The Dynamics of Criminal Behavior

Evidence from Weather Shocks

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ABSTRACT

While the persistence of criminal activity is well documented, this may be due to persistence in the unobserved determinants of crime. There are good reasons to believe, however, that there may actually be a negative relationship between crime rates in a particular area due to temporal displacement. We exploit the correlation between weather and crime to examine the short-run dynamics of crime. Using variation in lagged crime rates due to weather shocks, we find that the positive serial correlation is reversed. These findings suggest that the long-run impact of temporary crime-prevention efforts may be smaller than the short-run effects.

I. Introduction

The persistence of criminal activity is well documented. Higher crime today in any particular area is associated with higher crime tomorrow. This serial correlation is illustrated in Figure 1. A 10 percent increase in violent crime in a typical city this week is associated with 1.6 percent more violence the following week, conditional on jurisdiction-year fixed effects. The serial correlation for property

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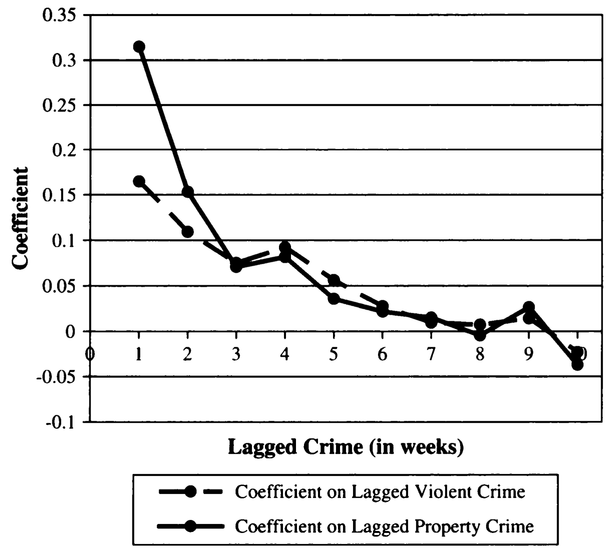

Figure 1
The Persistence of Crime
Note: This figure shows the coefficient estimates from two separate regressions of current crime (violent or property) on ten lags of crime (violent or property). The unit of observation is a jurisdiction-week. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Other covariates include jurisdiction fixed effects. Standard errors in parenthesis are clustered at the state*year*month level. Observations are weighted by the mean number of all crimes within the jurisdiction.

crime is even higher—10 percent more property crime this week is associated with over 3.1 percent more property crime the following week.[1]

One of the most common explanations for this autocorrelation is that potential offenders are influenced by the criminal behavior of others. In other words, a crime committed by one individual increases the likelihood that other individuals in the same locality will engage in criminal activity. In addition to social interactions that occur over a long time period through, for example, social learning, criminologists have long argued that forces such as imitation and revenge may lead to social multiplier or contagion effects that operate over very short time horizons such as days or weeks.[2] However, the persistence in crime rates over time also could be explained by the persistence of unobserved factors that determine the costs and benefits of criminal activity such as police presence and poverty levels. Moreover, there are good reasons to believe that, particularly over a short time horizon, there actually may be a *negative* relationship between crime rates in a particular area due to displacement—in other words, the shifting of criminal activity from one time or location to another. A study

1. Correlation is positive for further lags. For example, a 10 percent increase in violent and property crime this week is associated with 1.1 and 1.6 percent higher crime two weeks later respectively.
2. See Cook and Goss (1996) and Braga et al. (2001).

of juvenile curfews in Detroit, for example, found that afternoon crime nearly doubled after the introduction of the curfew (Hesseling 1994).[3]

Understanding the dynamics of criminal activity is of interest for both practical and theoretical reasons. If the *timing* of criminal behavior is more elastic to temporary changes in the costs of crime than is the total *amount* of criminal activity, then the effects of a police crackdown in a given week may be partially offset by increases in criminal activity in subsequent weeks. From a theoretical perspective, understanding the dynamics of criminal activity is important because it sheds light on the maximizing behavior of criminals. Beginning with Becker (1968), economic models of criminal behavior have generally been constructed and tested using a static framework. While these models have been very useful in understanding some features of criminal behavior, they are not well suited to explaining how criminal behavior changes over time in response to changes in various costs and benefits. The economic literature on crime dynamics is limited.[4]

In this paper, we exploit the correlation between weather and crime to analyze the short-run dynamics of criminal activity. Our aim is to determine the true persistence of criminal activity by estimating the causal relationship between crime rates in different time periods within the same locality. In other words, does more crime today lead to more or less crime tomorrow?

In order to eliminate any spurious serial correlation arising from persistent unobserved heterogeneity, we use an instrumental variable strategy that is based on weather shocks. Criminologists have long recognized that weather is strongly correlated with short-run fluctuations in crime, with hotter weather generally associated with more crime and inclement weather associated with less crime.[5] Drawing on crime-level data from the FBI's National Incident-Based Reporting System (NIBRS), we construct a panel of weekly crime data for 116 jurisdictions from 1995–2001. Simple OLS estimates confirm that violent and property crimes are highly correlated over time within localities. Weeks with above (below) average crime rates are typically followed by weeks with above (below) average crime rates, even after controlling for a rich set of jurisdiction-specific seasonality effects.

However, when we instrument for lagged crime with lagged weather conditions, we actually find the *opposite* result for both property and violent crime. The 2SLS estimates reveal that weeks with above average crime rates are followed by weeks with *below* average crime rates. Notably, our results do not appear to be driven by the persistence in weather conditions over time, or displacement of legal economic activity. Our models control for a series of jurisdiction-specific seasonality measures so that our identification essentially relies on deviations in expected weather patterns that influence crime rates in a particular locale.

The magnitude of the displacement is substantial. A 10 percent increase in violent crime due to a weather shock reduces violent criminal activity by about 2.6 percent

3. Similarly, prior work documents that juvenile violence peaks in the after-school hours on school days and in the evenings on nonschool days (Jacob and Lefgren 2003).
4. One notable exception is Lochner (1999).
5. Several channels are potentially responsible for this correlation. Psychologists have shown that higher temperatures increase aggression directly (Anderson 2001). Alternatively, adverse weather conditions may affect the cost of executing a particular crime and/or the availability and actions of potential victims. We discuss the implications of these various channels in later sections.

in the following week. Moreover, there is evidence that additional displacement occurs over a longer time horizon. The estimated reduction in violent crime over a month is 5.4 percent, more than double the estimated displacement for one week. These findings are consistent with a model in which the marginal utility (cost) of violence is decreasing (rising) in the amount of violence committed during the prior week. This would be true if, for example, an assailant who "settles a score" in one period feels less need to do so in a subsequent period.

The estimates for property crime are similar, though somewhat smaller. A 10 percent increase in property crime reduces property crime by about 2 percent the following week. Interestingly, while displacement of violent crime is apparent across multiple crime categories, displacement of property crimes is concentrated among crimes that involve highly valuable property, like car theft.[6] These results are consistent with a simple model in which transitory fluctuations in the costs of crime create an income effect that is manifested for multiple periods. This dynamic is similar to the one observed in a standard labor supply model with transitory shocks in the wage. Our findings suggest that criminal labor supply responds to transitory fluctuations in the wage in a manner that is similar to that of other self-employed individuals[7] and is also consistent with evidence on the presence of some forward-looking behavior of low-income populations.[8] At the same time, our findings are not consistent with the case of permanent-income offenders who face no liquidity constraints and have very long time horizon.

While we believe that these results provide interesting insight regarding the dynamic optimization behavior of criminals, the relevance of our results for crime-prevention policy depends on the nature of the policy. Like all instrumental variable estimates, our findings reflect a particular local average treatment effect—namely, the impact of an exogenous increase in those crimes that are elastic to weather conditions. To the extent the changes in crime due to weather shocks are similar to changes induced by police interventions; our findings suggest that the long-run impact of short-term police crackdowns may be smaller than the initial effects of these policies.[9]

II. Simple Dynamic Models of Property and Violent Crime

We present two simple models of property and violent crime. In these models, utility-maximizing criminals respond to transitory changes in the price of crime by shifting the time of their criminal activity, inducing a negative relationship between current and future crime. The goal of these models is to clarify the conditions under which temporal displacement may occur.

6. The value of property stolen also displays significant displacement: weeks where the value of stolen property is high are followed by weeks where the value is low.
7. See, for example, the studies of taxi drivers by Farber (2005) and Camerer, Babcock, Lowenstien, and Thaler (1997).
8. See studies of welfare recipients—for example, Grogger and Michalopoulos (2003).
9. Note that we are not suggesting that the effects of such crackdowns "decay" over time, but rather than criminals shift criminal behavior to a later date.

A. Property Crime

Given the financial motivations underlying property crime, a standard labor supply framework provides considerable insight regarding the potential displacement of property crime. Displacement, if it occurs, would plausibly come about through an *income effect*—in other words, a transitory increase in the benefits of crime generates a positive income effect which reduces the incentive to commit crime in subsequent periods. This suggests that displacement will not occur when individuals are unable to borrow and save. During lucrative periods, for example, nothing is saved so the next period's choice of crime is unaffected. Likewise during lean times, offenders are unable to affect available income in subsequent periods by borrowing. This type of period-by-period maximization also would occur if individuals were completely myopic (Case 1). If, on the other extreme, criminals are farsighted and have access to good credit markets, there will also be no displacement because a transitory change in the price of criminal activity will have a negligible effect on lifetime income (Case 2). In order to generate a linkage between lagged and current criminal activity, it is necessary to construct a model that allows individuals to either save or borrow across periods. On the other hand, the credit market must be sufficiently imperfect or the time horizon must be short enough for a transitory shock in the benefits of criminal activity to have a meaningful income effect (Case 3).

