0.077***
Defendant (Individual)	-0.038+	-0.093***
No Injury	-0.020+	-0.013
Dead	0.0053	0.061
Injuries/Broken Bones	-0.060***	-0.020
Internal Injuries	-0.021**	0.001
Severe Lacerations	-0.020**	0.005
Minor Injuries	-0.062***	0.003*
Other Major Injuries	0.129***	0.044
Loss of Teeth	-0.052***	-0.008
Unconsciousness	-0.043***	-0.006
Population	0.034	0.019
Plaintiff Count	0.024	0.021**
Number of Issues Adjudicated	-0.62***	-0.058

Source: Analysis of data from the RAND Institute of Civil Justice, 5,403 cases from 1985-1999. Frequency weights are applied to all analyses. The final column includes IPTW and thus adjusts using the ‘superweight’. Independent samples t-tests results comparing cases coded as crime and cases coded as not crime are reported. +=p<0.10, \*=p<0.05, \*\*=p<0.01, \*\*\*=p<0.001.

Using only the RAND stratification weights, 14 of the 23 independent variables had significantly different values for the crime and not-crime cohorts ( $p < 0.05$ ) and three others were significant at  $p < 0.10$ . However, once the super-weights were applied, six of the independent variables are significantly different at  $p < 0.05$ . Age, whether or not the jury had the opportunity to make punitive awards, the number of issues adjudicated, and all of the injury categories become non-significant after the super-weight is applied. Of the six variables that remain significant, three measure the same construct, whether or not the defendant is an individual. Given that one or two variables should be significant by chance, it appears the super-weights do a reasonably good job of balancing the two samples. Thus, the smaller sample size and super-weights were retained and used in subsequent analyses. Some additional discussion of the construction of the independent variables can be found in Appendix A.3.

#### Why Propensity Scores are Preferred to Covariates

The obvious alternative to the propensity score models is simply to use covariates to control for those factors that are related to selection into the ‘crime’ group and those factors that are related to theoretical predictors of extralegal jury awards. Taking up the latter issue first, the problem with not including variables to control for the effect of extralegal factors on jury awards is that the resulting price of crime estimates will include these factors when they should not be included in the crime price. Thus, the preferred approach would seem to be to model  $Y = \alpha + \beta_1 X_1 + \beta_2 X_2 + \varepsilon$  (3). In (3)  $X_2$  is a vector of variables that theoretically predict bias in jury awards with respect to actual victim losses. The model should yield an unbiased estimate of jury award by crime type,  $\alpha + \beta_1 X_1$ .

The problem with this model is that the vector  $X_2$  is not observable in the NIBRS data. If the awards associated with injury and other attributes in RAND are modeled with variables that are not in NIBRS, then the models will implicitly predict values in NIBRS as a function of attributes that are not observable in those data, including most of these selection factors. More formally, modeling covariates in the RAND model assumes that those covariates have the same mean in the NIBRS data, which is clearly not correct since many of the extralegal factors associated with biased jury awards are not observable in NIBRS.

Returning to the issue of using theoretical predictors to control for differences between crime and not crime cases, it may be the case that these attributes are heterogeneously related to the type of case. Certain factors cause jury's to award on extralegal factors differentially in crime cases than in medical malpractice cases. Thus, it is necessary to balance the crime and non-crime cases along this attribute. In this case, those differences are accounted for via weight, which effectively re-orders the data, rather than by a propensity score covariate, which would incorrectly imply an identical propensity score mean in the NIBRS for that observation. Unfortunately, the lack of extralegal factors in NIBRS means that any part of the jury award that is due to extralegal factors cannot be corrected in the crime price estimates.

## Results

Table 3.13 describes the differences in means and medians for the three analytic samples: (only cases affirmatively coded as resulting from crime, cases in the propensity score models, and all cases). Overall, the estimates are relatively consistent. The largest average awards are for those cases in the propensity model, but the average award for

those cases (\$981,600) is close to the estimate from all cases (\$895,401) and the median of the propensity cases and all cases differ by less than \$3,000. Awards for those cases affirmatively coded as crime cases are lower. Overall, the highest average award was in the propensity cases in 4 of the 9 injury categories, in the all cases for three injury categories, and in the crime cases for the other two categories.

Table 3.13. Award Estimates by Type of Injury, RAND Data Only

Injury Type	Costs by Injury Category					
	Crime Only (N=886)		Propensity Model (N=5,403)		All Cases (N=12,756)	
	Mean	Median	Mean	Median	Mean	Median
None	\$368,823	\$31,876	\$423,460	\$114,421	\$356,614	\$100,330
Dead	1,985,398	911,261	1,854,949	1,146,531	1,645,356	879,604
Injuries/ Broken Bones	358,638	74,154	708,886	222,316	585,164	165,076
Internal Injuries	1,482,423	190,622	1,744,959	661,737	798,798	325,917
Severe Lacerations	1,771,304	1,214,796	1,751,367	1,183,395	1,637,578	758,565
Apparent Minor Injuries	321,275	34,264	286,735	51,712	361,801	74,284
Other Major Injuries	844,058	139,350	1,143,109	197,595	1,065,753	216,282
Loss of Teeth	386,382	63,604	423,308	100,968	472,747	104,966
Unconsciousness	313,047	101,532	381,595	90,148	497,094	77,747
Average Award	\$737,251	\$110,827	\$981,600	\$197,423	\$895,401	\$194,479

Source: Analysis of data from the RAND Institute of Civil Justice, 12,918 cases from 1985-1999. Frequency weights are applied to all analyses. All dollars are calculated as 2003 Dollars.

### Conclusion

The selection of a smaller sample using common cross-combination of attributes and the creation of IPTW using propensity score analysis completes the preparation of the RAND data for interpolation onto the NIBRS data. Chapter 4 describes the interpolation and presents estimates of the costs of crime to victims. Chapter 5 describes the implications for policy and research.





## Chapter Four: Estimates of the Price of Crime

This chapter describes the analytic strategy to interpolate jury award data onto the NIBRS data and reports prices for 31 crime types. Chapter 3 described how mean values for each sub-group (each cross-combination of case attributes) was computed. In Chapter 4, the mean value for each cross-combination, or subgroup, from the RAND data is then interpolated onto the NIBRS data. Variation in this analysis results from the distribution of subgroups across crime categories. One subgroup may be observed in multiple crime categories, and multiple subgroups may be observed in each crime category. For example, a 30-39 male with major injuries may appear in NIBRS data in assault cases and in robbery cases. In addition, within the assault robbery cases in NIBRS there are also other combinations of age, gender and injury.

Thus, the average harm to a victim of a particular crime can be estimated as the average of the total harms of all victims of a particular type of crime in that category. That is, the total harms are summed across the heterogeneous distribution of injuries and victim attributes within that category, and divided by the number of cases. This process is repeated for each of the 31 crime categories for each of the three analytic samples: all cases, only cases affirmatively coded as being related to a crime, and the propensity score weighted model. Ex ante, the propensity score model is preferred, since it requires the weakest assumption about the generalizability of the RAND data, but results from all three analyses are presented. Notably, the approach described above is only applied to data on the most serious person crimes. More direct estimation is used to estimate the prices of less serious crimes.

The chapter is structured as follows. First, I address how to deal with cases where multiple crimes occur. Next, I describe the types of crimes for which estimates will be developed. The analytic strategy for each of those crimes is discussed, followed by a description of the price estimates for each category. The chapter concludes with a discussion of the differences between the NIBRS cases that were matched to the RAND data, and the implications of those differences for the generalizability of the results.

### Multiple Victimitizations during a Single Event

If, as is suggested above, the analysis should account not only for the risk that a robbery becomes a homicide, but also for the risk that a robbery includes less serious crimes. Finally, there is the issue of victims suffering victimization from multiple crimes. That is, the estimates generated above are interpretable only as the cost to a victim whose only charge is robbery, for instance. The approach taken here is simply to sum up the costs for each crime, for each victim. That is, for a robbery victim whose car was stolen, the total harm is the sum of the costs of the robbery (conditional on their age, gender and injury) and the costs of the stolen vehicle. The limitation of this approach is that it treats each criminal incidence as if the victim was not the same, and thus does not account for the fact that some injury patterns may be associated with victimization in more than one crime.

### Estimating Losses in Serious Property Crimes

Crimes are defined as most serious when the crime commonly results in property loss and lost wages or health care costs, and the crime results in indirect harms (pain and suffering, and/or fear). As described below, selection of crimes into this category is

based on FBI crime classifications and prior research on the costs of crimes to victims.

Ten crimes were assigned to this category:

- Murder/Non-negligent Manslaughter ;
- Kidnapping/ Abduction;
- Forcible Rape;
- Forcible Sodomy;
- Sexual Assault with Object;
- Forcible Fondling;
- Robbery;
- Aggravated Assault;
- Simple Assault;
- Intimidation;

Additionally, the price of crime was estimated for eight crime categories where the only direct losses were property losses. These crimes include:

- Arson;
- Extortion/ Blackmail;
- Burglary/ Breaking and Entering;
- Motor Vehicle Theft;
- Counterfeiting/ Forgery;
- Swindle;
- Credit Card;
- Impersonation;

For these crimes, an assumption was made that in addition to property loss, victims suffer indirect losses. As discussed below, the basis for this assumption was the finding in the RAND data that cases in the theft/conversion category generally had an indirect award as well as a direct award. For these types of crimes, property losses were observed directly from the NIBRS data. In addition, to account for the indirect losses, the cases were weighted. Weights were generated from the RAND data as the coefficient of indirect to direct losses in cases where there was no injury or only minor injuries.

