

Researching Crime After a Disaster: What We Can Learn From a Large Survey in New Orleans After Hurricane Katrina

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Frederick D. Weil¹

Abstract

In this article, I describe a large ($N = 7,000$) survey we conducted in greater New Orleans after Hurricane Katrina. We encountered many challenges in conducting the survey and used a number of creative means of collecting the data. Our survey, which included detailed questions about collective resources and can be aggregated to the census tract level, has great utility in investigating not only crime but also such questions as repopulation, blight reduction, resident stress, heart health, Airbnbs or short-term rentals, and foreclosures. The main difficulty was the time it took to conduct the survey because many interviews had to be done door-to-door and face-to-face to produce a representative sample. While we clearly outlined these limitations in papers we wrote, the survey duration raised questions of causal direction, and we had to conduct detailed tests of endogeneity to provide convincing evidence that our analyses were sound. I also briefly describe some of the other data we utilized, other surveys we conducted, and ethnographic and organizational work we did that not only assisted disaster recovery but also gave us insights into the social processes we investigated with our quantitative data.

Keywords

survey, Hurricane Katrina, New Orleans, social networks

¹Louisiana State University, Baton Rouge, LA, USA

Corresponding Author:

Frederick D. Weil, Department of Sociology, Louisiana State University, 126 Stubbs Hall, Baton Rouge, LA 70803, USA.

Email: fweil@lsu.edu

Introduction

Criminological research sometimes employs large, high-density resident surveys to investigate causal factors affecting different types of crime (Sampson et al., 2002). The goal is to assess the social context in which crime takes place, by comparing small neighborhood-level areas, often census tracts. Typically, researchers seek to contrast demographic and economic factors, as against collective resources like informal social control, social networks, civic and faith-based engagement, trust, and similar factors. While the Census Bureau and other government agencies can provide data to measure many demographic and economic factors, collective resources must usually be measured by resident surveys. Conducting large surveys that can be aggregated to the neighborhood level is difficult under ordinary circumstances, and only a limited number exist (Auspos, 2012). Conducting such a survey after a disaster, to assess the disaster's effects on crime or other outcomes, is more difficult still. Residents' lives are disrupted, as are the common methods of contacting them to conduct interviews: residents may have evacuated, telephone service may be disrupted, Internet access may be difficult, and it may be expensive and inconvenient to hire interviewers to conduct the survey.

In this article, I review the dominant theories that account for crime after a disaster, challenges in designing such research, some of the extant research findings, a large ($N = 7,000$) survey we conducted in greater New Orleans after Hurricane Katrina, some of our findings, limitations to our research, and steps we took to address them. We encountered many challenges in conducting the survey and used a number of creative means of collecting the data. Our survey, which included detailed questions about collective resources and can be aggregated to the census tract level, has great utility in investigating not only crime but also such questions as repopulation, blight reduction, resident stress, heart health, short-term rentals (e.g., Airbnbs), and foreclosures. The main challenge we encountered was the time it took to conduct the survey because many interviews had to be done door-to-door and face-to-face to produce a representative sample. While we clearly outlined these limitations in papers we wrote, the survey duration raised questions of causal direction, and we had to conduct detailed tests of endogeneity to provide convincing evidence that our analyses were sound.

My main task in this article is methodological. After reviewing the main theoretical frameworks commonly used to understand crime after a disaster, I begin by describing the logic of the research question, the kinds of data needed to address it, and alternatives that may be available. Next, I review some of the research that has been done that takes these frameworks into account. Then I describe our own approach to addressing this question: the data we collected, the challenges we faced, and the compromises and solutions we found. Then, I briefly summarize some of the results of our data analysis, focusing on crime questions (M. S. Barton et al., 2020; Weil, Barton, et al., 2019), but also noting other outcomes our data allowed us to examine. I also highlight ethnographic work we did on violent crime in one neighborhood that, notably, came to very similar conclusions as our quantitative work (Brown & Weil, 2020). Along the way, I

discuss the opportunities and limitations of our approach to data collection, what we can learn from such data, challenges of addressing reviewers' questions, and suggestions for improving data collection.

