
ICPSR UNDERGRADUATE RESEARCH PAPER COMPETITION, 2007

» First Prize «

Testing the Theory of Rational Crime With United States Data, 1994-2002

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ABSTRACT

Do criminals in the United States respond rationally to changes in incentives, or is crime inherently an irrational phenomenon? Building upon models used by Ehrlich (1973), Levitt (2002), and others, this paper uses a model of rational crime to examine the elasticities of seven index crimes with respect to changes in law enforcement expenditures and economic incentives using state-level United States data from the years 1994 through 2002. Our empirical results are consistent with the economic model of criminal behavior first proposed by Becker (1968), in which higher levels of law enforcement reduce crime through a deterrence effect, and other recent studies suggesting that aggregate crime rates have a significant rational component.

TABLE OF CONTENTS

Abstract	i
I. The Costs of Crime	1
A. Measuring Crime Rates and Trends	1
B. Damages and Costs of Crime	3
C. Costs of Protection and Law Enforcement.....	4
D. The Optimal Levels of Law Enforcement and Punishment	5
II. Rational Crime and Review of Literature	6
A. The Idea of “Rational Crime”	6
B. Two Methods of Increasing Punishment.....	7
C. Previous Empirical Studies	8
III. An Econometric Model	9
A. The Individual’s Crime Decision	9
B. The Aggregate Supply-of-Offenses Function	10
C. Correcting for Simultaneous Causality Bias	11
D. Correcting for Serial Correlation.....	12
IV. Data Sources.....	14
V. Discussion.....	15
A. Expected Results	15
B. Regression Results	16
C. Explaining the Anomalous Results for Income Inequality	19
D. Implications of the Estimated Elasticities on Law Enforcement Expenditures	20
VI. Conclusion	21
References.....	22

Do criminals in the United States respond rationally to changes in incentives, or is crime inherently an irrational phenomenon? Building upon models used by Ehrlich (1973), Levitt (2002), and others, this paper uses a model of rational crime to examine the elasticities of seven index crimes with respect to changes in law enforcement expenditures and economic incentives. The relative elasticities for different types of crime can be compared to theoretical predictions and our intuitions about which crimes should be more or less sensitive to changes in incentives. These elasticities have important policy implications for determining the optimal level of law enforcement expenditures and for evaluating the efficiency of various crime reduction proposals.

This structure of this paper is as follows. Section I motivates the topic by examining the costs of crime and concluding that, given a theory of criminal behavior, there exists an optimal level of law enforcement. Section II discusses the proposal that criminal behavior be modeled using a theory of rational crime and surveys the literature relevant to our analysis. Section III formalizes this theory into an empirical model, and Section IV identifies several sources of state-level data which will be used with the model. The final section provides the results of our empirical analysis and examines whether the theory of rational crime is consistent with these findings.

I. THE COSTS OF CRIME

A. Measuring Crime Rates and Trends

Two different measures of crime in the United States show roughly similar trends over the past three decades. Figure 1 presents the trend in crimes reported to police, as measured by the FBI's annual Uniform Crime Reports (UCR), scaled to equal 100 in the year 1979. These reported offense rates, however, present only a partial picture of crime in the United States. Only a fraction of crimes are reported to police each year, with many victims failing to report crimes out of personal pride, fear of reprisal, or the belief that a crime was unimportant or unsolvable (Bureau of Justice Statistics 2003). The National Crime Victimization Survey (NCVS), administered annually by the Bureau of Justice Statistics, seeks to remedy this problem by selecting a random sampling of households and interviewing residents about any crimes they have experience in the last year; when these individuals are contacted directly, they often describe crimes that they did not previously report to the police. Table 1, which presents NCVS

TABLE 1 – CRIME REPORTING RATES, 1995-2004

Year	Rape	Aggravated Assault	Robbery	Burglary	Larceny	Motor Vehicle Theft
1995	31.49%	54.89%	55.62%	50.71%	26.80%	74.99%
1996	28.98%	57.07%	55.31%	52.08%	28.76%	79.13%
1997	39.26%	60.12%	56.84%	51.20%	28.84%	79.87%
1998	31.25%	57.56%	63.75%	49.66%	28.67%	80.88%
1999	31.26%	55.85%	55.97%	48.37%	28.28%	83.53%
2000	60.13%	58.95%	60.81%	53.55%	30.07%	81.71%
2001	42.04%	59.22%	62.19%	54.73%	31.56%	83.37%
2002	49.08%	59.46%	68.59%	57.72%	32.87%	82.77%
2003	33.91%	62.50%	61.08%	54.40%	33.12%	79.23%
2004	47.83%	63.93%	53.53%	53.53%	32.36%	87.90%

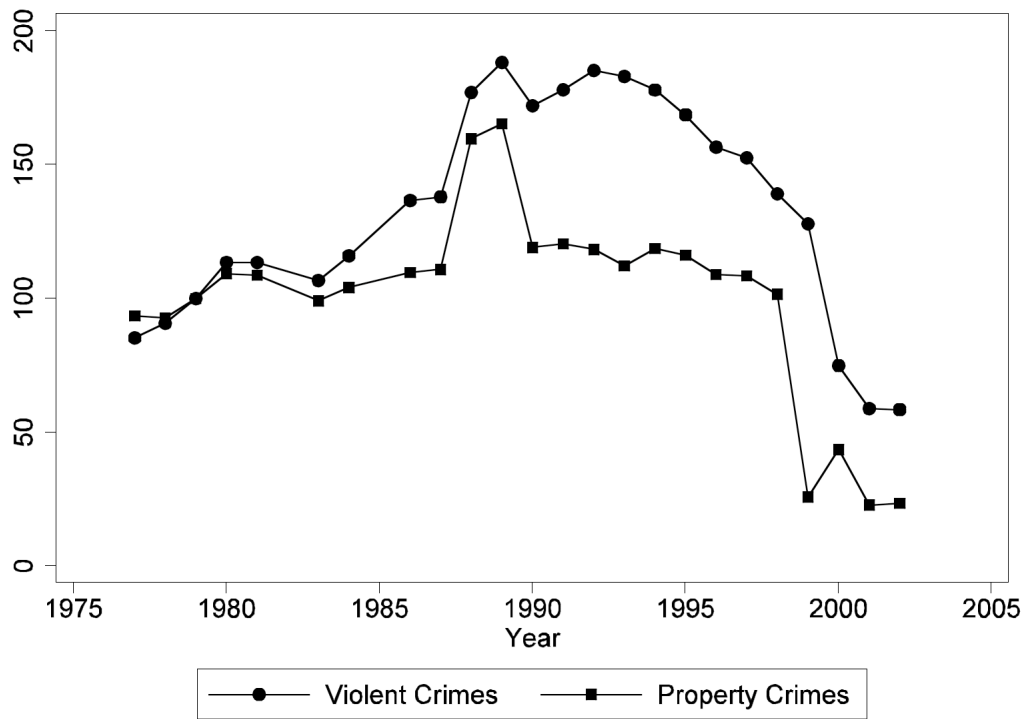


FIGURE 1 – CRIME RATES FROM THE UNIFORM CRIME REPORTS
(SCALED TO 100 IN 1979)

