UNEMPLOYMENT CHANGE AND HOMICIDE: AN EXPLORATION OF THE NATIONAL VIOLENT DEATH REPORTING SYSTEM

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Abstract

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(Under the Direction of David B. Richardson)

Two studies were undertaken as part of this project. We used homicide data collected by CDC's National Violent Death Reporting System (NVDRS), a state-level active surveillance system that provides data on all violent deaths in 16 US states. Data were obtained for 2003-2009. We used the NVDRS, unemployment data from the Bureau of Labor Statistics (BLS), and Current Population Survey (CPS) to estimate associations between unemployment level and homicide through three research aims.

First, we used all NVDRS homicide cases to examine the association between monthly change in unemployment and homicide rates. Information on homicides and population estimates were analyzed by Poisson regression to estimate rate ratios as a function of change in unemployment level over month and quarter in which a homicide event occurred (*Aim 1*). After adjustment for age, gender, race, median household income, and population density, county-level homicide rates increased an average of 2% (Rate ratio = 1.02; 95% CI: 1.00 – 1.05) per percentage point increase in unemployment level over the prior month. Unadjusted rate ratios for unemployment decreases of 2.5 percentage points or greater were dramatically more protective against homicide as compared to any other unemployment decreases (Rate ratio: 0.19; 95% CI: 0.15 – 0.25).

We used a case-crossover design to examine the change in risk of experiencing a workplace homicide as unemployment levels changed over a 1-month period (*Aim 2*). We examined unemployment change data for the month homicide event occurred (case period) and the two months before and after the case period (control periods). Conditional logistic regression models estimated the unemployment change-workplace homicide association across strata of community and victim-level characteristics.

Third, we assessed heterogeneity in the association by characteristics of the victim and workplace violence type (*Aim 3*). A 1-percentage point increase in unemployment over one month was associated with a small increase in the odds of a workplace experiencing a homicide (OR = 1.03; 95% CI = 0.94 – 1.12). County-level population density modified the odds ratio, and homicide risk was heterogeneous among victim race and workplace violence type; however, no measure of the unemployment-workplace homicide association resulted in a statistically significant effect measure.

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LIST OF ABBREVIATIONS

AK	Alaska
BLS	Bureau of Labor Statistics
CA	California
CDC	Center for Disease Control and Prevention
CI	Confidence Interval
COFI	Census of Occupational Fatal Injuries
CO	Colorado
CPS	Current Population Survey
GA	Georgia
KY	Kentucky
LRT	Likelihood Ratio Test
MD	Maryland
MA	Massachusetts
NVDRS	National Violent Death Reporting System
NJ	New Jersey
NM	New Mexico
NC	North Carolina
OK	Oklahoma
OLS	Ordinary Least Squares
OR(a)	Oregon – used solely in naming the state and for descriptive purposes.
OR(b)	Odds ratio – used on tables in the case-crossover analysis
RI	Rhode Island

RR	Rate ratio
SC	South Carolina
UT	Utah
US	United States
VA	Virginia
WHO	World Health Organization
WI	Wisconsin

Chapter 1: Overview

I. INTRODUCTION

Homicide is the second leading cause of violent death in the United States, behind only suicide. Homicide in the workplace is consistently among the top four causes of work-related fatal events for workers in the United States. Homicide events can negatively impact neighbors, cities and towns, and workplaces. Homicide has been linked to various aspects of the community in which one lives, including the racial and ethnic composition, household income, population density, and family structure (1-6).

Unemployment has long been considered a community stressor that contributes to the commission of violent acts at all levels of social aggregation (individual, familial, community, county, etc.) (4-6). However, attention has only been given to unemployment measured at a certain point in time. There is need to explore how a change in the unemployment level over time acts to affect homicide risk at the community-level.

This dissertation research extends the use of unemployment as an explanatory variable and considers the effect of changes in unemployment on homicide risk and rates.

Chapter 2: Literature Review

II. BACKGROUND AND SIGNIFICANCE

A. Introduction

This dissertation examined the association between county-level unemployment and homicide in the general population and in the workplace. This chapter reviews the literature on community level stressors, particularly unemployment, and the risk of crime, violence, and intentional injury. We explore the literature associated with associations between economic influences (such as unemployment) and the commission of violent acts whether toward oneself or others. We conclude this chapter by identifying gaps in the current understanding of the association between unemployment and homicide.

B. Community-level stressors and intentional injury

The use of community-level variables in the examination of individual outcomes is commonplace in a variety of studies that run the gamut of the social sciences. Higher level factors, such as community characteristics and stressors impact (whether it be positively or adversely) lower level outcomes such as personal achievements and proper healthy practices. The influence of community-level stressors is not lost on researchers investigating causes of intentional injury. Stress on individuals, families, and in communities, has been found to be associated with violent crime, suicide, homicide, and other violence. Further, multilevel models are

employed in widespread use, especially when the outcome of interest and its observed and unobserved determinants have a hierarchical structure (7).

Many community-level factors have been associated with stress and violence, including county and community poverty levels, median household income, average age of community members, racial composition, and education levels (1-6). Neighborhood and census blocks that have younger, less educated, and impoverished populations are generally more likely to have workplaces at high risk for violence (8). The same is true for neighborhoods and block groups with higher percentages of Hispanic and non-US born individuals and families, high single-parent, female-headed household, and homes in which children under the age of 18 are not living with the parents. Alternatively, areas whose populations are wealthier, more educated, and less diverse (containing mostly Caucasians) are much less likely to have workplaces that are at higher risk of violence (8).

Emergencies and natural disasters also introduce stress on the community level, and have been found to be associated with increased risk of intentional injury. Keenen et al, examined the incidence of inflicted traumatic brain injury in children after Hurricane Floyd in North Carolina from 1998 through 2001, and found that inflicted brain injury on children increased in the counties most effected by Hurricane Floyd during the six months following the disaster in comparison to the same region pre-disaster (Rate Ratio 5.1, 95% CI: 2.0 – 59.4). Their findings are suggestive of prompt changes in intentional injury that may occur promptly after a stressful community-level event. Other studies of emergency and disaster situations such as Hurricane Hugo, the Loma Prieta earthquake, and the Mt. Saint Helen's eruption

offer information that suggests that communities that are most affected by extreme social situations exhibit the highest rates of psychiatric morbidity, which may lead to the commission of drastic acts such as the infliction of intentional injury (9-11).

C. Unemployment as a community stressor

Unemployment is a well-known and extremely well documented community-level stressor. A common method used by many employers to respond to fluctuations in market demand and economic stress is to temporarily dismiss or “lay off” workers without pay (12-15). In recent decades, most workers who have been laid off have subsequently been rehired by their original employers (12,16). However, neither employees nor employers can predict when or if work will resume. In the time between their dismissal and return to work, the employee may contribute to the unemployment level. When layoffs occur, unemployment levels tend to increase.

From January 2007-2010, as many as 8 million jobs were lost in the United States. During that time 83,301 separate mass layoff events (work dismissals in which at least 50 employees are temporarily dismissed from work) and workplace closings occurred. These layoffs accounted for almost 90% of the total job loss during that time (17). Every US state was affected by this recession, causing sharp fluctuations in local and regional unemployment levels, (18-22). As of May 2012, the national unemployment level had yet to return to the pre-recession levels in 2007 (May 2012 level was 8.2%; before 2007 the yearly average was 4.6%) (23-24).

Unemployment does not occur uniformly among the United States’ population. Blacks and Hispanics have a higher probability of layoff/unemployment

than Caucasians. Hispanics usually experience unemployment 1.5 times that of other adults, while blacks, especially black men, are known to experience unemployment levels two to three times the rate among non-minorities.

As minority workers constitute a large share of blue-collar workers, and blue collar workers tend to experience layoffs more than white collar workers, unemployment levels among them are increased when mass, nationwide recessions occur. Furthermore, minority workers remain unemployed for longer periods or choose to relocate. (20-24).

D. Effects of unemployment on human psychology

Employment status, and unemployment in the household or community in which a person lives, play a role in the psychological state of the individual and the family unit. A considerable body of research supports the assumption that aggressive behavior in a society elevates with increasing levels of unemployment (26-27). This connection is based on the idea that aggression is an immediate reaction to frustration of the pleasure principle (28). Dollard et al. postulated that frustration leads to aggression and, in turn, aggressive behavior can be traced back to a frustration (29-31). Early evidence of frustration with economic stimulants and aggression found significant negative correlations between cotton prices and the lynching of blacks in the south of the United States between 1882 and 1930. Lynching was more likely to occur during periods of economic decline with high rates of unemployment, suggesting that people, and perhaps groups within society are more likely to act aggressively toward other groups (e.g. coworkers), during economically stringent times (32).

The relationship between unemployment and aggression is often described by the parabola function; that is, that at a certain maximum point, the positive relationship between unemployment and aggression is reversed because those who still hold jobs control their aggressive behaviors so that they do not lose them (33). Substantial support has been found for the assumption that aggressive behavior is more common among people who were laid off (34-35). We posit that aggression is a key component in the causal paths leading to homicide. Aggression spills over into violence, which leads to purposeful killing of others.

Based on this theoretical framework and line of research, Fischer developed a study that manipulated the participants' expectations to be unemployed and compared the actual unemployed and employed people with regard to their self-perception and actual aggression. This study noted a interaction between unemployment and self-esteem (as motivators for aggression) and found that participants who received information that it was very likely that they would be unemployed after graduating with their degree, or would continue to be unemployed were they already laid off, experienced higher levels of self-reported aggression than those who were employed and expected to be employed indefinitely. Among those who were highly self-aware and actualized, no effect of unemployment on self-reported aggression was reported (36). Self-awareness and self-actualization have been associated with higher educational attainment, white race, higher income, and being married, all of which are also inversely associated with homicide rates (37). This suggests evaluation of race, age, sex, and income as covariates in our study and potential effect measure modifiers.

E. The effect of the economy on violent acts

Unemployment, especially when occurring at high levels, has been associated with feelings of desperation and even rage (38-39). Such feelings often lead to inter- and intra-personal violence (34-36, 40-41). It is theorized, and supported in the literature, that the personal financial impact and uncertainty caused by unemployment can affect individuals and families in a way that can trigger a violent act (42-47). Unemployment is also known to be associated with intimate partner violence, especially when partners live in the same household (40, 41; 45, 48). It is also associated with familial homicide (killing of one's family members or entire family unit) and abuse of children (43, 49, 50).

Many studies have examined fatal violence as being associated with rising unemployment rate (42, 51-67). Homicide, the killing of a human being by another person (68), has also been associated with rising unemployment levels across all administrative units (census tract, county, state, and nation) and among all races and ethnic groups (69-83).

A parallel line of research has focused on the association between economic factors, particularly unemployment, and another type of violent act, suicide. Most studies of suicide seek to identify the link between joblessness, unemployment, threats to one's job, or psychological abuse (inside or outside of the workplace) and suicide (84-99). One of the more recent studies on this association sought to identify an association between economic fluctuations, including levels of economic activity and volatility of the New York Stock Exchange and monthly rates of death by suicide in New York City (84). This study concluded that the rate of suicide was 0.12

suicides per 100,000 person-years lower when economic activity was at its peak, as opposed to when activity was at its lowest point. Studies of unemployment's effect on suicide warrant attention because the same type of depression-aggression mechanism can trigger these events (84,87,90,94).

Previous studies of homicide and violent acts have focused on the absolute level of unemployment. Studies have found a positive association between unemployment and homicide, suicide, and other forms of violence against people (38, 74, 77, 79, 82-87). The authors of these studies conclude that it is plausible that homicide rates would increase as unemployment levels fluctuate. However, these studies have only measured unemployment across a gradient of rates and exclusively in cross-sectional or time-series designs (38). No study has examined the change in unemployment over time as a main exposure. This dissertation project attempts to address this gap by examining unemployment change as the main exposure and, we hypothesize, will address the temporal relationship between the onset of unemployment and homicide as well as magnitude to which varied levels of unemployment affect homicide risk. Studying the magnitude of change will bring valuable insights into the literature that will help define unemployment's effect on the incidence of homicide and other violent acts.

Falagas et al. performed a systematic review of studies that evaluated mortality in the general population in periods of economic crisis compared with periods prior to or after the crisis. They examined all-cause mortality in the general population and in specific age and sex groups, as well as mortality caused by specific causes, including cardiovascular disease, respiratory infection, chronic liver

disease, transport accidents, and homicide. In all but one of the eight studies, all-cause mortality rose during an economic crisis and fell to a lower rate once the crisis subsided. All of the six studies that reported data specific to homicide indicated that homicide rates rose during economic crisis and fell during times of prosperity (100).

The authors concluded that psychological factors, such as increased levels of stress or depression, are important indirect causes of the excess mortality observed during periods of economic crisis. Such alterations in the psychological status of individuals in periods of economic crisis may derive from uncertainty about the future, as well as from need for adaptation to many changes in life, including work aspects. This conclusion builds on prior work related to unemployment and general uncertainty and mortality.

Hall et al, studied black homicide victims and suspects, in which they examined unemployment as a conduit to self-hate and homicide. Their results suggest that age, unemployment, and prior felony convictions were significant in the explanation of self-hate, aggression, and homicide. The most robust variable in the study was unemployment, black-on-black homicides were 2.5 times more likely to involve victims who were unemployed at the time of the homicide (34).

F. Defining Workplace Violence

Considerable literature has focused on the effects of unemployment and other economic variables on depression, aggression, crime in general, person-to-person violence, and specifically homicide and suicide. One type of violence that has received very little attention as it pertains to an examination of structural and societal predictors is violence which occurs in the workplace. Research on violent workplace

injury has been addressed in the literature of several academic disciplines, including; medicine (101-106), public health (105-110), health and safety (111-117), labor and human resources (118, 119), business and economics (120-122), criminal justice (123-127), and the social and behavioral sciences, including sociology and psychology (128-133). Several epidemiological studies have addressed violence and homicide in the workplace in the past two decades (101-103, 105, 134-136).

Homicide is the second leading cause of death on the job for workers in the United States (43). Most of these assaults occur in service settings such as hospitals, nursing homes, and social service agencies. Most occupational homicides occur during robbery of the workplace (64%) (1-4). While the majority (1.3 million) (6) of workplace violence incidents are considered “minor” assaults, homicide represents nearly 12% of all work related fatalities in the US. The Occupational Safety and Health Administration (OSHA) reports that approximately 2 million people are victims of workplace violence in the U.S. every year (143). According to the U.S. Bureau of Labor Statistics' Census of Fatal Occupational Injury homicides have been among the top four most-common causes of death at work for the past 15 years, with an average of 590 deaths each year from 2000 to 2009 (44). Nearly 1 in 5 on-the-job fatalities result from homicides, almost half of which occur in the South (51). Almost 9% of businesses reporting an incident had no program or policy in place to address workplace violence prevention (55). Factors that place workers at risk for violence in the workplace include interacting with the public, exchanging money, delivering services or goods, working late at night or during early morning hours,

working alone, guarding valuables or property, and dealing with violent people or volatile situations.

In a case-control study of workplace homicide risks, Loomis, et al. found that workplaces having only one worker were at nearly three times the risk of homicide as those with more than one worker (OR = 2.9, 95% CI: 1.2, 7.2). The use of a single nighttime worker in occupations where money is exchanged (e.g. a third shift clerk in a convenience store or gas station, working by themself) resulted in near 400% increase in risk. (OR = 4.9, 95% CI: 2.7, 8.8) (146).

Police officers, corrections officers, and taxi drivers are victimized at the highest rates (138). Approximately 41 percent of all workplace homicides occur in the retail and leisure/hospitality industries (24% in retail, 17% in leisure/hospitality). Most workplace homicides take place in lower wage earning industries, where money is exchanged, where service often takes place into the evening and morning hours, and most often, where workers are alone or separated for lengths of time (1-4) (as with the hotel and transportation industries) (146). The literature suggests that the occurrence of homicide in the workplace is an opportunistic event that occurs at the culmination of a set of component causes, including, but not limited or restricted to, high or rising unemployment and joblessness (1-4, 94-99, 134-136).

1. Typology of workplace violence

Workplace violence can take many forms, ranging from physical assault and homicide, to verbal threats and bullying, or harassment. In its most general term, workplace violence encompasses both physical and non-physical, or psychological, violence. Though much of the previous research has focused on the physical nature

of workplace violence (1-5), an increasing number of researchers are beginning to examine the impact and harm caused by repeated psychological violence, such as sexual harassment, bullying and mobbing (a phenomenon of systemic hostile communication directed at one individual by a group of individuals resulting in social isolation of the targeted individual) (55-58). Psychological violence, especially repeated psychological violence can result in retaliatory actions against the perpetrator, such as murder or assault, or even the infliction of harm on one's self, from the shame or emotional toll of being repeatedly abused (74, 84 130-132). The emergence of bullying-related suicide has come to the forefront via television news broadcasts and special programming, electronic media, and print. It is currently being addressed in the literature (30, 59, 61, 128)

In March of 1995, California OSHA released a landmark document that established guidelines for workplace security. Within that document, they defined three specific types of workplace violence. Each "typology of workplace violence" describes the relationship between the perpetrator and the target of workplace violence. Initially, only the first three types of workplace violence were defined in the Cal/OSHA (139). Later, a fourth type was added by the FBI. The resulting four types are detailed below:

Type 1 - No relationship to workplace: Type 1 workplace violence is characterized by events perpetrated by individuals who have no connection with the workplace or an employee of the workplace (e.g., robbery). A common scenario that would constitute a type 1 workplace homicide is a convenience store robbery where an employee is killed.

Type 2 - Customer/client/patient: Type 2 encompasses violence directed at employees by individuals legitimately using services of the workplace (e.g., customers, clients, patients, students, inmates). Type 2 violence often occurs in healthcare and social services, where the patient is the perpetrated of the violence.

Type 3 - Co-Worker: This type includes violence against coworkers, supervisors, or managers by a present or former employee. An example of this would be an attack on a supervisor or co-worker as a result of a dispute that may or may not be directly related to the job itself.

Type 4 - Personal: This type of workplace violence is defined by violent acts perpetrated by someone who is not an employee, but has a personal relationship with an employee. This type can refer to domestic violence situations and is usually perpetrated by an acquaintance or family member while the employee is at work (139, 140).

Some of the same unemployment-related contextual factors that lead to homicide and suicide in the general population have also been seen to predict homicide in the workplace. All four types of workplace homicide have been found to be associated with economic cycles and unemployment. Workplaces at higher risk for type 1 and type 2 workplace homicide such as bars and nightclubs, convenience stores, pawn brokers, and liquor and jewelry stores, and hotels are all at greater risk of with greater unemployment (3, 8, 54, 103, 132, 134-135,). Many of these establishments are also found in areas of high population density and are often located in neighborhoods with lower socioeconomic status with female-led, or

incomplete family units, all of which are factors that can exacerbate the unemployment-homicide association. Mobile business units such as taxis are also commonly utilized in these types of areas, and most often are operated by individuals who live within high-risk zones. Taxis are at extremely high risk for types 1 and 2 workplace homicide (141). A number of risk factors are known to influence the occurrence of types 1 and 2 workplace violence. They include: contact with the public; exchange of money; delivery or passengers, goods, or services; working alone or in small numbers; working late at night or during early morning hours; and working in high-crime areas (2, 103).

Type 3 workplace violence and homicide are more common in industries that employ a larger number of workers. Such industries include manufacturing, construction, small business, office settings, and other white collar occupations. Type 3 workplace violence can also be triggered by unemployment. When an individual who is already under stress receives news that their job has abruptly ended, or, in some cases, that another individual has received a promotion or bonus in their stead, aggression can result. Most often, in the extreme case of job loss, workplace homicide can result (52-55, 61, 71-74). This is the very essence of type 3 workplace homicide.

Type 4 workplace violence and homicide occurrences are often perpetrated by a spouse, or through gang-related situations, the former of which is associated with unemployment and the economy (131). As unemployment rises, GDP, which has been found to be inversely associated with intimate partner violence, often falls (39, 47, 109). As a result, intimate partner violence occurs, sometimes within the

workplace setting. Hate crimes, and gang-related violence are much more rare in the workplace. No known association has been made between their occurrence and unemployment or other commonly known economic risk factors (46).

G. Worker characteristics and workplace homicide

Recent analyses of national and state surveillance data report differing rates of workplace homicide between genders, with men having homicide rates that are 3.1 to 5.8 times higher than women (111, 149). However, because women comprise a lower percentage of the workforce than do men, homicide makes up a larger percentage of the total work-related injury deaths among women. Approximately 10% to 30% of all male work-related fatal injury deaths are the result of homicide, while 40% to 57% of female work-related deaths are due to homicide (111, 143). Therefore, females are more likely to be murdered on the job than their male counterparts, making workplace homicide the second leading cause of death for female workers. As a result of higher homicide rates, female workers also experience a higher relative risk of dying due to intentional workplace injury compared to unintentional injuries (131, 144). These observed differences in occupational homicide by gender reflect variations in employment patterns by gender as well as hazards by industry.

Minorities and new immigrants also have a disproportionately elevated risk of workplace violence (145). Blacks have a 2.4 times higher workplace homicide rate than employed Caucasians (146). Data from 1996-2000 (the height of workplace violence in the United State) also indicated that workers from minority populations or who were foreign-born face a higher risk of workplace fatal assault than non-

Hispanic Caucasian workers. This may be due to the occupational choices made by certain groups (e.g. taxi drivers from one nationality, gas station and market owner/operators from another). Asian, Native Hawaiian, or Pacific Islanders accounted for over half of the workplace homicide victims, with a rate of 1.83 per 100,000, followed by black workers and Hispanic or Latino workers (6).

Older workers are generally at a higher risk for workplace homicide, especially workers aged 65 and older (111). Reported age-specific work-related homicide rates ranged from 0.4 to 0.9 per 100,000 employed until age 65 when the rates increase to 1.7 to 1.9 per 100,000. Younger workers (those aged 17 years and younger) also appear to have elevated rates (147). Workplaces with only male employees (OR = 3.1, 95% CI: 1.5, 6.5) or with black or Asian employees were also more likely to be killed at work (147).

H. The use of unemployment in epidemiological studies

Unemployment, when measured at an aggregate level as opposed to a description of the employment status of an individual, typically is a measure of the joblessness in a given geographic area, expressed as a percentage. Many studies of the effect of economic conditions on homicide, suicide, and crime have considered the unemployment level in the community and have described these associations for groups defined by demographic factors (34, 35, 50, 52, 67).

