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Is There Such a Thing as "Defended Community Homicide"?: The Necessity of Methods Triangulation

Ryan Spohn
Consortium for Crime and Justice Research and Juvenile Justice Institute, rspohn@unomaha.edu

Is There Such a Thing as "Defended Community Homicide"?: The Necessity of Methods Triangulation

Elizabeth Griffiths, Department of Sociology, Emory University, Atlanta, Georgia, USA
Robert D. Baller, Department of Sociology, University of Iowa, Iowa City, Iowa, USA
Ryan E. Spohn, Department of Sociology, Anthropology, and Social Work, Kansas State University, Manhattan, Kansas, USA
Rosemary Gartner, Centre of Criminology, University of Toronto, Toronto, Ontario, Canada

Abstract
  Data on homicides in Buffalo, New York, are analyzed to demonstrate the importance of "methods triangulation" for assessing the validity of quantitative measures. Defended community homicides are quantitatively operationalized as acts that occur in the offender's community against a nonlocal victim. Poisson models provide strong support for the existence of defended community homicide, which is significantly more common in residentially stable and racially homogenous neighborhoods. However, subsequent qualitative analyses of the victim and offender characteristics and motives of these homicides undermine the "defended community" concept. Qualitative analyses are necessary to assess the validity of quantitative measures in criminological research.

Keywords: defended community homicide, qualitative, quantitative, methods triangulation

Although social scientists have consistently emphasized the importance of triangulation for increasing confidence in the results of empirical inquiries (Berg, 2001; Campbell & Fiske, 1959; Denzin, 1978; Maxwell, 1996; Patton, 2002; Yin, 1994), we only infrequently use multiple methods to verify our findings. Triangulation is most useful when the methods employed have different strengths and weaknesses such that the strengths of one method remedy weaknesses in another. A common weakness of quantitative measures is that valuable information is lost in the process of quantification. This loss of information can be particularly damaging to measures of complex concepts. To assess the severity of this loss of information, qualitative data and "methods triangulation" should be employed (Patton, 2002, p. 556).

Methods triangulation can be differentiated from three other types of triangulation: "triangulation of sources," "analyst triangulation," and "theory/perspective triangulation" (Patton, 2002, p. 556). The triangulation of sources is employed when the origin of collected information is varied. For instance, to measure characteristics of children using a survey, the researcher may survey the children themselves and the children's friends, teachers, or parents. With analyst triangulation, multiple researchers interpret the same evidence in an attempt to control researcher bias. Finally, theory or perspective triangulation is used when a researcher interprets evidence from multiple points of view to attempt to eliminate alternative hypotheses. In this paper, we demonstrate the value of methods triangulation. Patton (2002, p. 556) describes methods triangulation as efforts to evaluate "the consistency of findings generated by different data collection methods."

The importance of methods triangulation for social science research more generally, and the largely quantitative discipline of criminology in particular, is an issue worthy of revisiting. We illustrate this argument by working with two measures of the concept "defended community homicide"- one relying on the type of quantitative data typically analyzed in aggregate-level studies of crime and one based on qualitative data. Comparing results from analyses based on these two measures allows us to present a cautionary tale about how the absence of triangulation can result in quantitative measures that lack validity.

The paper proceeds as follows. First, we discuss examples of quantitative criminological research that fail to employ triangulation and we suggest potential problems with construct validity in this research. Then we outline both our quantitative and qualitative analyses of defended community homicide. We conclude with a discussion about how this study evolved from a search for defended community homicide into a lesson about the importance of methods triangulation.

SIMPLE MEASURES, COMPLEX CONCEPTS

Social science research generally, and criminological research specifically, would be enormously disadvantaged if researchers were not able to rely on measures of individual attitudes and behaviors or community characteristics derived from surveyor census data. However, measures based on these sorts of data are often lacking in the detail and depth necessary to capture the complex concepts they are intended to operationalize. Moreover, with the growing use of large-scale public-use datasets researchers have increasingly relied on precoded measures that limit opportunities for triangulation. While a loss of context is inherent in the nature of quantification, the potential effects of quantification on one's interpretations and conclusions need to be recognized and acknowledged. We offer four examples of how measures relying on context-poor, quantified data may be limited in their ability to capture and estimate the prevalence, causes, and consequences of their underlying concepts: one in which the meaning of the underlying concept is oversimplified, two in which the meaning is assumed but not captured, and one in which the meaning is altered or distorted.

