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THE UNIVERSITY OF NEW HAVEN

ESTIMATING THE PSYCHOLOGICAL IMPACT OF MASS SHOOTING AND TERRORIST ATTACKS ON REMOTE POPULATIONS

A DISSERTATION

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of the requirements for the degree of

DOCTOR OF PHILOSOPHY CRIMINAL JUSTICE

BY

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ESTIMATING THE PSYCHOLOGICAL IMPACT OF MASS SHOOTING AND TERRORIST ATTACKS ON REMOTE POPULATIONS

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DEDICATION

To my fiancée, Caroline.

Her sacrifices and encouragement made this possible.

ABSTRACT

Most research examining the psychological impact of public mass-shootings and terrorist attacks focuses on the immediate victims (i.e., those at/near the scene of the crime or living nearby). Research consistently demonstrates that these directly targeted individuals experience a wide-array of adverse mental health outcomes following these traumatic events (Lowe & Galea, 2017; Wilson, 2014). What remains less understood, however, is how these violent episodes afflict the broader public who are exposed to the trauma largely through indirect means, such as media and word of mouth. While prior scholarship in this area remains quite limited, it also tends to suffer from several methodological limitations (e.g., cross sectional research design or case-study analysis of singular events). To address these limitations, this study employed a Time Series Cross Sectional (TSCS) framework, enabling an estimation of the impact of mass-shooting and terrorist attacks on the general public across 72 deadly events during a 6-year period (2012-2017). In addition, this study involved an empirical testing of dose-response theory (APA, 1980), utilizing characteristics of each event (casualty rates and level of media exposure) as proxy measures for trauma dosage. Overall, findings from this study indicate that remotely exposed communities are not seriously affected psychologically by these incidents. Research and policy implications for public health and media reporting are discussed.

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Chapter 1

Introduction

Terrorism is employed as a means to instill fear into a target population, in hopes that an anxious citizenry will pressure the government to change course on a particular issue (e.g., to withdraw combat forces from overseas combat zones). In other words, "terrorism is theater" (Jenkins, 1974). Accordingly, the primary target of these egregious acts of violence is the general population, who largely experience the trauma indirectly—typically through viewing sensationalized television and Internet news coverage. Of course, it is important to stress that terrorist attacks also produce varying levels of direct exposure to the general population, as scores of innocent people often are killed or wounded during each attack. Nevertheless, the overwhelming majority of people in the U.S. are extremely unlikely to be victimized directly through an act of terrorism. For example, between 2002 and 2016, 190 people (including the terrorist attackers) were killed in the US due to terrorism, or an average of 13 people per year (START, 2016). This means that the trauma most of the U.S. population experiences following a terrorist attack is fundamentally a form of indirect victimization—the severity of which appears influenced by a range of factors, such as level of exposure, pre-trauma, peri-trauma, and other post-traumatic influences.

In terms of indirect victimization from terrorism, there is already an abundance of survey research available showing that the American public remains quite fearful and anxious in response to the threat of terrorism. For example, in early March 2002 (nearly six months following 9/11), 49% of Americans worried a "great deal" about the possibility of future terrorist attacks (Gallup, 2016). Very little has changed since then. A 2018 Gallup survey tracking attitudes towards terrorism found that in the 2015-2018 period, between 40% and 51% of

Americans still worried a "great deal" about the possibility of future terrorist attacks in the U.S. (Gallup, 2019). A separate 2017 Chapman University Survey of American Fears also found that 43.3% of respondents were afraid or very afraid of a terrorist attack (Chapman University Survey of American Fears, 2017). Similarly, a 2016 Pew Research Center poll found that 40% of the public believed that the ability of terrorists to launch another major attack on the US is greater than it was at the time of the 9/11 attacks (Pew Research Center, 2016a). A December 2015 Public Religion Research Institute (PRRI) survey similarly found that 47% of the American public reported being very or somewhat worried that they or someone in their family would be a victim of terrorism (Jones & Cox, 2015). Concerns over terrorism also were at the forefront of voter's minds during the 2016 Presidential election. For example, a 2016 Pew Research Center study found that terrorism was the 2nd ranked voting issue during the 2016 election, with 80% of registered voters saying that the topic of terrorism was "very important" to their vote (Pew Research Center, 2016b). Thus, it is well understood that the general public in the U.S. remains quite fearful over the threat of terrorism.

In similar fashion, the public also has heightened fears over mass-shootings. To help investigate the scope of this fear, in August 2019 the American Psychological Association (APA) commissioned a survey comprised of a nationally representative sample of 2,017 adults living in the U.S. This survey found that over three-quarters of adults (79%) experience stress as a result of the possibility of a mass shooting (APA, 2019). Further, it was revealed that nearly a third (32%) of U.S. adults are so anxious by the prospect of mass shootings that they avoid visiting certain places and events. Around the same time, Gallup polls similarly found that 48% of U.S. adults are "very" or "somewhat" worried about being a victim of a mass shooting (Brenan, 2019).

Previously, Gallup concluded that in 2017, 39% of the population was worried about mass shootings, and 38% were worried in 2015 (Brenan, 2019).

What remains less understood, however, is the actual psychological impact that terrorist attacks and mass-shootings inflict on the general public. For instance, the authors of a recent review of 49 peer-reviewed studies investigating the effects of mass shootings on mental health concluded that "less is known about the psychological effects of mass shootings on indirectly exposed populations," and "more investigations are needed to understand the broader impact of mass shootings on unaffected communities" (Lowe et al., 2017, pp. 62, 79). Do these self-reported fears and anxieties manifest in a way that seriously affects the mental health and well-being of ordinary Americans (i.e., those not directly exposed to the attack)? How might the severity of a terrorist or mass shooting attack and its corresponding level of media exposure influence post-traumatic stress symptoms in the general population? Unfortunately, there is presently a dearth of quality scientific research available to answer these questions. A brief summary of a leading theoretical approach to be applied to better understand this phenomenon follows below, and general research findings are discussed.

Theoretical Application

The current research involved an empirical testing of dose-response theory, otherwise known as the "adversity stress model" (Bowman & Yehuda, 2004). This model asserts that greater doses of trauma exposure increase the risk for the development of post-traumatic stress symptoms (PTSSs). The relationship between dose-response and PTSS was first promulgated in *DSM-III*, which stated that "the severity, duration, and proximity of an individual's exposure to

the traumatic event are the most important factors affecting the likelihood of developing this disorder." (APA, 1980, p. 426) This concept is rooted in the etiology of Post-Traumatic Stress Syndrome (PTSD).

The concept of PTSD has attracted significant controversy since it was first introduced into the American Psychiatric Association's (APA) *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-III*) in 1980. The criteria for PTSD did not change between the releases of *DSM III* to *DSM IV* in 1994. However, there have been substantive changes in the most recent version of *DSM V*, published in 2013. For example, in *DSM-III* & *DSM-IV*, PTSD was considered an "anxiety disorder." DSM-*V* no longer considers PTSD an "anxiety disorder" and has placed it in a new diagnostic category named "Trauma and Stressor-related Disorders." The change stems from a body of research demonstrating how PTSD entails multiple emotions (e.g., shame, anger, etc.) outside of the fear/anxiety spectrum.

Another significant change from *DSM-III* and *DSM-IV* to *DSM-V* has to do with how exposure to a traumatic event is conceptualized. Importantly, this is also the most controversial aspect of recent changes. The *DSM-V* definition of trauma requires "actual or threatened death, serious injury, or sexual violence" (APA, 2013, p. 271). Previously, *DSM-III* and *DSM-IV* provided three qualifying types of exposure, to include direct personal exposure, witnessing of trauma to others, and indirect exposure through the trauma experience of a family member or other close associate. *DSM-III* and *DSM-IV* also did not stipulate whether witnessed exposures had to be experienced directly (first-hand) or whether media exposure constituted a type of witnessed exposure. In contrast, *DSM-V* stipulates that exposure through electronic media, television, movies, or pictures (unless work related) do not qualify as trauma exposure.

Consequently, the subjective criteria of witnessed exposure have been removed from the trauma definition and are now confined to the symptom criteria. PTSSs are codified in the *DSM-V* PTSD Checklist (PCL-5), a 20-item checklist designed to assess the presence and severity of PTSD symptoms (Weathers et al., 2013).

Thus, it is important to stress that proxy measures utilized in the current research for theoretical testing were used to assess how terrorist attacks and mass shootings impact post-traumatic stress symptomology and *not* PTSD rates. This is in line with past research efforts on this topic, as the majority of studies assessing the dose-response relationship on mass shootings similarly rely on self-report PTSS measures and not formal PTSD diagnoses (Wilson, 2014). Further, a PTSD diagnosis also requires clinical observation and cannot be assessed solely through the PCL-5 self-report screening questionnaire (Weathers et al., 2013).

General Empirical Findings of Dose-Response Theory

In a meta-analysis of 77 articles utilizing 85 data sets, researchers assessed the impact of 14 separate risk factors for PTSD in trauma-exposed adults (Brewin, Andrews, & Valentine, 2000). Individually, most of these risk factors produced modest effect sizes; however, important factors present during or after trauma, including trauma severity, lack of social support, and additional life stress, had relatively stronger effect sizes than pre trauma factors. More precisely, among the 14 risk factors assessed for PTSD, trauma severity presented the third largest effect size, with a weighted average correlation coefficient of .23. Lack of social support and life support exhibited slightly larger effect sizes, with weighted coefficients of .40 and .32, respectively. The other 11 risk factors were associated with effect sizes between .05-.19 In

accordance with the principles of dose-response theory, this meta-analysis confirmed that trauma severity is strongly associated with PTSD outcomes in adult populations.

In a separate review of 160 samples composed of over 60,000 people who had experienced a disaster event (one of the 102 events under review) between 1980-2001, researchers identified trauma severity as one of the leading individual-level risk factors for poor mental health outcomes (Norris, Friedman, & Watson, 2002a). Specifically, if an individual had severe exposure to the trauma (e.g., injury, threat to life, and extreme loss), or if they were living in the community that was affected, they were significantly more likely to experience adverse mental health effects. Thus, this large-scale review of the empirical literature on the mental health consequences of disasters provides firm support for a dose-response relationship.

Norris and colleagues (2002b) also examined whether disaster type (e.g., flood, hurricane, or mass violence) was associated with different mental health outcomes. They concluded that the "consequences of experiencing disasters caused by malicious human intent were unequivocal. Samples who experienced mass violence were far more likely than other samples to be severely or very severely impaired" (p. 244). Thus, this large-scale meta-analysis also indicates that the categories of trauma under review in this study (terrorism and mass shootings) are more likely to produce greater negative mental health effects than other types of natural tragedies.

Manmade mass killings are likely to elicit more adverse public health outcomes due to a perceived lack of control involved in these tragedies. In brief, perceptions of risk are highly influenced by the degree to which people feel that they have knowledge of or control over an

outside event (Slovic, 1987). Accordingly, terrorist attacks and mass shootings may create a sense of helplessness that undermines our feelings of personal and/or collective security. Further, the seemingly random and indiscriminate nature of these attacks may also shape our sense of personal safety (Stern, 1999). For instance, public anxiety remained elevated for weeks in the aftermath of the 2001 Washington, D.C., area sniper attacks, which killed 17 people and injured 10 others (Butler et al., 2003). People seemed to have an easier time distancing themselves from the urban violence, as one could actively avoid traveling into urban areas rife with violence. In contrast, the sniper attacks were perceived as more threatening, considering that an attack could randomly occur at "safe" locations and without warning.

Another meta-analysis investigated the extent to which disasters affected youth PTS symptoms. In short, Furr and colleagues (2010) conducted a quantitative synthesis of 96 studies published prior to 2009, involving 74,154 youth participants. Several variables were found to predict PTS in the aftermath of disasters, including female gender, higher death toll (greater than 25), proximity to disaster, personal loss, perceived threat, and distress at the time of the event. Importantly, proximity to disaster, threat to self, and distress at the time of the event each exhibited the strongest effect sizes. Specifically, these factors were significantly associated ($p < .001$) with medium to large effect sizes (correlation coefficient ranging from .33-.38) on youth PTS. Put another way, when youth were in closer physical proximity to the disaster site, or if their subjective experience involved greater perceived threat or general distress, then they were more likely to exhibit PTS. Therefore, these findings provide further backing of a dose-response relationship.

While the cited meta-analyses (Brewin et al., 2000; Furr et al., 2010; Norris et al., 2002) provide strong support for a dose-response relationship between the severity of trauma exposure and PTS, it is important to note that some studies have not found strong support for this relationship. For instance, in a longitudinal study assessing the PTSD of 967 patients after experiencing a motor vehicle accident, at 3-months post-accident and 1-year post-accident, measures of trauma severity were related significantly to PTSD outcomes (particularly delayed onset of PTSD), while other factors were more important in predicting the chronicity of PTSD (Ehlers et al., 1998). Specifically, negative interpretation of intrusions, persistent medical problems, and ruminations at 3-months post-accident were the most significant predictors of PTSD at 1-year post-accident. In a similar longitudinal study assessing the PTSD rates of 122 people involved in motor vehicle accidents, at 1, 3, 6, 9, and 12 months post-accident, none of the injury-related variables were significantly associated with PTSD risk (Ursano et al., 1999). However, several variables were suggestive of such a relationship. Odds ratios could not be calculated for self-injury at both 3-months and 6-months post-accident, as there were no uninjured subjects in the PTSD group. Additionally, a meta-analysis (Thomas et al., 2012) of 22 studies and a separate study on PTSD among female sexual assault victims (Kaysen et al., 2010) indicate that the dose-response association with PTS appears to weaken over time. In other words, other factors may be more important in determining the long-term chronicity of PTS.

Purpose of this Study

The purpose of this study was to assess how members of the general population residing in a state where terrorism or a mass-shooting occurs are psychologically impacted following a deadly mass shooting or terrorist event. As previously discussed, a variety of survey data

indicate that the public remains quite fearful and anxious over acts of terrorism and mass-shootings. These atrocities also appear to influence political decision-making, as evidenced by the high degree of concern over terrorism during the 2016 presidential election. However, less is known about how remotely exposed populations psychologically react to these types of atrocities. In other words, do these self-reported fears and anxieties produce greater levels of poor mental health and/or change the ordinary routines of people? There is also a short supply of research examining how characteristics of these atrocities (e.g., lethality and rate of news exposure) are related to the mental health of remotely exposed communities.

As will be reviewed in the chapters ahead, most research focuses on populations that are more directly exposed and tells us little about how the majority of the population is likely to be psychologically affected. In addition, there are several other research limitations that have been identified. The current research utilized a novel methodological approach to help address the limitations in prior research efforts. More specifically, all public mass-shootings and lethal terrorist attacks that occurred in the U.S. from 2012-2017 were assessed to better understand how these tragedies affect the publics' rate of psychological distress.

Measures of psychological health were drawn from the CDC's Behavioral Risk Factor Surveillance System (BRFSS), while mass-shooting attacks were obtained from the Mother Jones Mass-Shooting database, and terrorism events were derived from the Global Terrorism Database (GTD). Also, in addition to assessing the impact of the dose of the trauma (e.g., number of people killed/wounded), this study also explored how rates of media exposure (collected separately) relate to mental health outcomes. In terms of methodology, this study

employed a Time Series Cross Section methodology (TSCS) with multivariate regression (with fixed effects) as the mode of analysis.

There were two major expected advantages for this study. First, pre-incident measures on mental health were recorded. Most prior research failed to achieve this, which made assessing the impact of the event especially problematic. Second, this study examined 6 years of mass-shooting and terrorism event data; thus, findings of this study are more generalizable than prior efforts, which tended to employ a variety of different methods on singular events.

Lastly, it is worth highlighting that this research generates important policy implications. Findings from this study may help elucidate how mass-shootings and terrorist attacks affect the psychological health of the general public. Understanding the impact that these tragedies have on the public can enable us to prepare more effective public health responses. Further, the relationship between media exposure of mass-shooting and terrorism attacks and the publics' mental health was examined in this research. Findings in this area may improve our awareness of the possible risks associated with providing too much news coverage of these barbaric acts of violence. For example, if greater news coverage is associated with adverse psychological effects in the public following a mass-shooting/terrorism tragedy, then we can advise media about these risks and help devise guidelines to minimize them.

Chapter 2

Literature Review

This chapter assesses the empirical research testing dose-response theory in the context of mass-shootings and terrorist attacks. As previously discussed in Chapter 1, dose-response theory posits that increased trauma exposure leads to an elevated risk for the development of PTSS. This concept was originally promulgated in *DSM-III* (APA, 1980) and has since been applied to study psychological trauma in populations experiencing natural and man-made disasters (e.g., mass killings). Several meta-analytic studies confirm a dose-response relationship with adult and youth PTSS in the aftermath of disasters (Brewin et al., 2000; Furr et al., 2010; Norris et al., 2002). Assessing the impact of dose-response theory in the context of remotely affected populations from mass-shootings and terrorism will help us implement more effective public health initiatives and can be used to apprise media and government on the possible negative mental health consequences of high-volume terrorism or mass-shooting news coverage. To help simplify the myriad ways in which dose-response theory has been operationalized in prior research efforts, this literature review separates research into two distinct categories: directly targeted populations and remote populations.

First, studies that focus on directly targeted populations are examined for their academic contributions and methodological rigor. Specifically, studies are assessed that examine the mental health impact of terrorism and mass shooting events on individuals who were at the scene of the attack during the violent episode or were living within close proximity to a mass shooting or terrorist event (e.g., going to the same school or living in the same community where the attack occurred). Studies of this nature may include a wide array of operationalizations of "direct" and "indirect" exposure, which may include directly witnessing the violence first-hand,

emotional closeness to a killed or wounded victim, consumption of news/media coverage of the attack, physical proximity to the scene of the attack, and other sensory experiences (e.g., hearing gun shots or seeing people fleeing). Studies that fall under these categories are placed under the "directly targeted populations" section of this literature review.

Second, the next section of the chapter similarly provides a detailed review of the literature on the empirical testing of dose-response theory, as it has been applied to mass shooting events and terrorist attacks; however, only studies that incorporate remote populations are included. In this context, a remote population refers to a sample of participants who were not at the scene of a terrorist attack or mass shooting, and who were not living in close proximity to the where the violent episode unfolded. Studies of this nature typically compare a directly exposed sample of participants to a remote sample of participants in order to ascertain how differentially exposed groups are afflicted by a mass shooting or terrorist attack. A separate study assessing the mental health impact of police killings on remote populations of black participants also is discussed, as it has played a central role in shaping current research efforts. Accordingly, studies that fall under these categories are placed under the "remote populations" section of this literature review.

Finally, a succinct summary of key research findings and methodological limitations is presented. This portion of the chapter explains how the current study addressed prior research gaps and weaknesses, setting up discussion of the design, methodology, and statistical analyses employed in the current research.

Empirical Studies: Directly Targeted Populations (Mass Shootings)

In assessing the mental health effects of direct exposure to mass shootings, prior research has identified numerous studies over the past several decades (Lowe & Galea, 2017; Wilson, 2014). In terms of mass shootings at schools, the bulk of this research centers on 10 incidents, to include the 1984 Los Angeles elementary school shooting, 1988 Hubbard Woods school shooting, 1998 Thurston High school shooting, 2001 Santana High school shooting, 2007 Virginia Tech school shooting, 2007 NIU school shooting, 2007 Jokela High school shooting (Finland), 2008 Northern Illinois University school shooting, 2008 Seinäjoki University school shooting (Finland), and the 2014 University of California at Santa Barbara school shooting (Smith et al., 2017). Of note, several of these studies (e.g., Backholm and Björkqvist, 2012; Kaminski et al., 2010) utilize remote population samples and thus are incorporated into the "Remote Populations" literature review section.

The earliest of this research focused on how school children at the 49th Street Elementary School in South-Central Los Angeles were impacted following the 1984 sniper attack on the playground, in which 12 people were wounded and three others were killed (including the attacker). A total of 159 children (approximately 14.5% of the student body) at the school were assessed for PTSD 1 month after the attack, using the PTSD Reaction Index (Pynoos et al., 1987). Quantitative Analysis of Variance revealed a dose-response relationship; that is to say, children who were closer to the event or who were stronger acquaintances with the deceased demonstrated more PTSD symptoms than those with lower exposure. A follow up study was conducted 14 months after the attack, utilizing a sample of 100 elementary school students from the same school (Nader et al., 1990). While guilt feelings and being acquainted

with the deceased were found to be associated with more PTSD symptoms, grief reactions occurred independent of whether the child was directly or indirectly exposed.

The next mass shooting studied by researchers involved the 1988 Hubbard Woods elementary school, in which one child was killed and five others were wounded. In brief, researchers administered self-report questionnaires to 12 school personnel in order to assess physical proximity to attack site and emotional and sensory experiences related to the incident at both 6 and 18 months following the shooting (Schwarz et al., 1993). At the 18-month retest, individuals reported being closer to the attack site than previously reported; additionally, most participants enlarged or diminished their emotional, life threat, and sensory experiences. Enlargement of these experiences appeared to be associated with PTSD symptoms, while diminishment was associated with less anxiety and depression and greater self-confidence.

Research also has investigated the effects of the Hubbard Wood School shooting on emergency responders involved in the attack. Specifically, police (n=74), medical (n=20), and mental health (n=46) personnel who conducted emergency service work following the elementary school shooting completed questionnaires 6 months after the incident (Sloan et al., 1994). Interestingly, police recalled significantly fewer intrusive thoughts than other emergency responders. Groups also did not differ in intrusive or avoidance scores. The authors of this study noted that police might deny their symptoms, as other research has shown this can occur in firefighters (Durham et al., 1985). It is important to stress that this study utilized a post-test only research design, and the observations occurred 6 months after the attack.

Next, in response to the October 1991 shooting spree in Killeen, Texas, that killed 23 people, a team of researchers interviewed 136 survivor's 1-2 months after the attack, and then again 1 year later (North et al., 1997). Using the Diagnostic Interview Schedule/Disaster Supplement, 28% of participants met the criteria for PTSD in the acute post-disaster period. At the 1-year follow up, 17% were symptomatic of PTSD, while 24% reported a history of post-disaster PTSD. There were also no cases of delayed-onset PTSD (with symptoms beginning after the 6-month period). It also was revealed that for people with a psychiatric history—particularly depression—the risk of developing PTSD in the aftermath of a disaster is significantly greater.

A 3-year follow up was published by North and colleagues in 2002. Results from this sample (N=116) revealed that only about half of the PTSD cases identified previously (at any point in time after the attack) were in remission at the 3-year follow up. Further, those not in remission were found to have experienced increases in avoidance and numbing symptoms during the follow up period. Similar to the 1-year follow up report (North et al., 1997), there were no cases of delayed-onset PTSD identified during the 3-year follow up (North et al., 2002).

A separate 3-year longitudinal study was conducted in order to assess PTSD in a group of employees (N=77) of the Clayton Courthouse, located in St. Louis County, Missouri (Johnson et al., 2002). These employees were working at the Courthouse when the May 5, 1992, incident occurred. During the attack, the assailant's wife was killed, and five others were wounded. Participants were interviewed at 6-8 weeks after the attack, and then again approximately 1 year and 3 years later, using the Diagnostic Interview/Disaster Supplement. Perhaps most interesting in this study was the fact that only 5% of participants met the criteria for PTSD after the incident,

while 96% reported PTSD symptoms and 75% described the event as “very upsetting.” Meanwhile, 25% of participants met the criteria for a post-disaster disorder (e.g., alcohol use disorder, major depression, and PTSD due to other post-disaster events). The researchers suggested that the relatively low rate of PTSD (5%) may be the result of the incident being of smaller scale (one fatality), a shorter period of action (less than 10 minutes), and lesser exposure (few people saw the gunmen).

The next mass shooting incident reviewed by researchers occurred on July 1, 1993, in San Francisco. In total, eight people were killed, and six others were wounded (Classen et al., 1998). Acute stress symptoms of the employees in the building during the shooting were assessed just 8 days after the event (N=36) and then again 7-10 months for after the event (N=32). The Stanford Acute Stress Reaction Questionnaire was utilized to assess acute stress reactions during the first observation, while the Davidson Trauma Scale was utilized to assess PTSD in the second observation. Results from study show that approximately 1/3 of participants met the criteria for acute stress disorder during the first observation, and this diagnosis significantly predicted PTSD at the 7-10-month follow-up observation. A variety of limitations are worth noting, to include a small sample size, lack of a control group, and reliance on self-report assessments (as opposed to clinical diagnosis).

In response to the March 5, 2001, Santana High School Shooting that occurred in Santee, CA, killing two students and wounded 13, research assessed PTSD among 1,160 students at the high school (Wendling, 2009). Among participants, 247 students were directly exposed (e.g., witnessed a student shot or received medical services), 590 students had only heard or seen a shot fired from a distance, and 323 had no personal exposure (e.g., they only saw people running

or were on campus at the time). A staggering 25% of the directly exposed group suffered from PTSD or had partial PTSD at 8 to 9 months after the tragic event. PTSD rates for participants otherwise exposed were 9.7%, and 3.4% for those with no exposure. Overall rates of participant PTSD were 4.9% and 12.5% for partial PTSD, while depression rates were 18.7% for those directly exposed and 15.4% for all students. Results from this study also revealed a significant gender-exposure interaction, whereby directly exposed women scored significantly higher on PTSD and depression measures than their male counterparts. This study highlights the fact that PTSD can persist at elevated rates for many months after a school shooting incident, and that individuals who are exposed more directly to the traumatic shooting event suffer at higher rates of post-traumatic stress.

A 2006 school shooting at Dawson College in Canada also prompted researchers to explore the shooting's psychological impact, as well the effectiveness of treatment and services related response efforts (Séguin et al., 2013). Tragically, this event resulted in the death of two people, while another 19 students were injured. Approximately 18 months later, 948 people (students and employees) who were at the college on the day of the attack answered questionnaires (derived from the Canadian Community Health Survey) about their lifetime and post-incident mental health disorders. Most participants (79%) were present at the college on the day of the shooting; 50% heard gunshots, and 52% hid with peers during the shooting.

In response to the shooting, Séguin and colleagues (2013) determined that 18% of participants developed a mental disorder for the first time of their life, 1.8% of participants had PTSD, 5% had major depression, 5% had alcohol dependency, and 3% had social phobia. An additional 12% of participants who had prior mental health disorders continued to experience the

presence of disorders at the 18-month post-shooting questionnaire date. Thus, a total of 30% of the total sample experienced one or more mental health disorders in the aftermath of the shooting. This is actually double the number observed during a previous 2002 Canadian general population survey on mental health that used similar questionnaires. Of particular relevancy to the current research is the fact that the researchers identified a dose-response relationship. In other words, as exposure to the shooting increased, the risk for developing a mental health disorder increased.

The April 16, 2007, Virginia Tech shooting has also prompted researchers to study the mental health effects of the traumatic shooting. In brief, Littleton and colleagues (2009) recruited 293 female students who were enrolled at Virginia Tech at the time of the shooting. A survey that screened for sexual assault, depression, anxiety, and perceived social support was completed prior to the shooting. After the shooting, at 2-months and 6-months post shooting, surveys pertaining to risk and resilience were sent to the women who completed the initial survey. A variety of outcomes were assessed, to include PTSD symptoms, resource loss, and level of exposure (none, less severe, and severe). Of note, approximately 21% reported no direct exposure to the incident, 46% reported less severe direct exposure (e.g., on campus, saw police), and 33% reported more severe direct exposure (e.g., heard gunfire, in the building where the shooting occurred). The overwhelming majority of participants (94%) reported post-trauma symptoms at the 2-month (94%) and (90%) 6-month observation periods. Additionally, the pre-shooting observations of distress, social support, and resource loss each were associated with post-trauma symptomology. Of particular interest was the fact that none of the "exposure" variables significantly predicted post-trauma symptoms. The authors concluded that all

individuals in the environment may be similarly vulnerable to the communal nature of mass trauma.

A larger sample study (N=4,639) was conducted approximately 3 to 4 months after the Virginia Tech shooting in order to assess PTSD symptoms amongst enrolled VT students. Unlike the Littleton et al. (2009) study, this study, however, was cross-sectional and lacked a pre-test. In brief, Hughes and colleagues (2011) administered an online survey to enrolled VT students to assess PTSD symptomology using the Trauma Screening Questionnaire (TSQ). Descriptive statistics revealed that PTSD symptoms were prevalent amongst 15.4% of the respondents. Further, the concept "exposure" was operationalized in several ways, to include awareness of the event(s), proximity to the attack(s), trauma exposure(s), could not contact close friends, knew someone who was killed or injured, and knew someone who escaped uninjured. Results from multivariate regression analysis revealed that several exposure variables were significant predictors of PTSD symptoms. More precisely, the exposure variables that explained most PTSD symptoms include the inability to confirm the safety of friends, death of a (not close) friend, and death of a close friend. In contrast, age, gender, and race/ethnicity control measures were deemed non-significant (Hughes et al., 2011).

A follow-up study to Hughes and colleagues (2011) was conducted later that year, which sought to understand the relationship between communication about the attack and PTSD outcomes. More precisely, Scarpa and colleagues (2011) examined some of the participants (N=519) from the prior study, approximately 6 months post-shooting (the first study assessed PTSD at 3 months post shooting), with an additional 20-minute online survey. This "add on" survey incorporated measures of conveyance (i.e., sharing facts), convergence (i.e., sharing

thoughts and feelings) and forms of communication. The researchers were primarily interested in whether the form of communication (i.e., cell phones, messaging, and face-to-face communication) or messages (conveyance or convergence) were associated with PTSS outcomes amongst the participants.

A few noteworthy findings emerged from Scarpa and colleagues (2011) concerning the relationship between communication form and type (conveyance or convergence) and post-traumatic stress in the aftermath of the VT shooting. First, cell phones were most frequently used, followed by messaging. Face-to face communication was used the least. The authors also found that communication type was unrelated to post-traumatic stress. However, communication used for conveyance was positively associated with post-traumatic stress, while convergence was not. When interpreting these findings, the authors suggested that focusing on the event facts and details (conveyance) may increase cognitive reexperiencing of the event, which is a core feature of PTSD. Separately, measures of "exposure" also were assessed (e.g., being near the attack or knowing someone killed or injured). Interestingly, knowing someone killed and female gender were the most powerful predictors of post-traumatic stress within this study.

More recently, Smith and colleagues (2017) published a study in response to the Virginia Tech shooting in order to assess how physical proximity to the attack site and social proximity to the victims affect grief and post-traumatic stress reactions. Participants involved in this study include students enrolled at the campus at the time of the shooting (April 1, 2007). Observations were drawn from online questionnaires based on the Persistent Complex Bereavement Disorder (PCBD) checklist and the Trauma Screening Questionnaire, and were recorded at 3-4 months post shooting (T1, N=4,639) and again at one-year post shooting (T2, N=1,191).

Physical proximity was operationalized in several different ways, to include being off campus, on campus but not near shooting, in a neighboring dorm, in the building of the attack but away from hearing the shots, close enough to hear shots fired, or in the classroom where students were killed. Social proximity was categorized across four types, to include no relationships to any victims (no social proximity), acquaintance to a victim but not friend (low), a friend but not a close friend to one of the deceased (medium), and a close friend to one of the deceased (high). Results from this study indicate that both social proximity and perceived threats to others' safety (not self) at T1 significantly predicted grief reactions at T2. Importantly, physical proximity measures did not predict grief or peritraumatic threats to self or others' safety. In other words, emotional closeness to victims significantly predicted trauma and grief measures, while physical proximity to the attack (on campus) was not significant.

Approximately 10 months after the Virginia Tech shooting, yet another American university experienced a mass shooting. On February 14, 2008, a gunman opened fire in a classroom of over 120 students at Northern Illinois University (NIU), killing 5 and wounding 21 students. In response, Kumpula and colleagues (2011) sought to understand the onset and maintenance of PTSS following this traumatic event. More precisely, research efforts examined how experiential avoidance (EA) and peritraumatic dissociation (PD) were associated with post-traumatic stress symptomology across different time periods. To accomplish this, researchers utilized a three-wave longitudinal study on campus sexual revictimization amongst undergraduate women (N=532) at NIU. Data for Time 1 were collected prior to the shooting (up to a year and half prior to the shooting), Time 2 occurred 17 days post shooting, and Time 3 was collected 7 months post shooting. To measure EA and PD symptomology, participants

completed online-questionnaires, which included questions derived from the Acceptance and Action Questionnaire-II (AAQ-II) and the Peritraumatic Dissociative Experiences Questionnaire (PDEQ). EA (measured at T1, T2, and T3) and PD (measured at T2) were utilized in Path Analysis to determine if they were risk factors for each of the four PTSS clusters (hyperarousal, dysphoria, avoidance, and intrusions).

This study concluded that pre-shooting EA predicts intrusions and dysphoria at T2 and dysphoria and hyperarousal at T3. Additionally, PD, was strongly related to all four PTSS clusters at T2, but not at T3. Put another way, this study concluded that EA and PD may have differential temporal effects on the development of different PTSS symptoms. The authors suggest that screening individuals in the aftermath of serious traumatic experiences may help to better identify risk and needs of an affected population (Kumpula et al., 2011). Lastly, it is important to note that this study investigated PTSS symptomology amongst a sample of undergraduate women. Consequently, such findings may not be generalizable.

A separate study by Bardeen, Kumpula, and Orcutt (2013) was completed using data from the sexual revictimization survey of undergraduate female students at NIU (N=588) used in the previously discussed Kumpula et al., 2011 study. This research similarly involved three observations: T1 occurred up to a year and half prior to the shooting, T2 occurred at 7 months post shooting, while T3 occurred approximately 8 months after the shooting. Unlike the previous study examining EA and PD (Kumpula et al., 2011), this research investigated the temporal relationship between emotion regulation difficulties (ERD), exposure, and PTSS.

ERD has been defined as the ability to monitor, evaluate, and modulate emotional reactions within the context of goal-directed behavior (Gratz & Roemer, 2004), which has been associated with a range of psychopathologies including depression, borderline personality disorder, and anxiety disorders. The authors of this study (Bardeen et al., 2013) found that ERD prospectively predicted PTSS from T1 to T2 and T2 to T3, while PTSS prospectively predicted ERD only from T1 to T2. Accordingly, the authors suggested that ERD and PTSS are "reciprocally influential from pre to post-shooting" (p. 17). However, this follow-up study suffers from the same generalizability issue as the prior 2011 study, as all participants were undergraduate women.

More recently, researchers have examined the mental health impact of the May 2013 mass shooting incident, which occurred in the housing area of University of California-Santa Barbara (UCSB). This tragedy resulted in six UCSB students dying, while an additional 13 people were wounded across 17 separate crime scenes. In response to this attack, Smith and colleagues (2017) tested social cognitive theory of posttraumatic adaptation to see if pre-event protective factors (general self-efficacy and perceived social support) were associated with reduced PTSS and depression. This study utilized a longitudinal research design with UCSC students (N=70), with pre-event observations occurring 1 year prior to the attack, and post attack observations occurring 5-6 months after the attack.

The researchers were able to capture pre-event measures because there had been a prior study completed examining the relationship between school bullying and adjustment to college during their first year. Of note, exposure to the shooting was assessed using participant knowledge of the attack (i.e., specific details), seeing someone injured or killed, hearing screams

or gunshots, seeing the gunman or his vehicle crash, sustaining personal injury, or not being able to confirm the safety of friends, family, or loved ones. Responses were aggregated to create a total exposure score ranging from 0-8. Interestingly, bivariate correlations revealed that exposure level was generally not related to other study variables (with the exception of pre-event "general self-efficacy"). However, direct effects models revealed that exposure levels were significantly associated with PTSS severity. Other noteworthy findings include that higher pre-event protective factors were associated with improved mental health by bolstering post-event coping self-efficacy. Notable limitations of this study include its small sample size, the 1.5-year time gap between pre- and post-measures, as well as its generalizability.

Empirical Studies: Directly Targeted Populations (Terrorism)

Early efforts to study the mental health effects of directly exposed victims of a terrorist attack focused on the aftermath of the March 1, 1994, Brooklyn Bridge terrorist attack, in which a van full of Orthodox Jewish students were shot at—killing one and wounding three others. In response, a small-scale (N=11) study was commissioned approximately 8 weeks after the attack (Trappler and Friedman, 1996). In short, 11 of the survivors in the attack were examined for PTSD using the PTSD symptom scale (questionnaires and clinical evaluations); an additional group of students from the same community served as a control group. Results showed that four of the 11 attacked students (36.4%) had PTSD. Although the sample size was quite small, the levels of PTSD among those who directly experienced the attack were higher than the group not exposed. Major limitations of this study include a lack of pre-test and small-sample size.

