

EXCHANGE

FEAR OF CRIME, SOCIAL TIES, AND COLLECTIVE EFFICACY: MAYBE MASQUERADING MEASUREMENT, MAYBE DÉJÀ VU ALL OVER AGAIN*

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In "Social Integration, Individual Perceptions of Collective Efficacy, and Fear of Crime in Three Cities," Gibson, Zhao, Lovrich, and Gaffney (*Justice Quarterly*, Vol. 19, No. 3, 2002) examined survey data from three western U.S. cities. In each, they found the following: (1) Social integration (SI) has a significant direct impact on fear of crime; (2) collective efficacy (CE) has a significant impact on fear of crime; (3) SI has a significant impact on CE, contributing to an indirect effect of SI on fear via CE; and (4) perceived disorder has a significant direct effect on both fear of crime and CE. So, their most important findings were that

SI → CE → Fear

SI → Fear

Gibson et al. stated that their study was needed because, previously, criminologists have "not examined and/or integrated the construct of individual perceptions of collective efficacy [CE], and investigated how such processes collectively contribute to explaining fear of crime" (pp. 558-559). They drew two main conclusions:

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(1) "That social integration [SI] had the most important effect on individual perceptions of collective efficacy regardless of sociodemographic factors, prior victimization, and perceptions of neighborhood social disorder . . . in sum, social ties among neighbors (and to the neighborhood) may lead to attachments that result in the building of trust among neighbors and expectations that neighbors will be willing to intervene as agents of informal social control in appropriate situations" (p. 559); and (2) "increased perceptions of collective efficacy had a relatively large impact on alleviating fear of crime among residents, even after the effects of other known predictors of fear of crime were controlled" (p. 559).

Gibson et al.'s study has numerous positive attributes. Researchers have collected survey data from three different cities, so, potentially, we can learn if findings are stable across cities. Given the growing number of studies on collective efficacy that are based on data from the Project on Human Development in Chicago Neighborhoods (PHDCN)—a number that is likely to increase substantially in the near future now that the first community survey has been archived at the Interuniversity Consortium for Political and Social Research—it is important to learn how these dynamics operate in other cities. External validity is always an empirical question and cannot be predicted *a priori* (Taylor, 1994, pp. 164-165). Basing so much of what we know about CE on results from one city may not be wise. In addition to working outside Chicago, Gibson et al. worked in smaller cities (Boise, Spokane, and Council Bluffs). Fear of crime researchers, myself included, have perhaps relied too heavily on data from relatively large cities like Seattle (Bellair, 2000), Baltimore (Taylor, 2000), and Atlanta (Taylor & Hale, 1986). We need to investigate the determinants of fear in cities whose crime problems may be qualitatively and quantitatively different from those in big cities. Gibson et al.'s work addresses that need.

This article contains two interwoven threads. One thread describes specific concerns with Gibson et al.'s study. The second uses these concerns as a starting point for reflecting more broadly on past investigations and future needed investigations of SI and CE. In expanding on this theme, I pay close attention to variations in operationalizing and labeling these constructs, validation processes, and the implications of both for advancing our theoretical understanding of SI, CE, and fear of crime.

Four specific concerns surface about Gibson et al.'s work, its numerous strengths notwithstanding. First, Gibson et al. did not clearly establish the impact of SI on CE because they did not establish the discriminant validity of the two sets of indicators they used.

Researchers recognize that social capital is a product of and not synonymous with social networks. Collective efficacy, a particular type of social capital, is distinguishable from local social ties or SI. Nonetheless, even though we recognize that SI and CE are conceptually distinct, the indicators in Gibson et al.'s study that were used for these terms extend beyond simple indicators of social networks, for SI, and willingness to intervene, for CE. In short, I recognize and agree with Gibson et al. that the two concepts, SI and CE, are distinct. Nevertheless, they have not reassured us that, as operationalized in their study, the two sets of indicators have discriminant validity. Lacking that assurance, what they modeled in their study as an impact coefficient may be part of a misspecified measurement model.

Second, Gibson et al. did not replicate their model across cities. They failed to meet the minimum suggested standards for reproducing a structural equation model, as described later. Their model *may* replicate, but we do not know yet if it will.

Third, the central dynamics examined here, the authors' statement notwithstanding, *have* been previously investigated. Much of that work may have been overlooked because of shifts in preferred labels for constructs in the past two decades. There is an important conceptual overlap between what was called informal social control 20 years ago and what is called CE now. A close examination also reveals an important operational overlap between the processes examined in these earlier studies and Gibson et al.'s. Many of the specific indicators that Gibson et al. used were previously applied to constructs, such as willingness to intervene, attachment to place, and territorial cognitions. Paying attention to this earlier work can help researchers wrestle more successfully with current conceptual challenges in the topic area.