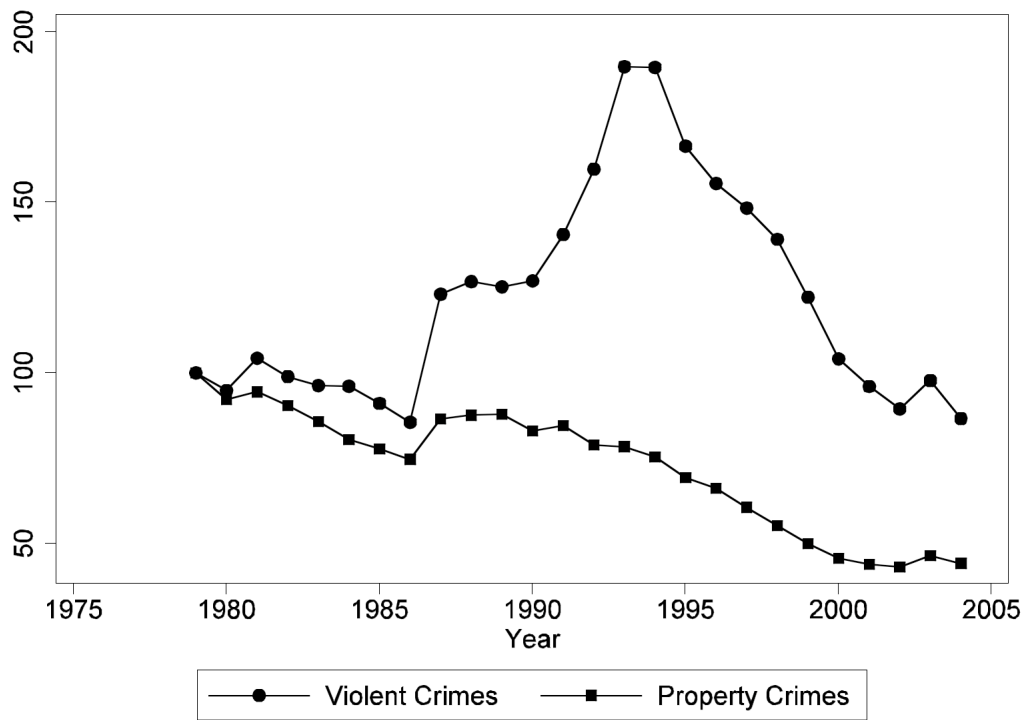


FIGURE 2 – CRIME RATES FROM THE NATIONAL CRIME VICTIMIZATION SURVEY
(SCALED TO 100 IN 1979)

data on reporting rates for several crime categories for the past ten years, shows that reporting rates can vary significantly across crime categories and from year to year. Crime rates derived from the NCVS are presented in Figure 2. Both the UCR and the NCVS data show crime rates falling in the early 1980s, rising later in the decade and in the early 1990s, and dropping precipitously at the end of the century. In 2004, the most recent year for which data is available, there were an estimated 5.2 million violent crimes and 18.7 million property crimes across the United States (Bureau of Justice Statistics 2005).

B. Damages and Costs of Crime

Crime imposes costs on society in several distinct ways. The easiest costs to measure are those arising from damage, destruction, or theft of ordinary property; these can generally be measured as a loss in market value. In addition to these, there are several other kinds of pecuniary costs, or costs that are measured in monetary terms, such as a loss of future income resulting from physical disability or from the destruction of business property. Any attempt to measure these losses will necessarily be subjective, but the losses are nevertheless naturally measured in monetary terms.

Crime is also responsible for certain nonpecuniary harms that may prove difficult to quantify. These are most evident in violent crimes such as murder, where the harms include “the value placed by society on life itself” (Becker 1968). But nonpecuniary or psychic harms can also be present in cases of property crime, such as when a stolen good has sentimental value above and beyond the market value of the good. While these sorts of harm may be extremely difficult to measure, they are no less “real” than pecuniary harms: many people would be willing to pay substantial sums to prevent these losses, and their willingness to pay shows that these constitute a real source of value for victims of crime. Cohen (1988) uses a set of jury verdicts to calculate the average costs, both pecuniary and non-pecuniary, for several different crime types. Cohen’s findings are presented below in Table 2.

Ehrlich (1973) also identifies losses due to socially inefficient investment by criminals in the course of criminal activity. For instance, the time that criminals spend engaged in criminal activities rather than legal occupations results in a loss of potential production, which is essentially an opportunity cost of crime to society. Because criminal activities are inherently non-market activities, the “wages” from crime are not priced at the market rate, which obstructs the efficient allocation of labor. Ehrlich also points to socially wasteful expenditure by criminals in order to protect against prosecution, including any resources a criminal uses to cover his tracks and all fees paid to defense attorneys to try and avoid conviction.

TABLE 2 – AVERAGE COSTS OF CRIME FROM JURY VERDICTS (COHEN 1988)

	Direct Losses	Pain and Suffering	Risk of Death	Total
Rape	\$4,617	\$43,561	\$2,880	\$51,058
Aggravated Assault	\$422	\$4,912	\$6,685	\$12,019
Robbery	\$1,114	\$7,459	\$4,021	\$12,594
Burglary	\$939	\$317	\$116	\$1,372
Larceny	\$176	---	---	\$176
Motor Vehicle Theft	\$3,069	---	\$58	\$3,127

It is occasionally objected that the value of one's life or physical integrity cannot be priced and should be considered "infinite," in that there is no amount of additional wealth that would make the victim as well off as he would have been without the crime; however, this objection is not actually a problem for the economic approach to crime. It may be true that if we know of a certain, impending crime like an aggravated assault, we should be willing to pay any amount – and certainly more than Cohen's \$12,000 estimate – to prevent it. For purposes of public policy, however, individual crimes are never a certainty, but rather are a manifestation of risk. From the policymaker's perspective, a stronger law enforcement policy does not prevent any particular instance of a crime, but rather reduces the *risk* of crime for each individual person. Block and Lind (1975) demonstrate that even if an individual believes that some harm is too great to be priced, the amount he will be willing to pay to *reduce the risk* of that harm must still be finite. The values presented here are those appropriate for cost-benefit analysis for policies that affect the *risk* of crime; it remains an open question what the appropriate compensation for a crime would be or whether such an amount actually exists.

C. Costs of Protection and Law Enforcement

1. Public Law Enforcement: While the damages caused by crime impose great costs on society, efforts to reduce crime through law enforcement are also very costly. A complete law enforcement system generally has three components. First, the government must form a police force to monitor violations of the law and to identify and arrest offenders. Second, there will usually be an independent court system to assess guilt. Third, there must be a corrections system to administer punishment, typically through imprisonment. All together, government law enforcement expenditures for the United States in 2001 are estimated at \$167.1 billion, or about 1.66% of U.S. GDP (Bureau of Justice Statistics 2004). These costs have been increasing rapidly, with police expenditures increasing more than 40% from 1994 to 2003 and the number of inmates in U.S. prisons increasing nearly fourfold in the last thirty years (Bureau of Justice Statistics, various years).

2. Private Self-Protection: In addition to public expenditures on law enforcement, many individuals purchase self-protection measures such as alarm systems and private security guards (Ehrlich 1973, Freeman 1996). While there is no comprehensive measure of these expenditures, some evidence suggests that they may be substantial. According to occupational employment data from the Bureau of Labor Statistics, approximately one million people in the United States report working as private security guards. Similarly, new alarm technologies such as Lojack have experienced high demand and rapid growth (Ayres and Levitt 1998).

Private self-protection measures come in two forms, each of which will be inherently inefficient. The first category of self-protection measures concerns observable protections such as car alarms with visual indicators, fences, and security guards. When these self-protection devices are observable to criminals, the effect is simply to shift crime from one potential victim to another; for instance, a car thief who sees an alarm system on one car will simply move on and steal another car instead. Expenditures on observable self-protection measures will have little or no effect on the overall crime rate, and thus their social benefit will not exceed their cost.