Theories of Disaster and Crime

Accounts of crime after a disaster generally take into account four ecological frameworks: altruism/social capital and vulnerability theories in postdisaster research, and social disorganization and routine activities theories in criminological research. Notably, these frameworks parallel each other to a considerable extent across research domains. The altruism/social capital and social disorganization frameworks stress the importance of social cohesion; and the vulnerability and routine activities frameworks stress the importance of individual motivations and structured inequality.

Postdisaster Theories

The theory of postdisaster altruism argues that "emergent" community organizations support survivors' efforts during recovery (A. H. Barton, 1969; Fritz, 1961/1996; Nobo & Pfeffer, 2012; Prelog, 2016; Quarantelli & Dynes, 1977; Rodriguez et al., 2006; Solnit, 2009). More recently, scholars have expressed this idea in terms of social capital theory while noting that certain forms of social solidarity could be harmful. For example, particular social groups may absorb resources while excluding other groups, or some communities may organize to avoid shared burdens (Aldrich, 2012; Meyer, 2018; Nakagawa & Shaw, 2004; Patterson et al., 2010; Weil, 2011; Weil et al., 2018).

By contrast, vulnerability theory focuses instead on the importance of inequality and conflict in the aftermath of disasters. Proponents of this view argue that recovery efforts disproportionately favor more advantaged groups, thereby magnifying historical inequalities (Bolin & Kurtz, 2018; Elliott & Pais, 2010; Howell & Elliott, 2018; Schultz & Elliott, 2013).

Social Disorganization, Community, and Crime

Researchers long argued that social processes and organizations play an important role in the etiology of crime, but it has been difficult to measure and show their effects. Historically, researchers spoke of social control (Janowitz, 1978; Shaw & McKay, 1942), and more recently of social capital (Coleman, 1988), collective efficacy (Sampson, 2012), civic engagement (Prewitt et al., 2014), and community organizations and institutions (Wo et al., 2016). These approaches coalesced into the social disorganization framework, which predicts that crime will be higher in neighborhoods with greater concentrations of socioeconomic disadvantage, racial/ethnic heterogeneity, and residential instability, because these factors result in weaker informal social control (Bursik, 1988; Bursik & Grasmick, 1993; Sampson, 2012; Sampson et al., 1997).

The importance of community for neighborhood crime has been well documented in research on neighborhood correlates of crime, but not always in the anticipated fashion (M. S. Barton et al., 2020; Bellair & Browning, 2010; Sampson, 2012; Weil, Barton, et al., 2019). For instance, while social networks might increase social control among residents, they could also increase solidarity among those engaged in crime or produce sympathy among neighborhood residents for their own delinquent youth (Bellair & Browning, 2010; Browning et al., 2004; Warner, 2007). This idea is often expressed as a contrast between “bonding” versus “bridging” social networks (Putnam, 2000; Woolcock, 1998) and parallels Wilson’s (1987) thesis about social isolation. Bonding social networks tend to express local social resources and connections and are more likely to develop in social isolation, while bridging networks can reduce crime by connecting resources, contacts, and job opportunities to potential offenders and encouraging them to utilize legal pursuits (Brown & Weil, 2020).

Spatial concentration is an inherent characteristic of this approach. Thus, Peterson and Krivo (2010) found that White neighborhoods tend to be surrounded by low-crime neighborhoods, while predominantly Black neighborhoods tend to be surrounded by neighborhoods with higher rates of disadvantage and crime. Similarly, research has found that violent crime was higher in neighborhoods located near areas of concentrated disadvantage (Chamberlain & Hipp, 2015; Mears & Bhati, 2006).

