model goes a step further and removes district-level confounding by using district fixed effects while restricting our interest to questions 3 and 4.

The second challenge, mentioned earlier, is that the conventional methodology for answering causal questions relies on the no-interference assumption. In Rubin's causal framework (e.g., Neyman, 1923; Rubin, 1978, 1980, 1986; Holland, 1986; Little and Rubin, 2000), the assumption ensures that, given two treatments under consideration, each unit in the population has only two potential outcomes: one outcome would be observed if a unit is assigned to the first treatment; the other outcome would be observed if a unit is assigned to the second treatment.

The no-interference assumption is violated in various disciplines, including criminology, education, epidemiology, and others. However, in the context of the multitude of studies examining causal effects, the literature reveals that only a few studies have tried to address the violation of this assumption. We will now briefly review this literature. Heckman, Lochner, and Taber (1998) considered the effects of changes in tuition policy on schooling and earnings. In this setting, the no-interference assumption is violated as a decrease in tuition may result in a substantial increase in the number of individuals attending college, thus reducing the relative earnings of college graduates. The authors used the general equilibrium theory to compute causal estimates.

Hong and Raudenbush (2006) considered the effect of kindergarten retention on student achievement, where the achievement of a given student depends on whether any student in his or her classroom was retained. Following the work of Verbitsky and Raudenbush (2004), the authors specified causal models in which the treatment assignment of all children in a school affects each child's potential outcome.

Halloran and colleagues (e.g., Halloran and Struchiner, 1995; Hudgens and Halloran, 2008) considered infectious diseases. They noted that a person's likelihood of infection often depends on who else is infected, which in turn depends on the vaccination status of other individuals in the person's local area or social network. Halloran and Struchiner (1995) defined causal effects by conditioning on the exposure to infection. Hudgens and Halloran (2008) took a more formal approach and defined causal effects under the assumption of "partial interference"; in other words, they assumed that it is possible to define mutually exclusive groups such that there is no interference between individuals in different groups even though interference may occur between members of the same group.

Sobel (2006) considered the Moving to Opportunity program, which randomly assigned eligible residents of disadvantaged neighborhoods to receive various forms of relocation assistance. In this setting, however, a study participant's decision to take advantage of the assistance and the related outcomes

may depend on whether the participant's friends or relatives were also assigned to receive assistance. Sobel defined the potential outcome for an individual as a function of the individual's treatment assignment and all individuals in the population. He demonstrated that, in the presence of interference between units, the standard way of estimating causal effects under the no-interference assumption in an experimental setting—the difference between the treatment and control means—would estimate the difference between two causal parameters: (1) the average effect of receiving a voucher on those who received it and (2) the "spillover" effect (or the effect of other residents relocating) on the individuals who did not receive a voucher. In fact, both effects could be negative (i.e., detrimental) while their difference (estimated by the difference between the treatment and control means) could be positive, leading to an erroneous conclusion about program impacts. Sobel noted that a researcher should carefully consider the issue of interference and then design a study that avoids it, e.g., a cluster-randomized study with no interference between clusters. Causal inference under interference has continued to develop with the infectious disease literature (Tchetgen Tchetgen, and VanderWeele, 2010; Halloran and Hudgens, 2011; VanderWeele and Tchetgen Tchetgen, 2011a, 2011b) and in econometrics (Graham, 2008; Manski, forthcoming).

Avoiding interference or reclaiming the no-interference assumption is not always possible or preferable. In fact, spillover effects may be of substantive policy interest. Building on our previous work (Hong and Raudenbush, 2006; Verbitsky and Raudenbush, 2004), we take a different approach that allows us to examine and estimate causal effects in the spatial setting by relaxing the no-interference assumption and redefining potential outcomes in order to model interference explicitly.

2 Causal Inference in Spatial Settings

2.1 Redefining Potential Outcomes and Causal Effects

To extend Rubin's causal framework, let Z_i denote the treatment assignment of beat i from the population of interest U, and let vector $\mathbf{Z}_{-i}$ denote the treatment assignment of the other beats in the population. We define potential outcomes for beat i as a function of the treatment assignments of all beats in the population, i.e., $Y_i(Z_i, \mathbf{Z}_{-i})$. A causal effect for beat i may still be defined as a difference between two potential outcomes, i.e., $\delta_i = Y_i(z_i, \mathbf{z}_{-i}) - Y_i(z_i^*, \mathbf{z}_{-i}^*)$, where z_i^* and $\mathbf{z}_{-i}^*$ are alternative treatment assignments of beat i and all the other beats in the population, respectively. The population-average causal effect is defined as the expected difference between two potential outcomes over the population of interest U, i.e., $\delta = \mathrm{E}[\delta_i]$.

Now let us examine our questions of interest. The first question that policymakers, such as a mayor or police chief, may ask when considering community policing is whether implementing community policing everywhere versus nowhere in the city decreases the crime rate. Let 1 denote community policing and 0 denote no community policing. Under the current policy, no beat is assigned to community policing (Figure 4, right panel). Under the alternative policy, every beat is assigned to community policing (Figure 4, left panel). Thus, for any beat i in the population, the "all or nothing" effect may be expressed as $\delta_i^{(1)} = Y_i(1, \mathbf{1}) - Y_i(0, \mathbf{0})$.

