DISCUSSION PAPER SERIES

IZA

IZA DP No. 8402

Less Cash, Less Crime: Evidence from the Electronic Benefit Transfer Program

Richard Wright
Erdal Tekin
Volkan Topalli
Chandler McClellan
Timothy Dickinson
Richard Rosenfeld

August 2014

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Less Cash, Less Crime: Evidence from the Electronic Benefit Transfer Program

Richard Wright
Georgia State University

Chandler McClellan
CBHSQ, SAMHSA

Erdal Tekin
American University, IZA and NBER

Timothy Dickinson
University of Missouri-St. Louis

Volkan Topalli
Georgia State University

Richard Rosenfeld
University of Missouri-St. Louis

Discussion Paper No. 8402
August 2014

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 8402
August 2014

ABSTRACT

Less Cash, Less Crime: Evidence from the Electronic Benefit Transfer Program *

It has been long recognized that cash plays a critical role in fueling street crime due to its liquidity and transactional anonymity. In poor neighborhoods where street offenses are concentrated, a significant source of circulating cash stems from public assistance or welfare payments. In the 1990s, the Federal government mandated individual states to convert the delivery of their welfare program benefits from paper checks to an Electronic Benefit Transfer (EBT) system, whereby recipients received and expended their funds through debit cards. In this paper, we investigate whether the reduction in the circulation of cash on the streets associated with EBT implementation had an effect on crime. To address this question, we exploit the variation in the timing of the EBT implementation across Missouri counties. Our results indicate that the EBT program had a negative and significant effect on the overall crime rate as well as burglary, assault, and larceny. According to our point estimates, the overall crime rate decreased by 9.8 percent in response to the EBT program. We also find a negative effect on arrests, especially those associated with non-drug offenses. Interestingly, the significant drop in crime in the United States over several decades has coincided with a period of steady decline in the proportion of financial transactions involving cash. In that sense, our findings serve as a fresh contribution to the important debate surrounding the factors underpinning the great American crime decline.

JEL Classification: H53, I38, J22, K42

Keywords: crime, cash, EBT, welfare, economy

Corresponding author:

Erdal Tekin
School of Public Affairs
American University
4400 Massachusetts Avenue NW
Washington, DC 20016-8070
USA
E-mail: tekin@american.edu

* We would like to thank the seminar participants at the Institute for the Study of Labor (IZA) and the European Society for Population Economics for useful comments and suggestions. This paper does not represent the policy of either the Substance Abuse and Mental Health Services Administration (SAMHSA) or the US Department of Health and Human Services (DHHS). The views expressed herein are those of the authors and no official endorsement by SAMHSA or DHHS is intended or should be inferred.

I. Introduction

The crime decline in the United States has been well documented (Levitt, 2004; Blumstein and Wallman, 2006). Crime increased sharply in the mid-1980s and continued to climb until the early 1990s, after which it dropped to levels not seen since the 1960s. A wide variety of explanations for the crime drop have been offered, including increased federal funding for community policing and better policing strategies (Corman and Mocan, 2001; Zhao, Schieder, and Thurman, 2002), changing demographics (Levitt, 1999), improving economic conditions (Raphael and Winter-Ebmer, 2001; Rosenfeld and Fornango, 2007), a potential mitigating effect of immigration (Wadsworth, 2010), the advent of new security technologies (Farrell et al, 2011), and unprecedented levels of imprisonment (Levitt, 1996; Donohue and Siegelman, 1998; Liedka, Piehl, and Useem, 2006). Although there is little consensus regarding the overall importance of any single factor, it is generally agreed that a significant fraction of the decline has yet to be identified empirically, with some arguing that it likely is the result of a complex interaction of multiple factors (see Blumstein and Wallman, 2006; Zimring, 2008).

Income-generating offenses such as larceny, burglary and robbery have fallen along with other forms of street crime since the 1990s (Federal Bureau of Investigation, 2013). Much of this offending is focused on the acquisition of cash – as opposed to alternative forms of monetary transfer such as debit or credit cards – because its liquidity and transactional anonymity are critical to the functioning of the underground economy (Varjavand, 2011). Criminologists have long known that most predatory street crime is motivated by a perceived need for cash, and that much of that cash is spent on hedonistic activities, especially illicit drug use (see Wright and Decker, 1994, 1997; Shover, 1996).

Economists also have emphasized the role that cash plays in fueling street crime, recognizing that neighborhood drug dealers, prostitutes, and pawn brokers are not inclined to accept other forms of payment for their services (see, e.g., Foley, 2011; Armey, Lipow, and Web, 2012). Although no quantitative studies have explored this relationship in detail, a series of field-based qualitative projects has carefully specified the mechanisms through which a desperate need for cash could motivate street crime (e.g., Wright, Topalli, and Jacques, 2013; Wright and Topalli, 2011; Topalli, Wright, and Fornango, 2002; Wright and Decker, 1994). Using a grounded theory approach developed through interviews with and observations of active street criminals, these studies have introduced an etiological model that explains both the importance of cash to offenders and its role in perpetuating their criminal activity. That model outlines the way in which background risk factors such as being born into a life of pervasive poverty can loosen individuals' bonds to conventional society and thereby lead some of them to participate in the oppositional culture of the streets. "Streetlife" is characterized by conspicuous consumption, fatalism, and a general disdain for mainstream values (see Anderson, 1999; Brezina, Tekin, and Topalli, 2009; Shover, 1996), which find their expression in the pursuit of illicit action, especially, but not exclusively, during intense periods of heavy drug and alcohol use (Wright and Decker, 1994; 1997). The pursuit of illicit action, in turn, quickly exhausts offenders' financial resources, leading to the commission of crime in order to acquire more cash to keep the party going (see Figure 1).

Figure 1 about here

The proposition that flows logically from this model is that cash is a necessary functional component of the etiological cycle that drives many sorts of predatory street crime. If that is so, then any reduction in the amount of cash in circulation should produce concomitant reductions in acquisitive street crimes (e.g., theft) and the secondary offenses committed in response to them (e.g., retaliatory assault).

There is significant evidence that the United States economy is in fact moving away from cash as a transactional medium. The proportion of financial transactions utilizing cash has steadily decreased due to the increased use of credit cards, which entered the United States market in the 1950s (followed two decades later by the advent of ATM and debit cards), as well as the more recent increase in mobile transactions (see Erling, 2013). Furthermore, over three-quarters of all non-cash payments in the United States in 2009 were made electronically and this represents a 9 percent increase from 2006 (Federal Reserve System, 2011).[1] In fact, cash transactions in the United States have been on the decline for a much longer period of time. Fifty years ago, cash was used in 80 percent of domestic payments. Today, that number is closer to 50 percent. Cash transactions should continue to decline as dependence on online banking and commerce increase. According to Littman and Oliver (2012), since 1990 debit transactions have increased by 2,700 percent while cash volume has grown at an annual rate of only 4 percent. Checks – once the primary method of benefits transfer to the poor – have declined in use by more than 50 percent. Importantly, the movement away from cash is not limited to the United States, as other countries, notably Sweden (Tomlinson, 2012) and Israel (Shamah, 2014), are purposefully pursuing ways to severely limit the use of cash. The question remains, however: has this shift in the way commerce is transacted had any effect on street crime?

[1] While electronic payments now exceed three quarters of all noncash payments, payments by check are now less than one-quarter (Federal Reserve System, 2011).

In poor neighborhoods where street offenses are concentrated, a significant source of circulating cash stems from public assistance or “welfare” payments. Prior to the late 1990s, the welfare assistance program, now referred to as Temporary Assistance for Needy Families (TANF) issued payments in the form of paper checks, which required a significant proportion of recipients with no access to conventional bank accounts (see Leyshon and Thrift, 1994, 1995) to cash them at independent check cashing establishments. This resulted in a situation whereby large amounts of cash were available to welfare recipients at one point in time each month (Ford and Beveridge, 2004)[2].

One possible explanation for how check-based welfare payments precipitate street crime is that this encourages recipients to expend their resources prematurely, leading them to turn to crime to supplement their income for the remainder of the month (Foley, 2011). This assumes, however, that a sizeable proportion of welfare recipients will turn to crime, which is unlikely (see Zhang, 1997; Hannon and DeFronzo, 1998; Fishback, Johnson, and Kantor, 2010). A more plausible explanation is that welfare recipients who have just cashed their checks represent especially attractive targets for predatory offenders desperate for cash to sustain their pursuit of illicit action (Wright and Decker, 1997).

A significant shift in welfare payment schemes has been introduced across the United States over the last two decades, with paper checks being replaced by the Electronic Benefit Transfer (EBT) program, a digital, debit card-based system. Mandated

[2] In addition, food assistance benefits, now referred to as Supplemental Nutrition Assistance Program (SNAP) benefits, were previously distributed as “food stamps”. Prior to their inclusion in the EBT system, these benefits were relatively fungible in that they could be exchanged between recipients illegally or traded in at vendors for cash. A key reason for establishing EBT was to prevent such illegal trafficking (see USDA, 2003), thus removing an additional yet indirect source of cash from the streets.

by the Federal government, but enacted at the state level, the changeover from paper to EBT was implemented variably within states, most often on a county-by-county basis.

In this paper, we hypothesize that the introduction of such a system will reduce the amount of cash circulated on the streets and thus disrupt the etiological cycle of criminality described above, resulting in a reduction in rates of both predatory and retaliatory street crimes. This paper provides the first empirical examination of this hypothesis. To do this, we assemble monthly data on various types of crimes from all of the counties in the state of Missouri between 1990 and 2011. Then we exploit the variation in the timing of EBT program implementation across counties over time to examine the impact on various crimes of reduced circulation of cash caused by the EBT program.[3] To the extent that there is no other plausible channel though which EBT implementation can cause an independent effect on crime, any association between EBT implementation and crime can then be attributed to removal of cash.

Our results indicate that the EBT program implementation is associated with a significant decrease in the overall crime rate and the specific offenses of burglary, assault, and larceny in Missouri. Our analysis points to suggestive evidence that the EBT program also reduced robbery. Finally, we find a reduction in arrests, those from non-drug offenses in particular, in response to the EBT program implementation. The remainder of the paper is organized as follows. In section II, we provide background information on the history of the EBT program in the United States and the state of Missouri. We

[3] Note that the EBT program was implemented incrementally in phases in many other states as well. Although it would be interesting to conduct a nationwide analysis, data on implementation dates at the county level for all states are not available. We exchanged emails with an official at the Economic Research Service (ERS) at the U.S. Department of Agriculture (USDA), who served as an evaluator of the system-wide EBT implementation in several states, in an attempt to identify EBT implementation dates at the county level for other states. He acknowledged that neither he nor anyone else involved in the implementation had recognized the potential for tracking county implementation dates for the purposes of research. We thank John Kirlin for providing that information.

describe our data in Section III and the estimation approach in Section IV. The results are summarized in Section V and a discussion is provided in Section VI.

