IT'S ALL RELATIVE:

ASSESSING PROMINENT EXPLANATIONS FOR U.S. HOMICIDE TRENDS

by

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ABSTRACT

A substantial body of research has accumulated over the past quarter century to explain recent fluctuations in U.S. homicide rates, and a number of plausible explanations have been offered. This dissertation research sought to assess the relative strength of the most "common" of these explanations, including changes in the economy, family structure, age structure, immigration, policing, corrections, drug markets, and guns. In addition to assessing the relative strength of these explanations, special emphasis was placed on the impact of methodological variation, including variable operationalization, unit of analysis, and time period covered, on the results. Meta-analytic techniques were used to quantitatively synthesize 5,082 effect sizes from 145 different studies examining the relationship between these and other possible explanations and post-WWII U.S. homicide trends published between 1990 and 2016. Results revealed strong support for the role of changes in single parent households, inflation, consumer sentiment, military involvement, racial heterogeneity, disadvantage, incarceration, gun prevalence, racial and gender inequality, felony arrest, and divorce/family disorganization. These results indicate that many of the most "common" explanations are not receiving strong empirical support (e.g., police force size, unemployment, drug markets, gun laws) and suggest other possible explanations for future research to pursue that have not received as much attention in the scholarly literature (e.g., racial heterogeneity, military involvement, alcohol consumption). For many of the explanations examined, the amount of empirical support was conditioned on methodology. The implications of these findings for research, policy, and theory are discussed.

Chapter 1

INTRODUCTION AND STATEMENT OF PROBLEM: THE U.S. "CRIME TRENDS PUZZLE"

Concern about changes in crime rates is not new. Whether considering the trends themselves, causes or correlates of crime trends, or criminal justice response to crime, scholars, politicians, the news media, and the general public have been enthralled by the ups and downs in crime, and violence and homicide, in particular, for quite some time. However, after almost two decades of sustained crime decline in the United States, crime rates have begun to rise again in several large U.S. cities (Rosenfeld, 2016; Rosenfeld, Spivak, & Irazola, 2017), drawing increased scholarly and public attention to fluctuations in crime and violence.

Criminologists and anyone attempting to understand current crime patterns are well-aware that the data needed to understand current trends in crime are limited. Official crime statistics, which allow researchers to not only examine crime in a given locale, but compare changes in crime in one jurisdiction to crime changes in other jurisdictions across the nation, is delayed in its availability to researchers and the general public. As such, it is difficult to say, until several years after the fact, how crime rates are changing over time and whether those changes represent an anomaly for a single jurisdiction or a broader national trend. Even more elusive, are the *causes* for the observed trends. In recent years, this lack of data on current crime trends (i.e., rises in homicide in many large cities in 2015 and 2016) and the potential causes for such increases has received considerable attention in public discourse, news media, and by politicians, even serving as a focal point in the 2016 Presidential debates. Unfortunately, these conversations were largely driven by anecdotes and speculation as to what the possible drivers of recent crime trends could be, and in large part, without backing by empirical evidence (see Rosenfeld, 2016; Rosenfeld et al., 2017 for notable exceptions).

With recent rhetoric about an "impending crime wave" coupled with calls to return to earlier tough on crime policy initiatives, an assessment of what the empirical evidence suggests are the largest risk and largest protective factors is in order. While this dissertation does not presume to address the more proximate issue of recent homicide increases and the reasons for these changes, it does address a much broader, but related question – what are the most promising and empirically supported explanations for changes in homicide rates over time? There have already been several notable and commendable attempts in the literature to answer this question (see especially Baumer, Rosenfeld, & Wolff, 2012; Blumstein & Wallman, 2006; Conklin, 2003; LaFree, 1998, 1999; Goldberger & Rosenfeld, 2008; Parker, 2008; Roeder, Eisen, & Bowling, 2015; Sharkey, 2018; Zimring, 2007), but contradictory conclusions and inconsistent results remain. In general, we have developed a consensus as to the "who, what, when, and where" of recent crime trends, but are still at a loss as to the *why* (Rosenfeld, 2004). Answering this question not only brings clarity to the existing literature, but may also provide guidance during times where we lack the readily available data to address changes in crime rates in real time. Specifically, in this dissertation, my aim is to provide empirical evidence to answer recent and longstanding debates regarding which factors do, and which do not, impact changes in homicide rates over time. This identification of the relative importance of common explanations can help us understand homicide trends historically, as well as offer empirically-based guidance for understanding current and future crime trends, particularly in times when data are not yet available to test proposed causal factors.

Since the Second World War, the United States has experienced rapid fluctuations in homicide and crime trends, with several notable “booms” and “busts” (LaFree, 1999). During this same time frame, the social, economic, and political climate of the U.S. has shifted considerably as well, with massive increases in incarceration, deindustrialization and the resulting concentration of poverty and joblessness, changes in policing, including community policing and the War on Drugs, changes in family structure, and rises in immigration. These changes resulted in increased attention and scholarly debate about the magnitude and nature of homicide trends, as well as what contributes to the shifts in homicide over time. As a result, a number of plausible explanations have been offered, and major assessments tend to focus on a similar set of core “common explanations,” including changes in economic conditions, family structure, age structure, immigration, policing, corrections, drug markets, and firearm availability. Each of these and related changes have been advocated, in varying degrees, as contributing to post-WWII changes in U.S. homicide rates. However, there is little consensus as to which factors mattered, and especially, which factors mattered most.

While the roles of many of these factors have been well-established in the cross-sectional literature, research suggests that the factors that contribute to changes in homicide rates over time are not necessarily the same as the factors that contribute to homicide at a single point in time. That is, conclusions drawn from cross-sectional research often differ from the conclusions drawn from longitudinal research (see e.g., Chiricos, 1967; Marvell & Moody, 1991, 1996; Phillips, 2006b; Wadsworth, 2010). For example, when considering the immigration-homicide relationship, and especially Sampson's (2006) claim that increased immigration may have been a primary driver of the 1990s crime drop, Wadsworth (2010) found that immigration was *positively* and significantly associated with homicide in his cross-sectional analysis. However, he found immigration was *negatively* and significantly related to changes in homicide rates over time, arguing that the positive relationship observed in cross-sectional research is likely spurious, driven by unobservable, location-based factors correlated with both immigration and homicide. He concluded, "Together, the time-series analyses of changes in homicide and robbery rates between 1990 and 2000 paint an interesting picture – and one that casts doubt on the implications the cross-sectional models have for understanding changes over time" (p. 546). This example demonstrates one the primary advantages of longitudinal research over cross-sectional research, namely the superiority of longitudinal designs to provide greater clarity on potential causal relationships. Causality is crucial to the question at hand, making a focus on longitudinal work here imperative. In fact, in his *Annual Review of Sociology* article on post-World War II violent crime "booms" and "busts," Gary LaFree (1999) suggested that part of the reason the 1990s crime decline was so unanticipated was because we were so overly dependent on cross-sectional research. Phillips (2006b) moves an additional step forward by not only acknowledging these inconsistent (and often-times divergent) findings between cross-sectional and longitudinal research, but actually offering a conceptual reason for these discrepancies, arguing that variables exert different temporary versus permanent influences on the outcome of interest. Cross-sectional research is better apt to capture these permanent (i.e., stock) influences and longitudinal research is better suited for explaining temporary (i.e., flow) effects, and the distinctions between them are critical. We live in an ever-changing society, and crime, like everything else, is shaped by broader social forces, including shifts in the economy, demographics, and the criminal justice system. As such, to truly understand the causes for homicide rate fluctuations, an examination of longitudinal research is needed.

Given substantial debate as to the primary causes, as well as inconsistencies both in terms of empirical findings in individual studies and broader overall conclusions, this dissertation aims to bring clarity to the crime trends literature by assessing the empirical support (or lack of support) for the most common explanations that have prevailed in the scholarly literature over the past 40 years. In doing so, I address two key objectives. The first objective is to establish the relative importance of prominent explanations of homicide trends. The second objective is to examine the impact of methodological variation on the relative importance of the explanations. Both of these underlying objectives are briefly outlined next.

Objective 1: Establish the Relative Importance of Explanations of Homicide Trends

In 1998, the *Journal of Criminal Law and Criminology*, published a special issue devoted to uncovering how and why violent crime had declined so precipitously in the 1990s. This special issue included 11 articles from top scholars in various fields and stemmed out of a National Institute of Justice (NIJ) and Northwestern Law School symposium allocated to addressing the question, “Why is crime decreasing?” In the forward to the special issue, Jeremy Travis (1998) notes that the main takeaway from the symposium from which the articles were based was that there was “no single cause or explanation for the recent decline" and that multiple factors likely came together in an unprecedented way to shape recent crime declines (see also Blumstein & Wallman, 2006; Rosenfeld, 2004 for similar conclusions).

Additionally, in narrative reviews of the literature, scholars come to polar conclusions regarding the importance of similar factors. As an example, in their contributed chapter to *The Crime Drop in America*, policing experts, John Eck and Edward Maguire, note very strongly a lack of evidence for the role of increased police strength on temporal changes in violent crime. After an in-depth examination of 27 empirical studies on the role of police strength, Eck and Maguire conclude, "We are not aware of *a single empirical study* that supports the claim that increases in the number of police officers are responsible for recent decreases in violent crime" (2006, p. 209, emphasis added). Even after limiting their assessment to only the most methodologically sound studies, their conclusion remained the same. Conversely, in a 2013 *Annual Review of Economics* article, deterrence expert, Daniel Nagin concludes, "Studies of changes in police presence, whether achieved by changes in police numbers or in their strategic deployment, *consistently* find evidence of a deterrent effect" (p. 83, emphasis added). How can scholars come to such polar conclusions regarding the impact of police force size on crime trends? These contradictory stances, though based in the extant empirical literature, can have important implications for policy in terms of crime prevention and reduction strategies, along with implications for future research on crime trends and theoretical development.

Twenty years have passed since the 1998 NIJ and Northwestern symposium, and hundreds of journal articles and books addressing this issue have accumulated, including several efforts to synthesize the crime trends and crime drop literature.

However, we are still not able to say with any certainty what contributed to the past quarter century's crime trends, much less which factors mattered *the most* and how much they mattered *relative to other plausible explanations*.

These observations inform the first objective of this dissertation – to establish the relative importance of tested explanations of contemporary U.S. homicide trends. To address this objective, I use meta-analytic techniques and systematically review the homicide trends literature that was published between 1990 and 2016 and that was conducted at the city-level or higher (e.g., cities, counties, MSAs, states, regions, and nation). The question of what factors contribute to crime rate changes over time has been asked and answered extensively in the literature. And while previous assessments and narrative syntheses have been desperately needed and have added to our understanding, scholars oftentimes have come to very different conclusions as evidenced in the example provided above. This dissertation is the first meta-analysis of this body of literature, and uses a new statistical tool to answer this age-old question in a different way (see Baumer et al., 2018 for an argument about the need to use different methodological approaches to move the crime trends literature forward). In doing so, I leverage the benefits of meta-analysis to move beyond previous efforts, in ways that would not have been possible with traditional narrative reviews or regression-based assessments.

Specifically, by answering this question with meta-analysis, three related findings and contributions can emerge. First, results can point to the most important factors that empirical research shows impact homicide trends. Second, I am able to statistically examine how commonly debated explanations hold up against empirical scrutiny, including noting which explanations matter and are receiving strong empirical support and which ones are not supported in the literature. Third, I can uncover promising directions for future research by identifying additional factors that results suggest are important contributors to homicide trends, but that have been largely missing from recent crime trends debates.

These meta-analysis results will largely advance the substantive discussion in the homicide trends literature by providing a comprehensive overview and assessment of the strength of empirical support for the most critical factors that impact contemporary homicide trends. Moving beyond the initial assessment, the second objective of the dissertation is to examine how methodological variation impacts these results. Specifically, the second objective is concerned not only with *which* factors matter, but also *when* (i.e., are some explanations important under certain methodological specifications, but not others, or does the strength of the association differ depending on methodology).

Objective 2: Determine Which Explanations Matter, and When

Impeding our ability to make sense of this "crime trends puzzle" (Baumer 2008) is the fact that there has been substantial variability in how these explanations have been tested, particularly with regard to research design and model specification, making it difficult to draw any firm conclusions (see also Baumer, 2008 and Spelman, 2008 for a similar argument). Empirical tests of even the most common explanations exhibit substantial variability in the operationalization of key constructs, competing explanations controlled for, unit of analysis, time period covered, and longitudinal research design, among other factors. Therefore, inconsistent results and contradictory conclusions may be partially attributable to methodological variation across studies. Some of these methodological factors have been debated in the literature, including measurement and unit of analysis. Others, such as longitudinal research design, have not received as much attention, but are still critically important, and I illustrate why in future chapters.

Therefore, this dissertation also aims to examine the impact of methodological variation on our findings and conclusions. To address this objective, I examine the impact of methodological variation using four sources of variation that have surfaced in the literature, both conceptually and empirically. Specifically, this portion of the dissertation aims to address two related considerations. First, I examine how our understanding of the most important factors changes under various methodological specifications. Are there any explanations that surface as strong under some specifications but weak under others? Second, I assess the stability of the most important predictors across different specifications to see if they are robust predictors of homicide trends or if they are sufficiently conditioned by methodology. Addressing this second objective has the potential to make several contributions to the literature, including identifying the factors that are consistently important, the factors that are consistently unimportant, and the factors that are sometimes important and sometimes not. This third set of factors – those that are important in some contexts but not others – may be especially informative to ongoing debates by shedding light on the conditions under which certain explanations are most likely to have an effect.

In sum, the primary objectives of this dissertation are two-fold. The first objective is to bring clarity to the crime trends literature by establishing the relative importance of each of the main explanations that have been put forth to explain contemporary homicide trends. The second objective is to uncover the role of methodological variation on these results. In essence, this dissertation is an examination of which factors matter, and when, to homicide trends.

Taken together, findings from this dissertation can make important contributions to both the homicide and crime trends literatures, as well as broader criminological literature on the causes and prevention of crime and violence. Empirically, the results will illuminate the most important explanations that future research on homicide trends needs to include. Failing to include these predictors can risk a seriously misspecified model due to omitted variable bias. Relatedly, results will show the predictors that are relatively weak, and that future researchers may choose to omit without fear of it biasing their results. In addition to the empirical implications, the results can have important implications for policy by illuminating the factors most strongly related to temporal trends in homicide, suggesting the most promising avenues for crime prevention and reduction. In terms of theory, I attempt to move beyond the focus on specific predictors and how they fare empirically, to identify broader themes and patterns that emerge in the results (see e.g., Baumer et al., 2018; Roth, 2009). Do the most important predictors group together in ways that may suggest certain theoretical traditions or theoretical avenues for future researchers to consider? In these ways, the findings in this dissertation can have important implications for research, policy, and theory, and the full implications of the main findings are expanded in the final chapter.

The dissertation proceeds as follows. Chapter 2 provides an overview of U.S. homicide trends and the most prominent explanations that have been offered for temporal changes in homicide. This chapter begins with an overview of post-WWII U.S. homicide trends. After presenting the overall trends guiding this body of research, I outline eight key explanations that have figured most prominently into debates on contemporary crime trends. These explanations are broadly grouped into the following categories: changes in economic conditions, family structure, age structure, immigration, policing, corrections, drug markets, and guns. Within each broad explanation, I primarily focus on the trends we've observed and the empirical research connecting these trends to temporal trends in homicide (or crime more broadly, where applicable). Given that researchers have tested these explanations using a variety of methodological techniques and because one of the main objectives of this dissertation concerns the role of methodological variation, increased attention is given to the variation in how these studies were tested and how this may impact the results and conclusions. This chapter also contains a more limited discussion of the theoretical or conceptual linkages.

Chapter 3 outlines the methods used in this dissertation research. This chapter begins with a discussion of the steps taken to identify and select articles for inclusion in the meta-analysis. Information on how data from selected studies were coded is provided, including the effect size for the magnitude of the relationships (the dependent variable) and the different sources of methodological variation (the independent and control variables). Methodological considerations and the analytic strategy are also presented. "Predictor domains" – or sets of both conceptually and statistically similar constructs – form the foundation for addressing the two key objectives and results presented in Chapter 4. Therefore, the predictor domains that emerged and will be used in the future analyses are first introduced in this chapter, as well.

Chapter 4 contains the results addressing the two main objectives, and the presentation of the findings proceeds in stages. This chapter begins with an overall rank-ordering of the relative importance of all of the predictor domains established in Chapter 3, and addresses objective one. After presenting the overall rank-ordering, I turn to the second objective, moving through results from a series of bivariate analyses. In these bivariate analyses, I examine the relative importance (and updated rank orderings) of the predictor domains for different subsample analyses based on different methodological specifications (including level of aggregation, time period covered, type of dependent variable, and type of longitudinal analysis). I continue this examination by presenting results from a series of multivariate models that assess the impact of several sources of methodological variation simultaneously, as another and more rigorous way to address the second objective. This chapter concludes with a final overall assessment of the strength and stability of the predictor domains based on all of the analyses in the dissertation.

Chapter 5 concludes with a summary of the key findings and contributions of this dissertation research. This chapter includes an assessment of the empirical support for the established *predictor domains* tested in this dissertation, with a focus on which predictors are most important, how the most common predictors fare, and what predictors we may be missing (objective 1). The assessment of the predictor domains also includes an assessment of how methodological variation impacts the results, by noting the predictor domains that received consistently strong, consistently moderate, consistently weak, and inconsistent empirical support across different methodological specifications (objective 2). Moving beyond the focus on individual predictor domains, this chapter also assesses how the eight *explanations* introduced in earlier chapters fare, more broadly. This chapter concludes with some of the main implications of the findings, in terms of implications for research, policy, and theory, as well as a discussion of the limitations of this work. I turn now to the discussion of U.S. homicide trends and the eight main explanations that have surfaced most commonly in the literature.

Chapter 2

U.S. HOMICIDE TRENDS & THEIR EXPLANATIONS: TRENDS, THEORY, & REEARCH

U.S. Homicide Trends, Post WWII to Present

To set the stage for the explanations presented in this chapter, I begin with a brief overview of national U.S. homicide trends from 1960 to 2016, before moving on to eight broad categories of explanations for these observed trends that dominate in the literature. As illustrated in Figure 1 and documented in previous scholarship, over the past five to six decades, the homicide rates in America have fluctuated widely, with several notable "booms" and "busts" (LaFree, 1999). Homicide rates were fairly stable from the end of the Second World War to the mid-1960s, at a rate of approximately 4 to 5 per 100,000 (Levitt, 2004). However, we began to see major fluctuations in homicide rates beginning in the 1960s. Homicide rates began to increase substantially in the 1960s and early 1970s, with the steepest increase occurring from 1963 to 1974, when homicide rates approximately doubled (LaFree, 1999; LaFree & Drass, 1996; Zimring, 2007). Overall, homicide rates peaked in 1980 at a rate of 10.2 per 100,000 (Blumstein & Rosenfeld, 1998; Levitt, 2004), before falling between 1980 and 1985. However, we observed another sharp increase in the national homicide rate from 1985 through the early 1990s before the largest and longest sustained decline in homicide rates in post-WWII history (LaFree, 1999), dubbed the "Great American Crime Decline" (Zimring, 2007). Scholars document around a 40 to 45 percent decline in homicide rates from 1991 to the turn of the century, a decline to levels not seen since the 1960s (e.g., Barker, 2010; Fox & Zawitz, 2000; Levitt, 2004; Parker, 2008; Rosenfeld, 2002, 2004; Zimring, 2007). Despite the massive decline in homicide rates in the 1990s, the magnitude of the decline was still not as steep as the increase in homicide rates in the 1960s and 1970s (LaFree, 1999). The decline in homicide rates observed in the 1990s lasted until the early 2000s, when homicide rates began to level off, remaining flat for the first few years of the twenty-first century (Wallman & Blumstein, 2006), and then increasing again slightly in 2005 and 2006 (Baumer & Wolff, 2014; Blumstein & Rosenfeld, 2008; Police Executive Research Forum [PERF], 2006; Rosenfeld & Goldberger, 2008; Rosenfeld & Oliver, 2008). This was followed by another decline in homicide rates beginning in 2007 (Rosenfeld & Oliver, 2008; Uggen, 2012) that lasted until 2011 (Butts & Evans, 2014; Parker et al., 2017). Recently, evidence from local news accounts and preliminary official statistics indicate homicides were on the rise again in 2015 and 2016, particularly in large U.S. cities (Rosenfeld, 2016; Rosenfeld et al., 2017; Wheeler & Kovandzic, 2017).

[Figure 1 here]

The Explanations

The remainder of this chapter focuses on the eight broad explanations introduced in Chapter 1. These eight groups have been selected for two main reasons. First, they have received the most attention in contemporary crime trends debates, and second, they have been empirically examined the most consistently over time. That is, they are "common" explanations both conceptually and empirically[1]. Table 1 shows

1Broader theoretical arguments, such as those related to changes in the legitimacy of social institutions (e.g., LaFree 1998) or long-term cultural shifts (e.g., Roth 2009), that are difficult to capture empirically are not included. Additional arguments that have not received as much scholarly attention as contributing to recent homicide trends (e.g., changes in alcohol consumption) are also not included here. However, explanations not focused on here will still be coded and analyzed, and results will be presented in subsequent chapters.

seven of the most comprehensive assessments of the recent crime trends literature. The primary purpose of this table is not to focus on any one scholar's conclusion regarding any of these explanations. Instead, it is provided to show that each of these eight broad explanations considered in this chapter (with the exception of immigration[2]) has been considered as a plausible and potentially important explanation in previous narrative syntheses of the crime trends literature, justifying its inclusion here and classification as a "common" explanation. This table also serves the purpose of illustrating the vast inconsistencies in conclusions from previous syntheses. For example, results reveal that the most debate centers around changes in age structure, policing, and drug markets, with about half finding support (indicated with a check mark) and about half concluding that particular factor played little to no role (indicated with an x). Other explanations, including incarceration, have much more consistent support, with every author noting increased incarceration played some role in recent crime trends. The weight of the evidence also suggests that changes in economic conditions are an important consideration, but as will be expanded in the next section, this varies based on which aspect of the economy researchers are considering. Family structure has not been considered as frequently as some of the other explanations, but some scholars do point to its role in contributing to contemporary crime trends. These inconsistencies and the overall empirical support are expanded in the following sections. Within each explanation below, I begin by providing the trends and conceptual connections before moving on to the empirical support and methodological inconsistencies.

[Table 1 here]

[2]The rationale for including immigration, despite more limited scholarly attention, is provided in the immigration section below.

Economic Conditions

The U.S. has experienced major changes in the economy since World War II, including economic growth coming out of the war and a strong economy in the 1960s. Deindustrialization, or the move from a manufacturing to a service-based economy, beginning in the 1970s brought with it a displacement of low-skilled workers from labor markets (particularly teenagers, young adults, and minorities), increased unemployment, and declines in wages for those that were still employed (Barker, 2010; Chiricos, 1987). The emergence of the "new economy," characterized by growth in the service sector and expansion of technology in the 1990s, brought about a period of economic revitalization, with declines in unemployment and poverty, and increases in wages and GDP (Barker, 2010; LaFree, 1999; Levitt, 2004; Parker, 2008; Rosenfeld, 2004; Zimring, 2007). Following a period of economic growth and prosperity, the U.S. experienced the longest and steepest recession since the 1930s Great Depression (Rosenfeld, 2014; Uggen, 2012). This "Great Recession" lasted from approximately 2007 to 2009 and primarily impacted middle-aged and middle-income Americans.

These changes in economic conditions have been linked to changes in crime rates, including both increases in homicide in the 1970s and 1980s, as well as the decline in homicide rates in the 1990s (Blumstein & Wallman, 2006; LaFree, 1999; Levitt, 2004; Parker, 2008). More recently, scholars have begun to discuss potential delayed effects from the 2007 economic recession on more recent crime increases (see 2013 special issue in the *Journal of Contemporary Criminal Justice* on "Crime and the Great Recession"). However, given that crime did not trend in the expected direction based on current economic conditions during the strong economy of the 1960s, when crime rates increased, or during the Great Recession in the 2000s, when crime rates continued to decline, some have question the presumed economy-homicide trends link, and have offered various explanations for the discrepancies. For example, researchers have proposed that the link between the state of the economy and homicide trends may operate indirectly through its impact on acquisitive crimes (Rosenfeld, 2009; Rosenfeld & Levin, 2016). Others have similarly suggested alternative plausible factors which may mediate the economy-homicide trends relationship, including home foreclosures (Rosenfeld, 2014), and have examined how criminal justice factors, including police size and incarceration, may moderate the relationship between economic conditions and homicide trends (Baumer et al., 2012).

Other scholars have focused more on the indicators used to capture current economic conditions, arguing they are ill-suited to capture the most important aspects of labor market shifts which may be impacting crime trends. These scholars critique traditional economic measures, and instead advocate for alternative indicators that may better capture changing labor market dynamics. For example, economic opportunities associated with changing labor market conditions are often assessed via measures of (un)employment. However, while some have argued that changes in unemployment are crucial for explanations of temporal trends in crime, others have argued that short-run changes in unemployment make it unlikely to account for more long-run changes in crime trends. Instead, they recommend increased attention to indicators that may better capture more long-run changes in labor market conditions, such as changes to wages (Gould et al., 2002) and labor market opportunity structures (Parker, 2008). For example, Gould and his colleagues (2002) note that “a secular decline in unskilled wages, as seen during the 1970s and 1980s represents a decline in the ‘permanent’ wages of uneducated workers, whereas cyclical unemployment fluctuations have more temporary implications" (p. 45). As a result, the U.S. has also observed widening wage inequality since the 1980s (Atkinson, Piketty, & Saez, 2011; Fowles & Merva, 1996; Light & Ulmer, 2016).

Furthermore, while the vast majority of the scholarly literature linking economic conditions to crime focuses on unemployment, poverty, and income inequality, Parker (2008) discusses this "construct dominance," or increased attention to these three factors, to the exclusion of other economic indicators (see also Sampson, 2002). She argues that typical economic measures are ill-suited to capture the changing nature of the economy that may have influenced the 1990s crime drop, in particular, and that other indicators, such as concentrated disadvantage and the removal of manufacturing jobs, may better inform our understanding of the role of economic opportunity structure on homicide trends.

Finally, some scholars have begun to focus on whether *perceptions* of economic well-being via more subjective economic indicators (e.g., the Consumer Sentiment Index) can better explain the connection between economic conditions and homicide trends, moreso than objective measures (e.g., Rosenfeld, 2009; Rosenfeld & Fornago, 2007; Rosenfeld & Oliver, 2008). Although homicide trends do not appear to coincide with trends in the economy in the early 2000s, they do parallel trends in consumer sentiment. From 2000 to 2006, homicides were increasing as consumer pessimism was rising (Rosenfeld & Oliver, 2008).

Overall, empirical support in multivariate research for the link between economic conditions and homicide trends is mixed. These conclusions, however, appear to be at least partially contingent on the actual aspect of the economy the researcher is tapping. For example, though there are still inconsistencies, the empirical research overall finds a stronger link between measures of economic deprivation (both absolute and relative deprivation) and homicide trends than when examining the role of unemployment. These discrepancies are not surprising given the complexity of the economy-homicide trends relationship and myriad of theoretical and conceptual connections, both direct and indirect, between the economy and crime.

While unemployment as a measure has long been criticized (see Sorrentino, 1979), it has a long history in the criminological literature, and is one of the most commonly included economic indicators when studying crime trends. As such, the discussion in this section focuses more on unemployment than the other economic indicators. I do address other economic indicators here too, but to a lesser extent. Although unemployment sometimes loads with other indicators of economic and social disadvantage (see e.g., MacDonald & Gover, 2005; Ousey & Kubrin, 2014; Ousey & Lee, 2002, 2004, 2007; Phillips, 2006a; Strom & MacDonald, 2007; Wadsworth, 2010), a number of studies have analyzed its independent effects on homicide trends across different methodological specifications, with contradictory results.

These inconsistent empirical results are what prompted Cantor and Land (1985) to specify and test their conceptual model of the unemployment-crime (U-C) relationship, which hypothesized simultaneous motivation (+) and opportunity (-) effects of unemployment on crime trends. In their national-level analysis from 1946 to 1982, they estimated both the contemporaneous (opportunity) and lagged (motivation) effects of unemployment on homicide trends. They found that the effects of unemployment and lagged unemployment were both insignificant when included in the same model. When only contemporaneous unemployment was included, they found a negative effect, as hypothesized. However, when lagged unemployment was included, they also found a negative effect, opposite of their hypothesis. In a *Journal of Quantitative Criminology* special edition on the U-C relationship, Greenberg (2001) called attention to the importance of considering the *duration* of unemployment when estimating the U-C relationship. In his national-level analysis of homicide trends from 1946 to 1996, he finds some evidence for negative effects for unemployment, lagged unemployment, and duration of unemployment when they are considered independently. However, he does not find an effect for any of these measures when he includes all three in the same model. Other researchers have considered the impact of unemployment on homicide trends at various levels of aggregation, and have found similar null effects, including tests at the city (McCall et al., 2008), state (Vieraitis et al., 2007), and national (Britt, 1994; Peterson & Bailey, 1991) levels. Conversely, researchers have also found positive effects for unemployment on homicide trends across various levels of aggregation and time periods (e.g., Cohen & Land, 1987; Marvell & Moody, 1997; Matthews et al., 2001), as well as negative effects (e.g., Phillips, 2006a; Rosenfeld & Oliver, 2008).

Table 2 illustrates these inconsistencies for unemployment in more detail, both in terms of findings and measurement and study design features. This table includes longitudinal studies of the unemployment-homicide trends relationship published in the past ten years (i.e., between 2007 and 2016). While I use this table to illustrate the inconsistencies, similar variation exists, and comparable tables could be made, for each of the main explanations presented in this chapter. As shown in the table, studies find positive, negative, and null effects, and no clear pattern emerges based on several different sources of variation, including operationalization of unemployment, unit of analysis, time period covered, dependent variable, or longitudinal research design[3]

[Table 2 here]

Despite the fact that scholars have argued that wages are an important indicator of economic conditions, and may actually be more important for advancing our understanding of homicide trends than the more commonly used unemployment, very few empirical studies actually assess this relationship. In one of the few empirical tests, Gould and his colleagues (2002) examine the impact of changes in wages on violent crime trends in U.S. counties from 1979 to 1997. Although they do not limit their analysis to homicide specifically, given the lack of studies that actually test this relationship, it is informative for the discussion here. They found that increases in wages decreased the violent crime rate from 1979 to 1992, and this effect was stronger than unemployment on violent crime trends during this period. However, during the period of crime decline (1993 to 1997 in their analysis), unemployment was a more important causal factor than wages.

There is some support in the literature for the role of changing wages and unemployment rates on changing homicide rates, but this effect may be dependent on time period under consideration. In one of the few studies to estimate the effects of wages on homicide trends, Baumer (2008) found that wage increases were inversely related to city-level homicide trends between 1980 and 2004, and this finding persisted across multiple models. In a later analysis, Baumer and his colleagues (2012) examined the possible invariancy of common explanations of crime trends at three

[3]These four types of methodological variation are highlighted here because they are the four sources of methodological variation used in subsequent analyses.

units of analysis often used in the study of crime trends (i.e., city, county, and state) (see also Land et al., 1990 for a similar approach in the cross-sectional literature). In contrast to earlier analysis, they found that wages did not exert an impact on homicide trends from 1980 to 2010 at any of the three levels of aggregation. Thus, while empirical tests have been rare, preliminary evidence suggests that the relationship between wages and homicide trends may have been stronger in the pre-crime drop era, whereas unemployment may play a larger role during times of crime decline.

As illustrated, empirical support for both unemployment and wages on homicide trends is inconsistent, but may point to important differences by historical period. In contrast, measures of economic deprivation exhibit much more stability in the literature. Specifically, whether including measures of absolute deprivation (e.g., poverty), relative deprivation (e.g., income inequality), or a composite index, researchers tend to find a positive association between economic deprivation and overall homicide trends, at least at the city-level (Beaulieu & Messner, 2010; LaValle, 2010; Matthews et al., 2001; McCall et al., 2008; Ousey & Kubrin, 2014; Wadsworth, 2010). Its impact at larger levels of aggregation, including counties (Phillips, 2006a), SMSAs (Fowles & Merva, 1996), states (Vieraitis et al., 2007) and the nation (Marvell & Moody, 1997; Saridakis, 2004), is less consistent. This is in line with Baumer et al.'s (2012) finding that their resource deprivation index was positively related to city-level homicide trends only, but was unrelated to county and state-level trends.

Given their separate conceptual linkages with homicide trends, scholars have made efforts to examine the distinct effects of both absolute and relative deprivation, as well as unemployment, on homicide trends in the same model. For example, Saridakis (2004) included both income inequality and unemployment measures (including duration of unemployment) in his national-level study of U.S. homicide trends from 1960 to 2000. He found that income inequality was positively related to homicide trends, while employment measures were unrelated. Additionally, Fowles & Merva (1996) separately estimated the effects of poverty, "wage inequality" (which they argue could be considered a proxy for income inequality), and unemployment on MSA homicide trends from 1975 to 1990 and found all three had a positive effect.

As Parker (2008) notes, empirical tests of the economy-crime trends relationship are dominated by these same routine indicators of unemployment, poverty, and income inequality. Following substantial industrial restructuring observed in recent decades, scholars have turned attention to the role of specific labor markets shifts, including deindustrialization, on temporal trends in homicide (e.g., Matthews et al., 2001; McCall et al., 2008; Ousey & Kubrin, 2014; Parker, 2004, 2008; Shihadeh & Ousey, 1998). When considering the impact on overall homicide trends, results are mixed. McCall and her colleagues (2008) found no effect of the percent employed in manufacturing on city-level homicide trends from 1970 to 2000, while Ousey and Kubrin (2014) found that the percent employed in professional or managerial occupations inversely impacted city-level homicide trends from 1980 to 2010. Given the uneven nature with which deindustrialization impacted minorities (Wilson, 1987), the majority of this work examines race-specific homicide trends. For example, Parker (2004) found that the change in manufacturing employment was associated with a decline in the black male homicide trend, but found no effect for the black female, white male, or white female homicide trends. She also finds that the change in the service industry was associated with an increase in the black female homicide trend, but none of the other race and gender-specific trends.

In sum, economic conditions have received a great deal of attention in the crime trends literature, both conceptually and empirically. However, the trends between economic conditions using typical indicators of economic strength (e.g., unemployment) and homicide trends do not always coincide, leading scholars to suggest and examine possible reasons why. Additionally, there has been a great deal of variation in how these relationships have been tested, and empirical support is mixed. This narrative review, however, suggests that the relationship may vary based on important methodological considerations, including those explored in more detail in subsequent chapters.

Family Structure

Beginning in the 1960s, the structure of the modern American family shifted considerably, with sharp increases in divorce rates, increases in the percent of children being born to unmarried parents, and increases in the percent of children being raised by single parents (Beaulieu & Messner, 2010; LaFree, 1999). We also began to see decreases in marriage rates, with the proportion of young people who had never been married rising (Puzone et al., 2000; Rosenfeld, 1997), and increases in non-marital cohabitation (Amato et al., 2007). Divorce rates peaked in 1979 before declining slightly in the 1980s and into the 1990s (LaFree, 1999), although they still remain high by historical standards.

These changes in family structure have had a profound impact not only at the individual and familial level, but also at the macro level, and these shifts, most notably the decline in family legitimacy associated with increasing divorce and single-parent households, were a common explanation for the 1960s crime wave, but not as frequently implicated in the 1990s decline (LaFree, 1999). Instead, discussions of the role of changes in family structure on more recent crime trends focus more on declines in domesticity (via declining marriage rates and increasing divorce rates) as a top contributor to the decline in adult and intimate partner homicides that began in the 1980s (Puzone et al., 2000; Rosenfeld, 2002, 2006). That is, rising divorce rates in recent decades have been associated with both the *increase* in overall homicides in the 1960s, as well as the *decline* in adult and intimate partner homicide trends in the 1980s.

This is likely due to the different theoretical mechanisms linking changes in divorce to changes in homicide over time. First, drawing from a social disorganization perspective (Shaw & McKay, 1942), divorce contributes to community disorganization and a breakdown in informal social control in the community, increasing crime and homicide as a result. Divorce is also theorized to increase homicide by increasing the criminal opportunity to commit crime (Cohen & Felon, 1979), increasing economic strain stemming from a single-headed household (Amato, 1995; Wilson, 1987), and removing male role models important for youth socialization (Anderson, 1990). Conversely, when it comes to familial homicides, some scholars argue that divorce may provide a safety net for spousal homicide because it offers a non-violent means of conflict resolution and means of escape before abuse turns lethal, termed the "exposure reduction hypothesis" in the literature (Dugan et al., 1999; Gillis, 1996)[4,5].

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[4]Alternatively, divorce may act as a stimulus for lethal violence (Gillis, 1986).

[5]A substantial body of research has also documented the role of changing legislation (e.g., VAWA) and associated increases in the availability of domestic violence services resources on declines in intimate partner homicide since the 1980s, particularly for male victims, for similar theoretical reasons (i.e., providing women a non-violent means of

The role of divorce and family disruption on temporal trends in *overall* homicide receives strong empirical support, with researchers often finding a positive association between changes in divorce rates and changes in homicide rates over time (Baumer et al., 2012; Beaulieu & Messner, 2010; Greenberg, 2001; Matthews et al., 2001; Ousey & Kubrin, 2014; Phillips, 2006a; Wadsworth, 2010). Observing that trends in divorce and homicide coincided, Greenberg (2001) used national time-series data from 1946 to 1997 and found that these two trends were co-integrated, meaning that they both tend to move in the same general direction. Multivariate studies confirm this relationship. Part of the reason for such consistency may be similarities in variable operationalization across studies, with researchers often measuring family disruption via the percent divorced or percent divorced males. Another common measure is percent single- (or female-) headed households (although this often loads with other indicators of social and economic disadvantage into a broader disadvantage index; see above). It is also found to be positively associated with overall homicide trends (Kovandzic et al., 2004, 2005; Rotolo & Tittle, 2006; Savolainen, 2000). In further examining the role of changes in family disruption on changes in homicide rates over time, Beaulieu and Messner (2010) combined measures of percent divorced *and* percent of children not living with both parents into a composite "family disorganization" index and found that the effect of this family disorganization index

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escape) (see e.g., Browne & Williams, 1989; Dugan et al., 1999; Greenfeld et al., 1998; Reckdenwald & Parker, 2011; Rosenfeld, 2002). While I assess the impact of this increased availability of domestic violence resources in the empirical analysis in Chapter 4, I do not consider it one of the main explanations here as the impact is limited to intimate partner homicides, specifically.

on decade-by-decade changes in homicide rates was much stronger than when they used just percent divorced alone.

This consistent support for family disruption (via divorce and single-headed households) on overall homicide trends holds up across different levels of aggregation. Studies conducted at various units of analysis regularly find a positive effect, including studies conducted at the city (Matthews et al., 2001; Ousey & Kubrin, 2014; Wadsworth, 2010), county (Phillips, 2006a), and national (Greenberg, 2001) levels. Additionally, in their city, county, and state-level analysis of homicide trends from 1980 to 2010, Baumer et al. (2012) found divorce to be a robust predictor, finding significant positive effects at all three levels.