Several studies also have attempted to capture the mental health effects that the 9/11 attacks had on people living in Manhattan during the time of this national tragedy. First, in just five to eight weeks after the 9/11 attacks, Galea and colleagues (2002) administered telephone interviews (employing a random-digit dialing technique) to 1,008 adults living in Manhattan in effort to estimate levels of public PTSD and depression. PTSD was assessed using the PTSD questionnaire from the National Women's Study, while depression was assessed using a modified (yet validated) version of the Structured Clinical Interview in the *Diagnostic and Statistical Manual of Mental Disorders.* A variety of "exposure" variables were captured, to include directly witnessing the attacks, symptoms of a panic attack during or soon after the attacks, friends or family killed in the attacks, loss of possessions, involvement in rescue efforts, and loss of a job because of the attacks.

Results from a multivariate logistic regression analysis indicate a number of significant predictors of PTSD and depression. In terms of PTSD, prior stressors over the past year (two or more), a panic attack, residence closer to the WTC (south of Canal Street), Hispanic ethnicity, and loss of possessions significantly predicted higher rates of PTSD. In terms of depression, prior stressors over the past year (two or more), a panic attack, low as compared to high social support, the death of a friend of relative in the attack, loss of a job due to the attack, and Hispanic ethnicity significantly predicted higher rates of depression (Galea et al., 2002).

Most strikingly, 7.5% of the overall sample population had symptoms consistent with PTSD, and 9.7% had symptoms consistent with depression. However, 20% of the population living near World Trade Centers had symptoms consistent with PTSD. To compare these statistics to pre-9/11 figures, the authors cite national level research, which estimates that the

prevalence of PTSD in the U.S. was 3.6% in 2000 (Satcher et al., 2001), while the prevalence of depression was 4.9% in the 1990's (Blazer et al., 1994). Thus, Galea and colleagues (2002) concluded that PTSD and depression rates were approximately double the national average in the aftermath of the 9/11 attacks, and significantly higher (approximately 4 times higher for PTSD) for those living in close proximity to the attack site. Further, several exposure variables predicted outcomes (increased PTSD and/or depression) consistent with dose-response theory, to include living near the attack cite, loss of possessions, loss of a friend or loved one, and loss of a job. Significant limitations of this study include its cross-sectional design and reliance on self-report data.

A separate post-9/11 study was commissioned approximately 6 months after the attack. In brief, a NY City-wide random sample of 8,236 of students (grades 4-12), including an oversampling of students closest to the World Trade Center site, were surveyed with the Diagnostic Interview Schedule for Children Predictive Scales (Hoven et al., 2005). In 28.6% of the cases, one or more anxiety or depressive disorders were identified, with the most prevalent being agoraphobia, separation anxiety, and posttraumatic stress disorder. Importantly, increased exposure levels were associated with increased symptoms. Results from logistic regression further indicated that exposure of a child's family member to the attack and prior trauma were significantly related to anxiety and depressive disorders.

Zimering and colleagues (2006) investigated the mental health impact of 9/11 by interviewing 109 relief workers who were either directly exposed to the attack on 9/11, or were indirectly exposed (e.g., second hand knowledge of the attack). The major advantage of this study was that it in addition to administering questionnaires, this research relied on clinical

interviews with licensed psychologists trained in the Clinician-Administered PTSD Scale (CAPS) criteria. PTSD rates of direct exposure were 6.4%, and those for indirect exposure were 4.6%. Accordingly, the authors concluded that both direct and indirect exposure of a terrorist attack can lead to PTSD. Unfortunately, a pre-deployment PTSD assessment of relief workers was not possible, given the immediacy of the need for their assistance following the attack. The authors' note that there could be an elevated pre-existing PTSD rate amongst the relief workers (given the hazards of these duties) which could impact the results. Nevertheless, this study noted that the 4.6% rate of PTSD amongst those indirectly exposed to 9/11 is fairly consistent with past research efforts revealing that people living outside of the targeted attack site who were indirectly exposed (via TV coverage) had a PTSD rate of 4% (Schlenger et al., 2002).

More recently, Boston area K-12 teachers (N=147) were surveyed 2-5 months after the 2013 Boston Marathon bombing, to assess teacher perceptions of classroom-wide psychiatric distress (Green et al., 2015). Five areas were examined to determine psychiatric distress, including emotional symptoms, conduct problems, hyper-activity, peer problems, and prosocial behavior. Student exposure to the terrorist attack, however, was not categorized as either "direct" or "indirect." Rather, all measures of exposure were used to create a combined exposure score. Nevertheless, the majority of exposure measures involved students seeing or hearing aspects of the attack or the subsequent man-hunt first hand, which are typical direct exposure measures. The results of this study suggested that teacher reports of student exposure to the terrorist attack and manhunt were associated with greater classroom-wide psychological distress (e.g., conduct problems, emotional symptoms, and prosocial problems).

Finally, a separate study by Comer and colleagues (2014) also found that exposure to the Boston's marathon bombing's post-attack manhunt negatively impacted children whose relatives (law enforcement or military) were involved in the manhunt. Specifically, a survey of Boston-area parents/caretakers (N=460) assessed their child's terrorist attack and manhunt experiences as well as their psychosocial functioning within the first 6 months of the attack. The results suggested that for children who had relatives involved in the manhunt, their likelihood of having PTSD was 5.7 times higher than those youth who did not have relatives involved in the manhunt.

Empirical Studies: Remote Populations (Mass Shootings)

In Lowe and Galea's (2017) review article*,* the authors were able to identify only six studies between 1984-2008 that examined the impact of mass shootings on remote populations (i.e., people who were not directly victimized at or in close proximity to the attack site). Similarly, Wilson's (2014) meta-analysis only identified two studies published from 1995-2014 that examined how mental health is impacted by less direct exposure (e.g., media coverage) of mass shooting events. Of note, the Fallahi et al. (2009) study appears in both of these reviews. Therefore, past research has identified seven studies between the years of 1984-2014 examining how mass-shootings affect remote populations.

The first of such studies investigated the indirect effects of the 1984 San Ysidro McDonald's Massacre (Hough et al., 1990). This shooting was considered the largest mass shooting in the U.S. until the Luby's shooting in Killeen, TX, that occurred in 1991. In total, 21 people were killed and an additional 15 people were injured during the attack at the San Ysidro McDonald's restaurant. To understand the mental health effects of the shooting on people not

directly involved, Hough and colleagues (1990) conducted a survey of 303 recently immigrant, poor, Mexican American women, who were 35-50 years old. This survey was actually an "add on" to another survey assessing late onset depression in a recent immigrant group of middle-aged Mexican American women. Thus, the sample was more of a convenience sample than purposive. Rather than assessing the immediate aftermath of the attack, this study was conducted 6 months after the attack. The researchers reasoned that if they conducted the survey too soon, it could disrupt how the Hispanic community mourns and might be too intrusive. Despite these limitations, the study found that approximately one-third of participants were seriously affected by the event, 12% had mild-severe PTSD at some point after the event, and 6% felt symptoms 6 to 9 months after the event. Importantly, women who had relatives or friends involved in the massacre and those with general social vulnerability (i.e., separated, divorced, unemployed, lower income, poor health, etc.) were most affected by the event.

Researchers also have measured physical proximity and emotional proximity (i.e., connectedness to the school) in response to the May 21, 1998 Thurston High shooting in Springfield, Oregon. In brief, research explored the longitudinal effects of peri trauma dissociative responses by surveying 80 respondents at 2 to 3 years after the incident (Curry, 2003). The sample consisted of students who were enrolled at Thurston High when the shooting occurred, young adults who graduated from Thurston high within 5 years before the incident, and students from another college town 40 miles away. The authors concluded that physical proximity measures predict the longitudinal direct effects (at 2 to 3 years post event) of distress from hyperarousal, intrusions, and avoidance. Separately, while controlling for physical proximity, emotional proximity to the school also predicted peri trauma dissociative response

and alexithymia (an inability to identify and/or describe his/her feelings). Elevated peri-trauma dissociative responses also significantly predicted the longitudinal effects of distress from intrusions. In summary, this study found positive support for a dose-response relationship. Noteworthy limitations include reliance on a small sample, as well as the fact that self-report observations from participants were recorded only once at 2 to 3 years after the incident.

The next few studies investigating how mass shootings affect remote populations focus on the 1999 Columbine school shooting. First, Stretesky and Hogan (2001) utilized a Rochester Institute of Technology (RIT) survey instrument administered before and after (April 15, 1999 – May 5, 1999) the school shooting (April 20, 1999). Importantly, this survey instrument was not created to assess student safety in response to the school shooting; rather, the questionnaire was created to inform RIT administrators about emotional, physical, and sexual abuse among women enrolled at RIT. Data happened to be collected both before (N=20) and after (N=102) the Columbine school shooting, thus enabling a natural experiment. A variety of questions were asked to women respondents pertaining to their feelings of safety. More precisely, these questions assessed how safe women felt walking alone after dark, riding a bus alone after dark, waiting alone after dark, walking past men after dark, and being alone at home after dark.

Both bivariate and multivariate statistical analyses confirmed the researchers' hypothesis, in that the pre-Columbine control group felt considerably safer than the experimental group (those surveyed after the school shooting). The researchers concluded that the findings provide strong support for the argument that "media portrayal of the Columbine shooting is the overriding factor influencing perceptions of safety among females at RIT during the time period under investigation" (Stretesky and Hogan, 2001, p. 440). There are a few notable limitations

with this study that are worth mentioning. The study employed a non-randomized design, had a small sample (only 20 women in the control group), and only involved female participants. It also only assessed “fear” and did not directly address other mental health related outcomes.

A more comprehensive investigation into the impact the Columbine shooting had on remote populations was completed by Addington and colleagues in 2003. Instead of relying on a relatively small sample in a single state, this study utilized data from the National Crime Victimization Survey (NCVS) School Crime Supplement (SCS) in order to compare students (both male and female) aged 12-18 from before (N=5,620) and after (N=2,777) the Columbine school shooting. The results from this study indicated that students reported only slightly more fear while at school. More precisely, 4% of students reported an increased frequency of fear, while the majority (77%) did not. There were also no significant changes detected in terms of avoidance behaviors (i.e., avoidance of hallways, cafeteria, entrances, etc.). It is important to note that this study utilized a national sample, as opposed to looking at Colorado only. Thus, if there was a localized effect on adolescent fear of crime within the state of Colorado or Columbine community, this study could not have captured such effects.

Another team of researchers investigated the impact of the Columbine school shooting on students by using the 1999 Youth Risk Behavior Survey (YRBS). The authors utilized logistic regression to determine if there were significant differences between feelings of safety and suicidal ideation before (N=12,049 students) and after (N=3,300 students) the Columbine school shooting (Brener et al., 2002). Findings from this study suggest that students who completed the survey post-Columbine were more likely to report feeling too unsafe to go to school and less likely to report considering or planning suicide than those surveyed before the

incident. Because this survey utilized a nationally representative sample of youth in the U.S., this study demonstrates that the Columbine school impacted youth across the country. This finding provides further backing that remotely exposed individuals to mass shooting atrocities can face adverse mental health effects.

Researchers also explored how the April 2007 Virginia Tech mass shooting affected remote populations. For instance, Fallahi and Lesik (2009) administered surveys to 145 female and 167 male students at Central Connecticut State University and asked how many hours of news coverage they watched on the school shooting, as well as questions about their self-reported symptoms of depression, anxiety, and stress-related symptoms. A series of multinomial logistic regression models were utilized to assess the relationship between the predictor (age, sex, minority status, and hours watched) and dependent measures (14 response variables). The results of this study revealed that as TV viewing of the shooting increased, so too did intrusive thoughts, distraction, fear, upset stomach, sleep disturbances, depression, anger, disorganization, and replaying of the event. Further, for every 1-hour of news coverage watched on the Virginia Tech school shooting, the odds of experiencing acute symptoms increased from 1.48 to 3.2 times, depending on the symptom. Importantly, this study did not utilize any pre-event measures. Thus, other factors unrelated to the Virginia Tech mass shooting (e.g., upcoming exams or other stressful events) may have influenced students' self-reported measures of stress.

Additionally, researchers investigated fear of crime levels among remote college students in response to the Virginia Tech mass shooting and the February 2008 mass shooting at Northern Illinois University (NIU). More specifically, a convenience sample of students at the University of South Carolina participated in a fear of crime survey before (N=749) the Virginia Tech

shooting and after (N=500), and both before (N=345) and after (N=357) the NIU mass shooting (Kaminski et al., 2010). Findings from this study are consistent with those from Fallahi and Lesik (2009). In brief, both the Virginia Tech and NIU mass shootings significantly increased fear of being victimized on campus and fear of crime more generally. The Virginia Tech shooting also increased the odds of students being fearful while walking alone after dark by 174%.

In Finland, Backholm and Björkqvist (2012) also investigated how the November 2007 Jokela school shooting (killing eight students) affected journalists' mental health. These individuals were either directly involved at the scene (n=27) or indirectly involved (i.e., they covered the incident but from their office or from another part of the country; N=169). A comparison group of 297 journalists who did not cover the incident also was utilized to compare those who had direct and indirect exposure to the scene of the attack. Interestingly, there were no statistically significant differences regarding severity of psychological distress symptoms between the groups. However, qualitative survey data indicated that 43% of the participants stated that the incident provoked some kind of personal reaction, such as general sadness, crying, fear, shock, or anxiety. Thus, all groups seemed negatively afflicted by the event, although the type of exposure did not matter.

Empirical Studies: Remote Populations (Terrorism)

Past research has assessed the degree to which terrorism impacts remote populations. Early efforts to accomplish this were conducted after the April 19, 1995 Oklahoma City bombing. In brief, approximately 6 months after the bombing, Sprang (1999) conducted

telephone interviews of adults in Oklahoma City (N=244) and adults (N=228) in Lexington, KY (approximately 800 miles from the attack site). To assess post-disaster stress, the Traumatic Experiences Inventory (TEI) was utilized. The TEI is a 25-item post-trauma symptom assessment tool. Sprang then divided the subjects into three groups: Group one (high exposure, N=109) consisted of those directly exposed (i.e., hearing, feeling, or seeing the blast); Group two (low exposure, N=145) consisted of people residing in Oklahoma City, but who did not experience the blast; and Group 3 (control group, N=228) consisted of the KY participants.

Analysis of variance revealed that both of the Oklahoma City groups had higher levels of post-traumatic stress than the control group. The high exposure group had higher levels of response measures across all categories (avoidance, reexperiencing, increased arousal, victimization, and post-traumatic stress disorder), and the low exposure group also had higher levels of response measures across all categories, when compared to the control group. Importantly, PTSD differences between each group achieved statistical significance, whereas some of the other measures did not (i.e., avoidance and reexperiencing). The high exposure group had a rate of 8.6% of diagnosable PTSD, while the low exposure group had a rate of 7.8%, and the control group's rate was .8% (Sprang, 1999). Accordingly, this study revealed a significant relationship between exposure, proximity, and post traumatic symptomology. Put another way, those living in the city of the terrorist attack (whether directly or indirectly exposed to the event) fared much worse in terms of mental health outcomes than individuals living in another state.

There are, however, a few notable limitations to Sprang's (1999) research. First, this study had no pre-test measures. Second, the data were collected 6 months after the terrorist

attack, which makes it difficult to assess how the intensity and duration of traumatic symptoms persist over time. Lastly, this terrorist attack was quite significant in magnitude (168 people killed); consequently, the findings may not reflect results derived from the more typical smaller-scale terrorist attacks.

A separate study by Pfefferbaum and colleagues (2001) assessed the degree to which bomb related television consumption contributed to posttraumatic stress symptoms (intrusion, avoidance, and arousal), in over 2,000 middle-school students approximately 7 weeks after the Oklahoma City bombing. Both emotional (i.e., knowing a victim) and television exposure were associated with post-traumatic stress symptoms. Further, for those with no physical or emotional exposure to the event, television exposure was related directly to post-traumatic stress symptomology. In short, these findings suggest that increased consumption of the bombing increased post-traumatic stress symptoms in children when other measures of exposure were not present.

Several studies also have investigated the impact that 9/11 had on the mental health of remote populations. First, in just three to five days after the 9/11 attacks, researchers conducted telephone interviews (using random digit dialing) with a national sample of 560 U.S. adults, asking about their reactions to the terrorist attacks, and their perceptions of their children's reactions (Schuster et al., 2001). Although no pre-test measures were assessed, the results of the telephone interviews suggest that the participants were affected by the 9/11 events in many noteworthy ways. For example, 44% of participants indicated they had substantial symptoms of stress, while 90% indicated they had at least one or more stress symptoms. Most coped by talking to others (98%), turning to religion (90%), participating in group activities (60%), and

making donations (36%). Parents also restricted their children's access to television (34%) and talked to their children about the topic for at least one hour or more (84%). Further, 35% of children had one or more stress symptoms, while 47% were worried about their own safety or the safety of their loved ones (Schuster et al., 2001).

Another survey by Schlenger and colleagues (2002) was administered just 1-2 months after 9/11 to a sample of 2,273 adults, including oversamples of New York City and the Washington, DC, metropolitan areas, as well as a sample from the rest of the country. Using a web-based survey design, post-traumatic stress symptoms were captured via self-report measures from the PTSD Checklist and Brief Symptom Inventory. A variety of measures for direct exposure were captured, to include proximity to crash site, family or friends injured/killed, being in the military or knowing someone close to you in the military, and levels of TV viewing per day. Several important findings are worthy of discussion. First, PTSD rates in NY were significantly higher (11.2%), while in D.C. the PTSD rate (2.7%) was lower than in the rest of the country (4.0%). Results from the multivariate analysis revealed that sex, age, direct exposure to the attack, and TV viewing consumption were associated with PTSD symptom levels. Of particular relevance is the fact that researchers found TV exposure rates were associated with PTSD rates even after controlling for direct exposure, content of TV coverage, and other sociodemographic characteristics.

The first-ever prospective longitudinal study that sought to investigate the mental health effects of 9/11 was conducted by Silver and colleagues (2004). This study utilized a national probability sample of adults in the U.S. (N=1,900) who were surveyed at just two weeks after 9/11, and then again at one-year post 9/11. The researchers were interested in determining

whether direct and proximal exposure were necessary preconditions for high levels of acute and posttraumatic stress symptoms, and whether increased exposure/proximity led to more traumatic stress symptoms.

To assess posttraumatic stress, during the first wave the researchers used the Stanford Acute Stress Reaction Questionnaire (SASRQ), as it is designed to measure symptoms within one month after a traumatic event. During wave two, the 17-item PTSD checklist was utilized. In terms of exposure measures, the participant witnessing the attack in person or being close to someone in the targeted buildings was considered direct exposure. Lesser forms of direct exposure included live media exposure, no live media exposure, as well as distance from the World Trade Center. Results from this analysis indicated that while post-traumatic stress symptoms clearly declined over the year, people who were directly and indirectly exposed experienced similar mental health effects. The authors concluded that “the requirement of direct and proximal exposure to the attacks and the expectation of a dose-response relationship between exposure and traumatic stress response are myths” (p. 130).

The largest 9/11 related study that examined the psychological impact of 9/11 using fully diagnostic structured interviews was published in 2010. In short, Henriksen et al. (2010) assessed how the presence of Axis 1 mental disorders (i.e., mental health and substance use) changed from observation one (2001-2002) to observation two (2004-2005). Participants (N=34,643, ages 20+) were interviewed as part of the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC). Most of the respondents (N=25,239) were only indirectly exposed, meaning they were only exposed remotely through television or radio. Others had a close friend or relative directly experience 9/11 (N=1,241), and a minority of

participants (n=170) reported direct experience to the attack itself. Overall, 7,791 participants reported no direct or indirect exposure to 9/11. Results from multiple logistic regression analyses found that higher exposure levels were associated with greater odds of having onset PTSD, any anxiety disorder, and any mental health disorder at observation two. When compared to participants who reported no exposure to 9/11, directly exposed participants had six times the odds of having PTSD, 2.5 times the odds of having an anxiety disorder, and approximately double the odds of having a mental disorder. Even just indirect exposure to 9/11 (i.e., television and radio), as well as having a family or friend directly exposed to the attack, were found to increase risks for the development of Axis 1 disorders. Accordingly, the results of this study suggest a dose-response relationship between exposure level and PTSD.

Another 9/11 related study assessed fear of terrorism measures in New York City and Washington D.C. between March-April 2006 (Nellis and Savage, 2012). Telephone interviews of 532 adults were conducted in order to assess how much news coverage participants consumed, along with numerous fear of terrorism and perceived risk of terrorism measures. Unlike past research, however, this study did not take place near the time of when terrorist events occurred; rather, the survey occurred nearly five years after 9/11. NYC and Washington DC were chosen deliberately due to the historical significance of 9/11. Results from multivariate regression indicated that exposure to terrorism related news is positively associated with perceived risk of terrorism to self and others, and fear of terrorism for others but not self. Other notable findings included that women are more fearful than men and that older participants tended to be less worried about terrorism for themselves, but not their families (Nellis & Savage, 2012).

Despite important findings, Nellis and Savage's (2012) study was cross sectional, had no pre-event measures, and was conducted during a time period where no terrorist events are occurring in NYC or Washington DC. In fact, according to the Global Terrorism Database, only one terrorist event occurred during the survey period. Specifically, on March 3, 2006, Mohammed Reza Taheri-azar drove his jeep into a crowd of people at the University of North Carolina-Chapel Hill, which killed none and wounded nine people. Consequently, it is likely that the terrorism news coverage consumed by participants in NY and DC focused predominantly on foreign terrorist events and not local events (as none occurred). Therefore, these findings are difficult to generalize.

The link between post-traumatic stress following terrorism and media exposure to the event also was established firmly in a 2009 meta-analysis. In brief, Houston (2009) assessed how media coverage of terrorism impacted post-traumatic stress among individuals living both in the city of the attack and living remotely (i.e., not in close proximity to the attack). Across 23 studies, Houston (2009) computed mean weighted effect sizes from samples from the same city as the terrorism event (nine studies, N=10,560), samples derived from a different city of the attack (nine studies, N=2,796), and national sample studies (five studies, N=7,054). Overall, the effect size for exposure of media coverage of terrorism and PTS was significant (r = .152, d = .31).

Results of Houston's (2009) meta-analysis also revealed that samples consisting of people from another city had a mean weighted effect size of .188, while national sample studies had a mean weighted effect size of .195. Studies examining participants only in the city of the attack had a smaller effect size (.110). Thus, Houston (2009) found support for his hypothesis

that the effect size of exposure to media coverage of terrorism and PTS is greater for people who are farther away from the event. In making this prediction, Houston believed that people living in the city of the attack have a variety of other ways to be "exposed" to the terrorist attack, to include direct experience or through hearing from about attack details from friends, family, or neighbors. Consequently, the effect of "media exposure" appears to be smaller on populations where the event occurred when compared to remote populations, as remote populations appear more dependent on news/media for information than directly exposed populations. It is worth noting that several other variables also significantly predicted PTS measures, including being a youth as opposed to an adult and exposure to terrorism through multiple media coverage (i.e., Internet related technologies).

Perhaps the most significant limitation of Houston's (2009) meta-analysis is that the majority of the studies included (17 of 23) were 9/11 event related; only two studies were related to the worst domestic terrorist attack in the U.S. (the 1995 Oklahoma City bombing), while the other U.S. based terrorism event pertained to the 2001 Anthrax attacks. The three other studies included in the meta-analysis focused on terrorism in Israel and the 2004 Madrid bombings. In other words, these attacks are not representative of what terrorism has been like in the U.S. in the post 9/11 era. Further, these studies suffer from the same series of methodological issues previously highlighted, (e.g., cross-sectional research design with no pre-test). Consequently, this meta-analysis fails to inform us completely about how typical U.S. based terrorist attacks afflict remote populations.

As mentioned, researchers from Israel have explored how terrorism affects the psychological health of remote populations. For instance, Israel suffered a wave of terrorist

attacks following the Al-Aqsa Intifada, which began in late September 2000 and lasted approximately five years. Over this period, Israel experienced 889 terrorist attacks, killing 1,042 persons and injuring 7,065 (Pat-Horenczyk et al., 2006). In response, in September 2003 researchers surveyed 913 adolescents (aged 12-18) from four different school locations (Braun-Lewensohn et al., 2010). Exposure was conceptualized as knowing someone who had been hurt in a terrorist attack or being physically exposed to an attack. These measures then were combined and aggregated at each of the four locations to create a composite exposure level for each of the four schools. Interestingly, rural youth from Southern Israel—where terrorist attacks are less common—experienced the least post-traumatic stress; however, adolescents living in the Jordan Valley or in Central Israel experienced higher levels of psychological distress compared to those from the most exposed area (i.e., where most terrorist attacks occurred) in Jerusalem. In trying to reconcile these counterintuitive findings, the authors suggest that there are likely underlying differences between the families who choose to live in rural vs. urban areas, which may affect their subjective vulnerability. Further, people living in Jerusalem areas where the terrorism risk is objectively highest may have benefited from prevention programs and mental health interventions.

Perhaps most noteworthy is the finding that the majority of adolescents in this Israeli sample (90%) experienced high rates of mild to severe PTS, even though one-third of participants indicated no objective exposure to terrorism (i.e., direct personal experience or through relatives). In response, the authors postulate that the 10% of the sample that did not experience PTS may have developed a resiliency towards terrorism (Braun-Lewensohn et al., 2010). Major limitations to this research include its reliance on self-report survey instruments

and its reliance on a cross-sectional design. Consequently, it is not clear what the underlying base-rate of PTS was within surveyed communities prior to the 2003 survey.

The March 11, 2004 terrorist bombings in Madrid, Spain, also prompted researchers to assess its psychological impact on the general population. In brief, Gabriel and colleagues (2007) assessed PTSD and major depression across three groups: persons injured in the attack (N=127), Residents (N=485) of Alcalá de Henares (a city within Madrid), and policemen involved in rescue operations (N=153) following the terrorist attack. PTSD was assessed 5 to 12 weeks after the terrorist attack using the Davidson Trauma scale, and other psychiatric illnesses were assessed via the Mini International Neuropsychiatric Interview (MINI). Rates of psychiatric illness amongst these three groups were 57.5%, 25.9%, and 3.9%, respectively. Of particular interest, prior psychoactive medication usage (before the terrorist attack) was the strongest predictor of PTSD and major depression among those injured, and of major depression and anxiety disorders among residents living in Alcalá de Henares (Gabriel et al., 2007). Additionally, police officers appeared to be much more psychologically resilient in the aftermath of the attack. Taken together, findings from this study show that terrorist attacks can impose a significant psychological burden both directly and indirectly against the affected community. Although these findings are consistent with past research, it is important to highlight that psychiatric measures were not assessed before the terrorist attack. Consequently, due to the usage of a weak research design (i.e., post-test only), findings should be interpreted cautiously.

Gadarian (2010) also utilized a novel experimental approach to investigate how terrorism impacts remote populations. In brief, she conducted a simulated experiment (known as the “Threat Experiment”), which included 1,229 adults recruited through YouGov/Polimetrix.

Importantly, the “terrorist event” introduced in the experiment was fictional. The experiment occurred online, and each participant was exposed to one of three conditions. The first condition (control croup) involved being exposed to a neutral and non-threatening story about the Indian economy. The second condition involved participants being exposed to terrorism news; although, the visual imagery was neutral and non-graphic. Condition three involved participants being exposed to scary visual imagery about terrorist events.

Gadarian (2010) found that respondents experiencing the scary-visuals condition felt significantly more negative emotions than the other groups. Her research further indicated that those who were exposed to the scary visuals about terrorism events were more likely to support “hawkish” policies to combat terrorism (e.g., more support for militarism, increased foreign policy spending, and support for the Iraq war). In sum, this research suggests that it is not simply exposure to terrorism news that determines emotional responses of viewers; rather, it is the sensationalistic portrayal of terrorism in the news that produces increased negative emotional responses. Further, this effect seems to influence political decision-making, whereby negative emotions tend to be associated with increased support for “protective” counter-terrorism policies. It is important to note that this study utilized simulated terrorist events, which may affect the generalizability of the findings.

More recently, Holman et al. (2014) compared how media consumption of the April 15, 2013 Boston Marathon bombing attack versus direct exposure to the attack affected acute stress responses. To accomplish this, an online survey was administered approximately 2 to 4 weeks after the attack to respondents in Boston (N=846), New York City (N=941) and across the U.S. (N=2,888). To assess acute stress, questions were derived from the Stanford Acute Stress

Reaction Questionnaire (SASRQ). In addition to measuring levels of acute stress, participants were assessed for direct and/or indirect exposure to the 9/11 attacks, the Sandy Hook school shooting, and Superstorm Sandy. Direct exposure was considered when a participant (self or close other) experienced one of these prior traumas, whereas indirect exposure was considered when the participant consumed live media coverage about the event. Researcher's hypothesized that prior exposure to a collective trauma could render some individuals more vulnerable to the effects of subsequent collective trauma events. Also, media exposure to the Boston Marathon bombing was assessed, and participants were grouped into one of three categories based on their daily consumption of bombing news coverage (1.5-2.9 hours per day, 3-5.9 hours per day, and 6 or more hours per day). Other control variables assessed include prior mental health, sex, education level, and ethnicity.

The results of this study reveal that participants in Boston and New York had similar acute stress symptom scores, while scores were lower nationwide. Most striking was the finding that respondents who watched more than 6 hours of daily news coverage about the Boston Marathon bombing were greater than nine times more likely to report high acute stress than respondents who had minimal media exposure of the attack. Additionally, prior direct exposure to the 9/11 terrorist attacks and the Sandy Hook School shooting were both significantly associated with acute stress; however, exposure to Superstorm Sandy was not. In other words, exposure to a prior collective trauma may render some individuals more at risk to acute stress when a similarly violent collective trauma reoccurs.

Holman, Garfin, and Silver (2014) also concluded that repeated engagement with trauma related media content for hours each day following a collective trauma can promote substantial

stress related symptoms. They further argued that "mass media may become a conduit that spreads negative consequences of community trauma beyond directly affected communities" (Holman, Garfin, & Silver, p. 93). The authors also emphasized that the media can play a crucial role in disseminating messages of solidarity and resilience following a collective trauma; however, they caution that watching too much news coverage of the attack may produce harmful psychological effects. Their recommendations to media outlets were to avoid disseminating graphic imagery and warning viewers if such content is to be shown.

Lastly, it is important to note that certain characteristics of terrorist attacks have been associated with greater news coverage. For example, in a review of all news coverage of terrorism incidents in the U.S. between 1980 and Sept. 10, 2001, Chermak & Gruenewald (2006) concluded that most terrorism incidents receive little or no news coverage; however, certain incidents are sensationalized in the news and do receive substantial coverage. Cases that receive increased news attention include incidents with casualties, links to domestic terrorist groups, airlines being targeted, or when a hijacking is used as a tactic. More recently, Kearns (2019) examined all of the terrorist episodes in the U.S. that are included in the Global Terrorism Database (GTD) between the years of 2006 and 2016, or a total of 136 terrorist episodes. The key finding from this study was that when controlling for target type, fatalities, and being arrested, attacks by Muslim perpetrators received approximately 357% more coverage than other attacks. Accordingly, details of a particular terrorist attacks may be associated with greater news coverage; consequently, such details may be important factors that help explain why certain events generate a larger "trauma dose" on society, whereas others may not.

Empirical Studies: Remote Populations (Police Killings)

Although this topic lacks a terrorism or mass-shooting focus, a recent study by Bor et al. (2018) played an important role in shaping current research efforts. This study investigated the impact that police killings of unarmed black men have on the mental health of remote black populations (i.e., black people who live in the State in which the shooting incidents occurred). In brief, Bor et al. (2018) utilized a population-based, quasi-experimental design with individual level data from the CDC's Behavioral Risk Factor Surveillance System (BRFSS) to estimate the impact of police killings of unarmed black American adults in the U.S. between the years of 2013-2015.

In analyzing the data, the researchers utilized difference-in-differences multivariate regression models specifying the number of poor mental health days as the outcome variable (derived from the BFRSS data) and the number of police killings of unarmed black Americans in the 3 months prior to interview (derived from the Mapping Police Violence database) as the primary exposure of interest. Additionally, Bor and colleagues (2018) adjusted for a variety of fixed effects, including year-month fixed effects to account for national secular trends in the outcome; state-month fixed effects to account for time-invariant state-level confounders and state-specific seasonality in mental health; day-of-week fixed effects; and individual-level age group (ranging from 18–24 years to 80 years and older in 5-year intervals), sex, and level of education fixed effects. They also conducted a falsification test by measuring these outcomes among a sample of Black Americans prior to the police shootings, to see if the effect only happens after the shootings.

Results from Bor and colleagues (2018) indicated that each additional police killing of an unarmed black American across each 90-day timeframe was associated an additional 1.7 poor mental health days per person per year, or a total of 55 million excess poor mental health days per year among black American adults in the USA. To put this in perspective, the researchers estimated that diabetes might be responsible for an additional 75 million poor mental health days among black Americans. Thus, police killings of unarmed black Americans produce a comparable mental health effect to that which diabetes imposes on black Americans.

Alternative Explanations

In light of the research reviewed in this chapter, if applying dose-response theory fails to uncover relationships between mass-shooting and terrorist attacks and the mental health outcomes of the public, then alternative explanations may be necessary. In such a case, this section offers three alternative possibilities: psychophysical numbing, the availability heuristic, and the influence of other mental health risk factors.

First, there is a possibility that lethal terrorist and mass shooting attacks have no appreciable effect on the mental health of the general population. Psychological research on emotional affect in the aftermath of genocide and mass violence suggests that this may be the case. In the simplest terms, the numbers of deaths associated with episodes of mass violence—no matter how large the numbers—ultimately fail to convey the true meaning behind an atrocity. According to Slovic and colleagues (2007), these figures merely represent “dry statistics” or “human beings with the tears dried off” (p. 79).

Mother Teresa captures the essence of “dry statistics” by stating “if I look at the mass, I will never act” (Slovic, 2007, p. 80). A quote from Dillard (1999) helps to further illustrate this point: “There are 1,198,500,000 people alive now in China. To get a feel for what this means, simply take yourself — in all your singularity, importance, complexity, and love — and multiply by 1,198,500,000. See? Nothing to it” (p. 47). Dillard is obviously joking when she says there is “nothing to it.” What she means is that the human brain is not equipped to handle the task. Despite how hard one tries, no person can multiply their own human condition by the millions. Similarly, Nobel Prize winning biochemist Albert Gyorgi struggles with the same problem when contemplating the possible suffering of a nuclear war: “I am deeply moved if I see one man suffering and would risk my life for him. Then I talk impersonally about the possible pulverization of our big cities, with a hundred million dead. I am unable to multiply one man’s suffering by a hundred million” (Slovic et al., 2007, p. 86). Even Joseph Stalin—one of the most brutal and murderous dictators to have ever lived—seemingly understood this phenomenon when stating “One death is a tragedy; one million is a statistic” (Shapiro, 2006, p. 724).

According to Slovic and Slovic (2015), humans suffer from “psychophysical numbing,” whereby even the most mathematically gifted human beings are psychologically limited when it comes to attaching feeling to numerical information. They help illustrate this problem by describing the psychophysics of brightness. For instance, in a dark room where one candle is lit, the room becomes noticeably brighter. A second candle adds more light, but not to the same degree that the first one did. As more candles are added, the relative impact that they each have on the brightness of the room becomes less and less significant. In effect, the eye increasingly loses sensitivity to changes in brightness as more candles are added. As Slovic and Slovic

(2015) argue, this phenomenon holds true when we consider the emotional impact of a natural disaster or manmade tragedy. As the number of deaths increase, so too does our insensitivity to the data.

As a consequence of psychophysical numbing, larger numbers fail to trigger the emotion or feelings necessary to motivate action. For instance, if you hear of news that a terrorist attack has killed 12 people in your home state, it will likely evoke a negative emotional response. Let us now say that the number of people killed in the attack was 18 instead of 12. The negative emotional response one has to either scenario could be the same. Psychophysical numbing would suggest that while the loss of lives from the attack has increased, the ability to appreciate each loss has not—or perhaps the change is so miniscule that it is not noticeable.

Research on psychophysical numbing is supportive of the theory. For example, Fetherstonhaugh and colleagues (1997) investigated how willing the public would be to fund various life-saving interventions for a hypothetical grant-funding agency. Respondents (165 University of Oregon Undergraduate students) were asked how many lives would need to be saved in order for the agency to receive $10 million in grant funds. Approximately two-thirds of the respondents believed that funding was appropriate when there was a larger at-risk population, with a median value of 9,000 lives needing to be saved out of a 15,000 at-risk population (implicitly valuing each life at $1,111), as compared to a median 100,000 lives needing to be saved out of a 290,000 at-risk population (implicitly valuing each at $100). Accordingly, it was deemed more important to save a larger proportion of a smaller population than it was to save greater than ten times as many lives. Put another way, life became less valuable and worthy of financial aid as the numbers of at-risk people increased.