Finally, prices were estimated for an additional 13 crimes where losses were assumed to be limited to property loss alone. While it is conceivable that some indirect

harm occurred in these cases as well, there was no empirical justification for the inclusion of an indirect loss, and so none was included. Thus, to the extent that there is an indirect loss in addition to the property loss, these estimates underestimate the true price of these crimes:

- Pocket-Picking;
- Purse-Snatching;
- Shoplifting;
- Theft from Building;
- Theft from Coin-Operated Machine;
- Theft of Motor Vehicle Parts;
- Theft from Motor Vehicle;
- All Other Larceny;
- Welfare Fraud;
- Wire Fraud;
- Embezzlement;
- Stolen Property Offenses;
- Destruction/ Vandalism;

## Results

### Estimating Losses in Serious Person Crimes

Table 4.1 describes the estimated prices for serious person crimes. Serious person crimes can result in economic losses of all three types (direct (wages and health), direct (property) and indirect (fear, diminished value of life, pain and suffering)).

Table 4.1. Estimates of Harms for Serious Injuries (RAND and NIBRS Data)

Crime Type	Crimes with Direct and Indirect Costs					
	<u>Crime Only</u>		<u>Propensity Model</u>		<u>All Cases</u>	
	Mean	N	Mean	N	Mean	N
Murder/Non-negligent Manslaughter	\$1,532,342	(N=1,146)	\$1,445,463	(N=818)	\$1,466,160	(N=2,369)
Kidnapping/ Abduction	64,358	(6,117)	140,697	(6,115)	161,433	(9,565)
Forcible Rape	64,774	(11,487)	107,486	(11,486)	190,366	(21,668)
Forcible Sodomy	103,359	(1,456)	425,168	(1,456)	229,024	(4,207)
Sexual Assault with Object	55,767	(1,100)	142,261	(1,100)	198,353	(2,585)
Forcible Fondling	52,958	(7,072)	162,235	(7,071)	195,264	(22,006)
Robbery	197,741	(31,542)	279,085	(31,524)	236,487	(45,458)
Aggravated Assault	223,180	(76,256)	283,793	(76,114)	307,768	(120,148)
Simple Assault	77,008	(342,201)	101,624	(342,201)	127,356	(501,645)
Intimidation	57,850	(137,362)	117,625	(137,362)	151,726	(175,605)

Source: Analysis of data from the RAND Institute of Civil Justice, 12,918 cases from 1985-1999 and 2000 NIBRS data. Frequency weights are applied to all analyses. All dollars are calculated as 2003 Dollars.

Overall, the propensity results generally predict the highest awards and the cases affirmatively coded as resulting from a crime produce the lowest awards. Crimes generally follow the expected pattern with respect to the ranking of crimes by severity. Homicides have the highest prices, and simple assaults and intimidation are the lowest. The least variation appears in the group of all cases, and the greatest variation appears in the crime category.

As will be discussed in Chapter 5, when these estimates are compared to extant estimates, they appear very similar. The relative ranking is similar in terms of the severity of crimes is similar, and the ratio of prices comparing more severe to less severe crimes is also similar. However, it is somewhat difficult to compare these estimates directly. This study has many more categories of crime than appear in other studies. Thus a direct

comparison of prices is difficult. For instance, there are four classifications of rape in the present study, and generally only one in other studies. Similarly, aggravated and simple assault are combined in most prior work, but disaggregated in this study. To create a set of estimates that are more easily compared to extant estimates, Table 4.2 presents the main crime categories presented in past work. The rape category is presented as the average of the sub-categories (weighted by the prevalence of each subcategory).

Table 4.2. Price Estimates for Aggregated Part One Crimes, Propensity Score Models

Crime Type	Crimes with Direct and Indirect Costs					
	Mean	90%	75%	Median	25%	10%
Murder	\$1,445,463	\$2,838,800	\$1,782,500	\$1,380,246	\$848,183	\$272,169
Rape	149,542	238,395	214,449	18,908	3,524	1,393
Robbery	279,085	605,225	334,515	88,915	68,326	18,908
Assault	134,770	334,515	155,270	66,644	13,285	611
Agg. Assault	283,794	569,701	294,335	89,815	27,956	1,900
Simple Assault	101,623	238,396	87,000	59,083	35,239	600

Source: Each of the estimates is derived from the study cited. All estimates are presented in 2008 dollars (Cohen (1988) is in 1985 dollars, Miller et al. (1996) are in 1993 dollars. The rape data in this study are a weighted average of forcible rape, forcible sodomy and sexual assault with an object. The assault data from this study includes a weighted average of aggravated assault and simple assault, assault data from other studies may include aggravated or attempted assaults. The French and McCollister study presents estimates for aggravated assault only. The larceny/theft value in the this study is the weighted average mean of NIBRS categories: Theft From Building, Theft From Coin-Operated Machine or Device, Theft From Motor Vehicle, Theft of Motor Vehicle Parts/Accessories, All Other Larceny.

The estimates in Table 4.2 suggest that there is a substantial skew in the awards, where a small number of cases have exceptionally large awards. For every serious crime other than murder, the mean award is substantially larger than the median, generally two to three times larger. It is also important to note that for these crimes it is common for a victim to suffer relatively small damages. In every crime but murder, the tenth percentile

of cases experiences damages that are an order of magnitude smaller than the mean award.

### Estimating Losses in Less Serious Property Crimes

As has been noted, not all types of crime are represented in the RAND data. Civil juries only hear cases where the expected award is above some unknown threshold of opportunity cost. This leads to the hypothesis that jury awards over-sample more serious crime and under-sample or exclude less serious crime. While much of the paper is concerned with the issue of how to deal with potential over-estimation in the most serious crimes, this section addresses the converse – how to estimate losses from crimes that are unlikely to result in physical harms, and thus that are unlikely to be represented in the RAND jury data.

To account for potential bias due to under-representation of crime types, crime types were in the RAND data stratified ex ante into two categories – those crime types that were expected to be represented in the RAND data, and those crime types for which no analogue in the RAND data was expected. In general, less serious crimes are assumed to have damages that are overwhelmingly related to property loss (while most criminal incidents fall into this category, including larceny, vandalism and shoplifting, most prior research suggests that these crimes account for a small percentage of total harms to victims). Thus, for crimes where losses are assumed to be due to property loss and not other economic losses or non-economic losses, property losses were calculated directly from NIBRS data.

However, it is reasonable to presume that some crimes that lead only to property loss will have an indirect cost to victims. While serious person crimes that involve



property loss have been discussed above, other property crimes, such as burglary have been consistently shown to have substantial indirect costs in addition to the property loss (Cohen, 1988; Miller et al., 1995; Rajkumar and French, 1997; Cohen et al., 2004; McCollister, 2004). In fact, since crime is an involuntary and unwanted transaction, it would be reasonable to presume that all criminal events involved some indirect loss. However, some of the indirect losses are so small that the variation introduced by estimation is likely to outweigh the gains in terms of explanatory power. The challenge is to determine which types of property crime should have an indirect element, and which should not. The somewhat arbitrary decision in this analysis was to include property crimes that could be charged as felonies as crimes that likely have an indirect component, and to assume no indirect cost for property crimes that do not have an indirect element.

The logic behind this assumption is straightforward. Non-monetary sanctions (correctional sentences) reflect society's evaluation of the harms associated with criminal offending (Becker, 1968; Ehrlich, 1973; 1996; Shavell, 1985; 1987a; 1987b; 1991; 1993; Polinsky and Shavell, 1992; 1994; 1999). Thus, it is reasonable to assume that more serious offending is associated with longer sentences. In practice, the legal system makes a clear distinction between cases that have the potential for one or more years of incarceration in state prison, and cases that are eligible for less than one year of incarceration, to be served in local jails. The assumption made here is that the distinction in severity of punishment is reflected in both direct and indirect losses.

In these property crimes cases, the assumption is that direct losses will be entirely due to property loss, and not a function of injury, lost wages or other direct economic damages. However, the RAND data does not distinguish between property loss and other

economic losses, such as lost wages and medical bills, so this assumption cannot be tested. The analytic strategy here is to use the RAND data that describes the relative losses between direct and indirect losses. Once that ratio is computed, the rate of indirect to direct losses becomes a weight in the analysis of NIBRS data. In order to do this, it is necessary to have substantial data about the relative indirect and direct losses. While a valid total loss is observable for all observations in the RAND data, only about 30 percent have valid values for both economic and non-economic losses (as described in Appendix A.2., additional values are calculable from other information in RAND). Overall, more than 5,500 observations were identified that record both direct and indirect losses. Among these cases, the ratio of indirect losses to direct losses is 0.7:1.<sup>18</sup> Thus, the estimated losses from the property crimes with the potential for a prison sentence are the product of 1.7 and the direct property losses observed in NIBRS.

Table 4.3. Price Estimates for Aggregated Part One Crimes, Propensity Score Models

Crime Type	Crimes with Direct and Indirect Costs					
	Mean	90%	75%	Median	25%	10%
Arson (N=5,423 )	\$16,979	\$3,621	\$1,096	\$850	\$323	\$170
Extortion/ Blackmail (220)	3,498	7,700	1,700	170	0	0
Burglary/ B&E (190,842)	4,444	5,279	2,210	782	222	12
Motor Vehicle Theft (135,685)	15,175	39,100	17,000	6,800	2,550	41
Counterfeiting/ Forgery (19,338)	8,208	1,870	459	34	0	0
Swindle (29,451)	4,389	7,990	1,771	170	2	0
Credit Card (21,384)	973	1,820	617	37	0	0

<sup>18</sup> In most of the published literature, the ratio of direct to indirect losses is about 1 to 1. Since a lower ratio of indirect to direct losses results in a smaller additional indirect component in the NIBRS data, this assumption appears to be relatively conservative.