We'll now formalize this intuition in the context of a simple model. We assume that an individual's utility each period is defined over consumption, c, and leisure, l, in the following way: $u_t = u(c_t, l_t)$. We will assume that $u(c_t, l_t)$ is increasing in both arguments and strictly concave. Each period a criminal must allocate a single unit of time between leisure and property crime, s. Because this time constraint must hold with equality, it must be the case that $l_t = 1 - s_t$. Each period, an individual earns $w_t s_t$ from criminal endeavors, where w_t is the net wage from criminal endeavors. Note that w_t reflects both the abundance of criminal opportunities and the period specific costs of engaging in criminal activity. In the context of our empirical analysis, fluctuations in the weather generate variation in w_t—perhaps due to weather-related changes in the supply of targets or in the disutility of committing crime outdoors.[10]

Case 1

The first case worth discussing is one in which individuals are unable or unwilling to save or borrow. In this case, the agent faces the following budget constraint: $c_t \leq w_t s_t$ each period. We assume that the criminal maximizes discounted lifetime utility subject to the budget constraints he faces. It is trivial to show that the first-order conditions are equivalent to those obtained from the period-by-period maximization problem. This is because each period's utility and budget constraint is unaffected by that which has gone before or that which will occur later. For this reason, a transitory shock to the benefit (wage) of property crime can have no impact beyond the current period.

10. Similar results could be obtained by assuming that weather affects the utility cost of engaging in criminal activity.

Case 2

Another extreme case is that criminals are farsighted and have access to perfect capital markets. While it is unlikely that most offenders have access to sophisticated capital markets, it still may be useful as a benchmark. Abstracting from uncertainty, each offender chooses a series of consumption and criminal activity to maximize his lifetime utility. The Lagrangian for the offender's optimization problem is:

$$(1) \quad \max_{\{c_0,..\},\{s_0,..\},\lambda} \sum_{t=0}^{T}\left(\frac{1}{1+\rho}\right)^t u(c_t, 1-s_t) + \lambda\left(\sum_{t=0}^{T}\left(\frac{1}{1+r}\right)^t w_t s_t - \sum_{t=0}^{T}\left(\frac{1}{1+r}\right)^t c_t\right).$$

where r is the interest rate at which an offender can borrow or lend, and T is the number of periods and is assumed to be large. In order to understand how a transitory change in the wage of crime affects subsequent criminal behavior, it is helpful to examine the first-order conditions:

$$(2) \quad \left(\frac{1}{1+\rho}\right)^t \frac{\partial u(c_t, 1-s_t)}{\partial c_t} - \lambda\left(\frac{1}{1+r}\right)^t = 0,$$

$$(3) \quad -\left(\frac{1}{1+\rho}\right)^t \frac{\partial u(c_t, 1-s_t)}{\partial l_t} + \lambda\left(\frac{w_t}{1+r}\right)^t = 0$$

together with the budget constraint. Equations 2 and 3 must hold for all t. It is now no longer true that the first-order conditions are equivalent to those consistent with period-by-period optimization. Instead, the ability to borrow and save implies that the marginal utility of lifetime income, λ, is a function of the wages of crime in every period. Thus, while an increase in w_t can induce a substitution effect that causes s_t to rise, the only mechanism through which it can affect s_{t+1} is through λ. This corresponds to an income effect. In dynamic models of labor supply, it is generally assumed that the lifetime income effects of a transitory wage shock are minimal—limiting the temporal displacement of property crime.

Case 3

Consider a model identical to the one above except with $T=2$. For simplicity, assume that the discount rate and the interest rate are both zero, and the utility function is separable in consumption and leisure. Under these assumptions, the first-order conditions are identical to those in Equations 2 and 3. What happens to crime at Time 2 when there is an exogenous shock to the net benefit of committing crime in Period 1? The comparative statics are straightforward:

$$(4) \quad \frac{ds_2}{dw_1} = -\frac{w_2}{\frac{\partial^2 u_2}{\partial l_2^2}} \frac{d\lambda}{dw_1} < 0$$

where $d\lambda/dw_1 < 0$ and the number in the subscript denotes the time period. Equation 4 indicates that an increase in the first-period wage of crime will reduce the amount of criminal activity committed in Period 2. This occurs because a transitory increase in the benefits of crime generates a positive income effect (lowering the marginal utility of wealth), which reduces the incentive to commit crime in subsequent periods. Assuming that the substitution effect dominates the income effect in the first period,[11] a transitory increase in the wage of crime will initially lead to higher levels of crime and will then reduce subsequent criminal activity. In this case, we will observe temporal displacement of property crime.

Even in this third case, it is interesting to question what type of property crime would generate the strong income effects necessary to observe temporal displacement. Clearly, it is unlikely that stealing a small inexpensive item could have any significant income effect. If any displacement occurs, we should observe it only for crimes that involve goods with substantial monetary value. Notably, this is exactly what we observe in the data. We find that car theft is characterized by *total* displacement, while all other property crimes are characterized by virtually *no* displacement.

B. Violent Crime

While the motives underlying violent crime are less often financial, temporal displacement still may occur. In the framework outlined below, temporal displacement may occur for two reasons. First, the benefits of violence may persist over time. This would be true if injuring an individual in the first period "settled a score" or "taught a lesson," reducing the need to do so again in the second period. Second, the costs of violence in one period may depend on the level of violence in the previous period. For example, alcohol and/or drugs are sometimes a cofactor in violent crimes (bar fights, domestic violence, etc.). It is possible that the violence that was precipitated by substance use in the first period may cause the offender to feel guilty, taming alcohol consumption in the second period. Alternatively, a violent act in the first period may result in arrest and/or greater police supervision in the second period.

Consider a simple two-period model. Assume that first-period utility is given by the following:

$$(5) \quad u_1 = g_1(v_1) - \theta_1 v_1,$$

where v_1 is violence in the first period, $g_1(v_1)$ is an increasing but concave function of v_1, and θ_1 is an exogenous per-unit cost of violence. Second-period utility is given by the following:

$$(6) \quad u_2 = g_2(v_2 + \delta v_1) - \theta_2(v_1)v_2,$$

where v_2 is violence in Period 2, δ is the fraction of the benefits of violence that carry over to the next period, $g_2(v_2 + \delta v_1)$ is an increasing and concave function, and $\theta_2(v_1)$ is the per-unit cost of committing violence in the second period.

11. If we thought of the amount of property crime as the dollars generated from criminal activity, $w_t s_t$, the amount criminal activity in Period 1 necessarily increases. In other words, the amount stolen would rise—even if the amount of time spent on criminal activity declined. In this case, a rise in the Period 1 wage of crime would increase Period 1 crime and reduce Period 2 crime. This would lead to the temporal displacement of property crime.

In our model, the per-unit cost of violence in the second period, $\theta_2(v_1)$, is an increasing and convex function of first-period violence. This assumption seems plausible.[12] The criminal's optimization problem involves choosing Periods 1 and 2 violence to maximize utility over the two periods. The first-order conditions of this problem are the following:

$$(7) \quad \frac{\partial U(v_1, v_2)}{\partial v_1} = \frac{dg_1(v_1)}{dv_1} - \theta_1 + \delta \frac{\partial g_2(v_2 + \delta v_1)}{\partial v_2} - \frac{d\theta_2(v_1)}{dv_1} v_2 = 0$$

and

$$(8) \quad \frac{\partial U(v_1, v_2)}{\partial v_2} = \frac{\partial g_2(v_2 + \delta v_1)}{\partial v_2} - \theta_2(v_1) = 0.$$

These conditions define the equilibrium level of crime in the two periods.[13]

What happens to violent crime if the cost of first-period violence exogenously increases (for example, because of a weather shock)? It is not surprising that that an increase in first-period violence results in a decrease in violent crime in the first period: $\frac{dv_1}{d\theta_1} < 0$.[14] The comparative static for second-period crime is more relevant for our analysis. In particular, what happens to violent crime in Period 2 when there is an exogenous shift in the cost of violent crime in Period 1?

$$(9) \quad \frac{dv_2}{d\theta_1} = \frac{-\delta \frac{\partial^2 g_2}{\partial v_2^2} + \frac{d\theta_2}{dv_1}}{\Delta} > 0$$

The denominator of Equation 9 must be positive in order for the second order conditions to hold. Both terms in the numerator are positive suggesting that second-period violence is likely to increase in the first-period cost of violence if either the benefits of violence are durable ($\delta > 0$) or the marginal cost of second-period violence is rising in first-period violence. These findings suggest that the displacement of violent crime can occur under plausible conditions.

III. Empirical Strategy

The previous section suggests that crime in one period may affect crime in the next period either positively or negatively. The basic empirical framework therefore relies upon estimating the following simple equation:

$$(10) \quad crime_{i,t} = \beta_0 + \beta_1 crime_{i,t-1} + \varepsilon_{it},$$

12. Violent acts in the first period may result in arrest or increased police supervision; injuries sustained in the first period could hamper the ability to commit violent crime in the second period; or, for crimes such as domestic violence, it is possible that guilt over a violent act in Period 1 would increase the cost of committing a similar act in Period 2.

13. Note that these conditions take into account that $\frac{g_2(v_2 + \delta v_1)}{\partial v_1} = \delta \frac{\partial g_2(v_2 + \delta v_1)}{\partial v_2}$.

14. In particular, $\frac{dv_1}{d\theta_1} = \frac{\frac{\partial^2 g_2}{\partial v_2^2}}{\Delta}$ where Δ is the determinant of the matrix of second order conditions and must be positive in order for the second order conditions to hold.

where $crime_{i,t}$ reflects the level of crime in jurisdiction i in period t and β_1 reflects the causal effect of an exogenous increase in criminal activity on the next period's crime. Following the models presented in Section II, β_1 is the net effect of social interactions and displacement. The empirical challenge is that $crime_{i,t-1}$ is almost certainly positively correlated to the error term ε_{it}, since factors affecting the costs and benefits of crime are likely persistent over time.

The ideal instrumental variable is correlated with criminal activity in period t-1 but uncorrelated to the error term. One candidate instrument is the weather. The correlation between criminal activity and weather conditions has been well documented.[15] It has been hypothesized that higher temperatures might increase aggression directly (see Anderson 2001), thus providing a transitory increase in the net benefit of criminal activity. Adverse weather conditions may affect the cost of executing a particular crime, due to changes in the ease of transportation or the likelihood of witnesses to the crime, which may influence the chance of arrest.

