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IDENTIFYING THE EFFECT OF UNEMPLOYMENT ON CRIME

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Identifying the Effect of Unemployment on Crime

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Abstract

Previous estimates of the effect of unemployment on crime commonly omit determinants of criminal behavior that vary with the business cycle, creating correlation between unemployment rates and the residuals in aggregate crime regressions. In this paper, we employ several strategies that attempt to minimize or break this correlation and eliminate the accompanying omitted variables bias to estimates of the effect of unemployment on crime. Using a state-level panel for the period from 1970 to 1993, we explore the sensitivity of crime-unemployment elasticity estimates to explicit controls for per-capita alcohol consumption, a factor that has been shown in the past to be pro-cyclical and a partial determinant of criminal behavior. In addition, we use prime defense contracts per-capita at the state level as an instrument for state unemployment rates. Both controlling for alcohol consumption and using instrumental variables to correct for omitted variables bias yields large effects of unemployment on the seven felony offenses recorded by the Department of Justice. Moreover, in contrast to previous research, we find significant and sizable positive effects of unemployment on the rates of specific violent, as well as property crimes.

JEL Codes: J6, K42

Keywords: Unemployment, Crime

1. Introduction

The proposition that unemployment induces criminal behavior is intuitively appealing and grounded in the basic 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. Moreover, workers that experience chronic joblessness have less to lose in the event of an arrest and incarceration. Hence, straightforward economic reasoning suggests that unemployment is an important determinant of the supply of criminal offenders and hence, the overall crime rate.

Despite the appeal of the "criminal supply" argument, empirical research to date has been unable to document a strong causal 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. For example, in an exhaustive analysis of 68 studies, Chiricos (1987) shows that fewer than half find positive and statistically significant effects of aggregate unemployment rates on the rates of most felony offenses.[1] Moreover, the crime-unemployment relationship is considerably weaker in time-series than in cross-sectional comparisons (Chiricos 1987, Freeman 1995). In addition, the sign of the effect of unemployment on violent crime is a matter of debate and is often found to be significantly

[1]The existence of a positive significant unemployment effect varies considerably by offense, ranging from 12 and 16 percent of the studies summarized for assault and murder to 47 and 52 percent for larceny and burglary. Entorf and Spengler (1998) using a state panel for Germany also find ambiguous unemployment effects. Studies using microdata, however, generally find a high association between joblessness and criminal activity (Freeman, 1996). Moreover, the demographic composition of convicted offenders consists disproportionally of persons who would command low legitimate wages: for example, the young and relatively less educated (Grogger, 1997).

negative (Cook and Zarkin 1985); this is especially so for murder rates. In light of these weak results, recent empirical research on the economic model of crime, as well as the public policy discussion, has tended to focus on issues of law enforcement, deterrence, and incapacitation.[2]

There are several reasons to suspect that the available evidence understates the effect of unemployment on crime. Given the fact that most studies of the crime-unemployment relationship rely on time-series variation in macroeconomic conditions, the common omission from time-series regressions of variables that exert pro-cyclical pressure on crime rates may lead to downwardly-biased estimates of the partial effect of unemployment on crime. For example, previous research shows that 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 and gun availability. In addition, declining incomes during recessions may reduce the consumption of consumer durables and other possible theft-worthy goods, thus providing fewer lucrative targets for criminal activity and partially offsetting any positive effect of unemployment on crime.

These examples provide illustrations of independent pro-cyclical movements in the supply of offenders and the stock of criminal opportunities. Most importantly, these simple examples suggest that previous estimates of the unemployment-crime relationship that do not control for these important crime fundamentals may systematically understate the effect of unemployment on crime. An additional problem, however, concerns the direction of causation. To the extent that

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[2]For an analysis of the incapacitation effects of prison populations and the deterrent effects of policing, see Levitt (1996, 1997). Lott and Mustard (1997) test for deterrence effects of laws that permit citizens to carry concealed handgun laws. For a test of the economic model of crime based on compensating differential theory and variation in the risk premium to criminal activity, see Viscusi (1986).

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 unemployment. Moreover, crime may in itself impede employment growth and contribute to regional unemployment levels.[3] Hence, in addition to problems associated with omitted variables, previous inferences may also be flawed due to simultaneity bias.[4]

In this paper, we employ several strategies to estimate the effect of unemployment on crime. First, we explore the sensitivity of estimates of the crime-unemployment relationship to the inclusion of available variables that vary with the business cycle and are commonly thought to affect crime rates. Using a state-level panel for the period 1970 to 1993, we first estimate the effect of state unemployment rates on the rates of the seven felony offenses recorded in the FBI's Uniform Crime Reports (UCR) using common specifications of aggregate crime equations and standard panel data techniques. Next, we explicitly control for several state-level measures of per-capita alcohol consumption and analyze changes in the estimated crime-unemployment elasticities resulting from the change in model specification. To the extent that estimates of the unemployment-crime relationship are biased by the omission of alcohol consumption, estimates of

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[3]Bound and Freeman (1992) as well as Nagin and Waldfogel (1995) find that conviction and incarceration reduce future job market prospects, increasing the probability of future unemployment. Grogger (1995) finds rather small and short-lived employment impacts of arrests. Willis (1997) finds that business formation and location is sensitive to local crime rates. In an alternative model suggesting reverse causation, 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 relative to legitimate opportunities.

[4]Simultaneity issues between crime and unemployment have 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.

the unemployment effects should change. This simple exercise gives some indication of the importance of such considerations in evaluating the validity of previous research.