In addition to being a consistent predictor across levels of aggregation, divorce remains a robust predictor across time periods, as well. As divorce has become more commonplace since the 1960s, and we've seen other shifts away from the traditional two-parent family, American's attitudes have also changed (Beaulieu & Messner, 2010). As such, questions emerge regarding whether the impact of divorce on homicide has weakened or remained stable over time, with implications for the theoretical mechanisms linking divorce to homicide trends, depending on the results. Beaulieu and Messner (2010) used city level data and examined the impact of changes in divorce rates on changes in homicide rates over three decades – from 1960 to 1970, 1970 to 1980, and 1980 to 1990 – and found that the influence of divorce remained stable (and positive) over time, which they note as more support for a "structural/control" theoretical connection rather than a "cultural/normative conflict" connection.

Although the relationship between divorce and overall homicide trends appears quite stable and positive across different methodological specifications, its impact on disaggregated trends is not as straightforward. This may be because of the different theoretical arguments linking divorce with intimate partner homicide (i.e., exposure reduction hypothesis predicts rising divorce rates reduced intimate partner homicide rates over time, whereas other theoretical mechanisms suggest a positive relationship between divorce and overall homicide trends). Using multivariate models, and examining intimate partner homicide trends specifically, empirical evidence finds that divorce is related to a decline in male victim intimate partner homicides, but has no impact on female victim intimate partner homicides (Dugan et al., 1999; Reckdenwald & Parker, 2011), consistent with the exposure reduction hypothesis.

Similar to how increases in divorce reduce exposure to violence, decreases in marriage also decrease the opportunity for violence to turn lethal. While highlighted in theoretical arguments (e.g., Rosenfeld, 2006), empirical tests of changing levels of domesticity (in terms of changes in rates of marriage) on crime trends are rare. Using data from St. Louis, Rosenfeld (1997) finds that most of the decline in intimate partner homicide is attributable to declines in marriage rates. But, Baumer (2008) used both the percent of households with married couples and the percent of households with cohabitating couples and found null effects in most models, including an adult homicide trends model (he did not test intimate partner homicide specifically). Reckdenwald and Parker (2011) found changes in marriage rates were related to changes in male-victim intimate partner homicide trends, but not female-victim intimate partner homicide trends, consistent with the exposure reduction hypothesis.

This is also in line with their findings that changes in rates of divorce impact male intimate partner homicide trends, but not female IPH trends.

Overall, changes in family structure, particularly in terms of changes in divorce, single parent households, and marriage, have primarily been discussed in the crime trends literature in reference to changes in intimate partner homicide rates over time. However, given the importance of family disruption as an established macro predictor of homicide in cross-sectional work (Land et al. 1990; Parker et al. 1999; Pratt and Cullen 2005), an indicator of family disruption is often included in studies of overall homicide trends, as well. Empirical support is strong for different measures of family disruption across levels of aggregation and time periods for overall homicide trends, but results are less consistent when considering disaggregated homicide trends.

Age Structure

Shifts in age structure is one of the most common explanations for post-World War II crime trends (LaFree, 1999), and have most notably been applied in reference to the Baby Boomer generation (i.e., those born in the years following WWII, 1946 to 1964), which is one of the largest generations in U.S. history. Members of this birth cohort began entering their highest crime-prone years (approximately 14 to 24) in the early 1960s and 1970s, when homicide rates began to rise (Fox & Piquero, 2003), and began aging out of the most crime-prone age group in the 1980s, when homicide rates began to fall (Fox, 1978; LaFree, 1999).

Scholars argue that demographic shifts in age structure associated with the large Baby Boomer cohorts likely *did* contribute to increases in homicide rates in the 1960s and 1970s (Sagi & Wellford, 1968; Steffensmeier & Harer, 1991), as well as the subsequent decline in the early 1980s. In fact, the size of the Baby Boomer generation is what spurred scholarly interest in the role of demographic shifts on crime trends (Zimring, 2007). During the 1960s crime boom, there was strong correlation in the two trends; when Baby Boomers hit their teen years, around 1962, crime rates also began to rise (Fox, 2006; LaFree, 1999; Zimring, 2007). As they began aging out of the most crime-prone years, crime rates began to fall. Age structure arguments have received less support for their role in the 1990s crime decline (Barker, 2010; Levitt, 2004; Rosenfeld, 2004, 2006). Skepticism here centers primarily on the timing and speed of these compositional changes. With regard to timing, the homicide rate began to rise in the mid-1980s, despite declines in the youth population beginning in 1980 (Fox, 2006). Additionally, the youth and young adult cohorts actually grew larger in the 1990s, prompting speculation of an impending crime wave and rise of youth "superpredators" (e.g., DiIulio, 1996; Fox, 1996; Fox & Pierce, 1994; Wilson, 1995). But, crime rates fell instead (Blumstein & Rosenfeld, 1998, 2008; Blumstein & Wallman, 2006; Fox & Piquero, 2003; LaFree, 1999; Levitt, 2004). In terms of speed, critics argue that shifts in age composition move too slowly to be associated with the rapid changes in crime rates (Blumstein & Rosenfeld, 1998; Blumstein, 2006; Fox & Piquero, 2003; LaFree, 1999; Levitt, 2004). Other doubters argue that for shifts in age structure to be a viable explanation, all else must be equal, which is rarely the case (Rosenfeld, 2004).

However, important countervailing forces may be at play, and the relationship between age structure and homicide trends needs to be considered in a multivariate context, accounting for these other factors. There is mixed empirical support for an association between changes in age structure and changes in homicide over time. Examining the time frame from 1946 to 1984, Cohen and Land (1987) conclude that most of the changes in homicide rates could be explained by shifts in age structure. Specifically, they found that changes in the percentage of those aged 15 to 29 accounted for 58% of the homicide rate trends during that time period. Other researchers also find either positive effects between youth age structure and homicide trends (e.g., Baumer, 2008; Becsi, 1999; Greenberg, 2001; Kaminski & Marvell, 2002; McCall et al., 2008; Mocan & Gittings, 2003; Ousey & Kubrin, 2014; Phillips, 2006a; Wadsworth, 2010), although just as common are null findings (e.g., Greenberg, 2001; Kovandzic, 2001; Kovandzic et al., 2004; 2009; Kovandzic & Vieraitis, 2006; Marvell & Moody, 1997, 1998, 2010; Vieraitis et al., 2007; Rosenfeld & Oliver, 2008). While not included as often, studies of the impact of age structure for adults past their most crime-prone age (i.e., 45 and older) have found an inverse relationship with homicide trends (e.g., Rosenfeld and Oliver 2008), and Baumer (2008) suggests that the increase in the relative size of the elderly population likely played a role in the 1990s decline, accounting for approximately 4 to 8% of the decline.

These findings do not appear to be contingent on different operationalizations of age structure. Youth age structure is often captured with some variant of %15-29 or a similar age range, and research suggests that the exact age categories do not appear to impact results (Land et al., 1990; Marvell & Moody, 1991). Adult age structure is typically measured as the percent of the population 45 years and older and elderly age structure measured as the percent of the population 65 years and older. While the exact operationalization doesn't appear to be important, researchers typically only include one measure to capture age structure, but some have argued for the need to include multiple age groupings (e.g., Marvell & Moody, 1991). Including both youth and adult age structure in the same model, Baumer (2008) found youth age structure positively impacted homicide trends and adult age structure negatively impacted homicide trends, and these findings held across several model specifications, including different levels of aggregation and time periods.

Evidence of a link between youth age structure and homicide trends is inconsistent across levels of aggregation. For example, positive effects are found at the city (McCall et al., 2008; Ousey & Kubrin 2014; Wadsworth, 2010), county (Phillips, 2006a), state (Besci, 1999; Mocan & Gittings, 2003) and national (Greenberg, 2001; Peterson & Bailey, 1991) levels, with null effects also found at various levels of aggregation, including MSAs (Fowles & Merva, 1996), states (Kovandzic et al., 2004; Moody & Marvell, 2010; Vieraitis et al., 2007), regions (Rosenfeld & Oliver, 2008), and nationally (Greenberg, 2001). However, in their identically specified models, Baumer et al. (2012) found a positive effect of youth age structure on homicide trends at all three levels of aggregation.

While no clear pattern emerges based on unit of analysis, some scholars have examined whether the relationship between age structure and homicide trends is symmetric or asymmetric. That is, they examine whether age structure impacts homicide trends similarly during times of homicide increase and during times of homicide decrease. Noting that homicide rates increased when the Baby Boomer generation entered the most crime-prone age group and declined at a similar rate when the Baby Boomer generation aged out of the most crime-prone age group, Cohen and Land (1987) used national level data to investigate whether the age-structure homicide relationship is symmetric and they found that it was. That is, they found that shifts in age structure contributed proportionately to the 1947 to 1980 increase in homicides as it did to the 1981 to 1984 decline. Conversely, using county-level data Phillips (2006a) included interactions between age structure and time and found the effect varies by time period, and the relationship is asymmetrical. Specifically, she found that the effect of age structure on homicide trends was stronger on the 1990s decline than it was on the 1970s and 1980s increase. These findings are consistent with relative cohort size arguments implying an asymmetrical relationship between age structure and crime trends.

As with some of the other explanations offered later in this section (e.g., immigration, drug markets), some evidence suggests that the relationship between age structure and homicide trends may also be contextual and contingent on other social and economic factors. For example, Phillips (2006b) found that the positive relationship between youth age structure and homicide trends was moderated by divorce, population size, income, and unemployment. Additionally, when commenting on factors impacting recent crime trends, scholars have noted that shifts in age composition alone likely did not account for the rapid changes we have observed in crime rates in recent history, but changes in age structure may have interacted in important ways with other social changes at the time, including factors impacting the *rate* of involvement in crime among different age groups (e.g., Conklin, 2003; Zimring, 2007). In sum, while changes in age composition have intuitive appeal based on the correlation in the trends, empirical evidence is lacking. However, there is evidence to suggest that the relationship between age structure and homicide trends may be contingent on historical period and/or other social and economic conditions.

Immigration

The U.S. has seen rapid expansion of immigration in recent years, with more than 13 million immigrants entering the U.S. between 1990 and 2000 alone, especially those migrating from Mexico (Sampson, 2008; Urban Institute, 2002; Wadsworth, 2010). In fact, the U.S. Hispanic population more than doubled between 1990 and 2010, to just over 50 million (Light & Ulmer, 2016). This represents a more than 50% increase in the foreign-born population during the decade (Sampson, 2008) and constitutes the largest influx of immigrants in U.S. history (Light & Ulmer, 2016). As a result, Latinos now constitute the largest minority at 15% of the population and foreign-born individuals make up 13% of the population (Light & Ulmer, 2016; MacDonald & Sampson, 2012), a change which Sampson (2008) notes is "one of the most profound social changes to visit the United States in recent decades" (p. 29). The largest growth in immigration occurred in the mid-1990s, at the exact time crime rates began to fall. And, immigration levels hit their peak in 1999, and crime rates began to level off around the same time, in the early 2000s (Sampson, 2008).

Although traditional criminological theory based in the Chicago tradition (e.g., Shaw & McKay, 1942) suggests that increases in immigration would be associated with increases in crime, this is often not the pattern we observe in empirical research. Instead, recent scholarship suggests that there is no evidence that increased immigration flows contribute to increases in crime, and increased immigration may even contribute to *declines* in crime over time. Given these findings in the empirical literature and the coinciding trends, criminologist Robert Sampson pointed to increased immigration in the 1990s and into 2000 as a driving force behind the 1990s crime decline in a 2006 *New York Times* op-ed (see also Sampson, 2008). Prior to his claim, immigration had not figured prominently into crime trends debates (see also Table 1), and his assertion served as a catalyst for future empirical inquiry in the area

(Barranco et al., 2018; Wadsworth, 2010)[6]. Since then, scholars have documented that cities with the highest rates of immigration in the 1990s, including New York, Los Angeles, San Jose, Dallas, and Phoenix, or border cities, such as El Paso and San Diego, experienced some of the largest drops in crime and homicide (Barker, 2010; MacDonald & Sampson, 2012; Sampson, 2008). Even within New York City, the homicide rate declined faster in precincts with higher concentrations of Hispanics (Greenberg, 2014).

Importantly, increased immigration flows not only contribute to crime declines by altering the base population from which crime rates are derived (i.e., demographic changes), but also changing the social fabric of U.S. cities, helping to revitalize communities in various ways including contributing to economic growth in the area, impacting family and community ties and informal social control, and increasing racial and ethnic diversity, shifting the racial composition of urban areas and impacting group exposure and altering relationships between other racial and ethnic groups (Lee & Martinez, 2002; Logan, Alba, & McNulty, 1994; Light & Ulmer, 2016; Ousey & Kubrin, 2009; Parker, 2008; Parker & Stansfield, 2015; Sampson, 2008; Wadsworth & Kubrin, 2007). That is, increased immigration may also bring with it widespread benefits to the broader community (MacDonald & Sampson, 2012), driving down crime. Despite compelling claims and preliminary evidence suggesting increased immigration may have played a role in the 1990s crime decline, some

[6]Although it is a relatively "new" explanation for recent homicide trends, immigration is considered as one of the now "common" explanations given the scholarly attention it has received as a plausible cause for recent homicide trends since Sampson's initial assertion (Sampson, 2006).

remain skeptical of it as the primary driving factor (Barranco et al., 2018; Baumer, 2008; Rosenfeld et al., 2007). Given its relative infancy as a top contributor to recent crime trends, longitudinal tests of the immigration-crime trends link are more limited than many of the other competing explanations. However, preliminary evidence does suggest increased immigration could have played a role in the 1990s crime decline.

Longitudinal studies of the immigration-homicide link tend to find either no effect of changes in immigration on changes in total homicide rates (Baumer, 2008; Chen, 2008; Kovandzic et al., 2004; LaValle, 2007, 2010; McCall et al., 2008; Wadsworth, 2010; Worrall & Kovandzic, 2007), or they find an inverse relationship (Ousey & Lee, 2007; Ousey & Kubrin, 2014; Stowell et al., 2009). While the predominate way to assess the immigration-crime trends link is with a measure of percent Hispanic or Latino, percent foreign-born, or an immigrant concentration index that combines these measures (sometimes with a measure of linguistic isolation), some scholars have drawn attention to the role of *recent* immigrants instead of all immigrants. For example, Wadsworth (2010) initially examined the longitudinal relationship between the change in immigration from 1990 to 2000 on the change in homicide rates from 1990 to 2000 using two separate measures of immigration – percent Latino and percent foreign-born – but he found no effect. However, using a measure of "new" immigration (which he defined as entering the U.S. in the past 5 years), he found that new immigrants were associated with declines in homicide rates in U.S. cities from 1990 to 2000, and estimates that growth in the immigrant population was responsible for approximately 9.3% of the decline in homicide rates during this time period.

The vast majority of studies that examine the relationship between changing immigration and overall homicide trends are conducted at the city level (e.g., Kovandzic et al., 2004; LaValle, 2010; McCall et al., 2008; Ousey & Kubrin, 2014; Wadsworth, 2010). However, although empirical tests are more limited, there is some preliminary evidence to suggest that this relationship may vary based on unit of analysis. For example, Baumer et al. (2012) found that immigration was significantly related to homicide trends at the city, county, and state levels. However, their measure of immigrant concentration was significant and negative in the city-level models, but significant and positive in both the county and state-level models. Because macro longitudinal research on this relationship began after Sampson's (2006) claims about the role of immigration increases on the 1990s crime drop, empirical tests have almost exclusively focused on this time period, and there is not enough variation yet to say how, or if, time period impacts results.

Scholars have suggested that the effect of rising immigration on homicide trends may be contextual, and depends on the type of homicide, as well as the location, being examined. For example, examining city-level trends from 1980 to 2010, Parker and Stansfield (2015) found rising immigration decreased black homicide trends, but had no effect on white homicide trends. Similar inconsistencies emerge when examining the impact on homicide trends disaggregated by type. Ousey and Kubrin (2014) found that while increased immigration was associated with declines in overall and drug-related homicide trends, it was not significantly related to temporal trends in felony, argument, or gang-related homicides.

During the influx of immigration during the 1990s, most immigrants settled in immigrant enclaves (Sampson, 2008). Scholars speculate this may be one reason for the supposed “Latino Paradox,” or the consistent finding that immigrants do better on a number of social indicators, including involvement in crime, than other groups living in similarly disadvantage areas (Martinez, 2002; Nielsen, Lee, & Martinez, 2005; Sampson, Morenoff, & Raudenbush, 2005). This has led some scholars to specifically consider the location, including whether the relationship is being assessed for the immigration-homicide trends link in a “new destination” or a “traditional destination” (Ousey & Kubrin, 2018; Shihadeh & Barranco, 2013). Considering the baseline immigrant population, Ousey and Kubrin (2014) found that the protective effect of increased immigration was enhanced in cities with a larger 1970 immigrant population base, and this held regardless of homicide type. That is, cities that had more immigrants in 1970 experienced steeper declines in homicide between 1970 and 2010 regardless of the type of homicide being examined.

Overall, research on the link between recent surges in immigration and homicide trends is in its infancy compared to the other explanations presented in this chapter. However, there is some evidence to suggest this shift was associated with recent crime declines, but that the relationship may be more complex, possibly dependent on whether scholars are assessing the impact of all immigrants or *recent* immigrants, the type of homicide considered, and the location being examined (i.e., traditional v. new destinations). Nevertheless, it is considered here given it has recently surfaced as a top contender for consideration in recent crime trends debates.

Policing

Policing has changed considerably over the past few decades. For one, in response to rapidly increasing crime rates in the late 1980s and early 1990s, President Clinton signed into law the 1994 Violent Crime Control and Law Enforcement Act.

This crime bill called for the hiring of 100,000 more police officers (Eck & Maguire, 2006; Zimring, 2007), and although nowhere near the projected 100,000 officers were hired, this still resulted in a substantial increase in police force size and police expenditures in most cities around this time frame (Barker, 2010). For example, Levitt (2004) notes a 14% increase in police force size between 1990 and 2000, and police expenditures approximately doubled (Barker, 2010). In addition to increased manpower, substantial innovations in policing have also occurred since the 1990s. Notable innovations include various hot spots policing initiatives, the use of Compstat, stop and frisk tactics, problem-oriented policing, broken windows/zero tolerance policing, and other forms of community policing. As such, when considering the impact of police on changes in homicide rates over time, discussions commonly focus on either changes in the quantity of police (i.e., changes in police force size and expenditures) or changes in the quality of policing (i.e., innovations in policing strategies).

Noting that crime declined in the 1990s in places where the number of police stayed the same or even declined, some began to turn their attention to specific policing strategies, arguing that despite reductions in manpower, police have become more *effective* at preventing and controlling crime. New York City is the most prominent example of this, given their unparalleled decline in crime and homicide in the 1990s (a 70% reduction in homicide in NYC compared to a 40% drop in homicide nationally) (Zimring, 2007). Local officials, including NYC Mayor Rudolph Guiliani and Police Commissioner William Bratton, strongly advocated for the role of police, including changing policing strategies, as the main reason for such an extreme decline. An examination of the trends, however, shows that crime rates actually started falling in NYC *before* any of the policing innovations were implemented (Levitt, 2004). Furthermore, cities that did not make any changes in their policing strategies (e.g., Los Angeles) and those that adopted alternate strategies (e.g., San Diego) also experienced significant declines in crime and homicide rates in the 1990s (LaFree, 1999; Levitt, 2004; Rosenfeld, 2002, 2004; Wintemute, 2006), calling the role of changing police strategies into question. This led many to conclude that changing police strategies may have been responsible for *some* of the drop in crime and homicide in *some* cities (e.g., the extreme drop in NYC above the national average), but that it is unlikely that these more localized explanations contributed much to the overall national trend (Eck & Maguire, 2006; Rosenfeld, 2004; Zimring, 2007). Critics also argue that just because there was increased emphasis on and discussion about policing innovations in the 1990s, does not tell us anything about how these policing strategies were implemented or how well they were implemented (see e.g., Eck & Maguire, 2000; Zimring, 2007).

In general there is considerable disagreement as to the role police played in crime trends, and the crime decline specifically, and no consensus as to whether they have any effect (Barker, 2010). For example, the increase in police force size has been strongly advocated by some as a top contributor to the drop in homicides in the 1990s (e.g., Levitt, 2004), while others have dismissed its role (e.g., Zimring, 2007), sometimes quite adamantly. Notably, after their extensive review of 27 studies that examined the effect of police strength on violent crime rates, policing scholars John Eck and Edward Maguire (2006) even conclude, “We are not aware of a single empirical study that supports the claim that increases in the number of police officers are responsible for decreases in violent crime” (p. 209). The impact of innovations in policing on crime trends has also received widespread attention, not only in the scholarly literature, but also among the public (Levitt, 2004).

Despite the increased emphasis, the empirical support is mixed. Earlier studies found that increased police presence either was unassociated with homicide trends or actually increased homicide trends (see Cameron, 1988; Marvell & Moody, 1996 for reviews), but these studies were heavily critiqued on methodological grounds for failing to account for the potential simultaneity in the police-crime relationship (see e.g., Levitt, 2004). More recent research that has given increased attention to the possibility of simultaneity, however, find that increased police force size contributed to a declines in homicide rates over time (e.g., Marvell & Moody, 1996; Levitt, 1997, 2002; Lin, 2009).

Increased police strength is often captured empirically with indicators of police manpower and resources, including measures that capture police force size, police expenditures, and arrest ratios, making it easier to test empirically than arguments about changing police strategies. However, the other policing factor, changes in policing strategies is hard to test empirically, due to data limitations.

Because policing is largely a local phenomenon, empirical tests are often conducted at the city or county-level or smaller, and studies examining policing effects at larger units of analysis may suffer from aggregation bias (Kelling & Bratton, 2015). In fact, Rosenfeld and Oliver (2008) included a measure of police force size when testing homicide trends at the regional level. They didn’t find an effect but also noted that their findings should be interpreted with caution because it was tested at the regional level.

In sum, policing has been one of the most touted explanations for recent crime trends. However, it is also one of the most contentiously debated factors. This is exacerbated by the difficulty with empirically testing arguments regarding the role of police on crime trends, specifically with regard to changes in policing strategies. Additionally, changes in police organization occur at the local level, making changes in policing unlikely to account for more national crime patterns that we've observed in recent decades. Instead, it is more plausible that police may have contributed to certain cities seeing sharper declines than the rest of the nation (e.g., New York City). As such, both changes in the quantity and quality of policing are assessed in this dissertation.

Corrections

Largely as a result of the "get tough on crime" movement and the War on Drugs and related changes in sentencing guidelines, incarceration rates in the United States skyrocketed beginning in the mid-1970s, increasing over 400% from approximately 100 prisoners per 100,000 population to over 500 prisoners per 100,000 residents by the end of the century (LaFree, 1999; Levitt, 2004; Parker, 2008; Rosenfeld, 2004), with much of the increase occurring in the 1990s (LaFree, 1999; Levitt, 2004; Zimring, 2007). Although incarceration rates soared across the board during this time frame, African-Americans were incarcerated at a rate greater than 6 times their white counterparts from the 1980s to the mid-1990s (Beck & Gillard, 1995). The correlation in trends (i.e., rising incarceration rates and falling crime rates) suggests a connection between the two. However, critics argue that incarceration plays little to no role because homicide rates rose sharply during the 1970s and late 1980s, despite rapid prison expansion (Zimring, 2007). Also, both the U.S. and Canada experienced crime declines from approximately 1991 to 2000. However, the incarceration rate in Canada remained fairly stable over this time period, and even decreased by 12% from 1994 to 2000, casting serious doubt on the role of incarceration in explaining crime declines in the U.S. (Farrell, 2013; Zimring, 2007). Scholars also note the diminishing returns associated with increased incarceration (i.e., with each additional person we incarcerate, the crime-reducing effects decline) (Johnson & Raphael, 2012; Spelman, 2006; Zimring, 2007), and have argued that we likely reached a period of diminishing returns in the 1980s *before* crime rates began to fall (Roeder et al., 2015). Conversely, supporters of incarceration's role assert that homicide rates likely would have risen even higher in the 1980s and 1990s due to counteracting forces (e.g., the proliferation of crack cocaine markets), had it not been for major increases in incarceration (Levitt, 2004; Spelman, 2006). For example, Rosenfeld (2006) credits incarceration with a 7.2% reduction in total homicide rates and an 18.9% reduction in adult homicide rates from 1985 to 1990. That is, homicide rates in the late 1980s and early 1990s would have been much higher had it not been for increases in incarceration.

While still increasing, incarceration growth began to slow considerably in the early 2000s (Wallman & Blumstein, 2006), prompting speculation that the reduced growth in incarceration rates since 2000 may be partially responsible for the homicide trend leveling off in the early 2000s, as well as contributing to the "blip" in homicides in 2005 and 2006 (Rosenfeld & Oliver, 2008). In more recent years, the net changes in incarceration have been approximately zero due to offenders being released at the same rate that they are being incarcerated (Domanick, 2010; Rosenfeld & Oliver, 2008).

Increases in incarceration rates is one of the most heavily debated explanations for contemporary crime trends, drawing both strong support and strong criticism from scholars. Considering the sheer magnitude of the increase in incarceration rates in recent decades, many experts conclude that at least *some* of the decline in homicide rates in the 1990s can be attributed to increases in incarceration, with estimates ranging anywhere from 10 to 25 percent of the drop due to increased incarceration (Barker, 2010; Levitt, 2004; Rosenfeld, 2006; Spelman, 2006. Western, 2006). Some scholars go on to make stronger statements regarding its role. For example, Marvell and Moody (1997) note that prison expansion is “probably a major reason why homicide declined after 1990” (p. 220) and Levitt (2004) notes that the evidence in favor of incarceration reducing crime is “very strong” (p. 178). However, noting that crime declined in Canada and other European countries without corresponding increases in incarceration, Farrell (2013) concludes that increased incarceration does not pass the “cross-national test,” debunking it as a plausible theory of the crime drop (see also Zimring, 2007 for a similar argument).

Overall, the weight of the empirical evidence does tend to support the link between increased incarceration and declines in the total homicide rate, (Buonanno & Raphael, 2013; Cohen & Land, 1987; Devine et al., 1988; Kovandzic et al., 2004a; Levitt, 1996; McCall et al., 2008; Rosenfeld & Oliver, 2008; Vieraitis et al., 2007). Studies assessing the impact of incarceration on homicide trends often use the state incarceration rate. Because incarceration data is not available at lower levels of aggregation, state data is used as a proxy, even in studies conducted at smaller units.

The general finding of an inverse relationship between incarceration trends and total homicide trends persists at various units of analysis, although the magnitude of the effect differs, depending on level of aggregation. In general, studies find stronger effects at the national level than at sub-national levels (see Marvell & Moody, 1998). In an effort to make sense of this finding, Marvell and Moody (1998) examined whether displacement or free-rider effects could partially explain the reduced findings at the sub-national level. After running separate regressions for all 50 states from 1929 to 1992, they found that out-of-state prison populations had a strong negative effect on in-state homicide trends, which they interpreted as strong evidence of free-riding effects, where nearby states benefit from high incarceration rates in surrounding states. They conclude that estimates derived from state-level studies actually underestimate the relationship between incarceration trends and homicide trends.

Despite fairly consistent evidence that increases in incarceration contribute to declines in the total homicide rate over time, the effect is less clear when homicide trends are disaggregated. For example, when homicide trends are disaggregated by race, scholars find that the protective effects typically associated with increased incarceration disappear for both White and Black homicide trends (LaFree & Drass, 1996; LaFree et al., 1992; Messner et al., 2001), and Parker (2004) found that state-level incarceration rates actually increased Black female homicide trends, while it had no effect on Black male homicide trends. Additionally, because incarceration is much more likely for adult offenders, from a pure incapacitation standpoint, incarceration is argued to have little effect on youth homicide offending (Blumstein, 2006; Spelman, 2006). Some evidence of this can be seen in the numbers estimated by Rosenfeld (2006) presented above, with the impact of increased incarceration on adult homicide trends much larger than on the juvenile homicide trend. Examining the impact of changing incarceration rates on race-specific youth homicide trends, Messner et al.

(2001) found that changes in incarceration rates were unrelated to black and white youth homicide trends.

Some argue that the effects of incarceration are subject to diminishing returns. That is, as incarceration rates rise, the added benefit of increasing each additional individual declines. After the incarceration rate reaches a certain level, it is no longer an effective crime reduction tool and may even increase crime (see e.g., Barker, 2010; Liedka et al., 2006; Roeder et al., 2015; Rose & Clear, 1998). Explicitly examining the issue, Roeder and his colleagues (2015) find evidence of diminishing returns beginning in the early 2000s. This, they argue, means that incarceration had no effect on the decline in violent crime rates from 2000 on. As such, the time period under consideration could be critical for understanding the link between incarceration and homicide trends.

Given some of the observed inconsistencies, particularly among a set of studies that used the same state-level data set and covered a similar time span, Spelman (2008) notes that the discrepancies are likely due to methodological artifacts, including the need to distinguish between short-run and long-run effects when estimating the incarceration-crime trends relationship. Increasing incarceration is primarily hypothesized to decrease crime rates through deterrence (Zimring & Hawkins, 1973) or incapacitation (Zimring & Hawkins, 1995). While incarceration growth may contribute to short-term declines in crime rates, scholars have also noted the detrimental effects of prison expansion, including breaking up families, depleting social capital in communities, and increasing unemployment rates (e.g., Rose & Clear, 1998; Rosenfeld, 2004), potentially increasing crime rates in the long-run. Given these potential long-term consequences of rising incarceration, distinguishing between the short-term and long-term effects is imperative, yet often lacking in the empirical literature.

Related to the role of incarceration on homicide trends, scholars have also considered the impact of specific sentencing legislation, such as the effect of three strikes laws. For example, Marvell and Moody (2001) used state-level data from 1970 to 1998 and found that states with three strikes laws (compared to states without laws) experienced 10 to 12 percent more homicides in the short-run and 23 to 29 percent more homicide in the long-run. Using city-level data from 1980 to 1999, Kovandzic and his colleagues (2002) also found positive effects, with cities with three strikes laws experiencing a 13 to 14 percent increase in homicides in the short-run and 16 to 24 percent increase in homicides in the long-run, compared to cities in states without laws.

In all, incarceration seems to be one of the most agreed upon factors impacting recent crime trends. Not only do scholars who have conducted narrative reviews of the literature note a connection, but multivariate tests also overwhelmingly find support. However, there is substantial debate as to the magnitude of its impact, and some evidence to suggest the strength of the relationship varies based on level of aggregation, with national level studies finding stronger effects, and time period covered, with incarceration reaching the point of diminishing returns in more recent years. Perhaps more importantly though, are questions of whether the changes in crime rates attributable to increases in incarceration are worth the monetary and social costs associated with mass incarceration.

Drug Markets

The emergence and waning of drug markets is often cited as a contributing factor to temporal trends in homicide. The U.S. has gone through many different drug eras including heroin in the 1960s to mid-1970s, cocaine in the early 1980s and crack cocaine in the late 1980s, marijuana in the 1990s (Johnson et al., 2006), and more recently, a return to heroin (Shapiro, 2016; Williams, 2016). Each of these drug eras had their own unique cultures and norms (Johnson et al., 2006), with some more conducive to violence than others. Historically, the rise and fall of different drug market eras has been linked to homicide rate fluctuations over time. For example, the increase in heroin use was linked to increases in crime rates in the 1960s (Wilson, 1975), crack cocaine markets were implicated in the rise and fall of youth homicide in the 1980s and 1990s (Blumstein, 1995), the rise of the marijuana generation was argued to contribute to the decline in homicide in the 1990s (Johnson et al., 2006), and more recently, rising heroin markets have been offered to explain recent increases in homicide in large U.S. cities (Rosenfeld, 2016; Rosenfeld et al., 2017).

Although each of these drug market eras has been linked at one point in time to crime trends, none have received as much scholarly attention as the emergence and waning of crack cocaine markets. Drawing on earlier theoretical models linking drug markets (as opposed to drug use) to homicide and violence (i.e., Goldstein, 1985), Blumstein (1995) articulated one of what's often considered to be one of the most compelling arguments to explain the sudden and rapid rise in youth homicide in the late 1980s. According to Blumstein (1995), the transition from powder cocaine to crack cocaine in the 1980s increased the number of transactions between sellers and buyers because crack was much cheaper but short-lasting and accompanied by an intense high. The increased demand coupled with increased arrest rates for adult sellers due to the War on Drugs contributed to the recruitment of juveniles, particularly non-white youths, into the market as replacement sellers. The increased demand (and associated profitability) and open-air nature of the market also increased the number of disputes. Because these disputes could not be resolved through the police or other mechanisms of formal social control, drug dealers had to find alternate means to handle them, often resorting to violence. Due to this high potential for violence, those involved in the drug trade increasingly armed themselves for protection. This instigated an "arms race" which spread beyond just those involved in the drug trade and into the broader community, where residents also armed themselves for protection. According to Blumstein, this diffusion of firearms and associated "community disorganization" that resulted contributed to the rapid rise and fall of youth homicide (but see Kleck, 1997 for a counter argument).

The increases in homicide in the 1980s were primarily accounted for by increases in homicides committed by youths with guns. Additionally, the homicide rate increase was steepest and began earlier in larger cities, where crack markets originated, before spreading to smaller cities (Blumstein, 2006; Rosenfeld, 2002). Blumstein's argument is appealing in many ways because it can account for many of these divergent trends (e.g., increases in firearm-related homicides, steepest increases for youths, increases beginning in large cities) (Rosenfeld, 2002; Levitt, 2004). As such, there is general consensus that shifts in drug market activity contributed to the increase in homicides in the 1980s and early 1990s. While Blumstein's argument is compelling, several scholars note that it cannot explain the decline in youth homicides in the 1990s as well as it can explain the increase in the late 1980s (e.g., Rosenfeld, 2002; Zimring, 2007), and have offered alternate, but related, explanations for the decline.

For example, Grogger (2006) incorporates an economic model to explain the link between drug markets and homicide rates, using the widespread diffusion of guns to explain the decline. He argues, like Blumstein (1995), that as transactions become more frequent, more sellers were needed to keep up with demand, with more and more arming themselves for protection. However, this potential for and proliferation of violence also increased the cost associated with obtaining crack, contributing to the decline in consumer demand, and eventually a decline in drug market violence and homicide (Grogger, 2006). Ousey and Lee (2007) empirically examined the related possibility that the homicide decline was due to drug markets becoming less lethal over time. Their results indicated that drug markets contributed to a decline in homicide because the drug market-homicide association has weakened over time (i.e., drug markets have become safer) as opposed to drug markets merely becoming less prevalent. As another possibility, Johnson and his colleagues (2006) argue that the rise of the younger marijuana generation, dissuaded from involvement in crack markets having seen the problems their older siblings, peers, and parents faced, contributed to decreases in crack markets and homicide in the 1990s (Curtis, 1998).

Although changes in drug market activity is a major explanation of contemporary homicide trends, empirically it is difficult to test these arguments and assessments are often based on the timing of the trends and intuitive appeal. Multivariate tests are actually very rare, and it is not common to see measures of drug market activity included as controls when it is not the primary focus (for exceptions see McCall et al., 2008; Ousey & Kubrin, 2014). This is an important omission given the role of drug markets in recent discussions about homicide trends. Although empirical tests are limited, some scholars find support for their role. For example, using city-level data from 1976 to 1996, Cork (1999) provides the first empirical test of Blumstein's (1995) argument and concludes that it is "highly plausible" (see also Baumer, 2008; Ousey & Lee, 2002, 2007).

Drug market activity is hard to capture empirically, and it is often operationalized using drug sales arrest rates, although the best way to capture drug markets has been debated. Specifically, questions arise about whether increased drug arrests actually indicate increased drug market activity or whether it just indicates increased *enforcement* of drug markets. Researchers have compared Arrestee Drug Abuse Monitoring (ADAM) program data, which includes indicators of actual drug use, such as cocaine-related deaths and hospital visits, to the drug arrest data. They find that the drug arrest data tends to be highly correlated with the ADAM data, increasing confidence in its validity as an indicator of drug market activity as opposed to just capturing changes in enforcement (see e.g., Ousey & Lee, 2007 for a discussion). Given this debate regarding appropriate measures to adequately capture changing drug markets, some scholars have replicated their initial findings with alternative drug market indicators to see if their findings are contingent on the specific drug market measure used (e.g., Ousey & Lee, 2002), and found that results were consistent across measures. However, using three different indicators of drug market activity – the cocaine/heroin arrest rate, cocaine mortality rate, and percent of cocaine/heroin arrests that were under 18 – Baumer (2008) consistently found that the two arrest indicators were positively associated with overall homicide trends but that the cocaine mortality rate was unassociated across models.

Given drug market activity is largely a local phenomenon, empirical tests are most commonly conducted at the city-level (e.g., Baumer, 2008; Baumer et al., 1998; Browne et al., 2010; Corman & Mocan, 2000; Greenberg, 2016; McCall et al., 2008; Ousey & Lee, 2002, 2004, 2007; Parker et al., 2011; Pepper, 2008; Strom & MacDonald, 2007). For the most part, these empirical tests at the city-level find a positive association between drug market activity and homicide trends (Baumer, 2008; Baumer et al., 1998; Browne et al., 2010; Ousey & Lee, 2002, 2007; Ousey & Kubrin, 2014; Parker et al., 2011), although some find no relationship (Baumer et al., 2012; Corman & Mocan, 2000; Greenberg, 2016), or even a negative one (McCall et al., 2008). Studies conducted at the state level and higher are relatively rare, but when examined, they tend to find no relationship between drug market activity and homicide trends (Baumer et al., 2012; Moody & Marvell, 2010).

However, evidence suggests that this relationship varies over time. For example, Ousey and Lee (2007) considered the question of stability or change in the drug market-homicide trends nexus. That is, they examined whether drug markets contributed to a decline in homicide rates because they have become less prevalent or because they have become less lethal over time. Their results indicated that the drug market-homicide trends association has weakened over time (i.e., drug markets have become safer). Additionally, McCall and her colleagues (2008) interacted their drug market activity indicator with time period and found drug market activity to be positively related to the increase in homicide trends in the 1990 and 2000 time periods, but the overall relationship from 1970 to 2000 was negative.

In addition to being historically contingent, scholars have also suggested that the drug market-homicide trend relationship is contextual and contingent on pre-existing structural features (Zimring & Hawkins, 1997). Testing this argument, Ousey and Lee (2002) found that the deleterious effect of within-city changes in drug markets on within-city changes in homicide rates was amplified in disadvantaged neighborhoods, and that this effect held across several alternative drug market measures. Ousey and Lee (2004) also note compelling reasons to expect the effects to differ by race, as well as reasons to expect differences in how structural features moderate the drug markets-homicide trend relationship by race. In support of their arguments, they find that drug markets positively affect both black and white homicide trends, but that the relationship is stronger for black homicide trends. As in their earlier analysis (Ousey & Lee, 2002), they also find that structural factors moderate the relationship between drug markets and homicide trends, but that they moderate the relationship differently for blacks and whites. Furthermore, Strom and MacDonald (2007) illustrate the importance of changes in economic and social characteristics on race-specific youth homicide victimization over time, independent of the effects of drug markets. They find that once they take social and economic indicators of disadvantage into account, the effect of drug markets disappears.

