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STRIKING AT THE ROOTS OF CRIME:  
THE IMPACT OF SOCIAL WELFARE SPENDING ON CRIME DURING THE GREAT DEPRESSION

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**ABSTRACT**

The Great Depression of the 1930s led to dire circumstances for a large share of American households. Contemporaries worried that a number of these households would commit property crimes in their efforts to survive the hard times. The Roosevelt administration suggested that their unprecedented and massive relief efforts struck at the roots of crime by providing subsistence income to needy families. After constructing a panel data set for 83 large American cities for the years 1930 through 1940, we estimated the impact of relief spending by all levels of government on crime rates. The analysis suggests that relief spending during the 1930s lowered property crime in a statistically and economically significant way. A lower bound ordinary least squares estimate suggests that a 10 percent increase in per capita relief spending during the Great Depression lowered property crime rates by close to 1 percent. After controlling for potential endogeneity using an instrumental variables approach, the estimates suggest that a 10 percent increase in per capita relief spending lowered crime rates by roughly 5.6 to 10 percent at the margin. More generally, our results indicate that social insurance, which tends to be understudied in economic analyses of crime, should be more explicitly and more carefully incorporated into the analysis of temporal and spatial variations in criminal activity.

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## **Striking at the Roots of Crime: The Impact of Social Welfare Spending on Crime During the Great Depression**

[T]hrough a broad program of social welfare, we struck at the very roots of crime itself....Our citizens who have been out of work in the last six years have not needed to steal in order to keep from starving. Of course, when we instituted those [New Deal] activities we did not have in mind merely the narrow purpose of preventing crime. However, nobody who knows how demoralizing the effects of enforced idleness may be, will be inclined to doubt that crime prevention has been an important by-product of our effort to provide our needy unemployed citizens with the opportunity to earn by honest work at least the bare necessities of life.

Franklin D. Roosevelt, April 17, 1939<sup>1</sup>

### **I. Introduction**

During the 1930s, as the economy sunk into the depths of a Depression with double-digit unemployment, there was widespread fear that the social and economic foundation of American society was crumbling. Faith in the capitalist market economy was shaken as millions of unemployed Americans quickly ran through their savings. Some sought desperate means to provide for their families' subsistence. In 1933 the Roosevelt administration, seeking to bolster the depressed economy, created the New Deal, a series of grand experiments that dramatically changed the interaction between the federal, state, and local governments and the economy. The centerpiece of the New Deal was the distribution of several billion dollars in funds to provide work relief jobs and direct relief to people throughout the country. The federal government for the first time in U.S. history took responsibility for aiding the unemployed and the poor. The relief provided during the New Deal went well beyond the scale seen before the 1930s and large amounts continued to be spent throughout the decade.

As the economy slid toward the trough of the Depression in the early 1930s, the burden of providing relief rested almost entirely on state and local governments and private charitable organizations. As unemployment rates rose toward 25 percent, average per capita relief spending in 114 of the largest cities rose nearly five-fold from \$3.90 in 1930 to \$18.70 in 1932 (constant 1967 dollars). After Roosevelt was inaugurated, his New Deal programs poured resources (sometimes in partnership with state and local

governments) into relief programs of all types. The new programs increased per capita relief expenditures by about 160 percent between 1932 and 1933. Even as unemployment rates (including relief workers as unemployed) were cut by more than half, by 1940, per capita expenditures on relief were still nearly three times the 1932 level.

At the most basic level New Deal relief programs accomplished the fundamental objective of providing income to people who had no prospects for private employment. Yet given the significance of the New Deal in U.S. economic and political history, surprisingly little quantitative research has explored the economic consequences of New Deal spending.<sup>2</sup> After assembling a new city-level panel data set, we examine whether the New Deal relief spending “struck at the very roots of crime,” as Roosevelt so eloquently stated in the opening quotation above. Recent work suggests that greater employment opportunities tend to mitigate criminal behavior in the modern era; therefore, the massive federal intervention during the Great Depression naturally raises the question: To what extent did New Deal work relief and income-support programs diminish criminal behavior?<sup>3</sup> The dramatic variations across cities and over time in the Depression’s economic shocks and in the government’s response provide a unique opportunity to empirically examine the effectiveness of social welfare programs in times of crisis because New Deal relief was targeted at those at the very bottom rungs of the income distribution.

The paper contributes more than an assessment of the New Deal’s success. It also adds a feature to the economic analysis of crime that previous scholars have tended to overlook or take as given. One of the central insights of the economic model of crime is that people become criminals “not because their basic motivation differs from that of other persons, but because their benefits and costs differ” (Becker 1968, 176). Among the many factors that can influence the benefits and costs of criminal activity include legal employment opportunities, deterrence (police, prisons, and capital punishment), education, social policies, and social insurance. Despite the potential importance of welfare or social insurance programs in the theoretical model of crime, empirical analysis that focuses primarily on their impact on crime is largely missing from the economics literature.<sup>4</sup>

One worry in identifying the true causal effect of New Deal spending on crime is that the distribution of federal relief funds over time and across cities was not exogenous. It was driven to some degree by economic distress in each city at each point in time. New Deal officials might have considered crime, along with unemployment and other measures of economic activity, as one of the factors that influenced their distribution of relief. Since they might have increased relief spending in response to increases in crime, ordinary least squares (OLS) estimates of the impact of relief spending on crime might be biased toward finding that increased relief spending did not contribute to reductions in crime.<sup>5</sup> To control for potential simultaneity and endogeneity in the form of correlation of New Deal relief with unobserved errors in the crime regression, we adopt an instrumental variables (IV) approach. Our results suggest that New Deal relief spending reduced property crime rates in American cities. While controlling for a variety of socio-economic conditions, police spending, time-invariant features of cities, and national shocks that simultaneously hit all cities at various points in time, the OLS estimates suggest that the relief spending during the 1930s was associated with a statistically significant reduction in property crime. Estimates using instrumental variables to control for endogeneity suggest that the OLS estimates understate the negative causal relationship between New Deal relief and crime. These IV estimates suggest an elasticity of the property crime rate with respect to per capita relief spending of between -0.57 and -0.97.

## **II. Relief Spending During the Great Depression**

When the Great Depression struck, provision of welfare and social insurance was the primary responsibility of local governments, with some specific support from state programs. Families in dire circumstances could have turned to various sources for assistance. A number of cities provided shelter and food in almshouses, while some cities provided relatively small amounts of cash assistance and in-kind aid to the poor. Private charities often distributed various forms of aid, of which a significant portion was funded by local governments. Nearly all states had established mothers' pensions for women who had lost their spouses and who had dependent children. Injured workers and their families received

aid through workers' compensation programs established during the 1910s. An increasing number of states during the early 1930s instituted old-age assistance that provided limited cash benefits to the elderly and about half the states offered cash benefits to the blind. Some local governments tried to provide work for the unemployed through limited public works projects. The aid was administered by social workers, charities, and local officials who tried to assess the recipients' needs and to some extent their "moral worthiness." Prior to 1933 the federal government played almost no role in providing relief spending beyond some aid to veterans.<sup>6</sup>

As the unemployment rate rose to nearly 25 percent and income fell between 1929 and 1933, state and local tax revenues fell. As seen in Table 1, the average per capita spending on relief in 114 leading U.S. cities rose more than four-fold from \$3.74 to \$18.06 (in constant 1967 dollars) between 1930 and 1932. This rise tested the limits of the state and local efforts to provide relief, and they called on the federal government for more assistance. During the fall of 1932 the Hoover administration provided some federal loans through the Reconstruction Finance Corporation to help some cities temporarily fund their relief budgets. Faced with national unemployment rates near 25 percent in 1933, the Roosevelt administration argued that the economy had become a national problem and thus the federal government should accept much greater responsibility for providing relief. Between 1933 and 1935, the federal government took over the vast majority of relief provision, as its share rose from 2 percent in 1932 to 79 percent by 1935 (see Table 1). Meanwhile average per capita relief spending in 114 major cities rose from \$18 in 1932 to \$48 in 1934 (1967 dollars), the first full year the federal New Deal was in operation. Despite significant drops in the number of people unemployed or on work relief during the rest of the decade, the per capita spending exceeded \$48 for the rest of the decade, peaking at over \$69 when unemployment spiked again in 1938. Following the practice of the period, we included emergency relief workers as unemployed. This group was not employed in the normal sense of the word because the payments they received were sharply limited, such that the typical family on relief during the 1930s received benefits that did not exceed 42 percent of annual manufacturing wages (see Table 1).