Despite its extensive use as a regressor and a covariate, the absolute unemployment level is rarely used as the main exposure in epidemiological studies. The concept of measuring the change in unemployment over time as the main

exposure is even more rare (89). Some studies have examined economic factors such as the change in GDP (65, 96), structural damage to property (broken windows), household income levels (67), and the number of welfare and/or single mother homes within a Block Group or other well-defined geographic region (35, 71-74, 76, 136) as the main explanatory variable. However, none have examined unemployment or unemployment change. This study will examine the change in unemployment over time as the main exposure in an epidemiological study of homicide.

1. Population density and the unemployment-homicide association

Population density has been known to modify the association between unemployment level and homicide (65, 67, 136). Several studies have stratified unemployment and homicide rates by population density (persons per square mile/kilometer) and have found it to be highly correlated with urban living, higher percentages of minorities, familial instability, and below average household income, all of which have been associated with higher levels of unemployment and higher homicide rates. Typically, Block Groups and counties that are more densely populated have higher rates of homicide, especially low-income areas with younger populations of minority racial and ethnic groups. They are more susceptible to street crime and gang activity and constitute a more racially diverse portion of the population, all of which are associated with higher economic inequality, higher percentage of single parent households and government aid recipients, and more severe unemployment levels, due primarily to racial imbalance and a concentration of low-skill and low-wage workers. (8, 54, 74, 79, 134).

I. Economic factors and homicide in the workplace

Two notable studies examined economic factors and homicide in the workplace or the susceptibility of a workplace to homicide. The first study by Ta et al included unemployment as part of a theoretical construct used in the prediction of homicide (8). In that work, Ta et al, examined the socioeconomic factors associated with the presence of workplaces belonging to industries reported to be at high risk for workplace homicide. In this study, the proportion of high risk workplaces was computed following spatial linkage of individual workplaces to 2000 U.S. Census Block Groups (n = 3,925). The study used exploratory factor analysis to summarize thirty census-derived socioeconomic variables into three distinct groups, namely poverty/deprivation, human/economic capital, and transience/instability. Associations between said variables and the propensity of a Block Group to contain those workplaces at higher risk for worker homicide were examined.

The study found that high-risk workplaces were more likely to appear in Block Groups with more poverty/deprivation or transience/instability and less human/economic capital. Each of the three summary social factors was associated with the proportion of workplaces in high-risk industries. Employment, in this case, was grouped into the poverty/deprivation factor. The unadjusted odds of having some, compared to none, proportion of high risk workplaces was greater for Block Groups with above median levels of poverty/deprivation (OR = 1.52, 95% CI: 1.28, 1.80). Contrastingly, above-median levels of human/ economic capital was associated with decreased unadjusted odds (OR = 0.87, 95% CI: 0.73, 1.03) of having some (vs. none) proportion of high risk workplaces. These same patterns

held in the adjusted model with only slight derivation (136).

Though this study does not pertain directly to the association between unemployment and workplace homicide, it does use unemployment as part of a construct used to model the association. The finding of an association between higher poverty and high-risk workplaces being within a Block Group suggests two possibilities: 1) the possibility that unemployment could affect the occurrence of homicide in the workplace, and 2) the possibility that poverty and income could serve as covariates when modeling the unemployment-workplace homicide association.

In a separate study, a significant relationship between unemployment level and workplace homicide was also reported (51). The study used data on occupational assaults from the BLS Census of Fatal Occupational Injuries to calculate the correlation between unemployment rates and occupational homicide rates. Monthly unemployment rates for the regions were correlated with the monthly occupational homicide rates for a four-year analysis period including all year from 1997 to 2000.

The study found a significant correlation coefficient of .258 ($P < .0001$) and concluded that a relationship exists between unemployment and occupational homicides. Increases and decreases in unemployment rates were positively correlated to increases and decreases in the occupational homicide rates.
The scatter diagram in **Chart 1** from Janicak, 2003 depicts the relationship between the monthly occupational homicide rates and the unemployment rates. An examination of the average monthly occupational homicide rates for 1997 to 2000,

indicated that the South Region experienced the highest rate workplace homicide rate, with .059 deaths per 100,000 employed persons and the Midwest Region experienced the lowest rate, with .033 deaths per 100,000 employed persons. The overall average monthly workplace homicide rate for the United States was .041 deaths per 100,000 persons employed during those years. The monthly average occupational homicide rates ranged from a high of .078 deaths per 100,000 employed persons in the South Region occurring in July to a low of .020 deaths per 100,000 employed persons in the Midwest Region for the month of June. This graphical representation of the data in Chart 1 exemplifies the type of work that has been done thus far with the unemployment-homicide association in that it plots the unemployment level at one point in time against a rate at the same point in time. These types of studies do not establish a temporal relationship between unemployment and homicide.

J. Gaps in the current literature

Prior studies have examined the association between unemployment and homicide or suicide in the general population. However, the effect that changes in economic circumstance, especially rapid changes in unemployment, may have on workplace homicide risk is less understood. Open questions include the effect of sudden transient changes in the level of unemployment, such as those that result from mass layoffs, may have on homicide rates or any other violent acts.

The previous literature and theory surrounding unemployment suggest that experiencing a layoff is stressful; and, much of the literature and theory surrounding unemployment indicate that, despite the temporary nature of layoffs, dismissed

employees are prone to feelings of desperation. An interesting observation is that these responses tend to increase as the length of unemployment increases; this may suggest that the sudden transient increases in unemployment may be less important as predictors of homicide than long term unemployment. On the other hand, if the level of unemployment is unusually elevated, as can be the case in times of a severe economic recession then individuals are often unable to perceive or encounter other means by which to provide for themselves and/or their dependents. As unemployment rises and recessions lengthen, questions remain about whether prompt changes in unemployment level impacts homicide rates.

In light of the recent global recession (which occurred from December 2007 – June 2009 , in which 83,000 mass layoff events (those where at least 50 employees as dismissed) and nearly eight million jobs were lost across the United States, it is plausible to hypothesize that drastic and sudden changes in unemployment rates and their widespread occurrence could precipitate the commission of more violent acts on at the regional and national levels. Again, the previous literature supports the likelihood of this result. However, no studies have examined county-level unemployment change as the primary exposure variable in an epidemiological study.

In order to better understand the impact that unemployment, and specifically, changes in the unemployment levels may have on workplace homicides, we propose to undertake a different approach than has been currently employed or suggested. We must begin to understand unemployment as a multi-faceted exposure that can occur suddenly or can be dispersed over time. Does a change in unemployment that is spread out over several months have the same impact as an identical change in

unemployment that occurred in one month, or over the course of a quarter? We address the temporal relationship between unemployment and homicide through both study designs implemented in this dissertation project. The time-series analysis examines the resulting rate change in the general population, while the case-crossover analysis reveals the change in workplace homicide risk that may occur as a result of the same change in unemployment.

K. Conceptualization of the unemployment-homicide association

We developed a conceptual model of the unemployment-homicide association based on our review of the literature. Our interpretation is found in **Figure 2.1.** The temporal sequence is divided into underlying individual and community characteristics that may lead one to commit homicide themself, or may lead to increased homicide rates in a community.

We posit that the characteristics of an individual and the community in which they live can contribute to the inherent level of unemployment they experience and the magnitude with which unemployment levels change in times of economic distress. Both sustained high unemployment and fluctuations in the unemployment level over time can have an effect on the psychology and physical infrastructure of each individual household and the community as a whole. Adverse psychological effects of high unemployment and unemployment change include aggression, depression, anger, and desperation on the part of the individual. When a sufficient number of individuals or families experience these adverse effects, the community is disrupted. Through this model, we posit that violence and crime can result from

one’s personal response to psychological morbidity (e.g. isolated robbery, homicide, familial abuse, or violence against an intimate partner) or from a similar response to similar feels that may result from living in an incohesive, disrupted community. Violence and crime were considered temporal antecedents to homicide and workplace homicide.

We further posit that infrastructure within a community can decay as a result of economic depravity and instability (e.g. high or sharply rising unemployment). Examples of said decay are property value loss, home abandonment, degradation of community reputation, decreases in police surveillance, and worsening of roadway, sidewalk, and recreational area conditions. This decay can cause shifts in community composition and crime levels that could result in increased crime and violence when stressors such as (in our case) high and rising unemployment are introduced.

L. Conclusion

The literature pertaining to unemployment and violence (including workplace violence) includes publications in several academic disciplines. It is well known that unemployment on county, state, and national scales is a risk factor for homicide and other violent acts. Unemployment and other economic factors have been studied extensively and have been determined to play a dynamic role in the stability of individuals, families, and societies. Unemployment has been found to be associated with violent acts and homicide when measured at a single point in time and regressed against the rate and risk of violence.

No previous study has evaluated the association between homicide and fluctuations in local unemployment rates that result from mass layoffs and workplace closings. Studying the magnitude of change brings valuable insight into the literature that will help define unemployment's effect on the incidence of homicide and other violent acts. This dissertation project attempts to address these gaps by examining unemployment change as the main exposure. We will address the temporal relationship between the onset of unemployment and homicide as well as magnitude to which varied levels of unemployment affect homicide risk through two analyses.

Table 2.1: Workplace shootings by industry

Industry	Percentage
Retail Trade	24
Leisure and Hospitality	17
Government	14
Transportation/Warehousing	11
Other Services	6
Financial Activities	6
Professional/Business Services	5
Manufacturing	4
Agriculture	4
Wholesale Trade	3
Construction	2
Other/Not Reported	4

Chart 1. Scatter plot of monthly unemployment rates by monthly homicide rates: 1997-2000

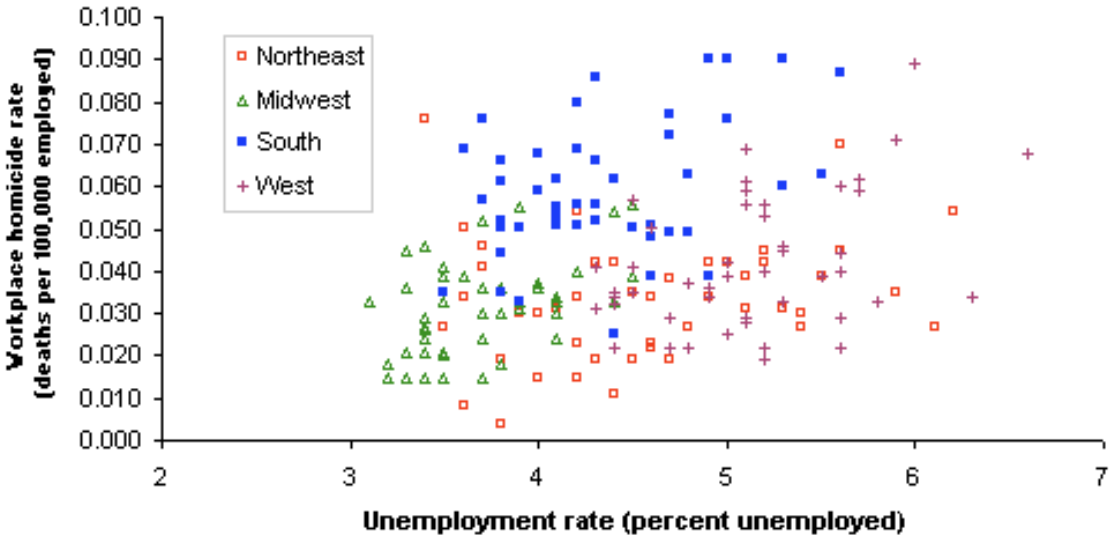
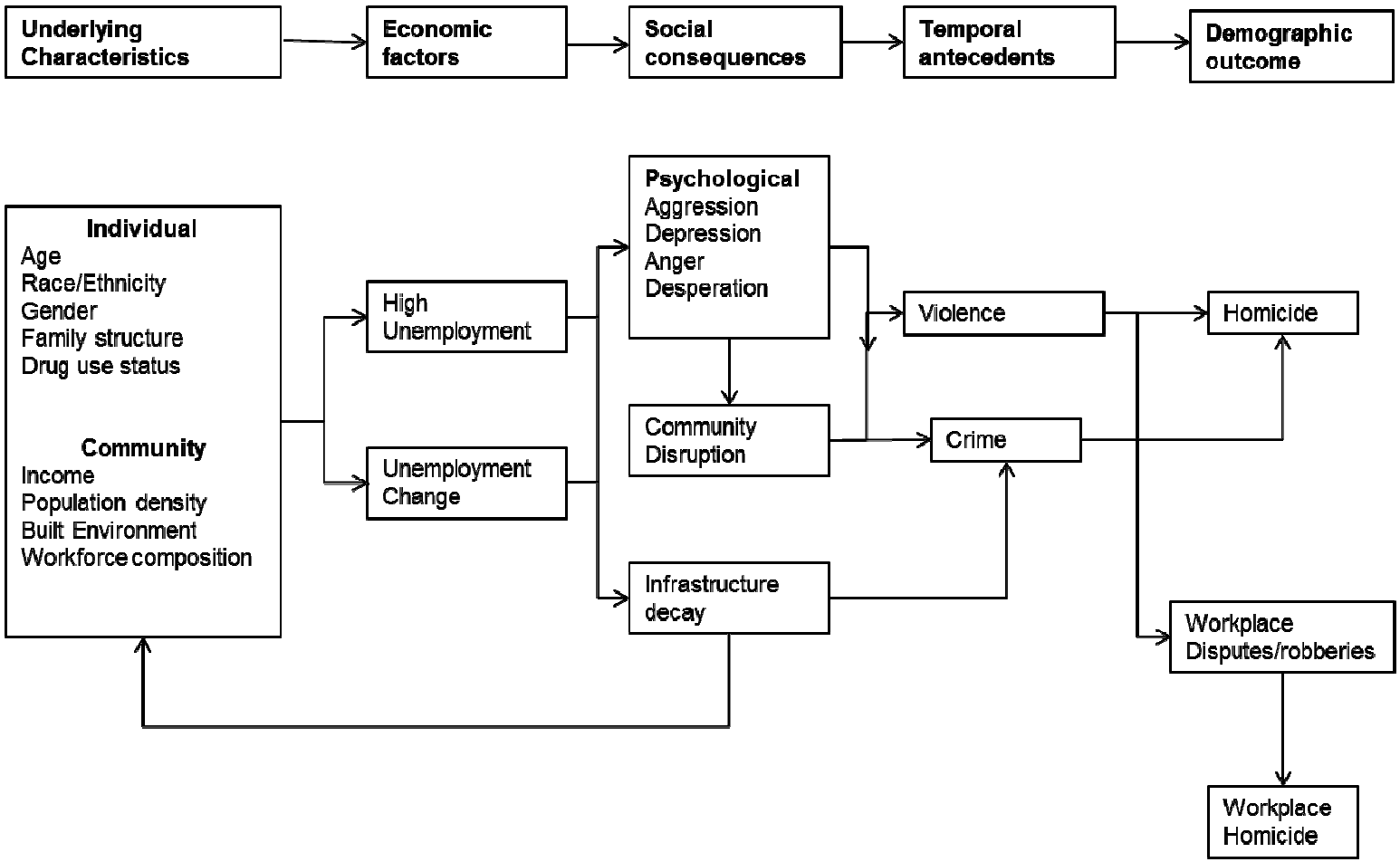


Figure 2.1: Conceptual Model for Unemployment Change and Homicide



Chapter 3: Specific Aims

III. STATEMENT OF SPECIFIC AIMS

This project examines the association between changes in monthly and quarterly unemployment levels and the risk of homicide within the NVDRS reporting region from 2003-2009. We examine the association between unemployment and homicide in the general population, as well as the association with homicide that occurs at work.

Homicide data were obtained for sixteen states (AK, CO, GA, KY, MA, MD, NC, NJ, NM, OK, OR, RI, SC, UT, VA, WI) from the National Violent Death Reporting System (NVDRS) for the years 2003-2009. County-level unemployment levels were obtained from the Bureau of Labor Statistics (BLS) and were used as the main exposure variable in the analyses. We examined the association between changes in county-level unemployment level and homicides outside of the workplace that occurred among adult non-institutionalized men and women residing in the sixteen NVDRS states over period of 2003-2009 (Aim 1). We also examined the association between county-level unemployment level and rates of homicide occurring to employees within workplaces in the 16 NVDRS states from 2003-2009 (Aim 2). Covariates, including victim-level demographic variables, such as age, sex, race/ethnicity, and community-level measures such as median household income

population density, and poverty within county populations were gathered from the U.S. Census Bureau, BLS, and from victim records within the NVDRS.

The project addresses the following specific aims:

Aim 1: Examine the association between monthly change in county-level unemployment levels and change in homicide rates.

This was done using the NVDRS data for **all individual homicide victims,** US Census population estimates, and BLS unemployment information to examine the relationship between homicide rates (per 100,000 population) and changes in unemployment among people residing in the states covered by the NVDRS during the period 2003-2009.This analysis was performed using the entire NVDRS victim population, regardless of whether or not the homicide act occurred within or outside of the workplace.

We use Poisson regression to calculate homicide rates and stratified by whether or not the individual killed was at work. Homicide rates were compared using the rate ratio effect measure. Homicide rates were compared among racial and ethnic groups, between sexes, among age groups, across states and years, and among community-level median household income and population density.

We hypothesized that an increase in the unemployment level over a 1-month (or 3-month) period would result in an increase in the homicide rate. We further hypothesized that the changes in the unemployment level will affect some racial,

age, and population groups differently. We explored effect modification by population density, as has been the case in other studies (55, 57, 134-136).

Aim 2: Estimate the association between unemployment change and homicide at the workplace level

A case-crossover analysis was conducted to investigate whether homicide risk was influenced by change in the county unemployment.

In the case-crossover design control periods are selected at different points in time within a referent window. This is a method for studying the effects of transient exposures on acute events; in this research, it was applied to the study of unemployment change on workplace homicide (149).

Control periods were defined as time periods close in time to a case event and were used in examining the unemployment change-workplace homicide association. Time-invariant covariates such as victim-level age, race, and sex, are assumed to be constant within each risk because of the length of the period of observation; therefore, they do not have a confounding effect within their given risk set so far as this assumption is held.

We hypothesized that an increase in the odds of workplace homicide will occur for every 1.0% increase in 1-month unemployment. We further hypothesized that population density will modify the effect of this 1-month unemployment change.

Aim 3: Assess effect modification of the odds ratio for the unemployment-homicide association among potentially time-varying victim- and county-level covariates for homicide in the workplace.

For this aim, we assessed effect modification by county-level population density and median household income and heterogeneity of the unemployment-homicide association within victim subgroups and event types. Models were compared using the same conditional logistic regression methods that were employed in Aim 2 and the same covariates that were used in Aims 1 and 2. However, this research aim also compared odds ratios among workplace homicide types.

The information we gain through this aim informs as to whether or not there is any exacerbation of the odds ratio due to the presence of one or more county-level effect modifiers and if there is a difference in the unemployment change-workplace homicide association among population subgroups and event types. We hypothesize that there will be modification due to county-level population density. We further hypothesize that the unemployment change-workplace homicide association will differ among workplace homicide types, specifically as it pertains to Type 1 workplace violence. This hypothesis is based on the correlation between unemployment and crime in the general population. Our expectation is that the odds of experiencing a workplace homicide event will be higher among workplaces that are located within areas that may have experienced exceptionally high rates of unemployment change during the study period.

Chapter 4: Methods

IV: METHODS

A. Overview of Methods

We used the National Violent Death Reporting System (NVDRS) data from 2003-2009 for our analyses of the association between unemployment level and homicide. First, we used all NVDRS homicide cases (within and outside of the workplace) to examine homicide rates in relation to changes in county unemployment level (*Aim 1*).

Second, we examined the change in risk of experiencing a homicide in the workplace through a case-crossover study design (*Aim 2*). We gathered unemployment change data for the month in which the homicide event occurred (hazard period) and the months directly before and after the hazard period (control periods). We used conditional logistic regression to model the unemployment change-workplace homicide association. As part of our case-crossover design, we assessed modification of the unemployment change-workplace homicide association by community-level characteristics (population density and median household income) (*Aim 3*) through the use of the likelihood ratio test (LRT) statistic. We also tested for heterogeneity of the odds ratio among victim subgroups (victim race, sex, and age) and workplace homicide typography.

B. Study Designs

This project used two study designs to accomplish its three research aims. A time-series analysis was used to examine the association between monthly change in county-level unemployment levels and change in homicide rates (Aim 1). A case-crossover study design was used to: (a) estimate the odds ratio for a workplace to experience a homicide occurrence as unemployment rates fluctuate across time; and, (b) assess effect modification and heterogeneity of the odds ratio for the unemployment- homicide association among potentially time-varying victim- and county-level covariates for homicide in the workplace.

C. Study Populations

The study population is comprised of all people residing within the NVDRS reporting area from 2003 through 2009 (**Figure 4.1**). The study area includes sixteen states (AK, CO, GA, KY, MA, MD, NC, NJ, NM, OK, OR, RI, SC, UT, VA, WI). Aim 1 examines homicide rates using records for all homicide victims within the NVDRS. Aims 2 and 3 incorporate the workplace homicide occurrences within the NVDRS to examine the change in risk associated with a change in the county-level unemployment level.

D. Data Sources

This dissertation project used several data sources to accomplish its specific aims. They are listed and described below:

1. National Violent Death Reporting System (NVDRS)

The NVDRS is an initiative through CDC whose goals are to: collect and analyze timely, high-quality data for monitoring the magnitude and characteristic of violent deaths at the national, state, and local levels; to ensure that violent death data are routinely and expeditiously disseminated to public health officials, law enforcement officials, policy makers, and the public; to track and facilitate the use of NVDRS data for researching, developing, implementing, and evaluating strategies, programs, and policies designed to prevent violent deaths and injuries at the national, state, and local levels; and to build and strengthen partnerships with organizations and communities at the national, state, and local levels to ensure that data collected are used to prevent violent deaths and injuries. NVDRS is a state-level active surveillance system that provides data on all violent deaths in funded states, including all suicides, homicides, deaths from legal intervention, deaths of undetermined intent, and unintentional firearm fatalities.

The NVDRS reports all victims and alleged perpetrators (suspects) associated with a given incident. NVDRS collects and links data from death certificates, coroner/medical examiner records (CME), and law enforcement/police reports (PR). Data found in CME and PR documents may come from the injury or death scene, ongoing investigations, or accounts of family members or friends. All data in the CME and PR are entered in NVDRS. Official reports from other data sources may also be utilized, and include, but not limited to: Child Fatality Review reports, crime lab results, Supplemental Homicide Reports, hospital discharge data,

court records, and firearm trace data from the Bureau of Alcohol, Tobacco and Firearms.