Finally, I suggest the kinds of studies we need to establish definitively the connections of interest to Gibson et al. Although designing and implementing such studies is enormously challenging, it may be useful to sketch clearly the kinds of studies we should be pursuing.

SI AND CE: THE MEASUREMENT MODEL QUESTION

Gibson et al. claimed that SI affects CE and that both of them influence fear. Making such a claim requires establishing that SI and CE are *operationally* distinct, on the basis of the actual indicators used. How does one do so?

Confirmatory factor analyses (CFA) conducted via structural equation modeling are currently the most accepted way of establishing such a claim (Bollen, 1989). CFA allows us to examine the

convergent versus discriminant validities (Campbell & Fiske, 1959) through the loadings of indicators on latent constructs and the correlations between the underlying constructs.¹ CFA also allows us to learn if our measurement model “fits” the data—the sample variance-covariance matrix typically—acceptably well. If one were to try and establish discriminant validity of the SI and CE indicators used by Gibson et al., via CFA, what levels of distinctiveness might one seek?²

A high level of discriminant validity would be shown if the researchers asked all the CE items to load on one latent factor, all the SI items to load on a second latent factor, all the CE items to have zero loadings on the SI factor, all the SI items to have zero loadings on the CE factor, the correlations between the latent constructs to be zero, and such a measurement model fitted the data acceptably well. We would conclude that the two latent constructs were indeed distinct and completely orthogonal.³

A moderate level of discriminant validity would be obtained if researchers specified a measurement model in which SI items can load only on the SI factor and CE items can load only on the CE factor, but with one difference: One would allow the two underlying latent factors to be correlated with one another. If the model fitted the data acceptably well, we would conclude that each indicator contributes only to one underlying factor, but that SI and CE are linked. Researchers who pursue the question of the conceptual distinctiveness of fear of crime versus perception of risk have found such linkages between constructs (LaGrange & Ferraro, 1989). The language often used to describe such a situation is that the two sets of constructs are linked, but at the same time distinct.

Low to extremely weak discriminant validity is expected in a CFA model allowing underlying SI and CE factors to be correlated and allowing various numbers of indicators to load on *both* factors. The more dual, nonzero loadings of indicators allowed, the weaker the expectation of distinctiveness; the more significant dual loadings observed, the weaker the evidence of discriminant validity, and the more blurred the underlying constructs. Such a situation presents an ambiguous conceptual picture (see Bollen's, 1989, pp.

¹ Campbell and Fiske's (1959) article discussed multitrait discriminant validity in a context in which multiple methods provided indicators for each trait. In my discussion, I left out multiple methods, since only one method is discussed in the article.

² In discussing these different measurement models, I omit a discussion of correlated versus independent error terms and of identification rules (Bollen, 1989).

³ I am ignoring here the considerable discussion about what “acceptable” fit means.

230-231, discussion of the two-factor oblique rotated solution for indicators of political democracy).

Given the theoretical processes connecting SI and CE, it is probably unrealistic to expect to achieve a high level of discriminant validity in a measurement model with these two types of indicators. But a moderate level of discrimination, in which the two sets of indicators emerge as "distinct but linked" seems a plausible expectation. Again, the question at issue here is not the abstract conceptual distinction between SI and CE but, rather, the corresponding pattern of convergent and discriminant validities we expect to see, given the *particular set* indicators used in the study.

To test for the conceptual distinctiveness of SI and CE, one would start with the most demanding measurement model described earlier and proceed to less stringent tests if and when one finds that the specified measurement model fails to fit the data. The lack of fit between expectations and sample data calls for "rethinking [a] process [that] is necessarily specific to the problem at hand. . . . Relevant questions include: Could additional [or fewer] latent variables underlie the observed variables?" (Bollen, 1989, p. 297). Unfortunately, Gibson et al. did not report the results of CFA tests for high, medium, and low levels of discriminant validity. They did not report testing along the lines described earlier. Such testing would help to convince us of the discriminant validity of these indicators and the existence of two separate, underlying factors in this particular study.