Between July 1933 and June 1935 the primary relief agency was the Federal Emergency Relief Administration (FERA). Federal FERA officials distributed funds to state governments through an opaque process in which the revealed distribution suggests that they paid attention to the level of economic distress in the state, the state's entreaties to FERA administrators, the state's own efforts to fund relief, and the political situation.<sup>7</sup> State governments then distributed the funds internally to local governments. FERA offered both direct relief and work relief.<sup>8</sup> Direct relief included programs that had no specific work requirements and assistance was provided in cash or in-kind, including subsistence items, such as food, shelter, clothing and household necessities, or medical care and hospitalization. Work relief, as the name connotes, required a labor contribution in return for the government assistance. FERA set a series of broad guidelines for its programs, but relied heavily on state and local officials to administer them and to determine the appropriate amounts of relief that individuals would receive. Applicants for relief applied to local offices, where officials met with them personally and determined their eligibility for relief based on a "budget-deficit" principle. Local officials calculated the difference between the family's total income and a hypothetical budget for a family of that size and used the deficit to determine the family's direct relief benefits or the amount that would be paid for work on a FERA project. The amount of relief actually distributed to a family in many cases fell short of the budget-deficit if FERA funds in the local area were limited, as local officials sometimes decided to stretch their limited resources by funding more relief cases at less generous amounts.

In response to a harsh winter and high levels of unemployment, FERA activities were supplemented temporarily by the Civil Works Administration (CWA) work relief program from November 15, 1933, through March 1934. Large numbers on the FERA relief rolls were transferred to CWA employment, where they received wages that were not based on the budget-deficit principle, but on prevailing local market wages. At its peak the CWA employed four million workers for a short period of time.<sup>9</sup>

In mid-1935 the Roosevelt administration redesigned the federal government's role in providing relief. The federal government continued to provide work relief for the unemployed who were

“employable” through the Works Progress Administration (WPA), but returned much of the responsibility for direct relief of “unemployables” to state and local governments. Applicants for aid were certified by state and local officials, who still considered a family’s budget-deficit when assessing its need for relief employment (Howard 1943, 380-403). The federal WPA then hired people from the certified rolls. Dissatisfied with its lack of control over work relief under the FERA, the WPA was administered more centrally by the federal government. Yet the WPA, like its FERA predecessor, faced a mixture of pressures as administrators decided how to distribute spending across the U.S. State and local governments lobbied for funds and federal administrators appear to have paid attention to local economic distress and political necessities (see Howard 1943, Fleck 1999b, Fishback, Kantor, and Wallis 2003, Wallis, Fishback, and Kantor 2006).

The federal government was not completely absent from providing direct relief to “unemployables,” as the Social Security Act of 1935 introduced joint state-federal versions of some earlier state programs, such as old-age assistance, aid to dependent children (replacing mothers’ pensions), and aid to the blind. Beginning in 1936, federal grants-in-aid became available on a matching basis to states administering approved plans under the Social Security Act. By the end of 1938, people in all but 8 states were receiving federal grants. The shift in focus of the federal relief efforts and the eventual reductions in federal emergency work relief programs caused the federal share to slowly decline to 57 percent by 1940.

### **III. Estimating the Effects of New Deal Relief on Criminal Behavior**

To carry out the study, we have developed a new panel data set that enables us to measure the relationships between relief spending and seven major categories of crime for 83 cities from 1930 to 1940 (a listing of cities is shown in Appendix Table 1). The U.S. Children’s Bureau published annual information on public relief assistance in 114 cities for 1929 through 1935 (Winslow 1937) and the U.S. Social Security Board updated the series and carried the data forward through 1940 (Baird 1942). In 1930 the Federal Bureau of Investigation (FBI) began the Uniform Crime Reporting (UCR) system which



relied on local police reports of criminal activity in their respective cities. Lawrence Rosen (1995, 228) describes the formation of the UCR as the “product of both a shared ideology and the structural interplay of social science, police, private philanthropy, and public administration in the 1920s. All these interests, to one degree or another, were committed to the major premise of ‘social progressivism’ . . . that science could improve the social health of the community.” The UCR represented the first systematic attempt to statistically document crime at the local level and even today remains one of the main sources of data for social scientific research on crime. Since we are examining the early days of the UCR, our sample is restricted to the 83 cities that led the way in reporting crimes to the FBI.

The FBI reported data on seven major crime categories: murder and nonnegligent manslaughter, rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft. For our purposes in this paper, we will consider property crimes to be robbery, burglary, larceny, and motor vehicle theft. The FBI defines robbery to be “the taking or attempt to take anything of value from the care, custody, or control of a person or persons by force or threat of force violence and/or by putting the victim in fear.” The FBI classifies robbery as a violent crime, but since it is committed to obtain something of value we have included it with property crimes for the purposes of testing whether New Deal relief mitigated theft “to keep from starving.”<sup>10</sup>

The quality of the data in the UCR likely varied over time as the system became more regimented and as cities became more accustomed to their new reporting roles. Our use of city fixed effects should help control for any systematic reporting disparities across cities and the use of year effects should help control for nationwide differences in reporting that varied from year to year. Further, in an analysis of the quality of the UCR data in comparison with other modern sources, Boggess and Bound (1997) suggest that the trends for robbery, burglary, auto theft, and murder in the UCR tended to match the trends in other sources during the 1980s. They indicate that crime reporting by victims and, hence, recording by police are more accurate when the crimes are more serious and committed by strangers. Trends for rape, aggravated assault, and larceny tended to vary more widely across sources because larceny is the least serious of offenses and the assaults and rapes are much more likely to be perpetrated by non-strangers.

The relief programs were designed to bolster the incomes of the unemployed and the non-working, unemployable poor. By giving unemployed workers jobs and increasing the incomes of other poor Americans, relief spending presumably decreased the incentives to commit property crimes. In Becker's economics of crime framework, a higher income or higher wages increased the opportunity cost of committing crime.<sup>11</sup> Work relief especially had the potential to effectively divert individuals' time and interest away from property crime toward law-abiding activities.<sup>12</sup> The effects of relief spending on violent crimes like murder, aggravated assault and rape are less clear to the extent that these crimes are less likely to be driven by pure economic motives. The relief spending potentially still might have reduced such crimes by reducing social stresses that might have contributed to more violent acts. But in general, we expect that violent crimes were less responsive to changes in relief spending.

Without controlling for other covariates, a simple difference-in-difference scatter plot suggests that cities with relatively larger increases in relief saw their crime rates decrease relatively more. Figure 1 plots the change in the average annual per capita property crime between 1930-1932 and 1933-1940 against the change in the average annual per capita relief spending for the same periods. The coefficient of the regression line in the figure is statistically significant at the 1 percent level. The inverse relationship between crime and relief spending in this very simple analysis is striking in light of the fact that endogeneity would tend to bias the relationship in a positive direction.

To control for the multiple attributes that may have influenced crime during the 1930s, we estimate the following regression equation:

$$\ln C_{it} = \beta_0 + \beta_1 \ln R_{it} + \beta_2 X_{it} + \gamma_t T + \lambda_i G + \varepsilon_{it} \quad (1),$$

where  $C_{it}$  is per capita property crimes in city  $i$  in year  $t$ ,  $R_{it}$  is per capita relief spending, and  $X_{it}$  is a vector of control variables that might have influenced the crime rate.  $T$  represents a vector of year fixed effects to capture any shocks that were experienced by all cities in a specific year.  $G$  is a vector of city fixed effects that control for unobservable factors that did not vary over time, but varied across the cities. The  $X_{it}$  vector of control variables contains a state-specific employment index (equal to 100 in 1929) and a

city-specific retail sales measure to help control for changes in general economic activity and regular employment opportunities available in each city. We also include estimates of the percentages of the population that were foreign born, black, illiterate, between the ages of 10 to 34, and the general fertility and infant mortality rates.<sup>13</sup> All else constant, the latter two demographic variables provide insights into the relative economic shocks that the cities experienced in each year, which were then manifest in fertility and infant mortality. We also control for per capita police spending within each city. In addition, two climate measures – the average monthly precipitation and the average monthly temperature in each city in each year – are added to control for any influences of temperature or precipitation on crime rates.<sup>14</sup> Finally,  $\varepsilon_{it}$  is random error.