The system defines a death due to violence as “a death resulting from intentional use of physical force or power against oneself, another person, or against a group or community”, which is the World Health Organization (WHO) definition of violence. The case definition includes suicides, homicides, deaths from legal intervention, deaths from undetermined intent, and unintentional firearm fatalities. For the purposes of this project, homicide is defined as death resulting from intentional use of physical force or power against another person or a group of other individuals. The determination as to whether an event is deemed a homicide, suicide, or other death is made based on a determination by the CME or a classification of death found on law enforcement records.

The NVDRS database includes demographic data for both victims and suspects, victim to suspect relationships, personal victim characteristics such as pregnancy, former and current military service, and homelessness, data on the injury event (e.g. date, time, and place of injury) and the death (e.g. time, place, and cause of death), weapon type, toxicology and wound location findings, and the circumstances that preceded the death. However, not all states have data available for each of the data elements. In addition, quantitative data are supplemented by two written narratives summaries of the death from the coroner/medical examiner file and the police report. Program staff create additional narrative summaries which include information pertaining to the location and type of crime, as well as the characteristics of the victim and suspect (where applicable) and other critical

elements that would serve to identify the circumstances surrounding a homicide event.

Victim records in the NVDRS are organized individually as observations in the data set. In this project's data set, one record represented one person who was a victim of a homicide in the NVDRS states during the years 2003-2009. Observations were assigned victim and incident identification numbers to count the total victims within a state and determine how many victims were in a specific crime (e.g. one person may be victim number 3 in one homicide event). A total of 28,249 victim records were available in the initial data set we received from CDC. Approximately 3% of these homicide victims were injured while the individual was at work (855 records).

A homicide is considered to have occurred "at work" when the victim(s) are at work or working when the event takes place. The designation is taken from the "Injured at Work" item on the death certificate which is completed for all injury victims with the exception of those less than 14 years of age. Workplace homicides can occur at the person's place of work or off-site during the course of work-related activities. In the NVDRS database, workplace homicides are coded dichotomously as at work and not at work.

To compile the "Injured at Work" variable, states are directed to follow identical priority rules which rank data sources in terms of their potential reliability for each data element. The priority rules (also known as primacy rules) for "Injured at Work" dictate the death certificate as the primary source, followed by any additional data that can be taken from the law enforcement write-up, and finally the coroner

and/or medical examiner records, in that order. Completion rates for this variable are high (from any single data source (known for 94.9% of victims) and even higher (known for 97.6% of victims) given the multiple data sources. It is assumed that all states follow the priority/primacy rules as directed, and that each state employs a uniform method for ascertaining the location of the event. According to CDC, in the current system, primacy rules in abstraction and entry of data are applied uniformly across all participating states.

Table 4.1 provides a list of the NVDRS states used in this dissertation, with starting year of surveillance. NVDRS data from the state of California are excluded from the analyses. CDC excludes California data from all data releases for epidemiological studies because these data have only been collected sporadically in a few counties. **Table 4.2** provides an enumeration of homicide events and victims by state in the original data set received from NVDRS.

The NVDRS was initiated in 2003. Since states began to participate in the NVDRS at different times, data do not exist for all years across all states. We do not believe that the staggered start time will affect this analysis of the unemployment-homicide association because the NVDRS data set being used still captures a variety of magnitudes in the unemployment shift such as those that occurred during the global recession of 2007 - 2009. The data set used in this dissertation project includes homicides that occurred through December 31, 2009.

Construction of the analysis data sets for this project first required the merging of the data sources (to the original NVDRS victims list. In order to facilitate data merging, the first task to be undertaken was to form the original data set

received by the NVDRS into a more workable list of homicide victims that contained only complete observations. In order to do this, deletions of a relative few incomplete observations was necessary. The process of cleaning the data set in this way is found below.

A total of 201 incidents were deleted because their incident dates fell outside of the designated study period. Because an incident date, and state and county FIPS codes were essential to merge data and apply exposure and covariate information, observations from the original NVDRS database were excluded from analysis data sets if those values were not present. As a result of missing FIPS codes and incident dates, a total of 323 victims (1.1% of the original data set) were excluded from the final analysis data set. These observations are described in detail in the remaining paragraphs of this subsection.

A total of 1,833 victim observations were initially missing incident dates. To prevent the loss of these incidents, date of death or date pronounced dead for each victim was used as the incident date if no date was recorded. Using these two death dates resulted in only 31 missing observations due to date. By using the death date as a proxy for the incident date, we make the assumption that the victim died on the same day, on the next day, or at least during the same month as the incident. For example, 99% of all homicides in North Carolina result within 30 days of their occurrence (154). A total of 582 observations lacked county FIPS codes. All of these observations were individually inspected and attempts were made to assign county FIPS codes based on the city or place code in their data line. Of those 582, 109 records did not contain a county, state, city, or place code, and were thus eliminated

from the data set. Therefore, 473 observations that did not originally have FIPS were preserved and included in the tentative analysis data set because their records included city and place codes for the site of the incident. These remaining observations were individually examined in order to attempt to assign county codes. Any observation for which city and place codes were not able to produce county FIPS codes were eliminated from the base analysis data set. This further inspection resulted in the elimination of 43 additional victims.

An additional 48 observations were excluded because of missing age, or an improbable age value (e.g. 140 years old). The final base enumeration of victims for the Poisson analysis data set was 27,926.

2. Bureau of Labor Statistics – Local Area Unemployment

The main exposure for both studies is the change in the county-level unemployment over a given period of time. Unemployment rates are calculated based on data obtained from the Current Population Survey (explained in detail below) which surveys households for information pertaining to the previous month at least by 12th day of the following month. The week when the household survey is given is referred to as the reference week.

The Bureau of Labor Statistics (BLS) follows the Current Population Survey definition of unemployment and considers a person unemployed if they had no employment, were available for work, except for temporary illness, and had made specific efforts to find employment some time during the 4-week period ending in the reference week. Individuals who were waiting to be recalled to a job from which they had been laid off are considered unemployed. The unemployment rate (referred to

as the “unemployment level” for this project) is defined as the ratio of unemployed persons to the civilian, noninstitutional labor force expressed as a percent.

The BLS documents changes in unemployment levels as percentages taken to one decimal place. Seasonally-adjusted unemployment rates and rate changes (calculated using unemployment insurance claims) are recorded in publically available BLS databases and contain unemployment data for each state, county, metropolitan areas, and most smaller cities and towns. BLS estimates are considered the gold standard for unemployment measurements.

State and local unemployment data were obtained from the Bureau of Labor Statistics (BLS) Local Area Unemployment database for each county within the NVDRS states (154). These data include monthly measures of the unemployment level (often referred to as the “unemployment rate”), and change from the previous month and quarter from the month measured within a given county on the month being measured.

These unemployment change measurements were merged to the NVDRS data file, by state and county FIPS codes as well as month and year of the homicide incident and constitute the exposure variables of each observation in the final analysis dataset. Changes in unemployment level are given for each month of every year and include the 1-month and 3-month (quarterly) changes for each month on record (e.g. the 1-month change for the month of March indicate the change in unemployment level that occurred from the midpoint of February to March; the quarterly change would be that which occurred from December until March. **Table**

4.3 details variability present in unemployment change measurements for all NVDRS states.

The use of 1-month and 3-month unemployment level changes as the exposure variables allows for an investigation of the hypothesis that homicide rates increase based on the length of unemployment “latency” periods.

3. United States Census Data – Current Population Survey

The United States Census is constitutionally mandated to collect population data on the United States households every ten years. Census counts are considered the “gold standard” for population enumeration. The Current Population Survey (CPS) is a jointly sponsored effort by the U.S. Census Bureau and the BLS that acts as the primary source of labor force statistics for the population of the United Sates.

The CPS is the basis for several important national economic statistics, including the national unemployment level and other economic indicators related to employment and earnings. The CPS is administered by the Census Bureau using a probability selected sample of approximately 60,000 occupied households among 824 independent sample areas across the nation. The CPS includes households from all 50 states and the District of Columbia. Households are surveyed for four consecutive months, are excluded from the survey for eight months, and then return for four additional months before leaving the sample permanently.

Each state sample is tailored to the demographic and labor market conditions in that particular state. Sample sizes are determined by reliability requirements that

are expressed in terms of the coefficient of variation, which is a relative measure of the sampling error that is calculated by dividing the sampling error by the expected value of a given characteristic measured by the survey.

The CPS is a strictly a sample of addresses. The U.S. Census is not able to know who occupies the sample households or even whether the household is occupied or eligible for interview prior to the first contact by a field representative.

The CPS survey methodology is designed to ensure a high degree of sample continuity on a month-to-month basis (as well as over the sample year) while allowing for constant replenishment of the sample without excessive burden to respondents. Surveys are conducted during the calendar week that includes the 19th of a given month. The questions given to respondents refer to activities given during the prior week, hence the reference to the 12th day of the month in the previous section. Each month during the interview week, field representatives and computer assisted telephone interviewers make attempts to contact and interview responsible persons living in each sample unit selected to complete a CPS interview. Households remain in sample for eight months. Therefore, each month, one-eighth enter the sample and one-eighth leave. An introductory letter containing a description of the CPS, offering a guarantee of confidentiality under the Privacy Act, and announcing the upcoming visit by a CPS field representative is sent to each sample household prior to its 1st and 5th months.

The initial interview is typically done in person. During this interview, the field representative determines the eligibility of the household. A household can be

disqualified from the CPS for three reasons. First, addresses that have been converted to permanent businesses, condemned, or demolished, or are outside of the boundaries of the sample area for which it was selected will be classified as Type C, and upon a full supervisory review of the circumstances surrounding the case, will be eliminated from the sample. Type C households are not eligible for interviews in subsequent months because the condition of the household is considered permanent.

Households that are intended for occupancy but are not occupied by an eligible individual(s) are classified as Type B ineligible units. Reasons for such ineligibility may include vacancy of the housing unit or occupancy of the unit by individuals who are not eligible for the survey (e.g. persons whose usual or permanent addresses are elsewhere, or who are enlisted in the Armed Forces). Type B units are eligible for inclusion in future months and are assigned to field representatives in subsequent sampling periods.

Finally, a household falls within Type A ineligibility if no useable data were collected. These households have been determined eligible by the field representative; however, they are not interviewed because the household members refuse to be interviewed, are absent during the interview period, or are unavailable to be interviewed for other reasons. All Type A cases remain in the sample and are assigned for interview in succeeding months. Even if the household initially declines the interview, the field representative must verify that the same household member still resides at the address before determining that a unit is a Type A noninterview.

All Type A classifications undergo a full supervisory review before a final decision is made. CPS representatives make every effort to keep Type A cases to a minimum.

The field representative has the option of conducting subsequent interviews over the telephone, at the approval of the respondent. CPS estimates that 85% of interviews in the second, third, and fourth months are conducted in this manner. Fifth month interviews are used to reestablish rapport with sample households as said interviews occur after an eight month dormancy by the household in the unit.

The response rate for the CPS is generally between 91-92%. Generally, between 4.5 and 5.5% refuse to be surveyed, while 2.5 – 3% of the sample is unable to be contacted. Nonresponse has historically been found to be highest in March. Prior to publication, a geographic adjustment for nonresponse is made at the household level.

The CPS is subject to data loss due to noninterview and nonresponse. To compensate for data loss, the weights of noninterviewing households are distributed among interviewed households. CPS uses three imputation methods to address noninterview and nonresponse issues. Before applying imputation methods CPS data managers merge daily date files and sort the results by state so that missing values are allocated by geographic regions. This ensures that missing values for geographically grouped sampling units receive values from their appropriated regions.

After sorting the data, all out of range or illogical answers are blanked, and imputation is performed to assign values to missing responses and “Don’t know” or

“Refused” responses. CPS edits demographic variables first, followed by labor force data and any other missing variables. All of the various edits performed to data lines are undertaken in a logical sequence, in accordance with the needs of subsequent edits, household edits and codes being addresses first, followed by demographic edits and codes.

The three types of imputation performed are as follows: First, relational imputation infers missing values from the other characteristics on the person’s record or within their household. Second, longitudinal methods are used to impute most of the missing labor force data. If a question is left blank and the interview is taking place in any of the interviewee’s subsequent months, the question is assigned the last month’s entry.

Finally, “hot deck” allocation assigns missing values from a record with similar characteristics. Hot decks are defined by variables such as age, race, and sex. Other characteristics used in hot decks vary and depend on the nature of the unanswered question(s). All CPS items that require imputation have an associated hot deck. Initial values for the hot decks are the ending values from the preceding month. As a record passes through the editing/imputation process, it either donates or receives a value from the hot deck.

Estimates portrayed in the BLS and CPS are based on returns from the entire panel of respondents. Data from each sample person is weighted by the inverse of the probability of the person being in the sample. Such estimation gives a rough measure of the number of actual persons that the sample person represents. Since

1985, most sample persons with the same state have had the same probability of selection. Through a series of estimation steps, the selection probabilities are adjusted for noninterviewers and survey undercoverage. Data from previous months are incorporated into the estimates through the composite estimation procedure.

As part of the CPS, the U.S. Census Bureau provides annual county-level population estimates that represent projected population counts as of July 1st of that year. These estimates are available for each state within the United States and some of the surrounding territories. They were used in the computation of stratified homicide rates in the Poisson analysis and also in the calculation of county-level population density for both the Poisson and case-crossover analyses. Estimates were obtained for the entire county-level population and stratified by race, sex, age, and/or Hispanic origin for each county within the NVDRS.

Specifically to this project, we obtained CPS estimates of socioeconomic indicators that could serve as confounding variables or effect modifiers in both study designs including: county-level median household income, and percent and number of persons living in poverty.

Table 4.4 provides a synopsis of the data elements needed to complete this study. **Table 4.5** details the variable construction and level of measurement for each study variable.

F. Statistical Methods

1. Poisson regression

We constructed a count data set that could be analyzed using Poisson regression to compute homicide rates and rate ratios. We obtained population estimates for each county and state within the NVDRS from 2003-2009. Each county-level population estimate was stratified by year, month, age, race, sex, but also included an enumeration for the total population. Homicide victim counts were tallied and merged into the stratified data set. Rates were then computed for each year/month/age/sex/race combination by dividing the number of cases in a given stratum by the total stratified population. Poisson regression was performed to average rates across unemployment level change strata.

The Poisson analysis uses the number of persons killed by homicide as the numerator. The denominator in Aim 1 is the number of individuals in a given age, race, and gender combination within the county in the victim's county of residence. The denominator is specific to the year and month of occurrence. For ease of interpretation, we report the rate per 100,000 person-years .

Each observation in the NVDRS victims list contained information on the victim's age, race, and sex, as well as the year, month, state, and county in which the homicide event took place. In order to be able to compute homicide rates and rate ratios for all counties within all NVDRS counties, we created a count data set based on the NVDRS database coding. To do this each county in the NVDRS data set was stratified by seven years (2003-2009), twelve months, two sex categories, five race categories, and eighteen 5-year age groups for each county in the data set.

Population estimates from the U.S. Census were merged for each age/sex/race/month/year combination. This created stratified data that were used to compute a rate for every state/county/year/month/age/sex/race.

Two types of problematic observations presented themselves as the Poisson data set was being assembled. First, simple descriptive statistics found that there were 19 observations where a case count was recorded, but the population figure from the U.S. Census for that county was zero. These counties were rural and lacked much racial and ethnic diversity; thus it is possible that the population of certain groups within those counties could have been as little as one small family (approximately 2-5 people). In order to prevent the loss of any complete cases (those with incidence dates and victim ages), we imputed a 1 into the population count for these areas. It is well understood that these observations could be influential to the any rates that are stratified by race and gender. A sensitivity analysis will be performed to evaluate the influence of inclusion of these observations.

Second, there was the problem of homicide victims with missing population counts. These victims were all killed in four rural Alaskan counties where no population numbers were available through the Current Population Survey. However, these observations were deleted from the analysis data set. This decision was made because no approximation of the population count could be made if no data were available for the county in which the event occurred. This situation was seen as different from the zero population issue because, in the case of a zero population, we know that there must have been a reasonably small number of

people of that race and sex in a given county when an event occurred. Imputing a one for the population to replace a missing number could have missed actual population significantly.

Bureau of Labor Statistics' data containing unemployment levels, monthly and quarterly measurements of unemployment, and labor force enumerations for each county in every NVDRS states within the study period also were merged with the list of NVDRS victims. Monthly and quarterly county-level unemployment change was the exposure variable. Unemployment change records were available through the BLS Local Area Unemployment (LAU) database for virtually every county in the NVDRS with one exception (Hanook, Alaska). BLS unemployment records were kept by year, month, state, and county and included a measurement of the non-seasonally adjusted unemployment level for that month, the change from the midpoint of the previous month to the midpoint of the recording month, and the change that occurred from the quarter before the event.

The BLS data were merged with NVDRS by merging on state and county FIPS codes, and year and month of even occurrence. Because of the stratification performed to create the count data set, all BLS information was merged to its corresponding county. This merging resulted in a level of exposure being assigned to virtually all counties and states within the count data set.

Information on poverty, household income, and land area variables from US Census records were also merged with NVDRS. Poverty and income have been examined by several studies of unemployment and violent acts as confounders and effect modifiers. There is a clear association between unemployment and the two

covariates (1-6) that must be examined in order to minimize confounding bias in this study. Median household income and percent (and number) of individuals in poverty on the county level were used as covariates in both the Poisson and case-crossover analyses undertaken in this project. Median household income and number in poverty were measured as whole numbers while percent in poverty was recorded as a percent to one decimal place. For comparison's sake, number in poverty was kept from the final model.

Land area, in squares miles was added to the data set as a variable for the population density covariate. Population density has historically been considered as a covariate in studies of economic factors and violence (8, 135-136). It provides an explanation of the urbanicity of a county or census tract which can be used to compare the demographic profile of a given area and to make inferences concerning the effect of race, ethnicity, and gender on the unemployment-homicide association within density strata. Population density was calculated by dividing the total county population by the number of square miles in the NVDRS county.

Finally, demographic characteristics were included in the NVDRS data set. These were used to provide population counts that acted as denominators for the Poisson regression analysis and, in the case of total population counts, the numerator, for population density calculations. For each year, the U.S. Census Bureau provides midpoint (July 1) population estimates for each county and state in the United States and surrounding territories. These estimates are based on the most recent decennial Census (2000 for this study) and are taken as part of the Current Population Survey.

The final data set included all counties within the NVDRS (except Hanook, Alaska) with demographic and Census information on population counts and socioeconomic information, as well as employment data, and counts of NVDRS victims that provide numerator data for rates and rate ratios. All time and demographic combinations that contained no cases and no population counts were excluded from the analysis data set.

Poisson regression analysis was performed using SAS version 9.2 (155) to accomplish Aim 1. Using the GENMOD procedure with the Poisson distribution and log link we compared homicide rates and calculated homicide rate ratios according to the following model:

$$\log(\text{rate}) = \beta_0 + \beta_1 X_{1(\text{unemployment})} + \beta_2 X_{2\ (\text{confounder})} + \ldots\ \beta_n X_n$$

In which log (rate) is the log of the homicide rate controlling for each covariate, β_1 is the change in the log rate for a one unit change in unemployment. The presence of potential confounding variables was initially assessed by first constructing a directed acyclic graph (DAG). Once confounding variables were selected from the DAG, the absolute value of the confounding rate ratio was computed to compare the rate ratio estimates for the association between unemployment change and being a homicide case from the fully-adjusted model to a model with potential confounder(s) removed. A criterion value ($\geq$0.10 change in estimate) was used to retain potential confounders from the models.

A linear spline term with one knot at zero (indicating no change in unemployment) was introduced into the final statistical model to evaluate any difference in the effect of unemployment for increases as compared to decreases. The results of this analysis did not differ substantially from those presented in Chapter 6 (Manuscript 1) and were less precise. For this reason, they are not given in this document.

2. Case-crossover

The case-crossover study design uses each case as its own control (149,151). Controls may differ from cases in values for the exposure of interest and other time-varying factors.

Thirteen workplace homicides from the original NVDRS data set were excluded due to illogical incident dates. Because we were interested in the effect unemployment level change has on the odds of a homicide event, we chose to only retain the primary victim listed under a specific workplace homicide event. The primary victim was deemed by law enforcement to be the most likely primary target of the crime or the person who attempted to intervene or confront the perpetrator in the event of a criminal act. The primary victim was listed as the first victim within the NVDRS data set under each respective incident. Fifty-one (6%) of the remaining workplace homicides had multiple victims, for which many of the observations' data were extremely limited. Many of the secondary and tertiary victim observations contained insufficient victim-level covariate information to be reliably used within the case-crossover analysis. Victim-level covariate information for the primary victim was retained from the original NVDRS data set in order to test for heterogeneity and modification of the odds ratio. The total number of primary victims comprised the

enumeration of workplace homicide occurrences (cases) in the case-crossover analysis.

Once events were ascertained and exposure and covariate information merged to each, we created control periods for two consecutive monthly intervals before and after a homicide event. In accordance with the design originally set by Maclure (149,151), we matched NVDRS records with four control periods to examine the effect of sharply rising or falling unemployment on the risk of workplace homicide. A total of 775 workplace homicide events were included in the study population of cases.

Table 4.6 compares the original data set received from the NVDRS to the final case-crossover analysis data set. The analysis data set contains workplace homicide cases in approximately the same proportions as the original data set received from the NVDRS. The exclusion of secondary and tertiary (and so forth) victims did not affect the distributions of workplace homicide greatly.

Table 4.7 displays the number of workplace homicides by year in each state. As part of the Restricted Access Data Agreement entered into for this dissertation, we are unable to display cell counts that are less than five and/or isolate cells to a degree that would allow the reader to deduce a cell count in a cell containing less than five events. Cells with zero events are displayed as such and are not given an asterisk. Therefore, values for all year and state stratum with less than five but greater than zero homicides in Table 4.7 have been replaced by an asterisk. Two asterisks have been placed in the cell representing New Jersey for the year 2007 to avoid the deduction of the cell count for New Jersey in 2008. Georgia was the only

state to average more than 25 workplace homicides per year, while Alaska, New Mexico, Rhode Island, and Utah experienced less than 5 workplace homicide deaths each year that they were under surveillance. Several states did not record a workplace homicide in 2009. This lack of reporting hints at a lag in the recording of the “Injured At Work” variable or in homicide reporting as a whole, possibly due to many of these still being under investigation.