In this model, the structural equation is given by:

$$(11) \quad crime_{it} = \mathrm{B}X_{it} + \beta_1 crime_{i,t-1} + \beta_2 weather_{it} + \varepsilon_{it}$$

where i indexes jurisdictions, and t indexes time period (in our analysis, a week), $weather_{it}$ is a vector of weather variables, and the vector X_{it} includes jurisdiction *year fixed effects, jurisdiction-specific fourth order polynomials in day-of-year and fixed effects for each month. The first stage is given by:

$$(12) \quad crime_{i,t-1} = \Gamma X_{it} + \gamma_1 weather_{i,t-1} + \gamma_2 weather_{it} + \eta_{it}.$$ [16]

To increase the efficiency of our estimates, we weight each observation by the average number of (violent or property) crimes committed each week within the jurisdiction.[17] The standard errors are cluster corrected at the state*year*month level as weather and criminal activity may be spatially and temporally correlated. In some models we include more than one lag in crime. When Equation 11 includes crime at t-1, t-2, t-3, and t-4, we instrument t-2 crime using t-2 weather conditions, t-3 crime using t-3 weather conditions, and so forth.

The key identifying assumption in this model is that, conditional on weather at time t and other covariates, weather at time t-1 cannot directly influence crime at time t. More formally, $\mathrm{cov}(\varepsilon_{it}, weather_{i,t-1} | weather_{it}, X_{it}) = 0$. In assessing this assumption, it is important to highlight several potential issues. A first concern is that weather, when measured at high frequency, is serially correlated. Therefore, in the

15. See Cohn's (1990) extensive literature review on the subject. More recent work on the issue by Rotton and Cohn (2000) and Field (1992) confirms the finding.

16. One might be concerned that this instrumenting strategy does not work because the first stage seems inconsistent with the recursive structure of the data-generating process. However, it is trivial to show that, if the exclusion restriction holds, our strategy yields an unbiased estimate of β_1.

17. To understand why we do this, suppose that criminal activity is generated by a Poisson process, the mean and variance or crime with a jurisdiction both equal $\bar{c}_i$. We discuss below that we normalize crime rates by the mean weekly incidence over the sample—doing so yields point estimates that can be interpreted as percentage effects. In this case, the variance of the normalized crime measure is $1/\bar{c}_i$. By weighting each jurisdiction by $\bar{c}_i$, which is the approximate inverse of the variance of its residual, we increase the precision of our estimates.

absence of good controls for current weather, lagged weather may be directly correlated with current crime because it will contain information regarding current weather. While we do control for current weather conditions in our models, it is possible that weather within a period is measured imperfectly, in which case lagged weather still may provide information regarding the unobserved aspects of current weather. For example, one day's maximum temperature likely provides information regarding the weather conditions at 1 a.m. of the next day. The combination of serial correlation in weather, along with imperfect measures of weather conditions in any one period, will violate the assumptions necessary for satisfactory identification and result in a *positive* bias in the coefficient on lagged crime. We can show that this bias decreases as the length of the time window expands. (For a formal proof, see Appendix A in Jacob, Lefgren, and Moretti 2004.) Additionally, we later show the results of a "reverse experiment" in which we estimate the effect of *future* crime on current crime, and instrument for future crime with future weather. The instrument coefficient on future crime is insignificant, suggesting that this type of bias in insignificant.

A second concern is that weather may lead to the displacement of noncriminal economic activity. To the extent that criminal activity is follows noncriminal economic activity, apparent displacement of criminal activity may not generalize to settings in which noncriminal activity remains constant. To assess whether empirically this is an important source of bias, we examine three pieces of evidence, all of which are discussed in detail in Section VI. First, we provide evidence that some noncriminal activities—namely, traffic patterns—are not significantly displaced by weather conditions. Second, we compare the impact of lagged crime on indoor and outdoor crimes separately. Third, we explore whether the relationship between the victim and offender of violent crime is associated with the magnitude of displacement. Taken together, these three pieces of evidence indicate that our finding of temporal displacement is unlikely to be driven only by noncriminal economic activity.

Finally, in interpreting our estimates it is important to realize that our strategy yields a particular local average treatment effect, LATE (Imbens and Angrist 1994). The estimates presented here reflect the impact of an exogenous increase in those crimes that are elastic to weather conditions. We will provide evidence in Section VI that weather affects a broad range of criminal behaviors. It is possible, however, that the set of criminals whose behavior is affected by the weather is different from the set of criminals who vary their behavior in response to transitory law enforcement activity. Thus, our findings might not fully generalize to other contexts.

IV. Data

Crime data are from the FBI's National Incident Based Reporting System (NIBRS), which was established as a voluntary crime reporting system in January 1989. The unit of observation in NIBRS is each criminal incident reported to police. NIBRS is organized into a system of files that gives information regarding type of offense, time, date, and location of the incident, characteristics of the victims and offenders, as well as the value and amount of any stolen property or illegal drugs.

The massive detail of the database allows us to aggregate data into categories and time periods of our choosing. Unfortunately, many of the largest jurisdictions do not to participate in NIBRS. The 2001 data file contains information on over three million incidents reported in 3,611 different jurisdictions including Austin, Texas, and Cincinnati, Ohio. Most jurisdictions in the sample, however, are very small with more than 70 percent reporting fewer than 500 crimes for the whole year. Earlier sample years contain information on even fewer jurisdictions.

Because the most severe crimes occur infrequently in the jurisdictions that we observe, we focus our analysis on broad aggregates such as violent and property crime. Violent crimes include simple and aggravated assault, intimidation, homicide, manslaughter, and sex crimes. Property crimes include extortion, counterfeiting, fraud, larceny, vehicle theft, robbery, and stolen property offenses.[18] Data on weather are from the National Climatic Data Center (NCDC). They contain daily readings of minimum and maximum temperature, and inches of precipitation for 24,833 weather stations in the United States. We average these weather measures across stations within each county to construct a county-level panel of daily weather conditions.

As discussed in Section IV, we aggregate the daily data for both crime and weather to the weekly level in order to reduce any bias due to the autocorrelation of weather.[19] Our primary analysis sample includes 116 jurisdictions for the period 1995–2001 with a total of 26,338 jurisdiction-week observations. In order to maximize the statistical power of our estimates, we have chosen the largest jurisdictions for which NIBRS data is available. While NIBRS is by no means a nationally representative sample of police jurisdictions in the United States, and the largest jurisdictions in the country tend not to participate, our sample does include a relatively diverse set of cities and counties. (For a complete list of the jurisdictions included in our sample, see Table 6.)[20]

Because some jurisdictions are larger than others, it is helpful to normalize the crime rate across jurisdictions to reduce heteroskedasticity. This specification also allows weather to have the same *percentage* effect on the crime rate—regardless of jurisdiction size. Researchers often address these concerns by using the natural log of the crime rate. This specification is not well suited for the current paper because, when examining specific types of crimes, it is sometimes the case that no

18. Robbery is included as a property crime because the underlying motive is financial. Vandalism is excluded for the same reason.

19. In some jurisdictions, multiple weeks in a single month contain no criminal activity due to misreporting by the police agency. In these cases, we restrict the sample in following manner. We drop jurisdiction-month observations in which the monthly crime rate is more than twice the jurisdiction-specific interquartile range below the median. We use the monthly crime rate because in some jurisdictions, it might be the case that no crime occurred over several days or weeks. By using a longer time period, we are more confident that we are excluding observations with gross underreporting. We choose to restrict the sample on the basis of median and interquartile range because these measures are less sensitive to periodic massive underreporting than are the mean and standard deviation. However, if monthly crime is normally distributed, our exclusion restriction corresponds to months in which criminal activity is more than 2.67 standard deviations below the mean.

20. For example, the jurisdictions in our sample span 17 states, with 5 percent of jurisdictions from the Northeast, 32 percent from the Midwest, 63 percent from the South, and 18 percent from the West. The five jurisdictions in our sample with the largest number of crimes reported are Chattanooga, Tenn.; Cincinnati, Oh. Austin, Tex; Nashville, Tenn.; and Memphis, Tenn. The jurisdictions with the five smallest number of crime reported are Rock Hill, S.C.; Iowa City, Ia.; Burlington, Vt.; Riley County, Kan.; and Pocatello, Id.

crimes of a particular type occur during the course of a given week. Our large sample size and massive number of covariates complicate the use of count models. Thus, in our primary specifications our measure of criminal activity is the number of crimes committed during the week divided by the average weekly incidence in the jurisdiction during the sample period.

In our sample, the mean weekly temperature in our sample is 58 degrees Fahrenheit. In the average week, there is only 0.11 inches of daily precipitation. In the 18 percent of weeks that experience some snowfall, there is an average of 0.40 daily inches of snowfall. Note that a number of jurisdictions appeared to be somewhat inconsistent in reporting snowfall. For this reason, we include only measures of temperature and precipitation in our preferred specifications, though our estimates are robust to the inclusion and use of snowfall.

V. Empirical Findings

A. Graphical Relationship between Lagged and Current Crime

Figures 2a and 2b illustrate the baseline serial correlation of crime in our data. To do so, we regress the violent or property crime rate in Period t on ten lags of crime within

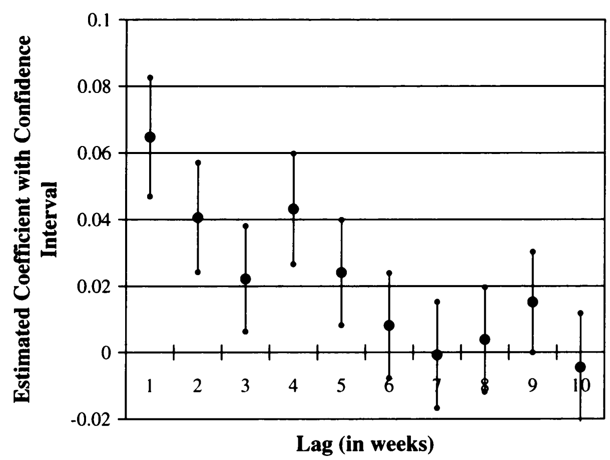

Figure 2a

The Estimated Relationship between Current and Lagged Violent Crime

Notes: Each panel figure shows the coefficient estimates from a regression of current violent crime on 10 lags of violent crime. The unit of observation is a jurisdiction-week. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Other covariates include average temperature and total precipitation in the current period; jurisdiction-year fixed effects, jurisdiction-specific fourth order polynomials in day-of-year, and fixed effects for month. Standard errors in parenthesis are clustered at the state*year *month level. Observations are weighted by the mean number of all crimes within the jurisdiction.