Routine Activities

The routine activities theory argues that the potential for crime exists when a motivated offender and suitable target converge in space and time in the absence of capable guardianship (Cohen & Felson, 1979; Felson, 1987). Accordingly, property crime may be less likely to occur in more disadvantaged neighborhoods because there are fewer and/or less lucrative opportunities, but violence may occur because violent crimes tend to occur near familiar places (Chamberlain & Hipp, 2015; Cohen & Felson, 1979). Like the social disorganization theory, this framework includes a spatial element (Chamberlain & Hipp, 2015; Mears & Bhati, 2006). For example, research by Chamberlain and Hipp (2015) found that property crime rates were higher in advantaged neighborhoods surrounded by disadvantaged areas. The authors suggest this was due to the greater prevalence of suitable targets in advantaged neighborhoods, which made it easier to obtain property obtained through theft or burglary.

Challenges of Research Design and the Logic of Inquiry

Ecological frameworks like these require appropriate ecological data to examine crime after a disaster. Base-line research designs generally begin with counts of crime incidents and demographic data in small areas like counties, neighborhoods, or census tracts. Such data are generally available from government sources like law enforcement or justice departments and the Census Bureau, as frequent as monthly or annually, and no less frequently than 5-year intervals. Neighborhood crime rates can be calculated by dividing crime counts by population counts, but in many cases, scholars

choose to analyze raw crime counts of the most serious crimes like homicide instead, because many or most neighborhoods have no instances of such crimes (Kubrin, 2003; Pyrooz, 2012; Valasik et al., 2017).

Study design becomes more challenging if we want to examine the effects on crime of collective social resources like informal social control, social networks, trust, civic engagement, or organizations, and more challenging still if we want to examine the causes of crime after a major disaster, whether natural or human-made. A key challenge in researching the effect of collective resources on crime is finding or collecting data at the neighborhood or tract level. If our dependent variable were at the individual level, like victimization or psychological well-being, or indeed, individual-level collective resources, then a standard survey might suffice. We could investigate individual-level dependent variables with a contextual or multilevel research design. A survey of, say, 500 to 1,500 respondents would be adequate for that task. However, when the incidence of crime is the dependent variable, then it must be measured at the aggregate level, for example, the tract level. For such an aggregate dependent variable, we would also need aggregate-level data on collective resources as well. Thus, if collective resources are measured by a survey, we would need a much larger sample, adequate to compute averages for each tract. Reviews of aggregate neighborhood studies generally report sample sizes with over 20 respondents per neighborhood across the study area (Auspos, 2012; Sampson et al., 2002). For New Orleans, a city with 180 census tracts, we would thus need a survey with almost 4,000 respondents, distributed appropriately across tracts.

Another challenge lies in distinguishing the different types and forms of collective resource indicators. Thus, following Coleman (1988) and Bourdieu (1986), Putnam (2000) defines social capital in terms of social networks, trust, and norms of reciprocity, also mentioning group membership, civic engagement, social support, faith-based engagement, and similar phenomena. Most scholars now distinguish between cultural (or subjective) and structural (or objective/behavioral) forms of social capital (C. J. Lee & Kim, 2013; Paxton, 1999; Prewitt et al., 2014; Van Deth, 2003), where trust and norms anchor the subjective side, and social networks and social behavior anchor the objective/behavioral side.

Within the realm of social networks, scholars have also distinguished between bonding, bridging, and linking networks (Aldrich, 2012; Putnam, 2000; Woolcock, 1998), where bonding networks are in-group ties, bridging networks are cross-group ties, and linking networks are ties to authorities. These distinctions have been posited for crime outcomes (Bellair & Browning, 2010; Beyerlein & Hipp, 2006; Desmond et al., 2010; Kubrin & Wo, 2016; M. R. Lee & Bartkowski, 2004; Wo et al., 2016), but most researchers have measured them indirectly. Generally, researchers postulate that bonding networks will be associated with higher crime levels, and bridging networks with lower crime levels, but empirical studies have often obtained inconsistent results. Only recently, have scholars been able to measure their effects more directly with survey or ethnographic data (M. S. Barton et al., 2020; Brown & Weil, 2020; Weil, Barton, et al., 2019) and have obtained results consistent with these hypotheses.