As for the second question, neighborhood leaders may ask whether implementing community policing in their beat, namely i, will decrease the crime rate there. Currently, beat i (Figure 5, right panel, central beat) is surrounded by two beats with community policing and three without community policing. Then, beat i's potential outcome if the current policy is continued is $Y_i(0, \mathbf{z}_{-i})$, where $\mathbf{z}_{-i} = [1, 0, 0, 1, 0]$ is a vector corresponding to the current treatment assignment of the surrounding beats. The potential outcome for beat i under the alternative policy, i.e., if it is assigned to community policing while everyone else's treatment assignment does not change (Figure 5, left panel), is $Y_i(1, \mathbf{z}_{-i})$. Thus, the "local treatment assignment" effect for beat i is $\delta_i^{(2)} = Y_i(1, \mathbf{z}_{-i}) - Y_i(0, \mathbf{z}_{-i})$.

In the case of the third question, a police chief—interested in implementing community policing in certain parts of the city but not in others because of budget constraints—asks about the effect on the focal area's crime rate if community policing is implemented in surrounding areas. Let us first consider a focal neighborhood i without community policing. Currently, none of its neighbors has community policing (Figure 6, right panel). Under the alternative policy, for example, two of its five neighbors have community policing (Figure 6, left panel). Then, the "neighbor" effect for this control beat could be expressed as $\delta_i^{(3a)} = Y_i(0, \mathbf{z}_{-i}) - Y_i(0, \mathbf{0})$, where $\mathbf{z}_{-i} = [1, 0, 0, 1, 0]$ is a vector indicating the treatment assignment of beat i's neighbors under the alternative policy. We can also define a "neighbor" effect for focal neighborhood i with community policing, i.e., $\delta_i^{(3b)} = Y_i(1, \mathbf{z}_{-i}) - Y_i(1, \mathbf{0})$, where $\mathbf{z}_{-i} = [1, 0, 0, 1, 0]$ is a vector indicating the treatment assignments of the other beats under the alternative treatment (Figure 7). Finally, the police chief wants to know if the "neighbor" effect for a control beat is different from that for a CAPS beat, i.e., "interaction" effect ($\delta_i^{(4)} = \delta_i^{(3b)} - \delta_i^{(3a)}$).

Figure 4. "All or Nothing" Effect: $\delta_i^{(1)} = Y_i(1, \mathbf{1}) - Y_i(0, \mathbf{0})$
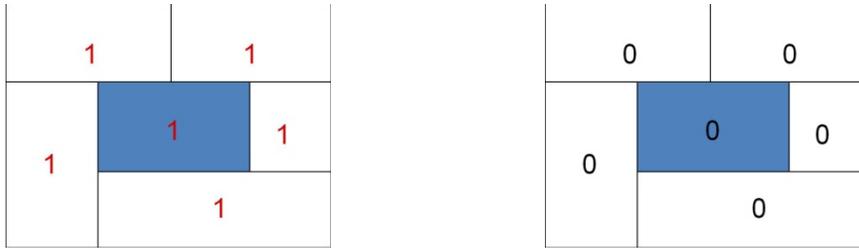

Figure 5. "Local Treatment Assignment" Effect: $\delta_i^{(2)} = Y_i(1, \mathbf{z}_{-i}) - Y_i(0, \mathbf{z}_{-i})$
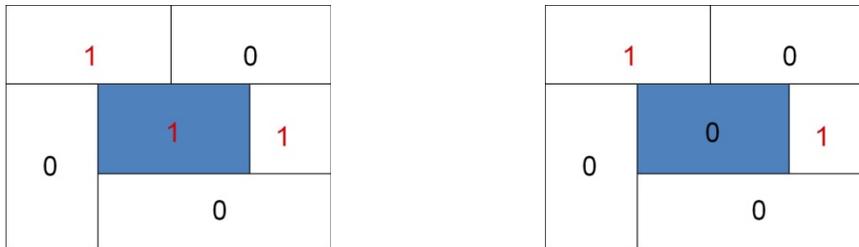

Figure 6. "Neighbor" Effect on a Control Beat: $\delta_i^{(3a)} = Y_i(0, \mathbf{z}_{-i}) - Y_i(0, \mathbf{0})$
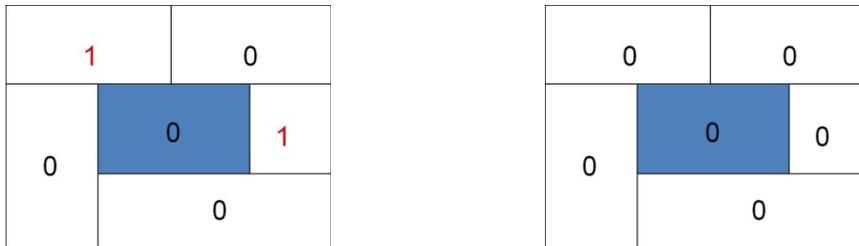

Figure 7. "Neighbor" Effect on a CAPS Beat: $\delta_i^{(3b)} = Y_i(1, \mathbf{z}_{-i}) - Y_i(1, \mathbf{0})$

NOTE: In all examples, beat *i* refers to the central beat in the picture and $\mathbf{z}_{-i} = [1, 0, 0, 1, 0]$.