What Gibson et al. did report on this issue were the results of an oblique rotation factor analysis "to assess whether perceptions of collective efficacy and social integration should indeed be treated as correlated but different constructs or whether they should be collapsed to represent one underlying construct" (p. 550). The use of an oblique rotation in this analysis, however, makes it uninformative. In exploratory factor analyses or principal components analyses, different types of rotations allow different levels of interrelationships among factors. Resulting factors can be rotated to different types of solutions in the factor space. Orthogonal rotations, of which varimax's approximation of simple structure (Thurstone, 1947; Thurstone & Chave, 1929) is the most popular, specify that the rotated factors remain orthogonal in the factor space.⁴ Oblique rotations, of which oblimin is the most popular, allow rotated factors to be nonorthogonal, or intercorrelated.

⁴ Varimax rotation attempts to rotate the factors to simple structure (Thurstone, 1947; Thurstone & Chave, 1929), so that factors or components are orthogonal to each other. Each (observed) variable has a "high" loading on just one factor or component and a zero or close-to-zero loading on other factors or components. More

Gordon (1968) pointed out that if you allow the factors to be correlated by using an oblique rotation, you have weaker evidence of separation at the conceptual level. The stronger the correlation between the factors, the less convincing is your argument that Concept A is separable from Concept B. With oblique rotations, the interpretations of the various factors are necessarily linked. By choosing an oblique rotation a priori, Gibson et al. expected strongly interrelated factors and thus conceptually blurred ones. Given the limitations of this type of analysis, it is not surprising that they reported that it "revealed mixed results that did not indicate a very clear portrayal of two entirely distinct factors" (p. 550). Such an analysis is insufficient to address the question of convergent and discriminant validities with these indicators.

Gibson et al. (p. 550) suggested additional reasons for not combining the two sets of indicators: the correlations between the indices, nonproblematic variance inflation factors (VIFs), face validity, and usage in other studies. These additional points, however, do not directly address the question. The correlations between the indices themselves do not directly address the *pattern* of *interitem* correlations; it is this pattern that is central to the question of convergent versus discriminant validities. VIFs are not directly relevant; they are clues to the degree of ill conditioning in the matrix of predictors. That ill conditioning depends on the entire set of predictors. Furthermore, Gibson et al. referred to the indices, not the more crucial patterns of interitem correlations. Face validity is an often misunderstood basis for constructing an index (Taylor, 1994, p. 148). Finally, usage in other studies is beside the point. The question is this: Given the *set* of indicators used in *this* study for SI and CE and with *these samples*, do we see the expected and appropriate patterns supporting convergent and discriminant validity? Gibson et al. did not provide an analysis to address this concern.

In fairness to Gibson et al., however, I hasten to point out that other researchers in the field often have similarly failed to conduct these needed analyses to address convergent and discriminant validities. Gibson et al. are not alone in failing to provide the evidence we need. Why might any of this matter? In structural equation modeling (SEM), the measurement model has implications for the impact model. Thus, the convergent and discriminant validity questions discussed here are potentially crucial. If the measurement model is misspecified, what Gibson et al. treated as an impact coefficient, the effects of SI on CE, should perhaps be more properly treated as part of convergent validity coefficients. We do not know

specifically, varimax rotation seeks to maximize the variance of the loadings on each factor or component.

at this time if we may have measurement coefficients masquerading as impact coefficients.

THE SIMILARITY-ACROSS-CITIES QUESTION

Gibson et al. reported that they tested a set of "stacked" models to see if their model replicated across cities. The overall model structure is the same in each city; there is the same network of exogenous impacts on mediating and outcome variables (the gamma matrix) and one impact of an endogenous variable (CE) on the fear-of-crime outcome (the beta matrix). In all cities, all the exogenous variables are allowed to correlate with one another (the phi matrix). The question is whether the strengths of the various impacts observed are *similar* across cities.

As there are levels of operational distinctiveness between sets of indicators, so, too, there are varying levels of model similarity. "Comparability or invariance in models represents a continuum" (Bollen, 1989, p. 356). At the lowest level, one has models of the same form: the "same dimensions and same patterns of fixed free and constrained values in Beta, Gamma, Psi, and Phi" (Bollen, 1989, p. 357). That is the case here. "In most applications to date, researchers assume that the form of two models is the same, and they concentrate on the similarity of parameter values within a given form. Similarity, like form, is a matter of degree" (Bollen, 1989, p. 356). In order of increasing stringency of tests for similarity, Bollen (1989, p. 357) suggested the following:

LOW: constrain the Beta and Gamma matrices to be equivalent

MEDIUM: in addition to the above, constrain the correlations between the independent variables (phi matrix) to be equivalent across the different samples

HIGH: in addition to the above, constrain the correlations between the dependent variables to be equivalent across the different samples.