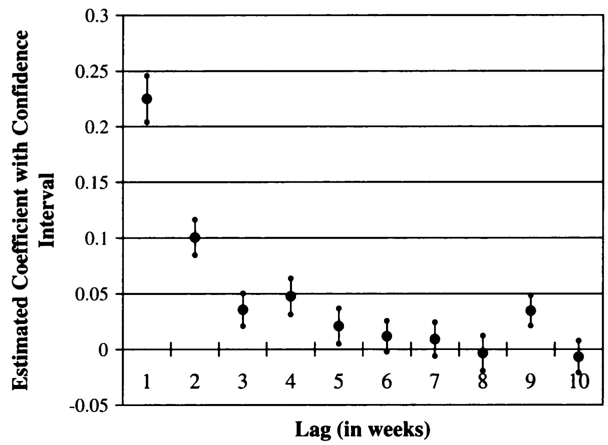

Figure 2b
The Estimated Relationship between Current and Lagged Property Crime
Notes: Each panel figure shows the coefficient estimates from a regression of current property crime on 10 lags of property crime. The unit of observation is a jurisdiction-week. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Other covariates include average temperature and total precipitation in the current period; jurisdiction-year fixed effects, jurisdiction-specific fourth order polynomials in day-of-year, and fixed effects for month. Standard errors in parenthesis are clustered at the state*year*month level. Observations are weighted by the mean number of all crimes within the jurisdiction.

the same jurisdiction, controlling for the same set of covariates that will be used in our primary estimation—namely, jurisdiction*year fixed effects, jurisdiction-specific fourth order polynomials in day of year as a smooth control for seasonality, month fixed effects, and average temperature and total precipitation in Period *t* as controls for current weather conditions. Standard errors are clustered by state-year-month to account for spatial and temporal correlation. The figures present the coefficients on all lagged crime variables. Figures 2a and 2b differ from Figure 1 in that it includes a detailed set of controls.[21]

The top panel shows that violence during each of the past five weeks is a statistically significant predictor of violence in the current period. The coefficients on the lags start at 0.065 and generally fall with distance from the reference week. By Week 6, the coefficients are statistically insignificant. In the bottom panel, we see that the

21. On average, about 93 violent crimes are reported each week in our jurisdictions; 56 of these are simple assault (for example, violence not involving a weapon or serious injury), and 17 are aggravated assault. In about 70 percent of cases, the victim knows the offender—indeed about 23 percent of all violence is between family members. Property crime is more common than violent crime with nearly 240 reported incidents per week. About 60 percent of these incidents involve larceny of some type. Other types of property crime are far less common. All of these means are calculated weighting each jurisdiction by the average number of weekly crimes in the jurisdiction.

serial correlation for property crime is even higher than that for violent crime (for example, the coefficient on the first lag is 0.22), but the autocorrelation appears to decay in a similar way. While these conditional correlations are statistically significant, it is interesting to note that they are smaller than the unconditional correlations presented in Figure 1. This suggests that factors associated with jurisdiction-specific time and jurisdiction-specific seasonality effects (for example, factors such as seasonal changes in the economy, or police interventions) have a strong influence on crime rates.

B. The Effect of Weather on Crime

Table 1 examines the relationship between weather and violent as well as property crime using the baseline set of controls described above. The dependent variable here is the number of incidents in a jurisdiction-week divided by the average number of weekly incidents in that jurisdiction over the entire sample period, so that the coefficients on the explanatory variables can be interpreted roughly as a percent change in the outcome. Looking first at Column 1, we see that weather—particularly temperature—is strongly correlated with violent crime. A ten degree increase in the average weekly temperature is correlated with about a 5 percent increase in violent criminal activity. Precipitation, on the other hand, is associated with reductions in criminal activity. An increase in average weekly precipitation of one inch is associated with a 10 percent reduction in violence. These effects are highly statistically significant—the F-statistic of joint significance is over 200.[22]

We see similar patterns for property crime, although weather appears to be less predictive of property than violent crime. As the average weekly temperature rises by 10 degrees, property crimes fall by about 3 percent. The coefficient on precipitation is not statistically significant. The F-statistic of joint significance for all weather variables is 51.

Columns 2–10 show the effect of temperature and precipitation on a variety of different types of crimes. This not only provides additional insight regarding the overall weather-crime relationship but, perhaps more importantly, allows one to better interpret the local average treatment effect of the estimates presented below. The results indicate that the effect of weather is quite consistent across all types of violent crime. Weather has a similar effect on domestic violence and violence against strangers; across crimes of varying levels of seriousness; regardless of whether a weapon was used; and for violent crimes involving juvenile as well as adult offenders. The results for various types of property crimes are roughly similar—higher temperature is always associated with more property crime—although the effect of precipitation varies more for property than violent crime.[23]

22. There is a convex relationship between temperature and crime—very hot temperatures result in a more than proportional increase in violence—but a concave relationship between precipitation and violence. (See Table 3 in Jacob, Lefgren, and Moretti 2004.)

23. Higher precipitation appears to be associated with increases in the incidence of burglary. We speculate that precipitation may reduce the probability of detection, perhaps because there are fewer people around to serve as potential witnesses or there is reduced visibility.

Table 1
OLS Estimates of the Relationship between Weather and Crime

	Panel A: Violent Crime									
	All Violent Crime (1)	Simple Assault (2)	Aggravated Assault (3)	Violent Crime by Family Member (4)	Violent Crime by Known Individual (5)	Violent Crime by Stranger (6)	Violent Crime with Weapon (7)	Violent Crime without Weapon (8)	Violent Crime with Gun (9)	Offender is a juvenile (10)
Temperature/100	0.478**	0.494**	0.633**	0.313**	0.434**	0.649**	0.569**	0.449**	0.416**	0.541**
	(0.028)	(0.032)	(0.062)	(0.050)	(0.032)	(0.054)	(0.061)	(0.033)	(0.180)	(0.071)
Precipitation	−0.097**	−0.089**	−0.171**	−0.046**	−0.094**	−0.109**	−0.141**	−0.090**	−0.120**	−0.157**
	(0.009)	(0.011)	(0.020)	(0.016)	(0.010)	(0.017)	(0.018)	(0.010)	(0.051)	(0.025)
F-statistic	203.5	148.01	85.47	25.85	138.00	96.84	80.72	137.86	5.26	46.1
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
R squared	0.47	0.40	0.20	0.29	0.45	0.26	0.27	0.42	0.05	0.223

	Panel B: Property Crime						
	All Property	Larceny	Shoplifting	Burglary	Vehicle Theft	Robbery	Property Value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Temperature/100	0.292**	0.313**	0.049	0.386**	0.190**	0.499**	0.161**
	(0.024)	(0.027)	(0.054)	(0.044)	(0.053)	(0.095)	(0.042)

(*continued*)

Table 1 *(continued)*

	Panel B: Property Crime						
	All Property	Larceny	Shoplifting	Burglary	Vehicle Theft	Robbery	Property Value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Precipitation	−0.008	−0.020**	0.035*	0.034**	0.007	0.033	0.002
	(0.008)	(0.010)	(0.019)	(0.014)	(0.023)	(0.031)	(0.014)
F-statistic	51.24	49.50	5.00	26.61	4.26	7.89	7.44
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
R squared	0.593	0.545	0.261	0.335	0.241	0.097	0.313

Notes: The unit of observation is a jurisdiction-week and the number of observations is 26,567. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. All models include jurisdiction-year fixed effects, jurisdiction-specific fourth order polynomials in day-of-year, and fixed effects for month. Standard errors are in parentheses. *P*-values are contained in brackets. Standard errors are clustered at the state*year*month level to take into account the correlation across jurisdictions within a state and within a jurisdiction over time. All weather variables are weekly averages. Precipitation is measured in inches. Property value indicates the total monetary value (in dollars) of stolen property in that jurisdiction-week. Observations are weighted by the mean number of all crimes within the jurisdiction. * statistically significant at the 10 percent level; ** statistically significant at the 5 percent level.

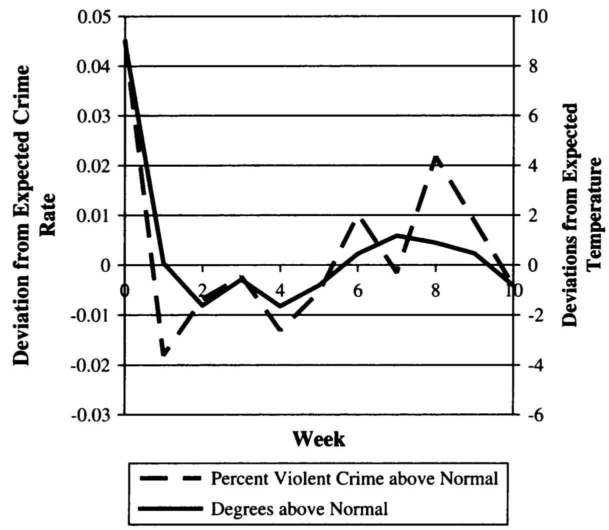

Figure 3a
Heat Waves and Violent Crime

Notes: For each panel, we regressed temperature on month fixed effects, jurisdiction*year fixed effects and jurisdiction-specific fourth order polynomials in day of year. We included periods in our sample in which the temperature in week 0 was more than 6 degrees Fahrenheit above predicted (using our controls) and the temperature in Week 1 was within 3 degrees of predicted. For each week, we show the average temperature residual—weighting each jurisdiction by the average number of crimes committed during a week. We also show the average violent crime residual, which is obtained by running a regressions with controls discussed above.

C. The Effect of Heat Waves on Crime

Here we illustrate the intuition behind our instrumental variable strategy using an example of extreme weather conditions—heat waves. We identify a set of unusually hot weeks that were followed by relatively normal weather.[24] If displacement occurs, we should see relatively high crime during the hot week and relatively low crime during the subsequent weeks.

Figure 3a shows the results for violent crime. The solid line shows average temperature during the hot week (Time 0) along with the temperature during the subsequent ten weeks. The dashed line shows how the average deviation of the violent crime rate from the predicted rate. In Week 0, both temperature *and* violent crime are higher than expected. Indeed, the violent crime rate is 4.5 percentage points higher than normal. The following week, temperature is close to its predicted value.

