

“Striking the Roots of Crime: The Impact of the New Deal on Criminal Activity”

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[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, 1939¹

I. Introduction

During the 1930s, as the economy sunk into the depths of 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 sought desperate means to provide for their families' subsistence. The Roosevelt administration, seeking to right the economic ship and aid those in distress, embarked on a series of grand experiments in 1933 that dramatically changed the interaction between the government and the economy, as well as the relationships between the federal and state and local governments. 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 that was provided during the New Deal went well beyond the scale seen before the 1930s and large amounts continued to be spent throughout the decade, even though the total number of unemployed (including relief workers) declined from the peak unemployment in 1933.

As the economy slid toward the trough of the Depression in the early 1930s, the burden of providing relief rested on state and local governments and private charitable organizations. The federal government played a limited role in the early part of the Depression by providing loans through the Reconstruction Finance Corporation so that state and local governments could fund relief in the latter part

of 1932. In 114 cities for which detailed time-series data are available, per capita relief spending rose from \$3.90 in 1930 to \$18.70 in 1932 (constant 1967 dollars). It was Roosevelt's New Deal, however, that revolutionized welfare spending both in the short term and the long term. The federal government poured resources (sometimes in partnership with state and local governments) into emergency work relief projects and direct relief during the First New Deal (1933-1935). After 1935 through the beginning of World War II, the federal government continued providing emergency work relief, while local governments took over the responsibility for the "unemployables." Under the Social Security Act enacted in 1935, the Roosevelt administration established a federal/state/local partnership to provide public assistance to dependent children, the blind, and the aged that replaced the much smaller state and local programs that had existed prior to the New Deal. Federal involvement in relief efforts led to dramatic increases in spending, as per capita relief expenditures increased by about 160 percent between 1932 and 1933 and by 1940 they were almost three times the 1932 level.

At the most basic level New Deal relief programs accomplished the fundamental objective of providing critically needed 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.² Using a new city-level panel data set that we have assembled, this paper examines Roosevelt's belief, expressed in the opening quotation above, that the New Deal "struck at the very roots of crime." The recent work of Raphael and Winter-Ebmer (2001) and Gould, Weinberg, and Mustard (2002) suggests that greater employment opportunities tend to mitigate criminal behavior in the modern era, so 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? The cross-sectional variation in the Depression's economic shocks across the United States and in the government's response provide a unique opportunity to empirically examine the effectiveness of social welfare programs in times of crisis.

The paper not only contributes to our understanding of the New Deal from an historical perspective, but 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 “Some persons 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 of their impact on crime is virtually absent from the economics literature.³

One worry in identifying the true causal effect of New Deal spending on crime is that the distribution of federal relief funds across time and cities was not exogenous, it was driven to some degree by economic distress in each city at each point in time. Since unemployment and crime are likely positively related and likely helped determine the distribution of relief, simple ordinary least squares (OLS) estimates of the impact of relief spending on crime might be biased upward.⁴ Omitted variables may bias our results as well. For example, if relief spending “crowded out” city expenditures on activities that may have deterred crime, such as police, prosecution, or prisons, the crime rate would have been higher. Because we cannot control for all variables that may have been directly linked to crime and indirectly to relief, we adopt an instrumental variables (IV) approach. Given that New Deal spending across states was driven in part by measures of state size and federal land ownership, which were unlikely to be related to crime within a specific city, we are able to use these variables to create instruments in the IV estimation. Our results suggest that New Deal relief spending had a statistically significant impact in reducing crime rates in American cities. The IV estimates suggest that property crime was up to 30 percent lower than it otherwise would have been in the absence of relief spending during the Great Depression. Further, the IV estimates indicate that there was substantial bias in the OLS estimates that caused them to understate the importance of relief spending in reducing criminal activity.

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 might 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' were covered under workers' compensation laws established during the 1910s. An increasing number of states during the early 1930s instituted old-age pensions 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 (see Skocpol 1992, ch. 2; Clark, Craig, and Wilson 2003).