Impersonation (19,817)	955	1,159	5	2	0	0
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Source: Each of the estimates is derived from the study cited. All estimates are presented in 2008 dollars (Cohen (1988) is in 1985 dollars, Miller et al. (1996) are in 1993 dollars.

Table 4.4 clearly shows the substantial skew that exists in the data. For two categories, the mean award is greater than 90 percent of awards, demonstrating that a very small number of cases have extreme awards. The mean award exceeds the seventy-fifth percentile awards in every category but motor vehicle theft. The median award is an order of magnitude smaller than the mean for most cases. It is also important to note that many cases result in \$0 in harm to the victim.

#### Estimating Losses in the Least Serious Property Crimes

The losses reported in table 4.4 are the mean and median losses from the NIBRS data only, on less serious crimes. The assumption associated with these estimates is that there is no indirect loss from these crimes (and no direct losses other than property loss). To the extent that there are indirect losses, this will tend to underestimate the losses from these types of crime.

Table 4.4. Estimates of Harms for Crimes with Property Losses Only, Plus Indirect Costs

Crime Type	Crime Only / Propensity Model / All Cases	
	Mean	Median
Pocket-Picking (N=4,235)	\$408	\$90
Purse-Snatching (5,772)	447	72
Shoplifting (1,963)	459	60
Theft from Building (125,141)	6,393	165
Theft from Coin-Operated Machine (884)	532	140
Theft of Motor Vehicle Parts (274,937)	408	210
Theft from Motor Vehicle (274,937)	990	100
All Other Larceny (367,978)	2,048	150

Welfare Fraud (95)	461	47
Wire Fraud (2,003)	1,930	275
Embezzlement (2,210)	9,781	1,633
Stolen Property Offenses (7,155)	3,341	500
Destruction/ Vandalism (460,629)	759	170

Source: Analysis of data from the RAND Institute of Civil Justice, 12,918 cases from 1985-1999 and 2000 NIBRS data. Frequency weights are applied to all analyses. All dollars are calculated as 2000 Dollars.

### Testing the Generalizability of the NIBRS Data

The results in this section depend on the assumption that the NIBRS data that were matched to the RAND data are equivalent to the RAND data that do not. The challenge in testing this assumption is that most of the candidate variables for comparison are different by construction—those cases that do not match RAND are different from those that did, and the lack of a difference is evidence of this difference. However, there is an opportunity to test the differences by comparing property loss data in NIBRS cases that did and did not match. Two types of crime are particularly amenable to this analysis: robberies and burglaries.

Independent samples t-tests can compare the expected award for cases where the economic losses from a crime can be a priori determined to be property only and where estimates are available from both sources. A finding of no significant differences supports the approach here. There are a total of 256,000 burglaries for which a property loss is observable in NIBRS. The mean loss is \$2,613 for those that match to the RAND data, and \$1,587 for those that do not match, however the difference is not statistically significant ( $p=0.33$ ). Property loss is observable in about 40,000 cases. The mean loss in

those cases that are observable in both datasets is \$1,510 and the mean loss in those cases observable only in NIBRS is \$1,221, which is statistically significant at  $p=0.09$ .

### Study Limitations

This present study has several advantages of prior work in this field, and these are described in the next chapter. There are, however, several limitations to the present work. The limitations mainly fall across two issues: the representativeness of the data used in the analysis and the theoretical grounds for using jury awards to estimate case outcomes.

#### Are the Results Representative of all Crimes?

First, with respect to empirical matters of representativeness, it cannot be demonstrated that the present data are—or are not—representative of the typical criminal victim. Data used in this study rely on civil court data, which implicitly assumes that cases that make it to civil court are similar to all crimes. In the vast majority of serious crimes, there is no law suit. For a case to proceed to a civil trial there must be a defendant—either the perpetrator or a negligent third party—that has sufficient resources to incentivize a lawsuit. Even in these instances, only a fraction of cases make it all the way to a jury trial. Thus, it is reasonable to question whether these cases, that are unlikely to be representative of all cases in terms of the financial attributes of the defendant, are representative of the harms suffered by the average victim. On the other hand, it may also be the case that the financial attributes of the defendant (or third-party) are orthogonal to the social cost of the victimization (conditional on injury and other observable characteristics), and to whether the offender is caught and prosecuted.

There are three specific reasons why the data may not be representative. First, it may be that the types of offenders and offenses observed in the jury data are not representative of all crime victims. To the extent possible, this study has relied on the variation in NIBRS to overcome this issue. The present study matches cross-combination of attributes in more than 70 percent of cases between the RAND data and the NIBRS data. This approach of matching micro-level data to micro-level data has real advantages over prior work that has used population level prevalence as the means of generating weights. Nevertheless, it is certainly possible that the 30 percent of cases that could not be matched would lead to different results had they been included.

Unfortunately, there is no obvious way to broadly test how the matched and unmatched NIBRS data differ. Clearly, the two groups will be different on the main attributes of interest, since a lack of overlap on these attributes this is the reason why the two data sets could not be matched. Since the two data sets cannot be matched, there is no way to compare mean differences in any measure of victim losses as there is no damage in NIBRS to compare to awarded or claimed losses in NIBRS. The one available data point for comparison – comparing observed property losses in cases that do and do not match to RAND data—show small and significant differences in robbery and larger, but non-significant differences in burglary. This suggests that the approach taken here may tend to overestimate losses, perhaps by about 20%.

The second empirical issue is whether the cases in RAND are more (or less) serious within each crime strata. That is, it is possible that two observations with the same age, gender and injury may well have unobservable attributes that clearly distinguish them. The criticism is that the cases that lead to a civil verdict are likely to be

more serious than those that do not. The way to overcome this limitation is to include more measures of victim and case attributes. This study does make some advances in this area, as the number of age categories is increased from three to five, and the number of injuries from five to nine. Nevertheless, the study relied on broad injury categories due to limitations in the NIBRS data.

The main threat from this issue is that there may be differences within crime categories. That is, it may be that the average rape that matches to RAND is different from the average rape that does not. The property loss comparison is instructive, but more so for those cases where property loss and indirect losses have relatively equal effect on the total award. For cases, like rape and homicide, where the indirect losses would be expected to dominate, this comparison is less instructive. Table 4.5 displays the percentage of NIBRS cases that match to RAND and the percentage that do not (for those crimes that relied wholly or in part on RAND award data to generate the estimate. The main finding of this table is that those crimes that have the largest indirect to direct ratio are least well matched.

Table 4.5. Percentage of Cases Matched

Crime Type	Percentage of NIBRS cases that match a RAND record		
	Match	No Match	Percentage Matching
Murder/Non-negligent Manslaughter	818	1,656	33.1%
All Rape	21,113	29,479	41.7
Forcible Rape	11,486	10,241	52.9
Forcible Sodomy	1,456	2,757	34.6
Sex. Assault w/Object	1,100	1,495	42.4
Forcible Fondling	7,071	14,986	32.1

Robbery	31,524	14,244	68.9
Assault	418,256	204,828	67.1
Aggravated Assault	76,114	44,828	62.9
Simple Assault	342,201	159,903	68.2
Burglary	229,486	65,421	78.0
Motor Vehicle Theft	142,111	36,803	72.1
Counterfeiting/Forgery	19,338	6,766	79.4
Swindle	29,451	9,882	74.9
Credit Card/ATM	21,384	5,718	78.9
Impersonation	19,817	5,311	78.9
Total	1,083,282	428,914	71.6

Source: NIBRS data with harms interpolated from RAND data.

In general, Table 4.5 suggests that other than homicide and rape, the percentage of NIBRS cases that match to RAND is close to the overall match rate of 70 percent. The lack of a match on homicide and rape suggests much greater caution should be exercised in using these numbers than the other numbers in this study. Clearly, there is a greater likelihood given this lower match rate that the losses from these crimes are not well modeled using this approach. For instance, the NIBRS data suggest that few rape victims have a serious injury (slightly more than 70 percent record no injury, and the overwhelming majority of the rest report minor injuries) and thus these records will match to jury awards for no injury or minor injuries. However, it is reasonable to assume that the indirect harms from rape with no injury are far larger than the indirect harms from other crimes with no injury. Chapter five discusses the implication for using these numbers given that limitation.

This study did attempt to control for differences between cases within the RAND data where there was (or was not) an obvious indicator of an underlying criminal event. Prior studies had limited sample sizes due to a focus on only those cases where a criminal



event could clearly be distinguished, and that was generally limited to sexual assault and assault. The limitations of this are clear—that the studies only result in price estimates for a few crimes, and also that within the included categories cases could be missed if a crime occurred, but the claimant sought relief based on a claim of something in addition to the intentional tort (such as negligence in the case of a motel owner that did provide secure rooms thus leading to a sexual assault). This study uses a propensity score model to re-weight observations to account for potential differences between cases. However, it is certainly possible that there remains unobserved heterogeneity related to damage awards.

In general, the propensity score generalizes the relationship between losses and injuries to more cases in RAND to allow more cases in RAND to be used in the analysis. Subsequently, this supports the interpolation of RAND data on to more cases in NIBRS. The assumption, which cannot be tested, is that the generalizability of the entire study is improved if a larger proportion of NIBRS data are used in the analysis. However, if the NIBRS cases that match to RAND are different from the NIBRS cases that do not (even when the number of NIBRS cases is expanded) than this adjustment does not improve generalizability. As noted, this can be tested to some extent by the comparison of property losses in burglary and robbery for cases in NIBRS that do, and do not, match to RAND. If, however, the cases that match to RAND are similar in property losses within crime categories, but different in other attributes (such as indirect damages) than this correction will not improve generalizability. As noted above, the types of cases that are least likely to match to NIBRS (homicide and rape) appear to be most dissimilar to the RAND cases.