"If a structural equation model with observed variables still matches the data under this highly restrictive hypothesis [the HIGH conditions noted above], the results are consistent with the assumption that the same model operates in both [here, all three cities] groups" (Bollen, 1989, p. 359). Relevant here are the low and medium criteria because the model has only one dependent variable.

To address the question of model replicability, Gibson et al. (p. 558) constrained 4 of the 11 parameters in the gamma matrix, carrying the impact of exogenous variables on endogenous variables, to yield equivalent parameters in each of the three cities. In short, they specified that only a *portion* of the gamma matrix should be

equivalent across sites. They also specified that the impact of collective efficacy on fear of crime, the one coefficient in the beta matrix, should be equivalent across sites. They reported an acceptable goodness of fit between this stacked model and the data.

Gibson et al.'s test would have met Bollen's definition of the "low" level of similarity had they specified that *all* the impact coefficients of the exogenous variables should be the same across the three cities, in addition to stipulating an equivalent impact of CE on fear of crime. They did not do so. Their stacked model test fails to meet Bollen's criteria for a "low" level of similarity. Therefore, we do not yet know whether their model is, as they claimed, similar across the three cities.

SI AND CE: THE CONCEPTUAL QUESTION

Related to the first question addressed, the distinctiveness of SI and CE, is another conceptual question: How does it happen that SI influences CE? What processes or mechanisms "carry" or "transmit" this influence? Gibson et al. offered several comments on this question: First, CE and SI lie "on a continuum and that one should not be collapsed into the other because it would undermine the process we are attempting to illuminate" (p. 550). Second, "Although our cross-sectional data did not permit us to capture temporal order, it would appear that social integration—in inclusive and supportive neighborhoods—may lead to increased perceptions of collective efficacy. Our rationale for such an assumption is based on theoretical claims put forth by scholars" (p. 559). Third, "Social ties among neighbors (and to the neighborhood) may lead to attachments that result in building of trust among neighbors and expectations that neighbors will be willing to intervene" (p. 559). Fourth, Gibson et al. referred to Morenoff, Sampson, and Raudenbush's (2001) finding that local organizational participation "promotes" CE. They referred to results of their own (details not shown), indicating effects of organizational participation on CE in two of the three cities.

The first and third comments suggest $SI = > CE$ over time. The second comment suggests that the surrounding social climate has a contextual impact on individual perceptions of collective efficacy. Both these ideas are key premises of, respectively, the systemic model of social control and territorial functioning and have already received considerable empirical support. I briefly summarize relevant points from this work, extract implications for current theoretical development, then turn to the question of organizational participation.

The Temporal Perspective

In their summary of work on neighborhood control, Bursik and Grasmick (1993, p. 18) stated:

The private, parochial and public networks capable of social control do not develop instantaneously. Rather, they slowly emerge through interaction among the residents over a period. Therefore the greater the level of residential stability that exists in a neighborhood, the less likely it is that such networks are able to control the threat of crime in an area since . . . ongoing instability makes it difficult to establish informal and formal organizations that can be maintained over time.

In short, over time social ties accumulate and allow for more CE. It is a temporally dependent process. If this is what Gibson et al. meant by their comment that the processes are on a continuum, and as described in the literature on the ecology of crime and fear, then we need longitudinal data to model that relationship. Gibson et al.'s surveys, however, were cross-sectional only. Cross-sectional data ought not be used to model a relationship that is presumed to be time dependent. Gibson et al. themselves recognized the limitations of their cross-sectional data set and highlighted the need for longitudinal data (p. 560). Nonetheless, they continued to refer to temporally dependent processes in explaining their cross-sectional findings.

The Contextual Perspective

Gibson et al.'s second comment about "conditioning" effects suggests that the presence of local SI allows CE to emerge. We have convincing cross-sectional empirical evidence on this point going back over four decades. Maccoby, Johnson, and Church (1958) found adults more willing to intervene with wayward juveniles in a homogeneous, more strongly integrated neighborhood than in a heterogeneous, more anomic one. As was noted in the quotation from Bursik and Grasmick (1993), SI itself is dependent on local homogeneity and stability.

Previous operationalizations of CE as territorial cognitions (Taylor, 1988) have observed effects of social integration on control (Taylor & Stough, 1978) and on problems that are directly caused by unregulated outsiders (Taylor, Gottfredson, & Brower, 1981) after controlling for neighborhood stability. The effects of SI on CE, however, are sometimes spatially limited (Taylor et al., 1981, Table 2).

The CE and SI dynamics under discussion here and that can influence fear of crime are clearly both temporally dependent and contextually bound. The ecological (Bursik & Grasmick, 1993) and

microecological principles (Taylor, 1997) behind these connections have been described. We need longitudinal data from clustered samples that will allow us to examine them. Expected papers from the PHDCN using the PHDCN's second panel of community surveys will undoubtedly shed light on these operations.