The related concept of collective efficacy was developed especially to assess informal social control relevant to crime (Sampson, 2012), though critics have questioned the operationalization of the concept, its dimensionality, its causal position, and whether it measures reputation or attribution rather than attitudes or behavior (Hipp, 2016; Hipp & Wo, 2015; Warner, 2007).

Most of these concepts can be measured by surveys, but because of the difficulties of conducting large sample surveys, scholars have sometimes sought alternative indicators that can also be measured in small geographical areas. Proxy measures are perhaps the major alternative to date, though theorists caution that they may be misleading as compared with direct measurement (Van Deth, 2003). Among the most used proxy measures are voting turnout, cooperation with the census, nonprofit organizations, household composition, or even home-ownership and residential mobility. And the most detailed geographies of measurement tend to be the county level (Dillard et al., 2013; Rupasingha et al., 2006; Rupasingha & Goetz, 2007), though a few are measured at the neighborhood, zip code, or census tract level (Aldrich, 2012; Doucet & Lee, 2015). When direct, survey-based measures have been used, most could only be aggregated to the fairly broad county level (Sherrieb et al., 2010). Yet it is hard to argue that one can measure, for example, informal social control at the county, rather than the neighborhood level, and when a disaster occurs in one or a small number of counties, analysts need finer grained geographical units like neighborhoods or census tracts to conduct aggregate analysis.

Besides aggregated survey data and proxy data, scholars have begun exploring the possibility of using big data measures like administrative data (Kyne & Aldrich, 2020; O'Brien et al., 2015), search data like Google, or social media data like Facebook or Twitter (Metaxa-Kakavouli et al., 2018). This work is quite recent, and it remains unclear, both how accessible the data will be and whether they can be used without compromising user confidentiality (Zuboff, 2019) and for those reasons, I will not discuss such data further here.

Previous Research Findings on Disaster and Crime

Recent research on crime after a disaster has generally been oriented to one or more of the ecological frameworks reviewed earlier (M. S. Barton et al., 2020; Curtis & Mills, 2011; Frailing & Harper, 2016a, 2016b; Frailing & Harper, 2017; Prelog, 2016; Spencer, 2017; Weil, Barton, et al., 2019; Zahnow et al., 2017; Zahran et al., 2009).

Researchers working in the social capital or disorganization frameworks tend to hypothesize that an increased sense of community after a disaster will result in lower levels of crime because survivors are more altruistic, not only looking out for their own interests but also those of their neighbors (M. S. Barton et al., 2020; Doucet & Lee, 2015; Leitner et al., 2011; Prelog, 2016; Weil, Barton, et al., 2019; Zahran et al., 2009). However, the use of proxy measures or geographical units larger than neighborhoods (e.g., counties) has sometimes produced inconsistent results. Prelog (2016) and Zahran et al. (2009) both used the number of nonprofit organizations as a proxy for collective efficacy and both analyzed counties, either nationally or in one state. Prelog

(2016) found that violent, and property crime levels were higher in counties with more nonprofit organizations, while Zahran et al. (2009) found that nonprofit density was negatively associated with levels of violent and property crime.

Doucet and Lee (2015), Weil, Barton, et al. (2019), and M. S. Barton et al. (2020) all used tract-level data from New Orleans in their investigations of postdisaster crime. Doucet and Lee (2015), who used proxy measures, found fewer homicides in tracts with stronger civic engagement, but only in disadvantaged neighborhoods. Weil, Barton, et al. (2019) and M. S. Barton et al. (2020), using survey-based measures, found that social trust and bridging social networks were negatively associated with several types of crime, bonding social networks was positively associated with crime, and that there was seldom an association of civic engagement with crime. These studies were critical not only because they measured collective resources in a fashion that more closely matched with the broader research on neighborhood and community correlates of crime (e.g., Bellair & Browning, 2010; Sampson, 2012; Wo et al., 2016), but also because the results showed different forms of collective resources were associated with postdisaster levels of crime.