In an attempt to assess the bias from all omitted factors and to correct for simultaneity, we re-estimate the crime equations by instrumenting state unemployment rates on an exogenous factor that is a well-known determinant of regional employment levels. We use state prime military contract awards as an instrument for state unemployment rates. Previous macroeconomic research on regional fluctuations demonstrates the strong relationship between military contract awards and state unemployment rates (Davis et. al. 1997, Hooker and Knetter 1997). Furthermore, the existing research suggests that the timing of these awards is independent of national and regional economic fluctuations. Hence, changes in contracts should be independent of the omitted factors that are both correlated with the unemployment rate and have independent effects on crime levels.

In our base models omitting controls for alcohol consumption we find highly significant, positive unemployment effects on property crime and no relationship or negative significant effects of unemployment on violent crime. For the three property crimes analyzed, these effects are modest, ranging from a one to four percent change in the crime rate associated with a one percentage point change in the unemployment rate. Controlling for alcohol consumption generally increases the unemployment effect for most crimes and yields positive significant elasticities for some violent crimes. Moreover, the crime-unemployment semi-elasticities from the specification including alcohol are often statistically distinguishable from the estimates omitting alcohol. The instrumental variable results show considerably larger crime-unemployment effects for both property and violent crimes. Moreover, in contrast to the findings of previous research,

the effect of unemployment on violent crime rates is significant and comparable in magnitude to the effects on property crimes.

2. Empirical Strategy and Data Description

Cook and Zarkin (1985) suggest four categories of factors that may create linkages between the business cycle and crime: (1) variation in legitimate employment opportunities, (2) variation in criminal opportunities, (3) consumption of criminogenic commodities (alcohol, drugs, guns), and (4) temporal variation in the response of the criminal justice system. While legitimate opportunities are tautologically pro-cyclical, many of these other crime fundamentals are also likely to vary with the business cycle. The quality and quantity of criminal opportunities may be lower during recessions (having clear implications for conditional effects on aggregate crime rates) as potential victims have less income, consume less, and expend more effort on protecting what they have. To the extent that alcohol, drugs, and guns are normal goods, consumption of these goods will be pro-cyclical. Furthermore, to the extent that these commodities induce criminal behavior, or in the least augment the lethality of criminal incidents, pro-cyclical consumption will induce pro-cyclical variation in crime.[5] Variation in policing and criminal justice activity over the business cycle is less clear since the quantity and efficacy of criminal justice

[5]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 the lethality of crime 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 more likely to reduce aggressive behavior (Fagan 1990).

activity depends on state tax revenues, community cooperation, and political pressures (Levitt 1997).

While Cook and Zarkin (1985) do not claim that the above schema provides an exhaustive list of connections between crime rates and the business cycle, their discussion illustrates how the individual marginal effects of the various cyclical determinants of crime make it difficult to predict the sign and magnitude of the reduced form relationship between crime rates and macroeconomic variables. For our purposes, the discussion illustrates how the omission of these factors from standard time series and cross-sectional aggregate crime regressions is likely to create correlation between the unemployment rate and the equation's error term, yielding biased estimates of the true relationship. Corrections for these potential biases can take several forms. Adding variables that affect crime and are correlated with unemployment to the model specification would effectively pull the relevant variables out of the equation's residual and eliminate the contamination of the unemployment rate. Alternatively, one could find a variable that determines unemployment yet is uncorrelated with the residual of the crime equation and estimate the aggregate equations using an instrumental variables estimator. Here we outline versions of both strategies.

Alcohol, Crime, and the Business Cycle

While it is extremely difficult to control for all factors that vary with the business cycle and affect aggregate crime rates, experimenting with observable variables should provide some indication of the sensitivity of crime-unemployment elasticity estimates to changes in model specifications. Ruhm (1995, 1997) has used state-level measures of per-capita alcohol consumption to analyze the relationships between alcohol consumption, various mortality rates,

and macroeconomic conditions. Interestingly, Ruhm (1995) finds considerable evidence that alcohol consumption is pro-cyclical, with the consumption of distilled spirits exhibiting the greatest sensitivity to changes in personal income. Hence, as a first pass at the data, we expand the specification of aggregate crime regressions to include measures of alcohol consumption and analyze the changes in results induced by the richer specification.

Using state-level data we first estimate the equation

$$Crime_{it} = \alpha_t + \delta_i + \gamma Unemployed_{it} + \beta X_{it} + \eta_{it}, \quad \textbf{(1)}$$

where i indexes states and t indexes years, $Crime_{it}$ is the log of the number of crimes occurring in state i during year t per 100,000 state residents, $Unemployed_{it}$ is the proportion of the workforce in state i in year t that is unemployed, X_{it} is a vector of standard controls, α_t is a year fixed effect, δ_i is a state fixed effect, γ gives the semi-elasticity of the crime rate with respect to the unemployment rate, β is the vector of parameters for the control variables in X_{it}, and η_{it} is the residual. The inclusion of state fixed effects eliminates any bias associated with time-invariant, inter-state heterogeneity (for example, differences in gun-ownership and availability, police inputs, or sentencing procedures). Allowing for time fixed effects eliminates national time trends in crime rates as well as any trends in the propensity to record crimes, a problem that appears to be important when analyzing UCR data (Freeman 1996, Levitt 1998). Hence, the crime-unemployment elasticity from equation (1) is identified by within-state variation in the state unemployment rate relative to the national rate.

To the extent that alcohol consumption is pro-cyclical and exerts an independent effect on aggregate crime rates, the variable $Unemployed_{it}$ in equation (1) will be negatively correlated with

the residual, η_{it}. Moreover, if the effect of alcohol consumption on crime is positive, estimates of the semi-elasticity, γ, from (1) will be biased downwards. To explore this possibility, we re-estimate the aggregate crime equation including state-level measures of alcohol consumption:

$$Crime_{it} = \alpha_t + \delta_i + \gamma Unemployed_{it} + \theta Alcohol_{it} + \beta X_{it} + \eta_{it}, \quad \textbf{(2)}$$

where $Alcohol_{it}$ is a vector of three variables measuring state-level per-capita consumption of beer, wine, and distilled spirits in state *i* in year *t*, and θ is a parameter vector. Comparing the coefficient estimate for the variable $Unemployed_{it}$ in model (2) to the comparable parameter estimate in model (1) indicates the size of the omitted variables bias due to excluding alcohol consumption from aggregate crime models. Below, we present simple statistical tests of the significance of the changes in the crime-unemployment semi-elasticity caused by the expanded specification.