### Limitations of the RAND Data

The RAND data have several other limitations that should be noted. One key limitation of the RAND data is that it does not include some key variables, such as race. African-Americans are over-represented as arrestees and as victims relative to the proportion of African-Americans in the population. However, African-Americans may be under-represented in the RAND data and thus the estimated harms may or may not be representative of this population.

As noted, the RAND data does not reflect data from all fifty states. While the data includes jurisdictions that contain about 40 percent of the US population, the distribution of awards may be higher in more urban areas than non-urban areas. The awards may also be skewed by the relative lack of observations in the Deep South (although Texas and Missouri are represented). To the extent that awards are different in the jurisdictions included in the RAND data than in the rest of the United States, the results will not be representative. In addition, because the data are not drawn from a random sample of US cities, other demographic factors, such as age and gender, may not be representative of all victims of crime. Since these variables are used to link data across the two datasets, it is difficult to observe the effects of the limited geographic coverage. However, since a single crime is likely to include events from a variety of different city sizes, some investigation of how awards on the interpolated data set vary by population size may show the extent to which different areas are associated with differential award patterns.

To test for these differences, I ran t-tests comparing the average award by age and gender of the victim. Differences are observed in awards as a function of age and gender. On average, women receive less (\$157,000) than men (\$183,590) and the difference is

highly significant ( $p < 0.001$ ). A bivariate regression finds that each additional year of age is associated with about \$700 dollars in harms ( $p < 0.001$ ). To account for the possibility that awards are non-linear in age (that older victims have smaller awards), a squared term is added to the model. This yields a much larger age effect, where each additional year of age is associated with \$2,000 in expected average award. However, it is difficult to interpret these findings since variation in awards by age is expected (due to differences in the types of harm from crime by age). Differences in gender are also difficult to interpret since it is possible that women may experience less serious injuries on average.

Differences in harms as a function of the population of a jurisdiction are more indicative of a lack of representativeness in the RAND data. In expectation, for example, the victim of a robbery in a large urban area may have larger losses due to the higher cost of living in urban areas, and thus more expensive property to steal. However, a t-test comparing awards in jurisdictions with populations over 100,000 (\$230,000) to jurisdictions under 100,000 in population ((\$127,000) shows significant differences in average award ( $p < 0.001$ ).

Combining these results in a linear regression (with a log link to account for award skew) suggests that population size explains more of the award than would be expected as a function of differences in cost of living. When logged award is regressed on population size there is an additional \$31,000 in average award for every additional 100,000 in population ( $p < 0.001$ ). When other independent variables are added to the model (age of victim (and age squared), victim gender, type of injury) the effect of population persists. In fact, population size does not appear to co-vary with the other independent variables, as an additional 100,000 in population is associated with a

\$30,772 increase in award on average. Finally, it should be noted that most of the gender effect on average award appears to be due to co-variation with injury and population size. While women average \$26,000 less in average award, controlling for injury and other factors reduces that difference to slightly less than \$10,000 ( $p < 0.001$ ).

### Representativeness of NIBRS

The choice to use NIBRS data instead of NCVS data means that the criminal data are themselves non-representative. I believe that the advantages of the NIBRS data—a more precise estimate of property loss, improved sampling of the most serious cases, and inclusion of important populations excluded in NCVS—outweighed the cost in terms of lost representation. Nevertheless, this may have introduced another source of error.

Three other limitations of the study are addressed below. First, it should be noted that cases that result in a civil award may not yield an unbiased estimate of all cases with criminal liability. While the most obvious source of bias—that weaker claims are more likely to be settled out of court—suggests that civil jury awards overestimate the true harms to victims of crimes, it is more likely that the risk tolerance of the parties to a claim rather than the size or strength of the case predict whether a case proceeds to a jury trial, and thus the direction of the bias is ambiguous. It may also be true that cases settled by a judge rather than a jury may tend to result in somewhat smaller verdicts. However, given that the RAND data includes only cases resulting in a jury trial, this bias cannot be tested empirically.

## Social Costs

With respect to theoretical issues, there remains a criticism of the jury award approach on the grounds that the aggregated costs to society will yield a different value than would a social cost calculation. Clearly, the jury award method does not yield an ex ante estimate of willingness to pay, and thus is likely to result in different estimates of social cost. The approach is better than one that relies on cost of illness, for several reasons. First, jury awards can be argued to be an objective, third party evaluation of the sum of the harms suffered by a particular victim. Cost of illness, by contrast, is merely a schedule of costs associated with an average injury that resembles the victim in the case under study. In addition, by relying solely on jury awards, and not a blended cost of illness and jury award estimate, far fewer assumptions are required, which is helpful from a theoretical perspective, since it limits the number of ways in which estimates are generated.

This paper does not argue that the final prices approximate the costs of illness that would have been generated from an ex ante study. However, in practice it is difficult to see how a study could be performed that captures the ex ante price of discrete crimes. The research literature includes several contingent valuation studies that develop estimates of the social cost of broad categories of crime. These authors correctly note that from a policymaking perspective, social cost is the right unit of analysis. For broad policy questions, such as the appropriate level of incarceration or appropriate number of police officers, a robust measure of social cost will best inform debate.

However, at the programmatic level, those estimates may have less value. A question about the social cost of gun violence is likely to elicit a more informed estimate

than the cost of drug-related offending. And for particular programs, broad measures of social cost are likely unattainable. For instance, to understand whether a program designed to rehabilitate prisoners returning to a handful of neighborhoods in a single city is cost-effective, it is difficult to specify a contingent valuation question that directly informs that analysis. The same issues relate to a specialized court for substance abusing defendants or a sanctioning scheme as an alternative to incarceration. For these types of programs, precise estimates of the benefits in the form of reductions in victim harm are an asset to evaluation.

### Conclusion

This study develops estimates of the distribution of prices for criminal victimization. The mean estimates are higher than much of the published literature, while the medians are in line with the lowest estimates due to the skew in the price distribution. Table 4.5 describes the comparison of my estimated means to extant estimates and Table 4.6 describes the comparison of my estimated medians to extant estimates.

Table 4.5. Comparison of New Price Estimates to Extant Estimates (mean)

Crime Type	Crimes with Direct and Indirect Costs					
	Jury Award Literature				C.V.	Roman
	Cohen 1988	Miller, Cohen 1996	Rajkumar & French 1997	French & McCollister 2008	Cohen, et al. 2004	Propensity Model
Murder/Non-negligent Manslaughter	\$2,842,242	\$4,178,096	\$7,772,054	\$9,322,054	\$10,825,622	\$1,564,186
Rape	102,218	129,629	74,271	219,566	264,502	156,295
Robbery	25,200	19,370	32,040	46,484	258,922	299,573
Assault	24,067	22,350	32,040	122,716	78,123	145,103
Burglary	2,745	2,235	1,909	4,362	27,901	5,556
Motor Vehicle Theft	6,257	5,960	1,666	9,141	5,412	18,973
Theft	362	551	1,067	1,475	819	2,655

Source: Each of the estimates is derived from the study cited. All estimates are presented in 2008 dollars (Cohen (1988) are estimated in 1985 dollars, Miller et al. (1996) are estimated in 1993 dollars). The rape data in Roman are a weighted average of forcible rape, forcible sodomy and sexual assault with an object. The assault data from the Roman study includes a weighted average of aggravated assault and simple assault, assault data from other studies may include aggravated or attempted assaults. The French and McCollister study presents estimates for aggravated assault only. The larceny/theft value in the Roman study is the weighted average mean of NIBRS categories: Theft From Building, Theft From Coin-Operated Machine or Device, Theft From Motor Vehicle, Theft of Motor Vehicle Parts/Accessories, All Other Larceny.

Table 4.6. Comparison of New Price Estimates to Extant Estimates (median)

Crime Type	Crimes with Direct and Indirect Costs					
	Jury Award Literature			C.V.	Roman	
	Cohen 1988	Miller, Cohen 1996	Rajkumar & French 1997	French & McCollister 2008	Cohen, et al. 2004	Propensity Model
Murder/Non-negligent Manslaughter	\$2,842,242	\$4,178,096	\$7,772,054	\$9,322,054	\$10,825,622	\$1,380,246
Rape	102,218	129,629	74,271	219,566	264,502	18,908
Robbery	25,200	19,370	32,040	46,484	258,922	88,915
Assault	24,067	22,350	32,040	122,716	78,123	155,270
Burglary	2,745	2,235	1,909	4,362	27,901	814
Motor Vehicle Theft	6,257	5,960	1,666	9,141	5,412	6,174
Theft	362	551	1,067	1,475	819	192

Source: Each of the estimates is derived from the study cited. All estimates are presented in 2008 dollars (Cohen (1988) are estimated in 1985 dollars, Miller et al. (1996) are estimated in 1993 dollars). The rape data in Roman are a weighted average of forcible rape, forcible sodomy and sexual assault with an object. The assault data from the Roman study includes a weighted average of aggravated assault and simple assault, assault data from other studies may include aggravated or attempted assaults. The French and McCollister study presents estimates for aggravated assault only. The larceny/theft value in the Roman study is the weighted average mean of NIBRS categories: Theft From Building, Theft From Coin-Operated Machine or Device, Theft From Motor Vehicle, Theft of Motor Vehicle Parts/Accessories, All Other Larceny.

Some of the differences in the present estimate are due to definitional issues. For instance, an assault in other studies may refer only to a simple assault, whereas in the present study, the assault price is an average weighted by the prevalence of both simple and aggravated assault. With respect to theft, this is likely the cause of the difference. This study includes many thefts from businesses, which includes relatively larger harms than other types of theft.