By contrast, scholars working in the vulnerability and routine activities frameworks tend to argue that disasters exacerbate inequality, leading to higher levels of crime (M. S. Barton et al., 2020; Prelog, 2016; Spencer, 2017; Weil, Barton, et al., 2019; Zahnow et al., 2017; Zahran et al., 2009). For instance, evacuations in the immediate wake of a disaster increase the availability of suitable targets (e.g., abandoned properties, isolated residents) and decrease the number of capable guardians (e.g., police, neighborhood residents). Research on postdisaster crime supports this proposition. For example, Thornton and Voigt (2007) reported that women residing in shelters in New Orleans after Hurricane Katrina were at increased risk of sexual assault due to limited guardianship. Frailing and colleagues (Frailing et al., 2015; Frailing & Harper, 2010; Frailing & Harper, 2016a; Frailing & Harper, 2017) note that disruptions to the New Orleans Police Department, such as damaged communications technology, desertions, and officer malfeasance correlated with increased levels of burglary and homicides in post-Katrina New Orleans.

Furthermore, disasters may create areas of high crime because recovery may not be distributed evenly, resulting in the higher prevalence of physical structures able to conceal illegal activities (Curtis & Mills, 2011) or because neighborhood differences in property damage may affect the availability of suitable targets (Zahnow et al., 2017). Research on post-Katrina New Orleans, for example, found that crime was higher in areas such as the Lower Ninth Ward that featured greater concentrations of disadvantaged residents or more blight and abandonment (Curtis et al., 2013; Curtis & Mills, 2011; Doucet & Lee, 2015).

Conducting a Large Survey After a Disaster

Our own research tried to address some of the lacunae in previous research and satisfy requisites in study design outlined above by conducting a survey of Hurricane Katrina survivors in greater New Orleans that (a) contained a rich array of questions measuring

collective resources and (b) was large enough ($N = 7,000$) to aggregate to small neighborhood units like census tracts. This data basis allowed us to conduct analyses that can seldom be done and, to my knowledge, has not previously been possible after a major disaster.¹ However, the difficulty of collecting such a large sample created issues that we had to overcome, as I will describe later in this section.

We conducted our survey beginning in June 2006, with most data collected from June 2007 to April 2011. Our analyses of crime outcomes included respondents located in Orleans parish (i.e., the City of New Orleans, $n = 5,060$), where we had crime data. For other analyses (e.g., repopulation), we used data from respondents in Orleans Parish and the adjoining St. Bernard Parish ($n = 5,729$). Our sample was representative of the population living in New Orleans after Hurricane Katrina, both demographically and geographically by neighborhood. The data did not deviate greatly from the joint age-gender-race/ethnic distributions for each parish (county) as reported in census population estimates for the year of the interview, and we weighted our sample according to these census estimates. We were not able to sample the pre-storm population or track evacuees wherever they went. However, there were evacuees in our sample; 18% of respondents were still evacuees at the time of their interview. The evacuee sample is demographically diverse and lived in neighborhoods throughout New Orleans prior to the storm, but we had no sampling frame with which to compare our evacuee sample.

The survey was conducted under difficult circumstances and, during the first year and a half, with virtually no research funding. It is notable that we collected the first 2,500 interviews with essentially no funding. During the first years, land line telephones were mostly not in service, and most cell phone plans still charged by the minute, making it difficult to interview lower income respondents. Many people who evacuated also had to obtain a cell phone with an area code local to where they evacuated, making them invisible to a New Orleans telephone sampling frame.

