INTRODUCTION

This research seeks 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 definitive conclusion. 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 research aims to make sense of this "crime trends puzzle," by employing a different methodological approach than past syntheses. Specifically, the authors use meta-analytic techniques to offer a comprehensive and systematic statistical assessment of the entirety of the U.S. 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 research explicitly focuses on longitudinal studies of aggregate homicide rates, including almost two more decades of research. As a result, this research 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 are crucial to achieving the goals in this work.

Therefore, the overall aim of this research is 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, we 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 the strength of these relationships.

LITERATURE REVIEW

The section focuses on four common explanations of contemporary homicide trends. These particular explanations have been selected for two main reasons. First, they have received the most attention in contemporary crime trends debates, particularly for their role in the 1990s crime drop, and second, they have been empirically examined the most over time. That is, they are “common” explanations both conceptually and empirically[1]. Table 1 shows 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 broad explanations we chose to focus on has been considered as a plausible and potentially important explanation in previous narrative syntheses of the crime trends literature, justifying its classification as a “common” explanation. This table also serves the purpose of illustrating the vast inconsistencies in conclusions from previous syntheses (with the exception of incarceration). For example, this table reveal that significant debate centers around changes in age structure and policing, 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). Incarceration, however, has had much more consistent support, with every author noting increased incarceration played some role in recent crime trends.

[Table 1 here]

The Economy

1One important omission is the role of drug markets, which has received substantial conceptual attention in the crime trends literature, but has not been empirically tested enough to include here.

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, 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.

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.

In sum, economic conditions have received a great deal of attention in the crime trends literature. 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 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.

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).

There is mixed empirical support for an association between changes in age structure and changes in homicide over time. Researchers tend to find 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.

In sum, while changes in age composition have intuitive appeal based on the correlation in the trends, empirical evidence is lacking.

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 or changes in the quality of policing.

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).

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.

Incarceration

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). 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).

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 (e.g., Roeder et al., 2015).

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 what we consider to be the most prominent and debated 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.

METHODS

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, which is crucial to minimize bias in results (Cooper, 2010). Given our focus, 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, captured the crime drop period (i.e., the 1990s) and were published between 1990 and 2016. 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. A study was considered "longitudinal" if it covered a minimum of a 10 year span and used a longitudinal modeling strategy or accounted for the time dimension in some way. Studies that merely pooled data for an extended period of time, but did not account for the longitudinal nature of the data, were excluded. The homicide trends literature 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 excluded.

In order to ensure a systematic approach to gathering the literature and to minimize the potential for omitting studies, a three-stage search strategy was used to select the included studies. First, searches were conducted in prominent criminology, sociology, criminal justice, and economic peer-reviewed journals from 1990 through 2016 (as available)[2]. Second, keyword searches were conducted in several electronic databases and online sources[3]. Third, published meta-analyses and narrative reviews related to any of the key explanations and aggregate crime rates were searched for additional articles (Petrosino, 1995; Pratt & Cullen, 2005). After initially identifying potential articles for inclusion through the three step procedure, articles that *appeared* to fit the scope conditions were selected for further review and coding.

Dependent Variable: Effect Sizes

2Journals include: *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*. The following journals were searched for a more limited time span (2012-2016): *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*.

3Electronic 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.

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, and often involves a process of standardization. Because most comparisons involve the relationship between two continuous variables, we use the standardized correlation coefficient, *r*. The standardized regression coefficient was used, if provided. When it was not provided, an approximation could usually be computed with available data (e.g., t-statistics; unstandardized regression coefficients).

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). Consistent with other research, we transformed 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. The combined *z*-score was converted back to *r* for the presentation of results (Wilson, 2001).

Analytic Strategy

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. There are several ways to account for this non-independence of observations, including through the use of multi-level modeling techniques.

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.

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. 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.

Because there is significant variation in study design and in the homicide trends literature and because a primary focus of this research is on the impact of methodological variation on the relationship between key explanations and homicide trends, we use the REM. Noting that effect sizes vary within studies as well as between them, we 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

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. Even if measures were conceptually similar, we 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, we 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 could not be combined under the broader predictor domain of family disruption, and instead were separated into two distinct predictor domains.

Independent Variables: 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, information about the study and the model from which the effect size was derived was coded, including information on research design and model specification. These data were used to construct independent variables and examine the impact of methodological variation in 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.

