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Identifying the Effect of Unemployment on Crime

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Abstract

In this paper, we pursue several strategies to identify the effect of unemployment rates on crime rates. Using a state-level panel for the period from 1971 to 1997, we estimate the effect of unemployment on the rates of seven felony offenses. We control extensively for state-level demographic and economic factors and estimate specifications that allow for state-specific time trends as well as state and year fixed effects. In addition, we use prime defense contracts per-capita and a state-specific measure of exposure to oil shock as instruments for state unemployment rates. We find sizable and significant effects of unemployment on property crime rates that are stable across model specifications and estimation methodology. Our most conservative estimates suggest that nearly 40 percent of the decline in property crime rates during the 1990s is attributable to the concurrent decline in the unemployment rate. The evidence for violent crime is considerably weaker. However, a closer analysis of the violent crime of rape yields some evidence that the employment prospects of males are weakly related to state rape rates.

JEL Codes: J6, K42

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1. Introduction

In 1998, the total crime index calculated by the Federal Bureau of Investigation (FBI) fell for the seventh straight year. Moreover, between 1993 and 1998 victimization rates declined for every major type of crime (Rennison 1999), with both violent and property crime rates falling by approximately 30 percent. Occurring concurrently with these aggregate crime trends was a marked decrease in the civilian unemployment rate. Between 1992 and 1998 the national unemployment rate declined in each year from a peak of 7.5 percent to a thirty-year low of 4.5 percent.

The concurrence of these crime and labor market trends suggests that recent declines in crime rates may be due in part to the current abundance of legal employment opportunities. To the extent that increased legitimate employment opportunities deter potential offenders from committing crimes, a decline in the unemployment rate such as that observed during the 1990s may be said to cause the declines in crime rates. Despite the intuitive appeal of this argument, empirical research to date has been unable to document a strong effect of unemployment on crime. Studies of aggregate crime rates generally find small and statistically weak unemployment effects, with stronger effects for property crime than for violent crime.[1] In fact, several studies find significant *negative* effects of unemployment on violent crime rates, especially murder (Cook and Zarkin 1985).

[1]Reviewing 68 studies, Chiricos (1987) shows that fewer than half find positive significant effects of aggregate unemployment rates on crime rates. More recently, Entorf and Spengler (2000) using a state panel for Germany also find ambiguous unemployment effects. Likewise, Papps and Winkelmann (1998) find little effect for a panel of regions from New Zealand. On the other hand, research looking at the relationship between criminal participation and earnings potential finds stronger effects. Grogger (1998) estimates a structural model of time allocation between criminal, labor market, and other non-market activities and finds strong evidence that higher wages deter criminal activity. Further evidence supporting an effect of low wages is provided in a panel study of U.S. counties by Gould et. al. (1999) and a panel study of British Labor Market Areas by Machin and Meghir (2000). Willis (1999b) looks at the effect of minimum wages on property crime.

There are several reasons to suspect that the available evidence understates the effect of unemployment on crime. Given that much of the previous research relies on time-series variation in macroeconomic conditions, the failure to control for variables that exert pro-cyclical pressure on crime rates may downwardly-bias estimates of the unemployment-crime effect. For example, alcohol consumption varies pro-cyclically (Ruhm 1995) and tends to have independent effects on criminal behavior (Boyum and Kleiman 1995). Similar patterns may exist for drug use (Corman and Mocan, forthcoming) and gun availability. In addition, declining incomes during recessions reduces purchases of consumer durables and other possible theft-worthy goods, thus providing fewer targets for criminal activity. If one were only interested in the question "How much should we expect crime to rise in the next recession?" then the reduced form OLS estimates would suffice. However, to assess the effect of unemployment on propensity to engage in criminal activities (the crime supply function) we must statistically sort out these other effects.

An additional problem associated with interpreting the empirical relationship between unemployment and crime concerns the direction of causation. To the extent that criminal activity reduces the employability of offenders, either through a scarring effect of incarceration or a greater reluctance among the criminally-initiated to accept legitimate employment, criminal activity may in turn contribute to observed unemployment. Moreover, crime level may itself impede employment growth and contribute to regional unemployment levels.[2] Hence, in addition to problems associated with omitted variables, previous

[2] Bound and Freeman (1992) and Nagin and Waldfogel (1995) find that conviction and incarceration increases the probability of future unemployment. Grogger (1995) finds small and short-lived employment impacts of arrests. Willis (1999a) finds that business formation and location is sensitive to local crime rates. Freeman et. al. (1996) present a multiple-equilibrium model where an exogenous increase in crime reduces the probability of getting caught, thus altering the returns to criminal activity

inferences may also be flawed due to simultaneity bias.[3] To be more precise, simultaneity upwardly biases OLS estimates of the causal effect of unemployment on crime.

In this paper we estimate the effect of unemployment rates on crime rates using a state-level panel covering the period from 1971 to 1997. We first use OLS regressions to estimate the effect of unemployment rates on the rates of the seven felony offenses recorded in the FBI Uniform Crime Reports (UCR). To mitigate omitted-variables bias, we take two precautions: (1) we control extensively for observable demographic and economic variables, and (2) we exploit the panel aspects of our data by estimating models that allow for state and year fixed effects as well as state-specific linear and quadratic time trends. In addition, we present two-stage-least-squares (2SLS) estimates using state military contracts and a measure of state exposure to oil shocks as instruments for unemployment rates. For property crime rates, the results consistently indicate that unemployment increases crime. The magnitude of these effects is stable across specifications and ranges from a 1 to 5 percent decline in crime caused by a one percentage point decrease in unemployment. For violent crime, however, the results are mixed with some evidence of positive unemployment effects on robbery and assault and the puzzling findings of negative unemployment effects for murder and rape.

In an attempt to resolve this latter paradox, we exploit the specific features of rape offenses. A real behavioral effect of unemployment on the propensity to commit violent acts may be statistically veiled

relative to legitimate opportunities.

[3]Simultaneity between crime and unemployment has been addressed in time series studies by Corman et. al. (1987) and Bushway and Engberg (1994). Whereas the former find no Granger causality in both directions using monthly data for New York City, the latter find two-way Granger causality using annual time series for 103 counties in Pennsylvania and New York from 1976 to 1986.

by the effect of pro-cyclical variation in the degree of interpersonal exposure of possible victims to potential offenders. This greater exposure may result from the fact that when more people are working and away from home, the quantity of encounters with potential offenders increases. Noting that in the overwhelming majority of rapes recorded in the UCR the perpetrator is male while the victim is always female, we first test for an empirical relationship between the rape rate and female unemployment rates. To the extent that a negative relationship still exists, we can be certain that the negative correlation between female unemployment and rape does not reflect the behavior of offenders but rather some other omitted factor that varies with regional employment cycles, such as an increase in the quantity of interpersonal interactions. Next, we add female unemployment rates to model specifications of the rape rate that include male unemployment rates. Here, the female unemployment rate serves as a control for all omitted factors not captured by the other control variables. The results from this exercise generally indicate that after controlling for female unemployment rates the effect of male unemployment rates on rape are either positive or insignificant.