This study produces a lower homicide estimate than appears in prior research. Most prior studies rely on an estimate of the statistical value of human life, rather than a direct estimate, which is how the homicide is estimated in this study. As discussed in Chapter 3,



homicide is a particularly difficult crime type to estimate. Prior studies have used the statistical value of a human life and either applied that estimate to all homicides, or used the estimate as a weight to adjust for risk of death associated with other crimes (generally because homicide data were unavailable). However, that strategy assumes no variation in the costs associated with homicide which may (or may not) be true from a social welfare perspective. While the approach used in this study does identify substantial variation in the costs of homicide, a substantial weakness of the approach with respect to homicide remains. That is, unlike other categories of crime, the victim is not being compensated by the jury, and thus the estimate is not an ex post proxy for willingness to accept. Thus, for homicide, I replace the number generated in this estimate for the standard approach to valuing homicide – using the value of a statistical life from the extant literature.

The other estimate generated here that is inconsistent with prior approaches is the estimate for the price of rape. While the mean value for rape is in line with prior estimates, rape has a lower price in this study than robbery, which reverses the usual ordering from other studies. In terms of injury, the profiles for rape and robbery look very similar, as about 70 percent of rape victims experience no injury or an apparently minor injury. Severe lacerations, broken bones, and other internal injuries are reported more commonly in robbery cases than in rape cases, and as shown in Chapter 4, these kinds of injuries are associated with much larger awards. This difference in injuries leads to the higher price for robbery. It should be noted that the difference between robbery and rape exists in both the means reported above and the medians (about \$100,000 for robberies compared to about \$20,000 in rape cases).

As discussed in the limitations section, less than half of all NIBRS observations for rape matched to a RAND subgroup. Thus, it appears that the jury approach, using the current dataset, may not be appropriate to estimate the price of rape. However, even if the data had matched at a higher rate, it is still likely that the estimate that was generated would be unsatisfactory. That is, the main limitation to using the jury compensation method is that much of the harm of rape is psychological. The empirical relationship between direct losses and indirect losses is much different for a rape victim than for victims of other crimes. Overall, direct and indirect awards are relatively equal. In the small number of cases that could be affirmatively coded as a rape, the mean jury award in the RAND data is about \$1.3M, which is almost entirely an award for indirect damages. Thus, I would propose using extant estimates of jury awards for rape from studies that gathered relatively large numbers of rape cases (see Cohen and Miller 1999). However, the problems with these past awards are serious, and further investigation of this question is appropriate. In sum, Table 4.7 presents the final estimates for awards in all categories.

Table 4.7. Price Estimates for Crimes

Crime Type	Crimes with Direct and Indirect Costs					
	Mean	90%	75%	Median	25%	10%
Murder	\$6,900,000	--	--	--	--	--
Rape	272,121	--	--	--	--	--
Robbery	279,085	\$605,225	\$334,515	\$88,915	\$68,326	\$18,908
Assault	134,770	334,515	155,270	66,644	13,285	611
Agg. Assault	283,794	569,701	294,335	89,815	27,956	1,900
Simple Assault	101,623	238,396	87,000	59,083	35,239	600
Arson	\$16,979	\$3,621	\$1,096	\$850	\$323	\$170
Extortion/ Blackmail	3,498	7,700	1,700	170	0	0
Burglary/ B&E	4,444	5,279	2,210	782	222	12
Motor Vehicle Theft	15,175	39,100	17,000	6,800	2,550	41
Counterfeiting/ Forgery	8,208	1,870	459	34	0	0
Swindle	4,389	7,990	1,771	170	2	0
Credit Card	973	1,820	617	37	0	0
Impersonation	955	1,159	5	2	0	0

Source: Each of the estimates is derived from the study cited. All estimates are presented in 2008 dollars (Cohen (1988) is in 1985 dollars, Miller et al. (1996) are in 1993 dollars.

## Chapter Five: Implications for Research and Policy

There is a substantial body of literature that has emerged over the last two decades that attempts to value the price of criminal victimization. The price estimates developed in this paper and in the extant literature have three important applications. First, they inform cost-benefit analysis of new policies and programs by creating a standardized metric with which to evaluate outcomes. Second, because the costs of crime vary between crimes, these estimates can inform the optimal allocation of criminal justice resources in crime control, where criminal justice system stakeholders must choose among a range of anti-crime strategies with heterogeneous impact on types of crime. Third, they provide new information that can be used to evaluate the effectiveness of crime control efforts over time.

While this paper expands the number of crime categories with an estimate from less than a dozen to more than 30, it is fair to ask whether the estimates are credible. While it is difficult to argue that price estimates are not important in developing evidence-based policies, it is fair to question whether these price estimates, or any price estimates, can credibly inform these discussions. Other approaches, such as contingent valuation, clearly result in estimates that are closer to their theoretical economic foundations.

However, it is far from certain that contingent valuation studies can be developed that create prices for particular crimes, which is crucial to evaluating all but the broadest of social policies. In order to evaluate programs with discrete outcomes, such as desistance from domestic violence, or drug crimes, or serious person crimes, prices must

be available to describe the benefits of crime reductions. Since the risks of victimization are so small for serious crimes, a reasonable person is unlikely to be able to make an informed judgment about their ex ante willingness to pay. Suppose it is shown that using DNA to solve burglaries doubles the number of burglary arrests. Suppose that the program could be taken to scale and applied everywhere in the United States. As a result, the real (average) risk of having a household burglarized is reduced from about 2 percent to about 1 percent for any American household. Is it plausible that a reasonable person could make an informed statement about their willingness to pay for that reduction?

If contingent valuation techniques cannot generate these estimates, the next best alternative appears to be a jury compensation approach. Although the theoretical foundation is shakier—the method only approximates ex ante willingness to accept—the technique has been shown to produce estimates that scholars have been willing to use in a wide variety of law and economics research. As noted earlier, the most cited study using this approach (Cohen, Miller and Wiersma, 1996) has been cited more often than the most famous study using those estimates (Levitt, 1996).

Precedent, however, is probably a necessary, but not sufficient, justification for using these estimates. Since there is no obvious way to externally validate these estimates, perhaps the best strategy for evaluating their strength is to consider each of the assumptions that underlay the estimates. Before doing so, it is worthwhile to separate out those estimates that have the most serious empirical problems—homicide and rape—and consider how well the other estimates withstand close inspection.

For the least serious property crimes that makes use of only NIBRS data, only two assumptions are required. First, the study assumes that individuals accurately report

losses to police. With respect to the reporting accuracy in NIBRS, I can find no prior research that validates the accuracy of NIBRS property loss data. However, there are some reasons to believe it is more accurate than survey data. First, the report tends to occur close to the time of the victimization, and thus there should be fewer re-call problems. Second, in many cases, the account given to the police is likely to be validated, which would occur if losses are insured and a claim is filed. Victims may also have incentive to respond truthfully, since accuracy of claims may assist them in getting items back if they are recovered by the police, while misstatements might reduce that chance. Second, the study assumes that the ten states in NIBRS can be generalized to the entire country. Since NIBRS only includes ten states that exclude most major metropolitan areas and likely have lower costs of living, the estimates are probably not perfectly representative of all victim losses. However, to the extent that there is bias, it is probably in the direction of under-estimating losses, and thus these data produce a conservative estimate.

There are several additional assumptions that are required for the estimates of the more serious property crimes where price estimates depend on both NIBRS and RAND data. First, it is assumed that juries accurately estimate indirect losses as a function of direct losses (the serious person crimes requires an additional assumption that they accurately estimate claims, as is discussed below). With respect to the accuracy of the civil jury, the research in Chapter 2 overwhelmingly suggests that on average, juries do about as well as experts and judges in evaluating claims. Second, the study assumes that the awards from RAND can be generalized to all crimes. This assumption rests on the principle that juries evaluate claims based on victim harms, and not other extra-legal case

attributes. The extant literature in Chapter 3 supports this claim. In addition, the comparison of property losses of those NIBRS cases that did and did not match to RAND suggests that if there is bias, it is 20 percent or less. Finally, this approach assumes that there should be an indirect loss associated with crimes that do not result in physical harm. Again, there is no way to test this assumption, but the assumption that people suffer harm from the loss of property in addition to the value of that property appears to be a small one.

Two assumptions are required for the estimation of serious person crimes. The first assumption is that juries are accurate estimators of losses in civil cases and that awards reflect the harms suffered by victims. Again, prior research in this area suggests that juries do a relatively good job of evaluating claims, and the civil jury system has remained a relatively stable pillar of the justice system for decades. Second, the study assumes that the awards to compensate for harms from an injury will not vary if the harm results from a crime as opposed to a non-criminal incident. This is a relatively strong assumption that does not hold, as reflected by the real differences in average awards between the two types of cases. However, the propensity score model appears to a good job balancing cases on observable attributes related to assignment of a case as a crime or not crime case. Finally, the study assumes that the omission of important data about the crime (such as victim race) and the lack of geographic overlap between the two datasets will not bias results. A test of the effect of population size on awards suggests that awards increase relatively quickly as city size increases. However, it cannot be determined whether that is a natural phenomenon reflecting differences in cost of living, severity of

criminal incidents and risk tolerance, or whether the study is overestimating harms in large cities (or underestimating harms in rural areas).

### Rape and Homicide

Ultimately, the study did not generate estimates for rape and homicide. AS discussed in chapter 4, the theoretical grounds for estimating homicide prices seem too limited to produce a credible estimate. Since the homicide victim cannot be made whole in any way, the study cannot be said to approximate the ex ante willingness to accept price of the victims. While it is unclear whether the jury verdict approach can plausibly be used to estimate homicide prices, the estimates generated here do show substantial variation in the price of homicide. This suggests additional research in the field, using contingent valuation approaches, to estimate confidence bounds on homicide prices. This would answer the question of whether social costs also vary by the type of victim, or whether society-level outcomes are unaffected by whom the victim is, just whether there was a homicide victim.