Taken together, empirical evidence suggests support for the role of changes in drug markets on homicide trends, but empirical tests are relatively rare in relation to the amount of scholarly attention this explanation has received in conceptual arguments. Research suggests that the relationship may be stronger at a more local level and is likely contingent on historical period and pre-existing structural factors, but still holds promise as a plausible explanation for contemporary homicide trends.

Guns

Firearms became increasingly available in the 1960s, with handgun sales quadrupling and long-gun sales doubling (Farley, 1980). The late 1960s and early 1970s was also a time of sharp increases in homicide rates, with much of the increase being in homicides committed with firearms (Fisher, 1976; Newton & Zimring, 1969), increasing from 55% of homicides that were gun-related in 1960 to 67% in 1975 (Farley, 1980). This time of increasing violence also led to more people arming themselves for protection, potentially contributing to even more violence (Farley, 1980). Two decades later, inexpensive, high capacity, semi-automatic pistols emerged on the market for the first time (Wintemute, 2006), along with an upsurge in homicide rates in the late 1980s and early 1990s. In fact, statistics show that almost all of the increase in homicide rates in the late 1980s and early 1990s was due to increases in homicides committed with handguns (Cook & Laub, 1998; Rosenfeld, 2004), and there were no increases in the rate of homicides committed with long-guns or other weapons (Blumstein & Rosenfeld, 1998; Fox & Zawitz, 2000). In fact, homicides with other weapons actually declined during this period (Rosenfeld, 2004). The increased lethality of pistols, as compared to revolvers that predominated in the past, contributed to more assaults ending in death instead of injury despite advancements in medical services that were occurring at the same time (Wintemute, 2006). Some scholars also argued that increased firearm prevalence also had a diffusion effect (e.g., Blumstein, 1995; see previous section), where more and more individuals armed themselves as a form of self-protection, thus increasing the probability of an argument or interaction escalating and becoming lethal. The decline in homicide in the 1990s was partially due to a reduction in homicides committed with firearms (Wintemute, 2006), although even non-gun homicides also declined (Zimring, 2007). Many scholars agree that the decline in the gun-related homicides was largely due to a decline in gun prevalence, but differ in whether they attribute the decline in prevalence to various gun control initiatives, such as changes in gun laws and more aggressive policing of guns, or to a reduced inclinations to carry (independent of the effects of gun control initiatives) (Blumstein & Rosenfeld, 1998). For example, Wallman and Blumstein (2006) note that there has been a decline in weapons arrests, but not because of diminished enforcement, suggesting that individuals are less inclined to carry firearms than they were in the past. However, beginning in the early 1990s, production of semi-automatic pistols declined (Wintemute, 2006), contributing to an overall decline in handgun availability, paralleling declines in gun homicides during the same time period.

One line of thinking regarding the relationship between guns and homicide is that more guns lead to more crime, and specifically homicide. Inherent in this argument is that if more guns lead to more crime, than less guns should lead to less crime, and one potential way to combat rising firearm violence is to enact laws and strategies aimed at reducing firearm prevalence. As such, a number of gun control initiatives (including legislation targeting firearm sales and purchases, firearm removal, and sentencing enhancements for firearm-related offenses) were implemented in the 1990s. These include the Brady Handgun Violence Prevention Act of 1993 which required a five day waiting period and stricter background checks on any individual attempting to legally purchase a firearm. The Brady Bill also banned felons from legally purchasing firearms (LaValle, 2010; Wintemute, 2006). Many states also passed right-to-carry (RTC) laws in the mid-to-late 1990s. Although tapping opposite aspects of gun arguments (i.e., “more guns, more crime” and “more guns, less crime," respectively), both of these changes to gun legislation occurred around the same time that homicide rates began to fall.

Overall, there is weak empirical support for the role of gun legislation on homicide trends. In fact, some of the initial studies of the role of gun legislation that found support were later critiqued on methodological grounds. For example, Lott and Mustard (1997) provided the first empirical test of their thesis and reported strong support for their argument. Though not homicide specific, they found that RTC laws were to credit for large declines in violent crime. However, future research has critiqued their study on serious methodological grounds. In a re-analysis using the original Lott and Mustard (1997) data, Black and Nagin (1998) found no support for the effect of concealed carry laws on homicide rates once they accounted for methodological concerns. Other studies have similarly not been able to replicate the results and have voiced criticisms (e.g., Ayres and Donohue 2003). Similarly, Ludwig and Cook (2000) found that the Brady Law had no impact on homicide trends. Additionally, LaValle (2010) used eight different measures of various gun control laws and after correcting for a number of methodological issues put forth in a recent National Academy of Science Report about gun policy research, he found limited support for the role of gun laws in 20 major U.S. cities from 1970 to 2005. Of the eight different measures he used, he only found significant negative effects for sentencing enhancements on the total homicide trend and he found that the Brady law had a negative effect on the gun-homicide trend. When testing the effects of various gun control legislation, dummy variables for whether the law has been enacted or time trend variables for years since implementation are commonly used. Empirical tests of the more guns, less crime argument typically consist of the inclusion of a dummy variable representing whether or not the state has concealed carry laws (aka right-to-carry laws) in place or a time trend variable which captures the number of years since implementation of the law.

There is more support for the more guns, more crime argument, and specifically that increases in homicide prevalence over time are associated with increases in homicide rates over time (Cook & Ludwig, 2006; Duggan, 2001; McDowall, 1991; Ousey & Lee, 2002, 2007; Parker et al., 2011; Sorenson & Berk, 2001; see also Hepburn & Hemenway, 2004 for a review). However, others find no effect (e.g., Moody, 2010; Moody & Marvell, 2010). Because it is so difficult to capture gun prevalence, proxy measures are often used. Some of the most commonly used indicators are the percent of homicides or suicides committed with a firearm (e.g., Baumer, 2008; Browne et al., 2010; Moody, 2010; Moody & Marvell, 2010; Narayan & Smyth, 2006; Ousey & Lee, 2002, 2007; Parker et al., 2011), although other proxies have also been used, including gun magazine subscription rates (Duggan, 2001), NRA membership rates (Dezhbakhsh et al., 2003; Moody & Marvell, 2010), and handgun sales (McDowall, 1991; Sorenson & Berk, 2001). These general findings tend to persist regardless of the specific proxy measure used and also across time periods and units of analysis.

The lack of empirical support for the role of gun legislation on overall homicides is not surprising. Instead, if there is a connection between changes in gun prevalence and changes in homicide rates over time, one would expect the effects to be stronger on gun homicide trends than other homicide trends, and empirical evidence supports this. For example, using county and state panel data from 1980 to 1999, Cook and Ludwig (2006) found that gun ownership was associated with increases in gun homicides over time, but had no impact on non-gun homicide trends. Similarly, scholars have found no effect of the Brady Bill on total homicide trends (LaValle, 2010; Ludwig & Cook, 2000), but a negative effect on the gun homicide trend (LaValle, 2010). Additionally, although studies do tend to find a positive association between proxies for gun prevalence and overall homicide trends, studies that consider the impact on firearm-specific homicides find even more support. Specifically, studies that include both overall homicide trends and trends disaggregated by weapon type find gun prevalence positively impacts firearm-related homicides, but has no impact on non-gun homicides (e.g., Baumer, 2008; Duggan, 2001), and that the relationship is stronger when examining firearm-related homicide compared to overall homicide trends (Duggan, 2001; Sorenson & Berk, 2001). Some researchers have also found the relationship varies based on demographic patterns, including finding stronger effects for youth homicide trends than adult homicide trends (e.g., Sorenseon & Berk, 2001), and stronger effects for male homicide trends than for female homicide trends (Sorenson & Berk, 2001).

In sum, several explanations have been put forth to explain recent changes in U.S. homicide rates. While the explanations covered above are by no means exhaustive, they capture some of the most prominent explanations for temporal changes in U.S. homicide rates that also have received sufficient empirical evidence to assess and include in the meta-analysis. Taken together, the crime and homicide trends literature suffers from a number of contradictory conclusions and inconsistent findings. I have attempted to document the overall patterns and trends in these key explanations, noting their correspondence or divergence from the homicide trend. Additionally, I also provided empirical evidence of the connection (or lack of connection) between each explanation and homicide trends, both overall, and considering the main sources of methodological variation that will be considered in more detail in subsequent chapters (i.e., measurement, unit of analysis, time period covered, overall versus disaggregated homicide, and short versus long-term impacts), where applicable.

Chapter 3

METHODOLOGY: SYSTEMATIC REVIEW & META-ANALYSIS

This chapter describes the methods used in this dissertation to address the two key objectives presented in Chapter 1. Specifically, meta-analytic techniques are used to quantitatively synthesize findings from this vast body of literature. Using this approach, I can address the first objective and identify which factors matter relative to the other explanations, and which do not. This approach also has the potential to reveal what may be missing from current debates because I coded and analyze all predictors and control variables, even those not emphasized in the eight explanations presented in Chapter 2. Using meta-analytic techniques, I can also address the second objective concerning how methodological variation impacts these findings and our understanding of which factors matter. Each of these will be discussed in more detail below.

Meta-analysis is a statistical tool that allows the researcher to estimate an overall average effect size for a given relationship of interest (e.g., the direction and magnitude of the relationship between incarceration trends and homicide trends), allowing me to quantitatively assess the strength of the relationship. With an explanation like incarceration, in particular, scholars often acknowledge that increases in incarceration in recent decades likely did have some impact on crime trends, but disagree as to the amount or how important it was relative to other causal factors. By performing separate meta-analyses for indicators capturing each of the prominent explanations that have surfaced in the literature, I can assess the *relative* importance of each explanation by rank-ordering the results by the absolute value of the average effect size (see also Nivette, 2011; Pratt & Cullen, 2005). Taken together, these results address the first objective.

Meta-analysis can also assess the variability of the relationship based on research design and model specification (Borenstein et al. 2009; Cooper 2010), making it an ideal tool to address the second objective, as well. That is, meta-analysis allows for a systematic assessment and identification of the impact of methodological variation on the estimated size of the relationship. One of the many advantages of meta-analysis is that methodological variation can be controlled for when calculating the average effect size estimate, allowing me to assess whether the variation in estimated average effect sizes is systematically related to research design and modeling strategies. Relatedly, this inquiry can also illuminate whether any of the explanations are robust across model specifications. In this way, meta-analysis allows for a deeper examination of the conditions under which certain factors matter, and when they don't, as well as identifying if any of the predictor domains are stable across methodological specifications.

Conducting a meta-analysis involves primary data collection from each of the studies (and models) included in the analysis. I begin by describing the methods used to systematically review and select articles for inclusion in the meta-analysis, including the selection criteria and how I identified potential articles and eventually determined whether they met the criteria for inclusion (the data and sample). After describing the methods used to identify the included articles, I discuss the dependent variable, effect sizes, which are estimates of the strength of the relationship of interest (e.g., the association between incarceration trends and homicide trends), and requires a process of standardization across studies. Effect sizes also serve as the unit of analysis in meta-analysis. Because these effect sizes are nested in models, which are also nested in studies, each study will be represented at least once and studies that present results from more than one model will contribute multiple effect sizes to the analysis. The following methodological considerations and analytic strategy section notes how I addresses this non-independence of observations and related methodological considerations. I then move on to a discussion of predictor domains, including what they are and how they differ from individual predictors, and present initial results of the established predictor domains that will be used throughout the rest of the analysis aimed at addressing Objectives 1 and 2. I conclude with a discussion of the independent and control variables which will be used to assess the impact of methodological variation (objective 2).

Data and Sample: The Included Articles

The first step in conducting a systematic review and meta-analysis is to determine a method to select articles for inclusion. The decision of where to search for articles and the inclusion and exclusion criteria is crucial to minimize bias in results (Cooper, 2010).

Selection Criteria/Scope Conditions

This meta-analysis includes aggregate, multivariate, longitudinal studies of homicide. These studies were limited to those that examined post-WWII U.S. homicide trends, were conducted at the city-level or higher, and were published between 1990 and 2016. Although a number of studies in the crime trends literature present univariate or co-integrated time series, because of the primary goal of assessing the relative importance of causal explanations, only multivariate regression-based studies will be included in this analysis. Multivariate analysis provide the best test of these causal arguments. This dissertation focuses on homicide trends, instead of crime trends more generally, because researchers often use homicide as their dependent variable due to its greater reliability, greater comparability across jurisdictions and over time, and because it is less plagued by issues of underreporting. Additionally, given these advantages over other crime types, homicide trends are often used as an indicator of crime trends, more generally (Barker, 2010; Blumstein & Wallman, 2006). A study was considered "longitudinal" if it covered a minimum of a 10 year span and used a longitudinal modeling strategy (e.g., time series analysis or pooled cross-sectional time-series analysis) or accounted for the time dimension in some way (e.g., change score model, inclusion of time trends, inclusion of time fixed effects). Studies that merely pooled data for an extended period of time, but did not account for the longitudinal nature of the data, were excluded. Given the focus on homicide *trends*, only longitudinal studies can truly assess the causes for such fluctuations. Additionally, research has established that findings are not necessarily the same in cross-sectional and longitudinal studies, and may be tapping different permanent versus temporary effects of the predictor of interest (e.g., Phillips, 2006b). The focus of the meta-analysis is on post-WWII U.S. homicide trends, notably the most prominent shifts, including homicide increases in the 1980s and the 1990s decline. I focus on this period because data, particularly official statistics, prior to 1960 are unreliable (see discussion in Cantor & Cohen, 1980). This focus is also concurrent with the vast majority of the empirical literature and most comparable with previous debates regarding the most plausible explanations for changes in crime rates over time. We saw a surge in longitudinal research beginning in the late-1990s and early 2000s when evidence of the crime drop was beginning to emerge. As such, including studies published from 1990 to present makes this a suitable time frame to capture a majority of the relevant empirical work[7]. The studies can capture time periods prior to 1990, as long as it was published in 1990 or later (e.g., a study that examines U.S. homicide trends from 1970 to 2010 would be included). Additionally, the focus on homicide trends requires aggregate studies, and therefore, individual level and multi-level[8] studies are excluded. The crime trends literature (and especially empirical studies using homicide as the barometer of crime trends, more generally) is vast, in large part due to increased scholarly attention after the 1990s crime decline. Therefore, studies conducted at smaller units of aggregation (e.g., neighborhoods) are also excluded. The decision to focus on empirical studies that address the question of homicide trends at these larger units of aggregation is for several reasons. First, as documented in earlier chapters, the national U.S. trend has exhibited marked variation since the 1960s, with smaller geographic units, such as cities, following similar trends (although at different magnitudes) until the early 2000s (McCall et al., 2011), when they began to diverge. Therefore, an understanding of contributions to such a comprehensive decline are important (see also Baumer & Wolff, 2014). Research has also established that cities follow the national trend (McDowall & Loftin, 2009). Second, an appreciation of causes of national-level variation sets the stage for future

[7]Data collection commenced in December 2016, making 2016 the last full year of published studies available at the time of data collection.

[8]My reference to multi-level studies here refers to nesting of two or more geographic units (e.g., neighborhoods nested within cities) and not to multi-level studies where time (e.g., year) serves as the lower level unit.

and more nuanced understandings of more localized variations from the national trend. For example, NYC stands out as an anomaly during the 1990s crime decline. However, crime rates declined everywhere, so the best possible explanations cannot be based in NYC alone, although something about NYC could have made it more primed to have such a larger decrease. Third, a focus on these larger units of aggregation allows for more straight-forward comparisons across studies. Finally, although much can be learned by looking to other nations, I focus on the U.S. in an effort to limit the number of explanations being examined and keep the dissertation manageable.

The focus of this dissertation and associated selection criteria, then, necessarily excludes some studies, including those that are cross-sectional, cross-national, examine a nation other than the United States, or studies in which the dependent variable is anything other than the homicide rate or count (e.g., the violent crime rate) from inclusion in the analysis.

Finding the Articles

In order to ensure a systematic approach to gathering the literature and to minimize the potential for omitting studies, which would bias the meta-analysis results, the empirical studies selected for inclusion in the meta-analysis were identified through a three-stage search strategy.

First, I conducted searches in prominent criminology, sociology, criminal justice, and economic peer-reviewed journals from 1990 through 2016[9]. Within each

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[9]The list of journals is as follows: *American Economic Review*; *Criminal Justice Review*; *Criminology*; *Homicide Studies*; *Journal of Crime and Justice*; *Journal of Criminal Justice*; *Journal of Quantitative Criminology*; *Journal of Research in Crime and Delinquency*; *Justice Quarterly*; and *Social Science Research*. If the journal was not in existence for the entire 1990-2016 time period, the available years were searched

e-journal, I went through all of the titles and pulled articles that appeared may be relevant based on the title alone. Second, I performed keyword searches in several electronic databases and online sources[10]. Relevant key terms include different combinations of the following words: "homicide trend;" "crime trend;" "American + homicide trend;" "U.S. + homicide trend;" "multivariate + homicide trend;" "longitudinal + homicide;" "homicide + pooled cross-sectional;" "homicide + change score;" "homicide + time-series;" "homicide + panel;" "temporal + crime rate;" "temporal + homicide rate." Third, I searched published meta-analyses and narrative reviews related to any of the key explanations and aggregate crime rates (Petrosino, 1995; Pratt & Cullen, 2005). Finally, although not part of the official search strategy, while reviewing the literature, I made note of key studies cited along the way and consulted the reference sections of the articles I read to identify additional articles for further review. In combination, I feel confident that this search strategy captured a majority of the *published* studies on contemporary U.S. homicide trends. One or more of these techniques is common for identifying articles for inclusion in systematic

(e.g., *Homicide Studies* began in 1997). The following additional journals were searched for a more limited time span, from 2012 to 2016: *American Journal of Criminal Justice*; *American Journal of Sociology*; *American Sociological Review*; *Law and Society Review*; *Journal of Criminal Law and Criminology*; and *Social Forces*. After searching the most recent 5 years in these journals, they were eliminated from this first step of the search strategy because they yielded too few results, and I assumed I would capture the relevant studies through one of the other two steps of the search strategy.

[10]Electronic databases and online sources searched include: Academic OneFile; Criminal Justice Abstracts; Google Scholar; National Criminal Justice Reference Service (NCJRS); Sociological Abstracts; and Web of Science Social Sciences Citation Index.

reviews (e.g., Cooper, 2010; Nivette, 2011; Ousey & Kubrin, 2018; Pratt & Cullen, 2005).

Determining Inclusion

After initially identifying potential articles for inclusion through the three step procedure, I read the abstracts (and methods sections when necessary) to ensure that articles fit the scope conditions identified previously. Studies that did not fit these scope conditions (e.g., cross-sectional; cross-national; individual level; violent crime trends) were excluded from further review. Articles that *appeared* to fit the scope conditions necessary for inclusion were selected for further review and eventual coding.

Dependent Variable: Effect Sizes

The effect size, or measure of the direction and magnitude of the relationship of interest, serves as the dependent variable in the meta-analysis (Littell et al., 2008). The effect size can be any metric as long as it is comparable across studies. Due to differences in how studies are conducted, this usually involves a process of standardization. Once standardized, I can pool the effects from all studies to calculate an average effect and distribution of effects for each "predictor domain" (discussed in more detail later in this chapter).

Standardizing Effect Sizes

Some of the most common approaches to standardizing effect size estimates across studies include the standardized mean difference, correlation coefficient, or odds ratio. The appropriate statistic depends on the nature of the data being analyzed. Because most comparisons involve the relationship between two continuous variables, and in line with previous meta-analyses of the effects of macro-predictors on aggregate crime (e.g., Nivette, 2011; Ousey & Kubrin, 2018; Pratt & Cullen, 2005), I use the standardized correlation coefficient, r. Specifically, the preferred metric is the standardized regression coefficient (i.e., beta coefficient), as opposed to the zero-order correlation.

The standardized regression coefficient was used, if provided. When it was not provided, an approximation of the standardized correlation coefficient could usually be computed with available data presented in the articles (e.g., t-statistics; unstandardized regression coefficients). For example, the below formulas were used to compute an approximation of the standardized correlation coefficient using the t-statistic (or z-statistic) associated with the unstandardized regression coefficients (Rosenthal, 1994; see also Ousey & Kubrin, 2018; Pratt et al., 2014; Yang & Lester, 2008 for a similar approach).

$$r = \frac{t}{\sqrt{t^2 + n - 2}}$$

and

$$r = \frac{z}{\sqrt{z^2 + n}}$$

where

t=the value of the t-statistic for the relationship of interest

z=the value of the z-statistic for the relationship of interest

n=sample size

In instances where t-statistics were not provided, they were estimated using the following formula:

$$t = \frac{b}{se}$$

where

t=the estimated t-statistic

b=the unstandardized coefficient for the relationship of interest

se=the standard error for the unstandardized coefficient

Fisher's r-to-z Transformation

Because *r* is constrained to values between -1 and +1, the sampling distribution is non-normal at all values other than zero, and particularly for larger values (Blalock, 1972). Therefore, most researchers transform the *r*-index into a *z(r)* score prior to combining the estimates. The *z*-score is preferable because it is unbounded and has an approximately normal distribution and the *r*-index and *z*-scores are practically identical at small *r*-values (i.e., less than 0.250). The combined *z*-score was converted back to *r* for the presentation of results (Wilson, 2001). The formula for the *r*-to-*z* transformation (i.e., Fisher's Z_r transformation) used in this process is provided below (Blalock, 1972; Pratt, 2001; Pratt et al., 2014; see also table in Cooper 2010, p. 174-175).

$$z(r) = 1.151\log[\frac{1+r}{1-r}]$$

In sum, estimates of the standardized correlation coefficient *r* were calculated from available statistics provided in each study. Each individual *r* was first converted into a *z(r)* score. The average effect size was computed by running an unconditional three-level random effects model, with the *z(r)* score as the dependent variable[11]. Although formulas were provided for each of these calculations, meta-analysis software (Comprehensive Meta-Analysis V3) and statistical software (StataIC 13)

[11]All models, including the unconditional model, also control for the sample size for the model from which the effect size was derived (grand-mean centered).

were used to calculate these values. Finally, after computing the average effect size and associated confidence intervals, estimates were transformed back to *r* prior to the presentation of results in Chapter 4.

Methodological Considerations and Analytic Strategy

Non-Independence of Observations

Empirical studies of homicide trends often include results from more than one statistical model in a single study. As such, one study may contribute multiple effect sizes to the meta-analysis. These effect sizes, then, are not independent because estimates from the same study share similar study design features. This is problematic because, just as in primary research, non-independence of observations can deflate standard errors, increasing the possibility of a Type I error.

There are several ways to account for this non-independence of observations, including choosing only one effect size estimate per study (Wilson, 2001) or averaging effect sizes for a single predictor across all models in a single study to create an overall average effect size for that predictor (Littell et al., 2008; Nivette, 2011; Wilson, 2001; Yang & Lester, 2008). These approaches are undesirable for several reasons, including because they risk a substantial loss of data and are subject to researcher bias. Alternatively, as with nested data in other analyses, an appropriate way to handle this data dependency is through the use of multi-level modeling techniques, namely fixed effects or random effects estimation.

Fixed Effects or Random Effects?

In the context of meta-analysis, fixed effects models (FEM) assume **one true effect size** exists which underlies all studies in the analysis and that any difference in observed effects across studies is due to sampling error alone. Therefore, if each study had an infinite sample size, the error would be zero and the estimated effect would be the exact same for all studies. When using FEM, the goal is to estimate this true effect. Because differences across studies are assumed to come from sampling error alone, studies with smaller sample sizes are given less weight because larger studies are assumed to have more precise estimates of the same underlying "true effect" (Borenstein et al., 2009; Littell et al., 2008).

Conversely, random effects models (REM) assume the **true effect size varies from study to study** and might be larger or smaller across samples and studies. Although the true effect size varies, it is assumed to be normally distributed and the goal of REM is to estimate the mean of the *distribution* of effects. In traditional random effects meta-analysis, the mean effect size estimate in a REM is influenced by two sources of variation: true variation in effect sizes across studies and sampling error. In the REM, weights are based on both sources of variance. Therefore, the observed effect using the REM is the overall mean effect plus the between study variation plus the sampling error. The traditional two-level REM can be extended to a three-level REM, which includes an additional source of variance – within study variation.

Analytic Strategy

Because there is significant variation in study design and modeling decisions in the homicide trends literature and because a primary focus of this dissertation is on the impact of methodological variation on the relationship between key explanations and homicide trends, I use the REM, which assumes a distribution of effects and the goal of REM is to estimate the mean of the distribution of effects.

Noting that effect sizes vary within studies as well as between them, I adopt the strategy used by Ousey and Kubrin (2018) who used a three-level REM to account for this additional source of variance. The three-level model, then, captures the variation both between and within studies as well as sampling error, and the formula used to compute the mean effect size is:

$$Y_{ij} = \beta_0 + u_{(2)ij} + u_{(3)j} + e_{ij}$$

where

Y_{ij}=the observed effect for model i in study j

β_0=the overall mean effect size

$u_{(2)ij}$=within-study variation

$u_{(3)j}$=between-study variation

e_{ij}=sampling error

Predictor Domains

The dissertation focuses on empirical assessments of the eight broad categories of explanations most commonly debated and tested in the literature on U.S. crime trends and highlighted in Chapter 2. To reduce the number of relationships estimated due to slightly different operationalizations, variables tapping similar underlying concepts were grouped together under a single "predictor domain." Predictor domains, then, as opposed to just predictors, are *groups of variables* that may have different operationalizations, but that represent the same underlying concept (e.g., the percent of individuals or families below the poverty line and infant mortality rate both are used in the literature to represent poverty/absolute deprivation). However, to be combined under the same "predictor domain," they must not only be conceptually similar, but empirically similar as well. Statistical tests were used to assess this heterogeneity and determine whether there were statistical differences between the different operationalizations (see next section). This approach helps to condense the total number of relationships being analyzed, while also minimizing any bias that may stem from combining effect sizes that may be conceptually similar, but empirically are too dissimilar to combine (Pratt & Cullen, 2005).

Assessing Heterogeneity of Predictor Domains

When pooling effect sizes, variables needed to represent a single underlying concept, but they also needed to be empirically similar enough to combine. Therefore, after grouping variables conceptually, I used statistical tests to assess whether variables with different operationalizations thought to represent a single underlying construct could be combined or whether there were significant differences between them. Even if measures were conceptually similar, I separated predictors and analyzed their relationship with homicide trends separately if statistical tests revealed that they exhibited evidence of statistical heterogeneity.

To assess whether different operationalizations could be combined, I generated a categorical variable with each of the different operationalizations coded. Each different operationalization was entered as a separate dummy variable into the unconditional three-level random effects model (with one operationalization left out as reference). If any of the operationalizations were significantly different, they were included as a separate "predictor domains." For example, this analysis revealed that single parent households and divorce/family disorganization could not be combined under the broader predictor domain of family disruption, and instead were separated into two distinct predictor domains. As another example, the analysis confirmed that both poverty and infant mortality could be combined under the predictor domain, "poverty," as there were not significant differences depending on which measure was used.

Established Predictor Domains

This dissertation focuses on the empirical assessments of variants of the eight major explanations most commonly debated and tested in the literature. The meta-analysis resulted in identification of 40 unique predictor domains and Table 3 below presents the mean effect size, 95% confidence intervals, p-value, and contributing number of estimates and studies from which these average effect sizes were derived, grouped by broad explanation. Within each broad explanation, often times several predictor domains emerged (with the exception of immigration and drug markets). In total, 28 predictor domains emerged representing all eight broad explanations as well as 12 additional predictor domains representing "other explanations" that emerged during data collection and coding.

The results presented in Table 3 were obtained from a three-level REM, which accounts for the clustering of effect sizes *within* studies as well as the variability *between* studies. The first 4 columns of Table 3 present results weighted by sample size and columns 5 and 6 present the unweighted mean effect size and p-value. Although the sample size adjusted effect sizes arguably present a more valid indication of the mean effect size, the unweighted effect sizes are reported for comparison purposes and in an effort to be as transparent as possible with the results. An examination of Table 3 reveals that results are similar whether the reader is looking at the sample size adjusted estimates or the unweighted estimates, with few notable differences in magnitude or significance when comparing the two estimates. One noteworthy exception is income inequality, which has a much larger and significant average effect size when considering the unweighted results compared to the weighted results (M_r=0.344 in the unweighted analysis compared to 0.146 in the weighted analysis).

The main purpose of Table 3 is to show the 40 predictor domains that emerged, their mean effects and distribution of effects, and frequency of empirical tests in the literature. Chapter 4 will focus on the results that address the two main objectives of this dissertation – the relative importance of the explanations and the role of methodological variation.

[Table 3 here]

I begin with a discussion of the established predictor domains. Ten predictor domains emerged representing various aspects of the economy. Unsurprisingly, these include measures tapping oft-used economic indicators in the criminological literature including: absolute deprivation (poverty and economic resources, which includes GDP, wages, income, and economic affluence measures), relative deprivation (income inequality), combined social and economic disadvantage (disadvantage/deprivation indices), public assistance (welfare and other measures of income assistance), various aspects of the labor market and labor market opportunity structures (employment, unemployment, and deindustrialization), as well as additional constructs that have not received as much empirical attention, including inflation and measures tapping more subjective aspects of the economy (consumer sentiment). It is important to note that the commonly used economic indicators of poverty, income inequality, unemployment, and income sometimes load with other economic (and social) indicators into a composite measure of economic disadvantage. Therefore, the results presented for each of these four indicators are only from models where their effects are estimated separately. If they are included in a composite index with other indicators of economic and social disadvantage, it is included in the "disadvantage" predictor domain. While it is often empirically difficult to disentangle the different effects of these economic factors in regular multivariate studies due to multicollinearity issues, this meta-analysis provides an alternate framework and statistical approach to capturing the relative importance of these different economic indicators.

Three predictor domains representing various aspects of family structure were also identified. These include measures tapping marriage and cohabitation, divorce and family disorganization, and single-parent households (including non-marital births). Although divorce and single-parent households are often used as indicators of family disruption, it is important to note that they are tapping two different aspects of family disruption as indicated by statistically significant differences between the two measures[12]. Additionally, despite the literature alluding to potentially differing effects of marriage compared to cohabitation on rates of lethal violence, and intimate partner homicide, in particular, statistical tests show cohabitation and marriage can be combined under the same predictor domain. This may be partially due to the fact that

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[12]Given the economic reality of a single income, single-parent households (often female-headed households) is also commonly used as an indicator of economic disadvantage and included in more inclusive disadvantage or deprivation indices combining indicators of both social and economic disadvantage. Single-parent households included in this predictor domain only include those instances in which this is a separate predictor in the regression equation. Instances where single-parent households was included in an index with other economic measures are counted in the disadvantage predictor domain.

the difference in the impact of marriage and cohabitation on lethal domestic violence has converged over time, and completely disappeared by 2005 (James & Daly, 2012).

Four predictor domains emerged representing demographic explanations centering on changes in age structure as contributing to changes in homicide rates over time. These predictor domains include the legalization of abortion, two measures of age composition (youth age structure and a combined adult and elderly age structure predictor domain, comprised of individuals not in the most crime-prone age group), and a measure tapping cohort-based explanations for homicide trends (relative cohort size). Only one predictor domain emerged tapping immigration, and statistical tests confirmed that composite indices of immigration (typically including a measure of percent foreign-born and percent Hispanic/Latino, sometimes with measures of linguistic isolation) could be combined with individual measures of Hispanic or Latino composition. Results also revealed that measures of immigrants entering the U.S. at any time could be combined with *recent* immigrants, despite distinctions in the empirical literature (Ousey & Kubrin, 2018; Wadsworth, 2010).

Turning to the criminal justice-related factors, three predictor domains emerged tapping various aspects of policing. The crime trends literature often discusses changes in both the quantity and quality of policing as contributing to recent changes in homicide rates over time. In line with these arguments, predictor domains capturing changes in police strength (police force size and expenditures) and more qualitative changes in policing (police strategy, which includes measures tapping increased emphasis on quality of life enforcement, community policing, and the use of Compstat), both emerged. Unfortunately, there were not enough studies that met the scope conditions for inclusion and assessed these different policing initiatives to estimate their effects separately, and they were combined under a single predictor domain, "police strategy." Additionally, a predictor domain I labeled "felony arrest" also emerged, combining indicators of the probability of arrest for serious Part I offenses, with the majority of these estimates coming from the probability of arrest for homicide. Statistical tests reveal that felony arrests (or more specifically, the probability of being arrested for engaging in a felony offense) has a significantly different impact on homicide trends than misdemeanor arrests, more commonly associated with broken windows/zero-tolerance policing initiatives which focused on more minor, "quality-of-life" initiatives, and which are included in the police strategy predictor domain.

The primary focus in Chapter 2 and in the extant literature related to changes in correctional policies and homicide trends was on incarceration. However, after collecting and coding the data, a total of four predictor domains emerged within the broad category of "corrections." These include predictor domains tapping various aspects of sentencing through punishment, including death. The four predictor domains that emerged are sentence enhancements (including legislation intended to increase the length or severity of punishment, often three strikes laws), incarceration, prison conditions (typically measured via the prison death rate), and the death penalty (including the conditional probability of receiving a death sentence, execution rate, and execution coverage). In each of these predictor domains, the included indicators represent an increasingly "get tough on crime" approach or deleterious prison conditions.

One predictor domains emerged representing drug market activity, primarily comprised of the drug sale arrest rate, drug mortality rate, and percent of homicides that were drug-related[13]. Additionally, two predictor domains tapping guns emerged. The first taps measures of gun prevalence, or the actual pervasiveness of guns in circulation in a given ecological area at a given point in time. Importantly, statistical tests confirmed that different, but often used measures to capture gun prevalence, including the percent of homicides or suicides committed with a firearm along with the percent of households with a firearm or NRA membership, can be combined under the single predictor domain of "gun prevalence." The second gun-related predictor domain taps two main aspects of gun laws (right to carry laws and gun control laws).

A number of "other" explanations also emerged when collecting and coding studies. These explanations were included to understand their relative ranking compared to the more commonly debated explanations emphasized in Chapter 2. A number of these "other explanations" have been included in longitudinal studies of homicide as control variables, but are central to the classic Chicago tradition, or other

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[13]Several authors include a "crack years" trend variable in their analyses. Often, this is included in an effort to capture the substantial spike in homicides in the late 1980s and early 1990s and in reference to Blumstein's (1995) argument, therefore tapping several measures including drug markets, gun prevalence, as well as a general time of increasing violence and homicide, particularly among young, black, males (i.e., it is a time trend capturing the "homicide epidemic"). I initially included this measure with the other drug market indicators and analyses confirmed this was an especially strong predictor of homicide trends (and quite different from the other drug market indicators used). As such, although often included in the models conceptually to capture increases in drug market activity and increasing lethality associated with the drug trade, I decided to exclude it from analyses because of questions of whether it is actually tapping aspects of the drug market as compared to "unknown" forces contributing to the spike in violence during the years the time trend captured. It is undisputed that violence rose substantially during these years, and therefore, the very strong average effect size obtained is no surprise, yet still does not shed light on whether it is drug markets, per se, that this measure if capturing rather than the general homicide epidemic during those years. Therefore, because it does not actually *explain* the homicide trend, it is excluded.

structural predictors that have become more commonplace in the homicide literature in recent years, including measures tapping population structure (population size, population density, and population structure indices), urbanicity (percent metropolitan population, metro area dummies), residential mobility (percent moved in the past 1-5 years, percent renters, percent vacant housing units), residential stability (percent living in the same house 5 years prior, percent owner-occupied housing units), racial and ethnic heterogeneity (heterogeneity indices and racial composition measures), and racial residential segregation (index of dissimilarity), as well as measures of racial and gender inequality (for example, ratio measures of employment, wages, income, education, or infant mortality rates). Additionally, another predictor domain capturing changes in routine activities, a competing theoretical perspective argued to impact homicide trends (and which originated to explain the increase in crime rates in the 1960s) (Cohen & Felson, 1979; Cohen et al., 1980), is also considered (percent civilian labor force that is female, households activity ratio, percent using public transportation). Alcohol consumption (alcohol consumption per capita, alcohol outlet density, percent of crashes with a drunk driver), the availability of domestic violence resources (including both domestic violence resources, such as shelters, and criminal justice response to domestic violence), education (educational attainment and expenditures), and military involvement (military employment rate)[14] also emerged.

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[14]Similar to the drug market trend variables, sometimes authors would include a dummy or trend variable to capture U.S. involvement in WWII. For similar reasons as not including the crack trends variable, this was also excluded from the military involvement predictor domain as there is no way to assess whether it is actually military involvement that this measure is tapping or rather a time period effect, which could be impacted by any number of “unobserved” or “unknown” characteristics. As with the crack trends variable, the effect is, as expected, quite strong.

Objective 1: Assessing the Relative Importance of Explanations

Following Pratt and Cullen (2005) and Nivette (2011), once average effect sizes were estimated for each of the “predictor domains,” they were ranked according to their *relative* effect on homicide trends. Most meta-analyses focus on the impact of one predictor of interest on the outcome. For example, meta-analysis have been conducted on the relationship between hot spots policing and crime rates (Braga, Papachristos, & Hureau, 2014), immigration and crime rates (Ousey & Kubrin, 2018); and the death penalty and crime rates (Yang & Lester, 2008). To assess the relative effects of several predictor domains on homicide trends, however, I conduct a series of meta-analyses, estimating the average effect size across each predictor domain (e.g., unemployment and homicide; youth age structure and homicide) and then rank-order them by the absolute value of their average effect sizes (Nivette, 2011; Pratt & Cullen, 2005). This allows me to address the first objective, which is concerned with the relative importance of different explanations on changes in homicide rates over time. These rank-ordered results will be presented in the next chapter.

Objective 2: Assessing the Impact of Methodological Variation

One of the many benefits of meta-analysis is that the impact of methodological variation on the effect size estimates can be statistically assessed. To do this, I coded information about the study and the model from which the effect size was derived, including information on research design and model specification. These data were used to construct independent variables and used to address the second objective – the impact of methodological variation – in both bivariate and multivariate analyses. In the meta-analysis literature, these are referred to as “moderator variables” because they are used to assess the conditioning effects of the methodological variation on the effect size estimates. Coding them and including them as independent variables serves several purposes. First, it serves to test the stability of the predictor domains across various specifications. After rank-ordering the established predictor domains, analyses were conducted for different subsamples of the total dataset (e.g., estimating average effect sizes for studies conducted at the city-level compared to county-, MSA-, state-, regional-, and national-levels). This allowed me to assess whether different research design features and model specifications changed the rank ordering of predictors. In this way, in addition to overall rank-ordering of the association between the predictor domain and homicide trends, predictor domains were also assessed in terms of their relative strength across different methodological specifications and eventually classified in terms of both their overall strength and stability across models (see also Pratt & Cullen, 2005). Second, it aids in identifying the magnitude and significance of these methodological factors on the effect size estimates. I also examine the impact of methodological variation in a multivariate context, by including each of the moderator variables as an independent variable in the three-level random effects model. Finally, coding these study features has the added benefit of helping to address the criticism of combining studies that are too dissimilar in a single meta-analysis or combining studies of better quality with less rigorous studies, sometimes referred to as the "apples and oranges" problem in the meta-analysis literature (Pratt & Cullen, 2005). Controlling for these sources of variation mitigates this concern (Pratt, 2001; Wolf, 1986).