Any number of methodological decisions could impact the results and we acknowledge that other researchers may have chosen different sources of methodological variation. However, we chose to focus on the unit of analysis, time period, and longitudinal research design, as examples because they are considerations that researchers have debated, on both conceptual and methodological grounds.

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 specific explanations (e.g., Baumer, 2008; Baumer et al., 2012; Kelling & Bratton, 2015; Marvell & Moody, 1998; Messner & Tardiff, 1986; Parker, 2008; Rosenfeld & Oliver, 2008). On the other hand, scholars have demonstrated the invariance of certain structural predictors on homicide rates regardless of level of aggregation (Land et al., 1990; Parker et al., 1999; McCall et al., 2010). 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. Furthermore, empirical tests of crime trends are conducted at all levels of aggregation. 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.

Therefore, we systematically assess the impact of 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. 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. To account for level of aggregation, the following units were coded: city, county, MSA, state, region, and nation. Dummy variables for each level were included in the multivariate models, with city-level serving as the reference.

Time Period Covered

The crime drop in the 1990s received an incredible amount of scholarly attention, and 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). Researchers have also demonstrated important differences in the factors impacting homicide trends by time periods (e.g., Parker et al., 2017; 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, we also consider the time period covered. A dummy time period variable was created and coded as 1 if the model only included years capturing the crime drop and post-crime drop years (first year>=1990), and 0 if the model included years *both* leading up to and during/following the 1990s crime drop (last year>1989 and first year<1990). This allows us to compare how the estimated effect size differs for studies examining the crime drop period only from those that examine a longer time span, including times of both homicide increase and homicide decrease in the same analysis (e.g., 1970 to 2010).

Longitudinal Research Design

The final methodological feature we systematically assess concerns the type of longitudinal analysis conducted. Specifically, we 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. 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, 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), number of years covered, and 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. We also included dummy variables for whether the estimate was derived from a model that controlled for each of the following competing explanations: economic conditions, age structure, policing, and incarceration. All variables were grand-mean centered to allow for interpretation of the constant in the multivariate models as the average effect size across studies.

RESULTS

Descriptive Statistics

We begin by providing basic descriptive statistics to provide some background information and contextualize the findings for the reader. The analyses presented in this paper are based on 2,453 effect sizes from 91 peer-reviewed journal articles or book chapters published between 1990 and 2016. On average, the effect size estimates came from studies covering a 29 year time span, including 15 independent variables, and with a sample size of 6,429.

Table 2 presents descriptive statistics for select study design features and reveals important methodological variation across the studies. This table includes a breakdown of the number of estimates and percentage of total estimates for the different sources of methodological variation that will serve as moderator variables in the multivariate models. Table 2 shows that economic conditions are by far the most frequently tested or controlled category, with 93.15% of the effect sizes analyzed 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 closely by estimates that come from models that include some measure of age structure (89.16%). Additionally, over half of the estimates are derived from models that control for criminal justice influences of incarceration (66.57%) or policing (66.53%).

Descriptive statistics also reveal that a majority of the estimates came from models conducted at the state-level (43.99%), followed by the county-level (24.30%), city-level (15.90%), and national-level (10.03%). A smaller handful of estimates came from models tested at the regional-level (4.20%) and very few came from tests at the MSA-level (1.59%). Almost all estimates included in the meta-analysis include *both* the period of increasing homicide rates in the 1980s and the rapid decline in homicide rates in the 1990s (92.54%). Descriptive statistics also reveal that a majority of estimates came from models that analyzed short-term change (76.48%). The impact of these study and model design features on the relative importance of explanations is further explored in the multivariate results presented below.

[Table 2 here]

Step One: 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. To assess the relative effects of several predictor domains on homicide trends, we conduct a series of meta-analyses, estimating the average effect size across each predictor domain (e.g., unemployment and homicide) and then rank-order them by the absolute value of their average effect sizes (Nivette, 2011; Pratt & Cullen, 2005).