2. Unemployment, Crime, and Time Allocation

The proposition that unemployment induces criminal behavior is intuitively appealing and grounded in the notion that individuals respond to incentives. Conceptualizing criminal activity as a form of employment that requires time and generates income (Witte and Tauchen 1994), a "rational offender" should compare returns to time use in legal and illegal activities and make decisions accordingly. Holding all else equal, the decrease in income and potential earnings associated with involuntary unemployment increases the relative returns to illegal activity.

To more formally illustrate the relationship between unemployment and crime, Figures 1A and 1B present a model of time allocation following that of Grogger (1998). In Figure 1A, the individual has discretion over *A* hours of time and non-labor income equal to the distance AB. The person converts non-market time into income by either engaging in legitimate employment or income-generating criminal activity. The returns to crime are diminishing and are given by the curved segment BCE. Diminishing returns follows from the assumption of rational choice: individuals first commit crimes with the highest expected payoffs (lowest probability of getting caught and highest stakes) before exploring less lucrative opportunities. Assuming that the returns to allocating a small amount of time to criminal activity exceed potential wages, the individual would supply time to the legitimate labor market only after higher-paying criminal opportunities have been fully exploited. This occurs at the point C where the person has allocated A-$t0$ time to crime and where the marginal return to crime equals potential wages. Beyond point C, wages exceed the returns to criminal activity (as is evident by the steeper slope of the budget constraint segment CD).

The budget constraint differs from that of a standard model of the labor-leisure choice in its implicit recursive structure. The individual first locates the point that equates the marginal returns to legitimate and illegitimate activities. Time allocations to the right of this point involve criminal activity only, while time allocations that exceed this level (to the left of t0) involve a mix of work in the legitimate market and time supplied to criminal activity. When there are no barriers to employment, the budget constraint is given by ABCD. In Figure 1A, the individual maximizes utility by devoting A-$t0$ time to criminal activity and supplying $t0$-$t1$ time to the labor market. For those for whom the returns to crime never exceed potential wages in the legitimate labor market, the budget constraint is simply that of the standard labor-leisure model. This is depicted in Figure 1B where the marginal income generated by criminal activity (given by the curve BD) is always less than the income generated by an additional hour of legitimate work (line BC).

This model can be used to illustrate how unemployment affects crime rates by analyzing the possible behavioral responses to an unemployment spell. For individuals with relatively low potential wages (initial returns to crime exceed wages), unemployment shifts the budget constraint from ABCD to ABCE. Whether this increases time allocated to criminal activity depends on the individual's preferences. For the person depicted in Figure 1A, such a shift unambiguously increases the time devoted to criminal activity. Since the optimal time allocation decision in the absence of unemployment occurs to the left of point C, the indifference curve representing the utility level at point C (U1) crosses the budget constraint with a relatively flatter slope – i.e., the marginal rate of substitution between non-market time and income at point C is less than the marginal rate at which the individual can convert time into income via both legitimate and illegitimate activity. For both constraints ABCD and ABCE, this individual will sacrifice more non-market time than the amount given by A-$t0$. Hence, for persons that engage in criminal activity while working, the model predicts that unemployment increases time allocated to crime.[4] On the other hand, an individual facing the constraints in Figure 1A who engages only in criminal activity (or engages in neither legitimate nor illegitimate activities), unemployment does not affect the time allocated to crime.

For those workers with wages that always exceed the marginal return to crime, unemployment shifts the budget constraints in Figure 1B from ABC to ABD. Here, whether or not the individual commits

[4]Grogger's work (1998) suggests that a substantial minority of employed out-of-school youths engage in some income-generating criminal activity. In an analysis of NLSY data, Grogger finds that nearly a quarter of the employed youths self-report committing crimes.

crime as a results of the unemployment spell depends on whether the return to the initial hour of criminal activity exceeds her reservation wage. Individuals with relatively high reservation wages will be unlikely to commit crimes as a result of an unemployment spell. On the other hand, individuals with relatively low reservation wages are more likely to attempt to offset income lost due to unemployment through criminal activity.

In sum, the theoretical model yields four possible types of individuals roughly defined by potential earnings in the labor market relative to the returns to criminal activity and preferences over income and non-market time. The theory predicts that for two of these four categories an unemployment spell will increase time allocated to criminal activity (and thus increase the crime rate) while for the remaining two categories there is no response to an unemployment spell. In the aggregate, while the relationship between unemployment and crime rates should be unambiguously positive, the magnitude of this relationship depends on the distribution of the unemployed across these four categories. This is an empirical question to which we now turn.

3. Empirical Strategy and Data Description

Our empirical strategy is to use a state-level panel data set to test for a relationship between state unemployment rates and the rates of the seven felony offenses. Our panel covers the period from 1971 to 1997 for the 50 states (Washington D.C. is excluded).[5] Since the main empirical tests rely on the aggregate reduced-form relationship between state unemployment rates and state crime rates, isolating the

[5]For several states in the early 1970s, we are missing data on several explanatory variables. Hence, rather than having 1,350 observations for the 27 year period we have 1,293 observations.

effect due to a behavioral response of the unemployed (that is to say, additional crimes committed by those suffering unemployment spells) requires careful consideration of other factors that vary systematically with regional business cycles and that affect crime rates.

Cook and Zarkin (1985) suggest four categories of factors that may empirically link the business cycle and crime: (1) legitimate employment opportunities, (2) criminal opportunities, (3) consumption of criminogenic commodities (alcohol, drugs, guns), and (4) the response of the criminal justice system. The crime effects of access to legitimate opportunities were the subject of the previous section and are tautologically pro-cyclical. The factors listed in the latter three categories are also likely to vary with the business cycle. The quality and quantity of criminal opportunities may be lower during recessions as potential victims have less income, consume less, and expend more effort on protecting what they have. If alcohol, drugs, and guns are normal goods, consumption of these goods will be pro-cyclical. Furthermore, if these commodities induce criminal behavior, or in the least augment the lethality of criminal incidents, pro-cyclical consumption will induce pro-cyclical variations in some crimes.[6] The extent of variation in policing and criminal justice activity over the business cycle is less clear since the quantity and efficacy of criminal justice activity depends on state tax revenues, community cooperation, and political pressures (Levitt 1997).