Independent Variables: Impact of Methodological Variation

Any number of methodological decisions could impact the results. The goal here, then, is not to assess *every* source of methodological variation. This would be next to impossible. Instead, I use four sources of methodological variation, in addition to operationalization of key concepts, as examples to illustrate the impact methodological variation has on the results, and specifically, on our understanding of the relative importance of common explanations. I acknowledge that another researcher may have chosen four different sources of methodological variation to examine, and future research evaluating these other sources of methodological variation is encouraged[15]. However, I chose these four – unit of analysis, time period covered, dependent variable, and longitudinal research design – as examples because they are considerations that researchers have debated, on both conceptual and methodological grounds. The specific reasons for selecting each source of variation are provided below.

Unit of Analysis

While theoretical arguments regarding the most appropriate unit of analysis are not common in the crime trends literature, previous cross-sectional macro research, has extensively debated this issue. On one hand, scholars argue that level of aggregation may matter a great deal. For example, when estimating the effects of income inequality on homicide rates, Messner and Tardiff (1986) argue that smaller units of aggregation (e.g., neighborhoods and even cities) are preferable to larger units, such as states or nations, because comparisons between two groups are more

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[15]Other sources of methodological variation future researchers may consider include, but are not limited to, endogeneity, serial correlation, specific types of disaggregated homicide (e.g., intimate partner homicide, youth homicide), location, as well as considerations specific to certain arguments (e.g., diminishing returns for incarceration; traditional v. new destinations for immigration).

likely to be made with those in nearby proximity. Researchers also often acknowledge that because policing is a local phenomenon, tests of the effects of policing on crime should be conducted at the local level (Baumer, 2008; Baumer et al., 2012; Kelling & Bratton, 2015; Rosenfeld & Oliver, 2008), and similar arguments apply when examining the impact of drug markets or local economic conditions on crime rates (Baumer, 2008; Baumer et al., 2012; Parker, 2008). In contrast to these more localized explanations, Marvell and Moody (1998) argue that due to possible displacement and free-rider effects, the relationship between incarceration and homicide may be stronger at the national level than sub-national levels. On the other hand, scholars have argued that unit of analysis should not matter for a general theory of homicide and have demonstrated the invariance of certain structural predictors, including resource deprivation, family disruption, and population structure, on homicide rates regardless of level of aggregation (Land et al., 1990; Parker et al., 1999; McCall et al., 2010).

These arguments for the most appropriate unit of analysis, however, are largely limited to a single explanatory factor, and the most appropriate unit of analysis for assessing the multi-faceted factors impacting crime trends has yet to be established. Additionally, even for the predictors that have been well-established in the cross-sectional literature as being invariant across unit of analysis, questions regarding the applicability of these findings to longitudinal research remain, with Land and his colleagues even suggesting that researchers examine the relationship between structural predictors and homicide with longitudinal data (1990, p. 955). Individual explanations aside, there is no a priori hypothesis regarding which level is the "correct" level of aggregation for examining crime trends (Baumer, 2008; Baumer et al., 2012), or if it even matters at all. For example, Baumer (2008) notes "It is not necessarily important that a particular unit of analysis be identified, a priori, as superior for studying crime trends, for the reality is that each of these units has conceptual merit, and there are important trade-offs in the choice of unit. Nonetheless, it would be useful to know the empirical implications of using different units of analysis, something that cannot be deciphered easily from existing research" (p. 132). Furthermore, empirical tests of crime trends are conducted at all levels of aggregation, often without a strong theoretical rationale. In fact, Baumer (2008) notes differences in unit of analysis as one of the major sources of methodological variation plaguing the crime trends literature, and complicating our understanding of the factors that impact crime trends. With one exception, there have not been any systematic tests in the crime trends literature regarding the role of unit of analysis. Baumer and his colleagues (2012) considered how the most commonly touted explanations for recent crime trends fared at three levels of aggregation (city, county, and state). Out of 11 baseline explanatory variables included across all models[16], they found that only divorce and youth age structure were robust predictors across all three levels of aggregation. Additionally, no strong theoretical rationales have been offered to expect crime trends, as a whole, to differ from one level to another. That is, in the crime trends literature, questions about the most appropriate unit of analysis remain (see also Baumer et al., 2012).

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[16]Explanatory variables include: population structure, %15-29, divorce rate, immigrant concentration, resource deprivation, unemployment rate, unemployment rate (first-differenced), average wage level, lagged state incarceration rate, police force size, and drug arrest rate.

These methodological differences in empirical tests, especially without a strong conceptual foundation, make this an important consideration to examine. In this dissertation, I am able to address this gap in the literature by systematically assessing different sources of methodological variation, including unit of analysis, to see if relationships are stronger at one level than another and examine whether it contributes to the inconsistent results obtained in the literature. That is, I'm able to assess whether this is, or is not, an important consideration for future researchers to be sensitive to. Given the lack of conceptual and empirical attention to the most appropriate unit of analysis for assessing crime trends as a whole, we really don't know the role it plays or how it may be impacting our results. Oftentimes, even when conducting studies at lower levels of aggregation, the rationale for such inquiry is the national crime pattern and researchers use whatever data they may have available to them, without a strong theoretical justification for one unit over another. As such, studies of crime trends are extremely diverse regarding the unit of analysis with which they test these competing causal claims. In this dissertation I consider the role of unit of analysis for two key reasons. First, this allows me to empirically assess whether the unit of analysis has any effect on the relationship between key predictors and homicide trends, and if so, what effect. Second, given the diversity of units employed in previous research, this approach allows me to control for this particular source of variation when estimating the average effect size. To account for level of aggregation, the following units were coded: city, county, MSA, state, region, and nation. Bivariate analyses were conducted for each of the six levels. In the multivariate models, dummy variables were included for each unit of analysis, with city-level serving as the reference.

Time Period Covered

The crime drop in the 1990s received an incredible amount of scholarly attention, with many of the main arguments serving as the focus of this dissertation implicated in either the rise in homicide rates in the late 1980s or the fall in the 1990s, with some of these explanations being very period-specific (e.g., emergence and waning of drug markets; legalization of abortion). Additionally, scholars have argued that certain explanations, such as drug market activity, are better apt to explain the increase in homicides in the late 1980s and early 1990s, than they are to explain the decline (Cook & Laub, 2002; Rosenfeld, 2002; Zimring, 2007). To illustrate the importance of considering the specific time period under consideration, consider this example. Examining several sources of variation for homicide trends in large U.S. cities from 1970 to 2000, McCall and her colleagues (2008) found that the drug sales arrest rate was significantly associated with a *decrease* in homicide trends over the full time period of their analysis. Given this unexpected finding, they conducted supplemental analysis where they interacted the drug sales arrest rate with time and found that drug market activity was associated with an increase in homicide trends in 1990 and 2000, as expected. They interpret these findings to mean that while increased drug market activity was associated with the increase in homicides in the 1980s and early 1990s, it cannot explain the decline in the 1990s, which is consistent with claims by many scholars that argue it is a better explanation for the youth violence epidemic than the decline (e.g., Cook & Laub, 2002; Rosenfeld, 2002; Zimring, 2007). LaFree (1999) notes that considering whether crime trends have symmetric or asymmetric relationships with the explanatory factors is a promising direction for future research on crime trends. Cross-sectional research assumes symmetry, but longitudinal research is able to examine this empirically. LaFree (1999) notes this assumption of symmetry “rule out the possibility that some longitudinal relations are fully or partially irreversible…perhaps a severe economic depression raises crime rates permanently, even when the economy returns to its post-depression levels. Or relatedly, perhaps an economy that is first depressed and then returns to its former level results in crime rates either higher or lower than they were initially” (p. 161). However, with few exceptions, this has not been examined much in the crime trends literature (see e.g., Cohen & Land, 1987; Gould et al., 2002; LaFree & Drass, 1997; Phillips, 2006a for exceptions), despite the potential implications and insights into crime trends it can provide. In these examinations, researchers have found that relationships between some of the key explanatory factors and crime trends is not always symmetric (e.g., unemployment, wages, and age structure). That is, factors that contribute to crime increases may not be the same factors that contribute to crime decreases. Finally, Parker and her colleagues (2017) demonstrate important differences in the factors impacting homicide trends by time period (see also Roeder et al., 2015).

Given conceptual reasons to expect the time period under consideration may impact the relationship between the predictor domains and homicide trends, as well as to control for the variation in years covered in the included studies, I also consider the time period covered. A categorical time period variable was created and coded as 1 if the model only included years prior to the 1990s crime drop (last year <=1989), 2 if the model only included years capturing the crime drop and post-crime drop years (first year>=1990), and 3 if the model included years *both* leading up to and during/following the 1990s crime drop (last year>1989 and first year<1990). Although it would be preferable to use 1991/1992 as the cutoff between the pre-crime drop and crime drop periods as many scholars have noted 1991 as the peak before the subsequent decline, at least for the overall national trend (e.g., LaFree, 1998; Levitt, 2004; Parker, 2008; Zimring, 2007), this resulted in too few estimates examining the crime drop period only (n=39). As such, using 1990 as the cutoff allows me to include studies that examined, for example the change from 1990 to 2000, in the crime drop period analysis. This is preferable given that these studies are explicitly examining the factors responsible for the 1990s crime decline. In the multivariate models, dummy variables were included for studies examining the pre-crime drop only period as well as studies including both years prior to and during/after the 1990s crime drop, with studies examining the crime drop period only serving as reference. In this way, it allows me to compare how the estimated effect size differs for the various predictors in the pre-crime drop compared to the crime drop period, as well as how the average effect size changes if scholars are assessing a longer time series, including both times of homicide increase as well as times of homicide decrease in the same analysis.

Additionally, a continuous variable was computed to represent the total number of years covered. For example, if a study examined temporal trends in homicide from 1970 to 2010, it would be coded as 41 to indicate that the study period covered 41 years. More years (or time points) are generally preferable when studying crime trends for several reasons. First, it increases the sample size. Sample size is determined by N x T (unit x time), so the more time points available, the larger the sample size. Second, as Spelman (2008) points out, “The longer the time-series, the more likely we are to identify its behavior properly,” noting that, “a longer time-series will both improve precision and reduce bias.” (p. 153) (see also Barker, 2010, footnote

3). Therefore, more years are generally preferable when attempting to understand the causal factors impacting crime trends.

Dependent Variable

For a study to be included in the meta-analysis, the dependent variable had to be the homicide rate. However, there are important reasons to expect differences for the factors contributing to disaggregated homicide trends as compared to overall homicide trends. For example, previous research suggests that the effect of family structure may differ for intimate partner homicide trends (Dugan et al., 1999; Gillis, 1996; Puzone et al., 2000; Rosenfeld, 2002, 2006), the effects of deindustrialization may differ for black versus white homicide trends (Matthews et al., 2001; Parker, 2004, 2008; Shihadeh & Ousey, 1998), and the effects of guns and gun laws would impact gun homicides differently (Cook & Ludwig, 2006; LaValle, 2010). As some explanations are better suited to explain disaggregated trends, I examine factors that impact total homicide trends compared to those that impact disaggregated homicide trends. Answering these questions is important for advancing our understanding of the current "crime trends puzzle" and acknowledging that crime trends are not universal, and as such, their explanations are likely not universal either (e.g., Parker, 2008; Rosenfeld, 2004). As such, I coded whether the dependent variable in the original model was the total homicide rate or was disaggregated in some way, as well as specific information on how it was disaggregated (e.g., race-, gender-, age-, or type-specific). Unfortunately, and as can be seen in Table 4.1, there are too few effect sizes to carefully examine the factors that impact different disaggregations separately (e.g., the factors most relevant to explaining intimate partner homicide trends, black homicide trends, or gun homicide trends). Therefore, I am only able to examine the factors related to overall homicide trends compared to disaggregated trends. Specifically, a dummy variable is computed for whether the estimate was derived from a model examining the total homicide trend (with disaggregated homicide serving as the reference). Given the grouping of all of the disaggregated trends together, I note these specific findings should be interpreted with caution, and this should be explore in more depth in future research.

Longitudinal Research Design

The final methodological feature I systematically assess in this dissertation concerns the type of longitudinal analysis conducted. Specifically, I consider whether the effect size comes from an analysis estimating short-term changes in homicide rates (e.g., year-to-year, decade-to-decade, or month-to-month change) or more long-term change in the homicide time series.

When it comes to longitudinal research, depending on the statistical technique used, scholars can assess either short-term (differences) or long-term change (levels), which may lead to vastly different conclusions. Analyses conducted in levels examine the long-term relationship between the predictor and crime trends, while analyses conducted in differences examine the short-term changes (e.g., the year-to-year fluctuations in the trend), and information on the long-term trend is lost (Greenberg, 2014). In fact, Spelman (2008) notes this as an important methodological factor contributing to inconsistent findings for the role of incarceration on homicide trends. Despite some attention to this as an important conceptual issue, empirically, researchers often difference their data to make their trend stationary, with little discussion of how this impacts their research question and associated findings. Additionally, when synthesizing the literature, scholars often treat all longitudinal studies the same. That is, when considering the role of temporal design in meta-analysis, scholars often compare results from all cross-sectional studies to all longitudinal studies (e.g., Chiricos, 1987; Marvell & Moody, 1991; Ousey & Kubrin, 2018; Pratt & Cullen, 2005), regardless of whether the authors are examining short or long-term change. To capture and systematically assess these differences, a dummy variable was included for whether the analysis captured short-term change (i.e., models conducted in differences, or change models, or fixed effects models), with models analyzing long-term change (i.e., models conducted in levels) serving as the reference.

Control Variables

A number of additional study design characteristics were also controlled in the multivariate analyses. These include continuous variables for sample size, number of independent variables included (not including year and unit fixed effects), and number of years covered. I also included a continuous variable with a count of the total number of competing explanations controlled (e.g., if the study included measures capturing unemployment and youth age structure, this would be coded as 2)17. Lastly, I included a continuous variable for publication year. Publication year is included as a crude proxy for methodological sophistication. It is assumed that as years pass, scholars become more aware and attuned to various methodological considerations in trends research, or related to the specific relationship they are examining, including

[17] Initially, I computed dummy variables for whether a measure capturing each of the above competing explanations were included in the analyses. Due to small sample sizes and to preserve degrees of freedom, this variable was excluded from eventual analysis and the count variable was used instead.

increased attention and sensitivity to issues such as endogeneity or other methodological considerations that do not have enough individual effect sizes to estimate separately. Publication year, instead, is included as one way to control for these important shifts over time. All variables were grand-mean centered to allow for interpretation of the constant in the multivariate models as the average effect size across studies.

Chapter 4

RESULTS: WHAT MATTERS AND WHEN

Before turning to the results, it is worth reiterating the two main underlying objectives of this dissertation. First, this dissertation seeks to determine the relative importance of prominent explanations for homicide trends. Second, I examine how methodological variation conditions the rank-ordering and relative importance of these explanations. The results addressing both of these objectives are presented in this chapter, and the presentation of the results proceeds in stages. I begin this chapter by providing basic descriptive statistics of the studies included in the meta-analysis to contextualize the studies and findings for the reader. I then present results addressing the first objective by providing the rank-ordering based on absolute values of the mean effect sizes for each of the 40 established predictor domains introduced in Chapter 3 (see also Nivette, 2011; Pratt & Cullen, 2005 for similar approaches). Understanding the relative rank order of these common predictors can help shed light on current debates in the crime trends literature regarding which factors matter, and which do not, as contributing to fluctuations in homicide rates over time, especially in light of contradictory conclusions and disagreement in the existing literature.

Equally important, and another key objective of this dissertation, is to understand the impact of methodological variation on the outcomes obtained and how this may condition the rank-ordering of the predictor domains. As such, after presenting the rank-ordering of the overall effect size estimates for the established predictor domains, I examine how the rankings, and our understanding of which factors matter, shift based on different methodological specifications. This examination includes updated rankings for estimates derived from studies conducted at different levels of aggregation, time periods covered, type of homicide examined, and type of longitudinal research design. This allows me to not only assess the overall strength of these predictor domains, but also how stable they are across different methodological specifications (i.e., are the same predictor domains consistently strong predictors across methodological specifications or strong in some contexts, but weak in others, therefore contributing to some of the inconsistencies in findings in the homicide trends literature). Finally, as an additional and more rigorous assessment of the impact of methodological specification on the effect size estimates, I present results from multivariate analyses that take into account and simultaneously models several facets of methodological variation. I argue that methodological variation is one source of inconsistent results in the literature, and some evidence of the impact of methodological variation has been alluded to in the literature and/or there may be conceptual reasons to expect differences (see Chapter 2), and this provides two ways to assess this.

After presenting the initial rank orderings, bivariate, and multivariate results, I conclude the chapter by assessing the overall strength and stability of each of the predictor domains across all of the analyses conducted and presented in this chapter. In this way, it provides an overall summary assessment of which factors matter relative to each other, as well as information on how these conclusions are conditioned by methodological differences.

Descriptive Statistics

I begin by providing basic descriptive statistics to provide some background information and contextualize the findings for the reader (Table 4). The analyses in this dissertation are based on 5,082 effect sizes from 1,126 models from 145 peer-reviewed journal articles or book chapters published between 1990 and 2016[18]. The multiple estimates and models coming from a single study underscores the importance of conducting the meta-analysis using a three-level random effects model, which can account for variation both between and within studies. On average, the effect size estimates came from studies covering a 26 year time span, including 14 independent variables, and with a sample size of 4,353. On average, studies contributed approximately 35 estimates per study to the meta-analysis. The models from which these effect sizes were derived, on average, controlled for some indicator of 4 of the 8 broad explanations that have received the most attention in the conceptual and empirical crime trends literature. Table 4 shows that economic conditions are by far the most frequently tested or controlled category, with 90.71% of the effect sizes analyzed in this dissertation coming from models that include at least one economic indicator. This predominance is not surprising given the number of different economic indicators that dominate in the literature and the variety of separate, but related, arguments with economic roots. This is followed by estimates that come from models that include some measure of age structure (83.22%). Additionally, over half of the estimates are derived from models that control for criminal justice influences of corrections (63.79%) or policing (62.12%). Less common are models that consider the impact of family structure (33.88%) and guns (31.09%). Despite drug markets being one of the main explanations for both the increase in homicides in the 1980s and the decline in homicides in the 1990s, empirical tests are relatively rare, with only 20.82% of estimates coming from models that included a measure of drug market activity.

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[18] A full list of the 145 included studies can be found in Appendix A.

This is likely due to data limitation issues, as authors have extensively discussed the difficulties with capturing drug market activity empirically (e.g., Ousey & Lee, 2007). Finally, immigration has rarely been considered in longitudinal studies of homicide trends, with only 20.41% of estimates coming from models that explicitly test or control for immigration or Latino/Hispanic composition. The vast majority of these studies have been conducted in the past 10 years, following Sampson's 2006 *New York Times* op-ed, where he suggested that immigration may be a major contributing factor to the 1990s crime decline (Wadsworth, 2010; Barranco et al., 2018). Descriptive statistics of relevant study design features (e.g., unit of analysis, time period) are presented in the section examining the impact of methodological variation.

[Table 4 here]

Objective 1: Establishing the Relative Importance of the Explanations

Table 5 presents the rank-ordering for all 40 established predictor domains, ranked by the absolute value of their mean effect sizes, to assess the *relative* strength of the explanations. Examining the average effect sizes (Mr) presented in Table 5 reveals that the average effect size for many of the predictor domains explaining temporal trends in homicide is relatively small, with 30 of the 40 established predictor domains having average effect sizes below 0.100. In the meta-analysis literature, an average effect size of less than 0.100 is generally considered "substantively unimportant" (Pratt & Cullen, 2005, p. 399; see also Andrews & Bonta, 1998). The top 10 average effect sizes, or those with an average effect size larger than 0.100, range from 0.123 for gun prevalence to 0.278 for single parent households. According to the guidelines in the meta-analysis literature, these would be considered to be substantial effects, and thus, these 10 predictor domains would be considered substantively important factors impacting homicide trends. Therefore, studies that exclude these predictor domains may be at risk for model misspecification if they are not included. Although not above 0.100, the predictor domains of racial and gender inequality (Mr=0.098), felony arrest (Mr=-0.098), and divorce/family disorganization (Mr=0.095), are very close to the 0.100 cutoff and are statistically significant. As such, I argue that these three predictor domains may also be important for future research to include and consider. Therefore, in total, 11 predictor domains are deemed to be critical for our understanding of homicide trends based on this initial assessment[19,20]. An examination of Table 5 also reveals that with a small number of exceptions, many of the predictor domains below the 0.095 threshold not only have relatively small average effect sizes, but are also not significantly different than zero. Even these non-significant predictor domains with relatively small average effect sizes are presented in this initial table, as it is important for the reader to see how all of the established predictor domains fared, and empirical evidence suggesting which factors *do not matter* is also critical to advancing crime trends debates.

[Table 5 here]

Thus far I have presented the reader with each of the established predictor domains that emerged (Table 3), as well as a rank-ordering of those domains (Table 5). Before moving on to the results addressing Objective 2, I discuss some of the main

[19] I do not count the predictor domains of relative cohort size or income inequality here, even though they have effect sizes over 0.100, because they are not statistically significant.

[20] To be consistent throughout, future analyses and discussion will also use 0.095 (when statistically significant) instead of 0.100 as the cutoff for considering factors to be "substantively important" and worthy of future discussion.

findings. Specifically, I focus on three key takeaways: 1) the most important predictor domains, 2) how the most common and often-debated explanations fare, and 3) less debated predictor domains that emerged as important.

It's All Relative: The Top-Ranked Predictor Domains

One of the primary contributions of this dissertation is the identification of the factors that have the strongest empirical support as important predictors of homicide trends. Therefore, I begin by focusing on the predictor domains that emerged as the strongest and most important predictor domains based on this initial assessment. These include 11 predictor domains that both had an average effect size over 0.095 and were statistically significant, and thus, are considered to be "substantively important" to our understanding of homicide trends. In fact, future studies of homicide trends that fail to account for these factors may be at serious risk for model misspecification. Table 6 provides the 11 important predictor domains, rank-ordered by their relative strength. An examination of these "Top 11" factors reveals a number of interesting conclusions.

First, four of the Top 11 predictor domains are structural features central to social disorganization theory. Results reveal that single-parent households, racial heterogeneity, disadvantage, and divorce/family disorganization are all substantively important to the study of homicide trends. This is an important finding as much of the crime trends literature has been critiqued for being atheoretical. Additionally, change is fundamental to classic Chicago School criminology, yet empirical tests often do not emphasize the dynamic process. Taken together, this suggests social disorganization, or an increased emphasis on the role of informal social control, may be crucial for our understanding of crime trends (this is also in line with Sharkey's (2018) recent book, *Uneasy Peace*, in which he suggests that a major reason for the 1990s crime decline was increased civic participation by community members; see also Barker, 2010 who argues major structural and cultural changes in city life may be a driving force behind the decline).

Second, military involvement, incarceration, and felony arrest all emerged as important predictors of homicide trends and all implicate formal social control as an important factor in understanding recent crime trends. Given the findings just noted about the structural predictors and role of informal social control, it seems theoretical arguments focusing on changes in social control (as opposed to say changes in motivation, or criminal opportunities) may hold the most promise for our understanding of the most viable explanations for contemporary crime trends, and especially theoretical arguments regarding the interplay between informal and formal social control (e.g., Rose & Clear, 1998).

Third, changes in economic conditions are critical to recent homicide trends. However, it is inflation, consumer sentiment, and disadvantage that surface here, as opposed to the more often used indicators of poverty, unemployment, and income inequality. This points to the need to consider other indicators that may better capture the economy and changing economic conditions than have typically been employed in research (e.g., Parker, 2008; Rosenfeld & Fornago, 2007).

[Table 6 here]

Table 7 breaks down the factors that are most associated with homicide increases over time compared to those most related to homicide decreases. These results reveal that empirical research has found stronger support for factors associated with increases in homicide rates over time compared to predictors that may serve as protective factors and are actually associated with a decrease in homicide rates over time. In general, there is much more of a consensus as to the factors that contributed to increases in crime over time, including the 1960s crime wave, and increases in crime rates in the late 1980s, but less about what factors contributed to the 1990s crime drop. The results here suggest that may be because the empirical evidence in favor of the explanations that contribute to homicide increases is stronger. Notably, gun prevalence and divorce, both of which were implicated in recent crime booms, appear on this list. Given the importance of crime prevention efforts, understanding both the factors that contribute to homicide increases as well as decreases is imperative. Future research should continue to explore whether these relationships are symmetric (implying, for example, that an increase in inflation is associated with an increase in the homicide trend, and that a decrease in inflation would be associated with a decrease in the homicide trend) (see LaFree, 1999).

[Table 7 here]

The Usual Suspects: How the Most Often Debated Explanations Fare

Chapter 2 focused on 8 broad classes of explanations that have received the most attention in the theoretical and empirical scholarly literature (i.e., changes in the economy, shifts in family structure, changing age structure, changes to the quantity and quality of policing, increased incarceration, emergence and waning of drug markets, changes in gun availability) or have emerged as potentially important explanations in recent years (i.e., surges in immigration). Table 8 breaks down the empirical support for these oft-debated factors to assess what the weight of the empirical evidence suggests.

An examination of Table 8 reveals that most of these explanations that we have devoted increased attention to are actually not receiving strong empirical support or surfacing as important predictors of homicide trends in longitudinal multivariate tests. This is evidenced by the much longer list on the right hand side of the table. Specifically, some of the most often-debated economic indicators of unemployment, poverty, and income, changes in family structure via declines in marriage, age structure and abortion, immigration, police force size and policing strategies, drug markets, and gun laws do *not* surface as important predictors of temporal trends in homicide. Results reveal that each of these predictor domains have average effect sizes of less than 0.081. Many of them, however, are actually much lower, including poverty which has an average effect size of 0.001.

This is not to say that none of the most common explanations matter. To be sure, some of these most common explanations have surfaced as extremely important, and these can be seen on the left-hand side of the table. Results reveal that single parent households, incarceration, gun prevalence, felony arrest (an indicator of police strength), and divorce/family disorganization *are* important in our understanding of homicide trends. But, the vast majority have not, suggesting that we are spending a lot of time on factors that are not receiving strong support in the empirical literature. These factors may be important, but they may be sufficiently conditioned by methodology, and their effects masked in this initial analysis. That is, they may matter *some* of the time, but not others. This possibility will be examined in more detail in the subsequent sections (i.e., Objective 2).

One additional comment deserves note. The top-ranked predictor domain is single parent households and another indicator of changes in family structure (divorce/family disorganization) also emerged as an important predictor. While changes in family structure have been an important consideration in the homicide trends literature, the vast majority of this discussion focuses on declining domesticity and exposure reduction associated with the decline in intimate partner homicides. Therefore, while changes in family structure have been offered as one reason for temporal changes in homicide, these discussions often limit the focus to intimate partner homicides. These results, however, suggest changes in family structure are an important consideration across homicide types, which is an important finding given the different theoretical mechanisms linking changes in family structure to changes in overall as compared to intimate partner homicide. This suggests we should broaden debates regarding the role of changes in family structure to incorporate it as an important causal factor for overall homicide trends, as well.

[Table 8 here]

Adding Some Missing Pieces to the Puzzle: New Factors for Consideration

Contrary to the previous section, we can shift our attention to the predictor domains that have not received as much attention in the crime trends literature, but that have surfaced as important in this meta-analysis.

Table 9 reveals that a number of the most important predictors of homicide trends are actually not the predictor domains that have been most heavily debated in the crime trends literature. That is, several new and promising explanations emerge as important, but have not received substantial attention in the crime trends literature. In fact, four of the top 5 of the most important predictor domains have been largely neglected in the extant literature. Instead, these estimates came from 1) studies that included these predictors as control variables, but they have not often been conceptually linked to changes in homicide rates over time (e.g., racial heterogeneity), or 2) explanations that have not received a lot of attention and instead have been assessed in one-off studies so there are relatively few empirical tests (e.g., consumer sentiment). This can also be seen by looking at the sample size in the far right-hand column of Table 9. Many of these sample sizes are quite small (especially in comparison to the sample sizes of tests of the most often-debated explanations presented in Table 8), illustrating their lack of attention in the crime trends literature. One noticeable exception is racial heterogeneity, which while often included in studies of crime trends, is typically included as a control variable and has not figured prominently into recent debates. While these explanations prove promising, it is also important to note that given their lack of attention in the conceptual literature, and corresponding lack of empirical tests, the support for these explanations should still be interpreted with caution and seen as preliminary[21]. Importantly, these results suggest these as potentially fruitful explanations for further inquiry and rigorous empirical tests. Overall, these results suggest moving beyond the same repetitive list of explanations, and looking towards new and promising directions.

[Table 9 here]

Although very broadly "economic conditions" is a common explanation for homicide trends, as discussed in Chapter 2, this is largely in reference to a select few economic indicators of unemployment, poverty, wages, and inequality. In comparison, the economic conditions predictor domains of inflation and consumer sentiment,

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[21]Although relatively rare, a minimum of 3 different studies had to assess a certain explanation for it to even be included in the meta-analysis, providing more credence to these results than if results came from a single study. For example, potentially promising explanations such as mental health institutionalization and decommodification were both excluded because less than three different studies that fit the scope conditions identified previously independently assessed their effects.

however, have received much less attention in the crime trends literature. Results suggest these economic indicators deserve increased empirical attention when examining homicide trends. Other factors that have been largely neglected, yet results reveal are important, include military involvement, racial heterogeneity, and racial and gender inequality.

Overall, a primary implication of these findings is that we are spending too much time on factors that do not matter and not enough time on other explanations that may actually be important for understanding homicide trends. In addressing the first objective and establishing the relative importance of explanations for homicide trends, I have pointed to what matters, what doesn't, and what's missing. Keeping these initial findings in mind, and acknowledging them as preliminary, we now turn to the results addressing the second objective of this dissertation research – a consideration of how methodological variation impacts these findings and conclusions. In this next set of analyses, I explore in more depth the stability of these findings across methodological specifications, including when and how results may shift. These findings, then, can also shed light on whether methodological variation can account for some of the inconsistencies we've observed in the extant literature.

Objective 2: Impact of Methodological Variation

The analysis in this section focuses on the impact of methodological variation on the overall mean effect size estimates (see also Lipsey, 1992; Pratt & Cullen, 2000, 2005 for a similar approach). This analysis serves two purposes. First, it allows for a consideration of the robustness of the results just presented. Second, and as argued in previous chapters, methodological variation may help explain some of the inconsistent results and conclusions drawn in the extant literature. Three approaches were used to address this second research objective.

First, updated average effect sizes and rankings were computed for different subsample analyses (i.e., bivariate analysis). I focus here on four main sources of methodological variation – unit of analysis, time period covered, dependent variable, and longitudinal research design – as examples to demonstrate the impact of methodological variation on the results. The bivariate results also illustrate which predictors are most important under various methodological specifications. The justification for each of these was presented in Chapter 3.

Second, I further examine the impact of methodological variation using multivariate models, which allow me to *simultaneously* assess the impact of a certain methodological feature while taking other study design and model specifications into account. Furthermore, the intercept in the multivariate models represents the mean effect size for a given predictor domain after taking into account the different sources of methodological variation.

Third, I conclude this chapter with an assessment of the strength and stability of the different predictor domains. This assessment takes into account how predictor domains fared across the different methodological specifications and analyses presented throughout this chapter. In this way, this is a final overall assessment, which functions as a summary and synthesis of the previous analyses, and contributes to both underlying objectives of this dissertation. Before turning to the bivariate analysis, I present descriptive statistics of relevant study design features considered in this section.

Descriptive Statistics

Table 10 presents descriptive statistics for select study design features and reveals important methodological variation across the studies. Specifically, this table includes a breakdown of the number of estimates and percentage of total estimates for the different sources of methodological variation that will be examined in more detail in this section. Descriptive statistics reveal that a majority of the estimates came from models conducted at the state-level (34.06%), followed by the city-level (23.77%), national-level (16.98%), and county-level (15.60%). A smaller handful of estimates came from models tested at the MSA-level (6.69%) and very few came from tests at the regional-level (2.89%). I also consider the time period covered. Almost all estimates included in the meta-analysis include *either* the period of increasing homicide rates in the 1980s and/or the rapid decline in homicide rates in the 1990s (98.54%). The main focus here is on whether the study is considering the time period prior to the 1990s crime drop *only*, and which also likely captures the period of rising homicide rates in the 1980s (12.36%) or whether the estimates come from a model that considers the period of decline and more recent years *only* (11.53%), or *both* critical time periods (76.11%). Approximately 63% of effect sizes come from the estimated effect of the predictor on total homicide, whereas 37% come from homicide disaggregated by some characteristic. The most frequent disaggregation is homicide type, with 16.69% of the effect sizes representing the relationship between the given predictor domain and homicide trends disaggregated by type (e.g., intimate partner homicide, gun homicide). This is followed closely, and almost tied with, homicide trends disaggregated by race (16.19%). A small percent of included estimates were derived from models where the homicide rate is disaggregated by gender (7.16%) or by age (5.82%). Given the small number of effect sizes for the different homicide disaggregations (and even moreso when you break these down into more meaningful categories such as what factors impact nonwhite, intimate partner, or youth homicide trends), analyses focus on the difference between overall and disaggregated trends. Descriptive statistics also reveal that a majority of estimates came from models that analyzed short-term change (67.79%). The impact of these study and model design features on the relative importance of explanations is further explored in the bivariate and multivariate results presented below.

[Table 10 here]

Bivariate Analysis[22]

Tables 11 through 14 present the results illustrating how the four types of methodological variation condition the relative importance of predictor domains established in the overall results presented above. As with the earlier analysis, all analyses here are conducted using the Fisher's z estimated effect size, but converted back to *r* for the presentation of results. All estimates also represent the sample size-adjusted estimates. Given the wealth of information and focus on the most important factors, only the predictor domains with average effect sizes at or above the 0.095

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[22]Due to the smaller samples sizes of the subgroup analyses presented in this section, some of the estimated average effect sizes are derived from a 2-level random effects model instead of the 3-level random effects model used above. In almost all of these instances the estimates come from a single study so reporting results from the 2-level instead of the 3-level random effects model is not problematic because there is no between study variability that needs to be modeled. When results are based on the 2-level REM instead of the 3-level REM, relevant footnotes are included in the following tables.

cutoff that were also statistically significant will be reported and discussed in this section[23].

Unit of Analysis

Because there has been much debate about the role of level of aggregation in the macro homicide literature and because appropriate attention to unit of analysis is largely lacking from the crime trends literature, and possibly an important source of inconsistent results, the following section examines the relative importance of each of the established predictor domains for six different levels of aggregation – city, county, MSA, state, region, and nation. Table 11 presents the substantively important predictor domains (i.e., those with a statistically significant average effect size over 0.095) for each level of aggregation.

Examining Table 11 reveals substantial differences by unit of analysis, including the finding that the predictor domains that surface as most important differs across levels of aggregation. Another important difference is the number of predictor domains that emerge as important for each of the unit categories. For example, the only important predictor domain to explain county-level homicide trends is felony arrest, as compared to 11 predictor domains that surface as important to explain national homicide trends. Oftentimes debates and discussions focus on the national-level trends and it is often acknowledged that cities and subnational units follow a similar pattern (at least until the early 2000s) (e.g., McDowall & Loftin, 2009). In fact,

[23] The full tables, with average effect sizes and updated rankings, for all 40 predictor domains for each of the subsample analyses can be found in the Appendix (Appendices B-E).

the national trend is often used as a justification for a number of studies focusing on temporal variation in crime. The predictor domains that are important here, then, likely present the most plausible answers for factors contributing to national crime patterns observed. Unsurprisingly, many of the most common explanations appear as important here, including economic conditions (income inequality and inflation), family structure (single parent households), age structure (youth age structure), policing (police strategy and felony arrest), corrections (incarceration), and guns (gun prevalence). Two notably missing explanations are immigration and drug markets, both of which have been implicated in national crime patterns. However, an inspection of these predictor domains reveals that neither of these explanations have been tested at the national level, suggesting areas for future research to explore (for an exception see Barranco et al. (2018) who examined immigration at the national level but which was published after data collection was complete and after the cutoff point to be considered for inclusion in the meta-analysis). Also present are less commonly offered explanations of shifts in alcohol consumption, routine activities, and military involvement.

Examining the substantively important predictor domains at the city-level reveals a number of structural predictors as important predictors of city-level homicide trends. These include: economic deprivation (via both income inequality and disadvantage), family disruption (via divorce), racial heterogeneity, urbanicity, and racial and gender inequality. Gun prevalence is also positively related to city-level homicide trends. These results largely confirm what has been well-established in the cross-sectional literature regarding the impact of structural features, and these results confirm their importance in explaining homicide trends, too, at least at the city-level.

Many of the factors that are important at the city-level are different than the factors important at the national-level and may provide some guidance as to why cities deviate from the national crime pattern (that is, why some cities' crime rates are increasing, when national homicide rates are going down or why some cities are increasing at an exponentially faster rate). For several of the common explanations discussed in Chapter 2, results were found to be contingent on pre-existing structural factors (e.g., age structure, drug markets, and immigration). These results prove promising by suggesting that factors that contribute to the national crime trend may interact with the factors that are deemed important at a more local level (e.g., the important predictor domains in the city-level analysis) to impact localized crime trends, and why some cities today are experiencing homicide increases, while others are experiencing homicide declines.

Turning to the county-level analysis, surprisingly only one predictor domain had an average effect size that reached the level of substantive importance. This, however, is not for a lack of predictor domains tested at the county-level. Indeed, 29 out of the 40 established predictor domains were tested at the county-level. The predictor domain with the second highest average effect size at the city level was well under the 0.095 cutoff (incarceration, M*r*=-0.070; p=0.04).

[Table 11 here]

Time Period Covered

Table 12 reveals the most important predictor domains for two critical time periods in recent U.S. history – the pre crime drop period (i.e., studies that did not include any years after 1989) and the crime drop/post crime drop period (i.e., studies that did not include any years prior to 1990). The third set of results presented in Table

12 is for studies that examine years both prior to and during/after the 1990s crime drop. These results suggest important differences by time period. In the pre-crime drop period, gun prevalence is the top ranked predictor, and also has a very large average effect size (M_r=0.636). However, gun prevalence did not emerge as important in either the crime drop or the pre and post-crime drop analyses, suggesting that, as other scholars have suggested, while the rise in gun prevalence was an important causal factor in the spike in homicides in the late 1980s and early 1990s, it cannot explain the 1990s decline (e.g., Rosenfeld, 2002; Zimring, 2007). This finding is also in line with descriptive statistics of the homicide trends which reveal that most of the increase in homicides in the 1980s were for homicides involving handguns, but that homicides declined across the board, including non-gun homicides, in the 1990s. During the pre-crime drop period, results also reveal that relative cohort size and single parent households are also both especially strong predictors of homicide increases, providing support for arguments linking changes in age structure (particularly growth in the baby boomer generation) and declining family legitimacy to pre-crime drop homicide increases.