A separate study on psychophysical numbing produced similar results as Fetherstonhaugh and colleagues (1997). In this research, 159 students at the University of Pennsylvania were asked to give $5 of their earnings from a paid psychological study to the charity *Save the Children* (Small et al., 2007). In condition one, participants were asked to feed a seven-year-old African girl named Rokia. In condition two, participants were asked to donate to Rokia; however, participants also were presented with African starvation statistics, which illustrated that millions more children were in need. Interestingly, condition two experienced a 40% decrease in donation contributions compared to condition one. The authors concluded that "statistical information dampens the inclination to give to an identifiable victim" (Small et al., 2007, p. 149).

While each of these studies (Fetherstonhaugh et al., 1997; Small et al., 2007) illustrate how people tend to be less giving when asked to help more people, they ultimately fail to capture the emotional affect that participants feel when they make these types of decisions. A follow-up study at Hebrew University attempted to address this issue. Here, 112 students were used to measure willingness to contribute to a victim in need (based on whether the victim was an individual or a group), as well as level of empathy and distress (Kogut & Ritov, 2005). The victims in this scenario were presented as either a sick child or a group of eight sick children who were being treated in a medical center and whose lives were in danger. Importantly, both conditions (individual and group) had the same basic background and storyline. The results revealed that people tended to feel more distress and empathy when they were considering a single identified victim, rather than when they were considering a group of identified victims. Furthermore, and in line with previous research, participants were significantly more likely to contribute to the single identified victim than the group victim category.

A follow-up study was conducted by Västfjäll and colleagues (2014) in order to assess the emotional affect of 107 undergraduates at a Swedish university given a scenario in which they were asked to donate to either one, two, or eight victims who were in need. Physiological measures of affect were measured in the facial Zygomaticus Major (ZM), while the participants were viewing the children in need and determining whether or not they would donate. Activity in the ZM region has been associated with self-reported pleasant emotions and is expected when a person exhibits feelings of compassion or the anticipated warmth of helping another person in need. In line with prior research, the researchers believed that positive affect would be highest when participants were put in a scenario where they would donate to a single child. The results confirmed this expectation. Participants donated more to a single victim, and had a higher physiological response (positive affect) when doing so, than in other scenarios with more victims. The authors concluded that these findings and those from previous research hint at a disturbing psychological tendency; specifically, our capacity to feel positive affect for people in need is extremely limited. Our positive affect appears to peak at a single person. Consequently, attention, feeling, and response appears to decline or fade with more than one victim, and these emotions eventually start collapsing at some higher value of N—a point at which victims become merely statistics.

Most recently, Baucum and John (2020) created a series of terrorist attack vignettes that either emphasized a conventional attack (explosives and firearms) or an unconventional attack (chemical, biological, radiological, and nuclear) with variation in the timing of deaths (instantaneous or delayed fatalities). Participants were randomly assigned to one of the vignette scenarios that had killed (or was expected to kill) either 20, 60, 200, 600, 1,800, 5,400, 16,400,

or 50,000 people. The 684 respondents then were asked to rate their fear, anger, and severity of a simulated terrorist attack (0-10 points were possible on each of the three measures). The findings were striking: whether 20 people were killed or 50,000 people were killed did not affect rates of fear and anger in any significant way. For instance, when 20 people were killed, average fear scores were 6.79, and when 50,000 people were killed, average fear scores were 6.77. Similarly, when 20 people were killed, average scores of anger were 7.47, compared to 7.66 when 50,000 people were killed. Importantly, these similarities were not just found at the low and high ends of the death toll. Each level of death toll (20, 60, 200, 600, 1,800, 5,400, 16,400, and 50,000) reflected remarkably similar fear and anger scores.

In contrast, the self-reported severity scores did increase at every level of the death toll. The average severity scores ranged from 6.31 (20 killed) to 8.44 (50,000 killed). It is important to note that while these scores did reflect incremental increases in self-reported rankings of severity, responses were not proportional to the increases in death tolls. For example, the severity score difference from 20 killed to 60 killed was .48. The severity score difference from 16,400 to 50,000 deaths was .29. Accordingly, while there were increases overall in severity scores, these scores do not reflect the death toll increases in a statistically proportionate way. Instead, this research suggests that our ability to evaluate the severity of an attack is extraordinarily crude, as very large differences in death toll may only be perceived very slightly. Baucum and John (2020) conclude by stating, there appears to be a "potential bifurcation between the public's affective and cognitive evaluations of terror attacks" (p. 399). Most striking is the fact that death tolls were found to be unrelated to both fear and anger scores. While this study provides additional supportive evidence of psychophysical numbing, it does

have its limitations. Perhaps most significantly, the terrorist events were hypothetical scenarios and therefore may lack real-world generalizability.

In addition to the possibility of psychological numbing, the "availability heuristic" may also assist in better understanding how terrorism and mass shooting incidents influence the general public's mental health. Tversky and Kahneman (1973) define the availability heuristic as "the process of judging frequency by the ease with which instances come to mind" (p. 207). Importantly, personal experiences, pictures, and vivid examples are more available to an individual than other events that happen to other people (i.e., statistics). For example, if someone has a bad experience with a police officer, this may have a disproportionate impact on how they view law enforcement, despite what other news articles they may have read that could indicate otherwise.

In similar fashion, the events under review in the current research are unlikely to directly involve the overwhelming majority of people residing in the state where an attack occurs. Their experiences are both less personal and vivid than those who were at the attack scene or who lost a loved one. Consequently, the availability heuristic would suggest that remote populations are less likely to perceive these events as memorable. While the public may be able to recall general details gleaned from news media, they are much more likely to lack the first-hand vivid experience of the attack or have a personal connection (e.g., knowing the perpetrator or a victim) to the crime scene. In contrast, the availability heuristic would indicate that if someone witnessed the attack first-hand and/or lost a loved one, then the event would be perceived as more memorable. If a terrorist attack or mass-shooting event fails to leave an indelible mark on

the collective conscious of the general public, then it also seems less likely that the general public would be psychologically affected.

Research on fear of crime helps to illustrate the availability heuristic. More specifically, from 1993-2018 violent crime dropped across the country from 79.8 violent crimes per 1,000 people (age 12 and older) to 23.2 (Pew Research Center, 2020). One might expect the general publics' view to correspond to these dramatic changes. However, this did not occur. While initial perceptions of violent crime dropped in the anticipated direction during the mid-1990s, they actually climbed back up to higher perceived rates while crime continued to drop. In effect, the general publics' perceptions of violent crime appear to be detached from reality (i.e., official data statistical trends).

The likely explanation for this phenomenon is that the public continues to learn about crime through news and media coverage. On any given day in the U.S., there is a surplus of crime events for news media to cover. These newsworthy events obscure the fact that there has been a steady decline of violence in the U.S. over several decades. The public is more inclined to remember sensationalistic news stories and forget or ignore statistical realities. The "if it bleeds it leads" paradigm in the news media ensures that coverage of sensationalistic violence is likely to persist no matter what the statistical trends of violence are in reality. Consequently, public perception remains detached from real world trends, which is precisely what the availability heuristic would predict. This same phenomenon also helps explain public perceptions of a terrorist threat. As was discussed in the Introduction chapter, fears of terrorism among the U.S. general population have changed little since 9/11. This is despite the fact that an estimated 13 people died annually from terrorism in the U.S. from 2002-2016 (START, 2017). If

statistical realities were taken into stronger consideration, we might expect to find that perceptions of terrorist threats and fear of terrorism would have diminished significantly in the nearly two decades following 9/11.

Officer involved police-shootings offer yet another illustration of the availability heuristic at play. For example, the number of people who have been killed by the police has been relatively constant during the 21st century (Zimring, 2017). This is not a problem that spontaneously appeared in the U.S. on the date of Michael Brown's death in 2014. What has changed is how these events are covered by law enforcement and the media. Prior to 2015, there was no sustained effort across the country to capture these events. Official data counts systematically underreported the death toll numbers (ranging around 300-500) in any given year. However, in 2015, the Washington Post (2020) began collecting data to produce a more comprehensive database, which estimates about 1,000 people die at the hands of police every year.

While the killing of Michael Brown (and subsequent fatal incidents involving the police) arguably ignited a firestorm of national attention on this issue, the reality is that police killings of this sort have been the status quo. Instead, what changed is how the public perceives these events, which is likely due to the dramatic uptick in news reporting of these events. Specifically, analysis of New York Times and Washington Post police shooting coverage from January 2014-July 2015 reveals a substantial increase of news coverage following the August 2014 Michael Brown killing (Zimring, 2017). Simply put, vivid and powerful images communicated through the media appear to be more determinative of how the public perceives these violent threats than does statistical information.

These three aforementioned examples (crime rates, terrorism incidents, and police shootings) illustrate that statistical realities are generally poor predictors of what the public thinks and feels. Accordingly, if statistical realities are poor predictors of how the public perceives public safety threats, then it would not be surprising to see that the mental health rates under review in this study remain equally detached when violent incidents do or do not occur.

Finally, it is also important to stress that the causes of mental health problems are complex. While much is still to be learned about the causes of mental illness, prior research has identified a myriad of genetic and environmental risk factors. According to the Mayo Clinic (2020), inherited traits, environmental exposures, and brain chemistry, as well as a variety of specific risk factors, increase the likelihood of mental illness. These risk factors include family history, life stress, chronic medical conditions, brain damage, trauma, substance use, childhood abuse or neglect, poor social relationships, and a prior mental illness.

To expand on this discussion, heritable traits have been linked to several mental health illnesses. For example, in 2013 researchers scanned thousands of genetic markers across 33,000 patients who had been diagnosed with at least one of the five major mental health disorders (autism, attention deficit hyperactivity disorder, bipolar disorder, major depression and schizophrenia), as well as a control group of 28,000 people with no psychiatric diagnosis (Serretti & Fabbri, 2013). Genetic variations were found to be associated with all five of the disorders. While these relationships were statistically significant, the authors concluded that can only account for only a small amount of risk for mental illness (Serretti & Fabbri, 2013). The National Institute of Mental Health (2020) also stipulates that while other factors likely play a

role in mental health, family history in particular may be one of the best indicators to determine a person's risk for developing a mental disorder.

Environmental exposures also have been shown to affect mental health rates. A common approach to demonstrate this relationship is to compare rates of income inequality with mental health outcomes. For example, a study examining older Americans (N=6,640, aged 70 or older), derived from the 1993-1994 Assets and Health Dynamics among the Oldest Old survey, found that income inequality was significantly associated with depression among older Americans (Muramatsu, 2003). These relationships were significant after controlling for demographic factors, SES, and physical health. In a separate study using data from the 2006 and 2008 BRFSS, researchers found associations between depression prevalence rates and income inequality (Messias et al., 2011). Simply put, the more unequal the income, the higher the rate of depression. This finding remained significant after adjusting for income per capita, education level of population, and percent over aged 65. Another study analyzing the County Health Rankings dataset, with measures derived at the county-level, similarly found that inequality is associated with a greater number of poor mental health days (Okulicz-Kozaryn, 2014).

In terms of chronic disease, research consistently has found a link to depression. Specifically, depression is found to co-occur in in 17% of cardiovascular cases and 23% of cerebrovascular cases (American Heart Association, 2014), as well as 27% of diabetes patients and greater than 40% of individuals with cancer (Lasser et al., 2000). Additionally, other types of mental illness also have been found to be associated with diabetes, obesity, asthma, arthritis, and cardiovascular disease (Chapman et al., 2005).

Furthermore, chronic stress is often associated with depression and anxiety (National Institute of Mental Health, 2020). According to Hammen's review of the research on stress and depression (2005), a substantial body of research illustrates that most episodes of major depression are preceded by stressful life events. A separate literature review of 14 studies conducted by Mazure (1998) concluded that stressors were 2.5 times more likely in depressed patients compared to controls. Further, when looking at community samples (eight studies), approximately 80% of depressed cases were preceded by major life events. Relatedly, a meta-analysis of 77 articles revealed that life-stress was a significant risk factor for PTSD in adult populations (Brewin, Andrews, & Valentine, 2000)

Past trauma can also play an important role in future mental health. Risk of physical and mental health issues typically goes up with the number of past traumatic events experienced (Harvard Health Publishing, 2019). In particular, individual risks are heightened with three or more of these experiences, which may include physical abuse, sexual abuse, emotional abuse, physical neglect, emotional neglect, witnessing domestic violence, substance misuse (within the household), mental illness (within the household), parental separation or divorce, and incarceration of a household member. There are additional types of events that can induce trauma that do not meet psychological requirements for the definition of trauma, such as a sudden death in the family, a stressful divorce, or caring for someone with a chronic or debilitating illness.

The strength of an individual's social relationships is also determinative of mental and physical health. Social relationships, in terms of both their quantity and quality, affect mental health, health behaviors, and mortality risk. Importantly, those who are most socially isolated

are at the greatest risk of poor health and early mortality (Umberson and Montez, 2010). Older adults who have been widowed and who are more socially isolated are particularly vulnerable. For example, in a community survey of older adults (N=110), those who reported fewer social support resources were associated with higher rates of depression and suicidal ideation (Vanderhost and McLaren, 2005). A recent systematic review of 63 studies, assessing the relationship between social environment and mental health among U.S. adults with disabilities, found that weaker social networks were related to depression, while family functioning, social interactions, and relationship quality were associated with mental health and wellbeing outcomes (Tough, 2017). As mentioned, social relationships also affect physical health. Of note, a meta-analytic review across 148 studies found that those with stronger social relationships had a 50% increased survival likelihood (Holt-Lunstad et al., 2010). This finding remained consistent across age, sex, initial health status, and cause of death.

In sum, the principles of psychophysical numbing and the availability heuristic suggest that our responses to remote terrorism and mass-shooting events may be significantly dampened when compared to those who experience the trauma in more direct ways. Because remote populations are less physically and socially proximate to the attack itself, these attacks may appear as less memorable. Further, the human mind remains ill-equipped to interpret statistical changes. This means that the details surrounding an event, in terms of number killed or wounded, may have less effect on public reactions. Lastly, there are a multitude of risk and causal factors that affect mental health. It’s therefore plausible that some of these factors (or a combination of factors) exert more influence over mental health outcomes. In effect, any

emotional response felt by remote terror attacks may be obscured by a wide array of other—and perhaps more powerful—genetic, psychological, and environmental influences.

Summary and Conclusions

This literature review illustrates that both directly targeted populations and remote populations experience a wide array of negative mental health outcomes in response to mass-shooting and terrorism atrocities. However, there is an overall lack of quality empirical research on how remote populations are impacted. Also, it is worth highlighting that only four of the mass-shooting related studies utilize a pre-event measure (Addington et al., 2003; Brener et al., 2002; Kaminski et al., 2010; Stretesky & Hogan, 2001), with few observation points (usually one or two). Similarly, terrorism related research on this topic is scant and tends to suffer from similar methodological problems (e.g., no pre-test measures). It is also largely 9/11 centric.

The fact that much of the research examining how terrorism afflicts remote populations focuses on the aftermath of 9/11 is a significant limitation, as this tragedy is in several ways an extreme outlier. For example, the magnitude of the 9/11 events far exceed (in terms of casualties and national resonance) the magnitude of any terrorist event that has happened in the U.S. since then. For instance, the highest death toll from a terrorist attack during the years of 2012-2017 occurred on October 1, 2017, in Las Vegas, which killed 59 and wounded 851 people. Most terrorist attacks are relatively small in scale and kill far fewer people. For instance, between 2002 and 2016, 190 people (including the terrorist attackers) were killed in the U.S. due to terrorism, or an average of 13 people per year. In contrast, nearly three thousand people were killed during the 9/11 terrorist attacks (CNN, 2019). The 9/11 events also precipitated an entire

series of events, to include substantial increases in counter terrorism spending, the invasion of foreign countries, the reorganization of the national security apparatus (to include the creation of the Department of Homeland Security), and passing of the U.S. PATRIOT ACT (Kraft & Marks, 2017). In other words, the impact 9/11 had on the collective conscious of American society is likely substantively different from the attacks under review in this study. Consequently, all previously discussed 9/11 related research may not be sufficiently generalizable to the present-day context. As a further consideration, other factors that potentially influence responses to remote terrorism and mass shooting events include psychophysical numbing, the availability heuristic, and the combination and complexity of mental health determinants. Each of these factors suggests that remote populations are less likely than directly targeted individuals to suffer psychologically.

In the following chapter, a comprehensive breakdown of the proposed methodology of the current research is discussed. In brief, this study utilized a TSCS design, and a variety of statistical tests were performed to assess how terrorism and mass-shooting events impact the public's mental health. More precisely, a TSCS design with multivariate regression analysis (with fixed effects) was employed to determine the relative strength between key predictor variables (e.g., severity of terrorism/mass shooting event, total news coverage) and mental health outcomes across a wide array of events (N=80) between 2012-2017. In addition, because total media coverage surrounding each event was measured, this research helps illuminate the relationship between media and rates of poor mental health following terrorism and mass shooting tragedies.

Finally, in order to advance scholarship in this area, Lowe and Galea (2017, p. 62) recommended that pre-incident and longitudinal research is needed. The current research attempts to address the issues identified by Lowe and Galea. This study placed remote populations under principal investigation. Further, the methodological approach undertaken in this study captured pre-incident measures of mental health, as it employed a longitudinal design. Consequently, results of this study should be of greater generalizability, as conclusions are not be drawn from singular and/or potentially anomalous cases.

Chapter 3

Methodology

The purpose of this study was to assess how members of the general population residing in a state where terrorism or a mass-shooting occurs are psychologically impacted following a deadly event. As previously discussed, prior research has focused largely on the impact that these atrocities have on directly exposed populations (i.e., people who personally experience the event or who live in close physical proximity to the attack site). Few studies (both mass-shooting and terrorism related) have attempted to measure the effects on remote populations (i.e., not living in close physical proximity to the attack site). Accordingly, this study aimed to assess how remote populations are affected, as psychological measures were drawn from people living throughout each state. Additionally, prior research on this topic has been mostly cross-sectional, with a limited number of studies assessing any pre-event psychological measures. Considering this limitation, this dissertation sought to capture pre-event psychological measures both before and after the events of interest. Also, the methodology enabled an examination of the impact of a multitude of both terrorism and mass-shooting attacks across a 6-year span, enhancing the generalizability of the findings.

Research Question

R1: Does level of media exposure or level of violence surrounding mass shooting and terrorist attacks affect mental health outcomes of remotely exposed populations?

Hypotheses

H1: As media coverage of terrorist or mass shooting events increase, poor mental health days increase during the month of attack.

H2: As media coverage of terrorist or mass shooting events increase, poor mental health days increase in the month following the attack.

H3: As lethal violence and casualties associated with terrorist or mass shooting events increase, poor mental health days increase during the month of attack.

H4: As lethal violence and casualties associated with terrorist or mass shooting events increase, poor mental health days increase in the month following the attack.

Data

For this research, three existing datasets were utilized. These datasets are publicly available online. A fourth dataset was compiled by the researcher in order to measure media coverage surrounding terrorist and mass shooting events.

First, the Behavioral Risk Factor Surveillance System (BRFSS) dataset was leveraged to measure the mental health of U.S. respondents. The BRFSS is the nation's premier system of health-related telephone surveys that collect state data about U.S. residents regarding their health-related risk behaviors, chronic health conditions, and use of preventive services. The BRFSS includes data from all 50 states, as well as the District of Columbia and three U.S. territories. The interview process is structured and averages about 23-28 minutes per interview. Interviews are completed each month, 7-days a week, during both daytime and evening hours. In total, the annual BRFSS dataset includes over more than 400,000 adult interviews, making it the largest continuously conducted health survey system in the world (CDC, 2019).

In terms of the data pertaining to the independent variables, this study relies on two additional open-source datasets of terrorist and mass shooting events. According to Dugan and

Distler (2016), there are three major advantages to this approach. First, there is a synergistic relationship between terrorists and the media. In other words, terrorists depend on the media to attract public attention and build support for their cause. Violence attracts more attention than most other types of news stories (Jerin and Fields, 1994). Consequently, the media bias that exists in favoring terrorism coverage helps to ensure that most violent attacks are sufficiently covered. Second, the ubiquity of media coverage in the U.S. suggests that shocking events that occur in any area (i.e., rural or urban) are likely to draw attention of some type of media. The social media boom has also increased access to reporting and better enables real time event reporting. Lastly, collection efforts are mechanized through a "pipeline" of news and automating story selections, which is accomplished through machine-learning technology. The sheer volume of data that is aggregated in online databases allows researchers to have greater access to information about where and when events are occurring, as well as additional details needed to make determinations about which acts constitute "terrorism."

There are also several drawbacks to open-source data collection of terrorist attacks. For example, according to Dugan and Distler (2016), there may be a bias toward newsworthy events. However, given the scope of this study, this bias actually may not be problematic. A primary focus of this study is to better understand how violent newsworthy events impact mental health outcomes. Events that are not newsworthy are unlikely to generate significant public attention, and thus cannot conceivably impact the quality of the general population's mental health. In other words, it is the newsworthy events that are under principal investigation in this study. If, however, this study sought to establish a relationship based on terrorist attacker proximity to attack site, then this bias could more dramatically impact the results. Separately, open-source

databases also suffer from inconsistencies across sources. For example, the numbers of people killed per attack could vary across sources, and these numbers could change in days or weeks after the initial accounts.

Efforts to measure terrorism also are complicated by the fact that "terrorism" has many definitions. Part of this problem stems from the fact that governments and multilateral institutions operate on compromise: sometimes it is in the interest of a state/group to proclaim that a certain group is a terrorist organization, and other times it is not. For example, the U.S. declared Hezbollah a terrorist organization in 1997. In contrast, in 2013 the EU considered only the military wing of Hezbollah to be a terrorist organization, while the rest of the group continues to be viewed as a legitimate political entity (Robinson, 2020). Accordingly, states and multilateral institutions may be reluctant to apply the label when their own interests are at stake (e.g., fear of retaliation at home or towards their deployed peacekeepers). Importantly, among practitioners and academics, Hudson and colleagues (2020) have argued that the notion that terrorism lacks a consensus definition is more a myth than reality. Rather, they argue that the majority of scholars actually agree that terrorism includes the use of violence against civilians and/or noncombatants, is political, and aims to generate psychological impact. Confusion over the term happens largely because terrorism is a prominent policy issue, and thus attracts a great deal of attention from nonacademic experts who are not well-versed in the literature.

To determine which terrorist events to include in the analysis, the Global Terrorism Database (GTD), maintained and updated by the Study of Terrorism and Responses to Terrorism (START) center at the University of Maryland was employed. Since 1970, the GTD has tracked domestic, transnational, and international terrorist incidents throughout the world. For each of

the more than 180,000 terrorist attack cases currently logged in the GTD, there is a plethora of reliable information about each event, to include the date, location, weapons used, nature of target, number of casualties, and information on the individual or group perpetrator (if known). In total, there are at least 45 variables for each case; however, more recent attacks may have up to 120 variables per case, due to enhanced data collection capabilities. According to START (2019), the GTD is the most comprehensive unclassified database on global terrorist attacks in existence.

Data from the Mother Jones (2019) database was utilized to identify the characteristics of non-terrorism related mass shooting events. This dataset defines mass shootings as single incident events that take place in a public place, resulting in four or more victims killed by the attacker. However, in 2013 the minimum of four fatal casualties was reduced to a minimum of three fatal casualties. The Mother Jones dataset is updated by journalists and requires a minimum of three or more sources per event (Stanford Geospatial Center, 2018).

What sets the Mother Jones dataset apart from other mass shooting databases (e.g., the Stanford Mass Shootings in America database, Gun Violence Archive, Mass Shooting Tracker, Mass Shootings in America) is that shootings stemming from “more conventional crimes” (i.e., armed robbery, gang violence, and domestic violence) are excluded (Stanford Geospatial Center, 2018). The rationale for choosing the Mother Jones database over alternative databases primarily hinges on this fact. Events that occur in public places and are not conventional crime events are more likely to generate public attention.

For instance, it is likely that the details of most of the 382 mass shootings in the U.S. in 2016, or 346 mass shootings in 2017 (Gun Violence Archive, 2019), are not well known to the

general population. These figures are substantially higher than Mother Jones figures. To help put this in perspective, only 17 events are included in the Mother Jones dataset over the same 2-year period. The Gun Violence Archive database has significantly higher mass shooting counts, primarily because it includes more conventional crimes and has no fatality requirement (it only matters that four people were shot per incident). In contrast, it is plausible to assume that attacks occurring only in public areas (e.g., school, café, or movie theater) and where there is a multiple fatality requirement (three to four minimum) are more likely to become newsworthy headlines, and thus garner increased public attention. Past research has shown that news coverage is a powerful means through which individuals are indirectly victimized (Warr, 1994). The Mother Jones dataset previously has been used to assess firearm type, perpetrator age, and shooting venue (Brown et al., 2018), to compare U.S. mass shootings event rates to Australia (Lemieux et al., 2015), and to assess the relationship between mass shooting events and policy intervention rates (Sanders & Lei, 2018). Scholars at the Harvard School of Public Health and Northeastern University have also used the Mother Jones dataset to illustrate how public mass shooting events have tripled since 2011 (Cohen et al., 2014).

A fourth dataset of media coverage surrounding terrorism and mass shooting events was constructed as part of this project. Recently, Kearns and colleagues (2019) utilized the LexisNexis database and *CNN* to measure media coverage of terrorism events from 2006-2016. LexisNexis searches news articles from national sources, such as *The New York Times*, *Wall Street Journal*, *The Washington Post*, and *USA Today*, as well as local newspapers from around the country. The current research employs a similar strategy to Kearns and colleagues (2019). Specifically, this researcher was unable to access the LexisNexis database and instead relied on

US Major Dailies. US Major Dailies provides access to "the five most respected U.S. national and regional newspapers: *The New York Times*, *Washington Post*, *Los Angeles Times*, *Chicago Tribune*, and the *Wall Street Journal*" (ProQuest, 2020). In addition, articles from *CNN* and *FoxNews* also were included. Inclusion of *Fox News* helped to ensure that news headlines associated with more of the politically conservative segment of the population were captured.

Descriptive Statistics

A series of scatter plots and histograms initially were generated to examine the data, assess underlying trends and seasonal patterns, and identify any outliers (Bernal et al., 2017). Additionally, univariate descriptive statistics were computed (e.g., average number of news articles for a mass shooting event at a school, or average number of civilians killed per event). For each independent and dependent variable, means, variability, as well the kurtosis and skewness were generated and assessed.

Multivariate Regression with Fixed Effects

Although randomized control trials (RCT) have long been considered the "gold standard" for assessing the effectiveness of interventions, they are not always possible when conducting research at the population level. Furthermore, there is also a need to evaluate interventions that already have been implemented. It is also the case that in the area of public health, rigorous research of the observational variety has helped explain how we have come to understand the impact of motor vehicle laws, safe foods, control of infectious disease, and smoking's relationship with longevity, just to name a few. In fact, the dominance of observational research in the public health field led Robert Sampson (2010) to conclude: "Observational and

experimental science should instead be partners in crime" (p. 496). Similarly, Sampson (2010) argued that criminology's top ten list of policy-induced sources driving the crime decline in recent years is also dominated by non-experimentally motivated research.

This study adopted an observational approach. More specifically, it utilized a time series cross section methodology (TSCS) with multivariate regression (with fixed effects) as the mode of analysis. Time series analysis is useful for aggregate-level analysis and is particularly useful if the focus of the research is on change (Pickup, 2015). In fact, Marvel and Moody (2008) proclaimed that the time series cross section (TSCS) design is probably the strongest for aggregate data. Similarly, Campbell and Stanley (1967) refer to TSCS methodologies as excellent quasi-experimental designs and consider TSCS to be among the best of the most feasible designs.

Time series analysis is the application of statistical models to time series data to examine the movement of social science variables over time. Time series allows researchers to estimate relationships within (over time) and between variables in order to test causal hypotheses, make predictions, and assess the impact of policy changes (Pickup, 2015). Relatedly, cross-sectional analysis involves measurement of and between cases, but lacks a temporal component. Put simply, changes in outcome measures over time are not captured in strictly cross-sectional research. TSCS combines both of these temporal and spatial components. More precisely, this means that TSCS methodologies enable researchers to assess variables that vary over time, together with variables that vary across units (and not over time). Purely time-series or cross-sectional research cannot accomplish this (Fortin-Rittberger, 2014).

The combination of spatial and temporal dimensions in TSCS analysis yields several

advantages. First, data used in TSCS is often referred to as "pooled" because it combines *N* spatial units (e.g., states or provinces) and T time periods (e.g., months or years), which produces a set of $N \times T = NT$ observations (Fortin-Rittberger, 2014). By combining time and space dimensions together, the number of total observations increases, which boosts statistical leverage. Often researchers have too many potential explanatory variables to measure, but too few cases for empirical testing. A TSCS helps resolve this "small N problem" by increasing the total number of data points, thereby increasing degrees of freedom. Further, this approach also helps to reduce collinearity among the explanatory variables, thus improving the accuracy of statistical estimates (Fortin-Rittberger, 2014; Worrall & Pratt, 2004).

Second, TSCS methodologies allow researchers to study variables that remain limited either across space or time (temporally invariant) with variables that change. For example, different institutions of power (e.g., federal vs. state laws) may not change over time, while other variables (e.g., crime or employment rates) may fluctuate from one time period to the next. Regression analysis with pooled data from a TSCS method can accommodate both types of these variables (Hicks, 1994). This enables stronger theoretical testing because the variation between time and space components can be captured simultaneously. Accordingly, a TSCS design enables researchers to model more complex relationships between estimator variables than strictly time-series or cross-sectional methodologies (Fortin-Rittberger, 2014).

Third, "by utilizing information on both the intertemporal dynamics and the individuality of the entities being investigated, one is better able to control in a more natural way for the effects of missing or unobserved variables" (Hsiao, 1986, p. 5). In other words, TSCS designs help detect population heterogeneity, as these variables are generally unmeasurable and

unobservable. The addition of unit-specific dummy variables helps to model this heterogeneity (and is discussed in more detail below). By including unit specific dummy variables, these "fixed effects" allow for all slope estimates for the variables modeled to remain constant across both unit and time while the intercepts will vary by unit and time.

An important component of time series research design is that it requires a clear differentiation of the pre-intervention and post intervention periods (Bernal et al., 2017), also known as "defining the counterfactual." This is defined by extrapolating the underlying trends observed before the intervention to the post-intervention period (Bernal et al., 2018). Considering the proposed study includes six years of data (using monthly measures), this will not be an issue. Past research on mental health outcomes following the deaths of unarmed African Americans from police shootings found that the greatest number of "poor mental health days" of the African American general population (i.e., those not directly involved with the victimization that took place but residing in the state of interest) occurred within 1-2 months after the shooting (Bor et al., 2018). Consistent with these findings, it is anticipated that the number of poor mental health days is likely to increase most significantly somewhere between the 30 to 60-day window following a mass shooting or terrorist event.

Utilizing multivariate regression will enable a better understanding of the relationship between the independent and dependent variables. In a multivariate model, the coefficient of each independent variable provides the effect of the independent variable on the dependent variable, while holding all of the control variables constant. For example, assessing H_1 would include all of the independent variables and fixed effects in the multivariate model, and would utilize the "poor mental days" as the dependent variable measure. If the relationship between the

key predictor variable (news coverage) is statistically significant ($p>.05$), then the null hypothesis (there is no relationship between media coverage and poor mental health days) would be rejected. Conversely, if the relationship between the news coverage variables and poor mental days is not statistically significant ($p<.05$), this result would fail to reject the null hypothesis and point to the conclusion that there is no relationship between news coverage of terrorism/mass shooting events and poor mental health days within the units of analysis.

A TSCS multivariate regression analysis with fixed effects makes practical sense for this study, given that the data entails aggregated measures from the U.S. population within several cross-sectional units over time. This study initially utilized approximately 3,600 observation points (12 months x 50 states x 6 years), although this was ultimately reduced to 3,312 observations due to underreporting requirements. This far exceeds Marvel and Moody's (2008) recommendation of having a minimum sample of 200 observations for TSCS analysis.

Measurement

There is one measurement within the BRFSS dataset "Healthy Days" module that serves as the primary dependent variable. This measure will be used for both the month of attack, and the month after attack. It is important to stress that this question has been shown to have a high degree of internal validity, construct validity, criterion-related validity, test-retest reliability, and is widely used to monitor trends in population mental morbidity (Slabaugh et al., 2017). Further, this question has a response rate of greater than 98%, which demonstrates that the question is easy to answer (Cordier, 2018). Of note, Question 1 is also the primary question that Bor et al. (2018) utilized for their outcome variable when assessing the impact of police-involved

shootings of unarmed black people. The following question will be utilized to produce measurements of the dependent variables (month of attack and month after attack):

Question: Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?

The CDC's BRFSS dataset will be used to measure monthly state-level measures of mental health across all 50 states between the years of 2012-2017. Preliminary frequency distributions using the 2017 BRFSS data indicated that surveys are distributed fairly evenly throughout the year (to include month of year and day of month), and that the sample size (450,016 total interviews) is large enough to aggregate individual level measures into state level measures. Specifically, each month during 2017 between 7% and 9.9% of the sample was collected, which indicates that data collection is fairly stable throughout the year. Additionally, in 2017 only two states (Alaska and Nevada) reported less than four thousand interviews for the year, or less than approximately 333 interviews per month. Only seven states (Alaska, Idaho, Delaware, Nevada, Louisiana, North Carolina, & Wyoming) reported less than five thousand interviews for the year, or less than approximately 416 interviews per month. In line with Fowler's (2014, p. 35) Confidence Ranges for Variability Attributable to Sampling guidelines, when stipulating a 95% confidence interval, a simple random sample of 300 will generate a confidence interval of .3. This means that the true population statistic is 3 percentage points above or below the calculated population mean.

It is important to note that Fowler's sampling guidelines assume that the data adheres to a simple random sampling strategy. The BRFSS, however, uses a disproportionate stratified

sampling strategy. In order to provide adequate sample sizes for smaller geographically defined populations of interest, BRFSS samples disproportionately within a state from strata that correspond to sub-state regions. The BRFSS utilizes a disproportionate stratified sample for landline phones, and random sampling for cell phones. For landline phones, all telephone numbers within the state are grouped into either a high or medium density strata. The BRFSS then samples these two strata to obtain a probability sample of all households within the state that have landline telephones. For cell phones, the BRFSS utilizes the Telecordia database of telephone exchanges. Numbers are then randomly selected to sample cell phone users within the state (CDC, 2019). Use of disproportionate sampling is quite typical in research spanning larger geographic areas. For instance, almost all samples of populations of geographic areas are stratified by regional variables, and national samples are typically stratified by region of the country and by population density parameters (i.e., urban, rural, and suburban). Further, as long as the probability of selection is the same across all strata, it will not hurt the precision of sample estimates and is thus usually a desirable feature of a sample design (Fowler, 2014).

The BRFSS data are also weighted to help remove bias from the sample. Weighting protocols help to ensure that data are representative of the population on several demographic characteristics to include sex, education, age, race, marital status, home ownership, phone ownership and sub-state region (CDC, 2019). For example, if the sample of Missouri for the month of July is disproportionately female, iterative proportional fitting (also known as "raking" weighting) will adjust the responses to ensure the sample accurately represents the proportions of females in the target population. Because BRFSS is not a finished product with state level estimates, it will be necessary to weight the data. The BRFSS has made this process easier for

researchers by including a final weight variable. More specifically, BRFSS (2018) suggests that "researchers conducting analysis of the variables from the core-only section should use the variable _LLCPWT for weighting." The variables to be used (healthy days) are from the core section. In order to compute a final weight for each participant, BRFSS rakes the design weight to 8 margins (gender by age group, race/ethnicity, education, marital status, tenure, gender by race/ethnicity, age group by race/ethnicity, and phone ownership). Accordingly, when generating monthly measures for each unit of analysis, the variable (_LLCPWT) is applied for weighting to ensure that sample demographic characteristics are representative of the state.

The next set of measurements pertains to the details of terrorist attacks. In terms of what events meet the inclusion criteria for a terrorist act, the GTD defines a terrorist attack as "*the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation*." (GTD, 2019) Further, the following three criteria must also be met:

1. The incident must be intentional – the result of a conscious calculation on the part of a perpetrator.

2. The incident must entail some level of violence or immediate threat of violence - including property violence, as well as violence against people.

3. The perpetrators of the incidents must be sub-national actors. The database does not include acts of state terrorism.

Additionally, *at least two* of the following three criteria must be present for an incident to be included in the GTD:

1. Criterion 1: The act must be aimed at attaining a political, economic, religious, or social goal.

2. Criterion 2*:* There must be evidence of an intention to coerce, intimidate, or convey some other message to a larger audience (or audiences) than the immediate victims.

3. Criterion 3: The action must be outside the context of legitimate warfare activities. Accordingly, the terrorist incidents to be utilized will fulfill the above criteria and are included within the GTD database. Of particular interest, the GTD provides casualty figures (killed & wounded) for each event. This will be one of two ways to measure the severity of a terrorist event. The other way to measure event severity is based on level of media exposure.