Table 3 presents the initial rank-ordering for all 18 established predictor domains. Examining the average effect sizes (Mr) presented in Table 3 reveals that the average effect size for many of the predictor domains explaining temporal trends in homicide is relatively small, with 13 of the 18 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 5 average effect sizes, or those with an average effect size larger than 0.100, range from 0.125 for divorce to 0.248 for single parent households. According to the guidelines in the meta-analysis literature, these would be considered to be substantial effects, and thus, these 5 predictor domains would be considered substantively important factors impacting homicide trends. Although not above 0.100, the predictor domain of felony arrest (Mr=-0.096) is very close to the 0.100 cutoff and is statistically significant. As such, we argue that felony arrest may also be important for future research to include and consider. Therefore, in total, 6 predictor domains are deemed to be critical for our understanding of homicide trends based on this initial assessment[4]. An examination of Table 3 also reveals that several of the predictor domains below the 0.090 threshold do have a statistically significant impact on homicide trends, but the effect is quite small in magnitude. Even these predictor domains with relatively small average effect sizes and those that are non-significant are presented in this initial table, as it is important for the reader to

4To be consistent throughout, future analyses and discussion will also use 0.090 (when statistically significant) instead of 0.100 as the cutoff for considering factors to be "substantively important" and worthy of future discussion.

see how all of the established predictor domains fared, and empirical evidence suggesting which factors *do not* have strong empirical support is also critical to advancing crime trends debates.

[Table 3 here]

Step Two: The 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, methodological variation may help explain some of the inconsistent results and conclusions drawn in the extant literature.

To examine the impact of methodological variation, we use multivariate analyses and include several model design features as "moderator" variables in a three-level random effects model (see also Ousey & Kubrin, 2018; Pratt, 2001; Pratt & Maahs, 1999; Tittle, Villemez, & Smith, 1978 for similar approaches). This allows us 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.

The multivariate analyses include estimation and presentation of results for each predictor domain representing the four most contentiously debated factors presented in earlier sections, as well as three additional predictor domains that emerged as particularly important in the initial analysis.

Economic Conditions

Table 4 presents the multivariate results for the five economic conditions predictor domains. For the economic conditions predictor domains, after controlling for multiple sources of methodological variation, as we observed in previous analyses, disadvantage remains significantly and strongly related to homicide trends. The multivariate analysis also confirms that two of the other economic conditions predictor domains – unemployment and welfare – are significantly associated with homicide trends, but the association is small in magnitude. Furthermore, economic resources are now only marginally related to homicide trends and poverty remains unrelated. The relative unimportance of these four predictor domains is not surprising given how they fared in the initial 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 4, multivariate results reveal that disadvantage is a significantly important predictor of homicide trends, even after controlling for several sources of methodological variation. However, results reveal that controlling for incarceration and unit of analysis both significantly impact the magnitude of the effect size. Specifically, when incarceration is controlled for, the effect size is reduced substantially and the impact of disadvantage practically disappears (b=-0.1583). The relationship is also reduced for studies conducted at the state-level of analysis compared to the city-level (b=-0.2938). None of the other moderator variables have an appreciable impact on the magnitude of this relationship.

Examining the results for the economic resources predictor domain in Model 2 of Table 4 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=0.4092 and b=0.8377, 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=-0.0114), but none of the other moderator variables have an impact.

Results in Model 3 of Table 4 show that poverty is sufficiently conditioned by methodology. Specifically, unit of analysis, time period, and the number of years covered each significantly impact the estimated size (and direction) of the relationship. The impact is particularly strong for studies conducted at the MSA-level (b=0.3255) compared to the city level, and those that consider the 1990s crime drop period only (b=-0.1375) compared to those that examine homicide both before and during the crime drop period.

Model 4 in Table 4 presents the multivariate results for unemployment. The average effect size across studies is very small (b=-0.0512), but evidence suggests that the magnitude of this relationship is also conditioned by the methods used. Specifically, the effect size is more negative when analyzing short-term change (b=-0.2586). Conversely, the average effect size increases when incarceration is controlled for (b=0.1781). 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 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 4 reveal that welfare has a small but statistically significant impact on homicide trends (b=0.0311), and the effect becomes significantly larger when studies are conducted at the national level (b=0.2165) or analyzing short-term change (b=0.1043). There is a slightly more negative effect when age structure is controlled in the same model (b=-0.0976).

[Table 4 here]

Age Structure

Table 5 presents the multivariate models for the two age structure predictor domains. Considering the youth age structure predictor domain, results reveal that the magnitude of the relationship between youth age structure and homicide trends is much larger at the national-level compared to the city-level (b=0.1881). Additionally, publication year exerts a small and negative effect, with a decrease for each year later the article was published (b=-0.0031).