Finally, organizational involvement or participation is important conceptually; it is one of the major mechanisms through which processes of public local control (Bursik & Grasmick, 1993) operate; in the case of block clubs, it is an important mechanism for the parochial level of local control as well (Wandersman, 1981). How did Gibson et al. address this topic? They excluded organizational involvement from the formal models because indicators were not available in one of the study sites, Council Bluffs (p. 549). In the two remaining sites, they learned that organizational participation had no independent effects on fear (p. 547); had a modest positive bivariate correlation with CE in both sites (p. 547); but had a net positive impact on CE only in Boise, not in Spokane. In short, when we look at the organizational component of SI, Gibson et al.'s central thesis, SI => CE, is not as consistently supported.

CE and SI: Content of Items

Investigating CE is tough for two reasons. First, the construct, despite the new label, has already been investigated in a number of previous incarnations. Hunter (1985) and Bursik and Grasmick (1993) referred to three levels of local control: private (close friends, family), parochial (neighbors, acquaintances), and public (organizations, leaders). Other terms that have been applied to some of the same dynamics include *informal social control* (Greenberg, Rohe, & Williams, 1982), *social capital* (Putnam, 2000), *willingness to intervene* (Hackler, Ho, & Urquhart-Ross, 1974), and *community capacity* (Chavis, Speer, Resnick, & Zippay, 1993).

Key terms for the reverse, such as *social disorganization* (Bursik, 1988; Kornhauser, 1978) and *community disorganization* (Sampson & Groves, 1989), are equally relevant and closely tied. In the past 10 years, writers seem to have moved away from the emphasis on "disorganization" and toward an emphasis on more positive attributes, such as "efficacy." To appreciate how similar some of these variously labeled processes are, consider Sampson, Raudenbush, and Earls's (1997, p. 918) definition of CE as "social cohesion among neighbors combined with their willingness to intervene on behalf of the common good." Now consider Bursik and Grasmick's (1993, p. 15) definition of social control: "the efforts of the community to regulate itself and the behavior of residents and visitors to the neighborhood to achieve this specific goal."

Another challenge in researching CE and its close kin arises from the various ways that researchers have addressed some components of social climate. We would readily agree that indicators of social networks, such as the number of friends or relatives in the community or the fraction of neighbors known by face or name, are completely separate from, albeit important facilitators of, CE. More generally, social networks are not to be confused with social capital (Massey, 2002). Nevertheless, should we include aspects of social climate that are undoubtedly influenced by social networks, such as Sampson et al.'s (1997) "social cohesion" or Gibson et al.'s SI, or presence of or participation in local organizations?

Researchers appear to have varied in their willingness to include local organizational participation and/or aspects of local social ties as indicators of CE or related ideas. Some of the earliest work explicitly attended to group action (Park & Burgess, 1924, cited in Bursik & Grasmick, 1993, p. 14). The implication is that participation in local organizations is a component of local social control or CE.

The organizational thread has been included in some of the most recent work. Sampson and Groves's (1989) three components of social disorganization in their analysis of community-level data from the British Crime Survey were the number of local friends, attendance at local organizational meetings, and problems with unsupervised teenage groups. Reanalyses of these data showed separate, significant impacts of each of these components on local victimization rates (Veysey & Messner, 1999).

Conceptual organizers have attempted to clarify the relevant interpersonal and group components by separating them into three different levels in the systemic model (Bursik & Grasmick, 1993; Hunter, 1985). The first operationalization of CE, however, leaves out the organizational component. Sampson et al. (1997; see also Sampson, 1997) used four items asking residents how likely to intervene they thought neighbors would be in various situations in which children were misbehaving and one item about intervening to prevent the closing of a local fire station. These five items represented informal social control. Another five items asked about "social cohesion" via inquiries about local neighbors' helpfulness, perceived trust, perceived local harmony, and perceived homogeneity. Since the two indices correlated .80 at the neighborhood level, they were combined "into a summary measure labeled collective efficacy" (Sampson et al., 1997, p. 920). Therefore, the first index addressed perceived willingness to intervene, local political control, and a complex mix of sense of community and instrumental helping, but not shared organizational participation.

Morenoff et al. (2001), reanalyzing data from the British Crime Survey, added to CE separate measures of organizational involvement in their neighborhood-level analysis—an index of local residents' participation in voluntary organizations, most of which were local. But they also added indicators of social networks: how many friends and how many relatives the average respondent had living in his or her neighborhood.