Accordingly, the terrorist incidents utilized adhere to the above criteria and are included within the GTD database. Of particular interest, the GTD provides casualty figures (killed & wounded) for each event and assesses which group (e.g., white nationalist, anti-Semitic, jihadist, etc.) the perpetrator belongs to or is influenced by. Table 1 presents a breakdown of all 46 deadly (i.e., at least 1 person killed) terrorist events that occurred from 2012-2017 (AK, DE, NV, and LA are excluded in the trimmed sample). Attacks that were unsuccessful are not included in the analysis, as they are unlikely to generate significant public attention. Also, the most recent attack listed below (12/22/17) will not be included, as the outcome measures cannot be generated from the 2017 BRFSS dataset.

Table 1

Terrorist Attack Dates

Date	Location	Identity group	Killed	Injured
~~12/22/17~~	~~Harrisburg, PA~~	~~Unknown~~	~~1~~	~~1~~
12/7/17	Aztec, NM	White extremists	3	0
10/31/17	New York City, NY	Jihadi-inspired extremists	8	13
9/24/17	Antioch, TN	Unknown	1	8
8/12/17	Charlottesville VA	Neo-Nazi extremists	1	19
7/5/17	Bronx, NY	Anti-Police extremists	2	1
6/14/17	Alexandria, VA	Anti-Republican extremists	1	6
5/26/17	Portland, OR	Anti-Muslim extremists (suspected)	2	1
5/20/17	College Park, MD	Unknown	1	0
5/19/17	Tampa, FL	Muslim extremists	2	0
4/13/17*	Fresno, CA	Anti-White extremists	4	0
3/20/17	New York City	White extremists	1	0
2/22/17	Olathe, KS	White extremists	1	2
1/31/17	Denver, CO	Jihadi-inspired extremists	1	0
1/6/17	Fort Lauderdale, FL	Jihadi-inspired extremists	5	6
11/28/16	Columbus, OH	Jihadi-inspired extremists	1	11
9/17/16	St. Cloud, MN	Jihadi-inspired extremists	1	10
9/16/16	Philadelphia, PA	Anti-Police extremists	2	5

8/13/16	New York City	Unknown	2	0
7/7/16	Bristol, TN	Anti-Police extremists	1	4
7/7/16	Dallas, TX	Anti-White extremists	6	9
6/12/16	Orlando, FL	Jihadi-inspired extremists	50	53
2/11/16	Columbus, OH	Muslim extremists	1	4
12/2/15	San Bernardino, CA	Jihadi-inspired extremists	16	17
11/27/15	Colorado Springs, CO	Anti-Abortion extremists	3	9
11/4/15	Merced, CA	Jihadi-inspired extremists	1	4
10/1/15	Roseburg, OR	Incel extremists	10	7
7/16/15	Chattanooga, TN	Muslim extremists	6	2
6/17/15	Charleston, SC	White extremists	9	0
5/3/15	Garland, TX	Jihadi-inspired extremists	2	1
2/10/15	Chapel Hill, NC	Anti-Muslim extremists	3	0
12/20/14	New York City, NY	Anti-Police extremists	2	0
12/18/14	Morganton, NC	Jihadi-inspired extremists	1	0
11/28/14	Austin, TX	Anti-Government extremists	1	0
10/23/14	New York City, NY	Jihadi-inspired extremists	1	3
9/12/14	Blooming Grove, PA	Anti-Government extremists	1	1
6/25/14	West Orange, NJ	Jihadi-inspired extremists	1	0

6/6/14	Cumming, GA	Sovereign Citizen	1	1
6/1/14	Seattle, WA	Jihadi-inspired extremists	2	0
5/23/14*	Isla Vista, CA	Incel extremists	7	13
4/27/14	Seattle, WA	Jihadi-inspired extremists	1	0
4/13/14*	Overland Park, KS	White extremists	3	0
11/1/13	Los Angeles, CA	Anti-Government extremists	1	4
4/17/13	West, TX	Unknown	15	151
4/15/13*	Boston, MA	Muslim extremists	6	280
2/7/13	Corona, CA	Anti-Police extremists	1	1
8/5/12	Oak Creek, WI	White extremists	7	4

Note. Retrieved from https://www.start.umd.edu/gtd/search/ Copyright (2009-2019) by University of Maryland.

It is important to note that five of the terrorist attacks span several days and involve multiple acts. For example, the Boston marathon bombing occurred on 4/15/2013, yet over the next few days the assailants committed additional attacks in Cambridge (4/18/13) and Watertown (4/19/2013), while attempting to flee from law enforcement. Events where the perpetrators manage to flee and are involved in more attacks immediately thereafter are treated as a singular event with a combined casualty figure count from all of the related attacks. An asterisk has been placed by the dates of the five combined events.

Because this study sought to understand the state level mental health impact of terrorist and mass shooting events on the general population (i.e., people not directly exposed or near the attack site), it is imperative that most of the general population be at least aware of these events. Consequently, the Mother Jones dataset likely provides the most accurate summary of all publicly noteworthy mass shooting events in the United States. Table 2 provides a breakdown of

all mass shooting events included in the Mother Jones dataset from 2012-2017. Please note that events that are struck-through are duplicated in the GTD database.

Table 2

Mass Shooting Attack Dates

Date	Location	Killed	Injured
11/14/17	Rancho Tehama, CA	5	10
11/5/17	Sutherland Springs, TX	26	20
11/1/17	Thornton, CO	3	0
10/18/17	Edgewood, MD	3	3
~~10/1/17~~	~~Las Vegas, NV~~	~~58~~	~~546~~
6/14/17	San Francisco, CA	3	2
6/7/17	Tunkhannock, Pennsylvania	3	0
6/5/17	Orlando, Florida	5	0
5/12/17	Kirkersville, Ohio	3	0
~~4/18/17~~	~~Fresno, California~~	~~3~~	~~0~~
~~1/6/17~~	~~Fort Lauderdale, Florida~~	~~5~~	~~6~~
9/23/16	Burlington, Washington	5	0
~~7/17/16~~	~~Baton Rouge, LA~~	~~3~~	~~3~~
~~7/7/16~~	~~Dallas, Texas~~	~~5~~	~~11~~
~~6/12/16~~	~~Orlando, Florida~~	~~49~~	~~53~~
2/25/16	Hesston, Kansas	3	14
2/20/16	Kalamazoo County, Michigan	6	2
~~12/2/15~~	~~San Bernardino, California~~	~~14~~	~~21~~
~~11/27/15~~	~~Colorado Springs, Colorado~~	~~3~~	~~9~~

10/31/15	Colorado Springs, Colorado	3	0
~~10/1/15~~	~~Roseburg, Oregon~~	~~9~~	~~9~~
~~7/16/15~~	~~Chattanooga, Tennessee~~	~~5~~	~~2~~
~~6/17/15~~	~~Charleston, South Carolina~~	~~9~~	~~1~~
6/11/15	Menasha, Wisconsin	3	1
10/24/14	Marysville, Washington	5	1
~~5/23/14~~	~~Santa Barbara, California~~	~~6~~	~~13~~
4/3/14	Fort Hood, Texas	3	12
2/20/14	Alturas, California	4	2
7/26/13	Hialeah, Florida	7	0
6/7/13	Santa Monica, California	6	3
4/21/13	Federal Way, Washington	5	0
3/13/13	Herkimer County, New York	5	2
12/14/12	Newtown, Connecticut	27	2
9/27/12	Minneapolis, Minnesota	7	1
~~8/5/12~~	~~Oak Creek, Wisconsin~~	~~7~~	~~3~~
7/20/12	Aurora, Colorado	12	70
5/20/12	Seattle, Washington	6	1
4/2/12	Oakland, California	7	3
2/21/12	Norcross, Georgia	5	0

Note. Retrieved from https://www.motherjones.com/politics/2012/12/mass-shootings-mother-jones-full-data/ Copyright (2020) by Mother Jones and the Foundation for National Progress.

Unlike the GTD, the Mother Jones database does not assess perpetrator motivation. Therefore, events that qualify as “mass shooting” events in the Mother Jones dataset could also qualify as “terrorism” events in the GTD. Accordingly, 13 of the 40 mass shooting events fulfill the GTD’s criteria of terrorist attacks. This analysis treated these duplicated events as terrorist

events only, given that the “terrorism” label is more appropriate once additional considerations surrounding perpetrator motivation are critically assessed by researchers.

In similar fashion, the Mother Jones dataset does not include information on the identity group that the perpetrator belongs to or is influenced by. This is because motivation is not a relevant factor when deciding about whether or not an event fulfills the mass shooting criteria. Importantly, however, perpetrator motivation is central to understanding whether or not a terrorist act can be defined as such. The implications of this are worth mentioning. For instance, when considering the relationship between terrorist events and media coverage, the analysis will be able to explore these associations between different types of terrorism (e.g., jihadist, white nationalist, anti-government, etc.). Similarly, the analysis can assess if jihadist or white nationalist terrorism produces greater or weaker relationships to mental health outcomes. On the other hand, comparisons cannot be made between the various underlying motivations of mass shooters.

Compiling data from official sources in the U.S. (e.g., Federal Bureau of Investigation) is problematic because legal definitions often confound terrorism with criminal activity. For example, most acts of terrorism are not prosecuted as acts of terrorism; rather, they are prosecuted for related events (Lafree & Dugan, 2014). For example, upon examination of the 30 charges against the Boston marathon bombers, none specifically mention terrorism. Instead, Dzhokhar Tsarnaev was charged with conspiracy to use a weapon of mass destruction resulting in death, and possession and use of a firearm in relation to a crime of violence resulting in death, among other charges. Perhaps the most notable effort to collect terrorism data from official sources in the U.S. is the American Terrorism Study, which includes information on more than

9,000 people in the U.S. who were indicted on criminal accounts related to terrorism (Dugan and Distler, 2017). These cases are limited to only federally indicted cases in the United States. Additionally, many terrorists simply die in the process of carrying out their attacks; consequently, they cannot be effectively prosecuted as terrorists and therefore are excluded from the sample.

The other way event severity can be measured is based on level of media exposure. Due to the rarity of terrorism and mass shootings in this country, media plays a crucial role in shaping the public understanding about these events. To measure level of media coverage following a terrorist episode, this study employed a method similar to the one utilized by Kearns et al. (2019). They examined all of the terrorist episodes in the U.S. that are included in the GTD between the years of 2006 and 2016 (a total of 136 terrorist episodes). Because the current research will include the years of 2016 and 2017, the data collected by Kearns and colleagues (2019) is not complete, as it does not extend beyond 2015.

In order to measure media coverage, Kearns and colleagues (2019) focused on two primary sources: LexisNexis Academic and CNN.com. LexisNexis searches news articles from national sources, such as *The New York Times*, *Wall Street Journal*, *The Washington Post*, and *USA Today*, as well as local newspapers from around the country. Due to access restrictions of LexisNexis, the current research utilized US Major Dailies, which includes news articles from *The New York Times*, *Washington Post*, *Los Angeles Times*, *Chicago Tribune*, and the *Wall Street Journal.* Kearns et al. (2019) also utilized CNN.com archives to obtain additional news coverage that was produced solely in digital format. In terms of keyword searches, perpetrator(s) name (if known), location, and other keywords about the incident were used. The authors

emphasized that over-inclusion was the goal during the initial data collection phase. Afterwards, they created a final list of articles that only had attack location, perpetrator(s), or victim(s) as the primary focus. They then removed articles that listed every attack of a given type, political or policy focused articles where the attack or perpetrators were an anecdote to a larger debate (e.g., abortion or gun control), and articles discussing vigils being held in other locations.

In addition to supplementing US Major Dailies with CNN.com, it was deemed beneficial to include FoxNews.com digital archives in the current study, as this is where more of the politically conservative segment of the population may learn about terrorist or mass shooting events. Inclusion of FoxNews.com helped provide a more accurate estimate of the amount of news coverage (and across the mainstream political spectrum) for each terrorism and mass shooting event.

In summary, this portion of the study sought to measure the level of media exposure of mass shooting and terrorist events. Building off of Kearns et al. (2019), the current research included more recent years, mass shooting events (derived from the Mother Jones database), and an additional primary news resource (Fox News). Measured variables then were utilized to assess the relationship between media exposure and mental health outcomes. Table 3 depicts the results produced through compiling the terrorism and mass shooting news coverage dataset.

Table 3

News Coverage Dataset

DATE	TYPE	Total Victims	News
1/1/18	TERR	2	0

12/7/17	TERR	3	1
11/14/17	MS	15	15
11/5/17	MS	46	106
11/1/17	MS	3	7
11/1/17	TERR	21	61
10/18/17	MS	6	4
10/1/17	TERR	910	226
10/1/17	TERR	9	12
9/12/17	TERR	2	3
8/12/17	TERR	20	42
7/5/17	TERR	3	11
6/14/17	MS	5	5
6/14/17	TERR	7	33
6/7/17	MS	3	0
6/5/17	MS	5	7
6/1/17	TERR	3	30
5/20/17	TERR	1	2
5/19/17	TERR	2	1
5/12/17	MS	3	1
4/13/17	TERR	4	15
3/20/17	TERR	1	2
3/1/17	TERR	3	16
2/1/17	TERR	1	0
1/6/17	TERR	11	65
12/1/16	TERR	12	31
10/1/16	MS	5	16
9/17/16	TERR	11	11
9/16/16	TERR	7	6
8/13/16	TERR	2	37

7/17/16	TERR	7	55
7/7/16	TERR	5	2
7/7/16	TERR	15	100
6/12/16	TERR	103	239
3/1/16	MS	17	20
2/20/16	MS	8	33
2/11/16	TERR	5	5
12/2/15	TERR	33	173
12/1/15	TERR	12	52
11/4/15	TERR	5	9
11/1/15	MS	3	4
10/1/15	TERR	17	85
8/1/15	TERR	12	36
7/16/15	TERR	8	79
6/17/15	TERR	9	91
6/11/15	MS	4	2
5/3/15	TERR	3	38
3/20/15	TERR	3	3
2/10/15	TERR	3	22
12/20/14	TERR	2	36
12/18/14	TERR	1	0
12/1/14	TERR	1	9
11/1/14	MS	6	34
11/1/14	TERR	4	5
9/12/14	TERR	2	19
7/1/14	TERR	1	0
6/8/14	TERR	5	18
6/6/14	TERR	2	1
6/1/14	TERR	2	1

6/1/14	TERR	20	63
5/1/14	TERR	1	0
4/13/14	TERR	3	29
4/3/14	MS	15	101
2/20/14	MS	6	4
11/1/13	TERR	5	45
9/16/13	MS	20	105
8/1/13	MS	7	4
6/7/13	MS	9	44
4/21/13	MS	5	2
4/17/13	TERR	166	64
4/15/13	TERR	286	359
3/13/13	MS	7	2
2/7/13	TERR	2	9
12/14/12	MS	29	119
10/1/12	MS	8	6
8/5/12	TERR	11	53
7/20/12	MS	82	134
5/20/12	MS	7	9
4/2/12	MS	10	28
2/21/12	MS	5	4

Note: MS=Mass Shooting, TERR=Terrorism

The number of news articles per event ranges from 0-359, with a combined average (mass shooting and terrorism) of 37.1 news articles per incident. As depicted in Table 4, terrorism incidents tend to produce more casualties than mass-shooting events (18.4 victims as opposed to 12.3 victims) and generate more news coverage (42.7 articles per event versus 27.3 articles per event). The top five incidents garnering the most attention from the press include: Boston, MA (4/15/13, terrorism), Orlando, FL (6/12/16, terrorism), Las Vegas, NV (10/1/17,

terrorism), San Bernardino, CA (12/2/15, terrorism), and Aurora, CO (7/20/12, Mass shooting).

Table 4

News Coverage Dataset Averages

Variable	Mean # of Victims	Mean # of News Articles
Mass shooting	12.3	27.3
Terrorism	18.4	42.7
Combined	16.2	37.1

Independent Variables

Table 5 lists independent variables that were utilized for statistical analysis in this study.

Table 5

Independent Variables

Independent Variable	Measurement
Attack	Whether or not a terrorist or mass-shooting attack occurred for a particular state during the particular month. Dichotomously coded (1 = attack occurred, 0 = no attack).
Mass shooting attack	Whether or not a mass-shooting attack occurred for a particular state during the particular month. Dichotomously coded (1 = mass shooting attack occurred, 0 = no attack).
Terrorist attack	Whether or not a terrorist attack occurred for a particular state during the particular month. Dichotomously coded (1 = terrorist attack occurred, 0 = no attack).
Attack Severity	Combined casualties from the mass shooting or terrorist attack (killed and wounded). The total number constitutes the number of casualties for the state during the month.
Media Coverage	Quantitative value of total electronic news coverage for each terrorist and mass shooting event (measured across 14-days following the attack).
Terrorist Attack by Ideology	Categorical variables consisting of the following categories: white supremacist, anti-government, jihadi inspired, & other

Attack Location	Categorical variables consisting of the following categories: school, religious place, & other public space
Spatial Lag	Spatial Effects model. The outcome measure (# of poor mental health days in past month) is averaged across all contiguous states for each month. For example, during January 2015 in Texas, this measure averages the number of poor mental health days in January 2015 in NM, OK, AR, and LA.
Temporal Lag	Lagged DV measure (T-1). This measure captures the outcome measure (# of poor mental health days in past month) from the month prior.
State	Fixed Effect
State-month	Fixed Effect
State-year	Fixed Effect

Attack type helps us better understand if perpetrator motivation (e.g., political or religiously inspired) makes the event more salient, which may impact the outcome measures differently. There is no available prior research that compares the differences between these two outcomes on mental health of the general population; however, comparing these differences may be worthwhile and shed light on whether the label "terrorism" or "mass shooting" is consequential towards mental health outcomes. Prior research also has shown that as attack severity increases, so too does media coverage surrounding the event (Chermak & Gruenewald, 2006; Kearns et al., 2019). Thus, attack location is included because it seems plausible that certain events (e.g., school shootings) may generate increased news coverage, and therefore exhibit a greater "dose" of trauma on society. Further, bivariate correlations are reported to help determine which event characteristics are associated with news reporting and victim totals.

Dependent Variables

Table 6 presents the outcome measures derived from the CDC's BRFSS dataset. Two outcome measures were generated from this data (for month of attack and month after attack).

Table 6

Dependent Variables

Dependent Variable	Measurement
Poor Mental Health (month of attack)	# of poor mental health days in past 30 days (summary measure). While population sizes differ, this is mitigated by the use of fixed effects.
Poor Mental Health (month after attack month)	# of poor mental health days in past 30 days (summary measure). While population sizes differ, this is mitigated by the use of fixed effects.

Conclusion

In summary, this study examined the association between lethal terrorist attacks and public mass-shooting incidents, with state-level mental health measures recorded during the month of and after each incident. Utilization of a TSCS research design with fixed effects enabled assessment of a multitude of terrorism and mass-shooting incidents across a 6-year period (2012-2017), while also carefully controlling for time and spatial trends. Such an approach allowed examination of both pre-incident and post-incident outcome measures, as monthly level measures were recorded across the entire 6-year period for all states under review. Further, unlike most prior research efforts tending to focus on a singular (and typically high-profile) incident, the current research assesses all lethal terrorism incidents and public mass-shootings carried out in the U.S. across a 6-year period. Thus, findings derived from this study should be of greater generalizability.

Chapter 4

Analysis and Findings

This chapter initially reports a series of descriptive statistics for both the outcome and predictor variables. Then, bivariate correlations are presented, which assisted in determining the variables meriting inclusion into the multivariate regression analysis. A variety of model diagnostics also were performed to ensure that the assumptions for TSCS analysis were met sufficiently. Finally, four multivariate regression models are presented, and the results are discussed.

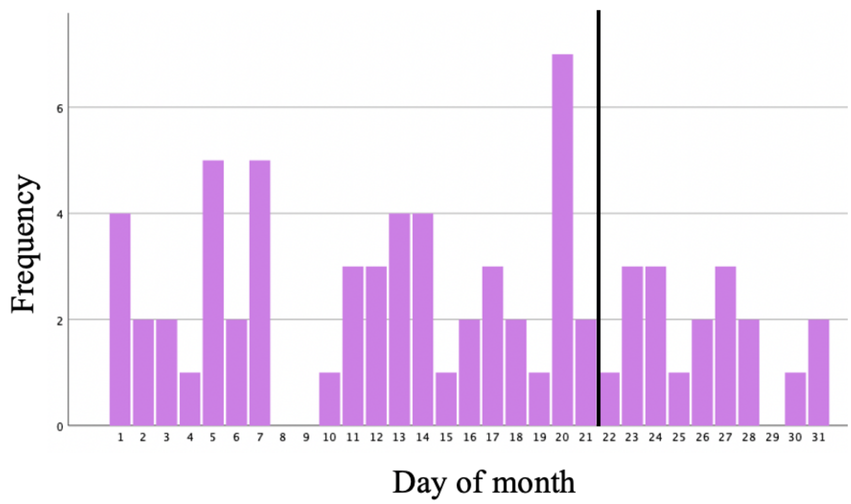
Descriptive Statistics

First, missing data concerns were mitigated before production of the descriptive statistic tables and figures. Specifically, 231 observations fell below the minimum observation sample size of 300 (see Appendix A). Thus, overall missing data from the entire sample of the dependent variable equates to 6.41%. When examining missing data on a state-by-state basis, missing data rates ranged from 0%-40%. Failing to account for missing data could pose a threat to statistical inferences. While there is no single established cutoff from the literature regarding an acceptable percentage of missing data in a dataset for valid statistical inferences (Dong and Peng, 2013), Schafer (1999) maintains that a missing data rate of 5% or less is inconsequential. Separately, Bennett (2001) maintains that statistical analysis is likely to be biased when greater than 10% of data are missing. In order to meet Schafer's (1999) more conservative 5% missing data figure, this study removed States with high missing data values (>20%), which included Alaska, Delaware, Louisiana, and Nevada. The trimmed sample contained 3,312 observations

across 46 states, with only 4.19% (139 observations) of data missing. Because the trimmed sample dropped four states from analysis, the total number of mass shooting and terrorism events under review in this study was reduced from 80 to 72.

Figure 1

Day of Month for Incident Data



Note. The solid black line denotes the days occurring beyond week 3; 18 events or 25% of all events occurred during this period.

Figure 1 illustrates the distribution of terrorist and mass-shooting events by day of month. Because this study utilizes monthly outcome measures, events that occur later in the month cannot be sufficiently captured by monthly measures without an adjustment to the event date. For instance, if a mass-shooting incident occurred on the 27th day of a month, only those interviewed during that month on day 27 or beyond could factor the impact of the mass-shooting into their interview response. Considering this, the current research utilized the following

month's outcome measures for any event that occurred beyond week 3 (days 22+). Thus, 18 of the 72 events (25% of the terrorist and mass-shooting incidents) utilized the following month's outcome measure for the number of poor mental health days over the past month. Overall, 75% of the events utilized the same month's outcome measures.

Histograms of the dependent variable for each state were produced and are reported in Appendix B. Table 7 depicts the descriptive statistics for the outcome measure (# of poor mental health days over past month) for each state in the trimmed sample across six years (2012-2017).

Table 7

Descriptive Statistics for Dependent Variable Measures by State

State	N	Mean	SD	Range	Skew	Kurtosis
AL	71	5.122	.754	3.005	.159	-.920
AZ	65	4.189	.589	2.967	.607	.508
AR	68	5.034	.830	3.540	.027	-.408
CA	59	3.868	4.31	2.446	.431	1.468
CO	72	4.039	.556	2.89	.556	1.132
CT	72	3.957	.622	3.903	1.355	4.195
FL	69	4.864	1.02	4.537	.224	-.351
GA	63	3.967	.577	2.269	.411	-.551
HI	72	3.052	.499	2.819	.644	1.29
ID	71	4.529	.918	5.736	.978	3.038
IL	64	3.733	.530	2.572	.318	.637
IN	72	5.232	.738	3.708	.777	.766
IA	72	3.452	.554	2.899	.462	.939

Table 7 (cont.)

Descriptive Statistics for Dependent Variable Measures by State

State	N	Mean	SD	Range	Skew	Kurtosis
KS	72	3.445	.401	1.688	.192	-.499
KY	72	4.795	.594	2.955	-.113	.537
ME	71	4.920	.775	4.093	.781	1.074
MD	71	3.697	.486	1.990	.375	-.484
MA	70	4.529	8.568	4.551	.603	.632
MI	79	4.317	.497	3.069	.003	-.201
MN	69	3.228	.402	2.075	.616	1.585
MS	61	4.435	1.352	8.668	-.205	3.377
MO	70	4.168	.682	3.968	.020	1.158
MT	67	3.714	.537	2.342	.095	-.569
NE	72	3.326	.550	2.538	.298	-.257
NH	69	4.030	.573	2.954	-.077	.657
NJ	63	3.673	.640	3.393	.986	2.276
NM	72	4.151	.566	2.668	.676	.648
NY	68	5.247	.930	5.681	1.394	4.604
NC	72	3.977	.536	2.967	.493	1.090
ND	72	4.083	.818	4.808	1.217	3.359
OH	72	4.320	.557	2.811	.454	.520
OK	72	4.497	.712	3.980	1.295	3.055
OR	66	5.021	.830	5.852	1.468	7.472

Table 7 (cont.)

Descriptive Statistics for Dependent Variable Measures by State

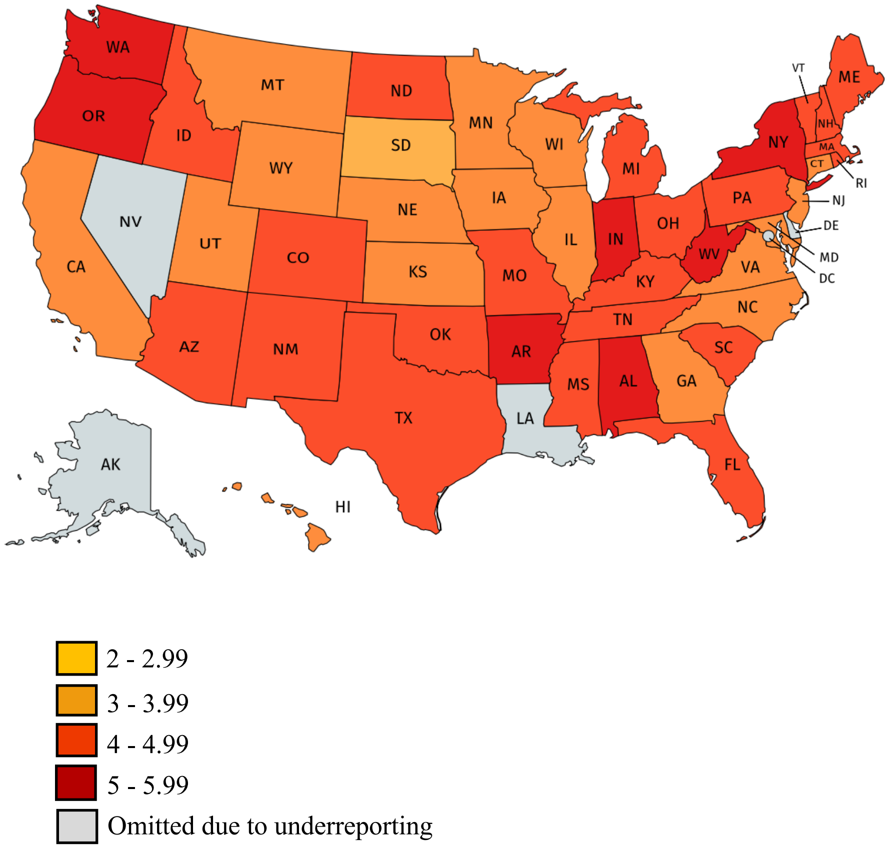
State	N	Mean	SD	Range	Skew	Kurtosis
PA	65	4.663	.487	2.483	.368	-.055
RI	68	4.303	.665	3.389	.610	.415
SC	72	4.469	.506	2.216	.948	.751
SD	72	2.903	.581	3.018	.217	1.66
TN	65	4.419	.835	4.326	.202	.270
TX	72	4.212	.755	3.660	.419	.061
UT	70	3.801	.505	2.584	.371	.491
VT	72	4.027	.657	3.099	.009	-.185
VA	68	3.756	.570	3.361	.865	2.377
WA	71	5.104	.485	2.012	.415	-.502
WV	71	5.013	.654	2.976	.446	-.414
WI	64	3.909	.682	3.602	.211	.703
WY	63	3.738	.778	4.029	.992	1.713

Note. SD=Standard Deviation; Skew=Skewness, Mean = Weighted Sample Mean

Figure 2 visually displays the data from Table 7 and serves to illustrate the wide-degree of variation of poor mental health rates (ranging from 2.903 - 5.247) across each of the 46 U.S. states in the sample. Because there is significant variation in poor mental health rates between states, this needs to be modeled into statistical estimates to reduce bias. The procedures taken to accomplish this are further discussed in the Model Diagnostics section.

Figure 2

Mean Poor Mental Health Days (Per Month) 2012-2017



Note. Map created in *MapChart.net*

It is also important to highlight variation of poor mental health within states across time periods. Histograms of Connecticut and Ohio are reported in Figures 3 and 4 to illustrate this variation visually. Histograms for each of the 46 U.S. States utilized in this study also appear in Appendix B.

Figure 3

Average Monthly Number of Poor Mental Health Days in Ohio

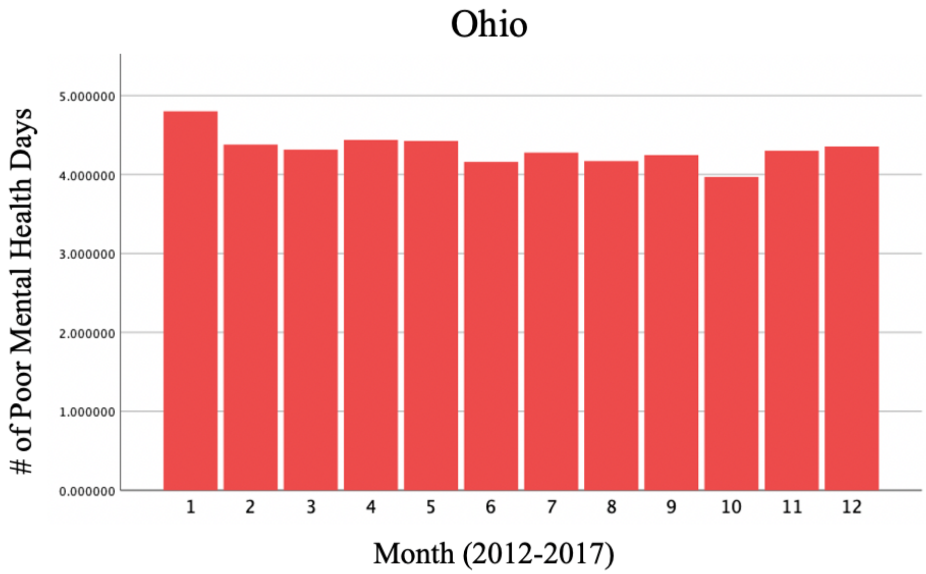
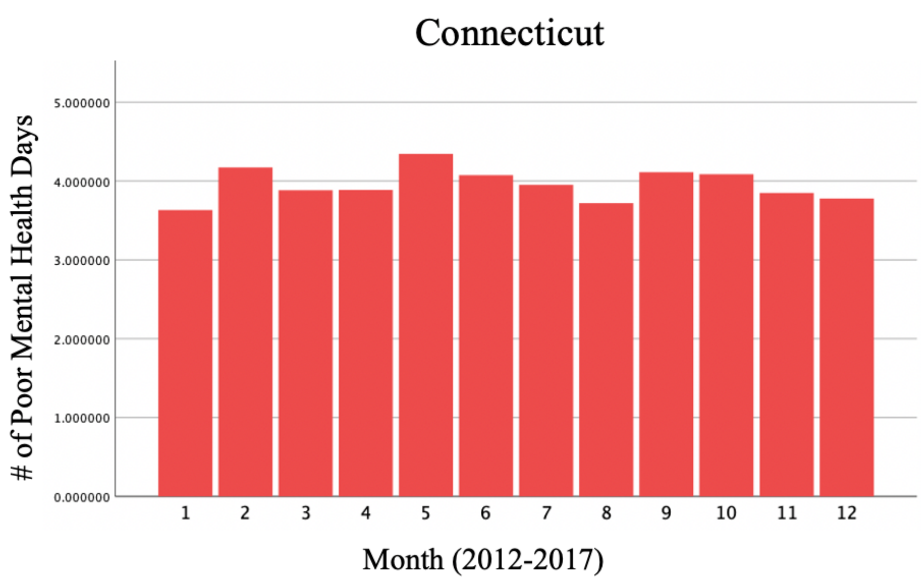


Figure 4

Average Monthly Number of Poor Mental Health Days in Connecticut



As can be seen with the Ohio and Connecticut examples, Ohio generally has less poor mental health days between June-October, whereas in Connecticut, August and January tend to have lower days of poor mental health. Of note, these histograms reflect only the 6-year monthly averages and therefore do not display the seasonality of any particular year. Accordingly, given that there are differing seasonality trends of mental health by state, time trends need to be controlled in statistical estimates. The steps taken to accomplish this are further explained in the Model Diagnostics section.

Descriptive statistics are reported for the terrorism and mass shooting events (N=72) in Table 8.

Table 8

Descriptive Statistics for Terrorism and Mass Shooting Events

Variable	Frequency	% of Sample
Mass Shooting Incident	26	36.12
Terrorism Incident	46	63.88
Multiple-Day Incidents	5	6.94
Religious Target	6	8.33
School Target	8	11.11
Jihadist Attacker	18	25
Anti-Police Attacker	4	5.55
White Power Attacker	7	9.72
Other Motivation	43	59.72

As Table 8 displays, the majority of incidents available for analysis (N=72) were terrorist attacks (63.88%). Approximately 7% of incidents involved attacks that spanned more than one day. In terms of motivation, Jihadist attacks accounted for the majority of defined motivations (25%), followed by White Power (9.72%) and Anti-Police (5.55%). The "other" category (59.72%) includes all mass shooting events (motivation is not assessed in the mass-shooting dataset) and anti-government terrorist perpetrators. In terms of target selection, religious sites were targeted in 8.33% of the attacks, and schools were targeted in 11.11% of the attacks.

To help display the descriptive statistics for the outcome measures following a mass-shooting or terrorist incident, histograms shown in Figures 5 through 8 were produced for the top two most covered (by news) terrorist attacks and the top two most covered (by news) mass-shooting incidents. Dose response theory would predict that these incidents are more likely than others to impact outcome measures, given that the trauma is at or near peak dosage when compared to other events. These events include: Sandy Hook, CT (mass shooting, 29 victims); Aurora, CO (mass shooting, 82 victims); Boston, MA (terrorist attack; 286 victims); and Orlando, FL (terrorist attack, 103 victims).