The first possibility is that in measuring a concept with context-poor data, we may oversimplify its meaning. For example, the Conflict Tactics Scale (CTS) (Straus, 1979), which was designed to measure the different ways in which intimate partners deal with conflicts in their relationships, became a target of criticism by feminist and other researchers for a number of reasons. To the extent that the CTS was used to document physical violence in these relationships, the concern was that it captured only those violent incidents that occurred in the context of settling disputes (therefore leaving out incidents that were not dispute-related), that it excluded some types of violence (such as sexual assault), and that it failed to capture the quality and meaning of violent incidents (Browning & Dutton, 1986; Brush, 1990; Dobash & Dobash, 1992). For instance, in its initial version, each violent act (e.g., a "slap") was recorded without attention to the intensity or repercussions of the event, or the meaning attributed to it by either party to the interaction. Using the CTS, Straus, Gelles, and Steinmetz (1980) found that violence against men by their female partners was as common as violence perpetrated by men against women. This controversial finding led Johnson (1995) to develop a typology of intimate violence that portrayed a fuller range and captured the more serious incidents of male intimate partner violence. He distinguished between "common couple violence" (which tended to be fairly well captured by the CTS) and "patriarchal terrorism," which is the overwhelming domain of men who control their intimate partners with severe violence and its threat, but which is rarely captured by the CTS. The major problem with the CTS, then, is that it docu-

mented only a limited range of physical violence in intimate relationships and did so without attention to the context and meaning of this violence.

A second possibility is that in measuring a concept with context-poor data, its meaning is assumed rather than captured. For example, studies on interracial homicide typically infer motive from the occurrence of the act itself, in part because much of this research uses a macro-structural perspective to explain rates of interracial violence, not incidents. According to a racial threat perspective, interracial homicide rates should be higher in areas where racial tensions are elevated as a result of racial inequality (McCall & Parker, 2005; Parker & McCall, 1999), economic competition between racial groups (Eitle, D'Alessio, & Stolzenberg, 2002; Jacobs & Wood, 1999), and racial heterogeneity (Messner & South, 1992). But interpersonal violence happens between individuals, not whole racial groups. Without having information on the motives of offenders, the relationship between offenders and victims, or the circumstances in individual cases of interracial homicide-information usually lacking in the quantitative data used in aggregate-level analyses-it is unclear what role "racial threat" plays in these incidents. But by assuming racial threat is at work in interracial homicides, alternative hypotheses or accounts are neglected. For example, greater racial mixing can create opportunities for both positive and negative between-group interactions, heightening both interracial marriage and interracial violence (South & Messner, 1986). Ifthe fact of greater intermarriage itself produces more interracial domestic violence, then it is more likely that the relationship between the victim and the perpetrator is a cause of increased interracial homicide, and not racial threat per se. Studies testing the racial threat hypothesis, then, essentially assume rather than document the meaning of the interracial homicides that are the basis for their dependent variable.

Similarly, studies of the diffusion of violence make assumptions about the meaning of the relationship between violence in one area and violence in neighboring locations, often relying on "diffusion" as a residual explanation for any spatial association not explained by other variables in the model. For example, in an analysis of southern U.S. counties, Baller, Anselin, Messner, Deane, & Hawkins (2001) found a positive relationship between the homicide rate and the average homicide rate of neighboring counties, controlling for a number of important social structural factors. In this case, the significant geographic relationship may indicate diffusion processes, but without examining characteristics of specific homicides it is not possible to know with any certainty that homicides in the South are a function of violence that occurs in neighboring areas. Like research on interracial threat, the meaning of the relationship between violence and violence in neighboring areas is assumed rather than directly measured in the macro literature on the diffusion of violence.