In Rubin's causal framework, it is sufficient to observe a sample of beats from population U. In this framework, however, due to the second dimension of the treatment—i.e., the treatment assignment of neighbors—a researcher may need to observe all beats in the population to some extent. For example, data on the beats' treatment assignments and relative locations may need to be observed for all beats in the population, but the covariates and the outcomes may need to be observed for only a sample. Such an approach may be especially important in observational studies, where beats self-select into (or are non-randomly assigned to) the treatments and treatment choice for a given beat may depend on the treatment of the neighbors. However, in an experimental evaluation, where beats are randomly assigned to treatments, it may be sufficient to observe only the study sample on all dimensions.

Rubin's causal framework may be incorporated into the new framework. If no interference is assumed, then $Y_i(Z_i, \mathbf{Z}_{-i}) = Y_i(Z_i)$ for all $i \in U$. Thus, based on the no-interference assumption, the "all or nothing" effect would be assumed to equal to the "local treatment assignment" effect. Moreover, the "neighbor" effect would be undefined as under both policies the focal neighborhood is assigned to the same treatment. However, if the no-interference assumption holds, the two frameworks are essentially equivalent.

2.2 Spatial Causal Assumptions

In the new definition for potential outcomes that allows for interference, beat i's potential outcome depends on the treatment assignment of other beats in the population. Hence, the number of potential outcomes per beat increases exponentially as the number of beats in the population increases (Little and Rubin, 2000). For example, if a finite population U has N beats with two alternative treatments under consideration, then each beat may have up to 2^N potential outcomes. In practice, some assumptions must be made in order to estimate the causal effects of interest.

Functional Form Assumption

The first assumption replaces and relaxes the no-interference assumption from Rubin's causal framework by taking advantage of the spatial nature of the setting. We assume that the potential outcome for beat i depends on the treatment assignment of beat i and some many-to-one function, F, of treatment assignments of i's neighbors, i.e., $Y_i(Z_i, \mathbf{Z}_{-i}) = Y_i(Z_i, F(\mathbf{Z}_{-i}))$.

In our case study, F is a proportion of contiguous beats assigned to treatment. This functional form implicitly assumes that (1) each contiguous beat's assignment to community policing has the same effect on the focal beat's

potential outcome; (2) treatment assignments of the non-contiguous beats do not affect the potential outcome of the focal beat; and (3) the other aspects of spatial relationship between the beats—for example, the number of contiguous beats or the percentage of the focal beat's boundary that has no contiguous neighbors but borders Lake Michigan—do not affect the potential outcome of the focal beat. The combination of the first two assumptions is analogous to the "stratified interference" assumption (Hudgens and Halloran, 2008). However, given the relative location of the beats, our "groups"—defined by each focal neighborhood and its contiguous neighbors—overlap and are not mutually exclusive unlike the case in Hudgens and Halloran (2008). Other possibilities for this function may depend on the relative areas of the beats, the proportion of the common boundary, the distance between the two beats, and so forth. The choice between various options depends on the problem of interest, the existing substantive theory, and the researcher's beliefs.

Spatial Ignorability Assumption

The second assumption parallels Rosenbaum and Rubin's (1983) ignorability assumption and is untestable. It says that, given observed pre-treatment covariates ($\mathbf{X}_i$), the treatment assignment ($Z_i, F(\mathbf{Z}_{-i})$) is independent of potential outcomes $\mathbf{Y}_i(z_i, F(\mathbf{z}_{-i}))$. Using probabilistic notation, the assumption states that for all $\mathbf{Y}_i(z_i, F(\mathbf{z}_{-i}))$,

$$P[Z_i, F(\mathbf{Z}_{-i})|\mathbf{X}_i, \mathbf{Y}_i(z_i, F(\mathbf{z}_{-i}))] = P[Z_i, F(\mathbf{Z}_{-i}|\mathbf{X}_i)]. \tag{1}$$

An important example of the ignorability assumption occurs in a randomized experimental setting, where every unit is assigned at random to its treatment; in other words, $P[Z_i, F(\mathbf{Z}_{-i})|\mathbf{X}_i, \mathbf{Y}_i(z_i, F(\mathbf{z}_{-i}))] = P[Z_i, F(\mathbf{Z}_{-i})]$ for all $\mathbf{Y}_i(z_i, F(\mathbf{z}_{-i}))$.

3 Did CAPS Affect Reported Personal Crime Rates in Chicago in the 1990s?

In this section, we present two ways of analyzing the data and results. Model 1 and its results are valid if the assumption of ignorable treatment assignment holds after controlling for past crime rates. Model 2 relaxes this assumption such that its results are valid if the assumption of ignorable treatment assignment holds after controlling for past crime and for any district-level confounding.