As the unemployment rate rose and income and tax revenues dropped between 1929 and 1933, state and local social welfare resources were quickly overwhelmed. During the fall of 1932 the Hoover administration provided 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. In fact, by 1935 the federal government's share of total relief spending had risen to 79 percent from 2 percent in 1932 (see Table 1). Not only did the federal government virtually take over the provision of relief, but the amount of spending jumped markedly. As reported in Table 1, 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. Table 1 also reports the relative generosity of the relief benefits. For the typical family on relief, benefits

never exceeded 42 percent of annual manufacturing wages. So while relief was extremely generous in historical perspective, a private sector manufacturing job certainly would have been more lucrative.

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 to some extent the political situation.⁵ State governments then distributed the funds internally to local governments. FERA offered both direct relief and work relief.⁶ 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. That is, local officials determined the deficit between the family's total income and a hypothetical budget for a family of that size. This budget-deficit served as the basis for 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. At its peak the CWA employed four million workers for a short period of time.⁷

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). It was the federal WPA then that 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 1999, Fishback, Kantor, and Wallis 2003).

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, 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 unique 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. One of the potential shortcomings of the UCR data is that the number of criminal offenses that were reported to the FBI were only the ones about which the local police knew. Nonetheless, our use of city and year fixed effects should control for any systematic reporting disparities across cities or across years, especially during the nationwide introduction of the UCR system. Moreover, the number of cities reporting to the UCR system was relatively small during its first decade, restricting our sample to 83.

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 the need to “steal in order to keep from starving.”⁸

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 crime. Put another way, a higher income or higher wages would have increased the opportunity cost of committing crime.⁹ Work

relief especially had the potential to effectively divert individuals' time and interest away from crime towards work.

Without controlling for other covariates, a simple difference-in-difference scatter plot suggests that cities with relatively large increases in relief saw their crime rates decrease more. The scatter plot shown in Figure 1 was created using the change in the average annual per capita property crime between 1930-1932 and 1933-1940 and the average annual per capita relief spending for the same period. 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 upward.

The regression model that we estimate is given by the following 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 national crime trends and G is a vector of city fixed effects that control for unobservable factors that did not vary over time, but varied across the cities, that may have led to different base levels of property crimes in the sample 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 general economic activity at the local level. We also include the percentages of the population that were foreign born, black, illiterate and between the ages of 10 to 34.¹⁰ We also control for per capita police spending within each city. Finally, ε_{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 unobservable must vary both across cities and across time. A prime candidate would be the extent of poverty and income inequality in an area. Measures of poverty that varied across time at the city level are generally unavailable for this period and the Depression's

shocks likely varied across different segments of the income distribution. 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.

To address this potential bias, we adopt an instrumental variables (IV) approach. To find effective instruments for New Deal spending we draw on Robert Fleck's (2001) analysis of the distribution of New Deal expenditures across states. He found that nonfederal land per capita, federal land per capita, income in 1932, and the fall in income between 1929 and 1932 explained 93 percent of the cross-state variation in New Deal spending. One of the most important variables that determined federal expenditures, in Fleck's view, was the physical size of the state because such a variable may have influenced "the way senate seats and/or electoral votes were apportioned" (p. 303). Relatively large states, especially with disproportionately smaller populations, would have wielded extra political influence because of the nature of Senate and Electoral College representation. A logical conclusion from Fleck's insights would be that major cities located within relatively larger states may have been able to secure more relief spending than comparable cities in relatively smaller states.

To develop a set of effective instruments for relief spending in the cities, we need to find variables that are 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 New Deal spending. We have created instruments that reflect the interaction between the national amount of per capita relief spending in each year and the extent of federal and nonfederal land ownership in the specific states in which the cities were located. Both instruments (per capita national relief interacted with federal land area per capita within the city's state and relief interacted with nonfederal land area per capita) were unlikely to have been determined by the relief spending or crime levels within any one specific city. The sizes of the states were established long before the 1930s and the specific economic or crime situation in any one city was unlikely to have been a significant force behind changes in national relief spending from year to year.