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 regional level studies, studies that control for economic conditions both contributing to a stronger protective effect of adult/elderly age structure on homicide trends (b=-0.1542 and b=-0.2342, respectively). However, the size of the adult/elderly age group actually has a more positive effect when examining the relationship at the county-level (b=0.1272) compared to the city-level.

[Table 5 here]

Policing

The felony arrest model in Table 6 (Model 1) reveals that the effect of felony arrest on homicide trends is relatively large (b=-0.0921), and is not conditioned by any of the moderator variables. Although slightly below the threshold of "substantive importance," these findings suggest that felony arrest may be an important predictor of homicide trends across methodological specifications.

The multivariate results in Table 6 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 (b=-0.0408). As with felony arrest, none of the moderator variables significantly impact the relationship between police size and expenditures and homicide trends. This is consistent with earlier analyses, with police force size and expenditures not surfacing as particularly important in the initial analysis. Taken together, these results suggest it is more about what police do that impacts homicide trends, than sheer numbers or resources.

[Table 6 here]

Incarceration

Table 7 shows the results for the incarceration predictor domain. Here, we see that, as with the initial analysis, incarceration is an important predictor of homicide trends, even after controlling for several sources of methodological variation (b=-0.1159). Table 7 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=-0.4951), consistent with 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 short-term change compared to long-term change (b=-0.1016). Despite arguments about diminishing returns and incarceration, results reveal there are no significant differences between studies examining the crime drop period only or longer time series.

[Table 7 here]

"Other" Important Explanations

Finally, multivariate results for the "other" explanations that emerged as particularly important in the initial analysis are presented in Table 8. Taking a closer look at the impact of methodological variation on two family structure predictor domains, single parent households and divorce, Table 8 reveals that after taking the multiple sources of methodological variation into account, both remain significant predictors of homicide trends, although, on average, the effect of single parent households is much larger (b=0.3353 and b=0.1266, respectively). Notably, the effect of single parent households is not conditioned by any of the moderator variables. However, the effect size is reduced for divorce when considering short-term change compared to long-term change (b=-0.0793).

Even after controlling for several sources of methodological variation, racial composition is also still positively associated with homicide trends (b=0.1316). However, results from the three-level random effects model also reveal that the relationship between racial composition and homicide trends is heavily influenced by unit of analysis, with results stronger at the city-level compared to larger levels of aggregation, including the county-level (b=-0.1946) and the state-level (b=-0.1572), but not impacted by any of the other study design features.

[Table 8 here]

DISCUSSION & CONCLUSION

The results from this analysis have provided empirical evidence to advance the crime trends debate. Specifically, we focus on three key take-aways: 1) the top-ranked predictor domains, 2) how the most common and often-debated explanations fare, and 3) the impact of methodological variation on results.

One of the primary contributions of this research is the identification of the factors that have the strongest empirical support as important predictors of homicide trends. These include 6 predictor domains that both had an average effect size over 0.090 and were statistically significant, and thus, are considered to be "substantively important" to our understanding of homicide trends. An examination of these "Top 6" factors reveals a number of interesting conclusions.

First, four of the Top 6 predictor domains are structural features central to social disorganization theory. Results reveal that single-parent households, disadvantage, racial composition, and divorce 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). Furthermore, the strong findings for disadvantage coupled with the importance of racial composition and the lack of importance of commonly tested economic predictors of unemployment and poverty, suggests that it is the concentrated nature of disadvantage that is most harmful and impactful on homicide trends.

Second, incarceration and felony arrest both also emerged as important predictors of homicide trends and both 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).

Results reveal that, with some important exceptions, several of the explanations that we have devoted increased attention to (i.e., economic conditions, age structure, policing, and incarceration) are actually not receiving strong empirical support or surfacing as important predictors of homicide trends in longitudinal multivariate tests. Results reveal that with the exception of disadvantage, incarceration, and felony arrest, predictor domains tapping these most commonly debated explanations have average effect sizes of less than 0.073. Many of them, however, are actually much lower, including welfare, which has an average effect size of 0.011.

This is not to say that none of the most common and debated explanations matter. To be sure, some of these most common explanations have surfaced as extremely important. Results reveal that disadvantage, incarceration, and felony arrest *are* important in our understanding of homicide trends. But, the initial rank-ordering indicated that the vast majority are not, suggesting that we are spending a lot of time on factors that are not receiving strong support in the empirical literature.