Although we have controlled for the employment situation, general economic activity, and a variety of other factors, there still may be worries that the relief coefficient is biased due to unobservable factors that were simultaneously correlated with crime and relief spending. Given that the analysis includes city and year fixed effects, the unobservables must vary both across cities and across time. A prime candidate would be the extent of poverty and income inequality in a city. Measures of poverty that varied across time over the entire decade at the city level are generally unavailable for this period and the Depression's shocks likely varied across different segments of the income distribution. We have included the infant mortality rate to help control for poverty because we believe that infant mortality was more of a problem for households in poverty. However, this is only a partial control. Greater poverty was likely to be associated with greater relief spending and with higher crime rates, which would impart a positive bias to the relief coefficient. Moreover, because relief expenditures resulted from a political decision-making process, one variable that relief administrators may have watched was the crime rate in a city. As a result, higher crime in a city may have translated into more relief spending, again imparting a positive bias in an OLS estimate.

To address this potential bias, we adopt an instrumental variables (IV) approach. After controlling for the set of correlates in the analysis, the instrument must be correlated with relief spending but uncorrelated with the estimated error term of the crime regression. Since our estimation uses data

from a panel of cities with year and city fixed effects, our instruments need to vary over time as well as across cities. To achieve this end, we have developed two instruments that help to explain the spatial and temporal allocation of relief spending by all levels of government during the 1930s. First, we have created an instrument that reflects the interaction between two variables that capture the amount of relief spending that was available nationwide and the political clout of the state in which each city was located. Specifically, the instrument is the interaction between the following two variables: the amount of per capita relief spending in each year in all states except the state in which the city was located and the per capita nonfederal land ownership in the specific state in which the city was located. We do not include the levels of each variable separately because the levels of these variables are collinear with the fixed effects in the model. Land area varies cross-sectionally across states but does not change over time, so it would be collinear with the city fixed effects. Meanwhile, the national level of relief spending varied across time, but not across cities, so it would be collinear with the time effects. The interaction of the two variables, however, varies both across time and state.

The two components of the interaction are expected to influence relief spending in the city of interest. Clearly, national trends outside of the state where a city was located would have influenced the time variation of relief spending within each city. Further, Robert Fleck (2001, p. 303) found that land area was an important determinant of the distribution of funds across states. He argued that relatively large states, especially those with disproportionately smaller populations, wielded extra political influence because of the nature of Senate and Electoral College representation. Therefore, after controlling for population size (as we do in the analysis of crime rates), cities located within relatively larger states may have been able to secure more relief spending than comparable cities in relatively smaller states.

At the same time the instrument is likely to be uncorrelated with unobserved factors in the second-stage crime regression. The sizes of the states were established long before the 1930s and the land area of the whole state had little impact on the population density of the cities located there because the state land areas were far larger than the areas covered by the cities and each state had significant rural populations. Meanwhile, the changes in each city's economic and criminal activity were unlikely to have

been significant forces influencing the national relief spending by federal, state, and local governments outside the state where the city was located. There was no national budget limit that served as a binding constraint on total relief spending, as the Roosevelt administration often drew additional funds from Congress throughout the New Deal, and budget deficits were nowhere near the levels they are today.

A second instrument that we have created is the number of months of “severe” or “extreme” wetness in each city in each year. The measure is based on the Palmer Drought Severity Index, which ranges from -6 to +6. Periods of severe wetness range from 3 to 4 on the index and extreme wetness is beyond 4. The purpose of the instrument is to capture extreme periods when the rainfall was so strong that it brought work relief projects to a halt in ways that could not be offset during the rest of the year. The projects generally were able to operate under a broad range of normal rainfall conditions. When the rainfall hit extreme levels, however, the outdoor projects were halted. Given that there were limits on how many hours relief workers could work each month, it was not easy to make up the lost time during the rest of the year. Approximately 6 percent of the city-years experienced a period of severe or extreme rainfall during the period we examine. A question still remains as to whether such heavy rainfall might have influenced crime rates as well. It has been documented that rainfall and temperature can influence crime and we have already included measures of average rainfall and temperature over the year to capture these effects in the second-stage crime regressions. We see no reason, a priori, that extreme rainfall would have an additional effect on the crime rate after controlling for the continuous measures of average rainfall.

Since there is no way to measure the unobservable error in the second-stage crime regression, we can never know for certain that our instruments are uncorrelated with the true error. We can at least examine whether the instruments are uncorrelated with the estimated error from the IV analysis using standard over-identification tests. The results of the tests suggest that the identifying instruments have not been inappropriately omitted from the second-stage crime equation.

Baseline OLS regression results are reported in Table 2 (see Appendix Table 2 for descriptive statistics of the data set). The table reports the OLS estimation of equation (1) where the dependent

variable is the natural log of total property crime (larceny, burglary, robbery, and auto theft) per capita. Since any omitted variable or simultaneity bias is likely positive, the actual effect of relief spending was probably more negative than the OLS coefficients will reveal. Nonetheless, the estimated coefficient for the property crime category is negative and statistically significant. The coefficient of -0.0713 is an elasticity that shows that a 10 percent increase in per capita relief spending was associated with an 0.713 percent decrease in per capita property crime. When we add the other control variables, the coefficient remains close to -0.07.

Because the 1930s was the first decade in which the Uniform Crime Reports were compiled, and new cities were joining the system throughout the decade, the panel data set is unbalanced. Therefore, we examine whether the inclusion or exclusion of specific city-year observations influenced the results. When we re-estimate the equation with a balanced panel of cities for which complete data were available, the sample size drops from 785 to 385. The relief coefficients in the last two columns of Table 2 continue to be statistically significant and they are slightly more negative than before at -0.079 without the controls and -0.103 when the controls are included.

Table 3 reports the OLS results for each individual crime. Most of the negative relationship between relief spending and total property crime came through reductions in robbery and auto theft. The coefficients on both were more negative than for the overall category and were statistically significant. The coefficient for burglaries was less negative and statistically significant, while the larceny coefficient was positive although not statistically significant. The estimated effect of relief on each of the violent crimes tends to be positive and in the case of aggravated assault is positive and statistically different from zero.

The OLS results suggest that New Deal relief spending contributed to reduced property crime, as economic theory and Roosevelt's intuition suggest. Yet there remains the possibility that the coefficient has a positive bias that causes it to understate the reduction in crime associated with more relief spending. Table 4 contains first- and second-stage IV results for aggregate property crime. The estimated coefficients of the instruments are individually statistically significant in the first-stage equation and an F-

test suggests that they are jointly significantly different from zero at the 1 percent confidence level. These results offer confidence that weak instruments are not undermining the consistency of the IV estimate or causing significant small-sample biases (see Bound, Jaeger, and Baker 1995). Moreover, in all of the estimations, the Hausman specification test indicates that the instruments are not significantly correlated with the estimated error of the second-stage crime regression. Thus, once other correlates are included in the estimation, it appears that the instruments themselves have not been inappropriately omitted from the crime equation.

The first-stage coefficients of the instruments are consistent with our expectations. The positive coefficient of the interaction between nonfederal land and national relief spending outside the state of interest suggests that when more national relief spending was available, cities within the relatively large states tended to attract more of these relief dollars. This finding is consistent with Fleck's (2001) view that relatively large, less densely populated states wielded disproportionate influence in the distribution of New Deal relief. The second instrument – months of severe or extreme precipitation within the city in each year – also performs as expected. Unusually heavy rainfall slowed the progress of relief projects and, hence, the distribution of overall relief funds.

The IV estimate of the effect of per capita relief spending on total property crime is substantially larger in absolute value than the OLS estimate. The estimated effect increases from -0.07 in the OLS estimation to -0.57 in the IV estimation using the unbalanced panel to -0.971 using the balanced panel. The coefficients are statistically significant as well. The results suggest that the OLS estimate was biased in a positive direction because relief administrators sought to spend more when crime rates were higher. The elasticity estimates from the IV suggest that a 10 percent increase in per capita relief spending decreased per capita property crime by between 5.7 and 9.7 percent, depending on whether one relies on the estimates from the balanced or the unbalanced panel.<sup>15</sup>

The elasticities from the IV estimation for the individual property crimes show similar patterns. As is the case for total property crimes, the IV elasticity estimates are substantially more negative than the OLS estimates. The overall property crime elasticity is reflective of the strong negative relationships

between relief spending and auto theft, robbery and burglary. These outweigh a positive and statistically insignificant relationship with larceny, which is the least accurately measured of the property crimes.