Figure 5

Histogram of Connecticut (December)

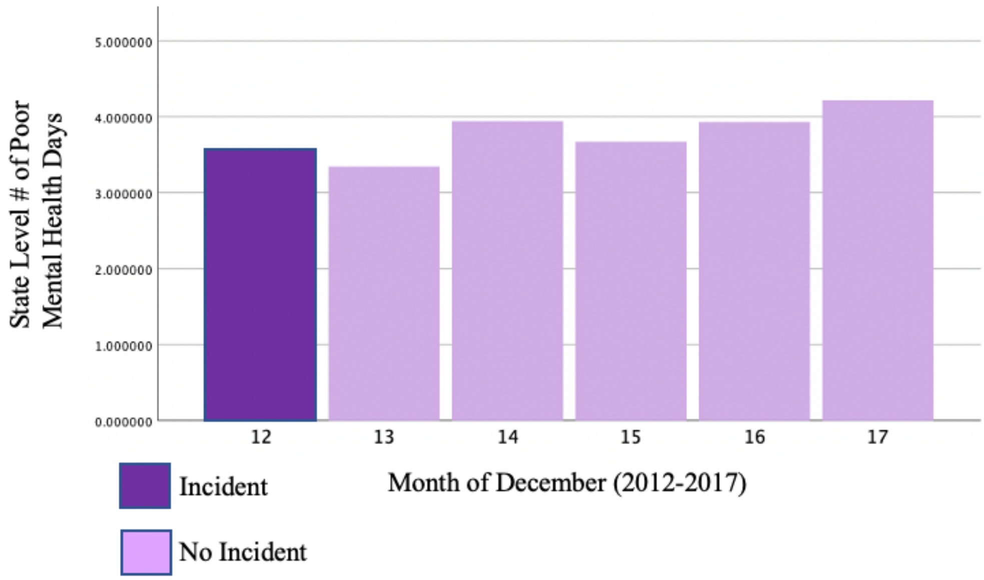


Figure 6

Histogram of Colorado (July)

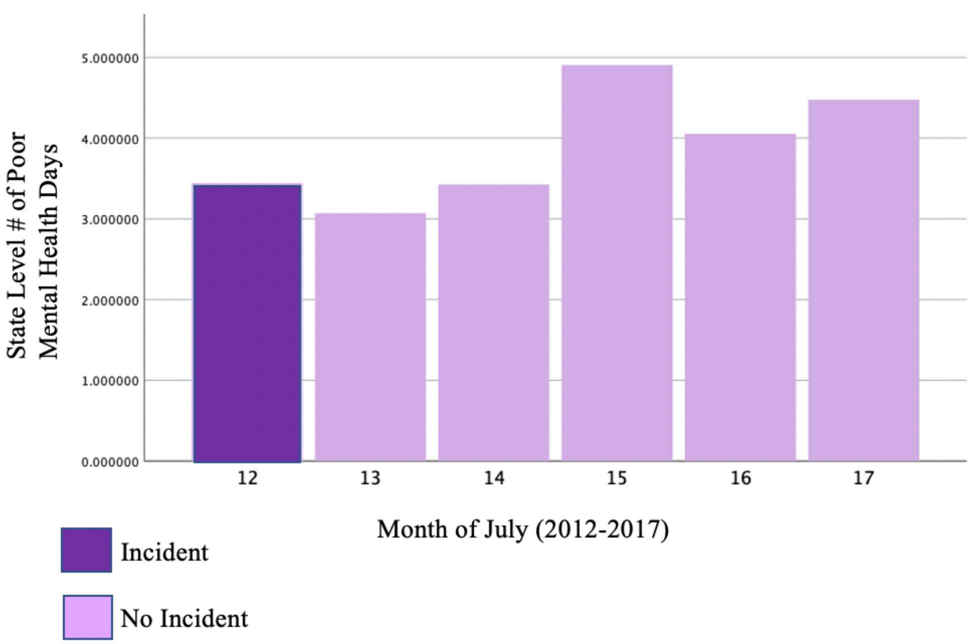


Figure 7

Histogram of Massachusetts (April)

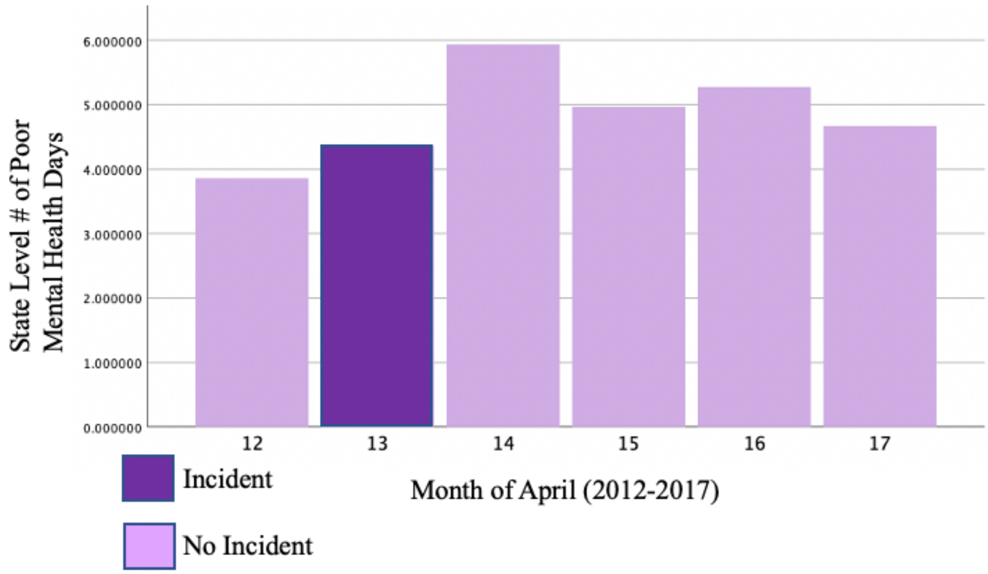
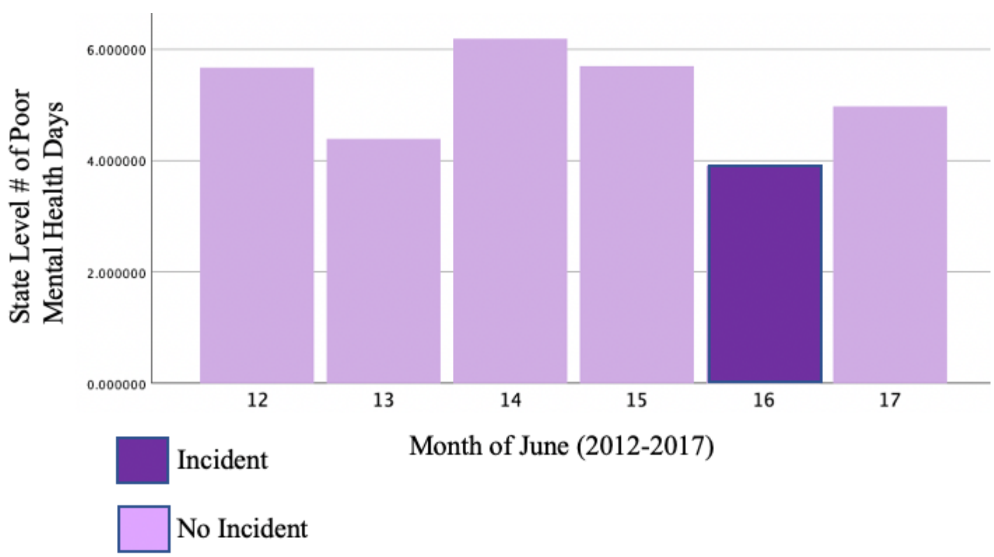


Figure 8

Histogram of Florida (June)



While each histogram illustrates variation during the month of when the attack occurred when compared to other years, there is no observable up-tick in poor mental health during the attack incident time period. In fact, the attack-month was not in the top three months (i.e., top 50%) for poor mental health for any of the incidents. While these descriptive findings alone should not be used to draw any firm conclusions, they do appear to suggest that there may be no association between terrorist or mass shooting incidents and the general public's quality of mental health.

Bivariate Analysis

After descriptive statistics were produced, a series of bivariate correlations were produced in SPSS. Importantly, bivariate correlations using the dependent variable (poor mental health days) were deemed unsuitable to report. Including such an analysis only would serve to highlight spurious results. Because this study involved both time and spatial factors, analysis of the dependent variable requires additional TSCS model specifications. Therefore, associations between the independent and dependent variables were assessed only in multivariate regression analyses, where both time and spatial effects were controlled carefully.

To better understand which types of mass shooting and terrorism events are associated with increased news coverage and victim counts (the primary independent variables of interest in this study), a series of bivariate correlations using terrorism and mass-shooting event characteristics were produced and assessed from the trimmed sample (N=72). In addition, bivariate correlations help indicate if any predictor variables pose multicollinearity concerns. While this analysis does not directly address the dependent variable of interest (poor mental

health days), it may help better understand which types of events are more likely to exert a greater dose of trauma on the targeted population. Also, this analysis was used to justify inclusion into subsequent multivariate regression analyses. A parametric statistic (Pearson's R) was selected, as each test has a sufficient number of observations (N>30) required for parametric assumptions. Tables 9 through 11 document the findings from the Pearson's R correlation tests.

Table 9

Bivariate Correlations for News

News Correlations	
Total Victims	.802**
News	1.00
Religious Target	.06
School Target	.056
Jihadist	.225 (p = .057)
Anti-Police	-.100
White Power	-.021
Multi-Day Attack	.277*
Def as Terrorism	.127
Def as MS	-.127

* p < 0.05; ** p < 0.01

Table 10

Bivariate Correlations for Total Victims

Total Victim Correlations	
Total Victims	1.00
News	.802**
Religious Target	-.02
School Target	-.03
Jihadist	.174
Anti-Police	-.074
White Power	-.074
Multi-Day Attack	.336**
Def as Terrorism	.074
Def as MS	-.074

* $p < 0.05$; ** $p < 0.01$

Table 11

Bivariate Correlations for Multi-Day Attack

Multi Day Attack Correlations	
Total Victims	.336**
News	.277*
Religious Target	.115
School Target	.077
Jihadist	-.032
Anti-Police	-.066
White Power	.095
Multi-Day Attack	1.00
Def as Terrorism	.092
Def as MS	-.092

* $p < 0.05$; ** $p < 0.01$

Results from the Pearson's R correlation results indicate that several characteristics, including locational information, motivation types, and whether the attack was defined as terrorism or mass shooting, were unrelated to both news coverage and total victims. Considering that these variables failed to demonstrate a theoretical linkage to the "dose" concept of dose-response theory, (i.e., they do not relate to victim counts or news coverage), they will not be considered in subsequent regression analyses. In other words, if these predictor variables do not increase news coverage or victim counts, there is no plausible linkage between these variables and mental health outcome variables. It is also worth noting that attacks categorized as "Jihadist" closely approached statistical significance ($r = .225$; $p = .057$). In terms of significant

findings, multi-day attacks were associated with both News (r = .277*) and Total Victims (r = .336**) variables. As such, the multi-day attack variable will be incorporated into further regression analyses. Finally, Total Victims and News were strongly correlated (r = .802**). This finding is unsurprising, as prior research has found seriousness of the attack to be a robust predictor of news coverage (Chermak & Jeffrey Gruenewald, 2006).

Model Diagnostics

Because TSCS is often regarded as complex, researchers tend to utilize a variety of different approaches. These different approaches often produce conflicting results, which "hinders its use as policy guide" (Marvel & Moody, 2008, p. 360). In effort to summarize the consensus that exists among scholars employing TSCS strategies, Marvel and Moody (2008, p. 361) delineated a list of things that all researchers employing TSCS should adhere to and stipulate in their publications. Such topics include basic design, time effects, unit time trends, stationarity, weighting, and a lagged dependent variable.

First, the basic design of a TSCS study should be a fixed effects model (Marvel & Moody, 1996, 2008). In fact, nearly all TSCS regressions within the field of criminology utilize the fixed effects model (Moody & Marvell, 2018). Accordingly, this is the method that was adopted in the current research. In formal terms, fixed effects modeling assumes that slope estimates for the variables in the model remain constant across the unit and time dimensions, but intercepts vary by unit and time. This approach is admittedly crude (Worrhall & Pratt, 2004, p. 37), but nevertheless will help to ensure that state and time specific factors affecting mental

health are adequately controlled. Because prior research supports the use of fixed effects modeling (Marvel & Moody, 1996, 2008), fixed effects modeling was employed via use of Stata.

Utilization of fixed effects helps to mitigate the effects of heterogeneity (i.e., unobserved variables that remain constant over time but vary cross-sectionally; Worrhall & Pratt, 2004). Specifically, unit dummy variables should be included for each unit except one. Unit dummies control for unobserved heterogeneity among units (Marvel & Moody, 1996). In other words, there may be a myriad of reasons why mental health varies from Connecticut to Alabama, which are not captured within the specific control variables inserted into the regression equation.

There are a variety of ways to detect heterogeneity, to include box plots of the dependent variable for each unit, or by setting up an F-test where the null hypothesis stipulates that all units share the same intercept, against the alternative in that intercepts vary across units (Fortin-Rittberger, 2014). If detected, inclusion of unit dummy variables helps control for this unobserved heterogeneity. In addition, time-specific dummy variables also should be included to all units except one, and the calculations of unit time trends should have separate variables for each unit, except one. Most TSCS analyses actually fail to use unit dummies and therefore produce unreliable results. For example, in a review of 195 political science TSCS articles, Wilson and Butler (2007) revealed that 60% did not use unit dummy variables. The current study avoids this problem.

Another challenge when using TSCS data is the issue of stationarity (Marvel & Moody, 2008). Data are stationary if their means, variances, and autocovariances (at various lags) remain across all time points (Worrhall & Pratt, 2004, p. 28), or if the dependent variable has a

true mean that it tends to return to over time (i.e., reversion to the mean) (Allen & Cancino, 2012, p. 158). For example, it is well known that crime rates fluctuate up and down over time. Time series data assumes that this is not the case, which means the data need to be forcibly made stationary. A common approach to correct for nonstationarity is to difference the data (Marvel & Moody, 2008). In other words, instead of using the raw data, "first differences" (or second, third, etc.), or the change in value from one point to the next, can be analyzed. If there is a relationship beyond the shared trend, then decreases in one variable should occur with decreases in the other (or increases with the increases; Dickey & Pantula, 1987). If time series has a unit root, then it is considered nonstationary. Detection of stationarity is typically accomplished through a Dickey-Fuller unit rest.

For time series data involving heterogeneous panels, a modified Dickey-Fuller test, such as the Im-Pesaran-Shin test, is more appropriate to assess stationarity (Worrhall & Pratt, 2004). This test employs a standardized *t*-bar test statistic, which is based on the augmented Dickey-Fuller statistics that are averaged across groups. Computations of the *t*-bar statistic have been coded in the econometric software packages, to include TSP and Stata (Im, Pesaran, & Shin, 2003). In the current research, unit root tests for stationarity were first assessed using the Dickey-Fuller test. Each state had a p-value < .05, indicating that each of the 46 panels of data contained stationarity data. Because the associated analysis involved TSCS, the Im-Pesaran-Shin test also was utilized. The t-bar test statistic (-30.9811) had a p-value of < .001. Therefore, we reject the null hypothesis (all panels contain unit roots) and accept the alternative hypothesis (some panels are stationary).

Another important issue that needs to be addressed in TSCS analysis is temporal autocorrelation (also referred to as serial dependence). In short, the values of one unit over a time period may be associated with values of another unit. For example, if estimating the mental health outcome measures for December in the state of Connecticut, one can reasonably assume that events that occurred in November may have some impact on mental health in December.

In terms of detecting temporal autocorrelation, the Durbin Watson *d* statistic from the residuals of the OLS regression model can be used to assess this. The *d* statistic ranges in value from 0-4. A *d* statistic of around 2 indicates non-autocorrelation, whereas closer to 0 indicates a positive autocorrelation and a value closer to 4 indicates a negative autocorrelation (Kenton, 2019). This study evaluated serial correlation by first assessing the Durbin Watson *d* statistic. An OLS regression model was used to compute the *d* statistic, with a reported value of .973. This value infers that the data has positive temporal autocorrelation. Because serial correlation in linear panel data models biases the standard errors and causes the results to be less efficient, a newer test (first developed by Woolridge in 2002) known as the Drukker-Woolridge test (Drukker, 2003) was performed. In Stata, the null hypothesis of this test is that there is no first-order autocorrelation. A p-value $<.05$ indicates that autocorrelation is present and the null hypothesis must be rejected. The predictor values assessed in the OLS regression model computed in Stata each had p-values $<.05$, indicating that temporal autocorrelation is present.

A common way to address autocorrelation within TSCS analysis is by lagging the dependent variable (Marvel & Moody, 2008; Worrhall & Pratt, 2004). In the above example, this would mean including a variable (T-1) into the regression equation. For example, September's mental health outcome measure is included as an independent variable for the

month of October. This is done for each state across 71 of the 72 months under review (January 2012 does not include a T-1 lag because 2011 data were not utilized). By including the T-1 independent variable in the multivariate regression equation, the past month's mental health of the unit is controlled. A lagged dependent variable typically removes much of the serial correlation because the lagged term of the dependent variable includes the lagged error terms (Fortin-Rittberger, 2014). The current research employed a lagged dependent variable as a means to control for temporal autocorrelation.

Yet another frequent challenge to TSCS analysis is panel heteroscedasticity. This happens when error variances for a given unit display time dependence. For example, if police departments are the unit of analysis, then it may be expected that small and large police forces contribute to non-constant error variances (Worrhall & Pratt, 2004, p. 38). Consequently, the regression must be weighted by a measure of the size of each unit (Marvel and Moody, 2008). The most popular method to address this is to weight the data by the square root of the variable thought to be responsible for heteroscedasticity. In criminological research, the variable that tends most often to be weighted is "population size." Instead of "weighting" the data, an alternative approach is to utilize "panel corrected standard errors" (PCSEs). According to Beck and Katz (1995), PCSEs are more accurate than alternative computations of OLS standard errors for TSCS data and are easy to estimate with statistical software. Of note, Stata allows researchers to weight the data (by square root of a specified variable) or to employ a PCSE approach (Worrhall & Pratt, 2004). To test for panel heteroscedasticity, this study employed the Breusch-Pagan test for heteroscedasticity. If the p-value of the Breusch-Pagan test is <.05, then

heteroscedasticity is present. The reported p-value for this test was .878, indicating that heteroscedasticity was not present.

Another challenge that must be dealt with is the issue of multicollinearity. This refers to predictor variables demonstrating strong associations with each other. Perfect multicollinearity would mean that one independent variable can perfectly predict variation in another independent variable. If left unaddressed, multicollinearity increases the standard errors of regression coefficients, making it more difficult to find statistically significant effects. To detect if multicollinearity is present, Tolerance and Variation Inflation Factor (VIF) scores were assessed as part of the analysis. Tolerance values range from 0 to 1. Values of less than .1 are considered problematic. The VIF is the inverse of the Tolerance value. VIF scores of greater than 10 indicate the presence of potentially harmful multicollinearity (Schroeder, 1990). If multicollinearity is present, a series of bivariate correlations can be used to assess which variables are highly associated with each other, and those that are strongly associated with each other can be excluded from the same multivariate regression equation (Meuleman et al., 2015). However, as shown in Table 12, multicollinearity was assessed using the Variance Inflation Factor command in Stata, and it was deemed to be not present.

Table 12

Variance Inflation Factors for the Independent Variables

Variable	VIF
News	4.02
Total Victims	3.48
Event Occurred	1.66
Multiday Event	1.27
Spatial Effects	1.00
Time Lag	1.00

Note. VIF scores > 10 indicate strong evidence of multicollinearity.

There are still two other primary issues of TSCS analysis that often get overlooked within criminological research, namely spatial autocorrelation and contemporaneous correlation (Worrall and Pratt, 2004). In terms of spatial autocorrelation, it is important to refer back to The First Law of Geography, which states that "everything is related to everything else, but near things are more related than distant things'' (Tobler, 1970, p. 236). In short, it is very important to pay attention to the effects of nearby places. For example, it is reasonable to assume that two contiguous states (that share a common border) will likely have many similarities and share at least some of the same problems (Fortin-Rittberger, 2014). Within criminology, recognition of the importance of spatial interdependence emerged in the 1990s. More recently, scholars have recognized the importance of this issue and have applied statistical methods and techniques to deal with it (Bernasco & Block, 2011, p. 37).

The potential for error correlation between units increases when the units are contiguous. In formal terms, spatial autocorrelation occurs when the value for one variable in X at location *j* is dependent (or associated with the value of variable X at location *I;* Worrall and Pratt, 2004, p. 39). For example, on October 1, 2017, there was a terrorist attack in Las Vegas that killed 59 people. Although Las Vegas is part of the state of Nevada, the city is actually quite close to parts of California, Arizona, and Utah. It is unreasonable to assume that the impact that this event had on the general public's mental health is confined solely to the state of Nevada. This event, and others like it, probably exerts influence on other states that are contiguous to the state where the event occurred. It is also likely the case that these contiguous units are simply more similar and share the same underlying problems (e.g., issues impacting mental health in Kansas may be quite similar to Nebraska).

If terms of resolving the issue of spatial autocorrelation, Worrall and Pratt (2004) advocate the use of a "spatial effects model," in which the effects of autocorrelation within the dependent variable are incorporated. This implies that the values of the dependent variable for each contiguous unit are averaged together and then entered into the model as another independent variable. It is also possible to substitute such a variable with lagged mean values and create a "spatial lag model." This study incorporated a spatial effects model as a means to control for spatial autocorrelation. As reported in the upcoming multivariate regression findings, the spatial effects variable was associated significantly ($p < .05$) with both outcome measures (30 and 60-day poor mental health measures), indicating neighboring states are associated with the mental health measures of one another.

Yet another challenge with TSCS is contemporaneous correlation. Contemporaneous correlation refers to the error correlation between two or more units (Worrall and Pratt, 2004). For example, if the 2016 Presidential election that occurred in November affected all of the units in the analysis (in terms of poor mental health days), then fixed effects modeling (i.e., month-year) will help control for this effect across all units. If, however, a major storm impacted only South Carolina and North Carolina, but no other units were affected, then this could create the issue of contemporaneous correlation. In contrast to heterogeneity between units, this type of heterogeneity cannot be modeled using time-specific dummy variables, because they serve to control for events that impact all units at any given time point (Fortin-Rittberger, 2014). Contemporaneously correlated errors can be detected through Pesaran's cross-sectional (CD) test (De Hoyos and Sarafidis, 2006).

The current research employed the CD test, which produced a CD test statistic (15.792) and corresponding p-value $< .001$. P-values $< .05$ are indicative of contemporaneous correlation. Therefore, this study needed to take additional steps to mitigate the issue of contemporaneous correlation. Although various ways exist to mitigate this issue, both Worrall and Pratt (2004) and Beck and Katz (1995) recommend using OLS regression with PCSEs, as PCSEs are accurate in the presence of contemporaneously correlated errors (Worrall and Pratt, 2004). As previously discussed, PCSEs are more accurate than alternative computations of OLS standard errors for TSCS data (Beck and Katz, 1995). OLS regression models utilizing PCSEs were computed in Stata and reported in Tables 13-16.

It is also important to assess outliers in the data. An outlier is a data point that is significantly different from the remaining data. Hawkins (2002, p. 170) defines outliers as "an

observation which deviates so much from the other observations as to arouse suspicions that it was generated by a different mechanism." According to Orr and colleagues (1990), outliers can occur due to the inclusion of subjects who are not in the population of interest, measurement error, and errors in preparing the data for analysis, or they may be legitimate data points that contain valuable information. If the cause of the outliers is unknown, Kruskal (1960) suggests running the analysis both with and without the outliers. Thus, if the summary findings are similar, then there is little reason to worry about the impact of outliers. Conversely, if the findings are different, then it can be assumed that outliers are impacting the results of the study. To detect outliers in multivariate analysis, Hadi's (1992, 1994) method can be utilized. Using this approach contributed to the identification and removal of five observations. Importantly, none of these state-month observations had a terrorist or mass-shooting event.

Multivariate Analysis

After completing the appropriate diagnostic procedures, four fixed effect and PCSE models (reported together) were estimated to assess the relationship between poor mental health days and the predictor variables using the remaining 3,169 observations. The first model assessed the relationship between the four predictor variables and state-wide mental health measures (# of poor mental health days in past month). The second model included a temporal lag and spatial effects variable to mitigate the issues of temporal and spatial autocorrelation that were discovered in the model diagnostics assessment. A PCSE model was estimated in addition to fixed effects, as a PCSE-based approach is the preferred method to deal with issues of contemporaneous error correlations. The third model employed the same predictors as the first model, but assessed the number of poor mental health days two months after the event. The

fourth model utilized the same predictors and outcome variable as model 3, but also included a temporal lag and spatial effects variable. Collectively and ultimately, regression analyses from the four models indicated there is no clear relationship between mass-shootings and terrorist incidents with the mental health outcome measures. Such findings are consistent with what was suggested by the previously presented descriptive statistics and histograms.

Table 13

Model 1: Fixed Effects and PCSE Panel Models for Mental Health Rates (Month of Incident)

Variables	**Panel Fixed Effects Model** Coefficient and Standard Errors	**Panel PCSE Model** Coefficient and Standard Errors
Event	.037 (.096)	.049 (.107)
# of Victims	-.004 (.003)	-.004 (.003)
News	.000 (.002)	.001 (.002)
Multiday Event	.223 (.423)	-.234 (.461)
	$F_{(43,119)} = .85$, $p = .493$, Within $R^2 = .001$	$W^2 = 2.55$, $p = 0.635$, $R^2 = .000$

*$p \le .05$, ** $p \le .01$
Note. Event = Terrorist or Massing Shoot Event

Both sets of results in Model 1 revealed no significant findings. The Fixed Effects model ($F_{(43,119)} = .85$, $p = .493$) was not significant and explained approximately .1% of the variation of the outcome measure. Similarly, the PCSE model ($W^2 = 2.55$, $p = 0.635$) was not significant and explained less than .1% of the outcome measure. Subsequently, results presented in Table 14 included both the temporal lag (T-1) and the spatial effect variable.

Table 14

Model 2: Fixed Effects and PCSE Panel Models for Mental Health Rates (Month of Incident) with Spatial Effects and Temporal Lags

Variables	**Panel Fixed Effects Model** Coefficient and Standard Errors	**Panel PCSE Model** Coefficient and Standard Errors
Event	.025 (.097)	-.052 (.109)
# of Victims	-.004 (.003)	-.003 (.003)
News	.000 (.002)	.001 (.002)
Multiday Event	.230 (.426)	-.191 (.398)
Temporal Lag	0.000 (.000)	0.000 (.000)
Spatial Effect	.074* (.029)	.286* (.029)
$F_{(62,919)}$ =, p = 0.125, Within R^2 = 0.003		W^2 = 101.76, p = 0.000, R^2 = .037

*p ≤ .05, ** p ≤ .01
Note. Event = Terrorist or Massing Shoot Event

In Model 2, the Fixed Effects model ($F_{(62,919)}$ =, p = 0.125 $_{(43,119)}$ = .85, p = .493) was not significant (although approaching statistical significance) and explained approximately .3% of the variation of the outcome measure. While the Spatial Effect variable itself was statistically significant (p = .011), its slope coefficient (.074) was relatively small. While the PCSE model was found to be statistically significant (W^2 = 101.76, p = 0.000, R^2 = .037), it explained only 3.7% of the variation of the outcome measure. Within the PCSE model, only the Spatial Effect variable was statistically significant (p = .016), but again produced a modest coefficient (.286). Taken together, the dose-response theory predictors (Event occurred, # of victims, news coverage, and multi-day event) failed to generate statistically significant findings. While the Spatial Effect variable was significant in both Fixed Effects and PCSE models, this contributed

minimally to the overall variation in the outcome measure (poor mental health days over past month), with R^2 values ranging from .3% to 3%.

As shown in Table 15, both sets of results produced in Model 3 indicated no significant findings. The Fixed Effects model ($F_{(43,117)} = 0.31$, $p = .873$, Within $R^2 = 0.000$) was not significant and explained less than .1% of the variation of the outcome measure. Similarly, the PCSE model ($W^2 = 1.39$, $p = .845$, $R^2 = 0.000$) was not significant and explained less than .1% of the outcome measure.

Table 15

Model 3: Fixed Effects and PCSE Panel Models for Mental Health Rates (two months after incident)

Variables	**Panel Fixed Effects Model** Coefficient and Standard Errors	**Panel PCSE Model** Coefficient and Standard Errors
Event	.063 (.098)	.068 (.107)
# of Victims	-.000 (.003)	-.001 (.003)
News	-.001 (.002)	.000 (.002)
Multiday Event	-.113 (.376)	-.331 (.473)
	$F_{(43,117)} = 0.31$, $p = .873$, Within $R^2 = 0.000$	$W^2 = 1.39$, $p = .845$, $R^2 = 0.000$

*$p \leq .05$, ** $p \leq .01$
Note. Event = Terrorist or Massing Shoot Event

Finally, Table 16 presents the results of Model 4. Both sets of findings contain the same outcome measure as Model 3 (# of poor mental health days 2 months after terrorist/mass shooting incident), but also include the temporal lag and spatial effect variables.

Table 16

Model 4: Fixed Effects and PCSE Panel Models for Mental Health Rates (two months after incident) with Spatial Effects and Temporal Lags

Variables	**Panel Fixed Effects Model** Coefficient and Standard Errors	**Panel PCSE Model** Coefficient and Standard Errors
Event	.058 (.099)	-.008 (.113)
# of Victims	-.000 (.003)	-.000 (.003)
News	-.001 (.002)	-.000 (.002)
Multiday Event	-.130 (.378)	-.421 (.338)
Temporal Lag	.000 (.000)	.000 (.000)
Spatial Effect	.075* (.029)	.285** (.029)
$F_{(62,918)}$ =, p = 1.32, Within R^2 = 0.002		W^2 = 98.17, p = .000, R^2 = .037

*p ≤ .05, ** p ≤ .01
Note: Event = Terrorist or Massing Shoot Event

In Model 4, the Fixed Effects model ($F_{(62,918)}$ =, p = 1.32, Within R^2 = 0.002) was not significant and explained approximately .2% of the variation of the outcome measure. While the Spatial Effect variable itself was statistically significant (p = .011), its coefficient (.075) was relatively small. The PCSE model was significant (W^2 = 98.17, p = .000, R^2 = .037) and explained 3.7% of the variation of the outcome measure (the exact same amount as in Model 2). Within the PCSE model, only the Spatial Effect variable was statistically significant (p = <.001), although it again generated a modest coefficient (.285).

While the Spatial Effect variable was significant in both the Fixed Effects and PCSE models, and for both of the dependent variables examined, this independent variable contributed minimally to the overall variation of the outcome measures (poor mental health rate over the past month and past two months). R^2 values ranged from .2% to 3.7%. In short, the dose-response

theory predictors (Event Occurred, Number of Victims, News Coverage, and Multi-Day Events) of these models failed to generate significant findings when measuring state-level mental health rates following a mass-shooting or terrorist incident.

Chapter 5

Discussion and Conclusions

Results from the TSCS multivariate regression analyses suggest that applying dose-response theory is not effective in explaining variation in mental health across the state where an incident occurred, during either the month of or month after a terrorist or mass-shooting incident. Considering the quantitative results, this chapter assesses how these findings compare to those of prior research. Next, limitations of this study are discussed, as well as potential areas for future research. Finally, policy implications of this study are explored for both public health professionals and news/media producers.

Prior Theory and Research

The lack of significant findings generated in this study does not necessarily contradict prior research on how mass-shooting events psychologically impact indirectly exposed populations, as very little on this topic actually is known. For example, in Lowe and Galea's 2017 meta-analysis on the mental health impact of mass-shootings, the authors concluded that "mass shootings are associated with a variety of adverse psychological outcomes in survivors and members of affected communities. Less is known about the psychological effects of mass shootings on indirectly exposed populations" (pg. 62).

In addition to the limited amount of prior research on indirectly exposed populations, according to Wilson's (2014) systematic review, the few studies that involved indirectly exposed communities "demonstrated effect sizes that were small compared to many of the other included studies." The "other included studies" Wilson is referring to are those including populations that

were impacted more directly (i.e., seeing and hearing incident details and/or knowing a victim). The two studies (Backholm et al., 2012; Fallahi and Lesik, 2009) that Wilson highlights in her review on indirectly exposed communities had effect sizes of .07 and .15 (Cohen's *d* was calculated into an overall effect size). In contrast, more directly targeted populations had effect sizes ranging from .06-.36. Equally important, the two indirectly exposed studies involved either a sample among journalists (Backholm et al., 2012) or the measuring of total news consumption from the sample (Fallahi and Lesik, 2009). It is plausible that journalists are more likely to have been exposed to more news/media coverage than the rest of the population. Also, by controlling for level of media consumption (Fallahi and Lesik, 2009), researchers were able to show that higher levels of media consumption were associated with increased adverse psychological effects. Given that level of media consumption could not be controlled at an individual level in the current research, it is less surprising that this effect was not found. Taken together, prior research on this topic area has suggested that effects on remotely afflicted communities are likely to be smaller than other types of exposure, or that such effects are simply unknown. The results of the current study confirm that these effects are relatively small.

In terms of the impact that terrorist attacks have on communities experiencing the trauma indirectly, most of the prior research focuses on 9/11 (Henriksen et al., 2020; Houston, 2009; Nellis and Savage, 2012; Schlenger et al., 2002; Schuster et al., 2001; Silver et al., 2004). As previously discussed, these existing findings (which demonstrate that remote populations can be adversely affected psychologically) should not be interpreted as typical psychological responses to terrorism. While 9/11 resulted in the deaths of nearly 3,000 people, the average death toll for the 46 terrorism attacks under review in the current research was 4.6. This means that the 9/11

attack was 647 times more lethal than the average terrorist incident under review in the current research. Casualty figures aside, 9/11 also represented (to many) an existential threat to the United States. This attack arguably precipitated a nationalistic fervor to combat a global terrorism threat, paving the way for multiple foreign wars in the years to come (i.e., the Global War on Terrorism, or GWOT). The 9/11 attack also contributed substantially to the restructuring of homeland defense, with an increased prioritization to combat terrorism. The same cannot be said for any of the terrorist attacks under review in this study. Consequently, the terrorist incidents under review in the current research are less likely to produce similar psychological effects to that of 9/11.

Other research on the Oklahoma City bombing, another attack that killed far more people (168 deaths) than any other in this study, found that psychological effects were most substantial in the city of the attack and less so in remote locations (Sprang, 1999). Other existing research also controlled for level of media consumption (Holman et al., 2014; Nellis and Savage, 2012; Pfefferbaum et al., 2001) to determine that indirect effects can occur at high levels of media consumption. It was not possible to measure consumption at an individual level in the current research, which may have also contributed to the lack of significant findings. Further, such studies may also be reporting spurious relationships, as there could be underlying fear or anxiety propensities that cause some people to watch more news in the first place.

There are also several reasons why dose response theory may poorly explain mental health outcomes in communities that are remotely impacted from mass shooting and terrorist atrocities. First, while prior research has found that mental health can be impacted following a mass shooting or terrorist tragedy, such findings almost invariably reflect participants who are

more directly embedded within the community that is victimized by the attacker(s) (e.g., being at the attack site when it occurred, or living nearby). In contrast, this study attempted to explore the psychological impact of people living across the state in which a violent attack occurred. Thus, the "dose" of the trauma experienced by people in this sample may have occurred through television or social media, or these individuals learned about event details through word of mouth. It is conceivable that only a small number of people in any monthly sample may have had a more direct connection to the event (if at all), given the average death toll across all events (N=72) stood at 5.1. Accordingly, participants in this sample likely experienced a limited personal connection to the event. Without a first-hand or direct experience to the event (this can be assumed for the majority of the sample), the availability heuristic discussed in the literature review would infer that these events are less memorable. Less memorable incidents may produce dampened psychological responses when compared to direct experiences. Had this study looked exclusively at the town of the attack (which is not possible given the limitations of the data), increased poor mental health responses would perhaps have been more readily detected.

Second, mental health is an amalgamation of individual, social, and environmental risk factors. For instance, prior research has shown that family history (NIMH, 2020), environmental exposures (Messias et al., 2010; Muramatsu, 2003), chronic disease (American Heart Association, 2014), chronic stress (National Institute of Mental Health, 2020), prior trauma, Roberts (2019), and the strength of social relationships (Tough, 2017) each play important roles in shaping the quality of one's mental health. Thus, given the complexity of mental health, any act of public violence may merely represent a proverbial "drop in the bucket" in terms of the

number of possible variables impacting mental health. It also seems logical that when a person quantifies their own number of poor mental health days over the past month, some of the more immediate and pressing issues that they are experiencing are most determinative of how they will respond.

Relatedly, research also has shown that the majority of people might be naturally resilient in their psychological responses to trauma. In effect, maintaining normative or baseline levels of psychological functioning in the aftermath to trauma exposure is not rare nor exceptional; rather, it is often the most typical response to potential trauma (Bonanno and Mancini, 2012). These resilient individuals generally will only experience slight disruptions in functioning (e.g., increased negative affect for a few weeks, decreased sleep, and decreased concentration) and exhibit fairly stable levels of healthy adjustment over time (Bonanno et al., 2005). For instance, in a study examining the relationship between resiliency amongst survivors near the 9/11 attack site, Bonanno and colleagues (2005) found that approximately 35% of the participants (N = 63) displayed resiliency. Resiliency ratings were assessed through self-report, as well as friend and relative ratings of the participants. Their results showed that the majority of people surveyed did not present evidence of psychopathology and that “the resilient trajectory was the most commonly observed outcome.” (pp. 993).

Similar findings on resilience have been found on the topic of loss in bereaved spouses, bereaved parents, and bereaved gay men. In brief, these findings indicate that most bereaved individuals actually follow a relatively stable resilience trajectory following significant interpersonal loss and therefore are not well suited for clinical intervention (Bonanno et al., 2005). In a more comprehensive review of the resiliency literature, a 2015 meta-analysis of 60 studies with 111 effect sizes found that trait resilience was negatively correlated to negative

indicators of mental health (mean *r* effect size: -.036) and had a positive correlation (mean *r* effect size: 0.50) to positive indicators of mental health. Consistent with Cohen's d effect size, these effect sizes were considered to be medium effect sizes (Hu et al., 2015). Thus, findings in the resiliency literature actually tend to be supportive of the null hypothesis in this dissertation in that there is no clear relationship between the trauma-related independent variables with mental health outcome measures.

Third, the principle of psychophysical numbing also might help explain why psychological responses are detached from the numerical information (i.e., number of victims) associated with each attack. As previously discussed, people generally are unable to calibrate their emotional responses in any type of statistically proportionate way. In this regard, it may not matter if 10 people are killed or if 25 people are killed per terrorism or mass shooting incident. Rather, what matters most may be the personal connection that a member of the public has to the event itself. These counterintuitive finding suggests that numerical information alone (i.e., # of victims) matters little when considering psychological responses.