With respect to rape, this study also suggests that using jury awards to indirectly estimate rape prices may also not be an effective strategy. The main limitation is that the variables used to interpolate the jury data onto the crime data—victim injuries—do not adequately capture the extent of the harm suffered by victims of rape. The alternative would be to use jury data alone in cases of rape to estimate prices. The data used in this study did not contain enough observations to allow for this, but other research has successfully gathered sufficient observations (see Cohen and Miller, 1999 but note that there are unresolved statistical problems in this study).



Thus, in conclusion, the estimates generated here appear to improve on existing price data. Though the limitations of this approach are real, these estimates rely on far fewer assumptions than past research. The assumptions that are subject to criticism are fewer in number than in past research, and the ones that remain have been heavily vetted in the scholarly literature. None of the assumptions alone appear to be so strong as to call into question the entire endeavor. Moreover, the weight of the assumptions does not appear to fall so heavily toward over- or under-estimation that the entire approach should be abandoned. Thus, the weight of the evidence supports the use of price estimates derived from civil jury awards.

#### Implications for Future Research

While this study makes several improvements on past studies using civil jury data, there is room for substantial improvement. In particular, these estimates could be improved if future studies of civil jury behavior explicitly coded cases in terms of whether there were criminal events that lead to the victim being harmed, and what type of event occurred. As noted, it is very likely that there are a substantial number of criminal incidents in the RAND data that cannot be affirmatively coded as a crime that did result from a criminal incident. In addition, coding additional information about both the victim (race) and the defendant (was the defendant the alleged perpetrator; what was the victim's age, race, association with the victim, etc.,) would improve future studies. Finally, additional information about the timing and location of the criminal event (rather than of the timing and location of the civil proceeding) would improve the study. More information about the type of crime in particular would substantially improve this line of research.

### Implications of the Research

The implications for research are relatively clear, but the implications for policy and practice are not as obvious. The clearest application of these data is for use in cost-benefit analyses of criminal justice policies and programs. Typically, the main effect of a criminal justice policy will be to change the rate of offending within the target population. Although there may be spillover effects, the main effect is to change the number and/or type of the distribution of criminal victimizations. Thus, the ‘benefits’ of the policy can be measured as that change, and those effects can be translated into a consistent metric (dollars) using the price data from this study.

The strength of the approach taken here is that it develops estimates of the price of crime that can be conditional on a set of observable attributes. Rather than simply applying a single point estimate to a broad crime type, the whole distribution can be used to relate prices of crime to effects of a policy or program. Where a policy or program targets only a sub-group of all victims, an analogous sample of the NIBRS population, with prices, can be selected from these data. Thus, much more precise costs and benefits of policies and programs can be estimated.

The implications for policy and practice are less clear. Rigorous cost-benefit analysis is a necessary but not sufficient condition for improved decision-making. That is, careful cost-benefit analysis will provide policymakers with better information than exists without CBA. But unless the information is coupled both with a willingness to make decisions based on that evidence, and with a finding that is actionable, there is scant reason to believe that better policies will result.

The largest barrier to effective policymaking resulting from careful cost-benefit studies is that the information is not actionable. That is, CBA enumerates the winners and losers from a policy or program, and thus whether the policy satisfies some normative standard, such as Pareto optimality or the Kaldor-Hicks condition. However, if the winners cannot somehow compensate the losers, the policy is unlikely to be implemented. Given that most governments operate with very constrained resources, it is reasonable to presume that most budgeting is a zero-sum game. In order for a new policy or program to be funded, those funds must be taken from some other area. This works quite well if the policy benefits the agency that funds the project. If the agency funding the project does not benefit enough to cover the losses, as is more commonly the case, the policy faces large barriers to implementation.

Consider the case of a proposed diversion program for drug-involved offenders. The program is designed to identify drug dependent arrestees and treat them in lieu of incarceration. The diversion program provides judicial oversight and supervision to those offenders while they complete a course of drug treatment, and successful participants have their charges dismissed. The program is significantly more expensive than business as usual case processing and the courts pay for the program. The main beneficiary of the program is the correctional system, as successful participants will not have to use prison resources since they will not be incarcerated. In addition, they may commit fewer crimes thus saving private citizens money (since fewer will be victimized) and saving the criminal justice system money as well, since there will be fewer new crimes to investigate and prosecute and fewer offenders to incarcerate.

It seems natural for the corrections system (the winner from a cost-benefit perspective) to compensate the court system (the loser from the cost-benefit perspective). The court incurs new costs to set up and run the diversion program, and the corrections system saves funds as a result. However, in reality, most of the cost savings from the program goes outside the criminal justice system. Since the demand for prison beds is far greater than the supply, prison beds tend to be filled. If one were to imagine a queue of offenders who were legally eligible for a prison bed based on the crimes they had committed (or violations of community supervision), that queue would likely be several times as big as the number of available. Even a large-scale diversion program would be unlikely to remove enough eligible offenders to actually reduce the number of prison beds used.

A standard approach to budgeting is to fund only those programs that have cost offsets. The approach, known as pay as you go in the United States federal government is widely used de facto or de jure to guide governmental decision making around the world. In the case of the diversion program described above, the winner (prisons) cannot reasonably compensate the courts for the cost of the diversion program, since the diversion program does not actually free resources for use elsewhere. Such a situation is more the rule than the exception with respect to new policies and programs in the criminal justice system.

It is important to note that an essential ingredient is missing from this policy cookbook. The real winner from this hypothetical diversion program—the public—is not included in this calculus. Generally, budget decisions are made within systems, and external concerns are excluded from decision-making. Thus, the most efficient solution to

this problem, asking the public to pay more to support a program for which they are the primary beneficiary, is not considered.

In order for CBA results to be both necessary and sufficient for decision-making, the results must include some accounting for how resources can be more efficiently employed. If the public is not included in the budget calculus for the diversion program, it seems unlikely that such a program will ever include more than a small number of participants. In fact, this is exactly what has happened in the United States with respect to drug courts. Drug courts are diversion programs for drug-involved arrestees that function much like the hypothetical diversion program described above. Although the first courts appeared in the United States, they have become increasingly prevalent around the world, particularly in the United Kingdom.

Most research suggests that drug courts in the U.S. are quite cost-beneficial. A substantial body of research estimates that there are between \$2.25 and \$2.75 in benefits for each dollar of new costs (Aos, et al., 2002; Bhati, Roman and Chalfin, 2008). The costs and benefits of U.S. drug courts are distributed in the same way as described above, where the court systems pays a substantial additional amount over business as usual case processing (on average \$4,000 to \$5,000 more per case). Much of the benefit of drug courts is in the form of reductions in victimization of private citizens, perhaps as much as 70% of all benefits (Bhati et al., 2008).

In the United States, start up funds for new drug courts mainly come from the federal government, and this funding lasts only two or three years. To sustain drug court operations, local funding is used, and this rarely includes new taxes. Thus, although the public wins they are not asked to offset court costs. And, because prison see only a minor

benefit, in the zero-sum game of criminal justice system budgeting there is no source to off-set the courts cost. And as a result, the programs would not be expected to grow. By our estimates, fewer than 5% of all individuals who arrested each year in the United States and who are at risk of drug abuse or dependence enter a drug court (Bhati, et al., 2008). There is little reason to believe this will change.

This phenomenon is not unique to drug courts. The High Scope/Perry Pre-school experiment was a random controlled study of a preschool program in Ypsilanti, Michigan beginning in the fall of 1962. Subjects were followed from age 3 to age 41 and subsequent evaluations found improvements in “children’s readiness for school and... subsequent educational success, economic success in early adulthood, and reduced number of criminal arrests” (Schweinhart, 2003:2). The program is often credited with facilitating the development of Head Start, an initiative by the U.S. federal government to promote early education for three and four year-olds. Despite the enormous interest in Head Start, early education remains relatively uncommon. Schweinhart, the author of many of the Perry preschool studies wrote in 2003 (more than 40 years after Perry began) that, “[t]he continuing challenge is to take such programs to scale so that all preschool programs for young children living in poverty” are exposed to the positive effects of the program (Schweinhart, 2002:2).

One reason that the Perry Preschool model was not adapted more widely was the same diffusion of benefits seen in the drug court model. That is, Schweinhart estimates that Perry Program yielded more than seven dollars in benefits for every dollar in cost. While the costs were paid by the schools system, most of the benefit went to private citizens. More than 65 percent of the benefits are from a reduction in the number of crime

committed against private citizens. The rest of the savings went to a variety of governmental agencies. Fourteen percent accrued to justice agencies, ten percent to the government at large in the form of new taxes, and three percent in welfare tax savings. Only seven percent of the benefit of Perry Preschool was returned to the educational system (mainly due to reductions in the need for special education services).

The important insight from the Perry Preschool evaluation is that the choice of the lens through which results are examined matters enormously. The usual approach to budgeting is to first identify offsets within the same budget. In the case of Perry Preschool, the costs of the program exceed the benefits within the educational system by more than two to one (and probably even more so since the benefits of reduced special education may occur over years). Thus, through that lens, Perry Preschool is not worth funding. Across all governmental agencies, the benefits exceed the costs by more than two to one, but the costs of moving funds from one agency to another may exceed the benefits. Only viewed through a lens of net social welfare is Perry Preschool a clear winner worth funding.