An independent review of a subsample of cases was conducted to assess reliability of how workplace homicides were classified according to the workplace typology outlined in Chapter 2, which are summarized below:.

Type 1 - Perpetrator has no relationship to workplace: characterized by events perpetrated by individuals who have no connection with the workplace or employee (e.g., robbery).

Type 2 - Customer/client/patient: violence directed at employees by an individual legitimately using services of the workplace (e.g., customers, clients, patients, students, inmates).

Type 3 - Co-Worker: includes violence against coworkers, supervisors, or managers by a present or former employee.

Type 4 - Personal: violent acts perpetrated by someone who is not an employee, but has a personal relationship with an employee.

The evaluation indicated a relatively high level of inter-rater agreement (Table 4.8). **Table 7.1** reports these classifications for the case-crossover data used for

primary analysis. **Table 4.9** follows the same data protection rules as Table 4.7 and displays the percentages of all NVDRS homicides that were coded as “Injured At Work”. Percentages are rounded to two significant figures. As previously stated, as a whole and across all years, approximately 3% of all NVDRS deaths were coded as being “Injured At Work”. Georgia, South Carolina, and Virginia, three of the four states with the most reported homicides, also coded the highest percentages of these homicides as being “Injured At Work”, surpassing the next closest by nearly an entire percentage point.

In order to assess the level of completeness of ascertainment of workplace homicides within the NVDRS data set in which we rely upon the “Injured At Work” variable to indicate a workplace event, we compared the proportion of all homicides in the NVDR that were classified as workplace homicides to previous reports based upon other data sources. In addition, we compared an estimate of the rate of workplace homicide in the NVDRS to previous reports of workplace homicide rates. **Table 4.10** reports the proportion of all homicide events reported as occurring within the workplace in the US by year. The values are derived as the number of workplace homicides as reported by BLS divided by the total number of homicide events (reported by Bureau of Justice Statistics, Uniform Crime Reporting). A dash represents a state and year in which no workplace homicide was reported. The values for the proportion of workplace homicides in the NVDRS (table 4.7) are similar to those reported in Table 4.9, suggesting that the classification of homicides as occurring at work based upon the “Injured at Work” variable in our data set leads to similar proportions of workplace homicides to those estimated using the BLS and

Uniform Crime Reporting data.

Table 4.11 displays the rate of workplace homicide (per 100,000 labor force) within the NVDRS states over the study period. These rates are calculated as the total number of workplace homicide victims divided by the total number of individuals in the labor force during each designated state and year within the study period. A dash represents a state and year in which no workplace homicide was reported.

Across all NVDRS states and study years, the rate of workplace homicides is consistently below 1.0 per 100,000 labor force. Only in South Carolina in 2003 does the rate exceed 1 per 100,000 labor force.

Table 4.12 reports the rate of workplace homicide per 100,000 labor force within the NVDRS analysis data set. Our data also finds that workplace homicide is a rare phenomenon. As with the rates in Table 4.11, we found that only in South Carolina in 2003 did the workplace homicide rate exceed 1.0 per 100,000 labor force.

Table 4.13 displays the differences between rates calculated through the BLS and Uniform Crime Reporting (Table 4.11) and the rates calculated within the NVDRS analysis data set (Table 4.12) to three digits. These differences were calculated by subtracting cells in Table 4.11 from the same cells in Table 4.12. A negative sign indicates that the rates in our data set were lower than those found through BLS and UCR. Reported rates in Maryland, New Jersey, and Wisconsin were systematically lower than the BLS/UCR across all reported years. Rates were lower in Colorado in all years (2005 was marginally lower – difference: -0.0001). Rates in Georgia were higher for all years reported (2006 was only marginally higher

– difference: 0.00008). Dashes represent our inability to calculate differences based in the unavailability of data either from the BLS/UCR or the NVDRS.

After applying four control periods to each case (3,875 observations total. Unemployment change in control periods represented the change that took place from the midpoint of the month before the control period to the midpoint of the month of the control period. Each case and its four controls were grouped into risk sets for statistical analysis. **Figure 4.2** provides a schematic of the study design framework. The figure represents one risk set. The notches within the referent window timeline represent instances of unemployment measurement (the hazard period and two one-month control periods that straddle each event's hazard period).

Unemployment level change (the exposure variable) was assigned for each case and control according to the event date and dates of control periods introduced by the case-crossover study design. For example, if a homicide event occurred in the second month of the year (February), the case observation in the data set would contain all of the victim-level and county-level characteristics pertaining that case for the month of occurrence as well as a measure of unemployment level change representing the change in unemployment that occurred from the midpoint of January to the midpoint in February. Each case in the case-crossover study was assigned four control observations. Keeping with the example of a February homicide event, the "past" control observation representing the same workplace one month prior to the event's occurrence would contain values for the same victim-level and county-level covariates and measures of the change in unemployment level from the midpoint of December of the previous year to the midpoint in January that

constituted the boundary of the hazard period. Furthermore, the control observation two months in the past would be comprised of the same covariate information with the only change being that the unemployment level for that observation would represent the change from the midpoint of November to the midpoint of December. The same pattern holds for the assigning of “future” controls.

The study design was chosen for two main reasons. First, as victim demographic characteristics (race, age, sex) and county-level community characteristics were found to be confounders of the unemployment-homicide association according to DAG analysis and they were assumed to not change within the narrow referent window, there was no need to consider them as potential confounders in modeling. The study design controls for the statistical influence of potential confounding variables because all characteristics are assumed to be held constant within the period of observation. Such a feature in our case-crossover study lent itself to more parsimonious modeling.

Furthermore, the case-crossover sampling approach excludes subsequent victims from the analysis but still allows for the analysis of effect modification due to workplace- and county-level characteristics that were also susceptible to variation even over the short referent window. These characteristics were uniform or varied only very slightly across all victims involved in the workplace homicide events.

We compared case workplaces to their respective controls using conditional logistic regression to estimate the average change in risk for a given 1-month change in the unemployment level. A 1-month unemployment level change is used because it allows for control by the case-crossover design by limiting change in

covariates in the referent window. It reflects a change from the midpoint of the previous month to the midpoint of the month in which the measure was taken. A 1-month change also captures the short-term effect of unemployment in the smallest time increment currently available.

Conditional logistic regression was used to model the log odds that a given workplace would experience a homicide occurrence based on unemployment level change. Unemployment level changes were entered as a continuous variable for all models. In a case-crossover study, time-fixed covariates (victim: age, gender, race; workplace: county-level population density, percent living in poverty, and median household income) are not considered as confounding variables in the statistical model. Therefore, the model estimates are for the change log odds for a one-unit increase in a given time-varying covariate. .

Regression on unemployment change controlling for each time-varying predictor variable was performed in SAS to compare the odds of a workplace homicide in the presence of that factor with the odds in the absence or varying of that factor. Odds ratios were derived by exponentiating the beta coefficient for each factor in a logistic regression model. Confidence intervals of ninety-five percent indicate the precision of each odds ratio. For each incremental change in the unemployment level exposure variable, we calculated an unadjusted or “crude” odds ratio. Additionally, adjusted odds ratios, representing the effect of level of unemployment increase adjusted for all other variables in the model, were computed using multivariate conditional logistic regression.

Time-varying confounding variables were assessed by examining the estimated association between unemployment level and homicide for the fully-adjusted model to a model with the potential confounder(s) removed. Covariates that resulted in more than a ten percent change in estimate were retained. Only time-varying variables were adjusted for in the model.

When building models to test the unemployment change-homicide association considering community- and victim-level factors, we tested whether a variable modifies the effect of exposure to sharp unemployment change using the likelihood ratio test (LRT). The LRT Chi-square assesses fit of models using the terms representing the interaction between the main exposure (unemployment change) and time-fixed victim and county-level covariates, which, in this study included victim's age, race, sex, county-level population density, median household income, and percent living in poverty. Interaction terms are eliminated from the model as their models do not produce a significant chi-square p-value (defined as <0.05) as compared to the more parsimonious model. Model comparison is performed using the LRT until the model with the least interaction terms but the best predictive ability is found. For each potential effect modifier, we examined the extent to which each variable exacerbates the relationship between the unemployment rate change and homicide (i.e. the extent to which the interaction between two variables departs from multiplicativity, or if it departs at all). We included interaction terms for characteristics for the primary victim including their age, race, and sex, and terms with workplace-level characteristics, including county-level population density, median household income, and percent of population living in poverty. We discerned

the presence of effect modification by population county-level characteristics (population density, median household income, and percent living in poverty) by calculating a likelihood ratio test (LRT) statistic. Interaction terms that did not produce a statistically significant Chi-square value ($p \leq 0.05$) were eliminated from consideration in a final model. We will report the results of this aim by providing the LRT's and odds ratio measurements for each model with and without interaction terms present.

We also used the likelihood ratio test to test for heterogeneity in the unemployment change-workplace homicide association among victim subgroups and workplace homicide types in the case-crossover study. Models that produced a statistically significant p-value ($p<0.05$) indicated that the magnitude of the odds ratio differed among or between categories of the community characteristic or workplace homicide type.

3. Quantification of bias in case-crossover study

Controls were created in our case-crossover study under the premise that the workplace existed during each month of the referent window. If a workplace did not exist during the referent window, but was used as a control in our study, the odds ratio for the unemployment change-workplace homicide association could be influenced in either direction (toward or away from the null), depending on the magnitude of unemployment change in that specific risk set and whether or not the unemployment level rose or fell during the risk set's referent window.

We estimated the potential for bias that may be introduced into our study through closings of case workplaces during their subsequent control periods (a violation of our assumption). BLS houses the frequency of workplace openings, closings, and relocations. We obtained BLS records for all states and all years of the NVDRS in order to evaluate the extent to which workplaces opened and closed during the study period.

Table 7.7 details all openings and closings in the NVDRS states for this study's catchment period as obtained from the BLS. From 2003-2006, workplace openings boomed, reaching a high of 8.6 percent net. Openings dropped dramatically in 2007, reflecting a recessionary period in the US economy. The years 2008 – 2009 saw massive workplace closings, which nearly erased the gains of 2003 – 2007.

The data set used for this study only includes data through December 2009, during which the US economy experienced a 1.6 percent net gain in workplace openings. What can introduce bias into this study is potential situation where the workplace where a homicide event closed within this study's referent window. Such a situation would potentially attenuate our measured odds ratio. If a workplace is not open, it cannot experience a homicide event. It is possible that the odds ratio estimates are biased each year to the degree that case workplaces closed during a control period within a given case's referent window. This potential bias is especially troubling in 2009, where a 12.3 net decrease in workplaces occurred. Such a pattern in closings could skew any trend associated with a change in unemployment level.

H. Protection of data and quality control

We implemented a set of quality assurance procedures as part of this project. These include checks of range and consistency on relevant data fields (e.g. demographics, income levels, unemployment rate changes, etc.), and review of potentially problematic coding decisions performed regularly by myself and the committee chair.

To ensure consistency and correctness of data obtained through CDC, the Census Bureau, and the BLS, all data used in this project were imported from their original databases. All data sets were merged in SAS using common variables, namely county and state FIPS codes. No merges were done manually. After merges took place, frequencies and descriptive statistics were computed for each resulting data set. This was done to notice any irregular data patterns or excess missing observations that may have occurred due to incorrect input or faulty merging through the SAS system.

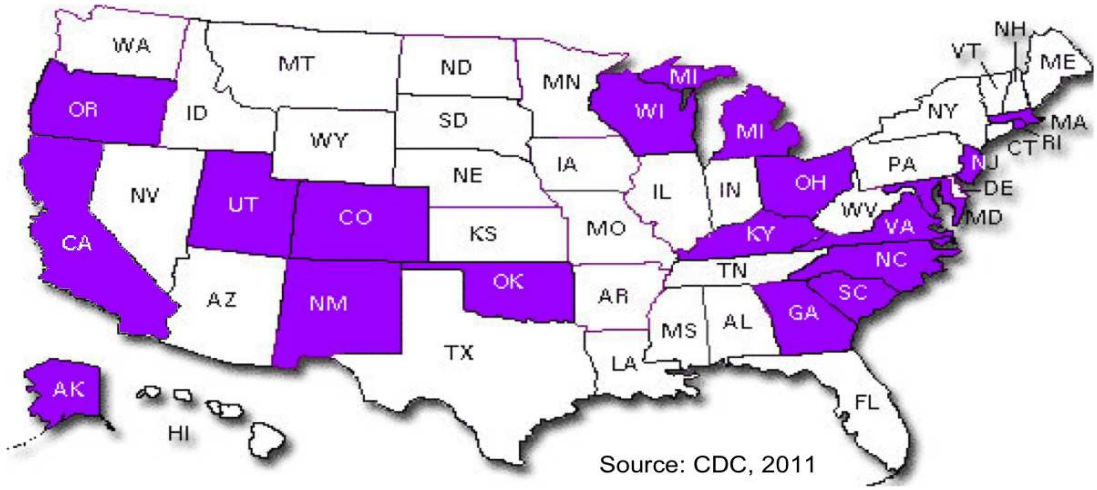


Figure 4.1: NVDRS States

Table 4.1: NVDRS states and years of surveillance

NVDRS State	Counties / Census Areas	Surveillance Start*
Alaska	29	2004
Colorado	64	2004
Georgia	159	2004
Kentucky	120	2005
Maryland	24	2003
Massachusetts	14	2003
New Jersey	21	2003
New Mexico	33	2005
North Carolina	100	2004
Oklahoma	77	2004
Oregon	36	2003
Rhode Island	5	2004
South Carolina	46	2003
Utah	29	2005
Virginia	136	2003
Wisconsin	72	2004

* All States available through 2009

Table 4.2 NVDRS cases by surveillance state

State	Number of victims	%	Total Homicides	%	Workplace Homicide Victims	%
Alaska	269	1.0	162	1.1	8	0.9
Colorado	1,165	4.1	774	5.2	35	4.1
Georgia	4,222	15.0	1,932	12.9	177	20.7
Kentucky	1,009	3.6	615	4.1	28	3.3
Maryland	3,610	12.8	2,361	15.8	68	8.0
Massachusetts	1,248	4.4	732	4.9	37	4.3
New Jersey	2,912	10.3	1,161	7.8	69	8.1
New Mexico	779	2.8	479	3.2	14	1.6
North Carolina	3,826	13.5	1,861	12.5	100	11.7
Oklahoma	1,408	5.0	760	5.1	35	4.1
Oregon	783	2.8	525	3.5	21	2.5
Rhode Island	204	0.7	130	0.9	6	0.7
South Carolina	2,548	9.0	1,232	8.3	99	11.6
Utah	279	1.0	231	1.5	7	0.8
Virginia	2,883	10.2	1,286	8.6	133	15.6
Wisconsin	1,104	3.9	688	4.6	18	2.1
NVDRS Total	28,249	100	14,929	100	855	100

Table 4.3: Variability in 1-month and 3-month unemployment level changes by state, 2003-2009

State	1 - month change						3 - month change					
	Range		Quantiles				Range		Quantiles			
	Min	Max	1%	Median	99%	IQR*	Min	Max	1%	Median	99%	IQR*
Alaska	-14.4	14	-6.8	0	6.8	1.6	-19.3	16.7	-13	-0.1	16.7	4
California	-7.1	7.3	-2.3	0	3.1	0.7	-9.7	12.8	-5.1	0	6.5	1.6
Colorado	-6.1	4.9	-1.8	0	2.1	0.6	-9.7	8.2	-3.2	0	3.8	1.1
Georgia	-9.9	11	-1.5	0	2	0.7	-8.6	10.8	-2	0.1	3.2	1.1
Kentucky	-7	6.8	-2.1	0	2.6	0.9	-7.2	11.9	-3.1	0	5.1	1.8
Maryland	-3.7	4.4	-1.5	0	2	0.6	-7.3	9.6	-3.9	0.1	4.3	1
Massachusetts	-3.9	5.2	-1.7	0	2.3	0.6	-7	8.6	-3.6	0	4.3	1.4
New Jersey	-3.2	3.4	-1.4	0	2	0.7	-6.8	8.1	-3.9	0.1	4.1	1.2
New Mexico	-3.2	7.6	-1.5	0	1.7	0.6	-9	10.8	-2.9	0	3.8	1.1
North Carolina	-5.3	5.3	-1.5	0	2.1	0.7	-8.6	11.3	-2.9	0.1	4.5	1.1
Oklahoma	-4.8	7.2	-1.1	0	1.4	0.6	-7.6	8.1	-1.8	0	2.7	0.8
Oregon	-4	6.3	-2.5	0	2.7	0.9	-7.5	10.8	-4.6	-0.1	5.7	2
Rhode Island	-1.5	2.7	-1.1	0	1.9	0.7	-2.3	4.1	-2	0.2	2.9	1.6
South Carolina	-4	4.3	-1.5	0	2	0.7	-3.9	6.4	-4.7	0	4.9	1.3
Utah	-9.5	13	-5.6	0	6.6	1.2	-9.5	12.6	-5.6	0	6.6	1.2
Virginia	-10.3	9.6	-1.4	0	1.9	0.5	-11	9.9	-2.4	0.1	3.5	0.9
Wisconsin	-5.9	4.5	-2.2	0	2.3	0.9	-6.6	8.9	-3.7	-0.2	4.8	2.1
Entire NVDRS	**-14.3**	**14**	**-2**	**0**	**2.4**	**0.7**	**-19.3**	**16.7**	**-3.7**	**0**	**4.9**	**1.2**

* IQR = Interquartile range

Note: A positive number indicates a rising unemployment level, while a negative number indicates a lowering of the unemployment level.

Table 4.4: Data elements needed for project completion by aim and collection method

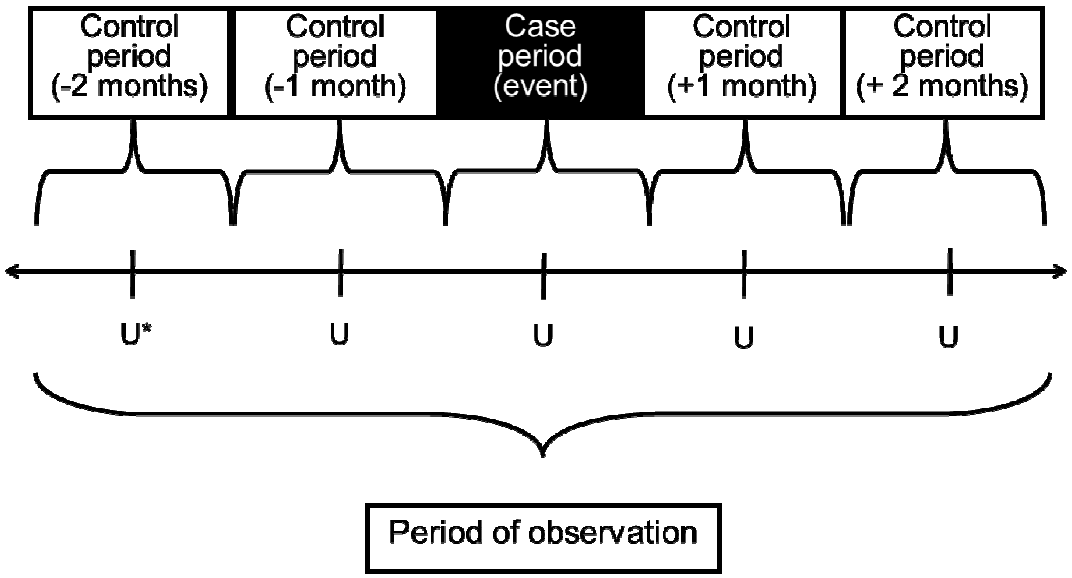
Category (Aims)	Data elements	Collection method
Outcome (All)	All available homicide records from NVDRS.	Obtained through the NVDRS 2003-2009*
Unemployment (All)	County unemployment rates for 2003-2009; changes in unemployment levels by county (monthly, quarterly)	Bureau of Labor Statistics**
County populations (1)	County populations for every county within the NVDRS, stratified by race, age, sex, and Hispanic origin.	U.S Census
Victim characteristics (1 & 2)	Age, sex, race/ethnicity, marital status, occupation, location of event (at work/not), drug use	NVDRS
Community characteristics (All)	Land area (square mi); poverty levels; median household income.	U.S. Census – Current Population Survey**

* Obtained via Restricted Access Data Agreement through CDC.

** Publicly available.

Table 4.5: Analysis variable recodings for analysis data sets

Variable type	Variable	Scale
Exposure	Monthly/Quarterly unemployment rate change	***Continuous***
Victim-level characterisics	Age	***18 five-year categories*** (0-4 to 85+ years)
	Sex	***Categorical*** Male Female
	Race	***Categorical (5 categories):*** White Black American Indian Asian/Pacific Island Other/unknown
	Hispanic Origin	***Categorical (2 categories)*** Hispanic non-Hispanic
	Drug use suspected	**Yes/No**
Community- level characteristics	Percent living in poverty	***Categorical (8 categories)*** 0-4.9 5-9.9 10-14.9 15-19.9 20-24.9 25-29.9 30-35.0 Greater than 35%
	Median Household Income	***Categorical (8 categories)*** Less than $20,000 $ 20,000-29,999 $ 30,000-39,999 $ 40,000-49,999 $ 50,000-59,999 $ 60,000-69,999 $ 70,000-79,999 $ Greater than $80,000
	Population density (persons per square mile)	***Categorical (7 categories)*** Less than 250 250-499 500-749 750-999 1000-1249 1250-1499 Greater than 1500



* U = Measurement of the 1-month change in unemployment level

Figure 4.2 Design framework for 1-month bidirectional control sampling.