3.1 Model 1: As if "Randomized" Given Earlier Crime Rates

We used a three-level generalized hierarchical linear model (Raudenbush and Bryk, 2002) that models extra-Poisson dispersion as a function of between-beat between-district random effects. In particular, level 1 models the crime trajectory over time within a beat, level 2 models the differences in crime trajectories across beats within a district, and level 3 models the differences in crime trajectories across districts.

Model 1

Let Y_{tij} be the number of personal crimes reported to the police and $expos_{tij}$ be the population size per 100,000 at time t in beat i in district j. We assume $Y_{tij}|\lambda_{tij} \sim$ Poisson($expos_{tij}, \lambda_{tij}$), where $E[Y_{tij}|\lambda_{tij}] = Var[Y_{tij}|\lambda_{tij}] = expos_{tij} * \lambda_{tij}$ and λ_{tij} is the expected reported personal crime rate at time t in beat i in district j.

At level 1 (Equation 2), the model is a piecewise linear trajectory of the expected natural logarithm of the reported personal crime rate for beat i in district j,

$$\ln(\lambda_{tij}) = \pi_{0ij} + \pi_{1ij} * Before_{tij} + \pi_{2ij} * PilotInt_{tij} + \pi_{3ij} * Pilot_{tij} + \pi_{4ij} * PostInt_{tij} + \pi_{5ij} * Post_{tij} \qquad (2)$$

for $t = 1, 2, \ldots, 16$, $i = 1, 2, \ldots, 279$, and $j = 1, 2, \ldots, 25$, where

   $Before_{tij}$ is the length of exposure in years to the pre-pilot period at time t for beat i in district j;

   $PilotInt_{tij} = 1$ if the t-th observation for beat i in district j occurs during the pilot or post-pilot periods and 0 if it occurs during the pre-pilot period;

   $Pilot_{tij}$ is the length of exposure in years to the pilot period at the time of the t-th observation for beat i in district j;

   $PostInt_{tij} = 1$ if the t-th observation for beat i in district j occurs during the post-pilot period; and $Post_{tij}$ is the length of exposure in years to the post-pilot period at the time of the t-th observation for beat i in district j.

The parameters of interest could then be interpreted as:

   π_{0ij} is the natural logarithm of the expected crime rate for beat i in district j at the start of the pre-pilot period;

   π_{1ij} is the annual change during the pre-pilot period in the natural logarithm of the expected crime rate for beat i in district j;

π_{2ij} is the difference in the natural logarithm of the expected crime rate between the end of the pre-pilot period and the beginning of the pilot period for beat i in district j;

π_{3ij} is the annual change during the pilot period in the natural logarithm of the expected crime rate for beat i in district j;

π_{4ij} is the difference in the expected natural logarithm of the crime rate between the end of the pilot period and the beginning of the post-pilot period for beat i in district j; and

π_{5ij} is the annual change during the post-pilot period in the natural logarithm of the expected crime rate for beat i in district j.

The level-2 model (Equation 3) explains the differences between the trajectories on the basis of the beat-level covariate, *CAPSnbs$_{ij}$*, which denotes the proportion of the contiguous beats receiving CAPS during the pilot period for beat i in district j and represents the second dimension of the treatment, i.e., neighbor's treatment assignment,

$$\pi_{kij} = \beta_{k0j} + \beta_{k1j} * CAPSnbs_{ij} + r_{kij}, \text{ for } k = 0, 1, \ldots, 5, \qquad (3)$$
$$\text{where } \boldsymbol{r}_{ij} = [r_{0ij}, r_{1ij}, \ldots, r_{5ij}]^T \sim N_6(\boldsymbol{0}, \mathbf{T}_\pi).$$

In this model, all six level-2 outcomes (π_{0ij}, π_{1ij}, ..., π_{5ij}) are allowed to vary by beat within district because of the random effects r_{0ij}, r_{1ij}, ..., r_{5ij}. This allows every beat within a police district to have a potentially different trajectory after controlling for the proportion of the contiguous beats receiving CAPS.

The level-3 model (Equations 4 and 5) explains the differences between trajectories on the basis of the district-level covariate, *CAPS$_j$*, which is 1 if district j was assigned to CAPS during the pilot period and 0 otherwise,

$$\beta_{k0j} = \gamma_{k00} + \gamma_{k01} * CAPS_j + u_{k0j}, \text{ for } k = 0, 1, \ldots, 5, \qquad (4)$$
$$\text{where } \boldsymbol{u}_{0j} = [u_{00j}, u_{10j}, \ldots, u_{50j}]^T \sim N_6(\boldsymbol{0}, \mathbf{T}_\beta)$$
$$\beta_{k1j} = \gamma_{k10} + \gamma_{k11} * CAPS_j, \text{ for } k = 0, 1, \ldots, 5. \qquad (5)$$

The trajectories are allowed to vary by district after controlling for district treatment assignment with the addition of six random effects at level 3 ($u_{00j}, u_{10j}, \ldots, u_{50j}$).