## Appendix A.1

### Recovering Missing Ratio's of Economic to Non-economic values

As shown in Table 2.1, total awards were available for 13,170 records in the RAND data. Complete records with both economic and non-economic damages initially were only available for 3,827 records. For the remaining 9,343 records, these missing values can be either imputed for all observations, or calculated from observed values for some and imputed for others. Cohen and Miller (1999) use a regression analysis where they regress total award on economic award to recover missing values for non-economic award. However, this assumes that the data are missing completely at random (MCAR) and are thus validly observed from other observations. Binary tests for differences suggest that the cases where no data are missing are different from the cases where data are missing across a wide range of indicators. An alternative would be to specify a model that predicts the ratio of non-economic to economic damages. Since the ultimate use of these data will be to create a ratio of non-economic to economic damages to weight economic costs in NIBRS, imputing such a ratio does not appear to contribute much new information to the analysis.

Thus, values for missing columns can be directly calculated for some records. In 269 records there are values for economic awards, but no corresponding values for non-economic awards. Where the economic award was equal to the total awards, the value for non-economic awards was calculated as zero. The 61 cases where the values did not match, the remainder of the difference between total award and economic award was allocated to the non-economic variable. Similarly, in 1,030 records there are values for



non-economic awards, but no corresponding values for economic awards. For the 855 cases where non-economic award was equal to the total awards, the value for non-economic awards was calculated as zero. The 173 cases where the values did not match, the remainder of the difference between total award and non-economic award was allocated to the economic variable.

Table A.I.

Dependent Variable Missing Values.						
Description	Total Award	Economic Award	Non-Economic Award	Unspecified	Percent Matching	Dropped
Complete Data	3,827	3,827	3,827	0	100%	0
Only Economic (no unspecified values)	269	269	<b>269</b>	0	77%	0
Only Non-Economic (no unspecified values)	1,030	<b>1,030</b>	1,030	0	83%	0
Economic and Unspecified	148	148	<b>111</b>	148	75%	37
Non-Economic and Unspecified	134	<b>95</b>	134	134	71%	39
Economic, Non-Economic, Unspecified	170	170	170	170	--	170
None	3	0	0	0	0%	3
Total Award = \$0	84	0	0	0	100%	0
Total	5,665	5,539	5,541	452	--	252
Only Unspecified	7,253	--	--	7,253	--	--

Note: All records include a non-missing value for total award (first row in Table A.I.). Values may be missing for Economic or Non-Economic award values, or the data may include a value where the type of award is 'Unspecified'. Values in bold can be calculated from other non-missing values according to the percentages in the 'Percentage Matching' column. Values for observations where Economic and Non-Economic awards are missing but non-missing values are present for unspecified categories are imputed in the next stage of the analysis.

For some records, data were not only missing for a damage category, but the record included values for an unspecified amount. In 148 records there are values for economic awards, there are values for unspecified awards but no corresponding values for non-economic awards. In 111 cases, the economic award was equal to the total award, and the value for non-economic awards was calculated as zero. The 37 cases where the values did not match were dropped. Similarly, in 134 records there are values

for non-economic awards and values for unspecified awards, but no corresponding values for economic awards. For the 95 cases where the non-economic award was equal to the total awards, the value for non-economic awards was calculated as zero. The 39 cases where the values did not match were dropped. Finally, in 87 cases, the total award was \$0, and this value was added to both the economic and non-economic variables.

Table A.2 reports the results of this analysis. Across all categories, the distribution of cases with economic and non-economic awards (as well as punitive awards) is about 30 percent lower than the average across all cases. This suggests that the cases where both types of damages are recoverable have smaller awards than cases where specific awards are not recoverable. To the extent that this persists through the remaining data cleaning steps (including the removal of outliers) and that it persists for cases with no or minimal injury (the cases that will be used in the calculation of non-economic damages in NIBRS, this will result in a conservative estimate of the losses in these cases.

Table A.2.

Distribution of Total Award, by economic and non-economic damages.

Category	Sub-Category	Total Award (adjusted)	Punitive Award	Economic Award	Non-Economic Award	Total (Econ+Non+Pun)
All Obs.		\$1,376,341	\$133,036	\$365,384	\$492,283	\$990,073
Crime						
	Crime	1,372,276	257,570	131,218	488,404	877,192
	No-Crime	1,376,643	123,765	382,815	492,572	999,152
Issue						
	Crime	1,372,276	257,570	131,218	488,404	877,192
	Malpractice	2,256,547	71,574	839,966	751,748	1,663,288
	Liability	1,150,014	94,952	294,542	459,736	849,230
	Other	1,356,582	473,041	180,285	569,920	1,223,246

Note: All records include a non-missing value for total award (n=12,918). The 'Other' category is mainly comprised of negligence cases. Values may be missing for Economic or Non-Economic award values, or the data may include a value where the type of award is 'Unspecified'. Thus, the component columns do not sum to the totals. However, missing data are recorded as '0' in the RAND data as are valid '0' observations. Total award is 'adjusted' to reflect 2003 dollars.

## Appendix A.2

### Selection into Civil Trial

The processing of civil cases results in only a small proportion of cases actually reaching trial (only 2-8 percent of all cases are ultimately adjudicated by a jury). This process is likely non-random (Ostrom, Rottman and Goerd, 1996; Danzon and Lillard, 1983; Vidmar, 1993). Generally, the extant literature asks whether the non-random selection of cases into the civil court system leads to cases with larger (or smaller) awards in the cases that are ultimately arbitrated by jury. For this paper, the question is slightly different. Suppose jury awards are, on average, greater than the expected award for a settlement. The critical question is whether those awards are greater because the average harm to victims is greater in jury cases, or is it due to differences resulting from the legal process that is unrelated to plaintiff harm? Past research generally reports that jury awards are higher than awards at settlement. However, if cases that are settled are biased downward from the true harm, than jury awards are a better source for the estimation of victim harms than settlement data. There is no empirical way to determine the extent of selection in this study given that there are no cases in the RAND data that are not adjudicated by jury. A review of the extant literature offers support for and against this hypothesis.

### Economic Theory

Economic research on decision-making in litigation is framed by the pioneering work of Landes (1971), Posner (1973) and Gould (1973) who develop theoretical rational choice frameworks to identify case attributes that predict pre-trial settlement. As noted, between

2 and 12 percent of all civil suits ultimately result in a jury-verdict (Ostrom, Rottman and Goerdt, 1996; Danzon and Lillard, 1983; Vidmar, 1993) and the plaintiff loses in about half of those cases. Thus, only about 1 in 25 civil cases results in a jury award.

Landes (1971) developed a theoretic model that identifies case attributes that predict the choice between settlement and trial in *criminal* cases. Both parties are utility maximizing subject to resource constraints. In the Landes model, the decision to settle or go to trial is a function of the probability of conviction at trial, the severity of the crime, the availability and productivity of the prosecutor's and defendant's resources, differential cost of a trial relative to settlement, and risk preferences (61). The process is one of strategic bargaining, where a settlement is reached out of court when both actors perceive the costs of the trial (including risk) exceed the benefits. The negotiated settlement is therefore bounded by the minimum defense offer and the maximum offer by the prosecutor (66).

Posner (1973) subsequently developed a similar model to predict out-of-court settlement in *civil* cases. Both parties are utility maximizing and risk neutral. The plaintiff's (p) minimum offer is the expected value of the litigation to the plaintiff ( $P_p J - C_p$ ) plus the settlement costs ( $S_p$ ). The expected value of the litigation is the present value of the judgment in the event of a win ( $J$ ), multiplied by the probability of a win ( $P_p$ ), minus the expected cost of litigation ( $C_p$ ). The defendant's (d) maximum offer is the expected cost of litigation ( $(1 - P_d)J + C_d$ ) to the defendant minus settlement costs ( $S_d$ ). The expected cost of litigation is the present value of the judgment ( $J$ ) multiplied by the probability of the defendant winning ( $1 - P_d$ ) plus the cost of litigation ( $C_d$ ).

Thus, the necessary, though not always sufficient sufficient, condition for settlement is that the plaintiff's minimum offer is less than the defendant's maximum offer:

$$P_p J - C_p + S_p < (1 - P_d)J + C_d - S_d \quad (2.1)$$

The case proceeds to trial when the plaintiff's minimum offer is greater than the defendant's maximum offer.

$$P_p J - C_p + S_p > (1 - P_d)J + C_d - S_d \quad (2.2)$$

The probability a case goes to trial is therefore a function of both parties' expectations about the trade-off between the costs of litigating a case ( $C_p$ ,  $C_d$ ) and settling the case ( $S_p$ ,  $S_d$ ) and the expected judgment ( $J$ ). Settlements will be smaller than verdict awards on average because settlement costs are expected to be much lower than litigation costs (417).

Gould (1973) adopts the same approach as Posner (1973) but relaxes the assumption that both parties are risk neutral. By incorporating the expected utility hypothesis developed by von Neumann and Morgenstern, Gould shows that risk aversion and risk preference by either or both parties can alter the probability a case is settled before going to trial. Moreover, the specific amount of the settlement depends on the bargaining power of both parties. If the plaintiff is likely to win (as  $P_p \sim 1$  and  $(1 - P_d) \sim 0$ ) the settlement will be close to the amount in the claim. Conversely, if  $P_p \sim 0$ , the settlement will be small (287-8).

The net effect of this bargaining on the relative size of jury awards to awards from a settlement is somewhat ambiguous. If  $1 < P_p < 0.5$  than the average jury award would be

expected to exceed the settlement, since an acceptable settlement offer will be reduced by the expected likelihood of a plaintiff win. Conversely, if  $0.5 < P_p < 0$ , then the average settlement will exceed the jury award since the plaintiff is expected to lose in these cases and receive no award. However, average jury awards are unambiguously likely to be greater than settlement awards since  $S_{p,d} < C_{p,d}$ . Thus, on average, the model predicts that jury awards will be larger, but only as a function of the differences in costs between litigation and settlement.