To put these effects into historical context, consider that the average level of per capita relief spending over the decade was approximately \$42 (constant 1967 dollars). The average absolute value year-to-year change was approximately \$10 (1967 dollars), which is 23.8 percent of the decade average. The two point estimates for total property crimes suggest that this relatively common 23.8 percent change in per capita relief spending would have reduced overall property crime rates by roughly 13 to 23 percent, with somewhat stronger effects on reported robbery, auto theft and burglary and little effect on larceny.

Since the violent crimes of murder, aggravated assault, and rape are less motivated by economic factors than are property crimes, we anticipated that relief spending would be less successful in reducing their incidence. This expectation is borne out in the OLS estimations in Table 3 and the IV estimations in Table 5. The OLS coefficients are positive for each of the three violent crimes but two of the three are statistically insignificant. The IV estimation suggests negative elasticities for murder and rape while the elasticity for aggravated assault stays positive, but none are statistically significant. The negative but statistically insignificant IV results for murder are similar to results found by Fishback, Kantor, and Haines (forthcoming) in their analysis of the homicide category from cause-of-death reports by cities to the U.S. Bureau of the Census during the period 1929 through 1937. In their analysis with a set of correlates more focused on demographic issues, the elasticity estimate using OLS and fixed effects was -0.069 and the IV estimate was -0.453.<sup>16</sup> Neither of the coefficients on which the elasticities are based was statistically significant at the 10 percent level.

Finally, there might be some concern that the repeal of Prohibition in 1933, which coincided with the introduction of the New Deal, may be confounding the estimated effects of relief spending. The year fixed effects should control for any impact that the national repeal of Prohibition had on crime across all cities, yet some cities were in counties that chose to continue to restrict the sale of alcohol after Prohibition was repealed. In 1935 only about nine percent of the cities in our sample were in counties that were entirely dry.<sup>17</sup> Year fixed effects would not capture these nuances in the law. If we restrict our



analysis to cities that were in wet counties so that the year dummies better capture the effects of the repeal of Prohibition, the estimated coefficients do not change substantially.

Although many of the coefficients of the control variables are statistically insignificant, there are some results worth noting. Nearly all of our estimates of the impact of per capita police spending on property crimes are negative, although only the OLS fixed-effects coefficients for auto theft, robbery, and burglary are statistically significant. Police spending had a relatively strong negative and statistically significant effect on the murder rate. Given our focus on relief spending, we have not attempted to control for endogeneity of police spending. We expect that most cities responded to higher crime rates by increasing police spending, so our coefficient estimates likely understate the negative effect of police spending on crime rates.

There is some evidence that increases in economic activity, measured as retail sales, were associated with increased criminal activity. The per capita retail sales elasticities were roughly 0.36 and statistically significant for total property crime rates in the IV regressions. The results are consistent with a “looting effect,” in which general increases in income expand the targets of opportunity for property crimes, in essence, raising the marginal benefit of committing crimes. The strongest effect was found for auto theft, which is not surprising since the purchase of automobiles in the 1930s was likely strongly tied to success in the general economy. It is important to note that we have not controlled for potential endogeneity between crime rates and economic activity, so that these relationships are correlations and should not be interpreted as causal.

#### **IV. Conclusion**

The economic downturn associated with the Great Depression pushed millions of American workers and their families into personal economic crises. With legal employment opportunities significantly limited by the Depression, some families facing desperate circumstances for the first time no doubt turned to illegal means for subsistence. The unprecedented relief spending accompanying the New Deal helped alleviate distress by providing work and income opportunities for the unemployed. One

salutary effect of the expansion in relief programs was a sharply diminished crime rate. Our empirical analysis suggests that New Deal relief lowered property crime in a statistically and economically significant way. A lower bound estimate suggests that a 10 percent increase in per capita relief spending during the Great Depression lowered property crime rates by close to 1 percent. This lower bound estimate likely understates the true effect on crime because cities typically responded to increases in crime by spending more on relief. When we control for this endogeneity using instrumental variable methods, we find that a 10 percent increase in per capita relief spending lowered crime rates by roughly 5.6 to 10 percent at the margin.

The effect of relief spending, which was targeted at the unemployed and lower income groups, might have had quite different effects on crime rates than did expansions in general economic activity. Economy-wide improvements may have been associated with higher crime rates, by providing more targets of opportunity. This “looting effect” was most prominent for theft of automobiles, a major consumer durable purchased more widely during upturns.

It appears that Roosevelt’s intuition expressed in his 1939 speech, quoted at the beginning of the paper, was accurate. New Deal relief spending, so carefully targeted at the lower end of the income scale during a major economic crisis, appears to have “struck at the very roots of crime” during the 1930s. The results in this paper add to a list of salutary effects that New Deal spending has been found to produce. Other recent studies suggest that relief expenditures were associated with lower infant mortality, higher birth rates, more in-migration, and expansions in economic activity.<sup>18</sup>

More generally, our results indicate that social insurance, which tends to be understudied in economic analyses of crime, should be more explicitly and more carefully incorporated into the analysis of temporal and spatial variations in criminal activity. Our results offer limited information about the modern worries about a culture of poverty and crime associated with expansions in the generosity of long-term welfare programs. After all, the Great Depression was a major crisis that led to the introduction of many of the features of the modern welfare system. The New Deal experience says much more about the impact of social insurance in settings where people are confronted with an extraordinary

economic misfortune. Our results suggest that for such people who are suddenly faced with greater temptation to steal, the availability of social insurance might well tilt the balance in favor of lawful behavior.

## FOOTNOTES

<sup>1</sup> Franklin D. Roosevelt, "Address at the National Parole Conference," Washington, DC, April 17, 1939. Accessible at The American Presidency Project, <http://www.presidency.ucsb.edu>.

<sup>2</sup> For recent empirical work examining the effects of county-level New Deal expenditures on infant mortality, geographic migration, and retail consumption, respectively, see Fishback, Haines and Kantor (forthcoming) and Fishback, Horrace, and Kantor (2006 and 2005). Wallis and Benjamin (1984), Margo (1994), and Fleck (1999a) consider questions of how New Deal relief programs affected private labor markets. See Cole and Ohanian (2004) for a more theoretical treatment of how the National Industrial Recovery Act's anticompetitive policies hampered the private economic recovery from the Great Depression.

<sup>3</sup> See, for example, Raphael and Winter-Ebmer (2001), Gould, Weinberg, and Mustard (2002), Machin and Meghir (2004), Edmark (2005), and Arvanites and Defina (2006) for recent econometric studies establishing the inverse relationship between labor market opportunities (employment and wages) and property crime rates.

<sup>4</sup> In two recent articles that provide a survey of the crime literature from an economics perspective (Witte and Witt 2001 and Levitt 2004), social insurance was ignored as a potential determinant of international or temporal variations in crime. Some notable exceptions in the literature include Zhang (1997), which is one of the only studies in economics to explicitly model and empirically measure the impact of welfare payments. He found that public housing assistance had much greater effects on crime reduction than such programs as Aid to Families with Dependent Children, Medicaid, or the school lunch program. Hashimoto (1987) examines the link between increases in the minimum wage with teenage crime. Hansen and Machin (2002) explore the effect of the minimum wage in the UK and find that areas that had relatively more low-wage workers who benefited from the minimum wage floor experienced reduced crime. Lochner and Moretti (2004) and Lochner (2004) explore the link between educational attainment and skill level and crime. They find rather significant social returns to programs that contribute to

educational attainment. Donohue and Siegelman (1998) consider the counterfactual case of reallocating money away from prisons and into targeted preschool programs. They contend that crime could be reduced without greater social spending if large-scale increases in prison expenditures were diverted to preschool interventions. Finally, in terms of other types of social policy that may influence crime, see Donahue and Levitt (2001) who argue that legalized abortion may account for up to 50 percent of the recent drop in crime.

Sociologists have taken up the question of how welfare influences crime, but, like economists, have not delved deeply into the question. See DeFronzo (1983), Devine, Sheley, and Smith (1988), and Hannon and DeFronzo (1998) for empirical studies of the link between public assistance and crime.

<sup>5</sup> When identifying the effect of unemployment on crime, Cook and Zarkin (1985) point out that there are a number of omitted variables that may vary with the business cycle that might affect crime, yet are not always easily included in empirical studies of crime. The factors that can lead to omitted-variable bias include measures of legitimate employment opportunities, the presence of criminal opportunities, the consumption of drugs, alcohol, or guns, and the quantity and effectiveness of the police and criminal justice system.