Alternatively, self-protection devices might be unobservable or concealed so that criminals do not know which victims are protected. These measures should succeed in causing criminals to be less likely to commit crime against *any* victim. But this effect includes a positive externality whose benefits will not all accrue to the purchaser, which creates a market failure that will lead to inefficient under-investment in self-protection. It may be possible to internalize part of the externality if insurance companies offer discounted premiums for owners of these devices, but in practice many people may be uninsured, and this will be an imperfect solution (Ayres and Levitt 1998).

In either case, self-protection will be inefficient. When self-protection is observable, its private benefit will exceed its minimal social benefit and it will be oversupplied; when it is concealed, its social benefit will exceed its private benefit and it will be undersupplied. Because private self-protection against crime is necessarily inefficient, high levels of self-protection should be cause for concern. Self-protection might be valuable if a small number of individuals demand much more protection than the government provides, but if a large number of people are purchasing self-protection, it would be more efficient to increase funding of public law enforcement instead.

3. Crime-Induced Behavioral Distortions: Finally, the social costs of crime must also include crime-induced distortions in the behavior of normal citizens, such as “avoiding Central Park after dark or moving to the suburbs” (Ayres and Levitt 1998). We can now recognize these costs as a variation on observable private self-protection: an individual forgoes some convenient behavior for a costlier alternative, which reduces his exposure to crime while shifting the risk to others who continue that behavior. Behavioral distortions deserve special concern, however, because they involve no outlay of funds and thus are difficult to account for. As with other self-protection measures, high levels of behavioral distortion indicate a need for more public law enforcement.

D. The Optimal Levels of Law Enforcement and Punishment

The competing costs of crime and law enforcement imply that there will be an optimal level of law enforcement that minimizes total costs and thus maximizes total social welfare. On this view, crime is a negative externality of freedom, and punishment can be seen as a Pigouvian tax that is costly to administer. This economic view of punishment differs greatly from other approaches; for instance, efficiency considerations will dictate a punishment scheme that is very different from a policy guided by proportional justice (Waldfoegel 1995).

Because law enforcement is costly, the optimal level of enforcement is not necessarily the same as the level that minimizes crime or that maximizes deterrence. In some cases, underdeterrence is optimal; in other cases, overdeterrence is optimal; and there may even be cases where the optimal level of enforcement results in some citizens being underdeterred and others overdeterred (Polinsky and Shavell 1984). In order to estimate the optimal level of law enforcement, we need a theory of how criminal activity responds to changes in law enforcement and other factors.

II. RATIONAL CRIME AND REVIEW OF LITERATURE

A. The Idea of "Rational Crime"

The theory of rational crime was first presented in a seminal paper by Gary Becker (1968), which proposed that the economic theory of rational choice can be used to explain the decision to engage in criminal behavior. According to Becker, "a person commits an offense if the expected utility to him exceeds the utility he could get by using his time and other resources at other activities." In this model, individuals are presented with the choice between a legal occupation and criminal activity, and they choose the option with a higher expected return. For legal occupations, the expected return is simply the wage paid. For criminal activity, the expected return is a probabilistic weighting of the benefits from a successful crime against the costs if caught and punished. The benefits may be monetary gains, as is often the case with property crimes, or they may be psychic gains arising from the satisfaction of some preference, as may be the case for crimes like rape and murder. The model can be extended to cases where an individual may divide his time between legal occupations and criminal activity in any proportion (Ehrlich 1973), or where criminals consider the ethical costs of crime as well as the monetary benefits (Block and Heineke 1975).

The economic view of crime assumes that even criminals respond to incentives, and therefore potential criminals can be deterred from crime by the threat of punishment. We need not make the extreme claim, however, that crime is fully or even primarily motivated by rational considerations. Indeed, there are many crimes, particularly violent crimes, where it seems unlikely that the criminal gives thorough consideration to the punishment. The economic approach to law enforcement instead relies on the lesser claim that rational considerations are at least one of several factors that can affect the behavior of some criminals. As Ehrlich (1973) explains, "our alternative point of reference . . . is that even if those who violate certain laws differ systematically in various respects from those who abide by the same laws, the former, like the latter, do respond to incentives." If this proves true, we should find that rational considerations have a measurable impact on aggregate crime rates. In theory, rational considerations should be particularly apparent if we control for tastes and other factors that affect psychic costs and benefits (Witte 1980). In practice, it will be impossible to identify or measure all relevant tastes, but better controls for tastes make it easier to isolate the rational component of criminals' decisions.

The intuitive objection that some crimes are much less determined by rational factors is not a flaw in the model, but instead is a hypothesis which we should be able to test: different crimes should have different elasticities of supply with respect to changes in punishment, depending upon how "rational" they are. Indeed, Ehrlich (1973) finds that crimes against property "vary positively with . . . income inequality . . . and with the median income," whereas "these variables are found to have relatively lower effects in the case of crimes against the person." Levitt (1998) finds that violent crimes and property crimes respond to punishment differently, with violent crimes responding mostly through an incapacitation effect and property crimes responding primarily through a deterrence effect. Witte (1980) similarly finds that "deterrence works through different variables for individuals who specialize in different types of crime."

B. Two Methods of Increasing Punishment

There are two methods by which policymakers can increase expected punishment. First, in theory, they can increase the severity of punishment by changing sentencing laws. The cost of this increase depends on the type of punishment used. In the case of fines, increasing the punishment is virtually costless; however, in cases of imprisonment, increasing the length of incarceration may be extremely costly. In practice, severity of punishment is often dictated by factors beyond the control of policymakers. Particular sentences are assigned by judges and juries, not politicians, and actual time served in prison may differ from the assigned sentence because of parole and other factors. Even when policymakers can adjust sentencing guidelines, making such changes may be slow and difficult, and concerns about fairness may prevent sentencing policies in one state from straying too far from those in adjacent states. Changing the severity of punishment may also be an unattractive mechanism for reducing crime if criminals are poorly informed about expected punishment or if they have difficulty distinguishing small changes in sentencing policy. We might also suspect that there will be diminishing marginal deterrence for each additional increase in the severity of punishment, for three reasons: First, just as we typically assume that individuals have diminishing marginal utility for wealth, it is reasonable to assume that they also exhibit diminishing marginal (dis)utility for punishment. Second, rational discounting means that the present value of later years in jail should be smaller than the present value of earlier years (Posner 1992). Third, later years of jail time can become increasingly uncertain due to the possibility of parole or of passing away before the full sentence is served.

Alternatively, policymakers can increase the certainty of punishment, which may be done by increasing expenditures on police and prosecutors. Increased funding can be used to increase the probability of arrest (that is, of identifying the person responsible for a crime) and to increase the probability of conviction given arrest (that is, of being able to prove guilt in court). It is reasonable to assume that as law enforcement expenditures increase, the marginal effect of an additional increase in expenditures diminishes.

Changes in the certainty and severity of punishment may differ not only in cost, but also in effectiveness, as criminals might not respond to these changes equally; the relative elasticities of crime with respect to changes in severity and changes in certainty depend largely on criminals' attitudes toward risk. If criminals are risk preferrers, they will have a greater response to changes in certainty than to changes in severity; if they are risk averse, the opposite will be true. Friedman (1984) demonstrates that theory alone cannot tell us what these attitudes will be; rather, it is an empirical question. Block and Gerety (1995) perform a series of experiments on convicts and college students and find that the prisoners show a strong preference for risk and are more easily deterred by increases in the certainty of punishment than its severity. Trumbull (1989) also finds evidence that certainty has greater deterrent effect than severity in both aggregate and individual level crime data. These findings suggest that, under current law enforcement and sentencing policies, risk averse and risk neutral individuals are largely deterred from crime, and those who partake in criminal activity are disproportionately risk preferrers. This explains Becker's (1968) observation that "a common generalization by persons with judicial experience is that a change in the probability has a greater effect on the number of offenses than a change in the punishment."

