THE DOUBLY VICTIMIZED RESIDENTS OF DISORGANIZED NEIGHBORHOODS:
SOCIAL DISORGANIZATION, COLLECTIVE EFFICACY,
AND POLICE OFFICERS’ USE OF EXCESSIVE FORCE

A Dissertation in
Crime, Law and Justice
by
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Submitted in Partial Fulfillment
of the Requirements
for the Degree of

Doctor of Philosophy

August 2008
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ABSTRACT

Police use of force has received much empirical attention over the last few decades, but little of that research has been driven by theory. In this dissertation, I propose and test a new theory of police officers’ use of excessive force based on the social disorganization tradition of research. The objective of this dissertation is therefore to explore the viability of this new theory by examining the relationship between neighborhood social disorganization, neighborhood collective efficacy, and police officers’ use of excessive force. Specifically, I hypothesize that neighborhood levels of poverty, racial/ethnic heterogeneity, and residential instability (i.e. neighborhood structural disadvantage) influence neighborhood levels of social cohesion and informal social control (i.e. neighborhood collective efficacy), which, in turn, influence police officers’ use of excessive force.

I utilize data from the Community Survey portion of the Project on Human Development in Chicago Neighborhoods to conduct Hierarchical Linear Modeling analyses that assess the neighborhood-level effects of the factors described above, net of both individual- and neighborhood-level controls for civilian reporting bias. I explore the direct and mediating effects of neighborhood structural disadvantage and neighborhood collective efficacy on neighborhood problems with police officers’ use of excessive force. The results of my analyses reveal that neighborhood structural disadvantage is significantly and positively related to police officers’ use of excessive force, while neighborhood collective efficacy is significantly and negatively related. These results suggest that a social disorganization theory of police use of force is strongly viable.
# TABLE OF CONTENTS

LIST OF TABLES .......................................................................................................................... vi

LIST OF FIGURES .......................................................................................................................... vii

ACKNOWLEDGEMENTS .................................................................................................................... viii

CHAPTER 1. INTRODUCTION ........................................................................................................ 1
  Research Objectives & Contributions of the Dissertation .......................................................... 5
  Organization of the Dissertation .................................................................................................. 8

CHAPTER 2. CURRENT EXPLANATIONS OF POLICE USE OF FORCE .................. 12
  Social Threat Theories .................................................................................................................. 15
    Empirical Tests of Social Threat Theories ................................................................................. 16
      Aggregate-Level Evidence ....................................................................................................... 18
      Individual-Level Evidence ....................................................................................................... 25
    Summary of the Evidence for Social Threat Theories .............................................................. 33
  Criminal Threat Theories ............................................................................................................ 34
    Empirical Tests of Criminal Threat Theories ........................................................................... 35
      Aggregate-Level Evidence ....................................................................................................... 37
      Individual-Level Evidence ....................................................................................................... 43
    Summary of the Evidence for Criminal Threat Theories ......................................................... 46
  Klinger’s Ecological Theory of Police Vigor ............................................................................... 51
    Klinger’s Theory as an Explanation for Police Use of Force .................................................... 53
  Chapter Summary ....................................................................................................................... 55

CHAPTER 3. THE SOCIAL DISORGANIZATION TRADITION .................................. 56
  The Origins of Social Disorganization ....................................................................................... 58
  Shaw and McKay’s Theory of Social Disorganization ................................................................. 59
  The Informal Social Control Reformulation .............................................................................. 62
  The Neighborhood Systemic Model .......................................................................................... 65
  The Emergence of a Collective Efficacy Explanation ............................................................... 67
  Empirical Tests of the Social Disorganization Tradition .......................................................... 70

CHAPTER 4. SOCIAL DISORGANIZATION & POLICE USE OF FORCE ........... 76
  Neighborhood Context and Police Use of Force ....................................................................... 77
  Linking Social Disorganization to Police Use of Force ............................................................. 82
  Research Questions and Hypotheses ......................................................................................... 87
    Neighborhood Structural Disadvantage ................................................................................... 88
    Neighborhood Systemic Model ............................................................................................... 90
    Neighborhood Collective Efficacy .......................................................................................... 91
    Neighborhood Systemic Model vs. Neighborhood Collective Efficacy ............................... 93
  Summary of Research Questions and Hypotheses ................................................................... 95
    Neighborhood Structural Disadvantage ................................................................................... 96
    Neighborhood Systemic Model ............................................................................................... 96
Neighborhood Collective Efficacy ................................................................. 96
Full Neighborhood Social Disorganization Tradition .............................................. 97

CHAPTER 5. DATA, MEASURES, & ANALYTIC STRATEGY ........................................ 98
Data ...................................................................................................................... 98
Defining Neighborhoods in the PHDCN ............................................................... 99
Measures ........................................................................................................... 102
Dependent Variable – Police Use of Excessive Force ........................................... 102
Accounting for Reporting Bias in Civilians’ Reports of Excessive Force ............... 104
Perceptions of the Police ..................................................................................... 107
Civilian Reporting Bias Control Variables ......................................................... 110
Primary Explanatory Variables of Interest – Neighborhood Social Disorganization .... 113
Analytic Strategy ............................................................................................... 117
Multi-Level Modeling Techniques ...................................................................... 119
Models for Analysis .......................................................................................... 123

CHAPTER 6. RESULTS ............................................................................................ 130
Descriptive Results ............................................................................................. 131
Bivariate Results ............................................................................................... 134
Civilian Reporting Bias Results .......................................................................... 138
Multivariate Results .......................................................................................... 141
Summary of Main Findings .................................................................................. 155
  Neighborhood Structural Disadvantage Hypotheses ........................................... 155
  Neighborhood Systemic Model Hypotheses ....................................................... 155
  Neighborhood Collective Efficacy Hypotheses .................................................. 155
  Full Neighborhood Social Disorganization Tradition Hypothesis ....................... 155
  First General Research Question ...................................................................... 156
  Second General Research Question .................................................................. 156

CHAPTER 7. DISCUSSION & CONCLUSION ............................................................ 157
Discussion of Results .......................................................................................... 158
Theoretical and Policy Implications ..................................................................... 165
Limitations of this Dissertation and Directions for Future Research ................... 169
Summary ............................................................................................................ 174

REFERENCES ...................................................................................................... 175

APPENDIX. PROJECT ON HUMAN DEVELOPMENT IN CHICAGO
NEIGHBORHOODS SURVEY INSTRUMENT ITEMS ........................................ 186
LIST OF TABLES

TABLE 2.1. Theory-Driven Studies of Police Use of Force During the Last 20 Years .. 49-50

TABLE 6.1. Descriptive Statistics .................................................................132

TABLE 6.2. Neighborhood-Level Bivariate Relationships with Police Use of Excessive Force .................................................................136

TABLE 6.3. Ordinal Regression Model Predicting Civilian Reporting Bias ..........139

TABLE 6.4. Ordinal Regression Models Predicting Police Use of Excessive Force .. 142-143
LIST OF FIGURES

FIGURE 4.1. Neighborhood Structural Disadvantage Hypotheses ........................................89

FIGURE 4.2. Neighborhood Systemic Model Hypotheses .........................................................91

FIGURE 4.3. Neighborhood Collective Efficacy Hypotheses .....................................................93

FIGURE 4.4. Neighborhood Collective Efficacy & Neighborhood Systemic Model Hypotheses ..........................................................95

FIGURE 6.1. Neighborhood Collective Efficacy and the Predicted Probability of Police Use of Excessive Force Becoming More of a Problem ...............151

FIGURE 6.2. Neighborhood Structural Disadvantage and the Predicted Probability of Police Use of Excessive Force Becoming More of a Problem ...............153
ACKNOWLEDGEMENTS

First, I want to thank my wonderful wife, Rachael, for her unwavering support and unconditional love over these past ten years and into the future. I am also especially grateful for the amazing patience and good humor that she has shown me during all the long days and late nights that were necessary to complete this dissertation and finally achieve my doctoral degree. I hope she understands how truly important she has been, and always will be, in my life.

I also would like to thank my parents, Philip and Sandra Hays, for all their encouragement, support, and love. Without them, I would have never had the opportunity to be where I am today. I hope they always know how much I love them and how much they have influenced the person that I have become.

Additionally, my deepest gratitude goes out to all my friends and relatives who have supported and encouraged me over these past five years. Along the way, I have come to lean on them all in many different ways – none of which has been more important than simply giving me the opportunity to escape the pressures of graduate school when I have so desperately needed them. For all of you who have been there in my times of need, I am sincerely thankful.

Finally, I would also like to thank all the members of my dissertation committee for their thoughtful comments, suggestions, and insights. I am particularly grateful to my chair, Eric Silver, for all his help over the past few years, both for this dissertation and for helping me develop into the professional academic that I am today. I am also very grateful to the other members of my committee, D. Wayne Osgood, Barrett Lee, Eric Plutzer, and Stephen Mastrofski. I feel extremely privileged to call each of the members of my committee not only mentors, but now colleagues as well, and I hope they feel the same way.
CHAPTER 1
INTRODUCTION

The scholarly literature on police officers’ use of force has expanded over the past few decades, yet there remains relatively little theory that explains such police behavior. The vast majority of policing studies remain atheoretical, and those studies that are theory-driven tend to rely on only two broad theoretical frameworks – social threat theories and criminal threat theories. While these frameworks have been established as strong explanations for police use of force, if the field is to move forward, alternative explanations must be put forth and empirically tested. In this dissertation, I seek to address the lack of empirically testable theories in the police use of force literature by proposing the appropriation of the criminological theory of social disorganization (typically used to explain rates of crime) for use by policing researchers in their examinations of police use of force.

Shaw and McKay’ (1942) theory of social disorganization has traditionally been used to explain the high rates of crime commonly found in inner-city neighborhoods. Born out of the Chicago School’s tradition of neighborhood ecology, Shaw and McKay viewed crime as the result of neighborhood context rather than the individual-specific factors that had previously garnered the attention of most criminologists. While the popularity of Shaw and McKay’s social disorganization theory has fluctuated over the years amongst criminologists, the role of neighborhood context has only very recently come to the attention of policing researchers. Two of the first researchers to consider how neighborhood context might influence police behavior were Slovak (1986) and Smith (1986). Slovak perfectly captured both researchers’ sentiment
when he lamented that “there is no solid lead to follow from the research of others in this regard, for almost no serious efforts to tie ecological variations within a city to police patterns in particular or to social control efforts in general have yet appeared” (1986:144).

In one of only a handful of empirical studies that have examined neighborhood context and police use of force since Slovak’s statement, Terrill and Reisig (2003:294) similarly commented that, “the literature dealing specifically with police use of force [in regards to neighborhood context] is even more remote.” Then, in an effort to rectify that gap in the literature, the researchers conducted an empirical examination of neighborhood context and police use of force. Unfortunately, while Terrill and Reisig (2003) observed a significant relationship between the two phenomena, they failed to adopt a theory as the driving force behind their research.\(^1\) Consequently, despite taking a step in the right direction, without a theoretical framework explaining how and why neighborhood context influences police behavior, we are no closer to truly understanding the relationship.

Several years prior to Terrill and Reisig’s research, however, Klinger (1997:278) noted that “few studies have considered the possibility that police action might vary across urban neighborhoods . . . [and] none contains any systematic theory linking police activity to the ecological contexts in which it occurs.” In an effort to address this issue, he therefore proposed an ecological theory of police behavior that focused on how ecological context might affect the vigor with which police officers did their jobs. Unfortunately, though, he did not apply his theory directly to police use of force behaviors, and did not attempt to empirically assess the validity of his theory.\(^2\) Thus, even though policing researchers have independently begun to both

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1 For more on this study, see my review in Chapter 3.  
2 For more on this study and Klinger’s theory, see my review in Chapter 2.
empirically examine and theorize about the nature of the relationship between neighborhood context and police use of force, a *theory-driven empirical analysis* has yet to be conducted.

In this dissertation, I echo the arguments made by Slovak, Terrill, Reisig, and Klinger - it is essential that policing researchers explore the role of neighborhood context in regards to police officers’ use of force. Unlike previous research, however, I am the first to combine a theoretical framework for understanding the influence of neighborhood context on police use of force with a rigorous empirical test of that framework. For this dissertation, I utilize the *social disorganization tradition* as the foundation for a new social disorganization theory of police use of force. Since Shaw and McKay’s (1942) original theory of social disorganization, the *tradition* of their research has lived on through the works of many other neighborhoods and crime researchers, including an increased emphasis on the role of neighborhood informal social control efforts (Kornhauser 1978), the rise of the neighborhood systemic model (Bursik and Grasmick 1993; Hunter 1985; Sampson and Groves 1989), and more recently, the emergence of the neighborhood collective efficacy perspective (Morenoff, Sampson, and Raudenbush 2001; Sampson, Morenoff, and Earls 1999; Sampson, Raudenbush, and Earls 1997). I therefore use this more broadly-defined *social disorganization tradition* as my theoretical framework for predicting police use of force in different neighborhoods, with a special emphasis on Sampson and colleagues’ concept of neighborhood collective efficacy (Morenoff et al. 2001; Sampson et al. 1999; Sampson et al. 1997).

To understand how and why neighborhood context might influence police officers’ use of force, one must first understand the arguments made within the social disorganization tradition. Briefly, Shaw and McKay (1942) originally conceived of neighborhood social disorganization as the inability of neighborhood residents to collectively define and achieve common goals, such as
crime prevention. They argued that neighborhood social disorganization resulted from deteriorating conditions within neighborhoods related to increasing rates of poverty, racial and ethnic heterogeneity, and residential instability. These factors, they posited, reduced the ability of neighborhood residents to control juveniles’ delinquent behaviors and also fostered criminal values and gangs, all of which, in turn, contributed to increased rates of juvenile delinquency in disorganized neighborhoods. Sampson and colleagues’ (Morenoff et al. 2001; Sampson et al. 1999; Sampson et al. 1997) collective efficacy approach differs from Shaw and McKay’s original theory by combining residents’ ability to informally control undesirable behavior with their level of social cohesion. Subsequently, they defined collective efficacy as the mutual trust and solidarity among neighbors (social cohesion) necessary for individual residents to willingly intervene on behalf of the neighborhood whenever inappropriate or unacceptable behaviors arose (informal social control).

Based on the ideas and concepts described above, I argue that residents of disorganized neighborhoods with low levels of collective efficacy will not only be vulnerable to higher rates of crime, but they may also be vulnerable to higher rates of police use, and even abuse, of force. As I will elaborate in more detail later in this dissertation, I expect that police officers working in disorganized neighborhoods will begin to use excessive levels of force as they come to realize that the residents of those neighborhoods are no more able to stop their inappropriate behavior than they are able to stop other undesirable behaviors (i.e. crime and delinquency).³ For the purposes of this dissertation, I follow the lead of other policing researchers and define excessive force as the use of any force that is beyond what is necessary to control an individual or effect

³ I do not believe, nor argue, that all, or even most, police officers will consciously choose to abuse their authority and use excessive levels of force in socially disorganized neighborhoods. Rather, I expect that officers will begin to use more force than is ordinarily necessary in order to accomplish their duties as quickly and easily as possible in neighborhoods that also tend to be very dangerous. I discuss the link between neighborhood disorganization and police use of excessive force in more detail in Chapter 4.
the arrest of a suspect, including the use of any force when none is required (for a detailed discussion of how excessive force is defined, see Geller and Toch 1996). It is in this sense that I expect the residents of socially disorganized neighborhoods to become doubly victimized, not only through the increased presence and activity of criminals in their neighborhoods, but also by the police who are supposed to protect them from those criminals.

**Research Objectives & Contributions of the Dissertation**

The main objectives of this dissertation are to present a new theoretical explanation for police use of force behavior and to determine whether this explanation is empirically viable. In order to do so, I explore the relationships between neighborhood context (e.g. structural conditions, social disorganization, and collective efficacy) and between-neighborhood variation in problems with police officers’ use of excessive force in Chicago, Illinois, during the 1990s. Using a framework based on the social disorganization tradition, I expect to find that highly disorganized neighborhoods not only experience higher rates of crime (as a large body of past research has already established), but also higher rates of police use of excessive force.

In addition to proposing and testing a new theoretical explanation for police use of force, a secondary objective of this dissertation is to contribution to the theoretical and methodological development of the literature on police use of force. Specifically, I seek to make three important contributions to the field. First, I conduct a detailed review of the theory-driven research on police use of force over the past 20 years. The purpose of this review is to demonstrate the continued need for the development and testing of more theoretical research of police officers’ use of force. Currently, the majority of research in the field is largely variable-driven, such that

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4 For a review of this research, see Chapter 3. Since the relationship between neighborhood social disorganization and crime has already been well-established, I do not empirically examine that relationship, and instead focus exclusively on the relationship between neighborhood social disorganization and police use of excessive force.
researchers often simply include all the explanatory variables they can find and see what significant results turn up. Unfortunately, while such variable-driven studies can help us identify what factors influence police officers’ use of force, they cannot tell us anything about why those factors are related to police behavior. Consequently, such research does little to help us understand how those factors might be manipulated to regulate the use of force.

Second, this dissertation also contributes to the police use of force literature by improving upon the existing research methods that are most commonly used. With the incorporation of a variety of neighborhood contextual measures that are necessary for testing a social disorganization theory of police use of force, it becomes necessary to analyze police use of force at both the individual- and neighborhood-levels simultaneously. The majority of the prior research on police use of force has been conducted solely at one level of analysis (i.e. either at the individual-level or the large-scale aggregate-level). Fortunately, however, recent advances in analytic methodologies now allow researchers to conduct analyses at multiple levels simultaneously. For this dissertation, I conduct a multi-level analysis of civilians’ reports of police officers’ use of excessive force that includes explanatory variables at both the individual- and neighborhood-levels. In doing so, I demonstrate a new method through which policing researchers can utilize multi-level modeling techniques to account for potential measurement error issues associated with civilian reporting bias.

Finally, with few exceptions, researchers studying police officers’ use of force have generally failed to incorporate neighborhood contextual factors into their studies. Most examinations have tended to focus either on individual-level (e.g. officer and suspect characteristics) or on large-scale aggregate predictors (e.g. city-, state-, or even nationwide population characteristics). Very few studies have considered the importance of neighborhood contextual factors. 

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5 See Chapter 4 for a review.
context. By using a social disorganization-based theoretical framework, this dissertation provides a strong theoretical explanation of police use of force behaviors. Through the incorporation of a variety of concepts and measures traditionally used in research on neighborhoods and crime, my research opens up a whole new range of explanatory measures for analysis, providing future researchers with a strong foundation on which to base their subsequent research. Thus, by incorporating concepts and measures from the neighborhoods and crime literature, this dissertation should not only open up many new avenues for theory-driven research, it may also help us better understand the causes of police officers’ use of force at the neighborhood level.

To briefly summarize, in addition to proposing and testing a new social disorganization theory of police use of force, there are three secondary contributions that I seek to accomplish in this dissertation. First, I conduct a detailed review of all the theory-driven empirical tests of police use of force in the last twenty years to help illustrate the serious need for new theories that might help explain police use of force behaviors, as well as the continued need for more empirical tests of the theories that have already been proposed. Second, I contribute to the methodological development of the field by introducing a new way in which multi-level modeling techniques can be used to study police officers’ use of force. Finally, through the appropriation of the social disorganization tradition which serves as a new theoretical framework for explaining police officers’ use of excessive force, I hope to illustrate how concepts and measures from the neighborhoods and crime literatures can help broaden our understanding of police use of force at the neighborhood-level.
Organization of the Dissertation

The remainder of this dissertation is organized into six chapters. In Chapter 2, I review the two theoretical frameworks that currently dominate the police use of force literature, and conduct a detailed review of all of the theory-driven empirical tests of those frameworks that have been conducted over the past 20 years. Based on this review, I then present a table that displays condensed summaries for each the reviewed studies, including the author(s) of each study, the data and methods they used, the specific theories they tested, and the general findings they obtained. Finally, I review Klinger’s ecological theory of police vigor. Unfortunately, as previously mentioned earlier, Klinger’s theory explains the amount of effort, or vigor, that police officers use in different ecological contexts, rather than the types or amount of force they use. Additionally, no empirical tests of his theory have been conducted to date. As a result, I do not include Klinger’s theory in my more detailed review. Nonetheless, because Klinger was the first to incorporate concepts related to the ecological context of different areas to the explanation of police behavior, I review the basic tenets of his theory, and then conclude the chapter by discussing how the theory might be appropriate not only for explaining levels of police vigor, but levels of police use of force as well.

In Chapter 3, I review the history and theoretical development of the social disorganization tradition. I begin by discussing the roots of Shaw and McKay’s original theory of social disorganization. The remainder of the chapter is dedicated to discussing all of the major additions to, losses from, and modifications of, Shaw and McKay’s original theory (i.e. the development of the social disorganization tradition). In this review, I discuss how the works of several prominent neighborhoods and crime researchers have left indelible marks on the tradition, including the increased emphasis on the role of neighborhood informal social control.
and the decreased importance of the cultural aspects (Kornhauser 1978), the disputed significance of neighborhood social ties and the neighborhood systemic model (Bursik and Grasmick 1993; Hunter 1985; Sampson and Groves 1989), and most recently, the emergence of the neighborhood collective efficacy perspective (Morenoff et al. 2001; Sampson et al. 1999; Sampson et al. 1997).

In Chapter 4, I propose my new theoretical framework for the explanation of police use of excessive force. I begin by reviewing the few existing studies that have attempted to link aggregate-level contextual factors to police use of force. I then discuss how specific concepts from the social disorganization tradition may be employed not only to explain civilians’ criminal behavior, but to explain police officers’ use of force as well. Following this discussion, I propose my social disorganization theory of police use of excessive force, with a special emphasis on the role of neighborhood collective efficacy. Finally, I conclude Chapter 4 by discussing the general research questions that I seek to answer in this dissertation, as well as the specific hypotheses that I empirically test so that I might answer those research questions.

In Chapter 5, I describe the data, measures, and analytic strategy that I use to empirically test the hypotheses proposed in Chapter 4. Of particular importance in this chapter is my discussion of the dependent variable that I use for this dissertation. The primary source of data for this dissertation comes from the Project on Human Development in Chicago Neighborhoods (PHDCN: Earls, Brooks-Gunn, Raudenbush, and Sampson 1997). The PHDCN is a survey of Chicago residents on a number of important issues facing their neighborhoods, including social disorganization and the lack of collective efficacy, as well as their perceptions of police officers’ use of excessive force. However, because I am primarily interested in the relationship between neighborhood social disorganization and actual police behavior (rather than residents’
perceptions of police behavior), I discuss in great detail the methods that I use to account for any potential measurement error in my dependent variable that might be due to civilian reporting bias. I then argue that once civilian reporting bias has been accounted for statistically, my measure should closely approximate actual police behaviors. Finally, I also discuss my analytic strategy for this dissertation. Like most research on individuals nested within neighborhoods, I face a number of statistical issues when analyzing data from the PHDCN. Therefore, I conclude this chapter by discussing the methodological problems I encounter, and how the use of multi-level modeling accounts for such problems and allows me to obtain accurate and reliable findings.

In Chapter 6, I present the results of my analyses. I begin by reviewing the descriptive statistics for each of the variables used in this dissertation, and then move on to the main findings. First, I present the findings of a number of preliminary models that I use to establish a relationship between neighborhood social disorganization and police officers’ use of excessive force. Additionally, I determine how much potential individual-level civilian reporting bias is present in my dependent variable. After briefly discussing the implications of those findings, I conclude Chapter 6 by presenting and interpreting the major findings of each of my models, step-by-step, leading up to my final model testing the robustness of a social disorganization explanation of police use of excessive force, net of the individual-level civilian reporting bias factors.

Finally, in Chapter 7, I bring this dissertation to a close by drawing together all of the preceding chapters and discussing the implications of this research. I begin by discussing how each of my hypotheses fares in light of my findings, and then summarizing what the outcomes of those hypotheses mean in terms of answering my larger research question. Is neighborhood
social disorganization, and neighborhood collective efficacy in particular, a strong explanation for variation in police officers’ use of excessive force? In addition to answering this question, I also consider the theoretical and policy implications of my findings. Lastly, I conclude this dissertation by discussing some of the limitations of my research and how they relate to a number of possible avenues for future research.
CHAPTER 2
CURRENT EXPLANATIONS FOR POLICE USE OF FORCE

Before discussing the application of social disorganization theory and its concepts to the use of force by police in the following chapters, it is necessary to understand how policing researchers currently explain police officers’ use of force. In the remainder of this chapter, I make my first contribution to the field of research on police use of force by conducting a detailed review of all of the major theory-driven studies of police use of force in the last twenty years.\(^6\) I extend my focus to all types of police use of force, rather than focusing exclusively on officers’ use of excessive force, for two reasons. First, in comparison to the field in general, the number of studies focusing exclusively on police officers’ use of excessive force is very limited. Second, whether it be the abuse of force (i.e. the use of excessive force), or the legitimate use of either lethal or non-lethal force, research on other forms of police use of force can still provide insight on why police officers’ might use excessive force as well. In other words, if some precipitating factor is sufficient to provoke the use of any kind of force by the police, it might also be sufficient to provoke the use of excessive force by some police officers. Thus, in an effort to better understand the reasons behind police use of force in general, my review includes studies of any, and all, forms of police use of force.

\(^6\) I confine my review of the theory-driven studies of police officers’ use of force to the last twenty years in order to limit this chapter to a reasonable length. Additionally, as I briefly review throughout this chapter, the historical evidence (prior to twenty years ago) has largely produced results similar to the results found in the studies that are reviewed here. Finally, because the research that has been conducted in the last twenty years has the additional benefit of using more sophisticated methodologies and analytic techniques, such research can generally provide more reliable and accurate estimates of how various explanatory variables influence police officers’ use of force. My review of the theory-driven empirical tests of existing police use of force theories is therefore limited to the research conducted since the late 1980s.
As briefly mentioned in Chapter 1, regardless of the type of police use of force being examined, many studies of policing in general do not rely on theoretical frameworks to help explain police officers’ behaviors (e.g. Bernard and Engel 2001; Garner, Maxwell, and Heraux 2002; Hagan 1989; Klinger 2004). Rather, many empirical studies of police behavior are driven primarily by the availability of data and by examining interesting combinations of explanatory and outcome variables. While such research can inform us about which factors may be related to police officers’ use of force, they cannot explain why those factors are related to police officers’ use of force. Absent such explanations, these studies therefore cannot help us understand how those factors might be manipulated to help control or reduce police officers’ use of force.

Consequently, since one of the primary goals of this dissertation is to further the theoretical development of the field, I review only the studies that have utilized a theoretical framework to propose and test hypotheses regarding police officers’ use of force (i.e. studies that are driven by theory).

Unfortunately, as I briefly mentioned above, there is not a very large body of theory-driven research on police officers’ use of force. Moreover, nearly all of the theory-driven research that does exist can be categorized into one of only two broad theoretical frameworks – social threat theories and criminal threat theories.⁷ Studies that are driven by social threat theories generally argue that certain, less powerful, groups within the larger population pose a social threat to the existing power hierarchy, and therefore become the primary targets of formal

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⁷ Some might consider police organizational explanations of police officers’ use of force to be a third theoretical framework for the study of police use of force. Unfortunately, no unified theoretical framework exists that connects the literatures on the various organizational factors that have been found to influence police use of force. That is, unlike studies testing other theoretical frameworks, there is no common theoretical argument that links organizational measures to police officers’ use of force. Instead, researchers have only been able to identify a wide variety of measures (e.g. police subcultures, administrative/departmental use-of-force policy, officer training or experience, etc.) that all explain police use of force in different ways. Thus, generally speaking, any explanations based on various police organizational factors cannot and should not be considered true theoretical frameworks of police use of force.
social control efforts, including, but not limited to, police officers’ use of force. Studies that are
driven by criminal threat theories, on the other hand, generally argue that criminals, as well as
other individuals, who pose a criminal threat to the physical safety of police officers or other
civilians, become the primary targets of police officers’ use of force.

In addition to these two more popular theoretical frameworks, Klinger (1997) has
recently proposed an ecological theory of police vigor which might also be used to explain police
officers’ use of force. Unfortunately, no empirical tests of Klinger’s theory have been conducted
to date. Nonetheless, as a possible avenue for the further theoretical development of police use
of force field, I review the basic tenets and arguments of his theory and discuss how it might be
used to not only explain police vigor, but police use of force as well.

In the remainder of this chapter, I discuss the basic tenets and arguments for all three of
the theoretical frameworks described above. I then conduct a detailed review of all the
aggregate- and individual-level empirical tests of the social threat and criminal threat theories
over the last twenty years. Again, because no empirical tests of Klinger’s theory have been
conducted as of yet, there are no findings to review. Subsequently, I review only the theoretical
arguments that he makes in his original proposal (1997). For each of the theory-driven tests of
social threat or criminal threat theories, however, I review in detail the specific theories and
hypotheses that the researchers tested, the data and methods that they used, the significant and/or
interesting results they obtained, and, finally, any important limitations of each study that might
affect any conclusions that might be drawn from the researchers’ findings.

I begin by reviewing the general arguments and findings of the empirical tests of social
threat theories, and then do the same for the empirical tests of criminal threat theories.
Following this section of the chapter, I then present Table 2.1 which displays brief synopses of
the studies reviewed here, including abridged summaries of each study’s tested theories/hypotheses, data sources and analytic techniques, and general findings. Finally, as the close to this chapter, I review Klinger’s ecological theory of police vigor, and discuss how some of police officers’ problems associated with extended periods of service in high crime areas might not only influence the vigor with which officers do their jobs, but levels of force they use as well.

**Social Threat Theories**

Social threat theories were the first theories to be used by researchers to explain police officers’ use of force, and can be traced back to Blalock’s original theory of minority group relations (1967). Social threat theories generally include any explanations that focus on the formal social control (i.e. police use of force) of less powerful groups within a larger population. Among the more specific aggregate-level theories that fall under the broader heading of social threat theories are the conventional version of conflict theory (e.g. Jacobs 1979; Jacobs and Britt 1979; Sorensen, Marquart, and Brock 1993), the political threat hypothesis (e.g. Jacobs and O’Brien 1998), and racial or minority threat hypotheses (e.g. Blalock 1967; Blumer 1958; Bobo and Hutchings 1996; Holmes 2000). At the individual-level, social threat theories include Black’s theory of law (Black 1976; Worden 1996), social script theory (Dwyer, Graesser, Hopkinson, and Lupfer 1990), and racial response bias arguments (e.g. Correll, Park, Judd, and Wittenbrink 2002; Correll, Park, Judd, Wittenbrink, Sadler, and Keesee 2007; Correll, Urland, and Ito 2006, Greenwald, Oakes, and Hoffman 2003).

Despite the variety of specific names of different social threat theories, they all generally contend that there is a conflict of interests among the different groups that make up any society,
and that it is this conflict that leads to police use of force. More specifically, they argue that in many stratified societies, but especially within western, capitalist societies such as our own, the powerful upper classes hold the financial, political, and, to a certain extent, moral authority which they can wield to protect their own interests. Unfortunately, however, the protection of the more powerful groups’ interests usually comes at the expense of the interests of the less powerful groups. In regards to the police use of force, Jacobs (1979) argued that “the more there are inequalities in the distribution of economic power and economic resources, the more one can expect that the social control apparatus of the state will conform to the preferences of monied elites” (914). In another piece he continued, “[because] the state's monopoly of violence is controlled by those who benefit from inequality, it follows that the control agents of the state [i.e. the police] should be more likely to use extreme force when economic inequality is most pronounced” (Jacobs and Britt 1979:403). Thus, according to social threat theories, as tools or agents of the upper-class’ interests, the police are expected to formally “control” members of the less powerful racial/ethnic and social class minority groups who pose a threat to the existing status hierarchy through all means available to them, including the use of physical force. Social threat theories therefore predict that members of minority groups will be more likely to experience police use of all kinds of force (legitimate and illegitimate, lethal and non-lethal) than members of majority groups.

**Empirical Tests of Social Threat Theories**

The empirical research testing social threat theories has typically utilized what policing researchers refer to as extra-legal variables to test their arguments. These variables measure various demographic characteristics of individuals that should have no legal bearing on how or
why police officers use force (hence the *extra*-legal term). For example, researchers testing social threat theories commonly examine the effect of both aggregate- and individual-level measures of race/ethnicity, gender, age, or social class, on police officers’ use of force. Those researchers then argue that because it is against the interests of those who benefit from society’s inequalities (i.e. the majority groups who tend to hold power) for racial/ethnic minorities and lower-class individuals in particular to rise in power and/or numbers, those same individuals should be the most likely to experience police use of force. As such, the majority of the aggregate-level tests of social threat theories focus on variation in rates of police use of force in areas with larger populations of racial/ethnic and social class minorities, while individual-level tests focus on how police officers differentially use force against racial/ethnic minorities in comparison to whites.  

 Historically, the empirical research testing both the aggregate- and individual-level effects of race/ethnicity and social class on police use of force has supported social threat theories (e.g. Binder and Fridell 1984; Binder and Scharf 1982; Blumberg 1986; Chamlin 1989; Goldkamp 1976; Hayden 1981; Horvath 1987; Jacobs and Britt 1979; Meyer 1980; Smith 1986). In the following sections of this chapter, I review all of the theory-driven aggregate- and individual-level empirical tests of social threat explanations that have been conducted over the last twenty years. Like their historical counterparts, the majority of these more recent studies have also found that aggregate-level measures of race/ethnicity and social class, to a certain extent, are positively related to the police use of force, while racial/ethnic minority suspects are also more likely to experience police officers’ use of force at the individual-level. I begin by

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8 At the individual-level, very few studies test how suspects’ social class influences police officers’ use of force. This is due primarily to the fact that it is extremely difficult to operationalize and measure social class based solely on a suspect’s appearance. Thus, most individual-level tests of social threat theories focus primarily on the effect of suspects’ racial/ethnic background.
reviewing the most recent aggregate-level evidence, all of which has focused on city-level police use of force.