<sup>6</sup> See Winslow (1937), Baird (1942), Skocpol (1992, ch. 2), Clark, Craig, and Wilson (2003), and Fishback and Thomasson (2006, 2:709).

<sup>7</sup> For an empirical analysis of the distribution of FERA grants, see Fleck (1999b) and Fishback, Kantor and Wallis (2003). For discussions of the administrative details of relief provision, see Brown (1940), Howard (1943), and U.S. National Resources Planning Board (1942).

<sup>8</sup> Our relief measure includes some privately administered relief spending, which accounted for roughly one-quarter of relief spending prior to 1933, but less than one percent thereafter. Privately administered relief in many cases came from government sources. More detailed breakdowns for urban areas can be found in Baird (1942). Several programs that might be considered public assistance were not included in the reported relief data. For example, FERA emergency education, student aid, and transient programs

were omitted. There is no information on the Civilian Conservation Corps, which provided employment in more isolated areas for up to two and a half young men from 1933 to 1940. Pandiani (1982) argues that the CCC probably contributed significantly to crime reduction in cities because the program targeted poor young men who were the most likely to have committed crime. We cannot perform an effective analysis of the impact of the CCC because we have not been able to obtain annual estimates of the number of CCC workers who were recruited from each city. Information on CCC spending in each state is inadequate because many northeastern cities contributed CCC workers but there were few CCC camps in the Northeast. Similarly, spending on the National Youth Administration was omitted. Furthermore, we have no data on the distribution of federal surplus commodities, which were bought from farmers as an agricultural relief measure. This form of relief did not account for a large share of aid in urban areas, but was important in rural areas. Also, information on earnings under the Public Works Administration was not included. These projects tended to hire more skilled workers and, thus, the program's exclusion probably does not affect our conclusions regarding the effect of relief on the urban poor. At the state level the correlation between per capita New Deal relief spending and New Deal public works spending for the years 1933 through 1939 was around 0.36. It is therefore possible that some of the effects that we are attributing to relief spending might be associated with New Deal public works spending. Since the public works spending itself was partly devoted to providing employment opportunities for the poor and the unemployed, we do not see this as a serious source of error in using the relief coefficient we estimate to get a sense of the overall impact of social welfare spending on crime.

<sup>9</sup> For discussions of the FERA and CWA policies, see Brown (1940, 218-98) and U.S. National Resources Planning Board (1942, 26-97).

<sup>10</sup> Burglary is defined as the "unlawful entry of a structure to commit a felony or theft." Larceny-theft is the "unlawful taking . . . of property from possession . . . of another . . . in which no use of force, violence, or fraud occurs." Motor vehicle theft is self-explanatory.

<sup>11</sup> See Zhang (1997) for an explicit theoretical model of how welfare payments influence the decision to

commit crime. Grogger (1998) provides a theoretical analysis of the incentive effects of wages on criminal behavior. Whether we view New Deal relief as an income supplement or as a means of raising reservation wages of individuals, the theoretical models predict the same outcome: relief should diminish the incentive to commit property crime.

<sup>12</sup> We have explored estimating separate effects of work relief and direct relief. The two forms of relief were high interactive and households moved between the types of relief and sometimes were parts of multiple programs simultaneously. As a result, the instruments we have developed have significant explanatory strength when analyzing total relief spending, but have very little strength in explaining the distribution of each form of relief.

<sup>13</sup> The percent black, percent foreign-born, percent illiterate, and age variables were reported by the Census for 1930 and 1940 and we used linear interpolations for the years between. Retail sales information was reported for 1929, 1933, 1935, and 1939. We interpolated the intervening years using personal income figures for the states.

<sup>14</sup> See, for example, Field (1992), Cohn and Rotton (2000), Rotton and Cohn (2003), and Simister and Cooper (2005).

<sup>15</sup> To check the robustness of the IV results, we have estimated the IV model with each instrument alone. When used alone, the instruments have the expected signs in the first stage. The F-statistics for the identifying instrument for the various crime types is 4.47 for the area/national relief interaction alone and 4.83 for the excessive rainfall alone. The results of the hypotheses tests for total property crimes, burglaries, robbery, and auto theft are very similar to those reported in Tables 4 and 5. The elasticity for total property crimes in the balanced panel is around -0.5 when the instruments are used alone. The elasticities for burglary, robbery, and auto theft are somewhat more negative using the area/national relief interaction than the reported results. The elasticities are somewhat less negative using the excessive rainfall measure as the only instrument.

<sup>16</sup> Fishback, Kantor, and Haines (forthcoming) did not report elasticities. We converted their coefficients

to elasticities using the means of \$25.08 for per capita relief and 11.689 for homicides per 100,000 people from the sample for 1929 through 1937. The homicide measure is broader than the murder measure we use. In the sample where the two measures overlap the correlation between the homicide and murder rates is around 0.90.

<sup>17</sup> We thank Koleman Strumpf for providing evidence on the decisions made by individual counties to remain wet or dry after Prohibition.

<sup>18</sup> See Fishback, Haines, and Kantor (forthcoming) and Fishback, Horraine, and Kantor (2005).



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**Table 1****Trends in Relief Spending for 114 U.S. Cities, 1930 to 1940**

| <b>Year</b> | <b>Unemp.<br/>Rate<br/>(Entire<br/>U.S.)</b> | <b>Per<br/>capita<br/>relief<br/>spending<br/>(1967 \$)</b> | <b>Average<br/>annual relief<br/>benefits as a<br/>percentage of<br/>annual<br/>manufacturing<br/>earnings</b> | <b>Federal<br/>share of<br/>relief<br/>spending</b> |
|-------------|--|---|--|---|
| 1930        | 9.0  | \$3.74  |  |   |
| 1931        | 16.3   | 9.06  |  |   |
| 1932        | 24.1   | 18.06   |  | 2.1%  |
| 1933        | 25.2   | 29.71   | 21.7%  | 51.8  |
| 1934        | 22.0   | 47.93   | 31.2   | 78.9  |
| 1935        | 20.3   | 51.03   | 33.3   | 78.9  |
| 1936        | 17.0   | 61.78   | 42.4   | 74.7  |
| 1937        | 14.3   | 52.24   | 37.6   | 72.1  |
| 1938        | 19.1   | 69.61   | 39.3   | 62.0  |
| 1939        | 17.2   | 63.32   | 38.3   | 62.5  |
| 1940        | 14.6   | 52.93   | 34.9   | 57.4  |

Notes: Per capita relief spending represents a population-weighted average for 114 cities. Relief per capita is the total of all direct relief, work relief and private relief funds. Direct relief includes direct relief under the FERA, by state and local governments, and categorical assistance for dependent children, old-age assistance, and aid to the blind. Prior to 1935 the categorical assistance categories refer to funds provided by state and local governments through mothers' pensions, old-age pensions, and state aid to the blind. Work relief includes payments to workers on state and local government, FERA, CWA, and WPA projects. Private relief is the value of relief funds from private and public sources administered by private agencies. Average annual relief benefits were calculated as the ratio of total relief expenditures per number of households on relief. The data source reported the information monthly and we summed across months for the annual estimate. The 1940 data were only reported through June, so we doubled the amount reported to derive the annual estimate. We do not have information on the federal share of relief prior to 1932, but it was probably similar to 1932's value. The federal share of relief information includes the cost of administering the programs. The 1932 federal figure includes \$3.7 million in federal workers' compensation payments. The state and local expenditures include workers' compensation, general relief, old-age assistance, aid to dependent children, aid to the blind, and state shares of unemployment compensation, WPA, CWA, and the National Youth Administration.

Sources: Relief spending data are reported in Baird (1942) and Winslow (1937). Population data are from Haines and ICPSR (2005). Linear interpolation was used for years between 1930 and 1940 censuses. For federal share of relief spending, see U.S. National Resources Planning Board (1942, 292, 598-603). Average relief expenditures per household were calculated from data on households receiving relief and total expenditures on relief in U.S. National Resources Planning Board (1942, 557-61). Average annual manufacturing earnings are from U.S. Bureau of the Census Bureau (1975, 166). Unemployment rates for the entire United States are calculated from Series Ba470, Ba474, and Ba477 in Carter, et. al. (2006, 2:82-83). Federal emergency relief workers were included as unemployed in this calculation.