If most individuals are risk averse, then the decision to engage in crime may also depend on wealth through a mechanism distinct from legitimate wage opportunities. If aversion to risk decreases as income increases, then for crimes punished by imprisonment, the optimal level of punishment to assign to wealthy offenders is greater than the optimal level of punishment for poor offenders; that is, as Polinsky and Shavell (1984) explain, “because of the greater cost-effectiveness of imprisonment when applied to the wealthy group, it may be more desirable to achieve a higher level of deterrence with respect to that group.” Other things being equal, a system of efficient punishment would threaten harsher punishments for the wealthy than for the poor.

While certainty and severity may have different elasticities, it is important to note that they are not completely independent choices. Andreoni (1991) warns that given the inevitable risks of wrongful convictions, a rational judge or jury member should be less likely to issue a conviction as the severity of punishment increases. The literature on jury psychology confirms that the severity of punishment has a significant effect on the probability of conviction. Waldfogel (1993) also notes that insofar as more harmful crimes might leave more evidence, crimes with a higher severity of punishment might naturally also have a higher probability of conviction.

It should be noted, therefore, that attempts to measure the effect on crime of certainty of punishment or of law enforcement expenditure are valid only insofar as severity of punishment is relatively constant. This is a common assumption in the empirical literature, and we will take it into account when developing our empirical model and assessing the validity of its findings.

C. Previous Empirical Studies

There has been a great deal of empirical work attempting to estimate the response of crime to changes in punishment. Much of the literature has been consistent with the predictions of the economic model of crime, but in many cases the results fail to achieve high statistical significance. Among the leading papers, Ehrlich (1973) is the first and remains among the most definitive. Using cross-sectional data on state- and county-level crime and arrest rates in the United States for the years 1940, 1950, and 1960, Ehrlich finds that increases in certainty and severity of imprisonment consistently show a negative effect on crime rates. In most cases, Ehrlich also finds certainty of imprisonment to have a greater effect on crime than severity of imprisonment (exceptions are burglary and assault). Ehrlich also finds that crime rates are higher in regions with high median incomes, where the returns to crime should be higher, and in regions with higher concentrations of people far below the median income, who presumably have fewer legitimate work opportunities. In each case, Ehrlich finds a stronger effect for property crimes than for violent crimes, which corresponds to the intuition that violent crimes are less susceptible to rational considerations. Ehrlich’s results do not show most of these findings to be statistically significant, but Vandaale (1978) reexamines Ehrlich’s data with several variations on the model specification and finds that the signs of the coefficients are robust to the various changes. Fisher and Nagin (1978) show that the instrumental variable used by Ehrlich and Vandaale to correct for simultaneous causality is inadequate, but the effect of this would likely be to *understate* the effects of punishment.

In addition to state and county crime data, estimates can also be made using data for individual cities. Examining panel data from about 100 of the largest cities in the United States from 1975 to 1995, Levitt (2002) finds a negative effect of the number of police officers per capita on crime. The coefficients in several of Levitt's regressions fail to reach high levels of statistical significance. However, Levitt cites three other studies that use different approaches and each also find a negative relationship, albeit all without clear statistical significance. While no single study finds statistical significance with this dataset, the consistent results from four different approaches provide strong support for the economic model to crime.

The effect of punishment and other economic incentives on crime can also be estimated using data on individual persons. Such estimates are difficult, however, because comprehensive crime and demographic data on a sufficiently large set of individuals is rarely available. An exception is data on criminal recidivism, though these datasets may be affected by selection bias. The conclusions of studies based on individual recidivism data are mixed. Witte (1980) finds that recidivism is negatively related to changes in punishment but unrelated to changes in legitimate wage opportunities. Myers (1983), on the other hand, finds recidivism rates to be unrelated to changes in punishment but negatively related to changes in legitimate wages. Should additional datasets become available, there is ample opportunity for more research using individual data.

III. AN ECONOMETRIC MODEL

This section develops the preceding discussion into the formal economic model that I will estimate using state-level data from the United States for the years 1994 through 2002. First, I will review the factors that should affect a rational individual's decision to engage in crime, as identified in the preceding sections. Next, I will propose a reduced-form supply-of-offenses function that takes these factors into account and which can be estimated using state-level data. Finally, I will account for the endogeneity of law enforcement expenditures by using expenditure on firefighters as an instrument and present regression results for seven different index crimes.

A. The Individual's Crime Decision

Ideally we would like estimate the effect of changes in incentives on the crime decision at the individual level, as our model is based on decisions made by individuals in light of their own personal circumstances. In practice, however, this is not possible because such detailed data is not available for a sufficiently large random sample of individuals. Nevertheless, an understanding of the individual's crime decision is necessary before we can generalize the model for use with aggregate data.

The theoretical discussion has identified several factors that should affect an individual's crime decision. First, individuals must consider the expected cost of crime. Our theory predicts that the likelihood of an individual choosing to commit a crime falls in response to an increase in either the certainty or the severity of punishment; it also predicts that wealthier individuals will be less likely to commit crime, as those used to a higher standard of living have more to lose if convicted of a crime and imprisoned. Next, an individual considers the potential gains from crime. For property crimes, which we assume to have an economic motivation, an individual's prospective gains from crime should be closely correlated with the wealth of his potential

victims, which we assume to include all individuals living in proximity to him. For violent crimes, we must consider psychic gains, which would be a function of the individuals' tastes. Finally, an individual must consider the opportunity cost of the time he devotes to crime, which should roughly correspond to the wage rate of legitimate work opportunities available to him.

B. The Aggregate Supply-of-Offenses Function

In order to estimate an aggregate supply-of-offenses function, the unit of measurement must be a region rather than an individual. For this analysis, we will define our regions to be individual states. Two factors motivate this choice. First, much of the data we use is readily available only at the state or federal level; comprehensive data is not available at the city or county level. Second, smaller regions are problematic because an individual may live in one region but commit crime in another, which would violate our model's implicit assumption that the amount of crime in a region is related to values measured in that same region.

When specifying the aggregate supply-of-offenses function, we cannot use the individual-level variables of individual wealth or tastes. Instead, we identify aggregate-level variables that roughly correspond to these and can serve as proxies. In place of an individual's own wealth, we substitute two variables. First, we use a measure of income inequality, which we will calculate as $100 \cdot G$, where G is the Gini coefficient. States with greater income inequality will generally have a greater number of people with relatively low wealth and work opportunities, and it is these individuals that the model predicts will be most likely to engage in crime. Second, we add the unemployment rate into the regressions, as higher unemployment rates correspond to higher likelihood that an individual has *no* legitimate work opportunities.

We next assume that, at the aggregate level, individuals' tastes should be closely correlated with various demographic factors. Because demographics change very slowly over time, and because our data covers only a few years, we can treat these as fixed effects, and our model will account for them with state and year dummy variables.

Recent empirical studies have chosen to omit severity of punishment from the supply-of-crime function (Levitt 1997, Levitt 2002), and we choose to do the same here for several reasons. While a rational individual's crime decision would in theory make use of this information, it is doubtful that any potential criminals would have enough information to form legitimate expectations about precise punishments in his jurisdiction. Indeed, there is no adequate source of information on criminal sentences available to researchers, and the task would be even more difficult for laypersons. Even if data on *assigned* sentences were available, criminals often serve far less time due to parole, so the assigned punishment would be inadequate for estimating the actual punishment. Furthermore, Ehrlich (1973) notes that time served will not even be proportional to the punishment perceived by the criminal due to discounting. Any estimate of the severity of punishment would consequently be highly speculative and distort our estimates.