Table 4.6 NVDRS workplace homicide victims before and after exclusions

State	Original data set victims	%	Analysis cases	%
Alaska	8	0.9	8	1.0
Colorado	35	4.1	33	4.3
Georgia	177	20.7	164	21.2
Kentucky	28	3.3	23	3.0
Maryland	68	8.0	64	8.3
Massachusetts	37	4.3	34	4.4
New Jersey	69	8.1	69	8.9
New Mexico	14	1.6	10	1.3
North Carolina	100	11.7	94	12.1
Oklahoma	35	4.1	30	3.9
Oregon	21	2.5	18	2.3
Rhode Island	6	0.7	5	0.6
South Carolina	99	11.6	91	11.7
Utah	7	0.8	6	0.8
Virginia	133	15.6	110	14.2
Wisconsin	18	2.1	16	2.1
NVDRS Total	855	100	775	100.0

Table 4.7: Workplace homicides by year and state

State	2003	2004	2005	2006	2007	2008	2009	Total
Alaska	*	*	0	*	*	*	0	8
Colorado	N/A	7	6	9	8	*	0	33
Georgia	N/A	25	28	28	27	26	30	164
Kentucky	N/A	N/A	8	5	*	*	*	23
Maryland	8	4	12	11	12	5	12	64
Massachusetts	*	6	6	7	*	*	6	34
New Jersey	11	13	11	14	**	*	9	69
New Mexico	N/A	N/A	*	*	*	*	0	10
North Carolina	N/A	17	12	16	15	25	9	94
Oklahoma	N/A	6	10	*	5	*	*	30
Oregon	*	*	*	*	*	*	5	18
Rhode Island	N/A	*	*	*	*	*	*	5
South Carolina	22	8	10	19	16	10	6	91
Utah	N/A	N/A	*	0	*	*	0	6
Virginia	19	10	20	19	7	20	15	110
Wisconsin	N/A	6	*	*	*	*	*	16
NVDRS Total	68	106	132	136	117	114	102	775

* Total workplace homicide victims is <5 but greater than 1

** Strata count >5 but less than 9 (actual count not disclosed to prevent deduction by simple math)

Table 4.8: Workplace homicide classifications by workplace homicide typology and rater

Typology	Rater 1	Rater 2
Type 1	14	11
Type 2	3	4
Type 3	2	3
Type 4	3	2
Unknown	3	3
Not in workplace	-	2
Total	25	25

Table 4.9: Proportion of NVDRS deaths coded "Injured at Work" by state and year

State	2003	2004	2005	2006	2007	2008	2009	All years
Alaska	*	*	0.00	*	*	*	0.00	0.03
Colorado	N/A	0.03	0.03	0.05	0.04	*	0.00	0.03
Georgia	N/A	0.04	0.05	0.04	0.03	0.04	0.05	0.04
Kentucky	N/A	N/A	0.04	0.03	*	*	*	0.03
Maryland	0.02	0.01	0.02	0.02	0.02	0.01	0.03	0.02
Massachusetts	*	0.03	0.03	0.05	0.02	0.02	0.03	0.03
New Jersey	0.03	0.03	0.03	0.03	**	*	0.03	0.02
New Mexico	N/A	N/A	0.04	0.02	*	*	0.00	0.02
North Carolina	N/A	0.03	0.02	0.03	0.03	0.04	0.02	0.03
Oklahoma	N/A	0.03	0.06	0.00	0.02	*	*	0.02
Oregon	*	*	*	*	*	*	0.06	0.03
Rhode Island	N/A	*	*	*	*	*	*	0.03
South Carolina	0.07	0.02	0.03	0.05	0.05	0.03	0.02	0.04
Utah	N/A	N/A	*	0.00	*	*	0.00	0.02
Virginia	0.05	0.03	0.05	0.06	0.03	0.06	0.04	0.05
Wisconsin	N/A	0.04	*	*	*	*	*	0.02
All NVDRS	**0.04**	**0.03**	**0.03**	**0.03**	**0.03**	**0.03**	**0.03**	**0.03**

* Proportion derived form less than 5 deaths but greate than 0 deaths

** Proportion derived from strata count >5 but less than 9 deaths

Table 4.10 Proportion of homicide events occuring within the workplace as reported by BLS (numerator), and Uniform Crime Reporting, NVDRS 2003-2009

State	2003	2004	2005	2006	2007	2008	2009
Alaska	0.07	0.00	0.00	0.00	0.00	0.00	0.00
Colorado	0.05	0.04	0.03	0.06	0.06	0.04	0.04
Georgia	0.05	0.03	0.05	0.04	0.03	0.03	0.02
Kentucky	0.03	0.04	0.05	0.03	0.00	0.06	0.05
Maryland	0.02	0.01	0.03	0.04	0.03	0.02	0.03
Massachusetts	0.02	0.03	0.03	0.04	0.03	0.02	0.03
New Jersey	0.03	0.03	0.03	0.03	0.04	0.02	0.05
New Mexico	0.07	0.02	0.05	0.00	0.02	0.03	0.03
North Carolina	0.05	0.03	0.02	0.02	0.04	0.04	0.03
Oklahoma	0.05	0.03	0.04	0.00	0.02	0.01	0.02
Oregon	0.00	0.00	0.04	0.02	0.00	0.05	0.06
Rhode Island	0.00	0.00	0.00	0.00	0.00	0.00	0.00
South Carolina	0.06	0.03	0.03	0.04	0.05	0.03	0.02
Utah	0.00	0.09	0.00	0.00	0.07	0.00	0.00
Virginia	0.05	0.03	0.04	0.05	0.04	0.06	0.05
Wisconsin	0.04	0.07	0.04	0.03	0.05	0.02	0.04

Table 4.11 Workplace homicide rate (per 100,000 labor force) as reported by BLS (numerator), and Uniform Crime Reporting, NVDRS states 2003-2009

State	2003	2004	2005	2006	2007	2008	2009
Alaska	0.90	-	-	-	-	-	-
Colorado	0.40	0.32	0.23	0.41	0.41	0.26	0.29
Georgia	0.75	0.47	0.67	0.59	0.46	0.39	0.23
Kentucky	0.30	0.46	0.55	0.30	-	0.54	0.44
Maryland	0.45	0.21	0.51	0.70	0.57	0.30	0.46
Massachusetts	0.09	0.18	0.15	0.20	0.18	0.12	0.17
New Jersey	0.28	0.32	0.27	0.34	0.38	0.18	0.40
New Mexico	0.91	0.45	0.88	-	0.43	0.42	0.53
North Carolina	0.54	0.40	0.34	0.33	0.58	0.52	0.39
Oklahoma	0.59	0.41	0.47	-	0.35	0.17	0.28
Oregon	-	-	0.22	0.16	-	0.26	0.35
Rhode Island	-	-	-	-	-	-	-
South Carolina	1.06	0.49	0.58	0.76	0.90	0.47	0.37
Utah	-	0.32	-	-	0.37	-	-
Virginia	0.53	0.31	0.53	0.55	0.40	0.51	0.51
Wisconsin	0.26	0.36	0.30	0.16	0.29	0.10	0.19

Table 4.12 Workplace homicide rate (per 100,000 labor force) within the NVDRS analysis data set, 2003-2009.

State	2003	2004	2005	2006	2007	2008	2009
Alaska	0.89	0.29	0.00	0.57	0.28	0.28	0.00
Colorado	N/A	0.28	0.23	0.34	0.30	0.15	0.00
Georgia	N/A	0.56	0.69	0.59	0.58	0.60	0.65
Kentucky	N/A	N/A	0.40	0.30	0.15	0.39	0.15
Maryland	0.31	0.14	0.41	0.37	0.44	0.17	0.39
Massachusetts	0.09	0.18	0.18	0.26	0.09	0.12	0.17
New Jersey	0.25	0.30	0.25	0.31	0.18	0.07	0.20
New Mexico	N/A	N/A	0.77	0.22	0.32	0.21	0.00
North Carolina	N/A	0.40	0.30	0.36	0.40	0.55	0.24
Oklahoma	N/A	0.36	0.70	0.06	0.29	0.23	0.28
Oregon	0.16	0.11	0.11	0.05	0.05	0.26	0.35
Rhode Island	N/A	0.36	0.18	0.17	0.17	0.00	0.18
South Carolina	1.16	0.39	0.58	0.95	0.89	0.47	0.32
Utah	N/A	N/A	0.08	0.00	0.29	0.07	0.00
Virginia	0.60	0.29	0.59	0.55	0.32	0.53	0.41
Wisconsin	N/A	0.23	0.07	0.03	0.10	0.06	0.06

Table 4.13 Differences between rates reported by by BLS and Uniform Crime Reporting and rates within NVDRS data set

State	2003	2004	2005	2006	2007	2008	2009
Alaska	-0.005	-	-	-	-	-	-
Colorado	-	-0.039	0.000	-0.075	-0.112	-0.110	-0.292
Georgia	-	0.090	0.023	0.000	0.124	0.207	0.420
Kentucky	-	-	-0.151	-0.001	-	-0.147	-0.290
Maryland	-0.138	-0.069	-0.101	-0.333	-0.135	-0.132	-0.066
Massachusetts	0.000	0.000	0.029	0.058	-0.088	0.000	0.000
New Jersey	-0.025	-0.024	-0.023	-0.023	-0.203	-0.111	-0.197
New Mexico	-	-	-0.110	-	-0.108	-0.211	-0.533
North Carolina	-	0.001	-0.046	0.023	-0.179	0.023	-0.153
Oklahoma	-	-0.060	0.235	-	-0.058	0.057	0.001
Oregon	-	-	-0.108	-0.106	-	0.000	0.000
Rhode Island	-	-	-	-	-	-	-
South Carolina	0.098	-0.100	0.000	0.189	-0.003	0.000	-0.046
Utah	-	-	-	-	-0.074	-	-
Virginia	0.076	-0.028	0.052	0.000	-0.075	0.025	-0.097
Wisconsin	-	-0.133	-0.231	-0.130	-0.194	-0.033	-0.128

Chapter 5: Descriptive Analyses

V. Descriptive Analysis of the NVDRS data set

We performed descriptive analyses to check for variable completeness, logical values, and develop our final regression models. We calculated various crude and stratified rates for the NVDRS catchment area.

A. Variable Completeness – NVDRS data set

Table 5.1 describes completeness of victim-levels variables in the NVDRS data set. The incident date was not available for 6.48% of NVDRS victims (1,833 individuals). As outlined in Chapter 4: Methods, we used the victim's death date as a proxy for the incident date. Only a small proportion of the covariate data are missing (approximately 2 percent or less). Based on the preliminary statistics presented above, it appears that the exclusion of observations due to incomplete confounder variable information did not play a major role in our study.

Variable completeness fluctuated widely by state. Missing data was more of an issue in Alaska, Oklahoma, Oregon, and Utah. Incidence dates were missing for at least 10% of observations in Alaska, Oklahoma, and Utah. Wisconsin suffered the least from missing data.

B. Descriptive Analysis of Poisson Data

We calculated homicide rates by level of unemployment and by level of unemployment change, as well as by categories of age, race, gender, population density, percent living in poverty, and median household income (**Table 5.2 through Table 5.7).**

Table 5.2 displays crude homicide rates (per 100,000 county-level population) in the NVDRS by state and surveillance year (states who were not under surveillance in a given year display an “N/A”). Overall, the NVDRS states examined in this study experienced 5.62 homicides per 100,000 population from 2003-2009. Utah had the lowest homicide rate overall (2.09 per 100,000 population). The state with the highest homicide rate overall was Maryland (9.23 homicide deaths per 100,000 population). In **Table 5.3,** we examine homicide rates across the NVDRS stratified by month of the year in each state. Homicide rates are 23 percent lower than the average in the month of February than the aggregated mean rate (5.62 per 100,000) – see Table 5.3 marginal value for Total NVDRS and all years.

Table 5.4 shows homicide rates by categories of age, gender, and race. The highest homicide rates occur among individuals between the ages of 20 and 34. Individuals aged 20-24 experience homicide at the highest rates within the NVDRS. Males had higher homicide rates than females. Blacks had the highest homicide rates of all races.

Table 5.5 further stratifies the NVDRS data by age category, race, and gender. Again, homicide rates are higher among males, with a notably high rate

among black males between the ages of 15 to 50. The rate was 93 homicide deaths per 100,000 among black males aged 20 to 24.

Crude homicide rates stratified by economic factors in the NVDRS are found in **Table 5.6**. When stratified by median household income (MHI) we found that homicide rates are inversely related to the amount of money a household (or family) earns. This relationship holds until the lowest income category. At that point, the homicide rate decreases by approximately 25 percent, although the confidence intervals surrounding the rate increases dramatically. As with MHI, we saw a nearly linear relationship until the highest category (Greater than 35% living in poverty). In this category, homicide rates drop, but the standard error increases. Population density presented a very different scenario. We stratified by 250 people per square mile and found that the rate remained almost completely steady until the 1,250 – 1,499 persons per square mile stratum, at which point homicide rates spiked to more than twice those of any of the previous strata.

Table 5.7 further stratifies homicide rates in racial categories by population density and median household income. Reported rates are for the entire NVDRS. We found that black males living in a population density of 1,250-1,499 persons per square mile experienced astounding homicide rates. As with other stratifications by race and gender, black males exhibit extremely high rates of homicide. Extremely low homicide rates (less than 10 deaths per stratum) were found among minority households earning less than $20,000 per year. It could also be the case that income data were not available, or were inaccurate among lower earning minorities when recorded by the Current Population Survey (CPS). CPS is a survey, and poor

minorities' information is often hard to obtain or purely speculation. Regardless of the reasoning, homicide rates were steady at zero for all populations except for male and female whites.

Table 5.1: Variable completeness in the NVDRS data set

Variable	Percent Missing or unknown
Incident date	6.48
Victim variables	
Sex	0.02
Age	0.22
Race	1.43
Hispanic Origin	2.16
County or residence	0.77
Marital Status	2.10
Incident variables	
County of incidence	1.94
Injured at work	1.43

Total victims = 28,249
Total workplace homicides = 855

Table 5.2: Crude homicide rates (per 100,000 person-years) by NVDRS state and Surveillance year

State	Year of surveillance							
	2003	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**All years**
Alaska	N/A	5.86	5.79	6.35	7.22	4.87	3.60	5.61
Colorado	N/A	4.85	4.24	3.73	3.75	3.91	3.88	4.05
Georgia	N/A	7.71	7.22	7.29	8.75	7.63	6.43	7.48
Kentucky	N/A	N/A	5.27	4.69	4.82	4.74	4.38	4.78
Maryland	9.89	9.34	9.77	9.70	9.49	8.95	7.51	9.23
Massachusetts	2.26	2.94	2.87	3.09	2.82	2.65	2.76	2.77
New Jersey	5.15	4.77	5.10	5.38	4.74	6.12	3.99	4.81
New Mexico	N/A	N/A	8.44	6.86	8.48	7.23	8.47	7.90
North Carolina	N/A	7.18	7.86	7.17	7.77	7.06	5.99	7.16
Oklahoma	N/A	6.61	6.43	6.56	6.98	6.84	6.82	6.71
Oregon	2.88	3.52	3.14	3.57	2.61	2.87	2.98	3.08
Rhode Island	N/A	3.32	3.43	3.55	2.60	3.18	3.19	3.21
South Carolina	8.27	8.07	8.30	9.43	8.50	7.92	7.76	8.32
Utah	N/A	N/A	2.56	2.12	2.48	1.64	1.79	2.09
Virginia	5.89	5.41	6.38	5.29	5.48	4.74	4.88	5.43
Wisconsin	N/A	2.90	4.21	3.41	3.47	2.84	2.88	3.28
Total NVDRS	5.65	5.79	5.95	5.73	5.89	5.39	4.99	**5.62**

*N/A = Not surveilled.

Table 5.3: Crude homicide rates (per 100,000 person-years) by NVDRS state and month of surveillance

State	Month of Year											
	Jan	**Feb**	**Mar**	**Apr**	**May**	**June**	**July**	**August**	**Sept**	**Oct**	**Nov**	**Dec**
Alaska	6.75	5.81	7.71	3.09	4.32	4.32	5.25	7.40	5.55	4.94	4.94	7.10
Colorado	3.95	3.01	3.27	4.50	3.95	4.54	4.71	3.99	3.99	4.46	4.58	3.65
Georgia	6.78	6.12	7.06	7.04	7.09	7.58	8.72	8.96	8.12	7.24	7.26	7.77
Kentucky	4.79	3.65	4.56	4.85	5.48	5.36	5.02	4.74	3.94	6.45	4.56	3.94
Maryland	9.82	7.30	8.45	9.29	10.35	9.91	10.10	8.67	9.38	9.07	9.32	9.07
Massachusetts	2.81	2.30	2.44	2.62	2.97	3.19	3.69	3.08	2.54	2.97	2.33	2.28
New Jersey	4.37	3.40	4.85	4.77	5.39	5.09	4.93	5.33	4.71	4.71	4.97	5.19
New Mexico	6.71	8.21	8.46	6.96	8.08	7.00	9.82	9.20	6.71	7.46	6.96	8.95
North Carolina	7.68	5.49	6.69	7.05	7.09	7.68	7.70	7.23	6.87	7.66	7.25	7.54
Oklahoma	6.73	6.38	6.26	7.02	6.38	6.78	8.41	7.08	6.49	6.26	6.49	6.21
Oregon	3.34	2.82	3.10	3.10	3.20	2.72	3.06	2.91	3.63	2.86	3.63	2.58
Rhode Island	1.53	1.92	3.45	2.88	2.49	4.03	4.03	5.56	2.15	4.03	2.68	3.84
South Carolina	8.32	6.37	7.41	7.89	9.08	7.89	9.08	9.84	9.16	8.32	7.89	8.56
Utah	2.12	2.02	2.01	1.93	1.47	1.75	2.21	3.31	2.12	2.67	2.30	1.47
Virginia	5.09	4.34	5.14	6.46	5.61	6.94	5.52	6.03	4.95	4.93	4.82	5.36
Wisconsin	2.64	2.97	3.01	3.19	3.62	3.37	4.13	3.22	3.12	3.88	3.48	2.79
Total NVDRS	**5.49**	**4.53**	**5.28**	**5.62**	**5.82**	**5.97**	**6.26**	**6.10**	**5.57**	**5.70**	**5.53**	**5.58**

Table 5.4: Age-, gender-, and race-stratified crude homicide rates (per 100,000 person-years) in NVDRS.

Group	Rate	95% CI
0 to 4	**3.73**	(3.53 - 3.95)
5 to 9	**0.64**	(0.55 - 0.73)
10 to 14	**0.95**	(0.85 - 1.06)
15 to 19	**8.39**	(8.09 - 8.70)
20 to 24	**14.63**	(14.24 - 15.04)
25 to 29	**12.11**	(11.75 - 12.49)
30 to 34	**9.11**	(8.79 - 9.45)
35 to 39	**7.05**	(6.78 - 7.33)
40 to 44	**5.92**	(5.68 - 6.17)
45 to 49	**5.06**	(4.84 - 5.29)
50 to 54	**4.03**	(3.82 - 4.25)
55 to 59	**3.10**	(2.91 - 3.31)
60 to 64	**2.66**	(2.43 - 2.87)
65 to 69	**2.42**	(2.20 - 2.66)
70 to 74	**2.06**	(1.84 - 2.31)
75 to 79	**1.95**	(1.72 - 2.22)
80 to 84	**2.06**	(1.78 - 2.38)
85 and older	**2.61**	(2.26 - 2.98)
Female	**2.53**	(2.47 - 2.59)
Male	**8.82**	(8.71 - 8.94)
White	**3.02**	(2.97 - 3.08)
Black	**18.43**	(18.14 - 18.73)
American Indian	**9.14**	(8.42 - 9.91)
Asian/PI/Other	**4.51**	(4.21 - 4.83)
Total Population	5.62	(5.55 - 5.69)

Table 5.5: Age-specific homicide rates (per 100,000) by race and gender

	White		Black		American Indian		Asian/Pacific Islander		
Age (years)	**Male**	**Female**	**Male**	**Female**	**Male**	**Female**	**Male**	**Female**	**Total by age**
0 to 4	2.98	2.31	9.37	7.03	8.71	4.16	1.90	2.62	3.73
5 to 9	0.51	0.53	1.18	0.95	0.76	1.18	0.76	0.76	0.64
10 to 14	0.61	0.52	3.40	1.56	0.37	0.78	1.32	0.17	0.95
15 to 19	4.64	1.80	50.07	6.31	9.57	4.28	14.12	1.74	8.39
20 to 24	8.60	2.58	93.05	11.04	25.52	5.19	18.17	3.33	14.63
25 to 29	7.41	2.63	78.90	9.60	30.26	8.39	10.66	2.21	12.11
30 to 34	6.46	2.45	55.01	8.68	26.21	7.95	7.86	1.95	9.11
35 to 39	5.63	2.50	36.31	7.59	17.81	6.17	8.39	2.53	7.05
40 to 44	4.92	2.61	26.50	7.12	23.44	8.14	6.36	2.47	5.92
45 to 49	4.87	2.22	21.26	6.00	13.84	4.95	6.52	1.18	5.06
50 to 54	4.10	1.69	17.06	4.47	10.10	2.07	8.54	1.91	4.03
55 to 59	3.10	1.48	14.66	2.57	5.52	3.17	7.07	1.92	3.10
60 to 64	2.91	1.36	10.55	3.08	9.57	1.75	3.78	1.49	2.66
65 to 69	2.42	1.48	10.06	2.16	9.56	3.63	3.67	2.89	2.42
70 to 74	2.43	1.18	6.72	2.30	5.89	1.57	6.23	0.57	2.06
75 to 79	1.86	1.33	8.65	2.48	14.47	2.15	3.35	2.41	1.95
80 to 84	2.16	1.67	6.76	2.87	0.00	0.00	1.94	1.28	2.06
85 and older	2.92	1.79	19.21	2.27	0.00	6.64	11.39	7.75	2.61

Table 5.6 Crude Homicide rates by economic factors in NVDRS

Economic Factor	Rate	95% CI
Median Household Income		
Less than $20,000	6.24	(2.97 - 13.09)
$ 20,000-29,999	8.42	(7.93 - 8.94)
$ 30,000-39,999	7.39	(7.21 - 7.57)
$ 40,000-49,999	6.36	(6.23 - 6.50)
$ 50,000-59,999	5.72	(5.58 - 5.87)
$ 60,000-69,999	4.86	(4.70 - 5.02)
$ 70,000-79,999	2.32	(2.17 - 2.48)
$ Greater than $80,000	1.81	(1.67 - 1.95)
Percent living in poverty		
0-4.9	1.73	(1.59 - 1.91)
5-9.9	4.14	(4.05 - 4.24)
10-14.9	5.72	(5.61 - 5.84)
15-19.9	7.53	(7.36 - 7.70)
20-24.9	8.78	(8.44 - 9.13)
25-29.9	9.84	(9.16 - 10.58)
30-35.0	9.75	(8.57 - 11.10)
Greater than 35%	8.73	(6.90 - 11.03)
Population density		
Less than 250	5.10	(4.99 - 5.21)
250-499	4.20	(4.07 - 4.37)
500-749	4.35	(4.17 - 4.53)
750-999	4.86	(4.62 - 5.11)
1,000-1,249	4.41	(4.13 - 4.70)
1,250-1,499	11.28	(10.85 - 11.73)
Greater than 1,500	6.91	(6.77 - 7.06)
Total	5.62	(5.55 - 5.69)

Table 5.7: Race- and gender-specific homicide rates by county-level median household income (MHI) and population density

County-level characteristic	Race Category							
	White		Black		American Indian		Asian/Pacific Islander	
	Male	Female	Male	Female	Male	Female	Male	Female
MHI								
Less than $20,000	10.89	4.91	0.00	0.00	0.00	0.00	0.00	0.00
$ 20,000-29,999	8.42	3.10	22.28	5.30	24.60	6.71	26.40	2.57
$ 30,000-39,999	6.12	2.75	30.05	5.90	17.31	4.68	17.82	2.89
$ 40,000-49,999	5.07	2.19	34.32	6.14	13.05	4.45	11.82	2.84
$ 50,000-59,999	3.66	1.56	42.79	6.40	7.00	3.18	8.34	2.31
$ 60,000-69,999	2.98	1.38	34.52	5.44	7.76	3.72	7.35	1.86
$ 70,000-79,999	1.80	0.97	16.62	3.48	8.84	3.30	1.88	1.28
Greater than $80,000	1.84	0.94	8.72	2.38	1.68	1.72	1.62	1.09
Population Density								
Less than 250	4.77	2.28	22.20	5.15	15.96	5.03	11.54	2.84
250 - 499	3.54	1.81	24.89	5.01	7.35	4.49	7.38	2.37
500 - 749	3.62	1.68	24.20	4.86	14.26	3.66	7.45	3.09
750 - 999	3.92	1.68	29.32	5.93	11.39	3.22	12.04	1.50
1000 - 1249	3.65	1.54	26.80	4.18	9.94	1.92	3.87	0.46
1250 - 1499	3.62	1.68	136.28	14.49	14.84	5.97	10.01	2.84
Greater than 1500	4.37	1.48	36.34	5.82	7.87	0.95	6.01	1.67

Chapter 6: Aim 1 Results (Manuscript 1)

VI. Change in unemployment level and homicide in 16 US states

A. Introduction

The association between unemployment and crime is a recurrent one in popular literature and political discourse, to such an extent this association is sometimes perceived to be a truism (8-35). For an individual, loss of employment has been associated with feelings of desperation and even rage, suggesting a psychological mechanism that might result in an association between unemployment and violent crime (26, 27). Feelings of desperation can lead to inter- and intra-personal violence (22-24, 28-29). It is theorized that the personal financial impact and uncertainty caused by unemployment can affect individuals and families in a way that can trigger a violent act through increased acts of crime and aggression (30-35).