Finally, measuring a concept with context-poor data may change or distort its original meaning, particularly if the concept is derived from ethnographic research. For instance, in an effort to test hypotheses developed out of Anderson's (1999) ethnographic study of Philadelphia neighborhoods, Baumer and his colleagues (2003) used the area-identified National Crime Victimization Survey to assess the character and quality of violence in census tracts. They developed a measure of "neighborhood disadvantage" using census data to predict characteristics of violent incidents (i.e., weapon use, resistance, and injury) that are assumed to represent the concept of"the code of the street," or toughness, masculinity, and aggression in the face of abject poverty. There is, however, a problem with attempting to test hypotheses from Anderson's (1999) ethnographic work with census data. According to Anderson (1999), both "decent" and "street" people live in the same poverty-stricken neighborhoods and share many of the same demographic characteristics. The code of the street, then, is not a characteristic of neighborhoods so much as peopleand more specifically, people in particular situations. Measuring the code of the street would require observational data; it cannot be assumed to exist based on the structural features of the environment or the demographic characteristics of people who live in those environments.

We cannot determine the extent to which conclusions from the types of research just described might be affected by context-poor measures of key concepts.[1] We can, however, demonstrate how conclusions drawn from an illustrative analysis of a context-poor quantitative measure would be altered by triangulation. To do this, we operationalize the concept of defended community homicide using the type of quantitative data commonly available to criminological researchers and then assess its validity using qualitative data and methods triangulation.

SEARCHING FOR DEFENDED COMMUNITY HOMICIDE: A CAUTIONARY TALE

We initially set out in this study to examine the incidence and predictors of defended community homicides, or homicides motivated by a desire to protect the well-being or integrity of the perpetrator's neighborhood. Studying homicide, in particular, is important for extending our knowledge of defended community crime. Heitgerd and Bursik (1987) argued that defended community crime is most likely to be found among more serious crimes, suggesting that a study of defended community homicide is appropriate. The substantive issue motivating our study was the possibility of a counterintuitive effect of social organization on defended community homicides. Previous work on the topic argues that measures of social organization, such as residential stability and racial

homogeneity, may be positively related to defended community homicide. The theory underlying this relationship suggests that community residents in stable neighborhoods (compared to those in less stable areas) are more apt to defend themselves from what they perceive as attacks against their community's integrity from outsiders who venture into the area (Green, Strolovitch, & Wong, 1998; Heitgerd & Bursik, 1987; Suttles, 1972). Evidence for defended community homicide is scant, however, and stems primarily from in-depth ethnographic research in a few neighborhoods (Suttles, 1972) and from Heitgerd and Bursik's (1987) quantitative analysis ofdelinquency among local juveniles in socially organized communities.

Central to the concept of defended community violence, then, is the interconnection between the victim's residential location and the offender's. The Heitgerd and Bursik study, however, did not rely on information about whether the victim was in fact from outside the community. Therefore, we believed that an examination of defended community homicide that could identify the residences of victims and offenders along with the location of the homicide incident would be an important addition to this line of research. We also believed that evidence of defended community homicide would provide a provocative extension of current neighborhood-level research, given that the theoretical underpinnings of defended community violence are counterintuitive according to classic social disorganization theory. That is, social organization should result in fewer homicides (Shaw, Zorbaugh, McKay, & Cortrell, 1929), not more.

Relying on prior work (Green et al., 1998; Heitgerd & Bursik, 1987; Suttles, 1972), we developed the following conceptual definition of defended community homicide. *Defended community homicides are perpetrated against nonresidents to protect the subjective and/or objective well-being of the perpetrator's neighborhood*. At a minimum, this definition requires that the residence of the victim be outside the offender's neighborhood. Subjective wellbeing can involve the reputation of an area or the level of fear among residents. This type of well-being might be enhanced by attacking someone to demonstrate a neighborhood's "toughness," or to alleviate residents' fear of the victim or those like the victim. Objective well-being can involve housing values or the level of crime in a neighborhood. Here an offender may attack someone whose presence is perceived to lower housing values or contribute to crime.[2] As described below, analyses of a quantitative measure of this concept produced results that are consistent with past work on defended community crime; however, subsequent qualitative analysis showed that the significant quantitative results are produced by an invalid quantitative measure. This result compelled us to emphasize the importance of methods triangulation in this paper.