Model 1 may be used to estimate the causal effects under four assumptions. First, given the earlier crime rate trajectory, an assumption of ignorable treatment assignment must hold. Controlling for earlier status is one of the most effective ways of reducing bias associated with confounding in non-randomized studies (Cook, Shadish, and Wong, 2008; Glazerman, Levy, and

Myers, 2003). Even though the ignorability assumption is untestable, we examine the sensitivity of the results to this assumption in section 3.2. Second, the assumption regarding the functional form of the treatment assignment of one's neighbors as it affects the potential outcomes of the focal unit also must hold. We noted earlier that the functional form in this case is the proportion of one's contiguous neighbors assigned to treatment. Third, the assumption about the functional form of the trajectory, i.e., the piecewise trajectory, must be specified correctly. This assumption could be checked by, for example, examining model fit statistics or residual plots. Fourth, the more technical modeling assumptions of the over-dispersed Poisson multilevel model, including homoscedasticity and the independence of random effects between levels, must hold. These assumptions may also be examined and relaxed if needed. When the four assumptions hold, Equations 2 through 5 provide a straightforward estimate of expected potential outcomes and causal effects of various policies. Even though we examine only instantaneous (i.e., an immediate effect of a policy at the beginning of the pilot period) effects for the four causal questions, Model 1 may also be used to examine the effects of these and other well-defined policies on the status and growth rates of the logarithm of the reported personal crime rate at any time during the pilot or post-pilot periods (Raudenbush, 2001).

Estimands of the Instantaneous Causal Effects

According to Model 1, the expected difference in potential outcomes (logarithm of the reported personal crime rate) between the end of the pre-pilot period (latter-half of 1992) and the start of the pilot period (first-half of 1993) for a beat in a control district (i.e., traditional policing) is $\gamma_{200} + \gamma_{210} * CAPSnbs$, where *CAPSnbs* is the proportion of neighbors assigned to CAPS during the pilot stage. The difference in potential outcomes for a beat in a CAPS district is $\gamma_{200} + \gamma_{201} + CAPSnbs * (\gamma_{210} + \gamma_{211})$. Therefore, the expected "all or nothing" effect on the beat's logarithm of the reported personal crime rate is $\delta^{(1)} = \gamma_{201} + 1.0 * (\gamma_{210} + \gamma_{211})$. The expected "local treatment assignment" effect when 40 percent of the beat's neighbors are assigned to CAPS during the pilot period is $\delta^{(2)} = \gamma_{201} + 0.4 * \gamma_{211}$. The expected "neighbor" effect of an additional 40 percent of contiguous neighbors assigned to CAPS for a control beat is $\delta^{(3a)} = 0.4 * \gamma_{210}$, while that "neighbor" effect for a CAPS beat is $\delta^{(3b)} = 0.4 * (\gamma_{210} + \gamma_{211})$. Finally, the "interaction effect" is $\delta^{(4)} = \delta^{(3b)} - \delta^{(3a)}$.

Similarly, other causal effects could be expressed in terms of the model parameters. However, given the data limitations, not all expected causal effects may be estimated reliably.

Empirical Results

Table 2 shows estimates of the fixed effects, standard errors, and corresponding p-values for the fixed effects, using the unit-specific estimates for Model 1 with the reported personal crime as an outcome. We examined the model fit (assumptions 3 and 4 discussed in section 3.1) via the residual plots and observed no significant violations.

Table 2. Fixed-Effects Estimates for Reported Personal Crime from Model 1

Name	Estimate	St. Error	p-Value	Signif.
Intercept, γ_{000}	7.203	0.192	0.000	***
CAPS, γ_{001}	0.535	0.544	0.336	
CAPSnbs, γ_{010}	0.671	0.448	0.136	
CAPSnbs*CAPS, γ_{011}	-1.234	0.618	0.046	**
Before, γ_{100}	-0.029	0.017	0.097	*
Before*CAPS, γ_{101}	-0.004	0.071	0.961	
Before*CAPSnbs, γ_{110}	-0.047	0.076	0.533	
Before*CAPSnbs*CAPS, γ_{111}	0.083	0.108	0.446	
PilotInt, γ_{200}	-0.140	0.019	0.000	***
PilotInt*CAPS, γ_{201}	0.100	0.077	0.219	
PilotInt*CAPSnbs, γ_{210}	0.077	0.082	0.349	
PilotInt*CAPSnbs*CAPS, γ_{211}	-0.197	0.117	0.094	*
Pilot, γ_{300}	0.045	0.014	0.005	***
Pilot*CAPS, γ_{301}	0.010	0.060	0.876	
Pilot*CAPSnbs, γ_{310}	0.018	0.064	0.778	
Pilot*CAPSnbs*CAPS, γ_{311}	-0.040	0.092	0.667	
PostInt, γ_{400}	-0.137	0.016	0.000	***
PostInt*CAPS, γ_{401}	0.031	0.066	0.649	
PostInt*CAPSnbs, γ_{410}	0.016	0.069	0.810	
PostInt*CAPSnbs*CAPS, γ_{411}	0.005	0.100	0.959	
Post, γ_{500}	-0.046	0.007	0.000	***
Post*CAPS, γ_{501}	-0.027	0.029	0.367	
Post*CAPSnbs, γ_{510}	-0.018	0.031	0.565	
Post*CAPSnbs*CAPS, γ_{511}	0.079	0.044	0.073	*