II. Background

Beginning in the early 1980s, the federal government gradually shifted toward the use of EBT as a means for disbursing government benefits to recipients. A number of EBT demonstration programs were implemented during the decade, culminating in congressional passage of the Hunger Prevention Act of 1988 and the Mickey Leland Memorial Domestic Hunger Relief Act of 1990 (Food and Nutrition Service, 2013). During the 1990s, political support for EBT increased as a result of an ideological shift reflected in the Conference Report on the Omnibus Budget Reconciliation Act of 1993 and the 1993 National Performance Review, which urged states to establish these systems (see Personal Responsibility and Work Opportunity Reconciliation Act, 1999; Office of the Vice President, 1993). Welfare reform legislation signed in 1996 required every state to develop systems to issue food stamp program benefits electronically by 2002.

Missouri lawmakers responded to these calls by enacting Missouri Revised Statute § 208.182 in 1994 (see Missouri Statutes, 2013), which stipulated the establishment of EBT pilot programs in Missouri counties with a population of 600,000 or more (thus including St. Louis City[4] and Jackson County, which contains Kansas City). These pilot programs were initiated in mid-1997, shortly after the federal government restructured welfare with the Personal Responsibility and Work Opportunity Reconciliation Act. During the testing of EBT, Missouri recipients of food stamps and

[4] The city of St. Louis is an independent city, but was included in the statute mandating the rollout of EBT statewide as follows; "The division of family services shall establish pilot projects in St. Louis City and in any county with a population of six hundred thousand or more."

temporary assistance residing in pilot locations received EBT cards in place of traditional paper checks. Funds were placed on the cards according to recipients' month of birth and the first letter of their surname; a practice continuing to the present day.[5] The switch to EBT was received positively by both retailers and recipients. It increased the speed and efficiency of transactions and the resemblance of the actual EBT cards to credit and debit cards reduced the stigma of making a purchase with distinctive checks (Missouri Department of Social Services, 2013a).

Missouri recipients of EBT are given detailed instructions on the use of their cards, including how and where they may be used, when monthly benefits are disbursed, and the fees and surcharges associated with withdrawing cash from automated teller machines (ATM) and point-of-sale (POS) terminals (Missouri Department of Social Services, 2013a). Although recipients of temporary assistance in Missouri were strongly urged by the Department of Social Services (DSS) to establish bank accounts during the initial years of the restructured program, the majority of individuals continue to receive temporary assistance via EBT cards (Missouri Department of Social Services, 2013b). This is consistent with national trends indicating that a significantly high proportion of the poor are "unbanked" or "underbanked" (FDIC, 2011; see, also, Rhine and Greene, 2012).

Figure 2 illustrates the EBT program implementation map of Missouri. As shown in the figure, the EBT program was implemented in eight phases in different sets of localities between June 1997 and May 1998.[6] The variation in the implementation dates

[5] Personal correspondence with Kay Martellaro, EBT/Food Distribution Unit Manager, Family Support Division, Missouri Department of Social Services.
[6] The numbers of counties in these phases are 8, 17, 19, 12, 7, 2, 6, and 44 in the order of implementation. These refer to the 114 Missouri counties and the city of St. Louis.

of EBT program across counties and over the period of 12 months is key to our identification of the impact of the program on crime.

Figure 2 about here

III. Crime Data

The crime data for Missouri come from the Uniform Crime Reporting (UCR) Program of the Federal Bureau of Investigation (FBI). The UCR represents "a nationwide, cooperative statistical effort of nearly 18,000 city, university and college, county, state, tribal, and federal law enforcement agencies voluntarily reporting data on crimes brought to their attention."[7] Under the UCR system, law enforcement agencies submit crime data either through a state UCR program or directly to the FBI's UCR program on a monthly basis. This consists mainly of crimes reported to the police by the general public, but may also include offenses that police officers discover or learn about through other sources.

For this investigation, we assembled county level monthly data on the following crimes: total crime, burglary, robbery, larceny, and motor vehicle theft. The mean crime rates weighted by county population are presented for our sample variables in Table 1. We present descriptive statistics for each crime rate for the full sample in column 1 and then separately for the observations with and without EBT. The rate of monthly total crime is about 468 per 100,000 persons per county. Larceny constitutes a large share of overall crime followed by assault and burglary. Robbery and motor vehicle theft are the least prevalent crimes in our data, with averages of 13 and 28 per month, and are heavily concentrated in urban areas. When we compare the average crime rates between EBT and non-EBT observations, we see that the rate is lower in observations with EBT program

[7] http://www.fbi.gov/about-us/cjis/ucr.

for all types of crime, which suggests that there may be a reduction in crime associated with EBT implementation.

It is important to acknowledge that monthly crime data from the UCR have been shown to suffer from reporting errors of varying degrees depending on the particular state (Maltz and Targonski, 2002; Marcotte and Markowitz, 2011). For example, some jurisdictions only report crimes to the FBI on an annual basis, while others report meaningful data every month. Inconsistent reporting does not appear to be a problem for Missouri, however, as none of its counties exhibit zero crime for eleven months and with a spike in crime in the month of December (a common occurrence with less diligent states). We illustrate the proportion of each crime reported in each month in Table 2. As shown in the table, the patterns for each crime appear be to be as expected, with peaks in summer months and lowest reporting in February due to fewer days. More importantly, there is no evidence of over-reporting in December.

Table 2 about here

While the preceding discussion is comforting in terms of the reliability of our crime data, it is probable that there is at least some error in reported crime from month to month. This should not, however, constitute a concern for our analysis. First, it is likely that any reporting error from month to month is random. This would reduce the efficiency of our estimates, but would not result in any bias. Second, any permanent differences across jurisdictions that might be responsible for systematic misreporting on the part of any particular jurisdiction or jurisdictions due to reasons such as administrative inefficiency, insufficient funding, or lack of appropriately trained staff should be captured by county fixed effects. Similarly, if misreporting occurs for all counties due to a statewide factor such as budget problems, that should be captured by month-by-year fixed effects. Finally, we also estimate our models controlling for county specific linear time trends, which should account for any county-level time-varying factors causing misreporting that have been trending linearly.

As a focus of our analysis Missouri provides a variety of advantages, chief among them is the fact that its crime rates trend closely to those for the nation as a whole. In Figures 3A and 3B, we illustrate the patterns in total crime rate and rates of Part 1 crimes for the state of Missouri and the nation, respectively.[8] Crime rates fell sharply in both Missouri and the United States during the analysis period. This pattern is present for both the overall crime rate and each of the Part I offenses. In general, the trends in crimes in Missouri appear to be quite similar to those of the United States averages.

Figures 3A and 3B about here

IV. Estimation Method

Our goal is to estimate the change in the rate of crime caused by the implementation of the EBT program. One key empirical challenge to accomplishing this goal stems from the possibility that factors leading people to carry cash may also be correlated with crime. For example, if people decide not to carry too much cash with them as a reaction to increased crime, then any observed negative relationship between the two would be over-stated. Our approach to guarding against this problem is to exploit a policy change that has no direct association with crime itself, but one that leads to a

[8] The UCR indexes two main categories of crime. Part I crimes, our focus in this study, include two categories: Violent crimes include forcible rape, aggravated assault, murder, and robbery. Property crimes include arson, burglary, larceny-theft, and motor vehicle theft. Part II crimes are less serious and involve such offenses as loitering, embezzlement, forgery and counterfeiting, disorderly conduct, prostitution, vandalism, vagrancy, and weapons offenses.

reduction in the circulation of cash on streets. Then, any reduction in crime associated with implementation of the policy can be attributed to the reduction in the circulation of cash. The validity of this approach hinges on whether any of the factors driving the policy change are correlated with street crime. There is no evidence to suggest that crime reduction was ever mentioned as a reason or justification for implementing the EBT program in Missouri or elsewhere. Rather, EBT policy was instituted to reduce program fraud, ensure ease of use of food benefits by program participants, and reduce the stigma associated with using food stamps (e.g., U.S. General Accounting Office, 1994). Therefore, any variation in the circulation of cash generated by the EBT implementation should be exogenous to crime.

A simple before-and-after approach may still be problematic, however, unless the timing and pattern of EBT program implementation across counties was effectively random with regard to both observable and unobservable characteristics of these counties. This assumption is unlikely in this case for several reasons. While all Missouri counties adopted the EBT program eventually, it can be argued that counties that launched the program earlier might have differed from those counties that launched it later. For example, it might have been easier to implement a new policy in smaller and less populated counties with lower bureaucratic burden. Alternatively, larger, urban counties may be more readily equipped with the technology and the staff expertise to implement the EBT program. In fact, the city of St. Louis and Jackson County (Kansas City) both of which maintain a population of 600,000+ were among the first counties to implement a pilot EBT program.

Since various jurisdictions began the implementation of the EBT program in different months between 1997 and 1998, this variation gives us the leverage to employ a difference-in-difference method. This method basically amounts to estimating the difference in average crime rates in the jurisdictions with an EBT program before and after the implementation net of the difference in average crime rates in those jurisdictions without an EBT program. The method assumes that, in the absence of the implementation of EBT policy, crime rates would have trended similarly between treatment and non-treatment counties. We argue that this is a plausible assumption as EBT implementation is likely to be exogenous to crime. This assumption is not directly testable, although valuable insights can be gained by comparing the crime trends for each set of treatment jurisdictions in the period prior to any EBT implementation. Note that we do not have a set of control counties that had never been treated since all counties eventually implemented an EBT program. In different months between 1997 and 1998, however, each set of treatment counties served as a control for one another. Thus, if the crime trends for each set of treatment counties evolve similarly over time, this can be interpreted as suggestive evidence in support of our empirical strategy. In Figure 4, we illustrate the trends in our crime measures between January 1990 and May 1997. As shown in the figure, the trends largely appear to parallel each other, with the possible exception of robbery for the period of 1992-93.

Figure 4 about here

To motivate our empirical strategy further, we provide additional visual evidence regarding the effect of EBT implementation on crime rates in Missouri. In Figure 5, we illustrate the trends in the average rates of our crime variables before and after EBT implementation up to 24 months, weighted by county population. The vertical line represents the month and the year in which the EBT program became effective in each of the treatment counties. Since the program went into effect at different points in time, the graph is centered in the month and year of implementation (time 0) and tracks crime rates in the months leading up to and after this time for 24 months. As shown in Figure 5, there appears to be a reversal in the trend for the rate of total crime right at the time of EBT implementation. With the exception of larceny, the individual crimes exhibit a similar pattern.