*Aggregate-Level Evidence*

In one of the most prominent tests of a social threat theory, Jacobs and O’Brien (1998) examined how black population size, income inequality, and racial inequality (white v. black median family income) were related to police use of deadly force across a large number of U.S. cities. The researchers extended Jacobs’ earlier research, which also tested social threat theories (Jacobs 1979; Jacobs and Britt 1979), by examining justifiable police killings of civilians (i.e. police use of deadly force). Jacobs and O’Brien’s dependent variable came from the FBI’s Supplemental Homicide Report (SHR) for 170 cities nationwide with populations 100,000 or greater during the year of 1980. Data for their social threat explanatory measures came from the 1970 and 1980 U.S. census reports. Based on what they referred to as political threat theory, Jacobs and O’Brien hypothesized that rates of police use of deadly force would be the greatest in cities where the black populations were the largest and where both income inequality in general and income inequality between whites and blacks were the most pronounced.

In order to test their hypotheses, Jacobs and O’Brien conducted Tobit regression analyses to account for the heavily skewed distribution of their dependent variable. Because incidents of police use of deadly force were, and continue to be, extremely rare, many cities nationwide report zero incidents in any given year. By using Tobit regression, the researchers were able to account for the skewed distribution of their police use of deadly force variable, and were able to obtain more accurate and unbiased estimates as a result. Additionally, the researchers conducted two separate Tobit regression analyses – one to test how city-level black population size, general
income inequality, and racial income inequality were related to overall rates of police use of deadly force, and another to test how those same explanatory variables were related specifically to the rates of police killings of blacks.

For their first set of analyses, the results of their Tobit analyses revealed that only racial inequality was significantly and positively related to rates of police use of deadly force. Neither city-level black population size nor general income inequality were found to significantly predict changes in the overall rates of police use of deadly force. Interestingly, the results of their second set of analyses revealed that black population size was significantly and positively related to increased rates of police killings of blacks specifically. Thus, Jacobs and O’Brien’s study provides partial support for social threat theories. Unfortunately, because they did not examine how their explanatory variables might have been related to other, non-lethal, forms of police force, it is difficult to generalize their findings. That is, because police officers’ use of deadly force is restricted specifically to situations in which a dangerous felon is attempting to escape (Tennessee v. Garner, 471 U.S. 1, 1985) variation in Jacobs and O’Brien’s explanatory variables may differently influence other non-lethal, and less restricted, forms of police use of force. Nonetheless, Jacobs and O’Brien’s study is among one of the most methodologically sophisticated pieces of research examining police use of force to date, and is consequently one of the most often cited sources documenting (partial) support for social threat theories.

In another examination of rates of police use of deadly force, Sorensen and colleagues (1993) found much stronger support for the specific social threat theory they tested. Like Jacobs and O’Brien, Sorensen and colleagues utilized data from the FBI’s SHR on 170 cities with populations over 100,000, but instead examined rates of police use of deadly force over a period of five years (between 1980 and 1984). They also used 1980 U.S. census data to create their
measures of social threat, which included city-level measures of overall income inequality (they used the GINI index where 0 indicates no inequality and 1 indicates complete inequality), black population size, and number of individuals living in poverty. Rather than making political threat hypotheses, though, Sorensen and colleagues used traditional conflict theory to hypothesize that cities with high levels of income inequality and large populations of blacks and impoverished individuals would also have the highest rates of police use of deadly force.

Sorensen and colleagues used basic OLS regression analyses to test their hypotheses. Unfortunately, unlike Jacobs and O’Brien, Sorensen and colleagues did not adjust for the skewed distribution of their dependent variable. Instead, the researchers conducted a second set of analyses using only cities with populations over 250,000 which effectively circumvented their heteroskedasticity problem by removing a large number of the cities in their analysis that had reported zero incidents of police officers’ use of deadly force. In both sets of analyses, however, the researchers observed similar results. They found that regardless of city size, all three of their measures of social threat were significantly and positively related to police killings of civilians. Consequently, unlike Jacobs and O’Brien’s later study, Sorensen and colleagues’ study provides strong support for social threat theories.

The fact that Sorensen and colleagues did not observe findings similar to those obtained later by Jacobs and O’Brien (1998) is somewhat surprising, especially given that both sets of researchers used largely the same data. One possible explanation for the differences in observed results between the two studies was Jacobs and O’Brien’s use of Tobit regression techniques to correct for the skewed distribution of rates of police use of deadly force. However, Sorensen and colleagues conducted a separate set of analyses, in which they had essentially eliminated their heteroskedasticity problems, and still obtained significant results. The most likely explanation for
the differences between the two studies is therefore the different measures of social threat that were used in each study, as well as the different control variables that were included in the different analyses. That is, it is possible that Sorensen and colleagues found stronger support for social threat theories because they did not include any measures of racial income inequality as did Jacobs and O’Brien. Regardless of differences between the two studies, however, both studies provide at least some support for social threat theories of police use of force.

In the final theory-driven study of rates of police use of deadly force conducted in the last 20 years, Liska and Yu (1992) also found support for social threat theories. Unlike the two above studies, however, they used Vital Statistics data from the National Center for Health Statistics between 1975 and 1979 to construct their dependent variable. In a preemptive effort to avoid heteroskedasticity problems in their dependent variable, Liska and Yu included in their research only cities with populations of 250,000 or more, resulting in a total sample size of 45 cities. In addition to the Vital Statistics data, the researchers also constructed three social threat measures – city-level percent non-white, income inequality (the GINI index), and racial segregation (using a dissimilarity index to measure the percentage of whites who would have to move to another area to produce an even white to non-white population distribution). Based on what the researchers simply called “threat hypotheses,” Liska and Yu hypothesized that all three of their social threat measures would be positively related to rates of police use of deadly force.

Liska and Yu tested their hypotheses using two sets of structural measurement models – one using their full sample, and one disaggregated by the race of the victim. The researchers found that percent non-white and racial segregation were two of the strongest predictors in both sets of analyses, net of a variety of controls, once again providing support for social threat theories. Like Jacobs and O’Brien (1998), however, they found no effect of overall income
inequality, in either set of analyses. Consequently, based on the three studies reviewed thus far, it appears that aggregate-level measures of race/ethnicity are better predictors of rates of police use of deadly force than are measures of income inequality.

In addition to the findings discussed above, Liska and Yu also found that percent non-white and racial segregation predicted similar rates of police use of deadly force for both whites and non-whites (there was once again no significant effect of income inequality). This finding stands in stark contrast to the findings of Jacobs and O’Brien (1998) that were described earlier. One possible explanation for these differences between studies is the different data and measures utilized by each set of researchers. Whereas Jacobs and O’Brien utilized SHR data to analyze rates of police use of deadly force on whites versus blacks in 1980, Liska and Yu utilized Vital Statistics data to analyze rates of police use of deadly force on whites versus non-whites during the 1970s. Consequently, it appears that while blacks were more likely to be victims of police officers’ use of deadly force in 1980, racial/ethnic minorities as a whole (i.e. non-whites) were no more likely than whites to be the victim of police officers’ use of deadly force during the 1970s. Regardless of these differences, however, Liska and Yu’s research provides more (partial) support for social threat theories.

Moving away from the empirical tests of social threat theories that have focused on rates of police use of deadly force, Holmes (2000) examined how well social threat theories explained rates of police officers’ use of excessive force. Data for his dependent variable came from the Department of Justice’s (DOJ) Police Brutality Study (PBS), and measured the number of civilian complaints of police officers’ use of excessive force that were reported to the DOJ between 1985 and 1990 (Holmes used the terms “police use of excessive force” and “police brutality” interchangeably). All cities with populations of 150,000 or larger that also had
municipal police departments with at least two complaints of police use of excessive force annually were included in the study, for a total of 115 cities nationwide. Holmes’ dependent variable was therefore the rate of civilian complaints of police use of excessive force per 100,000 individuals in the cities served by each municipal police department. In addition to the data from the PBS study, he used census measures of city-level percent black, percent Hispanic, and racial income inequality (ratio of white to black and Hispanic median household incomes) to measure social threat.

Using the minority threat theory to guide him, Holmes then hypothesized that cities with larger black and Hispanic populations, and cities with higher levels of racial income inequality would have the highest rates of civilian complaints of police use of excessive force. Holmes tested his hypotheses using basic OLS regression analyses, and corrected for the non-normal distribution of his dependent variable through the use of Poisson estimation techniques. The results of his analyses revealed that that all three of his social threat measures were positively and significantly related to civilian complaints of police use of excessive force. Consequently, Holmes’ study is the first to provide support for social threat theories as explanations for non-lethal police use of force behaviors.

Unfortunately, though, Holmes was not able to distinguish between complaints made by members of minority groups and was therefore unable to determine whether or not the use of force by police was directed primarily at some groups in comparison to others. Moreover, because his dependent variable relied on civilian reports of police use of force, he may have had measurement error problems in his dependent variable (resulting from civilian reporting bias)
which could have affected his results.$^9$ Since Holmes did not account for the possibility of such problems, his results may subsequently be inaccurate. Nevertheless, because his results were generally consistent with the other tests of social threat theories that I have already reviewed, it is unlikely that civilian reporting bias significantly affected Holmes’ findings.

In another study examining police officers’ use of excessive force, Smith and Holmes (2003) attempted to replicate Holmes’ (2000) findings using more sophisticated analytic techniques and an additional set of control variables. Like, Holmes’ earlier study, Smith and Holmes used the PBS and census data to create their dependent variable (rate of civilian complaints of police use of excessive force) and the same three social threat measures (city-level percent black, percent Hispanic, and racial income inequality). They also used the minority threat theory to propose the same hypotheses that Holmes tested earlier. In this study, however, Smith and Holmes included a number of organizational control variables to determine whether or not the manner in which police departments handled civilian complaints of police use of excessive force might have influenced the rates of those complaints. Additionally, rather than conducting Poisson-based OLS regression analyses, the researchers conducted negative binomial regression analyses to correct for the over-dispersion of their dependent variable.

Despite the changes from Holmes’ (2000) earlier study, Smith and Holmes more refined test yielded generally similar results. That is, like Holmes (2000), the researchers found that both the city-level measures of percent black and percent Hispanic were positively and significantly related to rates of civilian complaints of police use of excessive force, net of a variety of controls, including the organizational measures related to how police departments handled civilian complaints. Unlike Holmes’ earlier findings, however, in their more controlled

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$^9$ While I also use civilian reports of police officers’ use of excessive force in this dissertation, I account for any potential measurement error in my dependent variable that might result from civilian reporting bias. For more details on how I account for civilian reporting bias, see my discussion in Chapter 5.
model, the researchers did not find a significant effect of racial income inequality. Thus, Smith and Holmes study once again provides support for social threat theories of police use of force, but questions the role of racial income inequality. Even so, Smith and Holmes’s study improves on Holmes’ (2000) earlier study, and provides more aggregate-level evidence that social threat theories can not only explain police officers’ use of deadly force, but other forms of police force as well.

**Individual-Level Evidence**

Although many of the more well-known social threat theories (e.g. conflict, racial/minority threat, and political threat theories) are aggregate-level explanations of police use of force, a number of studies have sought to test them using individual-level data. These studies typically argue that police officers’ (and society in general) tend to take a more punitive view toward, and generally have more negative conscious or unconscious biases against, racial/ethnic minorities, just as social threat theories would expect. Overall, the individual-level research on the topic has been supportive of social threat theories of police use of force.

Much of the individual-level support for social threat theories come from simulation studies in which researchers used computers to assess how civilians (i.e. individuals who had no affiliation with the police or any police training) reacted differently in their decisions to use force against whites versus racial/ethnic minorities. Unfortunately, since most of this research has not directly assessed use of force responses of actual police officers, the findings of these studies should be considered suggestive in nature only. Nevertheless, the individual-level social threat theories that are at the core of these simulation studies argue that many police officers (just like

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10 As previously mentioned, because it is difficult to operationalize and measure social class based solely at the individual-level, most of the individual-level empirical tests of social threat theories focus primarily on the effect of suspects’ racial/ethnic background.
all individuals within our society) may be prone to what some researchers have called “racial  
response bias” (Correll et al. 2002). According to these researchers, the concept of “racial  
response bias” explains how cultural stereotypes related to the negative perception of  
racial/ethnic minorities can manifest as involuntary or unconscious reactions that, given time,  
can be mediated. In the case of having to make split-second decisions in response to potentially  
dangerous situations, however, researchers expect that racial response bias may cause both police  
officers and civilians alike to be more likely to use force against minorities than against whites.11  
Thus, even though some of the studies reviewed below do not examine actual police officers’ use  
of force behaviors, they can shed light on how police officers might respond.

Much of the simulation research on race/ethnicity and the use of force has been  
conducted by Correll and colleagues (Correll et al. 2002; Correll et al. 2007; Correll et al. 2006).  
In their two earlier studies (Correll et al. 2002; Correll et al. 2006), Correll and colleagues drew  
samples of undergraduate students from the University of Colorado and paid them eight dollars  
or gave them partial course credit to participate in their study (approximately 40 students for  
each study). After gathering their respective samples, the researchers then had the students play  
simple computer video games that presented the students with images of armed or unarmed  
images of white or black individuals. In the games, the researchers instructed the students to act  
as if they were police officers and to push a “shoot” button if the individual in the game was  
armed with a gun, or to push a “don’t shoot” button if the individual was holding some other  
non-weapon item (e.g. a bottle, cell phone, or wallet). Then, based on the racial response bias

11 Some might argue that some of the negative stereotypes about racial/ethnic minorities that persist in our culture  
may cause individuals to view them as being more dangerous. While such an argument might sound like it belongs  
in the criminal threat theories section that I discuss later in this chapter, racial response bias arguments are  
inherently social threat theories. Social threat theorists would argue that the powerful groups in our society have the  
ability to influence how the racial/ethnic minority groups that pose a threat to the status quo should be viewed by  
the public (i.e. they can help perpetuate, if not actually create, the negative stereotypes surrounding racial/ethnic  
minorities). Subsequently, if conscious or unconscious racial response bias does affect individuals’ decisions to use  
force, such evidence would be supportive of social threat, rather than criminal threat, theories.
argument discussed above, Correll and colleagues hypothesized that the students in each study would be more likely to incorrectly choose to “shoot” at images of black individuals holding non-weapon items than images of white individuals holding non-weapon items, and that they would be more likely to correctly choose to “shoot” images of black individuals with guns than images of white individuals with guns.\(^{12}\)

In both studies, Correll and colleagues (Correll et al. 2002; Correll et al. 2006) found support for their racial response bias hypotheses. Using simple analysis of variance (ANOVA) techniques in each study, they observed that, on average, students were more likely to make the correct decision to “shoot” armed blacks, but were also more likely to incorrectly decide to “shoot” unarmed blacks. While the results of these studies are compelling, because the researchers used a non-random sample of compensated civilians, it is difficult to generalize their results to how actual, trained, police officers might behave in real world situations. Additionally, the researchers only looked at the mean differences between outcomes (i.e. armed vs. unarmed and black vs. white), instead of conducting multivariate analyses in which they could have accounted for the effects of a variety of control variables (e.g. student participants’ demographic characteristics). Subsequently, while the findings of these two earlier studies by Correll and colleagues are supportive of social threat theories in general, they have a number of significant limitations that make the findings less compelling.

Around the same time that Correll and colleagues began their simulation tests, Greenwald and colleagues (2003) began a very similar study in a different part of the country. Greenwald

\(^{12}\) Correll and colleagues’ “racial response bias” hypotheses are consistent with social threat theories, in that the researchers expect that many individuals within our society (obviously including, but not limited to just to police officers and university students) have unconscious biases toward racial/ethnic minorities which make them more prone to perceive them negatively. As a result of this racial response bias, Correll and colleagues expect that racial/ethnic minorities should be more likely to experience all types of formal social control, including police officers’ use of deadly force.
and colleagues recruited 160 University of Washington undergraduate students to take part in a “virtual-reality weapons task” computer game. Unfortunately, the researchers provide no other information on how they obtained their sample (i.e. sampling strategy, compensation, etc.). They do describe their virtual-reality game in some detail, however. Students were instructed to act as if they were undercover police officers and push a “shoot” button as quickly as possible if the individual displayed in the game was dangerous (i.e. holding a gun), push a safety button if the individual was a fellow officer, or to do nothing if the individual was a non-dangerous civilian (i.e. not holding a gun).

Based on the same racial response bias argument made by Correll and colleagues (Correll et al. 2002; Correll et al. 2006), Greenwald and his colleagues then hypothesized that their sample of students would similarly be more likely to incorrectly choose to “shoot” unarmed black individuals than they would be to correctly shoot armed white individuals. Unfortunately, Greenwald and colleagues did not explicitly discuss their analytic strategy. However, based on simple comparisons of the proportions of whites incorrectly shot vs. blacks incorrectly shot, the researchers found that both white and black students were more likely to incorrectly choose to shoot unarmed black individuals in their game. Thus, Greenwald and colleagues’ study largely replicated those conducted by Correll and colleagues. Not surprisingly then, Greenwald and colleagues’ study suffers from many of the same limitations. Because they used university students, and because their sampling and analytic strategies were not discussed, generalizing their results to how actual, trained, police officers might act in similar real-world situations is once again problematic.

Fortunately, the most recent simulation study by Correll and colleagues (Correll et al. 2007) addressed many of the limitations of not only Greenwald and colleagues’ study (2003), but
of their own earlier studies as well (Correll et al. 2002; Correll et al. 2006). For this study, the researchers recruited both civilians and two separate groups of sworn police officers to participate in their computer simulation experiments. They randomly recruited 135 civilians to voluntarily participate in the study with the help of Colorado Department of Motor Vehicle (each participant was paid $20). Then, in addition to their civilian sample, Correll and colleagues also recruited 124 officers from the Denver Police Department and 113 officers from across the country who were in Denver for a training seminar to voluntarily participate as well (each officer was paid $50). All 372 civilian and police officer participants were then instructed on how to play the same computer game simulation that Correll and colleagues used in their earlier studies (Correll et al. 2002; Correll et al. 2006).

Once again, based on the racial response bias argument, Correll and colleagues (2007) hypothesized that all participants (i.e. both civilians and the two groups of police officers) would be more likely to incorrectly choose to “shoot” images of non-threatening blacks than non-threatening whites. For this study, however, they also hypothesized that the two groups of police officers would be less likely to make mistakes for both images of threatening and non-threatening whites and blacks (i.e. incorrectly choose to “shoot” images of unarmed individuals or incorrectly choose not to “shoot” images of armed individuals). After all their participants had played the game, Correll and colleagues once again conducted ANOVA tests. This time, however, they also examined the correlations between their findings and three city-level contextual variables – the total population of the cities in which the participants lived, the rates of violent crime for those cities, and the black population size for each city. Based on their ANOVA analyses, Correll and colleagues found support for both of their hypotheses. That is, they found that both their civilian participants and the two groups of police officers were more
likely to incorrectly choose to “shoot” non-threatening blacks than non-threatening whites. They also found that the police officers were less likely to make mistakes than were the civilian participants. Then, based on their correlational analyses, they found that the mean level of incorrect “shootings” of blacks was correlated with participants who lived in larger cities, cities with higher violent crime rates, and cities with larger black populations.¹³

Through their examination of both civilians’ and police officers’ decisions to shoot in their computer simulation game, as well as their correlational analysis of city-level context, Correll and colleagues’ most recent study provides relatively stronger support for social threat theories than their two earlier studies. Unfortunately, their study could still be improved methodologically by drawing random samples of police officer participants to reduce the potential for selection effects (i.e. certain types of police officers being more willing to participate in studies on the use of force). Additionally, by conducting multivariate regression analyses, Correll and colleagues could more accurately determine how all participants’ decision to “shoot” might be influenced not only by race, but by a host of other control factors as well. Despite the methodological shortcomings of all of the simulation studies that I have just reviewed, however, the studies provide support for the social threat theories.

Moving away from simulation studies, there has been only one other individual-level, theory-driven study which has provided support for social threat theories. Worden (1996) conducted a police officer-civilian encounter-based analysis of police officers’ use of excessive force. He utilized systematic observational data of police-civilian encounters from the 1977 Police Services Study (PSS). In the PSS, trained observers accompanied police officers on 900 patrol shifts across 24 police departments in three metropolitan areas (Rochester, New York, St.

¹³ Correll and colleagues’ research therefore also supports criminal threat theories. For more details on how criminal threat theories apply to this study, see my review below.
Louis, Missouri, and the Tampa-St. Petersburg, Florida area). During those 900 shifts, observers recorded 5,688 police officer-civilian encounters, of which police officers used excessive force in approximately 74 times (1.3% of all encounters). Then, based on Black’s theory of law (Black 1976), Worden hypothesized that racial/ethnic minorities would be more likely to experience excessive use of force by the police.

Worden conducted both bivariate and multivariate logistic regression analyses and determined that black civilians had greater odds of experiencing the use of excessive force by the police than whites, net of a number of controls including civilians’ mental condition (i.e. signs of inebriation or mental illness, the carrying of a weapon, physically resisting police instructions, and having negative demeanor toward the officers). Based on these results, he concluded that “officers are, on average, more likely to adopt a punitive or coercive approach to black suspects than they are to white suspects” (37), thereby supporting the arguments made by social threat theorists. Consequently, Worden was the first to actually demonstrate that police officers were more likely to use force on racial/ethnic minorities in real-life encounters, once again providing strong support for individual-level social threat theories.

The last empirical test of an individual-level social threat theory that I review here is the only theory-driven study in the last twenty years to find no relationship between race and police use of force. A few years before the simulation studies reviewed earlier became popular, Dwyer and colleagues (Dwyer et al. 1990) conducted a simple analysis of 60 crime scene vignettes to assess how police officers responded to a variety of individual and situational characteristics, including suspect’s race/ethnicity. They enlisted 142 officers from the Shelby County Sheriff’s Office (Memphis, Tennessee) to voluntarily participate in their study (officers received no compensation). Each officer was given a booklet with 60 crime scene vignettes and was asked to
decide whether they would 1) not draw their weapon, 2) draw, but not aim or fire their weapon, 3) draw and aim their weapon, but not fire it, or 4) draw their weapon, aim it, and shoot the suspect in the vignette. Across the vignettes a number of factors were manipulated, including the suspect’s race (white vs. non-white). Based on what they called “social script theory” (similar to the racial response bias argument made by Correll and colleagues [Correll et al. 2002; Correll et al. 2006; Correll et al. 2007]), Dwyer and colleagues hypothesized that the officers would be more likely to choose to shoot black suspects because of negative social scripts typically associated with racial/ethnic minorities in our society.

Dwyer and colleagues conducted multivariate OLS regression analyses to test their hypothesis and unexpectedly found that suspects’ race was not related to officers’ decisions to “shoot their weapon,” even at a relaxed level of significance (p > 0.25). Thus, unlike the simulation studies reviewed earlier, Dwyer and colleagues found that police officers were no more likely to use force on non-whites than on whites. Unfortunately, however, due to the nature of their study, it is unlikely that researchers were truly able to get at any automatic (unconscious) biases that might have affected the officers’ decisions to shoot. That is, since the participants were able to take their time to choose their response – rather than being required to respond quickly to a computer simulation – the officers could have simply given the response they thought was more socially desirable or politically correct (i.e. making the decision regardless of race). So, where the simulation studies might have been better able to measure unconscious police bias against minorities, Dwyer and colleagues study most likely did not. Consequently, as theirs was the only theory-driven test of social threat theories to find no significant relationship between race and police officers’ decisions to use force, the viability of social threat theories in general should not seriously be called into question.
Summary of the Evidence for Social Threat Theories

Overall, based on the aggregate- and individual-level empirical tests reviewed above, the social threat theoretical framework appears to be an adequate, if not strong, explanation for all forms of police use of force. Table 2.1 at the end of this chapter displays condensed summaries for each of the studies reviewed above. Reported in the table are the specific theories or hypotheses that were tested, the data and methods that were used, and the general findings of each of the studies. In addition to this table, I also briefly summarize and discuss some of the most significant and interesting findings of the empirical tests of social threat theories here.

First, based on my review of the theory-driven studies testing social threat theories over the past twenty years, both aggregate- and individual-level measures of social threat have been strongly and consistently related to many forms of police use of force, with one exception (Dwyer et al. 1990). As most of the aggregate-level tests revealed, measures of race/ethnicity appear to be much better predictors of rates of police use of force than are measures of social class (i.e. income inequality). An interesting consequence of these findings is the need for research on why threats from lower social class groups are not met with as much police use of force as threats from racial/ethnic minorities. Have changes in the economic structure of the U.S. affected how level of threat presented by lower social class groups is perceived by the powerful elites? Or, perhaps, has the demographic composition of the U.S. changed enough to make racial/ethnic minority groups even more threatening? Future research that examines how the powerful groups in our society perceive threat may help us answer such questions.

In addition to the finding that lower social class groups may not present as much of a social threat as once thought, the general consensus of the studies reviewed above was that blacks are much more likely to be the recipients of police use of force, not only compared to
whites, but also compared to other racial/ethnic minorities. Thus, based on both the aggregate- and individual-level research, it appears that blacks are perceived to present the most social threat to the powerful groups in our society. However, with the recent increases in the U.S. Hispanic population, it should be interesting to learn whether or not Hispanics begin, or already have begun, experiencing similarly high levels of police use of force. Is there a threshold at which the relative size of a racial/ethnic minority group begins to present enough of a social threat that the group’s members also begin to experience more formal social control? In order to determine if such a threshold exists, future research should continue to examine how different racial/ethnic minorities differentially experience police use of force.

In the next section of this chapter, I review the basic arguments made by theories that fall under the broader criminal threat theoretical framework, as well as the empirical tests of those theories. In comparison to the empirical research testing social threat theories, the empirical tests of criminal threat theories have found strong support both at the aggregate- and individual-levels much more consistently.

**Criminal Threat Theories**

Criminal threat theories argue that police officers use force in response to threatening (or perceived to be threatening) individuals, situations, and environments. Rather than attempting to use force in order to control groups or individuals who pose a social threat to the powerful groups in our society, criminal threat theories contend that police officers will use force on anyone, powerful or powerless, that poses a criminal threat to their own safety, or the safety of other officers or civilians. Among the theories included under the broader heading of the criminal threat theoretical framework are the danger perception theory (Fyfe 1980; MacDonald,
Kaminski, Alpert, and Tennenbaum 2001; MacDonald, Alpert, and Tennenbaum 1999; Sherman and Langworthy 1979), community threat or community violence theories (Holmes, Reynolds, Holmes, and Faulkner 1998; Kania and Mackey 1977; Sorenson et al. 1993), and what have been generally referred to as threatening acts hypotheses (Holmes 2000; Liska and Yu 1992; Smith and Holmes 2003; Worden 1996).

MacDonald and colleagues (2001) best described the general criminal threat argument by discussing not only how real, physical, and immediate threats were related to police officers’ use of force, but also how perceived threats could also influence officers’ use of force. They argued that “the level of police use of deadly force is contingent on the danger police officers experience (real or perceived) . . . [and that] police officers are more likely to use deadly force during time periods when (or in places where) they encounter greater levels of violence or view their jobs as being particularly hazardous” (159, emphasis added). And, even though MacDonald and his colleagues were referring specifically to deadly forms of police force, the danger-perception theory they used, and criminal threat theories in general, all contend that police officers should be more likely to use all types of force (legitimate and illegitimate, lethal and non-lethal) when they receive direct threats to their own safety (e.g. when dealing with an armed criminal suspect), and also when they perceive a potential threat as well (e.g. when working in an area with high levels of criminal activity).

Empirical Tests of Criminal Threat Theories

Empirical tests of criminal threat theories have used a variety of measures that assess criminal activity at the aggregate-level, and criminal dangerousness at the individual-level. Among the aggregate-level measures that are most commonly used in the literature are rates of
public violence (i.e. riots and violent protests), rates of homicides or other crimes, and even rates of arrest. At the individual-level, criminal threat measures often include individuals’ aggressive and violent behaviors toward police officers (i.e. resisting arrest or physically attacking an officer), the presence and/or displaying of a weapon, and the individual’s mental state (i.e. being mental ill or being under the influence of a controlled substance). Using these measures, criminal threat theorists expect that both aggregate-level rates of criminal activity and individual-level measures of dangerousness should be positively related to police use of force behaviors. Not surprisingly, the majority of researchers who have conducted empirical tests of criminal threat theories have found just that.

Below, I review all of the aggregate- and individual-level empirical tests of criminal threat theories over the past twenty years. Before doing so, however, let me quickly review the general findings of some of the older tests of criminal threat theories. Aggregate-level studies have consistently shown that areas with high rates of criminal activity also tend to have high rates of police use of force (e.g. Binder and Scharf 1982; Fyfe 1980; Jacobs and Britt 1979; Kania and Mackey 1977; Sherman and Langworthy 1979; Sherman 1986). Similarly, individual-level tests have found that police officers who dealt with more real or perceived criminals, violence, and dangerous crimes in general were significantly more likely to use force (e.g. Binder and Fridell 1984; Binder and Scharf 1982; Copeland 1986; Horvath 1987). Historically, then, there is strong support for criminal threat theories of police officers’ use of force. More recent empirical tests have been just as consistently supportive. In the next two sections of this chapter, I review the empirical tests of criminal threat theories, first at the aggregate-level, then at the individual-level.
Aggregate-Level Evidence

MacDonald and colleagues (MacDonald et al. 1999) were the first researchers to consider how aggregate-level rates of crime temporally corresponded with rates of police use of force. Specifically, they examined how high rates of homicide across the nation covaried with high rates of police use of deadly force over time. They used national homicide data from the FBI’s SHR for every month between the years 1976 and 1986 to conduct their study. Based on the danger perception theory, they argued that increases in rates of certain types of homicides would cause police to perceive higher levels of danger in their jobs which would subsequently lead to higher rates of police officers’ use of deadly force in the same month that the homicide rates spiked. Specifically, they believed that police officers would be most likely to react with deadly force when rates of robbery-related and justifiable civilian homicides (i.e. when a civilian uses deadly force to protect him or herself or loved ones) were high because the circumstances of those types of homicides were strong indicators of potentially dangerous situations in which police officers might also become involved.

In order to test their hypotheses, MacDonald and colleagues conducted an autoregressive integrated moving average (ARIMA) time series analysis that allowed them to control for the high level of temporal autocorrelation across their 132 months of homicide data. The results of their analyses revealed that during months in which both the rates of robbery-related and justifiable civilian homicides were high, rates of police officers’ use of deadly force were also high. Based on these findings, the researchers concluded that “the incidence of police use of deadly force closely follow[ed] the dangerousness of particular time periods” (162). Such a conclusion strongly supports threat theories. Unfortunately, however, while MacDonald and colleagues’ research was methodologically sound, they did not include any other covariates in
their ARIMA models to determine if there might have been alternative explanations for their findings. So, even though MacDonald and colleagues were able to demonstrate a temporal relationship between crime rates and police officers’ use of deadly force, they acknowledged that further research was necessary that included control variables and predicted other forms of police use of force.

In a follow-up to their earlier study, MacDonald and colleagues (2001) reexamined how national homicide rates affected rates of police use of deadly force over time. This time, however, they expanded the time frame of their analysis to include 21 years (1976 – 1996), and conducted additional stationarity tests to make sure that their findings were as reliable as possible. Monthly homicide data, including their dependent variable (police officers’ use of deadly force) once again came from the FBI’s SHR. As with their earlier study, they hypothesized that increases in robbery-related and justifiable civilian homicides should be related to increases in police officers’ use of excessive force. Then, in an effort to increase the reliability of their estimates, MacDonald and colleagues not only conducted another set of ARIMA time series analyses, they also conducted several stationarity tests. These stationarity tests allowed them to determine whether the processes driving the temporal relationships between their homicide rate measures and police use of deadly force were invariant over time (i.e. stationary) or if those processes changed over time (i.e. non-stationary). Had their tests revealed that the relationships were non-stationary, MacDonald and colleagues could not be certain that some other unmeasured temporal covariate was influencing both their independent and dependent variables (i.e. the relationships were spurious). Fortunately, the results of their stationarity tests indicated that the relationships they observed were indeed stationary over time, lending additional support to their substantive findings. And, as they had observed in their
earlier study, MacDonald and colleagues again found that periods of time which had higher rates of robbery-related and justifiable civilian homicides also tended to have higher rates of police use of deadly force.

Thus, in two separate studies, MacDonald and colleagues found strong support for criminal threat theories. Moreover, in their more recent study (MacDonald et al. 2001), the researchers also improved on their methodology and were therefore able to present more reliable findings. Unfortunately, however, while their stationarity tests revealed that the relationship that they had observed was not spurious, they still did not include any other covariates in their models to test for alternative explanations of changes in police use of deadly force over time. Furthermore, based on both studies by MacDonald and colleagues, which focused exclusively on police officers’ use of deadly force, the capacity of criminal threat theories to explain non-lethal police force is still unclear.

Fortunately, other studies have shown that criminal threat measures are capable of not only explaining other, non-lethal forms of police force, but that they are also robust to the inclusion of a variety of control variables. In a recent study in which MacDonald also took part, Alpert and MacDonald (2001) found that violent crime rates positively and significantly predicted police officers’ use of all types of force (i.e. both lethal and non-lethal). Data for their study came from an unidentified national PERF survey of law enforcement agencies that took place in 1998. A total of 265 agencies provided official data on police use of force incidents during the 1996 calendar year, which the researchers then used to calculate rates of police use of force per 100,000 individuals in the areas served by each agency. The violent crime rates for those areas served as the primary measure of aggregate-level criminal threat. Then based on a general criminal threat argument (they cited both the “reactive hypothesis” utilized in
MacDonald and colleagues’ earlier studies [1999; 2001] and the “community violence hypothesis” that other researchers have used, e.g. Sorensen et al. 1993), Alpert and MacDonald hypothesized that areas with high rates of violent crime should also have high rates of police use of all types of force.

To test their hypothesis, Alpert and MacDonald (2001) conducted basic bivariate and multivariate OLS regression analyses. In order to account for the skewed distribution of their police use of force variable (toward zero), they calculated its natural log for use in their analyses (see Jacobs and O’Brien 1998 for precedent). The results of their bivariate analyses revealed that increases in the rates of violent crime were strongly associated with increases in police use of force. Alpert and MacDonald then found that their violent crime rate measure was also the strongest predictor of police use of force of all the covariates included in their multivariate analysis. Subsequently, as a result of their focus of all forms of force used by the police, and because of their inclusion of a variety of control measures, Alpert and MacDonald’s results are not only more generalizable and robust, they also provide stronger evidence than previous studies in support of criminal threat theories of police use of force.