Note: *** $p \leq 0.01$, ** $0.01 < p \leq 0.05$, * $0.05 < p \leq 0.1$

First, let us consider the effect of the "all or nothing" policy. In other words, does the personal crime rate drop when everyone versus no one is assigned to community policing? We observed no statistically significant difference in the

logarithms of the reported personal crime rates in the beginning of 1993 between the two policies after controlling for the reported personal crime rate in the latter half of 1992 ($\hat{\delta}^{(1)} = \hat{\gamma}_{201} + 1.0 * (\hat{\gamma}_{210} + \hat{\gamma}_{211}) = -0.023$, $X^2_{df=1} = 0.268$, $p > 0.50$).

Second, let us consider the "local treatment assignment" effect. In other words, does a shift from "traditional" policing to CAPS offer an advantage in reducing the reported personal crime rate when 40 percent of contiguous beats are assigned to CAPS? We observed no statistically significant difference in the logarithms of the reported personal crime rates in the beginning of 1993 between the two policies after controlling for the crime rate in the latter half of 1992 ($\hat{\delta}^{(2)} = \hat{\gamma}_{201} + 0.4 * \hat{\gamma}_{211} = 0.019$, $X^2_{df=1} = 0.102$, $p > 0.50$).

Finally, let us consider the "neighbor" effect on the logarithm of the reported personal crime rate (after controlling for the reported personal crime rate in the second half of 1992). In other words, what is the effect of increasing the proportion of contiguous neighbors under CAPS by an additional 40 percent? We observed no statistically significant "neighbor" effect on a beat receiving traditional policing ($\hat{\delta}^{(3a)} = 0.4 * \hat{\gamma}_{210} = 0.031$, $t = 0.940$, $p = 0.35$). In addition, we detected no statistically significant "neighbor" effect on a beat receiving CAPS ($\hat{\delta}^{(3b)} = 0.4 * (\hat{\gamma}_{210} + \hat{\gamma}_{211}) = -0.048$, $X^2_{df=1} = 2.042$, $p = 0.15$). However, we detected a marginally significant "interaction" effect ($\hat{\delta}^{(4)} = -0.079$, $t = -1.680$, $p = 0.094$). Therefore, some evidence suggests that the "neighbor" effect may differ with the focal beat's treatment assignment.

3.2 Model 2: Controlling for District-Level Confounding

As mentioned above, 5 out of 25 districts were assigned to CAPS during the pilot stage, resulting in very low statistical power to detect the effect of policies that change the treatment of the focal area (for example, "all or nothing" or "local treatment assignment" effects), if the effect exists at all. Furthermore, the results of Model 1 suggest that, depending on the treatment assignment of the focal beat, changing the proportion of contiguous neighbors that are assigned to treatment may have a different effect on the focal beat's personal crime rate. However, the results are subject to potential bias if given past crime rates the assumption of ignorable treatment assignment fails to hold.

In this section, we relax the ignorability assumption by also controlling for district-level confounding, using the district indicators (*DISTRICT*1, …, *DISTRICT*25). The effect of any district-level covariate is confounded with the effect of district indicators. Therefore, we are unable to evaluate the causal effect of one's own treatment by using the model described in this section. Moreover, in contrast to Model 1, Model 2 allows for spatial dependence of beats across

Chicago because of the beats' proximity to each other, permitting spatial dependence of beat-level (i.e., level-2) random effects.

Model 2

To define Model 2, let Y_{ti} be the number of personal crimes reported to the police and $expos_{ti}$ be the population size per 100,000 at time t in beat i. We assume $Y_{ti}|\lambda_{ti}$ ~ Poisson($expos_{ti}$, λ_{ti}), where $E[Y_{ti}|\lambda_{ti}] = Var[Y_{ti}|\lambda_{ti}] = expos_{ti} * \lambda_{ti}$, and λ_{ti} is the expected reported personal crime rate at time t in beat i. We fit a two-level generalized spatial hierarchical linear model that combines a two-level generalized hierarchical linear model (Raudenbush and Bryk, 2002) with the spatial dependence for the error term model (Anselin, 1988; Ord, 1975) at level 2.

At level 1 (Equation 6), Model 2 is a piecewise linear trajectory of the expected logarithm of the crime rate for beat i and corresponds to Equation 2 of Model 1 with the subscript j removed, i.e.,

$$\ln(\lambda_{ti}) = \pi_{0i} + \pi_{1i} * Before_{ti} + \pi_{2i} * PilotInt_{ti} + \\ \pi_{3i} * Pilot_{ti} + \pi_{4i} * PostInt_{ti} + \pi_{5i} * Post_{ti} \tag{6}$$

for t = 1, 2, …, 16 and i = 1, 2, …, 279. The coding and meaning of the level-1 covariates are the same as in Model 1 (discussed in section 3.1).