Instrumenting with Prime Defense Contracts

While the analysis described above is well-suited to exploring the robustness of standard crime-unemployment elasticity estimates to the inclusion of additional crime fundamentals, this exercise is not likely to generate unbiased estimates of the true unemployment effect. Surely, alcohol consumption is not the only crime fundamental that varies with the business cycle and being able to explicitly control for all omitted factors is probably impossible. Moreover, even if one could observe all omitted factors, to the extent that the unemployment and crime rates are simultaneously determined, the effect of unemployment on crime using the single-equation estimators in equations (1) and (2) is not identified.

In an attempt to break the correlation between unemployment and the omitted factors

swept into the residual as well as the potential simultaneity between crime rates and unemployment, we re-estimate the model in equation (2) using Department of Defense (DOD) annual prime contract awards to each state as an instrument for the state unemployment rate. The effect of DOD prime contract awards on state unemployment rates has been well-documented by past research (Blanchard and Katz 1992, Hooker and Knetter 1997, Davis et. al. 1997). To satisfy the conditions of a valid instrument, prime contract awards must be a determinant of state unemployment rates yet be uncorrelated with the residual of equation (2). In evaluating whether these conditions hold, two key questions immediately come to mind: (1) to what extent is within-state variation in defense contract awards itself determined by regional economic conditions (i.e., are defense contracts exogenous to regional business cycles), and (2) do defense contact awards, in part, determine the contaminating omitted factors.

At the national level, the question of whether defense spending is exogenous to the business cycle boils down to the issue of whether the defense appropriations process is influenced by fiscal policy concerns. A review of post-war developments in defense spending suggests that this is not the case. Davis et. al. (1997) show that major shifts in defense spending over the post-war period strongly coincide with international developments affecting national security (for example, the onset of the cold war from 1954 to 1964, the military build-up under Carter and Reagan from 1976 to 1985, and the defense cutbacks driven by the end of the cold war from 1986 to 1992). 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. In particular, Mayer cites three factors that make the manipulation of defense spending a poor fiscal policy instrument: (1) the appropriation process is long, often extending two years or more

between initial DOD spending requests and congressional approval, (2) major portions of the defense budget, personnel expenditures in particular, 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 actual obligation of funds (the action that actually creates employment (Greenberg 1967)), is lengthy and may occur several years after budget adoption. These considerations strongly suggest that defense spending at the national level is ill-suited to respond to immediate macro-economic concerns and would require an unrealistic degree of foresight on the part of the executive and legislative branches to be used effectively.[6]

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 studies indicating that this is unlikely. Hence, here we will follow the lead of recent

[6]Mayer does note that once appropriations have been made, the DOD has considerable discretion over the timing of the obligation of the funds, and in so doing may be subject to political pressures. In time series regressions of monthly prime contract awards, Mayer finds evidence that monthly awards are significantly and positively related to lagged monthly unemployment rates. Nonetheless, the effect of lagged unemployment rates are small relative to the increases in spending observed in the months prior to midterm congressional and presidential elections. In addition, given the documented tendency of the DOD to spend-out remaining appropriated funds at the end of the fiscal year, it seems plausible that selective hoarding and awarding of contracts for political and economic considerations is most likely to occur within rather than between fiscal years. Given our focus on annual data, the pattern of within-year allocation of appropriated funds does not affect the analysis here.

macroeconomic and regional economic research and assume that state-level contract awards are exogenous with respect to state unemployment rates.

Concerning the second question, while it is unlikely that military spending has a direct effect on crime that is independent of the indirect effect operating through unemployment, it is possible that military spending exerts other indirect effects through crime fundamentals that vary with the business cycle. For example, to the extent that unemployment affects drug consumption, and that defense spending affects unemployment, contract awards will be correlated with the residual of the crime equation and, hence, will not satisfy the necessary conditions for a valid instrument. Clearly, one would expect a connection between unemployment and the consumption of criminogenic substances, as well as an effect of unemployment on the personal income and consumption of durable goods that provide criminal opportunities. Nonetheless, if the timing of contract awards is independent of regional economic cycles, and if the overall effect of military spending is small, the within state correlation between contract awards and aggregate unobserved crime fundamentals may be weak or non-existent.

In an analysis of the determinants of within-state variation in unemployment rates, Davis et. al. (1997) find that while the effect of military contracts on state unemployment is strong and highly significant, the effect of contracts is dwarfed in magnitude by the local effect of oil price shocks operating through state industry composition and the effect of independent state-specific responses to the national business cycle (Table 4). In other words, the variation in state unemployment rates that are driven by within-state variation in contract awards, in addition to being independent of larger regional cycles, is also small relative to the cumulative effects of all other determinants. Hence, in light of these results, we feel that if prime contract awards are

correlated with unobservables through an effect on unemployment, the correlation with aggregate movements is most likely weak. Nonetheless, we acknowledge this potential shortcoming and suggest that this qualification be kept in mind when interpreting the results presented below.