The inclusion of fixed effects in the model precludes the use of national relief spending or land area as stand-alone variables, so interactions are necessary. 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. Finally, while we have no reason to believe, a priori, that our instrumental variables would be correlated with crime or the omitted variables influencing both crime and relief, we used a Hausman (1983) specification test to examine this idea. The results of the test suggest that the identifying instruments themselves 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 greater in absolute value than the reported effects. Nonetheless, the estimated coefficient is statistically significant and suggests that a 10 percent increase in relief spending would have corresponded with decreased per capita property crime of about 0.7 percent. When we add the other control variables, none seems to have a statistically significant impact on crime, and the coefficient of the relief variable is relatively unchanged. The foreign-born, black, illiterate, and ages 10 to 34 variables are all linear interpolations of the values in the years between the decennial census years. When fixed effects are included in the analysis, the coefficients for these variables are capturing the impact of the city-specific time trends for these variables. Finally, per capita police spending, an endogenous variable itself, has a negative but statistically insignificant effect on crime. The inclusion or exclusion of the police variable has no impact on the relief coefficient. Finally, because the panel is unbalanced, we worried that the inclusion or exclusion of certain city-year combinations may have affected the result. When we reestimated the equation with a balanced panel of cities for which complete data were available, the sample size dropped from 798 to 407. There was little resulting change to the main OLS results previously reported, as shown in the last column of Table 2. The coefficient of the state employment

index becomes statistically significant with the balanced panel. A positive coefficient would be suggestive of a “looting effect.” That is, the better the economy, the more there was to steal, which could have caused overall property crime to increase.

Table 3 reports the OLS results for each individual crime and shows that relief spending significantly decreased burglary, robbery, and auto theft. The effect on larceny was not statistically different from zero. The estimated effect of relief spending on burglary, robbery, and auto theft separately was bigger in absolute value than that for the aggregated property crimes variable. The estimated effects of relief on each of the violent crimes (other than robbery which the FBI classifies as a violent property crime) tend to be positive and are not statistically different from zero. At least from this cursory OLS perspective, the income-enhancing aspects of the New Deal relief programs seem to have had an impact on people’s incentives to commit crimes for subsistence purposes.

The OLS results are certainly suggestive 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 the first- and second-stage IV results for aggregate property crime. The estimated coefficients of the instruments are individually highly significant in the first-stage equation and an F-test suggests that they are jointly different from zero at the 1 percent confidence level. The relatively large F statistic offers 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, a Hausman specification test indicates that the instruments are not significantly correlated with the estimated error of the second-stage crime regression and, thus, have not been inappropriately omitted from the crime equation.

The first-stage coefficients of the instruments provide some support for Fleck’s view that relatively large states wielded disproportionate influence in the distribution of New Deal relief. The nonfederal land interaction suggests that when more national relief spending was available, cities within the relatively large states tended to attract relatively more relief dollars. Such largesse to cities, however,

did not spillover to those states in which the federal government owned relatively more of the state's land. The federal government may have allocated more money for public land improvements to those states in which it owned relatively more of the state, as Fleck found. However, most of this land was located in less densely settled rural areas. Thus, it would not be surprising that in areas with more federal land the within state distribution of funds favored the rural areas over the major cities, such that a higher amount of federal land was associated with less funding within the cities.

The IV estimate of the effect of per capita relief spending on total property crime is substantially greater in absolute value than the OLS estimate. The estimated effect increases up to three times, from -0.07 in the OLS estimation to -0.24 in the IV estimation with the unbalanced panel. The results provide a strong indication that the OLS estimate was biased upward, as we had predicted. New Deal administrators probably distributed more relief funds to areas that had greater crime problems or relief money at the local level competed with other forms of social spending that may have mitigated the crime problem. The point estimates suggest that a 10 percent increase in relief spending would have decreased per capita property crime by a statistically significant 0.15 to 0.24 percent. To put the significant economic effect of relief spending into perspective, consider the difference in relief spending between 1932 and 1938, the year with the greatest New Deal relief spending. Evaluating this increase in per capita relief spending of \$50.60 (1967 dollars) using the IV estimates of -0.15 to -0.24 suggests that property crime was about 24 to 39 percent lower than it otherwise would have been without the increased relief expenditures. When we estimate the equation with a balanced panel of data, the IV estimate for the relief spending effect ranges from -0.20 when no other control variables are used (except for city and year fixed effects) to -0.43 when all of the other variables are included.