The inherent logic of criminal threat theories has not escaped social threat theorists. As such, many of the theory-driven empirical tests of criminal threat theories at both the aggregate- and individual-levels have included at least one measure of criminal threat. It should therefore not be surprising that all five of the aggregate-level social threat studies that were reviewed above also included measures of criminal threat and accordingly proposed and tested various criminal threat theories as well (Holmes 2000; Jacobs and O’Brien 1998; Liska and Yu; Smith and Holmes 2003; Sorensen et al. 1993). To conserve space, I do not review all the specific details of each study again. Instead, I simply review the researchers’ measure(s) of criminal
threat, the specific criminal threat theories guiding their hypotheses, and their findings as they relate to the broader criminal threat theoretical framework. Following the same order I used to review the studies above, I begin with Jacobs and O’Brien’s (1998) study of police use of deadly force.

For their criminal threat measure, Jacobs and O’Brien (1998) used city-level rates of homicides obtained from the FBI’s UCR to test the same “reactive hypothesis” that MacDonald and colleagues (1999) utilized. Specifically, they hypothesized that the rates of homicides in cities across the nation would be positively related to rates of police use of deadly force. The results of their Tobit analyses revealed that homicide rates indeed predicted police officers’ use of deadly force, net of the social threat measures that they had also found to be related to rates of police use of deadly force. Consequently, because their study was one of the more methodologically sound empirical analyses of police officers’ use of force, Jacobs and O’Brien’s findings provide very strong evidence not only in support of social threat theories, but in support of criminal threat theories as well.

Sorensen and colleagues’ (1993) used city-level violent crime rates (i.e. rates of homicide, rape, robbery, and aggravated assault) obtained from the FBI’s UCR to test what they referred to as the “community violence” hypothesis. They hypothesized that increases in violent crime rates should increase the level of dangerousness perceived by police officers, which should then result in increased rates of police use of deadly force. The results of Sorensen and colleagues OLS regression analyses revealed that rates of violent crime significantly predicted rates of police officers’ use of deadly force, net of their social threat measures, just as Jacobs and O’Brien (1998) found. Moreover, they found that city-level violent crime rates were the strongest predictors of police use of deadly force in all of their models. Thus, even though
Sorensen and colleagues’ study was not as methodologically sound as Jacobs and O’Brien’s study (see review above), it provides additional evidence in support of criminal threat theories of police officers’ use of force.

Liska and Yu (1992) also found support for criminal threat theories. They utilized three measures of criminal threat, all of which were obtained from the FBI’s UCR: the overall rate of index crimes (homicide/non-negligent manslaughter, rape, robbery, aggravated assault, larceny, burglary, automobile theft, and arson), rates of violent crimes only (the first four index crimes listed previously), and rates of homicides alone. Based on the threatening acts theory, they hypothesized that all three measures should be positively related to rates of police use of deadly force. The results of their structural-measurement models revealed that only the rates of homicides were significantly related to their dependent variable, but that it had one of the strongest effects. However, because the researchers included three crime rate measures simultaneously, it should not be surprising that the one measure of criminal threat that was likely to be perceived as the most dangerous (i.e. the homicide rate) was also the only measure to significantly predict rates of police use of deadly force. Thus, like the two previous studies of police use of deadly force (Jacobs and O’Brien 1998; Sorensen et al. 1993), Liska and Yu’s research provides further support for criminal threat theories.

The last two studies that empirically tested both social threat and criminal threat theories were Holmes’ (2000) and Smith and Holmes’ (2003) examinations of civilian complaints of police use of excessive force. Because the latter study was a replication of the former, and because they used the same criminal threat measures to obtain the same results, I review them together here. For both studies, the researchers followed Liska and Yu (1992) and used the same index crime rate measure (again obtained from the FBI’s UCR) and the same threatening acts
theory as the basis for their hypothesis. Unexpectedly, however, in both studies, the researchers found no significant relationship between index crime rates and civilian complaints of police use of excessive force. If one takes into account Liska and Yu’s (1992) earlier findings, however, one possible explanation for the researchers’ two null finding emerges. While Liska and Yu found that overall index crime rates were not significantly related to police officers’ use of deadly force, they did find that homicide rates alone significantly predicted variation in their dependent variable. Based on these differences in findings then, it appears that the homicide rate alone is the best predictor of the level of criminal threat perceived by police officers. Despite their null findings then, the two pieces by Holmes (2000) and Smith and Holmes (2003) shed more light on how police might perceive criminal threat differently for different types of crime.

Thus, with the exception of the last two studies reviewed above, the aggregate-level evidence in support of criminal threat theories of police use of force is quite strong. And, even the latter two studies were useful in that their results suggest that the homicide rate, in particular, may be the most important aggregate-level measures of criminal threat. In the following section, I review the empirical tests of individual-level criminal threat theories over the last 20 years.

**Individual-Level Evidence**

Individual-level criminal threat theories generally argue that police officers should be more likely to use all types of force when they perceive a criminal threat to their own safety or the safety of others. Accordingly, rather than studying how rates of crime influence rates of police use of force, individual-level tests of criminal threat theories typically examine how police officers use force during specific encounters with criminals or other potentially dangerous individuals.
Holmes, Reynolds, Holmes, and Faulkner (1998) examined the factors that influence how police officers perceive criminal threat. Although their study did not actually test police use of force behaviors, Holmes and colleagues’ research is valuable in that it helps shed light on the aspects of a police officer-civilian/criminal encounter that may lead police officers to perceive a threat and subsequently use force to prevent the escalation of the situation, or to gain control of the situation once it has already become dangerous. In order to determine what aspects of an encounter would cause police officers to use force, the researchers surveyed 662 sworn police officers from the London, Ohio, police training academy. In the survey, the officers were presented with a number of vignettes describing a variety of potentially threatening scenarios. Holmes and colleagues then asked the officers to decide whether the circumstances described in each vignette were threatening enough to necessitate the use of force. Based on what they called a “threat presentation” hypothesis, the researchers expected that officers would be more likely to perceive a threat that necessitated the use of force when the other individuals in the vignette were mentally or emotionally unstable, were suspected of committing a serious crime, or physically resisted arrest.

Unfortunately, Holmes and colleagues (1998) do not clearly specify how they analyze the data they obtained from their survey, but based on the presentation of their results, it appears that they conducted a number of multivariate OLS regression analyses. Whatever their actual analytic strategy, Holmes and colleagues found that scenarios in which individuals resisted arrest were the most likely to cause officers to perceive a threat serious enough for them to use force. Additionally, scenarios in which individuals had committed a more serious crime also caused officers to perceive enough threat to use force. Surprisingly, however, in scenarios where the officers encountered mentally or emotionally unstable individuals, the officers did not perceive
enough of a threat that they felt they had to use force. Holmes and colleagues’ study therefore provides some initial support for criminal threat theories, but because they do not examine police officers’ actual use of force behaviors, and because their methods are unclear, no definitive conclusions should be drawn from their results.

Somewhat surprisingly, Holmes and colleagues’ study was the only individual-level research in the last twenty years that tested a criminal threat theory exclusively. As a result, nearly all of the empirical tests of individual-level social threat studies that were reviewed above also included tests of criminal threat theories. To conserve space, I again do not review the data and methods of those studies in detail. Instead, as I did above, I review only the researchers’ measure(s) of criminal threat, the specific criminal threat theories guiding their hypotheses, and their findings as they relate to support for criminal threat theories. I begin by jointly reviewing all four simulation studies that utilized the same measures and specific theory, and additionally observed similar findings.

In each of the four simulation studies reviewed earlier in this chapter (Correll et al. 2002; Correll et al. 2006; Correll et al. 2007; Greenwald et al. 2003), the researchers presented their participants with images not only of individuals of different racial/ethnic backgrounds, but also images of individuals from all backgrounds with or without weapons. Based on the threatening acts theory, the researchers all hypothesized that their participants (both civilians and police officers) would be more likely to correctly and more quickly choose to “shoot” images of individuals holding weapons in comparison to individuals holding other non-weapon items (e.g. bottles, phones, or wallets). Not surprisingly, the researchers all found that their civilian participants were more likely to make more correct decisions faster when they were presented with images of individuals holding weapons (Correll et al. 2002; Correll et al. 2006; Greenwald
et al. 2003), although police officers responded more quickly and were correct more often in comparison to their civilian counterparts (Correll et al. 2007). The results of all four simulation studies were therefore supportive of not only social threat theories of police use of force, but criminal threat theories as well.

The final empirical test of both social threat and criminal threat theories was Worden’s (1996) study of police officers’ use of excessive force. Unlike the studies reviewed above, Worden utilized systematic observational data from the PSS. Therefore, he was able to test how criminal suspects’ actual behavior influenced police officers’ use of force. Again using the threatening acts theory, he hypothesized that suspects who physically resisted arrest or tried to attack officers would have greater odds of having excessive levels of force used on them. The results of his logistic regression analyses revealed that suspects whose actions toward police officers were threatening had greater odds of experiencing police use of excessive force, net of a variety of other control measures, including many of the social threat measures described earlier in this chapter. Thus, Worden’s study substantiates the findings of the simulation studies reviewed above, and suggests that criminal threat at the individual-level is indeed a strong explanation of police officers’ use of force.

Summary of the Evidence for Criminal Threat Theories

All together, the aggregate- and individual-level empirical tests that were reviewed above suggest that the criminal threat theoretical framework is not just a viable explanation for police officers’ use of force, but a very powerful explanation as well. Table 2.1 below presents brief summaries for each of the studies empirically testing criminal threat studies that were reviewed in this section. Again, for each study, the table displays the specific criminal threat theories that
drew the analyses, the data and methods that were utilized, and the general findings that were observed. Here, I briefly review some of the more interesting findings in regards to the empirical tests of criminal threat theories.

First, in many of the studies that tested both social threat and criminal theories, the measures of criminal threat were often among the strongest in the researchers’ analyses. Although a few studies found no significant effects of criminal threat measures when controlling for social threat measures (e.g. Holmes 2000; Smith and Holmes 2003), most studies found that measures of criminal threat better predicted police officers’ use of force than did social threat measures. This should not be surprising since criminal threats are more likely to have an immediate and potentially dangerous impact on the well-being of police officers than are social threats, which may not directly affect police officers at all. Furthermore, police officers are legally justified to use of force when they (or others) are physically threatened (i.e. presented with a criminal threat), but are not when they (or others) are socially threatened. Thus, future research should continue to simultaneously study both theories to further disentangle the direct and indirect relationships between social threat measures, criminal threat measures, and police officers’ use of force. Does the social threat that racial/ethnic minorities present lead to them becoming viewed as more of a criminal threat? What is the nature of the threat experienced by officers that leads them to use force? Answers to such questions may help us better understand the processes which lead police officers to use force.

Second, based on some of the aggregate-level studies reviewed above, it appears that homicide rates may be much better indicators of criminal threat than other rates of crime. This suggests that police officers are more likely to use force when they perceive more serious or harmful criminal threats. Research should therefore explore how the threat associated with
different types of crimes influence police officers’ behaviors. If the overall homicide rate is one of the best predictors of police use of force because it causes officers to be more concerned about their own well-being, what effect might rates of assaults on police officers, or rates of homicides of police officers, have on their use of force behavior? In areas where rates of assaults and homicides of police officers are high, will officers be more likely to use force to preempt more attacks? Will they use more force in retaliation to attacks on their fellow officers? Future research should attempt to answer these questions so that we may better understand how police officers’ perceive and respond to different kinds of criminal threats.

Finally, while a number of aggregate-level tests of criminal threat theories have demonstrated that crime rates can influence rates of police use of force, the majority of the theory-driven research at the individual-level has come from simulation studies. There have been a number of non-theory-driven studies at the individual- or encounter-level that have included measures of criminal threat (e.g. resisting arresting, assaulting officers) and show that police officers are more likely to use force when presented with a criminal threat (e.g. Garner et al. 2002). Unfortunately, in many of those studies, researchers had simply included as many variables in their analyses as they had available to them, with no theoretical expectations for what they might observe. Consequently, it is difficult to interpret their results in terms of theory. More individual-level, theory-driven, empirical tests of criminal threat theories are therefore needed that utilize other forms of data to determine how and why police officers respond to real-world criminal threats in the ways that they do.

In the last section of this chapter, I review Klinger’s (1997) ecological theory of police vigor – one of the only new theories of police behavior that has been proposed in recent years. Unfortunately, because no empirical tests of his theory have been conducted, it is difficult to
Table 2.1. Theory-Driven Studies of Police Use of Force During the Last 20 Years

Aggregates-Level Studies

<table>
<thead>
<tr>
<th>Author(s)</th>
<th>Data</th>
<th>Method(s)</th>
<th>Specific Theory Tested</th>
<th>Significant Findings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alpert &amp; MacDonald (2001)</td>
<td>Unidentified Police Executive Research Forum Study</td>
<td>OLS Regression</td>
<td>Reactive/Community Violence (CT)</td>
<td>Violent Crime Rate → (+) Rates of Police Use of Force</td>
</tr>
<tr>
<td>Holmes (2000)</td>
<td>Police Brutality Study; U.S. Census Report; Uniform Crime Report</td>
<td>Poisson-Based OLS Regression</td>
<td>Minority Threat (ST); Threatening Acts (CT)</td>
<td>Percent Black → (+) Percent Hispanic → (+) Racial Income Inequality → (+) Rates of Police Use of Excessive Force</td>
</tr>
<tr>
<td>MacDonald, Alpert, &amp; Tennenbaum (1999)</td>
<td>Supplementary Homicides Report</td>
<td>Autoregressive Integrated Moving Average Time Series</td>
<td>Danger Perception (CT)</td>
<td>Robbery-Related Homicide Rate → (+) Justifiable Civilian Homicide Rate → (+) Rates of Police Use of Deadly Force</td>
</tr>
<tr>
<td>MacDonald, Kaminski, Alpert, &amp; Tennenbaum (2001)</td>
<td>Supplementary Homicides Report</td>
<td>Autoregressive Integrated Moving Average Time Series; Stationarity Tests</td>
<td>Danger Perception (CT)</td>
<td>Robbery-Related Homicide Rate → (+) Justifiable Civilian Homicide Rate → (+) Rates of Police Use of Deadly Force</td>
</tr>
<tr>
<td>Sorensen, Marquart, and Brock (1993)</td>
<td>Supplementary Homicides Report; U.S. Census Report; Uniform Crime Report</td>
<td>OLS Regression</td>
<td>Conflict (ST); Community Violence (CT)</td>
<td>Percent Black → (+) Percent in Poverty → (+) Income Inequality → (+) Violent Crime Rate → (+) Rates of Police Use of Deadly Force</td>
</tr>
</tbody>
</table>

Notes: CT = Criminal Threat; ST = Social Threat
Table 2.1. Theory-Driven Studies of Police Use of Force During the Last 20 Years, Contd.

### Individual-Level Studies

<table>
<thead>
<tr>
<th>Author(s)</th>
<th>Data</th>
<th>Method(s)</th>
<th>Specific Theory Tested</th>
<th>Significant Findings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Correll, Park, Judd, &amp; Wittenbrink (2002)</td>
<td>University of Colorado</td>
<td>Analysis of Variance</td>
<td>Racial Response Bias (ST); Threatening Acts (CT)</td>
<td>Black Suspect → (+) Presence of a Weapon → (+) Decision to Use Deadly Force</td>
</tr>
<tr>
<td>Correll, Park, Judd, Wittenbrink, Sadler, and Keesee (2007)</td>
<td>Denver (Colorado) Department of Motor Vehicles; Denver Police Department; Denver Police Training Seminar</td>
<td>Analysis of Variance; Correlational</td>
<td>Racial Response Bias (ST); Threatening Acts (CT)</td>
<td>Black Suspect → (+) Presence of a Weapon → (+) Decision to Use Deadly Force</td>
</tr>
<tr>
<td>Correll, Urland, &amp; Ito (2006)</td>
<td>University of Colorado</td>
<td>Analysis of Variance</td>
<td>Racial Response Bias (ST); Threatening Acts (CT)</td>
<td>Black Suspect → (+) Presence of a Weapon → (+) Decision to Use Deadly Force</td>
</tr>
<tr>
<td>Dwyer, Graesser, Hopkinson, &amp; Lupfer (1990)</td>
<td>Shelby County (Tennessee) Sheriff’s Office</td>
<td>OLS Regression</td>
<td>Social Script (ST)</td>
<td>No Significant Results</td>
</tr>
<tr>
<td>Greenwald, Oakes, &amp; Hoffman (2003)</td>
<td>University of Washington</td>
<td>Unidentified</td>
<td>Racial Response Bias (ST); Threatening Acts (CT)</td>
<td>Black Suspect → (+) Decision to Use Force</td>
</tr>
</tbody>
</table>

Notes: CT = Criminal Threat; ST = Social Threat
determine its viability. Moreover, Klinger’s theory does not directly apply to the explanations of police use of force. Nonetheless, I review the basic arguments of his theory and discuss how it might be used to explain police use of force.

**Klinger’s Ecological Theory of Police Vigor**

Klinger’s (1997) ecological theory of police vigor is the only new theory of police behavior that has been proposed in recent years. While his theory was not specifically intended to explain police use of force, based on the theoretical arguments he makes, it would be relatively easy to empirically determine whether or not his theory constitutes a third viable explanation of police use of force. In this section, I therefore review Klinger’s theory and discuss how it might be modified for use as another theoretical framework for the study of police use of force.

In 1997, Klinger proposed his “ecological theory of police response to deviance.” His primary argument in this theory was that the amount of vigor police officers used to do their jobs should be related to the ecological contexts in which they work. In other words, he believed that the environments that police officers worked in on a day-to-day basis might influence the amount of effort that officers put into completing their duties. Specifically, he contended that four problems affecting police vigor would arise as a result of police officers working in high crime areas – 1) an increased tolerance for deviant and criminal behavior, 2) decreased perceptions of crime victims’ deservingness, 3) increased levels of cynicism toward the value of their crime-fighting efforts, and 4) the increased size of their workloads.

In regards to the first problem, Klinger argued that officers would put less effort into the execution of their duties when they believed that residents within their patrol beats were tolerant,
if not accepting, of deviant and criminal behaviors. Essentially, he believed that after working in areas where residents had become desensitized to crime, police officers might also become desensitized and consequently use less vigor in their duties since they no longer perceived certain types and levels of crime to be as serious as objective outsiders might. For example, if an officer worked in an area where prostitution and drug dealing were tolerated, if not accepted by residents, Klinger argued that police officers might also become tolerant of those behaviors and, as a result, put less effort into enforcing the laws regulating those behaviors. Thus, in high crime areas, police officers might use less vigor because they no longer viewed certain deviant or criminal behaviors as being worthy of their time and energy.

Second, Klinger contended that police officers may use less vigor when they perceive victims of crimes to be undeserving. Specifically, he argued that in areas with high levels of crime, police officers might deal with many crime victims who had helped precipitate their own victimization. For example, if officers responded to a lot of calls where an individual was assaulted or robbed while he or she was under the influence of some illicit drug, Klinger believed that those officers might start blaming the victim for bringing about their own victimization. Subsequently, if officers were forced to handle a large number of incidents in which the crime victim was not wholly without fault, then Klinger argued that police officers would begin to view those victims as being less worthy of their efforts.

Third, Klinger posited that neighborhoods with high levels of crime may cause the officers working in those neighborhoods to come to believe that their efforts are futile. That is, if police officers worked hard day-in and day-out to fight crime, but then continued to see problems despite their hard work, Klinger believed that those officers would start becoming cynical about the prospect of ever really fulfilling their duties. In turn, officers’ cynical attitude toward the
value of doing their duties might lead to officers putting less effort into them. In this way, Klinger expected that in areas where rates of crime had been high for long periods of time despite police officers’ best efforts, they would become less likely to put forth such effort in the future.

Finally, Klinger suggested that because police officers working in high crime areas had more crimes with which they had to deal, they also had deal with much heavier workloads. Then, when officers became overwhelmed, Klinger argued that they would start putting less effort into their jobs. Afterward, if their workload became smaller and more easily manageable because of their lack of effort, the officers would have no reason to ever go back to fully performing their duties. Thus, Klinger expected that police officers would use less vigor in high crime areas in order because less work ensured that they “got home on time every night.”

Klinger’s Theory as an Explanation for Police Use of Force

As a theory predicting police vigor, Klinger argued that four problems associated with working in high crime neighborhoods should reduce the amount of effort police officers use to complete their duties. On the surface then, one might expect that police officers would use less force in high crime areas since the use of force requires a good deal of physical effort and vigor. On the other hand, however, the use of force necessitates a lot more paper work (i.e. filling out use-of force reports). Consequently, following Klinger’s arguments for reduced police vigor in high crime areas, I discuss how the same four problems Klinger identified might also be related to reduced police use of force below.

First, an increased level of tolerance of criminal behavior is likely to decrease police officers’ use of force if those officers become so desensitized to crime that they do not perceive
anything as being worthy of their efforts, and therefore do not engage in any situations where the use of force might be necessary. Second, it also stands to reason that if police officers believe that crime victims are more deserving of whatever happens to them, and less deserving of their efforts, then those officers should also be less likely to use force to protect them. Third, if officers truly believed that nothing they do can effectively reduce the crime problems in their beat, it is likely that police officers would use less force. Finally, as mentioned above, since police officers are generally required to fill out forms documenting their use of force, it is also reasonable to expect officers to use less force, if only to avoid filling out more paperwork. Thus, as laid out above, it should be easy enough to apply Klinger’s theory of police vigor to police use of force as well. Unfortunately, however, without even testing these ideas, some of the evidence reviewed earlier in this chapter suggests that Klinger’s theory may not actually be a viable explanation of police officers’ use of force.

As many of the empirical tests of criminal threat theories revealed earlier, police officers are actually more likely to use force in high crime areas, if only because they are more likely to perceive greater criminal threats. Thus, the application of Klinger’s arguments in regards to reduced police vigor in high crime areas do not appear to similarly apply to police officers’ use of force. The question then arises – can police officers really use less vigor and more force in high crime areas? Such a question needs to be addressed both theoretically and empirically. Unfortunately, doing so is beyond the scope of this dissertation. Nonetheless, it also presents an interesting avenue for future research.
Chapter Summary

Recent studies of police officers’ use of force reviewed in this chapter support the contention made by other researchers (e.g. Bernard and Engel 2001; Garner, Maxwell, and Heraux 2002; Hagan 1989; Klinger 2004) that there is a severe lack of theory-driven research in the field of policing. As Table 2.1 displays, there were only 15 empirical studies over the past 20 years that have utilized theory to drive their analyses. This is regrettable. While non-theory driven research can tell us what factors are related to police officers’ use of force, without theory, we cannot understand why those factors should be related, nor can we hypothesize how changing those factors might affect police officers’ behaviors. Consequently, in order to further our understanding of police officers’ use of force, more theoretical explanations need to be proposed. Klinger’s (1997) ecological theory is a good start, but needs empirical testing to determine its viability as a theory for police use of force.

In this dissertation, I propose and test a new theory of police use of force. My dissertation therefore takes a significant step toward helping us understand not only why police officers use force, but also what we might do as a society to better control such behaviors. Before discussing my theory in detail, however, it is important to understand the research on which the theory is based. In the next chapter of this dissertation, I review the development of the social disorganization tradition that serves as the theoretical foundation for my social disorganization theory of police officers’ use of force.
CHAPTER 3
THE SOCIAL DISORGANIZATION TRADITION

The object of this dissertation is to relate police officers’ use of force behaviors to the contextual factors associated with the neighborhoods in which they work. Unfortunately, there has been very little research that considers exactly how and why neighborhood contextual factors should influence police officers’ use of force behaviors. My review of the theoretical literature examining police officers’ use of force in the previous chapter focused on the two primary categories of theories – social threat and criminal threat theories – that policing researchers currently use to explain police use of force behaviors (as well as Klinger’s theory that has the potential to also help explain such behaviors). While some of the measures that have been utilized to test the social and criminal theories are related to neighborhood context (e.g. racial/ethnic and socioeconomic neighborhood compositions and neighborhood crime rates), I argue that those measures, as well as many other previously untested measures, can also influence police officers’ use of force in a manner differently than what is expected based on existing theory. That is, rather than only influencing the social or criminal threat present in a neighborhood, I argue in this dissertation that neighborhood context also determines the level of social disorganization in a neighborhood, which may, in turn, influence police officers’ use force. In this chapter, I review the history of the social disorganization tradition to provide a foundation for my later arguments regarding how neighborhood social disorganization might be related to police officers’ use of excessive force specifically (see Chapter 4).
Although the theory of social disorganization is commonly and appropriately attributed to Clifford Shaw and Henry McKay, what I refer to as the social disorganization tradition can be attributed to a much larger body of researchers. For the purposes of this dissertation, *the social disorganization tradition* is the combination of both Shaw and McKay’s original theory and the works of a number of later researchers who have each made significant modifications to the concept of neighborhood social disorganization. Thus, for the remainder of this dissertation, any time I discuss neighborhood contextual concepts that are attributable to any of the researchers discussed in this chapter, I will refer to those concepts as coming from *the social disorganization tradition*, rather than from any particular researcher or group of researchers.

I begin my review of the social disorganization tradition by briefly discussing some of the research that influenced the work Shaw and McKay. I then review in more detail the major concepts and ideas that Shaw and McKay introduced with their theory of social disorganization. Following this review, I review and discuss the three most important modifications of Shaw and McKay’s original theory. First, I review Kornhauser’s research on the importance of neighborhood informal social control and discuss how her work led to a renewed focus on the mechanisms through which neighborhoods become socially disorganized. Next, I review and discuss the works of several researchers who have contributed to what is now often called the neighborhood systemic model of social disorganization. Finally, I review and discuss the recent research done by Sampson and colleagues that led to the conceptualization of neighborhood collective efficacy. I then conclude this chapter by reviewing which of the concepts from the social disorganization tradition have stood up empirically as the best predictors of neighborhood crime rates, and are consequently utilized in this dissertation as the concepts that should most likely be related to police officers’ use of excessive force.
The Origins of Social Disorganization

The theory of social disorganization is rooted in Clifford Shaw and Henry McKay’s research on juvenile delinquency in Chicago, Illinois, during the 1920s and 1930s. Before discussing their theory of social disorganization, however, it is important to briefly review some of the work that inspired their research. Since the early part of the last century, researchers have considered how the neighborhoods in which individuals live might influence how they behave. This focus on neighborhood contextual effects can be traced back to the Chicago School, of which Shaw and McKay were a part. Prior to their research on neighborhood social disorganization, however, several other researchers within the Chicago School had mapped out the growth of Chicago using concentric zones emanating outwards from the downtown area (Park, Burgess, and McKenzie 1925).

After mapping out five concentric zones within the city of Chicago, Park and colleagues (1925) observed that each of the zones they had identified not only had very different demographic, social, and economic compositions, they also had very different levels of crime. They found that the zone immediately surrounding the downtown area (i.e. the central business district) was mostly populated by recent immigrants to the United States who needed close access to employment and cheap housing prices, both of which resulted from the deteriorated housing stock in the area. They also found that this zone had the highest rates of crime in all of Chicago. Subsequently, Park and colleagues named this zone the transitional zone, because residents would move out in order to escape the area’s crime problems as soon as they had the financial means to do so, but were quickly replaced by an even newer cohort of immigrants also in need of access to downtown and cheap housing.\(^{14}\)

\(^{14}\) Individuals escaping the transitional zone typically moved to one of the three outer zones of Chicago identified by Park and colleagues – what they called the working class zone, the residential zone, and the suburban zone (listed
While most criminologists of the time believed that criminal behavior could be explained using characteristics of individuals, Park and colleagues’ finding that the transitional zone consistently had the highest rates of crime in all of Chicago, regardless of who was living there at the time, paved the way for what was then a new way of thinking about crime. This new direction for explaining crime was led by their colleagues, Clifford Shaw and Henry McKay. Based on Park and colleagues' finding, Shaw and McKay argued that, contrary to popular belief, it was not the people who lived in the transitional zone who caused rates of crime to be surprisingly high, rather it was the place in which they lived that gave rise to those high crime rates. In other words, Shaw and McKay believed that it was places, and not people, that were the key to explaining rates of crime.

Shaw and McKay’s Theory of Social Disorganization

Using Park and colleagues’ (1925) research as a foundation for their own work, Shaw and McKay’s theory of social disorganization was a direct result of their attempts to explain the spatial variation in crime rates across Chicago. This research ultimately led to their seminal work, Juvenile Delinquency in Urban Areas (1942), in which they first proposed linking neighborhood context to neighborhood juvenile arrest rates. Although their original theory has received numerous additions and modifications, the fundamental argument remains the same - variation in the neighborhood context influences the levels of crime, above and beyond the influences of the individual-level compositional factors related to the residents who live in them.

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from the nearest to the central business district to the furthest). Because these zones were not essential to Shaw and McKay’s development of their theory of social disorganization, they are not discussed in any more detail here.

While Shaw and McKay (1942) were primarily interested in explaining rates of juvenile delinquency and arrest rather than criminal behavior in general, their theory is commonly applied to the explanation of other forms of deviance and crime as well. Thus, for the purposes of this dissertation, I discuss Shaw and McKay’s original theory as an explanation for all types of crime, rather than for juvenile delinquency only.
Specifically, they argued that the negative effects of certain neighborhood structural characteristics would give rise to neighborhood residents being less likely and less able to come together, define, and ultimately achieve common goals, including the prevention of crime. Furthermore, they argued that that the negative effects of those structural characteristics would also lead to an increase in criminal values among neighborhood residents. Ultimately, it was the neighborhood’s inability to collectively define and achieve common goals, as well as the increase in criminal values throughout the neighborhood that Shaw and McKay termed social disorganization.

The three neighborhood structural characteristics that Shaw and McKay believed would lead to neighborhood social disorganization were poverty, racial and ethnic heterogeneity, and residential instability, which collectively measure what I call neighborhood structural disadvantage.\(^{16}\) Shaw and McKay found that Chicago’s transitional zone consistently had not only the highest rates of crime, but also had the highest rates of neighborhood structural disadvantage. As a result of this observation, Shaw and McKay argued that variation in a neighborhood’s level of structural disadvantage would predict its level of neighborhood social organization (or the lack thereof), which would, in turn, predict its rates of crime.

In other words, Shaw and McKay argued that because structurally disadvantaged neighborhoods were composed of many poor individuals (i.e. poverty), who were likely to be of many different cultural backgrounds (i.e. racial/ethnic heterogeneity), those individuals would be unwilling, or unable, to get to know each other and agree on a common set of values and goals for the neighborhood. Adding to this problem, as many of the immigrants that populated the transitional zone assimilated to American culture and found better paying jobs, they would move

\(^{16}\) For the remainder of this dissertation, when I discuss neighborhood structural disadvantage, I am referring to the three structural characteristics of neighborhoods that Shaw and McKay identified as precursors of neighborhood social disorganization – poverty, racial/ethnic heterogeneity, and residential instability.
away from the inner city to be replaced by new families. The constant in- and out-flow of residents (i.e. residential instability) further exacerbated the remaining residents’ inability to come together, get to know each other, and form a common set of goals and values. As this process repeated itself over the years, Shaw and McKay argued that it would be almost impossible for residents of structurally disadvantaged neighborhoods to collectively define a set of rules regulating which behaviors were acceptable or unacceptable in their neighborhood. Consequently, the researchers expected that structurally disadvantaged neighborhoods would become more vulnerable to deviant and criminal activities since rules of behavior that did not exist could not be enforced.

Shaw and McKay never explicitly gave neighborhood residents’ inability to regulate the behavior of individuals (described above) a specific name. Later researchers, however, would call it the *lack of informal social control* (e.g. Kornhauser 1978; Morenoff et al. 1997; Sampson et al. 1997; Sampson et al. 1999). This lack of informal social control was only one aspect of neighborhood social disorganization, though. In order to help explain why neighborhoods within the transitional zone continued to consistently have high rates of crime over long periods of time, Shaw and McKay posited that neighborhood structural disadvantage not only reduced neighborhood informal social control, but that it also increased deviant and criminal values among residents. They argued that once high rates of crime had entered a neighborhood and become a regular occurrence, some residents would become accustomed to crime. Eventually, they believed that a subculture which embraced deviant and criminal values would form because no other residents of a neighborhood would be willing to challenge those alternative values since there was no set of common values with which to begin. Essentially then, Shaw and McKay believed that deviant and criminal values could thrive and be transmitted from one generation of
residents to another.\textsuperscript{17} Thus, for Shaw and McKay, neighborhood social disorganization represented not only the inability of neighborhood residents to informally control deviant or criminal behavior, but also an increase in criminal values over time.

Unfortunately, while Shaw and McKay did observe a relationship between neighborhood structural disadvantage and juvenile arrest rates in Chicago, they never specifically sought to identify or empirically test any measures of neighborhood social disorganization. That is, even though they theorized about how social disorganization might mediate the relationship between neighborhood structural disadvantage and rates of crime, they never proposed how neighborhood informal social control or criminal values should be measured. Consequently, Shaw and McKay were only able to empirically link neighborhood structural disadvantage to increased rates of crime. Then, perhaps as a result of this omission, their theory of social disorganization fell out of favor for several decades until the work of other researchers again sparked interest in how neighborhood context might influence criminal behavior.

**The Informal Social Control Reformulation**

Over three decades after Shaw and McKay unveiled their original theory of social disorganization, Kornhauser (1978) published her dissertation research on what she generally described as “the social sources of delinquency.” In this research, she both lauded and critiqued Shaw and McKay’s original theory. While she agreed with their conclusion that places were important factors for explaining crime, she was also one of the first people to criticize Shaw and McKay for not clearly enough explicating the mechanisms through which neighborhood

\textsuperscript{17} While important in the original theory, Shaw and McKay’s arguments regarding the transmission of deviant and criminal values have worked their way out of most contemporary research examining neighborhood social disorganization. For a discussion of how and why deviant and criminal values may not be as essential to the theory of social disorganization, see Kornhauser (1978) and my discussion of her contributions to the social disorganization tradition below.
structural disadvantage lead to increased rates of crime (i.e. neighborhood social disorganization). And, through her criticisms of their original theory, Kornhauser ultimately made two important contributions to the larger social disorganization tradition.