In addition to the low likelihood that even rational individuals consider variation in the severity of punishment, there are several other reasons to think that this omission will have little to no impact on our estimates. Severity of punishment could affect our estimates only if it varies both across states and over time; if it is fixed in either dimension, then the fixed effects dummy variables will control for it. Because changing sentencing guidelines is typically a slow and

difficult process, we can reasonably expect punishments to be fixed over the short span of time used by our estimates. Similarly, concerns for justice and fairness will impose strict limits on the range of punishment permissible for a given type of crime, and we can further expect these concerns to pressure policymakers to choose values similar to adjacent states, so it is also reasonable to expect punishments to be fairly homogeneous across regions. Insofar as there are slight variations in severity of punishment, this would still require individuals to be able to distinguish those very small differences, and even then, the *marginal* effects of these differences would likely be small due to discounting.

The model requires one further simplification. According to our theory, changes in law enforcement expenditures affect the crime decision only indirectly by increasing the probability of arrest and conviction. Given the limited data available, we cannot adequately identify the precise structure of this effect. We instead estimate a reduced form model which assumes that law enforcement expenditures affect the crime decision directly. This will identify the effects of expenditure changes even without knowing their precise structural form.

We can now state a preliminary version of the empirical model:

$$\ln(CRIME_{sct}) = \beta_{0c} + \beta_{1c} \ln(POLICE_{st}) + \beta_{2c} \ln(AVGINC_{st}) + \beta_{3c} INCINEQ_{st} + \beta_{4c} UNEMP_{st} + \gamma_{sc} STATE_s + \delta_{ct} YEAR_t + \varepsilon_{sct} \quad (1)$$

where s indexes states, c indexes types of crime, t indexes time, $CRIME$ measures the number of offenses per capita, $POLICE$ measures expenditure on law enforcement per capita, $AVGINC$ measures mean income, $INCINEQ$ measures income inequality (as defined above), $UNEMP$ measures the unemployment rate, $STATE$ is a vector of dummy variables for each state, and $YEAR$ is a vector of dummy variables for each year. For the variables that do not already correspond to percentages, we use logarithms, so the coefficients represent elasticities; for instance, when $POLICE$ increases by one percent, $CRIME_c$ increases by β_{1c} percent. Because larger states represent larger samples and provide more accurate information, all regressions are weighted by state population.

C. Correcting for Simultaneous Causality Bias

Equation (1) will fail to provide consistent estimates of β_{1c} , however, because of a likely endogeneity of law enforcement expenditures with respect to crime rates. There are at least two reasons to expect these values to exhibit simultaneous causality. First, while we believe that increases in law enforcement will cause a decrease in crime, these increases are often motivated by a belief among policymakers that crime rates are rising, and a naïve regression would mistakenly associate the independent increase in crime with the increase in police, reporting a coefficient that is higher than the true effect. Second, higher crime rates are likely to result in higher marginal returns to law enforcement expenditures, which in turn should encourage policymakers to choose higher level of law enforcement spending, again creating a spurious positive correlation (Levitt 1997).

In order to correct for this, I will follow Levitt's (2002) example of using expenditure on firefighters as an instrument. Firefighting levels do not seem like they should be directly correlated with crime, but we would expect firefighting and law enforcement levels to respond similarly to many factors that are otherwise exogenous to our model, including local budgetary limits, unionization, and political shifts. Figure 3 graphs the logarithm of firefighting expenditures against the logarithm of police expenditures along with the best-fit line, which has an R^2 value of 0.6096. The relationship becomes even more striking when we remove state-fixed components and consider only variation *within* each state over time, as shown in Figure 4, which yields a first-stage F -statistic of 253.17 and an R^2 value of 0.9491. These findings suggest that expenditure on firefighters serves as a very strong instrument for expenditure on law enforcement. A one percent change in firefighting expenditures corresponds to a 0.71 percent change in police expenditures.

We now reformulate Equation (1) as the second stage of a two-stage least squares (2SLS) regression,

$$\ln(CRIME_{sct}) = \beta_{0c} + \beta_{1c} \ln(\widehat{POLICE}_{st}) + \beta_{2c} \ln(AVGINC_{st}) + \beta_{3c} INCINEQ_{st} + \beta_{4c} UNEMP_{st} + \gamma_{sc} STATE_s + \delta_{ct} YEAR_t + \varepsilon_{sct} \quad (2)$$

where \widehat{POLICE} is the predicted value of law enforcement expenditures after being instrumented on firefighting expenditures and the other exogenous variables.

D. Correcting for Serial Correlation

Standard OLS estimation procedures require that the residuals be serially uncorrelated, that is, $E[\varepsilon_{sct} | \varepsilon_{sct-1}] = 0$. When estimating empirical models of crime, however, this assumption might not seem plausible. Equation (2) will likely omit some of the determinants of crime, especially if our use of fixed effects for control variables is imperfect, and these effects will be treated as part of the error term. If any omitted factors are “sticky” and change over time only gradually, this will create serial correlation in the error term, as these portions of the error term are closely correlated from one year to the next.

We can correct for any serial correlation in our estimates by using partial differencing of degree ρ_c , where ρ_c is a measure of serial correlation in the error term. We estimate this value iteratively using the Cochrane-Orcutt method, with two additional enhancements. First, we use the Prais-Winsten transformation to avoid losing information from the first observation for each state, which is very important because our data set will have a very short time component (covering just a few years) relative to its wide cross-sectional component (covering 50 states), so this preserves many observations. Second, we use the method developed by Baltagi and Wu (1999) to compensate for a number of sporadic gaps present in our data set due to incomplete reporting of crime statistics for some states in some years.

Estimating the degree of serial correlation is difficult here because our data set is not large enough to determine if the serial correlation measured in any particular regression is statistically significant. Because we suspect that serial correlation is likely to be present in our model, we will assume that it is present whenever the estimated serial correlation is positive. If the estimated serial correlation is negative, on the other hand, we should disregard it for