Omission of any of these factors from aggregate crime regressions may bias the estimates of the

[6]The effects of guns, drugs, and alcohol on violent and property crime is a matter of some debate. Cook and Moore (1995) note that while guns do appear to increase lethality of criminal acts, the evidence concerning the effect of gun availability on the overall level of crime is mixed. Concerning drugs and alcohol, in behavioral experiments alcohol is more consistently found to lower inhibitions and increase aggressive behavior (Boyum and Kleiman 1995). Evidence concerning the pharmacological effects of illegal drugs are mixed with drugs such as marijuana being more likely to reduce aggressive behavior (Fagan 1990).

relationship we seek to measure. For example, assuming that the consumption of drugs and alcohol is negatively correlated with unemployment and positively correlated with crime, omitting these factors from the regression would bias estimates of the unemployment-crime effect downward. Similarly, pro-cyclical variation in criminal opportunities would also create a downward bias. To mitigate such omitted-variables bias, we control extensively for observable state-level covariates and exploit the panel aspects of our data set to net out variation in crime rates due to unobserved factors. The most complete model specification that we estimate is given by the equation

$$Crime_{it} \text{ ' } a_t \% d_i \% ?_i time_t \% ?_i time_t^2 \% ?Unemployed_{it} \% ßX_{it} \% ?_{it}, \qquad \textbf{(1)}$$

where i and t index states and years, $Crime_{it}$ is the log of the number of crimes per 100,000 state residents, $Unemployed_{it}$ is the unemployment rate, X_{it} is a vector of standard controls, a_t is a year fixed effect, d_i is a state fixed effect, $time_t$ and $time_t^2$ are linear and quadratic time trends, $?_i$ gives the state-specific coefficient on the linear trend while $?_i$ gives the state-specific coefficient on the quadratic time trend, ? is the semi-elasticity of the crime rate with respect to the unemployment rate, ß is the vector of parameters for the control variables in X_{it}, and $?_{it}$ is the residual.

We explicitly control for several variables. First, to account for pro-cyclical consumption of criminogenic commodities, we include a measure of alcohol consumption per capita (measured in gallons of ethanol) and the average income per worker (personal income divided by employment) for each state-year. While we would like to directly control for drug consumption and gun availability, these data are unavailable. Hence, we use income per worker to proxy for variation in consumption of criminogenic commodities.[7] We also include controls for the proportion of state residents that are black, living in poverty, and residing in metropolitan areas. To adjust for the effect of age structure on aggregate crime rates, we include seven variables that measure the distribution of the state population across age categories. Given the well-documented age-crime profile (Greenberg 1985, Grogger 1998, Hirshi and Gottfredson 1983), these controls are needed to insure that estimates of the crime-unemployment effect are not contaminated by changes in state age structures.

Finally, we include the incarceration rate in state prisons in all models. A positive effect of unemployment on crime is likely to lead to a positive correlation between unemployment and prison populations (assuming that some offenders are caught and sent to prison). If incarceration reduces crime rates via incapacitation and deterrence (a proposition supported by Levitt, 1996), omitting incarceration rates from equation (1) would downwardly bias the unemployment-crime effect. In all models we enter prison populations per 100,000 state residents measured in logs.

To be sure, our list of control variable is likely to be incomplete as it is impossible to observe all factors that affect crime and vary with regional cycles. To adjust further for unobservable variables, we exploit the panel aspects of our data set. By including state effects we eliminate all variation in crime rates caused by factors that vary across states yet are constant over time, while the inclusion of year effects eliminates the influence of factors that cause year-to-year changes in crime rates common to all states. State specific linear and quadratic time trends (following Friedberg 1998) eliminate variation in crime rates *within-state* caused by factors that are state specific over time. In these models, the unemployment-crime

[7]We also estimated all of our models using income per capita rather than income per worker. This did not change the results.

effects are identified using within-state variation in the unemployment rate (relative to the national rate) after netting out state-specific time trends. This is a particularly flexible specification that should certainly eliminate the influence of many unobserved factors.

An alternative approach that addresses omitted-variables bias would be to find instrumental variables that determine state unemployment rates yet are unrelated to possible contaminating omitted factors and to re-estimate Equation (1) using 2SLS. This approach carries the added benefit that the direction of causality is clearly established. As discussed above, the direction of causation may run from crime to unemployment. This would be the case if (former) criminals become unemployable, or if high crime rates discourage employment growth and drive away existing firms thus contributing to a state's unemployment rate.

Hence, to rule out reverse causation we estimate the crime-unemployment relationship using the specification discussed above but by instrumenting state unemployment rates. We employ two instruments: Department of Defense (DOD) annual prime contract awards to each state and a state-specific measure of oil price shocks. The annual prime contract awards are measured in thousands of dollars per capita. Our measure of state-specific oil price shocks is constructed as follows. For each state and each year we start with a variable measuring the proportion of employment in the manufacturing sector, MAN_{it}. This provides a rough measure of the importance of energy intensive industries where fuel costs are likely to be a relatively substantial component of production costs. Next, following Hooker and Knetter (1997) we construct an annual variable indicating changes in the relative price of crude oil, OIL_t, by dividing the producer price index for crude oil by the GDP deflator. Multiplying these two variables provides our measure of state-specific exposure to oil shocks ($Oil\ Costs_{it} = MAN_{it} * OIL_t$). The effects of both the prime contracts and oil costs variables on state unemployment rates have been well-documented by past research (Blanchard and Katz 1992, Hooker and Knetter 1994 and 1997, Davis et. al. 1997).

To be valid, the instruments must be exogenous determinants of unemployment rates and cannot be correlated with any omitted variables contained in the residual of the second-stage crime equation. Both variables appear to be exogenous determinants of unemployment. Oil prices are determined on world markets and hence should not be influenced by the unemployment rate in any one state and year. Moreover, it is unlikely that state unemployment rates affect the industrial structure of a state's employment base, though causation may clearly run in the opposite direction.

The question of whether defense spending exogenously determines unemployment rates boils down to the issue of whether the defense appropriations process is influenced by fiscal policy concerns. At the national level this does not appear to be the case.[8] However, even if national defense spending is affected by national unemployment rates, including year fixed effects in the crime model specification will eliminate any contamination of the instrument from this source. A more important issue concerns whether the spatial distribution of contract awards, holding aggregate appropriation constant, are determined in part by deviations in state unemployment rates from the national rate. Davis et. al. (1997) cite several detailed case

[8]Davis et. al. (1997) show that major shifts in defense spending strongly coincide with international developments affecting national security (the onset of the cold war, the military build-up under Carter and Reagan, and the defense cutbacks driven by the end of the cold war) rather than the national unemployment. In addition, Mayer (1991, pp. 183) presents a convincing argument that the defense appropriations process renders altering defense spending for fiscal policy purposes quite difficult, noting (1) the appropriation process is long, often extending two years or more between initial DOD requests and congressional approval, (2) major portions of the defense budget are uncontrollable since they are determined by the size of the armed forces, pay scales, and other factors that are immutable for political purposes, and (3) the delay between congressional approval and the obligation of funds (the action that creates employment (Greenberg 1967)), is lengthy and may occur several years after budget adoption.

studies indicating that this is unlikely. Hence, here we will follow the lead of recent macroeconomic and regional economic research and assume that state-level contract awards are exogenous with respect to state unemployment rates.