Figure 5 about here

While the patterns presented in Figure 5 are suggestive of a causal relationship between EBT program implementation and crime, a stronger test of our hypotheses would take advantage of both within- and between-county differences in the crime rates between blocks of counties with and without EBT implementation in each month in each year. Note that the levels of crime may very well be different across treatment and control counties. This does not present a problem for our identification because the difference-in-difference model estimates changes in and not the levels of the outcomes. We also allow the crime rates to trend differently across counties by accounting for county specific linear trends in our empirical analysis. The difference-in-difference model employed in our analysis can be formalized as follows:

$$\text{Crime}_{cmy} = f\,(\alpha \text{EBT}_{cmy} + \beta_c + \lambda_{my} + \text{Trend}_{cmy} + \varepsilon_{cmy}) \quad (1)$$

One key feature of our crime data is that it is a non-negative count with a large number of zeros. Therefore, it is more appropriate to employ a count model for the estimation of equation (1). Both a Poisson regression and a negative binomial regression are well suited to address count data. A potential drawback with the Poisson regression is that it forces the conditional variance to be equal to the mean. Negative binomial regression, on the other hand, does not impose such an assumption. A test for over-dispersion yields a statistically significant positive over-dispersion, i.e. conditional variances are larger than means, for all of our outcomes. We therefore use a fixed effects negative binomial model and assume that *f* follows a negative binomial distribution.[9] The unit of observation in equation (1) is at the county-year-month group level. Crime_{cmy} is the count for one of our crime outcomes in county c in month m in calendar year y. The EBT_{cmy} is our key treatment variable, which equals one if county c has an EBT policy in effect in month m in calendar year y, and 0 otherwise. The β_c is a vector of county fixed effects, which serve to account for any permanent differences across counties that may affect crime. The λ_{my} are month*year fixed effects that serve to control for seasonality in crime as well as any changes in crimes that are common to all counties. Equation (1) also includes county-specific monthly time trends, Trend_{cmy}, to account for the possibility that variation in the EBT policy implementation might be non-random and could be correlated with unobserved factors that vary by county and month*year, and that might affect crime. We also control for county population in equation (1). Finally, the ε_{cm} is an idiosyncratic error term. We guard against the possibility of the error term being correlated within counties by clustering standard errors at the county level (Bertrand, Duflo, and Mullainathan, 2004). Furthermore, we weight the regressions by annual county population. The coefficient of interest in Equation (1) is α, the impact of EBT policy on crime rates.

[9] Despite rejecting the equality of variance and mean assumption, we also estimated our models using a fixed effects Poisson model. The estimates obtained from these models are similar to those from the fixed effects negative binomial models and are available from the authors upon request.

As noted earlier, our identification strategy does not require the levels in crime rates between treatment and control counties to be equal prior to implementation of the EBT program. Rather, it assumes that, in the absence of the EBT program, the rates of crime could have trended similarly between the two types of counties. Although we cannot test this assumption directly, we further assess the credibility of our research design by examining whether there are any systematic changes in crime rates prior to the EBT implementation. One way to do this is to conduct an event study analysis, which would allow us to trace out the trends in various types of crimes month-by-month for the periods leading up to and following the implementation of the EBT program. In practice, we implement this by estimating negative binomial regressions for our crime outcomes on a set of indicators for the months to and from the month of the EBT implementation up to 18 months, along with county and month fixed effects. The estimates from this analysis are presented in Figure 6. They indicate that there is no evidence of systematic changes in crime rates in the months prior to implementing the EBT program and that our results would not simply reflect continuation of long-run pre-existing trends. Nevertheless, we also allow county specific unobservables to trend differently by including county specific trends in our specifications.

Figure 6 about here

V. Empirical Results

Our main results from the estimation of equation (1) are presented in Table 3. Each cell in the table presents the effect size of the EBT program implementation and its standard error on each of the six crime outcomes, including total crime, burglary, robbery, assault, larceny, and motor vehicle theft. We present the estimates obtained from three different specifications. In column (1), we show the results from a specification that controls for county fixed effects. Column (2) controls for both county fixed effects and month by year fixed effects. Finally, column (3) presents the results from our most comprehensive specification, which also accounts for county-specific linear trends. By adding various sets of controls into the models sequentially, we get a sense of the importance of unobserved differences across counties as well as statewide trends and the seasonality in various types of crimes.

Table 3 about here

As shown in column (1), there appears to be a negative association between EBT implementation and robbery, burglary, and motor vehicle theft. The results presented in column (2), however, clearly indicate the importance of accounting for statewide trends and seasonality in crime. While the estimate on robbery switches its sign and is no longer statistically significant, the other five models point to a negative association between EBT implementation and crime. For example, EBT implementation is associated with a 16.6 percent reduction in total crime rate per 100,000 persons. The magnitudes of the effects for individual crime outcomes are also sizeable, suggesting declines of 22.7 percent for assault, 13 percent for burglary, and 16.3 percent for larceny. While the effect is negative for motor vehicle theft, it is relatively small in magnitude and statistically indistinguishable from zero. When we also allow for the crime rates to trend differently across counties as shown in column (3), the general pattern obtained in column (2) remains the same, except that the effect sizes become smaller. For example, EBT implementation causes the total crime rate to decrease by 9.8 percent. Similarly, burglary, assault, and larceny decrease by 7.9 percent, 12.5 percent, and 9.6 percent, respectively.

The estimates for robbery and auto-theft are both positive, but neither is large or statistically significant. As shown in Table 1, the means for both of these outcomes, especially that for robbery, are very small. Moreover, the number of county-month-year observations with a value of zero is also much higher for these variables. Specifically, 18,936 observations are zero for robbery and 12,120 observations are zero for auto-theft. Accordingly, the identifying variation is much lower for these two outcomes, which contributes to insignificant estimates.

To put the significant results into perspective, we next calculate the number of crimes that are reduced as a result of the EBT program implementation. Given that the total crime rate per 100,000 persons was 482.62 in the counties in the month prior to the implementation of EBT program, an effect size of 9.8 percent implies that approximately 47 fewer crimes per 100,000 persons would be committed per county per month as a result of the program. Since the average population in these counties was 62,870, the reduction in the number of crimes would be about 30. The corresponding figures for burglary, assault, and larceny would be 3.7, 5.9, and 14.2 per month, respectively.

If the EBT program implementation had a negative impact on the number of crimes committed, then a related question is whether there has been a decrease in the number of arrests that coincided with the crime decline. Estimating arrest models is a particularly useful specification check because, if our hypothesis for the effect of EBT program implementation is correct, then we should expect to see fewer crimes being committed, which in turn should result in fewer arrests. Instead, if we find an increase in arrests, then one could suspect that EBT implementation might have coincided with a period of more intensive policing.[10] To answer this question, we assembled data on arrest records for Missouri counties at the monthly level between 1990 and 2011, which are also available from the UCR. Then we estimated models similar to equation (1) using arrests as the outcome measure. We estimated these models separately for total arrests, arrests for non-drug related offenses, and arrest for drug-related offenses. The results from these models are presented in Table 4.[11]

Table 4 about here

An initial look at Table 4 reveals that, similar to crime models, controlling for seasonality and county-specific linear trends makes an important difference on the estimate of the effect of EBT program on arrests. Focusing on the results from the most comprehensive specification displayed in column (3), there is a negative relationship between the EBT program implementation and arrests for non-drug related offenses. These offenses decreased by 9.2 percent in response to the EBT program. By contrast, the estimate on the effect of EBT program implementation on drug-related offenses is small and statistically insignificant. Note that the effect on total arrests is negative and 8.9 percent, although the estimate barely misses statistical significance (p-value=0.101). The number of arrests across all offenses averaged 213.5 per 100,000 persons per county in the month prior to EBT implementation. An effect size of 8.9 percent translates into approximately 12 (0.089*213.5*62,870/100,000) fewer arrests per month per county as a result of the EBT program implementation. Similarly, the average number of arrests for

[10] An alternative story may that criminals get more desperate in the post-EBT period as they struggle to find attractive victims. Consequently, they may increase the frequency of their criminal acts, leading to an increase in crime rates. The results discussed above indicate that this was not the case. Nevertheless, under such a scenario, we should also see an increase in arrests.

[11] The sample sizes in the arrest models are smaller than those in the crime models because not all counties submitted arrest data in every month.

non-drug offenses is 195.67 per 100,000 per month for the same period, which implies a reduction of arrests from these types of offenses by about 11 (0.092*195.67*62,870/100,000). Thus, almost all of the reduction in arrests associated with the implementation of EBT program is accounted for by arrests from non-drug related offenses.

The results summarized above are supportive of our hypothesis that EBT program implementation caused crime rates to decrease for the overall crime rate as well as a number of individual crimes including burglary, assault, and larceny. The results for arrests reaffirm this hypothesis. It was expected that similar patterns would be observed for robbery as for the other acquisitive offenses, but the effect of the EBT program on this offence is estimated without much precision. As noted earlier, robbery has a small mean and many counties in the sample evidence zero offenses in this category. This is not wholly unexpected, as robbery is concentrated in densely populated, urban areas (Kneebone and Raphael, 2011). In fact, most of the variance associated with this offense is observed in two Missouri counties: Jackson County where Kansas City is located and the city of St. Louis. These cities dominate two of our eight sets of treatment counties. The remaining six are overwhelmingly rural with few or no robberies. Accordingly, little identifying variation comes from these six sets of treatment counties, resulting in an estimate on robbery that is neither sizeable nor statistically significant. We therefore modified our difference-in-difference analysis by designating only St. Louis City and Jackson County as treatment counties and the rest as control counties. Note that doing so eliminates any weight in the estimates associated with the variation in six sets of rural counties. This analysis produced the expected negative robbery estimates in each of the three specifications in Table 3. Furthermore, the estimates are statistically significant in the specifications in columns 1 and 2. While the estimate in column 3 is negative, it was still imprecisely estimated in conventional levels, most likely due to relatively restricted variation in the robbery measure.

Displacement

A possible challenge to the results presented thus far is that a reduction in the circulation of cash on the streets might induce some criminals to travel to neighboring counties without an EBT program to conduct illegal acts. This would suggest that crime would just be displaced from a treatment county to a neighboring control county, resulting in no net change in overall crime in the state. While such rational behavior on the part of criminals is theoretically plausible, available evidence from the criminological literature suggests that most offenders tend to operate within their own geographical activity or awareness spaces, which usually do not extend beyond several blocks (Brantingham and Brantingham, 1981, 1984). Nevertheless, we implement three robustness analyses to address this concern.

In the first analysis, we estimate our models excluding counties that are located on either side of the borderline for EBT implementation. Therefore, any crime committed by criminals who travel to a non-EBT county is excluded from our analysis. Under the assumption that criminals might be able to travel to a neighboring county, but no further than that, this analysis should produce the effect of EBT implementation on crime net of any cross-border crimes. These results are presented in Table 5. Due to large reductions in sample sizes, these results are estimated with less precision; only the estimate on burglary remains statistically significant. However, all of the estimates with the exception of one on motor vehicle theft are negative. In sum, despite larger standard errors the patterns of effects obtained in Table 5 are consistent with our overall conclusion that EBT lowers crime rates.

Table 5 about here

Next, we estimate all of our models controlling for an indicator variable for whether a neighboring county has an EBT program effective in each of our county-year-month observations. As presented in Table 6, controlling for this variable does not cause an appreciable change to our conclusion that EBT program implementation results in lower crime rates overall. Focusing on column (3) of Table 6, EBT program implementation causes the overall crime rate to go down by 10.2 percent. Similarly, assault and larceny decrease by 13.7 percent and 11.3 percent, respectively. The estimate on burglary is still negative, but no longer significant at conventional levels (p-value = 0.22). Moreover, the estimate on the binary indicator of whether any of the neighboring counties had EBT is negative in four of the six models and never statistically significant.