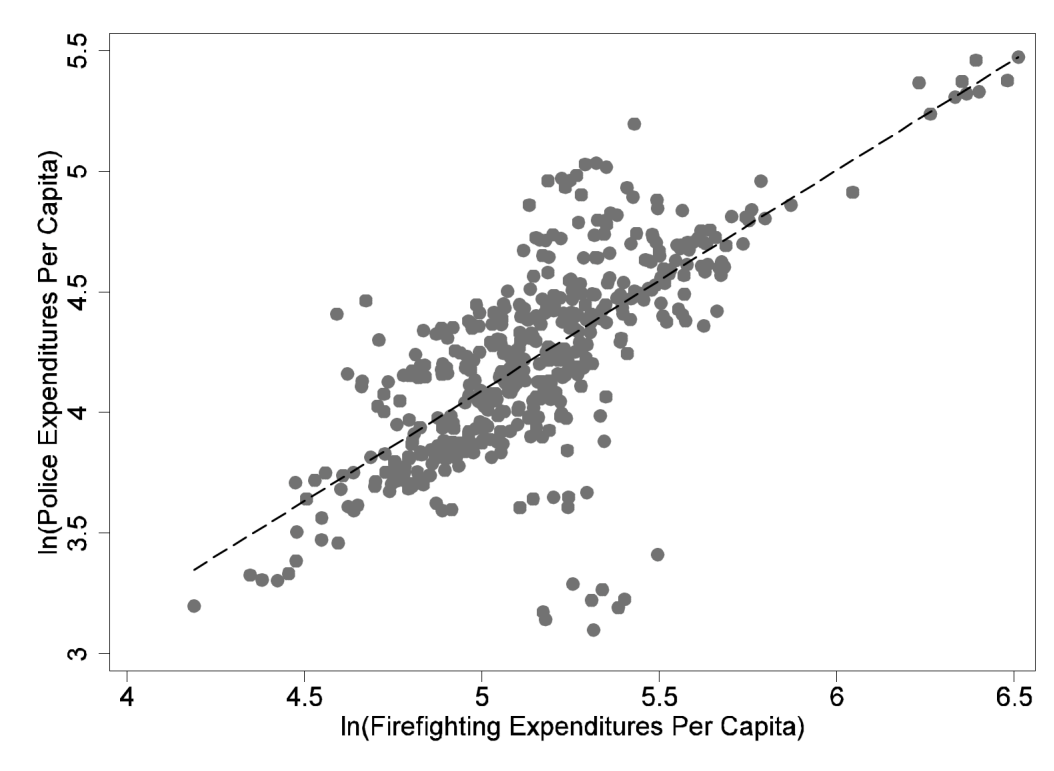


FIGURE 3 – POLICE EXPENDITURES VS. FIREFIGHTING EXPENDITURES (NO CONTROLS)

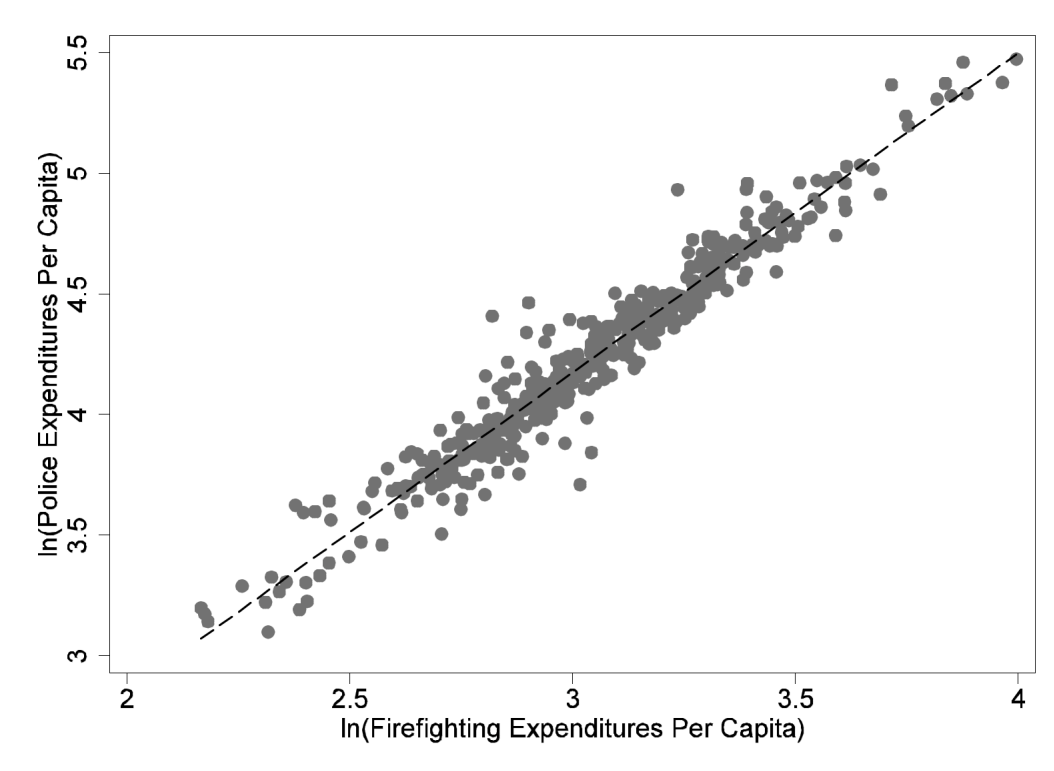


FIGURE 4 – POLICE EXPENDITURES VS. FIREFIGHTING EXPENDITURES (MINUS STATE-FIXED EFFECTS)

two reasons. First, given our beliefs about the factors that affect crime rates, it is difficult to imagine any potentially omitted variables that could be negatively correlated from year to year. Second, models with negative serial correlation are highly sensitive to the time period between observations in the data set, yet our one-year time periods were chosen arbitrarily, so this would likely yield unreliable results.

With this complete empirical model in hand, we will estimate Equations (1) and (2) for each type of crime in our dataset.

IV. DATA SOURCES

Data on reported offense rates are taken from the Uniform Crime Reports (UCR), published annually by the FBI. We restrict our analysis to “Type I” crimes, which are crimes that are likely to be detected whenever they occur. This excludes offenses such as drug use and prostitution, which often go undetected and cannot be measured reliably. Our analysis will focus on four types of property crime – robbery, burglary, larceny, and motor vehicle theft – and three types of violent crime – murder, aggravated assault, and rape. We will perform separate regressions for each of these seven crime categories. Following Levitt (1997), we will also estimate the model for violent crimes as a group and for property crimes as a group. We create these groups by “stacking” the observations for the different categories of crime rather than summing them, since the different categories of crime can differ greatly in frequency and summing them would give excessive weight to the more frequent crime types.

An important concern with the UCR data is measurement error due to underreporting of crime. But assuming that underreporting error is multiplicative – that is, that the number of crimes reported is some fraction r of the number of crimes that actually occur – and that the degree of underreporting is roughly the same for all states, then the findings in this paper should be robust to such effects. Multiplicative error becomes additive error when we use logarithms, and thus this measurement error affects the coefficients only for the constant term and the time-fixed effects, not the elasticities.

Information on state population and on state expenditures on law enforcement and firefighting come from the United States Census Bureau’s Annual Survey of Governments. Data exists for the years 1994-2000 and 2002; the Census Bureau did not collect data at the state level for 2001, so we exclude that year from our analysis.

Data on income distribution (mean family income and the Gini coefficient) are taken from State-Level Data on Income Inequality (Western et. al. 2004), which calculated these statistics using the United States Census Bureau’s annual March Current Population Survey (CPS). We expect changes in income distribution to be very gradual, but the small size of the CPS samples results in volatile estimates, so these calculations were based upon smoothed data in order to minimize distortions arising from sampling error. The nominal income values reported in the CPS were converted to real values (2004 dollars) using the CPI deflator. Data on state unemployment rates is provided in the Geographic Profile of Employment and Unemployment, a subset of the CPS data compiled annually by the Bureau of Labor Statistics.

Summary statistics for our data set are provided in Table 2.

TABLE 2 –SUMMARY STATISTICS

Variable	Mean	Standard Deviation		Minimum	Maximum
		Within State	Across States		
Population	5,393,806	307,731	5,936,515	470,000	33,872,000
Violent Crime	293	124	131	18	864
Murder	6	3	3	0.2	26
Rape	26	15	9	0.1	88
Aggravated Assault	262	115	123	15	799
Property Crime	2,776	1,883	770	135	7,516
Robbery	100	68	58	0.1	485
Burglary	668	510	177	10	2,481
Larceny	1,729	1,335	498	0.1	5,047
Motor Vehicle Theft	256	223	114	3	1,141
Expenditure on Police	176	18	48	66	421
Expenditure on Firefighters	73	8	26	22	181
Mean Family Income	61,895	3,889	7,863	44,655	86,783
100*Gini Coefficient	38.9	1.5	1.5	33.5	45.0
Unemployment Rate (Percent)	4.7	0.7	0.9	2.3	8.7

Notes: Crime rates are per 100,000 residents. Expenditure values are per capita. Data on income and expenditures are in 2004 dollars. The sample used is the set of all 50 U.S. states over the period 1994-2002. Data on crime are from reported offense rates from Uniform Crime Reports issued by the FBI. Population and expenditure data are from the Annual Survey of Governments issued by the Census Bureau. Income data are from Western et. al. (2004) and are derived from Current Population Surveys issued by the Census Bureau. Data on unemployment rates are from the Geographic Profile of Employment and Unemployment issued by the Bureau of Labor Statistics.