However, the effects of unemployment may also operate on a community level. Large changes in unemployment may negatively impact an entire community. It is theorized that community-level stressors play a role in the commission of a violent act. Community-level factors (including poverty, median household income, and average age of community members) have been found to be associated with

some types of violence (1-6). A large change in the unemployment level may impact a community in ways that are similar to other external events that appear to be outside the control of the community. For example, the stress introduced by natural disasters and emergency situations has been associated with psychological outcomes that may lead to the perpetration of violence (9-11).

The empirical support for the contention that changes in unemployment in a community lead to criminal behavior, and specifically to homicide, is inconsistent (42, 51-67, 157). Property crimes tend to show a positive association with unemployment; however, it is not clear that a rise in unemployment signals a rise in the rate of violent crimes such as homicide. There is evidence that economic distress and unemployment may increase the risk of self-harm (84-99). For example, a recent study suggested that the rate of suicide in NYC was 0.12 suicides per 100,000 person-years lower when economic activity was at its peak, as opposed to when activity was at its lowest point (84, 87, 90, 94).

Substantial support has been found for the assumption that aggressive behavior is more common among people who have recently been laid off, especially those who become or remain jobless in groups (22-23). The deterioration of property and communities that can occur with large-scale layoffs has also been found to lead to aggressiveness and criminal activity (20, 21, 26, 29, 31, 34). There are also suggestions that unemployment is associated with intimate partner and familial violence (28, 29, 33, 36). However, it is less clear that a rise or fall in unemployment level is associated with violent crime in the surrounding community in which unemployment levels are measured.

The purpose of this paper was to examine the hypothesis that a rise in unemployment level, as distinct from poverty, would be associated with increases in violent crime in the surrounding community in the following calendar month and quarter. To address this hypothesis, we examined the association between county-level unemployment change and homicide rates in 16 US states in 2003 - 2009.

B. Methods

Homicides

This study used homicide data collected by the CDC's National Violent Death Reporting System (NVDRS), a state-level active surveillance system that provides data on all violent deaths in 16 US states. An NVDRS violent death is defined as suicide, homicide, death from legal intervention, death of undetermined intent, and unintentional firearm fatalities. The NVDRS reports on all victims and alleged perpetrators (suspects) associated with each violent death. NVDRS does not perform primary data collection, but rather collects and links data from death certificates, coroner/medical examiner records, and law enforcement/police reports.

This analysis includes all reported homicide events from January 1, 2003 through December 31, 2009. The NVDRS was initiated in a 5 state region in 2003. Subsequently, additional states began to participate in the NVDRS; therefore data do not exist for all years for all 16 states. We did not include data from California because these data are not statewide.

A total of 1,833 (6.48%) victim observations were initially missing incident dates. Date of death (or date pronounced dead) for each victim was used as the

incident date for these observations. For the purpose of the current analysis, by using the death date as a proxy for the incident date, we make the assumption that the victim died during the same month as the incident. This is reasonable, for example, 99% of all homicide victims in North Carolina die within 30 days of their occurrence. A total of 582 observations lacked county FIPS codes. These observations were individually inspected and, and attempts were made to assign county FIPS codes based on the city or place, however, 109 records did not contain a county, state, city, or place code and these were excluded from the analysis. An additional 48 observations were excluded because of missing or illogical age. The final base enumeration of victims for the analysis data set was 27,926.

Unemployment level

State and local unemployment data were collected from the Bureau of Labor Statistics (BLS) Local Area Unemployment database (www.bls.gov/lau). These data include monthly measures of the change in unemployment level, from the previous month and quarter, estimates of the number of workforce-aged individuals, and the absolute unemployment level within each county on the month being measured. Unemployment levels are calculated based on household response data from the Current Population Survey. Unemployment levels estimate the percentage of all persons who did not have a job during the survey week, were currently available for a job, and were looking for work or waiting to be called back from a job from which they had been laid off. Changes in unemployment level are computed for each month of every year and include the 1-month and 3-month (quarterly) changes for each month on record.

Time-Varying Covariates

In order to calculate county-level homicide rates, we obtained population estimates for each county within the NVDRS from 2003-2009. County-level population estimates were stratified by year, month, age, race, and sex using the U.S. Census Bureau Current Population Survey projected population as of July 1st of each year. From the same data source, we obtained median household income, the percent of persons living in poverty, and the population density in each county for use as county-level covariates and examination as potential confounding variables and modifiers of the rate ratio. We calculated population density (persons per square mile) for each county by dividing the number of person living in the county by the land area (in square miles).

Statistical methods

Poisson regression analysis was performed using SAS version 9.2 (155). using the GENMOD procedure with the Poisson distribution and log link.
In this model the log of the number of homicide events was modeled as the dependent variable, and we included the log of the person-time at risk (defined as the total number of individuals in an age/race/sex stratum in a county during a given month) as an offset so that estimated model parameters describe the change in the log rate for a one unit change in unemployment (158). Rates and rate ratios were computed by exponentiating the estimated model coefficients.

C. Results

Across the sixteen NVDRS states, for the study years 2003-2009, county-level unemployment level ranged from 1.0 percent to 31.1 percent with 99% of all reported levels between 2.4 and 15.7 percent. One-month change in the unemployment level ranged from a 14.4 percent decrease to a 13.7 percent increase in unemployment level (99% within -3.7 and 4.9). Three-month unemployment changes ranged from a 19.3 percent decrease to a 16.7 percent increase (**Table 6.1).** The largest fluctuations in unemployment level were found in counties in the state of Alaska, likely due to the smaller workforce sizes in most counties in that state.

County-level homicide rates were computed for categories of change in unemployment. (**Table 6.2)** County-level homicide rates were relatively constant across most categories of unemployment change except for the largest monthly decreases, where the homicide rates were lowest. We examined whether homicide rates were higher in counties with higher unemployment (**Table 6.3**). Monthly homicide rates tended to rise with increasing unemployment level until unemployment reached 8%, at which point homicide rates fell by 1 person per 100,000 and then rose again as unemployment surpassed 10 percent. On average, the unadjusted homicide rates increased 4.0% for each percentage point increase in unemployment level over a 1-month period.

Rate ratios and 95% confidence intervals for incremental increases and decreases in unemployment after adjustment for age, race, gender, median household income and population density indicated only a slight increase in the

homicide rate ratio per 1 percentage point increase in the 1-month unemployment level change, on average across the NVDRS (**Table 6.4)**. Adjusted rate ratios were similar to unadjusted, but were slightly attenuated. Similar results were observed when we considered a 3-month change.

We also compared decreases in the monthly and quarterly unemployment levels of 2.5 percentage points or greater to any other unemployment decrease across the same time periods (**Table 6.4**). Monthly unemployment decreases of 2.5 percentage points or greater were associated with more than five times greater drop in decreasing homicide rates compared to all smaller unemployment decreases (Rate ratio: 0.19; 95% CI: 0.15 – 0.25). A similar association was found among 3-monthly unemployment decreases (Rate ratio: 0.32; 95% CI: 0.27 – 0.38). Statistical adjustment was not applied in this situation as cell sizes at such extreme unemployment change variations was not sufficient to produce a credible adjusted estimate.

D. Discussion

We found that, on average across the NVDRS region, unemployment change was associated with a modest change in the county-level homicide rate. When the unemployment level increased more than 2.5 percentage points in a given time period (month or quarter), the county-level homicide rate rose by 5% in the multivariable adjusted model. A 5.0 percentage point change in the 1-month county-level unemployment resulted in an 11% increase in county-level homicide. We also found that a 2.5 percentage points or greater decrease in unemployment was associated with more than 20 times greater drop in decreasing homicide rates,

relative to all other decreases in unemployment combined (Rate ratio for 1-month change: 0.08; 95% CI: 0.07 – 0.11; Rate ratio for 3-month change): 0.18; 95% CI: 0.15 – 0.21).

A 2.5 percentage point change in county unemployment was relatively uncommon in these data. Such extreme increases in unemployment (2.5 percentage points or greater) only comprised approximately 0.7 percent of the total 1-month county-months of observation and 4.36 percent of the 3-month observations of our data set. However, such changes in county level unemployment may be especially common in rural counties that have one or a few key employers whose ongoing financial stability is important for the small local economy. Fluctuations in unemployment to a level that would raise the homicide rate enough to result in such noticeable differences on national scale would only precipitate from mass layoffs occurring across the United States at depression-like levels.

We found that a 2.5 percentage point or greater decrease in unemployment was associated with a small protective effect on county-level homicide rates. This result, in conjunction with the increases or decreases found for 1 percentage point changes in unemployment demonstrate a step function as it pertains to the effect of larger decreases in over time. In a practical sense, our results suggest that slower and even sustained hiring over time gradually may produce a feeling in the community that the overall situation is improving. This improvement would occur little by little, and therefore we see only a modest attenuation of homicide rates. Conversely, mass hiring and business openings (as represented by 2.5 percentage point decreases) appear to be able to produce dramatically increased morale and

community cohesion as larger groups of works simultaneously return to work, hence the intensely protective effect.

Our results suggest that the influence of increases in unemployment on homicide rate is more of a gradual than an immediate impact. The use of a 1-month or a 3-month interval for unemployment change made little difference to homicide rate estimates. This finding is contradictory to our initial supposition that a quicker onset of unemployment would result in a larger increase in homicide rate. The combination of results from the 1-month and the 3-month measurements corroborate evidence in the literature surrounding the association between unemployment and aggression and desperation (32). One explanation may be that communities are able to see that people have been laid off and that the workers who remain may experience a similar fate. It may be that the initial shock to the community and the influx of individuals who have been laid off or separated from work produces an increased risk which is sustained as individuals and families begin to fear that their jobs and mode of living may be in jeopardy as well (34-35; 37). Such a social climate may create a situation that may lead to an increased homicide risk if sustained over several months (38). Thus we see an increase in homicide rates among both unemployment change measurement lengths.

Limitations

We addressed the problem of missing incident dates by using the victim's date of death, based on the assumption that death dates within the NVDRS can be used to approximate the actual date of the incident 99% of the time (154). The use of death dates could introduce bias into our study if one of the following scenarios is

true. First, a spurious association (or lack there) could be created if the death of the individuals who were missing incident dates systematically occurred in a different month than the actual incident. In the case of a county that had a large layoff in one month and no unemployment activity in a surrounding month, the death date could be recorded for a month in which unemployment change was much lower or non-existent. If this happened on a routine basis, the results would be influenced toward from the null (i.e. the rate ratio within higher levels of unemployment change would be attenuated because of higher homicide rates in the lower unemployment change levels).

One major concern for this analysis was the potential for confounding of the rate ratio due to unmeasured effects. We were unable to adjust for movement and migration patterns within contiguous and proximate counties. We were also unable to adjust for the baseline county-level crime levels' influence on homicide rates. We were also unable to adjust for other macro-economic factors, such as erosion of property values and other sources of wealth, and changes in cost of living. However, our analysis did adjust for variation in the inherent homicide rate among county-level age, race, and gender groupings, as well as median household income, and percent living in poverty, all of which are highly correlated with underlying crime rates (1-6, 8, 34, 35, 50, 52, 67, 22, 23, 56, 59-63, 67-69, 134-135). However, it is possible that these adjustments did not remove all of the statistical influence due to the baseline county-level crime rate.

Finally, we used annual population estimates as the denominator for county-level homicide rates while using monthly and quarterly unemployment level changes

as the main explanatory variable. If individuals who have been dismissed from work left their county of residence before the next population estimate was taken, we could be underestimating the true rate ratio.

The NVDRS may not be representative of the United States as a whole. Thus, the findings of this study cannot necessarily be generalized nationally. Even though homicide rates in the NVDRS track closely with yearly national rates during the same time period (within 0.5 deaths/100,000 person-years during the study period), only seventeen states were funded for data collection as of our analysis. In addition, the states represented include a diversity of ages, races, and population types (urban, suburban, and rural). Unemployment levels in each study state historically have experienced the same variation in unemployment levels during economic expansion and contraction and are generally comparable to the rest of the United States (155).

The NVDRS is one of a very few data sets available which allows researchers to examine the event, victim, and the perpetrator at the county level over multiple states. The NVDRS can be combined with a variety of exposure variables from other data sets to determine associations with violent death. We suggest the NVDRS is a useful resource for researchers. As more states are added and the data are updated, we anticipate that the NVDRS will be even more useful and generalizable.

E. Conclusion

This study examined the association between unemployment change and homicide rates. Changes in county-level unemployment level were found to be

associated with modest change in county-level homicide rates. Rate ratios were nearly identical for both 1- and 3-month unemployment change measurements. We caution that these results should not be interpreted as an identical effect of unemployment change regardless of the measurement interval; rather, we postulate that they are a demonstration of the sustained gradual effect of mass layoffs that, over a matter of months may lead to adverse effects at the community level.

We speculate that an increase in unemployment can have an impact as a stressor on the community level. As has been the case with various short-term intense community stressors such as natural disasters and terrorism events, changes in the economy of a community may precipitate changes in the community physical and social infrastructure that may result in an increased homicide rate over a relatively short span of time such as months.

Table 6.1: Variability in 1-month and 3-month unemployment level changes by state, 2003-2009

State	1 - month change						3 - month change					
	Range		Quantiles				Range		Quantiles			
	Min	Max	1%	Median	99%	IQR*	Min	Max	1%	Median	99%	IQR*
Alaska	-14.4	14	-6.8	0	6.8	1.6	-19.3	16.7	-13	-0.1	16.7	4
California	-7.1	7.3	-2.3	0	3.1	0.7	-9.7	12.8	-5.1	0	6.5	1.6
Colorado	-6.1	4.9	-1.8	0	2.1	0.6	-9.7	8.2	-3.2	0	3.8	1.1
Georgia	-9.9	11	-1.5	0	2	0.7	-8.6	10.8	-2	0.1	3.2	1.1
Kentucky	-7	6.8	-2.1	0	2.6	0.9	-7.2	11.9	-3.1	0	5.1	1.8
Maryland	-3.7	4.4	-1.5	0	2	0.6	-7.3	9.6	-3.9	0.1	4.3	1
Massachusetts	-3.9	5.2	-1.7	0	2.3	0.6	-7	8.6	-3.6	0	4.3	1.4
New Jersey	-3.2	3.4	-1.4	0	2	0.7	-6.8	8.1	-3.9	0.1	4.1	1.2
New Mexico	-3.2	7.6	-1.5	0	1.7	0.6	-9	10.8	-2.9	0	3.8	1.1
North Carolina	-5.3	5.3	-1.5	0	2.1	0.7	-8.6	11.3	-2.9	0.1	4.5	1.1
Oklahoma	-4.8	7.2	-1.1	0	1.4	0.6	-7.6	8.1	-1.8	0	2.7	0.8
Oregon	-4	6.3	-2.5	0	2.7	0.9	-7.5	10.8	-4.6	-0.1	5.7	2
Rhode Island	-1.5	2.7	-1.1	0	1.9	0.7	-2.3	4.1	-2	0.2	2.9	1.6
South Carolina	-4	4.3	-1.5	0	2	0.7	-3.9	6.4	-4.7	0	4.9	1.3
Utah	-9.5	13	-5.6	0	6.6	1.2	-9.5	12.6	-5.6	0	6.6	1.2
Virginia	-10.3	9.6	-1.4	0	1.9	0.5	-11	9.9	-2.4	0.1	3.5	0.9
Wisconsin	-5.9	4.5	-2.2	0	2.3	0.9	-6.6	8.9	-3.7	-0.2	4.8	2.1
Entire NVDRS	**-14.3**	**14**	**-2**	**0**	**2.4**	**0.7**	**-19.3**	**16.7**	**-3.7**	**0**	**4.9**	**1.2**

* IQR = Interquartile range

Note: A positive number indicates a rising unemployment level, while a negative number indicates a lowering of the unemployment level.

Table 6.2: Homicide rates by unemployment level change within the NVDRS

Change (percentage)	Unemployment change			
	1-month	95% CI	3-month	95% CI
Decreases				
2.5 or greater	1.14	(0.89 - 1.45)	1.85	(1.58 - 2.18)
2.0 to 2.4	5.84	(3.98 - 8.58)	5.82	(4.86 - 6.97)
1.5 to 1.9	4.68	(3.60 - 6.08)	4.71	(4.17 - 5.31)
1.0 to 1.4	5.10	(4.53 -5.74)	5.24	(4.94 - 5.55)
0.5 to 0.9	5.43	(5.21 - 5.65)	5.80	(5.63 - 5.98)
0.1 to 0.4	5.72	(5.61 - 5.84)	5.71	(5.57 - 5.86)
No change	**5.63**	**(5.43 - 5.85)**	**5.58**	**(5.31 - 5.87)**
Increases				
0.1 to 0.4	5.85	(5.73 - 5.98)	5.85	(5.70 - 6.00)
0.5 to 0.9	5.65	(5.47 - 5.83)	5.96	(5.80 - 6.14)
1.0 to 1.4	5.08	(4.72 - 5.48)	5.54	(5.28 - 5.80)
1.5 to 1.9	4.59	(3.98 - 5.30)	5.64	(5.28 - 6.03)
2.0 to 2.4	4.48	(3.46 - 5.81)	4.35	(3.93 - 4.81)
2.5 or greater	4.66	(3.47 - 6.27)	4.07	(3.71 - 4.45)

Table 6.3: Homicide rates by absolute unemployment levels in the NVDRS

Absolute Unemployment level	Homicide Rate (per 100,000)	95% CI
Percentage		
< 4%	3.74	(3.63 - 3.86)
4 - 4.9	5.85	(5.71 - 5.98)
5 - 5.9	6.58	(6.43 - 6.74)
6 - 6.9	6.26	(6.06 - 6.45)
7 - 7.9	6.60	(6.33 - 6.87)
8 - 8.9	5.44	(5.15 - 5.75)
9 - 9.9	5.47	(5.14 - 5.82)
> 10%	6.24	(5.96 - 6.54)

Table 6.4: Model estimates, unadjusted, and multivariable adjusted rate ratios for 1-month fluctuations in unemployment level change

Unemployment change model	Estimate	Standard Error	Rate ratios RR	95% CI
One percentage point change				
Unadjusted	0.0249	0.0119	1.04	(1.01 - 1.05)
Multivariable Adjusted	0.0209	0.0120	1.02	(1.00 - 1.05)
IQR Estimates				
0.7 percentage points (1-month)				
Unadjusted	0.0174	0.0083	1.02	(1.00 - 1.03)
Multivariable Adjusted	0.0146	0.0084	1.01	(1.00 - 1.03)
2.5 percentage or greater decrease against all other decreases				
Unadjusted	-1.6527	0.1263	0.19	(0.15 - 0.25)

Chapter 7- Results for Aims 2 and 3 (Manuscript 2)

VII: Case-crossover analysis of unemployment change and workplace homicide using National Violent Death Reporting System data

A. Introduction

Rapid fluctuations in the unemployment level in a community may have effects that go beyond the economic, and impact even those members of the community that remain employed. Similar to natural disasters, and other emergency situations that have been associated with community level stress, psychological morbidity and subsequent traumatic injury (8-10), layoffs and rapid increases in unemployment in a community may impact violent crime. The literature suggests that, in times of emergency or dire situations, societal normalcy and community cohesion are disrupted, which may lead to higher rates of intentional injury and homicide (1-6, 8).

High unemployment is an important community-level disruptor that has been linked to precursors of violence such as desperation, depression, and rage (34-51). Unemployment also is associated with familial disruption, intimate partner violence (40, 41, 45, 48), risk of self-harm (84-99) and higher homicide rates. Higher unemployment is often followed by poverty, deterioration of property, infrastructure, and government (93, 97-99).