As an additional methodological note, while the specifications given in equations (1) and (2) control for inter-state heterogeneity that are fixed through time and national year to year shifts in crime rates, the state and year fixed effects coupled with our additional control variables may fail to capture state-specific trends in crime. For example, increases in the proportion of the population incarcerated or overall changes in policing tactics that are difficult to measure may effect the time-path of crime within state and not be picked up by the included fixed effects or our set of controls (to be discussed below). To account for such possibilities, we also present estimates for models that add state-specific quadratic time trends. Specifically, we present OLS and instrumental variables (IV) estimation results for the equation

$$Crime_{it} = \alpha_t + \delta_i + \psi_i t_t + \omega_i t_t^2 + \gamma Unemployed_{it} + \theta Alcohol_{it} + \beta X_{it} + \eta_{it}, \qquad \textbf{(3)}$$

where t_t and t^2_t are scalar linear and quadratic time trends, and ψ_i and ω_i are state-specific coefficients on the trend variables. Here, the crime-unemployment effect is estimated using within-state variation in the state unemployment rate relative to the national rate around a state-specific quadratic time trend. To be sure, this rather un-parsimonious specification may over-control and net out much of the true effect of unemployment on crime rates. Nonetheless, the results from this expanded specification provide a nice robustness check.

Finally, since the denominator of the dependent variable in equations (1) through (3) is the state population, these models are heteroscedastic, with greater measurement error for smaller

states. Hence, in all data tabulations and models presented below, results are weighted by the state population.

Data Description

The data for this project are drawn from several sources. State-level observations over the period 1970 to 1993 for 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 rates of these seven offenses (expressed per 100,000 state residents) are the primary dependent variables of interest along with the total property crime (including burglary, larceny-theft, and motor vehicle theft) and the total violent crime rates (including murder, forcible rape, robbery, and aggravated assault). Annual observations for state populations and age structure are from the Bureau of the Census. State poverty rates and the percent 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 1993. These data have been compiled by Thomas B. Marvell and have been used in the past to study the effects of enhanced prison terms (Marvell & Moody 1995) and state determinate sentencing policies (Marvell & Moody 1996) on crime. State unemployment rates from 1976 to 1993 for all states and from 1970 to 1993 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.

State data for 1970 to 1993 on per-capita alcohol consumption come from various years of the Brewers Almanac and have been compiled by Ruhm(1995). Three measures of per-capita

consumption are available: annual per-capita consumption (in gallons) of distilled spirits, wine, and beer. Finally, data on defense prime contracts awarded to individual states for the fiscal years from 1970 to 1993 come from Knetter and Hooker (1997). Below, we use total defense contracts divided by the annual state population.[7]

Table 1 present summary statistics for all variables used in the model estimations below. The first column provides sample averages for each variable. The second column provides standard deviations for the variables after being averaged by year while the third column provides the standard deviations after being averaged by state. This information illustrates variation in the data occurring within and between states, respectively. Property crime is far more common than violent crime, with the highest crime rate being that for larceny (2,788 incidents per 100,000 persons) and the lowest crime rate being that for murder (9 incidents per 100,000 persons). The four demographic variables listed (Population 15-17, Population 18-24, Poor, and Metropolitan) give the proportion of the state population described by the variable label and are the main set of control variables used. These are standard regressors in aggregate crime regressions. Not surprisingly, per-capita consumption in gallons of beer is substantial higher than per-capita consumption of wine and distilled spirits.

3. Empirical Results

In this section, we first present base estimates of the crime-unemployment elasticities omitting controls for alcohol consumption for models of the total property and violent crime

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

rates, and for the seven specific crime rates reported in the UCR. Next, we add alcohol consumption to the specification and analyze changes in the parameter estimates. Finally, we present estimates of the crime-unemployment effects using military contracts per-capita as an instrument for state unemployment rates.

Base Estimates

Table 2 presents four model specifications for total property and violent crime. The first specification includes the state unemployment rate and the four control variables listed in Table 2 only. Next, we consecutively add state effects, year effects, and the state-specific quadratic time trends. Given the log-linear specification the coefficients for unemployment are approximately equal to the change in the respective crime rate associated with a one percentage point change in the unemployment rate.

Starting with the results for total property crime in columns (1) through (4), the crime-unemployment parameters in all specifications are significant and positive at the one percent level, ranging from approximately 1.2 to 3.8. Adding state and year effects (column (3)) causes a statistically significant increase in the elasticity estimate, while the inclusion of state-specific trends (column (4)) yields the most modest estimate of the unemployment effect. Concerning the violent crime rate models, state unemployment rates are not significant in any of the four specifications. All of the point estimates are close to zero, and even negative in two cases. Hence, our base estimates are consistent with patterns found in previous research. Unemployment is a significant determinant of property crime rates but not violent crime. In

addition, the magnitude of the effects are very similar to those found in previous and comparable panel studies.[8]

Concerning the performance of the four control variables, the sign and magnitudes of the coefficient estimates vary considerably across specifications. However, for the two richest specifications (the models including the state and year effects and the models including the state and year effects and the time trends), the effects of the control variables are somewhat uniform. In these regressions, the proportion of the state population between 15 and 17 years of age has positive significant effects on both the property and violent crime rates while the proportion between 18 and 24 is, in general, not significant. The proportion of the population residing in metropolitan areas has stronger positive effects for violent crime than for property crime, while the proportion of residents below the poverty line is, for the most part, not significant. Since the results for these background variables in the models estimated below do not differ substantially from those in Table 2, we suppress this output in all remaining tables.

Table 3 presents estimates of the crime-unemployment semi-elasticities for the seven specific crimes using the same four specifications presented in Table 2. For reference, the unemployment parameter estimates for the total property and violent crime models are reproduced. Again, the unemployment rate exerts positive significant effects on specific property crimes and no, or sometimes negative, effects on violent crime. For the three property crimes

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[8]Using a city-level panel for a similar time period, Levitt (1997) regresses the change in the log of the crime rate on the change in the unemployment rate including city fixed effects in the specification. Adding a fixed effect to a change regression is similar to including city-specific linear time trends and city fixed effects in a levels regression. Levitt finds a semi-elasticity from this specification of .99 for property crime (Levitt, Table 4, column (2)) and of -.25 for violent crime (Levitt, Table 3, column (2)). These estimates are quite similar to those reported here using quadratic time trends.