Whether our instrumental variables are correlated with unobserved determinants of crime rates that are swept into the second stage residuals is a more difficult question. For unobserved determinants that are spuriously correlated with unemployment rates, this is unlikely to be a problem. However, if certain omitted factors are themselves determined by unemployment rates (for example, drug consumption or gun availability), our instruments will be correlated with the second stage residuals. One would expect that unemployment affects the consumption of criminogenic substances, as well as the consumption of durable goods that provide criminal opportunities. If our control variables eliminate variation caused by these factors (alcohol consumption, income per worker, and various fixed effects and state trends), our 2SLS results should be valid. Nonetheless, we acknowledge this potential shortcoming.

The data for this project come from several sources. State data on seven felony offenses (murder, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft) come from the FBI's Uniform Crime Reports (UCR). The annual incidence of these seven offenses (expressed per 100,000 state residents) are the primary dependent variables of interest along with the total property crime (the sum of burglary, larceny-theft, and motor vehicle theft) and the total violent crime rates (the sum of murder, forcible rape, robbery, and aggravated assault). Annual data for state population and age structure are from the Bureau of the Census. State poverty rates, the proportion black, and the proportion of the state population living in metropolitan areas are from the decennial censuses for census years and are interpolated for years between 1970, 1980 and 1990, and projected forward for 1991 to 1997. These data, compiled by Thomas B. Marvell, have been used in the past to study the crime effects of enhanced prison terms (Marvell & Moody 1995) and state determinate sentencing policies (Marvell & Moody 1996).

State unemployment rates from 1976 to 1997 for all states and from 1971 to 1997 for the ten largest states come from the Current Population Survey Geographic Profile of Employment and Unemployment. The remaining unemployment figures are constructed from BLS unemployment rates for Labor Market Areas. Data for state personal income come from the Bureau of Economic Analysis while data on total employment and manufacturing employment come from the Bureau of Labor Statistics. Data on per-capita alcohol consumption comes from the Alcohol Epidemiological Data System maintained by the National Institute of Alcohol Abuse and Alcoholism, while data on state prison populations come from Bureau of Justice Statistics. Finally, data on prime defense contracts awarded to individual states come from Hooker and Knetter (1997).[9]

Table 1 presents summary statistics for all variables. The first column provides means, the next column provides standard deviations, while the final column provides the standard deviations net of state and year fixed effects.[10] Property crime is far more common than violent crime, with the highest crime rate being that for larceny (2,883 incidents per 100,000 persons) and the lowest crime rate being that for murder (9 incidents per 100,000 persons). As can be seen by comparing the figures in the second and third columns, much of the variation in crime rates is eliminated by controlling for state and year effects,

[9]Since all 2SLS models estimated below include year dummy variables, we do not convert military expenditures to constant dollars. Doing so does not effect the results.

[10]All figures in Table 1 are weighted by state populations as are all results presented below.

though much remains. The standard deviations after netting out inter-state variation and the national year-to-year changes are roughly 20 to 40 percent the base standard deviations in the second column. Allowing for these effects only eliminates half of the variation in state unemployment rates. For the more stable, slower changing variables (age structure, poor, black) netting our state and year effects eliminates a considerably larger portion of the variance.

4. Empirical Results

In this section we present our main results. First, we present OLS estimates of the crime-unemployment effects for the total property and total violent crime rates followed by results for each of the seven individual felony offenses. Next, we present comparable results instrumenting for state unemployment rates. For all crimes, we estimate three models: models including state and year effects, models including state effects, year effects, and state-specific linear trends, and models including state effects, year effects, and linear and quadratic trends. In addition, all specifications include the variables (with the exception of the two instruments) listed in Table 1.

OLS Regression Results

Table 2 presents regressions where the dependent variable is either the log of the total property crime rate or the log of the total violent crime rate. The first three columns provide the results for property crime while the next three columns provide the results for violent crime. In all property crime models, the effect of unemployment is positive and significant at the one percent level of confidence. The magnitude of the relationship indicates that a one percentage point drop in the unemployment rate causes a decline in the property crime rate of between 1.6 and 2.4 percent.

The results for violent crime are mixed. In the first specification, the coefficient is small and insignificant. Adding linear time trends increases the point estimate of the unemployment coefficient yet the variable is still insignificant at the 10 percent level (p-value=0.18). Finally, adding the quadratic time trends to the model increases the point estimate further and the coefficient is now significant at the 5 percent level of confidence. The fact that controlling for state-specific trends increases the coefficient on unemployment suggests that the state-specific crime trends driven by the omitted crime fundamentals tend to move in the opposite direction of the trends in unemployment rates over the time period covered by the panel.[11] For the one specification where unemployment exhibits a positive significant effect, the magnitude is considerably smaller than the comparable estimate for property crime. The results in column (6) indicate that a one percentage point decline in the unemployment rate causes a decline in the violent crime rate of one half of a percent.

Concerning the performance of the other variables listed in Table 2, prison incarceration rates generally exert negative effects on crime rates. These effects are significant for all of the property crime models but for only the final violent crime model.[12] Alcohol consumption is positive and significant in only

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[11]A simple statistical model illustrates this point. Suppose that for a two-state panel the true model is given by, $Crime_{it}$ = a + ß$Unemployed_{it}$ + ?$_1 time_t$ + ?$_2 time_t$ + e_{it}, but we estimate the mis-specified model, $Crime_{it}$ = a + ß$Unemployed_{it}$ + ?$_{it}$, omitting the time trends. The probability limit of the OLS estimate is given by, ß$_{OLS}$= ß + cov($Unemployed_{it}$, $time_t$)/ var($Unemployed_{it}$)*(?$_1$ + ?$_2$), where the bias due to omitting the trends is given by the second term in the equation. If unemployment is trending upwards (cov($Unemployed_{it}$, $time_t$)>0) and the predominant state trend in crime rates is negative (?$_1$ + ?$_2$ < 0) then the OLS coefficient estimate will be biased downwards (similarly if unemployment trends downwards and crime upwards). Another instance where allowing for linear and quadratic trends in state panel data yields a significant effect for an otherwise insignificant variable is found in Friedberg (1998). Investigating the effect of unilateral divorce laws on state divorce rates, the author finds that adding state trends yields significant effects that were not present in model specifications including state and year effects only.