Even though Gibson et al. referred to Sampson et al.'s (1997) Chicago study to defend their own operationalization of CE, they provided a different mix than was used by Sampson et al. For CE, Gibson et al. included five items in the index:

- Perceived willingness of neighbors to intervene if a suspicious person was hanging around. Similar items, with reference to specific locations, have also been used to gauge the strength of residential territorial functioning (Taylor, 1988) and willingness to intervene (Hackler et al., 1974).
- Perceived willingness of neighbors to take responsibility for the behaviors of other youths. Similar items were used by Maccoby et al. (1958) in their investigation of delinquency.
- Perceived willingness of neighbors to reciprocate social support.
- Perceived willingness of neighbors to help out with a stuck car. Mann (1954) labeled this item "latent neighborliness."
- Characterization of the neighborhood as a helpful place or one in which people go their own way. This item has been used previously in the literature to capture either attachment to place (Riger & Lavrakas, 1981; Shumaker & Taylor, 1983) or a sense of community (McMillan & Chavis, 1986).

So in the CE index, Gibson et al., like Sampson et al., referenced willingness to intervene, albeit with fewer items; instrumental helping; and a sense of community/attachment to place. Local political effectiveness, or public systemic control, the fire station-closing item in Sampson et al., was not referenced. Researchers report internal consistency coefficients in the .5 to .6 range.

At the core of the parochial level of systemic control is residents' perceived and actual willingness to intervene. These items, in varying numbers, continue to be consistently included in CE indices. Nonetheless, if we put Gibson et al.'s operationalization in a broader context, it seems that the preferred set of indicators of local social control or CE is mutating in three ways. Indicators of social disorganization, the opposite of systemic control, are dropping out. The most typical social disorganization item (Sampson & Groves, 1989) asks residents how much of a problem unsupervised teenage

groups are in their neighborhood. With the increasing attention given to social and physical incivilities, those items are relegated to disorder indices instead of CE. Second, researchers (Sampson et al., 1997; Gibson et al., 2002) seem increasingly willing to include indicators of sense of community or attachment to place. Third, researchers vacillate on whether to include indicators of public systemic control, such as organizational participation. Should it be attached to CE, integrated with SI, or handled differently?

Organizational involvement or participation is important conceptually. It is one of the major mechanisms through which public local control (Bursik & Grasmick, 1993) operates; in the case of block clubs, it is an important mechanism for the parochial level of local control as well (Taylor, Gottfredson, & Brower, 1984; Wandersman, 1981). Earlier, I described Gibson et al.'s findings for organizational participation. Gibson et al. treated organizational participation as a potential additional dimension of SI, but when they did, they found that their central thesis (SI = > CE) was not as consistently supported.

The SI index, like the CE one, is conceptually complex. The first two items assessed actual features of social networks (how many neighbors are known, how often you talk with neighbors). The third item—the neighborhood as a real home versus a place to live—has been used previously as an indicator of attachment to place (Riger & Lavrakas, 1981; Shumaker & Taylor, 1983). Therefore, we have one attachment-to-place item allocated to CE and one allocated to SI.

To sum up: At the ecological level, we have evidence that local social ties and organizations are influential on crime, even after local social control is controlled (Veysey & Messner, 1999), and that local ties and organizational participation can both influence CE (Morenoff et al., 2001). In their individual-level study, Gibson et al. similarly separated attributes of social network from systemic control. But when we examine individual items, we learn the following:

1. They included two attachment-to-place items and put them in different indices.
2. They excluded organizational participation as a component of SI; when they added it, it revealed different impacts on CE across the two sites where indicators were available. Other researchers have similarly varied in their willingness to include organizational participation.
3. Although the CE index includes "core" willingness-to-intervene items and the SI index includes "core" social network items, each index, in toto, is conceptually complex, drawing on items from other constructs as well.

Such decisions about operationalization have implications for the previously discussed issue of convergent versus discriminant validity. In the future, I hope researchers can better clarify conceptual boundaries and thus reduce such variations in how key constructs are operationalized.

It may be helpful to think at two levels of generality in clarifying the relevance of SI and CE to fear of crime. At the most general level, we have control-linked constructs, such as the three levels of systemic control, and social dynamics. At a more specific level of conceptualization, control-linked constructs separate out into the following components or content domains (Taylor, 1994): perceived willingness to intervene (Hackler et al., 1974), place-specific perceptions of responsibility accepted by self or neighbors (Taylor, 1988), and place-specific cognitions about expectations of control (Taylor, 1988). At a more specific level of conceptualization, social-linked constructs separate out into social network indicators, such as role density, multiplexity, network size, and reach (Burt, 1980); place-specific expectations about social legibility (Taylor, 1988); organizational availability and participation (Wandersman, 1981); and expectations about and histories of provided social support (House, Umberson, & Landis, 1988).