OPERATIONALIZING DEFENDED COMMUNITY HOMICIDE

To measure defended community violence quantitatively we used the most basic necessary conditions amenable to measurement with the type of context-poor data on homicides typically available to researchers: the incident should have occurred in the offender's neighborhood and the victim should have resided in a different neighborhood. We decided to add a comparative element to the quantitative analysis by separately studying intracommunity homicides. For these cases, the place of incident and the residences of the victim and the offender are in the same neighborhood. According to classic social disorganization theory, our measures of social organization should be negatively related to this latter type of homicide. To be consistent with the practice of much community-level research (Sampson, Morenoff, & Gannon-Rowley, 2002) we used census tracts to approximate neighborhoods.[3] By spatially locating the victim's address, the offender's address, and the incident location to produce our measure, we do not capture offender motives, the presence of multiple offenders, interethnic or interracial events, or other circumstances that might better refine our measure of defended community homicide. However, this location-based measure parallels other types of information-poor measures commonly used in criminological research. Our quantitative analysis attempts to determine if homicides fulfilling the most basic measurement criteria—i.e., those involving "outsiders" killed by "locals"—were more common in stable and homogenous neighborhoods.

THE QUANTITATIVE ANALYSIS
Data and Measures

To test for the possible *positive* effects of residential stability, racial homogeneity, and the interaction between the two on this quantitative measure of defended community homicide, we used 1990 census-tract data for the city of Buffalo, New York, combined with a victim-based homicide dataset that includes all homicides recorded by Buffalo police between January 1, 1980, and December 31, 1990.[4] Out of 453 known homicides in Buffalo during this 11-year period, we were able to obtain complete address information for 223 cases, 34 of which are defended community case—that is, homicides in which the offender lived in the tract of incident while the victim did not.[5] Of the 233 cases, 90 could be described as intracommunity homicides, wherein the victim and the offender both resided in the census tract in which the offence occurred. These cases are analyzed separately because, theoretically, social organization should be *negatively* related to intracommunity homicide. Complete address information refers to addresses that could be geocoded for all three of the following lo-

cations: victim residence, offender residence, and place of homicide incident.[6] The fact that complete address information was missing for 230 homicides suggests that sample selection bias is a strong possibility[7] (Berk, 1983; Stolzenberg & RelIes, 1997).

In our quantitative analysis, we aggregated defended community and intracommunity homicide cases to the level of census tracts and used Poisson estimation to predict the effects of social organization on defended-community versus intracommunity homicide. Counts of each type of homicide serve as dependent variables in the quantitative analysis. Both residential stability and racial homogeneity are included as indicators of social organization.[8] Residential stability is measured by the percentage of residents living in the same dwelling for at least five years. Our measure of racial homogeneity is the Herfindahl index, based on five racial categories: white, black, Asian, Native American, and other races. The index was computed by creating proportions of the population for each racial category. These proportions were squared and summed (for an illustration, see Iannaccone, 1991). The result indicates the chance of randomly selecting from a census tract two residents who occupy the same racial category. A multiplicative term is used to test for an interaction between residential stability and racial homogeneity. Prior to computing the multiplicative term, we centered the main effects at their respective means (Aiken & West, 1991). Centering allows us to interpret main effects, in models that contain multiplicative terms, as the effect of one variable on the dependent variable for units with average values on the other main effect variable (Aiken & West, 1991). Centering also alleviates multicollinearity among the main effects and multiplicative term.[9]

Following other work on the structural predictors of homicide, we include a wide array of control variables (Land, McCall, & Cohen, 1990). First, we control for the natural log of population size, constraining the coefficient for this variable to equal one. By forcing this effect to equal one, the effects produced by a Poisson model of homicide counts can be interpreted as effects on the homicide rate (see Osgood, 2000). Second, we include a resource deprivation factor. This factor is comprised of percentage black, percentage divorced or separated, percentage poor, percentage of families that are single headed, and median family income. All included items produce factor loadings greater than 0.5 in absolute value in a maximum likelihood factor analysis. The resource deprivation factor was created using regression scoring in Stata 7.0 (StataCorp, 2001). Finally, we also control for the following variables that did not load strongly on the resource deprivation factor as separate items: the Gini index of family income inequality, percentage of the civilian labor force that is unemployed, and percentage of the population aged 15-34. Census data used to compute these tract-level measures were downloaded from ESRI's online data archive (ESRI, 1998).