Kornhauser’s first major contribution to the social disorganization tradition was to emphasize the role of informal social control. She argued that criminologists should more seriously consider how neighborhood residents’ willingness to intervene on behalf of the neighborhood and regulate behaviors, without the help of formal sources of control (i.e. the police), might influence neighborhood rates of crime. Moreover, while her definition of informal social control was very similar to what Shaw and McKay (1942) described in their work (but never officially named), she focused less on residents sharing common goals and values, and argued that more attention should be paid to residents’ collective willingness to act when their neighborhood was threatened. In other words, Kornhauser argued that neighborhood structural disadvantage would make residents less willing to place themselves in harm’s way for the benefit of their neighbors, and consequently, the neighborhood would have no one to step forward and put a stop to deviant or criminal behaviors. Then, with no one willing to act, she believed that the neighborhood would become more vulnerable to being taken over by criminal activity. Kornhauser’s first major contributions to the social disorganization tradition was therefore the increased emphasis that she placed on residents’ ability to informally regulate behaviors in their neighborhood (i.e. neighborhood informal social control).

Kornhauser’s second major contribution to the social disorganization tradition might be considered a case of addition by subtraction. She contended that Shaw and McKay’s (1942) subcultural argument regarding the inter-generational transmission of criminal values contradicted their other arguments about why neighborhood structural disadvantage should lead
to increased rates of crime. She argued that, because Shaw and McKay expected that the residents of structurally disadvantaged neighborhoods were unable to come together and form and enforce common values that would condemn deviant behaviors and crime, they should just as well be unable to come together and form and enforce common values that would promote deviant behaviors and crime. In other words, she questioned why Shaw and McKay believed that neighborhood residents could come to an agreement that deviant and criminal behaviors should be acceptable, when they had also argued that those same people could not come together to agree on anything else.

In addition to this critique of the subcultural component of Shaw and McKay’s original theory of social disorganization, Kornhauser also criticized the researchers for trying to explain the consistently high rates of crime in structurally disadvantage neighborhoods through the inter-generational transmission of criminal values. She argued that because structurally disadvantaged neighborhoods had high levels of residential instability, each successive wave of immigrants (or other new groups) who moved into those neighborhoods should continue to experience social disorganization and consequently have high rates of crime. Specifically, she argued that because the populations of structurally disadvantaged neighborhoods were constantly turning over, those neighborhoods would always be socially disorganized over time. Thus, Kornhauser insisted that if neighborhood residential instability could explain high levels of social disorganization and crime over time, there was never any need to introduce the concept of inter-generationally transmitted criminal values.

Based on the two arguments I describe above, Kornhauser alleged that Shaw and McKay had not only unnecessarily introduced the concept of criminal values, but in doing so they had created a paradox in which neighborhood residents were unable to agree on values condemning
crime, but were nevertheless able to agree on values promoting crime. Accordingly, then, she argued that the subcultural element (i.e. increased criminal values) of the theory should be dropped (although some argue that it should be brought back — e.g. Anderson 1999; Warner 2003), and that researchers intending to study neighborhood social disorganization should instead focus on examining neighborhood informal social control. Thus, Kornhauser’s two major contributions to the evolution of the social disorganization tradition were 1) the stronger emphasis she placed on the role neighborhood informal social control, and 2) the purging of the subcultural element of Shaw and McKay’s original theory. Despite her contributions, though, the social disorganization tradition remained relatively unpopular until two new social-disorganization-based explanations of neighborhood crime rates emerged in the early 1990s.

The Neighborhood Systemic Model

A decade after Kornhauser’s contribution to social disorganization theory, several researchers separately began arguing for further revisions of the social disorganization tradition. Although Granovetter (1973) and Kasarda and Janowitz (1974) are credited with first acknowledging the importance of social ties and social networks, it was a number of later researchers who are credited with the creation of the neighborhood systemic model of social disorganization (Bursik and Grasmick 1993; Hunter 1985; Sampson and Groves 1989). These researchers contended that Shaw and McKay (1942) and Kornhauser (1978) missed an important mediating factor in their explanations of the relationship between neighborhood structural disadvantage and rates of crime. They argued that the existence of strong and frequent social ties amongst neighborhood residents were essential if those residents were to intervene on behalf of their neighborhood to regulate unacceptable behaviors (i.e. exercise informal social control). If
those ties did not exist, they argued, then residents would not willingly put themselves in harm’s way for the benefit of their neighbors or their neighborhood. Subsequently, the researchers expected levels of informal social control to be very low in neighborhoods with few or weak social ties, ultimately resulting in the neighborhood becoming more vulnerable to crime.

The notion of neighborhood social ties, which is at the heart of the neighborhood systemic model, draws on the work of Kasarda and Janowitz (1974). These researchers argued that social ties and social networks were important aspects of neighborhoods that strongly influenced how residents interacted with each other. If ties were weak, and networks were small, residents were expected to keep to themselves and not become involved in the affairs of their neighbors. If ties were strong, and networks were extensive, on the other hand, residents were expected to socialize and be friends with more of their neighbors and ultimately be more involved in the business of the neighborhood as a whole. Based on these ideas, proponents of the neighborhood systemic model argued that neighborhood structural disadvantage reduced residents’ ties to each other and their neighborhood, which in turn, increased neighborhood social disorganization. Thus, according to the neighborhood systemic model, both Shaw and McKay and Kornhauser were missing an important mediating factor – neighborhood social ties – that could help explain why structurally disadvantaged neighborhoods would have lower levels of informal social control, and, ultimately, higher rates of crime.

In addition to providing the social disorganization tradition with another mechanism for explaining how and why neighborhood structural disadvantage might be related to neighborhood

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18 The concept of social ties can also be traced back to the work of Granovetter (1973). Unlike Kasarda and Janowitz (1974), Granovetter argued for the strength of weak ties. He contended that weak ties to other individuals in a social network were sufficient to effectively transmit information and be beneficial to members of the network. While later researchers acknowledged the importance of weak neighborhood social ties (see review below), the original founders of the neighborhood systemic model perspective believed that strong ties and large social networks were the key to explaining low levels of neighborhood crime.
crime rates, proponents of the neighborhood systemic model were the first to empirically measure and test the viability of their arguments. Unfortunately, as I discuss in more detail below, even though the neighborhood systemic model has received much scholarly attention, the findings have been largely inconsistent. Nonetheless, proponents of the neighborhood systemic model made important contributions to the social disorganization tradition not only by identifying another potential mechanism for consideration, but also by being the first researchers to empirically assess the direct, indirect, and mediating effects of various neighborhood contextual factors on neighborhood rates of crime.

The Emergence of a Collective Efficacy Explanation

The most recent addition to the social disorganization tradition comes in the form of another mechanism with the potential to mediate the relationship between neighborhood structural disadvantage and neighborhood crime rates – neighborhood collective efficacy. The concept of neighborhood collective efficacy is attributed to Sampson and a number of his colleagues (Morenoff et al. 2001; Sampson et al. 1999; Sampson et al. 1997). Despite the recent theoretical and empirical development of the neighborhood systemic model, Sampson and colleagues argued that neighborhood social ties were not as important for linking neighborhood structural disadvantage to neighborhood crime rates as previous researchers had suggested. Moreover, they thought that Kornhauser’s (1978) exclusive focus on neighborhood informal social control was too narrow. As a result, they argued that an agreement on common goals and values, and more importantly, a mutual trust amongst neighbors that every resident of the neighborhood would be just as willing to intervene and exercise informal social control as everyone else would be enough to prevent social disorganization, and ultimately high rates of
crime. Furthermore, they believed that residents did not need to have frequent or strong social ties with their neighbors, as long as they trusted them to engage in informal social control efforts when necessary. Sampson and colleagues therefore defined neighborhood collective efficacy as the combination of two concepts – informal social control (i.e. the willingness to intervene on behalf of the neighborhood) and neighborhood social cohesion (i.e. the sharing of common beliefs, values, and a mutual trust among residents).

Using those concepts, Sampson and colleagues expected that high levels of neighborhood structural disadvantage would impair residents’ ability to get together, define common goals and values, and most importantly, they would impair their ability to trust each other. They then contended that, even if residents had strong social networks within their neighborhood, if they could not trust the individuals within those networks to make informal social control efforts, they would be unwilling to make any efforts themselves. As a result of this lack of mutual trust, Sampson and colleagues argued that informal social control efforts in the neighborhood would be weakened, and leave the residents vulnerable to deviant and criminal activities, even in the presence of strong social ties.

It should be noted that, unlike the previous modifications and reformulations of Shaw and McKay’s original theory of social disorganization discussed previously, Sampson and colleagues’ concept of neighborhood collective efficacy actually serves as a measure of neighborhood social organization. In other words, whereas Kornhauser and the proponents of the neighborhood systemic model generally defined neighborhood social disorganization as the presence of low levels of informal social control, Sampson and colleagues combined high levels

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19 Although social cohesion may appear to be very similar in nature to the systemic model’s notion of social ties, the focus here is more on the relationship among all neighborhood residents, rather than a few strong ties among friends and family members. In essence, Sampson and colleagues’ concept of social cohesion is consistent with the notion of weak ties described in detail by Bellair (1997) (see also, Granovetter 1973).
of informal social control with their concept of neighborhood social cohesion so that they were measuring the inverse of social disorganization. Rather than using neighborhood social disorganization to predict high rates of crime then, Sampson and colleagues used neighborhood collective efficacy to predict low rates of neighborhood crime. Despite this divergence from earlier conceptualizations of neighborhood social disorganization, the concept of neighborhood collective efficacy is nonetheless consistent with the larger social disorganization tradition since they are still trying to explain why neighborhood structural disadvantage might be related to neighborhood rates of crime.

Sampson and colleagues also contributed to the development of the social disorganization tradition by conducting empirical tests of the mechanisms that they proposed. Unlike tests of the neighborhood systemic model, however, research testing the concept of neighborhood collective efficacy has been largely supportive. Perhaps not surprisingly then, Sampson and colleagues’ contributions to the development of the social disorganization tradition include not only the conceptualization and empirical assessment of their neighborhood collective efficacy concept, but also the revival of scholarly and popular interest in the study of neighborhoods and crime. In the next section of this chapter, I review several of Sampson and colleagues’ tests of the collective efficacy concept, as well as a number of other studies that have examined the many other concepts commonly associated with the social disorganization tradition, including neighborhood structural disadvantage, informal social control, and social ties.²⁰

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²⁰ The concepts of collective efficacy, the systemic model, informal social control, social cohesion, and social ties have much in common. As previously discussed, the collective efficacy perspective differs from Kornhauser’s informal social control reformulation primarily because it views neighborhood social cohesion as being a second necessary component for predicting neighborhood crime rates while Kornhauser’s reformulation does not. It differs from the neighborhood systemic model as well because it views trust among residents as more important than simply having strong neighborhood social ties. Finally, as I discuss in the next section of this chapter, the collective efficacy perspective has received the most empirical support to date. Thus, the influence of neighborhood collective efficacy on police use of force will be of primary interest in this dissertation, although tests of the systemic model
Empirical Tests of the Social Disorganization Tradition

Since Shaw and McKay proposed their original theory of social disorganization, numerous studies have empirically analyzed many of the concepts and mechanisms associated with the social disorganization tradition. In the remainder of this chapter, I briefly review some of the more notable findings in regards to the influence of 1) the three measures of neighborhood structural disadvantage (i.e. poverty, racial/ethnic heterogeneity, and residential instability), 2) neighborhood informal social control only, 3) neighborhood social ties, and 4) neighborhood collective efficacy on neighborhood rates of crime. Additionally, because some researchers have recently begun to question the importance of neighborhood social ties (especially in comparison to the relative importance of neighborhood collective efficacy), I review the findings obtained by those researchers in more detail. First, however, I briefly review the empirical findings testing Shaw and McKay’s three measures of neighborhood structural disadvantage.

Research has widely and consistently supported a positive relationship between neighborhood structural disadvantage and neighborhood crime rates. As Shaw and McKay originally hypothesized, a large number of researchers have shown that increases in aggregate-level rates of poverty, racial and ethnic heterogeneity, and residential mobility are indeed related to increases in various rates of crime (e.g. Bursik and Grasmick 1993; Krivo and Peterson 1996; Morenoff et al. 1997; Peterson, Krivo, and Harris 2000; Sampson and Groves 1989; Sampson et al. 1997; Sampson et al. 1999; Sampson et al. 2001; Shaw and McKay 1942; Silver 2000; Warner and Rountree 1997; Veysey and Messner 1999; for a comprehensive review see Sampson, Morenoff, and Gannon-Rowley 2002). While the role of neighborhood structural disadvantage in general is one of the most well-established in all the neighborhoods and crime

will be included as controls. Subsequently, for the remainder of this dissertation, collective efficacy will be discussed as the primary explanatory concept of interest.
literature, of Shaw and McKay’s three original measures of structural disadvantage, the influence of neighborhood residential instability has been found to be the weakest (Sampson et al. 2002). Nonetheless, with a large body of supporting literature, the three measures of neighborhood structural disadvantage should be included in any study of neighborhood context, and will therefore be included in my analysis of police use of excessive force for this dissertation.

Research examining the specific influence of neighborhood informal social controls on neighborhood crime rates has been largely supportive as well. As Kornhauser (1978) first hypothesized, research has shown that when neighborhood residents are willing to intervene on behalf of their neighborhood to prevent deviant and criminal behaviors, crime rates tend to be low (Hunter 1985; Bursik 1999; Kornhauser 1978; Velez 2001; Veysey and Messner 1999). Moreover, much of the research that has included measures of neighborhood informal control as a part of Sampson and colleagues concept of neighborhood collective efficacy has also shown it to be negatively related to rates of crime (see review below). Thus, the neighborhood informal social control also appears to be an important neighborhood contextual concept that should be incorporated into this dissertation. However, as I discuss below, I will include measures of neighborhood informal social control as one component of Sampson and colleagues’ neighborhood collective efficacy, rather than examining its specific predictive power on its own. For more details on how neighborhood informal social control is included in the analyses I conduct for this dissertation, see Chapter 5.

While the importance of neighborhood structural disadvantage and informal social control have been widely supported and acknowledged in the neighborhoods and crime literature, researchers do not agree on the importance of neighborhood social ties (Bursik and Grasmick 1993; Hunter 1985; Sampson and Groves 1989). In the first empirical test of the
neighborhood systemic model, Sampson and Groves (1989) examined how neighborhood structural disadvantage influenced neighborhood residents’ participation in both formal (e.g. neighborhood or condominium associations) and informal or voluntary (e.g. neighborhood watch programs) organizations, the breadth of neighborhood friendship networks, and extent of unsupervised teenage peer groups. They found that where participation in neighborhood organizations was low and where neighborhood friendship networks were weak, unsupervised teenage peer groups strongly and significantly predicted higher rates of crime, thereby supporting the neighborhood systemic model of social disorganization.

Since Sampson and Groves’ original research on the neighborhood systemic model a number of researchers have attempted to replicate their findings, with varying degrees of success (e.g. Lowencamp, Cullen, and Pratt 2003; Sun, Triplett, and Gainey 2004; Veysey and Messner 1999). Other researchers, however, have argued that in many neighborhoods with low rates of crime, residents do not always have strong or frequent social ties or social networks with other members of their neighborhood, and most do not even have frequent interactions with many of their neighbors (e.g. Patillo-McCoy 1999; Sampson et al. 1997; Wilson 1996). These researchers suggested that the residents of many low-crime neighborhoods have most of their strong social ties and social networks outside of their neighborhood, and subsequently have few strong or frequent ties to their actual neighbors. And, although much of the research I have just described was theoretical or based on qualitative evidence with limited generalizability, other recent empirical analyses of large-scale survey data has not only supported those ideas, but some research has even suggested that strong neighborhood social ties may increase neighborhood rates of crime.
First, drawing on Granovetter’s (1973) piece “The Strength of Weak Ties,” Bellair (1997) found that infrequent social contact and weak social ties predicted lower levels of crime just as well, if not better, than frequent social contact and strong ties (although the greatest reductions in crime occurred in the presence of both weak and strong social ties). Moreover, he observed that neighborhood informal social control efforts were just as likely to occur in neighborhoods that had infrequent social contact and weak social ties amongst residents as they were to occur in neighborhoods with frequent contact and strong ties. Subsequently, Bellair suggested that weak neighborhood social ties (i.e. those ties among non-family, non-friend neighbors) might actually be more important predictors of informal social control efforts, and ultimately lower rates of crime, than stronger neighborhood social ties.

In addition to Bellair’s findings, other recent empirical evidence suggests that strong neighborhood social ties may actually impede informal social control efforts and result in increases in neighborhood crime rates. In their study examining the influence of both neighborhood social ties and neighborhood collective efficacy, Browning, Feinberg, and Dietz (2004) found that neighborhood collective efficacy was the better predictor of low neighborhood crime rates, and that strong neighborhood social ties were actually related to higher neighborhood crime rates. In light of these findings, Browning and colleagues suggested that having many strong social ties within a neighborhood might actually shield criminals from both informal and formal social control efforts. They specifically posited that if a criminal had strong familial or friendship ties to other residents of a neighborhood, then those residents might shield the criminal from any social control efforts, and thereby allow the criminal to continue offending and ultimately drive up neighborhood crime rates.
Finally, in addition to the Bellair’s (1997) and Browning and colleagues’ (2004) findings, central to Sampson and colleagues’ arguments (1997) regarding their concept of neighborhood collective was the notion that neighborhood residents did not need to have strong or frequent social ties, as long as they shared a mutual trust that everyone in the neighborhood was willing to exercise informal social control efforts when necessary. Not surprisingly, in a number of empirical tests, Sampson and colleagues found support for their hypothesis that neighborhood collective efficacy would reduce rates of neighborhood crime (Morenoff et al. 1997; Sampson et al. 1997; Sampson et al. 1999; Sampson et al. 2001; Sampson et al. 2002). Additionally, several other recent empirical tests of the negative relationship between neighborhood collective efficacy and a variety of aggregate-level measures of crime and deviance have been strongly supportive as well (Browning 2002; Browning et al. 2004; Cancino 2005; Reisig and Cancino 2004; Triplett et al. 2003). Thus, while a substantial body of research has called into question the importance of neighborhood social ties, the ameliorative role of neighborhood collective efficacy on rates of crime appears to be above reproach. Sampson and colleagues’ concept of neighborhood collective efficacy has therefore been, and continues to be today, one of the most significant modifications of the social disorganization tradition.

Based on the empirical evidence described above, the neighborhood social ties appear to be less important than some researchers had originally envisioned, and neighborhood collective efficacy may be one of the most important modifications to Shaw and McKay’s original theory of social disorganization to date. Nonetheless, to fully test the viability of the social disorganization tradition as a theoretical framework for explaining police officers’ use of excessive force, I will include measures of both neighborhood social ties (i.e. the neighborhood systemic model) and neighborhood collective efficacy (measured as informal social control and
neighborhood social cohesion). In the next two chapters of this dissertation, I discuss how and why I expect that neighborhood contextual concepts (such as those discussed in this chapter) may be related to police officers’ use of force, and I then present the actual measures and hypotheses that will be tested in this dissertation.
CHAPTER 4
SOCIAL DISORGANIZATION & POLICE USE OF FORCE

The primary object of this dissertation is to link neighborhood social disorganization to police officers’ use of excessive force. In order to do so, I propose and test a new social disorganization theory of police use of force. Along the way, I intend to provide a stronger understanding of police officers’ use of force behaviors by demonstrating the utility of neighborhood-level contextual concepts and multi-level modeling techniques for studies of police officers’ behaviors. Currently, few researchers have attempted to bring together any concepts or methodologies from the neighborhoods and crime literature to the study of police use of force. Moreover, since the reemergence of the social disorganization tradition, only a handful of studies have used any neighborhood-level contextual concepts to predict police officers’ use of force, and none have specifically used a social disorganization tradition framework to explain why police might use excessive force in disorganized areas. Rather, those few studies that have tested its concepts have generally looked for relationships without providing any theoretical explanations for what they found. In addition to the lack of research on neighborhood-level contextual effects, only a very small number of police use of force studies have embraced the use of the advanced analytic techniques which are prevalent in the neighborhoods and crime literature.

In the remainder of this chapter, I review and critique the small body of extant literature on the relationship between aggregate-level ecological context and police use of force. Then, after discussing how my research in this dissertation improves on those studies, I outline the
reasons that I believe that neighborhood social disorganization, and more specifically, neighborhood collective efficacy, should lead to increased problems with police officers’ use of excessive force. I conclude this chapter by presenting the two larger research questions that I seek to answer in this dissertation, and the eight specific hypotheses that I will to empirically test those questions.

**Neighborhood Context and Police Use of Force**

Currently, only three empirical studies have attempted to examine the relationship between aggregate-level contextual factors and police officers’ use of force (Kane 2002; Lawton 2007; Terrill and Reisig 2003). Unfortunately, while each of these studies has utilized concepts from the social disorganization tradition, none has actually developed a social-disorganization-based explanation for why those concepts should be related to police use of force behavior. Moreover, only one has actually studied neighborhoods, in comparison to alternative levels of aggregation (Terrill and Reisig 2003). Nonetheless, they provide clues as to which aggregate-level contextual factors might be related to police officers’ use of force. Additionally, all three of these studies utilized multi-level modeling techniques, and help to demonstrate how research on policing can benefit from a more widespread incorporation of such advanced techniques. Below, I review each of these studies in detail and discuss what policing researchers have learned thus far about the neighborhood context-police use of force link, identify where there are still gaps in our understanding of that link, and finally explain how I seek to fill in those gaps in this dissertation.

In the most recent study to consider neighborhood contextual effects, Lawton (2007) examined how a variety of individual- and contextual-factors influenced the amount of
(legitimate) force police officers used on civilians. Using official use-of-force report forms (that must be completed any time an officer uses force on a civilian) from the Philadelphia Police Department in 2002, Lawton conducted a multi-level analysis of individual police-civilian encounters with a number of police district-level controls for violent crime rates and racial heterogeneity. Although Lawton’s police districts were not intended to act as proxies for neighborhoods, his findings shed light on how the effect of aggregate-level predictors on police use of force can vary across geographic space. He found that district-level violent crime rates were positively related to police officers’ use of higher levels of non-lethal force (p < 0.15), but only when individual-level factors related to the police-civilian encounter (e.g. officer and civilian race and gender, and civilian behavior during the encounter) were not included in his model. Additionally, he found that racial heterogeneity was negatively related to police use of force, this time net of individual-level encounter controls (p < 0.15).

Based on these findings, Lawton’s study provides some tentative evidence that aggregate-level factors such as racial heterogeneity can be related to police use of force behavior. While his finding that district-level violent crime rates provides more support for the criminal threat theories discussed in Chapter 2, because he measured racial heterogeneity at the district-, rather than at the neighborhood-level, it is difficult to conclude that social disorganization measures, such as racial heterogeneity, are indeed related to police officers’ use of force. Furthermore, Lawton presented no theoretical explanation for why aggregate-level racial heterogeneity should be related to police use of force. Despite these qualifications, Lawton’s study takes a step in the right direction by not only empirically examining how a concept from the social disorganization tradition influenced police use of force, but also by being one of only two extant studies that test
both individual- and aggregate-level predictors of police use of force simultaneously using multi-level modeling techniques.\textsuperscript{21}

Kane (2002) takes another step towards legitimizing a potential relationship between neighborhood social disorganization and police officers’ use of force. Using two concepts from the social disorganization tradition – concentrated disadvantage and residential mobility – Kane attempted to predict variation in police officer misconduct (including, but not specific to, police officers’ use of excessive force). He created his concentrated disadvantage and residential mobility variables using U.S. census data in the same fashion as originally constructed by Sampson and colleagues’ (1997) measures (see Sampson et al. 1997 for a discussion). He then utilized data from official police records for 75 New York City police precincts, within 20 larger police divisions, between 1975 and 1996 to conduct a multi-level analysis of police misconduct over time. The results of his analyses revealed that both disadvantage and mobility were positively and significantly related to police misconduct at both the precinct- and division-level.

The results of Kane’s study provide compelling evidence that aggregate-level concepts related to the social disorganization tradition may be related to police officers use of force. Unfortunately, as with Lawton’s (2007) study, Kane’s study was not a strong test of the relationship between neighborhood social disorganization and police use of force for a number of reasons. First, although his dependent variable measured police use of excessive force (which is the primary variable of interest for this dissertation), it was actually a scale measure of a number of types of police misconduct, including corruption, administrative policy violations, and drug test failures. Because he did not disaggregate his dependent variable by type of misconduct, it is impossible to determine how much his explanatory variables (including the two concepts from the social disorganization tradition) actually predicted variation in police officers’ use of

\textsuperscript{21}See also my review of Terrill and Reisig (2003) below.
excessive force. Second, like Lawton (2007), Kane did not actually examine neighborhood-level effects. Instead, he analyzed precinct- and division-level effects and made no effort to discuss how those aggregations might compare to neighborhoods. Thus, even though he found significant effects for concentrated disadvantage and residential mobility, his levels of aggregation are a concern. Third, Kane’s multi-level analysis did not examine any individual-level predictors of police misconduct. Although he conducted a multi-level analysis, he used two aggregate levels, precincts and divisions, and did not control for any of the individual-level factors that may also influence police misconduct. Finally, like Lawton’s (2007) study, while Kane utilized concepts from the social disorganization tradition, he made no attempt to explain why those concepts should be related to police misconduct. As a consequence, Kane’s study again provides tentative, but by no means definitive, evidence that aggregate-level contextual measures might be related to police use of excessive force.

Finally, in the only study to rigorously examine neighborhood contextual effects, Terrill and Reisig (2003) used observational data from the Project on Policing Neighborhoods (POPN) study to examine whether neighborhood concentrated disadvantage influenced the level of force police officers used on crime suspects, net of neighborhood levels of crime and a host of individual-level controls for factors related to police-suspect encounters. The results of their multi-level analysis revealed that police officers were more likely to use higher levels of force on individuals they encountered in disadvantaged neighborhoods (concentrated disadvantage was measured following the lead of Sampson et al. [1997] as well) as compared to individuals they encountered in better neighborhoods. For the very first time then, Terrill and Reisig were able to specifically demonstrate that neighborhood context could in fact predict police officers’ use of force.
Unfortunately, just as with the cases of Lawton (2007) and Kane (2002), there are limitations of Terrill and Reisig’s (2003) study that must be taken into consideration before making any broad conclusions can be made. First, they tested only one concept from the social disorganization tradition (concentrated disadvantage). While they observed a statistically significant relationship ($p < 0.05$), including only one measure is still not enough to establish a strong relationship between neighborhood social disorganization and police use of force. Second, Terrill and Reisig examined the influence of concentrated disadvantage on only the legitimate use of force by the police. Based both on their research and that done by Kane (2002), it is still unclear whether or not neighborhood context can specifically predict police officers’ use of excessive force. Finally, like the two previous studies, Terrill and Reisig again do not develop any theoretical explanations for why neighborhood context should influence police officers’ use of force behaviors. Consequently, even though their study provides the strongest evidence of a relationship between neighborhood context and police use of force thus far, it only begins to address the object of this dissertation, which is to propose and test a social disorganization theory of police use of excessive force.

The three studies reviewed above provide tentative evidence which suggests that neighborhood contextual factors might practically be used to predict police use of force. In this dissertation, I intend to improve upon those studies by empirically determining whether neighborhood social disorganization is indeed a strong and robust explanation of police officers’ use of excessive force. The two primary criticisms of the three studies discussed above were 1) they did not explain how or why neighborhood social disorganization tradition concepts should be related to police use of force and 2) they examined only measures of structural disadvantage and did not consider the effects of neighborhood social ties or neighborhood collective efficacy.
This dissertation addresses both of the above criticisms. First, in the next section of this chapter, I describe in detail exactly how and why neighborhood social disorganization should predict not only neighborhood crime rates, but also neighborhood problems with police officers’ use of excessive force. Second, I move beyond the existing research to explain how a wide variety of concepts from the social disorganization tradition should relate to the police use of excessive force. Rather than focusing solely on one or two measures of neighborhood structural disadvantage (as do the studies reviewed above), I consider how all three measures of neighborhood structural disadvantage, and how both neighborhood social ties and neighborhood collective efficacy may also influence police use of excessive force. Consequently, one of the major contributions of my dissertation to the field of policing is my proposal and empirical testing of a new social disorganization theory of police officers’ use of force.

**Linking Social Disorganization to Police Use of Force**

As studies reviewed above demonstrate, there is some preliminary evidence to suggest that concepts from the social disorganization tradition can be used to predict police behavior, possibly including officers’ use of excessive force. However, because none of the above studies fully examined the influence of neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy, the extent and nature of the relationships between those concepts and police officers’ use of excessive force is still unresolved. Additionally, while the researchers discussed above do utilize a few of the concepts found in the neighborhoods and crime literature, none of them attempted to explain the theoretical mechanisms and/or processes through which neighborhood social disorganization might lead to police use of excessive force. Therefore, in this section, I propose one possible explanation for why police officers working in
socially disorganized neighborhoods, and especially in those with low levels of collective efficacy, might be more likely to use excessive levels of force.

As reviewed in Chapter 3, the social disorganization tradition predicts that crime rates will be the highest in neighborhoods where structural disadvantage (poverty, racial/ethnic heterogeneity, and residential instability) leads to a decrease in neighborhood informal social control, social ties, and collective efficacy, rendering those neighborhoods more vulnerable to crime. In general, I expect that this same process will also cause those neighborhoods to experience higher levels of police officers’ use of excessive force – that is, I expect that just as neighborhood social disorganization signals to criminals that they will be able to get away with criminal activities, it may also signal to police officers that they will be able to get away with the use of excessive force. Furthermore, I expect that the higher rates of crime in those neighborhoods will bring more police officers into contact with residents of those neighborhoods. In turn, I expect that this will provide an even stronger signal to police officers working in socially disorganized neighborhoods that the residents are incapable of controlling any type of deviant behavior, whether they are criminal acts committed by civilians or the use of excessive force by the police. Below, I explicitly lay out how I expect neighborhood social disorganization to lead to more police use of excessive force.

The link between neighborhood social disorganization and crime rates is well-established (for a detailed review, see Sampson et al. 2002). In socially disorganized neighborhoods, residents may come to rely on formal forms of social control (i.e. the police) more heavily since, by definition, they lack the collective capacity to handle the problems that arise in their neighborhoods on their own. In other words, where high levels of structural disadvantage lead to a loss of social ties and collective efficacy, neighborhood residents lack the means necessary to
“police” themselves. Consequently, without these means, it is logical to assume that socially disorganized neighborhoods will have a greater dependence on formal forms of social control than neighborhoods that are able to “police” themselves.

The fact that socially disorganized neighborhoods may come to rely more heavily on the police has multiple implications. First, an increased dependence of neighborhood residents on formal police action may further impair residents’ collective capacity to solve problems on their own. That is, if they continuously rely on outside sources of control, they may become dependent on it completely, such that neighborhood residents no longer even consider handling problems informally. Second, while the increased dependence on formal social controls may increase police officers’ presence in the neighborhood, it might also send a message to criminals that the neighborhood is vulnerable to victimization any time the police are not present. Consequently, overall rates of crime may not decrease, as criminals simply shift their activities to times and locations when and where the police are not present. Finally, and of most importance for this dissertation, social disorganization, and the lack of collective efficacy in particular, may also signal to police officers that their own deviant behavior, including the use of excessive force, will very likely go unreported and unpunished. In other words, as police officers come to realize that residents of socially disorganized neighborhoods lack the capacity to deal with the crime problems facing their neighborhoods (without outside assistance), they may also realize that those same residents are unable to organize to prevent, deter, and/or bring about punishment for police officers’ use of excessive force. Essentially then, such neighborhoods would have just one more problem that they could not handle, and, in the end, they become doubly victimized - both by the criminals who operate in their neighborhoods and by the police who are supposed to protect them.
To summarize, I expect that police officers will use more excessive force in socially disorganized, and especially low collective efficacy, neighborhoods because, just as those neighborhoods signal to criminals that they will be able to get away with committing crimes, they also signal to police officers that they will be able to get away with abusing their authority. Before continuing on, however, I should make two notes. First, research has shown that police officers’ use of force, though rare, is more common when dealing with criminals (e.g. U.S. Department of Justice 2005). Subsequently, it should be expected that police officers will use more force in socially disorganized neighborhoods as compared to socially organized neighborhoods simply because they deal with more criminals in disorganized neighborhoods and it takes at least some force for officers to arrest those individuals (e.g. handcuffing criminal suspects). I expect that the primary difference between socially organized and socially disorganized neighborhoods may then lie in whether or not the police use force on non-criminal residents as well. That is, if both criminals and non-criminal neighborhood residents of socially disorganized neighborhoods are having force used on them, those neighborhoods should experience relatively greater levels of police use of force than their socially organized counterparts. And, because any unjustified use of force on individuals who have not committed a crime must be considered “excessive,” if the police use any force on non-criminal residents of disorganized neighborhoods, those neighborhoods should have greater problems with the police use of excessive force as a whole.  

Based on the discussion above, I therefore expect that, in addition to the “normal” levels of force police officers use against neighborhood criminals (i.e. the force necessary to effect

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22 The use of force against some non-criminal individuals may be justified when police officers’ must physically control or restrain individuals in altered mental or physical states (e.g. mentally or emotionally unstable, or under the influence of a controlled substance), but who have not committed any crimes. In such cases, if it is for the benefit of the individual, the police officer, or others, any reasonable amount of force necessary to control the situation should not be considered excessive, even if no crime has been committed.
arrests), the *additional* use of force on *non*-criminal residents will result in socially disorganized neighborhoods experiencing both more police use of legitimate force and more police use of excessive force than their socially organized counterparts. Then on top of the illegitimate, and therefore excessive, use of force on non-criminal residents of socially disorganized neighborhoods, police officers should also be more likely to use more force than is necessary to arrest neighborhood criminals, which is a form of excessive force as well. Thus, both criminal and non-criminal residents of socially disorganized neighborhoods should have more problems with police officers’ use of excessive force, compared to the residents of socially organized neighborhoods.