On the other hand, while an increase in victims might indicate that more people in the general public know a victim and therefore exhibit a greater "dose" of trauma, this also seems somewhat illogical. The differences in deaths or total victims from the events under review appear to be too miniscule when compared to the general population living throughout the entire state. In other words, if three people died or if 10 people died, most people across the state still are unlikely to know a victim. If, however, victim numbers were of greater magnitude (i.e., in the hundreds or thousands), then this measure could conceivably exert greater influence on the outcome measure. This simply was not the case in the current research, as the average number

of total victims (killed and wounded) was 16.2, and the average number of fatalities per event was 5.1.

Research Limitations

While this study benefited from a large sample size, longitudinal research design, and greater generalizability, it does have its limitations. Three notable limitations include its reliance on aggregate level measures (as opposed to individual measures), lack of county or city-level data, and use of a singular question for outcome measures.

First, outcome measures (N=72) were analyzed at the aggregate level (state-month) as opposed to the individual level. The CDC BRFSS does not provide information pertaining to prior mental health (at least in a sufficient number of state-year samples) for each respondent. Therefore, it was not possible to control for these factors. Given this limitation, aggregate level measures were used to explore state-level changes in mental health. Thus, if a terrorist or mass-shooting did have an impact on individuals who were at a heightened risk for poor mental health, aggregate level measures may not have sufficiently captured such effects. Also, if an attack targeted a specific group of people for their race or religious background, then these incidents could impact these communities differently than the broader general population. Unfortunately, this study was not able to estimate group-based differences.

Also, aggregate measures were drawn from the sample across the entire state. Importantly, however, locational information within the state was not reported. While this information is collected by the CDC, it is not available for the public to view. Access to this protected information could not be obtained for the current research. It would have been useful

to analyze the data within each state to see if counties/cities/towns that are more physically proximate to a violent attack experience trauma as a higher level. For example, if it were possible to isolate the Boston respondents within the Massachusetts sample in April 2013, it would have enabled assessment of attack effects in both physically proximate (town/city of attack) and distant (state-wide) settings.

It should be stressed that even if such locational information was available, it still would have posed additional challenges. For example, the average number of participant observations per month across all states from 2012-2017 was 807. Numerous states had numbers in excess of 1,000 per month, while others had fewer than 500. If an attack happened in a town where there were only a small number of observations, it would have been fairly difficult to generate a representative sample. Considering this, it may not have been feasible to utilize individual observations at the town/city of the attack, as issues of underreporting would have generated a non-representative sample. It's plausible that in some major cities where an attack happens, this would have not been an issue. In other contexts, however, particularly in less populated states that have lower monthly BRFSS reporting per month, this would have posed a serious challenge.

Another limitation pertains to news consumption. While this study attempted to measure news consumption in the aggregate sense (# of news articles published in national and regional news outlets during the weeks following a mass-shooting or terrorist attack), it could not take into consideration what each participant in the BRFSS consumed on a monthly basis. This was not possible, as the BRFSS does not collect such information. If such information was available, a more accurate estimate of total news consumption could be generated. While imperfect, the measures utilized in this study attempted to capture how salient a particular attack was, which

indirectly enabled consideration of which types of events are more or less likely to be consumed by a general audience in the weeks following a mass-shooting or terrorist attack. For example, on December 2, 2015, a jihadist attacker killed 16 and wounded 17 people in San Bernadino, CA. Using the news coverage measures adopted in this study, this incident produced 173 news articles over a 2-week period. A separate jihadist attack happened about one-month prior in Merced, CA, resulting in 1 death, 4 wounded, and 9 published news articles. While it is not possible to ascertain how much any one individual in the California sample, in either month, consumed news related to either event, it seems quite plausible that the San Bernadino tragedy was more likely to be recognized, as it received nearly 20 times the amount of attention from the news sources. While such an approach is admittedly crude, because it cannot account for individual news consumption levels, it does help us better understand how the general public as a collective entity learns about these events. Given that the mental health outcome measures are aggregate scores and not individualistic, such an approach seems fitting given the objectives of this study. Lastly, it is important to note that the general public learns about news in many more ways that this study could account for. Other news forms include radio, social media, network TV, local TV, and other printed news content. Given the time and resource constraints of this project, it would not have been feasible for this researcher to estimate news consumption rates across each of these avenues.

Another limitation of this study pertains to its limited number of outcome measures. While the self-reported number of poor mental health days over the past month provides general insight into the nature of an individual's mental health, it lacks the nuance needed to differentiate different types of psychological responses. For example, this study would have benefited from

questions that assessed an individual's level of stress, anxiety, and fear. These psychological responses are the typical categories of psychological response that are more typically evaluated in response to a traumatic event, such as terrorism or a mass-shooting.

Other questions that try to capture how psychological responses manifest themselves within behavior also would have been of interest. For instance, are people less willing to go out to public events or engage in social activities in the aftermath of a tragedy? The CDC BRFSS dataset asks the following question which could have been useful: "During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?" Unfortunately, this question had to be dropped from the analysis due its high non-response rate (>50%). A representative sample would have been difficult to generate in many state samples because of this issue.

Areas for Future Research

Future research efforts should attempt to capture individual-level psychological risk factors, as well as the amount and types of news consumption for all participants. Considering this study found that these events generate no effect on aggregate levels (state-wide) of mental health, it would be of interest to know if individuals within a remote population who possess risk factors or who consume greater levels of news coverage about the atrocity are more likely to have worse psychological responses.

Separately, future research should examine how these events impact remote communities in a more localized context. For example, while this study found that there were no state-wide mental health effects in response to these events, this does not mean that there could not have

been more localized mental health effects. It is theoretically plausible that communities that are more physically proximate to the attack will be more socially connected to the victims and attacker. However, this might also suggest that news consumption and media would actually play less of a role than in this study. People who are more socially connected to the community where the event occurred are conceivably more likely to learn about event details through word of mouth than people who live further away. In addition, some people may have connections to a particular community (e.g., friends, relatives) even if they live in a separate town or city away from the attack site. Future research efforts should attempt to capture whether this factor is related to any psychological or behavioral responses. It's also worth noting that this study only examined the effects of the U.S. adult population (aged 18 and over).

Finally, future research should also explore how dose-response theory impacts youth following a terrorism incident or mass shooting. In particular, mass-shootings that target schools may impose more serious psychological harm for youth than other events under review in the current research. While the CDC's Youth Risk Behavioral Surveillance System (YRBSS) suffers from similar methodological limitations as the adult version (BRFSS), it surveys youth from 9^{th}-12^{th} grades and thus could prove useful in generating new research in this area.

Policy Implications

Results from this study suggest several important policy implications. First, because this study found that the level of media coverage surrounding a mass-shooting or terrorist event was unrelated to mental health outcome measures, there is reason for caution in considering its implications. Importantly, these findings should not be taken to suggest that how the media

portrays an event or the level of graphic content does not matter. For those people who are suffering psychologically in response to an atrocity, graphic imagery and continuous consumption of the news surrounding an event could make their psychological suffering considerably worse (Bernstein et al., 2007; Singer et al., 1998). Further, this study was not able to control for risk factors across the general population; therefore, there may still be some adverse impact on individuals who are at a heightened risk for having poor mental health.

Further, there may be other factors to consider that sensationalistic news coverage may be causing, such as fear, panic, or anxiety, which are not sufficiently captured by the poor mental health outcome measures utilized in this study. It is therefore premature to reach any sweeping conclusions about the nature of the relationship between the media's coverage of terrorism, mass-shootings, and the mental wellbeing of indirectly exposed communities.

In the aftermath of a horrific tragedy such a mass-shooting or terrorism incident, it seems clear that emergency management and crisis response teams should direct their efforts and resources toward those who have the highest exposure to the incident. Prior research has found that those who are exposed more directly to a trauma, such as a mass shooting (Lowe and Galea, 2017; Wilson, 2014), terrorism event (Schlenger et al., 2002), or another type of manmade or natural disaster (Norris, Friedman, & Watson, 2002a) are at a heightened risk for psychological trauma. Further, prior research has shown that risk factors matter in shaping one's psychological outcomes (Furr et al., 2010). Findings from the current research indicate that communities experiencing trauma predominantly through indirect means (typically through news coverage) are not likely to be seriously psychologically impacted. Accordingly, spreading out limited resources across a state or to communities that are less directly affected would be unwarranted

and could diminish resources where they may be needed most. Instead, if additional resources are available to assist with community members beyond the immediate victims and their loved ones, it may be beneficial to screen members of the victimized community and/or neighboring towns where trauma dosage is greater.

If indirectly exposed communities experience psychological trauma in the aftermath of a traumatic incident, several strategies are recommended to ameliorate symptoms. As recommended by Hobfoll and Watson (2007), five essential elements should be addressed, to include promoting a sense of safety, calming, self- and community efficacy, connectedness, and hope.

First, in the aftermath of a mass-shooting or terrorist attack, restoring a sense of safety in the community is critically important. Those who experience a loss in their sense of safety are at heightened risk for PTSD in the months following their exposure to the incident (Hobfoll and Watson, 2007) and are at increased risk to experience greater fear and anxiety (Bryant, 2006). Multiple strategies exist to improve perceptions of safety, and such strategies can be implemented at multiple levels (individual, group, organization, and community level). Interventions such as exposure therapy, as well as employing reality reminders and grounding techniques, can be effective. When working with children, reversing their ability to discriminate among indications of danger is another essential therapeutic goal. In a more general sense, it is imperative that community members are brought to a safe place, and it is made clear that they are safe (Hobfoll and Watson, 2007).

Shalev and Freedman (2005) highlight how safety also involves protection from bad news, rumors, and other interpersonal factors that may increase threat perception. Given the extant media environment in the U.S., this seems to be a growing concern. In recent years in the U.S., fringe ideas have been amplified across social media platforms and even corroborated by mainstream news outlets (Gerstell, 2020). Leadership must therefore strive to provide a timely and accurate assessment of the threat, and also be effective at disabusing fear inducing falsehoods that may promote insecurity and undermine messages of safety.

Next, public health officials should make it a priority to create a sense of calming. Following a traumatic incident, it is common to experience a heightened level of arousal, or alternatively, a numbing response. If a person experiences either state, their normal rhythms and patterns may be impaired. Consequently, this could fuel anxiety or anxiety related disorders, cause panic attacks, agitation, and depression, and act as a precursor to PTSD. Treatments for calming can involve either direct treatment or an indirect approach. In terms of direct treatment, therapeutic grounding is used to remind individuals that they are no longer in the threat-trauma condition (Hobfoll and Watson, 2007). A cognitive behavioral therapy (CBT) approach is recommended, specifically stress inoculation training (SIT). Through this method, breathing retraining exercises can be used to help reduce hyperventilation or disassociation. Deep muscle relaxation therapy, yoga, imagery, and music paired with relaxed states also can promote a sense of calming. While some pharmacological approaches could assist in initial calming, such as anti–adrenergic agents, antidepressants, and conventional anxiolytics, drugs such as benzodiazepines could actually increase the likelihood for PTSD (Gelpin, Bonne, Peri, Brandes, & Shalev, 1996), and therefore should be used cautiously.

At a more indirect or community-wide level, psychoeducation can be employed to help the public understand that their psychological responses are normal and expected under tragic conditions. Survivors of trauma therefore should avoid pathologizing their inability to remain calm. These messages should be reinforced through media and can be assisted with interactive websites and computer programs (Hobfoll and Watson, 2007).

Promotion of self and collective efficacy is important following a traumatic terrorist or mass-shooting incident. Self-efficacy pertains to the belief that a person's actions are likely to lead to positive outcomes (Bandura, 1997). This can lead to collective efficacy (in a medical and non-criminological sense), which occurs when a person feels they belong to a group that is likely to experience positive outcomes (Antonovsky, 1979). In contrast, in the aftermath of trauma, people may feel a sense of "can't do." Individual and group-administered CBT can be particularly effective to improve self-efficacy. Having a supportive family often is essential, as families typically are the main provider of mental health care in the aftermath of trauma (Hobfoll and Watson, 2007). Consequently, public health messages that seek to foster a sense of family in affected communities should be promoted.

Having social support and a sustained attachment to loved ones, also known as "connectedness," also is essential to trauma recovery. This may entail support activities, emotional understanding, problem solving, sharing of traumatic experiences, normalization of reactions and experiences, and coping strategies (Hobfoll and Watson, 2007). While there is ample evidence to suggest that connectedness is related to positive psychological outcomes, there is less clear evidence about how to translate these concepts into effective practice. What is

clearer is that community members should make every effort to identify those individuals who lack social support, as they are at a heightened risk of social isolation.

Finally, another essential element to address in the aftermath of trauma is the restoration of a community's sense of hope. Hope can be defined as "positive, action–oriented expectation that a positive future goal or outcome is possible" (Hobfoll and Watson, 2007, p. 298). Interventions can be both implemented at the individual level (CBT) and community level (mass messaging). According to Hobfoll and Watson (2007), community level interventions tend to be more effective than individual interventions. This may be because common problems are more efficiently identified than the time it may take to identify problems in multiple therapeutic sessions. Further, a community may be able to mobilize assets, networks, and social capital in ways that an individual therapeutic session cannot provide. Thus, community leaders and organizations can foster hope by implementing community driven interventions (home visits, rebuilding projects, blood drives) to build strength and achieve positive community goals.

Conclusion

In sum, this study provided an assessment of the impact of mass shootings and terrorism incidents on remote populations. In particular, the effect of media exposure and lethality of deadly events on mental health were assessed using a TSCS framework. The major benefit to this study is that it employed a longitudinal research design, and it covered many mass-shooting and terrorist events. Accordingly, pre-event measures were captured, enabling a better estimate of the relative effects of events than in past research efforts. Further, because 72 events were under review, findings from this study are likely to be of greater generalizability than prior

scholarship, which overwhelmingly focused on singular incidents. The current research also contributed to this topic area in a significant theoretical way, as empirical tests of dose-response theory on remotely exposed communities are in short supply.

Results from this study suggested that dose-response theory cannot account for changes in psychological health and wellbeing in communities that are predominantly exposed to mass-shooting and terrorist incident through indirect means. While remotely exposed communities are likely to learn about these atrocities through news, media, and word of mouth, the relative dose of trauma appears to be minimal when compared to those who experience the trauma in more direct ways (e.g., seeing or hearing the violence unfold or losing a loved one).

Several possible explanations may help explain the lack of psychological impact, to include the availability heuristic, psychophysical numbing, and the complex determinants of mental health. There are also several research limitations that may have contributed to the findings, such as lack of individual level data and county level data, as well as limited outcome measures. Nevertheless, these findings suggest that in the aftermath of a terrorist attack or mass shooting, emergency management teams should concentrate their resources on the individuals and communities who are most closely connected to the trauma, such as survivors and friends and family members of victims. Psychological screening of local community members, in the city of town of the incident, for risk factors and symptoms of psychological distress, may also be warranted.

APPENDIX A: Missing Data

Table A1

Missing Data

State	Mean monthly observations	# of months missing (< 300 per month)	% missing
AL	630.3	1	1.3
AK	317.7	29	40
AZ	834.6	7	9
AR	430.6	4	5
CA	943.5	13	18
CO	1068.1	0	0
CT	797	0	0
DE	369.4	20	27
FL	1646.2	3	4
GA	501.8	9	12.5
HI	630.7	0	0
ID	454.5	1	13
IL	439.5	8	11
IN	846.2	0	0
IA	613.7	0	0
KS	1452.7	0	0
KY	838.2	0	0
LA	490.3	17	24
ME	772.2	1	13
MD	1138.1	1	13
MA	1056.4	2	2
MI	874.3	2	2
MN	1287.1	3	4

MS	484.9	11	15
MO	589.4	2	2
MT	600.7	5	7
NE	1470.1	0	0
NV	338.4	27	37
NH	541.3	3	4
NJ	995.1	9	12.5
NM	636.9	0	0
NY	1110	4	5
NC	632.8	0	0
ND	526.4	0	0
OH	991.9	0	0
OK	618.8	0	0
OR	448.2	6	8
PA	845.8	7	9
RI	490.5	4	5
SC	937	0	0
SD	578.6	0	0
TN	490.9	7	9
TX	1015.2	0	0
UT	1000.2	2	2
VT	529.5	0	0
VA	720.2	4	5
WA	1102.6	1	13
WV	494.6	1	13
WI	500	8	11
WY	461.4	9	12.5

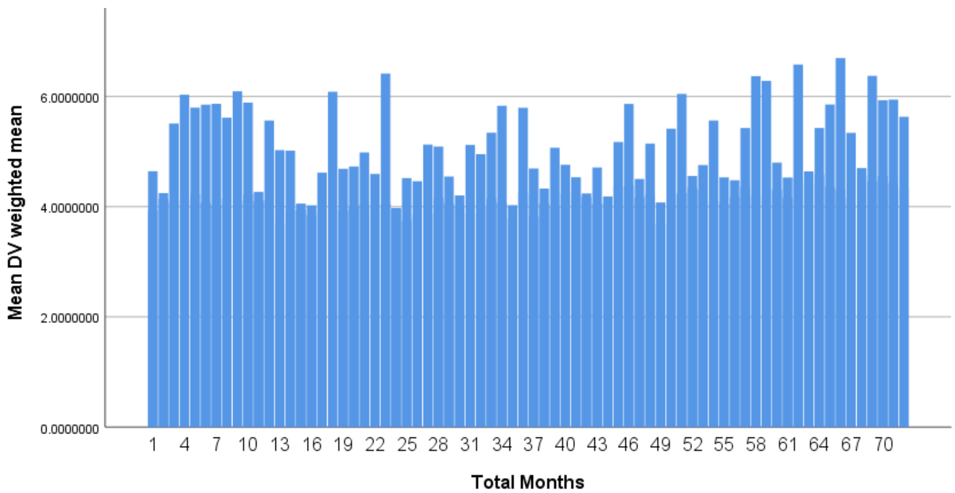
Note. States with > 20% of missing data were removed from the trimmed sample (AK, DE, LA, & NV)

APPENDIX B: Histograms of Dependent Variable

Note. A dashed white line represents a lethal terrorist attack occurring that month. A solid white line represents a public mass-shooting occurring that month.

Figure B1

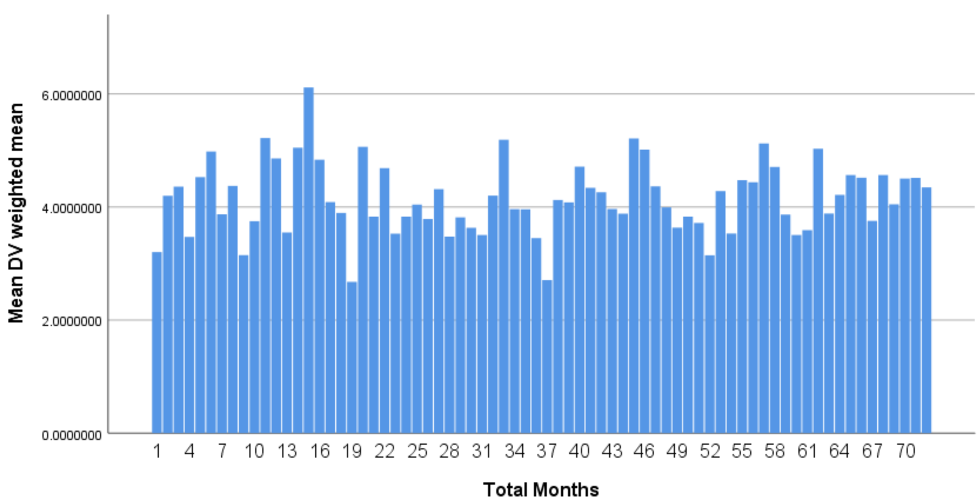
Average Monthly Number of Poor Mental Health Days in Alabama



State: Alabama
Mean MH measure: 5.122
Range: 3.005
SD: .754
Skewness: .159
Kurtosis: -.920
# of terrorism events: 0
# of mass-shooting events: 0

Figure B2

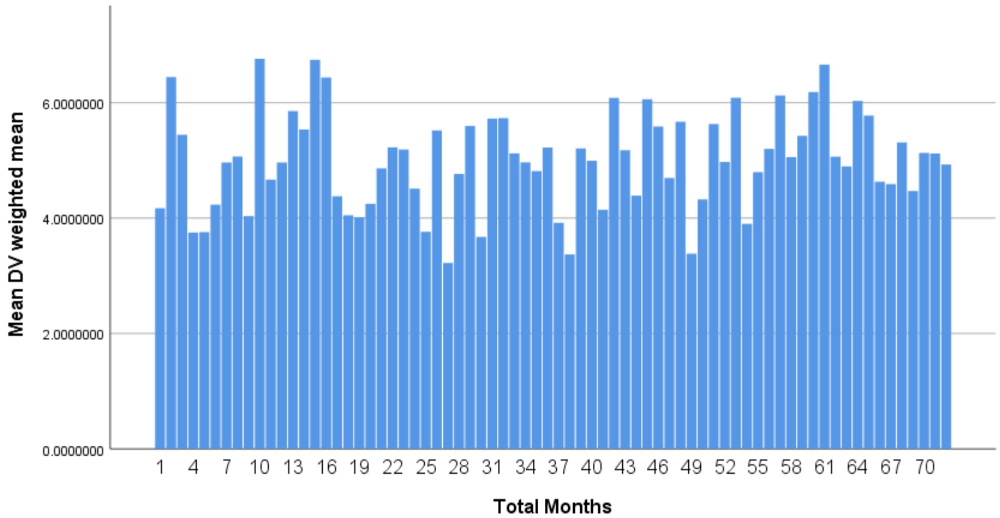
Average Monthly Number of Poor Mental Health Days in Arizona



State: Arizona
Mean MH measure: 4.189
Range: 2.967
SD: .589
Skewness: .607
Kurtosis: .508
# of terrorism events: 0
# of mass-shooting events: 0

Figure B3

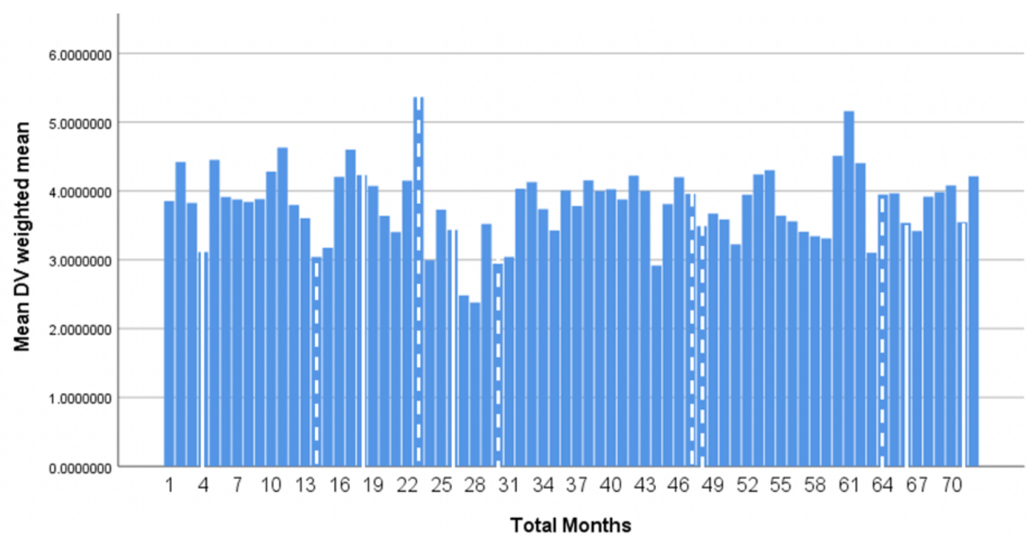
Average Monthly Number of Poor Mental Health Days in Arkansas



State: Arkansas
Mean MH measure: 5.034
Range: 3.54
SD: .830
Skewness: .027
Kurtosis: -.408
# of terrorism events: 0
# of mass-shooting events: 0

Figure B4

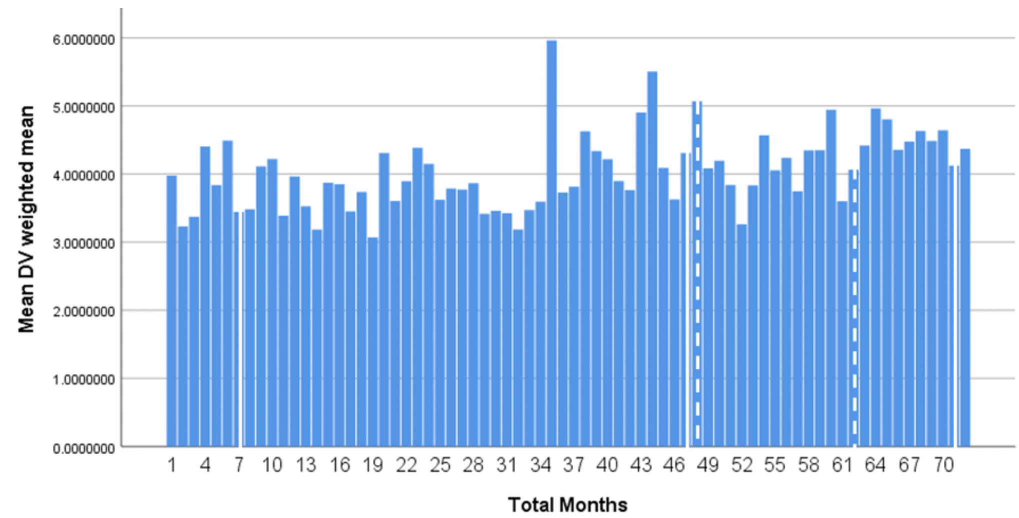
Average Monthly Number of Poor Mental Health Days in California



State: California
Mean MH measure: 3.868
Range: 2.446
SD: .431
Skewness: .431
Kurtosis: 1.468
# of terrorism events: 6
# of mass-shooting events: 5

Figure B5

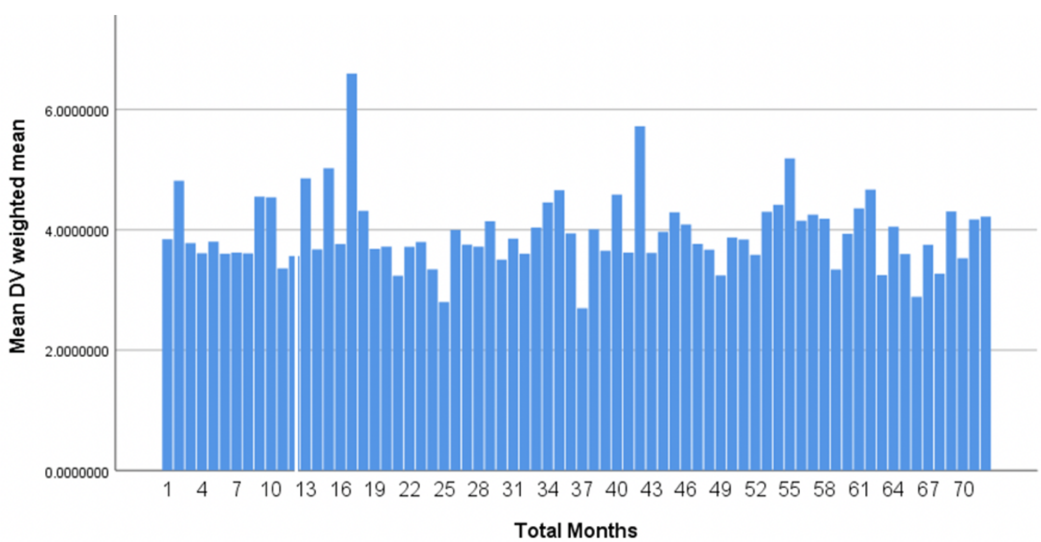
Average Monthly Number of Poor Mental Health Days in Colorado



State Colorado
Mean MH measure: 4.039
Range: 2.89
SD: .556
Skewness: .799
Kurtosis: 1.132
# of terrorism events: 2
# of mass-shooting events: 3

Figure B6

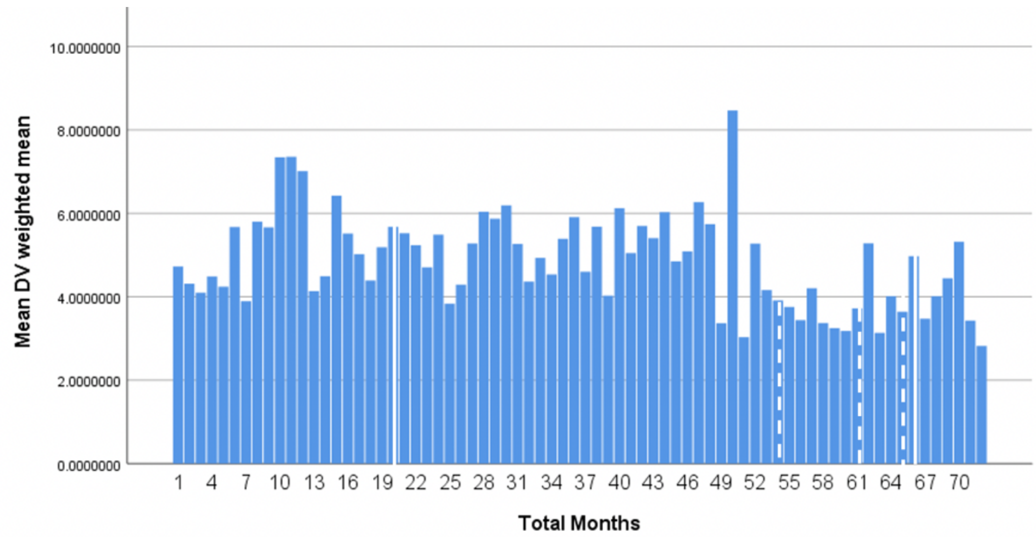
Average Monthly Number of Poor Mental Health Days in Connecticut



State: Connecticut
Mean MH measure: 3.957
Range: 3.903
SD: .622
Skewness: 1.355
Kurtosis: 4.195
# of terrorism events: 0
# of mass-shooting events: 1

Figure B7

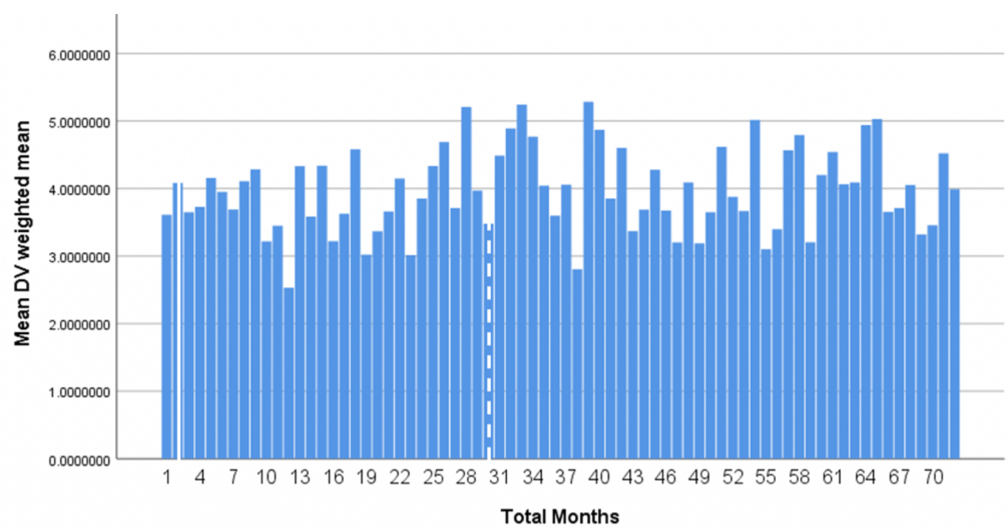
Average Monthly Number of Poor Mental Health Days in Florida



State: Florida
Mean MH measure: 4.864
Range: 4.537
SD: 1.02
Skewness: .224
Kurtosis: -.351
# of terrorism events: 3
# of mass-shooting events: 2

Figure B8

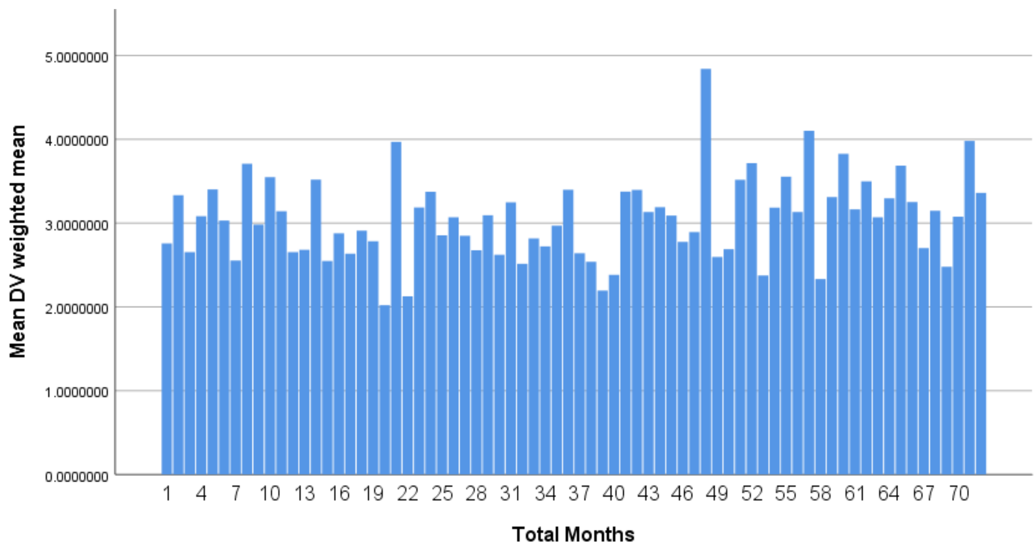
Average Monthly Number of Poor Mental Health Days in Georgia



State: Georgia
Mean MH measure: 3.967
Range: 2.269
SD: .577
Skewness: .411
Kurtosis: -.551
# of terrorism events: 1
# of mass-shooting events: 0

Figure B9

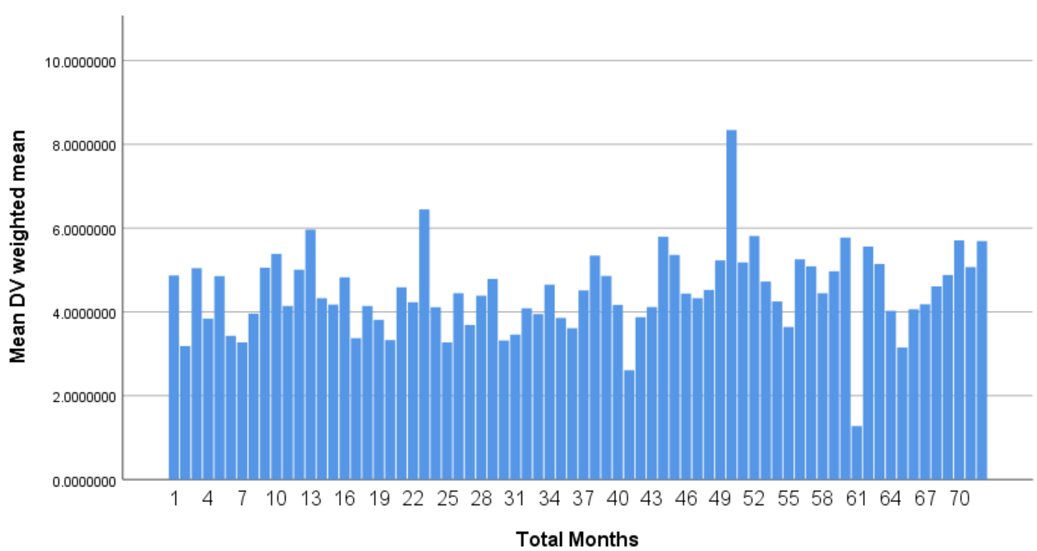
Average Monthly Number of Poor Mental Health Days in Hawaii



State: Hawaii
Mean MH measure: 3.052
Range: 2.819
SD: .499
Skewness: .644
Kurtosis: 1.29
# of terrorism events: 0
# of mass-shooting events: 0