Results

  Table 1 presents tract-level results of Poisson models of defended community and intracommunity homicide. Neither model exhibits over-dispersion.[10] The effects of resource deprivation are positive and significant in both models, but based on column three, the effect is significantly stronger in the model of defended community cases.[11] Percentage unemployed and percentage aged 15-34 are significantly related to intracommunity cases but not to defended community cases. Only the effects of age structure significantly differ between the models.

  The results of Table 1 also illustrate that residential stability increases defended community homicide. For intracommunity homicide, the effect of residential stability is negative, but not significant. These effects are significantly different. Results for racial homogeneity indicate that it is not related to either type of homicide.[12]

  Our results for the interaction term between residential stability and racial homogeneity are particularly important. We find that the positive effect of residential stability on defended community homicide is significantly stronger in racially homogeneous areas. The multiplicative effect is negative in the model of intracommunity cases, but it is not significant. Column three indicates that these effects are significantly different—a result that suggests that defended community violence exists in Buffalo.

  Our quantitative results raise a few intriguing issues. First, resource deprivation is a robust predictor of contemporary homicide. Second, residential stability increases defended community homicide while decreasing intracommunity homicide. This important finding suggests that defended com-

Table 1: Poisson models of defended community and intracommunity homicide counts (coefficients and robust standard errors in parentheses).

	Defended Community	Intra Community	Col. 1 vs. Col. 2
Resource deprivation	1.246* (0.223)	0.406* (0.175)	*
Gini index of income inequality	-3.216 (3.203)	5.450 (3.906)	N.S.
Percent unemployed	0.108 (0.076)	0.263* (0.059)	N.S.
Percent aged 15-34	0.049 (0.028)	-0.051 * (0.023)	*
Residential stability	0.099* (0.023)	-0.025 (0.020)	
Racial homogeneity	-1.158 (1.083)	0.900 (0.810)	N.S.
Residential stability X Racial homogeneity	0.243* (0.066)	-0.032 (0.088)	*
N	89	89	
Wald Chi-Square	75.68*	169.87*	

*$p < .05$ (two-tailed tests).
Note: Both models control for the natural log of population size. The coefficient for the natural log of population size is constrained to equal 1. Both models also include an intercept. The abbreviation "N.S." means not significant. A "*" in column three indicates that the coefficients in columns one and two are significantly different from one another.

munity homicide results, in part, from *social organization*: the same force that *prevents* most other forms of violence. Lastly, there is a conditioning effect of racial homogeneity on residential stability. That is, racial homogeneity increases the likelihood that residential stability will be translated into defended community homicide. This is again consistent with the theoretical expectations for defended community violence compared to other types of violence. We speculate that racial homogeneity makes it easier to distinguish community residents from outsiders who may threaten neighborhood interests. While the results for intracommunity homicide are generally consistent with classic social disorganization theory, the results for defended community homicides differ dramatically. Moreover, the results predicting defended community homicide are consistent with the counterintuitive effects expected with this form of homicide compared to other types of homicide.

With these conclusions in hand we might feel satisfied in arguing that, even with this relatively unsophisticated measure, we found evidence that defended community homicide exists in Buffalo and is shaped by the kinds of community characteristics identified in earlier work on defended community violence. We might also argue that an analysis based on a more precise measure of the concept could very well produce even stronger evidence consistent with theoretical notions of defended community homicide.