(burglary, larceny, and auto theft), unemployment has a positive effect and is significant at one percent in all specifications, with the exception of the two most restrictive specifications for auto theft. The crime-unemployment parameters are considerably smaller in the final models which include also state-specific time trends. The unemployment rate has the strongest effects on burglary rates (semi-elasticities of 4.235 and 1.821 in the models from columns (3) and (4), respectively) and the weakest effects on auto theft rates (semi-elasticities of 2.148 and 0.976 in the models from columns (3) and (4), respectively).

Concerning the specific violent crimes, while unemployment does have a positive significant effect on the rates of rape and robbery in the models with no fixed effects and with state effects only, these patterns do not survive the inclusion of year effects and time trends. In fact, the unemployment parameter from column (4) for rape is negative and significant at the 5 percent level. Similarly, murder rates exhibit a consistent significant *pro-cyclical* pattern while the rate of assault shows a significant pro-cyclical pattern in the models from the first two columns and no significant unemployment effect in columns (3) and (4). Hence, the basic patterns found in Table 2 are reproduced in greater detail in Table 3. Unemployment exerts positive moderate effects on specific property crime rates, as predicted by theory, while the effect of unemployment on violent crime is either statistically insignificant or even negative.

Controlling for Alcohol Consumption

Here we analyze changes in the crime-unemployment parameter estimates that result from adding measures of alcohol consumption to the model specifications. We focus on the last two specifications presented in Tables 2 and 3, the first controlling for state and year effects and the second allowing for state and year effects and state-specific time trends. Before discussing

changes in the unemployment coefficients, a brief discussion of the performance of the alcohol consumption variables is needed. Appendix Table A1 presents the results for the three alcohol consumption variables from regression models for total property and violent crime and the seven specific crimes using the two relevant model specifications. Included in the table are the test-statistics and P-values from an F-test of the cumulative significance of the three alcohol variables in the crime equations. Starting with the results for the specification including year and state effects, alcohol consumption generally has strong positive significant effects on nearly all of the crime rates. In those regressions where the coefficient on a specific consumption variables is negative and significant (for example, wine consumption exerts a negative significant effect on auto theft, beer and spirits consumption have negative significant effects on the murder rate), multiplying the consumption coefficients by the sample means for these variables and summing the effects yields positive overall effects of alcohol on crime (murder being the sole exception). In addition, in all regressions the three variables are highly significant.

Adding the state-specific time trends generally reduces the effects of alcohol on crime, and the three variables are not jointly significant in the murder regression. Nonetheless, in six of the nine models the three variables are jointly significant at one percent while in two of the remaining three models, they are significant at 5 percent. In addition, the cumulative effects on crime rates implied by the coefficient estimates are positive for all regressions with the exception of larceny. Hence, the results from Table A1 coupled with Ruhm's (1995) finding of pro-cyclical consumption suggest that omitting alcohol consumption from aggregate crime regressions may impart a serious downward bias to estimates of the crime-unemployment relationship.

Table 4 presents a comparison of the crime-unemployment semi-elasticities with and without controls for alcohol consumption. The first column reproduces the results from column (3) in Table 3 where the specification omits alcohol consumption and includes state and year effects. The second column gives the unemployment coefficients from models that add the three alcohol consumption variables to the specification of the first column. The third and fourth columns repeat this exercise with the additional controls for state-specific time trends. Starting with the state and year effects models, controlling for alcohol consumption increases the unemployment coefficient for all crime rates with the exception of auto theft. This change is statistically significant at 10 percent for the assault rate, at 5 percent for the burglary and total violent crime rate, and at 1 percent for rape.[9] Controlling for alcohol consumption yields a positive significant effect of unemployment on rape (significant at 1 percent) and robbery (significant at 10 percent).

Turning to the results from models including state time trends, the basic patterns are similar: Controlling for alcohol consumption increases the unemployment parameter estimates for all crime rates. These changes are statistically significant at the five percent level for the auto theft, robbery, and the overall violent crime rates. In addition, when the alcohol variables are included in the specification, unemployment has a positive and significant effect on the overall violent crime rate (at 5 percent), the robbery rate (at 1 percent), and the rate of aggravated assault (10 percent).

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[9]These figures are the p-values from F-tests that constrain the unemployment coefficient in the model including the three alcohol consumption variables to the corresponding value from the model excluding these variables.

In sum, adding alcohol consumption to the base specification yields several interesting results. First, for 17 out of 18 models the effect of unemployment increases and in several of these models the changes are statistically significant. Moreover, for several of the violent crimes where unemployment was found to have no effect on the crime rates in the base models in Table 3, adding alcohol consumption to the specification yields positive significant effects. Hence, the results in Table 4 indicate that estimates of the crime-unemployment relationship are quite sensitive to the chosen specification and that past estimates may suffer from significant omitted variables bias.

Instrumenting with Prime Defense Contracts

The results presented above demonstrate the potential downward bias to previous estimates of the crime-unemployment elasticity. Providing an unbiased estimate of this elasticity, however, requires breaking the correlation between the unemployment rates and the residuals in the aggregate crime equations. In this section, we present results from IV estimations of the crime-unemployment semi-elasticities using state per-capita prime military contract awards as an instrument for unemployment. To establish the correlation between state contract awards and state unemployment rates, Table 5 presents the first-stage regressions of the state unemployment rate on military spending. In all regressions, the military spending variable has a significant negative effect on state unemployment rates. In the regression including state and year effects and the regression including state and year effects and time trends, the coefficient on state military contracts per capita is negative and significant at the .001 percent level of confidence. Hence, the relationship between this variable and unemployment is strong.