Table 6 about here

In our third analysis, we redefined our treatment indicator such that it takes on the value of one if any of the neighboring counties has an EBT program in effect and zero otherwise. Again, the idea is that if there is a displacement effect associated with EBT implementation then one should obtain a positive association between this indicator and crime. As shown in Table 7, we do not find any evidence supporting this argument. There is no particular pattern in the sign of the indicator of EBT implementation in a neighboring county. Furthermore, the effect sizes are very small and none of them is estimated with statistical significance. To sum, the evidence obtained from the three robustness analyses reveals no indication of any migratory behavior by potential criminals across county borders in response to EBT implementation.

Table 7 about here

Program Participation

It is important to note that the timing of EBT implementation in Missouri coincides with a period of declines in the caseloads for Supplemental Nutrition Assistance Program (SNAP) and the Temporary Assistance to Needy Families (TANF) program (see Figure 7). Participants in these two programs together constitute the overall client base for the EBT program. It could be argued that the decline in crime associated with EBT implementation might be attributable to the fact that the period of interest was a time with a decreasing number of EBT card recipients, who are also the potential victims of street crime. While this may first appear like a plausible explanation, the identification of an EBT program effect in our empirical models does not come from a before-and-after comparison in the program implementation. Rather, we compare the change in crime associated with EBT between treatment and control counties. If caseloads of SNAP and TANF exhibited similar trends across counties, then our results should not be affected by this development. Furthermore, some of the variation in the declining caseloads for these two programs should be captured by county specific linear time trends. Nevertheless, we obtained data on the SNAP and TANF caseloads for individual Missouri counties between 1990 and 2011 and control for these two variables in our models along with the usual fixed effects, county specific linear trends, and county population. We present these results in Table 8. As illustrated in column (3), controlling for SNAP and TANF caseloads has little impact on the estimate of the effect of the EBT program. Similar to Table 3, the estimates on EBT for total crime, burglary, assault, and larceny are all negative and statistically significant, while the estimates on the other two crimes are positive, but small in size and imprecisely estimated.

Figure 7 and Table 8 about here

In an additional sensitivity analysis, we turn our attention to a type of criminal act which is unlikely to be associated with an immediate financial motivation, and therefore unlikely to be affected by EBT program implementation. One potentially good candidate for such placebo analysis, as explained above, is the crime of rape. The estimation of equation (1) with rape as the outcome variable produced an imprecisely estimated coefficient on the indicator of EBT program implementation. Next we repeated this analysis for arrests for rape and non-rape related sex offenses (e.g., prostitution, sex trafficking). Again, the estimate on the effect of EBT program implementation was not statistically significant in either case.[12]

Cash Reduction

Finally, we implement a back of the envelope calculation of the degree to which removal of cash on the streets is related to reduction of crime. In particular, we are interested in getting an estimate of ∂Crime/∂Cash. This term can be calculated as the ratio of ∂Crime/∂EBT and ∂Cash/∂EBT. Note that we already obtained an estimate of the former earlier. To get an approximate estimate for the latter, we calculate the total

[12] The estimates on rape and arrests for rape are 0.17 and 0.06, respectively. Neither of these estimates is statistically significant, but it is partly because of large standard errors. Note that rape is a rare crime in our sample with an average of 2.36 per month in the entire state. With such low variation, it is quite possible that any effect could be driven by outliers. Therefore, we acknowledge that the results on rape and arrests for rape should be viewed only as suggestive and caution must be exercised in interpreting them..

payments for SNAP and TANF in Missouri in 1997 in the treatment counties.[13] This amount is \$671.2 million, which can be interpreted as the maximum amount of cash that needs to be removed from circulation in order to produce a decrease of 9.8 percent in the total crime rate. Note that this figure is an upper limit since many TANF recipients can technically carry cash on them by withdrawing welfare payments from banks or ATM machines using their EBT cards. In any event, by removing something less than \$55.9 million in cash from circulation each month, we estimate that EBT achieved a nearly 10 percent decrease in the crime rate.

VI. Discussion

We have shown that the move from check-based welfare payments to EBT in Missouri is associated with a substantial drop in street crime, with significant effects for burglary, larceny, and assault, and some indication of a non-zero effect for robbery. These findings were robust to a number of alternate sources of variation in the data we tested. For example, they remain consistent when replicating the analysis using arrests instead of reported crimes. We also found that the effect of EBT was consistent across counties and that offending was not displaced from those counties that implemented EBT to those that had not yet done so. If that had been the case, the obvious policy implication would be that EBT implementation would only create a sustained reduction in crime if its application were universal (i.e., that all counties in a state would have to remove cash from the system simultaneously). Otherwise, crime decreases would be localized to counties that implemented EBT and would be offset by crime increases in neighboring

[13] Ideally, we would like to use the total payments in the particular month prior to EBT program implementation. However, we only have data on the average monthly payments for each program in each year.

counties where cash had not been removed. Our finding is consistent with criminological literature indicating that offenders tend to operate within their own geographical awareness space (Brantingham and Brantingham, 1981, 1984). More importantly it supports our contention that the removal of cash through EBT has negative effects on street crime that are enduring.

The most likely explanation for our overall findings is that moving from a check-based system to EBT effectively reduced the amount of cash on the streets available to be taken or used for illegal purposes[14]. Because cash is critical to the pursuit of illicit action, this served as a brake on the etiological cycle that drives street crime, slowing it such that the rate of offending was lessened, not only for predatory offenses like burglary and larceny, but also for assaultive disputes fueled by the heavy drug and alcohol use associated with participation in streetlife. Additionally, a reduction in cash may have had a distal effect on criminal assault, much of which is retaliatory in nature and flows directly from the predatory victimization of fellow offenders by those caught up in the etiological cycle (Jacobs, Topalli, and Wright, 2000; Topalli, Wright, and Fornango, 2002). Less cash meant that fewer such crimes were committed, so the rate of retaliation should have fallen accordingly.

Our results underline the critical role played by cash in the etiology of street crime, especially considering that they are based on a far from complete removal of cash from the local economy. Even though welfare recipients still can withdraw cash with their

[14] A direct test of our assertion that EBT implementation removed cash from the system would be to measure whether crime rates proximal to check cashing and payday lending establishments in neighborhoods where a significant proportion of benefits recipients reside changed pre- to post-EBT implementation. A number of studies employing geospatial analysis identify such establishments as crime “attractors” for the kinds of offenses investigated in this study (see e.g., McCord, Ratcliffe, Garcia, and Taylor, 2007; McCord & Ratcliffe, 2007).

cards, the reduction in cash on the streets ushered in by the introduction of EBT had widespread and significant effects on multiple types of offenses. It stands to reason that a more complete reduction in cash would have even stronger effects on street crime. This could be accomplished by restricting the use of EBT benefits to digital transactions only. Doing so would effectively dry up a significant source of the supply of cash available to victims and offenders in poor neighborhoods leading to concomitant decreases in street crime.

On its face, this may appear to be a highly desirable outcome. It is no secret that street crime disproportionately affects those living in poor neighborhoods. Yet, there are a number of potential unintended consequences of removing cash from such communities. Recall that the crux of our argument is that cash is a nearly indispensable transactional medium for illicit activity. But such activities are not limited to drug-use, prostitution, or trading in stolen goods. They also include legitimate services provided without official sanction (e.g., so-called shade tree automotive repair shops, unregistered daycare facilities) and those provided by individuals of questionable or outright illegal status (e.g., convicts or undocumented immigrants). In the absence of cash, these services and the people who perform them would effectively be shut out of both legitimate and illegitimate markets, with potentially serious knock-on effects for poor consumers and local economies.

EBT was not implemented as a crime-fighting tool, and the supplanting of cash by various forms of digital monetary transfer had been taking place for many years before EBT was established. Assuming these trends continue, the volume of cash available for transactions should continue to decrease as cash becomes less and less useful, whether or not access to cash via EBT is further restricted. Because this larger pattern in the reduction of cash transactions affects both the affluent and the poor, it should have a similar but even larger effect on street crime and the black market economy than EBT implementation. More affluent communities will be relatively unaffected. Poor communities should benefit from less street crime. The policy challenge, however, is to safeguard these benefits while minimizing the costs to the poor of the coming cashless economy.

References

Armey, L. E., Lipow, J. & Webb, N. J. (2012). The impact of rlectronic financial payments on crime. Available at SSRN: http://ssrn.com/abstract=2137266 or http://dx.doi.org/10.2139/ssrn.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics, 119*, 249-275.

Blumstein, A., & Wallman, J. (Eds.) (2006). *The crime drop in America*. New York, NY:Cambridge University Press.

Brantingham, P. J., & Brantingham, P. L. (1981). *Environmental criminology.* Beverly Hills, CA: Sage Publications.

Brantingham, P. J., & Brantingham, P. L. (1984). *Patterns in crime.* New York, NY: Macmillan Press.

Brezina, T., Tenkin, E., & Topalli, V. (2009). "Might not be a tomorrow": A multimethods approach to anticipated early death and youth crime. *Criminology, 47*, 1091-1129.

Corman, H., & Mocan, N. (2005). Carrots, Sticks, and Broken Windows*. *Journal of Law and Economics*, *48*(1), 235-266.

Donohue, J., & Siegleman, P. (1998). Allocating resources among prisons and social programs in the battle against crime. *The Journal of Legal Studies*, *27*(1).

Erling, G. (2013). Cash is dead, long live cash. *Journal of Payments Strategy & Systems*, *7*(1), 43-49.

Farrell, G., Tseloni, A., Mailley, J., & Tilley, N. (2011). The crime drop and the security hypothesis. *Journal of Research in Crime and Delinquency*, *48*, 147-175.

Federal Deposit Insurance Corporation (2012). National survey of unbanked and underbanked households: Executive summary. Retrieved from http://www.fdic.gov/householdsurvey/2012_unbankedreport_execsumm.pdf

Fishback, P. V., Johnson, R. S., & Kantor, S. (2010). Striking at the roots of crime: The impact of welfare spending on crime during the great depression. *Journal of Law and Economics*, *53*(4), 715-740.

Foley, C. F. (2011). Welfare payments and crime. *The Review of Economics and Statistics*, *93*(1), 97-112.

Food and Nutrition Service, United States Department of Agriculture. (2013). *A short history of SNAP*. Retrieved from http://www.fns.usda.gov/snap/rules/Legislation/about.htm

Ford, J. M., & Beveridge, A. A. (2004). “Bad” neighborhoods, fast food,“sleazy” businesses, and drug dealers: Relations between the location of licit and illicit businesses in the urban environment. *Journal of Drug Issues*, *34*(1), 51-76.

Hannon, L., & DeFronzo, J. (1998). The truly disadvantaged, public assistance, and crime. *Social Problems*, 383-392.