Key to developing appropriate indices for constructs is identifying the relevant content domains within the constructs. It is hoped that future indicators of SI and CE will span the aforementioned domains. Nevertheless, we are currently left with a set of additional constructs whose proper placement, when modeling fear of crime, is uncertain: attachment to place; sense of community; residential satisfaction; and social dynamics, such as perceived homogeneity or perceived organizational or leaders' effectiveness. Are these part of CE or part of SI or something separate? We will not know until we have studies that have used a full set of indicators and researchers who have carefully analyzed patterns of convergent versus discriminant validity.

20-YEAR INTEREST CYCLES, LOST LABELS, AND DÉJÀ VU ALL OVER AGAIN

In fairness to Gibson et al. (2002), two points deserve mention. It is often the case that interest in a psychological or sociological concept waxes and wanes over a number of decades (Dixon, 1971). The same is now occurring with the dynamics of central interest in this article: SI and CE. In the early 1970s to the mid-1980s, work in this area was spread among community psychology, environmental psychology, and sociology. The terms used then were *social disorganization* (Bursik, 1988), *willingness to intervene* (Hackler et al.,

1974), *territorial functioning* (Taylor, 1988), *attachment to place* (Shumaker & Taylor, 1983), *sense of community* (McMillan & Chavis, 1986), *organizational participation and neighboring* (Unger & Wandersman, 1983), *informal social control* (Greenberg & Rohe, 1986), and *local social ties* (Taylor et al., 1984). The ideas then were that

- local heterogeneity and/or local instability interferes with informal social control and/or the willingness to intervene;
- local social ties and/or local networks and/or local organizational participation strengthen informal social control and/or the willingness to intervene and/or territorial functioning and/or a sense of community and/or attachment to place; and
- the preceding dynamics affect fear of crime as well as crime itself or victimization.

In the past five years, interest in exactly the same dynamics or closely comparable ones has resurfaced, but the terminology has changed. The new labels are, as was noted earlier, *social capital*, *community capacity*, *collective efficacy*, and the like. In this article, we see this interest with the idea that SI => CE => less fear. Morenoff et al. (2001) looked at the effects of social dynamics on CE.

My guess is that it has been this change in the labels for the relevant constructs applied, even though the *specific indicators* are often the same or nearly the same, that is partly responsible for this earlier work being overlooked. In addition, on-line searching capabilities degrade rapidly as we move back more than a decade. Given these changes in applied labels, the multidisciplinary spread of the earlier work, and the limits of on-line searching, Gibson et al. probably should not be held remiss for overlooking this earlier work.

But one piece of work they did cite demonstrates the connections they investigated. Taylor et al. (1984) found significant impacts of local social ties and territorial functioning on fear of crime; they also found that the effects of local social ties on fear were mediated by territorial functioning. They observed this connection at the group and individual levels. In the street block-level analysis of fear by Taylor et al., the territorial functioning indicators were close to some of the CE items that Gibson et al. used. Two territorial cognitions investigated were "How responsible do you feel for what goes on (on the sidewalk in front of your house/in the alley behind your house)?" This wording is close to Gibson et al.'s item, "take responsibility for the behavior of youths other than their own," except that it is more spatially delimited, given the street-block focus of the earlier study. In addition, since in the earlier study the items were aggregated to the street block, it was therefore a measure of *collective*

efficacy. Other work that found connections between territorial cognitions and the rate at which residents called the police for social nuisances supports the idea of using these cognitions as indicators of CE (Taylor et al., 1981).

The social-ties item used in that earlier study was the proportion of residents that a respondent knew by face or name, averaged across the street block for the block-level analysis. This item is close to the SI item used by Gibson et al.: "How many neighbors do you know by name" (p. 549).

In the individual-level analysis of fear of crime in Taylor et al. (1984), all the variables were centered on their respective street-block means and then pooled across blocks to ensure that just individual-level dynamics were investigated. Again, the items used for territorial cognitions seem close to the CE items in Gibson et al.'s study: how responsible the respondent felt for what went on (on the porch or front steps/in the backyard). Two territorial cognition indicators used for local social legibility were "How well can you distinguish between strangers and people who belong (on your front porch or steps/in your backyard)?" These items seem close in spirit to the recognition item in the SI index of Gibson et al.'s study ("How many of your neighbors do you know by name?").