Second, it should also be noted that I do not wish to imply that *all* police officers working in socially disorganized neighborhoods will use excessive levels of force. Instead, I argue that *some* police officers may realize that their use of force in socially disorganized neighborhoods is much less likely to gain widespread public attention or result in their formal punishment. Then, even if a few “bad apples” use more excessive force in those neighborhoods, the overall rate of police use of excessive force should increase as well. In the worst case scenario, a greater proportion of police officers might be expected to use more excessive force in socially disorganized areas if and when their subculture dictates that such behavior is acceptable. Police subcultures can have powerful influence over police officers’ behaviors (e.g. Chappell and Piquero 2004; Skolnick 1966 Terrill Paoline, and Manning 2003; Waegel 1984a), and if the belief that people living in socially disorganized neighborhoods are more “deserving” of forceful behaviors, then officers may come to believe that the use of force is more acceptable in those neighborhoods. Nevertheless, if officers began using overly excessive levels of force on civilians (e.g. used deadly force against a misdemeanor offender), I would not expect such
behaviors to go unnoticed or unpunished simply because they occurred in a socially disorganized neighborhood. Rather, it is my contention that the “more common” abuses of force (e.g. unnecessary use of handcuffs, pressure holds, pain compliance techniques, impact maneuvers, etc.) should be more likely when police officers realize that neither neighborhood residents, nor their fellow officers, are likely to do anything to stop them.

Thus, for all the reasons discussed above, I expect that police officers will be more likely to use excessive levels of force in socially disorganized neighborhoods where residents do not have the capacity to address the problem (i.e. those with low levels of collective efficacy). Unfortunately, data do not currently exist that assess police officers’ perceptions of neighborhood collective efficacy, so it is not yet possible to test my theory definitively. However, simply discovering whether the basic pattern exists is also very important. The primary objective of this dissertation, therefore, is to link neighborhood social disorganization, and neighborhood collective efficacy more specifically, to police officers’ use of excessive force behaviors, as perceived by neighborhood residents.

**Research Questions & Hypotheses**

The primary goal of this dissertation is to examine the relationship between neighborhood social disorganization and police use of force behavior. As reviewed earlier in this chapter, a small number of recent empirical studies provide some compelling evidence that aggregate-level contextual factors can predict police use of force in general (Kane 2002; Lawton 2007; Terrill and Reisig 2003). To date, however, no research has convincingly demonstrated that the social disorganization tradition might be legitimately utilized as a theoretical framework for studying police officers’ use of excessive force. Thus, in this dissertation, I seek to answer the larger
research question – *is neighborhood social disorganization related to police officers’ use of excessive force?* Then, because most of the recent research testing concepts from the social disorganization tradition have found that the effect of neighborhood social ties (i.e. the systemic model) is not as consistent or powerful as the effect of neighborhood collective efficacy (see Chapter 2), I more specifically seek the answer to the question – *is neighborhood collective efficacy the best predictor of police use of excessive force, net of other neighborhood contextual factors such as neighborhood structural disadvantage and neighborhood social ties?*

In this section, I outline and discuss each of the specific hypotheses I test in this dissertation. I begin by considering the relationship between the three most prominent concepts from the social disorganization tradition – neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy. Although the latter two concepts are expected to mediate the relationship between the former concept and police officers’ use of excessive force, I must first demonstrate that there is a relationship between neighborhood structural disadvantage and police use of force that can be mediated. Thus, the first hypothesis I present and discuss below pertains to the direct relationship between the three neighborhood contextual factors that measure structural disadvantage and police officers’ use of excessive force.

**Neighborhood Structural Disadvantage**

In the preceding section of this chapter, I explain in detail how and why neighborhood social disorganization should be related to increased problems with the police use of excessive force. Briefly, I posited that neighborhood structural disadvantage should lead to decreases in neighborhood social ties (i.e. the systemic model), as well as neighborhood social cohesion and informal social controls (i.e. collective efficacy), which should then lead to an increase in police
officers’ use of excessive force. Nonetheless, I still expect to observe a direct relationship between neighborhood structural disadvantage and police use of excessive force. Because the concept of neighborhood structural disadvantage can be broken down into its three smaller components, I will examine the nature of each component’s relationship with police use of excessive force in order to determine which component might be the most important predictor. Therefore, I hypothesize that neighborhood poverty, racial/ethnic heterogeneity, and residential instability will all be positively related to police use of excessive force. Figure 4.1 below presents path diagrams for Hypotheses 1a through 1c.

Figure 4.1. Neighborhood Structural Disadvantage Hypotheses

**Hypothesis 1a: Poverty (Direct Effect)**

![Diagram for Hypothesis 1a]

**Hypothesis 1b: Racial/Ethnic Heterogeneity (Direct Effect)**

![Diagram for Hypothesis 1b]

**Hypothesis 1c: Residential Instability (Direct Effect)**

![Diagram for Hypothesis 1c]
Neighborhood Systemic Model

I test the neighborhood systemic model (Bursik and Grasmick 1993; Hunter 1985; Sampson and Groves 1989) by examining the relationship between the number of social ties among neighborhood residents and the police use of excessive force. While early studies found that neighborhood residents’ social ties decreased the levels of crime in their neighborhood, more recent research has suggested that residents’ social ties may actually inhibit informal social control efforts and consequently increase crime rates (e.g. Bellair 1997; Browning et al. 2004; Patillo-McCoy 1999; Sampson et al. 1997; Wilson 1996). As reviewed above, this effect was attributed to the tendency of residents with strong social ties to criminals to cover-up or hide criminal behavior in their social networks. In regards to the police use of excessive force, I expect that in neighborhoods where residents have more close social ties, more people will care about the well-being of their neighbors and be willing to act on their behalf. Consequently, in neighborhoods with larger social networks where residents are looking out for each other, I argue police officers should use less excessive force. I therefore hypothesize that neighborhood social ties will be negatively related to the police use of excessive force. Figure 4.2 below presents the path diagram for Hypothesis 2a.

In addition to the direct relationship between the two measures, I also expect that neighborhood social ties will mediate the relationship between neighborhood structural disadvantage and police use of excessive force. As reviewed in Chapter 3, the systemic model predicts that structural disadvantage will undermine neighborhood residents’ ability to form strong social ties, which will then reduce residents’ willingness to watch out for each other, and ultimately lead to an increase in crime rates. In regards to the police use of excessive force, I expect to observe a similar process of events. That is, neighborhood structural disadvantage
should decrease the number of strong social ties among neighborhood residents, which should then lead them to be less likely to watch out, or care, for what happens to their neighbors, which should, in turn, result in an increase in police officers’ use of excessive force. Thus, I hypothesize that *neighborhood social ties will mediate the relationship between neighborhood structural disadvantage and police officers’ use of excessive force*. Figure 4.2 below presents the path diagram for Hypothesis 2b.\(^{23}\)

**Figure 4.2. Neighborhood Systemic Model Hypotheses**

**Hypothesis 2a: Social Ties (Direct Effect)**

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Social Ties -> Police Use of Excessive Force
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**Hypothesis 2b: Structural Disadvantage & Social Ties (Mediating Effect)**

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Structural Disadvantage -> Social Ties
Social Ties -> Police Use of Excessive Force
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**Neighborhood Collective Efficacy**

The relationship between neighborhood collective efficacy (Morenoff et al. 2001; Sampson et al. 1999; Sampson et al. 1997) and police officers’ use of excessive force is the

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\(^{23}\) To conserve space, the three components of poverty, racial/ethnic heterogeneity, and residential instability, will be collectively depicted as “structural disadvantage” in the remaining path diagrams. However, as with Hypotheses 1a − 1c, the effects of all three components will be analyzed separately in order to determine which component of structural disadvantage is most strongly related to the mediating and dependent variables. Thus, in each of the remaining path diagrams presented in this chapter, I expect that all three components of structural disadvantage will have the same relationship (directionally) with the variable(s) they are predicting.
primary interest of this dissertation. A large body of research has shown that neighborhood collective efficacy is a strong predictor of neighborhood crime rates (for a review, see my discussion in Chapter 3). I similarly expect that neighborhood collective efficacy will be a strong predictor of police use of excessive force. Specifically, I expect that neighborhoods with low social cohesion (i.e. mutual trust and solidarity) and low informal social control (i.e. willingness to intervene for the common good of the neighborhood) will be unable to come together as a group and put a stop to police officers abusing their authority (i.e. using excessive force). I therefore hypothesize that neighborhood collective efficacy will be negatively related to police officers’ use of excessive force. Figure 4.3 below presents the path diagram for Hypothesis 3a.

As with the systemic model discussed above, I expect that neighborhood collective efficacy will not only predict police officers’ use of excessive force directly, but that it will mediate the relationship between neighborhood structural disadvantage and the police use of excessive force as well. In Chapter 3, I reviewed how neighborhood structural disadvantage makes it difficult for residents to come together, find a common set of goals and values, and then act to achieve those goals. As a result, residents never learn to trust that their neighbors will intervene if they see some inappropriate behavior or activity occurring in their neighborhood. Ultimately, this signals to criminals that they can get away with committing crimes, and higher overall rates of crime result. I expect that the same process should also lead to police officers realizing that they too can get away with inappropriate behaviors, such as the use of excessive force. Thus, I hypothesize that neighborhood collective efficacy will mediate the relationship between neighborhood structural disadvantage and police officers’ use of excessive force. Figure 4.3 below presents the path diagram for Hypothesis 3b.
Neighborhood Systemic Model vs. Neighborhood Collective Efficacy

While the primary objective of this dissertation is to establish a relationship between neighborhood social disorganization and police officers’ use of excessive force, I also seek to answer the question – *is neighborhood collective efficacy the best predictor of police use of excessive force, net of other neighborhood contextual factors such as neighborhood structural disadvantage and neighborhood social ties?* For two reasons, I expect that neighborhood collective efficacy should be a strong, significant predictor of police officers’ use of excessive force, net of both the effect of neighborhood social ties and the effect of neighborhood structural disadvantage (which should be fully mediated). First, a number of recent studies of the neighborhood systemic model have suggested that neighborhood social ties may actually foster criminal behavior (e.g. Bellair 1997; Browning et al. 2004; Patillo-McCoy 1999; Sampson et al. 1997; Wilson 1996). Second, in the only extant study to simultaneously examine the effect of neighborhood social ties and neighborhood collective efficacy on neighborhood rates of crime,
Browning and colleagues (2004) found that neighborhood collective efficacy was a better predictor of neighborhood crime rates. These studies therefore lead me to believe that neighborhood collective efficacy will be the best predictor of not only crime rates, but also neighborhood problems with police officers’ use of excessive force.24

Specifically, I believe that the mutual trust shared by neighbors to engage in informal social control behaviors (i.e. neighborhood collective efficacy) is the most important factor for reducing both problems with crime and problems with police use of excessive force, even in neighborhoods where residents do not know each other well (i.e. do not share close social ties). Sampson and colleagues once similarly argued that “[neighborhood collective efficacy] facilitates social control without requiring strong social ties or associations” (Morenoff et al. 2001:250). Subsequently, in neighborhoods where residents trust their neighbors to exercise informal social control when necessary and regardless of the strength of their ties to those neighbors, I expect there to be fewer problems not only with crime, but with police use of excessive force as well. Thus, for my fourth and final hypothesis, I hypothesize that neighborhood collective efficacy will still be the strongest and most robust predictor of officers’ use of excessive force when neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy are all tested simultaneously. Figure 4.4 below presents the path diagram for Hypothesis 4.

24 Although there is some reason to believe that it might (for a discussion see Sampson et al. 1999), I do not expect that neighborhood collective efficacy will mediate the relationship between neighborhood social ties and the police use of excessive force. Instead, I consider the systemic model and neighborhood collective efficacy to be competing explanations of both crime and the police use of excessive force. I therefore make no hypothesis regarding a mediation effect, and simply hypothesize that neighborhood collective efficacy will be the more powerful and robust predictor of police officers’ use of excessive force, net of the effect of neighborhood social ties.
Summary of Research Questions and Hypotheses

In this chapter, I reviewed the small body of research that had previously examined the relationship between aggregate-level contextual factors (which those researchers had only loosely connected to the social disorganization tradition) and police use of force. I then presented and discussed how the social disorganization tradition might be used as a framework for understanding police officers’ use of excessive force. I posited that neighborhood structural disadvantage should weaken neighborhood social ties and undermine residents’ ability to come together and control the behaviors occurring in their neighborhood, which I then argued would lead to a realization by some police officers that they would not get in trouble for using excessive levels of force, which would, in turn, an increase in the overall rates of police use of excessive force.

After developing and discussing this new theoretical framework for explaining police use of excessive force, I presented the primary research questions that I seek to answer in this dissertation. First, I attempt to answer the question: is neighborhood social disorganization related to police officers’ use of excessive force? Then, I attempt to answer the more specific
question: is neighborhood collective efficacy the best predictor of police use of excessive force, net of other neighborhood contextual factors such as neighborhood structural disadvantage and neighborhood social ties? In order to answer these questions, I then presented a number of hypotheses that will be tested using the data, measures, and analytic technique discussed in the next chapter. The following is a brief summary of those hypotheses:

**Neighborhood Structural Disadvantage**

**Hypothesis 1a:** Neighborhood poverty levels will be positively related to police officers’ use of excessive force.

**Hypothesis 1b:** Neighborhood racial/ethnic heterogeneity will be positively related to police officers’ use of excessive force.

**Hypothesis 1c:** Neighborhood residential instability will be positively related to police officers’ use of excessive force.

**Neighborhood Systemic Model**

**Hypothesis 2a:** Neighborhood social ties will be negatively related to the police use of excessive force.

**Hypothesis 2b:** Neighborhood social ties will mediate the relationship between neighborhood structural disadvantage and police officers’ use of excessive force.

**Neighborhood Collective Efficacy**

**Hypothesis 3a:** Neighborhood collective efficacy will be negatively related to police officers’ use of excessive force.

**Hypothesis 3b:** Neighborhood collective efficacy will mediate the relationship between neighborhood structural disadvantage and police officers’ use of excessive force.
Hypothesis 4: Neighborhood collective efficacy will still be the strongest and most robust predictor of officers’ use of excessive force, controlling for neighborhood structural disadvantage and neighborhood social ties.

Although the three studies reviewed at the beginning of this chapter provide some tentative evidence that aggregate-level contextual factors are related to police officers’ use of force, if I find support for the hypotheses presented here, I will be able to safely conclude that aggregate-level contextual factors can not only influence police behavior more generally, but also that neighborhood social disorganization is a viable explanation for police officers’ use of excessive force specifically. Additionally, if I find support for Hypothesis 4, which controls for other concepts from the social disorganization tradition, I will then have strong evidence that neighborhood collective efficacy, or the lack thereof, is the driving factor behind police officers’ use of excessive force in socially disorganized neighborhoods. Consequently, if I find support for both my general research questions and my more specific hypotheses, policing researchers will have a new, robust explanation for police use of force that should be considered in any future studies of neighborhoods and policing. In the next chapter, I discuss the data, measures, and analytic technique that are used to test my broader research questions and more specific hypotheses.
CHAPTER 5
DATA, MEASURES, & ANALYTIC STRATEGY

Data

The primary source of data for this dissertation is the Community Survey portion of the Project on Human Development in Chicago Neighborhoods (PHDCN: Earls et al. 1997). The Community Survey portion of the PHDCN is an ideal source of data for this study because it surveyed nearly 9,000 Chicago residents on a number of concepts from the social disorganization tradition, including their perceptions of neighborhood levels of informal social control and social cohesion (collective efficacy), their social ties to other neighborhood residents (the systemic model), as well as their perceptions of police officers’ use of excessive force. Currently, no other data source exists that contains measures of both concepts from the social disorganization tradition and measures of police officers’ use of force. Furthermore, the PHDCN also contains a number of individual-level demographic characteristics that will allow me to control for a variety of factors that may potentially influence respondents’ reports of police officers’ use of excessive force.25

The Community Survey portion of the PHDCN was originally conducted by Earls and colleagues between 1994 and 1995. Survey data come from in-home interviews of randomly selected Chicago residents 18 years of age and older. Surveyors used a three-stage sampling process to identify respondents throughout the entire city of Chicago. First, they sampled city blocks within neighborhood clusters (defined in more detail below), then dwelling units within each city block, and finally one adult resident within each sampled dwelling unit.

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25 More on how civilian reporting bias will be accounted for in this dissertation is discussed below.
The response rate for the survey was 75%, and the final sample size used for analysis was 8,765 respondents (of a possible 8,782) within 342 (of a possible 343) neighborhood clusters. On average, 26 respondents were surveyed within each neighborhood cluster (ranging from a minimum 8 respondents to a maximum of 62 respondents). Of the respondents analyzed, a slight majority were female (59%), most were racial or ethnic minorities (white 27%; black 40%; Hispanic 26%; Asian 3%; other race category 5%), nearly 60% had only a high school education or less (only a third of respondents had completed a four-year college degree, and less than 10% had graduate degrees), and approximately half of the respondents reported annual family incomes of under $25,000 (only 11% made more than $60,000 a year, and less than 2% made over $100,000). For more information on the Community Survey portion of the PHDCN, the original data, codebook, and survey instrument are available from the Inter-university Consortium for Political and Social Research (ICPSR).

Defining Neighborhoods in the PHDCN

Within the PHDCN, neighborhood clusters (referred to simply as “neighborhoods” for the remainder of this dissertation) were defined as geographically contiguous and socially homogenous communities (with respect to racial/ethnic composition, social class composition, housing density, and family structure) that took into consideration major geographic boundaries (e.g. railroad tracks, parks, freeways, etc.) as well as the researchers’ knowledge of Chicago’s local neighborhood structure (Sampson et al. 1997; Morenoff et al. 2001). 343 neighborhoods containing approximately 8,000 individuals each were identified. These neighborhoods serve as the unit of analysis for this study. Although the operationalization of “neighborhoods” in the PHDCN is intended to be “as ecologically meaningful as possible” (Sampson et. al 1997:919), it
is nonetheless important to consider how previous neighborhoods and crime researchers have defined and operationalized neighborhoods.

Researchers studying neighborhoods have long debated how to best define and operationalize their primary units of analysis. The way in which they do so can have important implications for the interpretation of neighborhood contextual effects. Should researchers use “real world” definitions based on residents’ perceptions of their neighborhood’s boundaries, or are those too subjective? Should they rely on governmental demarcations that are based primarily on population size rather than any substantive factors of interest, or are those too objective? Or should researchers create their own definitions based on the specific areas and types of phenomena they are primarily interested in studying, as was done in the PHDCN? Unfortunately, there are no clear answers to these questions.

While gathering data on residents’ perceptions of their neighborhoods’ boundaries might allow for a better “real world” operationalization, getting such information requires specific studies of certain locales, and cannot be used to make comparisons to, or broad generalizations about, other neighborhoods outside the area being studied. That is, while residents may be able to better identify the spatial (e.g. streets, buildings, or other physical landmarks) and social (e.g. the distance to friends and family) indicators that separate their own neighborhood from someone else’s neighborhood, those indicators would not apply to other places. Additionally, past research has shown that residents’ perceptions of their neighborhood’s boundaries can vary based on a variety of social and demographic characteristics, such as age, sex, race, and social class (Lee and Campbell 1997). For example, in their study of Nashville neighborhoods, Lee and Campbell (1997) found that black residents were more likely than whites to define their neighborhoods as being smaller, and through the use social, rather than spatial, indicators. Thus,
the use of residents’ perceptions to define and operationalize neighborhood boundaries can be problematic.

The use of boundaries as defined by government entities, on the other hand, entails its own set of advantages and disadvantages. The most commonly used government-defined boundaries come from the U.S. Census Bureau in the form of census tracts.\textsuperscript{26} The primary advantage of using census tracts as proxies for neighborhoods is the widespread availability of decennial data on a variety of demographic and housing-related topics across the nation. Subsequently, researchers using census tracts as their units of analysis are able to compare “neighborhoods” in many different locations and make stronger generalizations about the results they observe. However, while use of census tract boundaries as proxies for neighborhood boundaries has generally been accepted by both ecological and criminological researchers (e.g. Morenoff et al. 1997; South and Crowder 1997), such artificial, population-based boundary definitions may not accurately reflect the boundaries in which researchers’ phenomena of interest truly operates (e.g. Tienda 1991). Consequently, census tracts boundaries may not really encompass the same geographic area that the average resident might call his or her “neighborhood.” Even so, in the absence of widely available data sources that use more substantive definitions to create uniform neighborhood boundaries, the use of census tracts as proxies for neighborhoods is more the rule than the exception.

The final alternative for identifying neighborhood boundaries is to take the route chosen in the PHDCN. Rather than interviewing individuals and attempting to define neighborhood boundaries based on the reports of many residents, or using population-based governmental demarcations, researchers can also use a combination of the two approaches. In the case of the

\textsuperscript{26} Census tract boundaries are defined based on population size and are intended to encompass areas in which there approximately 4,000 residents (U.S. Census Bureau 1997).
PHDCN, researchers used individuals’ knowledge of Chicago (their own and that of Chicago city planners), spatial indicators (i.e. natural and manmade geographic boundaries), and census-based social indicators (researchers conducted cluster analyses of a variety of demographic characteristics). Unfortunately, however, as with the use of neighborhood residents’ perceptions, the use of a mixed methods approach is specific to only the city and neighborhoods in which the research is taking place. Nonetheless, since the PHDCN is currently the only existing study that has obtained information on both neighborhood social processes (i.e. social disorganization) and police use of (any type of) force, it remains an ideal data source for this dissertation. Additionally, based on previous, well-known and well-respected criminological research using the PHDCN and its operationalization of neighborhoods (e.g. Sampson et al. 1997; Sampson et al. 1999; Morenoff et al. 1997), I am confident that I will be able to accurately capture the neighborhood-level social processes that I am interested in studying.

In the remaining sections of this chapter, I discuss the measurement of my dependent variable, including a number of control variables I use to reduce measurement error in the dependent variable, and each of my explanatory variables. I conclude by addressing the methodological concerns associated with analyzing the reports of civilians clustered within neighborhoods and then discussing the analytic technique I use to test the relationship between neighborhood social disorganization and police use of excessive force.

**Measures**

**Dependent Variable – Police Use of Excessive Force**

The dependent variable for this dissertation is PHDCN respondents’ perceptions of police officers’ use of excessive force in their neighborhoods. Respondents were asked whether they
believed that police use of excessive force was “not a problem,” “somewhat of a problem,” or “a big problem?” The smallest percentage of residents reported “big” problems of police use of excessive force in their neighborhoods (7%), while slightly more (11%) reported that it was “somewhat of a problem.” Most residents (82%) reported that police use of excessive force was not a problem at all. Regrettably, however, the surveyors in the PHDCN provided no objective definition of “excessive” force to the respondents. As a consequence, how respondents responded to the question posed to them may have been dependent on two other questions that were not asked: 1) what does “force” mean to each respondent (i.e. some individuals may perceive verbal threats to be “force,” while others perceive only physical contact to be “force”), and 2) at what threshold does the use of force become excessive (i.e. some individuals may perceive higher levels of force to be justified in certain cases, while others may perceive the same amount of force to be unjustified in those cases)? Unfortunately, it is beyond the scope of this dissertation to empirically assess how individuals define “force” or at what threshold force becomes “excessive.” Fortunately, however, as I discuss in the following subsections of this chapter, I go to considerable lengths to eliminate any potential measurement error in my dependent variable that may be due to systematic civilian reporting biases.

Before moving on to the discussion of the primary explanatory variables of interest for this dissertation, it is important to briefly discuss how the results of my analyses should be interpreted. First, it should be noted that although the dependent variable is measured at the individual-level, responses are analyzed at the neighborhood-level. Thus, neighborhood differences in the average reported level of police use of excessive force serve as the primary outcome of interest. Additionally, because the survey question posed to respondents by the PHDCN researchers was not continuous in nature, ordinal regression analysis is utilized.
Subsequently, the results of my analyses will reveal whether or not any of the included explanatory variables will increase the odds that excessive force will be more of a problem in a neighborhood. More on the specific analytic strategy used in this dissertation is discussed below.

**Accounting for Reporting Bias in Civilians’ Reports of Excessive Force**

As described above, the dependent variable for this dissertation measures neighborhood residents’ *perceptions* of problems with police officers’ use of excessive force. The object of this research, however, is to observe a relationship between concepts from the social disorganization tradition and *actual police behavior*. Subsequently, the use of civilian reports of police behavior is an obvious problem. Based on the three reasons discussed below, however, I am confident that the use of this dependent variable can provide an accurate depiction of the true relationship between neighborhood social disorganization and police officers’ actual use of excessive force.

First, previous research suggests that the use of civilian reports in the study of police officers’ behaviors provides results that can be just as accurate and reliable as the two other commonly used sources of data on police behavior – *police officer self-report surveys* and *systematic observational data* (e.g. Parks 1982, 1984; Percy 1986; Son and Rome 2004). A common sense approach to critiquing civilian reports of police behaviors might suggest that both police officer self-report and systematic observation data should be more accurate and reliable sources of data on police behavior. The general argument against the use of surveys of civilian is that civilians’ reports of police behavior may be biased for, or against, the police. For example, if the spouse of a police officer is surveyed, he or she might provide a more positive report of police officers’ behaviors based on his or her general attitude toward the police. If a person with
past negative experiences with the police is surveyed, on the other hand, he or she might provide a more negative report of police behavior based on a more generally negative view of the police. Based on such arguments, some might then argue that police officer self-reports and systematic observational data should be more accurate and reliable since actual officers and trained observers are reporting on the behaviors they have done or seen, respectively. Interestingly, however, there is compelling evidence that both types of data may be no more accurate or reliable than civilian surveys, such as the PHDCN.

In an early study specifically intended to compare all three forms of data on police behavior, Parks (1982) found that police officers often report their own deviant behaviors inaccurately (including the use of excessive force) because they “face some incentives to record information that reflects favorably upon them, to the detriment of accurate recording” (1982:20). In fact, Parks concluded that there was a high level agreement between citizens’ and trained observers’ reports, and that police officer self-reports were “the primary locus of measurement error” in studies of police misconduct (1982:20). He furthermore concluded that some police officers will alter their behaviors in the presence of an observer so that even observational data may not be as accurate a reflection of “real-life” actions as many might believe. Thus, according to at least one researcher, “the evidence is not strong against the use of citizen reports” (1982:24).

More recent studies have further supported the argument that civilian reports can provide accurate and reliable data for the study of actual police use of force behaviors. Percy (1986) found that civilians’ and police officers’ evaluations of police actions were “roughly consistent” (1986:80). In another study, Mastrofski and Parks (1990) observed that some officers will admit to altering their behavior when an observer is present. Spano and Reisig (2006; see also Spano
came to a similar conclusion when they observed trained observers becoming involved in police officer-civilian encounters which ultimately altered the outcome of events. Finally, in their examination of police officer misconduct (including the use of excessive force), Son and Rome (2004) found that officers report higher levels of misconduct by their fellow police officers than do civilians. All together, these studies suggest that, in comparison to police self-report and systematic observational data, civilian reports may also be accurate and reliable resources for studying police use of force.

The second reason that I am confident that the respondents’ perceptions of police use of excessive force in the PHDCN are both accurate and reliable is that I utilize the average level of reported excessive force problems as my dependent variable. Because I use the perceptions of many respondents within each neighborhood, the neighborhood mean level of police officers’ use of excessive force is more likely to correspond with the actual level of the behavior than would the perceptions of any one respondent (for a similar argument, see Silver and Miller 2004). That is, even if a small number of respondents make inaccurate or biased reports about the problem of police use of excessive force in their neighborhood, by analyzing the average of all respondents’ reports, those inaccurate or biased reports should be evened out. Thus, when taking into account the accuracy and reliability of civilian reports in comparison to other potential data sources on police behavior, and the fact that the dependent variable for this dissertation is each neighborhood’s average level of excessive force as reported by its residents, the only reason that I must still be concerned about not accurately measuring true levels of police

27 In each of the studies discussed here (Parks 1982, 1984; Percy 1986; Son and Rome 2004), it should be noted that the researchers were comparing civilians’ observations of actual police behaviors to the observations of other police officers and trained observers. The data for my dependent variable measuring problems with police officers’ use of excessive force may capture some respondents’ actual observations of the use of excessive force by the police, but it very likely also captures respondents perceptions of problems that are not based on direct observations (i.e. based on vicarious reports of police use of excessive force told to them by other individuals).
behavior is the possibility that there might be a systematic, non-random pattern of bias related to neighborhood differences in the sample characteristics of the PHDCN.

A large body of research has found that there is in fact a systematic, non-random pattern of bias in regards to civilians’ perceptions of the police. Although this research does not specifically link perceptions of the police to inaccurate reporting of police use of excessive force, a number of studies by Weitzer and colleagues (Weitzer 1999, 2002; Weitzer and Tuch 2004) have found that many of the same factors that influence citizens’ perceptions of the police also influence citizens’ reports of police misconduct. Subsequently, because it is a form of police misconduct, it is reasonable to expect that those factors that influence civilians’ reports of police misconduct in general might also influence their reports of police use of excessive force. Thus, the third and final reason that I am confident in the use of the PHDCN data for this dissertation is that, I can use the findings of past research to identify and then account for the potential of civilian reporting bias in my analyses.

Perceptions of the Police

Civilians’ perceptions of the police is one of the most studied topics in the field of policing research (see Brown and Benedict 2002; Decker 1981). Over the last fifty years, researchers have identified a number of individual-level factors, and at least one aggregate-level factor, that have the potential to influence how civilians perceive the police. For the purposes of this dissertation, a review of these factors can help me determine what variables to include in my analyses in order to account for any bias that PHDCN respondents might have had for or against the police which could have influenced their reports regarding police officers’ behavior. I therefore argue that if I can control for those factors that may cause respondents to inaccurately
report problems with police use of excessive force, I should then be able to account for their effects statistically. In other words, if I can account for all the factors that may lead to civilian reporting bias, I can then examine how other explanatory variables are related to variation in police officers’ use of excessive force, net of those biasing factors. Thus, it is essential that I understand what factors are the most likely to influence how civilians perceive the police.

Perhaps the most consistent finding of researchers examining civilians’ perception of the police is that poor, young, minorities, with negative previous contacts with the police, and who live in high crime neighborhoods are the most likely to have negative views of the police. Of those five factors, an individual’s race or ethnic background appears to be the most important. Since the 1960s, dozens of studies have shown that minorities, and especially blacks, tend to hold more negative views of the police in comparison to whites (e.g. Bordua and Tifft 1971; Hagan and Albonetti 1982; Reisig and Parks 2000; Sampson and Jeglum-Bartusch 1998; Tuch and Weitzer 1997; Webb and Marshall 1995). Similarly, many studies have found that lower-class individuals are more likely to have negative views of the police compared to upper- and middle-class individuals (e.g. Cao, Frank, and Cullen 1996; Jacob 1971; Marenin 1983; Percy 1980; Sampson and Jeglum-Bartusch 1998).

In terms of age, many studies have also found that younger individuals view the police less favorably than do older individuals (e.g. Cao et al. 1996; Hadar and Snortum 1975; Kaminski and Jefferis 1998; Sampson and Jeglum-Bartusch 1998; Webb and Marshall 1995; Worrall 1999). Not surprisingly, a majority of research on individuals’ previous contacts with the police has also shown that those with direct previous negative experiences (e.g. being arrested, questioned, or physically or verbally abused by the police) are the most likely to hold negative views of the police, compared to those with more positive experiences (e.g. officers
responded to a request for help or helped to resolve a problem), or those with no direct positive or negative experiences (e.g. hearing stories of other people’s experiences) (e.g. Bordua and Tifft 1971; Jacob 1971; Weitzer and Tuch 2005; Worrall 1999).

Finally, there have been a handful of studies that have found a neighborhood-level contextual effect on perceptions of the police. Most of this research has focused on the effect of living in a high crime neighborhood. Specifically, these studies have indicated that individuals living in neighborhoods with high rates of crime tend to have greater fear of being victimized, and subsequently, also tend to view the police in a more negative light (e.g. Cao et al. 1996; Davis 1990; Percy 1986; Reisig and Giacomazzi 1998; Sampson and Jeglum-Bartusch 1998; Weitzer and Tuch 2005).

In addition to the five factors described above, a number of studies have also considered what influence individual-level factors such as gender and past criminal victimization may have. The findings of this research are inconsistent at best, however. In regards to gender, some researchers have found that males hold more negative views of the police (Cao et al. 1996; Hadar and Snortum 1975; Reisig and Giacomazzi 1998), while others have found that females hold more negative views (Correia, Reisig, and Lovrich 1996), and still others have found no gender effect (Marenin 1983; Murty, Roebuck, and Smith 1990; Percy 1980; Sampson and Jeglum-Bartusch 1998; Thurman and Reisig 1996; Worrall 1999). In regards to past criminal victimization experiences, results have been similarly inconsistent. Some researchers have found that individual’s who have been victimized in the past tend to hold more negative views of the police.

Some studies have considered the effect of living in different areas (e.g. census tracts, neighborhoods, cities, etc.) on perceptions of the police. Unfortunately, these studies have not clearly specified which aspects of living in different areas are expected to influence residents’ perceptions of the police (e.g. Cao et al. 1996; Jacob 1971; Murty et al. 1990). Furthermore, in two studies examining neighborhood-level compositional factors (i.e. racial/ethnic and social class compositions), Weitzer (1999, 2000) found that when controlling for their individual-level counterparts, the neighborhood-level effects disappeared. It is therefore unclear what effect neighborhood characteristics have on civilians’ perceptions of the police.
police (Carter 1985; Homant, Kennedy, and Fleming 1984), while at least one pair of researchers found that victims actually held more positive views (Thurman and Reisig 1996), and yet another pair found no effect of past victimization at all (Smith and Hawkins 1973).

**Civilian Reporting Bias Control Variables**

As reviewed above, there are a number of important factors that researchers have linked to individuals’ perceptions of the police. Although there is compelling evidence that civilian reports of police behavior can be just as accurate as police officer self-reports or systematic observational data (Parks 1982, 1984; Percy 1986; Son and Rome 2004), it is still important that I account for any potential variation in those reports that may be due to reporting bias associated with civilians’ perceptions of the police. Fortunately, the Community Survey portion of the PHDCN includes measures of many of the individual-level civilian reporting bias factors discussed above (e.g. race, age, gender, social class, and criminal victimization) and data from the Chicago Police Department on neighborhood-level crime rates is also available for inclusion in my analyses (discussed in more detail below).