24. We define unusually hot weeks as those in which the average weekly temperature is 6 degrees Fahrenheit warmer than predicted after controlling for jurisdiction-year fixed effects and jurisdiction-specific fourth order polynomials in day of year. To be included in our sample, the temperature of the following week must be within three degrees of the predicted temperature. There are 1,108 such weeks in our sample.

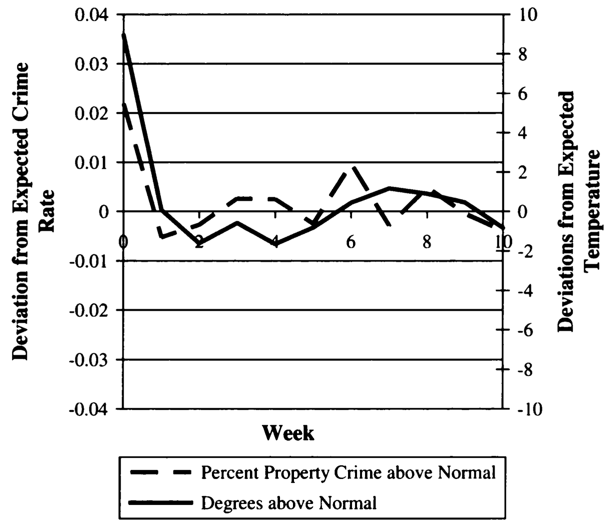

Figure 3b
Heat Waves and Property Crime
Notes: For each panel, we regressed temperature on month fixed effects, jurisdiction*year fixed effects and jurisdiction-specific fourth order polynomials in day of year. We included periods in our sample in which the temperature in week 0 was more than 6 degrees Fahrenheit above predicted (using our controls) and the temperature in Week 1 was within three degrees of predicted. For each week, we show the average temperature residual—weighting each jurisdiction by the average number of crimes committed during a week. We also show the average property crime residual, which is obtained by running a regressions with controls discussed above.

Violent crime, however, is nearly 2 percentage points *lower* than normal, suggesting displacement of about 40 percent over the subsequent week. Over the next ten weeks, both temperature and the rate of violent crime bounce around the predicted level, though violent crime is unusually high eight weeks after the hot week. Figure 3b shows the analogous results for property crime. During the initial hot week, property crime is more than 2 percentage points higher than expected. The following week it is about 0.5 percentage points lower. This is again consistent with displacement, though the drop in Week 1 is smaller than for violent crime. In the subsequent weeks, we again see a small amount of variation around the predicted crime rate.

D. IV Estimates of the Impact of Lagged Crime on Current Criminal Activity

The analysis of heat waves—an extreme weather shock—casts doubt on the *positive* serial correlation of crime that one observes in the raw data. Indeed, these results indicate that, over a short-time horizon, exogenous increases in crime may be followed by *decreases* in crime—suggesting temporal displacement in criminal activity. To more formally examine this for the full data sample, Tables 2 and 3 present OLS and IV estimates of the relationship between lagged and current crime. By way of reminder, the first and second stage specifications are given by Equations 11 and 12 respectively. As explained above, all models include jurisdiction*year fixed effects, month effects and the jurisdiction-specific fourth-order polynomials in day-of-year to control for seasonality. In order to account for the persistence of weather over time, they also control for *current* weather conditions including the weekly average of daily mean temperature, inches of precipitation. Our instruments are lagged average temperature and total precipitation.[25] To take into account that the error terms are not independent across jurisdictions or over time, we cluster the standard errors at the state*year*month level.[26]

Looking first at the violent crime results in Table 2, the OLS estimates show the strong positive correlation documented in Figures 2a and 2b. However, when we instrument for lagged violent crime using lagged weather conditions, the results are actually *reversed*. The IV estimate in Column 2, for example, indicates that a 10 percent increase in criminal activity in one week is associated with a 2.6 percent *decrease* the following week. Note that the first stage F-statistic is 213, indicating that our instruments are quite strong (which is also reflected in the precision of our estimates). In Columns 4, 6, and 8, we see that the effects of more distant lags are generally smaller than the first lag, though most estimates remain statistically significant and negative. The sum of the lags provides a measure of the total displacement over an extended period of time. For example, in Column 8, the sum of the four lags is -0.536, indicating that a 10 percent increase in violent crime during a particular week is associated with a reduction of roughly 5.4 percent over a one-month period—roughly double our estimate for the one-week period. Note that the magnitude of the implied displacement is quite large. For example, these results suggest that the actual impact of a violent crime-prevention program is *less than half* the magnitude of its contemporaneous impact. Note that these results are statistically significantly different from zero as well as one (which would indicate complete displacement).

The results for property crime in Table 3 reveal a similar pattern. In stark contrast to the positive correlations documented in OLS, the IV estimates suggest that lagged crime has a statistically significant *negative* effect on crime in the current period. The estimate in Column 2, for example, indicates that a 10 percent increase in property crime in one week will lead to a 2.0 percent decline in property crime the following week. Note that, like in violent crime, the IV estimates are not only statistically significantly different than zero, but also statistically significantly different from the OLS estimates. In all models, the sum of the lags is negative and statistically

25. We do not use snowfall due to concerns about the consistency of data collection. Our estimates are robust to the inclusion of snowfall measures.

26. In theory, one limitation of this type of clustering is that we allow for arbitrary autocorrelation between two weeks in the same month, state, and year, but not between two weeks in different months, even if they are consecutive weeks. We assume that this is not a major problem in our context. We have experimented with different clustering, including ones that include shorter and longer periods, and found our standard errors to be robust.

Table 2
OLS and IV Estimates of the Relationship between Current and Lagged Violent Crime

	Dependent Variable: Violent Crime in Period t							
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Crime t-1	0.083**	−0.260**	0.077**	−0.209**	0.072**	−0.215**	0.068**	−0.221**
	(0.010)	(0.054)	(0.009)	(0.050)	(0.009)	(0.049)	(0.008)	(0.050)
Crime t-2	—	—	0.052**	−0.172**	0.047**	−0.159**	0.043**	−0.159**
			(0.008)	(0.052)	(0.008)	(0.045)	(0.008)	(0.047)
Crime t-3	—	—	—	—	0.032**	−0.070	0.026**	−0.049
					(0.008)	(0.052)	(0.008)	(0.047)
Crime t-4	—	—	—	—	—	—	0.049**	−0.105**
							(0.008)	(0.054)

Sum of coefficients	0.083**	−0.260**	0.129**	−0.381**	0.151**	−0.444**	0.186**	−0.536**
	(0.010)	(0.053)	(0.014)	(0.068)	(0.018)	(0.086)	(0.010)	(0.128)
F-Statistic—first stage	—	213.58	—	112.5-113.0	—	81.1-78.3	—	59.9-66.0
[*p*-value]		[0.00]		[0.00-0.00]		[0.00-0.00]		[0.00-0.00]
Observations	26,338	26,338	25,929	25,929	25,893	25,893	25,853	25,853
Period *t* weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction*year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction-specific fourth order polynomial in day of year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The unit of observation is a jurisdiction-week. The number of observations varies depending on the number of lags included. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Standard errors are in parenthesis. Standard errors are clustered at the state*year*month level to take into account the correlation across jurisdictions within a state and within a jurisdiction over time. *P*-values for the *F*-statistics are shown in brackets. For models with multiple instruments, minimum and maximum *F*-statistic and *p*-value are shown. Observations are weighted by the mean number of all crimes within the jurisdiction. * statistically significant at the 10 percent level; ** statistically significant at the 5 percent level.

Table 3
OLS and IV Estimates of the Relationship between Current and Lagged Property Crime

	Dependent Variable: Property Crime in Period t							
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Crime t-1	0.279**	−0.201**	0.241**	−0.171**	0.233**	−0.176**	0.229**	−0.172**
	(0.012)	(0.083)	(0.010)	(0.080)	(0.010)	(0.084)	(0.010)	(0.084)
Crime t-2	—	—	0.127**	−0.136*	0.112**	−0.138*	0.104**	−0.149*
			(0.008)	(0.090)	(0.008)	(0.083)	(0.007)	(0.084)
Crime t-3	—	—	—	—	0.054**	−0.011	0.040**	−0.016
					(0.008)	(0.083)	(0.007)	(0.084)
Crime t-4	—	—	—	—	—	—	0.056**	−0.010
							(0.008)	(0.096)
Sum of coefficients	0.279**	−0.201**	0.368**	−0.307**	0.399**	−0.325**	0.429**	−0.326*
	(0.012)	(0.083)	(0.014)	(0.116)	(0.015)	(0.137)	(0.016)	(0.204)

F-statistic - first stage	—	64.06	—	33.0–34.5	—	21.9–23.6	—	15.7–18.4
[p-value]		[0.00]		[0.00–0.00]		[0.00–0.00]		[0.00–0.00]
Observations	26,338	26,338	25,929	25,929	25,893	25,893	25,853	25,853
Period t weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction*year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction-specific fourth order polynomial in day of year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The unit of observation is a jurisdiction-week. The number of observations varies depending on the number of lags included. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Standard errors are in parenthesis. Standard errors are clustered at the state*year*month level to take into account the correlation across jurisdictions within a state and within a jurisdiction over time. P-values for the F-statistics are shown in brackets. For models with multiple instruments, minimum and maximum F-statistic and p-value are shown. Observations are weighted by the mean number of all crimes within the jurisdiction. * statistically significant at the 10 percent level; ** statistically significant at the 5 percent level.

significantly different than both zero and one. The effects are somewhat smaller for property than violent crime, but are still substantial. In Column 8, the sum of the four lagged crime measures is −0.33, which implies that for property crime, the displacement that occurs over one month is roughly 50 percent more than that which occurs over one week.