Table 6 presents OLS and IV estimates of the crime-unemployment parameters for total property and violent crimes and for the seven specific crimes. Again, we present estimation results using only state and year fixed-effects as well as those using state-specific time trends. Given the importance of alcohol consumption demonstrated in the previous section, all regressions include the three alcohol consumption variables in the specification. Hence, the first and third columns of the table giving the OLS estimates are reproductions of the second and fourth columns of Table 4.

Starting with the state and year effects models, the IV estimates of the crime-unemployment parameters are considerably larger than the OLS estimates. For property crimes, the IV unemployment effects are three to four times the OLS unemployment effects and are all significant at the one percent level. According to these estimates, a one percentage point increase in the unemployment rates causes an approximately 14 percent increase in burglary rates, 11 percent increase in larceny, and an 8 percent increase in the rate of auto-theft. Concerning violent crimes, IV estimates of the unemployment effect are positive and significant at 5 percent for total violent crime, 1 percent for rape, and 10 percent for assault (the unemployment effect for murder remains negative yet insignificant with a p-value of .16 while the effect for robbery is positive and insignificant with a p-value of .17). Moreover, the IV estimate of the unemployment parameters are large (approximately 3.5 for the total violent crime, robbery and assault, and 19.5 for rape). Hence, the results excluding state-specific time trends indicate very large unemployment effects and suggest that OLS estimates for both violent and property crime suffer from considerable bias.

Similar to the patterns observed in Tables 3 and 4, the IV estimates for models including state time trends yield more modest effects of unemployment on crime than those excluding

trends. Nonetheless, the IV parameters are still substantially greater than the OLS parameter estimates. For the three property crimes, the unemployment effect is large and significant for burglary (p-value of .06) and auto theft (p-value of .06), while for larceny, instrumenting yields an insignificant crime-unemployment parameter quite close to zero. For burglary, the IV estimate is more than double the OLS estimate, implying that a one percent decrease in unemployment induces an approximately 4.5 percent decrease in the burglary rate. For auto theft, the IV estimate is more than three times the OLS estimate, implying a 7 percent change in the auto-theft rate in response to a one percentage point change in the unemployment rates.

For all violent crime models where state-specific time trends are included, instrumenting on military contract awards yields large and positive crime unemployment semi-elasticities. The effect of unemployment is significant at 10 percent for robbery, 6 percent for rape, 1 percent for assault, and is insignificant for murder (p-value of .16). With the exception of the particularly large estimate for aggravated assault (14%), the IV estimates from the final column of Table 6 indicate that a one percentage point decrease in the unemployment rates leads to a 6 to 7 percent decrease in the rates of the specific violent crimes.

Hence, the results from the IV estimates indicate that the relationship between crime and unemployment is considerably stronger than is suggested by previous research. Instrumenting on military contracts yields much larger unemployment effects for both specific property and violent crimes, even in the least restrictive specifications. Moreover, the results for violent crime contradict the common finding in previous research that unemployment and violent crime are unrelated.

4. Discussion

Previous research finds ambiguous effects of unemployment on crime: whereas in many cases unemployment positively effects property crime, for violent crime - and especially for murder - the relationship is found to be non-existent or of the wrong sign. In this paper, we show that high unemployment rates are an important factor contributing substantially to both property and violent crime rates. Our findings rely on the fact, that previous studies fail to control for the influence of crime fundamentals, such as alcohol consumption, that vary with the business cycle and thus impart a downward bias to estimates of unemployment effects. Correction for this bias and using proper instruments yields consistent positive effects of unemployment rates on the rates of all seven felony offenses recorded in the UCR.

To put the empirical results presented above into perspective, a discussion of recent trends in aggregate crime rates and recent developments in the U.S. labor market may be instructive. Between 1992 and 1996 (the latest year for which aggregate figures are available), the rates of all seven felony offenses decreased, some considerably. For example, over this time period, the rates of robbery and murder decreased by more than 20 percent, rape, auto theft, and burglary declined by more than 15 percent, aggravated assault declined by 12 percent, and larceny declined by slightly more than 4 percent (Department of Justice 1997). Concurrently, after peaking at 7.4 percent in 1992, the civilian unemployment rate declined steadily to 5.4 percent in 1996.

Using the unemployment effects estimated here, one can make out-of-sample predictions concerning the effects on crime rates of the two percentage point drop in the unemployment rate. Our IV parameter estimates from the least restrictive specification - including state-specific time trends - predict declines of 9 percent for burglary, half of a percent for larceny, 13 percent for

auto theft, 13 percent for murder, 14 percent for rape and robbery, and nearly 30 percent for assault. Expressed as a percentage of actual declines, our parameter estimates indicate that 50 percent of the decline in the burglary rate, 11 percent of the decline in larceny, 80 percent for auto theft, 70 percent for murder, 90 percent for rape, 60 percent for robbery, and 280 percent of the decline in assault, can be attributed to recent declines in the unemployment rate.

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 affective tools for combating crime. 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 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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Table 1
Summary Statistics

Variables	Means	Standard Deviation of Year Averages	Standard Deviation of State Averages
Property Crime	4,618.66	667.53	1,074.77
Burglary	1,324.06	156.48	355.06
Larceny	2,788.64	543.33	668.86
Auto Theft	506.96	78.96	200.01
Violent Crime	557.40	120.24	213.63
Murder	8.85	.65	3.66
Rape	33.11	7.27	11.97
Robbery	216.21	29.29	104.05
Assault	299.22	86.71	119.82
Unemployed	.07	.013	.013
Population 15-17	.05	.007	.003
Population 18-24	.12	.010	.006
Poor	.13	.012	.040
Metropolitan	.77	.008	.228
Beer	22.65	1.81	4.12
Wine	1.94	.32	.89
Spirits	1.74	.22	.71
Military Spending	.36	.16	.24

All crime rates are defined by reported incidents per 100,000 state residents. The three alcohol variables give apparent annual consumption in gallons per capita, and military spending gives the state per-capita awarded Department of Defense contract awards (in thousands). There are 1,198 observations.