**Table 2****OLS Estimates of Aggregate Property Crime, 1930-1940**

| <b>Variables</b>                  | <b>Unbalanced Panel</b> |            |            | <b>Balanced Panel<sup>a</sup></b> |           |
|-----------------------------------|-------------------------|------------|------------|-----------------------------------|-----------|
| Log of relief spending per capita | -0.0713*                | -0.0684*   | -0.0678*   | -0.0790**                         | -0.1029*  |
|                                   | (0.0224)                | (0.0256)   | (0.0257)   | (0.0320)                          | (0.0332)  |
| State employment index            |                         | 0.0013     | 0.0014     |                                   | 0.0072*   |
|                                   |                         | (0.0019)   | (0.0019)   |                                   | (0.0027)  |
| Log of retail sales per capita    |                         | 0.1832     | 0.1897     |                                   | 0.2514    |
|                                   |                         | (0.1422)   | (0.1431)   |                                   | (0.2465)  |
| Log of police spending per capita |                         |            | -0.0814    |                                   | -0.0852   |
|                                   |                         |            | (0.0868)   |                                   | (0.1208)  |
| Percent foreign born              |                         | 0.0144     | 0.01553    |                                   | 0.0460    |
|                                   |                         | (0.0115)   | (0.0114)   |                                   | (0.0365)  |
| Percent black                     |                         | -0.0143    | -0.0133    |                                   | -0.0609   |
|                                   |                         | (0.0370)   | (0.0372)   |                                   | (0.0577)  |
| Percent illiterate                |                         | -0.0060    | -0.0040    |                                   | 0.0189    |
|                                   |                         | (0.0283)   | (0.0283)   |                                   | (0.0379)  |
| Percent between ages 10 and 34    |                         | -0.0008    | -0.0017    |                                   | -0.0466   |
|                                   |                         | (0.0232)   | (0.0232)   |                                   | (0.0368)  |
| Log of city population            |                         | 0.1177     | 0.0991     |                                   | -1.288    |
|                                   |                         | (0.3971)   | (0.4061)   |                                   | (1.120)   |
| General fertility rate            |                         | -0.0052*** | -0.0049*** |                                   | -0.0057   |
|                                   |                         | (0.0027)   | (0.0027)   |                                   | (0.0040)  |
| Infant mortality rate             |                         | 0.0003     | 0.0004     |                                   | 0.0009    |
|                                   |                         | (0.0013)   | (0.0013)   |                                   | (0.0017)  |
| Average monthly precipitation     |                         | 0.0403**   | 0.0412*    |                                   | 0.0499*** |
|                                   |                         | (0.0161)   | (0.0160)   |                                   | (0.0267)  |
| Average monthly temperature       |                         | 0.0025     | 0.0021     |                                   | 0.0181    |
|                                   |                         | (0.0087)   | (0.0087)   |                                   | (0.0164)  |
| Year fixed effects                | included                | included   | Included   | included                          | included  |
| City fixed effects                | included                | included   | Included   | included                          | included  |
| N                                 | 798                     | 785        | 785        | 385                               | 385       |
| R <sup>2</sup>                    | 0.880                   | 0.881      | 0.881      | 0.826                             | 0.838     |

\* Statistically significant at better than 1 percent confidence level.

\*\* Statistically significant at better than 5 percent confidence level.

\*\*\* Statistically significant at better than 10 percent confidence level.

<sup>a</sup> A listing of the cities in the balanced panel is reported in Appendix Table 1. Forth Worth and San Antonio, Texas have been excluded because Texas demographic information was not reported until 1933.

Notes: The dependent variable is the natural log of total property crimes per capita. Property crimes include burglary, larceny, robbery, and automobile theft. White-corrected standard errors are in parentheses.

Sources: For the sources of the relief spending data, see Table 1. The crime data were collected from the U.S. Federal Bureau of Investigation (various years). The state employment index is from Wallis (1989) and the demographic data are from Haines and ICPSR (2005). Police spending was collected from U.S.

Bureau of Census, Financial Statistics (various years). We were able to collect retail sales information for the counties in which the cities were located for 1929, 1933, 1935, and 1939. Retail sales from 1929 and 1939 are from the amended ICPSR file and retail sales for 1933 and 1935 are from U.S. Department of Commerce, Bureau of Foreign and Domestic Commerce (1936, 1939). The population estimate used to create the per capita measure was based on straight-line interpolations between 1920, 1930, and 1940 data from Haines and ICPSR (2005). We interpolated values of per capita retail sales in the intervening years using estimates of state personal income from the U.S. Bureau of Economic Analysis (1989). For each year between 1930 and 1940, we divided state personal income by an estimate of state population. Then to interpolate per capita retail sales between the benchmark years of 1929, 1933, 1935, and 1939, we used a formula such as the following for 1931:  $R_{31} = R_{29} + (R_{33} - R_{29}) * (S_{31} - S_{29}) / (S_{33} - S_{29})$ , where R is per capita retail sales in the appropriate year for the county in which the city was located and S is per capita state personal income in the same year. Fertility and infant mortality data are from the U.S. Bureau of the Census, Birth, Stillbirth and Infant Mortality Statistics (various years). Rainfall and temperature data are from the National Climatic Data Center (NCDR). Text files of the data were accessed from <ftp://ftp.ncdc.noaa.gov/pub/data/cirs/> (August 2003). The NCDR reports historical monthly data by climate division within each state, so each city's climate information pertains to its respective climate division. If a city was located within two or more divisions, climate information was calculated as the average across the climate divisions in which the city was located.

**Table 3****OLS Estimates for Individual Crimes, 1930-1940**

| Variables                         | Property Crimes       |                       |                        |                       | Violent Crimes        |                       |                      |
|-----------------------------------|-----------------------|-----------------------|------------------------|-----------------------|-----------------------|-----------------------|----------------------|
|                                   | larceny               | burglary              | robbery                | auto theft            | murder                | aggravated assault    | rape                 |
| Log of relief spending per capita | 0.0852<br>(0.1044)    | -0.0538<br>(0.0375)   | -0.0854***<br>(0.0498) | -0.1480*<br>(0.0363)  | 0.1198<br>(0.1395)    | 0.1562***<br>(0.0924) | 0.2002<br>(0.1991)   |
| State employment index            | -0.0035<br>(0.0041)   | 0.0008<br>(0.0026)    | 0.0071**<br>(0.0033)   | 0.0005<br>(0.0027)    | 0.0160<br>(0.0100)    | 0.0157*<br>(0.0054)   | 0.0030<br>(0.0166)   |
| Log of retail sales per capita    | -0.1186<br>(0.2799)   | 0.1818<br>(0.2078)    | 0.2551<br>(0.2534)     | 0.5518**<br>(0.2441)  | -0.2294<br>(0.5716)   | -0.3444<br>(0.3368)   | 1.142<br>(1.054)     |
| Log of police spending per capita | 0.1193<br>(0.1237)    | -0.1644<br>(0.1287)   | -0.2756***<br>(0.1669) | -0.3770**<br>(0.1520) | -1.183***<br>(0.6403) | 0.2023<br>(0.2412)    | 0.6004<br>(0.7121)   |
| Percent foreign born              | -0.0547<br>(0.0594)   | 0.0093<br>(0.0170)    | 0.0145<br>(0.0221)     | 0.0116<br>(0.0207)    | -0.0159<br>(0.0544)   | 0.0178<br>(0.0421)    | -0.1800<br>(0.1304)  |
| Percent black                     | 0.0640<br>(0.0515)    | 0.0275<br>(0.0510)    | 0.1036***<br>(0.0586)  | -0.1703*<br>(0.0568)  | 0.1094<br>(0.1323)    | 0.0785<br>(0.0990)    | -0.1005<br>(0.4049)  |
| Percent illiterate                | -0.1629**<br>(0.0825) | -0.0645<br>(0.0550)   | -0.0132<br>(0.0732)    | 0.1829*<br>(0.0550)   | -0.2938<br>(0.2624)   | 0.0826<br>(0.1028)    | -1.434*<br>(0.4868)  |
| Percent between ages 10 and 34    | -0.0632<br>(0.0536)   | -0.0153<br>(0.0423)   | 0.1061**<br>(0.0464)   | 0.0672**<br>(0.0339)  | -0.0440<br>(0.1127)   | -0.0007<br>(0.0638)   | 0.0056<br>(0.2271)   |
| Log of city population            | 2.509***<br>(1.329)   | -0.2652<br>(0.5117)   | -1.318***<br>(0.6786)  | -0.9973*<br>(0.3875)  | -0.0576<br>(1.213)    | 1.032<br>(0.9886)     | 1.772<br>(2.514)     |
| General fertility rate            | 0.0003<br>(0.0073)    | -0.0106**<br>(0.0048) | 0.0029<br>(0.0058)     | 0.0006<br>(0.0038)    | -0.0104<br>(0.0161)   | -0.0056<br>(0.0077)   | -0.0695*<br>(0.0260) |
| Infant mortality rate             | -0.0058<br>(0.0055)   | 0.0003<br>(0.0020)    | 0.0038<br>(0.0027)     | 0.0007<br>(0.0021)    | -0.0039<br>(0.0092)   | -0.0037<br>(0.0043)   | -0.0060<br>(0.0109)  |
| Average monthly precipitation     | 0.0169<br>(0.0278)    | 0.0428***<br>(0.0224) | 0.0572***<br>(0.0323)  | 0.0422**<br>(0.0215)  | 0.0115<br>(0.0747)    | -0.0238<br>(0.0795)   | -0.0587<br>(0.1292)  |
| Average monthly temperature       | -0.0787<br>(0.0797)   | -0.0110<br>(0.0127)   | -0.0208<br>(0.0161)    | 0.0209<br>(0.0122)    | -0.0028<br>(0.0502)   | 0.0660**<br>(0.0267)  | -0.0415<br>(0.0937)  |
| Year fixed effects                | included              | included              | included               | included              | included              | included              | included             |
| City fixed effects                | included              | included              | included               | included              | included              | included              | included             |
| N                                 | 785                   | 785                   | 785                    | 785                   | 783                   | 784                   | 467                  |
| R <sup>2</sup>                    | 0.671                 | 0.784                 | 0.860                  | 0.785                 | 0.582                 | 0.804                 | 0.572                |