It is clear from the initial rank-ordering that while several of the most heavily debated factors for influencing homicide trends did surface as important (i.e., disadvantage, incarceration, and felony arrest), a number of new and understudied predictor domains also emerged as important (i.e., single parent households, racial composition, and divorce). 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.

We also considered the possibility that the impact of these explanations was masked in some contexts (i.e., that it was sufficiently conditioned by methodology), using multivariate models. That is, we considered whether the various explanations may matter *some* of the time, but not others.

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*. Furthermore, the results obtained from the multivariate models largely echo the results from the initial rank-ordering in terms of the importance of various explanations. That is, even after controlling for the different sources of methodological variation, single parent households, disadvantage, racial composition, incarceration, divorce, and felony arrest *still matter*, and have significant average effect sizes above the 0.090 cutoff. Results also confirm earlier conclusions that many of the economic predictor domains (i.e., economic resources, poverty, unemployment, and welfare) both age structure predictor domains (i.e., youth age structure and adult/elderly age structure), and police force size and expenditures are largely 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.

Second, the results from the multivariate analysis also showed the specific sources of methodological variation for each predictor domain. For example, the effect of incarceration on homicide trends is significantly larger when testing the relationship at the national level. 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. Taken together, the results from the multivariate analyses do provide some support for the impact of methodological variation on some of the inconsistent findings observed in the literature.

As a result of this research, we not only know what the strongest predictors of homicide trends are, but we also now know that empirical support for a number of the most "common" and debated explanations for fluctuations in homicide trends is actually quite weak (e.g., changes in economic conditions, age structure, and police force size). 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., family disruption and racial composition). These findings have several important implications. For example, these results 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.

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. 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.

Results also revealed that racial composition is one of the strongest predictors of homicide trends. This is an important finding, especially when coupled with the findings indicating the importance of single parent households, disadvantage, and divorce. These findings together point to the need to bring social disorganization to the forefront of theoretical explanations in the crime trends literature. 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.

In line with these findings, 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 top-ranked predictor domain. But, incarceration also surfaced as an important predictor of homicide trends. 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).

While these results are informative, this research is not without limitations. The findings 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, 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). 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.

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, may be contextual. As such, these are undoubtedly important empirical questions that still remain and warrant further empirical examination. Despite these limitations, this 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.

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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	⊟	⊟	▭	▭	⊟	⊟	⊟
Age Structure	▭	▭	⊟	▭	⊟	⊟	⊟
Policing	▭	▭	▭	⊟ (size); ▭ (strategy)	▭	⊟	⊟
Incarceration	⊟	⊟	⊟	⊟	⊟	⊟	⊟

[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. Descriptive Statistics of Relevant Study Design Features (n=2453 estimates; 91 studies)

	# of estimates	**% of estimates**
Explanations Included		
Economic Conditions	2285	93.15
Age Structure	2187	89.16
Policing	1632	66.53
Incarceration	1633	66.57
Missing	8	0.33
Unit of Analysis		
City	390	15.90
County	596	24.30
MSA	39	1.59
State	1079	43.99
Region	103	4.20
Nation	246	10.03
Time Period Covered		
Crime Drop Period Only (first year >=1990)	183	7.46
Pre & Post Crime Drop Periods (last year >1989 & first year <1990)	2270	92.54
Longitudinal Type		
Short-Term Change	1876	76.48
Long-Term Change	577	23.52

Table 3. Rank-Ordered Standardized Mean Effect Sizes by Predictor Domain[a]

Rank	**Predictor Domain**	**M_r**		**Rank**	**Predictor Domain**	**M_r**
1	Single Parent HH (34/13)	0.248*		**10**	Economic Resources (198/48)	-0.054*
2	Disadvantage (31/10)	0.162*		**11**	Police Size & Expenditures (233/37)	-0.044*
3	Racial Composition (161/47)	0.157*		**12**	Unemployment (126/32)	-0.026
4	Incarceration (261/35)	-0.141*		**13**	Firearm Legislation (117/14)	-0.022*
5	Divorce (69/20)	0.125*		**14**	Population Structure (113/28)	-0.020
6	Death Penalty (338/15)	-0.098		**15**	Immigration (41/15)	0.015
7	Felony Arrest (103/13)	-0.096*		**16**	Poverty (59/20)	0.014
8	Firearm Availability (52/10)	0.080*		**17**	Adult/Elderly Age Structure (145/19)	-0.012
9	Youth Age Structure (326/55)	0.073*		**18**	Welfare (46/12)	0.011