Table 2
OLS Estimates of the Semi-Elasticities of Total Property and Violent Crime with Respect to State Unemployment Rates

	ln(Property Crime)				ln(Violent Crime)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployed	2.500 (.384)	3.760 (.290)	3.274 (.218)	1.228 (.194)	-.194 (.503)	.114 (.291)	-.471 (.303)	.127 (.279)
Population 15-17	-18.877 (1.300)	-19.746 (.957)	12.679 (1.698)	14.393 (2.337)	-19.243 (1.701)	-26.81 (.962)	4.064 (2.367)	9.071 (3.36)
Population 18-24	8.135 (.805)	9.265 (.403)	.883 (.754)	-.588 (.850)	1.850 (1.054)	3.727 (.680)	1.661 (1.051)	2.308 (1.224)
Metropolitan	.729 (.047)	3.311 (.403)	2.910 (.237)	.365 (.347)	2.215 (.063)	2.616 (.405)	1.788 (.331)	1.364 (.499)
Poor	.087 (.223)	-2.607 (.292)	-2.051 (.175)	.228 (.157)	6.162 (.292)	.208 (.294)	.072 (.244)	-.149 (.226)
State Effects	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Time Effects	No	No	Yes	Yes	Yes	No	No	Yes
State Time Trends[a]	No	No	No	Yes	No	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. Each regression has 1,198 state-year observations for all 50 states excluding DC covering the period 1970 to 1993.
a. Includes a linear and quadratic time trend interacted with the set of state dummy variables.

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

	Specifications			
	(1)	(2)	(3)	(4)
All Property Crime	2.500 (.384)	3.760 (.291)	3.274 (.218)	1.228 (.194)
Burglary	2.118 (.409)	4.365 (.258)	4.235 (.271)	1.821 (.239)
Larceny	3.456 (.487)	4.392 (.424)	3.130 (.241)	.966 (.226)
Auto Theft	.158 (.444)	-.255 (.344)	2.148 (.417)	.976 (.343)
All Violent Crime	-.194 (.503)	.114 (.291)	-.471 (.303)	.127 (.279)
Murder	-1.381 (.131)	-2.058 (.298)	-3.186 (.361)	-1.684 (.401)
Rape	1.461 (.512)	.723 (.360)	-.209 (.391)	-.616 (.297)
Robbery	1.224 (.598)	1.264 (.367)	.120 (.442)	.215 (.400)
Assault	-1.325 (.561)	-.751 (.359)	-.414 (.373)	.378 (.334)
State Effects	No	Yes	Yes	Yes
Time Effects	No	No	Yes	Yes
State Time Trends[a]	No	No	No	Yes

The parameter estimates are the coefficients on the state unemployment variable from regressions where the dependent variable is the log of the respective crime rate. Crime rates are measured per 100,000 state residents. In addition to the fixed effect and time trends indicated at the bottom of the table, each regression includes controls for the percentage of residents between 15 and 17 and between 18 and 24 years of age, and the percentage of the population poor, and residing in metropolitan areas. Each regression has 1,198 state-year observations for all 50 states excluding DC covering the period 1970 to 1993. Standard errors are in parentheses.
a. Includes a linear and quadratic time trend interacted with the set of state dummy variables.

Table 4
Fixed-Effect Estimates of the Semi-Elasticities of Specific Crimes with Respect to State Unemployment Rates

	State and Year Effects		State and Year Effects and State Time Trends[b]	
	Excluding Alcohol	Including Alcohol[a]	Excluding Alcohol	Including Alcohol[a]
All Property Crime	3.274 (.218)	3.554 (.217)	1.228 (.194)	1.378 (.205)
Burglary	4.235 (.271)	4.825[d] (.263)	1.821 (.239)	2.029 (.253)
Larceny	3.130 (.241)	3.470 (.242)	.966 (.226)	.988 (.237)
Auto Theft	2.148 (.417)	1.887 (.433)	.976 (.343)	1.717[d] (.363)
All Violent Crime	-.471 (.303)	.221[d] (.307)	.127 (.279)	.623[c] (.298)
Murder	-3.186 (.361)	-3.123 (.376)	-1.684 (.401)	-1.373 (.431)
Rape	-.209 (.391)	1.062[e] (.372)	-.616 (.297)	-.279 (.318)
Robbery	.120 (.442)	.742 (.448)	.215 (.400)	1.122[d] (.423)
Assault	-.414 (.373)	.215[c] (.386)	.378 (.334)	.580 (.358)

The parameter estimates are the coefficients on the state unemployment variable from regressions where the dependent variable is the log of the respective crime rate. Crime rate are measured per 100,000 state residents. In addition to the fixed effect and time trends indicated at the bottom of the table, each regression includes controls for the percentage of residents between 15 and 17 and between 18 and 24 years of age, and the percentage of the population poor, and residing in metropolitan areas. Each regression has 1,198 state-year observations for all 50 states excluding DC covering the period 1970 to 1993. Standard errors are in parentheses.
a. Alcohol variables include the per-capita consumption (in gallons) of beer, wine and spirits. Coefficients on the alcohol variables are presented in Table 1A.
b. Include a linear and quadratic time trend interacted with the set of state dummy variables.
c. Unemployment parameter estimate statistically distinguishable from the parameter estimate from the model excluding alcohol at 10 percent.
d. Unemployment parameter estimate statistically distinguishable from the parameter estimate from the model excluding alcohol at 5 percent.
e. Unemployment parameter estimate statistically distinguishable from the parameter estimate from the model excluding alcohol at 1 percent.