With telephone interviewing mostly impractical, we adopted a multimethod approach. First, we conducted the survey through community and faith organizations, sometimes distributing a machine-readable paper questionnaire at meetings or religious services, asking respondents to fill them in there or at home, and then picking them up from the organization; sometimes giving respondents a web address where they could fill in the survey online. Second, *The Times-Picayune*, the major newspaper in New Orleans at the time, posted notices on their very active neighborhood discussion forums, and respondents could follow a link and complete the survey online. Third, we conducted interviews door-to-door and face-to-face throughout New Orleans neighborhoods. We chose routes through the different sections of neighborhoods and transects across flooded and nonflooded areas to obtain representativeness. This third method was important inasmuch as some population groups had trouble filling in a questionnaire themselves, whether on paper or online. The door-to-door interviewing accounted for about a third of the sample. All three methods covered most neighborhoods and most demographic groups, except that the door-to-door interviews helped bring up representation of less well-educated respondents. When telephones became

more operational and cell phone plans cheaper later in our sampling period, we decided not to conduct telephone interviews in order to maintain a more consistent method. And because our mixed methods included self-administered interviews, our costs were probably similar to an all-telephone sample. Notably, respondent cooperation was very high; many survivors had a strong desire to talk about their experience, and many said they hoped others who had to go through a disaster might benefit from our study.

Our sampling and interviewing procedures made it difficult to collect such a large sample quickly, and it took us several years to complete our interviewing, going neighborhood by neighborhood. Without the use of telephone interviewing, two main factors slowed our data collection time. First, we had to find ways of locating and contacting respondents efficiently. We established partnerships with over two hundred neighborhood associations, community groups, and faith-based organizations and employed a range of data collection strategies with them. For instance, some neighborhood associations publicized and endorsed the survey, distributed questionnaires to every *n*th residence in their neighborhoods, and received back completed questionnaires for us to pick up, and some organizations with good membership e-mail lists did the same with the online version of the survey. Since we all had an interest in obtaining representative samples, data collection usually went well, except where lack of literacy prevented some populations from filling out questionnaires without assistance. Still it took time to work with a diverse set of organizations.

Second, our door-to-door sampling, which addressed the issue of literacy, was slowed by a requirement set by our funders. As a condition of our research funding, we were required to use undergraduate interviewers from our own university, which is located an almost 4-hour round-trip drive from New Orleans. We could only make the trip on weekend days; the trips were long and strenuous; and the outdoor interviewing was often hot and rainy. As a result, it was hard to recruit and retain a large team of undergraduate interviewers. On an exceptional day, we might bring 15 student interviewers and collect over 100 interviews, and on a slow day, we might bring three or four interviewers and collect a dozen interviews. Our target became about 30 or 40 interviews per trip, but we could not always achieve that. Often, we could only persuade interviewers to go on one or two or three trips a month. If we had had the flexibility to hire local interviewers, while providing our own supervision, we could have cut the data collection time substantially. Indeed, while we were in the field, several well-resourced organizations like the Kaiser Family Foundation (Kaiser Foundation, 2007) and the Nielson Company (Palutis, 2008) each conducted door-to-door surveys of about 1,500 respondents in a few months, so we believe that this is a feasible method, if given the flexibility to hire locally-based interviewers.

We aggregated our survey data by census tract, with a mean of 21 respondents per tract, which is within a range commonly reported in aggregate neighborhood studies (Auspos, 2012; Sampson et al., 2002). We then merged the aggregated survey data with tract level data from government sources so that we could use collective resources as a predictor of neighborhood crime, repopulation, or other factors, and assess its impact as compared with demographic factors.

Despite these limitations, we are not aware of a similar sample after Hurricane Katrina, nor indeed after most disasters, that has this degree of coverage, both sampling and content (Buttenheim, 2010; Frankenberg et al., 2012; Sastry, 2009). We hope that our efforts will provide a kind of proof of concept that encourages future researchers and funders to attempt a similar sample after disasters, and hopefully improve on ours.

One potential limitation to our analytic approach is that both our survey data and the American Community Survey census data were collected over several years' time. Because we had to build up our survey sample neighborhood by neighborhood, we had to treat it as a cross-section rather than a time-series. Likewise, the intra-decennial American Community Survey requires 5 years to build up sample for New Orleans census tracts, but must be treated as a cross-section. And while crime statistics are collected on a real-time basis, we followed common research practices and used 3-year aggregations in order to reduce short-term fluctuations. Since these data sources each covered several years' time, and because they were contemporaneous with each other, we could not easily use time sequence as a means of establishing causal order.