[12]These effects are smaller than those found by Levitt (1996). However, unlike the study by Levitt we have made no attempt to address the simultaneity bias to OLS estimates of the crime-prison elasticity.

one of the property crime models and one of the violent crime models. This effect is knocked out by including the state time trend variables. In all models crime rates tend to be higher in states with larger metropolitan populations while there are no consistent patterns for the relationship between crime rates and either the proportion poor or the proportion black. Consistent with previous research on the age-crime profile, both property and violent crime rates are higher in states with higher proportions of their populations that are teenagers and young adults.

Income per worker exhibits negative effects on both property and violent crime rates and is significant in all models with the exception of the property crime model presented in column (3). Recall, we included this variable in an attempt to proxy for income effects on the demand for criminogenic substances, and hence, expected to see positive coefficients. These consistent negative effects suggest that the variable may be picking up the effect of an alternative dimension of legitimate labor market opportunities, namely earnings.

Table 3 presents separate estimates of the crime-unemployment effects for the seven specific crimes using the same three specifications. For reference, the results for the total property and violent crime models are reproduced. Since the results for the other control variables do not differ substantially from the patterns presented in Table 2, we suppress this output in this and all remaining tables. Starting with the three individual property crimes, the unemployment rate exerts positive and statistically significant effects (at the one percent level of confidence) in all models with the exception of the auto theft regression omitting the state-specific trends. The magnitudes of the effects are very stable across specifications again with the exception of auto theft. For the auto theft rate, adding the trend variables drastically increases the magnitude and significance of the unemployment rate, which points to a specific trend pattern in auto theft rates over time as compared to other crime rates. For the most complete specification, the crime-unemployment semi-elasticities are quite similar across offenses. A one percentage point decrease in the unemployment rate causes a two percent decrease in burglary, and 1.5 percent decrease in larceny, and a one percent decrease in auto theft.

The results for the specific violent crimes are considerably more variable. The coefficient on unemployment is *negative* for all three murder models and significant in the first two, though adding the linear and quadratic time trends drastically reduces the magnitude of this effect. The results for rape are unstable across specifications with a positive significant effect in the first specification, an insignificant effect when linear trends are added, and a puzzling negative and significant effect when both linear and quadratic time trends are included in the model. The results for robbery are stronger, with no significant effect when time trends are omitted and significant (at one percent) positive effects in the two models that include trends. The magnitude of the robbery-unemployment effects in the last two models are similar to the property crime effects, a reassuring finding considering that robbery, while a violent crime in nature, is motivated by the desire to steal someone else's property. Finally, unemployment is insignificant in all three assault rate models.

To summarize, we find positive and highly significant effects of unemployment on property crimes, both in the aggregate and for individual offenses. The magnitudes of these effects are generally consistent across specification.[13] The results for violent crime are considerably weaker. For the two most serious violent crimes of murder and rape, the effect of unemployment is either significant and wrongly-signed or

[13]The relative importance of these effects in explaining recent changes in crime rates is a question to which we will return in the conclusion.

is unstable across specifications, while there are no measurable effects on the rate of assault and some evidence of a positive unemployment effect for robbery.

2SLS Results

In this section, we present 2SLS estimates of the crime-unemployment semi-elasticities using military contracts and a state-specific measure of oil costs as instruments for the state unemployment rate. Recall, if our model specifications omit crime-determining factors that are correlated with unemployment and that are not picked up by the fixed effects and trends variables, the OLS results that we have presented thus far will be biased. Moreover, if crime rates reverse-cause unemployment rates, inferences from OLS results will be flawed.

Before discussing estimates of the unemployment effects, an evaluation of the strength of the first-stage relationship is needed. Table 4 presents the results from three first-stage regressions of unemployment on the military spending and oil costs variables. While the table only presents the coefficients for the two instruments, all of the control variables listed in Table 1 are included in the specification. In all models, military spending negatively affects the unemployment rate. This effect is significant at the one percent level in the first two specifications, but is insignificant in the final specification. As expected, the oil costs variable exerts a strong positive effect on unemployment that is highly significant in all three specifications. The results from F-tests of the joint significance of the two instruments are presented in the final row. For all models, the two variables are jointly significant at the 0.0001 level of confidence. Hence, with the exception of the military spending variable in the final specification, the first-stage relationships are fairly strong.

Table 5 presents the 2SLS estimates of the unemployment-crime effects for total property and violent crime and for each of the seven individual crimes. Again, we only report the unemployment coefficients and standard errors. For reference, we reproduce the OLS results from Table 3 for the three specifications. Since we have two instruments, we can perform a test of the implicit over-identification restriction in each model. The results of these tests are represented by the presence of an asterisk (following the coefficient estimate) indicating tests where the restriction is rejected at the 5 percent level of confidence. A rejection of the over-identification restriction indicates that the 2SLS estimates are sensitive to the choice of instruments.

Similar to the OLS results, unemployment exerts consistent, positive, and highly significant effect on the total property crime rate. For all specifications, the 2SLS results exceed the OLS results. While the estimates from OLS range from 1.6 to 2.3, the comparable range for the 2SLS results is 2.8 to 5.0. In contrast to the OLS findings, the strongest unemployment effect from the 2SLS models occurs in the most complete specification. For all 2SLS specifications of the total property crime models, the over-identification test fails to reject the restriction, thus indicating that these results are not sensitive to the choice of instruments.

Concerning individual property crimes, the pattern is fairly similar with a few exceptions. For the burglary rate, the 2SLS results are positive and significant at one percent in the first and third specification, while for larceny the 2SLS results are positive and significant in all regressions. Again, when significant, instrumenting yields stronger unemployment effects relative to OLS. For the first two auto theft models, the unemployment effects are positive yet insignificant. In the final specification however, unemployment exerts a large positive effect that is significant at the 5 percent level. Of the nine individual property crime models estimated, the over-identification restriction is rejected in only two (the first specification for auto theft and burglary). Hence, we interpret the findings for property crimes in Table 5 as strongly reinforcing the OLS results.

On the other hand, the 2SLS results for the violent crime models are not so strong. Unemployment is insignificant in all three estimates of the total violent crime models. For murder, the 2SLS unemployment effects are even more negative than those from the OLS regression. A similar pattern is observed for rape. For the two specifications where we find positive OLS unemployment effects for robbery, instrumenting yields a negative significant effect for the first (including linear time trends only) and a positive insignificant effect for the second (including linear and quadratic time trends). The one specification where the 2SLS model yields a positive significant unemployment effect is for the final specification of the assault rate. Here the instrumented point estimate exceeds the OLS estimate considerably and is significant at the 5 percent level.