V. DISCUSSION

A. Expected Results

The theory of rational crime yields a number of hypotheses that can be tested by examining the results of our regressions. First, because the endogeneity of law enforcement expenditures is believed to create a positive bias in that coefficient, we expect the 2SLS estimates (Equation (2)) to have consistently more negative point estimates than OLS (Equation (1)). We present both the OLS and 2SLS results below in order to evaluate this hypothesis.

The theory predicts that the 2SLS estimates of the coefficient on law enforcement expenditures should be negative for all forms of crime, though the argument for criminals being rational is somewhat less compelling for violent crime than for property crime. For property crime, higher mean family income indicates higher potential benefits from crime, so we expect positive coefficients; likewise, higher income inequality and higher unemployment signal lower opportunity costs to crime, so we expect to find positive coefficients on these as well. For violent crime, we expect similar effects for income inequality and unemployment, but mean family income has a somewhat different interpretation: higher mean family income indicates a higher standard of living, which in turn creates higher opportunity costs for time spent in prison, so we expect a negative coefficient.

We cautiously expect our model to have difficulty establishing that the estimated coefficients are statistically significant. We expect this not only because the three recent studies reviewed by Levitt (2002) fail to achieve statistical significance, but also because we can identify a number of weak links in our model. The results here are based on relatively small samples (about 340 observations) and use somewhat indirect proxies to measure what may be very small effects, and the use of an instrument and of weighted regressions will increase standard errors. Furthermore, our model measures only within-state effects, while some of our variables – mean income, income inequality, and unemployment – are likely to have a strong across-states effect that is not measured. We should also be cautious because we are testing approximately 60 coefficients at a 5% level of significance, which creates a substantial risk of Type I error.

B. Regression Results

Estimates of Equations (1) and (2) are presented for property crimes in Table 3 and for violent crimes in Table 4. Heteroskedasticity-robust standard errors are reported in parentheses beneath the estimated coefficients. An asterisk indicates statistical significance at the 10% level, and two asterisks indicate statistical significance at the 5% level.

We find that the use of expenditure on firefighters as an instrument leads to more negative estimates of the coefficient on law enforcement expenditures for all crime categories except rape. Of particular note are the coefficients for robbery, burglary, and motor vehicle theft, which are significantly positive in OLS but all have negative point estimates with 2SLS. This is consistent with the predicted effect of using an instrument to overcome the endogeneity bias.

For the 2SLS estimates of property crime, every single crime category displays the expected negative sign for the coefficient on law enforcement expenditures. While none of these individually reach the level of statistical significance, they are fully consistent with the hypothesis. For robbery, burglary, and motor vehicle theft, the coefficients on mean family income and unemployment are all properly signed, and three of these six coefficients are statistically significant. The coefficients for income inequality, however, are troubling: three of the four coefficients have the incorrect sign, two of them with statistical significance, and the fourth simply shows no effect.

Larceny appears to be an anomaly among the property crimes, with incorrectly signed point estimates for the coefficients on mean family income and on unemployment. This is unsurprising, as we intuitively recognize larceny to be the most impulsive of the property crime types. It would also be unsurprising if larceny were to actually *decrease* as economic conditions improve: the larceny category is dominated by petty theft, and as an individual's wealth increases, the returns to larceny become comparatively smaller while the opportunity cost of a prison term quickly increases.

Among the 2SLS estimates of violent crime, we do not find the expected sign on police expenditures for rape and find a near-zero effect on assault, but we do find a negative coefficient for murder. This is consistent with past studies which have found that, compared to other violent crime, murder appears to show a surprisingly strong response to increases in the number of police officers or arrest rates (Levitt 1997). For both murder and rape, we find the expected

TABLE 3 – PROPERTY CRIME REGRESSIONS

Variable	ln(All Property Crime)		ln(Robbery)		ln(Burglary)		ln(Larceny)		ln(M.V. Theft)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
ln(Expenditure on Police)	0.765 (1.510)	−3.523 (17.641)	1.795* (0.962)	−5.089 (11.662)	1.205** (0.404)	−5.687 (8.605)	−2.438 (1.677)	−3.009 (13.445)	2.118* (1.176)	−2.479 (9.617)
ln(Mean Family Income)	2.247 (3.216)	2.187 (3.275)	4.784* (2.548)	4.479 (2.938)	1.438 (1.209)	1.283 (1.967)	−2.385 (2.453)	−2.376 (2.372)	6.568** (2.054)	6.450** (2.210)
Income Inequality: 100*Gini Coefficient	−0.051 (0.074)	−0.057 (0.078)	−0.105** (0.041)	−0.109** (0.048)	−0.052** (0.026)	−0.062 (0.042)	0.023 (0.072)	0.022 (0.065)	−0.115** (0.045)	−0.121** (0.048)
Unemployment Rate	0.135 (0.130)	0.191 (0.246)	0.365** (0.103)	0.461** (0.143)	0.098* (0.055)	0.179 (0.120)	−0.139 (0.127)	−0.131 (0.220)	0.213** (0.094)	0.273* (0.142)
Fixed Effects	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time
Serial Correlation (ρ_c)	0	0	0	0	0.095	0.199	0	0	0	0
R-squared	0.928	---	0.995	---	0.998	---	0.972	---	0.995	---
Number of Observations	1373	1373	346	346	346	346	336	336	345	345

TABLE 4 – VIOLENT CRIME REGRESSIONS

Variable	ln(All Violent Crime)		ln(Murder)		ln(Rape)		ln(Aggravated Assault)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
ln(Expenditure on Police)	0.182 (2.377)	-2.800 (25.047)	-0.034 (0.378)	-3.548 (4.783)	-1.412 (1.397)	26.883 (32.377)	0.604 (0.484)	-0.064 (6.639)
ln(Mean Family Income)	-0.030 (4.826)	-0.095 (4.891)	-0.808 (0.912)	-0.884 (1.058)	-2.399 (2.320)	-2.975 (5.110)	1.497 (1.372)	1.485 (1.371)
Income Inequality: 100*Gini Coefficient	-0.038 (0.119)	-0.042 (0.127)	-0.025 (0.022)	-0.030 (0.027)	0.028 (0.066)	0.065 (0.144)	-0.068** (0.033)	-0.069** (0.035)
Unemployment Rate	0.069 (0.225)	0.108 (0.346)	0.047 (0.034)	0.093 (0.077)	-0.123 (0.103)	-0.521 (0.498)	0.114** (0.053)	0.122 (0.101)
Fixed Effects	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time	State & Time
Serial Correlation (ρ_c)	0	0	0	0	0	0	0.145	0.142
R-squared	0.941	---	0.9996	---	0.995	---	0.997	---
Number of Observations	1038	1038	346	346	341	341	346	346

negative sign on the coefficient for mean family income. Both murder and aggravated assault show the wrong sign for the coefficient on income inequality, and for aggravated assault it is even statistically significant. Aggravated assault in fact fails to achieve the correct sign on *all three* of these coefficients. One possible reason for this is that aggravated assault is considered less serious than murder or rape and is less likely to be premeditated, so it might be more impulsive and thus less responsive to rational considerations. Another possibility is that as income rises and the opportunity cost of imprisonment increases, individuals might exercise more restraint and substitute away from the more serious crime of murder to the lesser crime of aggravated assault, where the expected punishment for conviction is far less severe.