Figure B10

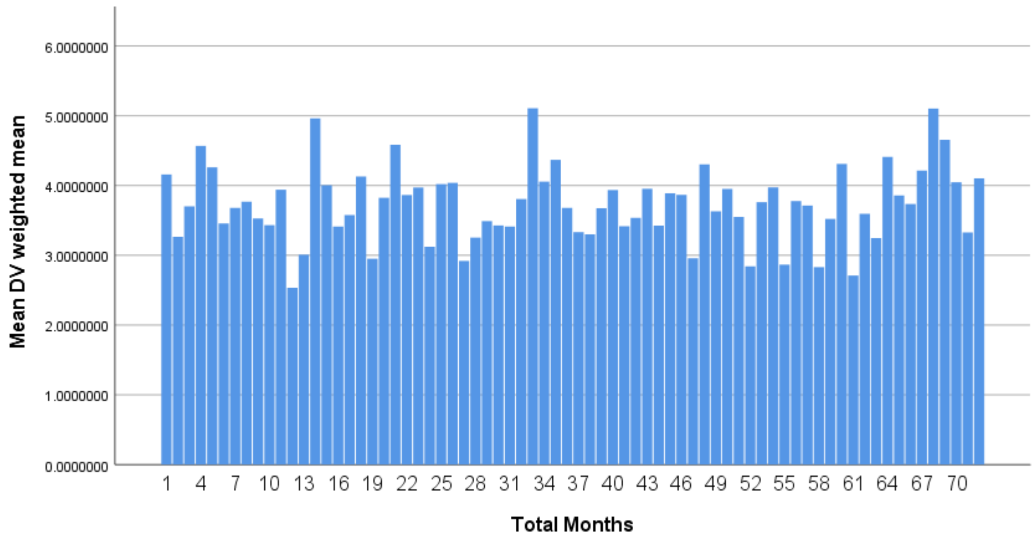
Average Monthly Number of Poor Mental Health Days in Idaho



State: Idaho
Mean MH measure: 4.529
Range: 5.736
SD: .918
Skewness: .978
Kurtosis: 3.038
# of terrorism events: 0
# of mass-shooting events: 0

Figure B11

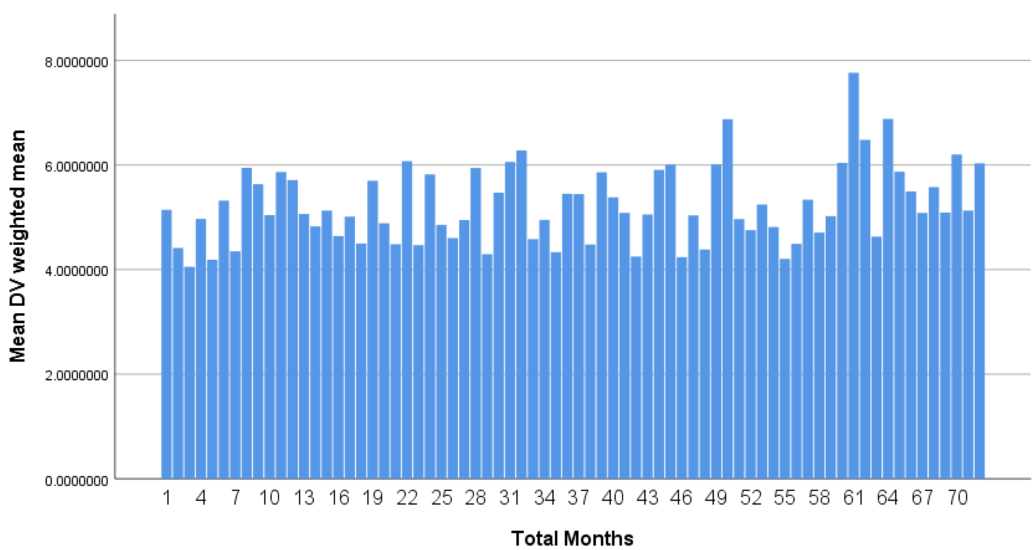
Average Monthly Number of Poor Mental Health Days in Illinois



State: Illinois
Mean MH measure: 3.733
Range: 2.572
SD: .530
Skewness: .318
Kurtosis: .637
# of terrorism events: 0
# of mass-shooting events: 0

Figure B12

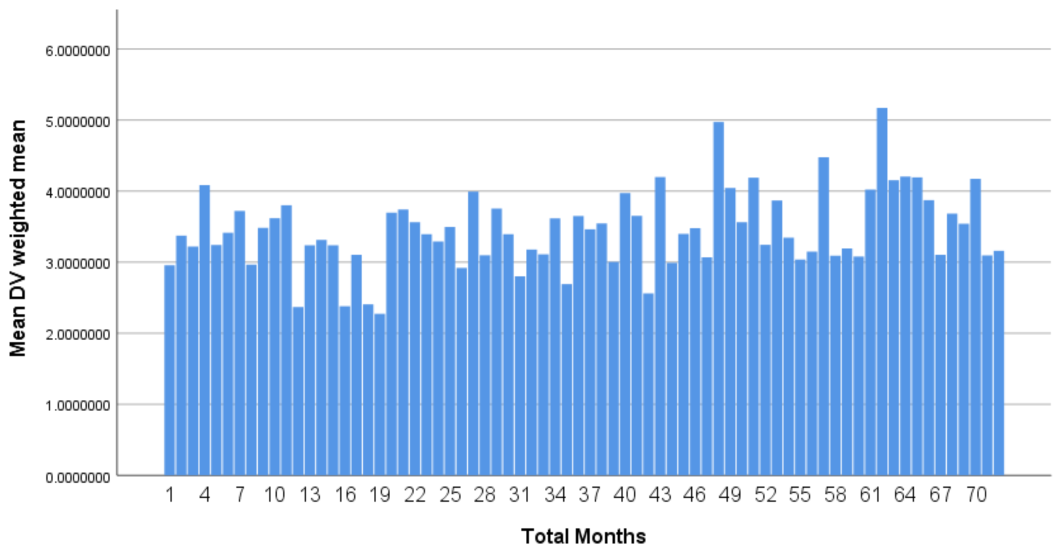
Average Monthly Number of Poor Mental Health Days in Indiana



State: Indiana
Mean MH measure: 5.232
Range: 3.708
SD: .738
Skewness: .777
Kurtosis: .766
# of terrorism events: 0
# of mass-shooting events: 0

Figure B13

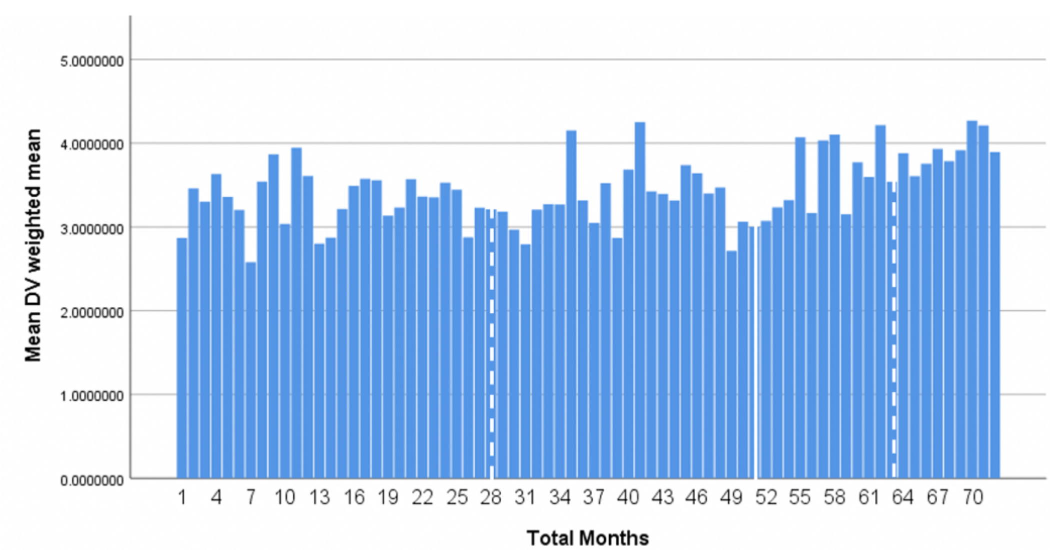
Average Monthly Number of Poor Mental Health Days in Iowa



State: Iowa
Mean MH measure: 3.452
Range: 2.899
SD: .554
Skewness: .462
Kurtosis: .939
# of terrorism events: 0
# of mass-shooting events: 0

Figure B14

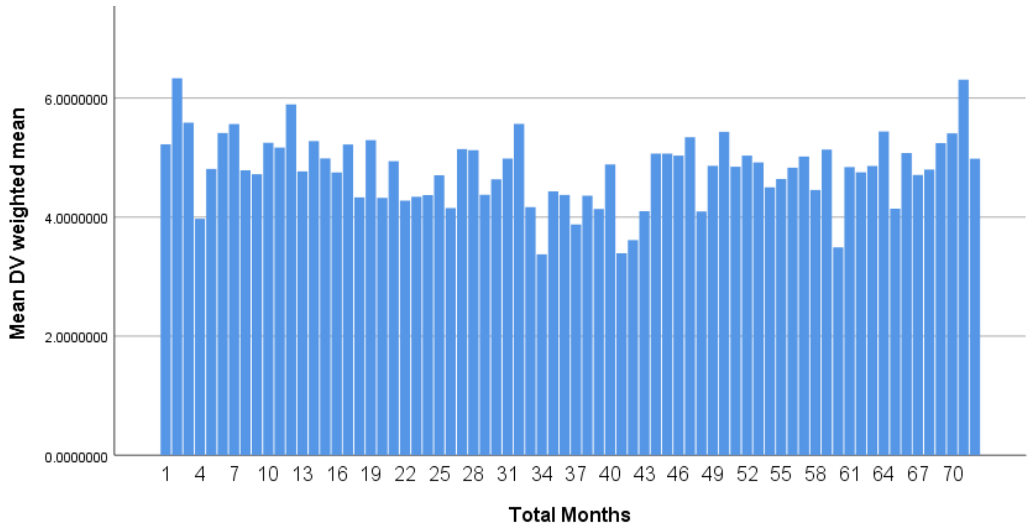
Average Monthly Number of Poor Mental Health Days in Kansas



State: Kansas
Mean MH measure: 3.445
Range: 1.688
SD: .401
Skewness: .192
Kurtosis: -.499
# of terrorism events: 2
# of mass-shooting events: 1

Figure B15

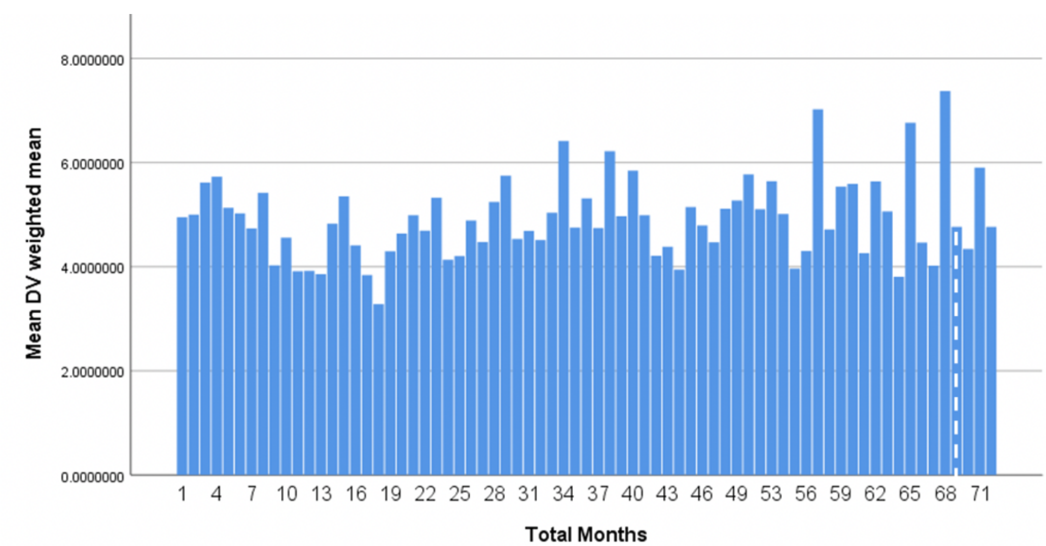
Average Monthly Number of Poor Mental Health Days in Kentucky



State: Kentucky
Mean MH measure: 4.795
Range: 2.955
SD: .594
Skewness: -.113
Kurtosis: .537
# of terrorism events: 0
# of mass-shooting events: 0

Figure B16

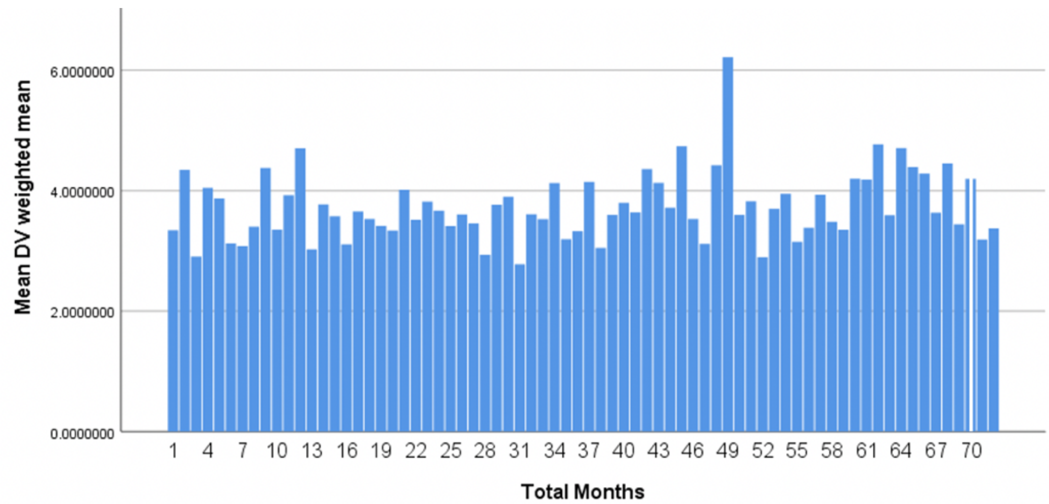
Average Monthly Number of Poor Mental Health Days in Maine



State: Maine
Mean MH measure: 4.920
Range: 4.093
SD: .775
Skewness: .781
Kurtosis: 1.074
# of terrorism events: 1
# of mass-shooting events: 0

Figure B17

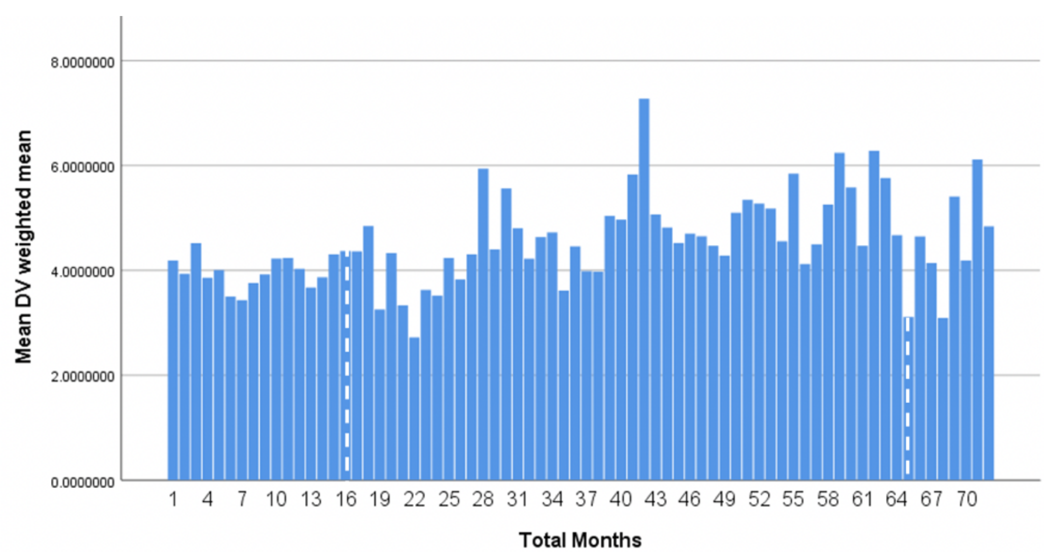
Average Monthly Number of Poor Mental Health Days in Maryland



State: Maryland
Mean MH measure: 3.697
Range: 1.990
SD: .486
Skewness: .375
Kurtosis: -.484
# of terrorism events: 0
# of mass-shooting events: 1

Figure B18

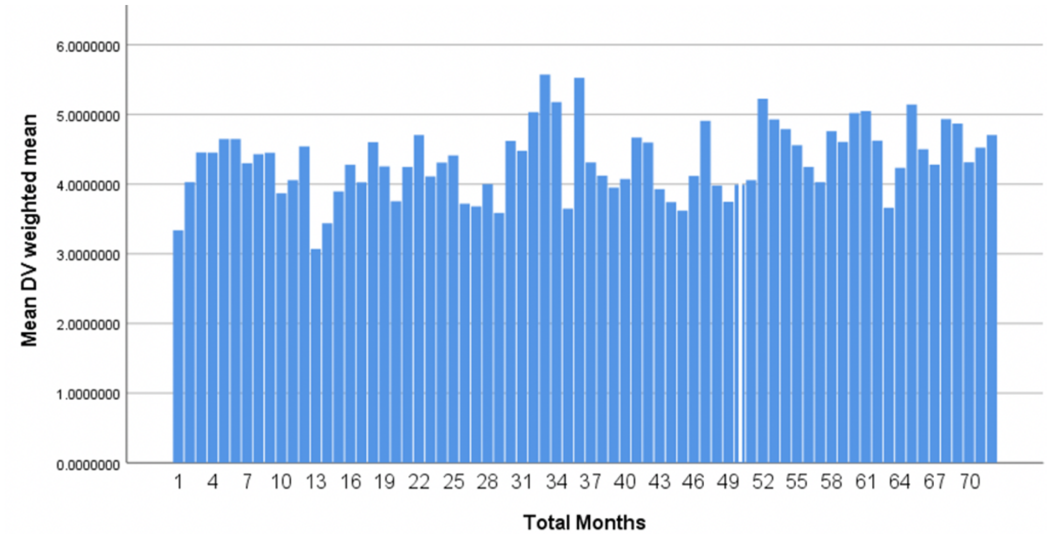
Average Monthly Number of Poor Mental Health Days in Massachusetts



State: Massachusetts
Mean MH measure: 4.529
Range: 4.551
SD: 8.568
Skewness: .603
Kurtosis: .632
# of terrorism events: 2
# of mass-shooting events: 0

Figure B19

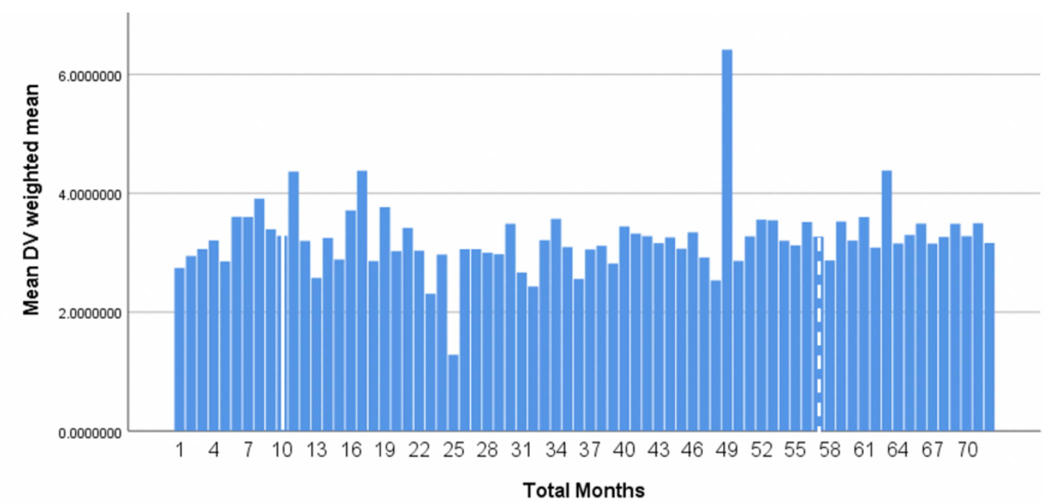
Average Monthly Number of Poor Mental Health Days in Michigan



State: Michigan
Mean MH measure: 4.317
Range: 3.069
SD: .497
Skewness: .003
Kurtosis: -.201
# of terrorism events: 0
# of mass-shooting events: 1

Figure B20

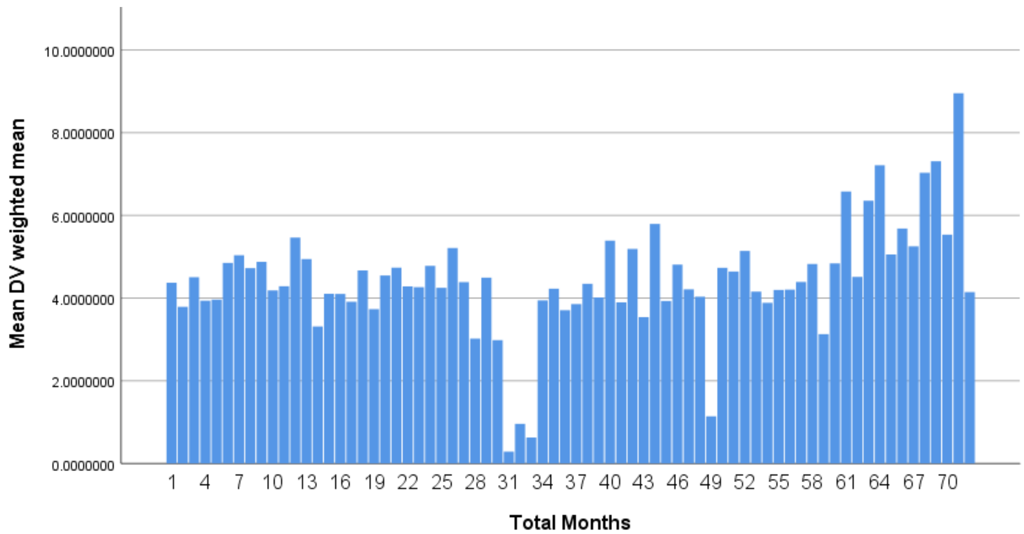
Average Monthly Number of Poor Mental Health Days in Minnesota



State Minnesota
Mean MH measure: 3.228
Range: 2.075
SD: .402
Skewness: .616
Kurtosis: 1.585
# of terrorism events: 1
# of mass-shooting events: 0

Figure B21

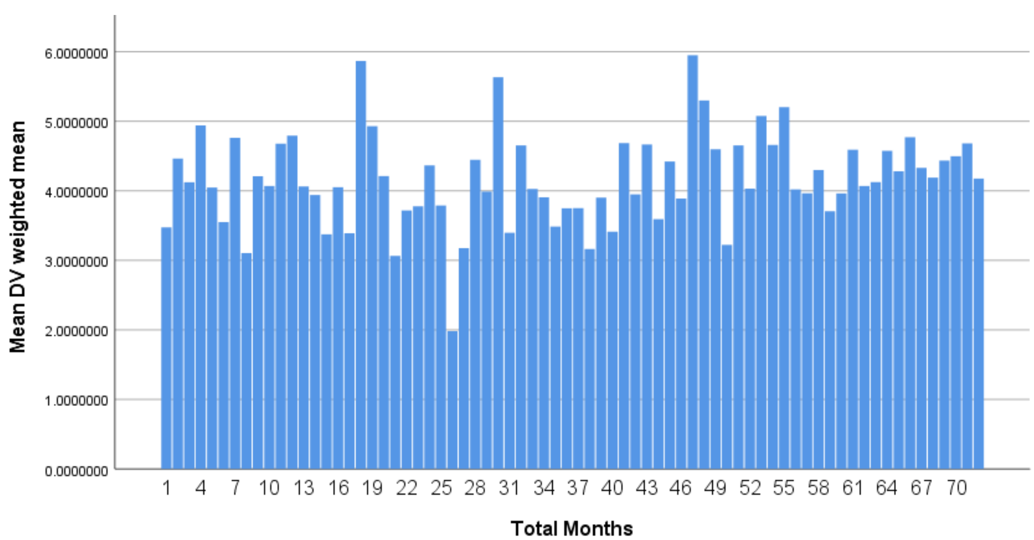
Average Monthly Number of Poor Mental Health Days in Mississippi



State: Mississippi
Mean MH measure: 4.435
Range: 8.668
SD: 1.352
Skewness: -.205
Kurtosis: 3.377
# of terrorism events: 0
# of mass-shooting events: 0

Figure B22

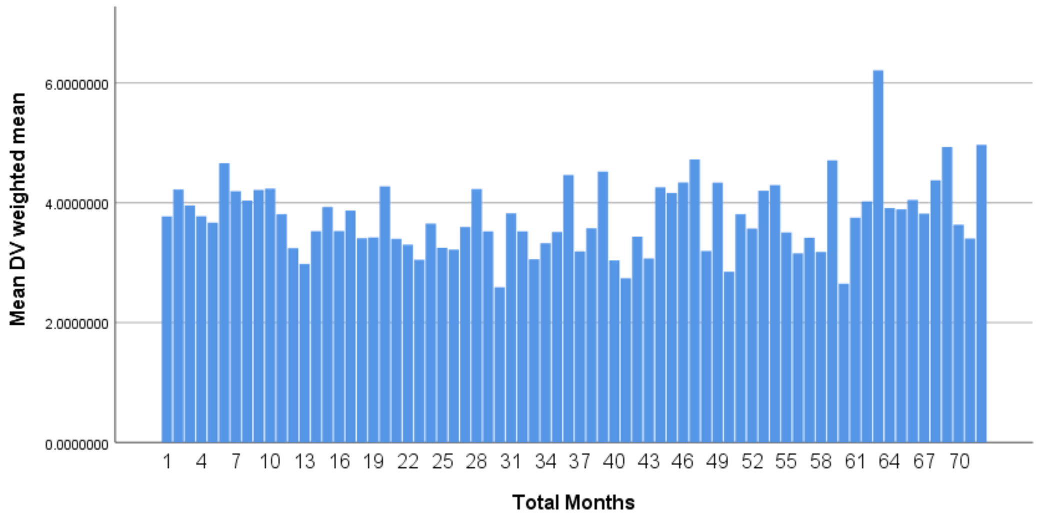
Average Monthly Number of Poor Mental Health Days in Missouri



State: Missouri
Mean MH measure: 4.168
Range: 3.968
SD: .682
Skewness: .020
Kurtosis: 1.158
# of terrorism events: 0
# of mass-shooting events: 0

Figure B23

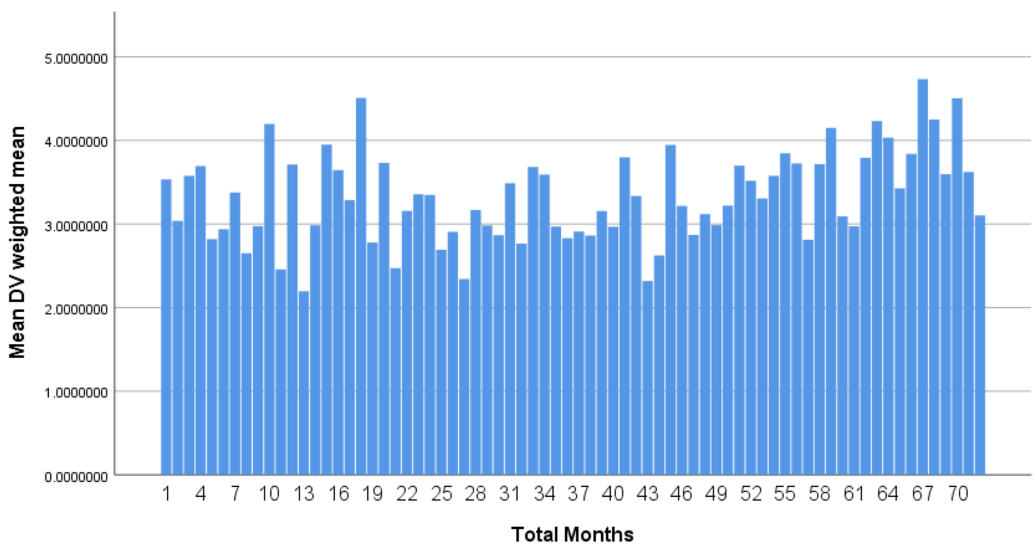
Average Monthly Number of Poor Mental Health Days in Montana



State: Montana
Mean MH measure: 3.714
Range: 2.342
SD: .537
Skewness: .095
Kurtosis: -.569
# of terrorism events: 0
# of mass-shooting events: 0

Figure B24

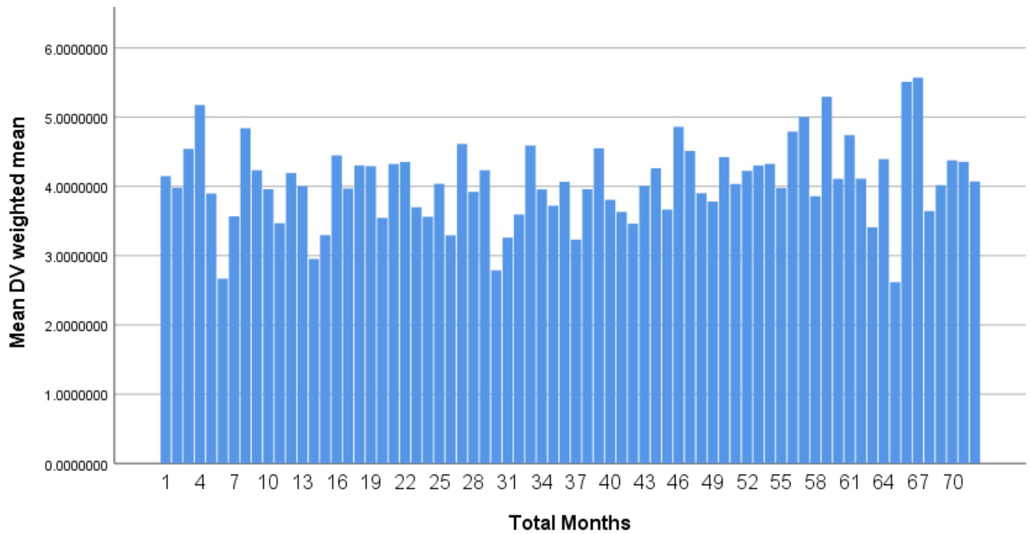
Average Monthly Number of Poor Mental Health Days in Nebraska



State: Nebraska
Mean MH measure: 3.326
Range: 2.538
SD: .550
Skewness: .298
Kurtosis: -.257
# of terrorism events: 0
# of mass-shooting events: 0

Figure B25

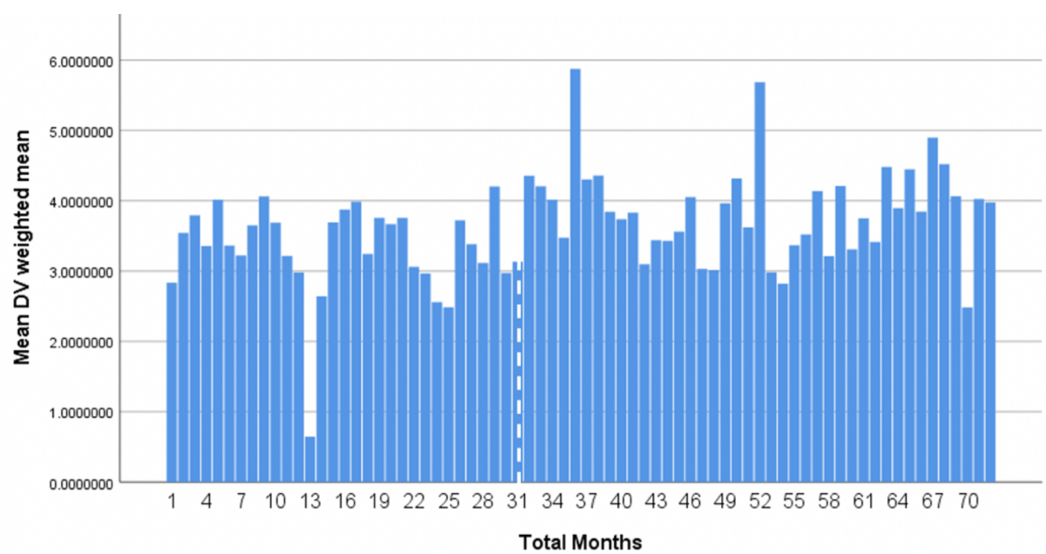
Average Monthly Number of Poor Mental Health Days in New Hampshire



State: New Hampshire
Mean MH measure: 4.030
Range: 2.954
SD: .573
Skewness: -.077
Kurtosis: .657
# of terrorism events: 0
# of mass-shooting events: 0

Figure B26

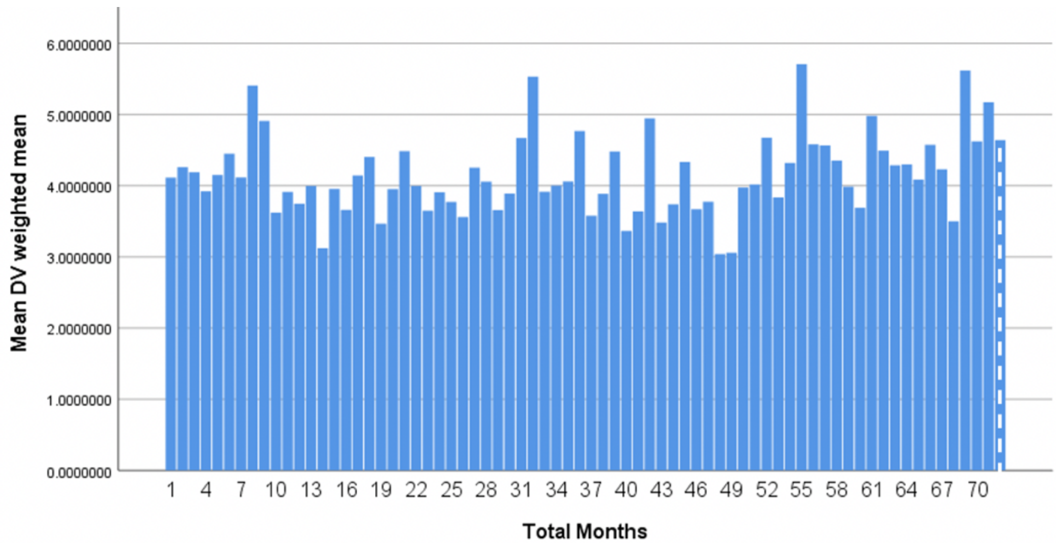
Average Monthly Number of Poor Mental Health Days in New Jersey



State: New Jersey
Mean MH measure: 3.673
Range: 3.393
SD: .640
Skewness: .986
Kurtosis: 2.276
# of terrorism events: 1
# of mass-shooting events: 0

Figure B27

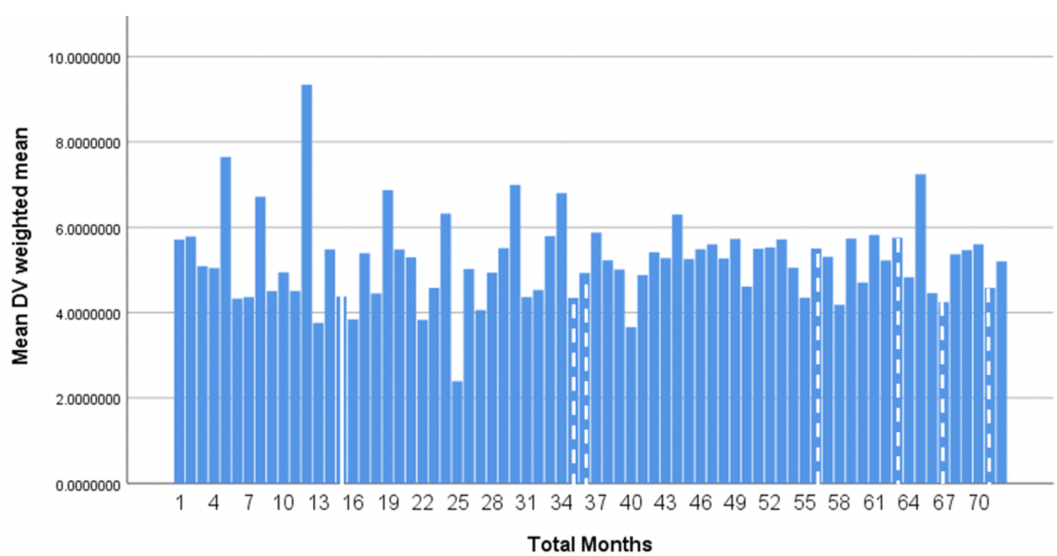
Average Monthly Number of Poor Mental Health Days in New Mexico



State: New Mexico
Mean MH measure: 4.151
Range: 2.668
SD: .566
Skewness: .676
Kurtosis: .648
# of terrorism events: 1
# of mass-shooting events: 0