Jacobs, B. A., Topalli, V., & Wright, R. (2000). Managing retaliation: drug robbery and informal sanction threats. *Criminology*, *38*(1), 171-198.

Kneebone, E., & Raphael, S. (2011). *City and suburban crime trends in metropolitan America*. Brookings Institution Metropolitan Policy Program.

Liedka, R. V., Piehl, A. M., & Useem, B. (2006). The crime-control effect of incarceration: Does scale matter? *Criminology & Public Policy, 5*(2), 245-276.

Levitt, S. D. (1996). The effect of prison population size on crime rates: Evidence from prison overcrowding litigation. *The Quarterly Journal of Economics*, *111*(2), 319-351.

Levitt, S. D. (1999). The limited role of changing age structure in explaining aggregate crime rates. *Criminology*, *37*(3), 581-598.

Levitt, S. D. (2004). Understanding why crime fell in the 1990s: Four factors that explain the decline and six that do not. *Journal of Economic Perspectives, 18*(1), 163-190.

Leyshon, A., & Thrift, N. (1994). Access to financial services and financial infrastructure withdrawal: Problems and policies. *Area, 26*(3), 268-275.

Leyshon, A., & Thrift, N. (1995). Geographies of financial exclusion: financial abandonment in Britain and the United States. *Transactions of the Institute of British Geographers, 20*(3), 312-341.

Littman, D. & Oliver, N. (2012). Meeting the demand for cash. *Forefront*, 20-23. Retrieved from: http://www.clevelandfed.org/Forefront/2012/winter/pdf/ff_2012_spring_13.pdf

Maltz, M. D., & Targonski, J. (2002). A note on the use of county-level UCR data. *Journal of Quantitative Criminology, 18, 297-318.*

Marcotte, D. E., & Markowitz, S. (2011). A cure for crime? Psychopharmaceuticals and crime trends. *Journal of Policy Analysis and Management*, *30*(1), 29-56.

McCord, E. S., & Ratcliffe, J. H. (2007). A micro-spatial analysis of the demographic and criminogenic environment of drug markets in Philadelphia. *Australian & New Zealand Journal of Criminology*, *40*, 43-63.

McCord, E. S., Ratcliffe, J. H., Garcia, R. M., & Taylor, R. B. (2007). Nonresidential crime attractors and generators elevate perceived neighborhood crime and incivilities. *Journal of Research in Crime and Delinquency*, *44,* 295-320.

Missouri Department of Social Services, Family Support Division, Electronic Benefits Transfer. (2013a). *Information for EBT cardholders*. Retrieved from: http://dss.mo.gov/fsd/fstamp/ebt/cardholder.htm

Missouri Department of Social Services, Family Support Division, Electronic Benefits Transfer. (2013b). *Benefit availability dates.* Retrieved from http://dss.mo.gov/fsd/fstamp/ebt/ebtdates.htm

Mo. Rev. Stat. § 208. 182 (2013). Retrieved from: http://www.moga.mo.gov/statutes/C200-299/2080000182.HTM

Office of the Vice-President (1993). *Reeingineering through information technology: Accompanying report of the national performance review*. Retrieved from: http://govinfo.library.unt.edu/npr/library/reports/it.html

Personal Responsibility and Work Opportunity Reconciliation Act, 64 Fed. Reg. 28763 (May, 27,1999) (to be codified at 7 C. F. R. pts. 274).

Raphael, S., & Winter-Ebmer, R. (2001). Identifying the Effect of Unemployment on Crime. *Journal of Law and Economics*, *44*, 259-283.

Rhine, S. L., & Greene, W. H. (2012). Factors that contribute to becoming unbanked. *Journal of Consumer Affairs*, *45*, 21-45.

Rosenfeld, R. & Fornango, R. (2007). The impact of economic conditions on robbery and property crime: The role of consumer sentiment. *Criminology, 45*, 735-769.

Shamah, D. (2013, September 24th). Government dream of a cashless society already a reality. The Times of Israel. Retrieved from, http://www.timesofisrael.com/government-dream-of-a-cashless-society-already-a-reality/

Shover, N. (1996). *Great pretenders: Pursuits and careers of persistent thieves*. Boulder, CO: Westview Press.

Federal Reserve System (2011). The 2010 Federal Reserve Payments Study: Noncash payment trends in the United States: 2006-2009.

Tomlinson S., (2012, March 20th). Sweden could be first country to go cashless as even churches are taking cards for offerings. *The Daily Mail*. Retrieved from http://www.dailymail.co.uk/news/article-2117643/Sweden-Country-cashless-churches-accepting-cards-offerings.html

Topalli, V., Wright, R., & Fornango, R. (2002). Drug dealers, robbery and retaliation. Vulnerability, deterrence and the contagion of violence. *British Journal of Criminology*, *42*(2), 337-351.

U.S. General Accounting Office (1994). *Food assistance: Reducing fraud and abuse in the food stamp program with electronic benefit transfer technologies*. Retrieved from: http://www.gao.gov/products/T-RCED-94-125.

U.S. Department of Agriculture (2003). The extent of trafficking in the food stamp program: 1999-2002. Retrieved from: http://www.fns.usda.gov/sites/default/files/Trafficking1999-2002.pdf

Varjavand, R. (2011). Growing underground economy: The evidence, the measures, and the consequences. *Journal of International Management Studies, 11*(3), 133-142.

Wadsworth, T. (2010). Is immigration responsible for the crime drop? An assessment of the influence of immigration on changes in violent crime between 1990 and 2000. *Social Science Quarterly*, *91*(2), 531-553.

Wright, R., & Decker, S. (1994). *Burglars on the job: Streetlife and residential break-ins.* Boston, MA: Northeastern University Press.

Wright, R., & Decker, S. (1997). *Armed robbers in action.* Boston, MA: Northeastern University Press.

Wright, R. & Topalli, V. (2103). Choosing street crime. In F. T. Cullen & P. Wilcox (Eds.), *Oxford handbook of criminological theory* (p. 461). New York, NY: Oxford University Press.

Wright, R., Topalli, V., & Jacques, S. (2014). Crime in Motion: Predation, Retaliation, and the Spread of Urban Violence. In *On Retaliation: Toward an Interdisciplinary Understanding of a Basic Human Condition*. New York: Berghahn Books.

Zhang, J. (1997). The effect of welfare programs on criminal behavior: A theoretical and empirical analysis. *Economic Inquiry*, *35*(1), 120-137

Zhao, J., Scheider, M. C., & Thurman, Q. (2002). Funding community policing to reduce crime: Have COPS grants made a difference? *Criminology & Public Policy*, *2*(1), 7-32.

Zimring, F. E. (2006). *The great American crime decline*. Oxford University Press