5. Are the Unemployed Less Violent?

The results presented in the previous sections paint a consistent portrait of the relationship between unemployment and property crime that confirms the simple theoretical arguments that we offer. While the magnitude of the relationship depends to a certain degree on the estimation method used, higher unemployment unambiguously increases property crime rates. The same, however, cannot be said for violent crime. In fact, for the two most serious violent crimes (murder and rape) the estimated effects of unemployment are strongly negative.[14] Interpreting these results literally would indicate that an

[14]Note that these counterintuitive results are very common in the literature.

unemployment spell decreases one's propensity towards violence. While possible, this seems unlikely considering the results for property crime rates and the possibility that violence may be a byproduct of economically motivated crimes. An alternative interpretation of these puzzling results is that in both our OLS and 2SLS models, we have failed to account for some violence-creating factor that varies systematically with unemployment rates.[15] One candidate would be the greater frequency of interactions between potential victims and offenders when a larger proportion of the population is working.

While in the previous section we attempted to address this issue through extensive controls and by employing instrumental variables, here we take an alternative tack in an attempt to resolve the counter-intuitive results for one of the violent crimes studied above. Specifically, we exploit the fact that for the crime of rape we can separately identify the unemployment rate of the offending and victimized populations. In the UCR, the count of reported forcible rapes is limited to incidents involving female victims. Of those incidents,[16] victimization survey results indicate that the offenders are males in over 99.5 percent of the cases. Moreover, arrest data indicates that over 99 percent of those arrested for forcible rape are male (U.S. DOJ 1997). Hence, for the most part, the offending population is male while the victimized population is female.

We use this information in the following manner. Since women are not among the offenders, a possibly negative relationship between state rape rates and female unemployment rates must be attributable

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[15]Recall, if unemployment is itself creating variation in relevant factors that we cannot observe, even our 2SLS estimates will be biased.

[16]Data from U.S. victimization surveys indicates that females are victims in 91.3 percent of reported cases. For the 8.7 percent where males are victims, 0.2 percent involve a female offender and 8.5 percent involve a male offender (U.S. DOJ 1997).

to factors other than a criminal behavioral response by women. Hence, if the empirical findings using female unemployment rates parallel those using aggregate unemployment rates, the omitted-variables interpretation is the correct one. Moreover, having identified a non-offending population, the unemployment rate for this population can be used as an added control to estimate the behavioral relationship between the unemployment rate of the offending population and the state rape rate.

Table 6 presents the results from this exercise. Here we use gender-specific unemployment rates taken from the Current Population Survey Local Area Unemployment Statistics Geographic Profile Series. Unfortunately, 1981 is the earliest year for which these data are available. To explore this relationship in full, we present results using gender-specific employment-to-population ratios as well as unemployment rates. The first four regressions in each panel correspond to the specification omitting trends, the next four add linear trends, while the final four add the quadratic trends. Again, all of the variables listed in Table 1 are included in all models.

Starting with the unemployment models in Panel A, the regression in columns (1), (5), and (9) present estimates for the aggregate unemployment rate. The pattern is similar to the results for the longer time period in Table 3. When the trends are omitted there is a positive yet insignificant unemployment effect (0.674), adding the linear trends yields a negative insignificant estimate (-0.305), while adding the quadratic trends yields a negative and significant (at 5 percent) estimate of unemployment on rape (-0.937). Columns (2), (6), and (10) present similar models where the female unemployment rate is substituted for the aggregate rate. The pattern is quite similar, with insignificant estimates for the first two specifications and a negative and significant point estimate in columns (10) of -0.914. Hence, the same pattern exists using the unemployment rate for a non-offending population.

Columns (3), (7), and (11) use the male unemployment rate instead. Here, the first specification yields a positive significant effect while the second and third specifications yield insignificant effects. The point estimates for male unemployment are consistently larger than those for the female unemployment and total unemployment rates.

Finally, in columns (4), (8), and (12), we add both the male and female unemployment rates to the specification. In all three regressions, the coefficient on female unemployment is negative. Moreover, these effects are significant in the first and third regressions. For male unemployment rates, all coefficient estimates are positive with a significant effect (at the one percent level) in the first specification (column (4)). Adding female unemployment rates increases the point estimate on the male unemployment coefficient in all models. Hence, the results from panel A yield more sensible findings for rape than those from the previous section: rather than being unrelated or negatively related to rape, the effects on rape of the unemployment rate of the offending population are generally positive and sometimes significant.

Panel B presents comparable results where employment rates are substituted for unemployment rates. Here, the “correct” sign would be negative. Using the aggregate employment rate in columns (1), (5), and (9), we consistently find employment effects of the wrong sign. In all specifications, employment exerts a positive and significant effect on rape. Hence, the perverse results are even stronger using employment rates. In the models that substitute female employment rate for the aggregate rate, there is a weakly significant positive effect in the first specification, and insignificant positive effects in the last two specifications. In contrast, the first two specifications of the model including male employment rates only yield weakly significant negative effects of male employment on rape rates, while in the final specification the point estimate is effectively zero.

Finally, controlling for both male and female employment rates simultaneously yields results similar to the comparable models using the unemployment rates. The coefficients on male employment become larger (more negative) and are significant at the one and five percent level in the first and second specification, respectively. In the final specification, the point estimate is still small and insignificant. Finally, for the first two specifications, female employment rates exert positive significant effects while in the third specification the variable is insignificant.

In sum, the strategy pursued in this section indicates that the "perverse" unemployment coefficients for some violent offenses are caused by omitted variables bias. One possible interpretation would be that in good times exposure to offenders is higher thus masking the negative effect of unemployment on the propensity to commit violent crimes. In the case of rape we can show that the employment prospects of males are weakly related to rape rates. Most importantly, the results for female unemployment rates indicate that the negative significant unemployment effects observed in Table 3 results from model mis-specification. While this strategy cannot be applied to murder rates due to fact that there is not a similarly clear distinction between offenders and victims, the results for rape suggest that a similar fix may yield findings in contrast to those presented above and may therefore solve this puzzle which is very common but unresolved in the literature.

6. Conclusion

The results presented here consistently indicate that unemployment is an important determinant of property crime rates. The strong effects on property crimes exist in models of aggregate property crime as well as models of the individual felonies. Moreover, the results for property crimes do not depend on the estimation methodology used, although we do find relatively stronger effects when we instrument for state unemployment rates. Hence, the results of this paper strongly confirm a basic economic model of the determination of property crimes.

We did not find such consistency for violent crimes. In our OLS results, we find some evidence that the economically-motivated violent crime of robbery is positively effected by unemployment rates. This finding, however, is not reproduced when we instrument for unemployment. For the crimes of murder and rape, our initial results indicate that unemployment is negatively related to these crimes. Upon closer examination of the rape models, however, this paradoxical results vanishes. These findings for rape cast doubt on a behavioral interpretation of the observed negative effects on murder – i.e., being unemployed reduces one's tendency to become violent and murder someone.