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

Table 4. Three-level REM for Economic Predictor Domains. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Disadvantage		**Model 2 – Econ Resources**		**Model 3 – Poverty**		**Model 4 – Unemployment**		**Model 5 – Welfare**	
Moderator	Coeff	Z	Coeff	Z	Coeff	Z	Coeff	Z	Coeff	Z
Age Control	0.1358 (0.137)	0.99	-0.0678 (0.065)	-1.05	-0.0464 (0.029)	-1.62	-0.1111 (0.075)	-1.49	-0.0976 (0.041)*	-2.38
Incar Control	-0.1583 (0.069)*	-2.29	0.0825 (0.067)	1.20	0.0667 (0.046)	1.46	0.1781 (0.078)*	2.29	0.0394 (0.036)	1.10
Police Control	0.029 (0.098)	0.30	-0.0385 (0.053)	-0.73	0.0381 (0.028)	1.36	-0.0849 (0.067)	-1.27	0.0567 (0.029)[+]	1.94
County	-0.2140 (0.120)[+]	-1.78	0.0467 (0.080)	0.59	-0.0396 (0.044)	-0.90	-0.0745 (0.130)	-0.57	0.0369 (0.046)	0.81
MSA	---		0.1204 (0.269)	0.45	0.3255 (0.079)*	4.11	-0.0040 (0.173)	-0.02	---	
State	-0.2938 (0.074)*	-3.98	0.0721 (0.080)	0.90	-0.0513 (0.035)	-1.47	-0.1233 (0.095)	-1.30	0.0695 (0.057)	1.23
Region	---		0.4092 (0.168)*	2.44	---		-0.1005 (0.125)	-0.81	---	
Nation	---		0.8377 (0.113)*	7.42	0.0011 (0.084)	0.01	-0.1716 (0.103)[+]	-1.67	0.2165 (0.072)*	2.99
Short-Term Change	0.0799 (0.110)	0.73	0.0111 (0.038)	0.30	-0.0252 (0.027)	-0.92	-0.2586 (0.053)*	-4.87	0.1043 (0.042)*	2.51
Crime Drop Period Only	0.0065 (0.057)	0.11	-0.0413 (0.144)	-0.29	-0.1375 (0.050)*	-2.74	0.2998 (0.157)[+]	1.91	---	
Constant	**0.1546 (0.034)***	**4.62**	**-0.0487 (0.102)[+]**	**-1.89**	**0.0057 (0.011)**	**0.51**	**-0.0512 (0.024)***	**-2.11**	**0.0311 (0.005)***	**5.74**
N/n	31/10		198/48		59/20		126/32		46/12	
BIC	-17.74		-77.76		-169.73		-1.067		-116.01	
Log Likelihood	34.62		86.48		119.52		44.06		86.72	

[a]All variables are grand-mean centered. Models also control for sample size, number of independent variables, number of years covered, and publication year. Results not reported in the table above but are available upon request.
[+]$p<.10$; *$p<.05$

Table 5. 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
Incar Control	0.0257 (0.053)	0.49	0.0651 (0.063)	1.04
Economic Control	0.0132 (0.077)	0.17	-0.2342 (0.047)*	-5.03
Police Control	0.0349 (0.045)	0.78	0.0223 (0.027)	0.84
County	-0.0172 (0.077)	-0.22	0.1272 (0.060)*	2.11
MSA	-0.0238 (0.156)	-0.15	-0.0278 (0.087)	-0.32
State	-0.0297 (0.077)	-0.39	0.0473 (0.029)	1.64
Region	0.0281 (0.109)	0.26	-0.1542 (0.063)*	-2.44
Nation	0.1881 (0.088)*	2.13	0.0679 (0.091)	0.75
Short-Term Change	0.0483 (0.046)	1.05	---	
Crime Drop Period Only	-0.0035 (0.097)	-0.04	-0.0270 (0.038)	-0.72
Constant	**0.0781 (0.016)***	**5.00**	**-0.0009 (0.006)**	**-0.16**
N/n	326/55		145/19	
BIC	198.54		-371.36	
LogLikelihood	-50.08		227.98	

[a]All variables are grand-mean centered. Models also control for sample size, number of independent variables, number of years covered, and publication year. Results not reported in the table above but are available upon request.
[+]p<.10; *p<.05