Figure 1: The Etiological Cycle of Street Crime

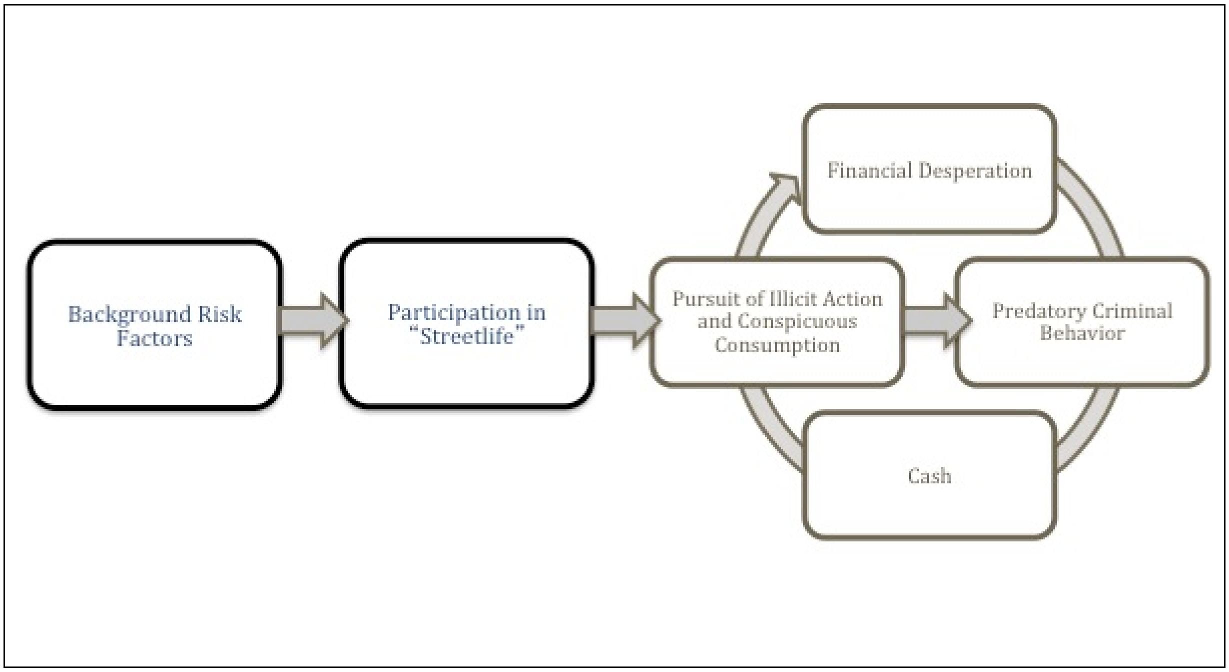


Figure 2: EBT Program Implementation Map for Missouri Counties

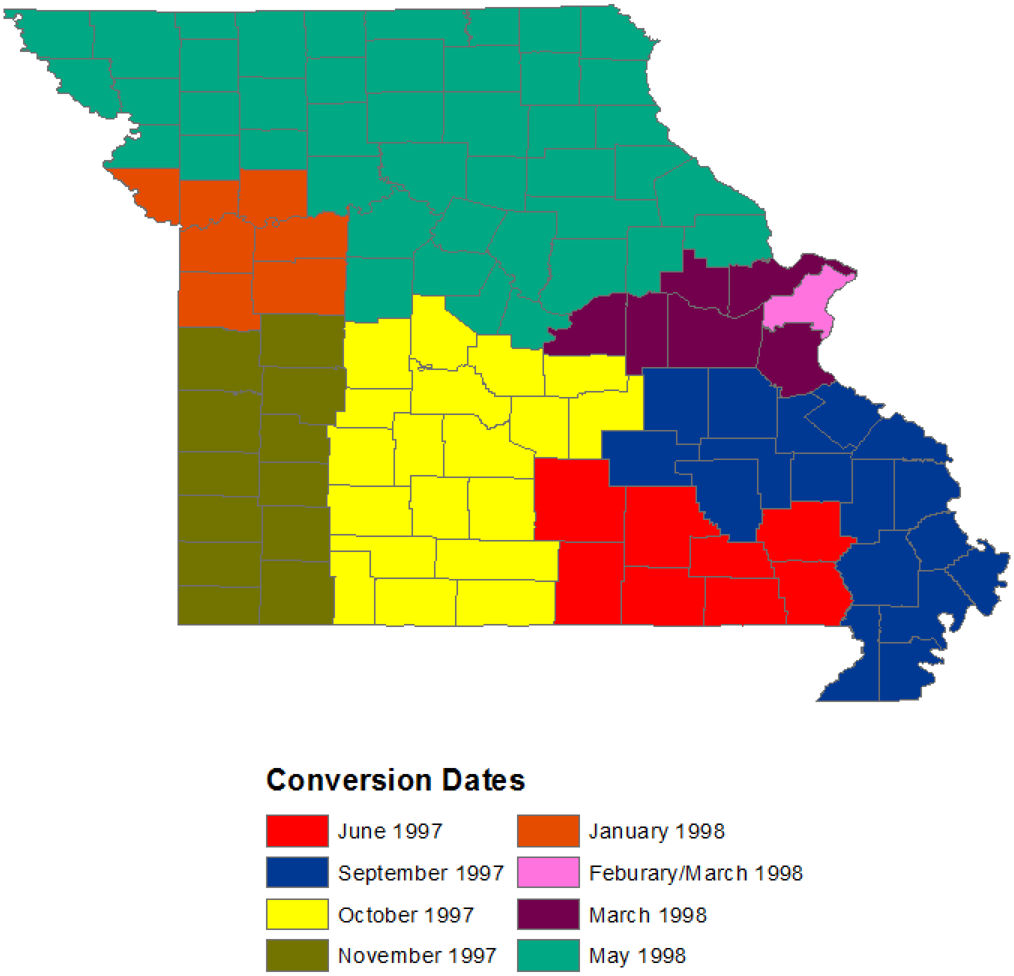
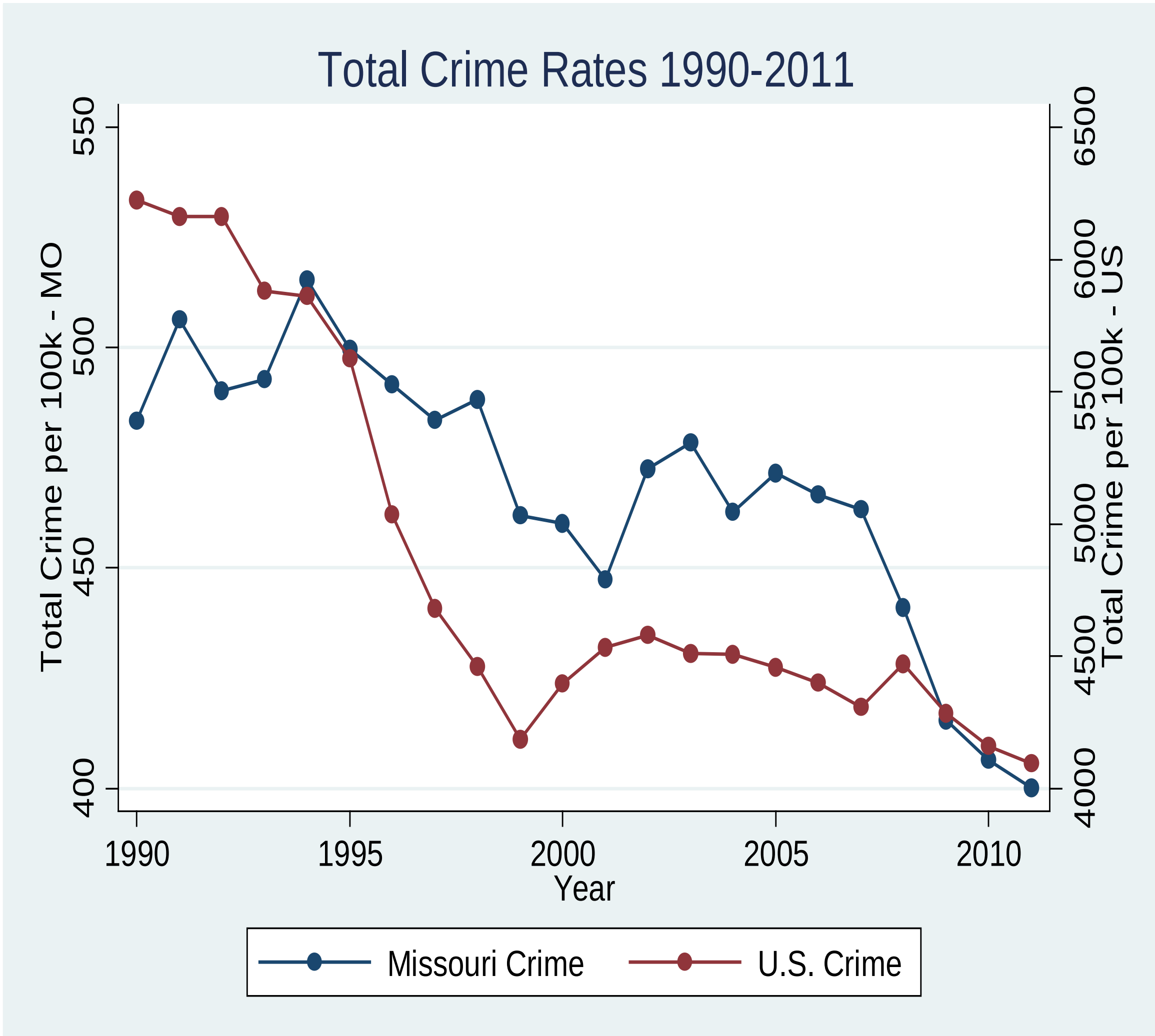
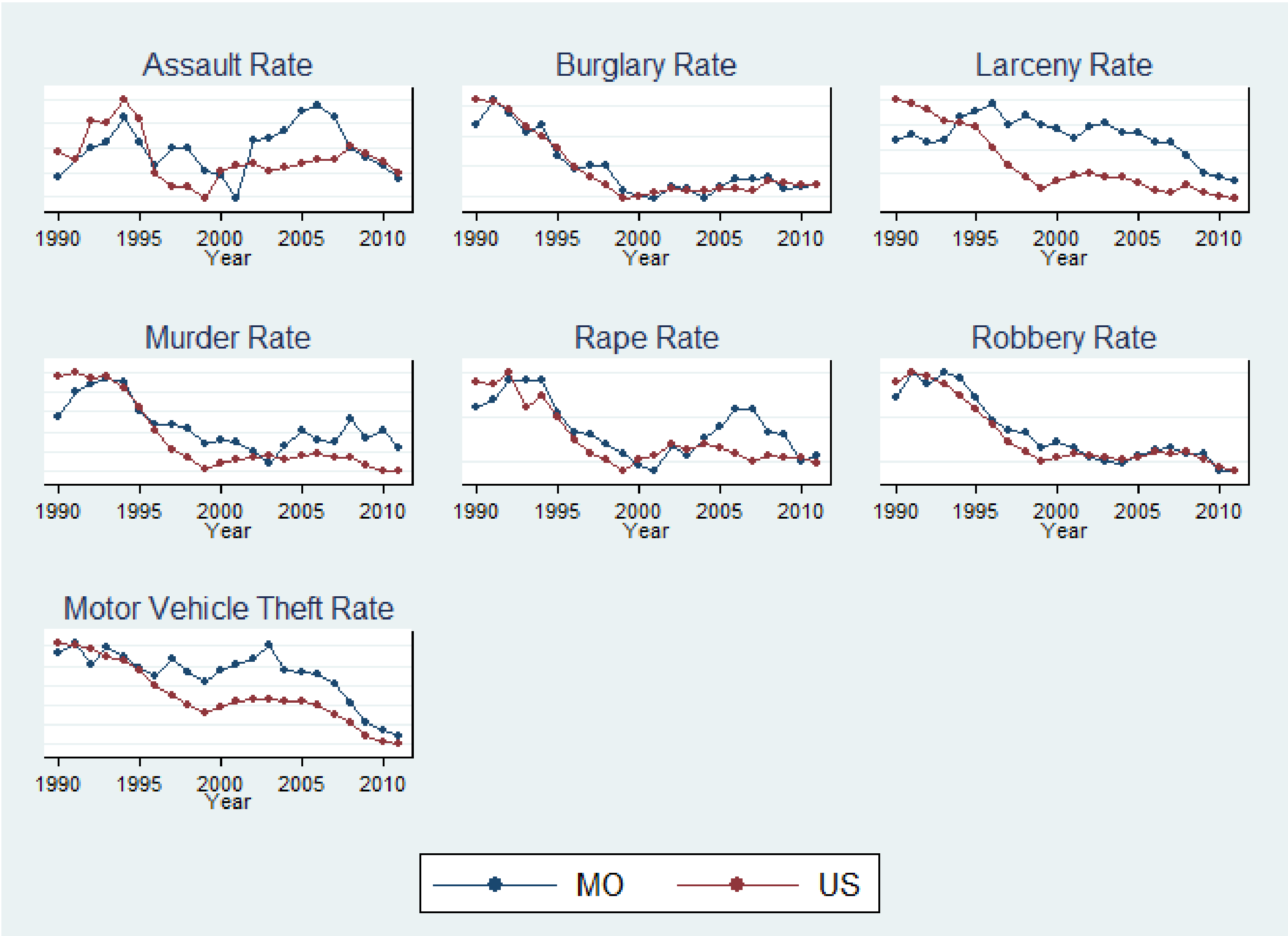


Figure 3A: Trends in Total Crime for Missouri and the United States



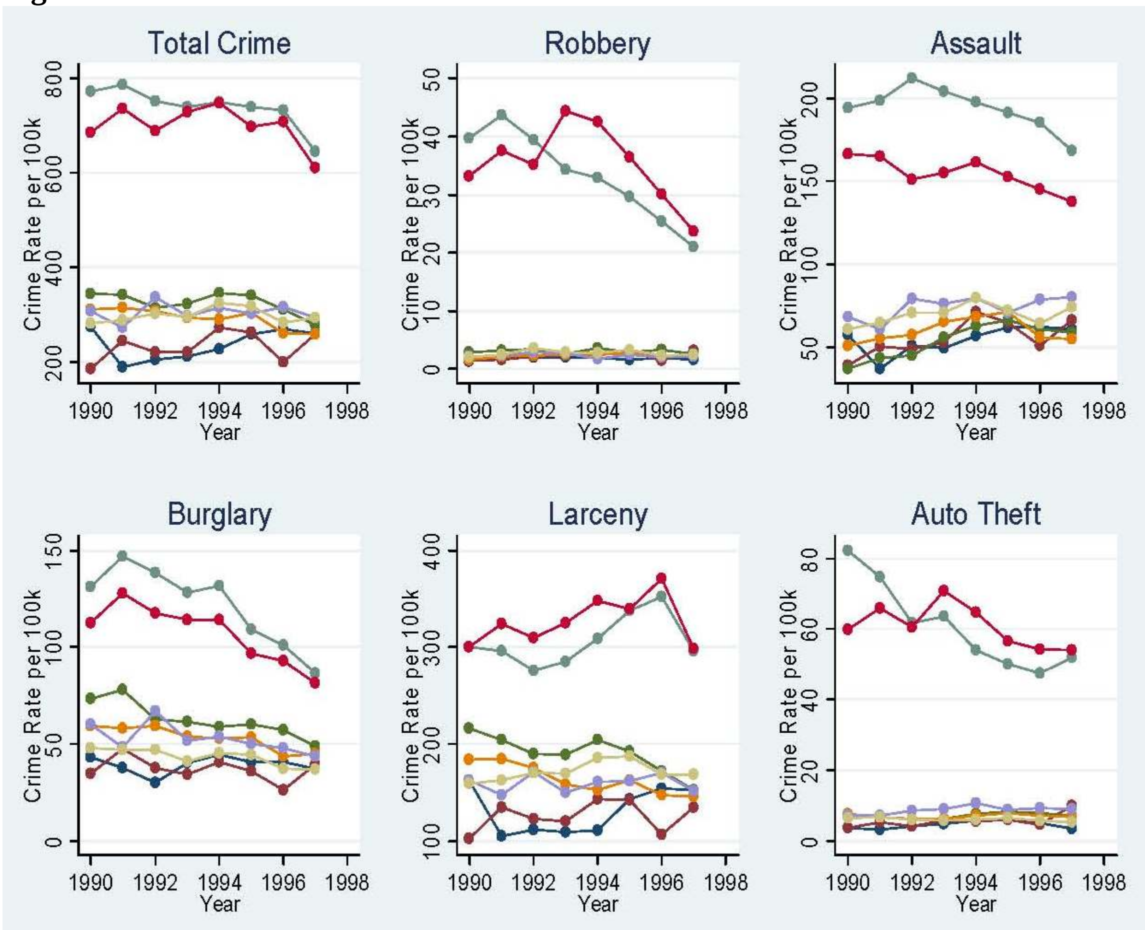
Note: Rates refer to crimes per 100,000 population.