In the opening paragraphs, we cite the recent downward trends in crime occurring during the 1990s. To put our results into perspective, it is instructive to work through how much of the recent declines can be explained by the decline in unemployment rates assuming that our estimation results are valid. Since our findings for rape indicate (1) that the unemployment effect on rape is weakly positive or insignificant, and (2) OLS estimates of the violent crime-unemployment relationship appear to be downwardly biased by omitted factors, we can assume that the unemployment effects on both murder and rape are zero. Moreover, since the estimation results generally indicate that the unemployment effect on assault is zero, we also omit this crime rate from these simple simulations. To present conservative estimates of the potential contribution of declining unemployment, we use the OLS estimates from the most complete model specification (Table 3, column 3).

Between 1992 and 1997 (the last six years of our panel), the rate of robbery decreased by 30 percent, the rates of auto theft and burglary declined by more than 15 percent, and larceny declined by slightly more than 4 percent. Concurrently, the unemployment rate declined from approximately 7.4 to 4.9 percent. Our OLS estimates from the most complete specification predict that the 2.5 percentage point decline in unemployment caused a decrease of 5 percent for burglary, 3.7 percent for larceny, 2.5 percent for auto theft, and 4.3 percent for robbery. Expressed as a percentage of actual declines, our estimates indicate that 28 percent for the burglary rate, 82 percent for larceny, 14 percent for auto theft, and 14 percent for robbery is attributable to the decline in the unemployment rate. If we look at the overall property crime rate, slightly more than 40 percent of the decline can be attributed to the decline in unemployment. Note, that these are conservative estimates for two reasons: we use the OLS estimates, which are considerably lower than the corresponding 2SLS estimates. Moreover, income per capita has in general a negative impact on crime rates, which can be considered as an additional impact of the business cycle on criminal behavior.

Hence, the magnitudes of the crime-unemployment effects presented here relative to overall movements in crime rates are substantial and suggest that policies aimed at improving the employment prospects of workers facing the greatest obstacles can be effective tools for combating crime.[17] Moreover, given that crime rates in the U.S. are considerably higher in areas with high concentrations of jobless workers (many inner-city communities, for example) and the fact that those workers with arguably the worst employment prospects (young African-American males) are the most likely to be involved with the criminal

[17]See Anderson (1999) for a recent comprehensive calculation of the costs of crime to society at large. He estimates the aggregate burden of crime - excluding the transfer of property - to more than $ 1 trillion.

justice system, employment-based anti-crime policies contains the attractive feature of being consistent with a wide-range of policy objectives.

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Figure 1

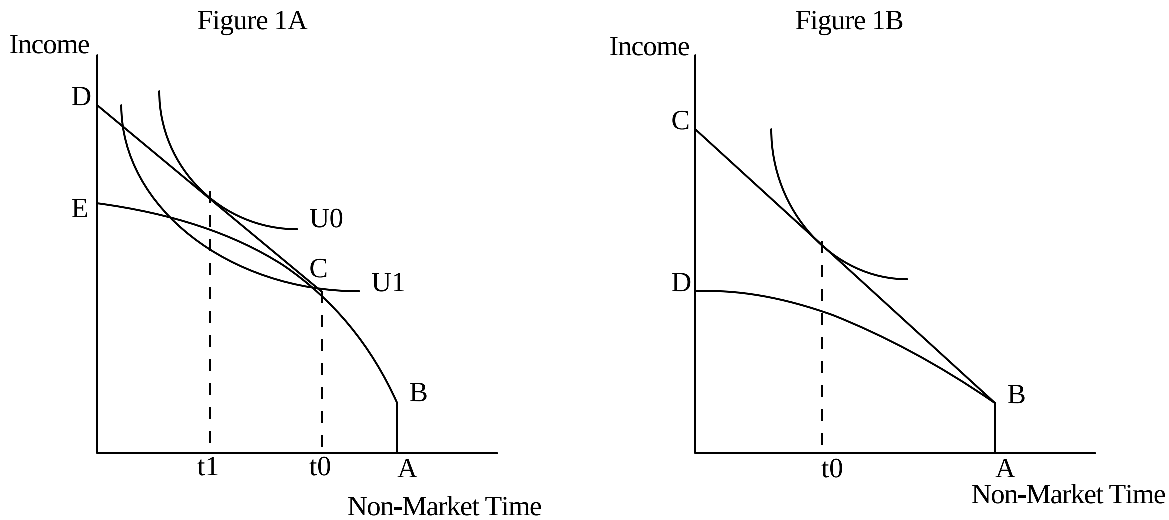
Figure 1A
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Figure 1B
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Table 1
Summary Statistics

Variables	Means	Standard Deviation	Standard Deviation Net of State and Time effects
Property Crime	4,674.81	1,158.20	434.24
Burglary	1,276.26	419.12	162.58
Larceny	2,883.48	725.10	268.67
Auto Theft	515.07	229.48	92.77
Violent Crime	585.51	264.35	68.85
Murder	8.58	3.49	1.29
Rape	34.36	11.67	5.97
Robbery	220.07	132.17	29.81
Assault	322.51	156.99	51.96
Unemployed	0.07	0.02	0.01
Prison Population	214.03	134.13	45.36
Alcohol Consumption	1.98	0.40	0.15
Metropolitan	0.77	0.17	0.02
Poor	0.13	0.04	0.02
Black	0.11	0.07	0.01
Income per worker	33.39	14.13	2.32
Population <15	0.23	0.03	0.007
Population 15-17	0.05	0.01	0.002
Population 18-24	0.12	0.02	0.005
Population 25-34	0.16	0.02	0.007
Population 35-44	0.13	0.02	0.004
Population 45-54	0.11	0.01	0.003
Population 55-64	0.09	0.01	0.003
Military Spending	0.38	0.31	0.14
Oil Costs	0.16	0.09	0.03

All crime rate as well as the incarceration rate in state prisons are defined per 100,000 state residents. Alcohol consumption is measured in consumption of gallons of ethanol per capita. Income per worker and military spending are measured in thousands of dollars per capita. The panel covers the period from 1971 to 1997. There are 1,293 observations.