To this point, we have examined up to four lags. However, it is possible that displacement could operate over an even longer time horizon. To examine this possibility, we calculate IV estimate for both violent and property crime in which we include up to ten lags and present the results in Figures 4a and 4b. Note that these results are directly comparable to the OLS estimates shown in Figures 2a and 2b. The top panel shows the results for violent crime. As we saw earlier, the first, second, and fourth lags are statistically significantly different from zero. The others hover around zero but are not consistently of one sign or another. At −0.37, the sum of the ten lags is negative and statistically significant at the 10 percent level, suggesting that all of the displacement in violent crime occurs within a month. The bottom panel shows the results for property crime. In contrast to the results we found earlier, none of the lags is statistically different from zero. Furthermore, the sum of lags is only −0.14 and statistically insignificant. Though the point estimates suggest that little displacement occurs over 10 weeks for property crime, the standard error on the sum of coefficients is too large to rule out substantial displacement. For both violent and

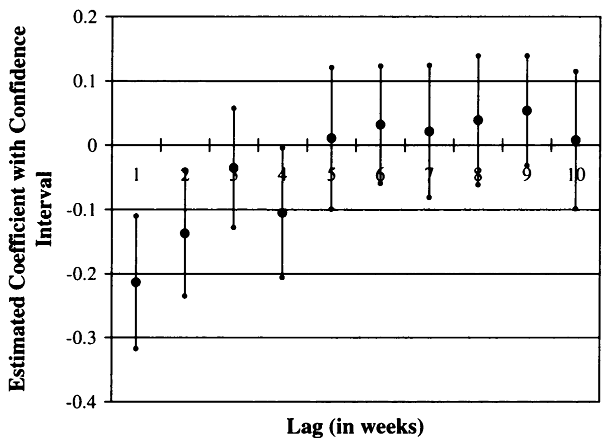

Figure 4a

IV Estimates of the Relationship between Current and Lagged Violent Crime

Notes: Panels show the IV estimates from a regression of current violent crime on 10 lags. The unit of observation is a jurisdiction-week. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Other covariates include average temperature and total precipitation in the current period; jurisdiction-year fixed effects, jurisdiction-specific fourth order polynomials in day-of-year, and fixed effects for month. Lagged crime is instrumented using lagged average temperature and total precipitation. See the text for more details. Standard errors in parenthesis are clustered at the state*year*month level. Observations are weighted by the mean number of all crimes within the jurisdiction.

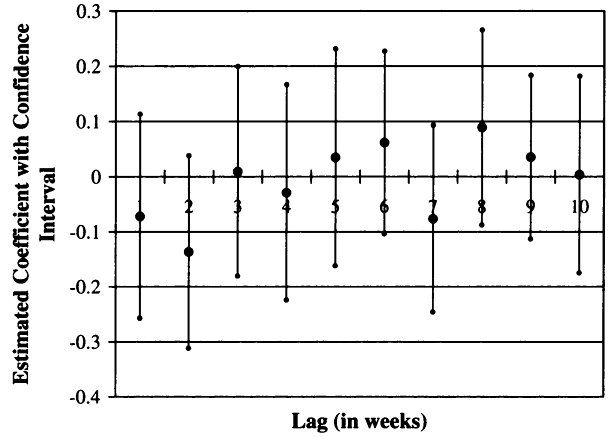

Figure 4b

IV Estimates of the Relationship between Current and Lagged Property Crime

Notes: Panels show the IV estimates from a regression of current property crime on 10 lags. The unit of observation is a jurisdiction-week. All crime rates are computed by dividing the actual number of crimes during a week by the average number of weekly crimes within the jurisdiction. Other covariates include average temperature and total precipitation in the current period, jurisdiction-year fixed effects, jurisdiction-specific fourth order polynomials in day-of-year, and fixed effects for month. Lagged crime is instrumented using lagged average temperature and total precipitation. See the text for more details. Standard errors in parenthesis are clustered at the state*year*month level. Observations are weighted by the mean number of all crimes within the jurisdiction.

property crime, however, the results do stand in stark contrast to the positive OLS estimates.

E. Estimates by Specific Crime Categories

It is of great interest to establish how our results vary by type of offense. This is complicated, however, by the fact that temporal displacement may occur across different types of crime. For example, an aggravated assault that is prevented by adverse weather conditions may result in a simple assault in a later period.[27] For this reason, a regression of simple assault on lagged simple assault may violate the assumptions necessary for valid instrumental variables identification. In particular, the instruments (lagged temperature and precipitation) may influence simple assault in the current period not only through its effect on lagged simple assault, but also through its effect on lagged aggravated assault or some other type of related crime in the prior period. To overcome this issue, when examining the effect of lagged crime on particular types of crime, we estimate models in which the left-hand-side variable is the

27. In practice, the difference between simple and aggravated assault can be fairly small. Indeed, relatively minor differences in the seriousness of injury can lead to a different categorization.

rate of the specific type of crime under examination while the right-hand-side variable is a lagged measure of *all* violent or property crime.[28]

Table 4 shows the findings for violent crime. The results suggest that an exogenous increase in violent crime leads to subsequent reductions in most violent crime categories. In particular, a 10 percent increase in *all* violent crime reduces simple and aggravated assaults by 3.5 and 2.9 percent respectively. Similarly, a 10 percent increase in all violent crime reduces violent crime against family members and individuals known to the offender by nearly 3.0 percent over one week. We see similar effects for crimes with and without weapons. The IV estimate for violent crime by strangers is not statistically significant, though the point estimate suggests some displacement.[29] In all cases, when examining a four-lag model, we observe substantial displacement for all types of violent crime.[30] While our specification allows us to examine whether or not displacement occurs for particular crimes, it is not possible to compare the magnitudes of the coefficients across crime types. Still, these models allow us to conclude that temporal displacement of violence appears to operate for a variety of different types of violent crime.

In Table 5, we examine the findings for different types of property crime. Remember that the simple labor supply framework outlined in Section II predicts that displacement should only operate for property crimes involving a fairly large monetary value, since the predicted income effect of stealing small items (for example, a candy bar) is likely trivial. Consistent with that prediction, the point estimates for the IV results suggest substantial displacement only for burglary and vehicle theft, although only the effects for vehicle theft are statistically significant. Interestingly, the 2SLS estimates for vehicle theft for four lags show enormous displacement. Because many property crimes—particularly those in the categories of larceny, shoplifting or robbery—involve relatively small amounts of money, it is not surprising that we find little effect for these crimes.

To more accurately capture property crime displacement, we estimate a model in which we measure property crime by the *total value* of the property stolen during a particular period.[31] Interestingly, the results in Column 7 indicate statistically significant displacement over a one-week period. A 10 percent increase in the value of property stolen in one week is associated with a decline in value of property stolen in the following week by nearly 6 percent. The results for the four-lag model are not precise enough to be informative. Overall, these results for property crime suggest some displacement over a short time period, although the results are not as robust as for violent crime.

28. Note that these models assume that there is no displacement from violent to property crime, or vice versa. While this is probably not strictly true, it is likely that the magnitude of this type of displacement is second-order. Estimates from models where the right-hand-side is the specific crime under consideration are available upon request. In general, they are qualitatively similar to the ones shown here.

29. We expect gang violence might be disproportionately concentrated in the category of violent crime by strangers. To the extent that one expects the violent crime displacement to reflect gang activity, the insignificance of the displacement result for violence committed by strangers is a bit surprising. This could reflect, however, the imprecision of the point estimates.

30. These estimates are computed from separate regressions in which we include one or four lags.

31. As with the other models, we normalize by diving by the average for the jurisdiction over the entire sample period. We obtain comparable results if we use the log of the total value of stolen property.

Table 4
OLS and IV Estimates of the Impact of All Lagged Violent Crime on Specific Types of Violent Crime

	Dependent Variable: Crime in Period t								
	All Violent Crime (1)	Simple Assault (2)	Aggravated Assault (3)	Violent Crime By Family Member (4)	Violent Crime By Known Individual (5)	Violent Crime by Stranger (6)	Violent Crime with Weapon (7)	Violent Crime without Weapon (8)	Violent Crime with Gun (9)
One lag									
OLS	0.083**	0.084**	0.064**	0.075**	0.080**	0.090**	0.036**	0.094**	0.021
	(0.010)	(0.012)	(0.018)	(0.016)	(0.011)	(0.015)	(0.017)	(0.010)	(0.050)
	—	—	—	—	—	—	—	—	—
IV	0.260**	0.354**	0.286**	0.265**	0.280**	0.120	0.268**	0.272**	0.185
	(0.054)	(0.063)	(0.118)	(0.091)	(0.063)	(0.104)	(0.107)	(0.066)	(0.362)
Sum of four lags									
OLS	0.186**	0.200**	0.140**	0.199**	0.180**	0.219**	0.112**	0.209**	0.111
	(0.010)	(0.022)	(0.032)	(0.030)	(0.022)	(0.029)	(0.033)	(0.021)	(0.079)
	—	—	—	—	—	—	—	—	—
IV	0.536**	0.649**	0.567**	0.381**	0.498**	0.545**	0.543**	0.506**	-0.447
	(0.128)	(0.151)	(0.224)	(0.182)	(0.140)	(0.212)	(0.244)	(0.165)	(0.600)
Period t weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction*year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(*continued*)

Table 4 *(continued)*

	Dependent Variable: Crime in Period t								
	All Violent Crime	Simple Assault	Aggravated Assault	Violent Crime By Family Member	Violent Crime By Known Individual	Violent Crime by Stranger	Violent Crime with Weapon	Violent Crime without Weapon	Violent Crime with Gun
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Month effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction-specific fourth order polynomial in day of year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Specifications in which the lagged crime rate is specific to the type of violent crime committed are inappropriate given that displacement of one type of violent crime might be manifested subsequently by a violent crime of a different type. Thus for all specifications, the lagged variable is the rate of all violent crimes. The dependent variable is number of crimes divided by the average number of those types of crimes in the jurisdiction. For the one-lag models, the parentheses contain standard errors that errors are clustered at the state*year*month level to take into account the correlation across jurisdictions within a state and within a jurisdiction over time. For the four-lag models, the parentheses contain the standard error of the sum of the coefficients on all four lags. Observations are weighted by the mean number of all crimes within the jurisdiction. * statistically significant at the 10 percent level; ** statistically significant at the 5 percent level.