Data for 11 individual-level civilian reporting bias controls come from the PHDCN. The dichotomous variable *male* measures respondents’ gender, where male = 1 and female = 0. Respondents’ *social class* is measured using a principal components factor analysis of each respondents’ highest level of education attained, annual family income, and occupational prestige (for precedent see Sampson et al. 1997; Sampson et al. 1999; Morenoff et al. 1997). Respondents’ *age* was calculated by subtracting their reported birthdates from the year the study was conducted (1994), and was then divided by ten (to ease interpretation and maintain a similar metric with the other variables included in analyses). Respondents’ *race/ethnicity* was measured
using five mutually exclusive dichotomous variables assessing the category that best represented their racial/ethnic background – white, black, Hispanic, Asian, or other. Finally, respondents’ past criminal victimization is measured using a scale of four dichotomous items assessing whether or not they have ever been the victim of a physical assault (including sexual assaults), a burglary, a larceny, or an act of vandalism. The individual-level mean for the past criminal victimization variable was 0.87, indicating that the average respondent had not experienced any criminal victimization in the past (alpha = 0.54).

Unfortunately, the PHDCN does not include any direct measures of respondents’ previous contacts with the police. However, in order to capture some of the variation in my dependent variable that might be attributable to bias associated with previous contacts with the police, I substitute a scale measure of respondents’ general feelings of legal cynicism. Respondents’ legal cynicism is a scale comprised of five items assessing how much they agreed or disagreed with statements regarding it being okay to break the law, doing whatever they want as long no one got hurt, there being no right or wrong ways to make money, domestic fighting being a private matter, and living for the day. Respondents were given the following response options, based on a 5-item Likert type scale: strongly agree, agree, neither agree nor disagree, disagree, or strongly disagree. The individual-level mean for legal cynicism was 2.35, indicating that the average respondent very slightly disagreed with all of the statements relating to legal cynicism (alpha = 0.65). Although respondents’ legal cynicism does not directly measure their past contacts with the police, the concept is likely to be related to their positive or negative views of the police. Additionally, the victimization variable discussed above may also capture some of

29 Because research has identified racial/ethnic minorities as being the more likely to hold negative views of the police, for the purposes of this dissertation, whites are used as the reference category. Therefore, in the analytic models discussed below, only the four minority group measures are included for testing, results should be interpreted as the effect of being in a particular racial/ethnic group in comparison to being white.
the variation in my dependent variable that might be due to reporting bias associated with previous police contacts.30

In addition to the 10 civilian reporting bias controls I discuss above, I include two more individual-level control variables using data from the PHDCN. Respondents’ marital status is a dichotomous variable, where married = 1 and unmarried = 0 (including never married, divorced, and widowed). Respondents’ mobility is a continuous measure assessing the number of household moves they have made in the past five years. Although there is no previous research that has linked either marital status or number of moves to civilian reporting bias, married individuals and those who have not moved often may be more settled in a neighborhood, and may have had more interactions with the police and better know the police officers who work there, for better or worse. Therefore, because each variable has the potential to influence respondents’ reports of police behavior, I include both as my final two individual-level civilian reporting bias control variables.

The final civilian reporting bias control variable that I include in my analyses is a neighborhood-level measure of crime rates. For this dissertation, only neighborhood homicide rates are examined, because measures of other crime rates for the PHDCN-defined neighborhoods are not available. This is not necessarily a problem however. Although the inclusion of other crime rates (e.g. other violent crime, property crime, or total crime) might be desirable, criminologists generally agree that data on homicides is the most accurately reported and reliably recorded (Biderman and Lynch 1991; Gove, Hughes, and Geerken 1985; O’Brien

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30 Items forming a scalar measure of respondents’ satisfaction with the police are also available in the PHDCN, but are not included in analyses because of their high correlation with the dependent variable ($r = -0.78$, $p < 0.000$), and the unclear causal relationship. While it is possible that respondents’ satisfaction with the police may lead to reporting bias affecting my dependent variable, past research has shown that police use of excessive force can also influence individuals’ satisfaction with the police (e.g. Son, Tsang, Rome, and Davis 1997). While the same argument might be applied to respondents’ more general feelings of legal cynicism, it was not as strongly correlated with the dependent variable ($r = 0.24$, $p <0.000$). Thus, because of the strong correlation with my dependent variable and the unclear causal sequencing, the scale of satisfaction with the police is not included in analyses.
Thus, for the purposes of this dissertation, homicide rates will serve as proxies for crime in general. Data for the homicide rates (per 100,000 population) variable come from the Chicago Police Department between 1991 and 1993 and are geo-coded to match neighborhood boundaries as they are defined by the PHDCN.\footnote{While many of the variables discussed in this section might be alternatively used to compare the capacity of a social disorganization explanation of police officers’ use of force to social threat or criminal threat explanations, because the primary focus of this dissertation is to come as close as possible to explaining actual police behavior, those variables that might also be used to measure other theories are instead used to account for civilian reporting bias. Implications of this study for the social threat and criminal threat theories are addressed in the final chapter of this dissertation.}

All together, I include 11 individual- and neighborhood-level civilian reporting bias controls in the analyses.\footnote{Although including a variety of neighborhood-level compositional control variables is possible using the PHDCN data (i.e. including neighborhood means of each individual-level control variable), the effect of neighborhood composition on individuals’ perceptions of the police is unclear. As reviewed above, with the exception of racial/ethnic and social class composition, there has been no research examining neighborhood compositional effects. And, the research that does examine racial/ethnic and social class compositional effects has generally found that those effects disappear when controlling for their individual-level counterparts (see Weitzer 1999, 2000). Thus, no additional neighborhood-level compositional effects are included in the analyses conducted in this dissertation.} If the results of this dissertation reveal that concepts from the social disorganization tradition are indeed related to police use of excessive force, net of these 12 controls, it will be reasonable to conclude that I have assessed variation in actual police behaviors, rather than change in respondents’ perceptions of the police.

**Primary Explanatory Variables of Interest – Neighborhood Social Disorganization**

As laid out in Chapter 4, the main objective of this dissertation is to observe a relationship between concepts from the social disorganization tradition and police officers’ use of excessive force. In order to determine whether or not this relationship truly exists, I utilize five neighborhood-level explanatory variables from the social disorganization tradition – concentrated disadvantage, concentrated immigration, residential instability, social ties, and collective efficacy.
Unlike the rest of the explanatory variables listed above, data for neighborhood concentrated disadvantage, neighborhood concentrated immigration, and neighborhood residential instability come from the 1990 U.S. Census of Population and Housing. Although the names and operationalization of these variables are not entirely consistent with the three measures of neighborhood structural disadvantage discussed in Chapter 3 (poverty, racial/ethnic heterogeneity, and residential mobility), they do capture the same three structural factors that lead to neighborhood social disorganization, and are collectively referred to as neighborhood structural disadvantage throughout the remainder of this dissertation. In fact, all three variables have been used in a similar fashion by previous social disorganization researchers (Morenoff 2001; Sampson et al. 1997; Sampson et al. 1999). Each variable was constructed using factor loadings (from a factor analysis of nine census items using alpha scoring and oblique rotation) as weights, which were then used to create disadvantage, immigration, and instability summary scales.\footnote{As briefly discussed earlier in this chapter, the level of aggregation in the PHDCN is the neighborhood, as defined by the original researchers. Census data is not available at this study-specific aggregation, however, Sampson and colleagues (1997) were able to geo-code census tracts to match the original researchers’ “neighborhood clusters.” Therefore, even though all three structural disadvantage factors were created using census tract data, they have been adjusted to fit the PHDCN’s and this dissertation’s definition of neighborhood.}

The \textit{concentrated disadvantage} factor is comprised of the neighborhood percent families living in poverty, percent families receiving public assistance (i.e. welfare), percent individuals unemployed, percent black, and percent female-headed households with children. The \textit{concentrated immigration} factor is comprised of two census characteristics, neighborhood percent Hispanic and percent foreign-born. Finally, the \textit{residential instability} factor is comprised of two census characteristics, neighborhood percent homes rented and percent residents not living in the same home in 1985. Because each of the above factors generally measure similar neighborhood structural characteristics, for the purposes of this dissertation, neighborhood
concentrated disadvantage, concentrated immigration, and residential instability serve as proxies for neighborhood poverty, racial/ethnic heterogeneity, and residential mobility respectively, and are expected to predict police officers’ use of excessive force in the same way. That is, each factor should be positively related to the dependent variable.\(^{34}\)

The remaining two neighborhood-level explanatory variables measuring concepts from the social disorganization tradition are neighborhood social ties (i.e. the systemic model) and the combination of neighborhood informal social control and neighborhood social cohesion (i.e. collective efficacy). Data for each variable come from the Community Survey portion of the PHDCN, and each variable are scalar measures comprised of the neighborhood mean level of z-scores for a variety of aggregated individual-level items (see Sampson et al. 1997; Sampson et al. 1999).\(^{35}\) Descriptive statistics for the neighborhood social ties and neighborhood collective efficacy scales are presented in Chapter 6. The original survey instruments for each item utilized here are presented in the Appendix at the end of this dissertation.

For the *neighborhood social ties* variable, I created a scale comprised of two items assessing the mean number of close relationships that respondents had with other residents of their neighborhood (for precedent, see Morenoff et al. 2001). Specifically, respondents were asked to identify the number of family members (including in-laws) and friends they had in their neighborhood, and were given the following response options: no family or friends, one or two, three to five, six to nine, or ten or more. Combined, the items in this scale measure the average

\(^{34}\) The three factors used to measure neighborhood structural disadvantage (concentrated disadvantage, concentrated immigration, and residential instability) are analyzed separately, rather than as a combined scale or latent variable, in order to determine which factor might drive the relationship between structural disadvantage and police officers’ use of excessive force.

\(^{35}\) All of my neighborhood-level explanatory variables were constructed using the neighborhood-level mean of all the respondents’ individual-level survey responses. In an econometric analysis of their similarly constructed neighborhood-level measures, Raudenbush and Sampson (1999) found that having approximately 20 respondents in each neighborhood produced sufficiently high enough levels of reliability. As described earlier in this chapter, there was an average of 26 respondents per neighborhood, so I am confident that my neighborhood-level explanatory variables are reliable measures of neighborhood social phenomena.
number of close social relationships (i.e. social ties) that respondents share within a neighborhood. The individual-level mean number of friends and family members was 2.56, indicating that the average respondent had one to five close relationships with others in their neighborhood (alpha = 0.42). This variable is used to test the neighborhood systemic model aspect of the social disorganization tradition. As discussed in Chapters 3, based on empirical tests of the systemic model, it is difficult to predict what effect neighborhood social ties will have on police officers’ use of excessive force behavior. Nevertheless, as outlined in Chapter 4, I expect that neighborhood social ties should be negatively related to the dependent variable.

The neighborhood collective efficacy scale is comprised of two other scales and was calculated using the neighborhood-level mean of all the z-scores for the respondents in each neighborhood. Data again come from the Community Survey portion of the PHDCN. Four items measure neighborhood informal social control. Respondents were asked how likely they believed their neighbors would be to intervene on behalf of the neighborhood if they witnessed children skipping school, vandalizing someone else’s property, disrespecting an adult, or fighting in an open area. Respondents were given the following response options based on a 5-item Likert type scale: very likely, likely, neither likely nor unlikely, unlikely, or very unlikely. The individual-level mean for neighborhood informal social control was 3.40, indicating that the average respondent thought that his or her neighbors were at least somewhat likely to exercise informal social control efforts (alpha = 0.82).

Six items from the PHDCN measure neighborhood social cohesion. For these items, respondents were asked how much they agreed or disagreed with statements regarding how

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36 Although the alpha for the neighborhood social ties scale is smaller than might be desired, the items used to construct the scale are the best measures available in the PHDCN. In separate analyses, not shown here, including both the neighborhood mean level of number of in-laws and friends separately (the two items in the current scale) led to similar results (available from the author). More on the effect of neighborhood social ties is discussed in the following chapters.
closely-knit their neighborhood was, whether neighbors were willing to help each other, whether neighbors did not get along well (reverse-coded), whether people tended to go their own way (reverse-coded), whether they did not shared similar values with their neighbors (reverse-coded), and whether their neighbors could be trusted. Respondents were given the following response options, again based on a 5-item Likert type scale: strongly agree, agree, neither agree nor disagree, disagree, or strongly disagree. The individual-level mean for neighborhood social cohesion was 2.75, indicating that the average respondent only very slightly agreed that neighbors got along well and shared similar values (alpha = 0.75).

At the neighborhood-level, the two scales used to create the collective efficacy scale (neighborhood informal social control and social cohesion) were highly correlated at 0.78 (p < 0.001). The neighborhood-level alpha for the two scales was 0.85. Following Sampson and colleagues (1999), the two scales were combined to measure neighborhood collective efficacy and are therefore used to determine whether or not neighborhood collective efficacy is a strong predictor of police officers’ use of excessive force. As discussed in Chapter 4, I expect that the neighborhood collective efficacy scale should be negatively related to the dependent variable.

**Analytic Strategy**

Before discussing the methodology of multi-level modeling techniques, I should first explain how I expect to be able to explain variation in police officers’ *actual* use of excessive force, rather than variation in *civilians’ perceptions* of the behavior, which might be biased for the reasons discussed earlier. In order for me to make any conclusions regarding the influence of concepts from the social disorganization tradition on actual police behavior, I must account for any potential variation in my dependent variable that might be explained by factors related to
civilians’ perceptions of the police. Based on that discussion, I then described 12 individual- and neighborhood-level control variables that I include in my analyses. Through the inclusion of these control variables, I can determine how much variation in my dependent variable may be the result of civilian reporting bias. Once I have determined how much variation is attributable to civilian reporting bias, I can then introduce my primary explanatory variables of interest to the analyses. If any of those explanatory variables are significantly related to, and explain additional variation in, my dependent variable, then I can be confident that they are related to actual police behavior, and not simply civilians’ reports of police behavior.

There is the potential that I will over-control for certain effects through the use of this strategy, however. For example, although past research has demonstrated that minorities tend to have more negative perceptions of the police (see review above), they may in fact also experience, and accurately report, more problems with police officers’ use of excessive force. If this is the case, I will have then attributed the explanation of some of the variation in my dependent variable to reporting bias, when it is really might be explained by other variables in my models.37 While this is a concern, it also means that if I obtain significant results for my primary explanatory variables of interest, I will have conducted a more conservative test (i.e. less is variation to explain to begin). Consequently, if such conservative tests nevertheless reveal significant results, then I can be even more confident that those results are robust and that a

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37 Unfortunately, it is beyond the scope of this dissertation to determine whether or not I over-control for civilian reporting. Research is needed that examines if, and potentially how much, civilians’ perceptions of the police actually bias their reporting of police behavior. While some research has shown that individuals’ who are not satisfied with the police are more likely to report police misconduct (see Weitzer 1999, 2002; Weitzer and Tuch 2004), no research has examined whether or not those individuals are more likely to inaccurately report police behavior because of their biases.
relationship between neighborhood social disorganization and police use of excessive force truly exists.

**Multi-Level Modeling Techniques**

For this dissertation, I conduct a multi-level analysis of the variables described above in order to determine if neighborhood social disorganization is related to police officers’ use of excessive force. Due to the clustered nature of the PHDCN data, I use multi-level modeling techniques that correct for the violation of the basic ordinary least squares (OLS) assumption of independence of observations. Traditional OLS regression assumes that the error terms amongst the cases being analyzed are not dependent upon other observed or unobserved factors. In the case of multi-level data, such as in the PHDCN, this assumption is violated. Because respondents who are nested (or clustered) within a neighborhood are more likely to be similar to each other than they are to be similar to respondents nested within other neighborhoods, the errors of cases within any given neighborhood may not be independent from each other. This dependence can then result in estimates of standard errors that are too small, and inaccurate significance tests. To correct for the violation of the independence of errors, I therefore employ the hierarchical linear modeling techniques (HLM) developed by Raudenbush and Bryk (2002). HLM allows me to account for this nesting effect by creating different equations and error terms for each level of data (i.e. one for respondents and one for neighborhoods).

By creating multiple equations and error terms for analysis, any similarity amongst respondents that is due to unobserved neighborhood-level differences is accounted for in the individual-level error term. In this way, the individual-level error terms are now independent from each other, thus correcting for the OLS assumption of independence. Then, with the
violation accounted for, HLM allows me to simply conduct basic OLS regression analyses. As a result of conducting analyses at multiple levels, the estimates obtained from HLM are more accurate than those that might be obtained using traditional OLS techniques. It should not be misunderstood, however, that HLM techniques will produce radically different results than OLS techniques for the same data. Rather, it is very likely that both OLS and HLM regression techniques will produce generally similar results (i.e. the same relationships between independent and dependent variables will be observed). However, because HLM provides more accurate significance tests and larger standard errors, what may have been significant in an OLS regression may not be significant in an HLM regression analysis.

In addition to producing more accurate estimates, HLM also allows me to differentiate individual-level compositional effects from neighborhood-level contextual effects. As mentioned above, because HLM creates separate equations (with separate error terms) for each level of data, I will be able to identify which effects may be due to the composition of the individuals living in a neighborhood (i.e. within-group differences) versus the general context of a neighborhood (i.e. between-group differences). In other words, HLM allows me to determine exactly how my neighborhood-level explanatory variables (i.e. social disorganization) influence police use of excessive force, while still accounting for the individual-level differences among respondents in different neighborhoods.

In terms of interpreting the neighborhood- and individual-level estimates obtained by HLM, one of the most important factors is how the individual-level variables are centered. For each of the models discussed below, all of the independent-level civilian reporting bias control variables are grand-mean centered. Although centering affects how my results should be interpreted, it does not affect model fit, predicted values, or residuals (Raudenbush and Bryk
While group-mean centering centers the variables around each neighborhood’s mean level of each variable, grand-mean centering centers the variables around the mean of the entire sample. Thus, where group-mean centering eliminates all between-neighborhood differences in the individual-level control variables, through the use of grand-mean centering, I can interpret the estimates of my neighborhood-level explanatory variables as the changes in the mean levels of police use of excessive force across neighborhoods, net of the individual-level differences among respondents in each neighborhood (i.e. the compositional effects).  

HLM also allows me to include random error terms for each variable included in my models. By adding in, or leaving out, random error terms, I can control whether or not the effect of any given variable is allowed to vary across higher levels of aggregation (Raudenbush and Bryk 2002). In other words, for this two-level analysis (individual- and neighborhood-levels), I can allow the effect of any of my individual-level control variables to vary across neighborhoods. However, because there is no theoretical reason to expect that the effect of my individual-level controls (e.g. race, ethnicity, age, gender) should vary from one neighborhood to another, I do not include any random error terms at the individual-level. The only random error term that is included in any of the models presented below is therefore set on my neighborhood-level intercept. By doing so, I set the effects of each of my individual-level control variables as being fixed within each neighborhood, but, at the same time, I also allow each neighborhood’s intercept, or constant, to be different. I therefore interpret the estimates of the models presented

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38 In separate analyses (not shown here, available from the author), the neighborhood mean levels of each individual-level reporting bias control were included in analyses to control for neighborhood compositional effects. Those analyses showed that there were no significant neighborhood-level compositional effects, and the exclusion of those additional controls did not drastically alter the magnitude of the estimates for any of the primary explanatory variables of interest (the five variables measuring concepts from the social disorganization tradition). As a result, the neighborhood-level means of the individual-level reporting bias controls were excluded from the final analyses discussed here (available from the author).  

121
in the following sub-section as changes from different levels of police use of excessive force across each neighborhood in Chicago.

All together, through the use of HLM techniques, I am able to account for the methodological problems of analyzing multiple levels of data, separately specify and analyze individual- and neighborhood-level effects, control how my results should be interpreted, and determine what variables, if any, should be allowed to vary across neighborhoods. As a result of having so much control over my analyses, I will be able to not only test the viability of a new explanation for police use of force behavior, but I will potentially also be able to improve upon the existing police use of force literature by demonstrating a new use for multi-level modeling techniques that should prove useful for any future studies of police use of force that utilize civilian survey data.

As I discussed earlier in this chapter, civilian reporting bias has the potential to manifest as measurement error in my dependent variable, police officers’ use of excessive force. However, by incorporating individual- and neighborhood-level control measures into my analyses, I can effectively account for any variation in respondents’ reports of police use of excessive force behavior that may be due to either positive or negative perceptions of the police in general. Thus, in addition to further demonstrating the usefulness of multi-level modeling techniques for studying police officers’ behaviors at the neighborhood-level, my research also provides police use of force researchers with a better way to account for reporting bias whenever they use civilian surveys to study police behavior.
**Models for Analysis**

The primary objective of this dissertation is to establish a relationship between neighborhood social disorganization and the police use of force behaviors. More specifically, I am interested in studying the neighborhood-level relationships between the three concepts from the social disorganization tradition and police use of excessive force, net of my multi-level controls for civilian reporting bias. In order to do so, I analyze a number of multi-level models using HLM to predict variation in the mean levels of police officers’ use of excessive force across Chicago neighborhoods.

The first three models I present test Hypotheses 1a – 1c, Hypothesis 2a, and Hypothesis 3a (presented in Chapter 4) and are intended to examine the relationship between each of the three concepts from the social disorganization tradition (neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy) and police officers’ use of excessive force. Although these models do not include any of the individual- or neighborhood-level civilian reporting bias controls, it is important that I determine whether any relationships exist in the first place. If they do not, it would be unnecessary to incorporate any control variables. Thus, Models 1 through 3 test whether or not each of the three concepts from the social disorganization tradition can independently predict variation in neighborhood mean levels of police officers’ use of excessive force, without controlling for any other factors. The individual- and neighborhood-level equations for the first three models can therefore be depicted as:
Model 1 – Neighborhood Structural Disadvantage Model

\[ Y_{ij} = \beta_{0j} + r, \]
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{Concentrated Disadvantage} + \gamma_{02} \text{Concentrated Immigration} + \gamma_{03} \text{Residential Instability} + u_0 \]

Model 2 – Neighborhood Systemic Model

\[ Y_{ij} = \beta_{0j} + r, \]
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{Social Ties} + u_0 \]

Model 3 – Neighborhood Collective Efficacy Model

\[ Y_{ij} = \beta_{0j} + r, \]
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{Collective Efficacy} + u_0 \]

If Models 1 – 3 reveal that neighborhood structural factors are related to police use of excessive force, Hypotheses 1a – 1c will be supported and I will have preliminary evidence that neighborhood social disorganization may be a viable explanation for police use of excessive force. However, the potential for measurement error in my dependent variable necessitates that I determine how much variation might be explained by civilian reporting bias before I draw any definitive conclusions. Therefore the fourth model I analyze tests the 13 civilian reporting bias control variables and can be depicted using the following individual- and neighborhood-level equations:
Model 4 – Civilian Reporting Bias Controls

\[ Y_{ij} = \beta_{0j} + \beta_1 \text{Legal Cynicism} + \beta_2 \text{Police Satisfaction} + \beta_3 \text{Crime Victimization} + \beta_4 \text{Male} \]
\[ + \beta_5 \text{SES} + \beta_6 \text{Married} + \beta_7 \text{Number of Moves Past 5 Years} + \beta_8 \text{Age/10} \]
\[ + \beta_9 \text{Black} + \beta_{10} \text{Hispanic} + \beta_{11} \text{Asian} + \beta_{12} \text{Other Race} + r \]
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{NH Crime} + u_0 \]

Model 4 allows me to assess the amount of variation in the dependent variable that may be due to civilian reporting bias. If Model 4 does not predict all of the variation in the dependent variable, it is safe to say that any remaining unexplained variation represents variation in police officers’ actual use of excessive force.\(^{39}\) Accordingly, the next three models again test Hypotheses 1a – 1c, Hypothesis 2a, and Hypothesis 3a, but this time test the capacity of each of the three concepts from the social disorganization tradition to explain actual police behavior, net of the controls for civilian reporting bias. Models 5 through 7 can be depicted using the following individual- and neighborhood-level equations:

Model 5 – Neighborhood Structural Disadvantage Model (Plus Controls)

\[ Y_{ij} = \beta_{0j} + \beta_1 \text{Legal Cynicism} + \beta_2 \text{Police Satisfaction} + \beta_3 \text{Crime Victimization} + \beta_4 \text{Male} \]
\[ + \beta_5 \text{SES} + \beta_6 \text{Married} + \beta_7 \text{Number of Moves Past 5 Years} + \beta_8 \text{Age/10} \]
\[ + \beta_9 \text{Black} + \beta_{10} \text{Hispanic} + \beta_{11} \text{Asian} + \beta_{12} \text{Other Race} + r \]

\(^{39}\) The 12 civilian reporting bias control variables used in this dissertation measure the most commonly identified factors that influence civilians’ perceptions of the police. Although I argue that the inclusion of these control variables eliminates any potential reporting bias, it is possible that there are other factors which may also influence civilians’ reporting of police behavior and should be included in my analyses. Unfortunately, such research is beyond the scope of this dissertation, and until new potential sources of reporting bias are identified, I am confident that the control variables utilized in this dissertation adequately capture the large majority of any bias that might exist.
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{NH Crime} + \gamma_{02} \text{Concentrated Disadvantage} + \gamma_{03} \text{Concentrated Immigration} \]
\[ + \gamma_{04} \text{Residential Instability} + u_0 \]

**Model 6 – Neighborhood Systemic Model (Plus Controls)**

\[ Y_{ij} = \beta_{0j} + \beta_1 \text{Legal Cynicism} + \beta_2 \text{Police Satisfaction} + \beta_3 \text{Crime Victimization} + \beta_4 \text{Male} \]
\[ + \beta_5 \text{SES} + \beta_6 \text{Married} + \beta_7 \text{Number of Moves Past 5 Years} + \beta_8 \text{Age/10} \]
\[ + \beta_9 \text{Black} + \beta_{10} \text{Hispanic} + \beta_{11} \text{Asian} + \beta_{12} \text{Other Race} + r \]
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{NH Crime} + \gamma_{02} \text{Social Ties} + u_0 \]

**Model 7 – Neighborhood Collective Efficacy Model (Plus Controls)**

\[ Y_{ij} = \beta_{0j} + \beta_1 \text{Legal Cynicism} + \beta_2 \text{Police Satisfaction} + \beta_3 \text{Crime Victimization} + \beta_4 \text{Male} \]
\[ + \beta_5 \text{SES} + \beta_6 \text{Married} + \beta_7 \text{Number of Moves Past 5 Years} + \beta_8 \text{Age/10} \]
\[ + \beta_9 \text{Black} + \beta_{10} \text{Hispanic} + \beta_{11} \text{Asian} + \beta_{12} \text{Other Race} + r \]
\[ \beta_{0j} = \gamma_{00} + \gamma_{01} \text{NH Crime} + \gamma_{02} \text{Collective Efficacy} + u_0 \]

Models 5 through 7 separately assess the direct effects of each of the three concepts from the social disorganization tradition that are of primary interest for this dissertation, net of the civilian reporting bias controls. As outlined in Chapter 4, I expect that neighborhood structural disadvantage should be positively related to police use of excessive force, and that neighborhood social ties and collective efficacy should each be negatively related police use of excessive force. If I observe that all three concepts are in fact significant predictors of police officers’ use of excessive force, net of the civilian reporting bias controls, I will have strong support for Hypotheses 1a – 1c, Hypothesis 2a, and Hypothesis 3a. Consequently, I should then be able to
draw more definitive conclusions about the relationship between neighborhood social disorganization and police use of force behaviors.

Based on how the three concepts are theorized to be related to the dependent variable (see Chapter 4), however, neighborhood social ties and collective efficacy should mediate the relationship between neighborhood structural disadvantage and police use of excessive force. In order to test for this mediation effect, I therefore separately introduce the neighborhood social ties and neighborhood collective efficacy variables into Model 5, thereby assessing the direct and indirect effects of structural disadvantage, net of controls. Subsequently, the eighth and ninth models analyzed in this dissertation test Hypothesis 2b and Hypothesis 3b, and can be depicted using the following individual- and neighborhood-level equations:

Model 8 – Neighborhood Systemic Mediation Model (Plus Controls)

\[
Y_{ij} = \beta_{0j} + \beta_1 \text{Legal Cynicism} + \beta_2 \text{Police Satisfaction} + \beta_3 \text{Crime Victimization} + \beta_4 \text{Male} + \beta_5 \text{SES} \\
+ \beta_6 \text{Married} + \beta_7 \text{Number of Moves Past 5 Years} + \beta_8 \text{Age/10} + \beta_9 \text{Black} \\
+ \beta_{10} \text{Hispanic} + \beta_{11} \text{Asian} + \beta_{12} \text{Other Race} + r
\]

\[
\beta_{0j} = \gamma_{00} + \gamma_{01} \text{NH Crime} + \gamma_{02} \text{Concentrated Disadvantage} \\
+ \gamma_{03} \text{Concentrated Immigration} + \gamma_{04} \text{Residential Instability} + \gamma_{05} \text{Social Ties} \\
+ u_0
\]

Model 9 – Neighborhood Collective Efficacy Mediation Model (Plus Controls)

\[
Y_{ij} = \beta_{0j} + \beta_1 \text{Legal Cynicism} + \beta_2 \text{Police Satisfaction} + \beta_3 \text{Crime Victimization} + \beta_4 \text{Male} + \beta_5 \text{SES} \\
+ \beta_6 \text{Married} + \beta_7 \text{Number of Moves Past 5 Years} + \beta_8 \text{Age/10} + \beta_9 \text{Black} \\
+ \beta_{10} \text{Hispanic} + \beta_{11} \text{Asian} + \beta_{12} \text{Other Race} + r
\]
\[
\beta_{0j} = \gamma_{00} + \gamma_{01} NH Crime + \gamma_{02} Concentrated Disadvantage + \gamma_{03} Concentrated Immigration
+ \gamma_{04} Residential Instability + \gamma_{05} Collective Efficacy + u_0
\]

Models 8 and 9 allow me to assess whether or not neighborhood social ties and neighborhood collective efficacy mediate the relationship between neighborhood structural disadvantage and police use of force as my social disorganization theory of police use of force suggests (see Chapter 4 for details). If I observe a significant mediating relationship, net of civilian reporting bias controls, I will have strong support for Hypothesis 2b and Hypothesis 3b, indicating that neighborhood social disorganization is indeed a valuable theory for explaining police officers’ use of force.

I test Hypothesis 4 using one last model in order to determine which of the more recent modifications to Shaw & McKay’s original theory – the systemic model or the collective efficacy model – is the more robust explanation of police officers’ use of excessive force. The tenth and final model analyzed in this dissertation therefore includes all three of the concepts from the social disorganization tradition together with the civilian reporting bias controls, and can be depicted using the following individual- and neighborhood-level equations:

Model 10 – Full Neighborhood Social Disorganization Model (Plus Controls)

\[
Y_{ij} = \beta_{0j} + \beta_1 Legal Cynicism + \beta_2 Police Satisfaction + \beta_3 Crime Victimization + \beta_4 Male + \beta_5 SES
+ \beta_6 Married + \beta_7 Number of Moves Past 5 Years + \beta_8 Age/10 + \beta_9 Black
+ \beta_{10} Hispanic + \beta_{11} Asian + \beta_{12} Other Race + r
\]

\[
\beta_{0j} = \gamma_{00} + \gamma_{01} NH Crime + \gamma_{02} Concentrated Disadvantage + \gamma_{03} Concentrated Immigration
+ \gamma_{04} Residential Instability + \gamma_{05} Social Ties + \gamma_{06} Collective Efficacy + u_0
\]
The results of Model 10 will allow me to determine which modification of the original theory of social disorganization is most useful for explaining police officers’ use of excessive force. Based on the recent empirical tests of the two concepts’ capacity for predicting neighborhood crime rates (see Chapter 3), and as outlined in Chapter 4, I expect that the collective efficacy variable will be best predictor of police officers’ use of excessive force, net of both neighborhood structural disadvantage, neighborhood social ties, and the civilian reporting bias controls. Should I observe this outcome, I will then have strong support for Hypothesis 4 and finally be able to conclude that the social disorganization tradition in general, and the neighborhood collective efficacy explanation in particular, constitutes not only a viable new theoretical explanation particularly for police officers’ use of excessive force, but also a valuable new approach for studying police behaviors more broadly. In the next chapter, I discuss the results of each of the above analyses.
CHAPTER 6
RESULTS

In this chapter, I present and discuss the results of the analyses described in Chapter 5. I first present the individual- and neighborhood-level descriptive statistics for each of the variables included in my analyses. Next, I present and briefly discuss the results of my bivariate analyses which assess the possibility of a relationship between the three main concepts from the social disorganization tradition that I focus on in this dissertation and police officers’ use of excessive force (i.e. Models 1 through 3 examine the effects of neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy). Then, because it is evident that bivariate relationships do in fact exist, I next discuss the results of my civilian reporting bias control analyses (Model 4) in order to determine how much variation in my dependent variable is due to the individual- and neighborhood-level factors past researchers have identified as predictors of civilians’ perceptions of the police. Finally, because there is still a large amount of variation in police officers’ use of excessive force still left to be explained, I conclude this chapter by presenting and briefly discussing the results of my multivariate analyses which include the controls for my civilian reporting bias measures. The results of these analyses will allow me to answer my two larger research questions and my eight more specific hypotheses (see 40 As discussed below, I present and discuss the results for each of the three measures of neighborhood structural disadvantage together. Although this is not technically a bivariate analysis, I discuss the effect of all three measures in this section of Chapter 6 because the three measures – concentrated disadvantage, concentrated immigration, and residential instability – are intended to assess the effect of the singular concept of neighborhood structural disadvantage on police use of excessive force. Moreover, because the three measures are typically analyzed together in the neighborhoods and crime literature (e.g. Morenoff et al. 1997; Sampson et al. 1997; Sampson et al. 1999), I do so here as well.)
Chapter 4) regarding the relationship between neighborhood social disorganization and police use of force behaviors.