- \* Statistically significant at better than 1 percent confidence level.
- \*\* Statistically significant at better than 5 percent confidence level.
- \*\*\* Statistically significant at better than 10 percent confidence level.

Notes: The dependent variable is the natural log of the various crimes per capita. White corrected standard errors are in parentheses.

Sources: See Table 2.



**Table 4****IV Estimates of Aggregate Property Crime, 1930-1940 (with First-Stage Results)**

| Variables   | Unbalanced Panel      |                       |                       |                       | Balanced Panel <sup>a</sup> |
|---|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------------|
|   | First Stage           | Second stage          | First Stage           | Second stage          | Second stage                |
| Log of relief spending per capita   |                       | -0.5695**<br>(0.2447) |                       | -0.5710**<br>(0.2432) | -0.9705**<br>(0.4184)       |
| <i>Instruments:</i>   |                       |                       |                       |                       |                             |
| Log of per capita relief spending nationally (outside of state of interest) x Per capita nonfederal land in state | 3.960**<br>(1.760)    |                       | 4.014**<br>(1.792)    |                       |                             |
| Months of severe or extreme precipitation in city i during year t   | -0.0322**<br>(0.0128) |                       | -0.0322**<br>(0.0128) |                       |                             |
| <i>Exogenous variables:</i>   |                       |                       |                       |                       |                             |
| State employment index  | -0.0036<br>(0.0036)   | -0.0016<br>(0.0028)   | -0.0037<br>(0.0036)   | -0.0016<br>(0.0028)   | 0.0018<br>(0.0060)          |
| Log of retail sales per capita  | 0.2993<br>(0.2469)    | 0.3617***<br>(0.1919) | 0.2884<br>(0.2486)    | 0.3647***<br>(0.1910) | 0.6268<br>(0.4527)          |
| Log of police spending per capita   |                       |                       | 0.1283<br>(0.1566)    | -0.0289<br>(0.1240)   | 0.0164<br>(0.2380)          |
| Percent foreign born  | 0.0619<br>(0.0328)    | 0.0501***<br>(0.0272) | 0.0600***<br>(0.0330) | 0.0506***<br>(0.0271) | 0.2823<br>(0.1281)          |
| Percent black   | -0.0902<br>(0.0748)   | -0.0718<br>(0.0615)   | -0.0913<br>(0.0752)   | -0.0717<br>(0.0618)   | -0.2182<br>(0.1358)         |
| Percent illiterate  | 0.1403**<br>(0.0703)  | 0.0648<br>(0.0608)    | 0.1370**<br>(0.0697)  | 0.0658<br>(0.0602)    | 0.2483***<br>(0.1443)       |
| Percent between ages 10 and 34  | -0.0088<br>(0.0345)   | -0.0206<br>(0.0286)   | -0.0071<br>(0.0346)   | -0.0209<br>(0.0286)   | -0.1258***<br>(0.0727)      |
| Log of city population  | -0.3322<br>(1.045)    | 0.1001<br>(0.7641)    | -0.3081<br>(1.028)    | 0.0934<br>(0.7641)    | -1.119<br>(2.350)           |
| General fertility rate  | 0.0134*<br>(0.0049)   | 0.0032<br>(0.0049)    | 0.128*<br>(0.0049)    | 0.0033<br>(0.0047)    | -0.0055<br>(0.0069)         |
| Infant mortality rate   | 0.0039<br>(0.0024)    | 0.0024<br>(0.0020)    | 0.0038<br>(0.0024)    | 0.0024<br>(0.0020)    | 0.0034<br>(0.0036)          |
| Average monthly precipitation   | 0.01136<br>(0.0300)   | 0.0271<br>(0.0225)    | 0.0102<br>(0.0301)    | 0.0274<br>(0.0226)    | 0.0064<br>(0.0495)          |
| Average monthly temperature   | 0.0459*<br>(0.0138)   | 0.0327***<br>(0.0195) | 0.0465*<br>(0.0139)   | 0.0327***<br>(0.0196) | 0.1017**<br>(0.0441)        |
| Year fixed effects  | included              | included              | included              | included              | Included                    |
| City fixed effects  | Included              | included              | included              | Included              | Included                    |
| N   | 785                   | 785                   | 785                   | 785                   | 385                         |
| F-test of instruments   | 5.11                  |                       | 5.08                  |                       | 2.88                        |
| Hausman test-statistic <sup>b</sup>   | 0.236                 |                       | 0.236                 |                       | 0.039                       |

<sup>a</sup> A listing of the cities in the balanced panel is reported in Appendix Table 1.

<sup>b</sup>The Hausman test in this case is a chi-squared test with one degree of freedom. The rejection of the hypothesis of no relationship between the instruments and the error term in the final-stage crime equation occurs at values of 1.32 for a p-value of 0.25 and 2.71 for a p-value of 0.10

\* Statistically significant at better than 1 percent confidence level.

\*\* Statistically significant at better than 5 percent confidence level.

\*\*\* Statistically significant at better than 10 percent confidence level.