Table 6. 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
Age Control	0.0679 (0.131)	0.52	0.0488 (0.032)	1.53
Incar Control	-0.0483 (0.157)	-0.31	-0.0370 (0.029)	-1.29
Econ Control	0.0152 (0.171)	0.09	-0.0018 (0.022)	-0.08
County	-0.0327 (0.205)	-0.16	0.0152 (0.026)	0.58
MSA	---		0.0870 (0.086)	1.01
State	0.0335 (0.131)	0.26	-0.0042 (0.017)	-0.24
Region	---		-0.0284 (0.061)	-0.47
Nation	0.0030 (0.270)	0.01	0.0664 (0.049)	1.36
Short-Term Change	0.0939 (0.110)	0.86	-0.0089 (0.023)	-0.38
Crime Drop Period Only	---		-0.0186 (0.050)	-0.37
Constant	**-0.0921 (0.022)***	**-4.26**	**-0.0408 (0.014)***	**-3.03**
N/n	103/13		233/37	
BIC	41.33		-573.43	
LogLikelihood	11.78		335.78	

[a]All variables are grand-mean centered. Models also control for sample size, number of independent variables, number of years covered, and publication year. Results not reported in the table above but are available upon request.
[+]p<.10; *p<.05

Table 7. Three-Level Random Effects Models for Incarceration Predictor Domain. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Incarceration	
Moderator	Coeff	Z
Age Control	0.0586 (0.062)	0.95
Economic Control	-0.0735 (0.061)	-1.21
Police Control	-0.0470 (0.025)[+]	-1.87
County	0.0593 (0.046)	1.30
MSA	---	
State	0.0176 (0.039)	0.45
Region	-0.0399 (0.048)	-0.83
Nation	-0.4951 (0.061)*	-8.06
Short-Term Change	-0.1016 (0.048)*	-2.13
Crime Drop Period Only	-0.0046 (0.048)	-0.10
Constant	**-0.1159 (0.007)***	**-15.70**
N/n	261/35	
BIC	-274.49	
LogLikelihood	184.55	

[a]All variables are grand-mean centered. Models also control for sample size, number of independent variables, number of years covered, and publication year. Results not reported in the table above but are available upon request.
[+]p<.10; *p<.05

Table 8. Three-Level REM for "Other" Predictor Domains that Emerged as Important. Coefficients (Standard Errors) and Z-Scores Reported[a]

	Model 1 – Single Parent HH		**Model 2 – Divorce**		**Model 3 – Racial Composition**	
Moderator	Coeff	Z	Coeff	Z	Coeff	Z
Age Control	-0.0640 (0.595)	-0.11	0.0982 (0.270)	0.36	-0.0632 (0.095)	-0.67
Incar Control	-0.2262 (0.435)	-0.52	-0.0202 (0.085)	-0.24	-0.0859 (0.072)	-1.20
Econ Control	0.4827 (1.146)	0.42	-0.5738 (0.496)	-1.16	-0.0535 (0.523)	-1.02
Police Control	-0.2090 (0.472)	-0.44	-0.0349 (0.114)	-0.31	-0.0280 (0.054)	-0.52
County	-0.0516 (0.523)	-0.10	-0.0591 (0.076)	-0.78	-0.1946 (0.099)*	-1.97
MSA	---		-0.2198	-0.94	0.1719	1.36

			(0.234)		(0.127)	
State	0.1488 (1.140)	0.13	0.0869 (0.074)	1.18	-0.1572 (0.093)*	-1.69
Region	---		---		-0.2442 (0.168)	-1.46
Nation	0.9077 (1.127)	0.81	0.1644 (0.252)	0.65	-0.1444 (0.146)	-0.99
Short-Term Change	-0.0517 (0.544)	-0.09	-0.0793 (0.036)*	-2.22	-0.0543 (0.038)	-1.44
Crime Drop Period Only	0.1918 (0.800)	0.24	0.0732 (0.075)	0.98	-0.0950 (0.117)	-0.81
Constant	**0.3353 (0.065)***	**5.16**	**0.1266 (0.052)***	**2.43**	**0.1316 (0.032)***	**4.10**
N/n	34/13		69/20		161/47	
BIC	90.38		-20.13		-64.66	
LogLikelihood	-15.22		48.17		80.60	

[a]All variables are grand-mean centered. Models also control for sample size, number of independent variables, number of years covered, and publication year. Results not reported in the table above but are available upon request.

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