This is the stage at which methods triangulation takes on particular importance. Fortunately, the data we analyzed contain rich qualitative information on the motives of the homicides and the characteristics of victims and offenders. This information allowed us to assess the validity of our quantitative measure. To do this we conducted a qualitative analysis of the 34 incidents identified as defended community homicides based on our quantitative measure.

THE QUALITATIVE ANALYSIS

Police files, coroner reports, newspaper articles, and other sources of information served as the basis for fairly detailed descriptions of each homicide in the Buffalo dataset. We used these descriptions or narratives to gain a sense of the circumstances and motives of offenders in these 34 homicides. At the heart of this analysis is a search for negative and positive cases (Maxwell, 1996). According to Maxwell (1996, p. 93), "identifying and analyzing discrepant data and negative cases is a key part of the attempt to falsify a proposed conclusion. Instances that cannot be accounted for by a particular interpretation or explanation can point up important defects in that account." In our analysis, negative cases are homicides identified as defended community homicides according to our quantita-

tive measure that do *not* fit the imagery of defended community crime as illegal acts committed in the name of community defense. Positive cases are consistent with this imagery. That is, defended community crime is motivated by the view, right or wrong, that it will somehow augment the subjective or objective well-being of an area.

A review of the descriptions of the 34 cases reveals that nearly all of the homicides identified by our quantitative measure are negative cases. In other words, most of these incidents fail to exhibit characteristics consistent with the imagery of defended community crime, including the motive of community defense. In fact, the most common scenarios among the cases involve quarrels among family members and intimate partners, not attacks on outsiders from rival neighborhoods. The following are illustrative examples. Keep in mind that the victim and offender did not reside in the same census tract and the victim was killed in the offender's tract.

> Case #9009: The victim and offender, both black, were brothers. Both were drinking at the offender's apartment. An argument ensued. The victim left and the offender followed him into an alley and stabbed him.

> Case #8025: The victim and offender, both white, were lovers. The victim was a woman and the offender was a man. While at the offender's apartment, the offender started an argument that was motivated by jealousy. The offender shot the victim.

These cases are clearly inconsistent with the imagery of defended community crime.

Only 4 of the 34 defended community cases exhibit characteristics that are remotely consistent with the concept of defended community crime. Narratives of these potentially positive cases follow.

> Case #8917: The offender was a 63-year-old black male. He was attacked by a group of teenagers with baseball bats, and subsequently shot and killed one of the teens (a 16-year-old black male).

> Case #8822: The offender was a 32-year-old black male. The offender appears to have had a dangerous reputation that prevented witnesses from coming forward. The offender and victim, a 42-year-old black male, had an altercation in the street, when the victim was stabbed.

> Case #8408: The offender, a black male, owned a store. The victim, also black, was with a group of friends goofing around after a party. They stopped by the store, which had just closed and knocked on the

window. The offender waved them away. They remained outside, so the offender got a gun and shot at the boys, hitting the victim.

Case #8321: The offender, a black male, and his friends encountered the victim, a black male, and his friends on the street. The victim's friendship group was very drunk. The offender's group attacked the victim's group because of a previous wrongdoing perpetrated by a member of the victim's group. The victim was stabbed.

Only these 4 cases show any resemblance to actual defended community crimes, but even in these cases a defended community motive is not obviously present. In cases 8917, 8408, and 8321 groups of outsiders were perceived as threatening by a resident or residents, but it is not clear if residents attacked to defend themselves or their neighborhood. Case 8822 is included because it raises the possibility that defended community homicides could be identified based on the reaction of the local community to the homicide. In cases where witnesses are hesitant to cooperate with police, the hesitancy may be caused by fear of the offender, as in case 8822, or the desire to protect a community defender.

According to these narratives most, if not all, of the 34 cases fail to exhibit evidence consistent with the concept of defended community homicide. This result indicates that our quantitative measure of defended community homicide, on its own, is not valid. Despite the fact that a quantitative analysis based on this measure produced findings wholly consistent with past theoretical and empirical work on defended community crime, and the fact that this measure is an improvement over measures used in past quantitative research on defended community crime, we would have been incorrect to conclude that the significant effects of residential stability in racially homogenous communities on this quantitative measure actually indicate the presence of defended community homicide in Buffalo, New York.