Considering the crime drop/post crime drop period, results point to disadvantage, alcohol consumption, and racial heterogeneity as top explanatory factors. The importance of disadvantage and racial heterogeneity, along with racial residential segregation, underscores the role of changes in broader structural changes throughout the U.S. on the 1990s crime decline. Although these predictor domains surface as most important here, they have not often been advocated as major contributing factors (see Barker, 2010; Parker, 2008; Sharkey, 2018 for notable exceptions). We do see criminal justice factors, particularly rising incarceration and increased use of capital punishment, also important here, but comparatively speaking their impact is not as strong. Finally, declines in alcohol consumption have not received much attention as a potentially important causal factor (see Parker & Cartmill, 1998; Parker et al., 2011 for exceptions), but results here strongly implicate changes in alcohol consumption as contributing to the decline. Notably, the effect sizes in the crime drop period analysis are much lower than in the pre-crime drop period (Mr=0.171 for the top ranked predictor domain in the crime drop period analysis as compared to Mr=0.636 in the pre crime drop period analysis), illustrating much stronger empirical support for factors driving the pre-crime drop increase in homicides than the decline.

Examining the top ranked predictor domains for the pre and post crime drop periods separately reveals that with only one exception (disadvantage) none of the other predictor domains deemed to be "important" are important during both time periods. However, most of the predictor domains that surface as important in either the pre or post crime drop periods alone also surfaced as important as important in studies considering both the pre crime drop and post crime drop periods in the same model (i.e., single parent households, relative cohort size, racial heterogeneity, incarceration, and disadvantage)[24]. This is important and illustrates that the factors that impact homicide trends, with the exception of disadvantage, which is a stable predictor across time periods, are period-specific. Additionally, the relative importance of these explanations can be masked when considering the longer time period. These findings

[24]Consumer sentiment and military involvement were not tested during just the pre crime drop or just the crime drop periods, making them ineligible to be ranked as an important predictor in multiple time periods considered.

suggest the need to capture these different period effects empirically (e.g., by including a pre-crime drop or crime drop dummy) as well as illustrate how researchers may come to completely different conclusions depending on the time period they are studying.

[Table 12 here]

Dependent Variable

Table 13 presents the top-ranked and important predictor domains to explain overall homicide trends as well as the top-ranked predictor domains to explain disaggregated homicide trends. While I provide the disaggregated trends here for the reader to see, I suggest that readers interpret these specific results with caution as the disaggregated trends category is ironically an aggregation of all of the different disaggregations (e.g., black, white, male, female, youth, intimate partner, gun, and so forth homicide trends) aggregated together in one group. Therefore, when interpreting these results the focus is not so much on the factors that impact disaggregated trends (as there are theoretical reasons to expect differences). *Instead*, I provide these results to show the predictor domains that emerged that have a consistently strong effect on homicide trends, whether they are disaggregated or not, and admittedly, there is more similarity in predictors than initially expected. A number of overlapping predictors emerge – single parent households, consumer sentiment, inflation, military involvement, and disadvantage – suggesting that these are stable predictors of homicide trends across type. Again, this is not to say that these five predictor domains do the best at explaining a certain type of disaggregated trend (e.g., youth homicide trend), but rather suggests these as important predictors to consider regardless of the type of homicide being examined.

One additional finding deserves note. Racial and gender inequality is the top-ranked predictor domain for overall homicide trends, even though it did not surface in the disaggregated trends subsample analysis, illustrating inequality along racial and gender lines has far reaching implications beyond the specific groups it immediately impacts, and is an important consideration for overall homicide trends, as well.

[Table 13 here]

Longitudinal Research Design

Table 14 presents results showing the impact of longitudinal research design on the relative importance of the established predictor domains. Specifically, I consider how the results shift when examining effect sizes that come from analyses assessing short-term compared to long-term change (e.g., analyses conducted in differences, including change models and fixed effects models, versus analyses conducted in levels). As opposed to the earlier comparisons of unit of analysis, time period covered, and dependent variable, which suggested there are a number of predictor domains that appear to be relatively robust to the certain methodological specification and surface as important predictors, for example in explaining both overall and disaggregated homicide trends, the lack of overlap in predictor domains here is striking. Specifically, only two predictor domains appear as "important" whether the researcher is assessing short or long-term change – disadvantage and racial heterogeneity. We have seen these two predictors consistently surface in the past comparisons, along with their relatively high initial overall rankings, so their importance across longitudinal research designs is not surprising.

What is notable here is that this analysis reveals that the factors that contribute to short-term fluctuations in homicide trends (e.g., the year-to-year variation) are quite different than those that impact long-term trends in homicide. This is especially important given the lack of conceptual attention to these differences in the literature (see Spelman, 2008 for a notable exception regarding the short-term versus long-term effects of incarceration). For example, incarceration does impact year-to-year fluctuations in homicide trends, but it does not have an appreciable long-term impact on homicide trends, which has important implications for long-term crime control and prevention strategies. A similar argument was made regarding the impact of wages on long-term homicide trends (Gould et al., 2002). Results here show that the economic resources predictor domain (which includes wages) does have an important impact on long-term homicide trends. In fact, this is the one of only two analyses where economic resources surfaces as important. In contrast, three other economic predictors – income inequality, inflation, and consumer sentiment – are linked to short-term fluctuations in homicide rates. Given that, as a whole, results have illustrated the importance of economic conditions as one of the top explanations to explain homicide trends, and also because results reveal that this importance is largely based on the measure of the economy we are tapping, increased attention to short-run fluctuations versus longer term trends is an important area for future inquiry. This also holds promise for forecasting crime trends. One additional finding deserves note. Sentence enhancements surfaced as an important predictor, and is positively related to long-term homicide trends. Although sentence enhancements are argued to decrease homicide rates (through deterrence or incapacitation), these results imply that they may have a long-term criminogenic impact on homicide rates, consistent with arguments noting the long-term negative effects of tough-on-crime policy initiatives (e.g., Clear, 2007; Kovandzic et al., 2004; Liedka et al., 2006; Petersilia, 2003; Rose & Clear, 1998; Western, 2006).

[Table 14 here]

Summary of Bivariate Results

Taken together, the bivariate analysis reveals substantial variation in which factors "matter" across these different specifications. Therefore, although only four examples of methodological variation, this helps explain some of the inconsistent findings and conclusions in the literature. That is, examining the changes in rankings as well as which factors are deemed important in each of the subsample analyses makes it easy to see how scholars may have come to completely different conclusions regarding the importance of different explanations and which factors matter and which do not. Also important from these analyses, however, is that a number of predictor domains *consistently* show up as important across the different methodological specifications analyzed, suggesting these as particularly important factors for understanding homicide trends.

However, as a further, and more rigorous, test of the impact of methodological variation, I use multivariate analyses and include each of these model design features as a "moderator" variable in a three-level random effects model (see also Ousey & Kubrin, 2018; Pratt, 2001; Pratt & Maahs, 1999; Tittle, Villemez, & Smith, 1978 for similar approaches). I limit the multivariate analyses to just those predictor domains that have at least 75 estimates to ensure that the sample size is large enough to conduct multivariate analyses with several independent variables. Part of the reason for the extensive bivariate analysis was because many of the predictor domains do not contain enough estimates to meet this threshold for multivariate analysis[25]. I turn now to the multivariate analysis and results. Additionally, after further examining the impact of methodological variation in a multivariate context, a final assessment of the overall "strength" and "stability" of these predictor domains will be provided to conclude the analysis and this chapter.

Multivariate Analyses

The multivariate analyses include estimation and presentation of results for each predictor domain with a sufficient number of estimates to perform multivariate analyses. Specifically, each of these models assesses the impact of the same nine methodological features on the average effect size. In each of the multivariate models, the following independent variables are included: 1) dummy variables for the unit of analysis, including county, MSA, state, region, and nation (reference=city level), 2) a dummy variable for whether the dependent variable was the total homicide rate (reference=disaggregated homicide), 3) a dummy variable for whether the original analysis assessed short term change (reference=long-term change), and 4) two dummy variables for time period, including whether the analysis only included years prior to the 1990s crime drop, or both years prior to and during/after the crime drop

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[25]Unfortunately, there were not enough contributing effect sizes to perform multivariate analyses on the following predictor domains (sample size noted in parentheses): Inflation (40), Consumer Sentiment (15), Income Inequality (26), Deindustrialization (74), Employment (30), Marriage/Cohabitation (26), Relative Cohort Size (45), Abortion (41), Police Strategy (45), Sentencing Enhancements (42), Prison Conditions (50), Education (45), Racial and Gender Inequality (58), Residential Mobility (40), Residential Stability (36), Military Involvement (17), Alcohol Consumption (39), Routine Activities (46), Domestic Violence Resources (35), and Racial Residential Segregation (39).

(reference=crime drop period only). Continuous control variables were also included capturing 1) sample size, 2) number of independent variables, 3) total number of years covered, 4) publication year, and 5) number of competing explanations controlled for in the original analysis (as identified in the eight key explanations originally identified in Chapter 2). All variables were grand-mean centered so that the intercept represents the average effect size once methodological variation is taken into account. In interpreting the results in the multivariate models, it is important to note that the coefficients are for the Fisher's z values, and have not been converted back to *r* as in previous analyses.

Economic Conditions

Table 15 presents the multivariate results for the five economic conditions predictor domains with enough effect sizes to run multivariate analysis. For the economic conditions predictor domains, after controlling for multiple sources of methodological variation, as we observed in previous analyses, disadvantage remains significantly related to homicide trends. The multivariate analysis also confirms that the other four economic conditions predictor domains – economic resources, poverty, unemployment, and welfare are not associated. The relative unimportance of these four predictor domains is not surprising given how they have fared thus far in the analysis, but results here also suggest important sources of methodological variation which may be masking their impact.

Beginning with disadvantage in Model 1 of Table 15, multivariate results reveal that while disadvantage is still a significantly important predictor of homicide trends, even after controlling for several sources of methodological variation, the magnitude of this relationship is still sufficiently conditioned by research design and model specification. Results reveal that the number of independent variables, number of years covered, unit of analysis, and type of dependent variable tested all significantly impact the magnitude of the effect size. Specifically, as more independent variables are added, the effect size decreases (b=-0.0112), but the relationship is more positive the more years that are covered (b=0.0062). Additionally, studies conducted at both the county-level and state-level find much smaller effects of disadvantage on homicide trends than studies conducted at the city level (b=-0.1203 and b=-0.3284, respectively). However, studies where the dependent variable is the overall homicide rate find a slightly larger effect than studies examining the effect of disadvantage on disaggregated homicide (b=0.0764). None of the other moderator variables, including competing explanations or time period covered, have an appreciable impact on the magnitude of this relationship. Furthermore, even after simultaneously accounting for the multiple sources of methodological variation, the average effect is still large in magnitude and significant (b=0.1371). These findings support earlier findings with disadvantage surfacing as an important predictor in both the overall and many of the bivariate analyses. For example, results here reveal that time period does not significantly impact the relationship between disadvantage and homicide trends. This is in line with disadvantage surfacing as a top predictor in all three time period subsample analyses. Furthermore, disadvantage was found to be an important predictor in the city- and MSA-level analyses, but not at the county- or state-levels (it was not tested at the regional or national levels). The multivariate results presented in Table 15 show that, indeed, effect sizes are significantly smaller at the county and state levels, increasing confidence in the bivariate results. Finally, although multivariate results show that the type of dependent variable significantly impacts the effect size, the effect is small in magnitude (b=0.0764), which is in line with disadvantage surfacing as an important predictor in both the total and disaggregated homicide subsample analyses, and the finding that the average effect size was slightly larger in the overall homicide subsample compared to the disaggregated homicide trends subsample (Mr=0.173 and Mr=0.117, respectively).

Examining the results for the economic resources predictor domain in Model 2 of Table 15 reveals that the impact of economic conditions on homicide trends is highly contingent on the unit of analysis, with the effect of economic resources on homicide trends being positive and large in magnitude at the regional and national levels compared to the city level (b=0.3859 and b=0.8422, respectively). The number of years covered also significantly impacts the results, with findings indicating more of a protective effect with each additional year covered (b=-0.0124), but none of the other moderator variables have an impact. After controlling for the different sources of methodological variation, the economic resources predictor domain is not significantly related to homicide trends, consistent with previous findings.

Results in Model 3 of Table 15 show that poverty is sufficiently conditioned by methodology. Specifically, unit of analysis and time period both significantly impact the estimated size of the relationship. The impact is particularly strong for studies conducted at the MSA-level (b=0.2527) compared to the city level, and those conducted prior to the 1990s crime drop only (b=-0.1218) compared to those that examine the crime drop period only. Although the estimated average effect size for poverty and homicide trends at the MSA level was not large enough to be deemed "important," poverty was among one of the most important predictor domains in the pre-crime drop time period analysis, and this is the only time it surfaced as important.

In line with these multivariate results that show that the relationship is significantly more negative during the pre-crime drop period than the crime drop period, the average effect size was negative in the pre crime drop period bivariate results, as well. Again, these multivariate findings confirm the earlier bivariate analyses.

Model 4 in Table 4.15 presents the multivariate results for unemployment. The average effect size across studies is very small (b=-0.0312), but evidence suggests that the magnitude of this relationship is heavily conditioned by the methods used. In fact, 5 of the 11 moderator variables exert significant effects on the strength of this relationship. Specifically, the effect size is quite a bit smaller or more negative when studies are conducted at the national level (b=-0.2272), when the dependent variable is the total homicide rate (b=-0.1475), and when analyzing short-term change (b=-0.1605). Conversely, the average effect size increases with each additional independent variable included in the original model (b=0.0086) and with each additional year covered (b=0.0060). The overall small but marginally significant findings for unemployment coupled with findings of a more negative effect for short-term effects of unemployment on homicide trends compared to long-term effects is consistent with Cantor and Land's (1985) theoretical model regarding the U-C relationship. Specifically, these results support the notion that unemployment may reduce crime in the short-term by limiting the opportunities for crime to occur, and may increase crime in the long-term by increasing motivation to engage in crime. When considering each of these sources of methodological variation, it is likely that the positive and negative effects counter each other out driving the estimated average effect across studies down to zero.

Finally, results for the welfare multivariate model presented in Model 5 of Table 15 reveal that welfare is not an important predictor of homicide trends. Additionally, this conclusion does not appear to be impacted by methodological variation, as none of the moderator variables significantly impacted results. This is in line with earlier analyses, where welfare did not reach the level of "important" in any of the previous tests.

[Table 15 here]

Family Structure

Taking a closer look at the impact of methodological variation on two family structure predictor domains, Table 16 reveals that after taking the multiple sources of methodological variation into account, divorce is not a significant predictor of homicide trends, while single parent households remains a strong and significant predictor (b=0.4209). Considering divorce, the evidence suggests that the effect is much more positive when estimating the relationship on total homicides compared to disaggregated homicides (b=0.1701). This finding is in line with theoretical arguments out of Classic Chicago School criminology suggesting that rises in divorce rates should positively impact homicide trends, as compared to theoretical arguments related to exposure reduction and divorce serving as a protective factors against intimate partner homicide trends. Divorce did emerge as an important predictor in the total homicide trends subsample analysis, but did not surface in the disaggregated trends subsample analysis. Given these empirical findings coupled with theoretical arguments, it is not surprising that the average effect size after taking into account the different sources of methodological variation is null. The type of longitudinal analysis also has a statistically significant impact on the magnitude of the relationship between divorce and homicide trends, with studies examining short-term change finding a more negative effect than studies examining long-term change (b=-0.0823).

Turning to the single parent household results in Model 2 of Table 16[26], we also see here that, just as in the divorce multivariate analysis, the average effect size for the single parent households predictor domain is much more negative when considering short-term as compared to long-term change (b=-0.7083). However, the impact is even stronger on the single parent household predictor domain than the divorce predictor domain. This is the only moderator variable to significantly impact the estimated size of the relationship, which is in line with single parent households surfacing as an important predictor domain in many of the earlier analyses (but not important in the short-term change subsample analysis).

[Table 16 here]

Age Structure

Table 17 presents the multivariate models for two age structure predictor domains, and results reveal that after taking into account methodological variation, youth age structure is significantly related to homicide trends (b=0.0833), while adult/elderly age structure is not. Considering the youth age structure predictor domain, results reveal that the magnitude of the relationship between youth age structure and homicide trends is conditioned by a number of factors, although the impact of each of these is quite small. For example, publication year and state and

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[26]Although there are only 73 contributing effect sizes for the single-parent households predictor domain, I include it here because it is close to the 75 effect sizes cutoff I used and because it has been established as important in earlier models.

national units of analysis exert small impacts (b=-0.0066, b=-0.0828, and b=0.0811, respectively). Whether the study considers the overall homicide rate or disaggregated homicide has more of an effect, with studies examining the overall homicide rate finding a much larger effect size than studies examining disaggregated homicide trends (b=0.1029).

In contrast to youth age structure, the average effect size for adult and elderly age structure (i.e., those not in the most crime prone age group) appears to be more strongly influenced by methodological factors, with larger levels of aggregation, including the MSA level (b=-0.3865) and regional level (b=-0.1767), and studies assessing short-term change (b=-.2306), finding more protective effects of adult and elderly age structure on homicide trends than studies conducted at the city-level or those analyzing long-term change.

[Table 17 here]

Immigration

The results presented in Table 18 reveal that immigration is not significantly related to temporal trends in homicide, consistent with earlier findings. In fact, immigration did not rise to the level of "important" in any of the previous analyses, and multivariate results suggest that this lack of a relationship is not being masked by methodological variation. That is, the relationship is not sufficiently conditioned by methodology, as only one of the moderator variables reached statistical significance. Although only studies conducted at the state-level had a significant impact on the size of the relationship, this effect is quite large in magnitude (b=0.1373).

[Table 18 here]

Policing

The felony arrest model in Table 19 (Model 1) reveals that the effect of felony arrest on homicide trends is relatively large, but is highly contingent on the time period studied. That is, studies considering years prior to the 1990s crime drop find a much more negative effect of felony arrest on homicide trends (b=-0.2912). Although the impact is not as large, studies examining the relationship between felony arrest and homicide trends also find a more negative effect when examining the overall homicide trend compared to disaggregated homicides (b=-0.0943), but studies published more recently find smaller effects (b=0.0011). These findings suggest that while felony arrest may be an important predictor of homicide trends, this is impacted by the type of dependent variable and the time period examined.

The multivariate results in Table 19 also confirm previous findings of a negligible role of police force size and expenditures on homicide trends (Model 2). That is, after taking into account the different sources of methodological variation, police size and expenditures are significantly related to homicide trends, but the average effect size is quite small. Additionally, although both the number of years covered and studies examining the overall homicide trend compared to disaggregated homicide trends both significantly impact the effect size (b=0.0023 and b=-0.0390), these effects are small in magnitude. This is consistent with earlier analyses, with police force size and expenditures not surfacing as important in the initial or any of the bivariate analyses.

[Table 19 here]

Corrections

In Table 20, Model 1 shows that, after controlling for several sources of methodological variation, the death penalty is not related to temporal trends in homicide. Results also suggest that this is not due to methodological differences, as only number of years (b=0.0046) and studies analyzing short-term change (b=-0.0909) significantly impact the effect size, yet the effects are quite small.

Model 2 shows the results for the incarceration predictor domain. Here, we see that, as with previous analyses, incarceration is an important predictor of homicide trends, even after controlling for several sources of methodological variation (b=-0.1380). Model 2 also shows that this inverse relationship between incarceration and homicide trends is even stronger when examined at the national-level compared to the city-level (b=-0.1889), consistent with earlier findings and also arguments that some of the effects of incarceration are masked at lower levels of aggregation (Marvell & Moody, 1998). Additionally, the effect of incarceration is more negative when considering total homicide trends compared to disaggregated homicides (b=-0.1017). Despite arguments about diminishing returns and incarceration, results reveal there are no significant differences between studies examining the period prior to or during the 1990s crime drop.

[Table 20 here]

Drug Markets

Examining Table 21, results show that after taking into account several sources of methodological variation, drug markets are not related to homicide trends. Furthermore, with one exception (publication year), this is not impacted by different methodological factors. This null finding is consistent with early findings, where drug markets did not surface as an important predictor domain in any of the earlier analyses. Although this is speculative given that the coefficients for the county and MSA levels are not significant, and the coefficient for the state level dummy is only marginally significant, results imply that the relationship between drug markets and homicide trends may be stronger at larger levels of aggregation, with the impact of each successive unit becoming larger, when compared to the city level. This is in contrast to arguments that the effects of drug markets are likely to be stronger at the city level, given it is a more local phenomenon.

[Table 21 here]

Guns

The multivariate results for the two gun-related predictor domains are both presented in Table 22. Model 1 reveals that changes in gun laws are not significantly related to temporal trends in homicide, and methodological variation does not have any significant impact on the results obtained. Again, this is consistent with earlier results, where gun laws did not surface as important in any of the previous models.

The relationship between gun prevalence is more susceptible to methodological influences, but results do confirm previous findings that gun prevalence is significantly and positively related to homicide trends. Even after controlling for methodological variation, the average effect size is quite large (b=0.1406). The relationship is much stronger when considering the pre-crime drop only period (b=0.6528), in line with gun prevalence being the top ranked predictor domain, with the largest effect size, in the pre-crime drop period bivariate analysis. The average effect size is also significantly larger at the national-level compared to the city-level (b=0.4012).

[Table 22 here]

"Other" Explanations

Finally, multivariate results for the "other" explanations that emerged during data collection and coding are presented in Table 23. Given that these were less common explanations, only population structure, urbanicity, and racial heterogeneity had enough contributing effect sizes for multivariate analyses. Although they were often included in empirical tests, they were primarily included as control variables, and have not figured prominently into debates as key explanations of contemporary homicide trends. These results largely confirm earlier analysis that changes in population structure and urbanicity are unrelated to homicide trends, but that racial heterogeneity plays an important role. Given the lack of empirical support presented here and in earlier analyses for population structure and urbanicity, and also because they have not figured prominently into debates in the extant literature, the discussion here focuses on the results for the racial heterogeneity predictor domain.

Even after controlling for several sources of methodological variation, racial heterogeneity is still positively associated with homicide trends (b=0.1032), and this effect size increases with each additional year covered (b=0.0047). However, it declines with each additional competing explanation controlled for (b=-0.0444). Results from the three-level random effects model also reveal that the relationship between racial heterogeneity and homicide trends is heavily influenced by unit of analysis, although not in a clear pattern. With the exception of the county-level, the relationship is more negative at larger levels of aggregation, including the state level (b=-0.1494) and regional level (b=-0.2789) (and the national level although results at the national level are not significant and also not as large). At smaller levels of aggregation, there is no significant difference between studies conducted at the county and city level, but studies conducted at the MSA-level finding a more positive effect (b=0.2700), compared to studies conducted at the city-level. Taken together, these results suggest that the relationship between racial heterogeneity and homicide trends is contingent on the unit of analysis and several study design features (number of years covered and number of competing explanations controlled for), however, the effect is still significant and large in magnitude, even taking these differences into account.

[Table 23 here]

Summary of Multivariate Results

The multivariate results were informative in several ways. First, they provided the average effect size for each of the relationships of interest, *even after controlling for methodological variation across and within studies*. As stated in each of the above sections, the results obtained from the multivariate models largely echo earlier results in terms of the factors that matter the most. That is, even after controlling for the different sources of methodological variation, disadvantage (b=0.1371), single parent households (b=0.4209), felony arrest (-0.1287), incarceration (b=-0.1380), gun prevalence (b=0.1406), and racial heterogeneity (b=0.1032) *still matter*, and have significant average effect sizes above the 0.095 cutoff. Results also confirm earlier conclusions that economic resources, poverty, unemployment, welfare, adult/elderly age structure, immigration, police force size and expenditures, death penalty, drug markets, gun laws, population structure, and urbanicity are unrelated to homicide trends, either because the average effect size is not significantly different from zero once simultaneously taking into account the different sources of methodological variation or because the effect size, even if significant, is relatively small (i.e., the largest significant intercept here is for police force size and expenditures at b=-0.0350). Results also confirm earlier conclusions that youth age structure is positively associated with homicide trends, but the result is not quite large enough to be deemed "important" (b=0.0833). One notable difference from earlier analyses is for the relationship between divorce/family disorganization and homicide trends. While initially divorce/family disorganization was deemed to be a strong predictor of homicide trends (although only marginally), ranked 13 in the initial analysis with an overall average effect size of 0.095, bivariate analyses began to suggest methodological variation may play an important role on the strength of this relationship. The multivariate results presented here confirm that after taking into account the different sources of methodological variation, divorce/family disorganization is *not* related to homicide trends.

Second, the results from the multivariate analysis also showed the specific sources of methodological variation for each predictor domain. For example, the effect of gun prevalence on homicide trends is significantly larger when testing the relationship at the national level or when considering the pre-crime drop period only. For the interested reader, Appendix F provides a summary table of the impact of methodological variation on the different predictor domains, illustrating the predictor domains that are the most impacted by methodological variation, and those that are the least impacted.

Third, these results also point to the sources of methodological variation that are most likely to impact results across the board, and which are least likely to have a significant impact. This is important because it provides more guidance to scholars as to what sources of methodological variation they should be most sensitive to, and which have little impact. Table 24 shows a breakdown of the count and percent of times a moderator variable significantly impacted the relationship. Here, we see that unit of analysis is an important consideration, impacting results anywhere between 10% of the time at the county-level to 38% of the time at the regional-level. The number of years covered, type of dependent variable (i.e., total or disaggregated) and type of longitudinal analysis (i.e., short-term or long-term change) are also important considerations, impacting results between 30 and 40% of the time. Surprisingly, in only 4 instances was there a significant difference between studies conducted during the pre-crime drop time period and those that only considered the crime drop years and there was never any difference between those that considered the crime drop years only from those that considered the 1990s crime decline in a longer time series.

[Table 24 here]

Strength and Stability of Predictor Domains

While the above bivariate and multivariate analyses are informative, it is important to take a step back and assess the impact of methodological variation *overall* on the obtained results. Which predictor domains are relatively strong *and* stable across different methodological specifications, thus establishing themselves as robust predictors of homicide trends? Which are consistently weak, and therefore, unimportant to the study of homicide trends? Which appear to be sufficiently conditioned by methodology, sometimes emerging as important predictors and other times not? Table 25 provides a summary of the overall strength and stability classifications that will be discussed below. In this way, the assessment of the overall strength and stability of these predictor domains contributes to both of the underlying objectives of this dissertation.

Strength

Using a similar strategy as Pratt and Cullen (2005), the overall strength of the predictor domain was established based on an assessment of the strength across the overall, each of the subsample analyses, and the multivariate analyses (when applicable). Specifically, for each analysis (e.g., overall, different units, time periods, dependent variable, type of longitudinal analysis, and multivariate models), I calculated an average effect size across each of the predictor domains (based on the absolute value of fishers z). Predictor domains were considered to be "high," "moderate," or "low" depending on where the absolute value of its average effect size fell compared to the overall pooled average effect size for all the predictor domains tested under that particular specification. Predictor domains that were more than two standard errors above the mean were classified as "high" for that particular model. Predictor domains that fell more than two standard errors below the mean were classified as "low." Predictor domains that had average effect sizes that fell between these two values were classified as "moderate." This same procedure was repeated for the overall meta-analysis, each of the subsample analyses, and the multivariate analyses. Predictor domains were classified based on where a majority of assessments fell[27]. For example, for the predictor domain of "consumer sentiment," the average effect size was at least two standard errors above the mean effect size in 71.4% of the models run, it was two standard errors below the mean effect size in 0% of the models run, and it fell somewhere in the middle in the remaining 28.6% of the models. As

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[27] Summary analysis available upon request.

such, the consumer sentiment predictor domain was ranked as “high” strength for the summary analysis[28].

Based on this criteria, ten of the 40 predictor domains were classified as “high,” 11 as “moderate,” and the remaining 19 predictor domains were classified as “low” in terms of their strength. Among the high strength predictor domains are single parent households, relative cohort size, inflation, consumer sentiment, military involvement, income inequality, racial heterogeneity, disadvantage, incarceration, and gun prevalence. Each of these top tier predictor domains had a significant overall average effect size greater than 0.100 in the initial analysis (with the exception of relative cohort size and income inequality which were both above 0.100, but not significant). The lowest overall average effect size of these high strength predictor domains was 0.123 (gun prevalence), well above the 0.100 cutoff for identifying predictor domains as “important” for our understanding of homicide trends. Additionally, each of these predictor domains deemed to be “strong” in relative strength had a mean effect size across all methodological specifications greater than 0.100, with the lowest being 0.117 for the disadvantage predictor domain.

The moderate strength predictor domains include the following 11 predictor domains: racial and gender inequality, felony arrest, divorce/family disorganization, youth age structure, alcohol consumption, death penalty, police strategy, routine activities, sentence enhancements, residential mobility, and racial residential

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[28] One predictor domain had a tie between two categories. Gun prevalence fell in the “high” category in 7 tests and in the “moderate” category in 7 tests (and in the “low” category in 0 tests). Given that it fell in the high category in both the overall analysis, and the multivariate analysis, its overall strength was classified as “high.”

segregation. For each of these, the average overall effect sizes for the predictor domains ranged substantially in terms of the size of the effect, from 0.018 for racial residential segregation to 0.098 for racial and gender inequality. For the most part, these predictor domains also fell in the "middle tier" of the initial overall analysis, and subsequent analyses confirmed those findings. That is, they are important predictors of homicide trends and/or surface as important in some contexts, but *other factors matter more*.

Finally, approximately half of the predictor domains are classified as low strength, and include abortion, marriage/cohabitation, police size and expenditures, economic resources, domestic violence resources, immigration, deindustrialization, welfare, drug markets, unemployment, gun laws, residential stability, education, employment, adult/elderly age structure, urbanicity, prison conditions, population structure, and poverty. In comparison to the "moderate" strength predictor domains, this category is more heavily dominated by predictor domains that had initially small average effect sizes in the overall analysis (e.g., all of the predictor domains in this category had an overall average effect size below |0.043|), with consistently smaller average effect sizes across the different methodological specifications.

Stability

In addition to the strength classifications of high, moderate, and low, I also assessed the stability of the 40 predictor domains to see whether the predictor domains that were considered to be "strong" were consistently strong, whether the weak predictor domains were consistently weak, or whether these assessments were sufficiently conditioned by methodological variation. Stability classifications were determined by examining the percentage of times a given predictor domain was classified as “important” or “unimportant” in the preceding analyses. Again, a predictor domain was considered to be “important” if the effect size estimate was greater than or equal to 0.095 and statistically significant[29].

Predictor domains that fell in one category (either important or unimportant) between 50 and 66% of the time were classified as being “low stability” because they were deemed to be highly inconsistent, important about half of the time and unimportant the other half. Predictor domains that fell into one category or another between 84 and 100% of the time they were tested were classified as “high stability” because they were consistently found to be either important or unimportant. Predictor domains that fell in one category or another between 67 and 83% of the time were classified as “moderate stability.” A predictor domain could only be classified as either “important” or “unimportant,” therefore these specific percentage breakdowns were derived by dividing 50 (or one half of the distribution) into thirds.

The vast majority of predictor domains were classified as “high” stability (n=25). These include the following predictor domains: consumer sentiment, military involvement, disadvantage, death penalty, police strategy, abortion, marriage/cohabitation, police size and expenditures, economic resources, domestic violence resources, sentencing enhancements, immigration, deindustrialization, residential mobility, welfare, drug markets, unemployment, gun laws, residential stability, education, employment, adult/elderly age structure, prison conditions,

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[29]Appendix G provides the count of the number of times a given predictor domain was considered to be important, the total number of times it was tested, and an overall percentage of how often it was deemed important out of the number of times it was tested. This table also includes summary information on when each of the predictor domains was considered important.

population structure, and poverty. However, what's important to note about these is that while 25 of the 40 predictor domains were classified as "high stability," only 3 of these were considered to be consistently *important* predictors of homicide trends. Therefore, the remaining predictor domains were *consistently unimportant* predictors. In fact, 13 of the predictor domains did not rise to the level of "important" in a single empirical assessment.

The moderate stability predictor domains (n=10) include the following: relative cohort size, inflation, racial heterogeneity, incarceration, divorce/family disorganization, youth age structure, alcohol consumption, routine activities, racial residential segregation, and urbanicity. These include predictor domains that fell into either the important or unimportant category between 67 and 83% of the time. These are the predictor domains that, more often than not, are found to be either important or unimportant, but they exhibit enough instability to fall in this in-between category. The predictor domain of divorce/family disorganization appears here and is the only predictor domain to surface as important in both directions. That is, when divorce did reach the level of substantive importance, it was more often than not positively related to homicide trends. However, it was also found to be an important predictor domain *negatively* related to homicide trends. This is likely due to competing theoretical mechanisms linking divorce to homicide (specifically, intimate partner homicide).

Finally, the low stability predictor domains exhibited the least consistency across the different methodological specifications, being considered important about just as often as they are deemed to be unimportant predictors of homicide trends. Five predictor domains were deemed to be "low stability" and include single parent households, income inequality, gun prevalence, racial and gender inequality, and felony arrest. Therefore, the relative importance of these five predictor domains is sufficiently conditioned on methodological specification. Several of the most contentiously debated explanations, including gun prevalence, and an indicator of police strength (felony arrest) are found in this category, illustrating why scholars may come to completely different conclusions about their role. Given that these "inconsistent" predictor domains are seen as being important approximately half the time, suggests that more research needs to be done to examine the contextual nature of these findings.

[Table 25 here]

In sum, the analyses presented in this chapter have been informative in highlighting the predictor domains most strongly associated with homicide trends, as well as providing empirical evidence of which of these predictors are *consistently* important, which matter *some of the time*, and which are consistently *unimportant*. These findings, including how they relate back to the two main objectives, as well as their implications for research, policy, and theory, along with the notable limitations of this work, will be addressed in in more detail in the following chapter.

Chapter 5

DISCUSSION & CONCLUSION: PUTTING THE PIECES TOGETHER

This dissertation sought to "take stock" of the state of empirical support surrounding common explanations for U.S. homicide trends. With the 1990s crime decline, scholars increasingly turned to change models and other longitudinal models to seek answers to the unanticipated, but welcomed, decline. In fact, LaFree (1999) even argued that part of the reason the 1990s crime decline caught us so off guard was because much of the research at the time was cross-sectional and longitudinal studies were relatively rare. However, shortly after academics began to acknowledge that the decline was a real and sustained decline (as opposed to a mere year or two fluctuation or anomaly), a flurry of books and review articles focusing on evaluating some of the most common explanations emerged, as well as books offering new theoretical arguments to explain the observed trends (e.g., Baumer et al., 2018; Blumstein & Wallman, 2006; Conklin, 2003; LaFree, 1999; Levitt, 2004; Goldberger & Rosenfeld, 2008; Parker, 2008; Sharkey, 2018; Rosenfeld, 2018; Zimring, 2007), and special issues in journals devoted specifically to understanding crime trends (e.g., *Journal of Criminal Law and Criminology* in 1998, *Justice Quarterly* in 2014, and *Journal of Quantitative Criminology* in 2016).

With this large and growing body of literature, a number of plausible explanations surfaced to explain temporal trends in crime. But, the methodological approaches employed in the crime trends literature are vast, and inconsistencies abound, impeding our ability to come to a single conclusion. As illustrated in the narrative review of the literature offered in Chapter 2, while there have been several attempts to assess our state of knowledge through narrative reviews, scholars have come to different, and sometimes polar conclusions as to what factors “matter,” which do not, and which matter *most*. However, these past narrative syntheses were limited in several ways, including an inability to simultaneously and statistically assess the strength of the relationship for various competing explanations, thus establishing their relative importance, as well as inability to assess exactly how and in what ways methodological variation impacted results.

This dissertation aimed to make sense of this “crime trends puzzle,” by employing a different methodological approach than past syntheses. Specifically, I used meta-analytic techniques to offer a comprehensive and systematic statistical assessment of the entirety of the homicide trends literature. Statistical syntheses of macro predictors of crime rates, more generally, are rare, with Pratt and Cullen’s (2005) meta-analysis providing one of the only examples to date. Their findings have been consequential in informing subsequent macro-level criminological research. However, despite its impact, their analysis included primarily cross-sectional studies and, given its earlier publication date, does not include any studies that were published after 1999. Almost 20 years of empirical research has accumulated since, with an increased focus on temporal trends in crime rates. Therefore, this dissertation research explicitly focused on longitudinal studies of aggregate homicide rates, including almost two more decades of research. As a result, the dissertation covered an extended time span which is critical for advancing our understanding of contemporary crime trends. Meta-analyses offer a number of advantages over traditional narrative reviews and syntheses of the literature, including the ability to statistically estimate the magnitude of a given effect, as well as examine the impact of methodological variation, both of which were crucial to achieving the goals in this dissertation.

The overall aim of this dissertation was to bring more clarity to our understanding of the factors that impact temporal trends in homicide over time, and specifically to address two interrelated objectives. First, I sought to establish the relative importance of the most common explanations for homicide trends. Second, given the methodological diversity in the crime trends literature, and in an effort to address previous inconsistencies based on narrative reviews, emphasis was placed on the impact of methodological variation on which factors matter. Specifically, this dissertation assessed the empirical status of eight broad explanations most commonly linked to crime rate changes and fluctuations over time, including changes in structural conditions (economic conditions and family structure), demographics (age structure and immigration) and crime and criminal justice related factors (policing, corrections, drug markets, and guns). In addition to these "common explanations," by assessing the entirety of the empirical literature on U.S. homicide trends, I was also able to identify and assess a number of less prominent explanations, that results suggest hold promise as important causal factors.

As a result of this research and the findings presented in the previous chapters, we not only know what the strongest and most stable predictors of homicide trends are, but we also now know that empirical support for a number of the most "common" explanations for fluctuations in homicide trends is actually quite weak (e.g., changes in drug markets, unemployment, poverty, gun laws, police force size, and policing strategies). Conversely, the statistical synthesis of the literature revealed that a number of factors are empirically strong predictors of homicide trends, yet these have been largely missing from discussions thus far (e.g., consumer sentiment, military involvement, racial heterogeneity). In addition to establishing these strong and weak predictors of homicide trends, this dissertation research reveals that, with a few notable exceptions, for the majority of factors argued to impact temporal variation in homicide, the level of empirical support is conditioned on methodological variation. This has two important and interrelated implications. First, it illuminates the conditions under which certain factors matter. Second, the fact that different predictor domains surfaced as important depending on different methodological specifications may partially explain the inconsistencies and why scholars have come to such different conclusions. That is, results revealed important differences across levels of aggregation, time period, dependent variable, and longitudinal research design, with different predictor domains surfacing as most important. The findings from this analysis illustrate that the decisions that we make as researchers can have a profound impact on our findings and the resulting conclusions and point to specific sources of methodological variation that scholars need to be sensitive to. For example, multivariate results revealed that 36% of the time a study was conducted at the national level, there was a statistically significant different between the estimate effect size of the magnitude of the relationship compared to studies conducted at the city-level. These and other implications of the findings will be addressed in more detail in the following sections.