As was the case with the OLS estimations, the control variables tend to have very little explanatory power. Retail sales, however, is the one variable in the unbalanced panel regression that is marginally statistically significant. The positive coefficient is again suggestive of the "looting effect" that positive economic growth may play in stimulating property crime.

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.¹¹ 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.

Similar to the OLS results, the estimated IV effects for the individual crimes indicate that relief spending had relatively greater impacts on burglary, robbery, and auto theft (see Table 5). The estimated elasticities for burglary, robbery, and auto theft were quite large, ranging from about -0.36 to -0.63. The coefficients for larceny, murder, aggravated assault, and rape were not statistically different from zero. Unfortunately, for the case of burglary, robbery, and aggravated assault, the Hausman test suggests that the instruments were inappropriately excluded from the second-stage crime regression.

IV. Conclusion

The economic downturn associated with the Great Depression pushed millions of American workers and their families into a personal economic crisis. With legal employment opportunities significantly limited by the Depression, families facing desperate circumstances 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 such income and employment opportunities was the diminished need to rely on criminal behavior. Our empirical analysis suggests that New Deal relief did indeed lower property crime in a statistically and economically significant way. This finding is consistent with a general economic model of crime where changes in work and income opportunities change the benefits and costs of criminal activity, thus altering individuals' legal/illegal labor supply decisions. In addition, the strong effect that relief spending had on

crime reduction corroborates President Roosevelt's observation that the New Deal "struck at the very roots of crime itself." More generally, our results indicate that social insurance, which tends to be ignored in economic analyses of crime, should be included as an additional factor in understanding temporal and spatial variations in criminal activity. For those individuals who are more likely to commit crime because of depressed economic opportunities, social insurance can tilt the balance in favor of lawful behavior.

FOOTNOTES

¹ 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>.

² For recent empirical work examining the effects of county-level New Deal expenditures on infant mortality and retail consumption, respectively, see Fishback, Haines and Kantor (2001) and Fishback, Horrace, and Kantor (forthcoming). Wallis and Benjamin (1984), Margo (1994), and Fleck (1999) 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.

³ 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 plays no role in understanding 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. Lochner and Moretti (2004) explore the link between educational attainment and crime. They find rather significant social returns to programs that contribute to high school completion. 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.

⁴ When identifying the effect of unemployment on crime Raphael and Winter-Ebmer (2001) point out that there are a number of omitted variables that vary with the business cycle that might affect crime, yet are not included in most crime studies.

⁵ For an empirical analysis of the distribution of FERA grants, see Fleck (1999c) and Fishback, Kantor and Wallis (2003). For discussions of the administrative details of relief provision, see Brown (1940) and U.S. National Resources Planning Board (1942).

⁶ 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. 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. But 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.

⁷ For discussions of the FERA and CWA policies, see Brown (1940, 218-98) and U.S. National Resources Planning Board (1942, 26-97).

⁸ 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.

⁹ 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.

¹⁰ 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.

¹¹ We thank Koleman Strumpf for providing evidence on the decisions made by individual counties to remain wet or dry after Prohibition.

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

Year	Per capita relief spending (1967 \$)	Average annual relief benefits as a percentage of annual manufacturing earnings	Federal share of relief spending
1930	\$3.74		
1931	9.06		
1932	18.06		2.1%
1933	29.71	21.7%	51.8
1934	47.93	31.2	78.9
1935	51.03	33.3	78.9
1936	61.78	42.4	74.7
1937	52.24	37.6	72.1
1938	69.61	39.3	62.0
1939	63.32	38.3	62.5
1940	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 are from ICPSR, file number 0003. 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).