DISCUSSION AND CONCLUSIONS

Had we stopped at the conclusion of our quantitative analyses, we would have had a paper describing evidence of defended community homicides, although none existed. That is, the significant positive effects of residential stability on homicides that involve resident offenders and nonresident victims suggested that defended community homicide is more likely to occur in residentially stable neighborhoods, especially in neighborhoods that are both stable and racially homogenous. However, our investigation of the case narratives revealed that the mechanism underlying these effects is not related to processes of community defense, and that

these effects are, in fact, type-1 errors. These revelations highlight the importance of methods triangulation as a means of evaluating the validity of quantitative measures of complex concepts, and the necessity of focusing on the meaning of concept operationalizations before drawing hasty conclusions from quantitative results.

Researchers who conduct aggregate-level, quantitative analyses of homicide now regularly attend to an array of potential problems that plague that sort of work. Doing so gives their analysis and conclusions greater rigor. In our case, had we limited ourselves to the quantitative analysis we described above, it would have been incumbent upon us to acknowledge and address several possible sources of error. Among these would be our use of census tracts as the unit of analysis. In any study of aggregate units, the modifiable areal unit problem (MAUP) is a consideration. While an areal unit based on administrative boundaries may be available in an electronic format convenient for researchers, these administrative units may be essentially meaningless as a social construct.[13] A second problem we would have had to address is our use of tract-level community characteristics, such as residential stability, measured at the 1990 census. In actuality, the homicide incidents used in the creation of our dependent variable occurred prior to the tract-level measurements of our independent variables. While we would not anticipate systematic bias resulting from this source of measurement error, we cannot be sure that none exists. Finally, our quantitative analysis suffered from an extensive missing data problem. While our analyses of case-level information and comparisons to prior geographic analyses of homicide suggest that this is not a serious problem, we cannot be certain that sample selection did not bias our quantitative results.

Yet, despite these potential sources of error in our analysis—error that would most likely work against findings supportive of the literature on defended community crime—we still found evidence consistent with predictions about defended community crime. In a sense, we could argue that greater attention to these sources of error might strengthen conclusions drawn from the quantitative analysis. Following accepted practice for validating results from this sort of quantitative analysis, however, does not address the more fundamental problem of measuring a complex concept with a relatively simple measure.

Our study serves as a cautionary tale about measuring complex concepts with context-poor quantitative data in the absence of methods triangulation to assess the validity of measures. The lesson from this tale is hardly a new one. Over 25 years ago, in his presidential address to the American Sociological Association, Hubert Blalock (1979) called for greater attention to measurement validity. While stating that "all measurement is to some degree indirect" (1979, p. 882), Blalock argued that some measures are better than others, depending on the measurement assump-

tions made. Thus these assumptions should be critically evaluated in research practice. Qualitative data and methods triangulation are invaluable means to this end.

NOTES

[1]Research on gang homicide provides an illustration of how different measures of a concept can yield very different estimates of a phenomenon. Police in Chicago and Los Angeles classify gang homicides in distinctly different ways (Howell, 1999; Rosenfeld, Bray, & Egley, 1999). Under the "Los Angeles definition" favored by Maxson and her colleagues, a homicide is classified as a gang homicide whenever any party to the homicide is a known gang member, regardless of the motive recorded by police (Maxson, Gordon, & Klein, 1986; Maxson & Klein, 1990, 1996). By contrast, the "Chicago definition" classifies gang homicides as those in which there is evidence of gang-related motive, such as defense of territory or inter-gang disputes. Using the Los Angeles definition, twice as many homicides would be identified as gang homicides.

[2]It is conceivable that defended community crime could target local victims. For example, a new resident may be attacked with the hope of keeping others like the new resident out of the area. Nonetheless, we focus on attacks directed against outsiders because Heitgerd and Bursik (1987) emphasize that defended community crimes are perpetrated specifically against outsiders.