C. Explaining the Anomalous Results for Income Inequality

While most of the regression results fit our model very well, the coefficients on income inequality generally run counter to what we had predictions. The results for income inequality, however, are clearly at odds with what we had predicted. Our expectation was that as income inequality increased, there would be more individuals with relatively more to gain and less to lose from crime, so the coefficient should be positive. Instead, we find precisely the opposite: the coefficient has a negative sign for all crime categories except larceny rape, and it is statistically significant for robbery, motor vehicle theft, and aggravated assault. At least four possible explanations exist for this anomalous result.

First, it might be that the form of our regressions prevents us from measuring changes in income inequality on the proper margin. In particular, the use of state-fixed effects in the previous models means that our estimates of the effect of income inequality on crime consider only year-to-year variation in income inequality within each state; any cross-sectional variation in income inequality among different states is absorbed entirely by the state dummies. But as the summary statistics in Table 3 show, about half of the variation in income inequality in our data set is cross-sectional variation, and much of the effect of income inequality might be neglected when we disregard this variation. Lee (1993) indeed finds strong evidence connecting crime rates to income inequality across states, despite finding no evidence linking changes in crime rates to changes in income inequality over time. While our regressions using state-fixed effects as controls were unable to make use of cross-sectional variation, it may be possible for future studies to make use of this variation by using a different scheme of control variables.

Another possibility is that our measure of income inequality, the Gini coefficient, doesn't really reflect the aspects of the wealth distribution that are of greatest importance for aggregate crime rates. The Gini coefficient takes into account the entire income distribution, and it is affected not only by changes in the gap between the very rich and the very poor, but also changes between adjacent social classes and even small changes within a social class. Yet it isn't these small differences in wealth that we should think significant enough to inspire criminal behavior, especially in the face of harsh punishments. Rather, we might think that the relationship between crime and resource distribution is dominated by the extremes. The potential gains from robbery or burglary will depend disproportionately on the assets of a community's wealthiest elites, who should be the obvious target for such a crime. Similarly, those individuals with the greatest incentive to turn to crime will be the community's very poorest members. This suggests that future studies might have more success by replacing the Gini coefficient with a measure of income inequality that is more sensitive to the extreme ends of the distribution.

We might alternatively think, as Freeman (1996) supposes, that the anomalous negative relationship we found actually reflects some other omitted variable. In particular, our proxies for wealth and for labor market opportunities are fairly crude, and Land et al. (1990) offer evidence that income inequality has a high level of colinearity with several other wealth indicators that might be the true source of the relationship we found. This again suggests that the expected results might appear if better measures of wealth or better control variables are used.

Finally, we might simply conclude that the reasoning which led from the rational crime model to the prediction that income inequality increases crime is tenuous and unsound. We should note that the predicted sign on income inequality was not a direct result of the model; rather, it is a prediction that likely, but not necessarily, follows from two other claims: first, an assumption that the wealth of the richest members of a community largely determines the prospective returns to crime in that community; and second, the prediction that poorer members of a community will be the most likely to resort to crime. Our findings do not necessarily contradict the more fundamental prediction in the second point, especially if the assumption about returns to crime being correlated with local wealth is wrong.

D. Implications of the Estimated Elasticities on Law Enforcement Expenditures

While the estimated coefficients on law enforcement expenditures are not terribly precise, as indicated by the relatively large standard errors, the point estimates represent our “best guesses” at the true elasticities, and we may consider what the policy implications of these estimates would be if they are accurate. In 2002, the most recent year for which data is available, approximately \$55.2 billion was spent on law enforcement in the United States, so a 1% increase in law enforcement expenditures would have a marginal cost of \$552 million. According to our estimates, this would result in about 5,185 fewer robberies, 16,400 fewer burglaries, 62,855 fewer larcenies, and 2,355 fewer motor vehicle thefts nationwide. We borrow Levitt’s (1997) estimates of the costs of crime and adjust for inflation, finding that each robbery has an average social cost of \$22,820, each burglary has an average cost of \$2,051, each larceny has an average cost of \$256, and each motor vehicle theft has an average cost of \$5,128. We calculate that, considering only these four property crimes, the marginal benefit of the 1% increase in police expenditures would be about \$180 million. This suggests either that policymakers believe law enforcement has an marginal social benefit with respect to violent crime which is roughly double that of property crime – roughly on the order of \$370 million – or that law enforcement expenditures are higher than the socially optimal level and thus inefficient.

These calculations are likely somewhat oversimplified. A more thorough cost-benefit analysis would have to take into account the costs of incarceration, which will depend on the precise mechanism by which law enforcement expenditures reduce crime. If this occurs through an incapacitation effect – police catch more criminals early and place them in jail, preventing repeat offenses – then prison costs will rise. If, on the other hand, the decrease in crime occurs through a deterrence effect – would-be criminals observe a greater expected punishment and decide not to partake in crime at all – then the number of individuals in jail will decrease and incarceration costs will fall. Expenditures on corrections in the United States totaled \$54.7 billion in 2002, so the effects of a change in incarceration rates could be tremendous. Distinguishing between deterrence and incapacitation, while very important for any comprehensive efficiency calculations, is a difficult problem and beyond the scope of this paper.

VI. CONCLUSION

While the results presented here often fall short of clear statistical significance and are not definitive, they are highly consistent with the rational theory of crime. For property crime in particular, nearly all estimates find a negative elasticity of crime with respect to increases in law enforcement expenditures. For all property crimes except larceny, we find that crime increases in response to increases in mean family income or unemployment. For violent crimes, the results are somewhat less decisive. There is limited evidence that murder, in particular, decreases both in response to increases in law enforcement expenditure and in response to increases in mean family income. It is unclear, though, whether this shows a true reduction in crime or if it simply indicates substitution away from murder toward less serious crimes such as aggravated assault. We also find that the use of an instrument to compensate for endogeneity bias is vital for obtaining consistent estimates of the elasticities. Our results do contradict the model's predictions for the effects of income inequality, and more research is needed to determine the cause of this. Aside from that anomaly, these results are all fully consistent with the findings of other leading papers in the field.

It should be noted that the findings here are somewhat limited in scope. Because our model includes state fixed effects, the estimates are based solely upon within-city variation over time, which tends to be small. The time period we consider is relatively short, so we must assume that it does not differ systematically from other time periods. Several of the factors we consider – mean family income, income inequality, and unemployment – might exhibit a much stronger effect across cities, in which case the elasticities estimated here might be understated. Our model also lumps together all fixed effects without distinguishing between the different social or demographic factors that they are composed of, and we do not determine whether the effects of rational considerations are large or small relative to these other factors.

There is ample room for further research on the economics of crime. The model presented here would benefit from additional observations, particularly over extended periods of time. Further studies might try to detect across-city effects or explore whether any important demographic factors change over time, both of which would indicate weaknesses in the fixed effects approach used here. While our aggregate data approach suffers from large standard errors, an analysis using individual data might be able to achieve more precise estimates. We can also imagine valuable data arising from large-scale experiments; for example, the federal government might provide a set of randomly-selected states with a grant of funds to be used exclusively for law enforcement. Further research on the economics of crime should help to determine if the mounting evidence that is consistent with the rational theory of crime, including the findings of this paper, correctly identify a significant relationship.

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