**Descriptive Results**

Table 6.1 below presents the descriptive statistics for all of the variables analyzed in this dissertation, including the dependent variable (police use of excessive force), my primary explanatory variables of interest (the three concepts from the social disorganization tradition), and each of the individual- and neighborhood-level controls for civilian reporting bias. Beginning with my dependent variable, there appears to be variation at the individual-level. At the individual-level, the dependent variable ranged from a minimum of 1 to a maximum of 3, with a standard deviation of 0.58.\(^{41}\) Additionally, the mean level of police officers’ use of excessive force was 1.25, indicating that the average respondent thought that use of excessive force by the police in his or her neighborhoods was not much of a problem.

Turning to the neighborhood-level explanatory variables of primary interest in this dissertation (measuring neighborhood social disorganization), there appears to be considerable variation across Chicago neighborhoods. First, regarding the three measures of neighborhood structural disadvantage, neighborhood concentrated disadvantage had a mean of 0 and ranged from a low of -1.65 to a high of 3.81, with a standard deviation of 0.99. Neighborhood concentrated immigration also had a mean of 0 and ranged from -1.63 to 3.07, with a standard deviation of 0.97. Finally, neighborhood residential instability had a mean of 0 as well, and ranged from a minimum of -2.33 to a maximum of 2.18, with a standard deviation of 0.98. Next,

\(^{41}\) As discussed in Chapter 5, although the dependent variable, police officers’ use of excessive force, is measured at the individual-level, for the purposes of analyses, HLM assesses the effect of all the explanatory and control variables on changes from the neighborhood mean level of police use of excessive force. More on the neighborhood-level variation in the dependent variable is discussed below.
Table 6.1. Descriptive Statistics

<table>
<thead>
<tr>
<th>Neighborhood-Level Variables (n_j = 342)</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
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<tr>
<td>Concentrated Disadvantage</td>
<td>0.00</td>
<td>0.99</td>
<td>-1.65</td>
<td>3.81</td>
</tr>
<tr>
<td>Concentrated Immigration</td>
<td>0.00</td>
<td>0.97</td>
<td>-1.63</td>
<td>3.07</td>
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<tr>
<td>Residential Instability</td>
<td>0.00</td>
<td>0.98</td>
<td>-2.33</td>
<td>2.18</td>
</tr>
<tr>
<td>Social Ties</td>
<td>2.57</td>
<td>0.37</td>
<td>1.59</td>
<td>3.91</td>
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<td>Collective Efficacy</td>
<td>3.89</td>
<td>0.26</td>
<td>3.17</td>
<td>4.73</td>
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<tr>
<td>Crime Rate</td>
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<td>0.96</td>
<td>1.30</td>
<td>5.10</td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>Individual-Level Variables (n_i = 8765)</th>
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</thead>
<tbody>
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<td>Police Use of Excessive Force</td>
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<tr>
<td>White</td>
</tr>
<tr>
<td>Black</td>
</tr>
<tr>
<td>Hispanic</td>
</tr>
<tr>
<td>Asian</td>
</tr>
<tr>
<td>Other Race</td>
</tr>
<tr>
<td>Age/10</td>
</tr>
<tr>
<td>Male</td>
</tr>
<tr>
<td>Socio-Economic Status</td>
</tr>
<tr>
<td>Married</td>
</tr>
<tr>
<td>Number of Moves Past 5 Years</td>
</tr>
<tr>
<td>Legal Cynicism</td>
</tr>
<tr>
<td>Past Victimization</td>
</tr>
</tbody>
</table>
my measure of the systemic model, neighborhood social ties, had a mean of 2.57, indicating that the average respondent had one to five close relationships with others in their neighborhood. The neighborhood social ties measure ranged from a low 1.59 to a high of 3.91, with a standard deviation of 0.37. Finally, the measure of neighborhood collective efficacy had a mean of 3.89, indicating that the average respondent thought that his or her neighbors shared some similar values and goals and were at least somewhat willing to intervene on the behalf of the neighborhood. The neighborhood collective efficacy variable ranged from a minimum of 3.17 to 4.73, with a standard deviation of 0.26.

In regards to the civilian bias control variables, there is a good amount of variation in the single neighborhood-level measure of crime. The neighborhood crime rate variable (homicides) had a mean of 3.14 and ranged from a minimum of 1.30 to a maximum of 5.10, with a standard deviation of 0.96. At the individual-level, 27% of the respondents were white, 40% were black, 26% were Hispanic, 3% were Asian, and 5% identified themselves as being of some other race. Additionally, 41% of the respondents were male and 35% were married. The mean socio-economic status for respondents was 0 and ranged from -4.08 to 4.33, with a standard deviation of 1.24. Turning to age, the average respondent was approximately 43 years of age, and respondents’ age ranged from 18 to 100 years old, with a standard deviation of 16 years. On average, respondents moved less than once in the past five years, while the total number of moves ranged from 0 to 5, with a standard deviation of 1.20. In regards to legal cynicism, the average respondent was not very cynical, while reports ranged from a low of 1 to a high of 5, with a standard deviation of 0.66. Finally, the average respondent had not been the victim of any crimes, although victimization ranged from a minimum of 0 experiences to a maximum of 4 previous experiences, with a standard deviation of 1.01.
Before describing my results, it is important to establish that there is variation in the dependent variable that can be explained. Unlike other multi-level analyses using HLM, the ordinal regressions that I conduct do not report both within- and between-neighborhood variance components. Instead, HLM reports only the between-neighborhood variance component. Fortunately, this is not a problem for the purposes of this dissertation. Because I am interested in what influence each of the variables described above has on the mean level of police officers’ use of excessive force across neighborhoods, only the between-neighborhood variance component is necessary.

In order to obtain the between-neighborhood variance component for my dependent variable, I ran an analysis of only my dependent variable, police use of excessive force, with no covariates included in the model. The results of this null model revealed that there is indeed significant variation in the mean level of police use of excessive force across neighborhoods. The between-neighborhoods variance component was 0.931 (p < 0.001). In the sections that follow, I will discuss the amount of between-neighborhood variation which can be attributed to the explanatory and/or control variables that are included in each model using the following equation: \( y = ((0.931 - x)/0.931) \times 100 \), where \( y \) represents the percentage of the variation explained by the covariates, 0.931 is the total amount of variation that is available to be explained, and \( x \) represents the variance component for each specific model that is tested. Below, I present the results of the bivariate analyses first.

**Bivariate Results**

Table 6.2 below presents the results of my three bivariate tests of Models 1 through 3 (presented in Chapter 5). These bivariate tests were necessary in order to demonstrate that a link
between neighborhood social disorganization and police officers’ use of force did in fact exist. Had none of the three concepts from the social disorganization tradition – neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy – been significant predictors of police use of excessive force, then none of the following analyses would have been necessary. Fortunately, however, with only one exception, all the primary explanatory variables were significantly related to the dependent variable.

Model 1 in Table 6.2 shows the bivariate results for the effect of neighborhood structural disadvantage on police officers’ use of excessive force. Although Model 1 is not a true bivariate analysis of the effect of each measure of neighborhood structural disadvantage on the dependent variable, because the three measures are commonly analyzed together and are intended to collectively measure one concept, I report on the effect of all three included in a single model. Therefore, as Model 1 shows, each of the three measures of neighborhood structural disadvantage are positively and significantly (p < 0.001) related to police officers’ use of excessive force. Of the three measures in this preliminary analysis, it appears that neighborhood concentrated disadvantage has the strongest effect on police use of excessive force, while neighborhood residential instability has the weakest. More on the substantive meaning of these effects is discussed later. Turning then to the amount of variation explained by Model 1, it appears that when no other explanatory variables or controls are included in the model, neighborhood structural disadvantage explains 70% of the variation in the dependent variable.42

Thus, at least in these preliminary analyses, neighborhood structural disadvantage appears to be

---

42 Although it may seem surprising that a single concept from the social disorganization tradition explains so much variation in my dependent variable (70%), recall that neighborhood structural disadvantage is comprised of three separate measures, all of which are comprised of a number of other measures (see Chapter 5 for details). Subsequently, the concept of neighborhood structural disadvantage is actually measuring the influence of a large number of factors on police use of excessive force, so it should not be wholly unexpected that such a large amount of variation is accounted for in Model 1.
Table 6.2. Neighborhood-Level Bivariate Relationships with Police Use of Excessive Force (n_j = 342; n_i = 8765)

<table>
<thead>
<tr>
<th>Neighborhood-Level Variables</th>
<th>Model 1</th>
<th></th>
<th></th>
<th>Model 2</th>
<th></th>
<th></th>
<th>Model 3</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Concentrated Disadvantage</td>
<td>0.78</td>
<td>2.18 ***</td>
<td>(0.05)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concentrated Immigration</td>
<td>0.32</td>
<td>1.38 ***</td>
<td>(0.04)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Residential Instability</td>
<td>0.14</td>
<td>1.15 ***</td>
<td>(0.05)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Social Ties</td>
<td>-0.15</td>
<td>0.86</td>
<td>(0.16)</td>
<td>-1.26</td>
<td>0.29 ***</td>
<td>(0.20)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Collective Efficacy</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Variance Explained</td>
<td>70%</td>
<td></td>
<td></td>
<td>7%</td>
<td></td>
<td></td>
<td>52%</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: *** p < 0.001; ** p < 0.01; * p < 0.05
an important predictor of police officers’ use of excessive force. The findings of Model 1 therefore provide some initial support for my neighborhood structural disadvantage hypotheses (Hypotheses 1a through 1c) described in Chapter 4.

Model 2 in Table 6.2 displays the results for bivariate effect of neighborhood social ties on police use of excessive force. Neighborhood social ties were not significantly related to the dependent variable, although the effect was in the expected direction. 43 This result is at least partially surprising since previous research on the systemic model has shown a significant, negative effect of neighborhood social ties on crime rates, but not too surprising given that other research has found that those same ties might actually promote crime (e.g. Bellair 1997; Browning et al. 2004; Patillo-McCoy 1999; Sampson et al. 1997; Wilson 1996). Nonetheless, in regards to its effect on police officers’ use of excessive force, the systemic model does not appear to be a viable explanation. The findings of Model 2 therefore provide some preliminary evidence that challenges my neighborhood systemic model hypotheses (Hypotheses 2a and 2c) described in Chapter 4.

Finally, Model 3 in Table 6.2 shows the results of the bivariate test of neighborhood collective efficacy on police officers’ use of excessive force. As expected, neighborhood collective efficacy was negatively and significantly (p < 0.001) related to police officers’ use of excessive force. Moreover, this bivariate test reveals that neighborhood collective efficacy has the potential to explain upwards of 52% of the variation in police officers’ use of excessive force. Although this test does not include any covariates, Model 3 nonetheless provides some tentative support for the collective efficacy hypotheses (Hypotheses 3a and 3b) described in Chapter 4.

43 Although the effect of neighborhood social ties was non-significant, with no covariates in the model, neighborhood social ties did explain 7% of the between-neighborhood variance in police officers’ use of excessive force.
To briefly summarize, Models 1 through 3 in Table 6.2 provide some initial support for relationships between two of the concepts from the social disorganization tradition and police use of excessive force. With the exception of the effect of neighborhood social ties, there is reason to believe that neighborhood social disorganization has the potential to be a viable new theoretical framework for explaining police use of excessive force. However, before any definitive conclusions are drawn, it must be shown that the concepts from the social disorganization tradition can explain variation in police use of excessive force net of controls for civilian reporting bias. In the next section of this chapter, I present the results of my analysis of the many factors typically associated with civilians’ perceptions of the police, and which have the potential to bias how respondents in the PHDCN report about police behaviors, including the police use of excessive force.

Civilian Reporting Bias Results

Table 6.3 below presents the results of my civilian reporting bias controls analysis (Model 4 from Chapter 5). In order to determine how much of the variation in police officers’ use of excessive force may be attributable to factors associated with civilians’ perceptions of the police, I included all 11 of the individual-level control variables, and the single neighborhood-level control variable, that were discussed in Chapter 5. As previously discussed, if there is any variation left in the dependent variable once I have accounted for the effects of the civilian reporting bias controls, this remaining variation likely represents true variation in police officers’ use of excessive force behaviors, rather than any reporting bias due to civilians’ perceptions of the police.44

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44 As previously discussed in Chapter 5, there is the possibility that I over-control for civilian reporting bias and eliminate some of the variation in police officers’ use of excessive force that may be due to other factors. For
example, minority respondents might not only make potentially biased reports about police use of force, but they may also actually experience more use of excessive force at the hands of the police. However, if I am able to obtain significant results for the concepts from the social disorganization tradition in which I am primarily interested, then I will have conducted a more conservative test than if I had not over-controlled for civilian reporting bias. Thus, while over-controlling for civilian reporting bias is not a major problem for this dissertation, it is an interesting topic for future research on civilians’ perceptions of, and the actual use of, police excessive force behaviors.

<table>
<thead>
<tr>
<th>Model 4</th>
<th>( \beta )</th>
<th>( \text{Exp}(\beta) )</th>
<th>Std. Err.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant 1</td>
<td>-3.31</td>
<td>0.04 ***</td>
<td>(0.21)</td>
</tr>
<tr>
<td>Constant 2</td>
<td>-1.16</td>
<td>0.31 ***</td>
<td>(0.04)</td>
</tr>
</tbody>
</table>

**Neighborhood-Level Variables (n_j = 342)**

<table>
<thead>
<tr>
<th></th>
<th>( \beta )</th>
<th>( \text{Exp}(\beta) )</th>
<th>Std. Err.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Crime Rate</td>
<td>0.47</td>
<td>1.60 ***</td>
<td>(0.06)</td>
</tr>
</tbody>
</table>

**Individual-Level Variables (n_i = 8765)**

<table>
<thead>
<tr>
<th></th>
<th>( \beta )</th>
<th>( \text{Exp}(\beta) )</th>
<th>Std. Err.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black</td>
<td>0.69</td>
<td>2.00 ***</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.75</td>
<td>2.12 ***</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Asian</td>
<td>0.24</td>
<td>1.27 ***</td>
<td>(0.22)</td>
</tr>
<tr>
<td>Other Race</td>
<td>0.11</td>
<td>1.11 *</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Age/10</td>
<td>-0.64</td>
<td>0.53 ***</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Male</td>
<td>0.17</td>
<td>1.18 ***</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Socioeconomic Status</td>
<td>-0.41</td>
<td>0.67 ***</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Married</td>
<td>0.04</td>
<td>1.04</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Number of Moves Past 5 Years</td>
<td>-0.04</td>
<td>0.96</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Legal Cynicism</td>
<td>0.31</td>
<td>1.36 ***</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Past Victimization</td>
<td>0.40</td>
<td>1.49 ***</td>
<td>(0.03)</td>
</tr>
</tbody>
</table>

Variance Explained 65%

Notes: *** p < 0.001; ** p < 0.01; * p < 0.05
Before discussing the amount of variation the civilian reporting bias control variables account for in my dependent variable, let me briefly describe some of the observed relationships. At the neighborhood-level, neighborhood crime rates were positively and significantly related to respondents’ reports of police officers’ using excessive force. At the individual-level, all racial/ethnic minority respondents had greater odds of reporting problems with police officers’ use of excessive force in comparison to white respondents. Older and higher socioeconomic status respondents had lower odds for reporting problems, while male respondents had greater odds of reporting problems with police use of excessive force. Not surprisingly, respondents who had higher levels of legal cynicism, or who had been criminally victimized in the past also reported more problems with police use of excessive force. Finally, neither marital status nor the number of moves in the past five years for each respondent were significantly related to respondents’ reports of police use of excessive force. At both the neighborhood- and individual-level then, these results are consistent with past research (see Chapter 5). Nevertheless, because I am primarily interested in the relationships between the concepts from the social disorganization tradition and police officers’ actual use of excessive force, the most important finding from Table 6.3 is that there is still a substantial amount of variation left to be explained even after accounting for the civilian reporting bias controls.

As Table 6.3 shows, the individual- and neighborhood-level civilian reporting bias control variables combine to account for only 65% of the variation in the dependent variable. While 65% of the variation may seem like a large percentage of the variation in police officers’

---

45 In the multivariate analyses discussed below, the effects of the individual- and neighborhood-level civilian reporting bias controls stay largely the same as they are reported here. That is, with the exception of small changes in effect size and the effect of a few variables becoming non-significant in later models, there are no major changes to the civilian reporting bias estimates discussed here. Consequently, because the concepts from the social disorganization tradition are of primary interest for this dissertation, I will not discuss any of the estimates of the civilian reporting bias controls throughout the remainder of this chapter.
use of excessive force, there is still a good amount of variation (35%) that remains unexplained. Subsequently, if the introduction of any of the concepts from the social disorganization tradition into this control model explains any additional variation in police officers’ use of excessive force, then it is safe to conclude that those concepts are explaining part of that 35% of the variation in the dependent variable that is not attributable to civilian reporting bias. Thus, for the remainder of the models discussed in this chapter (that all include the 12 civilian reporting bias controls), I will discuss how much additional variation in the dependent variable is attributable to the primary explanatory variables of interest above and beyond the amount of variation attributable to the control variables. Additionally, as I have explained both above and in Chapter 5, once I have accounted for the percentage of variation attributable to the civilian reporting bias controls, I will discuss any additional variance explained (above 65%) as the amount of variation explained in actual police use of excessive force.

**Multivariate Results**

Table 6.4 below presents the results of my multivariate tests of Models 5 through 10, as they were presented in Chapter 5. Briefly, Models 5 through 7 assess the effect of neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy on police use of excessive force, respectively, net of the 12 civilian reporting bias controls. Model 8 assesses whether or not neighborhood social ties mediates the relationship between neighborhood structural disadvantage and police use of excessive force. Model 9 assesses whether or not neighborhood collective efficacy mediates that same relationship. Finally, Model 10 includes all the variables used in this dissertation and assesses which concept from the social disorganization tradition is best predictor of police officers’ use of excessive force net of not
### Table 6.4. Ordinal Regression Models Predicting Police Use of Excessive Force

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 5</th>
<th>Model 6</th>
<th>Model 7</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\beta$</td>
<td>Exp ($\beta$)</td>
<td>Std. Err.</td>
</tr>
<tr>
<td>Constant 1</td>
<td>-2.49</td>
<td>0.08***</td>
<td>(0.25)</td>
</tr>
<tr>
<td>Constant 2</td>
<td>-1.17</td>
<td>0.31***</td>
<td>(0.04)</td>
</tr>
<tr>
<td><strong>Neighborhood-Level Variables ($n_j = 342$)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concentrated Disadvantage</td>
<td>0.37</td>
<td>1.45***</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Concentrated Immigration</td>
<td>0.25</td>
<td>1.29***</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Residential Instability</td>
<td>0.17</td>
<td>1.18***</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Social Ties</td>
<td></td>
<td>-0.01</td>
<td>0.99</td>
</tr>
<tr>
<td>Collective Efficacy</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crime Rate</td>
<td>0.19</td>
<td>1.21*</td>
<td>(0.08)</td>
</tr>
<tr>
<td><strong>Individual-Level Variables ($n_i = 8765$)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>0.82</td>
<td>2.26***</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.65</td>
<td>1.91***</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Asian</td>
<td>0.23</td>
<td>1.26***</td>
<td>(0.22)</td>
</tr>
<tr>
<td>Other Race</td>
<td>0.10</td>
<td>1.10</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Age/10</td>
<td>-0.60</td>
<td>0.55***</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Male</td>
<td>0.18</td>
<td>1.20***</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Socioeconomic Status</td>
<td>-0.35</td>
<td>0.71***</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Married</td>
<td>-0.03</td>
<td>0.97</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Number of Moves Past 5 Years</td>
<td>-0.06</td>
<td>0.94</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Legal Cynicism</td>
<td>0.30</td>
<td>1.34***</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Past Victimization</td>
<td>0.40</td>
<td>1.49***</td>
<td>(0.03)</td>
</tr>
<tr>
<td><strong>Variance Explained</strong></td>
<td>76%</td>
<td>64%</td>
<td>71%</td>
</tr>
</tbody>
</table>

Notes: *** p < 0.001; ** p < 0.01; * p < 0.05
Table 6.4. Ordinal Regression Models Predicting Police Use of Excessive Force (Contd.)

<table>
<thead>
<tr>
<th></th>
<th>Model 8</th>
<th>Model 9</th>
<th>Model 10</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\beta$</td>
<td>Exp ($\beta$)</td>
<td>Std. Err.</td>
</tr>
<tr>
<td>Constant 1</td>
<td>-2.37</td>
<td>0.09 ***</td>
<td>(0.42)</td>
</tr>
<tr>
<td>Constant 2</td>
<td>-1.17</td>
<td>0.31 ***</td>
<td>(0.04)</td>
</tr>
<tr>
<td><strong>Neighborhood-Level Variables ($n_i = 342$)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Concentrated Disadvantage</td>
<td>0.38</td>
<td>1.46 ***</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Concentrated Immigration</td>
<td>0.25</td>
<td>1.29 ***</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Residential Instability</td>
<td>0.17</td>
<td>1.18 ***</td>
<td>(0.05)</td>
</tr>
<tr>
<td>Social Ties</td>
<td>-0.03</td>
<td>0.97</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Collective Efficacy</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Crime Rate</td>
<td>0.19</td>
<td>1.21 ***</td>
<td>(0.08)</td>
</tr>
<tr>
<td><strong>Individual-Level Variables ($n_i = 8765$)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>0.81</td>
<td>2.25 ***</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.65</td>
<td>1.91 ***</td>
<td>(0.11)</td>
</tr>
<tr>
<td>Asian</td>
<td>0.23</td>
<td>1.26 ***</td>
<td>(0.20)</td>
</tr>
<tr>
<td>Other Race</td>
<td>0.10</td>
<td>1.10</td>
<td>(0.15)</td>
</tr>
<tr>
<td>Age/10</td>
<td>-0.61</td>
<td>0.55 ***</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Male</td>
<td>0.18</td>
<td>1.19 ***</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Socioeconomic Status</td>
<td>-0.34</td>
<td>0.71 ***</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Married</td>
<td>-0.03</td>
<td>0.97</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Number of Moves Past 5 Years</td>
<td>-0.06</td>
<td>0.94</td>
<td>(0.03)</td>
</tr>
<tr>
<td>Legal Cynicism</td>
<td>0.30</td>
<td>1.34 ***</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Past Victimization</td>
<td>0.40</td>
<td>1.49 ***</td>
<td>(0.03)</td>
</tr>
</tbody>
</table>

Variance Explained

<table>
<thead>
<tr>
<th></th>
<th>76%</th>
<th>78%</th>
<th>78%</th>
</tr>
</thead>
</table>

Notes: *** p < 0.001; ** p < 0.01; * p < 0.05
only the civilian reporting bias controls, but net of the other concepts as well.

As displayed in Model 5 of Table 6.4, neighborhood structural disadvantage is positively and significantly related to police officers’ use of excessive force, net of the individual- and neighborhood-level civilian reporting bias controls. As expected, each of the three measures of neighborhood structural disadvantage is positively related to the dependent variable, providing more support for Hypotheses 1a through 1c. Specifically, net of the civilian reporting bias controls, a one standard deviation unit increase in neighborhood concentrated disadvantage corresponds with a 45% increase in the odds that police use of excessive force will become more of a problem (p < 0.001). Similarly, for each one standard deviation unit increase in concentrated immigration and residential instability, the odds that police use of excessive force will become more of a problem increase by 29% and 18% respectively (p ≤ 0.001). Thus, it appears that, of the three measures of neighborhood structural disadvantage, the concentrated disadvantage variable (comprised of percent in poverty, on welfare, unemployed, black, and living in female-headed households with kids) is the strongest predictor of police officers’ use of excessive force. Together, however, the three measures account for 11% percent of the variation in actual police use of excessive force behavior (76% - 65% = 11%). Consequently, Hypotheses 1a through 1c are supported by the data, and after only one model, there appears to be some compelling evidence that the neighborhood social disorganization tradition might be a viable theoretical framework for studying police use of force behavior.

Model 6 in Table 6.4 shows the results for the systemic model, net of the individual- and neighborhood-level civilian reporting bias controls. Although Model 2 above showed no significant relationship between neighborhood social ties and police officers’ use of excessive force, I test for the relationship again, this time including the civilian reporting bias controls to
assess whether a suppression effect might exist. As shown in Model 6, however, no such effect is evident. As was found in Model 2 (discussed above), neighborhood social ties were not significantly related to police officers’ use of force, although they did predict change in the dependent variable in the correct direction once again. Not surprisingly, the addition of the neighborhood social ties measure contributed no additional explanation of the variance in the dependent variable (Model 4) (65% - 65% = 0%). Thus, based on Model 6, Hypotheses 2a is not supported by the data. While this finding does not necessarily preclude the possibility of a theoretical framework for explaining police use of force behavior that is based on the social disorganization tradition, it does make the possibility of a neighborhood systemic model explanation very unlikely.

In comparison to the findings for the systemic model, the possibility of a collective efficacy explanation of police use of excessive force appears to have much more support. As shown in Model 7 of Table 6.4, net of the individual- and neighborhood-level civilian reporting bias controls, neighborhood collective efficacy is strongly related to police officers’ use of excessive force. I hypothesized in Chapter 4 that neighborhood collective efficacy would be negatively related to the dependent variable (Hypotheses 3a and 3b), and Model 7 bears this out. Specifically, a one standard deviation unit increase in a neighborhood’s level of collective efficacy corresponds to a 48% decrease in the odds that police officers’ use of excessive force will become more of a problem (p < 0.001). Of the models discussed thus far, the effect of neighborhood collective efficacy is the largest in magnitude. However, compared to Model 5 from above, the introduction of neighborhood collective efficacy into the base civilian reporting bias model (Model 4) does not explain as much additional variation in the dependent variable. That is, neighborhood collective efficacy only explains 6% of the variation in police use of
excessive force (71% - 65% = 6%), whereas neighborhood structural disadvantage explained 11%. Nonetheless, based on the findings of Model 7, there is not only more evidence for a general social disorganization tradition theoretical framework for the explanation of police use of excessive force, but there is also strong evidence of a more specific neighborhood collective efficacy framework. Hypothesis 3a is therefore supported by the data.

To briefly summarize the findings of Models 5 through 7, neighborhood structural disadvantage and neighborhood collective efficacy were both strongly and significantly related to police officers’ use of excessive force in the expected directions. The effect of neighborhood social ties, however, was not significant. So, while Model 5 and Model 7 provide some compelling evidence that at least some of the concepts from the social disorganization tradition are related to police use of force behavior, to truly test the viability of social disorganization tradition theoretical framework, the data should also reveal that neighborhood social ties and neighborhood collective efficacy mediate the neighborhood structural disadvantage-police use of excessive force relationship. Thus, in Models 8 and 9 discussed below, I present the results of my two social disorganization mediation analyses.

Model 8 in Table 6.4 displays the results of the neighborhood systemic mediation model, net of the civilian reporting bias controls. Model 8 tests Hypothesis 2b from Chapter 4. As is evident from the table, however, no support is found for this hypothesis. Once again, neighborhood social ties were not significantly related to police use of excessive force. Interestingly, in comparison to Model 5, the magnitude of the concentrated disadvantage variable in Model 8 is approximately 2% greater after adding the neighborhood social ties measure (p < 0.001). This was the only important change between Models 5 and 8, however. The introduction of the neighborhood social ties variable again had no effect on the amount of
variance explained. Just as was observed in Model 5, Model 8 explained only 11% of the variation in police use of excessive force (76% - 65% = 11%). Based on Model 8 then, the data show no evidence that the amount of social ties that residents had with other people in their neighborhood is an important factor in predicting police officers’ use of force. Hypothesis 2a and 2b are therefore not supported by the data and a neighborhood systemic model explanation of police use of excessive force does not appear viable.

Unlike the results for Model 8, Model 9 in Table 6.4 reveals that neighborhood collective efficacy is indeed a viable explanation for police use of excessive force. Model 9 specifically tests Hypothesis 3b from Chapter 4, and although a full mediation effect is not observed, the results show that the introduction of the neighborhood collective efficacy variable does at least partially mediate the relationship between neighborhood structural disadvantage and police officers’ use of excessive force. Specifically, in comparison to Model 5, the introduction of the neighborhood collective efficacy variable reduced the magnitude of the effect of neighborhood concentrated disadvantage by 19% (p < 0.001), the effect of neighborhood concentrated immigration by 14% (p < 0.001), and the effect of neighborhood residential instability by 35% (and also reduced its level of significance to p < 0.05). The effect of neighborhood collective efficacy also was reduced. In comparison to Model 7, the magnitude of the effect of neighborhood collective efficacy in Model 9 is reduced by approximately 49%. Nonetheless, among the neighborhood-level explanatory variables of interest in Model 9, the neighborhood collective efficacy variable still had one of the strongest impacts on the dependent variable. That is, net of all the other covariates, a one standard deviation unit increase in neighborhood collective efficacy reduces the odds of having more problems with police officers’ use of excessive force by 28% (the level of significance is reduced to p < 0.01 as well). In comparison,
increases in neighborhood concentrated disadvantage correspond to a 37% increase in the odds that police officers’ use of excessive force is a problem, while increases in neighborhood concentrated immigration correspond to a 25% increase in the odds, and increases in neighborhood residential instability correspond to only a 12% increase in the odds. Thus, even though the magnitude of their effects are reduced by introducing the neighborhood collective efficacy variable into the model, the three measures of neighborhood structural still have significant, direct effects on police officers’ use of excessive force.

In regards to the amount of variation explained in Model 9, the combined effects of neighborhood structural disadvantage and neighborhood collective efficacy account for a total of 13% of the variation in police use of excessive force (78% - 65% = 13%). Compared to the structural-disadvantage-only model (Model 5), Model 10 explains an additional 2% of the variation in the dependent variable. While the inclusion of neighborhood collective efficacy does not explain a large amount of the variation in police officers’ use of excessive force above and beyond the structural-disadvantage-only model, combined they do account for well over a third of the total variation in the dependent variable (above and beyond the variation explained by the civilian reporting bias controls). Thus, even though the results of Model 9 show that neighborhood collective efficacy does not completely mediate the neighborhood structural disadvantage-police use of excessive force relationship, and it only accounts for a small additional percentage of the explained variance, it does partially mediate that relationship, and, of all the measures examined, it has one of the greatest impacts on police use of excessive force. Hypothesis 3b is therefore partially supported by the data, indicating that the social disorganization tradition more generally, and neighborhood collective efficacy in particular, may in fact be strong theoretical frameworks for explaining police officers’ use of excessive force.
The final model analyzed in this dissertation combines all three of the concepts from the social disorganization tradition – neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy – into a single analysis that tests my fourth hypothesis and my second larger research question regarding the importance of neighborhood collective efficacy within the social disorganization tradition. Briefly, I questioned whether or not neighborhood collective efficacy was the most important factor in predicting police use of excessive force in comparison to the other two concepts from the social disorganization tradition. Then, based on the arguments I laid out in Chapter 4, I hypothesized that net of all the other social disorganization tradition concepts, and net of all the individual- and neighborhood-level civilian reporting bias controls, neighborhood collective efficacy would be the best predictor of police officers’ use of excessive force. Should Hypothesis 4 be supported by the data, I will have very compelling evidence that neighborhood collective efficacy is the driving force behind a social disorganization tradition explanation for police officers’ use of excessive force behaviors.

Model 10 in Table 6.4 displays the results of my empirical test of Hypothesis 4. As shown, net of all the other covariates, neighborhood collective efficacy has one of the strongest influences on the odds that police use of excessive force is a problem relative to all the other measures from the social disorganization tradition. Somewhat surprisingly, however, it is actually neighborhood concentrated disadvantage that has the greatest impact on police use of excessive force. Specifically, a one standard deviation increase in neighborhood concentrated disadvantage corresponds with a 35% increase in the odds that police use of force will become more of a problem, compared to a one standard deviation increase in neighborhood collective efficacy, which corresponds to a 30% decrease in the odds. One possible explanation for this result is the large number of neighborhood structural characteristics that were captured by the
concentrated disadvantage measure (i.e. both racial and economic compositional characteristics, see Chapter 5 for details). Because it actually measured the largest number of neighborhood structural characteristics of all the variables utilized for this dissertation, it should not be too surprising that it has such a large impact on police use of excessive force. Nonetheless, neighborhood collective efficacy still has the second largest influence on police use of excessive force, providing at least partial support for Hypothesis 4.

In addition to this interesting finding, the inclusion of the neighborhood social ties variable from Model 9 to Model 10 appears to result in a suppression effect, such that a one unit increase in neighborhood collective efficacy now corresponds with a 2% greater decrease in the odds that police officers’ use of excessive force is a problem (p < 0.01). Substantively, what all these findings mean is that neighborhoods with high levels of collective efficacy (i.e. high social cohesion and high levels of informal social control), have much lower odds of having problems with police using excessive levels of force than do neighborhoods with lower levels of collective efficacy, net not only of the civilian reporting bias controls, but also neighborhood structural disadvantage and neighborhood social ties.

To help illustrate the effect of neighborhood collective efficacy, Figure 6.1 graphically depicts the predicted probabilities of low, average, and high collective efficacy neighborhoods having more problems with police use of excessive force (i.e. moving up a level on the scale of the dependent variable), holding all the other covariates in the model constant. As the figure shows, neighborhoods with low levels of collective efficacy are much more likely to experience

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46 To calculate the predicted probabilities presented in Figures 6.1, 6.2 (below), and 6.3 (further below), I created an equation using the estimates obtained from Model 10 and entered the minimum, mean, and maximum values for neighborhood collective efficacy and neighborhood structural disadvantage. The values of all the other neighborhood-level variables (including neighborhood crime rate) that were not of primary interest for each specific figure were held constant at their neighborhood-level means. Additionally, because all of the individual-level civilian reporting bias control variables were grand-mean centered, their total effect at the neighborhood level is equal to zero, so the individual-level estimates were not included in the equation to calculate predicted probabilities.
problems with police officers’ use of force (27.9%) than are neighborhoods with high levels of collective efficacy (5.4%). Thus, based on this final model, which includes all the other social disorganization tradition concepts and civilian reporting bias controls, it appears that neighborhood collective efficacy is both a very strong, and very robust, predictor of police use of excessive force.