During the economic recession that occurred between 2007-2009 , almost 8 million jobs were lost in the United States through 83,301 separate mass layoff

events (work dismissals in which at least 50 employees are temporarily dismissed from work) and workplace closings. These mass layoffs occurred within a relatively short period of time and accounted for almost 90% of the total job loss during that time, causing the unemployment level to nearly double (5). In the period directly surrounding the recession, suicide among-middle aged men rose 28 percent (159). The increase in unemployment from 5.8% in 2007 to 9.6 percent in 2010 was associated with a 3.8% increase in the overall suicide rate in the US, corresponding to about 1,330 suicides (160).

Despite evidence of an association between economic factors and homicide, few studies have examined the influence of unemployment on homicide in the workplace. Hendricks, et al. reported an elevated risk of convenience store robberies in areas with a high percentage of residents on public assistance, a low median rent, a low percentage of high school graduates, older buildings and structures, and a high percentage of single males, all of which typically coincide with higher levels of unemployment (156). Ta et al. found that high-risk workplaces for homicide were more likely to appear in neighborhoods with more poverty or instability and less human and economic capita (8).

While loss of employment is often posited as a trigger for violence, the effect of a short-term change in unemployment level in a community has not been previously examined in relation to the risk of workplace violence. We hypothesize that an increase in the unemployment level over a short period of time, such as occurs with layoff events, will produce personal and community stress and an increased risk of a workplace homicide event. To address this hypothesis, we used

the case-crossover methodology to examine the association between transient increases in monthly county-level unemployment change and changes in the odds of a workplace experiencing a homicide in the 16 states from 2003 – 2009.

B. Methods

This study used a case-crossover design to estimate the change in risk of experiencing a homicide event in the workplace as monthly unemployment levels change. The case-crossover study design uses each case as its own control(s) (149, 151), thereby controlling by design for potential confounding time-invariant factors. Controls only differ from cases with respect to time-varying covariates. Our analysis used a case-crossover study design to examine the change in risk of workplace homicide for 1-month increases in unemployment level. In this study, cases were workplaces in which a homicide occurred, and the main exposure was the unemployment rate for the county in which the case workplace was located in the month of the homicide.

In many case-crossover studies, controls are sampled from the person-time history of the cases and exposure data for each control period is obtained from the case. As in environmental studies that use case-crossover designs, such as in air pollution research, time-varying exposure information is derived from routinely collected data resources. For this study, exposure data on unemployment was obtained from an external source (Census Bureau) rather than by sampling control periods and collecting control data from the cases.

Case Data

This study used data on homicides collected from the National Violent Death Reporting System (NVDRS), a state-level active surveillance system that provides data on all violent deaths in 16 US states. The system defines a death due to violence as “a death resulting from intentional use of physical force or power against oneself, another person, or against a group or community”, which is the World Health Organization (WHO) definition of violence. This definition includes suicides, homicides, deaths from legal intervention, deaths from undetermined intent, and unintentional firearm fatalities. The NVDRS reports on all victims and alleged perpetrators (suspects) associated with each violent death. NVDRS collects and links data from death certificates, coroner/medical examiner records, and law enforcement/police reports . The determination as to whether an event is deemed a homicide, suicide, or other death is based on a determination by the county medical examiner or a classification of death found on law enforcement records. For the purposes of this study, homicide is defined as death resulting from intentional use of physical force or power against another person or a group of other individuals.

The NVDRS was initiated in a 5 state region in 2003. Subsequently additional states began to participate in the NVDRS; therefore data do not exist for all years for all states. The analysis includes homicide events from January 1, 2003 through to December 31, 2009. We did not include data from California because these data years are not statewide.

A homicide is considered to have occurred “at work” when the victim(s) are at work or working when the event takes place. The designation is taken from the

“Injured at Work” item on the death certificate which item is completed for all injury victims with the exception of those less than 14 years of age. Workplace homicides can occur at the person’s place of work or off-site during the course of work-related activities. In the NVDRS database, workplace homicides are coded dichotomously as at work and not at work.

To compile the “Injured at Work” variable, states are directed to follow identical priority rules which rank data sources in terms of their potential reliability for each data element. The priority rules for “Injured at Work” dictate the death certificate as the primary source, followed by any additional data that can be taken from the law enforcement incident report, and finally the coroner and/or medical examiner records, in that order. Completion rates for this variable are high (from any single data source (known for 94.9% of victims) and even higher (known for 97.6% of victims) given the multiple data sources. It is assumed that all states follow the priority rules as directed, and that each state employs a uniform method for ascertaining the location of the event.

We classified each workplace homicide by typology using the CAL/OSHA Guidelines (139). To do so, we read each narrative provided with the NVDRS database for keywords such that would indicate that a certain type of workplace homicide occurred. A determination was made based on each situation’s approximation to the CAL/OSHA guidelines and our interpretation of the NVDRS’s narrative for each event. **Table 7.1** is a simple outline of the workplace homicides by typology. The majority of events were classified as Type I, usually the result of a robbery. A homicide perpetrated by an establishment’s customer or client was

considered Type 2 if there was some evidence that the perpetrator was using the services of the workplace before committing the homicide.

Type 3 included violence against coworkers, supervisors, or managers by a present or former employee (e.g. an attack on a supervisor, attack on a supervisor or co-worker as a result of a dispute that may or may not be directly related to occupation itself). Type 4 workplace homicide was defined by violent acts perpetrated by someone who is not an employee, but has a personal relationship with an employee (145).

Unemployment level

Monthly unemployment data were collected from the Bureau of Labor Statistics (BLS) Local Area Unemployment database (www.bls.gov/lau). These data include monthly measures of the change in unemployment level, from the previous month and quarter, estimates of the number of workforce-aged individuals, and the absolute unemployment level within each county on the month being measured. Unemployment levels are calculated based on household response data from the Current Population Survey. Unemployment levels estimate the percentage of all persons who did not have a job during the survey week, were currently available for a job, and were looking for work or waiting to be called back from a job from which they had been laid off. Changes in unemployment level are given for each month of every year and include the 1-month changes for each month on record.

Time-varying covariates

We obtained population estimates for each county and state within the NVDRS from 2003-2009. Each county-level population estimate was stratified by year, month, age, race, and sex. The U.S. Census Bureau Current Population Survey releases county-level population estimates each year that represent the projected population as of July 1st of that year. From the same data source, we recorded median household income and the percent of persons living in poverty in each county for use as covariates in model building. Finally, we calculated population density (persons per square mile) for each county by dividing the number of person living in the county into the land area (in square miles). Population density and median household income were examined as potential modifiers of the unemployment change-workplace homicide odds ratio.

Statistical Methods

This analysis used the case-crossover methodology to examine the association between unemployment level change and workplace homicide. Because we were interested in the effect that unemployment level change has on the odds of a homicide event, we chose to only retain the primary victim listed under a specific workplace homicide event (the victim who was deemed by law enforcement to be the most likely primary target of the crime). Sixty-six events (8%) of workplace homicides had multiple victims. Victim-level covariate information for the primary victim was retained in order to consider the heterogeneity of risk among victim subgroups.

We defined as control periods the two consecutive monthly intervals before and after a homicide event. In accordance with the design originally set by Maclure (149, 151), we matched NVDRS records with four control periods to examine the effect of rapidly rising or falling unemployment on the risk of workplace homicide. Unemployment change in control periods represented the change that took place from the midpoint of the month before the control period to the midpoint of the month of the control period. Each case and its four controls were grouped into risk sets for statistical analysis. **Figure 7.1** provides a schematic of the study design framework.

Unemployment level change (the exposure variable) was assigned for each case and control according to the event date and dates of control periods introduced by the case-crossover study design. For example, if a homicide event occurred in the second month of the year (February), the case observation in the data set would contain all of the time-fixed victim-level characteristics as well as the county-level characteristics for that month as well as a measure of unemployment level change representing the change in unemployment that occurred from the midpoint of January to the midpoint in February. Keeping with the example of a February homicide event, the “past” control observation representing the same workplace one month prior to the event’s occurrence would contain values for the same victim-level and county-level covariates and measures of the change in unemployment level from the midpoint of December of the previous year to the midpoint in January that constituted the boundary of the hazard period. Furthermore, the control observation two months in the past would be comprised of the same covariate information with the only change being that the unemployment level for that observation would

represent the change from the midpoint of November to the midpoint of December. The same pattern holds for the assigning of “future” controls.

In an analysis of a non-recurrent event, the case is not eligible to experience the event during control periods that are subsequent to the case failure. This is clearest when the outcome is death. The control sample from the exposure information for risk periods neighboring the period of case failure. The approach is applied, for example, in studies of associations between ambient temperature and mortality, or PM10 and mortality. Similarly, in this study the workplace may or may not be open subsequent to the robbery; however, the size of the study base from which cases may arise is fairly stable over the one month interval between case and referent sampling. The contrast is concordance/discordance with respect to exposure in the case and referent periods. The goal in not sampling all referent periods, but rather examining contrasts between risk periods close in time is to minimize the need to adjust for temporal confounding.

Conditional logistic regression was used to model the log odds that a given workplace would experience a homicide occurrence based on given 1-month change in unemployment level. Odds ratios were derived by exponentiating the model coefficient representing the log odds of experiencing a workplace homicide event.

We included interaction terms for characteristics for the primary victim including their age, race, and sex, and terms with workplace-level characteristics, including county-level population density, median household income, and percent of population living in poverty. We discerned the presence of effect modification by median household income and population density by calculating a likelihood ratio

test (LRT) statistic. Interaction terms that did not produce a statistically significant Chi-square value ($p \leq 0.05$) were eliminated from consideration in a final model.

We also used the likelihood ratio test to test for heterogeneity in the unemployment change-workplace homicide association among victim subgroups and workplace homicide types. Models that produced a statistically significant p-value ($p<0.05$) indicated that the magnitude of the odds ratio differed among or between categories of the community characteristic or workplace homicide type.

Bias due to control under/over ascertainment

We know that the workplace existed at the time of the homicide; we assume that the workplace also existed during each of the four control months (adjacent in time to the case month). If a workplace did not exist during the referent window the odds ratio for the unemployment change-workplace homicide association could be influenced in either direction (toward or away from the null), depending on the magnitude of unemployment change in that specific risk set and whether or not the unemployment level rose or fell during the risk set's referent window.

We assessed the potential for bias that may be introduced into our study through closings of case workplaces during their subsequent control periods. BLS records information on the frequency of workplace openings, closings, and relocations. We obtained BLS records for all 16 states in the NVDRS and all years of our study in order to evaluate the extent to which workplaces opened and closed during the study period.

C. Results

A total of 775 workplace homicide events were included in the study. After selecting four control periods for each case (3,100 controls), there were 3,875 observations. The majority of cases and their subsequent controls occurred in times when unemployment fluctuated between $\pm$ 1 percent (2,852 observations – approximately 74% of the total possible observations). 607 homicide cases (78%) occurred when the absolute unemployment level was under 7 percent.

Ninety-nine events (12.9 % of all observations) were unknown as to their workplace violence typology (**Table 7.1**). This typically resulted from either a blank narrative or insufficient detail to make any reasonable judgment as to the nature of the crime. Georgia and South Carolina contributed 58 of these observations (thirty-six and twenty-two respectively). Virginia, Maryland, and North Carolina also contributed nine, ten, and eight observations respectively (twenty-seven total). A total of fourteen missing narratives were found in the remaining 11 states.. We determined that six cases were not workplace homicides after reading the narratives associated with those events which explicitly stated that the homicide took place away from work and outside of working hours. All cases and affiliated controls (24 controls total) were deleted from the data set and were not used in any model building or subsequent analyses.

Table 7.2 presents the estimated odds ratio for 0.5 percentage point unemployment change increments; the statistical models included only the main exposure and no covariates. None of these estimates are statistically significant.

County-level population density in the county in which the workplace homicide was committed was found to be an effect modifier. Odds ratios were found to be heterogenous among categories of primary victim race, and workplace violence type. The LRT's for each model and their respective p-values are found in **Table 7.3**.

Table 7.4 examines the heterogeneity of the odds ratios of workplace homicide by unemployment level change and race of the primary victim. We used no change in unemployment as the referent category within each racial classification. Because of the extreme sparseness of cases in the higher levels of unemployment change among American Indian and Asian/Pacific Islander populations, the odds ratio estimates become very unstable and unreliable. It appears that the odds ratio in the event of an unemployment increase and a decrease are reversed slightly for black employees in our study, and that they are more likely to experience a workplace homicide event when unemployment decreases.

Table 7.5 stratifies the odds ratio by the population density in the county where the homicide event occurred. The odds ratios for unemployment and workplace homicide increases with population density when unemployment change increases. Estimates for a 1-unit decrease in unemployment approached the null as population density reached more urban classifications. However, estimates were rather imprecise as the magnitude of unemployment increases and decreases becomes more severe.

In **Table 7.6** we examine the difference workplace homicide odds between strata of the type of workplace homicide act committed. We dichotomized the four

workplace violence types into those in which the victim has no relationship to the employee (Types 1 and 2), and those in which a relationship with the employee is known and documented (Type 3 and 4). No change in unemployment is the referent category within each employee relationship classification. The magnitude of the point estimate for the odds ratio doubles when there is a relationship with the employee being killed. However, findings are not statistically significant. As the odds ratio is the exponential of the beta coefficient for the log odds of homicide for a 1-unit change in the unemployment level, we see a nearly significant result at a one percent increase in unemployment, regardless of whether or not perpetrator had a relationship with the victim.

Table 7.7 details all openings and closings in the NVDRS states for this study's catchment period. From 2003 - 2006, workplace openings reached 8.6 percent net. Openings decreased dramatically in 2007. The years 2008 – 2009 saw an increase in workplace closings, which nearly erased the gains of 2003 – 2007.

D. Discussion

This study examined the effect of changes in unemployment level on the community (county) level on workplace homicide events. We found that a one percentage point change in the monthly unemployment level over a one-month period was associated with a small and not statistically significant, increase in homicide risk. We found that the county-level population density in the county where the workplace homicide event took modified the odds ratio. The unemployment change-workplace homicide association was exacerbated as population density

rose. We also found that the unemployment change-workplace homicide association was heterogeneous between race of the primary victim and workplace homicide typologies.

An increase in unemployment led to a slightly protective effect for blacks as compared to whites. Much of the unemployment literature suggests that blacks and other minorities are disproportionately affected by economic contractions (20-24). Homicide literature further indicates that homicide death rates are generally higher among black males (57-66). Based on our findings, we postulate that if black males were being dismissed from the workplace at disproportionately higher levels, more whites and black females would remain in the workplace after or during times of mass layoff. One must be in the workplace in order to experience a workplace homicide. Therefore, if black males were no longer in the workplace, they would carry their higher risk outside of the workplace and would thus be less likely to experience a homicide event at work. More whites in the workplace would raise their number of homicides events and thus contribute to this association.

We examined some interactions between community-level factors and individual characteristics. For example, we observed that county-level population density modified the odds ratio. As has been suggested in previous studies, more densely populated communities, especially those with higher percentages of minorities, generally experience higher homicide risk. Our results suggest that the higher risk of homicide in densely populated areas is exacerbated by unemployment change.

We offer two potential reasons for this relationship. First, in order for the unemployment level to fluctuate in densely populated areas, more workers must be dismissed from their jobs than in less dense areas. As more workers are dismissed, the number of potential perpetrators available to commit heinous workplace acts increases, thus increasing the risk of a workplace homicide event. Secondly, unemployment levels are consistently higher in very densely populated areas with higher percentages of minorities, regardless of the economic climate. We suggest that the community stress already present because of this relatively higher unemployment may turn to desperation and rage more readily than in less densely populated or predominantly white neighborhoods.

We hypothesize that community stress resulting from unemployment leads to workplace homicide. As unemployment levels increase and are sustained, individuals and families may experience distress or desperation. Those affected by adverse economic events may be likely to enter a workplace setting for several reasons, among which are robbery and retribution. Many of the workplace homicides in our study were criminal acts motivated by robbery.

Other reasons why a perpetrator may enter into a workplace and commit a homicide may be to collect a debt, avenge a firing or layoff, or attempt to resolve an interpersonal issue. These circumstances can be exacerbated by increases in unemployment. In many cases, the act could have been committed in a location other than the workplace. However, the workplace can afford a perpetrator with a captive audience; and the perpetrator may know where the potential victim will be located thus facilitating the commission of an act.

Potential bias

The “Injured at Work” variable used to enumerate cases in the crossover analysis may be problematic. As we mentioned previously, completion rates for this variable are very high. However, completion does not necessarily translate into reliability. Various states may differ procedurally in defining exactly what is meant by “at work”. Differing state guidelines and small differences in procedures between death certificates, law enforcement, and medical examiners disallow a standardized coding scheme for an injury occurring at work, hence the NVDRS’s more overarching definition.

Approximately 3% of all NVDRS deaths were coded as being “Injured At Work” during the study period (2003-2009). Georgia, South Carolina, and Virginia, three of the four states with the most reported homicides, also coded the highest percentages of these homicides as being “Injured At Work”, surpassing the next closest by nearly an entire percentage point. We compared the proportion of homicides occurring the workplace within the NVDRS data set to those reported by other sources (namely, the BLS and the Bureau of Justice Statistics’ Uniform Crime Reporting Database), and found the two data sources behaved very similarly pertaining to these proportions **Table 4.9 and 4.10**).

Reported workplace homicide rates (per 100,000 labor force) were systematically lower in our data set in Maryland, New Jersey, and Wisconsin than those reported by BLS/UCR across all years. Rates were lower in Colorado in all years (2005 was marginally lower – difference: -0.0001). Rates in Georgia were

higher for all years reported (2006 was only marginally higher – difference: 0.00008). Also, workplace homicide rates for the final year are either the same or less than those reported by BLS/UCR in all but one state, being Georgia. These results suggest that NVDRS states, on average, may underreport workplace homicide events and victims, except for Georgia, who appears to overreport, and that virtually all states underreport for the final year.

Ninety-nine (12.9 percent) of the case narratives were either missing, or did not contain enough information to classify them as one workplace violence typology or another. Six cases and their respective controls (24 observations total – less than 1 percent) were deleted because they were definitively found to not be workplace homicides at all. After careful inspection of the law enforcement and chief medical examiner's narrative, they were deemed to be not related to the workplace, and should, therefore, not be used in the case-crossover. The narratives for these observations specifically stated that the event took place off the clock and/or away from work. The "at work" distinction may have come from a key punch error or erroneous coding. This finding is significant to the case-crossover study in that it brings the potential for an unknown quantity of bias as it pertains to misclassification of workplace homicide types.

A large number of these narratives contained no detail pertaining to workplace violence type or were left blank. It is impossible to know whether or not some workplaces with very little or no detail actually constituted homicide within the workplace. A consequence of this may be that the stratum-specific unemployment-

homicide association may be skewed considerably depending on whether or not these misclassifications were systematic or simply occurred randomly.

We investigated the degree of bias that may result from the closure of a workplace within a case's referent window. If a workplace is not open, it cannot experience a homicide event. The potential for bias is greatest in 2009, where a 12.3 net decrease in workplaces occurred. Such a pattern in closings could distort estimates of association between homicide and change in unemployment level.

Generalizability

The NVDRS is not representative of the United States as a whole. Thus, the findings of this study cannot necessarily be generalized to the remaining states. Even though homicide rates in the NVDRS track closely with yearly national rates during the same time period (within 0.5 deaths/100,000 person-years during the study period), only seventeen states were funded for data collection as of our analysis. Michigan and Louisiana (newly funded as of 2010 data) were not included in the analysis. Even though the NVDRS is not representative of the entire U.S. population during the years of our current analysis, the states represented include a diversity of ages, races, languages, and population types (urban, suburban, and rural) (154).

E. Conclusion

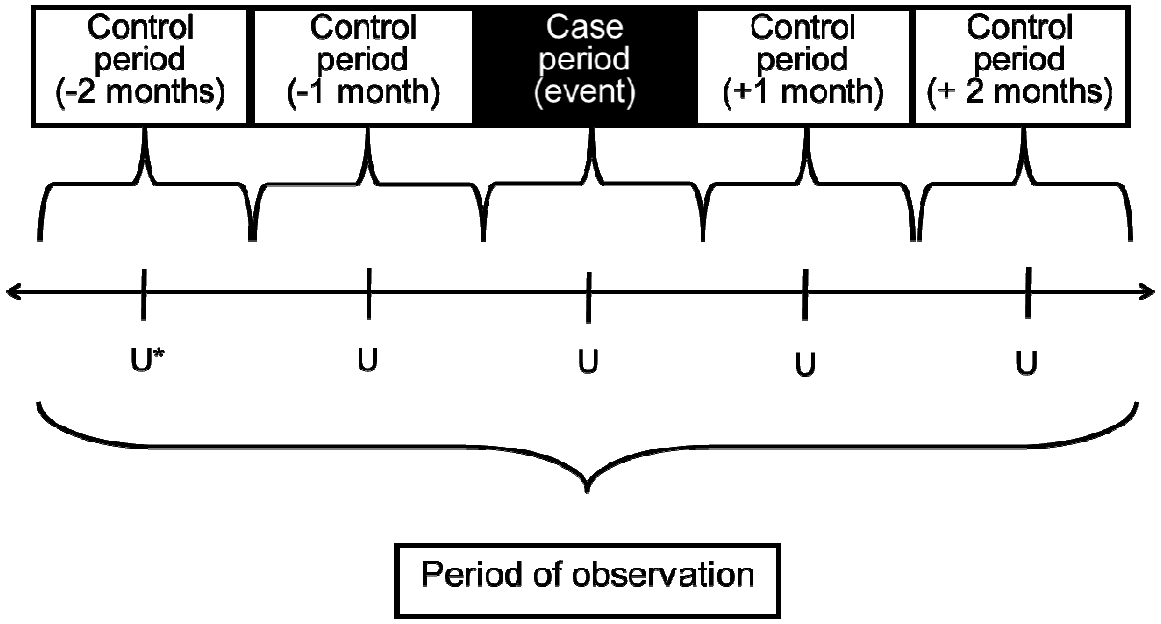
We employed a case-crossover design using all workplace homicide cases within the NVDRS from 2003 - 2009 to examine the unemployment change-workplace homicide association. The design, which used 1-month control periods

around each workplace homicide event, was used to investigate the prompt effect of a unemployment change on workplace homicide. We found that a one percentage point change in the unemployment level over a one-month period was associated with a slight, yet not statistically significant, increase in homicide risk. We also found that population density modified the unemployment change-workplace homicide association, and that is was inversely associated with unemployment level change. Homicide risk was found to be heterogeneous by victim race and workplace violence types.