Table 2

OLS Estimates of Aggregate Property Crime, 1930-1940

Variables	Unbalanced Panel				Balanced Panel ^a	
Log of relief spending per capita	-0.0713*	-0.0757*	-0.0779*	-0.0777*	-0.0758*	-0.0794*
	(0.022)	(0.022)	(0.023)	(0.023)	(0.028)	(0.030)
State employment index		0.0016	0.0013	0.0014		0.0044**
		(0.002)	(0.002)	(0.002)		(0.002)
Log of retail sales per capita		0.1853	0.1769	0.1860		0.1363
		(0.139)	(0.137)	(0.139)		(0.232)
Log of police spending per capita				-0.0722		-0.1133
				(0.082)		(0.100)
Percent foreign born			0.0091	0.0007		-0.0123
			(0.007)	(0.007)		(0.019)
Percent black			-0.0220	-0.0207		-0.0443
			(0.037)	(0.037)		(0.062)
Percent illiterate			-0.0075	-0.0064		0.0058
			(0.029)	(0.029)		(0.037)
Percent between ages 10 and 34			-0.0005	-0.0015		-0.0377
			(0.023)	(0.023)		(0.038)
Year fixed effects	included	included	included	included	included	included
City fixed effects	included	included	included	included	included	included
N	798	798	798	798	407	407
Adjusted R ²	0.864	0.864	0.864	0.864	0.821	0.823

* Statistically significant at better than 1 percent confidence level.

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

^a A listing of the cities in the balanced panel is reported in Appendix Table 1.

Notes: The dependent variable is the natural log of total property crimes per capita. Property crimes include burglary, larceny, robbery, and automobile theft. Huber-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 ICPSR file number 0003, as amended and corrected by Michael Haines. 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 the amended ICPSR file 0003. 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.

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

Variables	larceny	burglary	robbery	auto theft	murder	aggravated assault	rape
Log of relief spending per capita	0.0298 (0.062)	-0.0871* (0.033)	-0.1016** (0.048)	-0.1489* (0.034)	0.0665 (0.123)	0.1364 (0.092)	0.0223 (0.159)
State employment index	0.0021 (0.003)	-0.0004 (0.002)	0.0053 (0.003)	-0.0016 (0.003)	0.0145 (0.009)	0.0153* (0.005)	-0.0003 (0.015)
Log of retail sales per capita	0.0353 (0.206)	0.1816 (0.199)	0.3047 (0.247)	0.4783** (0.233)	-0.3092 (0.580)	-0.4849 (0.333)	0.5920 (0.966)
Log of police spending per capita	0.1361 (0.118)	-0.1681 (0.117)	-0.1958 (0.166)	-0.3336** (0.143)	-1.1623*** (0.607)	0.2218 (0.241)	0.4053 (0.628)
Percent foreign born	-0.0294 (0.027)	-0.0093 (0.011)	-0.0020 (0.014)	-0.0071 (0.011)	-0.0280 (0.034)	0.0119 (0.026)	0.0078 (0.073)
Percent black	-0.0058 (0.055)	0.0279 (0.047)	0.1501 (0.064)	-0.1394** (0.055)	0.1060 (0.110)	0.0353 (0.101)	-0.0156 (0.368)
Percent illiterate	-0.1400* (0.059)	-0.0504 (0.055)	-0.0186 (0.071)	0.1758* (0.054)	-0.2662 (0.251)	0.0906 (0.099)	-1.1345* (0.437)
Percent between ages 10 and 34	-0.0673 (0.050)	-0.0048 (0.042)	0.1180 (0.049)	0.0841** (0.034)	-0.0379 (0.113)	0.0131 (0.064)	0.1214 (0.226)
Year fixed effects	included	included	included	included	included	included	included
City fixed effects	included	included	included	included	included	included	included
N	798	798	798	798	796	797	480
Adjusted R ²	0.615	0.751	0.839	0.757	0.527	0.770	0.448