The level-2 model (Equations 7 and 8) explains the differences between the trajectories on the basis of two beat-level covariates—(1) $CAPSnbs_i$, which denotes the proportion of contiguous beats assigned to CAPS during the pilot period for beat i, and (2) $CAPS_i$, which is 1 if beat i is in a district assigned to CAPS during the pilot period and 0 otherwise—as well as the 25 district indicators. The level-2 model can be expressed as the following regression equations:

$$\pi_{0i} = \gamma_{01} * CAPSnbs_i + \gamma_{02} * CAPS_i * CAPSnbs_i + \sum_{m=1}^{25} \gamma_{0(m+2)} * \\ DISTRICTm_i + r_{0i}, \tag{7}$$
$$\pi_{ki} = \gamma_{k1} * CAPSnbs_i + \gamma_{k2} * CAPS_i * CAPSnbs_i + \sum_{m=1}^{25} \gamma_{k(m+2)} * \\ DISTRICTm_i, \text{ for k=1, 2, …, 5.} \tag{8}$$

We allow for spatial dependence in the random effects, i.e., $\boldsymbol{r}_0 = \rho \boldsymbol{W} \boldsymbol{r}_0 + \boldsymbol{u}_0$, where $\boldsymbol{r}_0 = [r_{01}, …, r_{0i}, …, r_{0279}]^T$, $\boldsymbol{u}_0 = [u_{01}, …, u_{0279}]^T \sim N_{279}(0, \tau \boldsymbol{I}_{279})$, $\boldsymbol{W}$ is a 279 x 279 row-standardized binary contiguity matrix (Anselin, 1988; Ord, 1975), and ρ is a spatial dependence parameter. If we solve the spatial dependence equation for $\boldsymbol{r}_0$ and perform a Maclaurin expansion in ρ, we get

$$\boldsymbol{r}_0 = (\boldsymbol{I}_{279} - \rho \boldsymbol{W})^{-1} \boldsymbol{u}_0 \approx (\boldsymbol{I}_{279} + \rho \boldsymbol{W} + \rho^2 \boldsymbol{W}^2 + \sum_{k=3}^{\infty} \rho^k \boldsymbol{W}^k) \boldsymbol{u}_0, \tag{9}$$

where $\mathbf{W}^k$ represents connections to the k-th order neighbors for $k = 1, 2, \ldots$. Since W is a row-standardized matrix, $|\rho| < 1$. Thus, the effect of the higher-order neighbors decreases by a power of ρ. It is important to note that if $\rho = 0$ then there is no spatial dependence and the model reverts to the standard generalized hierarchical linear model.

Estimands of the Instantaneous Causal Effects

Under the assumptions discussed above, Model 2 provides estimands for expected potential outcomes and causal effects. Let us consider the "neighbor" effect—that is, a policy change that affects the proportion of one's neighbors assigned to treatment—on the logarithm of the reported personal crime rate in the first half of 1993, controlling for the reported personal crime rate at the end of 1992. According to the model, the expected causal effect of such a policy for a beat in a control district is $\delta^{(3a)} = \gamma_{21} * (CAPSnbs - CAPSnbs^*)$, where $CAPSnbs$ and $CAPSnbs^*$ are the proportion of one's neighbors assigned to treatment under the two alternate policies. However, the expected causal effect of a similar policy on a beat in a CAPS district is $\delta^{(3b)} = (\gamma_{21} + \gamma_{22}) * (CAPSnbs - CAPSnbs^*)$. The "interaction" effect is $\delta^{(4)} = \delta^{(3b)} - \delta^{(3a)}$. Similarly, we can express other causal effects in terms of the parameters of the model. Although, as mentioned previously, this model is unable to estimate the causal effect of any policy change involving alteration of the treatment assignment of the focal beat without additional assumptions.

Empirical Results

Table 3 presents the estimates of the fixed effects, standard errors, and corresponding p-values using the unit-specific estimates from model 2 for reported personal crime as an outcome. We found no statistically significant causal effect of changing from 0 to 40 percent the proportion of contiguous beats assigned to CAPS for a beat in a control district on the reported personal crime rate in the beginning of 1993 after controlling for the reported personal crime rate at the end of 1992 ($\hat{\delta}^{(3a)} = 0.4 * \hat{\gamma}_{21} = 0.037$, $t = 1.438$, $p = 0.15$). However, increasing the proportion of contiguous neighbors assigned to CAPS by 40 percent for a beat in a CAPS district resulted in a statistically significant decrease in the reported personal crime rate in the beginning of 1993 after controlling for the reported personal crime rate at the end of 1992 ($\hat{\delta}^{(3b)} = 0.4 * (\hat{\gamma}_{21} + \hat{\gamma}_{22}) = -0.052$). In particular, for a beat in a CAPS district, changing the proportion of contiguous neighbors assigned to CAPS from 0.6 to 1.0 results in an 5.1 percent greater reduction in the reported personal crime rate ($e^{-0.052} = 0.949$). Finally, the

"neighbor" effect on beats assigned to control and those assigned to CAPS is statistically significantly different ($\hat{\delta}^{(4)} = -0.088$, $t = -2.450$, $p = 0.015$).