Table 5
OLS and IV Estimates of the Impact of Lagged Crime on Specific Types of Property Crime

	Dependent Variable: Crime in Period *t*						Dependent Variable
	All Property (1)	Larceny (2)	Shoplifting (3)	Burglary (4)	Vehicle Theft (5)	Robbery (6)	The total value of all stolen property in period *t* (7)
One lag							
OLS	0.279**	0.284**	0.176**	0.316**	0.257**	0.290**	0.170**
	(0.012)	(0.012)	(0.022)	(0.028)	(0.022)	(0.035)	(0.030)
IV	−0.201**	−0.064	0.179	−0.209	−0.599**	−0.009	−0.582**
	(0.083)	(0.093)	(0.192)	(0.160)	(0.213)	(0.367)	(0.265)
Sum of four lags							
OLS	0.429**	0.438**	0.336**	0.519**	0.416**	0.372**	0.343**
	(0.016)	(0.018)	(0.035)	(0.042)	(0.035)	(0.052)	(0.06)
IV	−0.326*	−0.168	−0.207	−0.064	−1.091**	−0.007	−3.99
	(0.204)	(0.201)	(0.352)	(0.313)	(0.481)	(0.642)	(3.311)
Period *t* weather	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction*year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(*Continued*)

Table 5 *(continued)*

	Dependent Variable: Crime in Period t						Dependent Variable
	All Property (1)	Larceny (2)	Shoplifting (3)	Burglary (4)	Vehicle Theft (5)	Robbery (6)	The total value of all stolen property in period t (7)
Month effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction-specific fourth order polynomial in day of year	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Specifications in which the lagged crime rate is specific to the type of property crime committed are inappropriate given that displacement of one type of property crime might be manifested subsequently by a property crime of a different type. Thus, for all specifications, the lagged variable is the rate of all property crimes. The dependent variable is number of crimes divided by the average number of those types of crimes in the jurisdiction. For the one-lag models, the parentheses contain standard errors that errors are clustered at the state*year*month level to take into account the correlation across jurisdictions within a state and within a jurisdiction over time. For the four-lag models, the parentheses contain the standard error of the sum of the coefficients on all four lags. Observations are weighted by the mean number of all crimes within the jurisdiction. * statistically significant at the 10 percent level; ** statistically significant at the 5 percent level.

VI. Robustness Checks

A. Temporal Displacement of Noncriminal Economic Activity

The main identifying assumption in our empirical strategy is that lagged weather conditions only influence current period crime through their influence on lagged crime. While high frequency variation in weather is unlikely to be correlated with many of the unobserved factors that determine the persistence of crime over time (for example, income levels, crime-prevention policies, etc.), weather may affect the intensity of noncriminal activity which, in turn, could influence the cost and/or benefit of crime. If inclement weather causes people to stay home in one period, for example, it may result in greater than expected economic activity in the following period, which could increase the benefits of crime (by increasing the availability of victims, for example). More generally, if weather displaces noncriminal activity and this activity influences the cost/benefit of criminal activity, our estimates may be biased.

To assess the empirical importance of this concern, we first provide evidence regarding the extent to which noncriminal activity is displaced by weather conditions. Though few measures of economic activity are reported at a sufficiently high frequency to examine this issue, we have collected information from the Federal Highway Administration (FHWA) that includes daily measures of traffic from in-road monitors in over 20,000 locations throughout the United States for 2000–2001. We regard traffic as a good summary measure for noncriminal economic activity.[32] We aggregate this data to the county-week level, so that our primary outcome measure is the number of vehicles counted in a particular county during a given week.[33]

Using data on weekly vehicle traffic for states represented in our sample, we find that weather conditions have only a small correlation with vehicle traffic. This is extremely informative itself because if weather does not have a strong impact on this type of economic activity over a one-week period, it is unlikely that weather would induce substantial amounts of displacement to bias our estimates. Table 7 shows that when we instrument lagged traffic with lagged weather conditions, we find no evidence of temporal displacement over one or four weeks, though the standard errors are in some cases quite large.[34] This is also inconsistent with widespread temporal displacement of economic activity.

32. The traffic volume data includes hourly traffic counts for each traffic station provided by permanent in-road traffic monitors. Geographic identifiers allow one to link each station to states and counties. Traffic data are available for 66 of the 92 counties included in the crime analysis. Recall that our primary analysis sample for crime includes 116 jurisdictions in 92 counties in 17 states. For more information on the traffic data, see: Office of Highway Policy Information at http://www.fhwa.dot.gov/policy/ohpi.

33. To be consistent with the crime estimates, we normalize these measures by dividing weekly counts by the average for that county over the sample period.

34. These results are reassuring, although ideally, one would like to look at a broader set of measures of economic activity to draw more general conclusions. A recent paper looks at the effect of weather on retail sales (Starr-McCluer 2000). The author finds some evidence of displacement. Unusually cold temperatures in a given month tend to depress sales in that month, but they lead to higher sales in the following month. The current and lagged effects do not completely offset each other. Moreover, unusually warm weather is a given month increases sales, but reduces sales in the month after. The current and lagged effects roughly offset each other. Unfortunately, data on retail sales are available only at the monthly level, which makes the comparison with our analysis difficult.

Table 6
Jurisdictions Included in the Analysis

Jurisdiction	Years in Sample	Jurisdiction	Years in Sample	Jurisdiction	Years in Sample
Adams County, CO	1997–2001	Grand Forks, ND	1995–2001	Paducah, KY	1998–2001
Aiken County, SC	1995–2001	Greenville Cnty, SC	1995–2001	Petersburg, VA	1995–2001
Akron, OH	1998–2001	Greenville, SC	1995–2001	Pocatello, ID	1995–2001
Albemarle County, VA	1997–2001	Greenwood, SC	1995–2001	Pontiac, MI	1997–2001
Alexandria, VA	2000–2001	Hamilton Cnty, OH	2000–2001	Portsmouth, VA	2000–2001
Anderson County, SC	1995–2001	Hampton, VA	2000–2001	Provo, UT	1995–2001
Arapahoe County, CO	1997–2001	Henrico County, VA	1999–2001	Redford, MI	1997–2001
Aurora, CO	1997–2001	Horry, SC	1995–2001	Richland County, SC	1995–2001
Austin, TX	1998–2001	Huntington, WV	2000–2001	Richmond, VA	2000–2001
Battle Creek, MI	1995–2001	Hutchinson, KS	2000–2001	Riley Cnty, KS	2000–2001
Beaufort County, SC	1995–2001	Idaho Falls, ID	1995–2001	Roanoke, VA	1999–2001
Berkeley County, SC	1995–2001	Iowa City, IA	1995–2001	Rock Hill, SC	1995–2001
Boise, ID	1995–2001	Jackson, TN	1999–2001	Roseville, MI	1996–2001
Burlington, VT	1999–2001	Jefferson County, CO	1997–2001	Saginaw, MI	2000–2001
Cedar Rapids, IA	1999–2001	Johnson City, TN	1998–2001	Salina, KS	2000–2001
Charleston County, SC	1995–2001	Junction City, KS	2000–2001	San Angelo, TX	2000–2001
Charleston, SC	1995–2001	Kalamazoo, MI	2000–2001	Sandy, UT	1995–2001
Charleston, WV	1999–2001	Kingsport, TN	1998–2001	Sioux City, IA	1995–2001

Charlottesville, VA	1997–2001	Knox County, TN	1998–2001	Southfield, MI	1996–2001
Chattanooga, TN	2000–2001	Knoxville, TN	2000–2001	Spartanburg Cnt, SC	1995–2001
Cherokee County, SC	1995–2001	Lakewood, CO	1997–2001	Spartanburg, SC	1995–2001
Chesterfield Cnty, VA	1999–2001	Layton, UT	1995–2001	Spotsylvania Cnt, VA	1999–2001
Cincinnati, OH	1998–2001	Lexington Cnty, SC	1995–2001	Springfield, MA	1996–2001
Clarksville, TN	1998–2001	Loudoun County, VA	1999–2001	Stafford County, VA	1997–2001
Cleveland, TN	1999–2001	Lynchburg, VA	2000–2001	Suffolk, VA	1997–2001
Coeur D'Alene, ID	1995–2001	Memphis, TN	2000–2001	Sumter, SC	1995–2001
Colorado Springs, CO	1997–2001	Murfreesboro, TN	1998–2001	Twin Falls, ID	1995–2001
Columbia, SC	1995–2001	Murray, UT	1997–2001	Virginia Beach, VA	1999–2001
Columbia, TN	1998–2001	Muskegon, MI	2000–2001	Warren, MI	1999–2001
Conroe, TX	1998–2001	Myrtle Beach, SC	1995–2001	Waterford, MI	2000–2001
Council Bluffs, IA	1995–2001	Nampa, ID	1995–2001	Waterloo, IA	1995–2001
Danville, VA	2000–2001	Nashville, TN	2000–2001	West Jordan, UT	1995–2001
Davenport, IA	1995–2001	Newark, OH	1998–2001	West Valley, UT	1996–2001
Dayton, OH	1998–2001	Newport News, VA	1998–2001	Worcester, MA	1995–2001
Des Moines, IA	1995–2001	Norfolk, VA	1999–2001	Wyoming, MI	1999–2001
Fairfax County, VA	2000–2001	North Charleston, SC	1995–2001	York County, SC	1995–2001
Fargo, ND	1995–2001	Norwalk, CT	1999–2001		
Florence County, SC	1995–2001	Oakland County, MI	1997–2001		
Florence, SC	1995–2001	Olathe, KS	2000–2001		
Garden City, KS	2000–2001	Orangeburg Cnty, SC	1995–2001		

Table 7
OLS and IV Estimates of the Relationship between Current and Lagged Traffic

	Dependent Variable: Traffic Volume in Period t							
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Traffic t-1	0.633**	0.040	0.618**	0.163	0.811**	−0.094	0.753**	0.092
	(0.024)	(0.219)	(0.030)	(0.214)	(0.009)	(0.172)	(0.016)	(0.207)
Traffic t-2	—	—	−0.091**	−0.045	—	—	−0.029*	−0.068
			(0.029)	(0.207)			(0.016)	(0.220)
Traffic t-3	—	—	0.025	−0.001	—	—	0.041**	−0.204
			(0.031)	(0.163)			(0.017)	(0.199)
Traffic t-4	—	—	0.031	−0.120	—	—	0.070**	−0.254
			(0.026)	(0.220)			(0.014)	(0.322)
Sum of coefficients	0.633**	0.040	0.583**	-0.002	0.811**	-0.094	0.833**	−0.435
	(0.024)	(0.219)	(0.034)	(0.450)	(0.009)	(0.172)	(0.010)	(0.789)
F-statistic from first-stage	—	9.35	—	—	—	18.65	—	—
regression [p-value]	[0.00]	[0.00]						
Observations	4,959	4,959	4,272	4,272	42,893	42,893	37,076	37,076

Notes: Standard errors in parenthesis. Standard errors are clustered at the state*year*month level to take into account the correlation across jurisdictions within a state and within a jurisdiction over time. In Columns 1 to 4, controls include period t weather controls, jurisdiction*year fixed effects, month fixed effects, and jurisdiction-specific fourth order polynomial in week of year. In Columns 5 to 8 controls include period t weather controls, jurisdiction fixed effects, year fixed effects, month fixed effects and state-specific fourth order polynomial in day-of-year. The sample in Columns 1 to 4 includes all 66 of the 92 counties in the crime sample for which traffic data are available. The sample in Columns 5 to 8 includes all 570 counties in the 17 states included in the crime analysis that also have traffic data. * significant at the 10 percent level; ** significant at the 5 percent level.