Finally, while this dissertation assessed the relative strength and stability of 40 different "predictor domains," the focus was inherently on the empirical relationship between variables often employed in the scholarly literature. While not an initial goal of this dissertation, the patterns in the results that emerged provided an ideal opportunity to move beyond this "variable-centered" approach that pervades the broader crime trends literature (see argument by Baumer et al., 2018; see also Roth, 2009). The crime trends literature has been critiqued for being largely atheoretical (see e.g., Barker, 2010; Baumer et al., 2018; Parker, 2008; Roth, 2009). As such, in this chapter I also take a step back to consider whether the most important predictor domains in combination may group together under a more cohesive theoretical or conceptual framework.

As a result of the findings presented in Chapter 4, we now know much more about which predictors and explanations matter, and when. I begin by focusing on the empirical status of individual predictor domains before turning my attention to the broader explanations and how they fare. I conclude with the research, policy, and theoretical implications of these findings and limitations of this dissertation work.

Bringing it All Together: The Main Findings

The results from this dissertation have provided empirical evidence as to what predictors matter, how common predictors hold up against empirical scrutiny, and what predictors we may have been missing. It also reveals how methodological variation impacts these results. That is, we also know what factors are consistently important, which are consistently unimportant, and which are sufficiently conditioned by methodology. And these results suggest that at least some of the inconsistency in earlier findings and syntheses can likely be attributed to methodological variation.

The Important, Common, and Missing Predictors

Upon completion of all analyses in Chapter 4, I computed an average effect size across all models estimated, including the initial overall assessment, the bivariate analyses, and the multivariate analyses. Based on this updated average effect size, 13 predictor domains emerged as “important,” with effect sizes above the 0.095 cutoff. In large part, these are the same predictor domains that were deemed to be "important" in the initial overall analysis, with a few notable exceptions. First, based on their consistently strong effects throughout subsequent models, and high average effect size across all models, both income inequality and relative cohort size have both been added. Both of these predictor domains also had large mean effect sizes in the initial analysis, but the results were not statistically significant, removing them from the initial list of top-ranked predictor domains. Because this updated assessment is based on the average effect size across the different models, information on whether it is significant or not is not available. As such, I include these two here given their consistent and significant findings across the other models. I will leave it to the readers to decide for themselves the weight to place on these two predictor domains, specifically. Second, divorce/family disorganization was important in earlier analysis, but has subsequently dropped from this list and been replaced by alcohol consumption. Although individual predictor domains have shifted somewhat in their updated rankings, results across models confirm these 13 predictor domains as substantively important for our study of homicide trends. Adding confidence to these results, when the sample size was large enough to allow for multivariate tests, the multivariate analyses also confirmed these as important predictors, and significantly related to homicide trends, even after simultaneously accounting for several sources of methodological variation.

These 13 most important predictor domains, their updated rankings based on the average effect size across all models, as well as a designation of their prominence in the crime trends literature (both in terms of the broader explanation they represent as well as in terms of the specific predictor being assessed) are noted in Table 26. In this way, the information contained in summary Table 26 illustrates some of the most important key takeaways from this dissertation – the most important predictors and whether these are commonly debated in the literature or have been largely missing. Classification of explanations and predictor domains as “common” or “missing” stems from the narrative review presented in Chapter 2. Additionally, the prevalence of the specific predictor domains can be seen in the total number of contributing effect sizes and studies. Many of the most “common” explanations contributed at least, and oftentimes well over, 100 effect size estimates, whereas there were far fewer contributing effect sizes from explanations that are largely “missing” from recent crime trends debates. One notable exception is racial heterogeneity, which although tested frequently, it was almost exclusively included as a control variable in the original models. It is clear from this table that while several of the most heavily debated factors for influencing homicide trends did surface as important across models (i.e., income inequality, relative cohort size, gun prevalence, felony arrest, incarceration, and disadvantage), a number of new and understudied predictor domains also emerged as important (i.e., racial heterogeneity, military involvement, racial and gender inequality, and alcohol consumption). Additionally, although broader explanations tied to some of these most important predictor domains have received considerable attention in the scholarly literature (i.e., family structure and economic conditions), the specific indicators that surfaced as important have not received as much attention (i.e., single parent households, consumer sentiment, and inflation). Therefore, 7 of the 13 most important *predictor domains* have been largely missing from discussions and debates about factors most related to homicide rate fluctuations over time. This has important implications for future research on crime trends, in terms of suggesting new and promising directions to pursue, as well as highlighting the factors most important to our understanding of homicide trends. The implications of these findings will be discussed in more detail in subsequent sections.

[Table 26 here]

The Impact of Methodological Variation

Table 27 presents an overall summary of the empirical status of the 40 predictor domains analyzed in this dissertation. Specifically, it notes the predictor domains that are consistently strong, middle of the road, consistently weak, and inconsistent across the different models estimated. Seven predictor domains emerged as “consistently strong,” meaning they were classified as high in strength and either high or moderate in stability. These include consumer sentiment, military involvement, disadvantage, relative cohort size, inflation, racial heterogeneity, and incarceration. Additionally, all seven of these also fall into the 13 top-ranked predictor domains presented in the previous section. Given their consistent support across methodological specifications, these results support increased attention to these factors in future scholarly work. Importantly, four of these seven have not received much attention in the crime trends literature – consumer sentiment, military involvement, inflation, and racial heterogeneity.

Approximately one-fourth of the predictor domains fell into a “middle of the road” category. These include predictor domains that were classified as moderate in strength and either moderate or high on stability. Several of the predictor domains in this category are common arguments, and empirical research supports the attention they’ve been given in the literature, as the results here do suggest that these factors matter. However, they do not have as strong of a relationship with homicide trends as the factors in the consistently strong category. Predictor domains in this category are consistent with findings in earlier reviews, where scholars note that these factors likely played *some* role, but they are not as important as other factors (especially youth age structure).

About half of the predictor domains analyzed were found to be consistently weak predictors of homicide trends. These include predictor domains that were classified as low in strength and either high or moderate in stability. One caveat about this category is that a number of these predictors, though deemed "consistently weak" in this analysis, are argued to impact specific types of disaggregated homicides moreso than what results would reveal here (e.g., marriage/cohabitation is theoretically expected to impact intimate partner homicide trends, deindustrialization is theoretically expected to impact black homicide trends, and gun legislation is expected to impact gun homicide trends). Therefore, while the results here show these predictors as consistently weak, more research is needed to see how they fare when examining the specific types of homicide they are argued to impact. Other predictor domains in this category, however, do not have theoretical reasons to believe they should impact different homicide disaggregations differently (e.g., police force size and expenditures), indicating that they are, in fact, consistently weak predictors of homicide trends, as results imply.

The final category is for the inconsistent predictor domains. These are predictor domains that ranked in any category for strength (i.e., high, moderate, or low), but that were classified as "low" in stability, meaning that their relationship with homicide trends is inconsistent across models and conditioned by methodological variation. Five predictor domains were deemed "inconsistent" and include single parent households, income inequality, gun prevalence, felony arrest, and racial and gender inequality. These predictor domains tap some of the most hotly debated explanations in the crime trends literature, including income inequality (especially in relation to whether relative deprivation or absolute deprivation matters more), felony arrest (an indicator of police strength), and gun prevalence. Additionally, despite being the top ranked predictor domain in the initial analysis, subsequent analysis has revealed that the impact of single parent households on homicide trends is highly inconsistent across methodological specifications, only surfacing as important about half the time. Finally, racial and gender inequality has not received much attention in the crime trends literature, but results here reveal that there are inconsistencies across studies. This may be because although the initial analysis revealed that racial and gender inequality could be combined under one predictor domain, they may need to be separate into two separate domains – racial inequality and gender inequality. Unfortunately, there were not enough estimates of each to examine them separately in this dissertation.

[Table 27 here]

What Does It All Mean?: Assessing the Empirical Status of Each of the Main Explanations

The section that follows includes an overall summary and assessment of the eight broad explanations for homicide trends, as well as the new and potentially viable explanations that emerged, but are less common in crime trends debates. To aid in the discussion, the appendix includes summary tables (Appendices H-P) that provide summaries of the empirical results for each of the predictor domains within a certain explanation empirically tested to explain homicide trends. Specifically, these tables contains the overall rank, the average *r* across specifications, and the overall assessment of the strength and stability for each of the predictor domains under the broader category. In this way, it provides a summary of all of the analyses presented in this dissertation in one place, allowing for a more holistic discussion of each of the main explanations and their empirical support.

Economic Conditions

The findings in this dissertation revealed that the different economic indicators either had a strong association with homicide trends (i.e., inflation, consumer sentiment, income inequality, and disadvantage) or their impact on homicide trends was quite small and negligible (i.e., economic resources, deindustrialization, welfare, unemployment, employment, poverty). An examination of the four predictor domains with average effect sizes above 0.100 reveals that in addition to having average effect sizes that are large in magnitude, these findings were statistically significant across most models and results were consistently in the same direction (i.e., a positive association between income inequality, inflation, disadvantage and homicide trends and a negative association between consumer sentiment and homicide trends). In addition to their relatively high average effect sizes and strength rankings, both consumer sentiment and disadvantage were deemed to be both high in strength and high in stability, while inflation was considered high in strength and moderate in stability, and income inequality, although a strong predictor, was highly inconsistent across models. Conversely, across most methodological specifications (and the overall model), economic predictor domains with average effect sizes smaller than 0.100 were statistically insignificant a majority of the time. Additionally, these were all assessed as low in overall strength and high in stability (i.e., economic resources, deindustrialization, welfare, employment, and poverty), indicating they are consistently weak predictors of homicide trends.

Taken together, the empirical evidence suggests that at least certain economic conditions are important predictors of homicide trends, although this is highly contingent upon the specific concept being considered. These results illustrate the gulf between those economic predictor domains argued to be important for temporal variation in homicide from those that are not (i.e., there are no “middle” ranked economic conditions predictor domains – they either matter or they don’t). Especially important to note is that the often used economic indicators of unemployment and poverty are not strong predictors of homicide trends. Results also confirm previous cross-sectional analyses of the importance of disadvantage, and demonstrate that not only is disadvantage a strong and stable predictor of spatial variation in homicide rates (e.g., Land et al., 1990; McCall et al., 2010; Pratt & Cullen, 2005), but is crucial to temporal trends, as well, with disadvantage consistently ranking among one of the most important factors impacting homicide trends, regardless of methodological specification. Furthermore, this strong and stable finding coupled with the importance of racial heterogeneity and the lack of importance of commonly tested predictors of unemployment and poverty, suggests that it is the concentrated nature of disadvantage that is most harmful and impactful on homicide trends.

These findings also imply that while economic conditions matter, we need to broaden our scope of conditions that we consider. As Parker (2008) notes, a majority of the literature linking economic conditions to crime trends focuses on traditional measures of poverty, inequality, and unemployment to the exclusion of other economic indicators. She argues that typical economic measures are ill-suited to capture the changing nature of the economy that may influence crime trends, and the 1990s crime drop, in particular. The results here support these arguments, and suggest we consider alternate economic indicators not used as often in the crime trends literature, with inflation and consumer sentiment emerging as particularly promising. Although these two measures are limited in that they are not available at a more local level, they hold promise as both measures are able to be forecast into the future, possibly providing some guiding evidence to criminologists about future crime patterns. Additionally, one of biggest mysteries is why sometimes during times of economic growth, crime rates rise, yet in other times of economic growth, crime rates fall. Results suggest that relative perceptions of the economy and economic well-being, as well as how you are doing compared to your neighbor may be more important considerations for our understanding of homicide trends.

Family Structure

Based on the results presented in this dissertation, we can conclude that changes in family structure do have some impact on homicide trends, but this is limited to changes in family structure that tap into “family disruption” as opposed to changes in family structure related to “declining domesticity.” Additionally, the final results and conclusions regarding the role of family disruption on homicide trends are not as strong as they were at the outset of this meta-analysis. Overall, results suggest that single parent households play a much larger role than divorce/family disorganization in impacting homicide trends. However, despite the overall large mean effect size, high overall ranking, and high strength classification, the relative importance of single parent households on homicide trends is heavily influenced by methodological specification, as indicated by its low stability classification.

Specifically, it was only found to be an "important" predictor in 8 of 13 models, illustrating that the magnitude of the relationship is highly inconsistent across methodological specifications.

In comparison, divorce/family disorganization has a smaller average effect size and it was ranked much lower. It was also classified as moderate in terms of both strength and stability. Taken together, these findings underscore the importance of considering these two aspects of family structure – and specifically family disruption – separately, and has important implications for theoretical arguments regarding the type of family disruption most salient under various conditions. The overall high assessment of single parent households is also consistent with the strong and stable assessment of disadvantage above. The disadvantage predictor domain included different index operationalizations that combined measures of economic *and* social disadvantage into a single composite measure. Oftentimes single parent households is included in this composite measure. Scholars have made efforts to identify the relative importance of the different component measures to homicide, more broadly (e.g., Stansfield & Parker, 2013), but given multicollinearity issues in traditional regression-based studies, it is hard to make this assessment. This meta-analysis, however, is able to contribute to these debates by simultaneously assessing the individual composite measures as well as the combined composite index to understand their relative importance.

Age Structure

With the exception of relative cohort size (and youth age structure to a degree), the other age structure predictor domains are relatively unimportant when considering their impact on homicide trends. Relative cohort size, however, ranks high on strength and moderate on stability, has a large average effect size across specifications, is ranked high overall, and the updated ranking across specifications is high. The average and overall effect sizes and rankings for the other three age structure predictor domains – youth age structure, abortion, and adult and elderly age structure – are much lower, and all under the 0.095 criteria used to establish importance. Youth age structure is close, and may therefore be interpreted as somewhat important, but empirical syntheses suggest that both abortion and adult and elderly age structure play little role in homicide trends. These results point to the influence of cohort size on homicide trends moreso that the effects of age composition, and is an important finding given the scholarly literature tends to focus more on age composition arguments.

Immigration

Only one predictor domain emerged to capture changes in immigration on temporal trends in homicide. Despite claims about the role of recent surges in immigration as a major causal factor for the 1990s crime drop (Sampson, 2006, 2008), empirical evidence does not support the argument of immigration as an important contributor to homicide trends, either in driving the decline or as contributing to homicide increases, despite rhetoric suggesting the latter. In fact, the immigration predictor domain ranks low in strength and high in stability, meaning from the empirical research on the immigration-homicide trends literature, which is notably smaller than most of the other major explanations, immigration is a consistently weak predictor of homicide trends. Further examination of the multivariate results confirmed these bivariate findings. That is, they confirmed immigration was not related to temporal trends in homicide, after controlling for a number of sources of methodological variation, and that only one of the moderator variables significantly impacted the relationship.

Policing

The meta-analysis revealed that, overall, policing factors play a relatively minor role in temporal changes in homicide rates, with some impact of felony arrest, but fairly negligible effects for the other two policing predictor domains – police strategy and police size and expenditures. For example, although felony arrest often surfaced as an important predictor, results revealed that it was one of the most inconsistent predictor domains across all of the analyses. In the initial assessment, the overall M_r for felony arrest just barely reached the 0.095 threshold for determining substantive importance used in this analysis, but it was included anyway. However, the average M_r across the different models is much higher, -0.128. These findings and the overall assessment of felony arrest as moderate in strength and low in stability illustrate that the probability of arrest for a serious felony crime is an important factor contributing to temporal trends in homicide and that its impact varies considerably by methodological specification. Other policing strategies, including zero-tolerance and quality of life policing initiatives (as measured primarily via misdemeanor arrests) do not play a critical role, and the impact of police size and expenditures is also negligible.

Corrections

The analyses in this dissertation revealed that incarceration is an important contributor to changes in homicide rates over time, and it is by far the strongest of the corrections predictor domains. The incarceration predictor domain was classified as high in strength and moderate in stability, illustrating that while it is an important factor impacting homicide trends, the relationship is stronger under certain methodological specifications, including national-level analyses. Additionally, although incarceration is classified as "high" in strength, it is one of the lowest ranking of these "important" factors. This is in line with many previous conclusions regarding the impact of incarceration (e.g., Barker, 2010; Levitt, 2004; Rosenfeld, 2006; Spelman, 2006), that while incarceration likely matters, other factors matter *more*. These dissertation results also revealed that the other three correctional predictor domains – death penalty, sentencing enhancements, and prison conditions – do not exert much of an effect, if any, on homicide trends.

Drug Markets

Despite arguments to the contrary (most notably, Blumstein, 1995), the results here do not support a strong connection between changing drug markets and homicide trends. In fact, in a majority of the analyses, results show a negative (but nonsignificant) relationship between changing drug markets and homicide trends, and not once does the drug market predictor domain surface as important. This may be because it is so hard to capture changing drug market activity empirically and the indicators often used (drug sales arrest rates) are actually measures of drug market enforcement instead of drug market activity (see Ousey & Lee, 2007 for a discussion). This may also be because of the time periods captured in the empirical studies. Many arguments regarding the changing nature of drug markets focuses on the emergence of crack cocaine markets on the increase in youth violence in the late 1980s, but there were not enough effect sizes to assess just this "pre-crime drop" period alone. Instead, all of the studies examined time periods that either captured only the crime drop period or *both* years prior to and during/after the crime drop. Some scholars have suggested that the emergence and waning of crack cocaine markets can better explain the increase in homicides in the 1980s than the decrease in the 1990s (e.g., Cook & Laub, 2002; Rosenfeld, 2002; Zimring, 2007), which may also explain the lack of finding here. Given that gun prevalence did surface as the most important predictor domain in the pre-crime drop subsample analysis, this is a possibility.

Despite these speculations, the results obtained in this dissertation all point to the fact that changing drug markets is a relatively and consistently weak predictor of homicide trends, ranking 30th out of 40 predictor domains in the initial analysis and dropping to 33rd out of 40 in the updated analysis based on an average of effect sizes across all of the analyses. Multivariate findings also confirmed the non-relationship between drug markets and homicide trends and revealed that few of the moderator variables impact this relationship.

Guns

Changes in gun availability were empirically captured and assessed via two predictor domains, gun prevalence and gun laws. The analyses presented in this dissertation revealed that, on the whole, gun prevalence is related to homicide trends, but that the strength of this relationship varied substantially across methodological specifications. Because of this, the gun prevalence predictor domain was ranked high in strength and low in stability. It was initially considered an “important” predictor of homicide trends, well above the 0.095 cutoff. However, in several of the subsequent analyses, the effect was quite a bit larger, and this was confirmed in the multivariate analyses, as well. Specifically, results revealed that while gun prevalence is an important predictor domain with a strong positive association with homicide trends, this relationship is even stronger when examining the relationship at the national level or when only considering years prior to the 1990s crime drop. This shift in the strength of the relationship across models brought its ranking up from being the 10th most important predictor in the initial analysis to being the 6th ranked predictor domain when considering the average effect across all models.

In contrast to gun prevalence, gun laws consistently did not have an impact on homicide trends, and this finding persisted throughout every single model. That is, in not one model did gun laws rise to the level of important. The relative importance of gun prevalence coupled with the lack of empirical support for gun laws may be because the gun control laws are not effective in decreasing the actual prevalence of firearms or are removing the firearms least likely to be used in the commission of homicides to begin with. For example, Brady laws and additional background check requirements have been critiqued on the basis that firearms are often (and can easily be) obtained illegally. Evidence also suggests that the firearms that are sold back in gun buy-back programs are sold back by older individuals and are not the guns being used in homicides anyway (Wintemute 2006).

"Other" Explanations

In addition to the "common" explanations, 12 predictor domains emerged representing "other" explanations during data collection and coding. While most of these were included in studies as control variables, not previously linked to changes in homicide rates, a number of them surfaced as important predictors and worthy of increased attention in the crime trends literature. Most notably, military involvement, racial heterogeneity, racial and gender inequality, and alcohol consumption should all be considered in future crime trends debates. The other 8 predictor domains in this section are relatively weak predictors of homicide trends, and this is fairly consistent across models, therefore the discussion here will be limited to the four predictor domains that advance our understanding of homicide trends.

Unfortunately, there were not enough contributing estimates to examine the impact of methodological variation on the predictor domains of military involvement, racial and gender inequality, and alcohol consumption, however, bivariate analyses revealed that with the exception of racial and gender inequality, analyses fairly consistently found these other three factors to be important predictors of homicide trends. Bivariate analyses also pointed to racial heterogeneity being an important and consistently strong predictor of homicide trends, yet multivariate analyses revealed that the relationship between racial heterogeneity and homicide trends is substantially impacted by methodological variation.

In sum, results reveal broadly that economic conditions and family structure matter. Additionally, incarceration and gun prevalence matter, although some of the legislation associated with increasing incarceration (i.e., sentencing enhancements) or limiting gun prevalence (i.e., gun laws) are ineffective at reducing homicide. The analyses also point to the need to look outside of the typical box of "common explanations" for other possible causes, and the results suggest military involvement, racial heterogeneity, racial and gender inequality, and alcohol consumption as promising starting points for future inquiry.

In addition to illuminating the explanations that are important and worthy of future scholarly attention in the study of homicide trends, the results here also indicate that, in general, changes in age structure (with the exception of relative cohort size), immigration, policing (with the exception of felony arrest), and drug markets are unrelated to homicide trends. These findings suggest that we shift our focus away from these explanations that have received a substantial amount of our attention, yet are not proving to be important predictors of changing homicide rates over time, and instead focus on the more promising explanations – both traditional and newly emerging explanations. Table 28 summarizes these conclusions regarding the overall empirical support for each of the eight broad explanations, as well as the new explanations that emerged.

[Table 28 here]

The Implications: Where Do We Go From Here?

Implications for Research

The results presented in this dissertation provide guidance regarding the predictors that should be included in future studies of homicide trends. Not including these factors may lead to model misspecification due to omitted variable bias. Although this is a concern that researchers studying crime trends should be attune to, the results here also point to a number of important predictors that are, unfortunately, somewhat limited in their data availability. For example, the economic predictor domains of inflation and consumer sentiment surfaced as important predictors of homicide trends. However, these measures are not available at lower levels of aggregation, such as the city level. Therefore, researchers would need to choose to conduct their analyses of homicide trends at the regional or national level or seek suitable proxies that tap similar aspects of the economy and perceptions of the economy at a more local level. These economic indicators, however, both have important implications for forecasting future crime trends because they are available on a monthly basis and can be forecast into the future, providing some indication of future homicide rate increases or decreases. In addition to illuminating the factors that *should* be included in future studies, the results from this dissertation also suggest that we limit attention to those factors that were found to be consistently weak predictors.

Results also reveal that although the homicide trends literature is largely silent as to which level of aggregation is most appropriate, this deserves more careful theoretical and empirical attention. Gathering the data needed for crime trends research is an extensive undertaking because of the additional component of needing to gather the data over multiple time points. Additionally, researchers must address and be sensitive to things like the comparability across years and changes in geographic boundaries over time. Therefore, in the absence of strong guiding theoretical or empirical reasons, a researcher's decision to analyze crime trends at one unit over another may be influenced by practical considerations, such as data availability. The decision about the unit of analysis to be used, however, should not be arbitrary, and results here demonstrate that this decision can have a profound impact on the results. Having a central data clearinghouse with data at several different levels of analysis will allow researchers to more easily address different questions regarding factors impacting crime trends at the most appropriate level for the research question (see also Baumer et al., 2012). It will also allow for comparisons across units in efforts to assess the robustness of a given explanation or uncover whether certain effects are being masked by aggregation bias or whether there are any spillover effects. Specifically, results indicate that incarceration and gun prevalence may be most appropriately tested at the national level because their effects may cross jurisdictional lines, but empirical tests comparing identically specified models at different levels of aggregation could confirm this.

Related to the most appropriate unit of analysis, future research may want to consider interactions between the top city-level predictors and top national-level predictors to empirically examine whether local structural conditions moderate the national level relationships. This may provide insight into why some cities experience larger increases or decrease than others and why homicide rates in some cities trend up, while the homicide rates in other cities trend down during the same time period under consideration.

Differences emerged in terms of the most important factors, depending on the time period under consideration. This points to two important implications for future research. First, authors should consider different periods of homicide increases and decreases separately. To not severely limit the sample size or length of the time series under consideration, the researcher could instead include interactions between dummy variables for period indicators capturing the “booms” and “busts” periods with the predictors of interest instead of conducting separate analyses by time period. Other researchers have similarly found that attention to these structural breaks in the data is critical and the factors that impact changes in homicide rates differs by time period (Parker et al., 2017). Second, given this finding, researchers should devote more attention to an examination of symmetry and asymmetry between the explanations and homicide trends. That is, increased attention to whether factors impact homicide trends during periods of crime increase and periods of crime decline are symmetric. To date, this has largely been ignored in the crime trends literature, instead assuming symmetry in relationships (LaFree, 1999).

The results also suggest that more attention needs to be devoted to the difference between factors impacting short-term fluctuations in homicide rates compared to the factors contributing to more long-term trends, with better conceptual reasons for the approaches we undertake as researchers. That is, we need to weigh the conceptual justifications and how conducting the analysis in differences as opposed to levels changes the nature of the research question versus the empirical justifications for conducting the analysis in levels or differences (e.g., differencing to achieve stationarity without regard to how that changes the nature of the question being addressed).

In addition to the results providing empirical evidence illustrating that researchers need to be sensitive to the methodological decisions they make, particularly in regard to unit of analysis, time period covered, dependent variable, and longitudinal homicide type, these results also point to a number of directions for future research to consider. Perhaps one of the biggest contributions of this work was revealing the understudied explanations that emerged as important predictors of homicide trends that have not received much, if any, conceptual attention in the crime trends literature (i.e., an identification of "what's missing" from current debates). Results revealed military involvement to be a protective factor against increases in homicide rates over time. Given the importance of incarceration, as well, researchers should consider whether military involvement specifically acts as a protective factor or whether these results more broadly indicate the role of institutionalization on homicide trends (possibly also including mental health institutionalization that did not have enough empirical tests to be included in the meta-analysis but that previous research has shown to impact homicide trends (Harcourt, 2011)). Results also revealed that racial heterogeneity is not only one of the strongest predictors of homicide trends, but also one of the most consistently strong predictors. This is an important finding, especially when coupled with the findings indicating the importance of single parent households and disadvantage. These findings together point to the need to bring social disorganization to the forefront of theoretical explanations in the crime trends literature (see theoretical implications section below). These findings, taken together, also suggest that it is the *concentration* of disadvantage that is especially salient (Wilson, 1987), and that segregation and exposure patterns play a role in our understanding of homicide trends. Relatedly, racial and gender inequality also surfaced as an important predictor, and in fact, surfaced as the number one predictor for "total" homicide trends indicating that the deleterious effects of racial and gender inequality extend beyond the groups being compared. Finally, results point to alcohol consumption as an important indicator for future consideration. Despite declines in alcohol consumption between 1980 and 2000 (Roeder et al., 2015), and connections of changes in alcohol consumption to changes in homicide rates in other nations and earlier time periods (e.g., Parker & Cartmill, 1998; Parker & McCaffree, 2012), alcohol consumption is not often considered in recent U.S. crime trends debates (for a notable exception see Parker & Cartmill, 1998; Parker et al., 2011). The first three predictor domains – military involvement, racial heterogeneity, and racial and gender inequality – were also considered "important" in the initial overall analysis and alcohol consumption emerged later as another important predictor of homicide trends that past research has not given much attention to. In addition to these four "new" explanations that emerged, new indicators of traditional explanations, including consumer sentiment to tap changing perceptions of economic conditions and inflation, may better capture changing economic dynamics impacting homicide rates. Results also point to the need to expand our thinking of the role of changing family structure, and particularly shifts in family disruption, beyond just how it impacts intimate partner homicide trends, but overall homicide trends, as well.

Another advantage of meta-analysis is that it can illuminate where the gaps are in terms of what hasn't been tested yet, or what has only received limited empirical attention, as well as point to suggestions for future research. For example, immigration has yet to be tested with national data (see Barranco et al., 2018 as an exception that was published after data collection was complete), despite arguments implicating increases in immigration in national crime trends (Sampson 2006, 2008). More tests at the county and state level also are need to identify the predictor domains most associated with homicide trends at those levels. In fact, only one predictor domain surfaced as important in the county-level analysis, indicating more research is needed to understand the factors that impact homicide trends as the county level.

Implications for Policy

By and large, the results in this dissertation suggest that with few exceptions (i.e., incarceration and felony arrest), there is little support for tough on crime policy initiatives and, in fact, the largest drivers of homicide trends are not related to traditional crime control policies. Instead, the factors driving homicide trends are largely structural. Therefore, policy initiatives aimed at reducing structural barriers and/or providing resources to ease the negative effects associated with these structural barriers is encouraged. Additionally, some contradictory findings emerged and, as with much prior research, highlight the need to be attuned to possible unintended consequences of policy initiatives. For example, single parent households emerged as the number one predictor in the overall rankings and the number two predictor in the updated rankings based on the average effect sizes across methodological specifications. But, incarceration also surfaced as an important predictor of homicide trends, often appearing as "important" across different analyses conducted. Ironically, increased incarceration, argued to reduce crime and homicide, also increases the likelihood of incarcerating fathers, and therefore, increasing the percentage of single parent households, a criminogenic factor. The interplay between such factors, including how increased incarceration may undermine informal social control efforts, continues to be an important consideration (see e.g., Clear, 2007; Clear et al., 2003; Kubrin & Weitzer, 2003; Rose & Clear, 1998).

Crucial to policy implications, gun prevalence materialized as one of the most important predictor domains, yet gun laws consistently had no impact on homicide trends. Scholars have documented potential reasons for not finding an impact of gun legislation on crime rates, including the ease with which firearms may be obtained illegally (Wintemute, 2006). The results here also demonstrate that the impact of gun prevalence is strongest at the national level, possibly suggesting spillover effects (see also arguments regarding incarceration and crime rates) and that attempts to limit gun prevalence need to be made at the national level, instead of the state or local level.

Given the empirical support (and especially lack of support) for current crime reduction policy, these results should be shared with organizations that publish similar assessments of "what works" in crime prevention and reduction strategies. One such outlet is the Campbell Collaboration, which is a non-profit that conducts systematic reviews to inform evidence-based policy on a number of topics in the social sciences, including crime and justice-related issues. In addition to the relative importance of common explanations that this dissertation examined, individual and more nuanced systematic reviews and meta-analyses focusing on specific explanations that surfaced (e.g., disadvantage and homicide trends) could also be beneficial to guide policy directives.

Implications for Theory

The crime trends literature is largely atheoretical. Some scholars have worked to integrate theory (e.g., Barker, 2010; Parker, 2008), but its role in crime trends debates is still limited. Although theory testing was not a goal of this dissertation, the results that emerged in terms of the top ranking domains across the analyses suggest possible avenues to pursue that seem particularly promising given the results. Specifically, I suggest that scholars should consider using social disorganization theory as a guiding framework, and particularly more recent extensions in the urban disadvantage literature, as well as extensions examining the relationship between formal and informal social control on crime rates.

Given that a number of the top predictors emphasize the role of social control in temporal changes in homicide rates, social disorganization theory may offer a hopeful start as a guiding framework. While this is not the only theory we can use to guide our work, it provides one possible example of integrating theory more centrally into the crime trends literature, which this body of literature needs more of. Much of the cross-sectional macro homicide literature has its roots in the Chicago School, with social disorganization theory and extensions (e.g., urban disadvantage). Explicit in social disorganization theory, however, is that ecological units are *dynamic*, thus change must be a central consideration. Although the element of change is inherent in social disorganization theory, most empirical tests are cross-sectional and this dynamic nature is not considered. The results in this dissertation, however, suggest that social disorganization theory may be a promising theoretical framework to use as a guide for future studies of crime trends. Specifically, with economic deprivation (via disadvantage and income inequality), family disruption (via single parent households and divorce/family disorganization), and racial heterogeneity all fairly consistently surfacing as top predictors, the results obtained from the preceding analyses support taking this approach. Additionally, racial and gender inequality also emerged as an important, but inconsistent predictor, suggesting increased scholarly attention to its role in recent crime trends. Scholars have also documented the interactions between informal and formal social control in this tradition. For example, Rose and Clear (1998) bring incarceration into the heart of social disorganization theory, and theorize about how formal social control may impact the effectiveness of informal social control, and provide one example of ways to integrate some of the top predictor domains that emerged in this dissertation.

Limitations

While the results obtained from this dissertation are informative, this research is not without limitations. The findings contained herein were based on an assessment of the existing empirical literature that has been published on homicide trends. This brings with it three inherent challenges. First, and arguably the biggest limitation, is that the results are only as good as the studies and estimates that went into the meta-analysis. That is, the "garbage in-garbage out" mantra applies here. While efforts were made to limit the "garbage in" by relying only on published research, which is arguably more rigorous, the inclusion of only published research presents another challenge in and of itself. Specifically, another criticism of meta-analysis is that it is sensitive to "publication bias." This is the idea that significant findings are more likely to be published than non-significant or contradictory findings, skewing the results of the meta-analysis (Borenstein et al., 2009; Card, 2012; Glass et al., 1981; Rosenthal, 1979). While this is a concern here, it is not as much of a concern as in previous meta-analyses because the focus was not just on one explanation. Instead, by collecting and coding data on all predictors in the included studies, it ensured that estimates were included for relationships of interest even if they were not the focus of the specific article (e.g., an article examining the relationship between incarceration and homicide trends likely also controlled for other explanations considered, such as unemployment, poverty, racial heterogeneity, and age structure). Descriptive statistics confirmed that, on average, studies controlled for 4 of the 8 broad explanations for temporal trends in homicide. Third, the focus in this dissertation was on homicides specifically. While the empirical research was limited to homicide for the reasons provided in earlier chapters, it would be fruitful to know how these explanations fare for other crime types, as well as how they fare in explaining homicide or crime trends in other nations.

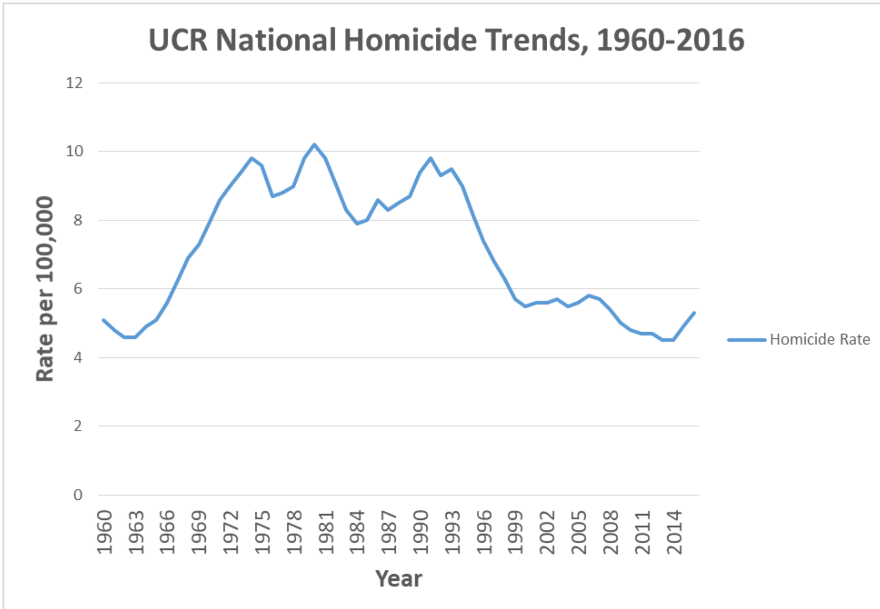
Related to the "garbage in-garbage out" limitation, this study also could not speak to the validity of the measures being assessed, only to the strength of the relationship between the variables we use and changes in homicide rates. For example, results revealed that the drug market predictor domain was unrelated to homicide trends. However, this could be more a function of how scholars have operationalized drug markets, moreso than their inability to explain temporal trends in homicide. This very concern has been voiced in the literature (see Ousey & Lee, 2007 for a discussion), and results in this dissertation can only confirm that drug market activity, the way it is commonly operationalized, is unrelated to homicide trends.

Unfortunately, because there were not enough contributing estimates, analyses could not be conducted for different homicide disaggregations. The extant literature has documented conceptual reasons to expect different effects on different types of homicide disaggregations, and empirical literature confirms this. In more recent years, scholars have started examining disaggregated homicide trends with increasing frequency. Therefore, as more studies accumulate over time, future research should consider the explanations that best explain certain types of homicide disaggregations.

Finally, certain methodological considerations specific to certain explanations require a more nuanced look than was possible here. The extant literature has suggested that some of the explanations, such as age structure, immigration, or drug markets, may be contextual. For example, research has found that the relationship between drug markets and homicide trends is contingent on pre-existing structural conditions (Ousey & Lee, 2002) and that the relationship between immigration and homicide trends is influenced by the location where immigrants settle as well (Ousey & Kubrin, 2014, 2018; Shihadeh & Barranco, 2013). In fact, it is these very explanations that previous research has suggested may be contextual that did not surface as important in the examination here, suggesting that increased attention to the nuances of these relationships may be crucial for our understanding of the role these factors play in homicide trends. As such, these are undoubtedly important empirical questions that still remain and warrant further empirical examination. Despite these limitations, this dissertation research has gone a long way in synthesizing the current homicide trends literature, brining clarity to the factors that matter, those that don't, and those that matter some of the time.

FIGURES

Figure 1 UCR Reported National Homicide Trends, 1960-2016



TABLES

Table 1 Summary of Previous Syntheses and Identification of Common Explanations[a]

Explanation	**Crime Booms & Busts**	**The Crime Drop in America**	**Why Crime Rates Fell?**	**6 Factors that Don't Matter, 4 that Do**	**Great American Crime Decline**	**Under-standing Crime Trends**	**What Caused the Crime Decline?**
	LaFree (1999)	**Blumstein & Wallman (2000)**	**Conklin (2003)**	**Levitt (2004)**	**Zimring (2007)**	**Baumer (2008)**	**Roeder et al. (2015)**
Economic Conditions	✓	✓	✗	✗	✓	✓	✓
Family Structure	✓	✓	✗	---	---	✓	---
Age Structure	✗	✗	✓	✗	✓	✓	✓
Immigration	---	---	---	---	---	---	---
Policing	✗	✗	✗	✓ (size); ✗ (strategy)	✗	✓	✓
Incarceration	✓	✓	✓	✓	✓	✓	✓
Drug Markets	✗	✓	✓	✓	✗	✗	✗
Guns	---	✓	✗	✗	---	---	✗

[a]Where scholars note the evidence is inconclusive, this is denoted with an "x" given the lack of established support for a given explanation.