### Prior Research

Danzon and Lillard (1983) apply the models developed by Landes (1971), Gould (1973) and Posner (1973) to study the dispositions of medical malpractice claims and address the issue of selection bias. The model is estimated from data drawn from two surveys of insurance company claim files closed in 1974 and in 1976. Less than 10 percent of cases were tried to verdict (which falls into the usual range of 2-12 percent). The mean award at verdict was \$102,000 and \$26,000 at settlement. However, jury awards are not normally distributed – half of the total award from all claims was explained by less than 3 percent of all claims (347). The authors observe that if the plaintiff's minimum ask ( $P_p J - C_p + S_p$ ) becomes negative, the claim will be dropped.

Danzon and Lillard use this model to estimate the unobservable variables, including potential award at verdict, potential settlement, minimum plaintiff offer and maximum defendant offer (346). On average, cases settle for 74 percent of their potential verdict. The difference between jury awards and settlement increases with plaintiff litigation cost and decreases with defendant litigation costs (375) as predicted by theory which incorporates a term discounting the expected value of a verdict for the probability

of winning and for litigation costs that are averted. Danzon and Lillard note that out-of-court settlements are strongly influenced by potential verdicts (375).

Viscusi (1988) analyzes a sample of 10,784 closed product liability claims closed between mid-1976 and mid-1977. Nineteen percent of claims were dropped, 77 percent were settled out of court and 4 percent were settled at court. The plaintiff won in court in less than 2 percent of all cases. Cases settled outside of court settled for less than the expected court award due to averted litigation costs, plaintiff risk aversion and the nonzero probability of losing (206). Viscusi asserts that out-of-court settlements understate the true value of pain and suffering as the litigation process lowers the plaintiff's reservation price and the defendant's maximum offer amount below the expected court award (214-5). Thus, while verdict awards are expected to be higher in cases adjudicated by a jury, it does not hold that the harms to victims in these cases are greater than in cases that are settled (215).

Sloan and Hsieh (1990) also adopt the standard utility maximization approach to analyze medical malpractice claims closed in Florida from October 1985 through March 1988. The distribution of case dispositions is similar to Viscusi—of the 6,612 observations, 16.5 percent were closed before a suit was filed, 72 percent were settled before a verdict and 11.5 percent of claims were decided at verdict or on appeal. In these data, the average award to plaintiffs is larger for cases disposed of later in the litigation process. Mean payments (in 1987 dollars) were \$30,000 for cases settled before a suit was filed, \$67,000 for cases settled after a suit was filed but before a verdict was reached, and \$95,000 for cases decided at verdict (1007). The authors propose that the higher awards are mainly explained by plaintiff risk taking, delay in receiving payment and



higher litigation costs (1026). Sloan and Hsieh do not entirely discount the possibility that cases with higher potential awards were selected for trial and they assert that the cases adjudicated in court would have settled for more on average than cases that actually settled (1027).

### Conclusion

Prior research generally concludes that out-of-court settlements are lower than awards at verdict because settlements are discounted for the risk of an adverse outcome for the plaintiff and averted litigation costs on the side of the defendant. Later research comparing awards from out-of-court settlements and from cases resulting in trial verdicts finds that settlement awards are almost always lower than awards at verdict. However, much of the difference between settlements and awards at verdict can be explained by economic theory. Thus, differences in award size may not be an indication that cases settled by jury have more serious harms than cases settled out of court. Put another way, if the null hypothesis is that the harms to victims in cases settled by jury and settled out of court are equal, then there is little evidence in prior research to reject that null.

## Appendix A.3

### Assessing the Effectiveness of the Propensity Score Weights

Table A.3.1 describes the differences in descriptive statistics between the original sample (labeled All) and the smaller sample produced by the propensity scores (labeled PS). The most striking difference between the two datasets is that most of the cases resulting in a punitive award are not included in the smaller sample. In the original dataset, 563 observations included a punitive award (4.4%) and the average award was more than \$133,000. In the sample created by the propensity scores, only 4 observations with a punitive award remain. This results from the inclusion of a dummy for punitive award in the algorithm that creates a cross-combination of all attributes. Thus, in order to be included in the final dataset, there must be at least one observation with a punitive award (and the other attributes in that row) in both the crime and not crime categories. Since punitive awards were relatively uncommon in general, it is not surprising few remain in the final dataset. However, since the theoretical literature identifies not having the option of a punitive award as a predictor of bias in jury awards, losing these observations may well reduce bias in the final sample, and may assist in balancing between the crime and not crime groups.

More importantly, balancing between the crime and not crime groups produces the curious result of making the mean award slightly more dissimilar. While the total award is virtually the same, the lack of any punitive awards in the smaller sample (the PS sample) means that the mean award is slightly larger once those cases are removed. Those differences are not evenly distributed between crime and not crime cohorts. While

the samples labeled as crime have a slightly smaller mean adjusted award (\$955,000 compared to \$974,000 in the full sample), the propensity score adjustments result in a large change in the not crime cohort, where there is an increase in mean award from \$1.33 million to \$1.49 million. Thus, the net result of the propensity score modeling has been to create a more balanced distribution of the dependent variable on the predictors, but slightly less balance in the adjusted means.

Table A.3.I.

Comparison of Total Award Distribution Before and after Propensity Score Weighting.					
Category	Total Award	Punitive	Total, Excluding Punitive	Percent any Shared Negligence	Mean Percentage of Shared Negligence
All (N=12,918)	\$1,432,689	\$133,036	\$1,301,968	25.4%	28.0%
PS (N=5,516)	1,430,350	497	1,429,853	26.1	
Crime Indicator - All					
Crime (N=895)	\$1,125,827	\$257,570	\$974,324	16.1	23.7
No-Crime (12,023)	1,459,306	123,765	1,330,387	24.3	28.2
Crime Indicator - PS					
Crime (N=609)	\$956,037	\$292	\$955,745	18.5	22.1
No-Crime (4,909)	1,490,251	593	1,489,728	15.8	28.4
Issue - All					
Crime (N=890)	1,125,827	257,570	974,324	16.1	10.8
Malpractice (N=2,219)	2,066,114	71,574	1,994,539	11.8	7.8
Liability (N=8,500)	1,284,391	94,952	1,189,435	30.3	28.5
Other (N=1,304)	1,322,731	473,041	849,690	11.4	23.9
Issue - PS					
Crime (N=609)	\$956,037	\$292	\$955,745	18.5	22.1
Malpractice (N=960)	2,630,792	1,867	2,628,343	10.4	25.9
Liability (N=3,385)	1,207,040	0	1,207,040	19.0	28.5
Other (N=564)	1,006,243	0	1,006,243	5.9	24.7

Note: All records include a non-missing value for total award (n=12,918 and N=5,516). All values are adjusted for inflation and presented in 2003 dollars. The 'Total Award' column includes all awards (before any reduction for shared negligence). The 'Total, Excluding Punitive' column is the total award without punitive award (before any reduction in shared negligence). 'Percent any Shared Negligence' is the percentage of all cases (in that category) that have any 'comparative' or shared negligence. 'Mean Percentage of Shared Negligence' is the average percentage reduction due to 'comparative' or shared negligence.

### Independent Variables

Table A.3.2 describes the means of the independent variables in the propensity score analysis. The independent variables fall into three categories: the exclusion restrictions which are the potential sources of selection as determined by the theoretical literature, variables that are common to both the RAND and the NIBRS data that will be used to impute values for victims of crime in NIBRS, and other variables that may predict jury award. All of these variables were used in propensity score model.

Among the selection variables, three variables were constructed to match the theoretical literature. First, as described above, the literature describing predictors of jury awards generally concludes that it is the presence of a defendant that is a business that is associated with larger jury awards, rather than the resources (the ‘deep pockets’) of the defendant. In the RAND data, defendants included businesses, the government and hospitals. Each of those was coded as ‘business defendants’.

Next, the literature suggests that when juries are not given the option of making a punitive award, awards for noneconomic and particularly economic losses may be higher. Thus, awards in these cases would overestimate the true harms to victims. However, these data are not observable in the RAND data. To approximate this source of bias, a dummy variable was coded that is 1 when there was a punitive award, and 0 in other cases. The intuition behind this approach is straightforward, that the covariation between the presence of a punitive award and other predictors should approximate the covariation between the absence of an award and other predictors.

Table A.3.2.

## Distribution of Independent Variables (Super-Weighted).

Category	Sub-Category	Mean
Selection Factors (N=5,516)		
Region	Midwest	20.6%
	South	4.0%
	East	43.4%
	West	32.0%
	Gender (Male=1)	42.9
	Reprehensiveness of the Defendant	28.0%
	Business as a Defendant	49.1%
	Availability of a Punitive Award to the Jury	0.1%
	Crime	11.0%
Common Variables		
Injury Type	Age	36.3
	No Injury	7.8%
	Dead	8.6%
	Broken Bones	19.7%
	Internal Injury	2.3%
	Severe Lacerations	1.6%
	Other Major Injury	57.7%
	Apparent Minor Injuries	24.4%
	Loss of Teeth	2.7%
Unconsciousness	1.2%	
Other Predictors		
Issue	Plaintiff Count	1.28
	Number of Issues	1.21
	Crime	11.0%
	Malpractice	17.4%
	Negligence	61.4%
	Other	10.2%

Note: All records include a non-missing value for total award (n=5,516). All values are adjusted for inflation and presented in 2003 dollars. The 'Total Award' column includes all awards (before any reduction for shared negligence). Injury categories are not mutually exclusive and sum to greater than one.

Finally, the civil jury literature suggests that all else being equal, when a defendant is particularly reprehensible the plaintiff would receive a larger award. Again, this extralegal factor would tend to cause awards to be overestimated. A variable that directly estimates defendant 'reprehensiveness' was also not available in the RAND data. Here

again, a similar logic was applied, that the inverse condition—a lack of reprehensiveness—could capture the co-variation associated with selection bias.

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