Figure B28

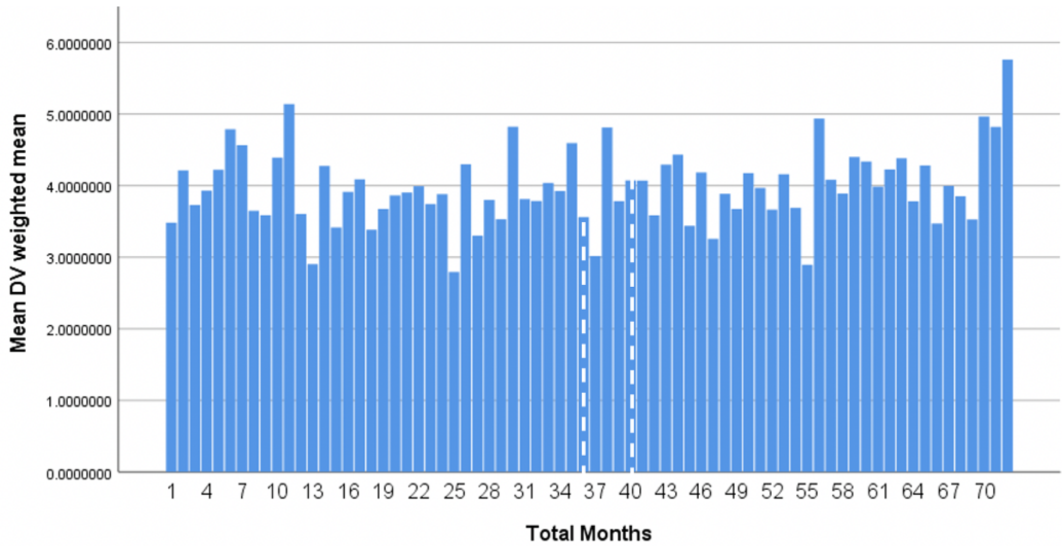
Average Monthly Number of Poor Mental Health Days in New York



State: New York
Mean MH measure: 5.247
Range: 5.681
SD: .930
Skewness: 1.394
Kurtosis: 4.604
# of terrorism events: 6
# of mass-shooting events: 1

Figure B29

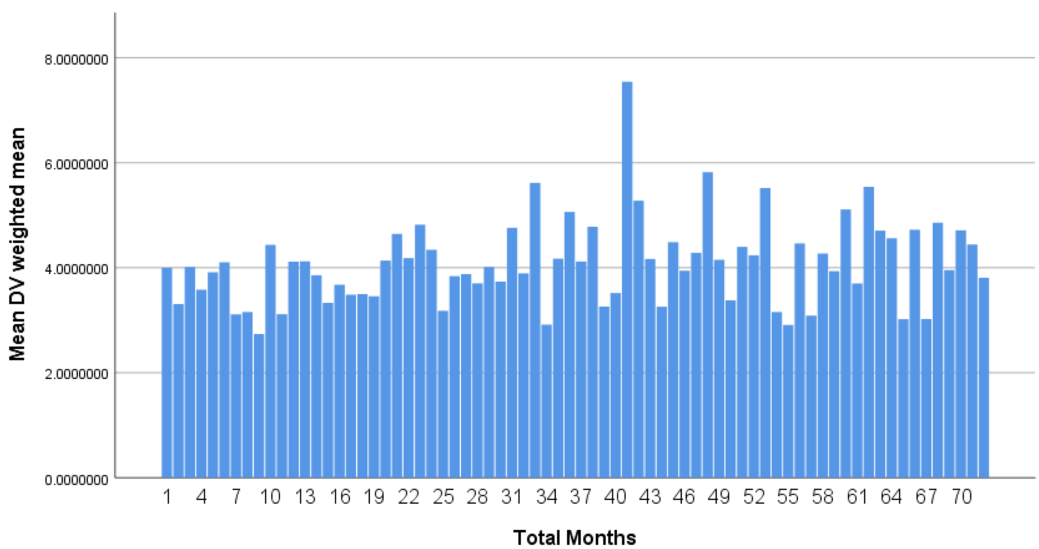
Average Monthly Number of Poor Mental Health Days in North Carolina



State: North Carolina
Mean MH measure: 3.977
Range: 2.967
SD: .536
Skewness: .493
Kurtosis: 1.090
# of terrorism events: 2
# of mass-shooting events: 0

Figure B30

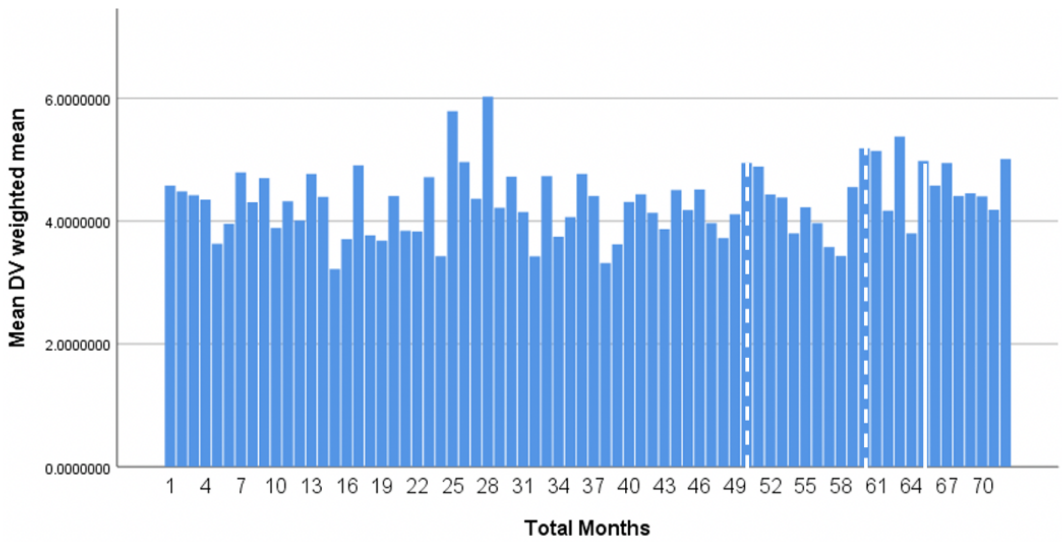
Average Monthly Number of Poor Mental Health Days in North Dakota



State: North Dakota
Mean MH measure: 4.083
Range: 4.808
SD: .818
Skewness: 1.217
Kurtosis: 3.359
# of terrorism events: 0
# of mass-shooting events: 0

Figure B31

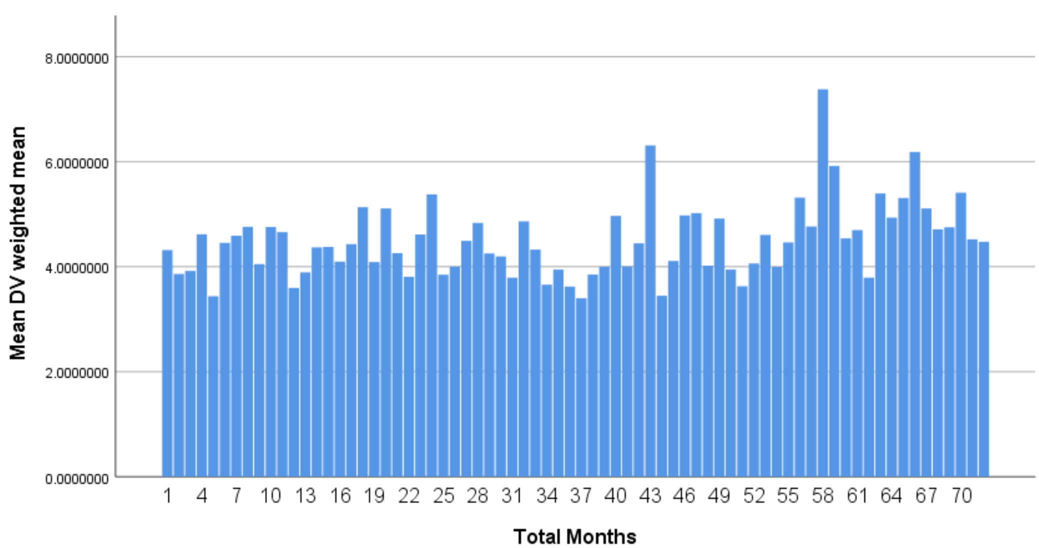
Average Monthly Number of Poor Mental Health Days in Ohio



State: Ohio
Mean MH measure: 4.320
Range: 2.811
SD: .557
Skewness: .454
Kurtosis: .520
# of terrorism events: 2
# of mass-shooting events: 1

Figure B32

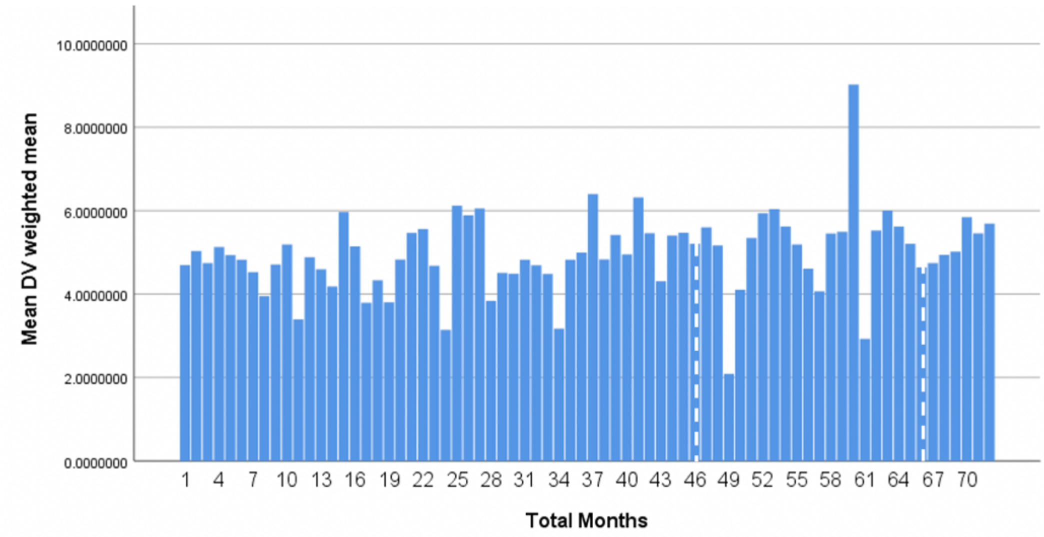
Average Monthly Number of Poor Mental Health Days in Oklahoma



State: Oklahoma
Mean MH measure: 4.497
Range: 3.980
SD: .712
Skewness: 1.295
Kurtosis: 3.055
# of terrorism events: 0
# of mass-shooting events: 0

Figure B33

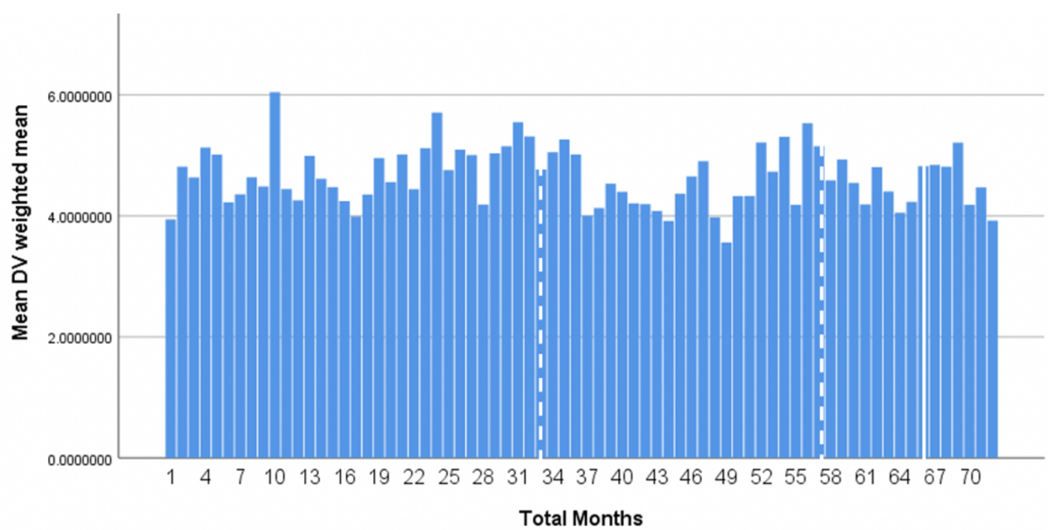
Average Monthly Number of Poor Mental Health Days in Oregon



State: Oregon
Mean MH measure: 5.021
Range: 5.852
SD: .830
Skewness: 1.468
Kurtosis: 7.472
# of terrorism events: 2
# of mass-shooting events: 0

Figure B34

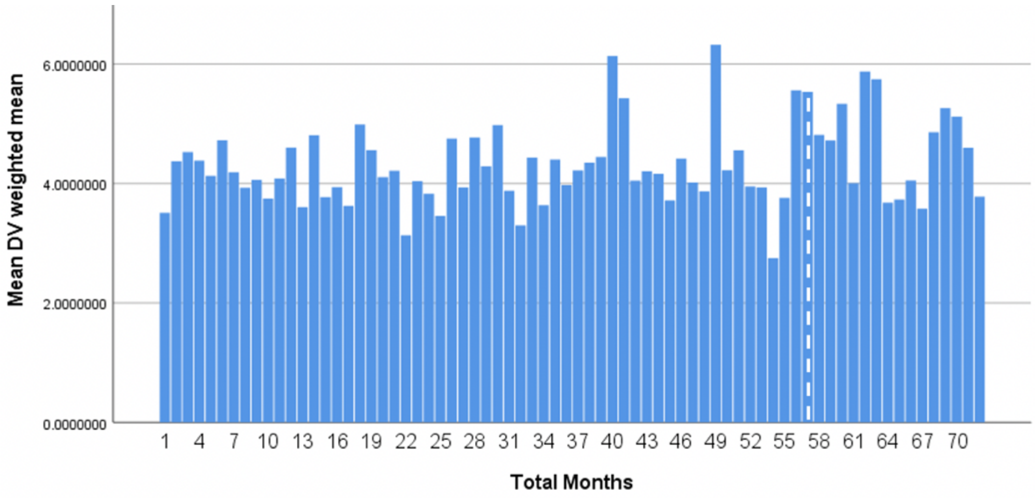
Average Monthly Number of Poor Mental Health Days in Pennsylvania



State: Pennsylvania
Mean MH measure: 4.663
Range: 2.483
SD: .487
Skewness: .368
Kurtosis: -.055
# of terrorism events: 2
# of mass-shooting events: 1

Figure B35

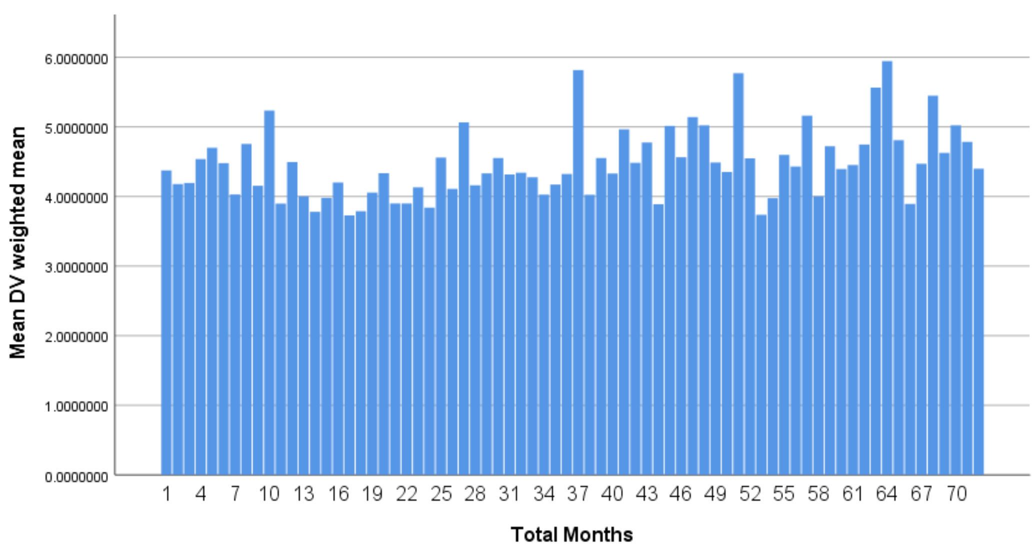
Average Monthly Number of Poor Mental Health Days in Rhode Island



State: Rhode Island
Mean MH measure: 4.303
Range: 3.389
SD: .665
Skewness: .610
Kurtosis: .415
# of terrorism events: 1
# of mass-shooting events: 0

Figure B36

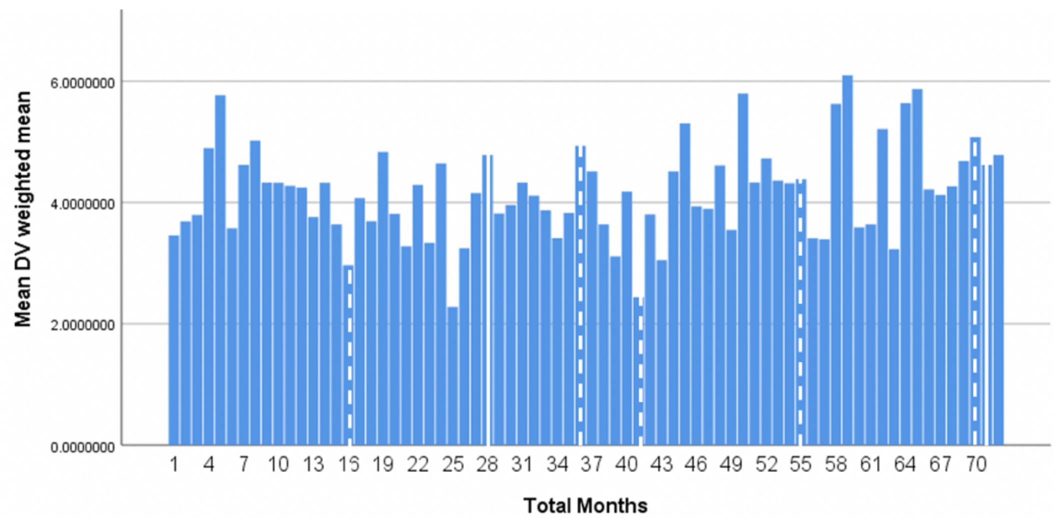
Average Monthly Number of Poor Mental Health Days in South Carolina



State: South Carolina
Mean MH measure: 4.469
Range: 2.216
SD: .506
Skewness: .948
Kurtosis: .751
# of terrorism events: 0
# of mass-shooting events: 0

Figure B37

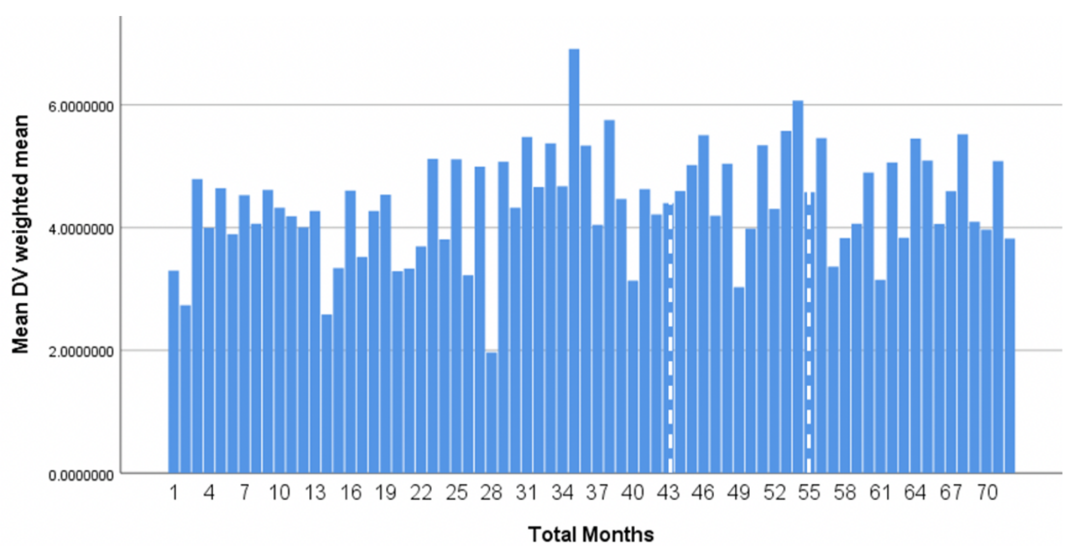
Average Monthly Number of Poor Mental Health Days in South Dakota



State: South Dakota
Mean MH measure: 2.903
Range: 3.018
SD: .581
Skewness: .217
Kurtosis: 1.66
# of terrorism events: 1
# of mass-shooting events: 0

Figure B38

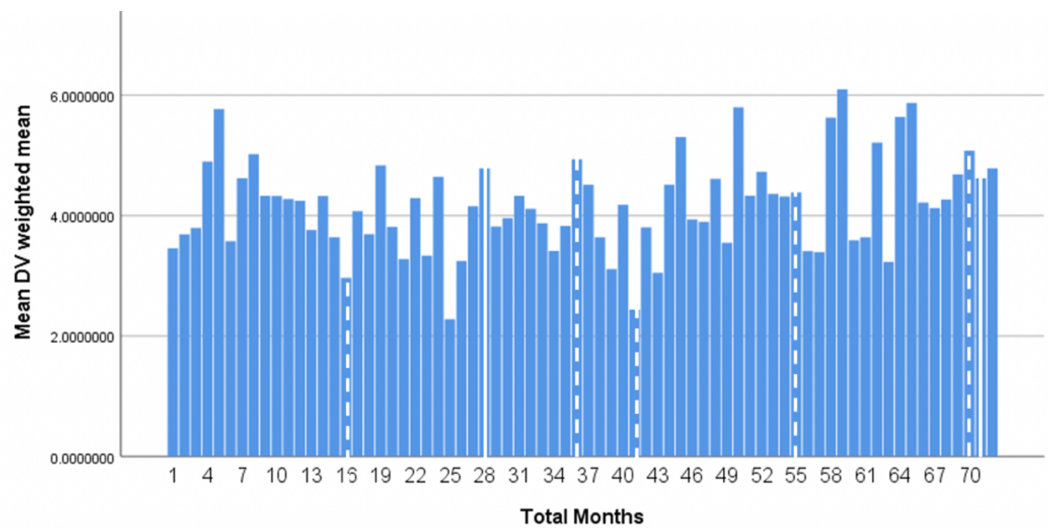
Average Monthly Number of Poor Mental Health Days in Tennessee



State: Tennessee
Mean MH measure: 4.419
Range: 4.326
SD: .835
Skewness: .202
Kurtosis: .270
# of terrorism events: 2
# of mass-shooting events: 0

Figure B39

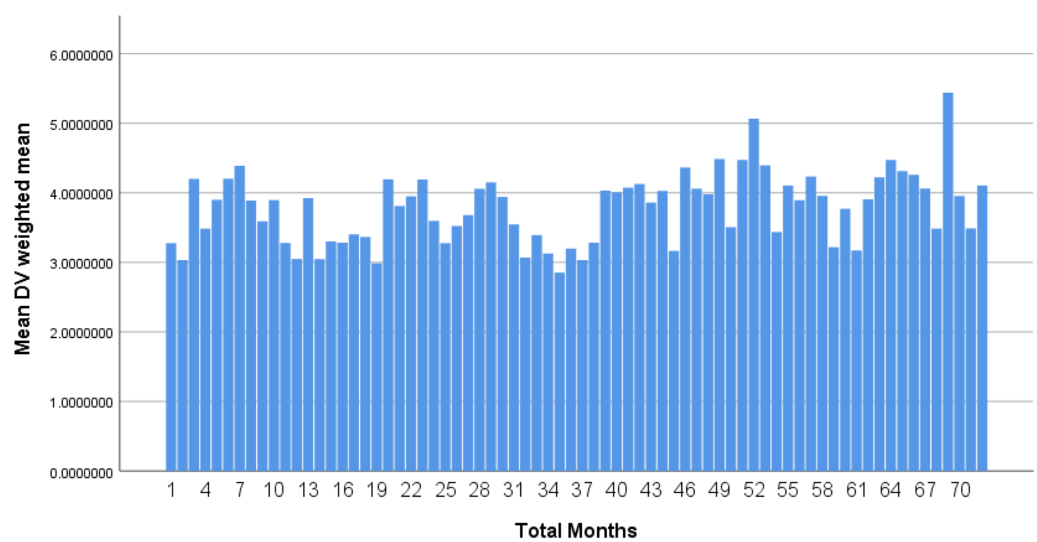
Average Monthly Number of Poor Mental Health Days in Texas



State: Texas
Mean MH measure: 4.212
Range: 3.660
SD: .755
Skewness: .419
Kurtosis: .061
# of terrorism events: 5
# of mass-shooting events: 2

Figure B40

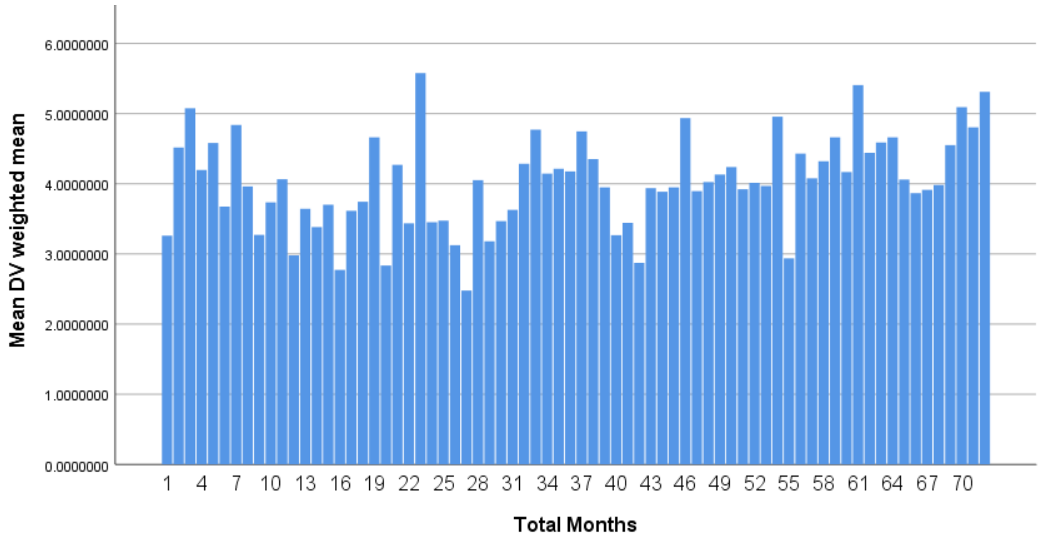
Average Monthly Number of Poor Mental Health Days in Utah



State: Utah
Mean MH measure: 3.801
Range: 2.584
SD: .505
Skewness: .371
Kurtosis: .491
# of terrorism events: 0
# of mass-shooting events: 0

Figure B41

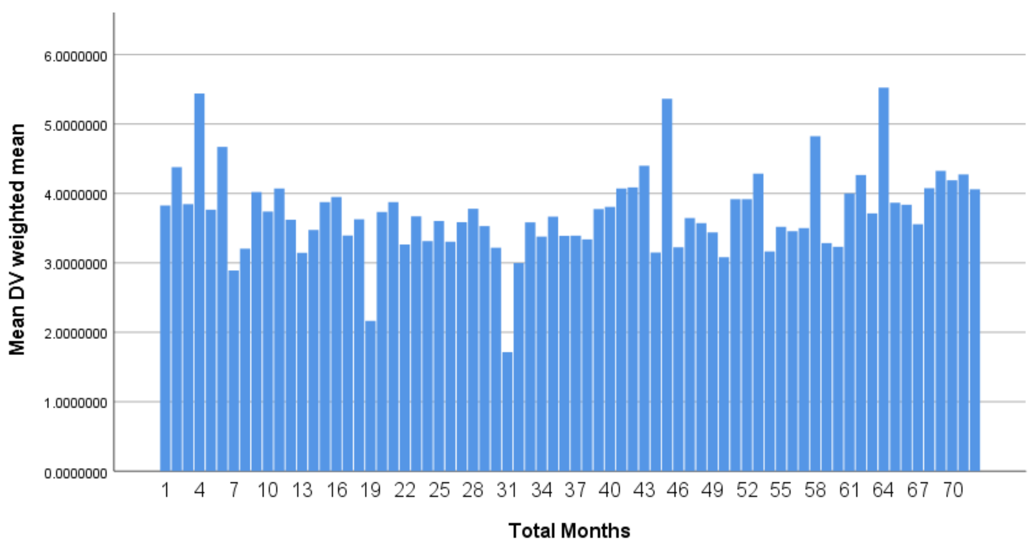
Average Monthly Number of Poor Mental Health Days in Vermont



State: Vermont
Mean MH measure: 4.027
Range: 3.099
SD: .657
Skewness: .009
Kurtosis: -.185
# of terrorism events: 0
# of mass-shooting events: 0

Figure B42

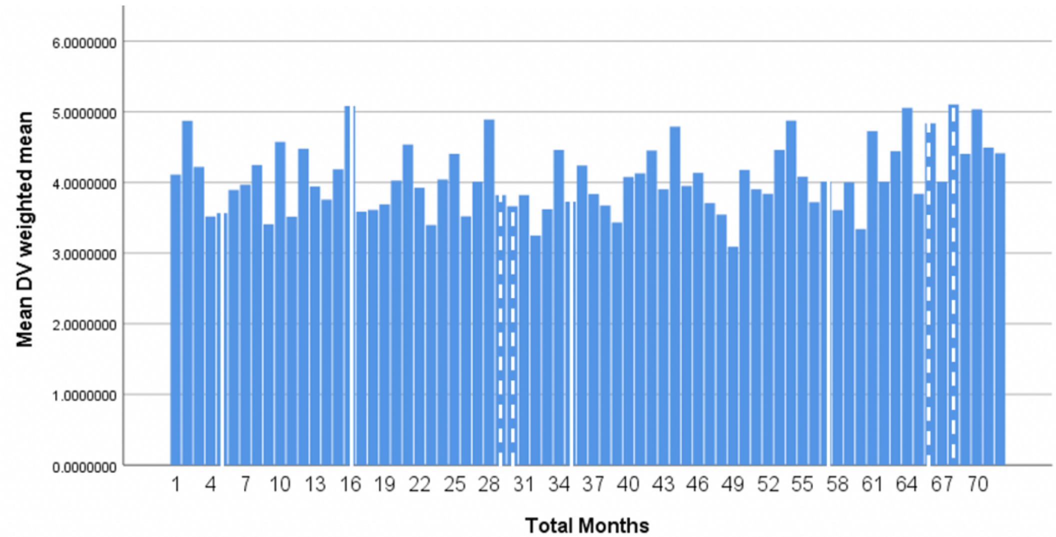
Average Monthly Number of Poor Mental Health Days in Virginia



State: Virginia
Mean MH measure: 3.756
Range: 3.361
SD: .570
Skewness: .865
Kurtosis: 2.377
# of terrorism events: 0
# of mass-shooting events: 0

Figure B43

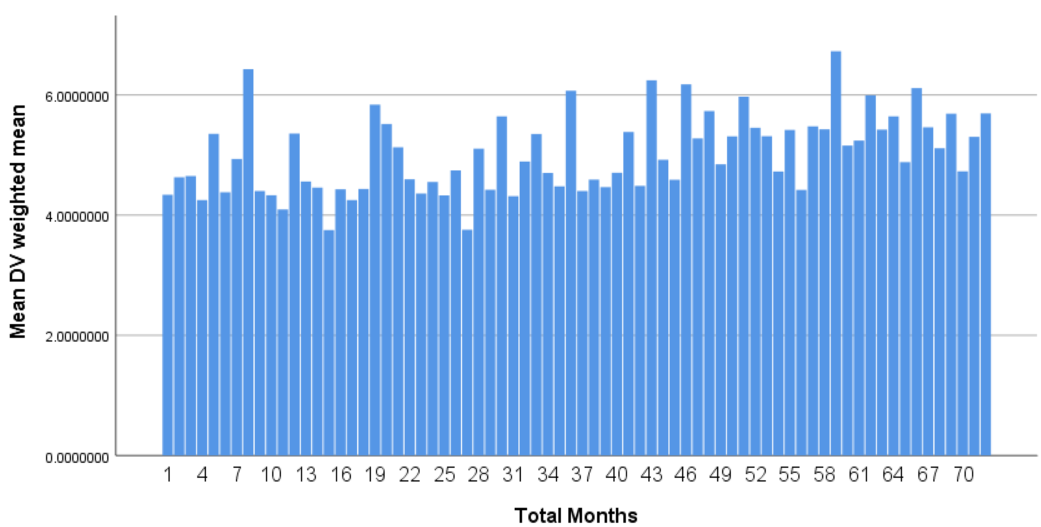
Average Monthly Number of Poor Mental Health Days in Washington



State: Washington
Mean MH measure: 5.104
Range: 2.012
SD: .485
Skewness: .415
Kurtosis: -.502
# of terrorism events: 4
# of mass-shooting events: 4

Figure B44

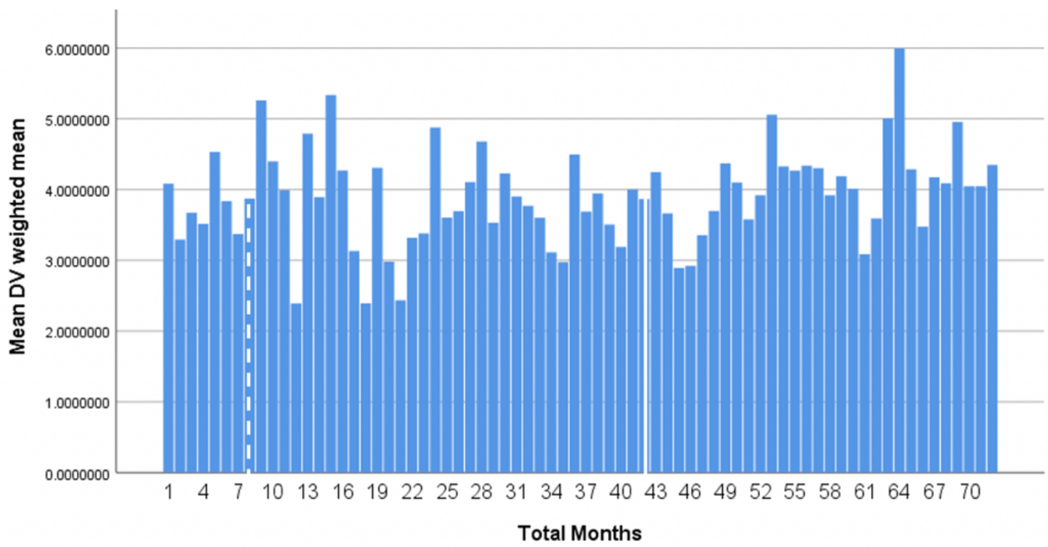
Average Monthly Number of Poor Mental Health Days in West Virginia



State: West Virginia
Mean MH measure: 5.013
Range: 2.976
SD: .654
Skewness: .446
Kurtosis: -.414
# of terrorism events: 0
# of mass-shooting events: 0

Figure B45

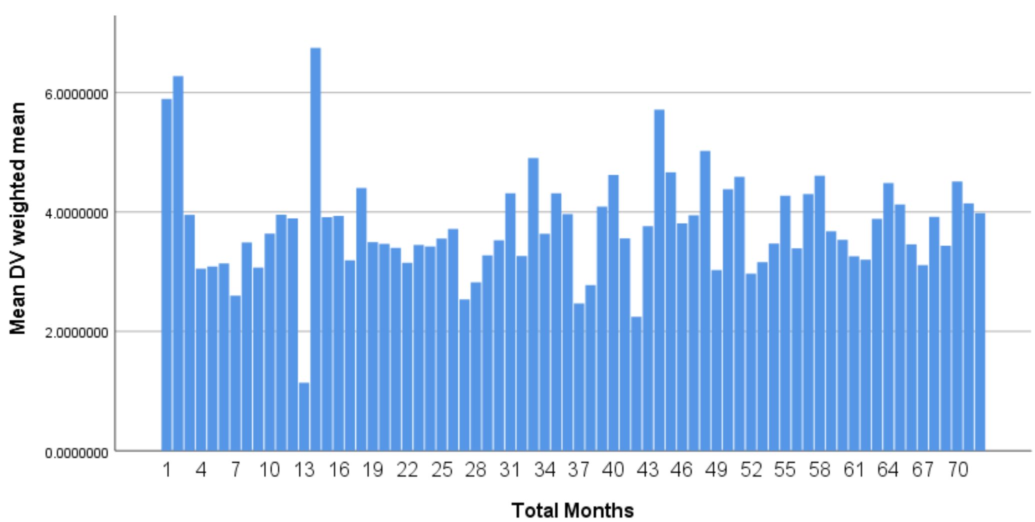
Average Monthly Number of Poor Mental Health Days in Wisconsin



State: Wisconsin
Mean MH measure: 3.909
Range: 3.602
SD: .682
Skewness: .211
Kurtosis: .703
# of terrorism events: 1
# of mass-shooting events: 1

Figure B46

Average Monthly Number of Poor Mental Health Days in Wyoming



State: Wyoming
Mean MH measure: 3.738
Range: 4.029
SD: .778
Skewness: .992
Kurtosis: 1.713
# of terrorism events: 0
# of mass-shooting events: 0

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