Sources: See Table 2

Table 5

## IV Estimates of Individual Crimes, 1930-1940

| Variables                         | Property Crimes     |                       |                       |                       | Violent Crimes        |                      |                       |
|-----------------------------------|---------------------|-----------------------|-----------------------|-----------------------|-----------------------|----------------------|-----------------------|
|                                   | larceny             | burglary              | robbery               | auto theft            | murder                | aggravated assault   | rape                  |
| Log of relief spending per capita | 0.7325<br>(0.9028)  | -0.8809**<br>(0.3889) | -1.324*<br>(0.4845)   | -1.134*<br>(0.3448)   | -0.3237<br>(0.7328)   | 0.1924<br>(0.5444)   | -0.1981<br>(0.8898)   |
| State employment index            | 0.0003<br>(0.0043)  | -0.0040<br>(0.0041)   | -0.0002<br>(0.0063)   | -0.0053<br>(0.0046)   | 0.0135<br>(0.0113)    | 0.0159**<br>(0.0063) | 0.0011<br>(0.0160)    |
| Log of retail sales per capita    | -0.3437<br>(0.5208) | 0.4693<br>(0.3035)    | 0.6855<br>(0.4184)    | 0.8945*<br>(0.3432)   | -0.0695<br>(0.6296)   | -0.3570<br>(0.3706)  | 1.379<br>(1.206)      |
| Log of police spending per capita | 0.0516<br>(0.1900)  | -0.0779<br>(0.1813)   | -0.1463<br>(0.2487)   | -0.2740<br>(0.2358)   | -1.136***<br>(0.6511) | 0.1985<br>(0.2377)   | 0.6355<br>(0.6919)    |
| Percent foreign born              | -0.0998<br>(0.1119) | 0.0669<br>(0.0444)    | 0.1007***<br>(0.0566) | 0.0802***<br>(0.0462) | 0.0154<br>(0.0745)    | 0.0153<br>(0.0560)   | -0.0996<br>(0.2519)   |
| Percent black                     | 0.1390<br>(0.1168)  | -0.0684<br>(0.0885)   | -0.0400<br>(0.1197)   | -0.2846*<br>(0.1080)  | 0.0603<br>(0.1599)    | 0.0827<br>(0.1190)   | -0.1233<br>(0.4145)   |
| Percent illiterate                | -0.2527<br>(0.1836) | 0.0501<br>(0.0993)    | 0.1584<br>(0.1222)    | 0.3195*<br>(0.0974)   | -0.2331<br>(0.2683)   | 0.0776<br>(0.1180)   | -1.291**<br>(0.6032)  |
| Percent between ages 10 and 34    | -0.0385<br>(0.0462) | -0.0469<br>(0.0503)   | 0.0588<br>(0.0588)    | 0.0295<br>(0.0510)    | -0.0601<br>(0.1152)   | 0.0007<br>(0.0662)   | 0.0027<br>(0.2273)    |
| Log of city population            | 2.517***<br>(1.350) | -0.2745<br>(0.9332)   | -1.331<br>(1.659)     | -1.008<br>(1.084)     | -0.0653<br>(1.303)    | 1.032<br>(0.9988)    | 1.335<br>(3.241)      |
| General fertility rate            | -0.0102<br>(0.0104) | 0.0029<br>(0.0079)    | 0.0230**<br>(0.0104)  | 0.0166**<br>(0.0075)  | -0.0031<br>(0.0204)   | -0.0062<br>(0.0103)  | -0.0639**<br>(0.0258) |
| Infant mortality rate             | -0.0085<br>(0.0084) | 0.0037<br>(0.0032)    | 0.0089**<br>(0.0044)  | 0.0047<br>(0.0036)    | -0.0020<br>(0.0097)   | -0.0039<br>(0.0050)  | -0.0025<br>(0.0144)   |
| Average monthly precipitation     | 0.0346<br>(0.0346)  | 0.0201<br>(0.0323)    | 0.0233<br>(0.0483)    | 0.0152<br>(0.0374)    | -0.0001<br>(0.0788)   | -0.0228<br>(0.0763)  | -0.0656<br>(0.1331)   |
| Average monthly temperature       | -0.1181<br>(0.1226) | 0.0393<br>(0.0297)    | 0.0545<br>(0.0373)    | 0.0808*<br>(0.0283)   | 0.0244<br>(0.0711)    | 0.0638<br>(0.0413)   | -0.0074<br>(0.1452)   |
| Year fixed effects                | included            | included              | included              | included              | included              | included             | included              |
| City fixed effects                | included            | included              | included              | included              | included              | included             | included              |
| N                                 | 785                 | 785                   | 785                   | 785                   | 783                   | 784                  | 467                   |

|                                     |       |       |       |       |       |       |       |
|-------------------------------------|-------|-------|-------|-------|-------|-------|-------|
| F-test of instruments               | 5.08  | 5.08  | 5.08  | 5.08  | 5.08  | 5.07  | 4.40  |
| Hausman test-statistic <sup>a</sup> | 1.413 | 0.785 | 0.550 | 0.785 | 1.018 | 2.430 | 2.849 |

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<sup>a</sup> The Hausman test in this case is a chi-squared test with one degree of freedom. The rejection of the hypothesis of no relationship between the instruments and the error term in the final-stage crime equation occurs at values of 1.32 for a p-value of 0.25 and 2.71 for a p-value of 0.10

\* Statistically significant at better than 1 percent confidence level.

\*\* Statistically significant at better than 5 percent confidence level.

\*\*\* Statistically significant at better than 10 percent confidence level.

Notes: The dependent variable is the natural log of the various crimes per capita. White corrected standard errors are in parentheses.

Sources: See Table 2.

**Appendix Table 1****List of 83 Cities in the Sample**


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|                  |                    |                      |
|------------------|--------------------|----------------------|
| Akron, OH *      | Grand Rapids, MI * | Portland, OR         |
| Albany, NY *     | Hartford, CT *     | Providence, RI *     |
| Atlanta, GA      | Houston, TX        | Reading, PA          |
| Baltimore, MD *  | Indianapolis, IN   | Richmond, VA *       |
| Birmingham, AL * | Jacksonville, FL   | Rochester, NY *      |
| Boston, MA       | Jersey City, NJ    | Sacramento, CA       |
| Bridgeport, CT * | Kansas City, KS    | Salt Lake City, UT * |
| Buffalo, NY      | Kansas City, MO    | San Antonio, TX *    |
| Cambridge, MA *  | Knoxville, TN *    | San Diego, CA        |
| Canton, OH *     | Los Angeles, CA    | San Francisco, CA    |
| Charlotte, NC    | Louisville, KY     | Scranton, PA *       |
| Chicago, IL      | Lowell, MA *       | Seattle, WA          |
| Cincinnati, OH * | Lynn, MA           | South Bend, IN       |
| Cleveland, OH *  | Memphis, TN        | Springfield, MA *    |
| Columbus, OH *   | Miami, FL          | St. Louis, MO        |
| Dallas, TX       | Milwaukee, WI      | St. Paul, MN         |
| Dayton, OH *     | Minneapolis, MN    | Syracuse, NY         |
| Denver, CO       | Nashville, TN *    | Tacoma, WA           |
| Des Moines, IA   | New Bedford, MA    | Toledo, OH           |
| Detroit, MI *    | New Haven, CT      | Trenton, NJ          |
| Duluth, MN       | New Orleans, LA *  | Tulsa, OK            |
| El Paso, TX      | New York, NY       | Utica, NY *          |
| Erie, PA *       | Newark, NJ         | Wichita, KS *        |
| Evansville, IN   | Norfolk, VA        | Wilmington, DE *     |
| Fall River, MA * | Oakland, CA *      | Worcester, MA *      |
| Flint, MI *      | Omaha, NE *        | Yonkers, NY          |
| Fort Wayne, IN   | Philadelphia, PA * | Youngstown, OH *     |
| Fort Worth, TX * | Pittsburgh, PA     |                      |

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\* Indicates cities which are included in the balanced panel of the dataset.

## Appendix Table 2

### Summary Statistics of Variables Used in the Analysis

| Variables   | Obs | Mean    | Std. Dev. |
|---|-----|---------|-----------|
| <i>Dependent variables</i>  |     |         |           |
| Log per capita total property crimes  | 798 | -4.381  | 0.491     |
| Log per capita larcenies  | 798 | -5.108  | 0.796     |
| Log per capita burglaries   | 798 | -5.833  | 0.578     |
| Log per capita robberies  | 798 | -7.761  | 0.920     |
| Log per capita auto thefts  | 798 | -6.121  | 0.572     |
| Log per capita murders  | 796 | -10.491 | 1.732     |
| Log per capita aggravated assaults  | 797 | -8.366  | 1.278     |
| Log per capita rapes  | 480 | -10.469 | 1.485     |
| <i>Endogenous variable</i>  |     |         |           |
| Log per capital relief spending (1967 \$)   | 798 | 3.289   | 1.095     |
| <i>Instruments</i>  |     |         |           |
| Log of per capita relief spending nationally (outside of state of interest) x Per capita nonfederal land in state | 798 | 0.059   | 0.061     |
| Months of severe or extreme precipitation in city i during year t   | 798 | 0.355   | 1.044     |
| <i>Exogenous variables</i>  |     |         |           |
| Log per capita police spending  | 798 | 1.814   | 0.545     |
| Employment index  | 798 | 90.291  | 11.580    |
| Log per capita retail sales   | 798 | -0.172  | 0.242     |
| Percent foreign born  | 798 | 12.223  | 7.822     |
| Percent black   | 798 | 7.647   | 9.650     |
| Percent illiterate  | 798 | 3.641   | 2.039     |
| Percent between ages 10 and 34  | 798 | 43.130  | 2.119     |
| Log of city population  | 798 | 12.596  | 0.806     |
| General fertility rate  | 785 | 63.071  | 9.726     |
| Infant mortality rate   | 785 | 51.640  | 14.685    |
| Average monthly precipitation   | 798 | 3.167   | 1.044     |
| Average monthly temperature   | 798 | 52.765  | 7.128     |

**Figure 1**

**Difference-in-Difference Plot of Change in Crime and Relief Between 1930-1932 and 1933-1940**