This study demonstrates the utility of the pairing of case-crossover methodology with a case-only database such as the National Violent Death Reporting System to examine the effect of unemployment change on workplace homicide. We were able to use publicly available data to create control observations for each workplace homicide that were used to examine the association of interest. The case-crossover methodology is useful in examining short-term changes in economic factors, but we discourage its use in further examinations of economic and other societal factors and their impact on health outcomes because of the significant potential for bias.

Table 7.1: Workplace homicides by typology

Type	Workplace Homicide Events	Percent
1 - Criminal Behavior	423	55.0
2 - Customer/Student/Inmate/Patient	91	11.8
3 - Employee or past employee	51	6.6
4 - Personal Acquaintance	105	13.7
Unknown / Insufficient Information	99	12.9
Total	769	



* U = Measurement of the 1-month change in unemployment level

Figure 7.1 Design framework for 1-month bidirectional control sampling.

Table 7.2: "Unadjusted" Odds ratios of workplace homicide by 1-month unemployment level change

		1-month	
Type of change		**OR**	**95% CI**
Decreases			
	-2.0%	0.90	(0.62 - 1.29)
	-1.5%	0.93	(0.70 - 1.21)
	-1.0%	0.95	(0.79 - 1.14)
	-0.5%	0.97	(0.89 - 1.06)
No change		**1**	**REF**
Increases			
	+0.5%	1.03	(0.94 - 1.12)
	+1.0%	1.05	(0.88 - 1.26)
	+1.5%	1.08	(0.82 - 1.42)
	+2.0%	1.11	(0.77 - 1.60)

Table 7.3: Assessment of Effect Modification and heterogeneity of the odds ratio of unemployment change in homicide in Case-crossover study

Interaction terms dropped	Log Likelihood*(-2)	Likelihood Ratio	Degrees of Freedom	p-value
Effect modification				
Null model	2385.87	N/A	1	N/A
Population Density only	2365.89	20.0	1	<0.0001
Median Household Income only	2385.37	0.5	1	0.4795
Heterogeneity (victim demographics)				
Null model	2385.87	N/A	1	N/A
Age	2385.37	0.5	1	0.4795
Race only	2332.85	53.0	3	<0.0001
Sex only	2384.70	1.2	1	0.2733
Heterogeneity (workplace violence type)				
Null model	2377.66	N/A	1	N/A
Workplace Violence Type	2080.28	305.6	8	<0.0001

Table 7.4: Odds ratios for unemployment change by race

Unemployment changes	Race					
	White (n = 461)		Black (n = 196)		Other (n=112)	
	OR	95% CI	OR	95% CI	OR	95% CI
Decreases						
-2%	0.99	(0.63 - 1.58)	1.05	(0.51 - 2.14)	0.52	(0.19 - 1.38)
-1%	1.00	(0.79 - 1.26)	1.02	(0.71 - 1.46)	0.72	(0.44 - 1.17)
No Change	1	Referent	1	Referent	1	Referent
Increases						
1%	1.00	(0.80 - 1.26)	0.98	(0.69 - 1.40)	1.32	(0.82 - 2.13)
2%	1.01	(0.63 - 1.60)	0.96	(0.47 - 1.95)	1.75	(0.68 - 4.52)

Table 7.5: Odds ratio for unemployment change by population density.

Population Density*	Decrease in unemployment				Increases in unemployment			
	2%		1%		1%		2%	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Density Categories								
250-499	0.77	(0.43 - 1.39)	0.88	(0.65 - 1.78)	1.05	(0.86 - 1.27)	1.10	(0.74 - 1.62)
500-749	0.80	(0.50 - 1.29)	0.89	(0.70 - 1.14)	1.07	(0.89 - 1.27)	1.14	(0.80 - 1.65)
750-999	0.84	(0.56 - 1.25)	0.91	(0.75 - 1.12)	1.09	(0.90 - 1.34)	1.20	(0.80 - 1.78)
1000-1249	0.87	(0.61 - 1.26)	0.94	(0.78 - 1.12)	1.12	(0.88 - 1.42)	1.25	(0.77 - 2.03)
1250-1499	0.91	(0.62 - 1.35)	0.96	(0.78 - 1.16)	1.14	(0.85 - 1.54)	1.31	(0.72 - 2.36)
Greater than 1500	0.95	(0.60 - 1.52)	0.98	(0.77 - 1.23)	1.17	(0.82 - 1.67)	1.36	(0.66 - 2.80)
Urbanicity classifications **								
Surburban (500 - 1,499 per sq mi)	0.77	(0.43 - 1.39)	0.88	(0.65 - 1.78)	1.05	(0.86 - 1.27)	1.10	(0.74 - 1.62)
Urban (1,500 per sq mi)	0.80	(0.50 - 1.29)	0.89	(0.70 - 1.14)	1.07	(0.89 - 1.27)	1.14	(0.80 - 1.65)

* No change and less than 250 persons per square mile as referent.

** No change and rural (less than 500 persons per square mile as referent)

Table 7.6: Odds ratios of workplace homicide by unemployment level change and workplace homicide type

Type of change	No relationship to employee		Employee relationship	
	OR	95% CI	OR	95% CI
Decreases				
-2%	0.72	(0.46 - 1.12)	0.53	(0.24 - 1.19)
-1%	0.85	(0.68 - 1.06)	0.73	(0.49 - 1.09)
No Change	1	Referent	1	Referent
Increases				
1%	1.18	(0.95 - 1.48)	1.37	(0.92 - 2.06)
2%	1.40	(0.90 - 2.18)	1.88	(0.84 - 4.23)

Table 7.7: Workplace openings, closings, and percent change in NVDRS 2003-200

Event	Year							
	2003	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**All years**
Openings	374,331	395,979	415,072	433,537	432,450	420,958	402,269	2,874,596
Closings	355,463	369,118	384,233	399,130	416,923	447,152	458,461	2,830,480
Net Change	18,868	26,861	30,839	34,407	15,527	-26,194	-56,192	44,116
Percent change	5.3	7.3	8	8.6	3.7	-5.9	-12.3	1.6

Chapter 8: Conclusion and Synthesis

VIII: Conclusions

A. Summary

Unemployment, specifically when occurring at high levels, is known to be associated with crime, violence, and homicide (38-70). Workplace violence has received relatively little attention in relation to community-level factors, in particular unemployment level. No previous study has evaluated the association between risk of homicide and monthly- or quarterly-changes in county unemployment levels, as might result from mass layoffs and workplace closings. In the wake of the most recent global recession, this dissertation project has begun to address these gaps by examining unemployment change in relation to homicide in 16 states in the NVDRS. Results from our analyses have addressed the temporal relationship between the onset of unemployment and homicide as well as magnitude to which varied levels of unemployment affect homicide risk.

The Poisson regression analysis used in this project (Chapter 6) found that the magnitude of unemployment change was associated with a modest change in the county-level homicide rate. When the unemployment level fluctuated more than 2.5 percent in a given time period (month or quarter), the county-level homicide rate rose by 5% in the multivariable adjusted model. An 11% increase in county-level

homicide resulted from a 5.0 percentage point change in the 1-month county-level unemployment.

The second study (Case-crossover: Chapter 7) also found that a one percentage point change in the unemployment level over a one-month period was associated with a slight, yet not statistically significant, increase in the risk of a workplace experiencing a homicide event (OR for a 1-percent change in the one-month unemployment level: 1.05; 95% CI: 0.88 – 1.26). Though the measures of effect in this study indicate that unemployment change was associated with a greater change in risk than was found in the Poisson analysis, the results were not statistically significant.

Aim 3 (found in Chapter 7: Results Aims 2 and 3) focused on assessing effect modification in our case-crossover study. We found that county-level population density modified the odds ratio, and homicide risk was heterogeneous among victim races and workplace violence type; however, no measure of the unemployment-workplace homicide association resulted in a statistically significant effect measure.

B. Discussion

The Poisson analysis undertaken in the first study (Chapter 6) found that the magnitude of unemployment change, regardless of the whether the change occurred over one month or one quarter, was responsible for only a marginal change in the county-level homicide rate per 1-unit change in the unemployment level. Only when the unemployment fluctuated more than 2.5 percent in a given month or quarter did the county-level homicide rate rise by 5% in the multivariable adjusted model.

A 2.5 percentage point change in county unemployment was relatively uncommon in these data. Such extreme increases in unemployment (2.5 percentage points or greater) only comprised approximately 0.7 percent of the total 1-month county-months of observation and 4.36 percent of the 3-month observations of our data set. However, such changes in county level unemployment may be especially common in rural counties that have one or a few key employers whose ongoing financial stability is important for the small local economy. Fluctuations in unemployment to a level that would raise the homicide rate enough to result in such noticeable differences on national scale would only precipitate from may layoffs occurring across the United States at depression-like levels.

The use of a 1-month or a 3-month interval for unemployment change made no difference in the resulting effect measures (rate ratios) in the time series analysis. This finding is contradictory to our prior supposition that a quicker onset of unemployment would result in a more dramatic increase in homicide risk. Many of 1-month unemployment change rate ratios were borderline or not statistically significant. Conversely, all 3-month unemployment change/homicide rate ratio estimates are statistically significant when compared to homicide rates resulting from no unemployment change over three months. This increased precision lends itself to the notion that a 3-month unemployment change may be the more appropriate measurement interval moving forward.

Although the rate ratios are nearly identical for monthly and quarterly unemployment changes, the results cannot be interpreted as being identical. Unemployment change varied considerably more in the 3-month measurements.

This variability along with the identical rate ratios seems to indicate that an economic situation that would cause unemployment levels to fluctuate on a quarterly basis so as to result in a considerable increase in risk is not as unlikely as we may have initially perceived. All NVDRS states experienced increases of at least 2.5 percent during a quarter within the study period. Five states experienced unemployment level increases of at least 5%, while eleven total states experienced increases of 4% or greater.

Our results from the Poisson analysis suggest that the influence of increases in unemployment on homicide rate is more of a gradual than an immediate impact. The use of a 1-month or a 3-month interval for unemployment change made little difference to homicide rate estimates. This finding is contradictory to our initial supposition that a quicker onset of unemployment would result in a larger increase in homicide rate. The combination of results from the 1-month and the 3-month measurements corroborate evidence in the literature surrounding the association between unemployment and aggression and desperation (32). One explanation may be that communities are able to see that people have been laid off and that the workers who remain may experience a similar fate. It may be that the initial shock to the community and the influx of individuals who have been laid off or separated from work produces an increased risk which is sustained as individuals and families begin to fear that their jobs and mode of living may be in jeopardy as well (34-35, 37). Such a social climate may create a situation that may lead to an increased homicide risk if sustained over several months (38). Thus we see an increase in homicide rates among both unemployment change measurement lengths. Another notable,

yet unfavorable explanation is that the association with unemployment change is confounded by other more time-fixed county-level factors that were associated with unemployment change in this time period

Even though unemployment change within the interquartile range was not responsible for large increases in homicide risk in the Poisson analysis it is well known that homicide is the result of the suspect and victim's entire circumstance. In this paper, like the existing literature (20-24), we observed how different demographic groups carry a greater burden of the homicide rate than others (Chapter 5). We found that rates are higher among some demographic groups than others. If the estimated rate ratio holds true among those groups who have extremely high homicides rates already, a 20% increase in the homicide rate based on a 5 percent point or more rise in unemployment could be catastrophic for a community. Likewise, the decrease in homicide rate for a 5 percent drop in unemployment could offer great protection to communities that have inherently higher homicide risks.

Our findings in the case-crossover study are consistent with several other studies (8, 37, 65, 67, 69, 134-135, 141) as it pertains to age and population density. We found that the odds ratio for a 1-unit change in unemployment level was doubled in magnitude when there is a relationship between the perpetrator and the victim. Because of its limited sample size our case-crossover study was only able to examine simple statistical interactions between unemployment change and a covariate. An attempt to assess such interactions in a workplace homicide study that

would result from the current NVDRS data would yield extremely imprecise, and perhaps inaccurate measures of effect.

Table 4.7 quantifies the numbers of workplace violence events over each year of the study period. It would appear that reporting of workplace homicides may experience a lag in some states. Five states within the NVDRS (namely Alaksa, New Mexico, Oregon, Rhode Island, and Wisconsin) do not have sufficient levels of workplace homicide to warrant reporting of their state's data. Alaska was the only state of these four who did not have a workplace homicide before 2009. While others (Georgia, North Carolina, South Carolina, and Virginia) are responsible for approximately 60% of the workplace homicides in the case-crossover study. This may be due to a larger degree of workplace violence in these states, or simply a more diligent reporting system within each of these states. As the NVDRS is an active surveillance system, it may be the case that the records were not made fully available by each state at the time of data collection for this study. Further iterations of the data set may include more (or in some cases, less) homicide and workplace homicide victims as records are finalized and made more accurate.

The "Injured at Work" variable used to enumerate cases in the crossover analysis may be problematic. As we mentioned previously, completion rates for this variable are very high. However, completion does not necessarily translate into reliability. Various states may differ procedurally in defining exactly what is meant by "at work". Differing state guidelines and small differences in procedures between death certificates, law enforcement, and medical examiners disallow a standardized

coding scheme for an injury occurring at work, hence the NVDRS's more overarching definition.

The results of our independent evaluation of the 25-case subsample illustrates the fact that individual coders can interpret workplace homicide narratives differently. All of the homicides included in the subsample were coded as a certain workplace homicide typology. However, the independent rater, who was an experienced injury epidemiologist disagreed with the coding of four Type 1 homicide events, two of which were deemed by the rater to have not occurred within the workplace setting.

If the same type of disagreement were to take place in the entire workplace homicide case data set, as many as 125 cases could change classification, which could dramatically sway the odds ratios displayed in Table 7.6, were many of the type 1 homicides were reclassified to type 3 or type 4. Such a reclassification could be plausible if coders were to determine that the a workplace homicides that were originally classified as taking place during a criminal act were deemed to have been instigated by a present or past employee or if the perpetrator were a personal acquaintance of the working victim.

As previously mentioned, ninety-nine (12.9 percent) of the case narratives were either missing, or did not contain enough information to classify them as one workplace violence typology or another. Six cases and their respective controls (24 observations total – less than 1 percent) were deleted because they were definitively found to not be workplace homicides at all. After careful inspection of the law

enforcement and chief medical examiner's narrative, they were deemed to be not related to the workplace, and should, therefore, not be used in the case-crossover. The narratives for these observations specifically stated that the event took place off the clock and/or away from work. The "at work" distinction may have come from a key punch error or erroneous coding. This finding is significant to the case-crossover study in that it brings the potential for an unknown quantity of bias as it pertains to misclassification of workplace homicide types.

A large number of records contained no detail whatsoever or were left blank pertaining to workplace violence type. We also know that several of the cases that made the analysis data set were not actually workplace homicides. Hence, it is impossible to know whether or not some workplaces with very little or no detail actually constituted homicide within the workplace.

A consequence of this may be that the, the stratum-specific unemployment-homicide association may be skewed considerably depending on whether or not these misclassifications were systematic, or simply occurred randomly. We would recommend a more structured and standardized approach to event description moving forward with NVDRS data collection. As more workplace homicide events occur, the correct classification of workplace homicide cases is vital to performing any study with NVDRS data.

A key strength in both studies was their use of our novel approach to measuring unemployment change as the primary explanatory variable. This project measures county-level unemployment change within a specified time period as the

exposure (in this case, over the month or quarter prior to the measurement month). To our knowledge, no other study has examined unemployment as an exposure in that way. By quantifying change in the month and quarter before an event, we were able to examine the effect of the exposure to layoff and firing events of varying severity over time and observe the effect of that change across sub-groups in the population. In the Poisson analysis, we were able to address the difference between a certain percentage rise or fall in unemployment level over a one-month period, versus quarterly change.

Another noteworthy aspect of our case-crossover study was our attempt to estimate the potential for bias that may have been introduced into the study through closings of case workplaces during their subsequent control periods (a violation of our assumption). BLS houses the frequency of workplace openings, closings, and relocations. To attempt to make this quantification, we accessed BLS records for all states and all years of the NVDRS and attempted to quantify the extent to which workplaces opened and closed during the study period. We discovered a 12.3% net loss in total workplaces in our catchment area during 2009. Such a pattern in closings could erase or skew any trend associated with a change in unemployment level. This analysis is prone to substantial bias and threats to accuracy of the measured odds ratio if workplaces did not exist during their control periods. Any association we noticed could be spurious due specifically to this issue.

A major limitation with our use of the NVDRS was its lack of generalizability to the nation. If the NVDRS data structure were to be able to include the entire United States, we would be better able to estimate the unemployment change-homicide

association. As previously noted, we have no information pertaining to many states who experienced very large increases of unemployment level. As previously mentioned, we would be especially interested in performing a subsequent analysis that includes Michigan or other newer states in the NVDRS data set. Moreover, we would be eager to be able to collect data that would allow for generalization. We would suggest or welcome the collection of data for each of the remaining states not currently found in the NVDRS. A more complete version of the NVDRS that captures all 50 states (or at least a more representative subset of the U.S.) would be appropriate for the facilitation of studies such as this one, and for the measurement and monitoring of homicide rates, as well as the introduction of homicide reduction and prevention programs. Having said this, we understand that such endeavors are extremely time-consuming, costly and perhaps not realistic.

C. Future Research Direction

This project is a demonstration of the flexibility and utility of the NVDRS. Our studies employed several covariates that were outside of the NVDRS, while using the NVDRS victims list to simply enumerate a set of homicide victims. Form this list, we were able to calculate homicide rates and rate ratios for unemployment level changes as well as implement the case-crossover design. However, we scratched the surface of what the NVDRS is capable in future studies. We would suggest that further research be undertaken pertaining to the NVDRS (such as examinations of other economic factors and homicide and studies of suicide), and that it be employed in situations where its available states can contribute to a generalizable result. We further encourage that this extensive resource be used across any

discipline of research so that all covariates and potential exposure variables can be taken into consideration to their furthest extent. We believe that this resource can lend much data and information to a cross-disciplinary approach and that it can be a viable resource for years to come. The addition of pertinent victim-, county-, and workplace-level covariates (where available) can greatly enhance the utility of the NVDRS database.

We suggest that unemployment change should be considered as a primary explanatory variable of interest in studies that pertain to non-fatal outcomes of violent crimes (e.g. robbery, battery, etc.), and that it be used as a covariate (or regression coefficient) in other studies. We believe that the change in unemployment level immediately prior to an event provides a better explanation of the financial and workforce climate in a given area than simply measuring the point-estimated unemployment level. Our Poisson analysis (Chapter 6) found that communities appear to deteriorate over time as unemployment fluctuates at higher levels. The same type of indicator could be used to help examine the temporal nature of community deterioration or cohesion. The measurement of the change in unemployment level (or any other financial or social indicator) can be easily calculated, given one is allowed access to the data. Unemployment change itself is available within the BLS – Local Area Unemployment database for each state in the United States starting in 1976.

D. Public Health Implications

In the event of a catastrophic series of mass layoff events on a nationwide scale (such as those that happened during the Great Depression), homicides could

rise significantly if the pattern of association found in our study holds. It is important that civic offices and workplaces remain cognizant of the potential for an increased risk of homicide as economic conditions worsen or become desperate. Our project's results suggest the need for vigilance in communities and workplaces during times of economic catastrophe. All workplaces should have violence prevention programs in place, and should examine the utility of said programs as financial indicators suggest an impending recession. The timeliness of violence prevention activities and measures could result in saved lives.

E. Conclusion

This dissertation project employed two studies to examine the effect of county-level unemployment level changes on homicide risk and rates within the NVDRS. We examined this association in the context of a time-series analysis and a case-crossover study design. We were fortunate to discover similar results for each study. Our main exposure, change in county-level unemployment levels, was found to be positively associated with only a modest change in homicide risk and rates. Increases in the unemployment level that could substantially affect homicide risk are very possible and extremely likely on the county-level during major economic contractions. A 2.5 percentage point increase in unemployment was responsible for a 5% increase in the homicide rate (Poisson) after adjusting for the statistical effects of other variables. The case-crossover study found that unemployment change was associated with a small increase in the odds of a workplace experiencing a homicide (OR = 1.03; 95% CI = 0.94 – 1.12). County-level population density modified the odds ratio, and homicide risk was heterogeneous among victim race and workplace

violence type; however, no measure of the unemployment-workplace homicide association resulted in a statistically significant effect measure. In a practical sense, it appears that unemployment change is a significant factor in the health and well-being of a community. The health of workers and their families depends directly on the economic success of a given area. When people and families are working, less violent death occurs. When work is disrupted, especially with high volatility or at a sustained high rate, state health and local health departments, governments, schools, and other agencies should turn to violence prevention programs.

Based on our results from the time series analysis, we conclude that measurements of the change in economic indicators should be made in at least quarterly intervals so as to capture the entire picture of economic volatility as it pertains to homicide risk. In the time series analysis, a 1-month measurement of unemployment change was too precise to appropriately capture the fullness of a county-level economic contraction and the community disruption that may have ensued afterward or during such an event. We suspect that the same may be true for the case-crossover analysis; however, to minimize variability within the reference period, we used 1-month unemployment fluctuations. Again, a lack of recorded cases made for results that were not statistically significant.

This project is a demonstration of the flexibility and utility of the NVDRS. The NVDRS is one of a very few data sets available which allows researchers to examine the event, victim, and the perpetrator at the county level. The NVDRS can be combined with a variety of exposure variables from other data sets to determine associations with violent death. We suggest the NVDRS is a useful resource for

researchers. As more states become available and the data are updated, we anticipate that the NVDRS will be even more useful and generalizable.

Our study employed several covariates that were outside of the NVDRS, while using the NVDRS victims list to enumerate a set of homicide victims. From this list, we were able to calculate homicide rates and rate ratios for unemployment level changes. Further research needs to be undertaken pertaining to the NVDRS that can allow the data to be employed in situations where its available states can contribute to a generalizable result. This resource can lend much data and information to a cross-disciplinary approach and that it can be a viable resource for years to come.

In light of the results of this project, we can offer two key conclusions. First, unemployment level change can (and should) used as an indicator of economic and social instability and is a viable and easily collected explanatory variable in epidemiological studies. Second, the case-crossover methodology can be applied to studies of social and economic factors and their effect on morbidity and mortality. Such methods can be used to formulate subsequent projects using only case records in a cost-effective and efficient manner that is computationally less taxing. We would encourage researchers to extend their efforts in this direction in situations where short-term exposures to social factors are available.

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