This temporal feature of our study design raised questions of potential endogeneity in our analyses. Thus, ironically, one of the strengths of our survey, which was large enough to be aggregated to the census tract level, contributed to a potential limitation in our analyses. To address this, we performed extensive tests of endogeneity (Weil et al., 2018). Our analyses across four detailed tests found no positive evidence of endogeneity, although the tests could not entirely exclude the possibility. As a simpler check on any change over time in collective resources, we regressed it by the number of days since the storm. At the aggregate tract level, changes in collective resource variables were small and often not significant. Still to take these changes over time into account, we controlled for the mean date of the survey interviews per tract in our analyses.

Our Findings

Our analyses of crime before and after Hurricane Katrina using these data helped shed new light on the effects of a disaster on crime (M. S. Barton et al., 2020; Weil, Barton, et al., 2019), and our ethnographic research in one New Orleans neighborhood (Brown & Weil, 2020) largely corroborated our quantitative analyses with qualitative methods. I summarize three of our central findings here.

First, we found that some, but not all, forms of collective resources affected crime, before and after the disaster. Thus, social trust had the most consistent association with lower crime rates. Bridging (out-group) networks were associated with less crime, while bonding (in-group) networks were associated with more crime, and the bridging associations were stronger than the bonding associations. These social network findings were paralleled in our ethnographic research (Brown & Weil, 2020). Finally, we found only modest and irregular associations of civic engagement with lower crime once concentrated disadvantage was taken into account.

Second, we found associations with crime of concentrated disadvantage and spatial lags. However, the associations for concentrated disadvantage varied across different types of crime: concentrated disadvantage was associated with higher levels of violent crime, as social disorganization theory would predict, but lower levels of nonviolent crimes of opportunity, as the routine activities theory would predict. Spatial lag was positively associated with all forms of crime, as different forms of diffusion theory would predict.

Finally, changes across time generally supported the altruism/social capital disaster theories and provided little support for the vulnerability/inequality disaster theories. As the former theories would predict, neighborhoods with greater trust and stronger bridging networks tended to have lower levels of crime in the aftermath of Hurricane Katrina, but the effects may have moderated somewhat in the longer term. There were few changes in the associations with civic engagement. Yet against the vulnerability/inequality disaster theories, the associations of crime with concentrated disadvantage either remained fairly unchanged, weakened, or even moved against the predicted direction. Changes in spatial lags were either small or moved in inconsistent directions.

Thus, our findings showed evidence of the effect of collective resources and structural factors on crime and provided some support for the altruism/social capital disaster theories but little support for the vulnerability/inequality disaster theories.

Discussion and Conclusion

Our research (M. S. Barton et al., 2020; Brown & Weil, 2020; Weil, Barton, et al., 2019) provided new evidence of the effect of collective resources and structural factors on crime in the context of a disaster. Prior research on postdisaster crime that sought to measure community generally did not utilize direct measures of collective resources but rather, used proxy measures like nonprofit organizations (Doucet & Lee, 2015, Prelog, 2016; Zahran et al., 2009). Some of this research also analyzed variation in composite rates of crime, rather than breaking out variation in specific types of crime (Spencer, 2017; Varano et al., 2010). And research also produced a range of results as to whether a major disaster tends to raise or lower overall crime rates (Curtis & Mills, 2011; Frailing & Harper, 2016a, 2016b, 2017). Our research was able to address many of these questions, both quantitatively by using our large new survey of New Orleans area residents, and qualitatively by conducting detailed ethnographic observations.