Figure 3B: Trends in Part I Crimes for Missouri and the United States



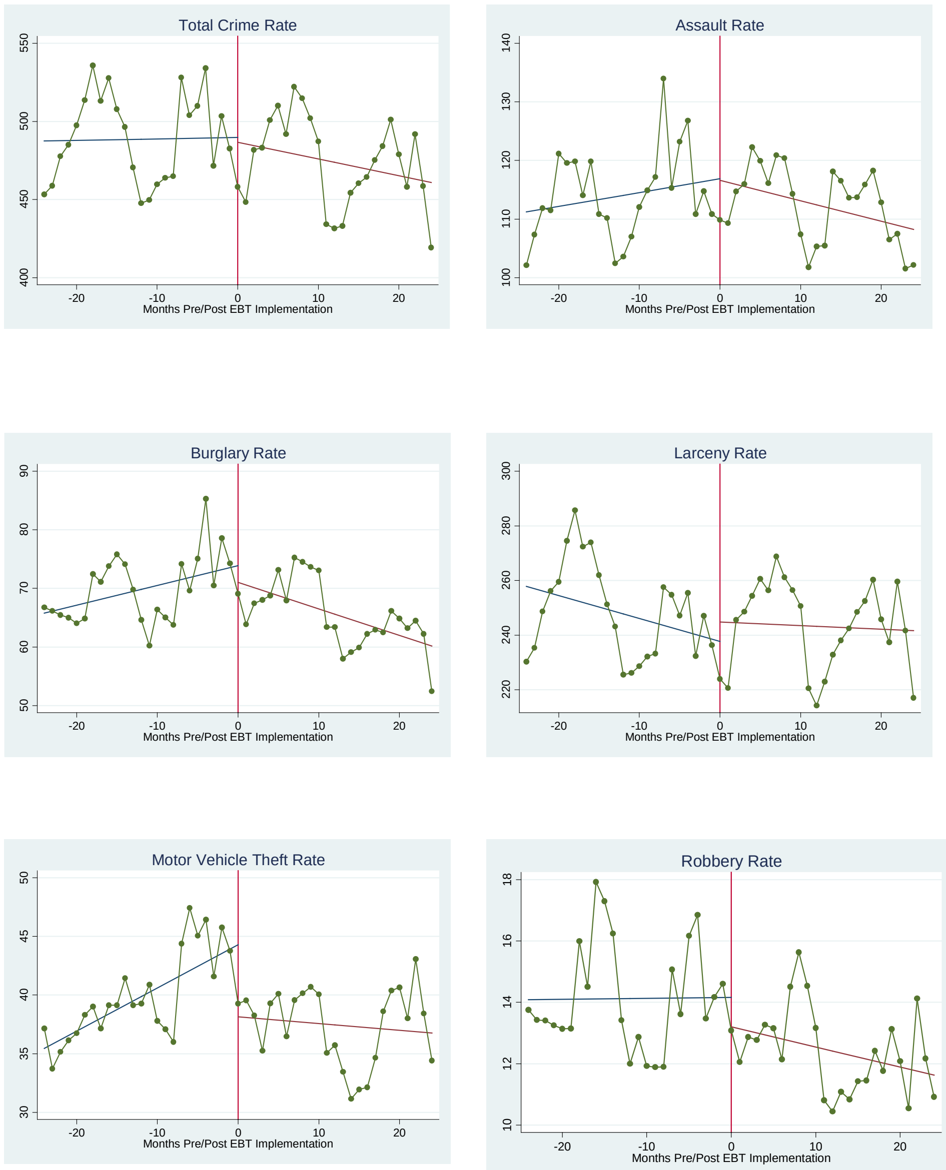
Note: Rates refer to crimes per 100,000 population.

Figure 4: Pre-treatment Period Trends in Crime



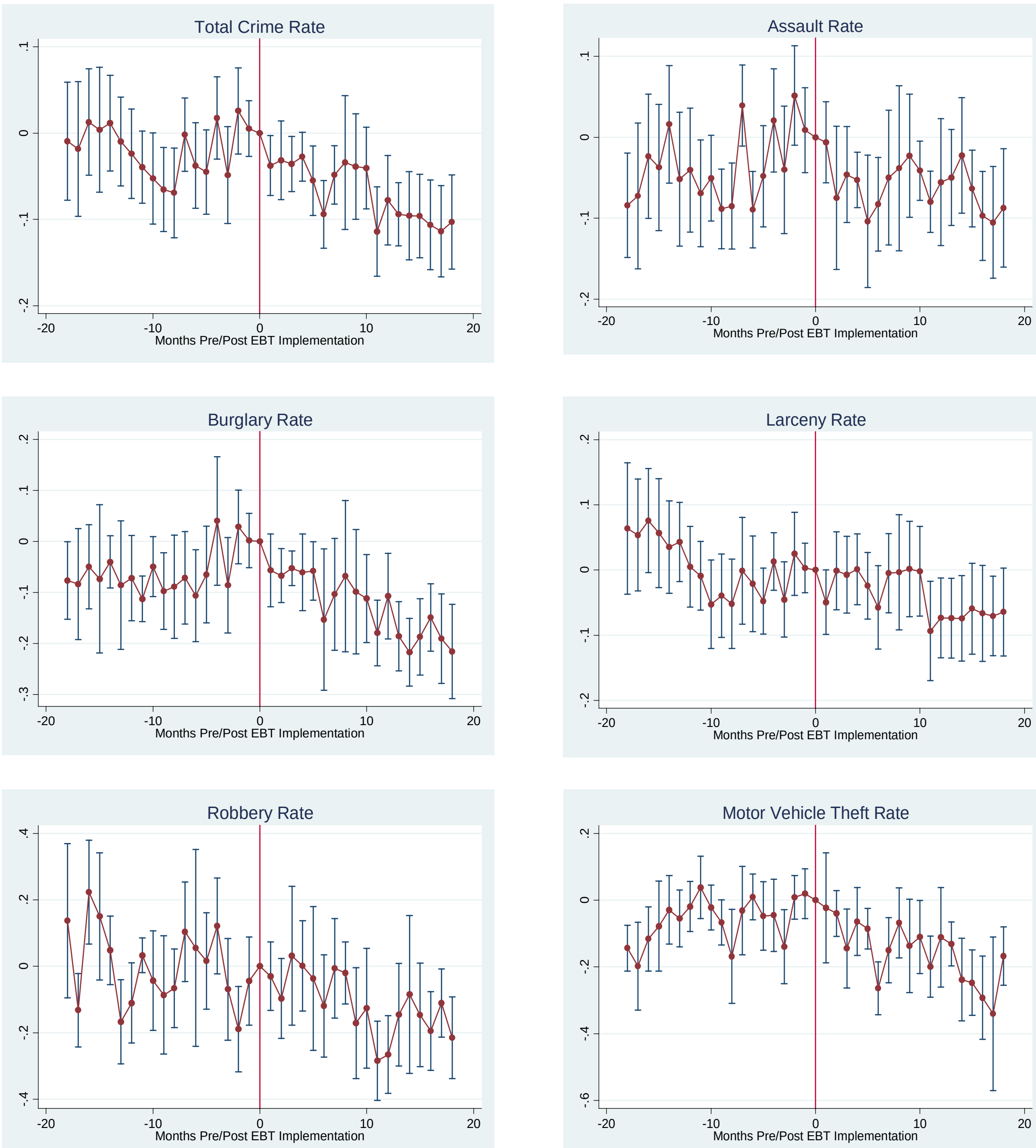
Note: Rates refer to crimes per 100,000 population.

Figure 5: Trends in Crime Rates Before and After EBT Program Implementation



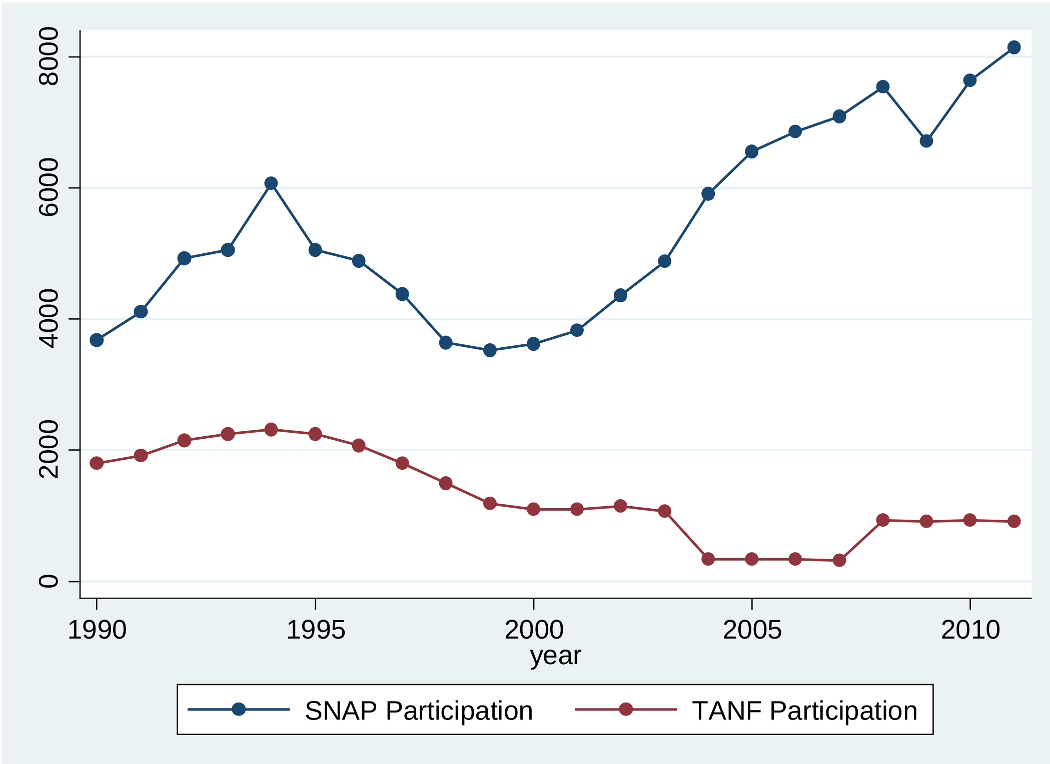
Note: Rates refer to crimes per 100,000 population.