As a second piece of suggestive evidence on temporal displacement in economic activity, we estimate separate models for indoor and outdoor crime. If weather induces displacement in noncriminal activity, we would expect to see more (less) outdoor activity after a bad (good) spell of weather. If crime follows economic activity, we also should expect more outdoor crime following bad weather (and vice versa) but *less* indoor crime. This suggests that our displacement results would be focused on outdoor as opposed to indoor crime. For violent crime, we find that an exogenous increase in violent crime leads to statistically significant reductions in both outdoor and indoor violence over a four-week period, although the one-week effect is only statistically significant for indoor crime. The property crime estimates are uniformly negative and similar in magnitude for indoor and outdoor crime, but only significantly different from zero in one of four cases (see Table 10 in Jacob, Lefgren, and Moretti 2004).

A third suggestive piece of evidence regarding the potential bias from the displacement of economic activity can be obtained by examining the relationship between offenders and victims for violent crime. It seems plausible that interactions with family members would be least sensitive to the degree of noncriminal economic activity. In Table 7, we showed that an exogenous increase in violent crime leads to statistically significant reductions in the amount of violence against family members during the following week. Although by no means definitive, taken together these results suggest that our finding of temporal displacement is not driven by displacement of noncriminal economic activity.

B. Endogenous Police Staffing Levels

A second concern is that police may departments may change staffing levels in response to weather conditions or small fluctuations in the crime rate. For example, police may know that on hot days there is more crime and thus increase the number of working officers on those days. Such behavior amplifies or attenuates the correlation between weather and crime but ultimately acts simply as another cost factor. Thus the consistency of our IV estimates is unaffected by such behavior.

A more serious concern would be if current staffing levels were correlated to lagged weather conditions. This would violate the exclusion restriction necessary for satisfactory identification. Such a situation might exist if police departments adjusted staffing levels going forward in response to small fluctuations in crime rates associated with weather conditions. To reduce concerns on this front, we contacted several police departments in our sample. The responding agencies indicated that they did not adjust staffing levels in response to nonemergency weather conditions. Furthermore, changes in staffing levels were proactive—not reactive. In other words, departments might increase staffing levels in response to a parade but *not* in response to a temporary and unexpected increase in number of reported offenses. These responses suggest that the endogeneity of police staffing levels is unlikely to be a meaningful source of bias.

C. Persistence in Weather

A third potential problem is that weather, when measured at high frequency, is serially correlated. While we control for current weather conditions in our models, it is possible that weather within a period is measured imperfectly, in which case lagged weather still may provide information regarding the unobserved aspects of current weather which would, in turn, directly impact current crime. As discussed earlier, this will violate the assumptions necessary for satisfactory identification and result in a *positive* bias in the coefficient on lagged crime. It is possible to show that this bias decreases as the length of the time window expands. For this reason, we aggregate to the weekly level.

To examine whether this source of bias is empirically unimportant in our current framework, we conduct a "reverse experiment" where we estimate the effect of *future* crime on current crime, and instrument for future crime with future weather. Specifically, we estimate a specification in which the dependent variable is crime in Period t, but the right-hand-side variable is crime in Period $t+1$ and the instrument is weather conditions in Period $t+1$. If measurement error in weather conditions is an important source of bias, the bias would be similar whether we look at lagged or future crime. Thus, we would expect the estimated impact of *future* crime on current crime to be positive and statistically significant. This is not the case (see Jacob, Lefgren, and Moretti 2004). In particular, the estimated impact of future crime is small in magnitude and statistically insignificant for both violent and property crime. This suggests that our results are not materially affected by imperfect weather measures.

D. Other Robustness Checks

We have experimented with a variety of alternative specifications. For a full list, see Tables 10 and 11 in Jacob, Lefgren, and Moretti (2004). We find that our results are robust to the exclusion of observations with violent crime rates more than twice the interquartile range above the median within the jurisdiction. This suggests that our findings are unaffected by periodic over-reporting of criminal activity. We also find that our results are comparable if we measure crime using the log number of violent or property crimes rather than scaling by the average number of crimes in the jurisdiction. Results using only temperature as an instrument are nearly identical to our baseline specification. When we limit our instruments to precipitation alone, we have very little statistical power, particularly in the case of property crime, although the four-lag violent crime results are comparable to baseline.

In our primary specification, we implicitly assume symmetry between the effect of an exogenous reduction in criminal activity and an exogenous increase in criminal activity. However, this need not be the case. For example, individuals prevented from committing a criminal act may do so in a subsequent period while individuals who engage in opportunistic crimes due to favorable weather conditions may not reduce their criminal activity during the following week. To examine the impact of positive weather shocks, we construct a variable that is the average temperature during a week if the temperature was above the average in the state during the month and zero otherwise. We use this as our only instrument in the first stage. To examine the impact of negative shocks, we do the analogous analysis using variation in temperature below the average. Over a one-week period, the displacement results appear to be larger for positive shocks than negative shocks. Over four weeks, however, the displacement effects appear roughly consistent across positive and negative shocks.

VII. Conclusion

In this paper, we exploit the correlation between weather and crime to examine the short-run dynamics of criminal activity. In sharp contrast to the positive serial correlation in crime rates reported in most studies, we find that higher crime in one week is followed by *less* crime in subsequent weeks. These findings suggest that the positive serial correlation in crime commonly reported is not an endogenous process driven by the optimization of offenders, but likely reflects persistence of unobserved factors that influence of criminal activity.

The magnitude of displacement in violent and property crime over a short-time horizon is substantial. A 10 percent increase in violent crime in one week leads to a 2.6 percent reduction in violent crime the following week. Over the course of four weeks, over half of the initial increase in violence will be mitigated through displacement. The results for property crime are somewhat smaller—a 10 percent increase in property crime results in a decrease of 2.0 percent the following week—and appear to be limited to high value property crimes, vehicle theft in particular.

The property crime results suggest that criminal behavior is consistent with a standard labor supply framework, where the observed displacement can be explained by an income effect. Criminals who are prevented from committing property offenses in a given week engage in higher levels of criminal activity during the subsequent weeks to make up for lost income. This behavior is consistent with a model in which offenders have at least some foresight and/or are some liquidity constraints. Consistent with the predictions of the model, we find that displacement arises *only* for crimes that involve highly valuable property, such as vehicle theft.

In the case of violent crime, a possible interpretation of the results is that the benefits of violence may be durable—in other words, the marginal utility (cost) of violence is decreasing (rising) in the amount of violence committed during the prior week. This would be true if, for example, an assailant who "settles a score" in one period feels less need to do so in a subsequent period. Similarly, a husband who abuses his wife in one period may be less inclined to do so in the next period, perhaps because of a sense of guilt or because he has received a warning from the police.[35]

Besides its theoretical implications, the uncovering of substantial temporal displacement also may have important policy implications. Our findings suggest that the long-run impact of temporary crime-prevention efforts may be smaller than the short-run effects. In the case of violent crime, the short-run impact of a one-week

35. Incarceration may in theory be a factor behind the displacement of property and violent crime. However, given the low arrest and conviction rates, it is unlikely that incarceration could explain a substantial portion of the observed displacement. Consider, for example, that each incident of violence reported produces only about 0.4 arrests. It is likely that at least half of these are either not charged with an offense or released almost immediately on bail (DiIulio 1996). Furthermore, even if the remaining individuals are involved in a reported act of violence 10 times per year, incarceration could only generate displacement of about 4 percent over one week, a small fraction of the total effect. This is even truer for property crime, where clearance rates are much lower. A related possibility is that the cost being caught is an increasing and convex function of the number of crimes committed. Because we are looking at week-to-week variation in crime rates, we suspect that in our context, this is not empirically very relevant. For most crimes, it is unlikely that offenders are caught, arrested, tried, sentenced, and freed in one week.

crime-prevention effort will be twice as large as the impact over one month. To the extent that policy makers engage in short-term policy experiments or anti-crime crackdowns, standard policy evaluations may prove misleading.

It should be noted, however, that the estimates presented here reflect a particular local average treatment effect—namely, the impact of lagged crime that is elastic to weather conditions. The relevance of our results for policy depends upon the extent to which the set of criminals and crimes whose behavior is affected by the weather is similar to the sets which respond to transitory law enforcement activity. Although we show that weather affects a broad range of criminal behaviors, it is important to recognize that our findings might not fully generalize to other contexts, particularly those that involve longer-term interventions such as improving economic opportunities for potential offenders or short-run interventions that involve the incapacitation of many potential offenders. It is also important to note that changes in weather conditions have no lasting impact on the perceived costs of criminal activity. This differs from extended police crackdowns, in which offenders may only gradually realize that the program has ended. The effects of such programs have been shown in some cases to extend even beyond the duration of the crackdown (see Sherman 1990). To the extent that police engaged in shorter and more predictable crackdowns, our findings would be more relevant.

Finally, it is useful to consider how these results inform the literature on social interactions. To the extent that social interactions operate at the week-to-week frequency we examine, our estimates should be interpreted as the net effect of these social interactions and displacement. In this case, our results suggest that the total temporal displacement more than offsets social interactions operating at the weekly level. Given the relatively high frequency of our data, our estimates cannot speak to the possibility of social interactions that operate in the long run through, for example, social learning. Thus, these findings do not rule out the importance of social interactions for explaining long-run differences in crime rates across localities.

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