Table 5
Results from Regressions of State Unemployment Rates on the Per-Capita Value of Military Contracts Awarded to State Contractors

	(1)	(2)	(3)	(4)
Military Spending	-.012 (.002)	-.007 (.003)	-.015 (.002)	-.011 (.003)
State Effects	No	Yes	Yes	Yes
Time Effects	No	No	Yes	Yes
State Time Trends[a]	No	No	No	Yes
F-Statistic[b] (P-Value)	27.062 (.000)	4.560 (.033)	37.878 (.000)	12.387 (.000)

In addition to the fixed effect and time trends indicated at the bottom of the table, each regression includes controls for per-capita consumption of beer, wine, and spirits, the percentage of residents between 15 and 17 and between 18 and 24 years of age, and the percentage of the population poor, and residing in metropolitan areas. Each regression has 1,198 state-year observations for all 50 states excluding DC covering the period 1970 to 1993. Standard errors are in parentheses.
a. Includes a linear and quadratic time trend interacted with the set of state dummy variables.
b. Test statistic from an F-test of the significance of the per-capita military contracts variable.

Table 6
OLS and Instrumental Variables Estimates of the Semi-Elasticities of Specific Crimes with Respect to State Unemployment Rates

	State and Year Effects		State and Year Effects and State Time Trends[a]	
	OLS	IV	OLS	IV
All Property Crime	3.554 (.217)	11.789 (1.812)	1.378 (.205)	2.301 (1.894)
Burglary	4.825 (.263)	13.363 (2.025)	2.029 (.253)	4.402 (2.409)
Larceny	3.470 (.242)	10.639 (1.787)	.988 (.237)	.215 (2.178)
Auto Theft	1.887 (.433)	8.083 (2.599)	1.717 (.363)	6.726 (3.607)
All Violent Crime	.221 (.307)	3.403 (1.776)	.623 (.298)	11.082 (4.041)
Murder	-3.123 (.376)	-2.859 (2.076)	-1.373 (.431)	6.344 (4.512)
Rape	1.062 (.372)	19.464 (3.670)	-.279 (.318)	6.724 (3.532)
Robbery	.742 (.448)	3.436 (2.512)	1.122 (.423)	6.742 (4.179)
Assault	.215 (.386)	3.508 (2.198)	.580 (.358)	14.486 (5.145)

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. In addition to the fixed effects and time trends indicated at the top of the table, each regression includes controls for per-capita consumption of beer, wine and spirits, the percentage of residents between 15 and 17 and between 18 and 24 years of age, and the percentage of the population poor, and residing in metropolitan areas. Each regression has 1,198 state-year observations for all 50 states excluding DC covering the period 1970 to 1993. Standard errors are in parentheses.
a. Includes a linear and quadratic time trend interacted with the set of state dummy variables.

Table A1
Crime-Specific Effects of Alcohol (Corresponding to the Regression Results Presented in the Second and Fourth Columns of Table 4)

	Property Crime	Burglary	Larceny	Auto Theft	Violent Crime	Murder	Rape	Robbery	Assault
Regressions including state and year fixed effects									
Unemployed	3.554	4.825	3.470	1.887	.221	-3.123	1.062	.742	.215
	(.217)	(.263)	(.242)	(.433)	(.307)	(.375)	(.372)	(.448)	(.386)
Beer	.015	.025	.010	.021	.016	-.012	-.002	.034	.007
	(.002)	(.003)	(.002)	(.004)	(.003)	(.004)	(.004)	(.004)	(.004)
Wine	.024	.068	.036	-.061	.104	.028	.185	.090	.098
	(.011)	(.014)	(.013)	(.023)	(.016)	(.019)	(.019)	(.023)	(.020)
Spirits	.120	.068	.139	-.008	.016	-.075	.333	-.098	.046
	(.017)	(.014)	(.019)	(.035)	(.025)	(.030)	(.023)	(.036)	(.031)
F-Statistic[a]	41.260	64.198	32.749	9.184	27.905	6.591	79.813	26.708	11.403
(P-Value)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Regressions including state and year fixed effects and state specific linear and quadratic time trends									
Unemployed	1.378	2.029	.988	1.717	.623	-1.373	-.279	1.122	.580
	(.206)	(.253)	(.237)	(.363)	(.298)	(.432)	(.318)	(.423)	(.358)
Beer	-.009	-.002	-.018	.021	.012	.005	.005	.011	.013
	(.003)	(.003)	(.003)	(.005)	(.003)	(.006)	(.004)	(.006)	(.005)
Wine	.072	.094	.075	.067	.060	.061	.031	.157	-.001
	(.015)	(.019)	(.017)	(.027)	(.022)	(.032)	(.023)	(.031)	(.026)
Spirits	.037	-.014	.043	.107	.065	.031	.085	.134	.001
	(.024)	(.029)	(.027)	(.043)	(.035)	(.050)	(.038)	(.050)	(.042)
F-Statistic[a]	11.967	8.709	18.081	11.582	7.307	1.614	2.943	12.950	2.627
(P-Value)	(.000)	(.000)	(.000)	(.000)	(.000)	(.184)	(.032)	(.000)	(.049)

All regressions include controls for the proportion of the population between 15 and 17 and between 18 and 24, the proportion of the population that is poor and the proportion of the population residing in a metropolitan areas. Standard errors are in parentheses. Each regression has 1,198 state-year observations.
a. F-statistic and P-value from a test of the collective significance of the three alcohol consumption variables.