* 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. Huber-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 ^a					
	First Stage	Second stage	First Stage	Second stage	First Stage	Second stage	First Stage	Second stage	Second stage	Second stage		
Log of relief spending per capita		-0.1515*** (0.0808)		-0.1636** (0.083)		-0.2298*** (0.122)		-0.2368** (0.120)		-0.1969** (0.1000)		-0.4255** (0.216)
<i>Instruments:</i>												
Per capita federal land in state x log per capita relief spending nationally	-7.987* (1.271)		-8.136* (1.286)		-7.307* (1.337)		-7.670* (1.336)					
Per capita nonfederal land in state x log per capita relief spending nationally	12.47* (1.682)		12.529* (1.717)		11.340* (1.881)		11.71* (1.874)					
<i>Exogenous variables:</i>												
State employment index			-0.0007 (0.003)	0.0021 (0.002)	-0.0023 (0.0033)	0.0007 (0.002)	-0.0027 (0.003)	0.0008 (0.002)				-0.0002 (0.004)
Log of retail sales per capita			0.4208*** (0.225)	0.2063 (0.136)	0.4300*** (0.229)	0.2312*** (0.137)	0.4011*** (0.228)	0.2419*** (0.138)				0.3533 (0.268)
Log of police spending per capita							0.2607 (0.162)	-0.0647 (0.089)				-0.2124 (0.140)
Percent foreign born					0.0332 (0.023)	0.0154 (0.014)	0.3178 (0.023)	0.0159 (0.014)				0.0820 (0.063)
Percent black					-0.0481 (0.083)	-0.0412 (0.043)	-0.0501 (0.083)	-0.0410 (0.043)				-0.1049 (0.083)
Percent illiterate					0.0946 (0.072)	0.0056 (0.035)	0.0907 (0.071)	0.0071 (0.035)				0.0947 (0.080)
Percent between ages 10 and 34					0.0029 (0.0358)	-0.0131 (0.024)	0.0093 (0.036)	-0.0147 (0.024)				-0.0927 (0.053)
Year fixed effects	included	included	included	included	included	included	included	included	included	included	included	included
City fixed effects	included	included	included	included	included	included	included	included	included	included	included	included
N	798	798	798	798	798	798	798	798	798	407	407	
F-test of instruments	28.05		27.16		18.67		20.23		13.96		7.06	
Hausman test-statistic	2.444		2.472		2.777		2.597		1.035		0.939	

^a A listing of the cities in the balanced panel is reported in Appendix Table 1.

* 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	larceny	burglary	robbery	auto theft	murder	aggravated assault	rape
Log of relief spending per capita	0.2907 (0.392)	-0.3590** (0.164)	-0.6326** (0.187)	-0.5641* (0.105)	-0.2760 (0.292)	-0.0295 (0.233)	-0.5707 (0.424)
State employment index	0.0032 (0.003)	-0.0014 (0.003)	0.0032 (0.004)	-0.0033 (0.003)	0.0131 (0.009)	0.0147* (0.005)	-0.0051 (0.015)
Log of retail sales per capita	-0.0564 (0.254)	0.2772 (0.200)	0.4913*** (0.272)	0.6242** (0.247)	-0.1858 (0.574)	-0.4260 (0.332)	0.9792 (1.029)
Log of police spending per capita	0.1238 (0.129)	-0.1553 (0.124)	-0.1707 (0.188)	-0.3140*** (0.171)	-1.145*** (0.607)	0.2299 (0.242)	0.3882 (0.619)
Percent foreign born	-0.0542 (0.058)	0.0166 (0.021)	-0.0486 (0.024)	0.0324*** (0.017)	0.0046 (0.045)	0.0277 (0.036)	0.1206 (0.118)
Percent black	0.0274 (0.060)	-0.0066 (0.053)	0.0826 (0.083)	-0.1921* (0.071)	0.0642 (0.118)	0.0140 (0.106)	-0.0577 (0.383)
Percent illiterate	-0.1622** (0.082)	-0.0272 (0.062)	0.0266 (0.073)	0.2111* (0.059)	-0.2382 (0.249)	0.1048 (0.101)	-1.032** (0.434)
Percent between ages 10 and 34	-0.0458 (0.038)	-0.0272 (0.043)	0.0741 (0.049)	0.0498 (0.039)	-0.0654 (0.117)	-0.0006 (0.066)	0.0704 (0.225)
Year fixed effects	included	included	included	included	included	included	included
City fixed effects	included	included	included	included	included	included	included
N	798	798	798	798	796	797	480
F-test of instruments							
Hausman test-statistic	0.348	10.9	4.24	1.52	0.945	4.12	1.50

* 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. Huber-White corrected standard errors are in parentheses.

Sources: See Table 2.

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

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	

* 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.
Log per capital relief spending (1967 \$)	798	3.289	1.095
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
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
Per capita federal land in state x log per capita relief spending nationally	798	0.013	0.047
Per capita nonfederal land in state x log per capita relief spending nationally	798	0.059	0.061

Figure 1

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