Figure 6.1. Neighborhood Collective Efficacy and the Predicted Probability of Police Use of Excessive Force becoming More of a Problem

While the results of Model 10 revealed only a small change in the magnitude of the effect for neighborhood collective efficacy, there were a few other small differences between the final model and some of the earlier models that were tested. The biggest change of note between Model 10 and several earlier models was the effect of neighborhood social ties switching from having a negative relationship with police officers’ use of excessive force to having a positive
relationship. Because the estimate for neighborhood social ties is still non-significant, however, it is difficult to determine what this change in effect truly means. The only other small differences between the models relate to the change in effect sizes of the three measures of neighborhood structural disadvantage. Compared to Model 9 (the neighborhood collective efficacy mediation model), the magnitude of the effects for neighborhood concentrated disadvantage and neighborhood concentrated immigration were reduced by less than 1% each, although both remained significant (p < 0.001), when the measure of neighborhood social ties is introduced into the analysis. The effect size and significance of neighborhood residential instability was unaffected between Models 9 and 10. Thus, it appears that neighborhood structural disadvantage has a direct effect on police use of excessive force, net of both neighborhood social ties and neighborhood collective efficacy. Substantively, this means neighborhoods with high levels of disadvantage, immigration, and residential instability have greater odds of experiencing problems with police use of excessive force than do neighborhoods with lower levels, and that this effect is not fully mediated by neighborhood social ties or levels of collective efficacy.

To illustrate the effect of neighborhood structural disadvantage, net of all other variables included in this analysis, Figure 6.2 below graphically depicts the predicted probabilities of having problems with police use of excessive force for low, mean, and high structurally disadvantaged neighborhoods. As the figure shows, the combined effects of all three measures of neighborhood structural disadvantage have a very strong influence on the probability of police use of excessive force being a problem. Specifically, neighborhoods with the lowest levels of structural disadvantage have only a 6.4% chance of having problems, while neighborhoods with

47 Because the effect of neighborhood social ties was not statistically significant, I do not present the predicted probabilities of neighborhoods with high, mean, and low levels of the variable having problems with police use of excessive force here (available from the author).
the greatest amount of structural disadvantage have a staggering 32.3% chance of experiencing problems with police use of excessive force. Consequently, it appears that the combined effect of all three neighborhood structural disadvantage measures has the strongest effect on the likelihood that a neighborhood will experience problems with police officers’ use of excessive force.

Figure 6.2. Neighborhood Structural Disadvantage and the Predicted Probability of Police Use of Excessive Force becoming More of a Problem

Together with the results of Model 10, Figures 6.1 and 6.2 suggest that both neighborhood structural disadvantage and neighborhood collective efficacy are strong, robust predictors of police officers’ use of excessive force behaviors, and that the influence of neighborhood social ties is negligible. On its own, neighborhood collective efficacy has one of the greatest influence on the odds that police use of force is a problem in a neighborhood, but the single neighborhood concentrated disadvantage factor and the combined effects of all three
measures of neighborhood structural disadvantage were the strongest predictors that a neighborhood would have problems with police officers’ use of excessive force. Thus, when all the concepts from the social disorganization tradition are included in the same model simultaneously, the data partially support Hypothesis 4, such that neighborhood collective efficacy is one of the strongest predictors of police officers’ use of excessive force, but only when compared to two of the three separate measures of neighborhood structural disadvantage and the single measure of neighborhood social ties.

In addition to partially supporting Hypothesis 4, the combined effects of all the concepts from the social disorganization tradition explain 13% of the variation in police use of excessive force behaviors (78% - 65% = 13%). In comparison to Model 9, Model 10 explains no more variation in the dependent variable. This is not surprising, however, because the only difference between the two models was the introduction of the neighborhood social ties variable. That is, because the effect of neighborhood social ties was very small in Model 10, and because it was non-significant, it cannot be expected to explain any more variation in police officers’ use of excessive force than in Model 9. Nevertheless, the results of Model 10 affirmatively answers my larger research question regarding the possibility of a relationship between neighborhood social disorganization and police use of excessive force. In other words, with the exception of the neighborhood systemic model, the evidence from the analyses presented above strongly suggests that the social disorganization tradition is a viable and strong theoretical framework for explaining police use of excessive force. The evidence furthermore suggests that the residents of socially disorganized neighborhoods are indeed doubly victimized by both the criminals who prey on them, and the police officers who are supposed to protect them.
Summary of Main Findings

Based on the analyses conducted in this dissertation, my main findings include:

Neighborhood Structural Disadvantage Hypotheses

1. Hypotheses 1a through 1c were supported by the data. All three measures of neighborhood structural disadvantage were positively and significantly related to police use of excessive force.

Neighborhood Systemic Model Hypotheses

2. Hypotheses 2a and 2b were not supported by the data. Neighborhood social ties were not significantly related to police use of excessive force, and did not mediate the relationship between neighborhood structural disadvantage and police use of excessive force.

Neighborhood Collective Efficacy Hypotheses

3. Hypothesis 3a was supported by the data, and Hypothesis 3b was partially supported by the data. Neighborhood collective efficacy was negatively and significantly related to police use of excessive force, but could only partially mediate the relationship between neighborhood structural disadvantage and police use of excessive force.

Full Neighborhood Social Disorganization Tradition Hypothesis

4. Hypothesis 4 was partially supported by the data. While neighborhood collective efficacy was one of the best predictors of the dependent variable, neighborhood concentrated disadvantage alone, and the combined effect of all three neighborhood
structural disadvantage measures, had the greatest overall influences on police use of excessive force.

**First General Research Question**

5. With the exception of neighborhood social ties, the other measures of the neighborhood social disorganization tradition were all significantly related to the dependent variable. The social disorganization tradition is therefore a viable theoretical framework for the explanation of police use of excessive force.

**Second General Research Question**

6. Neighborhood collective efficacy was one of the strongest predictors of police use of excessive force. However, the single measure of neighborhood concentrated disadvantage, and the combined effect of all three measures of neighborhood structural disadvantage actually had the greatest influence on police use of excessive force. Nonetheless, neighborhood collective efficacy appears to be a very important force driving the relationship between neighborhood social disorganization and police use of excessive force.
CHAPTER 7

DISCUSSION & CONCLUSION

How and why police officers use force against American civilians has been, and always will be, a controversial topic of much debate, and the focus of empirical research. The better we understand the intricacies of police use of force behaviors, the more we can do to address the rare, but tremendously consequential, instances of both legitimate and illegitimate police use of force. While even the legitimate use of force on civilians can have profound consequences for victims, police officers, and society as a whole, the use of illegitimate force by the police can also lead to racial/ethnic and class-based tensions between communities and the police officers who patrol them, which may in turn lead to protests, riots, and social violence, and the delegitimization of police and state authority. With such high stakes, it is no wonder that policing researchers have spent so much time and energy attempting to better understand the situations and people involved (both civilians and officers alike) that give rise to the illegitimate use of force by the police.

The primary goal of this dissertation was to add to our understanding of why police officers use one form of illegitimate force. Specifically, I considered how neighborhood context might influence police officers’ use of excessive force. Through my focus on neighborhoods and the police use of excessive force, I have added to our existing understanding of police use of force behavior by making three primary contributions. First, I conducted a detailed review of all the theory-driven empirical research that has been published over the last twenty years. Then, after arguing that research on police use of force could benefit from further theoretical
development, I also contributed to the methodological development of research on police use of force through the utilization of multi-level modeling techniques in ways previously unconsidered. Finally, I proposed that theory and concepts from the neighborhoods and crime literature might be appropriated and utilized to help us better understand police use of force at the neighborhood-level. More specifically, the final contribution of this dissertation was my application of the social disorganization tradition as a theoretical framework for studying police use of excessive force, and the incorporation of a variety of concepts from the neighborhoods and crime and social disorganization tradition literatures.

In addition to the overarching goal of contributing to a better understanding of police officers’ use of force, I also had two overarching research questions that I sought to answer in this dissertation. First, I asked whether or not neighborhood social disorganization could predict police officers’ use of excessive force. Second, I asked whether or not neighborhood collective efficacy was the most important factor in predicting police officers’ use of excessive force in comparison to other concepts from the social disorganization tradition. In the remainder of this chapter, I review how the results of my multi-level analyses helped me answer those more general research questions, as well as my more specific hypotheses. After reviewing the results of my analyses, I discuss the theoretical and policy implications of my findings. Finally, I conclude this dissertation by reviewing the limitations of my research and discussing a number of directions for future research based on those limitations.

Discussion of Results

In order to answer the two larger research questions restated above, I conducted a number of multi-level analyses of police officers’ use of excessive force that tested four sets of
hypotheses, each incorporating different concepts, or groups of concepts, from the social disorganization tradition. First, I hypothesized that neighborhood structural disadvantage would be positively related to police officers’ use of excessive force. Second, in a test of the systemic model, I hypothesized that neighborhood social ties would be negatively related to police officers’ use of excessive force, and that they would each mediate the relationship between neighborhood structural disadvantage and police use of excessive force. Third, I hypothesized that neighborhood collective efficacy would be negatively related to police officers’ use of excessive force, and that it would mediate the neighborhood structural disadvantage-police use of excessive force relationship. Finally, I hypothesized that, when all the concepts from the social disorganization tradition were included in a single analysis simultaneously, neighborhood collective efficacy would be the strongest predictor of police officers’ use of excessive force.

After controlling for a number of individual- and neighborhood-level factors related to civilian perceptions of the police, I found full or partial support for three of my four sets of hypotheses. Specifically, for my first set of hypotheses, I found that neighborhood structural disadvantage is one of the strongest predictors of police officers’ use of excessive force. The three measures of neighborhood structural disadvantage – neighborhood concentrated disadvantage, neighborhood concentrated immigration, and neighborhood residential instability – were all positively and significantly related to the dependent variable. Substantively, these results suggest that neighborhoods with high levels of poverty, racial/ethnic tension, and mobility are more likely to experience problems with police use of excessive force, just as my application of the social disorganization tradition predicted. I argued that just as disadvantaged neighborhoods are more vulnerable to crime, they should also be more vulnerable to police officers’ abuse of their authority.
While I expected to observe a relationship between neighborhood structural disadvantage and police officers’ use of excessive force, I also expected that relationship to be fully mediated by neighborhood social ties and neighborhood collective efficacy – the two mechanisms through which I expected neighborhood structural disadvantage to make neighborhoods more vulnerable to both criminals and police officers’ use of excessive force. However, in both of the mediation models that I tested, I found that the effect of neighborhood structural disadvantage remained both strong and significant. Moreover, in my final model that included neighborhood structural disadvantage, neighborhood social ties, and neighborhood collective efficacy simultaneously, the three measures of neighborhood structural disadvantage were still strong and significant predictors of police officers’ use of excessive force, and neighborhood concentrated disadvantage actually had a larger influence than neighborhood collective efficacy. This suggests that even net of two popular and commonly tested mediating concepts from the social disorganization tradition, neighborhood structural disadvantage still has a strong, direct effect on police use of excessive force. And, even though these findings challenge several of my hypotheses, they do not necessarily rule out the possibility of future use of the social disorganization tradition as a theoretical framework for explaining police use of force behavior.

On the contrary, the results mentioned above suggest only that neighborhood social ties and neighborhood collective efficacy are not the only ecological processes occurring within a neighborhood that might mediate the effect of neighborhood structural disadvantage on police use of excessive force. It might be that neighborhoods with high levels of structural disadvantage have (historically) sharp tensions between residents and the police that make civilians more fearful of the police so that they are unable or unwilling to come together to stand up to the police use of excessive force. Or, perhaps, residents of structurally disadvantage
neighborhoods are less likely to have the time, knowledge, or other resources necessary to file formal complaints against officers who use excessive force in their neighborhood. Or, it may also simply be the case that there is a direct effect of living in a highly disadvantaged neighborhood on police officers’ use of excessive force. It is certainly possible that some of the other aspects associated with living in a structurally disadvantaged neighborhood could also signal to police officers that they will be able to get away with the use of excessive force. Using a broken windows-type explanation (e.g. Wilson and Kelling 1982), for example, structurally disadvantaged neighborhoods will likely also have high levels of physical (e.g. graffiti, litter, and barred or broken windows) and social (e.g. loitering, panhandling, and public drug use) disorder, which can also signal to criminals and police officers alike that their deviant or illegal behavior will not be confronted or controlled. Unfortunately, an analysis of the physical and social disorder is beyond the scope of this dissertation, but does provide an interesting direction for future research. Even though the results of this dissertation challenge my mediation hypotheses, they nonetheless support my first set of hypotheses and provide some very compelling evidence that neighborhood structural disadvantage is in fact related to police officers’ use of excessive force.

In addition to the surprising direct effect of neighborhood structural disadvantage on police use of excessive force, another somewhat unexpected finding of this dissertation was the lack of support for my second set of hypotheses testing the neighborhood systemic model. Although some recent research has called into question the importance of neighborhood social ties in determining neighborhood crime rates (e.g. Bellair 1997; Browning et al. 2004; Patillo-McCoy 1999; Sampson et al. 1997; Wilson 1996), it is still surprising that the number of close familial and friendship ties that are shared among residents of a neighborhood have no influence
on police officers’ use of excessive force. Even in my bivariate tests, neighborhood social ties had no significant impact on police use of excessive force. Substantively, this suggests that social ties within a neighborhood do not influence the likelihood of the neighborhood being able to confront, and put a stop to, police officers’ abuse of authority. Thus, it appears that the number of close friends and family in a neighborhood is unrelated to police officers’ use of excessive force. Instead, the sharing of common goals and a mutual trust amongst neighborhood residents that one’s neighbors will intervene on behalf of the neighborhood (i.e. having neighborhood collective efficacy) seems to be more important.

Another surprising result related to the effect of neighborhood social ties was the reversal of the direction of its effect when neighborhood collective efficacy was included in the model (Model 10 in Table 6.4). Even though the effect remained non-significant, and cannot therefore be confidently differentiated from having no effect at all (i.e. having zero effect), neighborhood social ties switched from having a negative relationship with police officers’ use of excessive force in all previous models to having a positive relationship. Essentially, what this means is that when accounting for the shared goals, mutual trust amongst neighborhood residents, and their willingness to intervene on behalf of the neighborhood, the number of close social ties that residents have may actually increase the likelihood that police use of excessive force will become more of a problem. While such a possibility may not seem very intuitive, one explanation for this finding might be that when residents have many close social ties in collectively efficacious neighborhoods, individual residents might each expect that the other members of their social networks will deal with whatever problems might arise. Consequently, if each of the residents of a neighborhood relies on his or her neighbors to be the first to step up and handle a problem, then, in the end, no one steps up to handle the problem. In this way, it
might be possible that neighborhoods with many social ties may actually increase problems of police officers’ use of excessive force because residents all expect that the other members of their strong social network will be the first step up and handle the situation. Again, however, because the positive relationship between neighborhood social ties and police use of excessive was non-significant in the final model, any real relationship between the two variables cannot be confidently discussed. Thus, the results presented in this dissertation not only challenge my neighborhood systemic model hypotheses, but they also challenge the viability of neighborhood social ties as an explanation of police officers’ use of excessive force.

In comparison to the viability of a neighborhood systemic model explanation of police use of excessive force, a neighborhood collective efficacy explanation appears to be much more feasible based on the results of my third and fourth sets of hypotheses. As discussed in Chapter 6, the results of my analyses suggest that neighborhood collective efficacy is a very strong and robust predictor of police officers’ use of excessive force behaviors. As expected, I found evidence not only of a strong negative bivariate relationship, but also of a very strong negative relationship when controlling for civilian reporting bias, as well as all the other concepts from the social disorganization tradition. In fact, across all the models analyzed in this dissertation, the magnitude of the effect of neighborhood collective efficacy was consistently one of the strongest predictors of police officers’ use of excessive force. Despite this strong evidence, however, I also found that neighborhood collective efficacy did not completely mediate the relationship between neighborhood structural disadvantage and police officers’ use of excessive force, challenging my mediation hypothesis. Moreover, the single measure of neighborhood concentrated disadvantage, as well as the three combined measures of neighborhood structural disadvantage as a whole, had larger impacts on police officers’ use of excessive force, which
challenges my fourth and final hypothesis, to a degree. Nonetheless, I would argue that these findings say more about the capacity of neighborhood structural disadvantage to predict police officers’ use of excessive force than they do to undermine the importance of neighborhood collective efficacy’s effect. In other words, because it has one of the strongest relationships with police officers’ use of excessive force, net of a host of control and other explanatory variables, this dissertation nonetheless provides very compelling evidence that neighborhood collective efficacy can and should be considered a very strong and robust predictor of police officers’ use of excessive force.

So, what do my findings regarding the neighborhood collective efficacy-police use of excessive force relationship mean substantively? I believe that the findings discussed above support the argument I presented earlier in Chapter 4. Briefly, I argued that if neighborhood residents shared common goals and values with their neighbors, and if they could count on their neighbors to intervene on behalf of the neighborhood’s greater good (i.e. their neighborhood is collectively efficacious), then they would have the power to reduce not only crime, they would also have the power to reduce problems with police officers’ use of excessive force as well. I further argued that the relationship between neighborhood collective efficacy and police use of excessive force likely operates in a fashion similar to the relationship between neighborhood collective efficacy and levels of crime. That is, I expect that just as neighborhoods with low collective efficacy signal to individuals that they will be able to get away with criminal and deviant behavior, those neighborhoods also signal to police officers that if they use excessive levels of force, there will be no single individual, and especially no larger group (i.e. the entire neighborhood), who will do anything to stop them or get them in trouble. I then concluded my argument by making the case that as officers realize that the residents of low collective efficacy
neighborhoods do not have the ability to stop them, their use of excessive force should become more of a problem. And, just as I had expected, the results of this dissertation confirmed that levels of neighborhood collective efficacy are indeed negatively related to neighborhood problems with police officers’ use of excessive force.48

Thus, based on the results of this dissertation, I conclude that the social disorganization tradition more generally, and neighborhood collective efficacy in particular, are viable theoretical explanations for the police use of excessive force. Furthermore, I can also answer my two larger research questions affirmatively and conclude that neighborhood social disorganization is indeed related to police officers’ use of excessive force, and that neighborhood collective efficacy is one of the strongest predictors among those I examined. And, if one recalls the well-established relationship between neighborhood social disorganization and rates of crime (see Chapter 3), it becomes quite evident that socially disorganized and low collective efficacy neighborhoods are without doubt doubly victimized – both by the criminals who operate within them and by the police officers who are supposed to protect them.

Theoretical and Policy Implications

There are a number of theoretical and policy implications that can be derived from the research in this dissertation. First, in regards to the theoretical implications of my dissertation, it should be evident now more than ever that the field of policing needs more theory, and more theory-driven research. I began this dissertation by arguing that one of the contributions I sought to make to the field of policing was the development of a new theoretical framework for

48 Although I cannot be certain that the argument I lay out captures the actual mechanisms through which low neighborhood collective efficacy leads to increased police use of excessive force, it is beyond the scope of this dissertation to uncover exactly how collective efficacy and police officers’ use of excessive force behaviors are related. Future research should attempt to explicate the relationship.
the explanation of police officers’ use of force. As I have described above, I believe that I have succeeded in this venture. This dissertation provides compelling evidence that the social disorganization tradition can be suitably applied to the explanation of police use of force behaviors. And even though the results of this dissertation focus specifically on the relationship between neighborhood social disorganization and police officers’ use of excessive force, the fact that I found a relationship between the two measures should indicate to other policing researchers that other concepts from the social disorganization tradition and neighborhoods and crime literatures might be able to explain other police behaviors as well. Furthermore, the fact that I was able to utilize a traditionally criminological theory to propose an entirely new explanation for police officers’ use of force behaviors should suggest to other researchers that more theory is not only needed, but that it can come from anywhere.

Thus, in addition to demonstrating that the neighborhood social disorganization tradition is a viable theoretical framework for explaining police use of force behavior, my research should also push other policing researchers to propose new theories for explaining police behavior and/or test other existing criminological theories that might be applied to the explanation of police behavior. As reviewed in Chapter 2, there are currently only two empirically supported theoretical frameworks for explaining police officers’ use of force—three categories if this dissertation is added to the mix. Taken altogether, however, it is still very unlikely that policing researchers will be able to explain 100% of the variation in police officers’ use of force. So, while this dissertation helps further the theoretical development of the field, more theory-driven research is still needed if we are to fully understand why police officers use all types of force—legitimate or otherwise—on civilians and criminals alike.
Next, in terms of the policy implications of my dissertation, several important findings should be considered. First, it may be necessary to more closely monitor the behaviors of police officers working in socially disorganized, and especially low collective efficacy, neighborhoods. As I mentioned in Chapter 4, although I do not expect that all police officers working in such neighborhoods will begin to abuse their authority and use excessive levels of force more often, it can take the actions of only one officer to create a severe problem for an entire neighborhood. Police administrators should therefore be careful to pay close attention to how their officers behave in disorganized neighborhoods. Through the use of widely available technologies (e.g. patrol car dash cams, in-car computer systems, and even radio communication systems), an increased focus on officers working in disorganized neighborhoods should allow administrators to monitor potentially dangerous situations.

Second, making it easier for neighborhood residents to report problems and ensuring them that the use of excessive force will not only be properly disciplined, but that residents do not need to fear retaliation could go a long way toward reducing improper behavior in socially disorganized neighborhoods. In other words, by making police officers more accountable for their use of force, they may think twice before using excessive levels of force. This can be done in a couple of ways. Police departments can increase punishments for the improper use of force and handle problems internally, or civilian review boards can be created or expanded to handle police officers’ abuses of authority. Either way, increasing the accountability of police officers for their behaviors may be an effective way to reduce use of force problems in all types of neighborhoods.

The introduction and development of community policing and other similar programs into socially disorganized neighborhoods may be a third way to help alleviate problems of police
use of excessive force. Such programs that bring police officers and neighborhood residents together to collectively define and address neighborhood problems may be even more important in those neighborhoods because residents might not even know their neighbors, let alone the officers who work in their neighborhoods. If officers get to know the people they see on a daily basis, they may be less inclined to use excessive force on those people. Moreover, if they can come to understand the people living in disorganized neighborhoods, they may get a better sense of when, and how much, force is necessary to handle a situation. As a side benefit, the increased use of such programs may actually increase neighborhoods’ levels of social organization and/or collective efficacy. Consequently, neighborhoods that were once doubly victimized may see doubly-beneficial results of having increased levels collective efficacy – decreases in both police use of excessive force and decreases in crime rates as well.

Finally, the results of this dissertation should make it even more evident that our society still needs to address the problem of structural disadvantage within inner-cities. Not only is neighborhood structural disadvantage related to crime problems, but as this dissertation shows, it is also directly related to police officers’ use of excessive force. Unfortunately, there are no easy solutions for reducing societal problems, such as poverty, racial/ethnic tensions, and residential instability. Many government programs already exist that help individuals improve their job skills, tolerance and acceptance of others, and rates of home ownership. While the results of this dissertation might not help shed any light on more or better methods for addressing these larger problems, it does provide yet another good reason to keep working toward reducing neighborhood structural disadvantage.
Limitations of this Dissertation and Directions for Future Research

While I argue that the results of this dissertation provide very compelling evidence that neighborhood social disorganization is strongly and robustly related to police officers’ use of excessive force, there are a number of limitations of my research that I should discuss. Unfortunately, the majority of these limitations are related to the data that I used to conduct my research. I should be clear, however, that the PHDCN is not a poor source of data for research on neighborhood context and police use of force behaviors. On the contrary, as I mentioned in Chapter 5, the PHDCN is an ideal source of data for not only this dissertation, but for many other studies of neighborhoods and policing. Thus, the major limitations of the data used in this dissertation are related more to the type of data that are used, rather than anything related specifically to the PHDCN. In other words, the major limitations of this dissertation are attributable to the cross-sectional civilian-survey design of the study and its focus solely on Chicago, IL, during the 1990s.

The first, and perhaps most important, limitation of my dissertation research is that I rely on civilian respondents’ reports of police officers’ use of excessive force behaviors. Despite my attempts to account for any potential civilian reporting bias in my dependent variable, inevitably there will be some who still consider my research to be pertinent only to civilian perceptions of police behavior, rather than their actual police behaviors. In an ideal world, I would have data that accurately measures every instance of police officers’ uses of excessive force. Unfortunately, no such data exist. As I reviewed in Chapter 5 of this dissertation, there is a small, but significant, body of research that suggests that both police officer self-report and systematic observational data have measurement error issues that can be just as problematic for determining whether or not some explanatory variable is truly influencing police officers’ actual
behaviors. Thus, because there is no existing data source that includes completely accurate measures of police use of force and measures of neighborhood context – such as the neighborhood collective efficacy variable that is of primary interest for this dissertation – the PHDCN data is currently the best source of data available. Of course, if data do come available that includes both some alternative measure of police use of force (e.g. police officer self-reports or systematic observations) and measures of neighborhood context, future research should attempt to replicate the findings in this dissertation.

A second limitation of this dissertation is my failure to test my new theoretical framework for explaining police officers’ use of excessive force against the two categories of theories that currently dominate the literature – the social threat and criminal threat theories. As with the previous limitation, this limitation is also related to the possibility of measurement error issues in my dependent variable. While I review a number of social threat and criminal threat measures that are available in the PHDCN data, in order to account for any potential civilian reporting bias, I eliminated all the variation in my dependent variable that may have been due to those measures. For example, the PHDCN data include measures of respondents’ race/ethnicity and social class, which could be utilized to test social threat theories. The data also include measures of neighborhood crime rates, which could be utilized to test criminal threat theories. Regrettably, however, based on the nature of my dependent variable, I utilized all of those measures to account for the possibility of civilian reporting bias, making it impossible for me to test my social disorganization tradition theoretical framework against the social threat and criminal threat theories.

If I was to interpret the effects of race/ethnicity, social class, and neighborhood crime rates on police use of excessive force behaviors, however, the results of my dissertation would be
very supportive of both the social threat and criminal threat theoretical frameworks.\footnote{While the social threat and criminal threat measures that I discuss here were used as controls for civilian reporting bias, for the sake of this hypothetical discussion, I interpret their effects as if they actually influenced police use of excessive force behaviors, rather than influencing respondents’ reports of police behavior.} First, as shown in Table 6.4 in the previous chapter, each of the racial/ethnic minority groups (blacks, Hispanics, Asians, and other race group members) had significantly greater odds than whites to experience problems with police use of excessive force, net of all the other covariates in the model. Table 6.4 also shows that social class was significantly and negatively related to police use of excessive force, net of all the other covariates in the model. Both of these findings support the social threat explanation of police use of force. Finally, in regards to support for the criminal threat theoretical framework, Table 6.4 reveals that neighborhood crime rates were significantly and positively related to police use of excessive force, net of all the other covariates in the model. Thus, if I was to alternatively interpret the effects of the variables discussed above as measures of social threat and criminal threat, it appears that there would be support for all three theoretical frameworks (i.e. social threat, criminal threat, and social disorganization). Unfortunately, however, until researchers can disentangle the effects of social threat and criminal threat measures on civilians reports of police behavior versus actual police behaviors, future researchers should attempt to utilize non-civilian-survey data (that does not require them to account for the potential of reporting bias) to test the social disorganization tradition theoretical framework against the two currently dominate theories of police use of force.

A third limitation of this dissertation of this dissertation is my focus only on police officers’ use of excessive force. While a better understanding of why police officers use excessive levels of force on individuals living in socially disorganized neighborhoods is very helpful, it would be even more helpful to understand whether or not neighborhood social disorganization might also influence other forms of police use of force. Earlier in this
dissertation, I argued that socially disorganized and low collective efficacy neighborhoods would experience more police use of excessive force because they lacked the collective capacity to put a stop to deviant behavior, whether it might be crime or the abuse of police authority. However, I would also argue that socially organized and high collective efficacy neighborhoods could likely reduce the amount of legitimately used force by the police as well. I expect that such neighborhoods would not only be strongly organized and have high levels of collective efficacy, but they would also be more likely to have the social, political, and/or financial capital to influence how police officers use even the most justified forms of forceful behaviors. Future research should test this informal hypothesis to help further our understanding of how and why neighborhood context influences police officers’ use of force.

A fourth limitation of this dissertation relates to the cross-sectional design of the PHDCN data. Because I test for a relationship between neighborhood social disorganization and police officers’ use of excessive force at the same time, I cannot actually assess the temporal sequencing of events. In other words, I cannot accurately determine causality. Is it really neighborhood social disorganization that influences police officers’ use of excessive force? Or, is it possible that police use of excessive force contributes to neighborhoods becoming disorganized? While the latter scenario would be unlikely, it is conceivable that neighborhood problems with the police use of excessive force might cause residents to stay in their homes to avoid the police, which, in turn, might lead to residents being unable to get to know each other and form common goals, ultimately leading to more neighborhood social disorganization and less neighborhood collective efficacy. Thus, in order for me to determine whether or not neighborhood social disorganization truly causes police officers to use excessive levels of force,
I would need longitudinal data with measures of disorganization at one time and police use of excessive force at a later time.

Despite the above limitation, the research I conducted in this dissertation is valuable in terms of establishing a relationship between the social disorganization tradition and police use of force behavior. Although I cannot demonstrate temporal sequencing and causality due to the cross-sectional nature of the PHDCN data, the findings of my dissertation demonstrate that a relationship does exist. If one accepts the theoretical arguments for why neighborhood social disorganization should influence police officers’ use of force that I laid out in Chapter 4, there is justification for my conclusion that neighborhood social disorganization does in fact influence police officers’ use of excessive force. Nonetheless, future research should utilize longitudinal data to verify the arguments and mechanisms I use in this dissertation.

One final limitation of this dissertation regards the generalizability of my findings to other times and locations. Because the PHDCN data is limited to neighborhoods within Chicago during the mid-1990s, it is not clear whether or not my findings can be generalized to other cities or other times. Although the city of Chicago served as the inspiration and data source for nearly every significant contribution to the social disorganization tradition, the unique history and nature of the city (i.e. its numerous riots that resulted from police officers’ use of force and the extensive socioeconomic and racial/ethnic diversity) makes it different from many other cities across the country. Thus, future research should examine relationships between neighborhood context and police use of force in a variety of locales and at a variety of times in order to determine whether the findings of this dissertation are specific to Chicago in the mid-1990s, or if socially disorganized neighborhoods across both space and time have been, are currently being, or will be, doubly victimized by high levels of crime and police use of excessive force.
Summary

The object of this dissertation was to establish a relationship between neighborhood context and police use of force. Specifically I wanted to determine whether or not concepts from the neighborhood social disorganization tradition could be used to explain police officers’ use of excessive force. I argued that just as socially disorganized neighborhoods signal to civilians that they can get away with criminal and/or deviant behavior, they also signal to police officers that no one will stop them, and they will not get in trouble if, they use excessive levels of force. Using the Community Survey portion of the Project on Human Development in Chicago Neighborhoods and multi-level modeling techniques to account for civilian reporting bias, I found that neighborhood structural disadvantage (concentrated disadvantage, concentrated immigration, and residential instability) and neighborhood collective efficacy (shared goals and values, mutual trust, and the willingness to intervene on behalf of one’s neighborhood) strongly and significantly predicted neighborhood problems with police use of excessive force. Based on these results, I concluded that a theoretical framework based on the social disorganization tradition is a both a strong and viable tool for explaining police officers’ use of excessive force. Taken as a whole, this dissertation supports the unfortunate reality that residents of socially disorganized are indeed doubly victimized - both by criminals who operate in their neighborhoods and by the police who are supposed to protect them.
REFERENCES


APPENDIX

PROJECT ON HUMAN DEVELOPMENT IN
CHICAGO NEIGHBORHOODS SURVEY INSTRUMENT ITEMS

Neighborhood Collective Efficacy Measures

Social Cohesion Measures

“For each of these statements, please tell me whether you strongly agree, agree, disagree, or strongly disagree.”

Q11A This is a close-knit neighborhood.
Q11E People around here are willing to help their neighbors.
Q11F People in this neighborhood don’t generally get along with each other (reverse-coded).
Q11J In this neighborhood people mostly go their own way (reverse-coded).
Q11K People in this neighborhood do not share the same values (reverse-coded).
Q11M People in this neighborhood can be trusted.

Informal Social Control Measures

“For each of the following, please tell me if it is very likely, likely, unlikely, or very unlikely that people in your neighborhood would act in the following manner.”

Q12A If a group of neighborhood children were skipping school and hanging out on a street corner, how likely is it that your neighbors would do something about it?
Q12B If some children were spray-painting graffiti on a local building, how likely is it that your neighbors would do something about it?
Q12C If a child was showing disrespect to an adult, how likely is it that people in your neighborhood would scold that child?
Q12E If there was a fight in front of your house and someone was being beaten or threatened, how likely is it that your neighbors would break it up?

Neighborhood Social Ties Measures (The Systemic Model)

“Not counting those who live with you . . . ”

Q17a How many of your relatives or in-laws live in your neighborhood? Would you say none, one or two, three to five, six to nine, or ten or more?
Q17b How many of your friends do you have in your neighborhood? Would you say none, one or two, three to five, six to nine, or ten or more?
Legal Cynicism Measures

“I am going to read you some statements people sometimes make. For each, please tell me whether you strongly agree, agree, disagree, or strongly disagree with each.”

Q41A Laws were made to be broken.
Q41B It’s okay to do anything you want as long as you don’t hurt anyone
Q41C To make money, there are no right and wrong ways anymore, only easy ways and hard ways
Q41D Fighting between friends or within families is nobody else’s business
Q41F Nowadays a person has to live pretty much for today and let tomorrow take care of itself.

Past Victimization Measures

Q31 While you have lived in this neighborhood, has anyone ever used violence, such as in a mugging, fight, or sexual assault against you or any member of your household anywhere in your neighborhood?
Q32 While you have lived in this neighborhood, has your home ever been broken into?
Q33 While you have lived in this neighborhood, have you or another member of your household had anything stolen from your yard, porch, garage, or elsewhere outside your home (but on your property)?
Q34 While you have lived in this neighborhood, have you or another member of your household had property damaged, including damage to vehicles parked in the street, to the outside of your home, or to other personal property?
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