Figure 6: Event-Study Estimates of the Effect of EBT Program Implementation on Crime Rates



Notes: 18 months prior to and after implementation. The reference month is the month of implementation. The figure displays coefficients estimates and 95% confidence intervals. .

Figure 7: Trends for TANF and SNAP Caseloads in Missouri



Note: Figures refer to average county-month caseloads.

Table 1: Descriptive Statistics

Variable	Full Sample	EBT=1	EBT=0
Total crime rate	467.82	447.52	508.14
	(358.27)	(315.42)	(428.07)
Robbery rate	13.24	10.59	18.51
	(23.92)	(17.71)	(32.31)
Assault rate	115.36	114.03	117.98
	(90)	(73.88)	(115.5)
Burglary rate	69.14	62.68	81.97
	(58.01)	(47.37)	(73.11)
Larceny rate	229.74	223.26	242.60
	(152.26)	(145.4)	(164.3)
Auto theft rate	27.62	24.49	33.86
	(41.27)	(36.93)	(48.16)
SNAP caseload	5,966.41	6,066.37	5,748.7
	(14,631.4)	(14,402.25)	(15,117.06)
TANF caseload	1,424.11	906.5	2,552.18
	(5,346.23)	(3,339.52)	(8,044.73)
County Population	53,745.72	52,144.75	57,234.83
	(127,542.20)	(124,008.2)	(134,865.60)

Notes: Rates are per 100,000 persons. Standard deviations are in parentheses. Sample sizes for the full sample are 26,268 for total crime, assault, TANF, and population, 26,259 for robbery, 26,267 for burglary and larceny, 26,256 for auto theft, and 26,256 for SNAP.

Table 2: Distribution of Crimes across Months

Month	Total crime	Robbery	Assault	Burglary	Larceny	Auto-theft
January	7.58	8.20	7.29	7.94	7.46	8.36
February	6.82	6.82	6.92	6.72	6.75	7.08
March	7.96	7.59	8.21	7.70	7.97	7.70
April	8.01	7.46	8.53	7.59	8.01	7.35
May	8.55	7.94	9.19	8.20	8.50	7.71
June	8.67	8.03	8.90	8.27	8.79	8.22
July	9.26	8.73	9.26	9.10	9.33	9.25
August	9.34	9.02	9.07	9.28	9.51	9.37
September	8.87	9.01	9.02	9.06	8.75	8.79
October	8.80	9.22	8.68	8.98	8.79	8.79
November	8.05	8.74	7.54	8.56	8.02	8.59
December	8.09	9.25	7.38	8.61	8.12	8.79

Note: Each cell represents the proportion of crime reported in a particular month.

Table 3: The Effect of EBT Program Implementation on Crime in Missouri

Crime	**EBT**	**EBT**	**EBT**
Total Crime	0.026	-0.166***	-0.098***
	(0.063)	(0.042)	(0.036)
Robbery	-0.294***	0.026	0.028
	(0.105)	(0.086)	(0.040)
Assault	0.138	-0.227***	-0.125***
	(0.097)	(0.052)	(0.041)
Burglary	-0.198***	-0.130***	-0.079*
	(0.047)	(0.050)	(0.042)
Larceny	0.007	-0.163***	-0.096**
	(0.056)	(0.050)	(0.045)
Auto Theft	-0.113*	-0.061	0.028
	(0.063)	(0.049)	(0.039)
County Fixed Effects	Yes	Yes	Yes
Month-by-Year Fixed Effects	No	Yes	Yes
County Specific Linear Trends	No	No	Yes

Notes: Robust standard errors clustered at the county level are in parentheses. Each cell presents the coefficient on the indicator for EBT Implementation. All models are weighted by the average county population. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. The numbers of observations are 26,268 for total crime and assault, 26,259 for robbery, 26,267 for burglary and larceny, and 26,257 for auto theft.

Table 4: The Effect of EBT Program Implementation on Arrests in Missouri

Arrest	EBT	EBT	EBT
Total Arrests	0.262***	-0.100*	-0.089
	(0.044)	(0.057)	(0.054)
Non-drug Arrests	0.219***	-0.100*	-0.092*
	(0.042)	(0.057)	(0.054)
Drug Arrests	0.582***	-0.009	-0.012
	(0.065)	(0.094)	(0.103)
County Fixed Effects	Yes	Yes	Yes
Month-by-Year Fixed Effects	No	Yes	Yes
County Specific Linear Trends	No	No	Yes

Notes: Robust standard errors clustered at the county level are in parentheses. Each cell presents the coefficient on the indicator for EBT Implementation. All models are weighted by the average county population. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. The numbers of observations are 224,636 for total arrests, 24,585 for non-drug related arrests, and 20,419 for drug related arrests.

Table 5: The Effect of EBT Program Implementation on Crime in Missouri – Border Counties Excluded

Crime	EBT	EBT	EBT
Total Crime	-0.053	-0.104*	-0.046
	(0.095)	(0.059)	(0.041)
Robbery	-0.490***	0.001	-0.020
	(0.064)	(0.066)	(0.063)
Assault	-0.066	-0.222**	-0.106
	(0.154)	(0.100)	(0.076)
Burglary	-0.242***	-0.114**	-0.082**
	(0.070)	(0.054)	(0.041)
Larceny	-0.016	-0.063	-0.001
	(0.053)	(0.060)	(0.053)
Auto Theft	-0.145	0.009	0.046
	(0.102)	(0.085)	(0.045)
County Fixed Effects	Yes	Yes	Yes
Month-by-Year Fixed Effects	No	Yes	Yes
County Specific Linear Trends	No	No	Yes

Notes: Robust standard errors clustered at the county level are in parentheses. Each cell presents the coefficient on the indicator for EBT Implementation. All models are weighted by the average county population. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. The numbers of observations are 11,676 for total crime, assault, and burglary, 11,674 for robbery, 11,675 for larceny, and 11,671 for auto theft.

Table 6: The Effect of EBT Program Implementation on Crime in Missouri – Controlling for EBT Implementation in a Neighboring County

Crime	EBT	Neighbor	EBT	Neighbor	EBT	Neighbor
Total Crime	-0.104***	-0.020	-0.193***	-0.092**	-0.102***	-0.020
	(0.037)	(0.033)	(0.042)	(0.045)	(0.037)	(0.033)
Robbery	-0.295***	-0.412***	0.002	-0.167	0.022	-0.075
	(0.105)	(0.113)	(0.091)	(0.135)	(0.039)	(0.120)
Assault	0.137	-0.036	-0.266***	-0.139*	-0.137***	-0.041
	(0.098)	(0.089)	(0.054)	(0.079)	(0.047)	(0.069)
Burglary	-0.198***	-0.042	-0.113**	0.058	-0.060	0.067
	(0.047)	(0.079)	(0.056)	(0.092)	(0.049)	(0.066)
Larceny	0.010	-0.066	-0.203***	-0.139**	-0.113**	-0.057
	(0.056)	(0.056)	(0.048)	(0.055)	(0.045)	(0.046)
Auto Theft	-0.112*	0.085	-0.046	0.058	0.052	0.106
	(0.064)	(0.104)	(0.048)	(0.099)	(0.037)	(0.102)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year Fixed Effects	No	No	Yes	Yes	Yes	Yes
County Specific Linear Trends	No	No	No	No	Yes	Yes

Notes: Robust standard errors clustered at the county level are in parentheses. Each cell presents the coefficient on the indicator for EBT Implementation. All models are weighted by the average county population. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. The numbers of observations are 26,268 for total crime and assault, 26,259 for robbery, 26,267 for burglary and larceny, and 26,257 for auto theft.

Table 7: The Effect of EBT Program Implementation in Any Neighboring County on Crime in Missouri

Crime	EBT	EBT	EBT
Total Crime	-0.061*	-0.007	0.026
	(0.034)	(0.051)	(0.037)
Robbery	-0.228**	-0.168	-0.083
	(0.094)	(0.133)	(0.118)
Assault	-0.135**	-0.023	0.018
	(0.057)	(0.077)	(0.061)
Burglary	0.085	0.107	0.090
	(0.087)	(0.079)	(0.055)
Larceny	-0.073	-0.048	-0.005
	(0.049)	(0.068)	(0.053)
Auto Theft	0.166*	0.077	0.084
	(0.094)	(0.097)	(0.097)
County Fixed Effects	Yes	Yes	Yes
Month-by-Year Fixed Effects	No	Yes	Yes
County Specific Linear Trends	No	No	Yes

Notes: Robust standard errors clustered at the county level are in parentheses. Each cell presents the coefficient on the indicator for EBT Implementation. All models are weighted by the average county population. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. The numbers of observations are 26,268 for total crime and assault, 26,259 for robbery, 26,267 for burglary and larceny, and 26,257 for auto theft.

Table 8: The Effect of EBT Program Implementation on Crime in Missouri – Controlling for SNAP and TANF Caseloads

Crime	EBT	EBT	EBT
Total Crime	0.132**	-0.129***	-0.086**
	(0.055)	(0.042)	(0.034)
Robbery	-0.167**	0.083	0.025
	(0.084)	(0.063)	(0.041)
Assault	0.285***	-0.167***	-0.104**
	(0.085)	(0.054)	(0.044)
Burglary	-0.158***	-0.106**	-0.066
	(0.061)	(0.050)	(0.041)
Larceny	0.109**	-0.138***	-0.091**
	(0.048)	(0.048)	(0.041)
Auto Theft	-0.045	-0.045	0.010
	(0.079)	(0.043)	(0.039)
County Fixed Effects	Yes	Yes	Yes
Month-by-Year Fixed Effects	No	Yes	Yes
County Specific Linear Trends	No	No	Yes

Notes: Robust standard errors clustered at the county level are in parentheses. Each cell presents the coefficient on the indicator for EBT Implementation. All models are weighted by the average county population. *, **, and *** indicate that the estimate is statistically significant at the 0.10, 0.05 and 0.01 levels, respectively. The numbers of observations are 26,268 for total crime and assault, 26,259 for robbery, 26,267 for burglary and larceny, and 26,257 for auto theft.