[3]Using smaller units, such as blocks, would increase the odds that homicides perpetrated for reasons other than community defense would be counted as defended community homicides. In other words, moving to a smaller unit would increase the odds that offenders and victims who really lived in the same neighborhood would be counted as living in different ones. We return to the issue of the validity of using census tracts as our areal unit in the quantitative analysis after our presentation of results.

[4]Homicides studied here refer to deaths that resulted from the intentional use of force. Accordingly, justifiable homicides are included. Deaths of mothers and babies during or after abortions and deaths due to other forms of negligence are excluded. Infant deaths are only included if there was evidence of trauma or an official determination of homicide.

[5]In cases with multiple offenders, the residential address of the most heavily involved offender (i.e., the offender who dealt the death blow) was used to classify homicides.

[6]The details of geocoding are available upon request.

[7]In sensitivity analyses using available data at the incident level, we found that excluded and included cases are significantly different in a few ways. First, by definition, unsolved homicides are excluded from our analysis. Offenders in these cases are not known; therefore, addresses for offender residences are not available. Second, stranger homicides are underrepresented among included cases. Third, included cases actually overrepresent nonwhite victims, victims with prior criminal records, and unemployed victims. Included and excluded cases do not differ significantly on the following dimensions: victim sex, offender sex, victim marital status, offender marital status, offender employment status, offender race, offender prior criminal record, weapon use, and gun use. These results suggest that sample selection bias may be a problem, but ours is not the typical missing data problem. Poor and minority victims and victims with criminal histories are overrepresented in the homicide cases examined here. This restricts the external validity of our analysis: conclusions may not apply to homicides involving more affluent victims. The underrepresentation of strang-

er homicides in our data, because of unsolved cases (Riedel, 1993) and missing address information, suggests that we may be undercounting defended community cases that should be more likely to involve strangers; however, we are comforted by the fact that the proportion of our cases that involve local offenders and outside victims is consistent with past research. Of the 223 cases with complete address information in the Buffalo dataset, 15.2% involve local offenders and victims who resided elsewhere. Tita and Griffiths (2005) examined an extremely complete homicide dataset for the city of Pittsburgh and found that of the 420 homicides that occurred between 1987 and 1995, 18.3% involved local offenders and outside victims. Similarly, Groff and McEwen (2004) found that of the 2,781 homicides that occurred in Washington D.C. between 1990 and 2002, 20.1% involved local offenders and victims who lived elsewhere. These comparisons suggest that we did not severely undercount this type of homicide for Buffalo. We revisit the issue of sample selection following a discussion of our results.

[8]Note that our tract-level independent variables are measured for 1990 while the dependent variable refers to the prior ll-year period. Again we return to this potential source of measurement error after our presentation of the results.

[9]Ordinary least squares equivalents of the Poisson models presented below produce variance inflation factors that are all less than four, suggesting that multicollinearity is not a serious problem in our Poisson models.

[10]Spatial lags of each dependent variable were added to the models, using the "Anselin Alternative" method (see Land & Deane, 1992), but none reached significance. Further, their addition did not affect the substantive implications of the results. Accordingly, they are not included. Green et al. (1998) also find no spatial autocorrelation of racially motivated crime in their study of defended communities in New York City.

[11]Between model z tests are based on $(b1 - b2)/ sqrt((se1)^{**}2+(se2)^{**}2)$. This formula is designed for comparisons across independent samples. The models of table 1 refer to the same census tracts, but a different dependent variable is examined in each case, thus each case can be considered to be an independent sample.

[12]Conclusions regarding these main effects do not change if the interaction term, discussed next, is dropped from the models.

[13]In analyses not shown, we assess the locations of the 34 "defended community" incidents on a map of Buffalo to determine their proximity to census tract boundaries. Indeed, several of the incidents occurred relatively close to tract boundaries giving rise to two competing but plausible explanations. First, this raises the possibility that victims and offenders in these cases actually lived in the same neighborhood, not different ones (an issue attributable to the MAUP); or alternatively, differences between insiders and outsiders may be more salient for residents who live near neighborhood boundaries, causing these residents to engage in defended community crime before outsiders enter the neighborhood's interior (consistent with the theory of defended community violence).

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