Table 3. Estimates for Reported Personal Crime from Model 2

Name	Estimate	St. Error	p-Value	Signif.
CAPSnbs, γ_{01}	0.624	0.412	0.131	
CAPSnbs*CAPS, γ_{02}	-1.098	0.598	0.067	*
Before*CAPSnbs, γ_{11}	-0.066	0.046	0.157	
Before*CAPSnbs*CAPS, γ_{12}	0.099	0.066	0.130	
PilotInt*CAPSnbs, γ_{21}	0.092	0.064	0.151	
PilotInt*CAPSnbs*CAPS, γ_{22}	-0.221	0.090	0.015	**
Pilot*CAPSnbs, γ_{31}	0.003	0.049	0.953	
Pilot*CAPSnbs*CAPS, γ_{32}	-0.034	0.069	0.622	
PostInt*CAPSnbs, γ_{41}	-0.000	0.059	0.998	
PostInt*CAPSnbs*CAPS, γ_{42}	0.029	0.083	0.723	
Post*CAPSnbs, γ_{51}	-0.027	0.019	0.141	
Post*CAPSnbs*CAPS, γ_{52}	0.087	0.026	0.001	***
Spatial Dependence, ρ	0.633			

Note: *** $p \leq 0.01$, ** $0.01 < p \leq 0.05$, * $0.05 < p \leq 0.1$;
Estimates for the district indicators were omitted from this table.

4 Discussion

In this paper, we examined four policy-relevant causal questions regarding the impact on crime rates of implementing community policing in Chicago in the 1990s. We found no significant "all or nothing" effect and no "local treatment assignment" effect on a beat's reported personal crime rate. We detected a moderate "neighbor" effect on a beat's reported personal crime rate for a beat in a CAPS district. In particular, in early 1993 beats in a CAPS district with a larger proportion of neighbors in the CAPS condition experienced a greater drop in crime. We also detected a statistically significant "interaction" effect on the reported personal crime rate, implying that the "neighbor" effect differs for beats in control and CAPS districts. The results for "neighbor" and "interaction" effects were stable when we controlled further for district-level confounding.

The study design offered many benefits but also posed major challenges. The first challenge was the non-random assignment of districts to treatment during the pilot period. To address that challenge, we controlled for earlier crime rates and district-level confounding. However, if unobserved differences between

beats within the same district affect both the proportion of neighbors receiving CAPS and the crime rate, the estimated effects may still be subject to bias. As for the second challenge, for practical reasons, only five police districts were assigned to CAPS during the pilot period. Such a highly unbalanced treatment assignment in conjunction with small sample size resulted in low statistical power to detect treatment effects. Furthermore, police districts were assigned to the treatment condition as a whole, and only one interior district was assigned to CAPS during the pilot period. As a result, beats located in the control districts were primarily surrounded by other control beats, whereas beats in the CAPS districts were primarily surrounded by other CAPS beats. For example, during the pilot period no beats in districts assigned to CAPS had fewer than one-third of their neighbors also assigned to CAPS and no beats in control districts had more than half of their neighbors assigned to community policing. Moreover, 12 of 20 control districts did not share a boundary with any CAPS districts during the pilot period. This, in turn, reduced the statistical power to detect an effect of changing the treatment assignment of neighboring beats.

To answer the four causal questions, we extended Rubin's causal framework by relaxing the no-interference assumption and redefining potential outcomes and causal effects. The new framework offers several benefits. First, it encourages researchers to be more precise about the causal question of interest by (1) considering the relationship between units and (2) confronting explicitly the no-interference assumption. Second, relaxing the no-interference assumption allows researchers to answer new and interesting questions. For example, the extended framework allows to differentiate between "all or nothing" and "local treatment assignment" effects and to define the "neighbor" and "interaction" effects. These four causal questions could be relevant to policymakers (e.g., mayor, police chief, neighborhood leaders) depending on which public policy is under consideration. Third, the extended framework could help guide researchers' design decisions when planning experiments in spatial settings. For example, to estimate the "all or nothing" effect, it would be preferable to conduct a cluster-randomized experiment in multiple cities, with cities randomly assigned to different treatments. However, to estimate the "neighbor" effect, a study should randomly assign neighborhoods (or clusters of neighborhoods) to treatments (or treatment patterns). Thus, the extended causal inference framework naturally leads to the specification of a study design in spatial settings and to the conclusion that, if one's neighbors' treatment may affect one's potential outcome, then, in addition to the standard consideration of sample size and effect size (among other considerations), the spatial location of units and spatial effect size would affect the power of the study.

Finally, the approach presented in this paper is applicable to a variety of studies in spatial settings. For example, public health researchers are often

interested in evaluating the effects of community-level interventions—such as building a new grocery store or a community gym, enhancing a neighborhood's walking environment, or creating parks and children's playgrounds—on the residents' diet, physical activity, and obesity rates. Another common research interest is whether living in more or less affluent neighborhoods (as measured by residents' average socioeconomic status) improves health, educational attainment, or employment and earnings outcomes. Given that neighborhood borders are generally permeable, the intervention or condition of the proximate neighborhoods needs to be considered when estimating such effects.

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