Table 2 Illustration of Inconsistent Results in Homicide Trends Literature, Unemployment as an Example (n=11 studies)

Citation	Measure of Unemployment	Unit of Analysis	Time Period	Dependent Variable	Statistical Model	Results [a]	Sample Size (NxT)
City-Level Studies							
McCall, Parker, & MacDonald (2008)	% unemployed (contemporaneous)	Cities	1970-2000	Homicide rate (log)	FEM	0	332
County-Level Studies							
Phillips & Land (2012)	Unemployment rate (contemporaneous)	Counties	1978-2005	Homicide rate	FEM	-	11189
	Δ unempl rate (contemporaneous)	Counties	1978-2005	Homicide rate	FEM	0	11189
	Unemployment rate (contemporaneous)	Counties	1978-2005	Homicide rate	FEM	-	11189
	Δ unempl rate (contemporaneous)	Counties	1978-2005	Homicide rate	FEM	0	11189
MSA-Level Studies							
Stowell, Messner, McGeever, & Raffalovich (2009)	Δ % unemployed (contemporaneous)	MSAs	1994-2004	Δ homicide rate	REM (1^{st} diffs)	+	1028
	Δ % unemployed (contemporaneous)	MSAs	1994-2004	Δ homicide rate	REM (1^{st} diffs)	+	1028

	Δ % unemployed (contemporaneous)	MSAs	1994-2004	Δ homicide rate	REM (1st diffs)	+	1028
State-Level Studies							
Brandt & Kovandzic (2015)	Δ unempl rate (contemporaneous)	State (TX)	1994-2005	Homicide rate	ARIMA	0	143
Chen (2008)	Unemployment rate (contemporaneous)	States	1986-2005	Homicide rate (log)	FEM	0	999
Kovandzic, Vieraitis, & Boots (2009)	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	-	1499
	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	-[a]	1499
	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	-[a]	1499
	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	-[a]	1499
	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	0	1449
	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	0	1149
	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	-[a]	1499

	Unemployment rate (contemporaneous)	States	1977-2006	Homicide rate	FEM	-[a]	1499
Morris, Teneyck, Barnes, & Kovandzic (2014)	% unemployed (contemporaneous)	States	1990-2006	Homicide rate (log)	FEM	+	850
Phillips & Land (2012)	Unemployment rate (contemporaneous)	States	1978-2005	Homicide rate	FEM	-	1400
	Δ unempl rate (contemporaneous)	States	1978-2005	Homicide rate	FEM	0	1400
	Unemployment rate (contemporaneous)	States	1978-2005	Homicide rate	FEM	-	1400
	Δ unempl rate (contemporaneous)	States	1978-2005	Homicide rate	FEM	0	1400
Vieraitis, Kovandzic, & Marvell (2007)	% unemployed (contemporaneous)	States	1974-2002	Homicide rate	FEM	0	1225
Regional Studies							
Rosenfeld & Oliver (2008)	Δ unempl rate (contemporaneous)	Regions	1970-2006	Δ homicide rate (log)	Change score (1^{st} diffs)	-	140
Rosenfeld (2009)	Δ unempl rate (contemporaneous)	Regions	1970-2006	Δ homicide rate	FEM (1^{st} diffs)	-[a]	136
	Δ unempl rate (contemporaneous)	Regions	1970-2006	Δ homicide rate	FEM (1^{st} diffs)	-	136
	Δ unempl rate (contemporaneous)	Regions	1970-2006	Δ homicide rate	FEM (1^{st} diffs)	0	136

	Δ unempl rate (contemporaneous)	Regions	1970-2006	Δ homicide rate	FEM (1st diffs)	0	136
	Δ unempl rate (contemporaneous)	Regions	1976-2005	Δ felony homicide rate	FEM (1st diffs)	0	112
	Δ unempl rate (contemporaneous)	Regions	1976-2005	Δ argument homicide rate	FEM (1st diffs)	0	112
National Studies							
Harcourt (2011)	% unemployed (contemporaneous)	Nation	1934-2001	Homicide rate (log)	Prais-Winsten Regress	0	67
	% unemployed (contemporaneous)	Nation	1934-2001	Homicide rate (log)	Prais-Winsten Regress	0	67
	% unemployed (contemporaneous)	Nation	1934-2001	Homicide rate (log)	Prais-Winsten Regress	0	43
Phillips & Land (2012)	Unemployment rate (contemporaneous)	Nation	1978-2005	Homicide rate	FEM	0	28
	Δ unempl rate (contemporaneous)	Nation	1978-2005	Homicide rate	FEM	0	28
	Unemployment rate (contemporaneous)	Nation	1978-2005	Homicide rate	FEM	-	28
	Δ unempl rate (contemporaneous)	Nation	1978-2005	Homicide rate	FEM	0	28

+indicates statistically significant + effect found; - indicates statistically significant – effect found; 0 indicates null finding
Δ = change
[a]relationship is marginally significant ($p<.10$)

Table 3 Standardized Mean Effect Sizes (Mr) for Predictor Domains, Grouped by Main Explanation[a]

	Weighted by Sample Size				Unweighted		
Predictor Domain	M_r[b]	p-val[b]	LL_r[b]	UL_r[b]	M_r[c]	p-val[c]	N/n[d]
Economic Conditions (n=10)							
Consumer Sentiment	-0.204	.000	-0.274	-0.131	-0.199	.000	15/3
Deindustrialization	0.022	0.014	0.004	0.039	0.024	0.028	74/11
Disadvantage	0.143	.000	0.100	0.185	0.145	.000	109/22
Economic Resources	-0.035	0.087	-0.074	0.005	-0.033	0.103	277/56
Employment	0.010	0.416	-0.015	0.035	0.010	0.427	30/22
Income Inequality	0.146	0.174	-0.065	0.345	0.344	.000	26/5
Inflation	0.204	.000	0.108	0.296	0.200	.000	40/8
Poverty	0.001	0.939	-0.027	0.029	0.001	0.945	75/25
Unemployment	-0.012	0.609	-0.059	0.035	-0.013	0.587	211/41
Welfare	0.018	0.545	-0.041	0.077	0.020	0.503	117/21
Family Structure (n=3)							
Cohabitation/Marriage	0.040	0.180	-0.019	0.099	0.032	0.254	26/3

Divorce/Family Disorganization	0.095	0.031	0.008	0.179	0.098	0.025	142/31
Single-Parent Households	0.278	0.003	0.099	0.439	0.270	0.004	73/21
Age Structure (n=4)							
Abortion	-0.043	0.003	-0.072	-0.015	-0.049	.000	41/5
Adult/Elderly Age Structure	-0.010	0.588	-0.045	0.025	-0.008	0.619	209/21
Relative Cohort Size	0.232	0.061	-0.011	0.450	0.228	0.072	45/7
Youth Age Structure	0.081	.000	0.048	0.114	0.080	.000	501/70
Immigration (n=1)							
Immigration	0.025	0.194	-0.013	0.062	0.024	0.201	91/24
Policing (n=3)							
Felony Arrest	-0.098	0.020	-0.180	-0.016	-0.112	0.000	173/17
Police Force Size & Expenditures	-0.040	0.001	-0.063	-0.017	-0.040	0.001	291/43
Policing Strategies	-0.062	0.188	-0.153	0.030	-0.062	0.191	45/9
Corrections (n=4)							
Death Penalty	-0.064	0.107	-0.142	0.014	-0.071	0.076	704/24
Incarceration	-0.129	0.000	-0.177	-0.079	-0.122	.000	320/43
Prison Conditions	0.007	0.656	-0.023	0.037	0.009	0.512	50/4

Sentence Enhancement	-0.028	0.619	-0.136	0.081	-0.034	0.600	42/9
Drug Markets (n=1)							
Drug Markets	-0.016	0.842	-0.174	0.142	-0.035	0.684	98/13
Guns (n=2)							
Gun Laws	-0.012	0.432	-0.043	0.018	-0.012	0.432	201/19
Gun Prevalence	0.123	0.033	0.01	0.232	0.118	0.042	118/14
Other Explanations (n=12)							
Alcohol Consumption	0.070	0.003	0.023	0.116	0.084	0.011	39/11
Domestic Violence Resources	-0.032	0.151	-0.075	0.012	-0.022	0.289	35/5
Education	0.011	0.525	-0.024	0.047	0.012	0.494	45/16
Gender & Racial Inequality	0.098	0.032	0.008	0.187	0.093	0.185	58/7
Military Involvement	-0.152	.000	-0.225	-0.077	-0.124	0.029	17/4
Population Structure	-0.001	0.964	-0.065	0.062	0.002	0.957	175/33
Racial Heterogeneity	0.144	.000	0.090	0.197	0.147	.000	295/60
Racial Residential Segregation	0.018	0.749	-0.093	0.129	0.019	0.738	39/6
Residential Mobility	0.019	0.659	-0.067	0.106	0.016	0.721	40/10

Residential Stability	-0.011	0.462	-0.042	0.019	-0.011	0.471	36/7
Routine Activities	-0.061	0.120	-0.137	0.016	-0.063	0.111	46/14
Urbanicity	0.009	0.758	-0.050	0.069	0.009	0.763	117/21

[a]Analyses were conducted using Fisher's Z_r and converted back to r for presentation of results.
[b]Results are weighted estimates after controlling for sample size.
[c]Results are unweighted estimates.
[d]N=number of estimates; n=number of studies

Table 4 Descriptive Statistics of Included Estimates (n=5082 estimates; 1126 models; 145 studies)

	Mean	**Median**	**Min**	**Max**
# Years Covered	25.65	21	10	89
# IVs[a]	13.86	12	1	61
Sample Size	4352.57	691	17	73,343
# Estimates/Study	34.90	16	1	296
# Competing Explanations[b]	4.08	4	0	8
Explanations Included	# of estimates	% of estimates		
Economic Conditions	4610	90.71		
Family Structure	1722	33.88		
Age Structure	4229	83.22		
Immigration	1037	20.41		
Policing	3157	62.12		
Corrections	3242	63.79		
Drug Markets	1058	20.82		
Guns	1580	31.09		
Missing	28	0.55		

[a]Independent variable count does not include model fixed effects or time trend variables. Some missing data. Based on 5057 estimates (99.51%).
[b]Some missing data. Based on 5054 estimates (99.45%).

Table 5 Rank-Ordered Standardized Mean Effect Sizes by Predictor Domain (Overall)[ab]

Rank	Predictor Domain	M_r		Rank	Predictor Domain	M_r
1	Single Parent HH* [2]	0.278		**21**	Police Force Size & Expenditures* [20]	-0.040
2	Relative Cohort Size+ [3]	0.232		**22**	Economic Resources+ [23]	-0.035
3	Inflation* [4]	0.204		**23**	Domestic Violence Resources [27]	-0.032
4	Consumer Sentiment* [5]	-0.204		**24**	Sentence Enhancements [22]	-0.028
5	Military Involvement* [8]	-0.152		**25**	Immigration [25]	0.025
6	Income Inequality [1]	0.146		**26**	Deindustrialization* [26]	0.022
7	Racial Heterogeneity* [6]	0.144		**27**	Residential Mobility [30]	0.019
8	Disadvantage* [7]	0.143		**28**	Welfare [28]	0.018
9	Incarceration* [9]	-0.129		**29**	Racial Residential Segregation [29]	0.018
10	Gun Prevalence* [10]	0.123		**30**	Drug Markets [21]	-0.016
11	Racial & Gender Inequality* [13]	0.098		**31**	Unemployment [31]	-0.012
12	Felony Arrest* [11]	-0.098		**32**	Gun Laws [33]	-0.012
13	Divorce/Family Disorganization* [12]	0.095		**33**	Residential Stability [34]	-0.011
14	Youth Age Structure* [15]	0.081		**34**	Education [32]	0.011
15	Alcohol Consumption* [14]	0.070		**35**	Employment [35]	0.010
16	Death Penalty [16]	-0.064		**36**	Adult & Elderly Age Structure [38]	-0.010
17	Police Strategy [18]	-0.062		**37**	Urbanicity [37]	0.009
18	Routine Activities [17]	-0.061		**38**	Prison Conditions [36]	0.007
19	Abortion* [19]	-0.043		**39**	Population Structure [39]	-0.001
20	Marriage/Cohabitation [24]	0.040		**40**	Poverty [40]	0.001

[a]Rank based on absolute value of sample-size adjusted mean effect size estimates. All analyses were conducted using Fishers Z_r and converted back to r for presentation of results, including the rank-ordering in this table. M_r=mean estimated effect (r).
[b]Unweighted rank order noted in [brackets] for comparison purposes.
*$p<.05$; $^{+}p<.10$

Table 6 Substantively Important Predictor Domains, Rank-Ordered[a]

Overall Rank	Predictor Domain	Mr	LLr	ULr	Estimate N/ Model N
1	Single-Parent Households	0.278	0.099	0.439	73/21
2	Inflation	0.204	0.108	0.296	40/8
3	Consumer Sentiment	-0.204	-0.274	-0.131	15/3
4	Military Involvement	-0.152	-0.225	-0.077	17/4
5	Racial Heterogeneity	0.144	0.090	0.197	295/60
6	Disadvantage	0.143	0.100	0.185	109/22
7	Incarceration	-0.129	-0.177	-0.079	320/43
8	Gun Prevalence	0.123	0.010	0.232	118/14
9	Racial & Gender Inequality	0.098	0.008	0.187	58/7
10	Felony Arrest	-0.098	-0.180	-0.016	173/17
11	Divorce/Family Disorganization	0.095	0.008	0.179	142/31

[a]Effect sizes ranked by their absolute value; all predictor domains significant at p<.05.

Table 7 Factors that Increase and Factors that Decrease Homicide Trends[a]

Increase Homicide Trends						
Rank	**Predictor Domain**	**Mr**		**Rank**	**Predictor Domain**	**Mr**
1	Single-Parent HH	0.278		**5**	Gun Prevalence	0.123
2	Inflation	0.204		**6**	Racial & Gender Ineq	0.098
3	Racial Heterogeneity	0.144		**7**	Divorce/Family Disorg	0.095
4	Disadvantage Index	0.143				

Decrease Homicide Trends						
Rank	**Predictor Domain**	**Mr**		**Rank**	**Predictor Domain**	**Mr**
1	Consumer Sentiment	-0.204		**3**	Incarceration	-0.130
2	Military Involvement	-0.152		**4**	Felony Arrest	-0.098

[a]Only predictor domains significant at p<.05 presented.

Table 8 Common Explanations that Matter and Those that Do Not

Common Explanations that Matter			**Common Explanations that Do Not Matter**		
N/n	**Predictor Domain**	**Mr**	**N/n**	**Predictor Domain**	**Mr**
73/21	Single Parent HH	0.278*	501/70	Youth Age Structure	0.081*
320/43	Incarceration	-0.129*	704/24	Death Penalty	-0.064
118/14	Gun Prevalence	0.123*	45/9	Police Strategies	-0.062
173/17	Felony Arrest	-0.098*	41/5	Abortion	-0.043*
142/31	Divorce/Family Disorg	0.095*	26/3	Marriage/ Cohabitation	0.040
			291/43	Police Size & Expenditures	-0.040*
			277/56	Economic Resources	-0.035^{+}
			91/24	Immigration	0.025
			98/13	Drug Markets	-0.016
			117/21	Welfare	0.018
			211/41	Unemployment	-0.012
			201/19	Gun Laws	-0.12
			209/21	Adult & Elderly Age Structure	-0.010
			75/25	Poverty	0.001

Table 9 Important Predictor Domains that Have Not Received Much Scholarly Attention[a]

Overall Rank	**Predictor Domain**	**Mr**	**LLr**	**ULr**	**Estimate N/ Model N**
2	Inflation	0.204	0.108	0.296	40/8
3	Consumer Sentiment	-0.204	-0.274	-0.131	15/3
4	Military Involvement	-0.152	-0.225	-0.077	17/4
5	Racial Heterogeneity	0.144	0.090	0.197	295/60
9	Racial & Gender Inequality	0.098	0.008	0.187	58/7

[a]Effect sizes ranked by their absolute value; all predictor domains significant at p<.05.

Table 10 Descriptive Statistics of Relevant Study Design Features of Included Studies (adapted from Ousey & Kubrin, 2018, p. 67) (n=5082 estimates)

Model Design Feature	**N estimates**	**% of Total Estimates**
Unit of Analysis		
City	1208	23.77
County	793	15.60
MSA	340	6.69
State	1731	34.06
Region	147	2.89
Nation	863	16.98
Time Period Covered		
Pre Crime Drop (last year <=1989)	628	12.36
Crime Drop (first year >=1990)	586	11.53
Pre & Post Crime Drop (last year >1989 & first year <1990)	3868	76.11
Dependent Variable		
Overall Homicide	3207	63.11
Disaggregated Homicide	1875	36.89
Race-specific[a]	823	16.19
Gender-specific[a]	364	7.16
Age-specific[a]	296	5.82
Type-specific[a]	848	16.69
Longitudinal Type[b]		
Short-Term Change	3445	67.79
Long-Term Change	1637	32.21

[a]Results total more than 1875 (36.89%) due to results disaggregated by more than one of these categories (e.g., results disaggregated by both race and gender would be counted in both categories).
[b]Short-term change includes models conducted in differences, fixed effects models, and other change models. Long-term change includes models conducted in levels.

Table 11 Rank-Ordering of Predictor Domains by Unit of Analysis[a]

	City		County		MSA		State		Region		Nation	
Rank	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**
1	Income Inequality (13/2)	0.448	Felony Arrest (23/4)	-0.223	Racial Hetero (7/4)	0.384	Divorce / Family Disorg (3/2)	-0.403	Inflation (9/1)	0.266	Single Parent HH (39/6)	0.635
2	Gun Prev (32/5)	0.298			Disadv (24/1)	0.160	Racial Hetero (93/13)	0.227	Military Involve (9/1)	-0.230	Gun Prev (6/1)	0.392
3	Urban (4/3)	0.203			Racial Res Seg (24/1)	0.129	Pop Structure (5/3)	-0.111	Cons Sentiment (14/2)	-0.216	Income Inequality (6/2)	0.357
4	Racial Hetero (85/25)	0.180			Adult & Elderly Age Structure (2/1)	-0.123[b]	Incar (204/22)	-0.111	Incar (25/3)	-0.144	Incar (18/7)	-0.288
5	Divorce / Family Disorg (43/15)	0.170			Incar (25/1)	-0.122			Youth Age Structure (34/3)	0.130	Alcohol Consump(5/2)	0.281
6	Disadv (71/20)	0.160			Unempl (4/2)	0.093					Youth Age Structur	0.217

											e (96/14)	
7	Racial & Gender Inequality (58/7)	0.098									Inflation (29/6)	0.212
8											Routine Activities (15/4)	-0.207
9											Police Strategy (2/1)	-0.182[b]
10											Felony Arrest (51/3)	-0.176
11											Military (4/1)	-0.130[b]

[a]All predictor domains are significant at p<.05 level.
[b]Based on results from a 2-level REM.

Table 12 Rank-Ordering of Predictor Domains by Time Period Covered[a]

	Pre Crime Drop Period Only		Crime Drop Period Only		Pre & Post Crime Drop Periods	
Rank	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**
1	Gun Prevalence (12/1)	0.636	Disadvantage (35/7)	0.171	Single Parent Household (60/18)	0.335
2	Relative Cohort Size (8/2)	0.468	Alcohol Consumption (3/2)	0.167	Inflation (29/6)	0.254
3	Single Parent Household (10/4)	0.404	Racial Heterogeneity (18/8)	0.160	Relative Cohort Size (37/7)	0.248
4	Felony Arrest (46/3)	-0.277	Racial Residential Segregation (27/3)	0.126	Consumer Sentiment (15/3)	-0.204
5	Disadvantage (8/2)	0.203	Incarceration (37/6)	-0.120	Military Involvement (17/4)	-0.149
6	Population Structure (8/2)	0.147	Death Penalty (6/1)	-0.102	Racial Heterogeneit y (223/48)	0.140
7	Economic Resources (11/4)	0.133			Incarceration (277/36)	-0.134
8	Urbanicity (46/5)	-0.110			Disadvantage (66/16)	0.127
9	Poverty (4/3)	-0.105[b]			Racial and Gender Inequality (50/5)	0.115

[a]All predictor domains are significant at p<.05 level.
[b]Based on results from a 2-level REM.

Table 13 Rank-Ordering of Predictor Domains by Dependent Variable[a]

	Total Homicide		**Disaggregated Homicide**	
Rank	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**
1	Racial & Gender Inequality (8/1)	0.353	Single Parent Households (38/8)	0.393
2	Inflation (23/4)	0.245	Consumer Sentiment (4/1)	-0.248
3	Single Parent Household (35/14)	0.235	Routine Activities (20/6)	-0.181
4	Consumer Sentiment (11/3)	-0.188	Inflation (17/4)	0.164
5	Disadvantage (39/10)	0.173	Military Involvement (4/1)	-0.130
6	Racial Heterogeneity (190/50)	0.158	Disadvantage (70/14)	0.117
7	Military Involvement (13/3)	-0.155	Abortion (5/1)	-0.102
8	Gun Prevalence (64/11)	0.149		
9	Incarceration (265/36)	-0.140		
10	Felony Arrest (124/15)	-0.132		
11	Divorce/Family Disorganization (86/21)	0.127		

[a]All predictor domains are significant at $p<.05$ level.

Table 14 Rank-Ordering of Predictor Domains by Longitudinal Research Design[a]

	Short-Term Change		**Long-Term Change**	
Rank	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**
1	Income Inequality (19/4)	0.425	Single Parent Household	0.655
2	Inflation (40/8)	0.211	Alcohol Consumption	0.252
3	Consumer Sentiment (15/3)	-0.204	Felony Arrest	-0.221
4	Racial & Gender Inequality (54/5)	0.199	Racial Heterogeneity	0.189
5	Military Involvement (17/4)	-0.149	Disadvantage	0.163
6	Incarceration (283/37)	-0.141	Sentencing Enhancement	0.146[b]
7	Disadvantage (64/16)	0.128	Racial Residential Segregation	0.133
8	Racial Heterogeneity (171/43)	0.110	Youth Age Structure	0.103
9			Economic Resources	-0.101

[a]All predictor domains are significant at p<.05 level.
[b]Based on results from a 2-level REM.

Table 15 Three-Level Random Effects Models for Economic Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Disadvantage		**Model 2 – Economic Resources**		**Model 3 – Poverty**		**Model 4 – Unemployment**		**Model 5 – Welfare**	
Moderator	Coeff	Z	Coeff	Z	Coeff	Z	Coeff	Z	Coeff	Z
# IVs	-0.0112 (0.006)*	-2.01	-0.0023 (0.003)	-0.92	0.0020 (0.002)	1.16	0.0086 (0.005)$^{+}$	1.78	0.0012 (0.002)	0.54
# Years Covered	0.0062 (0.003)*	2.23	-0.0119 (0.002)*	-5.31	0.0020 (0.002)	1.18	0.0060 (0.002)*	3.54	0.0018 (0.002)	0.79
# Comp Explan	-0.0147 (0.014)	-1.04	-0.0086 (0.012)	-0.72	0.0023 (0.011)	0.21	-0.0239 (0.017)	-1.41	0.0097 (0.013)	0.77
Publication Year	-0.0038 (0.005)	-0.75	0.0079 (0.007)	1.14	-0.0018 (0.003)	-0.57	0.0046 (0.005)	0.88	0.0030 (0.008)	0.38
County	-0.1203 (0.059)*	-2.05	-0.0114 (0.074)	-0.15	-0.024 (0.046)	-0.05	0.0148 (0.108)	0.14	-0.0503 (0.105)	-0.48
MSA	0.0480 (0.081)	0.59	-0.0201 (0.172)	-0.12	0.2527 (0.088)*	2.89	0.0688 (0.180)	0.38	---	
State	-0.3284 (0.090)*	-3.66	0.0472 (0.072)	0.65	-0.0464 (0.029)*	-1.40	-0.0969 (0.078)	-1.25	-0.0416 (0.099)	-0.42
Region	---		0.3859 (0.166)*	2.33	---		-0.1315 (0.100)	-1.31	---	
Nation	---		0.8422 (0.094)*	9.00	-0.0788 (0.090)	-0.87	-0.2272 (0.081)*	-2.80	0.0477 (0.108)	0.44
Overall DV	0.0764 (0.038)*	1.99	0.0226 (0.038)	0.59	0.0416 (0.035)	1.18	-0.1475 (0.061)*	-2.41	0.0293 (0.054)	0.55

Short-Term Change	-0.0055 (0.043)	-0.13	-0.0051 (0.034)	-0.15	-0.0095 (0.044)	-0.21	-0.1605 (0.049)*	-3.28	0.0486 (0.073)	0.67
Pre CD Period Only	0.0323 (0.063)	0.51	0.1923 (0.161)	1.19	-0.1218 $(0.073)^{+}$	-1.67	-0.0587 (0.1843)	-0.32	0.0642 (0.096)	0.67
Pre & Post CD Periods	-0.0675 (0.063)	-1.07	0.1504 (0.141)	1.06	0.0221 (0.053)	0.42	-0.1092 (0.158)	-0.69	---	
Constant	**0.1371 (0.015)***	**8.86**	**-0.0449 (0.035)**	**-1.28**	**-0.0007 (0.012)**	**-0.06**	**-0.0312 $(0.016)^{+}$**	**-1.92**	**0.0307 (0.030)**	**1.04**
N/n	109/22		277/56		75/25		211/41		117/21	
BIC	-99.29		-187.35		-123.03		79.14		-125.03	
Log Likelihood	87.18		144.29		98.22		5.919		98.23	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.

$^{+}p<.10$; $*p<.05$

Table 16 Three-Level Random Effects Models for Family Structure Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Divorce		**Model 2 – Single Parent Households**	
Moderator	Coeff	Z	Coeff	Z
# IVs	-0.0039 (0.003)	-1.16	-0.0007 (0.009)	-0.07
# Years Covered	0.0016 (0.003)	0.57	-0.0065 (0.005)	-1.27
# Competing Explanations	-0.0150 (0.018)	-0.84	0.0112 (0.053)	0.21
Publication Year	0.055 (0.008)	0.64	-0.0033 (0.023)	-0.14
County	-0.0635 (0.064)	-1.00	0.0731 (0.255)	0.29
MSA	-0.1498 (0.162)	-0.92	---	
State	0.0854 (0.068)	1.26	-0.0049 (0.294)	-0.02
Region	---		---	
Nation	-0.1394 (0.150)	-0.93	0.2587 (0.340)	0.76
Overall DV	0.1701 (0.086)*	1.98	-0.0913 (0.119)	-0.76
Short-Term Change	-0.0823 (0.033)*	-2.48	-0.7083 (0.283)*	-2.50
Pre CD Period Only	0.0223 (0.059)	0.38	-0.1639 (0.345)	-0.48
Pre & Post CD Periods	-0.0444 (0.064)	-0.69	0.0814 (0.285)	0.29
Constant	**0.0604 (0.041)**	**1.47**	**0.4209 (0.041)***	**10.18**
N/n	142/31		73/21	
BIC	-149.00		123.92	
LogLikelihood	116.63		-27.64	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
$^{+}$p<.10; *p<.05

Table 17 Three-Level Random Effects Models for Age Structure Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Youth Age Structure		**Model 2 – Adult/Elderly Age Structure**	
Moderator	Coeff	Z	Coeff	Z
# IVs	-0.0008 (0.001)	-0.55	-0.0013 (0.001)	-1.40
# Years Covered	0.0011 (0.001)	1.26	-0.0013 (0.001)	-0.98
# Competing Explanations	0.0029 (0.012)	0.25	0.0007 (0.013)	0.05
Publication Year	-0.0066 (0.003)*	-2.22	-0.0061 (0.004)	-1.59
County	-0.0780 (0.050)	-1.56	0.0284 (0.046)	0.61
MSA	0.0654 (0.085)	0.77	-0.3865 (0.055)*	-7.04
State	-0.0828 (0.041)*	-2.03	0.0245 (0.033)	0.74
Region	-0.0679 (0.063)	-1.08	-0.1767 (0.059)*	-3.00
Nation	0.0811 (0.045)+	1.79	-0.0341 (0.080)	-0.42
Overall DV	0.1029 (0.030)*	3.47	-0.0035 (0.011)	-0.33
Short-Term Change	0.0159 (0.032)	0.49	-0.2306 (0.053)*	-4.32
Pre Crime Drop Period Only	0.05390 (0.092)	0.58	0.0516 (0.038)	1.35
Pre & Post Crime Drop Periods	0.0245 (0.069)	0.36	0.0173 (0.037)	0.46
Constant	**0.0833 (0.011)***	**7.60**	**0.0030 (0.007)**	**0.46**
N/n	501/70		209/21	
BIC	120.42		340.37	
LogLikelihood	-7.37		-584.58	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.

+p<.10; *p<.05

Table 18 Three-Level Random Effects Models for Immigration Predictor Domain. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Immigration	
Moderator	Coeff	Z
# IVs	0.0015 (0.002)	0.66
# Years Covered	0.0046 (0.004)	1.15
# Competing Explanations	-0.0150 (0.011)	-1.33
Publication Year	-0.0062 (0.008)	-0.79
County	0.0643 (0.059)	1.08
MSA	-0.0444 (0.067)	-0.67
State	0.1373 (0.059)*	2.31
Region	---	
Nation	---	
Overall DV	-0.0561 (0.035)	-1.58
Short-Term Change	0.0103 (0.043)	0.24
Pre Crime Drop Period Only	---	
Pre & Post Crime Drop Periods	-0.0606 (0.070)	-0.87
Constant	**0.0106 (0.022)**	**0.48**
N/n	89/23	
BIC	-95.17	
LogLikelihood	81.25	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
$^{+}p<.10$; $^{*}p<.05$

Table 19 Three-Level Random Effects Models for Policing Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Felony Arrest		**Model 2 – Police Size**	
Moderator	Coeff	Z	Coeff	Z
# IVs	-0.0036 (0.003)	-1.24	0.0019 (0.001)	1.57
# Years Covered	-0.0030 (0.004)	-0.80	0.0023 (0.001)*	2.20
# Competing Explanations	-0.0025 (0.030)	-0.08	-0.0014 (0.005)	-0.31
Publication Year	0.0011 (0.009)*	0.12	0.0005 (0.003)	0.21
County	-0.1652 (0.140)	-1.18	0.0139 (0.025)	0.56
MSA	---		0.0262 (0.058)	0.45
State	0.0501 (0.088)	0.57	-0.0074 (0.017)	-0.45
Region	---		-0.0264 (0.056)	-0.47
Nation	0.0616 (0.052)	1.19	0.0089 (0.034)	0.26
Overall DV	-0.0943 (0.046)$^{+}$	-2.05	-0.0390 (0.020)$^{+}$	-1.91
Short-Term Change	0.0326 (0.078)	0.42	-0.0079 (0.021)	-0.38
Pre CD Period Only	-0.2912 (0.124)*	-2.35	0.0407 (0.049)	0.84
Pre & Post CD Periods	---		0.0002 (0.041)	0.00
Constant	**-0.1287 (0.014)***	**-9.07**	**-0.0350 (0.011)***	**-3.08**
N/n	173/17		291/43	
BIC	-20.44		-742.01	
LogLikelihood	46.29		422.07	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
$^{+}$p<.10; *p<.05

Table 20 Three-Level Random Effects Models for Corrections Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Death Penalty		**Model 2 – Incarceration**	
Moderator	Coeff	Z	Coeff	Z
# IVs	0.0034 (0.003)	0.99	-0.0022 (0.003)	-0.77
# Years Covered	0.0046 (0.001)*	3.14	-0.0016 (0.001)	-1.10
# Competing Explanations	-0.0003 (0.014)	-0.02	-0.0053 (0.011)	-0.48
Publication Year	-0.0067 (0.008)	-0.81	-0.0018 (0.004)	-0.40
County	-1.219 (0.793)	-1.54	0.0507 (0.057)	0.89
MSA	---		-0.0403 (0.133)	-0.30
State	0.0095 (0.048)	0.20	0.0313 (0.053)	0.59
Region	-0.0022 (0.222)	-0.01	-0.0192 (0.060)	-0.32
Nation	---		-0.1889 (0.092)*	-2.04
Overall DV	-0.0131 (0.024)	-0.55	-0.1017 (0.043)*	-2.36
Short-Term Change	-0.0909 (0.070)*	-2.24	-0.0594 (0.062)	-0.96
Pre Crime Drop Period Only	-0.1322 (0.262)	-0.50	0.1252 (0.088)	**1.42**
Pre & Post Crime Drop Periods	-0.2132 (0.249)	-0.86	0.0806 (0.055)	1.45
Constant	**-0.0637 (0.061)**	**-1.04**	**-0.1380 (0.029)***	-4.69
N/n	696/24		320/43	
BIC	-743.56		-279.35	
LogLikelihood	424.15		191.59	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
[+]p<.10; *p<.05

Table 21 Three-Level Random Effects Models for Drug Market Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Drug Markets	
Moderator	Coeff	Z
# IVs	-0.0139 (0.009)	-1.53
# Years Covered	-0.0122 (0.012)	-1.03
# Competing Explanations	0.0579 (0.046)	1.26
Publication Year	-0.0425 (0.011)*	-3.76
County	0.2228 (0.231)	0.97
MSA	0.3742 (0.288)	1.30
State	0.4818 (0.265)$^{+}$	1.82
Region	---	
Nation	---	
Overall DV	-0.2061 (0.118)$^{+}$	-1.75
Short-Term Change	0.0984 (0.163)	0.60
Pre Crime Drop Period Only	---	
Pre & Post Crime Drop Periods	---	
Constant	**0.0325 (0.045)**	**0.72**
N/n	98/13	
BIC	-27.74	
LogLikelihood	43.67	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
$^{+}p<.10$; $*p<.05$

Table 22 Three-Level Random Effects Models for Guns Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Gun Laws		**Model 2 – Gun Prevalence**	
Moderator	Coeff	Z	Coeff	Z
# IVs	-0.0005 (0.001)	-0.55	0.0018 (0.003)	0.70
# Years Covered	-0.0023 (0.003)	-0.87	0.0041 (0.006)	0.68
# Competing Explanations	0.0083 (0.016)	0.52	-0.0151 (0.011)	-1.36
Publication Year	-0.0029 (0.004)	-0.73	-0.0013 (0.007)	-0.17
County	-0.0573 (0.063)	-0.90	-0.0740 (0.050)	-1.49
MSA	---		0.0815 (0.066)	1.23
State	-0.0198 (0.051)	-0.39	-0.0254 (0.042)	-0.61
Region	---		---	
Nation	---		0.4012 (0.125)*	3.21
Overall DV	-0.0141 (0.020)	-0.72	-0.0038 (0.028)	-0.14
Short-Term Change	0.0092 (0.051)	0.18	0.0516 (0.030)[+]	1.70
Pre Crime Drop Period Only	0.07074 (0.090)	0.79	0.6528 (0.209)*	3.13
Pre & Post Crime Drop Periods	0.0532 (0.086)	0.62	---	
Constant	**-0.0146 (0.014)**	**-1.01**	**0.1406 (0.009)***	**16.27**
N/n	201/19		118/14	
BIC	-311.92		-154.03	
LogLikelihood	195.74		112.79	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
[+]p<.10; *p<.05

Table 23 Three-Level REM for "Other Explanations" Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Population Structure		**Model 2 – Urbanicity**		**Model 3 – Racial Heterogeneity**	
Moderator	Coeff	Z	Coeff	Z	Coeff	Z
# IVs	-0.0022 (0.004)	-0.61	0.0008 (0.003)	0.25	0.0050 (0.003)$^{+}$	1.75
# Years Covered	-0.0040 (0.004)	-0.94	-0.0006 (0.002)	-0.40	0.0047 (0.002)*	2.37
# Competing Explanations	0.0623 (0.022)*	2.83	0.0064 (0.017)	0.38	-0.0444 (0.016)*	-2.74
Publication Year	-0.0138 (0.009)	-1.51	-0.0003 (0.005)	-0.06	0.0065 (0.006)	1.12
County	0.0842 (0.070)	1.20	-0.1871 (0.108)$^{+}$	-1.73	-0.1310 (0.083)	-1.58
MSA	-0.0794 (0.128)	-0.62	---		0.2700 (0.107)*	2.53
State	-0.2193 (0.151)	-1.45	-0.1847 (0.073)*	-2.55	-0.1494 (0.069)*	-2.17
Region	---		---		-0.2789 (0.142)*	-1.96
Nation	---		-0.0684 (0.102)	-0.67	-0.0972 (0.090)	-1.08
Overall DV	0.0096 (0.077)	0.12	0.0618 (0.071)	0.87	0.0267 (0.037)	0.72
Short-Term Change	-0.0907 (0.068)	-1.33	-0.1284 (0.052)*	-2.45	-0.0340 (0.037)	-0.92
Pre CD Period Only	0.2388 (0.157)	1.52	-0.3521 (0.150)*	-2.34	0.0101 (0.1194)	0.08
Pre & Post CD Periods	0.0506 (0.130)	0.39	-0.1125 (0.129)	-0.87	0.0086 (0.100)	0.09
Constant	**-0.0066 (0.023)**	**-0.29**	**-0.0310 (0.023)**	**-1.36**	**0.1032 (0.027)***	**3.97**
N/n	175/33		117/21		295/60	
BIC	161.50		-145.10		-178.09	
LogLikelihood	-39.43		110.65		140.23	

[a]All variables are grand-mean centered. Model also controls for sample size. Results not reported in the table above but are available upon request.
$^{+}p<.10$; $*p<.05$

Table 24 Summary of Multivariate Results, by Moderator Variable

Moderator	# times significant	# times not sig	Not tested	Total tested	% important
# IVs	3	17	0	20	0.15
# Years	6	14	0	20	0.30
# Competing Explanations	2	18	0	20	0.10
Publication Year	3	17	0	20	0.15
County	2	18	0	20	0.10
MSA	3	11	6	14	0.21
State	6	14	0	20	0.30
Region	3	5	12	8	0.38
Nation	5	9	6	14	0.36
Overall DV	8	12	0	20	0.40
Short-Term Change	7	13	0	20	0.35
Pre CD Period	4	14	2	20	0.20
Pre & Post CD Periods	0	16	4	16	0.00

Table 25 Summary of Strength and Stability of Mean Effect Size Estimates (adapted from Pratt & Cullen, 2005, p. 403)

	Strength			**Stability**		
Predictor	**High (n=10)**	**Mod (n=11)**	**Low (n=19)**	**High (n=25)**	**Mod (n=10)**	**Low (n=5)**
Single Parent HH	**X**					**X**
Relative Cohort Size	**X**				**X**	
Inflation	**X**				**X**	
Cons Sentiment	**X**			**X**		
Military Involvement	**X**			**X**		
Income Inequality	**X**					**X**
Racial Heterogeneity	**X**				**X**	
Disadvantage	**X**			**X**		
Incarceration	**X**				**X**	
Gun Prevalence	**X**					**X**
Racial & Gender Inequality		**X**				**X**
Felony Arrest		**X**				**X**
Divorce/Family Disorganization		**X**			**X**	
Youth Age Structure		**X**			**X**	
Alcohol Consumption		**X**			**X**	
Death Penalty		**X**		**X**		
Police Strategy		**X**		**X**		
Routine Activity		**X**			**X**	
Abortion			**X**	**X**		
Marriage/Cohabitation			**X**	**X**		
Police Size & Expenditures			**X**	**X**		
Economic Resources			**X**	**X**		
Dom Viol Resources			**X**	**X**		
Sentence Enhance		**X**		**X**		
Immigration			**X**	**X**		

Deindustrialization			**X**	**X**		
Residential Mobility		**X**		**X**		
Welfare			**X**	**X**		
Racial Residential Seg		**X**			**X**	
Drug Markets			**X**	**X**		
Unemployment			**X**	**X**		
Gun Laws			**X**	**X**		
Residential Stability			**X**	**X**		
Education			**X**	**X**		
Employment			**X**	**X**		
Adult & Elderly Age Structure			**X**	**X**		
Urbanicity			**X**		**X**	
Prison Conditions			**X**	**X**		
Population Structure			**X**	**X**		
Poverty			**X**	**X**		

Table 26 The Top-Ranked Predictor Domains Impacting Homicide Trends

Updated Rank	**Broad Explanation**	**Predictor Domain**	**Prominence in the Literature (Explanation)**	**Prominence in the Literature (Predictor)**
1	**Economic Conditions**	**Income Inequality**	Common	Common
2	**Family Structure**	**Single Parent Households**	Common	**Missing**
3	**Age Structure**	**Relative Cohort Size**	Common	Common
4	**Economic Conditions**	**Consumer Sentiment**	Common	**Missing**
5	**Economic Conditions**	**Inflation**	Common	**Missing**
6	**Guns**	**Gun Prevalence**	Common	Common
7	**"Other"**	**Racial Heterogeneity**	**Missing**	**Missing**
8	**"Other"**	**Military Involvement**	**Missing**	**Missing**
9	**Police**	**Felony Arrest**	Common	Common
10	**Corrections**	**Incarceration**	Common	Common
11	**Economic Conditions**	**Disadvantage**	Common	Common
12	**"Other"**	**Racial & Gender Inequality**	**Missing**	**Missing**
13	**"Other"**	**Alcohol Consumption**	**Missing**	**Missing**

Table 27 Empirical Status of the Predictor Domains

Consistently Strong[a] (n=7)	**Middle of the Road[b] (n=9)**	**Consistently Weak[c] (n=19)**	**Inconsistent[d] (n=5)**
Consumer Sentiment	Death Penalty	Abortion	Single Parent HH
Military Involvement	Police Strategy	Marriage/Cohabitation	Income Inequality
Disadvantage	Routine Activities	Police Force Size and Expenditures	Gun Prevalence
Relative Cohort Size	Sentence Enhancements	Economic Resources	Felony Arrest
Inflation	Residential Mobility	Domestic Violence Resources	Racial and Gender Inequality
Racial Heterogeneity	Divorce/Family Disorganization	Immigration	
Incarceration	Youth Age Structure	Deindustrialization	
	Alcohol Consumption	Welfare	
	Racial Residential Segregation	Drug Markets	
		Unemployment	
		Gun Laws	
		Residential Stability	
		Educational Attainment	
		Employment	
		Adult and Elderly Age Structure	
		Urbanicity	
		Prison Conditions	
		Population Structure	
		Poverty	

[a]Includes predictor domains classified as either high/high or high/moderate for strength/stability.
[b]Includes predictor domains classified as either moderate/high or moderate/moderate for strength/stability.
[c]Includes predictor domains classified as either low/high or low/moderate for strength/stability.
[d]Includes predictor domains classified as either high/low, moderate/low, or low/low for strength/stability.