Table 2
OLS Regressions of Total Property and Total Violent Crime on State Unemployment Rates and Variables Measuring State Demographic Structure

	ln(Property Crime Rate)			ln(Violent Crime Rate)		
	(1)	(2)	(3)	(4)	(5)	(6)
Unemployed	2.345 (0.205)	1.680 (0.192)	1.635 (0.182)	0.266 (0.295)	0.392 (0.297)	0.547 (0.275)
ln(Prisoners)	-0.129 (0.015)	-0.093 (0.014)	-0.108 (0.015)	-0.018 (0.021)	-0.028 (0.022)	-0.042 (0.022)
Alcohol Consumption	0.207 (0.023)	-0.147 (0.028)	-0.129 (0.028)	0.074 (0.034)	0.048 (0.044)	0.027 (0.043)
Metropolitan	0.875 (0.148)	[illegible] ([illegible])		0.754 (0.212)	1.510 (0.286)	0.922 (0.350)
Poor	-1.081 (0.156)	-0.207 (0.131)	0.076 (0.128)	-0.209 (0.223)	-0.195 (0.202)	-0.247 (0.194)
Black	1.508 (0.414)	-2.883 (0.807)	3.881 (1.475)	-3.475 (0.594)	-2.987 (1.246)	5.024 (2.229)
Income Per Worker	-0.010 (.001)	-0.025 (0.002)	-0.001 (0.004)	-0.012 (0.002)	-0.022 (0.004)	-0.016 (0.005)
Population < 15	-1.841 (0.469)	0.817 (0.479)	0.014 (0.637)	0.016 (0.674)	-1.412 (0.739)	-4.006 (0.963)
Population 15 to 17	8.338 (1.734)	14.379 (1.700)	10.360 (1.770)	7.064 (2.487)	4.729 (2.625)	9.758 (2.676)
Population 18 to 24	0.637 (0.676)	1.367 (0.578)	1.466 (0.633)	2.326 (0.971)	1.789 (0.893)	4.551 (0.956)
Population 25 to 34	1.395 (0.588)	7.123 (0.564)	7.611 (0.718)	7.277 (0.844)	7.127 (0.871)	7.474 (1.086)
Population 35 to 44	-5.862 (0.756)	-1.666 (0.890)	-0.525 (1.178)	1.174 (1.086)	0.569 (1.374)	-6.398 (1.781)
Population 45 to 54	2.206 (0.917)	5.508 (1.096)	[illegible] ([illegible])		-2.805 (1.693)	-1.305 (2.264)
Population 55 to 64	-4.751 (0.974)	-5.189 (0.921)	-3.575 (1.495)	-0.238 (1.397)	0.376 (1.421)	7.120 (2.261)
Linear Trends	No	Yes	Yes	No	Yes	Yes
Quadratic Trends	No	No	Yes	No	No	Yes

Standard errors are in parentheses. The dependent variable in each regression is the log of the respective crime rate per 100,000 state residents. All regression include a full set of state and year fixed effects. There are 1,293 observations covering the periods from 1971 to 1997.

Table 3
OLS Estimates of the Semi-Elasticities of Specific Crimes with Respect to State Unemployment Rates

	No State Time Trends	Linear Trends	Linear and Quadratic Trends
All Property Crime	2.345 (0.205)	1.680 (0.192)	1.635 (0.182)
Burglary	3.227 (0.251)	2.276 (0.251)	2.069 (0.243)
Larceny	[illegible] [illegible]		1.494 (0.188)
Auto Theft	-0.033 (0.468)	1.383 (0.462)	1.028 (0.406)
All Violent Crime	0.266 (0.295)	0.392 (0.297)	0.547 (0.275)
Murder	-2.523 (0.439)	-0.819 (0.477)	-0.751 (0.467)
Rape	1.239 (0.353)	0.092 (0.322)	-0.744 (0.298)
Robbery	0.006 (0.443)	1.419 (0.433)	1.724 (0.415)
Assault	0.293 (0.379)	0.083 (0.385)	0.183 (0.362)

Standard errors are in parentheses. The parameter estimates are the coefficients on the state unemployment variable from regressions where the dependent variables is the log of the respective crime rate. Crime rates are measured per 100,000 state residents. All of the regressions include the control variable listed in Table 1 as well as full sets of state and year fixed effects. Each regression has 1,293 observations and covers the period from 1971 to 1997.

Table 4
First-Stage Regressions of State Unemployment Rates on State Military Contracts Pre Capita and State Level Measure of Oil Costs

	(1)	(2)	(3)
Military Spending	-0.002		-0.004
	(0.002)		(0.003)
Oil Costs	0.091	0.064	0.088
	(0.014)	(0.013)	(0.015)
Linear Trends	No	Yes	Yes
Quadratic Trends	No	No	Yes
F-Statistic[a]	31.783	16.571	19.377
(P-Value)	(0.0001)	(0.0001)	(.0001)

Standard errors are in parentheses. All of the regressions include the control variables listed in Table 1 as well as full sets of state and year fixed effects. Each regression has 1,293 observations and covers the period from 1971 to 1997.

a. This is the test statistic (and p-value) from an F-test of the joint significance of the military spending and oil costs instrumental variables.

Table 5
OLS and Two-Stage Least Squares Estimates of the Semi-Elasticities of Specific Crimes with Respect to State Unemployment Rates

	No State Time Trends		Linear Trends		Linear and Quadratic Trends	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
All Property Crime	2.345 (0.205)	3.853 (0.939)	1.680 (0.192)	2.781 (1.170)	1.635 (0.182)	5.018 (1.134)
Burglary	3.227 (0.251)	3.758* (1.120)	2.276 (0.251)	-1.194 (1.619)	2.069 (0.243)	4.159 (1.367)
Larceny	2.335 (0.223)	3.824 (1.017)	1.467 (0.193)	4.753 (1.291)	1.494 (0.188)	5.759 (1.238)
Auto Theft	-0.033 (0.468)	2.693* (2.120)	1.383 (0.462)	2.552 (2.769)	1.028 (0.406)	4.754 (2.287)
All Violent Crime	0.266 (0.295)	0.449 (1.318)	0.392 (0.297)	-2.982 (1.878)	0.547 (0.275)	1.918* (1.514)
Murder	-2.523 (0.439)	-7.696* (2.071)	-0.819 (0.477)	-8.391 (3.152)	-0.751 (0.467)	-1.406 (2.537)
Rape	1.239 (0.353)	2.302* (1.582)	0.092 (0.322)	-6.525* (2.253)	-0.744 (0.298)	8.905* (2.100)
Robbery	0.006 (0.443)	-4.053 (2.046)	1.419 (0.433)	-4.459 (2.794)	1.724 (0.415)	2.827 (2.258)
Assault	0.293 (0.379)	2.590 (1.719)	0.083 (0.385)	0.279 (2.308)	0.183 (0.362)	4.026* (2.063)

Standard errors are in parentheses. The parameter estimates are the coefficients on the state unemployment rate from OLS and 2SLS models where the dependent variable is the log of the respective crime rates. Crime rates are measured per 100,000 state residents. All of the models include the control variables listed in Table 1 as well as full sets of state and year fixed effects. Each model uses a sample with 1,293 observations covering the period from 1971 to 1997.
*. Test of the over-identification restriction rejects the restriction at the 5 percent level.