Our results advanced the literature on collective resources and crime. First, we replicated the widespread finding that social trust is associated with lower crime levels which, as Kubrin and Wo (2016) point out, is common in both the social capital and collective efficacy approaches. Second, the distinction we found between bonding and bridging networks may help tie together disparate and apparently inconsistent findings in the literature as to whether social ties have positive, negative, or nonexistent effects on crime (Bellair & Browning, 2010; Hipp, 2010; Kubrin & Wo, 2016). Previous research examined bridging/bonding distinction with proxy measures like religious denominations or community organizations (Beyerlein & Hipp, 2006; M. R. Lee & Bartkowski, 2004; Warner et al., 2015; Wo et al., 2016), but generally not with social

networks among neighborhood residents. Third, like most other research (Kubrin & Wo, 2016; Sampson, 2012), we found little effect of civic engagement on reducing crime when concentrated disadvantage is taken into account. Recent research showing that nonprofits are associated with lower crime (Wo et al., 2016) has similarities, but we would argue that this may be a proxy measure as compared with our more direct measurement of citizen participation.

Our findings also contributed to our understanding of the effects of a major disaster on crime. We found support for the altruism/social capital disaster theory, which predicted that social solidarity would increase after a disaster and then return to latency as recovery proceeds, but little support for the vulnerability/inequality disaster theory, which predicts that rising social inequality after a disaster will produce more crime. Thus, as the first theory predicts, we found that the negative associations with crime of social trust and (to a lesser extent) bridging social networks strengthened after the storm, but then weakened somewhat as recovery proceeded. But against the second theory, we found that the effect of concentrated disadvantage on crime actually weakened after the storm. It is important to note that our findings concern only the associations of predictors with crime, not changes in the overall levels of crime, on which much of the literature focuses (Brezina & Kaufman, 2008; Curtis & Mills, 2011; Frailing et al., 2015; Frailing & Harper, 2010, 2016a, 2016b, 2017; Prelog, 2016; Spencer, 2017; Zahnow et al., 2017; Zahran et al., 2009). Yet as we noted, it is very challenging to investigate changes in neighborhood crime levels after a disaster like Hurricane Katrina. One must choose whether to evaluate absolute or per capita crimes levels. Yet either approach would produce results that are challenging to interpret because population levels fluctuated so extremely. We felt that changes in associations might be more interpretable.

Finally, our survey is one of the comparatively few (Auspos, 2012; Prewitt et al., 2014; Sampson, 2012; Sampson et al., 2002) large enough to aggregate to small geographical areas like neighborhoods or census tracts. And it is the only such survey of which I am aware that measures collective resources after a disaster. This survey permitted us to examine the etiology of crime after a disaster in a way that is rarely possible. Indeed, the survey allowed us to examine other outcomes as well, like repopulation (Weil et al., 2018; Weil & Rackin, 2019), blight reduction (Weil, 2012), short-term rentals (Weil, Shihadeh, et al., 2019), and cardiovascular health outcomes (Weil & Kroeger, 2020). Still the survey was conducted under difficult conditions, including population instability, restricted access to telephones or Internet, and a requirement that we utilize interviewers who lived a 4-hour round-trip from the site, that lengthened the time it took to conduct. That extended time, in turn, introduced questions of endogeneity that we had to address (Weil et al., 2018). Yet even taking all these factors into consideration, I think our study design has much to recommend it for future research on crime after a disaster, albeit with adjustments to overcome data collection challenges.

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Note

1. For reviews of other surveys conducted in the wake of Katrina, see Sastry (2009) and Adeola and Picou (2012). Also see Buttenheim (2010) for a general review of the challenges of collecting data after a major disaster.

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Author Biography

Frederick D. Weil is an associate professor in the Department of Sociology, Louisiana State University, Baton Rouge. He is currently researching community response to Covid-19 in New Orleans and the state of Louisiana with an NSF grant. He has been researching community recovery from Hurricane Katrina, crime, health, short-term rentals, and neighborhood change in New Orleans, as well as grassroots mentoring in New Orleans, Baton Rouge, and Chicago. He earlier conducted research on transitions to democracy in Germany and other countries.