Table 28 Summary of Overall Support for the Eight Broad Explanations

Broad Explanation	Broad Support	Notes
Economic Conditions	✓	Measure specific
Family Structure	✓	
Age Structure	✗	exception relative cohort size
Immigration	✗	
Policing	✗	exception felony arrest
Corrections	✓	
Drug Markets	✗	
Guns	✓	exception gun laws
Other Explanations	✓	military involvement; racial heterogeneity; racial and gender inequality; alcohol consumption

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Appendix A

LIST OF STUDIES INCLUDED IN META-ANALYSIS (N=145)

Authors and (Publication Year)
Albuquerque (2007)
Arvanites & DeFina (2006)
Bailey (1990)
Bailey & Peterson (1999)
Barber (2003)
Baumer (2008)
Baumer, Rosenfeld, & Wolff (2012)
Beaulieu & Messner (2010)
Besci (1999)
Black & Nagin (1998)
Braga (2008)
Brandt & Kovandzic (2015)
Britt (1997)
Britt, Kleck, & Bordua (1996)
Bronars & Lott (1998)
Browne, Strom, Barrick, Williams, & Parker (2010)
Chalfin (2015)
Chamlin (2014)
Chamlin, Cochran, & Lowenkamp (20020
Chen (2008)
Corman & Mocan (2000)
Corman & Mocan (2005)
Decker & Kohfeld (1990)
DeFina & Arvanites (2002)
Dezhbakhsh, Rubin, & Shepherd (2003)
Dezhbakhsh & Shepherd (2006)
Donohue & Levitt (2001)
Donohue & Levitt (2004)
Donohue & Levitt (2008)
Dugan (2002)
Dugan, Nagin, & Rosenfeld (1999)
Duggan (2001)
Evans & Owens (2007)
Fernquist (2000)
Foote & Goetz (2008)
Fowles & Merva (1996)
Gartner & Parker (1990)
Gould, Weinburg, & Mustard (2002)

Greenberg (2001)
Greenberg (2008)
Greenberg (2016)
Grinols, Mustard, & Staha (2011)
Grogger & Willis (2000)
Harcourt (2011)
Johnson & Raphael (2012)
Joyce (2004)
Kaminski & Marvell (2002)
Katz, Levitt, & Shustorovich (2003)
Kovandzic, Marvell, & Vieraitis (2005)
Kovandzic, Marvell, Vieraitis, & Moody (2004)
Kovandzic, Sloan, & Vieraitis (2002)
Kovandzic & Vieraitis (2006)
Kovandzic, Vieraitis, & Boots (2009)
LaFree & Drass (1996)
LaFree, Drass, & O'Day (1992)
LaValle (2007)
LaValle (2010)
LaValle & Glover (2012)
Levitt (1996)
Levitt (1997)
Levitt (1998)
Levitt (1999)
Levitt (2002)
Light & Ulmer (2016)
Lilley & Boba (2009)
Lin (2009)
Lott & Mustard (1997)
Ludwig (1998)
MacDonald & Gover (2005)
Martin & Legault (2005)
Marvell & Moody (1994)
Marvell & Moody (1995)
Marvell & Moody (1996a)
Marvell & Moody (1996b)
Marvell & Moody (1997)
Marvell & Moody (1998)
Marvell & Moody (1999)
Marvell & Moody (2001)
Matthews, Maume, & Miller (2001)
McCall & Land (2004)
McCall & Parker (2005)

McCall, Parker, & MacDonald (2008)
McDowall (1991)
McDowall, Loftin, & Wiersema (1992)
McVeigh & Cunningham (2012)
Miethe, Hughes, & McDowall (1991)
Mocan & Gittings (2003)
Moody (2001)
Moody (2010)
Moody & Marvell (2010)
Morris, TenEyck, Barnes, & Kovandzic (2014)
Narayan & Smyth (2006)
O'Brien & Stockard (2002)
O'Brien & Stockard (2003)
O'Brien & Stockard (2006)
O'Brien, Stockard, & Issacson (1999)
Oh (2005)
Olson & Maltz (2001)
Ousey (2000)
Ousey & Lee (2002)
Parker (2003)
Parker (2004)
Parker & Stansfield (2015)
Parker, Williams, McCaffree, Acensio, Browne, Strom, & Barrick (2011)
Pepper (2008)
Peterson & Bailey (1991)
Phillips (2006a)
Phillips (2006b)
Raphael & Winter-Ebmer (2001)
Reckdenwald & Parker (2011)
Rosenfeld (2009)
Rosenfeld & Oliver (2008)
Rotolo & Tittle (2006)
Ruther (2014)
Saridakis (2004)
Savage (2009)
Savolainen (2000)
Shepherd (2004)
Shihadeh & Ousey (1998)
Smith, Devine, & Sheley (1992)
Sorenson, Wrinkle, Brewer, & Marquart (1999)
Stack (1990)
Stack (1994)
Stack (1998)

Stowell, Messner, McGeever, & Raffalovich (2009)
Strom & MacDonald (2007)
Stults & Hasbrouck (2015)
Vieraitis, Kovandzic, & Marvell (2007)
Wadsworth (2010)
Wells, Ren, & DeLeon-Granados (2010)
Worrall & Kovandzic (2007)
Worrall & Kovandzic (2010)
Worrall (2004)
Worrall (2008a)
Worrall (2008b)
Yunker (2001)
Zimmerman (2004)
Zimmerman (2006)

Appendix B

RANK-ORDERING OF PREDICTOR DOMAINS BY UNIT OF ANALYSIS (FULL RESULTS)

	City		**County**		**MSA**		**State**		**Region**		**Nation**	
Rank	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**	**Pred Domain**	**Mr**
1	Income Inequality (13/2)	0.448*	Felony Arrest (23/4)	-0.223*	Racial Hetero (7/4)	0.384*	Divorce / Family Disorg (3/2)	-0.403*	Inflation (9/1)	0.266*	Single Parent HH (39/6)	0.635*
2	Gun Prev (32/5)	0.298*	Incar (18/4)	-0.070*	Disadv (24/1)	0.160*	Racial Hetero (93/13)	0.227*	Military Involve (9/1)	-0.230*	Gun Prev (6/1)	0.392*
3	Urbanicity (4/3)	0.203*	Disadv (12/2)	0.055*	Racial Res Seg (24/1)	0.129*	Pop Structure (5/3)	-0.111*	Cons Sentiment (14/2)	-0.216*	Income Inequality (6/2)	0.357*
4	Racial Hetero (85/25)	0.180*	Racial Hetero (49/9)	0.055*	Adult & Elderly Age Structure (2/1)	-0.123*[a]	Incar (204/22)	-0.111*	Incar (25/3)	-0.144*	Incar (18/7)	-0.288*

5	Divorce / Family Disorg (43/15)	0.170 *	Divorce / Family Disorg (37/7)	0.039 *	Incar (25/1)	-0.122 *	Disadv (2/1)	-0.085 *	Youth Age Structure (34/3)	0.130 *	Alcohol Consump (5/2)	0.281 *
6	Disadv (71/20)	0.160 *	Residential Mobility (3/1)	0.038 *	Unempl (4/2)	0.093 *	Imm (6/3)	0.085 *	Police Size & Expend (7/2)	-0.043 *	Relative Cohort Size (45/7)	0.232 +
7	Racial & Gender Inequality (58/7)	0.098 *	Education (12/3)	0.034 *	Income Inequality (7/1)	0.078 *	Gun Prev (34/4)	0.063 *	Unempl (16/3)	0.036	Youth Age Structure (96/14)	0.217 *
8	Single Parent HH (22/10)	0.087	Urbanicity (7/2)	-0.033	Residential Mobility (24/1)	-0.072 +	Felony Arrest (56/5)	-0.056 *	Racial Hetero (7/2)	-0.020	Inflation (29/6)	0.212 *
9	Economic Resources (37/14)	-0.083 +	Sentence Enhance (13/2)	-0.029	Divorce / Family Disorg (5/2)	0.064 *	Police Size & Expend (90/14)	-0.050 *	Death Penalty (9/1)	-0.009	Routine Activities (15/4)	-0.207 *
10	Felony Arrest (43/6)	-0.078	Imm (9/2)	0.027	Gun Prev (27/2)	0.055 *	Military Involve (4/2)	0.046	Economic Resources (16/3)	-0.006	Police Strategy (2/1)	-0.182 *[a]

11	Incar (30/10)	-0.067*	Gun Laws (37/5)	-0.024*	Drug Markets (24/1)	0.045*	Abortion (41/5)	-0.042*			Felony Arrest (51/3)	-0.176*
12	Residential Mobility (11/6)	0.063	Unemploy (26/7)	-0.23^{+}	Police Size & Expend (27/2)	-0.042*	Youth Age Structure (161/21)	0.041			Military (4/1)	-0.130*^{a}
13	Police Strategy (38/7)	-0.061	Death Penalty (48/1)	-0.022*	Employ (2/1)	-0.041*	Unempl (33/11)	-0.041*			Death Penalty (173/8)	-0.086
14	Unemploy (14/8)	0.060	Gun Prev (19/3)	0.017*	Residential Stability (3/1)	0.035*	Sentence Enhance (24/5)	0.032*			Urbanicity (36/3)	-0.076*
15	Youth Age Structure (69/21)	0.059*	Inflation (2/1)	0.015*	Youth Age Structure (28/3)	0.020	Employ (21/7)	0.027^{+}			Welfare (57/7)	0.076
16	Alcohol (22/5)	0.056	Police Strategy (5/1)	-0.012*	Economic Resources (27/2)	-0.017	Education (16/6)	0.024			Racial Hetero (54/7)	0.066
17	Sentence Enhance (4/1)	0.048*	Youth Age Structure	0.012*	Imm (29/3)	-0.016	Urbanicity (70/13)	-0.024			Education (4/2)	-0.065

			(113/12)									
18	Police Size & Expend (128/22)	-0.043*	Adult & Elderly Age Structure (106/4)	0.012	Population Structure (25/2)	-0.014	Alcohol Consump (12/4)	0.023			Divorce / Family Disorg (54/7)	-0.039
19	Adult & Elderly Age Structure (19/6)	0.037	Single Parent HH (8/3)	-0.012	Deindus (24/1)	0.009	Gun Laws (100/11)	-0.014			Economic Resources (33/8)	-0.034
20	Residential Stability (14/3)	-0.032	Police Size & Expend (28/5)	-0.010			Single Parent HH (4/2)	0.012			Police Size & Expend (11/2)	0.026
21	Deindus (50/10)	0.028*	Residential Stability (15/2)	-0.009			Economic Resources (92/20)	-0.012			Unempl (118/12)	-0.022
22	Marriag / Cohab (26/3)	0.027	Domestic Violence Resources (3/1)	-0.007			Welfare (9/4)	-0.007			Poverty (5/2)	0.002

23	Imm (47/20)	0.026	Population Structure (78/11)	0.004			Poverty (40/11)	-0.007				
24	Racial Res Seg (15/5)	-0.025	Poverty (8/3)	-0.004			Death Penalty (474/16)	-0.007				
25	Domestic Violence Resources (32/4)	-0.023	Welfare (31/5)	-0.004			Prison Conditions (50/4)	0.007				
26	Welfare (20/5)	-0.018	Economic Resources (72/13)	0.001			Adult & Elderly Age Structure (80/8)	0.006				
27	Poverty (21/8)	-0.015	Routine Activities (6/1)	0.000			Drug Markets (4/2)	0.004^{+}				
28	Routine Activities (24/8)	0.011										
29	Population	-0.008										

	Structure (67/19)											
30	Employ (7/3)	0.007*										
31	Education (12/4)	0.005										
32	Gun Laws (63/6)	-0.004										
33	Drug Markets (67/11)	0.000										

[a]Based on results from a 2-level REM.

[+]p<.10; *p<.05

Appendix C

RANK-ORDERING OF PREDICTOR DOMAINS BY TIME PERIOD COVERED (FULL RESULTS)

	Pre Crime Drop Period Only		**Crime Drop Period Only**		**Pre & Post Crime Drop Periods**	
Rank	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**
1	Gun Prevalence (12/1)	0.636*	Divorce/ Family Disorganization (10/5)	0.235	Single Parent Household (60/18)	0.335*
2	Relative Cohort Size (8/2)	0.468*	Disadvantage (35/7)	0.171*	Inflation (29/6)	0.254*
3	Single Parent Household (10/4)	0.404*	Alcohol Consumption (3/2)	0.167*	Relative Cohort Size (37/7)	0.248*
4	Felony Arrest (46/3)	-0.277*	Racial Heterogeneity (18/8)	0.160*	Consumer Sentiment (15/3)	-0.204*
5	Disadvantage (8/2)	0.203*	Racial Residential Segregation (27/3)	0.126*	Military Involvement (17/4)	-0.149*
6	Population Structure (8/2)	0.147*	Incarceration (37/6)	-0.120*	Income Inequality (26/5)	0.146
7	Economic Resources (11/4)	0.133*	Police Strategy (20/2)	-0.113	Racial Heterogeneity (223/48)	0.140*
8	Youth Age Structure (57/8)	0.128	Death Penalty (6/1)	-0.102*	Incarceration (277/36)	-0.134*

9	Welfare (42/4)	0.123	Unemployment (5/3)	0.072*	Disadvantage (66/16)	0.127*
10	Urbanicity (46/5)	-0.110*	Gun Laws (5/2)	-0.065*	Racial and Gender Inequality (50/5)	0.115*
11	Incarceration (6/4)	-0.107	Routine Activities (4/2)	0.063*	Residential Mobility (7/4)	0.110
12	Poverty (4/3)	-0.105*[a]	Gun Prevalence (27/2)	0.055*	Police Strategy (25/7)	-0.093
13	Racial Heterogeneity (54/7)	0.068	Residential Mobility (31/5)	-0.054	Death Penalty (568/17)	-0.080
14	Routine Activities (19/3)	-0.064	Drug Markets (24/1)	0.045*	Divorce/ Family Disorg (90/25)	0.077*
15	Death Penalty (130/9)	-0.043	Police Size & Expenditures (99/12)	-0.040[+]	Gun Prevalence (79/11)	0.075*
16	Inflation (11/3)	0.040	Residential Stability (5/2)	0.039*	Routine Activities (23/9)	-0.070
17	Residential Stability (8/2)	-0.037*	Adult & Elderly Age Structure (4/2)	-0.029*	Youth Age Structure (398/55)	0.069*
18	Unemployment (64/7)	-0.030	Population Structure (33/6)	-0.029*	Felony Arrest (127/14)	-0.067*
19	Adult & Elderly Age Structure (5/2)	0.030*	Single Parent HH (3/1)	-0.027*	Racial Residential Segregation (12/3)	-0.061
20	Gun Laws (18/2)	0.024	Poverty (4/2)	-0.023*	Alcohol Consumption (36/9)	0.056*
21	Education (5/2)	0.021	Economic Resources (40/8)	-0.015[+]	Urbanicity (70/17)	0.046
22	Residential Mobility (2/1)	0.016	Youth Age Structure (46/11)	0.015	Abortion (40/5)	-0.043*

23	Divorce/Family Disorganization (42/4)	0.015[a]	Immigration (41/9)	-0.013	Police Size & Expenditures (181/30)	-0.043*
24	Police Size & Expenditures (11/2)	-0.009	Employment (8/4)	0.012*	Economic Resources (226/47)	-0.042[+]
25			Racial & Gender Inequality (8/2)	0.009	Domestic Violence Resources (25/3)	-0.041
26			Deindust (29/3)	0.006	Immigration (50/16)	0.039
27			Domestic Violence Resources (10/2)	-0.002	Deindust (45/8)	0.031*
28					Sentence Enhancements (42/9)	-0.028
29					Marriage/ Cohabitation (26/3)	0.027
30					Employment (22/7)	0.018
31					Education (38/12)	0.016
32					Prison Conditions (49/4)	0.013
33					Gun Laws (178/15)	-0.013*
34					Unempl (142/33)	-0.012
35					Poverty (67/21)	0.011
36					Welfare (75/18)	0.010
37					Drug Markets (74/12)	-0.009
38					Adult & Elderly Age Structure (200/19)	-0.006

39						Population Structure (134/28)	-0.003
40						Residential Stability (19/5)	-0.001

[a]Based on results from a 2-level REM.

$^{+}p<.10$; $^{*}p<.05$

Appendix D

RANK-ORDERING OF PREDICTOR DOMAINS BY DEPENDENT VARIABLE (FULL RESULTS)

	Total Homicide		**Disaggregated Homicide**	
Rank	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**
1	Racial & Gender Inequality (8/1)	0.353*	Income Inequality (12/2)	1.000
2	Inflation (23/4)	0.245*	Single Parent Households (38/8)	0.393*
3	Single Parent Household (35/14)	0.235*	Consumer Sentiment (4/1)	-0.248*
4	Relative Cohort Size (19/4)	0.235^{+}	Relative Cohort Size (26/4)	0.187
5	Consumer Sentiment (11/3)	-0.188*	Routine Activities (20/6)	-0.181*
6	Disadvantage (39/10)	0.173*	Inflation (17/4)	0.164*
7	Racial Heterogeneity (190/50)	0.158*	Military Involvement (4/1)	-0.130*
8	Military Involvement (13/3)	-0.155*	Disadvantage (70/14)	0.117*
9	Gun Prevalence (64/11)	0.149*	Abortion (5/1)	-0.102*
10	Incarceration (265/36)	-0.140*	Incarceration (55/11)	-0.085
11	Felony Arrest (124/15)	-0.132*	Racial Heterogeneity (105/16)	0.077^{+}
12	Divorce/Family Disorganization (86/21)	0.127*	Drug Markets (64/6)	0.071*
13	Drug Markets (34/8)	-0.100	Alcohol Consumption (19/6)	0.066*
14	Youth Age Structure (356/58)	0.090*	Employment (6/2)	0.061*
15	Police Strategy (33/8)	-0.089	Residential Stability (6/2)	-0.057*

16	Death Penalty (399/20)	-0.084+	Racial & Gender Inequality (50/7)	0.055
17	Alcohol Consumption (20/9)	0.063+	Gun Prevalence (54/6)	0.048*
18	Economic Resources (207/49)	-0.051*	Youth Age Structure (145/18)	0.048+
19	Income Inequality (14/4)	0.046	Immigration (50/12)	0.045
20	Police Size & Expenditures (237/37)	-0.044*	Marriage/Cohabitation (22/3)	0.041
21	Racial Residential Segregation (2/1)	0.041*	Residential Mobility (30/4)	-0.033
22	Abortion (36/5)	-0.038*	Domestic Violence Resources (35/5)	-0.032
23	Sentence Enhancements (40/9)	-0.035	Urbanicity (27/3)	0.031
24	Marriage/Cohabitation (4/1)	0.034*	Police Strategy (12/3)	-0.029
25	Routine Activities (26/8)	0.033	Poverty (12/4)	-0.027
26	Deindustrialization (8/4)	0.025	Police Size & Expenditures (54/10)	-0.027
27	Education (31/12)	0.023*	Deindustrialization (66/8)	0.021*
28	Gun Laws (117/14)	-0.022*	Death Penalty (305/6)	-0.016
29	Urbanicity (90/19)	0.017	Economic Resources (70/14)	0.015
30	Welfare (56/13)	0.016	Racial Residential Segregation (37/5)	0.014
31	Immigration (41/15)	0.015	Sentence Enhancements (2/1)	0.013
32	Unemployment (135/35)	-0.013	Felony Arrest (49/5)	-0.013
33	Adult & Elderly Age Structure (150/19)	-0.012	Divorce/Family Disorganization (56/10)	0.011
34	Population Structure (121/28)	-0.010	Adult & Elderly Age Structure (59/4)	-0.010
35	Residential Mobility (10/6)	0.009	Education (14/4)	-0.009
36	Poverty (63/22)	0.007	Unemployment (76/7)	-0.007

37	Prison Conditions (50/4)	0.007	Welfare (61/10)	0.003
38	Employment (24/9)	0.003	Population Structure (54/10)	0.003
39	Residential Stability (26/4)	0.001	Gun Laws (84/10)	0.001

[a]Based on results from a 2-level REM.
$^{+}p<.10$; $^{*}p<.05$

Appendix E

RANK-ORDERING OF PREDICTOR DOMAINS BY LONGITUDINAL RESEARCH DESIGN (FULL RESULTS)

	Short-Term Change		**Long-Term Change**	
Rank	**Predictor Domain**	**Mr**	**Predictor Domain**	**Mr**
1	Income Inequality (19/4)	0.425*	Single Parent Household (38/6)	0.655*
2	Inflation (40/8)	0.211*	Alcohol Consumption (5/2)	0.252*
3	Consumer Sentiment (15/3)	-0.204*	Relative Cohort Size (45/7)	0.232^{+}
4	Racial & Gender Inequality (54/5)	0.199*	Felony Arrest (61/6)	-0.221*
5	Military Involvement (17/4)	-0.149*	Drug Markets (41/6)	-0.205
6	Incarceration (283/37)	-0.141*	Racial Heterogeneity (124/23)	0.189*
7	Disadvantage (64/16)	0.128*	Disadvantage (45/7)	0.163*
8	Gun Prevalence (63/8)	0.113	Police Strategy (22/3)	-0.157^{+}
9	Racial Heterogeneity (171/43)	0.110*	Gun Prevalence (55/8)	0.148^{+}
10	Divorce/Family Disorganization (82/23)	0.098^{+}	Sentencing Enhancement (2/1)	0.146*[a]
11	Routine Activities (42/13)	-0.077^{+}	Racial Residential Segregation (25/2)	0.133*
12	Felony Arrest (112/12)	-0.065*	Death Penalty (269/14)	-0.105^{+}
13	Youth Age Structure (314/51)	0.063*	Youth Age Structure (187/24)	0.103*
14	Alcohol Consumption (34/9)	0.052^{+}	Economic Resources (95/18)	-0.101*
15	Racial Residential Segregation (14/4)	-0.051	Divorce/Family Disorganization (60/12)	0.095
16	Sentence Enhancements (40/8)	-0.049	Income Inequality (7/1)	0.078*

17	Welfare (58/12)	0.047	Incarceration (37/6)	-0.077*
18	Abortion (29/4)	-0.046*	Domestic Violence Resources (11/3)	-0.072*
19	Residential Mobility (12/7)	0.045	Unemployment (50/9)	0.070*
20	Single Parent HH (35/16)	0.036	Routine Activities (4/2)	0.065*
21	Immigration (58/18)	0.034^{+}	Residential Mobility (28/3)	-0.051
22	Deindustrialization (46/9)	0.034*	Adult & Elderly Age Structure (4/2)	0.045*
23	Police Size & Expenditures (216/33)	-0.033*	Police Size & Expenditures (75/13)	-0.044
24	Marriage/Cohabitation (24/2)	0.031	Population Structure (66/14)	0.040
25	Unemployment (161/34)	-0.025	Urbanicity (72/10)	0.034
26	Economic Resources (182/45)	-0.018	Abortion (12/1)	-0.029*
27	Death Penalty (435/12)	-0.016	Welfare (59/9)	-0.015
28	Gun Laws (153/13)	-0.015*	Poverty (11/6)	0.015
29	Urbanicity (45/12)	-0.014	Marriage/Cohabitation (2/1)	-0.011
30	Population Structure (109/22)	-0.013	Employment (8/5)	0.010
31	Education (42/14)	0.013	Immigration (33/7)	-0.007
32	Employment (22/7)	0.011	Education (3/3)	-0.005
33	Adult & Elderly Age Structure (205/19)	-0.010	Gun Laws (48/6)	0.003
34	Residential Stability (31/6)	-0.009	Racial & Gender Inequality (4/2)	-0.003
35	Drug Markets (57/9)	-0.009	Deindustrialization (28/2)	0.002
36	Prison Conditions (50/4)	0.007		
37	Poverty (64/20)	-0.007		
38	Police Strategy (23/6)	0.004		
39	Domestic Violence Resources (24/2)	0.001		

[a]Based on results from a 2-level REM.
$^{+}p<.10$; $*p<.05$

Appendix F

SUMMARY OF MULTIVARIATE RESULTS, BY PREDICTOR DOMAIN

Predictor Domain	# significant moderator variables	# total moderators tested	% moderators significant
Disadvantage	5	11	0.45
Economic Resources	3	13	0.23
Poverty	3	12	0.25
Unemployment	5	13	0.38
Welfare	0	10	0.00
Divorce/Family Disorg	2	12	0.17
Single Parent HH	1	11	0.09
Youth Age Structure	4	13	0.31
Adult/Elderly Age Structure	3	13	0.23
Immigration	1	10	0.10
Felony Arrest	3	10	0.30
Police Size	2	13	0.15
Death Penalty	2	11	0.18
Incarceration	2	13	0.15
Drug Markets	3	9	0.33
Gun Laws	0	10	0.00
Gun Prevalence	3	11	0.27
Population Structure	1	10	0.10
Urbanicity	4	11	0.36
Racial Heterogeneity	6	13	0.46

Appendix G

STABILITY ASSESSMENT

Predictor Domain	# times tested	# times import-ant	% times import-ant	When Important	Stability Class
Single Parent Household	13	8	0.62	1. Overall 2. National 3. Pre CD 4. Pre & post CD 5. Total hom 6. Disagg hom 7. Long-Term Δ 8. Multivariate	Low
Relative Cohort Size	7	2	0.29	1. Pre CD 2. Pre & post CD	Moderate
Inflation	9	7	0.78	1. Overall 2. Regional 3. National 4. Pre & post CD 5. Total hom 6. Disagg hom 7. Short-term Δ	Moderate
Consumer Sentiment	7	6	0.86	1. Overall 2. Region 3. Pre & post CD 4. Total hom 5. Disagg hom 6. Short-term Δ	High
Military Involvement	8	7	0.88	1. Overall 2. Regional 3. National 4. Pre & post CD 5. Total hom 6. Disagg hom 7. Short-term Δ	High

Income Inequality	8	3	0.38	1. City-level 2. National 3. Short-term Δ	Low
Racial Heterogeneity	15	10	0.67	1. Overall 2. City-level 3. MSA-level 4. State-level 5. Crime Drop 6. Pre & post CD 7. Total hom 8. Short-term Δ 9. Long-term Δ 10. Multivariate	Moderate
Disadvantage	13	11	0.85	1. Overall 2. City-level 3. MSA-level 4. Pre CD 5. Crime drop 6. Pre & post CD 7. Total hom 8. Disagg hom 9. Short-term Δ 10. Long-term Δ 11. Multivariate	High
Incarceration	15	10	0.67	1. Overall 2. MSA-level 3. State-level 4. Regional 5. National 6. Crime drop 7. Pre & post CD 8. Total hom 9. Short-term Δ 10. Multivariate	Moderate
Gun Prevalence	14	6	0.43	1. Overall 2. City-level 3. National 4. Pre CD 5. Total hom 6. Multivariate	Low
Racial & Gender Inequality	8	5	0.63	1. Overall 2. City-level 3. Pre & post CD	Low

				4. Total hom 5. Short-term Δ	
Felony Arrest	13	7	0.54	1. Overall 2. County-level 3. National 4. Pre CD 5. Total hom 6. Long-term Δ 7. Multivariate	Low
Divorce/Family Disorganization	14	4	0.29	1. Overall 2. City-level 3. State (-) 4. Total hom	Moderate
Youth Age Structure	15	3	0.20	1. Regional 2. National 3. Long-term Δ	Moderate
Alcohol Consumption	10	3	0.30	1. National 2. Crime drop 3. Long-term Δ	Moderate
Death Penalty	13	1	0.08	1. Crime drop	High
Police Strategy	10	1	0.10	1. National	High
Routine Activities	11	2	0.18	1. National 2. Disagg hom	Moderate
Abortion	7	1	0.14	1. Disagg hom	High
Marriage/ Cohabitation	7	0	0.00		High
Police Size & Expenditures	15	0	0.00		High
Economic Resources	15	2	0.13	1. Pre CD 2. Long-term Δ	High
Domestic Violence Resources	8	0	0.00		High
Sentencing Enhancements	9	1	0.11	1. Long-term Δ	High
Immigration	12	0	0.00		High
Deindust	9	0	0.00		High
Residential Mobility	11	0	0.00		High
Welfare	12	0	0.00		High

Racial Residential Segregation	9	3	0.33	1. MSA-level 2. Crime drop 3. Long-term Δ	Moderate
Drug Markets	11	0	0.00		High
Unemployment	15	1	0.07	1. MSA-level	High
Gun Laws	12	0	0.00		High
Residential Stability	10	0	0.00		High
Education	11	0	0.00		High
Employment	10	0	0.00		High
Adult & Elderly Age Structure	13	1	0.08	1. MSA-level	High
Urbanicity	12	2	0.17	1. City-level 2. Pre CD	Moderate
Prison Conditions	5	0	0.00		High
Population Structure	13	2	0.15	1. State-level 2. Pre CD	High
Poverty	13	1	0.08	1. Pre CD	High

Appendix H

SUMMARY OF EFFECT SIZE ESTIMATES FOR ECONOMIC PREDICTOR DOMAINS

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Inflation	0.204	0.179	3	5	High	Moderate	---	---
Consumer Sentiment	-0.204	-0.186	4	4	High	High	---	---
Income Inequality	0.146	0.303	6	1	High	Low	---	---
Disadvantage	0.143	0.136	8	11	High	High	0.136*	5/11 sig
Economic Resources	-0.035	-0.021	22	28	Low	High	-0.045	3/13 sig
Deindustrialization	0.022	0.020	26	29	Low	High	---	---
Welfare	0.018	0.022	28	27	Low	High	0.031	0/10 sig
Unemployment	-0.012	0.008	31	37	Low	High	-0.031^{+}	5/13 sig
Employment	0.010	0.012	35	33	Low	High	---	---
Poverty	0.001	0.009	40	34	Low	High	-0.001	3/12 sig

[a]Results are slightly different than those presented in Table 4.12 because the Fisher's z values reported in Table 4.12 have been converted back to *r* here to make them more comparable with previous results.

[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).

$^{+}p<.10$; $^{*}p<.05$

Appendix I

SUMMARY OF EFFECT SIZE ESTIMATES FOR FAMILY STRUCTURE PREDICTOR DOMAINS

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Single Parent HH	0.278	0.398	1	2	High	Low	0.398*	1/11
Divorce/Family Disorganization	0.095	0.048	13	18	Moderate	Moderate	0.060	2/12
Marriage/Cohab	0.040	0.027	20	23	Low	High	---	---

[a]Results are slightly different than those presented in Table 4.13 because the Fisher's z values reported in Table 4.13 have been converted back to *r* here to make them more comparable with previous results.
[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).
$^{+}p<.10$; $^{*}p<.05$

Appendix J

SUMMARY OF EFFECT SIZE ESTIMATES FOR AGE STRUCTURE PREDICTOR DOMAINS

Predictor Domain	**Overall M_r**	**Mean M_r Across Models**	**Overal l Rank**	**Updated Rank**	**Overall Strength**	**Overall Stability**	**Multivariate M_r[a]**	**Multivariate Results[b]**
Relative Cohort Size	0.232	0.262	2	3	High	Moderate	---	---
Youth Age Structure	0.081	0.077	14	15	Moderate	Moderate	0.083*	4/13 sig
Abortion	-0.043	-0.049	19	17	Low	High	---	---
Adult/Elderly Age Structure	-0.010	-0.024	36	25	Low	High	0.003	3/13 sig

[a]Results are slightly different than those presented in Table 4.14 because the Fisher's z values reported in Table 4.14 have been converted back to *r* here to make them more comparable with previous results.
[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).
$^{+}p<.10$; $*p<.05$

Appendix K

SUMMARY OF EFFECT SIZE ESTIMATES FOR IMMIGRATION PREDICTOR DOMAIN

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Immigration	0.025	0.023	25	26	Low	High	0.011	1/10 sig

[a]Results are slightly different than those presented in Table 4.15 because the Fisher's z values reported in Table 4.15 have been converted back to *r* here to make them more comparable with previous results.

[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).

$^{+}p<.10$; $^{*}p<.05$

Appendix L

SUMMARY OF EFFECT SIZE ESTIMATES FOR POLICING PREDICTOR DOMAINS

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Felony Arrest	-0.098	-0.128	12	9	Moderate	Low	-0.128*	3/10 sig
Police Strategy	-0.062	-0.079	17	14	Moderate	High	---	---
Police Size & Expenditures	-0.040	-0.032	21	22	Low	High	-0.032*	2/13 sig

[a]Results are slightly different than those presented in Table 4.16 because the Fisher's z values reported in Table 4.16 have been converted back to *r* here to make them more comparable with previous results.

[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).

$^{+}p<.10$; $^{*}p<.05$

Appendix M

SUMMARY OF EFFECT SIZE ESTIMATES FOR CORRECTIONS PREDICTOR DOMAINS

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Incarceration	-0.129	-0.125	9	10	High	Moderate	-0.137*	2/13 sig
Death Penalty	-0.064	-0.054	16	16	Moderate	High	-0.064	2/11 sig
Sentencing Enhancement	-0.028	-0.045	24	19	Moderate	High	---	---
Prison Conditions	0.007	0.008	38	35	Low	High	---	---

[a]Results are slightly different than those presented in Table 4.17 because the Fisher's z values reported in Table 4.17 have been converted back to *r* here to make them more comparable with previous results.

[a]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).

$^{+}p<.10$; $^{*}p<.05$

Appendix N

SUMMARY OF EFFECT SIZE ESTIMATES FOR DRUG MARKET PREDICTOR DOMAIN

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Drug Markets	-0.016	-0.013	30	32	Low	High	0.032	3/9 sig

[a]Results are slightly different than those presented in Table 4.18 because the Fisher's z values reported in Table 4.18 have been converted back to *r* here to make them more comparable with previous results.
[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).
$^{+}p<.10$; $^{*}p<.05$

Appendix O

SUMMARY OF EFFECT SIZE ESTIMATES FOR GUNS PREDICTOR DOMAINS

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Gun Prevalence	0.123	0.165	10	6	Moderate	Low	0.140*	3/11 sig
Gun Laws	-0.012	-0.013	32	31	Low	High	-0.015	0/10 sig

[a]Results are slightly different than those presented in Table 4.19 because the Fisher's z values reported in Table 4.19 have been converted back to *r* here to make them more comparable with previous results.

[b]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).

$^{+}p<.10$; $^{*}p<.05$

Appendix P

SUMMARY OF EFFECT SIZE ESTIMATES FOR THE "OTHER" EXPLANATIONS PREDICTOR DOMAINS

Predictor Domain	Overall M_r	Mean M_r Across Models	Overall Rank	Updated Rank	Overall Strength	Overall Stability	Multivariate M_r[a]	Multivariate Results[b]
Military Involvement	-0.152	-0.131	5	8	High	High	---	---
Racial Heterogeneity	0.144	0.144	7	7	High	Moderate	0.103*	6/13 sig
Racial & Gender Inequality	0.098	0.116	11	12	Moderate	Low	---	---
Alcohol	0.070	0.109	15	13	Moderate	Moderate	---	---
Routine Activities	-0.061	-0.039	18	20	Moderate	Moderate	---	---
Domestic Violence Resources	-0.032	-0.026	23	24	Low	High	---	---
Residential Mobility	0.019	0.008	27	36	Moderate	High	---	---
Racial Residential Segregation	0.018	0.038	29	21	Moderate	Moderate	---	---
Residential Stability	-0.011	-0.016	33	30	Low	High	---	---
Education	0.011	-0.002	34	39	Low	High	---	---
Urbanicity	0.009	0.004	37	38	Low	Moderate	-0.031	4/11 sig

Population Structure	-0.001	0.000	39	40	Low	High	-0.007	1/10 sig

[a]Results are slightly different than those presented in Table 4.20 because the Fisher's z values reported in Table 4.20 have been converted back to *r* here to make them more comparable with previous results.
[a]Numbers refer to the number of significant moderator variables out of the total number of moderator variables tested for a given predictor domain (e.g., 3/10 sig means that 3 of 10 moderator variables in the multivariate analysis significantly impacted the magnitude of the relationship).
$^{+}p<.10$; $^{*}p<.05$