In sum, although the labels are different, partly because of changing tastes, a strong case can be made that earlier work established direct effects of SI on fear of crime, direct effects of CE on fear of crime, direct effects of SI on CE, and indirect effects of SI on fear via CE. That earlier work had the same limitation as Gibson et al.'s in that it also relied on cross-sectional data. But it had the advantage that it separated small-group from individual-level dynamics or, in the language of the systemic model, private from parochial control.

My point in referring to this earlier work is not only to highlight empirical points already established by different researchers and to encourage current researchers to ground themselves more fully in the extant work on an outcome variable, but also, what is perhaps more important, to suggest there are notable conceptual as well as empirical lessons to be extracted. I addressed earlier the question of the conceptual distinctiveness of SI and CE. In previous work, researchers have similarly wrestled with questions about the ways in which territorial functioning is different from attachment to place (Taylor, 1988); the connections between social ties and attachment to place (Riger & Lavrakas, 1981); whether attachment to place and sense of community are distinct (Hyde, 1998); how local organizations create informal control and tie into social networks (Unger & Wandersman, 1983; Wandersman, 1981); and how links

between social climate and territorial functioning—in the current lingo, how SI => CE—operate over time (Taylor, 1988). Efforts by current researchers to solve questions of the discriminant validity of SI versus CE and to describe the mechanisms whereby SI => CE => less fear may prove more fruitful if researchers consider more closely this earlier conceptual work.

WHAT WE NEED

To establish the dynamics of central interest in Gibson et al., we need a study or studies with the following characteristics:

1. A panel design, so that temporal dynamics can be investigated. Of particular interest is the idea that SI facilitates the emergence of CE over time.

2. A clustered sample design with a range of neighborhoods, varying from heterogeneous to homogeneous, and stable to unstable and, preferably, across multiple cities. Such a design will allow us to gain a better understanding of how static and dynamic community characteristics influence both SI and CE. The clustering by neighborhoods is important so that researchers, using multilevel models, can separate out, as Bursik and Grasmick (1993) described, the influences of private versus parochial versus public levels of systemic control. The use of multiple cities is important, as Gibson et al. pointed out, to learn how replicable models are from city to city. It is hoped that the multiple cities in the sample will be more geographically dispersed and racially varied than the three cities in Gibson et al.'s study. With a more diverse set of cities, impact coefficients may not be equivalent across cities. Researchers using multilevel models could learn about the determinants of weak versus strong coefficients.

3. A concerted multitrait-multimethod (Campbell & Fiske, 1959) effort to establish the convergent and discriminant validities of SI and CE with (a) detailed attention to measurement models through CFA as described earlier, (b) a good number of indicators for each, and (c) multiple methods of assessment (survey and behavioral observation, for example).

4. Serious conceptual attention to and clarification of the differences among social capital, social networks, organizational participation, collective efficacy, willingness to intervene, informal social control, sense of community, attachment to place, and social homogeneity as they apply to the fear of crime. Some of these confusions are widespread.⁵ In the fear-of-crime area, such confusion is

⁵ When asked to explain common misconceptions in sociology, Massey (2002) mentioned the confusion between social capital and social networks. Social capital "is used in so many contradictory ways as to render the concept meaningless. It has

perhaps deepened by a movement in the field toward the term *collective efficacy* and away from the term *social disorganization*.

The just-described study has not yet been completed, to my knowledge. It may never be. Soon-to-be-available analyses from the second panel of the PHDCN study should move us somewhat closer to this idea, but will continue to limit us to Chicago. Perhaps consortia of cooperating investigators will be able to frame and fund a broad effort along the lines described, taking us beyond the important things we will be learning about Chicago.

CLOSING COMMENT

Gibson et al. advanced our understanding of fear of crime by undertaking the crucially important question of model stability across different cities and using small to moderate-sized cities. They thought that their results were similar over sites and that SI affected fear directly and indirectly through CE in all sites. These points may be true, but I have suggested here that Gibson et al. did not establish them. Nonetheless, we all can agree that if our understanding of fear of crime, local control, and local social dynamics is to advance, we need more than single-city data building blocks and conceptualizing and measurement assessments that pay close attention to the multidisciplinary work already completed on these topics. Maybe the comments offered here will help future researchers obtain clearer answers to these important issues.

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become the one-size-fits-all explanation for just about everything in society today . . . social capital properly refers to productive value that can be extracted from social networks and organizations. It is not the same as those networks and organizations, which is a frequent confusion. Social capital must always be defined with respect to a specific purpose" (p. B4).

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